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Three Essays on Financial Economics

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An abstract of
A dissertation submitted to the Faculty of the
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Abstract

Three Essays on Financial Economics

By Jaemin Lee

This dissertation contains three essays on financial economics. The first essay studies the contagion of career concern and its implications for the mutual fund industry. Fund managers reduce their risk-taking activity when their workplace peers are dismissed due to underperformance. Consistent with peer dismissal increasing the salience of the employment risk, the effect is stronger for managers (i) with higher underlying employment risk, and (ii) with stronger social ties to the dismissed manager. The dismissal also affects managers in a different firm in the same building, suggesting that the result is unlikely to be driven by firm-level shock. The peer dismissal effect is particularly more pronounced for managers who face high incentives to take excessive risks, and their funds also experience 40 to 60 basis-point improvement in risk-adjusted performance during the year following peer dismissal. These results suggest that salient employment risk can alleviate the incentive misalignment between fund managers and fund investors. The second essay studies whether and how investors' social preference for Confederate Memorials affects their housing choice and its impact on the housing market. We find that Black, Democrat, and college-educated homeowners are less likely to live on Confederate memorial streets. Moreover, houses on Confederate streets sell for 3\% less. The Confederate effect does not spillover to adjacent houses, consistent with direct name rather than neighborhood effects. The price effect increases following attention-grabbing events that highlight racial underpinnings of Confederate symbols. Aversion to houses on Confederate streets also holds in experimental settings where house attributes are otherwise identical. The findings suggest that social norms can have important consequences for real estate markets. The third essay empirically analyzes the risk and return characteristics across firms sorted by their environmental and social (ES) ratings. We document that ES ratings have no significant relationship with average stock returns or unconditional market risk. Stocks of firms with higher ES ratings do have significantly lower systematic downside risk. However, the economic magnitude of such reduction in downside risk is small. Our results suggest that investors who derive non-pecuniary benefits from ES investing need not sacrifice financial performance.

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Peer Dismissal and Contagious Career Concerns

Jaemin Lee*

Abstract

I find that mutual fund managers reduce their risk-taking activity when their workplace peers are dismissed due to underperformance. Consistent with peer dismissal increasing the salience of the employment risk through social interaction, the effect is stronger for managers (i) with higher underlying employment risk, and (ii) with stronger social ties to the dismissed manager. The dismissal also affects managers in a *different* firm in the same building, suggesting that the result is unlikely to be driven by firm-level shock. I also find a stronger effect following a plausibly exogenous changes in non-compete laws that make the potential dismissal more costly. The peer dismissal effect is particularly more pronounced for managers who face high incentives to take excessive risks, and their funds also experience 40 to 60 basis-point improvement in risk-adjusted performance during the year following peer dismissal. These results suggest that salient employment risk, induced by peer dismissal, can alleviate the incentive misalignment between fund managers and mutual fund investors.

Keywords: Social interaction, career concern, risk-taking behavior, mutual funds.
JEL classification: D71, G11, G23, G41, J44, J64.

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1 Introduction

Getting fired from a job is a devastating life event. The afflicted experiences significant adverse financial effects owing to income loss, long-lasting career setbacks, and disruption of retirement plans (Couch and Placzek, 2010). They may also experience emotional turmoil stemming from isolation and lower self-esteem (Winkelmann and Winkelmann, 1998). The painful nature of potential dismissal evokes career concerns for workers and makes them exhibit risk-averse behaviors in response (e.g., Chevalier and Ellison, 1999). Importantly, prior studies suggest that the risk-averse behavior of workers in the financial industry, such as analysts and fund managers, can affect the informational environments and asset prices in the market.[1] Therefore, understanding how career concern occurs and spreads has an important implication not only for workers' behaviors, but also for the financial market. In this paper, I draw on the social finance literature to investigate whether career concerns are contagious among coworkers; that is, whether career concerns are influenced by "peer dismissal"—the dismissal of coworkers due to underperformance.

An extensive literature documents that social interaction shapes how economic agents make financial decisions and influences their underlying attitude (e.g., Ahern, Duchin, and Shumway, 2014). In this respect, peer dismissal can affect a worker's career concerns in several ways. First, the peer effect can arise through emotional contagion, which is the unconscious and empathic process of feelings spreading within a group (e.g., Barsade, 2002).[2] Because emotional contagion is especially strong for negative emotions, the distress of a dismissed peer can evoke fear of experiencing a similar outcome among other workers and further increase their perceived career concerns.[3] Second, the peer effect may occur through social learning (e.g., Ellison and Fudenberg, 1995). The dismissed

[1]For example, prior studies find that analysts under career concern issue more conservative forecast (Hong, Kubik, and Solomon, 2000; Clement and Tse, 2005), and Harford et al. (2019) find that the career concern also induces analysts to allocate less effort to firms that are less important to their careers. In addition, the mutual fund literature finds that fund managers reduce their portfolio risk when they are under career concern (e.g., Chevalier and Ellison, 1999; Kempf, Ruenzi, and Thiele, 2009; Pool et al., 2019), and a recent study by Han, Roussanov, and Ruan (2022b) finds that beta anomaly and IVOL puzzle are explained by mutual fund risk-taking.

[2]In the psychology literature, the notion of "emotional contagion" is well established in field studies in which various groups are shown to share similar mood and emotions (Totterdell, Kellett, Teuchmann, and Briner, 1998; Totterdell, 2000; Bartel and Saavedra, 2000), experimental settings (Barsade, 2002), and archival analysis based on Facebook friends (Kramer, 2012).

[3]Starting from Loewenstein (2000), both experimental (Kuhnen and Knutson, 2011; Guiso, Sapienza, and Zingales, 2018) and empirical evidence (Wang and Young, 2020; Liu, Sulaeman, Shu, and Yeung, 2023) show how emotions can affect economic behavior, particularly with regard to the risk aversion of investors.

peers can share their struggles, providing vivid insight on their experiences. Even without direct communication, workers who speculate about prospective job outcomes based on their peers' outcomes (Tan and Rider, 2017) may deduce the painful ramifications of being fired. Both of these channels can make underlying employment more salient for the remaining workers and increase their career concern, even without material changes in their employment risk.

I estimate the changes in career concerns evoked by peer dismissal by examining changes in the portfolio risk of mutual funds. Previous studies document that career-concerned managers take less risk on average because it reduces the likelihood of poor performance that can eventually lead to dismissal (Chevalier and Ellison, 1999; Qiu, 2003). Similarly, fund managers prompted by salient employment risk may decrease their portfolio risk following peer dismissal. Furthermore, under the salient employment risk channel, managers will respond more strongly to peer dismissal when facing high employment risk, for example, if they are performing poorly, are less experienced, and following bear markets when dismissal rates are high (Kempf, Ruenzi, and Thiele, 2009). On the other hand, peer dismissal can create opportunities for career advancement (Rosenbaum, 1979), which may induce managers to take more risks. In particular, peer dismissal opens opportunities for job positions or fund assets left behind by the dismissed peer, which can increase within-firm competition and create tournament-like incentives (e.g., Kempf and Ruenzi, 2008). Consequently, managers may be inclined to compete for higher compensation by taking more risks.

To cleanly identify fund managers' peer group, I construct a unique dataset of each fund manager's employment record by matching investment advisory firm information from N-SAR filings (EDGAR) to the CRSP and Morningstar mutual fund databases. N-SAR reports a list of advisory firms in charge of each fund's management, while the mutual fund dataset provides a list of fund managers of each fund. By matching the two datasets at the fund level, I am able to map each fund manager into a distinct advisory firm, which forms a relatively small group of work peers.[4] My dataset features over 18,000 U.S.-domiciled fund managers between 1994 and 2019. Approximately 9% of fund managers depart from their firm each year, on average.

Because firm departures can occur voluntarily, I condition on past performance to identify dis-

[4]Note that an advisory firm is a smaller entity than a management company ("fund family"), which typically hires advisory firm(s) to manage a subset of their funds. The advisory firm can also be a standalone entity, which serves as a sub-advisor for fund families.

missals. Prior studies find that *fund*-level turnover is negatively associated with prior performance (Khorana, 1996; Kostovetsky and Warner, 2015). Using the manager employment dataset, I similarly find that past one-year within-style fund returns and flows, as well as Morningstar Ratings (MS Ratings), significantly predict future *firm* departure. Based on this result, I define *Dismissal* as the situation in which a manager departs from their advisory firm while being in the bottom terciles of one of the three performance measures. As shown in Figure 1, approximately 75% of departing managers are classified as being dismissed, consistent with the majority of managers leaving due to underperformance.

In the first part of my analysis, I examine how fund managers change their fund risk in response to the dismissal of peers in the same investment advisory firm (*Peer dismissal*). I use the holding-based risk-taking measure of Kempf, Ruenzi, and Thiele (2009), which captures the fund manager's intended risk change as reflected in their mutual fund holdings (also see Huang, Sialm, and Zhang, 2011; Ma and Tang, 2019). The measure is calculated as the difference between the intended volatility of the most recent fund holdings and the realized volatility in the previous quarter, and it is orthogonal to unexpected changes in risk.[5]

I find that peer dismissal is associated with lower risk-taking behavior. That is, fund managers whose peers are fired for underperformance, ceteris paribus, reduce risk-taking behavior during the following quarter (*Peer dismissal effect*). I find similar results based on fund holdings: managers with recently dismissed peers decrease their high-risk stock holdings and increase their low-risk stock holdings to a greater extent than other managers.

Furthermore, consistent with salient career concerns being the main driver, the peer dismissal effect on risk-taking behavior is more pronounced among managers who are already facing high employment risk, such as those with poor performance (Kostovetsky and Warner, 2015) and less experience (Chevalier and Ellison, 1999), and when dismissals are more likely following bear markets (Kempf, Ruenzi, and Thiele, 2009). Peer-dismissed managers reduce their risk by 0.15% on average, which corresponds to a roughly 0.1 standard deviation decrease in risk-taking activity. Among poor-performing managers, the effect is amplified, with risk-taking activity decreasing up to 0.32 standard deviations. Given that a dismissed manager simultaneously affects 5.8 managers on average, the

[5]The primary benefit of this measure is that the change in volatility is only driven by changes in holdings: the fund and stock returns are based on the same period, that is, the previous quarter.

peer dismissal effect is economically significant.

Prior studies indicate that social connection strengthens both the peer effect (Huang, Hwang, and Lou, 2021) and emotional contagion (Lin and Utz, 2015). Consistent with these studies, I find that the peer dismissal effect is amplified when managers have strong social ties to the dismissed peer. Specifically, conditioning on peer dismissal, managers who are alumni of the same college and spend more than half of their working experience in the same firm with the dismissed peer exhibit up to four times greater reduction in risk relative to those without social ties.

I also run placebo tests to examine whether my results reflect any mechanical relationships. One concern is that the results may capture market-wide changes in risk. For example, it is possible that a post-recession drop in market volatility (Schwert, 1989) coincides with more frequent dismissals, which might not be fully absorbed by style-by-quarter fixed effects. Using index funds, for which managers have limited discretion and incentive to actively shift their risks, I do not find evidence of the peer dismissal effect. Additional tests using (i) departure of *outperforming* peers, and (ii) *non-departure* of underperforming peers do not exhibit peer dismissal effect, suggesting that contagion of career concern is the key driver of my results.

I perform a host of additional tests to confirm whether my results are robust and to rule out alternative hypotheses. First, I find similar peer dismissal effects using alternative risk-taking measures, such as changes in realized volatility or volatility ratios that account for risk levels (Huang, Sialm, and Zhang, 2011). The peer effect also strengthens as the number of dismissed peers increases, consistent with multiple dismissals increasing employment risk salience. Second, my results are not driven by the flow-driven trades of mutual funds (Coval and Stafford, 2007; Lou, 2012). Third, the peer dismissal effect remains similar when estimated within a group of firms located in the same city or those of similar sizes, which suggests that my results are not due to regional economic shocks or firm size effects. Lastly, including manager and fund fixed effects in lieu of firm fixed effect yields similar results, suggesting that the effects are not driven by unobserved manager and fund heterogeneity.

One important concern is that the negative relationship between peer dismissal and risk reduction is capturing correlated group effects (Manski, 1993).[6] For example, a firm hit by a negative shock may

[6]Other concerns discussed by Manski (1993), such as the reflection problem or selection, are unlikely to drive my

fire its worst-performing managers, while prohibiting its remaining managers from taking excessive risk to reduce the likelihood of further outflows. However, this alternative channel is difficult to explain my previous result that shows stronger response of socially connected managers, even when controlling for time-varying firm-specific shock. In addition, I also show that the dismissal affects the risk-taking behavior of managers in a *different* firm in the same building. These two results, (1) the within-firm heterogeneity and (2) across-firm contagion are difficult to reconcile based on the correlated group effect channel.

To further show direct evidence that the peer dismissal effect occurs through salient employment risk channel, I exploit state-level changes in the enforceability of non-compete agreements (NCAs), which introduce exogenous variation in underlying employment risk across firms located in different states.[7] NCA, which restricts departing employees from working for competing firms, increases the employees' cost of dismissal by restricting labor mobility (Liu, 2019).[8] Therefore, following strengthened NCA enforceability, fund managers will perceive greater career concerns under peer dismissal, which amplifies its effect on risk-taking behavior. Using a difference-in-differences (DID) design,[9] I find that, following peer dismissal, managers in treated states refrain from risk-taking activity more than managers in control states after an increase in NCA enforceability. I find no difference in risk-taking activity under peer dismissal between the two groups before the treatment. This result further complements my previous findings that the peer dismissal effect operates primarily through the employment risk channel.

In the last part of my paper, I analyze implications of the peer dismissal effect for mutual fund investors. First, I examine the mechanism underlying how fund managers achieve lower risk. Specifically, I decompose total volatility into systematic risk and idiosyncratic volatility (IVOL) and find

results. For example, reductions in risk-taking activities by peer managers are unlikely to lead to more frequent dismissal.

[7]As discussed in Ewens and Marx (2018), none of the changes were economically motivated (e.g., Jeffers, 2023). Instead, the changes were introduced by specific court rulings or ongoing legal proceedings. For example, the Florida Bar Association pushed a change in Florida (1996) because lawyers in the state were frustrated by uncertainty over the enforceability of NCAs. See Section 4.1 for more detail on NCAs.

[8]I find in my sample that managers indeed experience reduced labor mobility following the changes in enforceability. Specifically, contingent on leaving the firm, managers facing increased NCA enforceability are less likely to find a new job in the mutual fund industry in the following year. Similarly, Cici, Hendriock, and Kempf (2021) find a reduction in the departure rate of fund managers after NCA enforceability is strengthened.

[9]Following recent work that cautions against the potential bias of staggered difference-in-differences (DID) design, I rely on the stacked DID approach as suggested by Baker, Larcker, and Wang (2022). The authors demonstrate using simulations that the stacked approach is robust to the bias in the staggered approach and is more efficient than other methods introduced in recent studies (e.g., Callaway and Sant'Anna, 2021; Sun and Abraham, 2021).

that peer-dismissed managers decrease both market risk and IVOL, but not risks associated with other factors (e.g., Carhart, 1997; Fama and French, 2015).

Next, I examine whether the peer dismissal effect can mitigate agency problems in the mutual fund industry. Risk-taking behavior is generally viewed as a manifestation of misaligned incentives between fund managers and the underlying fund investors (e.g., Ma and Tang, 2019). Due to a convex flow-performance relationship (FPR) (e.g., Sirri and Tufano, 1998), fund managers face an option-like payoff when taking excessive risk, which can hurt fund performance and the underlying investors (Huang, Sialm, and Zhang, 2011). As discussed by Fama (1980), employment risk is an effective tool for mitigating agency problems. Peer dismissal can introduce additional costs to risk-taking activity by imposing salient employment risk on fund managers, thereby reducing their incentive to take excessive risks. Consistent with peer dismissal effectively mitigating ill-motivated risk-taking behavior, I find a stronger peer dismissal effect for agency-prone managers who face incentives to take excessive risks, as captured by those managing funds with high expense ratios (Gil-Bazo and Ruiz-Verdú, 2009) or strong convex FPRs (Ma and Tang, 2019) and those managing younger funds (Chevalier and Ellison, 1997).[10]

Lastly, I test whether peer dismissal can improve mutual fund performance. As discussed by Huang, Sialm, and Zhang (2011), ill-motivated risk-taking is detrimental to fund performance. Consistent with peer dismissal mitigating such behavior, I find a modest improvement in risk-adjusted return during the first year following peer dismissal. This corresponds to, for example, an increase of 26 basis points in the four-factor alpha (Carhart, 1997). In addition, the improvement is more pronounced for managers who face high incentive to take excessive risks, for whom performance increases by 53 basis points.

This paper is related to three strands of literature. First, I contribute to studies on social finance, with a particular focus on the microstructure of social transmission. Previous studies find that information senders are more likely to share successful strategies (e.g., Kaustia and Knüpfer, 2012; Bailey et al., 2018) because they want to make a good impression on others (Heimer, 2016) and

[10]Specifically, Gil-Bazo and Ruiz-Verdú (2009) argue that high-expense funds exploit naïve investors who are less responsive to expenses, which indicates that they are prone to agency problems (Huang, Sialm, and Zhang, 2011). Funds with a convex FPR have an incentive to take excessive risks (Sirri and Tufano, 1998), which is also indicative of agency problems (Ma and Tang, 2019). Lastly, younger funds face a more convex FPR (Chevalier and Ellison, 1997), consistent with investors learning about fund skill.

prefer to recount good memories rather than bad ones (Huang, Hwang, and Lou, 2021). Based on this idea, Han, Hirshleifer, and Walden (2022a) theoretically show how the self-enhancing nature of social networks contributes to the popularity of investment strategies. I add to this literature by showing that a *negative* outcome for peers can have a significant impact on investor behavior. This finding suggests that differences in the type of information, as well as its saliency, can lead to heterogeneity in peer effects (e.g., Chen and Hwang, 2022).

My paper also closely relates to studies on the incentive mechanism that mitigate agency-prone mutual funds' risk-taking behavior, typically viewed as a manifestation of agency problems (e.g., Huang, Sialm, and Zhang, 2011).[11] A recent study by Ma and Tang (2019) finds that one important mechanism is managerial ownership, which is associated with a reduction in risk-taking activity. This paper uncovers another mechanism, peer dismissal, that triggers career concerns for other managers (Fama, 1980). In particular, Kempf, Ruenzi, and Thiele (2009) and Cici, Hendriock, and Kempf (2021) find that fund managers reduce their risk-taking under systematic employment risks, such as recessions, over which firms have limited control. My evidence that peer dismissal primarily reduces risk-taking behavior by increasing employment risk, especially for agency-prone managers with high incentive to take more risk, highlights that firm turnover decisions and their spillover effects can mitigate agency issues in the mutual fund industry.[12] This also supports the view of Gibbons and Murphy (1992) that implicit incentives from career concerns complement the explicit incentives of compensation contracts.

My last set of contributions adds to the literature on career concerns and mutual fund behavior. First, my paper relates to studies on the role of career concerns in mutual fund risk choice. These studies typically capture managers' career concerns using time-invariant demographic traits (e.g., Chevalier and Ellison, 1997; Hong, Kubik, and Solomon, 2000). Although the literature agrees that career concerns are associated with less-risky decisions, its approaches are typically subject to endogenous matching. For example, Hong, Kubik, and Solomon (2000) find that younger analysts

[11] Other work consistent with this view includes Brown, Harlow, and Starks (1996), Chevalier and Ellison (1997), Kempf and Ruenzi (2008), Kempf, Ruenzi, and Thiele (2009), and Ma and Tang (2019), among many others.

[12] In the management literature, Connelly, Li, Shi, and Lee (2020) find that CEOs reduce their firm risk following the dismissal of a competing firm's CEO. While these authors rely on synthetic matching to control for counterfactual outcomes using firms in other industries, their approach cannot fully control for time-varying (e.g., industry-level) shocks. This paper, using a cleanly identified peer group and exogenous variations in underlying employment risk that is orthogonal to economic factors, provides evidence that peer dismissal effect is driven by salient employment risk

issue more conservative forecasts, but it is possible that they are simply selected to analyze more conservative firms. Acknowledging this possibility, Pool et al. (2019) capture changes in career concerns using decreases in personal wealth during the housing crisis and find that fund managers with greater career concerns reduce their delegated portfolio risk. I add to this literature by using peer dismissal to capture changes in career concerns and to highlight the role of social effects on career concerns that influence risk-taking incentives.

Second, I contribute to the growing literature on career concerns and mutual fund performance. Cici, Hendriock, and Kempf (2021) find that managers who face heightened employment risk driven by state-level changes in NCAs improve fund performance by exerting greater effort. I add to this literature by documenting how non-systematic changes in employment risk can improve fund performance, even in the absence of direct fund turnover (e.g., Khorana, 2001).

The remainder of this paper proceeds as follows. In Section 2, I describe my data and variables and explain how peer dismissal is identified. Section 3 examines the effect of peer dismissal on mutual fund risk-taking behavior. Section 4 uses a natural experiment to further establish that peer dismissal effect primarily occurs through salient employment risk channel. Section 5 studies the mechanism behind the reduction in risk-taking and its implications for mutual fund investors. I conclude my paper in Section 6.

2 Data and Summary Statistics

2.1 Mutual Funds Data

I collect mutual fund data from the Center for Research in Security Prices (CRSP) and Morningstar Direct. Following the procedure of Pástor, Stambaugh, and Taylor (2015), I match the two databases at the fund-class level based on ticker symbol, CUSIP number, and fund names. Specifically, I require each matched fund class to have similar monthly returns and total net assets (TNAs) between CRSP and Morningstar database. Using the matched sample, I group fund classes at the fund level by value-weighting their fund characteristics based on the previous quarter-end TNAs. I also collect mutual fund holding data from Thomson Reuters (S12).

Using the matched sample has several benefits. First, while Morningstar provides a snapshot of the most recent fund names, CRSP provides historical names for each fund. This allows me to match mutual fund data with funds in N-SAR filings on EDGAR based on fund names going back to 1994. Second, Morningstar provides a detailed historical list of fund managers responsible for each fund, which allows me to assign each manager to a distinct employer (investment advisory firm) based on the N-SAR filing, as detailed in the next section. Lastly, using the matched sample allows me to cross-check any potential errors regarding fund characteristics.

For the purpose of identifying peer dismissal, I use all CRSP-Morningstar matched funds domiciled in the U.S., including non-equity funds (e.g., bond mutual funds). This approach allows me to identify all dismissals within the company, not just those of equity fund managers.[13] When testing the relationship between peer dismissal and investment behavior of mutual funds, I focus on equity funds as defined by Morningstar Category,[14] and apply the standard filters used in the literature. Specifically, I exclude funds below $15 million (Elton, Gruber, and Blake, 2001; Chen et al., 2004), and funds with an age less than three years that are prone to incubation bias (Evans, 2010). Panel B of Table 1 shows the characteristics of the equity funds used in my analysis of risk-taking and performance.

2.2 Fund Manager Employment Data

To identify investment advisory firms that employ fund managers in Morningstar data, I rely on N-SAR and N-CEN filings on EDGAR. Registered investment management companies, often referred to as a "fund family," are required by the Investment Company Act of 1940 to file N-SAR to the Securities and Exchange Commission (SEC) semiannually. The report includes detailed financial information on mutual funds administered by the company, shares of funds sold to investors, brokerage commissions paid, and the company's affiliation ties to other brokerage and investment advisory firms. N-SAR was phased out and replaced by N-CEN in 2018.

[13] Ex ante, there is no reason to conjecture that peer dismissal affects investment behavior within the same asset group. If the relationship is due to salient employment risk, dismissal of any peers in the same company, including those who manage a different asset class, can saliently increase the attention to employment risk of other fund managers.

[14] Specifically, I focus on nine stock styles (small/mid/large cap × value/blend/growth) used in previous studies on domestic equity funds.

Important for my analysis, N-SAR and N-CEN report the list of investment advisory firms, identified with a distinct SEC registration number (*SEC-Number*), that are directly responsible for the management of their funds. First, I download all N-SAR and N-CEN reports from the EDGAR archive[15] and identify advisory firm(s) responsible for managing each fund. Next, I match this dataset to the CRSP-Morningstar matched fund sample using ticker and fund names. I am able to match over 95% of funds in the CRSP-Morningstar sample to those in N-SAR and N-CEN.

Using the matched sample of fund-firm-manager observations, I sort each manager into a distinct advisory firm as follows. First, if a distinct advisory firm exists for all funds run by the manager (as reported by Morningstar), then I assume that the manager works for that advisory firm. This is the case for over 60% of the manager-year observations. The remaining instances are due to (1) having multiple advisory firms that cover all funds run by the manager, or (2) having no advisory firms that cover all funds run by the manager. For these cases, I hand-collect manager employment information from Morningstar, LinkedIn, and advisory firm websites, and I assign managers into a distinct advisory firm each year. This procedure yields 18,608 U.S.-domiciled fund managers (146,252 manager-years) between 1994 and 2019. On average, approximately 9% of fund managers depart from their firm each year. This dataset maps each manager to distinct advisory firms they worked for and covers over 93% of manager-year observations in Morningstar. I also hand-collect manager characteristics from Morningstar, LinkedIn, and advisory firm websites, and acquire the office location of advisory firms from Form ADV. Panel A of Table 1 reports the number of managers and the departure rates during my sample period.

2.3 Identifying Manager Dismissal

My analysis begins with a two-step process to identify fund managers who are dismissed for underperformance. First, I identify instances in which managers leave their current firm. Second, among these managers, I identify those with subpar fund performance before their departure, using performance measures that are empirically associated with future departure based on my data.

Following Bonelli (2019), I assume that a manager leaves their current firm (*Departure*) if they discontinue working for the firm in the following year and cease to manage all funds they currently

[15] https://www.sec.gov/Archives/edgar/full-index/

manage (according to Morningstar). According to my manager employment record, roughly 9% of managers leave their firm on average each year.[16]

Next, to identify firm departures driven by poor fund performance, I empirically examine a set of performance measures that are associated with future departures. Specifically, I run the following linear probability model:

$$\begin{aligned} Firm\ Departure_{j,t} = \beta_1 Return_{i,j,t-4} + \beta_2 MS\ Rating_{i,j,t-1} + \beta_3 Flow_{i,j,t-4} \\ + X\Gamma_{i,j,t-1} + \delta_{style(i),t} + \epsilon_{i,j,t}, \end{aligned} \tag{1}$$

where i denotes fund, j manager, and t calendar quarter, respectively. The dependent variable, $Firm\ Departure_{j,t}$, is an indicator for firm departure of manager j. The independent variables of interest are $Return_{i,j,t-4}$, $Flow_{i,j,t-4}$, and $MS\ Rating_{i,j,t-1}$, which are past one-year raw return, fund flow,[17] and previous-quarter MS Rating of fund i managed by manager j, respectively. $\Gamma_{i,j,t-1}$ is a set of fund and manager characteristics. Note that the unit of observation is fund-manager-quarter, which reflects the fact that funds are often managed by multiple managers. Throughout my analyses, following Kostovetsky and Warner (2015), I assume that each manager contributes equally to fund performance and weight all observations by the inverse of the number of managers. I also include fund style-by-quarter fixed effect so that all performance measures are compared within the same fund style in a given quarter.

The results, provided in Table A1, show a negative and statistically significant relationship between firm departure and the three performance measures. The relationship is also economically significant. For example, a fund manager with 1-star MS Ratings faces a departure rate that is 3.9 percentage points higher compared with a manager with 5-star MS Ratings. Given that the unconditional firm departure rate is 2.6%, the effect is sizable.[18] Based on this empirical relationship, I

[16] In a few instances, a manager starts to work for a new firm, while continuing to manage the old funds. In all of these cases, I find that the old advisory firm changed its SEC number or was acquired by a new firm in the next year. I do not treat these cases as firm departures.

[17] The fact that flow negatively predicts departure, even after accounting for the effect of fund performance, is consistent with Kostovetsky and Warner (2015).

[18] Table A2 shows that these relationships continue to hold when tested separately within mutual funds of the same asset type; that is, within U.S. Equity, Balanced, International, Corporate and Municipal bonds, and other mutual funds. Furthermore, consistent with the literature, I find in Table A3 that the relationship between performance and departure is weaker (marginally stronger) for experienced (female) managers. I also find a marginally weaker relationship for fund managers with an MBA degree.

classify departing managers as *dismissed* if they were managing underperforming funds before their departure. Specifically, if a departing manager's fund return, flow, or MS Rating is in the bottom terciles within the same style group,[19,20] the manager is classified as dismissed. Figure 1 plots the distribution of fund performance of managers who experience firm departure. I find that roughly 75% of departing managers are classified as dismissed, which is consistent with the majority of managers leaving due to poor fund performance.

One important concern is that the several arbitrary choices I make when defining peer dismissal may introduce noise to my dismissal measure. For example, a fraction of managers classified as dismissed may in fact be voluntarily leaving the firm. However, I believe that this is unlikely to materially affect my results for the following reasons.

Importantly, when fund managers voluntarily leave their firm, they forego a severance package, which generally amounts to their base salary for several months or even up to a year. In my data, I find that the underperforming managers are significantly more likely to face difficulty in finding another job. For example, departing managers in the bottom performance terciles, compared to other departing managers, are (1) 30% less likely to find a job after in the following year, (2) 31% more likely to take more than a year to find a new job, and (3) 11% more likely to end up in a smaller firm[21]. Because of the significant worse off job market the underperforming managers face, the severance package is likely to be much more valuable for these managers, and they are much less likely to forego such compensation by voluntarily leaving their firm.

Nevertheless, it is still possible that a small fraction of underperforming managers may be voluntarily leaving the firm,[22] knowing they will be fired anyway. As the primary reason of their departure is due to underperformance, this may cause similar salience in employment risk for other managers. On the other hand, other fund managers may not be affected by voluntary departure. Using voluntary departures, defined as fund managers leaving the firm after having a good performance, whose performance is in the upper terciles, I find that these departures do not induce any changes in other peer managers' behaviors (see column 3 and 4 of Panel B in Table 7). Therefore, the noise

[19] When a fund manager runs multiple funds, I use the performance of the most underperforming fund.
[20] In the case of MS Ratings, this corresponds to ratings 1 or 2.
[21] The results are provided in Table A5.
[22] Defining dismissal using alternative thresholds for underperformance, such as bottom decile or quintile, instead of terciles, leads to qualitatively similar results in my paper.

in the dismissal measure due to the existence of voluntary departing managers is likely to create a bias *against* my main hypothesis[23].

2.4 Measure of Risk-Taking

To measure the extent of risk-taking by fund managers, I mainly use the holding-based risk-taking measure (Kempf et al., 2009; Huang et al., 2011; Ma and Tang, 2019), which captures the intended risk-taking of fund managers as reflected in mutual fund holdings. Specifically, *intended risk-taking* of a fund i is computed as the difference between intended volatility in quarter t and realized volatility in the prior quarter:

$$Risk\ Taking_{i,t}^{intended} = \sigma_{i,t}^{intended} - \sigma_{\ i,t-1}. \tag{2}$$

To compute the first term, the intended volatility ($\sigma_{i,t}^{intended}$), I use fund holdings in quarter t to compute the weight of each stock. Based on this weight, I calculate the standard deviation of daily fund returns, using stock return during the previous quarter. The idea is that managers project the future volatility of stocks based on past volatility and then shift their portfolio, which is reflected in next quarter holdings. The second term, realized volatility ($\sigma_{\ i,t-1}$), is the standard deviation of realized daily fund returns in quarter $t-1$. For ease of interpretation, I annualize the risk-taking measure by multiplying it by the square root of 252. In the robustness test section, I use alternative measures of risk-taking.

2.5 Summary Statistics

As discussed in Section 2.1, when analyzing the effect of peer dismissal on risk-taking behavior of mutual funds, I use a sample of domestic equity mutual fund and managers whose intended risk-taking measure is available from 1994 to 2018. To prevent other fund-level turnovers from affecting my result (e.g., Khorana, 2001), I exclude all fund-quarter observations that experience fund turnovers around a one-year window.

[23] I further explore for this possibility in Table A6 and find that only dismissal of peers who are unable to secure a new job evokes peer dismissal effect.

The summary statistics are provided in Panel B of Table 1. *Peer Dismissal* equals 1 if a manager's peer is dismissed in the previous quarter. Its mean is 0.17, suggesting that fund managers experience peer dismissal every 1.5-year on average. Panel C shows that managers with a dismissed peer do not exhibit an inferior fund return or MS Rating, but they do experience significantly less inflow compared with control managers. I find that this pattern is mainly driven by peer dismissals happening more frequently following bear markets. I examine whether my results capture these ex ante differences in my robustness test section.

3 Peer Dismissal and Risk-Taking

In this section, I empirically test competing hypotheses on the peer dismissal effect, as outlined in Section 1. Specifically, under the *salient employment risk hypothesis*, peer dismissal makes underlying employment risk more salient for other fund managers in the same company[24], thereby increasing their career concern. As shown in Figure 2, the likelihood of departure is a negative and convex function of fund performance: there is a large increase in the likelihood when the performance deteriorates, but a disproportionately less decrease when the performance improves. Therefore, when under career concern, fund managers have an incentive to reduce their portfolio risk to avoid ending up with a worse off performance (Chevalier and Ellison, 1999). Hence, following peer dismissal, fund managers on average reduce their portfolio risk[25]. Furthermore, the reduction in portfolio risk will be an increasing function of the underlying employment risk, since fund managers will be at a greater career concern as their higher employment risk becomes salient.

On the other hand, under the alternative *career advancement hypothesis*, the dismissed peer leaves

[24] Prior Psychology literature suggests a several trait for a salient event: when it relates to a potential threat (Öhman, Flykt, and Esteves, 2001), concerns socially related groups (Birmingham, Bischof, and Kingstone, 2008), and triggers an emotional reaction (Vuilleumier and Schwartz, 2001). Peer dismissal, which involves an employment risk (threat), workplace peers (social groups), and a dismissed peer's response (emotional reaction), is likely to be a salient event for other fund managers in the same company.

[25] It is also possible that if managers interpret peer dismissal as an increase in their termination threshold, extreme underperformers may *increase* risk to gamble on performance (Hodder and Jackworth, 2007; Hu, Kale, Pagani, and Subramanian, 2011). However, as discussed by Kempf, Ruenzi, and Thiele (2009), such a prediction implicitly assumes that managers are myopic because a higher portfolio risk also increases the likelihood of catastrophic performance, eliminating any potential for future employment. Empirically, the exact termination threshold below which managers increase their fund risk upon peer dismissal is uncertain, as the dismissal rate changes over time as a function of market conditions (e.g., Chevalier and Ellison, 1999; Zhao, 2005). In my sample, I do not find evidence of such behavior using an ad hoc threshold of extreme underperformance, such as bottom 1% or 5% within the same fund style.

behind job positions and asset management opportunities for other fund managers. This increases within-firm competition and creates tournament-like incentives (e.g., Kempf and Ruenzi, 2008). Therefore, following peer dismissal, fund managers increase their portfolio risk to compete for promotion and higher compensation opportunity.

I empirically test which channel dominates by examining the relationship between peer dismissal and mutual funds' risk-taking behavior during the following quarter. Next, I exploit the cross-sectional and time-series variations in employment risk and examine whether the variations in fund managers' response to peer dismissal is consistent with the hypotheses. Finally, I run a series of robustness tests of the baseline results, and further examine whether the results are driven by other alternative channels.

3.1 Baseline Results

I start my analysis by examining the relationship between peer dismissal and future risk-taking behavior by fitting the following weighted least squares (WLS) model:

$$Risk\ Taking^{intended}_{i,t} = \beta Peer\ Dismissal_{j,t-1} + \Gamma X_{i,j,t-1} + \epsilon_{i,j,t}. \tag{3}$$

The dependent variable, $Risk\ Taking^{intended}_{i,t}$, is the holding-based risk-taking measure as defined in Equation (2). The main independent variable of interest is $Peer\ Dismissal_{j,t-1}$, which is an indicator for whether the fund manager experienced peer dismissal in the previous quarter. Following Kostovetsky (2015), I weight all observations by the inverse of the number of the managers for each fund-quarter. All specifications include fund style-by-time fixed effect, which controls for time-specific risk-taking behavior for funds with the same style. All standard errors are two-way clustered at the fund and quarter level.

Table 2 presents the regression results. As shown in the first column, funds run by managers who experience peer dismissal are associated with 0.27% lower risk-taking. In the next two columns, I include time-varying characteristics related to the fund and its manager as control variables. The coefficient estimate is statistically significant (t-stats$=-5.43$). Ceteris paribus, a fund run by managers with a dismissed peer experiences a decrease of roughly 0.1 standard deviations in the risk-taking activity. Considering that a dismissed manager simultaneously affects 5.8 managers on average, the effect is economically significant.

In columns 4 and 5, I include an investment advisor fixed effect to control for unobservable firm heterogeneity. The size of the coefficient estimate is reduced, possibly due to smaller within-firm variation of the dependent variable, but the coefficient estimate remains statistically significant (t-stats$=-3.27$). Accounting for the within-firm variation, the economic magnitude remains quantitatively similar.[26] Overall, Table 2 provides a strong negative association between peer dismissal and future risk-taking, which I refer to as the "peer dismissal effect" throughout my paper.

Regarding fund characteristics, I find that expense ratio is positively associated with future risk-taking behavior, consistent with high-fee funds taking more risks due to agency issues (Gil-Bazo and Ruiz-Verdú, 2009). Risk-taking is also positively associated with fund turnover (Huang, Sialm, and Zhang, 2011) and negatively associated with fund age (Chevalier and Ellison, 1997). Fund flow shows a strong positive relationship with risk-taking activity, which may be driven by the flow-induced trades (Coval and Stafford, 2007; Lou, 2012). I examine whether this explains the baseline results in my robustness test. With regard to manager characteristics, younger managers who face a greater employment risk (Chevalier and Ellison, 1999) and female managers who are more risk-averse (Barber and Odean, 2001) engage in less risk-taking, although the latter is statistically insignificant after including advisor fixed effect, possibly due to low power from the small number of female managers.

3.2 Underperforming Managers

Previous studies document that fund managers with higher employment risk adjust their portfolio risk to reduce the likelihood of dismissal (Chevalier and Ellison, 1999; Kempf, Ruenzi, and Thiele, 2009). Intuitively, if the negative relationship between peer dismissal and risk-taking is driven by employment risk becoming more salient, I should expect to find a stronger effect among underperforming managers who face a high employment risk.

To test this idea, I regress an intended risk-taking measure on an indicator of peer dismissal and

[26] Specifically, I first demean the dependent variable across funds within each quarter, then again demean within each advisory firm. The standard deviation of the demeaned dependent variable is 0.80, which corresponds to an 8% reduction for peer-dismissed funds ($-0.0645/0.80 = -8\%$).

its interaction with various fund performance measures:

$$\begin{aligned} Risk\ Taking_{i,t}^{intended} = {} & \beta_1 Peer\ Dismissal_{j,t-1} \times Performance_{i,t-1} \\ & + \beta_2 Peer\ Dismissal_{j,t-1} + \Gamma X_{i,j,t-1} + \epsilon_{i,j,t}, \end{aligned} \tag{4}$$

where $Performance$ is a measure of fund performance, which differs across specifications. The single term for $Performance_{i,t-1}$ is included in the vector of control variables, $X_{i,j,t-1}$. All specifications include style-by-quarter fixed effects, so that performance measures are compared within the same fund style group.

The results are provided in Table 3. In the first column, I use the fund return during the past one year as a measure of performance. The interaction term between *Peer Dismissal* and past return is positive and statistically significant at the 1% level (t-stats=3.05), which suggests that underperforming managers exhibit a greater reduction in risk-taking behavior after peer dismissal. Next, following Kempf, Ruenzi, and Thiele (2009), I account for the fact that investors primarily care about the performance rank, rather than the absolute performance, when allocating their assets (Sirri and Tufano, 1998). Specifically, each fund is sorted into 100 ranks based on its past one-year return within its style category and then normalized so that the rank ranges from 0.01 to 1. Column 2 shows that underperforming managers exhibit a stronger peer dismissal effect when relative rank is used as a performance measure. Column 3 shows a similar result when the peer dismissal effect is estimated among funds with similar performance by including a style-by-quarter-by-rank fixed effect.

In columns 4 to 6, I use an indicator variable for bottom terciles performance as measured by past one-year return, past one-year flow, and MS Rating. Note that these measures are the same as those used previously to define *Dismissal*. I find a statistically stronger peer dismissal effect for funds in the bottom return and MS rating terciles group.

Lastly, I consider the joint effect of the three performances by constructing a composite measure, which is the sum of three indicators of bottom-terciles performance group. The results are provided in column 7. The interaction term between *Peer Dismissal* and $\mathbb{1}[Composite = 0]$ is statistically insignificant. This finding suggests that *Peer Dismissal* has little effect on changing the risk-taking behavior of managers who are not experiencing underperformance. However, the interaction term between *Peer Dismissal* and the indicator for the most underperforming group ($\mathbb{1}[Composite = 3]$) is statistically significant (t-stats$=-3.70$) with a large economic magnitude (-0.254). Furthermore, the peer dismissal effect increases monotonically with the composite score. Overall, the result in Table 3 is consistent with the peer dismissal effect taking place through the employment risk channel: peer dismissal makes employment risk more salient, especially for poor-performing managers who are vulnerable to being fired.

3.3 Junior Managers

In this section, I examine whether the peer dismissal effect is stronger for less experienced, junior fund managers. Chevalier and Ellison (1999) show that junior fund managers are more likely to experience *fund* turnover for poor performance, compared to more experienced, senior managers. I find a similar effect of poor performance with regard to *firm* turnover. Figure 2 shows that junior fund managers are more likely to leave their firm for having a poor performance, compared to experienced fund managers. In addition, the empirical relationship between past performance and the likelihood of departure is much more convex for junior managers, whereas the relationship is linear for experienced managers. These results suggest that, consistent with past study of fund turnover, junior fund managers are punished more harshly for their poor performance. In other words, they face higher underlying employment risk, compared to senior managers.

If the peer dismissal makes such underlying employment risk more salient, I would expect to find a stronger response from less experienced fund managers who face higher employment risk. I test this idea by estimating the peer dismissal effect separately for a group of managers sorted based on the length of their experience. Specifically, At each quarter, fund managers are sorted into terciles based on their experience each quarter. The lowest experience group is sorted as *Low Experience*, and the highest and second-highest group are sorted as *High Experience* and *Mid Experience*, respectively. I estimate the peer dismissal effect separately for these groups by interacting the *Peer Dismissal* dummy with indicators for the terciles groups.

The results are presented in Table 4. Column 1 shows that the peer dismissal effect is stronger for less experienced fund managers. Specifically, the peer dismissal effect is focused on managers with the lowest and second-lowest experience group, but insignificant for the most experienced group.

The difference of peer dismissal effect between the lowest and highest experienced group is also statistically significant at the 5% level ($pvalue = 0.0402$).

If less experienced fund managers are underperforming compared to the senior managers on average, the previous result may be capturing the underperformance effect as documented in section 3.2.[27] To account for this possibility, I use subsamples conditioning on their past performance in the next two columns. Specifically, I define underperformers as those whose past one year return is in the bottom terciles within the fund style-quarter group.

Column 2 shows that, following peer dismissal, less experienced fund managers continue to reduce their portfolio risk more than their senior counterparts, even when compared with similarly underperforming managers. Again, consistent with the full sample result in column 1, it is primary the less experienced managers responding to peer dismissal. Again, The difference of peer dismissal effect between the lowest and highest experienced group is also statistically significant at the 10% level ($pvalue = 0.0564$). In column 3, I find a qualitatively similar result when comparing within non-underperforming managers.

In summary, the results in Table 4 are consistent with the salient employment risk channel: less experienced fund managers who face higher employment risk compared to more experienced, senior managers, respond more strongly to peer dismissal as their underlying employment risk becomes more salient.

3.4 Bear Markets

In this section, I examine whether the peer dismissal effect is stronger during periods of increased career concerns. Prior studies show that fund managers face a higher likelihood of dismissal and fund closure during a recession (Chevalier and Ellison, 1999; Zhao, 2005), when the incentive to maintain their jobs dominates their incentive for compensation (Kempf, Ruenzi, and Thiele, 2009). I hypothesize that peer dismissal is more salient during economic downturns, with managers exhibiting greater reduction in their risk-taking behavior.

First, I confirm in my data that fund managers, and especially underperforming managers, face a

[27] While I do include past performance measures, such as past return and flow, as control variables, they not fully account for the performance effect if the linear relationship assumption is violated.

stronger likelihood of departure during the recession. In Table A4, I define recession as quarters when the past one-year market return is at the bottom 5%, 10%, and 25% of my sample period. I find that the marginal probability of firm departure is 0.95% higher when the market return is in the bottom decile of my sample period. In addition, the higher departure rate is primarily focused on underperforming managers.

Based on this result, I examine whether the peer dismissal effect is stronger following bear markets when employment concerns are elevated. Specifically, I run the following regression:

$$\begin{aligned} Risk\ Taking^{intended}_{i,t} &= \beta_1 Peer\ Dismissal_{j,t-1} \times Recession_t \\ &\quad + \beta_2 Peer\ Dismissal_{j,t-1} + \Gamma X_{i,j,t-1} + \epsilon_{i,j,t}. \end{aligned} \tag{5}$$

The results are presented in Table 5. Columns 1 through 3 show that the peer dismissal effect is stronger during the recession. Specifically, the peer dismissal effect is roughly six times stronger during recession, defined as the period when market return is in the bottom ventile (column 1). As the market return threshold of recessions increases (bottom decile and quintile, columns 2 and 3), the magnitude of the interaction term declines monotonically.

In column 4, I interact *Peer Dismissal* with continuous market return, instead of using an indicator for recession. This specification estimates how the peer dismissal effect linearly varies with the market return. The coefficient estimate of the interaction term is positive and highly significant (t-stats=3.61), which suggests that peer dismissal has a greater negative impact on risk-taking behavior when market returns are lower. In summary, Table 5 shows that the negative relationship between peer dismissal and risk-taking strengthens during years with poor market returns when job security concerns are elevated.

3.5 Social Ties with the Dismissed Peer

The results so far indicate that peer dismissal decreases risk-taking behavior by evoking a salient employment risk for managers. This section provides further evidence by examining whether the peer dismissal effect varies with managers' social ties to the dismissed manager. Following prior literature that emotional contagion and social learning are stronger when peers have social ties (Lin and Utz, 2015; Kumar, Rantala, and Xu, 2022),[28] I hypothesize that the peer dismissal effect is amplified when managers have stronger social ties to the dismissed manager.

I measure social ties along three key dimensions: educational background, age, and work relationship. The results are provided in Table 6. In column 1, I define a manager as having a social tie with the dismissed manager if they graduated from the same college. The estimated coefficient on the interaction term between *Peer Dismissal* and *Social Tie* is negative and statistically significant at the 5% level (t-stats=-2.09), which suggests more than a 1.3-fold increase in the peer dismissal effect compared with managers without educational ties.

In column 2, I define a manager as having social ties with the dismissed manager if their age difference is below the median among the group of peer-dismissed managers, which corresponds to an age difference of less than six years. While the estimate of the interaction term is negative, indicating a stronger peer dismissal effect, it is statistically insignificant (t-stats=-1.23).

Next, I define social bonds based on the manager's work relationship with the dismissed peer. Specifically, I first define the experience overlap between a manager and the dismissed manager as follows:

$$Experience\ Overlap_{j,t} = \frac{Overlap_{j,k,t}}{Tenure_{j,t}}, \tag{6}$$

where $Tenure_{j,t}$ is the number of years since the first appearance of the manager j in quarter t, and $Overlap_{j,k,t}$ is the years manager j and dismissed manager k spent together in the same firm as of quarter t. I define a manager as having a social relationship with the dismissed manager if $Experience\ Overlap_{j,t}$ is above the median among peer-dismissed managers, which corresponds to 0.5; that is, the manager spent more than 50% of their tenure with the dismissed peer. Column 3 shows that the interaction term is negative and statistically significant at the 10% level (t-stats=-1.85). In terms of economic magnitude, a manager with a close work relationship with the dismissed peer exhibits a three-fold greater reduction in risk-taking compared with those without such a relationship.

[28] Prior studies in general document stronger peer effects among groups with social connections. For example, Dimmock, Gerken, and Graham (2018) find a stronger contagion of misconduct among ethnically similar employees; Huang, Hwang, and Lou (2021) document a stronger rate of information transmission among retail investors with similar income, age, and gender; and McCartney and Shah (2022) find a stronger peer effect of mortgage refinancing among people of a similar race.

Lastly, I consider the joint effect of the social relationship by constructing a composite measure, which is the sum of three indicators of social ties. Column 4 shows that the interaction term between *Peer Dismissal* and $\mathbb{1}[Composite = 2+]$, an indicator for having two or more social ties, is negative and statistically significant at the 5% level (t-stats$= -2.62$).[29] These managers exhibit four times stronger reduction in risk-taking compared with managers without any ties. In summary, the results in Table 6 show that managers with closer social ties with the dismissed manager, captured through education, experience, and a composite measure, exhibit a greater reduction in risk-taking behavior.

3.6 Robustness Tests

3.6.1 Alternative Dependent and Independent Variables

In this section, I run a battery of robustness tests of my previous findings on the peer dismissal effect. First, I use alternative measures of risk-taking behavior. The results are provided in Panel A of Table 7. In column 1, I measure risk-taking as the change in *realized* volatility:

$$Risk\ Taking^{realized}_{i,t} = \sigma_{\ i,t} - \sigma_{\ i,t-1}. \tag{7}$$

Consistent with the results based on the holding-based risk-taking measure, I continue to find a negative relationship between peer dismissal and risk-taking. In column 2, I control for the average level of fund volatility using the ratio of the holding-based volatility and previous realized volatility (Huang, Sialm, and Zhang, 2011) as a dependent variable:

$$Risk\ Taking^{ratio}_{i,t} = \frac{\sigma^{intended}_{i,t}}{\sigma_{\ i,t-1}}. \tag{8}$$

I find a similar peer dismissal effect, which corresponds to a roughly 0.35% decrease in volatility relative to the previous quarter. In column 3, I use the absolute value of the holding-based risk-taking measure as a dependent variable. Although the coefficient estimate is slightly smaller than

[29] I find a qualitatively similar result when I separately estimate interactions for $\mathbb{1}[Composite = 2]$ and $\mathbb{1}[Composite = 3]$, although the interaction term for the latter is insignificant due to the small sample size. The economic magnitudes are very similar, but with only 164 observations with score$= 3$ (0.3% among observations with peer dismissal), the interaction term has insufficient power.

that in Table 2, it is negative and significant at the 1% level. This result suggests that the negative relationship between peer dismissal and risk-taking is mainly driven by funds engaging in less risk-taking behavior, rather than shifting down their risks.

Next, I use an alternative measure of peer dismissal, $log(1+\#\ Peer\ Dismissal)$, the log-normalized number of peer dismissals within the firm. Column 4 shows that the result continues to hold. In the last column, I separately estimate the effect of peer dismissal under single and multiple (> 2) peer dismissals. The peer dismissal effect under multiple peer dismissals is roughly 70% stronger compared with the effect under single dismissal, and the difference is marginally significant at the 10% level.[30] The result is consistent with multiple peer dismissals evoking more salient employment risk, as reflected in a greater reduction in risk-taking behavior.

3.6.2 Placebo Tests

Next, I run a placebo test using index funds and other firm turnover events. Index fund managers have limited discretion on portfolio allocation, and their performance is evaluated on how well their fund tracks its index. Therefore, these managers lack both the incentive and the ability to alter their risk-taking behavior under heightened employment risk. The results are provided in Panel B of Table 7. The first column shows little evidence that index fund managers reduce risk-taking behavior in response to peer dismissal. The estimated coefficient on *Peer Dismissal* is statistically insignificant (t-stats$=-0.21$), and its magnitude is less than one-tenth of that obtained from the sample of actively managed funds. I find similar results when using the realized risk-taking measure in column 2.

Next, I consider other manager turnover events within an advisory firm. The first variable, *Peer Advancement*, is an indicator variable when a peer manager departs the firm followed by a *good* performance, as defined by their past fund return, flow, or MS Ratings being in the upper terciles within style-quarter group. This variable directly corresponds to my independent variable of interest, *Peer Dismissal*, which captures firm departure followed by *negative* performance. Column 3 shows that the relationship between *Peer Advancement* and future risk-taking is statistically in-

[30] The marginally statistical significance may be due to low instances of multiple peer dismissals. Among the sample of peer-dismissed managers, more than 80% experience dismissal of a single peer.

significant and of a wrong sign (t-stats=0.06). I find a similar result using the realized risk-taking measure, as shown in column 4.

I also consider another firm turnover event, *Peer Demotion*, which is an indicator variable when a peer manager ceases to manage all funds in a given quarter, followed by negative performance, similarly defined as in *Peer Dismissal*. The difference is that, instead of leaving the firm, these managers stay in the same firm and start managing another fund.[31] Columns 5 and 6 show that the relationship between *Peer Demotion* and future risk-taking is statistically insignificant. In all four specifications (columns 3 through 6), *Peer Dismissal* continues to show a robust negative association with risk-taking measures, even when other turnover events are controlled for.

3.6.3 Correlated Group Effect

One important concern is that the peer dismissal effect may be capturing correlated group effect (Manski, 1993); that is, an unobservable, time-varying group-level shock may coincide with both peer dismissal and reduction in risk-taking behavior among remaining peers. For example, a firm suffering reputational damage (e.g., 2003 mutual fund trading scandal) will experience long-lasting significant outflows of its funds (Kisin, 2011), and it may have to fire its worst-performing managers. To reduce the likelihood of any further outflows, the firm can also prohibit its existing managers from taking excessive risk, which can spuriously appear as a peer dismissal effect. This time-varying firm-level shock cannot be fully absorbed by advisor fixed effect. In addition, because *Peer Dismissal* is defined at firm-quarter level, I cannot directly include firm-time fixed effect due to a collinearity problem.

I examine whether my results are driven by correlated group effect in two ways. First, building on the results from Table 6, I examine whether socially connected managers respond more strongly to peer dismissal while controlling for time-varying firm-level shock, that is, including firm-by-time fixed effect. In other words, I test for within-firm heterogeneity of peer dismissal effect with regard to social ties with the dismissed peer. If my previous results are driven by correlated group effect, I would expect to find a similar response from managers in the same company after accounting for firm-by-time fixed effect. If not, I should continue to find stronger response from socially connected

[31]Consistent with these cases being demotions, the majority of these managers experience reduction in their TNAs.

managers even within firm-quarter.

Second, I examine if the dismissal also affects fund managers in a different, but nearby firms. To this end, I focus on two different firms located in the same building but in a different office. Form ADV provides detailed office location of advisory firms, including their zipcode, building address, and office number. The managers in these two firms are likely to be socially connected: they work in the same mutual fund industry, and are also likely to run into each other in their everyday activity (e.g., in elevators during lunch and commuting hours). If my previous results are primarily driven by time-varying firm-level shock, I should not expect to find any evidence of contagion across firm.

To this end, I first iterate the social tie analysis (Table 6) while including advisor-by-quarter fixed effect in lieu of advisor fixed effect. As shown in the first three columns of Table 8, I continue to find stronger response for socially connected managers. Specifically, fund managers with similar age and long work relationships respond more strongly to peer dismissal, even after accounting for the common time-varying firm-level shock. The coefficient estimate on education remains negative ($t - value = -1.62$), although it is no longer statistically significant. Column 4 shows a similar result, based on the composite measure, that fund managers respond more strongly to peer dismissal as their social ties strengthens. These results suggest strong *within-firm-quarter* heterogeneity with regard to social connection with the dismissed manager, which is inconsistent with the correlated group effect hypothesis.

In column 5 and 6, I test for across-firm effect by including *Same Building Dismissal* dummy, which is an indicator for dismissal in a different firm within the same building according to Form ADV. Importantly, to make sure that the dummy variable does not capture a shock common to the fund family, I require that the advisory firms do not belong to the same family. The result shows that a dismissal in one company also reduces the risk-taking of managers in the other company in the same building. This result is consistent with contagion of career concern *across* firms, which is difficult to reconcile based on the correlated group effect. To sum up, the evidence of within-firm heterogeneity (while controlling for time-varying firm-level shock) and across-firm contagion is inconsistent with correlated group effect channel.

tab:t-departure-policy

3.6.4 Flow-Induced Trading

Previous studies document that mutual funds trade to accommodate investor flows (Coval and Stafford, 2007; Dubofsky, 2010; Lou, 2012). Although I include log-normalized fund flow as a control variable, any remaining nonlinear or time-varying relationship between fund flow and risk-shifting may be spuriously attributed to the peer dismissal effect. To account for this possibility, I first test the peer dismissal effect within a tight group of funds with similar flow. Specifically, in each quarter, I partition funds into 100 groups (percentile) based on their past one-year flow and examine the peer dismissal effect within each group by including its fixed effect.[32] As shown in column 1 of Table 9, I continue to find strong evidence of the peer dismissal effect with a similar economic magnitude. Partitioning funds into looser (50) or tighter (200) groups or comparing within each style-quarter-flow group yields similar results (columns 2 through 6).

Next, I test whether the peer dismissal effect is stronger for a subset of funds that are more subject to flow-induced trading. Pollet and Wilson (2008) find that mutual funds with small assets or those investing in large-cap stocks are less susceptible to flow-induced trading because they face less price impact or liquidity cost, and therefore can respond to fund flows by trading their existing holdings.[33] I also test whether the peer dismissal effect is weaker for negative-flow funds. These funds tend to liquidate their existing holdings, compared with positive-flow funds that diversify beyond their pre-holdings (Lou, 2012). If flow-induced trading is partly responsible for the peer dismissal effect, I should expect the peer dismissal effect to be weaker for small-sized, large-cap, and negative-flow funds. Columns 7 through 9 show that the peer dismissal effect is similar for these subsets of funds relative to others.[34] To sum up, Table 9 provides robust evidence that the peer dismissal effect is not a spurious result of flow-driven trades.

[32] Note that, on average, each quarter has roughly 1000 funds, so the fixed effect creates a tight comparison group.

[33] Specifically, Pollet and Wilson (2008) find that funds with large assets or those investing in small-cap funds face limits to scalability due to facing a larger price impact or liquidity cost.

[34] Note that all separate groups have a statistically significant peer dismissal effect when separately estimated (unreported).

3.6.5 Alternative Fixed Effects

In this section, I test the peer dismissal effect by including various fixed effects to rule out alternative explanations. First, I check whether the peer dismissal effect captures time-varying regional economic shock. Under such a shock, an investment advisory firm is more likely to fire its underperforming managers while also prohibiting the remaining managers from taking excessive risks. This may create a spurious relationship between peer dismissal and risk-taking, which is, in fact, mechanically driven by firm-level policy.

I control for the regional economic shock by including the city-by-quarter-by-style fixed effect, based on the location of the advisory firm as reported in Form ADV. The results are provided in Panel A of Table 10. The first column documents a similar peer dismissal effect when I control for regional shocks. Note that the fixed-effects approach does not fully account for the time-varying shocks *within* the city (e.g., at the firm level). I alleviate this concern using the DID approach, as detailed in Section 4.[35]

Next, I control for the effect of the size of the advisory firm. Ceteris paribus, these firms experience more frequent peer dismissal on average. They may also have better governance and provide a clear dismissal policy, under which managers refrain from agency-prone risk-taking behavior (Huang, Wei, and Yan, 2007). Although I include firm fixed effect and firm size as control variables, firm size varies over time, and its effect may also be nonlinear and time-variant. I flexibly control for firm size by including a firm size percentile fixed effect. In columns 5 and 6, I find a similar peer dismissal effect, which is statistically significant at the 1% level. In columns 7 and 8, I include firm size-by-quarter-by-style fixed effect. The magnitude of the peer dismissal effect remains similar, but it is statistically significant only at the 10% level, possibly due to a small number of comparison groups from including high-dimensional fixed effects.

Lastly, I test whether the peer dismissal effect is driven by unobservable manager or fund heterogeneity. Panel B of Table 10 shows that replacing the firm fixed effect with a manager or fund fixed effect yields a similar peer dismissal effect, despite the more stringent fixed effects. In summary, the peer dismissal effect survives the inclusion of granular fixed effects, and I find that the effect is

[35] Note that this specification increases adjusted R^2 by roughly 50% ($(0.593 - 0.385)/0.385 = 0.54$). The number of observations is reduced by dropping observations without an office location.

robust to controlling for regional economic shocks, firm size, manager attributes, and fund-specific factors.

3.7 Discussion on Learning Channels

In this section, I discuss other channels that may explain my previous results. Firstly, the results may be driven by changes in informational environments driven by peer dismissal. If fund managers exchange valuable information with their peers, the peers' departure may change the information set of other fund managers, which is reflected as changes in risk. Although it is theoretically unclear why this change should necessarily lead to *lower* risk, my results suggest that this channel is unlikely to explain the peer dismissal effect.

Specifically, recall that dismissed fund managers are defined as those with poor performance prior to their departure. To the extent that more skilled managers provide more valuable information, the departure of managers with good performance are likely to change the informational environment to a greater extent. As discussed in Panel B of Table 7, the departure of outperforming managers (*Peer Advancement*) do not affect other fund managers' portfolio risk (columns 3 and 4). In addition, I find that equity fund managers show a similar response to the dismissal of non-equity fund managers as to the dismissal of equity fund managers (unreported). Insofar as information is primarily shared among managers of funds with similar asset types, this result is also inconsistent with informational environment change channel.

Another possibility is that fund managers may be learning about changes in their firm's dismissal policy from peer dismissals. For example, the initial dismissal may signal that the firm is implementing a lower tolerance for retaining underperforming managers. In this case, the remaining poor performing managers may be rationally responding by reducing their portfolio risk, but not due to *salient* employment risk itself.

I test this idea by examining whether fund managers are more likely to leave their firm following previous peer dismissal. Specifically, I regress *Firm Departure*, an indicator for the manager j's firm turnover in current quarter, on *Peer Dismissal* during previous 1, 2, and 4 quarters. If the past peer dismissal positively predicts future manager departure—especially for underperforming managers—this may indicate that peer dismissal signals changes in firm turnover policy.

The results are provided in Table A7. In columns 1 through 3, *Peer Dismissal* equals one if manager j experienced peer dismissal in their firm in the previous quarter. Column 1 shows that peer dismissal in the previous quarter does not predict the future departure of a manager. In column 2 and 3, I partition managers based on their past one-year performance. Similarly as in previous analyses, I define underperformers as managers whose fund performance is in the bottom terciles with respect to fund return, fund flow, and Morningstar ratings within its fund category. I find that for both underperformers and non-underperformers, peer dismissal in the previous quarter does not predict future departure. In columns 4 through 9, I repeat the analyses using peer dismissal during the past 2 and 4 quarters. I find qualitatively similar results that past peer dismissal does not predict future departure, both for underperformers and non-underperformers.

3.8 Measurement Error in Peer Dismissal Variable

As discussed in Section 2.3, it is possible that voluntarily departing managers may be classified as peer dismissal, in which case may introduce noise to my *Peer Dismissal* variable. Because voluntarily departing managers would not introduce saliency to the remaining fund managers, the misclassification would bias against finding peer dismissal effect.

I account for this possibility running a placebo test by separating dismissed peers into those who immediately find a new job following the dismissal ("*Employed*") and those who do not ("*Unemployed*"). The idea is that those who find a new job—who are more likely to be a voluntary departure—would not evoke salient employment risk. This is precisely what I find in Table A6, which shows that fund managers' reduce their fund risk only in response to dismissed peers who do not secure immediate employment.

4 Difference-in-Differences Analysis

4.1 Background on Non-Compete Agreements

In this section, I use a natural experiment to further establish that the salient employment risk is the primary channel for the peer dismissal effect. To this end, I exploit state-level changes in enforceability of non-compete agreements (NCAs) as an exogenous shock. NCAs are often found in high-skill and high-paying jobs (Bishara, Martin, and Thomas, 2015), with an average incident rate of 42% in the management occupation in the finance and insurance industry (Starr, Prescott, and Bishara, 2021). NCAs, which typically prohibit departing employees from working for a competing firm or establishing their own firm for a finite period within a geographical region, are considered to increase the cost of potential dismissal of employees by restricting their labor mobility (Liu, 2019).[36,37] Intuitively, strengthening the enforceability of NCAs increases the cost of potential dismissal, and vice versa.

4.2 Research Design and Identifying Assumption

A recent study by Ewens and Marx (2018) identifies 14 state-level changes in the enforceability of NCAs, based on Supreme Court rulings and state legislature changes. The list is constructed by first searching through a comprehensive reference of state-level legislative and judicial changes (Malsberger, Brock, and Pedowitz, 2016), as well as Lexis/Nexis on Supreme Court decisions that affect NCAs. From the list of three dozen legislative or judicial changes, the authors ask two employment lawyers to narrow the list down to 14 material changes in the enforceability of NCAs. Building on this work, my empirical strategy is to implement DID analysis by comparing the peer dismissal effect between treated and control states, before and after the changes in NCA enforceability. The idea is that managers in treated states where the enforceability of NCAs becomes

[36] Generally, NCAs are enforceable even when employees are fired. In a few states, NCAs are not enforceable if the employee is laid off for reasons other than performance or misconduct (e.g., Arkansas, Iowa, Kentucky, Maine, Mississippi, New York); for example, see SIFCO Indus., Inc. v. Advanced Plating Techs., Inc. 867 F.Supp. 155 (S.D.N.Y. 1994). However, even in these states, NCAs are enforced when employees are fired for a fair cause, such as underperformance.

[37] NCAs are not necessarily used more often at large versus small firms. For specifics, see https://www.jamesbeck.com/2020/05/20/spotlight-on-trends-in-the-asset-management-industry-non-compete-and-non-solicitation-clauses/#single-post-content

stronger (weaker) will perceive a higher (lower) employment risk under peer dismissal[38] because of the higher cost of potential dismissal driven by restricted job mobility. By leveraging on plausibly exogenous variation in employment risk, my objective is to establish that peer dismissal affects risk-taking behavior primarily through salient employment risk channel.

The identifying assumption is that in the absence of law changes, the effect of peer dismissal on risk-taking would remain similar over time between treated and control states. This exogeneity condition is well supported. As discussed in Ewens and Marx (2018), none of the 14 changes was motivated by economic issues, such as the prospects of a certain industry or company (Bonelli, 2019). Instead, most of the changes were introduced by court rulings in ongoing legal proceedings.[39] Other studies exploiting these changes also document a similar trend in GDP per capita between treated and control states (e.g., Jeffers, 2023), which suggests the shift in NCA enforceability is unlikely to influence and to be influenced by distinct economic conditions in the treated states. I examine the validity of this assumption by whether the trend in the peer dismissal effect remains similar between funds in the treated and control states before the law changes.

A body of recent work in econometric theory cautions against the potential bias of a staggered DID design that involves multiple treatment groups with a different sequence, often referred to as two-way fixed effect (TWFE) DID. The idea is that its average treatment effect on the treated (ATT) includes the effect when the earlier-treated group is used as an effect control group for the later-treated groups (Baker, Larcker, and Wang, 2022). Under a heterogeneous treatment effect over time or across the treatment group, which is likely to be the case in my setting, the estimate of the average treatment effect based on TWFE staggered DID is biased (Sun and Abraham, 2021; Goodman-Bacon, 2021).[40] Instead of relying on the contentious staggered DID approach, I use the stacked DID approach (Gormley and Matsa, 2011; Cengiz et al., 2019; Deshpande and Li, 2019), which is the alternative estimator suggested by Baker, Larcker, and Wang (2022). They show that

[38] In C.1, using the DID approach, I examine whether fund managers *do* face heightened employment risk after NCA enforceability is strengthened. I find that this indeed is the case. Conditioning on their departure, managers in states that strengthened NCA enforceability are significantly less likely to move to a new advisory firm, compared with managers in control states. The reduction in job mobility is consistent with increased employment risk (Bonelli, 2019; Cici, Hendriock, and Kempf, 2021), which validates my research design.

[39] For example, a change in Florida (1996) was pushed by the Florida Bar Association. Specifically, lawyers in the state were frustrated by the unclear status of the enforceability of NCAs, which hindered offering confident guidance to their clients.

[40] For other recent studies on this issue, see De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021), and Imai and Kim (2021).

the stacked approach is robust to the bias in the staggered approach, and also more efficient than other methods introduced in recent studies (e.g., Callaway and Sant'Anna, 2021; Sun and Abraham, 2021).

4.3 Baseline Difference-in-Differences Result

In this section, I examine whether the negative relationship between peer dismissal and risk-taking behavior becomes stronger under strengthened NCA enforceability. Following the stacked DID approach of Cengiz et al. (2019) and Baker, Larcker, and Wang (2022), I first create event-specific panel datasets for each state-level shock. Each panel (*cohort*) consists of fund-manager-quarter observations of the treated state and control states around a five-year window of the event date.[41] For each dataset, I require funds to exist before and after the event. Among 14 states with changes in NCA enforceability, I find that the number of treated funds is very small in eight of the states.[42] As the panel datasets for these states would be extremely unbalanced, I exclude these states in my baseline DID analysis. In my robustness test, I incorporate all 14 states by creating a balanced panel using propensity score matching (PSM). The remaining six treated states that I mainly focus on are Colorado, Georgia, Illinois, Ohio, Texas, and Wisconsin.

I illustrate the data-construction procedure using Ohio, the earliest treated state in my sample (March 10, 2004). First, I select all funds exclusively managed by an advisor in Ohio before and after the five-year window of the event quarter (Q1, 2004). I then select all observations of control funds that are managed by advisors in never-treated states during the same time window. This procedure is repeated for the remaining five cohorts. I stack the six panel datasets, and fit the following regression:

$$
\begin{aligned}
Risk\ Taking^{intended}_{i,j,c,t} = {} & \beta_1\ PeerDismissal_{j,c,t} \times Treat_{j,c,t} \times Post_{c,t} \\
& + \beta_2 PeerDismissal_{j,c,t} \times Treat_{j,c,t} + \beta_3 PeerDismissal_{j,c,t} \times Post_{c,t} \\
& + \beta_4 PeerDismissal_{j,c,t} + \Gamma X_{i,j,c,t} + \delta_{i,c} + \lambda_{t,g,c} + \epsilon_{i,j,c,t},
\end{aligned} \tag{9}
$$

[41] To further eliminate the bias arising from staggered treatment timing (Sun and Abraham, 2021; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021), I do not use managers in not-yet and already-treated states as a control group; that is, all control group managers come from never-treated states. However, including not-yet and already-treated states as controls does not affect my results.

[42] Table A8 reports the number of funds around the event window for each of the treated states.

where i, j, and c indicates fund, manager, and event cohort, respectively. I include fund ($\delta_{i,c}$) and group-by-quarter fixed effect ($\lambda_{t,g,c}$), both of which are saturated within each event cohort (Baker, Larcker, and Wang, 2022). The group-by-quarter fixed effect controls for baseline risk-taking behavior in each quarter, separately for treated and control funds, which accounts for managers' response to employment risk that varies across region and time (Low, Meghir, and Pistaferri, 2010; Ellul, Pagano, and Schivardi, 2018).[43] $Treat_{j,c,t}$ equals +1 (−1) if the event state in cohort c strengthened (weakened) enforceability of NCAs,[44] and $Post_{c,t}$ is an indicator for the post-event period.

The results are provided in Table 11. Column 1 shows that the coefficient estimate on $PeerDismissal_{j,c,t} \times Treat_{j,c,t} \times Post_{c,t}$ is negative and statistically significant at the 1% level. This indicates that, upon stronger NCA enforceability, peer-dismissed managers in the treated states reduce risk-taking more than managers in control states. I find a similar result when I add fund and manager characteristics as control variables (column 2). In columns 3 and 4, I substitute group-by-quarter fixed effect with state-by-quarter, which more strictly controls for the time-varying risk-taking behavior across regions. I find similar results under this specification.

Next, I test for the validity of the parallel trend assumption. Specifically, I augment the model (9) with two terms that interact pre-treatment indicators with $PeerDismissal_{j,c,t} \times Treat_{j,c,t}$. The results in columns 5 and 6 support the parallel trend assumption: none of these interaction terms are economically or statistically significant. This outcome suggests that, before the change in NCA enforceability, managers in treated and control states exhibit a similar trend in peer dismissal effect.

Next, I test the parallel trend assumption more vigorously by estimating the following dynamic

[43] Specifically, the studies find that workers' employment insurance changes across time and region, which may yield state-dependent peer dismissal effect. The inclusion of a group-by-quarter fixed effect controls for this heterogeneity. However, I find a qualitatively similar result when I substitute the fixed effect with a simple time-by-cohort fixed effect ($\lambda_{t,c}$). while controlling for time-invariant treated group effect $Treat_{j,c,t}$.

[44] In my main DID analysis, all six treated states experienced strengthening of NCA enforceability.

DID model:

$$\begin{aligned} Risk\ Taking_{i,j,c,t} = & \ \beta\ PeerDismissal_{j,c,t} \\ & + \sum_{\tau \neq -1} \phi_\tau PeerDismissal_{j,c,t} \times Year_\tau \\ & + \sum_{\tau \neq -1} \theta_\tau PeerDismissal_{j,c,t} \times Year_\tau \times Treat_{j,c,t} \\ & + \Gamma X_{i,j,c,t} + \delta_{i,c} + \lambda_{t,s,c} + \epsilon_{i,j,c,t}. \end{aligned} \tag{10}$$

$Year_\tau$ is an indicator variable for each year surrounding the event date of each cohort; τ ranges from -5 to 4, and treatment begins in year $\tau = 0$. The regression coefficient for the triple interaction terms, θ_τ, captures the year-by-year difference in the peer dismissal effect between the treated and control groups. The year before the event year ($\tau = -1$) serves as a reference.

The estimates of θ_τ and their 95% confidence interval are plotted in Figure 3. During the pre-treatment period ($-5 \leq \tau \leq -2$), the coefficient estimates are small in magnitude and statistically insignificant. The estimates drift around zero, displaying no difference in the peer dismissal effect between managers located in treated and control states prior to treatment. Starting from the year of the law change ($\tau = 0$), the estimates become sharply negative and statistically significant over the next five years.[45] These results lend support to my key identifying assumption that, in the absence of treatment, the changes in the peer dismissal effect are similar between the two groups.

4.4 Robustness Tests

In this section, I test for the robustness of my main DID analysis results in several ways. First, I use propensity score matching (PSM) to assign control fund(s) to the treated funds. In addition to mitigating concerns that my main results are influenced by differences in confounding variables, this approach also creates a balanced panel while incorporating treated funds in all 14 states, including the eight states with a small number of funds.

Because the PSM approach is often criticized for its result being sensitive to arbitrary choices, I

[45] In Table A10, I further test for the parallel trend assumption by testing whether the quarterly pre-trend of peer dismissal effects is similar for the treated and control groups. I test for various forms of pre-trend, such as linear, convex, and concave function, but I do not find any evidence that the parallel trend assumption is violated.

verify whether my results are robust to different matching choices. Specifically, I perform one-to-one nearest-distance matching without replacement, as well as $1 : N$ matching with replacement, where $N = 1, 3$, and 5. I also use a sample in which I require funds to exist during at least 80% of the event window, which addresses the potential composition bias. I match based on average values from the prior event of fund characteristics that are associated with fund risk-taking behavior, according to Table 2. These characteristics include the quarterly return and flow of the fund, TNAs, fund age, expense ratio, and turnover ratio. I also require control funds to have the same style as the treated fund, and limit the difference between the predicted logits of the matched funds' propensity scores to be less than 0.10.[46]

I rerun the specification of column 6 in Table 11 using the matched samples. The results are provided in Table A11. In all matched samples, the coefficient estimates of the triple interaction term ($PeerDismissal_{j,c,t} \times Treat_{j,c,t} \times Post_{c,t}$) are negative and statistically significant. The statistical significance is slightly weaker when I require funds to exist during at least 80% of the event window, possibly due to the reduced power from having smaller observations, but the magnitude of estimates is very similar. In addition, in all matched samples, I do not find any evidence of a pre-trend.

Next, I conduct two additional tests that investigate whether my prior results are spurious. First, I run a placebo DID test using index funds around the same event window. Because index fund managers do not have the autonomy to deviate from their fund's benchmark index, I am able to test whether time- or state-specific market conditions spuriously generate my previous findings. The results are provided in Table A12. In all specifications, I do not find evidence of a stronger peer dismissal effect for treated funds following the law-change event in any specification. Using index funds, I rerun the same dynamic estimation as in Equation (10), and I do not find any meaningful differences in the peer dismissal effect following the event date.

Next, I run a second placebo test by shifting the treatment window to placebo periods before and after the actual law change dates. Specifically, for each cohort panel, I shift the treatment date by three, five, and seven years. Table A13 shows no significance difference in the peer dismissal effect for treated funds relative to control funds after the placebo law change date. The fact that only the true treatment window generates significant peer dismissal effects, while the placebo treatment

[46] In all the matched samples, I cannot reject the null that fund characteristics between treated funds and matched control funds are statistically different.

windows do not, confirms that my prior result is not simply driven by a chance.

5 Implications of the Peer Dismissal Effect

5.1 How Do Mutual Funds Reduce Portfolio Risk?

In this section, I provide implications of the peer dismissal effect for mutual fund investors. First, I analyze the mechanism underlying how fund managers reduce their portfolio risks following peer dismissal. Specifically, I examine whether peer-dismissed managers reduce systematic risk, idiosyncratic risk, or both. To investigate this idea, I first examine the change in systematic and idiosyncratic risk as captured by holding-based fund return, as in previous analyses. Specifically, I first decompose fund volatility used in Equation (2) into systematic and idiosyncratic components by running the following regression for each fund i in quarter t:

$$\tilde{r}_{i,\tau,t}^{intended} = \alpha_{i,t} + \sum_{k=1}^{N} \beta_{i,t}^{intended,k} F_{\tau,t}^{k} + \epsilon_{i,\tau,t} \tag{11}$$

where $\tilde{r}_{i,\tau,t}^{intended}$ is the holding-based intended fund return, in excess of daily risk-free rate, on day τ based on fund holdings in quarter t and actual fund returns in quarter t. $F_{\tau,t}^{k}$ is the daily return of each risk factor, which depends on the asset pricing model used. IVOL is computed as the standard deviation of residuals. I run a similar regression using realized fund returns. Lastly, I compute changes in systematic risk k and IVOL as follows:

$$\Delta\widehat{\beta}_{i,t}^{k} = \widehat{\beta}_{i,t}^{intended,k} - \widehat{\beta}_{i,t-1}^{realized,k} \tag{12}$$

$$\Delta IVOL_{i,t} = IVOL_{i,t}^{intended} - IVOL_{i,t-1}^{realized} \tag{13}$$

for each fund i in quarter t. Regarding asset pricing models, I consider CAPM, the Carhart (1997) four-factor model and the Fama and French (2015) five-factor model. Using these risk changes as a dependent variable, I repeat the exercise of examining the impact of peer dismissal, as in Equation (3).

The results are provided in Panel A of Table 12. The first column shows that the change in market beta exhibits a statistically significant reduction following peer dismissal. The reduction in IVOL after peer dismissal exhibits a similar pattern. In the next five columns, I find no evidence that peer-dismissed managers reduce other factor risks, as captured by beta loadings on size, value, momentum, profitability, and investment factors. These results demonstrate that the previously documented peer dismissal effect on total risk is primarily driven by the reduction in market risk and IVOL, not other factor risks.

Next, I examine whether changes in mutual fund holdings exhibit consistent evidence. Specifically, I examine whether peer-dismissed managers increase (decrease) their fund holdings in low- (high-) risk stocks more than managers without dismissal. First, I compute systematic and idiosyncratic risks for each stock-quarter, using daily stock returns during the previous one year. Specifically, I compute total risk as the standard deviation of daily returns, factor risks as regression loading from the Carhart (1997) four-factor model, and IVOL as the standard deviation of residuals from the regression.

Next, for each fund, I sort its holdings in quarter $t-1$ and t into NYSE-threshold quartiles based on their risk as of quarter $t-1$. Using the stock price as of quarter $t-1$,[47] I compute the weight of stocks in each risk quartile group in quarter $t-1$ and t, as well as the changes between the periods. Lastly, I sort funds into those that experienced peer dismissal and those that did not, and compare the difference in changes in weight for each risk quartile groups. I drop all fund-quarters that hold less than 30 stocks between the intervals.

The results are provided in Table 12. Panel B.1 compares the changes in weight of each quartile group sorted by total risk. I find that peer-dismissed funds, on average, increase (decrease) their holdings in the lowest (highest) risk stocks more than funds without peer dismissal by 0.22% (−0.21%). Furthermore, the difference between the two differences (0.22 – (−0.21)%) is also statistically significant at the 1% level. In Panels B.2 and B.3, I find a similar result when I replace total risk with market risk and IVOL, respectively. In Panel C, I repeat the same exercise using risks related to other factors. Consistent with the return-based risk results in Panel A, I do not find evidence that peer-dismissed managers exhibit any differences in trading behaviors for stocks with respect to these factor risks.

[47] Using stock price as of quarter t yields similar results.

The results in Table 12 confirm that my baseline peer dismissal effect—that is, the negative relationship between peer dismissal and the change in total risk—is also supported in the holdings data. Furthermore, these results provide robust evidence that peer-dismissed managers primarily reduce their market risk and IVOL, rather than other systematic factors that fund investors care less about. This result suggests that the peer dismissal effect need not necessarily hurt mutual fund investors by foregoing the risk premium of other factors. Nevertheless, it is also possible that peer-dismissed managers reduce portfolio risk by reducing other exotic risks, which investors perceive as alpha (e.g., Agarwal, Green, and Ren, 2018). I explore this possibility in Section 5.3.

5.2 Peer Dismissal and Agency Prone Managers

In this section, I examine whether the peer dismissal effect alleviates agency problems in the mutual fund industry. The literature typically views the risk-taking behavior of mutual funds as a manifestation of agency problems (Huang, Sialm, and Zhang, 2011; Ma and Tang, 2019), driven by misaligned incentives between fund managers and fund investors. From the perspective of investors, it is in their best interest to maximize the fund's risk-adjusted performance. However, fund managers have a strong incentive to take excessive risks, even at the potential cost of lower performance, due to its option-like payoff. For example, mutual funds on average face a convex FPR; that is, funds with outperformance experience large inflow, but disproportionately low outflow for poor performance(Chevalier and Ellison, 1997; Sirri and Tufano, 1998; Huang, Wei, and Yan, 2007). As fund managers' compensation is tied to their size,[48] mutual funds enjoy large profits when the risk-taking strategy is successful. On the other hand, they face little cost when the strategy fails, in which case the cost is borne by the underlying investors.

As argued by Fama (1980), employment risk is an effective tool for resolving the incentive misalignment problem. In this context, peer dismissal may potentially alleviate the excessive risk-taking behavior of fund managers. Peer dismissal introduces another layer of cost to risk-taking strategy by increasing the perceived likelihood of dismissal when the strategy fails, which can deter excessive risk-taking and reduce potential agency cost. This is also consistent with previous evidence in Panel A of Table 7 that peer dismissal effect is primarily driven by peer-dismissed managers engaging in

[48] Ma, Tang, and Gomez (2019) document that the majority of fund managers' incentives are tied to fund size, either directly through AUM-pay or indirectly through advisor-profit-pay.

less risk-taking behavior, rather than directly shifting down risks.

I explore this mechanism by testing whether the negative relationship between peer dismissal and risk-taking behavior is stronger for a subset of managers that are more prone to agency problems. I consider three measures that proxy for agency problems as suggested in prior literature: funds with a high expense ratio, those with a strong convex FPR, and younger funds. Specifically, Gil-Bazo and Ruiz-Verdú (2009) argue that high-expense funds exploit naïve investors who are less responsive to expenses, which indicates that they are prone to agency problems (Huang, Sialm, and Zhang, 2011). Furthermore, funds with a convex FPR have an incentive to take excessive risks (Sirri and Tufano, 1998), which is another indication of agency problems (Ma and Tang, 2019). Lastly, younger funds have a greater incentive to take risks because they face a more convex FPR, consistent with investors learning about fund skills (Chevalier and Ellison, 1997).

Table 13 investigates whether the peer dismissal effect is stronger for funds that are more prone to agency problems. In the first three columns, the samples are sorted into terciles based on each of the agency problem (AP) measures (*Low*, *Mid*, and *High AP*). I estimate the peer dismissal effect for each terciles group by interacting *Peer Dismissal* with an indicator for each AP terciles group. As shown in the first three columns, I find a significant peer dismissal effect for the *Mid* and *High* AP group, but not for the low AP group. In addition, the magnitude of the peer dismissal effect increases with the intensity of the agency problems.

Lastly, I examine the joint effect of the agency problems based on a composite score, calculated as the sum of the highest AP group indicators (*High AP*) of the three AP measures. In column 4, *Low AP*, *Mid AP*, and *High AP* correspond to composite scores of 0, 1, and +2, respectively.[49] Using the composite score, I find the strongest peer dismissal effect for the highest AP group, and the difference in effect between the high and low AP groups is statistically significant at the 5% level. In summary, the results provide strong evidence that peer dismissal reduces risk-taking the most among fund managers that are most prone to agency problems.

[49] In computing the composite measure, I find that observations with composition score 3 accounts for only 2% of all observations. Therefore, I aggregate scores 2 and 3 as the highest score (2+). Scores 0, 1, and 2+ compose 33%, 45%, and 22%, respectively. Note that using four scores yields a qualitatively similar result.

5.3 Peer Dismissal Effect and Mutual Fund Performance

In this section, I test whether peer dismissal can potentially improve the performance of mutual funds. Ex ante, it is unclear how peer dismissal affects fund performance. Although the results in Section 5.1 suggest that peer-dismissed managers do not reduce well-known factor risks, they may still reduce their portfolio's exposure to other exotic risks, which may be perceived as alpha by fund investors (e.g., Agarwal, Green, and Ren, 2018). If this is the case, peer dismissal may negatively affect fund performance. On the other hand, it is also possible that peer dismissal may improve fund performance. As discussed in Huang, Sialm, and Zhang (2011), ill-motivated risk-taking driven by agency problems are detrimental to fund performance, which may be partially alleviated by peer dismissal. In addition, Cici, Hendriock, and Kempf (2021) find evidence that employment risk induces fund managers to exert additional effort, which leads to positive fund performance. Similarly, peer dismissal may improve fund performance by triggering salient employment risk and induce managers to exert more effort and improve fund performance.

To answer this question, I run the following regression:

$$\Delta\overline{Performance}_{i,t-4:t+3} = \beta Peer\, Dismissal_{j,t-1} + X\Gamma_{i,j,t-1} + \epsilon_{i,j,t}, \tag{14}$$

where the dependent variable is the changes in a mutual fund's average risk-adjusted return from the year before to the year after the current quarter. I include a style-by-quarter fixed effect to control for any time-style-specific factors related to changes in fund performance. As a performance measure, I consider CAPM alpha, DGTW benchmark-adjusted alpha (Daniel and Titman, 1997), and the four-factor alpha of Carhart (1997), all in excess of fund fees. Panel A of Table 14 shows that, following peer dismissal, funds experience performance improvement with respect to all performance measures relative to the previous year.

In Panel B, I test whether the effect of peer dismissal on performance improvement increases with the intensity of the mutual fund's risk-taking incentives. To this end, I interact the peer dismissal indicator with the composite measure of agency problems used in Section 5.2. I find that the effect of peer dismissal is mainly focused on the highest agency problems groups, for which the performance improvement with regard to the four-factor alpha (Carhart, 1997) increases up to 52 basis points. Overall, the results in Table 14 are consistent with peer dismissal improving fund performance by mitigating agency frictions.

5.4 Performance Improvement Following Peer Dismissal through Effort Channel

The performance improvement that is particularly more pronounced for managers with high risk-taking incentives is consistent with peer dismissal reducing ill-motivated risk-taking behavior that is harmful for fund performance. However, this does not preclude the possibility that fund managers may exert extra effort for their fund management, as they become more concerned about their career following peer dismissal.

To explore this possibility, I capture fund managers' increased effort based on their performance of local investment following peer dismissal. Cicero, Puckett, Wang, and Zhang (2021) find that fund managers' visit to local companies via taxi visits are associated with superior investment performance, and conclude that fund managers' investment performance in local firms is due to their efforts to gather information.

Specifically, I collect zipcode of each stock's headquarter from their historical 10-K, 10-Q, and 8-K filings. Next, using the mutual fund holdings data, I compute the distance between the firm's headquarters and the mutual fund advisory firm's office, and divide the stocks into two groups based on whether this distance is less than or greater than 50 miles. I then compute the changes in the mutual fund's average risk-adjusted return from one year before to one year after a peer dismissal, separately for each distance group. I repeat the regression from equation (14) separately for each distance group.

As shown in Table 15, I do not find stronger performance improvement for the local firms. This suggests that the performance improvement following peer dismissal does not seem to be driven by fund managers' additional effort—at least based on the local investment measure. Nevertheless, this is not an exhaustive measure of fund managers' efforts, and it is still possible that part of the performance improvement may be stemming from other form of fund managers' endeavor to increase the fund performance following peer dismissal.

6 Conclusion

Using unique employment record data from the mutual fund industry, I provide novel evidence that career concerns are contagious among mutual fund managers. Specifically, I find robust evidence that, following the dismissal of their peers, fund managers reduce their risk-taking behavior. Consistent with this behavior being driven by the increase in perceived employment risk, the reduction in risk-taking is more pronounced when managers have a social connection with the dismissed peer and when managers are already experiencing heightened employment risk. I further show that such a relationship is unlikely to be driven by other alternative mechanisms, and occurs primarily through salient employment risk by exploiting state-level changes in enforceability of NCAs that introduced exogenous variations in underlying employment risk across managers located in different states.

These results have important implications for mutual fund investors. I show that the relationship between peer dismissal and the subsequent reduction in risk-taking is more pronounced for agency-prone managers. This finding suggests that the salient employment risk from peer dismissal introduces another layer of cost to excessive risk-taking strategies, typically viewed as misaligned incentives between fund managers and underlying investors. I also show that alleviating agency-driven risk-taking through peer dismissal can modestly improve fund performance (Huang, Sialm, and Zhang, 2011). Overall, my study highlights that the strategic turnover decisions of fund employees, and the corresponding spillover effect, can serve as an effective tool to mitigate agency issues in the mutual fund industry.

Confederate Memorials and the Housing Market

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Abstract

We find that Black, Democrat, and college-educated homeowners are less likely to live on Confederate memorial streets. Moreover, houses on Confederate streets sell for 3Confederate effect does not spillover to adjacent houses, consistent with direct name rather than neighborhood effects. The price effect increases following attention-grabbing events that highlight racial underpinnings of Confederate symbols. Removing Confederate school names is associated with price increases for local houses. Aversion to houses on Confederate streets also holds in experimental settings where house attributes are otherwise identical. The findings suggest that social norms can have important consequences for real estate markets.

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1 Introduction

Public debate regarding US Civil War Confederate memorials has intensified in recent years. The discussion centers on whether such memorials reflect underlying racism, particularly against Black Americans, or more innocuous Southern pride. While early Confederate memorials were typically located in cemeteries to honor the dead, more celebratory images such as Confederate generals on horseback began to be placed in public spaces during the Jim Crow era of the early 1900s, with another round of memorials occurring during the Civil Rights era of the 1950s-1960s. While statues in public spaces can carry strong symbolic meaning, Confederate memorials may also have direct economic effects on asset markets. In this article, we study the housing market implications of Confederate memorials by examining houses located on streets that honor the Confederacy.

Survey evidence indicates that attitudes towards Confederate memorials vary substantially with demographic attributes. In particular, Confederate memorials are viewed more negatively by Black Americans, Democrats, and individuals with higher levels of education. Our first analysis examines whether these demographic groups are less likely to own houses on residential streets that contain words that are widely associated with the Confederacy. We collect demographic information for every homeowner in the state of Florida, and we contrast the homeowners of 1,943 properties located on Confederate streets with the homeowners of matched control properties that are in the same census block group as the focal Confederate property. We find that houses on Confederate streets are 31% less likely to be owned by Black residents, 20% less likely to be owned by registered Democrats, and 15% less likely to be owned by individuals with a college education. The demographic evidence is robust to controlling for age, household income, house characteristics, and a propensity score matching approach.

There are several potential explanations for why certain demographic groups are more averse to living on Confederate memorial streets. First, homeowners may be put off by the street name itself, either because they dislike what it symbolizes, or because they are uncomfortable with others' negative views of the name (*street effect*). Moreover, some individuals may be averse to living in close proximity to residents who would choose to live on a Confederate memorial street, in which case they may avoid the entire neighborhood (*neighbor effect*).[1] Alternatively, it is possible that houses on Confederate streets may have unobservable amenities that happen to be valued differently by certain demographic groups (*amenity effect*).[2] For example, Mummolo and Nall (2017) and Martin and Webster (2020) find that Democrats prefer to live in areas that have greater population density.

In order to better understand the underlying mechanisms driving the residential sorting evidence, we also examine homeowner preferences for *Confederate Adjacent* homes, defined as the subset of homes in the same census block group that are in closest proximity to Confederate streets. We find no evidence that Confederate-street-averse demographic groups are less likely to live in *Confederate Adjacent* homes. The difference in homeowner preferences for Confederate and *Confederate Adjacent* homes is consistent with direct aversion to Confederate street names and inconsistent with the neighbor effect. In addition, to the extent that amenities tend to be more similar for very proximate homes, the results are also inconsistent with differences in unobserved amenities driving the results.

The residential sorting evidence suggests that aversion to Confederate memorials is strong enough to influence home purchases, but the findings are silent on the broader pricing implications. For example, if the sorting evidence is attributable to heightened demand from white residents, Republicans, and individuals with lower levels of education, then Confederate house values may not differ or even transact at a premium. On the other hand, if the sorting results primarily reflect reduced demand from populations averse to Confederate memorials, then houses on Confederate streets may trade at significant discounts.

We analyze the pricing implications of Confederate memorial streets by gathering sale prices and property characteristics from 2001-2020 using data collected from local government offices by ATTOM, a private data provider. Our primary sample is comprised of 5,895 home sales located on 1,446 Confederate memorial streets in 35 different states. Our identification approach involves comparing Confederate house sales with nearby non-Confederate houses that sold during the same

[1] Consistent with homophily, McCartney and Shah (2022) show that households are more likely to sell their homes when their neighbors have differing political beliefs, and Bayer et al. (2022) find evidence that Black and white homeowners are significantly more likely to move in response to receiving a neighbor of a different race.

[2] For example, past research explores the housing market implications of school investments, foreclosures and tax lien sales, freeways, and fracking (e.g., Black, 1999; Cellini, Ferreira, and Rothstein, 2010; Anenberg and Kung, 2014; LaPoint and Yale, 2022; Brinkman and Lin, 2022; and Gibbons, Heblich, and Timmins, 2021).

calendar quarter. Within a census-tract quarter, we find that Confederate properties are similar to non-Confederate properties along observable house attributes with the exception that Confederate houses tend to be older than control properties. Accordingly, in our main specification, we compare Confederate and non-Confederate transactions that took place in the same calendar quarter, within the same census tract, and within the same age quintile, while also directly controlling for the number of bedrooms and bathrooms, house age, building size, and lot size.

We find that houses on Confederate streets transact at prices that are 2.93% lower than similar non-Confederate properties. The mean house value during our sample is $240K, which translates into a dollar Confederate discount of roughly $7,000. The effect is robust to a number of alternative specifications, including finer geographic partitions (e.g., replacing census tract fixed effects with block-group fixed effects) or interacting census tract $\times$ quarter fixed effects with other house attributes (e.g., indicators for the number of bedrooms or bathrooms, building size or lot size quintiles, or a propensity score match).

Consistent with the residential sorting analysis, we find a pricing effect for Confederate properties but not for *Confederate Adjacent* properties. This finding provides further support for a direct Confederate *Street Name Effect* rather than a neighborhood effect. In addition, using listing information collected from *Zillow*, we document that Confederate properties experience other undesirable housing outcomes. In particular, Confederate homes are 9% more likely to have a slow sale, defined as being in the largest quintile of sell duration, and they are 10% more likely to be in the top quintile of sale discounts relative to listing price.

If the negative association between Confederate street names and house values is driven by reduced demand, we would expect the relation to be stronger in areas where aversion to Confederate memorials is likely to be stronger. At the county level, we categorize properties into two groups based on racial, political, and educational demographic information. Consistent with the sorting results, we observe that the Confederate street house value discount is more pronounced in regions with a higher proportion of Black residents, Democratic voters, and individuals with higher education levels. We also find some evidence that Confederate street discounts are smaller in the 11 former Confederate states, and we observe a positive (albeit insignificant) pricing effect for Confederate properties in the five former Confederate states with the most Confederate memorial statues.

Although public concern about Confederate memorials has been generally evident throughout our sample period,[3] events that raise awareness of the racial underpinnings of Confederate symbols may amplify the Confederate discounts. We measure variation in attention to Confederate symbols using Google search intensity for the term "Confederate Flag." We observe three noticeable spikes that correspond to the church shooting in Charleston, South Carolina in June 2015; the Unite the Right rally in Charlottesville, Virginia in August 2017; and the widespread Black Lives Matter protests against police brutality and racism that reached a peak in June 2020. Using a staggered difference-in-difference (DID) approach, we analyze the Confederate house street effect in the four quarters before and after the events. We find that Confederate street houses sell at an incremental -4.22% discount in the year following the event and an -8.13% discount in the quarter following the event.

Changing perceptions of Confederate memorials have led to a number of name changes in recent years. While few individual streets have been renamed to date, using data from the Southern Poverty Law Center, we are able to identify 23 elementary, middle, and high schools with names that were related to the Confederacy that subsequently changed names during our sample period and that have relevant house information available. In particular, we gather data from *Zillow* for school assignments for each house in the zip codes of name change schools. Using a staggered difference-in-difference empirical design, we find that houses located in Confederate school districts experience a 5.2% price increase over the following three years after the removal of the Confederate federate school names relative to otherwise similar houses located in the same zip code. While the districts with name changes are not exogenous, and name changes may be more likely to occur where concern about Confederate symbols is high, the evidence supports the view that aversion to Confederate memorials can influence house values.

Although our analysis controls for available house characteristics, concerns may remain that unobservable attributes could influence the results. We therefore also consider an experimental setting that allows us to examine potential homebuyers' choices in an environment where houses are truly identical except for street name. In particular, we conduct an experiment in which 1000 participants are asked to choose between pairs of houses with pictures and street names provided. In

[3] For example, in 2001 the Georgia state legislature acted to remove the Confederate battle emblem from the state flag, after adding the emblem to the flag in 1956.

this setting, we are able to vary house-name assignments across participants to isolate the effect of a Confederate street name on house choice. Consistent with the archival evidence, we find that respondents are significantly less likely to select a home on a Confederate street on average, and the effect is stronger among the participants who are likely to view Confederate memorials more negatively.

Our findings add to the research on the role of race in housing markets. A large literature explores discrimination in past and present-day mortgage markets, with researchers examining the effects of race on mortgage originations, approvals, interest rates, and refinancing.[4] Our work studies the effect of racial signaling in a contemporary context by exploring the housing market implications of Confederate memorials, which many people strongly associate with historical discrimination. The evidence that Black residents are less likely to live on Confederate streets and that Confederate memorial houses sell for less than other nearby properties suggests that symbols of historical discrimination can continue to have important housing market implications. The findings are consistent with recent work documenting other consequences of Confederate memorials for Black residents including worse labor market outcomes (Williams, 2021) and hate crimes (Rahnama, 2023).

Our analysis also contributes to the literature that focuses on how political views influence real estate investment decisions and prices. McCartney, Orellana-Li, and Zhang (2024) find that households are more likely to move when their neighbors are affiliated with the opposite political party. Other work emphasizes partisan views of climate change. For example, Bernstein et al. (2022) find that Democrats are significantly less likely to own houses exposed to sea level rise relative to Republicans. Similarly, Bernstein, Gustafson, and Lewis (2019) and Baldauf, Garlappi, and Yannelis (2020) find that houses exposed to sea level risk sell for significantly larger discounts in areas where homeowners are more likely to believe in climate change. Our evidence that discounts for Confederate streets are concentrated in areas with more left-leaning voters reinforces the views that differences in political ideology can have a sizeable impact on real estate prices.

[4] Past work that examines historical discriminatory housing policies such as racial covenants or biased lending practices includes Sood and Ehrman-Solberg (2023), Aaronson et al. (2021), and Fishback et al. (2022). Research on present-day housing markets includes Munnell et al. (1996), Ghent, Hernandez-Murillo, and Owyang (2014), Ambrose, Conklin, and Lopez (2021), Bhutta and Hizmo (2021), Avenancio-León and Howard (2022), Bartlett et al. (2022), Frame et al. (2021), Giacoletti et al. (2021), Kermani and Wong (2023), McCartney and Shah (2022), Park et al. (2021) and Goldsmith-Pinkham and Shue (2023).

More broadly, our study extends the literature that examines how personal ideologies influence financial decision making in a variety of settings. Hong and Kacperczyk (2009) show that "sin" stocks (e.g., alcohol, tobacco, and gaming companies) have lower relative valuations, consistent with reduced social preferences for these industries, and Hong and Kostovetsky (2012) document that democratic-leaning mutual fund managers are less likely to invest in companies that are deemed socially irresponsible. Homanen (2018) finds reduced deposit growth at banks that financed the Dakota Access Pipeline, specifically in socially conscious counties. Kempf and Tsoutsoura (2021) find evidence that credit analysts' political perceptions influence corporate credit ratings, and Duchin et al. (2023) find evidence that political attitudes influence corporate merger outcomes. Barber, Morse, and Yasuda (2021) estimate that venture capital investors are willing to forgo three percentage points in expected IRR when investing in funds whose objective is to generate positive social and environmental impact. We adopt a similar framework to examine house buyers' views of Confederate memorial street names. Our findings on the effects of Confederate memorials in the context of the housing market support the view that social norms can have important consequences for asset markets.

2 Residential Sorting on Confederate Properties

Views on Confederate memorials vary substantially with demographic attributes. For example, a 2021 PRRI survey of American views towards Confederate Monument Reform reveals striking differences across demographic groups. In particular, 82% of Black Americans support monument reform compared to only 13% who oppose reform, while the corresponding estimates for white Americans is much more split (47% support versus 52% against).[5] Similarly, support for Confederate monument reform is far stronger among Democrats (82% support reform) relative to Republicans (22%), and among college graduates (64%) relative to those who never attended college (44%). In this section, we proxy for views of the Confederacy using demographic information, and we analyze the effects of Confederate memorials on home purchase decisions.

[5]In the interest of brevity, we combine support and lean support into one category and oppose and lean oppose into a second category. More detailed survey results can be found here: https://www.prri.org/research/creating-more-inclusive-public-spaces-structural-racism-confederate-memorials-and-building-for-the-future/.

2.1 Residential Sorting into Confederate Memorial Homes

We collect detailed voter registration data for all residents in Florida from L2 data. L2 provides voter data separately by state, and we focus on Florida for two reasons. First, it contains the largest number of Confederate properties in our sample, and second, Florida is one of the few states that collect self-reported racial information when residents register to vote. In particular, the Florida voter registration form includes the following five categories: American Indian/Alaskan Native, Asian/Pacific Islander, non-Hispanic Black, Hispanic, and non-Hispanic white. In addition to race and ethnicity, the data includes information on house addresses, political affiliation, education level, homeowner age, and income.[6] The data provides a single snapshot of homeownership as of the end of 2020.

We identify addresses that honor the Confederacy (*Confederate streets*) by searching for street names that contain words associated with the Confederacy. Specifically, we consider addresses that contain variants of the word "Confederate," as well as "Dixie" which is the Confederacy's unofficial national anthem and a term commonly used to describe the 11 Southern states that seceded to form the Confederacy. We also consider addresses containing "Jefferson Davis," who was elected President of the Confederate States, "Robert E. Lee," who acted as the commander of the Confederate States Army, or "Thomas 'Stonewall' Jackson," who was another prominent Confederate military leader.[7] We acknowledge that our list of Confederate memorial streets is not exhaustive, for example, we do not track streets named after less well-known confederate military leaders. A meaningful fraction of homeowners must be aware that a street functions as a Confederate memorial for it to influence decision making in an observable way, and we therefore focus on the most salient Confederate names.[8] All other properties are classified as non-Confederate.

Confederate properties and non-Confederate properties may differ on several important dimensions, and these differences could drive any observed residual sorting. To alleviate this concern, in our

[6] Information on race is missing for roughly 5% of the sample, and college information is missing for 22%. To include as many Confederate street properties as possible, we set missing values of all independent variables to zero and include a corresponding missing variable indicator (see, e.g., Himmelberg, Hubbard, and Palia, 1999).

[7] More specifically, we search for street that contains: "confederate," "confed," "dixie," "dixi," "dixies," "dixieln," "dixielane," "dixieway," "robert e lee," "r e lee," "jeff davis," "jefferson davis," "stonewall jackson," and "stonewall jack," irrespective of its letter case.

[8] For example, the large mountainside carving at Stone Mountain Park in Georgia is comprised of Davis, Lee, and Jackson on horseback.

analysis we contrast Confederate properties to non-Confederate properties that are similar with respect to location, and we include controls for other demographic attributes and house characteristics. Specifically, we first match Confederate properties to non-Confederate properties in the same census block group, which corresponds to roughly one-quarter the size of a census tract. The resulting sample includes 1,943 Confederate properties and 111,147 control properties across 248 census block groups.[9] For each property, we collect information on the homeowner's age and income from L2. We also merge the L2 data with assessor data from ATTOM data solutions (ATTOM), which collects housing data from local government recorder and assessor offices. We collect the following property characteristics from ATTOM: the size of the house in square feet (*House Size*), the size of the lot in square feet (*Lot Size*), the number of bedrooms (*Bedrooms*), the number of bathrooms (*Bathrooms*), and the number of years since the house was first built (*Age*). Descriptive statistics for the merged sample are provided in Table IA1 of the internet appendix.

To test whether certain groups are less likely to own Confederate properties, we estimate the following cross-sectional regression:

$$Confederate_i = \alpha + \beta_1 Var + \beta_2 Control_i + FE_i + \epsilon_i. \tag{1}$$

The dependent variable, *Confederate*, is an indicator that is equal to one if the house is on a Confederate street and zero otherwise. *Var* is equal to either: *Race (Black)*, an indicator equal to one if all the owners of the house identify as Black, *Registered Democrat*, an indicator equal to one if the house owners are registered Democrats, and *Education (Some College)*, an indicator equal to one if all the house owners have at least some college education. We also consider a composite measure, *Demographic Score*, which is the mean of *Race, Democrat*, and *Education. Controls* include indicators for the specific number of bedrooms and bathrooms (up to five), and the natural logs of *Lot Size, House Size, Home Age, Owner Age,* and *Household Income.* FE denotes census block group fixed effects. We cluster standard errors at the block-group level.

Specifications 1-3 report estimates prior to including controls. We find that Black residents are 0.53 percentage points less likely to own a property on a Confederate street relative to other houses

[9]Table IA.1 provides additional summary statistics for the L2 sample.

of similar value in the same census block group, which reflects a roughly 31% decline relative to the mean value of *Confederate* (1.72%). In Specifications 2 and 3, we consider *Education (Some College)* and *Registered Democrat* as the primary independent variables. The estimates indicate that both college-educated individuals and Democrats are significantly less likely to own houses on Confederate streets. In Specification 4 we include the three demographic indicator variables together. The estimates on the three variables are all negative and at least marginally significant ($p < 0.10$). Specification 5 also confirms that the composite *Demographic Score* is highly significant.

Specification 6 adds the full set of controls. The estimate on *Demographic Score* falls slightly (from -0.91% to -0.79%) but the estimate remains highly significant ($t = -3.74$). We also note that the controls tend to be statistically insignificant, which is consistent with houses in the same census block group being similar on observable attributes. There are, however, slight differences with respect to lot size and the number of bathrooms. As a robustness check, we also consider a propensity-score matched sample. Specifically, we re-estimate Equation (6) after excluding *Demographic Score*. We define the predicted value from this regression as the propensity score, and we convert the propensity score to percentiles. We then repeat Specification 6 after replacing census block group fixed effects with census block group $\times$ propensity score percentile fixed effects. After including the additional fixed effects, the coefficient on the *Demographic Score* increases in magnitude to -0.87% and is more statistically significant ($t = 4.13$). Moreover, the estimates on all the controls are economically small and statistically insignificant, consistent with the propensity score model matching well on observable characteristics.[10]

Overall, the evidence from Table 16 indicates that demographic groups who tend to view Confederate memorials more negatively are less likely to live on Confederate properties. These findings suggest that disparate views regarding Confederate memorials are strong enough to influence a major financial decision.

[10] We also repeat Specification (7) after replacing *Demographic Score* with each of the three components separately. The estimates on Black, Democrat, and College are: -0.46 ($t = -2.06$), -0.28 ($t = -2.35$), and -0.34 ($t = -3.46$), respectively.

2.2 Residential Sorting into Confederate Memorial Homes – Underlying Mechanisms

There are several underlying explanations for why certain demographic groups may be less likely to live on Confederate streets. First, potential homeowners may have a direct aversion to the street name (*Street Name Effect*). For example, individuals may be averse to memorializing the Confederacy and wish to avoid frequent reminders of that period in history. In addition, homeowners may also be uncomfortable with the prospect of friends' or peers' negative views of the Confederacy and wish to avoid any negative connotations associated with living on a Confederate memorial street.

A second, broader potential explanation is that homeowners may be reluctant to live near anyone who would choose to live on a Confederate memorial property (*Neighbor Effect*).[11] Consistent with homophily, McCartney, Orellana-Li, and Zhang (2024) show that households are more likely to sell their homes when their neighbors have differing political beliefs, and Bayer et al. (2022) find evidence that Black and white homeowners are significantly more likely to move in response to receiving a neighbor of a different race. Third, it is possible that the sorting results are driven by unobserved amenities that are assessed differently by different demographics (*Amenity Effect*). For example, different demographics could assign different values to nearby parks or the ability to walk to restaurants.

Importantly, the *Street*, *Neighbor*, and *Amenity* explanations offer contrasting predictions regarding residential sorting on properties that are *Confederate Adjacent*, which we define as the closest nearby non-Confederate homes. If the *Street Effect* is the sole driver of the sorting results, then we would not expect to observe sorting for *Confederate Adjacent* properties. On the other hand, if the *Neighbor Effect* is an important contributing factor, then we would expect the sorting results to spillover to adjacent properties. In addition, to the extent that more proximate properties have more similar amenities, the *Amenity Effect* may also spillover to adjacent properties.

We examine whether residential sorting extends to adjacent properties by estimating the following

[11] Our *Street Name Effect* and *Neighbor Effect* mechanisms are similar to the "own-lot effect" and "external effect" channels discussed in a land-use regulation context in Turner, Haughwout, and Van Der Klaauw (2014).

regression:

$$Demographic_i = \alpha + \beta_1 Confed_i + \beta_2 ConfedAdj_i + Controls + FE_i + \epsilon_i. \tag{2}$$

The dependent variable, *Demographic*, is either *Race (Black)*, *Registered Democrat, Education (Some College)*, or *Demographic Score*. *Confederate* is defined as in Table 16, and *Confederate Adjacent* is an indicator that is equal to one if the property is located within x miles of the closest Confederate property, where we set x equal to values ranging from 0.05 miles to 0.50 miles.

Based on the average Confederate property lot size, the 0.05 mile cutoff corresponds to houses that are typically either one or two properties over from the closest Confederate property. Thus, this definition of *Confederate Adjacent* maps closely to the *Nearby Neighbor* measure employed by McCartney, Orellana-Li, and Zhang (2024).[12] The controls and fixed effects are the same as in Specification 7 of Table 16.

Panels B-D of Table 17 report the results for *Race, Education*, and *Democrat*, respectively, and Panel A reports the results for the composite measure, *Demographic Score*. Since the results do not differ dramatically across the three individual measures, we focus on *Demographic Score*, which offers the benefits of more precise coefficient estimates. In Specification 1, we define *Confederate Adjacent* properties as those properties located within 0.05 miles of a Confederate property ($n = 1,003$ *Confederate Adjacent* properties). We find that the coefficient on *Confederate Adjacent*, while negative, is economically small (less than one-third of the estimated effect on *Confederate*) and statistically insignificant. Further, the difference between *Confederate* and *Confederate Adjacent* is statistically significant. We find qualitatively similar results if we define *Confederate Adjacent* properties as properties located within 0.10, 0.25, or 0.50 miles of a Confederate property. The significant difference in the estimates between *Confederate* and *Confederate Adjacent* properties is consistent with a *Street Effect* being a significant contributor of the observed residential sorting. In contrast, the insignificant estimates on *Confederate Adjacent* homes are inconsistent with the

[12] While we expect the magnitudes of spillover consequences to be stronger for more adjacent properties, prior works suggest proximity to certain externalities, including recent foreclosures or brownfield sites, can affect the prices of properties located up to 0.25 miles away (Campbell, Giglio, and Pathak, 2011 ; Linn, 2013). Further, because the sample of control properties increases substantially as the distance increases (see Panel C of Table IA.1), imposing less stringent distance requirement may increase the power of the tests.

Neighbor Effect or the *Amenity Effect*.[13]

3 Transaction Data and Descriptive Statistics

The previous section suggests that the differing views on Confederate memorials are strong enough to influence where homeowners choose to live, but the findings do not speak to the broader pricing implications. To explore the potential pricing implications of Confederate streets, we purchase transaction-level data from ATTOM Data solutions (ATTOM). ATTOM collects housing data from local government recorder and assessor offices. The recorder data provides the sale price of the property (*Price*), its address, transaction date, and transaction deed type (e.g., foreclosure sales, or arms-length deals). The assessor data also provides many property-specific attributes.

We limit the sample to transactions for which assessor data contains non-missing information for the following five property characteristics: the size of the house in square feet (*House Size*), the size of the lot in square feet (*Lot Size*), the number of bedrooms (*Bedrooms*), the number of bathrooms (*Bathrooms*), and the number of years since the house was first built (*Age*). Following Graham and Makridis (2023), we restrict the sample to arm's-length, non-foreclosed sales of residential properties with sales prices of at least $10,000. Finally, since ATTOM's coverage prior to 2000 is very sparse, we limit the sample to transactions that occur between 2001-2020.

As in the previous section, we classify a street as "Confederate" if it contains variants of "Confederate," "Dixie," "Jefferson Davis," "Robert E. Lee," or "Thomas 'Stonewall' Jackson." For our pricing analysis, we require that both *Confederate* and control properties to have sold within the same calendar quarter. This allows for better matching but reduces the sample considerably relative to the residential sorting analysis, and as a result we select control properties from the census track rather than census block for our main analysis (we also consider census block for robustness). We construct the sample of census tract-quarter groups with at least one Confederate and one non-Confederate house transaction.

Panel A of Table 18 provides summary statistics. The sample contains 5,895 Confederate property

[13]The benefit of implementing Equation 2 is that it allows for straightforward testing of statistical differences for Confederate and *Confederate Adjacent* homes. For completeness, in Table IA.2 in the Internet Appendix we also consider a specification similar to Equation 1 in which we replace Confederate with *Confederate Adjacent*. Consistent with the evidence in Table 17, we do not observe a significant sorting effect for any of the demographic measures.

transactions for 4,052 unique Confederate properties. The Confederate sample includes 1,446 different streets, 574 census tracts, 254 different counties, and 35 different states. The control sample includes 80,304 transactions across 32,657 streets. Figure 4 displays the distribution of Confederate house transactions across states. Unsurprisingly, the highest concentration of Confederate transactions occurs in the Southeast. However, there are a considerable number of Confederate streets in other parts of the county including left-leaning states in the West and the Northeast (e.g., California and Massachusetts) as well as more conservative areas in the Midwest (e.g., Nebraska and Indiana). We note that some states, such as Texas, do not mandate public disclosure of house transactions (colored gray in the map), and therefore the ATTOM sample contains few observations from these states.[14]

Panel B of Table 18 reports the distribution of the housing attributes. The median house in the sample sells for $180,000, has 3 bedrooms and 2 bathrooms, and is 25 years old. The means of the continuous variables: (*Price, House Size, Lot Size,* and *Age*) are larger than the medians, and we use natural logs going forward to reduce the effects of outliers on the analysis. Panel C reports the correlation matrix for the variables. Intuitively, *Price* is positively correlated with *House Size*, *Lot Size*, *Bedrooms*, and *Bathrooms*, and negatively correlated with *Age*. We also see that *Confederate* exhibits meaningful correlations with several house attributes. In particular, Confederate houses tend to be smaller yet are positioned on larger lots.

We observe a particularly strong negative correlation between *Confederate* and *Age*. To better understand the difference in home age, in Figure IA1 in the Internet Appendix we present evidence regarding when Confederate and control streets were first named, as measured by the oldest house on the street in our sample. We find that 11% of Confederate streets were named prior to 1920, compared to only 4% of control streets. Moreover, a large fraction of Confederate streets were named during the 1940s and 50s, which coincides with the increase in Confederate memorials around the 1954 Supreme Court decision mandating desegregation. The popularity of Confederate Streets exhibits a clear drop beginning in the 1980s, and the decline has accelerated over the past 20 years.

[14]Excluding non-disclosure states from the sample has little effect on the findings.

4 Identification Approach

A challenge to assessing the impact of Confederate street names on house prices is determining the counterfactual price that would have occurred if the property were located on a non-Confederate street. Our approach is to compare Confederate properties to non-Confederate properties that are sold nearby (within the same census tract) and at roughly the same time (within the same calendar quarter) after controlling for differences in observable house characteristics.

Specifically, we estimate the following hedonic regressions:

$$Log(Price)_{i,t} = \alpha + \beta Confederate_{i,t} + \gamma X_{i,t} + FE + \epsilon_{i,t}, \tag{3}$$

where the dependent variable, $Log(Price)_{i,t}$, is the natural log of property i's sales price in quarter t. The independent variable of interest is $Confederate_{i,t}$, which is an indicator equal to one if the house is located on a Confederate street and zero otherwise. $X_{i,t}$ is a vector of house attributes that includes indicators for the specific number of bedrooms and bathrooms (up to five), and the natural logs of *Lot Size*, *House Size*, and *Age*.[15] We consider different sets of fixed effects (FE) in the model specifications. For example, to benchmark Confederate properties to non-Confederate properties that sold in nearby locations at roughly the same time, we include census tract × quarter fixed effects.

An additional concern is that the value of house characteristics might vary across census tract and time. For example, it is possible that older houses may sell at a premium in some areas and a discount in others. We address this concern by partitioning the house attribute into quintiles (relative to other houses that sold in the same census tract and quarter) and including the triple interaction to create census tract × quarter × house attribute quintile fixed effects. The number of control transactions in each census tract × quarter is relatively modest (12 for the median tract-quarter), which limits us to interacting census tract × quarter with one house attribute at a time. In our main analysis we report the results for *Age* since this variable exhibits the strongest correlation with *Confederate* (see Panel C of Table 18). We report the results for other attributes

[15] In untabulated analysis, we find similar results if we replace *Log (Lot Size)*, *Log (House Size)*, and *Log (Age)* with 100 separate indicators each for lot size, house size, and age percentiles.

in robustness analysis.

Our identifying assumption is that after controlling for the observable house attributes and fixed effects, any observable or unobservable characteristics that influence house prices will be similar between Confederate and non-Confederate properties except for street name. Although we cannot examine whether Confederate and non-Confederate properties differ with respect to unobservable variables, we can explore whether the identifying assumption holds for the subset of observable variables. Specifically, we estimate Equation (1) after replacing $Log(Price)_{i,t}$ with a house attribute (e.g., *Lot Size*) and removing that attribute as a control variable. The coefficient on *Confederate* thus captures whether there is a significant difference between Confederate and non-Confederate properties with respect to the attribute after including all the remaining control variables and fixed effects.

Table 19 reports the results of this analysis for each of the five house attributes used as control variables. Columns 1 and 2 report the differences (and t-stats) between Confederate and non-Confederate properties prior to including any fixed effects or controls, which is analogous to the simple correlations reported in Panel C of Table 18. Columns 3 and 4 report the differences after including the full set of controls and tract × quarter fixed effects, and Columns 5 and 6 report the differences after including the full set of controls and tract × quarter × age quintile fixed effects. The first row reports the results for log (*House Size*). We see that prior to including controls, Confederate houses are 7.2% smaller than non-Confederate houses. However, this difference falls to 1.44% after including controls and tract × quarter fixed effects and 0.45% after including controls and tract × quarter × age quintile fixed effects. In other words, although there are differences in house size between Confederate and non-Confederate properties, even if we could not directly control for *House Size*, our remaining controls and fixed effects effectively eliminate these differences. We observe similar patterns for *Log (Lot Size)*, *Bedrooms*, and *Bathrooms*.

Consistent with Panel C of Table 18, the largest difference between Confederate and Non-Confederate properties is age, 56.3% prior to any controls. Although we control for age in all of the analyses, the correlation raises concerns that the value effects of house age may vary by region and/or over time. We observe in Column 5 that including tract × quarter × age quintile fixed effects reduce the difference in age between Confederate and control properties to an economically small and statistically insignificant 1.5%. The evidence in Table 19 supports the view that our regression approach effectively controls for observable value-relevant house characteristics.

5 Confederate Memorial Streets and Housing Outcomes

In this section, we study the implications of Confederate street names for housing outcomes. Section 5.1 presents the baseline pricing results, Section 5.2 presents several robustness checks, Section 5.3 contrasts Confederate versus *Confederate Adjacent* properties, Section 5.4 considers other housing market outcomes including listing time and listing withdrawals, and Sections 5.5 and 5.6 explore cross-sectional and time-series heterogeneity in the pricing of Confederate properties.

5.1 Confederate Streets and House Prices – Baseline Results

In Table 20, we present the results from estimating Equation (3) using different sets of fixed effects. In Specification 1, we include quarter fixed effects. The coefficient on *Confederate* is -4.70%, which is statistically significant based on standard errors clustered at the census tract level.[16] In Specification 2, we control for geographical variation in house prices by adding census tract fixed effects. Specification 3 interacts tract fixed effects with quarter fixed effects, allowing for the geographical variation in prices to vary across time. The inclusion of tract × quarter fixed effects results in a slightly smaller discount relative to Specification 1 (4.21% versus 4.70%). However, the inclusion of the richer set of fixed effects results in a dramatic increase in the R-squared (81.52% versus 42.57%) and reduces the standard error of the estimate by roughly 50%.

Finally, given that differences in age between Confederate and non-Confederate properties potentially remain relevant after including tract × quarter fixed effects (see Columns 3 and 4 of Table 19), in Specification 4 we include tract × quarter × age quintile fixed effects. This specification helps to control for variation in the age discount (or premium) across regions and time. A disadvantage of this specification is that it shrinks the sample size, since 22% of all Confederate transactions have no corresponding control property (i.e., a non-Confederate property that sold in the same census tract, quarter, and age quintile). We find a slightly reduced, but statistically significant estimate of

[16] Clustering by both census tract and quarter results in virtually identical standard errors.

-2.93%.[17] The estimate also remains economically significant. In particular, the estimate translates to a roughly \$7,000 discount for the average house in our sample (2.93% × \$241,911).

We also note that the R-squared from Specification 4 increases to more than 88%. As pointed out by Oster (2019), the sizeable increase in R-squared as we include a richer set of fixed effects, coupled with the coefficient stability for the variable of interest, helps alleviate concerns that unobservable omitted variables drive the estimates. For example, if we conservatively assume that the maximum possible R-squared is 100% and that unobservables are equally important as observables (i.e., $\delta = 1$), a comparison of Specifications 1 and 4 would suggest that the true estimate on *Confederate* is -2.43%, which would still be statistically significant at a 5% under the (conservative) assumption that the standard errors remain unchanged from Specification 4.[18]

5.2 Confederate Streets and House Prices – Robustness

Although our approach carefully controls for house age, concerns may remain that the price effects of other important controls may exhibit significant variation across regions and time. The relatively small sample of transactions within a census tract quarter prevents us from interacting census tract × quarter with all control variables simultaneously. Instead, we interact census tract × quarter with each of the five control variables individually. For the continuous variables (*Age*, *House Size*, and *Lot Size*) we interact census tract × quarter with the quintile ranking of the variable (relative to other houses that sold in the same census tract and quarter), and for *Bed (Bath)*, we interact census tract × quarter with five separate indicators for houses with one, two, three, four, or five or more bedrooms (bathrooms). We also consider census tract × quarter × propensity score quintile fixed effects, where the propensity score for each property i is based on regressing Confederate on the control variables from Table 20 and census tract × quarter fixed effects.

[17] The reduced estimate could stem from either the revised specification or because some Confederate properties are effectively excluded from the analysis due a lack of a non-Confederate control. To explore the relative importance of these two channels, we repeat Specification 3 after excluding the 22% of Confederate transactions that are effectively excluded in Specification 4. We find that the revised estimate (-3.68%) falls roughly midway between the estimates in Specifications 3 and 4, suggesting that both factors contribute to the decline.

[18] Specifically, following Oster (2019), we estimate: $B^* = B\tilde{-}\delta[(B(0) - B\sim) \times (R_{max} - R\sim)/R(0)]$, where B^* is the true (unobservable) estimate; $B\sim$ is an estimate with full set of observable controls; $B(0)$ is estimate with smaller set of observable controls; R_{max} is the maximum possible R-squared; $R\sim$ is R-squared with full set of controls; $R(0)$ = R-squared with smaller set of observables, and δ is the importance of unobservables relative to observables. The estimates for the full set of observable controls (i.e., $B\sim$ and $R\sim$) are taken from Specification 4 of Table 20, and the estimates from the model with the smaller set of controls (i.e., $B(0)$ and $R(0)$) are taken from Specification 1 of Table 20.

We show the *Confederate* coefficient estimates and the 95% confidence intervals for each model in Figure 5. For reference, the first column reports the estimates for census tract × quarter × age fixed effects and is therefore identical to the baseline results reported in Specification 4 of Table 20. We find that the estimates on *Confederate* when controlling for the other four attributes or the propensity score are similar and range from -2.71% (*House Size*) to -3.17% (*Bathrooms*).

A second concern is that heterogeneity in house quality within the census tract (roughly half as large as a zip code) is driving our findings. To alleviate this concern, we more finely partition regions to the census block group, which is typically about one-quarter the size of the census tract. Because census blocks are smaller than census tracts, 22% of Confederate transactions do not have a corresponding non-Confederate property that sells in the same quarter. Despite the smaller sample size, we continue to find significant negative estimates, as shown in Figure 5.

Finally, we explore whether there are any clear time-series trends in the Confederate effect during the sample period. Figure 6 plots the rolling five-year estimates on *Confederate*. We find no evidence of a general time trend in the Confederate discount.[19] However, the effect does appear to be stronger during the financial crisis period when housing markets were very illiquid. Section IA2 and Table IA3 of the Internet Appendix provide additional evidence that the Confederate street effect is more pronounced during illiquid real estate markets, which is consistent with theoretical models that predict that the impact of heterogeneous preferences on prices should be larger when markets are more illiquid (Piazzesi, Schneider, and Stroebel, 2020).

5.3 Confederate Streets and House Prices – Street, Neighbor, or Amenity Effects

The evidence from Section 2.2 suggests that the residential sorting results are primarily driven by direct aversion to the street name itself (*Street Name Effect*), rather than other effects that may spillover to adjacent streets, such as aversion to living in close proximity to people who chose to live on a Confederate street (*Neighbor Effect*), or unobservable amenities (*Amenity Effect*) that are

[19] The lack of a trend is perhaps surprising given that several prominent racial events occurred during the sample period that served to increase awareness of the racial underpinnings of Confederate symbol. We explore shocks to the saliency of Confederate Symbols in greater detail in Section 5.6 We find quick and sizeable increases in Confederate discounts following these events, yet the price effects are relatively short-lived, which helps explain the lack of a general time trend.

likely to be very similar for adjacent properties (e.g., proximity to parks or restaurants). Thus, the residential sorting evidence suggests that the pricing discounts associated with Confederate streets should generally not extend to *Confederate Adjacent* properties.

To evaluate this prediction, we repeat the analysis after augmenting Specification 4 of Table 20 with an indicator for properties that are adjacent to Confederate streets (Confederate Adjacent). As in Section 2.2, we define a property as *Confederate Adjacent* if the property is located within x miles of the closest Confederate property (within the same census tract and sold in the same calendar quarter), where we set x equal to values of 0.05, 0.10, 0.25, or 0.50 miles. Table 21 tabulates the estimates for each of the four *Confederate Adjacent* measures. Across the four specifications, we observe that the estimates on *Confederate Adjacent* are always statistically insignificant, ranging from -1.62% to 2.66%. Further, the difference between Confederate and *Confederate Adjacent* is statistically significant in three of the four specifications.

In Panel B, we match the residential sorting (L2) sample by focusing on transactions that took place in Florida. The discount on Confederate properties is slightly larger than the full-sample estimates, although the estimate is less precisely estimated. In sharp contrast to the negative (and typically marginally significant) estimate on *Confederate*, the estimates on *Confederate Adjacent* are always positive and statistically insignificant, and the difference between *Confederate* and *Confederate Adjacent* is at least marginally significant (p ¡ 0.10) in all four specifications. Together, the findings in Table 21 echo the residential sorting results in Table 17, and they suggest that direct aversion to Confederate memorial streets contributes to the price discount observed for Confederate properties.

5.4 Confederate Streets and other Housing Market Outcomes

The price discounts documented for Confederate properties are consistent with some homeowners being averse to purchasing houses on Confederate streets. A related prediction is that Confederate houses, due to the weaker aggregate demand, may be more likely to have their listing withdrawn, may take a longer time to be sold, or may be more likely to sell at large discounts relative to their initial listing price. To test these predictions, we supplement the ATTOM transaction data with hand-collected information on listing dates, listing prices, and house withdrawals from *Zillow*. We search the *Zillow* website using the property addresses from ATTOM. We verify whether the searched outcome in *Zillow* is indeed the same property as the one reported in ATTOM by comparing their house characteristics. The listing and price history are very scarce prior to 2008, so we focus on the period from 2009 to 2020. Even for the later sample period, *Zillow* only provides information on listing dates and listing prices for a subset of properties. We are able to collect listing information for 2,334 of the 4,052 Confederate properties. Our final sample includes 2,619 listings of Confederate properties and 17,744 non-Confederate properties that were listed in the same census tract and quarter.

We construct three variables from the *Zillow* data: *Withdrawn* is an indicator equal to one if the house listing is subsequently withdrawn without selling; *Slow Sale* is an indicator equal to one if the difference between the selling date (or withdrawal date) and the listing date is in the top quintile; and *Large Discount* is an indicator equal to one if *log (Listing Price /End Price)* is in the top quintile of the distribution, where end price is defined as either the sales prices or the listing price on the date the property *i*s withdrawn. We also continue to include the other house attribute data reported in ATTOM.

Table IA4 of the Internet Appendix provides summary statistics (similar to Table 18) for the merged *Zillow*-ATTOM sample. We find that the median *End Price* and *Listing Price* are \$199,900 and \$189,900, which is similar to the median sale prices reported for the full sample in Table 18 (\$180,000). The average value of *Withdrawn* is 8.42%; the average values of *Slow Sale* and *Large Discount* are approximately 20% by construction; *Slow Sale* corresponds to properties that do not sell within (roughly) six months of the listing date, and *Discount* corresponds to discounts of 10% or larger.

To explore whether Confederate properties are more likely to experience bad market outcomes, we estimate the following regression:

$$Y_{i,t} = \alpha + \beta Confederate_{i,t} + \gamma X_{i,t} + FE + \epsilon_{i,t}, \tag{4}$$

where Y is either equal to *Withdrawn*, *Slow Sale*, or *Large Discount*. *Confederate* and X are defined as in Equation (3), and FE denotes census tract × listing quarter fixed effects.[20]

[20]Due to the more limited sample of properties with listing data (roughly half the size of the sale sample), we do not include census tract × listing quarter × age quintile fixed effects. As a middle ground, in Table IA.5 we repeat

Specifications 1-3 report the results for *Withdrawn*, *Slow Sale*, and *Large Discount*, respectively. We find the Confederate properties are 1.11 percentage points more likely to be withdrawn, 1.72 percentage points more likely to have a slow sale, and 2.01 percentage points more likely to sell at a large discount. Relative to the sample means of each variable, these estimates reflect percentage increases of 13.2%, 8.6%, and 10.1%, respectively, although the estimate for *Withdrawn* is not reliably different from zero. Collectively, the findings from Table 22 provide further support for the view that *Confederate* properties suffer from lower aggregate demand.

5.5 Confederate Streets and House Prices – The Role of Homeowner Demographics

We expect that the negative relation between Confederate street names and house values will be larger in areas where aversion to Confederate memorials is likely to be stronger. Motivated by survey evidence and the evidence of residual sorting in Section 2.1, we expect that the Confederate discount will be larger in areas with a greater fraction of Black residents, and in areas where the population is more highly educated and more politically liberal.

We collect ethnicity and education level information from the U.S. Census Bureau. We compute the percentage of Black residents (college graduates) out of the total population of adults for each county-year.[21] We partition the sample into groups based on whether the demographic variable for the county is above or below the median for a given quarter. The median sorts generate sizeable variation in our demographic variables of interest. For example, the fraction of Black residents in counties with above versus below median Black population is 27% versus 6%.[22] We collect information on Political affiliation from County Presidential Election Returns, provided by MIT Election Data and Science Lab.[23] The dataset provides county-level number of voters for the Democrat and Republican Party presidential candidates as well as the total number of voters for

the analysis using census tract × listing quarter fixed effects and census tract × age quintile fixed effects and find qualitatively similar results.

[21] The Bureau provides actual number of residents by each ethnicity in 2000 and 2010, while providing the estimates during the other periods. We rely on the ethnicity estimates provided up to 2019. At the time of writing, the Bureau has released the estimates for 2020 but not the actual value.

[22] The corresponding difference for Democrats is 58% versus 36%, and the difference for college educated is 34% versus 20%.

[23] https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/VOQCHQ

the five past presidential elections (2000-2016). For each county-year, we compute the percentage of Democratic party voters.

Specifications 1 through 3 of Table 23 report the results. For all three partitioning variables, the estimate on *Confederate* High is statistically significant. The estimates on Confederate Low are always smaller than the estimates on *Confederate High* and generally not statistically significant. For example, Specification 1 indicates that the Confederate discount is a significant -3.64% in counties with above median Black population compared to a statistically insignificant -1.98% in counties with below median Black population. We note, however, that the difference between the two estimates is not reliably different from zero. We observe similar patterns when we partition on the fraction of the country that voted Democrat (Specification 2) or the fraction of the country that is college educated (Specification 3).

Views on Confederate memorials also vary by region. For example, the 2021 PRRI survey of American views towards Confederate Monument Reform finds that support for monument reform is weaker in Southern States (41%) compared to states outside of the South (56%). We consider two proxies for the South. The first is the group of eleven states that belonged to the Confederacy (*Confederate State*).[24] We also zoom in on the five states that have the largest number of Confederate statues and monuments: Georgia, Virginia, North Carolina, Texas, and Alabama (*Top 5 Confederate State*).[25] Specification 4 indicates that the Confederate discount is a significant -4.18% in non-Confederate states compared to a statistically insignificant -1.93% in Confederate states, and Specification 5 shows that the differences are amplified for Top-5 statue states (-4.33 versus 0.64%).

We gauge the joint predictability of the individual measures by constructing a composite measure which is the sum of four indicator variables: *Non-Top 5 Confederate State* + *Black* + *Democrat* + *College*.[26] We partition *Confederate* into *Confederate High Composite*, *Confederate Mid Composite*, and *Confederate Low Composite*, where *High Composite* (*Low Composite*) equals one if the composite score is above (below) the median value of 2, and *Mid Composite* equals one if the composite score is equal to the median value. Specification 6 reports the results. We find that the estimate

[24] The specific states are Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia.

[25] https://www.splcenter.org/20190201/whose-heritage-public-symbols-confederacy

[26] We do not include Non-Confederate State since it is strongly related to Non-Top5 State. Adding Non-Confederate State to the composite measure yields qualitatively similar results.

on *Confederate Low* is a statistically insignificant 1.58%, while the estimate on *Confederate High* is a highly significant -5.52%. Further, the difference between the high and low composite groups is economically large (-7.11%) and statistically significant.

5.6 Confederate House Prices: Shocks to the Saliency of Confederate Symbols

We hypothesize that the Confederate street effect is likely to be stronger after major events that result in increased attention to the racial underpinnings of Confederate symbols. Following Da, Engelberg, and Gao (2011), we measure attention to the Confederacy using Google search frequency for the term "Confederate Flag". Figure 7 plots the time-series variation in monthly search frequency from January 2004 through December 2020, where the values represent search intensity relative to the maximum value. The figure indicates that the distribution of search intensity is highly skewed. For example, the 75th percentile is 4 and the 95th percentile is 7, out of maximum search score of 100.

The figure highlights three noticeable spikes in attention to the Confederate Flag. The first, and most dramatic (search score of 100), occurs in June of 2015, which corresponds to a mass shooting in Charleston, South Carolina. The shooter who killed nine Black parishioners at a Bible study had previously posted photos on his website with emblems associated with White supremacy and the Confederate flag. The shooting generated significant debate on the modern display of the flag and other commemorations of the Confederacy, and afterwards the South Carolina General Assembly voted to remove the Confederate flag from State Capitol grounds.

The second spike (search score of 16) occurs in August 2017. This follows the "Unite the Right" rally in Charlottesville, Virginia. The organizers' stated goals included the unification of the American White national movement and opposing the proposed removal of the Robert E. Lee statue. During the protest, a self-identified White supremacist intentionally drove his car into a group of counter-protesters, resulting in one death and numerous injuries. The third spike (search score of 44) occurs in June of 2020 and corresponds to the Black Lives Matter (BLM) protests over police brutality and racial injustice. Survey evidence suggests that between 15 and 26 million people participated in BLM demonstrations over the deaths of George Floyd and others, making the protests the largest movement in America's history.[27] Following the protests, Mississippi lawmakers voted to change the state flag that contained the Confederate battle emblem.

To explore the impact of these salient racial events, we consider a staggered difference-in-difference design. We limit the sample to the [-12, +12] month window, where period 0 is the month of the event. We then estimate Equation (1) after interacting *Confederate* with *Post*, an indicator that is equal to one for the post-event window (i.e., months 1 through 12) and zero for the pre-event window. Specification 1 of Table 24 reports the results. We find the coefficient on *Confederate* $\times$ *Post* is -4.92%, indicating that the discount for *Confederate* properties is 4.92% larger in the year following the salient racial events relative to the year prior to the event. To better understand the dynamics following the major events, in Specification 2 we decompose *Confederate* $\times$ *Post* into *Confederate* $\times$ *PostQ1*, *Confederate* $\times$ *PostQ2*, *Confederate* $\times$ *PostQ3*, and *Confederate* $\times$ *PostQ4*, where *Confederate* $\times$ *PostQ1* is an indicator equal to one if the transaction occurred in the quarter (i.e., three-months) following the event, and *PostQ2*–*PostQ4* are defined analogously. Specification 2 reports the results. We find that the discount increases substantially in the quarter following the event (-8.13%). However, the estimates for quarters two through four are insignificant, suggesting that the impact of increased attention is relatively short lived.[28] Specification 3 augments Specification 2 by including *Confederate* $\times$ *PreQ1*, where *PreQ1* is an indicator equal to one in the three months before the post-event window. We find that the coefficient on *Confederate* $\times$ *PreQ1* is statistically insignificant, which is inconsistent with pre-trends driving the increased discount in the period immediately following the event.

Finally, in Specification 4 we explore whether the sizeable discount in the quarter following the attention-grabbing events is concentrated around a particular event. Since the effects are concentrated in the quarter after the event, we shrink the event window to [-12,3] and then decompose Confederate $\times$ PostQ1 into three separate indicators for each event (*Charleston*, *Charlottesville*, and *BLM Protests*). The estimated discounts following all three events are sizeable, ranging from 7% (*BLM Protests*) to 11% (*Charlottesville*), although none of the individual estimates are signifi-

[27] https://www.nytimes.com/interactive/2020/07/03/us/george-floyd-protests-crowd-size.html

[28] All three events occur in the summer (two in June and one in August), raising the concern that our findings might be driven by seasonality in the Confederate discount. In Figure IA2, we explore whether the Confederate discount varies by calendar month. We find the estimates are stable, which alleviates the concern that seasonality drives the large discounts in the quarter immediately following the attention-grabbing events.

cantly different from zero. The patterns are consistent with all three events having similar effects on homeowner demand for Confederate properties.

6 Confederate Memorial Name Changes and House Prices

Changing perceptions of Confederate memorials have led to a number of name changes in recent years. In this section, we examine the impact of Confederate school name changes on house prices. We focus on school name changes rather than street name changes because street name changes are rare to date and school name changes potentially affect a much larger pool of potential home-buyers.[29]

The Southern Poverty Law Center (SPLC) maintains a list of Confederate memorials throughout the United States and tracks removals and name changes. From the SPLC dataset, we gather information on elementary, middle, and high schools whose names were related to the Confederacy (as defined in our street analysis) and subsequently changed names during our 2000 to 2020 sample period. We initially identify 42 name change schools in 38 distinct zip codes.[30]

We then extract from ATTOM the addresses of single-family houses that are located in one of the 38 zip codes. We further collect school district information, historical house sales prices, and house attributes of these properties from *Zillow*.[31] *Zillow* does not provide price information for properties in four of the zip codes, all of which are located in Mississippi, a state without mandatory transaction information disclosure. We also exclude properties located in zip codes whose school districts are unidentified according to *Zillow*. Lastly, we drop one zip code with no property price information available prior to the change period. We focus on the three-year window surrounding the school name change year, and we limit the sample to zip code-quarters with property sales in both name-change and no-name-change school districts.

[29]For school name change, there are 11,298 (10,631) transactions of changers (non-changers) during the three-year window of the school name change year, when we condition on zip code-quarters with property sales in both name-change and no-name-change school districts. For street name change, there are 150 (1,025) transactions of changers (non-changers) during the three-year window of street name changer year, located in 14 streets, based on the same conditions applied for school name change.

[30]We exclude three schools that were closed instead of changing their name, since changes in educational opportunities may have confounding effects on home values.

[31]The ATTOM dataset does not contain information on school assignments, and we also rely on *Zillow* for pricing data as well since the ATTOM sample ends in 2020 (when many of the name changes took place).

The final sample includes 21,929 transactions that are located in 23 zip codes. We identify 5 school name changes in 2016, 4 in 2017, 4 in 2018, 1 in 2019, and 9 in 2020. Roughly half (51%) of the transactions consist of houses that experienced a school name change; in addition, 27% of transactions occur after the name change, 54% occur prior to the name change, and 19% occur in the same year as the name change. In our baseline difference-in-difference analysis (Specification 1), we exclude transactions that occur in the event year; however, we include these observations in the event-time test (Specification 2). We also note that because many name changes occurred later in the sample, the post-sample period is often truncated.

To examine the impact of school name changes on home prices, we estimate a staggered difference-in-differences (DID) regression in which we compare transactions for houses in name-change school districts with transactions in the no-change districts within the same location-quarter, before and after the school name change occurs. Specifically, we estimate the following regression:

$$log(Price)_{i,t} = \beta_1 NameChg_i + \beta_2 NameChg_i \times Post_{i,t} + \gamma X_{i,t} + FE + \epsilon_{i,t}, \tag{5}$$

where $NameChg_i$ equals one if property i is located in a school district that changed its name and $Post_{i,t}$ equals one if property i is sold after the school's name change year, and zero if property i is sold prior to the name change year. For properties in no-change school districts, $Post_{i,t}$ is assigned based on timing of the name change in the same zip code. X includes controls for the specific number of bedrooms and bathrooms (up to five), and the natural log of *House Size*, and *Age*.[32]

Ideally, we would include census tract × quarter fixed effect as with our earlier analysis. However, school districts rarely cross census tract boundaries, and including these precise fixed effects reduces the sample of relevant transactions by over 80%.[33] Therefore, we relax this condition by including zip code × quarter fixed effect, along with census-block fixed effects. Our identification assumption is that, in the absence of school name change, properties located in changer and non-changer school districts would have experienced comparable price changes. It is important to acknowledge that the decision to change a school name is endogenous. For example, school name changes may be more

[32]The controls are identical to the baseline analysis except that we no longer include the natural log of *Lot Size* since this variable is generally not available from *Zillow*.

[33]Including tract × quarter fixed effects instead of zip code × quarter fixed effects yields qualitatively similar but statistically insignificant results.

likely to occur in more liberal or highly educated areas, in which case the estimates we observe for school names changes may not generalize to other areas.[34] Nevertheless, a positive significant coefficient on β_2 would indicate that, at least in certain areas, aversion to Confederate memorials is large enough to impact real estate prices.

Specifications 1 of Table 25 reports the difference-in-difference results. We find that the estimate on $NameChg_i Post_{i,t}$ is positive and significant. The estimate implies that houses in a school district appreciate by 5.21% when the school removes its Confederate name. In order to further explore the time-series dynamics surrounding school name changes, we re-estimate equation (5) after dropping $NameChg_i Post_{i,t}$ and instead including $NameChg_i \times Year(-2)$, $NameChg_i \times Year(-1)$, $NameChg_i \times Year(0)$, $NameChg_i \times Year(+1)$, and $NameChg_i \times Year(> +1)$, where $Year(-2)$ is an indicator equal to one if the transaction occurred two years prior to the name change, and the other variables are defined analogously.[35]

The results are reported in Specifications 2 of Table 25. We find that the coefficient on $NameChg_i \times Year(-2)$ and $NameChg_i \times Year(-1)$ are economically small and statistically insignificant, which suggests that our findings are unlikely to be attributable to pre-trends. The estimates on $NameChg_i \times Year(0)$ and $NameChg_i \times Year(+1)$ are both positive and marginally significant ($p < 0.10$), consistent with name changes having an immediate impact on prices, and the estimate on $NameChg_i \times Year(> +1)$ is even larger in magnitude, which suggests that the immediate price reaction does not reverse over the subsequent two years. In sum, the findings from Table 25 suggest that homes that are zoned to attend Confederate schools experience a significant price appreciation when the Confederate name is removed.

[34] From EducationWeek, we collect the list of schools whose names are related to Confederacy, but did not change its name prior to year 2022 (https://www.edweek.org/leadership/data-the-schools-named-after-confederate-figures/2020/06). We compare the county-level demographics of these schools to those of name-change schools used in our analysis. On average, we find that the fraction of college-educated (Democrat-voting) residents in name-change schools' counties are 18.5% (20.8%) higher compared to those in no-name-change counties; the mean-difference tests are statistically significant at 1% (10%) level. On the other hand, the two groups have similar Black populations.

[35] We group years +2 and +3 together because, due to the considerable number of name changes occurring in 2020, there are relatively few observations in each category.

7 Confederate Memorial Streets and House Preferences – Experimental Evidence

Although our analysis includes controls for several house features, concerns may remain that unobservable characteristics could be driving the results. In this section, we consider an experimental setting that allows us to examine potential homebuyers' choices in an environment where houses are truly identical except for street name. An additional advantage of the experimental design is that we can directly measure decision makers' views on Confederate memorials and examine whether these views influence house choices. To improve the transparency and consistency of our experimental evidence, we pre-registered the experiment with the Open Science Framework. The pre-registration document, which pre-specifies the hypothesis, the design, the sample size, and the proposed statistical analysis, is available here: https://osf.io/8jubg/.[36] We also summarize the experimental design in the next section.

7.1 Experiment Overview

The experiment is designed to place potential homebuyers in a situation where they are choosing between two similar houses. Specifically, each respondent was asked to imagine that they are moving to a new town and are looking for a home. They were presented with 10 pairwise comparisons of houses and informed that each of the hypothetical houses is in the same neighborhood, was built around the same time, and is similar in size (same number of bedrooms and bathrooms). For each pair of houses that they were presented, they were expected to choose where they would prefer to live. Respondents were obtained using Prolific, and we stipulated that they be US citizens, residing in the US, between the ages of 25 and 70, and with self-reported income of greater than $30,000.

The experiment is designed to place potential homebuyers in a situation where they are choosing between two similar houses. Specifically, each respondent was asked to imagine that they are moving to a new town and are looking for a home. They were presented with 10 pairwise comparisons of houses and informed that each of the hypothetical houses is in the same neighborhood, was built

[36]he preregistration is hosted by OSF (https://osf.io/). However, the document posted to OSF includes information on the authors' names. For the purposes of the review process, an anonymous version of the preregistration can be found here https://www.dropbox.com/s/q72im9j5arhkoh3/Confed-SurveyPrereg-Blind.pdf?dl=0.

around the same time, and is similar in size (same number of bedrooms and bathrooms). For each pair of houses that they were presented, they were expected to choose where they would prefer to live. Respondents were obtained using Prolific, and we stipulated that they be US citizens, residing in the US, between the ages of 25 and 70, and with self-reported income of greater than $30,000.

Respondents were presented with three photos for each house (front, kitchen, and bathroom), along with the property street name. The overall experiment consisted of five unique houses and five unique street names (including one Confederate memorial name: *Dixie St.*). We chose the five sets of house pictures with the goal that they would be viewed as similar in desirability.[37] The experiment design comprised twenty blocks of 10 pairwise comparisons (200 unique pairwise comparisons in total) so that each combination of name and house is considered in both left and right positions. Each participant was randomly assigned to one of the twenty blocks and was asked to choose between 10 pairwise comparisons (houses-name matches were internally consistent for each participant). In order to reduce the risk of response bias, respondents were not informed about the nature of the study, and only four of the ten comparisons include a Confederate street. An example of the survey is presented in Section IA.5 of the Internet Appendix.

Motivated by our analysis in Section Table 19 that studies saliency shocks, we also included a priming component in the experiment design. Before beginning the survey, participants were randomly assigned to one of two conditions: race issue priming vs. not primed. In particular, half of respondents were asked to read and summarize an article that underscores the racial underpinnings of Confederate symbols, and the other half were asked to read and summarize a control article without racial or Confederate references.

After completing the house choice portion of the survey, participants were asked to enter demographic information including their political preferences, their level of education, and their ethnicity, and their current state of residence.[38] We next directly ask participants "How would you feel about living on a street that honors the Confederacy?" using a 1 to 5 scale, where 1 denotes *Extremely*

[37]Photos were chosen from online searches for three bedroom, two bath houses located in Southern states and originally built in the late 1980s to early 1990s (matching the age of the average Confederate Street home). Control street names were chosen from the list of control street names from the archival evidence.

[38]We gathered information on the current state of residence, age, and gender to help provide diagnostic evidence on the representativeness of the sample. Based on comments we received when presenting our findings, we now also treat residing in a non-Confederate state, which we consider in our archival analysis, as a mediating variable in our experimental setting. However, we did not preregister this particular mediating variable (Non-Confederate State) in our report, so this specific evidence should be interpreted as "exploratory" or "non-preregistered."

Negative, 3 is *Neutral*, and 5 is *Extremely Positive*. This question is asked after the house comparisons, without the opportunity to go back, to ensure it does not influence their answers. We collected survey data for 1000 participants, resulting in 10,000 house choices with 4,000 involving a Confederate street.

Respondents were not tasked with directly choosing preferred address names, and we anticipate that for most respondents the photos are likely to be the first order determinant for house choice. However, we conjecture that concerns about Confederate symbols on average, and in particular by certain demographic groups, will influence house choice. Our primary hypothesis is that participants will be less likely to choose houses that are located on streets that honor the Confederacy (*H1*). We also expect that the relation will be stronger for participants who express greater direct negative reactions to Confederate memorials (*H2*). Indirectly, we expect the relation to be stronger for respondents with demographic traits that are typically associated with more negative views of the Confederacy including (e.g., Black participants, participants with higher education levels, participants who identify as Democrats, and those living outside the South). Our final prediction is that the negative reaction to a Confederate memorial street name will be stronger for participants that have been primed to consider the racial underpinnings of Confederate symbols (*H3*). Hypotheses *H1*, *H2*, and *H3* are designed to mirror the archival results reported in Section 5.1 (baseline results), Section 5.5 (demographic results), and Section 5.6 (shocks to saliency), respectively.

7.2 Experimental Summary Statistics

Panel A of Table 26 reports summary statistics on the demographics of the survey users. We find that more than 64% of users report having a negative view of Confederate streets (i.e., a score of 1 or 2), compared to less than 4% of users who report a positive view (i.e., a score of 4 and 5). 54% of the survey respondents identify as Democrats, 67% are college educated, 68% live in a non-Confederate state, while just 6.8% identify as Black.[39] We also report the means of each variable for different subsamples. For example, column 2 reports the means of all the demographic variables for the subsample of users who view Confederate memorials negatively. A comparison of columns 1 and 2 indicates that aversion to confederate memorials is, as expected, stronger among Democrats,

[39] The sample of respondents was 54% female (45% male, 1% non-binary/prefer not to say), and 70% of the respondents were between 25 and 45 years old.

college educated respondents, Black respondents, respondents living in a non-Confederate state, and respondents who received the priming article.

Panel B reports summary statistics on the frequency with which respondents selected certain houses, which we label *House 1* – *House 5*, where house numbers were defined based on their ex-post popularity. We observe some variation, with the most popular house (i.e., *House 1*) being selected 23.1% of the time and the least popular house (*House 5*) being selected 16.9% of the time. Importantly, the street names are randomly assigned to each house with equal likelihood, so differences in the quality of the house should not bias the estimates on house preferences.

Panel C reports summary statistics on the frequency with which respondents selected specific street names. All users saw each street name in four out of the 10 pictures, so the average street should be selected 20% of the time. We find that *Dixie Street* is only selected 18.9% of the time, or 5.5% less than expected, which is consistent with *H1*. In contrast, the other four control street names are selected between 20.0% and 20.7%. Consistent with *H2* and *H3*, the fraction of respondents selecting *Dixie Street* is lower among respondents who view Confederate memorials negatively (18.2%) and among respondents who received the priming article (18.7%).

7.3 Experimental Regression Results

To more formally test *H1*, we estimate the following linear probability model:

$$House\#1 = \beta_1 Dixie\,Dif + House\#FE_1 + House\#FE_2 + \epsilon_{i,t}, \tag{6}$$

where $House\#1$ is an indicator equal to one if the participant reports preferring the first house (i.e., the house on the left of the screen) to the second house (i.e., the house on the right of the screen), *Dixie Dif* equals one if the first house (i.e., the house on the left) is on *Dixie Street*, negative one if the 2nd house (i.e., the house on the right) is on *Dixie Street*, and zero if neither house is on *Dixie Street*, $House\#FE_1$ are a set of indicator dummies to indicate which house was the first (left) house seen by participants, and $House\#FE_2$ is defined analogously. Standard errors are clustered by participant.

Specification 1 of Table 27 reports the results. The estimate on *Dixie Dif* is -2.65%, indicating that participants are 2.65 percentage points (or 5.3%) less likely to select a house if it is located on *Dixie Street*. The finding supports our prediction that participants will, on average, be less likely to choose houses that are located on streets that honor the Confederacy.

To test *H2* and *H3*, we estimate the following linear probability model:

$$\begin{aligned} House\#1 &= \beta_1 Dixie\,Dif \times High\,Aversion + \beta_2 Dixie\,Dif \times Low\,Aversion \\ &\quad + \beta_3 High\,Aversion + House\#FE_1 + House\#FE_2 + \epsilon_{i,t}. \end{aligned} \tag{7}$$

In testing *H2*, we consider one direct proxy for *High Aversion* (*Negative Confederate Sentiment*) and four indirect proxies (*Democratic*, *College Educated*, *Black*, and *Non-Confederate State*). In testing *H3*, our measure of *High Aversion* is whether the respondent was asked to read the priming article (*Priming Article*). For each variable, the *Low Aversion* group includes all participants not classified as *High Aversion*.

Specification 2 of Table 27 reports the results where *High Aversion* is defined as *Negative Confederate Sentiment*. The estimates indicate that users who view Confederate memorials more negatively are 4.67 percentage points (9.3%) less likely to select houses on *Dixie Street*. This estimate is significantly different from zero at a 1% level and significantly different from the estimate on the *Low Aversion* group. Specifications 3-6 report the results for the indirect proxies for viewing Confederate memorials negatively. The evidence for the indirect proxies is generally consistent with our predictions. The one exception is for Black respondents; however, this test has low power since the sample of Black respondents is small (N = 68). Finally, in Specification 7 we find that respondents who received the priming article are less likely to select *Dixie Street* than those who did not (-3.03% versus -2.26%), however the two estimates are not reliably different from each other.

Overall, the experimental evidence in this section is highly consistent with our empirical evidence. In particular, the evidence from both approaches suggests that, on average, homebuyers are averse to purchasing homes on Confederate memorial streets, particularly among the subset of users who are more likely to view Confederate memorials negatively.

8 Conclusion

This paper studies the housing market implications for homes located on streets that honor the Confederacy. We find that houses on Confederate streets are 33% less likely to be owned by Black residents, 20% less likely to be owned by registered Democrats, and 17% less likely to be owned by individuals with a college education. Consistent with the sorting results, we find that properties located on Confederate streets sell at a 2.9% discount relative to otherwise similar nearby properties. Both the sorting and pricing results do not spillover to adjacent properties, suggesting that our findings are primarily attributable to a direct aversion to Confederate street names.

Several auxiliary tests suggest that Confederate houses' negative market outcomes are a result of reduced aggregate demand from certain homebuyers who wish not to glorify a part of America's history that is associated with White supremacy. First, the discount for Confederate properties is concentrated in areas where a high population of residents view Confederate memorials as symbols of racism, including areas with a larger Black population, more liberal voters, and states outside of the Southeast. Second, the discounts are larger after salient events that intensify the negative connotations associated with Confederate memorials. Third, the removal of Confederate school names is associated with significant house price increases. Fourth, the aversion to Confederate streets continues to hold in an experimental setting, with the effects being particularly strong among survey participants who are more likely to view Confederate memorials negatively.

The findings echo recent literature on the importance of social norms and preferences in determining asset values. Our evidence also speaks to public debate regarding the appropriateness of Confederate memorials. In particular, our analysis offers a market-based approach for uncovering the marginal homebuyer's preferences for Confederate memorials. Our findings inform considerations to rename Confederate streets. Although to date only a handful of Confederate streets have been renamed, existing discussions often emphasize the costs involved.[40] Our findings highlight potential benefits to homeowners by expanding the set of potential homebuyers.

[40] See, for example, and https://www.nbcmiami.com/news/local/how-much-would-it-cost-to-rename-dixie-highway/2195214/

Risk, Return, and Environmental and Social Ratings

Sudheer Chava, Jeong Ho (John) Kim, and Jaemin Lee*

Abstract

We analyze the risk and return characteristics across firms sorted by their environmental and social (ES) ratings. We document that ES ratings have no significant relationship with average stock returns or unconditional market risk. Stocks of firms with higher ES ratings *do* have significantly lower systematic downside risk, as measured by downside beta, relative downside beta, coskewness, and tail risk beta. However, the economic magnitude of such reduction in downside risk is small. Our results suggest that investors who derive non-pecuniary benefits from ES investing need *not* sacrifice financial performance.

Keywords: Downside Risk, ESG, Sustainable Investing.
JEL classification: G12, G32, M14.

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1 Introduction

Global assets that are managed using investment approaches that consider environmental, social, and governance (ESG) factors in portfolio selection have grown from USD 23 trillion in 2016 to USD 35 trillion in 2020 (see 2020 Global Sustainable Investment Review). ESG funds have also attracted record inflows during the recent COVID-19 pandemic.[1] While these trends accentuate the growing popularity of sustainable investing with investors, they also raise concerns over its financial implications.

Indeed, such trends triggered Trump administration's amendment to the Employee Retirement Income Security Act of 1974 (ERISA) requiring plan fiduciaries to select investments based solely on investment risk and return, implicitly suggesting that integrating ESG factors is costly to investors.[2] This amendment was amended yet again by the Biden administration to explicitly permit plan fiduciaries to consider ESG factors when financially material, suggesting that integrating ESG factors may be beneficial to investors. [3] Importantly, this policy uncertainty manifests the significant lack of consensus over the financial cost, or the benefit, of incorporating ESG factors into investment decision. We contribute to this ongoing debate on the relationship between risk, return, and ES ratings, with a novel focus on systematic downside risk.

On one hand, ESG investing can be costly if investors who derive non-pecuniary benefits drive equilibrium asset prices (e.g., Hong and Kacperczyk, 2009; Pástor, Stambaugh, and Taylor, 2021; and Pedersen, Fitzgibbons, and Pomorski, 2021). On the other hand, a key premise of ESG investing is that firms "do well by doing good," or high-ESG stocks exhibit stronger financial performance. More specifically, as Bénabou and Tirole (2010) hypothesize, corporate social responsibility (CSR) can lower firm systematic risk. Albuquerque et al. (2019) formalize and test this hypothesis by modeling CSR as an investment to increase product differentiation that allows high-CSR firms to face relatively less price-elastic demands, resulting in a lower elasticity of profits to aggregate shocks and lower systematic risk for such firms. We hypothesize that this benefit is particularly pronounced during market declines (see Lins et al. (2017) and Albuquerque et al. (2020)), or that

[1] https://tinyurl.com/y253312c
[2] https://tinyurl.com/y6rzae67. Also see Florida's proposals https://tinyurl.com/4m6desfm
[3] https://tinyurl.com/2t6byr8w

high-CSR firms have lower *downside* systematic risk.

In this paper, we provide the first evidence regarding the implications of a firm's ES ratings on its future exposure to systematic downside risk, as well as reexamining their implications for the firm's future stock returns and its future unconditional systematic risk. We find no meaningful relationship between the realized stock returns and ES ratings of a firm.[4] High-ES stock returns over longer horizons will be higher insofar as lower downside risk of high-ES firms mitigates large losses, which have a disproportionate impact on compound returns. We suggest that such reduction in downside risk translates to a future increase in compound annual returns close to 1% across firms.

Lins et al. (2017) find that firms with high ES scores had significantly higher stock returns during the 2008–2009 financial crisis, while Albuquerque et al. (2020) report a similar finding during the COVID-19 market crash. Of course, these periods are canonical examples of a declining market, i.e., precisely when high-ES firms would do well according to our ES investing proposition. Quintessentially, our measures of downside risk capture the typical benefit of ES policies during market declines that are practically more useful for portfolio selection. We discuss the relation of our paper to these papers in great detail later in Section 3.4.2.

While our focus on the implications of ES investing on systematic downside risk is novel, others have investigated its implications on unconditional risk exposures or firm-specific downside risk.[5] Hoepner et al. (2024) find that successful ESG engagements by a large institutional investor shorten the negative tail of return distributions for targeted firms over time, whereas we find that publicly accessible ES information from rating agencies can be used to identify firms with lower systematic downside risk in the cross-section. Ilhan et al. (2021) show that, using options data, firms with more carbon emissions exhibit higher tail risk, while Bolton and Kacperczyk (2021) find that stocks of such firms earn higher returns. Our results complement their evidence by showing that firms with better ES profiles, as well as those with lower climate change exposures, have lower systematic downside risk.

[4]While we focus on realized returns, realized returns are not the same as expected returns. See Chava (2014) and Pástor et al. (2022) for the relationship between a firm's environmental profile and expected returns.

[5]Earlier references include Godfrey et al. (2009); Oikonomou et al. (2012); Jo and Na (2012); Kim et al. (2014); and Krüger (2015). In Appendix A, we provide a summary of types of risks and returns (surrounding negative events) proposed to relate to ESG/CSR in the academic literature on ESG/CSR in asset pricing, as well as situating our paper in this literature.

We begin our analysis by examining patterns of future returns over the next month and unconditional market risk over the next 12 months for portfolios sorted on past ES scores from 1991 through 2016. We use ES ratings from MSCI KLD, a major ESG ratings provider. In line with the mixed results in the ES investment and performance literature,[6] we find no evidence that high-ES stocks outperform low-ES stocks. We do find that stocks of high-ES firms have lower market betas than those of low-ES firms. However, we show that this relation is explained away by the strong autocorrelation of market betas.

We then analyze patterns of future downside risk for portfolios sorted on past ES scores. Our primary measure of downside risk is the relative downside beta of Ang et al. (2006): downside market beta over periods when the excess market return is below its mean, controlling for the regular market beta. We find that firms with high ES scores have significantly lower future downside risk. Moreover, these relations continue to hold when we control for other firm-level characteristics (e.g., lagged downside risk, firm size). Our results remain similar when we consider two alternative proxies for downside risk: the coskewness of Harvey and Siddique (2000) and the tail risk beta of Kelly and Jiang (2014). We also find that both environmental (E) and social (S) components are equally important for predicting future downside risk. In addition, we provide suggestive evidence that lower climate change exposure also delivers lower downside risk.

Our estimates indicate that an interdecile-range increase in ES score is associated with small decreases in downside risk: The magnitude of these decreases represent about 3% of the interdecile range of the downside risk measures. However, the estimated coefficients of downside risk on ES performance, which capture only the average effect, might understate the economic significance of their relationship if the ES–downside risk link covaries negatively with the market. We confirm that this is indeed the case. A natural way to capture the joint effect is by looking at returns over the next 12 months, i.e., the contemporaneous period over which downside risk is measured.[7] Our

[6] For example, Hong and Kacperczyk (2009) find that “sin” firms in the alcohol, tobacco, and gaming industries earn significantly higher alphas than comparable firms in other industries. In contrast, Kempf and Osthoff (2007) find that stocks with high ES ratings have significantly higher alphas than stocks with low ES ratings, while Edmans (2011) demonstrates that the firms listed in the “100 Best Companies to Work For in America” earn significant positive alphas.

[7] First, returns over longer horizons capture the average effect: Even if a firm’s ES performance is financially immaterial based on returns over a short horizon and unconditional risk, high-ES stock returns over longer horizons will be higher insofar as lower downside risk of high-ES firms mitigates large losses, which have a disproportionate impact on compound returns. Second, returns over a 12-month horizon account for the effect of whether the year is itself a bad year.

results indicate that an interdecile-range increase in ES score leads to an increase in compound annual returns close to 1%.

Our results accentuate that integrating ES factors is *not* costly, suggesting that the rapid growth of ES investing is not puzzling. ES investors do not experience significantly lower returns nor are they exposed to higher risk; in fact, such investments provide small insurance-like benefits against market declines. At the same time, ES investors can enjoy non-pecuniary benefits.[8]

Finally, we provide evidence supporting two potential mechanisms behind the downside risk effects of firm-level ES performance. Using the firm-level news sentiment from RavenPack News Analytics as a proxy for the change in firm value, we test whether the value of high-ES firms is resilient in periods when many firms suffer a negative shock to their value. We *do* find that firm values for high-ES firms covary less with the average firm's value, especially when the average firm's value is declining. To the extent that (i) media coverage is influenced by the ES profile of the firm and (ii) returns in turn vary with media coverage, ES performance can impact the downside risk of the firm.

In addition, we examine whether the ES preferences of institutional investors can induce a pattern of institutional trading that is consistent with the negative relation between ES performance and downside risk. Using institutional trading data from Abel Noser, we find that when the market suffers extremely negative shocks, institutional investors hold on to high-ES firms which can give rise to the low downside risk of these firms. During normal times, however, institutional investors buy high-ES firms such that, unconditionally, they do not exert additional price pressure on these stocks. This is also consistent with the insignificant relation between ES ratings and unconditional market risk.

Eccles and Stroehle (2018) and Eccles et al. (2020) point out that there are data construction and integrity issues with MSCI KLD since 2013. Using ES ratings from MSCI KLD before 2013 only, we continue to find similar results, albeit slightly stronger. It is also possible that our proxy for ES may not accurately measure a firm's ES activities as ESG ratings of leading agencies disagree substantially (Berg et al., 2022). In Appendix C, we find similar, albeit weaker, results using

[8] See, e.g., Riedl and Smeets (2017); Hartzmark and Sussman (2019); Barber et al. (2021); and Hong and Shore (2023).

Sustainalytics ratings, another major ESG ratings provider.

Taken together, our results highlight that reduction in downside risk is a key pecuniary benefit of incorporating ES factors into investment decisions. Prior literature on the ES–financial performance link is mixed. If anything, investing in ES funds typically imposes large costs on mean-variance investors.[9] Moving beyond mean-variance analysis, we provide strong evidence that not only is integrating ES factors *not* costly, but also it helps long-term investors mitigate downside risk. However, this insurance-like benefit against market declines alone is not large. Hence, our evidence suggests that the recent growth of ES investing is driven by the non-pecuniary motives, coupled with the small insurance-like financial benefit.[10]

2 Data

Our analysis uses data from four major databases: (i) the MSCI KLD database on the ESG profile of companies, (ii) the CRSP database on stock returns, (iii) the RavenPack database on news sentiment, and (iv) the Abel Noser database on institutional trading. We also use COMPUSTAT to construct book-to-market ratios, accounting variables (return on equity (ROE), asset growth, and sales growth), and book leverage, as well as a dummy for dividend-paying firms. In this section, we describe the first two data sources in detail, and we outline the construction of the main variables used in our empirical analysis of the relationship between ES performance and downside risk. The remaining data sources are described later in Sections 4.1 and 4.2 when they are first used. The summary statistics are presented in Panel A of Table 28. For convenience, we include a complete list of variable definitions in Appendix B.

2.1 MSCI KLD Database

The data source for the firm-level ESG profile is MSCI ESG KLD Stats. This database contains annual information on the environmental, social, and governance performance of large publicly traded companies. MSCI KLD is one of the most widely used databases for ESG research by

[9] See Geczy et al. (2021).

[10] See, among others, Riedl and Smeets (2017); Hartzmark and Sussman (2019); Barber et al. (2021); and Hong and Shore (2023).

institutional investors and academics.[11]

The KLD database expanded its coverage over time, starting with S&P 500 companies during 1991–2000 then expanding to include Russell 3000 companies since 2003. The sample period is 1991–2016.[12] MSCI KLD classifies ESG performance into 13 granular categories: *environment*, *community*, *human rights*, *employee relations*, *diversity*, *product*, *alcohol*, *firearms*, *gambling*, *military*, *nuclear power*, *tobacco*, and *corporate governance*. Similar to Lins et al. (2017), we focus on the first six of these categories. We do not use the categories that penalize involvement in the six industries that reflect the inherent business of the firms. We do not use the corporate governance category in our main analysis because governance is generally outside the scope of CSR, but we consider this category in the robustness tests.

For each of the six categories we consider, MSCI KLD compiles information on both strengths and concerns. As we are interested in capturing both elements, we construct a net ES measure that adds strengths and subtracts concerns. For any given category, the maximum number of strengths and concerns varies over time; accordingly, we follow Lins et al. (2017) and scale the strengths (concerns) in each category by dividing the number of strengths (concerns) for each firm-year by the maximum number of strengths (concerns) in that category in that year. Note that these strength and concern indices range from 0 to 1 for each category-year. Our measure of net ES involvement in each category-year therefore ranges from -1 to $+1$.

Finally, we construct the total net ES measure of a firm by summing the measures of its net ES involvement across the six categories of environment, community, human rights, employee relations, diversity, and product. This measure ranges from -6 to $+6$, and it is our primary proxy for ES performance.[13] There is considerable dispersion in ES performance across firms within the same industry: The R-squared from a Fama-MacBeth regression of ES scores on industry fixed effects is less than 0.20. In this paper, we focus on the pecuniary implications of this within-industry

[11] Recent papers that have used this database include Hong and Kostovetsky (2012); Chava (2014); Krüger (2015); Borisov et al. (2016); and Lins et al. (2017).

[12] Note that the focus of this paper is on the predictive role of past ES performance, so annual information on the firm-level ESG profile spanning the years from 1991 to 2016 will be used to predict measures of risk and return over the period 1992—2017.

[13] Note that our measure of ES performance is linear. In unreported results, we use dummy variables for ES performance quartiles. The latter specification may be more appropriate if there are nonlinearities in the relation between ES performance and risk. Indeed, we find that the impact of ES performance on risk is not entirely linear, but more importantly it is monotonic and of comparable magnitude. The results are also very similar when we include dummy variables for other ES performance percentiles.

variation in ES performance.

2.2 CRSP Database

Stock return and market capitalization are constructed using the CRSP database. We confine our attention to NYSE/AMEX/Nasdaq stocks with share codes 10 and 11. We use daily and monthly returns from CRSP for the period covering January 1992 to December 2019. As usual, we use the one-month Treasury bill rate as the risk-free return rate, and we take the value-weighted return of all stocks from CRSP as the market return.

Our primary measure of downside risk is the relative downside beta (denoted by $\beta^- - \beta$), which is the downside beta of Bawa and Lindenberg (1977) (denoted by β^-) relative to the regular beta with respect to the market portfolio (denoted by β). We consider two alternative proxies for downside risk: the coskewness of Harvey and Siddique (2000) and the tail risk beta of Kelly and Jiang (2014). These two proxies also capture some aspects of downside covariation. We employ several proxies to measure a firm's downside risk because it is not clear a priori which measure is more appropriate for capturing the dimension of downside risk that may be related to the ES profile of a firm.

2.2.1 Downside Beta and Coskewness

We compute downside beta and coskewness in the same way as Ang et al. (2006). For each month t, we use daily returns over the 12-month period, from t to $t + 11$.[14] Let $\widetilde{r}_{i\tau}$ denote asset i's excess return on day τ, and let $\widetilde{r}_{m\tau}$ denote the market's excess return on day τ. We exclude stocks that have more than five missing observations from our analysis. First, we demean returns within each period, and we denote the demeaned excess return of asset i and the demeaned market excess return by $\widetilde{r}_{i\tau}$ and by $\widetilde{r}_{m\tau}$, respectively. We obtain estimates of the regular market β, denoted by $\widehat{\beta}_{it}$, in the usual manner:

$$\widehat{\beta}_{it} = \frac{\sum \widetilde{r}_{i\tau}\widetilde{r}_{m\tau}}{\sum \widetilde{r}^2_{m\tau}}. \tag{1}$$

[14]The reader might be concerned about the non-synchronicity of daily returns. First, we identified subsets of stocks in which there likely is a lot of non-synchronicity using the average daily dollar trading volume, following Welch (2021) We find very similar results when we drop stocks with trading volumes below the NYSE breakpoints, ranging from 10% to 50%. The results are also very similar when we recompute the betas, with one lead and one lag on the market rate of return included (in addition to the contemporaneous market rate of return) in the spirit of Dimson (1979) All of these results are available upon request.

We estimate the downside beta by conditioning the observations for which the realized excess market return is below its sample mean, $\widehat{\mu}_{mt} = \sum r_{m\tau}/T_t$, where T_t is the number of trading days over the 12-month period beginning in month t.[15] We denote the demeaned excess return of asset i and the demeaned market excess return conditional on the market excess return being below the sample mean by $\widetilde{r}_{i\tau}^{-}$ and $\widetilde{r}_{m\tau}^{-}$, respectively. We then calculate $\widehat{\beta}^{-}$ as

$$\widehat{\beta}_{it}^{-} = \frac{\sum_{\{r_{m\tau}<\widehat{\mu}_{mt}\}} \widetilde{r}_{i\tau}^{-}\widetilde{r}_{m\tau}^{-}}{\sum_{\{r_{m\tau}<\widehat{\mu}_{mt}\}} \widetilde{r}_{m\tau}^{-2}}. \tag{2}$$

Finally, coskewness is estimated as

$$\widehat{\text{coskew}}_{it} = \frac{\frac{1}{T_t}\sum \widetilde{r}_{i\tau}\widetilde{r}_{m\tau}^{2}}{\sqrt{\frac{1}{T_t}\sum \widetilde{r}_{i\tau}^{2}}\left(\frac{1}{T_t}\sum \widetilde{r}_{m\tau}^{2}\right)}. \tag{3}$$

2.2.2 Tail Risk Beta

Kelly and Jiang (2014) assume that extreme return events obey a power law, in which case the common time-varying component of return tails, λ_t, can be estimated for each month as

$$\lambda_t^{Hill} = \frac{1}{K_t}\sum_{k=1}^{K_t} \ln \frac{R_{k,t}}{u_t}, \tag{4}$$

where $R_{k,t}$ is the kth daily return that falls below an extreme value threshold u_t during month t, and K_t is the total number of such exceedances within month t. We follow Kelly and Jiang (2014) and define u_t as the fifth percentile of the cross-section each period.

We estimate the tail risk β, denoted by $\widehat{\beta}_{it}^{\text{tail}}$, as the regression coefficient of firm returns on the common tail risk component λ_t using 60 months of data following portfolio formation. To calculate tail risk betas, we require that firms have nonmissing return data for at least 36 months out of the total 60 months. Since computing tail risk betas requires a long time series of returns, analysis of tail risk as the dependent variable uses data ending in 2014 rather than 2017, as in the rest of the

[15] Instead of focusing on the observations for which the excess market return is below its sample mean, another way to estimate the downside beta is by focusing on the observations for which the excess market return is negative. Using this alternative condition cannot have a material impact on the estimates of downside beta: over a typical 12-month period, the excess market return is below its sample mean for 122 trading days, of which only 8 exhibit positive excess market returns.

analysis. Intuitively, stocks with high values of tail risk beta are more sensitive to tail risk, so they are deeply discounted when tail risk is high.

2.3 Our Main Sample

Panel C of Table 28 shows the number of stocks listed on NYSE, AMEX, and Nasdaq with non-missing ESG data (in the prior year) within each size decile (based on NYSE breakpoints). Note that the MSCI KLD coverage of small firms (i.e., firms with market value below the median NYSE market equity at the beginning of the year) is saliently sparse before 2004. This pattern is consistent with the fact that the KLD database only covered S&P 500 companies until 2000. More importantly, we risk averaging risk–CSR relationships from cross-sections of stocks that are quite different over time. For this reason, we use only big firms (i.e., firms with market value above the median NYSE market equity at the beginning of the year) in our main analyses. A sensible alternative approach would be to use all firms in the period after 2001 as the sample, since this is when KLD started expanding its coverage to include smaller companies. Accordingly, we examine this sample in our robustness tests.

3 Empirical Results

3.1 Unconditional Risk and Returns of ES Score-Sorted Portfolios

It would seem that a natural starting place for any assessment of costs, or benefits, of incorporating ES factors into investment decisions is to consider traditional mean-variance investors. In this section, we begin by examining patterns of future returns over the next month and unconditional market risk for portfolios sorted on their past ES score.

3.1.1 Returns of Portfolios Sorted by ES Score

At the beginning of each month t, we sort stocks into five quintiles based on their past ES scores. In particular, since our total net ES measure is annual, we sort stocks into portfolios at the beginning of each year based on ES measures from the prior year. We then examine monthly holding period returns from t to $t+1$.

Panel A of Table 29 reports the average returns of the equal- and value-weighted portfolios over the next month from t to $t+1$, along with the return difference between the highest and the lowest past ES quintile portfolios in the column labeled "High-Low," for which we compute the t-statistic by using three Newey–West (1987) lags.

The average returns of the various ES portfolios are similar, and they do not exhibit any obvious pattern. Firms in the highest ES-score quintile earn virtually the same equal-weighted average annual returns as firms in the lowest quintile, with a t-statistic of 0.04. The value-weighted high-minus-low ES-score portfolio average return is -5 bp per month ($t = -0.37$). The average returns of the long-short portfolios are not only statistically but also economically insignificant. Similarly, portfolio alphas do not demonstrate any pattern. The alphas of the value-weighted high-minus-low ES-score portfolio are negative but small, and they are statistically insignificant for each of these models. On an equal-weighted basis, the high-minus-low ES-score portfolio alphas are typically positive but insignificant.

Panel B of Table 29 repeats the same exercise as Panel A of Table 29, except it sorts firms on their ES scores within each industry, based on two-digit Standard Industrial Classification (SIC) codes. Again, none of the return spreads, which are economically small, are statistically significant, with t-statistics between -0.8 and 0.8.

Essentially, we find no evidence of high-ES firms outperforming, or underperforming, low-ES firms. These results suggest that (abnormal) returns cannot explain the preference for (or against) ES investing.[16]

3.1.2 Unconditional Risk of Portfolios Sorted by ES Score

In each panel of Table 29, the last row shows the average cross-sectional realized β of each quintile portfolio, where a stock's β is calculated using daily data over the next 12 months. Although these average betas are computed using multiple months of data, they are evaluated monthly. While this use of overlapping information is more efficient, it induces moving average effects. To adjust for

[16] We checked that these findings are unaffected by using the Fama-French 5-factor model, as well as by adding the Pastor-Stambaugh liquidity factor to our performance models.

this, we use 12 Newey–West (1987) lags in reporting t-statistics of differences in average market betas between the highest and lowest ES quintile portfolios.

The average betas for firms sorted on ES score alone (Panel A) do not demonstrate any pattern, but they do show a consistently decreasing pattern when we sort on ES score within each industry (Panel B). In this case, the difference in average market betas between quintile portfolios 5 and 1 is -0.038, which is statistically significant at the 1% level.

In summary, Table 29 demonstrates that ES scores do not have implications for return, but they seem to have implications for unconditional market risk: firms with high ES scores have low market betas in the future. These results are consistent with the model in Albuquerque et al. (2019), which predicts that CSR decreases *systematic* risk, as well as their empirical evidence. However, this relation does not control for other firm characteristics that might be correlated with future betas. In Section 3.3.1, we show that this relation is indeed explained away by other firm characteristics.

3.2 Downside Risk of Portfolios Sorted by ES Score

Economists have long recognized that investors care differently about downside losses than about upside gains, which begs a natural extension of the traditional mean-variance analysis by taking into account the asymmetric treatment of risk. According to this extension, systematic downside risk, rather than unconditional market risk, more closely corresponds to how investors actually perceive risk. We now examine patterns of future downside risk for portfolios sorted on past ES score.

Panel A of Table 30 lists the equal-weighted average downside risk characteristics of firms sorted on their ES scores into quintiles. Specifically, at the beginning of each calendar year, we sort firms into portfolios based on ES measures from the prior year. For each month, using daily data over the next 12 months, we calculate a firm's downside beta (Equation (2)) and coskewness (Equation (3)), as well as the firm's relative downside beta. We also compute a firm's tail risk beta using the next 60 months of data. Although these risk measures are computed using multiple months of data, they are evaluated monthly. To account for this, we use 12 Newey–West (1987) lags in reporting t-statistics of the differences in average realized downside risk between the highest and lowest ES quintile portfolios, except we use 60 Newey–West lags in the case of tail risk.

Panel A shows a consistently decreasing pattern between past ES scores and realized downside risk, based on relative downside beta and coskewness. The difference in average relative downside beta is -0.047, with a corresponding difference in average coskewness 0.019. These differences are significant at the 1% level. That is, when the market declines, the prices of high-ES stocks tend to decrease less than those of low-ES stocks *with comparable market risk exposure.* Moreover, high-ES firms with high coskewness tend to do better than low-ES firms with low coskewness when market volatility is high. These are also typically—though not always—periods of low market returns. Taken together, our results are consistent with high-ES firms' low downside risk.

In Panel B, we examine the robustness of ES score's implications for downside risk to controlling for industry by sorting stocks within each industry into quintiles according to their ES scores. Industry can be an important driver of the results in Panel A of Table 30 (and Table 29) for several reasons. First, some industries are considered more controversial than others.[17] Second, Fama and French (1997) show that market risk exposure varies substantially across industries.

Controlling for industry, high-ES firms continue to have low relative downside betas and high coskewness, and spreads in these measures of downside risk are still highly significant, with t-statistics of -2.6 and 3.1, respectively. Nevertheless, these differences are about half the magnitude of the corresponding differences in Panel A. This indicates that industry plays an important role in the negative relations between ES score and downside risk, even though it does not fully explain away the relation.

On the other hand, past ES score does not seem to predict future tail risk well. Panel A shows that tail risk betas across the ES quintiles do not demonstrate any pattern. Panel B shows that high-ES firms exhibit lower tail risk than low-ES firms within the same industry, but the corresponding spread in tail risk beta is still insignificant. However, in Section 3.3.2, we show that, controlling for other firm characteristics, past ES score does negatively predict future tail risk, consistent with high-ES firms' low downside risk.

Finally, Table 30 shows that, while realized downside betas for portfolios sorted by ES score alone do

[17] For example, KLD classifies participation in the production of alcohol, gambling, firearms, military, nuclear, and tobacco as "sinful."

not demonstrate any pattern, the 5–1 difference in downside betas for ES portfolios controlling for industry is negative and highly significant, with a t-statistic of -4.2. This result can be consistent with high-ES firms' low downside risk, but it can also be mechanically reflecting the relation between ES scores and future unconditional betas. Panel B of Table 28 shows that β and β^- are highly correlated, with a correlation around 0.83. Given this correlation, it is not surprising that patterns of β and β^- sorted on ES score are qualitatively the same. Therefore, we must be cautious to control for the effect of unconditional risk when measuring downside risk by focusing on relative downside beta, $(\beta^- - \beta)$, in lieu of downside beta, β^-.

In summary, Table 30 demonstrates that ES scores do have significant implications for downside risk based on relative downside risk and coskewness. Firms with high ES scores have low future downside risk that is not mechanically driven by their low unconditional market risk. These novel results suggest that, to investors who care more about downside losses than upside gains, the low downside risk of high-ES firms can be one pecuniary benefit of incorporating ES factors into their investment decisions. However, these relations do not control for various other firm characteristics that are related to future downside risk (e.g., past downside risk) or contemporaneously correlated with ES scores (e.g., firm size).

3.3 ES Score as a Predictor of Future Systematic Risk Exposure

There is little theoretical guidance regarding which firm characteristics determine the riskiness of a stock, but a number of studies have empirically explored how a stock's risk exposure is related to its firm characteristics.[18] In Table 31, we examine the negative relationship between ES scores and future systematic risks, controlling for the standard list of known cross-sectional effects. We run Fama–MacBeth (1973) regressions of realized risk exposure on various firm characteristics, including ES score, and past risk characteristics, all of which are known ex ante.

3.3.1 ES Score Does Not Predict Future Unconditional Risk Exposure

In Panel A, we first consider regressions of future unconditional beta and downside beta over the next 12 months on past variables at the individual firm level. All the independent variables in these

[18] See, e.g., Daniel and Titman (1997); Harvey and Siddique (2000); and Ang et al. (2006).

regressions are measured in a period before the realization of risk measures. These regressions are run monthly, so we use 12 Newey–West (1987) lags.

Independent variables in the first two columns include: (i) ES score, (ii) log of market capitalization, (iii) risk measures (i.e., unconditional β, relative downside β, coskewness, and tail risk β) over the past months, and (iv) industry fixed effects. The last two columns also include other firm characteristics: (i) the firm book-to-market ratio, (ii) its excess returns over the past 12 months, (iii) accounting measures of performance (i.e., return on equity (ROE), asset growth, and sales growth), (iv) book leverage, and (v) a dummy for firms that pay dividends.

The first column shows that past ES scores do not predict future unconditional betas. On the other hand, past betas are a strong predictor of future betas. Hence, the strong predictive pattern of future unconditional betas across portfolios sorted by ES score in Table 29 is explained away by the size effect and the strong 12-month autocorrelation of betas. Column 3 adds additional stock characteristics, only to confirm the robustness of this negative result.

In summary, we find no significant evidence that ES scores have unconditional risk implications. Recall from Table 29 that the average returns (risk-adjusted or not) from high-ES firms are no different than those from low-ES firms. Taken together, these two results accentuate the importance of moving beyond unconditional risk and return for assessing the financial implications of incorporating ES factors into investment decisions. Indeed, the predictive relation between ES score and future downside beta persists (Columns 2 and 4), highlighting the key difference between unconditional and downside risk.

3.3.2 ES Score Predicts Future Downside Risk Exposure

Panel B of Table 31 repeats the same exercise as Panel A, except we now examine whether future measures of downside risk—relative downside β, coskewness, and tail risk β—can be predicted by past ES score, controlling for other firm characteristics and risk characteristics. Note that relative downside beta and coskewness are computed over the next 12 months, so we use 12 Newey–West (1987) lags; tail risk beta is computed over the next 60 months, so we use 60 Newey–West lags.

The estimated coefficients of future relative downside beta on past ES score are negative, with

t-statistics around -4. Consider a 1.05-point increase in ES score, which corresponds to the interdecile range of ES score (Panel A of Table 28). The coefficient estimate in Column 4 of Panel B of Table 31 indicates that such an increase in ES score is associated with a decrease in relative downside beta of 0.017, controlling for the full list of firm and risk characteristics. This effect is of the same order of magnitude as the difference in relative downside beta between the highest and lowest quintile ES portfolios that control for industry (Panel B of Table 30). Hence, the significant effects of ES investing on decreasing relative downside beta are essentially independent of other firm characteristics and risk characteristics.

Moreover, high-ES firms tend to have high future coskewness and low future tail risk. Since firms with high coskewness or low tail risk tend to covary less with the market during market declines, these results are consistent with high-ES firms having low downside risk. The estimated coefficient on ES score indicates that a 1.05-point increase in ES score is associated with an increase in coskewness of about 0.013 (Column 5 of Panel B of Table 31), compared to the 5–1 quintile difference of 0.010 in coskewness for the ES quintiles within each industry (Panel B of Table 30). Recall that the 5–1 quintile differences in tail risk betas for the ES quintiles are insignificant. According to the last column of Panel B of Table 31, changing the ES score by 1.05 point is associated with a statistically significant decrease in tail risk exposure of 0.021.

In summary, we continue to find that ES scores have significant benefits in terms of downside risk, which are stronger after controlling for other cross-sectional effects: High-ES firms have low relative downside betas and high coskewness, as well as low tail risk betas. Not only are these effects statistically significant, they are larger than those of the portfolio analysis in Table 30. Taken together with our results on unconditional risk and return, reduction of downside risk seems to be a key pecuniary benefit of ES investing.

3.4 Interpreting the Magnitude of the Estimated Coefficients

The preceding analysis shows that stocks with high ES ratings have statistically significantly lower downside risk. This is consistent with the findings in the literature that these stocks had higher returns during the 2008–2009 financial crisis (Lins et al., 2017) and during the COVID-19 market crash (Albuquerque et al., 2020). While these effects are statistically significant, we should gauge their economic significance.

To interpret the economic magnitudes of the estimated coefficients reported in the Fama–MacBeth regressions, we consider an interdecile-range move across stocks in terms of ES score, or a 1.05-point increase in ES score. The coefficient estimates indicate that such an increase in ES score is associated with (i) a decrease in relative downside beta of 0.017 (which represents about 3% of relative downside beta's interdecile range), (ii) an increase in coskewness of 0.013 (which represents about 4% of the interdecile range of coskewness), and (iii) a decrease in tail risk beta of 0.022 (which represents about 2% of tail risk beta's interdecile range). Such reductions of downside risk seem economically small.

However, the economic effects of such reductions of downside risk might be understated if the downside risk of high ES stocks is varying over time, as suggested by Figure 8. In particular, the estimated coefficients of downside risk regressed on lagged ES performance might understate the economic significance of the ES-downside risk link if the downside risk advantage of high ES stocks covaries negatively with the market, i.e., the resilience of high ES stocks during the worse part of a year is heightened if the year is itself a bad year. We explore this possibility. The results are in Table 32.

In Panel A, we first consider panel regressions of realized risk—unconditional beta, downside beta, relative downside beta, and coskewness—in each year on past variables at the individual firm level. We include all the independent variables in Table 31, except including firm fixed effects in lieu of industry fixed effects.[19] All standard errors are double clustered by firm and time. Consistent with the results of our Fama–MacBeth regressions, ES ratings have no significant unconditional risk implications, whereas they do have significant benefits in terms of downside risk. Compared to the estimated coefficients on ES score from the Fama–MacBeth regressions, those from the panel regressions are similar, but slightly larger.

Panel B repeats the same exercise as Panel A, except we now interact ES performance with $1(NegMktRet)$ and $1(PosMktRet)$, where $1(NegMktRet)$ ($1(PosMktRet)$) is a dummy variable that is equal to 1 if the market's excess return is negative (positive) in a given year.[20] ES ratings

[19] Including industry fixed effects as in the Fama–MacBeth regressions leads to the same conclusions.

[20] Again, all standard errors are double clustered by firm and time.

continue to have no significant unconditional risk implications, whereas they do have significant downside risk benefits in both good and bad years. More importantly, the downside risk advantage of high ES stocks typically doubles in bad years, indicating that the ES-downside risk link covaries negatively with the market. Therefore, the estimated coefficients of downside risk on ES performance, which capture only the average effect, plausibly understate the economic significance of their relationship.

A natural way to capture the joint effect is by looking at realized returns over the next 12 months, i.e., the contemporaneous period over which our downside risk measures are calculated. First, returns over longer horizons capture the average effect: Even if a firm's ES performance is financially immaterial based on returns over a short horizon (Table 29) and standard, unconditional risk exposures (Tables 4 and 5), high-ES stock returns over longer horizons will be higher to the extent that lower downside risk of high-ES firms mitigates large losses, which have a disproportionate impact on compound returns. Second, returns over a 12-month horizon account for the effect of whether the year is itself a bad year.

In Table 33, we run Fama–MacBeth (1973) regressions of realized excess and DGTW-adjusted returns over the next 12 months on past ES scores.[21] In the first two columns, we control for realized market beta computed over the next 12 months. The last two columns instead control for realized downside beta and upside beta computed over the same period.[22] In all columns, we control for log-size, book-to market ratio, and past 12-month excess returns at the beginning of the period t, as well as realized return volatility and coskewness.

Again, we consider an interdecile-range move across stocks in terms of ES score, or a 1.05-point increase in ES score. The coefficient estimates indicate that such an increase in ES score is associated with a future increase in compound annual returns close to 1%. While these gains in long-term returns are modest in economic terms, they are non-trivial and substantially larger than what the estimated coefficients of downside risk on ES performance suggest, consistent with our results in Panel B of Table 32.

In summary, not only do our results provide strong evidence that integrating ES factors is *not*

[21] We compute the standard errors of the coefficients by using 12 Newey–West (1987) lags.

[22] Upside beta is effectively the covariance of a firm's stock return with the market return conditional on upside movements of the market.

costly, they also explain why long-term investors care more about ES issues (Starks et al., 2023): Such investors are more exposed to downside risk, so they rationally should be more concerned about ES issues, which can help them mitigate downside risk.

3.4.1 Role of Measurement Error

The economic significance of the negative relation between ES ratings and downside risk might still be understated because of a measurement problem: Our proxy for ES may not accurately measure a firm's ES activities. On one hand, the ESG ratings of leading agencies disagree substantially.[23] On the other hand, Eccles and Stroehle (2018) and Eccles et al. (2020) point out that there are data construction and integrity issues with MSCI KLD since 2013: in essence, post-2013 data are not updated properly since MSCI is phasing out KLD to MSCI IVA dataset.

Therefore, our analysis, which relies on KLD ratings alone and which contains post-2013 data, can be subject to a real errors-in-variables (EIV) problem. We did not worry about the EIV problem when establishing statistical significance, as it would work against us. But the EIV problem can lead to an attenuation bias that is of first-order importance for assessing the economic significance of the estimates in Table 31.

Nevertheless, addressing the potential attenuation bias is unlikely to lead to downside risk mitigation effects of ES activities that are much larger than what we obtain. First, a back-of-the-envelope calculation suggests that 97% of the variation in our ES scores must be noise if, in reality, an interdecile-range move across stocks in terms of ES score is associated with interquartile-range decrease in relative downside beta (which is half of the interdecile range). Second, in Appendix C, we find similar results using Sustainalytics ratings, another major ESG ratings provider: using ES ratings from multiple raters is unlikely to substantially increase the magnitude of the downside risk effect of ES activities. Third, using ES ratings from MSCI KLD before 2013 only, we also find similar results, albeit slightly stronger.[24]

[23] See Chatterji et al. (2016) and Berg et al. (2022).

[24] All of these results are available upon request.

3.4.2 Relation to the Literature

This is not the first paper to show that high ES stocks do better during market downturns. Specifically, Lins et al. (2017) find that high-ES firms had significantly higher stock returns during the 2008–2009 financial crisis, while Albuquerque et al. (2020) report a similar finding during the COVID-19 market crash. The economic effects we obtain are consistent with those of ES policies on stock returns surrounding unparalleled market-wide, negative events such as the 2008–2009 financial crisis or the COVID-19 market crash.

Our measures of downside risk estimate the benefit of ES policies during market-wide, negative events in a conservative way, entertaining a range of downside market outcomes instead of considering only the single most catastrophic event. Such events occur rarely by definition, so the substantial economic effects conditional on such events translate to relatively small reduction in our downside risk measures. To the extent that our measures of downside risk capture the typical benefit of ES policies during market declines, they are practically more useful for portfolio selection. In summary, using conservative measures of downside risk, we highlight that not only are ES firms resilient during rare episodes of market collapse considered in the literature, they continue to be resilient during more typical market declines. At the same time, we elucidate how the substantial economic effects found in this literature can be still consistent with modest value for long-term investors.

3.5 Robustness

3.5.1 Both E and S Predict Future Downside Risk Exposure (and G Does Not)

Before we turn to potential explanations for the negative relation between ES performance and downside risk, we split the total ES score into two components: (i) E(nvironmental) score (i.e., the environment category in the MSCI KLD database) and (ii) S(ocial) score (i.e., the five categories of community, human rights, employee relations, diversity, and product). We seek to determine whether a firm's aggregate ES performance or a specific component of a firm's ES score is important for avoiding stocks that covary strongly when the market dips. We also examine the G score (i.e., the corporate governance category) here.

We run Fama–MacBeth (1973) regressions analogous to those in the last three columns of Panel B of Table 31, except that we use one ESG component at a time in lieu of the total ES score.[25] The results are shown in Panel A of Table 34.

We find strong negative relations between both components of the total net ES score and all measures of downside risk. The estimated coefficients on the E score are significant, with t-statistics around -3, 7, and -2 for relative downside beta, coskewness, and tail risk, respectively; those on the S score are also highly significant, except in the case of tail risk. Moreover, the coefficient estimates indicate that both the E and S elements of ES activities are equally important for mitigating downside risk, based on relative downside beta and coskewness. To see this, first note that the standard deviations of the E and S scores are 0.12 and 0.39, respectively (Table 28),[26] so the standard deviation of the E score is one third of that of the S score. At the same time, the coefficients on the E score are three times larger than those on the S score for relative downside beta and coskewness. Only in the case of tail risk beta is the coefficient on the E score substantially larger than that on the S score.

In contrast, we find that the G score has no predictive ability for future downside risk. The estimated coefficients on the G score are not only substantially smaller than those on the E or S scores, but they are statistically insignificant when we control for other cross-sectional effects. These results are consistent with Hong et al. (2012); Servaes and Tamayo (2013); and Krüger (2015).

Finally, the same conclusions continue to hold when we analyze the relation between the total ES score, or one of its two components, and measures of downside risk, controlling for the G score (Panel B of Table 34). In summary, both the environmental and the social aspects of a firm's ES activities appear to be of similar importance for mitigating the firm's future downside risk.

[25] We find similar results when we use all three ESG components simultaneously.

[26] Note that this difference in the standard deviations of the E and S scores is mechanical: The E score is computed using only one category, thus ranging from -1 to $+1$, whereas the S score is computed using the five social categories, thus ranging from -5 to $+5$.

3.5.2 Climate Change Concerns Predict Future Downside Risk Exposure

No other aspect of ESG has attracted more attention than those related to climate change concerns. In addition to analyzing the relations between two components of ES performance and downside risk, we analyze the relation between a firm's climate change exposure and measures of its risk. Similar to Chava (2014), we define the firm's climate change score as its clean energy strength minus its climate change concern score, both of which are part of the KLD environment category. We note that focusing on the firm's climate change score reduces the sample period to 2000–2013: climate change concern score is available from 2000 onward, while clean energy strength score experienced a major change in definition in 2013 when it was split into multiple indicators, many of which are missing.[27]

We run Fama–MacBeth (1973) regressions analogous to those in Table 31, except that we use the firm's climate change score in lieu of its total ES score. The results are shown in Panel A of Table 34.

Similar to the results of Table 31, climate change score has no significant unconditional risk implications, whereas stocks with high climate change score (i.e., stocks with low climate change exposure) have significantly lower future downside risk, based on downside beta, relative downside beta, and coskewness. Such stocks also have lower future tail risk, although the relation is statistically insignificant.

While climate change score has significant benefits in terms of lowering downside risk, the economic effects are much smaller than those of the total ES score. This may very well stem from the fact that our climate change score, constructed using two MSCI KLD dummies, may not accurately measure a firm's climate change exposure. In addition, our sample period does not cover more recent times, especially since the Paris Climate Accords in 2015, when climate change concerns have substantially heightened. In this sense, we provide only suggestive evidence. A more accurate measurement of a firm's climate change exposure or a study of more recent times could reveal a much stronger downside risk mitigation effects. We hope that such a task will be undertaken by future research.

[27] We speculate that this issue is related to the data construction and integrity issues with MSCI KLD since 2013, as discussed earlier in Section 3.4.1.

3.5.3 ES Score Predicts Downside Risk in the Universe After 2001

In Panel A of Table 35, we consider the same regressions in the last three columns of Panel B of Table 31, except we use the sample of all firms in the period after 2001. We find that our main results, which uses the sample of big firms since 1991, the beginning of our sample, are robust: High-ES firms have low relative downside betas and high coskewness, as well as low tail risk betas in the cross-section of all firms in recent years. While these effects continue to be statistically significant, they are certainly smaller than those in Table 31.

This result can be due to the dependence of ES-downside link on size. To test this idea, we interact ES performance with $1(SmlCap)$ and $1(BigCap)$, where $1(SmlCap)$ ($1(BigCap)$) is a dummy variable that is equal to 1 if the firm's market value is below (above) the median NYSE market equity. The results are shown in Panel B of Table 35. The estimated slopes on ES Score$\times 1(BigCap)$ are significant and of similar magnitude to those in Table 31 in all columns. In contrast, the interactions that involve $1(SmlCap)$ are never significant, though their slopes indicate negative relations between ES score and downside risk for small firms.

In short, we find robust negative relations between ES performance and downside risk that are stable over time, primarily in the cross-section of large firms. A natural explanation is that these effects are due to patterns of institutional trading, as discussed in Section 4.2. These negative relations in the cross-section of large firms are strong enough to keep up the statistical significance of the same relations when pooled with small firms.

4 Potential Explanations

In this section, we discuss two general explanations that can give rise to the downside risk effects of firm-level ES performance.

4.1 Doing Well by Doing Good

A key assumption of our version of the ES investing proposition is that the value of high-ES firms is resilient in periods when many firms suffer a negative shock to their value, which can be reflected in the cross-section of stock returns to generate the negative relation between ES score and downside risk documented in Section 3. In turn, we test whether the firm values of high-ES firms covary less with the average firm's value when the average firm's value is declining. We find strong empirical support for this.

Ideally, we would construct a direct measure of changes in firm value due to corporate actions that raise ES scores. But this is a challenge in itself. Instead, we use the firm-level news sentiment from RavenPack Analytics as a proxy for changes in firm value.[28]

4.1.1 RavenPack Database

For each news story analyzed, RavenPack produces a sentiment score ranging from 0–100, where values above 50 indicate positive sentiment and values below 50 show negative sentiment. As advised by the RavenPack user guide, we filter for news stories in which the firm was prominent (i.e., a relevance score of 100), and we filter for the first story that reports a categorized event (i.e., a novelty score of 100). We measure daily news sentiment for each firm as the average of RavenPack's sentiment scores across all news for each firm-day observation.

We notice that in a significant fraction of the observations, the firm is missing daily news sentiment. In turn, betas computed using data on news sentiment at the firm level would be noisy. To address this concern, we conduct our analysis using news sentiment data by examining the quintile portfolios sorted by ES scores, as in Sections 3.1 and 3.2.

If a firm's news sentiment is a good proxy for its value change, we would expect an increasing relationship between realized returns and realized news sentiment at a high frequency, which we *do* find at the portfolio level in Panel A of Table 36. These relations are both statistically and economically significant: News sentiment alone explains 25% of the variation in contemporaneous returns across the portfolios. Similarly, there is a strong positive contemporaneous relation between market return and aggregate news sentiment[29] that is visually plain in Figure 9, which plots their daily values at the start of each month over time.

[28]Our approach is motivated by the literature which indicates that media releases contain a large amount of value-relevant information (e.g., Tetlock et al., 2008).

[29]Specifically, we measure daily aggregate news sentiment as the value-weighted average of daily firm news sentiment across all firms on each day.

4.1.2 Patterns of Sentiment Covariation Across Portfolios Sorted by ES Score

The exploratory analysis in the previous section indicates that the negative relation between ES score and downside risk may very well stem from a similar relation in the cross-section of firm values, as proxied by news sentiment. We now examine whether news sentiment for high-ES firms covaries less with the aggregate news sentiment during periods of low aggregate news sentiment by constructing sentiment-based measures of downside covariation in the same way as the corresponding measures based on stock returns.

Panel B of Table 36 reports the time-series averages of relative sentiment downside betas and sentiment unconditional betas for each quintile portfolio. Both measures of sentiment covariation demonstrate essentially monotonic patterns that are decreasing in ES score. Furthermore, the differences in the column labeled "High-Low" are significantly negative, with t-statistics of -6.0 and -4.7, respectively.[30] Panel C conducts the same analysis as in Panel B but controlling for industry. The differences in relative downside and unconditional betas continue to be consistently negative and highly significant.

Taken together, our results are consistent with firms "doing well by doing good" such that they can explain the downside risk effects of firm-level ES performance in stock returns. Firm values for high-ES firms covary less with the average firm's value, especially when the average firm's value is declining. These patterns are also economically significant: The 5–1 differences in relative sentiment downside betas between ES portfolios represent about 44% of the interdecile range of the relative sentiment downside beta (based on 25 portfolios formed on size and book-to-market). Considering the fact that news sentiment explains about 25% of the variation in stock returns, these patterns translate to relatively small reductions in downside risk due to ES performance in the stock market.

4.2 ES Preferences of Institutional Investors

Another possible explanation for the negative relation between ES score and downside risk documented in Section 3 is that a group of large investors have preference for high-ES firms such that, during market declines, these firms are less susceptible to selling pressure and they covary less with

[30] All the t-statistics in Panels B and C of Table 36 are computed using 12 Newey–West (1987) lags.

the market. Institutional investors potentially represent such a group.[31]

In particular, we examine how the direction of institutional trading covaries with market returns depending on firm-level ES performance. We hypothesize that, conditional on market declines, institutional investors tend not to sell high-ES stocks as the market falls: The institutional trading downside beta with respect to the market is negatively related to ES score. We use Abel Noser institutional trading data, which contain trading records of institutional investors that use Abel Noser's transaction cost analysis services.

For each firm-day observation, we calculate the net shares traded (i.e., shares purchased minus shares sold, or trading imbalance).[32] We then scale the trading imbalance by focusing on its direction, taking a value of 1 for net institutional buying, -1 for net institutional selling, and 0 for zero net position. Our sample contains trades of large firms (firms above the median NYSE market equity) by 762 institutions between 2000 and 2010, for a total of USD 31.3 trillion in trading.

4.2.1 ES Score Matters for Patterns of Institutional Trading

We consider two versions of trading downside beta. The first version estimates betas by regressing the direction of institutional trading of each firm on the market excess return using only the observations for which the realized market excess return is below its mean in each period, just as when computing β^-. It is not clear a priori when institutional investors step in, if at all, to alleviate the selling pressure on prices of high-ES firms; therefore, the second version uses only the observations for which the realized market excess return is below the 25th percentile of its distribution in each period. We then calculate the relative trading downside beta as the raw trading downside beta minus the trading unconditional beta.

In Table 37, we examine whether past ES scores can predict future realized measures of how institutional trading covaries with the market, where the t-statistics are computed using 12 Newey–West (1987) lags. The first column shows that past ES scores do not statistically significantly

[31] First, institutional investors increasingly exhibit preferences for high-ESG firms (Starks et al., 2023, and Cao et al., 2023). Second, institutional trading exerts significant price pressure in equity markets (Coval and Stafford, 2007, and Lou, 2012). Finally, our results obtain primarily in the cross-section of large firms, which are exactly what institutional investors tend to invest in (Gompers and Metrick, 2001).

[32] If a firm is not traded by any institution on a given day, but it has been traded at least once in the database, we assume that the institutions traded 0 shares that day.

predict future trading unconditional betas over the next 12 months. ES scores exhibit consistently negative relations with both versions of trading downside beta, raw or relative, but the estimated slopes on ES scores are statistically significant only for the second version of the trading downside beta (see the last two columns of Table 37). These results suggest that institutional investors *do* supply liquidity to high-ES firms during market declines, but they do so mainly during times of extreme market declines.

Taken together, we obtain institutional trading patterns that can explain the downside risk effects of firm-level ES performance: When the market suffers extremely negative shocks, institutional investors hold on to high-ES firms, which induces high returns and low downside betas for these firms. Consistent with the fact that the downside risk effects of firm-level ES performance are not large, our results indicate that the ES preferences of institutional investors, albeit significant, are not strong: Trading downside betas decrease by only 3–4% of their interdecile range for an interdecile-range increase in ES score.

5 Conclusion

Over recent decades, there has been a substantial growth (both in absolute dollars and relative to other investments) in the assets that are invested based on ESG considerations. Yet, the recent amendment to the ERISA, requiring fiduciaries to select investments based solely on investment risk and return, and its subsequent reversal highlight the fact that there is still no consensus on the financial implications of ESG investing. In this paper, we empirically analyze how a firm's systematic downside risk and, more generally, a firm's financial performance vary with its environmental and social ratings. We find strong evidence that stocks of firms with high ES ratings have significantly lower downside risk, whereas stocks of such firms do not differ from comparable stocks based on standard, unconditional market risk or average returns. We show that the downside risk reduction effect of ES policies translates to a gain in future annual returns close to 1% in the cross-section of firms. Our results suggest that investors deriving non-pecuniary benefits from ES investing need not sacrifice financial performance and therefore help explain the rapid growth of ES investing.

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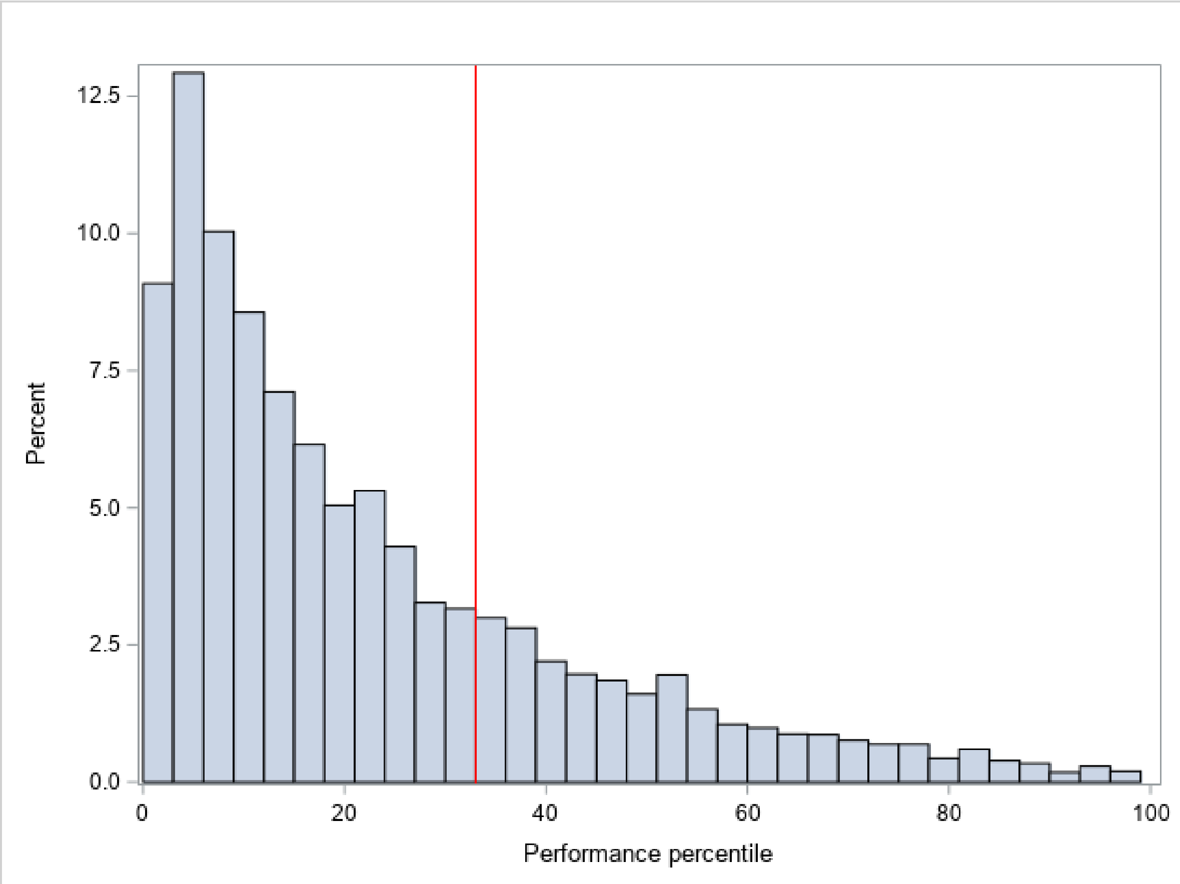

Figure 1. Fund performance distribution of departing managers

This figure plots the fund performance distribution of departing managers, as explained in section 2.3.

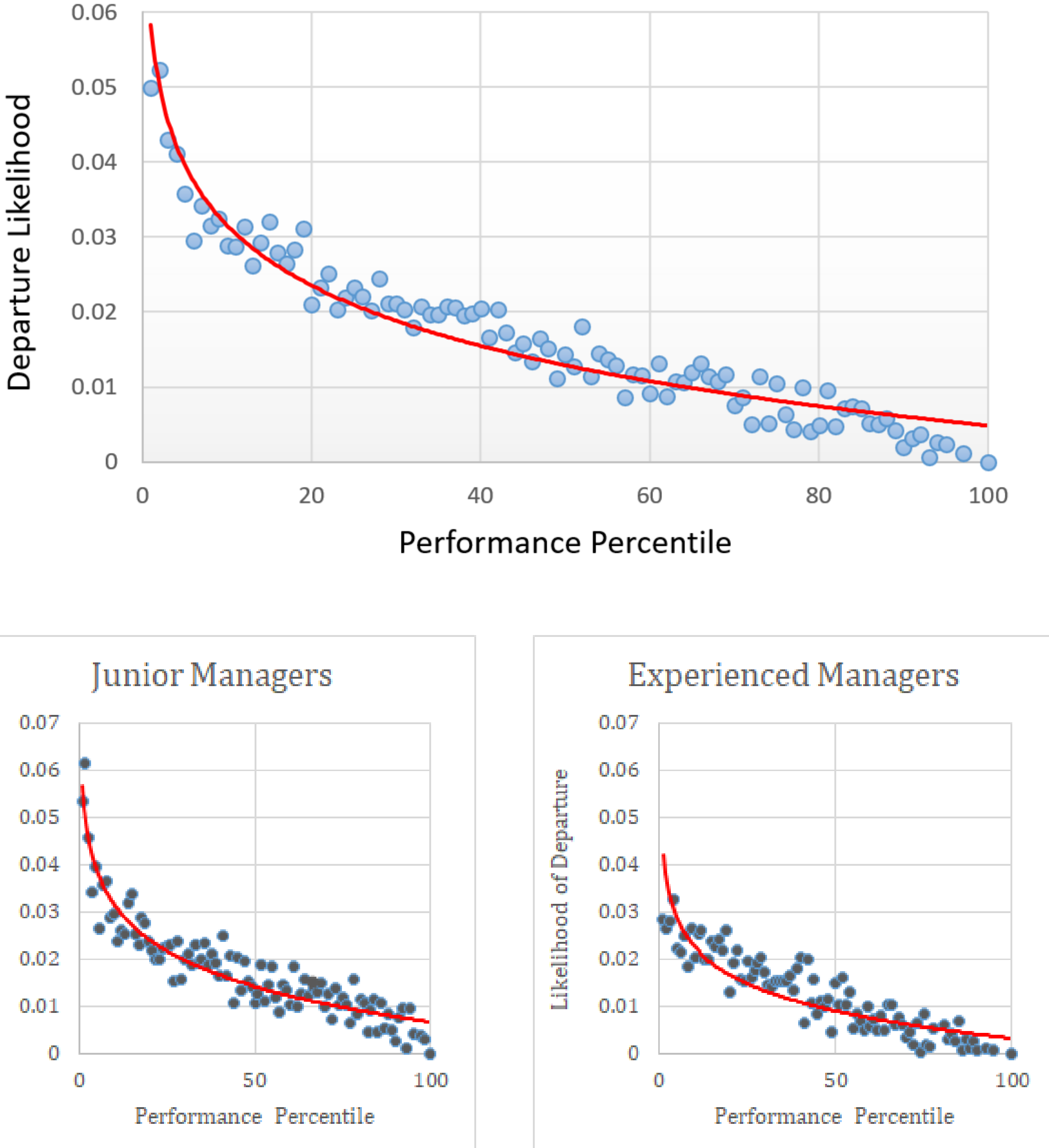

Figure 2. Past performance percentile and departure probability

This figure plots the empirical relationship between past one year fund performance and the likelihood of manager departure. In the first figure, I use all the samples as described in section 2. In the next to figures, I use subsamples of fund managers sorted into two groups based on their *Experience* at each quarter. The lower (upper) experience group is defined as Junior Managers (Experienced Managers).

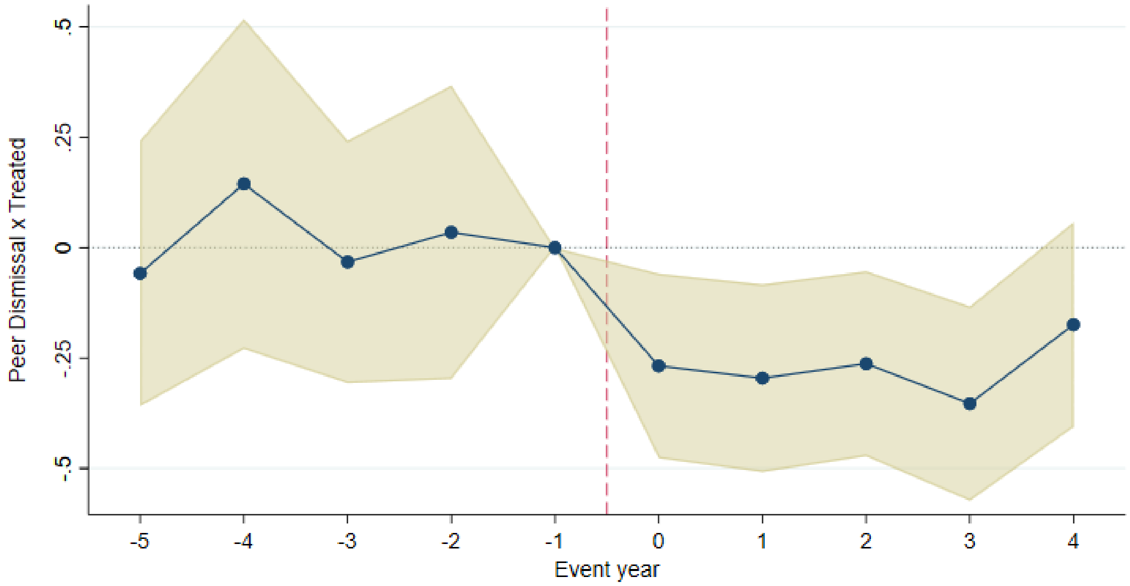

Figure 3. Dynamic Estimation around Changes in Non Compete Clause

This figure plots coefficient estimates of $PeerDismissal \times Year \times Treated$ and their 95% confidence interval from dynamic difference-in-differences regression surrounding five years state-level changes in non-compete clause, as specified in equation (10).

Figure 4. Confederate Street Locations

This figure plots the distribution of the Confederate streets with a transaction in our sample. Sample streets are represented by filled in circles. Grayed states are those without mandatory disclosure of house transaction information. Disclosure in the cross-hatched states varies at the county level.

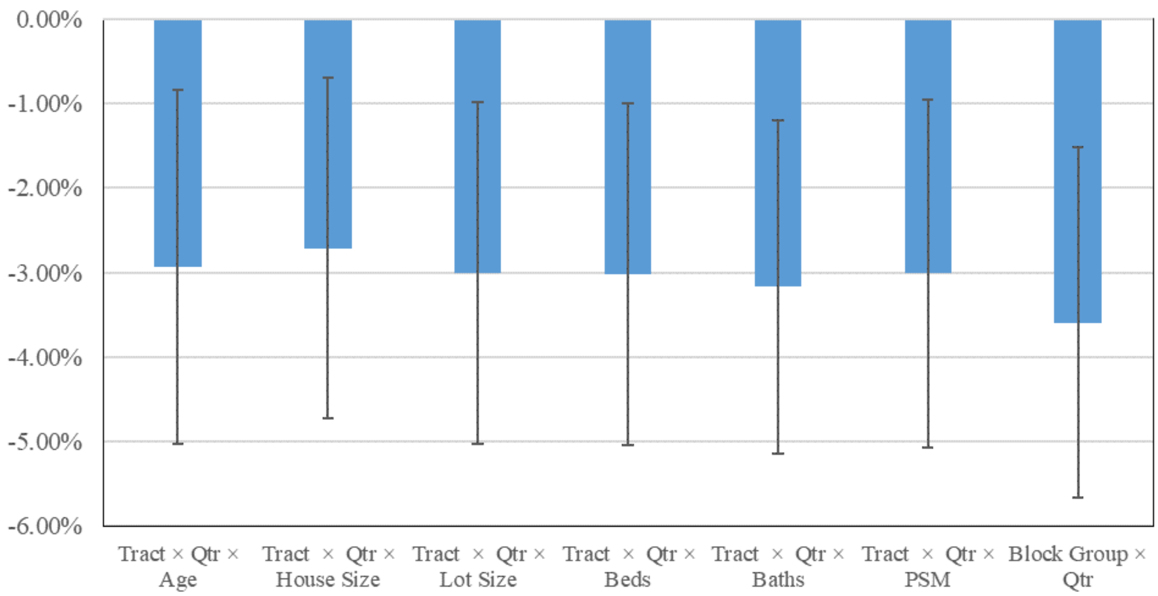

Figure 5. House Values and Confederate Street Names – Alternative Fixed Effects

This figure explores the sensitivity of the baseline findings to alternative fixed effect models. For reference, the first model reports the results from the baseline model that includes census tract × quarter × age quintile fixed effects (Specification 4 of Table 20). Models 2–6 replace age quintile fixed effects with house size quintile fixed effects, lot size quintile fixed effects, fixed effects for the number of bedrooms, fixed effects for the number of bathrooms, and propensity-score matched quintile fixed effects. In Model 7, we include block group × quarter fixed effects. The coefficients on *Confederate* are reported as blue bars and their 95% confidence intervals as error bars. The confidence intervals are computed based on standard errors clustered at the census tract level.

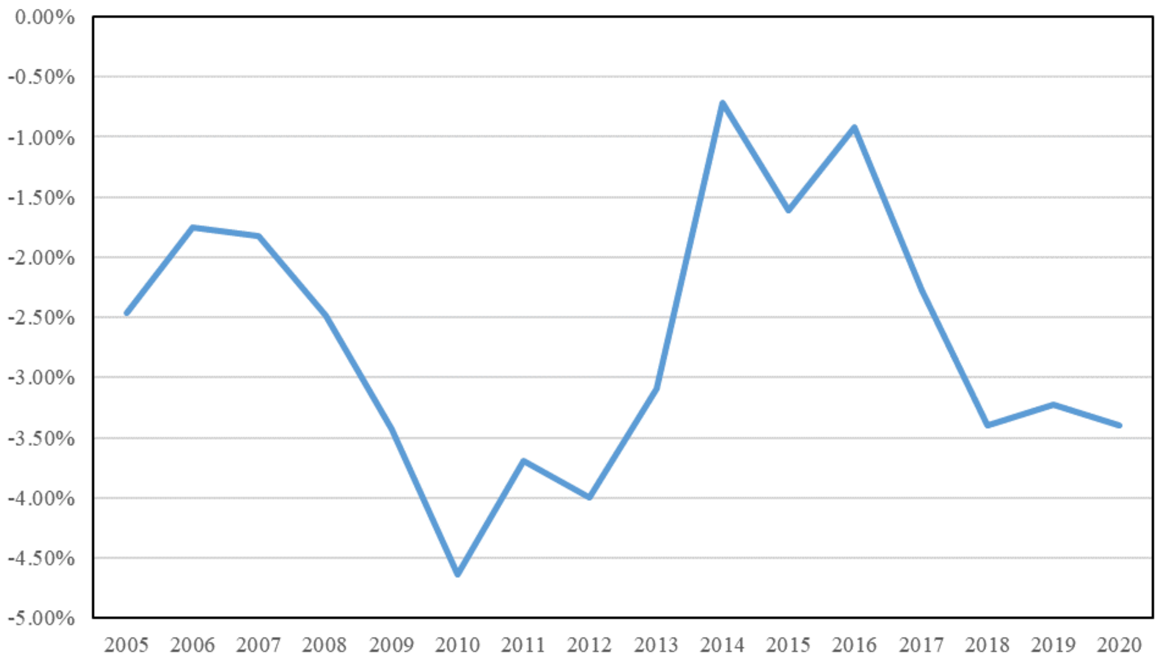

Figure 6. House Values and Confederate Street Names over Time

This figure plots the estimates on *Confederate* (i.e., the Confederate discount) from Specification 4 of Table 20 over 5-year rolling windows.

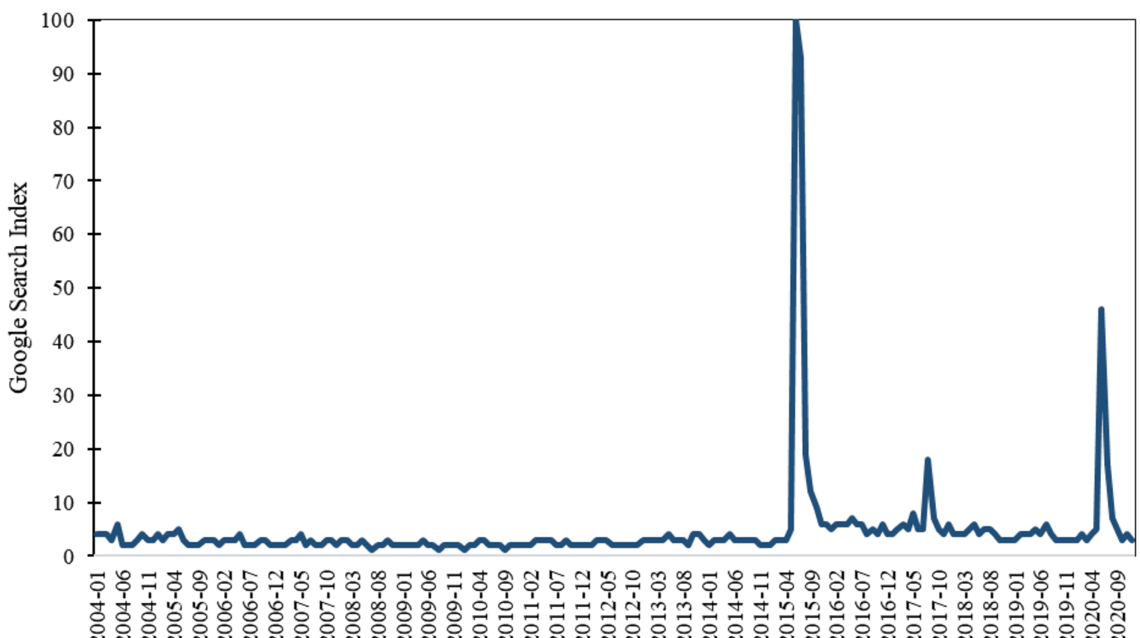

Figure 7. "Confederate Flag" Google Trend Search

The figure plots the Google Search Index for "Confederate Flag." The month with the highest search is benchmarked at 100.

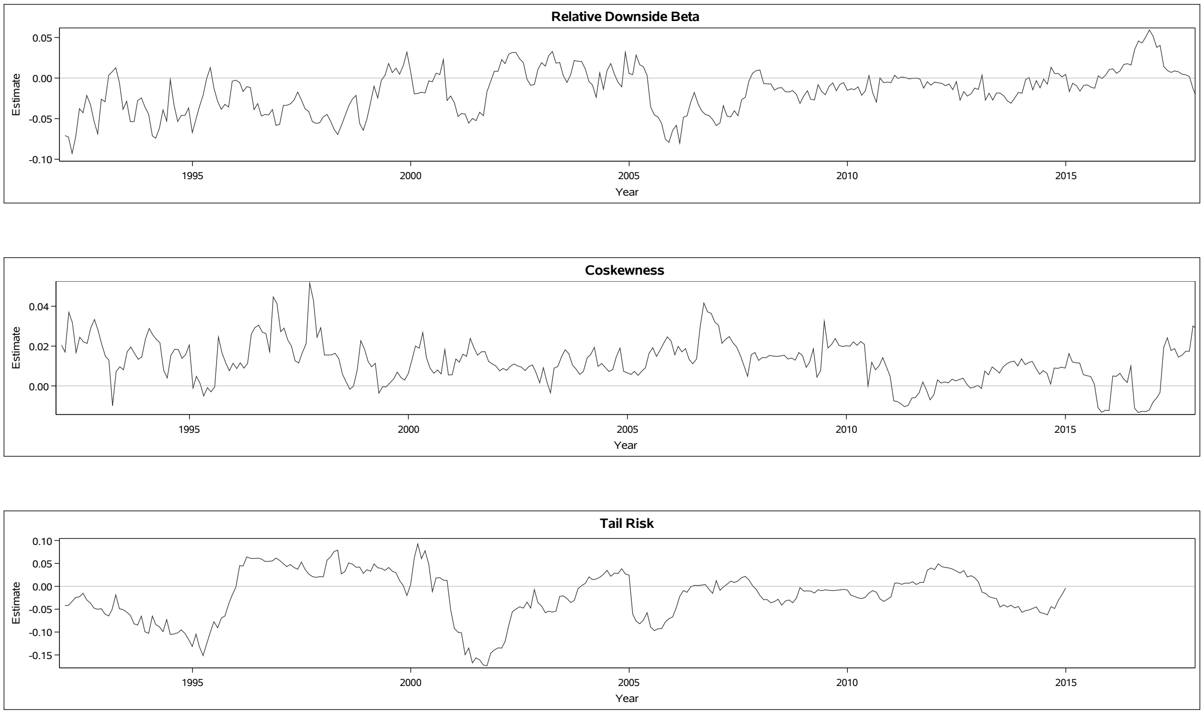

Figure 8. Monthly ES Coefficient Estimates

Plotted is the monthly ES coefficient estimate from monthly cross-sectional regression of downside risk measures on ES score and control variables. The control variables include lagged risk measures, log-normalized market capitalization in previous month, book-to-market ratio, standard deviation of daily return measured over past one year, excess return during past 12 months, dividend dummy, asset growth, sales growth, leverage, and return on equity.

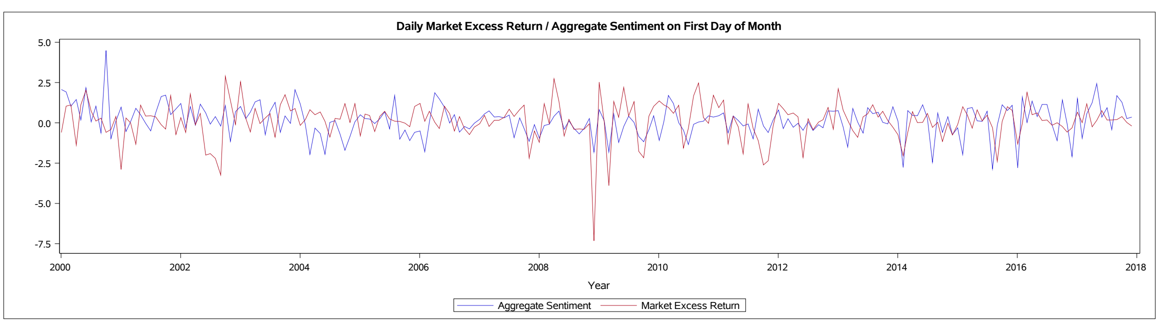

Figure 9. Aggregate News Sentiment and Market Excess Return

Plotted is the daily aggregate news sentiment and daily excess market return, on the first trading day of each month. Using all firms listed on NYSE, AMEX, or Nasdaq, we construct daily firm-level news sentiment as the average sentiment score of daily firm-level news. News published after 4:00 PM are attributed to the next trading day. We compute corresponding daily aggregate sentiment measures by value-weighting daily news sentiment of firms with at least one news. For comparison, both series are normalized to have mean zero and variance one. The time-series correlation during our sample period is 0.21.

Table 1: Summary Statistics

This table reports summary statistics. Panel A summarizes the manager employment records during my sample period (1994–2019). I report cross-sectional descriptive statics every 3 to 4 years. The records include all fund managers domiciled in the U.S., including managers of non-equity funds, which are used to identify firm departures and dismissals. Departing managers are classified as underperformers if their fund return, flow, or MS Ratings are in the bottom terciles within the same style group. Panel B summarizes the domestic equity fund- and manager-level variables used for my analysis. Panel C compares the past performance between funds that experienced peer dismissal in the previous quarter and those that did not. Detailed variable definitions are provided in Appendix A.

Panel A: Manager Employment Sample

Year	# Advisory Firms	# Managers (A)	# Departures (B)	Departure Rate (B)/(A)	Underperformers Among (B)
1994	506	2,313	147	0.0636	0.6797
1997	754	3,722	301	0.0809	0.6680
2001	854	4,976	479	0.0963	0.7514
2005	880	5,602	451	0.0805	0.7335
2009	995	6,036	611	0.1012	0.7671
2012	1,171	6,611	597	0.0903	0.7785
2015	1,266	7,351	688	0.0936	0.7485
2019	1,127	7,029	730	0.1039	0.6744

Panel B: Fund and Manager Variables

Fund variables (86,963 fund-quarter observations)

	Mean	Std.Dev.	25th	50th	75th
Risk taking^{intended}	0.9567	1.6977	0.1260	0.5433	1.2873
Cum. 1-Year Return	0.0904	0.1964	0.0078	0.1135	0.2019
Cum. 1-Year Flow	0.1268	0.6156	-0.1441	-0.0271	0.1752
MS Rating	3.03	1.07	2	3	4
Expense Ratio	0.0120	0.0065	0.0095	0.0118	0.0144
Turnover Ratio	0.7409	0.8737	0.2800	0.5500	0.9400
Fund Size ($ bil)	1.43	5.62	0.06	0.22	0.90
Fund Age (Year)	14.17	13.53	5.42	10.75	17.83
MgrPFund	2.40	2.07	1	2	3

Manager variables (116,773 manager-quarter observations)

	Mean	Std.Dev.	25th	50th	75th
Peer Dismissal	0.17	0.38	0	0	0
Age	46.90	10.24	39	46	53
MBA	0.59	0.49	0	1	1
Experience (Year)	8.50	6.46	3.5	7.08	12
Female	0.09	0.29	0	0	0
CFA	0.61	0.49	0	1	1
Advisor Size ($ bil)	46.06	142.64	0.59	4.48	30.72

Panel C: Performance Comparison (Peer Dismissal vs Controls)

	Peer Dismissal	Control	Difference	p-value
Cum. 1-Year Return	0.086	0.093	-0.00690	0.253
Cum. 1-Year Flow	0.083	0.145	-0.0613	0.000
MS Rating	3.024	3.063	-0.0384	0.214

Table 2: Peer Dismissal and Risk-Taking

This table analyzes the effect of peer dismissal on subsequent risk-taking behavior. The table reports estimates for the following weighted least squares (WLS) model:

$$Risk\ Taking^{intended}_{i,t} = \beta Peer\ Dismissal_{j,t-1} + \Gamma X_{i,j,t-1} + \epsilon_{i,j,t}.$$

The dependent variable, $Risk\ Taking^{intended}_{i,t}$, is a holding-based risk-taking measure as defined in Equation (2). The main independent variable of interest is $Peer\ Dismissal_{j,t-1}$, which is an indicator for whether the fund manager experienced peer dismissal in the previous quarter. All observations are weighted by the inverse of the number of the managers for each fund-quarter. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Dependent variable: $Risk\ Taking^{intended}_{i,t}$				
Peer Dismissal	-0.269***	-0.168***	-0.153***	-0.0666***	-0.0645***
	(-8.02)	(-5.60)	(-5.43)	(-3.34)	(-3.27)
Return		2.302**	2.342**	2.216***	2.224***
		(2.37)	(2.43)	(2.66)	(2.67)
Flow		0.312***	0.313***	0.293***	0.293***
		(8.54)	(8.55)	(9.65)	(9.66)
logAssets		0.0613***	0.0581***	0.0411***	0.0407***
		(4.56)	(4.38)	(4.01)	(3.98)
logAdvAssets		-0.0556***	-0.0472***	0.00303	0.00493
		(-5.58)	(-4.83)	(0.29)	(0.47)
logFundAge		0.0198	0.00978	-0.0679***	-0.0682***
		(0.63)	(0.32)	(-2.76)	(-2.78)
logExpRatio		24.69**	23.01**	11.21**	11.07**
		(2.43)	(2.35)	(2.20)	(2.21)
logTurnRatio		-0.0224	0.00959	0.184**	0.192***
		(-0.27)	(0.11)	(2.54)	(2.64)
MgrPFund		-0.0130*	-0.00950	-0.00471	-0.00307
		(-1.69)	(-1.25)	(-0.80)	(-0.52)
logMgrExperience			0.0587**		0.0106
			(2.47)		(0.74)
logMgrAge			0.268**		0.159**
			(2.39)		(2.57)
CFA			-0.0582		-0.0198
			(-1.63)		(-0.87)
MBA			0.00351		0.0328
			(0.11)		(1.55)
Female			-0.0990***		-0.0212
			(-2.74)		(-0.75)
Style × Time FE	Yes	Yes	Yes	Yes	Yes
Advisor FE	No	No	No	Yes	Yes
Observations	208,683	208,683	208,683	208,683	208,683
Adjusted R^2	0.187	0.200	0.205	0.384	0.385

Table 3: Peer Dismissal Effect and Fund Manager Performance

This table analyzes whether the peer dismissal effect is stronger for underperforming managers. The table reports estimates for the following weighted least squares (WLS) model:

$$\begin{aligned} Risk\ Taking^{intended}_{i,t} &= \beta_1 Peer\ Dismissal_{j,t-1} \times Performance_{i,t-1} \\ &+ \beta_2 Peer\ Dismissal_{j,t-1} + \Gamma X_{i,j,t-1} + \epsilon_{i,j,t}. \end{aligned}$$

The dependent variable, $Risk\ Taking^{intended}_{i,t}$, is a holding-based risk-taking measure as defined in Equation (2). The main independent variable of interest is $Peer\ Dismissal_{j,t-1} \times Performance_{i,t-1}$, which is an interaction term between the indicator for peer dismissal and the fund performance measure, which differs by specification. All observations are weighted by the inverse of the number of the managers for each fund-quarter. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Performance Measure	Past Return			Low terciles			Composite
	Return	Return Percentile		Return	MS Rating	Flow	
Peer Dismissal	-0.106***	-0.223***	-0.192***	-0.0256*	-0.0481**	-0.0440*	
	(-4.30)	(-3.97)	(-3.52)	(-1.72)	(-2.40)	(-1.85)	
Peer Dismissal							
$\times$ *Return*	0.522***						
	(3.05)						
$\times$ *Return Percentile*		0.322***	0.252***				
		(3.39)	(2.75)				
$\times$ *Low terciles*				-0.116***	-0.0681**	-0.0329	
				(-2.75)	(-2.22)	(-1.05)	
$\times\ \mathbb{1}[Composite = 0]$							-0.0211
							(-1.03)
$\times\ \mathbb{1}[Composite = 1]$							-0.0474*
							(-1.73)
$\times\ \mathbb{1}[Composite = 2]$							-0.107***
							(-3.34)
$\times\ \mathbb{1}[Composite = 3]$							-0.254***
							(-3.70)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advisor FE	Yes	Yes	Yes	No	No	No	No
Style $\times$ Time FE	Yes	Yes	No	No	No	No	No
Advisor $\times$ Group FE	No	No	No	Yes	Yes	Yes	Yes
Style $\times$ Time $\times$ Group FE	No	No	Yes	Yes	Yes	Yes	Yes
Observations	208,683	208,683	208,683	208,683	147,605	208,683	208,683
Adjusted R^2	0.388	0.387	0.522	0.418	0.447	0.414	0.440
High-Low (p-value)							0.00120

Table 4: Peer Dismissal Effect and Manager Experience

This table analyzes whether the peer dismissal effect is stronger for less experienced managers. The dependent variable, $Risk\ Taking_{i,t}^{intended}$, is a holding-based risk-taking measure as defined in Equation (2). At each quarter, fund managers are sorted into terciles based on their *Experience*. The lowest *Experience* group is sorted as *Low Experience*, and the highest and second-highest group are sorted as *High Experience* and *Mid Experience*, respectively. All observations are weighted by the inverse of the number of the managers for each fund-quarter. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent variable: $Risk\ Taking_{i,t}^{intended}$		
		Underperformers	
Sample	Full	Yes	No
Peer Dismissal			
$\times$ $\mathbb{1}$[*Low Experience*]	-0.0899***	-0.154***	-0.0506*
	(-3.30)	(-3.38)	(-1.91)
$\times$ $\mathbb{1}$[*Mid Experience*]	-0.0770***	-0.112**	-0.0452*
	(-3.17)	(-2.62)	(-1.87)
$\times$ $\mathbb{1}$[*High Experience*]	-0.0264	-0.0505	-0.0151
	(-0.96)	(-0.95)	(-0.59)
$\mathbb{1}$[*Mid Experience*]	-0.0129	0.000775	-0.0212
	(-0.44)	(0.02)	(-0.74)
$\mathbb{1}$[*High Experience*]	-0.0255	-0.00575	-0.0340
	(-0.62)	(-0.10)	(-0.87)
Control Variables	Yes	Yes	Yes
Advisor FE	Yes	Yes	Yes
Style $\times$ Time FE	Yes	Yes	Yes
Observations	208,683	70,199	138,484
Adjusted R^2	0.385	0.426	0.395

Table 5: Peer Dismissal Effect and Recession

This table analyzes whether the peer dismissal effect is stronger following bear markets. The table reports estimates for the following weighted least squares (WLS) model:

$$\begin{aligned} Risk\ Taking_{i,t}^{intended} &= \beta_1 Peer\ Dismissal_{j,t-1} \times Recession_t \\ &+ \beta_2 Peer\ Dismissal_{j,t-1} + \Gamma X_{i,j,t-1} + \epsilon_{i,j,t}. \end{aligned}$$

The dependent variable, $Risk\ Taking_{i,t}^{intended}$, is a holding-based risk-taking measure as defined in Equation (2). The main independent variable of interest is $Peer\ Dismissal_{j,t-1} \times Recession$, an interaction term between the indicator for peer dismissal and market recession, which differs by specification. *MktRet* is market return during the past one year. *Recession* is an indicator if *MktRet* is in the bottom group during the sample period. All observations are weighted by the inverse of the number of the managers for each fund-quarter. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Recession Definition	*Recession*=1 if *MktRet* is in bottom			*MktRet*
	5%	10%	25%	
Peer Dismissal	-0.0502***	-0.0452***	-0.0318**	-0.114***
	(-3.20)	(-3.32)	(-2.26)	(-4.69)
Peer Dismissal				
× *Recession*	-0.321***	-0.204*	-0.139**	
	(-2.89)	(-1.94)	(-2.45)	
× *MktRet*				0.671***
				(3.61)
Baseline Controls	Control Variables, Advisor and Style × Time FE			
Observations	208,683	208,683	208,683	208,683
Adjusted R^2	0.412	0.418	0.416	0.385

Table 6: Peer Dismissal Effect and Social Ties

This table analyzes whether the peer dismissal effect is stronger following a market downturn. The table reports estimates for the following weighted least squares (WLS) model:

$$\begin{aligned}Risk\ Taking^{intended}_{i,t} &= \beta_1 Peer\ Dismissal_{j,t-1} \times Social\ Ties_{j,t-1}\\ &+ \beta_2 Peer\ Dismissal_{j,t-1} + \Gamma X_{i,j,t-1} + \epsilon_{i,j,t}.\end{aligned}$$

The dependent variable, $Risk\ Taking^{intended}_{i,t}$, is a holding-based risk-taking measure as defined in Equation (2). A manager has a social tie with the dismissed manager if (column 1) they graduated from the same college, (column 2) their age gap is less than six years, or (column 3) the manager spent more than 50% of their career with the dismissed peer in the same firm. In column 4, I use a composite measure that adds up each indicator for the three social tie measures. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Social Ties			Composite
	Education	Age	Experience	
Peer Dismissal	-0.0580***	-0.0556***	-0.0543***	-0.0437**
	(-3.01)	(-2.75)	(-2.88)	(-2.23)
Peer Dismissal				
× *Social Ties*	-0.0768**	-0.0339	-0.106*	
	(-2.09)	(-1.23)	(-1.85)	
× $\mathbb{1}[Composite = 1]$				-0.0363
				(-1.59)
× $\mathbb{1}[Composite = 2+]$				-0.153**
				(-2.62)
Baseline Controls	Control Variables, Advisor and Style × Time FE			
Observations	208,683	208,683	208,683	208,683
Adjusted R^2	0.385	0.385	0.385	0.385

Table 7: Robustness Test

This table tests the robustness of the peer dismissal effect. Specifically, the table reports estimates for the augmented model of column 5 of Table 2. Panel A provides the result using different measures of risk-taking (columns 1 through 3) and peer dismissal (columns 4 and 5). Panel B provides the results of placebo tests using a sample of index funds (columns 1 and 2) and using other peer turnover events (columns 3 through 6). Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Alternative Dependent and Independent Variables

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	$Risk\ Taking^k$		$\lvert Risk\,Taking^k \rvert$	$Risk\,Taking^k$	
where $k =$	*realized*	*ratio*	*intended*	*intended*	
Peer Dismissal	-0.0669**	-0.00353***	-0.0526***		
	(-2.22)	(-3.70)	(-2.85)		
$log(1 + \#\,Peer\,Dismissal)$				-0.0713***	
				(-3.39)	
$\mathbb{1}[\#\,Peer\,Dismissal = 1]$					-0.0527***
					(-2.70)
$\mathbb{1}[\#\,Peer\,Dismissal = 2+]$					-0.0880***
					(-3.37)
Baseline Controls	Control Variables, Advisor and Style × Time Fixed Effects				
Observations	208,683	208,683	208,683	208,683	208,683
Adjusted R^2	0.932	0.364	0.456	0.385	0.385
F-test (p-value)					0.0905

Panel B: Placebo Test

	(1)	(2)	(3)	(4)	(5)	(6)
	Index Funds		Other Turnovers			
$Risk\ Taking^k$, w/ $k =$	*intended*	*realized*	*intended*	*realized*	*intended*	*realized*
Peer Dismissal	-0.00367	-0.00656	-0.0651***	-0.0697*	-0.0618***	-0.0680**
	(-0.21)	(-0.17)	(-3.00)	(-1.84)	(-3.27)	(-2.24)
Peer Advancement			0.00118	0.00559		
			(0.06)	(0.15)		
Peer Demotion					-0.0393	0.0158
					(-1.12)	(0.28)
Baseline Controls	Control Variables, Advisor and Style × Time Fixed Effects					
Observations	29,196	29,196	208,683	208,683	208,683	208,683
Adjusted R^2	0.464	0.968	0.384	0.932	0.384	0.932

Table 8: Correlated Group Effect

This table examines whether my results are subject to correlated group effect driven by time-varying advisor shock. In the first four columns, I test for within-firm heterogeneity by iterating Table 6 including advisor-by-quarter fixed effect in lieu of advisor fixed effect. In columns 5 and 6, I test for across-firm effect by including *Same Building Dismissal* dummy, which is an indicator for dismissal in a different firm within the same building according to Form ADV. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Within-firm Heterogeneity				Across-firm effect	
	Social Ties			Composite	Different firms in same building	
	Education	Age	Experience			
Peer Dismissal						-0.0640***
						(-3.27)
× *Social Ties*	-0.0420	-0.0513**	-0.116**			
	(-1.62)	(-2.05)	(-2.25)			
× $\mathbb{1}[Composite = 1]$				-0.0682***		
				(-3.10)		
× $\mathbb{1}[Composite = 2+]$				-0.0974*		
				(-1.92)		
Same Building Dismissal					-0.0605*	-0.0586*
					(-1.84)	(-1.81)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Advisor FE	No	No	No	No	Yes	Yes
Advisor × Time FE	Yes	Yes	Yes	Yes	No	No
Style × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	208,683	208,683	208,683	208,683	187,498	187,498
Adjusted R^2	0.590	0.590	0.590	0.590	0.385	0.385

Table 9: Flow-Driven Trade Hypothesis

This table tests whether the dismissal effect is robust to flow-driven trades (e.g., Coval and Stafford, 2007). Specifically, the table reports estimates for the augmented model of column 5 of Table 2. Columns 1 through 6 estimate the peer dismissal effect within a tight group of funds with similar flows. Columns 1 through 3 (4 through 6) partition funds based on their past flow within each quarter (each style-quarter). Columns 7 through 9 test whether the peer dismissal effect is weaker for funds that are less susceptible to flow-induced trading. In particular, column 7 focuses on large-cap funds, column 8 on small size funds, and column 9 on negative flow funds. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Comparing within tight flow group						Testing weaker effect for subgroup		
	# Groups within quarter			# Groups within style-quarter			Subgroup		
	50	100	200	5	10	20	Large cap	Small size	Negative flow
Peer Dismissal	-0.0608***	-0.0563***	-0.0672***	-0.0587***	-0.0621***	-0.0610***	-0.0737***	-0.0676**	-0.0633**
	(-2.97)	(-2.77)	(-3.41)	(-2.87)	(-3.04)	(-3.18)	(-2.82)	(-2.46)	(-2.24)
Peer Dismissal							0.0169	0.00536	0.00251
$\times \mathbb{1}[\textit{Subgroup}]$							(0.53)	(0.17)	(0.10)
Advisor FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Style × Time FE	Yes	Yes	Yes	No	No	No	No	No	No
Time × Group FE	Yes	Yes	Yes	No	No	No	No	No	No
Style × Time × Group FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	208,683	208,683	208,683	208,683	208,683	208,683	208,683	208,683	208,683
Adjusted R^2	0.415	0.440	0.492	0.407	0.432	0.476	0.385	0.390	0.392

Table 10: Alternative Fixed Effects

This table implements high-dimensional fixed effect regressions to rule out alternative hypotheses for the peer dismissal effect. Specifically, the table reports estimates for the augmented model of column 5 of Table 2. Panel A augments the model by including advisor city fixed effect (columns 1 through 4) and advisory size percentile fixed effect (columns 5 through 8). Panel B substitutes the advisor fixed effect with the manager fixed effect (columns 1 through 4) and fund fixed effect (columns 5 through 8). Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Regional Economic Shocks and Advisor Size

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Advisor City Fixed Effect				Advisor Size Fixed Effect			
Risk Taking^k, w/ k =	*intended*	*realized*	*intended*	*realized*	*intended*	*realized*	*intended*	*realized*
Peer Dismissal	-0.0594***	-0.0594**	-0.0737***	-0.0709***	-0.0593***	-0.0613**	-0.0647*	-0.102*
	(-3.11)	(-2.19)	(-2.98)	(-2.70)	(-2.98)	(-2.07)	(-1.90)	(-1.80)
Baseline Controls	Control Variables and Advisor FE							
Style × Time FE	Yes	Yes	No	No	Yes	Yes	No	No
Adv.City FE	Yes	Yes	No	No	No	No	No	No
Adv.City×Style×Time FE	No	No	Yes	Yes	No	No	No	No
Adv.Size FE	No	No	No	No	Yes	Yes	No	No
Adv.Size × Style × Time FE	No	No	No	No	No	No	Yes	Yes
Observations	187,498	187,498	187,498	187,498	208,683	208,683	208,683	208,683
Adjusted R^2	0.420	0.950	0.593	0.971	0.386	0.932	0.636	0.967

Panel B: Manager and Fund Fixed Effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Manager Fixed Effect				Fund Fixed Effect			
Risk Taking^k, w/ k =	*intended*	*intended*	*realized*	*realized*	*intended*	*intended*	*realized*	*realized*
Peer Dismissal	-0.0617**		-0.0746**		-0.0521*		-0.0749**	
	(-2.31)		(-2.22)		(-1.96)		(-2.31)	
$log(1 + \#\ Peer\ Dismissal)$		-0.0786**		-0.0934**		-0.0684*		-0.0933**
		(-2.24)		(-2.37)		(-1.96)		(-2.44)
Baseline Controls	Control Variables and Time × Style FE							
Manager FE	Yes	Yes	Yes	Yes	No	No	No	No
Fund FE	No	No	No	No	Yes	Yes	Yes	Yes
Observations	208,683	208,683	208,683	208,683	208,683	208,683	208,683	208,683
Adjusted R^2	0.328	0.328	0.899	0.899	0.363	0.363	0.901	0.901

Table 11: Difference-in-Differences Around Changes in Enforceability of Non-Compete Agreements

This table shows a stacked difference-in-differences analysis around state-level changes in the enforceability of non-compete agreements (NCAs). The table reports estimates for the model as specified in Equation (9). The dependent variable, $Risk\ Taking^{intended}_{i,t}$, is a holding-based risk-taking measure as defined in Equation (2). Treated states include Illinois, Texas, Ohio, Georgia, Colorado, and Wisconsin. Control states are states that did not experience changes in NCA enforceability during the sample period. The independent variable of interest, $PeerDismissal_{j,c,t} \times Treat_{j,c,t} \times Post_{c,t}$ captures the marginal effect of peer dismissal on risk-taking activity following changes in NCA enforceability of funds in treated states compared with funds in control states. Columns 5 and 6 augment the regression model by adding interaction terms between pre-treatment indicators and $PeerDismissal_{j,c,t} \times Treat_{j,c,t}$. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline				Pre-trend	
Peer Dismissal	-0.142***	-0.141***	-0.172***	-0.170***	-0.176***	-0.164***
	(-3.11)	(-3.07)	(-3.55)	(-3.54)	(-3.57)	(-2.76)
Peer Dismissal						
× *Treated* × *Post*	-0.232**	-0.246***	-0.271***	-0.281***	-0.283**	-0.274**
	(-2.59)	(-2.81)	(-2.94)	(-3.12)	(-2.61)	(-2.34)
× *Treated*	0.0879	0.106	0.118	0.136*	0.138	0.130
	(1.17)	(1.42)	(1.50)	(1.72)	(1.40)	(1.13)
× *Post*	0.175***	0.172***	0.212***	0.204***	0.210***	0.199***
	(3.44)	(3.43)	(3.88)	(3.83)	(3.79)	(3.02)
× *Treated* × *Prior 1 Yr*					-0.00957	-0.000748
					(-0.07)	(-0.00)
× *Treated* × *Prior 2 Yr*						0.0335
						(-0.18)
× *Prior 1 Yr*					0.0283	0.0166
					(0.42)	(0.23)
× *Prior 2 Yr*						-0.0454
						(-0.65)
Control Variables	No	Yes	No	Yes	Yes	Yes
Fund × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Time × Treated × Cohort FE	Yes	Yes	No	No	No	No
Time × State × Cohort FE	No	No	Yes	Yes	Yes	Yes
Observations	442,262	442,262	442,262	442,262	442,262	442,262
Adjusted R^2	0.528	0.533	0.554	0.560	0.560	0.560

Table 12: Mechanism of the Peer Dismissal Effect

This table analyzes how mutual funds reduce portfolio risk following peer dismissal. In Panel A, fund total risk is decomposed into systematic risk and idiosyncratic volatility (IVOL) using daily fund returns. Market risk (MKTRF) is estimated loading from the CAPM model, and IVOL is the standard deviation of the residuals. SMB, HML, and MOM are estimated loadings from Carhart's four-factor model, and RMW and CMA are from the Fama-French five-factor model. The dependent variable is changes from the realized risk in the previous quarter to the intended risk in the current quarter. Panels B and C examine changes in mutual fund holdings and analyze whether peer-dismissed managers increase (decrease) their fund holdings in low-risk (high-risk) stocks more than managers without dismissal. The risk of each stock is computed using past one-year daily returns based on the CAPM model (MKTRF and IVOL) and Carhart's four-factor model (HML, SMB, and MOM), and sorted into NYSE-threshold quartiles. Each cell reports the average (changes in) weight of stocks in the risk quartile, for each of the peer-dismissed funds and control fund group.

Panel A: Changes In Systematic Risk and Idiosyncratic Volatility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Risk Model	CAPM		Carhart 4			Fama French 5	
	MKTRF	IVOL	SMB	HML	MOM	RMW	CMA
Peer Dismissal	-0.00266***	-0.0228**	0.000867	-0.000371	0.0000253	-0.000989	-0.000701
	(-3.27)	(-2.26)	(1.11)	(-0.34)	(0.03)	(-0.68)	(-0.43)
Baseline Controls	Control Variables, Advisor and Style × Time FE						
Observations	208,683	208,683	208,683	208,683	208,683	208,683	208,683
Adjusted R^2	0.349	0.224	0.231	0.159	0.152	0.132	0.118

Panel B: Holding-Based Evidence - Total Risk, Market Beta, and Idiosyncratic Volatility

	Peer Dismissed Funds			Funds without Peer Dismissal					
B.1: Total risk	W_{t-1}	W_t	(A) ΔW_t	W_{t-1}	W_t	(B) ΔW_t	(A)-(B)	t-stats	p-value
1 (Low)	37.20%	37.54%	0.34%	37.53%	37.65%	0.11%	0.22%	2.37	0.02
2	29.11%	29.06%	-0.05%	30.57%	30.57%	0.01%	-0.05%	-0.56	0.58
3	19.57%	19.52%	-0.04%	19.31%	19.23%	-0.09%	0.04%	0.47	0.64
4 (High)	14.12%	13.88%	-0.25%	12.59%	12.56%	-0.04%	-0.21%	-1.22	0.22

	Peer Dismissed Funds			Funds without Peer Dismissal					
B.2: MKTRF	W_{t-1}	W_t	(A) ΔW_t	W_{t-1}	W_t	(B) ΔW_t	(A)-(B)	t-stats	p-value
1 (Low)	18.91%	18.95%	0.04%	20.03%	19.98%	-0.05%	0.09%	2.26	0.03
2	26.49%	26.53%	0.03%	26.33%	26.35%	0.01%	0.02%	0.36	0.72
3	29.41%	29.54%	0.14%	28.59%	28.66%	0.07%	0.07%	1.31	0.19
4 (High)	25.19%	24.98%	-0.21%	25.05%	25.01%	-0.04%	-0.17%	-3.16	0.00

	Peer Dismissed Funds			Funds without Peer Dismissal					
B.3: IVOL	W_{t-1}	W_t	(A) ΔW_t	W_{t-1}	W_t	(B) ΔW_t	(A)-(B)	t-stats	p-value
1 (Low)	35.24%	35.44%	0.20%	33.60%	33.77%	0.16%	0.04%	0.83	0.41
2	27.56%	27.62%	0.07%	27.55%	27.59%	0.04%	0.03%	0.69	0.49
3	20.75%	20.78%	0.03%	21.35%	21.33%	-0.02%	0.05%	0.85	0.40
4 (High)	16.45%	16.15%	-0.30%	17.49%	17.31%	-0.18%	-0.12%	-1.72	0.09

(*Continued*)

Panel C: Holding-Based Evidence - Other Factor Risks

	Peer Dismissed Funds			Funds without Peer Dismissal					
C.1: HML	W_{t-1}	W_t	(A) ΔW_t	W_{t-1}	W_t	(B) ΔW_t	(A)-(B)	t-stats	p-value
1 (Low)	38.66%	38.64%	-0.02%	37.98%	37.92%	-0.05%	0.03%	0.47	0.64
2	23.62%	23.70%	0.08%	24.05%	24.12%	0.07%	0.01%	0.26	0.79
3	19.74%	19.77%	0.03%	20.08%	20.13%	0.05%	-0.02%	-0.43	0.67
4 (High)	17.97%	17.89%	-0.09%	17.89%	17.82%	-0.06%	-0.03%	-0.59	0.56
	Peer Dismissed Funds			Funds without Peer Dismissal					
C.2: SMB	W_{t-1}	W_t	(A) ΔW_t	W_{t-1}	W_t	(B) ΔW_t	(A)-(B)	t-stats	p-value
1 (Low)	49.74%	49.89%	0.15%	48.27%	48.36%	0.09%	0.06%	0.55	0.59
2	26.85%	26.87%	0.02%	27.89%	27.94%	0.05%	-0.03%	-0.5	0.62
3	14.66%	14.69%	0.03%	15.20%	15.17%	-0.03%	0.07%	1.05	0.30
4 (High)	8.75%	8.55%	-0.20%	8.64%	8.53%	-0.11%	-0.09%	-1.14	0.26
	Peer Dismissed Funds			Funds without Peer Dismissal					
C.3: MOM	W_{t-1}	W_t	(A) ΔW_t	W_{t-1}	W_t	(B) ΔW_t	(A)-(B)	t-stats	p-value
1 (Low)	19.24%	19.10%	-0.14%	19.76%	19.57%	-0.19%	0.05%	0.54	0.59
2	23.71%	23.67%	-0.04%	24.55%	24.55%	0.00%	-0.03%	-0.44	0.66
3	25.91%	25.96%	0.05%	26.16%	26.15%	0.00%	0.05%	0.52	0.60
4 (High)	31.15%	31.27%	0.13%	29.53%	29.73%	0.19%	-0.07%	-0.39	0.70

Table 13: Peer Dismissal, Risk-Taking, and Agency Problem

This table analyzes whether the negative effect of peer dismissal on risk-taking behavior is stronger for funds that are more prone to agency problems: funds with high expense ratio (column 1), younger age (column 2), and strong convex flow-performance relationship (*FPR*, column 3). In each column, funds are sorted into terciles based on each agency problem (AP) measure: *Low*, *Mid*, and *High AP*. Column 4 estimates the joint effect of agency problems based on a composite score, computed as the sum of the highest AP group indicators ("*High AP*") in each of the three AP proxies. In column 4, *Low AP*, *Mid AP*, and *High AP* correspond to composite score of 0, 1, and +2 respectively. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Agency Problem (AP) Measure			
	High Expense	Young Age	Convex FPR	Composite
Peer Dismissal				
× *Low AP*	-0.0325	-0.0264	-0.0292	-0.0474**
	(-1.31)	(-0.96)	(-1.23)	(-2.09)
× *Mid AP*	-0.0495*	-0.0770***	-0.0645**	-0.0463**
	(-1.90)	(-3.17)	(-2.18)	(-2.21)
× *High AP*	-0.107***	-0.0899***	-0.0666**	-0.126***
	(-3.08)	(-3.30)	(-2.23)	(-3.30)
Control Variables	Yes	Yes	Yes	Yes
Advisor FE	Yes	Yes	Yes	Yes
Style × Time FE	Yes	Yes	Yes	Yes
Observations	208,683	208,683	158,485	208,683
Adjusted R^2	0.388	0.385	0.412	0.385
High-Low (p-value)	0.0168	0.0402	0.257	0.0373

Table 14: Peer Dismissal, Performance, and Agency Problem

This table analyzes whether risk-adjusted fund performance improves following peer dismissal. Panel A estimates the regression model (14). The dependent variable is the change in a mutual fund's average risk-adjusted return from the year before to the year after peer dismissal. Panel B examines whether the effect of peer dismissal on performance increases with the intensity of mutual funds' agency problems (AP), by interacting a peer dismissal dummy with the composite AP measure used in Table 13. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Peer Dismissal and Performance

	(1)	(2)	(3)	(4)	(5)	(6)
Performance	CAPM		DGTW		Carhart	
Peer Dismissal	0.0882***	0.0987***	0.0446*	0.0555**	0.0655**	0.0738**
	(2.71)	(2.77)	(1.74)	(2.08)	(2.43)	(2.60)
logAssets	-0.0790***	-0.268***	-0.0680***	-0.213***	-0.0515***	-0.175***
	(-5.58)	(-5.94)	(-5.35)	(-6.03)	(-4.17)	(-6.11)
logAdvAssets	0.0124*	-0.0317**	0.0113**	-0.0251**	0.00869	-0.0166
	(1.96)	(-2.20)	(2.29)	(-2.24)	(1.47)	(-1.34)
logFundAge	0.123***	0.427***	0.0940***	0.237***	0.0940***	0.343***
	(5.41)	(4.26)	(5.52)	(3.62)	(5.55)	(5.01)
logExpRatio	-5.709	7.197	-1.706	-0.843	0.0937	16.29
	(-0.67)	(0.94)	(-0.34)	(-0.18)	(0.01)	(1.30)
logTurnRatio	-0.0498	0.237*	0.137	0.416***	-0.0626	0.119
	(-0.33)	(1.84)	(1.43)	(4.12)	(-0.71)	(1.46)
MgrPFund	0.00868***	0.00829	0.00703*	0.0114*	0.00607**	0.000409
	(3.07)	(1.35)	(1.95)	(1.76)	(2.03)	(0.07)
Fund FE	No	Yes	No	Yes	No	Yes
Style × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	195,148	195,148	194,201	194,201	195,145	195,145
Adjusted R^2	0.459	0.475	0.290	0.313	0.195	0.219

Panel B: Peer Dismissal and Performance - by Agency Problem

	(1)	(2)	(3)	(4)	(5)	(6)
Performance	CAPM		DGTW		Carhart	
Peer Dismissal						
× *Low AP*	0.0712*	0.0821*	0.0162	0.0310	0.0427	0.0577*
	(1.80)	(1.92)	(0.53)	(0.93)	(1.47)	(1.81)
× *Mid AP*	0.0679**	0.0904**	0.0348	0.0533*	0.0505	0.0594*
	(2.09)	(2.48)	(1.17)	(1.83)	(1.65)	(1.87)
× *High AP*	0.154**	0.141**	0.110**	0.0968*	0.132***	0.129**
	(2.40)	(2.16)	(2.00)	(1.81)	(2.65)	(2.56)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Fund FE	No	Yes	No	Yes	No	Yes
Style × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
High-Low (p-value)	0.220	0.391	0.108	0.251	0.0626	0.162
Observations	195,148	195,148	194,201	194,201	195,145	195,145
Adjusted R^2	0.459	0.475	0.290	0.313	0.195	0.219

Table 15: Performance Improvement Following Peer Dismissal through Effort Channel

This table analyzes whether risk-adjusted fund performance improves after peer dismissals due to managers exerting more effort. For each stock held by a mutual fund, I compute the distance between the firm's headquarters and the advisory firm's office. I then divide the stocks into two groups based on whether this distance is less than or greater than 50 miles. The changes in the mutual fund's average risk-adjusted return from one year before to one year after a peer dismissal are computed separately for each distance group. The regression model from equation (14) is run again separately for each group. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Performance	CAPM		DGTW		Carhart	
Advisor-Firm HQ Distance	$\geq 50\,miles$	$< 50\,miles$	$\geq 50\,miles$	$< 50\,miles$	$\geq 50\,miles$	$< 50\,miles$
Peer Dismissal	0.00536**	0.00815*	0.00114	0.00416	0.00347	0.00505
	(2.03)	(1.71)	(0.66)	(1.05)	(1.62)	(1.10)
logAssets	0.000224	0.00190	0.000304	0.00265**	0.000602	0.00279*
	(0.20)	(1.25)	(0.38)	(2.31)	(0.59)	(1.94)
logAdvAssets	-0.000562	-0.00137	-0.000542	-0.00191*	-0.000406	-0.000987
	(-0.65)	(-1.24)	(-0.89)	(-1.93)	(-0.53)	(-0.89)
logFundAge	-0.00262	0.00262	-0.00189	0.00213	-0.00256	0.00268
	(-1.10)	(0.81)	(-1.06)	(0.80)	(-1.21)	(0.86)
logExpRatio	-0.478	1.143	-0.0406	1.323**	-0.247	1.152
	(-0.73)	(1.40)	(-0.11)	(2.22)	(-0.50)	(1.54)
logTurnRatio	-0.127***	-0.114***	-0.0901***	-0.0794***	-0.112***	-0.102***
	(-12.46)	(-10.96)	(-15.37)	(-9.39)	(-13.96)	(-11.20)
MgrPFund	0.000476	-0.000719	0.0000846	-0.000704	0.000374	-0.000578
	(0.98)	(-0.96)	(0.21)	(-1.18)	(0.83)	(-0.91)
Style × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	158118	132227	158110	130861	158118	132227
Adjusted R^2	0.411	0.117	0.387	0.0865	0.339	0.110

Table 16: Residential Sorting on Confederate Properties

This table reports estimates from the following linear probability model:

$$Confederate_i = \alpha + \beta_1 Var + \beta_2 Control_i + FE_i + \epsilon_i.$$

Confederate is an indicator that is equal to one if the house is on a Confederate street and zero otherwise. *Var* is equal to one of four demographic variables: *Race (Black)*, an indicator equal to one if all the owners of the house identify as Black; *Registered Democrat*, an indicator equal to one if all the owners of the house are registered democrats; or *Education (Some College)*, an indicator equal to one if all the owners of the house have at least some college education, or *Demographic Score*, defined as the mean of *Black*, *Democrat*, and *Some College*. *Controls* include indicators for the specific number of bedrooms and bathrooms (up to five), and the natural logs of *Lot Size, House Size, Home Age, Owner Age*, and *Household Income*. Fixed Effects denote census block group fixed effects (Specification 1-6) or census block group × propensity score matched percentile fixed effects. Detailed variable definitions are provided in Appendix A. The *t*-statistics, computed from standard errors clustered at the census block-group level, are reported in parentheses. The sample includes 113,090 properties, of which 1,945 (1.72%) are located on Confederate streets.

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Black Resident	-0.53%			-0.44%			
	(-2.12)			(-1.75)			
College		-0.35%		-0.36%			
		(-2.75)		(-2.77)			
Democrat			-0.26%	-0.18%			
			(-2.42)	(-1.75)			
Demographic Score					-0.91%	-0.79%	-0.87%
					(-4.15)	(-3.74)	(-4.13)
Log (Income)						-0.15%	0.19%
						(-1.53)	-0.82
Log (Age)						-0.16%	0.10%
						(-0.87)	-0.39
Log (House Size)						-0.24%	0.21%
						(-0.86)	-0.52
Log (Home Age)						0.26%	-0.02%
						-1.52	(-0.04)
Log (Lot size)						0.33%	-0.23%
						-2.08	(-0.49)
Bed2						0.87%	-0.20%
						-1.08	(-0.15)
Bed3						0.69%	-0.13%
						-0.81	(-0.11)
Bed4						0.60%	-0.17%
						-0.69	(-0.16)
Bed5						1.10%	-0.21%
						-1.22	(-0.13)
Bath2						-1.03%	1.35%
						(-2.16)	-0.97
Bath3						-0.89%	1.13%
						(-1.58)	-0.89
Bath4						-0.99%	1.03%
						(-1.66)	-0.75
Bath5						-1.26%	1.41%
						(-1.77)	-0.81
Block Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Block Group × PSM Percentile FE	No	No	No	No	No	No	Yes

Table 17: Residential Sorting on Confederate vs. Confederate Adjacent Streets

This table reports estimates from the following regression:

$$Demographic_i = \alpha + \beta_1 Confed_i + \beta_2 ConfedAdj_i + Controls + FE_i + \epsilon_i.$$

Demographic is either *Race (Black)*, *Registered Democrat*, or *Education (Some College)*. We also consider a composite measure, *Demographic Score*, defined as the mean of the three demographic variables. *Confederate* is defined as in Table 16, and *Confederate Adjacent* is an indicator that is equal to one if the property is located within x miles of the closest Confederate property, where we set x equal to values ranging from 0.05 miles to 0.50 miles. The controls and fixed effects are identical to Specification 7 of Table 16. The t-statistics, computed from standard errors clustered at the census block-group level, are reported in parentheses. Below the regression estimates, we also test whether the *Confederate* and *Confederate Adjacent* coefficients are significantly different from each other.

Panel A: Demographic Score	[1]	[2]	[3]	[4]
Confed	-2.52%	-2.54%	-2.60%	-2.53%
	(-3.80)	(-3.77)	(-3.55)	(-3.14)
Confed Adjacent	-0.76%	-0.42%	-0.31%	-0.07%
	(-1.15)	(-0.98)	(-0.64)	(-0.13)
Confed - Adjacent	-1.76%	-2.12%	-2.29%	-2.46%
	(-2.33)	(-3.33)	(-3.70)	(-3.98)
Adjacent Distance (*miles*)	<0.05	<0.10	<0.25	<0.50
Controls & Fixed Effects	Yes	Yes	Yes	Yes

Panel B: Race (Black)	[1]	[2]	[3]	[4]
Confed	-1.86%	-1.90%	-1.80%	-1.95%
	(-1.97)	(-1.96)	(-1.63)	(-1.56)
Confed Adjacent	-0.20%	-0.42%	0.15%	-0.17%
	(-0.22)	(-0.58)	-0.18	(-0.19)
Confed - Adjacent	-1.66%	-1.48%	-1.95%	-1.78%
	(-1.78)	(-1.81)	(-2.48)	(-2.14)
Adjacent Distance (*miles*)	<0.05	<0.10	<0.25	<0.50
Controls & Fixed Effects	Yes	Yes	Yes	Yes

Panel C: Education (Some College)	[1]	[2]	[3]	[4]
Confed	-2.89%	-2.96%	-3.36%	-3.01%
	(-2.27)	(-2.29)	(-2.61)	(-2.23)
Confed Adjacent	-1.51%	-0.81%	-1.51%	-0.32%
	(-1.29)	(-1.22)	(-3.11)	(-0.67)
Confed - Adjacent	-1.38%	-2.15%	-1.85%	-2.69%
	(-0.83)	(-1.56)	(-1.42)	(-2.12)
Adjacent Distance (*miles*)	<0.05	<0.10	<0.25	<0.50
Controls & Fixed Effects	Yes	Yes	Yes	Yes

Panel D: Democrat	[1]	[2]	[3]	[4]
Confed	-2.81%	-2.79%	-2.63%	-2.62%
	(-2.85)	(-2.80)	(-2.50)	(-2.32)
Confed Adjacent	-0.54%	-0.04%	0.43%	0.28%
	(-0.45)	(-0.06)	-0.67	-0.45
Confed - Adjacent	-2.27%	-2.75%	-3.06%	-2.90%
	(-1.64)	(-2.56)	(-3.14)	(-3.07)
Adjacent Distance (*miles*)	<0.05	<0.10	<0.25	<0.50
Controls & Fixed Effects	Yes	Yes	Yes	Yes

Table 18: Confederate House Properties: Descriptive Statistics

This table reports descriptive statistics for sample of Confederate and control house sales. We identify sales of houses that are located on Confederate memorial streets over the 2001-2020 sample period using data from ATTOM. We select corresponding control sales that occurred in the same calendar quarter within the same census tract. Panel A reports distinct number of transactions, houses, and regional districts in the sample for Confederate and control sales. Panel B reports descriptive statistics of house characteristics, and Panel C reports the correlations across the house characteristics, where the continuous house characteristics (*Price, House Size, Age*, and *Lot Size*) are analyzed after taking natural logs. Variable definitions are provided in Appendix A.

Panel A: Sample Size

	Transactions	Houses	Streets	Block Groups	Tracts	Counties	States
Confederate	5,895	4,052	1,446	698	574	254	35
Controls	80,304	70,040	32,657	1,682	574	254	35

Panel B: Distribution of House Characteristics

	N	Mean	Std. Dev.	Skewness	p25	Median	p75
Confederate	86,199	0.07	0.25	3.42	0	0	0
Price ($)	86,199	241,911	268,160	8	119,500	180,000	280,000
House Size	86,199	1767	847	4	1223	1570	2105
Bedrooms	86,199	3.1	0.8	0.7	3	3	3
Bathrooms	86,199	2.2	0.9	0.9	2	2	3
Age (years)	86,199	31	25.7	0.8	10	25	50
Lot Size	86,199	17,146	25,218	6	6,761	10,000	16,160

Panel C: Correlation Matrix

	Confederate	*Price ($)*	*House Size*	*Bedrooms*	*Bathrooms*	*Age*	*Lot Size*
Confederate	1	-0.06	-0.05	-0.04	-0.05	0.11	0.03
Price		1	0.6	0.37	0.57	-0.24	0.14
House Size			1	0.61	0.72	-0.33	0.34
Bedrooms				1	0.57	-0.23	0.21
Bathrooms					1	-0.38	0.15
Age						1	0.07
Lot Size							1

Table 19: Difference in House Characteristics of Confederate and Control Houses

This table compares the house attributes of Confederate houses and control houses from the same census tract that sold in the same calendar quarter. Column 1 reports the mean difference between Confederate properties and non-Confederate properties prior to including any controls or fixed effects. Column 3 presents mean difference after controlling for other house attributes and benchmarking Confederate transactions to other transactions that occurred in the same census tract and calendar quarter. Specifically, for each house characteristics, we report the estimate of β from the following regression model:

$$Char_{i,t} = \alpha + \beta Confederate_{i,t} + \gamma X_{i,t} + FE + \epsilon_{i,t},$$

where $Char_{i,t}$ is house characteristics of house i in quarter t, $Confederate_{i,t}$ is an indicator variable that takes the value of 1 if house i is located on a Confederate street, and 0 otherwise, X includes all the house characteristics (*House Size, Lot Size, # Bedrooms, # Bathrooms, Age*) excluding the characteristic that is the dependent variable, and FE denotes census tract × quarter fixed effects. Column 5 is similar to Column 3 except that it replaces Census Tract × Quarter Fixed Effects with Census Tract × Quarter × Age Quintile Fixed Effects. Columns 2, 4, and 6 report the t-statistic testing whether the difference reported in the previous column is different from zero. The t-statistics are computed from standard errors clustered at the census tract level.

	No Controls		Tract × Qtr. FE & Controls		Tract × Qtr. × Age FE & Controls	
	Difference	t-stat	Difference	t-stat	Difference	t-stat
	[1]	[2]	[3]	[4]	[5]	[6]
Log (House Size)	-7.21%	(-3.59)	-1.44%	(-1.87)	-0.45%	(-0.63)
Log (Lot Size)	11.17%	(1.96)	4.42%	(1.67)	4.20%	(1.78)
# Bedrooms	-0.116	(-3.75)	-0.012	(-0.62)	-0.026	(-1.27)
# Bathrooms	-0.19	(-4.10)	-0.008	(-0.72)	-0.008	(-0.62)
Log (Age)	56.32%	(4.83)	8.52%	(2.41)	1.50%	(1.47)

Table 20: House Values and Confederate Street Names

This table reports the pricing effect of houses located on Confederate memorial streets. Specifically, it reports estimate for the following regression specification:

$$Log(Price)_{i,t} = \alpha + \beta Confederate_{i,t} + \gamma X_{i,t} + FE + \epsilon_{i,t},$$

where $Price_{i,t}$ is sale price, and $Confederate_{i,t}$ is an indicator variable that takes the value of 1 if the house is located on a Confederate street, and 0 otherwise, $X_{i,t}$ is a vector of house attributes that includes indicators for the specific number of bedrooms and bathrooms, and the natural logs of *Lot Size*, *House Size*, and *Age*, and FE denotes various fixed effects that we introduce across the models including indicators for the quarter in which the property sold (*Qtr.*), the census tract where the property sold (*Tract*), the interaction of tract and quarter (*Tract* × *Qtr.*), and the interaction of tract, quarter, and the quintile ranking of the house's age relative to all other houses that in sold in the same census tract quarter (*Tract* × *Qtr.* × *Age*). More detailed variable definitions are provided in Appendix A. The *t*-statistics, computed from standard errors clustered at the census tract level, are reported in parentheses.

	[1]	[2]	[3]	[4]
Confederate	-4.70%	-3.97%	-4.21%	-2.93%
	(-2.22)	(-3.57)	(-3.84)	(-2.76)
Log (House Size)	84.09%	58.95%	59.30%	57.90%
	(13.2)	(32.13)	(35.22)	(31.00)
Log (Age)	1.34%	-5.69%	-6.31%	-7.19%
	(0.7)	(-5.85)	(-7.94)	(-8.34)
Log (Lot Size)	-4.99%	9.92%	10.32%	10.34%
	(-2.14)	(12.53)	(13.29)	(12.38)
Bed2	-6.27%	3.86%	1.52%	2.51%
	(-0.67)	(1.59)	(0.62)	(1.07)
Bed3	-16.63%	7.22%	4.44%	4.35%
	(-1.61)	(2.69)	(1.67)	(1.72)
Bed4	-17.16%	6.53%	3.71%	4.05%
	(-1.58)	(2.44)	(1.4)	(1.55)
Bed $\geq$ *5*	-18.13%	3.30%	0.65%	2.71%
	(-1.55)	(1.06)	(0.21)	(0.89)
Bath2	31.98%	11.67%	12.45%	8.18%
	(8.75	(9.99)	(11.51)	(9.43)
Bath3	54.28%	20.83%	20.79%	14.10%
	(10.6)	(13.47)	(14.56)	(11.73)
Bath4	76.13%	31.90%	31.15%	22.94%
	(13.28)	(15.07)	(15.81)	(12.15)
Bath $\geq$ *5*	111.28%	46.30%	44.62%	37.81%
	(12.46)	(15.48)	(15.5)	(12.55)
Fixed Effects	Quarter	Tract and Qtr.	Tract × Qtr.	Tract × Qtr. × Age
Observations	86,199	86,199	86,199	86,199
R-squared	42.57%	76.59%	81.52%	88.06%
FE Groups	80	80 & 497	4,683	21,848

Table 21: Home Values and street names
Confederate vs. Adjacent Properties

This table reports the pricing effect of houses located on Confederate memorial streets and houses adjacent to confederate memorial streets. Specifically, it reports estimate for the following regression specification:

$$log(Price)_{i,t} = \beta_1 Confederate_{i,t} + \beta_2 Confed\,Adj_{i,t} + \gamma X_{i,t} + FE + \epsilon_{i,t}.$$

Confederate Adjacent is an indicator that is equal to one if the property is located within x miles of the closest Confederate property, where we set x equal to values ranging from 0.05 miles to 0.50 miles. All other variables are defined as in Table 20, and FE denotes census tract × quarter × age quintile fixed effects. Panel A reports the results for the full sample of transactions, and Panel B reports the results for the subsample of transactions for properties in Florida. More detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the census tract level, are reported in parentheses. Below the regression estimates, we also test whether the *Confederate* and *Confederate Adjacent* coefficients are significantly different from each other.

Panel A: Full Sample (All States)

	[1]	[2]	[3]	[4]
Confed	-2.91%	-3.06%	-3.15%	-3.09%
	(-2.73)	(-2.79)	(-2.74)	(-2.61)
Confed Adjacent	1.98%	-1.81%	-0.91%	-0.35%
	(1.05)	(-1.43)	(-1.12)	(-0.51)
Confed - Confed Adjacent	-4.89%	-1.25%	-2.24%	-2.74%
	(-2.39)	(0.98)	(-2.26)	(-2.68)
Tract × Quarter × Age Fixed FE and Controls as in Table 20				
Adjacent Distance (*miles*)	<0.05	<0.10	<0.25	<0.50
Confed Adjacent Obs.	305	1,880	9,345	24,532
Total Observations	86,199	86,199	86,199	86,199

Panel B: Florida Sample

	[1]	[2]	[3]	[4]
Confed	-4.51%	-4.59%	-4.33%	-4.09%
	(-1.87)	(-1.84)	(-1.65)	(-1.54)
Confed Adjacent	3.11%	-0.33%	0.96%	1.12%
	(0.75)	(-0.14)	(0.69)	(0.98)
Confed - Confed Adjacent	-7.62%	-4.27%	-5.30%	-5.22%
	(-1.72)	(-2.03)	(-2.65)	(-2.38)
Tract × Quarter × Age Fixed FE and Controls as in Table 20				
Adjacent Distance (*miles*)	<0.05	<0.10	<0.25	<0.50
Confed Adjacent Obs.	73	406	1,821	4,068
Total Observations	20,709	20,709	20,709	20,709

Table 22: Listing Outcomes for Confederate Street Names

This table reports other housing market outcomes for houses located on Confederate street memorials. Specifically, it reports estimates from the following regression:

$$Y_{i,t} = \alpha + \beta Confederate_{i,t} + \gamma X_{i,t} + FE + \epsilon_{i,t},$$

where $Y_{i,t}$ is equal to *Withdrawn*, an indicator that is equal to one if the house listing is subsequently withdrawn without selling; *Slow Sale*, an indicator that is one if the difference between the selling date (or withdrawal date) and the listing date is in the top quintile; or *Large Discount*, an indicator that is one if *log(Listing Price /End Price)* is in the top quintile of the distribution, where end price is defined as either the sales prices or the listing price on the date the property is withdrawn. $X_{i,t}$ includes the vector of house attributes in Table 20 plus the natural log of the initial listing price. FE denotes census tract × quarter fixed effects. Detailed variable definitions are provided in Appendix A. The *t*-statistics, computed from standard errors clustered at the census tract level, are reported in parentheses.

	Withdrawn [1]	Slow Sale [2]	Discount [3]
Confederate	1.11%	1.72%	2.01%
	(1.61)	(2.19)	(2.39)
Log (House Size)	2.85%	8.47%	2.53%
	(2.4)	(3.96)	(1.47)
Log (Age)	-0.10%	2.08%	5.72%
	(-0.20)	(2.77)	(8.22)
Log(Lot Size)	0.41%	-0.82%	-0.21%
	(0.96)	(-1.06)	(-0.35)
Bed2	4.47%	-1.26%	-3.70%
	(2.23)	(-0.36)	(-0.90)
Bed3	2.15%	-3.10%	-5.99%
	(1.04)	(-0.90)	(-1.48)
Bed4	2.08%	-3.45%	-5.27%
	(0.96)	(-0.99)	(-1.25)
Bed5	1.69%	-4.33%	-3.03%
	(0.72)	(-1.15)	(-0.65)
Bath2	-1.26%	-3.62%	-4.77%
	(-1.75)	(-3.99)	(-4.52)
Bath3	-1.84%	-3.75%	-4.89%
	(-1.98)	(-2.83)	(-3.68)
Bath4	-0.58%	-3.56%	-2.08%
	(-0.40)	(-2.01)	(-1.04)
Bath5	-0.32%	5.33%	6.04%
	(-0.16)	(1.6)	(1.83)
Log (List Price)	-0.20%	7.53%	5.79%
	(-0.16)	(5.07)	(4.83)
Fixed Effects	Tract × Qtr.	Tract × Qtr.	Tract × Qtr.
Observations	20,363	20,363	20,363
R-squared	21.11%	26.34%	26.42%

Table 23: House Value and Confederate Street Names - Regional Demographics

This table reports Confederate discounts conditional on regional demographics. We repeat Specification 4 of Table 20 after partitioning *Confederate* into *Confederate Low* and *Confederate High* based on different regional demographics. The low demographic group is defined as: counties with a smaller Black population (Specification 1), fewer democratic voters (Specification 2), a smaller fraction of college educated individuals (Specification 3), the 11 states that belonged to the Confederacy (Specification 4), or the five states with the largest number of Confederate statues (Specification 5), *High Demographic* refers to regions with high demographic levels for each specification. Specification 6 considers a composite measure computed as: *High Black Population* + *High Democrat* + *High College* + *Non-Top5 Statues*). *Low (High)* composite is an indicator equal to one if the composite score is less than (greater than) the median value of 2, and *Mid Composite* is an indicator equal to one if the composite score equals 2. Below the regression estimates, we also test whether the *High Demographic* and *Low Demographic* coefficients are significantly different from each other. Detailed variable definitions are provided in Appendix A. The *t*-statistics, computed from standard errors clustered at the census tract level, are reported in parentheses.

	[1]	[2]	[3]	[4]	[5]	[6]
Confed Low	-1.98%	-0.93%	-2.68%	-1.93%	0.64%	1.58%
	(-1.48)	(-0.64)	(-1.80)	(-1.36)	(0.35)	(0.87)
Confed High (Black)	-3.64%					
	(-2.48)					
Confed High (Democrat)		-4.61%				
		(-3.17)				
Confed High (College)			-3.18%			
			(-2.29)			
Confed High (Non-Confederate State)				-4.18%		
				(-2.71)		
Confed High (Non-Top5 Statues)					-4.33%	
					(-3.46)	
Confed Mid (Composite)						-2.57%
						(-1.73)
Confed High (Composite)						-5.52%
						(-3.30)
Confed High - Confed Low	-1.66%	-3.68%	-0.50%	-2.25%	-4.97%	-7.11%
	(-0.89)	(-1.83)	(-0.25)	(-1.08)	(-2.24)	(-2.88)
Observations	86,186	86,186	86,186	86,186	86,186	86,186
R-squared	88.05%	88.06%	88.05%	88.06%	88.05%	88.06%
Controls & Fixed Effects	Specification 4 of Table 20					

Table 24: House Values and Confederate Street Names – Shocks to Saliency

This table reports the house pricing effects of Confederate memorial streets following salient events that increased awareness of racial underpinnings of the Confederate flag. We consider three events that correspond to large spikes in attention to the Confederate flag. The three events correspond with the Charleston church shooting (June of 2015), the Charlottesville "Unite the Right" rally (August 2017), and widespread Black Lives Matter protests (June of 2020). We limit the sample to the [-12, +12] window, where period 0 is the month of the event. In Specification 1, we repeat the estimate of Equation (1) after interacting *Confederate* with *Post*, an indicator equal to one for the post-event window (i.e., months 1 through 12), and zero for the pre-event window. Specification 2 partitions *Confederate* × *Post* into *Confederate* × *PostQ1*, *Confederate* × *PostQ2*, *Confederate* × *PostQ3*, and *Confederate* × *PostQ4*, where *Confederate* × *PostQ1* is an indicator equal to one if the transaction occurred in the quarter (i.e., three-months) following the event, and *PostQ2–PostQ4* are defined analogously. Specification 3 augments Specification 2 by adding an interaction term for the quarter prior to the event (*Confederate* × *Pre Q1*), and Specification 4 decomposes *Confederate* × *PostQ1* into three separate indicators for each event (*Charleston*, *Charlottesville*, and *BLM Protests*). Detailed variable definitions are provided in Appendix A. The *t*-statistics, computed from standard errors clustered at the census tract level, are reported in parentheses.

	[1]	[2]	[3]	[4]
Confederate	-0.49%	-0.50%	-1.15%	-0.55%
	(-0.29)	(-0.30)	(-0.66)	(-0.33)
Confederate × *Post Event*	-4.22%			
	(-1.92)			
Confederate × *Post Q1*		-8.13%	-7.45%	
		(-2.38)	(-2.16)	
Confederate × *Post Q2*		-2.67%	-2.03%	
		(-0.84)	(-0.63)	
Confederate × *Post Q3*		0.53%	1.17%	
		-0.11	-0.24	
Confederate × *Post Q4*		-3.58%	-2.39%	
		(-1.09)	(-0.86)	
Confederate × *Pre Q1*			2.64%	
			-0.85	
Confederate × *Post Q1* × *Charleston*				-8.22%
				(-1.37)
Confederate × *Post Q1* × *Charlottesville*				-11.05%
				(-1.61)
Confederate × *Post Q1* × *BLM Protests*				-7.02%
				(-1.34)
Tract × Quarter × Age Fixed FE and Controls as in Table 20				
Period (in months)	[-12,12]	[-12,12]	[-12,12]	[-12,3]
Observations	31,795	31,795	31,795	21,712
R-squared	87.43%	87.44%	87.44%	88.25%

Table 25: House Values and Confederate School Name Changes

This table reports the pricing effect of Confederate memorial school changes. Specifically, the table reports estimates for the following difference-in-difference regression specification:

$$\begin{aligned} log(Price)_{i,t} = \beta_1 NameChg_i \\ + \beta_2 NameChg_i \times Post_{i,t} \\ + \gamma X_{i,t} + FE + \epsilon_{i,t}, \end{aligned}$$

where $NameChg_i$ equals one if house i is located in a school district that changed its name from a Confederate name to a non-Confederate name, and $Post_{i,t}$ equals one if house i is sold after the school's name change year, and 0 if it sold prior to the name change year. X includes controls for the specific number of bedrooms and bathrooms (up to five), and the natural log of *House Size* and *Age*, and FE denote zip code × quarter fixed effects and block fixed effects. In Specification 2. we replace *Name Change* × *Post* with *Name Change* × *Year (-2)*, *NameChange* × *Year (-1)*, *NameChange* × *Year (0)*, *NameChange* × *Year (+1)*, and *NameChange* × *Year (¿+1)*, where *Year (-2)*, is an indicator equal to one if the transaction occurred two year prior to the name change, and the other event-time indicators are defined analogously. We limit the sample to the [-3,3] window, and Year 0 (the year of the name change) is excluded from the analysis in Specification 1. The t-statistics, computed from standard errors clustered at the census tract level, are reported in parentheses.

	[1]	[2]
Name Change	-4.13%	-3.66%
	(-0.65)	(-0.74)
Name Change × *Post*	5.21%	
	(2.96)	
Name Change × *Year (-2)*		-1.41%
		(-0.65)
Name Change × *Year (-1)*		0.38%
		(0.17)
Name Change × *Year (0)*		3.96%
		(1.88)
Name Change × *Year (+1)*		3.84%
		(1.71)
Name Change × *Year (>+1)*		6.25%
		(3.14)
Control variables	Yes	Yes
QTR × ZIP FE	Yes	Yes
Block FE	Yes	Yes
Observations	17,794	21,929
R-squared	80.32%	86.37%

Table 26: House Choices and Confederate Street Names – Descriptive Statistics for the Experimental Sample

The table reports summary statistics for the experimental sample. Column 1 reports the full sample results across all 1000 participants. Columns 2-7 report the results for respondent subsets based on the primary variables of interest. Detailed variable definitions are presented in Appendix A. Panel A reports the fraction of the sample in each category. Panels B and C report the relative frequency of each choice category, where 20% is the null.

	Full Sample [1]	Neg. Confed. Sentiment [2]	Democrats [3]	College Educated [4]	Black [5]	Non-Confed. State [6]	Priming Article [7]
Panel A: Demographic Variables							
Observations	1,000	644	544	670	68	676	508
Neg. Confed. Sentiment	64.40%	100.00%	83.30%	66.40%	76.70%	67.60%	67.70%
Positive Confed Sentiment	3.60%	0.00%	1.10%	2.70%	8.80%	3.30%	3.30%
Democrat	54.40%	69.60%	100.00%	58.10%	57.40%	55.60%	53.90%
College Educated	67.00%	69.10%	71.50%	100.00%	63.20%	67.80%	65.60%
Black	6.80%	8.10%	7.20%	6.40%	100.00%	4.40%	6.70%
Non-Confederate State	67.60%	71.00%	69.10%	68.40%	44.10%	100.00%	66.40%
Priming Article	50.80%	53.42%	50.40%	49.70%	50.00%	49.90%	100.00%
Panel B: House Choice							
House #1	23.10%	23.20%	23.00%	23.60%	24.10%	22.80%	22.30%
House #2	21.60%	21.40%	21.80%	22.20%	23.40%	21.90%	22.00%
House #3	20.30%	20.40%	20.90%	20.20%	18.20%	20.40%	20.80%
House #4	18.10%	18.30%	18.40%	17.70%	19.40%	18.10%	17.40%
House #5	16.90%	16.60%	15.90%	16.30%	14.90%	16.80%	17.40%
Panel C: Street Choice							
Dixie	18.90%	18.20%	18.40%	18.70%	20.00%	18.40%	18.70%
Kenwood	20.20%	20.60%	21.10%	20.60%	21.90%	20.10%	20.00%
Gresham	20.00%	20.00%	19.70%	19.70%	21.20%	19.70%	20.30%
Juniper	20.70%	20.90%	20.90%	20.60%	18.20%	21.50%	20.30%
Linden	20.20%	20.40%	19.90%	20.40%	18.70%	20.30%	20.60%

Table 27: House Choices and Confederate Street Names – Experimental Evidence

The table examines whether survey participants are less likely to choose houses on Confederate streets. Specifically, the table reports estimates for variants of the following regression:

$$House\#1 = \beta_1 Dixie\,Dif + House\#FE_1 + House\#FE_2 + \epsilon_{i,t},$$

where $House\#1$, is an indicator equal to one if the participant reports preferring the first house (i.e., the house presented on the left) to the second house (i.e., the house presented on the right), *Dixie Dif* equals one if the first house is on Dixie Street, negative one if the second house is on Dixie Street, and zero if neither house is on Dixie Street, $House\#FE_1$ are a set of indicator dummies to indicate which house was the first (left) house seen by participants, and $House\#FE_2$ is defined analogously. Specifications 2-7 report results after replacing *Dixie Dif.* with *Dixie Dif. High Aversion* and *Dixie Dif. Low Aversion* where *High Aversion* is measured as either: *Negative Confederate Sentiment (Neg Confed)*, *Democrat*, *College-Educated*, *Black*, *Non-Confederate State*, or *Priming Article* and *Low Aversion* includes all participants not classified as *High Aversion.* More detailed variable definitions are in Appendix A. The t-statistics, computed from standard errors clustered by participant, are reported in parentheses.

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Intercept	48.90%	48.90%	48.90%	48.91%	48.89%	48.89%	48.88%
	(28.94)	(29.87)	(29.87)	(29.85)	(29.83)	(29.86)	(29.85)
Dixie Dif.	-2.65%						
	(-2.51)						
Dixie Dif. × *Neg. Confed*		-4.67%					
		(-3.55)					
Dixie Dif. × *Non-Neg. Confed*		1.01%					
		(0.58)					
Dixie Dif. × *Democrat*			-4.00%				
			(-2.81)				
Dixie Dif. × *Non-Democrat*			-1.09%				
			(-0.69)				
Dixie Dif. × *College*				-3.37%			
				(-2.67)			
Dixie Dif. × *No College*				-1.19%			
				(-0.62)			
Dixie Dif. × *Black*					0.96%		
					(0.25)		
Dixie Dif. × *Non-Black*					-2.91%		
					(-2.66)		
Dixie Dif. × *Non-Confed. State*						-4.32%	
						(-3.34)	
Dixie Dif. × *Confed State*						0.85%	
						(0.47)	
Dixie Dif. ×*Priming Article*							-3.03%
							(-1.98)
Dixie Dif. × *No Prime Article*							-2.26%
							(-1.55)
Coefficient Difference		-5.68%	-2.91%	-2.17%	3.87%	-5.17%	-0.77%
		(-2.59)	(-1.35)	(-0.95)	-0.95	(-2.33)	(-0.36)
Observations	10,000	10,000	10,000	10,000	10,000	10,000	10,000
House FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	2.21%	2.34%	2.25%	2.23%	2.23%	2.32%	2.22%

Table 28: Summary Statistics

This table presents the summary statistics of the main variables used in our empirical analysis. Panel A reports time-series averages of the cross-sectional summary statistics of (i) measures of ESG profile (see Section 2.1), (ii) measures of risk (see Section 2.2) and (iii) firm characteristics, in our main sample (firms with market value above the median NYSE market equity since 1991). All firm characteristics, i.e., control variables, are winsorized at the 1% and 99% level within each month. Panel B shows the correlations among our risk measures in our main sample. Panel C shows the number of stocks listed on NYSE, AMEX, and Nasdaq with nonmissing ESG data (in the prior year) within each size decile (based on NYSE breakpoints). Detailed variable definitions are provided in Appendix B.

Panel A: Time-series Averages of Cross-sectional Summary Statistics

Variable	T	N	Mean	STD	10th	25th	50th	75th	90th
ES Score	312	727	0.0249	0.4445	-0.4792	-0.2253	-0.0070	0.2730	0.5754
E Score	312	727	-0.0010	0.1230	-0.1206	-0.0220	0.0041	0.0359	0.1394
S Score	312	727	0.0258	0.3936	-0.4165	-0.2045	-0.0078	0.2426	0.5141
G Score	312	727	-0.0583	0.1443	-0.1853	-0.1383	-0.0569	0.0000	0.0859
MktCap ($ mil)	312	727	14,374	30,644	1,896	2,748	5,201	12,461	30,576
Beta	312	700	1.0030	0.4190	0.5323	0.7156	0.9451	1.2256	1.5568
Downside beta	312	700	1.0016	0.4659	0.4730	0.6894	0.9451	1.2538	1.6022
Rel. downside beta	312	700	-0.0014	0.2592	-0.2989	-0.1461	-0.0020	0.1441	0.2941
Coskewness	312	700	-0.1305	0.1339	-0.2988	-0.2203	-0.1316	-0.0406	0.0405
Tail risk	276	626	0.6972	0.5151	0.1234	0.3511	0.6339	0.9613	1.3386
Dividend dummy	312	721	0.7566	0.4073	0.1731	0.3846	1.0000	1.0000	1.0000
Book-to-Market	312	723	0.4289	0.2773	0.1317	0.2322	0.3789	0.5738	0.7895
Past 12 mth exret	312	724	0.1288	0.3129	-0.2159	-0.0639	0.0947	0.2749	0.4975
Past 12 mth ret STD	312	724	0.0211	0.0076	0.0131	0.0158	0.0194	0.0245	0.0316
Return on equity	312	723	0.0370	0.0769	-0.0089	0.0183	0.0361	0.0559	0.0883
Asset growth	312	722	0.1194	0.2398	-0.0534	0.0040	0.0665	0.1589	0.3243
Sales growth	312	722	0.1011	0.2370	-0.1002	-0.0087	0.0659	0.1597	0.3243
Leverage	312	722	1.5371	2.6626	0.1210	0.2758	0.6136	1.4022	3.9901

Panel B: Time-series Averages of Cross-sectional Correlation of Risk Measures

	Beta	Downside beta	Rel. downside beta	Coskewness	Tail risk
Beta	1.0000	0.8311	-0.1246	-0.0413	0.4828
Downside beta		1.0000	0.4291	-0.3901	0.4440
Rel. downside beta			1.0000	-0.6624	-0.0047
Coskewness				1.0000	-0.0603
Tail risk					1.0000

Panel C: MSCI Coverage by NYSE Market Capitalization Breakpoint

	NYSE Size Breakpoint Decile										
Year	1	2	3	4	5	6	7	8	9	10	Total
1991	9	9	25	35	48	68	87	91	132	120	624
1992	12	11	30	26	52	63	79	97	129	134	633
1993	11	12	23	25	48	67	69	107	122	143	627
1994	10	7	23	30	41	59	59	103	139	152	623
1995	8	11	32	21	33	62	64	94	137	164	626
1996	8	17	28	23	30	44	61	103	147	170	631
1997	9	12	29	27	29	37	67	85	157	180	632
1998	8	11	20	28	31	47	47	92	157	179	620
1999	11	15	22	28	32	42	57	89	155	177	628
2000	13	20	24	26	34	40	67	79	146	170	619
2001	13	23	23	41	76	139	196	203	183	163	1,060
2002	13	24	22	46	85	158	186	189	178	152	1,053
2003	387	553	373	310	255	217	184	189	180	153	2,801
2004	471	619	322	281	236	213	202	180	172	155	2,851
2005	450	577	354	280	249	201	192	187	169	156	2,815
2006	466	593	326	267	268	177	188	173	166	158	2,782
2007	339	555	391	302	225	191	195	167	164	150	2,679
2008	404	503	382	324	222	210	174	158	162	153	2,692
2009	611	446	349	255	218	197	161	169	161	151	2,718
2010	641	433	343	272	227	180	164	170	169	150	2,749
2011	518	447	286	294	210	175	178	165	165	147	2,585
2012	462	419	291	286	205	186	169	164	175	157	2,514
2013	154	333	315	256	221	186	191	163	166	159	2,144
2014	93	279	352	300	237	185	211	167	182	172	2,178
2015	54	265	335	286	237	216	205	180	176	174	2,128
2016	77	338	300	248	221	212	185	168	170	163	2,082
Total	5,252	6,532	5,020	4,317	3,770	3,572	3,638	3,732	4,159	4,102	44,094

Table 29: ES-sorted Portfolio Returns and Unconditional Market Risk

This table presents patterns of future 1-month returns and unconditional market risk for portfolios sorted on their past ES score. Panel A reports the average returns of the equal- and value-weighted portfolios over the next month from t to $t+1$, along with the return difference between the highest and the lowest past ES quintile portfolios in the column labeled "High-Low". Panel B repeats the same exercise as Panel A, except it sorts firms on their ES scores *within* each industry, based on two-digit Standard Industrial Classification (SIC) codes. The last row in each panel shows the average cross-sectional realized β of each quintile portfolio, along with the difference between "High-Low", where a stock's β is calculated using daily data over the next 12 months. t-statistic of "High-Low" return (β) is computed using 3 (12) Newey–West (1987) lags. *** 1%, ** 5%, * 10% significance.

Panel A: ES Sort

	Low	2	3	4	High	High-Low	t-stat
				Return (Equal-weighted)			
Excess return	0.0104	0.0105	0.0102	0.0105	0.0104	0.0000	0.04
CAPM alpha	0.0040	0.0036	0.0031	0.0040	0.0036	-0.0004	-0.33
3F alpha	0.0025	0.0023	0.0021	0.0029	0.0026	0.0001	0.11
4F alpha	0.0032	0.0037	0.0031	0.0035	0.0036	0.0004	0.37
				Return (Value-weighted)			
Excess return	0.0091	0.0097	0.0088	0.0090	0.0086	-0.0005	-0.37
CAPM alpha	0.0035	0.0035	0.0019	0.0026	0.0021	-0.0014	-1.03
3F alpha	0.0032	0.0031	0.0016	0.0024	0.0024	-0.0008	-0.68
4F alpha	0.0030	0.0039	0.0017	0.0019	0.0028	-0.0001	-0.10
Market Beta	0.9790	1.0128	1.0258	0.9904	1.0030	0.0240	1.01

Panel B: ES Sort Within Industry

	Low	2	3	4	High	High-Low	t-stat
				Return (Equal-weighted)			
Excess return	0.0103	0.0102	0.0104	0.0108	0.0103	0.0000	-0.04
CAPM alpha	0.0033	0.0033	0.0039	0.0042	0.0036	0.0004	0.42
3F alpha	0.0020	0.0019	0.0026	0.0031	0.0026	0.0007	0.84
4F alpha	0.0032	0.0031	0.0036	0.0037	0.0034	0.0002	0.28
				Return (Value-weighted)			
Excess return	0.0091	0.0092	0.0088	0.0090	0.0088	-0.0003	-0.31
CAPM alpha	0.0033	0.0027	0.0028	0.0025	0.0024	-0.0009	-0.77
3F alpha	0.0029	0.0024	0.0023	0.0027	0.0025	-0.0004	-0.42
4F alpha	0.0029	0.0027	0.0024	0.0024	0.0030	0.0000	0.03
Market Beta	1.0262	1.0057	0.9921	1.0032	0.9882	-0.0380***	-2.92

Table 30: ES-sorted Portfolio and Downside Market Risks

This table presents patterns of future downside risks for portfolios sorted on their past ES score. Panel A reports the average realized downside β, relative downside β, coskewness, and tail risk β of each portfolio, along with the differences between the highest and the lowest past ES quintile portfolios in the column labeled "High-Low". Panel B repeats the same exercise as Panel A, except it sorts firms on their ES scores *within* each industry, based on two-digit Standard Industrial Classification (SIC) codes. All risk measures are computed using daily data over the next 12 months, except tail risk β which is computed using data over the next 60 months. t-statistic of "High-Low" is computed using 12 Newey–West (1987) lags except tail risk β, for which we use 60 lags. *** 1%, ** 5%, * 10% significance.

Panel A: ES Sort

	Low	2	3	4	High	High-Low	t-stat
Downside beta	1.0028	1.0218	1.0210	0.9775	0.9801	-0.0227	-1.00
Rel downside beta	0.0238	0.0090	-0.0048	-0.0129	-0.0229	-0.0468***	-4.92
Coskewness	-0.1409	-0.1307	-0.1324	-0.1258	-0.1220	0.0189***	3.39
Tail risk	0.6784	0.7192	0.7241	0.6794	0.6863	0.0079	0.28

Panel B: ES Sort Within-industry

	Low	2	3	4	High	High-Low	t-stat
Downside beta	1.0309	1.0115	0.9972	0.9914	0.9764	-0.0545***	-4.18
Rel downside beta	0.0047	0.0058	0.0051	-0.0119	-0.0117	-0.0165***	-2.62
Coskewness	-0.1360	-0.1337	-0.1313	-0.1255	-0.1262	0.0098***	3.10
Tail risk	0.7116	0.7222	0.7003	0.6814	0.6725	-0.0391	-1.44

Table 31: Fama MacBeth Regression Analysis

This table shows the results of Fama–MacBeth (1973) regressions of realized risk exposure on past ES score, risk characteristics, and other firm characteristics. All independent variables are measured in a period before the realization of risk measures. In Panel A, we use future unconditional β and downside β as dependent variables. In Panel B, we use future downside risks—relative downside β, coskewness, and tail risk β—as dependent variables. We include industry fixed effects, based on two-digit Standard Industrial Classification (SIC) codes. The regressions are run monthly. Because unconditional β, downside β, relative downside β, and coskewness are computed over the next 12 months, we use 12 Newey–West (1987) lags for standard error; tail risk β is computed over the next 60 months, so we use 60 Newey–West lags. *** 1%, ** 5%, * 10% significance.

Panel A: Beta Measures

	Dependent Variables			
	Beta	Downside Beta	Beta	Downside Beta
ES Score	-0.0047	-0.0233***	-0.0010	-0.0171***
	(-0.91)	(-3.77)	(-0.21)	(-3.01)
lag(Beta)	0.6381***	0.5879***	0.4615***	0.3640***
	(21.28)	(18.23)	(17.28)	(11.41)
lag(Coskewness)	-0.0086	0.0300	-0.0836***	-0.0634*
	(-0.23)	(0.57)	(-3.02)	(-1.73)
lag(Rel down beta)	0.0166	0.0971***	-0.0361*	0.0263
	(0.68)	(3.27)	(-1.84)	(1.18)
lag(Tail risk)	0.0887***	0.1066***	0.0865***	0.0987***
	(7.02)	(6.35)	(7.90)	(6.62)
log(Size)	0.0036	-0.0072	0.0129*	0.0062
	(0.51)	(-1.12)	(1.72)	(0.98)
Asset Growth			0.0165*	0.0211*
			(1.76)	(1.89)
B/M			0.0256	0.0292
			(1.57)	(1.59)
$\mathbf{1}$(*Dividend*)			-0.0284**	-0.0226
			(-2.48)	(-1.59)
Lag(12mth exret)			0.0956***	0.1088***
			(3.17)	(3.22)
Lag(12mth ret std)			9.1411***	12.5405***
			(8.93)	(9.01)
Leverage			0.0072***	0.0120***
			(3.99)	(5.95)
ROE			-0.0781**	-0.1630***
			(-2.35)	(-2.77)
Sales Growth			0.0203	0.0163
			(1.38)	(1.02)
Industry FE	Yes	Yes	Yes	Yes
# of months	312	312	312	312
Mean (R^2)	0.71	0.56	0.75	0.60
Mean (# obs)	672	672	668	668

Panel B: Downside Risk Measures

	Dependent Variables					
	Relative Downside Beta	Coskewness	Tail Risk	Relative Downside Beta	Coskewness	Tail Risk
ES Score	-0.0186***	0.0125***	-0.0276**	-0.0161***	0.0120***	-0.0208**
	(-4.14)	(6.87)	(-2.28)	(-3.81)	(7.67)	(-2.01)
lag(Beta)	-0.0502***	0.0049	0.3602***	-0.0975***	-0.0065	0.2114***
	(-3.22)	(0.57)	(5.68)	(-6.47)	(-0.48)	(3.97)
lag(Coskewness)	0.0387	0.0476**	0.0737**	0.0202	0.0256**	0.0157
	(1.51)	(2.42)	(2.09)	(0.92)	(2.38)	(0.49)
lag(Rel down beta)	0.0805***	-0.0021	0.0905***	0.0624***	-0.0098*	0.0293
	(4.93)	(-0.22)	(5.42)	(4.76)	(-1.72)	(1.62)
lag(Tail risk)	0.0179**	-0.0158***	0.1209***	0.0122	-0.0146***	0.1216***
	(2.39)	(-3.79)	(8.58)	(1.48)	(-2.79)	(5.91)
log(Size)	-0.0108**	-0.0007	-0.0555***	-0.0067*	0.0004	-0.0396***
	(-2.38)	(-0.25)	(-4.50)	(-1.68)	(0.16)	(-3.61)
Asset Growth				0.0046	0.0067*	0.0233
				(0.54)	(1.81)	(1.40)
B/M				0.0036	-0.0022	0.1021**
				(0.27)	(-0.40)	(2.42)
1(*Dividend*)				0.0058	-0.0025	-0.0390
				(0.67)	(-0.93)	(-1.55)
Lag(12mth exret)				0.0132	-0.0004	0.0017
				(1.10)	(-0.08)	(0.07)
Lag(12mth ret std)				3.3994***	1.304	8.5756***
				(4.05)	(1.60)	(4.44)
Leverage				0.0048***	-0.0023***	0.0138***
				(4.78)	(-2.83)	(3.78)
ROE				-0.0850*	0.0390**	-0.2074*
				(-1.73)	(2.39)	(-1.81)
Sales Growth				-0.0040	-0.0030	-0.0307*
				(-0.44)	(-0.63)	(-1.71)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
# of months	312	312	276	312	312	276
Mean (R^2)	0.29	0.31	0.46	0.32	0.34	0.50
Mean (# obs)	672	672	603	668	668	599

Table 32: Panel Regression Analysis

This table shows the results of panel regressions of realized risk in each year—unconditional β, downside β, relative downside β, and coskewness—on past ES score. The observations are at firm-year level. We include all control variables included in Table 31, except including firm fixed effects in lieu of industry fixed effects. We also include year fixed effects. $\mathbf{1}(NegMktRet)$ ($\mathbf{1}(PosMktRet)$) is a dummy variable that is equal to 1 if the market's realized excess return in a given year is negative (positive), and 0 otherwise. Standard errors are double clustered by firm and time. *** 1%, ** 5%, * 10% significance.

Panel A: Aggregate ES effect

	Dependent Variables			
	Beta	Downside Beta	Relative	
Downside Beta	Coskewness			
ES Score	-0.0304	-0.0528**	-0.0224**	0.0147***
	(-1.68)	(-2.64)	(-2.46)	(3.10)
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
R^2	0.6678	0.5203	0.1902	0.6412
Nobs	17,299	17,299	17,299	17,299

Panel B: ES effect conditional on market excess return

	Dependent Variables			
	Beta	Downside Beta	Relative	
Downside Beta	Coskewness			
ES Score	-0.0298	-0.0498**	-0.0200**	0.0135***
$\times\mathbf{1}(PosMktRet)$	(-1.63)	(-2.44)	(-2.07)	(2.80)
ES Score	-0.0359	-0.0788*	-0.0429***	0.0246**
$\times\mathbf{1}(NegMktRet)$	(-0.84)	(-1.90)	(-3.91)	(2.49)
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
R^2	0.6678	0.5204	0.1903	0.6413
Nobs	17,299	17,299	17,299	17,299

Table 33: Fama MacBeth Regression Analysis - 12 months return

This table shows the results of Fama–MacBeth (1973) regressions of realized excess and DGTW-adjusted return over the next 12 months on past ES score. In the first (last) two columns, we control for realized market beta (downside and upside beta) computed over the next 12 months using daily returns. In all specifications, we control for realized return volatility and coskewness over the next 12 months, as well as past log-size, past book-to-market ratio, and past 12-month excess return. We include industry fixed effects, based on two-digit Standard Industrial Classification (SIC) codes. The regressions are run monthly. We use 12 Newey–West (1987) lags for standard error. *** 1%, ** 5%, * 10% significance.

	Dependent Variables: log(1+Cumulative 12 months return)			
	Excess Return	DGTW-adj Return	Excess Return	DGTW-adj Return
ES Score	0.0084*	0.0088**	0.0094**	0.0098**
	(1.84)	(1.99)	(1.99)	(2.15)
Beta	0.1709***	0.1761***		
	(5.18)	(6.02)		
Downside Beta			0.0751***	0.0679***
			(4.00)	(3.61)
Upside Beta			0.0702***	0.0792***
			(4.34)	(4.49)
log(MktCap)	-0.0193***	-0.0105***	-0.0179***	-0.0094***
	(-4.34)	(-4.09)	(-4.00)	(-3.99)
B/M	0.0058	-0.0050	0.0045	-0.0066
	(0.41)	(-0.34)	(0.32)	(-0.44)
lag(12mth exret)	-0.0109	-0.0180	-0.0078	-0.0160
	(-0.59)	(-1.10)	(-0.43)	(-1.05)
12mth ret std	-15.6415***	-13.8976***	-14.5843***	-12.6262***
	(-11.38)	(-13.47)	(-9.51)	(-10.57)
Coskewness	-0.0054	-0.0010	-0.0023	-0.0210
	(-0.20)	(-0.05)	(-0.06)	(-0.51)
Industry FE	Yes	Yes	Yes	Yes
# of months	312	312	312	312
Mean (R^2)	0.3934	0.3229	0.3938	0.3237
Mean (# obs)	693	672	693	672

Table 34: Fama MacBeth Regression Analysis - ES Score Decomposition and Climate Score

This table shows the results of Fama–MacBeth (1973) regressions analogous to those in the last three columns of Panel B of Table 31, which regresses the realized downside risks on past total ES score. In lieu of the total ES score, Panel A uses one ESG component at a time. Panel B uses total ES score, or one of its two components, while controlling for the G(overnance) score. Panel C uses, in lieu of the total ES score, the firm's climate change score, which is defined as the firm's clean energy strength minus its climate change concern score, both of which are part of the environment category in the MSCI KLD database. Note that focusing on the firm's climate change score reduces the sample period to only 2000–2013. *** 1%, ** 5%, * 10% significance.

Panel A: Separate Effect

	Dependent Variables								
	Relative Downside Beta			Coskewness			Tail Risk		
E Score	-0.0421***			0.0329***			-0.0848*		
	(-3.25)			(6.68)			(-1.90)		
S *Score*		-0.0153***			0.0114***			-0.0175	
		(-3.44)			(7.04)			(-1.55)	
G *Score*			-0.0137			-0.0033			-0.0237
			(-1.11)			(-0.51)			(-0.66)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# of months	312	312	312	312	312	312	276	276	276
Mean (R^2)	0.32	0.32	0.32	0.34	0.34	0.34	0.50	0.50	0.50
Mean (# obs)	668	668	668	668	668	668	599	599	599

Panel B: Controlling for Governance

	Dependent Variables								
	Relative Downside Beta			Coskewness			Tail Risk		
ES Score	-0.0160***			0.0122***			-0.0211**		
	(-3.83)			(7.73)			(-2.05)		
E Score		-0.0425**			0.0337***			-0.0832*	
		(-3.26)			(6.74)			(-1.88)	
S *Score*			-0.0150***			0.0116***			-0.0181
			(-3.47)			(7.11)			(-1.57)
G *Score*	-0.0094	-0.0112	-0.0103	-0.0061	-0.0048	-0.0055	-0.0205	-0.0248	-0.0208
	(-0.77)	(-0.90)	(-0.85)	(-0.92)	(-0.72)	(-0.84)	(-0.58)	(-0.73)	(-0.57)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# of months	312	312	312	312	312	312	276	276	276
Mean (R^2)	0.32	0.32	0.32	0.34	0.34	0.34	0.50	0.50	0.50
Mean (# obs)	668	668	668	668	668	668	599	599	599

Panel C: Climate Change Score

	Dependent Variables				
	Beta	Downside Beta	Relative		
Downside Beta	Coskewness	Tail Risk			
ClimateChg Score	-0.0041	-0.0128***	-0.0087**	0.0061**	-0.0192
	(-0.92)	(-2.74)	(-1.98)	(2.27)	(-1.11)
Control variables	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
# of months	168	168	168	168	168
Mean (R^2)	0.7690	0.6245	0.3230	0.3437	0.5266
Mean (# obs)	732	732	732	732	680

Table 35: Fama MacBeth Regression Analysis - Robustness Check

This table shows the results of Fama–MacBeth (1973) regressions analogous to those in Table 31, which uses firms with market value above median NYSE market equity during period 1992–2017. Instead, we use the alternative sample of all firms in the period after 2001. Panel A considers the same regressions in the last three columns of Panel B of Table 31. Panel B interacts ES performance with $\mathbf{1}(SmlCap)$ and $\mathbf{1}(BigCap)$, where $\mathbf{1}(SmlCap)$ ($\mathbf{1}(BigCap)$) is a dummy variable that is equal to 1 if the firm's market value is below (above) the median NYSE market equity, and 0 otherwise. *** 1%, ** 5%, * 10% significance.

Panel A: Full Sample

	Dependent Variables		
	Relative Downside Beta	Coskewness	Tail Risk
ES Score	-0.0100**	0.0094***	-0.0159**
	(-2.09)	(4.47)	(-2.28)
lag(Beta)	-0.1062***	-0.0006	0.2138***
	(-9.18)	(-0.06)	(4.65)
lag(Coskewness)	0.0151	0.0300**	-0.0427
	(0.69)	(2.36)	(-1.16)
lag(Rel down beta)	0.0425***	-0.0068	-0.0018
	(4.95)	(-1.41)	(-0.10)
lag(Tail risk)	0.0096	-0.0130***	0.1047***
	(1.23)	(-3.55)	(6.31)
log(Size)	0.0162***	-0.0099***	-0.0230*
	(3.77)	(-2.75)	(-1.90)
Asset Growth	-0.0092	0.0048	0.0216
	(-0.84)	(1.17)	(1.04)
B/M	0.0092	-0.0081**	0.0853**
	(0.74)	(-2.59)	(2.32)
$\mathbf{1}(Dividend)$	-0.0089	0.0001	-0.0290**
	(-1.59)	(0.05)	(-2.43)
Lag(12mth exret)	-0.0060	0.0065	-0.0247
	(-0.54)	(1.56)	(-1.34)
Lag(12mth ret std)	3.6785***	0.5969	3.7840***
	(3.93)	(1.07)	(4.53)
Leverage	0.0046***	-0.0014***	0.0216**
	(3.84)	(-3.06)	(2.00)
ROE	-0.0224	-0.0003	-0.1874***
	(-1.32)	(-0.06)	(-3.16)
Sales Growth	0.0015	0.0013	0.0026
	(0.18)	(0.67)	(0.35)
Industry FE	Yes	Yes	Yes
# of months	192	192	156
Mean (R^2)	0.20	0.24	0.33
Mean (# obs)	1,989	1,989	1,822

Panel B: Separate Estimation Based on Size

	Dependent Variables		
	Relative Downside Beta	Coskewness	Tail Risk
ES Score $\times\mathbf{1}$*(BigCap)*	-0.0159**	0.0143***	-0.0140*
	(-2.52)	(6.34)	(-1.85)
ES Score $\times\mathbf{1}$*(SmlCap)*	-0.0053	0.0017	-0.0227
	(-0.46)	(0.33)	(-0.89)
lag(Beta)	-0.1067***	-0.0004	0.2133***
	(-9.38)	(-0.03)	(4.65)
lag(Coskewness)	0.0174	0.0290**	-0.0402
	(0.80)	(2.30)	(-1.11)
lag(Rel down beta)	0.0425***	-0.0067	-0.0019
	(4.97)	(-1.40)	(-0.10)
lag(Tail risk)	0.0093	-0.0130***	0.1043***
	(1.19)	(-3.54)	(6.37)
log(Size)	0.0155***	-0.0096***	-0.0232*
	(3.57)	(-2.64)	(-1.94)
Asset Growth	-0.0088	0.0046	0.0221
	(-0.79)	(1.11)	(1.06)
B/M	0.0094	-0.0082***	0.0860**
	(0.75)	(-2.66)	(2.35)
$\mathbf{1}$*(Dividend)*	-0.0088	0.0001	-0.0287**
	(-1.56)	(0.03)	(-2.45)
Lag(12mth exret)	-0.0056	0.0063	-0.0245
	(-0.51)	(1.51)	(-1.34)
Lag(12mth ret std)	3.7109***	0.5832	3.8088***
	(3.96)	(1.05)	(4.58)
Leverage	0.0045***	-0.0014***	0.0215**
	(3.82)	(-3.05)	(2.00)
ROE	-0.0217	-0.0007	-0.1880***
	(-1.28)	(-0.13)	(-3.17)
Sales Growth	0.0012	0.0014	0.0024
	(0.15)	(0.68)	(0.33)
Industry FE	Yes	Yes	Yes
# of months	192	192	156
Mean (R^2)	0.20	0.24	0.33
Mean (# obs)	1,989	1,989	1,822

Table 36: Doing Well by Doing Good – News Sentiment Patterns

We measure daily news sentiment for each firm as the average of RavenPack's sentiment scores across all news for each firm-day observation. We filter for news stories in which the firm was prominent (i.e., a relevance score of 100), and for the first story that reports a same categorized event (i.e., a novelty score of 100). Note that focusing on RavenPack's firm-level news sentiment data reduces the sample period to 2000–2017.
This table shows the results of our analysis using the portfolio news sentiment measures by examining the quintile portfolios sorted on their past ES scores, as detailed in Section 4.1.2. Note that we value-weight firm-level news sentiment to construct portfolio news sentiment. Panel A shows the results of Fama–MacBeth (1973) regressions of daily portfolio excess returns on contemporaneous, daily portfolio news sentiment, where we compute the t-statistics by using 5 Newey–West (1987) lags. We construct sentiment-based measures of downside covariation in the same way as the corresponding measures based on stock returns. Panel B reports the time-series averages of relative sentiment downside betas and sentiment unconditional betas for each quintile portfolio. Panel C conducts the same analysis as in Panel B, except it sorts firms on their ES scores *within* each industry, based on two-digit Standard Industrial Classification (SIC) codes. All the t-statistics in Panels B and C are computed using 12 Newey–West lags.

Panel A: Fama MacBeth Regression of Portfolio Excess Return on Portfolio Sentiment

	ES Sort		ES Sort Within-industry	
Return	*Equal Weighted*	*Value Weighted*	*Equal Weighted*	*Value Weighted*
Intercept	-0.0102***	-0.0203***	-0.0055***	-0.0173***
	(-4.65)	(-6.49)	(-3.77)	(-6.81)
AggSent	0.0002***	0.0004***	0.0001***	0.0004***
	(4.87)	(6.58)	(4.07)	(6.95)
N (# of days)	4,528	4,528	4,528	4,528
R^2	0.25	0.26	0.26	0.26

Panel B: Sentiment Beta Analysis - ES Sort

	Low	2	3	4	High	High-Low	t-stat
Beta	1.2274	0.9949	0.8714	0.8152	0.9238	-0.3036***	-4.67
Rel. Downside Beta	0.1329	-0.0126	0.0529	0.0088	-0.1573	-0.2901***	-5.96

Panel C: Sentiment Beta Analysis - ES Sort within Industry

	Low	2	3	4	High	High-Low	t-stat
Beta	1.2523	1.0323	0.8701	0.8242	0.9343	-0.3180***	-4.14
Rel. Downside Beta	0.1547	-0.0195	-0.0117	-0.0516	-0.1144	-0.2691***	-3.88

Table 37: ES Preferences of Institutional Investors – Trading Patterns

We measure daily institutional trading for each firm using Abel Noser institutional trading data. For each firm on a given day, we calculate the aggregate net shares traded by institutional investors, then scale the trading imbalance by focusing on its direction: 1 for net institutional buying, -1 for net selling, and 0 for zero net position. Note that focusing on Abel Noser's institutional trading data reduces the sample period to January 1999–January 2010.

This table shows the result of Fama-MacBeth (1973) regression of realized institutional trading β on past ES score, risk characteristics, and other firm characteristics. All independent variables are measured in a period before the realization of institutional trading pattern. For each firm, trading βs are computed using the direction of daily aggregate institutional trading over the next 12 months, as detailed in Section 4.1.2. We include industry fixed effects, based on two-digit Standard Industrial Classification (SIC) codes. The regressions are run monthly. We use 12 Newey–West (1987) lags for standard error. *** 1%, ** 5%, * 10% significance.

Dependent Variable	Trading Beta	Downside Trading Beta	Rel. Downside Trading Beta	Downside Trading Beta	Rel. Downside Trading Beta
Downside criteria		$MktEx_t < \overline{Daily\_MktEx}$		$MktEx_t <$ 25th $Daily\_MktEx$	
ES Score	0.2151	-0.0618	-0.2770	-1.2958**	-1.5109**
	(1.36)	(-0.24)	(-0.76)	(-1.98)	(-2.16)
lag(Beta)	0.6908***	1.7067**	1.0159	1.4032	0.7124
	(2.95)	(2.39)	(1.35)	(1.62)	(0.77)
lag(Coskewness)	-0.5191	-0.8699	-0.3507	2.9802	3.4994
	(-0.54)	(-0.55)	(-0.27)	(0.97)	(1.10)
lag(Rel down beta)	-0.6993**	0.1576	0.8569*	-0.0890	0.6103
	(-2.18)	(0.30)	(1.81)	(-0.10)	(0.68)
lag(Tail risk)	0.3475***	0.3014	-0.0461	0.7154	0.3679
	(3.88)	(1.26)	(-0.20)	(1.45)	(0.79)
log(Size)	0.5138***	0.2851	-0.2288*	-0.3467	-0.8606***
	(5.29)	(1.42)	(-1.84)	(-1.36)	(-3.44)
Asset Growth	0.1131	0.0910	-0.0221	0.6501	0.5370
	(0.46)	(0.22)	(-0.04)	(0.67)	(0.60)
B/M	-0.8282***	-0.9339	-0.1057	-2.5668**	-1.7386
	(-2.65)	(-1.54)	(-0.25)	(-2.07)	(-1.56)
$\mathbf{1}(Dividend)$	-0.0026	-0.3889	-0.3863	0.3405	0.3432
	(-0.02)	(-1.06)	(-1.27)	(0.57)	(0.63)
Lag(12mth exret)	0.1626	-0.2086	-0.3713	-0.2329	-0.3955
	(0.88)	(-0.34)	(-0.62)	(-0.30)	(-0.49)
Lag(12mth ret std)	31.8699**	17.7926	-14.0774	0.4765	-31.3935
	(2.60)	(0.42)	(-0.38)	(0.01)	(-0.89)
Leverage	0.1471***	0.3330***	0.1859***	0.4405***	0.2934***
	(3.65)	(6.93)	(5.55)	(3.39)	(2.64)
ROE	0.7760*	3.3676**	2.5916*	2.5014	1.7254
	(1.81)	(2.10)	(1.69)	(0.85)	(0.61)
Sales Growth	0.0031	0.5117	0.5086	1.8231**	1.8200**
	(0.01)	(0.92)	(0.98)	(2.21)	(2.39)
Industry FE	Yes	Yes	Yes	Yes	Yes
# of months	133	133	133	133	133
Mean (R^2)	0.15	0.12	0.12	0.12	0.12
Mean (# obs)	696	696	696	696	696

Table 38: Sustainalytics

This table shows the results of Fama–MacBeth (1973) regressions of realized risk exposure on past ESG profile from Sustainalytics. Following our main analysis in Table 31, we include the same control variables, and focus on big firms (i.e., market value above median NYSE market equity), except we use Sustainalytics' total ESG score in lieu of ES score constructed from KLD. The first three columns in each panel use the scores as it is, while the last three columns use their natural logarithms. In Panel B, we use subset of firms after excluding those with negative book value. The sample period is September 2009–December 2017. Industry fixed effects are based on two-digit Standard Industrial Classification (SIC) codes. The regressions are run monthly. Because relative downside β and coskewness are computed over the next 12 months, we use 12 Newey–West (1987) lags for standard error; tail risk β is computed over the next 60 months, so we use 60 Newey–West lags. *** 1%, ** 5%, * 10% significance.

Panel A: Full Sample

	Dependent Variables					
	Relative Downside Beta	Coskewness	Tail Risk	Relative Downside Beta	Coskewness	Tail Risk
	Raw Score			*Log Score*		
ESG Score	-0.0001	0.0004*	0.0013	-0.0125	0.0212**	0.0677
	(-0.61)	(1.96)	(1.19)	(-1.37)	(1.99)	(1.07)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
# of months	100	100	64	100	100	64
Mean (R^2)	0.34	0.37	0.56	0.34	0.37	0.56
Mean (# obs)	651	651	622	651	651	622

Panel B: Excluding Firms with Negative Book Value

	Dependent Variables					
	Relative Downside Beta	Coskewness	Tail Risk	Relative Downside Beta	Coskewness	Tail Risk
	Raw Score			*Log Score*		
ESG Score	-0.0002	0.0003*	0.0014	-0.0152*	0.0191*	0.0725
	(-1.01)	(1.72)	(1.33)	(-1.75)	(1.76)	(1.23)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
# of months	100	100	64	100	100	64
Mean (R^2)	0.35	0.38	0.57	0.35	0.38	0.57
Mean (# obs)	633	633	607	633	633	607

Appendix A Variable Definitions for Peer Dismissal and Contagious Career Concerns

A.1 Fund Variables (Table 1)

- *Risk taking*^{intended} – The difference between the intended volatility in the current quarter and the realized volatility in the previous quarter. See Equation (2) [Source: CRSP and Thomson Reuters (S12)].
- *Cum. 1-Year Return (Flow)* – Past one-year raw return (flow) [Source: CRSP].
- *MS Rating* – Morningstar Rating that ranges from 1 to 5 [Source: Morningstar].
- *Expense Ratio* – Expense ratio, calculated as value-weighting class-level expense ratios based on the previous quarter-end TNAs [Source: CRSP].
  - *Turnover Ratio* is computed analogously using the class-level turnover ratio.
- *Fund Size ($ bil)* – Total net assets (TNAs), calculated as the sum of the class-level TNAs as of the previous quarter-end [Source: CRSP].
- *Fund Age (Year)* – Fund age, based on the oldest fund class as of the previous quarter [Source: CRSP and Morningstar].
- *MgrPFund* – Number of fund managers who oversee the fund at the end of the previous quarter [Source: Morningstar].

A.2 Manager Variables (Table 1)

- *Peer Dismissal* – An indicator variable equal to 1 if the manager's peer in the same advisory firm is dismissed in the previous quarter [Source: EDGAR and Morningstar].
- *Age* – Fund manager's age as of the previous quarter-end [Source: Morningstar and Web Search].
  - *MBA, Female*, and *CFA* are collected using similar sources [Source: Morningstar and Web Search].
- *Experience (Year)* – Number of years since the manager first appeared in the Morningstar

dataset [Source: Morningstar].

- *Advisor Size ($ bil)*: Sum of the total assets managed by the manager's firm [Source: CRSP, EDGAR, and Morningstar].

A.3 Performance Variables (Table 3)

- *Return* – Past one-year fund raw return, net of fees [Source: CRSP].
- *Return Percentile* – Percentile of past one-year fund raw return net of fees, sorted within each fund style (Morningstar category) [Source: CRSP and Morningstar].
- *Low terciles* – An indicator variable equal to 1 if the fund performance is in the bottom terciles within each fund style (Morningstar category) [Source: CRSP and Morningstar].
- *Composite* – The sum of three indicators of *Low terciles*, which ranges from 0 to 3 [Source: CRSP and Morningstar].

A.4 Social Ties Variables (Table 6)

- *Education* – An indicator variable that equals 1 if the manager graduated from the same college as the dismissed peer [Morningstar and Web Search].
- *Age* – An indicator variable that equals 1 if the manager and the dismissed manager's age difference is below the median among the group of peer-dismissed managers. This corresponds to the age difference of less than six years [Morningstar and Web Search].
- *Experience* – An indicator variable that equals 1 if the manager spent more than 50% of their tenure with the dismissed manager [Morningstar and Web Search].
- *Composite* – The sum of three indicators of *Education*, *Age*, and *Experience* [Morningstar and Web Search].

A.5 Alternative Dependent and Independent Variables (Table 7)

- $Risk\ taking^{realized}$ – The difference between the realized volatility in the current quarter and the previous quarter. See Equation (7) [Source: CRSP].
- $Risk\ taking^{ratio}$ – The ratio of the intended volatility in the current quarter to the realized

volatility in the previous quarter. See Equation (8) [Source: CRSP and Thomson Reuters (S12)].

- *# Peer Dismissal* – The number of managers dismissed from the manager's advisory firm in the previous quarter [Source: EDGAR and Morningstar].
- *Peer Advancement* – An indicator variable equal to 1 if the manager's peer in the same advisory firm leaves the firm in the previous quarter following a good fund performance [Source: EDGAR and Morningstar].
- *Peer Demotion* – An indicator variable that equals 1 if the manager's peer in the same advisory firm ceases to manage all funds in the previous quarter following a bad fund performance, without leaving the firm. [Source: EDGAR and Morningstar].

A.6 Subgroup Variables (Table 9)

- *Large cap* – An indicator variable equal to 1 if the fund style covers large cap stocks, that is, the fund's Morningstar Category is large-value, -blend, or -growth [Source: Morningstar].
- *Small size* – An indicator variable equal to 1 if the fund size is in the bottom terciles of the previous quarter [Source: CRSP].
- *Negative flow* – An indicator variable equal to 1 if the fund's past one-year flow is negative [Source: CRSP].

Appendix B Performance and Firm Departure

In this Appendix, I examine the relationship between past fund performance and manager dismissal. I use all CRSP-Morningstar matched funds domiciled in the U.S., including non-equity funds (e.g., bond mutual funds).

In Table A1, I run the following linear probability model:

$$\begin{aligned} Firm\ Departure_{j,t} = {} & \beta_1 Return_{i,j,t-4} + \beta_2 MS\ Rating_{i,j,t-1} + \beta_3 Flow_{i,j,t-4} \\ & + X\Gamma_{i,j,t-1} + \delta_{style(i),t} + \epsilon_{i,j,t}, \end{aligned} \tag{5}$$

where i denotes fund, j manager, and t calendar quarter. The dependent variable, $Firm\ Departure_{j,t}$, is an indicator for firm departure of manager j. The independent variables of interest are $Return_{i,j,t-4}$, $Flow_{i,j,t-4}$, and $MS\ Rating_{i,j,t-1}$, which are past one-year raw return, fund flow,[33] and previous-quarter MS Rating of fund i managed by manager j, respectively. $\Gamma_{i,j,t-1}$ is a set of fund and manager characteristics.

In Table A2, I create a composite measure of fund underperformance $Low\ Performance$, which is the sum of indicators of past one-year return and flow and MS Ratings that are in the bottom terciles within each fund style in each quarter. In column 1, I confirm that this composite measure effectively captures my prior results in Table A1: the marginal rate of dismissal monotonically increases with this measure. For example, fund managers with $Low\ Performance = 3$ are 3.8 percentage points more likely to leave the firm relative to fund managers with $Low\ Performance = 0$, the reference group. In the next six columns, I repeat the same estimation within mutual funds of the same asset type. I find a similar relationship between performance and departure within all these subsamples.

In Table A3, I examine whether the relationship between firm departure and prior underperformance varies with manager characteristics. I find that female and less experienced managers are more likely to be fired for underperformance. In contrast, managers with an MBA degree are less likely to be fired for poor performance.

In Table A4, I analyze whether managers face greater career concerns following bear markets. I find

[33] The fact that flow negatively predicts departure, even after accounting for the effect of fund performance, is consistent with Kostovetsky and Warner (2015).

that unconditionally, fund managers are more likely to be fired during a recession. Additionally, the increased career concerns are focused on managers with underperformance.

Table A1: Performance and Firm Departure

This table examines the relationship between past fund performance and manager dismissal. Using the sample of funds domiciled in the U.S., including non-equity funds, the table reports estimates for the weighted least squares (WLS) model as in Equation (5). The dependent variable, $Firm\ Departure_{j,t}$, is an indicator for firm departure of manager j. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Raw Return		MS Rating		Fund Flow		All Performances	
Return (t-1)	-0.147***	-0.129***					-0.116***	-0.113***
	(-7.65)	(-7.71)					(-6.43)	(-6.59)
Return (t-2)	-0.107***	-0.0895***					-0.0720***	-0.0679***
	(-6.36)	(-5.51)					(-4.10)	(-3.87)
Return (t-3)	-0.107***	-0.0884***					-0.0877***	-0.0814***
	(-6.69)	(-5.94)					(-4.86)	(-4.76)
Return (t-4)	-0.114***	-0.0923***					-0.0768***	-0.0710***
	(-8.10)	(-7.04)					(-4.90)	(-4.65)
$\mathbb{1}[MS\ Rating = 1]$			0.0392***	0.0302***			0.0283***	0.0217***
			(14.52)	(11.77)			(11.12)	(9.01)
$\mathbb{1}[MS\ Rating = 2]$			0.0245***	0.0190***			0.0172***	0.0131***
			(16.01)	(13.09)			(11.48)	(9.19)
$\mathbb{1}[MS\ Rating = 3]$			0.0131***	0.00901***			0.00737***	0.00397***
			(11.59)	(7.85)			(6.72)	(3.62)
$\mathbb{1}[MS\ Rating = 4]$			0.00414***	0.00222**			0.000778	-0.000665
			(4.87)	(2.44)			(0.89)	(-0.75)
Flow (t-1)					-0.0127***	-0.0107***	-0.0131***	-0.0111***
					(-11.59)	(-10.34)	(-10.90)	(-9.26)
Flow (t-2)					-0.00670***	-0.00572***	-0.00449***	-0.00385***
					(-6.75)	(-5.91)	(-3.14)	(-2.75)
Flow (t-3)					-0.00354***	-0.00318***	-0.00308**	-0.00275*
					(-3.46)	(-3.18)	(-2.01)	(-1.88)
Flow (t-4)					-0.00410***	-0.00395***	-0.00509***	-0.00448***
					(-6.14)	(-5.91)	(-4.52)	(-4.23)
Control Variables	No	Yes	No	Yes	No	Yes	No	Yes
Style × Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	840,190	840,190	580,145	580,145	798,099	798,099	532,818	532,818
Adjusted R^2	0.0170	0.0423	0.0233	0.0518	0.0158	0.0518	0.0250	0.0593

Table A2: Firm Departure by Asset Types

This table examines the relationship between past fund performance and manager dismissal within mutual funds of the same asset type. The dependent variable, $Firm\ Departure_{j,t}$, is an indicator for firm departure of manager j. $Low\ Performance$ is the sum of indicators of past one-year return and flow and MS Ratings that are in the bottom terciles within each fund style in each quarter. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Full Sample	(2) US Equity	(3) Balanced	(4) International	(5) Corp.Bond	(6) Muni.Bond	(7) Others
$\mathbb{1}[Low\ Performance = 1]$	0.00691***	0.00471***	0.00743***	0.00898***	0.00951***	0.00705***	0.00575*
	(10.11)	(4.58)	(3.66)	(4.23)	(5.01)	(2.96)	(1.91)
$\mathbb{1}[Low\ Performance = 2]$	0.0189***	0.0180***	0.0178***	0.0290***	0.0166***	0.0139***	0.0200***
	(15.93)	(10.36)	(5.22)	(9.18)	(5.63)	(4.44)	(5.71)
$\mathbb{1}[Low\ Performance = 3]$	0.0380***	0.0452***	0.0331***	0.0498***	0.0239***	0.0201***	0.0455***
	(15.51)	(12.29)	(5.34)	(8.02)	(5.71)	(3.78)	(6.25)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Style $\times$ Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	532,818	209,768	67,637	68,342	98,359	53,848	34,864
Adjusted R^2	0.0593	0.0533	0.0393	0.0854	0.0614	0.0655	0.0833

Table A3: Firm Departure and Manager Characteristics

This table examines whether the relationship between firm departure and prior underperformance varies with manager characteristics. The dependent variable, $Firm\ Departure_{j,t}$, is an indicator for firm departure of manager j. $Low\ Performance$ is the sum of indicators of past one-year return and flow and MS Ratings that are in the bottom terciles within each fund style in each quarter. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
		Manager Characteristics		
		Female	log(1+Experience)	MBA
$\mathbb{1}[Low\ Performance = 1]$	0.00691***	0.00679***	0.0124***	0.00782***
	(10.11)	(9.71)	(5.93)	(8.36)
$\mathbb{1}[Low\ Performance = 2]$	0.0189***	0.0189***	0.0264***	0.0193***
	(15.93)	(15.29)	(8.72)	(12.76)
$\mathbb{1}[Low\ Performance = 3]$	0.0380***	0.0372***	0.0631***	0.0410***
	(15.51)	(15.40)	(11.30)	(12.99)
$\mathbb{1}[Low\ Performance = 1] \times Manager\ Char.$		0.00239	-0.00256***	-0.00170
		(0.75)	(-2.86)	(-1.38)
$\mathbb{1}[Low\ Performance = 2] \times Manager\ Char.$		-0.00193	-0.00352***	-0.000765
		(-0.41)	(-2.69)	(-0.42)
$\mathbb{1}[Low\ Performance = 3] \times Manager\ Char.$		0.0167*	-0.0119***	-0.00586*
		(1.77)	(-5.09)	(-1.68)
Control Variables	Yes	Yes	Yes	Yes
Style × Time FE	Yes	Yes	Yes	Yes
Observations	532,818	532,818	532,818	532,818
Adjusted R^2	0.0593	0.0594	0.0595	0.0594

Table A4: Firm Departure and Bear Markets

This table examines whether managers face greater career concerns following bear markets. The dependent variable, $Firm\ Departure_{j,t}$, is an indicator for firm departure of manager j. *Low Performance* is the sum of indicators of past one-year return and flow and MS Ratings that are in the bottom terciles within each fund style in each quarter. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Recession = 1 if market return is						*MktRet*	
	Bottom 5%		Bottom 10%		Bottom 25%			
Recession	0.00942***	0.00608	0.00942***	0.00321	0.00586***	0.00195	-0.0133**	-0.00373
	(2.63)	(1.57)	(2.63)	(1.26)	(2.79)	(1.26)	(-2.13)	(-0.82)
$\mathbb{1}[Low\ Performance = 1]$	0.00485***	0.00472***	0.00499***	0.00464***	0.00503***	0.00449***	0.00507***	0.00522***
	(7.85)	(7.66)	(7.96)	(7.20)	(8.09)	(6.56)	(8.05)	(6.98)
$\mathbb{1}[Low\ Performance = 2]$	0.0153***	0.0149***	0.0155***	0.0140***	0.0156***	0.0135***	0.0156***	0.0174***
	(14.38)	(13.54)	(13.73)	(13.88)	(14.01)	(12.53)	(13.99)	(12.11)
$\mathbb{1}[Low\ Performance = 3]$	0.0351***	0.0349***	0.0353***	0.0330***	0.0353***	0.0314***	0.0353***	0.0394***
	(14.68)	(14.20)	(14.44)	(14.24)	(14.54)	(12.48)	(14.54)	(13.26)
$\mathbb{1}[Low\ Performance = 1] \times Recession$		0.00249		0.00223		0.00163		-0.00438
		(0.61)		(0.99)		(1.02)		(-1.07)
$\mathbb{1}[Low\ Performance = 2] \times Recession$		0.0114***		0.0182***		0.00937***		-0.0212**
		(3.35)		(2.88)		(2.67)		(-2.36)
$\mathbb{1}[Low\ Performance = 3] \times Recession$		0.00645		0.0285**		0.0189***		-0.0483**
		(0.76)		(2.28)		(2.96)		(-2.61)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	532,818	532,818	532,818	532,818	532,818	532,818	532,818	532,818
Adjusted R^2	0.0386	0.0389	0.0386	0.0388	0.0386	0.0388	0.0386	0.0388

Table A5: Job Market of Underperforming Managers

This table examines whether underperforming managers face more difficult job market compared to other managers. The independent variable *Loser* equals 1 if a departing manager's fund return, flow, or MS Rating is in the bottom terciles within the same style group. Column (1) tests whether underperforming managers are less likely to find a new job after leaving their firm. The sample consists of all departing managers who disappear from the mutual fund industry or find a job immediately following the departure. The dependent variable $\mathbb{1}[Mover]$ equals 1 if the manager find a job immediately following the departure. In column (2) through (4), I test whether underperforming managers experiences longer job search, conditioning on finding a job within the next 2, 3, and 5 years. The dependent variables equal 1 if the managers takes more than a year of job search. In column (5), I test whether underperforming managers are more likely to move to a smaller firm. The sample consists of all departing managers who eventually find a new job. The dependent variable $\mathbb{1}[Smaller\,Firm]$ equals 1 if the manager moves to an advisory firm with less than one-half of the total asset compared with their previous firm (adjusted for the increase in market size). The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	$\mathbb{1}[Mover]$	$\mathbb{1}[1 < Move \leq 2]$	$\mathbb{1}[1 < Move \leq 3]$	$\mathbb{1}[1 < Move \leq 5]$	$\mathbb{1}[Smaller\,Firm]$
Loser	-0.0273***	0.0678**	0.0920***	0.0881***	0.0516**
	(-3.90)	(2.61)	(3.35)	(3.28)	(2.08)
Constant	0.0896***	0.217***	0.413***	0.356***	0.487***
	(11.92)	(9.35)	(17.07)	(15.51)	(23.25)
Observations	12,212	1,133	1,392	1,457	1,929
Adjusted R^2	0.00216	0.00428	0.00650	0.00612	0.00176

Table A6: Dismissal of Peers Who Secure New Employment

This table analyzes whether the peer dismissal effect exists when the dismissed peers secure a new employment. Specifically, I repeat the specification of Table 2 after partitioning peer dismissal into peers who immediately find a new job following the dismissal (*Employed*) and those who do not (*Unemployed*). The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
Peer Dismissal - Employed	0.0140		-0.00319
	(0.10)		(-0.02)
Peer Dismissal - Unemployed		-0.0913***	-0.0913***
		(-3.26)	(-3.27)
Advisor FE	Yes	Yes	Yes
Style × Time FE	Yes	Yes	Yes
Observations	208,683	208,683	208,683
Adjusted R^2	0.384	0.385	0.385

Table A7: Firm Dismissal Policy Change Hypothesis

This table examines whether managers face greater career concerns following bear markets. The dependent variable, *Firm Departure*$_{j,t}$, is an indicator for firm departure of manager j. *Low Performance* is the sum of indicators of past one-year return and flow and MS Ratings that are in the bottom terciles within each fund style in each quarter. The t-statistics, computed from standard errors two-way clustered at the fund and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dismissal during	Previous 1 Quarter			Previous 2 Quarters			Previous Year		
Sample	Full	Underperformers		Full	Underperformers		Full	Underperformers	
		Yes	No		Yes	No		Yes	No
Peer Dismissal	-0.000719	-0.000740	-0.00145	-0.00111	-0.000901	-0.00202	0.00144	0.00180	0.0000640
	(-0.54)	(-0.43)	(-1.25)	(-0.78)	(-0.52)	(-1.45)	(1.20)	(1.18)	(0.05)
Style × Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Advisor FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	883,019	536,577	346,518	883,019	536,577	346,518	883,019	536,577	346,518
Adjusted R^2	0.0943	0.114	0.0807	0.0943	0.114	0.0808	0.0943	0.114	0.0807

Appendix C Additional Difference-in-Differences Analysis

Table A8: Number of Funds in Treated States

This table reports the number of funds around the five-year event window for each of the 14 treated states that experienced changes in NCA enforceability.

Treated States	Number of Funds
Illinois	83
Texas	72
Ohio	47
Georgia	37
Colorado	32
Wisconsin	32
Florida	6
Oregon	6
Kentucky	5
Vermont	5
New Hampshire	3
Louisiana	2
South Carolina	1
Total	331

C.1 Non-Compete Agreements and Labor Mobility

In the section, I examine whether stronger NCA enforceability increases fund managers' employment risk. Specifically, I examine whether managers in treated states that strengthened (weakened) NCA enforceability exhibit reduced job mobility compared with managers in non-treated states.

Following the stacked approach of Cengiz et al. (2019) and Baker, Larcker, and Wang (2022), I first create 14 event-specific, quarterly panel datasets of all managers that left the firm in the three-year window around the event date.[34] Specifically, each panel ("*cohort*") includes managers from one of 14 treated states and control managers who are matched based on fund performance and manager characteristics. I choose control managers from a pool of never-treated states. I stack the 14 panels

[34] I focus on a relatively short window of three years because a fund manager's decision to leave a firm can potentially be endogenous. For example, if managers are aware of the change in enforceability of NCAs, they may exert a stronger effort to avoid departure (Cici, Hendriock, and Kempf, 2021). Consistent with this conjecture, I find that the unconditional turnover rate for treated states decreases after the law change.

and estimate the following DID regression:

$$Relocation_{j,c,t} = \beta\, Treat_{j,c,t} \times Post_{c,t} + \Gamma X_{j,c,t} + \delta_{c,t} + \lambda_{c,s} + \epsilon_{j,c,t}, \tag{6}$$

where j and c indicate fund manager and event cohort, respectively. $Relocation_{j,c,t}$ is an indicator variable for whether manager j moves to a new advisory firm in the following year. I include time ($\delta_{c,t}$) and state fixed effect ($\lambda_{c,s}$), both of which are saturated with indicators for event cohort (Baker, Larcker, and Wang, 2022). $Treat_{j,c,t}$ equals $+1$ (-1) if the treated state in cohort c strengthened (weakened) enforceability of NCAs. $Post_{c,t}$ is an indicator of the post-event period. The interaction term ($Treat_{j,c,t} \times Post_{c,t}$) captures the average treatment effect across the event cohorts, that is, the difference in the conditional likelihood of relocation between managers in treated and control states following the event.

The results are provided in Table A9. Column 1 shows that managers in treated states are 12.2 percentage points less likely to relocate to a new firm after NCA laws are strengthened. In columns 2 and 3, I add a style-by-cohort fixed effect and a vector of manager characteristics ($X_{j,c,t}$), and I find similar results. In columns 4 and 5, I test for the validity of the parallel trend assumption by including a pre-treatment dummy. The result supports the parallel trend assumption: relative to the benchmark period (three years before the treatment), none of the interaction terms, $Treat \times Prior1Yr$ or $Treat \times Prior2Yr$, are economically or statistically significant. To sum up, following the strengthened enforceability of NCAs, treated managers experience a reduction in job mobility, consistent with increased cost of employment risk (Bonelli, 2019; Cici, Hendriock, and Kempf, 2021).

Table A9: Change in Non-Compete Agreements and Employment Risk

This table examines whether stronger NCA enforceability increases fund managers' employment risk. Specifically, using a sample of managers who depart their firm, the table reports estimates for the model as in Equation (6). The dependent variable, $Relocation_{j,c,t}$, is an indicator variable for whether the manager moves to a new advisory firm in the following year. $Post_{c,t}$ is an indicator of the post-event period. The interaction term ($Treat_{j,c,t} \times Post_{c,t}$) captures the average treatment effect across the event cohorts; that is, the difference in the conditional likelihood of relocation between managers in treated and control states following the event. The t-statistics, computed from standard errors two-way clustered at the manager and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Treated						
× *Post*	-0.122***	-0.131***	-0.134***	-0.131**	-0.133**	
	(-2.59)	(-2.85)	(-2.87)	(-2.47)	(-2.15)	
× *Prior 2 Yr*					-0.00410	-0.00272
					(-0.05)	(-0.03)
× *Prior 1 Yr*				0.0109	0.00869	0.00989
				(0.15)	(0.11)	(0.12)
× *Post 1 Yr*						-0.168**
						(-2.10)
× *Post 2 Yr*						-0.141**
						(-2.00)
× *Post 3 Yr*						-0.0840
						(-1.19)
Baseline Controls	Cohort × Time and Cohort × State FE					
Cohort × Style FE	No	Yes	Yes	Yes	Yes	Yes
Control Variables	No	No	Yes	Yes	Yes	Yes
Observations	3,393	3,393	3,162	3,162	3,162	3,162
Adjusted R^2	0.196	0.258	0.314	0.314	0.313	0.313

C.2 Robustness of Difference-in-Differences Test

In this section, I further test for the parallel trend assumption by examining whether the pre-trends of the peer dismissal effect are different for treated and control groups. Specifically, I run the following regression model using five years prior to the event year:

$$\begin{aligned} Risk\ Taking_{i,j,c,t} = {} & \beta_1\, Time_{\tau,c} \times PeerDismissal_{j,c,t} \times Treat_{j,c,t} \\ & + \beta_2\, Time_{\tau,c} \times PeerDismissal_{j,c,t} \\ & + \beta_3\, Time_{\tau,c} \times Treat_{j,c,t} \\ & + \beta_4\, PeerDismissal_{j,c,t} \\ & + \Gamma X_{i,j,c,t} + \delta_{i,c} + \lambda_{t,s,c} + \epsilon_{i,j,c,t}, \end{aligned} \tag{7}$$

where $Time_{\tau,c}$ takes a value of one in the first quarter of the pre-event window and accrues over each following quarter. I also test for convex and concave time trend by using $log(Time_{\tau,c})$ and $Time^2_{\tau,c}$ instead. As shown in Table A10, none of the triple interaction terms is statistically significant, which suggests that the peer dismissal effect does not differ between the two groups prior to the event year.

Table A10: Linear Trend Before Event Period

This table tests for the parallel trend assumption by examining whether the pre-trends of the peer dismissal effect are different for treated and control groups. Specifically, the table reports estimates for the model as in Equation (7). The *t*-statistics, computed from standard errors two-way clustered at the manager and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
Measure of Time Trend	Linear trend	Log trend	Power trend
	$Time$	$log(Time)$	$Time^2$
Peer Dismissal	-0.212**	0.168**	-0.0681
	(-2.61)	(2.33)	(-1.50)
Time Trend × Peer Dismissal	-0.0107*	-0.124***	-0.000248
	(-1.92)	(-2.99)	(-1.07)
Treated × Peer Dismissal	0.0287	0.313	0.163
	(0.15)	(1.15)	(1.01)
Time Trend × *Treated* × *Peer Dismissal*	-0.00907	-0.0866	-0.000319
	(-0.56)	(-0.77)	(-0.45)
Fund × Cohort FE	Yes	Yes	Yes
Time × State × Style × Cohort FE	Yes	Yes	Yes
Observations	210,503	210,503	210,503
Adjusted R^2	0.648	0.648	0.647

Table A11: Difference-in-Differences Based on Propensity Score Matching

This table repeats a stacked difference-in-differences analysis around state-level changes in the enforceability of non-compete agreements (NCAs) in Table 11 using propensity score matched samples. Specifically, I perform one-to-one nearest-distance matching without replacement, as well as $1:N$ matching with replacement, where $N = 1, 3$, and 5. I also use a sample in which I require funds to exist during at least 80% of the event window. I match based on the average values from the prior event of fund characteristics that are associated with fund risk-taking behavior. These characteristics include the quarterly return and flow of the fund, TNAs, fund age, expense ratio, and turnover ratio. I also require control funds to have the same style as the treated fund, and limit the difference between the predicted logits of the matched funds' propensity scores to be less than 0.10. The t-statistics, computed from standard errors two-way clustered at the manager and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	Exist Before / After Event Period				Exist Before / After Event Period with > 80% obs			
Replacement	No	Yes			No	Yes		
Matching: 1 to N	1	1	3	5	1	1	3	5
Peer Dismissal	-0.167**	-0.171**	-0.185***	-0.205***	-0.231***	-0.307***	-0.216***	-0.204***
	(-2.51)	(-2.58)	(-2.89)	(-3.82)	(-2.71)	(-3.24)	(-3.94)	(-3.14)
Peer Dismissal								
× *Treated* × *Post*	-0.341**	-0.282**	-0.301**	-0.300**	-0.311*	-0.396**	-0.307*	-0.298*
	(-2.63)	(-2.17)	(-2.45)	(-2.50)	(-1.95)	(-2.40)	(-1.89)	(-1.85)
× *Treated*	0.222*	0.166	0.172	0.186	0.276*	0.360**	0.265*	0.255
	(1.83)	(1.34)	(1.54)	(1.65)	(1.83)	(2.40)	(1.67)	(1.59)
× *Post*	0.185**	0.180**	0.213***	0.214***	0.218**	0.301***	0.230***	0.216***
	(2.40)	(2.26)	(2.79)	(3.38)	(2.25)	(2.79)	(3.37)	(3.24)
× *Treated* × *Prior 1 Yr*	0.0117	0.0398	-0.0563	-0.140	0.182	0.0451	0.242	0.181
	(0.07)	(0.22)	(-0.34)	(-0.93)	(0.83)	(0.21)	(1.37)	(1.04)
× *Treated* × *Prior 2 Yr*	-0.0246	-0.0714	-0.0832	-0.134	0.0171	-0.0565	-0.0844	-0.119
	(-0.12)	(-0.36)	(-0.48)	(-0.76)	(0.04)	(-0.15)	(-0.23)	(-0.35)
× *Prior 2 Yr*	-0.0411	0.0265	0.0535	0.112	-0.121	-0.0116	0.0175	0.0519
	(-0.28)	(0.19)	(0.48)	(1.35)	(-1.01)	(-0.09)	(0.17)	(0.56)
× *Prior 1 Yr*	-0.0369	-0.0459	0.0522	0.138*	-0.0352	0.0620	-0.115	-0.0612
	(-0.29)	(-0.37)	(0.58)	(1.98)	(-0.18)	(0.30)	(-0.76)	(-0.50)
Baseline Control	Control Variables, Fund × Cohort and Time × State × Cohort Fixed Effect							
Observations	57,974	60,244	121,601	181,029	24,902	25,762	53,549	79,769
Adjusted R^2	0.601	0.602	0.606	0.605	0.627	0.639	0.645	0.646

Table A12: Difference-in-Differences Placebo Test Using Index Fund

This table repeats a stacked difference-in-differences analysis around state-level changes in the enforceability of non-compete agreements (NCAs) in Table 11 using index funds. The t-statistics, computed from standard errors two-way clustered at the manager and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Baseline		Pre-trend	
Peer Dismissal	-0.0457	-0.0440	-0.0576	-0.0791
	(-1.12)	(-1.13)	(-1.38)	(-1.59)
Peer Dismissal				
× *Treated* × Post	0.192	0.153	0.258	0.307
	(1.57)	(1.19)	(1.61)	(1.36)
× *Treated*	-0.0926	-0.0841	-0.188	-0.238
	(-0.84)	(-0.72)	(-1.25)	(-1.07)
× *Post*	0.00610	0.0103	0.0239	0.0448
	(0.13)	(0.22)	(0.48)	(0.78)
× *Treated* × *Prior 1 Yr*			0.337**	0.384*
			(2.05)	(1.74)
× *Treated* × *Prior 2 Yr*				0.0819
				(0.35)
× *Prior 1 Yr*			0.0568	0.0783
			(1.28)	(1.51)
× *Prior 2 Yr*				0.0750
				(1.18)
Control Variables	No	Yes	Yes	Yes
Fund × Cohort FE	Yes	Yes	Yes	Yes
Time × State × Style × Cohort FE	Yes	Yes	Yes	Yes
Observations	61,353	61,353	61,353	61,353
Adjusted R^2	0.723	0.724	0.724	0.725

Table A13: Difference-in-Differences Placebo Test by Shifting Event Window

This table repeats a stacked difference-in-differences analysis around state-level changes in the enforceability of non-compete agreements (NCAs) in Table 11 by shifting the treatment window to placebo periods before and after the actual law change dates. Specifically, for each cohort panel, I shift the treatment date by three, five, and seven years. The t-statistics, computed from standard errors two-way clustered at the manager and quarter level, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Years before event			Years after event		
	7	5	3	3	5	7
Peer Dismissal × *Treated* × *Post*	-0.0619	0.143	0.0920	-0.111	0.0352	-0.0527
	(-0.46)	(1.35)	(0.88)	(-1.59)	(0.50)	(-0.48)
Peer Dismissal × *Treated*	0.101	-0.0257	0.0102	0.0531	-0.0389	0.0147
	(0.85)	(-0.29)	(0.13)	(0.95)	(-0.71)	(0.22)
Peer Dismissal × *Post*	-0.00881	-0.106	-0.0752	0.0614**	0.0184	0.0667**
	(-0.12)	(-1.64)	(-1.32)	(2.02)	(0.67)	(2.00)
Peer Dismissal	-0.0607	-0.0153	-0.0306	-0.0735***	-0.0391	-0.0492*
	(-0.89)	(-0.39)	(-1.33)	(-2.69)	(-1.62)	(-1.95)
Control Variables	No	Yes	No	Yes	Yes	Yes
Fund × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Time × State × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	289,292	353,066	410,962	426,696	366,357	214,472
Adjusted R^2	0.519	0.529	0.535	0.568	0.579	0.595

Appendix D Variable Definitions for Confederate Memorials and the Housing Market

D.1 L2 Homeowner Data

- *Race (Black)* – an indicator equal to one if all the owners of the house identify as Black. [Source: L2].
- *Registered Democrat* – an indicator equal to one if all the owners of the house are registered Democrats. [Source: L2].
- *Education (Some College)* – an indicator equal to one if all the owners of the house have some college education [Source: L2].
- *Income* – the combined income of the owners of the house. [Source L2].
- *Age* – the age of the primary homeowner (in years). [Source L2].
- *Confederate* – An indicator variable equal to one if the house is located on a street that honors the Confederacy. We consider variants of the names "Robert E. Lee," "Jefferson Davis," "Confederate," "Stonewall Jackson," or "Dixie." [Source: L2 Data].
- *Confederate Adjacent* – An indicator equal to one if the property is located within x miles of the closest Confederate property, where we set x equal to values ranging from 0.05 miles to 0.50 miles.

D.2 ATTOM House Attributes

- *Confederate* – An indicator variable equal to one if the house is located on a street that honors the Confederacy. We consider variants of the names "Robert E. Lee," "Jefferson Davis," "Confederate," "Stonewall Jackson," or "Dixie." [Source: ATTOM Data].
- *Control House* – any house that sold in the same census tract and same calendar quarter as a Confederate house. [Source: ATTOM Data].
- *House Size (sq. ft.)* – House building area of the property in square feet. [Source: ATTOM Data].
- *# Bedrooms (Bathrooms)* – Number of bedrooms (bathrooms) in the property. [Source:

ATTOM Data].

  - *Bed2 (Bath2)* – an indicator equal to one if the house has 2 bedrooms (2 bathrooms) and zero otherwise. Other bed (bath) indicators are defined analogously.

- *Age (years)* – the difference between house sale date and house-built date, divided by 365; assuming that the house is built on the first date of built year. [Source: ATTOM Data].
- *Lot Size (sq. ft.)* – Lot size of the property in square feet. [Source: ATTOM Data].
- *Confederate Adjacent* – An indicator equal to one if the property is located within x miles of the closest Confederate property, where we set x equal to values ranging from 0.05 miles to 0.50 miles. [Source: ATTOM Data].

D.3 Zillow Listing Variables

- *Withdrawn* – an indicator equal to one if the house listing is subsequently withdrawn without selling. [Source: Zillow].
  - A house is considered to be sold if the sale listing is removed after the "sale pending" or "sold" indicator.
- *Slow Sale* – an indicator equal to one if the difference between the selling date (or withdrawal date) and the listing date is in the top quintile. [Source: Zillow].
- *Large Discount* – an indicator equal to one if log (Listing Price /End Price) is in the top quintile of the distribution, where end price is defined as either the sales price or the listing price on the date the property is withdrawn. [Source: Zillow].
- *Listing Price* – the asking price when the property is first listed. [Source: Zillow].

D.4 Regional and Demographic Variables

- *High Black Population* – an indicator equal to one if the house is located in a country with above median Black population. Median breakpoints are computed based on all transactions that occurred during the calendar quarter. [Source: US Census Bureau].
- *High Democrat* – An indicator equal to one if the house is located in a county where the percentage of votes for Democratic party presidential candidate in the county is above the

median. Median breakpoints are computed based on all transactions that occurred during the calendar quarter. [Source: MIT Election Data and Science Lab].

- *High College* – An indicator equal to one if the house is in a country with above median fraction of 4-year college graduates. Median breakpoints are computed based on all transactions that occurred during the calendar quarter. [Source: US Census Bureau].
- *Non-Confederate States* – an indicator equal to one if the house is in a state that was not one of the original 11 Confederate states (Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia.).
- *Non-Top 5 Statue* – an indicator equal to one if the house is in a state that is not in the top 5 in the total number of Confederate statues (Georgia, Virginia, North Carolina, Texas, and Alabama). [Source: https://www.splcenter.org/20190201/whose-heritage-public-symbols-confederacy].
- *Composite* – the sum of non-Top 5 Statue + High Black Population + High Democrat + High College.
  - *Low Composite (High Composite)* – an indicator equal to one if the Composite score is below (above) the median value of 2.
- *Mid Composite* – an indicator equal to one if the Composite score is equal to the median value of 2.

D.5 School Name Change Variables

- *Name Change* – An indicator equal to one if the house is located in a school district that changes its name.
- *Post* – An indicator equal to one if the transaction took place after the school name change.
  - *Year (+1)* – an indicator equal to one if the transaction occurred in the year after the name change.
  - *Year (¿ +1)* — an indicator equal to one if the transaction occurred in the two or three years after the name change.
  - *Year (-1)* – an indicator equal to one if the transaction occurred in the year prior to the name change.

D.6 Experimental Data

- *Street Names* – We manipulate the street names that correspond to a specific picture of a house. In particular, different participants see the exact same house with a different street name. This manipulation allows us to examine the impact of street names holding the house constant. In our study, we consider four non-confederate street names: Kenwood, Gresham, Juniper, and Linden and one confederate street name: Dixie.
  - The main independent variable of interest is Dixie Dif which is equal to one if the first house (i.e., the house on the left) is on Dixie Street, negative one if the 2nd house (i.e., the house on the right) is on Dixie Street, and zero if neither house is on Dixie Street.
- *Primed* – an indicator equal to one if the participant was randomly assigned to be in the primed group. Participants in the primed group begin the survey by reading an article that underscores the racist connotations of Confederate symbols (Confederate flag removals following the Charleston Church shooting). Participants in the non-primed group begin by reading an article of similar length on a subject unrelated to race (the harmful effects of social media on teens).
- *House #1* – an indicator equal to one if the participant reported preferring the first house (i.e., the house on the left of the screen) to the second house (i.e., the house on the right of the screen), and zero if the participant reported preferring the second house.
- *Negative Confederate Sentiment* – an indicator equal to one if the participant reported that they would feel either "extremely bad" or "somewhat bad" if they lived on a Confederate street.
- *Positive Confederate Sentiment* – an indicator equal to one if the participant reported that they would feel either "extremely good" or "somewhat good" if they lived on a Confederate street.
- *Black Respondent* – An indicator equal to one if the respondent identifies as "Black / African American."
- *Democrat* – An indicator equal to one if the respondent self-reported as usually voting "Democratic."

- *College Educated* – an indicator equal to one if the participant's self-reported education level is a bachelor's degree or higher.
- *Non-Confederate State* – an indicator equal to one if the participant resides in a state that was not one of the original 11 Confederate states (Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia).
- *House #FE1* – a set of indicator dummies to indicate which house was the first (left) house seen by participants. We include five different houses and thus four indicator variables (with the fifth house being the omitted group).
- House #FE2 – a set of indicator dummies to indicate which house was the second (right) house seen by participants. All other details are analogous to House #FE1.

Appendix E Literature related to ESG, Risk, and Return

This table summarizes types of risks and returns (surrounding negative events) proposed to relate to ESG/CSR in the academic literature on ESG/CSR in asset pricing. For each paper cited, we report the variable of interest.

Variable of Interest		**Paper**	**Data Source**
Risk	Idiosyncratic - unconditional	Luo and Bhattacharya (2009)	*Fortune*'s Most Admired Companies
	Idiosyncratic - downside	Kim, Li, and Li (2014)	MSCI KLD
		Diemont, Moore, and Soppe (2016)	Dutch Sustainability Ratings
	Systematic - unconditional	Oikonomou, Brooks, and Pavelin (2012)	MSCI KLD
		Jo and Na (2012)	MSCI KLD
		Albuquerque, Koskinen, and Zhang (2019)	MSCI KLD
		This Paper	MSCI KLD + Sustainalytics
	Systematic - downside	***This Paper***	MSCI KLD + Sustainalytics
	Total - unconditional	Jo and Na (2012)	MSCI KLD
	Total - downside	Hoepner, Oikonomou, Sautner, Starks, and Zhou (2024)	Proprietary engagement data
	Volatility during the COVID-19 crisis	Albuquerque, Koskinen, Yang, and Zhang (2020)	Thomson Reuters' Refinitiv + MSCI KLD
Return	Surrounding firm-specific, negative events	Godfrey, Merrill, and Hansen (2009)	MSCI KLD
		Krüger (2015)	MSCI KLD
	Surrounding market-wide, negative events	Lins, Servaes, and Tamayo (2017)	MSCI KLD
		Albuquerque, Koskinen, Yang, and Zhang (2020)	Thomson Reuters' Refinitiv + MSCI KLD

Appendix F Variable Definitions for Risk, Return, and Environmental and Social Ratings

B.1 ESG and climate change score (Table 2-8)

- *Category score* [Source: MSCI KLD]
  - computed each firm-year, for each of the following six categories: *environment, community, human rights, employee relations, diversity,* and *product*
  - net difference between a firm's total number of strengths and weaknesses, each of which divided by yearly maximum number of strengths and weaknesses in each category
- *E score* - *Category score* of *environment* category [Source: MSCI KLD]
- *S score* - sum of *Category score* of *community, human rights, employee relations, diversity,* and *product* category [Source: MSCI KLD]
- *ES score* - sum of *E score* and *S score* [Source: MSCI KLD]
- *Climate score* - clean energy strength minus climate change concern score (both part of *environment* category), following Chava (2014) [Source: MSCI KLD]
- *G score* - net difference between a firm's total number of strengths and weaknesses in *corporate governance* category, each of which divided by the yearly maximum number of strengths and weaknesses in *corporate governance* category [Source: MSCI KLD]

B.2 Risk measures (Table 2-11)

- The following measures are computed for each firm-month using *daily* returns over the following 12-month period, from t to $t + 11$, excluding stocks that have more than five missing daily returns during this horizon [Source: CRSP]
  - *Beta* - CAPM beta; see equation (1)
  - *Downside beta* - CAPM beta using days in which market excess return is below its average during 12-month period, following Ang et al. (2006); see equation (2)
  - *Relative downside beta* - *downside beta* minus *beta*, following Ang et al. (2006)
  - *Coskewness* - coskewness between firm excess return and market excess return during 12-month period; see equation (3)
  - *Upside beta* - CAPM beta using days in which market excess return is above its average during 12-month period, following Ang et al. (2006)
- *Tail risk* [Source: CRSP]
  - computed for each firm-month using monthly returns over the following 60-month period, from t to $t + 59$, excluding stocks that have less than 36 non-missing monthly returns during this horizon
  - slope coefficient estimate from a univariate regression of monthly firm returns on monthly tail risk component, computed as equation (4) following Kelly and Jiang (2014)

B.3 Portfolio returns (Table 2)

- *Excess return* - monthly average excess return of a portfolio [Source: CRSP]

- *CAPM alpha* - intercept from regression of monthly average excess portfolio return on market factor [Source: CRSP; Ken French's website]
- *3F alpha* - intercept from regression of monthly average excess portfolio return on market, size, and value factor [Source: CRSP; Ken French's website]
- *4F alpha* - intercept from regression of monthly average excess portfolio return on market, size, value, and momentum factor [Source: CRSP; Ken French's website]

B.4 Other return measures (Table 32-6)

- *1(PosMktRet)* - computed for each year; an indicator variable equal to one if the market's realized excess return is positive [Source: CRSP]
- *1(NegMktRet)* - computed for each year; an indicator variable equal to one if the market's realized excess return is negative [Source: CRSP]
- *Excess return* - yearly excess return of a firm over the following 12-month period, from t to $t+11$ [Source: CRSP]
- *DGTW-adjusted return* - yearly DGTW-adjusted return of a firm over the following 12-month period, from from t to $t+11$ [Source: CRSP]

B.5 Firm characteristics (Table 31-8, 10-11)

- *MktCap ($ mil)* - market capitalization (in $million) as of previous month-end [Source: CRSP]
- *Dividend dummy 1(Dividend)* - an indicator variable equal to one if the firm paid dividend [Source: Compustat]
- *Book-to-market (B/M)* - book value of equity divided by previous fiscal year-end market capitalization [Source: Compustat]
- *Past 12 mth exret* - excess return during the past 12-month period, from $t-12$ to $t-1$ [Source: CRSP]
- *Past 12 mth ret STD* - standard deviation of daily excess return during the past 12-month period, from $t-12$ to $t-1$ [Source: CRSP]
- *Return on equity (ROE)* - earnings before extraordinary items divided by previous common shareholders' equity [Source: Compustat]
- *Asset growth* - annual percent change in total assets [Source: Compustat]
- *Sales growth* - annual percent change in sales [Source: Compustat]
- *Leverage* - total liabilities divided by fiscal year-end market capitalization [Source: Compustat]
- *1(BigCap)* - a dummy variable that is equal to 1 if the firm's market value is above the median NYSE market equity as of previous year-end [Source: CRSP]
- *1(SmlCap)* - a dummy variable that is equal to 1 if the firm's market value is below the median NYSE market equity as of previous year-end [Source: CRSP]

B.6 News sentiment variables (Table 9)

- *Firm news sentiment* - computed for each firm-day; daily average of RavenPack's sentiment scores across news covering the firm [Source: RavenPack]
- *Portfolio news sentiment* - daily value-weighted average of *firm news sentiment* across firms in the portfolio [Source: RavenPack]
- *Aggregate news sentiment (AggSent)* - daily value-weighted average of *firm news sentiment* across all firms [Source: RavenPack]

B.7 Institutional trading variables (Table 37)

- The following measures are computed for each firm-month using *daily* returns over the following 12-month period, from t to $t+11$, excluding stocks that have more than five missing daily returns during this horizon
  - *Trading beta* - slope coefficient estimate from a univariate regression of monthly firm returns on scaled daily institutional trading [Source: CRSP; Abel Noser]
  - *Downside trading beta* - slope coefficient estimate from a univariate regression of monthly firm returns on scaled daily institutional trading, using days in which market excess return is below its average during 12-month period [Source: CRSP; Abel Noser]
    - also compute similar measure using 25th percentile of market excess distribution in each period as a threshold, instead of average excess market return
  - *Relative downside trading beta* - *Trading beta* minus *Downside trading* beta [Source: CRSP; Abel Noser]

B.8 Sustainalytics variable (Table 38)

- *Raw ESG Score* - Total ESG score from Sustainalytics (*total_esg_score*) [Source: Sustainalytics]
- *Log ESG Score* - log value of 1+*Raw ESG Score* [Source: Sustainalytics]

Appendix G Robustness Tests with Sustainalytics' ESG Ratings

In this appendix, we re-examine the relationship between ES performance and future risks, except we use Sustainalytics' data for the firm-level ESG profile. Sustainalytics measures how well companies are prepared for their most material ESG issues by using a customized weight matrix that defines the relative importance of each indicator and emphasizes the key ESG issues for each industry.[35] In turn, these raw scores are aggregated to produce a company's total ESG score (out of 100), as well as its three component scores: Environmental, Social and Governance. The sample period is from August 2009 to December 2017.

We run Fama and MacBeth (1973) regressions of future downside risk measures—relative downside β, coskewness, and tail risk β—on all the independent variables in Table 31, except we now use Sustainalytics' total ESG score instead of ES score constructed from KLD.[36] The results are in Table 38. The first three columns in each panel use the scores as is, while the last three columns use their natural logarithms.

Just like MSCI KLD, the Sustainalytics coverage of small firms (i.e., market value below the median NYSE market equity) is sparse: Slightly more than 10% of its firms are small. Accordingly, we examine the sample of big firms (i.e., market value above the median NYSE market equity), just like in our main analyses, in Panel A. Panel B examines a subsample that further excludes firms with negative book value.

In Panel A, the estimated coefficients of future relative downside beta on Sustainalytics ESG score are consistently negative, but insignificant. However, Panel B shows that stocks with high Sustainalytics ESG scores have significantly lower future relative downside beta by focusing on big firms with positive book value (Column 4).

Moreover, firms with high Sustainalytics ESG scores have significantly lower future downside risk, based on coskewness, in both panels. On the other hand, we find no evidence that Sustainalytics

[35] Importantly, this helps account for the fact that whether a given ESG issue is material likely varies systematically across firms and industries (Khan, Serafeim, and Yoon, 2016).

[36] We also considered regressions of future β and β^{-} on Sustainalytics' total ESG score, analogous to those in the last two columns of Panel A of Table 31. In summary, we continue to find no evidence that ES scores have unconditional risk implications. While the negative relation between ES score and future downside beta persists, it is no longer significant. This result is likely due to the fact that β^{-} is not a good measure of downside risk, as discussed earlier in Section 3.2.

ESG scores have implications for tail risk beta: Focusing on tail risk beta reduces the sample period to little more than 5 years, so our estimated coefficients could just be too noisy in this case.

In summary, we continue to find that a firm's CSR activities, as measured by Sustainalytics' total ESG score, have significant, but weaker, downside risk benefits. Therefore, using ES ratings from multiple raters is unlikely to lead to downside risk mitigation effects of ES activities that are substantially larger than what we obtain in this paper.

Appendix H Robustness Tests Using Panel Regression Approaches

In this appendix, we estimate panel regressions to investigate the relation between firms' ES performance and future risk attributes. We have found that high ES stocks have lower downside risk, but that the economic significance of the effect is modest. A skeptical reader might argue that the downside risk of high ES stocks is varying over time, as suggested by Figure 8, such that the Fama–MacBeth regressions understate the downside risk effects of firm-level ES performance. We explore this possibility. The results are in Table 38.

In Panel A, we first consider panel regressions of realized risk measures—regular beta, downside beta, relative downside beta, and coskewness—in each year on past variables at the individual firm level. We include all the independent variables in Table 31, except we include firm fixed effects in lieu of industry fixed effects.[37] All standard errors are double clustered by firm and time.

Consistent with our results from the Fama–MacBeth regressions, ES ratings have no significant relationship with unconditional market risk, while high ES stocks do have significantly lower systematic downside risk. The estimated coefficients on ES score are larger than those from the Fama-MacBeth regressions. While these results suggest that, indeed, the Fama–MacBeth regressions understate the downside risk effects of ES performance, increases in magnitude are not substantial at all.

Still, the estimated coefficients of downside risk measures regressed on ES performance might understate the economic significance of this benefit if the downside risk advantage of high ES stocks covaries negatively with the market, i.e., resilience of high ES stocks during the worse part of a year is heightened if the year is itself a bad year. To evaluate this idea, we interact ES performance with $1(NegMktRet)$ and $1(PosMktRet)$, where $1(NegMktRet)$ ($1(PosMktRet)$) is a dummy variable that is equal to 1 if the market's excess return is negative (positive) in a given year. The results are shown in Panel B of Table 38.[38]

Consistent with our results from the Fama–MacBeth regressions, ES ratings continue to have no significant relationship with unconditional market risk, while high ES stocks do have significantly lower systematic downside risk in both good and bad years. Interestingly, the downside risk advan-

[37] Including industry fixed effects as in the Fama–MacBeth regressions leads to the same conclusions.

[38] All standard errors are double clustered by firm and time.

tage of high ES stocks typically doubles in bad years, suggesting that, indeed, it covaries negatively with the market. Therefore, the estimated coefficients of downside risk measures regressed on ES performance, which represent only the average effect, plausibly understate the economic significance of this benefit. Importantly, note that interpreting the magnitude of this benefit in terms of returns, as reported in Table 32, addresses this concern: returns account for not only the average downside risk effect of ES ratings, but also its covariation with the market. This fact also helps explain our finding in Section 3.4 that seemingly slight reductions of downside risk deliver relatively large gain in long-term returns.

Internet Appendix for:
Confederate Memorials and the Housing Market

In this appendix, we discuss and tabulate results from select robustness tests referenced in the paper (Sections IA.1 – IA.4), and we describe additional details of the survey design (Section IA.5).

IA.1 Residential Sorting – Descriptive Statistics and Robustness

Table IA1 provides additional descriptive statistics on the merged L2-ATTOM dataset used in the residential sorting tests in Tables 1 and 2 of the paper. The final sample includes 1,943 Confederate properties and 111,147 control properties across 248 census block group that include at least one Confederate property. We find that the average control property (i.e., non-Confederate homes in the same census block) is 1.69 miles away from Confederate streets, with 32% of all control properties being located within a half-mile of a Confederate street.

Table 16 of the paper shows that houses on Confederate streets are less likely to be owned by Black residents, registered Democrats, and individuals with a college education. In Table IA2, we examine whether these results extend to *Confederate Adjacent* properties. Specifically, we re-estimate Specification 7 of Table 16 after replacing *Confederate* with *Confederate Adjacent*. *Confederate Adjacent* is an indicator that is equal to one if the property is located within x miles of the closest Confederate property, where we set x equal to values ranging from 0.05 miles to 0.50 miles. Thus, the objectives of this analysis mirror the analysis reported in Table 17 (based on Equation (2)), but the empirical test is more closely aligned to the baseline test in Table 16.[39]

Table IA2 reports the results of this analysis. For reference, Specification 1 reports the baseline results (i.e., Specification 7 of Table 16) where the dependent variable is Confederate. In Specification 2, we define *Confederate Adjacent* properties as those properties located within 0.05 miles of a Confederate property. We find the coefficient on *Demographic Score* is statistically insignificant. The estimated percentage effects (i.e., the coefficient estimates scaled by the mean of the dependent variable) is also only -6.37%, which is roughly one-eighth of the estimated effect in the baseline model (-50.86%). The results are qualitatively similar if we define *Confederate Adjacent* properties

[39] Despite this advantage, we prefer the empirical design reported in Table 17 because it allows for a formal test of the difference between the coefficients on *Confederate* and *Confederate Adjacent*.

as properties located within 0.10, 0.25, or 0.50 miles from a Confederate property. These findings mirror the results from Table 17 of the paper, and they further suggest that the observed residential sorting for houses on Confederate streets does not spillover to adjacent properties.

IA.2 Confederate House Prices and Market Liquidity

Both survey evidence and the residential sorting results (see Section 2) suggest that there is considerable heterogeneity in how individuals perceive Confederate street names. We expect that the impact of heterogeneous preferences on prices should be more pronounced when markets are more illiquid (Piazzesi, Schneider, and Stroebel, 2020). For example, consider a highly illiquid market where there is only one prospective house buyer and many prospective sellers. If the one prospective buyer dislikes *Confederate* streets, then the Confederate property will only be sold if the seller offers a significant discount. On the other hand, in highly liquid (or "hot") markets where the number of prospective buyers exceeds the supply of houses, it is more likely that a house will receive multiple offers. In this case, the winning bid for a Confederate property is less likely to be from an individual who dislikes Confederate streets, and thus, the magnitude of the Confederate discount should be considerably smaller.

We measure market liquidity using the county-level price growth during the previous quarter as reported by Zillow Home Value Index (ZHVI). We define a market as "*Liquid*" if it is in the top quintile of price growth. We also define a market as "*Very Liquid*" if it is in the top 5% of the distribution of price growth. We then estimate Specification 4 of Table 20 for *Less Liquid* markets (the bottom four quintiles), *Liquid* markets, and *Very Liquid* markets.

Table IA3 reports the results. Consistent with our conjecture, the Confederate discount is large in *Less Liquid* markets (-3.49%) and non-existent in *Liquid* markets (0.12%) or *Very Liquid* markets (-0.30%). Similarly, we expect that the cross-sectional differences documented in Table 23 will be attenuated in more liquid markets. To test this prediction, we repeat Specifications 1-3 of Table IA3 after interacting *Confederate* with *High Composite*, as defined in Table 23. The results of these analyses, reported in Specifications 4-6 of Table IA3, are consistent with this prediction. In particular, the incremental effect of *High Composite* is strongest in less liquid markets (-7.46%) and weakest in *Very Liquid* markets (-0.11%).

IA.3 Other Housing Market Outcomes – Descriptive Statistics and Robustness

Table IA4 provides summary statistics (similar to Table 18) for the merged Zillow-ATTOM sample used to examine the other housing market outcomes in Table 22. Our final sample includes 2,619 listings of Confederate properties and 17,744 non-Confederate properties that were listed in the same census tract and quarter. We find that the median *End Price* and *Listing Price* are \$190,000 and \$199,999, which is similar to the median sale prices reported for the full sample in Table 18 (\$180,000). The average value of *Withdrawn* is 8.42%. Although the average values of *Slow Sale* and *Large Discount* are approximately 20% by construction, the top quintile of *Slow Sale* corresponds to properties that do not sell within (roughly) six months of the listing date, while the top quintile of *Discount* corresponds to discounts of 10% or larger.

As discussed in the paper, due to the more limited sample of properties with listing data (roughly half the size of the sale sample), we are not able not include census tract × listing quarter × age quintile fixed effects. In the body of the paper (Table 22), we report the results using census tract × listing quarter fixed effects. To alleviate the concern that the value of older homes may vary significantly across census tract, we next repeat Table 22 after including both census tract × listing quarter fixed effects and census tract × age quintile fixed effects. The results of this analysis, reported in Table IA5, are qualitatively similar to the baseline results reported in Table 22.

IA.4 Confederate Discount by Calendar Month

In Section 5.6 of the paper, we document that the Confederate discount increases following events that result in increased attention to the racial underpinning of the Confederate symbols, with the effects being particularly pronounced in the quarter following the event. The three events we explore (*Charleston*, *Charlottesville*, and *BLM Protests*) all occur in the summer (two in June and one in August), raising the concern that our findings might be driven by seasonality in the Confederate discount. To explore whether seasonality in the Confederate discount could contribute to our findings, we first repeat our baseline regression (Specification 4 of Table 20) after replacing *Confederate* with *Confederate* interacted with each of the 12 calendar months. For example, *Confederate* ×

January estimates the magnitude of the Confederate discount for all Confederate transactions that took place during the month of January. To ensure that the seasonality estimates are not biased by the attention-grabbing events studied in Section 5.6, this analysis excludes the quarter immediately following the three attention-grabbing events. Finally, to reduce noise, and more closely parallel the quarterly analysis in the event-time tests, we define *Quarterly Average* as the average estimate across the subsequent quarter (i.e., month $t+1$, month $t+2$, and month $t+3$). Thus, if seasonality contributes to our event-time findings in Table 23, we should observe particularly large discounts in June and August.

Figure IA2 plots the *Quarterly Averages* separately for each calendar month. We do not observe dramatic differences across the estimates, with values ranging from -1.99% (July) to -4.22% (April). We note that the estimates for June (-2.83%) and August (-2.57%) are both slightly smaller than the full-sample estimate (-2.93%). Overall, we conclude that seasonality in the Confederate discount is unlikely to drive the large discount of -8.13% that we observe in the quarter following the salient racial events.

IA.5 Additional Experimental Details

IA.5.1 Priming and Non-Priming Articles

Half of respondents will be asked to summarize the following article as follows:

> **Please read the following article and summarize it with one or two sentences:**
>
> Republican South Carolina Governor Nikki Haley signed into law a measure to remove the Confederate battle flag from the state Capitol, the result of a years-long movement that was reignited by the murders of nine members of a historically Black church in Charleston.
>
> Before adding her signature to the legislation, Haley spoke of the Black victims, who were killed by a white man after they welcomed him into a prayer meeting.
>
> In the days after the shootings, photos emerged of the killer posing with the Confederate flag, a Civil War relic that is also seen as an emblem of racism. That sparked a nationwide debate about the flag's place in American culture. Many businesses stopped making and selling the flag and its images, and public officials discussed removing the flag from public grounds. That included South Carolina, which first flew the Confederate flag at Capitol in Columbia in 1962 as a response to the civil rights movement.
>
> The state legislature, which lost state Senator Clementa Pinckney, the church's top pastor, in

the shooting, responded by voting overwhelmingly this week to take the flag down.

Republican Senator Lindsey Graham of South Carolina praised the flag's removal. "After the horrific tragedy in Charleston, our state could have gone down one of two paths, division or reconciliation," Graham said. "I am thankful we chose the path of reconciliation."

Please write one or two sentences to summarize the article.

The other half of respondents will receive the following control article:

Please read the following article and summarize it with one or two sentences:

Legislators introduced a bipartisan bill aimed at protecting children from the harmful impacts of social media.

The bill, sponsored by Republican Senator Marsha Blackburn and Democratic Senator Richard Blumenthal, came as Congress held hearings on the dangers of social media for children and teens. The proposed Kids Online Safety Act includes three key elements:

Social media companies would be required to provide the ability to disable addictive features and allow users to opt-out of recommendations like pages or other videos to "like." It would also make the strongest privacy protections the default.

The bill would give parents tools to track time spent in the app, limit purchases and help to address addictive usage.

It would require social media companies to prevent and mitigate harm to minors, including self-harm, suicide, eating disorders, substance abuse, sexual exploitation and unlawful products for minors, like alcohol.

Dr. Dave Anderson, clinical psychologist at the Child Mind Institute, said the bill marks the sensible intersection of tech and public policy. "I think politicians are taking what we know from the science and saying, 'How do we build in these safeguards?'" Anderson said.

He said social media algorithms have evolved to show children only more of what they are interested in rather than a variety of viewpoints and that marks a dangerous change for children with mental health issues.

Please write one or two sentences to summarize the article.

IA.5.2 House Comparisons

In the following pages we present the questions from one of the 20 blocks of 10 pairwise comparisons (the five houses and five house names are presented in alternative combinations and positions in the remaining 19 blocks seen by other participants). Participants begin with the following instruction page.

For the next set of questions, imagine you are moving to a new town and are looking for a home.

In the 10 comparison questions that follow, each of the hypothetical houses is located in the same neighborhood, was built around the same time, and is very similar in size (same number of bedrooms and bathrooms).

For each pair of houses that you are presented, where would you prefer to live for your family home? The "next" arrow will appear at the bottom of the page after ten seconds (you must spend at least 10 seconds for each comparison, more time is fine).

Click the arrow to begin.

1 of 10

Which house would you prefer to live in?

612 Kenwood Ave.

592 Linden Ave.

2 of 10

Which house would you prefer to live in?

481 Gresham St.

423 Dixie Ave.

3 of 10

Which house would you prefer to live in?

353 Juniper Rd.

612 Kenwood Ave.

4 of 10

Which house would you prefer to live in?

592 Linden Ave.

423 Dixie Ave.

5 of 10

Which house would you prefer to live in?

353 Juniper Rd.

481 Gresham St.

6 of 10

Which house would you prefer to live in?

423 Dixie Ave.

612 Kenwood Ave.

7 of 10

Which house would you prefer to live in?

481 Gresham St. 592 Linden Ave.

481 Gresham St.

592 Linden Ave.

Which house would you prefer to live in?

423 Dixie Ave.

353 Juniper Rd.

Which house would you prefer to live in?

612 Kenwood Ave.

481 Gresham St.

10 of 10

Which house would you prefer to live in?

592 Linden Ave.

353 Juniper Rd.

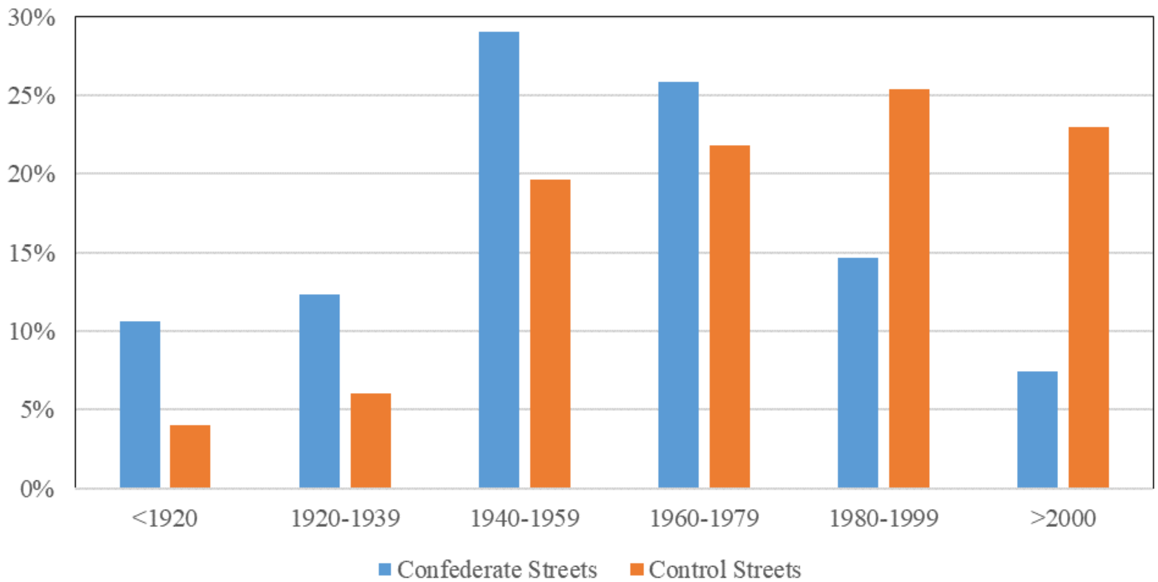

Figure IA1. Distribution of Confederate and Control Street Age

This figure plots the distribution of the age of Confederate and control streets, where street age is measured by the oldest house on the street. The blue bars report the percentage of all Confederate streets that were named during a specific time period, and the orange bars report analogous percent for control streets.

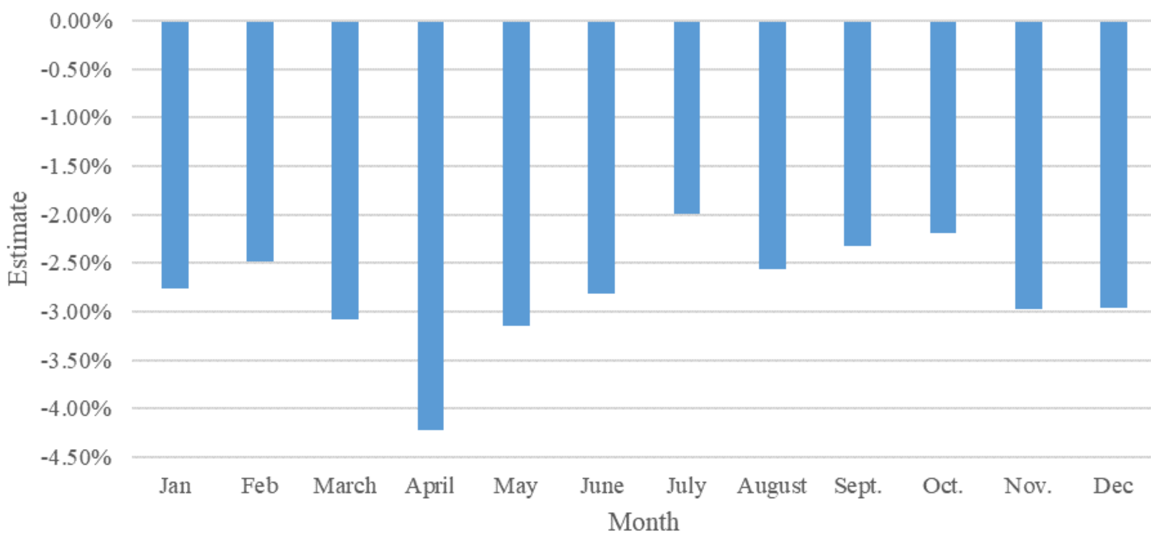

Figure IA2. Confederate Discount by Calendar Month

This table explores seasonality in the Confederate discount. We repeat the baseline regression (Specification 4 of Table 20) after replacing *Confederate* with *Confederate* interacted with each of the 12 calendar months. To parallel the quarterly analysis in the event-time tests, for each calendar month we report the quarterly average, defined as the average estimate across the subsequent quarter (i.e., month $t+1$, $t+2$, and $t+3$). The analysis excludes the quarter immediately following the three attention-grabbing events studied in Table 174.

Table IA1: L2 Dataset - Descriptive Statistics

This table reports descriptive statistics for the sample of Confederate and control houses provided by L2 data. The sample includes all Confederate houses in the state of Florida, and control properties, defined as houses in the same census block group. Panel A reports distinct number of houses, and regional districts for the tests reported in Tables 1 and 2 of the paper. Panel B reports pooled descriptive statistics for the owners of the house and house attributes collected from ATTOM assessor data, and Panel C reports descriptive statistics on the distance between Confederate and Control Houses. Variable definitions are provided in Appendix A.

Panel A: Sample Size					
	Total Homes	Unique Blocks-Groups	Unique Tracts	Unique Counties	
Confederate	1,943	248	199	41	
Control Houses	111,147	248	199	41	
Panel B: Distribution of Variables					
Variables	Mean	Std. Dev.	p25	Median	p75
Confederate	1.72%	13.00%	0.00%	0.00%	0.00%
Black	9.42%	29.21%	0.00%	0.00%	0.00%
College	62.60%	48.39%	0.00%	100.00%	100.00%
Democrat	22.09%	41.45%	0.00%	0.00%	0.00%
Income	$88,785	$58,690	$48,041	$72,000	$114,000
Buyer Age	60.97	16.12	50	63	73
House Size	2,240	1,092	1,500	2,000	2,700
Bedrooms	3.09	0.79	3	3	3
Bathrooms	2.24	0.58	2	2	3
Home Age (years)	33.88	21.16	17	30	47
Lot Size	45,902	113,925	8,000	11,000	26,000
Panel C: Distribution of Distance for Control Houses					
Distance	1.69	1.86	0.36	0.95	2.39
Confed_Near5	0.02	0.13	0	0	0
Confed_Near10	0.05	0.22	0	0	0
Confed_Near25	0.17	0.38	0	0	0
Confed_Near50	0.32	0.47	1	0	0

Table IA2: Residential Sorting on *Confederate Adjacent* Properties

This table reports estimates from regression of Confederate or *Confederate Adjacent* on demographic variables and controls. Confederate is an indicator that is equal to one if the house is on a Confederate Street and zero otherwise, and *Confederate Adjacent* is an indicator that is equal to one if the property is located within x miles of the closest Confederate property, where we set x equal to values ranging from 0.05 miles to 0.50 miles. The demographic variables and controls are defined as in Table 16, and the controls and fixed effects are identical to Specification 7 of Table 16. The t-statistics, computed from standard errors clustered at the census block-group level, are reported in parentheses. Below the regression estimates, we also report the estimated percentage effects, defined as the coefficient on *Demographic Score* scaled by the mean of the dependent variable.

	Confederate	<0.05	<0.10	<0.25	<0.50
Demographic Score	-0.87%	-0.11%	-0.51%	-0.91%	-0.58%
	(-4.13)	(-0.56)	(-1.43)	(-1.06)	(-0.52)
Log (Income)	0.19%	0.06%	0.39%	1.03%	1.94%
	(0.82)	(0.42)	(0.94)	(1.54)	(3.38)
Log (Age)	0.10%	0.12%	0.88%	1.88%	2.53%
	(0.39)	(0.34)	(0.77)	(1.27)	(1.77)
Log (House Size)	0.21%	-0.43%	0.10%	0.06%	2.33%
	(0.52)	(-1.03))	(0.07)	(0.05)	(1.47)
Log (Home Age)	-0.02%	0.08%	-0.37%	-0.78%	-2.06%
	(-0.04)	(0.32)	(-0.51)	(-0.51)	(-1.28)
Log (Lotsize)	-0.23%	-0.20%	0.16%	0.26%	2.87%
	(-0.49)	(-0.65)	(0.23)	(0.16)	(1.54)
Bed2	-0.20%	1.08%	-2.18%	-2.59%	-5.63%
	(-0.15)	(0.93)	(-1.00)	(-0.59)	(-1.75)
Bed3	-0.13%	1.33%	-1.70%	-1.37%	-2.42%
	(-0.11)	(1.12)	(-0.81)	(-0.32)	(-0.77)
Bed4	-0.17%	1.36%	-1.81%	-1.92%	-1.27%
	(-0.16)	(1.13)	(-0.80)	(-0.42)	(-0.37)
Bed5	-0.21%	1.34%	-2.21%	-3.48%	-4.13%
	(-0.13)	(1.09)	(-0.71)	(-0.59)	(-0.92)
Bath2	1.35%	0.19%	0.76%	1.21%	1.33%
	(0.97)	(0.52)	(0.7)	(0.62)	(0.77)
Bath3	1.13%	0.17%	0.67%	1.24%	-0.17%
	(0.89)	(0.45)	(0.67)	(0.65	(-0.10)
Bath4	1.03%	0.23%	1.31%	3.45%	3.13%
	(0.75)	(0.56)	(1.41)	(1.21)	(1.3)
Bath5	1.41%	0.60%	1.80%	5.60%	8.39%
	(0.81)	(0.95)	(1.4)	(1.54)	(2.44)
Observations	113,090	111,147	111,147	111,147	111,147
Block Group × PSM Percentile FE	Yes	Yes	Yes	Yes	Yes
Mean of Dep Var.	1.72%	1.68%	5.28%	17.08%	32.47%
Percentage Estimate					
Demographic	-50.76%	-6.37%	-9.60%	-5.32%	-1.79%

Table IA3. House Values and Confederate Street Names – The Role of Local Housing Market Conditions

This table reports Confederate discounts conditional on local housing market liquidity. Specifications 1-3 repeat Specification 4 of Table 20 after splitting the sample into *less liquid*, *liquid*, and *very liquid* housing markets. We define a property as *Less Liquid* if it located in a county that is in the bottom 80% of the distribution of price growth in the prior quarter, as reported by the Zillow House Value Index (ZVHI), *Liquid* denotes houses sold in the top 20% of the distribution, and *Very Liquid* refers to top 5% of housing markets. Specifications 4-6 augment Specifications 1-3 by including Confederate × *High Composite*, where *High Composite* is defined as in Table 23. Below the regression estimates, we also report the estimates on *Confederate* + *Confederate* × *High Composite*. Detailed variable definitions are provided in Appendix A. The t-statistics, computed from standard errors clustered at the census tract level, are reported in parentheses.

	Less Liquid (Bottom 80%) [1]	*Liquid* (Top 20%) [2]	*Very Liquid* (Top 5%) [3]	*Less Liquid* (Bottom 80%) [4]	*Liquid* (Top 20%) [5]	*Very Liquid* (Top 5%) [6]
Confederate	-3.49%	0.12%	-0.30%	-0.67%	1.71%	-0.27%
	(-3.11)	(0.07)	(-0.09)	(-0.57)	(0.77)	(-0.07)
Confed × High Composite				-7.46%	-3.58%	-0.11%
				(-3.40)	(-1.11)	(-0.01)
Confed + Confed High Comp				-8.12%	-1.87%	-0.38%
				(-4.08)	(-0.74)	(-0.06)
Controls and FE	Tract × Quarter × Age Fixed FE and Controls as in Table 20					
Observations	69,247	16,939	4,296	69,247	16,939	4,291
R-squared	87.76%	89.00%	87.93%	87.77%	89.00%	87.93%

Table IA4. Zillow -ATTOM Merged Dataset – Descriptive Statistics

This table reports descriptive statistics for the sample of Confederate and control house sales with housing information from ATTOM and listing information from Zillow. The sample begins in 2009 (the first year for which Zillow provides listing information) and ends in 2020. Panel A reports the distinct number of transactions, houses, and regional districts for the sample that examines the listing outcomes reported in Table 22 of the paper. Panel B reports descriptive statistics of house characteristics, and Panel C reports the correlations across the house characteristics. Variable definitions are provided in Appendix A.

Panel A: Sample Size

	Transactions	Houses	Streets	Block Groups	Tracts	Counties	States
Confederate	2,619	2,334	1,934	439	366	188	30
Control Houses	17,744	16,315	15,445	910	366	188	30

Panel B: Distribution of Housing Characteristics

	N	Mean	Std. Dev.	Skewness	p25	Median	p75
Confederate	20,363	0.13	0.33	2.22	0	0	0
End Price	20,363	$268,550.00	$369,519.00	$8.18	$118,000.00	$190,000.00	$295,000.00
Listing Price	20,363	$291,095.80	$423,708.00	$9.37	$127,000.00	$199,999.00	$310,000.00
Withdrawn	20,363	8.42%	27.78%	3.00%	0.00%	0.00%	0.00%
Slow Sale	20,363	20.00%	39.99%	1.50%	0.00%	0.00%	0.00%
Large Discount	20,363	19.58%	39.68%	1.53%	0.00%	0.00%	0.00%
Age	20,363	35.59	24.35	0.72	14	30	56

Panel C: Correlation Matrix

	Confederate	*End Price*	*Listing Price*	*Withdrawn*	*Slow Sale*	*Large Discount*	*Age*
Confederate	1	-0.05	-0.04	0.03	0.04	0.06	0.15
End Price		1	0.93	-0.03	-0.01	-0.24	-0.23
Listing Price			1	-0.04	0.02	-0.09	-0.22
Withdrawn				1	0.09	-0.06	0
Slow Sale					1	0.3	0
Large Discount						1	0.13
Age							1

Table IA5. Listing Outcomes for Confederate Street Names – Alternative Fixed Effects

This table repeats the analysis in Table 22 of the paper after adding Census Tract ×Age Quintile fixed effects.

	Withdrawn [1]	Slow Sale [2]	Discount [3]
Confederate	0.88%	1.89%	1.90%
	(1.2)	(2.32)	(2.21)
Log (House Size)	2.71%	8.78%	2.25%
	(2.16)	(4.29)	(1.31)
Log (Age)	0.07%	-0.07%	5.28%
	(0.09)	(-0.06)	(5.99)
Log (Lot Size)	0.43%	-0.90%	-0.69%
	(1.00)	(-1.14)	(-1.04)
Bed2	4.68%	-1.55%	-4.33%
	(2.19)	(-0.39)	(-1.08)
Bed3	2.43%	-3.66%	-6.50%
	(1.1)	(-0.94)	(-1.60)
Bed4	2.53%	-4.18%	-5.80%
	(1.07)	(-1.06)	(-1.34)
Bed5	1.55%	-5.65%	-3.24%
	(0.61)	(-1.36)	(-0.66)
Bath2	-0.58%	-2.91%	-4.43%
	(-0.78)	(-3.03)	(-4.20)
Bath3	-1.29%	-2.43%	-4.11%
	(-1.31)	(-1.76)	(-3.15)
Bath4	0.07%	-3.01%	-2.58%
	(0.05)	(-1.74)	(-1.38)
Bath5	-1.08%	3.69%	1.35%
	(-0.51)	(1.11)	(0.44)
Log (List Price)	-0.05%	8.06%	6.75%
	(-0.04)	(5.33)	(5.71)
Fixed Effects	Tract × Qtr. × Age Quintile		
Observations	20,363	20,363	20,363