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Introduction

This Ph.D. dissertation consists of three chapters in corporate finance and financial intermediation. The first chapter studies how revolving doors affect the incentives of credit rating analysts. The second chapter examines the importance of on-the-job-learning for mutual fund managers. The last chapter focuses on the role of limited attention by institutional shareholders for corporate decision-making.

The first chapter studies how the option to go work for an investment bank affects the incentives of credit rating analysts to issue accurate ratings. A major charge against rating agencies, voiced by policy makers and the popular press after the financial crisis, is that this "revolving door" distorts incentives by making rating analysts overly sympathetic to the interests of the underwriting investment banks. I contribute to the ongoing debate about revolving doors by hand-collecting a novel dataset that links the performance of 229 credit rating analysts between 2000 and 2010 to their subsequent career paths. I show that rating analysts who move to investment banks are significantly more accurate prior to their transition than other analysts rating similar securities at the same point in time. A key innovation of my empirical design is that I am able to compare performance both within the same analyst over time as well as across analysts rating similar products at the same point in time. This ensures that my results are not driven by selection of smart individuals into investment banking jobs, or by endogenous matching of revolving analysts to securities. My findings have important implications for the regulation of the revolving door phenomenon more broadly, suggesting that it may, on average, strengthen rather than weaken analysts' incentives to issue accurate ratings.

The second chapter examines how experience on the job affects the performance of mutual fund managers. While consumers often appreciate the experience of their surgeons or airplane pilots, little work exists that identifies the value of experience for top-level economic decision makers such as mutual fund managers. A main challenge for any study on the value of experience is identification, because many obvious proxies, such as tenure, are correlated with other unobserved variables such as baseline skill, age, or career concerns. To circumvent this problem, we look "inside" funds and exploit heterogeneity in experience for the same manager at a given point in time across industries. We show that mutual fund managers outperform in industries where they have obtained experience on the job. Two important implications of our study are that tenure may not be a powerful proxy for experience, and that experience is a valuable fund manager characteristic investors should care about.

The third chapter focuses on the effects of limited attention by institutional shareholders on corporate decision-making. While a growing literature in economics and finance studies limited attention, the impact of limited attention on corporate actions is largely unexplored. To fill this gap, we develop a new identification approach that constructs firm-level shareholder "distraction" measures, by exploiting exogenous shocks to unrelated parts of institutional shareholders' portfolios. We show that institutional shareholder attention matters for corporate investment, payout, and CEO pay. Specifically, we show that firms with "distracted" shareholders are more likely to announce diversifying, value-destroying acquisitions, more likely to grant opportunistically-timed CEO stock options, more likely to cut dividends, and less likely to fire their CEO for bad performance. Our results suggest that understanding managerial responses to temporally relaxed monitoring constraints may significantly improve our understanding of value-creation in firms.

Contents

Chapter 1

The Job Rating Game: The Effects of Revolving Doors on Analyst Incentives

> The implication of Dodd-Frank is that if you can just clamp down on rogue and conflicted analysts, the credit-rating industry will be reformed.
>
> William Harrington, *Wall Street Journal (2011)*

1.1. Introduction

Revolving doors – the possibility for monitors to be hired by the firms they monitor – are widespread in financial markets: financial regulators join banks they oversee, risk-controllers join trading floors they monitor, and credit analysts join entities they rate. Despite their common occurrence, revolving doors are often seen as a source of governance failure, rather than as an efficient economic mechanism. A commonly voiced concern is that revolving doors make monitors overly sympathetic to the interests of the monitored: *"the notion that you would be critical of some entity and then hope they hire you goes against what we know about human nature"* (Barney Frank, in Wall Street Journal (2011)). The public's critical stance on revolving doors is further underscored by recent regulatory efforts aimed at reducing their potential adverse effects: the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010 ("Dodd-Frank") requires credit rating agencies to disclose analyst transfers to entities they helped rate.[1]

While many observers view revolving doors as an economic distortion, ex-ante their net effect on monitoring performance is ambiguous. If monitors get hired as a quid pro quo for favors to their future employers or for their ability to influence their former colleagues (the "collusion" view), they may be willing to give their future employers favorable treatment, or focus too much on building their network at the expense of their monitoring performance (Eckert (1981)). In contrast, if monitors are hired primarily for their expertise (the "human capital" view), they will have a greater incentive to invest in their industry qualifications or to signal their expertise during their employment as monitors (Che (1995), Salant (1995), Bar-Isaac and Shapiro (2011)). Whether the human capital view or the collusion view dominates is an empirical question. The answer has important implications for determining the optimal regulatory response, and, more broadly, for understanding how concerns about future career prospects affect performance incentives.

The first challenge for empirical studies of revolving doors is that data on individual monitoring performance are scarce. The second challenge is identification because we do not observe how a monitor would have performed in the absence of revolving doors. The performance of non-revolving monitors provides a useful counterfactual, but such a comparison is complicated due to a number of potentially confounding factors. First, comparing the performance of monitors across time is problematic due to cohort effects and time-varying task environments. Second, even at the same point in time, monitors may be assigned to projects with different characteristics and levels of difficulty. Third, there could be unobserved heterogeneity across individuals. For example, we may observe that revolving monitors outperform not because they work harder but because they are inherently smarter.

[1] See section 932 of Dodd-Frank, which adds a new paragraph to section 15E(h)(5) of the Securities Exchange Act of 1934. Available on the SEC's website at `https://www.sec.gov/divisions/marketreg/ratingagency/wallstreetreform-cpa-ix-c.pdf`.

This study overcomes these empirical challenges by assembling a novel hand-collected dataset that tracks the career paths of 229 credit rating analysts at Moody's and links them to 22,188 securitized finance securities they rate between 2000 and 2010. In particular, I identify which analysts join an investment bank following their employment at Moody's. This empirical setting is ideal for studying revolving door effects for several reasons. First, credit ratings represent a publicly observable and relatively frequent measure of monitoring output by individual analysts. Subsequent corrections of the initial ratings issued by these analysts provide a useful proxy for analyst (in)accuracy. An attractive institutional feature of Moody's organization is that subsequent rating adjustments are generally performed by a separate internal surveillance team and are therefore not under the influence of the initial analyst. Second, I can identify the revolving door effect by comparing the performance of revolving and non-revolving analysts rating *similar* securities *at the same point in time*, while controlling for a rich set of observable and unobservable differences in the characteristics of these securities. Non-revolving analysts at the same rating agency and the same point in time provide a useful counterfactual because they face the same organizational environment and similar tasks, objectives, and other career concerns. Fourth, rating analysts produce relatively many output signals compared to other professions in the regulatory environment, such as lawyers, who usually work on few cases during their career. This feature of the data allows me to exploit changes in performance within the same individual and to separate incentive effects from the effect of time-invariant unobserved heterogeneity across analysts.

Studying revolving doors in the context of credit analysts in securitized finance is economically relevant for two main reasons. First, the market for securitized finance is of first-order economic importance with more than \$10 trillion of outstanding debt in the U.S. by the end of 2012, which is 1.4 times the size of the U.S. corporate bond market.[2] Distortions in the incentives of analysts rating these securities could

[2] Securities Industry and Financial Markets Association (SIFMA); reports available at `http:`

therefore have economically sizable consequences. Second, inflated credit ratings of securitized finance products have been identified as being at the origin of the last financial crisis,[3] and have at least partially been attributed to the revolving door between rating agencies and investment banks.[4]

My findings are broadly consistent with the human capital view of revolving doors. Prior to their departure to investment banks, analysts are significantly more accurate than other analysts rating similar products at the same point in time. An important feature of my data is that I can remove time-invariant heterogeneity across analysts by including analyst fixed effects, which ensures that my results are not driven by the selection of smart individuals into investment banking jobs. In addition, my results are robust to alternative measures of analyst accuracy, different subperiods, and estimation methods. Further tests exploiting the cross-section of securities rated by revolving analysts show that the effect of the revolving door is not unambiguously positive. Consistent with a bias of revolving analysts in favor of their future employers (see Cornaggia, Cornaggia, and Xia (2015)), they do not outperform on the securities underwritten by their future employers. However, given that these securities represent less than 7% of all securities rated by revolving analysts, they do not lead to economically sizable distortions in their aggregate performance.

A number of additional tests support the interpretation that revolving analysts outperform because of enhanced analyst effort. First, the outperformance of revolving analysts is larger for more complex securities, where one would expect analyst effort to matter more. Second, as opposed to a stable or gradual outperformance, I observe a sudden and strong improvement in the performance of revolving analysts during the last year prior to their departure. This performance improvement

//www.sifma.org.

[3]The Financial Crisis Inquiry Commission (2011) concluded that "the failures of credit rating agencies were essential cogs in the wheel of financial destruction. The three credit rating agencies were key enablers of the financial meltdown. The mortgage-related securities at the heart of the crisis could not have been marketed and sold without their seal of approval."

[4]See, for example, Wall Street Journal (2011) and Bloomberg News (2015).

is unrelated to the analysts' tenure at the time of their exit, which makes an alternative explanation based on differential analyst learning unlikely. Third, I exploit variation in the supply of investment banking jobs as an exogenous shock to analysts' likelihood of moving to an investment bank. I find that positive shocks to the supply of investment banking jobs increase average analyst performance and, in the cross-section of analysts, affect more strongly analysts who are ex-ante more likely to switch career.

While my main tests are designed to address identification issues, Figure 1.1 shows that two important insights emerge even from the raw data. The figure plots the number of analyst departures to investment banks and the average outperformance of departing analysts for five subperiods. First, analysts who depart to investment banks issue ratings that require fewer subsequent adjustments than ratings issued by other analysts (ca. 0.4 notches on average). Second, in most subperiods the average outperformance of revolving analysts increases monotonically with the hiring intensity by investment banks as measured by the number of departing analysts. Hence, even the raw data are supporting the human capital view of revolving doors.

Overall, my findings suggest that revolving doors may *on average* lead to improved, rather than reduced monitoring performance. This may explain why, despite the frequently voiced concerns, revolving doors have remained open in most professions. My results also imply that conflicts of interest arising from revolving doors are unlikely to have been a major driver of poor ratings quality in securitized finance prior to the financial crisis, despite the claims by some regulators and the public press. On the contrary, they suggest that the option to switch to a career in investment banking may represent a strong incentive for credit analysts to perform well, and that restricting the revolving door without changing other aspects of analyst compensation may lead to lower ratings quality. An excessive regulatory focus on conflicted *individual* analysts may further be detrimental if it shifts the regulator's attention away from addressing first-order drivers of poor ratings performance in securitized finance, as suggested in the opening quote of this paper.[5]

There is surprisingly little systematic evidence on revolving doors, given the public interest and regulatory concern for the topic. The few existing studies on the career concerns of credit and equity analysts have focused on the collusion view. The study most closely related to mine is Cornaggia, Cornaggia, and Xia (2015), who find that credit rating analysts award inflated ratings to their future employers prior to the employment transfer. My study confirms their results on the subset of securities underwritten by transitioning analysts' future employers, but shows that this effect is dominated by their higher accuracy on other securities. For sell-side equity analysts, Cohen, Frazzini, and Malloy (2012) report that analysts who get appointed as independent directors are overly sympathetic to management and poor relative performers, and Lourie (2014) finds that analysts who get hired by a firm they cover become more optimistic about their future employer, while becoming more pessimistic about other firms. Horton, Serafeim, and Wu (2015) document that banking analysts exhibit a stronger pattern in issuing optimistic forecasts at the beginning of the year and pessimistic forecasts at the end of the year when they are forecasting earnings of potential future employers. Studies of revolving doors in other contexts report mixed results. Supporting the collusion view, Spiller (1990) finds that regulators who preside over more lenient regulatory periods are more likely to get jobs in the industry, and i Vidal, Draca, and Fons-Rosen (2012) show that revolving door lobbyists' main asset is selling access to powerful politicians rather than regulatory expertise. Other studies support the human capital view of revolving

[5]The academic literature has, for example, pointed to distortions created by the "issuer pays" business model of credit rating agencies, such as an excessive focus on issuer relationships (He, Qian, and Strahan (2012), Efing and Hau (2015)), rating shopping (Benmelech and Dlugosz (2009), Mathis, McAndrews, and Rochet (2009), He, Qian, and Strahan (2015)), and rating catering (Griffin, Nickerson, and Tang (2013), He, Qian, and Strahan (2015)). In addition, interactions of the business model with the lack of investor sophistication (Skreta and Veldkamp (2009), Bolton, Freixas, and Shapiro (2012)), regulatory arbitrage (Opp, Opp, and Harris (2013)), and the business cycle (Bar-Isaac and Shapiro (2013)) have been identified as potential drivers of poor ratings quality in securitized finance.

doors. For example, deHaan, Kedia, Koh, and Rajgopal (2015) show that private law firms hire harsher SEC lawyers, and Cohen (1986) finds that private firms hire regulators who are generally less supportive of the industry. In addition, Lucca, Seru, and Trebbi (2014) document that gross worker outflows from the regulatory to the private sector are higher during times of higher enforcement activity, and Shive and Forster (2015) show that financial firms take significantly less risk after hiring former regulators.

1.2. Theoretical Framework and Empirical Strategy

The goal of this section is twofold. First, I provide a parsimonious framework that illustrates the human capital view of revolving doors and that predicts the main effect that I document in this paper. The partial equilibrium model features heterogeneous analysts working at a credit rating agency and a revolving door between the rating agency and an investment bank. I show that the presence of a revolving door can have positive effects on the ex-ante incentives of analysts to exert effort while they are employed at the credit rating agency, as in Bar-Isaac and Shapiro (2011) and Che (1995). Second, I use the model to derive testable cross-sectional predictions and to point out some key empirical challenges.

1.2.1. Theoretical Framework

Consider a credit rating agency (CRA) that employs a group of heterogenous analysts who each rate a project during their term. Analyst i chooses to exert effort $e_i \in [0,1]$, incurring a cost $e_i^2/2a_i$, where a_i denotes the innate ability of the analyst and is uniformly distributed over the interval $[\underline{a}, \bar{a}]$. The cost of effort is therefore increasing and convex in e_i, as in Bar-Isaac and Shapiro (2011), and decreasing in individual ability. If the rating by analyst i turns out to be accurate, which occurs with probability e_i, the CRA pays him w_{CRA}.

The analyst also decides whether he wants to participate in a lottery to be selected for a job at the investment bank (IB) after his term at the rating agency. The decision to participate in the lottery is indexed by l_i, which is equal to one if the analyst participates, and zero otherwise. Conditional on participating in the lottery, the probability of being hired by the investment bank is $p \in [0, 1]$.[6] Switching career is assumed to be costly as in Bond and Glode (2014): analysts who choose to become investment bankers incur a fixed cost c.[7] The expected utility of post-CRA employment at the investment bank is equal to $e_i w_{IB}$, where w_{IB} represents the expected rent from the investment banking job. Analysts are risk-neutral and have a discount rate of zero. The sequence of events is depicted in the figure below:

To sum up, my simple model relies on the following assumptions.

Assumption 1. *Analysts are heterogenous in their innate ability, i.e.,* $\underline{a} < \bar{a}$.

Assumption 2. *Switching career to investment banking is costly, i.e.,* $c > 0$.

Assumption 3. *Analysts' expected utility in an investment banking job is increasing in* e_i. *Specifically, it is equal to* $e_i w_{IB}$.

Assumption 3 follows Bar-Isaac and Shapiro (2011) and can be justified by anecdotal evidence that expertise in rating securitized finance securities is very valuable

[6] Following Bar-Isaac and Shapiro (2011), I assume that the probability of getting an investment bank job does not depend on e_i. Hence, my model reflects the possibility that investment banks may not directly observe analyst effort or performance. Alternatively, the probability of getting an investment banking job can be modeled to increase in the analyst's effort at the rating agency (see, for example, Che (1995)). This would be an alternative way of interpreting my results.

[7] The switching cost can be interpreted as a decrease in productivity, a direct disutility from relocating (see Bond and Glode (2014)), or a behavioral aversion against change or uncertainty. The implication of the switching cost is that not all analysts may prefer switching to investment banking after their employment at the CRA.

to investment banks (see, for example, Financial Times (2007)). Assuming the expected utility in an investment banking job to be linear in e_i is convenient but can be relaxed: the option to switch to investment banking will have positive effects on analysts' ex-ante performance incentives as long as there is some positive correlation between the analyst's performance at the CRA and his expected utility in an IB job (see Che (1995)). The expected utility of analyst i is therefore:

$$U(e_i, a_i, l_i) = e_i w_{CRA} - e_i^2/2a_i + l_i(p(e_i w_{IB} - c)) \tag{1.1}$$

For each analyst i, the condition under which he chooses to participate in the lottery is given by $e_i w_{IB} > c$, implying the following optimal choice of l_i^*:

$$l_i^* = \begin{cases} 1 & \text{if } e_i > \frac{c}{w_{IB}}. \\ 0 & \text{if } e_i \leq \frac{c}{w_{IB}}. \end{cases} \tag{1.2}$$

Hence, only analysts with effort e_i above a certain threshold would choose to participate in the lottery. Analysts with effort e_i below the threshold would never benefit from switching careers, as their expected rent from the IB job would not be large enough to offset the switching cost c. These analysts would therefore never choose to enter the lottery irrespective of the probability of being selected. Maximizing equation (1.1) with respect to effort e_i yields the following optimal choice of e_i as a function of the analyst's innate ability a_i:

$$e_i^* = \begin{cases} w_{CRA} a_i & \text{for } l_i = 0 \\ (w_{CRA} + p w_{IB}) a_i & \text{for } l_i = 1 \end{cases} \tag{1.3}$$

Note that analysts who choose to enter the lottery systematically exert greater effort than those who choose not to enter the lottery, i.e., $(e_i^*(a_i)|l_i = 1) > (e_i^* a_i|l_i =$

0). In addition, the optimal effort choice for those who choose to enter the lottery, $(e_i^*|l_i = 1)$, increases in the probability p of being hired by the investment bank.[8] This is the first positive effect of the revolving door. Combining equations (1.3) and (1.2) allows me to solve for the threshold ability level a_L above which analysts choose to participate in the lottery ($l_i^* = 1$) and exert relatively more effort:

$$a_L \equiv \frac{c}{w_{IB}(w_{CRA} + pw_{IB})} \tag{1.4}$$

The threshold ability level increases in the switching cost c and decreases in the rent from the investment banking job w_{IB}. More importantly, it also decreases in the probability p of being hired by the investment bank. This is the second positive effect of the revolving door: more analysts exert a greater effort when the prospects of being hired by the investment bank are high.

1.2.2. Key Predictions and Empirical Approach

The main prediction arising from my theoretical framework above is that analysts at the CRA perform better in the presence of the revolving door, i.e., when they have the option to participate in the lottery for an investment banking job. In other words, the average causal effect of the revolving door on the performance of analysts who choose to enter the lottery ("the treated") is positive (as proven in the Appendix, Section 1.6):

$$ATT = E(e_i|l_i = 1, a_i > a_L) - E(e_i|l_i = 0, a_i > a_L) = pw_{IB}0.5(a_L + \bar{a}) > 0 \tag{1.5}$$

The main challenge for empirical studies of revolving doors is that the counterfactual performance in the absence of the possibility to be selected for an IB job, $E(e_i|l_i = 0, a_i > a_L)$ in the above equation, is unobservable. Existing empirical studies have therefore resorted to using non-revolving monitors as a natural control

[8] This claim is immediate on taking the derivative of $(e_i^*|l_i = 1)$ with respect to p.

group (see, for example, Cohen (1986), Spiller (1990), Cornaggia, Cornaggia, and Xia (2015), and deHaan, Kedia, Koh, and Rajgopal (2015)). However, comparing ex-post differences in performance between revolving and non-revolving analysts does not yield an unbiased estimate of the average causal effect of revolving doors (henceforth abbreviated as ATT). In the following, the event of becoming a revolving analyst is indexed by D_i, which is equal to one if the analyst is eventually selected for an IB job, and zero otherwise. Observed differences in performance between revolvers and non-revolvers are linked to the average causal effect by the following equation (as proven in 1.6):

$$\begin{aligned} E(e_i|D_i=1) - E(e_i|D_i=0) = {} & \underbrace{pw_{IB}0.5(a_L+\bar{a})}_{\text{ATT}} \\ & + \underbrace{w_{CRA}0.5(a_L+\bar{a}-\theta(a_L+\bar{a})-(1-\theta)(\underline{a}+a_L))}_{\text{Selection bias}} \\ & - \underbrace{\theta pw_{IB}0.5(a_L+\bar{a})}_{\text{Attenuation bias}}, \end{aligned} \tag{1.6}$$

where θ refers to the share of lottery entrants in the population of non-revolving analysts. The selection bias is driven by the fact that revolving analysts are not randomly drawn from the population of analysts. They have a higher average baseline ability and, hence, would have performed better than the average analyst in the control group even in the absence of revolving doors. This selection therefore creates an upward bias in the estimation of the ATT. Since the control group contains some "treated" analysts who also entered the lottery but were not selected for an IB job, there will also be some attenuation bias. Attenuation bias is not a major concern because it will bias the estimate of the ATT downward.

Once we are able to condition on individual baseline ability, observed differences in performance between revolving and non-revolving analysts provide a lower bound of the average causal effect of interest: $E(e_i|D_i=1,a_i) - E(e_i|D_i=0,a_i) \leq ATT$.

In other words, we are only left with attenuation bias. Conditioning on individual baseline ability requires panel data, i.e., repeated observations on individual analysts. With panel data, we can remove the problem of selection bias by comparing the performance of revolving and non-revolving analysts while controlling for unobserved analyst heterogeneity through analyst fixed effects:

$$e_{it} = \lambda_i + \delta D_{i,t+h} + \epsilon_{it}, \tag{1.7}$$

where e_{it} is the performance of analyst i in period t, $D_{i,t+h}$ is an indicator equal to one if the analyst is selected for an investment banking job within the next h periods, and λ_i are individual fixed effects. The human capital view predicts that δ in the above regression is positive, which is the focus of my main tests.

An alternative empirical approach to assess the effect of revolving doors on analyst performance is to exploit changes in the probability of being hired by an investment bank (parameter p). Consider, for example, a change in p from p_1 to p_2, where $p_2 > p_1$. In my theoretical framework, this change leads to a weakly positive average change in analyst performance, i.e., $E(e_i|p_2) - E(e_i|p_1) \geq 0$ (see Appendix, Section 1.6). However, changes in p that affect the performance of all analysts at the same point in time are empirically not separable from other unobserved time-varying factors also correlated with rating performance, such as the economic outlook, underwriting standards, product complexity, recruiting standards, etc. A suitable empirical analysis therefore requires variables that affect the prospects of *some* analysts to be hired by an investment bank, but not of others. In Section 1.5, I exploit the event of an investment bank entering a new segment of the securitized finance market as a proxy for an increase in the supply of investment banking jobs and for a positive shock to the probability of being hired by an investment bank for analysts working in that segment. In addition, I can test whether, in the cross-section, this change in supply affects some analysts more than others. Specifically, my theoretical framework predicts that there exits a group of low-ability analysts whose performance is insensitive to changes in p (see Appendix, Section 1.6).

1.3. Data

An important implication of the human capital view illustrated above is that revolving doors positively affect ex-ante analyst effort and, thus, the accuracy of all ratings issued by revolving analysts. Focusing on the performance of revolving analysts in interactions with their future employers only, an approach used in some previous studies, may therefore underestimate the positive effects of revolving doors on analyst performance. The reason is that *all* securities benefit from revolving analysts building or showcasing their expertise, but potentially only *few* securities are helpful to curry favors to prospective employers. Hence, gauging the *net* effect of revolving doors requires analyzing the entire spectrum of securities rated by revolving analysts. In addition, the dataset should have two main features. First, as argued above, it needs to be a panel dataset with repeated performance measures at the individual analyst level in order to control for analyst heterogeneity. Such a dataset is not readily available, neither for monitors in general nor for credit analysts in particular.[9] To overcome this problem, I hand-collect a novel dataset that links individual analysts to the performance of the ratings they assign. Second, I need to be able to identify analysts who leave to investment banks after their employment at the rating agency. I collect this information from analysts' self-reported profiles on the professional networking website LinkedIn. The full dataset is described in more detail below.

My sample consists of all non-agency securitized finance securities issued in the U.S. and reported in SDC Platinum. Additional deal and tranche information is manually collected from Bloomberg. I restrict my sample to all issues between 2000

[9]Standard databases on corporate and securitized finance credit ratings (e.g., Mergent FISD, Bloomberg, or SDC Platinum) do not provide the identity of the individual lead analyst responsible for the rating by a given rating agency.

and 2010 that were initially rated by Moody's, because (i) data are sparse prior to 2000, (ii) my main measure of ratings accuracy requires three years of post-issuance performance data, and (iii) Moody's is the only rating agency that publicly discloses analyst names in the press release of a new rating on its website.[10] In addition to the name of the lead analyst responsible for the initial rating, I also collect data on subsequent rating changes for each security from Moody's website.

The securitized finance data are complemented with hand-collected biographical information from web searches; in the vast majority of cases from analysts' public profiles on LinkedIn. In particular, I gather information on the date when the analyst left Moody's, the identity of his first employer following the employment at Moody's, as well as information on previous employment, graduate, and undergraduate education. I am able to track a total of 229 analysts. As shown in Table 3.1, Panel B, 63 out of these 229 analysts subsequently go work for an investment bank that was ranked in the prestigious "The Bloomberg 20" ranking in the year prior to their exit,[11] 88 analysts leave to other employers, and 78 analysts have not left Moody's as of December 2013. The aforementioned investment banks also capture a large fraction of the underwriting market in securitized finance: they underwrite more than 80% of the securities in my sample (see Table 3.1, Panel C). As shown in Table 1.2, analysts with fewer years of prior work experience, no graduate degree, an undergraduate degree from an institution located in New York City, and a non-law undergraduate degree are more likely to leave to an investment bank. Interestingly, graduates from Ivy League institutions are less likely to subsequently work for an investment bank, although this relationship is not statistically significant.

As reported in Table 3.1, Panel A, my final dataset consists of 22,188 tranches from 4,520 securitized finance deals. All securities combined account for an ag-

[10] I am able to find corresponding analyst information from Moody's website in 71% of the cases.

[11] Since the ranking is only available from 2004 onwards and the composition of the ranked investment banks is fairly stable prior to 2008, I use the 2004 ranking to classify analyst exits prior to 2004. Figure 1.2 provides an overview of the top hiring banks in my sample. Table 1.4, Panel B, shows that my main findings are robust to alternative definitions of investment banks.

gregate issuance volume of ca. \$2.5 trillion, which represents at least 35% and therefore a sizable fraction of the aggregate U.S. non-agency securitized finance deal volume over this period reported by the Securities Industry and Financial Markets Association (SIFMA).[12] Using similar categories as in Griffin, Lowery, and Saretto (2014), I classify securities depending on the type of the deal's underlying collateral into eight collateral groups and three main market segments (asset-backed securities (ABS), mortgage-backed securities (MBS), and collateralized debt or loan obligations (CDO/CLO)). Classifying all securities by collateral type is important for my empirical approach of comparing performance across analysts, which is described in further detail below.

I also identify instances where analysts rate securities underwritten by their future employers by manually matching the name of the analyst's subsequent employer to the lead underwriting banks of the security reported in SDC Platinum. While it is not uncommon that analysts rate securities underwritten by their future employers, the majority of analysts who get hired by investment banks never rate securities of their future employers during their employment at Moody's (see Table 3.1, Panel B). As a result, securities underwritten by the future employer represent less than 7% of all securities rates by the average revolving analyst (see Table 3.1, Panel C).

1.3.1. Measuring and Comparing Analyst Performance

My main measure of rating (in)accuracy is the number of notches that the initial rating of a tranche has to be adjusted in the first three years after issuance, while controlling for observable tranche and deal characteristics. Defining accuracy based on subsequent rating actions is advantageous for two reasons. First, rating adjustments at Moody's are generally performed by a separate surveillance team and are

[12] Since SIFMA does not report agency asset-backed securities separately, I compute the aggregate deal volume as the sum of \$4.6 trillion of non-agency mortgage-backed securities and \$2.4 trillion of asset-backed securities (agency and non-agency). Hence, the 35% represent a lower bound estimate of the covered market share.

therefore not under the influence of the analyst who assigned the initial rating.[13] Second, credit rating agencies claim that their ratings are designed to be long-term and forward-looking in the sense that they should anticipate ups and downs of the business cycle.[14] Rating actions within the first few years after issuance, as opposed to longer horizons, can therefore be attributed to trends or events that might have reasonably been anticipated by the analyst at the time of issuance. In addition, my empirical approach described below circumvents the problem that subsequent ratings adjustments may be driven by changes in the fundamentals of the underlying collateral that could not have possibly been foreseen by the analyst at issuance.

Comparing rating performance across analysts is non-trivial because of potential non-random assignment of analysts to securities. For example, analysts often specialize in one or few collateral types, which may exhibit different patterns in performance. Even within a given collateral type and date, analysts may be assigned to securities with special characteristics, e.g., complex subordination structures or poor collateral quality. To circumvent this difficulty, I use the following two-step procedure. In a first step, I compute for each security the "abnormal" level of subsequent rating adjustments after controlling for observable differences in tranche and deal characteristics:

$$Rating\ Adjustment_j = \beta_1' D_j + \beta_2' X_j + \eta_j, \tag{1.8}$$

where $Rating\ Adjustment_j$ is the absolute difference (in notches) between the initial

[13]Michael Kanef, former head of the Asset Backed Finance Rating Group at Moodys Investors Service, testified before the U.S. Senate in 2007 that "monitoring is performed by a separate team of surveillance analysts who are not involved in the original rating of the securities, and who report to the chief credit officer of the Asset Finance Ratings Group". His testimony is available on the website of the U.S. Senate at `http://www.banking.senate.gov/public/index.cfm?FuseAction=Files.View&FileStore_id=e9c1a464-a73b-417a-a384-41c15315f8c2`.

[14]For example, Moody's writes the following about their approach to credit analysis: "As a rule of thumb, we are looking through the next economic cycle or longer. Because of this, our ratings are not intended to ratchet up and down with business or supply-demand cycles [...]" (available at `https://www.moodys.com/Pages/amr002003.aspx`).

rating of tranche j and the rating three years after issuance.[15] $D_j = (D_{Aaa}, D_{Aa1}, ..., D_C)$ is a vector of dummy variables indicating Moody's initial rating of the tranche, and X_j is a vector including tranche characteristics as well as characteristics of the corresponding deal. Tranche characteristics include the logarithm of the tranche principal value, level of subordination, weighted average life, coupon type, and an indicator equal to one if the tranche has an insurance wrap. Deal characteristics include the geographical concentration of the collateral, measured as the sum of the squared shares of the top five U.S. states in the deal's collateral as in He, Qian, and Strahan (2015), the level of overcollateralization, computed as the difference between the total collateral principal value and the combined principal value of the tranches as in Efing and Hau (2015), the weighted average loan-to-value (LTV) ratio and the weighted average credit score of the underlying collateral, the logarithm of the number of tranches in the deal, the logarithm of the average loan size (in USD), as well a vector of eight dummy variables marking the collateral type.[16] Controlling for this rich set of tranche and deal characteristics takes into account that some securities might be harder to rate and systematically face larger rating adjustments than others.

In a second step, I aggregate the residuals from the above regression into an (under)performance measure for each analyst i in a given collateral type z and semester t:

$$Inaccuracy_{izt} = \frac{1}{N} \sum_{j \in \mathcal{S}_{izt}} \widehat{\eta}_j \tag{1.9}$$

[15]In order to compute differences between ratings ("rating adjustments"), Moody's credit ratings are transformed into a cardinal scale, starting with 1 for Aaa and ending with 21 for C, as in Jorion, Liu, and Shi (2005). In my robustness tests reported in Table 1.4, Panel A, I consider rating adjustments over alternative horizons (one and five years) and find similar effects.

[16]Since information on some tranche and deal characteristics (specifically, the level of subordination, the weighted average life, insurance wrap, geographical concentration, LTV ratio, credit score, and average loan size) are available only for a subset of my data, I replace missing observations and include additional indicators equal to one if information on a given variable is not available. My robustness test in Table 1.4, Panel C, shows that my approach of replacing and controlling for missing observations does not affect my results. In fact, they get stronger if I restrict my sample to tranches with information on characteristics that are most commonly available.

Defining performance (or inaccuracy) at the analyst $\times$ collateral type level instead of at the analyst level allows me to compare analyst performance on a subset of products that are more similar in their economic fundamentals and has three key advantages. First, despite the similar overall time-series pattern, there are notable differences in rating performance across different collateral types at the same point in time (see Appendix, Figure A1.1). For example, whereas other collateral types have largely recovered after 2007, RMBS and CMBS ratings continue to underperform. It is therefore important to control for differences in the ratings performance of the overall collateral type when comparing performance across different analysts at Moody's, because they may not be fully captured by the observable tranche and deal characteristics included in the first-step regression. Second, Moody's internal organization structure follows a similar division (see Appendix, Figure A1.2), which ensures that analysts rating securities of the same collateral type face similar incentives, rating methodologies, and management leadership. Third, it allows me to exploit variations in the supply of investment banking jobs across different collateral types and investigate how they affect analyst performance (see Section 1.5). I will implement the idea of comparing analysts rating securities of *the same underlying collateral type* at the *same point in time* by regressing my measure of analyst inaccuracy on collateral type $\times$ semester fixed effects (see equation (1.10)).[17]

A potential concern about defining ratings accuracy based on subsequent adjustments is that it represents an ex-post measure of performance and cannot be observed in real time. Still, there may be good reasons to assume that investment banks observe signals about analyst performance that are unobservable to the econometrician but highly correlated with ex-post measures of performance. First, underwriting investment banks directly interact with rating analysts during the ratings process.

[17] While aggregating across all tranches rated by the same analyst in a given collateral type and semester has the advantage of reducing the influence of outliers, it is also possible to run my subsequent analysis at the individual deal level. The results, reported in Table A1.2, are both quantitatively and qualitatively very similar.

Second, they may receive signals through their social networks, e.g., other bankers who have directly worked with the analyst, former colleagues at Moody's, etc. While it is plausible that investment banks can observe signals of analyst performance, it is not a necessary condition to predict a positive incentive effect of revolving doors. As illustrated in my theoretical framework, assuming that the analyst's expected future pay at the investment bank is increasing in his skills as a credit rating analyst is sufficient for revolving doors to exert a positive influence on analysts' ex-ante incentives to enhance their qualifications.

Table 3.1, Panel C, reports descriptive statistics of my sample. Analyst inaccuracy, measured as the average abnormal 3-year rating adjustment of a given analyst in a given collateral type and semester, is roughly centered around zero and shows a substantial degree of variation, with a standard deviation of 4.3 notches.

1.3.2. Can Individual Analysts Influence Ratings?

A necessary condition for analyst incentives to play a role is that the ratings process for securitized finance products needs to provide sufficient room for individual analysts to affect the final rating of a security. This is not obvious given that the final rating decision is taken by a committee. Upon receiving a rating application from a potential customer, Moody's assigns a lead analyst to the ratings process. The lead analyst meets with the customer to discuss relevant information, which he subsequently analyzes with the help of Moody's analytical team. He then proposes a rating and provides a rationale to the rating committee, which consists of a number of credit risk professionals determined by the analyst. Once the rating committee has reached its decision, Moody's communicates the outcome to the customer and publishes a press release.[18] The ratings process at Moody's therefore provides ample opportunities for individual analysts to influence the final rating, even if the final decision is taken by a committee. Analysts guide meetings with the customer,

[18] See https://www.moodys.com/sites/products/ProductAttachments/mis_ratings_process.pdf for a description of the ratings process at Moody's.

request and interpret information, and play a key role in the rating committee by proposing and defending a rating recommendation based on their own analysis.

How much individual analysts are able to influence ratings is ultimately an empirical question. Fracassi, Petry, and Tate (2015) show that individual analysts are important for corporate bond ratings: they explain 30% of the within-firm variation in ratings. For securitized finance ratings, Griffin and Tang (2012) provide evidence that CDO ratings by a major credit rating agency frequently deviated from the agency's main model. Note that if individual analysts played no role in the ratings process, this would bias me against finding any significant differences in my across-analyst comparisons.

1.4. Main Results

This section presents my main results. I document that analysts who subsequently get hired by investment banks produce systematically more accurate ratings, as predicted by the human capital view of revolving doors. This difference in performance is robust to various measures of ratings accuracy, and is larger for complex securities where analyst effort should matter more. Additional tests confirm the interpretation that revolving analysts outperform because of enhanced effort.

1.4.1. Baseline Results

In order to gauge whether revolving doors strengthen or weaken analyst incentives to issue accurate ratings, I compare the performance of revolving and non-revolving analysts as follows. I first estimate analyst performance (or inaccuracy) in a given collateral type and semester using the two-step procedure described in Section 1.3.1. Then I regress this measure of analyst inaccuracy on an indicator equal to one if the analyst leaves to an investment bank within the next two semesters ($IB\,Exit_{i,t+1yr}$):

$$Inaccuracy_{izt} = \lambda_i + \lambda_{zt} + \delta IB\,Exit_{i,t+1yr} + \beta' X_{izt} + \epsilon_{izt}, \tag{1.10}$$

where $Inaccuracy_{izt}$ stands for the average inaccuracy of all tranche ratings issued by analyst i in collateral type z and semester t. λ_i and λ_{zt} are analyst and collateral type $\times$ semester fixed effects, respectively, and X_{izt} represents a vector of additional controls. Specifically, X_{izt} comprises the logarithm of the total number of deals rated by analyst i in collateral type z and semester t, the logarithm of one plus the analyst's tenure at Moody's (in semesters), the fraction of tranches underwritten by investment banks rated in "The Bloomberg 20" ranking,[19] as well as the average issuer market share.[20] All variables are defined in the Appendix. Note that since the dependent variable is analyst inaccuracy, the human capital view predicts $\delta < 0$ in the above regression. Standard errors are clustered at the analyst level.

Table 2.4, Panel A, reports the results. For comparison purposes, I also report results excluding analyst fixed effects in columns (1) and (2). Confirming the results from the simple sorts presented in Figure 1.1, analysts who leave Moody's to go work for an investment bank are on average 0.46 notches more accurate than other analysts rating tranches in the same collateral type and semester. When focusing on analyst performance during the last two semesters prior to the departure to the investment bank and including analyst fixed effects (columns (3) and (4)), the performance gap increases to 1.31 notches. This effect corresponds to 30% (= 1.310/4.34) of one standard deviation in analyst inaccuracy and is therefore economically sizable.

It is possible that, despite their aggregate outperformance, revolving analysts underperform on a subset of securities that are underwritten by their future employers. In order to test for the presence of such a potential bias, I interact the $IB\,Exit$ indicator with the fraction of tranches underwritten by the analyst's future employer. My coefficient estimates, reported in Panel B of Table 2.4, imply that revolving analysts underperform by 0.53 notches in the extreme case where all tranches rated by

[19]Griffin, Lowery, and Saretto (2014) show that securities issued by high-reputation investment banks have higher default rates.

[20]He, Qian, and Strahan (2012) show that a larger issuer market share is associated with worse tranche performance.

the analyst are underwritten by his future employer (see column (4)).[21] This finding is consistent with evidence reported by Cornaggia, Cornaggia, and Xia (2015), who document that analysts give more favorable ratings to their future employers in the last quarters before their departure. However, securities underwritten by the future employer constitute less than 7% (see Table 3.1, Panel C) and therefore a small fraction of all securities rated by the average revolving analyst. Hence, this reduced accuracy is dominated by revolving analysts' outperformance on other securities. In addition, prior to the last year of their employment with Moody's, analysts who go work for investment banks are 1.36 notches more accurate on the securities of their future employers.

One may argue that an increase in performance prior to analyst departure is not specific to analyst transitions to investment banks but could be observed for any other employment transfer. To test this argument, I perform a placebo test using analysts who depart to other employers. In Panel C of Table 2.4, I find that analysts who depart to other employers perform, if anything, worse than other analysts during the last year of their employment at Moody's. This suggests that the possibility to go work for other employers is no perfect substitute for the possibility to be hired by an investment bank. A potential explanation is that credit rating skill may be particularly valuable for tasks required by the investment banks, such as structuring securitized finance deals ahead of public offerings, or that investment banks may have a superior skill in observing and evaluating the performance of analysts while they are employed at the rating agency.[22]

In sum, the results presented in this section show that analysts who subsequently get hired by investment banks systematically produce more accurate ratings, consis-

[21] This point estimate is not statistically significantly different from zero.

[22] Such a special role of investment banks may be justified by the fact that rating analysts in securitized finance work very closely together with underwriting investment banks, as illustrated by Cetorelli and Peristiani (2012). When further refining the set of other employers, I observe an outperformance of similar magnitude for analysts who transfer to asset managers such as mutual funds or hedge funds (see Table A1.3). However, given the small sample size of only 20 analyst transitions to asset managers, I cannot conclude that this outperformance is statistically significant.

tent with the human capital view of revolving doors. In the following, I show that these results are robust to alternative measures of ratings accuracy and definitions of analyst departures to investment banks.

1.4.2. Robustness

Table 1.4 presents a number of robustness tests. Unless otherwise mentioned, I report results for the specification in Table 2.4, Panel A, column (4), and suppress all control variables for brevity. Panel A shows results for alternative measures of analyst performance. First, I aggregate tranches within each analyst and collateral type by value-weighting tranches by their principal amount instead of equal-weighting (see equation (1.9)), which produces economically similar estimates. As mentioned in the introduction, an attractive institutional feature of Moody's organization is that subsequent rating adjustments are performed by a separate surveillance team and are therefore not under the influence of the analyst who is responsible for the initial rating. In order to rule out potential exceptions to this rule, I compute a measure of analyst inaccuracy using only subsequent rating actions performed by different analysts than the one responsible for the initial rating.[23] The resulting estimates are very similar to my baseline, suggesting that the effect cannot be explained by bias in the ex-post adjustment of the initial ratings issued by revolving and non-revolving analysts. While effects are somewhat smaller if I look at rating adjustments over the first year of issuance only, they are similar when looking at a five-year horizon. Ratings by revolving analysts have both fewer downgrades and upgrades, but the effect is almost three times larger for downgrades. Hence, revolving analysts are not only more accurate, they also tend to be more pessimistic. I also see that securities rated by revolving analysts are less likely to be downgraded to default – a rating action that is typically tied to hard events such as covenant violations (see Griffin, Lowery, and Saretto (2014)) and therefore less subjective than other

[23] Since there are very few exceptions to the rule of assigning a separate surveillance analyst in my sample, I obtain a correlation coefficient of more than 98% between the two inaccuracy measures.

rating adjustments. Next, I measure ratings accuracy based on abnormal cumulative tranche losses over three years, which dramatically reduces the sample size but yields a result of similar economic magnitude. Abnormal cumulative tranche losses are computed as the absolute difference between the realized tranche loss and Moody's expected loss benchmark for the same rating category (see Moody's Investor Service (2001)). This result is very important for two reasons. First, since it does not rely on any adjustment for tranche characteristics, it shows that my results are not sensitive to the linear model for rating adjustments in equation (1.8). Second, cumulative tranche losses represent a measure of rating performance that does not require action on behalf of Moody's surveillance team. Finally, I also test two proxies of ratings accuracy that can be measured in real time. First, I use an indicator equal to one if the average tranche rated by the analyst has been rated by more than two rating agencies as a proxy for rating quality. The motivation for this measure is that tranche ratings by all three agencies are less likely to be shopped (see, for example, He, Qian, and Strahan (2015)). Consistent with my main finding that ratings issued by analysts who leave to investment banks are more accurate, they are also less likely to be shopped. Second, I show that the average initial yield of AAA-tranches rated by revolving analysts tend to be lower, suggesting that investors recognize their higher quality.

Panel B shows that I obtain very similar results if I consider alternative definitions for my key independent variable of interest, $IB\,Exit_{i,t+1yr}$. In the first two rows, I change the time horizon prior to the analyst's departure to six months and two years, respectively. The resulting estimates are very similar to my baseline coefficient. In the next two rows, I use alternative definitions for the set of hiring investment banks. Departures to investment banks in "The Vault Banking 50" ranking by prestige[24]

[24]Since "The Vault Banking 50" ranking by prestige is available only from 2008 onwards and comparable rankings are fairly stable prior to that year, I use the 2008 ranking, which is available at `http://www.vault.com/company-rankings/banking/most-prestigious-banking-companies/?sRankID=162&rYear=2008`.

and departures to the former five pure-play investment banks[25] yield similar, though statistically somewhat weaker results. In order to address potential concerns that my results may be specific to tranches issued during or shortly before the crisis, I show in Panel C that my findings survive if I only include tranche ratings issued before 2006. When I restrict my sample to tranches with complete information on the most commonly available tranche characteristics included in equation (1.8), the statistical significance of my results increases. Panel D shows that my results are not sensitive to the estimation method. A propensity score matching approach yields very similar estimates.

To sum up, I conclude that my main result is robust to various measures of analyst performance and definitions of analyst departures to investment banks.

1.4.3. The Influence of Deal Complexity

If my previous results are driven by enhanced rating effort by analysts who aspire to work for an investment bank, then one would expect the marginal impact of their additional effort to be larger for deals that are harder to rate. This section tests this hypothesis by interacting my main independent variable of interest, $IB\,Exit_{i,t+1yr}$, with different measures of average deal complexity.

Table 1.5 reports the results for different proxies for deal complexity. The first proxy is the average fraction of loans with low documentation, since it is arguably more challenging to rate deals with less tangible information about the quality of the loans in the underlying collateral. The second measure is the absolute skewness of the credit score distribution of the underlying loans. Anecdotal evidence reported by Lewis (2011) suggests that one of the many factors why securitized finance ratings were off-target was that they focused too much on average credit scores rather than on their full distribution. More diligent analysts may have taken the skewness of the

[25] The former five pure-play investment banks include Bear Stearns, Goldman Sachs, Lehman Brothers, Merrill Lynch, and Morgan Stanley.

underlying credit score distribution into account in their rating recommendation. The third proxy is the deal complexity measure proposed by He, Qian, and Strahan (2015) ("HQS"), which is computed as the number of tranches in a deal divided by the combined principal amount of the tranches.

All measures indicate that revolving analysts outperform more when they rate more complex deals. A one-standard-deviation increase in the average fraction of low-documentation loans increases the outperformance of revolving analysts by 0.6 ($= -2.494 \times 0.24$) notches, and a one-standard-deviation increase in the average absolute skewness of the credit score distribution increases their outperformance by 0.7 ($= -6.200 \times 0.12$) notches. While the interaction term in column (3) is not statistically significant, my estimates indicate that a one-standard-deviation increase in average deal complexity leads to an economically sizable increase in the performance gap of 1.1 ($= -2.595 \times 0.42$) notches. Overall, the results are consistent with the intuition that enhanced rating effort should matter more for deals that are harder to rate.

1.4.4. Alternative Explanations

My approach of comparing analyst performance both within the same analyst as well as across analysts rating similar securities at the same point in time rules out the impact of a number of potentially confounding factors suggested by the prior literature, most notably analyst baseline skill and non-random assignment of analysts to securities. In this section, I address two potential alternative explanations for my main result that analysts who get hired by investment banks outperform. First, could there be unobserved differences in learning across analysts? Second, could my results be driven by disincentives within Moody's organization rather than by positive incentives from revolving doors?

Unobserved Differences in Learning

Heterogeneity across analysts can lead to unobserved differences in the speed at which analysts learn. Hence, a potential concern could be that analysts who get hired by investment banks outperform because they have been learning at a faster rate than other analysts. Note that such a differential learning story would still be inconsistent with the collusion view and support the view of revolving doors as an economic mechanism that allocates skill to jobs with higher returns to skill. However, unlike the human capital view, it does not predict that rating analysts work harder in the presence of revolving doors. In this section, I present two pieces of evidence which are supportive of the human capital view and less consistent with unobserved differences in analyst learning.

First, a differential learning story would predict that revolving analysts gradually start to outperform over their tenure at the rating agency. To test this prediction, I split the observations of revolving analysts by the remaining time until their departure to the investment bank. Rather than a gradual improvement in performance, I observe a large and sudden increase in the performance of revolving analysts shortly before their departure (see Table 1.6, Panel A). There is no economically or statistically significant difference in performance during the early and middle stages of their tenure at Moody's. To further illustrate that revolving analysts outperform only shortly before their transition, I perform a placebo test where I replace the analyst's actual departure date with a random date between the start and end date of his employment at Moody's. Then I re-run the baseline regression presented in Table 2.4, Panel A, column (4), and obtain a placebo coefficient. Figure A1.3 plots the distribution of placebo coefficients after 1,000 runs. The null hypothesis that the baseline coefficient is drawn from the distribution of placebo coefficients is rejected at the 1% level.

Second, if analysts get hired by investment banks because they have been on an accelerated learning path, then one would expect the outperformance of revolving analysts during their last year to be attenuated if their tenure at Moody's has been very long. The incentive story, on the other hand, would predict outperformance to increase prior to the analyst's departure irrespective of the analyst's tenure. To test these different predictions, I repeat the analysis presented in Table 2.4, Panel A, column (4), by categorizing revolving analysts based on their tenure at the time of their departure to the investment bank. As reported in Table 1.6, Panel B, the outperformance of revolving analysts during their last year of employment at Moody's remains high even for the quartile of analysts with the longest tenure at exit, who have been with Moody's for ca. fourteen years.

Disincentives at Moody's

A second potential concern could be that my results reflect disincentives within Moody's organization as opposed to positive incentives from revolving doors. For example, if Moody's organization was strongly focused on expanding the company's market share, as suggested in the report by the Financial Crisis Inquiry Commission (2011),[26] it may have punished analysts who issued accurate ratings by not promoting them or by withholding their bonus. This interpretation cannot explain why accurate analysts may choose to seek employment elsewhere. However, it does not explain why only analysts hired by investment banks outperform and not the average analyst who transitions to other employers. The evidence reported in Table 2.4, Panel C, is therefore not consistent with this story.

To further investigate this potential concern, I look at the relationship between analyst performance and internal promotions at Moody's. I identify promotions based on changes in the analyst's job title mentioned in the press releases from Moody's website. The results, shown in Table 1.7, do not support the conjecture that Moody's punishes analysts for being accurate. Analyst who get promoted at

[26]The Financial Crisis Inquiry Commission (2011) reports that "a strong emphasis on market share was evident in employee performance evaluations" at Moody's.

Moody's are on average more accurate than other analysts rating similar securities at the same point in time. However, the relationship between performance and internal promotions is substantially weaker, both in economic and statistical terms, than the previously documented relationship between performance and departures to investment banks.

1.5. Variation in the Supply of Investment Banking Jobs

In this section, I provide additional evidence for the human capital view of revolving doors by exploiting how variation in the supply of investment banking jobs affects analyst performance. This complimentary approach is advantageous because changes in the supply of investment banking jobs provide exogenous shocks to the probability of an analyst to be hired by investment banks. Most importantly, they are unrelated to analysts' individual baseline skill, learning paths, and other career concerns.

I use the event of a new underwriting investment bank entering a collateral group as a shock to the supply of investment banking jobs. This event is useful for identifying the effect of changes in the supply of investment banking jobs for two reasons. First, since an investment bank may choose to enter only one collateral group at a time and not others, I can compare how the performance of analysts in that collateral group changes relative to the performance of analysts in other collateral groups that are not affected. Second, I can exploit whether, in the cross-section of analysts within the same collateral group, analysts with certain characteristics are more affected by the event than others. Specifically, my theoretical framework predicts that low-ability analysts and, more generally, analysts who are ex-ante less likely to leave to investment banks should be less affected by fluctuations in the supply of investment banking jobs (see Section 1.2.2). Exploiting these cross-sectional differences is important in order to rule out that my findings are driven by unobservable factors that are driving both analyst performance and investment bank entry (e.g., economic fundamentals), or by other changes that are directly induced by the entry of a new investment bank (e.g., underwriter competition, average analyst work load).

The following thought experiment illustrates my empirical approach. Consider two collateral groups, Student-loan ABS and Auto-loan ABS. Suppose now that an investment bank – called Goldman – starts to underwrite securities in Student-loan ABS but remains absent in Auto-loan ABS. My conjecture is that this event is going to increase the supply of investment banking jobs in the area of Student-loan ABS, both from Goldman as well as from other investment banks who may decide to follow, and thus the likelihood for analysts rating Student-loan ABS at Moody's to transition to an investment bank in the near future. In contrast, and by construction, the supply of investment banking jobs in Auto-loan ABS is not affected. I can therefore identify the impact of changes in the likelihood of being hired by an investment bank on analyst incentives by analyzing changes in the performance of analysts in Student-loan ABS and in Auto-loan ABS around the time of the investment bank entry.

To identify collateral group and semester observations where a new underwriting investment bank enters the market, I use the following approach. Using all non-agency U.S. securitized finance securities reported in SDC Platinum and assigning them to the eight collateral groups listed in Table 3.1, Panel A, I consider a collateral group to undergo an investment bank entry event in a given semester if an investment bank starts underwriting securities in that collateral group for the first time.[27] This yields 37 investment bank entry events. In order to verify that these events are indeed associated with an increase in the supply of investment banking jobs, I plot the difference in the average number of analysts who depart to investment banks

[27] I consider as investment bank underwriters all underwriters that at some point during my sample period appear in "The Bloomberg 20" investment bank ranking.

between the event collateral group and the control collateral group in event time. As shown in Figure 1.3, the number of analyst departures jumps significantly in the semester where an investment bank enters a new collateral group and remains elevated for the three following semesters. This pattern suggests that the entry of an investment bank is indeed a good proxy for more aggressive hiring by investment banks.

Next, I look at how analyst performance changes around the event. To this end, I regress my main measure of ratings inaccuracy on a set of nine event-time dummy variables labeled $t-4$, $t-3$, ..., $t+3$, $t+4$, where my convention is that dummy t takes on the value one in the collateral group and semester in which an investment bank entry event occurs. Since the event-time dummies do not vary within the same collateral type and semester, I only include market segment $\times$ semester fixed effects in this part of the analysis, in addition to analyst fixed effects and the same control variables as in Table 2.4, Panel A. Table 1.8, Panel A, and the red line in Figure 1.3 show the results. Two things are worth noticing. First, analysts in the event group outperform those in the control group between semesters $t-3$ to $t+2$, but perform similarly at the very beginning and at the very end of the event window. Second, and consistent with analysts anticipating the investment bank entry and the associated higher chances to move to investment banking, analyst performance starts to increase a few semesters before the event, reaches its peak in $t-1$, and then falls back to normal levels.[28]

Next, I investigate whether the increase in the likelihood of being hired by investment banks affects the performance of some analysts more than others. Specifically, the performance of analysts who are ex-ante less likely to move to an investment

[28]The finding that analysts are able to anticipate the investment bank entry is not surprising. According to former rating analysts, it usually takes several months to complete a ratings process, which means that analysts at Moody's who are working on the new deal will know about the investment bank entry well in advance. In addition, analysts might learn about the plans of an investment bank to enter a new collateral group even before that, either through talks with investment bankers, or through the media.

bank, such as analysts with low baseline ability, and analysts whose career path depends less on their ratings performance, should be less sensitive to changes in the outside option. In order to test this prediction, I use three criteria to separate analysts who should be ex-ante more or less likely to react to changes in the supply of investment banking jobs. The first proxy is a measure of analyst baseline ability, and is equal to the analyst's performance in the past two semesters. The intuition for this proxy is that, as discussed in my theoretical framework, analysts with low innate ability never choose to apply for investment banking jobs because their expected returns would never be high enough to cover their career switching cost. Next, I use the predicted values from the Probit regression of $IB\,Exit$ on ex-ante analyst characteristics presented in Table 1.2, column (1), as a measure of the analyst's ex-ante likelihood of switching career. The third proxy looks at the analyst's professional network. My conjecture is that analysts with weak professional networks need to rely more on showcasing their skill in order to obtain a job in investment banking, compared to analysts with strong professional networks. I use an indicator equal to one for analysts at Moody's who are working in the same country as the country of their most recent educational institution as a proxy for strong professional networks.

I regress analyst inaccuracy on an indicator *Event* $(-2, 0)$, which is equal to one in event semesters $t-2$ to $t=0$ in order to capture anticipation effects. Then I use the three proxies described above to perform sample splits. Table 1.8, Panel B, reports results. The results strongly confirm my hypothesis that the observed improvement in analyst performance is driven by enhanced analyst incentives due to better prospects of pursuing an investment banking career. First, as predicted by my theoretical framework, analysts with weak past performance do not outperform the control group (see columns (1) and (2)). Second, the outperformance is economically larger for analysts who are ex-ante more likely to leave to investment banking (columns (3) and (4)). Third, the outperformance is stronger for analysts with a weaker professional network, who arguably need to rely more on signalling their expertise in order to advance their career (columns (5) and (6)).

1.6. Conclusion

My paper contributes to the ongoing debate whether revolving doors strengthen or distort monitoring incentives. I hand-collect a novel dataset that links 229 individual credit rating analysts at Moody's to their career paths and to the quality of the ratings they assign. In contrast with the generally negative view on revolving doors, I find that credit analysts who are subsequently hired by investment banks are more accurate than other analysts rating similar securities at the same point in time. A notable exception is the small subset of securities that are underwritten by their future employers where they do not outperform. The results suggest that, because only few ratings are helpful to curry favors to future employers, but almost all ratings are helpful in signaling skill or building expertise, the positive effects of revolving doors can be economically sizable. They may also explain why, despite the frequently voiced concerns, revolving doors have remained open in most professions.

My paper also contributes to the debate about the sources of poor performance of securitized finance ratings prior to the financial crisis. Many observers have identified conflicted individual analysts as one of the drivers of poor ratings accuracy, and regulators have responded by imposing enhanced disclosure requirements on rating agencies in cases where employees transfer to a previously rated entity. My results imply that conflicts at the *individual* analyst level were unlikely a main driver of poor ratings performance and that, if anything, analysts may have performed better because of the possibility to be hired by an investment bank. Restricting the revolving door may therefore have the undesirable effect of discouraging rating analysts from developing and showcasing their expertise while employed at the rating agency.

While this paper focuses on the effects on performance incentives, revolving doors may affect monitoring quality through additional channels. For example, credit ratings quality may suffer if rating agencies systematically lose their more experienced or talented staff to investment banks, reducing their incentives to train new analysts (see Bar-Isaac and Shapiro (2011)). In addition, former analysts may help investment banks to game the rating system once they have left the rating agency.[29] On the other hand, there may also be additional positive effects of revolving doors that I am not capturing in my analysis. For example, the option for rating analysts to move to investment banking may positively affect the quality of the pool of applicants for positions at rating agencies, and many motivated applicants may no longer apply if career mobility is reduced. I leave the exploration of these additional channels to future research.

[29]Recent evidence reported by Jiang, Wang, and Wang (2015) supports this possibility.

Tables

Table 1.1: Summary Statistics

The table presents summary statistics for my sample, which comprises all U.S. non-agency securitized finance deals rated by Moody's between 2000 and 2010 with information identifying the lead analyst at issuance and information on the analyst's post-Moody's employment status. Panel A shows the breakdown of securities by collateral type. Panel B provides an overview of the subsequent career paths of the analysts in my sample and the number of analysts who, at some point during their employment at Moody's, rate securities underwritten by their future employers. Panel C reports descriptive statistics of key variables. Analyst performance is computed at the analyst × collateral type level in a two-step procedure using equations (1.8) and (1.9), i.e., one observation in my dataset refers to one analyst and collateral type and semester. A complete list of variable definitions is provided in the Appendix.

Panel A: Sample

	Number of Tranches	Number of Deals	Issuance Volume ($bn)
Segment: ABS			
ABS Auto	1,784	506	404
ABS Card	420	216	162
ABS Home	3,656	720	323
ABS Student	141	38	22
ABS Other	4,416	980	514
Segment: MBS			
CMBS	509	63	67
RMBS	10,361	1,726	914
Segment: CDO/CLO			
CDO	901	271	66
Total	22,188	4,520	2,473

Panel B: Number of Analysts By Subsequent Career Path

	All	No Exit	IB Exit	Other Exit			
				Other Bank	Asset Mgr.	Insurer	Other
Number of analysts	229	78	63	28	20	11	29
o/w rate future employer	26	0	26	0	0	0	0

Panel C: Descriptive Statistics

	N	Mean	Std. Dev.	0.25	Median	0.75
Dependent Variables						
Analyst Inaccuracy	1,476	0.60	4.34	-1.94	-0.76	1.18
Key Independent Variables						
IB Exit	1,476	0.29	0.45	0.00	0.00	1.00
IB Exit$_{t+1yr}$	1,476	0.08	0.27	0.00	0.00	0.00
Other Exit	1,476	0.34	0.47	0.00	0.00	1.00
Other Exit$_{t+1yr}$	1,476	0.08	0.27	0.00	0.00	0.00
Future Employer	1,476	0.02	0.11	0.00	0.00	0.00
Future Employer | IB Exit	427	0.07	0.20	0.00	0.00	0.00
Control variables						
Tenure	1,476	4.76	5.37	1.00	3.00	7.00
Number of deals	1,476	3.09	3.20	1.00	2.00	4.00
IB Underwriter	1,476	0.80	0.34	0.71	1.00	1.00
Issuer Market Share (in %)	1,476	0.65	0.99	0.00	0.30	0.87

Table 1.2: Predicting Analyst Departures to Investment Banks

The table reports results on the characteristics of analysts who depart to investment banks. *IB Exit* is an indicator equal to one if the analyst departs to an investment bank that was ranked in "The Bloomberg 20" ranking in the year prior to his departure, and is regressed on various analyst characteristics using a Probit model. *Prior Work Experience* refers to the logarithm of one plus the number of years of prior work experience, *Graduate Degree* is an indicator equal to one if the analyst has obtained a graduate degree prior to joining Moody's, *NYC Undergrad* indicates whether the analyst has obtained his undergraduate degree from an institution located in New York City, and *Ivy League* indicates whether the analyst has obtained his most recent degree prior to joining Moody's at an Ivy League institution. *Law Degree* and *Tech Degree* are indicators if the analyst's undergraduate degree is in law or in a technical field (mathematics / engineering / physics / computer science), respectively. In column (2), dummies indicating the calendar year of the begin of the analyst's employment with Moody's are included. Robust t-statistics are reported in parentheses.

	IB Exit	
	(1)	(2)
Female	-0.371	-0.630
	(-1.01)	(-1.40)
Prior Work Experience	-3.039	-3.508
	(-3.51)	(-2.94)
Graduate Degree	-0.941	-1.369
	(-2.44)	(-2.61)
NYC Undergrad	1.032	2.064
	(2.14)	(2.80)
Ivy League	-0.551	-0.735
	(-1.17)	(-1.15)
Law Degree	-0.832	-1.106
	(-1.42)	(-1.81)
Tech Degree	0.110	0.566
	(0.26)	(0.99)
Cohort dummies	No	Yes
N	93	73
Pseudo-R^2	0.252	0.339

Table 1.3: Analyst Performance and Departures to Investment Banks

The table reports results from regressing analyst inaccuracy on an indicator for analyst departures to investment banks. In columns (1) and (2), *IB Exit* is an indicator equal to one if the analyst eventually departs to an investment bank that was ranked in "The Bloomberg 20" ranking in the year prior to his departure. In columns (3) and (4), *IB Exit*$_{t+1yr}$ is an indicator equal to one only during the last two semesters of the analyst's employment at Moody's. Panel A presents baseline results. Panel B presents results for the interaction with the fraction of tranches that are underwritten by the analyst's future employer. Panel C reports results from a placebo test where *Other Exit* refers to analyst departures to other employers. All variables are defined in A3.1. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

Panel A: Baseline

	Analyst Inaccuracy			
	(1)	(2)	(3)	(4)
IB Exit	-0.456	-0.457		
	(-2.54)	(-2.52)		
IB Exit$_{t+1yr}$			-1.262	-1.310
			(-2.67)	(-2.76)
Tenure		0.011		0.491
		(0.11)		(1.38)
No. of deals		0.080		0.100
		(0.73)		(0.80)
IB underwriter		-0.067		-0.050
		(-0.24)		(-0.16)
Issuer market share		-0.058		-0.108
		(-0.69)		(-1.29)
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	No	No	Yes	Yes
N	1,476	1,476	1,476	1,476
R^2	0.675	0.675	0.764	0.764

Panel B: Interaction with Fraction of Tranches Underwritten by Future Employer

	Analyst Inaccuracy			
	(1)	(2)	(3)	(4)
IB Exit	-0.459	-0.460		
	(-2.48)	(-2.45)		
IB Exit × Future Employer	0.057	0.044		
	(0.08)	(0.06)		
IB Exit$_{t+1yr}$			-1.312	-1.361
			(-2.74)	(-2.83)
IB Exit$_{t+1yr}$ × Future Employer			1.850	1.892
			(1.55)	(1.57)
Future Employer			-1.339	-1.355
			(-1.84)	(-1.82)
Controls included	No	Yes	No	Yes
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	No	No	Yes	Yes
N	1,476	1,476	1,476	1,476
R^2	0.675	0.675	0.764	0.764

Panel C: Placebo Test with Departures to Other Employers

	Analyst Inaccuracy			
	(1)	(2)	(3)	(4)
Other Exit	0.339	0.344		
	(1.79)	(1.81)		
Other Exit$_{t+1yr}$			0.496	0.447
			(1.23)	(1.10)
Controls included	No	Yes	No	Yes
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	No	No	Yes	Yes
N	1,476	1,476	1,476	1,476
R^2	0.674	0.674	0.762	0.762

Table 1.4: Robustness

The table presents robustness tests. The baseline regression refers to column (4) from Table 2.4, Panel A. For brevity I only report coefficients of interest and suppress control variables. Economic effects are calculated as the reported coefficient times the standard deviation of the independent variable, divided by the standard deviation of the dependent variable. Panel A tests alternative measures of ratings accuracy. In the first line, I value-weight tranches by their principal amount instead of using equal weights as in equation (1.9). In the second line, I exclude all subsequent rating adjustments that are performed by the analyst responsible for the initial rating. *1(5)-yr Abnormal Rating Adjustment* refers to rating adjustments over a one and five-year horizon, respectively. Securities are considered as in *default* when Moody's assigns a rating below Ca within the first three years after issuance. For the next two measures, I use only rating downgrades or upgrades as opposed to all rating adjustments. *Abnormal cumulative losses* are computed as the absolute difference between the tranche's cumulative losses after three years and Moody's expected loss benchmark for the initial tranche rating category. > 2 *Initial Ratings* is an indicator equal to one if the tranches rated by the analyst are on average rated by more than two of the three major rating agencies. *Initial yield* is computed following He, Qian, and Strahan (2015). In Panel B, I use alternative definitions for departures to investment banks. $\textit{IB Exit}_{t+6m}$ and $\textit{IB Exit}_{t+2yrs}$ refer to departures to "The Bloomberg 20" investment banks in the following 6 months or 2 years, respectively. *Exits to "The Vault 50" Investment Banks* are analyst departures to investment banks ranked in "The Vault 50" ranking by prestige in 2008. *Exits to 5 Pure-Play Investment Banks* refer to exits to Bear Stearns, Goldman Sachs, Lehman Brothers, Merrill Lynch, or Morgan Stanley. In Panel C, first line, I exclude tranche ratings issued after 2005. In the second line, I include only tranches with complete information on the tranche principal amount, level of subordination, weighted average life, overcollateralization, insurance wrap, number of bonds in the deal, and coupon type. In Panel D, I report results from a propensity score matching procedure that matches each analyst who departs to an investment bank in the next year to his three nearest neighbors who rate securities in the same collateral type and semester, using the control variables from Table 2.4. I also report results from the same matching procedure while adding the analyst's performance over the previous two semesters to the set of matching variables.

	Coefficient	t-statistic	N	Econ. Effect
Baseline	-1.310	(-2.76)	1,476	-30.2%
Panel A: Alternative Measures of Analyst (In)Accuracy				
Baseline, value-weighted	-1.013	(-2.11)	1,476	-24.4%
Baseline, excl. adjustments by initial analyst	-1.380	(-3.10)	1,476	-31.8%
1-yr Abn. Rating Adjustment	-0.096	(-1.85)	1,476	-10.6%
5-yr Abn. Rating Adjustment	-1.430	(-2.98)	1,476	-33.8%
3-yr Abn. Downgrades	-1.340	(-2.83)	1,476	-30.9%
3-yr Abn. Upgrades	-0.024	(-0.95)	1,476	-13.0%
3-yr Abn. Default	-0.056	(-2.30)	1,476	-24.6%
3-yr Abn. Cumulative Losses	-1.478	(-1.34)	412	-20.7%
>2 Initial Ratings	0.114	(1.71)	1,476	22.8%
Initial Yield on AAA Tranches	-0.127	(-1.04)	759	-14.2%
Panel B: Alternative Definitions of IB Exit				
IB Exit_{t+6m}	-1.174	(-1.91)	1,476	-27.1%
IB Exit_{t+2yrs}	-0.996	(-2.17)	1,476	-23.0%
Exits to “The Vault 50” Investment Banks	-1.205	(-2.04)	1,476	-27.8%
Exits to 5 Pure-Play Investment Banks	-1.305	(-1.82)	1,476	-30.1%
Panel C: Sample Restrictions				
Drop tranches issued after 2005	-0.831	(-2.55)	1,058	-19.2%
Drop tranches with missing deal characteristics	-1.364	(-3.69)	764	-23.9%
Panel D: Estimation Method				
Propensity score matching	-0.779	(-1.92)	1,476	-18.0%
Propensity score matching, incl. past performance	-1.101	(-2.21)	952	-25.4%

Table 1.5: The Influence of Deal Complexity

The table presents results for interactions with proxies for average deal complexity. *Low Documentation* refers to the average percentage of loans with less than full documentation in the underlying collateral. *Absolute Credit Score Skewness* and refers to the absolute skewness of the credit score distribution of the loans in the underlying collateral. In column (3), deal complexity is computed as in He, Qian, and Strahan (2015) as the number of tranches in the deal divided by their combined principal amount. *t*-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

	Analyst Inaccuracy		
	Low Documentation	Abs. Credit Score Skewness	Deal Complexity (HQS)
	(1)	(2)	(3)
IB Exit$_{t+1yr}$	-0.001	-0.208	-0.895
	(0.00)	(-0.38)	(-1.74)
IB Exit$_{t+1yr}$ $\times$ Deal Complex.	-2.494	-6.200	-2.595
	(-2.04)	(-2.51)	(-1.42)
Deal Complexity	1.106	1.200	-0.449
	(1.69)	(0.90)	(-2.45)
Tenure	-0.146	-0.106	0.481
	(-0.30)	(-0.23)	(1.37)
No. of deals	0.357	0.502	0.105
	(2.03)	(2.88)	(0.84)
IB underwriter	-0.373	-0.171	-0.090
	(-0.72)	(-0.31)	(-0.29)
Issuer market share	-0.144	-0.012	-0.082
	(-1.22)	(-0.11)	(-0.99)
Collateral type $\times$ semester f.e.	Yes	Yes	Yes
Analyst f.e.	Yes	Yes	Yes
N	670	591	1,476
R^2	0.842	0.865	0.768

Table 1.6: Analyst Performance by Time Until Departure and Tenure at Departure

The table presents results for different subsamples of analysts who depart to investment banks. In Panel A, observations of departing analysts are grouped into quartiles based on the remaining time until their departure, and, in Panel B, based on their tenure at the time of departure. Quartiles are formed within a given calendar year. *t*-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

Panel A: Subsamples by remaining time until analyst departure

	Analyst Inaccuracy			
Quartile	Q1	Q2	Q3	Q4
Avg. time until departure (in years)	0.5	1.4	2.8	5.3
	(1)	(2)	(3)	(4)
IB Exit	-1.051	-0.368	-0.156	-0.011
	(-3.04)	(-1.06)	(-0.82)	(-0.04)
Tenure	0.082	0.143	0.170	0.169
	(0.67)	(1.11)	(1.40)	(1.37)
No. of deals	0.106	0.038	0.025	0.030
	(0.79)	(0.27)	(0.18)	(0.22)
IB underwriter	-0.211	-0.064	-0.163	-0.175
	(-0.62)	(-0.18)	(-0.48)	(-0.50)
Issuer market share	0.044	-0.004	-0.044	-0.037
	(0.49)	(-0.05)	(-0.47)	(-0.41)
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	No	No	No	No
N	1,197	1,128	1,152	1,146
R^2	0.664	0.688	0.687	0.698

Panel B: Subsamples by analyst tenure at time of departure

	Analyst Inaccuracy			
Quartile	Q1	Q2	Q3	Q4
Avg. tenure at departure (in years)	2.2	3.6	6.4	13.6
	(1)	(2)	(3)	(4)
IB Exit$_{t+1yr}$	-1.061	-2.246	-0.674	-1.337
	(-1.76)	(-2.56)	(-1.07)	(-2.58)
Tenure	0.658	0.575	0.746	0.584
	(1.41)	(1.24)	(1.63)	(1.28)
No. of deals	0.147	0.170	0.067	0.183
	(1.00)	(1.05)	(0.46)	(1.12)
IB underwriter	-0.048	-0.091	-0.036	-0.048
	(-0.12)	(-0.23)	(-0.09)	(-0.11)
Issuer market share	-0.031	-0.146	-0.169	-0.111
	(-0.32)	(-1.46)	(-1.90)	(-1.10)
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	Yes	Yes	Yes	Yes
N	1,189	1,149	1,194	1,091
R^2	0.769	0.770	0.770	0.769

Table 1.7: Analyst Performance and Promotions

The table presents results from regressing analyst inaccuracy on an indicator for analyst promotions, which are identified as follows. For all press releases from Moody's website mentioning a given analyst, I identify the analyst's job title. Matching job titles with salary information from www.glassdoor.com, I rank job titles from low to high average salary and classify an analyst as being promoted when his job title changes to a higher-salary category. In columns (1) and (2), *Promotion* is an indicator equal to one if the analyst gets promoted at least once during his tenure at Moody's. In columns (3) and (4), $Promotion_{t+1yr}$ is an indicator equal to one if the analyst gets promoted in the next year. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

	Analyst Inaccuracy			
	(1)	(2)	(3)	(4)
Promotion	-0.362	-0.393		
	(-1.88)	(-2.04)		
Promotion_{t+1yr}			-0.233	-0.275
			(-0.98)	(-1.14)
Tenure		0.075		0.501
		(0.72)		(1.36)
No. of deals		0.072		0.086
		(0.65)		(0.69)
IB underwriter		-0.049		-0.019
		(-0.17)		(-0.06)
Issuer market share		-0.058		-0.098
		(-0.66)		(-1.18)
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	No	No	Yes	Yes
N	1,479	1,479	1,479	1,479
R^2	0.674	0.674	0.761	0.762

Table 1.8: Exploiting Shocks to the Supply of Investment Banking Jobs

The table presents results from my analysis of analyst inaccuracy around the event where an investment bank enters a new collateral group as an underwriter. Panel A compares the inaccuracy of analysts in the event collateral group (i.e., the collateral group entered by the investment bank) and the inaccuracy of analysts in other collateral groups in the same market segment (ABS, MBS, or CDO/CLO) around the event. Analyst inaccuracy is regressed on a set of nine event-time dummy variables labeled $t-4$, $t-3$, ..., $t+3$, $t+4$, where my convention is that dummy t takes on the value one in the collateral group and semester in which an investment bank entry event occurs. Each column reports the coefficient on one of the nine dummy variables. Panel B focuses on event semesters $t-2$ to t and shows how the performance gap between the event and the control group differs for analysts with different characteristics. *Event* $(-2, 0)$ is an indicator equal to one in the two semesters prior to and including the event semester. *Past Performance* refers to the analyst's average inaccuracy in the collateral group during the previous two semesters, and is split into low and high groups within collateral type and date. $\widehat{Pr(IB\,Exit)}$ refers to the analyst's ex-ante predicted probability of leaving to an investment bank, estimated as the predicted values from the Probit model in Table 1.2, column (1), and is split at the median across all analysts in a given calendar year. *Professional Network* is an indicator equal to one if the most recent educational institution attended by the analyst is located in the same country as his office location at Moody's. All regressions include segment $\times$ semester fixed effects, analyst fixed effects, and the same controls as in Table 2.4. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

Panel A: Analyst performance around investment bank entry

	Analyst Inaccuracy								
	$t-4$	$t-3$	$t-2$	$t-1$	t	$t+1$	$t+2$	$t+3$	$t+4$
Event	-0.033	-0.561	-0.913	-1.493	-0.940	-0.453	0.008	0.443	0.272
	(-0.14)	(-2.20)	(-3.31)	(-4.66)	(-3.47)	(-1.87)	(0.03)	(1.59)	(1.03)
Controls suppressed									
Analyst f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Seg.$\times$ sem. f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Analyst performance by subsample

	Analyst Inaccuracy					
	Past Performance		$\widehat{Pr(IB\,Exit)}$		Professional Network	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
Event (-2,0)	0.716	-1.635	-0.050	-0.873	-3.015	-1.109
	(0.85)	(-2.61)	(-0.04)	(-1.44)	(-2.68)	(-2.46)
Controls suppressed						
Analyst f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Segment × sem. f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Chi^2 statistic	6.02		0.45		4.77	
p-value	0.014		0.503		0.029	
N	437	515	312	262	81	867
R^2	0.851	0.770	0.787	0.863	0.926	0.698

Figures

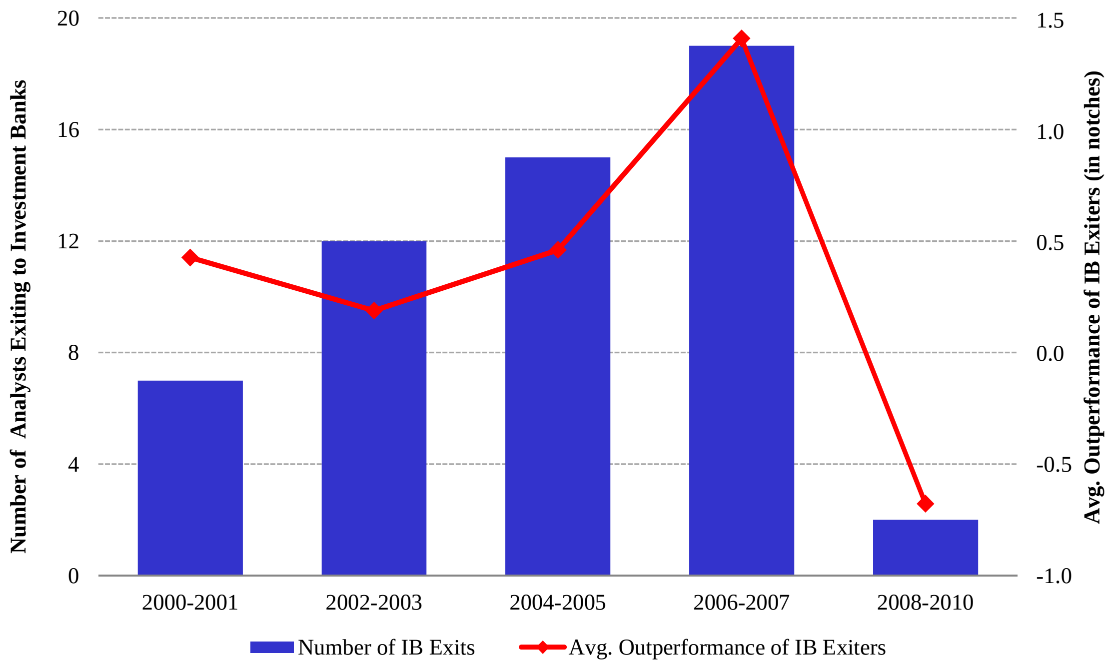


Figure 1.1: Departures to Investment Banks and Average Outperformance of Departing Analysts. The graph plots the number of analysts hired by investment banks and the average outperformance of departing analysts in each subperiod. Investment banks are all investment banks that were ranked in "The Bloomberg 20" ranking in the year prior to the analyst's exit. Outperformance is measured as minus one times the average abnormal absolute rating adjustment in the three years after rating issuance, following equations (1.8) and (1.9).

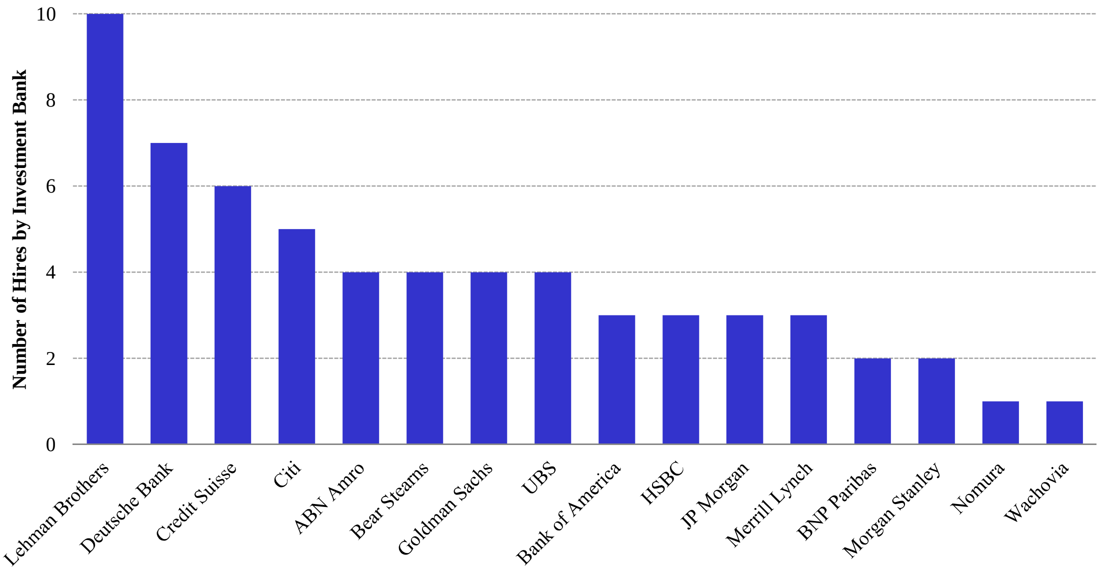


Figure 1.2: Number of Hires by Investment Bank. The graph plots the total number of Moody's analysts hired by each investment bank over the sample period. An analyst departure is classified as an exit to an investment bank if his subsequent employers was ranked in "The Bloomberg 20" ranking in the year prior to the analyst's departure.

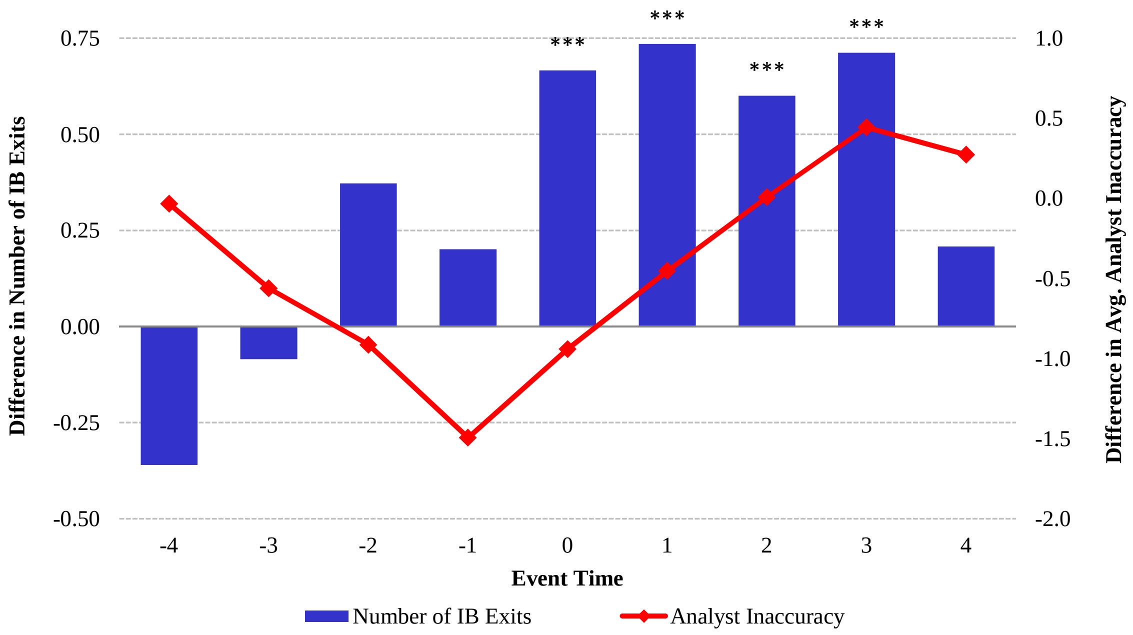


Figure 1.3: Event Study: Shocks to the Supply of Investment Banking Jobs. The graph plots the number of analysts departing to investment banks and average analyst inaccuracy around the event where an investment bank enters a new collateral group as an underwriter. The blue bars show the difference in the number of analysts who depart to investment banks between the event collateral group (i.e., the collateral group that the investment bank enters) and other collateral groups in the same market segment (ABS, MBS, or CDO/CLO) in the window $(-4, +4)$ around the event. For each collateral type and semester, the number of analysts who depart within the next year is regressed on a set of nine event-time dummy variables labeled $t-4$, $t-3$, ..., $t+3$, $t+4$, where my convention is that dummy t takes on the value one in the collateral group and semester in which an investment bank entry event occurs, as well as collateral type $\times$ semester fixed effects, analyst fixed effects, and the same controls as in Table 2.4. Each bar shows the coefficient on one of the nine dummy variables. The red line plots the coefficient estimates reported in Table 1.8, Panel A, i.e., the difference in analyst inaccuracy between the event and the control group, over the same event window. Asterisks ***, **, * indicate statistical significance on the 1%, 5%, and 10% level.

Appendix

A1.1 Proofs

Average causal effect of revolving doors

The average causal effect of revolving doors on the performance of analysts who choose to enter the lottery ("the treated") is given by:

$$\begin{aligned}
ATT &= E(e_i|l_i=1, a_i > a_L) - E(e_i|l_i=0, a_i > a_L) \\
&= \int_{a_L}^{\bar{a}} (e^*(a_i)|l_i=1, a_i > a_L)da - \int_{a_L}^{\bar{a}} (e^*(a_i)|l_i=0, a_i > a_L)da \\
&= (w_{CRA} + pw_{IB})0.5(a_L + \bar{a}) - w_{CRA}0.5(a_L + \bar{a}) = pw_{IB}0.5(a_L + \bar{a}) \\
&= pw_{IB}0.5(\frac{c}{w_{IB}(w_{CRA} + pw_{IB})} + \bar{a})
\end{aligned} \tag{1.11}$$

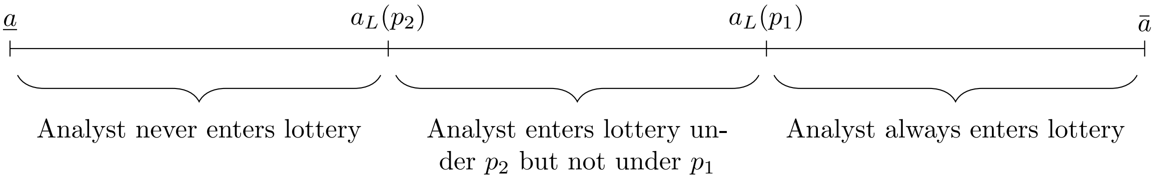
The above expression is larger than zero as long as the expected rent from an investment banking job and the switching cost are positive ($pw_{IB} > 0$ and $c > 0$).

Observed differences in performance

$$\begin{aligned}
E(e_i|D_i=1) - E(e_i|D_i=0) &= (w_{CRA} + pw_{IB})0.5(a_L + \bar{a}) \\
&\quad - (\theta(w_{CRA} + pw_{IB})0.5(a_L + \bar{a}) + (1-\theta)w_{CRA}0.5(\underline{a} + a_L)) \\
&= ATT + B - C, \\
where\ ATT &\equiv pw_{IB}0.5(a_L + \bar{a}), \\
B &\equiv w_{CRA}0.5(a_L + \bar{a} - \theta(a_L + \bar{a}) - (1-\theta)(\underline{a} + a_L)), \\
C &\equiv \theta pw_{IB}0.5(a_L + \bar{a}), \\
\theta &\equiv \frac{(1-p)(\bar{a} - a_L)}{(a_L - \underline{a}) + (1-p)(\bar{a} - a_L)}
\end{aligned} \tag{1.12}$$

Effect of a change in p

Consider a change in the probability of being hired by the investment bank, conditional on entering the lottery, from p_1 to p_2, where $p_1 < p_2$. Let $a_L(p_1)$ and $a_L(p_2)$ denote the two threshold levels of ability under the two scenarios p_1 and p_2, as defined in equation (1.4). The effect of a change from p_1 to p_2 on analyst performance differs for three groups of analysts, as depicted in the figure below:



The first group, analysts with ability $a_i < a_L(p_2)$, are analysts who choose not to enter the lottery in either scenario. The expected performance of these analysts is therefore insensitive to changes in the probability of being hired by investment banks.

$$\begin{aligned} &E(e_i|p_2, a_i < a_L(p_2)) - E(e_i|p_1, a_i < a_L(p_2)) \\ &= w_{CRA}0.5(\underline{a} + a_L(p_2)) - w_{CRA}0.5(\underline{a} + a_L(p_2)) \\ &= 0 \end{aligned} \tag{1.13}$$

The second group, analysts with ability $a_i > a_L(p_1)$, are analysts who choose to enter the lottery in either scenario. The expected change in the performance for this group of analysts is given by:

$$\begin{aligned} &E(e_i|p_2, a_i > a_L(p_1)) - E(e_i|p_1, a_i > a_L(p_1)) \\ &= (w_{CRA} + p_2 w_{IB})0.5(a_L(p_1) + \bar{a}) - (w_{CRA} + p_1 w_{IB})0.5(a_L(p_1) + \bar{a}) \\ &= (p_2 - p_1)w_{IB}0.5(a_L(p_1) + \bar{a}) \end{aligned} \tag{1.14}$$

The third group, analysts with ability $a_L(p_2) < a_i < a_L(p_1)$, are analysts who choose to enter the lottery in scenario p_2 but not in scenario p_1. The change in performance for this group of analysts is given by:

$$\begin{aligned} &E(e_i|p_2, a_L(p_2) < a_i < a_L(p_1)) - E(e_i|p_1, a_L(p_2) < a_i < a_L(p_1)) \\ &= (w_{CRA} + p_2 w_{IB})0.5(a_L(p_2) + a_L(p_1)) - w_{CRA}0.5(a_L(p_2) + a_L(p_1)) \\ &= p_2 w_{IB}0.5(a_L(p_2) + a_L(p_1)) \end{aligned} \tag{1.15}$$

First, note that the average change in performance in response to a positive change in p is either zero or positive for all three groups. Hence, the average change in analyst performance, which is a weighted average of the three groups, is weakly larger than zero (in other words, $E(e_i|p_2 - E(e_i|p_1) \geq 0$. Second, there may exist a group of low ability analysts, those with ability $a_i < a_L(p_2)$, whose performance is less sensitive to changes in p than that of analysts with higher ability.

A1.2 Variable Descriptions and Additional Evidence

Table A1.1: Variable descriptions

Variable	Description
Measures of Analyst (In)Accuracy	
Baseline	In a first step, the absolute difference (in notches) between Moody's initial rating of the tranche and the rating three years following the issuance is regressed on tranche and deal characteristics following equation (1.8). In a second step, the residuals from the first-step regression are aggregated at the analyst × collateral type × semester level by taking the arithmetic mean. Rating adjustments are obtained from Moody's website and tranche and deal characteristics are from SDC Platinum and Bloomberg.
Baseline, value-weighted	In a first step, the absolute difference (in notches) between Moody's initial rating of the tranche and the rating three years following the issuance is regressed on tranche and deal characteristics following equation (1.8). In a second step, the residuals from the first-step regression are aggregated at the analyst × collateral type × semester level by computing a weighted average where the weights are proportional to the tranche's principal amount. Rating adjustments are obtained from Moody's website and tranche and deal characteristics are from SDC Platinum and Bloomberg.
1-yr Abn. Rating Adjustment	In a first step, the absolute difference (in notches) between Moody's initial rating of the tranche and the rating one year following the issuance is regressed on tranche and deal characteristics following equation (1.8). In a second step, the residuals from the first-step regression are aggregated at the analyst × collateral type × semester level by taking the arithmetic mean. Rating adjustments are obtained from Moody's website and tranche and deal characteristics are from SDC Platinum and Bloomberg.
5-yr Abn. Rating Adjustment	In a first step, the absolute difference (in notches) between Moody's initial rating of the tranche and the rating five years following the issuance is regressed on tranche and deal characteristics following equation (1.8). In a second step, the residuals from the first-step regression are aggregated at the analyst × collateral type × semester level by taking the arithmetic mean. Rating adjustments are obtained from Moody's website and tranche and deal characteristics are from SDC Platinum and Bloomberg.
3-yr Abn. Downgrades	Downgrades are computed as the absolute difference (in notches) between Moody's initial rating of the tranche and the rating three years following the issuance if the initial rating is higher (otherwise it is set to zero). In a first step, the number of downgrades is regressed on tranche and deal characteristics following equation (1.8). In a second step, the residuals from the first-step regression are aggregated at the analyst × collateral type × semester level by taking the arithmetic mean. Rating adjustments are obtained from Moody's website and tranche and deal characteristics are from SDC Platinum and Bloomberg.

Continued on next page

Table A1.1 – continued

Variable	Description
3-yr Abn. Upgrades	Upgrades are computed as the absolute difference (in notches) between Moody's initial rating of the tranche and the rating three years following the issuance if the initial rating is lower (otherwise it is set to zero). In a first step, the number of upgrades is regressed on tranche and deal characteristics following equation (1.8). In a second step, the residuals from the first-step regression are aggregated at the analyst × collateral type × semester level by taking the arithmetic mean. Rating adjustments are obtained from Moody's website and tranche and deal characteristics are from SDC Platinum and Bloomberg.
3-yr Abn. Default	Tranches are considered in default when Moody's assigns a rating below Ca within the first three years after issuance. In a first step, this default indicator is regressed on tranche and deal characteristics following equation (1.8). In a second step, the residuals from the first-step regression are aggregated at the analyst × collateral type × semester level by taking the arithmetic mean. Rating adjustments are obtained from Moody's website and tranche and deal characteristics are from SDC Platinum and Bloomberg.
3-yr Absolute Cumulative Losses	In a first step, the absolute difference between the cumulative tranche losses, i.e., the principal balance write offs due to default, and Moody's expected loss benchmark for the tranche's initial rating category is computed. In a second step, the absolute differences obtained in the first step are aggregated at the analyst × collateral type × semester level by taking the arithmetic mean. Cumulative tranche losses are obtained from Bloomberg and Moody's expected loss benchmarks are retrieved from Moody's website (available at `https://www.moodys.com/sites/products/productattachments/marvel_user_guide1.pdf`).
> 2 Initial Ratings	An indicator function equal to one if the average deal is rated by more than two of the three major rating agencies (Moody's, S&P, Fitch). Initial ratings from the three major rating agencies are obtained from Bloomberg.
Initial Yield on AAA Tranches	Initial yields on AAA tranches are computed following He, Qian, and Strahan (2015). For tranches with floating coupon rates, the initial yield spread is equal to the spread (in basis points) over the benchmark specified at issuance as reported in Bloomberg. For tranches with fixed or variable coupon rates, the initial yield spread is computed as the difference between the coupon rate and the yield on a Treasury security whose maturity is closest to the tranche's weighted average life.
Key independent variables	
IB Exit	Indicator function equal to one if the analyst departs to an investment bank following his employment at Moody's. Investment banks are employers that were ranked in "The Bloomberg 20" ranking in the year prior to the analyst's departure. Post-Moody's employer information is obtained from public profiles on LinkedIn and web searches.

Continued on next page

Table A1.1 – continued

Variable	Description
IB Exit$_{t+1yr}$	Indicator function equal to one during the last two semesters of the analyst's employment at Moody's before he departs to an investment bank. Investment banks are employers that were ranked in "The Bloomberg 20" ranking in the year prior to the analyst's departure. Post-Moody's employer information is obtained from public profiles on LinkedIn and web searches.
Other Exit	Indicator function equal to one if the analyst departs to an employer other than an investment bank following his employment at Moody's. Investment banks are employers that were ranked in "The Bloomberg 20" ranking in the year prior to the analyst's departure. Post-Moody's employer information is obtained from public profiles on LinkedIn and web searches.
Other Exit$_{t+1yr}$	Indicator function equal to one during the last two semesters of the analyst's employment at Moody's before he departs to a non-investment bank employer. Investment banks are employers that were ranked in "The Bloomberg 20" ranking in the year prior to the analyst's departure. Post-Moody's employer information is obtained from public profiles on LinkedIn and web searches.
Future Employer	Fraction of tranches that are underwritten by the analyst's future employer. Underwriter information is obtained from SDC Platinum and manually matched with information on the analyst's post-Moody's employer obtained from public profiles on LinkedIn and web searches.
Control variables	
Tenure	Logarithm of one plus the number of semesters since the begin of the analyst's employment at Moody's, which is the earlier date of the analyst's reported start date on LinkedIn and his first appearance in the dataset.
Number of deals	Logarithm of one plus the number of deals rated by the analyst in a given collateral type and semester.
IB Underwriter	The fraction of tranches underwritten by an investment bank that was rated in "The Bloomberg Top 20" ranking in the year prior to ratings issuance. For ratings issued prior to 2005, the Bloomberg ranking from 2004 is used. Underwriter information is obtained from SDC Platinum.
Issuer Market Share	The market share of the tranche issuer based on the dollar volume of deals across all collateral types originated in the previous calendar year.
Measures of Deal Complexity	
Low Documentation	The average percentage of loans with less than full documentation in the underlying collateral of the deal. The percentage of loans with full documentation is obtained from Bloomberg.

Continued on next page

Table A1.1 – continued

Variable	**Description**
Abs. Credit Score Skewness	The absolute skewness of the credit score distribution of the loans in the underlying collateral of the deal. Skewness is computed in terms of quartiles of the credit score distribution using Bowley's formula. Quartiles of the credit score distribution are obtained from Bloomberg.
Deal Complexity (HQS)	Computed following He, Qian, and Strahan (2015) as the number of tranches in the deal divided by their combined principal amount.

Table A1.2: Baseline Results – Regressions at the Deal Level

The table reports results from Table 2.4 when running regressions at the individual deal level. Specifically, I estimate the following regression:

$$Rating\,Adjustment_{kiz} = \lambda_i + \lambda_{zt} + \delta IB\,Exit_{i,t+1yr} + \beta' X_{ki} + \eta_{kiz}, \tag{1.16}$$

where $Rating\,Adjustment_{kiz}$ is the average absolute difference (in notches) between the initial rating and the rating three years after issuance across all tranches of deal k rated by analyst i. λ_i and λ_{zt} are analyst and collateral type × issuance semester fixed effects, respectively, and X_{ki} represents the same vector of additional controls as in equation (1.8). All variables are defined in A3.1. In columns (1) and (2), *IB Exit* is an indicator equal to one if the analyst departs to an investment bank that was ranked in "The Bloomberg 20" ranking in the year prior to his departure. In columns (3) and (4), $IB\ Exit_{t+1yr}$ is an indicator equal to one in the last two semesters of the analyst's employment at Moody's before his departure to the investment bank. Panel A presents baseline results. Panel B presents results for the interaction with an indicator equal to one if the deal is underwritten by the analyst's future employer. Panel C reports results from a placebo test where *Other Exit* refers to analyst departures to other employers. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

Panel A: Baseline

	Analyst Inaccuracy			
	(1)	(2)	(3)	(4)
IB Exit	-0.331	-0.384		
	(-2.10)	(-2.40)		
IB Exit$_{t+1yr}$			-0.844	-0.951
			(-2.47)	(-2.67)
Tenure		-0.078		0.516
		(-1.08)		(1.86)
No. of deals		0.237		0.193
		(2.42)		(1.76)
IB underwriter		-0.103		-0.060
		(-0.85)		(-0.58)
Issuer market share		0.084		0.097
		(1.85)		(2.38)
Deal Controls	No	Yes	No	Yes
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	No	No	Yes	Yes
N	4,515	4,507	4,515	4,515
R^2	0.782	0.788	0.814	0.814

Panel B: Interaction with Fraction of Tranches Underwritten by Future Employer

	Analyst Inaccuracy			
	(1)	(2)	(3)	(4)
IB Exit	-0.319	-0.372		
	(-1.96)	(-2.24)		
IB Exit × Future Employer	-0.178	-0.190		
	(-0.75)	(-0.79)		
IB Exit$_{t+1yr}$			-0.855	-0.962
			(-2.46)	(-2.67)
IB Exit$_{t+1yr}$ × Future Employer			0.402	0.397
			(1.12)	(1.15)
Future Employer			-0.320	-0.290
			(-1.35)	(-1.23)
Deal and Other Controls	No	Yes	No	Yes
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	No	No	Yes	Yes
N	4,515	4,507	4,515	4,507
R^2	0.782	0.788	0.814	0.818

Panel C: Placebo Test with Departures to Other Employers

	Analyst Inaccuracy			
	(1)	(2)	(3)	(4)
Other Exit	0.373	0.332		
	(2.73)	(2.51)		
Other Exit$_{t+1yr}$			-0.049	-0.089
			(-0.25)	(-0.46)
Deal and Other Controls	No	Yes	No	Yes
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	No	No	Yes	Yes
N	4,517	4,509	4,517	4,509
R^2	0.782	0.787	0.813	0.817

Table A1.3: Departures to Other Employers

The table presents results for analyst departures to employers other than investment banks. *Other banks* refer to employment analyst transitions to banks and brokers that are not listed in "The Bloomberg 20" ranking in the year prior to the transfer, *asset managers* include mutual funds and hedge funds, and *others* comprise all other employers (e.g., other rating agencies, regulators, or law firms). t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

	Analyst Inaccuracy			
	Other Banks	Asset Managers	Insurers	Others
	(1)	(2)	(3)	(4)
Exit_{t+1yr}	1.424	-1.303	0.891	0.197
	(2.66)	(-1.31)	(0.71)	(0.41)
Tenure	0.374	0.496	0.453	0.445
	(1.05)	(1.38)	(1.27)	(1.24)
No. of deals	0.091	0.096	0.086	0.088
	(0.74)	(0.76)	(0.69)	(0.71)
IB underwriter	-0.014	-0.019	-0.016	-0.013
	(-0.04)	(-0.06)	(-0.05)	(-0.04)
Issuer market share	-0.100	-0.102	-0.100	-0.100
	(-1.22)	(-1.22)	(-1.20)	(-1.20)
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	Yes	Yes	Yes	Yes
N	1,479	1,479	1,479	1,479
R^2	0.763	0.763	0.762	0.762

Table A1.4: The Impact of Past Work Experience With Investment Banks

The table presents results from regressing analyst inaccuracy on past investment bank experience. *Past IB* is an indicator equal to one if the analyst has worked for an investment bank prior to his employment with Moody's. *Past Employer* refers to the fraction of tranches that are underwritten by the analyst's past employer. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

	Analyst Inaccuracy		
	(1)	(2)	(3)
Past IB	0.032	0.074	
	(0.16)	(0.35)	
Past IB × Past Employer		-0.884	-0.932
		(-0.99)	(-1.12)
Tenure	0.056	0.057	0.423
	(0.50)	(0.51)	(1.08)
No. of deals	0.087	0.084	0.124
	(0.76)	(0.73)	(0.96)
IB underwriter	-0.129	-0.112	-0.186
	(-0.39)	(-0.34)	(-0.50)
Issuer market share	-0.078	-0.078	-0.142
	(-0.95)	(-0.95)	(-1.76)
Collateral type × semester f.e.	Yes	Yes	Yes
Analyst f.e.	No	No	Yes
N	1,267	1,267	1,267
R^2	0.702	0.702	0.777

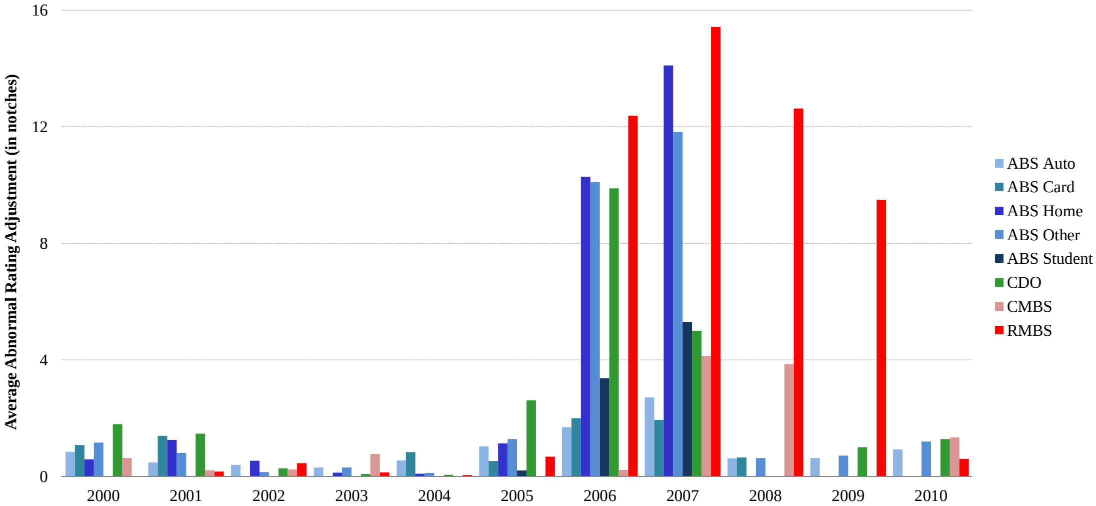


Figure A1.1: Rating Performance by Collateral Type. The graph plots average rating adjustments across eight collateral types and over time. Rating adjustments are computed as the absolute difference (in notches) between a tranche's initial rating and the rating three years after issuance, and are averaged across all tranches issued in a given collateral type and calendar year.

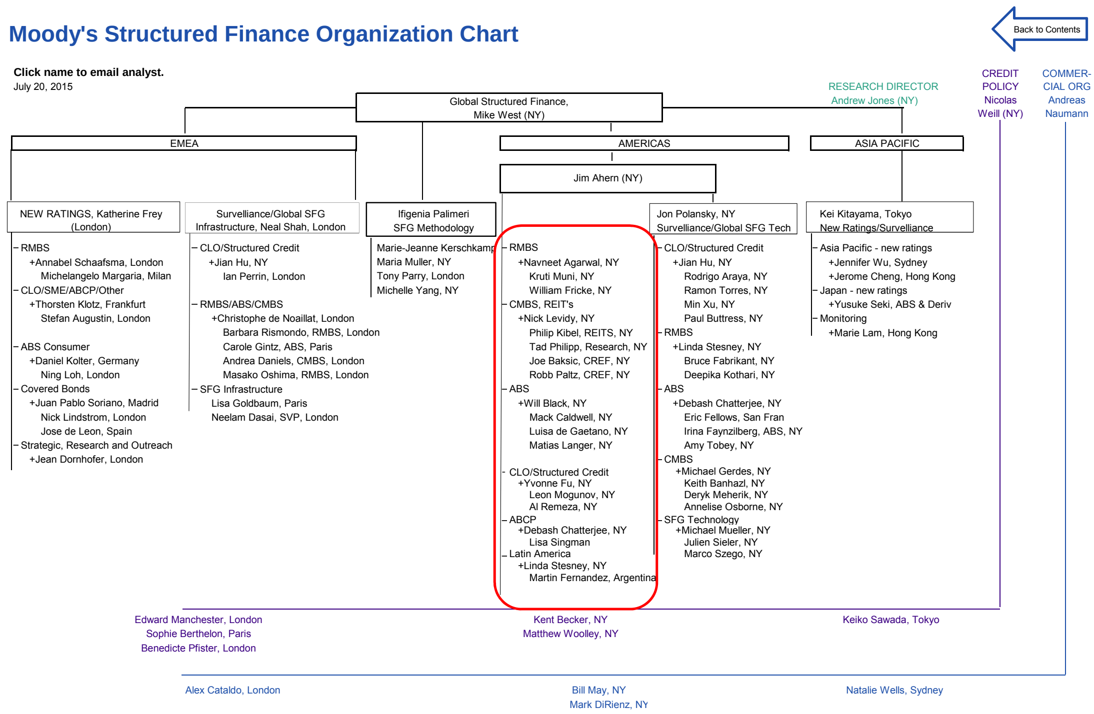


Figure A1.2: Organizational Structure at Moody's. The chart shows the organizational structure of the Structured Finance team at Moody's as reported on Moody's website (available at `https://www.moodys.com/research/Structured-Finance-Ratings-Quick-Check-Newsletter--PBS_SF161380`). The red line highlights the division of interest for this paper, i.e., new ratings in the Americas region.

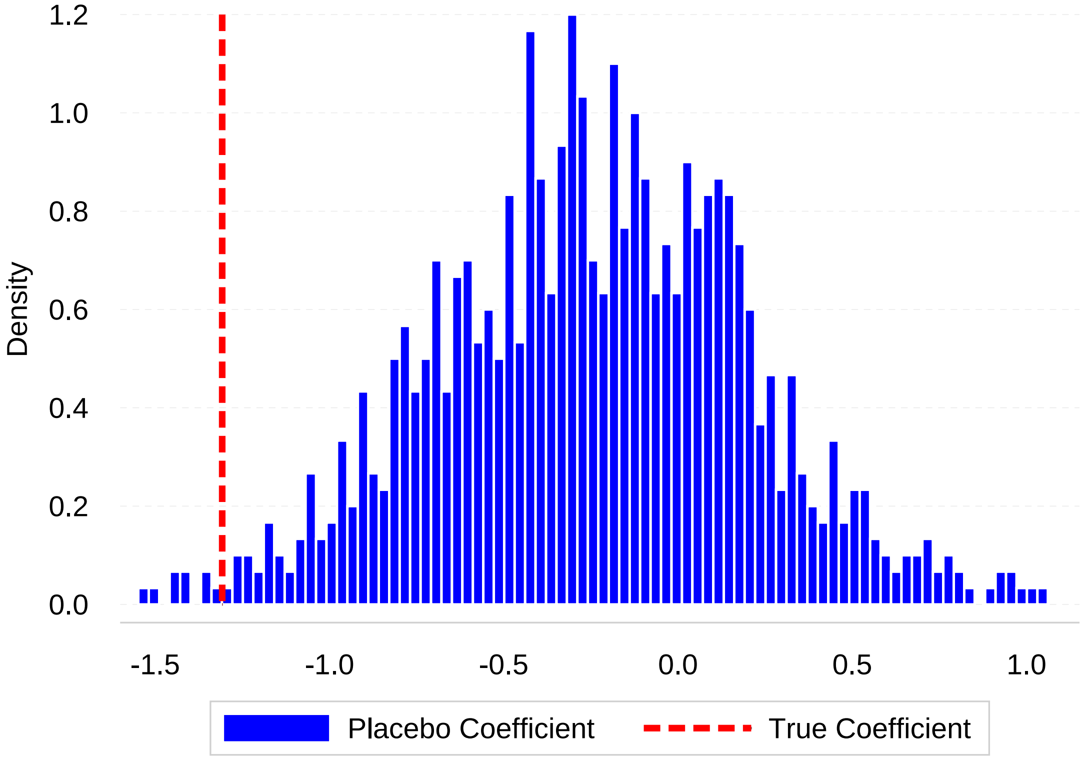


Figure A1.3: Falsification test using placebo analyst departure dates. The figure illustrates the output from a falsification test where I replace the analyst's actual departure date to the investment bank with a random date between the actual start and end date of the analyst's employment with Moody's. Depicted is the histogram of the regression coefficients of $IB\ Exit_{t+1yr}$ estimated from 1,000 placebo runs.

Chapter 2

Learning By Doing: The Value of Experience and The Origins of Skill for Mutual Fund Managers

Co-authors: Alberto Manconi and Oliver Spalt

> When the markets act up like this, one natural reaction is to rely on the insights of experienced managers. The argument goes that, because they have been around the block a few times, they'll be able to navigate their funds better this time around.
>
> *Wall Street Journal (2010)*

2.1. Introduction

Driving a car, flying an airplane, or writing an academic paper, are examples of activities in which learning by doing is important.[1] Most people are not born natural drivers, pilots, or scholarly writers – instead, they acquire the skill as they drive, fly, or write. Even controlling for general ability, there are likely large differences in performance between someone who, say, drives very little, and someone who drives a lot. As consumers, we value experience highly, and often prefer an experienced

[1] Learning by doing as a concept has a long history. Early writings emphasized the effects of learning by doing on educational outcomes (e.g., Dewey (1897)) and increases in individual worker productivity (e.g., Book (1908)). Starting with Arrow (1962), the concept has been applied to the study of firms and often refers to decreasing unit costs as function of output (e.g., Bahk and Gort (1993)). The economic literature on learning by doing is too large for us to review here; we refer the reader to available surveys, such as Thompson (2010).

pilot (or dentist) to an inexperienced one. While learning by doing and experience obviously play a role in many contexts, little work exists that analyzes the value of experience for top-level economic decision makers. Our paper aims to fill this gap by studying mutual fund managers. The mutual fund industry is a market segment of first-order economic significance, which as of 2011 manages almost $12 trillion dollars of investor wealth, or, alternatively, 23% of all assets of U.S. households (2012 Investment Company Fact Book). We exploit unique features of the mutual fund industry, and the available mutual fund data, to provide novel, comparatively clean, evidence indicating that learning by doing effects matter for this important set of professional investors.

Identification is the main challenge for any study on the value of experience and the impact of learning on output, because learning is unobservable. For instance, at first glance tenure might seem a reasonable proxy for fund manager experience. However, tenure could also proxy for effort, because junior managers might need to work harder to signal their type (e.g., Chevalier and Ellison (1999)). Moreover, if bad managers are eliminated by competition, or if the best managers go work for hedge funds (e.g., Kostovetsky (2010)), tenure is correlated with general ability. Further, managers with longer tenure might have a different standing within their organization, leading to different agency issues and explicit or implicit contractual arrangements influencing investment behavior and performance. For example, they might be overly conservative (e.g., Prendergast and Stole (1996)) or subject to greater risk of being fired for underperformance (e.g., Dangl, Wu, and Zechner (2008)). Lastly, tenure is correlated with age, which is again correlated with many other variables including cognitive ability (e.g., Korniotis and Kumar (2011)). In sum, it is extremely hard to identify the incremental value of experience using simple proxies like tenure or age. This is a central difficulty in all empirical work on learning by doing.

We develop a new approach to identifying the marginal impact of experience on mutual fund manager performance, building on two main ideas. First, we construct measures of experience, discussed in detail below, that are not linear functions of time. Age and tenure change one-for-one with calendar time (exactly so for age; approximately so for tenure). A key source of the identification problems highlighted above is the fact that many other variables are also highly correlated with calendar time. Our experience measures get around this problem. Second, we decompose a mutual fund into a collection of smaller industry sub-portfolios (ISPs). For example, instead of thinking of manager m as managing fund f in quarter q, we think of her as managing a healthcare ISP (the stocks held by fund f belonging to the healthcare industry) and a telecom ISP (the stocks held by fund f belonging to the telecom industry). If the level of experience differs across ISPs, we can use variation in industry experience *within* fund managers *at a given point in time* to identify the impact of experience on fund returns. The advantage of this strategy is that we do not need to rely on variation across managers, or across time, leaving us less exposed to the sort of omitted variable concerns described above. Fixed effects allow us to eliminate the confounding impact of all variables that do not vary across ISPs for a given manager-date combination. Important confounding factors we can thus exclude are, for example, general ability, educational background, tenure, age, fund characteristics, fund family characteristics, corporate governance at the fund level, and the overall state of the economy.

Our main results are as follows. Unconditionally, ISPs with an experienced fund manager outperform other ISPs by about 1.0% per quarter before fees on a four-factor risk adjusted basis. In regressions, using manager $\times$ date fixed effects, that spread widens to almost 1.4% per quarter. In addition, experienced managers make significantly better buying and selling decisions than inexperienced managers even if we use fixed effects to eliminate confounding variation on the manager-quarter, industry-quarter, and manager-stock level. These results suggest learning by doing and experience are first-order drivers of fund returns.

In deriving our experience measures, our main conjecture is that experience builds up mostly in difficult environments. Hence, a fund manager who navigates through a period of severe underperformance in a given industry (henceforth, an "industry shock") will gain more experience in that industry than if nothing unusual happens. That is, intuitively, we assume that fund managers resemble airplane pilots who gain experience not from plain sailing, but from flying through turbulence. This conjecture is directly motivated by Arrow's Arrow (1962) seminal work on learning by doing, who writes: "Learning is the product of experience. Learning can only take place through the attempt to solve a problem and therefore only takes place during activity." We operationalize this idea by recording industry-wide shocks, defined in detail below, for each industry and quarter in our dataset. We then use the number of past industry shocks observed by a manager over her career as a proxy for her experience in a given industry. The important feature of our experience definition is that it is not a linear function of time, i.e., the same manager might have more experience in, for example, the healthcare industry than in the telecom industry, *at the same point in time*.

Several results support our learning story. First, the outperformance of experienced managers is particularly pronounced during subsequent industry shocks. Second, there are decreasing marginal benefits of experience. Third, difference-in-differences results show managers perform better than their peers after obtaining experience, but not before. Fourth, managers with greater exposure to a shock industry learn more. Finally, several placebo tests suggest our results are not spuriously induced by our methodology.

Exactly how does experience translate into higher returns? While fully answering this question is a topic for future research, we provide a partial answer by analyzing holdings changes in anticipation of earnings announcements. We find that experienced fund managers trade in the direction of subsequent earnings surprises, and that they increase their holdings more before large earnings announcement returns.

This suggests that *one* channel through which experience leads to higher returns is an enhanced ability to interpret and act upon news around earnings announcements.

As a final step, we develop an experience index (EDX), which aggregates experience measure across all industries for a given manager at the fund level. EDX is a purely backward looking measure that can be constructed in real time. Funds that score highest on EDX obtain significant 4-factor risk-adjusted returns of 1.4% per year before fees, while low EDX funds break even at best.

The next section summarizes the related literature. We describe our method and the dataset in detail in Section 2.3. Section 2.4 presents our main results on fund manager experience and fund performance as well as robustness checks. In Section 2.5 we examine trading by experienced fund managers. Section 2.6 discusses alternative explanations. Extensions are discussed in Section 2.7. The final section concludes.

2.2. Contribution to the Literature

To the best of our knowledge, our paper is the first to focus exclusively on identifying the value of experience in the mutual fund industry. However, a small number of papers contain related results. Chevalier and Ellison (1999) find evidence that managers graduating from more prestigious colleges outperform, but they find no robust results for tenure. This is in contrast to earlier results by Golec (1996) who reported a positive tenure effect. Ding and Wermers (2009) find that managers with longer tenure outperform in large funds, which might have better governance structures, but underperform in smaller funds. Greenwood and Nagel (2009) document that young and old managers had different investment and return patterns for technology stocks during the late 1990s "tech bubble." Our evidence of learning by doing is related to but different from the contemporaneous evidence of Pástor, Stambaugh, and Taylor (2014), who find that skill rises with fund age once they control for the size of the mutual fund industry. Because our study uses variation within managers at a given point in time, our effects are orthogonal to the age, tenure, and skill effects that were the focus of these earlier studies.

Our study also contributes to the growing literature on investor learning. One strand of the literature analyzes rational learning theories (e.g., Mahani and Bernhardt (2007), Pastor and Veronesi (2009), Seru, Shumway, and Stoffman (2010), Linnainmaa (2011), Huang, Wei, and Yan (2011)). Another strand looks at alternative learning theories, such as, for example, naïve reinforcement learning (e.g., Kaustia and Knüpfer (2008), Barber, Lee, Liu, and Odean (2010), Chiang, Hirshleifer, Qian, and Sherman (2011), Bailey, Kumar, and Ng (2011), Campbell, Ramadorai, and Ranish (2013)). Malmendier and Nagel (2011) show that past macroeconomic shocks shape future financial decisions. This learning literature has mainly focused on individual investors and retail investors. Our study introduces new results on the relevance and profitability of learning for professional investors.

Lastly, our study contributes a new econometric approach to identifying fund manager skill (e.g., Berk and Green (2004), Fama and French (2010), Pástor, Stambaugh, and Taylor (2014)). Our results show that experienced managers can outperform passive benchmarks via stock-picking, which adds to a body of work suggesting that at least some funds can systematically outperform.[2] Our study is related to Kacperczyk, Sialm, and Zheng (2005), who show that mutual fund managers who concentrate their holdings in some industries have higher alphas, but our effects are not subsumed by their fund-level measure. As we show that experience from industry shocks tends to be particularly valuable in future industry shocks, our findings can help explain why mutual funds on average do better in recessions (e.g., Moskowitz (2000), Glode (2011)), but the experience-performance relation we document can-

[2]This literature is too large for us to review it here. Papers include Daniel, Grinblatt, Titman, and Wermers (1997), Cohen, Coval, and Pástor (2005), Kacperczyk, Sialm, and Zheng (2005), Bollen and Busse (2005), Kacperczyk and Seru (2007), Cremers and Petajisto (2009), Baker, Litov, Wachter, and Wurgler (2010), Berk and van Binsbergen (2012), Koijen (2012). See e.g., Wermers (2011) for an excellent survey.

not, by construction, be explained by recessions. Our trade-based results are in line with, and may even provide a learning-based economic rationale for, results in Chen, Hong, Huang, and Kubik (2000) and Schultz (2010) who find fund manager trading skill is observed predominantly for funds tilting towards growth stocks.

While many papers focus on identifying whether skill exists, fewer ask where it comes from. Skill could be related to time-invariant factors like IQ (e.g., Chevalier and Ellison (1999), Grinblatt and Keloharju (2012)) and measured skill could be time-varying because boundedly rational managers find it optimal to allocate attention differently over assets across the business cycle (e.g., Kacperczyk, Nieuwerburgh, and Veldkamp Kacperczyk, Nieuwerburgh, and Veldkamp (2011), Kacperczyk, Nieuwerburgh, and Veldkamp (2013)). In this paper, we add a new dimension by proposing that two otherwise identical fund managers can have different skill because their employment histories exposed them to different learning opportunities. Our results show that experience can be (i) theoretically important for understanding the origins of fund manager skill and (ii) a powerful predictor of fund performance.

On a broader level, our work addresses two central problems for the empirical literature on learning by doing identified in a recent survey by Thompson (2010): How to separate learning by doing from pure time, age, and size effects?, and: How to surmount empirical problems due to the poor quality of productivity data typically available to researchers? Our study directly tackles both of these problems. By using variation within manager-date cells as a source of identification, our approach minimizes omitted variable concerns. Further, our mutual fund data are close to ideal in many respects: (i) fund managers make economically substantial decisions, (ii) they are appropriately incentivized to do well, (iii) we observe the same individual repeatedly in an almost identical decision making environment, (iv) we can observe multiple decisions for the same manager at the same time, and (v) mutual fund performance measures provide a reasonably accurate real-time productivity gauge.

2.3. Method and Data

In this section, we first illustrate our approach and explain how we identify experience from looking at individual industry components of fund portfolios. We then describe in detail how we construct our main experience measure based on industry shocks. Finally, we explain how we measure performance for industry sub-portfolios, and describe the dataset we use in our empirical tests.

2.3.1. Experience and Learning

To fix ideas, consider a simple Bayesian learning model. In order to optimize her portfolio, a fund manager needs to form a prediction of the expected return of a stock, denoted by $\tilde{r}$. Her prior beliefs are that the return is normally distributed with mean r_0 and variance σ_0^2. An essential part of the fund manager's job is to process signals about $\tilde{r}$ and to update her beliefs accordingly. Suppose the manager obtains N independent signals, $s_n = \tilde{r} + \eta_n$, where η_n is normally distributed, has zero mean, and variance σ^2. Posterior precision (the inverse of the posterior variance) is then given by:

$$\rho_N = \sigma_0^{-2} + N\sigma^{-2}. \tag{2.1}$$

The precision of the estimate therefore increases with the number of signals N, independently of the realization of the signals. In other words, learning reduces uncertainty.

If, all else equal, a manager who is less uncertain about the environment she operates in outperforms other managers, returns will be a function the number of signals received. Specifically, if risk-adjusted fund returns α are an increasing function of the precision, i.e., $\alpha'(\rho_N) > 0$, then, all else equal, equation (2.1) predicts that manager m_1 should outperform manager m_2 if $N_{m_1} > N_{m_2}$.

To make the simplest possible assumption that allows us to separate our approach from alternatives in the literature, assume that N can be written as:

$$N = T + S_0 + E. \tag{2.2}$$

T denotes tenure and captures the idea that a manager will mechanically observe more signals – and therefore have more precise beliefs about $\tilde{r}$ – if she has a longer tenure. The second component, S_0, captures that some managers will have higher baseline skill than others. For example, they are more intelligent, or have received their education from an elite college. The subscript 0 indicates that baseline skill is time-invariant and fixed. In our formulation, managers with higher baseline skill receive more signals. E denotes experience.

The existing literature has mainly focused on the first two components. The innovation in our study is the third one: E. It captures that managers will not learn equally in every period. In some periods, more information will be produced, and the manager therefore receives more signals. Using the example from the introduction, while a pilot may learn something from flying in perfect conditions, she might learn much more from successfully navigating her plane through turbulent conditions. We refer to E as *experience*, with the implicit understanding that it is actually "excess" experience, unrelated to the pure passage of time.

Experience varies not only by time, but also by industry. For example, a fund manager who was exposed to bank stocks in the fourth quarter of 2007 (when bank stocks fell by almost 10%) might have a different learning experience compared to a manager in business equipment in the same quarter (the return on business equipment stocks was 0.1%). The central idea of our approach is to exploit variation of experience across industries i managed by manager m in quarter q. To do this, we decompose the portfolio held by manager m in quarter q into its industry components, which comprise, for example all stocks held by the fund that belong to the banking industry, healthcare etc. We call these industry-related parts of the portfolio *industry sub-portfolios* (ISPs).

Consider then a reduced-form model of performance for ISP i:

$$\alpha_{mqi} = \beta_1 T_{mq} + \beta_2 S_{0,m} + \beta_3 E_{mqi} + \Gamma' B_{mq} + \varepsilon_{mqi}, \tag{2.3}$$

which states that the risk-adjusted ISP return α_{mqi} of manager m in quarter q is a function of the components of N in equation (2.2), with the key difference that experience is now allowed to vary on the ISP level.[3] The model allows for an arbitrary set of variables, $\Gamma' B_{mq}$, that can vary across both managers and quarters. As discussed in the introduction, this set of variables includes a large range of covariates studied in the literature, such as manager age, fund characteristics, fund governance, and the state of the economy. As an empirical matter, the β's as well as Γ could be zero, in which case alphas would reflect pure luck.

Equation (2.3) shows that we can eliminate the effect of tenure, baseline skill, and all other, potentially time-varying, variables, B_{mq}, if we compare the performance of ISPs for the same manger at the same point in time. In our empirical work below, we implement this by estimating equation (2.3) with a full set of manager $\times$ quarter fixed effects. The coefficient of interest, β_3, is identified because experience varies within manager and date. Our main prediction is $\beta_3 > 0$, i.e. we conjecture that higher ISP alphas are a function of more ISP experience.

We assume here that tenure of the fund manager and baseline skill do not vary across ISPs for the same manager and quarter. This is trivially satisfied for the tenure and skill variables used in the prior literature: the number of years worked for, say, Fidelity, or the fact that the manager obtained a degree from an elite college

[3]While we believe a linear specification in equation (2.3) is a plausible starting point, the true data generating process need not be linear. As shown in Angrist and Pischke (2009, Theorem 3.1.6), the linear specification in our benchmark model is the best linear approximation, in a minimum mean squared error sense, to the conditional expectation function of α given a level of experience E. We have explored cross-effects between T, S, and E in our empirical work, but could not find evidence for substantial non-linearities along those dimensions.

do not vary across ISPs. We discuss the case of industry-specific skill and tenure in Section 2.6 below.

2.3.2. An Experience Proxy Based on Industry Shocks

To implement our approach, we need an experience measure that is not a linear function of time and that varies across industries for a given manager-quarter combination.

We start by the definition of experience given in the American Heritage Dictionary of the English Language (2000). According to the dictionary, experience is "active participation in events or activities, leading to the accumulation of knowledge or skill," suggesting that a defining feature of experience is that it comes from having to act in a particular period or event. This feature is also highlighted in the quote by Arrow (1962) cited in the introduction. But when will a fund manager be particularly "active" and "working towards solving a problem"? We conjecture that managers are relatively active, and that problem solving becomes particularly relevant, when times are rough. Our proposed experience measure therefore counts the number of times a manager has experienced what we label *industry shocks*.

We consider different industry shock definitions. In our baseline definition, a shock occurs in a given industry and quarter, if the value-weighted industry return is the lowest across all 12 Fama-French industries in the quarter. This cross-sectional approach is in line with the fact that rankings and relative performance are of particular importance in the mutual fund industry (e.g., Brown, Harlow, and Starks (1996)).

Clearly, learning and experience are multi-dimensional, and fund managers may get experience from many different sources. Our objective is not to provide an all-encompassing measure of experience, but, more narrowly, to identify states of the world in which learning about one particular industry is particularly likely. We believe industry shocks are a natural candidate, and their use can be justified on at least three, not mutually exclusive, grounds.

First, the dictionary definition, Arrow's quote, and the pilot metaphor all suggest that problem solving is important in the accumulation of skill. Low returns, which come with industry shocks, are the central problem for fund managers. Second, industry shocks may capture underlying economic events that may make it more profitable to rationally direct attention to those industries in an attempt to understand the current set of industry-fundamentals better. Thus, there may be rational incentives to learn in industry shock periods. Third, the focus on learning in bad times is supported by a large literature on organizational learning. For example, in a widely-cited survey article on organizational learning, Lapré and Nembhard (2010) write:

> Failure experience is theorized to be a particularly effective stimulant for learning because it is highly salient, directly challenges the notion that current practices are adequate, and thereby provokes interest in identifying and developing alternative approaches. Failures [...] create an urgency to reflect, challenge old assumptions, and innovate to achieve aspirations. [...] Several studies have shown that organizations do not initiate change when their performance is satisfactory or successful, but do embrace change when their performance is poor.

There are some strong similarities to what we think is important in our fund manager setting. Low returns in industry shocks, and associated scrutiny by investors, are "highly salient" to fund managers and the low returns may plausibly "challenge the notion that current practices are adequate" and "provoke interest in identifying and developing alternative approaches." Experiencing low returns in industry shocks may "create an urgency to reflect, challenge old assumptions, and innovate to achieve aspirations." In sum, we believe all three of the above arguments provide support for the use of industry shocks in our experience measure.

Table 2.1 lists industry shock quarters from 1992 to 2012. The number of industry shocks is not the same for all industries. This is a desirable feature of the definition, since it is plausible that learning opportunities are greater in some industries than others. We will, however, also use alternative definitions in our robustness checks, with a more even distribution of shocks across industries. A second notable feature from the table is that we assign the label "industry shock" also to quarters with positive returns (e.g., utilities in 1997Q2, with an industry return of 5.5%). This is adequate if managers, investors, and the media care mostly about the relative ranking of industries. We leave these quarters in our sample to be conservative and minimize our degrees of freedom, but we show in the Appendix that our results get stronger when we impose the additional restriction that an industry shock quarter must have a negative industry return.

With the definition of industry shocks in hand, we define our main experience measure for fund manager m in industry i and quarter q as:

$$E_{mqi} = \sum_{\tau<q} \text{IS}_{i\tau} \times I[w_{m,\tau-1,i} > 0.1], \tag{2.4}$$

where IS stands for an industry shock in industry i in quarter τ. We update E_{mqi} after each industry shock quarter. E_{mqi} varies within a manager-quarter cell because a fund typically invests in multiple industries and because a fund manager can have different levels of experience in different industries. It is precisely this variation that we are seeking to exploit in our tests below.

$I[w_{m,\tau-1,i} > 0.1]$ is an indicator equal to one if the weight of industry i in the fund managed by fund manager m at the end of quarter $\tau - 1$ exceeds 10%. This captures the natural assumption that learning occurs predominantly in domains of interest for decision-makers. Intuitively, if I am not exposed to an industry, a negative return in that industry is not a problem that I need to solve. Of course it is possible that managers learn, in an absolute sense, from an industry shock even if they do not hold that industry, or even if they hold very little of it, perhaps by reading about the other industry or by talking to other fund managers. But we argue that managers learn more, in a relative sense, in industries to which they are more exposed. In terms of our pilot metaphor, a pilot learns more from actually experiencing heavy turbulence then from simply reading about it. We examine the relation between exposure and learning further in Section 2.4.4 below.

While the industry weight is in principle chosen by the manager, we argue that making the experience measure contingent on lagged industry weight is innocuous. If the most skilled managers could anticipate the shock, they would scale back their exposure, and therefore be less likely to acquire experience by our measure. This would bias us against our hypothesis that managers with high values of E_{mqi} outperform.

2.3.3. Data

The starting piece of information is the fund manager's identity, obtained from Morningstar Direct. We combine this with information from the CRSP Mutual Funds Fund Summary table, and we manually screen the resulting merge.[4] Coverage of manager names is sparse before 1992, so we choose this year as the starting point. To be able to focus on individual fund manager experience, we restrict attention to funds managed by a single manager, as opposed to a team, and we keep only

[4] An earlier version of this paper used CRSP as the only source for fund manager names. Since recent literature (e.g., Massa, Reuter, and Zitzewitz (2010), and Patel and Sarkissian (2014)) shows that Morningstar has a significantly more accurate coverage of fund manager names, we use this database as our main source for names, in line with recent related work (e.g., Berk and van Binsbergen (2012), Pástor, Stambaugh, and Taylor (2014)). To match Morningstar to CRSP, we follow the procedure described in Pástor, Stambaugh, and Taylor (2014). In a small number of cases where Morningstar Direct does not provide a fund manager name but CRSP does, we use the information from CRSP. Furthermore, we manually screen manager names for different spellings, typos, etc. In some cases, a given fund is "intermittently" managed by a team: for example, the Dreyfus Premier S&P Stars Opportunities Fund is managed by Fred A. Kuehndorf in 2006, by a team including Fred A. Kuehndorf in 2007, and again by Fred A. Kuehndorf in 2008. In all such cases, we assign the long-run individual fund manager as the actual manager for the team-managed years, i.e. in our example Fred A. Kuehndorf is the fund's manager from 2006 to 2008.

managers that do not manage multiple funds. We further focus on actively managed equity funds with total net assets under management of at least \$5 million. We group together multiple share classes of the same fund using the Morningstar Direct and CRSP portfolio identifiers.

We merge these data, using the MFLinks database, to the mutual funds' quarterly holdings in the Thomson Reuters Mutual Fund Holdings Database. Further, we assign each stock in a given fund's portfolio to one of the Fama-French 12 industries, using the stock's historical SIC code (SICH) reported in the Compustat Fundamental Annual database (if available), or the SIC code reported in the CRSP Monthly Stocks database.

Table 3.1, Panel A describes our sample, which covers the period from 1992Q1 to 2012Q1. We have a total of 81 quarters, 4,024 fund managers in 2,609 funds and 26,612 unique ISPs. Funds have on average 10.0 ISPs per quarter, and an ISP "lives" for, on average, 29.5 quarters (median = 27.0). Managers are on average in our sample (managing any ISP) for a total of 24.6 quarters (median = 20.0). Panel B presents summary statistics for the industry shock indicators (IS) and the experience measure across all 441,282 manager-industry-quarter observations. About 8% of our observations come from industry shock quarters. The average of the experience measure is 0.37, and the maximum number of industry shocks experienced by a manager in our sample for a given industry is 13.

2.3.4. Measuring Fund Manager Performance

We present results from two broad approaches to measure the performance of fund managers. The first approach is based on measuring ISP performance from holdings. The second approach is based on analyzing trades. Because we observe fund holdings only at quarterly frequency, our performance measures do not capture managerial actions and trading within the quarter. Throughout, performance is measured before fees.

2.3.5. Holdings-Based Approaches

We start by constructing a series of daily ISP returns for all ISPs in our sample. The raw ISP return, R_{mtiq}, is defined as a weighted average of the returns of stocks in that ISP:

$$R_{mtiq} = \sum_{j \in i} w_{mij,q-1} R_{jt}, \tag{2.5}$$

where m denotes the fund manager, t denotes the day within the quarter, i denotes the ISP's industry, q denotes the current quarter, and $w_{mij,q-1}$ is the weight of stock j in the ISP at the end of the quarter $q-1$.

Our main measure of performance is the standard 4-factor model (Fama and French (1993), Carhart (1997)). Specifically, ISP performance is the α from the following regression which we run across all days t for each ISP in quarter q:

$$R_{mtiq} - R_{ftq} = \alpha_{mqi} + b_{mqi}\text{RMRF}_t + s_{mqi}\text{SMB}_t + h_{mqi}\text{HML}_t + m_{mqi}\text{UMD}_t + \varepsilon_{mtiq}. \tag{2.6}$$

R_{mtiq} is the return from equation (2.5), R_{ftq} is the risk-free rate, and RMRF, SMB, HML and UMD are the standard factors obtained from Kenneth French's website. We multiply α_{mqi} by 63 trading days and refer to this number as the risk-adjusted 4-factor ISP return, or, for brevity, the FFC alpha.

To minimize concerns that our results are specific to any one performance measure, we also use several other measures proposed in the literature. First, we also report results based on raw returns. Second, we use the 3-factor model. Third, Cremers, Petajisto, and Zitzewitz (2013) argue that mutual fund performance measures based on the standard factors can be biased, and propose alternative factors. We therefore use their 4-factor model, which replaces the factors in equation (2.6) with proxies for those factors constructed from benchmark indexes. Fourth, we also use their 7-factor model.[5] Fifth, as we are using daily data, stale prices could potentially

[5]Those models are labeled IDX4 and IDX7 in Cremers, Petajisto, and Zitzewitz (2013). We refer the reader to that paper for details on the factor construction. We obtain the factor return

be an issue. We therefore also report results from a Dimson (1979) correction as implemented by Lewellen and Nagel (2006), who estimate equation (2.6) with the sum of three lags of the excess market return as an additional factor.

Next, as an alternative to factor models, we present results from the characteristic-adjusted holdings-based performance measure of Daniel, Grinblatt, Titman, and Wermers (1997) (DGTW). Gormley and Matsa (2014) point out that regressing the DGTW measure on other variables, in our case the experience proxy, will generally lead to biased estimates. Following a recommendation by those authors, we therefore present results also for a modified DGTW approach (DGTW*) in which we regress the FFC alpha on a full set of benchmark-quarter fixed effects. To get ISP benchmarks, we compute for each ISP-quarter the weighted average book-to-market, size, and momentum quintiles of all stocks in that ISP. DGTW* therefore combines the within-quarter risk-adjustment of the 4-factor model with the benchmark-adjustment of the DGTW approach.

Finally, we also use a performance measure due to Cohen, Coval, and Pástor (2005) that measures performance of a fund managers by their holdings of stocks that are concurrently held by other skilled managers. We use the 4-factor alpha as an input to constructing this measure.

2.3.6. Trading-Based Approach

Analyzing changes ("trades"), rather than levels of portfolio holdings, has been suggested as a potentially more powerful way of detecting skill (e.g. Chen, Hong, Huang, and Kubik (2000), Kothari and Warner (2001)). We therefore use a trading-based approach as a second way to measuring ISP performance. Section 2.5 discusses the method and results in greater detail.

data from Antti Petaijsto's website.

2.4. Measuring Performance from Holdings

2.4.1. Sample Splits

Table 2.3 presents summary statistics for key variables in our dataset when we split our sample of ISP-level observations by fund manager ISP-level experience. Experienced ISPs represent about 19% of our total observations, and the mean experience level in this group is 1.96.

The average ISP has a 4-factor alpha before fees of 41 basis points per quarter, which is roughly in line with the fund-level estimates reported in Kacperczyk, Sialm, and Zheng (2005). There is a considerable difference in alphas across subsamples: consistent with our main hypothesis, the 4-factor alpha for experienced ISPs is a full 100 basis points higher. The results are very similar for the other risk-adjusted performance measures we consider, including the Fama and French (1993) 3-factor alpha (FF), Cremers, Petajisto, and Zitzewitz (2013) 4- and 7-factor alphas (CPZ4, CPZ7), 4-factor alpha with Dimson correction as in Lewellen and Nagel (2006) (LND), and the Cohen, Coval, and Pástor (2005) "company you keep" 4-factor alpha (CCP). While the difference is not statistically significant in this simple sorting exercise, experienced managers also outperform their inexperienced counterparts using the DGTW measure.

Note that experienced and inexperienced ISPs hold different types of stocks. Experienced ISPs have similar market betas, but load significantly less on value, size and momentum. As exposure to those factors is associated with a risk premium, this explains why we see no meaningful difference in raw returns, but substantial differences in the other models. Controlling for value, size, and momentum is thus important to accurately compare managerial performance by experience.

Experienced ISPs are larger, older, and part of larger and older funds. Experienced ISPs have larger industry shares, i.e., funds hold more of their assets in experienced industries. As we require industry shares to exceed 10% in order for experience to increase, this difference is partly by construction.

Following Kacperczyk, Sialm, and Zheng (2005) we compute an Industry Concentration Index (ICI). ICI is the sum of what we label “ICI components”. ICI components are for each fund-industry-quarter the squared deviation of the industry share of the fund from the average industry share across all funds in this industry and quarter. The data show that ICI, a fund-level variable, differs only little across experienced and inexperienced ISPs. By contrast, the ICI components of experienced ISPs deviate substantially from the average ISP. Note that the fixed effects we use in our main tests below eliminate any variable on the fun-level, including ICI, so the main results in this paper are orthogonal to the results presented in Kacperczyk, Sialm, and Zheng (2005).

Turning to managerial characteristics, managers of experienced ISPs have significantly longer tenure and industry tenure. Interestingly, we find no meaningful difference between experienced and inexperienced managers in terms of SAT score of their undergraduate institution, which we were able to collect for a subsample of 839 fund managers. This provides first evidence suggesting that the better performance of experienced managers we document is unrelated to baseline skill.

To provide some insight into how performance differences evolve over time, Figure 2.1 shows the cumulative 4-factor risk-adjusted performance from investing in a hypothetical portfolio of ISPs of experienced and inexperienced managers. Over our 20 year sample period, the experienced ISP portfolio has a performance of almost 110%, while the inexperienced ISP portfolio yields a risk-adjusted return close to zero over most of the sample period.

2.4.2. Regression-Based Evidence

The sorting results from the previous section show that experienced ISPs outperform by most standard performance measures. This is in line with managers learning from past industry shock experience. In this section we analyze if those sorting results carry over to a more rigorous multivariate setting.

As our baseline, we estimate the following version of equation (2.3):

$$\alpha_{mqi} = \lambda_{mq} + \beta_1 I(E_{mqi} > 0) + \beta_2 X_{mqi} + \varepsilon_{mqi}. \tag{2.7}$$

Here λ_{mq} are manager $\times$ quarter fixed effects; $I(E_{mqi} > 0)$ is an indicator equal to 1 if E_{mqi}, the experience of manager m in industry i in quarter q, is greater than zero; and X_{mqi} is a vector of control variables. The main coefficient of interest is β_1 which captures the impact of experience on ISP performance.

The manager $\times$ quarter fixed effects ensure that estimates are not driven by any variable that is fixed for the same manager in a given quarter. As highlighted above, this includes tenure, baseline skill, fund characteristics, and economy-wide effects. In most of our tests, X_{mqi} includes a dummy equal to one if the ISP's industry is going through an industry shock in the current quarter, because it is correlated with both experience and our performance measures. We allow standard errors to be correlated across ISPs managed by the same manager and across ISPs in the same industry in a given quarter, i.e. they will be of the general form:

$$\varepsilon_{mqi} = \nu_{mq} + \nu_{qi} + \bar{\nu}_m + \bar{\nu}_q + \eta_{mqi}, \tag{2.8}$$

where $\bar{\nu}_m$ and $\bar{\nu}_q$ are manager and quarter fixed effects and ν_{mq} and ν_{qi} are idiosyncratic factors on the manager-quarter and industry-quarter level, respectively. The manager $\times$ quarter fixed effects parametrically control for $\bar{\nu}_m$, $\bar{\nu}_q$, and ν_{mq} (because neither of these variables varies within manager-quarter cell), and we capture ν_{qi} by clustering at the industry-date level (Petersen (2009)).[6]

Table 2.4, Panel A presents presents our main results. The difference in risk-adjusted performance between experienced and non-experienced ISPs is 1.38% per quarter using the FFC 4-factor risk adjustment. This effect is economically large and shows that the unconditional 4-factor alpha difference documented in Table 3.1 cannot be explained by time-invariant factors on the manager-quarter level, such as tenure and skill. While we find a statistically and economically meaningful difference even in raw returns, the fact that experienced ISPs load less on value, size, and momentum means that results get stronger once we adjust performance for these factors.

The alternative performance measures we consider also show a positive association between experience and performance. The CPZ and CCP measures yield an experience effect of about 1.1% to 1.3%, and the LND measure suggests that stale prices are not an issue in our setting. The return difference is smaller for the DGTW measure, but, at 56 basis points per quarter, still very large in absolute terms. The modified DGTW measure, which adds benchmark $\times$ quarter fixed effects to the FFC model, yields a difference of 1.3%, similar to our benchmark.[7]

In sum, Panel A presents strong evidence for a positive link between experience and performance that is robust to different methods of risk-adjusting returns and shows up even in the raw returns. Finally, an F-test shows that the null hypothesis of the manager $\times$ quarter dummies being jointly zero can be rejected at any conventional significance level (p-value < 0.001).

In Panel B, we replace the experience dummy by a set of dummies equal to unity if E_{mqi} is equal to one, two, or more than two, respectively.[8] This non-linear specifi-

[6]We show in the Appendix that double-clustering by industry-date and fund yields very similar results as our baseline.

[7]The number of observations drops in columns (4) and (5) due to the availability of CPZ portfolio returns and in columns (8) and (9) due to the availability of DGTW-benchmark-assignments from Russ Wermers' website.

[8]We group observations with experience levels ≥ 3 in one bucket since such high experience

cation allows us to test the incremental impact of additional units of experience. The panel shows that, across all our performance measures, the first unit of experienced is most valuable and that additional units of experience tend to increase relative outperformance at a decreasing rate. Figure 2.2 presents the experience-performance relationship graphically for selected performance measures. This evidence is informative, since decreasing marginal benefit of experience is exactly what we would expect if the experience variable captures learning. By contrast, if the results in Panel A were somehow spuriously induced by our empirical method, it would not be obvious why the relation is concave.

In Panel C, we repeat the analysis from Panel A including an interaction term between experience and the industry shock indicator IS. The aim is to see if past experience is particularly valuable inside or outside future shock periods. We find that experience is valuable outside industry shock quarters for all our performance measures. With the exception of raw returns, experience is even more valuable in future shock periods. Conditional on being in an industry shock quarter, experienced ISPs outperform inexperienced ISPs by 4.63% for the 4-factor risk adjustment.

Overall, the results from Table 2.4 confirm the results from the univariate sorts. Experienced managers outperform inexperienced managers across a range of performance measures, experience is beneficial at a decreasing rate, and relative outperformance is particularly pronounced in industry shock quarters. Because of the manager $\times$ quarter fixed effects, the experience effect cannot be driven by tenure, baseline skill, or any other variable that does not vary within manager and date.

2.4.3. Placebo Tests

We run two placebo tests to make sure our findings are neither spuriously induced by how we construct the experience measure, nor by how we run our regressions. In the first test, we generate 10,000 sets of placebo industry shocks, where we randomly

levels represent only a small fraction of our observations ($< 5\%$).

choose one industry every quarter and assign it an industry shock. Hence, for each ISP and trial we obtain a new experience measure, which we refer to as "placebo" experience. We then rerun our baseline regression with this placebo experience measure, using the 4-factor alpha as the dependent variable.

For brevity, we refer to the experience measure used so far, based on the actual industry shocks, as the "true" experience. Placebo and true experience are mechanically positively correlated ($\rho = 0.4$ in our sample) because they can only go up. To make sure we are not picking up this correlation, we include both true and placebo experience measures in our regressions. The aim of the placebo test is then twofold. First, we check if, conditional on our experience variable, a placebo variable would have a strong effect on fund returns. Second, we check if our experience measure is robust to the inclusion of other, potentially correlated, placebo experience measures.

Figure 2.3 summarizes the results. The placebo coefficients are centered near zero and are often negative. By contrast, the coefficient on the true experience variable is centered near the baseline estimate of 1.38. The distribution of the true estimates is much tighter than the distribution of the placebo estimates. Even the largest coefficient we see on the placebo measure across all 10,000 runs is smaller than our baseline estimate of 1.38. These results are reassuring. They show that it is very unlikely that our experience measure is large and significant by chance. There is nothing in the construction of the variable, or the econometric approach, that would mechanically induce the effect. The explanation most consistent with these results is that the experience measure is picking up variation that is truly informative for predicting ISP performance.

As a second robustness test, we follow the bootstrap method of Kosowski, Timmermann, Wermers, and White (2006) and simulate 1,000 samples of ISP returns, imposing that alpha in the simulated data is zero. This procedure, described in greater detail in the Appendix, is using as an input 4-factor model residuals obtained from estimating equation (2.6). The results presented in the Appendix show that *all* alphas and *all* t-statistics across the 1,000 placebo runs are substantially below the alphas and t-values we find in our baseline analysis. There are two implications. First, those results show there is nothing mechanical in our procedure that would lead us to obtain higher alphas for experienced ISPs. We correctly fail to detect alpha in a placebo test where there is none by construction. Second, and more importantly, those results show that the large difference in alphas between experienced and inexperienced ISPs we find in our main tests cannot be induced by sampling variation ("luck") alone, even if we account for the fact that the cross-sectional distribution of alphas may be distinctly non-normal.

2.4.4. Exposure and Learning Intensity

Our definition of experience in equation (2.4) requires managers to hold at least 10% of their portfolio in a given industry before they can acquire experience.[9] In this section, we analyze the role of the weighting term in greater detail.

We start by asking if it is important to include a weighting term in the first place. We therefore run a horserace between our experience measure and an otherwise identical measure without the 10% requirement. The alternative measure, which we call Past IS, is then a simple count of the past industry shocks experienced by a manager. Table 2.5, specification (1) – which is otherwise identical to Table 2.4, specification (3) – shows that the Past IS measure has essentially no power to explain performance, while our baseline experience measure is largely unchanged. This directly shows that the weighting term is important: completely consistent with a learning story, putting more weight on an industry shock when managers are more exposed to that industry increases our ability to explain ISP performance.

[9] In the Appendix we show that the specific functional form of the indicator function is not very important. We obtain very similar results when we replace the 10% cutoff with an indicator that is one for the largest three industries for each fund, or with an indicator that is one if the weight assigned to an industry in a given fund is higher than the median industry weight across all funds.

The 10% threshold is close to an equal-weighted portfolio across the 12 industries we use. Managers may research more intensively industries with higher weights in their portfolios, so larger exposures may imply more learning. To investigate this, we replace the 10%-threshold indicator by the raw industry weight. Specifications (2) and (3) show that this modified measure has very similar properties to our baseline. The key advantage of the modified measure is that it allows us to isolate the impact of the industry weight from the number of industry shocks, which we do in specification (4). To conduct this test, we group all ISPs with positive modified experience measure into three groups by industry weight *conditional on* the number of shocks experienced. Hence, the high group contains ISPs that, for a given number of shocks, have been substantially exposed to the shock industry (the average industry weight in this group is 21.6%), while the average exposure in the low group is positive, but closer to zero (average industry weight is 4.0%).

The results in specification (4) show that experience depends strongly on the industry weight. Managers with very large exposures have significantly better subsequent performance, while managers with small positive exposures have effects very close to the inexperienced group. These findings are consistent with the idea that larger exposure to a shock industry increases learning intensity.

2.4.5. Difference-In-Differences Results

We use a difference-in-differences approach as an alternative way to document learning effects. We start with all ISPs that go through an experience shock in quarter q and do not have any industry shock in the preceding and subsequent 4 quarters. The event window is then $t \in [-4, +4]$ around the experience shock quarter q.[10] To get a clean comparison group, we retain all other ISPs managed by the same manager in the same quarter with complete data in the event window (i) if they have the same industry tenure and (ii) if they do not go through an industry shock in the event

[10]Similar results obtain with $[-5, +5]$ and $[-6, +6]$ event windows.

window. We then test if the performance of the ISP that goes through an industry shock improves relative to the other ISPs managed by the same manager over the same period, by estimating:

$$\alpha_{mti} = \lambda_{mqt} + \beta I(\Delta E_{m0i} > 0) + \varepsilon_{mti}, \tag{2.9}$$

separately for each quarter t , where $I(\Delta E_{m0i} > 0)$ is an indicator function equal to one for ISPs that go through an experience shock in quarter $t = 0$ ("treated ISPs"), and where λ_{mqt} are manager $\times$ quarter $\times$ event-quarter fixed effects.

Table 2.6 presents the results. As expected, ISPs that go through a shock do worse than other ISPs during the shock quarter. More interestingly, while there is no evidence of outperformance before the shock (Panel A), there is strong evidence of outperformance after the shock (Panel B). As shown in Panel C, the difference-in-differences of performance between $t \in [-4, -1]$ and $t \in [+1, +4]$ is 2.53 percentage points (t = 2.78). Figure 2.4 presents the results graphically. Consistent with learning from the industry shock experience, ISPs that go through a shock perform persistently better.

The results raise the bar for alternative explanations, because any omitted variable not captured by manager $\times$ date fixed effects would have to change precisely around the shock, and it would need to induce a long lasting performance differential between experienced and inexperienced ISPs. Note that both treatment and control group in our test live for the entire event period, so selection effects cannot explain those findings.

2.5. Measuring Performance from Trades

Analyzing changes in holdings ("trades"), rather than levels, may be a more powerful way to detect skill, because trades more closely reflect active managerial decisions (e.g., Chen, Hong, Huang, and Kubik (2000)). Finding a positive relation between experience and performance using trades would be useful in our setting because it would further minimize concerns that our previous results are due to misspecified regression models. We provide two sets of trade–based results.

2.5.1. Performance of Buys versus Sells

In our first set of trade-based results, we compare the performance of buys and sells. For each ISP and holdings report date, we classify a stock in that ISP as a net buy, if the observed change in the portfolio weight from beginning to end of the holdings period is larger than what would be predicted from stock price appreciation alone; it is a net sell if the observed change is smaller than the predicted change. Specifically, we follow Kacperczyk, Sialm, and Zheng (2005) and define:

$$\mathrm{NB}_{siq} = 1 \quad \text{if} \quad w_{siq} - \frac{w_{siq-1}(1 + R_{siq})}{\sum_s w_{siq-1}(1 + R_{siq})} > 0 \tag{2.10}$$

where w_{siq} is the weight of a given stock s belonging to industry i in the fund's portfolio between two fund reporting dates $q - 1$ and q, and R_{siq} is the stock's return between those dates. Net sells are defined analogously. This ensures that we are focusing on active trading by the fund as opposed to a mechanically changing composition of the fund's portfolio due to price changes.

We then regress, for each stock in each ISP and report date, its next-quarter return on a dummy equal to one if the stock was a net buy, an $E > 0$ dummy, an interaction term between the two, as well as different sets of fixed effects. We present results for three performance measures: raw returns, FFC returns, and DGTW returns and find overall very similar results.

The top panel in Table 2.7, specification (1) uses raw returns without any fixed effects. This is equivalent to computing the performance of a hypothetical equal-weighted portfolio long in stocks bought by each ISP and short in the stocks sold. Consistent with Chen, Hong, Huang, and Kubik (2000), buys outperform sells by a considerable margin for all fund managers. Importantly, buys outperform sells more for experienced managers. The difference is 38 basis points over the next quarter and highly statistically significant (t-statistic = 4.77).

The remaining two panels show we obtain very similar results when we use FFC or DGTW-adjusted returns instead of raw returns. This highlights an important advantage of the buy-sell approach: because we are essentially focusing on the difference between stocks bought and sold, risk-adjusting the individual returns does not matter much as long as buys and sells have similar risk characteristics. Omitted risk factors are therefore particularly unlikely to be an issue for those results.

Specifications (2) to (4) show that results are largely unchanged when we include manager × date, industry × date, or manager × industry fixed effects. This is reassuring and highlights that effects cannot be driven by any variable, observable or unobservable, that does not vary on those levels. The richness of the data allows us to even include manager × firm fixed effects alongside manager × date fixed effects. The resulting specification (5) indicates that trades by *the same manager* become better predictors of subsequent returns for *the same stock* after the manager obtains experience. This constitutes direct evidence in support of a learning story.

2.5.2. Trading around Earnings Announcements

In our second set of trade-based results, we analyze trades before earnings announcements. Earnings announcements are important corporate events in which fundamental information is revealed to the market. In addition, they are recurrent, and thus provide the fund manager with a natural opportunity to apply her experience. The literature has already established that some managers can predict earnings surprises, so the notion that it is possible to trade profitably in anticipation of a surprise is not implausible (e.g., Baker, Litov, Wachter, and Wurgler (2010)).

To implement the test, we collect all earnings announcements occurring in our sample period from IBES. We then estimate:

$$CAR_{msiq} = \lambda + \beta_1 I(E_{mqi} > 0) + \beta_2 I(\mathrm{NB}_{siq} > 0) + \beta_3 I(E_{mqi} > 0) \times I(\mathrm{NB}_{siq} > 0) + \beta_4' X_{msiq} + \varepsilon_{msiq} \tag{2.11}$$

where CAR denotes the cumulative abnormal return over a three-day $(-1, +1)$ window around the following earnings announcement date, $I(\mathrm{NB}_{siq} > 0)$ is one for net buys and zero for net sells, X_{msiq} are control variables, and λ are fixed effects. The announcement return is defined as:

$$CAR = \prod_{t=-1}^{+1} (1 + R_{st}) - \prod_{t=-1}^{+1} (1 + \bar{R}_{st}) \tag{2.12}$$

where $\bar{R}_{st}$ is the return on a matching size and book-to-market portfolio, as in Hirshleifer, Lim, and Teoh (2009). Following those authors, we control for book-to-market, firm size, turnover, institutional ownership, reporting lag, and the number of analysts covering the stock in IBES.

Prior literature has documented $\beta_2 > 0$ in equation (2.11). Our key prediction is $\beta_3 > 0$ as, under the learning hypothesis, experienced managers are better able to trade in anticipation of earnings surprises. This is indeed what we find in Table 2.8. Across the different specifications, net buying by experienced managers is associated with announcement returns that are 12 to 24 basis points higher. Relative to the baseline effect on the buy variable of between 28 and 44 basis points, this is an economically large increase. Consistent with the prior literature, the baseline effect on buys indicates that buying by mutual funds is a strong predictor of positive subsequent abnormal announcement returns.

As before, we include additional fixed effects. Specifications (2) to (4) show that results are largely unchanged when we include manager × date, industry × date, or manager × industry fixed effects. We also include manager × firm fixed effects alongside manager × quarter fixed effects in specification (5). The results provide again direct evidence of a learning story: the same manager becomes better at predicting earnings surprises for the same stock upon obtaining experience.

While we do not suggest trading before earnings surprises is the *only* channel through which learning could translate into higher returns, this analysis is useful for two additional reasons. First, the dependent variable in those tests are three-day returns and our results therefore unlikely depend on any specific risk-adjustment. Second, earnings surprises are always *relative* to earnings expectations. Because the stock price can drop substantially even after announcing high absolute earnings, observing that experienced fund managers are better at predicting earnings surprises is hard to justify by fundamentals and much more likely to be indicative of true skill.

In sum, Table 2.8 shows that experienced managers are better at predicting announcement returns, consistent with the hypothesis that the experience proxy captures learning by the fund managers. The robustness of the results to the inclusion of fixed effects make alternative stories particularly unlikely.

2.6. Industry-Specific Alternative Explanations

Our approach of comparing returns within manager across industries at the same point in time rules out a confounding impact of a large range of variables suggested to be important by the prior literature, including managerial baseline skill and tenure. In this section, we address three potential industry-specific concerns. First, are our results reflecting industry-specific managerial skill? Second, are there any omitted industry-level variables that drive our results? Third, can industry-specific attrition from the sample induce our results?

2.6.1. Industry-Specific Baseline Skill

The baseline model in equation (2.3) uses manager $\times$ quarter effects to eliminate the potentially confounding impact of unobserved managerial baseline skill, but *industry-specific* baseline skill would not be captured by those fixed effects.[11] To be clear about the difference, industry-specific baseline skill is a skill managers are endowed with *before* they enter our sample, perhaps because of a prior career in that industry, while experience is obtained in an industry *while* fund managers are in our sample. Industry-specific baseline skill can only matter in our context if it is not captured by overall IQ, education, or general ability of the manager, that is, there needs to be within-manager-and-date variation in skill across industries.

Our results in the previous section make an industry-specific baseline skill explanation unlikely. First, industry-specific baseline skill, a time-invariant difference in ability across industries within manager, by definition cannot explain our difference-in-differences results in Section 2.4.5. Second, industry-specific baseline skill does not predict a decreasing marginal benefit of experience. Third, it cannot explain why managers make better trades after obtaining experience in Table 2.7. Specifically, we find unchanged trading results when we control for manager $\times$ industry effects in specification (4) of Table 2.7, and therefore compare the same manager in the same industry before and after obtaining experience, which eliminates any manager-industry specific time-invariant variation, including industry-specific skill. Fourth, industry-specific skill cannot explain why the same manager gets better in predicting earnings surprises in specification (4) in Table 2.8. In short, while industry-specific baseline skill could explain stable differences in performance, it does not explain the changes in performance after obtaining experience we document.

We provide additional results here. We start by using observable variables that should be highly correlated with industry-specific skill. The first one is the ICI component, measuring how much the industry share for a given ISP deviates from the average. Table 2.9, specification (1) shows that the ICI Component variable is positively related to fund returns, consistent with the findings of Kacperczyk,

[11]Industry-specific tenure would also not be captured, but, since it is observable, it is easy to control for. We find all our results go through when we control for industry-specific tenure (see Appendix).

Sialm, and Zheng (2005). However, an additional unit of experience is valuable even conditional on the ICI component. In fact, the size and significance of the experience coefficient are hardly different from the baseline case.

The second industry skill proxy is industry share, i.e. the fraction of the fund's assets allocated to industry i. If a manager is inherently better at managing stocks in industry i, she might on average overweight it in her portfolio. Skilled managers might therefore be more likely to pass the threshold required to get an experience shock. Specification (2) shows that industry share indeed has a positive impact on fund performance. But, again, the experience coefficient is quite similar to our baseline model.

As a final proxy, we use the manager's pre–experience alphas (the average alpha while $E = 0$) as a direct estimate of industry-specific baseline skill. Specification (3) shows that the pre-experience alpha is strongly positively related to subsequent performance. The experience coefficient is lower when controlling for the pre-experience alpha but, at 88 basis points per quarter, still economically large and highly statistically significant.

All three variables above are positively related to performance, so there might be a role for industry-specific baseline skill. However, it is also possible that the variables themselves are mainly driven by learning effects, rather than industry-specific baseline skill. Because our focus is on identifying the role of experience, and because we have shown that our conclusions on the experience effect are largely unchanged by the inclusion of those variables, we do not pursue the issue further here.

A final concern could be that those ISPs that never obtain experience are short-lived underperformers. Our results could therefore be driven by the survival of more skilled ISPs. To address this issue, we repeat our analysis but drop all ISPs that never obtain experience from the sample. We are therefore comparing the same set of ISPs before and after obtaining experience. The results in specification (4) indicate that ISPs perform 1.2% per quarter better after becoming experienced, so ISPs that never become experienced are not a concern. We address further issues about attrition of underperforming ISPs from the sample in Section 2.6.3.

Overall, we conclude that industry-specific baseline skill is not spuriously inducing our earlier results.

2.6.2. Omitted Industry-Level Variables

A second potential concern could be omitted industry variables, such as industry-level risk factors not captured by the four-factor model. Some of our evidence above should already attenuate this concern. For example, it is not obvious how omitted industry variables would explain a decreasing marginal benefit of experience. It is also not obvious how they would explain the larger experience effect for larger holdings, given that alpha is a percentage measure. Finally, it is not obvious how they would explain the long-lived effects documented in the difference-in-differences results. While those results raise the bar for explanations based on industry variables, we further examine the issue in this section.

We first examine the issue for the trade-based performance tests. For the buy-sell analysis, specification (3) of Table 2.7 shows that the results are not materially affected by the inclusion of industry $\times$ date fixed effects. Hence, the outperformance of stocks bought by experienced managers is larger than the outperformance of stocks bought by inexperienced managers even if we compare trades in the same industry and quarter. For the earnings announcement tests (Table 2.8), we find the same result: including industry-date fixed effects leaves our results almost completely unchanged. Hence, any omitted industry-level variable, including time-varying and unobservable ones, cannot induce those results.

Next, we examine the issue for the holdings-based performance measures. We first consider industry return as an additional control variable to our main holdings-based regression. Table 2.10, specification (1) shows that our baseline results are essentially unaffected. Specification (2) adds 8 lags of industry returns to investigate if industry return dynamics, such as mean-reverting industry returns, impact our findings. Results indicate that industry dynamics are not driving our findings. This is consistent with our results in Table 2.4, Panel C: there we have shown that experienced managers tend to do particularly well in future industry shocks. If our documented outperformance were due to industry-level reversals, we should instead see that managers do especially well *outside* industry shocks, and poorly in industry shock quarters.

We next include industry volatility and 8 lags of industry volatility, measured as the standard deviation of daily returns in the quarter. Industries with greater uncertainty may be industries where smarter managers have an opportunity to apply their skill, which may be unrelated to experience. The results in specifications (3) and (4) show that this does not affect our estimates. Specification (5) shows that including both industry returns and industry volatility, together with their lags, is equally inconsequential for the experience effect.

Industry returns and industry volatility are observable variables that should capture a large fraction of potentially confounding variation at the industry level. It is still possible, however, that there are omitted unobservable time-varying variables, orthogonal to industry returns and volatility, driving some of our holdings-based results. We therefore include industry $\times$ quarter fixed effects along with the manager $\times$ quarter effects in specification (6). The coefficient on experience is now lower. It is important to note that, even then, the remaining effect of a risk-adjusted 88 basis points annually means that experience is a first-order driver of ISP returns. With a t-value of 2.96, the estimate is also highly statistically significant.

To sum up, we find for the holdings-based performance measures that the relation between experience and performance is unrelated to the observable variables industry return and industry volatility. If we include industry $\times$ quarter fixed effects, effects are attenuated, but still highly significant, both economically and statistically. The results from trading-based performance measures are completely immune to industry-specific factors. Overall, then industry-specific omitted variables are not affecting our main conclusion: experience is a first-order driver of fund manager performance.

2.6.3. Industry-Specific Attrition

Funds and fund managers decide in which ISPs they invest. Hence, a potential concern could be that worse ISPs leave our sample, thereby inflating our estimates. More technically, we have assumed in our baseline tests that exit is exogenous, while it may be endogenous.

Note that attrition is not an issue for us if selection is based on baseline skill, i.e. general ability that does not vary by industry. This is because the manager-date fixed effects control for any factor on that level, including any inverse Mills ratio from a well-specified Heckman selection model. Managers leaving the sample with *all* their ISPs is therefore not a problem for our estimates.

Theoretically, industry-specific attrition can affect our results in two ways. First, it could lead us to spuriously find learning effects when none are present. Second, it could point to a different learning mechanism, namely managers and funds learning about industry-specific fund manager skill (e.g., Seru, Shumway, and Stoffman (2010)).

While those concerns are theoretically valid, attrition after industry shocks is actually extremely rare in our data. The average probability of exiting an industry after going through an industry shock is 0.85%, relative to ISPs that went through a shock, and 0.07% relative to all ISPs in a quarter. For the average quarter in our data, this implies that 4 ISPs drop out of our sample following the shock. Those numbers are simply too small to have a meaningful impact on our findings. We present supporting evidence from a simulation exercise in the Appendix.

A second argument why selection and attrition is unlikely a significant driver of our results comes from the difference-in-differences estimates in Section 2.4.5. Those tests show that managers get better after a shock, i.e, alpha changes within manager, which is consistent with learning. By contrast, a selection/attrition story is about stable within-manager alphas spuriously correlating with experience through sample composition effects. Importantly, we condition on managers that stay in the sample for 9 quarters in the difference-in-differences regressions, so selection and attrition is, by construction, not a concern for those tests.

2.7. Extensions

2.7.1. Learning from Industry Booms and Other Periods

The above findings support the idea that fund managers gain experience during industry shock quarters, i.e., in bad times. But managers might also learn from other periods, and in particular from booms. This may not be implausible since some of the factors that motivate learning in industry shocks apply also to booms: industry booms are salient events that attract investor and media attention and, because of tournament incentives, managers might disproportionately care about booms for career and bonus reasons. On the other hand, booms may be the result of bubbles, and investor exuberance and media hype may make it harder to extract informative signals. Further, the literature on reinforcement learning cited in the introduction suggests that, because of the human tendency to credit yourself for success and blame others for failure (the self-serving attribution bias), there might be an increased tendency among fund managers in booms to confuse luck with skill. Both factors might hamper learning in booms. Consistent with the idea that learning in booms and busts are not symmetric, Lapré and Nembhard (2010) write in their survey of the organizational learning literature:

> "In contrast [to failure], success encourages preservation of the status quo, complacency about experimenting with new ideas, and risk aversion. Thus, success inspires a narrower scope of learning and change than failure." (our addition to the text in square brackets)

Hence, while there is reason to believe learning would be more pronounced in bad times than in booms, this is ultimately an empirical issue.

In Table 2.11 we rerun our baseline regressions, including IS_n and E_n, where subscripts n denote IS and experience measures constructed on the n-th industry return rank, ordered from 1 (bust) to 12 (boom). We show results from 12 different regressions, one in each line. We include the baseline parameters IS_1 and E_1, the shock and experience measures we have been using all along, in all regressions as additional controls. This is necessary, because the other experience measures are correlated with our baseline experience measure (between 0.27 and 0.47 in our data). If we did not include baseline experience, we could not tell if an observed effect would obtain because it is actually in the data, or because the used experience measure is correlated with our baseline experience measure.

The first line in Table 2.11 reproduces Table 2.4, Panel A, specification (3). The second line shows that the experience measure based on industry shocks E_1 is effectively unchanged while the experience measure E_2, constructed based on industry rank 2 (the second lowest rank), is much closer to zero and insignificant. A striking feature of the table is that the coefficient on E_1 is always highly significant and always markedly higher than the coefficients on alternative experience measures, while there is no clear pattern for the sign, size, and significance of the alternative experience measures.

Both the point estimate and statistical significance increases slightly for the highest industry ranks 11 and 12, although the pattern is not monotonic, and results for E_{11} are actually stronger than results for E_{12}. Overall, the analysis shows that while experience in industry shock periods always has a strong impact on fund returns, the evidence for learning effects in booms and other periods is at best weak.

2.7.2. Learning from the Time-Series of Industry Returns

Our baseline results have focused on fund manager experience based on industry shocks that are defined cross-sectionally, i.e., whenever an industry is the worst performing one in a given quarter. It is also plausible to think of industry shocks in terms of the time-series. For example, investors and the media frequently compare returns this period to past returns. We investigate if we can find experience effects also when experience is gained from industry shocks defined from the time-series of industry returns.

We compute a time-series based industry shock dummy IS^{TS} as follows: for every industry and quarter, we set IS^{TS} to one if the industry return is below the 10th percentile of returns in this industry over the last 40 quarters. We then compute a time-series based experience measure E^{TS} exactly as in equation (2.4), using IS^{TS} instead of IS.

Table 2.12 shows results that are qualitatively similar to the baseline case. Panel A splits the sample into experienced and non-experienced ISPs. Also here, we see that for most measures, experienced ISPs outperform inexperienced ISPs. Panel B replicates our regressions from Table 2.4. We again obtain qualitatively similar, quantitatively somewhat weaker, results. Specification (3) shows that, conditional on IS^{TS}, and net of any potentially confounding factor that does not vary within a manager across ISPs at a given quarter, experience increases ISP performance by 86 basis points (t-value = 3.73). Overall, the data are consistent with the view that managers learn also from the time-series of industry-returns.

One interesting implication of the time-series findings is that they add a new dimension to the literature cited in the introduction that finds mutual funds tend to do better in recessions. While existing explanations have focused on the higher marginal utility of wealth for investors in downturns (e.g., Glode (2011)), or the idea that obtaining informative signals becomes more valuable in downturns (e.g., Kacperczyk, Nieuwerburgh, and Veldkamp (2011)), our theory implies that mutual funds outperform in downturns because some fund managers learn from past downturns. The correlation between IS^{TS} and the market factor is in line with this idea ($\rho = -0.54$).[12]

2.7.3. Learning Spillover Effects

Our identification strategy is based on comparing ISPs for the same manager at the same point in time. This prevents us from identifying learning spillovers where what a managers learns in one ISP can be used to more profitably manager ISPs in other industries.

Importantly, any experience effect we uncover with our method is likely understating the true benefit of obtaining experience in a model which allows for experience spillovers. To see this, consider the polar case: if spillovers effects were very large, such that what a manager learns on one ISP can be transferred to another ISP one-for-one, we would not observe any experience effect using our method; both the ISP in the shock industry and all other ISPs for the same manager at the same point in time would have higher alphas, leaving the difference unchanged.

2.7.4. Experience at the Fund Level: EDX

In our last test, we investigate if the documented superior stock-picking ability of experienced managers at the ISP level shows up also at the fund level. We implement

[12] Note that our baseline effects are, by construction, not related to the business cycle as, there, we define industry shocks purely from the cross-section of industry returns. The cross-sectional IS measure has practically zero correlation with the market factor ($\rho = -0.02$).

this in the simplest way by looking at a weighted average of the individual industry experience measures (equation (2.4)), with weights corresponding to the weight of each industry in the fund at the end of quarter $q-1$, for each manager and quarter across all ISPs, to get a fund-level measure of experience: $\mathrm{EDX}_{mq} = \sum_i w_{mi,q-1} E_{mqi}$. An advantage of EDX is that it is, in principle, implementable in real time since it only depends on past holdings and past industry shocks.

To see if EDX is associated with higher returns, we sort funds into three EDX groups every month, low (bottom quintile of EDX), mid (quintiles 2 to 4), and high (top quintile). We obtain monthly fund returns after expenses from CRSP. We also compute before-expenses returns by adding 1/12 of the fund's expense ratio to the fund's return each month as in Fama and French (2010). Finally, we compute the monthly EDX portfolio return as equal-weighted average return across all funds in the respective portfolio.

Table 2.13, Panel A, shows that some of our findings from the ISP-level carry over also to the fund-level. Specifically, high EDX funds outperform low EDX funds by an economically substantive margin, based on the point estimates. Before fees, the difference based on on the point estimates is 14.4 basis points per month, or 1.7 percentage points per year. High EDX funds have an alpha of 11.3 basis points per month (1.4% per year) which is statistically significant (t-statistic = 2.24). While before fees high EDX funds outperform and low EDX funds break even, we find that after fees high EDX funds break even, while low EDX funds underperform. Panel B shows that sorting on tenure does not have EDX's ability to produce a meaningful spread in alphas.

All portfolio alphas in Table 2.13 are measured quite imprecisely. Since EDX is a weighted average of ISP level experience, finding high standard errors may not be surprising. Moreover, this pure sorting exercise does not allow us to control for the fixed effects we used above in our ISP analysis. Given these caveats, it is remarkable that the results line up as expected based on the ISP-level analysis. In sum, we conclude that experience effects can be detected also on the fund level. Refining the measurement of experience on the fund level may be a promising topic for future research.

2.8. Conclusion

We present a new approach to investigating the importance of learning by doing for fund managers. Our innovation is to exploit variation in experience across industry sub-portfolios (ISPs) *for a given manager at a given point in time.* We find that experience is valuable: ISPs managed by experienced fund managers outperform by 1.4% per quarter on a 4-factor risk-adjusted basis. Our approach ensures that this difference cannot be explained by factors that do not vary across ISPs for a given manager and quarter, including previously studied variables like age, tenure, education, IQ, corporate governance, fund characteristics, and the business cycle. Experience is associated with better trades, and the experience-performance relationship is increasing and concave. We find some supporting evidence that these results aggregate to the fund level. Measuring experience by a new EDX index that aggregates a manager's experience across ISPs, we find that high EDX funds outperform before fees, whereas low EDX funds do not.

Underlying our approach is the idea that experience and learning are not just linear functions of time. Specifically, we conjecture that investors learn relatively more in bad times, consistent with earlier investigations into learning by doing (e.g., Arrow (1962)). An important implication of our study for empirical researchers is that tenure might not be a powerful proxy for experience. Overall, our results suggest that learning by doing is important for professional investors in highly competitive markets, and that experience is a valuable fund manager characteristic investors should care about.

Our findings suggest a number of potentially fruitful areas for future research. For example, we have used a rather restrictive definition of experience on the industry-level, so our estimates of the value of experience may be lower bounds. It seems plausible that managers would also be able to obtain experience from other sources that do not vary by industry, and there may be learning spillovers across industries. It would be interesting to quantify the value of those other forms of experience. More broadly, it may be interesting to further explore how our ISP-level results can be optimally translated to the fund-level.

Tables

Table 2.1: Worst Performing Industries By Quarter

This table reports the worst performing industries for each quarter among all stocks in the NYSE, AMEX, and NASDAQ. We use the Fama-French 12 industry classification. Returns are value-weighted industry averages.

Quarter	FF12 Industry	Return	Quarter	FF12 Industry	Return
1992q1	Health	-0.131	2002q2	Business Equipment	-0.255
1992q2	Health	-0.059	2002q3	Business Equipment	-0.255
1992q3	Consumer Durables	-0.092	2002q4	Shops	0.002
1992q4	Oil, Gas, and Coal	-0.042	2003q1	Telecom	-0.109
1993q1	Health	-0.146	2003q2	Chemicals	0.054
1993q2	Consumer NonDurables	-0.075	2003q3	Telecom	-0.064
1993q3	Health	-0.024	2003q4	Shops	0.073
1993q4	Oil, Gas, and Coal	-0.072	2004q1	Consumer Durables	-0.023
1994q1	Health	-0.104	2004q2	Telecom	-0.026
1994q2	Consumer Durables	-0.065	2004q3	Business Equipment	-0.096
1994q3	Consumer Durables	-0.018	2004q4	Health	0.038
1994q4	Shops	-0.049	2005q1	Consumer Durables	-0.140
1995q1	Consumer Durables	0.005	2005q2	Chemicals	-0.054
1995q2	Oil, Gas, and Coal	0.032	2005q3	Consumer Durables	-0.025
1995q3	Oil, Gas, and Coal	0.020	2005q4	Oil, Gas, and Coal	-0.081
1995q4	Business Equipment	-0.037	2006q1	Utilities	-0.005
1996q1	Telecom	-0.022	2006q2	Business Equipment	-0.091
1996q2	Chemicals	-0.003	2006q3	Oil, Gas, and Coal	-0.028
1996q3	Telecom	-0.081	2006q4	Health	0.020
1996q4	Shops	-0.027	2007q1	Banks	-0.025
1997q1	Business Equipment	-0.044	2007q2	Utilities	0.003
1997q2	Utilities	0.055	2007q3	Consumer Durables	-0.061
1997q3	Chemicals	0.017	2007q4	Banks	-0.112
1997q4	Business Equipment	-0.109	2008q1	Banks	-0.153
1998q1	Utilities	0.048	2008q2	Banks	-0.166
1998q2	Manufacturing	-0.036	2008q3	Oil, Gas, and Coal	-0.265
1998q3	Banks	-0.212	2008q4	Consumer Durables	-0.397
1998q4	Oil, Gas, and Coal	0.004	2009q1	Banks	-0.234
1999q1	Utilities	-0.111	2009q2	Shops	0.082
1999q2	Health	-0.033	2009q3	Utilities	0.070
1999q3	Banks	-0.154	2009q4	Banks	-0.004
1999q4	Utilities	-0.077	2010q1	Utilities	-0.019
2000q1	Chemicals	-0.209	2010q2	Banks	-0.146
2000q2	Telecom	-0.142	2010q3	Banks	0.046
2000q3	Telecom	-0.118	2010q4	Utilities	0.038
2000q4	Business Equipment	-0.347	2011q1	Consumer Durables	0.007
2001q1	Business Equipment	-0.261	2011q2	Oil, Gas, and Coal	-0.057
2001q2	Telecom	-0.019	2011q3	Consumer Durables	-0.312
2001q3	Business Equipment	-0.347	2011q4	Business Equipment	0.080
2001q4	Telecom	-0.020	2012q1	Utilities	0.000
2002q1	Telecom	-0.089			

Table 2.2: Summary Statistics

The table presents summary statistics. Panel A provides key statistics about our sample. Panel B shows descriptive statistics of our main industry shock and experience measures. The sample is based on all single–manager mutual funds in the union of the CRSP Mutual Funds and Morningstar Direct databases, with available information identifying the fund manager, over the period 1992Q1–2012Q1.

Panel A: Sample

Number of Quarters	81
Number of Managers	4,024
Number of Funds	2,609
Number of ISPs	26,612
Number of Manager-ISP combinations	46,366
Avg. Number of ISPs per Fund (Median)	10.0 (11.0)
Avg. Life of ISP in Quarters (Median)	29.5 (27.0)
Avg. Life of Manager in Quarters (Median)	24.6 (20.0)

Panel B: Experience and Industry Shock Variables

Variable	Mean	St.Dev.	Min	Median	Max	N
IS	0.08	0.27	0.00	0.00	1.00	441,282
Experience	0.37	1.03	0.00	0.00	13.00	441,282

Table 2.3: Sample Splits

The table reports sample splits by experience for the main variables of interest. We report the sample average (All), the average for the subgroup of inexperienced managers ($E = 0$) and experienced managers ($E > 0$), as well as the t–statistic for the difference between the two subsamples. Reported t–statistics are based on standard errors that allow for clustering around industry × date in all rows except for fund manager characteristics, where standard errors allow for clustering at the manager level. Performance measures used are: Raw ISP returns, calculated as in equation (2.5), Fama and French (1993) three-factor alpha (FF), Fama–French–Carhart (1997) four-factor alpha (FFC), Cremers, Petajisto, and Zitzewitz (2013) four- and seven-factor alphas (CPZ4, CPZ7), four-factor alpha with Dimson correction as in Lewellen and Nagel (2006) (LND), Cohen, Coval, and Pástor (2005) "company you keep" four-factor alpha (CCP), and Daniel, Grinblatt, Titman, and Wermers (1997) CS-measure (DGTW).

Variable	All	$E = 0$	$E > 0$	t-stat
Experience	0.37	0.00	1.96	38.15
Performance Measures				
Raw ISP Return	3.19	3.20	3.12	-0.13
Market-Adjusted ISP Return	0.72	0.68	0.91	0.72
FF Alpha	0.43	0.30	1.00	3.74
FFC Alpha	0.41	0.22	1.23	5.32
CPZ4 Alpha	0.74	0.60	1.34	3.92
CPZ7 Alpha	0.76	0.60	1.46	4.55
LND Alpha	0.43	0.24	1.29	5.57
CCP Alpha	0.37	0.19	1.14	5.30
DGTW-Adjusted Return	0.39	0.35	0.54	0.82
Portfolio Characteristics				
4-Factor Loading MKT-RF	1.02	1.03	1.01	-2.26
4-Factor Loading HML	0.15	0.19	-0.01	-6.62
4-Factor Loading SML	0.25	0.26	0.20	-5.09
4-Factor Loading UMD	-0.01	0.01	-0.10	-5.41
Fund Age (quarters)	16.16	14.74	22.39	24.69
ISP Age (quarters)	14.77	13.22	21.51	27.96
Fund Size ($m)	959.51	894.79	1,241.82	16.10
ISP Size ($m)	94.55	68.82	207.84	24.47
Industry Share	0.11	0.10	0.19	32.48
Industry Concentration Index (ICI)	7.45	7.41	7.61	1.68
ICI Component	1.43	1.14	2.70	18.04
Fund Manager Characteristics				
Tenure (quarters)	12.78	10.82	21.32	30.91
Industry Tenure (quarters)	11.63	9.63	20.36	31.71
SAT Score	2,019	2,020	2,014	-0.53
N	441,282	358,993	82,289	

Table 2.4: Impact of Experience on ISP Returns

The table presents results from regressing ISP performance on experience. All regressions include manager × date fixed effects as well as an indicator variable equal to one if there is a shock in the industry of the ISP in the current quarter (coefficient not shown). Panel A presents the baseline results for the performance measures described in Table 2.3. In column (9), we assign each ISP-quarter to a DGTW-benchmark and then regress the ISP's FFC alpha on benchmark × date fixed effects. Panel B replaces the experience dummy by dummies indicating one, two, and above two units of experience. Panel C presents results for the interaction of experience with the industry shock indicator. t-statistics, reported in parentheses, are based on standard errors that allow for clustering around industry × date.

Panel A: Baseline

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience	0.566	1.005	1.376	1.091	1.180	1.369	1.303	0.561	1.304
	(2.01)	(4.48)	(5.75)	(4.69)	(5.19)	(5.97)	(5.82)	(2.38)	(6.19)
Mgr. × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.26

Panel B: Incremental Benefit of Experience

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience = 1	0.408	0.850	1.185	0.812	1.006	1.239	1.121	0.453	1.124
	(1.87)	(4.05)	(5.60)	(4.05)	(5.02)	(5.61)	(5.68)	(2.26)	(5.90)
Experience = 2	0.686	1.280	1.621	1.391	1.536	1.548	1.523	0.576	1.507
	(1.56)	(4.00)	(4.42)	(3.89)	(4.61)	(4.58)	(4.48)	(1.60)	(4.86)
Experience ≥ 3	0.905	1.176	1.687	1.631	1.336	1.565	1.608	0.849	1.627
	(1.57)	(2.99)	(3.82)	(3.58)	(3.08)	(3.95)	(3.89)	(1.80)	(4.32)
Mgr. × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.26

Panel C: Effect on the Next Industry Shock

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience	0.664	0.682	0.999	0.754	0.894	1.074	0.960	0.532	0.964
	(2.27)	(3.13)	(4.42)	(3.54)	(4.18)	(4.75)	(4.53)	(2.11)	(4.77)
IS × Exp.	0.944	3.103	3.627	3.226	2.735	2.829	3.290	0.285	3.307
	(1.21)	(3.68)	(3.80)	(3.11)	(2.78)	(3.32)	(3.72)	(0.55)	(4.20)
Mgr. × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.17	0.19	0.26

Table 2.5: Industry Exposure and Learning Intensity

The table reports results when managers' learning intensity from industry shocks can vary with industry exposure. Column (1) reports the estimates of a regression of Fama–French–Carhart (FFC) alphas on fund manager experience and Past IS, the number of industry shocks that the fund manager has been exposed to throughout her career. Columns (2) to (4) present results for a modified experience measure: $E'_{mqi} = \sum_{\tau<q} w_{i,\tau-1} \times IS_{i\tau}$. Specification (4) sorts ISPs with positive modified experience measure into terciles based on industry weight, conditional on the number of industry shocks experienced. All specifications include manager × date fixed effects as well as an indicator function equal to one if there is a shock in the industry of the ISP in the current quarter (coefficient not shown). t-statistics, reported in parentheses, are based on standard errors that allow for clustering around industry × date.

		Modified E		
	(1)	(2)	(3)	(4)
Experience	1.317	1.124	0.878	
	(5.80)	(4.28)	(3.44)	
Past IS	0.039		0.091	
	(0.56)		(1.13)	
Experience: Low				0.207
				(0.83)
Experience: Medium				0.812
				(3.26)
Experience: High				1.792
				(5.75)
Mgr. × Date FE	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	441,282
R^2	0.16	0.16	0.16	0.16

Table 2.6: Difference-In-Differences Approach

The table reports differences in Fama–French–Carhart alphas between experienced and inexperienced ISPs in event time around an experience shock. ISPs with an experience-shock event are all ISPs that go through an experience shock in quarter q ($t = 0$ in event time) and do not have any industry shock in the preceding and subsequent 4 quarters. The control group consists of all other ISPs managed by the same manager in the same event quarter q with complete data in the event window $t \in [-4, +4]$ (i) if they have the same industry tenure and (ii) if they do not go through an industry shock in the event window. All specifications include manager $\times$ quarter $\times$ event-quarter fixed effects and t-statistics, reported in parentheses, are based on standard errors that allow for clustering around industry $\times$ date.

Panel A: Pre-Industry Shock

	Event time relative to industry shock quarter			
	-4	-3	-2	-1
Experience	0.489	-0.265	-0.225	-1.440
	(0.41)	(-0.30)	(-0.21)	(-1.08)
N	6,052	6,052	6,052	6,052
R^2	0.31	0.30	0.31	0.27

Panel B: Post-Industry Shock

	Event time relative to industry shock quarter			
	+1	+2	+3	+4
Experience	2.421	1.028	0.959	4.264
	(1.94)	(1.28)	(0.77)	(2.64)
N	6,052	6,052	6,052	6,052
R^2	0.29	0.31	0.29	0.29

Panel C: Post-Pre Industry Shock

	Avg. Post-Pre
Experience	2.528
	(2.78)
N	48,416
R^2	0.29

Table 2.7: Returns on Buys and Sells

The table reports next-quarter returns of stocks purchased and sold by experienced and inexperienced managers. Each stock is classified at each fund-report date as either a net buy, or a net sell. A stock is a net buy (sell) if the fund manager increases (decreases) the weight of the stock in the overall portfolio net of price appreciation. Quarterly raw stock returns, Fama–French–Carhart (FFC) alphas, and DGTW-adjusted stock returns are regressed on a dummy equal to one if the stock is a net buy in a given fund, an $E > 0$ dummy, an interaction term, as well as different sets of fixed effects. The unit of observation is the fund-stock-date level and t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the stock level.

	Next-Quarter Outperformance of Stocks Bought vs. Stocks Sold				
	(1)	(2)	(3)	(4)	(5)
Raw returns					
$E = 0$	2.521	3.034	2.785	3.095	4.620
$E > 0$	2.899	3.347	3.169	3.424	5.057
Difference	0.378	0.313	0.384	0.329	0.437
t-statistic	(4.77)	(4.65)	(6.06)	(4.88)	(4.92)
FFC-Alphas					
$E = 0$	2.080	2.268	2.113	2.300	3.575
$E > 0$	2.479	2.685	2.517	2.657	3.949
Difference	0.399	0.417	0.404	0.357	0.374
t-statistic	(6.25)	(6.48)	(6.49)	(5.64)	(4.77)
DGTW-Adjusted Return					
$E = 0$	2.499	2.812	2.566	2.858	4.119
$E > 0$	2.844	3.154	2.968	3.211	4.632
Difference	0.345	0.342	0.402	0.353	0.513
t-statistic	(4.82)	(4.75)	(5.86)	(4.88)	(5.34)
Manager × Date FE	No	Yes	Yes	Yes	Yes
Industry × Date FE	No	No	Yes	No	No
Manager × Industry FE	No	No	No	Yes	No
Manager × Firm FE	No	No	No	No	Yes

Table 2.8: Experienced Managers' Trades Around Earnings Announcements

The table reports next-quarter earnings announcement returns of stocks bought and sold by experienced and inexperienced managers. Each stock is classified at each fund-report date as either a net buy, or a net sell. A stock is a net buy (sell) if the fund manager increases (decreases) the weight of the stock in the overall portfolio net of price appreciation. Cumulative abnormal returns over a 3-day window $(-1, +1)$ around the earnings announcement are regressed on a dummy equal to one if the stock is a net buy in a given fund, an $E > 0$ dummy, an interaction term, firm controls, and different sets of fixed effects. Firm controls include book-to-market, size (natural logarithm of market capitalization, in millions of dollars), stock turnover, percentage of institutional ownership (IO), reporting lag, and analyst coverage (natural logarithm of 1 plus the number of analysts covering the firm). The unit of observation is the fund-stock-date level and t-statistics, reported in parentheses, are based on standard errors allow for clustering at the stock level.

	(1)	(2)	(3)	(4)	(5)
Net Buy	0.281	0.313	0.296	0.321	0.441
	(13.86)	(14.31)	(14.43)	(14.81)	(15.47)
Experience	-0.031	-0.016	-0.017	-0.049	-0.108
	(-0.77)	(-0.32)	(-0.63)	(-0.60)	(-0.98)
Experience × Net Buy	0.120	0.130	0.143	0.135	0.237
	(3.34)	(3.57)	(3.98)	(3.67)	(4.95)
B/M	0.021	0.029	0.038	0.029	0.226
	(0.84)	(1.13)	(1.47)	(1.15)	(3.27)
Size	-0.047	0.008	0.007	0.007	-0.720
	(-1.60)	(0.27)	(0.24)	(0.24)	(-6.62)
Turnover	-0.027	-0.027	-0.027	-0.028	-0.020
	(-3.06)	(-3.16)	(-3.08)	(-3.26)	(-1.18)
IO	1.125	1.043	1.000	0.971	-1.180
	(5.66)	(5.27)	(5.09)	(4.96)	(-2.17)
Reporting lag	-0.019	-0.019	-0.018	-0.019	-0.008
	(-4.52)	(-4.63)	(-4.64)	(-4.79)	(-1.08)
$\log(1 + Analysts)$	0.037	0.048	0.038	0.037	0.092
	(0.64)	(0.84)	(0.67)	(0.65)	(0.96)
Manager × Date FE	No	Yes	Yes	Yes	Yes
Industry × Date FE	No	No	Yes	No	No
Manager × Industry FE	No	No	No	Yes	No
Manager × Firm FE	No	No	No	No	Yes
N	1,511,560	1,511,560	1,511,560	1,511,560	1,511,560
R^2	0.01	0.03	0.05	0.05	0.37

Table 2.9: Experience and Industry-Specific Skill

The table reports various checks for the potential effects of industry–specific baseline skill. In all specifications, the dependent variable is the Fama–French–Carhart alpha. In column (1), we include the average ICI component over the prior four quarters as an additional control variable. The ICI component is defined as the squared deviation of the industry share of a given ISP from the average industry share across all ISPs in a given quarter and industry. In column (2), the average industry share over the prior four quarters is included as a control variable. In column (3), we include the pre–experience alpha, defined as the average alpha obtained by a given manager in a given industry, as long as the value of the manager's experience in the industry equals 0. In column (4), the sample is restricted to all manager–industry combinations that at some point have a value of E greater than 0. All specifications include manager $\times$ date fixed effects as well as an indicator function equal to one if there is a shock in the industry of the ISP in the current quarter (coefficient not shown). t-statistics, reported in parentheses, are based on standard errors that allow for clustering around industry $\times$ date.

	(1)	(2)	(3)	(4)
Experience	1.378	1.237	0.875	1.201
	(5.66)	(5.23)	(3.54)	(3.73)
Average ICI component	0.013			
	(2.21)			
Average industry share		1.580		
		(2.11)		
Pre-experience alpha			0.762	
			(53.19)	
Manager $\times$ Date FE	Yes	Yes	Yes	Yes
N	262,839	262,839	436,169	134,233
R^2	0.17	0.17	0.33	0.36

Table 2.10: Experience and Omitted Industry-Level Variables

The table reports estimates of our baseline model, using the Fama–French–Carhart alpha as the dependent variable, while controlling for additional industry-level variables. In column (1) we control for the current industry return. Column (2) adds 8 lags of industry returns. Columns (3) and (4) include industry return volatility, as well as 8 lags of industry volatility, respectively, as control variables. Industry volatility is defined as the standard deviation of the daily industry returns net of the market return in a given quarter. Column (5) simultaneously includes all previsouly used controls. Column (6) includes industry × date fixed effects. All regressions include manager × date fixed effects as well as an indicator function equal to one if there is a shock in the industry of the ISP in the current quarter (coefficient not shown). t-statistics, reported in parentheses, are based on standard errors that allow for clustering around industry × date.

	(1)	(2)	(3)	(4)	(5)	(6)
Experience	1.233	1.091	1.240	1.219	1.093	0.220
	(4.77)	(4.64)	(5.34)	(5.41)	(5.00)	(2.96)
Industry Return	0.245	0.258			0.267	
	(5.46)	(5.80)			(6.28)	
Industry Volatility			1.368	1.238	1.504	
			(1.72)	(0.67)	(0.82)	
8 Lags of Industry Return	No	Yes	No	No	Yes	No
8 Lags of Industry Volatility	No	No	No	Yes	Yes	No
Manager × Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Date FE	No	No	No	No	No	Yes
N	441,282	205,960	441,282	205,960	205,960	441,282
R^2	0.18	0.20	0.16	0.17	0.20	0.34

Table 2.11: Learning from Industry Booms and Other Periods

This table reports results when learning can come from other periods. It shows coefficient estimates when the Fama–French–Carhart alpha is regressed on E_1, E_n, IS_1, IS_n and manager × date fixed effects. Every line represents results from one single regression. E_1 and IS_1 are the experience and industry shock dummies used in the previous tables. E_n and IS_n are the experience and industry shock variables when an industry shock is not based on the lowest industry return in a quarter (rank = 1), but on rank = n, where n = 12 denotes the highest industry return in the quarter (booms). The experience measures E_n are constructed otherwise as dummies based on equation (2.4). t-statistics, reported in parentheses, are based on standard errors that allow for clustering around industry × date.

Rank n	E_n	t-stat	E_1	t-stat	IS_n	t-stat	IS_1	t-stat
1 (Low)			1.376	(5.75)			-3.335	(-4.46)
2	0.172	(0.77)	1.351	(5.38)	-2.573	(-4.55)	-3.570	(-4.79)
3	0.367	(1.76)	1.306	(5.66)	-2.958	(-7.25)	-3.598	(-4.83)
4	0.029	(0.15)	1.330	(5.44)	-1.515	(-3.59)	-3.472	(-4.64)
5	-0.063	(-0.33)	1.372	(5.56)	-1.149	(-2.70)	-3.438	(-4.59)
6	0.268	(1.45)	1.287	(5.10)	-0.651	(-1.38)	-3.384	(-4.53)
7	-0.352	(-1.70)	1.448	(5.80)	0.244	(0.59)	-3.336	(-4.47)
8	-0.390	(-2.35)	1.467	(5.92)	0.071	(0.16)	-3.344	(-4.47)
9	-0.174	(-1.04)	1.442	(5.92)	0.986	(2.14)	-3.255	(-4.35)
10	-0.315	(-1.97)	1.449	(5.95)	0.951	(1.76)	-3.256	(-4.34)
11	0.350	(1.61)	1.202	(5.09)	2.859	(6.66)	-3.102	(-4.15)
12 (High)	0.239	(0.99)	1.113	(4.79)	3.096	(6.05)	-3.062	(-4.12)

Table 2.12: Experience from the Time-Series of Industry Returns

The table presents results when experience comes from the time-series of industry returns. Experience E^{TS} is calculated as in equation (2.4), but now based on IS^{TS} which is an industry shock measure based on the time-series of industry returns. IS^{TS} is a dummy equal to one if the industry return in the quarter is among the lowest four quarterly returns over the last 40 quarters. Panel A shows averages of E^{TS} and performance variables aver the whole sample and split by experience. Panel B presents regression results from Table 2.4 using the time-series based experience measure. t-statistics, reported in parentheses, are based on standard errors that allow for clustering around industry × date.

Panel A: Summary Statistics by Experience

Variable	All	$E^{TS} = 0$	$E^{TS} > 0$	t-stat
Experience^{TS}	0.44	0.00	2.01	54.68
Performance Measures				
Raw ISP Return	3.19	3.24	2.98	-0.53
Market-Adjusted ISP Return	0.72	0.69	0.84	0.55
FF Alpha	0.43	0.34	0.74	2.26
FFC Alpha	0.41	0.27	0.91	3.54
CPZ4 Alpha	0.74	0.63	1.11	2.77
CPZ7 Alpha	0.76	0.66	1.11	2.62
LND Alpha	0.43	0.30	0.90	3.50
CCP Alpha	0.37	0.24	0.84	3.59
DGTW-Adjusted Return	0.39	0.38	0.42	0.22
N	441,282	344,630	96,652	

Panel B: Regression-Based Results

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience^{TS}	0.165	0.604	0.856	0.635	0.544	0.790	0.818	0.179	0.688
	(0.64)	(2.80)	(3.73)	(2.84)	(2.58)	(3.67)	(3.89)	(0.87)	(3.68)
Mgr. × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.15	0.16	0.16	0.19	0.26

Table 2.13: Experience at the Fund Level: EDX and Performance

The table presents fund level results using the experience index EDX. For each quarter and fund, EDX is the weighted average of the individual ISP experience measures. In each quarter, funds are sorted into portfolios based on the value of EDX, low (below 20^{th} percentile), medium (between the 20^{th} and 80^{th} percentiles), and high (above the 80^{th} percentile). We compute the return of the respective portfolio as the TNA-weighted monthly return before and after expenses reported in CRSP. The abnormal return is the intercept from regressing the fund returns on the four FFC factors. Panel B repeats the analysis using tenure instead of EDX as a sorting variable. Below each coefficient estimate, the t-statistic is reported in parentheses. The table reports t-statistics in parentheses and the average number of individual funds in each portfolio.

Panel A: Sort on Experience

	Abnormal return (% per month)		Factor loadings before expenses				
	Before expenses	After expenses	Market	Value	Size	Mom.	Avg. N
Low EDX	-0.031	-0.116	0.947	-0.090	0.148	0.097	271
	(-0.42)	(-1.58)	(42.51)	(-2.42)	(4.44)	(5.32)	
Mid EDX	0.026	-0.049	0.958	0.018	0.012	0.009	534
	(0.52)	(-0.97)	(54.11)	(1.16)	(0.86)	(0.85)	
High EDX	0.113	0.031	0.956	-0.132	0.090	0.015	210
	(2.24)	(0.62)	(55.98)	(-7.66)	(4.79)	(1.18)	
High – Low	0.144	0.147	0.010	-0.042	-0.057	-0.082	
	(1.62)	(0.10)	(0.34)	(1.02)	(-1.49)	(-3.72)	

Panel B: Sort on Tenure

	Abnormal return (% per month)		Factor loadings before expenses				
	Before expenses	After expenses	Market	Value	Size	Mom.	Avg. N
Low EDX	0.048	-0.039	0.948	-0.105	0.092	0.013	196
	(0.69)	(-0.57)	(36.54)	(-3.67)	(2.90)	(0.91)	
Mid EDX	0.061	-0.019	0.952	-0.064	0.061	0.021	600
	(0.99)	(-0.31)	(39.21)	(-3.30)	(3.82)	(1.94)	
High EDX	0.076	0.000	0.930	0.003	0.011	0.024	219
	(1.52)	(0.00)	(45.69)	(0.18)	(0.93)	(2.13)	
High – Low	0.027	0.040	-0.019	0.108	-0.081	0.011	
	(0.32)	(0.64)	(0.57)	(3.28)	(2.38)	(0.63)	

Figures

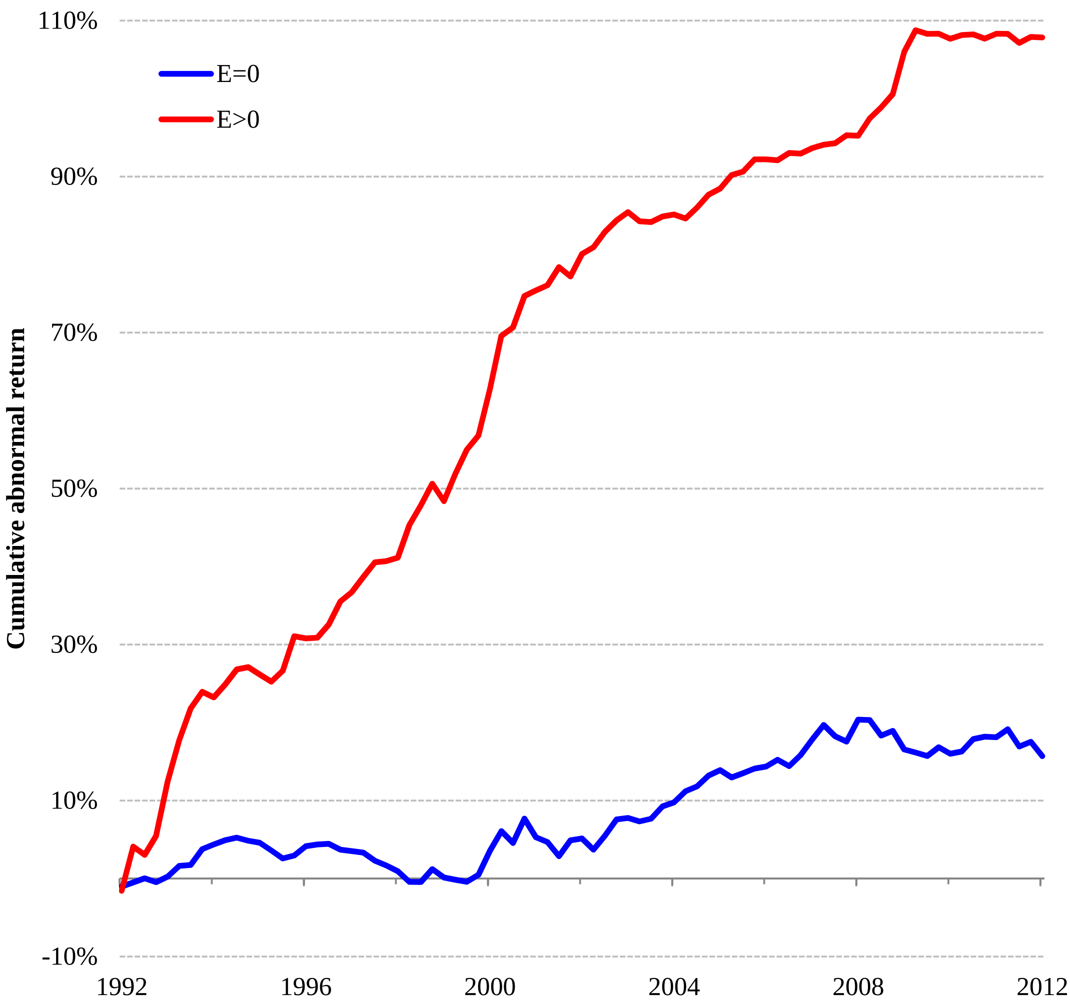


Figure 2.1: Investing with experienced and inexperienced managers. The graph shows the cumulative abnormal return of a (hypothetical) equal-weighted, quarterly rebalanced, portfolio of ISPs by managers that have experienced an industry shock in the past ($E > 0$) as well as a portfolio of the remaining, inexperienced, ISPs. Returns are risk-adjusted using the Fama–French–Carhart model.

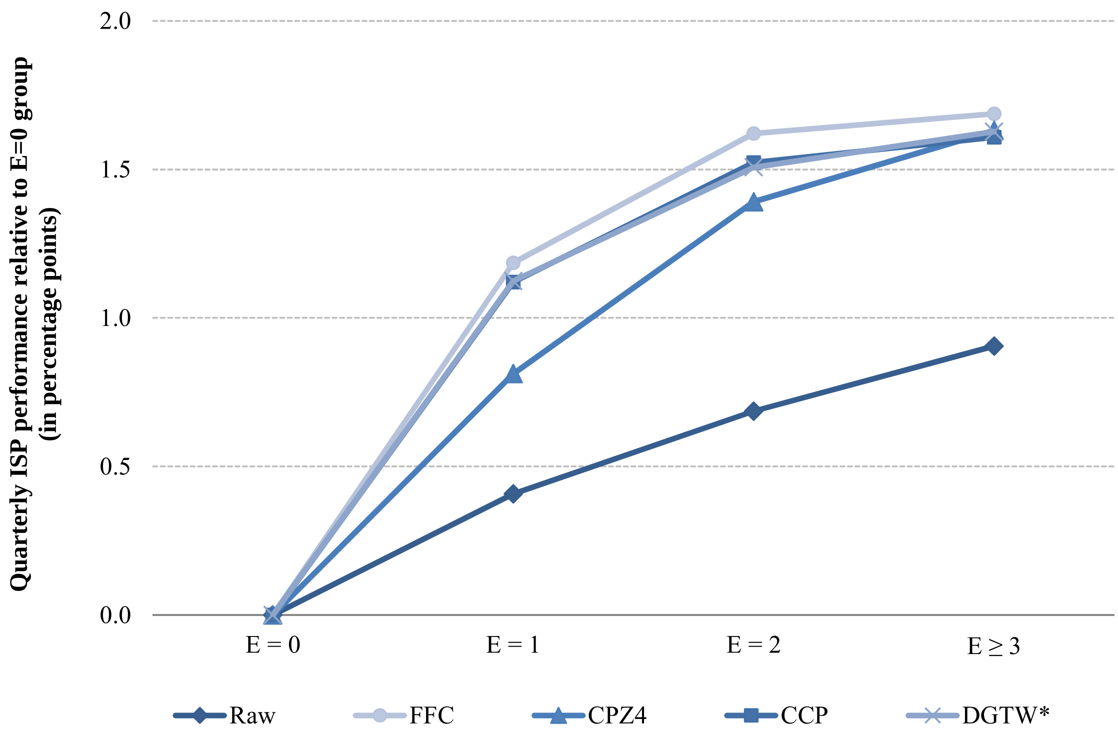


Figure 2.2: The Experience-Performance Relationship. The graph plots the effect of one incremental unit of experience as estimated in Panel B of Table 2.4.

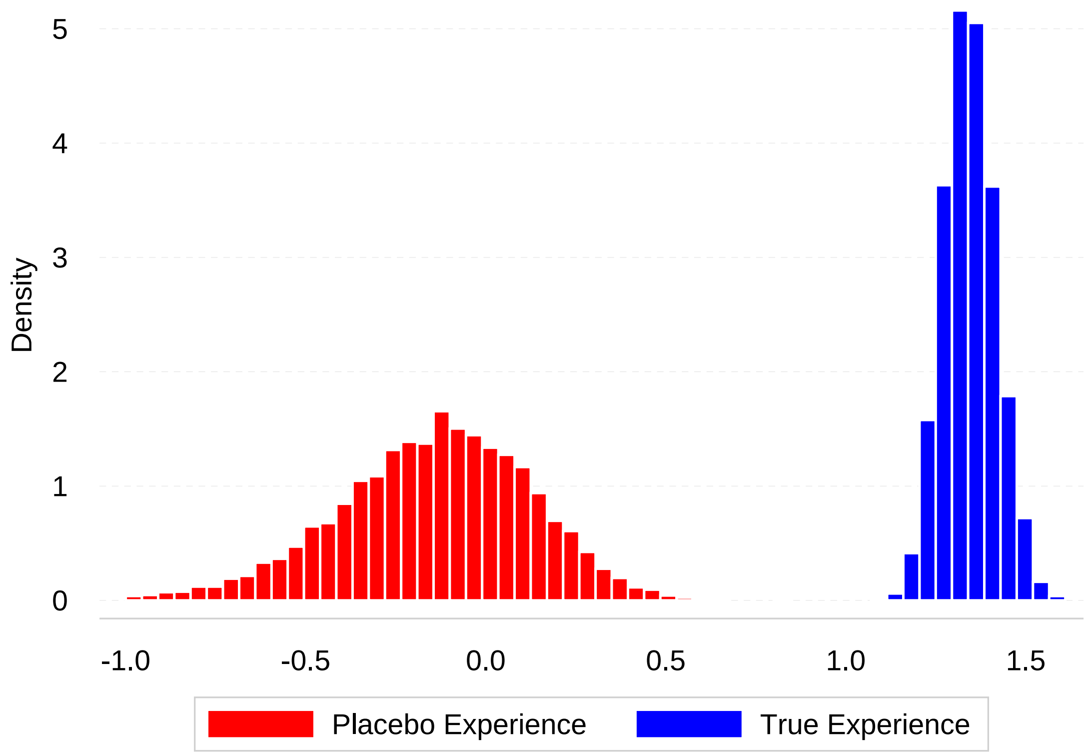


Figure 2.3: Placebo Test. The figure presents results for the placebo test described in section 2.4.3. A sequence of placebo industry shocks is generated by randomly selecting one Fama-French 12 industry every quarter as an industry shock quarter. Next, a pseudo experience measure is computed as in equation (4) based on this placebo industry shock series. We then rerun our baseline regression from Table 2.4, specification (3) with the placebo experience measure as an additional regressor. This procedure is repeated 10,000 times. The "True Experience" distribution is the histogram of the 10,000 coefficients on our baseline experience measure from the regression. The "Placebo Experience" distribution is the histogram of the 10,000 coefficients on the pseudo experience measure from the regression.

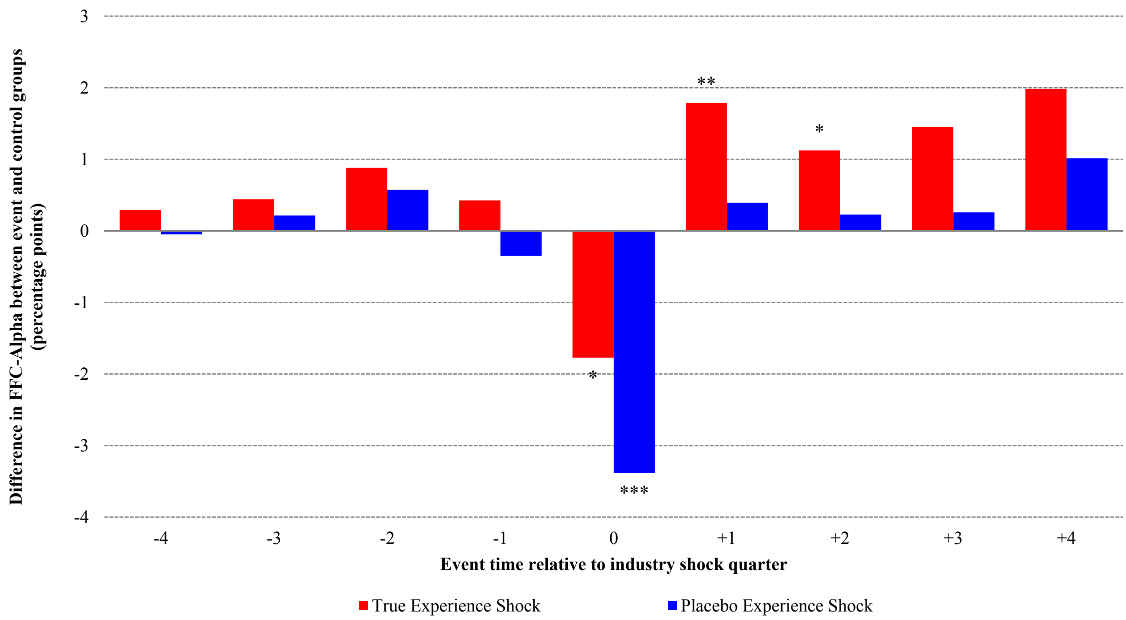


Figure 2.4: The Impact of Experience on Performance in Event Time. The graph plots the difference in Fama–French–Carhart alphas between experienced and inexperienced ISPs around an experience shock as reported in Table 2.6. ISPs with an experience-shock event are all ISPs that go through an experience shock in quarter q ($t = 0$ in event time) and do not have any industry shock in the preceding and subsequent 4 quarters. The control group consists of all other ISPs managed by the same manager in the same event quarter q with complete data in the event window $t \in [-4, +4]$ (i) if they have the same industry tenure and (ii) if they do not go through an industry shock in the event window. Asterisks ***, **, * indicate statistical significance of the difference between the high and low groups on the 1%, 5%, and 10% level, and are based on standard errors that allow for clustering around industry $\times$ date.

Appendix

Table A2.1: Alternative Clustering of the Standard Errors

The table repeats Panel A of Table 2.4 while clustering standard errors at the fund level (Panel A) and double-clustering standard errors around industry × date and fund (Panel B).

Panel A: Clustering at the fund level

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience	0.566	1.005	1.376	1.091	1.180	1.369	1.303	0.561	1.304
	(12.99)	(20.11)	(28.25)	(25.13)	(25.54)	(26.86)	(36.52)	(12.92)	(27.10)
Mgr. × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.26

Panel B: Double–clustering around industry × date and fund

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience	0.566	1.005	1.376	1.091	1.180	1.369	1.303	0.561	1.304
	(1.98)	(4.25)	(5.51)	(4.60)	(5.03)	(5.67)	(5.53)	(2.36)	(5.96)
Mgr. × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.26

Table A2.2: Alternative Experience Measures

The table repeats Panel A of Table 2.4 using alternative measures of experience. Panel A shows results for a modified cross-sectional industry shock measure that restricts industry shock quarters to quarters with negative industry returns. Panel B replaces the restriction $\mathrm{I}[w_{m,\tau-1,i} > 0.1]$ in equation (4) in the main paper by a dummy variable equal to one if industry i is among the top 3 industries held by manager m. Panel C replaces the same restriction by $\mathrm{I}[w_{m,\tau-1,i} > w_{all,\tau-1,i}]$, where $w_{all,\tau-1,i}$ refers to the median weight of industry i in the portfolio of all fund managers invested in that industry.

Panel A: Only Negative Industry Returns

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience	0.662	1.096	1.455	1.160	1.278	1.428	1.381	0.631	1.394
	(2.12)	(4.36)	(5.42)	(4.46)	(5.04)	(5.55)	(5.50)	(2.43)	(5.92)
Mgr. × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.26

Panel B: Top 3 Holdings

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience	0.624	1.082	1.439	1.137	1.223	1.415	1.382	0.582	1.337
	(2.05)	(4.56)	(5.67)	(4.63)	(5.09)	(5.82)	(5.82)	(2.34)	(6.17)
Mgr. × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.26

Panel C: Weight above Industry Median

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience	0.441	0.544	0.839	0.694	0.790	0.875	0.756	0.437	0.804
	(2.06)	(2.95)	(4.53)	(3.68)	(4.33)	(4.68)	(4.41)	(2.47)	(5.06)
Mgr. × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.26

Table A2.3: Industry Tenure

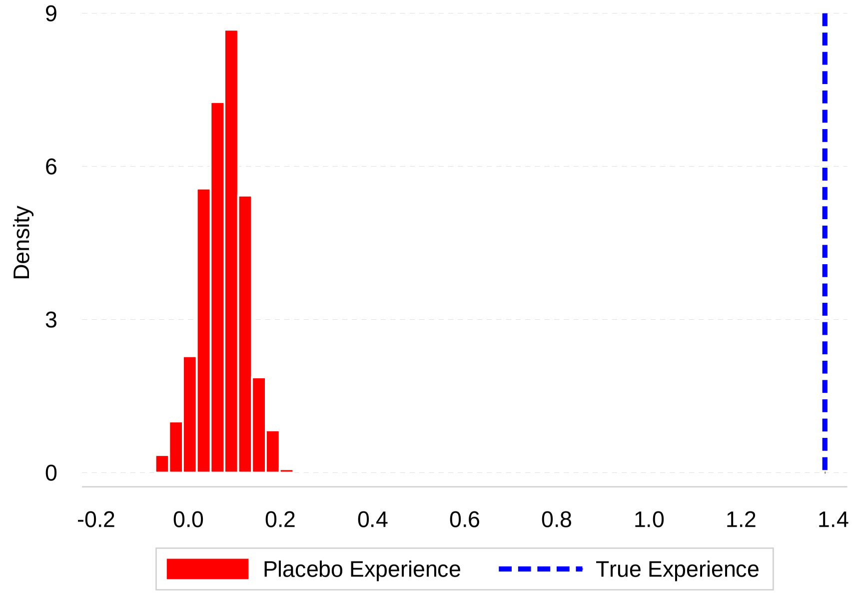
The table repeats Panel A of Table 2.4 while controlling for the manager's industry tenure.

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Exp.	0.595	1.007	1.374	1.112	1.202	1.367	1.289	0.593	1.314
	(2.11)	(4.50)	(5.73)	(4.76)	(5.28)	(5.93)	(5.73)	(2.52)	(6.21)
Industry	-0.018	-0.001	0.001	-0.013	-0.014	0.001	0.008	-0.022	-0.008
Tenure	(-1.40)	(-0.10)	(0.10)	(-0.99)	(-1.08)	(0.09)	(0.79)	(-1.73)	(-0.82)
Mgr.× Date	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.26

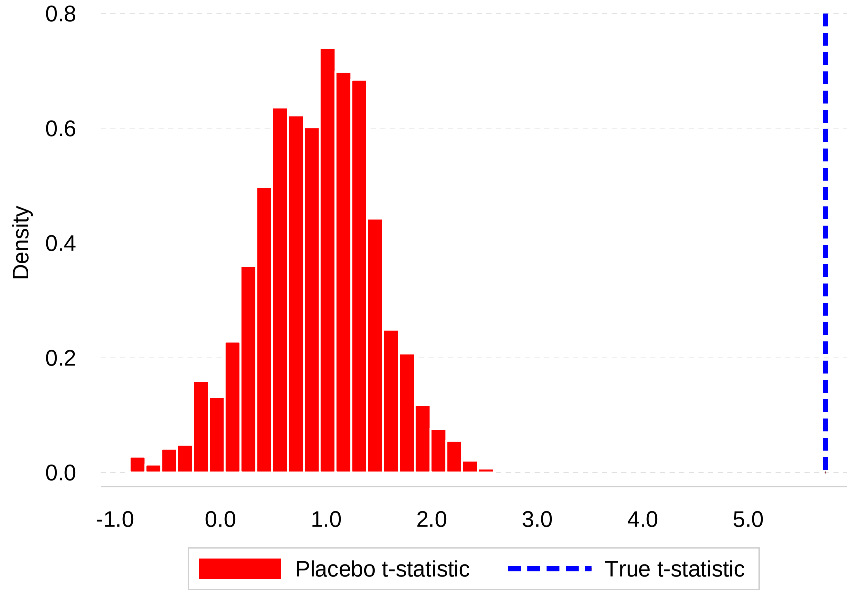
Table A2.4: Bootstrap Procedure

The table explains the steps for the bootstrap procedure described in section 2.4.3, which follows Kosowski, Timmermann, Wermers, and White (2006), to generate 1,000 bootstrapped alphas, under the null that all ISPs have zero alpha. The bootstrapped alphas are then regressed on fund manager experience, an indicator function equal to one if there is a shock in the industry of the ISP in the current quarter, and manager × date fixed effects.

Step	**Description**
1	We start by estimating equation (6) in the text for each ISP-quarter, saving the resulting FFC alphas, factor loadings, and residuals.
2	For each ISP-quarter, we draw a random sample (indexed by b) with replacement from the realized ISP residuals that were saved in step 1.
3	We compute hypothetical excess ISP returns as: $R^{(b)}_{mqi,t} = \hat{b}_{mqi}RMRF_t + \hat{s}_{mqi}SMB_t + \hat{h}_{mqi}HML_t + \hat{m}_{mqi}UMD_t + \hat{\varepsilon}^{(b)}_{mqi,t}$
4	We regress $R^{(b)}_{mqi}$ on the FFC factors, and estimate a bootstrapped $\hat{\alpha}^{(b)}_{mqi}$. In a given bootstrap replication b, $\hat{\alpha}^{(b)}_{mqi}$ need not equal 0, because the residuals $\hat{\varepsilon}^{(b)}_{mqi,t}$ from step 1 are sampled with replacement.
5	Using the panel of bootstrapped ISP alphas $\hat{\alpha}^{(b)}_{mqi}$, we estimate our baseline regression by regressing the bootstrapped alphas on the fund manager experience indicator, an indicator function equal to one if there is a shock in the industry of the ISP in the current quarter, and manager × industry fixed effects.
6	We repeat steps 1 to 5 for 1,000 bootstrap replications, storing the resulting estimates of the experience coefficient as well as the corresponding t-statistic.



(a) Distribution of Placebo Coefficients



(b) Distribution of Placebo t–statistics

Figure A2.1: Results from bootstrap procedure. The figure illustrates the output of the bootstrap based on the procedure by Kosowski, Timmermann, Wermers, and White (2006), as described in Table A2.4. Panel A2.1a displays the histogram of the estimated experience coefficients after 1,000 bootstrap replications, and Panel A2.1b shows the histogram of the corresponding t–statistics.

Table A2.5: Simulation of Non-Random Manager Attrition

The table presents the assumptions and results of the simulation of non-random manager attrition. An artificial dataset is simulated that mirrors our real dataset as closely as possible using the assumptions summarized in Panels A and B. The attrition rate varies between 0 and 30 percent and the simulation is repeated 1,000 times for each attrition rate. Simulated ISP-alphas are regressed on fund manager experience, manager × date fixed effects, as well as an indicator function equal to one if there is a shock in the industry of the ISP in the current quarter. Panel C reports the average magnitude and t-statistic of the experience coefficient for different attrition rates. t-statistics are based on standard errors allow for clustering around industry × date.

Panel A: Simulation Steps

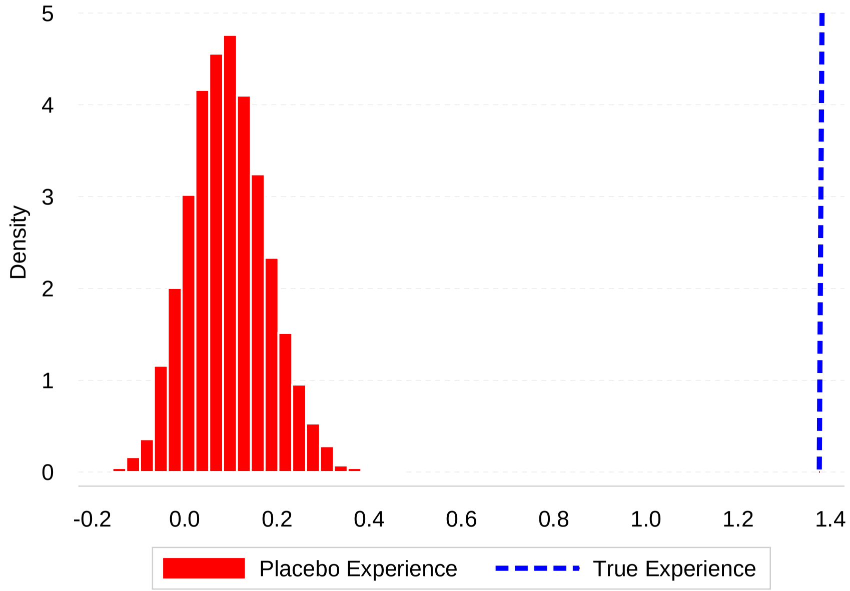
Step	**Description**
1	We start with 500 managers each managing 12 ISPs (6000 observations in total).
2	Each manager has a constant baseline skill α_{mi} in a given industry. α_{mi} is distributed $N(0.41, 10.89)$ (consistent with our data).
3	The quarterly ISP alpha is equal to the manager's baseline skill in the industry and an error term.
4	Every quarter one industry randomly experiences an industry shock, which lowers the alphas of all ISPs in that industry by 3 percentage points (consistent with our data).
5	The worst x% of all ISPs are removed at the end of every quarter, where x is varied between 0 and 30 percent.
6	Managers have a limited overall tenure T_m that follows a Poisson distribution with $\lambda = 20$ (consistent with our data). They stop managing all ISPs at the end of their tenure.
7	At the beginning of each quarter, all the ISPs that have been removed in steps (5) and (6) are replaced by new ISPs with average characteristics.
8	Experience E_{miq} sums up the number of industry shocks manager m has been exposed to in industry i up to time q. If an industry shock occurs, the manager learns from it with a probability of 40%.
9	After 80 quarters the simulation ends.

Panel B: Key Variable Assumptions

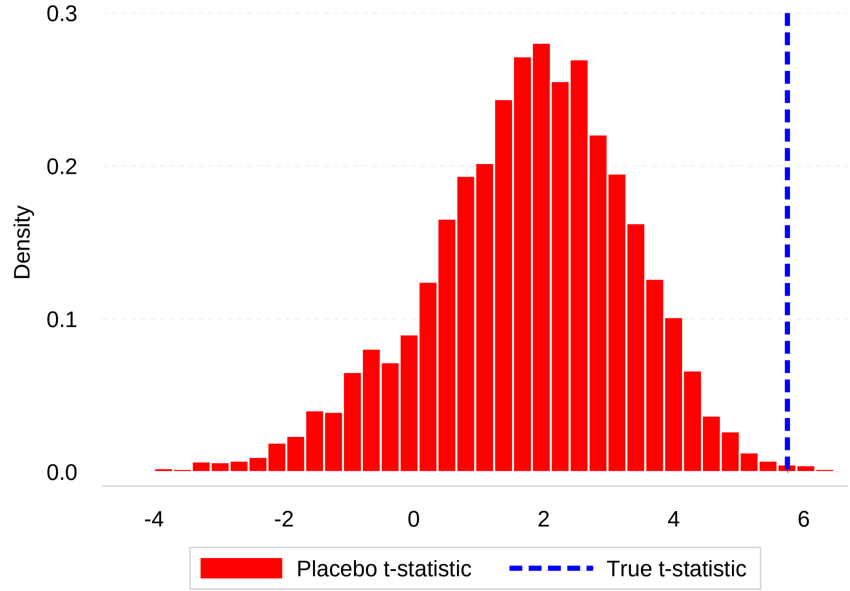
Variable	Definition
Quarterly ISP-alpha	$\alpha_{miq} = \alpha_{mi} + IS_{iq} \times (-3) + \varepsilon_{miq}$
Manager-industry skill	$\alpha_{mi} \sim N(0.41, 10.89)$
Error term	$\varepsilon_{miq} \sim N(0, 9)$
Manager tenure	$T_m \sim Pois(20)$
Experience	$\mathrm{E}_{mqi} = \sum_{\tau<q} \mathrm{IS}_{i\tau} \times \mathrm{I}[u_{m,\tau,i} \leq 0.4]$, where $u_{m,\tau,i} \sim U(0,1)$

Panel C: Regression Results

	Excluding fixed effects		Including fixed effects	
	Average Coefficient	Average t-statistic	Average Coefficient	Average t-statistic
Attrition (in %)	(1)	(2)	(3)	(4)
0	0.000	0.01	0.001	0.03
1	0.177	5.55	0.036	1.01
5	0.599	16.81	0.077	2.00
10	0.984	24.01	0.106	2.37
15	1.284	27.40	0.126	2.42
20	1.517	28.49	0.141	2.32
30	1.858	27.64	0.172	2.19



(a) Distribution of Simulated Experience Coefficients



(b) Distribution of Simulated Experience t–statistics

Figure A2.2: Results from the simulation of attrition. The figure illustrates the output of the simulation described in A2.5, assuming an attrition rate of 1%. Panel A plots the histogram of the estimated experience coefficients after 1,000 simulation runs, and Panel B shows the histogram of the corresponding t-statistics.

Chapter 3

Distracted Shareholders and Corporate Actions

Co-authors: Alberto Manconi and Oliver Spalt

3.1. Introduction

Attention is a resource in limited supply. Consumers usually do not compare *all* potential products when making choices; professors do not pay equal attention to *all* new academic papers in their research area; and mutual fund managers cannot focus equally on *all* stocks they hold or the thousands of stocks they could hold. Instead, salience matters and we often focus attention on products that are advertised prominently, papers written by high-profile authors, and stocks in industries considered either to be "hot", or in crisis. While a growing literature in economics and finance studies limited attention, the impact of limited attention on corporate actions is largely unexplored (e.g., Baker and Wurgler (2012)). Our paper aims to fill this gap by focusing on the link between managerial actions and *exogenous* variation in monitoring intensity, brought about by time-variation in how investors allocate attention across the stocks they hold in their portfolio. We exploit unique features of U.S. institutional holdings data to show that managers respond to temporarily looser monitoring, induced by investors with limited attention focusing their attention elsewhere, by engaging in investments that maximize private benefits at the expense of shareholders.

The key challenge is that distraction cannot be directly observed. Our identification strategy is designed to circumvent this difficulty. It has two main building blocks. First, we exploit data on a specific, but economically most important, set of shareholders: institutional investors that file form 13f with the SEC. As of 2012, they hold more than 70% of the aggregate market value of all NYSE/AMEX/NASDAQ stocks. In contrast to retail investors, large institutional investors are required to periodically report their portfolio holdings. We therefore observe the pool of institutional shareholders for each firm, and we observe for each institutional investor which other stocks they concurrently hold. This feature of the data enables us to capture shifts in investor attention by looking *"inside"* shareholders' portfolios. Specifically, we use exogenous shocks to unrelated industries held by a given firm's institutional shareholders to mark periods where shareholders are likely to shift attention away from the firm and towards the part of their portfolio subject to the shock. We then construct firm-level distraction measures by aggregating information about institutional investors for each firm, and we relate those measures to corporate actions. As a second building block, we conjecture that attention is not unbounded for institutional investors. This is consistent with recent findings in the literature we cite below. It is also supported by large-scale survey evidence from the Investor Responsibility Research Center Institute (IRRC (2011)) who document a direct link between institutional investor attention constraints and monitoring activity. They write: *"three-fourths of institutions report that time is the most common impediment to engagement [with corporations], while staffing considerations rank second."*

The following thought experiment illustrates our approach. Consider two otherwise identical firms 1 and 2 in a given industry and year. Firm 1's representative shareholder holds two stocks. The first is firm 1 itself, and the second is another firm belonging to a different industry, which for the sake of this example we call "banks". The representative shareholder of firm 2 does not hold any bank stocks.

Suppose now that there is an attention-grabbing event in the banking industry; for example, a banking crisis that sends prices of bank stocks falling. Assuming limited attention, the representative shareholder in firm 1 may, potentially rationally, shift attention towards banks and away from firm 1. As a result, monitoring intensity at firm 1 decreases, and the management of firm 1 has more room to pursue private benefits. In contrast, and by construction, firm 2 is not affected. We can therefore identify the impact of variation in investor attention on corporate policies by analyzing changes in policies of firm 1 relative to firm 2 around the time of the exogenous shock. Motivated by Barber and Odean (2008), we use “extreme” industry returns (both positive and negative) as our main empirical proxy for attention-grabbing events.

What happens when shareholders experience shocks to unrelated parts of their portfolio – we will call such investors “distracted” in the following – is an empirical question. One possibility, the least interesting, is that policies at firm 1 do not change. This could, for example, be due to the fact that board monitoring is all that matters; or it could indicate that lack of attention by distracted shareholders can be easily substituted by additional attention of those shareholders who are not distracted. A second possibility could be that managerial monitoring constraints are indeed relaxed and that managers react by becoming passive and “enjoying the quiet life” (Bertrand and Mullainathan (2003)). Finally, managers might actively seek to maximize private benefits. Our results provide strong evidence for the latter scenario.

We find that, when shareholders are distracted, managers make more value-destroying acquisitions. The M&A setting is close to ideal for our study, because we can match the time-variation in our firm-level distraction measures with time-variation in merger activity, and therefore minimize endogeneity concerns, as explained in detail below. Our baseline tests show that the probability of making an acquisition increases by about 30% for a one standard deviation increase in investor distraction. All our tests use industry × quarter fixed effects, so these findings cannot be explained by any variable that does not vary across firms within a given industry and quarter, such as investment opportunities, attractiveness of other target industries, the state of the business cycle etc. The results are also robust to including firm fixed effects, so any firm-level time-invariant unobservable factor that might influence the match between a firm and its shareholders cannot impact our findings. Additional tests using lags of the distraction measure make more general selection stories unlikely.

If managers make more acquisitions when shareholders are distracted, are those bad deals? Our tests indicate they are. First, we find that the distraction effect is concentrated in diversifying acquisitions, which are commonly thought to disproportionally benefit managers, for reasons of empire building or job security through more stable cash flows (e.g., Amihud and Lev (1981), Morck, Shleifer, and Vishny (1990)). Second, and consistent, bidder announcement returns are 33% lower, relative to the average, when shareholders are more distracted, and so are the combined bidder and target announcement returns ("synergies"). Third, over the three-year period following the deal, bidding firms have significantly lower stock returns if shareholders are distracted at announcement. All these results are consistent with the idea that managers take advantage of looser monitoring by tilting capital budgets towards diversifying, value-destroying acquisitions.

While our main tests are designed to address identification issues, Figure 3.1 shows that we can detect distraction effects even in the raw data. The figure plots quarterly takeover frequencies for 5-year subperiods of our 1980 to 2010 sample period when we sort shareholders for each firm into high and low distraction groups. Firms in the "high distraction" group are more likely to announce mergers in all six subperiods. The difference is economically sizeable and statistically significant in five of these periods. Hence, even in the raw data: if shareholders are distracted, firms are consistently more likely to announce takeovers.

Additional evidence suggests the distracted shareholder hypothesis is not specific to takeovers. Building on work by Bebchuk, Grinstein, and Peyer (2010), we show that CEOs are more likely to receive opportunistically-timed ("lucky") equity grants when shareholders are distracted. A one standard deviation increase in distraction increases the chance of a lucky grant by about 30%. As in the M&A setting, these findings are robust to industry-by-time as well as firm fixed effects, minimizing concerns about unobserved heterogeneity. This test is useful for at least three reasons. First, lucky grants are directly related to managerial wealth. Second, lucky grants are unlikely related to economic fundamentals at the granting firm. Third, lucky grants are in no obvious way related to our merger analysis, thus providing a useful alternative testing ground for the distraction measures used in the M&A analysis.

In a final set of tests, we find that CEOs are less likely fired after bad performance in years when shareholders are distracted, that firms are more likely to cut dividends when their shareholders are distracted, and that a portfolio strategy long in firms with non-distracted shareholders and short in firms with distracted shareholders earns significant abnormal returns.

In sum, our findings suggest that shareholder distraction has a measurable and economically important impact on a broad range of corporate actions. A unifying explanation for our results is that managers maximize their own private benefits at the expense of shareholders at times when institutional investors experience a shock to portions of their portfolio unrelated to the firm itself. This is consistent with temporal variation in monitoring intensity brought about by investors with limited attention shifting attention to other firms.

Our paper contributes to the behavioral corporate finance literature. We directly address an important open question highlighted in the survey of Baker and Wurgler (2012): *what is the impact of limited attention on corporate finance?* We also relate to a broader empirical literature in behavioral finance on distraction and limited attention (e.g., Barber and Odean (2008), DellaVigna and Pollet (2009), Hirshleifer, Lim, and Teoh (2009)) that is, in turn, linked to theoretical work on investor inattention in finance and economics (e.g., Hong and Stein (1999), Gabaix, Laibson, Moloche, and Weinberg (2006), Peng and Xiong (2006)). While most of this work has focused on retail investors and stock prices, little work exists to-date that to-date that analyzes attention effects for institutional investors. Related papers supporting the notion that limited attention frameworks can be useful for understanding important facts about mutual fund management include Fang, Peress, and Zheng (2011), who study the impact of media coverage on investment performance, and Kacperczyk, Nieuwerburgh, and Veldkamp (2013) who study optimal attention allocation over the business cycle. To the best of our knowledge, our paper is among the first to relate limited attention of institutional investors to corporate investment, executive pay, CEO turnover, and dividends.

Our paper also contributes to a growing literature seeking to identify exogenous changes in monitoring. Related papers include Bertrand and Mullainathan (2003) who exploit state adoptions of antitakeover laws, and Falato, Kadyrzhanova, and Lel (2013) who exploit variation coming from director deaths. As both law changes and director deaths are infrequent events, our study contributes by providing large-sample evidence on the resulting managerial actions when monitoring constraints are temporarily relaxed. Fich, Harford, and Tran (2013) also argue that institutional investors do not monitor all their holdings in an equal way – they focus on the stocks that represent a larger component of their portfolio. Our findings are complementary to theirs, suggesting that the investors' ability to monitor is subject to constraints, and time-varying.

3.2. Theory and Data

3.2.1. Theoretical Framework

To fix ideas, this section describes our theoretical framework and derives our key empirical prediction.

Managerial actions and shareholder monitoring

Suppose the firm is run by a manager who, absent shareholder monitoring, would maximize private benefits B and set $B = B^{max}$. For example, the manager might make privately beneficial investments or pay herself more. With shareholder monitoring of intensity K, the manager trades off private benefits with the cost imposed via the monitoring constraint, and chooses an optimal level of shirking $B^* \mid K < B^{max}$. In general, shirking will be a decreasing function of monitoring intensity, i.e., $B^* = f(K)$ with $f' < 0$.

We focus on monitoring from institutional investors, and there is a large theoretical literature motivating why and when institutions can be effective monitors. In this literature, different institutional monitoring mechanisms are often discussed under the headings "voice" and "exit". Voice involves direct forms of intervention, such as voting against management at the annual meeting, direct discussions with management, or taking over the company and dismissing incumbent management. The IRRC (2011) survey reports that only 15%-20% of institutional investors are not usually engaging with corporations, and that the most common forms of engagement are exchanges of letters and telephone calls, many of which are never made public. Exit proposes that institutions can also discipline managers by threatening to sell their shares in the secondary market (e.g., Admati and Pfleiderer (2009), Edmans (2009)). Survey evidence by McCahery, Sautner, and Starks (2011) suggests that this channel is empirically relevant. Edmans and Manso (2011) show that monitoring by multiple minority blockholders – as opposed to monitoring by only few large blockholders – can be an optimal equilibrium outcome. Our empirical design below will will therefore allow for monitoring by minority blockholders.

Monitoring intensity and limited attention

For the monitor, supplied monitoring intensity is itself based on a tradeoff between benefits and costs. Numerous papers in the literature analyze versions of this tradeoff, focusing for example on the direct cost of gathering information, stock market liquidity, the degree of investor protection, the degree of asymmetric information, and complementarities between managers and blockholder effort.

The key conjecture in our paper, which is new to the best of our knowledge, is that monitoring capacity is a scarce resource that can *temporarily* lead monitors to supply less than the otherwise optimal monitoring capacity K^*. One way to think about the mechanism is to frame the monitor's problem as optimally allocating attention subject to a limited attention constraint, in the spirit of the optimal limited attention literature in economics (e.g., Sims (2003), Kacperczyk, Nieuwerburgh, and Veldkamp (2011)).

To illustrate this in the simplest possible case, suppose that a potential monitor – in our setting an institutional shareholder – has a stake in two unrelated firms and can divide a fixed amount of attention $\bar{K}$ between them, such that $K_1 + K_2 \leq \bar{K}$. For example, a mutual fund manager decides every day how many hours to spend on obtaining information on the macroeconomy, different industries in her portfolio, or specific stocks within each industry. Assuming that the payoff to the monitor has the form $\pi = g(K_1) + h(K_2)$, where g and h are increasing concave functions, the optimal allocation (K_1^*, K_2^*) will equate the marginal benefits $\partial g/\partial K_1 = \partial h/\partial K_2$ (assuming an interior solution). Suppose now there is a positive shock to the marginal benefit of learning about firm 1. This shock could be real, and based on economic fundamentals, or merely perceived, i.e., due to psychological factors unrelated to fundamentals. In either case, the monitor would optimally shift attention towards firm 1 and away from firm 2, which, in turn, reduces the intensity of monitoring at firm 2.

Empirical approach and key prediction

The central idea of our empirical approach is to construct a firm-level proxy identifying temporal shifts in investor attention. In the above example, we identify times where monitors shift attention to firm 1, which decreases the supply of attention to firm 2. From firm 2's point of view, this implies a reduction in monitoring, since the new attention level K_2^{NEW} is smaller than K_2^*, which, in turn, implies a looser monitoring constraint faced by the manager, and therefore more room to maximize private benefits, i.e., $B^{NEW} = f(K_2^{NEW}) > B^* = f(K_2^*)$. With multiple shareholders, this will be true as long as a reduction in attention by one institutional shareholder cannot be instantaneously and costlessly substituted by other monitors, such as boards or other institutional shareholders. We summarize our key prediction as follows:

Distracted Shareholder Hypothesis: *If institutional shareholders shift attention away from the firm, this loosens monitoring constraints and managers have greater leeway to maximize private benefits.*

This prediction can be borne out in the data in two ways. First, managerial actions can be linked to investor attention if managers observe shareholders are distracted and then initiate private benefit maximizing projects. Managers, aided by their investor relations (IR) departments, will generally be aware of who their institutional shareholders are. Information on institutional holdings is readily available from various sources including the SEC's EDGAR system for 13f filings, Bloomberg terminals, or shareholder intelligence firms. Survey evidence suggests that CEOs and CFOs interact frequently with both IR departments and investors directly, which allows them to receive distraction signals quickly. For instance, manager can get a sense of shareholder distraction from fewer direct phone calls, fewer meeting requests by institutional investors, diminished news coverage, conference calls with fewer critical questions, from simply observing that many investors are focusing on "hot" or "crisis" industries (e.g., technology in 1999/00 and banks in 2007/08), or from direct communication with investors. Consistent with distraction being detectable from shareholder actions, we show below that distracted shareholders are less likely to participate in conference calls and that they are less likely to initiate a proposal in general meetings.

Second, and consistent with the large literature on managerial agency problems, managers might try to initiate bad deals even if they do not directly observe shareholder distraction. Shareholders have a higher probability of preventing a bad project when they are not distracted, either directly, via informal communication with the management, or indirectly, via the threat of "exit", i.e. of liquidating their holdings of the firm's stock. In both cases we would also observe a direct link between more privately optimal managerial actions and shareholder distraction. Hence, it is a sufficient, but not a necessary condition, to assume managers notice shareholders being distracted before embarking on projects that maximize their private benefits.

3.2.2. Data Sources

We combine data from a number of sources. The main source is the Thomson Reuters institutional holdings database. This database covers all institutional investors required to file form 13f with the SEC, which covers all institutions with assets exceeding $100 million in market value. Every quarter, institutions are required to report the number and market value of each share they hold, unless they own less than 10,000 shares or unless the shares they hold are worth less than $200,000 at the last day of the reporting period.

We also obtain stock prices from CRSP, financial reporting data from Compustat, and merger announcement data from SDC. We use "lucky" stock option grants information from Professor Lucian Bebchuk's website. Throughout the analysis, we exclude micro-caps, defined as stocks with market value below the 20th NYSE percentile breakpoint following Fama and French (2008), as they are not relevant for most institutional investors. Our resulting sample comprises 21,872 individual firms whose stocks are held by 6,207 institutions, over the period 1980–2010. We therefore capture essentially all of the US equity investment universe relevant for institutional investors.

3.3. Measuring Distraction

3.3.1. Variable construction

Our main variable of interest is a firm-level proxy for how much the "representative" institutional investor in a given firm f is distracted in a given period. We call this proxy *distraction*, and denote it by D. D is defined so that higher values are associated with shareholders that are more distracted. In terms of our main conjecture, a higher D implies temporarily looser monitoring constraints faced by the firm's managers.

The intuition behind D is straightforward and follows the approach in the thought experiment in the introduction: a given investor i in firm f is more likely distracted if there is an attention–grabbing event in another industry, and if that other industry is important in investor i's portfolio. We first compute an investor–level distraction score, and then aggregate across all investors in the firm. Specifically, we define D for each firm f and calendar quarter q as:

$$D_{fq} = \sum_{i \in F_{q-1}} \sum_{IND \neq IND_f} w_{ifq-1} \times w_{iq-1}^{IND} \times IS_q^{IND} \tag{3.1}$$

where F_{q-1} denotes the set of firm f's institutional shareholders at the end of quarter

$q-1$, IND denotes a given Fama-French 12 industry, and IND_f denotes firm f's Fama-French industry. IS_q^{IND} captures whether a distracting event occurs in an industry other than IND_f, and w_{iq-1}^{IND} captures how much investor i cares about the other industry. The weight w_{ifq-1} captures how important investor i is for firm f. We now explain the construction of these terms in greater detail.

First, w_{iq-1}^{IND} is defined as the weight of industry IND in the portfolio of investor i.[1] Second, IS_q^{IND} is an industry-level measure of whether something distracting is going on in industry IND in quarter q. We refer to IS as an *industry shock*. In most of our tests, we define IS as an indicator variable equal to one if an industry has the highest or lowest return across all 12 Fama-French industries in a given quarter. IS is motivated directly by Barber and Odean (2008), and can be justified on two, not mutually exclusive, grounds. On the one hand, extreme return periods are periods when learning about uncertainty can be particularly beneficial (e.g., Kacperczyk, Nieuwerburgh, and Veldkamp (2011)). Hence, there can be, all else equal, a rational incentive to shift attention towards extreme-performing industries. On the other hand, IS could capture psychological effects. For example, retail investors and the media might focus "too much" on out- and underperformers, which, in turn, might give an incentive to institutional managers to shift some of their attention to the segment most salient to their investors. Both mechanisms suggest that IS might be effective in marking industries that are more attention-grabbing than others *in a given quarter*, which is precisely what we need for our identification strategy. An important advantage of this definition is that industry shocks used in the construction of D are not mechanically related to the fundamentals of the firm we are interested in, since the firm's own industry is excluded. Thus, IS is a plausible candidate for identifying exogenous shocks to investor attention. We also examine alternative measures of attention-grabbing events suggested by Barber and

[1] We assign each stock in i's portfolios to one of the 12 Fama-French industries based on its historical SIC code (Compustat data item SICH). Whenever the historical SIC code is not available, following Fama and French (2008), we replace it by the CRSP SIC code (data item HSICCD).

Odean (2008) in our tests below.

The two previous terms measure, for each investor i of firm f, whether something distracting is going on in an unrelated industry (IS_q^{IND}) and whether investor i cares about the unrelated industry (w_{iq-1}^{IND}). In a final step, we aggregate investors to obtain a firm–level distraction measure. Given the large differences between institutional investors, their holdings, and their motivation to monitor, equally weighting all investors is inappropriate. Therefore, we take a weighted average, with weights w_{ifq-1}. We give more weight to investor i if (i) firm f has more weight in i's portfolio, and (ii) if i owns a larger fraction of firm f's shares. The former captures that investors will on average spend more time and effort analyzing the biggest positions in their portfolio (Fich, Harford, and Tran (2013)). The latter captures that managers will care more about their largest shareholders, who also have the largest incentive to monitor, as suggested, for example, by the IRRC (2011) survey.

We therefore define:

$$w_{ifq-1} = \frac{QPFweight_{ifq-1} + QPercOwn_{ifq-1}}{\sum_{i \in F_{q-1}} \left(QPFweight_{ifq-1} + QPercOwn_{ifq-1}\right)}. \tag{3.2}$$

Here, $PercOwn_{ifq-1}$ is the fraction of firm f's shares held by investor i, and $PFweight_{ifq-1}$ is the market value weight of firm f in investor i's portfolio. To minimize the impact of outliers and measurement error, we sort all stocks held by investor i in quarter $q-1$ by $PFweight_{ifq-1}$ into quintiles, denoted $QPFweight_{ifq-1}$. Similarly, we sort firm f's shareholders by $PercOwn_{ifq-1}$ into quintiles $QPercOwn_{ifq-1}$. Finally, we scale by the term in the denominator so that the weights w_{ifq-1} add up to one.

In sum, our investor distraction measure (3.1) depends on whether shocks occur in other industries, whether investors care about those other industries, and whether investors that are most affected by the unrelated shock are potentially important monitors.

3.3.2. Distraction Events and Impact on Monitoring Supply

One might ask what the economic nature of the distraction events captured by IS is, and whether the distraction events can have a prolonged impact on monitoring capacity.

The leading examples of economic fundamentals underlying distraction events are unanticipated significant industry-specific changes in the competitive landscape, technology, demand, or regulation. These events take time to unfold, and to be understood. They can thus draw on limited attention capacity for a protracted period of time, and therefore lead to looser monitoring of industries that are not in the focus. Prominent large-scale examples of longer-term distraction events are the recent banking crisis (e.g., 2007Q4 industry return: -10.1%), the tech bubble (e.g., 2000Q1 industry return: $+14.8\%$), or the oil spill in the Gulf of Mexico (2010Q2 industry return: -11.4%). Those events grabbed the attention of investors and the media for an extended period of time and made them focus on one specific industry.

While some distraction events may stretch over extended periods of time, this is not a necessary condition to observe a prolonged impact on monitoring capacity, for two reasons. First, it may take time to fully understand the impact of a significant unanticipated event even if the event itself is short. Examples include natural disasters, technological breakthroughs, court rulings, or new legislation. Second, even short-term distraction events can lead to temporal changes in the relative marginal benefit of supplying attention, which can lead institutions to re-optimize their attention allocation, and therefore monitoring capacity supply, across their portfolio.[2] In the limiting case, if the relative marginal benefit of obtaining information increases permanently, investors with limited attention might permanently shift attention away from an industry even if the distraction event itself is very short.[3]

[2]This rational attention allocation mechanism is similar to Kacperczyk, Nieuwerburgh, and Veldkamp (2013) who analyze attention shifts across the business cycle, which are also longer term shifts in attention.

[3]To be sure, we would expect institutional investors to eventually adjust attention capacity, for

In sum, we argue that our distraction measure based on quarterly industry returns can capture shifts in investor attention and therefore exogenous changes in monitoring constraints on a time-scale that would be relevant for managerial actions.

3.3.3. Does D Measure Distraction?

We now provide direct evidence suggesting that D captures institutional investor distraction.

Evidence from conference calls and shareholder proposals

We start by analyzing two settings in which institutional investors interact with a company. We ask whether there is less interaction when D is high, which would be consistent with D measuring distraction.

The first setting is conference call participation. If shareholders are distracted, they presumably are less likely to ask a question in a conference call, either because they are not attending the call, or because they did not prepare a question. To test this, we obtain a large dataset on conference call participation, comprising 13,308 conference calls for 1,198 firms between 2003 and 2010.[4] We regress the logarithm of one plus the number of active conference call participants on D and control variables described below. Specifications (1) and (2) in Table 3.2, Panel A show that conference call participation is indeed lower when D is high, i.e. when shareholders are distracted according to our measure.

The second setting we consider are shareholder proposals in general meetings. If

example by hiring additional staff, if distraction events are long enough. However, hiring employees takes time, and staff with the right expertise needed after a sudden economic change is most likely in short supply then. It is therefore implausible that *all* institutional investors can adjust attention capacity quickly and easily by hiring additional staff *at the same time*.

[4] Alexander Wagner and Romain Boulland graciously provided the conference call data. We refer the reader to the description of the datasets to be found in Druz, Wagner, and Zeckhauser (2015) and Boulland and Dessaint (2014). The latter paper is one of the first to propose conference call attendance as a measure of investor attention. For our tests, we use the union of the two datasets to maximize coverage, but we find similar results when we look at each dataset separately.

institutional shareholders are distracted prior to a meeting, we should see fewer proposals by institutional shareholders. For this test, we obtain data from RiskMetrics on 4,551 shareholder proposals in 1,024 firms between 1997 and 2010. We regress the logarithm of one plus the number of proposals initiated by institutional shareholders on a measure of distraction and controls described below. Because proposals need to be prepared and submitted in advance of a general meeting, we use the average of D over the four quarters before the quarter of the annual meeting as our distraction measure in this test. Specifications (3) and (4) in Table 3.2, Panel A, show that a higher level of shareholder distraction is associated with fewer proposals.

The above results on conference calls and shareholder proposals provide direct evidence suggesting that more distracted shareholders, as measured by our variable D, are less likely to actively engage with corporations. Those effects are economically meaningful. A one standard deviation change in distraction leads to 19% $(= -2.671 \times 0.07)$ fewer active conference call participants and 6% $(= -0.834 \times 0.07)$ fewer proposals by institutions.

Both tests include industry × date fixed effects, so that we compare firms within the same industry at a given point in time. The results are therefore not due to any industry-date specific omitted variable. Importantly, we control for the level of institutional ownership, and we control for institutional ownership concentration ("Top 5 share") as in Hartzell and Starks (2003), so our results are not subsumed by standard measures of institutional ownership structure. Additional controls include firm size, Tobin's Q, cash flow, and the level of the firm's cash holdings (we provide a complete list of variable definitions in the Appendix). Specifications (2), (4) and (6) show that our results also obtain when we include firm fixed effects.

The proposal setting allows us to conduct an informative placebo test. Because we know who initiates proposals, we replace the dependent variable we used in specifications (3) and (4), proposals initiated by institutional shareholders, by proposals initiated by non-institutions. Specifications (5) and (6) suggest our measure of distraction is largely unrelated to proposals by non-institutions. This is strongly supportive of D capturing institutional shareholder distraction, and has two implications. First, our results are not due to our distraction measure being correlated with an omitted variable that would reduce the number of *all* proposals for that company and date. Second, D seems to do a good job in picking up the variation it was designed to pick up: activity of institutional shareholders.

In sum, both tests suggest D captures how closely institutional shareholders are monitoring the firm.

Evidence from portfolio changes

An alternative approach to showing D captures investor distraction is to analyze trading behavior. The tests in this section are motivated by the insight that large changes to positions in an investor's portfolio do usually not occur by accident. Rather, large changes in either direction entail deliberation and analysis. They require attention. Hence, a distracted shareholder who focuses his attention on some other parts of the portfolio is less likely to make very large changes to positions in stocks currently not in the focus. We therefore test if large holdings changes are less likely to occur in periods when an institutional shareholder is distracted. To measure distraction of an individual shareholder, we look at the institution-specific component of our distraction measure in equation (3.1), i.e. the individual investor's exposure to the shock industries, measured as $\sum_{IND \neq IND_f} w_{iq-1}^{IND} \times IS_q^{IND}$.

Panel B in Table 3.2 presents results for different measures of portfolio changes. Specifications (1) to (3) use the absolute change in the holdings of a specific stock in the portfolio of a specific investor as dependent variable. We focus on absolute values because attention is required for changes in either direction. Following Chen, Harford, and Li (2007), we include as controls (omitted for brevity) the lagged fraction of shares of a given firm held by a given investor, the lagged weight of the stock in the investor's portfolio, lagged investor size (log of total AUM), current and lagged stock returns for the stock, current and lagged turnover, turnover in the same quarter one year ago, the firm's book-to-market ratio, and the number of days between the announcement date and quarter end. In all tests, we exclude stocks in the shock industries.

Specification (1) shows that distracted shareholders, those with above median exposure to the shock industries in a given industry-quarter, are associated with smaller changes to their portfolio holdings. Because we include industry $\times$ quarter fixed effects, this is not due to smaller changes to stocks in this industry and quarter in general. Specification (2) includes stock $\times$ quarter fixed effects. We find that *the same stock* is less likely to experience large changes in its portfolio weight if it is held by a distracted investor. The fixed effects ensure that any stock-quarter specific factor, including firm-specific news, the stock's return, and the type of stock (e.g., small, big, value, growth), is not inducing the results. Specification (3) adds investor fixed effects and shows that the results from specification (2) survive even if we eliminate all time-invariant variation on the investor level, which includes, for example, investor type. We interpret those results as strongly supportive of the idea that D captures investor distraction and, given the fine fixed effects, it is not clear what alternative story could explain our findings.

Specifications (4) and (5) show those results also obtain for alternative dependent variables. Specification (4) uses the change in the fraction of a given firm's stock held by a given investor and specification (5) uses churn rates (e.g., Gaspar, Massa, and Matos (2005), Yan and Zhang (2009)), computed for each investor across all stocks in a given industry held by this investor.

In sum, the results on portfolio changes presented here, as well as the results on conference calls and shareholder proposals presented in the previous section, suggest that D is a useful measure of institutional shareholder distraction.

3.4. Main Results

This section presents our main results. We focus first on the likelihood of announcing an acquisition when shareholders are distracted. We then examine whether these acquisitions are value-destroying.

3.4.1. Merger Frequency

With looser shareholder monitoring, self-interested managers have an incentive to distort investment budgets to maximize their private benefits. Acquisitions, especially diversifying ones, are leading examples of such suboptimal investment.

In this section, we document a relation between the frequency of acquisitions and investor distraction. Analyzing takeovers is interesting because they represent substantial discretionary investments, and because we can precisely observe their announcement dates. Managers decide the timing of the deal, which allows us to relate the temporal variation in merger activity to the temporal variation in our distraction measure. By contrast, most other forms of corporate investment are disclosed in one aggregate figure in the financial statements and do not allow us to see when individual investments are actually initiated. An important added advantage for identification purposes is that expenditure for takeovers is much less sticky than other forms of corporate investments.

We regress an acquisition announcement indicator on investor distraction using linear probability models. All our tests include industry × quarter fixed effects, so that we compare firms within the same industry at a given point in time, as in our motivating example in the introduction. This allows us to rule out the effect of any factors that do not vary within industry-date. Additional controls include firm size, Tobin's Q, and cash flow as in Malmendier and Tate (2008), as well as institutional ownership and institutional ownership concentration ("Top 5 share") as in Hartzell and Starks (2003). We also control for the level of the firm's cash holdings. We provide a complete list of variable definitions in the Appendix. Standard errors are clustered at the firm level in all regressions.

Table 3.3 presents the results. In Panel A, we find that the probability of announcing a merger is higher when shareholders are distracted. A one standard deviation increase in the distraction measure D is associated with a 29% ($= 0.052 \times 0.07/1.24\%$) higher merger probability. The effect is in the same order of magnitude as the effect of institutional ownership and ownership concentration, which are both significant and yield a change of 14% and –42% relative to the mean for a one standard deviation shift in ownership or concentration, respectively. Hence, investor distraction has an economically significant impact on takeover activity, over and above that of well-known institutional ownership characteristics.

We next test a finer prediction of the distracted shareholder hypothesis, and distinguish between within-industry and diversifying deals, defined on the basis of FF12-industries. The literature suggests that diversifying deals, in particular, can increase managerial private benefits at the expense of shareholder value because they can reduce CEO human capital risk, and because they offer a chance to venture into industries that are considered fashionable, glamorous, reputable etc. (e.g., Amihud and Lev (1981), Morck, Shleifer, and Vishny (1990)). We should thus expect a stronger impact of shareholder distraction on diversifying deals. The estimates in Table 3.3, column (2), support this hypothesis.[5] The impact of shareholder distraction on diversifying deals is nearly twice as strong as on acquisitions in general: a one standard deviation increase in distraction increases the chance of a diversifying deal by 65% ($= 0.116 \times 0.24 \times 0.07/0.30\%$). Further, while the effect of shareholder distraction is strongly significant for diversifying deals, it is weaker for within-industry transactions which are less likely motivated by managerial private benefits (column

[5]The baseline probability of of observing a diversifying and a within-industry merger conditional on observing a merger announcement are different (24% and 76%, respectively). For ease of comparing the impact of distraction, we therefore report coefficients divided by the baseline probability in columns (2), (3), (5), and (6).

(3)). Interestingly, the impact of institutional ownership and ownership concentration is, if anything, stronger for within-industry deals, highlighting again that we are capturing a different effect with our distraction measure.

Our results in Panel A relate acquisition announcements to institutional shareholder distraction *in the deal quarter*, because this allows us to most cleanly identify the effect of interest. In general, the assumption that managers could act upon shareholder distraction by announcing a takeover within (at a maximum) a three-month period is not unreasonable, as typical transactions take about ten weeks from first contact between bidder and target to finally announcing the takeover (e.g., Fruhan (2012)). Still, the process will often take longer than one quarter. To allow for this, we repeat our analysis from Panel A, but now average D over quarters -2 to 0 relative to the deal quarter. The underlying assumption is that there are a number of critical steps in the takeover process, and that a deal initiated when the shareholders are distracted has a greater chance of being continued in the next quarter, even if investors are now less distracted. Panel B shows that our results become even stronger when we allow for this alternative timing convention.

In sum, the results in this section provide strong support for the Distracted Shareholder Hypothesis: limited shareholder attention allows distraction shocks to translate into more privately optimal managerial actions via temporarily looser monitoring constraints. Moreover, they are inconsistent with monitoring by the boards or other shareholders being a perfect substitute for distracted institutional shareholders, nor are they consistent with the "quiet life" alternative hypothesis.

3.4.2. Alternative Explanations and Unobserved Heterogeneity

Our results in Table 3.3 are in line with the idea that limited investor attention relaxes managerial monitoring constraints. In this section, we discuss necessary conditions for alternative explanations and argue why we believe unobserved heterogeneity is unlikely to induce our results.

We emphasize that any alternative story has to explain a number of facts simultaneously. First, because we compare firms within industry and date, it would have to explain why a "shock", i.e. *either* extreme positive *or* negative returns in an unrelated industry would increase takeover activity in some firms but not others. Second, it would also have to explain why the affected firms are precisely the ones whose institutional shareholders are exposed to the shock industry. Third, it would need to be unrelated to institutional ownership or institutional ownership concentration. Fourth, it would need to explain why we see more diversifying deals. In this section we add two additional pieces of evidence that further raise the bar for a feasible alternative hypothesis.

First, we include firm fixed effects to control for time-invariant unobserved heterogeneity. Columns (4) to (6) in Panels A and B in Table 3.3 show that our results are not affected. This rules out, for example, selection stories in which some unobservable, time-invariant, variable matches firms that – for whatever reason – are more likely to do diversifying takeovers with investors exposed to "shock" industries.

Second, we exploit time-variation in shareholder distraction. We re-estimate specification (5) in Table 3.3, Panel A, but add four lags of D as additional regressors. The top panel of Figure 3.2 summarizes the results. Shareholder distraction has a significant effect on merger frequency in the current quarter as well as in the following two quarters, while the effect of D falls to essentially zero once we look at mergers announced beyond $q = +2$. This pattern is consistent with the view that it can often take more than one quarter from deal initiation to announcement, but likely not much longer than three quarters. It seems non-trivial to explain these patterns with plausible alternative stories based on unobservable time-varying variables.

In sum, we conclude that these patterns raise the bar for alternative explanations of our results based on unobservable variables, both time-invariant or time-varying.

3.4.3. Robustness and Alternative Specifications

Table 3.4 presents robustness tests. Unless otherwise mentioned, we report results for the specifications in Table 3.3, Panel A, columns (2) ("OLS") and (5) ("FE") on diversifying deals, and suppress all control variables for brevity.

Panel A shows results for alternative investor distraction proxies. Barber and Odean (2008) propose two alternative ways to measure attention-grabbing events: trading volume and news. For trading volume we follow Barber and Odean (2008) and define the shock industry to be the one with the highest current-quarter trading volume normalized by the average trading volume over the previous four quarters. For news, we use Factiva to count newspaper articles about a given industry. To remove the effect that some industries might be in general more in the news than others, we follow a similar approach as in Da, Engelberg, and Gao (2011) and calculate the abnormal increase in news articles as the difference in logs of the number of news articles reported in Factiva in a given quarter, minus the median number of news articles during the previous four calendar quarters. Panel A shows that both alternative definitions of attention-grabbing events, based on trading volume and based on news, produce results qualitatively very similar to our return-based measure, even though measuring distraction from returns seems to be empirically more powerful.

Next, we define distraction based on *either* extreme positive *or* extreme negative returns alone, whereas our main measure looks at both. We find again that, while the baseline measure is more powerful, the results are qualitatively very similar. Finding action in both extremes is useful, because it is consistent with the attention-grabbing nature of extremes, and suggests again that we are not capturing something fundamental about either firms or investors picking good or bad industries. We also find similar effects when we define extreme industry returns based on the time-series of returns. In those tests we define extreme industry-return-quarters as quarters where the industry return is in the bottom or top decile of the distribution of quarterly industry returns over the previous 40 quarters.

We then compute distraction measures on subsets of the 13f investors. Specifically, we compute distraction using only the largest 5, 10, or 20 investors by percentage of shares owned in the firm and find that our results are robust. Especially when we measure distraction over three quarters, the results are highly significant for all groups and specifications.

In Panel B we add additional control variables. A first potential concern may be that, within a given Fama-French industry, some firms are mechanically related to the shock industry because they are misclassified (even though we note that it is not obvious why such a firm would be more likely to engage in a value-destroying acquisition). We define a variable *Relatedness to shock industry* as follows: we first obtain, for each firm, the set of closely related firms from the Hoberg and Phillips (2010) text-based industry classification dataset from Professor Gerard Hoberg's website; we then compute, for each firm, the percentage of related firms which operate in the shock industries to obtain a firm-specific proxy for the severity of the misclassification problem. Because the Hoberg-Phillips data, which come in firm-pairs, start only in 1996, we use the first available firm-pair in all previous periods in which we observe both firms in our data (our results are qualitatively unchanged if we do not backfill the relatedness measure). The results in Panel B indicate that relatedness to the shock industries does not induce our results. The results are qualitatively very similar to our baseline for both the contemporaneous distraction measure as well as the moving-average distraction measure, and the point estimates actually increase when we control for relatedness.

In the next two tests, we investigate whether our effects are robust to controlling for additional institutional investor characteristics. First, we control for the share of institutional ownership by independent and long-term institutions (ILTI) following Chen, Harford, and Li (2007). Second, we control for the combined share of ownership by non-transient investors, defined following Bushee (2001). Our results in Panel B show that the temporal variation in investor distraction is largely unrelated to investor type – presumably because the fraction of ILTI and non-transient investors is much less variable over time. In other words, these findings suggest that our earlier results are driven by a temporary lack of shareholder attention, rather than a change in the kind of shareholders faced by the firm. Consistent with this view, Panel B also shows that our results become, if anything, stronger when we directly control for average investor size, average investor portfolio concentration, or average churn rate of investors holding the stock.

We also verify that our results are not driven by changes in the valuation of firms in the portfolio of investors who are facing large shocks. First, we control for the weighted average flow of the institutional investors holding the stock. Second, we control for the degree of institutional price pressure, defined following Coval and Stafford (2007) as the difference between mutual fund flow-induced purchases and flow-induced sales in a given stock and quarter divided by the average trading volume of the stock from prior quarters. Panel B shows that our results remain largely unchanged when we include these two control variables. Finally, the last rows of Panel B show that our results are qualitatively unchanged when we jointly control for all aforementioned variables.

In Panel C we start by checking if our results are related to deals done in the shock industries themselves. Panel C shows that, even though we lose a non-trivial fraction of our takeovers our results remain largely unchanged if we exclude deals where the target belongs to either a positive or negative shock industry. Our findings are therefore not due only to deals in shock industries. Panel C also shows that our results are robust to restricting our merger sample to completed deals and to excluding serial acquirers, defined as firms making 5, or 10, bids in our sample, respectively. Finally, in Panel D, we show that our results are robust to the use of an alternative estimation method, the conditional Logit model.

We conclude that our main results are robust to a large set of different definitions of attention-grabbing events, additional control variables, different takeover subsets, and alternative estimation methods.

3.4.4. Merger Performance

In the previous sections, we have documented that firms are more likely to make an acquisition, especially a diversifying one, if their institutional investors experience shocks to unrelated parts of their portfolios. In this section, we show that those deals are value destroying.

Before presenting results, we emphasize again what we do and do not assume. We do not assume that *all* shareholders are distracted when D is high. We do assume that higher D proxies for times when the *representative* shareholder is distracted – that is, we assume that lack of attention by one investor cannot be costlessly and instantaneously compensated for by increased attention by other investors. In the takeover context, this has two implications. First, we may observe a short-term stock price reaction even when shareholders are distracted. Second, if shareholders are distracted when the announcement is made, not all information about the merger might be instantaneously incorporated into the stock price, and there could be long-run abnormal stock returns. We provide evidence supporting both predictions.

Announcement effects

Table 3.5 presents results for short-term effects around the merger announcement date. We regress 3-day $(-1, +1)$ bidder abnormal announcement returns on a set of control variables capturing deal, bidder, and target characteristics following Moeller, Schlingemann, and Stulz (2004) and Baker, Pan, and Wurgler (2012). Control variables also include institutional ownership, the Top 5 share, an indicator for new economy firms, and the log of the number of deals in the industry to capture times of heightened M&A activity. As before, we are interested in comparing acquirers with and without distracted shareholders within industry and quarter, so we include acquirer industry $\times$ quarter fixed effects in all regressions. We also present results that additionally control for the target industry. All regressions are estimated using weighted least squares, where the weights are inversely proportional to the estimation variance of the abnormal returns. Standard errors are clustered by announcement month.

Specifications (1) and (2) in Panel A of Table 3.5 show that higher shareholder distraction during the quarter of the merger announcement is associated with lower abnormal returns. In the three days around the announcement, bidders lose an additional 43 basis points ($= 0.07 \times 0.062$) when the shareholders are distracted, which, relative to the average announcement return of -131 basis points, is an economically large effect.

Specifications (3) and (4) in Table 3.5, Panel A, repeat the analysis for synergies, defined as the weighted average (by market capitalization) of bidder and target announcement returns as in Bradley, Desai, and Kim (1988). Our results indicate that synergies are lower in deals announced when the shareholders are distracted, consistent with the view that such acquisitions are of lower quality. If the marginal deal in the absence of distracted shareholders has zero synergies, the results indicate that the marginal deal with distracted shareholders is overall value-destroying.

Panel B repeats our analysis when we measure distraction over quarters $q = -2$, 1, and 0. As in our earlier tests, results tend to get stronger in economic terms, especially for synergies, where the coefficients now more than double. As before, these results indicate that using the longer window to measure distraction is capturing more of the relevant variation in investor distraction.

Long-run effects

If not all information is impounded in the price at the announcement date, or if managers can successfully hide some adverse information about the deal initially, we might expect negative long-run abnormal returns for takeovers announced when investors are distracted. We analyze long–run returns using Ibbotson's Ibbotson (1975) returns across time and security (IRATS) method combined with the Fama and French (1993) three-factor model, as in Peyer and Vermaelen (2009), as well as the calendar-time approach (Fama (1998)).

In both tests, we split the sample into high and low distraction bidders within each industry as follows. We first compute for each bidder the average distraction over quarters $q = -2$, 1, and 0, the long-window distraction measure used in Tables 3.3 and 3.5, as our earlier tests indicates it has more power to capture investor distraction relevant for M&A deals. We then define high and low distraction bidders as those bidders with above (below) median distraction values in a given bidder industry and announcement year.

Figure 3.3 presents results based on the IRATS approach. While the low-distraction group exhibits a modest downward trend in their abnormal returns, the bidders with distracted shareholders experience substantially negative abnormal returns over the 36 months following the deal. Over this three-year period, the cumulative abnormal risk-adjusted return for high distraction bidders is a negative 9.5%, and the high minus low difference is 6.1%. Hence, the effect is economically large. As indicated by the grey bars, the difference between the two groups is highly statistically significant for most of the sample period. We also find that the cumulative return for the high distraction group presented in Figure 3.3 is significantly different from zero at the 1% level in almost all post-event months. Importantly, the figure also shows that there is little sign of a difference before the announcement month, thus reinforcing a causal interpretation of our effects.

We also use the calendar-time portfolio approach to complement our findings from the IRATS method. Specifically, we compute the returns on a long-short distraction portfolio using all sample firms that have announced an acquisition in the previous 6, 12, 18, 24, or 36 months. The strategy buys high-distraction bidder stocks, sells low-distraction bidder stocks, and equal-weights stocks within the long and short legs of the portfolio. Table 3.6, Panel A presents the average monthly abnormal returns using the Fama and French (1993) three-factor model. For high distraction bidders, abnormal returns are always negative and economically large. In contrast, the abnormal returns of low distraction bidders are almost always positive, economically small, and statistically insignificant. Over the full 3-year period, high-distraction bidder stocks underperform low-distraction bidder stocks by a statistically significant risk-adjusted 6.1% ($= 0.17 \times 36$). Panel B adds the Carhart (1997) momentum factor. While both high and low distraction portfolios perform somewhat better then, the difference in performance between the long and short leg of the portfolio remains very similar: a risk-adjusted 36-month return of 6.5% ($= 0.18 \times 36$).

While there is a long-standing debate in the literature about whether event-time or calendar-time approaches are more appropriate when analyzing long-run returns, the above results show this is not a big issue in our setting. Both methods yield very similar results suggesting that deals initiated when shareholders are distracted are performing significantly worse than deals when shareholders are not distracted. These findings further support the hypothesis that managers pursue their private benefits at the expense of shareholders when monitoring constraints are temporarily relaxed.

3.4.5. Exit: Holdings Changes around Announcements

Institutional investors can influence corporate choices via “voice” or “exit”. In this section, we use holdings data to investigate the exit channel. Specifically, we test whether distracted investors are less likely to sell their stakes in the firm when the firm announces a bad deal. If investors are less likely to sell ex post, this will weaken the disciplining role of exit ex ante.

We define bad deals as takeovers with a 3-day bidder announcement return in the bottom quintile in a given year (our results are not materially affected if we use alternative cut-off points to identify bad deals). As in Section 3.3.3, we identify distracted shareholders as those investors with above median exposure to the shock industries in a given industry-quarter. We then run the following investor-stock-level regression:

$$Exit_{ifq} = \beta_1 D_{iq} + \beta_2 BadDeal_{fq} + \beta_3 D_{iq} \times BadDeal_{fq} + \beta_4' X_{ifq} + \varepsilon_{ifq}, \qquad (3.3)$$

where $Exit_{ifq}$ refers to one of three different measures of selling for investor i in firm f in quarter q. The first definition follows Parrino, Sias, and Starks (2003) and uses a dummy variable *Large Decrease* which indicates whether the holdings change is in the bottom quintile of the full-sample distribution. The second definition, *Negative Change in Ownership*, equals the percentage change in the fraction of the firm's stock held by investor i in firm f in quarter q if that change is negative, and zero otherwise. The third measure is an indicator variable *Sell All*, equal to one if the investor sells her entire stake in the firm. X_{ifq} are firm-level control variables following Chen, Harford, and Li (2007), including current and lagged stock returns, current, lagged, and one-year lagged turnover, the firm's book-to-market ratio, and the number of days between the announcement date and quarter end. We also control for the lagged fraction of shares in a firm held by a given investor, the lagged weight of the stock in the investor's portfolio, and lagged investor size. We further include industry × quarter as well as investor fixed effects in all regressions. Our main prediction is that β_3 is negative, i.e. that selling around bad deals is less likely if shareholders are distracted. Note that because shocks underlying our distraction definition can be either positive or negative return events, we do not have a prediction for the baseline effect on D_{iq}.

Results are reported in Table 3.7. Specification (1) shows that distracted investors are 30% (= −0.604/2.010) less likely than non-distracted investors to reduce their holdings by a large amount when the firm announces a bad acquisition. Specification (2) shows that the effect becomes even stronger once we exclude "dedicated" investors, according to the Bushee (2001) classification, who, by definition, are less likely to exit. Specifications (3) to (6) show that we obtain similar results for the other two exit measures. In particular, distracted investors are 31% (= −0.369/1.183) less likely than non-distracted shareholder to liquidate their entire stake after bad M&A announcements.

In sum, the effect of investor distraction on the propensity to sell after a bad takeover announcement is economically meaningful. Results are consistent with the Distracted Shareholder Hypothesis and the ex-ante motivation of managers to engage in privately optimal deals.

3.4.6. Mandatory Shareholder Votes and Deal Structure

In this section we exploit an institutional feature to provide additional evidence in support of the distracted shareholder hypothesis. Listing rules in most exchanges require a shareholder vote for bidding firm shareholders if the deal involves issuing more than 20% of new shares, but not otherwise.[6] A formal shareholder vote on a deal would almost surely draw investor's attention. Therefore, all else equal, a manager who tries to exploit shareholder distraction has an incentive to structure the deal such that shareholders do not have to vote. Intuitively, managers may try to structure the deals such that they fly under the shareholders' radar as much as

[6] For example, NYSE listing rule 312.03(c) states that "shareholder approval is required prior to the issuance of common stock [...] in any transaction [...] if: (1) the common stock has, or will have upon issuance, voting power equal to or in excess of 20 percent of the voting power outstanding." Similar rules apply to Nasdaq and NYSE MKT.

possible.

In a first test, Figure 3.4 plots histograms of the percentage of common equity issued by the bidder for bins close to the 20% cutoff of interest for high and low distraction firms (relative to the median in industry and year) separately. While we see a pronounced spike just left to the 20% cutoff for the high distraction group, we observe no such spike (in fact, we observe a dent) for the low distraction group. This is consistent with managers trying to avoid crossing the 20% threshold if their shareholders are distracted.

More formally, we next implement a regression discontinuity test. Specifically, we ask whether there is a discontinuous decrease in shareholder distraction at the 20% cutoff. Table 3.8 presents results. Consistent with our hypothesis, we find a pronounced discontinuity at the 20% cutoff which makes a vote mandatory. The effect is highly statistically significant for a large range of reasonable bandwidths and increases as we focus on samples closer to the cutoff.[7]

In Panel B of Table 3.8 we run a series of placebo tests in which we repeat our analysis for cutoff values of 10%, 15%, 25%, 30%, 40%, and 50%. We do not find any trace of a meaningful difference, either statistically or economically, for any of those alternative cutoffs. Hence, our results are specific to the 20% cutoff, which is exactly where the shareholder vote becomes binding.

While this test is indirect, we believe it strengthens our identification substantially. The results are consistent with managers trying to limit the effectiveness of the voice channel. It is not obvious what alternative story would explain those findings.

[7]We perform a kernel-weighted, local fourth-order polynomial regression of shareholder distraction on the percentage of common equity issued, using a triangular kernel function. Our results are robust to using alternative kernel functions (uniform and Epanechnikov) and alternative polynomials (second, third, and fifth order).

3.4.7. Influence of CEO Power and Board Strength

In the final set of tests in this section, we examine whether CEOs who are more powerful relative to their board find it easier to exploit shareholder distraction. This could be the case because strong boards that act in the interest of shareholders are more likely to step in and supply additional monitoring capacity when other shareholders are distracted. By contrast, if the board is weak, it may be easier for the CEO to persuade board members to go along with a proposed deal.

We use three standard measures of CEO power. First, we follow Bebchuk, Cremers, and Peyer (2011) and compute the CEO pay slice as the fraction of the aggregate compensation of the top five executives captured by the CEO. Second, we compute an indicator variable, board dependence, as one minus board independence, defined following Duchin, Matsusaka, and Ozbas (2010) as equal to one if the majority of directors are classified as independent in RiskMetrics. Third, we follow Harford and Li (2007) and Hermalin and Weisbach (1998) and use CEO tenure as a proxy for CEO power. We then rerun specifications (2) and (5) in Table 3.3, Panel A, including the CEO power variable and the interaction between CEO power and distraction. We control for CEO age in the CEO tenure tests.

Because data are only available for a subset of firms between 1997 and 2007, we lose more than 75% of our sample in this test.[8] Nevertheless, the results shown in Table 3.9 support the hypothesis that stronger CEOs are more likely to take advantage of shareholder distraction. A one-standard deviation increase in the CEO pay slice amplifies the effect of distraction by 46% ($= 0.211 \times 0.13/0.058$), the presence of a dependent board amplifies the effect by 81% ($= 0.083/0.103$), and a one-quartile longer CEO tenure amplifies the effect of distraction by 18% ($= 0.017/0.095$).

[8]We include a dummy for missing data, and an interaction between the dummy and distraction in our regressions to obtain more precise estimates on the coefficients with available data. By construction, our effect of interest, the interaction between distraction and our CEO power variables, is not affected. We obtain almost identical estimates on the interaction effect when we drop all firms with missing data.

3.5. Beyond M&A: Evidence From Other Settings

Our previous results showed that shareholder distraction matters for takeover decisions. While the takeover setting is attractive for identification purposes, the Distracted Shareholder Hypothesis might apply also to many other corporate actions beyond acquisitions. The aim of this section is to provide evidence from other settings – option grants to executives, dividend cuts, and CEO turnover – and to take another look at stock returns to estimate the cost of distraction to shareholders.

3.5.1. Lucky Option Grants

The ideal alternative setting for identification purposes would be a corporate action which (i) shows sufficient temporal variation, (ii) benefits managers, (iii) is unlikely to benefit shareholders, (iv) reflects a deliberate choice by the firm's managers, and (v) is of interest to institutional shareholders. We propose opportunistically-timed stock option grants as such an alternative action, and show below that we find very similar patterns as for takeovers.[9] This is reassuring, because it indicates our findings are not specific to acquisitions.

We build on work by Yermack (1997) and Bebchuk, Grinstein, and Peyer (2010) who show that managers can extract rents by opportunistically timing their equity grants. Specifically, the latter authors define "lucky grants" as stock option grants awarded on days with the lowest stock price in a given month. For a pre-specified number of stock options, such a timing pattern maximizes the value of the grant and therefore benefits managers at the expense of shareholders. We obtain data on lucky grants for the 1996 to 2005 period from Professor Lucian Bebchuk's website. Following Bebchuk, Grinstein, and Peyer (2010) we use as dependent variable a dummy equal to one if there was at least one lucky grant in the last fiscal year and control for a number of firm characteristics, CEO tenure, a dummy equal to one if

[9] According to the IRRC (2011) survey, compensation is one of the top items that prompts institutional shareholders to engage with corporations.

CEOs were hired from the outside, and two variables capturing CEO ownership. We also control for the level of level of institutional ownership and ownership concentration. As before, we include industry $\times$ year fixed effects, so that we compare lucky grants at firms with and without distracted shareholders within the same year and industry. We use a linear probability model to estimate the probability of receiving a lucky grant, and cluster standard errors by firm. As our dependent variable is a yearly measure, we average the quarterly distraction measure D for each firm and year.

The estimates reported in Table 3.10 show that the probability of receiving a lucky grant increases when shareholders are distracted. This increase is economically large: a one standard deviation change in distraction increases the chance of a lucky grant by 32% relative to the baseline ($= 0.04 \times 1.042/0.13$). Specifications (2) and (3) show that including firm fixed effects does not meaningfully alter the size or significance of our results, suggesting that we are not capturing some time-invariant unobserved factor. Interestingly, while the distraction variable is related to lucky grants, institutional ownership and ownership concentration are not, again reinforcing our earlier conclusion that we are capturing a different effect. As before, we can further strengthen our case for identification by looking at lags of the distraction measure. The bottom panel of Figure 3.2 shows that we do not observe action in the lags of the distraction measure, which again should attenuate concerns about unobserved variables spuriously inducing our results.

We can get a back-of-the-envelope estimate of the benefit for a CEO from shareholder distraction from lucky grants as follows. Bebchuk, Grinstein, and Peyer (2010) estimate the average gain to a CEO per lucky stock option grant to be on the order of $1.5 million. This is a large gain to a CEO *conditional on* receiving a lucky grant. Given a change in the annual probability of seeing a lucky grant induced by a one standard deviation change in distraction of about 4% implied by our estimates in Table 3.10, specification (3), the gain of a CEO from additional lucky grants when shareholders are distracted is therefore about $60,000 per year in expectation.

Specifications (4) to (6) repeat the exercise for lucky director grants, denoted by a dummy equal to one if at least one director received a lucky grant. The results show that distraction also tends to increase the incidence of lucky director grants. While statistical significance is not overwhelming, these findings suggest one channel that can enhance the ability of CEOs to benefit from shareholder distraction: the willingness of directors to provide additional monitoring capacity can be adversely affected by the potential for maximizing their own private benefits. Just like for CEOs, directors profit from lucky stock option grants because for a pre-specified number of stock options, a lower strike price increases the value of the option package.

Overall, these findings on lucky grants provide additional evidence strongly consistent with the notion that self-interested managers maximize private benefits when investor distraction temporarily loosens monitoring constraints. The findings indicate that the Distracted Shareholder Hypothesis is not M&A specific. Rather, limited investor attention can impact corporate actions more broadly.

3.5.2. Dividend Cuts

The second setting we consider is dividends. Managers may be more inclined to cut dividends in periods when shareholders are distracted. One reason is that, all else equal, managers may have an incentive to issue more bad news in such periods. A second reason is that, in such periods, self-interested managers may find it easier to divert funds that would otherwise have been paid out as dividends.

To test this conjecture, we collect all quarterly dividend announcements made by companies listed on the NYSE, AMEX and NASDAQ stock exchanges between 1980 and 2010. Following Grullon, Michaely, and Swaminathan (2002), we restrict our sample to ordinary quarterly cash dividends in U.S. dollars, paid to ordinary common shares. We then define a dividend cut dummy which is equal to one if the announced dividend this quarter is smaller than the dividend announced in the previous quarter. Finally, we estimate a linear probability model, and regress the dividend cut dummy on our distraction variable and controls (institutional ownership and the top 5 share, firm size, Tobin's Q, cash flow, and cash holdings.)

Table 3.11 presents results. The coefficient on the distraction measure is positive, indicating a greater propensity to cut dividends when shareholders are distracted. As before, our regression includes industry $\times$ quarter dummies, so common shocks to the propensity to cut dividends on the industry-date-level are not driving these results. Specification (2) shows results are robust to including firm fixed effects.

Thus, consistent with the distracted shareholder hypothesis, firms are more likely to cut dividends when their shareholders are distracted.

3.5.3. CEO Turnover

We next look at forced CEO turnover. We conjecture that, if shareholders are distracted, CEOs may be less likely to be fired after bad performance.

To implement this test, we follow Fisman, Khurana, Rhodes-Kropf, and Yim (2013) and classify a turnover as forced, if (i) it is not due to death of the CEO, (ii) the CEO is less than 60 years of age, and (iii) if the CEO is not subsequently reported in Execucomp as the CEO of another firm. Using this approach, we can identify 650 forced CEO turnovers in our data. The performance measure we use is return on assets (RoA). RoA is defined as net income divided by lagged total assets and we use in our tests the average RoA over turnover year and the year before the turnover year. Distraction is calculated as the average quarterly distraction during the company's fiscal year.

Table 3.12 presents results. Specifications (1) and (2) are OLS regressions which, as usual, include industry × date fixed effects. As expected, we find that underperforming CEOs are less likely to be fired when their shareholders are distracted. Our variable of interest is the interaction term between shareholder distraction and firm performance. We find a positive and significant coefficient on this interaction term, which indicates that forced CEO turnover is less sensitive to prior performance when shareholders are distracted. The impact of distraction is economically meaningful. For example, based on specification (2), a one-standard-deviation increase in distraction reduces the performance-sensitivity of CEO turnover by about 28% (= 0.839 × 0.05 / –0.148).

While these results are strongly supportive of the Distracted Shareholder Hypothesis, there is one caveat: specification (3) shows that we lose statistical significance when we include firm fixed effects. Hence, even though it is not obvious what that variable should be, we cannot rule out that a time-invariant unobserved variable on the firm-level is inducing those results. That said, the point estimate on the interaction remains very similar. In addition, while the performance variable keeps its expected sign and has a largely unchanged point estimate, it, too, loses statistical significance. The lower t-statistics in specification (3) may therefore reflect lower power of the firm fixed effects test, rather than the influence of an unobserved variable.

In sum, we believe the results in Table 3.12 provide additional support for the hypothesis that shareholder distraction is associated with time-variation in monitoring constraints.

3.5.4. Stock Returns

We have already shown in Table 3.6 that shareholder distraction has long-term effects on stock returns in firms that announce a takeover. In this section we examine whether shareholder distraction has an effect on stock returns also in firms not involved in a takeover. This test is informative, because many value-destroying actions self-interested managers can take are unobservable to the econometrician. Stock returns can act as a summary measure of the economic impact of these actions.

We implement a long-short calendar-time approach as follows. At the end of each month, we sort all stocks in CRSP into a low distraction portfolio (below median distraction measure in a given industry and month) and a high distraction portfolio (above median). We then obtain three time-series of monthly returns by holding the high distraction portfolio, the low distraction portfolio, and the portfolio long in high distraction stocks and short in low distraction stocks, respectively, for one month. Returns to all three portfolios are risk-adjusted using the Fama-French-Carhart 4-factor model. We capture the potential long-term impact of decisions taken when shareholders are distracted, by using distraction measures computed as a moving average over various horizons from 2 to 12 quarters prior to and including the current quarter. To focus on managerial responses to shareholder distraction other than M&A we exclude all firms which engaged in an M&A transaction as a buyer in the period over which we measure distraction.

Table 3.13, Panel A, presents results. The central result across all specifications is that we find significant underperformance in firms with distracted shareholders, but no abnormal performance for the non-distracted firms. Thus, the results in Table 3.13 provide strong support for the distracted shareholder hypothesis. In terms of magnitudes, we find that high distraction firms underperform their low distraction peers in the same industry and date by 13 to 16 bps per month, depending on the period used to compute distraction. This suggests that managers engage in value-reducing actions, beyond M&A, on an economically significant scale when their shareholders are distracted.

One potential concern with Panel A may be that high distraction firms are simply different on some unobserved dimension, which might induce stable difference in returns. Following our approach for takeovers and CEO lucky grants, we propose to look at lags of the distraction measure to address this concern. The motivation is that, if differences in distraction reflect stable differences in potentially unobservable firm characteristics, we should see that past distraction measures predict returns just as well as current ones. Panel B shows this is not the case. The difference between the high and low distraction groups is most pronounced for distraction computed starting with the current month, and decreases almost monotonically when we lag the distraction measure. That the initial lags are still useful to some degree as sorting variables is expected if we believe distraction has longer-term effects on stock returns. But, importantly, the last two columns show that distraction lags of two years or more do not predict excess returns. The return difference we observed in Panel A is therefore not due to time-invariant differences between firms with and without distracted shareholders.

In sum, the results in Table 3.13 suggest the distracted shareholder hypothesis applies to corporate actions beyond acquisitions, and that the associated reduction in shareholder value is on an economically meaningful scale.

3.6. Conclusion

This paper advances and tests a new hypothesis on the link between limited shareholder attention and corporate actions. The *Distracted Shareholder Hypothesis* holds that monitoring intensity faced by corporate managers is time-varying because institutional investors with limited attention may temporarily, and potentially rationally, shift attention to other segments of their portfolio. We construct a firm-level proxy for shareholder distraction, by identifying times when institutional investors experience shocks in unrelated parts of their portfolios.

We find strong evidence suggesting that managers can exploit shareholder distraction by engaging in privately optimal corporate actions. Specifically, investor distraction has economically important effects on the likelihood of announcing a merger, on merger performance, CEO pay, dividend cuts, CEO turnover, and stock returns. Our results suggest that understanding managerial responses to temporally relaxed monitoring constraints may significantly improve our understanding of value-creation in firms.

Tables

Table 3.1: Summary statistics

The table presents summary statistics for the main sample comprising all non-microcap stocks with a non-missing quarterly distraction measure over the period 1980-2010. Distraction is the weighted average exposure of firm shareholders to the shock industries. Institutional ownership is the fraction of the firm's stock owned by institutional investors. Top 5 share is the share of institutional ownership controlled by the five largest investors. Institutional holdings are measured at the quarter-end prior to the acquisition or dividend announcement. A complete list of definitions of our dependent and control variables is provided in the Appendix.

	N	Mean	Std. Dev.	0.25	Median	0.75
Dependent variables						
Merger (in %)	251,449	1.24	11.05	0.00	0.00	0.00
Diversif. merger (in %)	251,449	0.30	5.50	0.00	0.00	0.00
Within-industry merger (in %)	251,449	0.94	9.65	0.00	0.00	0.00
Dividend cut	251,449	0.24	4.87	0.00	0.00	0.00
Key independent variables						
Distraction	251,449	0.16	0.07	0.11	0.15	0.21
Control variables						
Institutional ownership (IO)	245,389	0.43	0.28	0.20	0.43	0.65
Top 5 share of IO	251,449	0.53	0.21	0.37	0.48	0.67
Size ($m)	240,687	1,294.64	6.02	360.72	1,165.76	3,915.76
Tobin's Q	236,852	1.92	1.57	1.08	1.37	2.07
Cash flow	221,729	0.10	0.12	0.04	0.09	0.15
Cash holdings	235,484	0.32	0.23	0.14	0.27	0.46

Table 3.2: Measuring distraction

The table relates shareholder distraction to conference call participation and shareholder proposals (Panel A) and investor trading (Panel B). In Panel A, columns (1) and (2), the dependent variable is the number of active conference call participants (i.e. the number of people who speak and are not company executives), normalized by the average number of active call participants during the prior four quarters. In columns (3) and (4) ((5) and (6)), the dependent variable is the logarithm of one plus the number of proposals by institutional (other) shareholders, respectively. Control variables include the lags of institutional ownership, the Top 5 share, log of total assets, Tobin's Q, cash flow, and cash holdings, and are not reported for brevity. In columns (1) and (2), distraction is measured during the quarter of the conference call and industry × quarter fixed effects are included in the regression. In columns (3) to (6), distraction is measured over the four calendar quarters preceding the quarter of the shareholder meeting and industry × year fixed effects are included. Reported t-statistics are robust to clustering by firm. In Panel B, columns (1) to (3), the dependent variable is the absolute value of the quarterly change in the weight of a firm's stock in an investor's portfolio. In column (4), the dependent variable is the absolute value of the quarterly change in the fraction of the firm's stock owned by the investor. Distracted is a dummy variable equal to one for institutional investors with above median exposure to the shock industries (as in equation (1) but without summing across investors) in the industry and quarter, and zero otherwise. Control variables included, but not shown are: lagged fraction of shares in a firm held by a given investor, lagged weight of the stock in the portfolio, lagged investor size (log of total assets), current and lagged stock return, current and lagged turnover, turnover in the same quarter one year ago, the firm's book-to-market ratio, and the number of days between the announcement date and quarter end. In column (5), the dependent variable is the churn rate of the investor's industry portfolio in a given quarter, computed at the industry-quarter level following Yan and Zhang (2009). Control variables include lagged investor size and investor flows. Reported t-statistics are robust to clustering at the investor × date level.

Panel A: Conference call participation and shareholder proposals

			Proposals by...			
	Call Participants		Institutions		Others	
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction	-1.799	-2.671	-0.520	-0.834	-0.175	-0.017
	(-1.90)	(-2.55)	(-2.05)	(-2.23)	(-0.57)	(-0.03)
Controls suppressed						
Industry × date FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes
N	8,353	8,353	3,047	3,047	3,047	3,047
R^2	0.05	0.17	0.41	0.65	0.54	0.76

Panel B: Investor trading

	Absolute change in PF weight			Abs. chg. ownership	Churn rate
	(1)	(2)	(3)	(4)	(5)
Distracted t	-0.009	-0.009	-0.003	-0.004	-0.389
	(-6.45)	(-6.85)	(-3.32)	(-4.25)	(-7.07)
Controls suppressed					
Industry × quarter FE	Yes	No	No	No	Yes
Stock × quarter FE	No	Yes	Yes	Yes	No
Investor FE	No	No	Yes	Yes	No
N	38,432,994	38,432,994	38,432,994	35,674,570	1,455,887
R^2	0.34	0.33	0.34	0.05	0.02

Table 3.3: Distraction and merger announcement frequency

The table reports results from a linear probability model which regresses an indicator for announcing an acquisition on our measure of shareholder distraction. The dependent variable is equal to one if the firm announces at least one merger bid in a given quarter, and zero otherwise. Diversifying deals are identified based on Fama-French-12 industries. For reasons of easier comparison, in columns (2), (3), (5), and (6), coefficients are divided by the probability of observing a diversifying and a within-industry merger conditional on observing a merger announcement, i.e., by 24% and 76%, respectively. In Panel A, distraction is measured during the quarter of the merger announcement. In Panel B, distraction is measured over the three quarters including and preceding the announcement quarter. Reported t-statistics are robust to clustering by firm.

Panel A: Distraction measured over one quarter

	Merger	Diversifying merger	Within-industry merger	Merger	Diversifying merger	Within-industry merger
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction t	0.052	0.116	0.033	0.044	0.070	0.027
	(3.75)	(3.83)	(2.10)	(3.13)	(2.32)	(2.27)
IO	0.006	0.008	0.005	0.004	0.012	0.003
	(3.88)	(3.10)	(2.98)	(1.61)	(1.83)	(1.00)
Top 5 share	-0.025	-0.029	-0.024	-0.014	-0.029	-0.009
	(-11.97)	(-6.70)	(-9.79)	(-5.27)	(-5.19)	(-3.11)
Log size	0.004	0.004	0.004	0.000	0.000	0.000
	(11.13)	(7.04)	(9.46)	(0.03)	(0.14)	(0.15)
Tobin's Q	0.000	0.000	0.000	0.001	0.000	0.001
	(0.96)	(2.94)	(-0.21)	(2.28)	(0.23)	(2.66)
Cash flow	0.006	-0.004	0.009	0.021	0.021	0.021
	(2.16)	(-1.08)	(2.96)	(4.94)	(2.14)	(4.59)
Cash holdings	0.019	0.000	0.025	0.014	0.021	0.013
	(8.63)	(0.03)	(9.14)	(3.38)	(2.25)	(2.81)
Industry × quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	Yes	Yes	Yes
N	208,761	208,761	208,761	208,761	208,761	208,761
R^2	0.02	0.01	0.02	0.07	0.06	0.07

Panel B: Distraction measured over three quarters

	Merger	Diversifying merger	Within-industry merger	Merger	Diversifying merger	Within-industry merger
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction MA(-2,0)	0.096	0.240	0.050	0.103	0.194	0.075
	(4.70)	(5.37)	(2.31)	(4.58)	(3.77)	(3.22)
Controls suppressed						
Industry × quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	Yes	Yes	Yes
N	208,761	208,761	208,761	208,761	208,761	208,761
R^2	0.02	0.01	0.02	0.07	0.06	0.07

Table 3.4: Robustness

This table presents robustness checks. The baseline regression refers to column (2) from Table 3.3, Panel A. For brevity we only report coefficients of interest and suppress control variables. In Panel A, we use alternative definitions of industry shocks. Trading volume defines the shock industry to be the one with the highest trading volume normalized by the average trading volume during the previous four calendar quarters, as in Barber and Odean (2008). The news-based distraction measure assigns a shock to the industry with the highest abnormal increase in news articles, which we define as the log difference of the number of news articles reported in Factiva in a given quarter, normalized by the median number of news articles during the previous four calendar quarters, following Da, Engelberg, and Gao (2011). Extreme negative (positive) returns only considers the industry with the lowest (highest) quarterly return as a shock industry. The time-series definition of shocks defines extreme industry-return-quarters as quarters where the industry return is in the bottom or top decile of the distribution of quarterly industry returns over the previous 40 quarters. In the following rows, distraction is measured using only the top 5 (10, 20) shareholders of the firm ranked by the size of their ownership stake. In Panel B, the baseline regression is rerun with additional controls. Relatedness to shock industry is defined as the % of firms which operate in the shock industries out of the total sample of closely related firms, where the latter are defined as in Hoberg and Phillips (2010). ILTI ownership refers to the share of institutional ownership by independent and long-term institutions, as defined in Chen, Harford, and Li (2007). Non-transient ownership controls for the ownership by dedicated and quasi-indexing investors as defined in Bushee (2001). Investor size refers to the aggregate size of the institutional investor's equity holdings. Investor concentration is measured using the Herfindahl index. Investor churn rates are computed following Gaspar, Massa, and Matos (2005). Investor flows are calculated as the difference between the total value of the investor's stock holdings at the end of the quarter and at the beginning of the quarter, adjusted for the price appreciation of the stocks in the portfolio. Institutional price pressure is defined following Coval and Stafford (2007) as the difference between mutual fund flow-induced purchases and flow-induced sales in a given stock and quarter, divided by the average trading volume of the stock from prior quarters. In Panel C, we restrict the mergers (i) to takeovers where the target is not in a negative (positive) shock industry, (ii) to deals which eventually get completed, and (iii) to non-serial acquirers. In Panel D, we estimate the baseline regression using a conditional Logit model.

	OLS		FE		
	Coeff.	*t*-stat	Coeff.	*t*-stat	N
Baseline	0.116	3.89	0.074	2.40	208,755
Panel A: Alternative measures of distraction					
Trading volume	0.116	2.97	0.054	1.39	225,044
News	0.074	1.70	0.004	0.12	224,442
Extreme negative returns	0.112	2.94	0.066	1.75	227,277
Extreme positive returns	0.079	2.05	0.037	0.87	229,061
Time-series returns	0.095	2.15	0.029	0.62	200,926
Only Top 5 investors	0.041	2.57	0.029	1.88	206,814
Only Top 10 investors	0.054	2.75	0.037	1.77	206,814
Only Top 20 investors	0.079	3.61	0.054	2.20	206,814
Only Top 5 investors MA(-2,0)	0.083	3.68	0.091	3.39	208,667
Only Top 10 investors MA(-2,0)	0.091	3.11	0.099	2.82	208,667
Only Top 20 investors MA(-2,0)	0.120	3.72	0.132	3.19	208,667
Panel B: Additional controls					
Relatedness to shock industry	0.136	2.81	0.087	1.64	155,071
Relatedness to shock industry (Distraction MA(-2,0))	0.393	4.82	0.302	3.16	155,071
ILTI ownership	0.116	3.86	0.070	2.31	208,761
Non-transient ownership	0.116	3.87	0.074	2.35	207,460
Avg. investor size	0.141	4.22	0.091	2.59	202,776
Avg. investor concentration	0.136	4.15	0.091	2.60	202,854
Avg. investor churn rate	0.116	3.85	0.074	2.33	208,761
Avg. investor flow	0.116	3.83	0.070	2.32	208,750
Inst. price pressure	0.141	2.80	0.066	1.31	140,867
Inst. price pressure (Distraction MA(-2,0))	0.343	4.39	0.289	3.22	140,867
All at once	0.141	2.17	0.074	1.13	116,337
All at once (excl. relatedness and pressure)	0.141	4.24	0.091	2.61	201,758
All at once (Distraction MA(-2,0))	0.463	4.14	0.376	2.84	116,337
Panel C: Sample restrictions					
Exclude if target is in negative shock industry	0.087	3.11	0.045	1.55	208,761
Exclude if target is in positive shock industry	0.087	3.17	0.054	1.83	208,761
Include only completed deals	0.091	3.50	0.054	1.95	208,761
Exclude serial acquirers (>10 deals)	0.107	3.70	0.066	2.17	208,681
Exclude serial acquirers (>5 deals)	0.112	4.02	0.070	2.38	207,435
Panel D: Estimation method					
Conditional Logit	9.225	4.87	n.a.	n.a.	90,049

Table 3.5: Merger performance

The table presents results from regressions of acquirer announcement returns and synergies on shareholder distraction. In Panel A, distraction is measured during the merger announcement quarter. In Panel B, distraction is measured during the three calendar quarters including and preceding the merger announcement quarter. Cumulative abnormal announcement returns (CARs) are calculated using the Fama-French (1993) model estimated over trading days (-280,-31) and are measured over a (-1,+1) event window. Synergies are defined following Bradley, Desai, and Kim (1988) as the weighted sum (by market capitalisation) of the bidder and target cumulative abnormal announcement returns. All regressions are estimated using Weighted Least Squares where weights are equal to the inverse of the estimation variance of the abnormal returns. Acquirer and target industries are defined based on the 12 Fama-French industries. Acquirer controls include institutional ownership, the Top 5 share, return on assets, book-to-market ratio, and log market capitalization. Deal controls consist of relative deal size and a list of dummy variables indicating whether the deal is a cash deal, a stock deal, a tender offer, hostile, a diversifying merger, or competed. Target controls are return on assets, book-to-market ratio, log market capitalization, a new economy dummy, and the log number of deals announced in the same year and target industry. All dependent and control variables are defined in the Appendix. Reported t-statistics are robust to clustering by announcement month.

Panel A: Distraction measured over one quarter

	Acquirer CAR(-1,+1)		Synergies(-1,+1)	
	(1)	(2)	(3)	(4)
Distraction t	-0.052	-0.062	-0.035	-0.033
	(-3.00)	(-3.38)	(-1.87)	(-1.73)
Acquirer and deal controls	Yes	Yes	Yes	Yes
Target controls	No	Yes	No	Yes
Acquirer industry × year FE	Yes	Yes	Yes	Yes
Target industry dummies	No	Yes	No	Yes
N	2,663	2,263	2,296	2,207
R^2	0.17	0.22	0.27	0.29

Panel B: Distraction measured over three quarters

	Acquirer CAR(-1,+1)		Synergies(-1,+1)	
	(1)	(2)	(3)	(4)
Distraction MA(-2,0)	-0.066	-0.082	-0.096	-0.108
	(-1.68)	(-1.88)	(-2.29)	(-2.55)
Acquirer and deal controls	Yes	Yes	Yes	Yes
Target controls	No	Yes	No	Yes
Acquirer industry × year FE	Yes	Yes	Yes	Yes
Target industry dummies	No	Yes	No	Yes
N	2,663	2,263	2,296	2,207
R^2	0.17	0.22	0.27	0.29

Table 3.6: Calendar-time portfolios

The table reports results from the calendar-time portfolio approach. At the end of each calendar month, we form a long-short distraction portfolio using all firms that have announced an acquisition in the previous 6 (12, 18, 24, 36) months. The strategy purchases high distraction stocks and sells low distraction stocks, where high (low) distraction stocks are those with above (below) median distraction values in a given bidder industry and announcement year and distraction is measured during the three calendar quarters including and preceding the merger announcement quarter. Returns are equally weighted within the constituent portfolios and we require a minimum of three stocks in each portfolio. Panel A calculates the average monthly abnormal returns of this long-short strategy using the Fama-French (1993) 3-factor model. Panel B uses the Fama-French (1993) and Carhart (1998) 4-factor model.

Panel A: Fama-French 3-factor model

	6m	12m	18m	24m	36m
High distraction	-0.280	-0.210	-0.230	-0.270	-0.210
	(-1.59)	(-1.59)	(-2.10)	(-2.75)	(-2.43)
Low distraction	0.030	0.050	0.040	0.040	-0.020
	(0.19)	(0.47)	(0.35)	(0.38)	(-0.22)
High - Low	-0.310	-0.240	-0.230	-0.280	-0.170
	(-1.48)	(-1.73)	(-2.22)	(-2.90)	(-2.13)
N	355	371	377	383	383

Panel B: Fama-French and Carhart 4-factor model

	6m	12m	18m	24m	36m
High distraction	-0.250	-0.110	-0.120	-0.160	-0.100
	(-1.38)	(-0.84)	(-1.11)	(-1.67)	(-1.24)
Low distraction	0.020	0.140	0.140	0.160	0.100
	(0.14)	(1.19)	(1.33)	(1.67)	(1.14)
High - Low	-0.280	-0.240	-0.240	-0.300	-0.180
	(-1.33)	(-1.66)	(-2.20)	(-3.03)	(-2.28)
N	355	371	377	383	383

Table 3.7: Holdings changes around merger announcements

The table reports results from our analysis of holdings changes during the quarter of an M&A announcement. Large Decrease is a dummy variable equal to one if the percentage change in the fraction of the firm's stock held by the investor is in the bottom quintile of the full-sample distribution, and zero otherwise. Negative Change in Ownership is defined as the absolute percentage change in the fraction of the firm's stock held by the investor if that change is negative, and zero otherwise. Sell All is a dummy variable equal to one if the investor sells its entire stake in the firm, and zero otherwise. Distracted is a dummy variable equal to one for institutional investors with above median exposure to the shock industries (as in equation (1) but without summing across investors) in the industry and quarter, and zero otherwise. Bad Deal refers to M&A announcements with an abnormal announcement return in the lowest quintile of the distribution within a given announcement year. Our definition of dedicated investors, which are excluded in columns (2), (4), and (6), follows Bushee (2001). Control variables are the lagged fraction of shares in a firm held by a given investor, the lagged weight of the stock in the portfolio, lagged investor size (log of total assets), current and lagged stock returns for the stock, current and lagged turnover, turnover in the same quarter one year ago, the firm's book-to-market ratio, and the number of days between the announcement date and quarter end. Reported t-statistics are robust to clustering at the investor $\times$ date level.

	Large decrease		Neg. Δ ownership		Sell all	
	(1)	(2)	(3)	(4)	(5)	(6)
Distracted	0.152	0.197	0.101	0.121	0.060	0.082
	(1.06)	(1.28)	(1.12)	(1.24)	(0.99)	(1.27)
Bad deal	2.010	2.060	1.652	1.713	1.154	1.183
	(8.59)	(8.04)	(10.65)	(10.09)	(9.37)	(8.81)
Bad deal $\times$ Distracted	-0.604	-0.710	-0.474	-0.540	-0.325	-0.369
	(-2.25)	(-2.43)	(-2.70)	(-2.83)	(-2.42)	(-2.53)
Controls suppressed						
Industry $\times$ quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Investor FE	Yes	Yes	Yes	Yes	Yes	Yes
Exclude dedicated investors	No	Yes	No	Yes	No	Yes
N	543,344	459,445	543,344	459,445	543,344	459,445
R^2	0.09	0.09	0.11	0.11	0.12	0.12

Table 3.8: Mandatory shareholder vote – regression discontinuity

The table reports estimates of the discontinuity in shareholder distraction at the 20% of common stock issued cutoff that requires a mandatory shareholder vote, using a regression discontinuity (RD) design. The percentage of common stock issued to finance the transaction is calculated as the total deal value times the percentage financed through common stock as reported in SDC, divided by the market capitalization of the acquirer measured at the end of the last trading day prior to the announcement. The dependent variable is shareholder distraction. In Panel A, we report estimates of the discontinuity in shareholder distraction at the 20% cutoff for various bandwidths. Panel B reports RD estimates for alternative cutoff choices. All estimates are obtained using a fourth-order local polynomial non-parametric regression and a triangular kernel function following Calonico, Cattaneo, and Titiunik (2014b) and Calonico, Cattaneo, and Titiunik (2014a). Robust bias-corrected z-statistics are reported in parentheses.

Panel A: Cutoff at 20%

	Distraction				
Bandwidth	0.02	0.04	0.06	0.08	0.10
Jump at cutoff	-0.195	-0.183	-0.157	-0.136	-0.128
	(-3.47)	(-3.87)	(-3.98)	(-3.96)	(-4.08)
N	82	177	248	350	430
N to the left of cutoff	50	109	158	223	278
N to the right of cutoff	32	68	90	127	152

Panel B: Placebo Cutoffs

	Distraction						
Cutoff point	0.10	0.15	0.20	0.25	0.30	0.40	0.50
Jump at cutoff	0.005	-0.045	-0.128	-0.021	-0.026	0.079	0.001
	(0.15)	(-1.22)	(-4.08)	(-0.61)	(-0.48)	(1.30)	(0.01)
N	924	599	430	349	264	217	178
N to the left of cutoff	646	383	278	216	152	112	105
N to the right of cutoff	278	216	152	133	112	105	73

Table 3.9: Influence of CEO power

The table presents results from the baseline regressions in Table 3.3, Panel A, columns (2) and (5), while interacting distraction with measures of CEO power. CEO Pay Slice is the fraction of the aggregate compensation of the top five executives captured by the CEO. Board Dependence is an indicator function equal to one if less than 50% of directors are classified as independent in RiskMetrics. CEO Tenure is the quartile of the CEO's tenure in the firm measured across all firms in a given quarter. Since information on CEO pay slice, board dependence, and CEO tenure is available only for a subset of our data, we estimate our models with an additional indicator, Missing Interaction Variable, equal to one if the information on the interaction variable is not available, as well as the interaction term of the missing interaction variable dummy and distraction. Reported t-statistics are robust to clustering at the firm level.

	Diversifying Merger					
	OLS			FE		
	CEO Pay Slice	Board Dependence	CEO Tenure	CEO Pay Slice	Board Dependence	CEO Tenure
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction t	0.058	0.103	0.095	0.012	0.058	0.045
	(1.28)	(2.91)	(2.11)	(0.28)	(1.57)	(1.03)
Distraction t × Interaction Var.	0.211	0.083	0.017	0.194	0.091	0.017
	(2.13)	(2.29)	(1.83)	(1.97)	(2.54)	(1.65)
Interaction Variable	-0.029	-0.012	-0.004	-0.033	-0.012	-0.004
	(-1.86)	(-1.82)	(-2.44)	(-2.04)	(-1.97)	(-2.40)
Distraction t × Missing Inter. Var.	0.265	0.091	0.033	0.252	0.103	0.041
	(1.96)	(1.98)	(0.90)	(1.88)	(2.18)	(1.08)
Missing Interaction Var.	-0.037	-0.008	-0.008	-0.045	-0.008	-0.008
	(-1.67)	(-0.92)	(-1.14)	(-1.88)	(-1.01)	(-1.23)
Controls suppressed						
CEO age dummies	No	No	Yes	No	No	Yes
Industry × quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	Yes	Yes	Yes
N	208,761	208,761	208,761	208,761	208,761	208,761
R^2	0.01	0.01	0.01	0.06	0.06	0.06

Table 3.10: Investor distraction and lucky equity grants

The table reports results from a linear probability model that regresses lucky grants on the average investor distraction during the firm's fiscal year. CEO (Director) lucky grant is a dummy variable equal to one if the CEO (a director) received an option grant on a date where the lowest price of the month prevailed and zero otherwise as in Bebchuk, Grinstein, and Peyer (2010). All dependent and control variables are defined in the Appendix. Reported t-statistics are robust to clustering at the firm level.

	CEO lucky grant			Director lucky grant		
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction t	1.042	0.967	1.024	0.481	0.636	0.634
	(2.99)	(2.31)	(2.35)	(1.68)	(1.77)	(1.72)
IO	-0.030	0.054	0.059	0.029	-0.052	-0.037
	(-0.72)	(0.52)	(0.55)	(0.83)	(-0.66)	(-0.47)
Top 5 share	-0.021	-0.105	-0.189	-0.062	-0.062	-0.110
	(-0.30)	(-0.99)	(-1.75)	(-1.20)	(-0.66)	(-1.05)
Relative size	-0.009	0.003	-0.009	-0.004	0.013	0.020
	(-1.52)	(0.14)	(-0.34)	(-0.85)	(0.68)	(0.94)
New economy	0.026	0.233	0.226	0.030	-0.032	-0.027
	(0.84)	(2.25)	(1.96)	(1.14)	(-0.32)	(-0.28)
CEO outsider	0.004	-0.045	-0.035	0.014	0.007	0.000
	(0.20)	(-0.95)	(-0.71)	(0.88)	(0.21)	(0.00)
Log CEO tenure	0.012	-0.030	-0.026	-0.005	-0.023	-0.024
	(1.42)	(-1.67)	(-1.47)	(-0.63)	(-1.48)	(-1.54)
CEO ownership $> 5\%$ and $< 25\%$	0.042	0.060	0.014	0.033	0.022	0.024
	(1.60)	(0.75)	(0.17)	(1.46)	(0.42)	(0.43)
CEO ownership $> 25\%$	0.073	0.212	0.156	-0.007	0.009	0.038
	(1.11)	(1.59)	(1.10)	(-0.18)	(0.13)	(0.51)
Tobin's Q			0.009			-0.016
			(0.81)			(-1.65)
Leverage			0.211			-0.106
			(1.93)			(-1.08)
Asset tangibility			-0.081			0.151
			(-0.60)			(1.43)
Log size			0.010			-0.006
			(0.31)			(-0.22)
Log firm age			-0.075			-0.049
			(-1.84)			(-1.32)
Industry $\times$ year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	Yes	No	Yes	Yes
N	2,507	2,231	2,121	3,049	2,789	2,645
R^2	0.06	0.36	0.35	0.04	0.34	0.35

Table 3.11: Investor distraction and dividend cuts

The table reports results from a regression of dividend cuts on investor distraction. Dividend cut is an indicator variable equal to one if the dividend is smaller than the dividend amount declared in the previous quarter, and zero otherwise. The data consist of all quarterly dividend announcements made by companies listed on the NYSE, AMEX and NASDAQ stock exchanges between 1980 and 2010 after applying the standard filters by Grullon et al. (2002). All dependent and control variables are defined in the Appendix. Reported t-statistics are robust to clustering at the firm level.

	Dividend Cut	
	(1)	(2)
Distraction t	0.137	0.117
	(2.31)	(1.89)
IO	-0.008	0.007
	(-1.59)	(0.81)
Top 5 share	-0.005	-0.002
	(-1.01)	(-0.22)
Log size	0.000	-0.007
	(-0.12)	(-3.68)
Tobin's Q	0.010	0.014
	(8.13)	(8.64)
Cash flow	0.002	-0.003
	(0.13)	(-0.18)
Cash holdings	0.014	0.015
	(3.41)	(1.34)
Industry × quarter FE	Yes	Yes
Firm FE	No	Yes
N	97,244	97,244
R^2	0.03	0.07

Table 3.12: Investor distraction and forced CEO turnover

The table reports results from a linear probability model that regresses forced CEO turnover on shareholder distraction. The dependent variable is an indicator function equal to one if there is a forced CEO turnover in a given firm and year. We identify forced CEO turnovers following Fisman, Khurana, and Rhodes-Kropf (2014), who define CEO turnovers as forced if (i) they are not due to death, (ii) they occur at less than 60 years of age, and (iii) the CEO is not subsequently reported in Execucomp as the CEO of another firm. RoA is defined as net income divided by lagged total assets as in Bebchuk et al. (2010) and is measured over the two years preceding the turnover year. Distraction is calculated as the average quarterly distraction during the company's fiscal year. Control variables are the same as in Table 10, columns (3) and (6). Reported t-statistics are robust to clustering by firm.

	Forced CEO Turnover		
	(1)	(2)	(3)
Distraction t	-0.015	-0.025	-0.079
	(-0.20)	(-0.27)	(-0.77)
RoA	-0.143	-0.148	-0.123
	(-2.48)	(-2.00)	(-1.24)
Distraction $t \times$ RoA	0.631	0.839	0.583
	(2.10)	(2.17)	(1.19)
Controls	No	Yes	Yes
Industry $\times$ year FE	Yes	Yes	Yes
Firm FE	No	No	Yes
N	22,862	10,013	10,013
R^2	0.01	0.03	0.27

Table 3.13: Investor distraction and stock returns

The table reports the estimates of calendar-time portfolio regressions applied to the entire sample, excluding M&A bidders. Panel A forms portfolios based on moving averages of shareholder distraction. At the end of calendar month t, all CRSP stocks are sorted into portfolios based on a moving average of their investors' distraction with length 3 quarters (MA(-2,0)), 5 quarters (MA(-4,0)), etc. until 13 quarters (MA(-12,0)). The sorting is done within industry and date. Panel B forms portfolios based on lags of the 3–quarters moving average distraction (MA(-2,0)), where the sorting is done again within industry and date. The first specification corresponds to the first column of Panel A, where D MA(-2,0) is not lagged. We then consider lags at 2, 4, 6, 8, and 12 quarters. In both panels, portfolio performance is measured as the alpha (in monthly percentage points) from a Fama-French-Carhart 4-factor model, for stocks with high and low distraction, and for the High – Low portfolio.

Panel A: Moving average D

Moving average:	MA(-2,0)	MA(-4,0)	MA(-6,0)	MA(-8,0)	MA(-12,0)
High distraction	-0.153	-0.153	-0.149	-0.156	-0.138
	(-2.12)	(-2.12)	(-2.09)	(-2.16)	(-1.86)
Low distraction	-0.002	0.006	0.002	0.008	-0.011
	(-0.03)	(0.07)	(0.02)	(0.10)	(-0.13)
High – Low	-0.150	-0.159	-0.151	-0.164	-0.127
	(-3.12)	(-3.49)	(-3.52)	(-3.91)	(-2.94)
N	369	369	369	369	369

Panel B: Lags of D MA(-2,0)

Lags of D:	0	2	4	6	8	12
High distraction	-0.153	-0.104	-0.085	-0.039	-0.003	0.044
	(-2.12)	(-1.42)	(-1.25)	(-0.53)	(-0.04)	(0.57)
Low distraction	-0.002	-0.008	0.010	0.070	0.041	0.047
	(-0.03)	(-0.10)	(0.11)	(0.86)	(0.53)	(0.60)
High – Low	-0.150	-0.096	-0.094	-0.109	-0.044	-0.003
	(-3.12)	(-1.95)	(-2.03)	(-2.61)	(-1.04)	(-0.07)
N	369	363	357	351	345	333

Figures

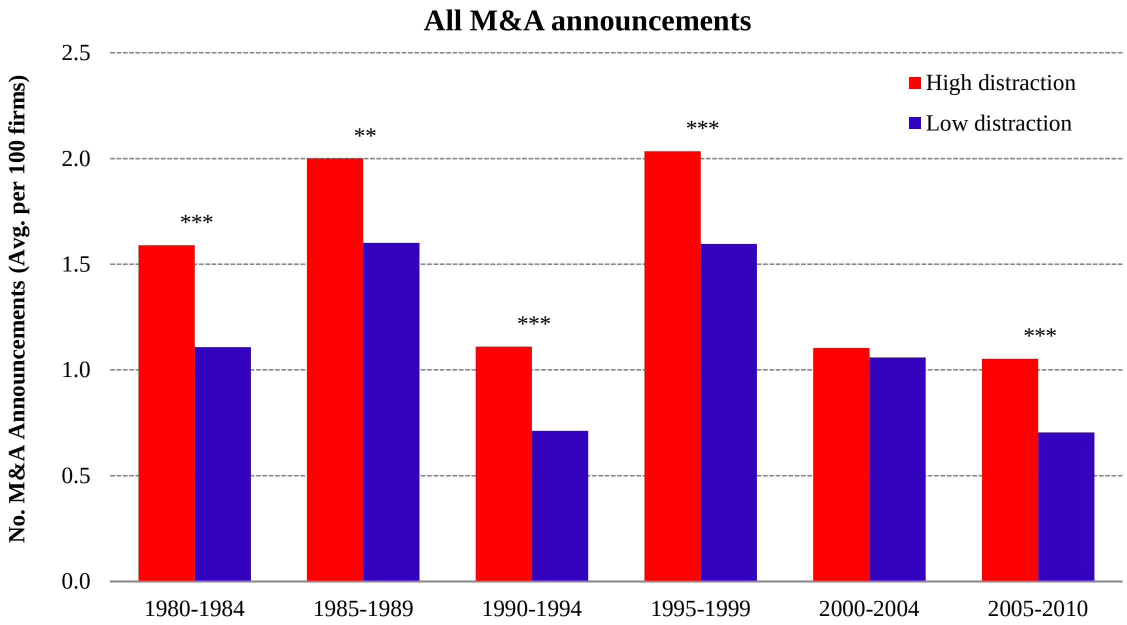
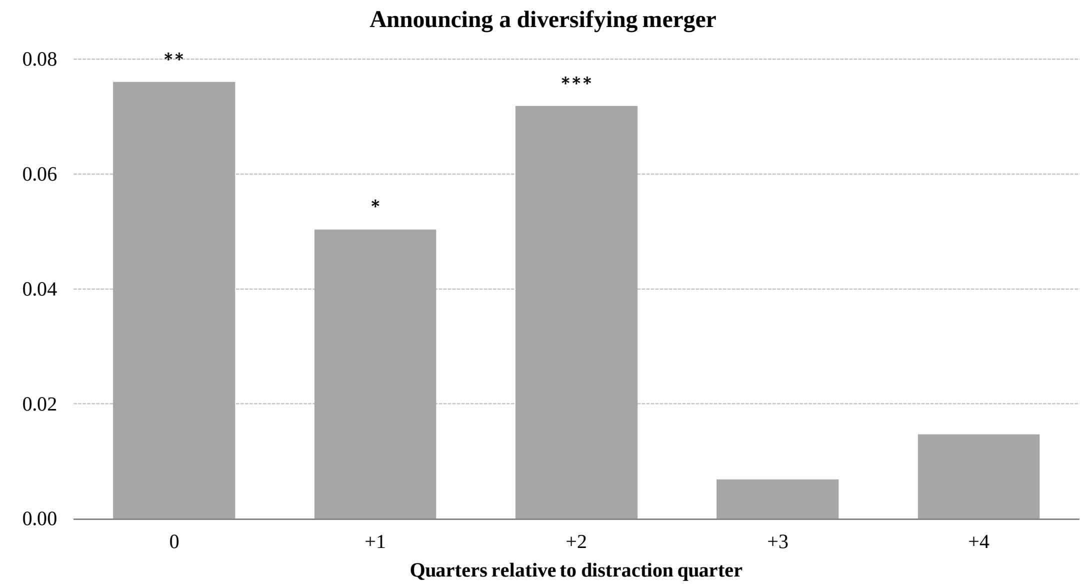


Figure 3.1: Merger frequency and distraction
The graph plots the average quarterly number of merger announcements for the subgroups of high and low distraction firms over time. High (low) distraction firms are defined as those with above (below) median shareholder distraction within a given industry and quarter. Asterisks ***, **, * indicate statistical significance of the difference between the high and low groups on the 1%, 5%, and 10% level and are based on standard errors that allow for clustering at the firm level.



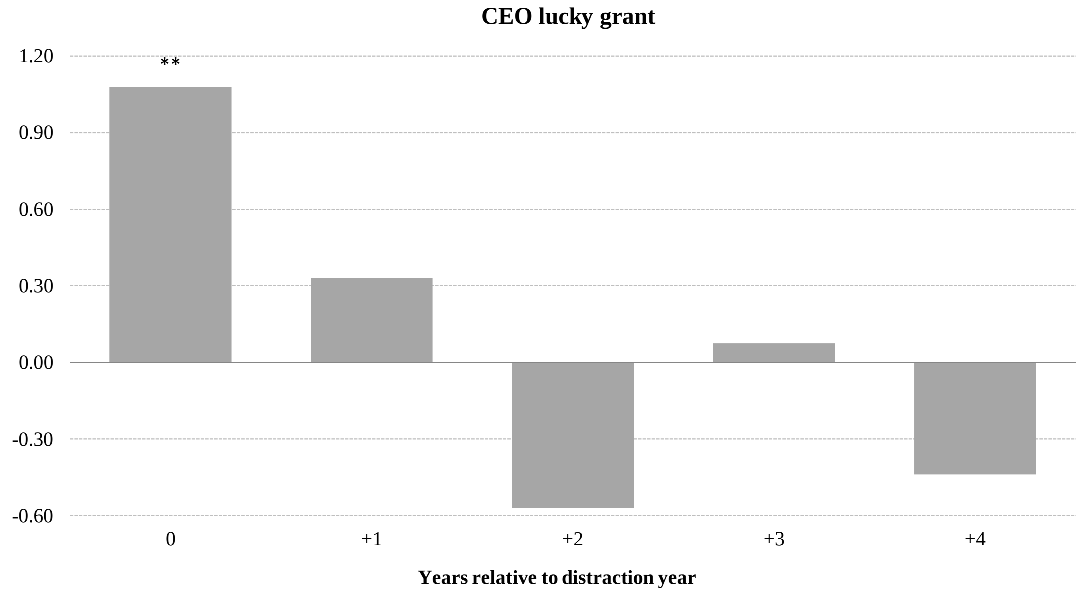


Figure 3.2: Timing of distraction
The graph plots the coefficient estimates from our baseline regressions reported in column (5) of Table 3.3 and in column (3) of Table 3.10 if we simultaneously include four lags of shareholder distraction. Asterisks ***, **, * indicate statistical significance on the 1%, 5%, and 10% level.

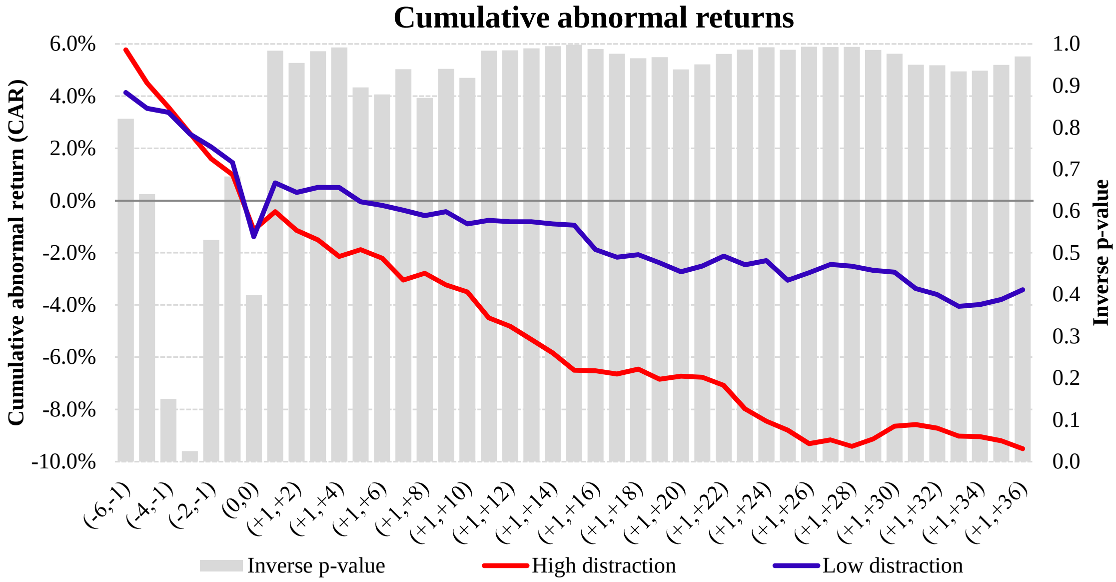
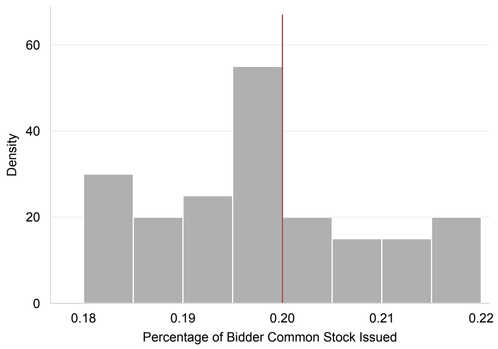
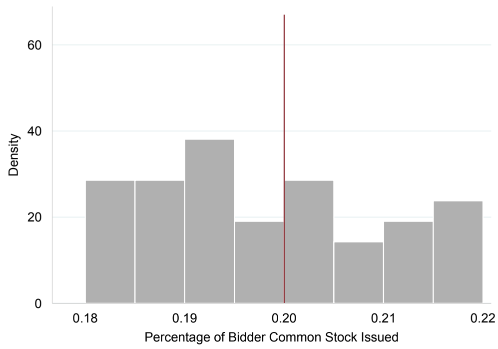


Figure 3.3: Stock performance around merger announcements (IRATS) The graph plots the long-run (cumulative) abnormal returns of bidder stocks for the subgroups of high and low distraction. High (Low) distraction stocks are those with above (below) median distraction values in a given bidder industry and announcement year, where distraction is measured during the 3 calendar quarters including and preceding the merger announcement. Abnormal returns are calculated using Ibbotson's (1975) returns across time and security (IRATS) method combined with the Fama-French (1993) three-factor model for different event time windows (event time 0 is the month of the merger announcement). The following regression is run for each subsample and each event month j: $(R_{it} - R_{ft}) = a_j + b_j(R_{mt} - R_{ft}) + c_j SMB_t + d_j HML_t + \varepsilon_{it}$ where R_{it} is the monthly return on security i in the calendar month t relative to event month j. R_{ft}, R_{mt}, SMB_t, and HML_t are the monthly risk-free rate, the monthly return on the value-weighted CRSP index, and the monthly return on the size, and book-to-market factor in the calendar month t corresponding to event month j, respectively. Cumulative abnormal returns (CAR) are sums of the intercepts of cross-sectional regressions over the relevant event-time periods. The secondary axis shows the inverse p-value (= (1 – p)) of the Chi-squared test for the hypothesis that CARs for the high and low distraction portfolios are equal.



(a) High distraction firms



(b) Low distraction firms

Figure 3.4: Distraction and distribution of equity payment around the 20% cutoff

The figure plots the histograms of the percentage of bidder stock issued to finance the transaction around the 20% cutoff that requires a mandatory shareholder vote, for low and high distraction firms. The percentage of common stock issued is calculated as the total deal value times the percentage financed through common stock as reported in SDC, divided by the market capitalization of the acquirer measured at the end of the last trading day prior to the announcement. High (low) distraction firms are defined as those with above (below) median shareholder distraction within a given acquirer industry and calendar year.

Appendix

Table A3.1: Variable descriptions

Variable	Description
Dependent variables	
Merger	Dummy variable equal to one if a firm announces an M&A transaction in a given calendar quarter and zero otherwise. We consider all majority-stake acquisitions recorded in SDC Platinum between 1980 and 2010 with a minimum deal value of $1 million.
Diversifying merger	Dummy variable equal to one if a firm announces a diversifying M&A transaction in a given calendar quarter and zero otherwise. An M&A deal is considered diversifying if the acquirer operates in a different FF12 industry than the target company.
Within-industry merger	Dummy variable equal to one if a firm announces a within-industry M&A transaction in a given calendar quarter and zero otherwise. An M&A deal is considered within-industry if the acquirer operates in the same FF12 industry as the target company.
Acquirer CAR(-1,+1)	Cumulative abnormal announcement returns of the acquirer are calculated using the Fama-French (1993) model estimated over trading days (-280,-31) and are measured over a (-1,+1) event window around the announcement date.
Synergies (-1,+1)	The weighted sum (by market capitalization) of the bidder and target cumulative abnormal announcement returns, following Bradley, Desai, and Kim (1988).
CEO (Director) luck	Dummy variable equal to one for firm-years where the CEO (a director) received an option grant on a date where the lowest price of the month prevailed and zero otherwise, as in Bebchuk, Grinstein, and Peyer (2010).
Dividend cut	Dummy variable equal to one if the quarterly cash dividend is smaller than the dividend amount declared in the previous quarter, and zero otherwise.
Forced CEO Turnover	Dummy variable equal to one for firm-years with a forced CEO turnover and zero otherwise. Forced CEO turnovers are identified following Fisman, Khurana, and Rhodes-Kropf (2014) as CEO departures that (i) are not due to death, (ii) occur at less than 60 years of age, and (iii) involve a CEO who is not subsequently reported in Execucomp as the CEO of another firm.
Control variables - all regressions	
Inst. ownership (IO)	Fraction of the firm's stock owned by institutional investors as reported in the Thomson Reuters 13f database, measured at the quarter-end prior to the event period.
Top 5 share of IO	Fraction of the firm's stock owned by the five largest institutional investors as reported in the Thomson Reuters 13f database, measured at the quarter-end prior to the event period.
Log size	Logarithm of total book assets as of the prior fiscal year end.
Tobin's Q	Ratio of the market to the book value of assets as of the prior fiscal year end.
Control variables - Merger frequency and dividend cuts	

Continued on next page

Table A3.1 – continued

Variable	Description
Cash flow	Earnings before extraordinary items plus depreciation, normalized by lagged total assets.
Cash holdings	Cash plus receivables, normalized by lagged total assets.
Control variables - M&A announcement returns	
Acquirer (Target) RoA	Net income over assets.
Acquirer (Target) B/M	Book value of equity divided by market capitalization.
Relative size	Total deal value divided by acquirer market capitalization.
Cash	Dummy variable equal to one if the deal is 100% cash financed and zero otherwise.
Stock	Dummy variable equal to one if the deal is 100% equity financed and zero otherwise.
Tender	Dummy variable equal to one if the deal is a tender offer and zero otherwise.
Hostile	Dummy variable equal to one if the deal is hostile and zero otherwise.
Diversifying	Dummy variable equal to one if the acquirer operates in a different FF12 industry than the target company and zero otherwise.
Competed	Dummy variable equal to one if a bid gets announced by a competing bidder and zero otherwise.
New economy	Dummy variable equal to one for target firms with SIC codes as defined in Murphy (2003), and zero otherwise.
Number of deals	The number of transactions announced in the target's FF12 industry in a given year.
Control variables - Lucky grants and forced CEO turnover	
Relative size	Logarithm of the ratio between the previous-year-end market capitalization of the firm and the median market capitalization of all firms in that year.
New economy	Dummy variable equal to one for firms with SIC codes as defined in Murphy (2003), and zero otherwise.
CEO outsider	Dummy variable equal to one if the CEO was not employed in the firm before becoming the CEO, and zero otherwise.
CEO tenure	Logarithm of one plus the number of years that the CEO served in the company.
CEO ownership $>$ 5% and $<$ 25%	Dummy variable equal to one if the CEO holds between 5-25% of the firm's stock, and zero otherwise.
CEO ownership $>$ 25%	Dummy variable equal to one if the CEO holds more than 25% of the firm's stock, and zero otherwise.
Leverage	Ratio of the book value of long-term debt over total assets.
Tangibility	Defined following Berger, Ofek, and Swary (1996) as $0.715 \times receivables + 0.547 \times inventory + 0.535 \times capital + cash$, normalized by total assets.
Firm age	Logarithm of one plus the number of years since the firm appears on CRSP.

Table A3.2: Summary statistics - all samples

The table presents summary statistics for different samples used in our analysis. Panel A provides descriptive statistics for the full sample comprising all non-microcap stocks with a non-missing quarterly distraction measure over the period 1980-2010. Panel B reports descriptive statistics for our merger sample, which consists of 3,239 majority-stake acquisitions with a minimum deal value of $1 million announced between 1980 and 2010. Descriptive statistics for the lucky grants sample, spanning years 1996-2005, are shown in Panel C. Panel D reports statistics for the sample of forced CEO turnovers. Distraction is the weighted average exposure of the firm's shareholders to the shock industries. A complete list of definitions of our dependent and control variables is provided in Table A3.1.

Panel A: Full sample (10,006 firms)

	N	Mean	Std. Dev.	0.25	Median	0.75
Dependent variables						
Merger (in %)	251,447	1.24	11.05	0.00	0.00	0.00
Diversif. merger (in %)	251,447	0.30	5.51	0.00	0.00	0.00
Within-industry merger (in %)	251,447	0.94	9.65	0.00	0.00	0.00
Dividend cut	106,224	3.83	19.19	0.00	0.00	0.00
Key independent variables						
Distraction	251,447	0.16	0.07	0.11	0.15	0.21
Control variables						
Institutional ownership (IO)	245,389	0.43	0.28	0.20	0.43	0.65
Top 5 share of IO	251,447	0.53	0.21	0.37	0.48	0.67
Log size	240,685	7.17	1.80	5.89	7.06	8.27
Tobin's Q	236,848	1.92	1.57	1.08	1.37	2.07
Cash flow	221,723	0.10	0.12	0.04	0.09	0.15
Cash holdings	235,479	0.32	0.23	0.14	0.27	0.46

Panel B: Merger sample (1,556 firms, 3,239 acquisitions)

	N	Mean	Std. Dev.	0.25	Median	0.75
Dependent variables						
Acquirer CAR(-1,+1)	3,014	-0.013	0.06	-0.04	-0.01	0.01
Synergies (-1,+1)	2,572	0.014	0.06	-0.02	0.01	0.04
Key independent variables						
Distraction	3,239	0.16	0.07	0.11	0.15	0.21
Control variables						
Institutional ownership (IO)	3,196	0.50	0.23	0.34	0.51	0.67
Top 5 share of IO	3,239	0.43	0.17	0.30	0.39	0.51
Acquirer RoA	3,232	0.04	0.08	0.01	0.04	0.08
Acquirer B/M	3,211	0.56	0.40	0.30	0.48	0.73
Acquirer mktcap ($m)	3,239	1,798.93	5.30	548.57	1,507.10	5,017.98
Relative size	3,239	0.41	0.69	0.05	0.16	0.48
Cash	3,239	0.26	0.44	0.00	0.00	1.00
Stock	3,239	0.33	0.47	0.00	0.00	1.00
Tender	3,239	0.20	0.40	0.00	0.00	0.00
Hostile	2,921	0.07	0.25	0.00	0.00	0.00
Conglomerate	3,237	0.24	0.43	0.00	0.00	0.00
Competed	3,239	0.08	0.27	0.00	0.00	0.00
Target RoA	2,790	0.00	0.14	0.00	0.02	0.06
Target B/M	2,759	0.68	0.50	0.36	0.57	0.87
Target mktcap ($m)	2,845	190.05	5.65	54.61	172.90	625.66
New economy	3,237	0.14	0.34	0.00	0.00	0.00
Number of deals	3,239	12.45	3.30	5.00	13.00	29.00

Panel C: Lucky grants sample (2,750 firms, 992 CEO lucky grants)

	N	Mean	Std. Dev.	0.25	Median	0.75
Dependent variables						
CEO luck	7,680	0.13	0.34	0.00	0.00	0.00
Director luck	9,351	0.10	0.30	0.00	0.00	0.00
Key independent variables						
Distraction	75,562	0.16	0.04	0.13	0.16	0.19
Control variables						
Institutional ownership (IO)	68,585	0.42	0.27	0.19	0.42	0.64
Top 5 share of IO	70,338	0.54	0.22	0.37	0.49	0.68
Relative size	75,562	2.20	1.32	1.16	1.89	2.98
New economy	75,562	0.12	0.32	0.00	0.00	0.00
CEO outsider	13,336	0.26	0.44	0.00	0.00	1.00
CEO tenure	13,496	13.20	2.41	8.00	16.00	27.00
CEO ownership > 5% and < 25%	25,085	0.10	0.30	0.00	0.00	0.00
CEO ownership > 25%	25,085	0.02	0.14	0.00	0.00	0.00
Tobin's Q	68,067	1.93	1.60	1.08	1.38	2.08
Leverage	68,636	0.19	0.18	0.03	0.15	0.30
Tangibility	65,561	0.50	0.17	0.42	0.52	0.59
Size ($m)	69,254	1,223.55	6.42	333.02	1,123.63	3,896.87
Firm age	59,721	11.29	2.99	6.00	13.00	25.00

Panel D: Forced CEO turnover sample (3,156 firms, 754 forced CEO turnovers)

	N	Mean	Std. Dev.	0.25	Median	0.75
Dependent variables						
Forced CEO turnover	26,043	0.02	0.16	0.00	0.00	0.00
Key independent variables						
Distraction	26,043	0.16	0.05	0.13	0.16	0.19
RoA	22,862	0.06	0.09	0.02	0.05	0.10
Control variables						
Institutional ownership (IO)	22,105	0.64	0.20	0.50	0.66	0.79
Top 5 share of IO	23,101	0.40	0.12	0.32	0.38	0.46
Relative size	26,043	0.52	1.34	-0.51	0.31	1.36
New economy	26,043	0.12	0.33	0.00	0.00	0.00
CEO outsider	13,336	0.26	0.44	0.00	0.00	1.00
CEO tenure	22,346	5.48	2.44	3.00	6.00	10.00
CEO ownership $>$ 5% and $<$ 25%	25,084	0.10	0.30	0.00	0.00	0.00
CEO ownership $>$ 25%	25,084	0.02	0.14	0.00	0.00	0.00
Tobin's Q	22,804	2.00	1.46	1.16	1.51	2.22
Leverage	23,017	0.19	0.16	0.04	0.16	0.29
Tangibility	22,614	0.46	0.16	0.37	0.48	0.55
Size ($m)	23,131	2,475.99	5.29	744.84	2,083.19	7,169.00
Firm age	25,565	17.49	2.45	10.00	19.00	34.00

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