Electoral Pressure and Strategic Disclosure of Congressional Trades

Amy Hutton
amy.hutton@bc.edu

Susan Shu
susan.shu@bc.edu

Danling Song
danling.song@bc.edu

August 2025
Preliminary—please do not cite without permission

Abstract

We investigate whether Members of Congress strategically disclose stock trades in response to electoral pressure. Using a comprehensive sample of congressional trades between 2013–2023, we find that legislators disproportionately disclose less profitable trades during election periods, when scrutiny is highest. Moreover, some legislators violate the 2012 STOCK Act by delaying required pre-election disclosures until after elections. These behaviors are especially pronounced among legislators in close races and those representing economically distressed districts, where voter resentment of elite privilege is stronger. We also examine politicians' choice of disclosure *method* and show that, compared to electronic filings, trades filed on paper forms are more profitable, consistent with "paper" trades being more informed. Leveraging differences in politicians' systematic disclosure patterns, we identify a subset of "Suspicious Politicians" who consistently trade more profitably than their peers. Our findings highlight how weak disclosure rules and lax enforcement facilitate strategic behavior by political insiders, underscoring the need for stronger oversight to ensure politicians' accountability to their constituents.

Keywords: Congressional Trading; STOCK ACT; Insider trading
JEL classification: G14; G18; D72; M41; M48
Data availability: All data are available from public sources identified in the paper.

*All authors are from the Carroll School of Management, Boston College, 140 Commonwealth Avenue, Chestnut Hill, MA 02467. The authors acknowledge generous financial support from the E&Y Foundation.

1 Introduction

Stock trading by Members of Congress has drawn significant scrutiny from the public and academics alike. Members of Congress are not ordinary investors, as they are privy to non-public information from their legislative duties, i.e., "congressional knowledge" (Gao and Huang, 2016; Christensen, Mikhail, Walther, and Wellman, 2017; Jagolinzer, Larcker, Ormazabal, and Taylor, 2020; Baldauf, Favilukis, Garlappi, and Zheng, 2025). Recent academic research (e.g., Abdurakhmonov, Snider, Ridge, and Hasija, 2023), extensive media coverage, and public criticism reflect a widespread perception that congressional knowledge exists broadly and has been used for politicians' personal financial gain. This perception has motivated financial innovations, including ETFs (e.g., Unusual Whales Subversive Dem/Rep Trading) and investor platforms explicitly designed to track and mimic congressional trades.

Similar to corporate insider trading, the use of privileged information by Members of Congress at the expense of other investors can undermine the integrity of the financial markets (Blau, Griffith, and Whitby, 2022). Despite the public's concern, the oversight of congressional stock trading remains lax, especially in comparison to corporate insider trading. The Stop Trading on Congressional Knowledge (STOCK) Act enacted in 2012 does not prohibit Members of Congress from trading stocks, even stocks of companies directly affected by their legislative duties. Prior to the STOCK Act, legislators were only required to disclose their trades annually, significantly delaying public awareness of their trading activities. The STOCK Act notably shortened this reporting interval, requiring legislators to report securities transactions exceeding $1,000 within 45 days of execution. However, even this improved disclosure window stands in stark contrast to the two-business-day reporting requirement imposed on corporate insiders. Moreover, enforcement of congressional disclosure requirements is weak with minimal

consequences (Schroeder, 2014). Violators typically face a nominal $200 fine, which is often waived.[1] Accountability has come, instead, from public scandals celebrated by political opponents and, ultimately, perhaps from voters who have the power to impose electoral consequences.[2]

In this study, we examine whether Members of Congress exploit lenient stock trading disclosure rules and lax enforcement of those rules to strategically disclose their trades to avoid electoral consequences and remain largely unaccountable to their constituents. Specifically, we conjecture that regulatory leniency creates opportunities for Members to delay disclosure of their informed trades during election cycles when public and media scrutiny intensifies.[3] In response to heightened potential for political backlash during critical campaign periods, politicians have incentives to exploit regulatory loopholes and avoid the electoral consequences of stock trading scandals just prior to their election bids. This perspective shifts the research focus from documenting abnormal trading profits per se to examining politicians' disclosure practices.

Our first test utilizes a comprehensive sample of congressional trades disclosed between 2013 and 2023 and examines whether and how electoral pressure affects the *timing* of Members' disclosures.[4] Given heightened scrutiny by political opponents and the media before and during

[1] For example, in 2022, Rep. Bill Keating reported two stock trades beyond the 45-day deadline. His office stated that the delay fell within the House Ethics Committee's grace period and, consequently, no fine was imposed. See https://www.globaltimes.cn/page/202211/1278983.shtml.

[2] Sen. Kelly Loeffler provides an example of electoral consequences stemming from insider trading allegations. Although ultimately cleared by a Justice Department investigation into her stock transactions during the early stages of the COVID-19 pandemic, Loeffler faced significant voter backlash. She spent crucial months of her 2020 campaign battling insider trading accusations rather than campaigning, severely damaging her public image and electoral prospects, and ultimately lost her Senate seat. See this *New York Times* article: https://www.nytimes.com/2020/06/15/us/politics/congress-trading-stock-loeffler-burr.html.

[3] Anecdotal evidence supports regarding legislators' strategic use of disclosure timing. For instance, the twitter account "*@congresstrading.com*", affiliated with an investment forum, criticized Nancy Pelosi for intentionally disclosing multimillion dollar Nvidia stock trades on the Friday before the Christmas weekend, describing the timing as "a deceptive tactic" intended to minimize media scrutiny.

[4] Given the lax enforcement of the STOCK Act's disclosure requirements, observable data cover only disclosed trades and may be selected. Two unobserved margins remain: (i) non-disclosure through non-compliance or trading via channels that evade reporting, and (ii) selective disclosure of trades. The incidence and profitability of undisclosed trades are unknown, so inferences apply only to the disclosed sample – a limitation share by prior studies of congressional trades.

elections, we expect Members' disclosure of their profitable trades to vary with election cycles. We focus on profitable trades because they are perceived to be informed by nonpublic congressional knowledge and thus are the hotbeds for political scandals. Specifically, we compare the profitability of stock trades disclosed by Members in the high electoral-pressure window (i.e., the month immediately preceding primaries through election season) to the profitability of trades disclosed in other periods. We find that congressional trades disclosed during electoral-pressure windows exhibit lower profitability, consistent with Members strategically delaying the disclosure of profitable trades to avoid the electoral consequences of potential stock-trading scandals. To solidify our interpretation that the strategic disclosure behavior is driven by electoral pressure, we conduct a falsification test where we examine the disclosure behavior of retiring politicians, who do not face reelection pressure. We find no evidence that retiring politicians exhibit the same strategic delays in disclosing their profitable stock trades.

In cross-sectional analyses, we examine whether legislators' strategic disclosure behavior is more pronounced for legislators facing greater electoral pressure. We document that the effect is more pronounced for legislators involved in tight election contests and those representing economically distressed districts, where voters are more likely to resent the perceived abuse of power by political elites. These results lend support to our interpretation that Members' disclosure choices are affected by electoral considerations.

A notable example of a politician's strategic disclose choices is Rep. Mike Garcia's sale of Boeing shares weeks before his congressional committee released a highly critical report related to Boeing's involvement in fatal crashes. Despite executing the transaction well before the 2020

election, Garcia did not disclose it until after narrowly winning re-election by a margin of only 333 votes – significantly beyond the STOCK Act's mandated 45-day disclosure period[5].

Taking our lead from this potentially extreme example, we design our next set of tests to document whether *other* legislators are also willing to violate the law to delay disclosing stock trades until after elections. For this analysis, we focus on trades that, under the STOCK Act, must be disclosed before the upcoming election, the subsample of congressional trades executed at least 45 days prior to elections. If legislators adhere to the disclosure requirements, these trades would be disclosed before the election, i.e., any trades disclosed after the election are by definition *Delayed Disclosures* that violate the STOCK Act. Using this subsample, we compare legislators' tendency to delay the disclosure of stock trades executed in election years versus similar trades (executed during the same calendar dates) in non-election years.[6]

Consistent with electoral pressure influencing disclosure behavior, we demonstrate that the likelihood of *Delayed Disclosure* is higher in election years. Furthermore, cross-sectional analyses of the *Delayed Disclosure* indicate that the differential tendency to delay disclosure in election versus non-election years is most pronounced for legislators' profitable trades, the one that are most vulnerable to criticism by political challengers and the media. Additionally, the tendency to delay disclosure in election years is also concentrated among legislators involved in tight election contests and those hailing from economically distressed districts where voter resentment of the perceived abuse of power by political elites is stronger. Collectively, these results provide

[5] See https://www.politico.com/news/2024/07/10/democrats-mike-garcia-boeing-stock-00167225.

[6] To compare legislators' disclosure choices in election years to their choices in nonelection years, we set an indicator variable, *Delay Disclosure* to 1 if a trade is disclosed after the first Tuesday after the first Monday in November, and 0 otherwise. In election years this Tuesday is election day and in nonelection years it is our *pseudo* election day. *Delay Disclosure* set to one indicates that the Member violated the 45-day STOCK Act disclosure requirement; in election years it also means the Members' disclosure did not happen until after election day.

consistent support for our inference that electoral pressure motivates legislators to delay disclose of stock trades until after elections, even when doing so violates the law.

In addition to choosing the timing of their disclosures, politicians also choose *the form of disclosures*. Legislators have the choice of filing paper forms or using the electronic filing system to report their trading activities. In 2012 the House Ethics Committee issued disclosure guidelines for the newly passes STOCK Act "strongly encourage[ing] all filers to use the electronic filing system" noting that doing so would "reduce errors and greatly simplify future filings." While the majority of Congressional trading activity moved to electronic reporting, *some* politicians continue to submit paper forms.[7] The persistent use of paper filings long after electronic filings had become the norm underscores that the *form* of disclosure, in addition to the timing, can be a deliberate, strategic choice. If politicians who continue to use paper forms do so to limit or delay public access to their filings, then we expect "paper" trades to be more informed and thereby more profitable. This is, in fact, what we find: Trades disclosed on paper forms are more profitable than those filed electronically.

Having demonstrated that *some* politicians strategically disclose trading activity (use paper forms and delay disclosing beyond the 45-day requirement), we next examine individual politicians' disclosure choices, identify those whose systematic behaviors raise suspicions that they trade on congressional knowledge and document whether their trades generate higher abnormal returns than their peers. Consistent with our prediction, we document significantly higher abnormal returns for trades executed by *Suspicious Politicians* whose disclosure pattern suggests their trades are informed by private congressional knowledge.

[7] See a related news article: https://readsludge.com/2019/11/13/ethics-committee-republicans-defy-committees-own-financial-disclosure-recommendations/?utm_source=chatgpt.com. As of 2025, paper filings have not been fully phased out.

We contribute to the literature by providing systematic evidence that legislators strategically disclosure their stock trades to avoid electoral consequences. We show that Members of Congress exploit lenient stock trading disclosure rules and lax enforcement to strategically time the disclosure of their trades around election cycles. A key finding is that legislators will violate the STOCK Act's disclosure requirements when doing so permits them to hide their profitable trades until after elections. Aside from recent evidence showing positive market reactions to the disclosure of congressional stock purchase (Abdurakhmonov, Snider, Ridge, and Hasija, 2023), existing research has primarily focused on the profitability of congressional trades., i.e., examining whether legislators leverage their congressional knowledge in their stock trades, with mixed findings (e.g., Boyd, Cheng, and Ziobrowski, 2004 and 2011; Hanousek, Jo, Pantzalis, and Park, 2022; Eggers and Hainmueller, 2013 and 2014; Belmont, Sacerdote, Sehgal, and Hoek, 2020; Huang and Xuan, 2023; Karadas, Schlosky, and Hall, 2021). By shifting the focus from trading activity alone to strategic disclosure behavior, this study offers important insights for policy reforms aimed at enhancing accountability for elected officials.

2 Background and Hypothesis Development

2.1 Background: Congressional stock trading and the STOCK Act of 2012

Congressional stock trading has long been a source of public concern. Members of Congress routinely gain access to nonpublic information through hearings, investigations, and policy deliberations, and their legislative actions can directly affect firms and industries. The *Stop Trading on Congressional Knowledge* (STOCK) *Act*, enacted on April 4, 2012, sought to address these concerns by explicitly prohibiting legislators from using material nonpublic information "derived from such person's position" or "gained from the performance of such person's official

responsibilities" for private financial gain[8,9]. The Act clarifies that legislators, their families and staff are subject to the same Rule 10b-5 insider trading prohibitions as corporate insiders[10].

Unlike outright restrictions on trading, the Act relies primarily on disclosure: any securities transaction exceeding $1,000 must be reported no later than 45 days after execution. However, the disclosure regime has proven weakly enforced. Violations are subject only to nominal $200 fines, which are often waived. Oversight is decentralized, leaving compliance largely to political self-policing. As a result, accountability for questionable trades has often come not from regulators but from the electoral arena. Political challengers and national committees of both parties often fan the flames of the public scandals by directly attacking incumbents for their stock trades in the press and on social media.

The following examples illustrate the electoral consequences and reputational costs of congressional stock trading. During the 2022 midterms, in New Jersey's highly competitive 7th District, the incumbent Democrat Tom Malinowski came under heavy fire from his Republican opponent, Tom Kean, and national GOP operatives. They branded him with the hashtag *#tradingtom* and the nickname "Trader Tom" on social media. Campaign statements accused Malinowski of "*betting against the American economy during the pandemic*", claiming he had

[8] More recently, the ongoing debate over congressional stock trading has led to several bipartisan bills aimed at prohibiting Congressional trading of individual stocks entirely. For example, the "TRUST in Congress Act" bill introduced in January of 2025 would require Members of Congress, their spouse and dependent child to place specified investments into a qualified blind trust until 180 days after the end of their tenure as a Member of Congress. https://www.congress.gov/bill/119th-congress/house-bill/396/text accessed February 20, 2025.

[9] STOCK Act, Pub. L. No. 112-105, 126 Stat. 291, 292 (2012). The law also restricts participation in IPOs and requires reporting of activities involving the gather of information from government sources for the purposes of financial investments.

[10] Interestingly, to avoid fallout from the new law, corporate law firms are advising publicly traded companies to not provide any material, non-public information to Member of Congress or their staff. Law firms are also warning their clients to "scrupulously avoid providing to any third party, or trading on, any material, non-public information received from Congress. In addition, publicly traded companies should be prepared for additional scrutiny if a Member [of Congress] or staffer discloses that he or she has traded in the company's stock." See a related news articles in https://www.hklaw.com/en/insights/publications/2012/07/what-you-need-to-know-about-the-stock-act accessed February 20, 2025.

"*blown through the law more than 140 times, earning himself a bipartisan ethics probe*."[11] Similarly, in the 2018 primaries, *The Washington Post* reported on political challengers attacking several incumbents for their stock trades in the opioid industry while co-sponsoring a measure that would hamper the Drug Enforcement Administration's enforcement efforts against the industry. The article detailed four incumbents' stock trades and quoted *Project on Government Oversight*, a nonpartisan government watchdog, who spelled out the conflict of interests: "we don't want Members of Congress to be promoting legislation that only helps their portfolios and not the government or the public."[12]

Media investigations have further elevated the issue. *The New York Times* identified 97 members of Congress who had traded stocks, including stocks of companies directly under their committees' jurisdictions. Government ethics experts noted in the *Times* article that "the trading patterns uncovered by the *Times* analysis underscore longstanding concerns about the potential for conflicts of interest or use of inside information by Members of Congress".[13] *Business Insider* launched its "Conflicted Congress" project in 2021, creating a scorecard of lawmakers' conflicts of interest. Two of its metrics – violating of the STOCK Act and legibility of disclosure forms – speak directly to disclosure practices of congressional trades.

[11] See this news article: "Stock trading law a campaign issue in NJ", *Asbury Park Press*, June 16, 2022, published a week after the Congressional primary in NJ.

[12] The article highlighted that Senator Sheldon Whitehouse traded stock in CVS; Rep. Peter Welch (D-VT) and Rep. Gus Bilirakis (R-FL) traded stock in Rite Aid; Rep James Sensenbrenner (R-Wis) traded stock in AbbVie, a manufacturer of Vicodin, a wildly abused narcotic painkiller. Documenting the political fallout, the article provides details about the challengers who entered the race to fight the incumbents' corruption and abuse of power. "Why I chose to run: Congressional challengers use bill backed by drug companies against incumbents", May 13, 2018, the *Washington Post*.

[13] "These 97 Members of Congress Reported Trades in Companies Influenced by Their Committees", the *New York Time*, September 13, 2022. See also *Business Insider* "Conflicted Congress" project that highlights more than 50 Members of Congress who violated the STOCK Act's financial disclosure rules. https://www.businessinsider.com/conflicted-congress-key-findings-stock-act-finances-investing-2021-12, Released December 2021; accessed February 20, 2025.

These episodes reveal the central weakness of the STOCK Act: enforcement is minimal, with modest fines that are rarely imposed, leaving members wide latitude in how they disclose their trading activities. What legislators fear most are the electoral consequences that can arise when their disclosures attract public attention. In this environment, disclosure becomes a strategic tool. Members have incentives to strategically time their disclosures, select the form of their disclosures, and otherwise exploit the system's loopholes to avoid accountability. This weakly enforced regime provides the institutional foundation for our research where we explore how legislators manage their disclosure practices to avoid political backlash and electoral consequences.

2.2 Existing Research and Hypothesis Development

As discussed earlier, despite concerns that Member of Congress trade on and benefit from nonpublic information arising from "congressional knowledge", existing research provides mixed evidence on whether congressional members earn abnormal returns from their stock trades. In particular, recent work finds little evidence that politicians earn abnormal returns after the STOCK Act (see Huang and Xuan, 2023; Karadas, Schlosky and Hall 2021 and Belmont, Sacerdote, Sehgal, and Hoek 2020).[14]

There are several challenges when empirically examining legislators' abnormal trading profits. First, politicians may trade stocks for legitimate, legal reasons, such as portfolio diversification or liquidity needs. Unlike corporate insider trading, there is no reliable method to distinguish trades driven by congressional knowledge from those motivated by other legitimate

[14] As noted above, the lax enforcement of the financial disclosure requirement makes it possible that some Members of Congress do not disclose their most profitable trades. If the Members of Congress who choose not to disclose their trades are the ones with the greatest congressional knowledge, then the lack of observable positive abnormal returns could be an artifact of a selection bias in observable trades. This data limitation makes the study of the profitability of congressional trades difficult.

reasons.[15] Further, politicians also invest in shares of their "Home State" companies to demonstrate their alignment and commitment to their constituents (Mehta, Srinivasan, and Zhao, 2020), potentially forgoing abnormal trading profits in exchange for greater political capital.[16]

Rather than examining whether congressional members earn abnormal trading profits, we shift the focus from trading activity alone to legislators' strategic disclosure choices. To identify legislators' incentives to engage in strategic disclosure, we focus on the role of electoral pressure. We expect electoral pressure to intensify as a legislator's next re-election approaches (Shepsle, Van Houweling, and Abrams, 2009; Lindstadt and Vander Wielen, 2011; Treul, Thomsen, Volden, and Wiseman, 2022), i.e., every two years for Representatives; every six years for Senators. A public scandal could jeopardize their re-election bid. As noted above, accusations of congressional insider stock trading garner sensational press coverage, especially during the election cycles. Challengers frequently point to incumbents' profitable stock trades to convince voters that their political opponents abuse their power, lack integrity and are untrustworthy. To avoid such damaging political scandals, we expect Members of Congress to strategically disclose less profitable trades during the highly scrutinized election periods.[17] The lower profitability of the disclosed trades is *prima facie* evidence that the trades were not based on privileged "congressional knowledge." This leads to our first hypothesis:

H1: During periods of high electoral pressure, congressional members report lower abnormal profits from their stock trades compared to other periods.

[15] When it comes to corporate insiders, existing literature shows that while opportunistic trades are associated with future abnormal returns, routine trades are not (see e.g., Cohen, Malloy, and Pomorski, 2012).

[16] Moreover, political science research suggests that legislative power of Congress and its Committees has declined over the last two decades as they have lost power over the legislative agenda to majority leaders in both Chambers. See Mann and Ornstein (2016) and Farhang (2021). Loss of legislative power likely results in less congressional knowledge based on legislative activities and fewer opportunities for profitable, informed trading.

[17] Implicit in our argument is that recent trading activity is more salient to voters than historical trading activities. Given short news cycles and the accompanying short memories of constituents, we rely on this assumption without explicitly validating it. If the assumption is wrong, we expect our tests to have low power.

Given lax enforcement of the STOCK Act's 45-day disclosure requirement, we further hypothesize that congressional members have incentives to delay disclosing stock trades, especially profitable trades, beyond election day. This is especially likely if the perceived benefits from noncompliance with the STOCK Act's 45-day disclosure requirement (e.g., avoiding negative backlash from alleged insider trading immediately before elections) and delaying the disclosure until after election outweigh the expected costs (e.g., modest fines and infrequent ethics investigation).[18] This leads to our second hypothesis:

H2: Congressional members are more likely to delay disclosure of stock trades beyond Election Day in election years compared to equivalent periods in non-election years.

While all candidates are susceptible to public scandals and the related electoral consequences, we expect the strategic delay to be more pronounced among legislators facing greater electoral pressure. In particular, we expect to see representatives involved in tight election contests and those hailing from economically distressed districts to engage in more strategic timing of their disclosures, including violating the STOCK Act to postpone disclosure of their trades until after the election.

An interesting institutional feature of our setting is that in addition to deciding on the *timing* of their disclosures, legislators also get to choose the *form* of their disclosure. Even though the Ethics Committee has encouraged electronic filing to enhance accessibility and transparency, legislators still decide whether to report their trades on paper forms or to use the electronic filing system. If legislators believe that filing paper forms will limit access to their reports, then we

[18] To date, no legislator has been forced to resign because of insider trading activities or violations of the STOCK Act's disclosure rules. Most congressional stock trades (87%) are disclosed in compliance with the Stock Act (i.e., within 45 days of execution). However, a significant percentage (1.27%) of trades that are executed at least 45 days before elections are disclosed after the election (i.e., for these trades legislators choose to violate the 45-day disclosure requirement and strategically delay disclosure until after the election). In non-election years, the comparable rate is 0.29%.

expect legislators to use paper forms for their most informed trades. If legislators report their informed trades on paper forms, then we expect these trades to be more profitable than trades filed using the electronic system.

Given the choice of disclosure form (paper vs electronic) and the lax enforcement of the timely disclosure requirements, it is feasible that observing politicians' disclosure behaviors could reveal their *type*, i.e., disclosure patterns reveal which politicians trade on private congressional knowledge.[19] We conjecture that politicians who report their trades on paper forms or have a proclivity for untimely disclosure (often reporting their trades more than 45 days after execution) are *Suspicious Politicians*, i.e., are likely to be the politicians who trade on private congressional knowledge and thus earn higher abnormal returns than their peers. This leads to our final hypothesis:

H3: Trades executed by suspicious politicians – identified through disclosure timing and form – are more profitable than trades by their peers.

3 Data and Sample

The primary data used in this paper are stock transactions by Members of Congress. Following the passage of the *STOCK Act of 2012*, Members of Congress have been required to publicly disclose their stock trades in a timely manner, specifically, within 45 days of execution. We obtain congressional trading data from the *Capitol Trades Database* provided by 2iQ Research, which compiles Periodic Transaction Reports (PTRs) submitted to the Clerk of the House of Representatives and U.S. Senate's Select Committee on Ethics. The *STOCK Act*, passed on April

[19] *Business Insider*'s "Conflicted Congress" project uses a set of six factors to assess potential conflicts of interest among lawmakers: (1) the **STOCK Act violations**, (2) **staffer's STOCK Act violations**, (3) **legible disclosures**, (4) amendment filings, (5) qualified blind trust, and (6) individual stock trades versus mutual funds. See details in https://www.businessinsider.com/financial-conflicts-congress-members-rated-2021-12?page=methodology. In a similar spirit, we formally test whether politicians with systematically questionable disclosure practices earn higher abnormal returns.

4th of 2012, initially mandated electronic filing and online posting of PTRs. However, a subsequent amendment to the Act adopted on April 15th, 2013, fundamentally altered several key provisions. The amendment eliminated the mandate to create a searchable, sortable, downloadable database of legislators' disclosures of their stock trades and eliminated the requirement of electronic filings (permitted legislators to continue to use paper forms). As a result, there is no consistent public record of PTRs for trades in 2012. The *Capitol Trades Database* and our sample period begins in 2013 and covers the 113th to the 118th Congresses (2013-2023).

We start with 129,517 stock transactions disclosed during this period. We exclude amended disclosures when the initial disclosure is available[20], aggregate same-day, same-stock, same-direction transactions by the same politician, drop trades with missing control variables. These steps reduce the sample to 93,376 trades, which we use as the baseline dataset. From this baseline, we construct subsamples tailored to specific tests. For instance, trades by politicians seeking re-election form the basis for our electoral pressure tests on trading profitability. Overall, 318 unique politicians – or about 33% of all serving politicians during the sample years - disclosed at least one stock transaction, in 2,895 unique stocks. Table 1 summarizes the construction of our sample and the derivation of test-specific subsamples.

We merge additional data with the *Capitol Trades Database* to construct needed variables. Stock return data comes from *CRSP* and accounting data from *Compustat*. Analyst coverage data are obtained from I/B/E/S. Legislators' characteristics, including serving sessions in Congress, home state, tenure and party affiliation are collected from the *Biographical Directory of the United States Congress*. Congressional committee membership data is shared by Professor Charles

[20] There are trades executed by the same politician, in the same stock, in the same direction, on the same day, but on different filings. In such cases, the second filing is regarded as an amendment, and only the trades reported in the initial filing are retained in the sample.

Stewart III from MIT's political science department. Data on Members of Congress who announce retirement or leave office early, as well as with primary election dates, come from *BallotPedia* and the Federal Election Commission (FEC). Election vote shares are obtained from the *CQ Press Voting and Election Collection Database* and supplemented with data from the Center for Effective Lawmaking Legislative Effectiveness Scores (LES) project. Congressional in-session dates are taken from historical floor calendars available on Congress.gov. Constituency-level economic characteristics (i.e., household income and unemployment rates) are sourced from the Bureau of Economic Analysis and the U.S. Census Bureau.

4 Empirical Results: Electoral Pressure and Politicians' Strategic Disclosure

We begin our empirical analysis by examining the *timing* of politicians' disclosures. First, we examine whether electoral pressure affects Members' disclosure of their profitable stock trades. Given heightened scrutiny from political opponents and voters before and during elections, we expect Members' disclosure of their profitable trades (i.e., those likely to have benefited from congressional knowledge) to vary with election cycles and to be delayed until after elections. Specifically, in section 4.1 we examine whether trades disclosed during electoral-pressure windows (i.e., the month immediately preceding primaries through election season) exhibit lower profitability than trades disclosed in other times. In section 4.2 we examine whether politicians, beyond Rep Mike Garcia, are willing to violate the law to delay disclosing their trades until after elections.

*4.1 **Profitability of Trades during Pressure Windows***

4.1.1 Research Design and Baseline Results

The sample of 87,932 transactions is used to examine whether Members of Congress selectively disclose less profitable stocks trades during the months leading up to their re-election

bids. Specifically, we compare the profitability of trades disclosed inside versus outside the electoral pressure window.

We construct a variable, *PressureWindow*, to capture periods when electoral pressure is heightened for legislators seeking re-election (or election to another office, e.g., House member to Senator, Governor, Attorney General, Vice President, etc.). For legislators from heavily partisan districts (i.e., safe Blue or Red), the primary election typically determines re-election outcomes, whereas general elections are more consequential in politically competitive districts (i.e., Purple). Accordingly, for each politician, the *PressureWindow* is defined as the period from one month prior to the primary election through the November general election. Legislators from states with early primaries therefore face longer *PressureWindow* than those from states with later primaries or run-off systems (e.g., Louisiana). Appendix B illustrates the variation in state-level primary schedules during the 2024 election cycle.

To measure profitability, we follow Belmont, Sacerdote, Sehgal, and Hoek (2020) and calculate the Cumulative Abnormal Returns (*CARs*) for each trade, denoted as $Profit$[21]. *CARs* are computed over 30-, 45-, and 60-calender-day event windows using the Fama-French four-factor model (Fama and French, 1993; Carhart, 1997). For purchases, $Profit$ equals the *CAR*; for sales, $Profit$ equals the negative of *CAR*, capturing losses avoided by selling prior to a price decline. The 45-day horizon is our primary measure, as it aligns with the STOCK Act's disclosure requirement, making it particularly relevant.[22]

[21] Following prior literature on corporate insider trading (e.g., Lakonishok and Lee, 2001) and on congressional trading (e.g., Belmont et al., 2020; Stephan et al., 2021), we do not weight profitability measures by transaction size.

[22] If a Member of Congress has private information from which they hope to profit, then they would initiate a transaction to front run and benefit from a price movement that will happen at a future point in time when the market learns what the Member already know. The ideal return window would cumulate returns until the public revelation of the legislators' private information. However, the timing of the public revelation is not obvious or systematic. Thus, we run our tests using alternative cumulation periods ranging from 30 to 60 days following the execution of the trade. Our findings are similar using these various cumulation periods.

Table 2 Panel A presents comparative statistics. Across all horizons, the average $Profit$ of trades disclosed during the *PressureWindow* are significantly negative, while the corresponding *CARs* are insignificantly different from zero in the *Non-PressureWindow*. The lack of significantly positive returns for political trades is in line with prior literature that documents no abnormal profits gained by congressional trades (e.g., Belmont et al., 2022). More importantly for this paper, the means differential in the *CARs _D30*, *_D45*, and *_D60* are lower by 0.21%, 0.27%, and 0.37%, respectively and are significantly different across *Pressure* and *Non-Pressure Windows*. Hence, the gap is driven by depressed *CARs* in the *PressureWindow* rather than unusually high *CARs* outside it. This univariate evidence suggests that legislators when facing electoral pressure strategically disclose their less profitable trades.

As a related observation, *Disclosure Lapse* – the days from execution to disclosure – averages 49 days in the *PressureWindow* versus 42 days otherwise, with STOCK Act violation rates are 15.9% versus 11.5%, respectively. These high levels of violations demonstrate weak enforcement of the STOCK Act. The longer lapses and higher violation rate under *PressureWindow* also point to strategic delays in disclosure, which we examine directly in the next section.

To formally test the effect of electoral pressure on the profitability of congressional trades disclosed, we estimate OLS-based regressions at the transaction level using the following specification:

$$Profit_{i,j,c} = \alpha + \beta_1 Pressure\ Window_{i,j,c} + \beta_k Controls + \delta_j + \gamma_c + \varepsilon_{i,j,c} \quad (1)$$

We include several control variables. Specifically, we add firm characteristics such as *ROA*, *Book to Market ratio*, market capitalization (*Size*), the number of analysts covering the traded stock (*#Analysts*). In addition, we also include an indicator for whether the traded firm is headquartered in the legislator's home state (*Home State*). Legislators trade local stocks either because they have

superior access to local information (Kaslovsky, 2022; Ma, Pan, Rouen, and Wellman, 2022) or for political purposes, such as signaling commitment to constituents rather than pursuing financial gains (Grimmer, Messing, and Westwood, 2012). Table 2 Panel A also reports comparative statistics for these control variables. Detailed definitions of all variables are provided in Appendix A. Finally, δ_j and γ_c denote politician and congressional session fixed effects, respectively, with standard errors clustered at both levels.

A negative and significant β_1 coefficient in Model (1) supports the hypothesis that legislators selectively disclose less profitable trades during periods of heightened electoral pressure. Table 2 Panel B reports results for politicians seeking re-election. Consistent with our expectation, the coefficients on *PressureWindow* are negative and significant across all *CAR* horizons. Specifically, congressional trades disclosed during the electoral pressure period yield between 0.2% and 0.4% lower *CARs* relative to trades disclosed outside this period.[23]

Most control variables show no systematic patterns, except for *HomeState*, which carries a negative coefficient. This suggests that legislators trading stocks of their home-state firms are not solely motivated by financial returns. Rather it seems likely legislators own these stocks in pursuit of political benefits gained by signaling commitment to local economic interests.

To strengthen our inference that strategic disclosure is tied to electoral incentives, we conduct a falsification test using the subsample of retiring politicians, as reported in Table 2 Panel C. Because retiring politicians face no re-election pressure, we do not expect them to engage in strategic disclosure. Consistent with this prediction, the estimated coefficients on *PressureWindow* are positive but insignificant. While the smaller sample size could reduce statistical power, the

[23] Our main results are robust if we instead use buy-and-hold abnormal returns (*BHARs*). We compute *BHARs* using two alternative benchmarks: (i) the CRSP value-weighted index, and (ii) the average buy-and-hold returns of the same industry-size portfolio. We use *CARs* in the main tests because they allow for cleaner identification of short- and medium-term abnormal performance and are less sensitive to compounding effects than *BHARs*.

opposite sign relative to the baseline test indicates that the null results are unlikely to be solely driven by low power. This finding supports the interpretation that only legislators facing electoral pressure strategically time their disclosures.

Finally, Table 2 Panel D presents a series of robustness checks based on *CAR_D45*. Results for *CAR_D30* and *CAR_D60* are qualitatively similar. Columns 1 and 2 restrict the sample to trades executed when Congress is in session.[24] Because congressional recesses often overlap with election campaigns, a concern is that lower profits during the *PressureWindow* reflect reduced congressional knowledge while legislators are away from Washington, rather than strategic disclosure. By focusing exclusively on trades executed while Congress is in session, we mitigate this concern. The coefficient on *PressureWindow* remains negative (-0.002) and significant, mirroring the baseline results.

Columns 3 and 4 address potential biases from overlapping *CARs*. When multiple trades occur close in time, return windows overlap, which understate standard errors and inflate statistical significance. To alleviate this concern, we retain only the earliest trade within each overlapping return window. Despite the reduction in sample size, the coefficient of interest remains negative (-0.004) and significant, consistent with the baseline findings.

Finally, Columns 5 and 6 use a subsample that controls for potential seasonality effects in congressional trading profitability. Specifically, we construct a pseudo-PressureWindow for non-election years using the same calendar dates as in election years and drop observations outside the pseudo-*PressureWindow* in non-election years. This approach allows for a direct comparison of

[24] To validate the underlying assumption that congressional members benefit from trading on congressional knowledge, we compare the abnormal returns of trades executed when Congress is in session versus out of session. *In session trades* have higher abnormal returns. These untabulated findings suggest that legislators' access to "congressional knowledge" enhances their trading outcomes. Such knowledge arises from ongoing legislative activities, policy negotiations, and privileged informational flows. This evidence is notable because prior literature concludes that congressional trades are on average unprofitable or indistinguishable from ordinary investors.

trades executed on identical calendar days across election and non-election years. The coefficient on *PressureWindow* remains negative (-0.003) and significant, consistent with the conclusion that the lower profitability of disclosed trades is attributable to electoral pressure rather than to seasonal patterns in legislators' trading activities.

4.1.2 Cross-sectional Analyses

To reinforce the baseline findings, we conduct cross-sectional analyses by splitting the sample along dimensions that are expected to amplify electoral pressure and re-estimate Model (1).

The first split distinguishes between *Vulnerable* and *Safe* legislators. *Vulnerable* legislators are defined as those whose vote shares in the upcoming election - either the primary or the general - fall below 60%, whereas *Safe* legislators surpass this threshold in both elections. This classification is anchored to the election that a legislator is about to face, so it aligns closely with the actual electoral pressure confronting legislators. In addition, this definition captures electoral vulnerability in both strongly partisan areas, where intra-party primary challenges are decisive, and in swing areas, where general election outcomes are highly competitive.

Next, we consider voter sentiment toward politicians profiting from their positions. A growing body of political science research shows that economic conditions shape voters' trust in government and perceptions of corruption. In particular, lower-income and economically distressed voters express stronger skepticism about political integrity and are more likely to punish officeholders they perceive as exploiting public office for private gain (Treisman, 2014; Duch and Stevenson, 2010). To capture this variation, we employ two district-level proxies: unemployment rate and household income. Survey evidence further supports that lower-income voters are more skeptical about government integrity and hold a stronger distrust towards politicians who misuse

their positions for personal gain.[25] Hence, legislators from *High Unemployment* (above-national-average unemployment rate) and *Low Income* (below-national-average household income) constituencies are expected to face greater electoral pressure to avoid disclosing profitable trades close to elections.

Table 3 presents the cross-sectional results. Columns 1 and 2 show that the negative effect of *PressureWindow* is concentrated among *Vulnerable* legislators. For this group, the coefficient is significantly negative and economically meaningful (coefficient = -0.005). In contrast, the coefficient for *Safe* legislators is not different from zero. The difference between these groups is statistically significant (*p*-value = 0.020). Columns 3 and 4 reports results for legislators from districts with varying unemployment levels. Consistent with expectations, legislators from *High Unemployment* constituencies disclose trades with significantly lower *CAR_D45* during the *PressureWindow* (coefficient = -0.004). By contrast, the coefficient for *Low Unemployment* constituencies is smaller and statistically insignificant. The difference between these groups is also statistically significant (*p*-value = 0.020). Columns 5 and 6 focus on household income. As expected, legislators from *Low Income* constituencies exhibit a significantly negative coefficient on *PressureWindow* (coefficient = -0.005). In contrast, the coefficient for *High Income* constituencies is statistically insignificant. The difference between the two groups is also statistically significant (*p*-value = 0.000).

[25] A recent TIMEs article highlights the widening divide in trust of institutions among top and bottom income brackets. Citing survey research (https://www.edelman.com/trust/2025/trust-barometer), the article notes that "48% of low-income respondents trust institutions, averaged across business, government, media, and NGOs—compared to 61%, on average, among high income respondents." https://time.com/7208097/the-precipice-of-a-grievance-based-society/, accessed February 22, 2025. Pew Research Center notes "among income groups, a majority of lower-income adults (61%) say government should do more to solve problems, while smaller shares of upper- and middle-income adults (53% and 48%, respectively) say the same." https://www.pewresearch.org/2024/06/24/governments-scope-efficiency-and-role-in-regulating-business/, accessed on February 22, 2025.

*4.2 **Elections and Violations of the Stock Act – Delayed Disclosure***

Section 4.1 examines how electoral pressure influences disclosure timing by analyzing the profitability of disclosed trades inside versus outside electoral pressure windows. While informative, this approach cannot fully disentangle whether electoral pressure affects actual trading decisions or the timing of disclosures, since most trades are typically disclosed within a short window of their execution. In this section, we turn to a more direct test of disclosure timing. If disclosure timing is shaped by electoral incentives, we expect politicians to exploit the weak enforcement of the STOCK Act by strategically delaying the disclosure of their trades until after elections. We therefore examine whether legislators are willing to violate the STOCK Act by postponing disclosures beyond the statutory deadline in order to avoid scrutiny during election periods.

4.2.1 Research Design and Baseline Results

We construct a subsample that allows a direct comparison of disclosure behaviors in election versus non-election years. The treatment group consists of trades executed at least 45 days before the election day in election years, while the control group includes trades executed at least 45 days prior to pseudo-election days in non-election years (i.e., the corresponding date, the first Tuesday after the first Monday in November). Under the STOCK Act, such trades should be disclosed prior to the election (or pseudo-election) date. By construction, any trade disclosed after election day (or pseudo-election day) necessarily violates the Act's disclosure requirement. This design enables an empirical test of whether legislators are willing to incur such violations in order to delay disclosure until after elections. Non-election years serve as the natural benchmark for comparison.

To ensure that delayed disclosures are plausibly motivated by electoral considerations, we exclude trades disclosed more than three months after the election (or pseudo election) date, as extremely late disclosures are unlikely to be election driven. The final subsample consists of 61,217 Congressional trades, of which 28,382 are executed at least 45 days before an election day and 32,835 before a pseudo-election date. Descriptive statistics for this subsample are reported in Table 4 Panel A.

Using this subsample, we estimate the following regression:

$$Delayed\ Disclosure_{i,j,c} = \alpha + \beta_1 Elect\ Year_{i,j,c} + \beta_k Controls + \delta_j + \gamma_c + \varepsilon_{i,j,c} \quad (2)$$

The dependent variable, *Delay Disclosure*, is an indicator set to one when a trade is disclosed after the actual or pseudo election day. The key independent variable, *Elect Year*, is an indicator for whether the trade is executed in an election year or not. This specification directly tests whether politicians are more likely to strategically delay trade disclosures until after election when facing electoral pressure. Control variables, fixed effects, and clustering follow those in Model (1). A positive and significant β_1 would support our expectation.

Table 4 Panel B presents the baseline results. The coefficient on *Elect Year* is positive and significant, indicating that legislators are more likely to violate the STOCK Act's disclosure requirement by delaying their trade disclosures until after election day during election years relative to non-election years. Economically, although delayed disclosure is relatively rare - only 13% of trades in the sample are disclosed more than 45 days after execution - a 0.5 percentage point increase in the likelihood in election years relative to non-election years is notable.

4.2.2 Cross-sectional Analyses

To solidify the interpretation of the baseline results, we conduct cross-sectional analyses based on trading profitability, electoral vulnerability and voter sentiment.

Table 5 Columns 1 and 2 split the sample into *Profitable* and *Nonprofitable* trades, based on positive or negative *CAR_D45*. The coefficients on *ElectYear* are positive and significant in both groups, but the effect is larger for *Profitable* trades (0.006 vs. 0.004). The difference in coefficients between the two groups is marginally significant ($p = 0.100$). This finding is consistent with politicians more inclined to delay the disclosure of profitable trades, which are more likely to draw scrutiny and criticism from political rivals.

Columns 3 and 4 show results by electoral vulnerability. *Vulnerable* politicians exhibit a stronger and statistically significant tendency to delay disclosures in election years, whereas the coefficient for *Safe* politicians is smaller and insignificant. The difference between the two groups is statistically meaningful (p-value = 0.010), indicating that electoral pressure affects disclosure timing behavior for politicians more at risk of losing their seats.

Columns 5 to 8 examine voters' resentment, proxies by district-level unemployment and income conditions. Political scandals involving congressional insider trading are likely to generate greater backlash in areas of high unemployment and low household income. Results reveal that politicians from *High Unemployment* and *Low Income* areas significantly delay disclosures in election years, while the coefficients for *Low Unemployment* and *High Income* areas are insignificant. The difference in coefficients for *Low Income* vs *High Income* is significant (p-value = 0.010).

5 *Form* of Disclosure: Paper Forms vs Electronic Filings

In addition to choosing the *timing* of their disclosures, politicians also choose the *form* of their disclosures - whether to file a paper form or to use the electronic filing system to report their trading activities. In concert with the passage of the *Stock Act* in 2012, the House Ethics Committee issued guidelines that "strongly encourage[d] all filers to use the electronic filing system" noting

that doing so would "reduce errors and greatly simplify future filings". Despite the Ethic Committee's guidelines, a nontrivial share of trading activities is reported on paper forms throughout the sample period. Additionally, compared to electronic filings, paper forms bundle together substantially more transactions (an average of 22.8 transactions for paper forms vs. only 8.2 for electronic filings) and report transactions of larger dollar value. This suggests that politicians strategically bundle together their larger trades and report them in less accessible formats.

Table 6 Panel A documents the time trend in legislators' choice of the form of their disclosure – paper forms or electronic filings. In 2013 no transactions were filed electronically. Two years later the majority were electronic filings (58.7% in 2015). It is feasible that politicians who continued to use paper forms (rather than electronic filings) did so to make access to their disclosures more difficult for the public and the press.[26] If this is the case, then we expect "paper trades" to be more informed and thereby more profitable than those disclosed using electronic filings.

In Panel B of Table 6 we examine whether the profitability differs for trades disclosed on paper forms vs those filed electronically. Noting that it took a few years for electronic filings to become the norm, we conjecture that a politician's decision to use paper forms later in the sample period is a more strategic disclosure decision. To test this conjecture, we sequentially drop years from early in the transition period and reassess difference in profitability of trades reported with different disclosure methods. When we drop the first three years of the sample, 2013-2015, *CAR_D45* and *CAR_D60* become significantly more positive for trades disclosed on paper form

[26] See a related news article: https://readsludge.com/2019/11/13/ethics-committee-republicans-defy-committees-own-financial-disclosure-recommendations/?utm_source=chatgpt.com. As of 2025, paper filings have not been fully phased out.

compared to those filed electronically, with a mean difference of 15-16 basis points. Going down the rows in Panel C, it is noteworthy that the mean difference demonstrating the superior profits of trades disclosed on paper forms reach 23-25 basis points. One interpretation of the findings in Panel C is that legislators who continue to use paper forms to disclosure their trades enjoy superior returns because their trades are informed by congressional knowledge.

6 Suspicious Politicians

Having demonstrated that *some* politicians strategically disclose trading activity (use paper forms and delay disclosing beyond the 45-day requirement), in this section we review individual politicians' disclosure choices and identify those politicians whose systematic disclosure behaviors raise suspicions that they trade on congressional knowledge. Once identified as *Suspicious Politicians*, we next examine whether their trades generate higher abnormal returns than their peers.

Our approach follows Cohen, Malloy, and Pomorski (2012), who distinguish "opportunistic" from "routine" insiders at the trader level. Here, we classify legislators using two disclosure-based criteria. First, based on disclosure *timing*, we set the indicator variable, *Suspicious_1*, equal to one if a politician's propensity to violate the STOCK Act's 45-day disclosure rule is above the sample median (13% of the trades). Second, based on disclosure *form*, we set the indicator variable, *Suspicious_2*, equal to one if a politician continued to use paper filings in 2017 or later, despite the widespread adoption of electronic filings. Because the second classification requires observing a politician's filing method after 2017, it necessarily reduces the sample size. We also define a *Suspicious Superset*, capturing members flagged under either criterion.

Table 7 presents regression results with the dependent variable, *CAR_D45*, regressed on these alternative indicators of suspicious disclosure behaviors. For each we find that trades by suspicious politician earn significantly higher abnormal returns than their peers. Under *Suspicious_1* and *Suspicious_2* their trades earn about 0.2% higher abnormal returns, while trades by the *Suspicious Superset* earn a slightly larger premium, 0.3%. Table 7 results are consistent with the idea that suspicious disclosure practices are not random but instead point to politicians informed trading.

7 Additional Analysis

To complement the earlier findings that politicians disclose lower profit trades during electoral pressure windows, in this section we examine whether electoral pressure also affects the *volume* of congressional trades disclosed during pressure periods. Given heightened scrutiny from political opponents and voters before and during elections, legislators are expected to scale back their disclosure activity to reduce visibility.

7.1 Research Design and Baseline Results

To examine whether electoral pressure also affects the volume of congressional trades disclosed, we construct a politician-month dataset, aggregating each legislator's disclosed transactions within a calendar month. To ensure meaningful variation, we restrict the sample to legislators who disclosed at least one trade during the sample period and include only the months in which they served in Congress, thereby forming a balanced politician-month panel.

We employ two measures of politicians monthly trading volume: (1) *Transact_$Vol*, the total dollar value of disclosed transactions, and (2) *Transact_Num*, the number of disclosed transactions. Reported dollar amounts are based on categorical ranges (see the range categories in the examples of paper filings in Appendix C Panel A). We assign the midpoints of each range as

the transaction value, except for the open-ended top category ("Over $1,000,000"), where we conservatively use the lower bound. Descriptive statistics for our dataset of politician monthly trading volume are presented in Table 8 Panel A. The distributions of both volume measures are highly positively skewed with many zeros and extreme, positive values, reflecting the fact that the majority of legislators do not trade stocks each month. Instead, they tend to trade relatively infrequently, but in some instances when they do trade in relatively larger amounts.

To account for the prevalence of zeros and the skewness in positive values, we estimate regressions using a Poisson pseudo-maximum likelihood (PPML) estimator in the following specification:

$$Volume_{j,t} = \alpha + \beta_1 PressurePeriod_{j,t} + \beta_k Controls + \delta_j + \gamma_y + \varepsilon_{j,t} \quad (3)$$

The key independent variable, $PressurePeriod_{j,t}$, equals one for months spanning from one month before the primary election through the general election month (i.e., November) in election years, and zero otherwise. Control variables include *Seniority* (indicator for serving in Congress more than four years), *Powerful* (indicator for party leaders, committee chairs, or membership on powerful committees such as Ways and Means[27]). At the market level, we control for aggregate market return (*Mkt_Ret*) and market volatility (*Mkt_Volatility*) to capture market-wide conditions. We also include politician (δ_j) and year (γ_y) fixed effects. Standard errors are two-way clustered at the politician and year levels.

A negative and significant β_1 coefficient on *PressurePeriod* in Model (3) indicates that legislators disclose fewer trades when facing electoral pressure. Results reported in Panel B of Table 8 confirm this prediction. In Column 1, the coefficient on *PressurePeriod* for *Transact_$Vol* is -0.152, implying a 14% (i.e., $(e^{-0.152} - 1) \times 100\%$) decline in total disclosed dollar volume

[27] We follow the political science literature to define *Powerful*. Specifically, our method is the same as Legislative Effective Scores (LES). See details in https://thelawmakers.org/data-download.

during election pressure months. This effect is stronger for *Transact_Num* in Column 2, where the coefficient on *PressurePeriod* of -0.152, which translates to a 19% reduction in the number of disclosed transactions[28]. To mitigate possible concerns with classify the entire month of November as part of *PressurePeriod*, when several weeks that fall after the election day[29], we re-estimate Model (3) using a politician-biweekly dataset. In the biweekly data only the first half of November is classified as part of the pressure window, the second half of November is classified as non-pressure. Results are presented in Columns 3 and 4 of Panel B. We continue to find a negative and significant coefficient on *PressurePeriod*, demonstrating the robustness of the findings.

7.2 Cross-sectional Analyses

To reinforce the baseline findings, in Table 9 we present several cross-sectional analyses, re-estimating Model (3) for the same subsamples reported earlier (Table 3). Panel A of Table 9 demonstrates that the decline in disclosed trading activity during *PressurePeriod* is concentrated among *Vulnerable* legislators. For this group, the coefficients are significantly negative and economically meaningful (-0.202 for *Transact_$Vol* and -0.278 for *Transact_Num*), whereas the estimates for *Safe* legislators are small and insignificant. The differences between the groups are statistically significant (p-value = 0.010). These results support that legislators facing closer electoral contests are more likely to reduce their disclosed trading activity.

Panel B splits the sample by unemployment rate and reveals a similar pattern. Legislators from *High Unemployment* districts disclose significantly fewer trades during pressure months (-0.202 for *Transact_$Vol* and -0.275 for *Transact_Num*), while those from *Low Unemployment* districts show no comparable reduction. The differences across subsamples are highly significant.

[28] Untabulated results for a falsification test using the subsample of retiring politicians also support that our expectation. There is no strategic reduction in disclosure for them before elections.

[29] Untabulated results show that if we instead classify the entire month of November in election years as a non-pressure period, the regression coefficients on *Pressure Period* remain negative and significant.

Panel C examines household income and provides mixed evidence. Legislators from *Low Income* districts exhibit significantly lower dollar volume of disclosed trades in pressure months (-0.233 for *Transact_$Vol*), consistent with our expectation that electoral pressure is stronger when voters are economically distressed. In contrast, the findings for the number of trades (*Transact_Num*) indicate that both *Low* and *High Income* legislators reduce disclosure of trading activity in pressure periods.

Taken together, our examination of the strategic reduction in disclosures of trading volume in election-based pressure periods reinforces our interpretation of the findings presented in Section 4 that legislators strategically disclose their less profitable stock trades in response to electoral pressure.

8 Conclusion

Our study highlights a critical and previously underexplored dimension of congressional stock trading: Members of Congress strategically disclosure stock trades in response to electoral pressures. While prior research largely focused on documenting whether political trades earn abnormal returns to establish whether Members of Congress trade on privileged information, we show that *how* and *when* legislators disclose their trades is itself a central mechanism through which they manage political risk and avoid accountability.

We provide several novel findings. First, we document that trades disclosed during electoral pressure windows – defined as the months leading up to primaries through the general election – exhibit significantly lower abnormal returns. This pattern suggests that legislators take advantage of lax enforcement of disclosure requirements and strategically delay the disclosure of their profitable trades until after elections, thereby reducing the risk of attracting public scrutiny when voter attention and political opposition are at their peak. We provide complementary

evidence that politicians strategically scale back both the number and the dollar value of trades disclosed during electoral pressure periods. These reductions in profits and volume are concentrated among electorally vulnerable politicians and those representing economically distressed constituencies, where voters' resentment of elite politicians' self-dealing is stronger. Second, we directly demonstrate that legislators are willing to violate the law to delay disclosing trades prior to elections. This pattern indicates that political incentives outweigh legal compliance, and importantly, that politicians – through their disclosure choices – are able to avoid accountability to their constituents.

Third, we examine the *form* of disclosure and find that paper versus electronic filings provide additional insight into politicians' strategic behavior. Despite guidance from the Ethics Committee strongly encouraging electronic filing, some legislators continued to rely on paper forms long after electronic filings became the norm. Compared to electronic filings, disclosures on paper lack standardization and digital searchability, and are associated with greater evidence of information advantage, i.e., higher abnormal returns. This suggests that disclosure form can be opportunistically chosen to obscure trading activity.

Fourth, moving from transaction level data to the trader level, we classify legislators based on their suspicious disclosure behaviors. Legislators with a higher propensity to violate the 45-day disclosure requirement as well as legislators who continued using paper filings after 2017 are identified as "suspicious", and we demonstrate that their trades earn significantly higher abnormal returns than those of their peers.

Our findings contribute to the broader literature on politicians' insider trading and disclosure regulation by shifting the focus from trading outcomes alone to include their choice of disclosure practices. From a policy perspective, the persistence of late filings, and continued use

of paper forms underscore the urgent need to strengthen enforcement of the STOCK Act and to mandate machine-readable electronic filings and the creation of easily accessible databases of congressional trades. Lax enforcement of the existing laws allows legislators – through their strategic disclosure choices – to avoid accountability to their own constituents. Addressing loopholes and lax enforcement is vital to ensure that Members of Congress are held to the same, if not higher, standards of transparency and ethical conduct as corporate insiders.

References

Abdurakhmonov, M., Snider, R. E., Ridge, J. W., & Hasija, D. (2023). Perceptions of political Self‐Dealing? An empirical investigation of market returns surrounding the disclosure of politician stock purchases. *Strategic Management Journal*, *44*(5), 1168-1198.

Baldauf, M., Favilukis, J. Y., Garlappi, L., & Zheng, K. (2025). Profiting from real estate: So easy a Congressman can do it. *Available at SSRN 3801378*.

Belmont, W., Sacerdote, B., Sehgal, R., & Van Hoek, I. (2020). *Relief rally: Senators as feckless as the rest of us at stock picking* (No. w26975). National Bureau of Economic Research.

Carhart, M. M. (1997). On persistence in mutual fund performance. *The Journal of finance*, 52(1), 57-82.

Cherry, I., Heitz, A., & Jens, C. (2017, July). Change in capitol: How a 60 Minutes exposé and the STOCK Act affected the investment activity of US Senators. In *28th Annual Conference on Financial Economics and Accounting*.

Christensen, D. M., Mikhail, M. B., Walther, B. R., & Wellman, L. A. (2017). From K Street to Wall Street: Political connections and stock recommendations. *The Accounting Review*, *92*(3), 87-112.

Cohen, L., Malloy, C., & Pomorski, L. (2012). Decoding inside information. *The Journal of Finance*, *67*(3), 1009-1043.

Duch, Raymond M., and Randy Stevenson. "The global economy, competency, and the economic vote." *The Journal of Politics* 72.1 (2010): 105-123.

Eggers, Andrew C., and Jens Hainmueller. "Capitol losses: The mediocre performance of Congressional stock portfolios." *The Journal of Politics* 75, no. 2 (2013): 535-551.

Eggers, Andrew C., and Jens Hainmueller. "Political capital: Corporate connections and stock investments in the US congress, 2004-2008." *Quarterly Journal of Political Science* (2014): 2012-26.

Fama, E. F., & French, K. R. (1993). Common risk factors in the returns on stocks and bonds. *Journal of financial economics*, 33(1), 3-56.

Farhang, Sean. "Legislative Capacity & Administrative Power Under Divided Polarization." *Daedalus* 150.3 (2021): 49-67.

Gao, M., & Huang, J. (2016). Capitalizing on Capitol Hill: Informed trading by hedge fund managers. *Journal of Financial Economics*, *121*(3), 521-545.

Grimmer, J., Messing, S., & Westwood, S. J. (2012). How words and money cultivate a personal vote: The effect of legislator credit claiming on constituent credit allocation. *American Political Science Review*, *106*(4), 703-719.

Hanousek Jr, J., Jo, H., Pantzalis, C., & Park, J. C. (2023). A dilemma of self-interest vs. ethical responsibilities in political insider trading. *Journal of Business Ethics*, *187*(1), 137-167.

Huang, R., & Xuan, Y. (2020). *'Trading' Political Favors: Evidence from the Impact of the STOCK Act*. SSRN.

Jagolinzer, A. D., Larcker, D. F., Ormazabal, G., & Taylor, D. J. (2020). Political connections and the informativeness of insider trades. *The Journal of Finance*, *75*(4), 1833-1876.

Karadas, Serkan, Minh Tam Tammy Schlosky, and Joshua Hall. "Did politicians use non-public macroeconomic information in their stock trades? Evidence from the STOCK Act of 2012." *Journal of Risk and Financial Management* 14.6 (2021): 256.

Kaslovsky, J. (2022). Senators at home: Local attentiveness and policy representation in congress. *American Political Science Review*, *116*(2), 645-661.

References (continued)

Lindstädt, R., & Vander Wielen, R. J. (2011). Timely shirking: time-dependent monitoring and its effects on legislative behavior in the US Senate. *Public Choice*, *148*, 119-148.

Ma, M., Pan, J., Rouen, E., & Wellman, L. (2022). *Legislators' Demand for Firms' Financial Statements: Evidence from U.S. Congressional Redistricting Events* (Working Paper 22-058). Harvard Business School.

Mann, Thomas E., and Norman J. Ornstein. *It's even worse than it looks: How the American constitutional system collided with the new politics of extremism*. Basic Books, 2016.

Mehta, M. N., Srinivasan, S., & Zhao, W. (2020). The politics of M&A antitrust. *Journal of Accounting Research*, *58*(1), 5-53.

Schroeder, J. L. (2014). Taking stock: Insider and outsider trading by congress. *Wm. & Mary Bus. L. Rev.*, *5*, 159.

Shepsle, K. A., Van Houweling, R. P., Abrams, S. J., & Hanson, P. C. (2009). The senate electoral cycle and bicameral appropriations politics. *American journal of political science*, *53*(2), 343-359.

Stephan, A. P., Walther, B. R., & Wellman, L. A. (2021). Profiting from connections: Do politicians receive stock tips from brokerage houses? *Journal of Accounting and Economics*, *72*(1), 101401.

Tahoun, A. (2014). The role of stock ownership by US members of Congress on the market for political favors. *Journal of Financial Economics*, *111*(1), 86-110.

Treisman, Daniel. "Income, democracy, and leader turnover." *American Journal of Political Science* 59.4 (2015): 927-942.

Treul, S., Thomsen, D. M., Volden, C., & Wiseman, A. E. (2022). The primary path for turning legislative effectiveness into electoral success. *The Journal of Politics*, *84*(3), 1714-1726.

Volden, Craig, and Alan E. Wiseman. 2014. *Legislative Effectiveness in the United States Congress: The Lawmakers*. New York: Cambridge University Press; updated at www.thelawmakers.org.

Volden, Craig, and Alan E. Wiseman. 2018. "Legislative Effectiveness in the United States Senate," *Journal of Politics* 80(2): 731-735; updated at www.thelawmakers.org.

Ziobrowski, A. J., J. W. Boyd, P. Cheng, and B. J. Ziobrowski. 2011. "Abnormal Returns From the Common Stock Investments of Members of the US House of Representatives." *Business and Politics* 13 (1): 4.

Ziobrowski, A. J., P. Cheng, J. W. Boyd, and B. J. Ziobrowski. 2004. "Abnormal Returns from the Common Stock Investments of the US Senate." *Journal of Financial and Quantitative Analysis* 39 (4): 661–76

Appendix A
Variable definitions

This table provides detailed descriptions of key variables. The sample period is from 2013 to 2023. Data on congressional stock trades are from *2iQ Capitol Trades database*. Stock return data comes from *CRSP*; accounting data from *Compustat* and analyst coverage from *I/B/E/S*. Legislators' characteristics, including serving sessions in Congress, home state, tenure years, party affiliations are from *Biographical Directory of the U.S. Congress*. Congressional committee membership data is from Professor Charles Stewart from MIT. Information on members of Congress who left office early or announced retirement, as well as primary election dates is from *BallotPedia* and *the Federal Election Commission (FEC)*. Election vote share data is obtained from the *CQ Press Voting and Election Collection Databases* and supplemented with data from *the Center for Effective Lawmaking's Legislative Effectiveness Scores (LES)* project. Congressional in-session data are from historical floor calendars on *Congress.gov*. Constituency-level household income data, and unemployment rate are from *the Bureau of Economic Analysis* and *United States Census Bureau*. Continuous variables are winsorized at the 1st and 99th percentiles.

Dependent variable

Variable	Definition
CAR_D30 / CAR_D45 / CAR_D60	Cumulative abnormal returns over a 30- (45-/60-) calendar-day event window. Abnormal returns are determined as the difference between the stock's actual returns and the expected returns estimated using Fama-French plus momentum four-factor model. Execution day of a trade is designated as Day 0.
Transact_$VOL	The total dollar value of all trades disclosed by a politician during a month, where the dollar value is calculated using the midpoint of the reported value range. Politicians are only required to disclose transaction value ranges, not exact amounts. This variable is constructed at the politician-month level, and at the politician-biweekly level for robustness checks.
Transact_NUM	The total number of trades disclosed by a politician during a month. This variable is constructed at the politician-month level, and at the politician-biweekly level for robustness checks.
Delayed Disclosure	An indicator variable that equals 1 if a trade is disclosed more than 45 days after execution and after the first Tuesday following the first Monday in November and 0 otherwise. This date is designated by U.S. federal law as Election Day for federal offices. In election years, it represents the actual election date for politicians facing reelection, whereas in non-election years it serves as a pseudo-election date.

Appendix A (continued)
Variable definitions

Independent variable

Variable	Definition
PressureWindow	Electoral pressure window is defined as the period from one month before the primaries to the general election in election years for politicians seeking reelection. The indicator variable equals 1 if a trade is disclosed during this window and 0 otherwise.
PressurePeriod	An indicator equal to one if a month falls between the month of the primary election and November (inclusive) in an election year, and zero otherwise. This variable is constructed at the politician-month level and only applies to politicians who are running for reelection.
ElectYear	An indicator variable that equals 1 if a trade is executed in an election year and 0 otherwise.
Suspicious_1	An indicator equal to one if a politician's propensity to disclose trades late exceeds the sample median (13%), and zero otherwise. This variable is constructed at the politician level.
Suspicious_2	An indicator equal to one if a politician continues to use paper filings even in 2017 or later, and zero otherwise. This variable is constructed at the politician level.
Seniority	An indicator equal to one if a politician has served in Congress for more than four years as of the observed month, and zero otherwise.
Powerful	An indicator equal to one if a politician holds a leadership or influential position, and zero otherwise. Specifically, a politician is considered powerful if they serve as Speaker, Majority Leader, Minority Leader, Committee Chair, or if they sit on a powerful committee such as Appropriations, Ways and Means.
Mkt_Ret	The monthly return of the overall stock market, sourced from the monthly CRSP value-weighted index.
Mkt_Volatility	The monthly volatility of the overall stock market, measured as the standard deviation of daily returns within the month, using the daily CRSP value-weighted index.
ROA	Traded firm's net income for the current fiscal year scaled by total assets at the prior year-end.
Book-to-mkt	Traded firm's book value of assets divided by its market capitalization at the prior year-end.
Size	The natural logarithm of the traded firm's market value of assets at the prior year-end.
Leverage	Traded firm's book value of total debt to total assets
#Analyst	The number of analysts following the traded firm in the trading year. Logged transformation is used in regression models.
HomeState	An indicator that equals 1 if the traded firm is headquartered in the politician's home state and 0 otherwise.

Appendix A (continued)
Variable definitions

Other variables

Variable	Definition
Disclosure_Lapse	The number of days between the execution date of a trade and its disclosure date.
Violation	An indicator that equals 1 if a trade violates the 45-day disclosure requirement under the STOCK Act, and 0 otherwise.
Profitable	An indicator variable that equals 1 if a trade's *CAR_D45* is positive, and 0 if negative.
Vulnerable	An indicator variable that equals 1 if a trade is made by vulnerable politicians, and 0 otherwise. Electoral vulnerability is determined based on the results of the upcoming election. A politician is classified as "*Vulnerable*" if their vote share in either the primary or the general election falls below 60%, whereas a politician whose vote shares exceed 60% in both elections is classified "*Safe*".
Low_Income	An indicator variable that equals 1 if a trade is made by a politician who hails from a district or state with the average household income falling below the national average in the year of the trade; otherwise, it is classified as "*High Income*".
High_Unemploy	An indicator variable that equals 1 if a trade is made by a politician who hails from a district or state with a unemployment rate greater than the national average in the year of the trade; otherwise, it is classified as "*Low Unemployment*".

Appendix B

Example of primary dates

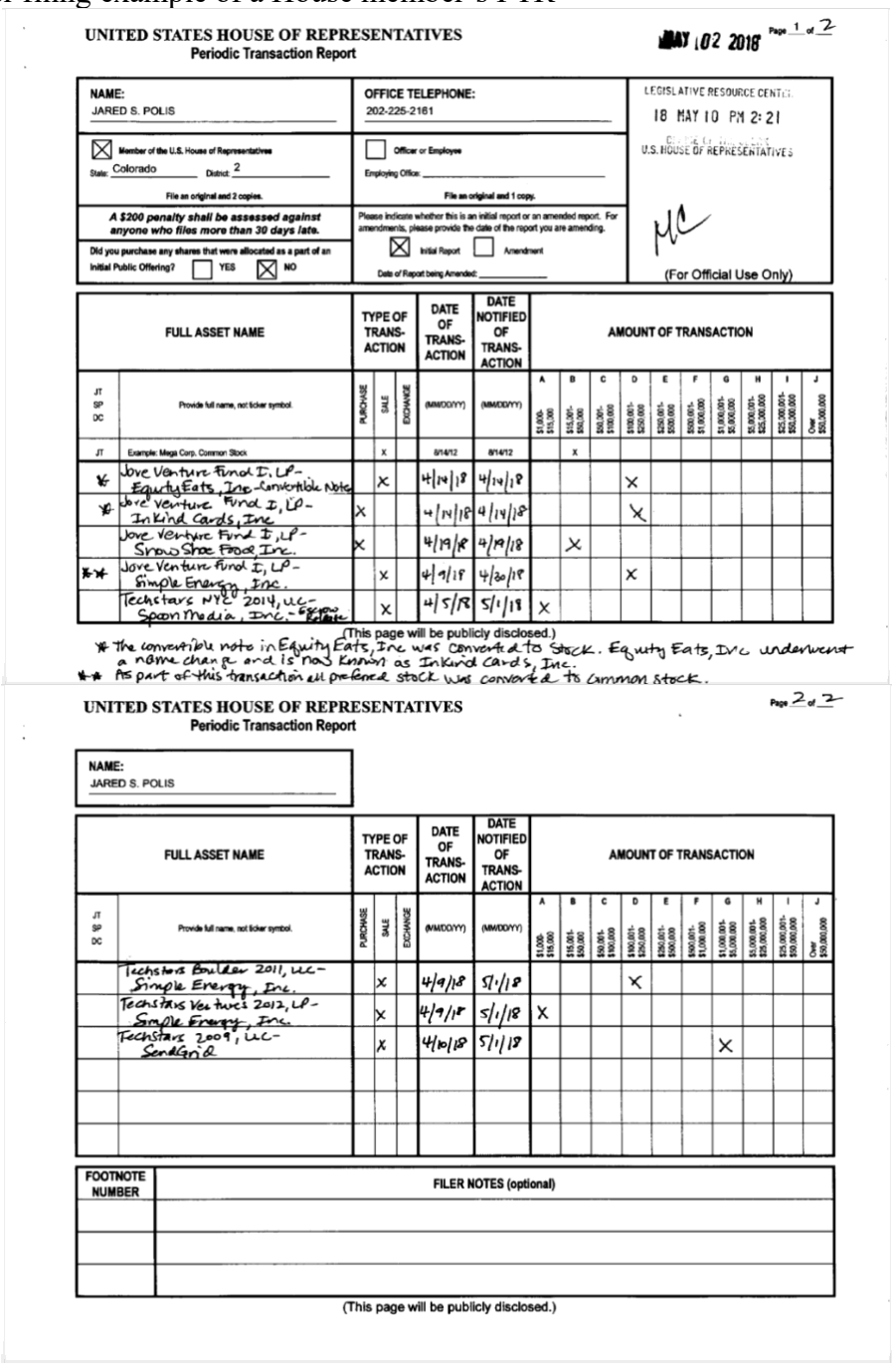
This appendix presents primary dates of all states for the 2024 election cycle. The timing of congressional primaries varies significantly across states and even within the same state across different election cycles. This variation is central to the construction of our key independent variable, *PressureWindow*.

State	Primary date	State	Primary date
Alabama	3/5/24	South Carolina	6/11/24
Arkansas	3/5/24	Oklahoma	6/18/24
California	3/5/24	Virginia	6/18/24
North Carolina	3/5/24	Colorado	6/25/24
Texas	3/5/24	New York	6/25/24
Illinois	3/19/24	Utah	6/25/24
Ohio	3/19/24	Arizona	7/30/24
Vermont	4/13/24	Tennessee	8/1/24
Pennsylvania	4/23/24	Kansas	8/6/24
Indiana	5/7/24	Michigan	8/6/24
Maryland	5/14/24	Mississippi	8/6/24
Nebraska	5/14/24	Missouri	8/6/24
West Virginia	5/14/24	Washington	8/6/24
Georgia	5/21/24	Hawaii	8/10/24
Idaho	5/21/24	Connecticut	8/13/24
Kentucky	5/21/24	Minnesota	8/13/24
Oregon	5/21/24	Wisconsin	8/13/24
Iowa	6/4/24	Alaska	8/20/24
Montana	6/4/24	Florida	8/20/24
New Jersey	6/4/24	Wyoming	8/20/24
New Mexico	6/4/24	Delaware	9/3/24
South Dakota	6/4/24	Massachusetts	9/3/24
Maine	6/11/24	New Hampshire	9/10/24
Nevada	6/11/24	Rhode Island	9/10/24
North Dakota	6/11/24	Louisiana	11/5/24

Appendix C

Examples of paper and electronic filings[1]

Panel A: paper filing example of a House member's PTR



UNITED STATES HOUSE OF REPRESENTATIVES
Periodic Transaction Report

MAY 02 2018 Page 1 of 2

NAME: JARED S. POLIS

OFFICE TELEPHONE: 202-225-2161

LEGISLATIVE RESOURCE CENTER
18 MAY 10 PM 2:21
U.S. HOUSE OF REPRESENTATIVES

☒ Member of the U.S. House of Representatives
State: Colorado District: 2
File an original and 2 copies.

☐ Officer or Employee
Employing Office:
File an original and 1 copy.

A $200 penalty shall be assessed against anyone who files more than 30 days late.

Please indicate whether this is an initial report or an amended report. For amendments, please provide the date of the report you are amending.
☒ Initial Report ☐ Amendment
Date of Report being Amended:

Did you purchase any shares that were allocated as a part of an Initial Public Offering? ☐ YES ☒ NO

(For Official Use Only)

FULL ASSET NAME		TYPE OF TRANSACTION			DATE OF TRANSACTION	DATE NOTIFIED OF TRANSACTION	AMOUNT OF TRANSACTION									
JT SP DC	Provide full name, not ticker symbol.	PURCHASE	SALE	EXCHANGE	(MM/DD/YY)	(MM/DD/YY)	A $1,001 - $15,000	B $15,001 - $50,000	C $50,001 - $100,000	D $100,001 - $250,000	E $250,001 - $500,000	F $500,001 - $1,000,000	G $1,000,001 - $5,000,000	H $5,000,001 - $25,000,000	I $25,000,001 - $50,000,000	J Over $50,000,000
JT	Example: Mega Corp. Common Stock		X		8/14/12	8/14/12		X								
*	Jove Venture Fund I, LP - EquityEats, Inc. - Convertible Note		X		4/14/18	4/14/18				X						
*	Jove Venture Fund I, LP - InKind Cards, Inc	X			4/14/18	4/14/18				X						
	Jove Venture Fund I, LP - Snow Shoe Food, Inc.	X			4/19/18	4/19/18		X								
**	Jove Venture Fund I, LP - Simple Energy, Inc.		X		4/9/18	4/20/18				X						
	Techstars NYC 2014, LLC - Spoon Media, Inc. - Equity Private		X		4/5/18	5/1/18	X									

(This page will be publicly disclosed.)

* The convertible note in EquityEats, Inc was converted to stock. EquityEats, Inc underwent a name change and is now known as InKind Cards, Inc.
** As part of this transaction all preferred stock was converted to common stock.

UNITED STATES HOUSE OF REPRESENTATIVES
Periodic Transaction Report

Page 2 of 2

NAME: JARED S. POLIS

FULL ASSET NAME		TYPE OF TRANSACTION			DATE OF TRANSACTION	DATE NOTIFIED OF TRANSACTION	AMOUNT OF TRANSACTION									
JT SP DC	Provide full name, not ticker symbol.	PURCHASE	SALE	EXCHANGE	(MM/DD/YY)	(MM/DD/YY)	A $1,001 - $15,000	B $15,001 - $50,000	C $50,001 - $100,000	D $100,001 - $250,000	E $250,001 - $500,000	F $500,001 - $1,000,000	G $1,000,001 - $5,000,000	H $5,000,001 - $25,000,000	I $25,000,001 - $50,000,000	J Over $50,000,000
	Techstars Boulder 2011, LLC - Simple Energy, Inc.		X		4/9/18	5/1/18				X						
	Techstars Ventures 2012, LP - Simple Energy, Inc.		X		4/9/18	5/1/18	X									
	Techstars 2009, LLC - SendGrid		X		4/10/18	5/1/18							X			

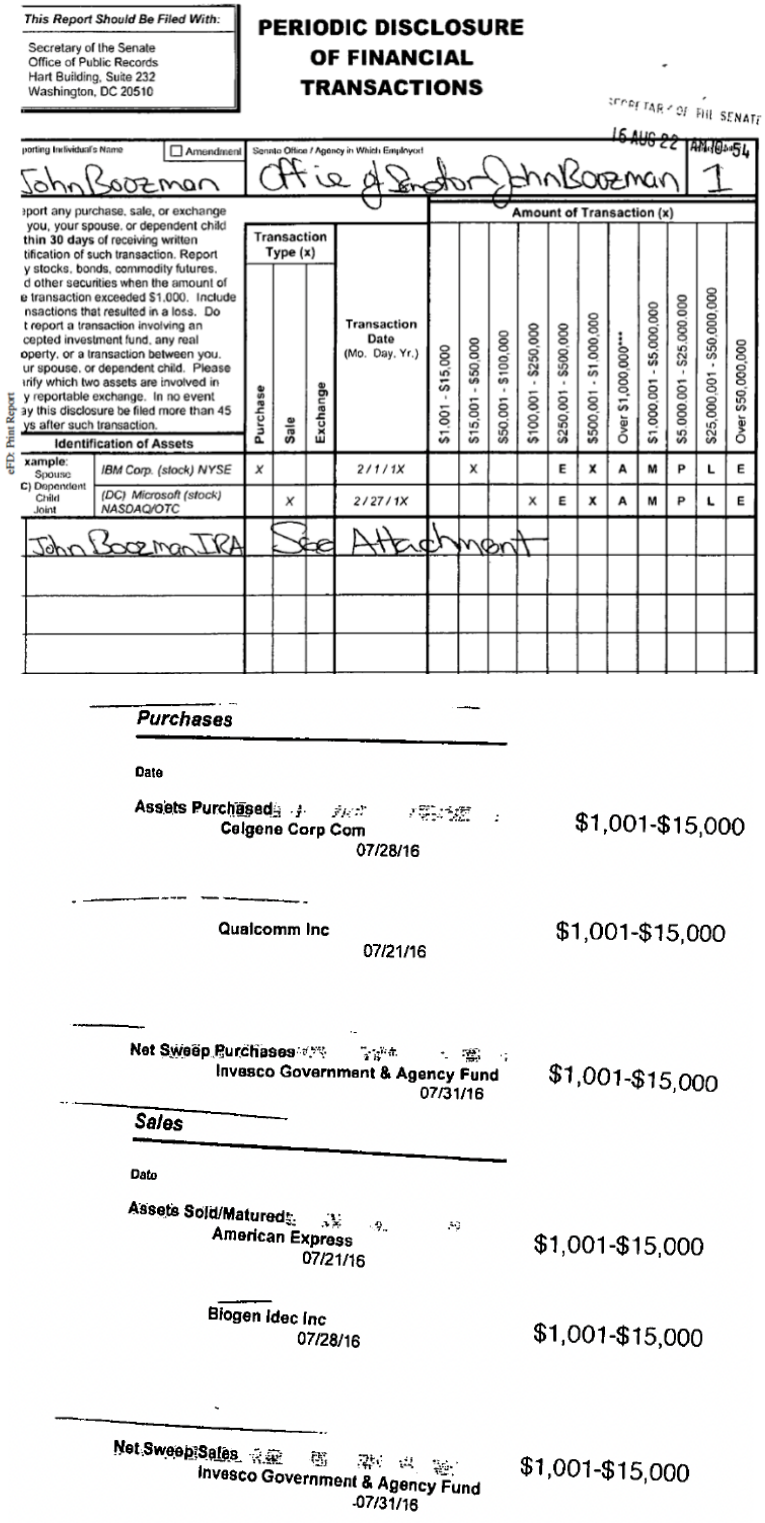
FOOTNOTE NUMBER	FILER NOTES (optional)

(This page will be publicly disclosed.)

[1] The House and Senate ethics offices use two separate systems for disclosing members' stock trades.
House system: https://disclosures-clerk.house.gov/FinancialDisclosure;
Senate system: https://efdsearch.senate.gov/search/home

Appendix C (continued)
Examples of paper and electronic filings

Panel B: paper filing example of a Senator's PTR



This Report Should Be Filed With:
Secretary of the Senate
Office of Public Records
Hart Building, Suite 232
Washington, DC 20510

PERIODIC DISCLOSURE OF FINANCIAL TRANSACTIONS

16 AUG 22 AM 10:54

Reporting Individual's Name: John Boozman
☐ Amendment
Senate Office / Agency in Which Employed: Office of Senator John Boozman
1

Report any purchase, sale, or exchange you, your spouse, or dependent child within 30 days of receiving written notification of such transaction. Report any stocks, bonds, commodity futures, and other securities when the amount of the transaction exceeded $1,000. Include transactions that resulted in a loss. Do not report a transaction involving an accepted investment fund, any real property, or a transaction between you, your spouse, or dependent child. Please identify which two assets are involved in any reportable exchange. In no event may this disclosure be filed more than 45 days after such transaction.

eFD: Print Report

Identification of Assets		Transaction Type (x)			Transaction Date (Mo. Day, Yr.)	Amount of Transaction (x)										
		Purchase	Sale	Exchange		$1,001 - $15,000	$15,001 - $50,000	$50,001 - $100,000	$100,001 - $250,000	$250,001 - $500,000	$500,001 - $1,000,000	Over $1,000,000***	$1,000,001 - $5,000,000	$5,000,001 - $25,000,000	$25,000,001 - $50,000,000	Over $50,000,000
Example: Spouse C) Dependent Child Joint	IBM Corp. (stock) NYSE	X			2/1/1X		X			E	X	A	M	P	L	E
	(DC) Microsoft (stock) NASDAQ/OTC		X		2/27/1X				X	E	X	A	M	P	L	E
John Boozman IRA			See Attachment													

Purchases

Date

Assets Purchased

Celgene Corp Com 07/28/16 — $1,001-$15,000

Qualcomm Inc 07/21/16 — $1,001-$15,000

Net Sweep Purchases

Invesco Government & Agency Fund 07/31/16 — $1,001-$15,000

Sales

Date

Assets Sold/Matured

American Express 07/21/16 — $1,001-$15,000

Biogen Idec Inc 07/28/16 — $1,001-$15,000

Net Sweep Sales

Invesco Government & Agency Fund 07/31/16 — $1,001-$15,000

Appendix C (continued)
Examples of paper and electronic filings

Panel C: electronic filing example of a House member's PTR

Filing ID #20006510

PERIODIC TRANSACTION REPORT

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

FILER INFORMATION

Name: Hon. Pete Sessions
Status: Member
State/District: TX32

TRANSACTIONS

ID	Owner	Asset	Transaction Type	Date	Notification Date	Amount
		Trinity Industries, Inc. (TRN) FILING STATUS: New	S	12/29/2016	12/29/2016	$15,001 - $50,000

INITIAL PUBLIC OFFERINGS

○ Yes ◉ No

CERTIFICATION AND SIGNATURE

☑ 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. Pete Sessions , 01/9/2017

Panel D: electronic filing example of a Senator's PTR

UNITED STATES SENATE
FINANCIAL DISCLOSURES

Return to the search tab to select another report.

Periodic Transaction Report for 01/07/2016

The Honorable Thomas R Carper (Former Senator)

Filed 01/07/2016 @ 12:57 PM

Print Report

The following statements were checked before filing:

☑ I certify that the statements I have made on this form are true, complete and correct to the best of my knowledge and belief. ☑ I understand that reports cannot be edited once filed. To make corrections, I will submit an *electronic* amendment to this report.

Transactions (6 transactions total) 0 Self 0 Joint 6 Spouse 0 Dependent Child

#	Transaction Date	Owner	Ticker	Asset Name	Asset Type	Type	Amount	Comment
6	12/23/2015	Spouse	--	Weight Watchers Intl Inc	Stock	Purchase	$1,001 - $15,000	--
5	12/08/2015	Spouse	--	Weight Watchers Intl Inc	Stock	Purchase	$1,001 - $15,000	--
4	12/01/2015	Spouse	--	Under Armour Inc Cl A	Stock	Purchase	$1,001 - $15,000	--
3	12/14/2015	Spouse	--	HSBC Contingent Autocall on Anadarko Petroleum *Rate/Coupon:* 9.00% *Matures:* 09/05/2017	Corporate Bond	Sale (Full)	$1,001 - $15,000	--
2	12/04/2015	Spouse	--	JPM Contingent Autocall on Amgen *Rate/Coupon:* 10.15% *Matures:* 08/31/2018	Corporate Bond	Purchase	$1,001 - $15,000	--
1	12/03/2015	Spouse	--	HSBC Contingent Autocall on Bristol Myers Squibb *Rate/Coupon:* 8.5% *Matures:* 08/31/2018	Corporate Bond	Sale (Full)	$1,001 - $15,000	--

Table 1
Sample Construction

Sample selection of trades Description	# Trades dropped	# Trades remaining
All stock trades		129,517
Exclude amended disclosure when an initial disclosure is available	(3,016)	126,501
Aggregating same-day, same-stock, same-direction trades by the same politician	(24,282)	102,219
Exclude trades with missing stock returns information	(6,639)	95,580
Exclude trades with missing control variables	(2,095)	93,485
Exclude singleton observations	(109)	93,376
Final sample of stock trades disclosed		93,376
Test-specific subsamples		
Trades made by politicians seeking re-election (Tables 2 and 3)		87,932
Trades used for *Delayed Disclosure* tests (Tables 4 and 5)		61,072
Trades used for *Suspicious Politician* tests (Table 7)		64,360
…Politician Months used for *Volume* tests (Tables 8 and 9)[a]		26,186
…Politician Biweekly obs. used for *Volume* tests (Table 8)[a]		53,472
Additional sample details		
# Unique stocks traded and disclosed		2,895
# Unique politicians in Congress during the sample years		960
# Unique politicians that disclosed at least one stock trades		318
Percentage		33%

[a] For the Volume tests we construct samples of politician-month and politician-biweekly observations that cover 318 unique politicians * the number of months (or biweeks) in which they served in Congress, yielding 26,186 politician-month and 53,472 politician-biweekly observations.

Table 2
Effect of electoral pressure on congressional trading profitability

This table examines whether legislators strategically disclose less profitable trades while engaged in re-election bids, *PressureWindow*. Panel A presents comparative statistics for observations in *Pressure Window* vs. *Non-Pressure Window*. Panel B reports the baseline results for politicians seeking re-election. Panel C provides results of a falsification test for retiring politicians. The dependent variables, *CAR_D30*, *CAR_D45*, and *CAR_D60*, are the cumulative abnormal returns over event windows of 30, 45, and 60 calendar days after the execution date, respectively, estimated using Fama-French four-factor model. The key independent variable, *PressureWindow*, is an indicator for whether a trade is disclosed by a politician under intensified electoral pressure. Panel D presents robustness tests. Columns 1 and 2 address concerns that the observed effect is driven by pre-election recesses rather than electoral pressure by restricting the sample to trades executed when Congress is in-session. To eliminate concerns that standard errors are understated, in Columns 3 and 4 we drop trades with overlapping event windows. Columns 5 and 6 addresses seasonality concerns by limiting the sample to only trades disclosed during the same calendar time in election and nonelection years. All variables are defined in Appendix A. The *t*-statistics are reported below coefficient estimates in parentheses. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively, using a two-tailed *t*-test.

Panel A: transactional level data to examine profitability test (comparative stats)

	PressureWindow (N=25,186)				*Non-PressureWindow* (N=61,751)				Diff in Mean
Variables	Mean	P10	Median	P90	Mean	P10	Median	P90	
Dependent variables									
CAR_D30	-0.0022***	-0.0995	-0.0005	0.0905	-0.0001	-0.0886	-0.0002	0.0871	-0.0021***
CAR_D45	-0.0026***	-0.1227	-0.0015	0.1142	0.0001	-0.1111	0.0001	0.1107	-0.0027***
CAR_D60	-0.0033***	-0.1386	-0.0021	0.1286	0.0004	-0.1268	0.0000	0.1268	-0.0037***
Independent variables									
ROA	0.0619	-0.0150	0.0550	0.1696	0.0669	-0.0074	0.0594	0.1760	-0.0050***
Book-to-Mkt	0.4855	0.0554	0.3225	0.9869	0.4893	0.0565	0.3186	1.0309	-0.0037
Size	10.5488	7.9457	10.6866	12.7453	10.6778	8.2909	10.7547	12.8159	-0.1290***
Leverage	0.2836	0.0425	0.2675	0.5259	0.2833	0.0415	0.2666	0.5211	0.0003
#Analyst	19.8687	1.0000	20.0000	36.0000	20.9512	2.0000	21.0000	38.0000	-1.0825***
HomeState	0.1083	0.0000	0.0000	1.0000	0.0874	0.0000	0.0000	0.0000	0.0209***
Others									
Disclosure_Lapse	49.3437	12.0000	28.0000	65.0000	42.7310	11.0000	27.0000	49.0000	6.6127***

Violation	0.1590	0.0000	0.0000	1.0000	0.1150	0.0000	0.0000	1.0000	0.0440***
Vulnerable	0.3676	0.0000	0.0000	1.0000	0.3640	0.0000	0.0000	1.0000	0.0037
High_Unemploy	0.8586	0.0000	1.0000	1.0000	0.7788	0.0000	1.0000	1.0000	0.0798***
Low_Income	0.6095	0.0000	1.0000	1.0000	0.6145	0.0000	1.0000	1.0000	-0.0049

Table 2 (continued)
Effect of electoral pressure on congressional trading profitability

Panel B: Baseline regression for politicians seeking re-election

		CAR_D45		*CAR_D30*	*CAR_D60*
	Pr. Sign	(1)	(2)	(3)	(4)
PressureWindow	-	-0.003**	-0.003***	-0.002**	-0.004**
		(-2.55)	(-2.59)	(-2.16)	(-2.37)
ROA			0.003	0.003	0.005
			(0.48)	(0.52)	(0.68)
Book-to-Mkt			0.001	0.001*	0.000
			(1.42)	(1.95)	(0.65)
Size			0.001	0.000	0.001
			(1.53)	(1.37)	(1.21)
Leverage			0.006	0.004	0.005
			(1.61)	(1.31)	(0.95)
#Analyst			-0.000	-0.000	-0.000
			(-0.69)	(-0.03)	(-0.68)
HomeState			-0.003*	-0.001	-0.004***
			(-1.88)	(-0.91)	(-2.60)
Politician FE		Yes	Yes	Yes	Yes
Congress FE		Yes	Yes	Yes	Yes
S.E. Cluster by Politician and Congress		Yes	Yes	Yes	Yes
No. of observations		87932	87932	87932	87932
Adj. R-Squared		0.0034	0.0036	0.0037	0.0036

Table 2 (continued)
Effect of electoral pressure on congressional trading profitability

Panel C: Falsification test – Retiring politicians

		CAR_D45		*CAR_D30*	*CAR_D60*
	Pr. Sign	(1)	(2)	(3)	(4)
PressureWindow	0	0.005	0.005	0.005	0.007
		(1.39)	(1.40)	(1.61)	(1.58)
ROA			-0.004	-0.002	0.011
			(-0.16)	(-0.13)	(0.27)
Book-to-Mkt			-0.000	0.000	0.001
			(-0.09)	(0.16)	(0.24)
Size			0.001	0.001	0.002
			(0.48)	(0.53)	(1.07)
Leverage			-0.002	0.002	0.000
			(-0.25)	(0.24)	(0.05)
#Analyst			-0.000	-0.000	-0.000
			(-0.64)	(-1.16)	(-0.80)
HomeState			0.001	0.005	-0.004
			(0.09)	(0.71)	(-0.63)
Politician FE		Yes	Yes	Yes	Yes
Congress FE		Yes	Yes	Yes	Yes
S.E. Cluster by Politician and Congress		Yes	Yes	Yes	Yes
No. of observations		5434	5434	5434	5434
Adj. R-Squared		0.0005	-0.0003	-0.0023	0.0026

Table 2 (continued)
Effect of electoral pressure on congressional trading profitability

Panel D: Robustness checks

Dependent variable:		*CAR_D45*					
Concerns:		"In Session" trades		Exclude transactions with overlapping return windows		Same calendar dates in election & non-election years	
	Pr. Sign	(1)	(2)	(3)	(4)	(5)	(6)
PressureWindow	-	-0.002*	-0.002*	-0.004*	-0.004*	-0.003*	-0.003*
		(-1.81)	(-1.82)	(-1.95)	(-1.95)	(-1.89)	(-1.90)
Control		No	Yes	No	Yes	No	Yes
Politician FE		Yes	Yes	Yes	Yes	Yes	Yes
Congress FE		Yes	Yes	Yes	Yes	Yes	Yes
S.E. Cluster by Politician and Congress		Yes	Yes	Yes	Yes	Yes	Yes
No. of observations		57673	57673	22735	22735	52876	52876
Adj. R-Squared		0.0040	0.0042	0.0047	0.0046	0.0052	0.0057

Table 3
Cross-sectionals: effect of electoral pressure on congressional trading profitability

This table examines whether Table 2 results are more pronounced for electorally vulnerable politicians and for politicians hailing from economically distressed districts. Columns 1 and 2 compare trades executed by vulnerable vs. safe politicians; Columns 3 and 4 (5 and 6) compare trades executed by politicians hailing from high and low unemployment districts (from low and high household income districts). *p*-values indicate whether differences in coefficients across subsamples are significant. *t*-statistics are reported below coefficient estimates in parentheses.

		CAR_D45					
Vulnerability		Vulnerable	Safe	High Unemploy	Low Unemploy	Low Income	High Income
	Pr. Sign	(1)	(2)	(3)	(4)	(5)	(6)
PressureWindow	-, 0	-0.005**	-0.001	-0.004***	-0.000	-0.005***	-0.000
		(-2.41)	(-1.11)	(-2.63)	(-0.06)	(-3.24)	(-0.19)
ROA		0.005	0.003	0.007	-0.009	0.005	0.002
		(0.33)	(0.55)	(0.95)	(-0.65)	(0.46)	(0.25)
Book-to-Mkt		0.001	0.000	0.001	0.002	0.002*	-0.000
		(1.12)	(0.80)	(1.07)	(1.09)	(1.77)	(-0.55)
Size		0.000	0.001	0.001*	-0.000	0.001*	0.000
		(0.40)	(1.55)	(1.72)	(-0.25)	(1.68)	(0.33)
Leverage		0.001	0.009***	0.006	0.008	0.007	0.005
		(0.09)	(3.21)	(1.34)	(1.23)	(1.20)	(1.32)
#Analyst		-0.000	-0.000	-0.000	0.000	0.000	-0.000*
		(-0.77)	(-0.04)	(-0.86)	(0.57)	(0.20)	(-1.91)
HomeState		-0.001	-0.003**	-0.003**	0.000	-0.003	-0.002
		(-0.38)	(-2.13)	(-2.01)	(0.08)	(-1.18)	(-0.82)
p-value for diff. in coef.		0.020		0.020		0.000	
Politician FE		Yes	Yes	Yes	Yes	Yes	Yes
Congress FE		Yes	Yes	Yes	Yes	Yes	Yes
S.E. Cluster by Politician and Congress		Yes	Yes	Yes	Yes	Yes	Yes
No. of observations		32095	55825	70568	17355	53900	34027
Adj. R-Squared		0.0045	0.0044	0.0024	0.0104	0.0057	0.0027

Table 4
Electoral pressure and delayed disclosure

This table examines the timing of stock disclosures, focusing on whether trades executed in election years are more likely to violate disclosure requirements of the STOCK Act. The sample includes trades executed more than 45 days before the election (or pseudo-election), ensuring that any post-election disclosure constitutes a violation of the disclosure requirement under the STOCK Act. Panel A reports the summary statistics of the subsample; Panel B presents the baseline regression results. The dependent variable, *Delayed_Disclosure*, is an indicator for whether a trade is disclosed after the election (or pseudo-election). The independent variable, *ElectYear*, is an indicator of whether a trade is executed in an election year. All variables are defined in Appendix A. The *t*-statistics are reported below coefficient estimates in parentheses. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively, using a two-tailed *t*-test.

Panel A: Descriptive statistics for trades executed more than 45 days before the election or pseudo election day

Variables	N	Mean	SD	P10	P25	P50	P75	P90
Dependent variables								
Delayed Disclosure	61072	0.0057	0.0753	0.0000	0.0000	0.0000	0.0000	0.0000
Independent variables								
ElectYear	61072	0.4624	0.4986	0.0000	0.0000	0.0000	1.0000	1.0000
ROA	61072	0.0645	0.0908	-0.0114	0.0158	0.0574	0.1077	0.1719
Book-to-Mkt	61072	0.4876	0.6110	0.0557	0.1497	0.3208	0.6074	1.0193
Size	61072	10.65	1.81	8.22	9.51	10.75	11.93	12.78
Leverage	61072	0.2852	0.1874	0.0453	0.1463	0.2676	0.3973	0.5268
#Analyst	61072	20.59	13.12	2	11	21	29	37
HomeState	61072	0.0941	0.2920	0.0000	0.0000	0.0000	0.0000	0.0000
Others								
Disclosure_Lapse	61072	29.75	26.1431	11	17	26	35	43
Violation	61072	0.0840	0.2774	0.0000	0.0000	0.0000	0.0000	0.0000
Profitable	61072	0.4989	0.5000	0.0000	0.0000	0.0000	1.0000	1.0000
Vulnerable	61072	0.3601	0.4800	0.0000	0.0000	0.0000	1.0000	1.0000
Low_Income	61072	0.6106	0.4876	0.0000	0.0000	1.0000	1.0000	1.0000
High_Unemploy	61072	0.8079	0.3940	0.0000	1.0000	1.0000	1.0000	1.0000

Table 4 (continued)
Electoral pressure and delayed disclosure

Panel B: baseline results

Dependent variable:		*Delayed Disclosure*	
	Pr. Sign	(1)	(2)
ElectYear	+	0.005**	0.005**
		(2.02)	(2.02)
ROA			-0.001
			(-0.32)
Book-to-Mkt			-0.001
			(-1.33)
Size			0.000
			(0.84)
Leverage			-0.003**
			(-2.17)
#Analyst			0.000*
			(1.69)
HomeState			-0.001
			(-1.57)
Politician FE		Yes	Yes
Congress FE		Yes	Yes
S.E. Cluster by Politician and Congress		Yes	Yes
No. of observations		61072	61072
Adj. R-Square		0.3123	0.3124

Table 5
Cross-sectionals effect of electoral pressure on delayed disclosure

This table examines whether Table 4 results are more pronounced for profitable trades, electorally vulnerable politicians and for politicians hailing from economically distressed districts. Columns 1 and 2 focus on trading profitability; Columns 3 and 4 compare electoral vulnerability; Columns 5 and 6 (7 and 8) compare disclosures of politicians hailing from high and low unemployment districts (from low and high household income districts). *p*-value for the differences in coefficients between subsamples are reported. The dependent variable, *Delayed_Disclosure*, is an indicator of whether a trade is disclosed after the election (or pseudo-election). The independent variable, *ElectYear*, is an indicator of whether a trade is executed in an election year. All variables are defined in Appendix A. The *t*-statistics are reported below coefficient estimates in parentheses. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively, using a two-tailed *t*-test.

Dependent variable:		*Delayed Disclosure*							
Subsample:	Pr. Sign	Profitable (1)	NonProfitable (2)	Vulnerable (3)	Safe (4)	Low Income (5)	High Income (6)	High Unemploy (7)	Low Unemploy (8)
ElectYear	+, 0	0.006**	0.004*	0.007*	0.004	0.005*	0.009	0.006**	0.006
		(2.09)	(1.90)	(1.88)	(1.12)	(1.81)	(1.16)	(1.98)	(0.67)
ROA		-0.004	0.002	-0.001	-0.003	0.003	-0.012*	0.001	-0.001
		(-0.96)	(0.61)	(-0.19)	(-0.86)	(0.94)	(-1.70)	(0.27)	(-0.19)
Book-to-Mkt		-0.001	-0.001	-0.001	-0.001	-0.000	-0.002	-0.000	-0.002*
		(-1.28)	(-1.02)	(-1.53)	(-0.99)	(-0.52)	(-1.32)	(-0.49)	(-1.66)
Size		0.001	-0.000	0.000	0.000	0.000	0.000	-0.000	0.001
		(1.56)	(-0.28)	(0.01)	(0.96)	(1.26)	(0.01)	(-0.06)	(0.91)
Leverage		-0.004**	-0.002	-0.004*	-0.002	-0.000	-0.012**	-0.003**	-0.005
		(-2.05)	(-1.00)	(-1.75)	(-1.08)	(-0.50)	(-2.43)	(-1.98)	(-1.04)
#Analyst		-0.000	0.000*	0.000	0.000	0.000	0.000*	0.000	0.000
		(-0.29)	(1.74)	(1.22)	(0.74)	(0.24)	(1.76)	(1.55)	(0.69)
HomeState		-0.001*	-0.001	-0.001	-0.001	-0.001	-0.002	-0.002**	0.005
		(-1.90)	(-0.74)	(-0.91)	(-1.12)	(-1.30)	(-0.62)	(-2.14)	(1.52)
p-value for diff. in coef.		0.100		0.010		0.010		0.37	
Politician FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Congress FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
S.E. Cluster by Politician and Congress		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations		30447	30586	26403	34658	45478	15589	49337	11726
Adj. R-Square		0.3476	0.2878	0.2156	0.4242	0.4368	0.0954	0.3892	0.0477

Table 6
Electronic vs Paper Filings

This table compares politicians' trades disclosed through electronic filings versus paper forms. Panel A reports the annual number of paper and electronic filings. There is an increasing trend towards electronic filings over the sample years, following strong recommendations from the ethics committee of the Congress. Panel B presents how the profitability gap between trades disclosed via electronic vs paper filings has evolved over time. The numbers in parentheses are *t*-statistics.

Panel A: Trend in filing methods – paper vs. electronic

	Paper		Electronic		Total
Year	No. of filings	%	No. of filings	%	
2013	639	100.00	0	0.00	639
2014	379	54.69	314	45.31	693
2015	279	41.27	397	58.73	676
2016	265	37.43	443	62.57	708
2017	258	36.39	451	63.61	709
2018	272	35.14	502	64.86	774
2019	153	25.16	455	74.84	608
2020	122	19.00	520	81.00	642
2021	83	15.49	453	84.51	536
2022	87	20.52	337	79.48	424
2023	52	16.61	261	83.39	313
Total	2589	38.52	4133	61.48	6722

Table 6 (continued)
Electronic vs Paper Filings

Panel B: Evolution of profitability differential – paper vs electronic (rolling exclusion of early years)

		Paper	Electronic	Paper – Electronic Diff in Mean		
Sample Window	Start Year	No. of Obs		*CAR_D30*	*CAR_D45*	*CAR_D60*
All years	2013	59301	34075	0.0000	0.0005	0.0007
				(-0.06)	(0.73)	(0.86)
Drop 2013	2014	52950	34075	0.0000	0.0006	0.0008
				(0.06)	(0.94)	(1.01)
Drop 2013-2014	2015	47107	32187	0.0004	0.0012	0.0013
				(0.63)	(1.61)	(1.64)
Drop 2013-2015	2016	41910	29205	0.0008	0.0015*	0.0016*
				(1.29)	(1.92)	(1.88)
Drop 2013-2016	2017	38085	26293	0.0015**	0.0022***	0.0023**
				(2.32)	(2.72)	(2.47)
Drop 2013-2017	2018	32851	23179	0.0016**	0.0025***	0.0025**
				(2.21)	(2.75)	(2.46)
Drop 2013-2018	2019	25713	19750	0.0016*	0.0023**	0.0025**
				(1.89)	(2.26)	(2.25)

Table 7
Suspicious vs Non-Suspicious Politicians

This table compares the trading profitability of politicians classified as *suspicious* vs *non-suspicious* based on two alternative definitions. In Columns 1 and 2, a politician is defined as *suspicious* if their propensity to violate the STOCK Act by disclosing trades late exceeds the sample median level of 13%. In Columns 3 and 4, a politician is classified as *suspicious* if they continue to use paper filings even in 2017 or later. By 2017, electronic filing had become the norm, such that continuing to use paper filings is plausibly interpreted as a deliberate disclosure strategy intended to obscure trading activities. This second classification requires observing a politician's filing method in 2017 or later, and thus reduces the sample size. Columns 5 and 6 present results for the *super set* of suspicious politicians, those in Columns 1 and 2 or in Columns 3 and 4.

		(1)	(2)	(3)	(4)	(5)	(6)
	Pr. Sign			*CAR_D45*			
Suspicious_1	+	0.002**	0.002**				
		(2.30)	(2.16)				
Suspicious_2				0.002*	0.002*		
				(1.77)	(1.71)		
Suspicious Superset						0.003**	0.003**
						(2.26)	(2.30)
ROA			0.008		0.012		0.012
			(0.94)		(1.24)		(1.26)
Book-to-Mkt			-0.000		-0.001		-0.001
			(-0.57)		(-1.16)		(-1.15)
Size			-0.000		-0.000		-0.000
			(-1.14)		(-0.65)		(-0.69)
Leverage			0.005		0.013**		0.013**
			(1.27)		(2.38)		(2.36)
#Analyst			-0.000		-0.000		-0.000
			(-0.22)		(-1.11)		(-1.11)
HomeState			-0.002		-0.001		-0.001
			(-1.14)		(-0.92)		(-0.87)
Congress FE		Yes	Yes	Yes	Yes	Yes	Yes
Industry FE		Yes	Yes	Yes	Yes	Yes	Yes
S.E. Cluster by Congress and Industry		Yes	Yes	Yes	Yes	Yes	Yes
No. of observations		93376	93376	64360	64360	64360	64360
Adj. R-Squared		0.0090	0.0094	0.0104	0.0110	0.0104	0.0110

Table 8
Effect of electoral pressure on disclosure of trading activity

This table presents whether legislators reduce the disclosure of trading activity while facing electoral pressure. The volume analysis is conducted at the politician-month (or politician-biweekly) level. Panel A presents the summary statistics for the 26,736 politician month observations. Panel B presents the regression results. In Columns 1 and 2, pressure period includes all weeks in November in election years, while in Columns 3 and 4, the first half of November in election years is treated as the pressure period and the second half as the non-pressure period. The results are from Poisson pseudo-maximum likelihood (PPML) regressions. Dependent variables are the total dollar value of disclosed transactions (*Transact_$VOL*) and the number of disclosed transactions (*Transact_NUM*), respectively. The key independent variable, *PressurePeriod*, is an indicator of whether the politician faces intensified electoral pressure during the month (or biweek). Politician and year fixed effects are included. Standard errors are clustered by politician and year. All variables are defined in Appendix A. The *t*-statistics are reported below coefficient estimates in parentheses. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively, using a two-tailed *t*-test.

Panel A: Descriptive statistics for politician month observations

	N	Mean	S.D.	P10	P25	Median	P75	P90
Dependent variables								
Transact_$VOL	26736	69826	346153	0	0	0	0	65000
Transact_NUM	26736	2.2131	8.8001	0.0000	0.0000	0.0000	0.0000	4.0000
Independent variables								
Pressure Period	26736	0.2111	0.4081	0.0000	0.0000	0.0000	0.0000	1.0000
Seniority	26736	0.4497	0.4975	0.0000	0.0000	0.0000	1.0000	1.0000
Powerful	26736	0.5736	0.4946	0.0000	0.0000	1.0000	1.0000	1.0000
Mkt_Ret	26736	0.0092	0.0421	-0.0435	-0.0171	0.0119	0.0316	0.0544
Mkt_Volatility	26736	0.0088	0.0058	0.0044	0.0054	0.0075	0.0111	0.0149
Others								
Vulnerable	26736	0.4390	0.4963	0.0000	0.0000	0.0000	1.0000	1.0000
High_Unemploy	26736	0.6329	0.4820	0.0000	0.0000	1.0000	1.0000	1.0000
Low_Income	26736	0.5920	0.4915	0.0000	0.0000	1.0000	1.0000	1.0000

Table 8 (continued)
Effect of electoral pressure on disclosure of trading activity

Panel B: Regression results

		Monthly obs.		Biweekly obs.	
		(1)	(2)	(3)	(4)
	Pr. Sign	*Transact $Vol*	*Transact _Num*	*Transact $Vol*	*Transact _Num*
PressurePeriod	-	-0.152**	-0.215***	-0.110*	-0.107**
		(-2.10)	(-3.85)	(-1.69)	(-2.03)
Seniority		-0.914***	0.010	-0.615**	0.109
		(-2.81)	(0.04)	(-2.46)	(0.46)
Powerful		0.314***	0.128	0.210**	0.071
		(2.71)	(1.47)	(2.30)	(0.87)
Mkt_Ret		0.409	1.027**	0.656	0.637*
		(0.67)	(2.30)	(1.30)	(1.71)
Mkt_Volatility		4.128	0.646	2.012	-1.764
		(0.75)	(0.16)	(0.50)	(-0.52)
Politician FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes
S.E. Cluster by Politician and Year		Yes	Yes	Yes	Yes
No. of observations		26736	26736	53472	53472
Pseudo R-Squared		0.6580	0.5980	0.4864	0.4563

Table 9
Cross-sectionals: effect of electoral pressure on disclosure of trading activity

This table examines whether Table 8 results are more pronounced for electorally vulnerable politicians (Panel A) and for politicians hailing from economically distressed districts (Panel B from high and low unemployment districts and Panel C from low vs high household income districts). *p*-values indicate whether differences in coefficients across subsamples are significant. All variables are defined in Appendix A. *t*-statistics are reported below coefficient estimates in parentheses. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively, using a two-tailed *t*-test.

Panel A: electoral vulnerability

Vulnerability		Vulnerable	Safe	Vulnerable	Safe
		(1)	(2)	(3)	(4)
	Pr. Sign	*Transact_$VOL*		*Transact_NUM*	
PressurePeriod	- ; 0	-0.202*	-0.086	-0.278***	-0.145**
		(-1.78)	(-0.92)	(-3.23)	(-2.02)
Seniority		-0.738	-1.108***	-0.084	-0.073
		(-1.57)	(-2.64)	(-0.19)	(-0.21)
Powerful		0.455***	0.283	0.173	0.236
		(3.10)	(1.39)	(1.26)	(1.48)
Mkt_Ret		0.355	0.423	1.131*	0.894
		(0.41)	(0.49)	(1.81)	(1.40)
Mkt_Volatility		5.946	1.624	2.427	-1.614
		(0.79)	(0.21)	(0.49)	(-0.26)
p-value for diff. in coef.		0.010		0.010	
Politician FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes
S.E. Cluster by Politician and Year		Yes	Yes	Yes	Yes
No. of observations		11736	15000	11736	15000
Pseudo R-Squared		0.6479	0.6953	0.6126	0.6130

Table 9 (continued)
Cross-sectionals: effect of electoral pressure on disclosure of trading activity

Panel B: Voter sentiment (proxied by unemployment rate)

Voters' Sentiment		High Unemploy	Low Unemploy	High Unemploy	Low Unemploy
		(1)	(2)	(3)	(4)
	Pr. Sign	*Transact_$VOL*		*Transact_NUM*	
PressurePeriod	- ; 0	-0.202**	0.148	-0.275***	-0.072
		(-2.57)	(1.02)	(-4.02)	(-0.73)
Seniority		-0.877**	-1.320**	0.273	-0.531
		(-2.18)	(-2.27)	(0.83)	(-1.12)
Powerful		0.240*	0.213	0.045	0.034
		(1.79)	(0.75)	(0.44)	(0.14)
Mkt_Ret		0.752	-1.134	1.675***	-1.091
		(1.13)	(-0.79)	(3.35)	(-1.13)
Mkt_Volatility		4.775	7.669	2.821	-10.070
		(0.82)	(0.45)	(0.66)	(-0.92)
p-value for diff. in coef.		0.000		0.000	
Politician FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes
S.E. Cluster by Politician and Year		Yes	Yes	Yes	Yes
No. of observations		16920	9816	16920	9816
Pseudo R-Squared		0.6993	0.5663	0.6465	0.5298

Table 9 (continued)
Cross-sectionals: effect of electoral pressure on disclosure of trading activity

Panel C: Voter sentiment (proxied by household income)

Voters' Sentiment		Low Income	High Income	Low Income	High Income
		(1)	(2)	(3)	(4)
	Pr. Sign	*Transact_$VOL*		*Transact_NUM*	
PressurePeriod	- ; 0	-0.233**	-0.080	-0.182**	-0.215***
		(-2.24)	(-0.85)	(-2.06)	(-3.05)
Seniority		-0.614	-1.265**	-0.187	0.002
		(-1.20)	(-2.38)	(-0.41)	(0.01)
Powerful		0.281**	0.441***	-0.110	0.323***
		(1.97)	(3.35)	(-0.99)	(2.78)
Mkt_Ret		0.948	-0.154	1.160*	0.852
		(1.01)	(-0.20)	(1.87)	(1.33)
Mkt_Volatility		3.509	3.177	-0.732	1.087
		(0.37)	(0.48)	(-0.12)	(0.21)
p-value for diff. in coef.		0.010		0.230	
Politician FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes
S.E. Cluster by Politician and Year		Yes	Yes	Yes	Yes
No. of observations		15829	10907	15829	10907
Pseudo R-Squared		0.6203	0.7027	0.5311	0.6770