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ESSAYS ON CORPORATE GOVERNANCE AND THE IMPACT OF REGULATION ON FINANCIAL MARKETS

PROEFSCHRIFT

ter verkrijging van de graad van doctor aan Tilburg University op gezag van de rector magnificus, prof. dr. E.H.L. Aarts, in het openbaar te verdedigen ten overstaan van een door het college voor promoties aangewezen commissie in de aula van de Universiteit op dinsdag 28 augustus 2018 om 10.00 uur door

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Antonino Emanuele Rizzo
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Chapter 1

Afraid to Take a Chance? The Threat of Lawsuits and its Impact on Shareholder Wealth

Abstract

The large costs of shareholder litigation in the U.S. pose a significant threat to companies. A first-order open question is whether shareholders gain from such a threat, because it disciplines managers, or whether the costs of the current litigation system outweigh the benefits. In this paper, I document that, on average, the threat of lawsuits has a negative net shareholder wealth effect. To identify this effect, I develop a novel empirical approach that exploits judicial turnover in federal district courts to generate exogenous variation in a firm’s likelihood of facing adverse legal outcomes. I find that a higher threat of a lawsuit, as proxied by a higher likelihood of facing investor-friendly judges in a district court, causes an economically large drop in the value of firms headquartered in that district. Additional tests support the view that the threat of lawsuits is harmful to shareholders because it stifles value-creating managerial risk-taking. On a broader level, my results suggest that fear of litigation may lead to substantial misallocation of capital in the U.S.
1. Introduction

Shareholder litigation can impose large costs on companies.\textsuperscript{1} A shareholder lawsuit absorbs managers’ attention, entails legal and settlement fees, and damages the company’s reputation. Over the last decades, these costs have provoked an intense debate about the optimal design of the shareholder litigation system. An important question in this debate is whether the threat that the large costs of a shareholder lawsuit pose to companies benefits or harms shareholders.

Theoretically, the answer is not obvious. I examine two conflicting hypotheses about the threat of shareholder litigation. The first one is the legal protection hypothesis, which argues that the threat of shareholder lawsuits is beneficial to shareholders, because it disciplines managers. This hypothesis is in line with studies in law and finance, which suggest that the threat of shareholder litigation helps solve agency problems (e.g. La Porta, Lopez-de Silanes, Shleifer, and Vishny (1998)). Similarly, there are studies in the legal literature that highlight the positive role of shareholder lawsuits in deterring managerial misbehavior (e.g. Cox (1997)). The second hypothesis, which I label the overdeterrence hypothesis, argues that the threat of shareholder lawsuits is harmful to shareholders, because it undermines managerial incentives to engage in value-creating but risky projects. According to this hypothesis, the reputation and career concerns associated with the threat of shareholder litigation impose excessive pressure on managers, which might generate managerial myopia (e.g. Stein (1988)) and discourage investments in innovation (e.g. Acharya and Subramanian (2009), Manso (2011)).

Ultimately, which hypothesis dominates and whether the net effect of the threat of shareholder lawsuits on shareholders is positive or negative are empirical questions. The answers to these questions have important implications for the design of the corporate governance and the shareholder litigation system. The aim of this paper is to provide an answer to these questions.

Empirically establishing which of the two hypotheses prevails is challenging because the threat of shareholder lawsuits is not randomly assigned. Two endogeneity concerns are particularly relevant. First, firm-level and local-level unobservables can affect at the same time the threat of

\textsuperscript{1}For example, previous studies document a drop in firm value between 9% and 14% around the filing of a shareholder lawsuit (e.g. Karpoff, Lee, and Martin (2008b), Gande and Lewis (2009)).
litigation and shareholder wealth, and therefore lead to biased inference. Second, a drop in stock prices can increase the probability that shareholders initiate a legal action against the company, such that causality runs from financial outcomes to litigation rather than the other way around.

To solve these issues, I develop a novel empirical approach that exploits variation in a federal district court’s corporation-friendliness generated by judicial turnover. Such variation influences the threat of lawsuits because it changes a firm’s probability of facing adverse legal outcomes. This identification strategy addresses the endogeneity concerns above in two ways. First, the rules of judicial independence in federal district courts guarantee that the timing and causes of a judge turnover are plausibly exogenous to firm characteristics. Federal judges vacate their office only upon death, resignation, or impeachment. Second, focusing on variation at the federal district level provides sufficient granularity to control for time-varying state-level factors. In particular, I compare the evolution of shareholder wealth among firms operating in the same state, but different districts. By doing so, I remove the effect of any unobserved state-level factor that may be correlated with judge turnover and responsible for the effect on firm outcomes. I use firm fixed effects and industry-date fixed effects to rule out other sources of confounding variation.

Using this new approach, I find that the *overdeterrence* hypothesis, which implies that a higher threat of a lawsuit harms shareholders, has stronger support in the data than the *legal protection* hypothesis. To discriminate between the two hypotheses, I study the effect of the threat of lawsuits on firm value. This is a natural choice, because market valuation incorporates all implications of the threat of shareholder litigation into a single forward-looking measure. I document that in federal districts where changes in courts’ judicial benches lead to increases in the threat of lawsuits, firms experience 35 basis points lower cumulative abnormal returns in the 21-day window around the events, compared to firms in the control group. Over longer horizons, this effect becomes even more pronounced. In districts where a judge turnover increases the threat of lawsuits, firms experience 1.5% lower abnormal stock returns over the next 12 months, compared to companies in the same state but not exposed to the increase in the threat of litigation.
These results are robust to a variety of alternative specifications, including different definitions of shareholder wealth, and limiting the sample to episodes of judge turnover due to death or due to reaching the retirement age. In a particularly restrictive test, I achieve finer granularity by comparing contemporaneous changes in corporate value among firms headquartered in different districts within the same metropolitan statistical area. This test allows me to rule out within-state unobserved heterogeneity.

At the core of my empirical strategy is the idea that we can use a court’s attitude toward corporations to identify variation in the threat of shareholder litigation. I construct the measure of courts’ attitudes toward corporations as the average political ideology of all active judges of a federal district court. To classify political ideologies, I use the liberal-conservative dichotomy: a conservative judge is more corporation-friendly than a liberal judge. The use of political ideology to proxy for a judge corporation-friendliness finds support in a large political science literature. For example, Epstein, Landes, and Posner (2012) state, and empirically show, that “Justices appointed by Republican Presidents are notably more favorable to business than Justices appointed by Democratic Presidents”.2

An important assumption underlying the use of courts’ attitudes to measure the threat of shareholder litigation is that variation in corporation-friendliness has a material impact on a firm’s expected legal outcome. I provide two tests of this assumption, using a sample of shareholder class action lawsuits filed between 1996 and 2015. First, I document that facing a more liberal judge in courts implies a higher likelihood of adverse legal outcomes for firms. A one standard deviation increase in judge investor-friendliness leads to a 4% higher probability that the class action lawsuit is not dismissed and that investors manage to obtain a settlement. Second, I show that firms experience a larger value loss upon the filing of a shareholder lawsuit when courts are more investor-friendly. A larger negative stock price reaction suggests that the market anticipates that higher court investor-friendliness leads to higher settlement probability and larger potential settlements.

In the second part of the paper, I investigate the economic channel underlying the negative impact of the threat of shareholder litigation on firm value. The results are consistent with the specific channel predicted by the overdeterrence hypothesis: in response to a higher threat of lawsuits, managers have an incentive to invest in projects that make litigation less likely, even if these projects are not maximizing the value of the firm. For example, it may be privately optimal for the manager to forgo a risky positive-NPV project that raises the likelihood of a lawsuit. I provide four tests of this hypothesis.

First, I analyze corporate investment policies. In line with the predictions of the overdeterrence hypothesis, firms exposed to more investor-friendly courts invest less in risky projects, as measured by R&D expenditures (e.g. Hall and Lerner (2010)), and generate fewer patents. In addition, I document that firms become less risky, as measured by stock volatility or idiosyncratic volatility, when there is a higher likelihood to face more investor-friendly judges.

Second, I explore whether managers facing a higher threat of shareholder litigation take other actions to decrease the probability of a lawsuit. Since earnings, and in general negative accounting performance, are among the leading causes of shareholder lawsuits (e.g. Field, Lowry, and Shu (2005)), managers facing a higher threat of lawsuits should have stronger incentives to report positive financial results, and may choose to inflate their earnings through the use of aggressive accounting practices. In line with this prediction, I find that firms exposed to a higher threat of shareholder lawsuits have higher announced earnings and more positive earnings surprises. In addition, firms exposed to a higher threat of shareholder lawsuits have larger discretionary accruals.

Third, I study the impact of the threat of lawsuits on a different group of stakeholders: bondholders. Studying bondholder reactions is informative because the overdeterrence hypothesis has clear implications for this group of claimholders. Given their payoff structure, bondholders bear most of the costs of a risky project failure without capturing the majority of the benefits. Therefore, they may prefer a low risk project to a high risk one, even if the high risk one has a larger NPV. As a consequence, the managers’ behavior predicted by the overdeterrence
hypothesis should have a positive effect on bondholders. In line with this, I observe that in districts where the threat of lawsuits increases due to judge turnover, firms have higher abnormal bond returns around the event, compared to firms in the other districts of the same state. These results also show that the baseline finding of a negative relation between investor-friendliness and shareholder wealth is not simply reflecting higher expected losses from lawsuits, because in that case, bondholder returns should decrease alongside shareholder returns. The fact that bondholder and shareholder wealth move in opposite directions suggests that managers are actively shifting the risk of the firm.

Finally, I explore whether cross-sectional heterogeneity in companies’ responses to changes in the threat of lawsuits supports the *overdeterrence* hypothesis. A direct prediction of this hypothesis is that the negative effect on shareholder wealth should be more pronounced when managers’ reputation and career concerns are stronger. In line with this prediction, I document a stronger *overdeterrence* effect among firms with highly reputed CEOs, and thus where the CEO’s concerns to protect her reputation are higher (e.g. Diamond (1989)). In addition, I show that the adverse effect of the threat of lawsuits on shareholder wealth is stronger among firms more vulnerable to financial distress. In financially vulnerable firms, the risk of employment loss for the manager, and thus her career concerns, are magnified (e.g. Gilson (1989), Eckbo, Thorburn, and Wang (2016)). These cross-sectional tests raise the bar for alternative explanations. Any alternative story must be able to explain not only the negative relation between the threat of lawsuits and firm value, but also the observed cross-sectional heterogeneity in firms’ responses.

In sum, I bring substantial new evidence to the debate on the current U.S. litigation system. Collectively, my results suggest that the threat of a shareholder lawsuit has unintended economic consequences on companies. Results also suggest that the managers’ reaction to a higher threat of lawsuits is important for understanding the negative impact of the threat of lawsuits on firm value. Managers may choose suboptimal levels of corporate risk-taking in order to reduce the probability of incurring the large costs coming from shareholder lawsuits. This behavior results in value loss due to forgoing profitable investment opportunities. A higher threat of lawsuits can
therefore lead to deadweight costs to firms and to the overall economy.

This paper makes two contributions to the literature. First, it contributes to the broad literature that investigates how different institutional arrangements protecting the rights of shareholders affect corporate outcomes, such as firm valuation (e.g. La Porta, Lopez-de Silanes, Shleifer, and Vishny (2002), Shleifer and Wolfenzon (2002), Claessens, Djankov, Fan, and Lang (2002)), dividend payout (e.g. La Porta, Lopez-de Silanes, Shleifer, and Vishny (2000)), and access to finance (e.g. Reese and Weisbach (2002)). I add to this literature by studying the net shareholder wealth effect of a specific legal rule, the shareholders’ right to sue the company and its officers, and I document that this effect can be negative.

Second, this paper contributes to the literature that studies managerial incentives to invest in long-term risky projects. Previous papers focus on the impact of takeover pressure (e.g. Stein (1988), Atanassov (2013)), investor protection (e.g. John, Litov, and Yeung (2008)), the fear of early project termination by outside investors (e.g. Von Thadden (1995)) and characteristics of managerial contracts (e.g. Manso (2011), Ederer and Manso (2013)). This paper adds to this literature by showing that the fear of shareholder litigation plays a significant role in shaping managers’ incentives to engage in risky projects, and leads them to boost short-term earnings at the expense of long-term growth.

This paper is closely related to recent studies by Appel (2016) and Lin, Liu, and Manso (2017). The empirical identification of these papers relies on the staggered adoption of the universal demand laws in U.S. states, which impose significant hurdles to derivative lawsuits. Appel (2016) documents that the threat of stockholder lawsuits tends to improve firms’ corporate governance. By contrast, Lin, Liu, and Manso (2017) show that increasing the risk of litigation at the state level stifles innovation. These studies provide valuable evidence on individual dimensions relevant to firm value, but with opposite signs. My study, by focusing on a net measure of shareholder wealth, can shed light on the the sign of the overall effect on shareholders, which may be more informative for evaluating the efficiency of the current litigation system. Furthermore, an advantage of my identification strategy, over strategies that exploit state-level variation, is
that I can account for within-state unobserved heterogeneity, which can be important if local factors influence the threat of shareholder litigation.

2. Does the Threat of Shareholder Lawsuits Benefit or Harm Shareholders?

Whether the threat of shareholder litigation is beneficial or harmful for shareholders is theoretically unclear, and in this section I provide further details to support this claim.

Specifically, I consider two conflicting hypotheses about the impact of the threat of lawsuits on shareholder wealth. According to the first hypothesis, the legal protection hypothesis, the threat of shareholder litigation is beneficial, because it improves investor protection against managerial misbehavior. This hypothesis is in line with studies in law and finance. For example, in the seminal law and finance paper of La Porta, Lopez-de Silanes, Shleifer, and Vishny (1998), the authors include shareholder access to courts in their investor legal protection index.

There are several reasons why better investor protection might be associated with higher firm value. First, strong legal protection of investors can lead to lower expropriation because it lowers the expected benefits from diversion. As a consequence, since the private benefits of control are smaller, investors are willing to pay more for the firm assets and this raises firm value (e.g. La Porta, Lopez-de Silanes, Shleifer, and Vishny (2002), Shleifer and Wolfenzon (2002)). Second, better investor protection can lead to increased corporate risk-taking and higher firm value because it reduces insider incentives to protect their private benefits (e.g. John, Litov, and Yeung (2008)). Third, strong investor rights enhance external pressure on managers. This can limit overinvestment in declining industries and lead to more efficient and value-enhancing capital allocation (e.g. Wurgler (2000)). Fourth, better investor protection can increase shareholder wealth because it improves access to stock market financing and reduces the cost of capital. This, in turn, facilitates the exploitation of firm growth opportunities (e.g. Doidge, Karolyi, and Stulz (2004)) and investments in riskier but value-creating projects (e.g. Brown, Martinsson, and
Petersen (2013)). Finally, the external pressure imposed by the threat of shareholder lawsuits can incentivize managers to institute shareholder-friendly governance practices (Appel (2016)). To the extent that these practices represent an improvement in the corporate governance of firms, this can lead to higher firm value (e.g. Gompers, Ishii, and Metrick (2003), Durnev and Han (2005)).

The second hypothesis, the *overdeterrence* hypothesis, posits that the threat of lawsuits can have negative consequences for shareholders. According to this hypothesis, an increased shareholder litigation risk deters managers from engaging in value-creating but risky projects. This argument builds on two related strands of literature. The first strand of literature documents that excessive external pressure can distort managers’ incentives and lead them to focus on short-term gains at the expense of the long-term interests of shareholders. For example, the pressure on managers imposed by the threat of takeovers can generate managerial myopia (e.g. Stein (1988)). Similarly, Von Thadden (1995) shows that short-term biases of investments can also arise due to the fear of project termination by outside investors. The second strand of literature documents that legal, institutional and contractual factors that punish failures can erode managerial incentives to invest in innovation. Acharya and Subramanian (2009) document that creditor-friendly bankruptcy laws that generate excessive liquidation upon failure can stifle ex-ante firm risk-taking. Manso (2011) and Ederer and Manso (2013) show that managerial incentive schemes should exhibit tolerance for short-term failure and reward for long-term success if they are to stimulate high-risk innovative investments. In a recent working paper, Lin, Liu, and Manso (2017) show that tightening the risk of litigation at the state level can hinder managerial discretion and stifle innovation.

There are two reasons why shareholder litigation imposes external pressure on managers. First, shareholders may initiate legal actions even absent corporate fraud. Such actions are triggered by stock prices drops, which are attributed to managers’ wrongdoings, but can simply be the consequences of the failures of law abiding business decisions. This is consistent with the evidence on “frivolous” lawsuits. This definition refers to lawsuits filed whenever there is
a significant drop in a firm’s stock price, without proper investigation about any underlying culpability of the firm and thus lacking strong legal merit. The sole purpose of these claims is to extract settlement fees from the company (e.g. Bebchuk (1988)). Second, shareholder litigation involves the risk of direct and indirect losses for managers. In terms of direct losses, officers are personally liable if they are found to have breached their fiduciary duties. In terms of indirect losses, managers found culpable of corporate misconduct often lose their jobs, face diminished employment prospects and, in general, suffer from reputation losses (e.g. Karpoff, Lee, and Martin (2008a), Fich and Shivdasani (2007), Brochet and Srinivasan (2014)).

To sum up, the tension between these two hypotheses emphasizes an existing gap on the desirability of shareholder litigation rights. The net shareholder wealth effect of the threat of lawsuits is ex ante ambiguous.

### 3. Empirical Strategy

#### 3.1 Measuring the Threat of Lawsuits

Two problems emerge when studying the impact of the threat of lawsuits on shareholder wealth. First, the threat of lawsuits is inherently difficult to measure, because it is not directly observable. Second, the threat of lawsuits is not randomly assigned to companies. One source of potential endogeneity concerns are firm-level unobservables, which can affect both the threat of lawsuits and firm value. For example, the quality of the firm’s corporate governance can both influence firm value (e.g. Gompers, Ishii, and Metrick (2003)) and managerial propensity for misbehavior. Similarly, higher managerial quality can have a positive impact on firm value and at the same time can reduce the likelihood of a lawsuit (Field, Lowry, and Shu (2005)).

To address these issues, I measure the threat of lawsuits faced by a company using the attitude towards corporations of the federal court of the district where the company is headquartered. I define a court’s attitude towards corporations as the average political ideology among all active judges operating in the court. To infer a judge’s political ideology, I rely on the plausible
assumption that politicians tend to nominate judges that share their political views. This assumption implies that one can proxy for the political ideology of a federal district court judge using the political views of the president and state senators who nominated the judge. Once I determine whether a judge is Republican or Democrat, I classify the former as being more corporation-friendly than the latter.

To understand why a court’s attitude towards corporations can be used to generate variation in the threat of lawsuits, it is informative to describe the threat of lawsuits as the product of two components: the probability that shareholders initiate legal action against the company and the expected litigation outcome in court. A company defendant in a shareholder lawsuit faces a judge that is randomly selected from the federal district’s panel of judges. The political ideology of the judge assigned to the case, in turn, influences the expected outcome of the lawsuit. Therefore, the first-order effect of a judicial turnover that changes a court’s corporation-friendliness is to change the second component of the threat of lawsuits, namely the expected litigation outcome. As a second-order effect, a change in a court’s corporation-friendliness can also influence the probability that shareholders file a lawsuit, precisely because the expected benefits of doing so have changed. This assumption is in line with a large political science literature (e.g. Epstein, Landes, and Posner (2012)). Moreover, I provide direct empirical support for the impact of a judge’s ideology on litigation outcomes in section 4.4.

3.2 Identification Strategy

I argue that variation in a court’s corporation-friendliness can be exploited to obtain exogenous variation in the threat of lawsuits faced by a company. This argument rests on two key elements. First, the timing and the causes of variation in a court’s attitude are plausibly exogenous from the viewpoint of a firm headquartered in a given district. The reason is that variation in a court’s corporation-friendliness originates solely from judicial turnover, because courts’ attitudes towards corporations depend on individual judges’ political ideologies. At the same time, the rules of judicial independence in federal district courts ensure that federal judges vacate their
office only upon death, resignation or impeachment. These causes are presumably exogenous to firm characteristics. I provide further details on the frequency and types of judicial turnover in section 4.2.

While judges leaving the bench is plausibly exogenous (most clearly in the case of death), the choice of the new judge is not. First, since the president nominates the new judge, changes in a court’s attitude might be correlated with political cycles at the national level, which are known to affect firm outcomes (Santa-Clara and Valkanov (2003), Belo, Gala, and Li (2013)). Second, the senators of the state in which the judge takes office are crucial in the nomination and approval process. This suggests that it is important to control for time-varying local variables that might influence both shareholder wealth and the threat of lawsuits.

The second key element of my approach addresses these issues. I exploit the fact that the most economically relevant U.S. states include multiple districts within their borders. This provides me with sufficient granularity to rule out state-level time-varying unobserved heterogeneity. I implement this solution by including state × date fixed effects in the regression. Provided that the state is the relevant dimension for the unobserved local factors, this solution removes local sources of unobserved heterogeneity. To further control for unobserved heterogeneity, I include district and industry × date fixed effects. District fixed effects control for any time-invariant unobserved heterogeneity between federal district, like an historical political proclivity of a district court toward republicans or democrats. Industry × date fixed effects take care of potential time-varying omitted factors at the industry level. These factors may confound my analysis if firms belonging to certain industries tend to cluster in specific districts.

In the baseline setting, thus I estimate the following model:

\[ y_{it} = \alpha_i + \gamma_{i(s)t} + \lambda_{i(j)t} + \beta LThreat_{i(k)t} + X_{it} \delta + \epsilon_{it} \quad (1.1) \]

where \( y \) is the outcome of interest for firm \( i \), in period \( t \). The variable \( LThreat \) is the measure of a court’s attitude toward corporations, and its construction is detailed in section 4.2. \( X_{it} \) is a matrix of firm-level control variables. \( \alpha_i \) are firm fixed effects. \( \gamma_{i(s)t} \) are state × date fixed effects.
\( \lambda_{i(j)t} \) are industry \( \times \) date fixed effects. The identifying assumption for \( \beta \) to provide an unbiased estimate of the causal effect of the threat of lawsuits on shareholder wealth is that conditional on the inclusion of firm-level controls and the set of fixed effects, my measure of the threat of lawsuits is as good as randomly assigned.

4. Data and Descriptive Statistics

4.1 Federal District Court Data

The U.S. federal court system includes 94 district courts in the 50 states, Washington, D.C., Puerto Rico, Guam, U.S. Virgin Islands, and Northern Marianas Islands. This means that there is at least one district court in each state, with larger states having between two and four districts. Appendix B and Figure C.1 illustrate how the U.S. federal court system is split into the 12 circuits and the 94 district courts. The inclusion of state \( \times \) date fixed effects implies that the variation I exploit to estimate my coefficients of interest comes from states with more than one district court. This poses no concern for the representativeness of my sample, as roughly 80% of the CRSP-Compustat merged firm-year observations pertain to multiple-courts states.

Federal courts have subject matter jurisdiction over all cases based upon federal laws. Among these, lawsuits arising from violation of federal securities laws have a prominent place. Both in terms of number of settlements in federal class actions and in terms of total dollar value of these settlements, securities lawsuits are the single most important type of lawsuits in federal courts (Eisenberg and Miller (2010)). For example, Fitzpatrick (2010) reports that securities class action lawsuits account for over 70% of the total dollar value of settlements coming from a set of federal lawsuit types including: Securities, labor and employment, consumer, employee benefits, civil rights, debt collection, antitrust, and commercial.

To link firms to federal district courts, I use firm headquarters location. Therefore, I focus on a firm’s home court, ignoring the other possible venues in which plaintiffs can file a suit. This choice is justified if the firm’s home court is the most relevant court from the viewpoint
of a company. Evidence shows that the firm’s home court is indeed the most important court for a company, which provides support for my approach. For example, Cox, Thomas, and Bai (2009) report that, according to many practicing attorneys, it is highly impractical for them to file a securities class action suit in a venue that is different from the defendant’s headquarters. The company would immediately present a likely successful motion to relocate the suit, and such a motion would be highly time consuming and expensive. As a result, to avoid these costs, plaintiffs file directly in the firm’s home district. The authors show that in their sample 85% of class action lawsuits are filed in the district court of the company’s headquarters. Using the sample of securities class action lawsuits from the Stanford Securities Class Action Clearinghouse, I document a similar percentage (84%).

4.2 Judge Ideology Data

I obtain information about the identity of judges in U.S. federal district courts from the History of the Federal Judiciary available on the Federal Judicial Center website. In each year, I consider all active judges, excluding senior judges.\(^3\) To classify a judge as being corporation-friendly or investor-friendly, I adopt the traditional conservative/liberal distinction: I define conservative judges as being more pro-business than liberal judges. I use the ideology score developed by Giles, Hettinger, and Peppers (2001) to measure a judge’s liberality or conservativeness.\(^4\) Starting from the NOMINATE Common Space score of Poole and Rosenthal (1997), the ideology score identifies individual judges’ policy preferences by computing the mean common space score for the state congressional delegation of the president’s party in the year of the judge’s appointment (Giles, Hettinger, and Peppers (2001)). Therefore, the underlying intuition is that the ideology of the president and the relevant senators who nominated the judge is a strong indication of the

\(^{3}\)Senior status is a form of semi-retirement for U.S. federal judges. I choose to exclude senior judges for three reasons. First, it is a discretionary choice of the judge to take senior status instead of full-retirement. Second, there is heterogeneity in the caseload of senior judges and again it partially depends on the individual judge’s choice. Third, a judge that takes senior status still creates a vacancy. In any case, I repeat my main tests using a measure that includes senior judges, and the results are virtually unchanged.

orientation of the judge itself. The ideology score ranges from -1, for most liberal judges, to +1, for most conservative judges. For ease of interpretation, I reverse the score multiplying it by -1. Therefore, higher scores will be associated to more liberal, and hence investor-friendly, judges. Finally, I aggregate these scores at the district court level by taking the mean, obtaining a measure of the average attitude toward corporations of each of the 94 U.S. federal district courts. I label this measure $LThreat$.

The classification of a conservative judge as being more pro-business than a liberal judge is supported both by conventional wisdom and previous research. First, Republicans, are traditionally viewed as the pro-business party. In addition, such dichotomy naturally emerges by looking at the pattern of legislative reforms in the 20th and early 21st centuries. Coffee (2015) points out that, in terms of legislative decisions, “the two major political parties in the United States have aligned themselves with the rival camps - Democrats with the plaintiff’s bar; Republicans with the business community”. Second, this classification finds strong support in the political science literature (e.g. Rowland and Carp (1996), Haire, Lindquist, and Hartley (1999), Epstein, Landes, and Posner (2012)). In subsection 4.4, I also provide direct empirical evidence in support of this classification.

Table 1, Panel A, presents summary statistics for the judge and court related variables. This panel shows statistics about the number of judges in federal district courts, the total number of turnover events, as well as the average turnover per year in federal district courts. The numbers indicate that, on average, there is a full turnover of courts every 9.7 years (7.14/0.74). In the last row of the panel, I show statistics about the changes in $LThreat$ driven by changes in the panel of judges. In Table C, I report summary statistics about judges and turnover episodes separately for each district court. This table shows the average number of judges, the distribution of turnover episodes, the average number of turnover per year, and the average value of $LThreat$ in all district courts.
4.3 Firm-Level Data

My firm-level sample includes all companies in the CRSP-Compustat merged dataset, for the fiscal years from 1993 through 2015. I exclude financial firms (SIC codes 6000-6999), regulated utilities (SIC codes 4900-4999) and firms headquartered outside the US.

Since the headquarters address reported in Compustat tapes is the current location of a firm’s principal executive office, not the historical one, I follow Heider and Ljungqvist (2015) and extract company historical headquarters addresses from regulatory filings. When I am not able to extract the headquarters location from a SEC filing, I complement this data with information in the WRDS SEC Analytics Suite. The starting year of my sample is dictated by the availability of historical headquarters information from these two sources. In the next step, I match the zip code of a firm headquarters address to the U.S District Court with jurisdiction over the corresponding area.

I obtain a mapping of companies’ zip codes to the corresponding metropolitan statistical areas (MSA) from the Missouri Census Data Center website. When the information for a given zip code is missing, I complement this data with the mapping provided by the U.S. Census Bureau.\(^5\) If I cannot link a zip code to a MSA or CSA, I code the observation as missing.\(^6\)

To construct the sample of bond returns, I obtain trades reported to FINRA TRACE between July 2002 and December 2015. As TRACE was first implemented on July 1, 2002, the number of observations in tests using bond returns is significantly lower than in other tests of the paper. I collect information about additional characteristics of bonds, such as time-to-maturity and rating, from the Fixed Income Security Database (FISD). I follow Bessembinder, Jacobsen, Maxwell, and Venkataraman (2016) in the choice of the filters to apply in order to construct the bond sample. Moreover, I adopt the approach suggested in Bessembinder, Kahle, Maxwell, and Xu (2008) to obtain firm-level bond abnormal returns. Specifically, I exclude non-investment grade bonds, and when a firm has multiple bonds outstanding, I consider the firm as a portfolio of

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\(^5\) Data from the Missouri Census Data Center is available at http://mdc.missouri.edu/websas/geocorr12.html. U.S. Census Bureau data is available at https://www.census.gov/population/metro/data/other.html.

\(^6\) 8.53\% of the observations in my sample cannot be linked to any MSA.
bonds and I compute the value-weighted average bond return.

Table 1, Panel B reports statistics about stock return variables. These are the main dependent variables of the paper, and I discuss them in detail below. Finally, Table 1, Panel C shows statistics on the other firm-level variables used below.

4.4 Shareholder Class Action Lawsuits

In this section, I start by providing some details on shareholder class action lawsuits, and the sample I use in the empirical analysis. I collect information on shareholder class action lawsuits from the Stanford Securities Class Action Clearinghouse. The dataset includes all securities class action lawsuits filed in federal courts between 1996 and 2016. For each lawsuit, I obtain information about the filing date, the district court, the identity of the judge assigned to the case and the status of the case. The large majority of these lawsuits (89% according to Kim and Skinner (2012)) allege a violation of SEC Rule 10b-5, which means that the plaintiffs claim that the defendant released materially false and misleading voluntary disclosures or regulatory filings conveying overly positive firm prospects (e.g., earnings growth).

A well-developed line of literature (e.g. Romano (1991), Alexander (1991), Choi (2004)) has focused on documenting the issue of “frivolous” litigation: companies that experience a large drop in their stock price in a short period of time face a high likelihood of a securities fraud class action, regardless of the existence of any underlying fraud on the part of the company or its officials. However, defendants in these non-meritorious lawsuits still face a positive probability that the court find them guilty. In addition, getting rid of even frivolous litigation is not free of costs: a fraud class action lawsuit may last a significant amount of time, during which the company incurs lawyers’ fees, the costs of negative publicity with both customers and suppliers, and executives suffer from the distraction coming from dealing with discovery. In sum, managers have incentives to reduce the incidence of litigation, and in order to do so it makes sense to avoid large swing in stock prices. I exploit this line of reasoning when exploring the economic mechanism in section 6.2.
Before moving to the main analysis of the paper, I first confirm empirically that shareholder lawsuits involve large losses, and thus a significant threat, for the companies and their managers. Moreover, I provide two tests supporting the use of courts’ political attitudes as a measure of the threat of lawsuits. Panel A of Table 2 shows statistics about the abnormal stock returns around the announcement of a shareholder class action lawsuit. I define firm abnormal stock returns as the cumulative abnormal returns (CAR) from the Fama-French 3-factor model over the 11-day event window around the announcement of a class action. The average (median) 21-day FF-3 factor CAR is \(-10.54\% \, (\,-5.86\%\,).\) These numbers are broadly consistent to those reported by previous studies. For example Gande and Lewis (2009) report average \([-10,1]\) cumulative abnormal return (in excess of CRSP value-weighted index) equal to \(-14.45\%.\) This indicates that the announcement of a shareholder class action lawsuit leads to an economically large loss in shareholder wealth for the firm, and thus represents a significant concern for managers.

In Panel B of Table 2, I run a linear probability model to show that the ideology of the judge assigned to the case has a significant impact on the probability of a negative outcome in court for the company.\(^7\) The dependent variable is an indicator equal to 1 when the class action lawsuit ends with a settlement or with a trial outcome favorable to the investors, and 0 otherwise. The main independent variable is the ideology of the judge assigned to the case, as defined earlier in this section. Across all specifications, the judge ideology coefficient is positive and significant. Thus, results indicate that an increase in the investor-friendliness of the judge assigned to the case leads to a higher probability of an adverse legal outcome for companies.

In Panel C of Table 2, I test whether the court’s investor-friendliness influences the negative stock price reaction around the filing of a shareholder class action lawsuit. If an increase in investor-friendliness of a court leads to a higher probability that investors obtain a settlement, and if it increases the size of the potential settlement, this should be reflected in stock prices, as the market anticipates larger expected losses for the company. Coefficients in Panel C indicate that firms exposed to one standard deviation increase in investor-friendliness of courts have 2.3% lower FF 3-factor CAR in the 11-day window around the filing of a class action lawsuit. This

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\(^7\)Using a probit model, instead of a linear probability model, yields qualitatively very similar results.
effect corresponds to 22% of the sample average. Collectively, results in Panel B and C support the assumption that an increase in the proportion of investor-friendly judges in a district court leads to a higher threat of lawsuits for firms.

5. Main Results

The main objective of this paper is to test whether the data is more consistent with the legal protection hypothesis or the overdeterrence hypothesis. To do this, I examine stock market returns. The stock market provides the perfect laboratory to discriminate between the two hypotheses, as market valuation reflects investors’ expectations about all factors relevant to future performance. This is useful, since the threat of lawsuits may simultaneously affect multiple firm-level outcomes. Studying stock market reaction thus allows me to measure the net effect of the threat of shareholder lawsuits on firm value.

5.1 Short-Term Market Response to Changes in the Threat of Lawsuits

In this section, I use an event study approach to examine the short term impact of changes in a court’s judicial panel on stock returns. I define an event as the day in which a change in the panel of judges generates an increase in a court’s average ideology score. As event date, I select the date in which the Senate confirms the President’s nomination. Firms headquartered in federal districts experiencing the increase in the threat of lawsuits constitute the treatment group. I construct a control group by selecting, for each event, all companies operating in the same state of the event court but in federal districts whose courts’ average ideology scores do not increase. I consider a symmetric event window of 21 days ([−10, 10]) around the change in a court’s judicial panel. This choice is driven by the fact that it is difficult to establish with precision the exact date in which investors incorporate into prices the information about the change in the panel of

\[ \text{The results in this section are robust to alternative event day definitions, including changes determined both by judges leaving the court and by new judges joining the bench.} \]
judges. In addition, investors may rely on different sources and thus acquire the information at slightly different points in time.\textsuperscript{9}

Table 3 shows that the threat of lawsuits has an economically large and statistically significant negative effect on short-term event returns. The reported estimates are obtained by running the regression in equation (1.1). As dependent variable, I use the CAR\([-10,10]\) from the Fama-French 3-factor model estimated over the \([-231, -31]\) interval (column (1) to (3)). The inclusion of district court fixed effects removes time-invariant unobserved differences among federal districts. The use of state \(\times\) date fixed effects effectively allows the comparison between treatment firms and the control group, composed of firms headquartered in the other districts of the same state. I include industry \(\times\) date fixed effects to take care of time-varying unobserved differences between industries. Finally, in column (3) I substitute district court fixed effects with the slightly more stringent firm fixed effects, and the results remain robust. Intuitively, these regressions compare firms headquartered in the Ohio Northern District with firms headquartered in the Ohio Southern District. Thus, regression coefficients are estimated by exploiting variation coming from the different evolution of shareholder wealth in the Ohio Northern District in response to a change in the panel of judges, compared to the evolution of shareholder wealth in the Ohio Southern District, where firms do not experience a change in their court’s attitude. The coefficient in column (2) indicates that firms in the treatment group have 33 basis points lower abnormal returns in the 21 days around a judge turnover event, compared to firms in the control group.

To address reverse causality concerns, I study the dynamic effect of changes in the threat of shareholder litigation. In particular, a potential concern may be that the choice to nominate a judge with specific orientations might be correlated to characteristics of firms headquartered in the district. If this is true, however, I should observe an “effect” of the change in courts’ judicial benches also before the change itself takes place. Figure 1 plots cumulative point estimates from a modified version of equation (1.1), where I allow the effect of changes in courts’ judicial panel to vary across days, from day -30 to day 30. The dependent variable is the daily abnormal return

\textsuperscript{9}The results are robust to using a shorter (11-day) event window or a longer (41-day) event window. However, if I shrink the event window down to less than 11 days the coefficients are not significant, although with the correct sign.
from the Fama-French 3-factor model. The figure shows that the pattern of daily abnormal
returns is consistent with a causal impact of the threat of shareholder lawsuits on stock market
reaction: There is a dramatic difference in the pattern of stock market returns after the change
in a court’s panel of judges, but there is no evidence of a pre-trend before the event.

To obtain the results reported in Table 3, I use all episodes of judicial turnover that increase
the degree of investor-friendliness of courts, regardless of the actual size of the increase. However,
it is interesting to study whether the magnitude of the effect on stock returns grows as the increase
in the threat of lawsuits becomes bigger: more substantial variation in court attitudes should
cause larger stock price reactions. In Table C.3, I document that this is indeed the case. To do
that, I focus on episodes of turnover that generate particularly large increases in the threat of
lawsuits. Specifically, I restrict the sample to increases at least equal to the 75th percentile of
the distribution of changes. The first two columns of the table show that the effect on short-term
stock returns is between 2 and 2.8 times bigger than in the baseline test of Table 3. This is
reassuring, and it provides further support for the use of court attitudes as measure of the threat
of lawsuits.

5.2 Long-Term Stock Returns

Understanding the implications of a change in courts’ attitudes for firm value involves a high
degree of processing complexity. Investors might be slow to incorporate into stock prices the
effect of intangible, hard to process information (Cohen, Diether, and Malloy (2013)). Therefore,
the short-term stock price reaction documented in the previous section might not capture the
full effect of a higher threat of lawsuits on firm value.

To explore this possibility, I study stock returns over a longer period of time. I rely on the
same event study setting used to document the short-term results: an event is defined as the date
in which a change in the panel of judges leads to an increase in a court’s investor-friendliness.
Treated firms are firms operating in districts where the turnover event occurs. Control firms are
firms operating in the other districts of the same state. In contrast to section 5.1, the dependent
variable is the 12-month buy-hold size-BM adjusted stock return.

The last 3 columns of Table 3 documents a negative reaction of stock prices to increases in $L\text{Threat}$ over the 12 months after the event. The reported estimates are obtained by running equation (1.1) with long-term event returns as dependent variable. Coefficients indicate that the cumulative effect in the next 12 months is five times as big as the stock price reaction in the 21-day window. Specifically, column (5) shows that in district where the threat of lawsuits increases, firms experience 1.3% lower size-B/M buy-hold abnormal returns over a 12-month event window, compared to firms in the control group.

As in the previous section, I deal with the issue of reverse causality. I use again the modified version of equation (1.1), which allows me to study the dynamic effect of a turnover in district courts on shareholder wealth. In this section, I consider the month in which a court becomes more investor-friendly as $month = 0$, and I include indicator variables for months $-12$ to $+12$ in event time. Figure 2 plots the cumulative point estimates of this set of dummies for months in event time. The graph can be interpreted as the difference in Fama-French 3-factor cumulative abnormal returns between firms headquartered in a district that experiences the event and the control group. There seems to be no “effect” of judge turnover before the change occurs, which is supportive of a causal interpretation of the results.

Finally, in the last two columns of Table C.3, I document that more substantial increases in the threat of lawsuits lead to larger negative long-term stock returns. This result is complementary to the one reported in the previous section, and increments the confidence in the use of judicial turnover to capture variation in the threat of lawsuits.

5.3 Robustness Tests

My interpretation of the results presented in the previous section is that an increase in the threat of lawsuits causes a decline in shareholder wealth. However, there are other plausible explanations for these results. A particularly relevant one is that I might be capturing local effects that correlate with changes in courts’ judicial benches. Previous studies show how local
economic variables might significantly affect firm outcomes, such as stock returns or investments (e.g. Pirinsky and Wang (2006), Dougal, Parsons, and Titman (2015)). These local variables operate at a finer geographical level than the state. Thus, if they correlate with my measure of the threat of lawsuits, then state × date fixed effects will not remove all relevant local unobserved heterogeneity, and the coefficient $\beta$ will be biased.

To address these concerns, I exploit the geographical flexibility offered by the fact that common definitions of local economic areas, such as Metropolitan Statistical Area (MSA), do not overlap with federal districts: the largest MSAs include, within their borders, portion of multiple districts. The MSA is one of the most widely used definitions of local area in the literature on local effects (Pirinsky and Wang (2006), Kedia and Rajgopal (2009), John, Knyazeva, and Knyazeva (2011)). This allows me to change the control group in the event study setting. Specifically, in this section control firms are firms headquartered in the same metropolitan statistical area (MSA), but in different districts. To make sure that the coefficients of interests are estimated from variation occurring within local economic areas, I employ a modified equation (1.1) in which I substitute state × date fixed effects with MSA × date fixed effects. The results of these regressions can be interpreted as comparing firms in the same MSA-by-date pair, but exposed to different levels of courts’ corporation-friendliness. Column (1) of Table 4 shows that the short-term results are robust to the change in the control group. The coefficient indicates that an increase in $L_{\text{Threat}}$ leads to 41 basis point lower cumulative abnormal returns in the 21-day window around the event. Column (2) of Table 4 shows that the long-term results are similarly robust. The coefficient indicates that an increase in $L_{\text{Threat}}$ leads to 1.8% lower cumulative abnormal return in the 12 months after the event.

In the last 4 columns of Table 4, I report two robustness tests for the main results of the paper. In columns (3) and (4), I focus on episodes of judicial turnover that are most likely exogenous. Specifically, I restrict the set of events used in the baseline tests in sections 5.1 and

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10In addition, the MSA definition of the Office of Management and Budget indicates that social and economic ties are important to determine its boundaries: “Metropolitan Statistical Areas have at least one urbanized area of 50,000 or more population, plus adjacent territory that has a high degree of social and economic integration with the core as measured by commuting ties.”
5.2 to changes in judicial benches generated by judge turnover for which the cause is explicitly described as “Death”, “Retirement” or “Impeachment & Conviction”. I also include in this restricted sample of turnover those cases in which the retirement occurs upon reaching the retirement age. Thus, I exclude events caused by “Appointment to Another Judicial Position”, “Reassignment”, “Resignation” and “Retirement” when the judge does not retire as soon as she becomes eligible. Coefficients in columns (3) and (4) remain negative and statistically significant. The economic magnitude of the effect of an increase in the threat of lawsuits on the abnormal returns is 36 basis points (column (3)) and 1.8% (column(4)).

In the second robustness test, I exclude firms changing headquarters during my sample period. These observations can pose a threat to my identification if firms moving headquarters are different in some unobserved dimension. As a result of excluding firms changing headquarters, I lose around 15% of the observations in my sample. The last two columns of Table 4 report the results of the short-term and long-term event studies, respectively. In both cases, the drop in firm value caused by an increase in the threat of lawsuits remains negative and statistically significant. The economic magnitude of the effect is 39 basis points lower 21-day cumulative abnormal returns and 1.1% lower 12-month cumulative abnormal returns.

6. Economic Channel and Heterogeneity in Responses

In this section, I explore the economic mechanism underlying the negative impact of the threat of lawsuits on shareholder wealth, and I investigate whether it is consistent with the overdeterrence hypothesis.

6.1 Firm Investments and Risk

The overdeterrence hypothesis predicts that an increase in the threat of lawsuits undermines managerial incentives to invest in projects that involve a high probability of failure, even if these projects have positive NPV. Managers may wish to avoid the increase in the likelihood

\footnote{These cases collectively account for 13% of the events in my sample.}
of a stockholder suit associated with these projects, because lawsuits can be very costly for them (e.g. Fich and Shivdasani (2007)). Therefore, an increase in the threat of lawsuits should induce a decrease in risky investments. To rule out the possibility that managers shut down only low-quality, inefficient projects, I test whether the reduction in risky investments leads to lower quantity and quality of the innovation produced by a company.

I use R&D expenditure as a proxy for risky investments. An R&D project is a high-uncertainty investment, characterized by a high probability of failure (e.g. Hall and Lerner (2010)), thus it represents an investment likely to be affected by the overdeterrence mechanism. I utilize the logarithm of citation-weighted number of patents to measure innovation outputs. The use of a citation-weighted count is motivated by the recognition that a simple count of patents does not allow to distinguish between high-quality and low-quality patents (e.g. Hall, Jaffe, and Trajtenberg (2005)).

To perform these tests, I use a panel of firm-year observations from 1993 to 2015. I run the following modified version of equation (1.1):

\[
y_{it+1} = \alpha_i + \gamma_{i(s)t} + \lambda_{i(j)t} + \beta L\text{Threat}_{i(k)t} + X_{it}\delta + \epsilon_{it+1} \tag{1.2}
\]

where \( y \) is the outcome of interest for firm \( i \), in period \( t + 1 \). \( L\text{Threat} \) in this section is a continuous variable and it is defined as the average ideology score across all active judges in a court. \( X_{it} \) is a matrix of firm-level control variables. \( \alpha_i \) are firm fixed effects. \( \gamma_{i(s)t} \) are state × year fixed effects. \( \lambda_{i(j)t} \) are industry × year fixed effects.

The first column of Table 5 presents results of equation (1.2) with R&D expenditures\(_t/\text{Assets}_{t-1}\) as dependent variable. The coefficient on \( L\text{Threat} \) in the R&D expenditures regression is negative and significant. These results indicate that firms exposed to a one standard deviation increase in \( L\text{Threat} \) have ratios of R&D expenditures to total assets that are, on average, 40 basis points lower than firms in the counterfactual group. This difference is equal to 6.12% of the sample average. In column (2), I rerun equation (1.2) using \( \ln(\text{Cite-weighted Patent}) \) as dependent variable. The coefficient in column (2) indicates that a one standard deviation in-
crease in $LThreat$ generates a 13% reduction in citation-weighted number of patents over the next year. An increase in the threat of lawsuits is thus associated with a substantial reduction in the quantity and quality of innovative investments.

Another direct prediction of the overdeterrence hypothesis is that managers respond to an increase in the threat of lawsuits by reducing their firms’ risk. The rationale for a decrease in firm risk is that it reduces the incidence of negative corporate outcomes that are associated with a higher probability of shareholder lawsuits. To test whether managers’ actions impact their firms’ risk, I rerun equation (1.2) using two risk measures as dependent variables. The first one is stock volatility, computed following Gormley and Matsa (2016) as the square root of the sum of squared daily stock returns over calendar year $t$. The second one is idiosyncratic volatility, computed as the square root of the sum of squared residuals on Fama-French-Carhart 4-factor model over calendar year $t$. Results of these tests are shown in the last two columns of Table 5. Using both measures of risk, the coefficients indicate that managers are successful in decreasing the risk of firms headquartered in districts whose courts become less corporation-friendly. A one standard deviation increase in $LThreat$ is associated with 1.3 percentage points lower stock volatility and 1.2% lower idiosyncratic volatility. Both coefficients are significant at the 1% level. These results are thus in line with the overdeterrence hypothesis, because they show that managers respond to a higher threat of lawsuits by reducing their firm risk.

### 6.2 Firm Profitability and Earnings Management

Several studies show that external pressure from investors can lead managers to boost short-term profits and neglect long-term growth (Stein (1988), Von Thadden (1995)). The threat of shareholder litigation can impose pressure on managers to appear profitable, because disappointing earnings are one of the most common causes of stockholder lawsuits (Skinner (1997), Field, Lowry, and Shu (2005)). As a result, a direct prediction of the overdeterrence hypothesis is that an increase in the threat of lawsuits should strengthen managers’ incentives to report positive financial results. In particular, since firms that miss analyst forecasts usually suffer significant
declines in their stock price (e.g. Degeorge, Patel, and Zeckhauser (1999)), managers should be concerned with reporting positive earnings relative to analysts’ expectations. In this section, I test this prediction.

Table 6 presents results of equation (1.2) using quarterly earnings data. In column (1), the dependent variable is constructed as Actual$-\text{Forecast}_q$/Book Value of Equity per Share$_{q-1}$, where $q$ indexes the quarter. Actual is defined as the announced quarterly earnings, while Forecast is defined as the median of all analyst forecasts issued before the earnings announcement (a more detailed definition is provided in the Appendix). The coefficient indicates that one standard deviation increase in the threat of lawsuits is associated with 11 basis points higher earning surprises, which is equal to 98% of the sample average. However, managers should not have strong incentives to push performance much beyond the analyst expectations, because the additional benefits in terms of reduced probability of lawsuits are limited. Their goal should be to just meet or exceed the consensus forecast. Column (2) tests this intuition. The dependent variable is an indicator variable that takes value 1 if the scaled difference between Actual and Forecast lies between 0 and the 25th percentile of its own non-negative distribution. The coefficient indicates that firms headquartered in districts whose threat of lawsuits increases by one standard deviation have 1.2% higher probability to have earnings that just beat analyst expectations. These results are interesting, because they suggest that managers actively try to reduce the probability of a lawsuit by reporting positive financial results.

Higher reported earnings, however, do not necessarily imply that the firm profitability has improved. Accounting earnings can be manipulated, and the increase could simply reflect a change in the extent to which firms manage their earnings. As a matter of fact, if a higher threat of lawsuits strengthens the pressure on firms to appear profitable, managers may have heightened incentive to inflate their companies’ earnings. In the last column of Table 6, I test this prediction. Using the modified version of the Jones (1991) model developed by Dechow, Sloan, and Sweeney (1995), I document that increases in the $L\text{Threat}$ variable are associated with higher discretionary accruals. This result suggests that at least part of the increase in
reported earnings is due to managers inflating their companies’ financial results. If considered together with evidence on the drop in firm value in sections 5.1 and 5.2, the results in this section indicate that managers might increase short-term earnings at the expense of long-term growth.

6.3 The Threat of Lawsuits and Bondholder Returns

In this section, I examine the effect of a higher threat of lawsuits for bondholders. The specific economic channel proposed by the *overdeterrence* hypothesis has clear implications for bond returns. The desire to reduce the likelihood of a lawsuit induces managers to decrease investments in risky projects. While this has negative consequences for shareholders, from a bondholder viewpoint this might be positive news. Indeed, shareholder limited liability and bondholders’ payoff structure imply that if risky investments turn out to be successful, shareholders capture most of the gains. By contrast, if they turn out to be failures, bondholders bear most of the costs (Jensen and Meckling (1976)). Therefore, if the *overdeterrence* hypothesis is operative, I expect a higher threat of lawsuits to be associated with higher bondholder returns.

To test this prediction, I use again an event study approach, whose goal is to compare the abnormal bond returns for treatment firms to the returns for control firms. The treatment sample includes all firms headquartered in federal districts in which a change in a court’s judicial bench increases the proportion of investor-friendly judges. For each of these changes, I include firms headquartered in the other districts of the same state in the control group. For each firm of the resulting sample, I compute bond abnormal returns by matching individual bonds to a portfolio of other bonds selected on time-to-maturity and rating category (see e.g. Bessembinder, Kahle, Maxwell, and Xu (2008)). The average value-weighted return on this portfolio represents the event bond expected return. To be consistent with section 5.1, I consider a 21-day symmetric event window to compute the abnormal returns.

Table 7 reports the results of these tests. The number of observations is significantly lower than in other tests of the paper, and the main reason is that bond data in TRACE is only available starting from July 2002. Despite this, all columns of Table 7 show a significant positive impact of
an increase in $LThreat$ on bondholder returns. Looking at column (2), the coefficient indicates that in districts where the threat of lawsuits increases following a change in courts’ judicial benches, firms have 10 basis points higher abnormal bond returns over the [-10,10] window as compared to firms in the control group.

These results are useful, because they allow to rule out the possibility that the drop in firm value due to an increase in the threat of lawsuits is simply reflecting an increase in the expected losses from lawsuits. The observed increase in bondholder wealth instead suggests that managers are actively reducing the riskiness of the firm.

6.4 The Threat of Lawsuits, Firm and Manager Characteristics

To shed further light on the interpretation of my main findings, I exploit variation in the sensitivity of cumulative abnormal returns to the threat of lawsuits across firm and manager characteristics. In this section, I employ subsample analyses applied to the event study setting of section 5.1.

Variation in the Strength of Managers’ Reputation and Career Concerns

As a first test, I use cross-sectional variation in the strength of managerial reputation concerns. Starting from Diamond (1989), several studies have argued that reputation concerns are stronger when reputation is more valuable. Following this logic, I employ a commonly used measure of CEO reputation: CEO tenure (e.g. Milbourn (2003)). In line with the prediction of the overdeterrence hypothesis, I document that the loss in shareholder wealth due to an increase in the threat of lawsuits is concentrated among firms with highly reputed CEOs, as proxied by CEO with high tenure. Columns (1) and (2) of Table 8, Panel A, show that an increase in $LThreat$ leads to 56 basis points lower 21-day cumulative abnormal returns among firms with above median CEO tenure, while the effect is substantially smaller and not statistically significant for firms with below median CEO tenure.

In the last four columns of Table (8), Panel A, I explore cross-sectional variation in the
strength of managerial career concerns. I examine whether firms’ responses to the increase in the threat of lawsuits are related to their ability to withstand an adverse shock to future cash flows. Financially vulnerable firms are firms more exposed to the worst consequences of a lawsuit, such as financial distress or even bankruptcy. Thus, the manager’s career concerns are magnified, because the risk of employment loss for the manager becomes bigger as the probability of financial distress grows larger (Gilson (1989), Eckbo, Thorburn, and Wang (2016)). Consequently, I expect a stronger impact of the threat of lawsuits on shareholder value in financially vulnerable firms.

To test this prediction, I use two different proxies of financial vulnerability. First, I use a firm’s modified Altman z-score, defined as in MacKie-Mason (1990). Second, I employ the measure of Bharath and Shumway (2008) to proxy for a firm’s probability of default. Both variables are constructed to measure the likelihood of corporate defaults, and thus a company’s financial vulnerability. I find that firms that are more financially vulnerable experience a larger drop in cumulative abnormal returns in response to the increase in the threat of lawsuits. As reported in columns (3) and (6) of Table 8, Panel A, firms with low values of Altman z-score and high probability of default exhibit abnormal stock returns that are between 57 and 60 basis points lower than firms in the control group. By contrast, firms with high values of Altman z-score and low probability of default do not exhibit a statistically significant decrease in abnormal stock returns.

Variation in Firm Corporate Governance

The main results of the paper raise an important question: why do shareholders passively sustain the loss of firm value? They could try to combat the harmful consequences of the threat of lawsuits by changing the optimal managerial compensation scheme. There are at least two reasons why wage setting might not be entirely effective. First, changing the compensation scheme to remove the risk-reduction incentives requires a very sophisticated and costly monitoring device. It seems unlikely that individual shareholders possess such level of detailed knowledge (Amihud and Lev (1981), Hermalin and Katz (2000)). In addition, dispersed shareholders have
little incentives to supervise management and take an active interest in how the company is run. This is the traditional collective action problem.

One of the reasons why the board of directors exists is precisely to represent shareholders in the monitoring and compensation-setting tasks. However, when it comes to the threat of stockholder lawsuits, director incentives are likely to be aligned with those of the management. Directors are often sued alongside executives in stockholder suits, and they are exposed to large losses (Fich and Shivdasani (2007), Brochet and Srinivasan (2014)). To support this conclusion, I again resort to subsample analyses to explore cross-sectional heterogeneity in the strength of directors’ reputation concerns. To proxy for directors’ reputation concerns, I use the percentage of independent directors in the company’s board. Previous studies document that outside directors are more sensitive to reputation losses (e.g. Jiang, Wan, and Zhao (2015)). Columns (1) and (e) of Table 8, Panel B, show the results of these analyses. The effect of the threat of shareholder litigation is concentrated in firms with higher percentage of independent directors.

Another potential solution to the collective action problem is the presence of large shareholders. Large shareholders have both sufficient incentives to monitor management and the power to implement the desired changes (e.g. Shleifer and Vishny (1986)). In line with this, I document that the adverse impact of the threat of lawsuits on stockholder wealth is concentrated among firms with lower presence of blockholders. In columns (3) and (4) of Table 8, Panel B, I show that the negative effect of $L\text{Threat}$ on shareholder wealth only obtains in companies with below median blockholder ownership. Similarly, columns (5) and (6) document that the negative effect of $L\text{Threat}$ on shareholder wealth is stronger in companies with below median percentage of ownership in the hands of the top 5 institutional investors. These results lend support to the argument that monitoring by large institutional investors may help mitigate the agency conflict associated with the threat of lawsuits.
7. Conclusion

The large economic losses caused by shareholder lawsuits to companies have spurred a vivid debate on the optimal design of the shareholder litigation system. My paper contributes substantial new evidence to this debate. I show that the threat of shareholder lawsuits can have negative economic consequences on shareholders. This threat induces managers to choose inefficiently low levels of risky investments and to focus on short-term profits at the expense of long-term gains.

To address the challenge arising from the endogenous relation between the threat of shareholder litigation and firm outcomes, I develop a novel empirical approach that exploits exogenous variation in judicial bench composition at the federal district court level. In particular, I focus on adverse shifts in the threat of lawsuits generated by decreases in the proportion of corporation-friendly judges in a district court.

Using this new approach, I document that firms operating in federal districts where the threat of lawsuits increases due to judge turnover have substantially lower stock returns. After the change in courts’ judicial benches, these firms display 35 basis points lower cumulative abnormal returns in the 21-day window around the event, and 1.5% lower cumulative abnormal returns in the next 12 months. In the second part of the paper, I find support for the specific channel predicted by what I label the overdeterrence hypothesis. I hypothesize that the drop in firm value is caused, at least partially, by managers forgoing risky value-enhancing investment projects to reduce the likelihood of legal claims filed against them. Results in the second part of the paper support this hypothesis. First, I find that firms exposed to increases in $L_{Threat}$ curb risky investments, such as R&D projects, and generate fewer patents. I also observe a concurrent reduction in firm risk. Two measures of firm risk, stock volatility and idiosyncratic volatility, decrease significantly in response to increases in $L_{Threat}$. Second, I find that managers more likely ending up with investor-friendly judges have higher announced earnings, more positive earnings surprises and larger discretionary accruals. This suggests that a higher threat of lawsuits exerts stronger pressure on managers to report positive financial results. Third, I show that when the threat of lawsuits increases, bondholder wealth moves in the opposite direction of shareholder
wealth. This test is consistent with the view that managers are actively lowering the firm risk, while does not support the idea that the negative impact of the threat of lawsuits on firm value is simply due to the market anticipating larger losses from lawsuits.

My paper focuses on the *ex ante* effect on shareholders of a higher threat of lawsuits and is silent on other channels through which shareholder litigation can affect shareholders. For example, shareholders can benefit from the ability to recover the economic losses caused by managerial misbehavior through the litigation process (e.g. Cox (1997)). On the other hand, several studies question the ability of individual shareholders to recover damages from alleged fraud (e.g. Thakor, Nielsen, and Gulley (2005)). Further research is needed to determine the overall effect of these additional channels on shareholders.
Table 1: Firm, Court and Judge Statistics

This table shows summary statistics. Panel A presents summary statistics for judge and court-level variables used in the paper. LThreat is the average district court ideology score. Number of judges is the number of active judges in a federal district court, excluding senior judges. Turnover events counts the episodes of changes in the composition of judicial panels in federal district courts. ∆LThreat measures the change in LThreat for each episode in which the composition of a federal district court’s judicial panel changes. Panel B reports summary statistics for event study abnormal returns, both short-term abnormal returns and long-term abnormal returns. Panel C shows summary statistics for other firm-level variables, including the corporate policies analyzed in the paper’s tests. A complete list of definitions for these variables is provided in the Appendix.

Panel A: Summary Statistics – Judge and Court Variables

<table>
<thead>
<tr>
<th>Court-Level Variables</th>
<th>N</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>25%</th>
<th>50%</th>
<th>75%</th>
</tr>
</thead>
<tbody>
<tr>
<td>LThreat</td>
<td>2,093</td>
<td>−0.08</td>
<td>0.19</td>
<td>−0.21</td>
<td>−0.06</td>
<td>0.06</td>
</tr>
<tr>
<td>Number of judges</td>
<td>2,093</td>
<td>7.14</td>
<td>5.40</td>
<td>3.00</td>
<td>5.00</td>
<td>9.00</td>
</tr>
</tbody>
</table>

Changes in Courts’ Judicial Benches

<table>
<thead>
<tr>
<th></th>
<th>N</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>25%</th>
<th>50%</th>
<th>75%</th>
</tr>
</thead>
<tbody>
<tr>
<td>Turnover Events</td>
<td>1,475</td>
<td>0.74</td>
<td>0.62</td>
<td>0.36</td>
<td>0.55</td>
<td>0.95</td>
</tr>
<tr>
<td>∆LThreat</td>
<td>1,475</td>
<td>0.06</td>
<td>0.07</td>
<td>0.02</td>
<td>0.05</td>
<td>0.08</td>
</tr>
</tbody>
</table>

Panel B: Summary Statistics – Event Study Returns

<table>
<thead>
<tr>
<th>Short-Term Abnormal Returns</th>
<th>N</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>25%</th>
<th>50%</th>
<th>75%</th>
</tr>
</thead>
<tbody>
<tr>
<td>FF 3-factor CAR [-10,10] (%)</td>
<td>84,891</td>
<td>−0.11</td>
<td>16.63</td>
<td>−8.36</td>
<td>−0.57</td>
<td>7.30</td>
</tr>
<tr>
<td>Size-B/M Buy-Hold [0,12] (%)</td>
<td>97,533</td>
<td>−2.09</td>
<td>69.03</td>
<td>−39.21</td>
<td>−11.44</td>
<td>19.24</td>
</tr>
</tbody>
</table>

Panel C: Summary Statistics – Other Firm-Level Variables

<table>
<thead>
<tr>
<th></th>
<th>N</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>25%</th>
<th>50%</th>
<th>75%</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Market Capitalization</td>
<td>62,828</td>
<td>5.40</td>
<td>2.14</td>
<td>3.84</td>
<td>5.37</td>
<td>6.86</td>
</tr>
<tr>
<td>Book-to-Market</td>
<td>62,828</td>
<td>0.76</td>
<td>1.55</td>
<td>0.27</td>
<td>0.48</td>
<td>0.83</td>
</tr>
<tr>
<td>Capital Expenditures (%)</td>
<td>62,828</td>
<td>7.90</td>
<td>11.82</td>
<td>1.95</td>
<td>4.16</td>
<td>8.69</td>
</tr>
<tr>
<td>R&amp;D Expenditures (%)</td>
<td>62,828</td>
<td>8.11</td>
<td>18.08</td>
<td>0.00</td>
<td>0.37</td>
<td>8.68</td>
</tr>
<tr>
<td>Stock Volatility</td>
<td>62,828</td>
<td>65.56</td>
<td>49.65</td>
<td>37.75</td>
<td>55.47</td>
<td>79.07</td>
</tr>
<tr>
<td>Idiosyncratic Volatility</td>
<td>62,828</td>
<td>63.05</td>
<td>49.74</td>
<td>34.79</td>
<td>52.77</td>
<td>77.11</td>
</tr>
<tr>
<td>Leverage</td>
<td>62,828</td>
<td>19.49</td>
<td>19.28</td>
<td>0.92</td>
<td>15.24</td>
<td>32.15</td>
</tr>
<tr>
<td>Reported Earnings</td>
<td>202,135</td>
<td>0.02</td>
<td>0.12</td>
<td>0.00</td>
<td>0.24</td>
<td>0.52</td>
</tr>
</tbody>
</table>
Table 2: Shareholder Class Action Lawsuits

This table shows summary statistics and other tests involving shareholder class action lawsuits. Panel A shows descriptive statistics for abnormal returns from Fama-French 3-factor model over the 3-day or 7-day event window around the announcement of a shareholder class action lawsuit. Panel B reports results of a linear probability model of shareholder class action outcomes on Judge Ideology, controls and different sets of FE. The dependent variable is coded as 1 when the shareholder class action terminates with a settlement, or a trial outcome favorable to investors. Judge Ideology is the Giles, Hettinger, and Peppers (2001) measure of judge ideology, for the judge assigned to the case. Controls include beginning of the year logarithm of market capitalization, log market-to-book, stock volatility and previous 12 months stock return. Panel C reports coefficients from regression of Fama-French 3-factor model CAR [-10,10] on LThreat, controls and different sets of FE. LThreat is the average district court ideology score. Controls include beginning of the year logarithm of market capitalization, log market-to-book, stock volatility and previous 12 months stock return. Industry FE are based on the Fama-French 12-industry classification. A complete list of definitions for these variables is provided in the Appendix.

Panel A: Summary Statistics – Filing Date Abnormal Returns

<table>
<thead>
<tr>
<th>FF 3-factor CAR [-10,10] (%)</th>
<th>N</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>25%</th>
<th>50%</th>
<th>75%</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1,631</td>
<td>-10.54</td>
<td>30.42</td>
<td>-24.67</td>
<td>-5.86</td>
<td>5.68</td>
</tr>
</tbody>
</table>

Panel B: Impact of Judge Ideology on Settlement Probability

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Judge Ideology</td>
<td>0.107***</td>
<td>0.111***</td>
<td>0.102**</td>
</tr>
<tr>
<td></td>
<td>(3.89)</td>
<td>(4.16)</td>
<td>(3.64)</td>
</tr>
<tr>
<td>Controls</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>District FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry FE</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>1,126</td>
<td>1,126</td>
<td>1,126</td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>0.07</td>
<td>0.10</td>
<td>0.11</td>
</tr>
</tbody>
</table>
### Panel C: Impact of the Threat of Lawsuits on FF 3-factor CAR [-10,10]

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>LThreat (%)</td>
<td>-11.970**</td>
<td>-13.075**</td>
<td>-13.618***</td>
</tr>
<tr>
<td></td>
<td>(−2.22)</td>
<td>(−2.49)</td>
<td>(−2.73)</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>District FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year FE</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry FE</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>1,631</td>
<td>1,631</td>
<td>1,631</td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>0.12</td>
<td>0.13</td>
<td>0.14</td>
</tr>
</tbody>
</table>
Table 3: Effect of the Threat of Lawsuits on Stock Returns

This table presents results of event studies around changes in courts’ judicial benches. The treatment group is composed of firms exposed to an increase in the threat of lawsuits due to changes in court composition, while the control group is composed of firms operating in the same state but in federal districts that do not experience the event. Columns (1) to (3) use as dependent variable the 21-day cumulative abnormal return (CAR[-10,10]) from the Fama-French 3-factor model. In columns (4) to (6) the dependent variable is the buy-hold size-B/M adjusted return over the 12-month window (Size-B/M Buy-Hold[0,12]). Controls include: Size, beginning of the year logarithm of market capitalization; Log Book-to-Market; Previous 12 months stock return; Stock volatility. Industry × date FE are based on the Fama-French 12-industry classification. *-statistic based on standard errors clustered at the district court level are shown in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively. A complete list of definitions for these variables is provided in the Appendix.

<table>
<thead>
<tr>
<th></th>
<th>FF 3-factor CAR[-10,10]</th>
<th>Size-B/M Buy-Hold[0,12]</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>LThreat (Binary) (%)</td>
<td>-0.358***</td>
<td>-0.325***</td>
</tr>
<tr>
<td></td>
<td>(-2.66)</td>
<td>(-2.67)</td>
</tr>
<tr>
<td>Size</td>
<td>0.134****</td>
<td>-0.208**</td>
</tr>
<tr>
<td></td>
<td>(3.32)</td>
<td>(-1.94)</td>
</tr>
<tr>
<td>Book-to-Market</td>
<td>0.013</td>
<td>-0.035</td>
</tr>
<tr>
<td></td>
<td>(0.19)</td>
<td>(-0.20)</td>
</tr>
<tr>
<td>Previous 12-month Return</td>
<td>-3.714***</td>
<td>-4.000***</td>
</tr>
<tr>
<td></td>
<td>(-6.87)</td>
<td>(-5.81)</td>
</tr>
<tr>
<td>Stock Volatility</td>
<td>-0.021</td>
<td>-0.023</td>
</tr>
<tr>
<td></td>
<td>(-0.31)</td>
<td>(-0.16)</td>
</tr>
<tr>
<td>District Court FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>State × Date FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry × Date FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Firm FE</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Observations</td>
<td>84,891</td>
<td>84,891</td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>0.03</td>
<td>0.06</td>
</tr>
</tbody>
</table>
Table 4: Controlling for Local Heterogeneity and Robustness Tests

This table presents results of event studies around changes in courts’ judicial benches. The treatment group is composed of firms exposed to an increase in the threat of lawsuits due to changes in court composition, while the control group is composed of firms operating in the same state but in federal districts that do not experience the event. The dependent variable is either the 21-day cumulative abnormal return from the Fama-French 3-factor model (FF3 CAR[-10,10]) or the buy-hold size-B/M adjusted return over the 12-month window (Size-BM BHAR[0,12]). The first two columns show results obtained by substituting state times date FE with MSA times date FE. Columns (3) and (4) show results obtained by restricting the sample of judge turnover to episodes of death or retirement upon reaching the retirement age. Columns (5) and (6) report results obtained by excluding firms changing headquarters. t-statistic based on standard errors clustered at the district court level are shown in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively. A complete list of definitions for these variables is provided in the Appendix.

<table>
<thead>
<tr>
<th></th>
<th>MSA × Date FE</th>
<th>Death or Retirement</th>
<th>Excluding Movers</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>FF3 CAR[-10,10]</td>
<td>Size-BM BHAR[0,12]</td>
<td>FF3 CAR[-10,10]</td>
</tr>
<tr>
<td>LThreat (Binary) (%)</td>
<td>–0.410***</td>
<td>–1.766****</td>
<td>–0.362***</td>
</tr>
<tr>
<td></td>
<td>(–2.77)</td>
<td>(–2.99)</td>
<td>(–2.86)</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>District Court FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>State × Date FE</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry × Date FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>MSA × Date FE</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Observations</td>
<td>95,046</td>
<td>111,302</td>
<td>65,677</td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>0.05</td>
<td>0.07</td>
<td>0.06</td>
</tr>
</tbody>
</table>
Table 5: The Threat of Lawsuits and Firm Investment and Risk Choices

This table reports coefficients from firm-level panel regressions of R&D expenditure, citation-weighted number of patents and risk variables on $L\text{Threat}$, controls, firm FE, state × year FE and industry × year FE. $L\text{Threat}$ is the average district court ideology score. Controls include beginning of year logarithm of market capitalization, log market-to-book, stock volatility (columns (1) and (2) only), leverage and profitability. A complete list of definitions for dependent and independent variables is provided in the Appendix. Industry × date FE are based on the Fama-French 12-industry classification. $t$-statistic based on standard errors clustered at the district court level are shown in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>LThreat</td>
<td>$-0.017^{***}$</td>
<td>$-0.727^{**}$</td>
<td>$-0.072^{***}$</td>
<td>$-0.063^{***}$</td>
</tr>
<tr>
<td></td>
<td>($-2.41$)</td>
<td>($-2.02$)</td>
<td>($-3.82$)</td>
<td>($-3.24$)</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Firm FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>State × Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry × Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>Observations</td>
<td>62,828</td>
<td>45,207</td>
<td>62,828</td>
<td>62,828</td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>0.74</td>
<td>0.60</td>
<td>0.54</td>
<td>0.54</td>
</tr>
</tbody>
</table>
Table 6: The Threat of Lawsuits, Firm Earnings and Earnings Management

This table reports coefficients from firm-level panel regressions of firm’s earnings surprises and discretionary accruals on \( L\text{Threat} \), controls, firm FE, state \( \times \) date (year-quarter) FE and industry \( \times \) date FE. \( L\text{Threat} \) is the average district court ideology score. Controls include beginning of quarter logarithm of market capitalization, log market-to-book, stock volatility, previous 12 months stock return and profitability (column (3) only). A complete list of definitions for dependent and independent variables is provided in the Appendix. Industry \( \times \) date FE are based on the Fama-French 12-industry classification. \( t \)-statistic based on standard errors clustered at the district court level are shown in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

<table>
<thead>
<tr>
<th>Actual–Forecast</th>
<th>0( \leq A–F\leq P_{25}^{+} )</th>
<th>Discretionary Accruals</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>LThreat</td>
<td>0.006***</td>
<td>0.063**</td>
</tr>
<tr>
<td></td>
<td>(3.90)</td>
<td>(2.21)</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Firm FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>State ( \times ) Date FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry ( \times ) Date FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>202,135</td>
<td>202,135</td>
</tr>
<tr>
<td>Adjusted ( R^2 )</td>
<td>0.17</td>
<td>0.11</td>
</tr>
</tbody>
</table>
Table 7: The Threat of Lawsuits and Bondholder Returns

This table presents results of event studies around changes in courts’ judicial benches. The treatment group is composed of firms exposed to an increase in the threat of lawsuits due to changes in court composition, while the control group is composed of firms operating in the same state but in federal districts that do not experience such event. All columns use as dependent variable bond abnormal return computed as the difference between the event bond return over the [-10,10] window and the return of portfolio of bonds matched on time-to-maturity and rating over the same [-10,10] window. Controls include beginning of year logarithm of market capitalization, log market-to-book, stock volatility, previous 12 months stock return and profitability. Industry FE are based on the Fama-French 12-industry classification. \( t \)-statistic based on standard errors clustered at the district court level are shown in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>IThreat (Binary) (%)</td>
<td>0.101***</td>
<td>0.099***</td>
<td>0.095**</td>
</tr>
<tr>
<td></td>
<td>(3.02)</td>
<td>(2.76)</td>
<td>(2.56)</td>
</tr>
<tr>
<td>Controls</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Firm FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>State × Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry FE</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>5,061</td>
<td>5,061</td>
<td>4,350</td>
</tr>
<tr>
<td>Adjusted R(^2)</td>
<td>0.14</td>
<td>0.15</td>
<td>0.22</td>
</tr>
</tbody>
</table>
Table 8: The Threat of Lawsuits Effect across Firm and Manager Characteristics

This table reports results of subsample analyses using event studies around changes in courts’ judicial benches. In Panel A, I report results of subsample analyses based on above and below median CEO tenure, Altman z-score and default probability, as defined using the “naïve” measure of Bharath and Shumway (2008). Panel B reports results of subsample analyses based on above and below median percentage of independent directors, blockholder ownership and top 5 institutional investor ownership. All columns present results from equation (1.1). The dependent variable is the 21-day cumulative abnormal return from the Fama-French 3-factor model. A complete list of definitions for these variables is provided in the Appendix. Controls include beginning of year logarithm of market capitalization, log market-to-book, stock volatility and previous 12 months stock return. \( t \)-statistic based on standard errors clustered at the district court level are shown in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

### Panel A: Variation in Managers’ Reputation and Career Concerns

<table>
<thead>
<tr>
<th>CEO Tenure</th>
<th>Altman z</th>
<th>Default Probability</th>
</tr>
</thead>
<tbody>
<tr>
<td>Low</td>
<td>High</td>
<td>Low</td>
</tr>
<tr>
<td>LThreat (Binary) (%)</td>
<td>0.152</td>
<td>−0.555***</td>
</tr>
<tr>
<td>(0.86)</td>
<td>(−2.90)</td>
<td>(−3.59)</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Distric Court FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>State × Date FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry × Date FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>24,426</td>
<td>21,843</td>
</tr>
<tr>
<td>Adjusted ( R^2 )</td>
<td>0.06</td>
<td>0.08</td>
</tr>
</tbody>
</table>

### Panel B: Variation in Firm Corporate Governance

<table>
<thead>
<tr>
<th>Independent %</th>
<th>Blockholder %</th>
<th>Top 5 %</th>
</tr>
</thead>
<tbody>
<tr>
<td>Low</td>
<td>High</td>
<td>Low</td>
</tr>
<tr>
<td>LThreat (Binary) (%)</td>
<td>−0.222</td>
<td>−0.405**</td>
</tr>
<tr>
<td>(−1.02)</td>
<td>(−2.36)</td>
<td>(−4.66)</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Distric Court FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>State × Date FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry × Date FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>13,942</td>
<td>11,470</td>
</tr>
<tr>
<td>Adjusted ( R^2 )</td>
<td>0.07</td>
<td>0.10</td>
</tr>
</tbody>
</table>
Figure 1: Short-Term Market Reaction to Changes in the Threat of Lawsuits

This figure plots cumulative point estimates from a modified version of equation (1.1), where I allow the effect of changes in courts’ judicial panel to vary across days, from day -30 to day 30. The dependent variable is the daily abnormal return from the Fama-French 3-factor model. An event is defined as the day in which a change in a court’s judicial panel leads to an increase in the threat of lawsuits. The regression includes dummies for days in event time, firm FE and state × date FE. The graph also shows the 95% confidence interval.
This figure plots cumulative point estimates from a modified version of equation (1.1), where I allow the effect of changes in courts' judicial panel to vary across months, from month -12 to month 12. The dependent variable is the monthly abnormal return from the Fama-French 3-factor model. An event is defined as the month in which a change in a court’s judicial panel leads to an increase in the threat of lawsuits. The regression includes dummies for months in event time, firm FE and state × date (year-month) FE. The graph also shows the 95% confidence interval.
## APPENDIX

### A Description of Variables

<table>
<thead>
<tr>
<th>Variable</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Main independent variable</strong></td>
<td></td>
</tr>
<tr>
<td>LThreat</td>
<td>Average Giles, Hettinger, and Peppers (2001) ideology score at the federal district court level. Computed including all active judges in a given year, excluding senior judges. The measure ranges from -1, for most corporation-friendly judges, to +1, for most investor-friendly judges.</td>
</tr>
<tr>
<td>LThreat (Binary)</td>
<td>Indicator variable that takes value 1 the date in which a judge turnover leads to an increase in the variable LThreat in a district court. The date is defined as the day in which the Senate confirms the judicial nominee.</td>
</tr>
<tr>
<td><strong>Dependent variables</strong></td>
<td></td>
</tr>
<tr>
<td>FF 3-factor CAR [-10,10]</td>
<td>Cumulative abnormal returns over the 21-day window around the change in a court’s panel of judges, calculated using the Fama-French 3-factor model estimated over trading days (−231,−31).</td>
</tr>
<tr>
<td>Size-B/M Buy-Hold [0,12]</td>
<td>Cumulative abnormal returns over the 12 months after the change in a court’s panel of judges, calculated as stock i’s return in excess of stock i’s benchmark portfolio return over the same 12 months. The benchmark portfolio is the corresponding 25 Fama and French portfolios formed on size and book-to-market.</td>
</tr>
<tr>
<td>R&amp;D Expenditure</td>
<td>Measured as R&amp;D&lt;sub&gt;t&lt;/sub&gt;/Assets&lt;sub&gt;t−1&lt;/sub&gt;. Where R&amp;D is R&amp;D expenditure (XRD) at the end of December year &lt;i&gt;t&lt;/i&gt; and Assets is book value of assets (AT) at the end of December year &lt;i&gt;t − 1&lt;/i&gt;. If R&amp;D expenditure is missing, I substitute with 0.</td>
</tr>
<tr>
<td>Cite-weighted Patent</td>
<td>Count of a firm’s number of patents weighted by future citations received and adjusted for truncation (as in Hall, Jaffe, and Trajtenberg (2005))</td>
</tr>
<tr>
<td>Stock Volatility</td>
<td>Square root of the sum of squared daily returns over calendar year &lt;i&gt;t − 1&lt;/i&gt;. To adjust for differences in the number of trading days, the raw sum is multiplied by 252 and divided by the number of trading days. Calculated from CRSP.</td>
</tr>
<tr>
<td>Idiosyncratic Volatility</td>
<td>Square root of the sum of squared residuals from 3-factors model estimated from daily returns over calendar year &lt;i&gt;t − 1&lt;/i&gt;. To adjust for differences in the number of trading days, the raw sum is multiplied by 252 and divided by the number of trading days. Calculated from CRSP.</td>
</tr>
<tr>
<td>Actual−Forecast</td>
<td>Difference between the actual quarterly earnings and the analyst consensus earnings forecast. The former is defined as the announced quarterly earnings as reported by I/B/E/S, divided by the book value of equity per share at the end of the corresponding fiscal quarter. The latter is defined as the median of all 1- or 2-quarter-ahead forecasts issued or reviewed in the last 60 days before the earnings announcement by analysts covering the firm, divided by the book value of equity per share at the end of the corresponding fiscal quarter.</td>
</tr>
<tr>
<td>0 ≤ A−F ≤ P&lt;sub&gt;25&lt;/sub&gt;</td>
<td>Indicator variable that takes value 1 if the difference between the actual quarterly earnings and the analyst consensus earnings forecast is between 0 and the 25th percentile of the non-negative distribution of Actual−Forecast.</td>
</tr>
<tr>
<td>Variable Description</td>
<td>Definition</td>
</tr>
<tr>
<td>----------------------</td>
<td>------------</td>
</tr>
<tr>
<td>Discretionary Accruals</td>
<td>Residuals from the following regression $TA_{iq} = \alpha_1 (1/ATQ_{iq-1}) + \alpha_2 (\Delta REV_{iq} - \Delta REC_{iq}) + \alpha_3 (PPE_{iq})$ where $TA$ is total accruals; $ATQ$ is quarterly book value of assets; $\Delta REV$ is revenues (REVTQ) in quarter $q$ less revenues in quarter $q-1$ scaled by lagged total assets; $\Delta REC$ is accounts receivables (RECTQ) in quarter $q$ less accounts receivables in quarter $q-1$ scaled by lagged total assets; and $PPE$ is gross property plant and equipment in quarter $q$ scaled by lagged total assets.</td>
</tr>
<tr>
<td>Abnormal Bond Return[-10,10]</td>
<td>Abnormal firm-level bond returns over the 21-day window around the change in a court’s panel of judges, calculated as firm $i$’s bond return less firm $i$’s benchmark portfolio bond return over the same 21-day window. The benchmark portfolio is composed of bonds matched on time-to-maturity and rating. I create maturity and rating categories as in Bessembinder, Kahle, Maxwell, and Xu (2008). To obtain a firm-level abnormal return, I value-weight the different bond issues of the same firm.</td>
</tr>
<tr>
<td>Settlement Probability</td>
<td>Indicator variable equal to 1 when a shareholder class action lawsuit terminates with a settlement. In the few cases in which the case goes to trial (21 cases since 1996), the indicator takes value 1 if the trial ends with an outcome in favor of investors.</td>
</tr>
<tr>
<td>Shareholder CA by $t = 3$</td>
<td>Indicator variable equal to 1 for firms that are defendants in a shareholder class action lawsuit in the following 3 years.</td>
</tr>
<tr>
<td>Other variables</td>
<td></td>
</tr>
<tr>
<td>Log of Market Capitalization</td>
<td>Natural logarithm of price times shares outstanding from CRSP.</td>
</tr>
<tr>
<td>Log Market-to-Book</td>
<td>The natural log of the ratio of the market value of equity to the book value of equity. Book equity is total book value of assets, minus total liabilities, plus balance sheet deferred taxes and investment tax credit if available, minus preferred stock liquidating value if available, or redemption value if available, or carrying value. Market equity is price times shares outstanding from CRSP.</td>
</tr>
<tr>
<td>Leverage</td>
<td>Ratio of long-term debt (DLTT) plus short-term debt (DLC) over the lagged book value of assets (AT).</td>
</tr>
<tr>
<td>Return$_{t-1}$</td>
<td>Cumulative annual stock return over the 12 months of calendar year $t-1$.</td>
</tr>
<tr>
<td>CEO Tenure</td>
<td>The number of years the executive has been CEO at this firm as of year $t$. Computed from ExecuComp supplemented by BoardEx.</td>
</tr>
<tr>
<td>Default Probability</td>
<td>The “naive” measure of default probability constructed as in Bharath and Shumway (2008).</td>
</tr>
<tr>
<td>Altman z</td>
<td>Defined as $0.33 \times \frac{EBIT}{Total\ Assets} + 1.0 \times \frac{Sales}{Total\ Assets} + 1.4 \times \frac{Retained\ Earnings}{Total\ Assets} + 1.2 \times \frac{Working\ Capital}{Total\ Assets}$.</td>
</tr>
<tr>
<td>Independent %</td>
<td>The fraction of independent (outside) directors sitting on the board of the firm in year $t$. Computed from BoardEx.</td>
</tr>
<tr>
<td>Blockholder %</td>
<td>The fraction of shares outstanding held by blockholders in year $t$. Calculated from Thomson Reuters Institutional Managers (13f) Holdings.</td>
</tr>
<tr>
<td>Top 5 %</td>
<td>The fraction of shares outstanding held by the top 5 institutional investors in year $t$. Calculated from Thomson Reuters Institutional Managers (13f) Holdings.</td>
</tr>
</tbody>
</table>
### U.S States and Federal District Courts

This table shows the number of federal district courts in each of the U.S states and territories.

<table>
<thead>
<tr>
<th>State</th>
<th># Courts</th>
<th>State</th>
<th># Courts</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alabama</td>
<td>3</td>
<td>Nebraska</td>
<td>1</td>
</tr>
<tr>
<td>Alaska</td>
<td>1</td>
<td>Nevada</td>
<td>1</td>
</tr>
<tr>
<td>Arizona</td>
<td>1</td>
<td>New Hampshire</td>
<td>1</td>
</tr>
<tr>
<td>Arkansas</td>
<td>2</td>
<td>New Jersey</td>
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</tr>
<tr>
<td>California</td>
<td>4</td>
<td>New Mexico</td>
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</tr>
<tr>
<td>Colorado</td>
<td>1</td>
<td>New York</td>
<td>4</td>
</tr>
<tr>
<td>Connecticut</td>
<td>1</td>
<td>North Carolina</td>
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</tr>
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<td>Delaware</td>
<td>1</td>
<td>North Dakota</td>
<td>1</td>
</tr>
<tr>
<td>Florida</td>
<td>3</td>
<td>Northern Marian</td>
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</tr>
<tr>
<td>Georgia</td>
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<td>Ohio</td>
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<td>Guam</td>
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<td>Oklahoma</td>
<td>3</td>
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<td>Hawaii</td>
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<td>Oregon</td>
<td>1</td>
</tr>
<tr>
<td>Idaho</td>
<td>1</td>
<td>Pennsylvania</td>
<td>3</td>
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<tr>
<td>Illinois</td>
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<td>Puerto Rico</td>
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<tr>
<td>Indiana</td>
<td>2</td>
<td>Rhode Island</td>
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<td>Iowa</td>
<td>2</td>
<td>South Carolina</td>
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<td>South Dakota</td>
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</tr>
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<td>Kentucky</td>
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<td>Tennessee</td>
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<td>Louisiana</td>
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<td>Texas</td>
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<td>Utah</td>
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<tr>
<td>Maryland</td>
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<td>Vermont</td>
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</tr>
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<td>Massachusetts</td>
<td>1</td>
<td>Virgin Islands</td>
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<td>Michigan</td>
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<td>Virginia</td>
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<td>Minnesota</td>
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<td>Mississippi</td>
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<td>West Virginia</td>
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<td>Missouri</td>
<td>2</td>
<td>Wisconsin</td>
<td>2</td>
</tr>
<tr>
<td>Montana</td>
<td>1</td>
<td>Wyoming</td>
<td>1</td>
</tr>
</tbody>
</table>
### Summary Statistics by District Courts

This table shows summary statistics for each U.S. district court for the sample period 1993-2015. The second column reports the average number across years of active judges. The third column shows the total number of turnover episodes by district court. The fourth column reports the average number across years of judge turnover episodes. The last column reports the average value across years of the variable $L_{Threat}$ in the district court.

<table>
<thead>
<tr>
<th>District Court</th>
<th>Avg. # of Judges</th>
<th>Tot. # of Turnover</th>
<th>Avg. Turnover</th>
<th>Avg. $L_{Threat}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>D. District Columbia</td>
<td>13.84</td>
<td>58</td>
<td>1.57</td>
<td>0.07</td>
</tr>
<tr>
<td>C.D. California</td>
<td>23.03</td>
<td>101</td>
<td>2.73</td>
<td>-0.05</td>
</tr>
<tr>
<td>D. Arizona</td>
<td>9.68</td>
<td>41</td>
<td>1.11</td>
<td>-0.09</td>
</tr>
<tr>
<td>D. Colorado</td>
<td>6.27</td>
<td>27</td>
<td>0.73</td>
<td>-0.11</td>
</tr>
<tr>
<td>D. Connecticut</td>
<td>6.81</td>
<td>27</td>
<td>0.73</td>
<td>0.17</td>
</tr>
<tr>
<td>D. Delaware</td>
<td>3.59</td>
<td>11</td>
<td>0.30</td>
<td>-0.12</td>
</tr>
<tr>
<td>D. Idaho</td>
<td>1.86</td>
<td>6</td>
<td>0.16</td>
<td>-0.23</td>
</tr>
<tr>
<td>D. Kansas</td>
<td>5.35</td>
<td>22</td>
<td>0.59</td>
<td>0.00</td>
</tr>
<tr>
<td>D. Maine</td>
<td>2.59</td>
<td>11</td>
<td>0.30</td>
<td>0.03</td>
</tr>
<tr>
<td>D. Maryland</td>
<td>9.08</td>
<td>44</td>
<td>1.19</td>
<td>0.06</td>
</tr>
<tr>
<td>D. Massachusetts</td>
<td>12.00</td>
<td>31</td>
<td>0.84</td>
<td>0.07</td>
</tr>
<tr>
<td>D. Minnesota</td>
<td>6.54</td>
<td>24</td>
<td>0.65</td>
<td>0.20</td>
</tr>
<tr>
<td>D. Montana</td>
<td>2.81</td>
<td>13</td>
<td>0.35</td>
<td>-0.13</td>
</tr>
<tr>
<td>D. Nebraska</td>
<td>3.03</td>
<td>17</td>
<td>0.46</td>
<td>-0.27</td>
</tr>
<tr>
<td>D. Nevada</td>
<td>4.86</td>
<td>28</td>
<td>0.76</td>
<td>-0.14</td>
</tr>
<tr>
<td>D. New Hampshire</td>
<td>2.65</td>
<td>9</td>
<td>0.24</td>
<td>-0.17</td>
</tr>
<tr>
<td>D. New Jersey</td>
<td>14.84</td>
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Table C.3: Effect of Large Increases in the Threat of Lawsuits on Stock Returns

This table presents results of event studies around changes in courts’ judicial benches. In this table, I focus on large changes in the threat of lawsuits. The treatment group is composed of firms exposed to an increase in the threat of lawsuits that is at least equal to the 75th percentile of the distribution of changes, while the control group is composed of firms operating in the same state but in federal districts that do not experience any increase in the threat of lawsuits. Columns (1) and (2) use as dependent variable the 21-day cumulative abnormal return (CAR[-10,10]) from the Fama-French 3-factor model. In columns (3) and (4) the dependent variable is the 12-months cumulative abnormal return (CAR[0,12]) from the Fama-French 3-factor model. Controls include beginning of the year logarithm of market capitalization, log market-to-book, stock volatility and previous 12 months stock return. Industry × date FE are based on the Fama-French 12-industry classification. \( t \)-statistic based on standard errors clustered at the district court level are shown in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively. A complete list of definitions for these variables is provided in the Appendix.

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<td>Large Increase in LThreat (%)</td>
<td>−0.691**</td>
<td>−0.818*</td>
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<td>(−2.01)</td>
<td>(−1.87)</td>
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<td>−0.032*</td>
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<td>−0.041**</td>
<td>(−2.46)</td>
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[Control variables omitted from table]

Distric Court FE: Yes, Yes, Yes, Yes
State × Date FE: Yes, Yes, Yes, Yes
Industry × Date FE: No, Yes, No, Yes
Observations: 5,918, 5,419, 2,901, 2,560
Adjusted \( R^2 \): 0.06, 0.05, 0.20, 0.22
Figure C.1: Federal Court System

This figure shows the distribution of federal district courts and federal circuits among the U.S. states and territories. The numbers associated to the different colors represent the 11 U.S. federal circuits, which span multiple states. Each state, when applicable, is divided into its federal districts. (available at http://www.uscourts.gov/about-federal-courts/federal-courts-public/court-website-links)
Chapter 2

Is the Stock Market Biased Against Diverse Top Management Teams?

with Alberto Manconi and Oliver Spalt

Abstract

We study how the market responds to top management team diversity, using a new text–based measure of team diversity and a novel database covering over 70,000 top executives in 6,500 firms over the period 1999–2014. We find that analysts systematically underestimate the earnings and future returns of firms with a diverse top management team. Firm characteristics related to the way the firm is managed do not explain the effect; in contrast, variables related to the management team’s professional, educational, age, and gender diversity appear to capture some of it. More experienced analysts display a smaller bias against diverse firms, consistent with diverse firms being harder to understand. Finally, diverse stocks earn excess returns, suggesting that the market as a whole responds to diversity in a way similar to analysts.
1. Introduction

Corporate leadership diversity is increasingly central — and controversial. BlackRock Inc.’s proxy voting guidelines openly encourage firms to “take into consideration the diversity of experience and expertise” when determining the structure of their leadership team, threatening to oppose director nominations if firms pay “insufficient attention to diversity,”¹ and recently a group of large investment funds petitioned the SEC, calling for enhanced diversity disclosure of board nominees.² At the same time, there is evidence of resistance to diversity, for instance among early stage investors (Ewens and Townsend (2017)) or venture capitalists (Gompers and Wang (2017)).

This level of interest and debate around managerial diversity suggests that it is important for companies to understand how financial markets respond to it. And yet, in comparison to a large body of research in management devoted to this topic (Hambrick (2007)), there is still relatively little work on it in the financial economics literature, with many first-order questions still left unanswered. We attempt to fill this gap. Using a novel, hand-collected, extensive database on top management teams, and an innovative approach to measuring their diversity, we present fresh evidence on the stock market valuation of top management team diversity.

Studying diversity presents two main challenges. The first one is measuring top management team diversity for a set of firms that spans a large portion of the stock market. To address this challenge, we resort to a new source of managerial background data: managerial biographies that firms are required to disclose to the SEC in their annual reports. The SEC requires all listed firms to disclose biographies of top executives, containing information about, for example, their educational background and prior work experience. These new data allow us to cover nearly

²“...We believe better disclosure about the board’s skills, experiences, gender, race, and ethnic diversity can help us as investors determine whether the board has the appropriate mix to manage risk and avoid groupthink. For these reasons, we urge the Commission to initiate a rulemaking process to require better disclosure.” The petition was submitted in 2015. The signatories included the California Public Employees Retirement System (CalPERS), the Washington State Investment Fund, the Connecticut Retirement Plans and Trust Fund, the California State Teacher’s Retirement System, the Illinois State Board of Investment, the Ohio Public Employees Retirement Systems, the New York State Common Retirement Fund, and the North Carolina Department of State Treasurer. The full text can be found at: https://www.sec.gov/rules/petitions/2015/petn4-682.pdf.
7,000 unique firms over the period from 1999 to 2014, tracking over 70,000 individual executives, and assembling what is, to our knowledge, one of the largest datasets on diverse management teams.

The second challenge is more conceptual. While diversity is a broad, multi-faceted aspect of management teams (e.g., Jackson, Joshi, and Erhardt (2003)), most existing work on diversity focuses on individual, easy-to-measure, managerial attributes such as age, tenure, nationality, or gender. Such studies capture some relevant dimensions of diversity, but at the same time may miss many others. We apply tools from textual analysis (Hanley and Hoberg (2010), Hoberg and Phillips (2016)) to the managerial biographies in our data, to obtain a measure that captures many dimensions of diversity simultaneously, without limiting ourselves to a pre-specified and restricted set of dimensions.

How should we expect the market to value managerial diversity? A first possibility is that diversity per se does not matter; rather, what matters is what companies with a diverse management team do. After controlling for its relation to corporate characteristics and policies, therefore, any effect of diversity on stock prices should vanish. A second possibility is that the perception of diversity itself matters, over and above corporate actions. It could be that the market believes some of the “hype” surrounding diversity, and associates a positive value to it. Or alternatively, market participants may take a skeptical attitude towards diversity, and discount it. Our results are broadly consistent with this last hypothesis.

Our first set of tests revolves around analyst forecasts. We focus on analysts for two reasons. First, they are central information intermediaries in financial markets, who by gathering, analyzing, and producing information for the investment community have the potential to influence asset prices and corporate value (Kothari, So, and Verdi (2016)). As a result, the way analysts respond to management team diversity informs the way the market as a whole will respond. Second, unlike for other market participants, the expectations of analysts are made public in the form of earnings forecasts and price targets. We are thus able to directly relate management team diversity to those expectations.
Our baseline finding is that analysts appear to systematically underestimate the ability of firms with a diverse top management team (henceforth, diverse firms) to generate earnings. This effect is statistically very robust, and economically substantial: firms in the top diversity quartile exhibit earnings forecast errors that are 6 to 9 basis points larger than firms in the bottom quartile. Relative to the average E/P ratio of 47 basis points in our sample, therefore, analysts expect the more diverse firms to be 19% less profitable than they really are. Similarly, analysts systematically underestimate stock price targets: diverse firms exhibit, on average, one year-ahead returns exceeding those implied by target prices by 58 to 154 basis points.

Several additional tests strongly suggest that these findings are driven by the analysts’ perception of management team diversity, beyond any material differences in the way diverse firms are managed. First, firm-level characteristics such as governance strengths, organizational capital, or complexity of the firm’s activities do not appear to explain the earnings forecast error. Second, and in contrast, nearly one half of the forecast error on diverse firms can be explained by team-level characteristics such as shared employment history, education, gender, age, and national background of top managers. Third, inexperienced analysts’ earnings and price forecast errors for diverse firms are much larger than compared to experienced analysts. In fact, analysts in the top experience quartile (based on a measure of experience due to Bouchaud, Ciliberti, Landier, Simon, and Thesmar (2016)) make earnings forecast errors about two-thirds smaller than analysts in the bottom experience quartile, and they make virtually no target price error. In other words, characteristics of the top management team, but not the firm, help explain the forecast error; and it is the least experienced analysts, who underestimate the earnings and stock price performance of diverse firms.

These findings are consistent with analysts implicitly discounting firms with diverse top management teams. This could be due to the fact that, because they are harder to fit into familiar categories, such firms lack “legitimacy” to the eyes of the analysts (Zuckerman (1999)). It can also be because diverse teams are harder to understand. This channel is similar to one proposed by Hirshleifer, Hsu, and Li (2015), in their study of mispricing of innovative originality. Those
authors write that the literature on ambiguity aversion suggests that “when investors perceive higher uncertainty about an investment opportunity, they view it more skeptically,” and that there is “psychological evidence showing that observers tend to interpret signals with lower processing fluency with greater skepticism, and view the subject matter of such signals as riskier.”\(^3\) Translated to our setting: processing fluency is plausibly higher for homogenous teams than for diverse teams. Analysts may thus be more confident assessing the quality of a homogenous team, and since perceived ambiguity is higher for diverse teams, they view them with greater skepticism. Greater analyst experience helps reduce the uncertainty, compensating for this effect. The fact that the discount appears related to team rather than firm characteristics further indicates that we are not picking up the spurious effect of some corporate attribute; instead, analysts expectations are informed by the features of the top management team that define its diversity.

Finally, we find that the market responds to management team diversity in a similar way as the analysts. We present two pieces of evidence in this sense. First, we document high average returns on diverse firms. Depending on the risk adjustment, a portfolio of diverse firms earns on average 25–46 bps per month in excess of a portfolio of homogenous firms during our sample period, consistent with investors discounting diverse firms. Second, the abnormal returns for diverse firms are realized around earnings and news release dates, indicating that the market revises (excessively negative) expectations about diverse stocks on days when new firm-specific information becomes publicly available.

Combined, our paper presents strong evidence that analysts, and more in general investors, systematically discount diverse firms. Given that fundamental differences between diverse and homogenous firms do not seem to explain the observed discount, we conclude that market participant perceptions are important for understanding the impact of top management team diversity on valuation. This perception channel is, to the best of our knowledge, new to the literature on diversity in top management teams.

\(^3\)We refer the reader to Hirshleifer, Hsu, and Li (2015) on these points.
1.1 Relation to Existing Literature

Our paper contributes to the literature by documenting a systematic discount for diverse stocks on part of analysts and the market. Studying how analysts and investors respond to top management team diversity is motivated by two facts. First, the idea that the diversity of corporate leadership matters for corporate outcomes is long standing, and firmly rooted in a large management literature, as well as a growing literature in finance (see e.g. Jackson, Joshi, and Erhardt (2003), Harrison and Klein (2007), Nielsen (2010) for surveys of the management literature; see below for references in finance). Second, there is anecdotal and scientific evidence suggesting sophisticated investors and analysts often pay close attention to the quality of the management team (e.g., Du Pont Capital (2014), Brown, Call, Clement, and Sharp (2015), Gompers, Gornall, Kaplan, and Strebulaev (2016)). The interest among institutional investors such as BlackRock Inc. and the group of investors petitioning the SEC, mentioned in the introduction, indicates that diversity has become an especially salient dimension of the management team’s quality. Because of the controversy surrounding diversity, it is important for corporations as well as researchers to understand how the market responds to it. These facts provide strong a priori ground to study the link between top management team diversity and valuation, and our study is, to the best of our knowledge, one of the most comprehensive on this link.

Our study is related to the literature on the role of biased investor expectations (e.g., Barberis, Shleifer, and Vishny (1998), Daniel, Hirshleifer, and Subrahmanyam (1998)). A large literature has linked biased expectations to valuation, and in particular to stock market anomalies. Related to our paper, Engelberg, McLean, and Pontiff (2017b) study the role of biased cash-flow expectations on a broad set of 97 anomalies. Engelberg, McLean, and Pontiff (2017a) document that analysts have downward-biased expectations about firms in anomaly portfolios, and argue that one channel through which anomalies manifest is investors following biased analyst expectations. In a similar vein, Bouchaud, Ciliberti, Landier, Simon, and Thesmar (2016) and Asness, Frazzini, and Pedersen (2017) study biased analyst expectations as a source of the returns to “quality” investing. Our paper provides new and complementary evidence supporting the view
that analyst forecasts can reflect underlying biases, and on their link to market valuation.

Our paper is also related to an important corporate finance literature on the diversity of corporate boards (see e.g., Ferreira (2010) for a survey). Examples include studies on the effects of women on boards (e.g., Adams and Ferreira (2009), Ahern and Dittmar (2012), Adams (2015), Kim and Starks (2016)), studies on CEO power vis-à-vis the board (e.g., Adams, Almeida, and Ferreira (2005), Fahlenbrach (2009), Bebchuk, Cremers, and Peyer (2011)), studies on nationality of board members (e.g., Masulis, Wang, and Xie (2012)), studies on variation in expertise and prior work history (e.g., Güner, Malmendier, and Tate (2008), Coles, Daniel, and Naveen (2015)), and studies that combine several characteristics into an index (e.g., Gompers, Mukharlyamov, and Xuan (2016), Giannetti and Zhao (2016), Adams, Akyol, and Verwijmeren (2017)).

There are three key differences between our study and most of the existing literature on diversity in finance. First, almost all papers in the literature use a bottom-up approach, i.e., they measure diversity by relying on one or more variables hypothesized to capture the relevant dimension of group heterogeneity (e.g., age, gender, education, etc.). By contrast, our approach is top-down: we rely on similarities in biographical texts, which eliminates the need to narrow the focus down to a small, pre-specified set of dimensions and allows us to capture similarities on a very detailed level. By using a top-down approach, our study complements the bottom-up approach in the existing literature. This contributes fundamentally new evidence on the impact of diversity on financial outcomes.

Second, we focus on the top management team of executives, rather than corporate boards. While boards may matter more for the broad strategic direction of a company, the top management team is more likely relevant for the firm’s day-to-day operations. Compared with the literature on corporate boards, few papers focus on top management teams, even though good theoretical and empirical reasons suggest looking at top management teams can be incrementally valuable (see e.g., Landier, Sauvagnat, Sraer, and Thesmar (2013)). Our findings thus provide new evidence from top management teams, to complement existing work on corporate boards.

Third and final, our results suggest top management team diversity induces differential in-
vestor perceptions and promotes misvaluation. The role for investor perceptions is specific to 
the stock market setting, and thus a new contribution relative to existing work which focuses on 
firm-level corporate outcomes.

2. Background, Measurement, and Data

2.1 Background on Top Management Team Diversity

The idea that top management teams matter for firm outcomes has a long tradition in the 
management literature. In their seminal paper, Hambrick and Mason (1984) argue that key to 
understanding why organizations act or perform the way they do is the analysis of the biases 
and disposition of their “upper echelon,” i.e., their top executives. A central conjecture in 
Hambrick and Mason (1984) is that an executive’s cognitive frame, which determines her values 
and beliefs, and therefore, ultimately, her corporate decisions, can be proxied for by observable 
characteristics. In a review article on the vast upper echelons literature, Hambrick (2007) writes:

“Given the great difficulty obtaining conventional psychometric data on top execu-
tives (especially those who head major firms), researchers can reliably use informa-
tion on executives’ functional backgrounds, industry and firm tenures, educational 
credentials, and affiliations to develop predictions of strategic actions... researchers 
generated substantial evidence that demographic profiles of executives (both 
individual executives and top management teams) are highly related to strategy and 
performance outcomes.”

The empirical approach we use in this paper, to analyze biographical texts on corporate executives 
with respect to how similar they are, is consistent with the upper echelon paradigm.

2.2 Measuring Diversity from Biographical Texts

A main innovation of our study is to propose a new way of measuring top management team 
diversity, which builds on recent advances in textual analysis in the finance literature.

The core of our data are biographical texts which all listed U.S. firms need to file for each 
top executive and year with the SEC under Regulation S-K of the U.S. Securities Act of 1933.
Items 401(b), (c), and (e) require firms to identify each executive officer or other significant (non-director) employee, and report their principal occupations and employment over the past five years plus any material information on relevant business experience and professional competence. The following is one example of a text firms provide in response to this SEC requirement. It is from General Electric’s 2009 proxy statement and describes the company’s CEO, Jeffrey Immelt:

Mr. Immelt joined GE in corporate marketing in 1982 after receiving a degree in applied mathematics from Dartmouth College and an MBA from Harvard University. He then held a series of leadership positions with GE Plastics in sales, marketing and global product development. He became a vice president of GE in 1989, responsible for consumer services for GE Appliances. He subsequently became vice president of worldwide marketing product management for GE Appliances in 1991, vice president and general manager of GE Plastics Americas commercial division in 1992, and vice president and general manager of GE Plastics Americas in 1993. He became senior vice president of GE and president and chief executive officer of GE Medical Systems in 1996. Mr. Immelt became GE’s president and chairman-elect in 2000, and chairman and chief executive officer in 2001. He is a director of the Federal Reserve Bank of New York, a trustee of Dartmouth College, and was recently named a member of President Obamas Economic Recovery Advisory Board.

For each firm, information about each of its top management team members is provided in filings available in electronic form from the SEC on its EDGAR website. We use a web crawler to retrieve these data, going back until 1999 (coverage issues and changes in layout requirements dictate our starting year). Diversity in our study is the degree of dissimilarity in the backgrounds of a firm’s executive officers, as represented in the biographies reported in the firm’s filings. To measure diversity, we rely on the cosine similarity method, a well-established method widely used in a recent strand of the finance literature (e.g., Hanley and Hoberg (2010), Hoberg and Phillips (2016)).

Firms provide biographies either in the annual report, or in the proxy statement. We thus electronically scan forms “10-K,” “10KSB,” or “DEF 14A” in the SEC EDGAR database for each firm and year. In 10-Ks, the biographies are usually provided in Item 10 or Item 4A. In proxy statements (DEF 14A), which have a less standardized structure, the biographies can often be found in a specific section whose title refers to “Executive Officers” or “Management.” We employ a web-crawling algorithm written using Python to collect and process the biographies.
We use human intervention whenever the non-standard format of a firm’s filing does not allow the program to extract the biographies. Using this approach, we obtain a raw sample of 59,863 firm-year observations, consisting of 420,428 executive biography-year observations.

Next, we build the main dictionary. To this end, we take the list of all unique words used in all biographies in year \( t \). Following Hoberg and Phillips (2016) we restrict attention to words classified as either nouns or proper nouns. We also keep adjectives, because words like “international” can carry informational value in our context. Also following Hoberg and Phillips (2016), we exclude words that appear in more than 25% of all biographies in a given year because such words are unlikely to convey meaning (e.g., “company”). The resulting list of \( N \) words is the “main dictionary” and it is represented by a vector of length \( N \). We then summarize each biography’s usage of the \( N \) words of the “main dictionary” by means of an \( N \)-vector. The \( n \)-th entry of a biography’s \( N \)-vector is 0 if the \( n \)-th dictionary word is not used in the biography, or \( x \), where \( x \) is the number of times the \( n \)-th word appears in the biography. The output is, for firm \( k \) in year \( t \), a \( M \times N \) matrix, where \( M \) is the number of executives in the top management team of firm \( k \) in year \( t \).

Table B.1 illustrates typical words in the biographies by showing the 100 most frequently used words in the “main dictionary” for the year 2011. As is evident from this list, texts relate to many different areas that are plausibly related to similarities between executives, including: industries (“technology”, “bank”, “engineering”), functional backgrounds (“operations”, “marketing”, “sales”), job titles (“controller”, “treasurer”, “CEO”), geography (“international”, “global”, “California”), education (“degree”, “bachelor”, “MBA”). Frequent words also cover dimensions of similarity that are potentially relevant, but harder to measure (“leadership”, “responsibility”, “governance”). Overall, the list highlights an advantage of the text-based approach: we get a very detailed and, at the same time, high dimensional, image of similarities across executives.

Some words in the list also illustrate a disadvantage of text-based methods: measurement error. For example, the most used word in the year 2011 is “position,” which is unlikely to
signal similarity among executives. A word like “position,” which is commonly used but likely unrelated to diversity, will noise up our diversity measure, and therefore work against us finding strong effects, but it should not otherwise bias our findings.

For each biographical text associated with executive $i$, company $k$, and year $t$, vector $T_{ikt}$ is a row in the $M \times N$ matrix and describes the biography’s word usage. For each pair of executives $i, j$ of company $k$, in year $t$, we then define the similarity of two biographical texts as:

$$CS_{ijkt} = \frac{T'_{ikt}T_{jkt}}{\|T_{ikt}\| \times \|T_{jkt}\|} = \frac{\sum_{n=1}^{N} T_{nikt} \times T_{njkt}}{\sqrt{\sum_{n=1}^{N} T_{nikt}^2} \times \sqrt{\sum_{n=1}^{N} T_{njkt}^2}}.$$  

(2.1)

$CS$ is the cosine of the angle between $T_{ikt}$ and $T_{jkt}$ in Euclidean space, and is thus bounded between 0 and 1. We then define diversity for a given firm-year as:

$$D_{kt} = 1 - \overline{CS}_{kt},$$  

(2.2)

where $\overline{CS}_{kt}$ is the average of $CS_{ijkt}$ over all $[M \times (M - 1)]/2$ executive pairs in firm $k$ in year $t$. We consider firms with only one reported top executive as maximally homogenous and thus set $D = 0$.

To get an intuition for the diversity measure, consider a simple example with only two executives and a word dictionary of only two words “Blue” and “Red.” If executive $i$’s biography reads “Blue,” her vector $T_{ikt}$ is $(1 \ 0)$. If executive $j$’s biography is also “Blue,” then $T_{jkt} = (1 \ 0)$ and, using the definition above, $CS_{ijkt} = (1 \times 1 + 0 \times 0)/(\sqrt{1} \times \sqrt{1}) = 1$. Hence, if executives have identical biographies, $CS = 1$ and, therefore, $D = 0$, i.e., diversity for this top management team is zero. Suppose now that executive $j$’s biography reads “Red.” Then, $T_{jkt} = (0 \ 1)$ and $CS_{ijkt} = (1 \times 0 + 0 \times 1)/(\sqrt{1} \times \sqrt{1}) = 0$. It follows that $D = 1$, which means this team of top executives is maximally diverse.

We provide selected examples of firms with low diversity score (bottom diversity quartile)

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The Hoberg and Phillips (2016) 25% filter is designed to delete most of such common words. The word “position” in the year 2011 apparently just missed the 25% cutoff.
and high diversity score (top diversity quartile), according to equation (2.2), in Appendix C. For brevity, we only show three executives for each team, and we underline common words not eliminated by any of our filters above. The first example, Anaren, Inc., shows that the texts capture similarities in functional experience (“Electrical Engineering”) and educational background (“Master’s Degree in Business Administration from Syracuse University”). The second example, Collectors Universe, Inc., represents an example of a firm with high diversity score. The texts of the biographies show that the three executives have different backgrounds in terms of prior work experience, area of expertise and education.

A potential concern with analyzing texts in SEC filings is that the executives may not write the biographies themselves. We do not believe this is a serious limitation in our setting. First, while executives are not writing the bios, it is likely that many, if not most, would at least read them, given this is detailed personal information to be widely distributed among investors in a formal document. Second, the underlying biographical information (e.g., where the executive obtained her MBA or whether she has worked for a given company in the past) does not depend on who writes the biography. Third, the SEC requires certain items to be part of the bio, so the ability to “cherry-pick” entries is limited. Finally, if someone else writes the bio, or if there is some cherry-picking, we expect this to lower the signal-to-noise ratio, which would work against us finding any results.

2.3 Data

We merge the firm-level diversity measures with the CRSP-Compustat Merged database. In our tests, we drop all firms with missing or negative book value of equity and utilities (sic codes 4900-4999). Our final sample, after searching for biographical texts, and after applying our filters above, has data on 73,692 individual executives, in 6,898 unique firms, and has 38,971 useable firm-years. All returns and firm level variables are winsorized at the 1st and 99th percentiles.

Table 1 presents summary statistics. In Panel C, we show time-series averages for various variables of interest when firms are sorted, each year, by their diversity measure (all variables are
defined in the appendix.) Diverse firms are smaller on average; however, at $2.8 billion of average market capitalization, they are not small in absolute terms. Diverse firms are similar to homogenous firms in their book-to-market ratio, and previous 12-month stock returns (Momentum). Diverse firms do not differ much from homogenous firms in terms of institutional ownership, but they have slightly lower number of analysts following, and slightly higher share turnover, earnings volatility, and number of days between the end of fiscal quarter and the earnings announcement date. Finally, diverse firms tend to have moderately smaller top management teams, but longer biographical texts for each executive.

2.4 Team-Level Correlates of Text-Based Diversity

Building on the large literature on textual analysis and studies that employ textual analysis in economics, we argue that similarities in biographical texts provide meaningful information on similarities between individuals. To bolster this case, we now show that the text-based diversity measure is correlated with a range of individual variables that are plausibly associated with the diversity of a top management team.

We obtain individual measures from two sources: (i) BoardEX, which has data on executives in a wide range of firms over our sample period, (ii) ExecuComp, and (iii) the biographical texts themselves. We first construct two employment-related variables (details on the definitions are provided in the appendix): company overlap, which measures for each firm-year the number of unique company names that appear in the biographies of at least two executives, and thus captures prior work experience; tenure standard deviation, which captures the within team variation in the time each executive has spent working in the top management team at the current company. We also include two education-related variables: university overlap, which captures whether executives on the team went to the same universities; and elite university standard deviation, which captures whether a team executive went to one of the ivy league universities. Finally, we include three variables on demographic diversity within the top management team: nationality mix, age standard deviation, and gender standard deviation.
Table 1, Panel D, presents correlations between the various individual measures and the text-based diversity measure. Just as one would expect, the diversity measure is lower for teams in which multiple executives are linked to the same set of companies, and for teams whose members have greater overlap in their tenures as top executives in the current firm. Also in line with expectations, diversity is lower in teams in which multiple executives have obtained their education from the same universities. The pairwise correlations across all three dimensions are significant at the 1% level. Diversity tends to be higher in firms with higher within-team dispersion in ivy league university attendance (significant at the 5% level).

Among demographic variables, we find that diverse teams are more likely to have members with different nationalities (significant at the 5% level), which accords well with intuition. In line with expectations, age standard deviation and gender standard deviation are positively correlated with text-based diversity (significant at the 5% and 1% level, respectively).

Overall, we conclude that the text-based measure aligns well with a number of observable dimensions of diversity. The text-based diversity measure seems to capture particularly well what Jackson, Joshi, and Erhardt (2003) call Task-Related Diversity, i.e., diversity in function, prior experience, and education which, according to those authors, are more likely to be related to “knowledge, skills and abilities needed in the workplace” than pure demographic variables. Combining the examples we provided above, we are therefore confident that our measure provides a useful gauge of the diversity of a top management team.

While it is reassuring to see the text-based top-down measure broadly lines up with individual bottom-up measures, it bears emphasizing that a key advantage of the top-down text-based measure is that it can capture information from many dimensions simultaneously – to be precise, it can capture \(N\) dimensions, the length of the word dictionary, which is around 55,000 in the average year in our sample, and thus much larger than the number of bottom-up categories a researcher can reasonably pre-specify. The top-down measure may therefore capture a lot of information which individual bottom-up measures may miss. A second advantage of the text-based approach to measuring diversity is that it aggregates this large number of individual
3. Baseline Results

This section presents our main result: financial analysts systematically underestimate the earnings of firms with a diverse top management team. We first establish this fact in the data, in progressively tighter regression specifications. We show that the result that analysts underestimate diverse firms is robust, and also shows up in target prices. We explore the economic mechanism underlying these findings in later sections.

3.1 Earnings Forecasts

To investigate the market’s response to top management team diversity we would, ideally, like to examine the expectations of market participants on a given stock at a given point in time. This kind of information, however, is not available in general. Therefore, we turn to what we believe is the best available substitute: financial analyst forecasts.

Analyst forecasts make the expectations of analysts public. Thus, they reveal to us the response to management team diversity of a key class of market participants. They are also important for a second reason. Analysts are central information intermediaries in financial markets; they gather, analyze, and produce information that investors rely on in their pricing and trading decisions (Kothari, So, and Verdi (2016)). It is thus possible that the response to diversity of the market as a whole reflects analyst expectations. In other words: Analyst forecasts matter to us in their own right, as an observable gauge of how the market assesses top management team diversity, or because of their impact on the formation of the market’s assessment.

In our first test, we look at analyst earnings forecasts. The variable of interest is the earnings-to-price ratio E/P, defined as earnings per share divided by the stock price at the end of the calendar quarter prior to the earnings announcement date. We compare actual E/P ratios to forecasts. Earnings forecasts are defined as the one- or two-quarter ahead earnings per share
forecast, issued or reviewed during the 60-day period preceding the earnings announcement
date by an analyst covering the firm as reported in the IBES database. This variable is also
standardized by the stock price at the end of the previous calendar quarter. Finally, we define
the forecast error as the (signed) difference between actual and forecast E/P ratios.

Prior work shows that analyst forecasts are on average too optimistic, a fact that is often
attributed to conflicts of interest (e.g., Michaely and Womack (1999), Hong and Kubik (2003)).
The absolute level of forecast E/P ratios or their deviation from the actual realized ratios,
therefore, need not be very informative in our setting. Instead, we base our tests on the relative
level of actual minus forecast returns and ask whether analyst forecasts are, all else equal, more
pessimistic for diverse firms than homogenous firms. We implement this test by estimating the
following regression:

$$y_{iat} = \alpha + \beta D_{it} + \gamma' x_{iat} + \epsilon_{iat}$$ (2.3)

The dependent variable is the E/P ratio forecast error on stock $i$, made by analyst $a$ at time$t$; alternatively, it is the actual E/P ratio or the forecast. $D_{it}$ denotes top management team
diversity for firm $i$ at time $t$. $x$ is a vector of control variables including the natural logarithm of
market capitalization, log-book-to-market ratio and prior 12-month stock returns as of the end
of the fiscal quarter preceding the forecast, analyst coverage (equal to the natural logarithm of
1 plus the number of analysts covering the stock), reporting lag, reporting lag square and cube,
institutional ownership, earnings volatility, earnings persistence, and turnover as in Hirshleifer,
Lim, and Teoh (2009), and the number of earnings forecasts issued by the analyst in the current
quarter (all variables are defined in the appendix). We also include various combinations of fixed
effects, by date, analyst $\times$ date, and industry $\times$ date.

The results are reported in Table 2. Across all specifications, we find a strong positive rela-
tionship between E/P forecast error and $D$; that is to say, analysts systematically underestimate
the future earnings of diverse firms. The result is robust to a number of control variables, as well
as to a fine mesh of analyst $\times$ date fixed effects. The effect implied by the estimates of Table 2
is also sizable: one standard-deviation increase in $D$ (0.20, close to the inter-quartile range) is
associated with a 6 (= 0.20 × 0.304, specification (4)) to 9 basis points (specification (1)) larger forecast error. Relative to the average E/P ratio of 47 basis points in our sample, these effects appear economically substantial.

Importantly, this result is driven by the actual earnings realization, rather than the forecast itself. When we decompose the forecast error into its “actual” and “forecast” components, we find a weak relationship between top management team diversity and the E/P forecast, statistically indistinguishable from null at conventional significance levels. In contrast, we observe a strong positive relationship between $D$ and actual E/P ratio. This provides a first indication of the channel for the analysts’ response to top management team diversity, suggesting it is not related to material differences in the way diverse firms are managed. If analysts took a negative view on diverse firms, reflecting e.g. bad management practices at those firms, one would expect lower, or at least not higher, realized earnings. We find the opposite: in other words, diverse firms tend to beat consensus analyst earnings forecasts relative to homogenous firms. We come back to this point in Section 4., where we discuss the economic mechanism.

3.2 Target Prices

The finding that analysts discount diverse firms is robust to an alternative context: target prices. We compute for each stock $i$ and month $m$ the expected one-year ahead return implied by analysts’ target price forecasts (“forecast”), and we compare it with the corresponding ex post realized one-year return (“actual”). The expected one-year ahead return is defined as $TP_{i,m+12}/P_{im} - 1$, where $TP_{i,m+12}$ is the average one-year ahead target price across all active analyst forecasts on firm $i$ in month $m$ in the IBES database, and $P_{im}$ is the current stock price. The ex post realized return is the 12 months cumulative stock return ex-dividend as reported in CRSP.

We implement this test by regressing the actual minus forecast return difference on diversity; date fixed effects, which ensure we are comparing firms in the same year and month; control variables are the logarithm of market capitalization, book-to-market, momentum, turnover, id-
iosyncratic volatility, return_{m-1}, the fraction of firm shares held by institutional investors, the number of target price forecasts issued by the analyst in the current month, the number of analysts issuing a target price estimate in the current month, and dispersion, defined as the standard deviation of the price targets divided by the average price target.

Table 3 presents the results. Similarly to what we show in Table 2, here too we find a positive relationship between forecast error and top management team diversity. The effect is robust across all specifications and survives the inclusion of analyst × date and industry × date fixed effects. It is also economically meaningful: One standard-deviation increase in diversity associates with a 58 bps (= 0.029 × 0.20, columns (4)) to 154 bps (column (2)) larger forecast return error. In other words, the analysts underestimate the stock price performance of diverse firms, besides their future earnings.

We conclude that the results in Tables 2 and 3 are strong evidence for the view that analysts systematically discount diverse firms. They are not optimistic enough when judging the future performance of firms with diverse top management teams.

4. Economic Mechanism

The above results establish that diversity is associated with low analyst earnings forecast and target prices. The interesting question is why that happens. One possibility is that diverse firms are simply managed in a fundamentally different way than homogenous firms, and analyst forecasts reflect some negative aspect of their management. But as we argued, the fact that we observe systematic forecast errors speaks against this interpretation. If anything, given the indication from Table 2 that actual E/P ratios are higher for diverse firms, if analyst forecasts reflected a difference in actual management, analysts should be more positive about diverse firms.

An alternative possibility is that the perception of diversity affects analyst forecasts, over and above what diverse firms actually do. In this section, we explore this interpretation. We produce three pieces of evidence. First, we show that characteristics of the firm and the way it is managed do not capture the discount that analysts appear to apply to diverse firms. Second, we
show that characteristics of the top management team do explain, at least in part, that effect. Third, we show that the effect is more pronounced among less experienced analysts, and discuss the implications of this result.

4.1 Firm-Level Determinants

Table 1, Panel D, indicates that diversity is correlated with a set of firm-level variables. Diverse firms tend to be smaller, have lower number of analysts following, higher earnings volatility, higher reporting lag, and higher share turnover. All of our tests control for those variables, indicating that the findings of Tables 2 and 3 are not driven by them. In addition, we have shown that our baseline findings are robust to the inclusion of industry \( \times \) date fixed effects, indicating that they are not driven by diverse firms clustering in particular industries. We now look more closely at several other alternative drivers. We ask if our measure of top management team diversity is in fact picking up the effect of a number of firm-level characteristics such as governance quality, firm-level diversity, complexity, or organizational capital.

Diversity as a Proxy for Corporate Governance or Firm-Level Diversity

Firms with diverse top management teams may differ from homogenous firms because they are managed in a fundamentally different way. A key dimension of management, which is likely salient to financial analysts and may be reflected in their forecasts, is corporate governance quality. To examine this possibility, we measure of governance strength by the Total Number of Governance Strengths index from the RiskMetrics KLD STATS database. The Total Number of Governance Strengths is an index based on a set of underlying “governance strengths,” focusing on dimensions such as compensation, ownership, and transparency. The index covers individual companies on a yearly basis throughout our sample period.\(^5\) Speaking against diversity returns being driven by governance, we find that the raw correlation between governance and diversity

\(^5\) The coverage of the index is limited to S&P500 and KLD400 Social Index firms until 2000; following that year, it is progressively expanded, to cover the 3000 largest U.S. companies by market capitalization. For details on the variables in the KLD STATS dataset, please see: http://cdnete.lib.ncku.edu.tw/93cdnet/english/lib/Getting_Started_With_KLD_STATS.pdf.
is negative and close to zero ($\rho = -0.03$).

We reproduce the earnings forecast test of Table 2 controlling for the Governance Strengths index, in Table 4, specification (2). The coefficient on $D$ is statistically indistinguishable from the baseline estimate of specification (1), and the coefficient on the governance index itself is small and insignificant.\footnote{The coefficient estimate in specification (1) differs from the one reported in Table 2 because all the variables in Table 4 have been standardized so as to have mean 0 and standard deviation 1, to facilitate a direct comparison between their economic effects.}

In unreported results, we reproduce this test using the E-Index proposed by Bebchuk, Cohen, and Ferrell (2009) as a firm-level governance quality measure. One caveat is that the E-Index is available only until 2006; we thus use the last available value for each firm for our entire sample period. Here too, the correlation between governance and diversity is negative and close to zero ($\rho = -0.02$); and controlling for the E-Index in the earnings forecast error regressions leaves the coefficient on $D$ virtually unchanged. Taken together, these findings indicate that diversity unlikely proxies for corporate governance quality in our test.

Rather than conventional corporate governance, top management team diversity may proxy for the diversity of the workforce, or, more broadly, for an awareness to diversity issues within the firm. Ex ante, it is not obvious whether this should be viewed positively or negatively by analysts. Analysts may take it as lack of focus on shareholder value creation, and penalize diverse firms in their forecasts. But alternatively, firms hiring the most talented employees irrespective of their background may increase employee satisfaction, resulting in a better expected performance (e.g., Edmans (2011)).

With this caveat in mind, we measure Workforce diversity based on six non-top management related diversity strengths provided in the KLD STATS dataset,\footnote{Specifically, we form an index by summing over the following 6 diversity strengths: (i) promotion of minorities and women, (ii) work-life-benefits at the company (iii) whether the firm does significant amounts of business with women or minority owed subcontractors or suppliers, (iv) employment of the disabled, (v) gay and lesbian policies, and (vi) other diversity strengths.} and reproduce the earnings forecast error test of Table 2, controlling for firm-wide diversity in specification (3) of Table 4. The coefficient on firm-wide diversity is negative and marginally significant; but again, the coefficient on top management team diversity is nearly identical to the baseline. Thus, the top
management team diversity variable does not appear to reflect the impact of diversity further down in the organization.

**Diversity and Complexity**

Top management team diversity could affect analyst forecasts because diverse *firms* are more complex and harder to understand. This argument has two parts. First, analysts may take a more negative view of, and make larger forecast errors on, more complex firms. This could be because complexity reflects some fundamental risk, or because it associates with greater uncertainty and lower information quality and analysts are ambiguity averse, or just find it harder to assimilate value-relevant information from more complex financial disclosures, promoting forecast errors. The second part of the argument is that diversity indeed associates with firm-level complexity. We start by taking this point to the data – obviously, if diverse teams are not associated with more complex firms, the diversity-complexity channel cannot be operative.

We use two approaches to measure complexity: fundamental and text based. We propose two proxies for fundamental complexity. The first proxy is the prior year’s idiosyncratic stock return volatility, computed from daily returns using the Fama-French-Carhart four-factor model. The second one is a Hirschman-Herfindahl index (HHI) over segment sales obtained from the Compustat Segments file, which has been used in prior work as a complexity proxy (e.g., Loughran and McDonald (2014)). The underlying motivation is that firms with substantial operations in multiple segments are more complex than firms which predominantly operate in one line of business.

We also propose two text-based complexity measures, building on a recent strand of the textual analysis literature in finance which analyzes texts in financial disclosures (e.g., Loughran and McDonald (2013)). We first test whether 10Ks of diverse firms differ in readability, defined as the ease with which investors and analysts “can assimilate value-relevant information from a financial disclosure” (Loughran and McDonald (2014), p.1649). Readability is thus a measure of textual complexity. As a proxy for readability, we look at the 10K file size in the SEC’s EDGAR
A second textual dimension we analyze is the tone of the annual reports. We rely on a widely-used word list that has recently been developed by Loughran and McDonald (2011) specifically to analyze financial texts such as those in 10Ks: Uncertain Words. Uncertain words are words denoting uncertainty, with an emphasis on the general notion of imprecision, for example: “approximate”, “depend”, “indefinite”, or “uncertain.” We compute the rate of uncertain words in the text of the 10K form, and use it in our tests as a measure of complexity.

Table C.3 present results. The first four columns show that 10Ks are indeed more complex, i.e. they are larger and use significantly more uncertain words, when they are issued by firms with diverse top management teams. Two, not mutually exclusive, reasons may explain this. The first reason is that the texts reflect fundamentals, and that the companies themselves are more complex and have business models associated with greater uncertainty. A second possibility why text in diverse firms’ 10Ks are more complex and uncertain relates to the inner workings of diverse teams. When evaluating a business opportunity, diverse teams are likely to have greater diversity of opinion than homogenous teams. If there is greater variation of opinions within the team, then this variation may be reflected in information the firm discloses to investors, thus leading to longer documents that use more uncertain language.

Among the fundamental complexity measures (columns (5) and (6)), the evidence is mixed. We find a strong relation between diversity and idiosyncratic volatility, but no relation between diversity and segment HHI. All results C.3 control for industry × year fixed effects, so they are not simply reflecting reporting conventions in certain industries, or special events in certain industries, and all results also control for market capitalization, book-to-market, and past returns.
Having established that there is indeed a link between measures of complexity and diversity, we next ask if the discount applied by analysts to diverse firms is due to complexity, rather than top management team diversity. Specifications (4)–(7) in Table 4 reproduce the baseline test of Table 2, controlling for idiosyncratic volatility, business HHI, 10-K file size, and uncertain words rate. The findings are clear: although most of these variables (10-K file size, business HHI, and idiosyncratic volatility) do show up significantly in the regression, the coefficient on diversity is virtually unaffected in all specifications. Thus: our baseline effect is not due to greater firm-level complexity, either fundamental or text based.

Text-Level Determinants and Organizational Capital

Finally, we check whether the diversity measure picks up other characteristics of the biographical texts a firm issues. One concern may be that it is the length of the biographical text that matters, rather than the content. When we repeat the test of Table 2 controlling for the Bio Length (column (8) of Table 4), the coefficient on our diversity measure is unaffected, and the coefficient on Bio Length is small and statistically insignificant.

A second variable which determines variation in the available texts, and which might be correlated with diversity, is the number of executives in the top management team. Making sure our results are not reflecting team size is particularly relevant, as Boguth, Newton, and Simutin (2016) relate it to the fragility of organizational capital, which in turn might affect the way diverse firms are managed and through that channel, by the arguments we have discussed above, analyst forecasts. We control for team size in column (9) of Table 4. Although the coefficient on team size is negative and significantly different from zero, including it in our tests does not affect the significance and magnitude of the coefficient on our diversity measure. This indicates that we are not picking up the effect of team size.

Related to the argument that team size might reflect the firm’s organizational capital, in a separate test we directly control for the Eisfeldt and Papanikolaou (2013) organizational capital
index OC. Again, although the coefficient on OC is significantly different from zero, it does not affect the size and significance of the effect of top management team diversity. We conclude that the diversity measure does not pick up effects related to biography length or top management team size.

As a final step, in specification (11) of Table 4 we include all the variables discussed in the preceding sections, along with top management team diversity. In this specification, some of those variables lose significance (for instance, 10K file size and workforce diversity), and the effect of others appears greatly reduced (for instance, team size and organizational capital index). In contrast, the significance and magnitude of the coefficient on diversity $D$ is unchanged. Combined, the evidence from these tests indicates the relationship between diversity and analyst forecast errors is not due to diversity proxying for observable firm-level characteristics.

### 4.2 Team-Level Characteristics

Our second piece of evidence relates to what characteristics of the management team contribute to explaining the relationship between top management team diversity and analyst forecast errors. As we have seen, $D$ correlates with team characteristics related to managerial employment history, education, and demographics in ways that make intuitive economic sense. We now ask whether these variables capture some or all of the baseline effect documented in section 3.

Following an approach similar to the tests presented in Table 4 and the preceding sections, we reproduce the earnings forecast test of Table 2 including the team characteristic variables one at a time, and then jointly, along with top management team diversity. The results are reported in Table 5. Individually, most of the team characteristics show up significantly, with the exceptions of company overlap, the diversity in elite university attendance and executive age; controlling for some of those characteristics reduces the magnitude of the coefficient on top management team diversity, especially executive tenure, age, and gender diversity.

In specification (9), which controls for all team characteristics at the same time, the coefficient on our diversity measure $D$ is reduced by nearly one half, relative to the baseline in specification
(1). This points to two conclusions. First, the information that drives the analysts’ systematic discount of diverse firms indeed has to do with the management team and its diversity – in contrast to other characteristics of the firm and its management, which as discussed in the previous sections do not explain the baseline result of Table 2. Second, even though specific dimensions of diversity such as those included in Table 5 explain some of our baseline effect, after controlling for those dimensions our measure retains a nontrivial portion of its explanatory power. This validates our approach, and suggests that a top-down measure, such as we propose, can capture the many facets of diversity in a way that bottom-up measures miss.

4.3 Analyst Experience

As a third piece of evidence, we study how the forecast error associated with top management team diversity varies across analysts with different levels of experience. We base this analysis on a measure of analyst experience following Bouchaud, Ciliberti, Landier, Simon, and Thesmar (2016), defined as the number of months since the analyst starts following a given firm, based on the information reported in the IBES database. The average analyst in our data follows 4.5 firms in 4.3 different industries. She has an overall career experience of about nine years, but only slightly over 3 years with any given firm (Table 1.B).

We sort analysts into quartiles based on their firm-level experience, and run the test of Table 2 separately for analysts with high (top quartile) and low (bottom quartile) experience. The results are reported in Table 6.A. They indicate that the earnings forecast error documented in the previous section is largely confined to the least experienced analysts. Forecast errors for the most experienced analysts are economically small (the coefficients on $D$ are one– to two–thirds the size of the corresponding coefficients for inexperienced analysts), and in the regression specifications with the finer mesh of fixed effects they become statistically indistinguishable from zero.

We find similar effects when looking at target prices, in Table 6.B. There too, analysts in the top quartile by our experience make smaller errors – virtually none at all, after controlling for
analyst × date and industry × date fixed effects (specification (4)).

In sum, these findings indicate that analysts systematically underestimate the earnings and future stock price of diverse firms when they have a shorter experience with them. The most experienced analysts make smaller (or no) forecast errors.

The combined evidence from the previous sections points to a cognitive channel, related to the perception of diverse firms, explaining the discount that analysts appear to apply to those firms. Our findings indicate that the baseline effect of Table 2 is better explained by characteristics related to the diversity of the management team, rather than the way the firm is managed. Furthermore, the evidence on analyst experience suggests that the effect disappears once the analyst has accumulated experience on the firm, such that facing and understanding a diverse management team no longer presents a cognitive challenge.

5. Impact of Diversity on the Stock Market

The results above establish that analysts systematically discount firms with diverse top management teams. The interesting question, which we now ask, is if the market does too. There are two reasons to expect that this may be the case. First, investors may respond to diversity the same way as analysts do; for instance, that could be because buy side analysts interpret information on top management teams in the same way as the sell side analysts that are the focus of our tests in the previous sections. Second, investors may simply follow analysts, who are more negative on diverse firms.

5.1 Returns on Diverse Firms

To test for the market’s response to top management team diversity, as our starting point we look at the returns earned by the stocks of diverse firms. If the market applies a discount to diverse stocks in a similar way as the analysts, we should expect those stocks to exhibit positive returns.
We form a “diversity strategy” as a value-weighted portfolio long in firms in the top diversity quartile and short firms in the bottom diversity quartile. To ensure that the information about diversity from the firm’s financial reports is available to investors when the portfolio is formed, we use the value of our diversity measure as of December year \( t - 1 \) to form the portfolio from July year \( t \) until June year \( t + 1 \). We then measure the portfolio’s performance, in raw return terms as well as adjusting for risk with the Fama–French 3-, 4-, and 5-factor models.

Table 8, panel A, reports the average raw returns and the intercepts from the factor models, along with the associated \( t \)-statistics. Regardless of the risk-adjustment, diverse stocks outperform homogenous stocks; the returns earned by the long–short portfolio are non-trivial, and range between 25 and 46 basis points on a monthly basis depending on the risk adjustment.

Panel B of Table 8 reports Fama and MacBeth (1973) regressions of monthly stock returns on our measure of diversity, along with a number of firm-specific control variables: log of market capitalization and book-to-market, returns over the previous 12 months, returns over the previous month, idiosyncratic volatility, and share turnover. Across all specifications, the coefficient on \( D \) is positive, and the implied economic effects are meaningful: one standard-deviation increase in top management team diversity is associated with between 4 (\( = 0.20 \times 0.203 \), column (3)) and 15 (column (1)) basis points higher returns on a monthly basis. More diverse firms, therefore, exhibit higher average returns.

### 5.2 Evidence from Firm-Specific Information Releases

The evidence of positive excess returns on diverse firms need not mean that the market undervalues diverse firms. An alternative explanation could be that diverse stocks are “quality” stocks. Indeed, the results of Table 2 indicate that diverse firms tend to have higher E/P ratios, suggesting higher profitability; and profitability has recently been suggested as a signature characteristic of “quality” stocks which have high risk-adjusted returns (e.g., Novy-Marx (2014), Asness, Frazzini, and Pedersen (2017)).

\(^{10}\)While different studies differ in their empirical approaches and definitions of quality, the common intuition is based on the dividend discount model, which can be written as: \( P/B = \text{Profitability} \times \text{Payout ratio}/(r - g) \). This
To test more directly whether the market misprices diverse stocks, we turn to an approach recently proposed by Engelberg, McLean, and Pontiff (2017b). Those authors argue that stock returns on earnings announcements and other corporate news days can be used to detect mispricing. The key idea in this test is that systematic and economically large swings in the day-to-day return differences between stocks around information release days are less likely due to risk (i.e., changes in discount rates), because most risk factors are unlikely to show systematic and large day-to-day swings. Rather, predictable changes in return differences around information-release days are indicative of mispricing.

Engelberg, McLean, and Pontiff (2017b) propose a specific version of a mispricing model, motivated by the literature on biased investor expectations and stock market anomalies (e.g., Barberis, Shleifer, and Vishny (1998), Daniel, Hirshleifer, and Subrahmanyam (1998)). Namely, they posit that investors have downward-biased expectations on stocks in the long leg of anomaly portfolios (and vice versa for the short leg). Upon information releases, such as earnings announcements, investors partly correct their mistake, thus inducing higher stock returns for long-leg stocks precisely on days in which a firm releases information. They show that this view is consistent with return patterns for a large set of documented anomalies from the recent finance literature. Our goal here is to see whether the returns on diverse stocks documented above follow the same pattern, which would indicate that biased expectations are at least partially driving those results.

We follow Engelberg, McLean, and Pontiff (2017b) and estimate:

\[ R_{it} = \alpha_t + \beta_1 D_{it} + \beta_2 E_{dayit} + \beta_3 D_{it} \times E_{dayit} + \gamma' x_{it-1} + \varepsilon_{it}, \]

identity implies, for example, that fixing price-to-book, the payout ratio, and the growth rate, more profitable stocks should have higher returns. This should not be confused with the view that “better firms have higher returns,” which is a fallacy in a rational expectations equilibrium. The higher returns to profitability are either driven by diverse firms being more risky, which would be consistent with rational expectations, or by misvaluation (see e.g., Bouchaud, Krueger, Landier, and Thesmar (2016) for a recent mispricing explanation of the profitability anomaly). From the standard vantage point of rational expectations, results in the management and finance literatures on the link between diversity and performance measures other than stock returns do not in any way imply positive excess returns on diverse firms.
where $R_{it}$ is the return of stock $i$ on day $t$, $D_{it}$ is the last available diversity score for firm $i$, $E_{day_{it}}$ is an indicator variable equal to one if firm $i$ announces earnings on day $t$, $x_{it-1}$ is a vector of controls, and $\alpha_t$ is a calendar-day fixed effect, which eliminates any day-specific variation such as, for example, macroeconomic shocks, or day-of-the-week effects.

The coefficient of interest is $\beta_3$, which measures whether return differences vary systematically with the level of diversity around announcement days. As Engelberg, McLean, and Pontiff (2017b) explain, rational expectations would predict $\beta_3 = 0$, because “in the rational expectations framework, return-predictability is explained by ex-ante differences in discount rates, which should not change in a predictable manner on firm-specific information days.” By contrast, $\beta_3 \neq 0$ indicates mispricing.\(^{11}\)

Table 7, specification (1), presents results. The coefficient on diversity ($\beta_1$) shows that diverse firms outperform homogenous firms on non-announcement days by about 0.5 basis points (calculated using a 0.2 difference in $D$, equal to one standard deviation and roughly the difference in $D$ between the first and fourth quartiles from Table 1). This return difference may reflect compensation for systematic risk, but it is uninformative about mispricing.

The central result is that the coefficient on the interaction term $\beta_3$ is over 10 times bigger, indicating that diverse firms outperform homogenous firms strongly on, and particularly on, earnings announcement days. At 6 bps (assuming again a diversity difference of 0.2), and with a $t$-statistic of 2.90, the incremental effect is both statistically and economically large. This large difference in the return difference between diverse and homogenous stocks between earnings days and non-earnings days constitutes strong support for a mispricing explanation, since rational risk premia would need to vary systematically and by an extremely large magnitude to explain the results.\(^{12}\)

\(^{11}\)Engelberg, McLean, and Pontiff (2017b) examine in detail two alternative possibilities for $\beta_3 \neq 0$: (i) rationally higher correlations with the market on earnings announcement days, and (ii) data mining, i.e. a mechanical effect by which outperforming companies are those that have positive news in a given period. While the joint hypotheses problem makes it impossible to completely rule out these alternative stories, the evidence in Engelberg, McLean, and Pontiff (2017b) suggests that they explain at best a part of the higher returns on earnings announcement days.

\(^{12}\)For example, using the CAPM, and assuming a risk-free rate of 1%, a market risk premium of 4%, 250 trading days, and a beta of 1 for homogenous firms, would require the beta of diverse firms to increase from a level of 1.28 on non-announcement days to a beta of 4.9 on announcement days to be consistent with the above results.
While earnings days may be particularly informative, the underlying logic – that priors are updated on information release days – should apply also to other firms-specific news days. Specification (2) thus replaces the earnings announcement indicator in the above regression by an indicator variable for firm-specific news days, \( N_{day} \). We define firm-specific news days as days for which we can find any news items linked to the firm in the Ravenpack dataset in which the company plays an important role in the main context of the story (as defined by Ravenpack). We find that the return difference between diverse and homogenous firms is significantly higher on news days (coefficient = 0.079, \( t = 5.19 \)), which is again consistent with mispricing.

Because the news items used to construct \( N_{day} \) include news about earnings announcements, specification (3) includes both the \( E_{day} \) and \( N_{day} \) variables and the interactions with diversity. The results are very similar to specifications (1) and (2).

Finally, specification (4) includes as control variables returns, volatility (returns squared), volume, as well as the lagged value of each variable over each of the last 10 days. The results remain unchanged.

It is difficult to determine with great confidence what fraction of the returns documented in Panel A of Table 8 are coming from mispricing. Nevertheless, to get a sense, consider the following back-of-the-envelope calculation. For the average firm in our sample about 76% of days each year are non-information release days, 22% are days with news other than earnings, and the remaining 2% are earnings release days. Assume that the 0.5bp outperformance of diverse firms on non-news days are a “rational” return premium due to risk. (Note this is conservative, as those returns may also be due to mispricing.) Further assume that the incremental returns on news days and earnings announcement dates of 1.6bps and 5.8bps, respectively, are due to mispricing. Based on these assumptions, about 50% of the total return gap between diverse and homogenous firms in a year is due to mispricing, but the actual number could obviously be much higher. The predictable outperformance on the 4 earnings days alone contributes already 24%. While these numbers are crude, they clearly suggest that mispricing explains a substantial fraction of diversity returns.

\[ \text{After the announcement, the beta of diverse stocks would need to decrease again to its original level of 1.28.} \]
In sum, the significant interaction terms in Table 7 show that diverse firms enjoy, all else equal, *predictably* higher returns on information release days. This is hard to square with rational expectations, and therefore provides evidence in favor of a mispricing explanation for the returns on the “diversity strategy” documented above. In particular, the results are consistent with biased expectations of investors as emphasized in Engelberg, McLean, and Pontiff (2017b), which, in our setting, means that diversity returns are due to investors being too pessimistic on firms with diverse top management teams, consistent with the evidence on analysts.

6. Conclusion

We study how analysts and the market respond to top management team diversity, a corporate feature that has been capturing the attention of the media, consulting firms, regulators, investors, and scholars. Our key innovation is that we measure diversity from within-team similarities in biographical texts which executives are required to file with the SEC. Using this new approach, we assemble a dataset which covers a total of more than 70,000 executives in over 6,500 firms.

We find that analysts systematically underestimate earnings and target stock prices of diverse firms. Characteristics related to the way the firm is managed do not appear to explain this effect, which is more closely related to variables that capture aspects of management team diversity, such as executives’ shared professional and educational background and within management team variation in age, tenure, and gender. These findings point suggest that the analysts’ discount of diverse firms is not due to what diverse firms do, but rather to the way their are perceived. Consistent with diverse firms being harder to understand, more experienced analysts make smaller forecast errors on them.

Finally, we show evidence suggesting that the market also misprices diverse stocks. A long-short portfolio based on our measure of top management team diversity earns abnormal returns, consistent with investors undervaluing diverse stocks. In line with recent papers which also conjecture that biased investor expectations are driving important market anomalies (e.g., Bouchaud, Ciliberti, Landier, Simon, and Thesmar (2016), Engelberg, McLean, and Pontiff (2017b), and
Asness, Frazzini, and Pedersen (2017)), the misvaluation appears to be corrected on days when firm-specific news are released.
Table 1: Descriptive Statistics

This table presents summary statistics. Panel A shows statistics for the main firm-level variables used in our analysis. Panel B shows statistics for the main analyst-level variables used in our analysis Definitions of all variables are provided in the Appendix. Panel C shows summary statistics for firms in the diversity quartiles. The last column of this panel reports the t-statistic for the difference between diverse and homogenous, based on standard errors clustered by firm. Panel D shows correlation coefficients between Diversity and a set of team-level variables. \( a, b \) and \( c \) denote significance at the 1%, 5% and 10% levels, respectively.

### Panel A: Firm-level Statistics

<table>
<thead>
<tr>
<th></th>
<th>Avg. N</th>
<th>Mean</th>
<th>St.Dev</th>
<th>25%</th>
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<th>75%</th>
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<tr>
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<td>0.51</td>
<td>0.83</td>
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<td>7.00</td>
<td>13.00</td>
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<td>Reporting Lag (days)</td>
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### Panel B: Analyst-level Statistics

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<th>Avg. N</th>
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### Panel C: Summary Statistics by Diversity Quartiles

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<th>3</th>
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### Panel D: Correlations between Team-Level Characteristics

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<td>(2) Tenure St. Dev</td>
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Table 2: Diversity and Analyst EarningsForecasts

This table compares analyst earnings forecast with ex-post realized values. In the first row, the dependent variable is Actual, and it is computed as the announced earnings, divided by the stock price at the end of the last quarter before the announcement. In the second row, the dependent variable is Forecast, computed as the 1- or 2-quarter-ahead forecast issued or reviewed in the last 60 days before the earnings announcement by an analyst covering the firm as reported by IBES, divided by the stock price at the end of the last quarter before the announcement. In the last row, the dependent variable is A − F, computed as the scaled difference between actual and forecast. In all columns, controls include log of market-cap in June year 𝑡, and log book-to-market ratio, log (1 + Number of Analysts), Reporting Lag, Reporting Lag squared and cubed, Institutional Ownership, Earnings Volatility, Earnings Persistence, Turnover, and the number of firms the analyst follows. Date FE are based on year-quarter dates. Industries are based on the Fama-French 12-industries definition. Standard errors are clustered by date (year-quarter).

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Table 3: Diversity and Analyst Target Prices

This table compares returns computed from analyst forecasts of target prices with ex-post realized returns. In the first row, the dependent variable is Actual, and it is computed as the 12 month cumulative stock return excluding dividends as reported in CRSP. In the second row, the dependent variable is Forecast, computed as $TP_{m+12}/P_m - 1$, where $TP_{m+12}$ is the average one-year ahead target price across all active forecasts in the current month, and $P_m$ is the current stock price. In the last row, the dependent variable is $A - F$, computed as the difference between actual ex post realized return and forecast. Controls are the logarithm of market capitalization, book-to-market, momentum, turnover, idiosyncratic volatility, $\text{return}_{m-1}$, the fraction of firm shares held by institutional investors, the number of target price forecasts issued by the analyst in the current month, the number of analysts issuing a target price estimate in the current month, and dispersion, defined as the standard deviation of the price targets divided by the average price target. Date FE are based on year-month dates. Industries are based on the Fama-French 12-industries definition. Standard errors are clustered by date (year-month).

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**Table 4: Diversity and Firm-Level Observables**

This table regresses the measure of analyst error ($A - F$) on diversity and other firm-level observables characteristics. In all columns, controls include log of market-cap in June year $t$, and log book-to-market ratio, log ($1 +$ Number of Analysts), Reporting Lag, Reporting Lag squared and cubed, Institutional Ownership, Earnings Volatility, Earnings Persistence, Turnover, and the number of firms the analyst follows. Date FE are based on year-quarter dates. $t$-statistics are based on standard errors clustered by date (year-quarter). Definitions of all variables are provided in the Appendix.

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Table 5: Diversity and Team-Level Observables

In this table we regress the measure of analyst error ($A - F$) on diversity and other team-level observables characteristics. In all columns, controls include log of market-cap in June year $t$, and log book-to-market ratio, log (1 + Number of Analysts), Reporting Lag, Reporting Lag squared and cubed, Institutional Ownership, Earnings Volatility, Earnings Persistence, Turnover, and the number of firms the analyst follows. Date FE are based on year-quarter dates. $t$-statistics are based on standard errors clustered by date (year-quarter). Definitions of all variables are provided in the Appendix.

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Table 6: The Impact of Analyst Experience

Panel A compares analyst earnings forecast with ex-post realized values. The dependent variable is $A - F$, computed as the scaled difference between actual and forecast. We split the sample based on a measure of firm-level analyst experience, computed following Bouchaud, Ciliberti, Landier, Simon, and Thesmar (2016), as the number of months since the analyst first started to follow the firm. We report values of $A - F$ for analysts in the bottom quartile of experience (Low), top quartile of experience (High), and the difference between the two. In all columns, controls include log of market-cap in June year $t$, and log book-to-market ratio, log $(1 + \text{Number of Analysts})$, Reporting Lag, Reporting Lag squared and cubed, Institutional Ownership, Earnings Volatility, Earnings Persistence, Turnover, and the number of firms the analyst follows. Panel B compares analyst forecasts of target prices with ex-post realized returns, and reports values of $A - F$ for analysts in the bottom quartile of experience (Low), top quartile of experience (High), and the difference between the two. Controls are the logarithm of market capitalization, book-to-market, momentum, turnover, idiosyncratic volatility, $\text{return}_{m-1}$, the fraction of firm shares held by institutional investors, the number of target price forecasts issued by the analyst in the current month, the number of analysts issuing a target price estimate in the current month, and dispersion, defined as the standard deviation of the price targets divided by the average price target. Date FE are based on year-quarter dates. Industries are based on the Fama-French 12-industries definition. Standard errors are clustered by date (year-quarter).

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Panel A: The Impact of Analyst Experience on Earnings Forecasts
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<td>(2.93)</td>
</tr>
<tr>
<td>High</td>
<td>−0.005</td>
<td>0.039</td>
<td>0.013</td>
<td>0.005</td>
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<tr>
<td></td>
<td>(−0.56)</td>
<td>(2.86)</td>
<td>(1.03)</td>
<td>(0.35)</td>
</tr>
<tr>
<td>Difference</td>
<td>0.044</td>
<td>0.052</td>
<td>0.049</td>
<td>0.060</td>
</tr>
<tr>
<td></td>
<td>(2.64)</td>
<td>(3.47)</td>
<td>(2.57)</td>
<td>(2.60)</td>
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<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Date FE</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Analyst × Date FE</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry × Date FE</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</table>
Table 7: Diversity Returns on Information-Release Days
This table reports results from a regression of daily returns on Diversity, information-release day dummy variables, interactions between Diversity and information-release day variables, day fixed effects and controls. Information-day variables are dummies equal to one on earning announcement dates (Eday), or corporate news release dates (Nday), respectively. Control variables included in specification (4) are lagged values for each of the past 10 days for stock returns, stock returns squared, and trading volume. Standard errors are clustered by day. $t$-statistics are shown in parentheses.

<table>
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<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
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<tr>
<td>Diversity</td>
<td>0.022</td>
<td>0.015</td>
<td>0.015</td>
<td>0.011</td>
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<td></td>
<td>(2.26)</td>
<td>(1.51)</td>
<td>(1.46)</td>
<td>(1.15)</td>
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<td>Eday</td>
<td>0.055</td>
<td>0.030</td>
<td>0.029</td>
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<tr>
<td></td>
<td>(0.67)</td>
<td>(0.37)</td>
<td>(0.36)</td>
<td></td>
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<tr>
<td>Eday × Diversity</td>
<td>0.290</td>
<td>0.244</td>
<td>0.244</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.90)</td>
<td>(2.43)</td>
<td>(2.43)</td>
<td></td>
</tr>
<tr>
<td>Nday</td>
<td></td>
<td>0.038</td>
<td>0.037</td>
<td>0.037</td>
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<tr>
<td></td>
<td></td>
<td>(2.93)</td>
<td>(3.06)</td>
<td>(3.12)</td>
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<tr>
<td>Nday × Diversity</td>
<td>0.079</td>
<td>0.060</td>
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<tr>
<td></td>
<td>(5.19)</td>
<td>(4.15)</td>
<td>(4.26)</td>
<td></td>
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<tr>
<td>Day FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>Controls</td>
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<td>No</td>
<td>No</td>
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<td>5,118,355</td>
<td>5,118,355</td>
<td>5,118,355</td>
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<tr>
<td>Adjusted $R^2$</td>
<td>0.30</td>
<td>0.30</td>
<td>0.30</td>
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</table>
Table 8: Diversity and Stock Returns

Panel A presents returns for the value-weighted portfolio of Diverse firms, Homogenous firms, and the portfolio that goes long on Diverse firms and short on Homogenous firms. We show raw portfolio returns, Fama-French 3-factor portfolio alphas, Fama-French-Carhart 4-factor alphas, and Fama-French 5-factor alphas. To predict returns from July year \( t \) through June year \( t + 1 \) we use values of Diversity as of December year \( t - 1 \). Portfolios are formed using Diversity quartiles. Panel B presents monthly Fama and MacBeth (1973) regressions. Market Capitalization is the log of market-cap in June year \( t \), and Book-to-Market is log book-to-market ratio defined in the appendix. Momentum is defined as the cumulative return from month \( m - 12 \) to month \( m - 2 \). Turnover is the average daily share turnover over calendar year \( t - 1 \). Idiosyncratic volatility is the standard deviation of residuals from a Fama-French-Carhart four-factor model estimated on daily returns over calendar year \( t - 1 \). Return \( t - 1 \) is the one-month lagged return. Columns (1) and (2) weight observations by market capitalization in June year \( t \). Columns (3) and (4) use equal weighting. \( t \)-statistics based on Newey and West (1987) standard errors with 12 monthly lags are shown in parentheses.

<table>
<thead>
<tr>
<th>Panel A: Returns by Diversity Quartiles</th>
<th>Diverse</th>
<th>Homogenous</th>
<th>D–H</th>
<th>Diverse</th>
<th>Homogenous</th>
<th>D–H</th>
</tr>
</thead>
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<tr>
<td>Raw returns (%)</td>
<td>0.90</td>
<td>0.43</td>
<td>0.46</td>
<td>2.65</td>
<td>1.31</td>
<td>3.14</td>
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<tr>
<td>FF 3-factor (%)</td>
<td>0.29</td>
<td>-0.09</td>
<td>0.38</td>
<td>2.77</td>
<td>-1.06</td>
<td>3.06</td>
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<tr>
<td>FF 4-factor (%)</td>
<td>0.32</td>
<td>-0.03</td>
<td>0.34</td>
<td>3.04</td>
<td>-0.41</td>
<td>2.80</td>
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<tr>
<td>FF 5-factor (%)</td>
<td>0.14</td>
<td>-0.11</td>
<td>0.25</td>
<td>1.37</td>
<td>-1.18</td>
<td>1.90</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: Fama-MacBeth Regressions</th>
<th>Value-Weighted</th>
<th></th>
<th>Equal-Weighted</th>
<th></th>
</tr>
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<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Diversity</td>
<td>0.744</td>
<td>0.669</td>
<td>0.203</td>
<td>0.335</td>
</tr>
<tr>
<td></td>
<td>(3.51)</td>
<td>(3.39)</td>
<td>(1.05)</td>
<td>(2.32)</td>
</tr>
<tr>
<td>Market Capitalization</td>
<td>-0.082</td>
<td>-0.221</td>
<td>-0.016</td>
<td>-0.168</td>
</tr>
<tr>
<td></td>
<td>(-1.95)</td>
<td>(-3.41)</td>
<td>(-0.33)</td>
<td>(-3.02)</td>
</tr>
<tr>
<td>Book-to-market</td>
<td>0.102</td>
<td>0.070</td>
<td>0.132</td>
<td>0.049</td>
</tr>
<tr>
<td></td>
<td>(0.72)</td>
<td>(0.63)</td>
<td>(1.15)</td>
<td>(0.54)</td>
</tr>
<tr>
<td>Momentum</td>
<td>-0.092</td>
<td>-0.193</td>
<td>0.031</td>
<td>-0.022</td>
</tr>
<tr>
<td></td>
<td>(-0.16)</td>
<td>(-0.34)</td>
<td>(0.08)</td>
<td>(-0.06)</td>
</tr>
<tr>
<td>Return(_t-1)</td>
<td>-3.462</td>
<td>-2.724</td>
<td>-4.49</td>
<td>-0.434</td>
</tr>
<tr>
<td></td>
<td>(-3.47)</td>
<td>(-4.49)</td>
<td></td>
<td>(-2.30)</td>
</tr>
<tr>
<td>Idiosyncratic Volatility</td>
<td>-0.745</td>
<td>-1.952</td>
<td>-0.002</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-3.30)</td>
<td>(-2.30)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Turnover</td>
<td>0.015</td>
<td>-0.002</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.84)</td>
<td>(-0.38)</td>
<td></td>
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<tr>
<td>Observations</td>
<td>446,013</td>
<td>444,248</td>
<td>446,013</td>
<td>444,248</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>0.09</td>
<td>0.15</td>
<td>0.03</td>
<td>0.07</td>
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</table>
## APPENDIX

### A Additional Variable Descriptions

<table>
<thead>
<tr>
<th>Variable</th>
<th>Description</th>
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</thead>
<tbody>
<tr>
<td><strong>Main independent variable</strong></td>
<td><strong>Diversity</strong></td>
</tr>
<tr>
<td></td>
<td>Degree of similarity among the members of the executives team. This variable is computed applying text–based analysis to executives biographies as reported in firms 10-K and DEF-14A SEC filings. It can take on values in the interval $[0;1]$, with 0 representing the Homogenous firms and 1 Diverse firms.</td>
</tr>
<tr>
<td><strong>Variables in Table 5</strong></td>
<td><strong>Company Overlap</strong></td>
</tr>
<tr>
<td></td>
<td>For every pair of executives in a given team we compute the number of company names that appear in the biographies of both executives, then we take the average over all executives pairs. Source: Executive biographies.</td>
</tr>
<tr>
<td></td>
<td><strong>Tenure St. Dev</strong></td>
</tr>
<tr>
<td></td>
<td>Standard deviation of the number of years each executive on the team has worked for the company in the current role. Source: Boardex supplemented by Execucomp.</td>
</tr>
<tr>
<td></td>
<td><strong>University Overlap</strong></td>
</tr>
<tr>
<td></td>
<td>For every pair of executives in a given team we compute the number of university names that appear in the biographies of both executives, then we take the average over all executives pairs. Source: Executive biographies.</td>
</tr>
<tr>
<td></td>
<td><strong>Elite University St. Dev.</strong></td>
</tr>
<tr>
<td></td>
<td>Standard deviation of an indicator variable that takes value 1 if an executive attended one of the following university, at any academic level: MIT, Stanford University, University of Chicago, Brown University, Columbia University, Cornell University, Dartmouth College, Harvard University, Princeton University, University of Pennsylvania, Yale University, and 0 otherwise. Source: Executive biographies.</td>
</tr>
<tr>
<td></td>
<td><strong>Nationality Mix</strong></td>
</tr>
<tr>
<td></td>
<td>One minus the Herfindahl concentration index for nationality. Source: Boardex.</td>
</tr>
<tr>
<td></td>
<td><strong>Executive Age St. Dev.</strong></td>
</tr>
<tr>
<td></td>
<td>Standard deviation of the age of the executives constituting the top management team. Source: Boardex supplemented by Execucomp.</td>
</tr>
<tr>
<td></td>
<td><strong>Gender St. Dev.</strong></td>
</tr>
<tr>
<td></td>
<td>Within executive team standard deviation of an indicator variable that takes value 1 when the executive is a woman and 0 otherwise. Source: Boardex supplemented by Executive biographies.</td>
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</tbody>
</table>
**Additional Variable Descriptions (Continued)**

<table>
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<th>Variable</th>
<th>Description</th>
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<tr>
<td><strong>Variables in Table 4</strong></td>
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<tr>
<td>Team Size</td>
<td>Natural logarithm of the number of executives constituting the top management team. Source: Executive biographies.</td>
</tr>
<tr>
<td>Bio Length</td>
<td>Natural logarithm of the average number of words in the biographies of each top management team member. Computed after applying the filter described in Section 2.2. Source: Executive biographies.</td>
</tr>
<tr>
<td>OC</td>
<td>Eisfeldt and Papanikolaou (2013) measure of organizational capital.</td>
</tr>
<tr>
<td>Governance Strengths</td>
<td>Index computed by analyzing compensation, reporting quality, political accountability, firm’s involvement in public policy, and ownership. Source: RiskMetrics KLD STATS</td>
</tr>
<tr>
<td>Workforce Diversity</td>
<td>Index computed by combining (i) promotion of minorities and women, (ii) work-life-benefits at the company (iii) whether the firm does significant amounts of business with women or minority owed subcontractors or suppliers, (iv) employment of the disabled, (v) gay and lesbian policies, and (vi) other diversity strengths. Source: RiskMetrics KLD STATS</td>
</tr>
<tr>
<td>10-K File Size</td>
<td>The natural logarithm of the file size in megabytes of the SEC EDGAR complete submission text file for the 10-K filing.</td>
</tr>
<tr>
<td>Uncertain Words</td>
<td>Percentage of words within the 10-K that are classified as uncertain using the Loughran and McDonald (2011) word list.</td>
</tr>
<tr>
<td>Business HHI</td>
<td>The sum of the squared business segment shares reported for the firm in the COMPUSTAT Segment database based on company sales.</td>
</tr>
<tr>
<td>Idiosyncratic Volatility</td>
<td>Standard deviation of residuals from 4-factors model estimated from daily returns over calendar year ( t - 1 ).</td>
</tr>
<tr>
<td><strong>Other variables</strong></td>
<td></td>
</tr>
<tr>
<td>Analysts Following</td>
<td>Natural logarithm of the number of analysts issuing an earning (target price) forecast in the current quarter (month).</td>
</tr>
<tr>
<td>Book-to-market</td>
<td>The natural log of the ratio of the book value of equity to the market value of equity. Book equity is total assets at the end of December year ( t - 1 ), minus total liabilities, plus balance sheet deferred taxes and investment tax credit if available, minus preferred stock liquidating value if available, or redemption value if available, or carrying value. Market equity is price times shares outstanding at the end of December of ( t - 1 ).</td>
</tr>
<tr>
<td>Earnings Persistence</td>
<td>The first-order autocorrelation coefficient of quarterly earnings using 4 years of data.</td>
</tr>
<tr>
<td>Earnings Volatility</td>
<td>The standard deviation during the previous 4 years of the deviations of quarterly earnings from the corresponding 1 year ago earnings.</td>
</tr>
<tr>
<td>Institutional Ownership</td>
<td>Natural logarithm of ( IO/(1 - IO) ). Where ( IO ) is the portion of shares outstanding held by institutional investors in a given quarter.</td>
</tr>
<tr>
<td>Momentum</td>
<td>Cumulated continuously compounded stock return from month ( j - 12 ) to month ( j - 2 ), where ( j ) is the month of the forecasted return.</td>
</tr>
<tr>
<td>Reporting Lag</td>
<td>Number of days between the the end of the current quarter and the earnings announcement date.</td>
</tr>
<tr>
<td>Return(_m-1)</td>
<td>Stock return in month ( m - 1 ).</td>
</tr>
<tr>
<td>Turnover</td>
<td>Average daily share turnover ((\times 100)) over calendar year ( t - 1 ).</td>
</tr>
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</table>
Table B.1: Common Words in Executive Biographies

This table shows the list of the 100 most commonly occurring terms in the main dictionary based on executive biographies in the year 2011.

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<th>Word</th>
<th>Rank</th>
<th>Word</th>
<th>Rank</th>
<th>Word</th>
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<td>bachelor</td>
<td>68</td>
<td>accountant</td>
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</tbody>
</table>
This table shows two sets of executive biographies, one for a team with low diversity score, and one for a team with high diversity score. We highlight common words not eliminated by any of our filters by underlining them.

<table>
<thead>
<tr>
<th>Company</th>
<th>Executive Biography</th>
</tr>
</thead>
<tbody>
<tr>
<td>Anaren, Inc.</td>
<td>Lawrence A. Sala – Lawrence A. Sala joined the Company in 1984. He has served as President since May 1995, as Chief Executive Officer since September 1997, and as Chairman of the Board of Directors since November 2001. Mr. Sala became a member of the Board of Directors of the Company in 1995. He holds a Bachelor’s Degree in Computer Engineering, a Master’s Degree in Electrical Engineering and a Master’s Degree in Business Administration, all from Syracuse University.</td>
</tr>
<tr>
<td>2012</td>
<td>Carl W. Gerst, Jr. – Carl W. Gerst, Jr. has served as Chief Technical Officer and Vice Chairman of the Board since May 1995 and served as Treasurer from May 1992 to November 2001. Mr. Gerst previously served as Executive Vice President of the Company from its founding until May 1995, and has been a member of the Company’s Board of Directors since its founding in 1967. He holds a Bachelor’s Degree from Youngstown University and a Master’s Degree in Business Administration from Syracuse University.</td>
</tr>
<tr>
<td>Anaren, Inc.</td>
<td>Gert R. Thygesen – Gert R. Thygesen joined the Company in 1981 and has served as Senior Vice President, Technology since November 2005 and served as Vice President of Technology from September 2000 until November 2005. He previously served as Vice President, Operations from April 1995 to September 2000, and as Operations Manager from 1992 until 1995. Mr. Thygesen holds a Bachelor of Science Degree and a Master’s Degree in Electrical Engineering from Aalborg University Center, Denmark.</td>
</tr>
<tr>
<td>Collectors Universe, Inc.</td>
<td>Michael J. McConnell – Michael J. McConnell has served as the Company’s Chief Executive Officer since March 2009 and since July 2007 has been a director of the Company. He also serves as a Non-Executive Director of PaperlinX Limited and Redflex Holdings Limited. From 1998 to 2008, Mr. McConnell was a Managing Director and a member of the Executive Committee of Shamrock Capital Advisors, Inc., a manager of private equity, real estate and direct investment funds, including the Shamrock Activist Value Funds. Prior to joining Shamrock in 1994, Mr. McConnell held various management positions at PepsiCo, Merrill Lynch and Kidder Peabody. Mr. McConnell formerly served on the boards of directors of Ansell Limited, Nuplex Industries, Force Corporation, iPass, Inc., Port-link International, MRV Communications, Cosmoline Limited and Neo Technology Ventures. Mr. McConnell received his B.A. in Economics from Harvard University and his MBA from the Darden School of the University of Virginia.</td>
</tr>
</tbody>
</table>

D Quartile = 1  
D Quartile = 1  
D Quartile = 1  
D Quartile = 4
David G. Hall – David G. Hall has served as President of Collectors Universe since October 2001 and as a Director since its founding in February 1999. From April 2000 to September 2001, Mr. Hall served as the Chief Executive Officer of the Company and as Chairman of the Board from February 1999 to October 2001. Mr. Hall was a director of Professional Coin Grading Service, Inc., and was its Chief Executive Officer from 1986 to February 1999, when it was acquired by the Company. Mr. Hall was honored in 1999 by COINage Magazine as Numismatist of the Century, along with 14 other individuals. In 1990, Mr. Hall was named Orange County Entrepreneur of the Year by INC. Magazine. In addition, Mr. Hall has written A Mercenary’s Guide to the Rare Coin Market, a book dedicated to coin collecting. Mr. Hall invented and introduced the concept of and developed the business of independent third party grading of high value collectible coins and sports cards. He is also known in the numismatics community as one of the leading experts in identifying and grading high value collectible coins and he is in demand as a speaker at coin conventions and trade shows.

Joseph J. Wallace – Joseph J. Wallace became the Company’s Chief Financial Officer in September 2005. Prior to becoming Chief Financial Officer, he was the Company’s Vice President of Finance from November 2004 and Controller from June 2004. From 1997 to 2003, Mr. Wallace was Vice President of Finance, Chief Financial Officer and Secretary of STM Wireless, Inc., a publicly traded company engaged in the business of developing, manufacturing and marketing satellite communications products and services. Mr. Wallace is a Fellow of the Institute of Chartered Accountants in Ireland, and a CPA in the State of California.
Table C.3: Diversity and Complexity

This table regresses measures of firm complexity on Diversity and firm-level variables. The dependent variables are the logarithm of 10-K file size, the log of the number of words in the 10-K, the proportion of uncertain words, the proportion of weak modal words, idiosyncratic volatility and the HHI of firm business segments. In each column we include industry $\times$ year effects, based on Fama-French 12 industries. Additional controls are: log market cap, log book-to-market ratio, and continuously compounded stock return from months $t = -12$ to $t = -2$. Standard errors are clustered by firm. $t$-statistics are shown in parentheses. Definitions of all variables are provided in the Appendix.

<table>
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<tr>
<th></th>
<th>10-K File Size</th>
<th>10-K Word Count</th>
<th>Uncertain Words</th>
<th>Weak Modal Words</th>
<th>Volatility</th>
<th>Segment HHI</th>
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<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
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<td>Diversity</td>
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<td>0.049</td>
<td>0.034</td>
<td>-0.002</td>
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<td>(3.57)</td>
<td>(4.53)</td>
<td>(4.56)</td>
<td>(-0.45)</td>
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<td>Additional Controls</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Industry $\times$ Year FE</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>Observations</td>
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<td>17,224</td>
<td>17,224</td>
<td>17,224</td>
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<tr>
<td>Adjusted $R^2$</td>
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<td>0.29</td>
<td>0.34</td>
<td>0.61</td>
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Chapter 3

Does Regulation Distort Asset Prices? Evidence from a Reform of Trust Investment Law

with Stefano Cassella and Oliver Spalt

Abstract

This paper uses changes in the law of trust investment, which were introduced in a staggered fashion across the U.S. between 1986 and 2007, to study the effect of regulation on asset prices. The central element of the reform we exploit is the introduction of the prudent investor rule, which reorients the standards of “prudence” trust investment managers must adhere to in their investments. Effectively, the law change removed barriers for bank trusts to invest into riskier, more speculative, stocks which were considered imprudent, and therefore infeasible investments, under the old regime. The reform therefore induces plausibly exogenous shifts in asset demand from bank trusts, which, during our sample period, represent on average more than 25% of institutional money invested in US public equities. We show that following the reform, bank trusts massively sell prudent stocks, and buy riskier, more speculative, stocks. Consistent with models of price pressure and limited arbitrage, stock returns in the 12 months after the law change move in the direction of aggregate bank trust buying and selling. Our findings provide novel evidence suggesting that uninformed institutional demand can have a large and prolonged impact on stock prices. Our results are also consistent with a variant of the Merton (1987) model in which regulation, prohibiting investors from trading a subset of stocks, distorts asset prices.
1. Introduction

The question of whether stock ownership by institutional investors has any impact on asset prices has received considerable attention in the last three decades (Lakonishok, Shleifer, and Vishny (1992), Warther (1995), Bennett, Sias, and Starks (2003), Coval and Stafford (2007), Frazzini and Lamont (2008), Massa, Schumacher, and Wang (2017)). This is an increasingly important question, as the representative investor in the equity market progressively shifts from being an individual to being an institution (French (2008), Stambaugh (2014)).

To the extent that institutions and individual investors exhibit different preferences over certain stock characteristics (Lakonishok, Shleifer, and Vishny (1992), Gompers and Metrick (2001)), or if sophisticated institutional managers and retail investors hold heterogeneous beliefs or information about the value of a stock (Barberis and Shleifer (2003), Barber and Odean (2007), Cassella and Gulen (2018)), increasing institutional footprint in the equity market may induce incremental demand for certain stocks and stock characteristics. This incremental demand may lead stock prices to differ across institutional ownership regimes.

In this paper, we ask whether uninformed institutional demand, broadly defined as institutions’ positive or negative excess demand for certain stocks which is not primarily driven by information or by the fundamental value of these securities, can affect the pricing of assets in the cross-section of stocks, and whether such demand has a stabilizing or destabilizing role for asset prices (Lakonishok, Shleifer, and Vishny (1992)).

From an ex-ante standpoint, these questions do not have a clear answer. The traditional finance view is that stocks have perfect substitutes, and therefore riskless arbitrage opportunities guarantee that any investor’s uninformed demand never leads to a departure of a stock’s price from its underlying value. In other words, demand curves for stocks are flat (Fama (1970), Scholes (1972)). An alternative strand of literature challenges this view, on the ground that no stock has close enough substitutes to allow for truly riskless arbitrage (Shleifer (1986), Kaul, Mehrotra, and Morck (2000), Wurgler and Zhuravskaya (2002)), and that such limits to arbitrage may be exacerbated if arbitrageurs have short horizons and are uncertain on how long will it take
for their trading strategies to result into profits (De Long, Shleifer, Summers, and Waldmann (1990), Shleifer and Vishny (1997)).

Establishing empirically if and how uninformed institutional preferences affect stock returns is therefore important. But it is also very challenging. First, institutional preferences cannot be observed directly, and the researcher is left to infer them through the analysis of institutions’ portfolio holdings (e.g., Falkenstein (1996)). Unfortunately, inferring institutional preferences from holdings is subject to the critique that institutions’ holdings are determined as an inextricable combination of uninformed preferences and beliefs-based demand. Second, studying the joint dynamics of institutional trading and stock returns, an approach used by prior literature to offer evidence of institutions’ influence on asset prices, is subject to a similar critique. Simply put, we do not know whether the observed relation is due to beliefs and information, or uninformed preferences.¹

An ideal experiment to establish whether uninformed institutional preferences have any causal impact on prices, and whether they push stock prices away from or towards their fundamental value, would use random variation in institutional preferences that is unrelated to the fundamental value of a security. In this paper, we argue that recent regulatory changes in trust investment law offer a setting which resembles the optimal experiment in many key aspects. Thus, this setting allows us to provide novel evidence on the impact of uniformed institutional demand on asset prices.

Following the literature, we hypothesize that regulation is one of the key underlying determinants of institutional preferences (O’Barr, Conley, and Brancato (1992), Del Guercio (1996)). In this respect, we focus on the fiduciary duty standards that apply to institutional money managers in the US. A major revision of such standards, implemented in a staggered fashion across states between 1986 and 2007, introduces a new definition of prudent delegated asset management (e.g. Schanzenbach and Sitkoff (2007)). In the shift from the old to the new definition of prudence,

¹The tension existing in the literature between the two interpretations may give a sense of how difficult it is to separate beliefs- and information-based stories from preference-based stories. For example, Gompers and Metrick (2001) propose that the connection between institutional ownership and stock returns can be ascribed to uninformed institutional demand shocks, whereas Sias, Starks, and Titman (2006) suggest institutional trading stems from information, and facilitates the incorporation of such information into prices.
some stocks lose their prior connotation of prudent (imprudent), and hence their inherent property of sheltering (exposing) some money managers from (to) the risk of lawsuit for breach of fiduciary duty. Crucially, the loss of a stock’s prudence status due to the law change is plausibly unrelated to any stock-specific information or shock to beliefs.

Our tests present evidence of a causal link between regulation and institutional preferences, as money managers affected by the change in regulation rebalance their portfolios away from stocks that under the new law have lost their prudence advantage (e.g., low-volatility, old, dividend paying stocks) and towards stocks that have lost their imprudence penalty (e.g., high-volatility, young, non-dividend paying stocks). Furthermore, our findings indicate that as the transition from the old to the new law takes place, and the impacted institutions rebalance their portfolios accordingly, stocks previously classified as prudent (imprudent), experience lower (higher) returns in the subsequent 12 months. Importantly, there is very weak evidence that such returns revert on a longer horizon. We interpret this as evidence that law-driven institutional preferences can have a destabilizing role for asset prices: stocks able to potentially protect (harm) institutions from (through) lawsuits under the old fiduciary standard, may have traded at a premium (discount) in that period. As the new fiduciary rule is introduced, stocks lose their attribute of prudent or imprudent, and the initial deviation of stock prices from the value that would have been observed in absence of regulatory constraints is corrected. We rationalize our findings as an instance of the model of financial markets proposed by Merton (1987).

Our empirical design relies on a shift in the fiduciary standard that regulates bank trusts’ duty of prudence. In our sample period 1983-2010, bank trusts represent about 25% of institutional money invested in US public equities, and about 16% of stock market value. We exploit the staggered migration of state-level legislation in matters of trusts’ fiduciary duties, from an old and obsolete legal standard, named the Prudent Man law (Scott (1959)), to the new Uniform Prudent Investor Act (henceforth UPIA), as a source of variation in law-driven institutional preferences.

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2We calculate the fraction of institutional money that is under the control of bank trusts from 13(f) filings with the SEC. We complement this data with data from French (2008), where we calculate US institutions’ share of stock market value as 100% minus the combined share held by individual investors directly, and by foreign investors.
In the first part of our analysis, we provide evidence of a causal link between the rule of law and institutional preferences. We implement a difference-in-differences estimation strategy, which is vastly used in the corporate finance literature (e.g., Bertrand and Mullainathan (2003), Gormley and Matsa (2016)), but less so until now in the asset pricing and investments literature. Our identification strategy is not only based on multiple treated and controls (Atanasov and Black (2015)), but also capitalizes on a unique feature of our institutional setting: the change in fiduciary duty standards that we study does not constitute a regulatory change for any institutional investor other than trusts. Therefore, non-trusts institutions constitute an ideal within-state control group, as they are contemporaneously affected by the same political economy or business cycle factors as bank trusts, but they are not affected by the introduction of UPIA statutes.\footnote{For instance, under the investment company act (1940), mutual funds are subject to fiduciary duties, but such duties are less concerned with prudent investing, and more with managerial “compensation for services”. Similarly, pension funds, who where originally subject to the prudent man rule, abandoned such discipline with the Employee Retirement Income Security Act (ERISA) or 1974.}

Our coefficients of interest are estimated out of variation coming from the different evolution of portfolio holdings between trusts and the control group, after controlling for fixed time invariant differences between institutional investors, as well as time-varying state-level heterogeneity. Our tests suggest that, across a set of prudence-related characteristics (CAPM $\beta$, dividend yield, firm age, profitability, S&P membership, and return volatility), which we borrow from prior literature (Del Guercio (1996), Bennett, Sias, and Starks (2003)), trusts and non-trusts’ allocation to prudent and imprudent stocks is very different prior to the enactment of the UPIA. The combined trusts’ overallocation to stocks deemed prudent before the law change, and underallocation to stocks considered imprudent, is 6% on average across prudence-related characteristics. After the introduction of the UPIA reform, this deviation in the holdings of trusts and non-trusts is largely resolved, as it drops towards zero in the 12 quarters after the enactment of the law.

In the second part of the paper, we ask whether the rebalancing in stock holdings induced by the prudence regulatory change has any bearing on asset prices. In this respect, we borrow our intuition from the Merton (1987) model of financial markets with information frictions. In the model, some investors are prevented from investing in certain stocks. While Merton (1987)
frames such investor base constraint as due to different information sets across investors, he himself considers prudent-man laws as another potential mechanism which would lead to an observationally similar setting\(^4\). The application of the Merton (1987) model to our setting suggests that under the prudent-man law, ceteris paribus, stocks deemed imprudent are undervalued compared to the case in which there is no investor base constraint. Therefore, when the UPIA reform is introduced, and the investor base constraint loosens up for these imprudent stocks, one prediction of the Merton (1987) model is that these stocks will experience higher returns than prudent ones. Furthermore, after the full removal of the constraint, stock returns will not differ anymore in their expected returns across prudence levels, and any remaining expected return differential should be accounted for by differing exposure to systematic sources of risk. This is precisely what we find. In a conditional-characteristic regression, which extends the unconditional model of Daniel and Titman (1997) as proposed in Baker and Wurgler (2006), we find that between 1983 and 2010, the relation between prudence-related characteristics, and future returns, is conditional on the enactment of the UPIA. Taking volatility as the representative prudence characteristic, high-volatility (imprudent) stocks in our sample underperform all other stocks by about 0.7 percentage points a month, after controlling for these stocks’ differential risk exposure\(^5\). The picture looks entirely different in the year following a large UPIA reform\(^6\). For instance, our estimates suggest that following a UPIA reform of the same size observed when California adopts the new fiduciary standard, the return of high-volatility stocks (which trust funds in the state now intend to buy) is 29bps higher. This is consistent with our intuition, as well as with the disappearance of the investor base constraint imposed on high-volatility stocks by the old prudent-man rule. These findings survive an extensive battery of robustness tests,

\(^4\)Merton (1987) writes “Although not directly an incomplete-information issue, the existence of prudent-investing laws and traditions as well as other regulatory constraints, can also rule out investment in the firm by some investors. The effect of these constraints on investor behavior is captured in our model because these investors act as if they did not know about the firm.”

\(^5\)Like in Fama and French (2008), we control for risk by including size and book to market in our cross-sectional regressions.

\(^6\)At any point in time, we quantify the “size” of the UPIA reform events, by constructing a time-series which reflects how much institutional capital is affected by the reform in the previous year. We call this the “preference-shocked institutional capital”, and provide further detail on its construction later in the paper.
aimed at assessing the soundness of our evidence against a large set of alternative hypotheses. Finally, at longer horizons, we do not find statistical evidence of any reversal in returns, thus providing further support to our conjecture that pricing distortions took place under prudent-man, and are resolved with the enactment of the new regulation.

This paper relates with, and contributes to, several strands of literature. First and foremost, our work is related to prior research on whether institutional presence and activity in the equity market affect stock returns. In this area, the papers that are closer to ours are Gompers and Metrick (GM 2001), and Coval and Stafford (CS 2007). We share with GM the conjecture that institutions feature uninformed preferences which can alter the relative pricing of stocks. They provide evidence consistent with a correlation between institutional preferences, as revealed by their holdings and trading activity, and cross-sectional stock returns. Furthermore, in the setting studied by GM, cross-sectional stock returns are generated by a combination of static institutional preferences, and time-varying institutions’ share of the equity market. Simply put, the fact that the representative institution exhibits unconditional preference for certain stocks (e.g., large stocks), together with the fact that over time institutions increase their presence in the equity market, leads to inflated prices of institutions’ most favored stocks. We differ from GM in two main ways. First, in our paper we pinpoint one underlying driver of institutional preferences, namely regulation. Since regulation changes across institutions and over time, our setting entertains the notion of both cross-sectional variation in preferences across different institutions, as well as time-series variation in institutional preferences due to regulatory reforms. Second, GM relies on the time-series steady secular increase in the institutional share of the equity market, as a source of variation to the uninformed preferences of the representative investor. Our approach sharpens the econometric inference, and strengthens the evidence of a causal link between institutional preferences and asset prices, as the UPIA reform provides a unique setting to identify

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7 In order for an alternative story to account for our findings, one needs to entertain time-series variation in the relation between prudence characteristics and future returns, which correlates but is unrelated to the UPIA. We check for the likelihood of such alternative story by controlling for unobservable time-varying factor risk premia, or for competing observable aggregate time-series effects, possibly related to macroeconomic shocks, or investor sentiment. We also address the possibility this is not a true prudence-story, in that the characteristics we use to proxy for prudence, may proxy for non-prudence related characteristics that exhibit time-varying compensation for risk. Our results are robust to these, as well as other, alternative stories.
the timing and size of uninformed demand shocks, and the ensuing change in stock returns that such shocks entail. CS study the relation between a stock’s sale by mutual funds who experience financial distress (i.e., capital outflows), and future stock returns. They find that when a stock is subject to sales that are arguably driven by funds’ financial distress, rather than stock-specific news of fundamental value, its subsequent return exhibits a U-shape: first, there is a price decline and negative returns; then, one observes a rebound in price and positive returns. CS interpret this as evidence of a price-pressure story: an initial price decline that allows a liquidity provider to expect a positive return from taking the buying side of the transaction, and a later price rebound that enables such liquidity provider to realize the positive profit from the trade. Like CS, we are interested in disentangling the effect of information and beliefs-based institutional trading on prices, from the effect played by uninformed demand shocks. Like CS, we empirically focus on events that create coordinated actions across a large number of institutions. Our differences from CS are both conceptual and empirical. Conceptually, we focus on the effect of shocks to institutional preferences for certain stock characteristics, on the pricing of all stocks that exhibit such characteristics. In our setting, a negative shock to institutional preferences towards a stock characteristic, possibly due to regulatory changes, leads to a decline in prices for all stocks exhibiting that characteristic. When the object of a fire-sale is a specific stock, rather than a stock characteristic, it is not clear that this represents a shock to institutional preferences akin to the one we focus on in this study. Empirically, CS find evidence of a price-pressure story, in that there is price reversal after a fire-sale takes place. Our results lack evidence of such reversal after the enactment of the UPIA. We provide support for the intuition that regulation shapes asset ownership and asset prices (Merton (1987)), and permanent regulatory shifts that alter institutions’ preferences towards certain stock characteristics, can cause changes in the relative pricing of such characteristics.

Our paper also joins a growing literature on the role of regulation and financial intermediation in financial markets (Del Guercio (1996), Chen, Yao, and Yu (2007), Hankins, Flannery, and Ni-

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8Lakonishok, Shleifer, and Vishny 1992 note that coordination across various institutional investors is likely a prerequisite to find any effect of such institutions’ actions on equilibrium asset prices.

9See Coval and Stafford (2007), Figure 2.
malendran (2008), Ambrose, Cai, and Helwege (2011), Ellul, Jotikasthira, and Lundblad (2011), Cao, Han, and Wang (2017), Da, Larrain, Sialm, and Tessada (2018)). Our findings represent the first formal test of Del Guercio (1996), who argues for a causal link between regulatory constraints and institutions’ portfolio choice. Our main contribution is showing that such regulatory constraints actually matter for the pricing of equities. Our work relates to Chen, Yao, and Yu (2007), who study the effect of regulatory pressure on fire-sales in the bond market. In the spirit of Del Guercio (1996), and similarly to what we argue, they propose that regulation can be more or less binding for certain insurance companies than for others. Their findings of price-pressure effects, as well as their focus on the bond market, which are shared with Ellul, Jotikasthira, and Lundblad (2011), differ from our finding of persistent changes in pricing, and from our focus on stocks. Hankins, Flannery, and Nimalendran (2008) share with this paper the question of whether the UPIA reform changes trust funds’ portfolio allocations. We differ in three main ways. They only focus on one prudence-related characteristic (dividend-yield), whereas we search the literature for references to characteristics associated with prudence, and end up testing our theory with 6 distinct such characteristics. Unlike them, we adopt traditional difference-in-differences estimation strategies which allow to conclusively argue for a causal link between regulation and portfolio choice (Roberts and Whited (2013), Atanasov and Black (2015)). Finally, they fall short of any test on the linkage between preference shocks, and asset prices. Cao, Han, and Wang (2017) devise trading strategies that can be used to profit from regulation-induced under-reaction to news. Their focus is ERISA, the regulatory reform which took place in the 1970’s and freed pension funds from the prudent-man rule, long before our sample and our data starts, and before the enactment of the UPIA. In their setting, preferences are fixed, but they differ across institutions. Our setting is wider, as we not only observe heterogeneity in preferences that stems from heterogeneity in regulation, but also concentrate on changes in such preferences, and inherently in institutional investors’ heterogeneity. Finally, like Da, Larrain, Sialm, and Tessada (2018), we argue that financial intermediation, and the design of financial institutions, can be destabilizing for asset prices. Their focus is on how regulation and institutions can create
retail investors’ induced trading and price-pressure. Our analysis shows that regulation to which financial intermediaries are subject, can lead asset prices to differ from their fundamental value.

The rest of the paper is structured as follows. Section 2 provides details on the institutional background; Section 3 describes data sources and variable construction; Section 4 tests for the existence of a causal link between regulation and institutions’ portfolio choice. Section 5 provides evidence of regulation and preference-induced predictable patterns in the cross-section of stock returns. Section 6 provides final remarks and concludes.

2. Institutional Background

2.1 Trust Funds

In this section we offer institutional background information on trust funds and trust law, most of which we source from the work of prominent legal scholars (e.g., Sitkoff (2003) and Schanzenbach and Sitkoff (2007)). A trust is an investment fund that an individual, henceforth the settlor, sets up with the intention to support one or more fund beneficiaries. The settlor selects a manager for the fund, henceforth the trustee, who is to oversee and manage the fund wealth, as well as the disbursement of such wealth to the beneficiaries. In earlier history, the trustee would be in charge of facilitating the conveyance of land and its value from the settlor to the beneficiaries. Over time, trust funds have turned into investment vehicles whose holdings include financial assets such as stocks and bonds, and trustees have exercised increasing discretion over fund holdings, as well as fund disbursements.

Since the beneficiaries constitute the residual claimants to the wealth of the fund, and the trustee manages the fund operations, the natural setting to analyze the trustee-beneficiaries dynamics is the well known setting of separation of ownership (by the beneficiaries) from control (by the trustee). Prior literature uses such framework to understand the interaction between corporations’ shareholders and management (Jensen and Meckling (1976)). On the other hand, while corporate executives are kept in check by the threat of being fired, the monitoring by
corporate boards, and the existence of an active market for corporate control (Jensen and Ruback 
(1983), Barclay and Holderness (1989)), these governance mechanisms are absent in trust funds. 
In particular, beneficiaries rarely manage to remove trustees, as the law perceives such removal 
as a potential breach of the settlor’s original will. It is perhaps due to the difficulty of removing 
a sitting trustee that a market for the control of trusts has not developed. Finally, only recently 
monitoring entities such as “trust-protectors” have emerged in trust law with the purpose of 
overseeing trustees’ behavior. In such a complex and highly constrained legal setting, trustees’ 
fiduciary duties emerge as a law-based mechanism to align the incentives of trust managers with 
the goals of the trust beneficiaries.

2.2 Trusts’ Fiduciary Duty of Prudence

Trust law subjects a trustee to the primary fiduciary duties of loyalty and care to the trust 
beneficiaries. Should the trustee be in breach of such fiduciary duties, she would face the risk 
of in-court litigation in the state where she operates, and the risk of personal liability if found 
guilty.

One key element of a trustee’s fiduciary role is the duty of prudence. Until the 1990s, the 
generally accepted standard of prudence in trust investments was the so-called prudent-man 
rule. This rule descended from the 1830 court decision in the seminal case of Harvard College 
v. Amory, and inspired the Restatement Second of Trusts (Scott 1959), which is considered 
the most influential text in trust law (Longstreth (1986), Gordon (1987)). In particular, the 
restatement encouraged courts to assess the prudence of each trustee’s investment based on 
the individual characteristics of such investment (Badrinath, Gay and Kale 1989). Breach of 
prudence would result in personal liability for the trustee (Halbach 1961). This, in turn, would 
dissuade trust managers from investing in stocks which exhibited characteristics that were deemed 
“imprudent”, and thus in breach of fiduciary duties, by the courts (e.g, young, unprofitable, 
non-dividend paying stocks, with high return volatility). Between 1959, when the Restatement 
was published, and the end of the 1980s, the principles governing trust fiduciary law remained
virtually unchanged. In the same period, by comparison, other institutions faced less stringent fiduciary duties (Bushee 2001). For instance, the fiduciary duty of mutual fund managers and investment advisors, which were established via the Investment Company Act (1940) and the Investment Advisers Act (1940), did not share the same prudent-man legal doctrine as trust law, and there is no evidence that such money managers faced legal constraints related to the breach of prudence (Del Guercio (1996)). Similarly, insurance companies faced little prudence-related constraints. As for pension funds, these had initially shared the prudent-man discipline with trust investments, but the advent of the Employee Retirement Income Security Act (ERISA, 1974) replaced the prudent-man rule with a fiduciary standard which promoted portfolio diversification in the spirit of modern portfolio theory (Markowitz (1952)). This legislative intervention, thus, progressively reduced the role of the prudent-man default rule for pension fund investments. Between the end of the 1980s and the first half of the 1990s, a legislative stimulus paved the way for a reform of the prudent-man rule. In 1992, the American Law Institute drafted the Third Restatement of the Law of Trusts, a legal recommendation which led to the Uniform Prudent Investor Act (UPIA), a trust law reform which nowadays every U.S. state adheres to. According to the new rule, courts evaluate assets “in the context of the trust portfolio as a whole, and as a part of an overall investment strategy having risk and return objectives reasonably suited to the trust” (UPIA, sec. 2).

3. Data and Variable Construction

3.1 Institutional Investors’ Data

Institutional equity holdings and trading data for each quarter between January 1983 (three years before the first law change) and December 2010 (three years after the last law change) are obtained from the Thomson Reuters Institutional Holdings (13F) Database. All institutional investors with more than $100 million in equity ownership must report their holdings to the SEC in the 13F filings. The reporting requirements mandated by the SEC encompass various
types of institutional managers, such as banks, investment companies, pension funds, insurance companies, and brokerage houses. In order to identify the manager’s institutional type we employ the type code variable provided by Thomson Reuters, which classifies institutions in: bank trust departments; insurance companies; independent investment advisers; mutual funds; and unclassified institutional investors\textsuperscript{10}. Throughout the paper we refer to bank trust departments as *bank trusts* or simply *trusts*, and to the remaining institutions as *all other institutions*.

Table 2 reports summary statistics separately for the sample of trusts and for the sample of all other institutions. Similarly to what documented in previous studies (e.g. Gompers and Metrick (2001)), the median share of institutional equity holdings managed by trust funds in our sample period is around 25%, and it ranges from $70.4$ Billions to $269$ Billions in to 2010, both expressed in 2015-dollars.

Following the law literature (Schanzenbach and Sitkoff (2007), Schanzenbach and Sitkoff (2017)), we consider a trust’s state of residence as the relevant jurisdiction in matters of breach of fiduciary duty. We obtain information about the institutional investors’ historical headquarters location from the SEC Analytic Suite database, which gathers such information from the SEC Edgar website. For banks, we complement this information using bank-level data provided by the Federal Deposit Insurance Corporation (FDIC), and available online. Since there is no common identifier between the two databases, we merge information about headquarters location with the Thomson Reuters 13F Database using fund name. After performing this step, we are able to retain 77% of the institutional investors present in the Thomson Reuters 13F Database, accounting for 76% of the total dollar value invested by institutions.

### 3.2 Firm-Level Data

Stock returns and other firm-level characteristics are obtained from the CRSP/Compustat merged database. In tests with stock returns, we include all stocks with share code 10-11, traded in NYSE, AMEX and Nasdaq. Following the literature (Del Guercio (1996), Bennett, Sias, and

\textsuperscript{10}Since the type code variable in the Thomson Reuters database is not reliable after 1998, we use the extended classification available in B. J. Bushee website.
Starks (2003), Gompers and Metrick (2001)), our analysis focuses on a set of six firm-level characteristics: capm beta, firm age, dividend yield, profitability, stock return volatility, and S&P Index membership\textsuperscript{11}. The exact variable definition is provided in the Appendix. In a given year, we define a stock as prudent if it has low beta, high age, high dividends, high profitability, low return volatility, or if it is part of the index. We define a stock as imprudent if it has high beta, low age, low dividends, low profitability, its returns are highly volatile, or the stock is not part of the index. We also include in the analysis the first principal component of the six prudence-related variables, $PC$, which is intended to capture common variation across the individual, possibly noisy, prudence proxies. $PC$, which explains 37\% of the common variation in the individual prudence-related characteristics, is positively correlated to firm age, dividend yield, profitability, and S&P Index membership, and negatively correlated to capm beta and and stock return volatility. So, low values of $PC$ identify imprudent stocks, and high values identify prudent stocks.

### 3.3 Overallocation

The starting point of our analysis is the conjecture that the prudent-man rule may have created distortions in the portfolio choice of trust fund managers. In particular, we hypothesize that the prudent-man standard may have induced trust funds to allocate a larger (smaller) fraction of their wealth to prudent (imprudent) stocks, than the fraction that one would have observed in absence of the law. An empirical difficulty in assessing and quantifying this distortion is that we do not observe bank trusts’ optimal allocation in absence of the prudent-man rule. While later we perform formal regression analysis using a diff-in-diff empirical strategy, here we seek instead introductory evidence of the allocation distortions that we suspect exist. We do this by analyzing trust fund behavior under the prudent-man standard, and by exploiting one of the unique features of our empirical setting: as the prudent-man rule is a fiduciary standard that

\textsuperscript{11}Del Guercio (1996) adopts S&P rating as a measure of prudence, but due to data limitation we cannot extend it to our sample of interest. We replace it with the stock index membership status, as Del Guercio (1996) proposes this as a valid approximation.
only applies to trust fund managers, but not to all other institutions (e.g., mutual funds), prior to the enactment of the UPIA evidence of distortion in bank trusts’ allocation may be found in the difference between trusts’ holdings and the holdings of other institutions. So, we measure regulation-induced distortions in trust funds’ preferences by means of the following \textit{Overallocation} variable:

\begin{equation}
\text{Overallocation}_{i,s,t} = W_{i,s,t}^T - W_{i,s,t}^{NT}
\end{equation}

where $W_{i,s,t}^T$ is the weight of stock $i$ in the portfolio of trusts in state $s$, at time $t$, and $W_{i,s,t}^{NT}$ is the weight of stock $i$ in the portfolio of all the other institutional investors in state $s$, at time $t$. This measure reflects the different portfolio choices of bank trusts in a state, relative to the portfolio choices of all other institutions headquartered in the same state. Positive values of the overallocation variable for a given stock or stock characteristic, indicate that trust funds tilt their portfolio towards that stock more than other institutions. On the other hand, negative values of overallocation for a stock or stock characteristic, indicate that trust funds shy away from that stock compared to non-trust institutions. Overallocation is calculated for trust funds in each state three years prior to the adoption of the UPIA, and for groups of stocks that are formed based on the six prudence attributes mentioned above. In particular, we sort stocks into a “prudent” and a “imprudent” portfolios, based on terciles of the underlying characteristics. Table 3 shows time-series cross-sectional average \textit{Overallocation} for the prudent and imprudent groups, as well as the difference in average \textit{Overallocation} between the two groups (\textit{P-NP}). Such difference is meant to capture the overall “prudence-tilt” of trust funds compared to non-trust institutions, which stems from the simultaneous overallocation to prudent stocks, and underallocation to imprudent ones. The table presents evidence consistent with our conjecture above: under the prudent-man rule, trust fund managers overallocate wealth to prudent stocks (e.g., low \textit{CAPM} $\beta$), and underallocate wealth to imprudent ones, compared to unconstrained non-trust institutional money managers. The extent of these differences in allocation is large: across prudence characteristics, the median overallocation to prudent stocks is 2.4%, and the median underallocation is about 2.2%. Furthermore, across all of our six prudence-related stock
characteristics, as well as their principal component, there is consistent evidence of overallocation to prudent stocks, and underallocation to imprudent ones. Finally, the overall prudence tilt, which ranges from 3.1% to 10.4%, is always statistically different from zero, and for 4 of the 7 prudence characteristics, such tilt is statistically different from zero at the 1% level\textsuperscript{12}. While this preliminary evidence is suggestive of the existence of allocation distortions that are due to the prudent-man rule, it must be interpreted with caution. For instance, it may be the case that the managers of trust and non-trust funds are fundamentally different. The settlor’s choice of opening a trust, as opposed to other delegated asset management, may reflect the settlor’s preference for prudence. As a consequence, her discretion over the choice of a trustee, may lead to the appointment of trustees who are inherently more prudent than asset managers employed by other institutional investors. To rule out this and other possible explanations, we now turn to our full diff-in-diff analysis.

4. Changes in Bank Trusts’ Allocation after UPIA

4.1 Portfolio Holdings

In this section, our goal is to study more formally the impact of the enactment of UPIA statutes on the portfolio holdings of bank trusts. Our difference-in-difference (DiD) analysis aims at comparing trust funds’ behavior before and after the law change. The simple DiD design uses trust funds located in states where the law has not passed yet as a counterfactual control group for the treated trust funds, which are located in a state where UPIA has been adopted. Although simple, this design may not be ideal in our settings. The main challenge is that there are other state-level time-varying factors that might change at the same time as the introduction of UPIA statutes and affect the holdings of bank trusts. Examples of such factors are other contemporaneous legislative events, or state-level political and economic cycles. Ignoring these factors might result in biased inference, and the observed coefficients would not reflect the causal

\textsuperscript{12}The exceptions are Profitability, where the statistical significance of the prudence tilt drops to 5%, and S&P Index, where the statistical significance of the prudence tilt drops to 10%.

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impact of the UPIA enactment. To address this challenge, we estimate a triple difference-in-differences model of institutional holdings. We start from an institutional-level panel dataset, and we use, as dependent variable, the fraction of an institution’s portfolio invested in firms belonging to the prudent and imprudent portfolios. The existence of institutional investors who are unaffected by the prudent-investor rule, allows us to introduce a control group composed of all institutions that are not bank trusts. By comparing bank trusts’ holdings with the holdings of institutions that are subject to the same state-level factors with the exception of the change in fiduciary standards, we go a long way in making sure that the observed coefficients reflect the impact of the enactment of UPIA statutes. Specifically, in every quarter \( q \), we sort stocks based on one of the aforementioned prudence variables. For each sort, we use the 30th and 70th percentile to calculate the fraction of holdings allocated in that quarter to prudent and imprudent stocks, by institutional investor \( j \), headquartered in state \( s \). We call this variable \( \text{Portfolio Share}_{j,s,q} \), and argue that changes in this variable may reveal changes in an institution’s preference towards prudent or imprudent stocks.

We run the following diff-in-diff specification:

\[
\text{Portfolio Share}_{j,s,q} = \alpha_j + \alpha_{sq} + \beta \text{UPIA}_{s,q} \times \text{Trust}_j + \epsilon_{j,s,q}
\]  

(3.2)

\( \alpha_j \) are institutional investor fixed effects, which control for permanent differences in portfolio choices between trusts and all other institutions, arising, for example, due to unobserved managerial traits. \( \alpha_{sq} \) are state × date fixed effects, which effectively enable the comparison between the treatment group - trusts - and the control group - all other institutions - within the same state at the same point in time. The inclusion of these higher-order fixed effects directly addresses issues concerning differences in business cycles or political economy across states.

\( \text{UPIA}_{s,q} \) is an indicator variable that takes value 1 if state \( s \) has adopted the prudent-investor rule in quarter \( q \). \( \text{Trust}_j \) is a indicator variable for bank trusts. The two variables \( \text{UPIA}_{s,q} \) and \( \text{Trust}_j \) do not enter the equation in isolation, because they are absorbed by the state × date and institutional investor fixed effects, respectively. The coefficient of interest in our analysis is \( \beta \),
which captures the changes in trust fund managers’ revealed preferences towards prudent (im-
prudent) stocks when UPIA is introduced in their state, and after controlling for the concurrent
changes observed in the control group that is unaffected by the law, but otherwise exposed to
the same state-level time-varying phenomena.

Table 4 reports the results of estimating equation 3.2 for the six firm-level characteristics that
are associated with prudence-related preferences, and their first principal component. Results in
Table 4 show that bank trusts sell their holdings of low risk, old, high dividends, high profitability
firms, and firms that are members of S&P Index, which we label “prudent” stocks. At the same
time, trust managers purchase stocks of young firms, firms with high risk, low dividends, low
profitability, and firms that are not members of S&P Index, which we label “imprudent” stocks.
The point estimates also indicate that the rebalancing is economically meaningful. For example,
bank trusts reduce their holding of firms that are members of the S&P Index by 6.7%. Similarly,
bank trusts reduce their holdings of low Volatility stocks by 3.2%, while increase their holdings
of high Volatility stocks by 1.4%. Importantly, we find evidence of trust funds rebalancing out
of prudent stocks and towards less prudent ones, for each of the 6 variables that the literature
suggests as proxy for lawful prudence. In the last column of Table 4, we provide a more formal
test of this statement. We perform a joint test on all the interaction terms $UPIA \times Trust$ for
prudent and imprudent portfolios. The p-value of this test, which we report between brackets,
can be interpreted as the probability of observing the coefficients in Table 4 under the null
hypothesis that the enactment of UPIA statutes has no effect on bank trusts’ portfolios. The
results show that we can reject the null hypothesis that the coefficients $\beta$ on the interaction
terms are jointly zero at the 1% level.

Figure 1 presents analogous results in graphical form. This set of graphs plots point estimates
on the interaction terms $UPIA \times Trust$ from a modified version of equation 3.2, where we allow
the effect of $UPIA$ to change every semester, from semester $-6$ to semester $+12$. Each data point
in the graph effectively represents trust funds’ overallocation to high and low-prudence stocks
around the UPIA introduction, starting from the difference in allocation between trusts and all

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other institutions three years before the adoption of the prudent-investor rule (semester −6).
This evidence not only confirms the results presented in Table 4, but provides further support
for our empirical design, which rests on the key assumption of absence of a pre-trend before the
enactment of UPIA statutes (e.g. Roberts and Whited (2013)). A potential concern here is that
the staggered adoption of the prudent-investor rule occurs in states where trusts are investing in
a different fashion than trusts in other states. In this case, the observed effect on their allocation
would not stem from the causal impact of UPIA introduction, but from a pre-existing behavior
of trusts in these states. However, under this alternative explanation, we would expect to observe
an effect of the event even before the event itself takes place. Figure 1 suggests that this is not
the case.

Overall, results in this section indicate that the introduction of UPIA statutes leads to sig-
nificant changes in bank trusts’ portfolio composition. More importantly, the direction of these
changes is consistent with the disappearance of prudence-related distortions in trust funds’ port-
folio choice.

4.2 Changes in Overallocation

One concern with our analysis so far is that, while we focus on the changes in bank trusts’
revealed preferences for prudent stocks, and obtain the prudence-related characteristics from
the literature, such characteristics may correlate with other non-prudence related ones. For
instance, if we document trust funds’ declining ownership of old stocks and increasing ownership
of young ones, it is possible that such rebalancing activity is not only capturing adjustments
in bank trusts’ portfolio exposure to firm age, but to some other characteristic such as firm
size. While size is mentioned by prior literature as a characteristic over which institutions as
a whole have a preference, it is not explicitly linked to prudence. If age proxies for size, and
size is not a prudence-related variable, the rebalancing activity by trust funds that we aim at
linking causally to their changed fiduciary duties, may be due to some unobserved economic
force other than the fiduciary reform. To reduce this concern, a different approach to study
the impact of the enactment of UPIA statutes on bank trusts’ holdings is to analyze changes in
trusts’ Overalllocation. As described in section 3.3, this variable captures the different portfolio
allocation that trust and non-trust institutional managers select for a given stock. Therefore,
Overalllocation preserves the comparison between bank trusts and the control group in the same
state at the same time, which we advocated in section 4.1. At the same time, an economic model
of overallocation takes the analysis from the institution-level to the individual stock level, thus
allowing to control for other firm characteristics which can correlate, and yet be distinct from,
prudence. We operationalize this approach by estimating the following model:

\[
\text{Overalllocation}_{i,s,q} = \alpha_i + \alpha_q + \beta_1 \text{UPIA}_{s,q} + \beta_2 \text{CharGroup}_{i,q} + \beta_3 \text{UPIA}_{s,q} \times \text{CharGroup}_{i,q} + \gamma X_{i,q} + \epsilon_{i,s,q}
\]

(3.3)

where Overalllocation is defined as in section 3.3; \( \alpha_i \) and \( \alpha_q \) are firm and date (year-quarter) fixed
effects, respectively; \( \text{UPIA}_{s,q} \) is an indicator variable that takes value 1 if state \( s \) has adopted
the prudent-investor rule in year-quarter \( q \); \( \text{CharGroup}_{i,q} \) is an indicator that takes value 1
for firms that belong to either the prudent or imprudent group based on each of the prudence-
related characteristics we consider; \( X_{i,q} \) is a set of firm-level characteristics, including Size, BTM,
Momentum, Return_{t-1}, S&P Index, and Turnover.

Table 5 shows that, after controlling for a host of firm-level characteristics, results remain
overall consistent with those in the previous section: bank trusts’ managers significantly reduce
their overallocation to prudent stocks (e.g., low-volatility stocks), while at the same time increase
their allocation to imprudent stocks (e.g., high-volatility stocks). The interpretation of the
coefficients is straightforward. Taking the column relative to Volatility, we see that the allocation
of bank trusts to low Volatility firms was 1.4% larger than that of other institutions under the
prudent-man fiduciary standard, while after the enactment of UPIA statutes bank trusts invest
0.2% less in high Volatility stocks than other institutions. At the same time, before the adoption
of the prudent-investor rule high Volatility firms had a weight in the portfolios of bank trusts 0.7%
smaller than in the portfolios of other institutions, while after the enactment of UPIA statutes
bank trusts invest 0.5% more in high Volatility stocks than other institutions. The same message

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emerges by looking at the column relative to the variable *Dividend Yield*: under prudent-man rule bank trusts display a 1.1% higher allocation in stocks paying high dividends relative to other institutions, and 2.0% lower allocation in imprudent stocks. After the introduction of UPIA statutes, trust display a 1.6% higher allocation to high *Dividend Yield* stocks, and completely eliminate the difference between their allocation in low *Dividend Yield* stocks and the one of all other institutions. Notably, we find consistent results across all 6 variables related to prudence considerations. We perform again a joint test on the interaction terms *UPIA × CharGroup*. In the last column of Table 5, we ask what is the probability of obtaining the coefficients we report in the table if the adoption of the prudent-investor rule has no effect on trusts’ holdings. We reject the null hypothesis that the coefficients $\beta_3$ on the interaction terms are jointly zero at the 1% level.

This concludes the analysis of the causal link between regulation and institutional money managers portfolio choices. We now turn to analyzing the potential implications of trust funds’ portfolio adjustment in the aftermath of the UPIA adoption, on the pricing and returns of prudent and imprudent stocks.

5. The Impact of UPIA Enactment on Stock Returns

In the previous two sections, we establish that under the prudent-man rule, bank trusts would hold disproportionately more (less) wealth in prudent (imprudent) assets than other institutional managers that were not subject to such fiduciary rule. We then provide evidence consistent with a causal link between the legal environment and institutional portfolio choices. Upon the enactment of the UPIA, which replaces the prudent-man discipline with one based on modern portfolio theory, trusts rebalance their portfolio accordingly. In particular, stocks that, considered in isolation, where deemed prudent under the prudent-man rule, lose their prudence-status after the law change, and trusts sell them. At the same time, stocks that were deemed imprudent under the prudent-man standard, do not have such unfavorable connotation under the UPIA, and trusts buy them. All this evidence points to the existence of a regulation-driven deviation
of trust funds’ portfolios from the allocation that would have been observed in absence of the prudent-man fiduciary constraints.

In this section, we ask whether the UPIA reform, and the changes to institutions’ portfolio choices that it entails, also induces detectable changes in the pricing of stocks that were considered prudent (imprudent) prior to the reform. Before proceeding with formal statistical analysis, we believe it is necessary to address two points. The first point provides the rationale for the hypothesis that the UPIA reform may be associated to stock return predictability. The second one concerns the novelty of our approach and our findings on the linkage between institutional trading and returns, in comparison to the approach pursued in other studies that have argued for the existence of such a linkage. As we discuss these points, we offer causes for concerns and empirical remedies, which we then incorporate in our statistical tests.

5.1 Preference-Based Demand Shocks, and Asset Prices

We reason that under prudent-man, heightened demand for prudent securities might have pushed the price of such securities upward, compared to the price these securities would have had in absence of institutional constraints. Similarly, diminished demand for imprudent stocks, may have caused them to be undervalued in the prudent-man era, compared to a scenario with no prudent-man regulation. So, when the prudent-man rule is removed, we conjecture that this relative overpricing of prudent over imprudent investments should be corrected.

Our hypothesis on the behavior of asset prices around the UPIA reform maintains that prudent and imprudent stocks will be priced differently before and after the law change. To provide intuition, we relate our research to the model of capital markets of Merton (1987). In the model, informational frictions prevent some investors from investing in certain stocks. Merton (1987) himself notes that a setting similar to the one obtained in the presence of information frictions can be based on the existence of prudent-investing standards, which inhibit or disincentivize some investors from holdings certain (imprudent) stocks\textsuperscript{13}.

\textsuperscript{13}Note that, as of 1987, when Merton’s paper was published, virtually no state had yet enacted any major reform to the fiduciary duties of trusts. Therefore, Merton’s reference to the prudence-related frictions likely
In the Merton model, some securities cannot be held by all investors. We refer to this as an investor base constraint. In equilibrium, the value of such securities, which correspond to the imprudent stocks according to the prudent-man standard, is lower than the value that one would observe in absence of the investor base constraint\textsuperscript{14}. Stocks with a tighter investor base constraint must also offer higher expected returns than the ones that characterize stocks with a larger investor base. Importantly, if one were to shock the investor base of prudent stocks, which is initially limited by prudence-related concerns, and allow for a broadening of the investor base, the price of these stocks should increase\textsuperscript{15}.

We argue that the UPIA reform entails such a loosening of the investor base constraint for stocks that were deemed imprudent under the prudent-man standard. Stocks that were imprudent and were hence avoided (at least partially) by trust fund managers are the ones who exhibit the biggest loosening of the investor base constraint when UPIA is introduced. Therefore, we hypothesize that cross-sectional stock returns observed after the introduction of the UPIA should be positively correlated with a stock’s perceived imprudence prior to the law change. Extremely imprudent stocks should offer higher returns than the rest of the cross-section, as these are the ones for which the loosening of the constraint is the most apparent. Conversely, stocks that were deemed prudent in the past, and have therefore already an unconstrained investor base prior to UPIA, should feature the least change in their value, and hence offer the lowest returns in the cross-section after UPIA is enacted.

In the Merton (1987) model, the change in value that we have described above as a consequence of changes in the breadth of the investor base would take place immediately, as investors would react instantly to the regulation change. In reality, the results observed in the first part of the paper suggest that trust funds do not adjust their portfolios immediately after the UPIA introduction, but they do so over a long time span. This indicates that cross-sectional return predictability might be observed over a long time-span.

One could still argue that, to the extent that cross-sectional return predictability exists due

\textsuperscript{14}See Merton (1987), Equation 20.
\textsuperscript{15}See Merton (1987), Equation 28.d.
to the enactment of the UPIA, and such change is public information, arbitrage capital could immediately be deployed in a simple trading strategy. In particular, arbitrageurs would start buying imprudent stocks and short-selling prudent ones. This should yield them positive expected returns, due to the argument set above. Furthermore, if close substitutes exist for each stock, as suggested for example by Scholes (1972), one could think of this strategy as a riskless one. Finally, as more arbitrageurs pursue this same strategy, they would bid up the price of imprudent stocks over prudent ones, thus allowing the cross-sectional return predictability patterns outlined above to take place over a short period of time, or even be instantaneous. Critical in this argument is the assumption that the arbitrage opportunity described above is riskless. In this respect, many recent studies have argued that the arbitrageur may often be (at least partly) dissuaded from performing the trading strategy described above, because perfect substitutes rarely exist in the cross-section (Wurgler and Zhuravskaya (2002), Greenwood (2005)). Arbitrageurs may also face judgement as to the merit of their trades, on horizons that are shorter than the one over which the strategy should be active to produce profits with certainty (Shleifer and Vishny (1997)). From this standpoint, only if arbitrageurs coordinate their efforts can they guarantee that their strategies will lead to positive expected returns over a short period of time (Stein (2009)). So, if arbitrage capital does not intervene to profit from UPIA, cross-sectional return predictability can be a phenomenon that takes place over a long time. Even Merton (1987) realizes that often the time-scale over which phenomena are observed in a model can differ from the horizon over which we observe such phenomena unfold in the real world\textsuperscript{16}. All in all, we conjecture that predictable patterns in stock returns might exist over a long period after the introduction of the UPIA. We consider plausible to set an upper boundary of 12 quarters to the time-span over which one should expect UPIA-induced predictability, as this is the horizon over which we document portfolio rebalancing by trust funds. On the other hand, arbitrage capital could indeed be used to profit from the UPIA reform. In this case, return predictability might be more apparent right after the enactment of the reform, and be weaker or missing in the reminder of the 12 quarters.

\textsuperscript{16}Merton (1987) writes "the time frame over which such corrective action takes place can be considerable and even in the long run, it may not be complete."
over which we document portfolio rebalancing by trusts.

Finally, after the prudent-investing laws change, we hypothesize that sorting stocks based on prudence-related variables should not lead to any predictable returns in the cross-section. This is because any cross-sectional difference in investor base constraints which stems from prudent-man is absent after the UPIA reform. Therefore, we conjecture that, on a sufficiently long horizon, no cross-sectional abnormal differences in expected returns should be observed after the enactment of the new fiduciary duty standard. To sum up, we posit that i) at the enactment of the UPIA, stocks previously deemed imprudent should exhibit an increase in value, and hence higher stock returns than the rest of the cross-section; ii) such stock return predictability can take a long period to unfold, as agents take into account real-world trading considerations when adjusting their portfolios; iii) on longer horizons, prudence-related variables are not informative of any cross-sectional return predictability pattern.

While the discussion above provides the foundation for the return predictability test that we perform below, a word of caution is needed, for two reasons. First, the Merton (1987) model provides an exact theoretical framework in the case some investors completely shy away from certain stocks. In reality, as our overallocation analysis of Table 3 shows, in the prudent-man era trust funds do not entirely shy away from imprudent stocks, but they reduce their exposure to them. Second, the discussion above formulates hypothesis in a ceteris paribus fashion. This means that return predictability stemming from the enactment of the UPIA is expected after controlling for differences between prudent and imprudent stocks, which are not directly linked to their lawful prudence, such as their exposure to priced risk factors, or firm size. Thus, in our tests below, we go at great lengths to control for differences between prudent and imprudent stocks’ expected returns, as well as other stock characteristics which do not capture prudence.

5.2 Preference versus Information-Based Trading, and the UPIA

Previous literature has showed that institutional trading of a stock is associated to changes in the pricing of that stock, and stock return predictability (e.g., Coval and Stafford (2007), Lou (2012)).
However, absent an indication on the underlying cause of the observed institutional trading, one can never be sure that the trading activity initiated by institutions is indeed the causal driver of stock returns, or whether the apparent link between trading and returns reflects an underlying omitted variable, such as information on the future value of the stock being traded\textsuperscript{17}. In this respect, our setting potentially resolves this interpretation issue. The choice of the timing of the UPIA reform in a state is arguably exogenous to changes in the relevant information concerning any individual stock. Any trust-initiated trading that is caused by the UPIA is therefore likely orthogonal to the information-based trading of a stock.

One might still worry about the interplay between the UPIA reform and stock-level information. Here, we consider four possible cases concerning the availability of information on the future value of a stock: i) positive information exists for some prudent stocks; ii) negative information exists for some prudent stocks; iii) positive information exists for some imprudent stocks; iv) negative information exists for some imprudent stocks. Cases i) and iv) do not entail confounding effects in our analysis, because if anything such cases would work against finding the cross-sectional return predictability patterns documented above\textsuperscript{18}. On the other hand, scenarios ii) and iii) might entail information-driven confounding effects, as information produces return predictability in the same direction that we propose should be observed after the introduction of the UPIA. While the confounding effect of information is a valid consideration when studying stock-level predictability, our focus is in the pricing implications of UPIA for wide baskets of stocks, either prudent or imprudent. This focus on broad sets of assets, rather than on the return on any individual stock, may reduce concerns that we are picking information-related effects.

All in all, concerns of stock-level information stories suggest that we should conduct our analysis by testing for UPIA-related return predictability within broad sets of stocks, prudent or imprudent, and that by doing this, any information-related effect will if anything work against

\textsuperscript{17}In studies that use mutual funds as the institution of choice, a further complication arises from the fact that mutual funds’ managers respond to fund flows, and such flows reflect the beliefs and preferences of retail investors.

\textsuperscript{18}For instance, if a stock that was deemed prudent before the law is not sold after the passage of the UPIA, possibly because a trust has superior information on the high-quality of the stock, this stock will likely experience appreciation in the future. On the contrary, our hypothesis in the previous section suggests lower, rather than higher, future returns for low-volatility stocks. So, the existence of good news for prudent stocks will, if anything, reduce the negative link between stocks’ prudence and future returns.
finding our hypothesized link between UPIA and returns. We shape our main test below to comply with this suggestion, and we later address any remaining concerns: i) time-varying risk premia associated with prudence characteristics; ii) the existence of macroeconomic confounding effects that the adoption of the UPIA can correlate with; iii) industry and political-economy confounding effects, in the robustness section.

5.3 Baseline Tests

In the first analysis, we regress monthly stock returns on indicator variables for portfolios sorted on characteristics, and the interaction between these indicator variables and a variable that captures the staggered adoption of UPIA statutes. Specifically, we estimate:

$$\text{Ret}_{i,m} = \alpha_i + \alpha_m + \beta_1 \text{CharGroup}_{i,m-1} + \beta_2 \text{PSIC}_{m-1} \times \text{CharGroup}_{i,m-1} + \gamma X_{i,m-1} + \epsilon_{i,m}$$ (3.4)

where $\text{Ret}_{i,m}$ is the monthly stock return of firm $i$ in month $m$; $\alpha_i$ and $\alpha_m$ are firm and date (year-month) fixed effects, respectively; $\text{CharGroup}_{i,m}$ is an indicator that takes value 1 if firm $i$ in month $m$ belongs to either the Prudent or Imprudent group based on each of the prudence-related characteristics. We use the value of each characteristic in June year $t$, to forecast returns from July year $t$ to June year $t + 1$. $X_{i,m-1}$ is a set of firm-level characteristics. Finally, $\text{PSIC}_{s,m}$ is a variable intended to capture the effect of UPIA statutes on stock returns, and it is defined as:

$$\text{PSIC}_{m} = \sum_{s=1}^{51} 1(\text{UPIA}_{s,m,m-12} = 1) \times W_{s,m}^T$$ (3.5)

where $1(\text{UPIA}_{s,m,m-12} = 1)$ are indicator variables that take value one when state $s$ has enacted a UPIA statute in the previous 12 months; $W_{s,m}^T$ is the ratio of the total dollar value invested by bank trusts in a state in month $m$, the month in which the law passes, over the total dollar value invested by all institutional investors across all states in month $m$. Therefore, this variable reflects the fraction of total institutional investors’ money affected by the adoption of prudent-investor rule at any point in time. The underlying intuition is that when a UPIA statute is
adopted in a state where bank trusts collectively account for a larger portion of the aggregate institutional investor portfolio, we expect their rebalancing to have a stronger impact on stock returns.

Table 6, Panel A, shows the results of this analysis. The coefficient $\beta_1$ on $\text{CharGroup}_{i,m}$ captures the stock returns of a given portfolio in “normal” times, while $\beta_2$ gives the differential effect of portfolio membership in the 12-month period after the enactment of a UPIA statute. Across all variables associated with prudence, the adoption of the prudent-investor rule leads to lower returns for stocks classified as Prudent, and to higher returns for stocks classified as Imprudent. In other words, stocks that due to the shift in institutional preferences are sold by bank trusts experience lower returns, and stocks that are bought experience higher returns. To interpret the economic magnitude of the coefficients, let us consider the average state that passes a UPIA statute. When such event occurs, the fraction of total institutional money affected by the shift in preferences increases by 0.56%. Considering the column relative to $\text{Volatility}$, this translates into a negative effect on stock returns for low $\text{Volatility}$ stocks of 21 basis points per month, and a positive effect on the returns of high $\text{Volatility}$ stocks of 26 basis points per month.

If the effect on stock returns observed in Panel A reflects the correction of a mispricing existing before the adoption of the new fiduciary standard, then we should observe no reversal after such a mispricing is corrected. On the other hand, if the observed pattern of stock returns is driven by temporary liquidity effects, as liquidity providers require a premium to accommodate the change in bank trusts demand, we should see that in subsequent periods the initial effect on returns is reversed. In order to discriminate between these two possibilities, we study the impact of UPIA enactment on the returns of Prudent and Imprudent stocks two and three years after the adoption of the statutes.

Results of these tests are reported in Panel B and Panel C of Table 6. The coefficients in both panels indicate the presence of a limited reversal effect. Indeed, across all variable the coefficients on the interaction terms $\text{PSIC} \times \text{Prudent}$ and $\text{PSIC} \times \text{Imprudent}$ enter with the opposite signs relative to Panel A. However, these coefficients are substantially smaller than their counterparts
in Panel A, and never statistically different from zero. Looking at the point estimates in the column relative to Volatility, and ignoring the lack of statistical significance, an increase in PSIC equal to 0.56% translates, during the second year after the event, into a positive effect on stock returns for low Volatility stocks of 9 basis points per month, and a positive effect on the returns of high Volatility stocks of 12 basis points per month.

To sum up, the evidence coming out from Table 6 is consistent with changes in institutional preferences having a direct effect on asset prices. The old prudent-man standard, by giving trusts incentives to tilt their portfolio toward stocks perceived as safe, causes the price of Prudent stocks to be too high relative to their fundamental value, and the price of Imprudent stocks to be too low. After the incentive to be overallocated to safe stocks disappear due to the change of the fiduciary standard, trading by trust leads to the correction of the pre-existing mispricing.

5.4 Alternative Explanations and Robustness Tests

Results in the previous section suggest that the adoption of the prudent-investor rule, by causing trusts to rebalance out of Prudent stocks and into Imprudent stocks, significantly affect the returns of firms in these categories. Although our setting allows us to rule out several competing explanations for our findings, there remain potential concerns about the interpretation of the observed effect on stock returns. In this section, we present four sets of tests that represent our best attempt to address these concerns.

Controlling for Economy-wide Trends and Changing Exposure to Priced Factors

The first concern is that the staggered introduction of UPIA statutes might correlate with other economy-wide trends, or events, that differentially influence the returns of Prudent and Imprudent stocks. If this is true, the inclusion of date fixed effects would not solve the resulting omitted variable bias, and the coefficient $\beta_2$ in equation 3.4 would be biased. Similarly, the exposure to priced risk factors of Prudent and Imprudent stocks might change in a way that correlates with the adoption of UPIA statutes. In alternative, the price of such risk factors might vary over time,
and given their different exposure, Prudent and Imprudent stocks might be differently affected by such variation. Again, the consequence of this would be a bias in our coefficient of interest. As a first test to address both issues, we estimate the following modified version of equation 3.4:

\[
\begin{align*}
\text{Ret}_{i,m} &= \alpha_i + \alpha_m + \beta_1 \text{CharGroup}_{i,m-1} + \beta_2 \text{PSIC}_{m-1} \times \text{CharGroup}_{i,m-1} + \gamma X_{i,m-1} \\
&\quad + \lambda M_{m-1} \times \text{CharGroup}_{i,m-1} + \delta X_{i,m-1} \times \text{PSIC}_{m-1} + \epsilon_{i,m} 
\end{align*}
\] (3.6)

where the new elements are the last two terms. \(M_{m-1}\) is a set of macroeconomic controls, which are interacted with the variable \(\text{CharGroup}_{i,m-1}\). Thus, the coefficient \(\lambda\) on this term captures the potential effect that these macroeconomic variables have on Prudent and Imprudent stocks, and that in the absence of this term might be biasing our main coefficient \(\beta_2\). As for the last term, it is constructed as the interaction between the set of firm-level controls \(X\), and \(\text{PSIC}\). Since the characteristics in the set \(X\) are intended to proxy for loadings on factors and to control for other known sources of return predictability, the inclusion of this term enables the effect of these variables to change at the same time as the introduction of UPIA statutes.

Table 7, Panel A, reports the results obtained by estimating equation 3.6. Across all variables, the coefficients on the interaction terms between Prudent stocks and \(\text{PSIC}\), and between Imprudent stocks and \(\text{PSIC}\) remain statistically significant and economically large.

As a second way to control for the potential change in the exposure to priced factors of Prudent and Imprudent stocks, we adopt the fixed effect approach advocated by Gormley and Matsa (2014). Specifically, we substitute date fixed effect in equation 3.4 with DGTW portfolios-time fixed effects, where DGTW portfolios are the 125 portfolios formed on size, book-to-market, and momentum constructed as in Daniel, Grinblatt, Titman, and Wermers (1997). This approach effectively demeans all the variables with respect to the corresponding portfolio average at each date. Our results, as shown in Table 7, Panel B, are robust to the inclusion of DGTW portfolios-time fixed effects.
Controlling for State- and Industry-level Factors

A second concern for the interpretation of the results in Table 6 relates to state and industry specific effects. There are two reasons why state effects might be a concern: first, firms belonging to certain types cluster in certain states (e.g. tech stocks in California), so that the introduction of a UPIA statute might simply capture the evolution of returns of different types of stocks across states; second, it is possible that the adoption of the prudent-investor rule in a state coincides with a change in the appetite of investors in that state for certain stocks, and to the extent that investors have a preference for local stocks (e.g. Coval and Moskowitz (1999)), this will affect the returns of firms in the state. The argument for industry effects is similar: if firms in certain industries are disproportionately represented in a state, at the moment of the adoption of a UPIA statute in that state, we might be capturing the different returns of firms in these industries. Our solution relies on the inclusion of higher-order fixed effects. In particular, in Table 7, Panel B and Panel C, we substitute date fixed effect with state × date and industry × date fixed effects, respectively. The message that emerges from both panels is that our results are overall robust to controlling for state-time and industry-time effects.

Alternative Specifications

Finally, we make sure that the results in the baseline analysis of section 5.3 are robust to changes in the filters we apply and the definition of the sorting variables. To this end, we repeat our baseline analysis with the following modifications (unreported tests): (i) we drop firms whose market capitalization is in the bottom NYSE size decile, to investigate whether our results are disproportionately driven by micro-caps stocks; (ii) we drop firms whose price is below five dollars, to address potential market microstructure issues; (iii) we do not include firm fixed effects in the analysis, to address the potential bias arising from including firm fixed effects in finite sample highlighted by Pástor, Stambaugh, and Taylor (2015); (iv) we use quintiles of the sorting variables, instead of terciles, to construct the Prudent and Imprudent portfolios. Our results remain qualitatively unchanged using all these alternative specifications.
6. Conclusion

This paper studies whether shocks to uninformed institutional preferences move asset prices. We exploit a unique setting based on the staggered adoption of a new trusts’ fiduciary duty standard across US states between 1986 and 2007. The introduction of the UPIA, which removes the obsolete prescription to evaluate the prudence of investments considered in isolation, generates variation in bank trusts’ preferences for certain stock characteristics, but not in the preferences of other institutional investors. Such variation is plausibly exogenous to stock-specific information or changes in beliefs.

Our paper proceeds in two steps. First, we study the impact of the enactment of UPIA statutes on the portfolio holdings of bank trusts. We provide causal evidence that the UPIA reform, which mandates that courts should judge on a bank trust manager’s adherence to fiduciary duty by assessing the adequacy of the portfolio as a whole, leads to significant changes in the bank trusts’ portfolio choices. Trusts reduce their holdings of stocks that were deemed prudent in isolation under the old prudent-man rule, and to which they were overallocated. At the same time, trusts increase their holding of stocks that were deemed imprudent under the old prudent-man rule, and to which they were underallocated.

Second, we analyze the implications of trust funds’ portfolio rebalancing in the aftermath of the UPIA adoption on the pricing and returns of prudent and imprudent stocks. In particular, we conjecture that a relative overpricing of prudent over imprudent stocks existed under the prudent-man standard, and that the introduction of UPIA should lead to the correction of such mispricing. This mechanism accords well with the model of capital markets of Merton (1987). In support to our hypothesis, we observe that in the twelve months after the removal of the prudent-man rule, prudent stocks have lower returns and imprudent stocks have higher returns. In addition, we do not find any statistically significant evidence of a reversal in the second or third year after the UPIA adoption, which is consistent with the correction of a pre-existing mispricing.
### Table 1: Uniform Prudent Investor Act by Year

This table lists the states that have adopted the Uniform Prudent Investor Act, or similar legislation, together with the year of adoption.

<table>
<thead>
<tr>
<th>State</th>
<th>UPIA Statute Enactment</th>
<th>State</th>
<th>UPIA Statute Enactment</th>
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Table 2: Summary Statistics

This table shows summary statistics for the institutional investors sample as of December of each year. The table reports the following summary statistics separately for the sample of bank trusts, and for the sample including all the remaining institutional investors: *No. Funds* is the number of institutional investors active at the end of each year; *Mean* is the mean total dollar value of equity held by an institutional investor (in millions of dollars); *Median* is the median total dollar value of equity held by an institutional investor (in millions of dollars); *Fraction of Total* is the fraction of the total dollar value of equity held by all institutions accounted for by each of the two groups.

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<td>Median</td>
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Table 3: Determinants of Overallocation

In Panel A, we present comparisons of average values of Overallocation for groups based on a set of characteristics the literature associates with prudence. The set includes CAPM β, defined as the loading on the market factor from the CAPM model; Dividend Yield, defined as the ratio of dividends in the fiscal year ending in calendar year t−1 over the market capitalization of the stock; Firm Age, defined as the number of months since the firm first appeared in the CRSP dataset; Profitability, defined as the ratio of earnings in the fiscal year ending in calendar year t−1 over the market capitalization of the stock; S&P Index, an indicator variable for whether the firm is a member of the S&P 1500 index; Volatility, defined as the standard deviation of monthly stock returns over the previous 12 months; PC, defined as the first principal component of the six prudence-related variables. In Panel B, we present comparisons of average values of Overallocation for groups based on a set of other characteristics, including Size, Book-to-Market, and Momentum. Overallocation is defined as in section 3.3, and it is measured in each state three years before the enactment of the UPIA statute in that state. For all variables, except for S&P Index, Prudent and Imprudent groups are based on terciles of the characteristic. t-statistic based on standard errors clustered at the firm level are shown in parentheses. A more detailed definition of all variables is provided in the Appendix.

Panel A: Prudence-related Variables

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<th>Dividend Yield</th>
<th>Firm Age</th>
<th>Profitability</th>
<th>S&amp;P Index</th>
<th>Volatility</th>
<th>PC</th>
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<td>0.044**</td>
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<td>0.019</td>
<td>0.053***</td>
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<td>-0.029***</td>
<td>-0.023***</td>
<td>-0.022***</td>
<td>-0.012**</td>
<td>-0.021**</td>
<td>-0.021***</td>
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<td>P - NP</td>
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<td>0.104***</td>
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<td>0.033**</td>
<td>0.031*</td>
<td>0.074***</td>
<td>0.042***</td>
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<tr>
<td>(4.01)</td>
<td>(4.95)</td>
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<td>(2.35)</td>
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Panel B: Other Variables

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Table 4: Changes in Portfolio Composition - Triple Difference-in-Differences

This table presents results obtained estimating the following institution-level panel regression:

\[ \text{PortfolioShare}_{j,s,q} = \alpha_j + \alpha_{sq} + \beta UPIA_{s,q} \times \text{Trust}_j + \epsilon_{j,s,q} \]  

(3.7)

\( \text{PortfolioShare}_{j,s,q} \) is the fraction of the portfolio invested in the relevant characteristically-based group by institutional investor \( j \), headquartered in state \( s \), in year-quarter \( q \). \( \alpha_j \) and \( \alpha_{sq} \) are institutional investor and state \( \times \) date fixed effects, respectively. \( UPIA_{s,q} \) is an indicator variable that takes value 1 if state \( s \) has adopted the prudent-investor rule in year-quarter \( q \). \( \text{Trust}_j \) is an indicator variable for bank trusts. For all variables, except for S&P Index, Prudent and Imprudent groups are based on terciles of the characteristic, as measured at the end of quarter \( q \). The indicator variable \( UPIA \) is constructed based on the dates indicated in Table 1. In the last column, we test that coefficients on all six prudence-related variables are jointly zero, and we report the chisquare test statistic and the corresponding p-value. Date FE are based on year-quarter dates. \( t \)-statistic based on standard errors clustered at the trustee level are shown in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively. A complete list of definitions for these variables is provided in the Appendix.

<table>
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<tr>
<th>CAPM ( \beta )</th>
<th>Dividend Yield</th>
<th>Firm Age</th>
<th>Profitability</th>
<th>S&amp;P Index</th>
<th>Volatility</th>
<th>PC</th>
<th>Joint Test Chi-Square (p-value)</th>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>UPIA ( \times ) Trust</td>
<td>-0.030***</td>
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<td>-0.032***</td>
<td>-0.021**</td>
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<tr>
<td></td>
<td>((-3.58))</td>
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<td>((1.18))</td>
<td>((0.17))</td>
<td>((-4.04))</td>
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<td>((-2.31))</td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
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<td>0.008*</td>
<td>0.023***</td>
<td>0.067***</td>
<td>0.014**</td>
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<td>Yes</td>
<td>Yes</td>
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Table 5: Changes in Overallocation

This table presents results obtained estimating the following firm-level panel regression:

\[
\text{Overallocation}_{i,s,q} = \alpha_i + \alpha_q + \beta_1 UPIA_{s,q} + \beta_2 \text{CharGroup}_{i,q} + \beta_3 UPIA_{s,q} \times \text{CharGroup}_{i,q} + \gamma X_{i,q} + \epsilon_{i,s,q} \tag{3.8}
\]

\textit{Overallocation} is defined as in section 3.3; \(\alpha_i\) and \(\alpha_q\) are firm and date fixed effects, respectively; \(UPIA_{s,q}\) is an indicator variable that takes value 1 if state \(s\) has adopted the prudent-investor rule in year-quarter \(q\); \(\text{CharGroup}_{i,q}\) is an indicator that takes value 1 for firms that belongs to either the Prudent or Imprudent group based on each of the prudence-related characteristics we consider; \(X_{i,q}\) includes \(\text{Size}, \text{BTM}, \text{Momentum}, \text{Return}_{t-1}, \text{S&P Index}\) (except for the \(\text{S&P Index}\) and \(\text{PC}\) analyses), and \(\text{Turnover}\). For all variables, except for \(\text{S&P Index}\), Prudent and Imprudent groups are based on terciles of the characteristic, as measured at the end of quarter \(q\). We run separate regressions for low and high groups. In the last column, we test that coefficients on all six prudence-related variables are jointly zero, and we report the chi-square test statistic and the corresponding p-value. Date FE are based on year-quarter dates. \(t\)-statistic based on standard errors clustered at the firm level are shown in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively. A complete list of definitions for these variables is provided in the Appendix.

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<th>Profitability</th>
<th>S&amp;P Index</th>
<th>Volatility</th>
<th>PC</th>
<th>Joint Test Chi-Square (p-value)</th>
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<td></td>
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<tr>
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<td>0.011***</td>
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<td>0.001</td>
<td>0.012**</td>
<td>0.014***</td>
<td>0.017***</td>
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<td>−0.014**</td>
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</tr>
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<td>(−3.35)</td>
<td>(−4.31)</td>
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<tr>
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<td>0.020***</td>
<td>0.004</td>
<td>0.012***</td>
<td>0.009*</td>
<td>0.012***</td>
<td>0.016***</td>
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<td>(3.73)</td>
<td>(1.66)</td>
<td>(3.80)</td>
<td>(4.68)</td>
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<td>Yes</td>
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<td><strong>Date FE</strong></td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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</table>
Table 6: Impact of UPIA Statute on Stock Returns

This table presents results obtained estimating the following firm-level panel regression:

\[ Ret_{i,m} = \alpha_i + \alpha_m + \beta_1 \text{CharGroup}_{i,m-1} + \beta_2 \text{PSIC}_{m-1} \times \text{CharGroup}_{i,m-1} + \gamma X_{i,m-1} + \epsilon_{i,m} \]  

(3.9)

where \( Ret_{i,m} \) is the monthly stock return of firm \( i \) in month \( m \); \( \alpha_i \) and \( \alpha_m \) are firm and date (year-month) fixed effects, respectively; \( \text{CharGroup}_{i,m-1} \) is an indicator that takes value 1 if firm \( i \) in month \( m-1 \) belongs to either the Prudent or Imprudent group based on each of the prudence-related characteristics we consider; \( \text{PSIC}_{m-1} \) is a variable that measures the fraction of institutional investor capital affected by the enactment of a UPIA statute in a state, and it is defined as in equation 3.5. \( X_{i,m-1} \) includes \( \text{Size}, \text{BTM}, \text{Momentum}, \text{Return}_{t-1}, \text{S&P Index} \) (except for the \( \text{S&P Index} \) and \( \text{PC} \) analyses), and \( \text{Turnover} \). In Panel A, the variable \( \text{PSIC} \) is used to predict returns for the 12-month period after the passage of the law. In Panel B and Panel C, the variable \( \text{PSIC} \) is used to predict returns for months 13 to 24, and 25 to 36 after the passage of the law, respectively. We run separate regressions for low and high groups. For all variables, except for \( \text{S&P Index} \), Prudent and Imprudent groups are based on terciles of the characteristic. We use values of each characteristic in June year \( t \) to form Prudent and Imprudent portfolios from July year \( t \) to June year \( t+1 \). Date FE are based on year-month dates. \( t \)-statistic based on standard errors clustered by date are shown in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively. A complete list of definitions for these variables is provided in the Appendix.

<table>
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<th>Panel A: Effect on Returns from 1 to 12 Months after Law Change</th>
<th>CAPM ( \beta )</th>
<th>Dividend Yield</th>
<th>Firm Age</th>
<th>Profitability</th>
<th>S&amp;P Index</th>
<th>Volatility</th>
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<tr>
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<td>0.000</td>
<td>0.005***</td>
<td>0.004***</td>
<td>0.009***</td>
<td>0.005***</td>
<td>0.007***</td>
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<td></td>
<td>(2.40)</td>
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<td>(4.48)</td>
<td>(3.62)</td>
<td>(5.22)</td>
<td>(3.64)</td>
<td>(4.65)</td>
</tr>
<tr>
<td>PSIC * Prudent</td>
<td>-0.244**</td>
<td>-0.336**</td>
<td>-0.279***</td>
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<td>-0.237**</td>
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<td>-0.338***</td>
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<td>(-2.51)</td>
<td>(-2.72)</td>
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<td></td>
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<tr>
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<td>-0.009***</td>
<td>-0.007***</td>
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<td>(2.93)</td>
<td>(3.05)</td>
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Panel B: Effect on Returns from 13 to 24 Months after Law Change

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<th>Firm Age</th>
<th>Profitability</th>
<th>S&amp;P Index</th>
<th>Volatility</th>
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<td></td>
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<td>0.001</td>
<td>0.005**</td>
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<tr>
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<td>(0.98)</td>
<td>(0.88)</td>
<td>(0.72)</td>
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<td><strong>Analysis for Imprudent Stocks</strong></td>
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<td></td>
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<td></td>
</tr>
<tr>
<td>Imprudent</td>
<td>−0.002</td>
<td>−0.001</td>
<td>0.005***</td>
<td>−0.003*</td>
<td>−0.005**</td>
<td>−0.001</td>
<td>−0.002</td>
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Panel C: Effect on Returns from 25 to 36 Months after Law Change

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<th>S&amp;P Index</th>
<th>Volatility</th>
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<td>0.003**</td>
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</table>
This table presents results obtained estimating the following firm-level panel regression:

\[
Ret_{i,m} = \alpha_i + \alpha_m + \beta_1 CharGroup_{i,m-1} + \beta_2 PSIC_{m-1} \times CharGroup_{i,m-1} + \gamma X_{i,m-1} + \epsilon_{i,m}
\]

(3.10)

where \( Ret_{i,m} \) is the monthly stock return of firm \( i \) in month \( m \); \( \alpha_i \) and \( \alpha_m \) are firm and date (year-month) fixed effects, respectively; \( CharGroup_{i,m-1} \) is an indicator that takes value 1 if firm \( i \) in month \( m - 1 \) belongs to either the Prudent or Imprudent group based on each of the prudence-related characteristics we consider; \( PSIC_{m-1} \) is a variable that measures the fraction of institutional investor capital affected by the enactment of a UPIA statute in a state, and it is defined as in equation 3.5. \( X_{i,m-1} \) includes \( Size, BTM, Momentum, Return_{t-1}, S&P \) Index (except for the \( S&P \) Index and PC analyses), and \( Turnover \). In Panel A, Controls Interacted includes the interactions between all control variables in the set \( X \) with the \( PSIC \) variable, as well as the interactions between a set of macroeconomic variables with \( CharGroup \). The macroeconomic variables include: T-Bill rate, inflation rate, monthly growth in unemployment, logarithm of the growth in consumption expenditures, NBER recession dummy, and the sentiment index defined as in Baker and Wurgler (2006). In Panel B, we substitute date FE with DGTW \( \times \) date FE. In Panel C, we substitute date FE with state \( \times \) date FE. In Panel D, we substitute date FE with industry \( \times \) date FE, defined using the Fama-French 48-industry classification. We run separate regressions for low and high groups. For all variables, except for \( S&P \) Index, Prudent and Imprudent groups are based on terciles of the characteristic. We use values of each characteristic in June year \( t \) to form Prudent and Imprudent portfolios from July year \( t \) to June year \( t+1 \). Date FE are based on year-month dates. \( t \)-statistic based on standard errors clustered by date are shown in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively. A complete list of definitions for these variables is provided in the Appendix.
Panel B: DGTW \times \text{Date Fixed Effects}

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<th>Firm Age</th>
<th>Profitability</th>
<th>S&amp;P Index</th>
<th>Volatility</th>
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<td>0.006***</td>
<td>0.003***</td>
<td>0.008***</td>
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<td>$-0.197^*$</td>
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<td>$-0.261^{***}$</td>
<td>$-0.224^{**}$</td>
<td>$-0.264^{***}$</td>
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<td>$(-4.01)$</td>
<td>$(-2.14)$</td>
<td>$(-2.61)$</td>
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<tr>
<td><strong>Analysis for Imprudent Stocks</strong></td>
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<tr>
<td>Imprudent</td>
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<td>(2.23)</td>
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Panel C: State \times \text{Date Fixed Effects}

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<th>Profitability</th>
<th>S&amp;P Index</th>
<th>Volatility</th>
<th>PC</th>
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<td>Prudent</td>
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<td>0.008***</td>
<td>0.005***</td>
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<td>(4.77)</td>
<td>(3.52)</td>
<td>(5.61)</td>
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<td>0.311**</td>
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<td>0.388***</td>
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### Panel D: Industry × Date Fixed Effects

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<th>Profitability</th>
<th>S&amp;P Index</th>
<th>Volatility</th>
<th>PC</th>
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<td>0.005***</td>
<td>0.003***</td>
<td>0.010***</td>
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<td>(3.99)</td>
<td>(4.05)</td>
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<td>0.001</td>
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<td>0.297^{***}</td>
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<td>1,528,921</td>
<td>1,528,921</td>
<td>1,528,921</td>
</tr>
</tbody>
</table>
Figure 1: Changes in Portfolio Composition

This figure plots point estimates from the coefficients in the following equation:

\[
\text{Portfolio Share}_{j,s,q} = \alpha_j + \alpha_{sq} + \beta \text{Trust}_j \times \sum_{t=-6}^{12} 1(time = t) + \epsilon_{j,s,q}
\]  

where \( \text{Portfolio Share}_{j,s,q} \) is the fraction of the portfolio in the relevant characteristically-based group held by institutional investor \( j \), headquartered in state \( s \), in year-quarter \( q \). \( \alpha_j \) are institutional investor fixed effects. \( \alpha_{sq} \) are state \( \times \) date fixed effects. \( \text{Trust}_j \) is a indicator variable for bank trusts. \( 1(time = s) \) are indicator variables that take value 1 if state \( s \) has adopted the prudent-investor rule in each semester \( t \) in event time, from semester -6 to semester 12. Event time is defined relative to the adoption of a UPIA statute as described in Table 1.
Figure 1: Changes in Portfolio Composition
Figure 1: Changes in Portfolio Composition
APPENDIX

A Description of Variables

<table>
<thead>
<tr>
<th>Variable</th>
<th>Description</th>
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<tr>
<td><strong>Prudence-related Characteristic</strong></td>
<td></td>
</tr>
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<td><strong>CAPM Beta</strong></td>
<td>For any stock, CAPM Beta is calculated over a window of prior 60 observations, and over that period, a minimum of 24 observations is required for the stock to be admitted into a portfolio.</td>
</tr>
<tr>
<td><strong>Firm Age</strong></td>
<td>For any stock, Age is calculated as of June of year t, as the number of months from its first appearance in CRSP.</td>
</tr>
<tr>
<td><strong>Dividend Yield</strong></td>
<td>Ratio of dividends over the market capitalization of the stock. Dividends are as of the fiscal year ending in calendar year t-1, and are measured through the first available of the following: i) dividend per share (DVPSXC); ii) ordinary dividends (DVC); (iii) cash dividends (DV).</td>
</tr>
<tr>
<td><strong>Profitability</strong></td>
<td>Ratio of firm earnings over the market capitalization of the stock. Firm earnings are calculated as income before extraordinary items (IB), plus deferred taxes (TXDI) when available, minus dividends paid to preferred stock when available. When IB is not available, we use income before extraordinary items from the cash flows statement (IBC). Earnings are as of the fiscal year ending in calendar year t-1.</td>
</tr>
<tr>
<td><strong>S&amp;P Index</strong></td>
<td>An indicator variable for whether the firm is a member of the S&amp;P 1500 Super Composite Index.</td>
</tr>
<tr>
<td><strong>Volatility</strong></td>
<td>For any stock, Volatility is computed as the standard deviation of monthly stock returns over the prior 12 months. A minimum number of 9 monthly returns is required for the calculation.</td>
</tr>
<tr>
<td><strong>Other Firm-level Characteristic</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Book-to-Market</strong></td>
<td>The ratio of the book equity for the fiscal year ending in calendar year t-1 divided by the market value of equity at the end of December of year t-1. The book value of equity is calculated as stockholder equity, plus deferred taxes and credits, minus the book value of preferred stock. Stockholders’ equity is the first available from the following list: i) the Compustat item (SEQ); ii) the book value of common equity (item CEQ), plus preferred stock (item PSTK); iii) the book value of total assets (AT) minus the book value of total liabilities (LT). Deferred taxes and credits are measured through the first available of the following: i) the Compustat item (TXDITC); ii) the sum of balance-sheet deferred taxes (TXDB) and investment tax-credit, (TCB); iii) zero. The book value of preferred stock is the first available of the following: i) redemption value (PSTKRV); ii) liquidation value (PSTKL); iii) par value (PSTK); iv) zero.</td>
</tr>
<tr>
<td><strong>Momentum</strong></td>
<td>The cumulated continuously compounded stock return from month j12 to month j2, where j is the month of the forecasted return.</td>
</tr>
<tr>
<td><strong>Return_{t-1}</strong></td>
<td>The lagged one month stock return.</td>
</tr>
<tr>
<td><strong>Size</strong></td>
<td>The market value of common equity. Market value of equity is the product of the price (PRC) at the end of June of year t times the contemporaneous number of shares outstanding (SHROUT).</td>
</tr>
<tr>
<td><strong>Turnover</strong></td>
<td>The average ratio between trading volume (VOL) and shares outstanding (SHROUT) over the previous 12 months.</td>
</tr>
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</table>
Table A.2: Changes in Portfolio Composition - Difference-in-Differences

This table presents results of panel regressions of bank trust managers’ proportion of portfolio invested in various characteristics tercile groups on an indicator for whether the trustee headquarter state has adopted a UPIA statute, bank trust managers' fixed effects, and date fixed effects. The sorting variables are described in Table 3, and defined more in details in the Appendix. The indicator variable *UPIA Statute* is constructed based on the dates indicated in Table 1. Date FE are based on year-quarter dates. *t*-statistic based on standard errors clustered at the trustee level are shown in parentheses.

### Panel A: Volatility

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<td>$0.019^{***}$</td>
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<tr>
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<tr>
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<td>16,906</td>
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### Panel B: PC Groups

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<td>$0.008^{**}$</td>
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<td>0.71</td>
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Bibliography


Scott, Austin Wakeman, 1959, Restatement (second), of trusts, Section 174, (Boston, Little, Brown).


