Legislative Trades*

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Abstract

We examine whether congressional stock trade disclosures shape investors' fiscal policy expectations. Using over 4,254 House trade reports (2014–2022), we find that purchase disclosures increase stock returns by 17-29 basis points at the ticker-day level with stronger effects for trades by relevant committee members, involving firms with high fiscal dependence, or during periods of budgetary uncertainty. Legislators' actual purchases predict the passage of economically-relevant bills, and such disclosure effects are concentrated when bills have not yet passed. Firms linked to such disclosures and relevant bills subsequently exhibit increased procurement contract amounts, and these real effects lend further support to the fiscal-policy expectation mechanism.

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1 Introduction

Members of the U.S. Congress trade financial assets while wielding direct authority in shaping fiscal, regulatory, and industry policies. These trades have drawn increasing media scrutiny in recent years.[1] Before 2012, there were no explicit legal restrictions on congressional trading based on nonpublic information, and disclosure requirements were governed by the Ethics in Government Act of 1978, which mandated only annual reporting. The landscape changed with the passage of the STOCK Act (Stop Trading on Congressional Knowledge Act) in 2012, which introduced a requirement that members report trades within 30 to 45 days of transactions, although penalties for noncompliance remain minimal.

Our paper proposes a new hypothesis: if insider trading concerns are valid, then the STOCK Act may have enhanced the transmission of such information to capital markets by requiring more timely disclosures. Under this hypothesis, congressional trade disclosures should contain information about future fiscal policy outcomes (e.g., procurement contracts and federal spending bills) and have effects on market expectations of firms' future fundamentals. We find empirical evidence that individual stock prices rise significantly following purchase disclosures, especially when they originate from relevant committee members, involve firms with high fiscal dependence on government contracts, or occur during periods of elevated budgetary uncertainty. We further show that legislators' actual trades predict the passage of fiscally expansionary bills and that disclosure effects are concentrated when economically-relevant bills have not yet passed — when policy uncertainty is highest. Our proposed "fiscal policy expectation" mechanism distinguishes this paper from the existing literature, which has largely focused on assessing congressional portfolio outperformance and its ongoing likelihood (see, e.g., Ziobrowski,

[1] For example, New York Times, https://www.nytimes.com/interactive/2022/09/13/us/politics/congress-members-stock-trading-list.html; WSJ, https://www.wsj.com/articles/health-secretary-nominee-tom-price-proposed-bill-benefiting-his-puerto-rico-investments-1485253807; Business Insider, https://www.businessinsider.com/congress-stock-act-violations-senate-house-trading-2021-9

Cheng, Boyd, and Ziobrowski (2004), Ziobrowski, Boyd, Cheng, and Ziobrowski (2011), Eggers and Hainmueller (2013), Blonien, Crane, and Crotty (2025)).

We next elaborate on two central findings of this paper: the benchmark stock market disclosure effect and supporting evidence for the mechanism. First, individual stock returns respond positively to congressional trade disclosures. Specifically, we analyze all 4,254 electronically readable trading reports filed by members of the U.S. House of Representatives between 2014 and 2022. Our analysis focuses on the full universe of daily S&P 500 stocks over the same period. When a stock purchase is disclosed on a given day, the corresponding stock's daily return increases by approximately 17-29 basis points on average. These effects are robust after controlling for firm- and macro-level factors, stock and time fixed effects, and clustering by firm and date. These effects emerge only on disclosure dates – not on the original transaction dates – indicating that the price impact is tied to the public revelation of trading information rather than to the trades themselves. We also examine heterogeneities across members, firms, and time. The return response is significantly stronger when disclosures originate from the congressional committee members overseeing the traded firms' industries, when the firms exhibit high fiscal dependence on government procurement contracts, and during periods of elevated budgetary uncertainty (e.g., around debt ceiling debates).

These patterns are consistent with a fiscal policy information channel; investors appear to interpret such disclosures as signals of future policy shifts affecting firm cash flows or fundamentals. Supporting this view, we also find that firm-days with disclosures subsequently exhibit higher analyst earnings forecast revisions, suggesting that these market reactions reflect changes in expectations about firms' future fundamentals rather than transitory price pressure.

More formally, our proposed mechanism implies two testable predictions. One, legislators' actual trades should contain information about the direction of future policy decisions. Two, the stock market response to trade disclosures should be stronger when the disclosure occurs before rather than after the passage of the relevant bill. We find

supportive evidence for both predictions.

We link each House bill introduced between 2014 and 2022 (a total of 32,184 bills) to its relevant industries via the committees overseeing it, and test whether legislators' actual purchases in those industries predict the bill's eventual passage. We find that pre-decision buying by relevant committee members significantly forecasts bill approval even after controlling for extant benchmark predictors of legislative survival such as deliberation length and party alignment. This predictive power is concentrated in fiscally expansionary bills – those proposing increases in government spending or investment – while completely disappearing for contractionary bills. This is consistent with the notion that trades signal expectations of forthcoming fiscal outlays.

Next, we examine the second testable prediction. Consider two scenarios. In Scenario 1, the disclosure of a trade occurs after the vote on the relevant bill; in Scenario 2, disclosure precedes the vote. If disclosures convey information about forthcoming policy, their price impact should be larger when legislative outcomes remain uncertain (i.e., Scenario 2) and muted once policy direction has been resolved (i.e., Scenario 1). Consistent with this prediction, we find that disclosure effects are large and statistically significant when they precede the passage of the associated bill but small and insignificant when they follow it. This timing pattern, which we refer to as our "conditional results," supports the view that congressional disclosures influence markets not as ex post confirmations but as signals of anticipated policy direction.

Finally, we provide evidence that congressional trades have real fiscal consequences through outlays. Using the Scenario 2 framework, we follow Brogaard, Denes, and Duchin (2021) and Xu, Yang, and You (2025) and leverage transaction-level obligation data from `USAspending.gov` to obtain federal procurement contract awards to firms linked to both the disclosed trades and the subsequently passed bills. We find that firms in industries connected to the passed bills receive significantly higher post-event procurement obligations than comparable non-linked firms. The effect is economically large: linked firms are awarded roughly $300 million to $820 million more in federal contracts over the following

3–12 months. These results show that congressional trades not only predict legislative outcomes and trigger market reactions but also precede actual fiscal flows to the traded firms, in turn supporting the fiscal policy expectation mechanism.

Our paper contributes to several strands of research. The literature linking fiscal policy and financial markets often highlights the role of political connections in shaping firm and institutional outcomes. Existing research shows that firms with ties to policymakers benefit through increased financial support (Acemoglu, Johnson, Kermani, Kwak, and Mitton (2016)), improved firm values (Goldman, Rocholl, and So (2009)), and greater access to government bailouts (Faccio, Masulis, and McConnell (2006)). Politicians may also channel economic favors through contracts (Brogaard, Denes, and Duchin (2021), Tahoun (2014)) or influence hedge funds and private equity (Gao and Huang (2016), Faccio and HSU (2017)). Beyond these mechanisms, recent work explores how fiscal expectations affect market participants. Bianchi, Gómez-Cram, and Kung (2024) and Xu and You (2025) study how investors, including analysts (Xu, Yang, and You (2025)), price policy-relevant information. Our research is the first to establish congressional trade disclosures as fiscal shocks to the capital market, offering a novel perspective within the fiscal expectations literature.

Second, in the context of the congressional insider trading debate, research has traditionally centered on abnormal returns (e.g., Ziobrowski, Cheng, Boyd, and Ziobrowski (2004), Ziobrowski, Boyd, Cheng, and Ziobrowski (2011), Eggers and Hainmueller (2013), Blau, Whitby, and Wilson (2018), Belmont, Sacerdote, Sehgal, and Van Hoek (2022), Blonien, Crane, and Crotty (2025)). Our approach shifts the focus to the timing and content of actual trades. Using the universe of bills and their alignment with disclosed trades, we provide a new empirical strategy to assess whether congressional trading behavior embeds informational advantages tied to legislative outcomes. In addition, by tracing trade disclosures to subsequent bill progress and asset price reactions, we show that these disclosures can signal future policy in ways that markets absorb.

2 Legal Background and Data

2.1 A History of U.S. Laws on Congressional Trading

This section outlines the legal timeline and current framework governing congressional financial disclosures, reviews key developments in recent years, and discusses the renewed public scrutiny following recent political events. These developments also inform our choice of sample period.

Before 2012, there was no statutory framework explicitly prohibiting members of the United States Congress from trading securities based on non-public information acquired through their official duties. Instead, ethical obligations were loosely defined under the Ethics in Government Act of 1978, which required annual disclosure of financial holdings and transactions by public officials: https://www.congress.gov/bill/95th-congress/senate-bill/555. These filings were often delayed and incomplete.

In response to mounting public and media scrutiny regarding potential conflicts of interest and the misuse of insider knowledge, Congress enacted the STOCK Act in April 2012: https://www.congress.gov/bill/112th-congress/senate-bill/2038. For the first time, the law explicitly prohibited members of Congress, their staff, and other federal employees from leveraging non-public information gained through their roles for personal financial gain. More concretely, the STOCK Act mandated that trades exceeding $1,000 be disclosed within 30 days of notification or 45 days of the transaction date, whichever comes earlier. There are asset class exemptions (e.g., mutual funds, ETFs, U.S. Treasury bonds etc.).[2] Late disclosures incur only a $200 fine. In April 2013, Congress passed an amendment that rolled back certain transparency provisions of the original legislation: https://www.congress.gov/bill/113th-congress/senate-bill/716. Notably, the amendment eliminated the requirement for electronic filing of

[2]The extensive list exempts members, if they choose, from reporting certain transactions involving mutual funds, exchange-traded funds, or any other asset that is an excepted investment fund (EIF) (see the financial disclosure report instructions for the definition of an EIF); holdings in a blind trust; real property; cash accounts (e.g., checking, savings, and money markets); U.S. Treasury bonds, bills, and notes; pensions; and any asset that is solely incidental to the trade or business of an entity.

financial disclosure forms and removed the provision mandating that these documents be made available in an online, searchable database.

The COVID-19 pandemic brought renewed scrutiny to congressional stock trading. Notably, Senators Richard Burr and Kelly Loeffler faced allegations of insider trading after selling substantial stock holdings following private briefings on the virus's potential impact. While investigations were conducted, no charges were filed, highlighting the difficulties in enforcing the STOCK Act's provisions. In response, members of Congress proposed two bills – the Ban Congressional Stock Trading Act and the TRUST in Congress Act – but neither of them advanced.

Three pieces of information are particularly relevant for our research. First, following the STOCK Act and its 2013 amendment, members of Congress are required to disclose all stock transactions. This requirement motivates our focus on stock trading as the asset class of interest and defines the starting point of our sample period in 2014. Second, we focus on digitally-typed disclosures (PDFs), which reflect the format most accessible to the media and the public in real time.[3] Additional details on data sources are provided in Section 2.2. Third, this statutory requirement significantly reduced the lag between a transaction and its public disclosure, thereby creating observable "disclosure events" that our research exploits to test asset pricing responses. Anti-insider-trading enforcement mechanisms remain limited, with no automatic criminal liability, which makes our findings on the conservative side. While the law established a formal disclosure obligation, it is unclear whether – if insider trading occurs – it has become less prevalent. If anything, more timely disclosures could accelerate the market's incorporation of the information. These disclosures serve as the basis of our empirical dataset and underpin the identification strategy used throughout this study.

[3]In a separate project, Dong, Xu, and Yu (2025) examine all handwritten, scanned disclosures.

2.2 Data

Our analysis combines several novel datasets that link congressional stock trade disclosures to firm characteristics, market outcomes, legislative activity, and federal procurement flows. In this section, we describe the disclosure database and its summary statistics in detail while deferring the discussion of other datasets to Section 4, where they are analyzed.

We collect all publicly available financial transaction reports filed by members of the U.S. House of Representatives from 2014 through 2022 under the STOCK Act's reporting requirements. Our raw data comes from the complete database at https://disclosures-clerk.house.gov/FinancialDisclosure, what the website refers to as "Periodic Transaction Reports."[4] These reports are obtained as a PDF file and digitization is done using Python.

Figure 1 provides an example of a periodic transaction report. Each periodic transaction report filed by a member of the House of Representatives includes several key fields. The Owner field indicates whether the transaction was conducted by the member, their spouse, or a dependent. The Asset field provides the name, type, and the ticker symbol of the security (if available), along with a basic description – for example, whether the asset is held in a blind trust and hence a passive investment. The Transaction Type specifies whether the asset was purchased (P) or sold (S). The Transaction Date records the calendar date on which the trade took place, while the Notification Date refers to when the filer was notified of the transaction, which could be immediate, via a monthly account statement or from a broker. The Amount field reports the approximate dollar value of the trade; only a range (e.g., \$1,001-\$15,000) is disclosed. Finally, the Digitally Signed Date – the date the report was electronically certified – serves as the immediate

[4]There is a separate website for Senate disclosures (https://efdsearch.senate.gov/search/home/). In this study, we focus on the House of Representatives. First, senators typically attract greater public attention due to the higher stakes associated with each seat; therefore, testing our hypothesis using House members provides a more conservative benchmark. Second, while we considered incorporating Senate disclosures into our framework, differences in legislative schedules across chambers would complicate our analysis later (involving the life cycles and outcome of bills, as discussed in Section 4). To maintain a cleaner and more coherent setting, we limit our focus to the House.

publication date, marking when the information becomes publicly available.

[Insert Figure 1 here]

We obtain 4,254 electronically-readable periodic transaction reports filed by members of the U.S. House of Representatives between 2014 and 2022. These disclosures come from 235 unique House members and account for a total of 37,226 reported trades across all asset types, including 31,543 trades specifically involving stocks. In terms of stock trading coverage, the dataset spans 640 distinct stock tickers in the S&P 500 universe over time, indicating a wide range of corporate securities actively traded by members of Congress during this period.

[Insert Figure 2 here]

[Insert Figure 3 here]

Figure 2 presents the rolling (past) 30-day totals of congressional stock purchases, showing the number of actual purchases (thin blue line) alongside the number of disclosed purchases (thick green line). As expected, the disclosed transactions consistently lag behind actual trades by approximately one month, aligning with the STOCK Act's 30- to 45-day disclosure requirement. This consistent delay suggests that disclosure timing is more likely driven by regulatory deadlines than by deliberate strategic release, an important consideration for our specification. We provide formal evidence in support of this interpretation in Section 3.1. The use of a 30-day rolling window also provides a simple visual lens to assess the timeliness of reporting, showing that most trades are indeed disclosed within the required timeframe. In fact, Figure 3's Panels (a) and (b) validate that most trades are being disclosed within 45 days.

In addition to illustrating the obvious lagging pattern, Figure 2 also reveals meaningful variation in congressional trading activity over time. Several notable disclosure peaks correspond to major macro volatility events, such as the mid-2015 global stock market volatility, the early 2020 Covid-19 events, and a pronounced increase around mid-2022

during heightened geopolitical tension linked to the Russia-Ukraine war. There are also disclosure peaks that coincide with political events. For instance, there is a disclosure surge in early 2017 following the start of a new (Trump) administration, and another disclosure spike in late 2021 when the SEC announced efforts to strengthen enforcement of the STOCK Act. These aggregate fluctuations are not the primary focus of our paper – which, as we explain later, exploits ticker-level return responses – but Figure 2 provides valuable insights into our empirical design. For one, our main specification should include year-month fixed effects to address overall macroeconomic trends and regulatory signals. Appendix Figure A1 offers a very different pattern.

Figure 3's Panel (c) shows the distribution of asset type classifications, distinguishing between self-directed (non-passive) and delegated (passive) trades. It is not surprising that most trades are stock trades due to the reporting exemptions for other asset classes. Figure 3's Panel (d) reports the number of individual disclosure files referencing a given number of tickers. Most of the files contain fewer than 10 unique stocks.

Congressional stock trades have a sensible distribution across industries as categorized by NAICS 2-digit codes (see Figure 4). The Information sector (NAICS 51), which includes technology firms, and the Finance and Insurance sector (NAICS 52) account for a substantial share of total trades. Additionally, Manufacturing (NAICS 31-33) emerges as the single most traded category, with over 1,500 transactions, which is expected as Tech firms typically use NAICS 33, 51, and/or 54. Appendix Figure A2 presents the distribution of congressional stock purchases across industries.

[Insert Figure 4 here]

3 Stock Market Effects

This section examines how the market responds to the public disclosure of congressional trades. Leveraging the gap between transaction and disclosure dates, we treat each disclosure as an informational event. In Section 3.1 we assess whether disclosure

timing is exogenous (relative to our research question and context). In Section 3.2 we estimate average stock return responses. Finally, in Section 3.3 we explore cross-sectional and time-series heterogeneities and earnings forecast revisions that should lend support to our mechanism hypothesis later in the paper.

3.1 Exogeneity Test

Previous graphical evidence discussed in Section 2.2 suggests that disclosure timing appears to be mostly consistent with regulatory deadlines, generating a clear lagging pattern in Figure 2. In this section, we formally assess whether disclosure dates are strategically timed, given our research question. Specifically, we test whether they cluster around major monetary or fiscal announcement events when investor attention would be knowingly diverted, depend on recent individual stock performance, or tend to occur on specific weekdays, which could bias subsequent return analyses.

We formulate the following panel specification,

$$\begin{aligned} ReportBuy_{1,i,t} = \alpha + \beta_1 \times I_{FOMC,[-3,1]} + \beta_2 \times I_{BillPassing,[-3,1]} \\ + \beta_3 \times PastPerformance_{i,t-N} + \beta_4 \times EPU_{ym(t)} + \epsilon_{i,t}, \end{aligned} \tag{1}$$

where $ReportBuy_{1,i,t}$ indicates whether purchases of stock i are disclosed on day t. This is a binary variable (1 if disclosed, 0 otherwise). $I_{FOMC,[-3,1]}$ is an indicator variable equal to one if the disclosure date falls within a window from three days before to one day after an FOMC meeting date. Similarly, $I_{BillPassing,[-3,1]}$ equals one if the disclosure date falls within a window from three days before to one day after any legal bill's floor voting date. These variables capture potential strategic timing, as disclosures released around major macroeconomic or fiscal events may coincide with periods when investor attention is diverted and thus draw less scrutiny. $PastPerformance_{i,t-N}$ captures past performance metrics for stock i, including short-term lagged returns, realized volatility, and Amihud (2002)'s illiquidity over N-day horizons. These variables account for another type of timing strategy in which a member might choose to disclose a trade when the

stock's recent performance has been weak, anticipating that the disclosure could boost its price. Finally, $EPU_{ym(t)}$ is the Economic Policy Uncertainty index (EPU) measured at the year-month level, controlling for broader, slow-moving macroeconomic and political uncertainty. β_1, β_2 and β_3 are of interest to test strategic timing.

[Insert Table 1 here]

Table 1 presents the regression results. We find no evidence that congressional trade disclosures are systematically timed to avoid or coincide with major macroeconomic announcements or Congressional voting events. Moreover, the timing of disclosures is not predictable based on recent stock-specific performance or non-linear measures such as volatility or illiquidity. Interestingly, disclosure dates are (contemporaneously) correlated with the EPU index at the aggregate level. This is hard to reconcile with a predictive interpretation, but this result motivates the inclusion of time fixed effects in later specifications.

Another concern is potential variation by day of the week; for instance, disclosing on Fridays could be perceived as strategic as investor attention or sentiment patterns could be different. Appendix Figure A3 demonstrates that most trading records are released on weekdays, and we find that the distribution of frequencies is relatively uniform across weekdays (Monday–Friday). For convenient matching with trading days, we only consider reports published on a weekday in our analysis.

Furthermore, we examine the possibility that members of the House of Representatives engage in strategic trading by investing in firms headquartered within their legislative districts to obtain political support from local voters. Empirical evidence contradicts this mechanism: as shown in Appendix Figure A4, there is no systematic tendency for politicians to favor district-based firms in their portfolios. This pattern allows us to rule out the pursuit of local voter favor as a primary explanation for their investment behavior.

3.2 Main Specification

We next examine stock return responses to congressional trade disclosures. We estimate two complementary specifications at the ticker-day level: one using the full sample and following Bianchi, Gómez-Cram, and Kung (2024), who use a rich set of continuous firm- and macro-level controls (Section 3.2.1),[5] and the other focusing on a high-salience subsample that isolates days when only one ticker has a reported purchase disclosure (Section 3.2.2). Together, these two approaches allow us to assess the average ticker-level response across various specifications and sample choices.

3.2.1 Full Sample

We start with the following specification following Bianchi, Gómez-Cram, and Kung (2024):

$$Ret_{i,t} = \alpha + \beta \times ReportBuy_{i,t} + \boldsymbol{\delta} \times \boldsymbol{Macro_t} + \boldsymbol{\gamma} \times \boldsymbol{Firm_{i,t}} + \varepsilon_{i,t}, \tag{2}$$

where $Ret_{i,t}$ denotes daily stock returns i on day t (source: CRSP), and $ReportBuy_{i,t}$ captures whether purchases of stock i are disclosed on day t by any House members, either as a binary variable (1/0) or as the number of purchases reported on the same day t. The specification includes control variables following Bianchi, Gómez-Cram, and Kung (2024) and Pástor and Stambaugh (2003). These include $\boldsymbol{Macro_t}$, a vector of top-50 macroeconomic announcement variables capturing daily macro surprises (see list in Appendix Table A1), and $\boldsymbol{Firm_{i,t}}$, which includes intraday illiquidity, lagged returns, and analysts' forecast revenue and earnings surprises. We include fixed effects at the stock and year-month levels (following Bianchi, Gómez-Cram, and Kung (2024) and Chen, De, Hu, and Hwang (2014)). Standard errors are clustered by firm and date. The analysis uses daily data from the full S&P 500 universe over 2014-2022, excluding windows surrounding FOMC meetings ([-3,1]) and the initial Covid-19 NBER-recession

[5] Bianchi, Gómez-Cram, and Kung (2024) study stock return responses to stock mentions in the tweets of members of Congress.

period (February to April 2020) to avoid confounding macro shocks. Coefficient estimate of β is of interest.

[Insert Table 2 here]

Table 2 reports the results; the top panel uses the binary report buy variable, and the bottom panel uses the number of reported purchases. Across all columns, the coefficient estimates of interest are all positive and statistically significant. In Column (1), where only double clustering is applied and no controls are included, the coefficient on $ReportBuy_1_{i,t}$ is 0.002041 and statistically significant at the 1% level, suggesting a 20 basis point increase in returns on the day a purchase is disclosed. Columns (2)-(4) add firm controls, macroeconomic announcement controls, and fixed effects, and demonstrate highly significant results with similar coefficient magnitudes. The coefficient estimate in the most saturated specification is 0.001666, statistically significant at the 1% level. When a new stock purchase record is published, the corresponding ticker-daily returns increase by 17 bps (or 0.08 SD) on average.

To put the estimated return effect into perspective, consider the market capitalization ($MCAP) of the affected firms. For a large stock with a market cap between $1–3 trillion, a 17 basis point increase in daily return translates to a market value gain of approximately $2–5 billion. For an average stock in the sample with a market cap around $80 billion, the same return impact implies a gain of about $140 million in market value.

Results in the bottom panel are similar when using the number of reported purchases rather than a binary indicator, suggesting that the intensive margin is unlikely to drive the main effect. This is expected because the number of trades reported on the same day typically ranges from 1 to only 8 (i.e., 1∼8 House Representative(s) disclosing purchase trades of the same ticker on the same day). Appendix Table A2 shows that stock market returns do not exhibit any significant change on the actual transaction date.

3.2.2 High Salience Sample

Table 3 examines stock return responses within the "Single-Ticker Active" subsample, which isolates *days* when only one ticker has a reported purchase disclosure and the *firm* exhibits above-median investor attention. Figure 5 presents two firm-level measures constructed to capture investor attention to individual tickers in two aspects. The left panel shows the average number of analysts covering each firm in a given quarter based on I/B/E/S data. The right panel reports each firm's average daily trading volume calculated from CRSP. Firms above both medians are classified as having above-median investor attention in our empirical analyses. This single-ticker active sample should reduce potential cross-ticker correlations and enhance the salience of each disclosure event. Appendix Table A3 shows the summary statistics of variables in Table 3.

[Insert Figure 5 here]

Under the specification with day fixed effects (Panel (a) of Table 3), the estimated coefficient on ReportBuy_1 in the active sample is approximately 0.0028, implying a 28 basis point increase in daily returns on disclosure days. This effect is economically meaningful and larger than the baseline estimates in Table 2, consistent with the expectation that disclosure signals are more salient when only one ticker is involved and investor attention is high. Panel (b) produces comparable magnitudes when daily market returns are included as controls instead of day fixed effects, reinforcing the robustness of the result to alternative ways of absorbing common shocks.

In sharp contrast, the placebo sample, which comprises firms with below-median investor attention, shows coefficients that are near zero and statistically insignificant under both estimation frameworks and panels. The absence of any detectable return response in this placebo group underscores that the observed effects are not driven by coincidental market-wide movements or spurious correlations. Together, these results demonstrate that the positive return response to congressional trade disclosures is particularly concentrated in settings where the signal is likely to be most visible to market participants.

[Insert Table 3 here]

3.3 Robustness, Heterogeneities, and Earnings Forecasts

First, Table 4 presents a series of robustness checks for the baseline results in Tables 2 and 3. Across all robustness columns, ReportBuy_1 coefficient estimates remain positive, statistically significant, and of similar magnitude. Specifically, the effect persists when we (i) restrict the sample to pre-2020 observations to address concerns about pandemic-driven market dynamics, (ii) exclude trades from House majority leaders to rule out leadership-specific behavior, (iii) control for prior-day returns to absorb short-term momentum, and (iv) include contemporaneous sell disclosures to net out opposing trading signals. The coefficients on ReportBuy are consistently positive, statistically significant, and only slightly smaller in magnitude than those for ReportBuy_1, indicating that the return response scales proportionally with the number of disclosed trades but does not substantially exceed the extensive-margin effect.

[Insert Table 4 here]

Appendix Table A4 also examines stock return responses within a "Single-Ticker Alternative Active" subsample, which as before isolates days when only one ticker has a reported purchase disclosure but now only considers the *firm* with an above-the-median number of analysts. Results remain intact and, in fact, are even a bit stronger.

In terms of heterogeneities, we examine how the stock return effect varies across institutional, political, and economic contexts. Specifically, we test whether the effect is stronger when disclosures: (i) are made by committee members overseeing the ticker's industry, (ii) originate from non-passive trades, (iii) involve firms with greater fiscal dependence on government contracts, or (iv) occur during periods of heightened budgetary uncertainty. These heterogeneity analyses lay the empirical groundwork for our proposed "fiscal policy expectations" mechanism, which is examined in the subsequent section.

Table 5 presents cross-sectional heterogeneities in the stock returns effect. Columns (1), (2), (6), and (7) across both samples and specifications show that the effect is stronger

in economic magnitude when disclosures originate from members of congressional committees overseeing the firm's industry (see Appendix Table A5 for the mapping). This result is consistent with the view that investors perceive these trades as more informative. Columns (3) and (8) verify that the effect mostly comes from non-passive (self-directed) trades, which should better reflect legislators' own discretion and information.

Furthermore, we construct cross-sectional fiscal dependence (FD) measures to capture a firm's or industry's reliance on federal government procurement contracts. The idea is that if congressional trades convey information about future fiscal policy, then investors should respond more strongly to a firm's trade disclosures when the firm is more closely linked to federal spending policies. Specifically, we follow the methodology of Xu, Yang, and You (2025) and compute FD using `USAspending.gov` data as the ratio of a firm's federal contract revenue to its total annual revenue, measured at the firm-year level and then averaged across years. For the heterogeneity analysis, we aggregate these firm-level FD values at the two-digit NAICS industry level, excluding NAICS 92 (Public Administration).[6] To retain broader coverage, we classify firms based on their industry-level FD. Figure 6 displays the cross-industry distribution of FD rankings. Columns (4), (5), (9), and (10) show that the return response is larger for firms with a higher fiscal dependence on government contracts. Taken together, the cross-sectional heterogeneity evidence in Table 5 provides an initial indication that investors may interpret these disclosures as informational shocks that alter their expectations about future fiscal policy.

[Insert Figure 6 here]

[Insert Table 5 here]

We also explore whether being in a budgetary uncertainty period amplifies the return effect of disclosures. The analysis includes interaction terms between ReportBuy variables and an event variable of debt ceiling percentage changes (see Appendix Figure A5).

[6]Commercial datasets such as Capital IQ occasionally assign NAICS 92 to public agencies or quasi-governmental entities, and their measured fiscal dependence in our dataset is invariably near 100%.

Significant and positive interaction term coefficients in Table 6 indicate that the effect intensifies during periods of elevated budgetary uncertainty. This suggests that in uncertain fiscal environments, markets are more responsive to congressional trades as potential signals of forthcoming policy direction.

[Insert Table 6 here]

Finally, Figure 7 presents the estimated coefficients of cumulative I/B/E/S forecast revisions on ReportBuy_1 shocks. For each horizon x, we compute the cumulative change in mean EPS forecasts from t_{D-5} to t_{D+x} standardized by the firm's historical standard deviation, following Bianchi, Gómez-Cram, and Kung (2024). The panel regression controls for recent earnings surprises, illiquidity, and volatility and includes firm and report-date fixed effects with standard errors clustered by firm. The analysis is conducted on the Single-Ticker Active subsample.

This figure complements our main and heterogeneity analyses above because it allows us to test whether congressional trade disclosures not only move stock prices contemporaneously but also shift analysts' expectations about firms' future earnings. Evidence of upward forecast revisions following disclosure events would suggest that investors interpret these disclosures as informative signals about firms' future fundamentals, which is consistent with our proposed fiscal policy expectations channel. Appendix Figure A6 displays the corresponding coefficients of a ReportSell_1 shock.

[Insert Figure 7 here]

4 Fiscal Policy Expectation Mechanism

Building on the evidence above, we propose a fiscal policy expectations channel as a plausible mechanism through which congressional trade disclosures influence market expectations of firms' future fundamentals. This mechanism implies two testable predictions:

(1) {**Bill Outcome Predictability:**} Legislators' actual trades should contain information about the direction of future policy decisions.

(2) {**Conditional Results:**} The stock market response to trade disclosures should be stronger when the disclosure occurs before rather than after the passage of the relevant bill.

In Sections 4.1 and 4.2, we develop empirical frameworks to test these predictions and directly link them to the main return-based analysis in Section 3.2.

4.1 Bill Outcome Predictability

In this section, we investigate whether *actual* congressional trades convey information about forthcoming fiscal policies — a fiscal policy expectations mechanism. Specifically, we test whether legislators' actual purchases predict the subsequent passage of bills that could affect the traded firms. Using a bill-industry (Bill-NAICS $\{b, j\}$) framework, we examine whether pre-decision congressional buys from relevant committee members forecast bill passage and assess the robustness of this relationship. We then show that the predictive effect persists under alternative bill–committee and bill-level specifications and is concentrated in fiscally expansionary bills rather than contractionary ones.

We begin by assembling a comprehensive dataset of all bills introduced in the U.S. House of Representatives between 2014 and 2022, totaling 32,184 bills. 27,308 of them are assigned to committees. Each bill first undergoes committee review and, if approved, proceeds to the House floor for a final vote. Each committee can be mapped to specific industries (see Appendix Table A5), enabling us to link each bill to its associated industries and construct a bill–industry (Bill–NAICS) level dataset. This structure allows us to examine industry-specific trading activity in the days leading up to major legislative decisions.

The following specification examines whether the likelihood of a bill b's passage can be predicted by actual buys in the relevant industry j within 30 days preceding the bill's

final decision date. This unit of observation is therefore at the bill-NAICS level:

$$BillPass_{b,j} = \alpha + \beta \times ActualBuy_{t(b)-1,t(b)-30,j} + \boldsymbol{\gamma} \times \boldsymbol{Controls_b} + \varepsilon_{b,j}. \quad (3)$$

Control variables in $\boldsymbol{Controls_b}$ include two predictors of bill survival drawn from Yano, Smith, and Wilkerson (2012): the number of days between a bill's proposal and decision dates, $DecisionLength_b$, and an indicator for whether the sponsoring representative belongs to the House majority party, $I_{SameParty,b}$. These two variables serve as benchmark controls. The regression specification also includes industry and day fixed effects to account for persistent sector-level characteristics and common macroeconomic shocks. $ActualBuy_{t(b)-1,t(b)-30,j}$ measures the total number of actual buys in industry j stocks made by members of the House committee overseeing the bill during the 30 days preceding its decision date. $ActualSell_{t(b)-1,t(b)-30,j}$ can be similarly measured. β is the coefficient of interest. Appendix Table A6 presents the summary statistics of variables applied in the above regression framework.

Table 7 presents the regression results. From Columns (1) and (2), the two benchmark variables that explain bill survival, capturing the legislative calendar and political alignment, respectively, exhibit expected signs and significance levels given prior literature (e.g., Yano, Smith, and Wilkerson (2012)). This provides additional validation of the model's structure and our independently-created database.

Columns (3) and (4) include our key variable $ActualBuy_{t(b)-1,t(b)-30,j}$. The coefficient estimate is positive and statistically significant at the 1% level. This finding suggests that legislators' trading activities anticipate policy outcomes: bills associated with industries experiencing concentrated buying by relevant committee members are more likely to have a passing outcome. The magnitude of the effect is economically meaningful: a one standard deviation (SD) increase in industry-specific purchase volume is associated with approximately a 1.1 percentage point increase in the probability of bill passage relative to an average passage rate of 8%. As a robustness test, Column (4) further controls for the total number of actual sell trades during the same time interval prior to the bill's

decision date.

[Insert Table 7 here]

We show a series of robustness tests: sample choices, unit of observations, and alternative horizons. First, across all columns of Table 8, the coefficients on $ActualBuy_{t(b)-1,t(b)-30,j}$ remain positive, statistically significant, and similar in magnitude, indicating that the main result is not driven by modeling choices or sample composition. Specifically, the effect persists when we (i) consider only a pre-2020 sample, (ii) exclude trades from majority leaders, (iii) drop passive trades, and (iv) include additional controls for contemporaneous industry or market conditions. Table 9 tests whether the predictive relationship between congressional trades and bill passage holds under alternative aggregation frameworks. Columns (1) and (2) aggregate trades at the bill–committee level, and Columns (3) and (4) collapse them to the bill level. In both settings, the coefficient on $ActualBuy$ remains positive and statistically significant, while benchmark controls continue to behave as expected. Finally, Appendix Tables A7 and A8 consider alternative horizon choices and show robust results.

[Insert Table 8 here]

One important heterogeneity to study is expansionary bills versus contractionary ones. We anticipate results being stronger when bills are expansionary. We classify U.S. House bills into Expansionary (P), Neutral (N), and Contractionary (S) using a two-step approach. First, we apply a keyword-based method, labeling bills based on the relative frequency of spending- versus selling-related terms in their titles and descriptions. Bills are labeled as Expansionary if spending-related keywords (e.g., "financial aid," "funding," "public expenditure," "budget approval") appeared more frequently, and as Contractionary if selling- or cutback-related terms (e.g., "downsizing," "divestment," "budget cut," "liquidation") dominated.[7] Bills with no matches or equal counts

[7]The set of expansionary words includes: spend, spending, expenditure, expenditures, budget, appropriation, appropriations, funding, allocation, allocations, grant, grants, subsidy, subsidies, disbursement,

were labeled Neutral. Second, we manually reviewed Neutral cases and reclassified them when their content clearly aligned with expansionary or contractionary fiscal intent.

[Insert Figure 8 here]

Out of 27,308 House bills introduced between 2014 and 2022, we classify 9,254 as expansionary and 1,674 as contractionary. Figure 8 plots the rolling three-month counts of expansionary and contractionary bills introduced in the U.S. House of Representatives from 2014 to 2022. Expansionary bills – those involving proposed increases in government spending or investment – consistently outnumber contractionary bills throughout the sample period. Peaks in expansionary activity often coincide with major political or economic events, including transitions in congressional control. By contrast, contractionary bills remain relatively stable and lower in volume, suggesting that legislative activity tends to emphasize fiscal expansion over restraint for most of the period studied. One additional interpretation is that, in an environment dominated by expansionary policy activity, sell trades are less likely to convey meaningful fiscal information and therefore elicit weaker market responses.

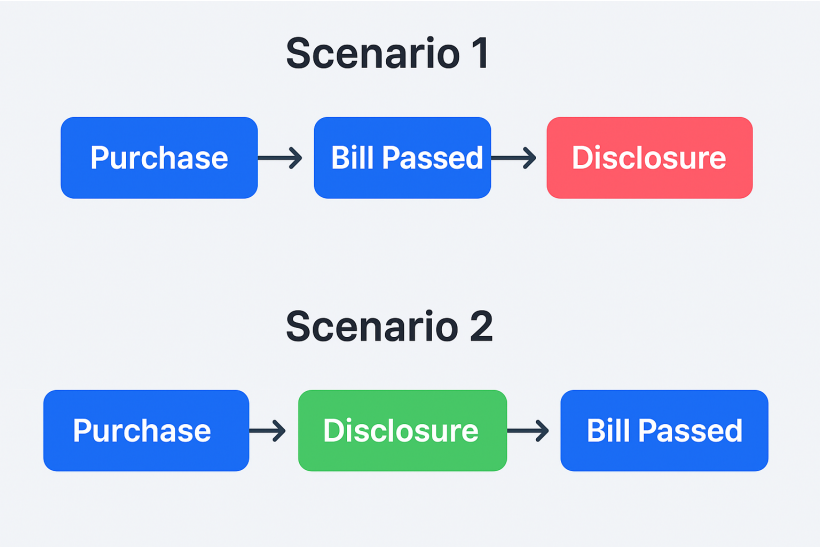
[Insert Table 10 here]

Table 10 complements Table 7 by showing that the predictive power of congressional purchase trades for bill passage is indeed stronger if we focus on expansionary bills. For expansionary bills, a one SD increase in actual purchases in the relevant industry significantly predicts a 0.72% higher chance of an expansionary bill passing, suggesting that legislators may act in anticipation of increased government spending. By contrast, the coefficient for contractionary bills is small, negative, and statistically insignificant.

disbursements, fiscal, outlay, outlays, financing, monetary, government spending, public funds, public spending, treasury, exchequer, revenue expenditure, financial support, financial aid, aid, public expenditure, investment, investments, expense, expenses, operational cost, cost, costs, budgetary, appropriated, disbursed, allocated, financial allocation, fiscal allocation, government aid, program spending, spending measure, fiscal policy, expenditure measure, budget approval. The set of contractionary words includes: sell, selling, divest, divestment, divestitures, privatize, privatization, disinvestment, cutback, cutbacks, reduction, reduce, downsizing, liquidate, liquidation, asset sale, sale, sell-off, disposal, dispose, auction, auctioning, cost-cutting, streamline, streamlined, efficiency, efficiencies, cost saving, savings, contraction, austerity, defunding, budget cut, privatise, offload, outsourcing, restructure, restructuring, reorganization, reorganize, fiscal restraint, expense reduction, reduce spending, sell government assets, privatize public assets, asset disposal, government sale, divestiture, expenditure reduction, operational cutback.

4.2 Conditional Results

We next examine whether the stock market reacts more strongly to congressional trade disclosures when they occur before, rather than after, the passage of the relevant bill. To illustrate the conceptual framework, we include a diagram below. This test directly evaluates the second implication of the fiscal policy expectation mechanism, as described earlier in Section 4: if disclosures convey information about forthcoming policy, their price impact should be larger when legislative outcomes remain uncertain (i.e., Scenario 2) and should be muted once the policy direction has been resolved (i.e., Scenario 1). In other words, this anticipatory interpretation aligns with the fiscal expectations hypothesis, in which disclosures are viewed as informative about future congressional action.



One empirical challenge in implementing this test is to identify a sufficiently salient sample. We begin with all purchase disclosure events. For Scenario 1, we apply the criterion such that at least one bill was passed within the 21 days before the disclosure but none within 21 days after. For Scenario 2, we retain disclosure events where at least one bill is passed within the 21 days after the disclosure date but no such bill is passed within the 21 days before the disclosure. Each such case spans a 43-day window centered on the disclosure day. We also distinguish between bills that are related to the disclosure and those that are not.

[Insert Table 11 here]

We estimate our main ticker-day specification separately for the two scenarios, including the full set of firm- and macro-level controls, fixed effects, and double clustering. Table 11 presents the test results. Appendix Tables A9 and A10 display detailed summary statistics of the variables applied in this table.

Columns (1)-(3) use ReportBuy_1, a binary indicator for disclosure events, while Columns (4)-(6) use ReportBuy, which captures the number of members reporting purchase disclosures of stock i on day t. The results show a sharp contrast: the coefficient estimates of ReportBuy variables are statistically significant and positive, and in addition, are statistically greater in Scenario 2 than in Scenario 1 across all columns. The coefficient equality test is shown at the bottom of the table. This pattern indicates that the market reaction is concentrated in disclosure events occurring before policy resolution, consistent with the interpretation that investors respond to congressional trades as information shocks indicating forthcoming fiscal policy actions.

We also examine an intuitive source of heterogeneity: whether the bill's content is linked to the stock's NAICS industry through the Committee-NAICS mapping described above. In Scenario 2, Columns (2) and (5) restrict the sample to bills with content relevant to the disclosed stock's industry, and their coefficients are larger in magnitude than those in Columns (1), (3), (4), and (6). Finally, coefficient estimates under Scenario 2 are also larger in magnitude compared to those from benchmark results in Tables 2 and 3, which is expected given the design.

Table 12 extends the conditional results (Table 11) with four robustness checks and three heterogeneity splits. For Scenario 2 (when disclosures come before the bill passage), coefficients on $ReportBuy_1$ and $ReportBuy$ stay positive and significant across (i) pre-2020 subsamples, (ii) dropping House Majority Leader trades, (iii) adding lagged return ret_1, and (iv) controlling for ReportSell, with magnitudes broadly in the 0.003-0.008 range. The effect is stronger in magnitude when trades are by relevant committee members, non-passive (self-directed) rather than delegated, and in high fiscal dependence

firms, reinforcing an informational, policy-linked interpretation. By contrast, for Scenario 1, results are systematically weaker and insignificant overall.[8]

[Insert Table 12 here]

4.3 Real Effects

In this section, we extend Scenario 2 and provide evidence for real effects, exploring whether firms linked to the disclosed trades and subsequent bills actually receive more federal procurement contracts afterward. This analysis directly evaluates whether the market reactions we observe are grounded in actual fiscal outcomes, as predicted by the fiscal policy expectations mechanism.

We take Scenario 2 events and classify firms by whether they operate in industries linked to the passed bill using our committee–NAICS mapping. We then measure the total dollar amount of federal contract obligations these firms receive (from `USAspending.gov`)[9] over the post-event horizon (in units of $100 million); this is our dependent variable. The key treatment indicator equals one if the firm's industry is linked to the passed bill.

Table 13 shows that firms in industries linked to the passed bill experience significantly larger post-event procurement awards than non-linked firms. Panel (a) measures procurement obligations from the disclosure date, and Panel (b) measures from the bill passage date. Each is tracked over 3-, 6-, 9-, and 12-month horizons. Controlling for industry-specific trends in procurement awards and common market-wide shocks, we find that firms in linked industries receive approximately $300 million to $820 million more in federal contract obligations over the subsequent 3–12 months. These results show that congressional trades not only predict bill passage and move stock prices, but also precede real increases in federal spending toward the traded firms, reinforcing the interpretation that these disclosures embed information about forthcoming fiscal allocations.

[Insert Table 13 here]

[8] Appendix Tables IA.1 to IA.7 display detailed regression results for Table 12.

[9] We thank the authors of Xu, Yang, and You (2025) for sharing their raw procurement obligation data.

4.4 An Illustrative Case Study

To complement our aggregate analyses, we present an illustrative case that highlights the mechanisms documented above. This example traces a specific sequence of legislative activity, trading behavior, and subsequent disclosure by a member of Congress, linking it to both contemporaneous stock price movements and related federal spending legislation. By zooming in on an individual disclosure event, this case study demonstrates how congressional trades can embed information about forthcoming fiscal policy actions, reinforcing the interpretation of disclosure events as informative signals to the market.

David B. McKinley represented West Virginia's 1st congressional district as a member of the U.S. House of Representatives. He served on the Committee on Energy and Commerce and was affiliated with the Republican Party. His term in office spanned from January 3, 2011 to January 3, 2023.

- 5/21/2014: 113th congress H.R. 4701 was introduced, which amends the Public Health Service Act to require the Secretary of Health and Human Services (HHS) to conduct or support epidemiological, basic, translational, and clinical research such as Lyme disease and other tick-borne diseases. The budget was $338 million.[10]
  TMO daily ret = 0.74%
- 8/19/2014: He purchased Thermo Fisher Scientific (ticker=TMO, NAICS=33; a life science research company).
  TMO daily ret = 0.27%
- 9/2/2014: He disclosed this purchase record.
  TMO daily ret = 2.09%
- 9/8/2014: The bill passed the Committee on Energy and Commerce.
  TMO daily ret = 0.28%
- 9/9/2014: The bill passed the House.
  TMO daily ret = -0.78%



[10] https://www.congress.gov/bill/113th-congress/house-bill/4701

5 Conclusion

Our paper posits and demonstrates that stock trades by members of Congress contain information about future legislative outcomes, which gets priced into the market through disclosure events by shaping investors' expectations of fiscal policy. Stock prices react positively to purchase disclosures, particularly when they originate from relevant committee members, involve firms with high fiscal dependence on government contracts, or occur during periods of heightened budgetary uncertainty, such as around debt ceiling debates.

We further show that these disclosure effects are concentrated when the associated bills have not yet passed, and that firms linked to such disclosures subsequently receive more federal procurement contracts, providing evidence of real effects and a fiscal policy channel. Taken together, our findings suggest that congressional trade disclosures serve as one conduit through which future fiscal policy information is transmitted to capital markets, and that the timely disclosure requirements introduced by the 2012 STOCK Act may have amplified this transmission, contributing to greater volatility in firms' cash flow expectations in the past decade.

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Filing ID #20009718

PERIODIC TRANSACTION REPORT

Clerk of the House of Representatives • Legislative Resource Center • 135 Cannon Building • Washington, DC 20515

FILER INFORMATION

Name: Hon. Barbara J Honorable Comstock
Status: Member
State/District: VA10

TRANSACTIONS

ID	Owner	Asset	Transaction Type	Date	Notification Date	Amount	Cap. Gains > $200?
		iShares Exponential Technologies ETF (XT) [ST] FILING STATUS: New	S	06/05/2018	06/07/2018	$50,001 - $100,000	☐
		Mastercard Incorporated (MA) [ST] FILING STATUS: New	P	06/07/2018	06/11/2018	$50,001 - $100,000	☐
		Microsoft Corporation (MSFT) [ST] FILING STATUS: New	P	06/05/2018	06/07/2018	$15,001 - $50,000	☐
		Vanguard FTSE Emerging Markets ETF (VWO) [ST] FILING STATUS: New	S	06/05/2018	06/07/2018	$50,001 - $100,000	☐

☑ I CERTIFY that the statements I have made on the attached Periodic Transaction Report are true, complete, and correct to the best of my knowledge and belief.

Digitally Signed: Hon. Barbara J Honorable Comstock 06/11/2018

Figure 1: **A Periodic Transaction Report Example.**
This figure presents screenshots from an original disclosure file. Reported variables include the asset's name and class, type of transaction, transaction date, date of notification, and transaction amount range. In addition, our research is interested in the date on which the report is signed and released to the public.

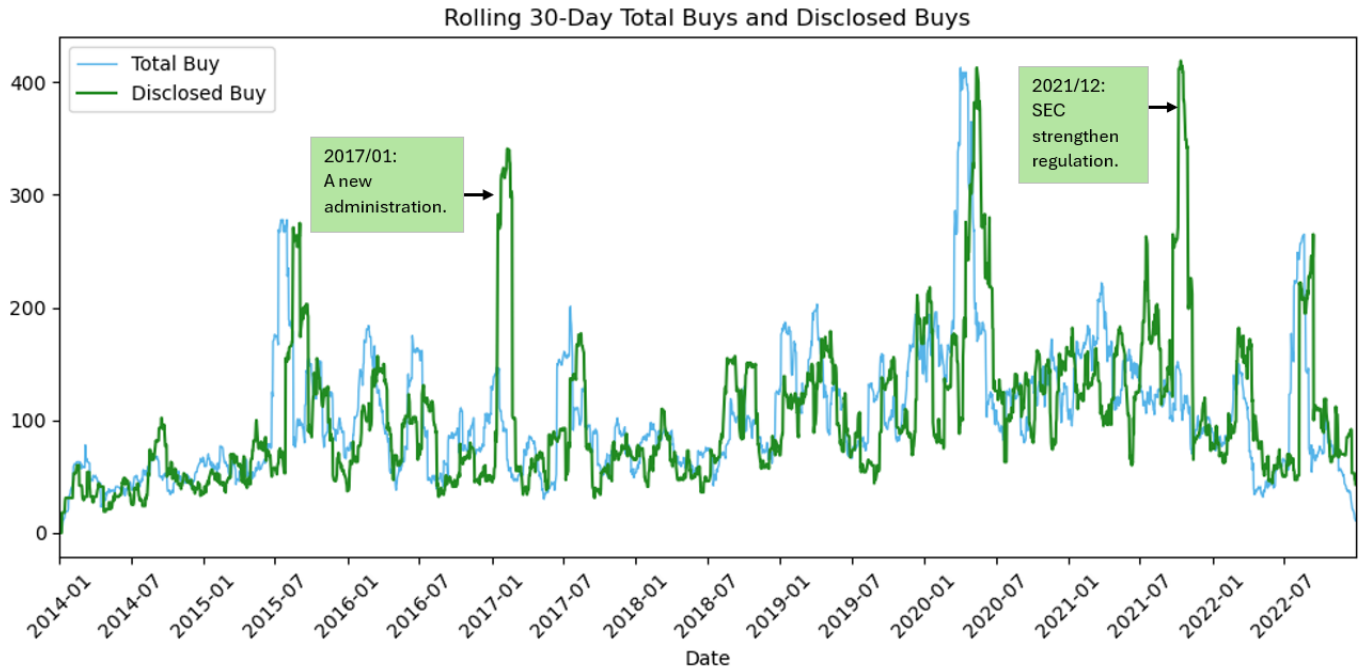


Figure 2: **Number of Actual Buys vs. Disclosed Buys over Time.**
This figure presents the time variation in the total actual and disclosed stock purchases (i.e., buys) from all members of the House of Representatives in a 30-day rolling window. Appendix Figure A1 provides the sell counterpart.

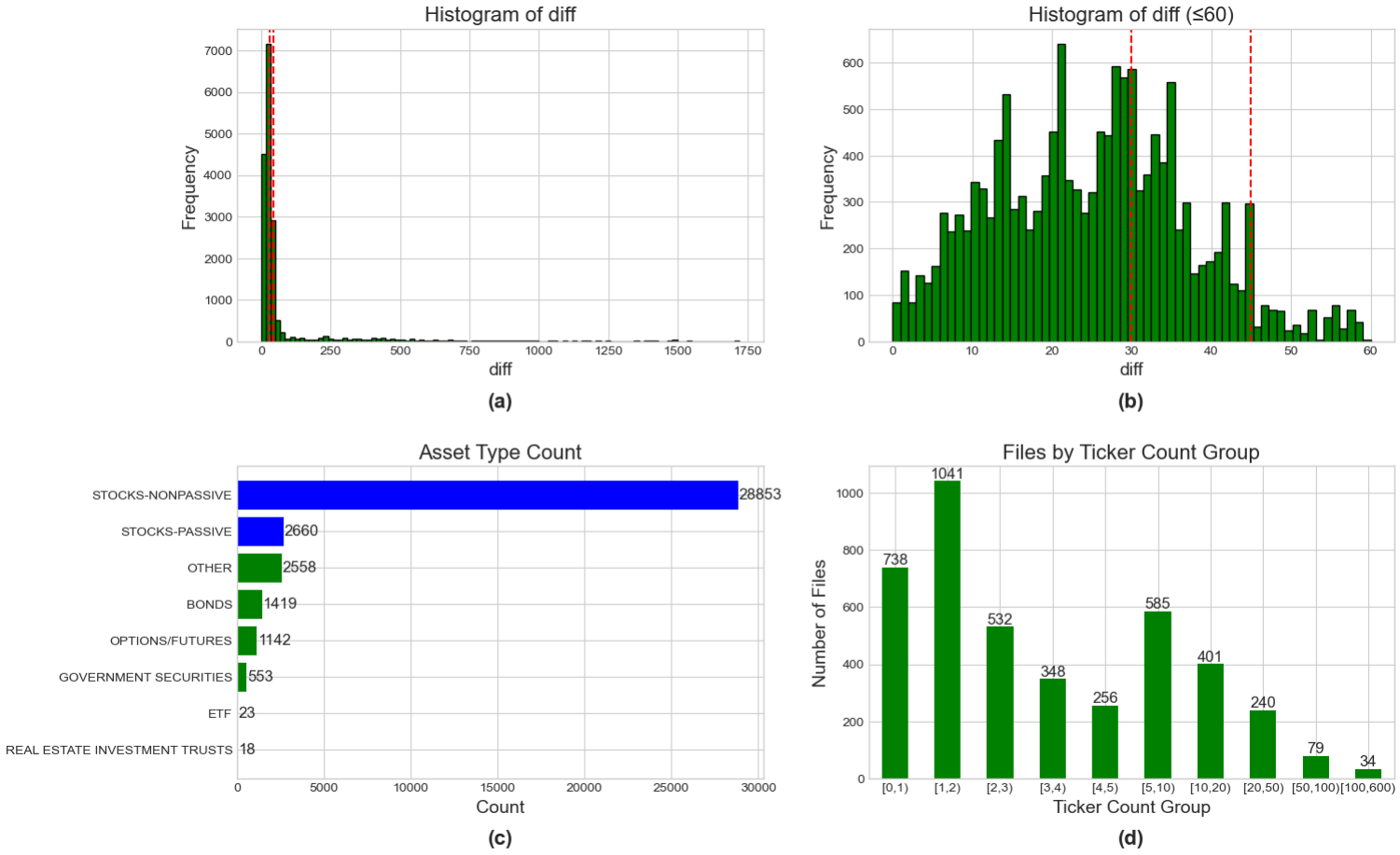
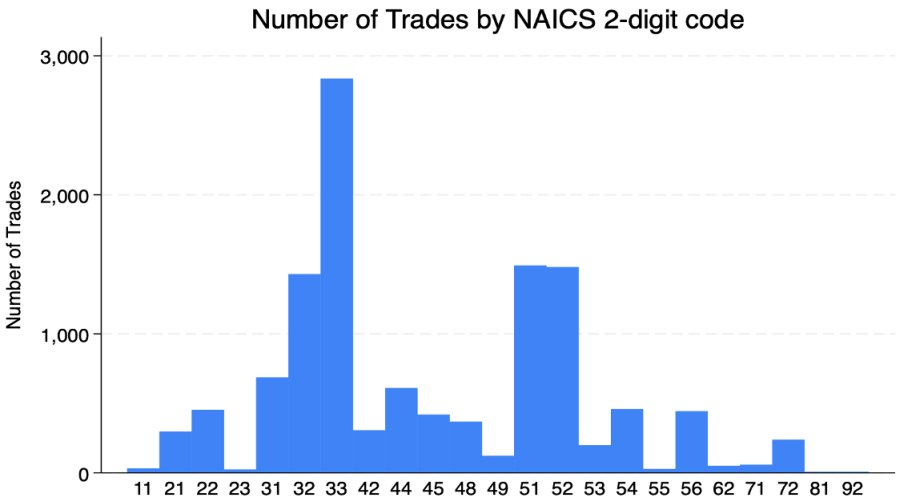


Figure 3: **Other Descriptions.**
Panel (a) plots the differences between actual transaction dates and disclosure dates, with the two red lines indicating Day 30 and Day 45, respectively. Panel (b) provides an enlarged view of the distribution, truncated at Day 60. Panel (c) shows the distribution of asset type classifications, distinguishing between self-directed (non-passive) and delegated (passive) trades. As discussed in Section 2, members of Congress are generally exempt from reporting transactions in most asset classes other than stocks. Panel (d) reports the number of individual disclosure files referencing a given number of tickers; for example, "1041" indicates that 1,041 individual files each mention only one ticker.



NAICS	Sector Name
11	Agriculture, Forestry, Fishing and Hunting
21	Mining, Quarrying, and Oil and Gas Extraction
22	Utilities
23	Construction
31-33	Manufacturing
42	Wholesale Trade
44-45	Retail Trade
48-49	Transportation and Warehousing
51	Information
52	Finance and Insurance
53	Real Estate and Rental and Leasing
54	Professional, Scientific, and Technical Services
55	Management of Companies and Enterprises
56	Administrative and Support and Waste Management
61	Educational Services
62	Health Care and Social Assistance
71	Arts, Entertainment, and Recreation
72	Accommodation and Food Services
81	Other Services (except Public Administration)
92	Public Administration

Figure 4: **Stock Trades across Industries.**
This bar chart displays the total number of stock trades aggregated by two-digit NAICS industry codes across all disclosure files in our sample period. For ease of interpretation, the corresponding industry names are shown on the right. Figure A2 is the purchase counterpart.

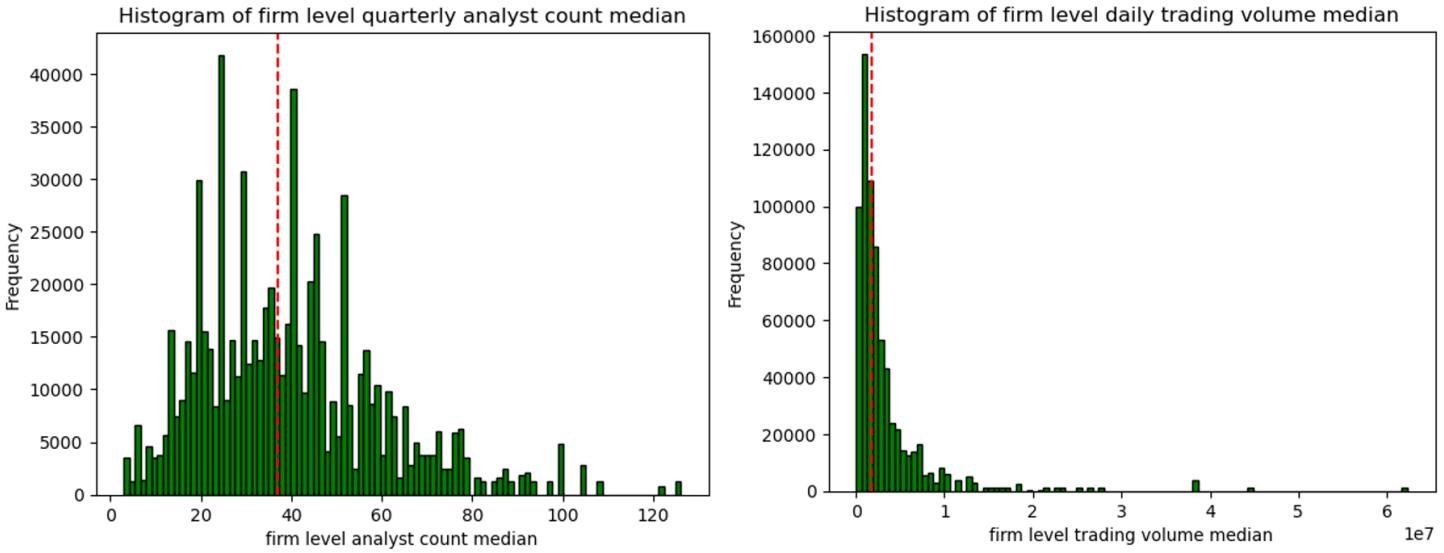


Figure 5: **Firm-Level Investor Attention Indicators.**
This figure presents two firm-level measures constructed to capture investor attention to a given ticker: the number of analysts covering the firm each quarter (left panel, sources: authors' calculations and I/B/E/S) and the average daily trading volume (right panel, source: CRSP). The two dashed red lines indicate the median values within each distribution.

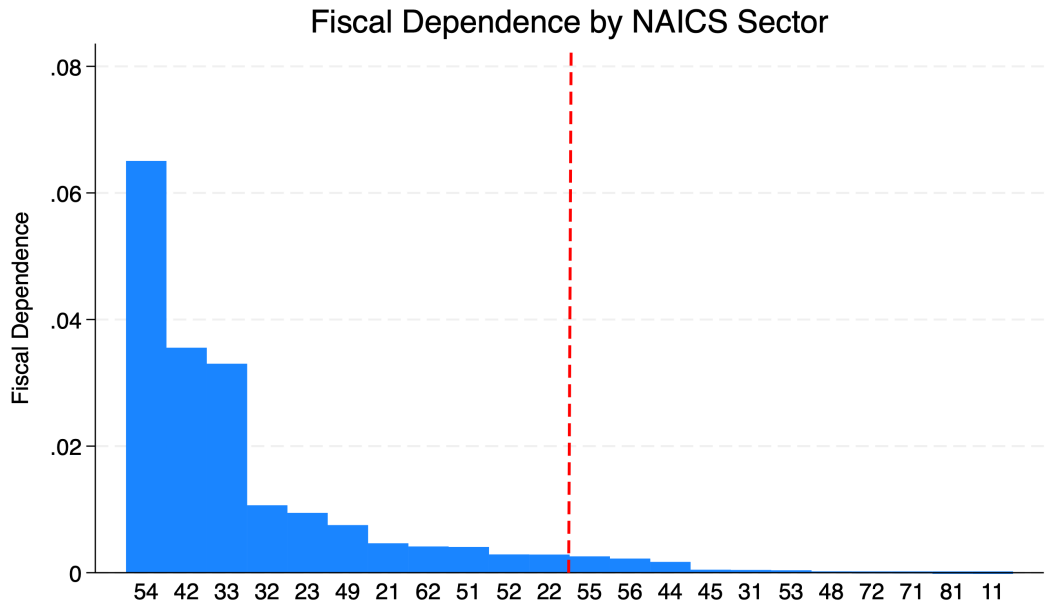


Figure 6: **Fiscal Dependence.**
This bar chart illustrates the variation in fiscal dependence (FD) on government contracts across industries. We construct firm-level FD using archival data from USAspending.gov, which provides detailed information on the timing and dollar amounts of federal contract obligations to each firm. For each firm-year, we calculate FD as the ratio of total government contract revenue to the firm's total annual revenue. We then aggregate firm-year FD measures to the firm level and the two-digit NAICS industry level for our heterogeneity analysis. We use industry-level FD because not all firms within an industry receive procurement contracts, and this approach allows us to retain a larger set of firms in the analysis. Consistent with our research focus, we systematically exclude NAICS Sector 92 (Public Administration), which does not encompass private firms in the conventional sense. Commercial datasets such as Capital IQ occasionally assign NAICS 92 to public agencies or quasi-governmental entities, and their measured fiscal dependence in our dataset is invariably near 100%. The vertical dashed red line indicates median value.

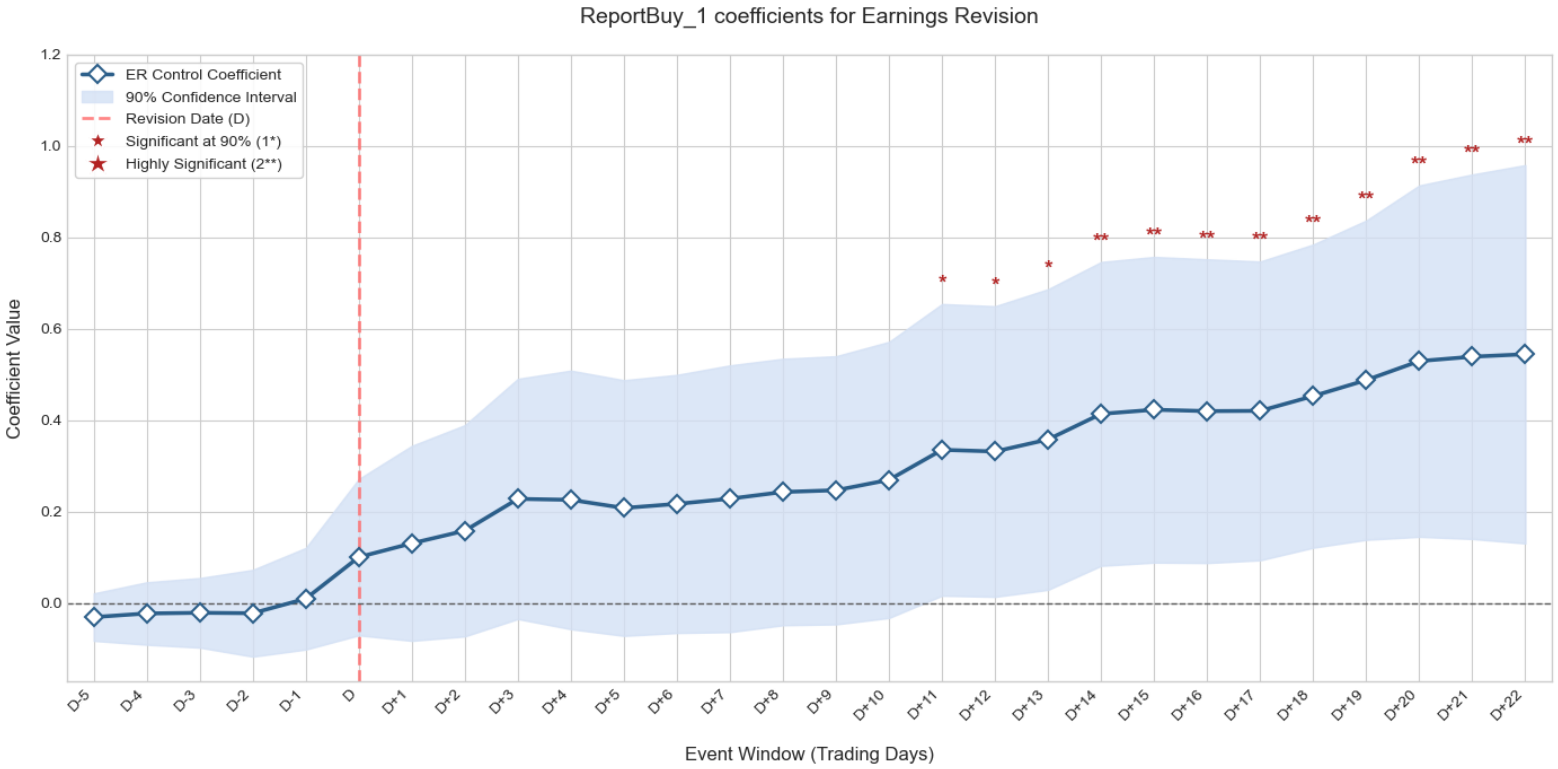


Figure 7: **Coefficients of Cumulative I/B/E/S Forecast Revisions on a ReportBuy$_1$ Shock.**
This figure presents the estimated coefficients (and their 90% confidence intervals) from a panel regression of cumulative I/B/E/S forecast revisions on a ReportBuy$_1$ shock. Forecast revisions are constructed following Bianchi, Gómez-Cram, and Kung (2024): for each interval x of interest, we compute the cumulative change in mean EPS forecasts from t_{D-5} to t_{D+x} and standardize this change by dividing it by the firm-specific sample standard deviation. The specification adopts Table 3, Panel A; it controls for recent earnings surprises, recent past illiquidity, recent past volatility, and firm (ticker) and disclosure report date fixed effects. It uses standard errors clustered by firm. The estimation is conducted on the Single-Ticker Active subsample. ***, p-value <1%; **, <5%; *, <10%. Appendix Figure A6 shows the corresponding coefficients of a ReportSell$_1$ shock.

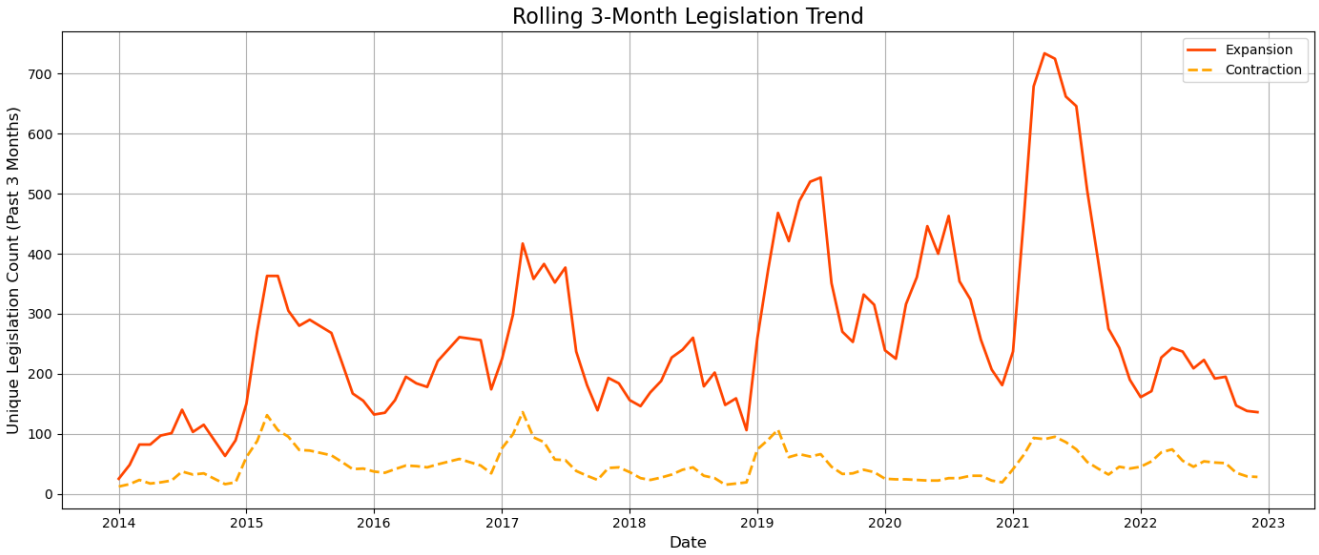


Figure 8: **Expansionary or Contractionary Nature of a Legal Bill.**
This figure plots the time-varying number of fiscally expansionary (solid orange line) and contractionary (dashed yellow line) bills measured within a three-month rolling window over our sample period. We collect all bills sponsored or proposed by members of the U.S. House of Representatives during this period (N = 27,308) and classify them as fiscally expansionary, contractionary, or neutral based on the summary text provided for each bill. Bills are coded as fiscally expansionary if they contain language indicating net increases in discretionary spending, the establishment of new transfer programs, or reductions in taxes or fees. Bills are coded as fiscally contractionary if they contain language signaling net reductions in spending (including program eliminations or funding cuts) or net increases in government revenues through mechanisms such as new taxes, tariffs, fines, or penalties. The remaining bills are either low in fiscal salience (e.g., procedural or regulatory) or too general to be classified (e.g., appropriations).

Table 1: **Exogeneity Test.**
This table tests whether recent monetary policy events, fiscal policy events, or stocks' past performance have predictive power for whether a ticker i purchase record is reported on day t. ReportBuy_1 equals one if a purchase record of ticker i is disclosed on day t. $I_{FOMC,[-3,1]}$ equals one if day t is in the window of [FOMC_meeting_date - 3 days, FOMC_meeting_date + 1 day]. $I_{BillPassing,[-3,1]}$ equals one if day t is in the window of [Bill_passage_date - 3 days, Bill_passage_date + 1 day]. ret_1 is the past one-day return for ticker i at date t. ret_5 is the past five-day return for ticker i at date t. illiq_5 follows the illiquidity measure proposed by Amihud (2002), and we construct the Amihud illiquidity measure using five-day rolling windows. volatility_5 is a volatility metric based on the square root of the sum of squared past 5-day returns. $EPU_{ym(t)}$ is the monthly Economic Policy Uncertainty (EPU) index constructed by Baker, Bloom, and Davis (2016). Double-clustered standard errors at the ticker and disclosure date level are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

DV:		ReportBuy_1	
Stock FE:	X	X	X
Double cluster:	X	X	X
$I_{FOMC,[-3,1]}$	0.001000	0.000972	0.000973
	(0.000863)	(0.000862)	(0.000867)
$I_{BillPassing,[-3,1]}$	0.000045	0.000215	0.000218
	(0.000478)	(0.000472)	(0.000473)
ret_1			-0.008227
			(0.006328)
ret_5			-0.005790
			(0.006403)
illiq_5			0.999501
			(0.738218)
volatility_5			0.002474
			(0.004695)
$EPU_{ym(t)}$		0.000016***	0.000016***
		(0.000006)	(0.000006)
Constant	0.004485***	0.002602***	0.002539***
	(0.000370)	(0.000696)	(0.000707)
N	1263315	1263315	1256542
R^2	0.0088	0.0090	0.0090

Table 2: **Stock Return Responses to Congressional Trade Disclosures: Full Sample and Preliminary Evidence.**
This table considers the same specification as in Bianchi, Gómez-Cram, and Kung (2024) at the ticker-daily level. ReportBuy_1 equals one if a purchase record of ticker i is disclosed on day t. ReportBuy equals the number of purchase records of ticker i being disclosed on day t. We use the same list of control variable choices as in Bianchi, Gómez-Cram, and Kung (2024): Firm-time control variables include standardized unexpected earnings (SUE) and standardized unexpected revenue (SUR) measures from Froot, Kang, Ozik, and Sadka (2017), Amihud illiquidity considering a five-day window, and past five-day return volatility. Macro-level control variables include announcement surprises from Bloomberg's Macroeconomic Announcements dataset (please see Appendix Table A1 for a detailed list of the macro-level control variables.). To conserve space, estimated coefficients of constants and control variables are omitted; full estimation results are available upon request. Double-clustered standard errors at the ticker and disclosure date level are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

DV:	Daily stock returns			
Firm controls:	Yes	Yes	Yes	Yes
Macro controls:	No	Yes	Yes	Yes
Stock FE:	No	No	Yes	Yes
Year-Month FE:	No	No	No	Yes
	(1)	(2)	(3)	(4)
ReportBuy_1	0.002041***	0.001850***	0.001816***	0.001666***
	(0.000746)	(0.000609)	(0.000609)	(0.000612)
N	1095174	1095174	1095174	1095174
R^2	0.00084	0.012	0.013	0.024
ReportBuy	0.001410***	0.001291***	0.001264***	0.001145**
	(0.000538)	(0.000440)	(0.000439)	(0.000453)
N	1095174	1095174	1095174	1095174
R^2	0.00084	0.012	0.013	0.024

Table 3: **Stock Return Responses to Congressional Trade Disclosures: Single-Ticker Active Sample.** This table focuses on the subsample $\{i, t\}$ consisting of (a) days on which no more than one ticker has a disclosed purchase record and (b) firms with above-median investor attention as measured by analyst coverage and historical trading volume (see also Figure 5). We refer to this subsample as the "Single-Ticker Active" sample in the paper. Under our hypothesis, this subsample should intuitively show more salient patterns than the full sample. The remaining stocks are examined in the Placebo sample. This table uses either day fixed effects (Panel A) or daily market returns (Panel B), and as a result, we do not include macro controls. See Table 2 for a detailed list of firm control variables. To conserve space, estimated coefficients for the constant and control variables are omitted; full estimation results are available upon request. Standard errors clustered at the ticker level are reported in parentheses, which is appropriate in this analysis given that the subsample contains only one ticker with purchase disclosures on any given day (i.e., no cross-ticker correlations are possible). ***, p-value <1%; **, <5%; *, <10%.

Panel A: With day fixed effects.				
DV:	Daily stock returns			
Firm controls:	Yes	Yes	Yes	Yes
Stock FE:	Yes	Yes	Yes	Yes
Day FE:	Yes	Yes	Yes	Yes
Daily S&P market returns as controls:	-	-	-	-
Stock subsample:	Active Stocks		Placebo Stocks	
	(1)	(2)	(3)	(4)
ReportBuy_1	0.002806**		0.000030	
	(0.001329)		(0.001356)	
ReportBuy		0.001843		-0.000508
		(0.001215)		(0.001139)
N	233755	233755	473328	473328
R^2	0.28	0.28	0.25	0.25

Panel B: Without day fixed effects, but with daily market returns.				
DV:	Daily stock returns			
Firm controls:	Yes	Yes	Yes	Yes
Stock FE:	Yes	Yes	Yes	Yes
Day FE:	No	No	No	No
Daily S&P market returns as controls:	Yes	Yes	Yes	Yes
Stock subsample:	Active Stocks		Placebo Stocks	
	(1)	(2)	(3)	(4)
ReportBuy_1	0.003759**		-0.000199	
	(0.001675)		(0.001367)	
ReportBuy		0.002833**		-0.000550
		(0.001277)		(0.001172)
N	233759	233759	473333	473333
R^2	0.25	0.25	0.23	0.23

Table 4: **Robustness tests for Tables 2 and 3.**
Panels A and B provide robustness evidence to Tables 2 and 3. To conserve space, estimated coefficients for the constant and control variables are omitted; full estimation results are available upon request. Standard errors are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

	Panel A: Full Sample (Table 2)					**Panel B: Single-Ticker Active Sample (Table 3-A)**				
DV:	Daily stock returns					Daily stock returns				
Firm controls:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Macro controls:	Yes	Yes	Yes	Yes	Yes	-	-	-	-	-
Stock FE:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month FE:	Yes	Yes	Yes	Yes	Yes	-	-	-	-	-
Day FE:	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
	Benchmark	Pre-2020 only	Drop majority leaders	Control for ret(1)	Control for report sells	Benchmark	Pre-2020 only	Drop majority leaders	Control for ret(1)	Control for report sells
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ReportBuy_1	0.001666***	0.001013**	0.001627***	0.001651***	0.001606***	0.002806**	0.002654*	0.002810**	0.002806**	0.002858**
	(0.000612)	(0.000454)	(0.000617)	(0.000609)	(0.000590)	(0.001329)	(0.001607)	(0.001337)	(0.001330)	(0.001334)
ReportSell_1					0.000770					-0.001302**
					(0.000604)					(0.000633)
N	1095174	753378	1095174	1095174	1095174	233755	172064	233755	233755	233755
R^2	0.024	0.025	0.024	0.024	0.024	0.28	0.25	0.28	0.28	0.28
ReportBuy	0.001145**	0.000751**	0.001121**	0.001135**	0.001112**	0.001843	0.002593*	0.001843	0.001843	0.001879
	(0.000453)	(0.000376)	(0.000456)	(0.000451)	(0.000437)	(0.001215)	(0.001411)	(0.001219)	(0.001215)	(0.001217)
ReportSell					0.000308					-0.000744*
					(0.000426)					(0.000438)
N	1095174	753378	1095174	1095174	1095174	233755	172064	233755	233755	233755
R^2	0.024	0.025	0.024	0.024	0.024	0.28	0.25	0.28	0.28	0.28

Table 5: **Heterogeneities: Non-time Series Variables.**
Panels A and B provide important heterogeneity evidence for Tables 2 and 3, respectively. Columns (1) and (6) consider the purchase disclosure records of ticker-days ($\{i,t\}$s) coming from the House committee members of the same industry as ticker i, which can be understood as a committee of knowledge. The mapping is delegated in Appendix Table A5; for instance, the Committee of Agriculture is matched with the NAICS 2-digit code "11 – Agriculture, Forestry, Fishing and Hunting." Columns (2) and (7) consider $\{i,t\}$s when purchase disclosure records do not come from a member of the committee for that industry. Columns (3) and (8) consider only non-passive trading records; stocks explicitly held within *trusts* documented in the "subholding" section of the record are classified as passive trades. Columns (4) and (9) consider industries with high fiscal dependence (FD) during our sample period, meaning those with an above-median procurement contract amount as a fraction of total revenue; see Figure 6 for more details. Columns (5) and (10) consider industries with low FD. To conserve space, the estimated coefficients for the constant and control variables are omitted; full estimation results are available upon request. Standard errors are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

	Panel A: Full Sample (Table 2)					**Panel B: Single-Ticker Active Sample (Table 3-A)**				
DV:	Daily stock returns					Daily stock returns				
Firm controls:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Macro controls:	Yes	Yes	Yes	Yes	Yes	-	-	-	-	-
Stock FE:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month FE:	Yes	Yes	Yes	Yes	Yes	-	-	-	-	-
Day FE:	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
	Committee	Non-Committee	Non-Passive	Firms with High Fiscal Dependence	Firms with Low Fiscal Dependence	Committee	Non-Committee	Non-Passive	Firms with High Fiscal Dependence	Firms with Low Fiscal Dependence
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ReportBuy_1	0.002420**	0.001512**	0.001948***	0.001815***	0.001387**	0.008115*	0.001969	0.002806**	0.003818***	0.001168
	(0.000948)	(0.000651)	(0.000640)	(0.000698)	(0.000603)	(0.004182)	(0.001465)	(0.001329)	(0.001198)	(0.003683)
N	1095174	1095174	1095174	747374	347799	233755	233755	233755	167159	66592
R^2	0.024	0.024	0.024	0.026	0.023	0.28	0.28	0.28	0.31	0.28
ReportBuy	0.001955***	0.000959**	0.001363***	0.001293**	0.000838*	0.007571***	0.000840	0.001843	0.003194***	-0.000436
	(0.000709)	(0.000482)	(0.000482)	(0.000515)	(0.000475)	(0.002742)	(0.001327)	(0.001215)	(0.001066)	(0.002670)
N	1095174	1095174	1095174	747374	347799	233755	233755	233755	167159	66592
R^2	0.024	0.024	0.024	0.026	0.023	0.28	0.28	0.28	0.31	0.28

Table 6: **Heterogeneities: Budgetary Uncertainty Periods.**
Panels A and B provide heterogeneity evidence for Tables 2 and 3, respectively, interacting with a time-series indicator of heightened budgetary uncertainty periods. Appendix Figure A5 highlights the month of, and the month preceding, each official change in the debt ceiling. We interpret these two months as having high budgetary uncertainty. To conserve space, estimated coefficients for the constant and control variables are omitted; full estimation results are available upon request. Standard errors are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%.

	Panel A: Full Sample (Table 2)		**Panel B: Single-Ticker Active Sample (Table 3-A)**	
DV:	Daily stock returns		Daily stock returns	
Firm controls:	Yes	Yes	Yes	Yes
Macro controls:	Yes	Yes	-	-
Stock FE:	Yes	Yes	Yes	Yes
Year-Month FE:	Yes	Yes	-	-
Day FE:	No	No	Yes	Yes
	(1)	(2)	(3)	(4)
ReportBuy_1	0.001427**		0.001845	
	(0.000647)		(0.001378)	
ReportBuy		0.000998**		0.001224
		(0.000477)		(0.001245)
Interaction with changes in debt limit	0.021515**	0.015032*	0.056348*	0.040662
	(0.010074)	(0.008035)	(0.029599)	(0.027735)
N	1095174	1095174	233755	233755
R^2	0.024	0.024	0.28	0.28

Table 7: **Predicting Bill Passing Using Actual Buys.**
This table examines whether the likelihood of a bill passing can be predicted by actual congressional stock buys in the relevant industry j within the 30 days preceding the bill's final decision date. This unit of observation is therefore at the bill-NAICS level. Specifically, we consider all proposed legal bills in the House of Representatives during our sample period and obtain the bill outcomes (=1 if it passed the committee and passed the chamber; =0 otherwise). We consider three main independent variables. The first two, $DecisionLength_b$ and $I_{SameParty,b}$, are drawn from Yano, Smith, and Wilkerson (2012), who document that bills with longer deliberation periods and those proposed by members of the majority party are positively associated with the likelihood of passage. $DecisionLength_b$ denotes the number of days between a bill's proposal date and its final decision date. $I_{SameParty,b}$ equals one if party affiliations align. We introduce and construct a new predictor, $ActualBuy_{t(b)-1,t(b)-30,j}$, which measures the total number of actual buys in industry j stocks made by members of the House committee overseeing the bill during the 30 days preceding its decision date. $ActualSell_{t(b)-1,t(b)-30,j}$ can be similarly measured. Standard errors are double-clustered by industry and bill passage date. ***, p-value <1%; **, <5%; *, <10%.

DV:	Bill Passing Likelihood			
Unit of observation:	Bill-Naics (b, j)			
Industry FE:	Yes	Yes	Yes	Yes
Day FE:	No	Yes	Yes	Yes
	(1)	(2)	(3)	(4)
$DecisionLength_b$	0.000854***	0.000791***	0.000790***	0.000790***
	(0.000220)	(0.000129)	(0.000128)	(0.000128)
$I_{SameParty,b}$	0.054505***	0.030799***	0.030754***	0.030754***
	(0.008318)	(0.005568)	(0.005595)	(0.005600)
$ActualBuy_{t(b)-1,t(b)-30,j}$			0.010697**	0.010006**
			(0.004269)	(0.003691)
$ActualSell_{t(b)-1,t(b)-30,j}$				0.000899
				(0.001626)
Constant	0.023283***	0.041497***	0.039382***	0.039271***
	(0.006601)	(0.008071)	(0.008223)	(0.008274)
N	55394	55269	55269	55269
R^2	0.14	0.53	0.54	0.54

Table 8: **Robustness Tests for Table 7.**
This table shows the results of various relevant robustness tests. Columns (1) and (2) only consider years prior to 2020. Columns (3) and (4) drop trades from majority leaders. Columns (5) and (6) drop passive trades. Columns (7) and (8) control for industry-level macroeconomic conditions; the employment growth data is obtained from the BLS database, and the real gross output growth rate data is obtained from the BEA database. Standard errors are double-clustered by industry and bill passage date. ***, p-value <1%; **, <5%; *, <10%.

DV:	Bill Passing Likelihood							
Unit of observation:	Bill-Naics (b, j)							
Industry FE:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FE:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Pre-2020		Drop majority leaders		Drop passive trades		Include industry macro controls	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$DecisionLength_b$	0.000807***	0.000807***	0.000789***	0.000789***	0.000790***	0.000790***	0.000789***	0.000789***
	(0.000099)	(0.000099)	(0.000128)	(0.000128)	(0.000128)	(0.000128)	(0.000127)	(0.000127)
$I_{SameParty,b}$	0.040040***	0.040053***	0.030755***	0.030754***	0.030735***	0.030733***	0.030578***	0.030578***
	(0.006297)	(0.006275)	(0.005595)	(0.005599)	(0.005592)	(0.005596)	(0.005620)	(0.005625)
$ActualBuy_{t(b)-1,t(b)-30,j}$	0.009970*	0.011014**	0.010613**	0.009938**	0.010731**	0.010017***	0.010615**	0.010017**
	(0.005371)	(0.004435)	(0.004265)	(0.003659)	(0.003935)	(0.003319)	(0.004317)	(0.003740)
$ActualSell_{t(b)-1,t(b)-30,j}$		-0.001532		0.000877		0.000946		0.000779
		(0.001920)		(0.001639)		(0.001708)		(0.001584)
Constant	0.047404***	0.047620***	0.039428***	0.039324***	0.039474***	0.039358***	0.038640***	0.038548***
	(0.007438)	(0.007482)	(0.008222)	(0.008269)	(0.008184)	(0.008245)	(0.008213)	(0.008262)
N	36572	36572	55269	55269	55269	55269	55176	55176
R^2	0.56	0.56	0.54	0.54	0.54	0.54	0.54	0.54

Table 9: **Bill Passage Predictability: Alternative Frameworks.**
This table tests the predictability of a bill passing using two alternative data frameworks. Columns (1) and (2) consider the bill-committee level, and Columns (3) and (4) consider the bill level. In our main analysis (Table 7), we focus on the bill-industry level, where the mapping from tickers to industries is considered more direct. However, one bill could go through multiple committees, and a single committee could be linked to multiple industries. We collapse the data into bill-committee- and bill-levels by aggregating the actual buys and sells. Standard errors for Columns (1) and (2) are double-clustered by committee and bill passage date, and those for Columns (3) and (4) are double-clustered by bill and bill passage date. ***, p-value <1%; **, <5%; *, <10%.

DV:	Bill Passing Likelihood			
Unit of observation:	Bill-Committee (b, c)		Bill (b)	
Industry FE:	-	-	-	-
Committee FE:	Yes	Yes	-	-
Day FE:	Yes	Yes	Yes	Yes
	(1)	(2)	(3)	(4)
$DecisionLength_b$	0.000674***	0.000674***	0.000696***	0.000696***
	(0.000142)	(0.000142)	(0.000126)	(0.000126)
$I_{SameParty,b}$	0.042252***	0.042227***	0.038535***	0.038536***
	(0.007434)	(0.007456)	(0.004881)	(0.004879)
$ActualBuy_{t(b)-1,t(b)-30,c}$ / $ActualBuy_{t(b)-1,t(b)-30}$	0.011977***	0.010292***	0.005486**	0.005513*
	(0.003383)	(0.002934)	(0.002358)	(0.002925)
$ActualSell_{t(b)-1,t(b)-30,c}$ / $ActualSell_{t(b)-1,t(b)-30}$		0.002478		-0.000031
		(0.001527)		(0.001479)
Constant	0.037109***	0.036332***	0.053537***	0.053546***
	(0.011176)	(0.011385)	(0.010395)	(0.010400)
N	24416	24416	17660	17660
R^2	0.49	0.49	0.48	0.48

Table 10: **Bill Passage Predictability Using the Economic Nature of a Bill.**
This table provides an important heterogeneity test for Table 7 given the expansionary or contractionary nature of a bill. See Figure 8 for the bill classification description. Standard errors are double-clustered by industry and bill passage date. ***, p-value <1%; **, <5%; *, <10%.

DV:	Bill Passing Likelihood					
Unit of observation:	Bill-Naics (b, j)					
Industry FE:	Yes	Yes	Yes	Yes	Yes	Yes
Day FE:	Yes	Yes	Yes	Yes	Yes	Yes
Bill Nature:	Expansionary	Neutral	Contractionary	Expansionary	Neutral	Contractionary
	(1)	(2)	(3)	(4)	(5)	(6)
$DecisionLength_b$	0.000735***	0.000788***	0.000116	0.000735***	0.000788***	0.000116
	(0.000153)	(0.000136)	(0.000108)	(0.000153)	(0.000136)	(0.000108)
$I_{SameParty,b}$	0.025894**	0.033012***	0.015274	0.025908**	0.033016***	0.015287
	(0.009801)	(0.006032)	(0.015291)	(0.009796)	(0.006037)	(0.015291)
$ActualBuy_{t(b)-1,t(b)-30,j}$	0.007827**	0.013283**	-0.001005	0.008510**	0.012647**	-0.001191**
	(0.003222)	(0.005745)	(0.000702)	(0.003034)	(0.005622)	(0.000525)
$ActualSell_{t(b)-1,t(b)-30,j}$				-0.000980	0.000749	0.000284
				(0.001980)	(0.002217)	(0.000719)
Constant	0.028511**	0.052568***	0.039657***	0.028628**	0.052482***	0.039608***
	(0.010429)	(0.008827)	(0.011724)	(0.010464)	(0.008840)	(0.011709)
N	19604	32166	3145	19604	32166	3145
R^2	0.61	0.59	0.88	0.61	0.59	0.88

Table 11: **Conditional Results.**
This table incorporates the fiscal expectations mechanism into the main stock response specification and compares two conditional cases, as illustrated in the diagram in Section 4.2: (i) Scenario 1, in which the bill passes before the trading disclosure, and (ii) Scenario 2, in which the bill passes after the trading disclosure. For the empirical design of Scenario 2, for example, we identify purchase disclosure events (involving only a single disclosed ticker) that have no bill passed within the 21 days preceding the disclosure date but at least one bill passed within the 21 days following it. As a result, each such disclosure case spans a 43-day window. The empirical design of Scenario 1 is defined similarly, with bill passage occurring during the first 21 days of the event windows. Standard errors are double-clustered by ticker and event date. ***, p-value <1%; **, <5%; *, <10%.

DV:	Daily stock returns					
Firm controls:	Yes	Yes	Yes	Yes	Yes	Yes
Macro controls:	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE:	Yes	Yes	Yes	Yes	Yes	Yes
Day FE:	Yes	Yes	Yes	Yes	Yes	Yes
Bill content matched with stock NAICS?	All	Yes	No	All	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Scenario 2: Purchase → Disclosure → Bill passing						
ReportBuy_1	0.006221***	0.007834***	0.004856**			
	(0.002115)	(0.002710)	(0.001857)			
ReportBuy				0.005071***	0.006337***	0.004091***
				(0.001561)	(0.002176)	(0.001370)
Constant	0.003710*	0.001079	0.006515***	0.003707**	0.001073	0.006516***
	(0.002012)	(0.000848)	(0.002311)	(0.001744)	(0.000844)	(0.002297)
N	15171	8049	7121	15171	8049	7121
R^2	0.16	0.14	0.19	0.16	0.14	0.19
Scenario 1: Purchase → Bill passing → Disclosure						
ReportBuy_1	-0.000308	0.002284	-0.004345*			
	(0.001923)	(0.001679)	(0.002599)			
ReportBuy				-0.000474	0.002272**	-0.003660**
				(0.001434)	(0.001124)	(0.001497)
Constant	0.004948***	-0.000906	0.010175***	0.004964***	-0.000933	0.010164***
	(0.001340)	(0.000923)	(0.001780)	(0.001307)	(0.000923)	(0.001721)
N	19436	9876	9560	19436	9876	9560
R^2	0.17	0.074	0.25	0.17	0.074	0.25
Coefficient equality test between the two scenarios (p-value)	1.12%	4.08%	0.20%	0.45%	4.84%	0.01%
H0: Both coefficients are equal.	Reject	Reject	Reject	Reject	Reject	Reject

Table 12: **Conditional Results: Robustness and Heterogeneity.**
This table shows additional robustness and heterogeneity tests for Table 11. To conserve space, we only report the main estimated coefficients of interest and delegate full regression tables to Appendix Table IA.1 to IA.7. ***, p-value <1%; **, <5%; *, <10%.

DV:	Daily stock returns					
Firm controls:	Yes	Yes	Yes	Yes	Yes	Yes
Macro controls:	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE:	Yes	Yes	Yes	Yes	Yes	Yes
Day FE:	Yes	Yes	Yes	Yes	Yes	Yes
Bill content matched with stock NAICS?	All	Yes	No	All	Yes	No
Regressor:		**ReportBuy$_1$**			**ReportBuy**	
Scenario 2: Purchase → Disclosure → Bill passing						
Robustness: Pre-2020	0.003636**	0.004076*	0.002944	0.003486**	0.003826**	0.003161
	(0.001788)	(0.002142)	(0.002546)	(0.001573)	(0.001786)	(0.002401)
Robustness: Drop Majority Leader	0.006251***	0.007870***	0.004879**	0.005086***	0.006356***	0.004102***
	(0.002123)	(0.002718)	(0.001863)	(0.001564)	(0.002179)	(0.001373)
Robustness: Control for ret(1)	0.006031***	0.007670***	0.004772**	0.004934***	0.006210***	0.004056***
	(0.002238)	(0.002721)	(0.001971)	(0.001589)	(0.002183)	(0.001449)
Robustness: Control for Report Sell	0.005945***	0.007775***	0.004284**	0.004762***	0.006183***	0.003562**
	(0.002047)	(0.002696)	(0.001731)	(0.001557)	(0.002181)	(0.001348)
Heterogeneity: Committee	0.007141**	0.008150*	0.004504	0.005903**	0.007190*	0.004141
	(0.002908)	(0.004460)	(0.003552)	(0.002591)	(0.003726)	(0.003181)
Heterogeneity: Non-Passive	0.006920***	0.008778***	0.005231**	0.005474***	0.007008***	0.004224***
	(0.002071)	(0.002815)	(0.001906)	(0.001595)	(0.002252)	(0.001406)
Heterogeneity: High Fiscal Dependence	0.008351***	0.008861***	0.007740***	0.006353***	0.007363***	0.005383***
	(0.002268)	(0.002871)	(0.002432)	(0.001720)	(0.002256)	(0.001906)
Scenario 1: Purchase → Bill passing → Disclosure						
Robustness: pre-2020	-0.000241	0.001057	-0.002689	0.000596	0.001677*	-0.001846
	(0.001098)	(0.001130)	(0.002166)	(0.000859)	(0.000966)	(0.001472)
Robustness: Drop Majority Leader	-0.000390	0.002189	-0.004400*	-0.000529	0.002211*	-0.003688**
	(0.001935)	(0.001713)	(0.002594)	(0.001438)	(0.001144)	(0.001493)
Robustness: Control for ret(1)	-0.000306	0.002306	-0.004313	-0.000431	0.002283**	-0.003546**
	(0.001977)	(0.001703)	(0.002679)	(0.001433)	(0.001135)	(0.001511)
Robustness: Control for Report Sell	-0.000593	0.002126	-0.004685*	-0.000600	0.002062*	-0.003799**
	(0.001958)	(0.001687)	(0.002667)	(0.001375)	(0.001141)	(0.001619)
Heterogeneity: Committee	-0.001695	-0.000699	-0.003933	0.000463	0.001866	-0.001526
	(0.002327)	(0.002961)	(0.002628)	(0.001981)	(0.002800)	(0.002246)
Heterogeneity: Non-Passive	0.001458	0.003072*	-0.001542	0.000400	0.002702**	-0.002404
	(0.001558)	(0.001675)	(0.001948)	(0.001454)	(0.001149)	(0.001639)
Heterogeneity: High Fiscal Dependence	-0.000086	0.002117	-0.003882	-0.000559	0.002315*	-0.004101**
	(0.002132)	(0.001913)	(0.002852)	(0.001665)	(0.001184)	(0.001567)

Table 13: **Evidence of Real Effects: Congressional Trading and Firm Procurement.**
This table uses events from Scenario 2 to further test whether firms in industries linked to the bill (i.e., through the affiliated committee) receive more procurement contracts subsequently than firms in industries not linked to the bill. In Panel A (B), we consider N months after the bill passes (disclosure). In each panel, we consider 3-, 6-, 9-, and 12-month windows when calculating cumulative procurement obligation earnings. Bill_NAICS_match equals one if firms are linked to the bill and zero otherwise. Standard errors are double-clustered by ticker and event date. ***, p-value <1%; **, <5%; *, <10%.

Panel A: Procurement after Disclosure.				
DV:	Procurement after Disclosure			
Industry FE:	Yes	Yes	Yes	Yes
Date FE:	Yes	Yes	Yes	Yes
Horizon:	3-month	6-month	9-month	12-month
Bill_NAICS_match	3.032946**	4.605702***	6.407495***	8.164856***
	(1.159611)	(0.884036)	(1.935673)	(2.176225)
Constant	0.813533	2.076065*	2.967956*	4.193874*
	(0.667380)	(1.107706)	(1.619870)	(2.234099)
N	330	332	332	333
R^2	0.096	0.10	0.10	0.11

Panel B: Procurement after Bill-Passing.				
DV:	Procurement after Bill-Passing			
Industry FE:	Yes	Yes	Yes	Yes
Date FE:	Yes	Yes	Yes	Yes
Horizon:	3-month	6-month	9-month	12-month
Bill_NAICS_match	2.977968**	4.959120***	6.200368***	8.328133***
	(1.164885)	(1.753516)	(1.536135)	(2.464168)
Constant	0.847404	1.819425*	3.122431*	4.106705*
	(0.646607)	(1.064591)	(1.524237)	(2.042843)
N	329	327	328	330
R^2	0.10	0.11	0.10	0.11

Paper Appendix

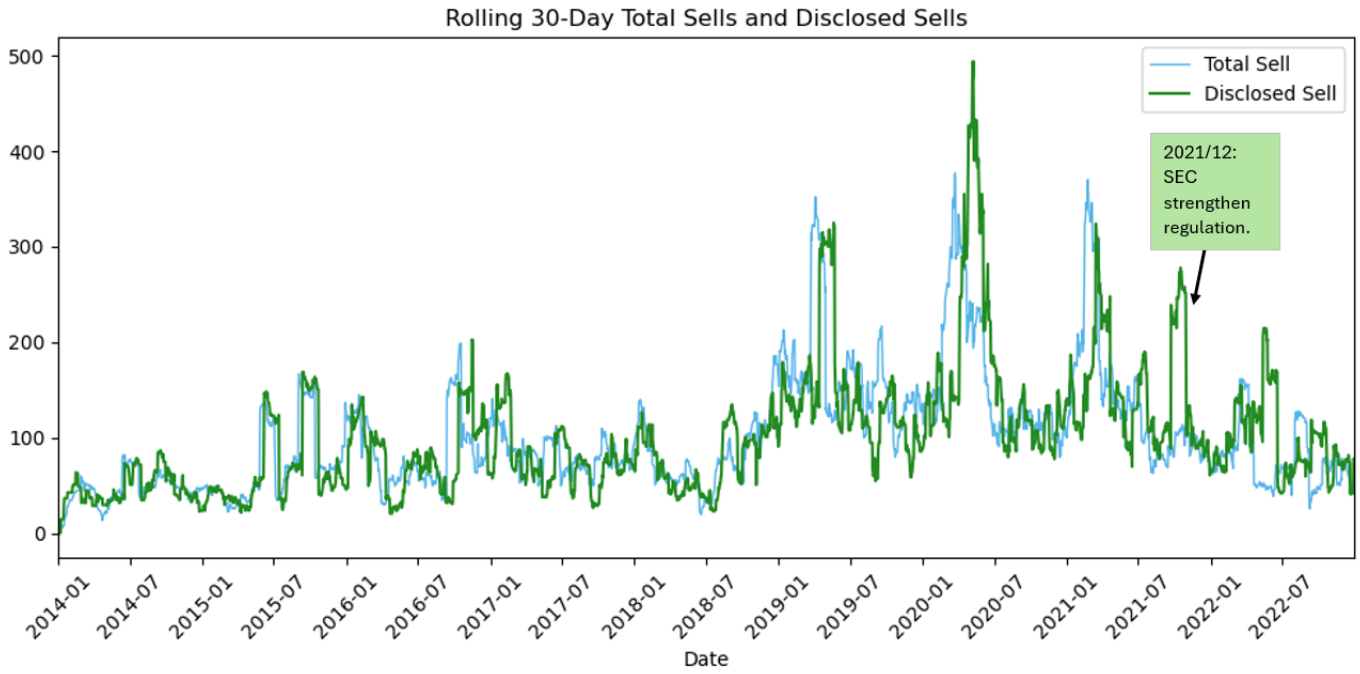


Figure A1: **Time Variation in the Total Actual and Disclosed Stock Sells from All House Representatives in a 30-day Rolling Window.** Figure 2 is the purchase counterpart.

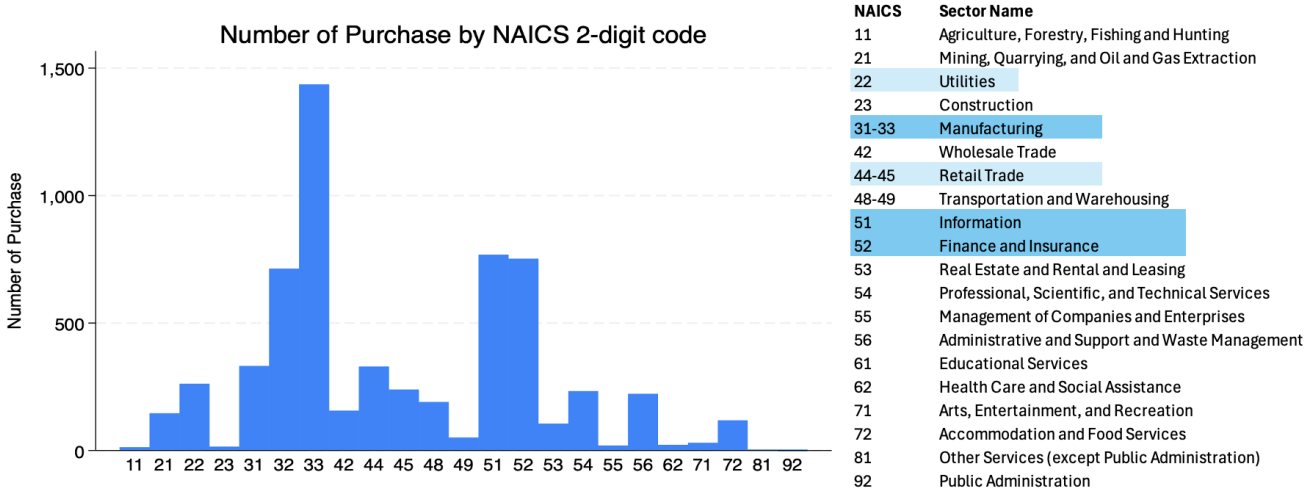


Figure A2: **Stock Purchases across Industries.**
This bar chart displays the total number of stock purchases aggregated by two-digit NAICS industry codes across all disclosure files in our sample period. For ease of interpretation, the corresponding industry names are shown on the right. Figure 4 is the total trade counterpart.

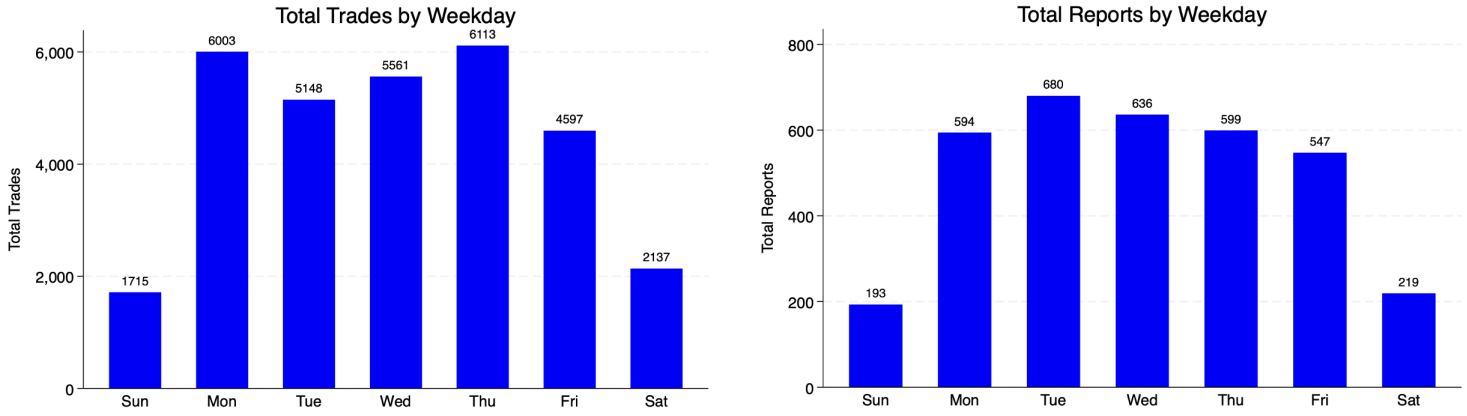
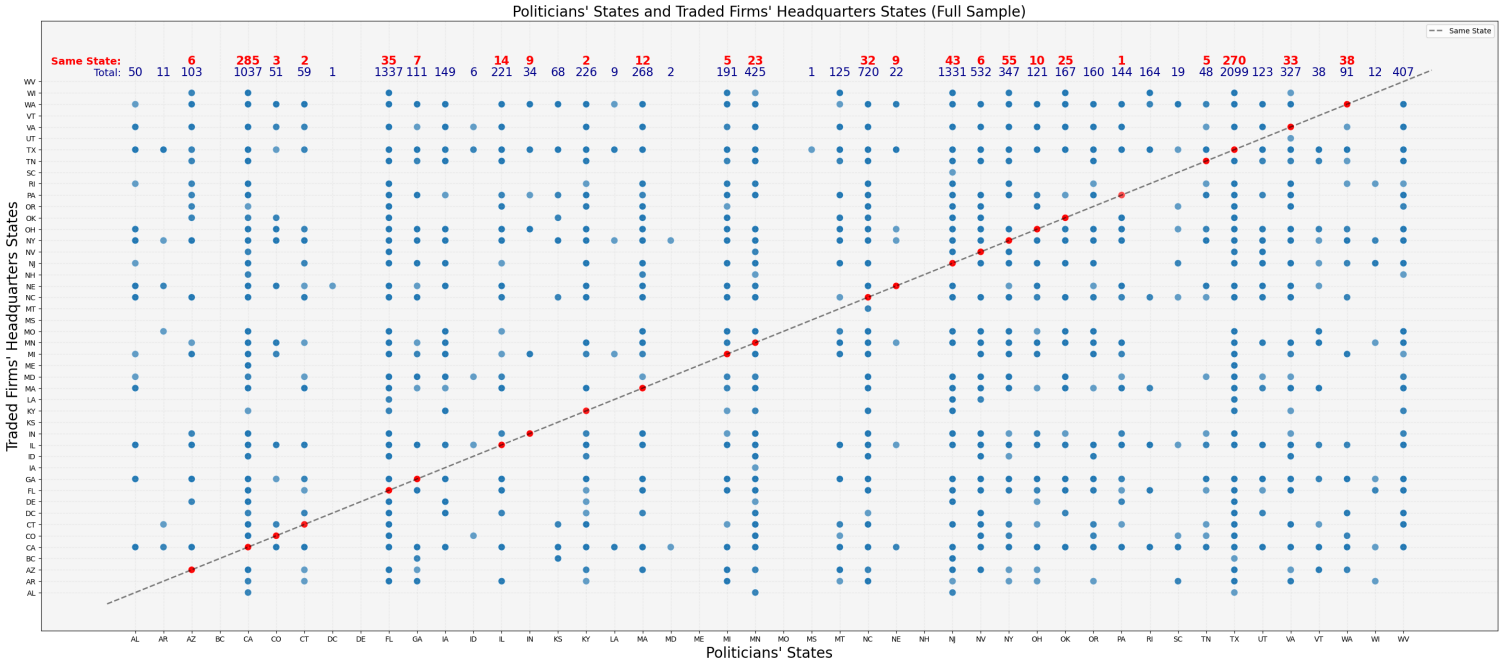
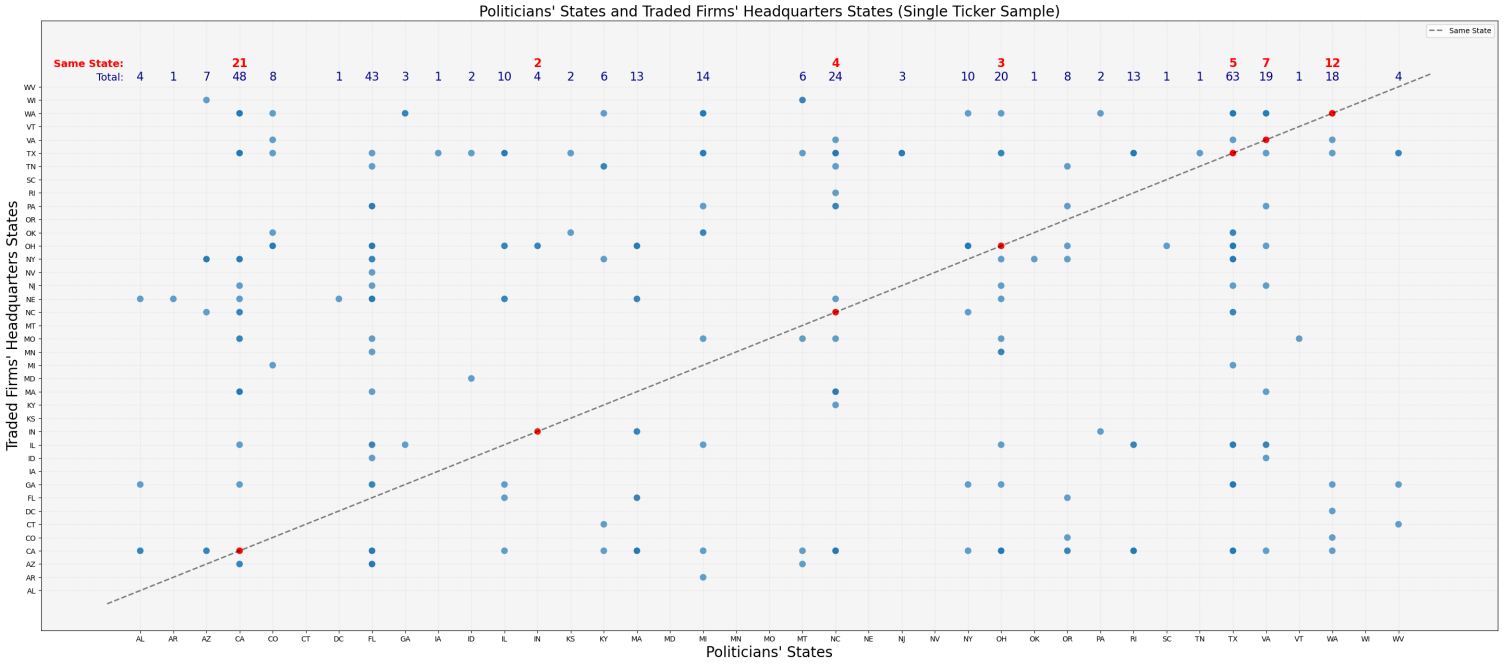


Figure A3: **Frequencies of Trades and Disclosure Reports by Weekdays.**
These two figures illustrate the frequency of stock trading disclosures across weekdays. The left figure presents this frequency at the ticker level, and the right figure presents this frequency at the disclosure file level.



(a) Full Sample



(b) Single Ticker Sample

Figure A4: **State Distribution of Corporate Headquarters and Politicians' Legislative Districts.**
This figure displays the relationship between the headquarters of firms whose stocks are traded by politicians and the legislative districts of these politicians. The pattern in this figure indicates that politicians' investment choices are not driven by a desire to win the favor or goodwill of local voters.

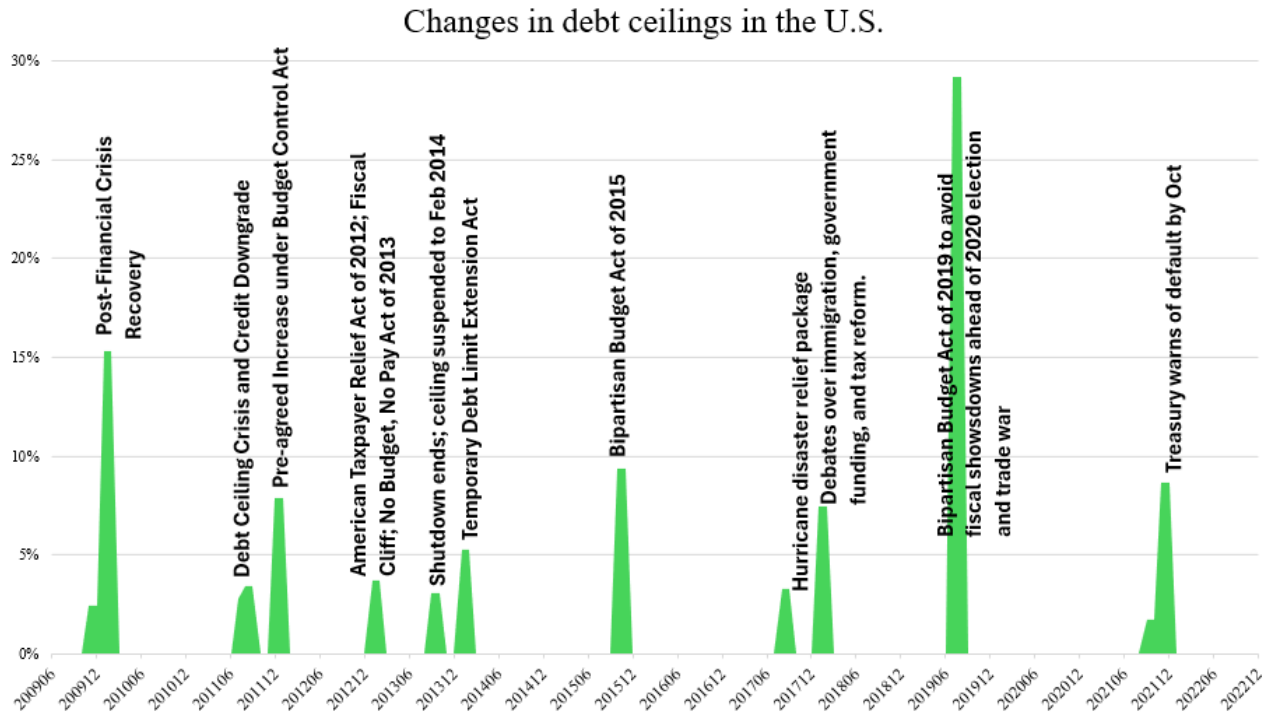


Figure A5: **Debt Limit Change**.
The figure highlights the month of and the month preceding each official change in the U.S. federal debt ceiling from 2008 to 2023.

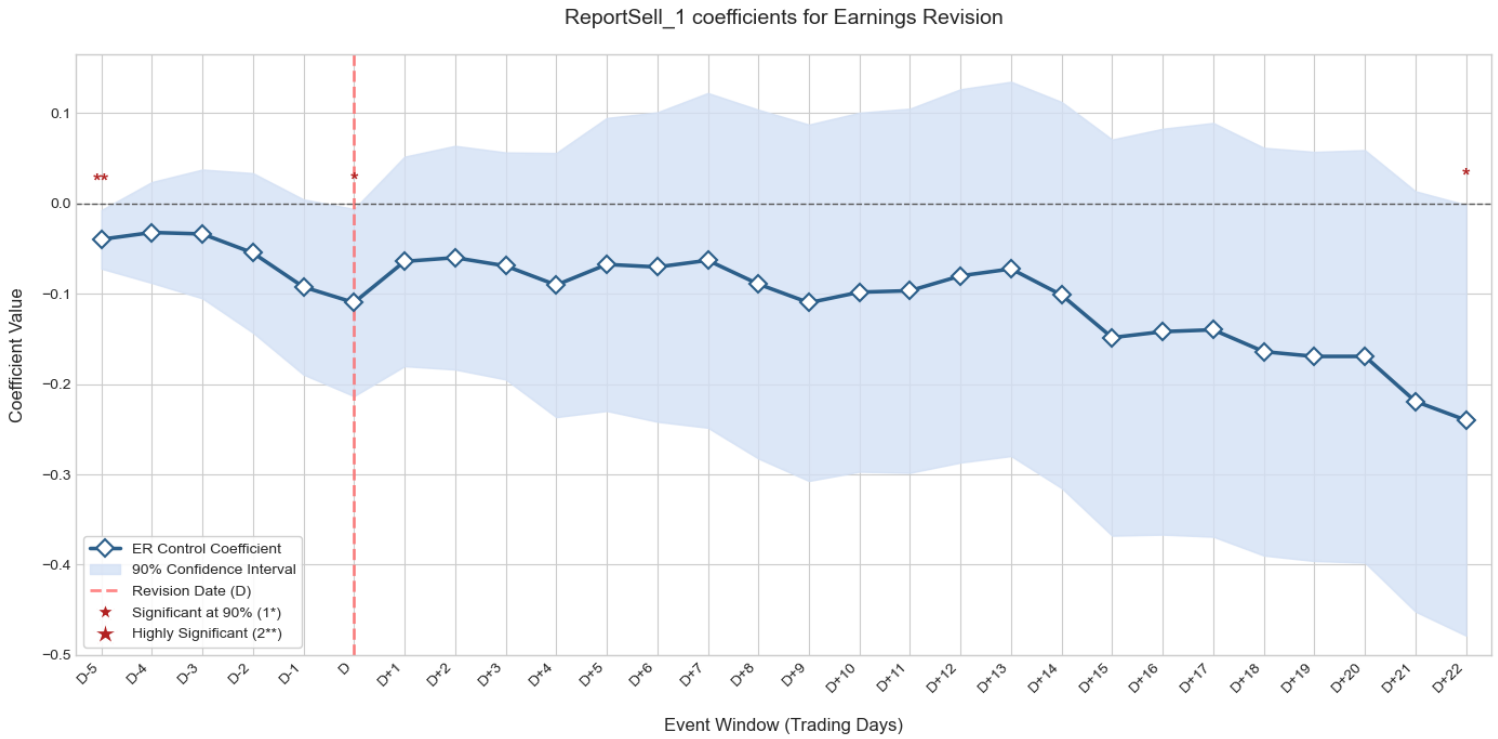


Figure A6: **Coefficients of Cumulative I/B/E/S Forecast Revisions on a Report Sell Shock.** The corresponding plot for a Report Buy shock is in Figure 7.

Table A1: **Summary Statistics of Variables Included in Table 2.**
This table presents summary statistics for the variables used in the regression analyses reported in Table 2.

Variable	count	mean	sd	min	max	p5	p25	p50	p75	p95
ret	1101558	0.00	0.02	-0.61	2.45	-0.03	-0.01	0.00	0.01	0.03
reportbuy	1101694	0.01	0.09	0.00	8.00	0.00	0.00	0.00	0.00	0.00
reportbuy_1	1101694	0.00	0.07	0.00	1.00	0.00	0.00	0.00	0.00	0.00
SUE5	1101645	-0.00	0.01	-6.18	1.54	0.00	0.00	0.00	0.00	0.00
SUR1	1101118	0.00	0.13	-2.47	2.47	0.00	0.00	0.00	0.00	0.00
illiq_2_5	1095864	-0.00	0.00	-0.00	0.00	0.00	0.00	0.00	0.00	0.00
volatility_2_5	1101211	0.04	0.03	0.00	2.47	0.01	0.02	0.03	0.05	0.09
ADP CHNG Index	1101694	0.00	0.23	-1.64	9.90	0.00	0.00	0.00	0.00	0.00
CHPMINDX Index	1101694	0.01	0.22	-2.48	2.12	0.00	0.00	0.00	0.00	0.00
CNSTTMOM Index	1101694	-0.01	0.22	-2.96	4.20	0.00	0.00	0.00	0.00	0.00
CONCCONF Index	1101694	0.01	0.21	-1.88	2.53	0.00	0.00	0.00	0.00	0.00
CONSSENT Index	1101694	-0.01	0.33	-4.30	2.62	-0.08	0.00	0.00	0.00	0.08
CPI CHNG Index	1101694	0.01	0.21	-1.52	4.56	0.00	0.00	0.00	0.00	0.00
CPI XYOY Index	1101694	0.01	0.22	-2.03	4.74	0.00	0.00	0.00	0.00	0.00
CPI YOY Index	1101694	0.01	0.22	-1.48	4.44	0.00	0.00	0.00	0.00	0.00
CPUPXCHG Index	1101694	0.00	0.22	-1.44	4.31	0.00	0.00	0.00	0.00	0.00
DGNOCHNG Index	1101694	0.01	0.27	-2.06	7.73	0.00	0.00	0.00	0.00	0.00
DGNOXTCH Index	1101694	0.00	0.23	-1.47	7.99	0.00	0.00	0.00	0.00	0.00
ECI SA% Index	1101694	0.00	0.12	-2.55	2.55	0.00	0.00	0.00	0.00	0.00
EMPRGBCI Index	1101694	-0.00	0.19	-3.19	2.58	0.00	0.00	0.00	0.00	0.00
ETSLTOTL Index	1101694	0.00	0.23	-3.75	2.86	0.00	0.00	0.00	0.00	0.00
FDDSSD Index	1101694	0.01	0.21	-2.27	5.14	0.00	0.00	0.00	0.00	0.00
FDIDFDMO Index	1101694	-0.00	0.22	-2.79	3.14	0.00	0.00	0.00	0.00	0.00
FDFDFDCH Index	1101694	0.00	0.22	-2.31	3.86	0.00	0.00	0.00	0.00	0.00
GDPC1QOQ Index	1101694	0.00	0.21	-2.60	2.60	0.00	0.00	0.00	0.00	0.00
HOUST Index	1101694	0.00	0.22	-2.19	3.29	0.00	0.00	0.00	0.00	0.00
INJCJC Index	1101694	-0.00	0.07	-1.39	1.51	0.00	0.00	0.00	0.00	0.00
IP CHNG Index	1101694	0.00	0.18	-4.50	2.06	0.00	0.00	0.00	0.00	0.00
IP MFGCH Index	1101694	0.01	0.16	-3.33	2.22	0.00	0.00	0.00	0.00	0.00
JOLTTOTL Index	1101694	-0.00	0.21	-4.04	2.42	0.00	0.00	0.00	0.00	0.00
MICH Index	1101694	0.01	0.16	-2.20	3.04	0.00	0.00	0.00	0.00	0.00
MBAINDX Index	1101694	0.00	0.23	-2.05	3.81	0.00	0.00	0.00	0.00	0.00
MBAREFI Index	1101694	0.00	0.21	-2.37	2.37	0.00	0.00	0.00	0.00	0.00
MNFCTRNG Index	1101694	0.00	0.11	-1.29	2.91	-0.06	0.00	0.00	0.00	0.05
MTSLCHNG Index	1101694	-0.01	0.45	-13.46	8.28	-0.08	0.00	0.00	0.00	0.07
NAPM Index	1101694	-0.00	0.19	-3.05	2.10	0.00	0.00	0.00	0.00	0.00
NAPMEMPL Index	1101694	0.00	0.21	-2.30	4.61	0.00	0.00	0.00	0.00	0.00
NAPMNEWO Index	1101694	0.00	0.05	-1.30	1.78	0.00	0.00	0.00	0.00	0.00
NAPMPRIC Index	1101694	-0.00	0.06	-1.80	1.13	0.00	0.00	0.00	0.00	0.00
NAPMSUPL Index	1101694	0.00	0.07	-1.46	1.78	0.00	0.00	0.00	0.00	0.00
NAPMTERM Index	1101694	0.02	0.28	-3.90	2.93	0.00	0.00	0.00	0.00	0.00
NAPMUDMN Index	1101694	0.01	0.21	-2.18	3.01	0.00	0.00	0.00	0.00	0.00
NAPMUNPR Index	1101694	0.00	0.22	-2.70	3.16	0.00	0.00	0.00	0.00	0.00
NAPMUPNT Index	1101694	0.00	0.22	-3.17	2.57	0.00	0.00	0.00	0.00	0.00
NFP TCH Index	1101694	0.01	0.24	-0.74	10.07	0.00	0.00	0.00	0.00	0.00
NHSLTOT Index	1101694	0.01	0.22	-2.64	3.11	0.00	0.00	0.00	0.00	0.00
NHSPSTOT Index	1101694	-0.00	0.20	-2.15	3.02	0.00	0.00	0.00	0.00	0.00
OUTFGAF Index	1101694	0.01	0.20	-1.69	4.56	0.00	0.00	0.00	0.00	0.00
PCE CRCH Index	1101694	-0.00	0.16	-3.46	1.57	0.00	0.00	0.00	0.00	0.00
PITLCHNG Index	1101694	0.01	0.24	-0.50	10.20	0.00	0.00	0.00	0.00	0.00
RCHSINDX Index	1101694	-0.00	0.21	-2.57	2.46	0.00	0.00	0.00	0.00	0.00
RSTAMOM Index	1101694	0.00	0.23	-3.41	7.21	0.00	0.00	0.00	0.00	0.00
TMNOCHNG Index	1101694	0.00	0.22	-2.93	2.93	0.00	0.00	0.00	0.00	0.00
USCABAL Index	1101694	0.00	0.10	-2.09	2.12	0.00	0.00	0.00	0.00	0.00
USMMMNCH Index	1101694	0.01	0.24	-1.76	8.97	0.00	0.00	0.00	0.00	0.00
USPHTMOM Index	1101694	-0.00	0.21	-1.93	6.35	0.00	0.00	0.00	0.00	0.00
USTBTOT Index	1101694	-0.00	0.23	-4.19	2.55	0.00	0.00	0.00	0.00	0.00
USURTOT Index	1101694	-0.01	0.24	-9.39	0.49	0.00	0.00	0.00	0.00	0.00

Table A2: **Actual Transaction Day.**
This table complements Table 2 and shows regression results with Buy_1 and Buy as the independent variables. In this specification, Buy_1 equals one if ticker i is purchased on day t. Buy equals the number of purchase transactions for ticker i on day t. Macro-level control variables include announcement surprises from Bloomberg's Macroeconomic Announcements dataset. To conserve space, the estimated coefficients of constants and control variables are omitted; full estimation results are available upon request. Double-clustered standard errors at the ticker and disclosure date level are reported in parentheses. ***, p-value <1%; **, <5%; *, <10%

DV:	Daily stock returns	
Double cluster:	X	X
Firm controls:	X	X
Macro controls:	X	X
Stock FE:	X	X
ym FE:		X
Buy_1	-0.000514	-0.000569
	(0.000662)	(0.000557)
Constant	-0.000002	0.000039
	(0.000279)	(0.000293)
Observations	1094917	1094917
R-squared	0.013	0.024
Buy	-0.000356	-0.000367
	(0.000534)	(0.000473)
Constant	-0.000002	0.000038
	(0.000279)	(0.000293)
Observations	1094917	1094917
R-squared	0.013	0.024

Table A3: **Summary Statistics of Variables Included in Table 3.**
This table presents summary statistics for the variables used in the regression analyses reported in Table 3.

Variable	count	mean	sd	min	max	p5	p25	p50	p75	p95
Single-Ticker level										
ret	236483	0.00	0.02	-0.39	0.85	-0.03	-0.01	0.00	0.01	0.03
reportbuy_1	236501	0.00	0.03	0.00	1.00	0.00	0.00	0.00	0.00	0.00
reportbuy	236501	0.00	0.03	0.00	5.00	0.00	0.00	0.00	0.00	0.00
SUE5	236495	-0.00	0.01	-6.18	0.59	0.00	0.00	0.00	0.00	0.00
SUR1	236401	0.00	0.13	-2.47	2.47	0.00	0.00	0.00	0.00	0.00
illiq_r_5	231485	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
volatility_m_5	235687	0.04	0.03	0.00	0.97	0.01	0.02	0.03	0.05	0.09
sprtrn	236501	0.00	0.01	-0.04	0.05	-0.02	-0.00	0.00	0.01	0.01

Table A4: **Stock Return Responses to Congressional Trade Disclosures: Single-Ticker Alternative Active Sample with Firm Attention Measured by Analyst Coverage.** This table complements Table 3 and focuses on the subsample $\{i, t\}$ consisting of (a) days on which only one ticker has a disclosed purchase record on that trading day and (b) firms with above-median investor attention as measured by analyst coverage (also see Figure 5). We refer to this subsample as the "Single-Ticker Alternative Active" sample in the paper. The remaining stocks are examined in the Placebo sample. This table uses either day fixed effects (Panel A) or daily market returns (Panel B), and as a result, we do not include macro controls. See Table 2 for a detailed list of firm control variables. To conserve space, the estimated coefficients for the constant and control variables are omitted; full estimation results are available upon request. Standard errors clustered at the ticker level are reported in parentheses, which is appropriate in this analysis given that the subsample contains only one ticker with purchase disclosures on any given day (i.e., no cross-ticker correlations to account for). ***, p-value <1%; **, <5%; *, <10%.

Panel A: With day fixed effects.				
DV:	Daily stock returns			
Firm controls:	Yes	Yes	Yes	Yes
Stock FE:	Yes	Yes	Yes	Yes
Day FE:	Yes	Yes	Yes	Yes
Daily S&P market returns as controls:	-	-	-	-
Stock subsample:	Active Stocks		Placebo Stocks	
	(1)	(2)	(3)	(4)
ReportBuy_1	0.003247**		-0.001043	
	(0.001271)		(0.001434)	
ReportBuy		0.002100**		-0.001339
		(0.001063)		(0.001400)
N	357935	357935	349150	349150
R^2	0.27	0.27	0.25	0.25

Panel B: Without day fixed effects, but with daily market returns.				
DV:	Daily stock returns			
Firm controls:	Yes	Yes	Yes	Yes
Stock FE:	Yes	Yes	Yes	Yes
Day FE:	No	No	No	No
Daily S&P market returns as controls:	Yes	Yes	Yes	Yes
Stock subsample:	Active Stocks		Placebo Stocks	
	(1)	(2)	(3)	(4)
ReportBuy_1	0.003590**		-0.001164	
	(0.001510)		(0.001452)	
ReportBuy		0.002548**		-0.001229
		(0.001123)		(0.001466)
N	357938	357938	349153	349153
R^2	0.25	0.25	0.22	0.22

Table A5: **House Committee and NAICS Code Descriptions.**
This table presents the mapping between U.S. House of Representatives committees and their corresponding two-digit NAICS industries.

House Committee	NAICS Code Description
Agriculture	11 – Agriculture, Forestry, Fishing and Hunting
Appropriations	Not applicable (Covers all sectors through government funding)
Armed Services	33 – Manufacturing
	54 – Professional, Scientific, and Technical Services
Budget	Not applicable (Budget oversight across all sectors)
Education and the Workforce	61 – Educational Services
	71 – Arts, Entertainment, and Recreation
	62 – Health Care and Social Assistance
Energy and Commerce	22 – Utilities
	31–33 – Manufacturing
	42 – Wholesale Trade
	21 – Mining, Quarrying, Oil and Gas Extraction
Ethics	Not applicable (Deals with government ethics and standards)
Financial Services	52 – Finance and Insurance
	53 – Real Estate Rental and Leasing
Foreign Affairs	92 – Public Administration
	54 – Professional, Scientific, and Technical Services
Homeland Security	56 – Administrative and Support; Waste Management and Remediation Services
House Administration	Not applicable (Deals with internal House operations)
Judiciary	92 – Public Administration
	54 – Professional, Scientific, and Technical Services
Natural Resources	21 – Mining, Quarrying, Oil and Gas Extraction
Oversight and Accountability	55 – Management of Companies and Enterprises
Rules	Not applicable (Legislative rule-setting)
Science, Space, and Technology	54 – Professional, Scientific, and Technical Services
Small Business	44–45 – Retail Trade
	72 – Accommodation and Food Services
	81 – Other Services (except Public Administration)
	42 – Wholesale Trade
	54 – Professional, Scientific, and Technical Services
Transportation and Infrastructure	48–49 – Transportation and Warehousing
	23 – Construction
Veterans' Affairs	62 – Health Care and Social Assistance
Ways and Means	52 – Finance and Insurance

Table A6: **Summary Statistics of Variables Included in Table 7.**
This table presents summary statistics for the variables used in the regression analyses reported in Table 7.

Variable	count	mean	sd	min	max	p5	p25	p50	p75	p95
pass	75797	0.08	0.27	0.00	1.00	0.00	0.00	0.00	0.00	1.00
$DecisionLength_b$	75797	40.60	99.96	-1.00	723.00	-1.00	-1.00	1.00	26.00	257.00
$I_{SameParty,b}$	75797	0.61	0.49	0.00	1.00	0.00	0.00	1.00	1.00	1.00
$ActualBuy_{t(b)-1,t(b)-21,j}$	75797	0.12	0.63	0.00	19.00	0.00	0.00	0.00	0.00	1.00
$ActualBuy_{t(b)-1,t(b)-25,j}$	75797	0.14	0.69	0.00	21.00	0.00	0.00	0.00	0.00	1.00
$ActualBuy_{t(b)-1,t(b)-30,j}$	75797	0.15	0.77	0.00	24.00	0.00	0.00	0.00	0.00	1.00
$ActualBuy_{t(b)-1,t(b)-35,j}$	75797	0.19	1.17	0.00	30.00	0.00	0.00	0.00	0.00	1.00
$ActualBuy_{t(b)-1,t(b)-40,j}$	75797	0.20	1.28	0.00	33.00	0.00	0.00	0.00	0.00	1.00
$ActualSell_{t(b)-1,t(b)-21,j}$	75797	0.17	1.01	0.00	33.00	0.00	0.00	0.00	0.00	1.00
$ActualSell_{t(b)-1,t(b)-30,j}$	75797	0.20	1.14	0.00	35.00	0.00	0.00	0.00	0.00	1.00

Table A7: **Robustness Tests for Table 7: Alternative Horizons.**
This table provides robustness tests for Table 7 by changing the horizon of $ActualBuy_{t(b)-1,t(b)-n,j}$. Specifically, we consider alternative horizons where $n = 25, 30, 35$, and 40. All other specifications remain identical to those in Table 7. Standard errors are double-clustered by industry and bill passage date. ***, p-value <1%; **, <5%; *, <10%

DV:	$BillPass_{b,j}$			
Double cluster:	X	X	X	X
Industry FE:	X	X	X	X
Day FE:	X	X	X	X
History:	[-25,-1]	[-30,-1]	[-35,-1]	[-40,-1]
$DecisionLength_b$	0.000790***	0.000790***	0.000790***	0.000790***
	(0.000128)	(0.000128)	(0.000128)	(0.000128)
$I_{SameParty,b}$	0.030785***	0.030754***	0.030714***	0.030701***
	(0.005588)	(0.005595)	(0.005582)	(0.005580)
$ActualBuy_{t(b)-1,t(b)-n,j}$	0.009674**	0.010697**	0.006488	0.006117
	(0.004604)	(0.004269)	(0.004323)	(0.004370)
Constant	0.039746***	0.039382***	0.039965***	0.039966***
	(0.008239)	(0.008223)	(0.008319)	(0.008339)
N	55269	55269	55269	55269
R^2	0.54	0.54	0.54	0.54

Table A8: **Robustness Checks for Table 7: Alternative Specifications and Horizon Choice with n = 21.** This table provides robustness tests for Table 7 in two channels. In the first channel (Columns (1)–(4)), the regressions include only $DecisionLength_b$ and $I_{SameParty,b}$ as independent variables, while varying the fixed-effect specifications. The second specification (Columns (5)–(6)) presents robustness test results in which the horizon of the independent variable $ActualBuy_{t(b)-1,t(b)-n,j}$ is set to $n = 21$. All other specifications remain identical to those in Table 7. Standard errors are double-clustered by industry and bill passage date.

DV:			$BillPass_{b,j}$			
Double cluster:	X	X	X	X	X	X
Industry FE:		X		X	X	X
Day FE:			X	X	X	X
	(1)	(2)	(3)	(4)	(5)	(6)
$DecisionLength_b$	0.000851***	0.000854***	0.000792***	0.000791***	0.000791***	0.000791***
	(0.000228)	(0.000220)	(0.000133)	(0.000129)	(0.000128)	(0.000128)
$I_{SameParty,b}$	0.057552***	0.054505***	0.030846***	0.030799***	0.030819***	0.030818***
	(0.009314)	(0.008318)	(0.005923)	(0.005568)	(0.005593)	(0.005594)
$ActualBuy_{t(b)-1,t(b)-21,j}$					0.009473*	0.009191**
					(0.004796)	(0.004115)
$ActualSell_{t(b)-1,t(b)-21,j}$						0.000448
						(0.001641)
Constant	0.021555***	0.023283***	0.041432***	0.041497***	0.039877***	0.039821***
	(0.007284)	(0.006601)	(0.008304)	(0.008071)	(0.008253)	(0.008297)
N	55394	55394	55269	55269	55269	55269
R^2	0.11	0.14	0.52	0.53	0.54	0.54

Table A9: **Summary Statistics of Variables Included in Table 11, Scenario 2.** This table presents summary statistics for the variables used in the Scenario 2 regressions reported in Table 11.

Variable	count	mean	sd	min	max	p5	p25	p50	p75	p95
ret	15229	0.00	0.03	-0.30	0.22	-0.04	-0.01	0.00	0.01	0.04
reportbuy	15230	0.05	0.27	0.00	7.00	0.00	0.00	0.00	0.00	0.00
reportbuy_1	15230	0.05	0.21	0.00	1.00	0.00	0.00	0.00	0.00	0.00
sue5	15229	0.00	0.00	-0.16	0.10	0.00	0.00	0.00	0.00	0.00
sur1	15225	0.00	0.18	-2.45	2.41	0.00	0.00	0.00	0.00	0.00
illiq_2_5	15177	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
volatility_2_5	15230	0.05	0.04	0.00	0.31	0.01	0.02	0.04	0.06	0.12
ADP CHNG Index	15230	0.00	0.06	-1.64	0.63	0.00	0.00	0.00	0.00	0.00
CHPMINDX Index	15230	0.00	0.16	-2.00	1.61	0.00	0.00	0.00	0.00	0.00
CNSTTMOM Index	15230	0.03	0.43	-1.62	4.20	0.00	0.00	0.00	0.00	0.00
CONCCONF Index	15230	0.01	0.17	-1.56	2.53	0.00	0.00	0.00	0.00	0.00
CONSSENT Index	15230	0.01	0.36	-2.26	2.23	0.00	0.00	0.00	0.00	0.12
CPI CHNG Index	15230	0.00	0.15	-1.52	2.28	0.00	0.00	0.00	0.00	0.00
CPI XYOY Index	15230	-0.02	0.25	-2.03	3.39	0.00	0.00	0.00	0.00	0.00
CPI YOY Index	15230	-0.01	0.16	-1.48	2.22	0.00	0.00	0.00	0.00	0.00
CPUPXCHG Index	15230	-0.02	0.22	-1.44	2.88	0.00	0.00	0.00	0.00	0.00
DGNOCHNG Index	15230	0.00	0.37	-3.23	7.73	0.00	0.00	0.00	0.00	0.00
DGNOXTCH Index	15230	0.09	0.83	-2.21	7.99	0.00	0.00	0.00	0.00	0.00
ECI SA% Index	15230	0.01	0.15	-1.27	1.27	0.00	0.00	0.00	0.00	0.00
EMPRGBCI Index	15230	-0.01	0.31	-3.79	1.33	0.00	0.00	0.00	0.00	0.00
ETSLTOTL Index	15230	0.01	0.18	-1.72	2.42	0.00	0.00	0.00	0.00	0.00
FDDSSD Index	15230	0.00	0.12	-0.71	5.14	0.00	0.00	0.00	0.00	0.00
FDIDFDMO Index	15230	-0.03	0.30	-2.79	1.74	0.00	0.00	0.00	0.00	0.00
FDFDFDCH Index	15230	-0.01	0.17	-1.93	1.93	0.00	0.00	0.00	0.00	0.00
GDPC1QOQ Index	15230	-0.03	0.28	-2.60	1.30	0.00	0.00	0.00	0.00	0.00
HOUST Index	15230	-0.01	0.19	-1.83	1.46	0.00	0.00	0.00	0.00	0.00
INJCJC Index	15230	0.00	0.07	-0.97	1.51	0.00	0.00	0.00	0.00	0.00
IP CHNG Index	15230	0.00	0.22	-2.44	3.00	0.00	0.00	0.00	0.00	0.00
IP MFGCH Index	15230	0.00	0.26	-4.72	4.17	0.00	0.00	0.00	0.00	0.00
JOLTTOTL Index	15230	-0.01	0.27	-4.04	2.42	0.00	0.00	0.00	0.00	0.00
MICH Index	15230	-0.04	0.67	-6.76	3.04	0.00	0.00	0.00	0.00	0.00
MBAINDX Index	15230	0.02	0.31	-2.05	3.81	0.00	0.00	0.00	0.00	0.00
MBAREFI Index	15230	0.04	0.27	-1.18	2.66	0.00	0.00	0.00	0.00	0.00
MNFCTRNG Index	15230	0.07	0.63	-1.54	17.46	-0.05	0.00	0.00	0.00	0.14
MTSLCHNG Index	15230	-0.14	1.50	-13.46	8.28	-0.51	0.00	0.00	0.00	0.07
NAPM Index	15230	-0.01	0.27	-2.67	2.10	0.00	0.00	0.00	0.00	0.00
NAPMEMPL Index	15230	0.05	0.44	-1.84	4.61	0.00	0.00	0.00	0.00	0.00
NAPMNEWO Index	15230	0.00	0.09	-0.65	1.46	0.00	0.00	0.00	0.00	0.00
NAPMPRIC Index	15230	0.00	0.07	0.00	1.13	0.00	0.00	0.00	0.00	0.00
NAPMSUPL Index	15230	0.01	0.12	-0.81	1.95	0.00	0.00	0.00	0.00	0.00
NAPMTERM Index	15230	0.00	0.25	-3.66	2.68	0.00	0.00	0.00	0.00	0.00
NAPMUDMN Index	15230	0.01	0.24	-2.18	4.15	0.00	0.00	0.00	0.00	0.00
NAPMUNPR Index	15230	0.04	0.37	-2.47	3.16	0.00	0.00	0.00	0.00	0.00
NAPMUPNT Index	15230	0.00	0.16	-2.46	1.37	0.00	0.00	0.00	0.00	0.00
NFP TCH Index	15230	0.01	0.14	-0.60	1.47	0.00	0.00	0.00	0.00	0.00
NHSLTOT Index	15230	0.01	0.23	-1.61	3.11	0.00	0.00	0.00	0.00	0.00
NHSPSTOT Index	15230	-0.01	0.20	-1.54	2.01	0.00	0.00	0.00	0.00	0.00
OUTFGAF Index	15230	-0.02	0.26	-2.29	2.08	0.00	0.00	0.00	0.00	0.00
PCE CRCH Index	15230	-0.07	0.73	-7.56	1.26	0.00	0.00	0.00	0.00	0.00
PITLCHNG Index	15230	0.00	0.12	-0.37	10.20	0.00	0.00	0.00	0.00	0.00
RCHSINDX Index	15230	0.00	0.22	-1.90	1.68	0.00	0.00	0.00	0.00	0.00
RSTAMOM Index	15230	-0.03	0.33	-3.41	3.10	0.00	0.00	0.00	0.00	0.00
TMNOCHNG Index	15230	-0.01	0.20	-2.93	2.93	0.00	0.00	0.00	0.00	0.00
USCABAL Index	15230	0.00	0.08	-1.25	1.90	0.00	0.00	0.00	0.00	0.00
USMMMNCH Index	15230	0.08	0.86	-1.76	8.97	0.00	0.00	0.00	0.00	0.00
USPHTMOM Index	15230	-0.03	0.22	-1.93	2.29	0.00	0.00	0.00	0.00	0.00
USTBTOT Index	15230	0.00	0.13	-1.21	2.46	0.00	0.00	0.00	0.00	0.00
USURTOT Index	15230	-0.02	0.21	-2.14	0.99	0.00	0.00	0.00	0.00	0.00
bill_naics_match	15230	0.53	0.50	0.00	1.00	0.00	0.00	1.00	1.00	1.00

Table A10: **Summary Statistics of Variables Included in Table 11, Scenario 1.** This table presents summary statistics for the variables used in the Scenario 1 regressions reported in Table 11.

Variable	count	mean	sd	min	max	p5	p25	p50	p75	p95
ret	19443	0.00	0.03	-0.25	0.44	-0.04	-0.01	0.00	0.01	0.05
reportbuy	19443	0.06	0.28	0.00	10.00	0.00	0.00	0.00	0.00	0.00
reportbuy_1	19443	0.05	0.21	0.00	1.00	0.00	0.00	0.00	0.00	0.00
sue5	19442	0.00	0.00	-0.06	0.06	0.00	0.00	0.00	0.00	0.00
sur1	19437	0.00	0.18	-2.47	2.46	0.00	0.00	0.00	0.00	0.00
illiq_2_5	19443	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
volatility_2_5	19443	0.05	0.05	0.00	0.54	0.01	0.02	0.04	0.07	0.16
ADP CHNG Index	19443	0.00	0.05	-0.44	0.63	0.00	0.00	0.00	0.00	0.00
CHPMINDX Index	19443	0.02	0.26	-2.12	2.12	0.00	0.00	0.00	0.00	0.00
CNSTTMOM Index	19443	-0.01	0.42	-2.19	4.20	0.00	0.00	0.00	0.00	0.00
CONCCONF Index	19443	0.04	0.30	-1.82	2.33	0.00	0.00	0.00	0.00	0.00
CONSSENT Index	19443	0.00	0.31	-4.30	1.68	0.00	0.00	0.00	0.00	0.00
CPI CHNG Index	19443	0.00	0.13	-1.52	1.52	0.00	0.00	0.00	0.00	0.00
CPI XYOY Index	19443	-0.01	0.12	-1.36	1.36	0.00	0.00	0.00	0.00	0.00
CPI YOY Index	19443	-0.01	0.11	-1.48	1.48	0.00	0.00	0.00	0.00	0.00
CPUPXCHG Index	19443	-0.01	0.13	-1.44	2.16	0.00	0.00	0.00	0.00	0.00
DGNOCHNG Index	19443	0.02	0.38	-1.96	7.73	0.00	0.00	0.00	0.00	0.00
DGNOXTCH Index	19443	0.06	0.65	-1.47	6.62	0.00	0.00	0.00	0.00	0.00
ECI SA% Index	19443	0.01	0.14	-0.64	1.27	0.00	0.00	0.00	0.00	0.00
EMPRGBCI Index	19443	-0.04	0.39	-3.79	1.44	0.00	0.00	0.00	0.00	0.00
ETSLTOTL Index	19443	0.01	0.19	-1.72	1.65	0.00	0.00	0.00	0.00	0.00
FDDSSD Index	19443	0.00	0.12	-2.27	1.57	0.00	0.00	0.00	0.00	0.00
FDIDFDMO Index	19443	0.01	0.14	-2.44	1.39	0.00	0.00	0.00	0.00	0.00
FDFDFDCH Index	19443	0.01	0.14	-1.93	1.93	0.00	0.00	0.00	0.00	0.00
GDPC1QOQ Index	19443	0.01	0.14	-1.95	1.95	0.00	0.00	0.00	0.00	0.00
HOUST Index	19443	0.01	0.14	-1.83	2.19	0.00	0.00	0.00	0.00	0.00
INJCJC Index	19443	0.00	0.05	-0.93	1.51	0.00	0.00	0.00	0.00	0.00
IP CHNG Index	19443	0.00	0.24	-2.44	2.06	0.00	0.00	0.00	0.00	0.00
IP MFGCH Index	19443	0.02	0.25	-3.33	1.94	0.00	0.00	0.00	0.00	0.00
JOLTTOTL Index	19443	0.01	0.37	-4.04	2.42	0.00	0.00	0.00	0.00	0.00
MICH Index	19443	-0.03	0.69	-6.76	3.04	0.00	0.00	0.00	0.00	0.00
MBAINDX Index	19443	0.02	0.27	-2.05	2.34	0.00	0.00	0.00	0.00	0.00
MBAREFI Index	19443	0.03	0.29	-1.48	2.66	0.00	0.00	0.00	0.00	0.00
MNFCTRNG Index	19443	0.33	2.05	-1.54	17.46	-0.07	0.00	0.00	0.00	0.37
MTSLCHNG Index	19443	-0.17	1.06	-5.56	8.28	-2.22	0.00	0.00	0.00	0.02
NAPM Index	19443	-0.03	0.31	-2.67	1.15	0.00	0.00	0.00	0.00	0.00
NAPMEMPL Index	19443	0.02	0.26	-1.84	2.30	0.00	0.00	0.00	0.00	0.00
NAPMNEWO Index	19443	0.00	0.03	-0.65	1.46	0.00	0.00	0.00	0.00	0.00
NAPMPRIC Index	19443	0.00	0.05	-1.80	1.13	0.00	0.00	0.00	0.00	0.00
NAPMSUPL Index	19443	0.00	0.03	-0.81	1.95	0.00	0.00	0.00	0.00	0.00
NAPMTERM Index	19443	-0.02	0.28	-1.46	2.93	-0.24	0.00	0.00	0.00	0.00
NAPMUDMN Index	19443	0.06	0.45	-1.31	4.15	0.00	0.00	0.00	0.00	0.00
NAPMUNPR Index	19443	0.05	0.39	-1.26	3.16	0.00	0.00	0.00	0.00	0.00
NAPMUPNT Index	19443	-0.01	0.19	-3.17	1.37	0.00	0.00	0.00	0.00	0.00
NFP TCH Index	19443	-0.01	0.06	-0.60	1.47	0.00	0.00	0.00	0.00	0.00
NHSLTOT Index	19443	0.00	0.25	-1.21	3.11	0.00	0.00	0.00	0.00	0.00
NHSPSTOT Index	19443	-0.02	0.20	-1.77	1.54	0.00	0.00	0.00	0.00	0.00
OUTFGAF Index	19443	-0.02	0.29	-2.29	1.63	0.00	0.00	0.00	0.00	0.00
PCE CRCH Index	19443	-0.06	0.72	-7.56	1.26	0.00	0.00	0.00	0.00	0.00
PITLCHNG Index	19443	0.00	0.04	-0.25	0.50	0.00	0.00	0.00	0.00	0.00
RCHSINDX Index	19443	0.01	0.25	-1.90	1.68	0.00	0.00	0.00	0.00	0.00
RSTAMOM Index	19443	0.00	0.10	-0.62	1.09	0.00	0.00	0.00	0.00	0.00
TMNOCHNG Index	19443	-0.01	0.18	-2.93	1.95	0.00	0.00	0.00	0.00	0.00
USCABAL Index	19443	0.00	0.11	-1.34	1.90	0.00	0.00	0.00	0.00	0.00
USMMMNCH Index	19443	0.00	0.11	-0.14	8.97	0.00	0.00	0.00	0.00	0.00
USPHTMOM Index	19443	-0.01	0.25	-1.93	1.65	0.00	0.00	0.00	0.00	0.00
USTBTOT Index	19443	0.00	0.08	-0.95	1.43	0.00	0.00	0.00	0.00	0.00
USURTOT Index	19443	0.00	0.11	-2.14	0.99	0.00	0.00	0.00	0.00	0.00
bill_naics_match	19443	0.51	0.50	0.00	1.00	0.00	0.00	1.00	1.00	1.00

Internet Appendices

IA Data Appendix

Construction of control variables:

- Firm level control variables
  - SUE: SUE follows the standardized unexpected earnings measure constructed by Froot, Kang, Ozik, and Sadka (2017). The detailed form is

$$\mathrm{SUE}_{it} = \frac{\mathrm{AE}_{i,t} - \mathrm{FE}_{i,t}}{\mathrm{P}_{i,t}} \tag{IA.1}$$

    where i implies the individual stock while t implies the quarter. $\mathrm{AE}_{i,t}$ is the quarterly earnings per share announced for quarter t of stock i. $\mathrm{FE}_{i,t}$ is the mean of analysts' forecasted earnings per share, and $\mathrm{P}_{i,t}$ is the quarter-end stock price.
  - SUR: SUR follows the standardized unexpected revenue measure constructed by Froot et al. (2017). The detailed form is

$$\mathrm{SUR}_{it} = \frac{(S_{i,t} - S_{i,t-4}) - r_{i,t}}{\sigma_{i,t}} \tag{IA.2}$$

    where $S_{i,t}$ is the quarterly revenue as of fiscal quarter t for firm i. $r_{i,t}$ and $\sigma_{i,t}$ are the average and the standard deviation, respectively, of $S_{i,t} - S_{i,t-4}$ over the preceding eight quarters.
  - Illiquidity: The illiquidity measure follows the Amihud illiquidity measure proposed by Amihud (2002). It is defined as the average ratio of absolute return to dollar trading volume over a specific time window:

$$\mathrm{ILLIQ}_{i,t}^{(T)} = \frac{1}{T}\sum_{d=1}^{T} \frac{|R_{i,t-d}|}{\mathrm{DollarVolume}_{i,t-d}} \tag{IA.3}$$

    where $|R_{i,t-d}|$ is the absolute return of stock i on day $t-d$, and $\mathrm{DollarVolume}_{i,t-d}$ is the corresponding trading volume in dollars, computed as the number of shares traded multiplied by the absolute closing price. In this paper, we construct three versions of the Amihud illiquidity measure using rolling windows of 5, 10, and 22 trading days.
  - Volatility: To measure stock return volatility over different horizons, we define a rolling volatility metric based on the square root of the sum of squared past returns. For each stock i and for a given time window $m \in \{5, 10, 22\}$, the volatility at time t is computed as

$$\mathrm{volatility}_{i,t}^{(m)} = \sqrt{\sum_{j=1}^{m} r_{i,t-j}^2}, \tag{IA.4}$$

    where $r_{i,t-j}$ denotes the return of stock i at time $t-j$. The returns are lagged by one period to avoid look-ahead bias, and the window includes exactly m most recent daily returns prior to t. In this paper, we construct three versions of this volatility measure using rolling windows of 5, 10, and 22 trading days.
- Macro level control variables: Following the methodology proposed in Bianchi, Gómez-Cram, and Kung (2024), we construct a normalized macroeconomic surprise variable using Bloomberg's Macroeconomic Announcements dataset. For each announcement and date t, this dataset provides the actual realized value ($AC_{i,t}$) and the

survey median forecast value ($SUM_{i,t}$), which reflects the market consensus expectation collected by Bloomberg prior to the release. Then we identify the macroeconomic announcements that have the top 50 relevance scores provided in Bloomberg's Macroeconomic Announcements dataset. The final sample comprises 51 macroeconomic announcements.

For each macroeconomic event identified by its ticker symbol, we compute the normalized macroeconomic surprise variable, which is defined as:

$$\text{macro_control}_{j,t} = \frac{AC_{j,t} - SUM_{j,t}}{\sigma_j} \tag{IA.5}$$

where $AC_{i,t}$ is the actual realized value and $SUM_{i,t}$ is the value of the survey median forecast. σ_j is the standard deviation for $AC_{j,t} - SUM_{j,t}$ in each event j.

Bill classification details

- Data source

  Our raw sample includes all bills introduced in the United States House of Representatives between 2014 and 2022 as documented on the official Congress website. The official bill summaries and recorded actions for each legislative bill during this period are utilized for classification analysis.

- Classification procedure

  Bills were systematically categorized according to their projected direct fiscal impact based on human coding of their legislative content. The classification scheme consisted of three mutually exclusive categories:

  Fiscal Expansion (P): Bills primarily designed to increase net government expenditure or decrease net government revenue. This category included proposals for:

  - New or increased government spending
  - Tax reductions
  - Tax credits or deductions
  - Subsidies or direct financial incentives

  Fiscal Contraction (S): Bills primarily designed to decrease net government expenditure or increase net government revenue. This category included proposals for:

  - Spending cuts or budget reductions
  - Tax increases
  - Elimination of tax deductions or credits
  - Reduction or termination of benefit programs

  Fiscally Neutral (N): Bills assessed as having no significant impact on government expenditure or revenue. This category was applied in two scenarios:

  - The bill summary contained no information relevant to fiscal impact analysis.
  - The bill's provisions, as described in the summary and actions, pertained solely to non-fiscal conditions (e.g., procedural changes, symbolic resolutions, authorizations without appropriations).

  Classification based on the above criteria is performed manually and each bill's official text summary and recorded legislative actions are reviewed to determine its primary fiscal intent and assign it to the most appropriate category (P, S, or N).

Table IA.1: **Conditional Results - Heterogeneity - Committee.**
This table complements Table 12 by presenting detailed heterogeneity results that consider purchase disclosure records at the ticker-day level (i, t) made by House committee members whose committees belong to the same industry as ticker i, for both Scenario 1 and Scenario 2. Standard errors are double-clustered by ticker and event date. ***, p-value <1%; **, <5%; *, <10%.

DV:	Daily stock returns [-21, +21] of a ReportBuy Event					
Firm controls:	Yes	Yes	Yes	Yes	Yes	Yes
Macro controls:	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE:	Yes	Yes	Yes	Yes	Yes	Yes
Day FE:	Yes	Yes	Yes	Yes	Yes	Yes
Bill content matched with stock NAICS?	All	Yes	No	All	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Scenario 2: Purchase, Disclosure, Bill passing						
ReportBuy_1	0.007141**	0.008150*	0.004504			
	(0.002908)	(0.004460)	(0.003552)			
ReportBuy				0.005903**	0.007190*	0.004141
				(0.002591)	(0.003726)	(0.003181)
Constant	0.003963***	0.001342	0.006783***	0.003960***	0.001345	0.006771***
	(0.001348)	(0.000832)	(0.002274)	(0.001348)	(0.000831)	(0.002274)
N	15171	8049	7121	15171	8049	7121
R^2	0.16	0.13	0.19	0.16	0.13	0.19
Scenario 1: Purchase, Bill passing, Disclosure						
ReportBuy_1	-0.001695	-0.000699	-0.003933			
	(0.002327)	(0.002961)	(0.002628)			
ReportBuy				0.000463	0.001866	-0.001526
				(0.001981)	(0.002800)	(0.002246)
Constant	0.004952***	-0.000805	0.009900***	0.004920***	-0.000838	0.009863***
	(0.001294)	(0.000912)	(0.001726)	(0.001287)	(0.000914)	(0.001721)
N	19436	9876	9560	19436	9876	9560
R^2	0.17	0.073	0.25	0.17	0.074	0.25

Table IA.2: **Conditional Results - Heterogeneity - Non-Passive.**
This table complements Table 12 by presenting detailed heterogeneity results that consider only non-passive purchase trades, where stocks explicitly held in *trusts* documented in the "subholding" section of the record are classified as passive trades, for both Scenario 1 and Scenario 2. Standard errors are double-clustered by ticker and event date. ***, p-value <1%; **, <5%; *, <10%.

DV:	Daily stock returns [-21, +21] of a ReportBuy Event					
Firm controls:	Yes	Yes	Yes	Yes	Yes	Yes
Macro controls:	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE:	Yes	Yes	Yes	Yes	Yes	Yes
Day FE:	Yes	Yes	Yes	Yes	Yes	Yes
Bill content matched with stock NAICS?	All	Yes	No	All	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Scenario 2: Purchase, Disclosure, Bill passing						
ReportBuy_1	0.006920***	0.008778***	0.005231**			
	(0.002071)	(0.002815)	(0.001906)			
ReportBuy				0.005474***	0.007008***	0.004224***
				(0.001595)	(0.002252)	(0.001406)
Constant	0.003683*	0.001058	0.006492***	0.003692**	0.001058	0.006510***
	(0.001929)	(0.000833)	(0.002288)	(0.001690)	(0.000831)	(0.002278)
N	15171	8049	7121	15171	8049	7121
R^2	0.16	0.14	0.19	0.16	0.14	0.19
Scenario 1: Purchase, Bill passing, Disclosure						
ReportBuy_1	0.001458	0.003072*	-0.001542			
	(0.001558)	(0.001675)	(0.001948)			
ReportBuy				0.000400	0.002702**	-0.002404
				(0.001454)	(0.001149)	(0.001639)
Constant	0.004848***	-0.000932	0.009933***	0.004902***	-0.000950	0.010007***
	(0.001284)	(0.000922)	(0.001706)	(0.001269)	(0.000923)	(0.001678)
N	19436	9876	9560	19436	9876	9560
R^2	0.17	0.074	0.25	0.17	0.074	0.25

Table IA.3: **Conditional Results - Heterogeneity - High Fiscal Dependence.**
This table complements Table 12 by presenting detailed heterogeneity results that only consider tickers in industries with high fiscal dependence (FD) during the sample period, defined as those with above-median procurement contract amounts relative to total revenue, for both Scenario 1 and Scenario 2. Standard errors are double-clustered by ticker and event date. ***, p-value <1%; **, <5%; *, <10%.

DV:	Daily stock returns [-21, +21] of a ReportBuy Event					
Firm controls:	Yes	Yes	Yes	Yes	Yes	Yes
Macro controls:	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE:	Yes	Yes	Yes	Yes	Yes	Yes
Day FE:	Yes	Yes	Yes	Yes	Yes	Yes
Bill content matched with stock NAICS?	All	Yes	No	All	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Scenario 2: Purchase, Disclosure, Bill passing						
ReportBuy_1	0.008351***	0.008861***	0.007740***			
	(0.002268)	(0.002871)	(0.002432)			
ReportBuy				0.006353***	0.007363***	0.005383***
				(0.001720)	(0.002256)	(0.001906)
Constant	0.002953**	0.001009	0.007773***	0.002974**	0.001035	0.007793***
	(0.001403)	(0.000951)	(0.002497)	(0.001397)	(0.000943)	(0.002474)
N	10648	6553	4095	10648	6553	4095
R^2	0.18	0.16	0.22	0.18	0.16	0.22
Scenario 1: Purchase, Bill passing, Disclosure						
ReportBuy_1	-0.000086	0.002117	-0.003882			
	(0.002132)	(0.001913)	(0.002852)			
ReportBuy				-0.000559	0.002315*	-0.004101**
				(0.001665)	(0.001184)	(0.001567)
Constant	0.004652***	-0.001167	0.010338***	0.004687***	-0.001208	0.010386***
	(0.001259)	(0.000926)	(0.001693)	(0.001222)	(0.000925)	(0.001642)
N	13752	7389	6363	13752	7389	6363
R^2	0.18	0.079	0.28	0.18	0.079	0.28

Table IA.4: **Conditional Results - Robustness - Pre-2020.**
This table complements Table 12 by presenting detailed robustness test results that only consider the sample period before 2020, for both Scenario 1 and Scenario 2. Standard errors are double-clustered by ticker and event date. ***, p-value <1%; **, <5%; *, <10%.

DV:	Daily stock returns [-21, +21] of a ReportBuy Event					
Firm controls:	Yes	Yes	Yes	Yes	Yes	Yes
Macro controls:	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE:	Yes	Yes	Yes	Yes	Yes	Yes
Day FE:	Yes	Yes	Yes	Yes	Yes	Yes
Bill content matched with stock NAICS?	All	Yes	No	All	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Scenario 2: Purchase, Disclosure, Bill passing						
ReportBuy_1	0.003636**	0.004076*	0.002944			
	(0.001788)	(0.002142)	(0.002546)			
ReportBuy				0.003486**	0.003826**	0.003161
				(0.001573)	(0.001786)	(0.002401)
Constant	-0.000612	-0.000626	-0.002219*	-0.000630	-0.000632	-0.002271*
	(0.000493)	(0.000568)	(0.001195)	(0.000486)	(0.000573)	(0.001180)
N	7425	4594	2831	7425	4594	2831
R^2	0.087	0.10	0.10	0.088	0.10	0.10
Scenario 1: Purchase, Bill passing, Disclosure						
ReportBuy_1	-0.000241	0.001057	-0.002689			
	(0.001098)	(0.001130)	(0.002166)			
ReportBuy				0.000596	0.001677*	-0.001846
				(0.000859)	(0.000966)	(0.001472)
Constant	-0.001598***	-0.001632**	-0.001677*	-0.001639***	-0.001680**	-0.001689*
	(0.000597)	(0.000702)	(0.000906)	(0.000594)	(0.000696)	(0.000903)
N	8609	6053	2556	8609	6053	2556
R^2	0.074	0.077	0.11	0.074	0.077	0.11

Table IA.5: **Conditional Results - Robustness - Drop the Majority Leader.**
This table complements Table 12 by presenting detailed robustness test results that exclude records of purchases made by majority leaders, for both Scenario 1 and Scenario 2. Standard errors are double-clustered by ticker and event date. ***, p-value <1%; **, <5%; *, <10%.

DV:	Daily stock returns [-21, +21] of a ReportBuy Event					
Firm controls:	Yes	Yes	Yes	Yes	Yes	Yes
Macro controls:	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE:	Yes	Yes	Yes	Yes	Yes	Yes
Day FE:	Yes	Yes	Yes	Yes	Yes	Yes
Bill content matched with stock NAICS?	All	Yes	No	All	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Scenario 2: Purchase, Disclosure, Bill passing						
ReportBuy_1	0.006251***	0.007870***	0.004879**			
	(0.002123)	(0.002718)	(0.001863)			
ReportBuy				0.005086***	0.006356***	0.004102***
				(0.001564)	(0.002179)	(0.001373)
Constant	0.003710*	0.001079	0.006514***	0.003707**	0.001073	0.006516***
	(0.002020)	(0.000848)	(0.002311)	(0.001746)	(0.000844)	(0.002297)
N	15171	8049	7121	15171	8049	7121
R^2	0.16	0.14	0.19	0.16	0.14	0.19
Scenario 1: Purchase, Bill passing, Disclosure						
ReportBuy_1	-0.000390	0.002189	-0.004400*			
	(0.001935)	(0.001713)	(0.002594)			
ReportBuy				-0.000529	0.002211*	-0.003688**
				(0.001438)	(0.001144)	(0.001493)
Constant	0.004953***	-0.000902	0.010180***	0.004968***	-0.000928	0.010166***
	(0.001339)	(0.000923)	(0.001780)	(0.001306)	(0.000923)	(0.001721)
N	19436	9876	9560	19436	9876	9560
R^2	0.17	0.074	0.25	0.17	0.074	0.25

Table IA.6: **Conditional Results - Robustness - Control for ret(1).**
This table complements Table 12 by presenting detailed robustness test results that include an additional control variable: the one-day lagged return of each ticker i, for both Scenario 1 and Scenario 2. Standard errors are double-clustered by ticker and event date. ***, p-value <1%; **, <5%; *, <10%.

DV:	Daily stock returns [-21, +21] of a ReportBuy Event					
Firm controls:	Yes	Yes	Yes	Yes	Yes	Yes
Macro controls:	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE:	Yes	Yes	Yes	Yes	Yes	Yes
Day FE:	Yes	Yes	Yes	Yes	Yes	Yes
Bill content matched with stock NAICS?	All	Yes	No	All	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Scenario 2: Purchase, Disclosure, Bill passing						
ReportBuy_1	0.006031***	0.007670***	0.004772**			
	(0.002238)	(0.002721)	(0.001971)			
ReportBuy				0.004934***	0.006210***	0.004056***
				(0.001589)	(0.002183)	(0.001449)
Constant	0.003729	0.001085	0.006524***	0.003725**	0.001079	0.006522***
	(0.002377)	(0.000860)	(0.002352)	(0.001874)	(0.000855)	(0.002335)
N	15171	8049	7121	15171	8049	7121
R^2	0.16	0.14	0.20	0.16	0.14	0.20
Scenario 1: Purchase, Bill passing, Disclosure						
ReportBuy_1	-0.000306	0.002306	-0.004313			
	(0.001977)	(0.001703)	(0.002679)			
ReportBuy				-0.000431	0.002283**	-0.003546**
				(0.001433)	(0.001135)	(0.001511)
Constant	0.004868***	-0.000933	0.010126***	0.004881***	-0.000959	0.010106***
	(0.001310)	(0.000927)	(0.001757)	(0.001275)	(0.000928)	(0.001694)
N	19436	9876	9560	19436	9876	9560
R^2	0.18	0.076	0.25	0.18	0.075	0.25

Table IA.7: **Conditional Results - Robustness - Control for Report Sell.**
This table complements Table 12 by presenting detailed robustness test results that include two additional control variables: ReportSell_1 and ReportSell, for both Scenario 1 and Scenario 2. ReportSell_1 equals one if a sale record of ticker i is disclosed on day t. ReportSell equals the number of sale records of ticker i being disclosed on day t. Standard errors are double-clustered by ticker and event date. ***, p-value <1%; **, <5%; *, <10%.

DV:	Daily stock returns [-21, +21] of a ReportBuy Event					
Firm controls:	Yes	Yes	Yes	Yes	Yes	Yes
Macro controls:	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE:	Yes	Yes	Yes	Yes	Yes	Yes
Day FE:	Yes	Yes	Yes	Yes	Yes	Yes
Bill content matched with stock NAICS?	All	Yes	No	All	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Scenario 2: Purchase, Disclosure, Bill passing						
ReportBuy_1	0.005945***	0.007775***	0.004284**			
	(0.002047)	(0.002696)	(0.001731)			
ReportSell_1	0.002737	0.000879	0.004309***			
	(0.001814)	(0.002680)	(0.001617)			
ReportBuy				0.004762***	0.006183***	0.003562**
				(0.001557)	(0.002181)	(0.001348)
ReportSell				0.001843	0.001991	0.002137*
				(0.001129)	(0.001680)	(0.001124)
Constant	0.003658*	0.001064	0.006404***	0.003674**	0.001032	0.006469***
	(0.001930)	(0.000846)	(0.002344)	(0.001715)	(0.000845)	(0.002306)
N	15171	8049	7121	15171	8049	7121
R^2	0.16	0.14	0.19	0.16	0.14	0.19
Scenario 1: Purchase, Bill passing, Disclosure						
ReportBuy_1	-0.000593	0.002126	-0.004685*			
	(0.001958)	(0.001687)	(0.002667)			
ReportSell_1	0.003152	0.001889	0.003445			
	(0.002103)	(0.001894)	(0.002676)			
ReportBuy				-0.000600	0.002062*	-0.003799**
				(0.001375)	(0.001141)	(0.001619)
ReportSell				0.000776	0.002075	0.000627
				(0.002318)	(0.001300)	(0.002803)
Constant	0.004884***	-0.000925	0.010066***	0.004949***	-0.000957	0.010145***
	(0.001335)	(0.000926)	(0.001776)	(0.001298)	(0.000925)	(0.001709)
N	19436	9876	9560	19436	9876	9560
R^2	0.17	0.074	0.25	0.17	0.074	0.25