Corporate Communications with Government Executive Officials: Evidence from the STOCK Act

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Abstract

This paper examines how insider trading restrictions on government officials affect corporate disclosure by altering incentives to share information with firms. The Stop Trading on Congressional Knowledge (STOCK) Act prohibited executive branch officials from profiting from non-public information, potentially reducing firms' ability to obtain insights relevant for forecasting and disclosure. Using a difference-in-differences design, we find that firms with significant government contracts reduce management forecasts post-STOCK Act, with the effect strongest among firms highly dependent on government sales, politically engaged firms, and those in competitive industries. This decline coincides with a shift from point to range forecasts, consistent with increased uncertainty. We also document reduced price informativeness and a higher implied cost of capital, highlighting broader capital market consequences. Our findings provide new evidence on how regulatory efforts to limit insider trading can have unintended effects on corporate transparency and investor information environments.

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

***JEL Code*:** 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; Nagar, Schoenfeld, and Wellman, 2019). 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 privledged 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.

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. For instance, former Health and Human Services Secretary Tom Price, while serving in Congress, traded shares in health-related companies shortly before sponsoring or voting on favorable legislation. He also purchased discounted stock in Innate Immunotherapeutics after receiving a private investment offer. Although his trades complied with disclosure rules at the time, they fueled concerns about potential self-dealing and highlighted the type of conflicted behavior the STOCK Act aimed to deter through enhanced transparency and restrictions on the use of non-public information.[1]

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.[2] Even today, government officials hold financial stakes in firms they regulate, creating both access to non-public information and incentives for informal exchanges.[3]

Although the Act does not explicitly prohibit private communication between firms and officials, it

[1] See *New York Times*, "Tom Price's Investments Echo Rounds of Questions Before," January 17, 2017, https://www.nytimes.com/2017/01/17/us/politics/tom-price-investments.html.

[2] The 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.

[3] A 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-wsjs-investigation-into-the-stock-trades-of-government-officials-11665491360?mod=djem10point.

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.

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 non-EPS (earnings per share) 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.

We find that firms with substantial government contracts experienced an 8.62% decline in management forecast frequency, equivalent to 0.31 fewer forecasts per firm per year. This reduction in disclosure coincides with a shift from precise point forecasts to broader range forecasts, with the likelihood of issuing range forecasts increasing by 8.6% post-STOCK Act, indicating heightened uncertainty. The decline in forecast frequency is most pronounced for firms facing high demand uncertainty from government contracts and those with greater political engagement, underscoring that firms most reliant on government-sourced information were disproportionately affected. Robustness checks confirm that the forecast frequency results hold across alternative sample periods, forecast windows, and the exclusion of regulated industries, with pre-trend power analysis from Roth (2022) providing no evidence to reject the parallel trends assumption. Beyond disclosure effects, several of our results suggest an increase in the firms' information uncertainty. Analyst forecast errors increase, and there is a 12.6% decline in equity price informativeness. Furthermore, firms with major government customers that reduced their voluntary disclosures after the STOCK Act's implementation experienced a significant increase in their implied cost of capital. Together, these findings highlight the far-reaching consequences of restricted government

information flows on corporate disclosure and capital market outcomes.

Further, we explore the heterogeneity in disclosure responses based on firms' exposure to government-related uncertainty. Firms with high revenue dependence on government contracts typically face lower business risk due to stable demand (Cohen and Li, 2020). However, if the STOCK Act makes it more difficult for senior management to obtain reliable information from government officials, they may struggle to mitigate uncertainty and reduce disclosure as a result (Anantharaman and Zhang, 2011; Guay, Samuels, and Taylor, 2016). Consistent with this mechanism, we find that firms with high exposure to fluctuating government priorities, contract renewals, or regulatory changes exhibit pronounced reductions in management forecast activity, including annual and quarterly forecasts, while maintaining the frequency of EPS forecasts. These firms also shift from precise point forecasts to broader range forecasts, suggesting greater uncertainty in predicting future performance.

We also find that these effects are magnified for firms with strong government ties, such as those heavily reliant on government sales, major government customers, or operating in less concentrated industries. These firms exhibited pronounced reductions in management forecast activity, including annual and quarterly forecasts, while maintaining the frequency of EPS forecasts. This reduction in disclosure activity extended to fewer calendar days with management forecasts and a shift from precise point forecasts to less precise range forecasts. [4] These patterns suggest that firms with high exposure to fluctuating government priorities, contract renewals, or regulatory changes—sources of business uncertainty and political risk—are more affected by changes in the information environment following the STOCK Act.

Furthermore, we find that post- STOCK Act, firms with major government contracts are more likely to replace point forecasts with range forecasts, potentially reflecting greater uncertainty in their ability to predict future performance. Importantly, the shift toward range forecasts and reduced precision in earnings guidance cannot be attributed solely to Regulation Fair Disclosure (Reg FD), which was passed in 2000. While Reg FD requires the public disclosure of material non-public information shared with analysts or investors, our findings suggest that the STOCK Act, by limiting access to proprietary information from executive branch officials, plays a more critical role in the observed changes in disclosure behavior. The restricted flow of government-sourced information post-STOCK Act appears to hinder firms' ability to

[4] We also document a decrease in the number of calendar days with management forecasts, consistent with the overall reduction in disclosure activity.

accurately forecast earnings, especially for those heavily dependent on government contracts.

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 political 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 also examine the capital market consequence of the STOCK Act through price informativeness, analyst forecast accuracy, and implied cost of capital (ICC), highlighting the broader consequences of reduced voluntary disclosures. Firms reliant on government contracts exhibited a marked decline in price informativeness, suggesting that diminished disclosures weakened market participants' ability to incorporate firm-specific information into stock prices. At the same time, analyst forecast errors increased while forecast dispersion declined, indicating that analysts struggled to accurately assess firm performance despite reaching more similar estimates. As price informativeness declined, these firms also experienced a notable 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, including both the Senate and the House of Representatives. 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 conse-

quences 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, likely due to their direct involvement in procurement, regulatory enforcement, and policy execution.

To better understand the underlying mechanism, we contextualize our results regarding whether the forecast reductions stem from reduced informal communication by government officials or increased managerial uncertainty. Our evidence suggests that both channels contribute, but the stronger forecast reductions among firms with high government revenue dependence, greater political engagement, and exposure to procurement uncertainty indicate that the loss of private information flow is the dominant driver. If firms were simply reacting to general uncertainty, we would expect more uniform disclosure reductions across firms. Instead, the decline is concentrated in 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—firms maintain EPS forecasts but scale back other guidance and shift to 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 preponderance of 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 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 to be driven by random chance. Third, we test alternative event dates and time windows, confirming that

our findings are not an artifact of time measurement. Fourth, we address sample composition concerns by excluding firms that never issued management forecasts and restricting the sample to unregulated industries, with results remaining statistically significant. Fifth, we estimate a fixed-effects Poisson regression to address potential limitations of log-transformed dependent variables, again confirming the robustness of our findings. Finally, we rule out the influence of outliers by demonstrating that forecast frequency changes occur broadly across firms rather than being driven by a few extreme observations. 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, Core, and Verdi, 2014; Nagar, Schoenfeld, and Wellman, 2019). By focusing on the flow of government-sourced information through private communication with executive branch officers, our findings reveal that restricted access to this critical information disrupts managers' ability to forecast firm performance accurately. However, this disruption does not significantly elevate investor uncertainty to a level that would compel firms to increase voluntary disclosures. These results underscore the distinctive role of government-sourced information in shaping corporate disclosure decisions and highlight the nuanced impact of regulatory changes like the STOCK Act on corporate transparency.

Second, our paper contributes to the literature on political connections by examining how firms leverage government ties to gain access to critical information. Prior research views political connections as a means of securing regulatory advantages, preferential contracting, and financial benefits (e.g., Akey, 2015; Cooper, Gulen, and Ovtchinnikov, 2010; Wellman, 2017; Ovtchinnikov, Reza, and Wu, 2020). However, we show that these connections also serve as key information channels that shape firms' disclosure behavior. While prior studies explore the role of government contracts in influencing firm decisions (Cohen and Li, 2020; Cohen et al., 2022; Samuels, 2021), we focus on how executive branch officials provide firms with regulatory and procurement insights—access that was disrupted by the STOCK Act.

This exogenous shock to government-firm information flows heightened uncertainty, leading firms to scale back voluntary disclosures. Our findings refine the literature by demonstrating that political ties do more than yield financial advantages—they also influence corporate transparency. By differentiating between procurement-based executive relationships and broader legislative ties, we refine the literature, demonstrating that political connections do more than yield financial advantages—they also shape corporate transparency. This distinction underscores the broader implications of regulatory constraints on government-firm interactions.

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; Huang and Xuan, 2023; Lazzaretto, 2024; Wei and Zhou, 2023; Hanousek Jr et al., 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. 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 that efforts to curb individual misconduct must carefully consider their broader effects on firms' ability to communicate effectively with investors.

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 an unprecedentedly 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 ignited an immediate backlash, forcing Congress to act.

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.[5] This rapid legislative shift is particularly striking given that three previous versions of the bill had languished in committee, underscoring 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 in discretionary information-sharing, even in cases where communication remained legally permissible under FAR.[6] 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

[5] A 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).

[6] The 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.

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.[7] . 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.[8] 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.

In 2023, Senator Kirsten Gillibrand introduced the STOCK Act 2.0 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.[9] A key factor behind widespread non-compliance is the nominal fine of $200 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 introduction of the STOCK Act 2.0 signals continued regulatory efforts to address insider trading risks. Even in 2025, Former President Joe Biden endorsed a congressional stock trading ban, reigniting debates about the ethical use of non-public information by government officials.

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

[7] See https://www.congress.gov/bill/113th-congress/senate-bill/716 for details.

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

[9] See https://www.businessinsider.com/congress-stock-act-violations-senate-house-trading-2021-9.

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 hypothesis:

H1: *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: *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 hypothesis:

H3: *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

Prior research has highlighted various types of management forecasts, including overall, earnings, sales, cash, and capital expenditure forecasts (Han and Wild, 1991; Wasley and Wu, 2006; Beyer et al., 2010; Cheng and Lo, 2006; Lu and Wu Tucker, 2012). This study examines the annual frequency of these management forecasts (Frequency), encompassing all types provided by management. The data is sourced from the I/B/E/S Guidance database.

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. The timeline of firm-year measures is illustrated in Figure 1.

We developed the sample by first retrieving firms from the COMPUSTAT database with continuous yearly performance data from 2008 to 2015. This resulted in a preliminary sample of 52,464 firm-years for 6,558 unique firms. Management forecasts for these firms were obtained from the I/B/E/S Guidance

database. Firms without forecasts were assigned a frequency of zero. The key variable, Frequency, is calculated as the natural logarithm of one plus the aggregate frequency of all forecasts, including annual or quarterly earnings and non-earnings forecasts (e.g., sales forecasts and capital expenditure (CAPEX) forecasts). We winsorized Frequency at the 1% and 99% levels for all tests.

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.[10]

[10]U.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.

A similar case involved J. Steven Griles, Deputy Secretary of the Interior, who maintained close ties to energy firms during his tenure despite a formal recusal. He repeatedly met with executives from companies holding federal leases and facing regulatory scrutiny, facilitating access and raising concerns about undue influence. Griles was later convicted of obstructing a Senate investigation into these relationships, underscoring how even unelected officials in procurement-adjacent roles can act as key conduits of privileged information and access.[11]

These cases 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 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 2 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:

$$Frequency_{f,t} = \alpha + \beta Post_t \times MajorGovCustomer_{f,t} + \chi_{f,t} + \phi_f + \phi_t + \varepsilon_{f,t}, \quad (1)$$

[11] U.S. Department of Justice. (2007). *Former Deputy Secretary of the Interior J. Steven Griles Sentenced for Obstruction of Senate Investigation into Abramoff Lobbying Activities.* Retrieved from https://www.justice.gov/archive/opa/pr/2007/June/07_crm_432.html.

where $\text{Frequency}_{f,t}$ represents the number of voluntary management forecasts issued by firm f in year t, measured as the natural logarithm of one plus the total number of forecasts. The independent variables include MajorGovCustomer and Post, both dummy variables.

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. On average, 37 percent of firms issue some management forecasts, and the overall sample mean is 2.39 forecasts per year (logarithm of frequency of 0.87 per year). Approximately 15.7 percent of sample firms have been a major government supplier for at least three of the previous years before the STOCK Act (mean MajorGovCustomer = 0.15), 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 and and B of Table 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 0.75 total forecasts, while their counterparts issue 0.862 in the pre-period and 0.917 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. The difference between annual forecasts for firms that are major government customers and those that are not statistically different for the pre- or post-period. Firms labeled as major government customers issue fewer quarterly forecasts during both the pre- and post-period, and this difference is statistically significant at the 1 percent level. 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* × *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.[12]

To assess the timing of these effects, we estimate a dynamic event-study model where *Post* × *MajorGovCustomer* is replaced with interactions between *MajorGovCustomer* and annual indicators surrounding the STOCK Act's implementation. Figure 3 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 3 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.[13]

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 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* × *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.[14] This

[12] The estimated effect of the STOCK Act on management forecast frequency is obtained from Table 3, Column 4, where the coefficient on *Post* × *MajorGovCustomer* is −0.078. Since the dependent variable is the natural logarithm of (1 + 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.

[13] A discussion of this method and its implementation in Stata is available at https://github.com/mcaceresb/stata-pretrends.

[14] The estimated effect of the STOCK Act on forecast precision is obtained from Table 4, Column 4, where the coefficient

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 has the added benefit of helping to rule out potential mechanical results caused by Regulation Fair Disclosure (Reg FD). Reg FD mandates that material, non-public information shared with select individuals, including government officials, must also be publicly disclosed. If firms previously engaged in more voluntary disclosures due to Reg FD, the STOCK Act's restrictions on private communication may have reduced the forecast frequency because politicians have less incentives to exchange information, leading to less frequent public disclosures required by Reg FD from the firm side. However, Reg FD only requires that the disclosure be made equally to all recipients and that the same information shared with the government must also be made available to the public. However, it does not dictate the manner in which the information is disclosed, whether it's a point forecast or a range forecast.

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. Specifically, if firms cannot benefit from privileged insights from politicians, it is rational to expect they will issue more range forecasts (an interval of management forecasts) rather than point forecasts (a single number of management forecasts). Table 4 presents the results, which is consistent with our expectations. We find that firms with government customers were significantly more likely to issue range forecasts following the STOCK Act's enactment. This shift toward less precise disclosures suggests that these firms previously obtained valuable information from executive branch officials, which helped them issue more precise forecasts. The decline in forecast precision following the Act's passage indicates that our findings cannot be entirely attributed to Reg FD and instead reflect the broader consequences of altered government information-sharing incentives on corporate disclosures.

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

While our baseline findings indicate that the STOCK Act influenced firms' voluntary disclosure decisions, isolating its effects from other factors remains a challenge. The observed decline in management

on *Post* × *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 $(0.059/0.689) \times 100 = 8.6\%$.

forecasts could stem not only from regulatory changes but also from firms responding to increased business uncertainty due to a loss of valuable information channels. Given that firms with government contracts often rely on insights from policymakers to anticipate contract renewals and regulatory shifts, changes in the availability of this information may have affected disclosure practices differently across firms.

These findings suggest that prior literature may have underestimated the role of privileged information flow in politically connected firms. While studies such as Akey (2015) emphasize the financial benefits of political ties—such as increased contracting opportunities—our results highlight a different mechanism: discretionary access to regulatory and procurement insights that shape disclosure strategies. The STOCK Act's disruption of these information flows increased uncertainty, causing firms to reduce voluntary disclosures. This evidence challenges the assumption that political connections are primarily valuable for direct financial benefits and instead demonstrates that they also serve as critical sources of information advantages in corporate decision-making.

In this section, we explore heterogeneity in disclosure responses by analyzing two key channels: (1) demand uncertainty and (2) 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:

$$\begin{aligned} Frequency_{f,t} = \alpha + \beta_1 Post_t \times HighDemandVar_f + \beta_2 Post_t \times LowDemandVar_f \\ + \chi_{f,t} + \phi_f + \phi_t + \varepsilon_{f,t} \end{aligned} \tag{2}$$

where $\chi_{f,t}$, ϕ_f, ϕ_t, and $\varepsilon_{f,t}$ are defined as in Equation (1), and *HighDemandVar* and *LowDemandVar*

represent the above- and below-median demand variables defined earlier.

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* × *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 statistically significant declines in management forecast frequency, while their low-exposure counterparts show no meaningful changes.

These findings suggest that firms with stronger ties to government business experienced greater uncertainty following the STOCK Act, leading to a more cautious approach to voluntary disclosure. Rather than prohibiting the flow of information, the Act changed the incentives for government officials to engage with firms, potentially making key regulatory and policy insights less accessible. Among the three indicators, the proportion of government sales (*HighGovSalesRatio*) has the strongest effect, likely because revenue stability is a primary concern for investors. As firms faced greater uncertainty about contract renewals and regulatory changes, managers may have chosen to limit voluntary disclosures rather than risk providing guidance based on incomplete or less reliable information. This aligns with prior research showing that when firms encounter heightened uncertainty, they often reduce voluntary disclosures rather than risk providing unreliable guidance (Anantharaman and Zhang, 2011; Guay, Samuels, and Taylor, 2016). Additionally, firms with lower-quality disclosures may face negative investor reactions, as external investors, such as fund managers, tend to limit investments in firms with less transparent reporting (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, underscores 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 incentives could have led politically engaged firms—both those with established connections and those seeking influence—to reassess the trade-offs of voluntary disclosure, potentially resulting in observable changes 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 (*ConnectedPolitican*). We obtain these measures from OpenSecrets and classify firms into high and low engagement groups based on their ex-ante median values. This classification allows us to assess whether firms with stronger political ties adjusted their voluntary disclosure behavior differently in response to the STOCK Act.

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 the coefficient for the

low-engagement group is insignificant and in the opposite direction. 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. We test for indirect political engagement by examining whether firms engage with the government through industry dynamics rather than direct financial contributions. In highly competitive industries, firms have stronger incentives to seek regulatory advantages and maintain government relationships to remain competitive (Kepler, 2021). To measure the role of industry competition in shaping disclosure behavior, we calculate the sales-based Herfindahl-Hirschman Index (HHI), where a higher HHI reflects lower competition and, consequently, fewer coordination benefits and proprietary disclosure costs.

Table 7 presents the cross-sectional results of this test. In Column 2, the coefficient on *Post* × *MajorGovCustomer* is negative and significant at the 10% level, suggesting that firms operating in more competitive industries reduced their voluntary disclosures after the STOCK Act. These results indicate that firms facing greater market competition had stronger incentives to maintain government relationships and comply with shifting regulatory expectations, potentially leading to adjustments in their disclosure practices.

Taken together, these results support the idea that political engagement—both direct and indirect—played a significant role in shaping firms' voluntary disclosure decisions following the enactment of the 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. Thus, we reject H2 (null) and show evidence that both direct and indirect political engagement influenced firms' voluntary disclosure decisions post-STOCK Act, as firms with stronger political ties reduced disclosures when the benefits of executive branch connections diminished.

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

To systematically capture discussions related to government contracting, regulatory uncertainty, and political risk, we construct three text-based measures using firm-level conference call transcripts. The details of these variable constructions are in Table A1 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. Table 8 presents the regression estimates for these measures, showing how firms adjusted their disclosure patterns post-STOCK Act.

The variable *Gov_Contract_Terms* quantifies the extent to which firms discuss government contracts using a curated dictionary of procurement-related terms. The coefficient on *Post* × *MajorGovCustomer* is negative and statistically significant, indicating a decline in government contracting discussions post-STOCK Act. This result suggests that firms may have become became 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.

We adopt the *Political_Risk* measure from Hassan et al. (2019), which quantifies firms' political text exposure to risk and uncertainty. The regression results indicate a sharp rise in perceived political risk, with The coefficient on *Post* × *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 *Political_Risk_Terms* 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. A widely used measure of price informativeness, stock return synchronicity (R^2), captures the extent to which firm-specific information 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.[15]

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

$$r_{f,t} = \alpha + \beta_1 r_{m,t-1} + \beta_2 r_{m,t} + \beta_3 r_{m,t+1} + \beta_1 r_{i,t-1} + \beta_2 r_{i,t} + \beta_3 r_{i,t+1} + \epsilon_{f,t} \tag{3}$$

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 arriving to the securities market:

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

Higher values indicate greater firm-specific information impounded in stock prices. We present our results in Table 9, Column 1, showing that the coefficient on $Post \times MajorGovCustomer$ is negative and statistically significant at the 10% level, representing a 14.4 percent decline in price informativeness. 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.[16] 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,

[15] Some 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.

[16] The estimated effect of the STOCK Act on stock return synchronicity is obtained from Table 9, Column 1, where the coefficient on $Post \times MajorGovCustomer$ is -0.155. Since the dependent variable is *IDIOSYN*, the percentage decline is given by $e^{-0.155} - 1 \approx -14.4\%$. The mean pre-STOCK Act value of *IDIOSYN* for government contractors is -1.603 (from Table 2), translating to a post-STOCK Act value of approximately -1.286. This result suggests that the flow of firm-specific government-related information into stock prices was disrupted, reducing price informativeness.

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 9 shows that firms with major government customers exhibit a statistically significant decline in *GPIN* post-STOCK Act, with an estimated reduction of approximately 2.3% in private information arrival.[17] We further add the equity controls in Column 4, and our results remain robust.

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. Analysts rely on both public disclosures and private interactions with firms and government officials to refine their expectations. 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 forecast errors (*FCST_error*) and forecast dispersion (*Dispersion*) from the IBES Summary file for annual forecasts. Table 10 presents the regression results. Column 1 shows that the interaction term *Post* × *MajorGovCustomer* is positive and statistically significant for forecast errors (*FCST_ERROR*), 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.

In contrast, Column 2 shows that forecast dispersion (*DISPERSION*) decreased post-STOCK Act for major government customers, with the interaction term negative and statistically significant. This suggests that, 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 homogeneity in their forecasts.

[17] The estimated effect of the STOCK Act on *GPIN* is obtained from Table 9, Column 3, where the coefficient on *Post* × *MajorGovCustomer* is −0.010. The pre-STOCK Act mean of *GPIN* for government contractors is 0.426 (Table 2). Applying the estimated decline, the new mean is approximately $0.426 - 0.010 = 0.416$, corresponding to a 2.3% reduction $(0.010/0.426 \times 100)$.

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 accurate forecasts. The increase in forecast errors underscores 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:

$$\begin{aligned} ICC_{i,t} = \alpha + \beta_1 \text{Post}_i \times \text{MajorGovCustomer}_t \times \text{Frequency}_{it} + \beta_2 \text{Post}_i \times \text{MajorGovCustomer}_t \\ + \beta_3 \text{Frequency}_{it} + \chi_{it} + \phi_i + \phi_t + \varepsilon_{it}, \end{aligned} \tag{5}$$

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 11 presents the results. The coefficient on *Post* × *MajorGovCustomer* is positive across all columns but only statistically significant in Columns 4 and 5, presenting some evidence that ICC increased post-STOCK Act for major government customers.

The triple interaction term *Post* × *MajorGovCustomer* × *Frequency* is statistically significant across four of the five ICC measures. During the post-STOCK Act period, government contractors who reduced disclosures by one additional management forecast experienced an increase in their implied cost of capital (ICC) by approximately 0.9 percentage points on average, as shown in Table 11 Column 5. This finding suggests that firms with major government customers that reduced their voluntary disclosures

after the STOCK Act's implementation experienced a significant increase in their implied cost of capital, highlighting the critical role of transparency in sustaining favorable financing conditions.

These findings align with our synchronicity and analyst forecast results, suggesting that firm-specific information became more constrained post-STOCK Act. Analysts exhibited higher forecast errors, suggesting greater difficulty in assessing firm performance due to reduced access to private information. At the same time, forecast dispersion declined, indicating that analysts increasingly relied on the same, limited public disclosures. These changes likely reinforced investor uncertainty, which could be associated with less precise firm valuations, leading to a higher cost of capital.

Together, these findings highlight an unintended consequence of the STOCK Act: while designed to curb insider trading, the regulation may have diminished the flow of value-relevant information, increasing capital market frictions for firms reliant on government business relationships. By restricting the incentives for government officials to share firm-specific insights, the Act may have indirectly weakened the informational efficiency of equity markets, with ripple effects on investor perceptions, analyst accuracy, and firms' financing costs.

5. Additional Analysis

5.1. Congressional Stock Holdings and Corporate Disclosure

Prior research (Cherry, Heitz, and Jens, 2018; Huang and Xuan, 2023; Wei and Zhou, 2023) documents that the STOCK Act curtailed insider trading by members of Congress, raising the question of whether firms previously held by these legislators adjusted their voluntary disclosure practices in response. Before the Act, congressmen may have privately exchanged firm-specific information, leveraging their positions for capital market advantages. With increased scrutiny and legal risks post-STOCK Act, such interactions (though unobservable) may have become less frequent, potentially prompting firms to shift toward greater public disclosure to compensate for the loss of these private informational channels.

Beyond informal communication, members of Congress can also exert influence over government contract awards in certain cases (Witko, 2011; Fazekas, Ferrali, and Wachs, 2023). If a congressman holds a stake in a firm negotiating a government contract, they may have incentives to engage with executive branch officials to sway final decisions in favor of the firm. This raises an important empirical

question: did firms with prior congressional ownership exhibit different disclosure behaviors after the STOCK Act's passage?

To examine this, we obtained data on congressional stock holdings from OpenSecrets and constructed a new variable, *CongressOwn*, which equals one if a firm was ever held by a congressman between 2008 and 2011, prior to the STOCK Act. We first employed a difference-in-differences framework, replacing the primary treatment variable with *CongressOwn*. Additionally, we introduced a triple interaction term, $Post \times MajorGovCustomer \times CongressOwn$, to assess whether the disclosure responses differed between firms previously held by congressmen and those that were not.

The results, presented in Table 12, provide insight into the role of congressional ownership in shaping disclosure practices. Column 1 shows that firms previously held by congressmen significantly reduced their management forecasts following the STOCK Act, though the effect is slightly weaker than our main results, with significance at the 10% level. This suggests that, while these firms may have lost access to certain informational advantages, their strong political connections may have mitigated the full impact of the Act.

In Column 2, the triple interaction term is marginally significant at the 10% level, aligning with our prediction that congressional intervention in contract-awarding processes is rare, as legislators seek to maintain power independence. These findings suggest that, prior to the STOCK Act, congressmen played a notable—albeit secondary—role in private firm communications, with executive branch officials likely serving as the primary conduit for privileged information.

5.2. Alternative Disclosures and Non-Financial Reporting

To better understand the breadth of the STOCK Act's impact on corporate voluntary disclosures, we examine whether its effects vary across different forecast categories and extend to non-financial disclosures. Our results indicate that the STOCK Act led to a significant reduction in the frequency of management forecasts, particularly for long-term projections. Annual forecasts declined more than quarterly forecasts, suggesting that firms became more cautious about sharing long-term expectations post-STOCK Act. Additionally, non-earnings forecasts, such as capital expenditure projections, experienced a sharper decline compared to earnings forecasts. This pattern implies that disclosures more sensitive to

government-related information flows were scaled back, while firms maintained routine earnings forecasts to avoid potential negative market reactions. Moreover, the reduction in the number of forecasting days reinforces the conclusion that firms broadly curtailed their forecasting activity following the Act's implementation.

Prior research suggests that investor reliance on ESG-related information for trading purposes was relatively limited in 2012 (Van Duuren, Plantinga, and Scholtens, 2016; Caplan, Griswold, and Jarvis, 2013). If private communications between firms and government officials before the STOCK Act primarily involved financially relevant insights, then we would expect the Act to have little to no effect on non-financial disclosures, such as environmental and social reporting. Consistent with this expectation, our analysis finds no significant change in these disclosures, suggesting that the Act primarily curtailed the private exchange of financial information rather than general corporate reporting. These findings further support the conclusion that the STOCK Act's effects were concentrated on disclosures with potential financial relevance to government officials. The full empirical analysis, detailed discussion, and supporting empirical results are presented in the Online Appendix.

5.3. Government Contracts and Major Customers

In Table B2, 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. This distinction allows us to compare firms with varying reliance on government revenue. The results show that the coefficient on *Post* × *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.

6.1. Propensity Score Matching

We redo the analysis in Table 3 using propensity score matching (PSM), implementing nearest-neighbor matching without replacement. First, we estimate propensity scores through a logistic regression with treatment status as the dependent variable and log market value, leverage, book-to-market ratio, and return on assets as covariates. Each treated observation is then matched individually with the control observation having the nearest propensity score, ensuring no control observation is matched more than once. The resulting matched sample mitigates differences between treated and control groups regarding key observable characteristics. The results are presented in Table OA.3

6.2. Placebo Test

To assess whether unobserved confounders drive our results, we 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. Further details are provided in the Online Appendix Section OA0.3.

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.4). 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 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.5 and discussed in the Online Appendix, 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. Poission Regression

Cohn, Liu, and Wardlaw (2022) point out that there are limitations with linear estimations based on the logarithm of 1 plus an outcome. Following the researchers' suggestion to mitigate concerns about our previous findings, we employed a fixed-effects Poisson model in robustness checks. As shown in Table B3 in the Appendix, our conclusions hold with this alternative model specification, suggesting that the log1plus method does not drive our findings.

The comprehensive robustness tests, including alternative time and period measurements, different

treatment and control group selections, and the Poisson regression analysis, consistently support our initial findings. These tests confirm that the STOCK Act significantly influences the voluntary disclosure behavior of firms, particularly those with substantial government business connections. By examining forecast precision, we further demonstrate that the reduced interactions with executive branch officers, as mandated by the STOCK Act, likely diminish the flow of private information to management. This comprehensive set of robustness tests strengthens the reliability of our results, suggesting the significant role of the STOCK Act in shaping firms' voluntary disclosure practices.

6.5. 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 an 8.62% decline in management forecast frequency following the Act's enactment, translating to 0.31 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.

Beyond disclosure frequency, we find that affected firms shift from precise point forecasts to broader range forecasts, signaling increased uncertainty. This shift cannot be fully attributed to Regulation Fair Disclosure (Reg FD), highlighting the distinct impact of changes in government information-sharing incentives. Furthermore, we document broader capital market consequences, as firms reducing disclosures exhibit lower price informativeness and a higher implied cost of capital, underscoring the economic significance of these information frictions.

Our findings extend prior research on political connections by demonstrating that firms with strong government ties were not only leveraging these relationships for policy influence but also relying on them for privileged information flow. Prior studies (e.g., Akey, 2015; Cohen et al., 2022) highlight that political connections provide financial benefits through preferential contracting or regulatory leniency, but our findings reveal that these connections also play a crucial role in shaping firms' information environments. The STOCK Act introduced an exogenous shock to government-firm information flow, revealing that firms dependent on political ties for disclosure and forecasting faced significant uncertainty when those channels were disrupted. This finding refines existing literature by emphasizing that political connections not only enhance firm performance but also influence corporate transparency.

More broadly, our results contribute to the literature on voluntary disclosure, political connections, and regulatory oversight. We show that government-sourced information plays a unique role in shaping corporate transparency and that restricting access to such information can meaningfully alter firms' disclosure behavior. While the STOCK Act was designed to increase accountability among government officials, it also introduced frictions in the information environment, influencing firms' ability to provide guidance to investors. These results underscore that insider trading regulations, while targeting individual misconduct, may have broader, unintended consequences for corporate decision-making and capital markets.

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Tables and Figures

Figure 1. Timeline for Management Forecasts

This figure illustrates the timeline of the sample period, 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.

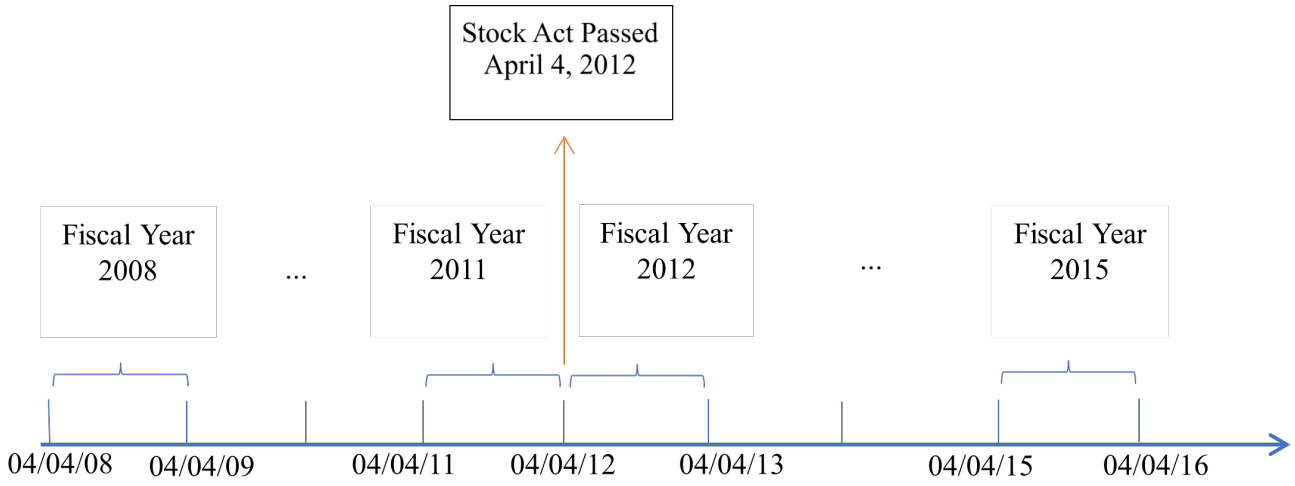

Figure 2. The Frequency of Management Forecasts

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.

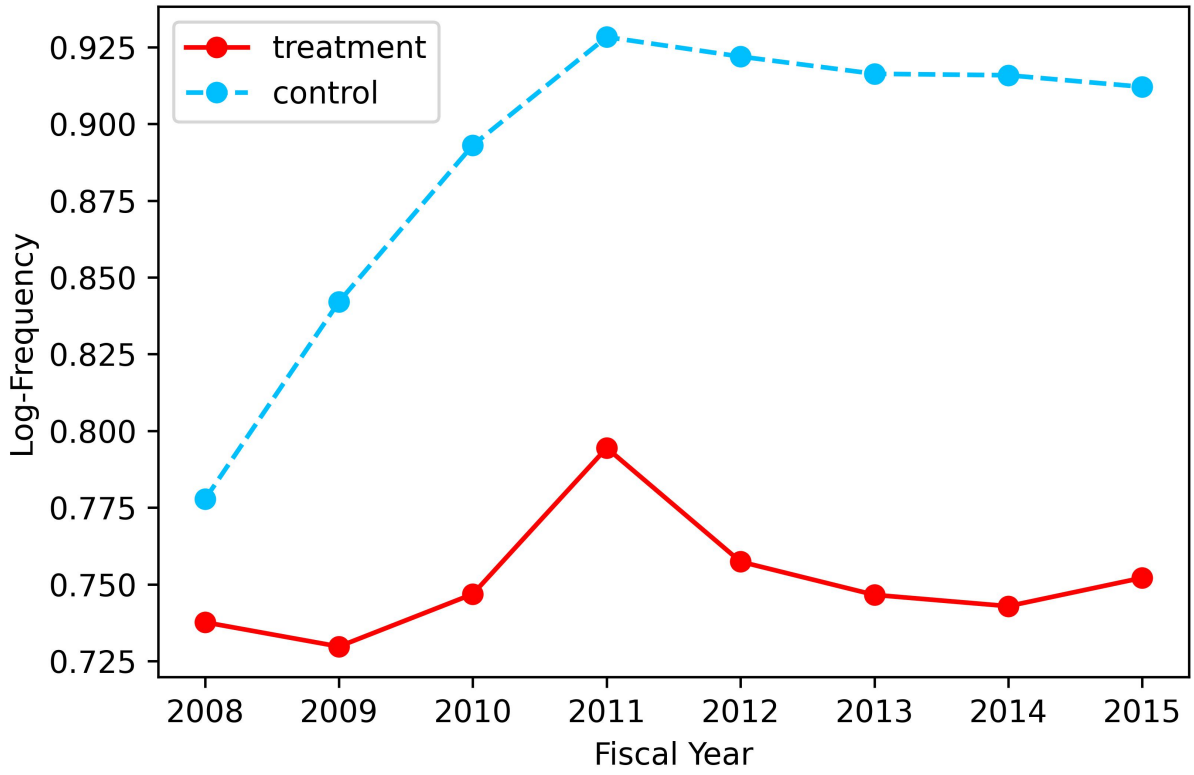

Figure 3. Roth (2022) Pre-Trend Power Analysis

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* × *MajorGovCustomer* is replaced with interaction terms between *MajorGovCustomer* and yearly indicator variables.

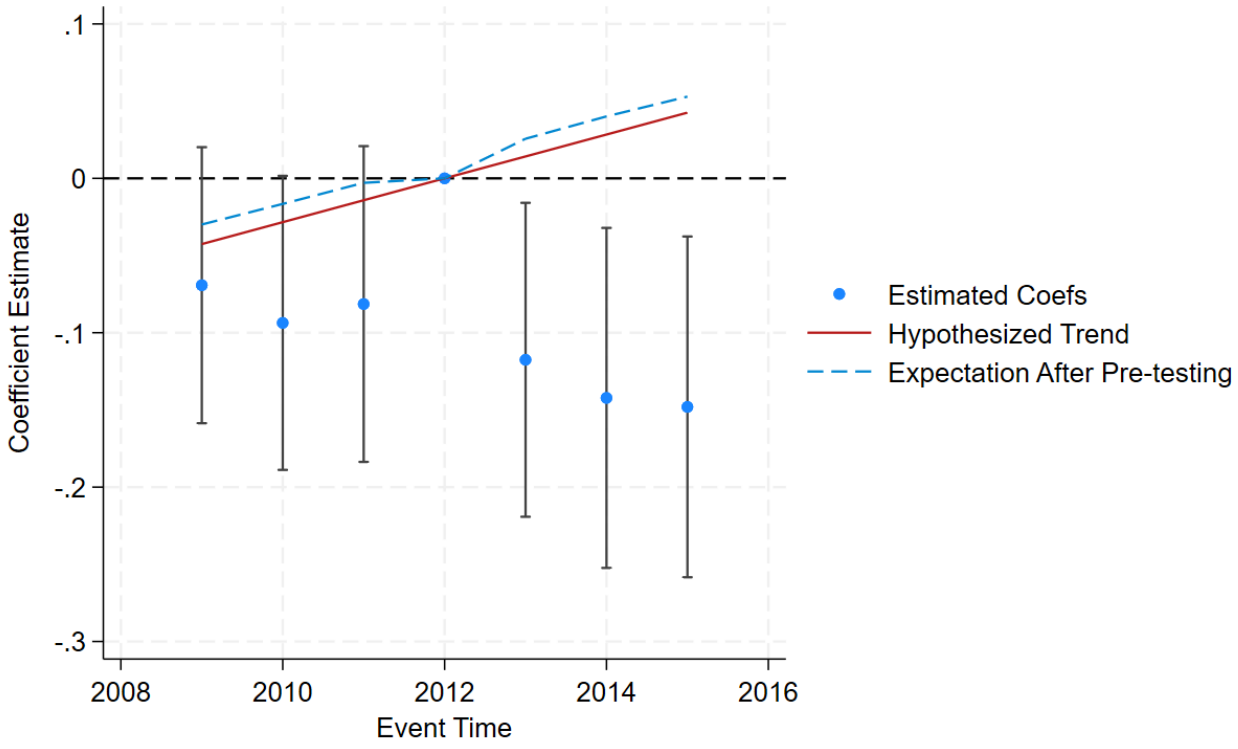

Table 1. Summary Statistics

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 A1.

	N	Mean	Std	25th	Median	75th
MajorGovCustomer	12,074	0.15	0.36	0.00	0.00	0.00
Disclosure Measures:						
Frequency	12,074	0.87	1.25	0.00	0.00	2.08
RangeForecast	2,535	0.70	0.34	0.55	0.83	1.00
AnnualForecasts	12,074	0.68	1.07	0.00	0.00	1.39
Price Informativness and Analysts:						
IDIOSYN	11,609	-1.42	1.52	-2.15	-1.34	-0.56
GPIN	7,880	0.45	0.10	0.38	0.47	0.54
FCSTERROR	6,570	0.15	0.79	0.00	0.01	0.02
DISPERSION	6,219	0.08	0.48	0.00	0.00	0.01
Textual Analysis:						
Gov_Contract_Terms	12,074	0.02	0.15	0.00	0.00	4.40
Political_Risk	12,074	112.93	195.73	0.00	67.12	139.46
Implied Cost of Capital:						
GLS_ICC	9,887	9.63	6.78	6.19	8.33	11.14
OJM_ICC	9,887	9.32	13.46	3.26	5.47	9.94
CAT_ICC	9,887	-7.58	19.93	-19.85	-8.54	3.36
PEG_ICC	9,887	13.36	14.89	2.50	9.23	18.03
AVG_ICC	9,887	5.74	11.22	-1.04	3.28	9.02
Controls:						
Size	12,074	6.52	2.15	5.01	6.52	7.98
LEV	12,074	0.18	0.19	0.00	0.15	0.26
BM	12,074	0.61	0.59	0.27	0.48	0.79
ROA	12,074	-0.04	0.24	-0.04	0.03	0.07
Sigma	11,609	0.128	0.077	0.075	0.110	0.161
Skewness	11,609	0.189	0.713	-0.285	0.150	0.610
Kurtosis	11,609	-0.082	1.137	-0.860	-0.386	0.349

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 A1. 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									
Frequency	0.752	0	933	0.862	0	4996	-0.109	(-2.571)	**
RangeForecast	0.689	0.800	209	0.710	0.833	1265	-0.022	(-0.855)	
IDIOSYN	-1.603	-1.527	880	-1.639	-1.505	4754	0.036	(0.664)	
GPIN	0.426	0.474	517	0.424	0.474	2896	0.002	(0.404)	
FCSTERROR	0.182	0.006	558	0.170	0.006	2581	0.011	(0.275)	
DISPERSION	0.118	0.003	531	0.086	0.003	2433	0.032	(1.347)	
Gov_Contract_Terms	0.070	0.000	933	0.014	0.000	4996	0.055	(9.927)	***
Political_Risk	194.441	105.560	933	104.424	67.634	4996	90.018	(11.877)	***
GLS_ICC	9.350	9.027	819	9.047	8.256	3994	0.303	(1.505)	
OJM_ICC	9.748	6.102	819	9.742	5.638	3994	0.006	(0.011)	
CAT_ICC	-5.548	-5.628	819	-8.878	-9.077	3994	3.329	(4.628)	***
PEG_ICC	10.672	6.227	819	12.190	8.202	3994	-1.518	(-2.823)	***
AVG_ICC	5.610	4.076	819	5.152	3.198	3994	0.458	(1.185)	
Size	6.243	6.311	933	6.340	6.309	4996	-0.098	(-1.327)	
LEV	0.186	0.172	933	0.156	0.12	4996	0.030	(4.614)	***
BM	0.684	0.6	933	0.666	0.527	4996	0.018	(0.835)	
ROA	-0.024	0.037	933	-0.044	0.028	4996	0.020	(2.429)	**
Panel B: Post-STOCK Act Period									
Frequency	0.75	0	921	0.917	0	5224	-0.167	(-3.92)	***
RangeForecast	0.732	0.800	139	0.697	0.830	922	0.036	(1.160)	
IDIOSYN	-1.286	-1.189	884	-1.208	-1.167	5091	-0.078	(-1.76)	*
GPIN	0.453	0.486	657	0.463	0.495	3810	-0.010	(-2.365)	**
FCSTERROR	0.147	0.005	584	0.117	0.005	2847	0.030	(0.934)	
DISPERSION	0.055	0.003	550	0.083	0.002	2705	-0.027	(-1.291)	
Gov_Contract_Terms	0.048	0.000	921	0.011	0.000	5224	0.037	(7.697)	***
Political_Risk	169.521	92.910	921	96.525	58.155	5224	72.997	(11.804)	***
GLS_ICC	10.877	9.012	823	10.001	8.065	4251	0.875	(2.900)	***
OJM_ICC	9.572	5.909	823	8.792	5.117	4251	0.780	(1.543)	
CAT_ICC	-2.592	-4.283	823	-7.719	-9.377	4251	5.127	(6.456)	***
PEG_ICC	14.462	9.979	823	14.764	10.475	4251	-0.302	(-0.510)	
AVG_ICC	7.666	4.619	823	5.943	2.971	4251	1.723	(3.720)	***
Size	6.627	6.86	921	6.726	6.772	5224	-0.099	(-1.309)	
LEV	0.213	0.182	921	0.187	0.162	5224	0.026	(3.839)	***
BM	0.587	0.514	921	0.543	0.424	5224	0.044	(2.427)	**
ROA	-0.019	0.026	921	-0.047	0.025	5224	0.028	(3.988)	***

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, *Frequency*, is defined as the natural logarithm of 1 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 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 A1.

	(1)	(2)	(3)	(4)
		Frequency		
Post × *MajorGovCustomer*	-0.074**	-0.073**	-0.084***	-0.078**
	(-2.11)	(-2.13)	(-2.67)	(-2.53)
MajorGovCustomer	-0.052	-0.002		
	(-0.57)	(-0.03)		
Size		0.159***		0.078***
		(10.42)		(5.67)
LEV		0.036		0.117**
		(0.26)		(2.11)
BM		-0.036		0.042***
		(-1.07)		(2.81)
ROA		0.309***		-0.037
		(3.87)		(-1.07)
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	0.11	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* × *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 A1.

	(1)	(2)	(3)	(4)
		RangeForecast		
Post × *MajorGovCustomer*	0.070**	0.071**	0.061*	0.059*
	(1.97)	(2.00)	(1.77)	(1.71)
MajorGovCustomer	-0.057	-0.058		
	(-1.13)	(-1.15)		
Size		-0.005		-0.032
		(-0.62)		(-1.51)
LEV		-0.075		0.012
		(-1.04)		(0.18)
BM		-0.046		-0.011
		(-1.48)		(-0.40)
ROA		0.008		-0.032
		(0.13)		(-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. *HighGovSalesRatio* (*HighGovCustomNum* / *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 quantile for firms with major government customers from 2008 to 2011; otherwise, it equals zero. *LowGovSalesRatio* (*LowGovCustomNum* / *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 quantile 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 A1.

	(1)	(2)	(3)
		Frequency	
Post × *HighGovSalesRatio*	-0.119**		
	(-2.17)		
Post × *LowGovSalesRatio*	-0.021		
	(-0.53)		
Post × *HighGovCustomNum*		-0.097*	
		(-1.93)	
Post × *LowGovCustomNum*		-0.042	
		(-1.12)	
Post × *HighGovSalesVol*			-0.104**
			(-2.44)
Post × *LowGovSalesVol*			-0.072
			(-1.32)
Size	0.079***	0.079***	0.078***
	(5.70)	(5.70)	(5.63)
LEV	0.120**	0.119**	0.115**
	(2.18)	(2.15)	(2.08)
BM	0.043***	0.042***	0.042***
	(2.88)	(2.79)	(2.80)
ROA	-0.038	-0.036	-0.036
	(-1.10)	(-1.06)	(-1.07)
Firm FE	YES	YES	YES
Year FE	YES	YES	YES
Observations	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. Columns 1 and 2 present results for firms with federal contributions (*FedeContribution*) 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 A1.

	(1)	(2)	(3)	(4)
			Frequency	
	FedeContribution		*ConnectedPolitician*	
	High	Low	High	Low
Post × *MajorGovCustomer*	-0.118***	0.000	-0.070*	-0.049
	(-2.90)	(0.00)	(-1.78)	(-1.04)
Size	0.091***	0.073***	0.103***	0.041***
	(4.39)	(3.82)	(4.13)	(2.92)
LEV	0.104	0.136*	0.167*	0.103*
	(1.19)	(1.95)	(1.76)	(1.75)
BM	0.048*	0.043**	0.034	0.031***
	(1.72)	(2.41)	(0.94)	(2.64)
ROA	-0.083	-0.013	0.019	-0.038
	(-1.21)	(-0.35)	(0.27)	(-1.22)
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

Table 7. Indirect Political Activity Engagements

This table presents the OLS regression results from cross-sectional tests examining the coordination benefits associated with market competition as measured by sales characteristics. Columns 5 and 6 present results for the subset of firms where the industry's HHI is above and below the median, reflecting firms in noncompetitive (competitive) industries. 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 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 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 indicated by ***, **, and * for the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Table A1.

	(1)	(2)
	Frequency	
	High HHI	Low HHI
Post × *MajorGovCustomer*	-0.063	-0.096**
	(-1.53)	(-2.03)
Size	0.094***	0.063***
	(4.97)	(3.17)
LEV	0.140*	0.089
	(1.93)	(1.07)
BM	0.045**	0.038*
	(2.05)	(1.86)
ROA	-0.017	-0.054
	(-0.32)	(-1.24)
Firm FE	YES	YES
Year FE	YES	YES
Observations	6,048	6,026
R-squared	0.90	0.91

Table 8. 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 *Political_Risk*, which capture the count of words associated with government contract terms and political risk. 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 A1.

	(1) *Gov_Contract_Terms*	(2) *Political_Risk*
*Post*MajorGovCustomer*	-0.021*	41.840***
	(-1.77)	(3.35)
Size	0.005	5.705***
	(0.80)	(3.15)
Lev	0.021*	1.699
	(1.87)	(0.11)
BM	0.010	4.774
	(1.41)	(0.75)
ROA	0.000	-24.636
	(0.03)	(-1.61)
Observations	12,074	12,074
R-squared	0.431	0.081
Ind FE	NO	NO
Firm FE	YES	YES
Year FE	YES	YES

Table 9. Price Informativness

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 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. The regression includes firm and year fixed effects and controls for firm characteristics such as *Size*, *LEV*, *BM*, and *ROA*. When examining stock price synchronicity measures, we follow Hutton, Marcus, and Tehranian (2009) and 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 A1.

	(1) *IDIOSYN*	(2) *IDIOSYN*	(3) *GPIN*	(4) *GPIN*
Post × *MajorGovCustomer*	-0.155**	-0.171**	-0.010*	-0.010*
	(-2.11)	(-2.34)	(-1.72)	(-1.65)
Size	-0.109***	-0.128***	-0.001	-0.003
	(-2.79)	(-3.31)	(-0.71)	(-1.41)
Lev	-0.133	-0.110	-0.013	-0.012
	(-1.00)	(-0.82)	(-1.32)	(-1.18)
BM	-0.102**	-0.094**	-0.001	-0.002
	(-2.28)	(-2.13)	(-0.43)	(-0.96)
ROA	0.212	0.177	0.011**	0.010*
	(1.20)	(1.01)	(2.10)	(1.92)
Sigma		-0.934***		-0.016
		(-3.03)		(-0.94)
Skewness		0.162***		0.001
		(6.10)		(0.83)
Kurtosis		-0.079***		-0.001
		(-4.30)		(-0.93)
Observations	11,609	11,609	7,880	7,628
R-squared	0.25	0.25	0.50	0.49
Cluster	Firm	Firm	Firm	Firm
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table 10. Analyst Forecast Properties

This table presents the OLS regression results analyzing the impact of private communication between politicians and public firms on the analysts. *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* × *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 A1.

	(1) *FCSTERROR*	(2) *DISPERSION*
Post × *MajorGovCustomer*	0.069**	-0.039*
	(1.98)	(-1.73)
Size	-0.045	-0.026**
	(-1.54)	(-2.32)
LEV	0.187	0.011
	(1.56)	(0.4)
BM	-0.007	0.005
	(-0.13)	(0.21)
ROA	-0.1	-0.077*
	(-1.07)	(-1.81)
Firm FE	YES	YES
Year FE	YES	YES
Observations	6,570	6,219
R-squared	0.761	0.809

Table 11. 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* × *MajorGovCustomer* × *Frequency*, 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 *Frequency* 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 A1.

	(1) *GLS_ICC*	(2) *OJM_ICC*	(3) *CAT_ICC*	(4) *PEG_ICC*	(5) *AVG_ICC*
Post × *MajorGovCustomer*	0.007	-0.004	0.009	0.020**	0.011
	(1.14)	(-0.36)	(0.81)	(2.26)	(1.62)
Post × *MajorGovCustomer* × *Frequency*	-0.007***	0.001	-0.011**	-0.014***	-0.009***
	(-3.06)	(0.24)	(-2.34)	(-3.64)	(-3.54)
Frequency	-0.002	0.002	-0.003	-0.007**	-0.004*
	(-1.14)	(0.47)	(-0.70)	(-2.43)	(-1.79)
Size	-0.024***	-0.034***	-0.078***	-0.072***	-0.053***
	(-10.68)	(-5.00)	(-12.59)	(-16.16)	(-16.06)
LEV	-0.008	0.013	-0.035	-0.019	-0.016
	(-0.95)	(0.66)	(-1.55)	(-1.30)	(-1.33)
BM	0.003	-0.006	0.067***	-0.013**	0.016***
	(1.15)	(-0.60)	(7.96)	(-2.02)	(3.45)
ROA	0.004	0.003	-0.087***	-0.073***	-0.054***
	(0.53)	(0.13)	(-4.75)	(-6.79)	(-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 12. 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, *Frequency*, is defined as the natural logarithm of 1 plus the total number of management forecasts issued annually by the firm. *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 A1.

	(1)	(2)
	Frequency	
Post × *CongressOwn*	-0.043**	-0.041*
	(-2.00)	(-1.78)
Post × *MajorGovCustomer* × *CongressOwn*		-0.020
		(-0.33)
Post × *MajorGovCustomer*		-0.075**
		(-2.02)
Size	0.080***	0.079***
	(5.75)	(5.70)
LEV	0.123**	0.120**
	(2.23)	(2.16)
BM	0.044***	0.043***
	(2.92)	(2.90)
ROA	-0.037	-0.037
	(-1.08)	(-1.07)
Firm FE	YES	YES
Year FE	YES	YES
Observations	12,074	12,074
R-squared	0.91	0.91

Appendix

A. Variable Description

Table A1. Variable Definition

Variable	Definition	Source
AVG_ICC	Equally-weighted average of the AGR_ICC, CAT_ICC, GLS_ICC, and PEG_ICC measures of the cost of capital.	*Lee, So, and Wang (2021)*
BM	Ratio of the book value of equity to the market value of equity.	*COMPUSTAT*
DISPERSION	Standard deviation of analyst earnings per share forecasts normalized by share price at the end of the previous quarter	*IBES*
CAT_ICC	Internal rate of return equating a firm's forecasted cash flows to its current market price, following Claus and Thomas (2001).	*Lee, So, and Wang (2021)*
ConnectedPolitician	Number of connected politicians.	*OpenSecrets*
HHI	Sales-based Herfindahl-Hirschman Index of the firm's industry.	*COMPUSTAT*
DISPERSION	Standard deviation of analyst earnings per share forecasts normalized by share price at the end of the previous quarter	*IBES*
FCSTERROR	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 $\frac{EPS\_MEAN_{y-1} - EPS\_ACTUAL_y}{Price_{y-1}}$	*IBES*
FedContribution	Political contribution amount for firms in federal congressional elections.	*OpenSecrets*
Forecasts	Total number of all types of management forecasts issued annually by the firm.	*IBES Guidance*
Frequency	Natural logarithm of one plus the total number of all types of management forecasts (*Forecasts*) issued annually by the firm.	*IBES Guidance*
GLS_ICC	Internal rate of return equating a firm's forecasted cash flows to its current market price, following Gebhardt, Lee, and Swaminathan (2001).	*Lee, So, and Wang (2021)*

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Table A1. Variable Definition - Continued

GovCustomNum	The total number of major government customers from 2008 to 2011, four years before the enactment of the STOCK Act.	*COMPUSTAT Segment*
GovSalesRatio	The ratio of government sales to overall sales from 2008 to 2011, four years before the enactment of the STOCK Act.	*COMPUSTAT Segment*
GovSalesVol	The annual volatility of government sales from 2008 to 2011, four years before the enactment of the STOCK Act.	*COMPUSTAT Segment*
HighGovCustomNum	A dummy variable that equals one if the firm's *GovCustomNum* is at or above the sample median for firms with major government customers; otherwise, it equals zero.	*COMPUSTAT Segment*
HighGovSalesRatio	A dummy variable that equals one if the firm's *GovSalesRatio* is at or above the sample median for firms with major government customers; otherwise, it equals zero.	*COMPUSTAT Segment*
GPIN	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.	*Duarte, Hu, and Young (2020)*
Gov_Contract_Terms	Count of average occurrences of government contracting-related terms in a firm's conference call transcripts in a given year. The term set includes "government contract," "government procurement," "federal contract," "public sector contract," "Department of Defense," "DoD contract," "GSA Schedule," "federal funding," "procurement decision," "contract renewal," "government bid," and "contract termination."	*Thomson Reuters Firm conference call transcripts*
HighGovSalesVol	A dummy variable that equals one if the firm's *GovSalesVol* is at or above the sample median for firms with major government customers; otherwise, it equals zero.	*COMPUSTAT Segment*
IDIOSYN	The firm-specific measure of the firm-specific information arriving to the securities market based on (1-RSQ) from the modified index model regression	*CRSP*
Kurtosis	Kurtosis of monthly returns over the calendar year	*CRSP*
LEV	Ratio of total debt to total assets.	*COMPUSTAT*
MajorGovCustomer	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.	*COMPUSTAT Segment*
LowGovCustomNum	A dummy variable that equals one if the firm's *GovCustomNum* is below the sample median for firms with major government customers; otherwise, it equals zero.	*COMPUSTAT Segment*

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Table A1. Variable Definition - Continued

LowGovSalesRatio	A dummy variable that equals one if the firm's *GovSalesRatio* is below the sample median for firms with major government customers; otherwise, it equals zero.	*COMPUSTAT Segment*
LowGovSalesVol	A dummy variable that equals one if the firm's *GovSalesVol* is below the sample median for firms with major government customers; otherwise, it equals zero.	*COMPUSTAT Segment*
Post	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.	*The STOCK Act*
OJM_ICC	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)*
PEG_ICC	Internal rate of return equating a firm's forecasted cash flows to its current market price, following Easton (2004).	*Lee, So, and Wang (2021)*
Policy_Uncertainty_Terms	Count of average occurrences of regulatory and policy uncertainty-related terms in a firm's conference call transcripts in a given year. The term set includes "regulatory enforcement," "compliance requirement," "government oversight," "executive order," "legislation impact," "federal budget cuts," "policy uncertainty," "regulatory shift," "political risk," "STOCK Act," "Regulation Fair Disclosure," and "lobbying restrictions."	*Thomson Reuters Firm conference call transcripts*
Political_Risk	A measure of the share of the conversation devoted to risk associated with political topics is thus the weighted sum of bigrams associated with political text that are used in conjunction with synonyms for risk or uncertainty. The measure is averaged and scaled by number of total words and multiplied by 1,000 at firm-year level.	*Hassan, Hollander, Van Lent, and Tahoun (2019)*
RangeForecast	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.	*Thomson Reuters*
Size	Natural logarithm of the equity market value.	*COMPUSTAT*
ROA	Ratio of income before extraordinary items to total assets.	*COMPUSTAT*
RSQ	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 $r_{f,t} = \alpha + \beta_1 r_{m,t-1} + \beta_2 r_{m,t} + \beta_3 r_{m,t+1} + \gamma_1 r_{i,t-1} + \gamma_2 r_{i,t} + \gamma_3 r_{i,t+1} + \epsilon_t$ where r_q, $r_{m,q}$, and $r_{i,q}$ are excess returns of the stock, the market, and the stock's industry during year t.	*CRSP*
Sigma	Volatility of monthly returns over the calendar year	*CRSP*
Skewness	Skewness of monthly returns over the calendar year	*CRSP*

B. Additional Tables and Figures

Table B1. 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
Exclude observations with missing values for control variables	12,074

Table B2. Government Contracts

This table presents the OLS regression results from cross-sectional tests on demand uncertainty, assessed through the characteristics of major government customers and contractors. The dependent variable, *Frequency*, is defined as the natural logarithm of one plus the total number of management forecasts issued annually by the firm. *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. *OnlyGovContract* 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*, *ROA*, *LOSS*, *EarnVol*, *Return*, *Turnover*, and *StkVol*. 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 A1.

	(1)	(2)	(3)	(4)
		Frequency		
*Post*MajorGovCustomer*	-0.095**	-0.091**	-0.077**	-0.075**
	(-2.50)	(-2.46)	(-2.41)	(-2.37)
MajorGovCustomer	0.064	0.052		
	-0.7	-0.6		
*Post*OnlyGovContrator*	-0.006	-0.031	0.022	0.012
	(-0.16)	(-0.80)	-1.05	-0.59
OnlyGovContrator	0.329***	0.161**		
	-5.03	-2.57		
Size		0.152***		0.078***
		-9.97		-5.67
LEV		0.024		0.115**
		-0.17		-2.09
BM		-0.036		0.042***
		(-1.07)		-2.82
ROA		0.300***		-0.037
		-3.75		(-1.08)
Observations	12,074	12,074	12,074	12,074
R-squared	0.12	0.19	0.9	0.91
Ind FE	Yes	Yes	No	No
Firm FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm

Table B3. Private Communication and Management Guidance: Poisson Regression

This table shows the Poisson regression results examining the effect of private communication between politicians and public firms on the frequency of overall management forecasts. The dependent variable, *Forecasts*, is defined as the total number of management forecasts issued annually by the firm. The independent variable of interest is the interaction term *Post* × *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 A1.

	(1)	(2)	(3)	(4)
		Poisson		
		Forecasts		
Post × *MajorGovCustomer*	-0.110*	-0.109*	-0.121**	-0.111**
	(-1.80)	(-1.77)	(-2.21)	(-2.07)
MajorGovCustomer	-0.083	0.027		
	(-0.60)	(0.20)		
Size		0.208***		0.175***
		(10.65)		(5.87)
LEV		0.252		0.253**
		(1.14)		(2.37)
BM		-0.043		0.161***
		(-0.56)		(3.85)
ROA		0.887***		0.037
		(4.11)		(0.31)
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

OA0.1. 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.1.

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; Caplan, Griswold, and Jarvis, 2013). 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 (2021) and Zhang (2021) to analyze private communications on environmental and social topics using conference call data.

The results, presented in Table OA.2, 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.1. 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* × *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 A1.

	(1)	(2)	(3)	(4)	(5)
	AnnualForecasts	*QuarterlyForecasts*	*EPSForecasts*	*NonEPSForecasts*	*ForecastDays*
Post × *MajorGovCustomer*	-0.061**	-0.035	-0.016	-0.078**	-0.053**
	(-2.09)	(-1.62)	(-0.75)	(-2.55)	(-2.37)
Size	0.086***	0.030***	0.025***	0.075***	0.056***
	(6.40)	(3.39)	(3.15)	(5.65)	(5.62)
LEV	0.132**	0.075	0.077**	0.103*	0.072*
	(2.46)	(1.61)	(2.02)	(1.96)	(1.71)
BM	0.034**	0.031***	0.026***	0.040***	0.027**
	(2.57)	(2.74)	(3.16)	(2.83)	(2.57)
ROA	-0.022	-0.033	-0.003	-0.039	-0.031
	(-0.74)	(-1.17)	(-0.13)	(-1.21)	(-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.2. 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* × *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 A1.

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

OA0.2. Propensity Score Matching

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 observation is then paired with the control observation that has the closest propensity score, ensuring that no control observation is used more than once. This matching process helps balance the treated and control groups in terms of key observable characteristics. The results are reported in table OA.3.

Table OA.3. Propensity Score Matching

This table presents the propensity score matched 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, *Frequency*, is defined as the natural logarithm of 1 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 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 A1

	(1)	(2)	(3)	(4)
		Frequency		
Post × *MajorGovCustomer*	-0.130**	-0.109*	-0.126***	-0.113***
	(-2.10)	(-1.81)	(-2.96)	(-2.67)
MajorGovCustomer	-0.021	0.004		
	(-0.22)	(0.05)		
Size		0.148***		0.090***
		(7.29)		(2.93)
LEV		0.058		0.358***
		(0.30)		(2.61)
BM		-0.013		0.019
		(-0.28)		(0.65)
ROA		0.274***		-0.041
		(3.07)		(-0.49)
Observations	3,708	3,708	3,708	3,708
R-squared	0.16	0.22	0.92	0.92
Ind FE	Yes	Yes	No	No
Firm FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm

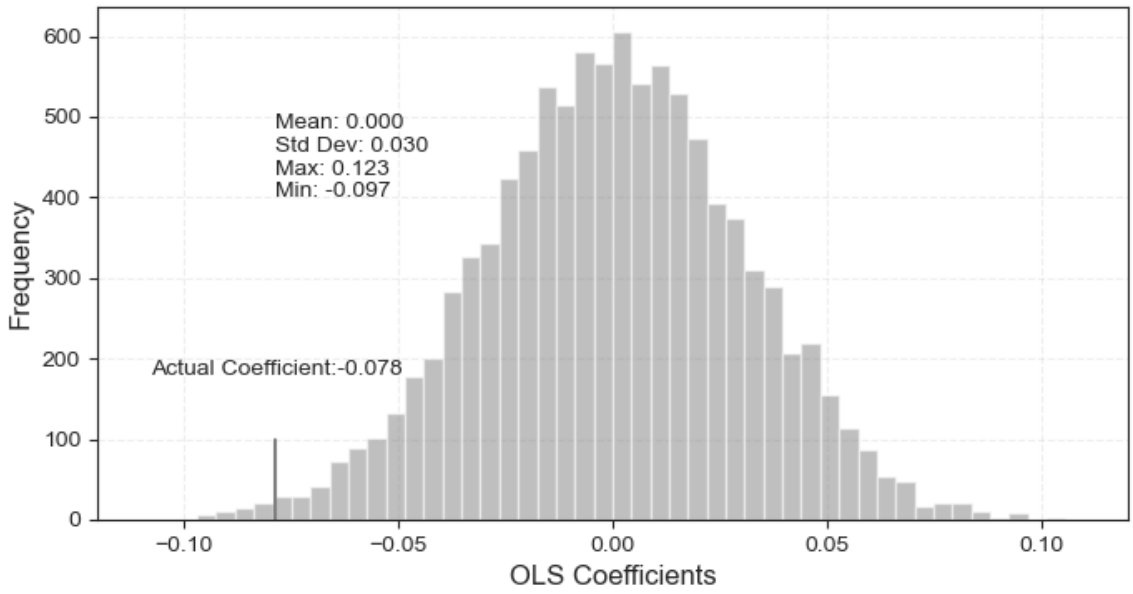
OA0.3. Placebo Test

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 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.

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. All variable definitions are defined in Table A1



OA0.4. Alternative Data Sample and Event Dates

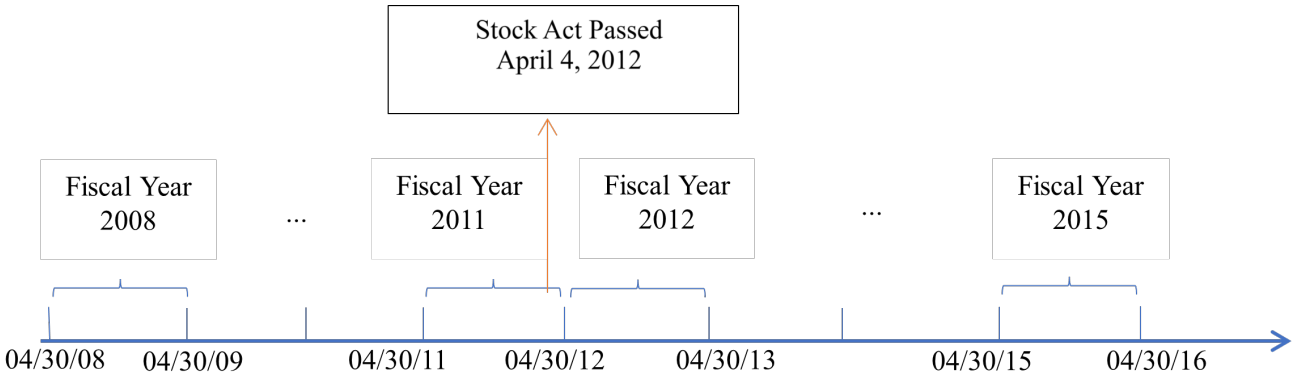
Since the STOCK Act's passage date does not align with the end of the fiscal year, we employed two alternative time measures to address this issue. We first applied the commonly adopted fiscal year method, excluding all observations from 2012, the year the STOCK Act was passed, and retaining data for 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.4 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.

Considering the uncertainty about the duration of the STOCK Act's impact on executive branch officers' activities, we also analyzed results using a three-year window before and after the act's passage. We examined the commonly used six-year period, excluding the 2008 and 2015 fiscal years, instead of the overall eight years in our primary result. Table OA.4 Column 3 shows the outcome remains significant and consistent with our main result. This finding confirms that the STOCK Act's supervisory power covers most of the Obama administration period. Cherry, Heitz, and Jens (2018) suggest that the date the *60 Minutes* episode on Congressional insider trading was aired, November 13, 2011, was critical to the passage of the STOCK Act. Our results are also robust to using November 13, 2011 as the event date.

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

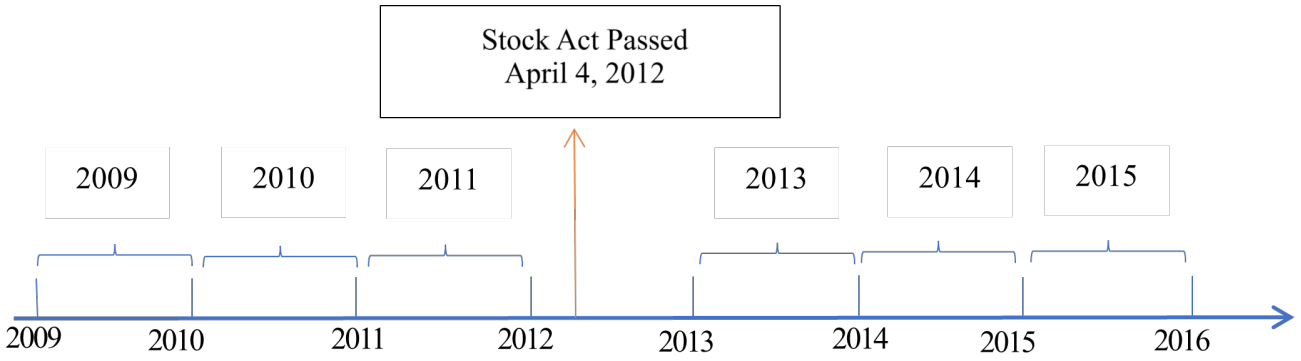


Table OA.4. Time Robustness Tests

This table presents the results of OLS regressions using three different potential time measures for the STOCK Act: excluding the action year (Column 1), using an alternative annual window (Column 2), and using a six-year sample period (Column 3). The dependent variable, *Frequency*, is defined as the natural logarithm of 1 plus the total number of management forecasts issued annually by the firm. The independent variable of interest is the interaction term *Post* × *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 A1.

	(1)	(2)	(3)
		Frequency	
	Exclude Year 2012	Alternative Fiscal Year	Six-year Period
Post × *MajorGovCustomer*	-0.098***	-0.078**	-0.057**
	(-2.64)	(-2.52)	(-2.04)
Size	0.064***	0.069***	0.085***
	(4.53)	(5.36)	(5.60)
LEV	0.074	0.099*	0.070
	(1.24)	(1.78)	(1.26)
BM	0.213	0.194	0.058***
	(1.33)	(1.18)	(3.32)
ROA	-0.053	-0.028	0.005
	(-1.39)	(-0.83)	(0.15)
Firm FE	YES	YES	YES
Year FE	YES	YES	YES
Observations	10,932	12,074	9,103
R-squared	0.89	0.91	0.93

OA0.5. 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.5, 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* × *MajorGovCustomer* is -0.156 with a significance level of 0.05.

Table OA.5 Column 3 highlights the results for unregulated industries, excluding SIC codes 6000-6999, 4900-4999, and 8000-8099, which correspond to the financial, utility, and pharmaceutical industries, respectively. Public companies in these industries are always supervised by the US Securities and Exchange Commission (SEC), the US Department of Energy, and the US Food and Drug Administration (FDA). These regulated firms can be considered both potential firms communicating privately with the government and heavily regulated firms. Therefore, we separated the data to avoid potential attribution concerns. As shown in the last Column of Panel B, the coefficient on *Post* × *MajorGovCustomer* is significant at the 0.1 level. This result confirms to a certain extent that unregulated industry firms are affected mainly by the STOCK Act. In contrast, regulated firms might be continuously monitored under the Obama administration, limiting the feedback mechanism of the STOCK Act.

Table OA.5. Alternative Sample Tests

This table presents the OLS regression results using alternative samples of firms: those making at least one management forecast (Column 1), control firms without government business for the entire sample period (Column 2), and firms in unregulated industries (Column 3). The dependent variable, *Frequency*, is defined as the natural logarithm of 1 plus the total number of management forecasts issued annually by the firm. The independent variable of interest is the interaction term *Post* × *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 A1.

	(1)	(2)	(3)
		Frequency	
	Using Firms Covered in I/B/E/S	Using Clean Control Firms	Excluding Firms in Regulated Industries
Post × *MajorGovCustomer*	-0.157**	-0.071**	-0.065**
	(-2.37)	(-2.30)	(-1.97)
Size	0.179***	0.083***	0.080***
	(5.64)	(5.74)	(5.67)
LEV	0.268**	0.125**	0.115**
	(2.40)	(2.22)	(2.07)
BM	0.122***	0.045***	0.041***
	(3.16)	(2.94)	(2.67)
ROA	-0.049	-0.044	-0.038
	(-0.58)	(-1.25)	(-1.12)
Firm FE	YES	YES	YES
Year FE	YES	YES	YES
Observations	6,101	11,682	11,313
R-squared	0.82	0.91	0.90