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Abstract

We examine the effects of the sudden abolition of trading commissions by major brokerages in 2019 and subsequent increase in retail-investor base on corporate governance. Firms with greater retail ownership experienced higher abnormal returns around the abolition of commissions. Firms with positive abnormal returns in response to the abolition of commissions subsequently saw decreases in institutional ownership and a decrease in shareholder voting. These firms also experienced a decline in environmental, social, and governance (ESG) scores, which is concentrated in governance measures. The results suggest that even relatively modest entry costs can be important in determining retail-investor influence in corporate governance.

JEL classification: G11, G14, G24, G41, K22

Keywords: Corporate Governance, ESG, Financial Technology, Retail Investors, Stock Market Participation

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1. Introduction

What influence do retail shareholders have on corporate governance? Finance and legal scholars have analyzed the impact of changes in shareholder base (from retail to institutional or vice versa) on corporate governance and performance. The primary empirical challenge has been the problem of endogeneity: while a change in shareholder base can affect firm governance and performance, better or worse performance or governance can also attract certain types of investors. The existing scholarship has attempted to disentangle the effects by trying to identify an exogenous shock, such as a firm's inclusion in a stock market index (Appel, Gormley, and Keim 2016).

In this paper, we attempt to shed light on this important issue by using the sudden abolition of trading commissions by major brokerages in 2019 as a potential natural experiment. The introduction of zero-commission trading by major brokerages (Charles Schwab, E-Trade, and TD Ameritrade) reduced retail investors' cost of entering the stock market and can thus be linked to an unexpected increase in retail ownership at certain firms—independent of any governance features of those firms. This research design allows us to analyze the impact on corporate governance of the growth in retail ownership, given the sudden and unexpected nature of the announcement by major brokerages.

First, we find evidence consistent with the reduced entry cost translating to a significant increase in retail ownership (and a decrease in institutional stockholding). In an event study analysis, we find that firms with higher *existing* non-institutional ownership experienced greater abnormal returns on October 1, 2019, when the major brokerages suddenly announced the introduction of zero-commission trading platforms. Second, we find that firms that experienced higher abnormal returns on October 1, 2019 thereafter experienced a sustained increase in non-institutional ownership. We find no evidence of a pretrend in non-institutional ownership in the years preceding the introduction of zero-commission trading. The economic and statistical significance of the increase in retail ownership we document persists using four different regression methodologies.

Third, with respect to the impact of retail investors on corporate governance, we first show theoretically that if retail shareholders are not as active in participating in corporate governance as institutional shareholders (by, for instance, being less likely to vote), an increase in retail ownership will not necessarily translate to a decline in influence by the remaining (including passive) institutional investors.¹ The theoretical ambiguity, therefore, leaves room for an empirical assessment. Empirically, we use proposal-level data to find that firms that experienced higher abnormal returns on October 1, 2019 also saw a significant decline in shareholder voting after trading commissions were abolished. Similar to the institutional ownership result, we also do not find any pretrend in non-voting in the years before the introduction of zero-commission trading.

Fourth, we find that firms with greater abnormal returns on October 1, 2019 experienced a decrease in environmental, social, and corporate governance (ESG) metrics, as measured by the MSCI

¹ This is in contrast to a situation where passive institutional investors *increase* their holdings—including in such scenarios involving a firm's inclusion in a stock market index. In such a case, passive institutional shareholders' influence over management will unambiguously increase. The reason for the present ambiguity, as demonstrated in more detail in the theory section, has to do with retail investors' lack of participation and other (active) institutional shareholders' vote (or exert influence) in line with passive institutional shareholders. For instance, if retail investors never vote and active institutions do not vote in line with passive institutions, an increase in retail ownership and a concurrent decrease in active institutions' ownership can actually increase the relative power of passive institutions.

database, after 2019. Furthermore, when we disaggregate ESG performance into the sub-scores for environmental (E), social (S), and corporate governance (G) issues, we find that the post-2019 decline for the treatment group is most pronounced in the governance (G) component. We find no significant change in these companies' performance on "E" or "S" issues after the wide introduction of zero-commission trading. The best reading of the decrease in the ESG scores of the firms with high abnormal returns could be that the quality of their corporate governance, as measured by the methodology of data providers like MSCI, declined after 2019.

All specifications include firm and year fixed effects, allowing the regression estimates to account for within-firm and within-year variation in the dependent variables. As a robustness check, we find evidence suggesting that it was the abolition of trading commissions, rather than the introduction of fractional trading, that explains the rise in retail ownership during our sample period. As another robustness check, we re-rerun our tests using abnormal returns from a large number of placebo dates from the year before October 1, 2019. The coefficient corresponding to October 1, 2019 is in the upper tail of the distribution, showing that this date is an outlier compared to the distribution of coefficients on other days.

Overall, this paper makes two principal contributions to the literature. First, the paper advances the literature on the role of entry costs in determining whether individuals enter the stock market. Previous work looking at Scandinavian lottery winners and the inheritors of large bequests has shown that a modest fixed entry cost can explain why many retail investors choose not to invest in the stock market ([Andersen and Nielsen 2011](#); [Briggs et al. 2021](#); [Vissing-Jorgensen 2002](#)). Our study is broadly consistent with these papers: the removal of the relatively modest trading commissions led to a significant systematic increase in retail ownership at affected firms. Unlike previous studies, however, our finding documents a relationship between entry costs and retail investor entry into a broad swathe of publicly-traded U.S. companies.

Second, the paper adds to the literature on the distinctive role of retail investors in corporate governance, relying on a previously unexplored research design. As individuals with small stakes in firms run by professionals and lacking the ability to monitor managers, retail investors can be seen as rationally apathetic toward corporate affairs ([Black 1990](#)). Existing empirical evidence suggests that retail shareholders are far less likely to vote than other stockholders and are less concerned with pushing management to better its ESG performance ([Brav, Cain, and Zytlick 2022](#)). Earlier scholars have also documented how an (exogenous) increase in institutional ownership leads to better corporate governance and performance ([Aghion, Reenen, and Zingales 2013](#); [Appel, Gormley, and Keim 2016](#)). Our results, showing a decrease in shareholder voting and ESG scores at firms exposed to zero-commission trading, are consistent with this literature on retail investor influence in corporate governance. They also are consistent with the earlier results on the impact of institutional ownership on better corporate governance. This paper's contribution lies in establishing a predictive relationship between the 2019 abolition of entry costs for retail investors and the subsequent corporate governance changes consistent with retail shareholders' apathy toward corporate governance.²

² In earlier work, we found similar stock return reactions to zero-commission trading, as well as subsequent changes in corporate governance, for a handful of companies popular among retail investors and known as "meme stocks" ([Aggarwal, Choi, and Lee 2024](#)). This paper broadens the earlier work by generalizing it to the broad array of firms popular with retail investors.

The paper is organized as follows. In Section 2, we describe the background literature on the role of entry costs in individuals' decisions to enter the stock market. Section 2 also surveys the evidence on retail or institutional investor behavior. Section 3 presents a simple, stylized model exploring the effects of changing the composition of investor base on voting outcomes. The model illustrates that the overall effect of the replacement of institutional investors by retail investors on the (remaining) institutional investors' influence on corporate affairs is ambiguous. Section 4 describes the different sources of data used in our empirical analysis and presents summary statistics. Section 5 details the events surrounding the October 1, 2019 abolition of trading commissions by major brokerages, and shows how stocks that already had a higher share of non-institutional investors exhibited higher abnormal returns that day. Section 5 then shows the main result from our paper: the introduction of zero-commission trading was followed by a persistent increase in non-institutional ownership at firms that had the highest abnormal returns on October 1, 2019.

Section 6 documents the corporate governance effects of zero-commission trading. We find that treated firms saw an increase in shareholder non-participation, as measured by the percentage of votes that were neither cast for or against a proposal, nor marked as abstentions. We also show that the treated group saw a decrease in its ESG scores, consistent with pro-ESG institutional players being replaced by retail investors who are relatively apathetic about these topics. We find that treated firms specifically experienced a decrease in scores on corporate governance, rather than environmental or social issues. Section 7, which contains various robustness tests described earlier, reiterates the relationship between zero-commission trading and retail influence on corporate governance. The last section concludes the paper with possible implications and issues for future research.

2. Background and Relevant Literature

The impact of shareholder base on corporate behavior has been one of the most important questions that finance and legal scholars have examined in recent years. While a sweeping generalization is not possible, the existing research suggests that as the share of institutional ownership rises, firms tend to be better governed and to perform better. One of the biggest challenges in this area has been dealing with endogeneity: shareholder base can affect firm governance and performance, but the latter will presumably also affect what types of investors would decide to become shareholders.

A number of studies have examined the relationship between shareholder base and corporate behavior. [Aghion, Reenen, and Zingales \(2013\)](#), for instance, showed that greater institutional ownership is associated with more and better innovation, as measured by R&D spending. The study, based on long-term panel dataset of over 800 U.S. public companies, does not allow us to draw a concrete causal claim. Later studies attempt to deal with the reverse causality issue more directly by identifying certain exogenous changes in the ownership structure. By far the most popular change the scholars have utilized is the reconstitution of certain stock market indexes, such as Russell 1000 and 2000. The underlying story is that when a company's stock gets included (or excluded) from an index, this leads to an increase (or decrease) in institutional ownership particularly by (mostly) passive funds that rely on an index-based investment strategy. Utilizing such technique, [Boone and White \(2015\)](#) find that an inclusion in Russell 1000 and 2000 indexes leads to greater management disclosure, more analyst coverage, and higher liquidity. Using the similar technique and examining the impact on corporate governance more directly, [Appel, Gormley, and Keim \(2016\)](#) show that an inclusion in the indexes also lead to better governance structures, such as more independent directors, removal of antitakeover devices, and more equal voting rights. [Chen, Dong, and Lin \(2020\)](#) similarly find that the index inclusion leads to better CSR ratings, arguing that institutional shareholders can generate positive

social impact. However, [Wei and Young \(forthcoming\)](#) question the validity of using index reconstitution as an identification strategy, claiming this reflects a selection rather than treatment effect.

Other scholars have examined the behavior of institutional and retail shareholders more directly. With respect to retail shareholders, earlier research has shown that, while institutional shareholders actively participate in voting, retail investors generally show limited interest in voting their shares. According to [Kastiel and Nili \(2016\)](#), retail investors make up approximately 25% of the average public company's shareholder base, but their votes amount to only about 10% of votes at annual meetings. This lack of retail voting has been consistent over the past two decades, with retail investors voting only 29% of their shares, compared to the substantial 90% vote rate of institutional investors ([Broadridge 2015](#)). Research suggests that retail voter participation varies based on factors such as ownership stake, the company's return on assets, and the presence of ISS-opposed proposals on the ballot ([Brav, Cain, and Zytlick 2022](#)). Smaller firms, for example, tend to experience higher retail voter participation. From 2012 to 2015, the retail participation rate decreased by 1 percentage point annually, reaching 28% in 2016 and then increasing by 1 percentage point to 29% in 2017. In addition to being disinclined to vote in annual corporate meetings, retail investors also tend to exhibit different preferences from institutional investors in how they vote. [Brav, Cain, and Zytlick \(2022\)](#) document that ESG shareholder proposals tend to receive weaker support from retail investors as compared to the overall sample. Nevertheless, the low turnout of retail investors underscores the dominance of institutional investors in election results.

A natural question is why some firms carry a larger retail clientele than others. Two strands of the behavioral finance literature seem relevant. First, [Kumar \(2009\)](#) shows that, at an aggregate level, individual investors disproportionately hold stocks that are low-priced and exhibit high idiosyncratic volatility and positive return skewness, while institutional investors exhibit a relative aversion to those same stocks. This lottery-stock profile maps closely onto our treated firms: our results indeed confirm that firms with above-median non-institutional ownership are smaller, less profitable, more volatile, and lower-priced than other firms. Second, retail investors are drawn to attention-grabbing stocks: [Barber and Odean \(2008\)](#) show that individual investors disproportionately buy stocks in the news, with high abnormal volume, or with extreme recent returns, and [Barber, Huang, Odean, and Schwarz \(2022\)](#) find that this tendency is especially pronounced among users of commission-free platforms like Robinhood (as compared to other retail investors). This helps characterize the firms already favored by retail investors—the firms on which, we find, the abolition of trading commissions had the greatest stock price effect.

Beyond the cross-sectional question of which firms retail investors favor, a related literature examines what drives them into the market in the first place. We address the endogeneity concern by relying on the intuition that the extent of retail shareholding should depend on the entry costs faced by retail investors. Prior work has shown that individuals become more likely to enter the stock market after receiving a large sum of money, such as by way of bequest or winning a lottery ([Andersen and Nielsen 2011](#); [Briggs et al. 2021](#); [Vissing-Jorgensen 2002](#)). These authors suggest that even modest fixed entry costs play a significant role in determining retail investor participation in the financial system. We focus on the 2019 abolition of trading commissions by major online brokerages as a sudden decrease in entry costs for retail investors. Unlike the work on bequests and lotteries, this shock to entry costs is relevant to the U.S. markets and applies to a broad swathe of companies and investors. The abolition of trading commissions was unexpected and represents a true shock to entry

costs and does not suffer from the potential selection effect displayed by the Russell index classification strategy described earlier.

A brief background on the events may be helpful here. On October 1, 2019, the major online brokerage Charles Schwab eliminated commissions for all its customers. They were quickly followed by the remaining major online brokerages, TD-Ameritrade and E-Trade. Collectively, these three entities had dominated the online brokerage business. The significance of the wide introduction of zero-commission trading cannot be overstated. [Adams, Kasten, and Kelley \(2023\)](#) find that transaction costs for retail investors became an order of a magnitude smaller after the abolition of commissions in 2019 and represented a “wealth transfer from brokers to retail clients.” This significant decrease in entry costs for retail investors is consistent with contemporaneous reports that Schwab’s announcement “sent shock waves across Wall Street” ([Gittelsohn and Massa 2019](#)).³

The announcement of zero-commission trading on October 1, 2019 is a suitable setting for testing the effects of a sudden decrease in entry costs for retail investors for two reasons. First, it was unexpected. Multiple industry sources characterized the abolition of commissions as “unexpected” or a “surprise,” given that Schwab and its competitors had based their business model on trading commissions for decades ([Karim 2019](#); “[Schwab Launches Commission-Free Trades](#)” 2019). Second, Schwab’s announcement almost immediately led to the market concluding that zero-commission trading was the long-term reality. Charles Schwab, E-Trade, and TD Ameritrade shares dropped 10, 16, and 25 percent on October 1, 2019 ([Bernard 2019](#)) and industry experts concluded the same day that trading commissions were likely to systematically converge to zero ([Gittelsohn and Massa 2019](#)). Figure 1 plots stock prices and trading volumes for these three brokerages around the abolition of trading commissions. All three stocks show a dramatic decrease in price and upsurge in trading volume on October 1, 2019, the day Schwab announced the introduction of zero-commission trading.

Given the significant effects of even modest changes in entry costs on stock market participation from the prior literature, the abolition of trading commissions arguably should have increased retail investor participation in the capital markets and in turn exacerbated the influence of retail investors in U.S. corporate governance. This paper first investigates whether zero-commission trading did in fact lead to increased retail ownership at firms most affected by Schwab’s announcement on October 1, 2019. After finding an economically and statistically significant rise in retail ownership, it then examines what effects this change in ownership base had on corporate governance at these firms.

We define our treatment group of firms most affected by Schwab’s announcement as companies that experienced the highest abnormal returns on October 1, 2019 (as captured by the top quartile of the sample). These are the companies we expect to gain the most from retail investors’ lower barriers of entry. Firms that expect a significant influx of retail shareholders would experience excess returns around Schwab’s announcement because the market would reasonably anticipate that these companies would benefit from lower costs of capital. The clearest link between retail investor influence and firm value would be through these investors’ provision of higher liquidity ([Barot, Kaniel, and Sraer 2016](#)). The higher liquidity provided by retail investors could in turn reduce our treated

³ Although Robinhood Markets was already in operation then, their size was relatively small. Robinhood user base, in fact, increases significantly after the fourth quarter of 2019. See Robinhood’s Prospectus, available at: <https://www.sec.gov/Archives/edgar/data/1783879/000162828021015076/robinhood424.htm>.

group's cost of capital going forward (Amihud and Mendelson 2000, Diamond and Verrecchia 1991).⁴ Therefore, higher abnormal returns on October 1, 2019 are a proxy for a firm's exposure to an exogenous increase in retail investor interest after the introduction of zero-commission trading.

3. Theoretical Model and Hypothesis Development

What are the implications of increased retail ownership on corporate governance and the agenda promoted by institutional investors? A priori, there is no clear answer. On the one hand, the decrease of non-institutional ownership might suggest that institutional investors' voting power will be reduced; thus, one might suspect that their preferred corporate policies may be less likely to be implemented. On the other hand, as the literature review illustrates, retail investors are in general much less likely to vote (or to attempt to indirectly influence management) compared to institutional investors. This pattern might indicate that, with more passive retail investors, institutional investors—despite their reduced overall ownership—may wield *comparatively* greater voting power within the universe of the shares voted. For this reason, the critical inquiry is which investor group is being replaced by the incoming retail investors, and how their respective voting patterns differ. In this section, we consider a simple model to analyze these dynamics.

Suppose there are three groups of shareholders: (1) passive institutional shareholders; (2) active institutional shareholders; and (3) retail shareholders.⁵ Consistent with the findings from Appel, Gormley, and Keim (2016), we assume that passive institutional shareholders (such as index mutual funds and pension funds with diversified holdings) do not exit the company and always vote and exercise their franchise rights as shareholders. At the same time, both active institutional shareholders (such as hedge funds or actively managed mutual funds) and retail shareholders are more likely to engage in entry and exit and are comparatively less likely to exercise their shareholder franchise rights. From this perspective, we note that the setting for the influx of retail investors in a firm's stocks does not exactly mirror the setting in which a firm's stock gets included in a stock market index. The latter setting will tend to increase passive institutional investors' holding, while the current setting is assumed to affect active institutional investors' holding.

Suppose, initially, that there are three types of shareholders and the fractions of respective types of shareholders are given by: α for the passive institutional shareholders, β for the active institutional shareholders, and γ for the retail shareholders, where $\alpha + \beta + \gamma = 1$.⁶ As mentioned earlier, while passive institutional shareholders always vote, the probability that the active institutional shareholders vote on any given corporate governance matters is $\lambda \in [0,1]$ and the corresponding probability for the retail shareholders is $\mu \in [0,1]$. Given the generally low level of participation by retail shareholders, we assume $\lambda > \mu$. Furthermore, conditional on voting, the probability that the

⁴ Notably, meme stock companies, such as GameStop and AMC, after experiencing a surge in their stock prices, raised large sums of money by issuing new equity to the investors (Aggarwal, Choi, and Lee 2024).

⁵ It is important to distinguish between active institutional investors (who trade according to information) and passive institutional investors (who consistently hold on to their shares). Although retail investors can also actively trade based on the news of October 1, 2019, the eventual increased retail ownership suggests that some institutional investors sold their shares in respond to the news, and these were in turn purchased by newly-entered retail investors.

⁶ Presumably, the shares of outstanding stock owned by (or respective demands for stock by) passive institutional, active institutional, and retail shareholders would be determined based on some company and market characteristics. We are shying away from delving into those more fundamental questions in this overly simplified model. In terms of increase in retail ownership ($\delta_3 > 0$), without formally modeling that aspect, we can attribute that to lowering of the cost of stock ownership, for instance, through zero commission on trading.

active institutional shareholders' votes are consistent with those of passive institutional shareholders is $\rho \in [0,1]$ and the corresponding probability for the retail shareholders is $\sigma \in [0,1]$. While it may be natural to assume that $\lambda > \mu$, whether active institutional shareholders vote more like passive institutional shareholders, compared to retail shareholders, may likely depend on the issue. Hence, we assume that $\rho \gtrless \sigma$.

With these parameters, when there is an issue to be voted on, the fraction of shares that cast votes will be equal to $\alpha + \beta\lambda + \gamma\mu < 1$. The fraction of votes that endorse the passive institutional shareholders' preferences are given by $\alpha + \beta\lambda\rho + \gamma\mu\sigma < 1$. The fraction of non-votes is given by $\beta(1 - \lambda) + \gamma(1 - \mu)$ and the fraction of shares that vote against the passive institutional shareholders' preferences is given by: $\beta\lambda(1 - \rho) + \gamma\mu(1 - \sigma)$.⁷

One straightforward way of measuring the passive institutional shareholders' influence on voting matters is to take the difference between the fraction of votes that align with the passive institutional shareholders' preferences and the fraction of votes that are against. When we do so, we get:

$$\alpha + \beta\lambda\rho + \gamma\mu\sigma - (\beta\lambda(1 - \rho) + \gamma\mu(1 - \sigma)) = \alpha + \beta\lambda(2\rho - 1) + \gamma\mu(2\sigma - 1).$$

Note that, conditional on $\rho > 1/2$ and $\sigma > 1/2$, as $\beta\lambda$ or $\gamma\mu$ increases, the passive institutional shareholders' degree of influence over corporate policies will increase. The opposite will be true when $\rho < 1/2$ or $\sigma < 1/2$. In other words, the expressions $(2\rho - 1)$ and $(2\sigma - 1)$ tell us how closely active institutional shareholders' and retail shareholders' preferences are aligned with those of the passive institutional shareholders. All of this is prior to the new influx of retail investors.

Now suppose there is a change in shareholder base in such a way that the three respective types of shareholders' ownership fractions changes to $\alpha + \delta_1$, $\beta + \delta_2$, and $\gamma + \delta_3$, where $\delta_i \gtrless 0$ and $\delta_1 + \delta_2 + \delta_3 = 0$. If we are thinking of a scenario where passive institutional shareholders are increasing their ownership interest while the other two types are exiting (as in the case of index reconstitution), we would have $\delta_1 > 0$ while $\delta_2 < 0$ and $\delta_3 < 0$. Similarly, if retail shareholders are entering the shareholder base while some institutional shareholders are exiting, we would have $\delta_3 > 0$ while $\delta_1 < 0$ and $\delta_2 < 0$. When the shareholder composition changes, given the different propensities of voting, the overall participation rate will also change. After the change, the fraction of votes becomes:

$$(\alpha + \delta_1) + (\beta + \delta_2)\lambda + (\gamma + \delta_3)\mu = \alpha + \beta\lambda + \gamma\mu + (\delta_1 + \delta_2\lambda + \delta_3\mu).⁸$$

⁷ We can allow for all three ways of casting one's vote on a proposal: vote yes, vote no, or abstain. To the extent that other shareholders' votes do not align with the passive institutional shareholders' vote, we can treat them as being against the passive institutional shareholders' expressed preferences.

⁸ The model assumes that the incumbent retail investors and the newly entered retail investors have the same probability of voting their shares. But this overlooks the fact that these are different groups of retail investors: the former group was already invested in the capital markets, while the latter group was deterred from entering the market due to small trading commission fees. In practice, there may be a reason to believe that the latter may be even less likely to vote (than the former), given their sensitivity to transaction costs.

With the identity of $\delta_1 = -\delta_2 - \delta_3$, the last expression in the parenthesis becomes $-\delta_2(1 - \lambda) - \delta_3(1 - \mu)$. Not surprisingly, when $\delta_1 > 0$, $\delta_2 < 0$, and $\delta_3 < 0$, we see that $-\delta_2(1 - \lambda) - \delta_3(1 - \mu) > 0$ and the fraction of shares that vote increases. By contrast, when $\delta_3 > 0$, $\delta_1 < 0$, and $\delta_2 < 0$, assuming that $\lambda > \mu$, $\delta_1 + \delta_2\lambda + \delta_3\mu = \delta_1(1 - \mu) + \delta_2(\lambda - \mu) < 0$ and the fraction of shares that vote decreases because the retail investor group was assumed to be the least likely to vote among the three shareholder groups.

Remark 1: If retail shareholders are less likely to vote than either passive or active institutional investors ($1 > \lambda > \mu$), as the share of retail shareholders increases while the shares of active and passive institutional investors decrease ($\delta_3 > 0$, $\delta_1 < 0$, and $\delta_2 < 0$), the fraction of shares that vote decreases ($\delta_1 + \delta_2\lambda + \delta_3\mu = \delta_1(1 - \mu) + \delta_2(\lambda - \mu) < 0$).

What about the influence of the passive institutional shareholders? With the change in shareholder base, the measure of the passive institutional shareholders' influence becomes:

$$\begin{aligned} & (\alpha + \delta_1) + (\beta + \delta_2)\lambda(2\rho - 1) + (\gamma + \delta_3)\mu(2\sigma - 1) \\ & = \alpha + \beta\lambda(2\rho - 1) + \gamma\mu(2\sigma - 1) + \{\delta_1 + \delta_2(\lambda(2\rho - 1)) + \delta_3\mu(2\sigma - 1)\}, \end{aligned}$$

where the last expression in the curly brackets measures the change in the passive institutional shareholders' influence. As earlier, with the identity of $\delta_1 = -\delta_2 - \delta_3$, we get: $\delta_1 + \delta_2(\lambda(2\rho - 1)) + \delta_3\mu(2\sigma - 1) = \delta_2(\lambda(2\rho - 1) - 1) + \delta_3(\mu(2\sigma - 1) - 1)$. Given the assumptions on $\{\lambda, \mu, \rho, \sigma\}$, we have: $\lambda(2\rho - 1) - 1 < 0$ and $\mu(2\sigma - 1) - 1 < 0$.

Two special cases of interest are: (1) when passive institutional shareholders increase their ownership interest while the other two types of shareholders exit, and (2) when retail shareholders increase their ownership while the (active) institutional shareholders exit. First, when $\delta_1 > 0$, $\delta_2 < 0$, and $\delta_3 < 0$, $\delta_2(\lambda(2\rho - 1) - 1) + \delta_3(\mu(2\sigma - 1) - 1) > 0$. That is, when the passive institutional shareholders increase their holdings, their influence over corporate policies grows.

The result is more ambiguous, however, for the second case: when $\delta_3 > 0$, $\delta_1 < 0$, and $\delta_2 < 0$, $\delta_2(\lambda(2\rho - 1) - 1) + \delta_3(\mu(2\sigma - 1) - 1) \gtrless 0$. This is because while $\delta_3(\mu(2\sigma - 1) - 1) < 0$, when $\delta_2 < 0$, we get $\delta_2(\lambda(2\rho - 1) - 1) > 0$. When retail shareholders increase their holdings while the institutional shareholders exit, the passive institutional shareholders' influence can either increase or decrease. The answer depends on, among others, relative alignment of passive institutional shareholders' preferences with those of active institutional shareholders (ρ) and the retail shareholders (σ). For instance, when active institutional shareholders' preferences are not aligned with those of passive institutional shareholders, i.e., $\rho \approx 0$, we are more likely to have $\delta_2(\lambda(2\rho - 1) - 1) + \delta_3(\mu(2\sigma - 1) - 1) > 0$ and the passive institutional shareholders' influence will in fact *grow* as retail shareholders increase their ownership.

Remark 2: As the fraction of retail shareholders increases ($\delta_3 > 0$), the influence of passive institutional investors can either increase or decrease ($\delta_2(\lambda(2\rho - 1) - 1) + \delta_3(\mu(2\sigma - 1) - 1) \gtrless 0$).

More generally, whether the large passive institutional shareholders' influence will increase or decrease will depend on, among others, two important factors: (1) probability of each shareholder type exercising their franchise rights (λ and μ) and (2) the propensity of each shareholder type voting in line with the passive institutional shareholders ($2\rho - 1$ and $2\sigma - 1$). If we were to assume that the retail shareholders' participation rate is lower than that of active institutional shareholders, $\lambda > \mu$, when the degrees of preference alignment are positive but about the same, i.e., $2\rho - 1 \approx 2\sigma - 1 > 0$, the shift of shareholder base towards retail shareholders will decrease the influence of the passive institutional shareholders. Presumably, the propensity for each shareholder type to exhibit voting behavior that is aligned with the passive institutional shareholders will depend on the issue. One would assume that the retail shareholders' preference alignment is different between environmental and social issues compared to governance issues. More specifically, [Chen, Dong, and Lin \(2020\)](#) and [Brav, Cain, and Zytynick \(2022\)](#) suggest that (passive) institutional investors are more likely than retail investors to vote in favor of adopting pro-ESG measures. There is also evidence that active institutional investors, such as hedge funds or index funds, are actively supporting ESG measures ([Barzuza, Curtis, and Webber 2020](#); [Nathan 2018](#)). In terms of our model, this would suggest that $\rho > \sigma$, and thus, when $\delta_3 > 0$, $\delta_1 \approx 0$, and $\delta_2 < 0$, the overall effect would tend to be diluting passive institutional investors' voting power.

4. Data and Summary Statistics

This paper's empirical analysis is based on standard data sources. We collect information for the period from three years before to three years after the abolition of trading commissions (i.e., between October 1, 2016 and October 1, 2022). We drop all firms belonging to the finance sector (SIC codes 6000–6999), because these firms may have experienced abnormal returns due to the direct impact on their revenue models, not necessarily due to the effect of an influx of retail owners. Table 1 collects the definitions of the different variables used in the paper's empirical analysis. Table 2 presents summary statistics for the different tables used in the empirical analysis in Panel A.

One of the outcome variables of the paper is the fraction of a company's shares owned by non-institutional holders. While the institutional background and theoretical model detailed in Sections 2 and 3, respectively, outline a (predictive) link between the advent of zero-commission trading and retail ownership, we lack firm-level data about the percentage of shares held by retail participants. Instead, following the literature, we proxy retail ownership by the fraction of shares not held by institutional investors. The data for institutional ownership comes from the Thomson Reuters 13-F institutional holdings dataset. This dataset contains information about the fraction of a company's shares held by relatively large institutional investors.⁹ Following [Backus, Conlon, and Sinkinson \(2021\)](#), we construct a continuous variable *Non-Institutional Ownership* to equal 1 minus the fraction of total institutional ownership reported in 13-F filings, winsorized at the 2.5% level. When we compare firms with above- and below-median levels of non-institutional ownership (for either the full sample or solely the pre-zero-commission period), retail investors seem to “prefer” companies that are smaller, have lower return on assets, have higher cash ratios, are less leveraged, and have lower stock prices.

⁹ Institutions with assets under management below \$100 million are not obligated to file 13-F reports. See <https://www.sec.gov/divisions/investment/13ffaq>.

We use standard sources for firm stock prices and time-varying financial controls. Stock price data comes from the Center for Research in Security Prices (CRSP). We also use CRSP to calculate stock volatility, defined, following [Larcker et al. \(2015\)](#), as the natural logarithm of the standard deviation of the firm’s daily returns. We use Compustat to get information for firm size, as proxied by the natural logarithm of firm assets, profitability as measured by return on assets (net income divided by assets, as in [Heese, Pérez-Cavazos, and Peter 2022](#)), and cash and debt ratios. We lag all financial controls by one year to ensure they are pre-determined and not affected by the treatment itself. Despite the fact that we lag them, there could be some concern that these time-varying controls are endogenously affected by the introduction of zero-commission trading: for instance, the profitability of firms popular with retail investors could change after new retail investors purchase their stock after 2019 ([Gormley and Matsa 2016](#)). We winsorize all financial controls at the 2.5% level.

We collect information on firm ESG performance using the ESG scores reported in the MSCI ESG Ratings data. The MSCI data is the standard measure for ESG in the corporate finance literature, and is used by banks, insurers, and institutional investors when incorporating ESG considerations into their investment decisions ([Chen, Dong, and Lin 2020](#); [Pástor, Stambaugh, and Taylor 2022](#)). We collect information about firms’ overall industry-adjusted ESG scores, as well as their sub-scores on the environmental (“E”), social (“S”), and corporate governance (“G”) “pillars,” as reported by the MSCI.

We acknowledge that there is a robust debate about whether ESG is relevant to firm performance ([Aswani et al. 2024](#); [Berk et al. 2025](#); [Bolton and Kacperczyk 2021](#); [Khan et al. 2016](#)), and some scholars contend that ESG metrics like those of MSCI could contain measurement errors ([Berg et al. 2022](#)). However, we view the debate about the utility and measurement of ESG as outside the scope of this paper. Instead, we start from the premise that at least some investors and experts view ESG as relevant to investment decisions ([Tang, Yan, and Yao 2026](#)), and document a change in the ESG—and specifically governance—scores of the treated firms in our empirical design using the ESG measure that is most widely accepted in the finance literature.

We obtain information on shareholder voting at the proposal level from the Institutional Shareholder Services (ISS) database. Following [Geoffroy \(2018\)](#), we define shareholder non-participation as the percentage of outstanding votes that were not cast for or against the proposal or marked as abstentions.¹⁰ We winsorize the shareholder non-voting rate at the 2.5% level.

5. Zero-Commission Trading and Retail Ownership

In this section, we establish the two central results that inform our argument on the effect of retail investors’ entry costs and their influence on corporate governance. First, we show that firms that were most likely to experience an increase in retail ownership after the abolition of trading commissions experienced positive abnormal returns on October 1, 2019. We proxy this expectation of an exogenous increase in retail ownership after the reduction of entry costs by the share of non-institutional ownership just before October 1, 2019. Second, and more importantly, we find that stocks experiencing higher abnormal returns on October 1, 2019 subsequently saw a significant increase in

¹⁰ This definition implies that broker non-votes would be counted alongside other non-votes. However, we do not think this affects our analysis of non-voting, since the institutional environment surrounding broker non-voting is invariant across the sample period. Any relative change in non-votes at treated firms is thus plausibly attributable to the abolition of trading commissions rather than any feature of broker non-voting.

non-institutional ownership (based on the 13-F data), compared to stocks that experienced lower abnormal returns. This differential uptick in non-institutional ownership suggests that the abnormal returns on October 1, 2019 predict future growth in retail ownership.

We first conduct an event study using the standard Fama-French three-factor model to estimate the abnormal return for stocks in the Compustat database. We use a 100-trading-day estimation window to measure the expected returns for each stock, as in [Takaoka \(2006\)](#), with no gap between the estimation and event windows. All our results are robust to instead estimating abnormal returns using a longer 255-day estimation window and allowing for a 45-day buffer between the estimation and event windows. We define the event window as October 1, 2019, the day the brokerages announced that they will introduce zero-commission platforms.

There is strong reason to believe that the market priced in the effect of zero-commission trading on the day of the brokerages' announcement itself. Charles Schwab, E-Trade, and TD Ameritrade shares dropped 10, 16, and 25 percent on October 1, 2019 itself ([Bernard 2019](#)). According to one source, Schwab's announcement on October 1 "sent shock waves across Wall Street," and industry experts concluded the same day that zero-commission trading was the inevitable market equilibrium ([Gittelsohn and Massa 2019](#)). Figure 1 visually shows how these brokerage stocks saw dramatic decreases in price (Panel A) and substantial increases in trading volume (Panel B) on October 1, 2019. We only keep firms for which we have at least 70 trading days of return information during the estimation window. Our results persist if, instead of the Fama-French model, we use the four-factor model of [Carhart \(1997\)](#).

To assess whether the abolition of trading commissions differentially impacted firms that are (already) popular with retail investors, we regress the abnormal return on October 1, 2019 against non-institutional ownership as measured just before the Schwab announcement. Institutional ownership in the 13-F data is reported on a quarterly basis. Coincidentally, September 30 of every year is one of the reporting dates. We fortuitously have the non-institutional ownership for each firm for the day just before Schwab's announcement. Panel B of Table 2 compares the non-institutional ownership from September 30, 2019 as well as basic financial variables for firms with above- and below-median abnormal returns on the day of Schwab's announcement. Panel B shows that firms with above-median abnormal return had 9.33% higher non-institutional ownership just before the wide introduction of zero-commissions (0.363 versus 0.332), and this difference was statistically significant ($p < 0.01$). Firms with above-median abnormal returns were larger than other companies ($p < 0.1$), as well more profitable ($p < 0.01$) and more volatile ($p < 0.1$). We also compared these firms' cash ratio and debt ratio. Firms with above-median abnormal returns were more leveraged and had lower cash ratios than other companies, but neither of these differences was statistically significant.

Table 3 presents the results of the event study regression. As is standard in event studies, we include industry (as measured by Fama-French 17 industry group, as in [Güner et al. \(2008\)](#) and [Kim et al. \(2025\)](#)) fixed effects and cluster standard errors at the industry level.¹¹ Non-institutional ownership has a positive and highly significant coefficient. The results in column (1) imply that a one-standard deviation increase in non-institutional ownership as of September 30, 2019 corresponds to an abnormal return on October 1, 2019 that is 0.132 percentage points higher. Column (2) shows that a firm that was in the top quartile of non-institutional ownership as of September 30, 2019 experienced an abnormal return that was 0.295 percentage points higher. Table 3 therefore uses market data to

¹¹ The results are qualitatively similar if we proxy industry by the two-digit SIC code.

show that the unexpected nature of the commission abolition had an economically significant effect on firms that could ex-ante expect to experience an increase in retail ownership.

We next establish our main result: that the firms that experienced positive and significant abnormal returns on October 1, 2019 subsequently also saw their ownership base become less institutional-dominated. If the elimination of trading commissions reduces entry costs and expected retail growth increases firm value, then abnormal returns should be correlated with retail growth. This is because a reduction in retail entry costs is a demand shock especially affecting the retail clientele. If markets accurately estimate the expected rise in retail demand for firms, the firms that experienced the highest abnormal returns around the Schwab announcement would likely be those whose marginal investor is retail, which is why we expect the rise in retail ownership to be largest there. Because demand curves for individual stocks slope down (Shleifer 1986), the market capitalizes the anticipated retail inflow on the announcement date. Therefore, the abnormal return is the market's forward-looking estimate of where retail demand will concentrate, and firms with the highest abnormal returns on October 1, 2019 should, on average, see greater increases in retail ownership in the post period.

We create two different proxies to capture the companies that had the largest stock price reaction to zero-commission trading. First, we set a continuous variable $Treat_1$, equal to the abnormal return the company experienced on October 1, 2019. Second, we construct a dummy variable $Treat_2$, which equals 1 if the firm's abnormal return on October 1, 2019 was in the sample's top quartile.

We estimate the following regression model for firm i in quarter q and year y :

$$\begin{aligned} Non - Institutional Ownership_{iq} &= \alpha Post_q + \gamma Treat_i + \beta Post_q \times Treat_i + \pi X_{iy} + \delta_i + \theta_q \\ &+ \varepsilon_{iq} \end{aligned} \tag{1}$$

The dependent variable, $Non - Institutional Ownership_{iq}$, is the ownership stake of non-institutional investors (based on the 13-F data). Following Backus, Conlon, and Sinkinson (2021), we use this quantity as a proxy for retail holders' ownership stake in the firm. Equation (1) uses data between October 1, 2016 and October 1, 2022—three years before and after the abolition of commissions. $Post_q$ is a dummy equaling 1 for the period after October 1, 2019, while $Treat_i$ captures the firm's abnormal return calculated using the Fama-French model the day commissions were abolished (through either the $Treat_1$ or $Treat_2$ proxies). X_{iy} is a vector of the lagged firm financials mentioned in Section 4. δ_i and θ_q are firm and quarter fixed effects, whose inclusion allows us to control for firm-specific idiosyncratic factors as well as secular time trends. We cluster the standard errors at the firm level.

Table 4 presents the results from estimating equation (1). Columns (1) and (3) use $Treat_1$ as the proxy for the reaction to the abolition of commissions, while columns (2) and (4) use $Treat_2$. In all four specifications, the interaction between $Post_q$ and the treatment term has a positive and statistically significant coefficient. The baseline result in column (1) implies that a one-standard deviation increase in abnormal returns on October 1 corresponds to a subsequent rise in non-institutional ownership of 0.74 percentage points. This rise equals 2.08% (2.87%) of the mean (median) non-institutional ownership in the sample. The baseline result in column (2) implies that a firm's abnormal return on October 1, 2019 being in the top quartile leads to a 1.53 percentage point

increase in non-institutional ownership. This rise equals 3.79% (5.23%) of the sample mean (median) non-institutional ownership. The abolition of trading commissions is thus associated with a statistically and economically significant rise in non-institutional ownership in firms most affected by Schwab's announcement on October 1, 2019, relative to other firms and controlling for firm fixed effects and time trends.¹²

A central contention of this paper is that the reduction in retail investors' entry costs due to the introduction of zero-commission trading likely led to (or is predictive of) an increase in non-institutional ownership for firms that saw the largest abnormal returns in response to Schwab's October 1, 2019 announcement. However, one potential concern with this claim is that these firms may have been gaining retail owners in preceding years as well, with the observed change having little to do with zero-commission trading.

To address this concern, we estimate a dynamic version of the baseline model in column (1) of Table 4, replacing $Post_q$ with a series of dummy variables for the individual years around the abolition of trading commissions. We denote T as the period between October 1, 2019 and October 1, 2020, i.e., the year immediately after the introduction of zero-commission trading. Following [Gopalan, Gormley, and Kalda \(2021\)](#), we set the year preceding the treatment ($T-1$) as the base year. Figure 2 presents the results of this dynamic model, displaying the 95% coefficients for the interaction between abnormal returns and the year dummies. We do not find any evidence of a pretrend in non-institutional ownership for the treatment group in the years preceding the introduction of zero-commission trading. In Table 5, we present the results from re-running Equation (1) in 2016-19, the pre-abolition period, with the coefficient of interest now corresponding to the interaction between $Treat$ and the continuous time (year) variable. This coefficient is not significant in any specification, underlining the lack of pretrends.

Despite the lack of significance of any pre-period coefficient in Figure 2 and the null results in Table 5, one may argue that there seems to be a visual upward trend. We acknowledge this visual trend, and partially address it by running a joint F-test that the pre-period coefficients ($T-4$ through $T-2$) are statistically indistinguishable from zero. This joint test fails to reject the null hypothesis ($F(3, 2837) = 0.46, p = 0.71$), further suggesting there are no differential pretrends.

Beyond pretrends, another concern with our results is that the coefficient of interest is both attenuated and less significant, with $p < 0.1$ rather than $p < 0.05$, in the models including all financial covariates. We address this concern in two ways. First, there is a theoretical reason for significance weakening once we include financial controls. Retail trading activity could be correlated with these covariates, especially volatility: [Foucault, Sraer, and Thesmar \(2011\)](#), for example, show that retail trading causally increases stock volatility. Even though we lag firm financials by a year, Figure 2 suggests that the effect of trading commission abolition builds up in the post-period. This could make volatility, along with other financial covariates a "bad control" as described by [Angrist and Pischke \(2009\)](#) as well as [Gormley and Matsa \(2016\)](#), whose inclusion works against finding an effect.

¹² It should be noted that, while the announcement was made on October 1, 2019, the commissions were not actually abolished until a week later. Accordingly, the influx of retail investors would not have occurred until sometime after October 1, 2019. To this extent, our first result suggests expectations by early responders as to how companies that were already popular among retail investors would face an increased demand for their stocks from the expected influx of retail investors, and our second result confirms such expectations.

Second, and more concretely, we re-run Equation (1) using three alternative regression models. Figure 3 presents results from estimating p -values using the wild cluster bootstrap method described in [Roodman et al. \(2019\)](#), drawn from 999 replications. The points at which the confidence curve in the figure crosses the relevant dashed reference lines delineates the bounds of the 90% and 95% confidence intervals. Regardless of what measure of *Treat* we use, the results retain their significance: $p < 0.05$ for the baselines, and $p < 0.1$ for the full model.

We also re-run equation (1) on samples where we balance the covariates between treated firms (those in the top quartile of abnormal returns, i.e., with *Treat*₂ equal to 1) and others using the entropy-balancing ([Hainmueller 2012](#)) and inverse probability weighting ([Wooldridge 2010](#)) techniques. We perform this balancing based on the pre-zero-commission means for the financial controls used in our analysis. The results in Table 6 show that our finding—firms that experienced higher abnormal returns on October 1, 2019 subsequently saw an increase in retail ownership—is undisturbed by either balancing method.¹³

Therefore, while the coefficient of interest is less significant in the model with all financial covariates, this could be driven by a bad controls problem and the correlation of elevated retail investing with some of these covariates. Moreover, the persistence of results across four different regression approaches provides some reassurance that our findings are robust and economically meaningful.

Even if one were to be unconvinced of the results regarding the post-abolition increase in retail holdings documented in Table 4, the event study evidence from Table 3 is economically meaningful standing alone. When entry costs for retail investors fell thanks to the Schwab announcement, the firms most exposed to retail demand became more attractive to hold and cheaper to trade, and the market capitalized that expected benefit immediately. These firms could have become more attractive because, as discussed in Section 2, greater retail participation raises liquidity, reducing a stock's required return and thus raising the price ([Amihud and Mendelson 2000](#), [Diamond and Verrecchia 1991](#)). Because demand curves for individual stocks slope down ([Shleifer 1986](#)), the *expectation* of future retail inflows could raise prices on announcement, regardless of how much inflow ultimately materializes or how precisely it can later be measured.

There is also a measurement issue: the abnormal return from the event study is a clean, single-day measure easily captured using stock price data. On the other hand, the subsequent change in retail ownership in Table 4 is measured using 13-F data, which is a noisier proxy ([Backus, Conlon, and Sinkinson 2021](#)). It is therefore unsurprising that the announcement return is estimated more sharply than the realized ownership change. While the credibility of the announcement-day result does not hinge on the ownership analysis, we think there is sufficient reason to be reasonably confident in both findings.

¹³ The persistence of the results using reweighting methods also provides assurance that our results are not driven by firms that experienced positive abnormal returns being fundamentally different from other companies. Under such a theory, these companies may have gained non-institutional owners after 2019 not because of zero-commission trading, but because of other observable factors, such as greater profitability.

6. The Governance Effects of Retail Investor Entry

The influx of retail investors into certain companies after the sudden elimination of the trading commission had concrete corporate governance implications. In this section, we examine how the rise in retail ownership at firms most affected by the 2019 abolition of trading commissions changed shareholder non-voting and ESG scores. However, we note that the results in this section should be treated with lower epistemological confidence than the findings regarding abnormal returns and retail ownership from earlier. This is because multiple factors unrelated to trading commissions could possibly determine a firm's governance arrangements.

6.1 Shareholder Non-Voting

An important corporate governance outcome variable we study is shareholder non-voting. As explained earlier, we measure non-voting at the proposal level, and define it as the percentage of outstanding shares not cast for or against a proposal or marked as abstentions. We first estimate the following OLS regression model to assess changes in shareholder-nonvoting for proposal p voted on by shareholders of firm i in year y :

$$\begin{aligned} \text{Non} - \text{Voting}_{piy} &= \alpha \text{Post}_p + \gamma \text{Treat}_i + \beta \text{Post}_p \times \text{Treat}_i + \pi X_{iy} + \delta_i + \theta_y \\ &+ \varepsilon_{piy} \end{aligned} \quad (2)$$

The variables on the right-hand side of the equation have the same definitions as explained earlier in equation (1). Once again, we cluster standard errors at the firm level. Our conjecture is that the coefficient for the interaction term $\text{Post}_p \times \text{Treat}_i$ will be positive and significantly different from zero. This is because most non-participation in shareholder voting is typically attributed to retail investors.¹⁴ If the firms that experienced the highest abnormal returns on October 1, 2019—i.e., those with the largest values of Treat_i —saw the greatest subsequent increase in retail ownership, as shown in Table 4, we should expect them to also see an increase in shareholder non-participation.

Table 7 presents the results from estimating equation (2). To proxy for Treat_i , we use not just Treat_1 and Treat_2 , which are based on the abnormal return on October 1, 2019, but also a variable called “Retail Growth Ratio,” which is the ratio of non-institutional ownership on December 31, 2019 (the first quarter where 13-F data is reported after the abolition of commissions) to non-institutional ownership on September 30, 2019 (the last reporting date before the abolition). We winsorize this variable at the 2.5% level. Note that the inclusion of year fixed effects does not wash away the main effect of Post_p , since proposals in the year 2019 may have Post_p equal to 0 or 1 depending on whether the relevant meeting was before or after October 1, 2019.

Across all six specifications in Table 7, using either the returns-based Treat_1 and Treat_2 , or retail ownership-based Retail Growth Ratio proxy, the coefficient estimate for the interaction between Post and Treat_i is positive and significant. This strongly indicates that shareholder non-voting went up for companies that were most heavily affected by the abolition of trading commissions. The

¹⁴ This prediction follows directly from the model in Section 3 (including Remark 1): an increase in retail ownership ($\delta_3 > 0$), at the expense of active institutional investors ($\delta_2 < 0$), raises the fraction of non-votes $(1 - (\alpha + \delta_1) - (\beta + \delta_2)\lambda - (\gamma + \delta_3)\mu)$, as retail shareholders exercise their franchise rights with lower probability than institutional holders ($\mu < \lambda$).

baseline specification in Column (1) shows that a one-standard deviation increase in abnormal returns in response to the Schwab announcement is associated with a subsequent rise in non-voting by 0.40 percentage points, which equals 1.71% (2.03%) of the mean (median) level of non-voting in the regression sample. Column (2) finds that a stock belonging to the top quartile of abnormal returns saw a 1.6 percentage point rise in non-voting. This equals 6.87% (8.18%) of mean (median) shareholder non-voting in the regression sample. Finally, a one-standard deviation increase in Retail Growth Ratio is associated with a subsequent rise in non-voting by 0.46 percentage points, which equals 1.97% (2.34%) of the mean (median) level of non-voting in the regression sample.

In Figure 4, we estimate a dynamic version of Equation (2), interacting abnormal returns on October 1, 2019 with dummy variables for years around the abolition of trading commissions. We do not find any significant pretrends in the period before the Schwab announcement, suggesting that our results on non-voting are in fact driven by the introduction of zero-commission trading.

The non-voting results are consistent with retail investors being much less likely to vote their shares (than institutional investors) and the influx of retail shareholders (replacing certain institutional investors) thus reducing the overall votes cast. It is notable that elimination of relatively small trading costs by the online brokerages translated to a sustained increase in shareholder non-participation at affected companies.

6.2 ESG Scores

While shareholders traditionally exert influence in corporate governance by voting on director elections and proposals initiated by management or shareholders (Easterbrook and Fischel 1983), they may also affect other dimensions of governance. For instance, Brav, Cain, and Zytznick (2022) find that retail investors are less likely than institutional investors to support ESG proposals. This suggests that the entry of retail investors after 2019 may have led to a decrease in ESG metrics at the firms most affected by zero-commission trading. We first estimate the following OLS model for the ESG scores of company i whose ESG score is reported in the MSCI data on date t in year y :

$$ESG_{ity} = \alpha Post_t + \gamma Treat_i + \beta Post_t \times Treat_i + \pi X_{iy} + \delta_i + \theta_y + \varepsilon_{ity} \quad (3)$$

Panel A of Table 8 presents the results from estimating equation (3). The results are consistent with ESG scores decreasing at firms affected by Schwab's announcement, with the interaction between $Post$ and both proxies for abnormal returns bearing a negative and significant coefficient. Column (1) entails that a one-standard deviation increase in abnormal returns is associated with a later decrease in ESG score equal to 1.48% (1.48%) of the mean (median) ESG rating in the regression sample. In Column (2), firms with abnormal returns in the top quartile saw a subsequent fall in ESG equal to 4.12% (4.12%) of the mean (median) sample value. This is consistent with Brav, Cain, and Zytznick's (2022) contention that retail investors are less likely to support ESG proposals. In Figure 5, we estimate a dynamic version of equation (3) to show that there are no pretrends: none of the coefficients for the interaction of abnormal returns on October 1, 2019 with year dummies before the abolition of commissions is significant.

To investigate further which aspect of the treated firms' ESG scores decreased after the wide introduction of zero-commission trading, we re-estimate equation (3) separately using the three

“pillar” scores for environmental (“E”), social (“S”), and corporate governance (“G”) issues as the dependent variable. Panels B, C, and D of Table 8 present the results of these empirical specifications. Interestingly, we find no statistically significant changes in treated companies’ MSCI environmental (E) or social (S) scores after the abolition of trading commissions. Meanwhile, in Panel D, we find that treated firms saw a statistically significant decrease in their MSCI corporate governance (G) scores after October 1, 2019. A one-standard deviation increase in abnormal returns in response to the Schwab announcement is associated with a later decrease in corporate governance score equal to 1.8% (1.76%) of the mean (median) governance pillar score in the regression sample. Similarly, firms with abnormal returns in the top quartile saw a subsequent fall in ESG equal to 2.68% (2.61%) of the mean (median) sample value.

Overall, our finding of the decrease in ESG scores is consistent with an increase in the (relative) influence of retail investors, who typically exert less pressure on corporations to prioritize ESG than institutional players (Brav, Cain, and Zytznick 2022, Chen, Dong, and Lin 2020).¹⁵ As the ownership stake of ESG-conscious institutional investors decreased for firms most affected by the 2019 abolition of commissions, these companies may have faced less pressure to pay attention to these metrics. Consistent with this theory, the ESG scores of treated firms decreased by approximately the same proportion that their retail ownership rose. However, when we separately analyze the “E,” “S,” and “G” factors that comprise the widely-used ESG acronym, we find that this decrease is concentrated in the corporate governance of treated firms. There is no significant change in these companies’ performance on environmental or social fronts. This, too, is somewhat consistent with Brav, Cain, and Zytznick (2022), whose results also indicate that the greatest gulf between retail and other shareholders is on “G” rather than “E” or “S” issues. Thus, zero-commission trading and the resultant influx of retail investors seems to have led to increased shareholder apathy at the most affected firms, but does not seem to have made them more or less “prosocial.”

7 Robustness Tests

In this section, we supplement the results with three robustness tests. First, we find evidence that retail-heavy firms also experienced positive and significant abnormal returns in response to the introduction of zero commission by a smaller brokerage firm, Interactive Brokers, consistent with our thesis that entry costs matter for retail participation in the stock market. Second, we examine the introduction of fractional trading (introduced by some brokerage firms), that plausibly made it easier for retail investors to participate in the stock market. Third, we re-run the retail ownership analysis using 50 randomly selected placebo dates, and confirm that our event date (October 1, 2019) is a significant outlier.

7.1 The IBKR Lite Announcement

The online brokerage market is highly concentrated, with Schwab, E*Trade, and Ameritrade being the dominant players. This is why Schwab’s October 1, 2019 announcement was an important shock signaling the new importance of zero-commission trading. This is also evident from stock price and trading volume changes shown in Figure 1. However, a smaller online brokerage, Interactive

¹⁵ As shown in Section 3 (including Remark 2), the effect of increased retail ownership on passive institutional shareholders’ influence is ambiguous in general, depending on the relative alignment parameters (ρ, σ) and participation probabilities. The negative coefficient on overall ESG—driven primarily by the G pillar—is consistent with the case where retail shareholders’ preferences diverge from those of passive institutional investors (i.e., σ is low).

Brokers, had *previously* announced the rollout of its own zero-commission platform, “IBKR Lite,” on September 26, 2019 (Osipovich and Beilfuss 2019). Contemporaneous press reports stated that Interactive Brokers had “launched a no fee ‘trade war’ that promises to save investors money.”

We do not think the September 26, 2019, IBKR Lite announcement invalidates the paper’s focus on the Schwab announcement of October 1. First, Interactive Brokers is a relatively small brokerage, with assets under management about 4% of Schwab’s. More importantly, even after the Interactive Brokers announcement, it was not entirely clear that larger online brokers like Schwab, Ameritrade, or E-Trade would follow suit. Indeed, it “surprised the market when...Schwab announced that it too would eliminate trading commissions.”¹⁶ As we can see from Figure 1, stock price reactions to the IBKR Lite announcement were muted compared to those to the Schwab announcement.

However, we would still expect that the IBKR Lite announcement would have the same directional effect on stock prices of firms with higher retail involvement as the Schwab announcement, given that both involved the abolition of trading commissions and reduction of entry costs for retail investors. Moreover, we cannot rule out that the IBKR announcement at least partially presaged the Schwab announcement. Therefore, we re-run the event study analysis using September 26, 2019, the date of the IBKR Lite announcement, as the event date. Table 9 presents the results of this analysis. As in Table 3, we include industry fixed effects and cluster standard errors at the industry level. Non-institutional ownership continues to have a positive and statistically significant coefficient. In fact, the magnitude of the coefficients corresponding to retail ownership are higher than in Table 3. This could be because the IBKR announcement at least partially suggested that Schwab and the other dominant brokerages would follow suit and cut commissions. Regardless, the positive and highly significant abnormal returns of firms popular with retail investors on a second date corresponding to the abolition of trading commissions provides further support for the argument that entry costs are relevant to retail investor entry into the stock market.

7.2 Was it Fractional Trading?

Next, we address the possibility that the driving force behind the entry of retail investors was not the October 1, 2019 abolition of trading commissions, but instead was the introduction of fractional trading. Fractional trading allows investors to purchase a fraction of a company’s shares, rather than having to purchase stock in whole units. Previous research has shown that fractional trading has led to an upsurge in interest in “meme stocks” popular with retail investors as well as companies that are household names, such as Apple and Disney (Bartlett, McCrary, and O’Hara 2022). Therefore, an alternative explanation for our results is that the recent rise in retail ownership is driven by fractional trading rather than the elimination of trading commission.

We do not find this explanation very plausible, in part because of the timing of the major online brokerages introducing fractional trading. Charles Schwab became the first major online brokerage to introduce fractional trading, announcing that it would let customers purchase a fraction of shares on October 17, 2019. However, even when Schwab announced the introduction of fractional trading, media coverage linked this development to its elimination of trading commissions a few weeks earlier on October 1, 2019. An investment manager told the media that he viewed Schwab’s

¹⁶ See <https://intrinsicinvesting.com/2019/10/28/schwab-effectively-killed-the-business-that-birtherd-it-over-40-years-ago-by-announcing-commission-free-trading/>.

introduction of fractional trading as being “complementary with the commission cut as it removes any remaining friction around single stock trading” (Fitzgerald 2019). Thus, the introduction of fractional trading more likely to be seen as a continuation of the reduction in entry costs for retail investors that began on October 1 with zero-commission trading rather than an independent source of the corporate governance changes documented in this paper.

Nevertheless, we re-run the event study analysis from Table 3, using as dependent variable the Fama-French abnormal returns for the introduction of fractional trading by Schwab on October 17, 2019. Table 10 presents the results of this placebo event study, where we again regress the abnormal return against non-institutional ownership as of September 30, 2019. In neither of the specifications is the coefficient for non-institutional ownership positive and significant. The institutional background of fractional trading being an adjunct or follow-on development to the elimination of trading commissions and the null results from Table 10 indicate that our results are unlikely to be driven by investors’ newfound ability to purchase fractions of shares.

7.3 Placebo Events

Finally, we re-run Equation (1), analyzing the rise in retail ownership using abnormal returns from a large number of placebo dates from 2019, to confirm that the results obtained using the October 1, 2019 event are distinctive (Hwang and Kim 2017). We estimate abnormal returns for our sample firms for 50 placebo event dates in the year leading up to Schwab’s announcement that it was abolishing commissions. Figure 6 plots the coefficients for the interaction between *Post-Abolition* and each of the placebo abnormal returns. The red line delineates the coefficient for the interaction between *Post-Abolition* and the abnormal return on October 1, 2019. The coefficient corresponding to October 1, 2019 lies in the upper tail of the distribution (one-sided p -value of 0.039). Therefore, the difference-in-differences results using abnormal returns from October 1, 2019 seem to be an outlier compared to the distribution of coefficients on other days.

8. Conclusion

How sensitive is retail ownership to trading costs, and what is the impact of a shareholder base on corporate governance? This paper’s contribution is to first demonstrate a causal link between lowered entry costs and an increase in retail investor stock market participation and, second, to show how retail shareholder apathy affected corporate governance at firms most affected by their influx. We find that firms with high pre-existing levels of retail ownership experienced positive abnormal stock returns in response to the announcement of zero-commission trading. Firms with positive stock reactions to the announcement subsequently experienced an economically significant growth in non-institutional ownership.

In theory, if retail shareholders’ participation rate (on governance issues) is relatively low, it is unclear whether the remaining (passive) institutional owners’ influence over governance gets stronger when other (more active) institutional shareholders exit. As we noted, this is in contrast to a situation where (passive) institutional shareholders increase their ownership, in which case, their influence will increase. This paper has tried to gauge retail investor impact by looking at vote participation rates and ESG scores. We show that corporate governance at affected firms reflects the apathy of the new retail shareholders. Specifically, this paper has shown that an increase in retail investor participation in the capital markets is associated with a decrease in voting rates at affected companies. It also may have brought about less ESG-friendly corporate behavior among firms that saw an increase in retail

ownership. This decrease in ESG performance is concentrated in firms' corporate governance (or "G") scores, rather than their environmental or social ratings. This is plausibly due to retail shareholders' lack of interest and participation in the mechanics of governance.

While the research on the impact of shareholder base on corporate governance remains active, several challenges and questions remain. The first is to examine investors' behavior more directly. Like earlier research, we have relied on outcome and indirect measures, due largely to data limitations. Thus, more work regarding how different types of investors actually participate in shareholder franchise remains on the research agenda. The question invokes not only the difference between institutional and retail shareholders but also distinguishing among various institutional investors, such as passive mutual funds, active hedge funds, pension funds, and retail investors. Furthermore, due to the popularity of the zero-commission trading platform, we have seen an increase in stock market participation by retail investors, but we are yet to observe any increase in participation in shareholder franchise. Given that the seeming popularity of zero-commission trading through mobile phone apps has been relatively recent and more innovation is taking place, allowing, for instance, retail investors to access proxy materials and cast their votes on mobile phones, it may be likely that the retail investors' behavior regarding casting votes can change in the near future.

Finally, we refrain from making normative judgments about whether the shareholder apathy associated with the influx of retail investors is "bad" for corporate governance. While many corporate commentators and proxy advisors, such as ISS, equate more shareholder participation with "better" governance, the empirical evidence for such an association is mixed at best ([Yermack 2010](#)). What our empirical work does show is that entry costs significantly affect retail investors' stock market participation, and that lowering these costs causes an influx of apathetic retail holders who bring down average shareholder participation in public companies.

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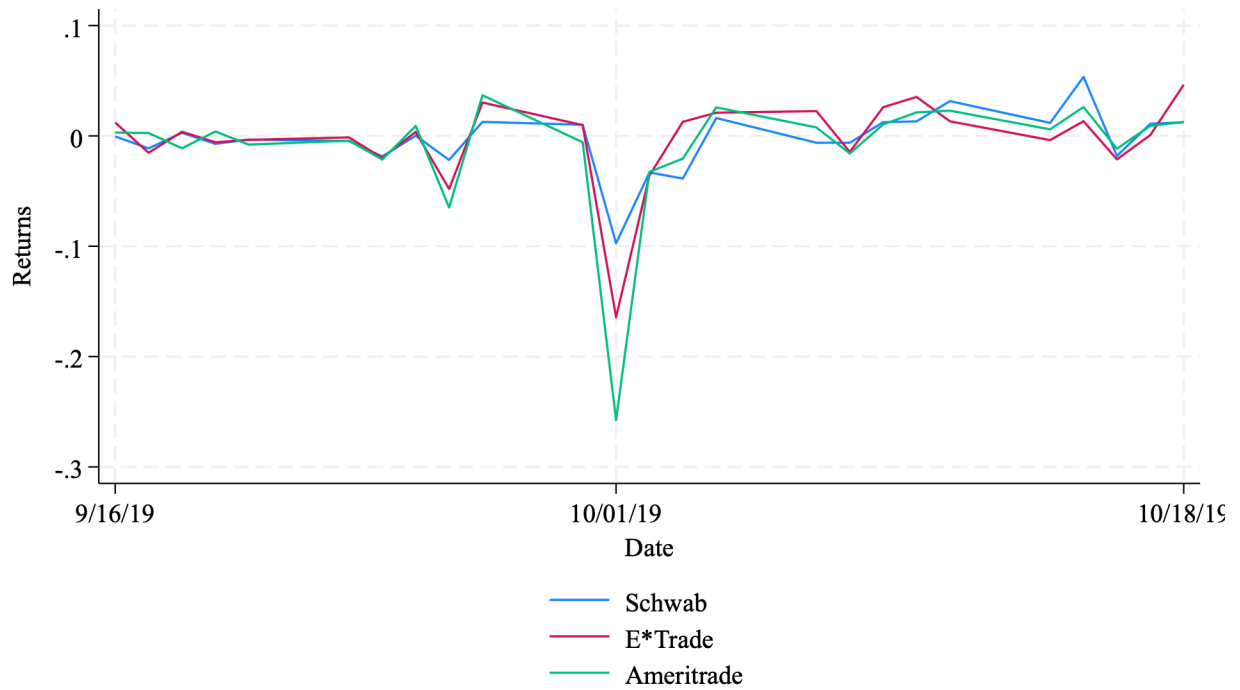
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Figure 1

Returns and Volumes for Online Brokerages around Zero-Commission Announcements

Panel A plots stock returns for three major online brokerages: Charles Schwab, E*Trade, and Ameritrade, around the announcement of zero-commission trading by Charles Schwab on October 1, 2019. Panel B plots their trading volumes around the same dates.

Panel A. Stock Returns



Panel B. Trading Volumes

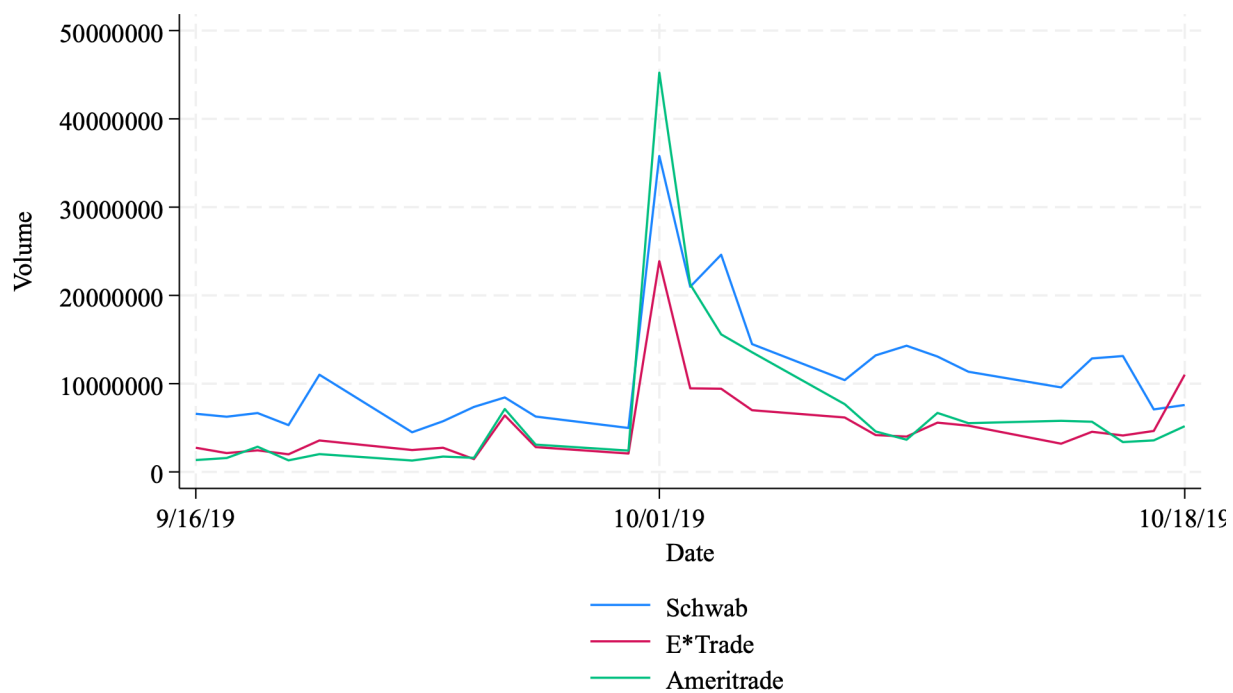


Figure 2

Testing for Pretrends: Changes in Non-Institutional Ownership

This figure shows the 95% coefficients for the interaction between year dummies and firm abnormal returns on October 1, 2019, in a fully saturated model where the dependent variable is the fraction of non-institutional ownership, as derived from 13-F filings. We use year T-1 as the baseline. Year T represents the period between October 1, 2019 and October 1, 2020.

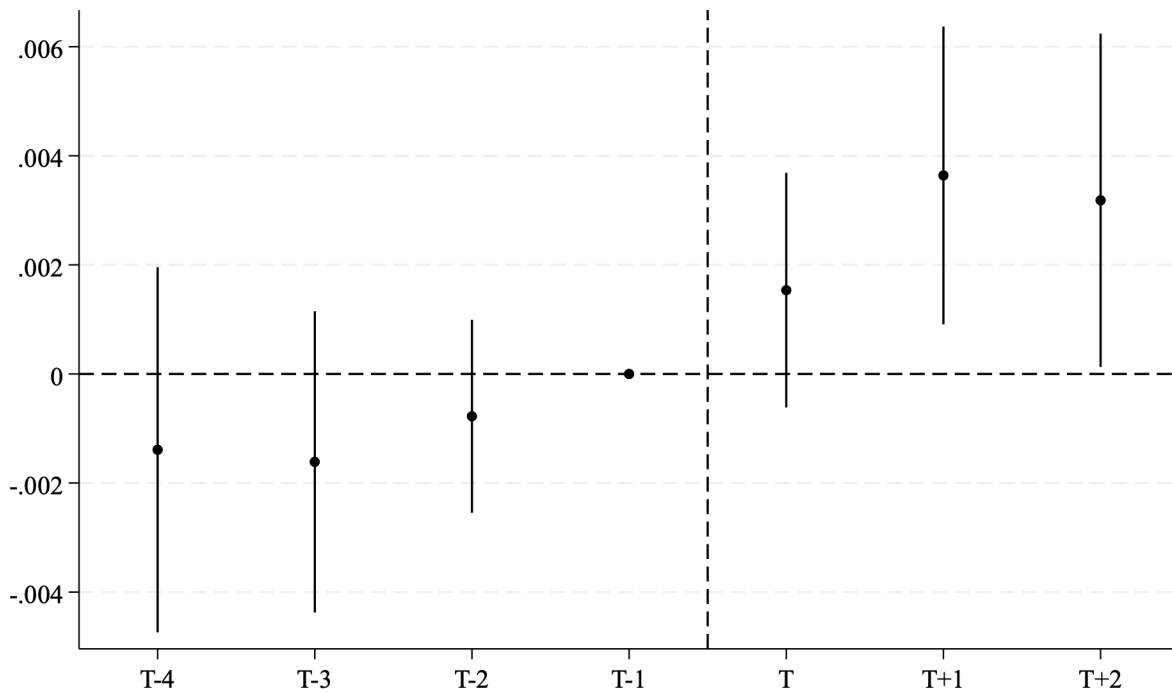
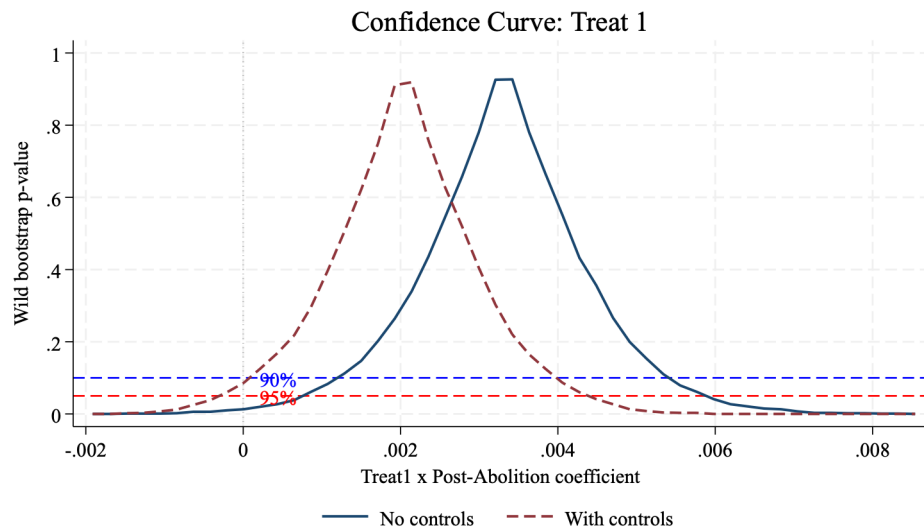


Figure 3

Changes in Non-Institutional Ownership: Wild Cluster Bootstrap Coefficients

This figure shows wild cluster bootstrap p -values for the interaction between the *Post-Abolition* dummy and the treatment proxy in the model where the dependent variable is the fraction of non-institutional ownership. Each curve traces the p -value for the null hypothesis that the interaction coefficient equals the value on the horizontal axis; the points at which a curve crosses the dashed reference lines give the bounds of the 90% and 95% confidence intervals. Solid lines report the specification without controls, and dashed lines the specification with the lagged financial controls used throughout the paper. Panel A uses *Treat1*, and Panel B uses *Treat2*. P -values are computed using the wild cluster bootstrap of [Roodman et al. \(2019\)](#) with 999 replications, clustering at the firm level.

Panel A. *Treat1*



Panel B. *Treat2*

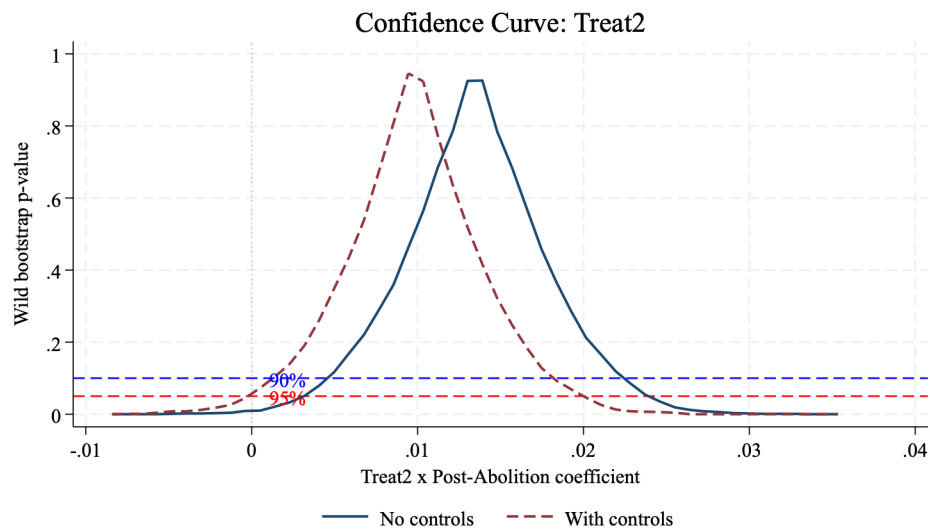


Figure 4

Testing for Pretrends: Changes in Non-Voting

This figure shows the 95% coefficients for the interaction between year dummies and firm abnormal returns on October 1, 2019, in a fully saturated model where the dependent variable is the percentage of shareholder non-votes, at the proposal level. We use year T-1 as the baseline. Year T represents the period between October 1, 2019 and October 1, 2020.

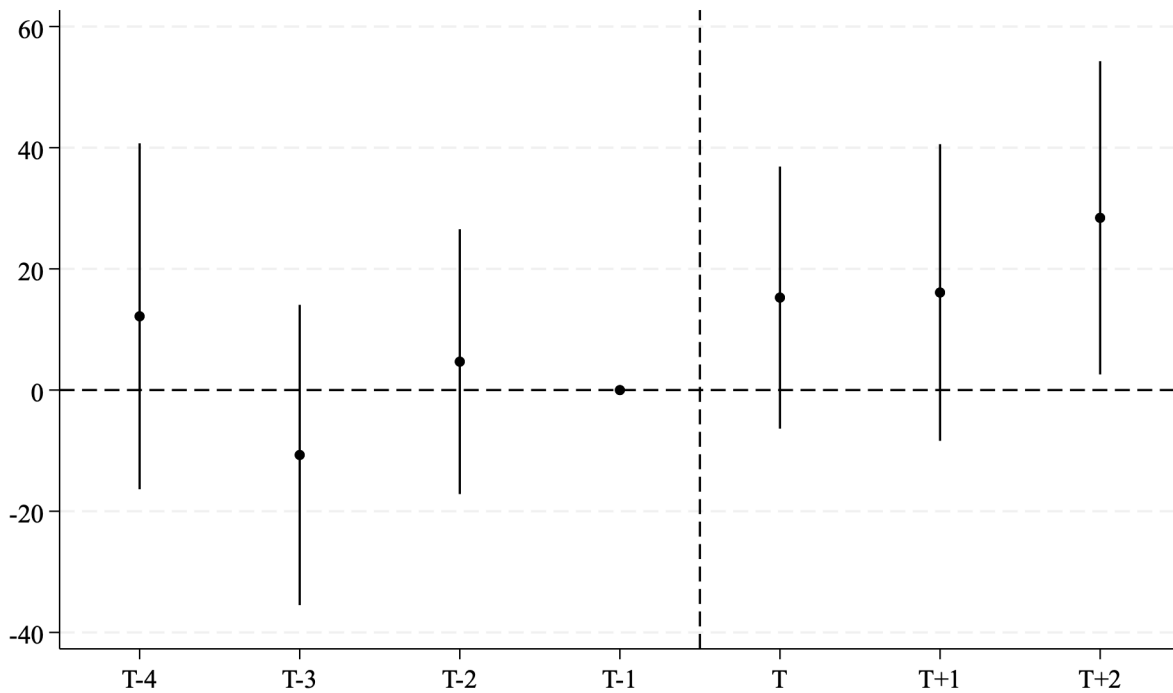


Figure 5

Testing for Pretrends: Changes in MSCI ESG Score

This figure shows the 95% coefficients for the interaction between year dummies and firm abnormal returns on October 1, 2019, in a fully saturated model where the dependent variable is the firm's ESG score, as reported by MSCI. We use year T-1 as the baseline. Year T represents the period between October 1, 2019 and October 1, 2020.

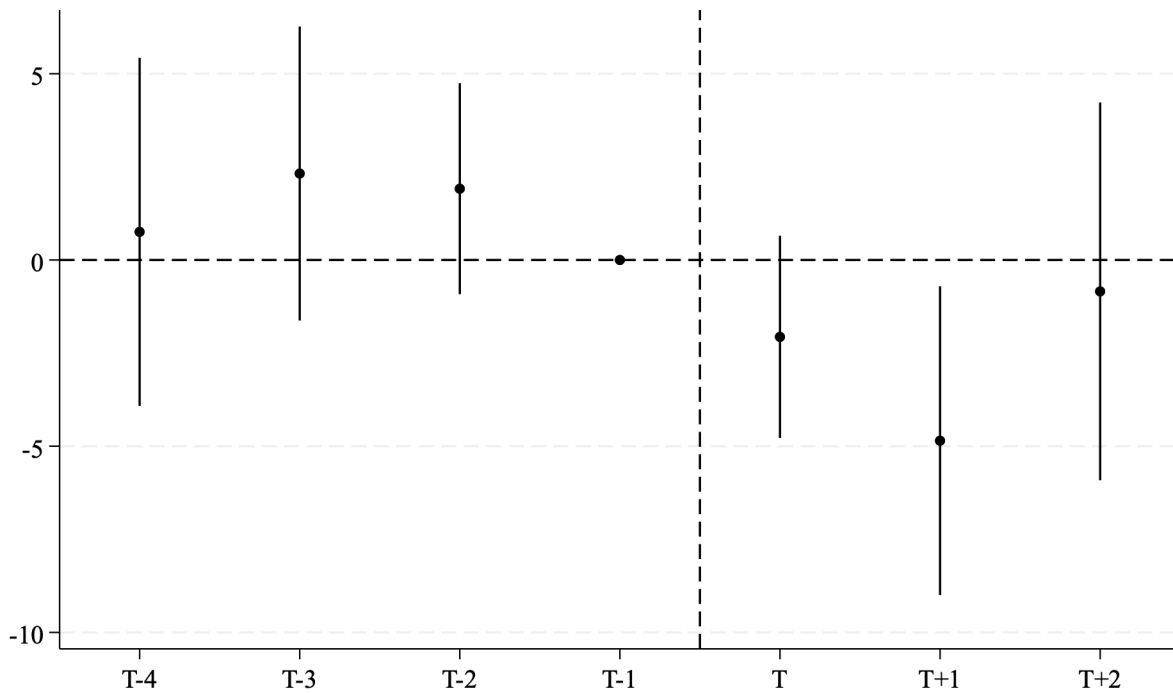


Figure 6

Placebo Test for Rise in Retail Ownership

This figure plots the distribution of placebo difference-in-differences coefficients on the interaction between abnormal returns and the *Post-Abolition* dummy, where the dependent variable is the fraction of non-institutional ownership, as derived from 13-F filings. For fifty placebo trading dates leading up to October 1, 2019, we treat that day's Fama-French abnormal return as the firm-level treatment intensity and re-estimate the specification in column (1) of Table 4, and record the coefficient on the interaction term. Each observation in the histogram corresponds to one such placebo date. All specifications include firm and quarter fixed effects, with standard errors clustered at the firm level. The red dashed line marks the coefficient obtained using the actual October 1, 2019 abnormal returns. The one-sided empirical p -value is 0.039.

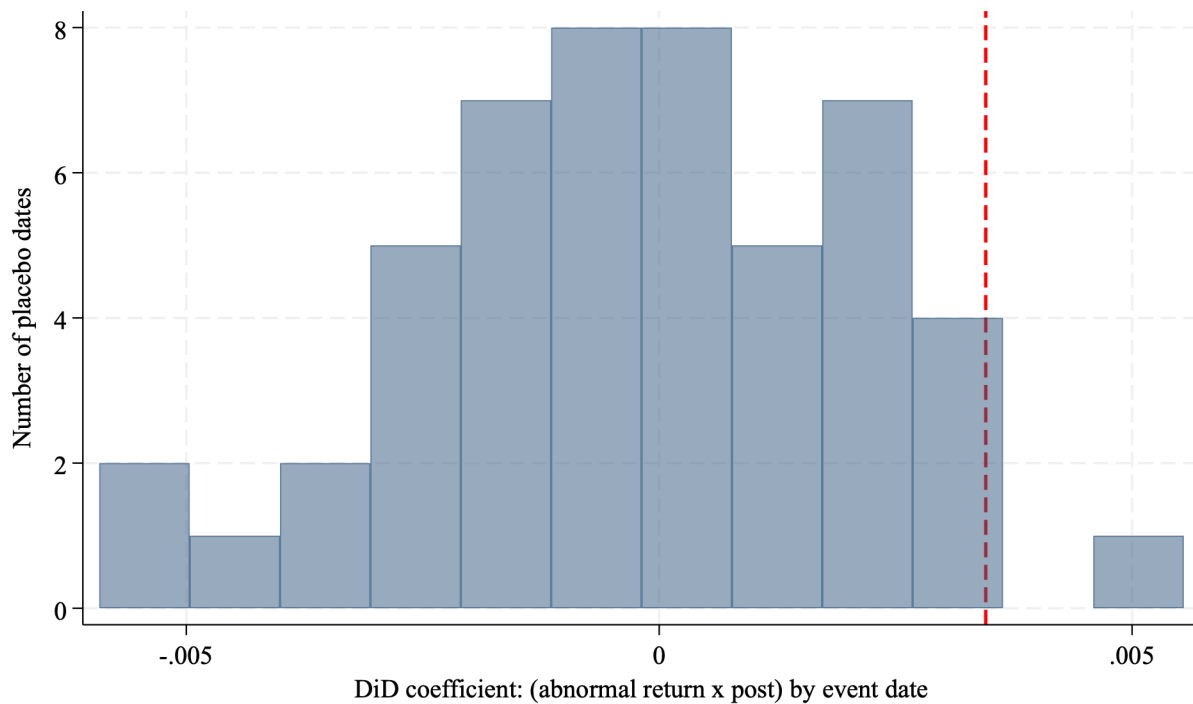


Table 1

Variable Definitions

Variable	Definition	Source
Cash Ratio	The ratio of the firm's cash holdings to its total assets, winsorized at the 2.5% level.	Compustat
Debt Ratio	The ratio of the firm's total debt (both current and long-term) to its total assets, winsorized at the 2.5% level.	Compustat
ESG Score	A firm's industry-adjusted composite ESG score.	MSCI
Ln(Assets)	The natural logarithm of the firm's total assets, winsorized at the 2.5% level.	Compustat
Ln(Volatility)	The natural logarithm of the standard deviation of the firm's daily stock return over the year, winsorized at the 2.5% level.	Compustat
Non-Institutional Ownership	One minus the fraction of total institutional ownership reported in 13-F filings, winsorized at the 2.5% level.	Thomson Reuters
Post-Abolition	An indicator equal to 1 after the October 1, 2019 abolition of trading commissions by major online brokerages.	Manual checks
Retail Growth Rate	The ratio of the retail ownership fraction on December 31, 2019 to that on September 30, 2019, winsorized at the 2.5 level.	ISS
Return on Assets	A firm's net income divided by total assets, winsorized at the 2.5% level.	Compustat
Shareholder Non-Voting	The percentage of outstanding shares, at the proposal level, that were not cast for or against a proposal, or abstained, winsorized at the 2.5% level.	ISS
Top Quartile Non-Institutional Ownership	An indicator equal to 1 if the firm's non-institutional ownership was in the top quartile as of September 30, 2019.	Thomson Reuters
Treat₁	The abnormal return experienced by the firm on October 1, 2019, using the Fama-French three factor model, winsorized at the 2.5% level.	WRDS; Manual checks
Treat₂	An indicator equaling 1 if the firm's abnormal return (computed using the Fama-French three-factor model) on October 1, 2019 was in the top quartile of the given sample.	WRDS

Table 2**Summary Statistics**

Panel A presents summary statistics for different samples used in the empirical analysis. Panel B compares the means for non-institutional ownership and financial variables for firms with above- and below-median abnormal returns on October 1, 2019 in the event study sample. The ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Panel A. Overall Summary Statistics*Event Study Sample*

	N	Mean	SD
Non-Institutional Ownership	2765	.347	.318
Abnormal Return	2765	-.084	2.334
Ln(Assets)	2760	7.059	2.045
Return on Assets	2750	-.026	.193
Cash Ratio	2723	.157	.191
Debt Ratio	2645	.243	.218
Ln(Volatility)	2762	-3.65	.426

13-F Sample

	N	Mean	SD
Non-Institutional Ownership	67213	.355	.312
Treat ₁	67220	-.077	2.227
Treat ₂	67220	.25	.433
Ln(Assets)	67071	7.176	2.046
Return on Assets	66790	-.021	.177
Cash Ratio	66097	.159	.18
Debt Ratio	64333	.258	.217
Ln(Volatility)	67129	-3.614	.49

Proposal-Level Sample

	N	Mean	SD
Percentage Non-Votes	107668	23.18	14.342
Treat ₁	107668	-.002	.02
Treat ₂	107668	.25	.433
Retail Growth Ratio	107668	1.042	.326
Ln(Assets)	107308	7.617	2.207
Return on Assets	106733	-.003	.161
Cash Ratio	105470	.144	.165
Debt Ratio	101288	.268	.206
Ln(Volatility)	107575	-3.714	.504

Panel B. Differences between Firms based on Abnormal Return on October 1, 2019

	Above-Median Abnormal Return (1)	Below-Median Abnormal Return (2)	Difference (1)–(2)	N
Non-Institutional Ownership	.363	.332	.032***	2765
Ln(Assets)	7.133	6.984	.149*	2760
Return on Assets	-.013	-.038	.026***	2750
Cash Ratio	.152	.163	-.012	2723
Debt Ratio	.246	.239	.007	2645
Ln(Volatility)	-3.637	-3.663	.027*	2762

Table 3**Event Study Analysis**

This table presents the results of a linear regression model where the dependent variable is the abnormal return for each firm on October 1, 2019, when major brokerages announced they would abolish trading commissions. We use the Fama-French three-factor asset pricing model to compute abnormal returns. Non-Institutional ownership is measured as 1 minus the fraction of institutional ownership as reported in 13-F data as of September 30, 2019. All models include industry fixed effects. All standard errors are robust and clustered at the industry level, and t-statistics are presented in parentheses. All variables are described in Table 1. The ^{***}, ^{**}, and ^{*} denote significance at the 1%, 5%, and 10% levels.

	(1)	(2)
Non-Institutional Ownership	0.415** (2.777)	
Top Quartile Non- Institutional Ownership		0.295** (2.847)
Ln(Assets)	0.0880*** (3.324)	0.0885*** (3.487)
Return on Assets	1.639*** (3.490)	1.629*** (3.445)
Cash Ratio	-0.256 (-1.424)	-0.260 (-1.453)
Debt Ratio	0.0752 (0.475)	0.0488 (0.305)
Ln(Volatility)	0.630*** (4.390)	0.642*** (4.456)
Observations	2,765	2,608
R-squared	0.032	0.048
Industry Fixed Effects	Yes	Yes

Table 4

Changes in Non-Institutional Ownership

This table presents the results of a linear regression model where the dependent variable is the share of non-institutional ownership, based on 13-F data. All models use data from fiscal years 2016 to 2022, and *Post-Abolition* equals 1 for reporting dates after October 1, 2019. All models include firm and quarter fixed effects. All standard errors are robust and clustered at the firm level, and t-statistics are presented in parentheses. All variables are described in Table 1. The **, *, and * denote significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)
Treat ₁ × Post-Abolition	0.00331** (2.536)		0.00204* (1.738)	
Treat ₂ × Post-Abolition		0.0135** (2.461)		0.00980* (1.882)
Ln(Assets)			-0.0688*** (-14.32)	-0.0688*** (-14.31)
Return on Assets			-0.0524*** (-4.145)	-0.0529*** (-4.176)
Cash Ratio			-0.0249** (-1.971)	-0.0251** (-1.981)
Debt Ratio			0.0541*** (4.032)	0.0538*** (4.019)
Ln(Volatility)			0.0419*** (9.368)	0.0421*** (9.457)
Observations	58,469	58,469	55,095	55,095
R-squared	0.915	0.915	0.920	0.920
Firm Fixed Effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes

Table 5

Pretrend Test: Changes in Non-Institutional Ownership

This table presents the results of a linear regression model where the dependent variable is the share of non-institutional ownership, based on 13-F data. All models use data from 2016–19, where *Post-Abolition* equals 0. All models include firm and quarter fixed effects. All standard errors are robust and clustered at the firm level, and t-statistics are presented in parentheses. All variables are described in Table 1. The **, *, and * denote significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)
Treat ₁ × Year	0.000803 (1.474)		0.000699 (1.312)	
Treat ₂ × Year		0.00372 (1.623)		0.00349 (1.528)
Ln(Assets)			-0.0521*** (-7.932)	-0.0522*** (-7.962)
Return on Assets			-0.0319** (-2.322)	-0.0314** (-2.286)
Cash Ratio			0.0113 (0.723)	0.0112 (0.714)
Debt Ratio			0.0227 (1.331)	0.0222 (1.301)
Ln(Volatility)			0.0126** (2.265)	0.0127** (2.287)
Observations	36,360	36,360	34,243	34,243
R-squared	0.956	0.956	0.958	0.958
Firm Fixed Effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes

Table 6

Change in Non-Institutional Ownership: Reweighted Sample Analysis

This table presents the results of a linear regression model where the dependent variable is the share of non-institutional ownership, based on 13-F data. For Panel A, we run these equations on an entropy-balanced sample, where we balance the first moment of pre-treatment means of covariates for firms in the top quartile of abnormal returns on October 1, 2019 and other companies. For Panel B, we run the equations on an inverse probability weighted sample. All models use data from fiscal years 2016 to 2022, and *Post-Abolition* equals 1 for reporting dates after October 1, 2019. All models include firm and year fixed effects. All standard errors are robust and clustered at the firm level, and t-statistics are presented in parentheses. All variables are described in Table 1. The ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Panel A. Entropy Balanced Sample

	(1)	(2)	(3)	(4)
Post-Abolition	0.000 (0.002)	-0.006 (-1.572)	0.001 (0.420)	-0.002 (-0.752)
Treat ₁ × Post-Abolition	0.004** (2.495)		0.002* (1.668)	
Treat ₂ × Post-Abolition		0.017*** (2.641)		0.010* (1.762)
Ln(Assets)			-0.070*** (-12.317)	-0.070*** (-12.312)
Return on Assets			-0.052*** (-3.384)	-0.052*** (-3.397)
Cash Ratio			-0.030* (-1.824)	-0.030* (-1.836)
Debt Ratio			0.058*** (3.332)	0.058*** (3.320)
Ln(Volatility)			0.053*** (9.196)	0.053*** (9.219)
Observations	49,081	49,081	48,071	48,071
R-squared	0.905	0.905	0.913	0.913
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes

Panel B. Inverse Probability Weighted Sample

	(1)	(2)	(3)	(4)
Post-Abolition	-0.000402 (-0.178)	-0.00617* (-1.738)	0.000867 (0.385)	-0.00266 (-0.794)
Treat ₁ × Post-Abolition	0.00377** (2.547)		0.00224* (1.679)	
Treat ₂ × Post-Abolition		0.0170*** (2.702)		0.0103* (1.781)
Ln(Assets)			-0.0707*** (-12.42)	-0.0707*** (-12.42)
Return on Assets			-0.0484*** (-3.195)	-0.0486*** (-3.208)
Cash Ratio			-0.0304* (-1.885)	-0.0306* (-1.895)
Debt Ratio			0.0597*** (3.422)	0.0594*** (3.410)
Ln(Volatility)			0.0545*** (9.400)	0.0545*** (9.423)
Observations	49,633	49,633	48,335	48,335
R-squared	0.905	0.905	0.912	0.912
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes

Table 7

Changes in Shareholder Non-Voting

This table presents the results of a linear regression model where the dependent variable is the percentage of shareholder votes not cast for or against a proposal or marked as abstentions. All models use proposal-level data from fiscal years 2016 to 2022, and *Post-Abolition* equals 1 for reporting dates after October 1, 2019. All models include firm and year fixed effects. All standard errors are robust and clustered at the firm level, and t-statistics are presented in parentheses. All variables are described in Table 1. The ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)	(6)
Post-Abolition	0.895 (1.313)	0.494 (0.729)	-0.612 (-0.731)	0.945 (1.323)	0.612 (0.861)	-0.346 (-0.405)
Treat ₁ × Post-Abolition	19.97* (1.883)			11.49 (1.100)		
Treat ₂ × Post-Abolition		1.598*** (3.796)			1.322*** (3.210)	
Retail Growth Rate × Post-Abolition			1.402*** (2.883)			1.208** (2.526)
Ln(Assets)			-2.189*** (-5.047)	-2.175*** (-5.019)	-2.186*** (-5.055)	-2.189*** (-5.047)
Return on Assets			-4.183*** (-3.513)	-4.264*** (-3.587)	-4.223*** (-3.574)	-4.183*** (-3.513)
Cash Ratio			2.885** (2.162)	2.819** (2.118)	2.900** (2.179)	2.885** (2.162)
Debt Ratio			5.485*** (4.790)	5.303*** (4.644)	5.496*** (4.790)	5.485*** (4.790)
Ln(Volatility)			2.614*** (6.100)	2.628*** (6.125)	2.622*** (6.099)	2.614*** (6.100)
Observations	94,389	94,389	94,389	87,320	87,320	87,320
R-squared	0.697	0.698	0.697	0.706	0.706	0.706
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Table 8

Changes in ESG Scores

This table presents the results of a linear regression model where the dependent variable is the firm's industry-adjusted ESG score or ESG pillar scores (depending on the panel), as reported by MSCI. All models use data between October 1st, 2016 and October 1st, 2022, and *Post-Abolition* equals 1 for the period after October 1st, 2019. All models include firm and year fixed effects. All standard errors are robust and clustered at the firm level, and t-statistics are presented in parentheses. All variables are described in Table 1. The ^{***}, ^{**}, and ^{*} denote significance at the 1%, 5%, and 10% levels.

Panel A. Overall ESG Scores

	(1)	(2)	(3)	(4)
Post-Abolition	-0.0324 (-0.752)	0.0224 (0.486)	-0.0460 (-1.012)	0.00636 (0.131)
Treat ₁ × Post-Abolition	-4.009** (-2.419)		-3.703** (-2.164)	
Treat ₂ × Post-Abolition		-0.198*** (-2.855)		-0.187*** (-2.623)
Ln(Assets)			0.0975 (1.456)	0.102 (1.531)
Return on Assets			-0.0447 (-0.217)	-0.0398 (-0.195)
Cash Ratio			0.158 (0.792)	0.155 (0.774)
Debt Ratio			-0.478*** (-2.711)	-0.464*** (-2.625)
Ln(Volatility)			-0.208*** (-2.912)	-0.213*** (-2.970)
Observations	8,503	8,503	7,989	7,989
R-squared	0.840	0.840	0.839	0.840
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes

Panel B. Environmental Pillar Score

	(1)	(2)	(3)	(4)
Post-Abolition	-0.0218 (-0.592)	-0.00328 (-0.0875)	-0.0147 (-0.381)	0.00230 (0.0586)
Treat ₁ × Post-Abolition	-0.803 (-0.580)		-0.983 (-0.691)	
Treat ₂ × Post-Abolition		-0.0692 (-1.149)		-0.0612 (-0.976)
Ln(Assets)			0.130** (2.343)	0.132** (2.370)
Return on Assets			-0.364* (-1.924)	-0.363* (-1.923)
Cash Ratio			0.0863 (0.435)	0.0851 (0.428)
Debt Ratio			-0.109 (-0.635)	-0.103 (-0.597)
Ln(Volatility)			-0.0120 (-0.195)	-0.0129 (-0.209)
Observations	8,503	8,503	7,989	7,989
R-squared	0.899	0.899	0.900	0.900
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes

Panel C. Social Pillar Score

	(1)	(2)	(3)	(4)
Post-Abolition	0.0184 (0.552)	0.0276 (0.761)	0.0361 (1.058)	0.0423 (1.136)
Treat ₁ × Post-Abolition	0.00263 (0.00207)		0.151 (0.115)	
Treat ₂ × Post-Abolition		-0.0358 (-0.688)		-0.0243 (-0.453)
Ln(Assets)			0.0808* (1.664)	0.0809* (1.666)
Return on Assets			-0.214 (-1.532)	-0.215 (-1.539)
Cash Ratio			-0.108 (-0.696)	-0.109 (-0.700)
Debt Ratio			-0.127 (-1.006)	-0.122 (-0.966)
Ln(Volatility)			-0.0633 (-1.220)	-0.0624 (-1.205)
Observations	8,502	8,502	7,988	7,988
R-squared	0.804	0.804	0.810	0.810
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes

Panel D. Corporate Governance Pillar Score

	(1)	(2)	(3)	(4)
Post-Abolition	0.0199 (0.502)	0.0614 (1.482)	3.18e-06 (7.68e-05)	0.0356 (0.820)
Treat ₁ × Post-Abolition	-5.342*** (-4.066)		-4.635*** (-3.450)	
Treat ₂ × Post-Abolition		-0.141** (-2.561)		-0.119** (-2.120)
Ln(Assets)			-0.112** (-2.403)	-0.107** (-2.287)
Return on Assets			0.703*** (4.299)	0.711*** (4.339)
Cash Ratio			0.231 (1.386)	0.229 (1.367)
Debt Ratio			-0.147 (-1.120)	-0.149 (-1.126)
Ln(Volatility)			-0.189*** (-3.412)	-0.197*** (-3.555)
Observations	8,503	8,503	7,989	7,989
R-squared	0.717	0.716	0.722	0.721
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes

Table 9**Validation from IBKR Lite Announcement**

This table presents the results of a linear regression model where the dependent variable is the abnormal return for each firm on September 26, 2019, when IBKR announced it would abolish trading commissions. We use the Fama-French three-factor asset pricing model to compute abnormal returns. Non-Institutional ownership is measured as 1 minus the fraction of institutional ownership as reported in 13-F data as of September 30, 2019. All models include industry fixed effects. All standard errors are robust and clustered at the industry level, and t-statistics are presented in parentheses. All variables are described in Table 1. The ^{***}, ^{**}, and ^{*} denote significance at the 1%, 5%, and 10% levels.

	(1)	(2)
Non-Institutional Ownership	1.220*** (9.911)	
Top Quartile Non-Institutional Ownership		0.864*** (6.658)
Ln(Assets)	-0.0912*** (-2.987)	-0.0898*** (-2.981)
Return on Assets	1.768*** (5.195)	1.740*** (5.303)
Cash Ratio	-0.599** (-2.392)	-0.609** (-2.381)
Debt Ratio	0.238* (1.873)	0.158 (1.292)
Ln(Volatility)	-1.075*** (-5.179)	-1.038*** (-5.213)
Observations	2,561	2,561
R-squared	0.127	0.126
Industry Fixed Effects	Yes	Yes

Table 10**Was it Fractional Trading?**

This table presents the results of a linear regression model where the dependent variable is the abnormal return for each firm on October 17, 2019, when Charles Schwab announced it was introducing fractional trading (“FT”). We use the Fama-French three-factor asset pricing model to compute abnormal returns. Non-Institutional ownership is measured as 1 minus the fraction of institutional ownership as reported in 13-F data as of September 30, 2019. All models include industry fixed effects. All standard errors are robust and clustered at the industry level, and t-statistics are presented in parentheses. All variables are described in Table 1. The ^{***}, ^{**}, and ^{*} denote significance at the 1%, 5%, and 10% levels.

	(1)	(2)
Non-Institutional Ownership	-0.0595 (-0.316)	
Top Quartile Non- Institutional Ownership		-0.0238 (-0.200)
Ln(Assets)	0.00587 (0.226)	0.00707 (0.292)
Return on Assets	0.902** (2.945)	0.905** (2.922)
Cash Ratio	0.374 (1.099)	0.374 (1.097)
Debt Ratio	0.152 (0.767)	0.156 (0.799)
Ln(Volatility)	-0.403* (-2.046)	-0.406* (-2.047)
Observations	2,554	2,554
R-squared	0.042	0.042
Industry Fixed Effects	Yes	Yes

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