

# Trading on Access?

## Examining the Financial Activity of US Elected Officials

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### Abstract

Concerns about elected officials using their status and position for personal benefit can undermine public trust and confidence and cause citizens' to question the motivation behind public policies and actions. Following the 2008 financial crises, such concerns helped motivate the passage of the 2012 STOCKS Act that requires members to disclose their holdings and activities. Fresh concerns emerged during and after the pandemic about members using their access and position to make financial trades following briefings and hearings. Apart from selected incidents, the extent to which members engage in trading activity consistent with them using their status and position for personal benefit is uncertain. Using the universe of financial disclosures between 2012 and 2023 at the member-level, member-year level, and member-firm level we examine the extent to which members' disclosed financial activity correlates with their access to political power and information. Examining the relationship across many different measures and leveraging both between-member and within-member comparisons, we find little evidence consistent with members leveraging their status and position for personal benefit. While some members may indeed trade on their status, our characterization suggests that such behavior is the rare exception rather than the rule.

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# 1 Who Invests?

Concern over the influence of money in politics is one of the few remaining truly bipartisan refrains in an increasingly polarized America. Within this umbrella exists three aspects that dominate the public perception of monied influence in politics: lobbying, campaign contributions, and the ways in which political elites leverage their power and influence for personal gain. Much work has explored the connection between lobbying and campaign finance on politics using empirical, formal, and qualitative approaches, but the ability to investigate the ability of political elites to use their position for personal benefit has proven more elusive. To be clear, important work documents an association between a politician’s portfolio and the firms that contribute to their campaigns, as well as those with headquarters in their home state (Eggers and Hainmueller, 2014). In addition, a rich vein of research outside of political science has struggled to find systematic evidence of the benefits to insider trading, although see work by Huang and Xuan (2020) whose findings suggest that the passage of the STOCK Act greatly curtailed the ability of politicians to make money off their investments.

Concerns about whether officials use their position for personal gain matter for citizens’ confidence in the legitimacy of democratic governance and the policies it produces and surveys reveal that the this feature as thought to be an important feature of democracy (see, for example, Brightline Watch, <https://brightlinewatch.org/>). Fears that policy is being made for the good of the politician rather than the good of the people can lead to decreased satisfaction with the political system – threatening its’ legitimacy and perhaps even its’ existence. (See, for example, Louis the XVI :). Perhaps in response to such normative concerns – perhaps made more salient by the 2008 Global Financial Crisis – Congress enacted the “Stop Trading On Congressional Knowledge” (STOCK Act) to bolster the existing regulations on politician investing behavior and financial disclosures. The Covid-19 pandemic and attendant media coverage of possible insider trading by members of Congress prompted

a further groundswell of bipartisan proposals to enact further restrictions – including H.R. 1579, H.R. 336, S. 3494 and S. 3631 – but successful policy changes have yet to be realized from those initial efforts.<sup>1</sup>

Our question is simple: is there evidence from the available mandated financial reporting that members of Congress are acting in ways that would suggest they are leveraging their status and position to benefit themselves in the financial markets? While a rich literature has examined the performance of their financial portfolios and found them wanting – suggesting that they are not particularly adept at picking stocks – we focus instead on the timing and nature of their reported trades to assess whether they are related to information or access that they may have as a result of their position in Congress. Put differently, we do not assume that financial under-performance suggests that members are not leveraging their position because it is possible that members may take moves that fail to be financially rewarding. A member trading a stock whose CEO is going to testify before their committee seems suspicious regardless of whether the member’s profit on that transaction is financially superior to the overall market. By focusing on the timing and portfolio of investments rather than the overall value of the portfolio we seek to characterize the extent to which financial transactions appear to be motivated by the members’ position and status.

To be clear, firm inferences are hard, if not impossible, given the nature of the disclosures and the inability to fully know the connections and interactions of elected officials. Both raise non-trivial concerns. Members may leverage inside information for trades made by closely related individuals who are not subject to the STOCKS act. Just as donors can evade the financial limits of campaign finance laws by contributing through family and friends, members who act through family and friends are beyond the scope of our investigation. Relatedly, our ability to characterize the access of members is limited to what we can publicly observe – we know which members are on which committees, and which members

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<sup>1</sup>[Lachlan Markay \(2020\)](#); [Burns \(2020\)](#)

receive contributions from which firms and organized interests, but we have no ability to map the myriad of ways that elected officials may relate to publicly traded firms.

Despite these inferential limitations, our characterization of the underlying empirical regularities helps illuminate the extent to which elected members of Congress are engaging in activity that may lead some to question whether they are using their position and status for personal gain. To do so we leverage the financial disclosures required by the STOCKS Act and we connect such activity to member-level data (e.g., party, committee membership), district characteristics (e.g., electoral vulnerability, district demographics), and firm-level characteristics (e.g., whether firm employs a politician’s constituents). While we analyze the data in various aggregations to elucidate the associations of interest, at the core of our analysis is the analysis of over 56 million politician-firm dyads over the period of 2012-2023.

By examining the nature and timing of disclosed member activity using a variety of analyses conducted at the member-level, member-year-level, and member-publicly traded firm level and employing a rich set of fixed effects to help isolate the nature of the observed variation, we obtain several important conclusions and characterizations about who invests, where, and when. First, despite concerns and popular impressions of the extent to which members use their status and position to improve their financial well-being while in office, less than half of members of Congress invest – that only a minority of members are even invested in the financial markets suggests that to the extent that members trade on their status and position, that this does not describe the modal member of Congress. Indeed, the demographics of financially active members of Congress largely mirror the patterns found in the public writ large; the modal investor is a white male with a background in medicine. Second, and more importantly, using both between and within member comparisons, we find very evidence that generic trading activity is associated with positions that have traditionally been the source of power and policy influence in Congress. With the exception of a few select committees – Banking, Finance, and Urban Affairs and Science, Space, and Technology in

the House and the Judiciary and Taxation (Joint) committee in the Senate – there is little to suggest that changes plausibly related to changes in one’s access to political power and potentially valuable information – such as becoming a member or the chair of a powerful congressional committee – corresponds to changes in members’ investing behavior. Indeed, the more frequent between-member associations we find that the association between status and trading is more suggestive of a selection-effect; members who already are likely to make financial trades are more likely to join certain committees. To probe the relationship in more detail beyond summary measures of investing activity, we also examine the characteristics of the specific assets being traded to determine if the overall patterns mask security-specific patterns that are consistent with members leverage their status and position when making trades. In so doing, we corroborate existing work that finds correlations between investing and lobbying – members are more likely to trade shares of companies that lobby them or their bills – but we fail to reproduce evidence linking investment decisions with the geographic location of firms. (Work examining correlations between investing behavior and campaign contributions is underway.) Finally, our examination of whether investment decisions were influenced by particularly impactful events that members may have had privileged access to – including briefings on the severity of Covid-19 – are inconclusive, but ongoing.

Overall, our investigation is unable to identify systemic correlations between members’ financial investment decisions and observable changes in their status and position within Congress. Alongside extant findings that the portfolio performance of members’ financial portfolios is often underwhelming this suggests, but certainly cannot prove, that members use their status and position to enrich themselves via the financial markets. The lack of a robust association between aspects related to political status and power and financial activity suggests that the extreme examples highlighted in journalistic coverage may be the exception rather than the rule - perhaps because the financial costs of the electoral risk associated with being caught for wrong-doing are thought greater than the potential benefits of any such transactions. Put differently, the largely null relationships we find are consistent with a

story of behavior in equilibrium, wherein politicians largely avoid taking advantage of their privileged positions, presumably for fear of losing office if discovered. Such a conclusion is arguably good news, assuming it reflects a lack of influence of money in politics, at least through the channel of investor positions among elected officials.

## 2 Politicians and the Stock Market

Most existing research examines whether there is evidence of politicians trading based on privileged information obtained from serving on relevant committees. Initial studies corroborated media reports on insider trading, finding that members of Congress investing decisions exhibited suspicious timing and outperformed market indices ([Ziobrowski et al., 2004, 2011](#)). More recent analyses suggest that (1) there is limited evidence of insider trading and, even if there is, that (2) politicians are not very good at capitalizing on their privileged information ([C. Eggers and Hainmueller, 2013](#)). That said, [Eggers and Hainmueller \(2014\)](#) find that politicians invest more in firms located in the districts they represent, and more in firms whose PACs contributed to their campaigns.

In the background of these studies, and in the public's perception writ large, are the twin assumptions that politicians both want to use privileged information to make money, and that they can. While the former assumption may seem unassailable, the latter is more complicated. First, it must be the case that the work of legislating exposes politicians to information about firms, markets, and industries that (1) the market doesn't already have and that (2) they can use to successfully anticipate stock price fluctuations. Although things like congressional hearings satisfy the former requirement (hearings are not published until typically two months after they are held and the member is arguably able to affect the coverage and outcome through their questions and actions), it is difficult to determine the value of the information provided. For example, both the Senate Finance Committee and

the House Financial Service Committee meet twice per year with the Chair of the Federal Reserve who is privy to whether the Fed will adjust the federal funds rate on which all other lending, inflation, employment, and exchange rates are based. Although this is critically important and useful information for predicting what the market is likely to do, even a cursory review of the transcripts from these hearings reveals a practiced evasion on the part of Fed Chairs who seek to avoid having to provide any information at all for fear of upsetting the markets.

Of course, the Fed's independence mandates that chairs must not influence the market, meaning perhaps the theoretical value of these hearings is undermined by the professionalism of the chairs who attend them. Perhaps other hearings in which specific firms testify about the impact of regulations or policies on their profitability provide equally valuable information, albeit without the tight-lippedness of the Fed. Certainly this is the logic behind a New York Time expose in 2022 that summarized all the potential conflicts of interest investing politicians confronted when serving on congressional committees ([Parlapiano et al., 2022](#)). But even here, it is not obvious that politicians can fully act on the information. Concerns about public scrutiny and the fear that bad publicity may lead to an electoral threat at the hands of a canny challenger able to leverage those financial actions against the incumbent may limit the desire of elected officials to act on information they are able to obtain. Assuming that politicians have two goals – making money and retaining office – it is straightforward to understand how the latter might dominate the former, even if the politician might technically be able to gain an edge on the market.

Nevertheless, the preceding intuition lays out some clear expectations about politician investment behavior. First, all else equal, access to more valuable (private) information should be associated with an increase in investing behavior. Second, all else equal, insulation from challenger threats – and the financial risk associated with losing – should be associated with an increase in investing behavior. We summarize these conceptual dimensions of interest

in Table 1, highlighting that those least likely to invest are the electorally vulnerable who do not have access to valuable information, while those most likely to invest are those with access to valuable information who are either electorally secure or unconcerned about reelection for other reasons (i.e., those planning to retire).<sup>2</sup> As the table makes clear, we should not expect to see evidence of politicians exploiting their position for financial gain in three out of the four conditions.

	ACCESS	NO ACCESS
SECURE	Invest	Can't invest
INSECURE	Won't invest	Can't / won't invest

Table 1: Conceptual map of dimensions along which politician investments should vary. Columns indicate their ability to invest on the basis of access to valuable information (or lack thereof). Rows indicate the risks associated with insider trading if discovered by a challenger, where electorally secure politicians have less to fear if their trades are highlighted by potential challengers.

This broad intuition motivates the empirical investigation that follows where we leverage variation in the electoral vulnerability of members and their status in the House to evaluate the nature and type of reported financial transactions. As previously noted, the resulting characterizations are, at best, only suggestive and empirical patterns that are consistent with these expectations cannot necessarily confirm the claim that members are leveraging their position in an effort to improve their financial well-being. A null hypothesis may paradoxically be somewhat easier to interpret as a lack of difference suggests either: 1) members are not making financial trades as a result of their status and position, 2) nearly every member is engaged in such activity (i.e., financial transitions are driven by membership in the House as a whole rather than variation in the within-House statuses we examine), or 3) members are making their moves in portfolios that are not disclosed under the STOCKS act. While the latter two are certainly possible, sometimes when there is no barking, it is

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<sup>2</sup>It is certainly possible that electorally vulnerable members may be more likely to invest if they think the value of the trades they can make while serving exceeds the expected future gains of continued membership, but this seems hard to imagine.



because there is no dog.

### 3 Data

To test the expectations described above, we merge several sources of data and provide a rich characterization of who invests, what they invest in, where they invest, and when they invest. The investment data we analyze was obtained from Capitol Trades<sup>3</sup>, a subsidiary of 2iQ<sup>4</sup> which gathers and processes the individual stock trading disclosure forms that politicians are required to file every quarter. These disclosures must be filed for any purchase, sale, or exchange of a stock, bond, security, or other investment vehicle made by either the politician or their spouse. These data are recorded at the transaction level, and contain identifiers for both the politician (via their Bioguide ID) and for the firm in which they invest (via the GVKEY identifier used by Compustat). We link the politicians to several other datasets using their Bioguide ID (with the help of several other crosswalks). These include data on politician sociodemographic information compiled by *Congressional Quarterly*; data on politician committee assignments and legislative behavior, also from *Congressional Quarterly*; data on politician campaign contribution receipts from OpenSecrets and the FEC; and data on politician lobbying experiences from *LobbyView*. Similarly, we link the firms to several other datasets using a combination of their GVKEY as well as (laboriously) constructed crosswalks to campaign contribution data from OpenSecrets (<https://www.opensecrets.org/>); lobbying data from LobbyView (Kim, 2018); and subsidiary details from *ReferenceUSA* (<https://referenceusagov.com/>).

The resulting dataset provides a rich description of the network of politicians and firms across four characteristics: campaign contributions, lobbying, geographic ties, and

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<sup>3</sup><https://www.capitoltrades.com/>

<sup>4</sup><https://www.2iqresearch.com/>

investing behavior. The unit of analysis is a politician-firm dyad, where the latter are subset to publicly traded firms. To avoid selecting on potential dependent variables, we include all dyads based on the union of all four behaviors, meaning we observe a politician-firm dyad even if the politician doesn't invest in that firm (or even at all). Statistical models using these data use dyad cluster-robust standard errors calculated a la [Aronow, Samii and Assenova \(2015\)](#).<sup>5</sup> The politician-firm data allows us to explore which firms members trade on – e.g., firms that are located in the member's district, firms regulated by legislation under the purview of the member's committee, etc.

In addition to the politician-firm dyad-level data, we also aggregate the data in two additional ways to more fully describe the scope and nature of investing by elected officials. First, we aggregate the data to the politician-date and describe who does and doesn't invest, every day, writ large. This data allows us to examine temporal variation in member behavior – perhaps as a result of deciding to retire, participating in an oversight hearing, or ascending to a committee chair. Statistical models run on these data cluster standard errors on the politician for inference. Second, we aggregate the data to the politician, aggregating over all years between 2012 and 2023 to describe who invests in general. Our analysis starts with the most aggregate version of the data, describing who does and does not invest, before digging into more detailed analysis using the politician-day and dyadic versions.

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<sup>5</sup>Dyad-cluster robust standard errors are not calculated in this version of our working paper. Instead we rely on two-way cluster robust standard errors calculated on the firm and the politician. Insofar as these are anti-conservative, we don't expect our largely null results to be overturned when we implement the dyad-cluster robust standard errors. The dyadic data are huge though, and it takes a few hours to replicate the current results, even without the fastest version of the dyad-cluster robust algorithm known to humankind, developed by Bisbee.

## 4 Methods

Given our emphasis on the first order task of characterizing the empirical regularities of politicians' investment activity and the extent to which that activity is suggestive of members using their status and position for self-gain, our analysis is primarily descriptive. Even so, we estimate the correlates of financial activity using OLS to help characterize the relationships of interest, with standard errors clustered as described above. In so doing, we test the robustness of our results using various combinations of fixed effects that help highlight the sources of variation that identify the different relationships of interest. In particular, we use politician fixed effects to capture variation either over time or across firms, we employ year and firm fixed effects to account for cross-sectional variation when comparing politicians to each other, and we leverage two-way fixed effects to identify within-politician variation conditional on both over-time and across-firm trends. We also include dyad fixed effects to isolate purely over-time variation in our variables of interest, as well as politician-year and firm-year fixed effects to more precisely isolate alternative comparisons.

Importantly, none of the methods we employ can provide a convincingly causal interpretation because nothing in the data can be considered plausibly exogenous. Although one might be tempted to interpret within-politician variation as causal (for example, if we were to show that assignment to committee chair of the House Ways and Means Committee corresponds to an increase in investing behavior), it is also possible that assignment to the position resulted from the legislator's familiarity with the relevant industry, which also explains fluctuations in their investing behavior.

That said, we explore two sources of variation that may provide more convincing evidence of causal relationships. First, we use data on politician scandals to examine if member's change their behavior post scandal. As an unforeseen/unexpected shocks to the politician's concerns about reelection, this may change their willingness to leverage their

position for self-gain. On the one hand, perhaps they feel more threatened and are less likely to actively trade on the information and access they have and focus on reelection. On the other hand, perhaps they become a lame duck who no longer is constrained by how active trading may be perceived by voters.<sup>6</sup> Second, we use the rich time-series nature of the aggregate data to look for anomolous spikes in investment behavior following especially salient events. Using outlier analysis for time series data to detect such events and an interrupted time series models to provide an estimate of a plausibly causal association related to that event allows us some ability to identify the impact of the detected event. Even so, it is important to be clear on what the analysis is doing – we are interpreting sizable shifts post-hoc rather than estimating the effects of a pre-determined list of seemingly important events identified a priori. Neither of these approaches provide an entirely convincing demonstration of causality, although we underscore the difficulty in obtaining robust internal validity in this setting due to the seeming impossibility of random assignment along the dimensions of interest (although we are always open to creative experiment-in-the-field ideas!).

## 5 Which Elected Officials Invest?

To begin, we start with politician-level data to summarize behavior overall between 2012 and 2023. While these descriptions cannot untangle the causes or consequences of such behavior, they are useful for describing the changing environment and helping benchmark the potential scope of the concern – if few members are actively making and reporting financial transactions while in office then concerns are limited.<sup>7</sup> Conversely, if many members are frequently engaged in buying and selling securities of non-trivial amounts than this suggests the importance of examining the causes and consequences of that activity in more detail.

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<sup>6</sup>Not included in this draft.

<sup>7</sup>Or should focus on whether members are able to avoid and evade the reporting requirements.

- Between 2012 and 2023, 43.7% of members of the House filed STOCK reports
  - 38.4% of women in the House invest, compared to 45.3% of men
  - Only 16.1% of Black politicians invest, compared to 48.4% of whites
  - 46.2% of Republicans compared to 40.7% of Democrats
- Between 2012 and 2023, 56.9% of the Senate filed STOCK reports
  - Men: 58.1%, Women: 52.8%
  - 65% of Republicans compared to 49.4% of Democrats
  - 63.9% of BAs, 51.6% of MAs, and 56% of Professional degree holders.

To help benchmark the incidence of members' involvement in the financial markets relative to similarly situated citizens, we can compare how the characteristics of those members who report having made reportable financial transactions during this time period compare to the characteristics of the general public based on self-reported data collected by Gallup in 2023.

Figure 1 presents a comparison of the general public, members of the House, and members of the Senate. While the comparison is complicated by the fact that members' activity is based on the required financial disclosures and the measures for the public are based on self-reports given to a public opinion survey, the analysis helps establish the extent to which members are atypical in their financial dealings. Of primary interest for those concerned with members using their status and position for personal gain is if elected officials are more involved than otherwise similar members of the public.

As the top panel reporting the overall percentage of each group with financial investments makes clear, the percentage of elected officials who indicate that they have financial holdings according to the legally required disclosures is less than the 61% of the US population who self-reported having financial holdings when asked by Gallup in 2023. While

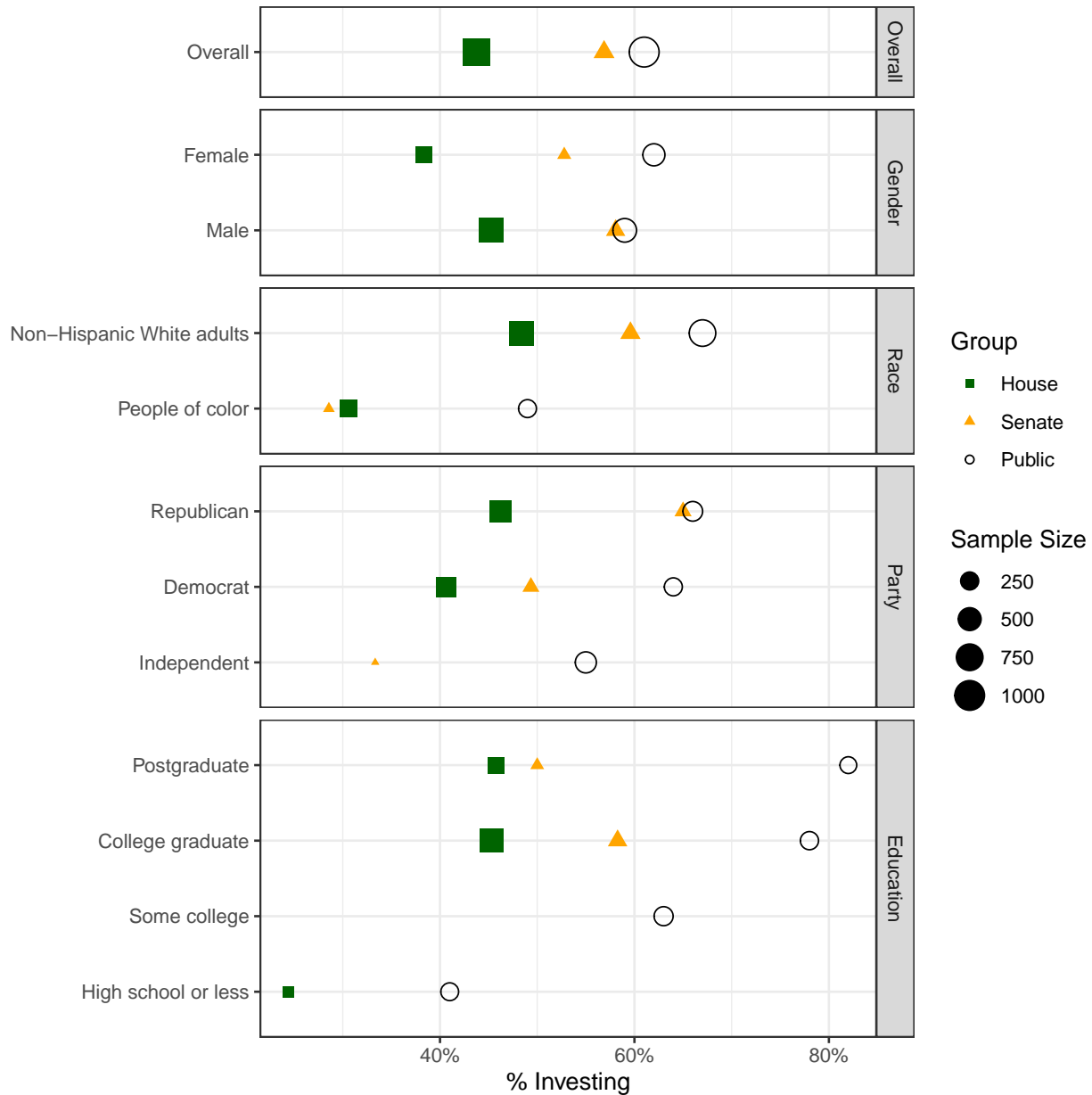


Figure 1: Proportion investing by covariates, broken out by members of the House (green squares), Senate (gold triangles) and the general public (hollow circles, based on Gallup data from 2023: <https://news.gallup.com/poll/266807/percentage-americans-owns-stock.aspx>).

a direct comparison is complicated because they are based on different measures and the precise difference is consequently hard to know, it seems likely that elected officials are not far more likely than the general public to have financial holdings. Elected officials are less likely to be involved even conditional on demographic features – although the relationship

between demographic features and investing activity is broadly consistent, with the exception of gender which, contrary to the results in the general public, suggests less investment by women compared to men in Congress. To be clear, comparisons are complicated not only by the different measures being used, but also because of the lack of overlap between the general public and elected officials – there are only three Independent senators over the period of analysis, only 14 non-white members of the senate, and no politicians with only “some college” (cite Carnes). Moreover, evaluating the most prognostic dimension of general investing behavior – household income – is complicated by the fact that all politicians fall into the upper-most income response category used by Gallup given their congressional salary.<sup>8</sup> Even so, in almost every case, elected officials seem to be less likely to have financial holdings than members of the general public. Contrary to popular perception then, it would appear that the first-order expectation of a pervasive problem with politicians exploiting public office for personal gain is not supported in the data.

## 5.1 Financial predictors of investing

For a subset of politicians, we can also observe their initial assets when elected. We obtain these data from OpenSecrets’ Personal Financial Disclosures (PFD) database, which covers the period between 2004 and 2018. We focus on annual reports and calculate the total number and total value of stocks owned, broken out by whether they are held in an exempt fund, an exempt trust, or a qualified blind trust. We grab the earliest report in which a politician appears in the PFD data. Since these data only go back to 2004, in the comparisons that follow we focus on members of the House who were elected later than 2004 to ensure we are focusing only on their financial positions prior to election to Congress.

Writ large, the majority of members of the House did not invest in the stock market

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<sup>8</sup>We plan to use politician addresses to calculate home values as a proxy for wealth, although the data is still being collected.

after being elected, regardless of whether they held any assets prior to holding public office, although those with assets were more likely to invest than those without (see Table 2). Conversely, the opposite pattern holds in the Senate, where the majority of Senators do invest regardless of whether they previous held stocks. The net conclusion is that prior assets are associated with more than a 6 percentage point increase in the likelihood of investing in the stock market between 2012 and 2023 in the House, but less than a 4 percentage point difference in the Senate.

	House		Senate	
	Invested	Never invested	Invested	Never invested
Any assets	0.473	0.527	0.611	0.389
No assets	0.412	0.588	0.573	0.427

Table 2: Proportion of members of the House (left) and the Senate (right) investing in the stock market between 2012 and 2023 (columns) compared to whether they held any assets in stocks at the time they were elected to the House (rows).

Those who actively traded between 2012 and 2023 were elected to Congress with an average of 32 assets, valued at almost \$4m, compared those who didn’t invest who only held an average of 4 assets valued at \$472,362 (see Figure 2). Again, we highlight the striking differences between the House of Representatives and the Senate, with one’s prior financial situation being prognostic of trading stocks while holding public office among members of the House, but not for those in the Senate.

## 5.2 Political predictors of investing

Even if members appear less likely than the public to report financial holdings, this does not mean that those who do should be spared from further scrutiny. That more than 40% of the House and 50% of the Senate over this time period disclose financial activity raises questions about the nature and implications of the disclosed activity.

Recall from Table 1 that to be able to use their status and position to improve their



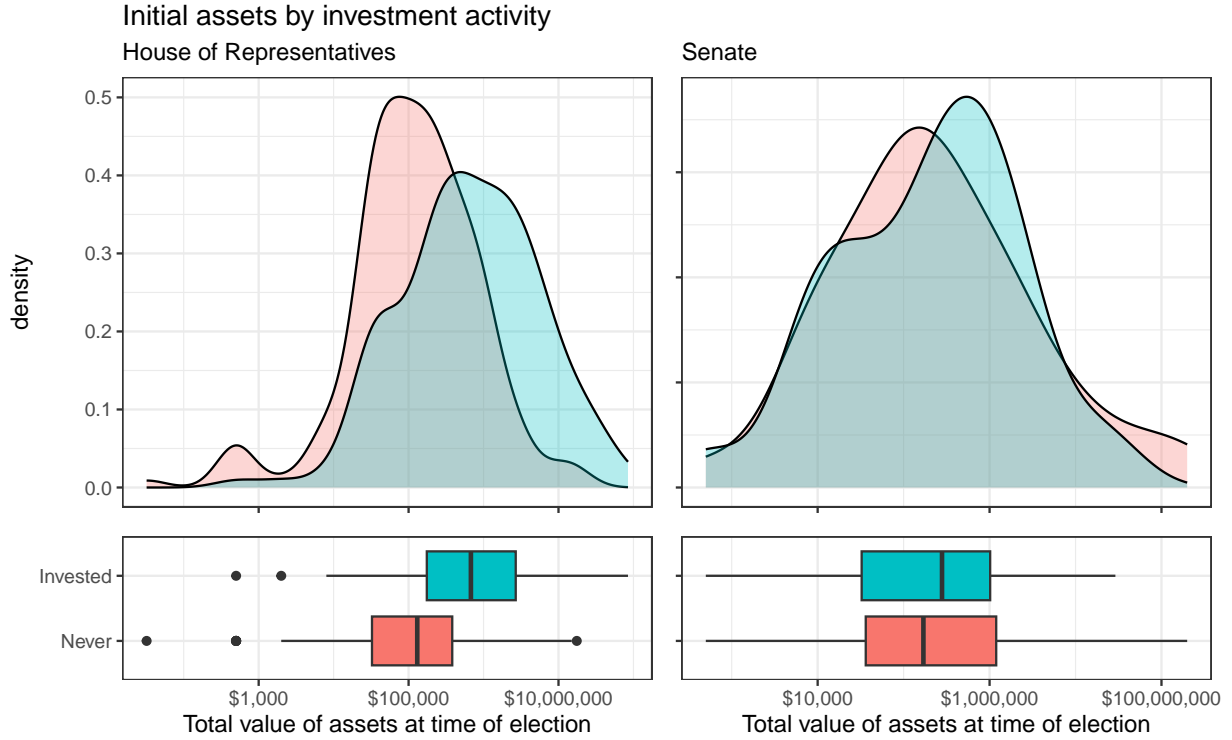


Figure 2: Initial assets for the House (left) and the Senate (right) broken out by whether the politician actively traded while in office between 2012 and 2023.

personal financial well-being (in ways beyond that which is the result of the overall economy), politicians need to be able to access valuable information that they can leverage when buying or selling their holdings. Measuring the nature and extent of the information that members possess, however, is clearly an impossible task. As a result, we conduct a second-best analysis to examine whether a politician’s position within the federal government predicts variation in their investing behavior. In particular, are those serving on congressional committees more likely to invest than those without access to these sources of potentially valuable information? And does the transition from minority to majority status predict changes in investing patterns given that the majority party in the House is well-known to be more powerful?

Based on a robust literature that identifies positions of influence and effectiveness (Volden and Wiseman, 2009) we focus on the effect of holding, obtaining, or losing several

positions of status in Congress. In particular, we proxy for access to plausibly actionable (private) information using the following political traits:

- Whether the politician is the Speaker of the House (**speaker**)
- Whether the politician serves on a “powerful” congressional committee (as per [Volden and Wiseman \(2009\)](#), Appropriations, Budget, Rules, Ways & Means, **power**)
- Whether the politician serves as the chair of a committee (**chair**)
- Whether the politician is the Majority leader or Minority leader (**maj\_leader**, **min\_leader**)
- Whether the politician is in their first term (**freshman**)
- Whether the politician’s party has the majority of their chamber (**majority**)
- Whether the politician serves as the chair of a subcommittee (**subchr**)
- The politician’s vote margin in their previous election (**vote\_pct**)

Our identification strategy to estimate the association between these positions of potential access/influence/information and member activity involves separately estimating each position on whether a member ever invests over the period of analysis. To isolate the importance of between-member and within-member variation over time we estimate each bivariate regression using three fixed effects strategies. First, we use congress-fixed effects to capture the variation in investing behavior between politicians with a given congress (i.e., on average, are members of the majority party more likely to invest than members of the minority?). Second, we use politician-fixed effects to capture the variation in investing behavior within-politicians resulting from changes in member status over time (i.e., are elected officials more likely to invest when their party becomes the majority and, as a result, be in a position to better control the congressional agenda?). Third, we use both politician- and

congress-fixed effects (i.e., two-way fixed effects) to exploit variation within politicians over time, demeaning by the congress to net out systemic changes in investing behavior.

Figure 3 presents the results for members of the House of Representatives for each of the status measures and each of the fixed effect strategies.

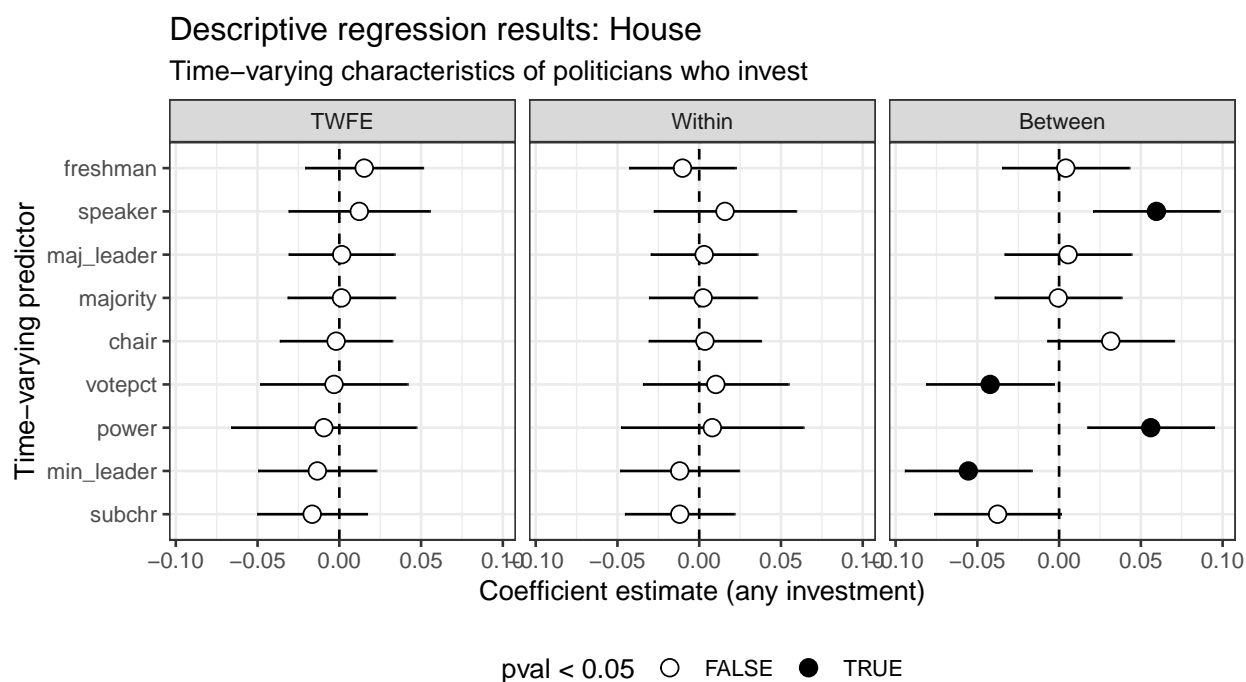


Figure 3: Coefficients from bivariate regressions predicting whether the politician ever invests as a function of time-varying covariates (y-axis), broken out by fixed effects specification (columns). Points colored by statistical significance at the 95% threshold.

Overall, there is very little evidence that time-varying characteristics are correlated with the decision to invest within politicians; no feature is statistically distinguishable when using either two-way fixed effects or a within-member comparison to estimate the correlation between the position and the likelihood of the member making an investment of any kind. (Similar conclusions hold when applying this analysis to Senators – see SI.) Only when we rely on between-member comparisons – comparisons that are likely confounded by other between-member differences! – do we find associations between members’ positions and their likelihood of investing. While the between-member estimates graphed in the third panel of

Figure 3 also suggest that members on power committees are more likely to invest than those that are not, the results regarding electoral vulnerability are unexpected. Contrary to theoretical expectations regarding electoral vulnerability, those with larger winning vote margins in the preceding election are consistently *less* likely to invest. Moreover, interacted specifications reveal no evidence to support the expectation that the combination of security and access is required to reveal this type of behavior; the more electorally secure are significantly less likely to invest than the more insecure writ large and serving on a valuable committee or as committee chair does nothing to change this reduced probability of investing.

The fact that statistically distinguishable associations emerge only when conducting a between-member design is important for interpreting the meaning of the detected relationships. The fact that the statistically distinguishable differences are only present in the between-politician analyses suggests that the difference in investing activity is unrelated to the position itself because we do not observe similar changes among individuals who gain or lose those statuses. Put differently, our pattern of results suggest that those who become either Speakers of the House, or serve on powerful congressional committees are also the type who are more likely to invest, while minority leaders and those who are more electorally secure are less likely to be investor-types. Whereas a naive comparison of between politician behavior might suggest that those on power committees are more likely to invest – perhaps using the information their status affords – by isolating different sources of variation using various combinations of fixed effects our findings suggest the opposite causal pathway; members who are more likely to be investors are also more likely to be assigned to more powerful positions.

Digging deeper, we also explore whether there are important differences based on the specific congressional committees. Even among so-called “power” committees there is likely to be variation in the opportunity and access members may have to obtain private information they could leverage in making financial transactions. To explore this possibility

we conduct the investigation using an indicator for specific committee assignments to allow the relationship to vary.

Figures 4 and 5 plots the results for the House and Senate respectively.

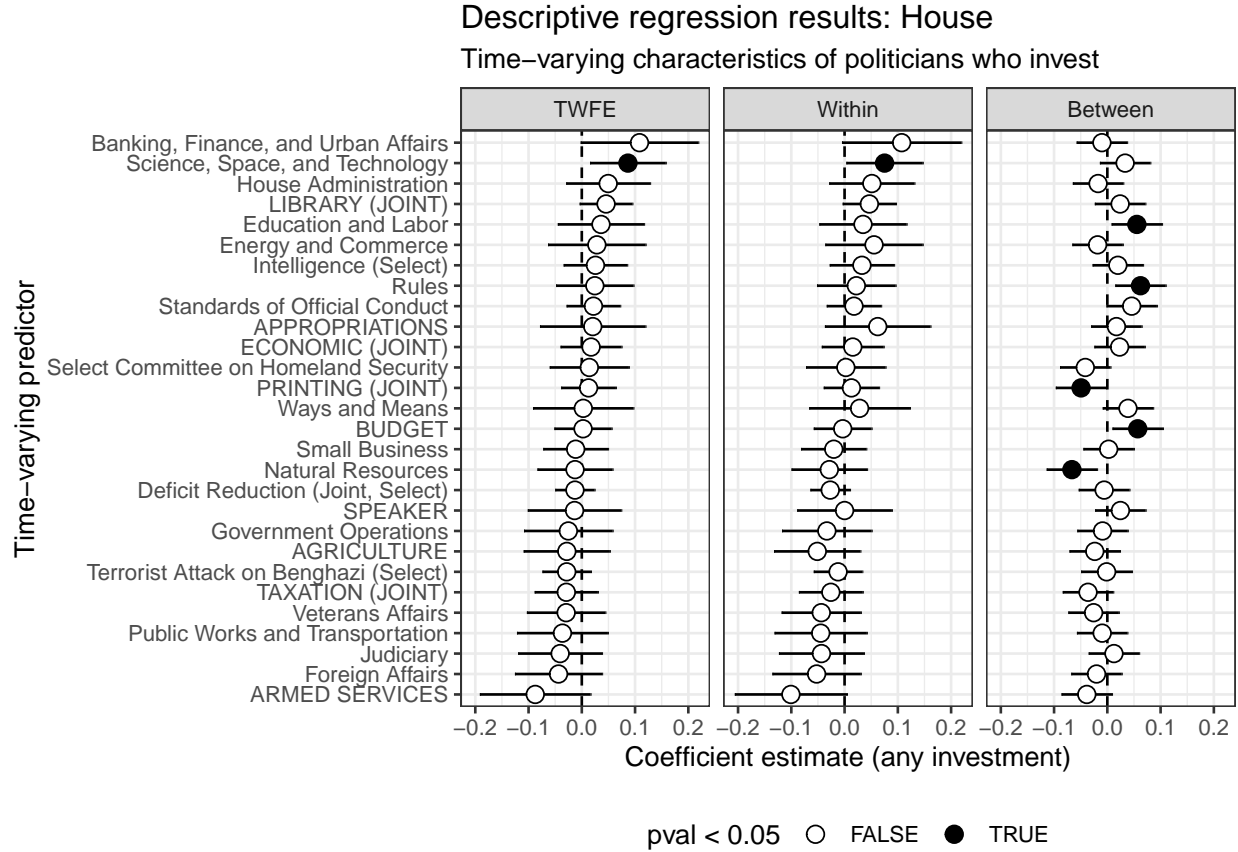


Figure 4: Coefficients from bivariate regressions predicting whether the politician ever invests as a function of time-varying covariates (y-axis), broken out by fixed effects specification (columns). Points colored by statistical significance at the 95% threshold. Members of the House of Representatives.

We find that only a few committee assignments that are significantly associated with investing behavior after isolating over-time variation within a given legislator. In particular, the Banking, Finance, and Urban Affairs and Science, Space, and Technology committees in the House are positively associated with increasing investing behavior (although banking is just over the 5% threshold); as are the Judiciary and Taxation committees in the Senate. Put substantively, being assigned to one of these committees increases the probability that

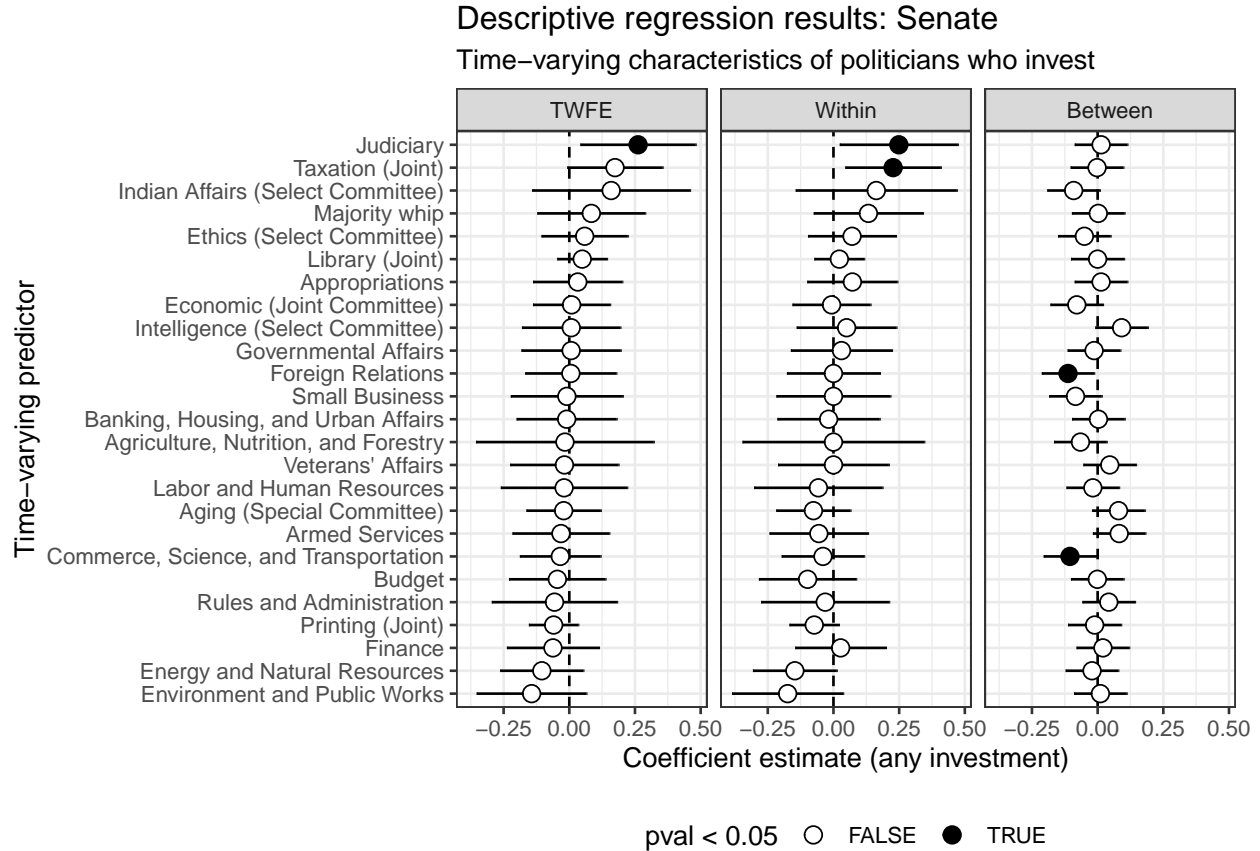


Figure 5: Coefficients from bivariate regressions predicting whether the politician ever invests as a function of time-varying covariates (y-axis), broken out by fixed effects specification (columns). Points colored by statistical significance at the 95% threshold. Members of the Senate.

the politician invests by slightly less than 10 percentage points relative to activity made by that same politician when not on the committee. However, there are two reasons to remain skeptical of these patterns. First, these regression results are not robust to a multiple comparisons correction. Second, different conclusions obtain in the Senate if we predict not the linear probability that a politician invests, but instead the logged number of investments (see SI). Even though there is some evidence consistent with members becoming more likely to make a financial transaction when a select few committees, the evidence is hardly an overwhelming and compelling indictment of politicians' behavior. Evidence of members acting in ways that suggest they are using their status and position to help improve their

personal financial well-being by making financial trades is scant.

### 5.3 Where do Elected Officials invest?

While treating the preceding significant coefficients with skepticism is well-founded, a blunt conclusion that there are no relationships of concern is also premature. Prior analyses examined variation in the coarse measure of whether an elected official made an investment of any kind, but it is both possible and plausible that elected officials who condition their investment decisions on privileged information do so in a more focused fashion than can be revealed by the aggregate analysis above. For example, a politician who decides to invest in a particular firm based on learning about their prospects at a committee hearing might not show up when we look at overall investing activity across all politicians and across all hearings for a particular committee in a particular year.

To better identify the types of behaviors of theoretical interest, we implement a dyadic analysis that links every politician to every publicly-traded firm that appears at least one financial disclosure form between 2012 and 2023. The raw descriptives of this adjacency matrix are visualized in Figure 6, highlighting the extreme skew both among the firms that are invested in, and the politicians that do the investing. In addition, the figure lays bare the rare events nature of the data, with the vast majority of firm-politician dyads sharing no investment link.

With either a binary indicator for whether politician  $i$  invests in firm  $j$  in a given period  $t$ , or the logged number of transactions, as the outcome, we turn to a variety of operationalizations of the right-hand side predictors. The underlying quantity of conceptual interest is whether the politician trades a firm’s stock in response to learning about a given firm’s financial prospects. Although we clearly cannot determine what politicians know and when, we operationalize this possibility using several possible measures that include:

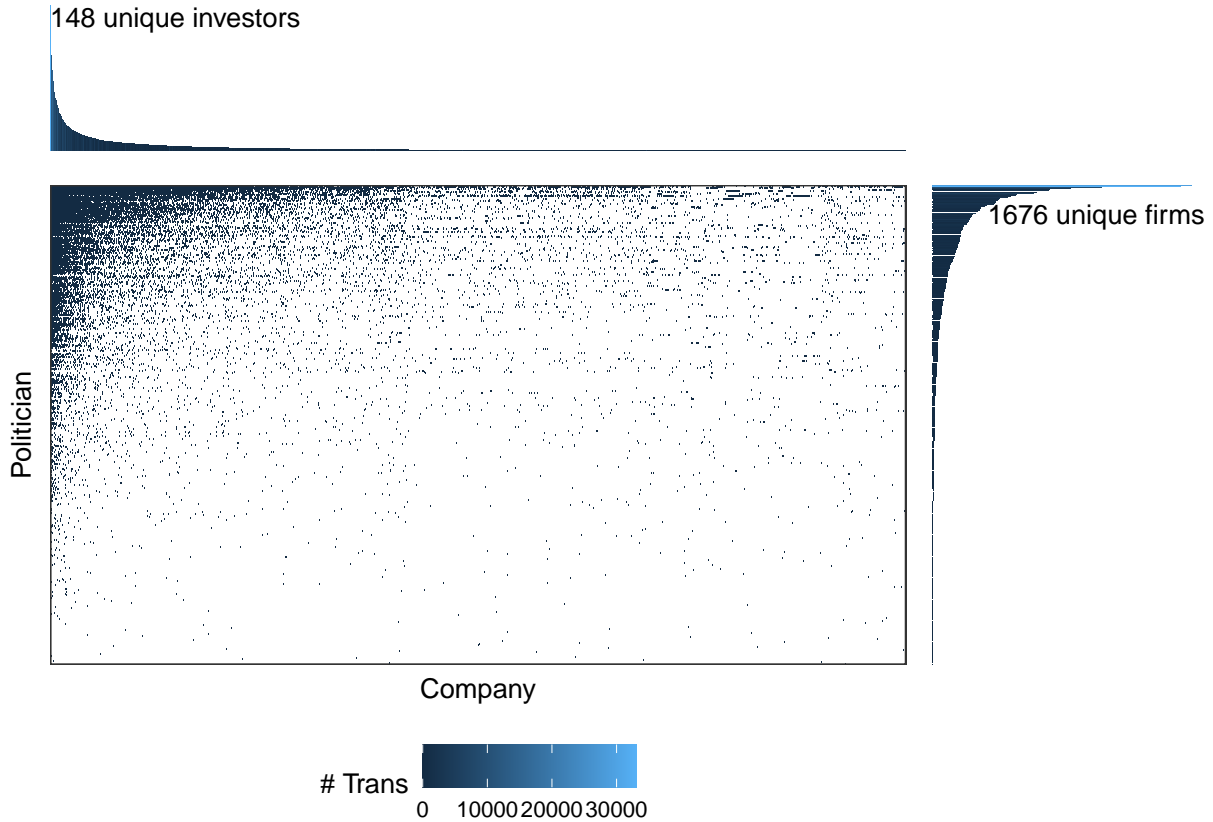


Figure 6: Adjacency matrix of transactions between a politician (y-axis) and a firm (x-axis), aggregating over both the House and the Senate. Total unique investors by firm visualized with the marginal figure at the top, and total unique firms invested in by politician visualize with the marginal figure at the right.

- **Lobbying:** Firms that lobby the committee on which a politician serves, a bill that a politician (co-)sponsors, or target the politician herself are perhaps better known to the politician.
- **Geography:** Firms that are located in the politician's constituency (state or district), or who own subsidiaries who employ the politician's constituents, are perhaps better known to the politician.
- **Campaign Contributions:** Firms that donate to a politician's campaign theoretically obtain access to the politician, opening a pathway along which investment-



relevant information might travel.<sup>9</sup>

For each, our question is simply whether the politicians are more likely to trade the stock of the specific firm if: the firm lobbies the politician or committee, the firm has a stake in the member’s district, and whether the firm (or the industry PAC associated with that firm) contribute to the member.

### 5.3.1 Lobbying

We start with the analysis of lobbying by constructing a dyadic dataset in which every politician is linked with every firm that appears in both the lobbying and transaction data.

Our measure of lobbying comes from [Kim \(2018\)](#), where politicians and firms are linked by the number of bills sponsored by the politician that are lobbied by a given firm.<sup>10</sup> This approach requires us to link firms found in both the lobbying data and the stock transaction data by the company’s GVKEY identifier. The resulting dataset omits 4,154 firms out of the 9,039 in the stock transaction data that don’t have a GVKEY identifier, and 1,257 out of 60,111 lobbying relationships. We operationalize both the outcome and predictor variables as either the raw counts of transactions / bills, logged versions of both +1, and dummies indicating whether a given politician-firm dyad has ever invested / lobbied. Two-way cluster robust standard errors are temporary stand-ins for the dyad-cluster robust estimation that will be eventually implemented.

As in our prior analyses, we again apply a rich set of fixed effects specifications to an annual aggregation of the data, where we observe each politician-firm investment behavior

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<sup>9</sup>Results TBD.

<sup>10</sup>We are unable to precisely determine the amount of money spent on lobbying a politician, since the majority of lobbying reports include a number of targets for a given sum, and only rarely include named politicians in these summaries.

and lobbying relationship for each year between 2012 and 2020. We plot these results in Figure 7, and put the various combination of fixed effect specifications we use on the y-axis. The left panel models the probability of buying the stock of a lobbying firm and the right panel models the probability of selling. (Additional findings using alternative measures of investing behavior can be found in the SI.)

Regardless of the fixed effect specification we use to account for unmeasured variation, we find a correlation between lobbying and members’ investing behavior. Across all sources of possible variation in lobbying behavior denoted by the various combinations of fixed effects (y-axes), the coefficients measuring the statistical association are consistently positive and statistically significant at the 95% level. That said, the strength of the association attenuates substantially as we isolate variation to within the firm-politician dyad, demeaning by year. When looking at the average variation within a firm-political dyad by year, the estimated association is only 0.005 of a standard deviation – less than half the magnitude of the relationship found when we look across both politicians and firms in a given year.

To put the magnitude of these scaled coefficients in perspective, and help interpret the magnitude of these statistically distinguishable associations, Table 3 reports the raw proportions in the dyadic lobbying data.

	No investment	Any investment	Proportion
No lobbying	56,073,762	28,401	0.051%
Any lobbying	55,306	491	0.88%
Ratio			17.25X

Table 3: Descriptive proportions: Lobbying

As reported, across all pairwise dyads linking each politician with each publicly traded firm, there are only 491 instances in which a politician invested in a firm that lobbied them. However, this constitutes almost 1% (0.88%) of the total number of dyads with any lobbying relationship, which is itself more than 17 times larger than the same ratio calculated among dyads with no lobbying link (0.051%). Thus, while these marginal effects are very small,

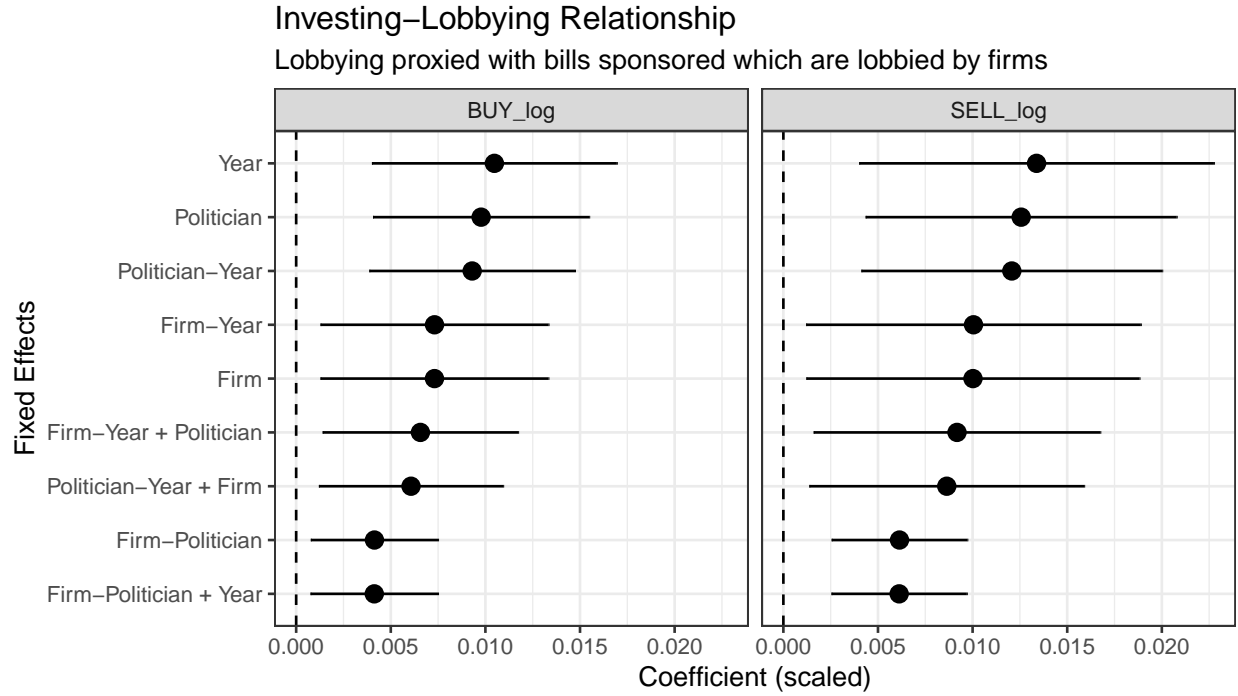


Figure 7: Coefficients capturing relationship between different measures of investing behavior (columns) as a function of lobbying by a given firm (rows), implementing different levels of fixed effects (y-axes).

they are relative to even smaller baselines, given the dyadic structure of our data.

### 5.3.2 Geography

Next, we turn to whether elected members are more likely to trade firms with a physical presence in their district. To explore the possible effects of geographic-based representation, we linking politicians (specifically, Members of the House of Representatives) with the firms they invest in on the basis of where the firm employs the politician’s constituents on the basis of their subsidiaries. For example, Apple employed roughly 40 constituents per each of its 276 stores in 2018, along with over 6,500 in its customer service center in Austin, Texas, and more than 4,300 at its headquarters in Cupertino, California (see Figure 8). This links Apple with roughly one-half of the members of the House of Representatives in 2018 (206 out of 435 in total) by virtue of employing their constituents. Are these politicians more

likely to invest in Apple compared to those politicians whose constituents are not employed by Apple?<sup>11</sup>

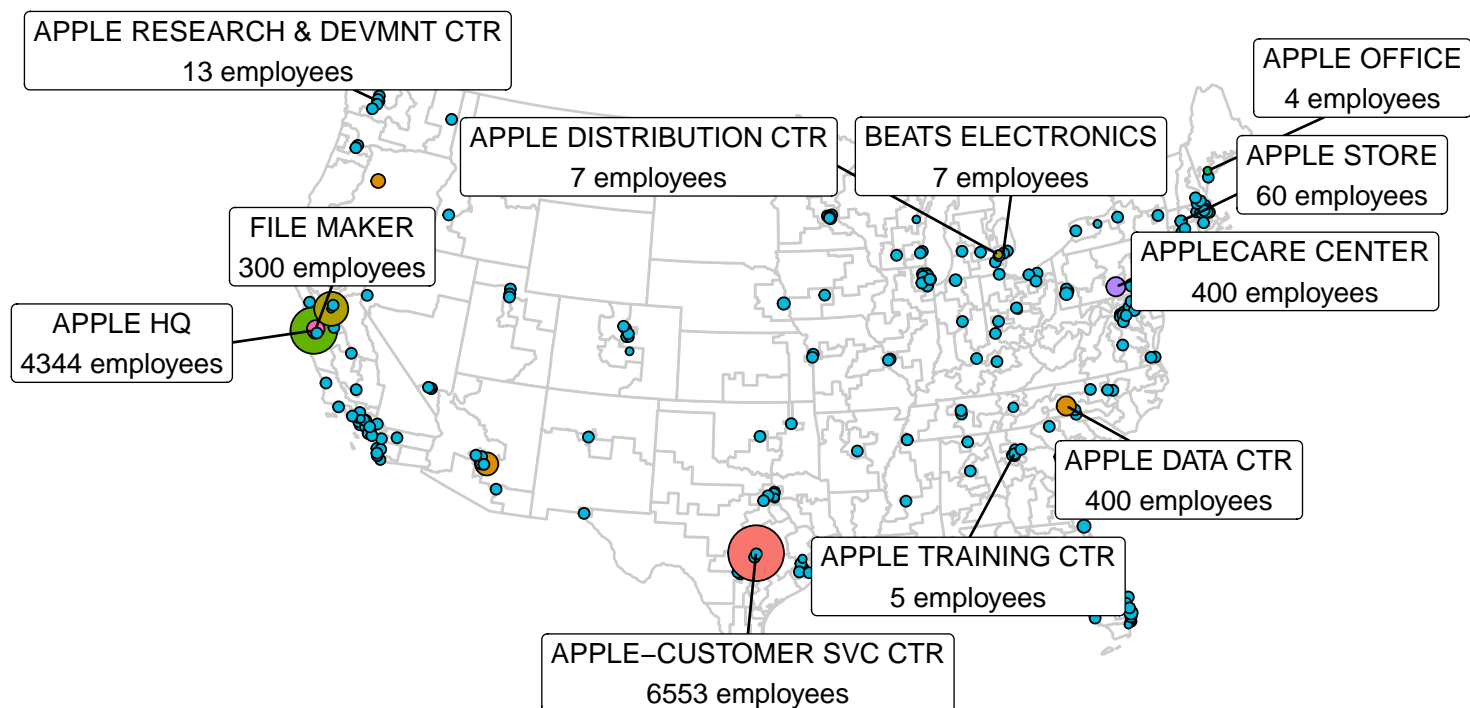


Figure 8: Distribution of Apple Inc.'s locations in 2018, sized by the number of employees.

As with our preceding analyses, we do not interpret the resulting relationships as

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<sup>11</sup>Currently we only have data on the geographic distribution of firm subsidiaries up to 2018, although we are hoping to obtain additional data extending the period of analysis to 2023.

causal, although we may be more confident in extrapolating certain possible causal pathways on the basis of the fixed effects specifications we apply. As illustrated in Figure 9, the most striking associations obtain when we fail to hold constant time- and politician-invariant firm characteristics. This is consistent with a spurious association story in which firm size is positively associated with a politician’s investment decisions and with the number of subsidiaries found in the politician’s district. In other words, larger firms are more likely to be targets for politician investment, and are more likely to employ the politician’s constituents.

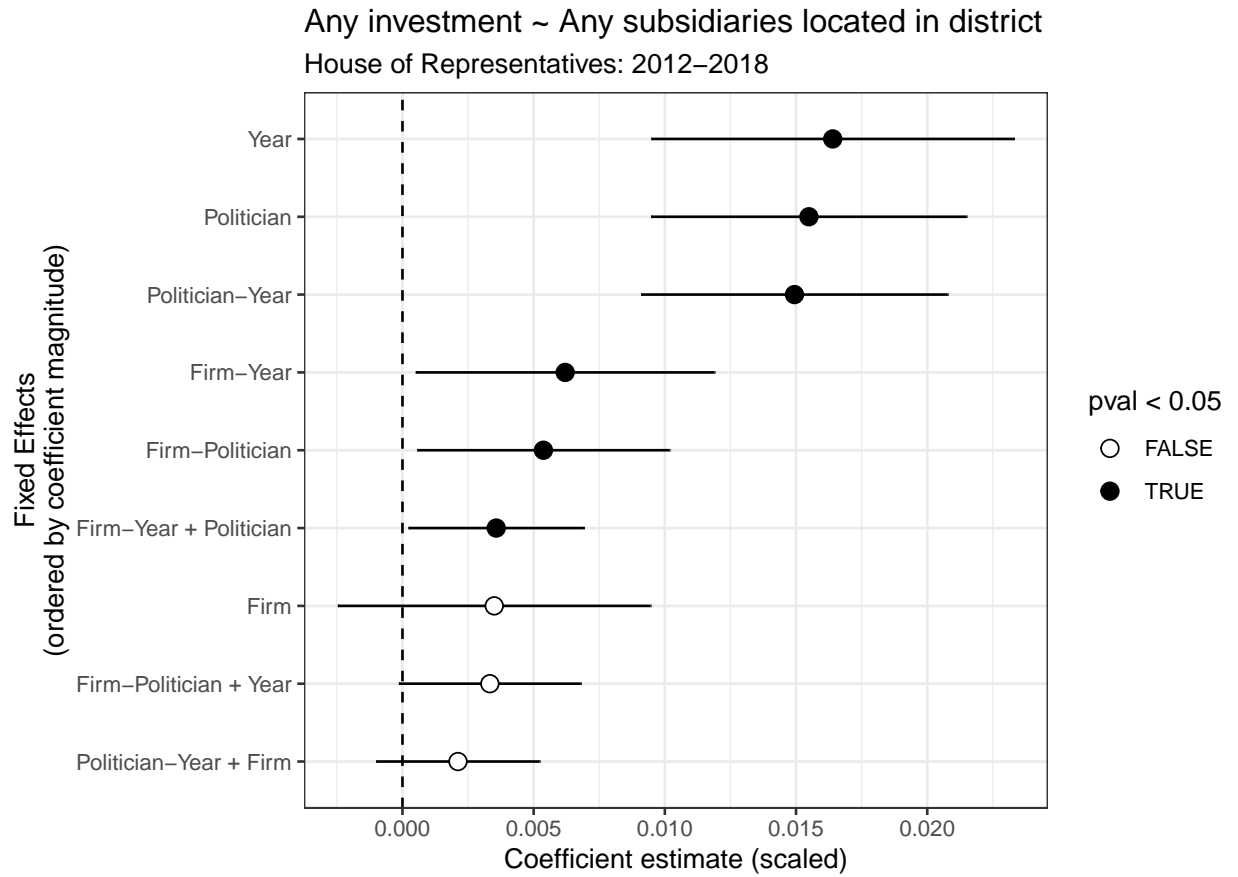


Figure 9: Coefficient estimates (scaled) predicting a dummy indicator for any investments by a politician in a firm in a given year as a function of a dummy indicator for the existence of subsidiaries the firm owns in a politician’s district in a given year. Two cluster robust standard errors (clustered on the politician and the firm) presented as horizontal bars. Points colored by whether the cluster-robust p-value is less than 0.05. Y-axis indicates the fixed effects design, and are ordered in terms coefficient size.

Looking down the rest of the y-axis suggests that the strength of the association

continues to attenuate, both in terms of statistical and substantive significance, as we move toward isolating sources of variation that might be more plausibly interpreted as causal. The weakest results obtain when we implement politician-year and firm fixed effects. This specification means that we are looking within a politician in a given year across all the firms they invest in, demeaning each firm by its overall level of investing. A similarly weak result is found when we implement dyad-fixed effects and demean by year. Nevertheless, it is worth noting that all of these associations are positive, albeit small, and persistent pattern that is incompatible with a noisily (or even precisely) estimated null result.

Similar patterns are evident when shifting analysis to senators. Here, we calculate the number of subsidiaries and the number of employees in the senator’s state as the main predictor of interest. As illustrated in Figure 10, the evidence is consistent with a selection story wherein the largest firms are both that are the targets of politician investments and those that have more subsidiaries.

As a final test, we turn to the location of the headquarters themselves. It might be the case that politicians have closer relationships with firms that are headquartered in their district / state, creating a link that influences investing behavior. The approach is identical to the results presented above, except that we only predict variation in whether the politician’s congressional district (for members of the House) or state (for senators) is home to a given firm’s headquarters. As illustrated in Figures 11 and 12, similar patterns emerge, albeit even weaker in the House of Representatives. In general, and in contrast with the findings documented in [Eggers and Hainmueller \(2014\)](#) who used data prior to the passage of the STOCK Act, there is little evidence to suggest that geographic ties are associated with investing relationships.

As above, we present the conditional means for the proportion of dyad-years in which the politician invests in a given firm, broken out into whether the firm is located in the politician’s constituency to aid in substantive interpretations. While all comparisons still

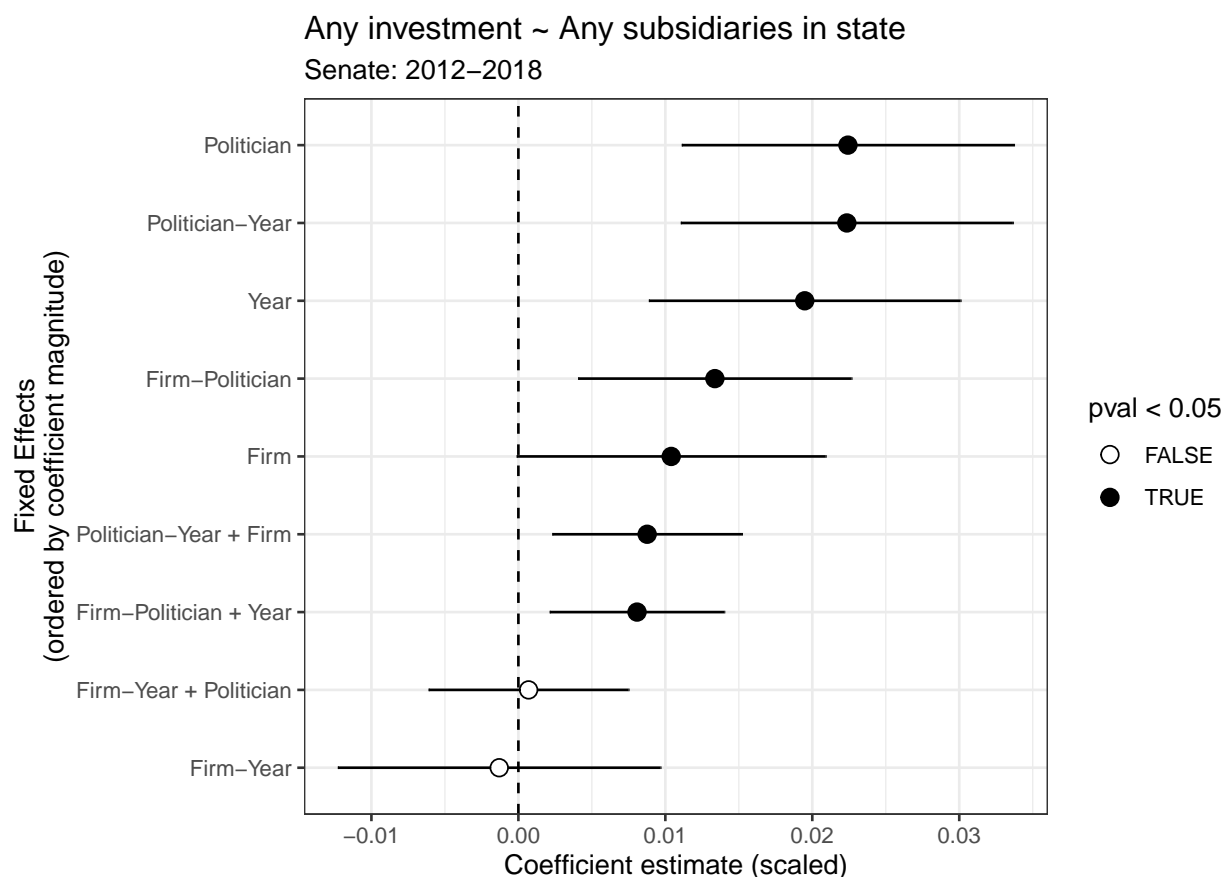


Figure 10: Coefficient estimates (scaled) predicting a dummy indicator for any investments by a Senator in a firm in a given year as a function of a dummy indicator for the existence of subsidiaries in a senator’s state in a given year. Two cluster robust standard errors (clustered on the politician and the firm) presented as horizontal bars. Points colored by whether the cluster-robust p-value is less than 0.05. Y-axis indicates the fixed effects design, and are ordered in terms coefficient size.

suggest greater investing behavior when the geographic link is open, the magnitude of this difference relative to the baseline ranges between 2 and 4 times, much smaller than the 17X multiplier found in the lobbying data.

	House			Senate		
	None	Any	%	None	Any	%
No Subsidiaries	11,072,372	14,428	0.13%	2,269,162	1,828	0.081%
Any Subsidiaries	875,738	3,094	0.352%	422,692	1,164	0.275%
Ratio			2.7X			3.4X

Table 4: Descriptive proportions: Subsidiaries

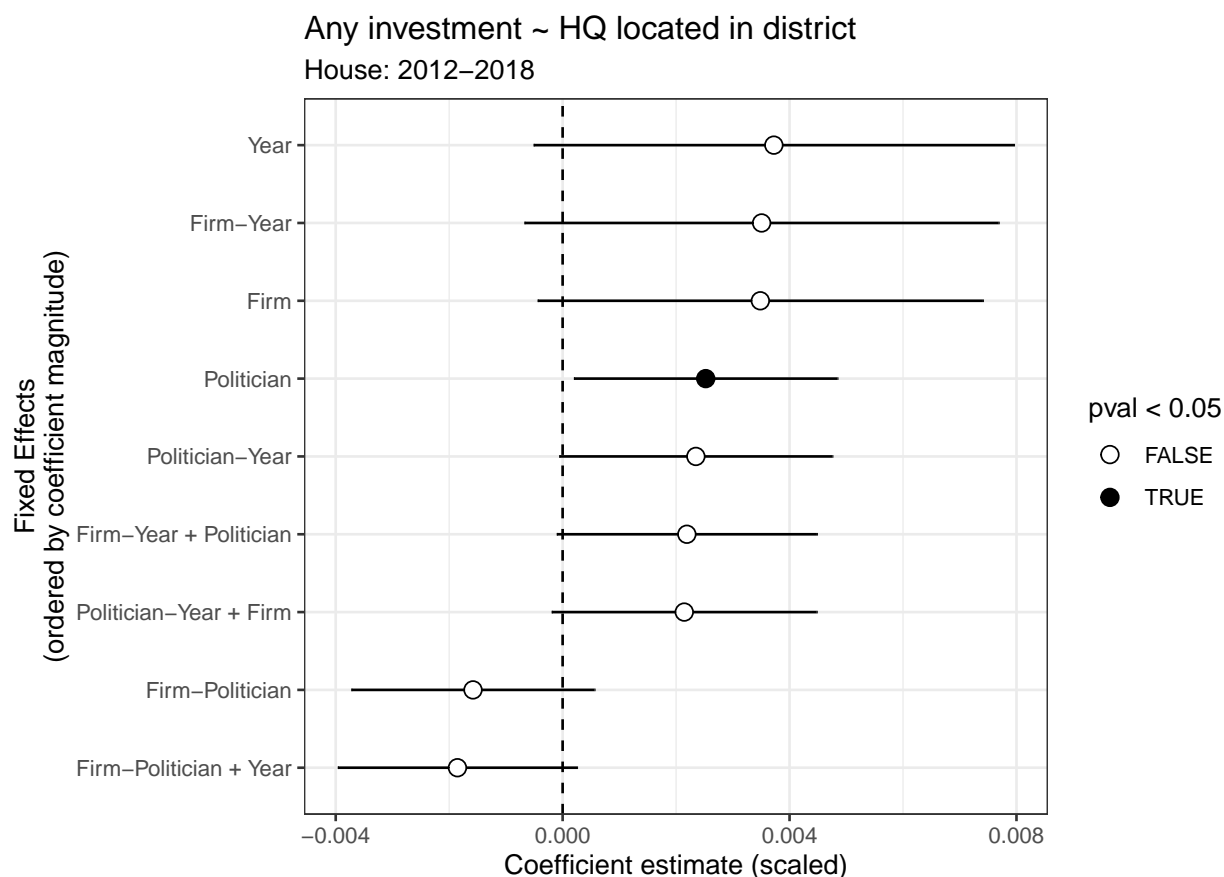


Figure 11: Coefficient estimates (scaled) predicting a dummy indicator for any investments by a Senator in a firm in a given year as a function of a dummy indicator for the existence of a firm’s headquarters in a senator’s state in a given year. Two cluster robust standard errors (clustered on the politician and the firm) presented as horizontal bars. Points colored by whether the cluster-robust p-value is less than 0.05. Y-axis indicates the fixed effects design, and are ordered in terms coefficient size.

	House			Senate		
	None	Any	%	None	Any	%
No Headquarters	11,935,906	17,450	0.146%	2,667,168	2,934	0.110%
Any Headquarters	12,204	72	0.587%	24,686	58	0.234%
Ratio			4.02X			2.1X

Table 5: Descriptive proportions: Headquarters

### 5.3.3 Campaign Contributions

To be analyzed, conditional on an army of RAs who can help merge firms with PACs / a generous Money-in-Politics scholar who already has this. This is complicated as perhaps this



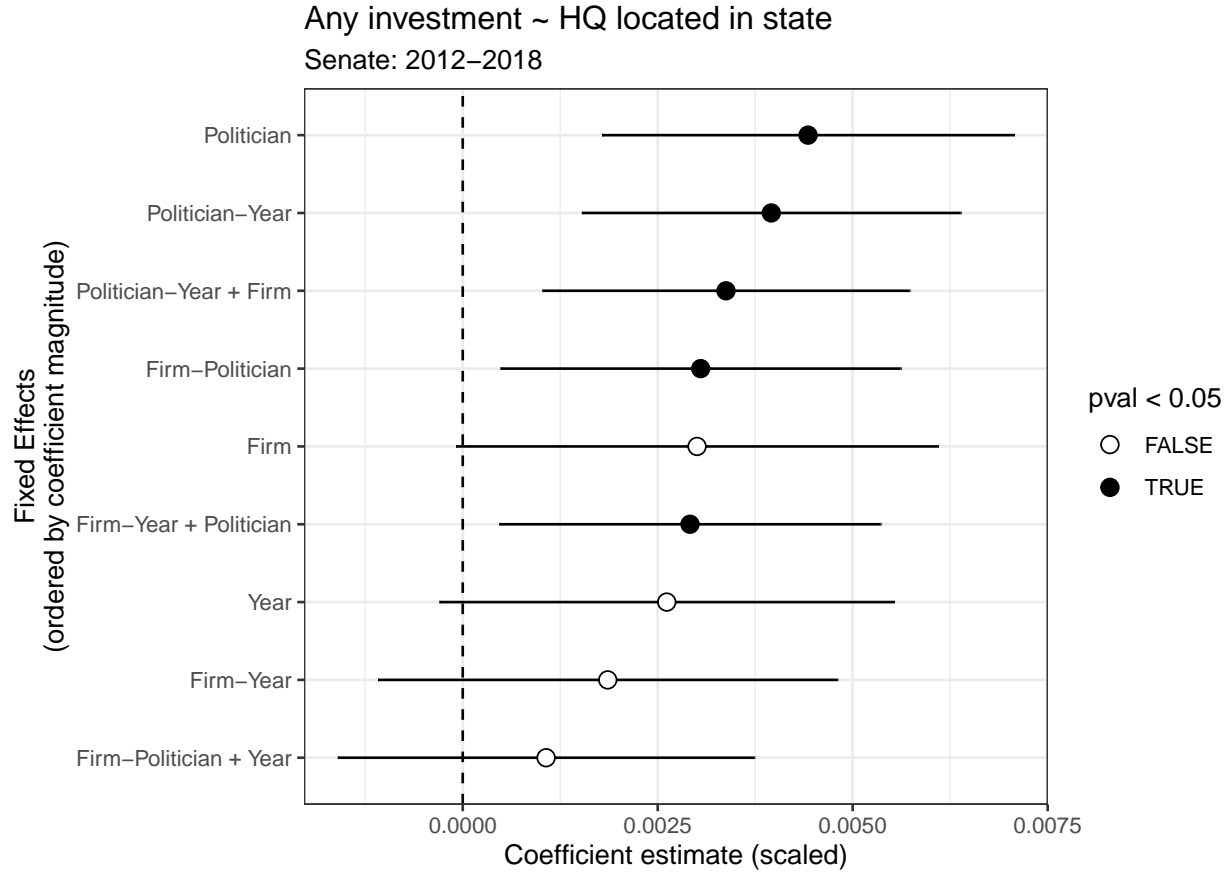


Figure 12: Coefficient estimates (scaled) predicting a dummy indicator for any investments by a Senator in a firm in a given year as a function of a dummy indicator for the existence of a firm’s headquarters in a senator’s state in a given year. Two cluster robust standard errors (clustered on the politician and the firm) presented as horizontal bars. Points colored by whether the cluster-robust p-value is less than 0.05. Y-axis indicates the fixed effects design, and are ordered in terms coefficient size.

occurs via industry/association PACS?

## 5.4 When do Elected Officials invest?

Thus far our analyses reveal that investing behavior among politicians seems more a consequence of politician-type than of external pressures and opportunities. The patterns we find suggest that elected officials are either investors or not and their investing behavior does not obviously change in ways that suggest that they are using their status or position to make

financial trades. On net, this characterization is inconsistent with the popular perception that politicians abuse their privileged information for personal gain.

But how do we reconcile this with the documented evidence of this kind of behavior by journalists? For example, there is compelling evidence indicating that both Kelly Loeffler and Bill Burr (as well as several others) used privileged information about the impact of Covid-19 on the United States to inform investment decisions (Lachlan Markay, 2020; Burns, 2020). Similar behaviors were also documented around the time of 2008’s Great Recession and Global Financial Crisis and the reporting on these issues were arguably important for creating the necessary political momentum to pass the 2012 STOCK Act that was designed to limit these abuses of public office (<https://www.businessinsider.com/congressional-insider-trading-revealed-on-60-minutes-2011-11>). If we are unable to document these types of specific incidents using statistical methods, perhaps the entire endeavor of aggregate quantitative description is an inappropriate tool for the research question at hand – either because the behavior is so rare (an optimistic take) or because of the coarseness of the available measures (a pessimistic take)?

To test this, we analyze transactions at the politician-day level to and explore whether we can see evidence of the documented questionable behaviors in 2020 in the reported data. We start by plotting the weekly data in Figure 13, revealing a noticeable spike in both the number of politicians filing transactions, as well as the total number of transactions, in the beginning of 2020 (highlighted with a vertical grey bar), around the time when the private committee hearings were being held that supposedly informed illicit trading decisions by attending politicians. Of particular note are the clearly visible spikes in terms of the total number of transactions within the Senate around this time, especially among transactions that involved selling stocks.

However, the purely descriptive visualization of Figure 13 leaves several important questions unanswered. First, from an inferential perspective, how confident can we be that

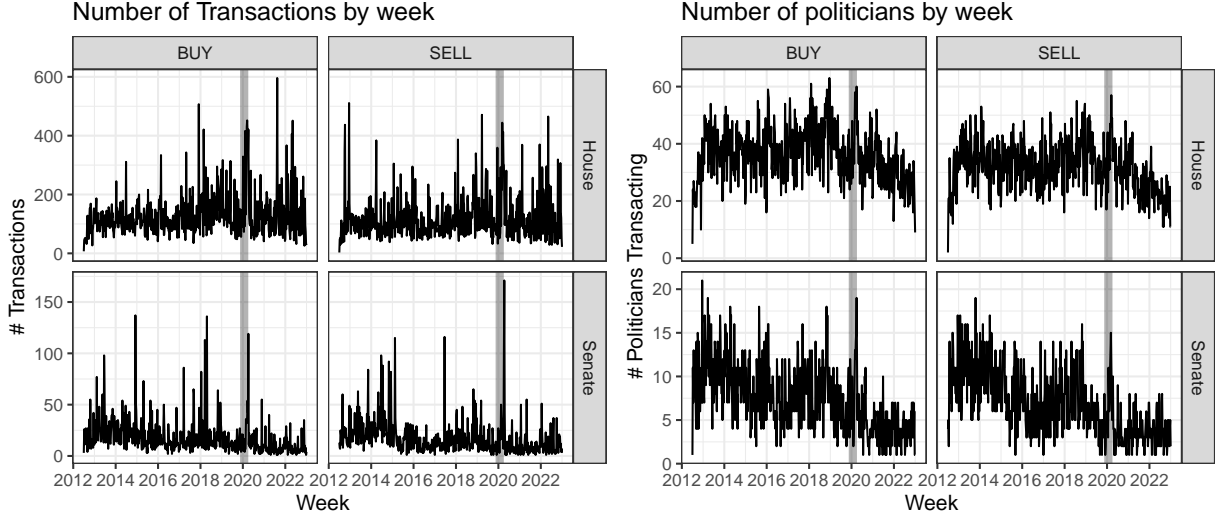


Figure 13: Total number of transactions and number of politicians filing by week between 2012 and 2023. Vertical gray bars indicate initial exposure to information about the severity of Covid-19.

these are significantly different from the general ebb and flow of transactions made by politicians over the period of analysis? To evaluate this question, we implement the Rosner Test (Rosner, 1983) for outliers in time-series data, examining the top 10 largest outliers by transaction type, chamber, and measure (total transactions per day or number of politicians per day). We plot the results for total transactions by chamber and type in Figure 14, and include alternative measures (average transactions per politician, and total politicians) in the Appendix. As is clear, although there are many indications of outliers in the data, there is little consistent evidence pointing to insider trading with respect to the Covid-19 pandemic, and the early committee hearings that are thought to have spurred certain senators' trading decisions.<sup>12</sup> In particular, the spike in Senators' sales of stocks in the spring of 2020 occurred in mid-April of that year, after which the extent of the disruption of the pandemic on the economy had already arguably become common knowledge.

Second, what is the appropriate counterfactual here? Despite the ocular analysis sug-

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<sup>12</sup>When using the measure of total politicians buying or selling stock by day, the majority of outliers are not statistically significant at the 95% level.

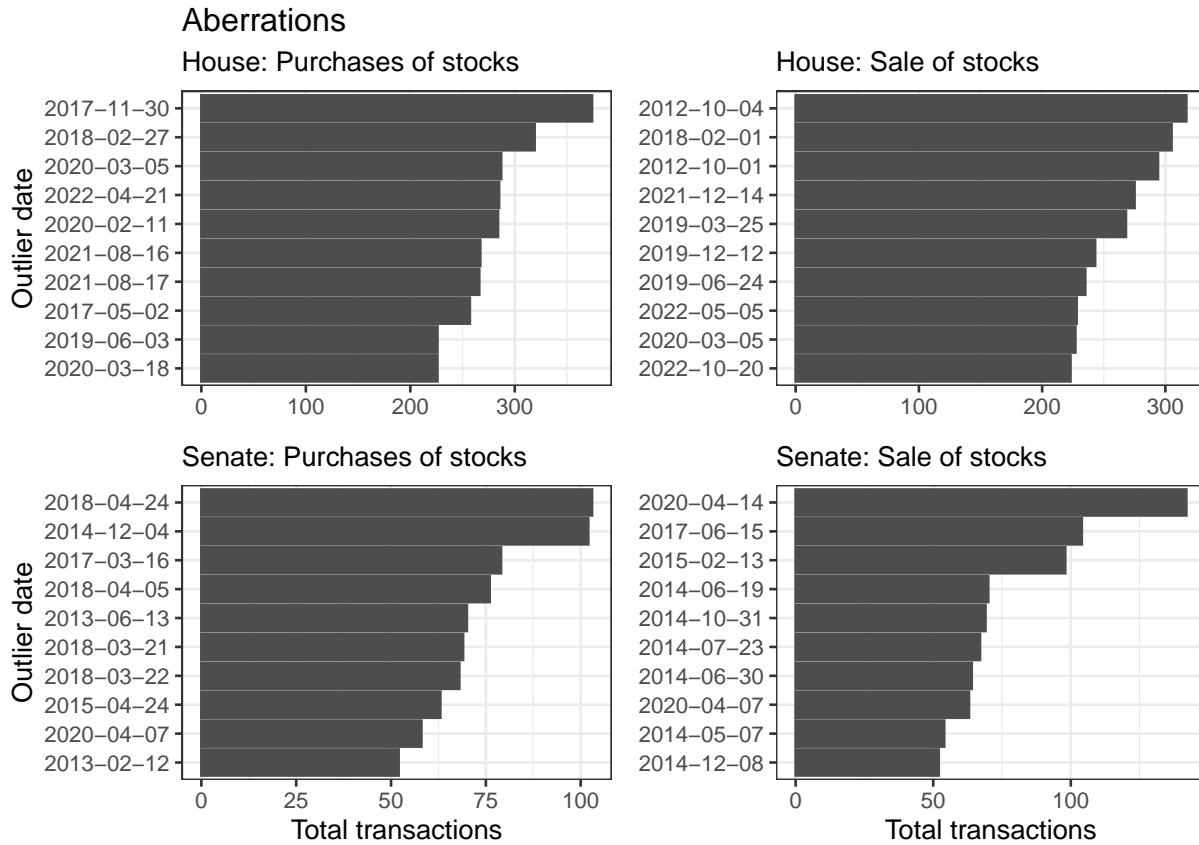


Figure 14: Outlier detection using the Rosner test. Top 10 largest outliers by total transactions per day, disaggregated by the House and Senate (rows) and by the type of transaction (columns).

gesting that sales of stocks spiked following the initial reports of Covid-19, does this necessarily mean that politicians were transacting on insider information related to the pandemic? Or were they perhaps behaving rationally in response to the growing concern with the ongoing and worsening pandemic in the first few months of 2020?

As a final piece of evidence, we look at the trading activity of the politicians implicated in the insider trading scandal, and look for evidence of an increase in trades just after the closed-door Congressional hearing on January 24th, 2020, and before the start of the Department of Justice investigation into prohibited insider trading on March 30th, 2020. As illustrated in Figure 15, there is some visual evidence of an increase in trading activity following the closed-door hearing, although the largest spikes only occurred after the start

of the DOJ investigation.

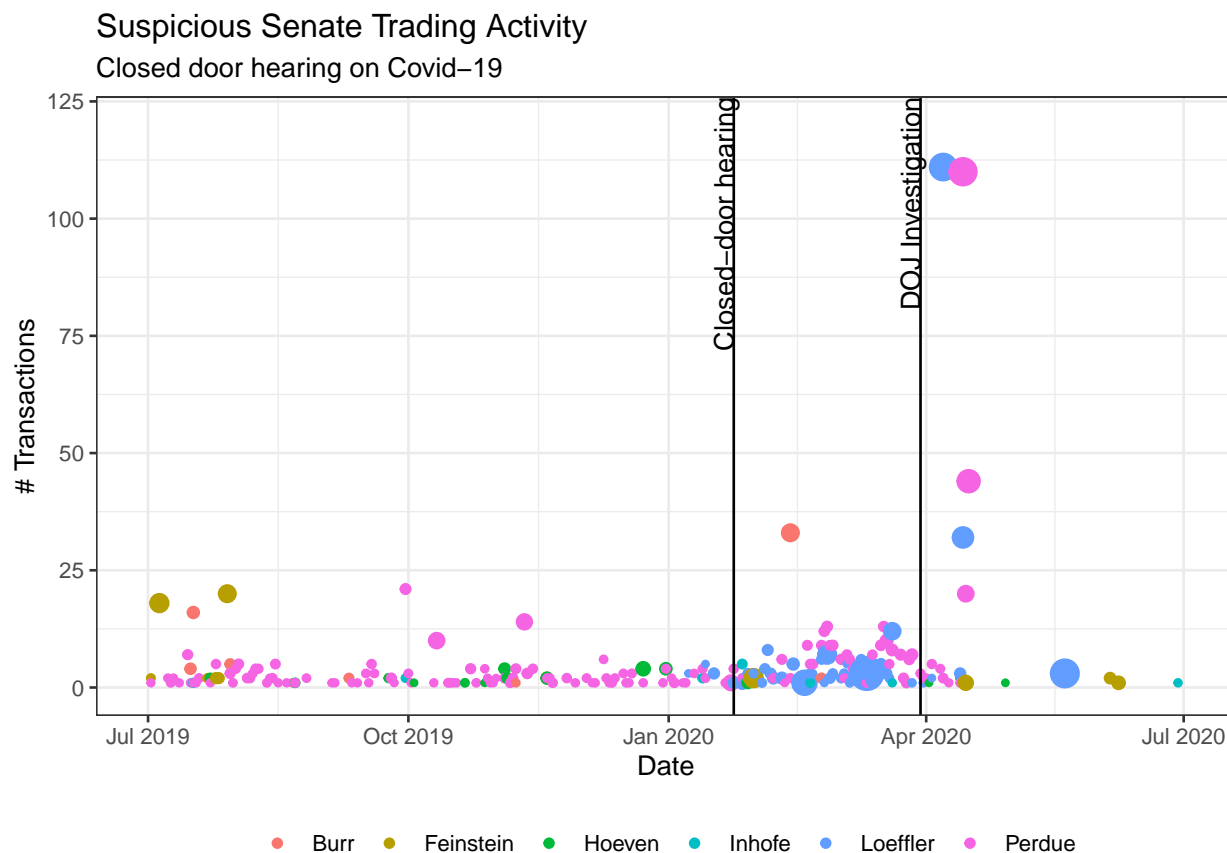


Figure 15: Trading activity before closed-door hearing, between closed door hearing and DOJ investigation, and after DOJ investigation.

Breaking out each politician into their own plot (see Figure 16) highlights that smoking gun evidence is only found for Kelly Loeffler, who conducted a flurry of trades immediately following the closed-door hearing. But even here, the timing doesn't totally line up, with her initial trades preceding the hearing, and several days of intense trading after the DOJ investigation began. Furthermore, she only took office on January 6th, 2020, explaining the lack of data prior to this point.

Examining trading data at this level of detail moves us beyond the realm of aggregate empirical analysis. In so doing, it highlights the limitations of statistical approaches to social science research questions, but also their benefits. The evidence suggests that – at least –

## Suspicious Senate Trading Activity

Closed door hearing on Covid-19

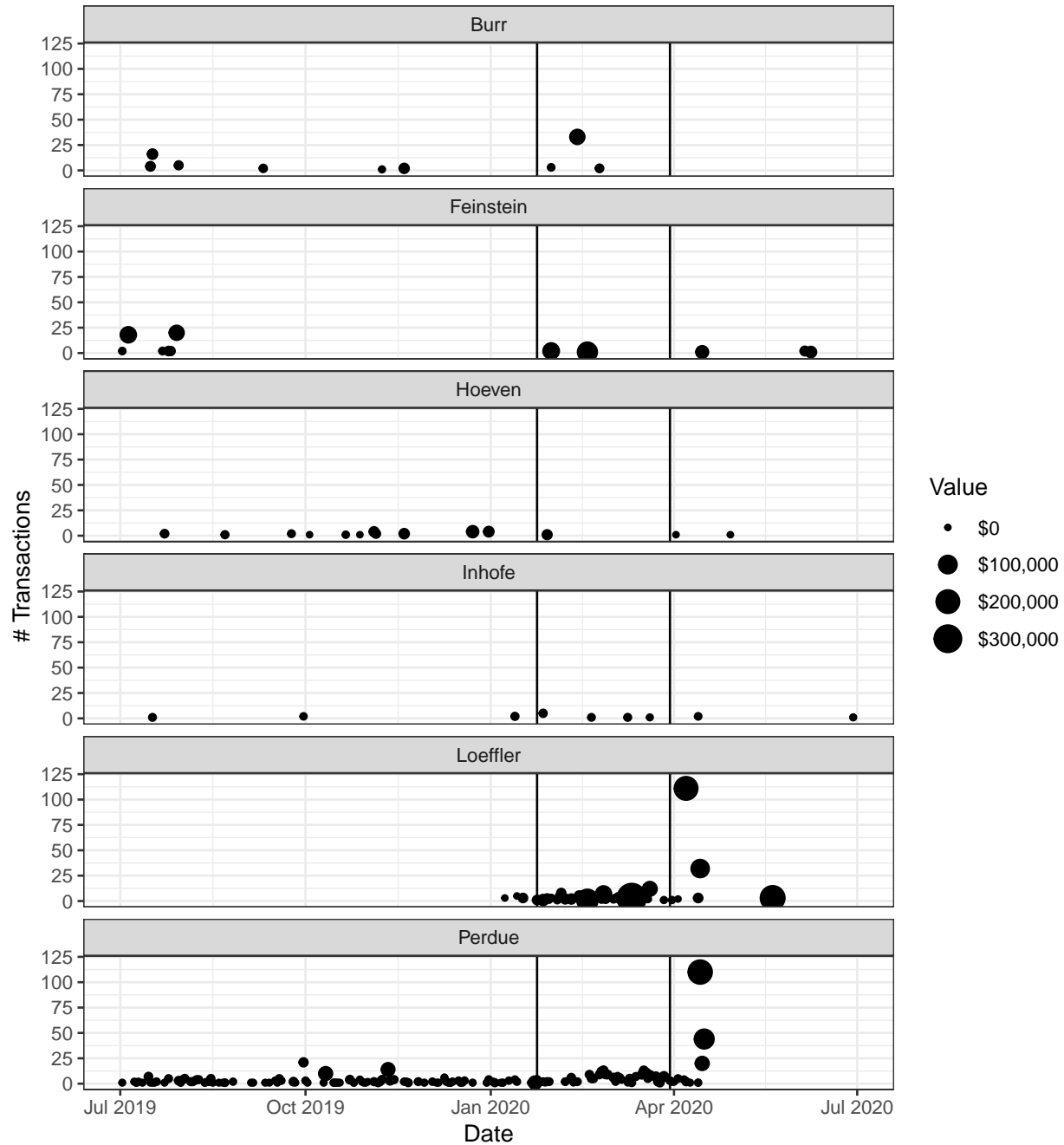


Figure 16: Trading activity, broken out by implicated Senator.

Kelly Loeffler traded on privileged information obtained from holding public office. And this evidence would be swamped by standard regression approaches to inference, whose reliance on averaging would suppress the patterns revealed by simple descriptive visualization.

(Indeed, a diff-in-diff regression finds no support for the conclusion that the differences in trading patterns among Senators who did and did not attend the closed-door hearing [first difference] significantly changed before and after the hearing occurred [second difference].) However, descriptive visualization provides little in the way of inferential structure. Yes, Loeffler’s trading looks suspicious, but without more structure approaches to inference, we can’t say more than this.

## 6 Conclusion

The extent to which elected officials use their position and status to benefit themselves – perhaps at the expense of their constituents – is a concern at the forefront for representative government. The legitimacy of any government depends on the confidence and support of the governed and questions how best to maintain that trust and support is an issue of increasingly pressing concern given historically low levels of trust in national political institutions and, perhaps related, fears about potential democratic backsliding. The increasing belief that the government is corrupt and politicians are out for their own benefit arguably contributes to both of these trends.

We explore the contours of members’ financial activity using financial disclosures required by the STOCK Act to probe the extent to which the recorded activity is consistent with members using their status and access for self-gain. By examining when, who, and how members engage in financial transactions we provide a comprehensive characterization of the empirical regularity that suggests several important conclusions about the nature of member activity.

By aggregating the data in several different ways – by member, by member-year, and by member-firm – and by employing a rich combination of fixed effect combinations to account for various systemic features of the relationship we are able to identify the nature of the

variation in investing behavior that occurs.



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## 7 Appendix

### 7.1 Fixed Effects Robustness

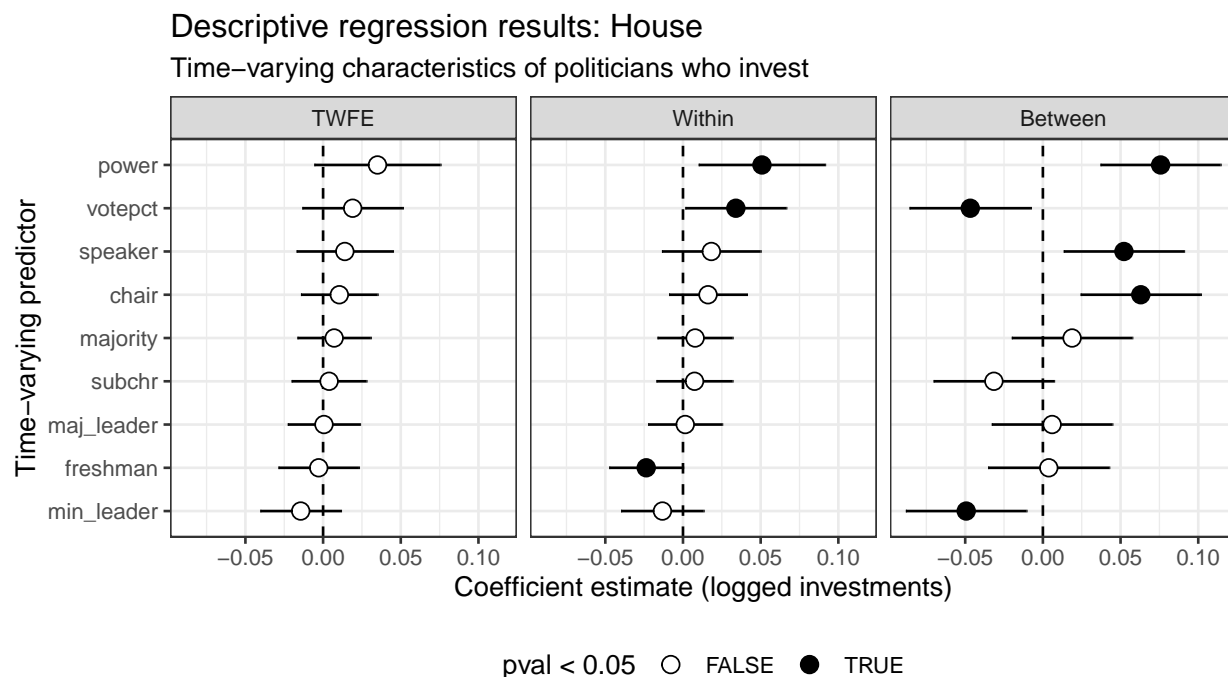


Figure 17: Coefficients from bivariate regressions predicting whether the logged number of investments as a function of time-varying covariates (y-axis), broken out by fixed effects specification (columns). Points colored by statistical significance at the 95% threshold. House of Representatives.

### 7.2 Who invests?

Who are these people? Why doesn't everyone invest?

Pure descriptives based on demographics:

- Between 2012 and 2023, 43.7% of members of the House filed STOCK reports
  - 38.4% of women in the House invest, compared to 45.3% of men

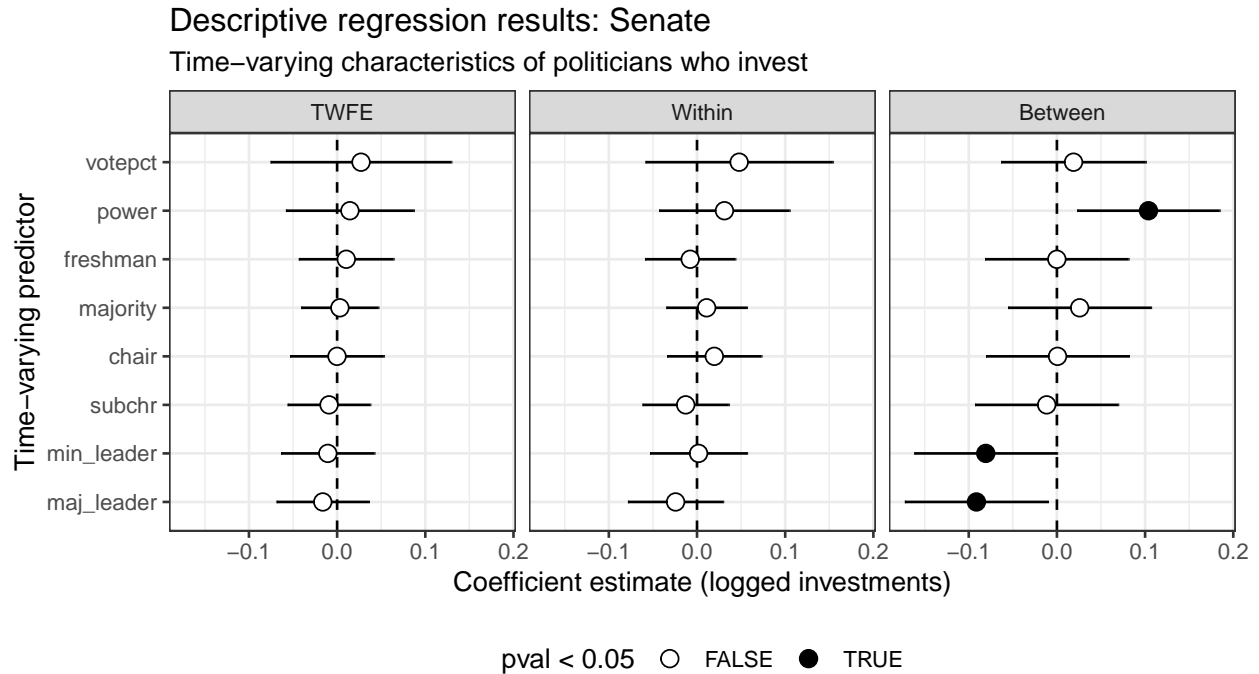


Figure 18: Coefficients from bivariate regressions predicting whether the logged number of investments as a function of time-varying covariates (y-axis), broken out by fixed effects specification (columns). Points colored by statistical significance at the 95% threshold. Senate.

- Only 16.1% of Black politicians invest, compared to 48.4% of whites
- 46.2% of Republicans compared to 40.7% of Democrats
- Generally evidence of more educated being more likely to invest (although potentially inverted U-shaped?)
- Perhaps surprisingly, those with backgrounds in Business or banking are not the most likely to invest. Those would be doctors.
- Between 2012 and 2023, 56.9% of the Senate filed STOCK reports
  - Men: 58.1%, Women: 52.8%
  - There are only 14 non-whites in the data for the senate, to YMMV on how meaningful the proportions are by race.
  - 65% of Republicans compared to 49.4% of Democrats

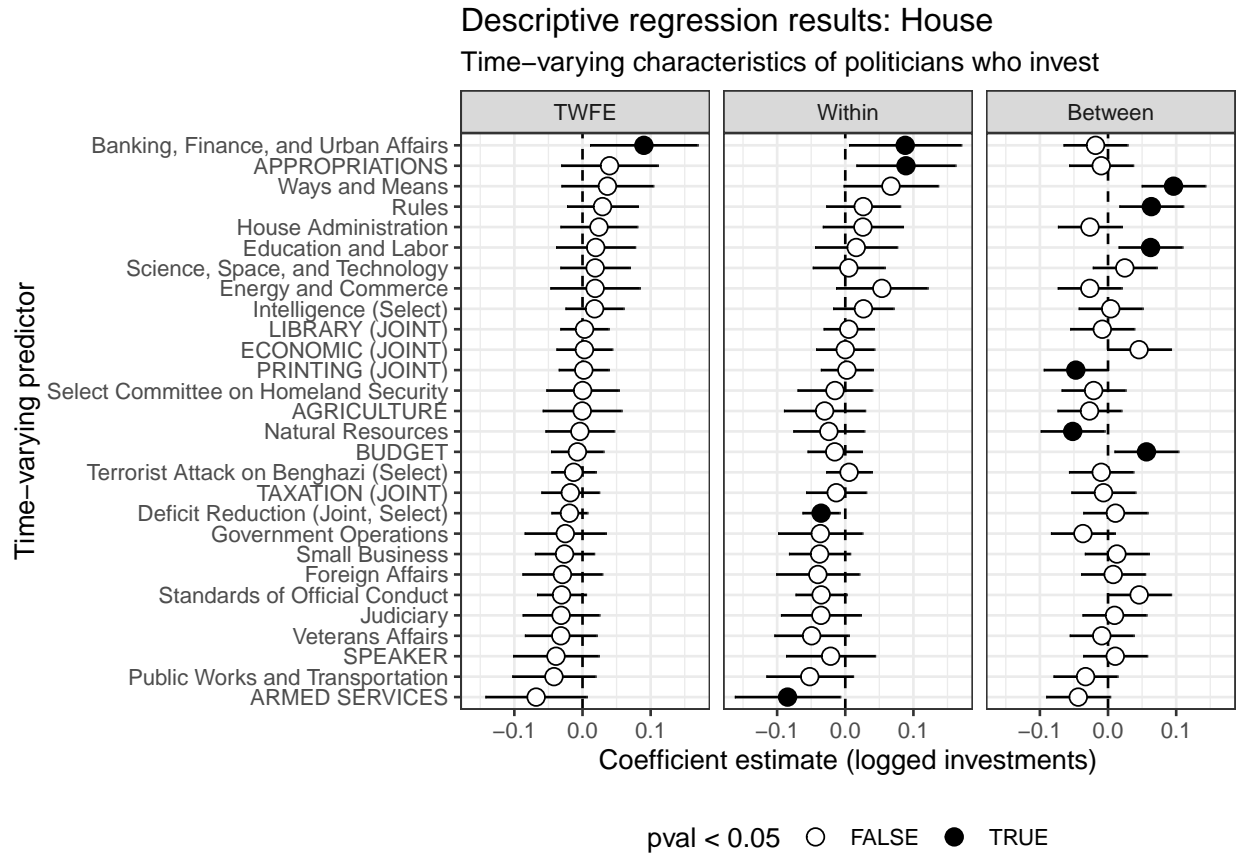


Figure 19: Coefficients from bivariate regressions predicting whether the logged number of investments as a function of time-varying covariates (y-axis), broken out by fixed effects specification (columns). Points colored by statistical significance at the 95% threshold. House of Representatives.

- 63.9% of BAs, 51.6% of MAs, and 56% of Professional degree holders.
- Smaller numbers here as well, but 67.6% of Business or Banking (n = 34), 43.9% of law (n = 57)

**Theory:** More electorally vulnerable politicians will NOT invest due to fear of attack ads from challengers in future elections. Investing is a perk of office but one that is more available to the more electorally secure politicians.

**Implications:** More electorally secure politicians should be more likely to invest in the stock market. Operationalize using previous winning vote margin as a predictor.

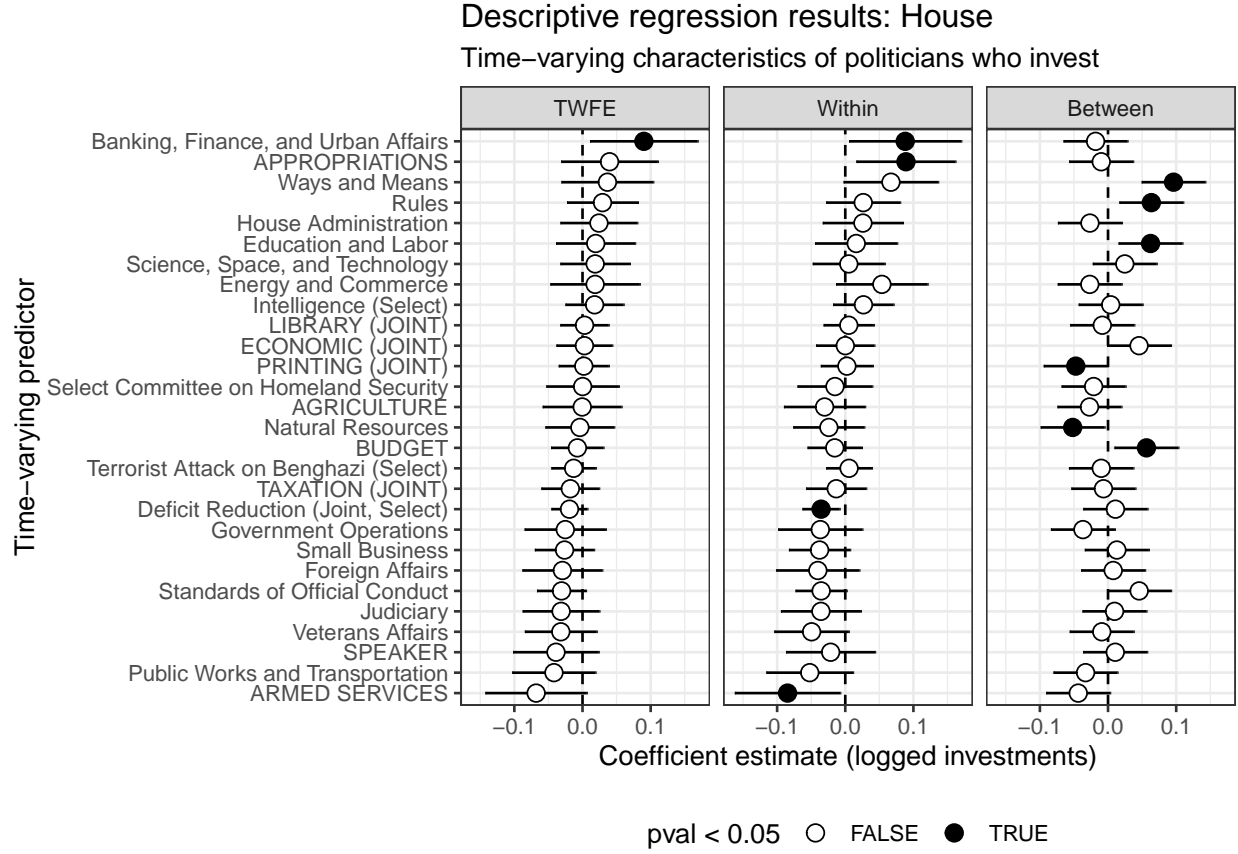


Figure 20: Coefficients from bivariate regressions predicting whether the logged number of investments as a function of time-varying covariates (y-axis), broken out by fixed effects specification (columns). Points colored by statistical significance at the 95% threshold. Senate.

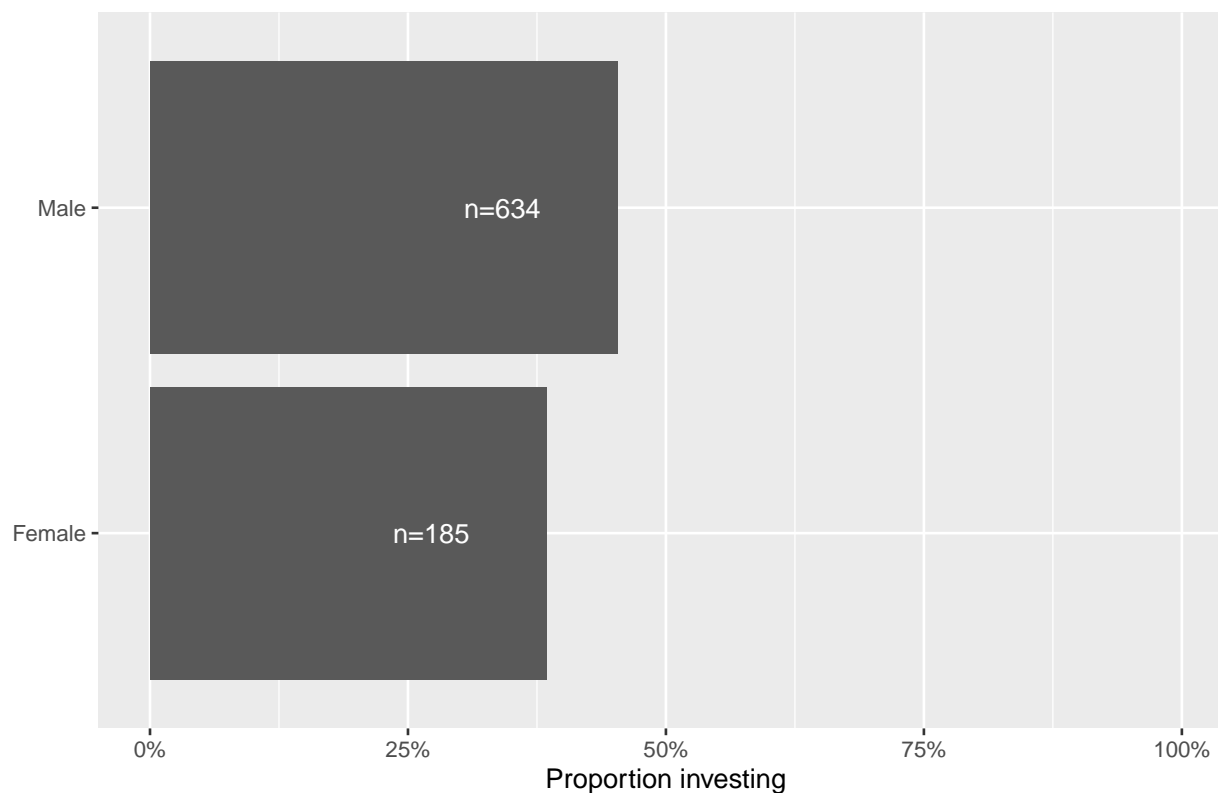
$$invest_{i,t} = \alpha_i + \delta_t + vote_{pct}_{i,t-1} + \varepsilon_{i,t} \quad (1)$$

Basic linear probability model with candidate and year fixed effects. No causal ID here (maybe we could think of scandals or something?) but a robust descriptive exercise. With only politician FEs, modest evidence on one of the measures (logged number of transactions), and rest are in expected direction but small.

Yet no evidence of this mattering with two-way FE (if anything, more electorally secure are LESS likely to invest, but not statistically or substantively significant).

## Proportion investing by gender

House: 2012 – 2023

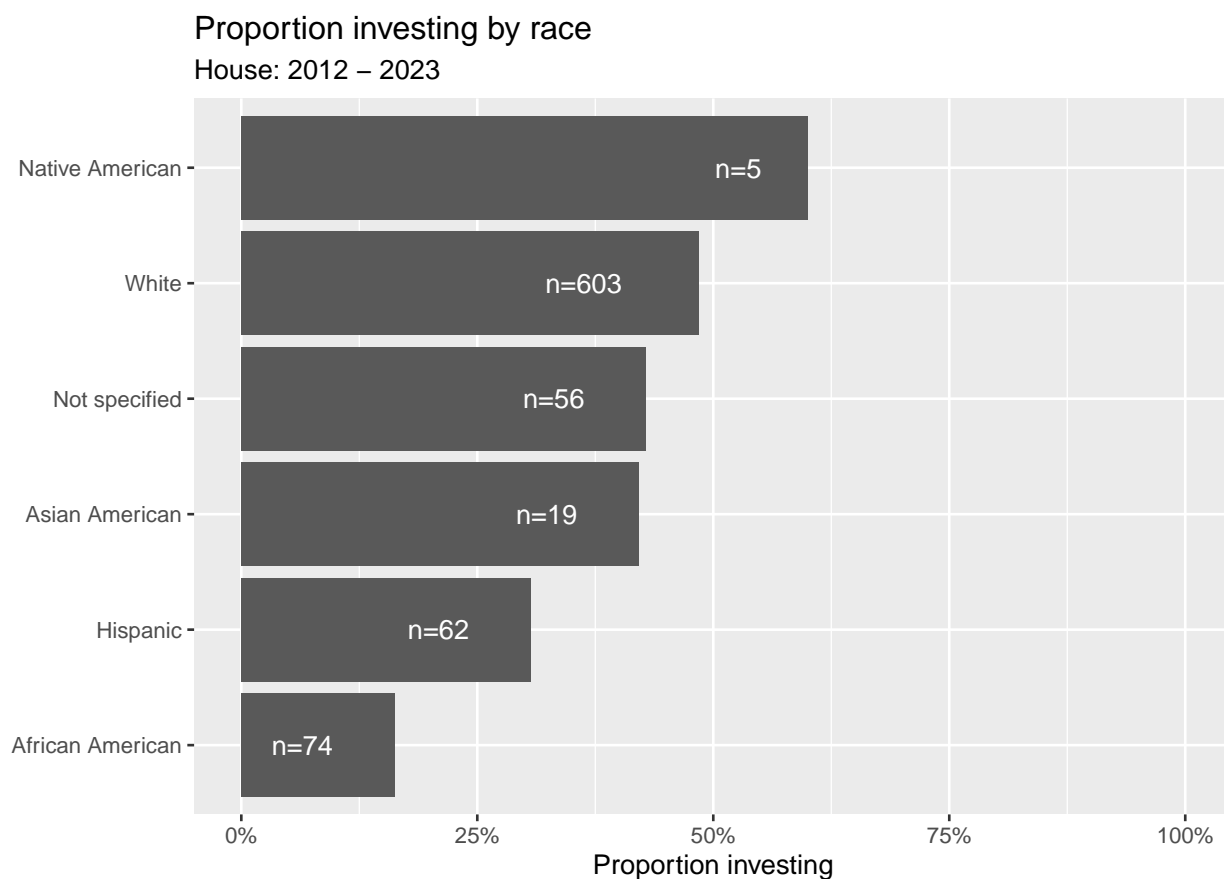


Dependent Variables:	$\log(nTrans+1)$	nTrans	anyTrans
Model:	(1)	(2)	(3)
<i>Variables</i>			
scale(votepct)	0.0596** (0.0290)	1.114 (2.391)	0.0047 (0.0114)
<i>Fixed-effects</i>			
bioguide	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	2,642	2,642	2,642
R <sup>2</sup>	0.84735	0.89369	0.71235
Within R <sup>2</sup>	0.00241	$2.13 \times 10^{-5}$	0.00011

*Clustered (bioguide) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

Table 6: Caption



Dependent Variables:	log(nTrans+1)	nTrans	anyTrans
Model:	(1)	(2)	(3)
<i>Variables</i>			
scale(votepct)	0.0333 (0.0290)	-0.9055 (3.700)	-0.0015 (0.0118)
<i>Fixed-effects</i>			
bioguide	Yes	Yes	Yes
congress	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	2,642	2,642	2,642
R <sup>2</sup>	0.85551	0.89467	0.71879
Within R <sup>2</sup>	0.00076	1.36 × 10 <sup>-5</sup>	1.14 × 10 <sup>-5</sup>

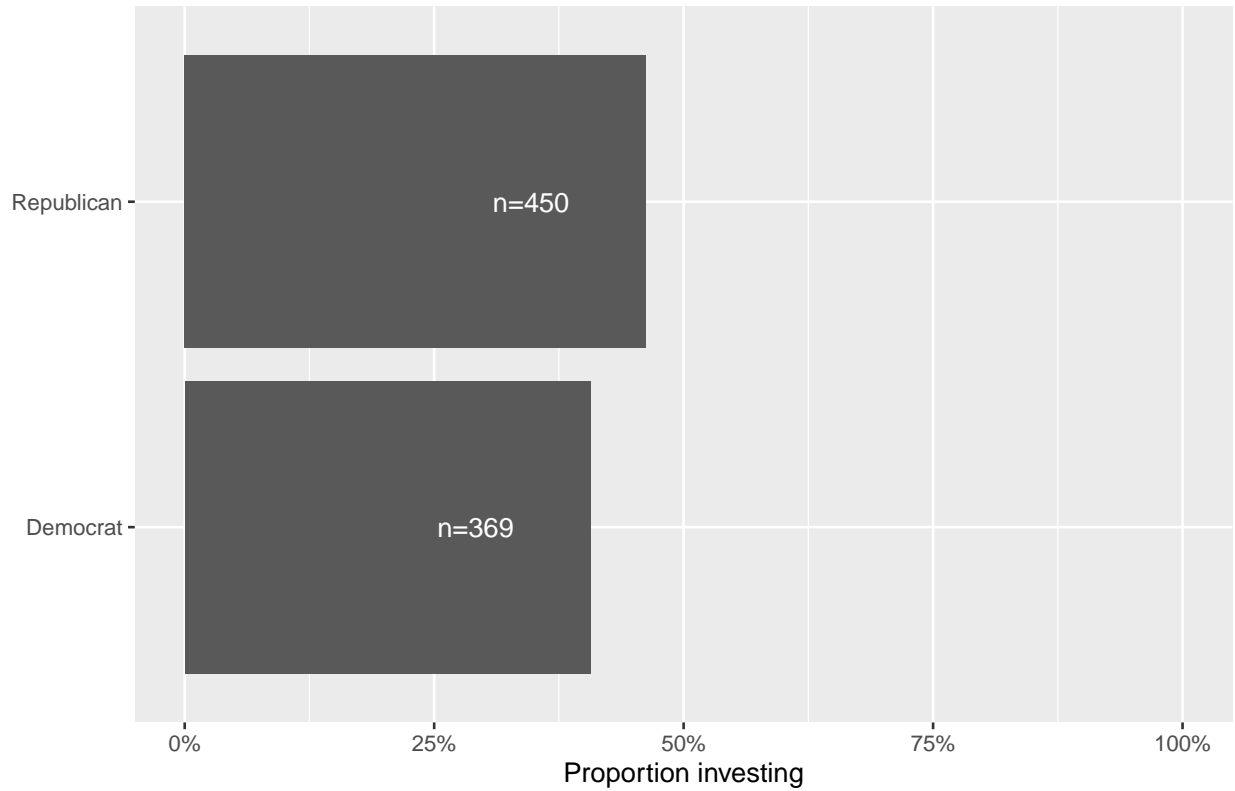
*Clustered (bioguide) standard-errors in parentheses*  
*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

Table 7: Caption

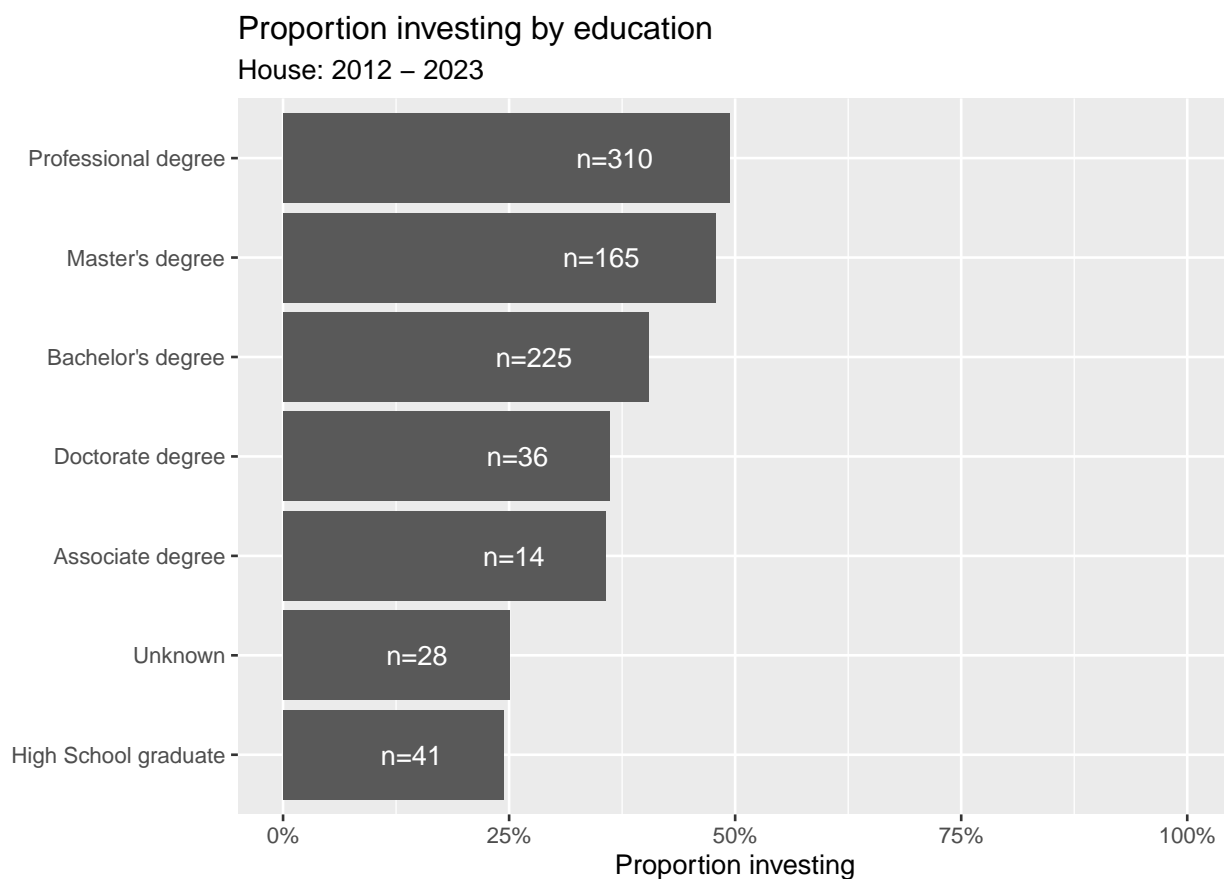


### Proportion investing by party

House: 2012 – 2023



What about (even less identified) between politicians? If anything, the more electorally VULNERABLE are those who are more likely to invest, but we are riding through endogeneity city with the top down here.



Dependent Variables:	log(nTrans+1)	nTrans	anyTrans
Model:	(1)	(2)	(3)
<i>Variables</i>			
scale(votepct)	-0.0819*** (0.0184)	-11.14* (4.892)	-0.0197*** (0.0037)
<i>Fixed-effects</i>			
congress	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	2,642	2,642	2,642
R <sup>2</sup>	0.01466	0.00263	0.01185
Within R <sup>2</sup>	0.00217	0.00070	0.00177

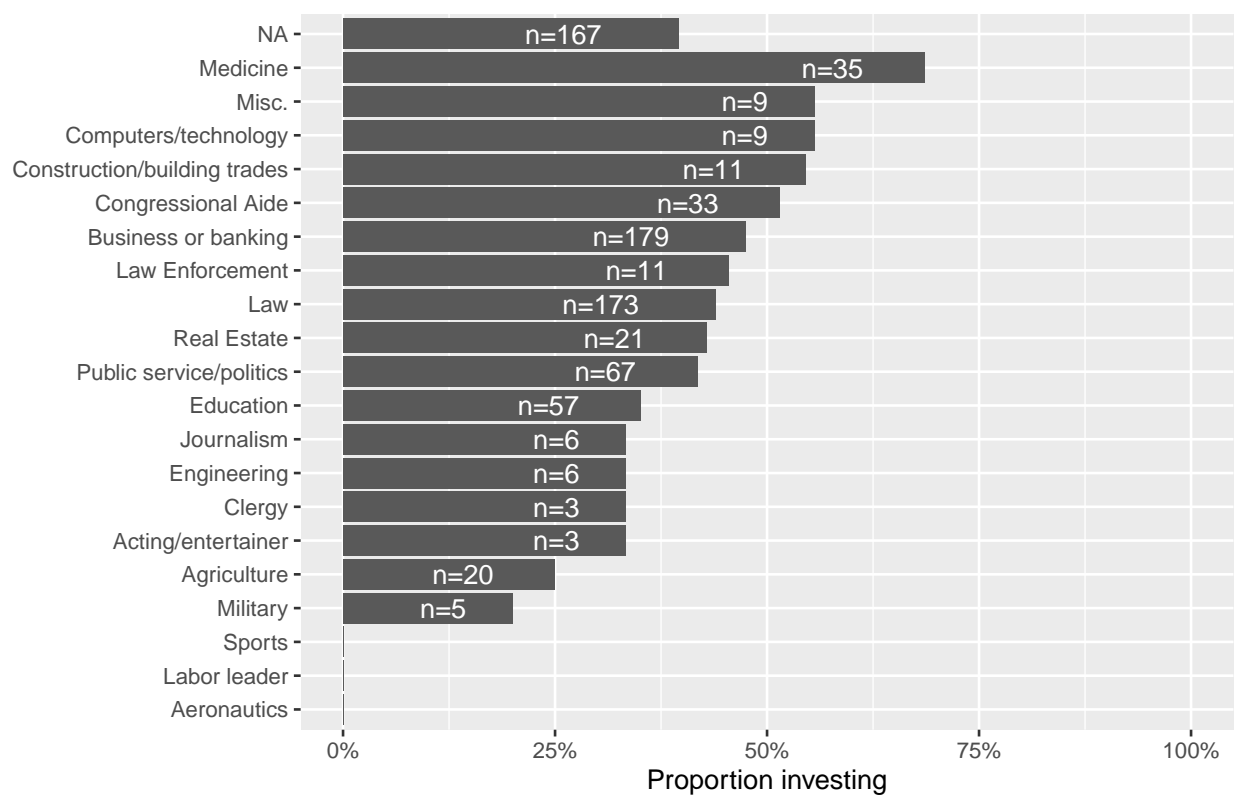
Clustered (congress) standard-errors in parentheses

Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1

Table 8: Caption

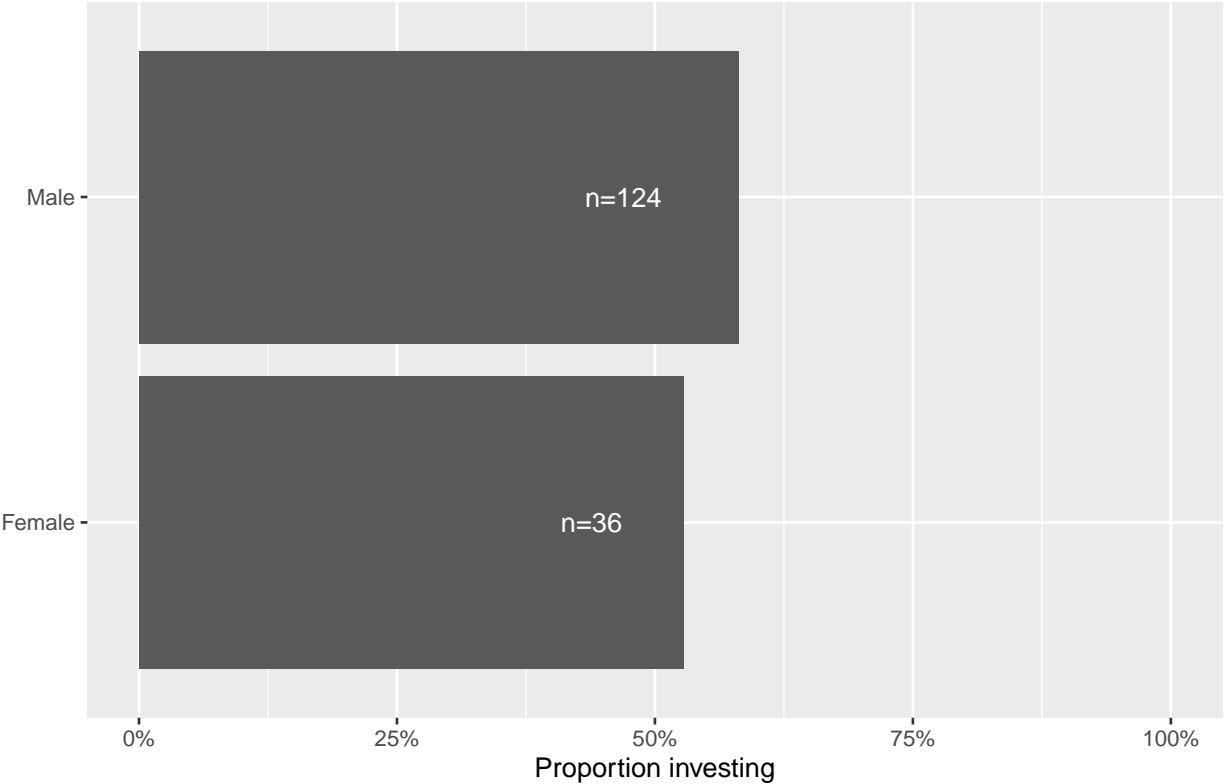
## Proportion investing by occupation

House: 2012 – 2023



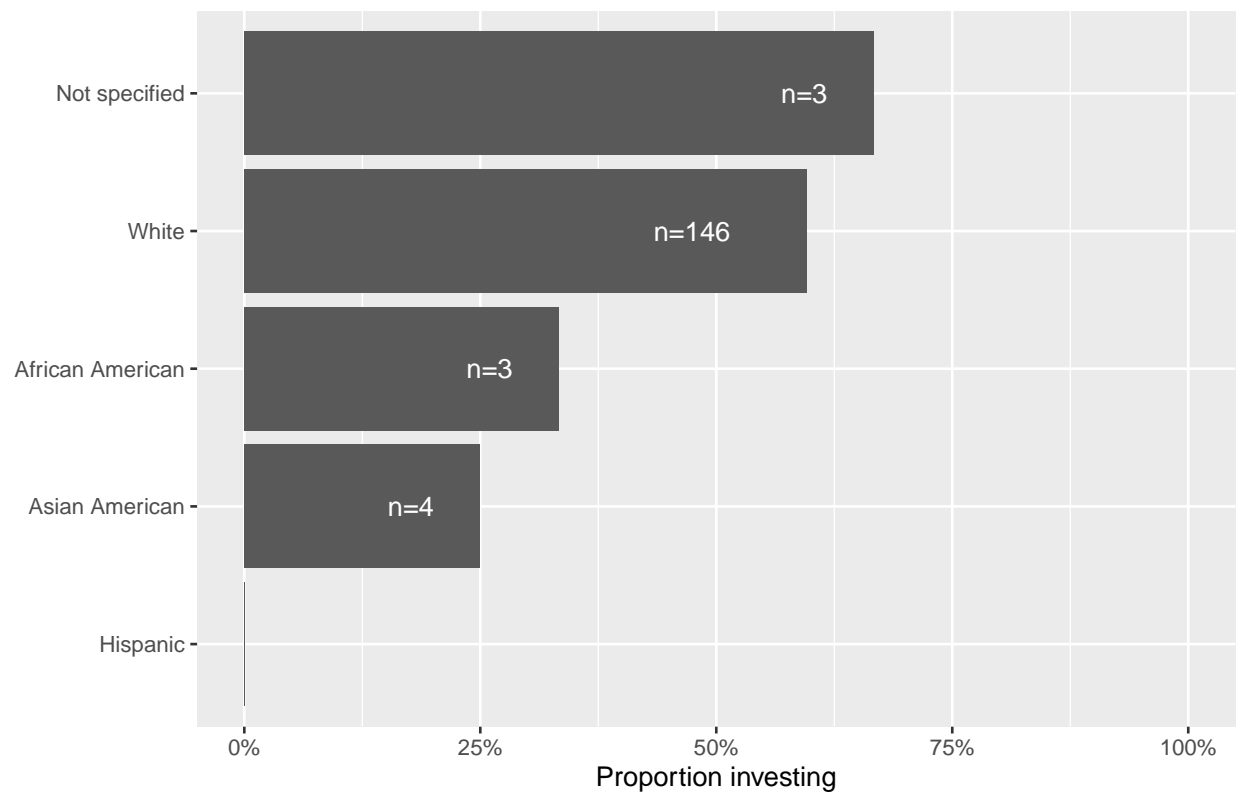
Proportion investing by gender

Senate: 2012 – 2023



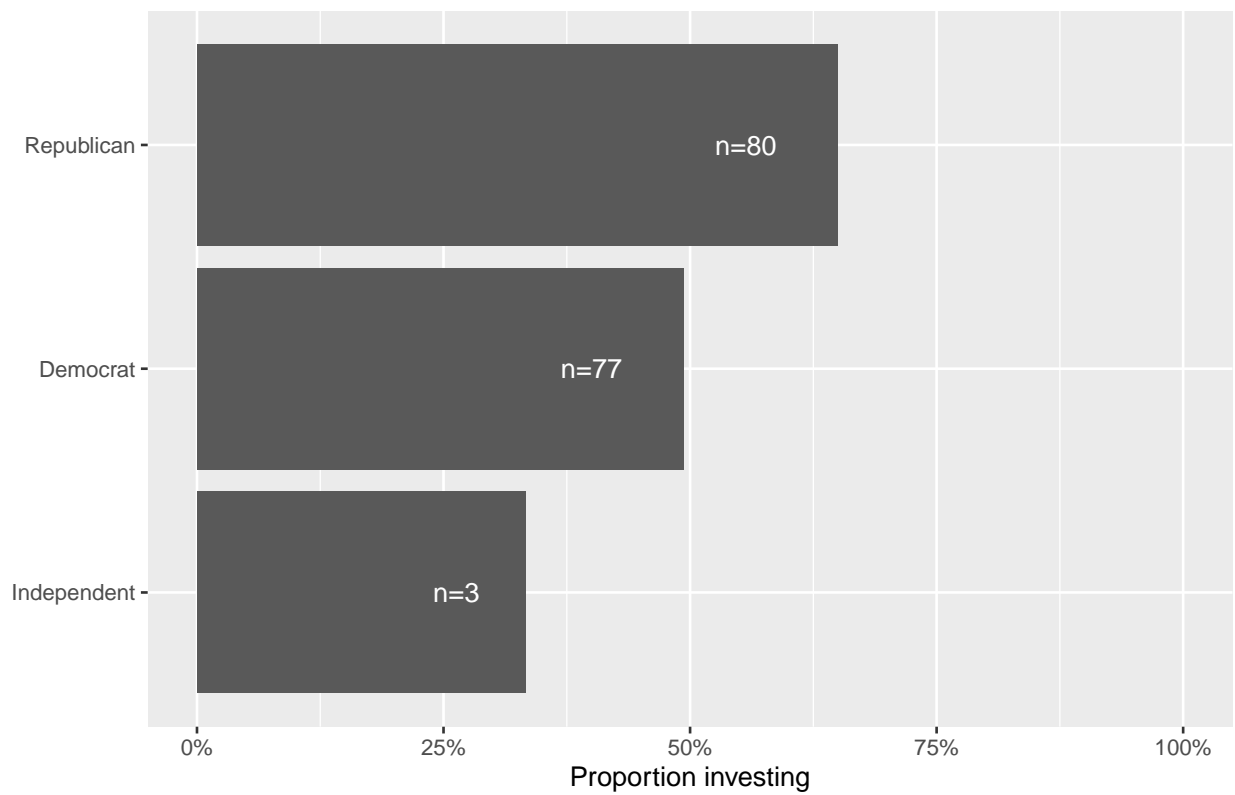
### Proportion investing by race

Senate: 2012 – 2023

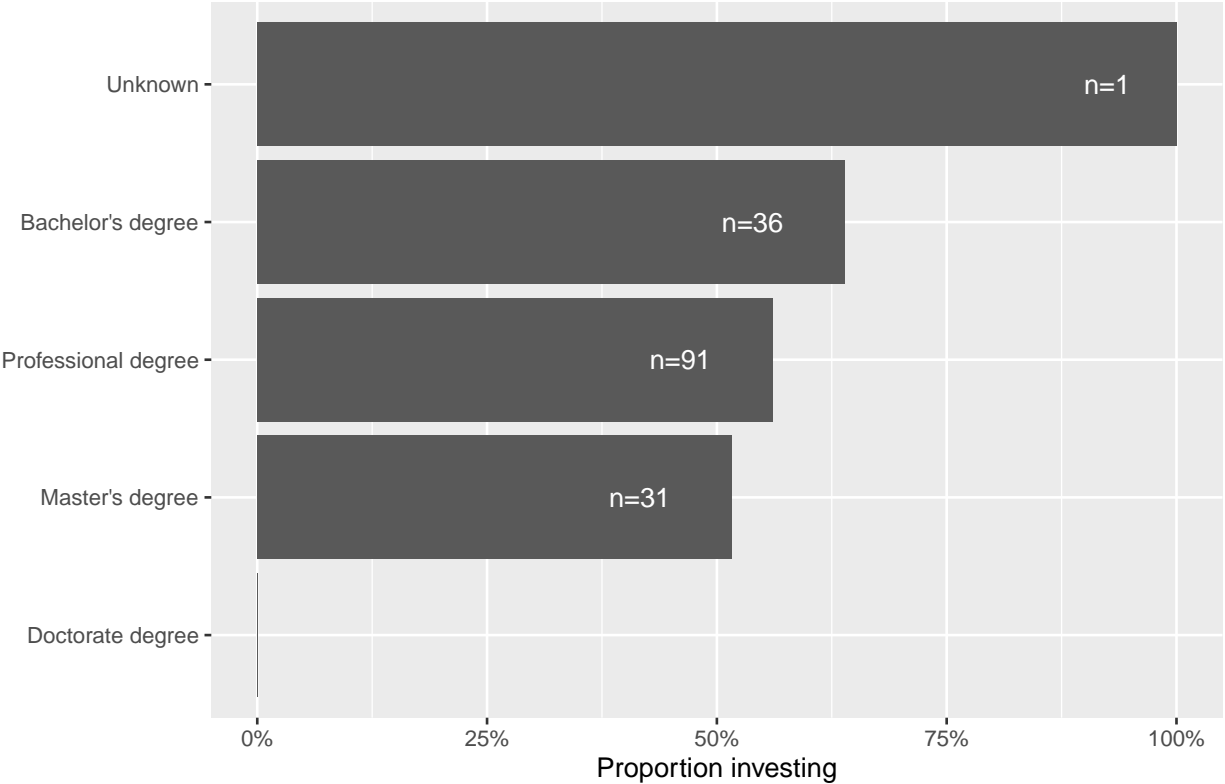


### Proportion investing by party

Senate: 2012 – 2023



Proportion investing by education  
Senate: 2012 – 2023



### Proportion investing by Occupation

Senate: 2012 – 2023

