

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 during their reelection bids legislators avoid disclosing profitable trades, as well as trades likely to be *perceived* as informed. Controlling for when congressional trades are executed, we document that politicians take full advantage of the lax disclosure requirements (45 days) to avoid disclosing stock trades prior to elections. More concerning, some legislators seeking reelection take advantage of lax enforcement of the 2012 STOCK Act and violate it 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 strongest. We also examine politicians' choice of disclosure *form* and show that, compared to electronic filings, trades filed on paper forms are more profitable, consistent with “paper” trades being more informed. Leveraging differences in politicians' systematic disclosure patterns, we identify a subset of “*Suspicious Politicians*” who consistently trade more profitably than their peers. Our findings highlight how weak disclosure rules and lax enforcement facilitate strategic behavior by political insiders, underscoring the need for stronger oversight to ensure politicians' accountability to their constituents.

Keywords: Congressional Trading; the STOCK Act; Informed Trading

JEL classification: G14; G18; D72; M41; M48

Data availability: All data are available from public sources identified in the paper.

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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, a.k.a., “congressional knowledge” (Gao and Huang, 2016; Christensen, Mikhail, Walther, and Wellman, 2017; and Jagolinzer, Larcker, Ormazabal, and Taylor, 2020). 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; Hanousek, Jo, Pantzalis, and Park, 2022). The public perception, fueled by extensive negative media coverage, is that congressional knowledge exists broadly among politicians and that it has been used to inform their 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.

Despite 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 (Ziobrowski, Cheng, Boyd, and Ziobrowski, 2004 and 2011; Eggers and Hainmueller, 2013 and 2014), particularly after the STOCK Act (Huang and Xuan, 2020; Belmont, Sacerdote, Sehgal, and Hoek, 2020). Taking a different approach, Abdurakhmonov, Snider, Ridge, and Hasija (2023) examine how investors react to Congressional Members’ disclosed trades and show that investors *perceive* Members’ trades as information-driven and respond accordingly.

In this study, we shift the focus from whether congressional trades are profitable to whether legislators *strategically disclose* their stock trades. Specifically, we investigate whether Members

of Congress strategically time and obfuscate the disclosure of their potentially informed trades to avoid electoral consequences. The STOCK Act does not prohibit Members of Congress from trading stocks, even those directly affected by their legislative duties. Instead, it imposes a disclosure requirement: legislators must report securities transactions exceeding \$1,000 within 45 days of execution.¹ However, the enforcement of the STOCK Act is weak, with minimal consequences.² Accountability has come, instead, from public scandals, and, ultimately, from voters who have the power to impose electoral consequences (Ferraz and Finan, 2008).³

The following anecdote exemplifies this strategic disclosure behavior. While serving on the House Transportation and Infrastructure Committee, Rep. Mike Garcia sold Boeing shares on August 10, 2020, shortly before the Committee released a highly critical report on Boeing's involvement in fatal crashes. Boeing's stock price fell sharply after the report's release. Rep. Garcia did not disclose the sale until November 23, after narrowly winning re-election and well beyond the STOCK Act's 45-day disclosure deadline.⁴

We provide systematic evidence on legislators' strategic disclosure practice, through strategic timing and choice of disclosure form. Our primary test focuses on strategic *timing*: specifically, whether legislators delay the disclosure of their likely informed trades to avoid the highly scrutinized election period. We then turn to the *form* of disclosure: that is, whether legislators obfuscate their likely informed trades by using paper filings, which are harder to access and process, instead of the more transparent electronic filings. Finally, using these suspicious

¹ The 45-day disclosure window for legislators stand in stark contrast to the two-business-day reporting requirement for corporate insiders. Prior to the STOCK Act, legislators were only required to disclose their trades annually.

² Violators typically face a nominal \$200 fine, which is often waived. See the financial disclosure instruction guide for the Members of Congress at <https://ethics.house.gov/financial-disclosure-instruction-guide/>.

³ In Appendix C, we document that incumbents identified in a *New York Times* expose as trading on "congressional knowledge" are nearly twice as likely as their peers to exit Congress.

<https://www.nytimes.com/interactive/2022/09/13/us/politics/congress-members-stock-trading-list.html>.

⁴ See <https://www.politico.com/news/2024/07/10/democrats-mike-garcia-boeing-stock-00167225>.

disclosure patterns as markers, we identify “suspicious” politicians (i.e., those who likely trade on congressional knowledge) and examine whether their trades have higher profitability than those of their peers.

In our first set of empirical 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 legislators’ disclosures.⁵ Given heightened scrutiny by political opponents and the media leading up to elections, we expect the disclosure of trades likely informed by congressional knowledge to vary with the election pressure. Our primary measure of likely informed trades is the return predictability (or implied profitability) of congressional trades. Following prior literature on corporate insider trading (Seyhun, 1986; Piotroski and Roulstone, 2005), we treat profitability as *prima facie evidence* that the trade was based on nonpublic congressional knowledge. Beyond profitability, we construct a secondary measure of likely informed trades based on two other trade characteristics: trade size (since large trades are more likely to be driven by information) and trades in politically sensitive firms (whose fortunes depend critically on congressional decision-making). We label these *Suspicious Trades*.⁶ Consistent with legislators strategically delaying the disclosure of informed trades to avoid the electoral consequences of stock-trading scandals, we find that congressional trades disclosed during electoral-pressure windows (i.e., the month immediately preceding primaries through the election season) exhibit lower profitability compared to other periods. Additionally, legislators are also significantly less likely to disclose *Suspicious Trades*

⁵ Given the lax enforcement of the STOCK Act’s disclosure requirements, only disclosed trades are observable. Two unobserved margins remain: (i) selective disclosure (i.e., non-disclosure through non-compliance with the law) or (ii) trading via channels that evade reporting requirements (i.e., a non-family member executes the trade). The incidence and profitability of undisclosed trades are unknown. Thus, inferences apply only to the disclosed sample – a limitation shared by prior studies of congressional trades.

⁶ We identify four industries – defense, financial services, healthcare and energy – for which both the House and the Senate have standing committees with continuing jurisdiction. The oversight, legislative, regulatory, and appropriations activities of these committees can materially affect firms operating in these four industries.

during electoral-pressure windows.

To strengthen our inference that legislators strategically time the disclosure of their trades to avoid negative electoral consequences, we conduct cross-sectional analyses. Specifically, we examine whether legislators' strategic disclosure behaviors vary with electoral pressure. Indeed, we find that the effect is more pronounced for legislators involved in tight election contests and for legislators representing economically distressed districts, where voter resentment of the perceived abuse of power by political elites is strongest. In addition, we conduct falsification tests that examine the disclosure behavior of retiring legislators, who do not face reelection pressure. In contrast to legislators facing election pressure, retiring politicians do not exhibit strategic disclosure behaviors.

While the evidence thus far suggests that legislators strategically time the disclosure of likely informed trades, our inference is confounded by the timing of legislators' *trading* actions. A legislator's decision to delay trading in the months prior to an election could, at least in part, explain our earlier finding that fewer suspicious trades are disclosed during electoral pressure windows.⁷ To isolate legislators' *disclosure* decisions, that is, to separate the timing of the disclosure from the timing of the trades, we conduct a second set of empirical tests, where we restrict the sample to congressional stock trades *executed* just before an election. We then examine legislators' *disclosure* timing for these trades. We separately consider two cases: (1) strategic disclosures that delay the disclosure until after the election, without necessarily violating the STOCK Act, and (2) the more egregious strategic disclosures that not only delay reporting trades until after the election, but in doing so, also violate the mandated 45-day disclosure period.

We start with a subset of congressional trades executed within 45 days before the election

⁷ Untabulated results also help mitigate this concern. Specifically, legislators' monthly trading volume does not differ significantly between pressure and non-pressure months.

or pseudo-election day (i.e., the Tuesday after the 1st Monday in November).⁸ For these trades, legislators can delay the disclosure past election day without violating the STOCK Act. We examine legislators' disclosure timing in election years, using nonelection years as a natural benchmark for comparison. We hypothesize that legislators are more likely to delay the disclosure past the *Tuesday in November* in election years compared to non-election years. For cleaner interpretation, we compare congressional trades executed the same number of days prior to the election or pseudo-election day by including *relative-day fixed effects*. This fixed-effects structure permits us to assess whether politicians strategically time their disclosures in election years relative to non-election years, after controlling for when the trade was executed.⁹ We find that, in election years, politicians are more likely to strategically time disclosure of their trades until after election day, consistent with politicians responding to electoral pressure. In cross-sectional analyses, we demonstrate that the strategically timed disclosure is more pronounced for *Profitable* and *Suspicious Trades*, as well as for legislators involved in tight election contests and those representing economically distressed districts.¹⁰ Once again, cross-sectional analyses support our interpretation that legislators' disclosure choices are affected by electoral considerations.

In the earlier anecdote, Rep. Mike Garcia delayed disclosing his stock sales until after winning his re-election bid and in so doing violated the STOCK Act's mandated 45-day disclosure period. To capture this more egregious form of delayed disclosure, our next set of tests examines

⁸ For brevity, throughout the paper, we refer to the *Tuesday after the 1st Monday in November* as the *Tuesday in November*. In election years, this Tuesday is election day; in nonelection years, we use it as a pseudo-election day.

⁹ We note that untabulated findings indicate that politicians' volume of trading activity does not differ across election and nonelection years.

¹⁰ An alternative explanation is that legislators are simply too busy campaigning before elections to trade or disclose their stock transactions. Untabulated tests suggest that legislators do not trade less during electoral-pressure windows, as monthly trading volume does not differ significantly between pressure and non-pressure months. Additionally, the cross-sectional results regarding *Profitable* and *Suspicious Trades* further mitigate this concern, as it is unlikely that legislators are too busy to disclose profitable trades during the pressure window but not too busy to disclose unprofitable trades.

whether legislators are 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., congressional trades executed at least 45 days before election day. If legislators adhered to the disclosure requirements, these trades would be disclosed before the election; any trades disclosed after the election are by design *delayed disclosures* that violate the STOCK Act.

Consistent with electoral pressure influencing disclosure behavior, we demonstrate that the likelihood of delaying disclosures beyond statutory requirements is greater in election years compared to non-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* and *Suspicious Trades*, the ones that are most likely informed by congressional knowledge and thereby vulnerable to criticism from the media and political challengers. Additionally, the tendency to delay disclosure 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 strongest. Collectively, these results provide consistent support for our inference that electoral pressure motivates legislators to delay disclosure of their 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. Unlike corporate insiders, Members of Congress 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 passed 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*.¹¹ The persistent use of paper filings 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 continue to use paper forms 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.

Finally, having documented legislators’ strategic choice of timing and form of disclosure, we examine whether we can use these disclosure patterns to identify opportunistic politicians, the ones most likely to have engaged in trading on private congressional knowledge. We find that these strategic disclosure patterns are indeed useful for identifying politicians prone to informed trading, as indicated by the significantly higher profitability of their trades compared to those of their peers.

This paper makes several contributions. First, we provide the first systematic evidence of legislators’ strategic disclosure of their stock trades, shifting the focus from trading performance to disclosure behavior. While prior studies find little evidence of abnormal returns,¹² we show that politicians themselves appear to view their trades as potentially inappropriate and manage the *visibility* of potentially informed trades through the timing and form of disclosures. Leveraging these disclosure patterns, we identify “suspicious politicians” who earn significantly positive alphas on trades executed when Congress is in session, when informational advantages are most

¹¹ See 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 yet been fully phased out.

¹² There are several empirical challenges when trying to detect legislators’ abnormal trading profits. First, there is no reliable method to distinguish trades motivated 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 potentially forgoing abnormal trading profits in exchange for greater political capital (Coval and Moskowitz, 2002; Eggers and Hainmueller, 2014; Mehta, Srinivasan, and Zhao, 2020; Ma, Pan, Rouen, and Wellman, 2022). Our findings caution against interpreting the lack of abnormal trading profits as evidence that politicians do not trade on congressional knowledge

likely. These findings help reconcile prior mixed results by showing that, although broad samples of political trades may be noisy due to diversification and liquidity motives, strategic disclosure behavior isolates trades more likely driven by congressional knowledge.

Second, we contribute to the literature that examines the role of electoral pressure in shaping decisions of politically connected parties. Our study is related to Mehta and Zhao (2020), who show that SEC-relevant legislators appear to influence the timing and severity of SEC enforcement actions in ways that protect their reelection prospects. Relatedly, Ramanna & Roychowdhury (2010) show that politically connected firms strategically manage earnings immediately before elections to avoid political scrutiny that could jeopardize the firm and its affiliated politicians. Bird, Karolyi, and Ruchti (2023) and Jennings, Kartapanis and Yu (2020) also show that elections create incentives for executives to alter their disclosure and financial reporting practices. We contribute by shifting the focus to elected politicians' personal financial disclosures and demonstrating that their disclosure behaviors are systematically shaped by electoral incentives. The tendency to delay or obscure disclosures is concentrated among incumbents facing reelection and is strongest in competitive races, providing evidence that politicians adjust transparency in response to expected voter scrutiny.

Finally, our findings have implications for regulatory design and enforcement. We show that lenient enforcement and procedural flexibility, such as extended filing windows and alternative reporting formats, potentially undermine the effectiveness of transparency mandates. Our evidence suggests that evaluating disclosure regimes requires attention not only to whether information is eventually released, but also to *when* and *how* the information becomes publicly accessible. More broadly, our findings add to the literature that underscores the complexities of accounting and disclosure regulations and the importance of enforcement for the effectiveness of

such regulations (e.g., Christensen, Hail, and Leuz, 2013, 2016; Leuz and Wysocki 2016).

2. Background and Hypothesis Development

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

Congressional stock trading has long been a source of public concern. Members of Congress routinely gain access to nonpublic information through hearings, investigations, and policy deliberations; their legislative actions can directly affect firm and industry prospects. Notably, both the Senate and the House have standing committees that oversee key industries, including defense, financial services, energy and healthcare.¹³ In addition to ad hoc investigative hearings, standing committees hold annual oversight hearings with invited representatives from key industries to support and inform legislators' regulatory and funding decisions.¹⁴

The *Stop Trading on Congressional Knowledge (STOCK) Act*, enacted in 2012, explicitly prohibits 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. The Act clarifies that legislators, their families and staff are subject to the same Rule 10b-5 insider trading prohibitions as corporate insiders.

Not an outright restriction 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 to be weakly enforced. Violators are subject only to

¹³ In the Senate, these are Armed Services (defense), Banking, Housing, and Urban Affairs (financial services), Energy and Natural Resources (energy), and Health, Education, Labor, and Pensions (healthcare). In the House, these are Armed Services (defense), Financial Services (financial services), Energy and Commerce (energy and healthcare), and its Health Subcommittee (healthcare).

¹⁴ See the Congress meetings and hearings at <https://www.congress.gov/help/committee-schedule-meetings-hearings>.

nominal \$200 fines, which are often waived.¹⁵ Oversight is decentralized, leaving compliance largely to politicians' self-policing. As a result, accountability for questionable trades has often come not from regulators but from the electoral arena. Political challengers and national committees of both parties often fan the flames of the public scandals by directly attacking incumbents for their stock trades in the press and on social media.

The following examples illustrate the electoral consequences and reputational costs of congressional stock trading. During the 2022 midterms, in New Jersey's highly competitive 7th District, the incumbent Democrat Tom Malinowski came under heavy fire from his Republican opponent, Tom Kean, and national GOP operatives. They branded him with the hashtag *#tradingtom* and the nickname "Trader Tom" on social media. Campaign statements accused Malinowski of "*betting against the American economy during the pandemic*", claiming he had "*blown through the law more than 140 times, earning himself a bipartisan ethics probe.*"¹⁶ 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.¹⁷

Media investigations have further elevated the issue.¹⁸ *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

¹⁵ 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".

¹⁶ "Stock trading law a campaign issue in NJ", *Asbury Park Press*, June 16, 2022.

¹⁷ See <https://www.nytimes.com/2020/06/15/us/politics/congress-trading-stock-loeffler-burr.html>.

¹⁸ See https://www.washingtonpost.com/investigations/why-i-chose-to-run-congressional-challengers-use-bill-backed-by-drug-companies-against-incumbents/2018/05/13/b171594e-4d8f-11e8-84a0-458a1aa9ac0a_story.html.

to the disclosure practices of congressional trades.¹⁹ Shortly afterwards, the *New York Times* published an expose entitled “These 97 Members of Congress Reported Trades in Companies Influenced by Their Committees”, identifying Members of Congress who had traded stocks of companies directly under their committees’ jurisdictions.²⁰

Wary of the negative electoral consequences, Members of Congress have incentives to strategically time their disclosures, select the form of their disclosures, and otherwise exploit the system’s loopholes. The weak enforced regime of the 2012 STOCK Act provides the institutional foundation for our research that examines how legislators manage their disclosure practices to avoid political backlash and electoral consequences.

2.2 Existing Research and Hypothesis Development

Despite the concern that Members of Congress trade on and benefit from nonpublic information arising from congressional knowledge, existing research provides mixed evidence on whether Members earn abnormal returns from their stock trades. Recent work finds little evidence that politicians earn abnormal returns after the STOCK Act (Huang and Xuan, 2023; Belmont, Sacerdote, Sehgal, and Hoek 2020).

Rather than examining whether congressional members earn abnormal trading profits, we shift the focus from trading activity alone to legislators’ strategic disclosure choices. Specifically, we focus on the role of electoral pressure in legislators’ strategic disclosure decisions. We expect electoral pressure to intensify as legislators approach their next re-election - every two years for Representatives and every six years for Senators (Shepsle, Van Houweling, and Abrams, 2009;

¹⁹ See *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.

²⁰ See “These 97 Members of Congress Reported Trades in Companies Influenced by Their Committees”, the *New York Time*, September 13, 2022.

Lindstadt and Vander Wielen, 2011). Accusations of congressional insider stock trading garner sensational press coverage, especially during 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.²¹ Such accusations negatively affect incumbents' electoral outcomes (Ferraz and Finan, 2008). To avoid such political scandals, during highly scrutinized election periods, Members of Congress may strategically disclose fewer stock trades that are likely perceived as driven by congressional knowledge. This leads to our first hypothesis:

H1: During periods of high electoral pressure, congressional members report fewer trades likely perceived as informed by congressional knowledge.

To identify likely informed trades, we primarily rely on the profitability of a trade. Lower profitability of the disclosed trades is *prima facie* evidence that the trades were not based on privileged "congressional knowledge." Beyond that, trades that can be construed as informed by congressional knowledge include large trades and in shares of firms whose fortunes depend critically on congressional decision-making.

Hypothesis 1, while informative about legislators' disclosure choices, is confounded by their actual trading decisions. Our next hypothesis specifically focuses on legislators' disclosure of their trades, conditional on the trades having already taken place. We conjecture that congressional members have incentives to delay disclosing stock trades executed before an election until after the election. The tendency to delay disclosure past the *Tuesday after the 1st Monday in November*, i.e., election day in election years and a pseudo-election day in non-election years, is likely stronger in election years. This leads to our next hypothesis:

H2: For congressional stock trades executed before an election, Members of Congress are more likely to delay disclosure until after the Tuesday after the 1st Monday in November in

²¹ For instance, a *Washington Post* article, "Why I chose to run: Congressional challengers use bill backed by drug companies against incumbents", May 13, 2018.

election years compared to nonelection years.

There are two possible scenarios for trades executed before an election. If the trade was executed within 45 days of the *Tuesday after the 1st Monday in November*, then the legislator can delay disclosure until after election without violating the STOCK Act. However, if the trade was executed more than 45 days before that day, delaying disclosure until after the election would constitute a violation of the STOCK Act. Nevertheless, as the earlier anecdote about Rep. Garcia suggests, some politicians may still choose to violate the law if they perceive the benefits of noncompliance (e.g., avoiding negative backlash from allegation of insider trading immediately before an election) outweigh the expected costs, which include modest fines and infrequent ethics investigation.²²

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

An interesting institutional feature of our setting is that, in addition to deciding on the *timing* of their disclosures, legislators also choose the *form* of their disclosure. Although the House Ethics Committee explicitly encourages electronic filing to enhance accessibility and transparency, legislators are still permitted to decide how to report their trades – on paper forms or using the electronic filing system.²³ If legislators believe that filing paper forms limits or delays access to

²² To our knowledge no legislator has been forced to resign because they traded on congressional knowledge or violated the STOCK Act's disclosure rules.

²³ The STOCK Act initially mandated electronic filing and online posting of Periodic Transaction Reports (PTRs).

information about their trading activities, then we expect them to use paper forms for their informed trades. This leads to our third hypothesis:

H3: Congressional trades reported on paper forms are more profitable than those reported using the electronic system.

3. Data and Sample

The primary data used in this paper consists of stock transactions by Members of Congress reported following the passage of the STOCK Act in 2012. We obtain congressional trading data from 2iQ Research's *Capitol Trades Database*. The transaction-level trading data is compiled from the Periodic Transaction Reports (PTRs) submitted to the Clerk of the House of Representatives and to U.S. Senate Select Committee on Ethics. The *Capitol Trades Database* begins in January of 2013. Thus, our sample period covers the 113th to the 118th Congresses (2013-2023).²⁴

Starting with the 128,596 stock transactions disclosed during our sample 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.²⁵ These steps reduce the sample to 93,557 trades, which is our baseline dataset. There are 3,002 unique stocks traded by 362 unique politicians. Given that 960 individuals served in Congress between 2013 and 2023, approximately 37.7% of congressional members disclosed at least one stock transaction during our sample period. Table 1 summarizes the construction of our sample and the derivation of test-specific subsamples.

However, a subsequent amendment to the Act adopted on April 15th, 2013, altered several 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.

²⁴ Although the STOCK Act was signed into law on April 4, 2012, its implementation was delayed in practice. Consistent with this implementation delay, there is no public PTR record in 2012. See <https://federalnewsnetwork.com/congress/2012/09/court-puts-temporary-hold-on-stock-act-reporting-requirement/>. Also see <https://www.govexec.com/oversight/2012/12/obama-signs-stock-act-delay-senior-executives/60051/>.

²⁵ 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.

We merge additional data with the *Capitol Trades* Database. Stock return data comes from CRSP and financial reporting data from Compustat. Analyst coverage data are obtained from I/B/E/S. Legislators' characteristics, including time served in Congress (tenure), their home state and party affiliation are collected from the *Biographical Directory of the United States Congress*. Data on Members announcing retirement or decisions to leave office early, as well as dates of primary elections, is 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. Dates over which Congress is in-session are taken from historical floor calendars available from 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

Our primary empirical analysis examines the strategic *timing* of legislators' stock trades disclosures. Given heightened scrutiny from political opponents and voters in the weeks and months prior to elections, we expect legislators' disclosure behaviors to vary with election cycles. Section 4.1 examines whether legislators delay the disclosure of their likely informed trades during electoral-pressure windows (i.e., the month immediately preceding primaries through election season). Tests presented in Sections 4.2 and 4.3 isolate disclosure effects after controlling for the timing of trade execution and document whether politicians' delay disclosures until after elections to evade accountability.

4.1 Reduced Disclosure of Informed Trades during Electoral Pressure Windows

4.1.1 Variables, Research Design and Baseline Results

We construct a variable, *PressureWindow*, to capture periods when electoral pressure is

heightened for legislators seeking re-election.²⁶ For legislators from heavily partisan districts (i.e., 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 formally test whether politicians avoid disclosing informed trades during the electoral pressure windows, we estimate OLS-based regressions at the transaction level using the following specification:

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

where δ_j and γ_c denote politician and congressional session fixed effects, respectively, and standard errors are clustered by politician and congressional session.

As noted above, we use two proxies for potentially informed trades. Our primary measure is the profitability of politicians' trades, which we treat as *prima facie evidence* that the trade was informed by private congressional knowledge. The focus on trade profitability to infer informed trades is consistent with the existing literature examining corporate insider trading. Our second proxy, labeled *Suspicious Trades*, identifies trades that can be construed as informed because the trade is large (over \$15K)²⁷ or involves the stock of firms whose fortunes depend critically on congressional decision-making. Industries with standing committees in both Houses of Congress

²⁶ We include instances where Members of Congress are seeking election to another office, e.g., a House member seeking election to the Senate or a Senator seeking election to the Governorship of their home state.

²⁷ PTR filings report transaction values only in dollar ranges rather than exact dollar amounts. The lowest reported range is \$1,001-\$15,000. We classify a trade as large if its reported value exceeds this lowest bracket.

are identified as facing significant congressional oversight. These include the defense, financial services, energy and healthcare industries.

To measure implied profitability, we follow Belmont, Sacerdote, Sehgal, and Hoek (2020) and calculate the Buy-and-Hold Abnormal Returns (*BHARs*) for each trade, denoted as *Profit*.²⁸ *BHARs* are computed over 30-, 45-, and 60-calender-day holding periods using the Fama-French four-factor model (Fama and French, 1993; Carhart, 1997). For purchases, *Profit* equals the *BHAR*; for sales, *Profit* equals the negative of *BHAR*, 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.²⁹ In addition, we also include the raw cumulative 45-day return (*RAW_D45*) as this measure is readily observable.

Table 2 presents descriptive statistics for our variables of interest and control variables. Across all cumulation periods, the average *Profit* of trades disclosed during the *PressureWindow* are significantly negative, while the corresponding *BHARs* are insignificantly different from zero in the *Non-PressureWindow*.³⁰ Importantly for our research question, the mean differences in the *BHARs* (*_D30*, *_D45*, *_D60*) and *RAW_D45* for trades disclosure during *PressureWindow* versus *Non-PressureWindow* are negative and significant. This univariate evidence suggests that legislators, when facing electoral pressure, strategically disclose less profitable trades. Similarly, legislators are also less likely to disclose *Suspicious Trades* during *Pressure Windows*.

We include controls for firm characteristics, such as *ROA*, *Book-to-Market ratio*, market

²⁸ 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.

²⁹ 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 future price movement when the market learns what the Member already knows. However, the timing of the public revelation is uncertain. Thus, we run our tests using alternative holding periods ranging from 30 to 60 days following the execution of the trade. Findings are similar using the alternative holding periods.

³⁰ The *BHAR* results for trades disclosed during *Non-PressureWindows* align with prior findings indicating that the mean abnormal return for congressional trades is not significantly different from zero (e.g., Belmont et al., 2022).

capitalization (*Size*), the number of analysts covering the traded stock (*#Analysts*). In addition, we 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 (Eggers and Hainmueller, 2014; Mehta, Srinivasan, and Zhao, 2020). Detailed definitions of all variables are provided in Appendix A.

Two noteworthy statistics in Table 2 are first, *Disclosure Lapse* – the number of days from execution to disclosure – averages 43 days in the non-pressure window versus 49 days in the *PressureWindow* (a 14% increase) and second the rate of *Violation* of the STOCK Act is 12% in the non-pressure window versus 15% in the *PressureWindow* (a 25% increase). The longer disclosure lapses and higher violation rate during *PressureWindow* are consistent with strategic delays in disclosure by politicians in response to electoral pressure.

Table 3 Panel A presents the regression analysis of our baseline model. A negative and significant β_1 coefficient in Model (1) supports the hypothesis that legislators avoid disclosing informed trades during periods of heightened electoral pressure. Consistent with expectation, the coefficients on *PressureWindow* are negative and significant across all *BHAR* horizons in Panel A. Economically, congressional trades disclosed during the electoral pressure period yield between 0.2% and 0.5% lower mean *BHARs* relative to trades disclosed outside this period. The reduction is 0.7% when raw returns are used (*RAW_D45*). Most control variables show no systematic patterns, with the exception of *HomeState*, which has a significantly negative coefficient indicating that politicians' *HomeState* trades are unprofitable.³¹ Inferences are similar when we use the

³¹ Many of the control variables have significant coefficient estimates in Table 3 when *Suspicious Trades* is the

indicator variable. *Suspicious Trades*, as an alternative measure of informed trades. Together the results reported in Panel A lend support to the hypothesis that legislators avoid disclosing informed trades (i.e., *Profitable* and *Suspicious Trades*) during periods of heightened electoral pressure.

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, who face no re-election pressure. The results are presented in Table 3 Panel B. In contrast to the results in Panel A, the coefficients on *PressureWindow* are insignificant. While the smaller sample size reduces statistical power, the opposite sign of the coefficient estimates of interest (relative to the baseline results) indicates that the null results in Panel B are not driven solely by lower power. Thus, we interpret the falsification test results as supporting our interpretation of the findings in Panel A that legislators facing electoral pressure strategically time their disclosures.

Finally, Table 3 Panel C presents various robustness checks using *BHAR_D45* and the indicator variable, *Suspicious Trades*.³² Columns 1 and 2 restrict the sample to trades executed when Congress is in session.³³ 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 disclosure. By focusing exclusively on the subsample of trades executed while Congress is in session, we mitigate this concern. The coefficients for *PressureWindow* remain negative and significant, mirroring the baseline results presented in Panel A.

Columns 3 and 4 use a subsample that controls for potential seasonality effects in

dependent variable because the four industries identified as heavily influenced by congressional decision making are populated by larger, more regulated firms.

³² Results for *BHAR_D30*, *BHAR_D60*, and *RAW_D45* are qualitatively similar.

³³ 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" while in session enhances their trading outcomes.

congressional trading profitability. Specifically, we construct a *pseudo-PressureWindow* for non-election years using the same calendar dates as the *PressureWindow* in the last election. Observations outside of these pressure windows are dropped to allow for a direct comparison of trades executed on identical calendar dates across election and non-election years. The coefficients on *PressureWindow* remain negative and significant, consistent with the conclusion that the lower profitability of disclosed trades and the less frequent disclosure of *Suspicious Trades* is attributable to electoral pressure rather than to seasonal patterns in legislators' trading activities.

Finally, Column 5 addresses potential biases from overlapping return periods. 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 (-0.005) and significant, consistent with the baseline findings.

4.1.2 Cross-sectional Analyses

To strengthen our interpretation of baseline results, we perform cross-sectional analyses to assess 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 share of votes in the upcoming election - either the primary or the general - falls below 60%, whereas *Safe* legislators surpass this threshold in both elections. This classification is anchored to the upcoming election, 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 (Hibbing and Alford, 1982; Lewis-Beck and Stegmaier, 2000; Duch and Stevenson, 2010; Palmer and Whitten, 2011; Treisman, 2015). To capture this variation, we employ a district-year-level proxy: the unemployment rate. We expect legislators representing *High Unemployment* districts (with an above-national-median unemployment rate) to face stronger incentives to strategically manage trade disclosures around elections.

Table 4 presents the cross-sectional results. Panel A shows that the negative effect of *PressureWindow* is concentrated among *Vulnerable* legislators. For this group, the coefficients are significantly negative and economically meaningful. For our primary measure, *BHAR_D45*, returns are 0.8% lower when electoral pressure is high. Similarly, *Suspicious Trades* are 2.9% less likely to be disclosed. In contrast, the coefficients for *Safe* legislators are not significantly different from zero. Differences in the coefficient estimates across the two groups are statistically significant (p -values = 0.000 and 0.004, respectively). Panel B of Table 4 demonstrates that the coefficients on *PressureWindow* are only significantly negative when legislators hail from economically depressed districts. In contrast, legislators from districts with lower unemployment rates do not exhibit the same strategic disclosure behavior.

4.2 Isolating the Disclosure Effect – Elections and Strategic Disclosures

The analysis in Section 4.1, while informative, does not fully disentangle legislators' two decisions, when to *execute* a trade and when to *disclose* it. In this section we specifically control for trade execution dates, to isolate politicians' strategic disclosure decisions. To do so we focus on trades executed within 45 days *prior* to the 2nd Tuesday after the 1st Monday in November

(election day in election years and pseudo-election day in non-election years). These trades can be disclosed after the election day without violating the STOCK Act. Figure 1.A illustrates this sample construction

Using this subsample, we estimate the following regression:

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

The dependent variable, *Post Elec Disc*, is set to one when a trade is disclosed after the election (or pseudo-election) day.³⁴ The key independent variable, *Elect Year*, equals one if a congressional trade occurs in an election year. A positive and significant β_1 suggests that legislators are more likely to strategically delay disclosing trades until after the *Tuesday in November* in election years. We include *relative-day fixed effects*, defined as the number of days between the trade execution date and the *Tuesday in November*, to permit a direct comparison of legislators' disclosure decisions for trades executed the same number of days prior to election and pseudo-election days. Control variables and clustering follow those in Model (1).

Table 5 reports a positive and significant coefficient on *Elect Year* across both specifications, OLS in Column 1 and Logit in Column 2. Economically, the likelihood of a post-election disclosure (i.e., after the election or pseudo-election day) is 4.6% higher in election years. These findings are important because they demonstrate that politicians' disclosure decisions are affected by electoral pressure after directly controlling for the timing of their trades.

To provide further support for our interpretation, we conduct cross-sectional analyses, reported in Table 6. Columns 1 and 2 examine *Profitable* and *Unprofitable* trades, based on

³⁴ *Post Elec Disc* also requires the trade to be disclosed no later than 90 days after the election (or pseudo-election) day. This 90-day window is intended to capture *Post Elec Disc* cases plausibly tied to election-related disclosure management. We impose this restriction primarily to exclude trades disclosed after unusually long delays—sometimes as much as two or three years later—as such disclosures are unlikely to reflect election-related timing incentives. Our results are not sensitive to the 90-day cutoff.

BHAR_D45. The coefficient on *ElectYear* is positive and significant for *Profitable* and *Unprofitable* trades (0.068 vs. 0.027, respectively), indicating that legislators are more likely to delay disclosing all trades in election years. Interestingly, however, legislators' tendency to delay disclosures in election years is more pronounced for trades that are likely to be informed by congressional knowledge (i.e., *Profitable* trades), as the difference in coefficient estimates is statistically significant ($p = 0.000$).

Columns 3 and 4 examine whether the propensity to delay disclosing stock trades until after elections is more prominent for *Suspicious Trades*, the ones we conjecture could be construed as informed by congressional knowledge. Again, while politicians are more likely to delay disclosing all trades in election years, this tendency is greater for *Suspicious Trades* and the difference in coefficients between the two groups is statistically significant ($p\text{-value} = 0.034$).

Columns 5 and 6 examine cross-sectional variations arising from electoral vulnerability. The results suggest that *Vulnerable* legislators exhibit a greater tendency to delay disclosures in election years compared to *Safe* politicians. The difference in coefficient estimates for the two groups is statistically significant ($p\text{-value} = 0.000$), indicating that electoral pressure has a greater effect on legislators' strategic delay when they are in a more competitive race.

Columns 7 to 8 split the sample based on district-level unemployment to capture variation in voters' resentment of the perceived abuse of power by political elites. Results reveal that politicians from *High Unemployment* areas are significantly more likely to delay disclosures in election years, while the coefficient for *Low Unemployment* areas is negative and marginally significant. The difference in coefficients for the two groups is significant ($p\text{-value} = 0.000$). Taken together, results in this section indicate that politicians take advantage of lax disclosure requirements under the STOCK Act to avoid accountability to their constituents.

4.3 Delayed Disclosure and Violations of the STOCK Act

The previous section suggests that, within the bounds of the STOCK Act's 45-day disclosure requirement, legislators are more likely in election years to delay disclosure of their stock trades until after the election. In this section, we examine a more egregious form of delayed disclosure: disclosures delayed beyond the STOCK Act 45-day reporting requirement, as was the case with Rep. Garcia. Specifically, we focus on trades executed *more than 45 days* before an election and examine legislators' disclosure timing around the election. If electoral incentives are sufficiently high, some legislators may exploit the weak enforcement of the STOCK Act and strategically delay the disclosure of their trades until after the election, *despite violating the law*.

For these tests, we construct two subsamples, as illustrated by Figure 1.B. The first, *General Sample*, retains all congressional trades executed between January 1st and 45 days prior to election day or a pseudo-election day. This *General Sample* includes 62,173 Congressional trades. The second, *Limited Sample*, excludes trades executed more than 135 days prior to election day or a pseudo-election day, effectively limiting our focus to trades executed within a 90-day window (i.e., executed between 135 and 45 days prior to the *Tuesday in November*) when voter attention is heightened and the incentive to conceal controversial trades is strongest in election years. The *Limited Sample* includes 19,292 congressional trades.

Using these two subsamples, we re-estimate Model (2) using both OLS and Logit regressions, replacing *Post Elect Disc* with *Delayed Disclosure*. Similar to *Post Elect Disc*, the dependent variable, *Delayed Disclosure*, is set to one when a trade is disclosed after the election or pseudo-election day.³⁵ The difference in the dependent variables is when *Delayed Disclosure* is

³⁵ As with *Post Elec Disc*, for the indicator variable *Delayed Disclosure*, disclosures delayed beyond the 90-day post-election window are assigned a value of zero, because disclosures delayed beyond 90-days post-election are unlikely to be election motivated. The results are not sensitive to this restriction.

set to one the legislator violated the disclosure requirements of the STOCK Act by waiting to disclose until after the election or pseudo-election day. The key independent variable, *Elect Year*, is an indicator equal to one if a trade is executed in an election year. Control variables, fixed effects, and clustering follow those in the previous section.

Table 7 presents the results. Columns 1 and 2 present the OLS and Logit results for *Limited Sample*, and Columns 3 and 4 present the results for *General Sample*. Across all specifications, the coefficient on *Elect Year* is positive and significant, suggesting that legislators are swayed by electoral pressure when disclosing their stock trades, even though the delay violates the disclosure requirement of the STOCK Act.

To provide further support for our interpretation of these findings, we conduct cross-sectional analyses to assess whether legislators' tendency to delay disclosure and violate the STOCK Act is concentrated among informed trades (i.e., *Profitable* or *Suspicious Trades*) and among legislators facing tight elections and those representing economically distressed districts. Table 8 presents OLS regressions for the *Limited* and *General Samples* in Panels A and B, respectively.³⁶ The results are largely consistent with earlier cross-sectional findings: electoral pressure has a greater effect on legislators' decision to delay disclosure until after elections for informed trades (i.e., *Profitable* and *Suspicious Trades*) and when the legislator faces a competitive race or hails from economically depressed district.

5. Additional Analysis

5.1. Form of Disclosure: Paper Forms vs Electronic Filings

In addition to choosing the *timing* of their disclosures, politicians also choose the *form* of

³⁶ In Table 8 we present OLS regressions so that we can present F-test to assess whether the coefficient estimates differ across the subsamples. To ensure that our findings are robust, we also re-run the cross-sectional analyses using Logit models with qualitatively similar results.

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 Ethics Committee’s guidelines, a nontrivial share of trading activities (63.4%) during the sample period is reported on paper forms. Examples of paper and electronic filings are provided in Appendix D.

Interestingly, comparing the details of trading activity reported on paper forms versus in electronic filings demonstrate that the two disclosure forms differ significantly. First, as reported in Table 9 Panel A, legislators bundle and report on a single paper form a substantially larger number of individual transactions (an average of 22.72 transactions per paper form versus only 8.36 per electronic filing). Second, the average dollar value of trades reported on paper is also significantly larger than the average dollar value of trades reported in electronic filings (\$28,314 per transaction for paper filings vs. \$25,737 for electronic filings).³⁷ Taken together, these statistics suggest that some politicians bundle multiple larger transactions to report them on paper forms, a less accessible format than electronic filings.

Panel B documents the transition from paper forms to electronic filings.³⁸ In 2013, no transactions were filed electronically. Two years later, the majority were electronic filings (59.15% in 2015), and by 2020 over 80% of filings were electronic. Nevertheless, some politicians

³⁷ The dollar value per trade uses the midpoint of each transaction’s reported range.

³⁸ 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). 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. These descriptive statistics suggest that some legislators revert to paper filings to lower the visibility of large trades that they disclosed in violation of the STOCK Act.

continued to rely on paper forms – 15.12% of filings still use paper forms as recently as 2023. As of 2025, paper filings have yet to be fully phased out. Politicians who continue to file paper forms beyond 2015 may do so because they are not technologically adept or because they want to make their disclosures less accessible. If the latter prevails, then we expect “paper trades” to be more informed and thus more profitable than those disclosed electronically.

In Panel C, we compare the profitability of trades disclosed on paper forms versus those filed electronically. When we include all sample years, the average abnormal returns (i.e., *BHAR_D30*, *BHAR_D45*, and *BHAR_D60*) are indistinguishable across the two disclosure forms. However, the power of this test is likely compromised during earlier transition years when the use of paper filings was less likely to be driven by strategic behavior. Once we drop the transition years, 2013-2015, the differences in the trading profits become economically and statistically meaningful: *BHAR_D45* and *BHAR_D60* for paper trades exceed those for electronic filings by roughly 20 basis points. As we sequentially exclude more of the early years in the sample period, the profitability gap widens to 25-29 basis points. The largely monotonic strengthening of the profitability premium suggests that, over time, as electronic filing becomes the norm, trades disclosed on paper are increasingly associated with private congressional knowledge. This evidence points to legislators’ potential strategic choice of disclosure form to conceal likely informed trades.

5.2 Use Strategic Disclosure Patterns to Identify Suspicious Politicians

So far, transaction-level analyses reveal that likely informed congressional trades are more likely to be disclosed beyond the 45-day requirement and reported on paper. In this section, we ask whether we can use observed strategic disclosure patterns to identify legislators that potentially trade on congressional knowledge. We conjecture that legislators who report their trades on paper

forms or have a proclivity for untimely disclosure (i.e., often reporting their trades more than 45 days after execution) are more likely to trade on private congressional knowledge and thus earn higher abnormal returns than their peers.

We classify individual legislators as *Suspicious* or *Non-Suspicious* based on the *timing* and *form* of their stock trade disclosures. 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 mean (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 2016 or later, despite the widespread adoption of electronic filings.³⁹ The sample size drops because the second classification requires observing a politician's filing method after 2016. We also define a *Suspicious_Superset*, capturing members flagged under both criteria.

Table 10 Panel A presents regression results with the dependent variable, *BHAR_D45*, regressed on these indicators of politicians with a propensity for suspicious disclosure behaviors. For each case, we find that in general suspicious politicians earn significantly higher abnormal returns than their peers (0.3% higher 45-day buy and hold returns).

Finally, we exploit the additional insight gained from legislators' strategic disclosure behavior to identify trades informed by congressional knowledge – something that the prior literature has struggled to do. In Panel B, we report *Suspicious Politicians'* buy-and-hold mean returns for trades executed when Congress is in session, when they are most likely to benefit from congressional knowledge. Interestingly, the mean alphas are positive and largely significant across

³⁹ Our definition of *Suspicious_Form* assumes that, by 2016, nonstrategic politicians have transitioned to electronic filing. While the choice of a cutoff year is subjective, we note that the results are fairly robust to alternatives. The statistical significance strengthens modestly when we assume a later transition year (e.g., the coefficient for 2017 is 0.002 with a *t*-stat of 2.11) and weakens when we assume an earlier one (e.g., the coefficient for 2015 is 0.002 with a *t*-stat of 1.55).

various holding periods and across all three classification criteria of suspicious politicians. The finding that legislators earn significantly positive alphas on their ‘in-session’ trades potentially reconciles our evidence with previous literature that fails to find consistent evidence that political insiders trade profitably. Because legislators may trade for diversification and liquidity reasons, a general sample of political insider trades may not yield significant results due to a lack of power. We show that the strategic disclosure behavior identified in this study can help researchers identify trades motivated by congressional knowledge.

6. 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 implied profitability. Additionally, these trades are less likely to be *Suspicious Trades* that can be construed as being informed by congressional knowledge. Next, employing a fixed effects structure that controls for when congressional trades are executed, we find legislators take advantage of lax disclosure requirements to strategically delay the disclosure of their trades until after elections, thereby reducing the risk of attracting public scrutiny when the attention of voters and political opposition are at a peak. Second, we directly demonstrate

that legislators are willing to violate the law and delay disclosing stock trades until after their re-election bids. Taken together, these systematically timed disclosures demonstrate that politicians' electoral concerns outweigh their adherence to the intent or the letter of the law. Instead, they take full advantage of lax disclosure requirements and lax enforcement 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 analysis to the politician level, we classify legislators based on their suspicious disclosure behaviors. Legislators with a higher propensity to violate the 45-day disclosure requirement as well as legislators who continued using paper filings after 2016 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' informed 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 as well as 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. Closing loopholes and strengthening enforcement are necessary to hold Members of Congress to the same standards of transparency and ethical conduct that apply to corporate insiders.

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Appendix A Variable definitions

Dependent variables

Variable	Definition	Source
<i>BHAR_D30 / BHAR_D45 / BHAR_D60</i>	Buy-and-hold abnormal return measured over the 30/45/60 calendar days following the transaction date (Day 0). For each trade, we calculate the stock's buy-and-hold raw return from Day 0 through Day 30/45/60 and compare it with the buy-and-hold return predicted by the Carhart four-factor model (Fama and French, 1993; Carhart, 1997). The difference between the raw return and the factor-model benchmark represents the abnormal return over the holding period. For sell transactions, the measure is multiplied by -1.	2iQ and CRSP
<i>RAW_D45</i>	Buy-and-hold raw return over the 45 calendar days following the transaction date. For sell transactions, the measure is multiplied by -1.	2iQ and CRSP
<i>Suspicious Trades</i>	Indicator variable equal to one for trades that are more likely to be informed by Congressional Knowledge. A trade is classified as suspicious if its reported dollar value exceeds \$15,000 (i.e., not in the smallest bracket of trade size) or if the traded firm operates in one of four industries whose fortunes depend critically on congressional decision-making: defense, healthcare, finance, and energy.	2iQ and CRSP
<i>Post_Elec_Disc</i>	Indicator variable that captures strategic disclosure following elections. For trades executed within 45 calendar days prior to an election day (or a pseudo election date in non-election years), <i>Post_Elec_Disc</i> is set to one if the trade is disclosed after the election day (or the pseudo election day) but within 90 days following that date, and zero otherwise. Election day follows U.S. federal law as the Tuesday after the 1 st Monday in November. Figure 1.A illustrates the timing structure.	2iQ
<i>Delayed Disclosure</i>	Indicator variable for egregious disclosure delays that violate the STOCK Act. For trades executed more than 45 calendar days before an election day (or pseudo election date in non-election years), <i>Delayed Disclosure</i> is set to one for trades disclosed after election day (or a pseudo election day) but within 90 days following that date, and zero otherwise. Election day is defined under U.S. federal law as the first Tuesday following the first Monday in November. Figure 1.B illustrates the timing structure.	2iQ

Appendix A (continued)
Variable definitions

Independent variables

Variable	Definition	Source
<i>PressureWindow</i>	Electoral pressure window is defined as the period from one month prior to the politician's state-specific primary election through the general election in election years. The indicator equals one if a trade is disclosed during this window and zero otherwise.	BallotPedia
<i>ElectYear</i>	An indicator variable that equals one if a trade is executed in an election year and zero otherwise.	2iQ
<i>Suspicious_Timing</i>	An indicator equal to one if a politician's propensity to disclose trades beyond the 45-day window of the STOCK Act exceeds the sample mean (13%), and zero otherwise. This variable is constructed at the politician level.	2iQ
<i>Suspicious_Form</i>	An indicator equal to one if a politician continues to use paper filings in 2016 and beyond, and zero otherwise. This variable is constructed at the politician level.	2iQ
<i>Suspicious Superset</i>	An indicator equal to one if a politician is classified as suspicious under both <i>Suspicious_Timing</i> and <i>Suspicious_Form</i> , and zero otherwise. This variable is constructed at the politician level.	2iQ
<i>ROA</i>	Traded firm's net income for the current fiscal year scaled by total assets at the prior year-end.	Compustat
<i>Book-to-mkt</i>	Traded firm's book value of assets divided by its market capitalization at the prior year-end.	Compustat and CRSP
<i>Size</i>	The natural logarithm of the traded firm's market value of equity plus the book value of its debt at the end of the prior year.	Compustat and CRSP
<i>Leverage</i>	Traded firm's book value of total debt to total assets at the end of the prior year-end.	Compustat
<i>#Analyst</i>	The number of analysts following the traded firm in the trading year. Logged transformation is used in regression models.	I/B/E/S
<i>HomeState</i>	An indicator that equals one if the traded firm is headquartered in the politician's home state and zero otherwise.	2iQ

Appendix A (continued)
Variable definitions

Other variables

Variable	Definition	Source
<i>Vulnerable</i>	An indicator variable that equals one if a trade is made by vulnerable politicians, and zero otherwise. Electoral vulnerability is determined based on the results of the upcoming election. A politician is classified as <i>Vulnerable</i> if their share of votes in either the primary or the general election falls below 60%, whereas a politician whose share of votes exceed 60% in both elections is classified as <i>Safe</i> .	CQ Press Voting and Election Collection; Legislative Effectiveness Scores (LES)
<i>High_Unemploy</i>	An indicator variable that equals one if a trade is made by a politician who hails from a district or state with an unemployment rate greater than the national average in the year of the trade; otherwise, it is classified as <i>Low Unemployment</i> .	Bureau of Economic Analysis; United States Census Bureau
<i>Profitable</i>	An indicator variable that equals one if a trade's <i>BHAR_D45</i> is positive, and zero if negative.	2iQ and CRSP
<i>Disclosure_Lapse</i>	The number of days between the execution date of a trade and its disclosure date.	2iQ
<i>Violation</i>	An indicator that equals one if a trade violates the 45-day disclosure requirement under the STOCK Act, and zero otherwise.	2iQ

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	2024-03-05	South Carolina	2024-06-11
Arkansas	2024-03-05	Oklahoma	2024-06-18
California	2024-03-05	Virginia	2024-06-18
North Carolina	2024-03-05	Colorado	2024-06-25
Texas	2024-03-05	New York	2024-06-25
Illinois	2024-03-19	Utah	2024-06-25
Ohio	2024-03-19	Arizona	2024-07-30
Vermont	2024-04-13	Tennessee	2024-08-01
Pennsylvania	2024-04-23	Kansas	2024-08-06
Indiana	2024-05-07	Michigan	2024-08-06
Maryland	2024-05-14	Mississippi	2024-08-06
Nebraska	2024-05-14	Missouri	2024-08-06
West Virginia	2024-05-14	Washington	2024-08-06
Georgia	2024-05-21	Hawaii	2024-08-10
Idaho	2024-05-21	Connecticut	2024-08-13
Kentucky	2024-05-21	Minnesota	2024-08-13
Oregon	2024-05-21	Wisconsin	2024-08-13
Iowa	2024-06-04	Alaska	2024-08-20
Montana	2024-06-04	Florida	2024-08-20
New Jersey	2024-06-04	Wyoming	2024-08-20
New Mexico	2024-06-04	Delaware	2024-09-03
South Dakota	2024-06-04	Massachusetts	2024-09-03
Maine	2024-06-11	New Hampshire	2024-09-10
Nevada	2024-06-11	Rhode Island	2024-09-10
North Dakota	2024-06-11	Louisiana	2024-11-05

Appendix C

Validation Test: Congressional Trading Scandal and Election Outcome

In this appendix, we provide evidence that validates the underlying assumption that politicians targeted by informed trading scandals face electoral consequences. Specifically, we examine the election outcomes for the 2022 midterms, the election cycle that followed the *New York Time* expose.¹ Focusing on the 447 Members of Congress whose terms ended in January of 2023, we compare exit and defeat rates for incumbents targeted vs not targeted by the *New York Times* article.

Findings presented in Table C Panel A indicate that targeted incumbents are significantly more likely to be unseated, 19.8% of the target incumbents exit Congress vs 10.3% of the non-targeted incumbents. Unseated incumbents exit Congress because they either lost their reelection bids or chose not to run for reelection.² Among those incumbents who remain in the race, 8.3% of *NYT*-targeted incumbents lose their reelection bid, compared with 4.9% of nontargeted incumbents. Moreover, targeted incumbents receive 61.4% of the votes compared to 64.7% for nontargeted incumbents. The difference in defeat rates for incumbents is economically large (a 70% increase) but not statistically significant, while the difference in the share of votes is statistically significant.

In Panel B of Table C, we control for politician characteristics by matching the 84 *NYT*-targeted incumbents who sought reelection in 2022 to similar incumbents seeking reelection at some point during the sample period. We match incumbents based on party affiliation, party status (majority or minority), gender, race, seniority and vote share (%) in their last election. Panel B yields result qualitatively similar to those in Panel A.

¹ <https://www.nytimes.com/interactive/2022/09/13/us/politics/congress-members-stock-trading-list.html>.

² We focus on unseated incumbents—those who leave Congress either because they lose reelection or because they choose not to run again. This focus is motivated by prior research showing that incumbents make strategic retirement decisions when their electoral prospects weaken (Gelman and King, 1990; Stone, Fulton, Maestas, and Maisel, 2010).

Table C
Validation Test: Congressional Trading Scandal and Election Outcome

Panel A: Comparison of incumbents in the 2022 midterm election

	Identified by the 2022 NYT article	NOT identified by the 2022 NYT article	Diff	<i>p</i> -value
Number of incumbents who lost re-election <i>Defeat rate (%)</i>	7 out of 84 8.33%	16 out of 330 4.85%	3.48%	0.213
Number of incumbents who either lost or did not seek re-election <i>Exit rate (%)</i>	19 out of 96 19.79%	36 out of 350 10.29%	9.50%***	0.012
Average share of votes (%) for incumbents who sought re-elections	61.43%	64.73%	3.30%**	0.019

Panel B: Comparison using one-on-one matched incumbents from 2013 to 2023

	Identified by the 2022 NYT article (N = 84)	Matched group (N=84)	Diff	<i>p</i> -value
Number of incumbents who lost re-election <i>Defeat rate (%)</i>	7 out of 84 8.33%	3 out of 84 3.57%	4.76%	0.192
Average share of votes (%) for incumbents who sought re-elections	61.43%	66.27%	4.84%**	0.021

Appendix D Examples of paper and electronic filings³

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

MAY 10 2018 Page 1 of 2

UNITED STATES HOUSE OF REPRESENTATIVES
 Periodic Transaction Report

NAME: JARED S. POLIS	OFFICE TELEPHONE: 202-225-2161	LEGISLATIVE RESOURCE CENTER: 18 MAY 10 PM 2:21 U.S. HOUSE OF REPRESENTATIVES (For Official Use Only)
<input checked="" type="checkbox"/> Member of the U.S. House of Representatives Date: Colorado District 2 <small>File an original and 2 copies.</small>	<input type="checkbox"/> Officer or Employee Employing Office: _____ <small>File an original and 1 copy.</small>	
A \$200 penalty shall be assessed against anyone who files more than 30 days late.		
Did you purchase any shares that were allocated as a part of an Initial Public Offering? <input type="checkbox"/> YES <input checked="" type="checkbox"/> NO		Please indicate whether this is an initial report or an amended report. For amendments, please provide the date of the report you are amending. <input checked="" type="checkbox"/> Initial Report <input type="checkbox"/> Amendment Date of Report being Amended: _____

JT SP DC	FULL ASSET NAME <small>Provide full name, not ticker symbol.</small>	TYPE OF TRANSACTION		DATE OF TRANSACTION (MMDDYY)	DATE NOTIFIED OF TRANSACTION (MMDDYY)	AMOUNT OF TRANSACTION												
		PURCHASE	SALE EXCHANGE			A \$1,000-\$10,000	B \$10,001-\$25,000	C \$25,001-\$50,000	D \$50,001-\$100,000	E \$100,001-\$250,000	F \$250,001-\$500,000	G \$500,001-\$1,000,000	H \$1,000,001-\$2,500,000	I \$2,500,001-\$5,000,000	J Over \$5,000,000			
JT	Example Mega Corp. Common Stock		X	01/01/12	01/01/12		X											
* JT	Jove Ventures Fund I, LP - Equity Eats, Inc. Convertible Note		X	4/14/18	4/14/18				X									
* JT	Jove Ventures Fund I, LP - Ink Kind Cards, Inc.		X	4/14/18	4/14/18				X									
	Jove Ventures Fund I, LP - Snow Shoe Foods, Inc.		X	4/19/18	4/19/18		X											
* JT	Jove Ventures Fund I, LP - Simple Energy, Inc.		X	4/9/18	4/30/18				X									
	Techstars NYC 2014, LLC - Spoon Media, Inc. - Private		X	4/5/18	5/1/18	X												

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

(This page will be publicly disclosed.)

MAY 10 2018 Page 2 of 2

UNITED STATES HOUSE OF REPRESENTATIVES
 Periodic Transaction Report

NAME: JARED S. POLIS	
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JT SP DC	FULL ASSET NAME <small>Provide full name, not ticker symbol.</small>	TYPE OF TRANSACTION		DATE OF TRANSACTION (MMDDYY)	DATE NOTIFIED OF TRANSACTION (MMDDYY)	AMOUNT OF TRANSACTION												
		PURCHASE	SALE EXCHANGE			A \$1,000-\$10,000	B \$10,001-\$25,000	C \$25,001-\$50,000	D \$50,001-\$100,000	E \$100,001-\$250,000	F \$250,001-\$500,000	G \$500,001-\$1,000,000	H \$1,000,001-\$2,500,000	I \$2,500,001-\$5,000,000	J Over \$5,000,000			
	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 - SandGrid		X	4/10/18	5/1/18							X						

FOOTNOTE NUMBER	FILER NOTES (optional)

(This page will be publicly disclosed.)

³ 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 D (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 Date Type	Notification Date	Amount
		Trinity Industries, Inc. (TRN)	S	12/29/2016	12/29/2016 \$15,001 - \$50,000
FILING STATUS: New					

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



[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
Illustrations of Sample Construction for Tests of Disclosures Delayed until after Elections

This figure illustrates the timing of trade execution and disclosure for the subsamples of transactions used to examine whether legislators are more likely to delay disclosing trades until after *the Tuesday after the 1st Monday in November* in election years vs non-election years.

Figure 1.A depicts trades executed within the [-45, 0] window prior to election day (or a pseudo-election day in non-election years) for which disclosure can be delayed until after elections without violating the disclosure requirements of the STOCK Act. Tests presented in Section 4.2.

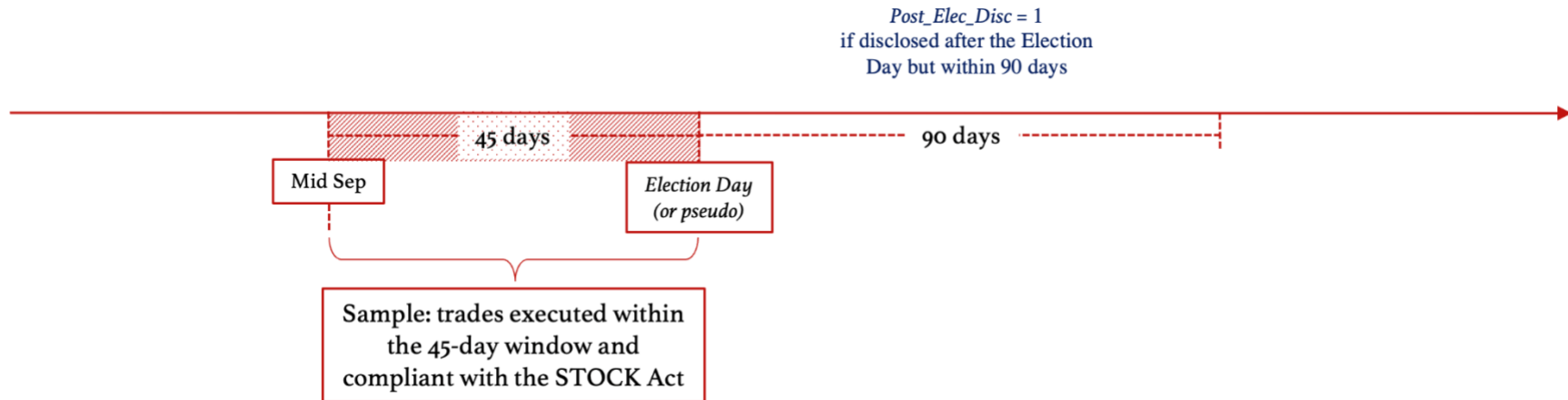


Figure 1 (continued)
Subsamples for tests of Delayed Disclosures around Election Day

Figure 1.B illustrates the setting used in Section 4.3 to examine whether politicians are willing to violate the law to strategically delay disclosure until after their re-election bid. Depicted are trades executed more than 45 days before election days (or pseudo-election day in non-election years) for which disclosure delayed until after the election is, by construction, a violation of the STOCK Act.

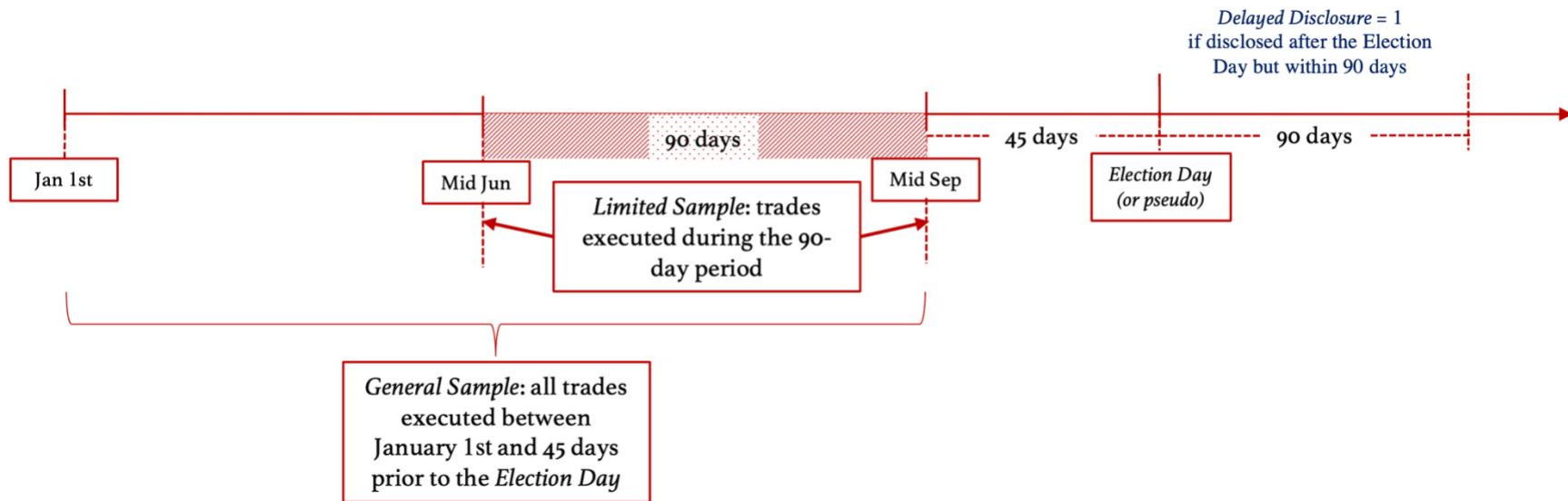


Table 1
Sample Construction

Description	# Trades dropped	# Trades remaining
All stock trades		128,596
Exclude amended disclosure when an initial disclosure is available	(3,022)	125,574
Aggregating same-day, same-stock, same-direction trades by the same politician	(23,493)	102,081
Exclude trades with missing stock returns information	(5,851)	96,230
Exclude trades with missing control variables	(1,473)	94,757
Exclude singleton observations	(1,200)	93,557
Baseline sample of stock trades disclosed		93,557
<hr/>		
Test-specific subsamples		
Trades made by politicians seeking re-election (Tables 2, 3 and 4)		88,115
Trades used for <i>Post_Elec_Disc</i> tests larger subsample (Tables 5 and 6)		10,524
Trades used for <i>Delayed Disclosure</i> tests <i>General Sample</i> (Tables 7 and 8)		62,173
Trades used for <i>Delayed Disclosure</i> tests <i>Limited Sample</i> (Tables 7 and 8)		19,292
<hr/>		
Additional sample details		
# Unique stocks traded and disclosed		3,002
# Unique politicians in Congress during the sample years		960
# Unique politicians that disclosed at least one stock trades		362

Table 2
Comparative Statistics

This table presents comparative statistics for political trades disclosed in a *Pressure Window* vs. a *Non-Pressure Window*. A transaction is identified as disclosed during a *Pressure Window* if it is disclosed by a politician seeking reelection between one month prior to the state-specific primary and the general election in an election year. All variables are defined in Appendix A. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively, using a two-tailed *t*-test.

Variables	<i>PressureWindow</i> (N=24,496)					<i>Non-PressureWindow</i> (N=63,619)					Diff in Mean
	Mean	SD	p10	p50	p90	Mean	SD	p10	p50	p90	
Dependent variables											
<i>BHAR_D30</i>	-0.0027	0.1009	-0.1017	-0.0005	0.0937	0.0000	0.0928	-0.0923	0.0003	0.0924	-0.0027***
<i>BHAR_D45</i>	-0.0035	0.1225	-0.1258	-0.0017	0.1169	0.0004	0.1180	-0.1175	0.0007	0.1182	-0.0039***
<i>BHAR_D60</i>	-0.0045	0.1416	-0.1432	-0.0025	0.1344	0.0006	0.1375	-0.1347	0.0011	0.1361	-0.0051***
<i>RAW_D45</i>	-0.0033	0.1535	-0.1602	0.0000	0.1502	0.0023	0.1302	-0.1306	0.0018	0.1360	-0.0056***
<i>Suspicious Trades</i>	0.6406	0.4798	0.0000	1.0000	1.0000	0.6626	0.4728	0.0000	1.0000	1.0000	-0.0220***
Control variables											
<i>ROA</i>	0.0631	0.0921	-0.0146	0.0555	0.1707	0.0668	0.0916	-0.0075	0.0592	0.1757	-0.0037***
<i>Book-to-Mkt</i>	0.4811	0.6141	0.0550	0.3203	0.9791	0.4896	0.6124	0.0568	0.3194	1.0298	-0.0085*
<i>Size</i>	10.5155	1.8581	7.9183	10.6601	12.6948	10.6828	1.7899	8.2935	10.7596	12.8159	-0.1673***
<i>Leverage</i>	0.2828	0.1885	0.0395	0.2669	0.5268	0.2838	0.1856	0.0433	0.2674	0.5216	-0.0010
<i>#Analyst</i>	19.7767	12.9774	2.0000	20.0000	36.0000	21.0813	13.1607	2.0000	21.0000	38.0000	-1.3046***
<i>HomeState</i>	0.1131	0.3168	0.0000	0.0000	1.0000	0.0869	0.2817	0.0000	0.0000	0.0000	0.0262***
Other descriptive stats											
<i>Discclosure Lapse</i>	49.3437	51.7863	12.0000	28.0000	65.0000	42.7310	37.7386	11.0000	27.0000	49.0000	6.6127***
<i>Violation</i>	0.1539	0.3609	0.0000	0.0000	1.0000	0.1243	0.3300	0.0000	0.0000	1.0000	0.0296***

Table 3
Strategic Disclosure of Informed Congressional Trades During Electoral Pressure Windows

This table examines whether politicians avoid disclosing informed trades while engaged in re-election bids. Panel A presents the baseline regression results for politicians seeking re-election. The dependent variables, *BHAR_D30*, *BHAR_D45*, and *BHAR_D60*, measure buy-and-hold abnormal returns over 30-, 45-, and 60-calendar-day periods following the execution date, respectively, benchmarked against the Carhart four-factor model. *RAW_D45* measures the raw buy-and-hold returns over the 45-day window. For sell transactions, returns are multiplied by -1. *Suspicious Trades* is an indicator for trades that are more likely to be informed by congressional knowledge, identified as large trades with dollar values exceeding \$15,000 or trades of stocks of firms operating in one of four industries whose fortunes depend critically on congressional decision-making (defense, energy, finance, or healthcare). The key independent variable, *PressureWindow*, is an indicator for whether a trade is disclosed by a politician seeking reelection between one month prior to the state-specific primary and the general election in an election year. Panel B provides results of a falsification test for retiring politicians. Panel C presents robustness tests. 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: Baseline Results

Dependent variable:	Pr. Sign	<i>BHAR_D45</i> (1)	<i>RAW_D45</i> (2)	<i>BHAR_D30</i> (3)	<i>BHAR_D60</i> (4)	<i>Suspicious Trades</i> (5)
<i>PressureWindow</i>	-	-0.004** (-2.53)	-0.007*** (-3.91)	-0.002* (-1.85)	-0.005** (-2.35)	-0.023** (-2.27)
<i>ROA</i>		0.009 (1.12)	0.003 (0.33)	0.006 (0.93)	0.013 (1.32)	-0.451*** (-6.13)
<i>Book-to-Mkt</i>		0.000 (0.10)	0.001 (1.27)	0.001 (1.18)	-0.001 (-0.72)	0.021** (2.17)
<i>Size</i>		0.001 (1.29)	-0.000 (-0.46)	0.000 (1.16)	0.001 (1.44)	0.016*** (2.99)
<i>Leverage</i>		0.002 (0.53)	0.003 (0.78)	0.003 (0.76)	-0.001 (-0.22)	-0.094*** (-4.99)
<i>#Analyst</i>		0.000 (0.16)	0.000 (0.88)	0.000 (0.56)	-0.000 (-0.10)	-0.004*** (-11.02)
<i>HomeState</i>		-0.004** (-2.18)	-0.009*** (-3.77)	-0.001 (-1.04)	-0.005*** (-2.74)	0.010 (0.39)
Politician FE		Yes	Yes	Yes	Yes	Yes
Congress FE		Yes	Yes	Yes	Yes	Yes

S.E. Cluster by Politician and Congress	Yes	Yes	Yes	Yes	Yes
No. of observations	88115	88115	88115	88115	88115
Adj. R-Squared	0.0044	0.0086	0.0049	0.0049	0.2246

Table 3 (continued)

Panel B: Falsification Tests: Disclosure choices of retiring politicians

Dependent variable:		<i>BHAR_D45</i>	<i>RAW_D45</i>	<i>BHAR_D30</i>	<i>BHAR_D60</i>	<i>Suspicious Trades</i>
	Pr. Sign	(1)	(2)	(3)	(4)	(5)
<i>PressureMonths</i>	0	0.001 (0.21)	-0.009 (-1.21)	0.003 (0.99)	0.002 (0.55)	0.009 (0.99)
<i>ROA</i>		0.000 (0.01)	0.022 (0.49)	-0.015 (-0.51)	0.002 (0.05)	-0.197*** (-3.26)
<i>Book-to-Mkt</i>		0.001 (0.59)	0.005 (1.43)	0.001 (0.44)	0.000 (0.11)	0.008 (1.57)
<i>Size</i>		0.001 (0.82)	-0.003* (-1.80)	0.001 (0.89)	0.003* (1.81)	0.009* (1.89)
<i>Leverage</i>		0.006 (0.64)	-0.012 (-0.74)	0.008 (0.76)	0.005 (0.59)	-0.032* (-1.85)
<i>#Analyst</i>		-0.000 (-0.61)	0.000 (0.04)	-0.000 (-0.51)	-0.000 (-0.77)	-0.001 (-1.38)
<i>HomeState</i>		-0.002 (-0.22)	-0.021 (-1.64)	0.007 (0.78)	-0.005 (-0.61)	0.019 (0.98)
Politician FE		Yes	Yes	Yes	Yes	Yes
Congress FE		Yes	Yes	Yes	Yes	Yes
S.E. Cluster by Politician and Congress		Yes	Yes	Yes	Yes	Yes
No. of observations		5442	5442	5442	5442	5490
Adj. R-Squared		0.0020	0.0066	0.0032	0.0062	0.0527

Table 3 (continued)

Panel C: 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. Columns 3 and 4 addresses seasonality concerns by limiting the sample to only trades disclosed during the same calendar time in election and nonelection years. To eliminate concerns that standard errors are understated, in Column 5 we drop trades with overlapping event windows.

Dependent variable:	Pr. Sign	"In Session" trades only		Same calendar dates in election & non-election years		Exclude transactions with overlapping return windows
		<i>BHAR_D45</i>	<i>Suspicious Trades</i>	<i>BHAR_D45</i>	<i>Suspicious Trades</i>	<i>BHAR_D45</i>
		(1)	(2)	(3)	(4)	(5)
<i>PressureWindow</i>	-	-0.003* (-1.77)	-0.016** (-2.33)	-0.005*** (-2.61)	-0.016** (-2.34)	-0.005* (-1.80)
<i>ROA</i>		0.007 (0.72)	-0.220*** (-5.58)	-0.002 (-0.17)	-0.270*** (-5.60)	0.013 (0.76)
<i>Book-to-Mkt</i>		-0.001 (-0.57)	0.014** (2.30)	0.001 (1.22)	0.018*** (2.65)	-0.001 (-0.28)
<i>Size</i>		0.000 (0.81)	0.007*** (2.68)	0.001 (1.39)	0.010*** (2.83)	0.000 (0.54)
<i>Leverage</i>		0.003 (0.52)	-0.039*** (-2.81)	0.004 (0.76)	-0.040** (-2.42)	-0.005 (-0.88)
<i>#Analyst</i>		0.000 (0.33)	-0.002*** (-7.71)	0.000 (0.96)	-0.002*** (-7.58)	0.000 (0.10)
<i>HomeState</i>		-0.003 (-1.52)	-0.006 (-0.43)	-0.001 (-0.68)	0.013 (0.71)	-0.006 (-1.52)
Politician FE		Yes	Yes	Yes	Yes	Yes
Congress FE		Yes	Yes	Yes	Yes	Yes
S.E. Cluster by Politician and Congress		Yes	Yes	Yes	Yes	Yes
No. of observations		57760	57760	53264	53264	21862
Adj. R-Squared		0.0049	0.0991	0.0052	0.1011	0.0076

Table 4

Cross-sectional Analyses: Strategic Disclosure of Congressional Trades and Electoral Pressure Windows

This table examines whether Table 3 results are more pronounced for electorally vulnerable politicians and for politicians hailing from economically distressed districts. Panel A compares trades executed by vulnerable vs. safe politicians, while Panel B compares 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. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively, using a two-tailed *t*-test.

Panel A: Vulnerable vs. safe politicians

Dependent variable:		<i>BHAR_D45</i>		<i>Suspicious Trades</i>	
Subsample:	Pr. Sign	Vulnerable	Safe	Vulnerable	Safe
		(1)	(2)	(3)	(4)
<i>PressureWindow</i>	- ; 0	-0.008*** (-3.15)	-0.001 (-0.92)	-0.029** (-2.14)	-0.012 (-1.26)
<i>ROA</i>		0.003 (0.16)	0.013* (1.78)	-0.389*** (-6.56)	-0.549*** (-3.80)
<i>Book-to-Mkt</i>		0.000 (0.24)	-0.000 (-0.27)	0.005 (0.56)	0.046*** (2.83)
<i>Size</i>		-0.000 (-0.10)	0.001* (1.66)	0.011 (1.50)	0.025*** (3.69)
<i>Leverage</i>		-0.004 (-0.41)	0.006* (1.79)	-0.107*** (-3.67)	-0.086*** (-3.17)
<i>#Analyst</i>		-0.000 (-0.41)	0.000 (0.79)	-0.004*** (-8.65)	-0.003*** (-8.51)
<i>HomeState</i>		-0.003 (-0.86)	-0.004** (-2.05)	-0.006 (-0.29)	0.052 (1.00)
<i>p</i> -value for diff in coef.		0.000		0.004	
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		32320	55782	32320	55782
Adj. R-Squared		0.0057	0.0046	0.2885	0.1851

Table 4 (continued)

Panel B: High- vs. low-unemployment districts

Dependent variable: Subsample:	Pr. Sign	<i>BHAR_D45</i>		<i>Suspicious Trades</i>	
		High Unemploy (1)	Low Unemploy (2)	High Unemploy (3)	Low Unemploy (4)
<i>PressureWindow</i>	- ; 0	-0.004** (-2.50)	-0.003 (-0.65)	-0.025** (-2.20)	-0.013 (-0.93)
<i>ROA</i>		0.015* (1.70)	-0.010 (-0.53)	-0.487*** (-5.43)	-0.322*** (-5.57)
<i>Book-to-Mkt</i>		0.000 (0.01)	0.001 (0.30)	0.019 (1.61)	0.028*** (3.15)
<i>Size</i>		0.001 (1.50)	-0.000 (-0.11)	0.019*** (2.74)	0.010** (2.03)
<i>Leverage</i>		0.002 (0.41)	0.004 (0.54)	-0.098*** (-4.60)	-0.082*** (-2.70)
<i>#Analyst</i>		-0.000 (-0.14)	0.000 (0.87)	-0.004*** (-9.92)	-0.003*** (-6.42)
<i>HomeState</i>		-0.004** (-2.40)	-0.000 (-0.03)	0.020 (0.66)	-0.049* (-1.91)
<i>p-value for diff in coef.</i>		0.120		0.050	
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		70337	17769	70337	17769
Adj. R-Squared		0.0042	0.0101	0.2324	0.2018

Table 5
Strategic Disclosure Delay of Stock Trades and Election Cycles

This table examines whether politicians strategically disclose their trades around election day while remaining compliant with the STOCK Act. The sample focuses on trades executed within the 45 days prior to election day (or a pseudo-election day in non-election years), a window in which a post-election disclosure can satisfy the STOCK Act's disclosure requirement. See Figure 1.A. The dependent variable, *Post_Elec_Disc*, is an indicator equal to one if a trade is disclosed after the election (or pseudo-election) day. The key independent variable, *ElectYear*, indicates whether the trade is executed in an election year. The regressions include relative-day fixed effects, so identification comes from comparing trades executed the same number of days before the election or pseudo-election day. Columns 1 and 2 report results from OLS and Logit models, respectively. 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:	Pr. Sign	<i>Post_Elec_Disc</i>	
		(1)	(2)
<i>ElectYear</i>	+	0.046*** (5.39)	1.199*** (7.88)
<i>ROA</i>		0.021 (0.59)	0.218 (0.38)
<i>Book-to-Mkt</i>		0.000 (0.03)	-0.034 (-0.40)
<i>Size</i>		-0.000 (-0.01)	-0.022 (-0.58)
<i>Leverage</i>		-0.042** (-2.31)	-0.480 (-1.62)
<i>#Analyst</i>		0.000 (1.15)	-0.002 (-0.46)
<i>HomeState</i>		-0.017* (-1.85)	-0.149 (-0.91)
Model		OLS	Logit
Relative Day FE		Yes	Yes
Politician FE		Yes	Yes
Congress FE		Yes	Yes
S.E. Cluster by Politician and Congress		Yes	Yes
No. of observations		10524	8230
Adj. R-Squared		0.6675	0.6797

Table 6

Cross-sectional Analyses: Strategic Disclosure Delay of Stock Trades and Election Cycles

This table examines whether the disclosure timing patterns in Table 5 are more pronounced for *Profitable* and *Suspicious Trades*, electorally vulnerable politicians and for politicians hailing from economically distressed districts. Columns 1 and 2 compare profitable and non-profitable trades. Columns 3 and 4 compare suspicious and non-suspicious trades. Columns 5 and 6 compare trades by electorally vulnerable versus safe politicians. Columns 7 and 8 compare politicians representing districts with high versus low unemployment rates. The dependent variable, *Post_Elec_Disc*, is an indicator equal to one if a trade is disclosed after the election (or pseudo-election) day. The independent variable, *ElectYear*, is an indicator of whether a trade is executed in an election year. *p*-value for the differences in coefficients between subsamples are reported. The regressions include relative-day fixed effects, so identification comes from comparing trades executed the same number of days before the election or pseudo-election day. 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:	<i>Post_Elec_Disc</i>							
Subsample:	Profitable	Non-Profitable	Suspicious Trades	Non Suspicious	Vulnerable	Safe	High Unemploy	Low Unemploy
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>ElectYear</i>	0.068*** (5.63)	0.027** (2.34)	0.056*** (5.34)	0.027* (1.95)	0.096*** (5.53)	0.037*** (3.64)	0.059*** (6.12)	-0.061** (-2.24)
<i>ROA</i>	0.031 (0.61)	0.014 (0.28)	0.021 (0.43)	0.002 (0.04)	0.077 (1.17)	-0.026 (-0.68)	0.000 (0.01)	0.144* (1.80)
<i>Book-to-Mkt</i>	-0.000 (-0.02)	-0.001 (-0.18)	-0.000 (-0.01)	0.003 (0.36)	0.012 (1.26)	-0.001 (-0.09)	-0.005 (-0.81)	0.017 (1.13)
<i>Size</i>	-0.000 (-0.09)	-0.002 (-0.46)	-0.000 (-0.01)	-0.001 (-0.24)	0.007 (1.59)	-0.002 (-0.95)	-0.001 (-0.43)	-0.002 (-0.45)
<i>Leverage</i>	-0.037 (-1.47)	-0.043* (-1.72)	-0.054** (-2.31)	-0.034 (-1.21)	-0.036 (-0.96)	-0.042** (-2.22)	-0.045** (-2.45)	-0.071* (-1.73)
<i>#Analyst</i>	0.000 (0.91)	0.000 (1.07)	0.001 (1.47)	-0.000 (-0.72)	0.000 (0.22)	0.001* (1.65)	0.000 (1.49)	-0.001 (-1.00)
<i>HomeState</i>	-0.018 (-1.36)	-0.012 (-0.97)	-0.022* (-1.82)	0.005 (0.41)	-0.007 (-0.33)	-0.012 (-1.31)	-0.022** (-2.38)	-0.002 (-0.07)
<i>p</i> -value for diff in coef.	0.000		0.034		0.000		0.000	

Relative Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Politician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Congress FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
S.E. Cluster by Politician and Congress	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	5202	5289	6480	4016	2886	7634	8870	1651	
Adj. R-Squared	0.6562	0.6928	0.6603	0.6947	0.6906	0.7027	0.6796	0.7521	

Table 7
Violation of STOCK Act Reporting Deadline and Election Cycles

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 2, *Limited Sample*, includes trades executed during [-135, -45] days relative to the election (or pseudo-election) day; the subsample used in Columns 3 and 4, *General Sample*, includes all trades executed more than 45 days before the election (or pseudo-election). See Figure 1.B. 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. The regressions include relative-day fixed effects, so identification comes from comparing trades executed the same number of days before the election or pseudo-election day. 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:		<i>Delayed Disclosure</i>			
Sample:		Limited Sample		General Sample	
	Pr. Sign	(1)	(2)	(3)	(4)
<i>ElectYear</i>	+	0.008*	1.106**	0.006**	1.009***
		(1.83)	(2.31)	(2.38)	(3.01)
<i>ROA</i>		0.003	0.263	-0.001	0.040
		(0.56)	(0.46)	(-0.22)	(0.08)
<i>Book-to-Mkt</i>		-0.003**	-0.136	-0.001	-0.024
		(-2.06)	(-1.00)	(-1.46)	(-0.48)
<i>Size</i>		0.000	0.010	0.000	0.008
		(0.05)	(0.31)	(0.61)	(0.29)
<i>Leverage</i>		-0.007**	-0.639*	-0.003*	-0.388
		(-2.34)	(-1.79)	(-1.79)	(-1.50)
<i>#Analyst</i>		0.000	-0.003	0.000*	0.001
		(1.11)	(-0.77)	(1.68)	(0.28)
<i>HomeState</i>		-0.002	0.048	-0.001	-0.145
		(-1.39)	(0.23)	(-1.63)	(-0.74)
Model		OLS	Logit	OLS	Logit
Relative Day FE		Yes	Yes	Yes	Yes
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		19292	1114	62173	10755
Adj. / Pseudo R-Squared		0.4611	0.3509	0.4688	0.5772

Table 8
Cross-sectional Analyses: Violation of STOCK Act Reporting Deadline and Election Cycles

This table examines whether Table 7 results are more pronounced for *Profitable* and *Suspicious Trades*, electorally vulnerable politicians and for politicians hailing from economically distressed districts. Panel A uses the *Limited Sample* of trades executed [-135, -45] days prior to an election or pseudo election day, while Panel B uses the *General Sample* that includes all trades executed at least 45 days prior to the election day or pseudo election day. Columns 1 and 2 focus on trading profitability; Columns 3 and 4 compares suspicious and non-suspicious trades; Columns 5 and 6 compare electoral vulnerability; Columns 7 and 8 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) and thereby violates the disclosure requirements of the STOCK Act. The independent variable, *ElectYear*, is an indicator of whether a trade is executed in an election year. The regressions include relative-day fixed effects, so identification comes from comparing trades executed the same number of days before the election or pseudo-election day. 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 Limited Sample

Subsample:	<i>Delayed_Disclosure</i>							
	Profitable (1)	Non-Profitable (2)	Suspicious Trades (3)	Non Suspicious (4)	Vulnerable (5)	Safe (6)	High Unemploy (7)	Low Unemploy (8)
<i>ElectYear</i>	0.010** (2.03)	0.006 (1.44)	0.009** (2.16)	0.005 (0.93)	0.015** (2.09)	0.001 (0.21)	0.010** (2.04)	-0.002 (-0.18)
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>p</i> -value for diff in coef.		0.099		0.154		0.000		0.001
Relative Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Politician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Congress FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
S.E. Cluster by Politician and Congress	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	9733	9520	12485	6783	8230	11054	15074	4209
Adj. R-Squared	0.4433	0.4894	0.4526	0.5454	0.4664	0.5178	0.5128	0.4430

Table 8 (continued)
Cross-sectional Analyses: Violation of STOCK Act Reporting Deadline and Election Cycles

Panel B: OLS using General Sample

Dependent variable:	<i>Delayed Disclosure</i>							
Subsample:	Profitable	Non-Profitable	Suspicious Trades	Not Suspicious	Vulnerable	Safe	High Unemploy	Low Unemploy
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>ElectYear</i>	0.007*	0.003	0.006*	0.003	0.008*	-0.001	0.008**	-0.016
	(1.89)	(0.95)	(1.90)	(0.57)	(1.81)	(-0.08)	(2.34)	(-0.71)
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value for diff in coef.		0.034		0.018		0.000		0.000
Relative Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Politician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Congress FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
S.E. Cluster by Politician and Congress	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	31423	30718	40134	22000	22529	39625	49747	12412
Adj. R-Squared	0.4947	0.4306	0.4001	0.5666	0.2516	0.5733	0.3820	0.6229

Table 9
Electronic vs Paper Filings

This table compares politicians' trades disclosed via electronic filings versus paper forms. Panel A reports statistics on the number of trades reported in the 2,693 paper filings and in the 4,344 electronic filings as well as statistics on the dollar value per trade reported for the two types of filings. Panel B reports the annual number of paper vs electronic filings at the filing level and at the politician level. 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	Diff in Mean
No. of trades per filing	Paper	2,693	1	2	4	14	54	22.72	14.77***
	Electronic	4,344	1	1	3	7	18	8.36	
Dollar value per trade (\$)	Paper	61,015	8000	8000	8000	32000	65000	28314.31	2577.74***
	Electronic	35,215	8000	8000	8000	32500	40500	25736.57	

Table 9
Electronic vs Paper Filings

Panel B: Filing methods over time – paper vs. electronic

Filing level					
Year	Paper		Electronic		Total
	No. of filings	%	No. of filings	%	
2013	664	100.00	0	0.00	664
2014	403	55.43	324	44.57	727
2015	288	40.85	417	59.15	705
2016	269	36.95	459	63.05	728
2017	269	35.30	493	64.70	762
2018	282	35.07	522	64.93	804
2019	160	25.36	471	74.64	631
2020	125	19.23	525	80.77	650
2021	89	15.89	471	84.11	560
2022	92	19.91	370	80.09	462
2023	52	15.12	292	84.88	344
Total	2693	38.27	4344	61.73	7037
Politician level					
Year	Paper		Electronic		Total
	No. of filings	%	No. of filings	%	
2013	121	100.00	0	0.00	121
2014	90	56.60	69	43.40	159
2015	60	43.17	79	56.83	139
2016	51	37.50	85	62.50	136
2017	40	30.53	91	69.47	131
2018	42	34.15	81	65.85	123
2019	31	24.60	95	75.40	126
2020	27	21.09	101	78.91	128
2021	20	15.50	109	84.50	129
2022	23	21.10	86	78.90	109
2023	14	15.73	75	84.27	89
Total	519	37.34	871	62.66	1390

Table 9 (continued)
Electronic vs Paper Filings

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

Sample Window	Start Year	Paper	Electronic	Diff in Mean (Paper - Electronic)		
		No. of transactions		<i>BHAR_D30</i>	<i>BHAR_D45</i>	<i>BHAR_D60</i>
All years	2013	60887	34680	0.0000 (0.050)	0.0005 (0.737)	0.0007 (0.934)
Drop 2013	2014	54417	34680	0.0003 (0.513)	0.0009 (1.308)	0.0012 (1.462)
Drop 2013-2014	2015	48371	32764	0.0007 (1.197)	0.0014* (1.848)	0.0017** (2.033)
Drop 2013-2015	2016	42995	29670	0.0013** (2.070)	0.0019** (2.412)	0.0021** (2.270)
Drop 2013-2016	2017	39039	26665	0.0019*** (2.828)	0.0025*** (2.924)	0.0026*** (2.687)
Drop 2013-2017	2018	33622	23478	0.0021*** (2.777)	0.0027*** (2.932)	0.0029*** (2.714)
Drop 2013-2018	2019	26369	19947	0.0018** (2.134)	0.0024** (2.227)	0.0026** (2.108)

Table 10
Suspicious Politicians

This table compares the trading profits of *Suspicious Politicians* to those of their peers, where *Suspicious Politicians* are identified based on their strategic disclosure behaviors (timing and form). In Panel A Column 1, a politician is defined as *Suspicious* if their propensity to violate the STOCK Act by disclosing trades more than 45 days after execution exceeds the sample mean level of 13%. In Panel A Column 2, a politician is classified as *Suspicious* if they continue to use paper filings in 2016 or later. By 2016, 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 2016 or later, and thereby reduces the sample size. Panel A Column 3 presents results for politicians who both have a propensity to delay disclosing beyond the statutory 45-day window and continue to use paper forms in 2016 or later. Panel B reports mean abnormal returns for in-session trades by *Suspicious Politicians* over alternative return windows. The *t*-statistics are reported 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: Abnormal returns for *Suspicious Politicians*

Dependent variable:		<i>BHAR_D45</i>		
	Pr. Sign	(1)	(2)	(3)
<i>Suspicious_Timing</i>	+	0.003** (2.37)		
<i>Suspicious_Form</i>	+		0.003** (2.29)	
<i>Suspicious_Superset</i>	+			0.003** (1.97)
<i>ROA</i>		0.018* (1.85)	0.020 (1.65)	0.015 (1.64)
<i>Book-to-Mkt</i>		-0.001 (-1.35)	-0.001 (-0.89)	-0.000 (-0.42)
<i>Size</i>		-0.001 (-0.93)	-0.000 (-0.33)	-0.000 (-0.04)
<i>Leverage</i>		0.002 (0.48)	0.004 (0.71)	0.005 (0.98)
<i>#Analyst</i>		0.000 (1.06)	0.000* (1.67)	0.000* (1.66)
<i>HomeState</i>		-0.003 (-1.20)	-0.004** (-2.00)	-0.003* (-1.89)
Industry FE		Yes	Yes	Yes
Congress FE		Yes	Yes	Yes
S.E. Cluster by Firm and Congress		Yes	Yes	Yes
No. of observations		93557	71202	71202
Adj. R-Squared		0.0063	0.0021	0.0022

Table 10 (continued)
Suspicious Politicians

Panel B: Abnormal returns for “In-Session” trades by *Suspicious Politicians*

Suspicious Politicians:		<i>Suspicious_Timing</i> (N = 21840)	<i>Suspicious_Form</i> (N = 30643)	<i>Suspicious_Superset</i> (N = 8743)
<i>BHAR_D45</i>	Mean	0.0014**	0.0011*	0.0022*
	<i>t</i> -stat	(2.02)	(1.92)	(1.94)
<i>BHAR_D30</i>	Mean	0.0009	0.0007	0.0021**
	<i>t</i> -stat	(1.61)	(1.45)	(2.32)
<i>BHAR_D60</i>	Mean	0.0018**	0.0013**	0.0023*
	<i>t</i> -stat	(2.16)	(1.97)	(1.74)