RETAIL SHAREHOLDER PARTICIPATION IN THE PROXY PROCESS: MONITORING, ENGAGEMENT, AND VOTING

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Abstract

We study retail shareholder voting using a nearly comprehensive sample of U.S. ownership and voting records over the period 2015–2017. Analyzing turnout within a rational choice framework, we find that participation increases with ownership and expected benefits from winning and decreases with higher costs of participation. Even shareholders with negligible likelihood of affecting the outcome have non-zero turnout, consistent with consumption benefits from voting. Conditional on participation, retail shareholders punish the management of poorly performing firms and are more likely to exit the firm after voting against incumbent management. We show that retail voting decisions are impactful, altering proposal outcomes as frequently as those of the "Big Three" institutional investors. Overall, our evidence provides support for the idea that retail shareholders utilize their voting power as a means to monitor firms and communicate with incumbent boards and managements.

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1. Introduction

A central premise of corporate governance research is the shareholder collective action problem, as dispersed individual shareholders may have weak incentives to gather information and monitor the companies they invest in. Research tends to focus on those hired to act on behalf of individual investors: firm management and directors, and, in more recent decades, the institutional investors who make investment choices and vote on behalf of underlying investors. While previous research has produced extensive empirical analysis on institutional investor voting, little is known about turnout and voting by retail shareholders—those who invest for their own accounts—whose preferences, access to information, and incentives to monitor likely differ from those of institutional investors.

Utilizing a sample of U.S. retail shareholder voting data covering virtually all regular and special meetings during the three years 2015 to 2017, we provide the first detailed empirical analysis of retail shareholder turnout and voting decisions. We find that retail domestic shareholder aggregate share ownership is sizable, averaging 26% of shares outstanding, declining from an average of 38% for firms in the smallest size quintile to 16% for firms in the largest size quintile. The number of retail investors, however, strongly increases with firm size, with firms in the largest size quintile held by more than a quarter million retail accounts, on average.

Since institutions who report their votes are effectively mandated to vote, retail shareholders offer a unique opportunity to obtain a more complete picture of shareholder engagement in the proxy process. We analyze the retail turnout decision within a rational choice framework that ties turnout to a voter's probability of being pivotal, her costs of participation, and the benefits of success. Consistent with this framework, we find that turnout increases with stake size and benefits from winning and decreases with costs of voting. Retail shareholders turn out more when a portfolio firm underperforms or for special meetings, which serve as proxies for potential benefits—especially when they own a larger portion of the firm. Holding constant the portions of the firms they own, accounts turn out more for higher-value investments, again consistent with a relationship between benefits from winning and turnout. Higher costs, proxied by restrictions on the shareholder's access to her preferred voting method, result in lower turnout.

Despite the lack of an apparent "civic duty" to vote in shareholder elections, we find non-zero turnout even for a shareholder with a very low stake in a firm and thus a negligible likelihood of being pivotal. Shareholder turnout in corporate elections is positively associated with aggregate turnout in the shareholder's county in political elections, consistent with consumption benefits from voting that cannot be easily explained by variation in financial benefits from voting. We also evaluate information-based and preference-based theories that could potentially explain non-zero turnout and find evidence that turnout increases with proxies for information. Our results suggest that both financial and non-financial motives play a role in retail shareholder turnout.

Conditional on the decision to turn out, we study how public information is incorporated into retail shareholder voting decisions. We find that retail shareholders punish the management of poorly performing firms, as proxied by low valuation, low profitability, and stock price performance. Retail shareholders are more supportive of incumbent management of firms in which they hold larger stakes, suggesting individuals self-select into firms of which they approve of the management teams. This latter evidence is buttressed by our results on exit. We find that retail shareholders are more likely to exit the firm after voting against incumbent management, especially in director elections, evidence which is consistent with the findings by Li, Maug, and Schwartz-Ziv (2019) regarding mutual fund trading subsequent to shareholder meetings, and, recently, the heterogenous preference model in Levit, Malenko, and Maug (2021).

Our data allow us to compare the impact of the retail shareholder vote with that of institutions. We ask how voting outcomes within our sample period would have changed under various counterfactual scenarios for turnout, voting choices, and retail ownership structures. Consistent with the idea that retail shareholders vote differently from other shareholders, when we alter retail shareholder votes in close elections so they vote like other voting blocs, management-sponsored proposals are more likely to fail and shareholder-sponsored proposals are more likely to pass. The frequency of altered voting outcomes due to the modified retail shareholder voting decisions is similar to that when we alter the voting decisions of the "Big Three" institutional investors. We similarly find that shifts in retail ownership result in a substantial change in voting outcomes, again consistent with a persistent difference in voting relative to that of institutional investors.

We use data on institutional shareholder voting to document substantial differences in voting between retail and institutional shareholders on specific proposal categories. Compared to institutional investors, retail shareholders do not support environmental, social, and governance (ESG) proposals to the same degree. Institutions support environmental and social proposals somewhat more often than retail shareholders, potentially consistent with different incentives between investing for one's own account versus the accounts of clients, but institutions also support shareholder governance proposals to a far greater degree than do retail shareholders. The overall retail shareholder support for environmental and social proposals masks substantial heterogeneity across retail shareholders: retail shareholders with large stake sizes support environmental and social proposals less often than institutions, but retail shareholders with small stake sizes support environmental and social proposals more often than institutions.

Retail shareholder voting is correlated with proxy advisor recommendations, implying that there is some information that retail shareholders and proxy advisors both observe and incorporate into their voting decisions. We find, however, that the sensitivity to proxy advisor recommendations is far lower among retail shareholders than institutional investors. This difference in sensitivity between retail and institutional investors does not vary across portfolio size and other observable characteristics: for example, large retail shareholders' sensitivity to proxy advisor recommendations is similar to that of small retail shareholders, not to institutional investors of similar size. To the extent that wealthier accountholders have access to or are willing to pay for more refined information, these results provide evidence inconsistent with different access to information driving the different sensitivity to proxy advisors.

Our results speak to the role of small shareholder voting in monitoring firms. Shareholders' channels of disciplining management are commonly outlined following Hirschman's (1970) classic framework as "voice or exit." Investors can "exit" by selling their shares when they are dissatisfied with management or use "voice" by communicating with the management and the board. The expanding power of institutional investors has placed increased emphasis on the latter mechanism as monitoring by institutional investors became a plausible solution to the collective action problem (Gilson and Kraakman (1991) and Black (1992)). The advent of mandatory voting disclosure by mutual funds in 2003 gave rise to a large literature on institutional investor voting.[1]

[1] Empirical studies of mutual fund voting decisions have used the mandatory vote disclosures to examine how firm and fund characteristics are associated with fund voting decisions, including funds' own governance practices and costs of monitoring (Morgan et al. (2011)), business ties with portfolio firms (Davis and Kim (2007)), other cross-holdings (Matvos and Ostrovsky (2008)), peer effects (Matvos and Ostrovsky (2010)), tax-driven incentives (Dimmock et al. (2018)), investment horizons (Duan and Jiao (2016)), and proxy advisor recommendations (Iliev and Lowry (2015) and Malenko and Shen (2016)). Bubb and Catan (2020) and Bolton et al. (2020) expand on this work by breaking down the party structure of different mutual funds. A more recent theoretical literature extends some of the insights from work on turnout in political science to study shareholder elections. Zachariadis et al. (2020) study the relationship between preferences and turnout by shareholders with discretionary participation such as retail

In contrast to the literature on mutual fund voting, little is known about retail shareholder voting. Van der Elst (2011) studies the turnout of small shareholders – those who own less than 5% of voting rights – in several European countries and documents a relationship between turnout and ownership structure. Schmidt (2017) surveys retail shareholders at a single publicly listed German firm, finding that participation increases with investment experience, age, and financial sophistication. In the U.S., Kastiel and Nili (2016) show that overall investor turnout has remained roughly constant over the past two decades even though this period saw a number of technological, regulatory, and corporate governance changes meant to strengthen retail shareholder participation. They also find that when brokers are restricted from voting on behalf of beneficial owners who do not cast a ballot, the overall non-voting rate increases by ten percentage points. The historical perception is that when they do vote, retail investors vote with management (Stewart (2012), Chasan (2013)), while work by Maug (1999) and Edelman, Jiang, and Thomas (2019) assumes that shareholders with small stakes vote randomly. However, no academic work has directly tested these assumptions.

Several papers, including Kastiel and Nili (2016) and Gulinello (2010), have pushed for changes to promote greater participation among retail shareholders, and the SEC has made increased retail participation a regulatory priority.[2] Others, such as Hart and Zingales (2017), have argued for shareholder preferences as the ultimate objective function of firms. As Fisch (2017) has argued, retail shareholders have "skin in the game" and will select to monitor and engage only if they are adequately informed, whereas institutional votes are cast by intermediaries. Our study

shareholders. Bar-Isaac and Shapiro (2020) examine the participation decision by a large blockholder at a firm with many small shareholders with private signals.

[2] In 2015, the SEC held a roundtable on methods to increase retail voting participation and in 2019, the SEC's Investor Advisory Committee recommended changes to the proxy system in part to increase retail voting participation.

adds to these efforts by studying the decisions of direct owners to participate and by comparing their voting with that of institutional investors.

The evidence provided in this paper is also relevant to the renewed focus on the efficacy of monitoring and stewardship by large institutional investors (Coates (2018)). As Gilson and Gordon (2013) trace, a growing movement towards diversification and changing regulations regarding retirement savings in the latter half of the 20th century have shifted savings away from individual stock ownership towards concentrated institutional ownership. This concentrated power has drawn attention to the incentives faced by fund advisors and whether they allocate adequate resources towards monitoring of portfolio firms (Kahan and Rock (2019), Lewellen and Lewellen (2018), Fisch, Hamdani, and Davidoff Solomon (2019), Bebchuk and Hirst (2019)). Some, including Lund (2018) and Griffith (2020), have proposed reducing or eliminating the power of institutional intermediaries to vote. Given retail shareholders' significant ownership in public firms, our study provides an indication of what voting may look like if these shareholders were given more power.

The paper is organized as follows. In Section 2, we provide the hypothesis development. Section 3 provides institutional background on the proxy voting process and how shares are owned and voted. Section 4 describes the retail shareholder voting data and descriptive statistics on ownership, turnout, and voting. Section 5 presents evidence pointing to the impact of retail investor participation. Section 6 provides empirical results on the retail decision to turn out. Section 7 provides evidence on the factors associated with retail support for management and shareholder proposals. Section 8 offers some concluding remarks.

2. Theory and hypothesis development

To set the stage for the empirical analyses, we present the canonical political science framework of the decision to vote in political elections and an overview of the literature on voter turnout. To date, it has been challenging to test models of participation in corporate elections with data on investment advisor voting decisions since these institutions are effectively required to vote (Bar-Isaac and Shapiro (2020)). We study shareholder participation decisions directly, utilizing insights from the political science and political economy literatures.

2.1. General utility framework for participation

The rational choice framework of Riker and Ordeshook (1968) sets the utility from a voter's participation, U, as,

$$U = P * B - C + D \tag{1}$$

B captures the difference to the voter, measured in utility, between her more favored proposal outcome succeeding relative to it failing (her "benefit"). P is the probability that her vote would change the outcome from her disfavored choice to her favored choice—that is, the probability that she is pivotal. C is her cost of voting, and D is any consumption benefit from voting. She would vote if and only if $U > 0$.

The "paradox of voting" is that—assuming $D = 0$ and given that the likelihood of a voter's pivotality, P, is negligible in most elections—even with very small costs of voting, C, the benefit to a voter of winning, B, would have to be unrealistically high to induce voting (Downs (1957)). Feddersen (2004) notes that in response to the paradox of voting, theorists have generally assumed that the decision to vote is non-strategic or independent of other strategic choices. However, there is extensive empirical research in the political science literature showing that the individual components of Eq. (1) correlate with voter participation, even if it is not obvious how benefits of voting outweigh costs for any voter (Geys (2006), Blais (2006), Smets and van Ham (2013), and Cancela and Geys (2016)).[3]

We build on insights from the theory of rational voter participation, as well as more recent extensions to the corporate voting setting, to guide our analysis of retail shareholder participation. Denoting the account's ownership share of the firm as α, the likelihood of pivotality, P, should be increasing in α. B can be expressed as the sum of (i) the financial benefits from winning, which is the utility from $\alpha \times b_f$, in which b_f is the dollar benefits to the firm from winning, and (ii) any social benefit from winning, if the shareholder places positive weight on social benefits. Since we observe large variation in α both across shareholders in a given firm and across portfolio firms for a given shareholder, shareholder elections provide a unique setting for testing the basic structure of the framework in Eq. (1). In particular, we explore the relationship between turnout and proxies for pivotality, costs and benefits from participation, and the interaction among these variables. Our aim is to assess whether the turnout decision is driven by these fundamental costs and benefits, including whether Eq. (1) captures the basic structure of turnout.[4] We also present three broad categories of models of voter participation that attempt to explain non-zero turnout: (i) models that are based on differences in information, (ii) models based on differences in voter preferences, and (iii) models in which voters derive consumption utility from voting, *D*.

2.1.1. Information-based models

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[3] Voter participation has been linked with variation in proxies for the pivot probability, P, (Geys (2006), Blais and Dobrzynska (1998), Oliver (2000), Cox and Munger (1989), and Agranov et al. (2018)). This literature also finds that differences in participation are linked to benefits from participation, B (Hogan (1999), Patterson and Caldeira (1983), Aker, Collier, and Vicente (2017), and Filer and Kenny (1980)). Turnout has also been shown to vary systematically with costs to voting, C (Hill and Leighley (1993), Wolfinger, Highton, Mullin (2005), Walker, Herron, and Smith (2019), and Kirchgaessner and Schulz (2005)).

[4] The structure in Eq. (1) yields no clear predictions regarding the shape of turnout with respect to α and how turnout varies with the interaction of α and variables proxying for the increase in firm value, b_f. These depend on how pivotality, P, varies with α, and how monetary gain from winning translates into utility.

Information-based models tend to assume that voting is costless and then try to explain the "paradox of not voting": why any voter fails to turn out despite costless voting (Feddersen and Pesendorfer (1996)). Matsusaka (1995) proposes a rational-choice information-based model with a consumption benefit from voting, D, and finds that voters with less information or ability to evaluate the candidates choose to abstain to avoid voting for the incorrect candidate. Feddersen and Pesendorfer (1996) show that even when voting is costless, uninformed voters strategically abstain to allow informed voters to decide the outcome. More recently, Bar-Isaac and Shapiro (2020) extend this intuition to a corporate setting by introducing a large blockholder. The blockholder differs in the amount of information she has, and they show that if she has imprecise information she may want to only partially vote her shares so as to avoid "drowning out the information by other shareholders." One central insight from these models is that uninformed shareholders are more likely to abstain under the assumption that their interests are aligned with informed shareholders. A commonality among these theories is that information is positively associated with turnout, which we test with proxies for information.

2.1.2. Preference-based models

A second group of models, preference-based game-theoretic models with no role for private information, tend to predict positive turnout even with costly voting. Palfrey and Rosenthal (1983) study a voting game in which voting is costly and every voter has complete information about the preferences and voting costs of every other voter. They show that it is possible to arrive at equilibria with significant turnout, independent of electorate size. However, Palfrey and Rosenthal (1985) introduce uncertainty about preferences and costs of participation, which causes voters with positive costs of participation to abstain, leading to near-zero turnout. More recently, Myatt (2015) studies costly voting when there is aggregate uncertainty about the popularities of the candidates. In this setting, when voters are unsure which candidate is more popular, turnout is substantial and increases with the importance of the election and with the precision of voters' beliefs about the candidates' popularities. Importantly, voters' asymmetric prior beliefs result in an "underdog effect" where the perceived leading candidate sees greater turnout for her competitor. This effect increases the likelihood that the election will be a close race, resulting in a higher turnout and the possibility that the more popular candidate loses.

Zachariadis, Cvijanovic, and Groen-Xu (2020) build on Myatt (2015) to study how shareholder participation and voting outcomes depend on a firm's ownership structure. They recognize that some shareholders, such as mutual funds, are mandated to vote, whereas the rest, denoted discretionary shareholders, select whether to cast a vote. Aggregate uncertainty over the discretionary shareholders' preferences leads to significant participation. Discretionary voters' participation choice depends on the ownership fraction of regular voters and their known support for the proposal, the discretionary voters' benefit-to-cost ratio associated with voting, and the uncertainty of their preferences. As in Myatt (2015), they find that in equilibrium there is an "underdog effect" in that disagreeing discretionary voters turn out more with weaker support by regular shareholders. Because we observe the turnout decisions of discretionary voters, our setting allows us to test some of the model's predictions. In particular, we use proxies for the preferences of retail shareholders and regular voters to evaluate whether retail shareholders turn out more when they have underdog preferences.

2.1.3. Models based on altruistic motives

Models in this category explain positive turnout by introducing altruistic motives or consumption benefits from voting. As emphasized by Riker and Ordeshook (1968), voters may experience utility benefits, such as satisfaction from compliance with the ethic of voting, satisfaction from affirming allegiance to the political system, satisfaction from deciding, and satisfaction from the act of informing oneself for the decision, all of which may generate high turnout in political elections despite the low probability of being pivotal. Feddersen and Sandroni (2006) present a model in which no voter is pivotal but voter preferences extend beyond which candidate wins to include the social cost of the election. Models with consumption benefits are buttressed by lab experiments, which tend to show utility from voting itself (e.g., Fowler (2006)).

Our setting provides a useful contrast to political elections since it is unclear whether motivations relating to civic duty and altruism play a role in participation in financial markets. We explore whether consumption benefits appear in shareholder voting by examining whether there is positive turnout among shareholders in settings where their chance of being pivotal is negligible.

2.2. Shareholder voting decisions

In this subsection, we discuss predictions for how retail shareholders make their voting choices conditional on the decision to participate. Although there is an extensive empirical literature on mutual fund vote choices, the theoretical literature provides relatively little guidance on how individual investors should make vote choices.[5]

We ask first whether the retail shareholder vote differs from the institutional vote. Retail and institutional voting choices may differ for several reasons. Retail shareholders invest for their own accounts, whereas mutual fund managers generally manage the investments of others. Retail shareholders may therefore place a greater weight on portfolio value maximization than social

[5] A notable exception is Maug and Rydqvist (2009), who study sincere and strategic shareholder voting on management proposals. Consistent with their strategic voting model, they find that while pass rates are independent of majority requirements, shareholders adjust their behavior in response to higher majority rules by voting for proposals more often. More recently, Levit, Malenko, and Maug (2020) study how shareholder voting and trading decisions are determined in a setting in which shareholders differ due to heterogeneity in their preferences. Since we do not observe retail shareholder trading decisions, we cannot study their predictions linking the changing shareholder base to voting outcomes, shareholder value, and welfare changes.

surplus as compared to institutions. We also explore whether differences in voting are attributable to the greater size and diversification of funds that may capture differences in access and interpretation of information about the firm and the proposal on the ballot.

The influence of proxy advisor recommendations merits special mention. There is extensive research focusing on the influence of proxy advisors on mutual fund votes, particularly focused on the extent to which they causally influence fund decisions or instead serve as a reflection of client preferences. To the extent that retail shareholders do not have access to ISS recommendations, their voting choices provide a unique opportunity to compare the extent to which ISS recommendations comove with retail shareholder voting and institutional shareholder voting. If retail voting decisions comove with ISS recommendations to the same degree as that of institutions, this would be consistent with the idea that proxy advisors serve as a means to aggregate institutional investor preferences that are shared by retail investors. The extent to which the retail response to ISS recommendations differs from that of institutions may, however, indicate the degree to which ISS recommendations aggregate sources of information not available to retail shareholders or reflect preferences that differ from retail preferences.

Finally, we also study the relationship between voting and the decision to exit a firm by selling. As discussed in the introduction, shareholders have long been viewed as having a choice between exit and voice. Li, Maug, and Schwartz-Ziv (2019) study mutual fund trading subsequent to shareholder meetings and find that mutual funds reduce their holdings if the outcome of the election is different from the votes they cast. They argue that models in which shareholders hold differences of opinion predict a relationship between voting choices and exit such that disagreeing shareholders exit the firm. We similarly ask whether exit decisions by retail shareholders are related to disagreements with management.

3. The proxy voting process

This section provides a summary of the proxy voting process, focusing on how share ownership is structured and how shares are voted.[6] As shareholders typically do not attend shareholder meetings in person, voting occurs mostly through proxies that are solicited before the meeting. This process of proxy solicitation differs depending upon whether the shares are owned by registered owners or by beneficial owners. A *registered owner* holds securities in certificated form or in electronic form ("book-entry") through a direct registration system, which allows an investor to have her ownership of securities recorded by the issuer without having a physical certificate issued. Registered owners are often an issuer's management, directors, employees, or pension fund (Daly (2017), Racanelli (2018)). A *beneficial owner* (or "street name" owner) holds shares in a custodial account with an intermediary or custodian. The beneficial owner is considered the holder of a "securities entitlement in a financial asset," meaning she has a pro rata interest in all like securities of the intermediary held in common by all other customers who own the same security. Most shares are now held as beneficial shares—75% to 80% of all public issuers' shares, according to one estimate (Racanelli (2018)). Online Appendix C provides in detail the process by which registered and beneficial shares are matched to their owners and sent proxy materials.

Retail investors typically manage their stockholdings through a broker. Brokers generally maintain proprietary online platforms that allow their investors to log in, view information about their accounts, and execute trades. Other platforms provide retail investors with information on how to vote their shares, but brokers are not required to connect these platforms directly to the retail investors' brokerage accounts. As a result, investors on these platforms must navigate to a

[6] The material in this section and in Online Appendix C draws upon the Securities and Exchange Commission, Concept Release on the U.S. Proxy System (2010), Kahan and Rock (2008), and Fisch (2017).

different website run by a proxy services provider to submit voting instructions to their broker. For example, ProxyVote.com, run by Broadridge Financial Solutions, is an online platform that enables shareholders to attend shareholder meetings virtually. Before each shareholder meeting that the investor is eligible to attend, ProxyVote sends an email with instructions on the process by which the investor can view proxy materials and vote. Shareholders may cast their votes online, through mail-in ballots prior to the meetings, or by telephone (voice response system).

As emphasized by Fisch (2017), unlike institutional investors, retail investors cannot provide customized voting guidelines to their broker and thus they must indicate a voting decision for each individual item on the proxy. If they fail to submit their votes to their broker, then their shares are categorized as broker nonvotes. For "routine" matters, the broker may determine whether and how votes should be cast, where "routine" is determined by New York Stock Exchange Rule 452 and approved by the SEC. Broker voting is subject to the rules of the exchange of which the broker is a member, not the listing exchange of the firm, and nearly all brokers are subject to NYSE regulations (Hirst (2017)). Routine proposals are generally considered to consist of auditor ratifications and proposals to adjourn the meeting, and explicitly exclude director elections and proposals affecting shareholder rights.

There has recently been a push to increase retail investors' participation in proxy voting, especially through the use of digital platforms. The SEC has attempted to further facilitate the increased use of electronic forums through its rulemaking, most notably by adopting the Notice and Access system, described in greater detail below, to encourage the use of electronic platforms (Securities and Exchange Commission (2010)). On its investor site, www.investor.gov, the SEC provides educational materials about the proxy voting process for the average retail investor, and a number of issuers and shareholder organizations also provide links to this information.

4. Data and descriptive statistics

4.1. Retail shareholder voting data

U.S. retail shareholders, whether registered or beneficial, do not publicly report their shareholdings or votes, making it challenging to conduct empirical research on their voting decisions. In this study, we utilize a novel dataset of retail shareholder votes provided to us under a confidentiality agreement with Broadridge Financial Solutions, Inc. The data contain all annual and special meetings over the three-year period from 2015 to 2017 for firms for which it serves as the service provider, constituting 17,937 meetings for 6,782 firms.

For each meeting, the dataset contains the voting records, including failures to vote, for each retail shareholder account that is a beneficial owner with voting rights in the firm as of the record date of the meeting. The dataset defines an account as "retail" if the account does not use Broadridge's online proxy voting product for institutional investors and financial advisors, ProxyEdge, or does not come from third-party vote agents via Broadridge's Consolidated Data Feed. The data we received include single-client family investment offices, which we include in our definition of retail.[7] Non-U.S. shareholder accounts are aggregated into a single observation for each meeting, allowing us to observe only the aggregate number of non-U.S. retail shareholder votes cast, so we remove them for all analyses. All data provided to us by Broadridge were first anonymized by Broadridge so that individual investor accounts are unidentifiable. Broadridge

[7] In practice, there may be a handful of small hedge funds and multi-client family investment offices included in the data. Internal Broadridge research has found this to be a trivial number of non-retail participants; in any event, with forty-six million accounts in our data, it is implausible that any non-retail shareholders in the data could make a substantial difference, and our results are robust to removing large accounts. To assess the impact of large accounts on our analyses, we reproduce two of our key results on turnout and voting excluding all account-years with portfolios greater than one million dollars. The results, reported in Online Appendix Table A17, are virtually identical to the results in the main paper.

assigns a unique code, the key to which Broadridge retained, so voting can be tracked across firms and over time without revealing any data on account numbers, names, or street addresses.

A retail investor account is associated with its broker through an anonymized broker ID, the key to which Broadridge has retained. Thus, if an individual holds an individual account with a broker, a joint account with her spouse with that same broker, and an individual account with a different broker, we observe these as three separate accounts. To further protect shareholder identity, Broadridge excludes data whenever there is only one shareholder in a zip code.

Each account-meeting level observation includes the number of shares in the firm held by the individual as of the record date of the meeting and the shareholder's zip code. For each proposal on the meeting agenda, we observe whether the shareholder voted and, conditional on voting, her voting decision, as well as the management recommendation. Proposal text and firm CUSIP are included in a second dataset separate from the retail voting data, requiring a merge of the two datasets. In total, the data contain approximately 461 million account-meeting level observations from 46 million accounts, 7.0 million of which vote at least once in the three-year period.

4.2. Non-proprietary data

We use several public sources of data. We obtain proposal-level data from the ISS Voting Analytics database including, for each proposal, its description, sponsor, total voting results, and the recommendation on the proposal by ISS, the most influential proxy advisor. We further retrieve additional proposal-level data from SharkRepellent, which duplicates some ISS data and allows for error correction. Online Appendix Table A1 provides a categorization of the proposals into a set of 12 categories and Online Appendix Table A2 details the number of proposals by type included in the retail voting data. The number of proposals increases from 16,583 in 2015 to nearly

20,000 in 2017, including, each year, roughly 500 shareholder-sponsored proposals, of which roughly 200 per year are environmental or social proposals.

Votes by mutual funds and other registered management investment companies, including the Big Three institutional investors, BlackRock, Vanguard, and State Street, come from ISS Voting Analytics, which we match to institutional ownership data from Thomson Reuters and the Center for Research in Security Prices (CRSP) Mutual Funds dataset. We gather information on ownership of brokerage accounts from the Federal Reserve Board's 2016 Survey of Consumer Finances, a nationally representative cross-sectional survey of U.S. households. Families participating in the SCF respond to questions on financial and nonfinancial assets, debt, employment, income, and household demographics, providing the most comprehensive and highest quality microdata on U.S. household wealth (Bricker, Henriques, and Moore (2017)).

We obtain county vote totals for the 2016 presidential election from CQ Voting and Elections. From the Census Bureau, we obtain the voting-eligible population and zip code-level demographic information. Zip code employment comes from the Bureau of Labor Statistics and zip code-level adjusted gross income data is from the IRS website. For securities data, we use data from CRSP to calculate firms' lagged annual abnormal return and dividend yield. We use accounting data from Compustat to calculate Tobin's q and return on assets (ROA). Appendix A.1 provides information on the sources and construction of these variables.

The merging process between the Broadridge data and other datasets, which involves proposal-by-proposal matching with ISS Voting Analytics, is extensive and is detailed in Appendix A.2. In Appendix A.3, we further discuss the process of cleaning errors in the proposal-level ISS Voting Analytics dataset. Appendix A.4 provides a description of the construction and merging of mutual fund voting data from Form N-PX via ISS Voting Analytics.

4.3. Descriptive statistics

We now turn to describing retail shareholder characteristics, the characteristics of retail ownership at the firm level, and retail voting. Online Appendix D provides a detailed example of a single meeting at an anonymized major U.S. firm to provide an initial impression of the scope of the retail voting data.

4.3.1. Retail shareholder characteristics

Table 1, Panel A provides a description of the retail shareholder accounts in the sample. For each account-year, we add up the reported equity stakes on record dates to produce an account-year-level snapshot of portfolio holdings. We also use account zip codes to merge in zip code-level IRS income data. Accounts hold an average (median) of roughly four (two) securities, similar to the evidence in Barber and Odean (2000). Panel A shows a large spread between the median account (roughly $13,000 in value) and the average account (roughly $130,000), reflecting a strong right skew in the distribution of the account values. The average account dividend yield is 2%. We calculate yearly market abnormal return for an account as the equal weight buy-and-hold return on its securities, assuming the account held all securities for the past year, minus the CRSP value-weighted index return. The abnormal return of accounts in the sample averages to near zero in the aggregate. Finally, the accounts derive from zip codes with substantially higher income than the average zip code income of $61,000 in our U.S. Census dataset.

We sort accounts into account value quintiles in Table 1, Panel B. Accounts in the lowest portfolio value quintile average $588 and hold fewer than two securities, whereas accounts in the top quintile average close to $650,000 and hold nine securities, on average. The market-adjusted abnormal return increases across account value quintiles, though dividend yield is constant at roughly two percent. Next, although we do not observe the entire trading records of these accounts, we proxy for how frequently accounts are turning over their assets based on the rate at which accounts invest and divest in portfolio firms. An account's firm purchase rate is the proportion of firms it currently owns that were added to the portfolio in the past year; the account's firm sale rate is its proportion of firms owned last year that were removed from its portfolio in the past year. The average firm purchase rate declines from 43% to 33% as we go from lowest-value quintile to highest-value quintile, whereas average firm sale rates vary little across account quintiles, with a range of 33% to 36%. Finally, voting participation, that is, the portion of accounts voting (rather than the portion of shares that are cast) increases from 3% at the smallest quintile to 15% in the largest account value quintile. Fig. 1, Panel A displays this evidence.

Table 2, Panel A details retail shareholder ownership at the firm level. Overall domestic retail ownership averages 25%–27% of shares outstanding each year, rising to 35–40% in the smallest quintile of firms; an additional 4% is held by non-U.S. retail investors. The table reports the average and median number of investors per firm in thousands. Unsurprisingly, larger firms are owned by more investors: while the median firm in the smallest size quintile is held by roughly two thousand accounts, the median firm in the largest size quintile is held by roughly 120 thousand accounts. Table 2, Panel B describes the yearly distribution of ownership stakes, defined as an account's shares in a given firm divided by the firm's shares outstanding, in millionths. Each year, the median account owns about 0.13 millionths of a firm; the average account stake ranges from 5.42 to 6.27 millionths of the firm. Predictably, for smaller firms, each individual retail stake tends to own a larger portion of the firm. Fig. 1, Panel B displays some of this evidence.[8]

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[8] The Online Appendix further documents our data and coverage. Table A3 details the percentage of firms in the CRSP universe for which we have a match in the Broadridge retail dataset. In total, our final sample consists of about 3,200 firms each year in our retail dataset that match to both CRSP and ISS, as compared to 3,766 U.S. publicly listed firms as of 2015 in Kahle and Stulz (2017), with the discrepancy mostly attributable to small firms not covered by ISS. Our final dataset contains only publicly traded firms. Online Appendix Table A4 analyzes household ownership of brokerage accounts using data from the Survey of Consumer Finances. It shows that the probability of owning a

4.3.2. Retail voting characteristics

Retail voting can be described at two levels of weighting: by retail shares, which emphasizes the largest shareholders and is more informative about firm outcomes; or by retail accounts, which are more reflective of the small retail accounts that comprise the bulk of accounts but a smaller fraction of shares. Table 3 provides ballot-level statistics: retail voters cast ballots for 32% of shares owned, reflecting the decision of only 11% of accounts to participate, indicating that retail shareholders with small equity stakes are less likely to cast votes. For special meetings, turnout rises to 38% (by shares) or 15% (by accounts). This evidence provides an initial indication of heterogeneity in retail participation by account attributes and meeting characteristics which we study later in Section 6 within the general utility framework for participation.

76% of shares cast (58% of accounts participating) support management on every proposal on the ballot at annual meetings, indicating that a substantial fraction of retail voters, especially small ones, oppose management on at least one proposal. Online Appendix Table A6 expands on Table 3 by conditioning on the proposal types voted at the meeting.

Table 4 contains information on turnout and voting choices at the proposal level. Cast (%) reflects proposal-level turnout, defined as votes For and Against divided by shares outstanding.[9] For (%) represents support, defined as the number of votes For divided by the total cast For and Against. The three sets of columns are labeled All votes, providing the firm-wide voting totals,

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brokerage account increases with the household's education, age, income, and net worth and decreases with household size. Households that report more willingness to take financial risks, that have a savings or retirement account, or that invest in mutual funds or hedge funds also are more likely to own brokerage accounts. Online Appendix Table A5 breaks down ownership by industry; telecommunications firms tend to be more widely held than other industries, perhaps reflecting the size of some major technology firms.

[9] Since this table is at the proposal level, we count a ballot as cast only if it makes a selection For or Against on the proposal in question. Elsewhere, we count it as cast if the ballot is submitted. Because almost all submitted votes make a selection, the two metrics are highly similar.

Retail votes, providing the total retail voting results, and Retail accounts, providing retail voting results weighting each account equally instead of by number of shares.

Table 4, Panel A classifies proposals by sponsor. Non-retail shareholders are far more likely to cast votes, with a 79% aggregate turnout rate across all investors, whereas retail shareholders vote For or Against on only 31% of proposals, by shares (or 11%, by accounts). This difference reflects the fact that many institutional shareholders are effectively mandated to vote. As measured by shares owned, retail shareholders are somewhat less supportive of management proposals than are non-retail, and substantially less supportive of shareholder proposals. However, small retail accounts support shareholder-submitted proposals more than large retail accounts do.

Panel B of Table 4 describes variation in turnout and support by firm size. Retail shareholder turnout decreases with firm size, whereas turnout by shareholders collectively shows no such pattern. The bottom part of Panel B reports on shareholder proposals. Shareholder proposals in small firms receive a substantial degree of retail and non-retail support, but support declines as we move to larger firms, especially among retail shareholders. Importantly, small accounts tend to support shareholder proposals more than large accounts do for firms of any size.

Panel C of Table 4 provides information on retail voting by proposal category. Retail turnout is highest (46%) for proposals regarding mergers and acquisitions, whereas for the overall electorate, turnout varies little across categories. Retail and non-retail support for M&A transactions exceeds support for other management proposals, which may reflect management control over the timing of such transactions. As in Panel A, shareholder proposals (environmental, social, and governance) receive weaker support from retail shareholders relative to the overall electorate. The weaker support of social responsibility proposals by retail shareholders as compared to institutions may reflect the different incentives involved in managing one's own account as compared to managing money for others, which we discuss in greater detail in Section 7.3.

Finally, Panel D of Table 4 shows voting split by sponsor and recommendations by management and ISS. The overall electorate shows a large difference in voter support between management proposals that are supported by ISS and those opposed by ISS. We find a more muted variation in retail shareholder support between ISS-supported and ISS-opposed proposals with an even starker difference within shareholder proposals. For the overall electorate, shareholder proposals supported by ISS have 36% support and those opposed by ISS have 8% support, but for retail voting, that gap is smaller: 17% vote in favor of proposals supported by ISS whereas 14% vote in favor of those opposed by ISS. We examine this difference in detail in Section 7.3.[10]

5. Influence of retail vote on voting outcomes

In this section, we ask whether retail shareholder participation and voting preferences are important determinants of voting outcomes. We ask whether shocks to either retail participation, retail ownership, or, conditional on participation, retail voting preferences, would have altered observed outcomes. We document the number of failed (successful) proposals that pass (fail) under our counterfactual scenarios and then compare whether the impact of changes to retail participation, ownership, and voting preferences differs from similar shocks to other non-retail

[10] We provide additional descriptive statistics on retail shareholder voting in the Online Appendix. Table A7 provides information on shareholder proposals, breaking into finer subcategories and adding in voting by the Big Three asset management funds. The Big Three vote strongly against environmental and social proposals, but they support a substantial portion of governance proposals. We report on how retail voting varies by voter and firm characteristics in Online Appendix Table A8. Low-value accounts are highly unlikely to vote, but conditional on voting, they are far more likely to support shareholder proposals and less likely to support management proposals. Online Appendix Table A9 compares frequent to infrequent voters. The voting behavior of infrequent voters is of special interest should regulatory changes be made that increase retail participation. We find that, whereas frequent voters tend to turn out consistently across all proposal types, infrequent voters cast their ballots for major transactions far more than for other proposal types. Infrequent voters are also far more supportive of all types of shareholder proposals than are frequent voters.

voters that serve as benchmarks. As we show, the effect on outcomes from shocks to retail voting is as large as that of other voting groups that we consider.

We begin by considering the scenario in which retail participation is set to zero and assess the collective retail shareholder impact on voting outcomes. We compare the resulting change in outcomes to similar shocks to the participation of two other groups of voters: (i) all non-retail shareholders and (ii) the "Big Three" institutional investors. In our second set of tests, we assess how proposal outcomes change when we increase the retail ownership of firms that have low retail ownership or decrease the retail ownership of firms that have high retail ownership. In our third set of tests, we limit the sample to close elections and evaluate voting outcomes if retail shareholders voted using different decision rules, holding observed participation rates fixed. We compare to the change in outcomes that would result if the Big Three institutional investors voted using those decision rules. The subset of proposals that we use in all tests is constructed as follows. First, we remove routine proposals and director elections, which are less likely to be contested. Second, we remove proposals for which passage requires sufficient votes as a percentage of outstanding shares, since removing votes mechanically causes these proposals to fail.

Table 5, Panel A illustrates how many proposals would have different outcomes if a group's participation rate were set to zero. We pool together management and shareholder proposals. Columns (1) and (2) provide the number of passing and failing proposals, respectively. Under the hypothetical that the voting rate for a given group goes to zero, columns (3) and (4) reflect the number of proposals whose outcome would flip, while columns (4), (5), and (6) provide the number of proposals whose final percentage counts would move by five, ten, and twenty percent, respectively. The consequences of eliminating retail voter participation are given in the first row in the panel. Setting retail participation to zero, 122 (39) proposals that passed (failed) would switch outcomes and fail (pass). The second row, in which Big Three participation is set to zero, shows that the resulting change in voting outcomes is similar to the removal of retail shareholders. In the third row in Panel A, we set the participation of all non-retail voters to zero and thus allow retail investors to decide the outcomes of these proposals. This counterfactual leads to more flipped proposals, reflecting the fact that non-retail voters comprise the bulk of shares cast and that the retail vote often substantially diverges from the non-retail vote.

In Table 5, Panel B, we hold the rate of participation and voting choices fixed and alter the ownership structure of the firm by shifting ownership between retail and non-retail shareholders while holding constant total shares owned, as well as each group's participation rates and percent in favor. We begin by calculating the standard deviation of retail ownership of all firms in the sample, 18.4%, which we use as the yardstick by which we shift retail ownership. Next, we sort firms into quintiles of retail ownership and ask how an increase (decrease) in ownership for firms in the bottom, second, and third (third, fourth, and largest size) quintile impacts vote outcomes. We report the consequences of these ownership changes separately for management and shareholder proposals. The results show that reducing retail ownership leads to change in outcomes for management proposals from pass to fail and shareholder proposals from fail to pass. An increase in retail ownership leads to more successful management proposals and fewer successful shareholder proposals, consistent with retail having stronger support for management than other shareholders in close votes.

Table 5, Panel C provides our final set of counterfactual tests, in which we hold fixed participation and then measure how many close proposals would flip outcomes if retail voters (or Big Three voters, respectively) were to vote with the proposal support rates that different groups of voters had for that proposal. Specifically, each row shows how many proposal outcomes would change if a subset of retail voters (or Big Three Voters, respectively) voted like the following shareholders: (i) retail voters; (ii) non-retail voters; (iii) Big Three voters; (iv) all in favor; or (v) all opposed. To ensure a consistent comparison across the two voting groups, the number of votes we alter for a proposal is limited to the minimum of the number of retail votes and the number of Big Three votes. We report the results separately for shareholder and management proposals and limit to close elections whose final overall vote result was between 40 and 60 percent of the threshold for passage. Columns (1) and (2) contain the number of passing and failing proposals in the subset. Similar to Bach and Metzger (2019) and Babenko, Choi, and Sen (2019), we find that management tends to win a disproportionately high fraction of close votes. Columns (3) and (4) (or (5) and (6), respectively)) reflect the number of proposals whose outcome is changed under the hypothetical that retail voters (or Big Three voters, respectively) alter their voting decisions.

The main takeaway from Panel C is that the consequences of altering retail shareholder voting preferences are of the same magnitude as altering Big Three voting preferences. For example, were retail shareholders to vote like all other non-retail shareholders, 17 shareholder proposals that had actually failed would now pass and 35 management proposals that had passed would now fail. When we repeat the test for the Big Three and ask how voting outcomes would change had they voted like all other non-retail shareholders, we find similar results.[11]

[11] As discussed in Appendix A.4, we only observe the votes of funds that appear in ISS Voting Analytics' N-PX dataset and therefore may undercount the shares held by the Big Three. In Online Appendix Table A10, we provide a robustness check in which we scale up the observed Big Three votes to the total holdings by Big Three open-end mutual funds and ETFs on each firm, calculated from CRSP. The larger Big Three share ownership increases the impact of Big Three shareholders in Panel A, though it is still roughly comparable to that of retail investors.

6. Retail shareholders' decision to participate

In this section, we provide evidence on retail shareholder participation. We adopt the standard political science utility framework presented in Section 2 to shareholder voting and then evaluate theories of participation related to information and preferences.

6.1. Cost-benefit tradeoff

In this subsection we ask the following: (i) whether retail shareholder turnout increases with greater financial benefits of voting; (ii) whether it decreases with higher costs to participation; and (iii) whether turnout is non-zero even when the financial benefits of voting are negligible.

6.1.1. Benefits from winning

As described in Section 2, a voter's utility from participation depends on her expected costs and benefits from participation plus any consumption benefits from voting. The benefit of winning is captured by the term $P \cdot B$ in Eq. (1). In this subsection, we empirically evaluate the relationship between turnout and proxies for the voter's probability she is pivotal, P, and her benefits of success conditional on being pivotal, B.

A plausible null hypothesis is that turnout is unrelated to $P \cdot B$. Nearly all retail shareholders have an ex-ante pivot probability near zero, implying that variation in benefits of the election should not correlate with utility from voting; in fact, a prominent viewpoint in political science is that voters vote for purely expressive reasons (see, e.g., Brennan and Hamlin (1998)). Furthermore, retail shareholders may not find it profitable to engage in costly monitoring of their portfolio even if they were certain to be pivotal.

The results in this subsection reject the null hypothesis that there is no relationship between turnout and $P \cdot B$. We document below that across retail shareholders in a given firm, those who hold larger stakes are more likely turn out to vote. Further, within retail accounts, investors are more likely to vote at firms where they hold a larger stake. Turnout also increases when the expected benefit from winning, B, as measured by various proxies, is higher. As suggested by the structure of the $P{\cdot}B$ term, the interaction between ownership and benefits is positive: owners with larger stakes are more sensitive to variation in benefits across firms.

6.1.1.1. Share ownership, α

We begin by considering the relationship between turnout and the fraction of the firm owned by the account, α. The setup in Eq. (1) predicts that turnout increases with α because the likelihood of pivotality, P, and the benefits of winning, B, both increase with α.

Although our goal is to document factors whose variation explains turnout, we are limited by endogeneity concerns. We attempt to address issues of omitted variables and endogeneity by incorporating high-dimensional fixed effects. We compare turnout within a given meeting, within a given account-year, and within a given account-firm, thereby controlling for meeting-invariant, account-year-invariant, and account-firm-invariant heterogeneity.

We estimate specifications of the form:

$$Cast_{amct} = \beta_0 + \beta_1 \log(\alpha_{amct}) + \beta_2 X_{amct} + (\phi_m + \phi_{at} + \zeta_{Ind} + \phi_{ac}) + \varepsilon_{amct} \quad (2)$$

in which a indexes accounts, m indexes meetings, c indexes firms, and t indexes time. X_{amct} is a vector of covariates. ϕ_m denotes meeting fixed effects, to control for variation in benefits to the firm from the proposal, which are replaced, in different specifications, by: account-year fixed effects, ϕ_{at}, to control for the composition of retail accounts at a meeting; industry fixed effects, ζ_{Ind}; and account-firm fixed effects, ϕ_{ac}, to control for variation in account-firm-specific voting propensities. Columns (1) and (2) of Table 6 provide results estimating Eq. (2) with meeting fixed effects, columns (3) and (4) include account-year and industry fixed effects, columns (5) and (6) include both meeting and account-year fixed effects, and column (7) includes meeting, account-year, and account-firm fixed effects. To allow for multiple high-dimensional fixed effects and multi-way clustering to be computationally practicable, we use a sample of randomly selected accounts for our account-level regressions. We demean our right-hand-side variables by the average across all meetings in the sample so that the intercept can be interpreted as the turnout for an observation with average values of all covariates.

The results in columns (1), (3), (4), (5), and (7) all show a strong positive relationship between share ownership, α, and turnout. We postpone discussion of columns (2) and (6) because they include interaction terms with α. Doubling an account's stake size results in a propensity to vote 1.8 percentage points higher relative to a baseline account at the same firm meeting (column (1)). For a given account, doubling the stake size results in a 0.3 percentage points higher likelihood of turnout (column (3)), which increases to roughly 0.6 percentage points once we control for firm covariates (column (4)) or absorb cross-meeting variation entirely (column (5)). These are economically large changes given that the overall turnout rate is roughly 8–9%, as shown in the intercepts.[12] The estimates in column (7), in which we add account-firm fixed effects, indicate that when a shareholder purchases more of a given stock, she tends to vote more than she did beforehand, by 0.6 percentage points. The evidence is clear: shareholders who own larger stakes in a given firm are more likely to participate in voting, and a given shareholder is more likely to vote at firms she owns more of.

[12] This turnout rate is slightly lower than the 11% reported in Table 3. The discrepancy is driven by the different weighting schemes used by the tables: Table 3 weights all accounts within a meeting equally, then aggregates across meetings, treating each meeting equally, whereas Table 6 weights all account-years equally. As a result, Table 6 places more weight on firms with more shareholders and accounts with fewer securities as compared to Table 3.

As discussed in Section 2, although there is a clear prediction regarding the direction of the relationship between turnout and ownership α, there is no clear prediction regarding the shape of the relationship; it depends on the relationship between pivotality P and α, the concavity of the shareholder's utility function, and the relationship between utility and turnout. Fig. 2 presents evidence on the association between shareholder turnout and α. Given the large dispersion in retail ownership and the small stakes held by most retail investors, we split the support of α into four intervals so that each of the four scatterplots provides a different range for share ownership. While the first interval describes the turnout of the large number of accounts in our sample who own very small stakes, the fourth interval describes the turnout of the smaller number of wealthy accounts with large stake sizes. We observe a consistently increasing but concave relationship between turnout and α.[13]

Although large shareholders turn out far more often than small shareholders, our evidence on the concavity of turnout with respect to α suggests that the differences between mid-sized, small, and very small shareholders are more important for determining heterogeneity in turnout than the difference between large and mid-sized shareholders.

6.1.1.2. Benefit from success, b_f

If shareholders respond to financial benefits of voting, then their propensity to vote ought to be higher at poorly performing firms and important meetings because the benefit of winning, b_f, is higher. We now ask whether proxies for high b_f are associated with higher turnout. We estimate specifications as in Eq. (2), including the following meeting-level variables to proxy for the

[13] Since high market capitalization firms tend to have shareholders with a smaller share ownership than smaller firms, we report in Online Appendix Fig. B1 how the relationship between turnout and α varies within firm size sorts. As with the full sample, we find that within each size quintile, retail shareholder turnout is concave with respect to α.

difference in firm value between proposal outcomes: yearly abnormal return, ROA, Tobin's q, and an indicator for special meetings. We assume that poor performance or the calling of a special meeting are associated, on average, with voting options in which the expected difference in value between winning and losing would be higher. We also include the following additional covariates: log market equity, an indicator for whether the firm paid a dividend, the firm's institutional ownership percentage, and account-year and industry fixed effects. While excluding meeting fixed effects, ϕ_m, potentially introduces omitted variable bias by comparing turnout across different meetings, it allows us to test the association between meeting-invariant variables and turnout. Table 6, Panel A, column 4 provides the results. We find that special meetings and poorly performing firms, as measured by Tobin's q and ROA, see significantly higher turnout, as predicted. The results are consistent with shareholders performing a monitoring role, turning out to vote when the financial consequences are greater.

Although Table 4, Panel B shows turnout decreasing with firm size, Table 6, Panel A, Column 4 shows that, once we control for α and account-year fixed effects, turnout increases with firm size. This is again consistent with greater turnout when there are larger utility benefits to the shareholder. Although there is no simple predicted relationship between a firm's ownership structure and the likelihood of pivotality of small shareholders, we also find that turnout decreases significantly with institutional ownership. Appendix Table A11 uses additional measures of institutional ownership and finds that this relationship is robust.

6.1.1.3. Interaction of share ownership and the benefit from success, b_f

In this section, we explore how the sensitivity of turnout with respect to the shareholder's benefit from success from the vote outcome varies with α. As a shareholder's portion of the firm owned increases, her monetary benefit from a gain in firm value increases. However, because her utility from an additional dollar may be concave, the framework in Section 2 does not generate a clear prediction for how turnout varies with the interaction of α and proxies for benefits to the firm from winning.

To assess this relationship, we turn to the specification in Eq. (2) in which we examine shareholder turnout with meeting fixed effects. Table 6, Panel A, column (2) provides regression results in which we include interactions of log α with the proxies for variation in the financial benefit from success, b_f, that were significantly associated with turnout: Tobin's q, special meeting, and ROA. We find that turnout's correlations with Tobin's q and special meeting are stronger with larger α. In column (6) we further add account-year fixed effects; in this specification, ROA also shows a stronger correlation with turnout with larger α.

These results are consistent with a multiplicative structure in which larger benefits from voting are experienced most by those with larger ownership of the firm. Moreover, the evidence is consistent with retail shareholders that are motivated at least in part by the anticipated financial consequences of casting their ballot. Not only do they vote more when the company they own performs poorly or when the firm is contemplating a large transaction, but their turnout is most sensitive when they own a larger stake.[14]

6.1.2 Net costs

In Eq. (1), shareholders weigh expected financial benefits $P{\cdot}B$ against net costs $C - D$, in which D represents any consumption, or "expressive," benefit of voting other than the direct

[14] The multiplicative structure of Eq. (6) also implies that larger dollar benefits from voting are experienced most by those with large ownership, α, in larger market capitalization firms. We estimate the regression in Eq. (2), adding in the triple interaction of $\log \alpha$, $logME$, and either Tobin's q, special meeting, or ROA and report the results in Online Appendix A15. The coefficients on the special meeting interaction are significant in both specifications, and the coefficients on Tobin's q and ROA interactions are significant in one specification each.

financial benefit of expected electoral victory. As discussed in Section 2.1.3, many political scientists believe it implausible that the likelihood of a voter being pivotal in a political election is high enough to overcome costs of voting, and empirical studies—generally experimental—have produced evidence for utility from voting itself.

In this section, we begin by evaluating the propensity for participation among shareholders for whom $P{\cdot}B$ is near zero to assess whether there exist shareholders for whom the consumption benefit outweighs the costs of voting such that the net cost, $C - D$, is negative. We then ask whether there is an association between voting in political and corporate elections, which may imply heterogeneity across voters in net costs of participation that spans election types. Finally, we exploit an exogeneous change in the voting methods available to some accounts to study the effect on turnout of changing costs of participation.

6.1.2.1. Consumption benefits of voting, D

An interesting feature of our setting is that altruistic motives would not be expected to matter as they would in political elections. Shareholder voting is commonly understood to be intended to maximize shareholder profits, and we have already demonstrated in previous sections that retail shareholders appear to turn out more when the opportunity for an increase in share value is greater. A consequent null hypothesis would be that shareholders who own a minute portion of a firm, and therefore face no realistic possibility of being pivotal, should exhibit zero turnout.

Table 6, Panel B, column (1) provides turnout levels for shareholders with small stake sizes. Specifically, we estimate Eq. (2) without covariates or fixed effects other than a series of indicator variables for $\alpha > 10^{-9}$,…,$\alpha > 10^{-6}$, so that the regression intercept represents turnout among shareholders who own a portion of the firm $\alpha \leq 10^{-9}$, and each coefficient represents additional turnout for shareholders in the next higher α group. Shareholders who own less than one billionth of the firm's shares outstanding have 2.7% turnout. Those who own less than one hundred millionth of the firm have 3.4 percent turnout (0.7+2.7), those with less than one ten millionth of the firm have 6.4 percent turnout (3.0+0.7+2.7), and those with less than one millionth have 9.1 percent turnout (2.7+3.0+0.7+2.7). These numbers remain little changed in column (2), where we limit the sample to firm meetings at which no proposal in the final voting outcome comes within 30% of its vote threshold in either direction. In column (3), we limit the sample to stake sizes lower than $100 in value and still find significant nonzero turnout.

Given the small stakes that they own, even if these shareholders were to be pivotal, the expected financial gains would be negligible. Thus, just as in political elections, the evidence from shareholder elections appears consistent with $D \geq C$ for at least some shareholders, even without an obvious civic duty or Kantian imperative to vote. In Online Appendix Table A18, we further limit the sample to firms that, pursuant to Thomson Reuters, have a single institutional owner with more than 50% holdings. For these firms, we know that these shareholders' probability of being pivotal is zero. However, we find positive turnout even in this subsample, consistent with the idea that some shareholders derive consumption benefits from voting.

We next explore whether D may be indexed by proposal type. Investors may feel a civic duty to vote on proposals that have a broader impact, such as shareholder proposals on environmental or social issues. Column (4) in Table 6, Panel B shows that, as compared to the reference group (annual meetings with no shareholder proposals on the ballot) there is no significant relationship between social responsibility proposals on the ballot and turnout for a particular meeting. However, shareholders do turn out more for ballots with governance-related proposals, suggesting that there may be some content-based reasons for the increased turnout.

As mentioned in Section 2.1, successful SRI proposals may generate positive social surplus and it is possible that some accounts place positive weight on such surplus. That is, the benefit from voting on SRI proposals may derive both from the financial consequences associated with such proposals and the social surplus generated by a victory, rather than by the consumption benefit from voting itself. We would therefore expect the utility benefit from voting on SRI proposals to increase with share ownership, α. Online Appendix Table A19 provides analysis similar to that in columns (2) and (6) of Table 6, Panel A, in which we include an indicator for whether there is an SRI proposal on the ballot and the interaction of log α with this SRI indicator variable. While we find a positive significant relationship in the specification without account-year fixed effects, the association between participation and the interaction of α and SRI on the ballot is insignificant in the specification with account-year fixed effects.

6.1.2.2. Relationship with political turnout

The evidence in Table 6, Panel A, in which the R^2 increases from 0.04 to 0.79 when account-year fixed effects are included in column (3), is consistent with large variation across shareholders in turnout propensity and net consumption benefits of voting, $C - D$.[15] In this subsection, we ask whether net benefits extend across different types of elections by documenting whether an individual's propensity to vote in corporate elections correlates with her propensity to vote in political elections. While we do not observe whether an accountholder votes in political elections, we do observe her county's aggregate turnout. To the extent that the decision to locate

[15] The explanatory power of person and area-level variables is itself a question of interest in the political science literature. Matsusaka and Palda (1999) find that standard area-level variables capture 15% of variation in political election turnout, substantially higher than we capture with our zip code and account-level variables, whereas they find that including person-specific past turnout captures 30% of variation in turnout in political elections, substantially lower than our R^2 from including account-specific fixed effects. The existence of demographic similarity may have the potential to produce unintentional coordination that can enhance the impact of retail shareholder votes, as Kandel, Masa, Simonov (2011) find for Swedish retail shareholder trades.

in a certain county is determined in part by characteristics that correlate with net consumption benefits, we ask whether turnout in corporate elections is correlated with turnout in political elections. A positive association would bolster the evidence that certain individuals have a higher propensity to vote that cannot be easily explained by variation in financial benefits from voting.

We provide results from regressions estimating Eq. (2) in column (5) of Panel B of Table 6. We include the account's county-level political turnout in the 2016 presidential election. We find that political turnout in the account's county is positively correlated with shareholder turnout. This association may be driven by several causes—those who turn out in political elections may have lower costs of acquiring information, may have lower costs of voting, or may experience greater altruistic benefits of voting. In any event, the evidence is consistent with a person-specific element to turnout that carries across types of elections.

6.1.2.3. Costs of voting, C

How do shocks to costs, C, affect shareholder turnout? In this subsection, we use a triple-differences approach to measure how the removal (or addition) of a certain voting method available to some shareholders differentially changes the costs of voting for the shareholders who prefer those methods. In particular, we show that the bundle of materials that an account receives, which affects both its readily available information and its available voting methods, substantially impacts its likelihood of turning out. We then attribute the impact to the loss of the option to vote by mail or telephone (as opposed to internet) when that is the account's preferred voting method. In what follows we describe the setup.[16]

[16] Online Appendix E provides additional information on our identification approach, beginning with a specification of turnout which incorporates information materials and then deriving our triple-difference empirical specifications.

There are three forms of materials regarding the vote that an individual may receive: (i) Hard Copy materials, consisting of a complete copy of proxy materials sent to the shareholder via mail, including the proxy statement, annual financials, and ballot or vote instruction form; (ii) Notice, a mailed one-sheet notice to announce the meeting with information on how to obtain a complete package of proxy materials or use the service provider's online website for voting; or (iii) E-Delivery, in which links are delivered via e-mail to direct the shareholder to either the online voting website or to brokerage firms' investor mailboxes for voting.

Accounts choose to receive either (i) Hard Copy, (ii) E-delivery, or (iii) the firm Default delivery method. Firms may choose to send Hard Copy or Notice or may choose a mixture of the two (Notice to some shareholders, Hard Copy to others). The following table shows that the actual materials received by an account depend on a combination of the firm's choice and the shareholder's choice (removing mixtures for simplicity):

		Firm Choice:	
		(a) Hard Copy	(b) Notice
Shareholder Choice:	(i) Hard Copy	Hard Copy	Hard Copy
	(ii) E-Delivery	E-Delivery	E-Delivery
	(iii) Default	Hard Copy	Notice

An account's choice of Hard Copy or E-Delivery is determinative of the materials it receives; for the rest of the accounts, the firm's choice of Hard Copy or Notice determines their materials. We observe both the firm's choice and the retail shareholder account's choice at the time of initial delivery. For example, if an account receives a Notice, and then subsequently requests Hard Copy materials for the meeting, we observe the account's selection as Notice.

To identify the effects of the materials an account receives on turnout, we exploit variation resulting from the subset of firms that switch their choice of materials either from Notice to Hard Copy or Hard Copy to Notice during our sample period. Because the firm's choice affects only accounts that chose Default, Default accounts comprise our treatment group. We use a triple-differences approach — (i) across firm choice whether to switch or not, (ii) across time whether post-switch or not, and (iii) across shareholder choice whether Default or not.

We begin by presenting graphical evidence from firms switching from Hard Copy to Notice and those switching from Notice to Hard Copy. We first show that firms switching their choice of materials alters the materials that Default shareholders receive while leaving non-Default shareholders unaffected. The top part of Fig. 3 shows the portion of accounts receiving Hard Copy materials leading up to and following the switch, split by whether the account chose E-Delivery, Hard Copy, or Default. The combination of the firm's choice and the account's original choice is almost completely determinative of the materials the account receives. The E-Delivery and Hard Copy groups continue as before following the switch, whereas the Default group switches materials almost completely.

The bottom part of Fig. 3 shows turnout leading up to and following the switch in delivery method, split by whether the account chose E-Delivery, Hard Copy, or Default. We normalize each line by scaling by the voting rate in year -1. The graphs again show extremely strong parallel pre-trends, with nearly indistinguishable lines between the three groups. Default accounts at firms that switch from Hard Copy to Notice or Notice to Hard Copy in 2017 have virtually no pre-switch pre-trend in voting rates from 2015 to 2016, nor are there pre-trends in non-Default accounts, which are our placebo groups. The E-Delivery and Hard Copy groups' turnout rates continue as before following the switch, strong placebo tests substantiating our identification strategy. The Default group, by contrast, sees a large drop in turnout when firms switch to Notice and a large rise in turnout when firms switch to Hard Copy.

We adopt the following specification to model account turnout as a function of the materials the account receives:

$$Cast_{act} = \beta_0 + \beta_1 M_{act} + \beta_2 X_{act} + \theta_{ac} + \phi_{at} + \lambda_{ct} + \nu_{act} \quad (3A)$$

in which a indexes accounts, c indexes firms, and t indexes time. $Cast$ reflects whether the account turns out to vote, and M represents whether the account received Hard Copy Materials. The materials received by an account as compared to the materials the account received in other periods are a function of changes in the firm's decision:

$$\begin{aligned} M_{act} \quad = \quad & \delta_0 - \delta_{1A} SwitchHCtoN_c * Post_{ct} * D_{a0} + \delta_{1B} SwitchNtoHC_c * Post_{ct} * D_{a0} \\ & + \gamma_2 X_{act} + \theta_{ac} + \phi_{at} + \lambda_{ct} + \varepsilon_{act} \end{aligned} \quad (3B)$$

in which $SwitchHCtoN_c$ equals 1 if a firm switches from Hard Copy to Notice (or vice versa for $SwitchNtoHC_c$), $Post_{ct}$ equals 1 if it is after the firm's switch, and D_{a0} equals 1 if the account had selected Default at time 0. The $Switch$ variables shift the bundle of materials the account receives, which may affect its turnout. The account's turnout as compared to the account's turnout in other periods is thus an indirect function of changes in the firm's decision:

$$\begin{aligned} Cast_{act} \quad = \quad & \delta_0 - \delta_{1A} SwitchHCtoN_c * Post_{ct} * D_{a0} + \delta_{1B} SwitchNtoHC_c * Post_{ct} * D_{a0} \\ & + \gamma_2 X_{act} + \theta_{ac} + \phi_{at} + \lambda_{ct} + \varepsilon_{act} \end{aligned} \quad (3C)$$

To absorb as much variation as possible, we include two-way fixed effects at the account-firm, account-year, and firm-year levels. Account-firm fixed effects ensure we compare the same account at the same firm over time. Account-year fixed effects ensure we compare the same account at the same time to other firms in its portfolio. Firm-year fixed effects ensure we compare across accounts at the same firm at the same time.

Importantly, we do not require that the firm's decision to switch materials is exogeneous. The primary identifying assumption for the triple-differences setup is parallel trends in materials received and in turnout among Default shareholders at switching firms as compared to non-Default and non-switching firms. Fig. 3 shows clear evidence of parallel pre-trends; our identifying assumption is that those trend lines would remain similar if, counterfactually, accounts did not receive different materials following the switch.

Table 7 provides results of regressions formally estimating Eq. (3A), (3B), and (3C). Column 1 estimates Eq. (3B), with receipt of Hard Copy materials on the left-hand side. We find that a firm switching its delivery methods yields a 89.8–91.7 percentage point change in the likelihood of receiving Hard Copy materials for Default accounts, as compared to different years of the same account-firm, different firms in the same account-year, and different accounts in the same meeting. Column (2) provides regression estimates of Eq. (3C). We find that a firm switching its delivery methods yields a 2.5–4.2 percentage point change in turnout. Finally, in column (3), we estimate Eq. (3A), the effects of materials on turnout, by effectively scaling the combined results of column (2) by column (1) to estimate the size of the effect (Duflo (2001), de Chaisemartin and D'Haultfoeuille (2018)). We find that a change in receipt of Hard Copy materials causes a 3.2 percentage point change in turnout. Moving from Notice to Hard Copy increases turnout among Default accounts from roughly 6% to 10%, and moving from Hard Copy to Notice decreases turnout among Default accounts from roughly 9% to 6%, for an effect size of roughly 50%–66%.

The results indicate that a change to the firm's choice of materials causes a large change in turnout for those accounts who are affected by it. This evidence does not, however, help to distinguish whether the change in turnout is driven by a change to information availability or by a change in access to voting. We therefore turn to evaluating the reason for the effect of a switch to Notice on retail shareholder turnout. A little-discussed provision of the SEC's Notice and Access rule restricts those receiving Notice instead of Hard Copy from voting by mail or telephone. Thus, a firm switching information materials also alters the voting methods easily available to Default accounts. Prior research has focused solely on the implications of a change to information availability for turnout. In what follows, we limit the analysis to firms that used Hard Copy in year $t-1$ (and potentially switched to Notice in year t) and focus on the voting method used by voters the year *before* a firm switch. We begin with Default accounts that voted in year $t-1$, and estimate a difference-in-difference specification comparing the turnout in year t across switching and non-switching firms and those who voted by internet versus those who voted by non-internet in $t-1$. As a placebo test, we repeat the exercise for non-Default accounts. We display the results graphically in Fig. 4. For each group, we estimate the following specification:

$$\begin{aligned} Cast_{ac} \;=\; & \gamma_0 + \gamma_1 SwitchHCtoN_c * LastYearNonInternet_a + \gamma_2 SwitchHCtoN_c \\ & + \gamma_3 LastYearNonInternet_a + \varepsilon_{ac} \end{aligned} \qquad (4)$$

in which a indexes accounts and c indexes firms, and $LastYearNonInternet_a$ refers to voting by methods other than the internet in the year prior. Table 8 contains our estimation results. In the left two columns, we regress turnout in 2016, keeping only those accounts that voted in 2015. In the right two columns, we regress turnout in 2017, keeping only those accounts that voted in 2016. The first and third columns contain Default accounts, who we expect to be affected by firms switching from Hard Copy to Notice; the second and fourth columns contain non-Default accounts, our placebo test. Standard errors are clustered by meeting and account.

The results are striking. For Default accounts that voted by mail or telephone in 2015, there is a 45 percentage point drop in voting in 2016 when their firm switches from Hard Copy to Notice, but there is no drop for Default accounts that voted by internet in 2015. Non-Default accounts see no significant differences between switching and non-switching firms or between internet and non-internet voters, consistent with the fact that they saw no change to their available voting methods. We see similar evidence if we limit to 2017 switches instead of 2016.

We conclude that the effects of Notice and Access are driven by available voting methods, not information materials. This finding has several implications. First, it shows that the decision to participate is quite cost-sensitive—affected retail shareholders see their turnout drop by as much as 45 percentage points. Second, Bach and Metzger (2019) and Babenko, Choi, and Sen (2019) have recently documented how management is able to win close votes by changing retail participation via the way information is delivered to retail investors. Our evidence suggests that it is not the information material per se that shapes participation but rather the access to the voter's preferred voting method. Third, this result appears consistent with Column (6) of Panel B in Table 6, which suggests that turnout rates are highly correlated with age. This evidence has implications for research on political voting studying how electors' demographics, including age, digital access and literacy, impact their decision to participate and vote (Serdült et al. (2015), Germann and Serdült (2017), Goodman et al. (2018)). Finally, the evidence in this section suggests that increasing turnout does not require eliminating Notice and Access, but rather modifying it to improve voting access.

6.2. Potential implications for information and preference-based models

In this section, we further explore the extent to which our data sheds light on models from the political science and shareholder voting literature that attempt to explain non-zero voter turnout.

6.2.1. Information-based models

As discussed in Section 2, political elections feature higher turnout among informed voters (see Matsusaka (1995) for a discussion), and several political science papers have theorized information-based participation. The main insight in these models is that a voter with a less informative signal regarding the candidates can ascertain, conditional on being the pivotal voter, that her private signal likely clashes with those of the more informed voters, and she is better off abstaining.

We cannot observe the information set of the shareholders in our sample and, given that the accounts are anonymized, we cannot form proxies for shareholder-level informedness. We do, however, observe the shareholder's account value and characteristics measured at the zip code and county level, which may serve as proxies for shareholder information. In particular, the shareholder zip code-level percentage with a bachelor's degree and the percentage of employees in the county who work in finance or insurance may serve as proxies for the level of informedness of shareholders residing in that locality.

Table 6, Panel B, column (6) shows that, although the portion holding college degrees is not significantly related to turnout, the portion of a county that works in finance or insurance has a large positive correlation, potentially consistent with higher turnout among those with greater information. Those with larger portfolio values are also significantly more likely to turn out for a firm meeting, even controlling for the size of the particular stake in that firm, which could be a proxy for informedness.

6.2.2. Preference-based models

We next turn to predictions from models that seek to link variation in shareholder preferences and turnout. The political science literature features several theories that connect voter preferences to participation, such as Myatt (2015). Zachariadis et al. (2020) apply some of these insights to a shareholder voting context. In this section, we seek to establish the relationship between preferences and turnout.

Following Zachariadis et al. (2020), we limit the sample of proposals to shareholder governance proposals, which have more heterogeneous institutional popularity than other proposal types and which are often the most contentious and outcome-uncertain proposals on the ballot. Zachariadis et al. (2020) select this subset to minimize information heterogeneity across shareholders and proposals. We calculate a shareholder's preference for governance proposals as her average vote on governance proposals in other meetings, which we refer to as her "governance score." As a result, our sample in this section is necessarily limited to accounts that own at least two securities and that sometimes turn out. By constructing shareholder preferences in this manner, our preference measure is not dependent on turnout on the proposal in question.

We estimate specifications of the form:

$$Cast_{apmct} = \beta_0 + \beta_1 G_{a,-m} + \beta_2 X_{apmct} + \beta_3 G_{a,-m} X_{apmct} + \varepsilon_{apmct} \quad (4)$$

In which a indexes accounts, p indexes proposals, m indexes meetings, $-m$ denotes meetings other than m, G is an account's governance votes at other meetings, and X_{apmct} is a vector of characteristics about the account, proposal or meeting. Online Appendix Table A12 contains results from a regression estimating Eq. (4). We do not find evidence that retail turnout among voters who tend to support governance proposals differentially changes with institutional support as compared to voters who tend to oppose governance proposals. A shareholder who generally supports governance proposals is not significantly more likely to turn out when institutional support for the proposal is weak, or vice-versa. In all, we cannot reject the null of no relation, though in all columns the signs are in the direction predicted by Zachariadis et al. (2020).

7. Determinants of support for management and shareholder proposals

In the previous section we examined the individual decision whether to vote, and earlier we showed that retail voters, as a bloc, have a substantive effect on voting outcomes. In this section, we turn to analyzing retail investors' support for management and shareholder proposals, conditional on casting a ballot, to better understand individuals' voting decisions and how they impact overall outcomes. We first conduct the analysis at the account level to evaluate a given retail investor's decision, and then at the meeting level to address how firm-level variables affect the firm's overall voting outcome. We conclude the section by evaluating how a retail investor's decision to exit a firm is associated with her voting decisions in the previous year.

7.1. Account-level evidence

We estimate the following main specification:

$$\begin{aligned} WithMGMT_{apmct} = \; & \beta_0 + \beta_1 \log(\alpha_{amct}) + \beta_2 X_{apmct} + \theta_t + \psi_{PropCat} + \zeta_{Ind} \\ & +(\phi_p + \phi_m + \phi_{at} + \phi_{am} + \phi_{ac} + \phi_{a,PropCat}) + \varepsilon_{apmct} \end{aligned} \tag{5}$$

where a indexes accounts, p indexes proposals, m indexes meetings, c indexes firms, and t indexes year-months. The dependent variable, $WithMGMT_{apmct}$, is a binary variable that equals one if the account votes in line with management recommendation and zero if it votes against, multiplied by 100. That is, $WithMGMT_{apmct}$ equals 100 if the account votes for a management proposal or against a shareholder proposal. X_{apmct} is a vector of covariates, including firm-meeting level covariates (log market equity, yearly abnormal return, a binary variable for dividend yield, Tobin's q, return on assets, and special meeting), firm-proposal-level variables (whether ISS's recommendation was in opposition to management's recommendation), and account-level or zip-code level variables (log account portfolio value, county 2016 presidential turnout percent, log zip code income, zip code fraction of over-65 year-olds, zip code density, zip code fraction with bachelors, zip code fraction with post-bachelors). $\psi_{PropCat}$, ζ_{Ind}, and θ_t are proposal category, industry, and year-month fixed effects, which are replaced, in different specifications, by $\phi_p, \psi_m, \phi_{at}, \phi_{am}, \phi_{ac}$, and/or $\phi_{a,PropCat}$: proposal, meeting, account-year, account-meeting, account-firm, and account-proposal category fixed effects, respectively. All standard errors are clustered at the meeting and account level, and we weight observations so that each account-year has equal weight.

Table 9 displays the results estimating Eq. (5). Each column includes different fixed effects to focus on variation in different independent variables. Column (1) includes only proposal category, industry, and year-month fixed effects, and is designed to give a general view of the relationship between voting choices and firm, firm-proposal, account, and zip-code level variables. In column (2), we include proposal fixed effects—comparing how different accounts vote on the same proposal—to provide a more precise estimate of account-level and zip code-level coefficients. In column (3), we include account-year fixed effects in addition to proposal category and industry fixed effects, focusing exclusively on the comparison between different securities in an account's portfolio to provide our best estimate of firm-level variables. In column (4), we include account-meeting fixed effects and account-proposal category fixed effects, which allow us to focus on ISS recommendations—how an account votes on an ISS-recommended proposal as compared to other proposals at the same meeting (and as compared to how that account generally votes in that proposal category). In column (5), we include account-year and proposal fixed effects, to provide a sharp focus on the account's ownership α, comparing voting choices against other firms held by the account in the same year and against other accounts voting on the same proposal. Finally, in column (6), we add to column (5) account-firm fixed effects, to additionally compare a given retail investor's voting choices at meetings of the same firm in different years.

The intercept in Table 9, column (1), shows that the average support for (opposition to) management across accounts with average levels of covariates is 85.5% (14.5%). We draw the following three conclusions from Table 9. First, retail voters punish the management of poorly performing firms, with a strong sensitivity to abnormal returns, and return on assets. Based on the results in column (1), retail shareholders are 1.6 percentage points more likely to oppose management (or 11.0% of the 14.5% average opposition) at a firm whose returns are one standard deviation below average. They similarly are 1.7 percentage points more likely to oppose management (or 11.7% of average opposition) at a firm whose return on assets is one standard deviation lower than average. Even when comparing different votes by a single account, a one standard deviation lower abnormal return is associated with 0.91 percentage points higher opposition to management (column (3)).

Second, based on column (1), we observe that ISS opposition to management is associated with lower retail support for management by 2.7 percentage points (or 18.6% of average opposition). Even when comparing the account's other votes at the same meeting and in the same proposal category, the association is 1.5 percentage points, suggesting that there is some information regarding proposals that both ISS and retail shareholders observe. However, this estimate is far lower than the 25 percentage point influence by ISS reported by Malenko and Shen (2016).[17]

Third, larger equity stakes tend to vote more in favor of management. A doubling of stake size is associated with a roughly 1.1 percentage point increase in support for management (or 7.5% of average opposition) as reported in column (1). This is partially driven by across-account variation, but also by within-account variation: column (5) shows that an account with multiple securities votes significantly more in favor of those securities that it owns more of. However,

[17] Bubb and Catan (2020) conclude that heterogeneity in sensitivity to ISS recommendations is the dimension that most explains mutual fund voting. The low observed sensitivity that we document among retail shareholders implies that the ideological space of retail shareholders likely differs substantially from those of mutual funds.

column (6), which includes account-firm fixed effects, shows that shareholders whose stake in a firm increases between meetings are no more likely to vote in support of management.

7.2. Proposal-level evidence

All regressions so far have focused on disaggregated account decisions, but we are also interested in aggregate proposal results and how they are associated with firm-level variables. We therefore aggregate retail votes up to the proposal level so that each meeting is weighted equally (rather than weighting each account-year equally), which also permits comparison with non-retail voting decisions. For a proposal p, we define the variable $WithMGMT_{pmct}$, as follows:

$$WithMGMT_{pmct} = \frac{\sum_a WithMGMT_{apmct}}{\sum_a WithMGMT_{apmct} + \sum_a AgainstMGMT_{apmct}}$$

It is the percent of votes that are cast as For votes (on management proposals) or Against votes (on shareholder proposals) out of the total votes cast For and Against, multiplied by 100. We estimate regressions of the form:

$$WithMGMT_{pmct} = \alpha + \beta_1 X_{mct} + \beta_2 Z_{pmct} + \theta_t + \psi_{PropCat} + \varepsilon_{pct} \tag{6}$$

where X_{mct} is a vector of firm-level variables and Z_{pmct} is a vector of proposal-level variables. $\psi_{PropCat}$ and θ_t are proposal category and year-month fixed effects. We weight observations so that each firm meeting is weighted equally and cluster at the firm-meeting level.

Table 10 reports regression results estimating Eq. (6). The first three columns provide results for institutions' voting, and the final three columns contain results for retail voting. Columns (1), (2), (4), and (5) include industry, proposal category, and year-month fixed effects, whereas columns (3) and (6) substitute firm fixed effects for industry fixed effects. The sample for these regressions includes director elections and say-on-pay votes, which dwarf other proposal types in frequency. Columns (1) and (4) of Table 10 show that for a firm with zero abnormal return and average values for all other variables, 88.3% of mutual fund shares and 89.3% of retail shares vote with management (as represented by the intercepts).

As before, we find that retail shareholders' voting decisions respond to firm performance. The estimates in column (4) show that an otherwise average firm experiencing a -37% abnormal return (roughly one standard deviation below zero) experiences retail opposition to management increasing from 10.7% (the intercept in column (4)) to 12.3%. By contrast, although mutual fund votes show some sensitivity to a firm's performance as measured by its return on assets, its overall sensitivity to performance is far less than that of retail voters once ISS recommendations are accounted for. The coefficients on ROA or Tobin's q are not significantly different between mutual funds and retail (F-stats 2.07 and 0.14), whereas the coefficients on firms' abnormal returns are highly significantly different (F-stat 76.34). These results are consistent with retail voters serving a monitoring role in poorly performing firms.

An even more striking difference is the sensitivity to proxy advisor opposition. ISS opposition to management is associated with a 1.8% difference in retail voting outcomes, but a 50.7% difference in institutional voting outcomes. To the extent that retail voters do not observe ISS recommendations prior to voting, the negative retail coefficient implies that retail shareholder voting decisions are likely driven by the same underlying factors that drive ISS recommendations. The larger magnitude of institutional investors' sensitivity to ISS recommendations reflects the strong influence of ISS recommendations on fund voting (Ertimur, Ferri, and Oesch (2013), Iliev and Lowry (2015), Malenko and Shen (2016)) and is also consistent with retail shareholders not having access to as accurate information as that gathered by ISS. We explore this difference more in Section 7.3 below.[18]

7.3. Retail and institutional voting

Table 10 reveals a noticeable difference between retail shareholders and institutions: a higher sensitivity to ISS recommendations for institutional investors, potentially caused by structural differences between the two groups, most notably institutional investors' potential for conflicts of interest in voting and their fiduciary duty to vote. It may also be a result of heterogeneity within each group, driven by the attributes of the investor's portfolio. For example, differential access to information or costs of monitoring portfolio firms, as well as variation in shareholder time horizons, preferences, and exposure to idiosyncratic risk, could each be associated with variation in voting choices. The differences between retail and institutional investors in average levels of these factors could produce differences in average voting choices.

Since our goal is to explain differential voting between retail shareholders and institutional shareholders, we consider three portfolio attributes that we can observe for our anonymized retail shareholders and for which there are large differences within and across retail and institutional shareholders: the portfolio's value, turnover ratio, and number of firms. Although these variables may not capture structural differences between retail and institutional shareholders—for example, no retail shareholders have a fiduciary duty to vote—they may be associated with variation in demand for or access to information or, alternatively, preferences related to risk exposure or time horizons. If shareholders with higher-valued portfolios have access to or greater willingness to pay

[18] Online Appendix Table A13 splits the sample by proposal type into four proposal categories consisting of director elections, say on pay, shareholder proposals, and other proposals. The results across proposal categories are broadly similar to those in Table 10, though director elections receive more support. Online Appendix Table A14 provides corresponding results comparing retail voting to that of the Big Three fund families.

for more granular information, and they interpret that information as funds would, then we may observe that larger retail investors resemble smaller funds in their voting behavior. Similarly, a shareholder with a high portfolio turnover may have a different investment horizon and may require access to higher frequency sources of information, and so differences in turnover ratios between retail and institutional investors may be driving the observed sensitivity to ISS. Finally, a shareholder with more firms in her portfolio may have less exposure to idiosyncratic risk and may not require detailed information on individual investments in her portfolio.

We estimate Eq. (5) as in Table 9, column (3), but with the following two modifications. We first allocate retail and institutional investors into bins by account value, account turnover, or account breadth. Account value bins correspond to one segment of the log 10 scale, turnover bins are five equally spaced bins on the unit interval, and log number of firms in an account's portfolio is spaced over the range [1,8). Second, for each of our right-hand side variables, we include the interaction of the variable with the account value bins, turnover ratio bins, or number of firm bins, respectively.

The estimated sensitivities of both retail and institutional investors to ISS recommendations across account, turnover, and breadth bins are presented in Fig. 5. The top panel of Fig. 5 provides the coefficients on ISS opposition to management interacted with each account value bin, the middle panel provides the coefficients on ISS opposition to management interacted with each turnover ratio bin, and the bottom panel provides the coefficients on ISS opposition to management interacted with each bin of log number of firms in the portfolio. Fig. 5 shows that retail shareholders, at any level of account value, turnover, or breadth are far less sensitive than institutions to ISS opposition to management—that is, the different sensitivity is not a product of these observable differences between the shareholders' portfolios.

It is plausible that as the value of an account's portfolio increases, the value of information to the shareholder would increase and she would invest in access to more accurate sources of information. As a result, retail shareholders with high account values may resemble institutions with similar account values in their demand for or access to more granular information on portfolio firms. However, to the extent that large retail accounts acquire additional information, our evidence shows that that such information does not lead these wealthier shareholders to vote more similarly with ISS recommendations. This may be because they do not, in fact, decide to acquire information; because any information that they do acquire is uncorrelated with that utilized by ISS; or because, to the extent the information they acquire is correlated with that utilized by ISS, they may interpret the information differently than institutions.

We note that retail shareholders also show a much greater sensitivity to recent performance than do funds, particularly to lagged yearly abnormal returns. Yearly abnormal returns may be a highly salient and cheap form of information for retail investors, in contrast to ISS recommendations, that institutions can pay to improve upon. We therefore ask whether retail shareholders whose portfolios resemble those of institutional investors may not have such sensitivity to past firm performance. We follow the analysis presented in Fig. 5 and report the coefficients on the lagged yearly abnormal return interacted with the quintile sorts by account value, portfolio turnover, and breadth. Online Appendix Fig. B2 shows that retail investors are far more sensitive to yearly returns than funds at any level of account value, turnover ratio, or log number of firms in the portfolio. Differences in account value, the extent of portfolio diversification, and turnover ratio do not appear to be driving the greater retail sensitivity to yearly returns. Retail and institutional shareholders appear to have deeper structural differences in their voting choices not captured by variation in these portfolio characteristics.

As discussed in Section 2.2, retail and institutional investors may have different incentives with respect to their voting choices. Although either type of shareholder may place weight on the social surplus generated by its investments, retail shareholders, investing for their own accounts, may place a greater relative weight on maximizing the returns of their portfolios than do institutional managers. Retail shareholders' apparent greater concern for the maximization of financial returns is consistent with the evidence in Table 4, Panel C, in which we find that the share-weighted retail support for SRI proposals is lower than that of the aggregate shareholder base. Online Appendix Fig. B3 provides a more direct comparison of retail and institutional voting on SRI proposals. We again sort retail shareholders by account value, and, limiting to SRI proposals, regress support for the proposal on the intercept for each account value sort. The analysis shows that it would be incorrect to interpret the retail shareholder lower support for SRI proposals as reflecting the preferences of a typical retail shareholder. Instead, we document substantial heterogeneity, with retail shareholders with small equity stakes providing stronger support for SRI proposals than institutions provide. It is the smaller number of larger retail shareholders with greater equity stakes that tend to oppose such proposals much more often than institutional shareholders, consistent with the notion that investors oppose SRI when it is their money at stake.

7.4. Support for management and exit

We next ask whether an account's voting decisions in a given year are associated with the decision to exit the following year—in particular, whether retail shareholders decide to exit from firms where they disagree with the existing management and board of directors. Such an exit would be consistent with the idea that retail shareholders actively monitor their portfolio investments.

To set up the analysis we first remove firm-years in which the firm is not in the data the following year, leaving most firms in 2015 and 2016 and none in 2017. Then, for each account-firm-year in which account a owns firm c in year t, we define the dependent variable $StillOwnNextYear_{act} = 1$ if account a still owns firm c in year $t + 1$. In total, 69% of account-firm-years own a firm in the following year conditional on the firm being in the data in the next year. As explanatory variables, we include whether or how the account voted at the firm's meeting in year t. We include the account's log stake value and the firm's institutional ownership, both measured at time *t*, as explanatory variables. Because the decision to sell is made after the meeting, we also include a set of performance-related firm-level variables measured at time *t+1*. All specifications include year-month, industry, and account-year fixed effects. Standard errors are clustered at the firm level.

We present the regression results in Table 11, column (1), by assessing the relationship between turnout and retention of one's stake. We find that an account is more likely to retain a security if it had participated in the voting process. The positive coefficient on Tobin's q indicates that accounts are more likely to keep their strong performers. Accounts tend to hold on to the largest portfolio firms and are much more likely to retain firms comprising their largest stakes.

We next ask whether voting choice (conditional on turnout) is associated with retention next year. We limit the analysis to account-firm-years that voted at time t. Aggregating across proposals, we measure an account's percent voting with management, $withMGMT_{at}$, as the account's number of votes in line with management recommendations divided by its number of votes cast during the firm's meeting in year *t*.

Table 11, columns (2)–(4), presents the regression results. From column (2), we see that accounts are more likely to retain securities for which they vote along with management. However, since accounts are more likely to oppose management on shareholder proposals than on management proposals, just using the overall *withMGMT* might create a bias for firms with more management proposals. We therefore further break retail support down for management proposals, defining $withMGMT_{at}^{PropCat}$ as an account's number of votes in line with management recommendations in a certain proposal category divided by its number of votes cast in that proposal category. We calculate this for management-sponsored proposals, shareholder-sponsored proposals, and certain subcategories of management-sponsored proposals: director proposals, say-on-pay-proposals, and other management-sponsored proposals.

In column (3), we separately estimate retention on support for management proposals and shareholder proposals. Within an account-year, the association between supporting management and retaining the stock is stronger with management proposals than with shareholder proposals; the F-stat and p-value for the difference are 11.6 and 0.001, respectively. In column (4), we further break management proposals into subcategories. Column (4) shows that it is not just any management proposals connected to exit—votes against directors are strongly predictive of exiting the firm. The difference between director proposals and other proposals are all highly significant (F-stats of 25.1, 16.9, and 31.0 as compared to shareholder proposals, say on pay proposals, and other management proposals, respectively).

While retail shareholders are more likely to retain securities that they vote on, they express their dissatisfaction by voting against directors prior to exiting. They are also more likely to exit when they disagree with management on shareholder proposals; that is, when there are shareholder proposals on the ballot that they support. These results are consistent with the evidence in Li, Maug, and Schwartz-Ziv (2019), who find that mutual funds are more likely to reduce their stake size if their votes are opposed to voting outcomes, and with the notion that retail shareholders endogenously select into firms based on agreement with management. The exit results in Table 11 may reflect trading at the extensive margin whereas the results in Table 9, showing a significant correlation between ownership of the firm and support for management, may reflect selective trading at the intensive margin. More generally, the evidence in this section is in line with Levit, Malenko, and Maug (2020), and Levit, Malenko, and Maug (2021), in whose model trading and voting decisions are interconnected.[19]

8. Conclusions

In this paper we study U.S. retail shareholder voting using a detailed sample of anonymized voting records over the period 2015-2017. We show that retail ownership is sizable: on average, aggregate share ownership by retail investors is 35–40% in small firms and 16% in large firms. We study the retail turnout decision and find that retail shareholders participate more when a portfolio firm underperforms or for special meetings, which serve as proxies for potential benefits of winning—especially when they own a larger portion of the firm. Holding constant the portion of the firm they own, accounts turn out more for higher-value investments, again consistent with a relationship between benefits from winning and turnout. We show that restrictions on a shareholder's access to her preferred voting method cause a large decrease in turnout. Furthermore, despite the lack of an obvious "civic duty" to vote in shareholder elections, we document non-zero turnout even for a shareholder with a very low stake in a firm and thus a negligible likelihood of being pivotal, consistent with consumption benefits from voting. This evidence is consistent with

[19] We have also looked at the effect of an "adverse voting result" on an account's propensity to retain the following year—for example, if the shareholder voted against a proposal that passed, or vice versa. For retail shareholders, these type of voting results on management proposals are highly correlated with opposition to management proposals, and adverse voting results on shareholder proposals are highly correlated with support for shareholder proposals, that it leads to extremely high standard errors with little power to model account exits.

findings in the political science literature in which turnout in presidential elections is substantial and varies with costs and benefits even for voters whose likelihood of being pivotal is low.

Conditional on the decision to turn out, we study how public information is incorporated into retail shareholder voting decisions. We find that retail shareholders punish the management of poorly performing firms. Retail shareholders are more supportive of incumbent management of firms in which they hold larger stakes, suggesting retail investors select into firms for which they approve of the management teams. This latter evidence is buttressed by our results on exit, in which we find that retail shareholders are more likely to exit after voting against incumbent management, especially in director elections.

The conventional wisdom on retail shareholders is that, lacking a large stake, they have little incentive to monitor firms. To date, however, there has never been an opportunity to empirically test the conventional wisdom. Some predictions are borne out by our data—retail shareholder participation is quite low compared to non-retail shareholders, who generally have some legal obligation to vote on behalf of their clients. On the other hand, we find that retail shareholders appear most likely to vote when monitoring is most needed, and their votes are informed by firm circumstances. Ultimately, we conclude that in contrast to the common caricature of retail shareholder voting as arbitrary and inconsequential, these investors can and do provide meaningful feedback to firms through the voting process.

Appendix A. Details on data and variable construction

A.1 Data sources and construction of variables

For securities data, we use data from the Center for Research in Security Prices (CRSP). For each month t, we calculate a firm's lagged annual return for the one-year period ending in month $t - 1$ by compounding one-month holding period returns over the 12-month period. We calculate annual abnormal returns for that same period as the annual return minus the value-weighted annual return from CRSP. The variable yearly abnormal return used in our analyses is the buy-and-hold abnormal return measured as of one month prior to the record date. The one-year dividend yield is calculated as the difference between the buy-and-hold return including dividends and the buy-and-hold return excluding dividends. The difference between returns including and excluding dividends used to compute the dividend yield is described on the CRSP website as the "Income Return," and is available at: http://www.crsp.org/products/documentation/crsp-calculations. Market equity is computed as price time shares outstanding from CRSP, measured as of the record date month. Some of the descriptive statistics and analyses rely on allocating firms into market capitalization quintiles. The market capitalization breakpoints we use for these sorts are from Ken French's website at Tuck School of Business at Dartmouth College: http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.

We calculate book equity as the difference between stockholders' equity and preferred stockholders' equity, with certain substitutions in the case of missing variables, as described in Daniel and Titman (2016). We slightly alter the code provided on the WRDS website, available at https://wrds-www.wharton.upenn.edu/pages/support/applications/risk-and-valuation-measures/market-book-mb-ratio. Stockholders' equity uses Compustat variable SEQ or, if it is missing, the sum of Total Common Equity (CEQ) and Preferred Stock Par Value (PSTK) or, if either of those are missing, total assets (AT) minus liabilities (LT) minus minority interest (MIB). Book equity is defined as (i) stockholder's equity, minus (ii) preferred stockholder's equity, which is equal to preferred stock redemption value (PSTKRV) or, if missing, preferred stock liquidating value (PSTKL) or, if missing, preferred stock carrying value (PSTK), plus (iii) if not missing, balanced sheet deferred taxes (TXDITC), minus (iv) if not missing, the FASB106 adjustment (PRBA from the Compustat Pension Annual dataset).

Tobin's q is the ratio of market value of assets to book value of assets (AT), where the market value of assets is defined as the sum of book value of assets (AT) and the market equity minus the book equity, as in Bhojraj et al. (2017). ROA is the ratio of EBIDTA to assets (AT), as in Brav et al. (2020). We winsorize Tobin's q, ROA, and dividend yield at the 1% and 99% levels. We also use Total q from Peters and Taylor (2016), available on WRDS. Bartlett and Partnoy (2018) argue that Tobin's q is not adopted properly in empirical work and recommend as a potential alternative the Total q from Peters and Taylor (2016). We repeat the analyses in Sections 6 and 7 using Total q instead of Tobin's q, and find substantially similar results. See Online Appendix Table A16.

We obtain county vote totals for the 2016 presidential election from CQ Voting and Elections and the count of voting eligible adult population from the Census Bureau available at: https://www.census.gov/programs-surveys/decennial-census/about/voting-rights/cvap.2016.html. We compute the 2016 county presidential turnout as the number of county residents who cast ballots in the 2016 U.S. presidential election divided by the number of adult citizens. For county level variables, we merge to zip codes using the USPS ZIP Code Crosswalk files from the Housing and Urban Development's Office of Policy Development and Research (PD&R). Zip code-level demographic information is from the Census Bureau. Fraction over 65 is the fraction of zip code residents above age 65, defined as (DPSF0010015 + DPSF0010016 + DPSF0010017 + DPSF0010018 + DPSF0010019) / DPSF0010001). Density is calculated as the zip code population divided by land area in square meters, (DPSF0010001 / AREALAND). Fraction with bachelors and fraction with post-bachelors are zip-code level five-year averages from the U.S. Census as of 2017. Zip code employment is from the Bureau of Labor Statistics. We construct the variable fraction in Finance/Insurance by dividing the number of employed workers in Finance/Insurance by all-industries employment, both at the zip code level. Adjusted gross income data at the zip code level is from the IRS website available at: https://www.irs.gov/statistics/soi-tax-stats-individual-income-tax-statistics-zip-code-data-soi. The variable we use in our analyses denoted Zip code AGI is the average adjusted gross income in the prior calendar year in the account's zip code.

Information on whether a meeting was regular annual meeting or a special meeting is from ISS Voting Analytics. We construct an indicator variable equal to one for special meetings. The recommendation on the proposal by the proxy advisory firm Institutional Shareholder Services (ISS) is from ISS Voting Analytics. The variable ISS against management as a binary variable that equals one if ISS has a recommendation other than "For" for a management proposal, or a "For" recommendation on a shareholder proposal. Institutional ownership is equal to the number of shares owned by institutions divided by the shares outstanding in the year prior to the meeting, both from Thomson Reuters.

Table A1 below provides descriptive statistics on variables used in our analyses. Firm-meeting variables are weighted at the firm-meeting level; firm-proposal variables are weighted at the firm-proposal level; zip code variables are weighted at the account level.

Table A1. Descriptive statistics

	Average	Stdev.	25th perc.	Median	75th perc.
Firm-meeting variables					
Market equity (millions)	7,461	28,383	199	962	3,846
Yearly abnormal return	-0.02	0.37	-0.22	-0.02	0.17
Dividend Yield Binary	0.48	0.50			
Tobin's q	1.96	1.44	1.06	1.43	2.23
ROA	0.03	0.24	0.02	0.08	0.14
Special meeting	0.08	0.27			
Institutional Ownership	0.69	0.29	0.51	0.77	0.91
Firm-proposal variables					
ISS against management	0.10	0.31			
Zip code variables					
2016 presidential turnout	0.63	0.08	0.58	0.63	0.68
Zip code income	11.35	0.56	10.95	11.26	11.64
Over 65	0.14	0.07	0.10	0.13	0.16
Density	1,829	4,898	224	716	1,509
Portion with Bachelors	0.45	0.19	0.30	0.44	0.59
Portion with Post-Bachelors	0.19	0.11	0.10	0.17	0.25
Portion in Finance/Insurance	0.02	0.01	0.01	0.02	0.03

A.2. Matching of retail voting sample to ISS Voting Analytics

A2.1. Overview

To combine the proposals in the ISS Voting Analytics database with those in the retail shareholder set, we merge the ISS Voting Analytics database at the meeting level with the retail shareholder data by 6-digit CUSIP, meeting date, and record date.

We merge at the proposal level using the order of the proposals within a meeting and their textual descriptions from the retail shareholder voting data and ISS Voting Analytics, which we describe in greater detail in Appendix A2.2. Within matched meetings, the retail voting sample and ISS Voting Analytics have roughly identical proposal slates, with one important exception: for 72% of meetings with director elections, the retail voting dataset reports the number of returned votes on the director elections but not the choices on individual directors. As a result, we exclude these director election proposals from analyses of substantive voting decisions. The remaining minor inconsistencies are in how the two sources treat withdrawn proposals, other minor items that appear on the proxy ballot (for example, some ballots include checkboxes for shareholders to indicate that they have no conflict of interest), as well as a handful of proposals that appear to be erroneously missing from ISS Voting Analytics. Appendix A.3 provides additional information regarding erroneous ISS Voting Analytics data that we corrected in the course of matching the retail voting proposal data to ISS Voting Analytics.

Next, we merge additional proposal-level information from SharkRepellent. Unlike ISS Voting Analytics and the retail voting data, SharkRepellent proposals are unordered, so we match proposals by voting results and, using text matching, by proposal categories. We merge the retail shareholder voting data with CRSP at the 6-digit CUSIP-month level, matching the record date month in the shareholder voting data to the data month for CRSP. We restrict the analysis to firms in CRSP with share codes 10 or 11 and with a valid share price and shares outstanding information as of the record month. Following the merging with CRSP and ISS Voting Analytics, the final dataset has 4,725,390,872 account-proposal level observations.

Using linking procedures from the Compustat/CRSP Merged Dataset, which links Compustat gvkeys to CRSP permnos, we merge at the firm level with Compustat, so that each meeting merges with the last Compustat fiscal year that ended on or before the record date. We merge the IRS zip code income data with the retail shareholder data at the zip code-calendar year level, lagging the zip code data by one year.

Online Appendix Table A3, Panel A describes the portion of firms in the retail sample that we are able to match to CRSP in each year of the three-year sample period. We achieve coverage of 86% in 2015, 89% in 2016, and 90% in 2017, with higher coverage for larger firms, based on NYSE size quintiles. Panel B describes coverage of the retail shareholder voting data of the intersection of ISS Voting Analytics and CRSP. The overall coverage is high, at 96% in 2015, 99% in 2016, and 98% in 2017, with higher coverage for larger firms. Of the firms in CRSP that do not match to our retail voting data, most are small firms that also do not appear in ISS Voting Analytics; many of the remainder are unmatched because our retail data exclude meetings with record dates in 2014. We also report on institutional ownership in the firms covered in the retail data. To this end, we merge the retail voting data with institutional 13F ownership data from Thomson Reuters at the 6-digit CUSIP-year level, lagged by one year. Online Appendix Table A3, Panel C provides the coverage of retail voting firms in CRSP by institutional ownership quintile.

For the subset of firms in CRSP that also appear in the Thomson Reuters 13F data, we achieve coverage of 95%, 96%, and 97%, respectively, in each of 2015–2017.

A.2.2. Details on merging

This section of the appendix provides the methodology for the proposal-level merger of the ISS Voting Analytics and retail voting datasets. The two datasets include slightly different samples of firms: of the 7,606 unique 6-digit CUSIPs in ISS Voting Analytics and the 6,782 unique 6-digit CUSIPs in the retail voting data, 5,849 are in both. Nearly all of the 1,757 firms that appear in only ISS Voting Analytics are investment funds. Nearly all of the 933 firms that appear only in the retail voting data are non-public firms.

The retail voting sample data come in the form of two separate datasets: one at the firm-meeting-account level, in which each row contains a string of votes representing the votes of an account for all proposals at that meeting (or is blank, if the account did not vote); and one at the proposal level, in which each row contains the text of a single proposal at a meeting. Both datasets include meetings from 2015 to 2017, although the proposal-level set excludes meetings with 2014 record dates. The string of shareholder votes in the retail voting data is ordered as the proposals appear on the ballot; however, meetings vary in their numbering (some are numbered, some are lettered, some have roman numerals or identifying tags). The retail voting dataset lacks any proposal-level identifying information other than the within-meeting order of votes. Thus, the proposal-level merge between the retail voting data and ISS Voting Analytics requires a three-way merge between retail voting data, retail proposal data, and ISS Voting Analytics.

We begin by ordering the proposals in the retail proposal set so that they properly reflect the actual ballot order. From 90,964 proposals spanning 17,937 meetings in the original retail proposal set, there are 90,787 remaining once we remove proposal slates which are overall duplicates in CUSIP, meeting date, record date, proposal text and number of proposals (we retain one of the proposal slates). We remove meetings from the retail proposal set for which the meeting ID does not appear in the retail voting dataset. Following this step, we match these retail proposals to ISS Voting Analytics prior to matching to the other retail voting dataset so that we can use this match to correct any mis-orderings that remain.

Meetings with multiple types of securities or multiple share classes may have different slates of proposals. For example, preferred stockholders may elect a different set of directors but otherwise vote for the same ballot items as common stockholders. Meetings in ISS Voting Analytics and the retail voting sample are defined slightly differently when there are multiple proposal slates. ISS Voting Analytics treats different proposal slates as separate meetings; the retail voting dataset labels the slates differently within the same meeting. Thus, a proposal that is voted on as part of two different proposal slates will appear as a duplicate. For consistency, we adopt the convention of reporting as a "meeting" a unique CUSIP-meeting date-record date.

To match proposals across the ISS Voting Analytics and retail voting data, we begin by matching meetings by 6-digit CUSIP, meeting date, and record date. Of 18,925 meetings in the ISS Voting Analytics set (of which 15,549 have CUSIPs which appear in the retail voting sample) and 17,731 meetings in the retail voting data (of which 15,683 have CUSIPs which appear in ISS Voting Analytics), 14,587 meetings are in both datasets. There are several hundred meetings which match by CUSIP but not by meeting date and record date. Many appear to be due to simple discrepancies in the record date between the datasets, whereas others may be due to incorrect CUSIP matches. Finally, 89 are due to the fact that proxy contests are not in the retail voting data.

Because ISS Voting Analytics lists multiple proposal slates as separate meetings, for the 622 cases in which ISS Voting Analytics has multiple meetings by the same firm on the same day (166 of which are in the retail voting data), we separately hand-match their proposals to proposals from corresponding meetings. We also hand-match the 21 additional meetings with multiple profiles that are in ISS Voting Analytics but not in the previous group of 622.

Next, for all of the remaining meetings, we match using the number of proposals at the meeting and the order of proposals. In both datasets, proposals within a meeting appear in the order in which they appear on the ballot. However, various discrepancies arise between the two datasets, in which both do not include precisely the same proposals in precisely the same order. Sources for these discrepancies include: (i) the retail voting data frequently condense multiple director election proposals into a single row with proposal text "#DIRECTOR" rather than a separate proposal for each director with the actual proposal text; (ii) the retail voting proposals are ordered unsystematically, with a mix of lexicographic and other kinds of ordering; (iii) there are some proposals about which the firms take different approaches, such as proposals to permit "other business," check boxes to indicate whether the voter has a conflict of interest in the vote, and withdrawn proposals; (iv) ISS Voting Analytics is missing several hundred proposals from its dataset, apparently erroneously (in such cases, the proposals are apparently numbered properly within ISS Voting Analytics but one of the numbers is missing); and (v) for many meetings, ISS Voting Analytics, apparently erroneously, lists each proposal twice.

To deal with these issues, for those meetings matched on CUSIP, meeting date, and record date, we provisionally match their constituent proposals in order, then use additional factors to properly merge the datasets proposal by proposal, including the proposal's text description given in each dataset. ISS Voting Analytics proposals have a brief item description of the proposal produced by ISS Voting Analytics. For each proposal, the retail voting data have the first several hundred characters of the proposal text directly from the proxy statement. Starting from our match at the meeting level, we match at the proposal level in a series of stages. If two matched meetings have the same number of proposals, then we provisionally match the proposals in order. Because both ISS Voting Analytics and the retail voting data list their proposals in the order they appear on the proxy ballot, this should accurately match the two in most cases. As an added check, we conduct a text match to flag potentially mismatched proposals, that we later hand-check.

Our text match is designed as follows. First, for each pair of meetings that are matched by 6-digit CUSIP, record date, meeting date, and number of proposals, we calculate the string distance between the text description for all combinations of each of the ISS Voting Analytics proposals and each of the retail voting proposals within the matched meeting. The string distance we use is the Jaccard distance, which is the number of shared 5-character strings divided by the total number of 5-character strings. This *generates*, for a meeting with *n* proposals, an *n*x*n* matrix of Jaccard distances, in which (*j,k*) represents the ISS Voting Analytics proposal in the *j*'th spot's distance from the retail dataset proposal in the *k*'th spot, and in which the diagonal represents the distances from the proposals "across from them" in the other dataset. We calculate a score for the meeting as the ratio of the sum of the lowest alternative row or column versus the sum of the diagonal, where a score of 1 indicates that each of the proposals match up better to the proposals across from them in the provisional match than they do to any other proposal in the meeting. For those meetings with scores below 0.99 or flagged for another reason, we check all proposals in the meeting by hand. Matches may be flagged if either (i) there is only one proposal in the meeting, but the proposal text in the retail data is not "#DIRECTOR," or (ii) there are multiple ISS Voting Analytics proposals with "Elect Director" in the item description but one of the retail proposal texts is "#DIRECTOR", implying that director elections for that meeting were condensed in the retail data.

If an ISS Voting Analytics meeting and a retail dataset meeting that matched on CUSIP and meeting and record date do not have the same number of proposals, then, since the most likely reason is that the retail dataset frequently condenses multiple director elections into a single "#DIRECTOR" proposal, we similarly "condense" the ISS Voting Analytics meeting by removing all but one "Elect Director" proposal. If, after this process, the two matched meetings have the same number of proposals, then we repeat the process described above: we provisionally merge each "condensed" ISS Voting Analytics meeting to its corresponding retail dataset meeting on number of proposals, and, if they match, generate a match score and hand-check those with scores below 0.99 or flagged for another reason as described above. If matched meetings still have a different number of proposals, then we manually hand-match their proposals.

Following this process, from the original 14,587 matched meetings we manually hand-match the proposals at 303 meetings (2,112 proposals), for which we find a match from the retail dataset to ISS Voting Analytics on at least one proposal for 301 meetings (1,919 matched proposals). These are cases in which ISS Voting Analytics has duplicate meetings on the same day or the ISS Voting Analytics and retail dataset meetings do not have the same number of proposals even after condensing. We hand-check the proposals for 760 meetings (3,217 proposals) in which the number of proposals is the same, but the match score is below 0.99, or they are flagged for other reasons, for which we find a match from the retail dataset to ISS Voting Analytics on at least one proposal for 759 meetings (3,215 proposals). We algorithmically match, and do not further check, the proposals at 13,524 meetings (68,048 proposals). Proposals that are algorithmically matched belong to meetings that match on CUSIP, meeting date, meeting day, and number of proposals, have a text match score greater than or equal to 0.99 on the ISS Voting Analytics Item Description and retail proposal text. Last, we remove three meetings because we cannot confirm from their constituent proposals that the meetings themselves were correct matches.

As a final check on our matching process, we verify with the subset of hand-checked meetings that the match score we generate is a strong predictor of proper matching and that scores above 0.99 have a low chance of being incorrectly matched. For the 593 hand-checked proposals with match scores below 0.95, just 170 (28.7%) were properly provisionally matched, but for the 2,350 proposals with scores between 0.95 and 0.99, 2,346 (99.8%) were properly provisionally matched. An additional 274 proposals had scores above 0.99 but were flagged for other reasons; 270 (98.5%) of these were properly provisionally matched. Finally, we also hand-checked 1617 proposals that were not flagged for any reason; all were properly provisionally matched.

The merge of the retail proposal dataset with ISS Voting Analytics generally confirms the proper order of the retail proposals and permits a merge to the retail voting dataset. For those that we hand-code, we also use the manually-checked original retail proposal order and re-order appropriately to ensure that we can properly merge with the retail voting dataset. We then merge the combined ISS Voting Analytics-retail proposals set with the retail voting dataset. Starting with 89,850 proposals in the original retail proposal set, we remove 78 that are duplicates, leaving 89,772 proposals. There are 89,652 proposals remaining once we remove proposal slates which are duplicates in CUSIP, meeting or record date, and number of proposals, but which are not identical in proposal text (we remove all copies of such proposal slates, since we have no way to properly identify them). Of these, 89,571 proposals properly match to the retail voting set by

CUSIP, meeting date, record date, number of proposals at the meeting, and sequence number. 73,084 of these proposals (14,578 meetings) match to ISS Voting Analytics.

We run two additional checks using variables that we did not use for our merges. First, although the retail voting dataset has no identifying information to distinguish proposals at a meeting other than the votes themselves, votes on the annual frequency of say on pay are uniquely distinguishable from other votes using the retail voting dataset because the votes are 1's, 2's, and 3's instead of For's or Against's. Of 2,483 proposals for which the retail voting dataset votes are 1's, 2's, and 3's and for which there was a meeting match to ISS Voting Analytics, 2,479 were properly matched to a retail proposal dataset frequency of say-on-pay proposal, a success rate of 99.8%. Second, both the retail voting dataset and the ISS Voting Analytics dataset include proposal-level management recommendations, so we can use these to cross-compare our results. Of 73,084 proposals, the management recommendations differ in 70. From spot-checking, these appear to be cases in which the proposals are properly matched but the ISS and retail datasets differ in their recorded management recommendations (generally because the proposal was withdrawn).

We subsequently merge this sample with CRSP, leaving 54,876 proposals. We then merge with SharkRepellent to correct certain ISS Voting Analytics numbers (as reported in Appendix A.3 below), though we do not drop observations that do not match to SharkRepellent. We hand-correct 42 entries where both ISS Voting Analytics and SharkRepellent incorrectly report 0 votes For and Against. We drop proposals where the number of votes outstanding is reported incorrectly and cannot be corrected, where no votes are reported (almost always where the firm did not report the results of that proposal in the original 8-K or the proposal was withdrawn prior to voting), and where For votes were reported but not Against, leaving a final sample of 53,952 proposals.

A.3 Correction to ISS Voting Analytics information and match to Shark Repellent

In the course of matching the retail voting proposal dataset to ISS Voting Analytics, we found that ISS Voting Analytics reports erroneous numbers of outstanding shares and vote counts in a portion of its observations. This error affects observations in 2017. In this subsection, we describe how we correct these erroneous entries.

For all meetings in year 2017 for fields with more than 9 digits for outstanding shares, votes for, votes against, votes abstained, or say on pay frequency votes, we find that ISS Voting Analytics dataset cuts off the final digits of the number. For example, a share count of '123,456,789' would be reported in ISS Voting Analytics as '12,345,678.' We correct the errors using data from SharkRepellent, which contains information on outstanding shares, votes for, votes against, votes abstained, and say on pay frequency votes. We first match SharkRepellent to ISS Voting Analytics at the meeting level (by CUSIP, record date, and meeting date) and proposal level (by votes for, votes against, and votes abstained).

For those observations that do not match with SharkRepellent and are candidates to have digits cut off, we identify observations in 2017 that ISS Voting Analytics report as having 8 digits and CRSP reports at least 80,000,000 outstanding shares, and we hand-code the correct numbers using public filings. For a small handful of observations where (i) we do not have shares outstanding numbers from SharkRepellent and (ii) shares outstanding from the record date month from CRSP is approximately 100 or 1,000 times the ISS Voting Analytics number, we multiply the ISS Voting Analytics number by 100 or 1,000 to reach an approximate number.

In total, we correct 20,037 entries across 11,629 proposals with digits cut off, inappropriate zeros, or other inconsistencies. We also run further diagnostics to confirm that ISS Voting Analytics numbers are accurate other than the issue described here. Note that we choose to continue to use the ISS Voting Analytics proposal data rather than SharkRepellent despite the errors because it can be matched at the proposal level with the voting data, as detailed in Appendix A2.2, whereas SharkRepellent cannot be, and ISS Voting Analytics has larger coverage.

A.4 Construction of the mutual fund voting records

We use four data sources to form the mutual fund voting dataset. For fund voting information, we use filings on Form N-PX filed with the SEC. Form N-PX is required of all registered management investment companies. We obtain Form N-PX via the Mutual Fund Vote Records dataset within the ISS Voting Analytics Database. The dataset contains the voting decision for each fund on each proposal for each firm that it owns. We also use the CRSP US Mutual Fund Database, which contains whether a fund is index-based or not and the 13F institutional share ownership via the Thomson Reuters S12 dataset, which indicates the ownership of each firm by each fund among 13F filers at the time of the quarterly filing. The fourth dataset is the WRDS Mutual Fund Links (MFLINKS), which is designed to link funds in the CRSP Mutual Fund dataset to the Thomson Reuters Mutual Funds (S12) dataset.

The ISS Mutual Fund Vote Records cannot be directly merged to the Thomson Reuters S12 share ownership dataset since the two have no shared identifier and they write fund names differently. We therefore construct our dataset as follows. First, we use text matching to match ISS Mutual Fund Vote Records with the CRSP Mutual Fund dataset by fund name. Of the 11,208 unique funds in the ISS dataset, we match 452 (4.03%) to CRSP using exact text matching. We then use the STATA *matchit* function for fuzzy matching, hand-check each match, and conduct additional hand-coding. Following this process, we match 9,244 (82.48%) funds to CRSP.

Next, starting from the CRSP dataset, we use the MFLINKS dataset to match each CRSP fund to a linking identifier by fund. We use that linking identifier to match to Thomson S12 by fund. Finally, we match the ISS Mutual Fund Vote Records to the Thomson Reuters S12 via our links. We match by fund, firm CUSIP, and date. For ISS, the relevant date is the record date of the meeting; for Thomson Reuters, the relevant date is the report date of the 13F filing. We limit to matches in which the record date of the meeting is within 180 days of the record date of the 13F filing. Generally, there is more than one 13F record date within 180 days of a meeting record date for a given fund and firm; we keep only the 13F closest chronologically to the meeting record date.

In total, the ISS Voting Analytics Database contains 15.7 million unique fund proposals that match to proposals in our dataset. Of those, 13.5 million (85.9%) match to a CRSP fund, and 8.7 million (55.4%) match to a Thomson Reuters S12 fund-firm 13F filing, where the record date of the meeting is within 180 days of the record date of the 13F filing.

In our counts of mutual fund share-weighted votes, we only include votes from Form N-PX where we can retrieve the shares held by the fund from the CRSP Mutual Fund Dataset. Thus, we somewhat underestimate the ownership by mutual funds. The term Big Three refers to Vanguard, Blackrock, and State Street. For the proposals in this sample, 78% of Big Three funds (and 70% of Blackrock funds) for which there is N-PX voting data are matched to share numbers from Thomson Reuters; this is likely an underestimate of our true coverage of Big Three ownership, since the larger funds are more likely to have matches in CRSP and Thomson Reuters.

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Table 1. Retail investor characteristics

This table reports information on retail investors covered in the retail dataset. Retail characteristics are generated as follows: first, for each firm meeting, we use each account's holdings on the record date as a "snapshot" of that account's yearly holdings in the firm. We keep only one meeting of each firm per year. Second, for each account, we aggregate the holdings in the portfolio at the account-year level. Number of firms in portfolio is defined as the number of firms in a given year for which the account holds shares on the firm's record date. Account value is defined as the sum of an account's individual firm stake values, where individual stake values are calculated as the product of the number of shares in the firm held by the account and the price of the stock at the end of the record date month, as provided by CRSP. Dividend yield is defined as the difference between the firm's buy-and-hold return with dividends and without dividends. The account-year-level composite dividend yield is calculated as the account's dividends received summed over the firms held by that account divided by the account's total portfolio value. Market abnormal return for an account is calculated as the buy-and-hold abnormal return, using the CRSP value weighted index return as a benchmark, on the securities in the account, assuming the account held all securities for the past year. Firm purchase rate and sale rate are the portion of portfolio firms that have been added or removed in the past year, respectively. To evaluate characteristics of the home area of the accounts in the sample, we obtain adjusted gross income data at the zip code level from the IRS website. Zip code mean AGI refers to the mean adjusted gross income in the account's zip code. Voting rate is defined as the number of ballots cast divided by number of voting opportunities. Panel A includes summary statistics by year. In panel B we first average each account value over its years in the data and then sort accounts into quintiles by account value. We then report the average of each of the characteristics described in panel A.

Panel A: Retail investor characteristics by year

	2015			**2016**			**2017**		
	Avg.	Med.	Stdev.	Avg.	Med.	Stdev.	Avg.	Med.	Stdev.
Num. of firms in portfolio	4.01	2.00	6.94	4.16	2.00	7.18	4.23	2.00	7.71
Account value	126,740	13,804	7,968,903	122,556	12,979	6,870,664	132,087	13,717	8,136,010
Dividend yield	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02
Market abnormal return	0.00	0.00	0.23	0.00	0.02	0.28	0.02	-0.03	0.33
Zip code mean AGI	102,502	76,596	87,867	105,404	79,198	89,372	104,148	79,925	83,580

Panel B: Average retail investor characteristics by account value

	Account value quintile				
	Smallest	2	3	4	Largest
Num. of firms in portfolio	1.47	1.88	2.54	4.20	9.16
Account value	588	4,077	13,131	39,814	649,064
Dividend yield	0.01	0.02	0.02	0.02	0.02
Market abnormal return	-0.03	0.00	0.02	0.02	0.02
Firm purchase rate	0.43	0.40	0.38	0.36	0.33
Firm sale rate	0.34	0.36	0.36	0.35	0.33
Zip code income	89,326	96,830	101,238	106,746	123,510
Voting rate	0.03	0.05	0.07	0.09	0.15

Table 2. Retail investor ownership characteristics

This table reports information on ownership characteristics by retail shareholders. The sample is limited to proposals in the retail dataset that are matched with data from ISS Voting Analytics and CRSP. Panel A provides information on the number of investors and aggregate retail ownership for the full sample and across firm size quintiles. Panel B provides information on the distribution of individual retail shareholders' equity stakes relative to the company's shares outstanding. For each firm size quintile and for the full sample, we determine the average retail stake size, as well as the 25th, 50th, 75th percentiles. Firm size is calculated as the product of CRSP variables csho and prc, and quintiles are determined using the NYSE size breakpoints from Ken French's website. "# Investors" refers to the number of retail investors in the sample, in thousands, who own shares in the firm. "Retail Ownership" is the percentage of outstanding shares of the firm held by domestic retail investors in the sample.

Panel A: Number of accounts and aggregate ownership

	2015				2016				2017			
	# Investors (thousands)		Retail ownership (%)		# Investors (thousands)		Retail ownership (%)		# Investors (thousands)		Retail ownership (%)	
Firm size quintile:	Avg.	Median	Avg.	Median	Avg.	Median	Avg.	Median	Avg.	Median	Avg.	Median
Smallest	4	2	40	34	4	2	39	34	5	2	35	33
2	8	4	18	14	10	5	19	15	10	5	17	14
3	16	9	15	12	16	9	15	12	17	9	15	11
4	31	19	14	12	30	18	13	11	34	21	14	11
Largest	267	110	16	15	286	118	16	15	297	125	16	14
Full Sample	35	5	28	20	38	5	27	19	39	5	25	20

Panel B: Distribution of retail stake as a fraction of outstanding shares (in millionths)

	2015				2016				2017			
Firm size quintile:	Avg.	25th	Median	75th	Avg.	25th	Median	75th	Avg.	25th	Median	75th
Smallest	84.17	1.10	5.50	22.49	74.71	0.94	5.09	22.06	72.54	0.53	3.40	16.53
2	21.10	0.68	2.51	7.80	18.00	0.29	1.59	5.96	15.93	0.26	1.38	5.27
3	10.03	0.35	1.32	3.90	9.27	0.26	1.04	3.37	8.35	0.20	0.98	3.26
4	5.48	0.20	0.69	1.89	3.34	0.17	0.61	1.75	5.56	0.16	0.55	1.60
Largest	0.61	0.03	0.08	0.26	0.58	0.02	0.08	0.26	0.53	0.02	0.07	0.23
Full Sample	6.27	0.03	0.13	0.58	5.42	0.03	0.13	0.54	6.11	0.03	0.12	0.52

Table 3. Retail voting by meeting

This table reports voting results at the ballot level. % Cast is the proportion of ballots cast as a fraction of the number of shares outstanding. % Voting only with management refers to ballots that entirely match management recommendations. % at least one against management refers to ballots with at least one vote that deviates from management's recommendation. The columns with header "Retail votes" are at the shareholder vote level while the columns with header "Retail account" are at the retail account level, where each account is weighted equally.

	Retail votes			**Retail accounts**		
	% Cast	% Shares voting only with mgmt.	% At least one against mgmt.	% Cast	% accounts voting only with mgmt.	% At least one against mgmt.
All meetings	32	76	24	11	59	41
Annual meeting	32	76	24	11	58	42
Special meeting	38	79	21	15	74	26

Table 4. Retail voting and meeting proposals

This table reports information on retail voting limiting the sample to retail dataset proposals that are matched with data from ISS Voting Analytics and CRSP. Each entry represents the average of all firm votes in the category. "All votes" contains the overall voting results from ISS Voting Analytics, with corrections from SharkRepellent and CRSP, as described in Appendix A.3. "Retail Votes" contains domestic retail voting results. "Retail accounts" weighs domestic retail voting results at the account level. "Cast (%)" refers to the sum of the number of votes cast for and against divided by the number of potential votes as reported by ISS Voting Analytics. For and against votes exclude say-on-pay frequency votes and certain director votes for which the only retail voting data is on the number of votes cast. "For (%)" is the number of votes for divided by the number of votes cast. Panel A shows voting sorted by the identity of the sponsor, management or shareholder. Panel B shows voting by sponsor and firm size quintile. Panel C shows retail voting by proposal categories. Panel D shows voting sorted by sponsor and management and ISS recommendations.

Panel A: Retail voting by proposal sponsor

	All votes		**Retail votes**		**Retail accounts**	
	Cast (%)	For (%)	Cast (%)	For (%)	Cast (%)	For (%)
All	79	93	31	91	11	87
Management	79	95	31	93	11	89
Shareholder	75	30	28	18	11	29

Panel B: Retail voting by firm size quintile

	All votes		**Retail votes**		**Retail accounts**	
	Cast (%)	For (%)	Cast (%)	For (%)	Cast (%)	For (%)
Management sponsored:						
Size quintile:						
Smallest	73	93	36	90	12	84
2	83	95	31	94	11	88
3	83	96	29	94	11	89
4	83	96	28	95	11	91
Largest	78	97	27	95	11	92
Shareholder sponsored:						
Size quintile:						
Smallest	70	45	43	39	12	46
2	81	47	35	26	10	40
3	82	38	29	22	12	33
4	79	36	28	22	11	32
Largest	74	27	27	15	11	26

Panel C: Retail voting by proposal category

	All votes		Retail votes		Retail accounts	
	Cast (%)	For (%)	Cast (%)	For (%)	Cast (%)	For (%)
Management:						
Elect director	78	97	29	95	11	93
Financial statements/Auditor	87	99	32	97	11	95
Governance - board and shareholder rights	77	94	33	92	12	88
Governance - compensation	74	90	32	87	11	76
Governance - other	77	91	40	90	14	84
Major transactions - issuance, buyback, distribution, stock split, or conversion	72	89	32	83	11	74
Major transactions - M&A	77	98	46	94	18	91
Other	78	82	34	89	12	87
Shareholder:						
Environmental	73	23	26	13	12	24
Social	74	19	27	15	11	27
Governance	77	38	29	21	11	31

Panel D: Retail voting by management and ISS recommendations

	All votes		Retail votes		Retail accounts	
	Cast (%)	For (%)	Cast (%)	For (%)	Cast (%)	For (%)
Management-sponsored:						
Management For & ISS For	79	97	30	94	11	89
Management For & ISS Against	72	76	34	87	10	80
Shareholder-sponsored:						
Management Against & ISS For	76	36	28	17	11	28
Management Against & ISS Against	73	8	26	14	12	25

Table 5. Impact of retail voting

This table describes changes in voting outcomes under hypothetical changes in both the decision to vote, changes in retail ownership, and the voting preferences of certain groups of shareholders. Panel A provides the number of proposals whose outcome would change if a voting group's participation were set to zero. The sample consists only of proposals for which the voting base is the number of votes cast rather than the number of outstanding shares. We exclude routine proposals including auditor ratification and meeting adjournments, as well as director elections. Each row in Panel A designates a voting group whose participation is set to zero in the hypothetical. Columns (3) (and (4) reflect the number of proposals flipped under the hypothetical, and columns (5), (6), and (7) provide the number of proposals whose final percentage counts move by five, ten, and twenty percent, respectively. Panel B provides the number of proposals whose outcome would change if ownership were shifted between retail and non-retail shareholders. We use the same sample as in Panel A and change retail ownership by 18.4% which is the standard deviation of retail ownership of all firms in the sample. Firms are sorted into quintiles of retail ownership and we ask how an increase (decrease) in ownership for firms in the bottom, second, and third (third, fourth, and largest size) quintile impacts vote outcomes. We report the consequences of these ownership changes separately for management and shareholder proposals. In Panel C we hold fixed observed shareholder participation and report the number of proposals whose voting outcome would change if a voting group's preferences were altered. The two voting groups whose preferences we alter are those of retail shareholders, in the middle two columns, and the Big Three institutional investors, BlackRock, Vanguard, and State Street, in the right two columns. Voting choices are altered to the voting choice of the group described in the row header. To ensure a consistent comparison across the two voting groups, the number of votes we alter for a proposal is limited to the minimum of the number of retail votes and the number of Big Three votes. The sample in Panel C consists of the proposals in Panel A whose final overall number of votes in favor was between 4/5 and 6/5 of the number of votes required to pass. That is, for a standard proposal which would pass by a majority of cast ballots, Panel C limits to proposals that received 40% to 60% in favor. In all panels, columns (1) and (2) ("# passing proposals" and "# failing proposals") refer to the actual number of passing and failing proposals in each of the panel's samples. In Panel C, columns (3) and (4) reflect the number of proposals whose outcome is changed under the hypothetical that retail voters alter their voting preferences, and columns (5) and (6) reflect the number of proposals with changed outcomes under the hypothetical that the Big Three voters alter their voting preferences. In all panels, retail votes come from Broadridge and are limited to domestic retail shareholders, overall vote totals come from ISS's Voting Analytics dataset, and mutual fund votes come from a merge of Form N-PX, CRSP Mutual Funds, and Thomson Reuters S12 as described in Appendix A1. In our counts of Big Three votes, we only include votes from N-PX for which we can match the fund to an ownership count for that firm from Form 13-F.

Panel A: Consequences due to shocks to retail participation

	Actual count		**Change if group participation goes to zero**				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Group whose participation goes to zero:	# passing proposals	# failing proposals	# passing proposals flipped to fail	# failed proposals flipped to pass	# of 5% movers	# of 10% movers	# of 20% movers
Retail voters	11,545	1,392	122	39	1144	465	132
Big Three	11,545	1,392	59	64	536	120	39
All non-retail shareholders	11,545	1,392	404	165	7,881	5,032	2,105

Panel B: Consequences due to shocks to ownership structure

	Actual count		Change due to shocks to retail ownership				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Retail ownership quintile whose ownership is either increased or decreased:	# passing proposals	# failing proposals	# passing proposals flipped to fail	# failed proposals flipped to pass	# of 5% movers	# of 10% movers	# of 20% movers
Management proposals:							
Bottom quintile, + stdev.	2,297	27	0	3	20	4	0
Second quintile, + stdev.	2,236	55	1	10	31	2	0
Third quintile, + stdev.	2,141	35	0	9	55	6	0
Third quintile, - stdev.	2,141	35	12	0	36	2	0
Fourth quintile, - stdev.	2,185	32	20	0	77	9	1
Top quintile, - stdev.	2,476	30	21	1	247	30	0
Shareholder proposals:							
Bottom quintile, + stdev.	45	219	3	0	3	0	0
Second quintile, + stdev.	48	248	5	0	11	1	0
Third quintile, + stdev.	56	355	8	0	22	1	0
Third quintile, - stdev.	56	355	0	7	14	0	0
Fourth quintile, - stdev.	46	325	0	8	32	5	0
Top quintile, - stdev.	15	66	0	3	26	9	0

Panel C: Consequences due to shocks to retail voting preferences

	Actual count		Retail voters alter vote		Big Three voters alter vote	
	(1)	(2)	(3)	(4)	(5)	(6)
	# passing proposals	# failing proposals	# passing proposals flipped to fail	# failed proposals flipped to pass	# passing proposals flipped to fail	# failed proposals flipped to pass
Management proposals:						
Group voting decisions to adopt:						
Retail voters	243	88	0	0	11	23
Big Three	243	88	32	4	0	0
All non-retail shareholders	243	88	35	0	24	8
All in favor	243	88	0	14	0	28
All opposed	243	88	84	0	64	0
Shareholder proposals:						
Group voting decisions to adopt:						
Retail voters	62	166	0	0	11	4
Big Three	62	166	0	9	0	0
All non-retail shareholders	62	166	0	17	3	19
All in favor	62	166	0	43	0	53
All opposed	62	166	1	0	14	0

Table 6. Retail shareholder decision to cast a ballot

This table provides regression results describing retail shareholder turnout decisions. The dependent variable is equal to 1 if the account casts a ballot and 0 otherwise, multiplied by 100. α is defined as the account's number of shares held divided by the firm's number of shares outstanding as of the record date month, from CRSP. Log market equity is the log of market equity, computed as price time shares outstanding from CRSP as of the record date month. Yearly abnormal return refers to the firm buy-and-hold return for the period 13 months to 1 month prior to the record date minus the value weighted market return from CRSP. The dividend indicator is a binary variable equal to one if there is a positive difference in the firm's return with dividends and without dividends (*ret* and *retx* from CRSP, respectively). Tobin's q is book value plus market equity minus book equity, divided by book value. ROA, return on assets, is EBITDA divided by total assets. Special meeting is a binary variable equal to 1 for special meetings. Institutional ownership is equal to the number of shares owned by institutions divided by the shares outstanding, in the year prior to the meeting, both from Thomson Reuters. SRI on ballot is a binary variable equal to one if any proposals at the meeting are shareholder environmental or social proposals. Shareholder governance on ballot is a binary variable equal to one if any proposal at the meeting is a shareholder governance proposal. Log (Number of proposals on ballot) is the log of the number of proposals on the ballot. Log account value is the log of the total value of the account in the calendar year, defined as the sum across all firms held by the account of the product of share price and number of shares owned. 2016 county presidential turnout is the number of county residents who cast ballots in the 2016 U.S. presidential election obtained from CQ Voting and Elections, divided by the number of adult citizens from the Census Bureau. Log Zip code AGI is the average adjusted gross income in the prior calendar year in the account's zip code. Fraction over 65 is the fraction of zip code residents above age 65, from the Census, defined as $(DPSF0010015 + DPSF0010016 + DPSF0010017 + DPSF0010018 + DPSF0010019)/DPSF0010001)$. Density is the population divided by land area in square meters ($DPSF0010001/AREALAND$). Fraction with bachelors and fraction with post-bachelors are zip-code level five-year averages from the U.S. Census as of 2017. Fraction in Finance/Insurance is equal to the number of employed workers in Finance/Insurance divided by all-industries employment, both at the zip code level, from the Bureau of Labor Statistics. In Panel A, columns 1–2 use meeting fixed effects, columns 3–4 use industry and account-year fixed effects, columns 5–6 use meeting and account-year fixed effects, and column 7 uses meeting, account-year, and account-firm fixed effects. In Panel B, columns 1–3 use no fixed effects, column 4 uses industry and account-year fixed effects, and columns 5 and 6 use meeting fixed effects. In Panel B, columns 1–3, the reference category is accounts with ownership less than or equal to 10^{-9}. In addition, column 2 is limited to meetings in which no proposal comes within 30 percentage points of a different outcome and column 3 is limited to accounts with account stake values of under $100. Industry fixed effects use Fama French industry categories. In Panel A and columns 4–6 of Panel B, all right-hand side variables are demeaned, so that the intercept reflects the turnout of an observation with average levels of each covariate. Observations are weighted by the inverse of the number of meetings for the account-year, so that each account-year is weighted equally. Standard errors clustered at the account and meeting level are in parentheses. Number of clusters refers to the number of distinct meetings. *, **, and *** represent significance at the 0.05, 0.01, and 0.001 levels, respectively.

Panel A: Retail shareholder turnout decisions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log(α)	1.751***	0.890	0.250***	0.601***	0.585***	0.250	0.573***
	(0.055)	(0.501)	(0.020)	(0.025)	(0.026)	(0.250)	(0.106)
Log(ME)				0.470***			
				(0.036)			
Yearly abnormal return				0.187			
				(0.221)			
Dividend indicator				0.139			
				(0.097)			
Tobin's q				-0.119***			
				(0.032)			
ROA				-0.845**			
				(0.279)			
Special meeting				4.871***			
				(0.427)			
Institutional ownership				-1.047***			
				(0.162)			
Log(α) × Log(ME)		0.052*				0.021	
		(0.024)				(0.011)	
Log(α) × Tobin's q		-0.204***				-0.031*	
		(0.027)				(0.013)	
Log(α) × ROA		0.384				-0.445***	
		(0.253)				(0.129)	
Log(α) × Special meet.		1.535***				1.681***	
		(0.251)				(0.220)	
Log(α) × Inst. owner.		-0.074				-0.209**	
		(0.203)				(0.067)	
Intercept	7.865***	7.921***	9.422***	9.422***	9.429***	9.652***	10.460***
	(0.020)	(0.045)	(0.063)	(0.053)	(0.001)	(0.055)	(0.011)
Industry FE			Yes	Yes			
Meeting FE	Yes	Yes			Yes	Yes	Yes
Account-Year FE			Yes	Yes	Yes	Yes	Yes
Account-Firm FE							Yes
N	6,497,253	6,753,702	6,183,205	6,047,147	6,183,191	6,047,134	4,440,020
Number of clusters	3,153	7,874	8,271	7,880	8,260	7,870	7,644
R^2	0.04	0.04	0.79	0.80	0.80	0.80	0.88

Panel B: Retail shareholder turnout decisions and consumption benefits of voting

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample	No close proposals	Stake less than $100	Full sample	Full sample	Full sample
$\alpha > 10^{-6}$	2.448***	3.134***	1.157***			
	(0.205)	(0.242)	(0.332)			
$\alpha > 10^{-7}$	2.722***	2.396***	0.408			
	(0.126)	(0.178)	(0.250)			
$\alpha > 10^{-8}$	3.007***	2.702***	1.328*			
	(0.189)	(0.419)	(0.542)			
$\alpha > 10^{-9}$	0.745***	0.469	-0.344			
	(0.205)	(0.460)	(0.535)			
SRI on ballot				0.133		
				(0.082)		
Shareholder governance on ballot				0.253**		
				(0.086)		
Log(Number of proposals on ballot)				-0.063		
				(0.103)		
Log(α)				0.587***	1.596***	1.611***
				(0.024)	(0.056)	(0.055)
Log(ME)				0.421***		
				(0.039)		
Institutional ownership				-1.237***		
				(0.157)		
Special meeting				4.874***		
				(0.417)		
Log account value					0.371***	0.369***
					(0.009)	(0.010)
2016 county presidential turnout					1.611***	
					(0.332)	
Log zip code income						-1.184***
						(0.097)
Fraction over 65						14.117***
						(0.537)
Density						-0.000***
						(0.000)
Fraction with bachelors						-0.430
						(0.518)
Fraction with post-bachelors						-0.427
						(0.801)
Fraction in Finance/Insurance						20.872***
						(2.514)
Intercept	2.677***	2.297***	2.397***	9.351***	7.992***	8.569***
	(0.127)	(0.179)	(0.119)	(0.034)	(0.025)	(0.054)
Industry FE				Yes		
Meeting FE					Yes	Yes
Account-Year FE				Yes		
N	6,894,960	2,757,938	276,723	6,056,453	6,456,515	6,352,277
Number of clusters	8,274	6,094	7,556	7,910	8,215	8,214
R^2	0.01	0.01	0.00	0.80	0.04	0.04

Table 7. Effect of information materials on turnout

This table reports regression results describing how the availability of information materials shapes shareholder turnout decisions. The sample is limited to annual meetings held at firms that switch delivery methods a single time over the sample period 2015-2017 (along with 200 additional randomly selected firms that did not switch delivery methods). We further restrict the sample to firms and accounts that appeared in the data in 2015. The first column presents the estimation of the likelihood of receiving Hard Copy Materials (multiplied by 100) on the triple interaction of (i) whether the firm switches its information materials choice in the sample period (separated by the direction of the switch), (ii) whether the meeting in question is post-switch, and (iii) the proportion of firms in the account's portfolio in 2015 for which the account chose Default information materials. The second column presents the estimation of the likelihood of casting a ballot (multiplied by 100) on the triple interaction terms. The third column presents the estimation of the likelihood of casting a ballot (multiplied by 100) on the receipt of Hard Copy Materials by scaling column 2 by column 1, following Duflo (2001). All regressions include account-firm, account-year, and firm-year fixed effects. Standard errors clustered at the account and meeting level are in parentheses. *, **, and *** represent significance at the 0.05, 0.01, and 0.001 levels, respectively.

	(1) Hard Copy Materials × 100	(2) Cast × 100	(3) Cast × 100
$SwitchHCtoN_c \times Post_{ct} \times Default_{a0}$	-91.657***	-2.554***	
	(0.807)	(0.254)	
$SwitchNtoHC_c \times Post_{ct} \times Default_{a0}$	89.934***	4.202***	
	(1.398)	(0.849)	
$Hard\ Copy\ Materials_{act}$			3.248***
			(0.317)
Account-Firm FE	Yes	Yes	Yes
Account-Year FE	Yes	Yes	Yes
Firm-Year FE	Yes	Yes	Yes
N	1,406,262	1,406,262	1,406,262
Number of clusters	306	306	306
R^2	0.98	0.94	0.94

Table 8. Effect of available voting methods on turnout

This table reports regression results documenting how the availability of voting methods impacts shareholder turnout decisions. The sample is limited to annual meetings held at firms that switch delivery methods a single time over the sample period 2015-2017 (along with 200 additional randomly selected firms that did not switch delivery methods). The sample is further limited to firms that selected Hard Copy delivery methods in 2015 and to accounts that appeared in the data in 2015. The left two columns are limited to accounts that voted in 2015, with voting in 2016 as the dependent variable, and excludes switching firms that did not switch in 2016. The right two columns are limited to accounts that voted in 2016, with voting in 2017 as the dependent variable, and excludes switching firms that did not switch in 2017. The first and third columns contain accounts that selected Default; the second and fourth columns contain accounts that did not select Default. The right-hand side variables include i) whether the firm switched delivery methods, ii) whether the account, when voting the previous year, voted by internet, and iii) their interaction. Standard errors clustered at the account and meeting level are in parentheses. *, **, and *** represent significance at the 0.05, 0.01, and 0.001 levels, respectively.

	2016		**2017**	
	Default shareholders	Non-default shareholders	Default shareholders	Non-default shareholders
Switching firm × Did not vote by internet	-44.830***	1.157	-46.582***	-1.839
	(2.888)	(2.259)	(1.853)	(2.634)
Switching firm	-0.873	1.169	0.725	0.850
	(1.567)	(1.612)	(1.763)	(1.696)
Did not vote by internet	-13.772***	2.913	-15.373***	3.159
	(2.603)	(1.750)	(1.119)	(2.160)
Intercept	76.587***	71.350***	78.134***	74.559***
	(1.267)	(0.802)	(1.172)	(0.953)
N	15,818	55,176	25,230	90,742
Number of clusters	114	114	122	122
R^2	0.221	0.002	0.291	0.001

Table 9. Retail shareholder voting decisions

This table reports evidence on account-level voting decisions with observations at the account-proposal level. The dependent variable is a binary variable that equals one if the account voted in line with management's recommendation, and zero if it voted against, multiplied by 100. The analysis is limited to account-proposals in which the account voted on the proposal and excludes routine proposals (auditor ratification and meeting adjournment). α is defined as the account's number of shares divided by the firm's number of shares outstanding on the record date month, from CRSP. Log market equity is the log of market equity computed as price time shares outstanding from CRSP, as of the record date month. Yearly abnormal return refers to the firm buy-and-hold return for the period 13 months to 1 month prior to the record date minus the value weighted market return from CRSP. The dividend indicator is a binary variable equal to one if there is a positive difference in the firm's return with dividends and without dividends (*ret* and *retx* from CRSP, respectively). Tobin's q is book value plus market equity minus book equity, divided by book value. Return on assets, ROA, is EBITDA divided by total assets. Special meeting is a binary variable equal to one for special meetings. Institutional ownership is equal to the number of shares owned by institutions divided by the shares outstanding in the year prior to the meeting, both from Thomson Reuters. ISS against management is a binary variable that equals one if ISS has a recommendation other than "For" for a management proposal, or a "For" recommendation for a shareholder proposal. Log account value is the log of the total account value for that account in the calendar year defined as the sum across all firms held by the account of the product of share price and the number of shares owned. 2016 county presidential turnout is the number of county residents who cast ballots in the 2016 U.S. presidential election from CQ Voting and Elections, divided by the number of adult citizens from the Census Bureau. Log zip code AGI is the average adjusted gross income in the prior calendar year in the account's zip code. Fraction over 65 is the fraction of zip code residents above the age 65, from the Census, defined as $(DPSF0010015 + DPSF0010016 + DPSF0010017 + DPSF0010018 + DPSF0010019)/DPSF0010001$. Density is the population divided by land area in square meters ($DPSF0010001/AREALAND$). Fraction with bachelors and fraction with post-bachelors are zip-code level five-year averages from the U.S. Census as of 2017. Fraction in Finance/Insurance is equal to the number of employed workers in Finance/Insurance divided by all-industries employment, both at the zip code level, from the Bureau of Labor Statistics. Column 1 includes proposal category, industry, and year-month fixed effects; column 2 includes proposal fixed effects; column 3 includes proposal category, industry, and account-year fixed effects; column 4 includes account-meeting and account-proposal category fixed effects; column 5 includes proposal and account-year fixed effects; and column 6 includes proposal, account-year, and account-firm fixed effects. Industry fixed effects use Fama French industry categories; proposal category fixed effects use the proposal categories set forth in Online Appendix A1. All right-hand side variables are demeaned, so that the intercept reflects the turnout of an account with average levels of each covariate. Observations are weighted by the inverse of the number of meetings for the account-year, so that each account-year is weighted equally. Standard errors clustered at the account and meeting level are in parentheses. Number of clusters refers to the number of distinct meetings. *, **, and *** represent significance at the 0.05, 0.01, and 0.001 levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Log(α)	1.098***	0.823***	0.458***		0.238***	-0.350
	(0.061)	(0.064)	(0.044)		(0.045)	(0.285)
Log(ME)	1.036***		0.605***			
	(0.116)		(0.063)			
Yearly abnormal return	4.404***		2.469***			
	(0.705)		(0.269)			
Dividend indicator	1.370*		0.107			
	(0.560)		(0.252)			
Tobin's q	0.334		0.130			
	(0.191)		(0.091)			
ROA	7.027**		3.174**			
	(2.690)		(1.229)			
Special meeting	-4.709***		-2.865***			
	(1.143)		(0.763)			
Institutional ownership	-1.790		0.836			
	(1.058)		(0.489)			
ISS against management	-2.725***		-2.429***	-1.483***		
	(0.483)		(0.373)	(0.311)		
Log account value	0.020	0.061**				
	(0.023)	(0.022)				
2016 county presidential turnout	-1.436	-1.208				
	(1.118)	(1.078)				
Log zip code income	1.934***	1.884***				
	(0.300)	(0.292)				
Fraction over 65	6.005***	5.825***				
	(1.120)	(1.101)				
Density	-0.000***	-0.000***				
	(0.000)	(0.000)				
Fraction with bachelors	-3.491*	-0.020				
	(1.553)	(0.015)				
Fraction with post-bachelors	-7.065**	-0.092***				
	(2.563)	(2.54)				
Fraction in Finance/Insurance	3.413	5.893				
	(7.613)	(7.295)				
Intercept	85.495***	85.690***	86.357***	87.980***	86.574***	86.559***
	(0.187)	(0.117)	(0.046)	(0.008)	(0.008)	(0.055)
Proposal Category FE	Yes		Yes			
Industry FE	Yes		Yes			
Year-Month FE	Yes					
Proposal FE		Yes			Yes	Yes
Account-Year FE			Yes			
Account-Meeting FE				Yes		
Account-Proposal Category FE				Yes		
Account-Year FE					Yes	Yes
Account-Firm FE						Yes
N	7,388,040	7,488,217	7,771,765	7,701,840	7,880,494	7,856,887
Number of clusters	7,239	6,794	7,591	5,056	7,460	6,772
R^2	0.09	0.15	0.58	0.81	0.60	0.65

Table 10. Comparison of retail and institutional investors' decisions

This table reports regression results on shareholder voting with votes aggregated to the proposal level. The dependent variable is the number of votes cast in line with management's recommendation divided by the number of votes cast For or Against, multiplied by 100. Log market equity is the log of market equity computed as price time shares outstanding from CRSP, as of the record date month. Yearly abnormal return is the firm buy-and-hold return for the period 13 months to one month prior to the record date, minus the buy-and-hold value weighted market return from CRSP. The dividend indicator is a binary variable equal to one if there is a positive difference in the firm's buy-and-hold return with dividends and without dividends (*ret* and *retx* from CRSP, respectively). Tobin's q is book value plus market equity minus book equity, divided by book value. Return on assets, ROA, is EBITDA divided by total assets. Special meeting is a binary variable equal to one for special meetings. Institutional ownership is equal to the number of shares owned by institutions divided by the shares outstanding in the year prior to the meeting, both from Thomson Reuters. ISS against management is a binary variable that equals 1 if ISS has a recommendation other than For for a management proposal, or a For recommendation for a shareholder proposal. Columns 1 through 3 include institutional voting results and columns 4 through 6 contain retail shareholder voting results. All columns except 3 and 6 include industry fixed effects and columns 3 and 6 include firm fixed effects. Industry fixed effects use Fama French industry categories; time fixed effects are at the year-month level; proposal category fixed effects use the proposal categories set forth in Online Appendix A1. All right hand side variables are demeaned over all observations in the sample, so the intercept reflects the average vote for an observation with mean values of those covariates. Observations are weighted so that each meeting is weighted equally. Standard errors clustered at the meeting level are in parentheses. Number of clusters refers to the number of distinct meetings. *, **, and *** represent significance at the 0.05, 0.01, and 0.001 levels, respectively.

	Institutional voters			Retail voters		
Vote with management	(1)	(2)	(3)	(4)	(5)	(6)
Log(ME)	0.824***	0.751***		0.527***	0.527***	
	(0.148)	(0.100)		(0.086)	(0.086)	
Yearly abnormal return	0.109	-1.105*	-0.413	4.369***	4.396***	2.853***
	(0.764)	(0.503)	(0.439)	(0.424)	(0.423)	(0.329)
Dividend indicator	2.096***	-0.396	-3.555**	-0.326	-0.437	1.708*
	(0.496)	(0.316)	(1.085)	(0.284)	(0.283)	(0.788)
Tobin's q	0.282	0.387**	0.609	0.332***	0.331***	0.491*
	(0.181)	(0.119)	(0.321)	(0.098)	(0.098)	(0.203)
Return on assets	7.477***	1.729	-1.382	3.842***	3.734***	1.442
	(1.799)	(1.155)	(2.148)	(0.876)	(0.883)	(1.312)
Special meeting	-7.769***	-3.603***	-3.032**	-1.118	-1.000	-0.234
	(1.482)	(0.904)	(1.025)	(0.739)	(0.742)	(0.656)
Institutional ownership	6.760***	4.074***	3.562	2.743***	2.703***	-0.519
	(1.037)	(0.679)	(2.200)	(0.581)	(0.583)	(1.746)
ISS against management		-50.721***	-46.684***		-1.781***	-1.802***
		(0.787)	(0.709)		(0.428)	(0.330)
Intercept	88.335***	88.449***	88.780***	89.334***	89.305***	89.570***
	(0.230)	(0.149)	(0.148)	(0.127)	(0.127)	(0.106)
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes		Yes	Yes	No
Proposal Category FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE			Yes			Yes
N	33,116	32,998	32,674	33,392	33,263	32,942
Number of clusters	7,781	7,771	7,447	7,884	7,873	7,552
R^2	0.14	0.62	0.77	0.17	0.17	0.65

Table 11. Retail shareholder voting decisions and exit

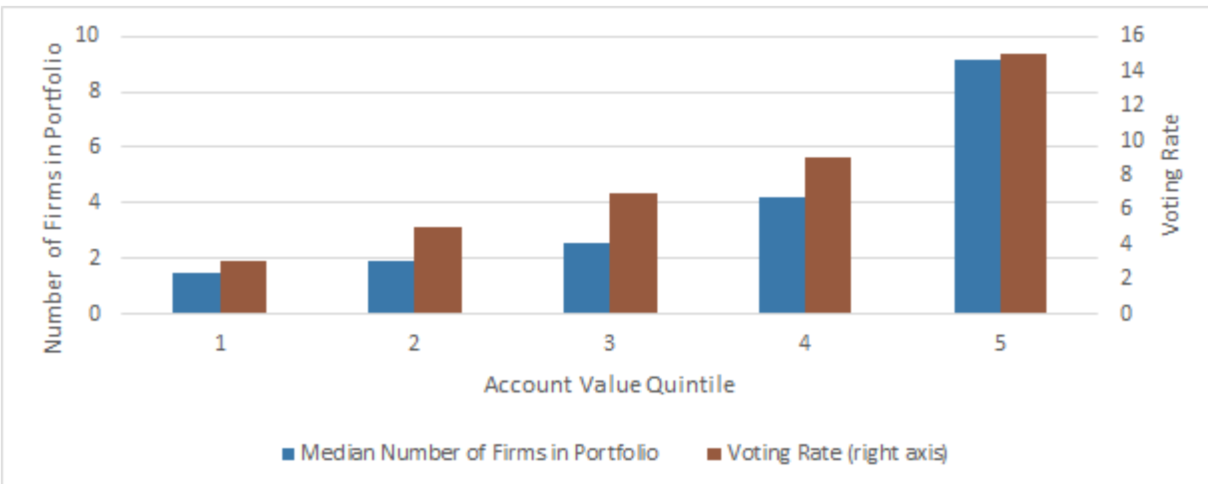
This table describes the relationship between changes in ownership between years t and $t+1$ and firm and account characteristics. The data is limited to accounts who, at year t, hold firms that appear in the data in year $t+1$. The dependent variable, still own next year, is equal to 1 if the account holds the firm in $t+1$. Cast ballot is an indicator variable equal one if the account cast a ballot in t. α is defined as the account's number of shares divided by the firm's number of shares outstanding as of the record date month, from CRSP. Log market equity is the log of market equity computed as price time shares outstanding from CRSP, as of the record date month. Yearly abnormal return refers to the firm's buy-and-hold return for the period 13 months to 1 month prior to the record date minus the value weighted market return from CRSP. The dividend indicator is a binary variable equal to one if there is a positive difference in the firm's return with dividends and without dividends (*ret* and *retx* from CRSP, respectively). Tobin's q is book value plus market equity minus book equity, divided by book value. ROA, return on assets, is EBITDA divided by total assets. Institutional ownership is equal to the number of shares owned by institutions divided by the shares outstanding in the year prior to the meeting, both from Thomson Reuters. Yearly abnormal return, the dividend indicator, return on assets, and Tobin's q are as of year $t+1$. In columns (2)–(4), we start from a random sample of accounts and limit to account-meetings in which the account cast a ballot. WithMGMT is the fraction of proposals at year t on which the account voted in line with management. We also include WithMGMT for the following subcategories of proposals: (i) management-sponsored proposals; (ii) shareholder-sponsored proposals, and certain subcategories of management-sponsored proposals; (iii) director proposals; (iv) say-on-pay-proposals; and (v) other management-sponsored proposals. All columns include year-month, industry, and account-year fixed effects. Industry fixed effects use Fama French industry categories. Proposal category fixed effects use the proposal categories in Online Appendix A1. Observations are weighted by the inverse of the number of meetings for the account-year, so that each account-year is weighted equally. Standard errors clustered at the account and meeting level are in parentheses. Number of clusters refers to the number of distinct meetings. *, **, and *** represent significance at the 0.05, 0.01, and 0.001 levels, respectively.

Still own next year	**Unconditional** (1)	**Conditional on turnout** (2)	(3)	(4)
Cast ballot$_t$	1.112***			
	(0.153)			
WithMGMT{on all proposals}		1.511***		
		(0.205)		
WithMGMT{on management proposals}			1.466***	
			(0.282)	
WithMGMT{on shareholder proposals}			0.451***	0.473*
			(0.108)	(0.233)
WithMGMT{on director proposals}				3.306***
				(0.535)
WithMGMT{on say-on-pay proposals}				0.716**
				(0.264)
WithMGMT{on other management proposals}				0.006
				(0.279)
Log(α_t)	0.826***	1.388***	1.493***	1.557***
	(0.063)	(0.066)	(0.072)	(0.120)
Log(ME$_t$)	1.662***	2.165***	2.657***	3.024***
	(0.122)	(0.105)	(0.212)	(0.496)
Institutional ownership$_t$	-4.558***	1.222	3.608*	3.648*
	(0.874)	(0.661)	(1.395)	(1.548)
Yearly abnormal return$_{t+1}$	-0.113	0.744	-0.969	1.091
	(0.555)	(0.490)	(0.879)	(1.125)
Dividend indicator$_{t+1}$	-0.174	0.661***	0.404*	1.076**
	(0.582)	(0.121)	(0.198)	(0.366)
Tobin's q$_{t+1}$	0.799***	0.192	1.065	-10.371*
	(0.150)	(0.807)	(2.121)	(4.778)
ROA$_{t+1}$	-0.206	-4.114***	-3.392*	-1.135
	(0.788)	(0.735)	(1.413)	(3.429)
Intercept	69.848***	41.001***	32.002***	17.895
	(0.105)	(1.817)	(5.455)	(13.896)
Year-month FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Account-Year FE	Yes	Yes	Yes	Yes
N	4,350,339	3,894,466	2,319,325	564,108
Number of clusters	2,415	2,412	336	125
R^2	0.7651	0.7667	0.7967	0.8146

Figure 1. Ownership characteristics by account value and firm size

This figure graphs retail investor ownership characteristics by account value and firm size. For each firm meeting we use each account's holdings on the record date as a "snapshot" of that account's holdings in the firm. We keep only one meeting of each firm per year. Then, for each account, we aggregate the holdings in the portfolio at the account-year level. Account value is the sum of an account's individual firm stake values, where the stake value is the number of shares owned by the account multiplied by the record date month share price. Panel A shows, for each account value quintile, the median number of firms in the portfolio (left axis) and the average account voting rate (right axis), defined as the number of ballots cast divided by number of voting opportunities. Panel B shows, for each firm size quintile, the median retail ownership (left axis), defined as the percentage of outstanding shares of the firm held by domestic retail investors in the sample, as well as the median number of retail accounts, in thousands, who own shares in the firm (right axis). Firm size is calculated as the product of CRSP variables *csho* and *prc*, and quintiles are determined using the NYSE size breakpoints from Ken French's website.

Panel A: Number of firms in the portfolio and voting rate by account value quintile



Panel B: Retail ownership and number of accounts by firm size quintile

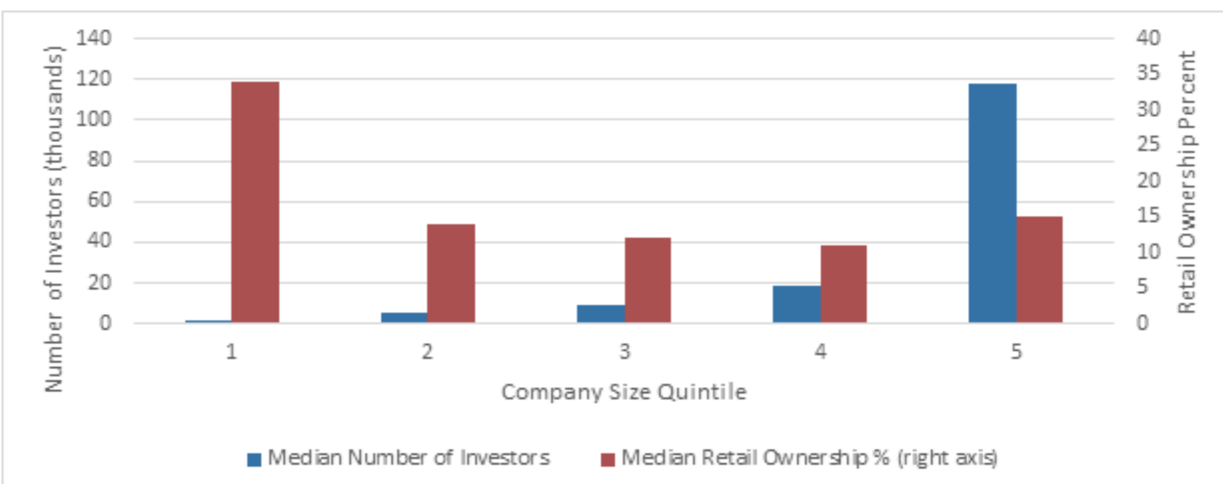


Figure 2. Relation between voter participation and ownership

This figure graphs the relationship between retail voter turnout and ownership of the firm. We plot a binned scatterplot of turnout on stake size, α, defined as the account's number of shares held divided by the firm's number of shares outstanding on the record date month, from CRSP. Each dot represents the average turnout for accounts whose ownership fraction of the firm falls within the increment of α. Each of the four colored scatterplots provides a different range for share ownership, α. The first describes how turnout varies with share ownership in the range of [0 10^{-4}] with increments of 10^{-6}; the second, in the range of [0 10^{-3}] with increments of 10^{-5}; the third in the range of [0 10^{-2}] with increments of 10^{-4}; and the fourth in the range of [0 10^{-1}] with increments of 10^{-3}.

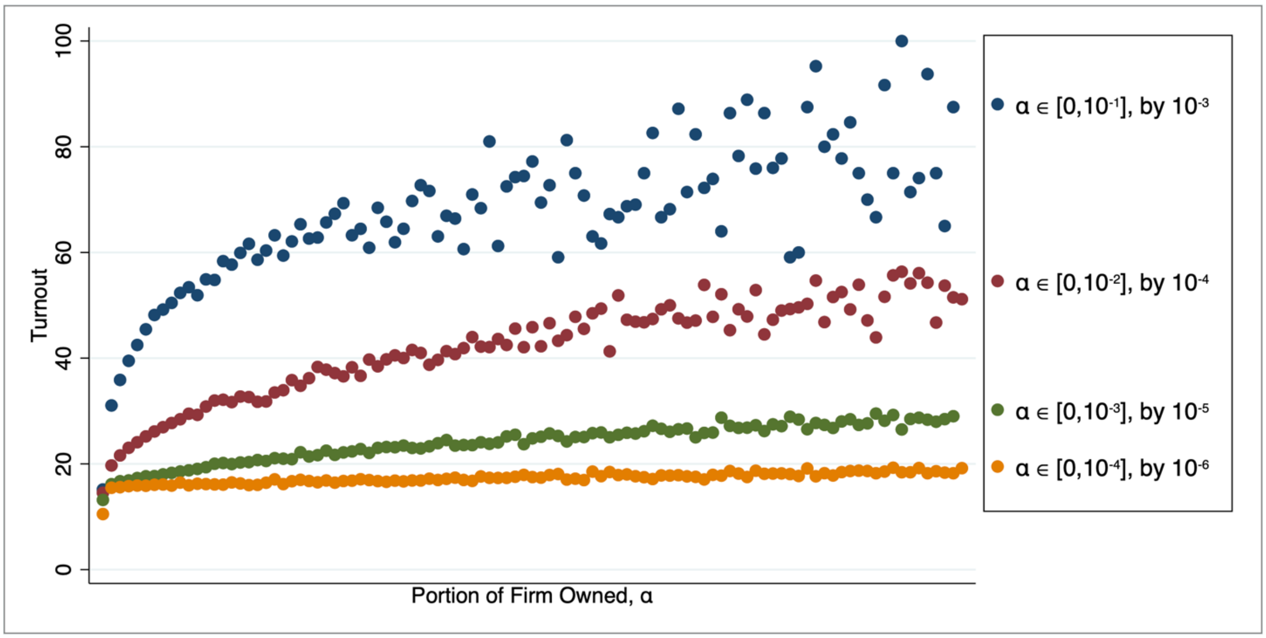


Figure 3. Firm and account choice of information materials and turnout

The top two subplots show the relationship between the materials an account receives, the account delivery method selection, and firm switches in delivery method selection. The y-axis is the percentage of accounts that received Hard Copy materials. The bottom two subplots show the relationship between account turnout, account delivery method selection, and firm switches in delivery method selection. The y-axis is the fraction of accounts that voted, divided by the fraction of accounts that voted in year -1, so all lines are normalized to 1 at year -1. In both panels, the x-axis reflects the year of the meeting minus the year of the firm switch, with year 0 reflecting the year the firm switched. The sample is limited to annual meetings and firms that switch delivery methods a single time over the sample period 2015-2017, as well as to firms and accounts that appeared in the data in 2015. Red lines reflect accounts that selected Hard Copy in their first year in the data; the green lines reflect accounts that selected Default in their first year in the data; the blue lines reflect accounts that selected E-Delivery in their first year in the data. Subplots on the left contain firms that switched from Hard Copy to Notice. Subplots on the right contain firms that switched from Notice to Hard Copy.

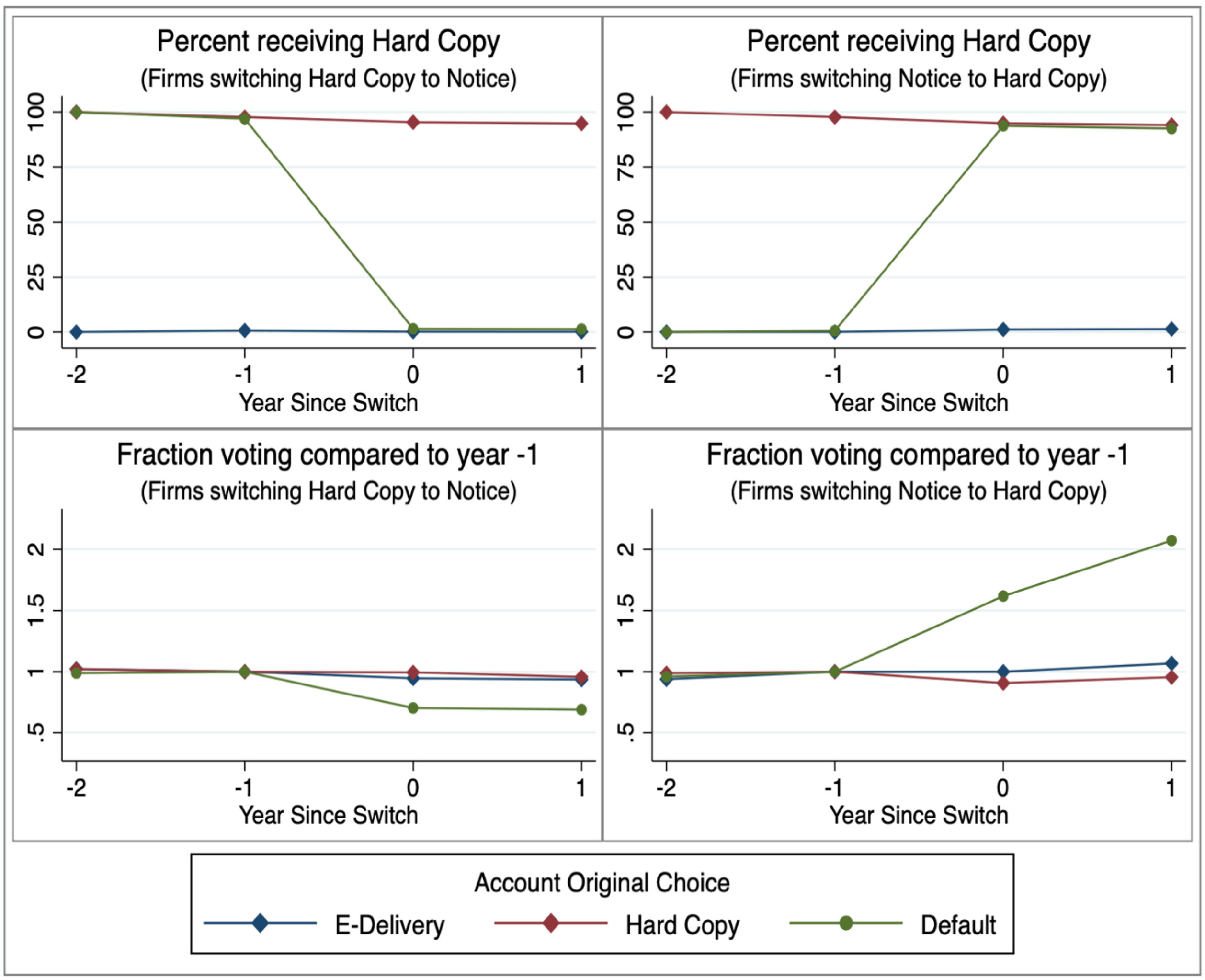


Figure 4. Account voting method, delivery method selection, and turnout by Default and non-Default accounts

This figure shows the relationship between account turnout, account voting methods, account delivery method selection, and firm switches in delivery method selection. The sample is limited to annual meetings held at firms that switch delivery methods a single time, in 2017 (along with 200 additional randomly selected firms that did not switch delivery methods). The sample is further limited to firms that selected Hard Copy delivery methods in 2015 and to accounts that voted in 2015. The x-axis reflects the meeting year. The y-axis is the percentage of accounts that turned out and voted. Red lines reflect firms that did not switch delivery methods, and blue lines reflect firms that switched delivery methods in 2017. The subplots in the top row provide information on voting only by Default accounts. The subplots in the bottom row provide information on non-Default accounts. On the left we report turnout by Default accounts that voted by methods other than internet in 2015 whereas on the right we show turnout by Default accounts that voted by internet in 2015.

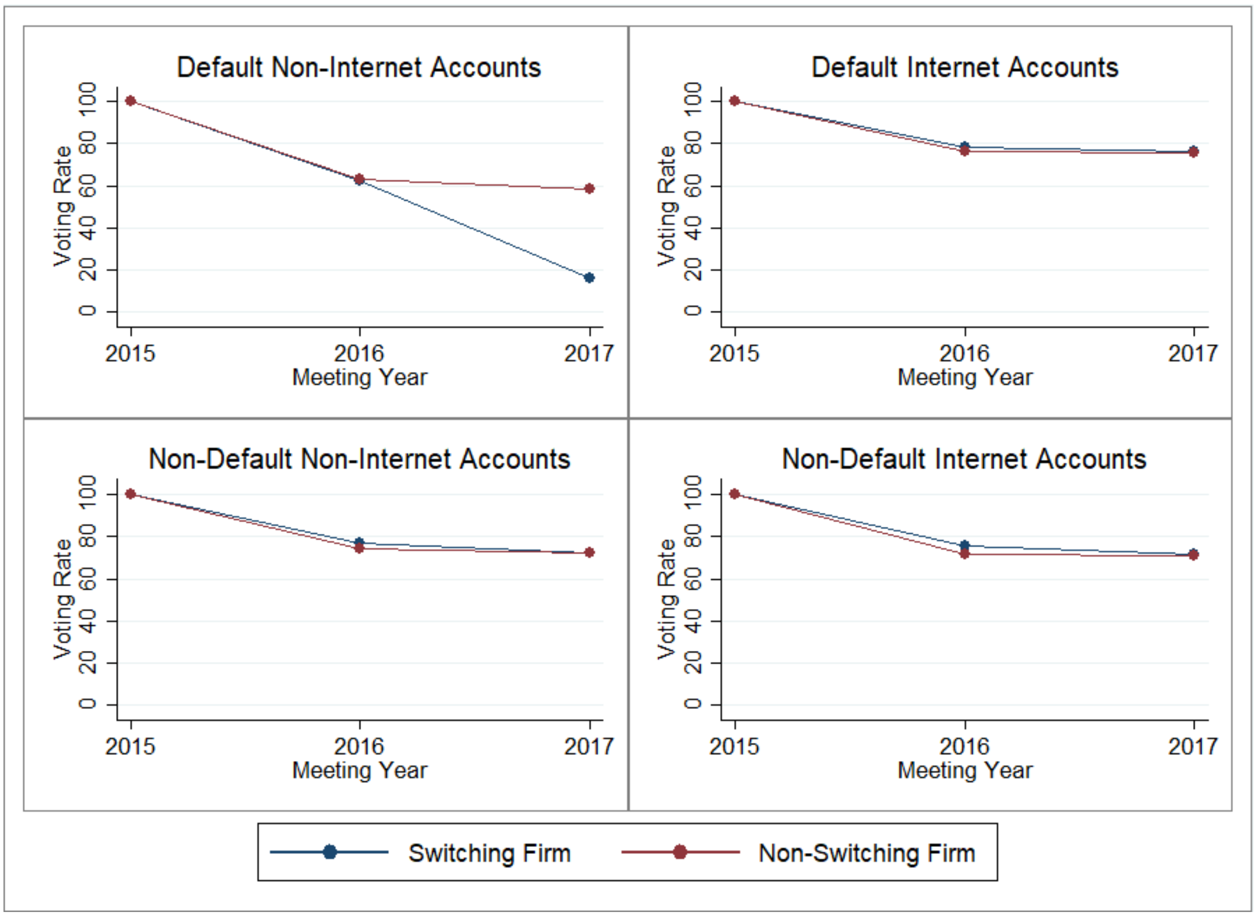


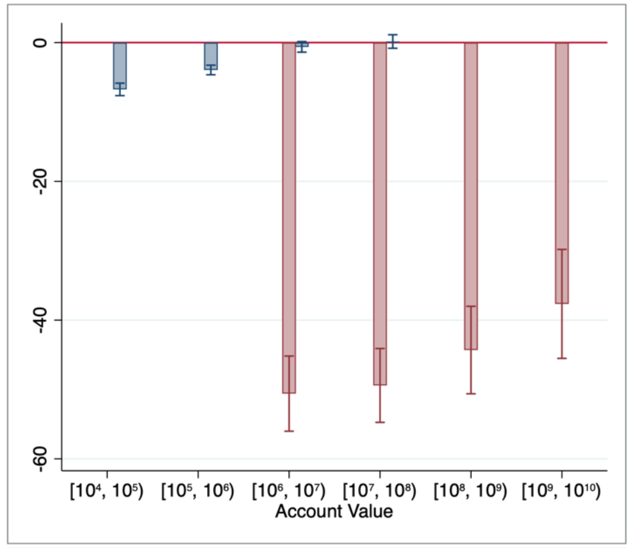
Figure 5. Sensitivity of voting to ISS recommendations

This figure graphs the sensitivity of voting choice to ISS recommendations by owner type. We estimate the following specification for retail accounts:

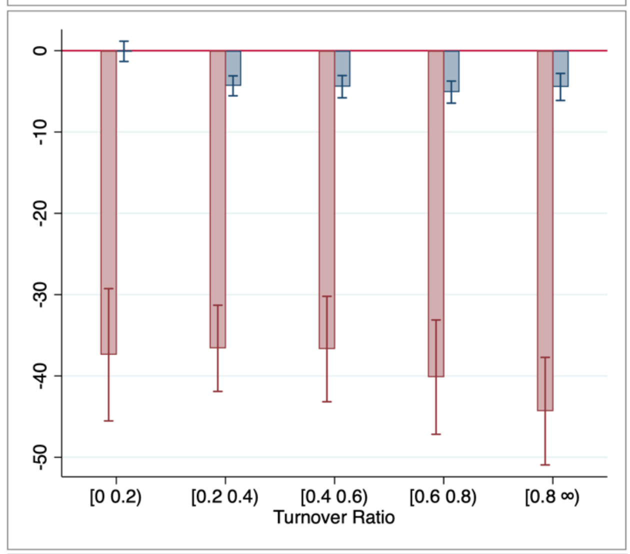
$$WithMGMT_{apmct} = \beta_0 + \sum_{j=1}^{J} \beta_{1j} Bin_j X_{apmct} + \sum_{j=1}^{J} \beta_{2j} Bin_j + \beta_3 Z_{apmct} + \psi_{PropCat_p} + \zeta_{Ind} + \phi_{at} + \varepsilon_{apmct}$$

where a indexes accounts, p indexes proposals, m indexes meetings, c indexes firms, and t indexes years. Bin_j is a binary variable equal one if an account value (or turnover ratio, or log number of firms in portfolio) falls within the j'th bin. Account value bins correspond to six segments of the log 10 scale ([10^4 10^5), [10^5 10^6), [10^6 10^7), [10^7 10^8), [10^8 10^9), [10^9 10^{10})). Turnover bins are [0 0.2), [0.2 0.4), [0.4 0.6), [0.6 0.8), [0.8 ∞), where the final bin includes the small group of investors with reported turnover ratios greater than 1, and log number of firm bins correspond to seven equally spaced segments ([1 2), [2 3), [3 4), [4 5), [5, 6), [6, 7), [7,8)). The dependent variable, $WithMGMT_{apmct}$, is a binary variable that equals one if the account votes in line with management's recommendation and zero if it votes against, multiplied by 100. X_{apmct} is a vector of covariates including yearly abnormal return, Tobin's q, return on assets, and whether ISS's recommendation was in opposition to management's recommendation. Z_{apmct} is a vector of additional covariates, including log market equity, a dividend indicator, institutional ownership, and special meeting. For additional information on the covariates included in X_{apmct} and Z_{apmct} see Table 9. $\psi_{PropCat_p}$, ζ_{Ind}, and ϕ_{at} are proposal category, industry, and account-year fixed effects, respectively. β_{1j} and β_3 are each vectors of coefficients. We report the retail investor sensitivity to ISS recommendations across account bins in Panel A, turnover ratio bins in Panel B, and portfolio breadth bins in Panel C. We repeat the estimation as described above for institutional investors. This yields sensitivities to ISS recommendations for both types of investors across account, turnover ratio, and breadth bins which we report in the figures below. For retail, account value is the total account value for that account in the calendar year, defined as the sum across all firms held by the account of the product of share price and number of shares owned. For funds, account value is calculated as its portfolio value. For retail, we calculate turnover ratio using CRSP's definition, and take the minimum of purchases and sales divided by account value over the course of the year. For funds, turnover ratio comes from CRSP. For retail shareholders, log account number of firms in the portfolio is the log of the number of firms held by the account in the retail dataset in a calendar year; for funds, it is the log of the fund's number of N-PX securities in a calendar year. The analysis is limited to account-proposals in which the account voted on the proposal and excludes routine proposals (auditor ratification and meeting adjournment). For both retail and institutions, we only include bins where there are a sufficient number of distinct voters. Observations are weighted by the inverse of the number of meetings for the account-year or fund-year, so that each account-year and fund-year is weighted equally. 95% confidence intervals are clustered at the account and meeting level.

Panel A: Account value bins



Panel B: Turnover ratio bins



Panel C: Log number of firms in account