Electoral Pressure and Strategic Disclosure of Congressional Trades

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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.

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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." 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). The public perception, fueled by extensive, negative media coverage, is that congressional knowledge exists broadly and that it has been used to inform politicians' stock trades. 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, and importantly led to the enactment of The Stop Trading on Congressional Knowledge (STOCK) Act in 2012.

However, despite the abundant anecdotal evidence that Members of Congress profit from nonpublic "congressional knowledge," academic literature provides mixed evidence on whether politicians earn abnormal returns from their stock trades (see Gao and Huang, 2016; Christensen, Mikhail, Walther, and Wellman, 2017; Jagolinzer, Larcker, Ormazabal, and Taylor, 2020; and Baldauf, Favilukis, Garlappi, and Zheng, 2025), particularly after the STOCK Act (Huang and Xuan, 2023; Karadas, Schlosky, and Hall, 2021; and Belmont, Sacerdote, Sehgal, and Hoek, 2020).[1] Taking a different approach, Abdurakhmonov, Snider, Ridge, and Hasija (2023) examine

[1] There are several empirical challenges when trying to detect legislators' abnormal trading profits. First, there is no reliable method to distinguish trades driven by congressional knowledge from those motivated by other legitimate reasons such as diversification or liquidity. 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.

how investors react to Congressional Members' disclosed trades and show that investors perceive Member trades as information-driven and respond accordingly.

In this study, we focus on politicians' strategic disclosure practices regarding their stock trades. Specifically, we investigate whether Members of Congress strategically obfuscate their potentially informed trades to avoid electoral consequences. Evidence of such strategic behavior would reveal the Members themselves believe their trades are inappropriate. We examine both the timing and the form of the legislators' disclosures. For our primary test, we focus on timing: do legislators delay the disclosure of their likely informed trades to avoid the highly scrutinized election period? We then focus on the form of disclosure: do legislators obfuscate likely informed trades by using paper filings, which are harder to access and process, instead of the more user-friendly electronic filing? Finally, having identified these suspicious disclosure patterns, we use them to pinpoint "suspicious" politicians, i.e., those who likely trade on congressional knowledge, and examine whether their trades generate higher abnormal returns than those of their peers.

The 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 the 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. Accountability has come, instead, from public scandals, and, ultimately, from voters who have the power to impose electoral consequences.

In the first set of tests, we utilize a comprehensive sample of congressional trades disclosed between 2013 and 2023 to examine whether and how electoral pressure affects the *timing* of Members' disclosures.[2] Given heightened scrutiny by political opponents and the media before and during elections, we expect the disclosure of profitable trades to vary with election cycles. Consistent with prior literature on corporate insider trading, we use the return predictability of insiders' trades, which we label as trading profits, to capture legislators' nonpublic congressional knowledge. 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 the election season) to the profitability of trades disclosed in other periods. We find that congressional trades disclosed during electoral-pressure windows exhibit lower profitability. This is 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 examining 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

[2] 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.

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 the stock sale until after narrowly winning re-election by a margin of only 333 votes. His delayed disclosure was well beyond the STOCK Act's mandated 45-day disclosure period[3].

Taking the insight from Representative Garcia's potentially extreme example, we design our next set of tests to document whether other legislators are also willing to violate the law to avoid disclosing stock trades before elections. For this analysis, we focus on trades that, under the STOCK Act, must be disclosed before the upcoming election, i.e., 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 disclosure* that violate the STOCK Act. Using this subsample and nonelection years as a natural benchmark for comparison, we document whether legislators' tendency to delay the disclosure of stock trades in violation of the law is greater in election years.

Consistent with electoral pressure influencing disclosure behavior, we demonstrate that the likelihood of delaying disclosures beyond statutory requirements is higher in election years. Furthermore, cross-sectional analyses indicate that the differential tendency to delay disclosure in election versus non-election years is most pronounced for legislators' profitable trades, the ones that are most likely informed and thereby vulnerable to criticism by political challengers and the

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

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 consistent support for our inference that electoral pressure motivates legislators to delay disclosure 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." By 2015 the majority of Congressional trading activity was filed electronically, although *some* politicians continue to submit paper forms.[4] 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.[5]

Having demonstrated that *some* politicians strategically disclose trading activity (use paper forms and delay disclosing beyond the 45-day requirement), we use these disclosure patterns to

[4] 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.

[5] This profitability gap is muted in the full sample but emerges clearly once we drop the early years in the sample. That is, starting with trades reported in 2016 and beyond, paper trades earn higher *CARs* than electronic trades.

identify suspicious politicians likely to have engaged in trading with private congressional knowledge. We then examine whether their trades generate higher abnormal returns than their peers. Consistent with our prediction, we document that these *Suspicious Politicians* earn significantly higher abnormal returns.

We contribute to the literature by providing the first 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. Our finding add to the existing research that focuses on the profitability of congressional trades and provides 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).[6] 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

[6] These papers examine the average profitability of politicians' stock trades to assess whether politicians leverage their congressional knowledge for financial gains. An interesting exception to examining average trading profits per se is the recent work by Abdurakhmonov, Snider, Ridge, and Hasija (2023), who document a positive market reaction to the disclosure of congressional stock purchase.

policy deliberations, and their legislative actions can directly affect firm and industry prospects. 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[7,8]. The Act clarifies that legislators, their families and staff are subject to the same Rule 10b-5 insider trading prohibitions as corporate insiders[9].

Unlike outright restrictions on trading, the STOCK 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.[10] 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.

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[7] 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.

[8] 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.

[9] 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.

[10] 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. However, we have been unable to verify the existence of a "House Ethics Committee's grace period".

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 "*blown through the law more than 140 times, earning himself a bipartisan ethics probe*."[11] Similarly, Senator Kelly Loeffler spent crucial months of her 2020 campaign battling insider trading accusations that severely damaging her public image and electoral prospects. Despite being cleared by a Justice Department investigation, she ultimately lost her Senate seat in November of 2020.[12]

During 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."[13]

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[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] See this *New York Times* article: https://www.nytimes.com/2020/06/15/us/politics/congress-trading-stock-loeffler-burr.html.

[13] 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*.

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".[14] *Business Insider* launched its "Conflicted Congress" project in 2021, creating a scorecard of lawmakers' conflicts of interest. Two of its metrics – violating the STOCK Act and legibility of disclosure forms – speak directly to the disclosure practices of congressional trades.

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. Fearing negative electoral consequences when their stock trades attract public attention, strategic disclosure behaviors are likely. Specifically, Members of Congress 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 noted above, despite the concern 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

[14] "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.

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).

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 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.

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

[15] Examining corporate insider trading, 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).

profitable trades during the highly scrutinized election periods.[16] 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.

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 allegation of insider trading immediately before elections) and delaying the disclosure until after election outweigh the expected costs (e.g., modest fines and infrequent ethics investigation).[17] This leads to our second hypothesis:

H2: Members of Congress are more likely to delay disclosure of stock trades and violate the law in election years compared to nonelection 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 engage in more strategic timing of their disclosures, including violating the STOCK Act to postpone disclosure of their trades until after the election.

[16] 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.

[17] To date, no legislator has been forced to resign because of insider trading activities or violations of the STOCK Act's disclosure rules.

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 expect legislators to use paper forms for their most informed trades. If legislators who trade on congressional knowledge continue to report their trades on paper forms, then we expect these trades to be more profitable than those reported using the electronic system.[18]

Given the choice of disclosure form (paper versus 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.

[18] Generally, once a politician switches to electronic filings, they continue to report their trades using the electronic filing systems. We identify 24 instances where a politician switches back to paper filings. See footnote 29 for details.
[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.

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 in 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 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 filed in 2012. Thus, the *Capitol Trades Database* and our sample period begin 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, aggregate same-day, same-stock, same-direction transactions by the same politician, drop trades with missing control variables.[20] These steps reduce the sample to 93,376 trades, which we use as the baseline dataset. From this baseline, we construct subsamples tailored for 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

[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.

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 Stewart III from MIT's political science department. Data on Members of Congress who announce retirement or a decision to 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., 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 at other times. In section 4.2 we examine whether politicians, similar to Rep Mike Garcia, are willing to violate the law to delay disclosing their stock 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 stock 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., safely 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

[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.

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]

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 research that documents that the average abnormal profits gained by congressional trades is not significantly different from zero (e.g., Belmont et al., 2022). Importantly for our research question, the mean differential in the *CARs _D30*, *_D45*, and *_D60* for trades disclosure during *PressureWindow* versus *Non-PressureWindow* are negative, -0.21%, -0.27%, and -0.37%, respectively and significantly different from zero, indicating that Members report less profitable trades during *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.

Related, noteworthy statistics in Table 2 Panel A include: 1.) *Disclosure Lapse* – the days from execution to disclosure – averages 49 days in the *PressureWindow* versus 42 days otherwise, and 2.) the rate of *Violations* of the STOCK Act is 15.9% in the *PressureWindow* versus 11.5%

[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 knows. 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.

otherwise. For all congressional trades reported during our sample period the frequency of violations of the STOCK Act is 13% (untabulated), demonstrating the generally weak enforcement of the STOCK Act. However, the longer disclosure lapses and higher violation rate during *PressureWindow* point to strategic delays in disclosure by politicians in response to electoral pressure.

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 by politician and congressional session.

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 appears 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 trades by retiring politicians; 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 insignificant. While the smaller sample size could reduce statistical power, the opposite sign relative to the baseline test in Panel B 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 using *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 D.C., rather than strategic

[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*.

[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.

disclosure. By focusing exclusively on the subsample of trades executed while Congress is in session, we mitigate this concern. The coefficient for *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 potentially understates standard errors and overstates 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 statistically 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 the PressureWindow in prior election years. Observations outside of these pressure windows are dropped to allow for a direct comparison of 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 our interpretation of the findings presented in Table 2, next we present cross-sectional analyses that documents whether strategic disclosure behavior is more pronounced for legislators facing amplified electoral pressure. 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 the legislator. 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.

The second split considers voter sentiment. A growing body of political science research shows that economic conditions shape voters' trust in government and perceptions of corruption. In particular, 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 a district-level proxy: unemployment rate. We expect legislators representing *High Unemployment* districts (with above-national-median unemployment rate) to face greater electoral pressure.

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 (coef. = -0.005). In contrast, the coefficient for *Safe* legislators is not significantly different from zero. The difference in the coefficient estimates for the two 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* districts disclose trades with significantly lower *CAR_D45* during the *PressureWindow* (coef. = -0.004). By contrast, the coefficient for *Low Unemployment* districts is smaller and statistically insignificant. The difference in the coefficient estimates for these two groups is also statistically significant (p-value = 0.010).

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 the timing decision for trade execution or the timing of the disclosures per se. In this section, we design direct tests of how the choice of disclosure timing per se is affected by legislators' upcoming elections. Specifically, we zoom in on trades executed 45 days or more before an election and document whether legislators disclose these trades before or after the election day. If disclosure timing is shaped by electoral incentives, we expect some politicians, particularly those facing close races, to exploit the weak enforcement of the STOCK Act and strategically delay the disclosure of their trades until after the election even though doing so violates the law. Accordingly, in this section our tests directly examine whether legislators are more likely to violate the STOCK Act by postponing disclosures of their stock trades beyond the statutory deadline to avoid public scrutiny in election years. We use nonelection years as a natural benchmark for comparison.

4.2.1 Research Design and Baseline Results

For these tests we construct two subsamples that allow a direct comparison of legislators' choice of disclosure timing in election versus non-election years. Figure 1 illustrates. The first, *larger subsample* retains all Congressional trades executed between January 1 and 45 days prior to the Election Events in early November, where the Election Event is an Election Day in an election year and a Pseudo-Election Day in a nonelection year. This *larger subsample* includes 61,072 Congressional trades (28,240 from election years and 32,832 from non-election years). The second, more *limited subsample* excludes trades executed more than 135 days prior to the Election Events, effectively isolating trades executed during the 90-day "high-salience" window that falls

roughly between mid-June to mid-September when voter attention peaks and the incentive to conceal potentially controversial trades before an election is strongest. The *limited subsample* includes 24,486 Congressional trades (9,983 from election years and 14,503 from non-election years).[25]

Using these two subsamples, we estimate the following regression using both OLS and a Logit model:

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

Figure 1 also illustrates how the dependent variable, *Delayed Disclosure*, is defined. *Delayed Disclosure*, is set to one when a trade is *both* disclosed more than 45 calendar days after its execution *and* disclosed with 90 days *after* the Election Event.[26] *Delayed Disclosure* is set to zero otherwise. Requiring trades classified as *Delayed Disclosure* to be disclosed within 90 days of the Election Event ensures that the violation of the STOCK Act is plausibly tied to election incentives. The key independent variable, *Elect Year*, is an indicator for whether the trade is executed in an election year or non-election year. Control variables, fixed effects, and clustering follow those in Model (1). A positive and significant β_1 supports our expectation that some politicians strategically delay disclosing stock trades until after elections, even when doing so violates the law.

Table 4 presents the baseline results. Columns 1 and 2 present the OLS results, and Columns 3 and 4 the Logit estimates. Columns 1 and 3 use the *larger subsample* (i.e., trades executed more than 45 days before the election or pseudo-election), while Columns 2 and 4 restrict

[25] The smaller number of trades executed in the "high salience" window in election vs nonelection years indicates that legislators trade less in election years. We formally test whether this is the case in section 7 below.
[26] Recall that the Election Event is the first Tuesday after the first Monday in November. In election years it is the Election Day; in nonelection years it is the Pseudo-Election Day.

the analysis to the *limited subsample*, excluding trades executed more than 135 days prior to the election or pseudo-election to more tightly anchor the sample to election events in early November.

Starting with the OLS specifications, Columns 1 and 2 report positive and significant coefficients on *Elect Year*, indicating a 0.5-0.9 percentage point increase in the likelihood of a *Delayed Disclosure* in election years relative to non-election years. The Logit estimates in Columns 3 and 4 yield similar results. The coefficients on *Elect Year* remain positive and significant, indicating that a similar relation exists even when we *exclude* politicians who never delay their disclosures.[27] That is, even when we focus on the subsample of politicians who are willing to violate the STOCK Act, we find that their willingness to violate the law is greater in election than nonelection years.

4.2.2 Cross-sectional Analyses

To provide further support for our interpretation we conduct cross-sectional analyses to assess if the strength of the relation documented in Table 4 varies with trading profitability, electoral vulnerability and voter sentiment. We begin by estimating OLS regressions for the split samples and reporting p-value for differences in the coefficient estimates. The OLS results are presented in Panel A of Table 5 for the larger subsample and in Panel B for the limited subsample of trades anchored more tightly to elections.

Examining Panels A of Table 5, Columns 1 and 2 split the sample into *Profitable* and *Unprofitable* trades, based on positive or negative *CAR_D45*. The coefficients on *ElectYear* are positive and significant for both types of trade (0.006 vs. 0.004) and the larger coefficient estimate for *Profitable* trades is marginally significant (p = 0.100). In Panel B again the coefficients on

[27] In Column 3 and 4, a substantial number of observations are lost because of the inclusion of politician fixed effects in the logistic regression: If a politician's delayed disclosure behavior is constant throughout her tenure (i.e., she never delays or always delays then) all her trades are dropped from this analysis.

ElectYear are positive and significant for both profitable and unprofitable trades (0.011 vs. 0.007), but in the tightly anchored subsample the larger coefficient for *Profitable* trades is significantly greater ($p = 0.000$). These findings are consistent with politicians being more inclined to delay the disclosure of their profitable trades and violate the law in election years.

Columns 3 and 4 of Panels A and B examine cross-sectional variation arising from electoral vulnerability and documents that *Vulnerable* politicians exhibit a greater tendency to delay disclosures in election years compared to *Safe* politicians. The difference between coefficient estimates for the two types of politicians is statistically significant (p-values = 0.010 and 0.000 in Panels A and B, respectively), indicating that electoral pressure affects disclosure timing behavior for politicians facing a higher risk of losing their re-election bid.

Finally, Columns 5 to 6 splits the sample based on district-level unemployment to capture variation in voters' resentment of the perceived abuse of power by political elites. Specifically, we conjecture that political scandals involving congressional insider trading are likely to generate greater backlash in areas of high unemployment. Results reveal that politicians from *High Unemployment* areas significantly more likely to delay disclosures in election years, while the coefficients for *Low Unemployment* areas are insignificant. The difference in coefficients for *High* vs *Low Unemployment* is significant (p-values = 0.010 and 0.000 in Panels A and B, respectively).

To ensure these findings are robust we re-run the cross-sectional analysis using Logistical models while noting several limitations of the Logit specification: A substantial number of observations are lost because of the inclusion of politician fixed effects in the logistic regression, the coefficient estimates from the Logit models are difficult to interpret, and we cannot assess whether the coefficient estimates for the various subsamples are significantly different as we did with the p-values reported for the OLS regressions. We present estimates of the coefficients on

Elect Year for Logistical regressions using the *larger subsample* of trades in Panel C of Table 5 and for the *limited subsample* in Panel D. Inferences remain the same as those drawn from the OLS regressions with one exception: the insignificant coefficient on *Elect Year* for the *High Unemploy* subsample in Panel D. It is likely that this insignificant coefficient results from a lack of power in the smaller sample, given the similar magnitude to the coefficient estimates on *Elect Year* for the *High Unemploy* subsamples in Panels C and D (coefficient of 1.895 and 1.903, respectively).

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 (63.5%) during the sample period is reported on paper forms.

Interestingly, comparing the magnitudes of trading activity reported on a paper form versus in an electronic filing demonstrates that the two disclosure forms differ significantly. First, as reported in Table 6 Panel A, legislators bundle and report on a single paper form a substantially greater number of individual transactions (an average of 22.8 transactions per paper form versus only 8.2 per electronic filing). Second, the average dollar value of trades reported on paper are also significantly larger than the average dollar value of trades reported in electronic filings ($28,330

per transaction for paper filings vs. $25,532 for electronic filings).[28] Taken together these statistics suggest that some politicians bundle multiple larger transactions and report them on paper forms, a less accessible format than electronic filings.

Table 6 Panel B documents the transition from paper forms to electronic filings.[29] In 2013 no transactions were filed electronically. Two years later the majority were electronic filings (58.7% in 2015), and by 2020 over 80% of filings were electronic. Nevertheless, some Members continued to rely on paper forms – 16.61% of filings were still paper as recently as 2023.[30] Politicians who continue to file paper forms beyond 2015 may do so because they (and their staff) are not technologically adept or because they want to make their disclosures more difficult to access. If it is the latter explanation, then we expect "paper trades" to be more informed and thus more profitable than those disclosed electronically.

In Panel C of Table 6 we compare the profitability of trades disclosed on paper forms versus those filed electronically and track how the profitability gap evolves as the early transition years are sequentially excluded. Including all sample years the average abnormal returns (i.e., *CAR_D30*, *CAR_D45*, and *CAR_D60*) are indistinguishable across the two disclosure forms. Once we drop the transition years, when the electronic filing system is being adopted, 2013-2015, the differences in the trading profits become economically and statistically meaningful: *CAR_D45* and *CAR_D60*

[28] Consistent with the volume tests presented later in Section 7, the dollar value per trade uses the midpoint of each transaction range's upper and lower bounds.

[29] Once a legislator adopts electronic filing, she seldomly switches back to paper. We identify only 24 reversions back to paper forms which report a total of 217 Congressional stock trades. Abnormal returns for the 217 trades are neither statistically different from zero nor higher than those of other trades in the sample. However, the average dollar value of these trades is significantly larger than the sample mean ($35,229 vs. $27,290 per trade). In addition, 58% of these trades miss the STOCK Act's 45-day disclosure requirement (the comparable mean for the sample is 13%), and the average disclosure lapse is an extraordinary 276 days (unwinsorized). These descriptive statistics suggest that some legislators revert to paper filings to lower the visibility of large trades that they disclosed late in violate the STOCK Act.

[30] 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.

for paper trades exceed those for electronic filings by roughly 15-16 basis points. As we sequentially exclude more of the early years in the sample period, the profitability gap widens to 25 basis points. The monotonic strengthening of the profitability premium suggests that overtime trades disclosed on paper are increasingly selectively the trades associated with private congressional knowledge. As electronic filing becomes the norm, it appears that the minority of legislators who persist with the use of paper forms are progressively a more distinctive group of legislators who employ strategic disclosure practices to hide their informed trades.

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.

Similar to Cohen, Malloy, and Pomorski (2012) who distinguish "opportunistic" from "routine" insider traders, we classify individual legislators as Suspicious or Not based on the *timing* and *form* of their disclosures of their stock trades. First, based on disclosure *timing*, we set the indicator variable, *Suspicious_Timing*, 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_Form*, equal to one if a politician continued to use paper filings in 2017 or later, despite the widespread adoption of

electronic filings.[31] 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 politicians with a propensity for suspicious disclosure behaviors. For each we find that trades by suspicious politicians earn significantly higher abnormal returns than their peers. Under *Suspicious_Timing* and *Suspicious_Form* 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 less 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 disclosed trades, we construct a politician-month dataset, aggregating each legislator's disclosed transactions within a

[31] Our definition of *Suspicious_Form* assumes that by 2017 nonstrategic politicians have transitioned to the electronic filing system. Although we have not identified an objective cutoff for the transition year of nonstrategic filers, our findings are generally robust to reasonable variations in this assumption. The estimated coefficient remains stable at 0.002. The statistical significance of this variable modestly strengthens when we move the assumed transition year further out in time (e.g., shifting it to 2018 results in a *t*-stat of 2.11), and weakens slightly when we accelerate the adoption year (defining politicians who continue to use paper filings in 2016 or later as suspicious results in a *t*-stat of 1.55).

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, forming a balanced politician-month panel.

We employ two measures of politician 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 Panels A and B). 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 most legislators do not trade stocks each month. Instead, they tend to trade relatively infrequently, but in some instances, they trade large amounts in a single month.

Following recent guidance for outcomes with many zeros and skewed positive values (Cohn, Liu, and Wardlaw, 2022; Chen and Roth, 2024; Li and Wang, 2024), we estimate Model 3 using a Poisson Pseudo-Maximum Likelihood (PPML) estimator.[32]

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

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. Then, $\exp(\beta_1) - 1$ is the proportional change in expected

[32] According to recent literature in econometrics (Cohn, Liu, and Wardlaw, 2022; Chen and Roth, 2024) and accounting work that adopts the same approach (Li and Wang, 2024), PPML consistently estimates average effects when zeros are prevalent. It avoids the interpretational issues that arise when applying log or log-like transformations to zero-inflated variables and delivers a directly interpretable estimator. Moreover, it handles high-dimensional fixed effects without the convergence issues common to Logit and Probit estimations.

monthly trading volume associated with $PressurePeriod_{j,t}$. Moreover, 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 the Ways and Means Committee[33]). We also control for aggregate market return (*Mkt_Ret*) and market volatility (*Mkt_Volatility*) to capture market-wide conditions. Finally, we include politician (δ_j) and year (γ_y) fixed effects and 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 the total dollar volume disclosed during election pressure months. This effect is stronger for *Transact_Num* in Column 2, where the coefficient on *PressurePeriod* of -0.215 translates to a 19% reduction in the number of transactions disclosed.[34]

To mitigate concerns that several weeks in November fall after the general election and ought not to be classified as part of the *PressurePeriod,* 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.[35]

[33] 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.
[34] Falsification test (untabulated) employing the subsample of retiring politicians (similar to Panel C of Table 2) finds no strategic reduction in the volume of disclosed trades for retiring politicians prior to elections.
[35] Moreover, untabulated results show that when the entire month of November in election years is classified as part of the non-pressure period the regression coefficients on *Pressure Period* remain negative and significant.

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 (e.g., in Table 3). Table 9 Columns 1-4 split by electoral vulnerability. Using PPML, the coefficient of *PressurePeriod* for *Transact_$VOL* is negative and significant for vulnerable politicians (i.e., coef. = -0.202 corresponding to 18% less dollar value) during electoral pressure periods, but insignificant different from zero for safe politicians (coef. = -0.086). For *Tranact_NUM*, the coefficient of *PressurePeriod* is -0.278 for vulnerable politicians (i.e., 24% fewer transactions) and -0.145 for safe politicians (i.e., 13% fewer transactions) and both are significantly different from zero. Testing for differences in the coefficient estimates across the two subsamples, the decline in both measures of disclosed trading activity is greater for vulnerable legislators (*p*-values = 0.010), indicating that legislators facing closer electoral contests are more likely to reduce their disclosed trading activity.

Columns 5-8 split the sample of politician months by the unemployment rate of the politician's home state or district. Legislators from *High Unemployment* states or districts disclose significantly less dollar value (i.e., coef. of *Transact_$Vol* = -0.202) and fewer transactions (i.e., coef. of *Transact_Num* = -0.275) during pressure periods, while those from *Low Unemployment* districts show no comparable reduction in disclosed trading activity during pressure periods. The differences across subsamples are highly significant (*p*-values = 0.000). These findings support the conjecture that politicians have stronger electoral incentives to avoid disclosing stock trades when local labor markets are weak and voters are especially sensitive to perceived self-dealing.

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

4.1 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 focuses on documenting whether Congressional trades earn abnormal returns to establish whether Members of Congress trade on privileged information, we show that *when* and *how* 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 the attention of voters and political opposition are at a 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 characteristics, 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." We demonstrate trades by suspicious politicians 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 including an examination of legislators' choice of disclosure practices. From a policy perspective, the persistence of late filings, and continued use of paper forms underscore the 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 necessary to ensure that Members of Congress are held to the same standards of transparency and ethical conduct as corporate insiders.

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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 unemployment data is 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	*Delayed Disclosure* set to one if a trade is disclosed (i.e., its filing date is) (i) more than 45 calendar days after its execution date and (ii) it is disclosed after the Election Day (i.e., the actual in election years; the pseudo in non-election years); and zero otherwise. In addition, all trades disclosed more than 90 days after the Election Day or pseudo election date are set to zero. Election Day is the first Tuesday following the first Monday in November under U.S. federal law.

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_Timing	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_Form	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*".
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*".

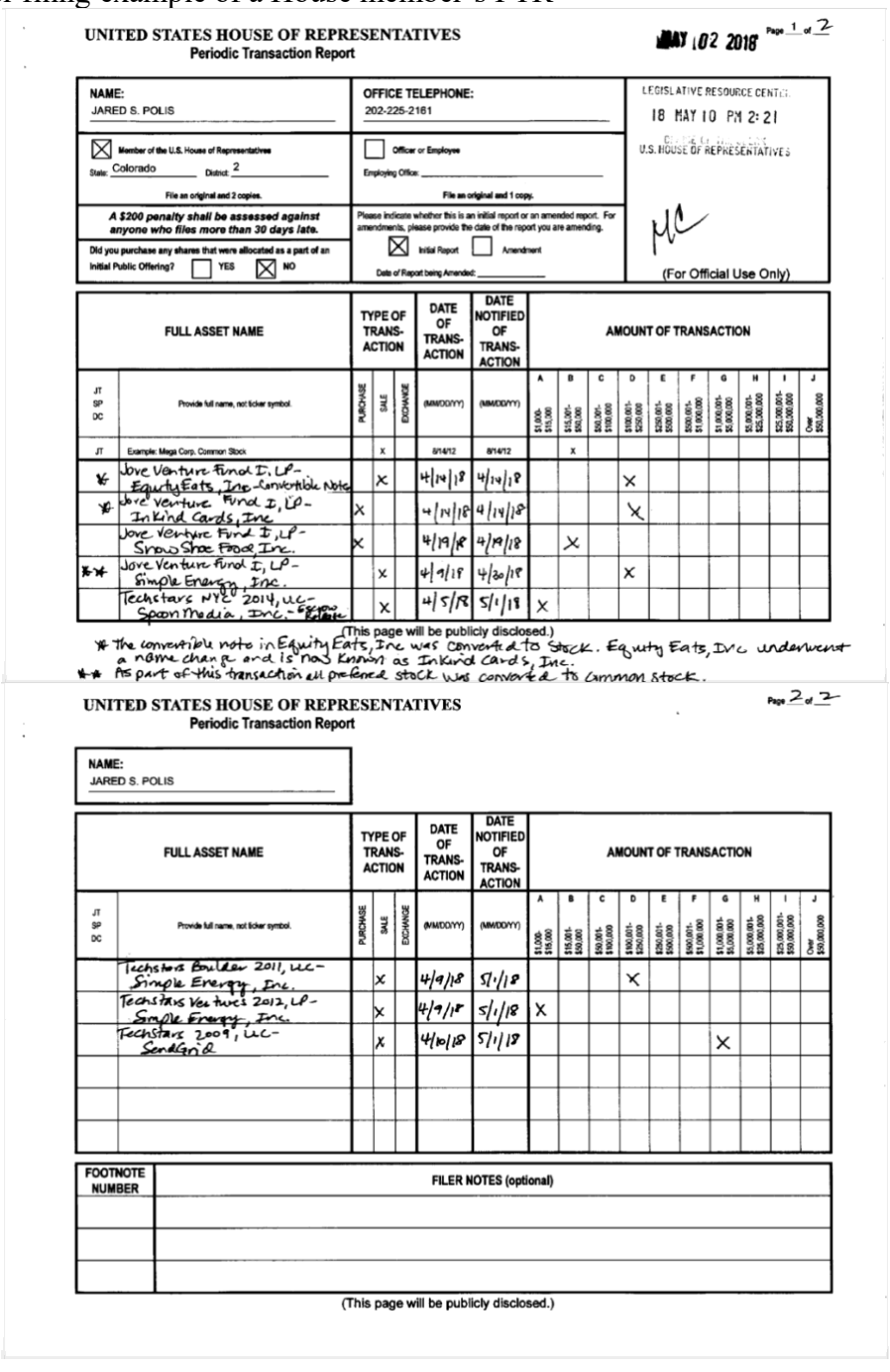
Appendix B
Example of primary dates

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 Balance		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			

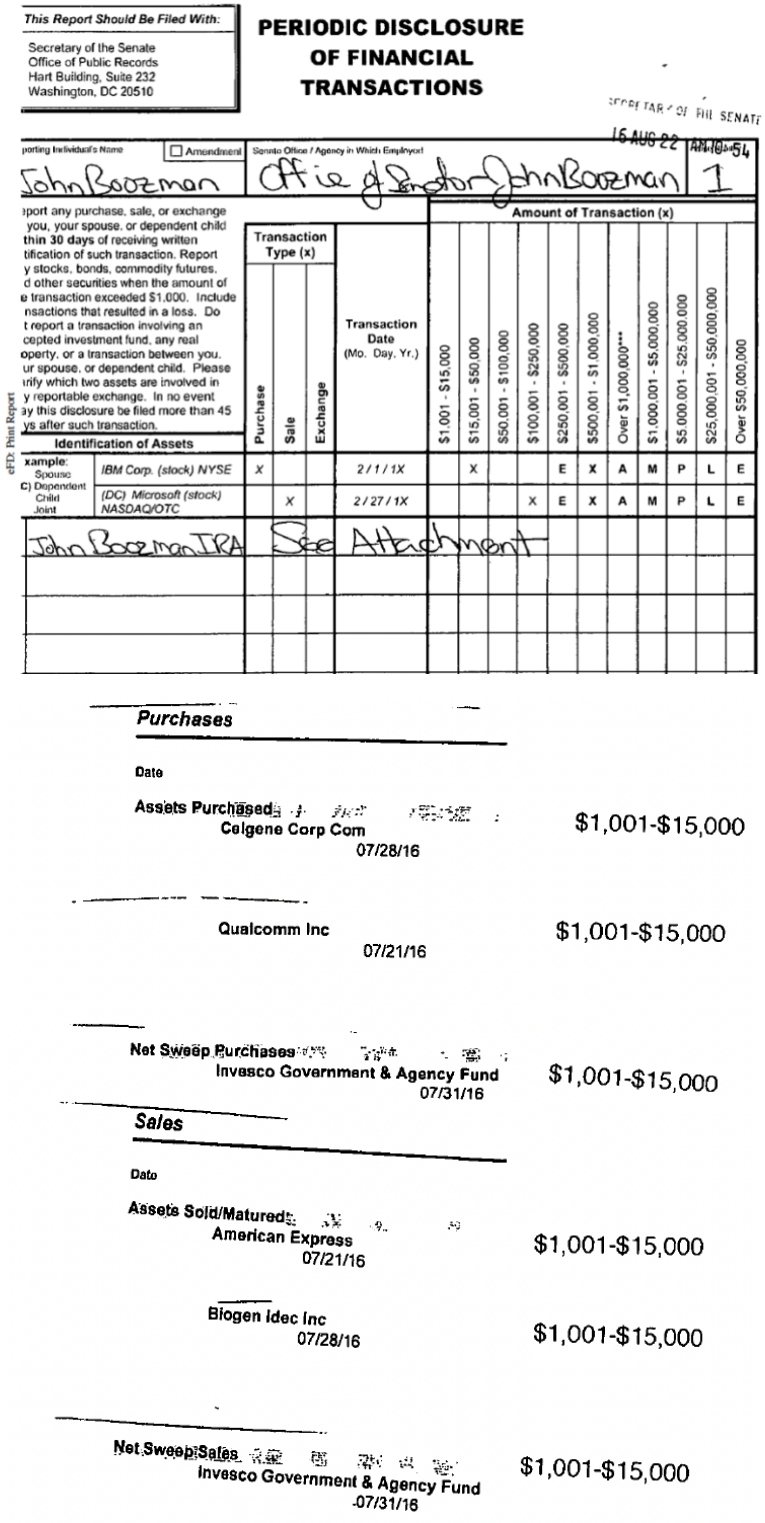
FOOTNOTE NUMBER	FILER NOTES (optional)

(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

SECRETARY OF THE SENATE
16 AUG 22 AM 10:54

Porting 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	--

Figure 1
Research design for the Delayed Disclosure

This figure illustrates the construction of the two subsamples employed in the Delayed Disclosure tests (section 4.2). The *Larger Subsample* retains all Congressional trades executed between January 1 and 45 days prior to the Election Events in early November. In election years the Election Event is an Election Day; in nonelection years it is a Pseudo-Election Day. The *Limited Subsample* further excludes trades executed more than 135 days prior to the Election Events, thereby isolating trades executed during the 90-day "high-salience" window that falls roughly between mid-June to mid-September when voter attention peaks and the incentive to conceal potentially controversial trades before an election is strongest. Also noted here are disclosures classified as *Delayed Disclosures*: those trades disclosed within the 90 days after the Election Event, in violation of the law given the trade was executed more than 45 days before the Election Event.

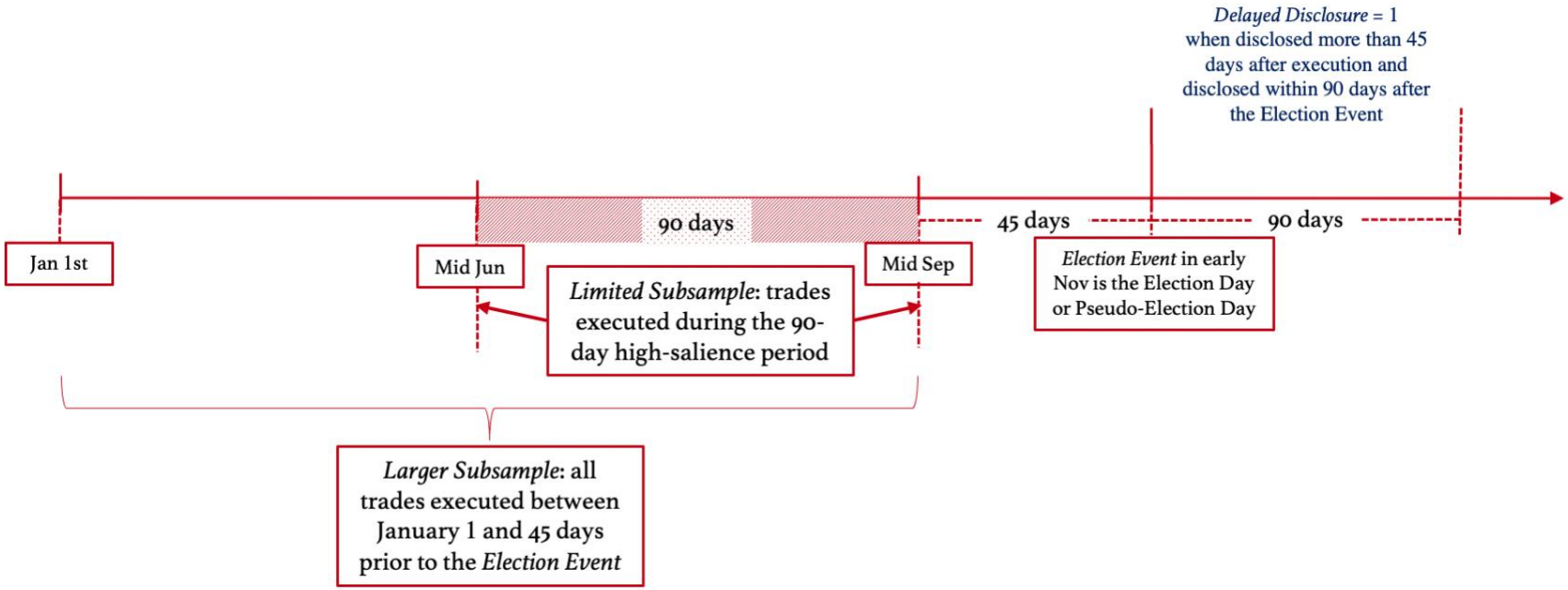


Table 1
Sample Construction

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
Baseline 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 larger subsample (Tables 4 and 5)		61,072
Trades used for *Delayed Disclosure* tests limited subsample (Tables 4 and 5)		24,486
Trades used for *Suspicious Politician* tests (Table 7)		64,360
…Politician Months obs. 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. We begin with the 318 unique politicians who file at least one PTR and multiplied by the number of months (or biweeks) in which the politician served in Congress. This process yields 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	0.8365	0.0000	1.0000	1.0000	0.7381	0.0000	1.0000	1.0000	0.0984***

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-sectional analysis: 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 compare trades executed by politicians hailing from high and low unemployment districts. *p*-values indicate whether differences in coefficients across subsamples are significant. *t*-statistics are reported below coefficient estimates in parentheses.

		CAR_D45			
		Vulnerable	Safe	High Unemploy	Low Unemploy
	Pr. Sign	(1)	(2)	(3)	(4)
PressureWindow	-, 0	-0.005**	-0.001	-0.004***	-0.001
		(-2.41)	(-1.11)	(-2.63)	(-0.26)
ROA		0.005	0.003	0.006	-0.004
		(0.33)	(0.55)	(0.85)	(-0.28)
Book-to-Mkt		0.001	0.000	0.001	0.001
		(1.12)	(0.80)	(1.43)	(0.65)
Size		0.000	0.001	0.001	0.000
		(0.40)	(1.55)	(1.53)	(0.35)
Leverage		0.001	0.009***	0.007	0.005
		(0.09)	(3.21)	(1.49)	(0.80)
#Analyst		-0.000	-0.000	-0.000	0.000
		(-0.77)	(-0.04)	(-0.74)	(0.08)
HomeState		-0.001	-0.003**	-0.003**	0.002
		(-0.38)	(-2.13)	(-2.18)	(0.46)
p-value for diff. in coef.		0.020		0.010	
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		32095	55825	67473	20444
Adj. R-Squared		0.0045	0.0044	0.0026	0.0095

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 subsample used in Columns 1 and 3 includes 61,072 Congressional trades executed more than 45 days before the election (or pseudo-election); the subsample used in Columns 2 and 4 includes 24,486 Congressional trades executed more than 45 days but less than 135 days before the election (or pseudo-election). See Figure 1. The dependent variable, *Delayed Disclosure*, is an indicator for whether a trade is disclosed after the election (or pseudo-election), and thereby violates the STOCK Act's 45-day disclosure requirement. 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*			
		OLS Results		Logit Results	
	Pr. Sign	(1)	(2)	(3)	(4)
ElectYear	+	0.005**	0.009**	1.194**	1.662*
		(2.02)	(2.08)	(2.42)	(1.96)
ROA		-0.001	0.001	0.154	0.521
		(-0.32)	(0.24)	(0.18)	(0.54)
Book-to-Mkt		-0.001	-0.002*	-0.187**	-0.246*
		(-1.33)	(-1.77)	(-2.12)	(-1.87)
Size		0.000	0.000	0.044	0.080
		(0.84)	(1.13)	(0.80)	(1.15)
Leverage		-0.003**	-0.004	-1.294**	-1.369**
		(-2.17)	(-1.51)	(-2.40)	(-2.07)
#Analyst		0.000*	0.000	0.009	0.003
		(1.69)	(0.90)	(1.56)	(0.44)
HomeState		-0.001	-0.002	-0.344	-0.640
		(-1.57)	(-1.57)	(-0.81)	(-1.41)
Politician FE		Yes	Yes	Yes	Yes
Congress FE		Yes	Yes	Yes	Yes
Cluster by Politician & Congress		Yes	Yes	Yes	Yes
No. of observations		61,072	24,486	20,157	1,994
Adj. R-Squared		0.3124	0.3669		
Pseudo R-Squared				0.5239	0.3444

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 compare politicians hailing from high vs. low unemployment 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.

Panel A: OLS using trades executed at least 45 days prior to an election or pseudo election day

Dependent variable:		*Delayed Disclosure*					
Subsample:		Profitable	NonProfitable	Vulnerable	Safe	High Unemploy	Low Unemploy
	Pr. Sign	(1)	(2)	(3)	(4)	(5)	(6)
ElectYear	+, 0	0.006**	0.004*	0.007*	0.004	0.007**	0.003
		(2.09)	(1.90)	(1.88)	(1.12)	(2.00)	(0.38)
ROA		-0.004	0.002	-0.001	-0.003	0.002	-0.007
		(-0.96)	(0.61)	(-0.19)	(-0.86)	(0.67)	(-1.12)
Book-to-Mkt		-0.001	-0.001	-0.001	-0.001	-0.000	-0.003**
		(-1.28)	(-1.02)	(-1.53)	(-0.99)	(-0.01)	(-1.99)
Size		0.001	-0.000	0.000	0.000	0.000	0.000
		(1.56)	(-0.28)	(0.01)	(0.96)	(0.32)	(0.34)
Leverage		-0.004**	-0.002	-0.004*	-0.002	-0.002	-0.008*
		(-2.05)	(-1.00)	(-1.75)	(-1.08)	(-1.37)	(-1.75)
#Analyst		-0.000	0.000*	0.000	0.000	0.000	0.000
		(-0.29)	(1.74)	(1.22)	(0.74)	(1.27)	(1.44)
HomeState		-0.001*	-0.001	-0.001	-0.001	-0.001**	0.003
		(-1.90)	(-0.74)	(-0.91)	(-1.12)	(-2.02)	(0.73)
p-value for diff. in coef.		0.100		0.010		0.010	
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		30447	30586	26403	34658	47181	13881
Adj. R-Square		0.3476	0.2878	0.2156	0.4242	0.4074	0.0579

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

Panel B: OLS using trades executed between 135 and 45 days prior to an election or pseudo election day

Dependent variable:		*Delayed Disclosure*					
Subsample:		Profitable	NonProfitable	Vulnerable	Safe	High Unemploy	Low Unemploy
	Pr. Sign	(1)	(2)	(3)	(4)	(7)	(8)
ElectYear	+, 0	0.011**	0.007*	0.018**	0.003	0.011*	0.001
		(2.15)	(1.92)	(2.26)	(0.67)	(1.92)	(0.09)
p-value for diff. in coef.		0.000		0.000		0.000	
Control		Yes	Yes	Yes	Yes	Yes	Yes
Politician FE and Congress FE; S.E. Cluster by Both		Both	Both	Both	Both	Both	Both
No. of observations		12144	12301	10775	13703	18306	6174
Adj. R-Square		0.3840	0.3561	0.3129	0.5140	0.4780	0.0840

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

Panel C: Logit model estimated using trades executed at least 45 days prior to an election or pseudo election day

Dependent variable:		*Delayed Disclosure*					
Subsample:		Profitable	NonProfitable	Vulnerable	Safe	High Unemploy	Low Unemploy
	Pr. Sign	(1)	(2)	(3)	(4)	(7)	(8)
ElectYear	+, 0	1.266**	1.221**	1.643***	-0.771	1.895**	0.876
		(2.33)	(2.39)	(3.05)	(-1.10)	(2.10)	(0.69)
Control		Yes	Yes	Yes	Yes	Yes	Yes
Politician FE and Congress FE; S.E. Cluster by Both		Both	Both	Both	Both	Both	Both
No. of observations		2481	9577	2837	16042	16317	2253
Pseudo R-Square		0.3956	0.5302	0.1895	0.7895	0.6712	0.2222

Panel D: Logit using trades executed between 135 and 45 days prior to an election or pseudo election day

Dependent variable:		*Delayed Disclosure*					
Subsample:		Profitable	NonProfitable	Vulnerable	Safe	High Unemploy	Low Unemploy
	Pr. Sign	(1)	(2)	(3)	(4)	(7)	(8)
ElectYear	+, 0	1.690*	1.601*	3.365***	-2.089**	1.903	0.249
		(1.88)	(1.75)	(3.86)	(-2.33)	(1.46)	(0.16)
Control		Yes	Yes	Yes	Yes	Yes	Yes
Politician FE and Congress FE; S.E. Cluster by Both		Both	Both	Both	Both	Both	Both
No. of observations		945	837	1271	358	815	793
Pseudo R-Square		0.3707	0.3482	0.2811	0.6282	0.4017	0.1811

Table 6
Electronic vs Paper Filings

This table compares politicians' trades disclosed via electronic filings versus paper forms. Panel A reports the number of trades and dollar value per trade for the two types of filings. Panel B reports the annual number of paper and electronic filings. Panel C 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: Bundled trades and Trade size

	Filing Type	N	P10	P25	Median	P75	P90	Mean
# of trades per filing	Paper	2589	1	2	4	14	54	22.83
	Electronic	4133	1	1	3	8	18	8.18
	Diff in Mean							14.66***
Dollar value per trade	Paper	2589	8000	8000	8000	32000	65000	28330
	Electronic	4133	8000	8000	8000	24000	40500	25532
	Diff in Mean							2797***

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

		Paper	Electronic	Diff in Mean (Paper – Electronic)		
Sample Window	Start Year	No. of trades		*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_Timing	+	0.002**	0.002**				
		(2.30)	(2.16)				
Suspicious_Form				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 using Poisson Pseudo-Maximum Likelihood estimation. 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								
PressureWindow	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

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

Panel B: Effect of electoral pressure on disclosure of trading activity

		Monthly obs.		Biweekly obs.	
		(1)	(2)	(3)	(4)
	Pr. Sign	*Transact $Vol*	*Transact _Num*	*Transact $Vol*	*Transact _Num*
Pressure Period	-	-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 (Columns 1 to 4) and for politicians hailing from high and low unemployment districts (Columns 5 to 8). The full sample consists of 26,736 politician months; *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.

Cross-sectionals		Vulnerable	Safe	Vulnerable	Safe	High Unemploy	Low Unemploy	High Unemploy	Low Unemploy
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr. Sign	*Transact_$VOL*		*Transact_NUM*		*Transact_$VOL*		*Transact_NUM*	
Pressure Period	- ; 0	-0.202*	-0.086	-0.278***	-0.145**	-0.202**	0.148	-0.275***	-0.072
		(-1.78)	(-0.92)	(-3.23)	(-2.02)	(-2.57)	(1.02)	(-4.02)	(-0.73)
Seniority		-0.738	-1.108***	-0.084	-0.073	-0.877**	-1.320**	0.273	-0.531
		(-1.57)	(-2.64)	(-0.19)	(-0.21)	(-2.18)	(-2.27)	(0.83)	(-1.12)
Powerful		0.455***	0.283	0.173	0.236	0.240*	0.213	0.045	0.034
		(3.10)	(1.39)	(1.26)	(1.48)	(1.79)	(0.75)	(0.44)	(0.14)
Mkt_Ret		0.355	0.423	1.131*	0.894	0.752	-1.134	1.675***	-1.091
		(0.41)	(0.49)	(1.81)	(1.40)	(1.13)	(-0.79)	(3.35)	(-1.13)
Mkt_Volatility		5.946	1.624	2.427	-1.614	4.775	7.669	2.821	-10.070
		(0.79)	(0.21)	(0.49)	(-0.26)	(0.82)	(0.45)	(0.66)	(-0.92)
p-value for diff. in coef.		0.010		0.010		0.000		0.000	
Politician FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
S.E. Cluster by Politician and Year		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations		11736	15000	11736	15000	16920	9816	16920	9816
Pseudo R-Squared		0.6479	0.6953	0.6126	0.6130	0.6993	0.5663	0.6465	0.5298