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Three Essays in Financial Economics

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Three Essays in Financial Economics

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Abstract

Three Essays in Financial Economics

By Kiseo Chung

This dissertation examines the role of manager's incentive and behavioral bias affecting their managerial decisions. The first essay (Changing Career Incentives and Risk-Taking in the Mutual Fund Industry) examines the career incentives of mutual fund managers and its relation to their risk taking behavior. In this chapter, I find significant changes in career incentives for mutual fund managers in recent years and corresponding shifts in managers' risk taking behavior. Successful funds receive less inflows in recent years, and poor performing funds are more likely to receive outflows. The termination decision has also become more sensitive to recent performance. Managers respond rationally to the changes in the career incentives by taking less risk. The increased performance scrutiny has fallen disproportionately on experienced managers. As a result, the Chevalier and Ellison (1999) finding that inexperienced managers take less risk than experienced managers is overturned in the more recent period, consistent with commensurate shifts in their career incentives. The second essay (CEO Home Bias and Corporate Acquisitions), coauthored with Clifton Green and Breno Schmidt, investigate the effect of CEO's home bias on firm's investment decisions. In this chapter, we find that CEOs are significantly more likely to purchase targets near their birth place, consistent with either informational advantages or familiarity bias. Evidence from bidder announcement returns supports the latter view. Acquirer returns are significantly lower for CEO home bias acquisitions, and the relation is robust to controls for firm and industry characteristics. The negative announcement effect is stronger when the target is located further away, among poorly-governed firms, and when the CEO has a deeper birth place connection. CEOs' post-acquisition trading behavior also supports a familiarity bias interpretation. Our findings suggest that CEO home bias influences firm investment. The third essay (Off-style Holdings of Mutual Funds) examines whether mutual funds hold stocks that do not match their stated investment style on a regular basis, and explore the motivation behind such holdings. I find that funds hold a significant portion of their holdings in stocks that do not match their stated investment style (20%-35%) which is consistent with S.E.C. regulation 35d. The reason for holding "off-style" stocks could be because of information sharing between funds or "co-insurance" between funds. I find evidence that supports both.

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Changing Career Incentives and Risk-Taking in the Mutual Fund Industry

Kiseo Chung*†

Abstract

I find significant changes in career incentives for mutual fund managers in recent years and corresponding shifts in managers' risk taking behavior. Successful funds receive less inflows in recent years, and poor performing funds are more likely to receive outflows. The termination decision has also become more sensitive to recent performance. Managers respond rationally to the changes in the career incentives by taking less risk. The increased performance scrutiny has fallen disproportionately on experienced managers. As a result, the Chevalier and Ellison (1999) finding that inexperienced managers take less risk than experienced managers is overturned in the more recent period, consistent with commensurate shifts in their career incentives.

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1 Introduction

The mutual fund industry, which saw assets under management grow by more than 50-fold during the two decades following 1980, showed signs of maturing by the late 1990s. 93% of net inflows were captured by six leading fund companies, and nearly half of the remaining 648 companies experienced outflows (Whitford (1999)). New fund introductions hit a 10-year low (Wahal and Wang (2011)), and the number of households owning mutual funds exceeded those with at least $20,000 of wealth to invest (Gremillion (2005)). Index funds began to capture a greater fraction of market share as public awareness grew following a congressional subcommittee investigation into whether mutual fund fees were appropriate for the service provided.[1]

Furthermore, along with the SEC's goal in 2000 of increasing the role of independent directors to address conflicts of interest of fund managers (Roye (1999)), mutual fund clientèle began to shift away from retail towards institutional investors. These industry-wide changes negatively affected mutual fund managers' sense of job security. Termination probability increased from 10% in 1992 to 27% in 2013. The time when fund managers thought their tenure was "a divine right" was ending (Petruno (1995)). However, how these industry-wide changes affect fund managers' incentives in managing a fund, specifically the incentive to take more or less risk, and whether this effect varies by manager's career stages, has not been explored.

In this article, I examine whether changing industry dynamics have changed managers' career incentives, and I explore the effects of these incentives on managers' risk-taking behavior. Previous work has established an asymmetric performance-flow relation, with very successful funds receiving inflows while the remaining funds exhibit a weak performance-flow relation (e.g., Ippolito (1992); Sirri and Tufano (1998); Del Guercio and Tkac (2002);

[1] SEC Testimony: A. Levitt re Transparency in the U.S. Debt Market : https://www.sec.gov/news/ testimony/testarchive/1998/tsty1398.htm

Del Guercio and Reuter (2014)). Manager pay is tied to assets under management, and the convex performance-flow relation therefore creates incentives to take risk to reach the top group (e.g., Brown, Harlow, and Starks (1996); Chevalier and Ellison (1997); Huang et al. (2011)). On the other hand, risk exposes managers to termination if the risks are ill-advised (e.g., Khorana (1996); Chevalier and Ellison (1999); Kostovetsky and Warner (2015)). Thus, the combined effect of compensation incentives and career concerns influences manager risk taking (e.g., Kempf, Ruenzi, and Thiele (2009)).

I document significant shifts in managers' career incentives. Net inflows to top performing funds are smaller in the more recent period compared to the earlier period. For example, a one percentile increase in the annual performance rank for managers in the top quintile led to a 2.21% increase in Total Net Assets (TNA) during the Pre-2000 period, while the same change produces a 1.33% increase in TNA Post-2000. Moreover, unlike the Pre-2000 period, the bottom quintile of fund managers experiences statistically significant outflows in the Post-2000 period, with a one percentile rank decrease leading to a 0.39% decrease in TNA.

The sensitivity of fund manager termination to performance has also increased in recent periods. During the Pre-2000 period, the relation between a manager's most recent annual performance and manager termination is both economically and statistically weak. However, the Post-2000 relation is strong, with a 1% drop in annual Carhart 4-factor alpha increasing the probability of termination by 2.7%, which is economically meaningful relative to the average termination probability of 14% during this period. Moreover, the termination decision has also become sensitive to fund flows in the recent period, with a 1% decrease in net inflows increasing the probability of termination by 3.56%.

The combined effect of reduced inflows to successful funds and greater risk of termination for poorly performing funds reduces incentives for fund managers to take risk in the more recent period. Consistent with the importance of career incentives, I find that fund managers do reduce the average level of risk in their portfolios. For example, the tracking error measured relative to the 4-Factor model decreases by 30% on average during the more recent period compared to the earlier period.

I next explore whether changes in the mutual fund industry have affected junior and experienced fund managers differently. Theoretical studies offer conflicting predictions regarding how risk taking varies with experience. For example, Prendergast and Stole (1996) argue that inexperienced managers anti-herd in an attempt to signal they have good information, while experienced managers herd so as to not contradict their previous actions. On the other hand, Scharfstein and Stein (1990) and Avery and Chevalier (1999) predicts that experienced managers herd less as there is less uncertainty about their ability. Empirical evidence is also mixed. Graham (1999), Jegadeesh and Kim (2010), Greenwood and Nagel (2009), Boyson (2010), and Yim (2013) find evidence that less-experienced professionals take more risk, whereas Lamont (2002), Chevalier and Ellison (1999), and Hong, Kubik, and Solomon (2000) find evidence consistent with experienced professionals taking more risk. As a result, it is unclear how changes in career incentives and respective risk taking behavior documented above would differ for junior and seasoned managers.

I find that the decrease in convexity in the performance-flow relation over time is similar in magnitude for both experienced and inexperienced managers. Specifically, the increase in net inflows associated with a one percentile increase in performance rank drops from 2.68% (2.47%) in the earlier period to 1.32% (1.09%) for the recent period for inexperienced (experienced) managers, and the differences across groups are not statistically different in either time period. The evidence suggests the changes in the performance-flow relation apply equally to new and experienced managers.

On the other hand, the effect of changed industry dynamics has had a differential effect on the risk of termination. In the earlier period, the sensitivity of termination to performance is greater for inexperienced managers, which is consistent with previous literature (Chevalier and Ellison (1999)). However, while the sensitivity of termination to recent performance has not materially changed for inexperienced managers, it has increased substantially for experienced managers. As a result, the termination-performance sensitivity is greater for experienced managers in the recent period. Specifically, a 1% drop in 4-factor alpha increases the probability of termination by 3.3% for experienced managers compared with 0.56% for inexperienced managers.

The smaller risk of termination combined with a similar performance-flow relation provides inexperienced managers with greater incentives to take risks in the recent period relative to experienced managers. Consistent with the differing career incentives, I find strong and consistent results that inexperienced fund managers take significantly greater risks compared to their seasoned counterparts. The difference in Tracking Error for the average manager between the experienced and inexperienced groups is 0.43%. This is substantial, given that the average Tracking Error across all managers is 1.07% and the standard deviation is 0.56%. On the other hand, consistent with extant studies, I find inexperienced managers take less risk relative to experienced managers in the earlier period. The relative shift in risk taking behavior is consistent with commensurate shifts in fund manager career incentives.

Taken together, my analysis reveals significant shifts in the career incentives faced by mutual fund managers, with experienced managers in particular facing greater performance scrutiny than during in the earlier period of the 1990s. Moreover, I document corresponding shifts in manager risk-taking behavior that are consistent with the changes in the likelihood of termination and the performance flow relation. As a result, a prominent finding in Chevalier and Ellison (1999), that junior mutual fund managers take less risk than seasoned managers, reverses in more recent data, consistent with commensurate shifts in managers' career incentives.

My findings contribute to the literature on manager termination (e.g., Khorana (1996); Chevalier and Ellison (1999); Kostovetsky and Warner (2015)), the performance-flow relation (e.g., Ippolito (1992); Sirri and Tufano (1998); Del Guercio and Tkac (2002); Del Guercio and Reuter (2014)), mutual fund risk taking (e.g., Kempf, Ruenzi, and Thiele (2009);

Korniotis and Kumar (2011); Hu, Kale, Pagani, and Subramanian (2011)), and the effect of experience on decision making (e.g., Greenwood and Nagel (2009)). I document significant shifts in the career incentives faced by mutual fund managers. Compensation-based incentives to take risk, through the convex flow-performance relation, have weakened over time, and job security incentives to avoid risk have increased, through a greater sensitivity of termination to recent performance. I find evidence that managers have responded rationally to the reduction in risk stimulus and increase in risk deterrent by taking less risk.

The paper proceeds as follows. In Section 2, I describe the sample and construction of the variables. Section 3 discuss how the two career incentives for managers differ in the recent period when compared to the earlier period and how this change affects managers' risk taking. In Section 4, I examine how the change in career incentives and risk taking differs between experienced and inexperienced managers. Section 5 explores alternative explanations and robustness checks, and Section 6 concludes.

2 Data and Variable Construction

2.1 Sample Selection

My primary source of mutual fund data is Morningstar Direct. Morningstar Direct provides not only data on fund return and characteristics, but also short bios of fund managers who are in charge of each fund.[2] From each mutual fund's website inside Morningstar Direct, I extract each fund manager's specific information. This includes their educational background and their graduation year, prior work history, whether they hold financial certificates, and when they received these certificate. Unfortunately, less than 10% of the fund managers sampled have complete information, and some of the observations are in-

[2]Patel and Sarkissian (2015) find managerial structure accuracy is highest at 96% for Morningstar Direct when compared to Securities and Exchange Commission (S.E.C.) filings and recommend using Morningstar Direct data for mutual fund manager specific analysis.

correct/different across funds. For example, James L. Barber, a fund manager at Vanguard who started his career at Stanford Endowment and then moved to Alliance Bernstein, uses a nickname, Rocky. In the beginning of his career, he is listed as James L. Barber, but recent entries list him as Rocky Barber. Moreover, for some cases, Morningstar attaches a different person's information to the wrong person when these individuals have the same first and last name. Also, Morningstar includes middle names for some funds while excluding them for others as the database updates its records based on new information provided by the funds.

To alleviate these concerns but also to fill in missing information in Morningstar, I hand-collect information on fund managers through multiple sources. First, I collected data on each fund manager's birth year and month, previous addresses, and email addresses from the Lexis Nexis Online Public Records Database following the methodology proposed by Pool, Stoffman, and Yonker (2012). I searched the database starting with the fund managers' name as provided by Morningstar and manually matched with other information provided by Morningstar, as well as with the employment history and location of employment provided by the SEC via the Investment Adviser Public Disclosure (IAPD) website.[3] [4] With this process, I was able to collect public records for 5,993 fund managers out of 6,869 fund managers that have ever existed in the Morningstar Direct database, including exact birth year and month. This is by far the most extensive public records data on fund managers managing active U.S. equity mutual funds. As a result, without having to lean on the approximate age calculation method suggested by Chevalier and Ellison (1999), I am now able to better proxy fund manager experience with exact age and analyze the impact

[3]http://www.sec.gov/answers/iapd.htm provides registration and employment history of registered investment advisors, but does not provide the full history of employment. Dates are not perfect since dates in the system are the date each fund registered the manager and, on average, do not have information on fund managers who left the industry more than 10 years ago.

[4]When there are multiple people under the same name, I base my search on undergraduate graduation year, which is one of the better populated variables in Morningstar, and subtract 22 from that year to get a rough estimate of birth year. Then, I match their earlier addresses with the location of both undergraduate and graduate school locations. Lastly, I look for email addresses containing their current/past mutual fund.

of experience on different fund manager behaviors.

Second, I used Linkedin, EDGAR and each mutual fund's official website to supplement employment history and educational background. With this process, I was able to correct or add information on the year and school of graduation, whether the managers hold a financial certificate not reported in Morningstar, and the year they started working in the investment industry for a subset of fund managers. The precise year of career initiation is important as nearly 20% of fund managers did not start their career in the investment industry immediately after receiving their undergraduate degree, as is assumed in the previous literature. For example, Sandeep Bhatia, a fund manager of RidgeWorth, earned his Ph.D. in chemical engineering in 1993 and shifted his career to the investment industry when he earned his MBA in 2000. He would be categorized as one of the more experienced fund managers by the previous standards, but he is in fact a relatively inexperienced fund manager. Given the ambiguity in defining the investment industry and insufficient description of past occupations for some managers, the variable defining when a fund manager enters the investment industry is in part prone to subjective discretion.[5] Thus, I use this new variable in the robustness check section. When only the year of birth and the career beginning year are available, I assume that each person was born and started working in July since the margin of error is the smallest and most people graduate from school in May/June. One caveat of the Morningstar data is that it suffers from survivorship bias and a backfilling issue since it only includes fund managers who were still working in the industry as of 1992. To alleviate this concern, the sample starts from 1992 and runs until 2014.

Since the focus of this paper is the risk taking behavior of mutual fund managers, I focus on actively managed U.S. domestic open-end equity mutual funds.[6] Following Elton,

[5] I excluded work experience that does not have any a priori reason to expect that it is related to investment industry experience.

[6] I used a variety of investment category specification provided by Morningstar to eliminate non-equity funds. First, I only included funds with the Broad category group as equity. Then, I only included funds with

Gruber, and Blake (1996), I require funds to satisfy a certain lower bound of total net assets (TNA) to alleviate concerns regarding return outliers. I modify Elton, Gruber, and Blake's (1996) criteria and require average TNA to be at least $5 million, but at the same time require the maximum time-series TNA to be at least $15 million in order to include fund-quarter observations that meet the general criteria of $15 million but whose time-series average is deflated due to extremely small initial and/or closing quarter TNA. I then eliminate fund-quarter observations with TNA less than $10 million.[7] Also, I only include funds that have more than 2 fund managers throughout the history of each mutual fund, as most of the funds with fewer than 2 managers throughout were self-owned, extremely small size funds, or were missing observations which would bias my analysis. Moreover, I eliminated all fund manager-quarter observations if a fund manager managed the fund for a period of less than 180 days, as it is both not possible to precisely measure the risk taking of a manager who managed the fund for a short period of time, and also because it is difficult to see that such a manager could have much discretion in choosing the risk level of a fund. Last but not least, I eliminated fund manager-quarter observations if a single manager was managing more than 10 funds in a single cross-section, as these managers are mostly supervisors or directors of a mutual fund trust and thus it is difficult to assume that they would participate in the day-to-day operation of investment strategy.[8] If a fund has multiple share classes, I value-weighted across share classes. In the final sample, I have 300,004 fund-fund manager-quarter observations with 3,112 unique funds and 5,640 unique fund managers. My baseline assumption in choosing who is the lead manager of a

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a Morningstar Category of large value, large growth, large blend, midcap value, midcap growth, midcap blend, small value, small growth, or small blend. Third, I eliminated funds with the Morningstar institutional category of S&P 500 tracking, world large core, or materials. Fourth, I eliminated fixed income funds and commodities funds using the Broad category group and international municipal bonds fund using US broad asset class. Finally, I manually eliminated funds with names that include S&P, Russell, Index, Nasdaq, and Dow in order to exclude index funds.

[7]The result is almost identical whether I follow Elton, Gruber, and Blake (1996) and use the TNA cutoff to be the time-series average of $15 million or the method above.

[8]Using other numbers between 5 and 10 result in qualitatively similar results.

fund when there are more than two managers concurrently listed as managing a fund is to use each manager's experience. My main specification uses overall experience seniority, but I augment that with each fund's seniority and funds managed by a unique manager in the robustness check section.

2.2 Variable Construction

I use measures of active management (deviation from a benchmark) as proxies for differences in risk taking across managers. Even if active management measures are driven by private information and fund manager ability, the fact that these measures will differ from fund to fund based on how actively a fund manager takes the bet on private information provides evidence that these measures can also be interpreted as risk taking measures. One caveat with regard to these measures is that they require relatively long periods of time-series data, ranging from 12 to 36 months of return time-series, to precisely estimate how active each mutual fund/fund manager is. My main variable of active management is Tracking Error. In calculating the Tracking Error, I use two different factor models. The first is the Fama and French (1993) and Carhart (1997) 4 factor model. The second is a one-factor model using a combination of the primary prospectus objective index and the S&P 500 Total Return index as factor returns. For this measure, I fill in missing prospectus benchmark observations with the most common benchmark, S&P 500 Total Return index.[9] My data on the monthly 4 factor returns are from the Center for Research in Security Prices (CRSP).

In the robustness check section, I use alternative measures of risk taking. The first is Amihud and Goyenko's (2013) R^2. I regress the future twelve months of monthly fund excess return over the one month T-bill rate on Fama-French-Carhart 4 Factor return to get the

[9] In an unreported table, I also use the S&P 500 Total Return separately as an alternative benchmark because Sensoy (2009) states that "almost one-third of actively managed, diversified U.S. equity mutual funds specify a size and value/growth benchmark index in the fund prospectus that does not match the fund's actual style." The results are robust to the choice of benchmark index.

R^2. Since a high R^2 implies lower risk taking/selectivity, I subtract the R^2 measure from one to be in accordance with other measures that capture risk taking. I will refer to this measure as AG Rsq. I also use holdings based measures, the Return Gap of Kacperczyk, Sialm, and Zheng (2008), and the Active Share of Cremers and Petajisto (2009) and Petajisto (2013).[10] Then, I construct dummy variables that *a priori* are expected to affect the risk taking behavior of fund managers: the certified financial analyst (CFA) designation, MBA. degree, team managed, and female manager separately. Last but not least, I include a cohort dummy in order to separate out the effect of experience from "the generation effect" as suggested by Yao, Sharpe, and Wang (2011). I create a generation dummy based on when the fund manager was born, since it has been shown that the market conditions each person experienced earlier in their life have a major impact on their future risk taking or managing behavior (e.g., Malmendier and Nagel (2011); Dittmar and Duchin (2015)). For example, fund managers who were born in the investment industry during the 1940s are all given a 1940 dummy that equals one while others are zero.

2.3 Summary Statistics

Table 2, Panel A reports the summary statistics of fund manager and fund attributes for the full sample. The mutual fund managers' age distribution is similar to that of earlier papers, with a standard deviation of 9.38, but the average age of 46.2 is higher by 2 to 3 years. On average, the manager level termination probability is 13%, which is lower than what previous literature finds based on fund level termination. Fund managers whose age is above 60 amount to 9% of the fund manager population. Female fund managers also consist of 9% of the total population, and about 60% (56%) of the fund managers have CFA (MBA) degree. Net inflows to funds are on average 7%, with a median value of negative 5%. My main variable of interest, Tracking Error, has a mean of 1.19% (1.41%) with 0.7%

[10] The data on Active Share is available from the website of Antti Petajisto at http://www.petajisto.net/data.html

(0.99%) standard deviation at a monthly frequency when estimated using Fama-French-Carhart 4 Factor model (One Factor model with combination of Objective index and S&P 500 as factor returns). The distribution of log fund TNA is highly skewed, as evidenced in the literature, with a mean of 19.55 and standard deviation of 1.64. The log of fund family TNA is also highly skewed, with a mean of 23.32 and a standard deviation of 2.23.

Panel B (C) reports the same statistics for a subset of junior (senior) fund managers and their funds. Junior (senior) fund managers are defined as managers with age in the bottom (top) 40th percentile of each cross section. The age gap between average fund manager in the junior group and in the senior group is 18 years. Other notable differences are that funds managed by junior managers receive higher net inflows than the funds managed by seasoned managers. Also, on average, junior fund managers tend to have higher Tracking Error.

Panel D (E) reports summary statistics for funds during the the earlier period (more recent period). Most of the fund manager and fund attributes have changed significantly between these two periods. The most notable difference is the increase in termination probability. While the termination probability at an annual basis was 7% during the earlier period, it has become 14%in the more recent period. Also, average net inflows have decreased from 14% in the earlier period to 5%in the more recent period. Lastly, the measure of risk taking has decreased for both measures of Tracking Error.

3 Results

3.1 Changes in Career Incentives and Risk Taking

I begin by examining whether the two career incentives that affect fund manager risk taking, asymmetric flow-performance relationship and fear of being terminated, differs in the more recent period compared to the earlier period. In dividing the sample into two sub-periods, I use year 2000 as the cutoff since previous literature documents change in the mutual fund industry by the late 1990s.[11] However, there is nothing magical about the year 2000 *per se*. I find consistent results when I use any year surrounding year 2000 as breakpoints.

I follow Sirri and Tufano (1998) in analyzing the flow-performance relationship where the dependent variable is net percentage growth in fund TNA in year t and the independent variables are the overall net percentage growth of funds in the same investment category, the size of the fund in the previous period, the net expense ratio, the monthly return standard deviation during which performance is measured, and the percentile performance ranking. I rank each fund performance for the measurement period compared to other funds in the same investment category and I divide the performance into three or five groups: Low Performer, Mid Performer (Divided this group into Mid-Low, Mid-Mid and Mid-High Performer when using five groups) and High Performer. Low Performer, Mid Performer and High Performer are defined as Min(RANK,0.2), Min(RANK-Low Performer,0.6; when divided into five groups, 0.2 is assigned for each of the three Mid groups), and (RANK - Low Performer - Mid Performer) respectively. I use the Prospectus Objective category group as a group in which performance ranking is calculated for each fund. I additionally include the lagged natural log of fund family size since uninformed investors are expected to choose to invest in funds that are well known and have greater size. Most importantly, I include the interaction between both the Low Performer and High Performer with a Post-2000 dummy variable. The coefficient on this interaction will provide evidence on how on average net inflows to top and bottom performers are different in the more recent period from those of the earlier period. Then, I run a regression of flow on the above independent variables. Standard errors are double clustered at the fund and year level.

Table 2 provides the results of the flow-performance relationship analysis. Coefficients on most of the variables have a similar magnitude and significance as compared to findings in previous literature. Most importantly, the results once again provide strong evidence of

[11] Wahal and Wang (2011) find that there is significant difference in how funds operate before and after the late 1990s.

a nonlinear and asymmetric relationship between past performance and net inflow. However, three results stand out when the results on the Pre-2000 period are compared to those of the Post-2000 period. First, net inflow to top performers is greater for the Pre-2000 period. A one percentile increase in the category rank for the managers in the top 20th percentile in performance group leads to a 2.21% increase in TNA during the Pre-2000 period as opposed to a 1.33% increase in TNA during the Post-2000 period. There is still a disproportionately large net inflow to top performers in the more recent period, but the relationship is less convex. Second, net inflows respond positively to expense ratios in the earlier period, but are negatively correlated with expense ratios in the more recent period. This is consistent with investors realizing the importance of expense ratios in the choice of mutual funds in the more recent period. Third, the coefficient on fund family size is significantly positive for all periods and specifications, implying that investors do prefer larger fund families when allocating their investment. In order to test whether there is significant difference in net inflows to top performers between the two periods, I interact the High Performer with Post-2000 dummy variable. The negative and significant coefficient on the interaction indicates that net inflows to top performers are smaller in the more recent period compared to the earlier period.

For the manager termination analysis, past literature used termination at the fund level as suggested by Chevalier and Ellison (1999). This measure defined a manager as terminated if the fund manager is no longer the managing the fund in year t+1. The threat of being terminated from each fund was then used to explain the risk taking behavior of fund managers. However, in order for this career concern to affect managerial behavior, the termination must be a forced termination but it is difficult to disentangle a forced termination from a voluntary leave when fund level termination is used. Moreover, because of an increase in the number of funds available from the 1990's onward, an increasing number of moves of fund managers between funds and fund families has occurred. Therefore, being terminated in one fund is no longer the huge career concern that it previously was.

In order to take into account these additional considerations, I introduce a new measure of termination which is at a manager-level. With data that accurately tracks the employment history of mutual fund managers, I am now able to better disentangle forced termination from voluntary leave and transfer to other funds. I use the following criteria in determining whether a manager is terminated or not. If a fund manager controls a smaller number of funds in the current year than in the previous year, or if the manager controls the same number of funds in the final year of fund management, but shows a decrease in TNA of more than 30% during this time,, I define the manager as being demoted/terminated and assign 1 for that year and 0 otherwise. However, if the total TNA for the manager increased by more than 30 percent, I assign 0 to account for the fact that the manager voluntarily left the mutual fund industry. In this specification, all performance measures are value weighted at the fund manager level. If a fund manager manages multiple funds at a same time, the monthly gross return of each fund is value weighted by their respective TNA. Manager level termination, although not perfect, takes into account the recent increase in fund manager multitasking, strategic allocation of fund managers by fund families, increase in fund managers moving from one fund family to another, and voluntary termination. The time-series average of yearly manager level termination probability is 13 percent, while it is 25%for the fund-level measure. I argue that the fund level termination is capturing part of the non-forced turnover of fund managers that the manager level measure is able to eliminate. I use the manager-level termination for my main analysis and report the fund-level termination in the robustness check section and find that the results are consistent.

I run a logit regression of the termination dummy on the interaction between the Post-2000 dummy variable and current year alpha, estimated using the Fama-French-Carhart 4 Factor model, and fund and fund manager characteristics. In order to control for the fact that seasoned managers are more likely to retire regardless of performance and for normal retirement, I include a dummy variable that is equal to 1 if the lead manager is older than 60 (Age60+). Also, I include abnormal flow, which is the residual from the regression of flow on past returns, as flows could also affect the termination decision of fund families along with performance. The results are provided in Table 3. First, during the Pre-2000 period, the effect of current year performance is both economically and statistically weak. However, during the Post-2000 period, the effect of current year performance is strong. At the margins, a 1% drop in current year 4 Factor Alpha increases the probability of termination by 2.7%. Combined with the fact that the average termination probability during this period is 14%, this indicates a significant risk of being terminated. Second, the effect of past performance on firing decisions tends to go at least three years back. Each individual annual Alpha of the past three years is highly correlated with termination probability. Third, I find that the coefficient on the abnormal flow is negative for both periods, but its effect on termination is greater in the more recent period. This shows that fund families are taking into account not only the net inflows to funds, but also manager performance when the fund families make firing decisions. In order to test whether there is significant difference in the sensitivity of termination to current year Alpha between two periods, I interact $Alpha_t$ with the Post-2000 dummy variable. The negative coefficient on the interaction term provides evidence that in the more recent period, termination is more sensitive to performance than in the earlier period. It seems that fund families are not able to wait for managers to produce alpha in the more recent period, even though these managers could have been granted more time in the earlier period. Lastly, the coefficient on the Post-2000 dummy variable itself shows that manager level termination probability is higher on average in the more recent period, assuming the alpha equals 0.

The combined effect of less convex flow-performance relation and greater risk of termination suggests fund managers will take less risk in the more recent period. The results are provided in Table 4. In order to test for differences in overall risk taking between the recent period and the earlier period, I first fit a quarterly time-trend to average Tracking Error measured using a 4 Factor model. Consistent with what career incentives show, I find that Tracking Error decreases by 0.3% for each quarter. Then, I run a panel regression of Tracking Error on the Post-2000 dummy variable. Again, I find that Tracking Error decreases by 30% on average during the more recent period compared to that of the earlier period. The changes in career incentives led to less risk taking by fund managers on average in the more recent period.

3.2 Changes in Career Incentives by Career Stage

Both theoretical studies and empirical findings offer conflicting evidence on how risk taking varies with experience. Taking into account findings that document different career incentives for managers at different career stages, as well as the fact that fund manager tenure is no longer a given, I test how career incentives of seasoned and junior managers are different in the recent period compared to that of the earlier period.

In order to test for the difference, I divide the sample into funds that are managed by seasoned managers and ones that are managed by junior managers using age. I use age as a best available proxy for experience because the data does not exist on when each fund manager started working in the investment industry. Then, I test how the two career incentives for each group differ in the recent period compared to those of the earlier period. Lastly, I divide the sample into two periods and test for the relative difference between the two groups in each period.

First, I test whether investors react differently to junior and seasoned fund managers in terms of inflows to top performers and thus provide different incentives to each group of managers. Given that mutual fund managers receive a fixed proportion of assets under management as compensation, if the flow-performance relationship is different for funds managed by different experience groups, fund managers' incentive in taking risk would also be affected. I find that the overall decrease in net inflow to top performers found in Table 2 is similar in magnitude for both the experienced and inexperienced group. Table 5 shows that the increase in net inflows to top performers per each percentile increase in performance rank drops from 2.68% (2.47%) in the earlier period to 1.32% (1.09%) for the recent period for inexperienced (experienced) managers.

Then, I test for relative difference in net inflows to top performers between junior and seasoned managers. Table 6 provides the results of the analysis for both the more recent period and the earlier period. I find that for both the earlier period and the more recent period, net inflows to top performers is not statistically different between the inexperienced group and the experienced group. The change in industry dynamics has affected both of the groups in a similar magnitude. This finding show that compensation incentive does not differ between experienced managers and inexperienced managers for both periods.

Next, I test whether fund families react differently to junior and seasoned fund managers in their termination decisions, which would give each group of managers different career concern. Given that the threat of being terminated was found to be one of the largest incentives to take fewer risks in Chevalier and Ellison (1999), if career concern is different for funds managed by different experience groups, fund managers' incentive in taking risk would also be affected. I find that the overall increase in sensitivity of termination to performers found in Table 3 is concentrated on seasoned managers. Table 7 shows that the change is not statistically significant for junior managers and termination even becomes less sensitive to performance in some specifications. On the other hand, the increase in sensitivity is statistically significant and economically large for seasoned managers.

Table 8 provides the results concerning the relative difference in career concern between junior and seasoned managers for both the more recent period and the earlier period. The most interesting finding is that the sign of the coefficient on the interaction term between current year alpha and de-meaned age changes between the two periods. For the earlier period, all of the coefficients on the interaction term are positive, with some being significant.[12] However, for the more recent period, the coefficient on the interaction term becomes negative and statistically significant. In this period, a 1% drop in current

[12]In an unreported table, I find positive and significant coefficient for this interaction term for the 1992 to 1995 period and for the exact same sample in which Chevalier and Ellison (1997) uses.

year 4 Factor Alpha increases the probability of termination by 3.3% for the experienced group while it increase the termination probability by 0.56% for the inexperienced group at the margins. Another interesting finding is that the coefficient on abnormal flow becomes larger in magnitude and the statistical significance becomes stronger in the more recent period.

This can be interpreted as termination probability being more sensitive to the risk-adjusted performance of junior fund managers for the earlier period, while the termination probability is more sensitive to the risk-adjusted performance of seasoned managers for the more recent period. Moreover, fund families started to put a greater emphasis on net inflow along with performance. As a result, while there is no compensation incentive for the inexperienced fund managers to take more or less risk compared to their seasoned counterparts, career concern provides different incentive to these groups in different times. In the earlier period, when risk of termination was greater for the junior managers compared to that of the seasoned managers, seasoned managers would have greater incentive to take risk. On the other hand, in the more recent period, when risk of termination was greater for the seasoned managers compared to that of the junior managers, junior managers would have greater incentive to take risk. In the next section, I test whether fund managers react rationally to these incentives by analyzing their risk taking behavior in the wake of these new findings.

3.3 Changes in Risk Taking by Career Stage

Recent papers in psychology and socio-economics have found that there is a negative relationship between age and risk tolerance for Americans in general using a Survey of Consumer Finances (SCF) (e.g., Grable et al. (2006), Yao et al. (2011)). This is in contrast with what most of the previous literature found. Combined with the results I have found up to this point, I now test whether risk taking does differ between different experience groups for the earlier period and the recent period.

Table 9 presents the regression results of risk taking measures on variables that are expected to influence risk taking a priori. Panel A (B) reports the results for the sub-period of the Post-2000 (Pre-2000). I use Tracking Error as the dependent variable with the first three specifications using the Fama-French Carhart 4 Factor model and the latter three specifications using combination of Objective Index and S&P 500 as the only factor. I use the Prospectus Objective dummy to control for systematic differences in risk taking behavior between different investment style funds, as small cap funds will tend to have higher variation in their returns than funds mainly focusing on large stocks. Controlling for fund styles is important, as junior managers tend to manage smaller funds and seasoned managers tend to manage larger funds; without controlling for differences in styles, my results would be spurious. Later in the robustness check section, I test whether the findings in Table 9 are robust to different fund size groupings. I also control for the generation effect — that people who have experienced similar socioeconomic environments have similar risk tolerance — by including generation dummy variables (Malmendier and Nagel (2011)).

The most important finding in Panel A of Table 9 is that experience has a significantly negative impact on risk taking behavior regardless of the specifications and how risk is calculated for the more recent period. Employing a Tracking Error calculated using the 4 Factor model, my baseline specification calculates that the economic magnitude of difference in risk taking between the average manager from the junior and seasoned groups is 0.83%, *ceteris paribus*. Given that the population mean for Tracking Error is 1.18% with a standard deviation of 0.43%, 0.43% represents a 0.77 standard deviation change. In order to increase the statistical power of my test and to test whether the effect of experience on risk taking is monotonic, I create a categorical variable for experience instead of using a continuous variable. In specifications 2 and 5, I construct Exp Group as a categorical variable that varies from 1 to 2, with 1 corresponding to the more inexperienced group, and 2 to the more seasoned group. Therefore, a negative coefficient on Exp Group would indicate that junior fund managers as a group take more risks compared to their seasoned counterparts.

Moreover, in specifications 3 and 6, I define Seasoned (Middle) as a dummy variable that equals one if a fund manager is in the top 40 (middle 20) percentile in each cross section of fund manager age and 0 otherwise. As a result, a negative coefficient on each variable would indicate that the seasoned managers take more risk when directly compared to junior managers. Consistent with the result on Exp variable, I find that junior fund managers as a group take significantly more risk for the more recent period. Moreover, there is a monotonic increase in risk taking by the middle and seasoned manager group when compared to their junior counterparts. For the 4 factor model case, the Junior (Middle) group takes 0.23 (0.11)%higher Tracking Error compared to seasoned managers.

Two of other findings are also noteworthy. First, regardless of the specifications, female managers take less risk compared to male fund managers. This evidence is in line with previous findings in both the finance and the psychology literature that males take more risk (Jianakoplos and Bernasek (1998)). Second, managers with larger fund size take significantly less risk. Since the market impact of large funds is much greater than that of smaller funds, it is difficult to deviate much from the benchmark. In my Robustness check, I verify that this is not driving my results.

However, when the same analysis is done for the earlier period, I find that junior fund managers take less risk. In panel B of Table 9, I find that junior fund managers take less risk compared to seasoned managers for this period. This finding is consistent with earlier studies, and also with evidence I find in earlier tables. During the earlier period, junior fund managers had less incentive to deviate from their benchmark, mostly due to greater career concern. Consistent with these incentives, the coefficients on experience variables show junior managers took relatively more risk than their seasoned counterparts. The change in mutual fund industry dynamics changed the incentive scheme for both junior and seasoned managers in the late 1990s and fund managers reacted to the changed incentives.

This strong pattern of more risk taking by junior managers in the recent period with less risk taking in the earlier period is driven by drastic changes in the career incentive of fund managers of different experience groups in the two periods. In earlier periods, while the upside of risk taking was comparable to seasoned manager group, the downside of risk taking was more apparent for junior fund managers when compared to seasoned managers. However, the downside has changed in the direction that favors the junior fund managers in the more recent period with similarity in the upside remained. This leads to my finding that inexperienced managers took less risk than experienced managers in the earlier period, which is consistent with Chevalier and Ellison (1999).

Table 10 provide different variations of the baseline model provided in Table 9. I use different definitions of who the lead manager is. The first three specifications, Lead II, assumes the lead manager of a fund is the manager who has worked at the fund the longest, as opposed to my baseline specification where lead manager is assumed to be the manager with seniority in age. The next three specifications use a subsample of funds that are managed by a single manager (Lead III). Regardless of which definition I use and which specification I use, I find strong consistent results that junior fund managers on average take greater risk as compared to their seasoned counterparts during the more recent period. On the other hand, in Panel B of Table 10, I again find consistent results with Panel B of Table 9 where I show junior fund managers take less risk than their seasoned counterparts during the earlier period. Even for the specifications where I could not find statistical significance, the sign and economic magnitude of the coefficient is consistent with earlier findings. This provides added evidence that my results are not confined to a specific definition of who the lead manager is.

3.4 Robustness Checks

Earlier findings that link experience with risk taking could be driven by the fact that there is an inverse correlation between Tracking Error and market capitalization of a fund's holdings. That is, all other things being equal, a large cap fund has lower Tracking Error than a small cap fund. Furthermore, given that junior managers manage smaller funds, on average, we would expect more Tracking Error for junior managers simply because they manage smaller funds. In order to alleviate these concerns, I divide all funds into two groups based on Morningstar's categorization of fund size capitalization. Using Morningstar Category grouping, I run the same regressions for risk taking by experience for large and mid/small cap funds separately. Results are reported in Table 11.

In Table 11, I again find results consistent with my findings in Table 9 for all fund size groups. Funds managed by junior fund managers take significantly more risk when compared to their seasoned counterparts for the more recent period. The results for the earlier period are also consistent throughout the fund size group with earlier finding. With these results, I show that my findings of the relationship between risk taking and experience are robust to the negative correlation between Tracking Error and fund market capitalization.

Next, I use other measures of risk taking to test whether my finding is robust to the choice of measuring risk in Table 12. The first alternative measure I use is Amihud and Goyenko (2013) R^2 (AG Rsq). I regress the future twelve months of monthly fund excess return over the one month T-bill rate on Fama-French-Carhart 4 Factor return to get the R^2. As I noted previously, since a high R^2 implies lower risk taking/selectivity, I subtract the R^2 measure from one to be in accordance with other measures that capture risk taking. The R^2 measure has the benefit of not having to know or define the specific benchmark each mutual fund is using and thus is able to successfully detect funds that are truly active in picking stocks against funds that invest in multiple index funds and hide under the radar of other active management measures. The results of the first two specifications show the consistent result that junior fund managers take more risk in the more recent period compared to their seasoned counterparts when AG Rsq is used. The next two measures are based on the holdings of each mutual fund. In order to use holdings-based measures, it is necessary that I construct a map between the Morningstar, CRSP and Thomson databases. I follow the methodology provided in Berk and Van Binsbergen (2015) and Pástor, Stambaugh, and Taylor (2015) in mapping between Morningstar and CRSP Mutual Fund Database.

In a nutshell, I independently map CRSP MFDB to Morningstar Principia CDs and then Morningstar Direct to Morningstar Principia CDs. I used monthly returns, TNA, CUSIP, Ticker, fund names, and dividends to map the datasets. In the end, I was able to map 90.2%of fund-month observations in Morningstar to CRSP Mutual Fund Database. Then, I use MFLINKS from Wharton Research Data Services (WRDS) to map Morningstar Data to Thomson mutual fund holdings database. I randomly select funds from my mapping and verify that my mapping is robust.

The second alternative measure of risk taking is Return Gap as suggested by Kacperczyk et al. (2008). Return Gap captures unobserved actions of mutual fund managers and is measured by the difference between actual fund return and hypothetical fund return based on aggregated past reported holdings. I use the average of future 3 month Return Gap measures in order to test if junior fund managers take more unobserved actions. The next two specifications show that junior fund managers take more risk in the more recent period compared to their seasoned counterparts. The third alternative measure of risk taking is Active Share from Cremers and Petajisto (2009). Active Share is measured by the difference in holdings compared to their benchmark index at a certain point in time when mutual funds report their holdings to the SEC. I follow the specification in their paper in explaining Active Share and add manager-specific dummy variables that are of interest to my project, such as CFA, MBA, and Female. In Cremers and Petajisto (2009), they show that past Tracking Error is most closely related to Active Share measure and also include Log $Size^2$ to take into account the nonlinear effect of fund size on risk taking. Consistent with their finding, I find that Tracking Error has the largest economic magnitude in explaining the Active Share and that these two variables are positively correlated with each other. Also, I again find that junior fund managers take more risk in the more recent period. On the other hand, for the earlier period, the results are not statistically significant. This could be due to the fact that while the risk taking incentives favor the seasoned managers during this period, the difference in those incentives between seasoned and junior fund managers are not as stark as during the recent period. As a result, I find results that are statistically significant and economically large for the more recent period throughout all specifications.

In Table 13, I check for the robustness of my findings on the risk of termination with manager level termination. Following Chevalier and Ellison (1999), I focus on manager termination of single-manager funds. The results are consistent with my findings in the earlier tables. I find that there is an increase in sensitivity of termination to performance in the recent period and that this change is concentrated on seasoned managers, thereby providing junior managers an incentive to take more risk compared to the seasoned group.

Lastly, I test whether my findings hold for the actual investment industry experience rather than using age as a proxy for experience. I hand-collected the year each fund manager started working in the investment industry by going through each fund manager's bio and determining when the person started their investment career. Unfortunately, due to lack of these biographies available for most fund managers, the number of fund managers with this variable is significantly smaller than that using age. Moreover, in cases where it was unclear whether a certain job would count as an investment industry experience, I used my subjective judgment, resulting in potential bias.

In Table 14, I run the same analysis as in Table 9 analyzing the relationship between risk taking and experience. Industry experience is the number of years each fund manager worked in the investment industry and Industry Exp Group is a categorical variable that equals 1 for the inexperienced group and 2 for the experienced group based on median value of each cross sectional industry experience. Industry Middle is a dummy variable that equals 1 if a fund manager's industry experience is between the 40th to 60th percentile of each cross section and 0 otherwise. Industry Seasoned is a dummy variable that equals 1 if a fund manager's industry experience is in the top 40th percentile in each cross section and 0 otherwise. Throughout all specifications, I find consistent results with earlier tables that junior fund managers take more risk in the more recent period compared to seasoned fund managers and that seasoned fund managers take more risk compared to junior fund managers in the earlier period. With this finding, I provide evidence that fund manager age works as a good proxy for manager experience in the mutual fund industry.

4 Conclusion

In this paper, I document shifts in the career incentives of mutual fund managers. Compared to the earlier period, I find that fund managers receive less inflow for outperformance and suffer a greater risk of being terminated during the more recent period. Moreover, I document corresponding shifts in manager risk-taking behavior that are consistent with the changes in the likelihood of termination and the performance flow relation. I find that mutual fund managers on average take fewer risks in the recent period when compared to that of the earlier period.

I find that the change in the flow-performance relationship documented earlier does not differ between inexperienced and experienced managers. The decrease in convexity in the flow-performance relationship over time is similar in magnitude for both experienced and inexperienced managers, and the difference in the flow-performance relationship across experience groups is not statistically different in either time period. However, I find that the increase in sensitivity of termination to performance documented earlier is concentrated on experienced managers. As a result, termination-performance sensitivity is greater for experienced managers in the recent period, while it was greater for inexperienced managers in the earlier period. Given that career concern represents the cost of risk taking while the chance to earn high income represents the benefit, lessened career concern combined with similar benefits to risk taking gives inexperienced fund managers greater incentive relative to experienced managers to take risk in the recent period.

Consistent with the incentives, I find that in the recent period, inexperienced managers take greater risk relative to experienced managers, whereas the opposite is true during the earlier period. This finding is robust to choice of benchmark returns, factor models and ways of measuring risk taking. This result that inexperienced managers take less risk than experienced managers in the earlier period is consistent with Chevalier and Ellison (1999). However, I document that fund families' greater performance scrutiny on experienced managers in the more recent period has led to a reversal of this finding, with inexperienced managers taking more risk than experienced managers.

Given these clear shifts in career incentives and their differential impact on junior and senior managers, the question of the causes of these shifts naturally arises. Possible reasons for the shifts could include the increased presence of institutional investors in the mutual fund industry and/or increased competition from passive funds and other funds that have a similar investment style. Examining the impact of these factors could be an important area for future research. Moreover, since that mutual fund managers overall take fewer risks in the recent period, it would be of interest to look into the performance of mutual funds. As one manager interviewed by Foley (2016) indicated, mutual fund managers may simply be trying to "eke out a little outperformance against a benchmark such as the S&P 500." Whether this is the case or if mutual fund managers are taking more efficient bets is another area of future research.

CEO Home Bias and Corporate Acquisitions

Kiseo Chung, T. Clifton Green, and Breno Schmidt[1]

Abstract

We find that CEOs are significantly more likely to purchase targets near their birth place, consistent with either informational advantages or familiarity bias. Evidence from bidder announcement returns supports the latter view. Acquirer returns are significantly lower for CEO home bias acquisitions, and the relation is robust to controls for firm and industry characteristics. The negative announcement effect is stronger when the target is located further away, among poorly-governed firms, and when the CEO has a deeper birth place connection. CEOs' post-acquisition trading behavior also supports a familiarity bias interpretation. Our findings suggest that CEO home bias influences firm investment.

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1 Introduction

In 2010, after considering roughly 400 possible targets, Indiana-based manufacturer of funeral caskets Hillenbrand Inc. announced a plan to acquire K-Tron International Inc., a Pitman, New Jersey firm which engineers industrial coal crushers and feeding equipment (including a machine to shoot raisins into breakfast cereal). Despite the considerable difference in product lines, K-Tron provided Hillenbrand CEO Kenneth Camp with a unique benefit. Although Camp said the location in Pitman had no influence on his decision to buy the company, he acknowledged: "When I heard it was in Pitman I thought people would say I spent all this money to go see my mother." Camp was raised in Pitman and his mother Edith still lived nearby in his childhood home.[2]

In this article, we study the effects of CEO home bias on corporate acquisitions. Specifically, we analyze whether CEOs are more likely to acquire companies located near their birth place. We explore whether CEO home bias acquisitions are in the best interest of shareholders, and we examine whether home bias mergers reflect beneficial information advantages, potential private benefits to the CEO, or an underlying bias towards the familiar.

A well-established literature in equity markets finds that investors like to invest close to home, and evidence is mixed regarding whether local preferences reflect informational advantages or a bias towards the familiar. Coval and Moskowitz (2001) and Ivkovic and Weisbenner (2005) find that investors' local stock holdings outperform, and Kang and Stulz (1997) find that foreign investors avoid stocks with high information

[2] Details are taken from an article in the *Philadelphia Inquirer* (Fernandez, 2010). Hillenbrand's stock price fell by (CAPM-adjusted) 2.5% in the three-day window around the merger announcement.

asymmetry. On the other hand, Seasholes and Zhu (2010) and Pool, Stoffman, and Yonker (2012) find no benefits to local investing, and they observe a greater propensity to invest locally among less experienced investors, which is more consistent with familiarity bias.[3]

As with equity investments, a local preference for corporate investment may occur for informational reasons. For example, CEOs' educational or professional network connections may cluster geographically, which could lead to worthwhile investment opportunities close to home (e.g. Cohen, Frazzini, and Malloy, 2008; Cai and Sevilir, 2012). Cultural awareness of a geographic region may also facilitate the process of merging, which could also lead to more local mergers (Ahern, Daminelli, and Fracassi, 2015).

On the other hand, CEOs may also be susceptible to familiarity bias. Place attachment and place identity are well-established concepts in environmental psychology (e.g. Manzo, 2003), and familiarity is viewed as a central cognitive element of place attachment (Scannell, and Gifford, 2010). Familiarity has been linked to confidence in risky gambles (Heath and Tversky, 1991), and measures of CEO overconfidence have previously been linked to corporate investment (e.g. Malmendier and Tate, 2008; Hirshleifer, Low, and Teoh, 2012; and Ben-David, Graham, and Harvey, 2013). We consider CEOs' regional upbringing as a source of deep-seated familiarity, and we study whether a CEO's birth place influences the firm's acquisition behavior.[4]

As part of our identification strategy, we distinguish between in-state and cross-state acquisitions and similarly classify targets as being near or far from the acquirer based

[3] Other work includes French and Poterba (1991), Tesar and Werner (1995), Huberman (2001), Coval and Moskowitz (1999), Grinblatt and Keloharju (2001), Bhattacharya and Groznik (2008), and Parwada (2008).
[4] We refer to the region of a CEO's childhood as their "birth" place to denote upbringing and help differentiate it from their current place of residence. Empirically, our geographic measures emphasize CEO's place of residence during their teenage years. Section 2 describes the measures of CEO origin.

on geographic distance. The rationale is that we expect the effect of CEO home bias on target selection, either through unique information channels or through a bias toward the familiar, to be incrementally stronger when the target is further away from acquirer.

We find evidence that CEO home bias influences corporate acquisitions. Following an approach similar to Rhodes-Kropf and Robinson (2008), we compare actual targets to hypothetical targets with similar characteristics. We observe that the acquirer firm CEO grew up in the same region as the target in roughly 14% of mergers, compared to 6% in a sample of characteristic-matched hypothetical targets, and the difference in likelihood is statistically significant. We also find evidence that the increased propensity for home bias mergers is stronger among cross-state mergers.

To help distinguish between informational advantages and potentially detrimental familiarity-based explanations for CEO home bias in corporate acquisitions, we examine bidder returns around the announcement of the deal. We find bidder merger announcement returns are significantly lower when the target is located near the CEO's hometown. The magnitude of the effect is also economically significant: after controlling for firm and deal characteristics, we find that these acquisitions are associated with a negative CAR of -1.67%. In contrast, we find no significant value effect in cross-state mergers with no CEO home bias, with an estimated bidder announcement return of 0.09%. We also find no significant effect of home bias on bidder returns for in-state mergers, which reinforces the view that CEO home bias is more important when the target is further away from the acquirer.

If home bias acquisitions are more likely to reflect managerial objectives rather than value maximization (e.g. Morck, Shleifer, and Vishny, 1990), we would expect the practice to be more prevalent among poorly governed firms. Consistent with the managerial objectives hypothesis, we find stronger negative bidder announcement return evidence among poorly governed firms. The governance results provide additional evidence in support of the interpretation that home bias cross-state acquisitions reflect manager preferences.

We anticipate that the effect of CEO home bias on birth state merger activity will be stronger when the CEO holds a deeper connection to their birth state. Place attachment is generally thought to be the result of a long-term connection (Altman and Low, 1992) and we conjecture that CEOs who attended college in their birth state or resided there in early adulthood will hold stronger attachments. Consistent with a familiarity interpretation, we find that bidder firm announcement returns are significantly lower for home bias cross-state mergers when the CEOs attended college in the target state or lived there after college.

We also consider measures of home bias based on geographic distance, as some cross-state mergers may be geographically close for firms in small states or those near state borders. Consistent with the home state results, we find stronger negative bidder returns when the target is close to the CEO’s hometown (less than 100 miles) yet far from the acquirer headquarters (greater than 100 miles). Moreover, as before, the negative announcement evidence is stronger among poorly governed acquirers and also when the CEO has a stronger educational or residence connection to their birth state. Our findings are robust to alternative econometric approaches and when considering longer-horizon returns.

Taken together, our findings suggest that markets react negatively to CEOs’ proclivity to purchase cross-state targets from their birth state. The evidence is consistent with a bias for the familiar that leads to over-optimism regarding the value of the merger. Alternatively, CEOs may understand that home bias mergers are inefficient and undertake such investments for personal rather than firm reasons. We distinguish between these interpretations by examining CEO insider trading around merger announcements. We find CEOs are roughly twice as likely to purchase company stock following the announcement of a home bias merger relative to non-home bias mergers, and we observe no analogous trading pattern for board members or other company executives. The evidence that CEOs purchase company stock following home bias merger announcements is less consistent with rent extraction through pet projects, and supports the view that CEO home bias mergers reflect familiarity-based optimism.

Our evidence of a familiarity-driven birth state home bias is consistent with Pool, Stoffman, and Yonker (2012), who find mutual fund managers are more likely to invest in companies with headquarters in their birth state with no evidence of outperformance. Our results are also in line with Cornaggia, Cornaggia, and Israelsen (2015), who find credit analysts rate municipal bonds issued in their birth states more favorably. Our setting is most closely related to Yonker (2016b), who finds that home state CEOs are significantly less likely to lay off employees than their non-local peers following industry distress. We document the complementary finding that out-of-state CEOs are more likely to invest in their home states through acquisitions.[5]

The remainder of the paper proceeds as follows. In Section 2 we describe the sample and construction of the home bias variables, Section 3 examines the effects of CEO

[5] We recognize Jiang, Qian, and Yonker (2016) as independent contemporaneous work that also documents a home bias in corporate acquisitions. While their findings generally support our own results, they find evidence home advantage for a subset of public target mergers.

birth state on the acquisition propensity, Section 4 studies the effects of home state bias on announcement returns, Section 5 studies insider trading around merger announcements, and Section 6 concludes.

2 Data and Variable Construction

This section describes the acquisition sample and provides details for the construction of the CEO home bias related variables.

2.1 Acquisition Sample

The merger data come from the Securities Data Company (SDC). After collecting all mergers from 1985 to 2014, we impose the following three data requirements which are similar to those in Masulis, Wang, and Xie (2007). First, the acquirer is a publicly traded company with stock returns data available in Center for Research in Security Prices (CRSP); the bidder is allowed to be either a publicly traded or private. Second, the deal value represents at least 1% of the acquirer's market value, as measured at the fiscal year end before the announcement. Third, we require the bidder to be identified in either the BoardEx or the ExecuComp database. We also require CEO information at the time of the announcement (dates of employment are occasionally missing early in the sample period).

The bidder firm CEO data were obtained from both the BoardEx and ExecuComp. Boardex data contains detailed profiles of US executives and board members, covering virtually all US public companies.[6] ExecuComp data contains detailed information on executive compensation data for past and current S&P 1500 firms. We are able to match

[6] Cohen. Frazzini and Malloy (2008) provide a more detailed description of the database. See also Ferreira and Matos (2012), Cohen, Frazzini, and Malloy (2012), and Schmidt (2015).

15,526 mergers from SDC data with Boardex/ExecuComp that have engaged in mergers of public/private targets for our sample period.

2.2 Measuring CEO Home Bias

In order to identify each CEO's birth state, we collect information on his or her full name, age, and firm name from BoardEx/ExecuComp.[7] Using the CEO's name and age for each acquisition in our sample, we collect data on each CEO's birth state and previous addresses from the Lexis Nexis Online Public Records Database following the methodology of Pool, Stoffman, and Yonker (2012). Specifically, we search by CEO name and age, and we also use other information such as employment history and email addresses to pinpoint the correct person. In order to further guarantee each CEO's identity, we also require that the firm employing the CEO when the deal was announced corresponds to one of the employers listed in the CEO's Lexis Nexis personal file.

For the CEOs for whom we could obtain a unique Lexis Nexis ID, we use the first five digits of their social security number to get their home state. Alternately, for CEOs whose unique Lexis Nexis ID could not be identified, we use firm name, CEO name, and age in Google to search for their home state. In order to be included in our sample, data on the birth state of the acquirer firm CEO must be available. We were able to collect CEO public records data for 12,221 mergers, which represents 79% of the number of mergers and 94% of total deal value for the mapped set of SDC and Boardex/ExecuComp mergers.

[7] Currently, US citizens typically obtain social security numbers (SSNs) near birth. For CEOs during the sample period, they were more likely to obtain SSNs prior to their first jobs or when obtaining a driver's license. Yonker (2015a) indicates that a majority of the CEOs in a similarly-constructed sample received their SSN when they were between the ages of 14 and 17. Therefore, "birth" state is more accurately described as home state during the mid-teenage years.

We match the SDC and CEO birth state merged dataset with data from the Center for Research in Security Prices (CRSP) and Compustat, from which all financial and accounting variables are obtained. Our merger sample consists of 8,790 acquisitions after applying the initial data requirements. In cases where the zip code is missing for either acquirer or the target firm in SDC database, we use the headquarters zip code variable in Compustat when available. Our resulting distance merger sample consists of 8,001 mergers.

Our first measure of home bias is based on the CEO's state of upbringing. *Home Bias* $_{State}$ is a dummy variable that is equal to one when the acquirer firm CEO birth state is equal to target headquarters state. We partition the merger sample into in-state and cross-state mergers by defining the dummy variable *Cross-State Merger,* which is one if the acquirer and target headquarters states differ. We use headquarters' state rather than state of incorporation as the latter is often chosen for regulatory rather than operational reasons.

Our second measure of CEO home bias is based on the geographic distance between the target firm headquarters and the CEO's hometown. To obtain information on the CEO's birth town, we search the public records data from Lexis Nexis. We attribute the oldest available address that matches the birth state implied by the Social Security Number as the CEO's birthplace. If no address is available that matches the SSN-implied state, we use the zip code of the largest city in the state as a proxy for hometown.[8]

Based on the CEO's hometown, we then use the latitude/longitude of the zip codes in the census files to determine the distances between the target firm headquarters and acquirer CEO hometown.[9] We define *Home Bias* $_{Distance}$, which is one if the distance

[8] The results are very similar if we use the state capital instead of the largest city for observations that do not listed addresses with state matches.
[9] Sources: www2.census.gov/geo/tiger/TIGER2010/ZCTA5/2010/, www2.census.gov/geo/tiger/TIGER2015/ZCTA5/; also see: www2.census.gov/geo/pdfs/maps-data/data/tiger/tgrshp2010/TGRSHP10SF1AA.pdf

between the target headquarters and the acquirer firm CEO's hometown is less than 100 miles. Analogous to cross-state mergers but capturing geographic distance rather than state borders, we create the dummy variable *Faraway Merger* which is one if the target firm headquarters is located more than 100 miles from the acquirer firm headquarters.

2.3 Sample Summary Statistics

Table 1 presents summary characteristics for the merger sample. The main takeaway from the table is that deal size and firm characteristics are generally similar for CEO home bias mergers and the full sample. Cross-state and faraway mergers also do not differ materially from other types of mergers. Although there is overlap in our measures of CEO home bias, the state and distance home bias measures do capture different samples. For example, among CEO birth state home bias mergers, the CEO grew up more than 100 miles from the target 32% of the time. On the other hand, in 27% of the mergers in which the CEO grew up within 100 miles of the target, they resided in a nearby state rather than in the target state. The differences are greater among distant home bias mergers. Only half of *Cross-State Home Bias* $_{State}$ mergers also qualify as *Faraway Home Bias* $_{Distance}$ mergers, and vice versa.

3 Results

3.1 Home Bias and Acquisition Propensity

We begin by exploring the relation between the geographic location of CEO upbringing and the location of corporate acquisitions. In particular, we examine whether acquirer firm CEOs show a greater tendency to acquire targets from the same geographic region as their birth place. In order to test this hypothesis, for each merger we select hypothetical targets that match the characteristics of the actual target. We then test whether CEOs' home regions are more often represented in actual targets than in the control set of comparable targets.

Our approach is similar to Rhodes-Kropf and Robinson (2008). For each merger with a public target, we consider hypothetical targets from the CRSP-Compustat universe that operate in the same 48 Fama-French industry. We narrow the set of hypothetical targets by selecting those in the same market capitalization and book-to-market ratio quintiles of the actual acquired firm. We also require hypothetical targets not to have participated in a merger in a four-year window (-2, +2) around the announcement date of the actual merger. If no company meets the criteria, we remove the book-to-market restriction and only use industry and size restrictions. In our setting, and unlike Rhodes-Kropf and Robinson (2008), we fix the acquirer firm because we are interested in the tendency of the bidder firm CEO to acquire home region targets. Using this approach, we obtain a sample of 3,340 actual public target mergers, with an average of 13 hypothetical candidates for each merger.

Panel A of Table 2 reports the propensity of CEO *Home Bias* $_{State}$ mergers along with the corresponding likelihood that a hypothetical merger includes a target that matches the birth state of the acquirer CEO. We observe that in 14.7% of mergers, the target headquarters state matches the birth state of the acquirer CEO. However, in the sample of characteristic-matched hypothetical targets, only 5.8% of targets match the state of the CEO's upbringing, and the difference in likelihood is statistically significant.[10] On the

[10] Our hypothetical targets are matched using information available only for public targets. Our propensity tests therefore rely on public target mergers. However, we observe similar proportions of home bias mergers among private target mergers. For example, the fraction of *Home Bias* $_{State}$ mergers with private targets is

other hand, mergers that do not exhibit CEO state home bias occur less frequently than in the control sample, 85.3% vs. 94.2%, and the difference again is statistically significant. The evidence suggests that CEO home bias mergers occur more often than expected by chance.

Panel A also partitions the merger sample into in-state and cross-state acquisitions. Consistent with the full sample, cross-state CEO home bias mergers occur significantly more often than the control group, whereas non home bias mergers occur significantly less often than expected by chance. In-state mergers happen more often than in the control group for both home bias and non home bias mergers, which is perhaps unsurprising since nearby mergers are likely less costly to implement than other similar firms matched on size, book-to-market, and industry. However, we do observe a greater proportional increase in the likelihood between actual and hypothetical acquisitions for home bias mergers than for non home bias mergers, although the difference is not statistically significant. Repeating the propensity analysis in Panel B using distance-based measures of home bias yields similar results.

In Panels C and D of Table 2, we report the results from probit regressions, where the dependent variable is 1 for actual mergers and 0 for hypothetical mergers. Consistent with the univariate results, the first column in Panels C and D indicates that home bias mergers happened significantly more often than in the control group. Including dummy variables for distant mergers and an interaction term reveals that the increased propensity of home bias mergers is stronger among distant mergers. We observe that *Cross-State Home Bias* $_{State}$ mergers have 2.85% greater probability of being an actual merger at the

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13.5%, of which 3.4% (10.1%) are cross-state (in-state). The analogous numbers for public target mergers in Table 2 are 14.7% and 4.1% (10.6%).

margins, which is significant at the one percent level. The evidence is similar for *Faraway Home Bias* $_{Distance}$.

In column (4), we add controls based on the absolute difference in market value of equity between the acquirer and the target and the absolute difference in book-to-market ratio between acquirer and the target as in Rhodes-Kropf and Robinson (2008). Consistent with Rhodes-Kropf and Robinson (2008), we find evidence that firms with similar book-to-market ratios and those that differ in size are more likely to merge. However, distant home bias mergers remain significantly more likely than expected by chance.

Our probit approach includes several hypothetical targets for each actual target, which has the effect of placing greater relative emphasis on mergers with more available hypothetical targets. We explore whether the results depend on the specific hypothetical targets chosen using simulation evidence. We start by randomly selecting one hypothetical target for each acquirer. We then repeat this process 1,000 times to produce 1,000 different samples, with each sample consisting of the actual targets plus and an equal number of hypothetical targets. When then we run individual probit regressions for each of the 1000 samples. Columns (5) through (8) report the average coefficients across the simulations and the empirical p-values for each coefficient, i.e. the proportion of coefficients with non-negative estimates. The simulation evidence produces home bias results similar to our base-line probit regression result, although the incremental effect on home bias on distant mergers is weaker. Taken together, the evidence in Table 2 provides compelling evidence that mergers are more likely to take place when the acquirer firm's CEO grew up in the same region as the target.

3.2 Market Response to Home Bias Acquisitions

The tendency for CEOs to invest in the region of their upbringing could reflect comparative advantages. For example, CEOs' informational networks may cluster geographically, which could lead to worthwhile investments (e.g. Cohen, Frazzini, and Malloy, 2008; Cai and Sevilir, 2012). Cultural awareness of a geographic region may also improve assimilation, which could also lead to more local mergers (Ahern, Daminelli, and Fracassi, 2015). On the other hand, CEOs' local investments may also reflect familiarity bias. Familiarity is associated with increased confidence in risky gambles (Heath and Tversky, 1991), and measures of CEO overconfidence have previously been linked to corporate investment (e.g. Malmendier and Tate, 2008; Hirshleifer, Low, and Teoh, 2012; and Ben-David, Graham, and Harvey, 2013). In this section, we examine bidder announcement returns to explore whether home bias acquisitions are driven by informational advantages or are better explained by a bias for the familiar.

3.2.1 Acquirer Returns Following CEO Home Bias Mergers

We measure bidder announcement effects using market-model adjusted stock returns around merger announcements as in Moeller, Schlingemann, and Stulz (2004), Masulis, Wang, and Xie (2007), and Schmidt (2015). Market-model estimates are obtained using the daily CRSP value-weighted index as the proxy for market returns. The estimation period is from 230 days to 11 days before the announcement. Announcement dates are obtained from SDC, and three-day cumulative abnormal returns are computed around these dates. We control for extreme outliers by winsorizing CARs at the 1st and 99th percentiles each year.

We follow Schmidt (2015) in selecting control variables. We include *Log Total Assets* to capture acquirer size, which has been shown to negatively affect bidder performance (e.g. Moeller, Schlingemann, and Stulz, 2004). Tobin's Q also has a documented negative effect on announcement returns (e.g., Lang, Stulz, and Walkling, 1991). We follow Gillan, Hartzell, and Starks (2011) and Masulis, Wang, and Xie (2007) and use *Industry Tobin's Q* rather than firm-level Tobin's Q due to concerns regarding endogeneity. We also similarly include *Industry Leverage*.

Shleifer and Vishny (1989) suggest that managers may enter new lines of business when threatened by poor performance, a view supported by the evidence in Morck, Shleifer, and Vishny (1990). We follow Morck, Shleifer, and Vishny (1990) and use the change in operating income during the prior three years as a measure of performance (*Δ Income*). To account for past performance of the bidder, we include *Price Run-up*, which is the bidder's buy and hold abnormal return from 230 to 11 days before the announcement as in Masulis, Wang, and Xie (2007).

Acquirer announcement returns have been shown to be related to the method of payment and the type of target (e.g., Chang, 1998, Moeller, Schlingemann, and Stulz, 2004; and Officer, Poulsen, and Stegemoller, 2009). To account for this variation, we include controls for the type of target (a *Public* dummy variable), and the medium of payment (*Cash Deal* and *Stock Deal*).[11] We also include *Relative Deal Size* to control for the size of the deal.

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[11] In unreported tests, we also include interactions between the target type and the type of payment since the chosen medium of exchange is often related to the target characteristics (Officer, Poulsen, and Stegemoller, 2009). The coefficients of interest are almost identical to those in Table 3 and we omit the interaction terms for brevity.

Table 3 presents the results from the bidder CAR regressions. Each specification includes year fixed effects and standard errors are clustered by Fama and French 48 industries. Columns (1) and (5) include our home bias indicator variables alone in the regression. Both show negative coefficients, although only the coefficient on *Home Bias* $_{Distance}$ is statistically different from zero. The same pattern emerges in (3) and (7) when the set of control variables are added. Specifically, we find that when bidder firms announce the acquisition of a target that is located within 100 miles of where the CEO grew up, the bidder experiences three-day abnormal returns of -0.63%.

Including distance indicator variables and home bias interaction terms in columns (2) and (6) reveals that the negative announcement response to CEO home bias acquisitions is concentrated among distant mergers. For example, a cross-state merger in which the bidder CEO grew up in the target state results in incremental -1.82% announcement return. Similarly, announcing the acquisition of a target greater than 100 miles away that is within 100 miles of the CEOs birthplace produces an incremental -1.70%. The results are very similar after including the control variables.

Another result from Table 3 is that the negative response to distant mergers, -0.69% (-0.67%) on average for cross-state (faraway) mergers, is concentrated among CEO home bias mergers. For the subset of cross-state (faraway) mergers that do not exhibit CEO home bias, the announcement returns are considerably less negative, at -0.40% for cross-state mergers and -0.35% (and insignificantly different from zero) for faraway mergers.

The evidence of negative market reaction to CEO home bias mergers in Table 3 is more consistent with familiarity bias influencing corporate investment decisions rather than suggesting that these mergers reflect valuable information obtained through the CEO's network.

3.3 Corporate governance and CEO home bias acquisitions

Masulis, Wang, and Xie (2007) find that entrenched managers are less susceptible to market discipline and may therefore be more likely to engage in value-destroying acquisitions. In this section, we examine whether CEOs of poorly governed firms are more likely to engage in home bias mergers, and whether these mergers are more poorly received by the market on announcement.

In order to proxy for entrenched CEOs, we use the entrenchment index (E-index) of Bebchuk, Cohen, and Ferrell (2009). We also consider a measure of concentrated holdings by independent long-term institutions as in Chen, Harford, and Li (2007). Chen et al. (2007) find that greater institutional ownership is associated with stronger post-merger performance, which they attribute to the active external monitoring role of such institutions. For each governance measure, we use the median level to divide the acquisition sample into a well-governed and a poorly-governed group.

Table 4 repeats the probit analysis from Table 2 after partitioning the sample into good and poor corporate governance samples. We observe that the home bias coefficients are considerably larger among the poorly government subsamples, which indicates a greater propensity for targets to be selected from near the acquirer CEO's birthplace among poorly governed firms. However, we do not find robust evidence for differences across samples in the incremental likelihood for home bias cross-state or faraway mergers across, which is consistent with both nearby and distant CEO home bias mergers being more likely among poorly governed firms.

In Table 5, we test our hypothesis that home bias mergers are more likely to be perceived negatively when conducted by poorly governed firms. The table provides bidder return results as in Table 3 for the governance subsamples. Consistent with home bias mergers being influenced by manager preferences, we find negative and significant coefficients for *Cross-State* and *Faraway* home bias mergers for the high entrenched and low institutional holdings groups while the effect is not significant for the better governed subsamples. The evidence that the negative market effect of home bias cross-state mergers is stronger among poorly governed firms is consistent with the view that CEOs are influenced by familiarity bias.

3.4 Strength of Home Region Connection

If the effect of CEO birth region on merger activity reflects a bias towards the familiar, we may expect it to be stronger when the CEO has a deeper connection to their home state. We explore this conjecture using measures of birth-state strength of connection based on the CEO's educational background and their residence history. Specifically, we match school names from CEOs' education backgrounds provided by Boardex with data from the U.S. Department of Education on accredited higher education institutions and find each institution's location.[12] We define a strong education connection if the acquirer firm CEO attended a higher education institution that is located within their home state.

Our second strength of connection measure is based on each CEO's residence history. The Lexis Nexis database provides address histories for each person beginning in their early- to mid-twenties. In our sample, roughly 65% of each CEO's past address history became available between the ages of 18 and 25. We conjecture that CEOs who

[12] http://ope.ed.gov/accreditation/GetDownLoadFile.aspx.

continued to live in their home state into early adulthood will hold stronger connections to their home state.

The propensity results are presented in Table 6. For both the home and education strength of connection measures, we find a greater propensity for home bias mergers than for CEOs with weaker home region connections. The interaction terms with distant mergers are not incrementally significant, which suggest the strong connection leads to more near and distant home bias mergers.

The bidder return results are presented in Table 7. As expected, when CEOs who attended college in their birth state acquire a cross-state home bias target, the 3-day CAR of the acquirer is -1.8 (-2.13) % and it is significantly different from zero at the 5 (1) percent level for the state (distance) based variables. On the other hand, if the home bias CEO has no educational connection to their home state, the merger announcement CARs are negative but insignificantly different from 0. We find very similar patterns when we measure strength of connection using an early adult address in the home state. As can be seen from the last four columns, CEOs who lived in their birth state after their teenage years have negative and significant 3-day CARs that vary from -1.4 to -2.44% while CEOs who moved away from their home state prior to adulthood are associated with negative but insignificant CARs when acquiring a cross-state home bias target.

Yonker (2016a) classifies CEOs who obtain SSNs after age 21 as “foreign.” Using this approach, we classify 293 CEOs as foreign, representing roughly 8% of the CEO population. We might expect foreign CEOs to exhibit a less strong connection with their U.S. home state, particularly when the connection is not established until adulthood. In tabulated results, we find negative but insignificant three-day announcement returns when foreign CEOs engage in home bias cross-state mergers.

3.5 Public vs. Private Targets

In Table 8, we divide the merger sample into public and private targets. Consistent with past literature, we find a negative market response on average to public target mergers and a positive reaction to private target mergers (-1.49% and 1.07%). In Table 8, we control for this differential effect as well as the full set of controls in Table 3. The first three columns provide evidence on acquirer CARs when the target was a publicly traded company. Regardless of whether we use state-level measures of home bias, as in columns (1) and (2), or distance-based measures as in column (3), we find that distant home bias mergers have significantly underperform by 1.3 to 2.2%.[13]

The last three columns of Table 8 provide evidence for private targets. Aside from the constant term, which captures the differences in the announcement effect of public vs. private targets, the economic magnitude and the statistical significance of the coefficients are very similar. For private target mergers, we find that cross-state/distant home bias mergers with CEO birth city close to target headquarters underperform by 1.4-2.0%. This provides evidence that our finding is not confined to specific subset of target firms.

For the subset of public target mergers, we are able to explore whether home bias CEOs pay larger takeover premiums by examining the target price announcement response.

[13] In contemporaneous work, Jian, Qian, and Yonker (2016) find evidence of a positive market response to cross-state, state home bias public target mergers. Their sample is taken from ExecuComp for 1992-2014, which generally covers S&P 1500 firms (S&P 500 firms for 1992-1993). We broaden the merger sample to also include smaller public acquirers listed in BoardEx from 1985-2014, and our sample of public target mergers is more than twice as large as theirs. The evidence in Jian, et al. (2016) is consistent with larger bidder firms being less likely to engage in value destroying home bias mergers, perhaps due to better monitoring.

Columns 4 and 5 of Table 8 report regressions of public target 3-day price responses using the same set of controls as for bidder returns. Although neither coefficient is significantly different from zero, the coefficients for distant home bias targets are positive for both state and geographic distance home bias measures, which is generally consistent with paying relatively more for distant home bias targets.

3.6 Robustness checks

Thus far, we have relied on bidder announcement returns to measure the effect of CEO home bias on the value of the firm. In this section, we discuss some potential econometric concerns with such an approach and provide a series of robustness checks which, by and large, confirm our main conclusions.

Endogeneity is often a major concern in corporate finance studies. In our setting, causal interpretations of the coefficients of interest are only valid if, conditional on our other explanatory variables, the "CEO home bias'' is randomly assigned. To illustrate this omitted variable problem, suppose that birth rates are higher in exactly the same target states that, for whatever reason, are associated with value destroying acquisitions. In this case, our results could be driven by this omitted variable. In order to address this problem, we try to control for the joint distribution of acquirers and targets using simulations.[14]

To illustrate this approach, consider first the subsample of cross-state acquisitions. For each cross-state acquisition in which the CEO birth state was the same as the target state, we randomly choose another acquisition with the same bidder and target state but with different CEO birth state. This produces a sample in which the likelihood of a CEO

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[14] An alternative approach would be to include fixed-effects for all combinations of acquirer-target states, yet this is infeasible due to the large number of pairwise state combinations relative to the number of cross-state acquisitions.

home bias is fifty percent. Next, we run a regression of bidder announcement returns on the CEO home bias dummy and the controls described in Table 3. To prevent our results from being driven by this particular choice of control acquisitions, we repeat this process 1,000 times and use the distribution of coefficients to draw our statistical inferences.

Table 9 presents the results using both the states (Panel A) and distances (Panel B) as our measure of birth region proximity. For brevity, we only report the empirical distributions and empirical p-values for the Home Bias coefficients. Consistent with our previous results, we find a negative and significant impact of home bias, but only for distant mergers. For example, in Panel A, the home bias coefficients for in state mergers are not statistically significant, and the economic magnitude is roughly $1/7^{th}$ of the cross state mergers. The results for distance-based home bias mergers in Panel B are similar.

Another potential problem with the interpretation of the coefficients in Table 3 is that our approach relies on bidder announcement returns, whereas it is possible that the market incorrectly assesses the relative merits of home bias mergers. In Table 10, we estimate the longer-term effects of CEO home bias on the value of the firm using a calendar time approach which is less susceptible to econometric issues (Barber and Lyon, 1997). The calendar time strategy involves buying each home bias merger beginning three days after the announcement and holding for 6, 12, and 24 months. We use the Fama-French 3-factor model to risk-adjust returns, and report the monthly alpha for the set of home bias mergers.

To control for average post-merger performance, we also calculate 3-factor alpha for a randomly drawn set of matched non-home bias mergers based on the location and industry of the merged firms as in Table 9. Table 10 reports the average alpha for the 1000 simulated merger portfolios, as well as the empirical p-value that the merger portfolio underperforms the simulated portfolio. The evidence in Table 10 does not support the view that the initial negative reaction to distant home bias deals reflects misreaction. Abnormal returns following home bias mergers are negative on average and significantly worse than the matched sample of non-home bias mergers.

3.7 CEO Home Bias Mergers and Insider Trading

Our findings suggest that markets react negatively to CEOs' proclivity to purchase cross-state targets from their birth state. The evidence is consistent with a bias for the familiar that leads to over-optimism regarding the value of the merger. On the other hand, it is possible that CEOs understand that these mergers are inefficient, and yet engage in them as a type of rent seeking behavior. The evidence that markets react more negatively to home bias cross-state mergers when the firm is poorly governed, as well as when the CEO has a stronger connection to their birth state, is consistent with both interpretations.

In order to test whether home bias mergers are more consistent with familiarity bias or a pet project motivation, we examine insider trading by CEOs. If CEOs understand that home bias mergers are inefficient but engage in them for private benefits, we would expect a smaller investment in their company stock around the merger announcement compared to non-home bias mergers. However, if familiarity leads CEOs to be unduly optimistic about the prospects of the merger, we would be more likely to observe home bias CEOs buying company stock. We also examine board members and other executives' trading behavior as a benchmark which can be compared with the behavior of CEOs.

We follow the simulation approach in Table 8 for our insider trading analysis. For each cross-state or distant home bias merger, we randomly select a matching merger that has the same bidder and target state, but with different CEO birth state and repeat this process 1,000 times. For each of these 1,000 simulations, we count the number of mergers in which the net trade (sum of shares purchased less shares sold) executed by the CEO, director, or executive during (-60, -10) and (2, 60) trading days around the announcement date was positive.[15] We then count number of simulations where the probability of each group purchasing their own stock is greater for home bias mergers compared to their matched sample to get an empirical p-value.

The results are reported in Table 11. Consistent with the familiarity bias hypothesis, in Panel A we find CEOs of home bias mergers are roughly twice as likely to purchase company stock following the deal announcement relative to non-home bias mergers. Home bias CEOs also appear slightly more likely to purchase shares in the 60 days prior to the announcement, although the difference is statistically insignificant. On the other hand, we find no analogous purchasing pattern for directors or other executives. The table shows directors and other executives are less likely to purchase following home bias acquisition announcements, although the difference from the non-home bias match is not statistically significant. The insider trading evidence supports the view that CEOs' optimism following birth state acquisitions may be influenced by familiarity.

In Panel B, we examine cases where CEOs appear to be alone in their optimism. Specifically, we consider mergers where CEOs purchase shares around the announcement, but directors and other executives do not purchase shares. Although this pattern is rare, Table 10 indicates it is much more likely for home bias mergers than non-home bias

[15] For directors and executives, we take the cross sectional mean of the group for each date and sum over the window to define whether the group made a purchase. We find similar results using an alternative approach in which we count the number of mergers in which the largest trade executed was a purchase for each group. Results are also similar when excluding insiders from the board member group.

mergers. For example, for cross-state mergers, home bias CEOs purchase, and no other executives purchase, 2.25% of the time. This number is only 0.5% for non home bias mergers. The insider trading evidence supports the view that the market's negative reaction to CEO home bias acquisition reflects familiarity driven CEO optimism.

4 Conclusions

We consider CEOs' regional upbringing as a source of deep-seated familiarity, and we explore study whether a CEO's birth state location influences the firm's acquisition behavior. We find that CEOs are roughly one third more likely to acquire cross-state targets from their birth states than expected by chance. Although home bias cross-state acquisitions represent a relatively small proportion of the overall merger sample, they allow us to identify the effects of home bias. We also study measures of geographic distance and provide simulation evidence to help preclude that our findings are specific to the sample. Our findings support the view that home bias influences investment policies.

We distinguish between informational advantages vs. familiarity-based explanations for CEO home bias by examining bidder returns around the announcement of the deal. We find bidder announcement returns for cross-state home bias mergers are -1.67% vs. 0.09% for cross-state mergers when the CEO was not born in the target state, and the differences are statistically significant after controlling for firm and deal characteristics.

We also consider home region investing preferences using measures of distance from target headquarters to CEO hometown. Consistent with the home state results, we find stronger negative bidder returns when the target is close to the CEOs hometown (less than 100 miles) yet far from the acquirer headquarters (greater than 100 miles). We find that the negative announcement effect of home bias in cross-state or distant mergers is stronger when the CEO has a deeper connection to his or her birth state. The effect is also stronger among poorly governed firms, which is consistent with these projects reflecting manager preferences rather than informational advantages. We find evidence that CEOs are significantly more likely to purchase company shares following home bias acquisition announcements, consistent with familiarity-driven optimism interpretations rather than explanations related to private benefits.

Our investment home bias findings complement Yonker (2016b), who finds that home state CEOs are less likely to lay off employees. We document the complementary finding that out-of-state CEOs are more likely to invest in their home states through acquisitions. More generally, our findings of a familiarity-oriented birth state home bias are consistent with evidence from mutual funds managers and credit rating analysts (Pool, Stoffman, and Yonker, 2012; Cornaggia, Cornaggia, and Israelsen, 2015), and support the interpretation that familiarity can lead to misplaced confidence in the success of an acquisition.

Off-style Holdings of Mutual Funds

Kiseo Chung*

Abstract

I test whether mutual funds hold stocks that do not match their stated investment style on a regular basis, and explore the motivation behind such holdings. I find that funds hold a significant portion of their holdings in stocks that do not match their stated investment style (20%-35%) which is consistent with S.E.C. regulation 35d. The reason for holding "Off-style" stocks could be because of information sharing between funds or "co-insruance" between funds. I find evidence that supports both views.

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1 Introduction

A mutual fund's stated investment style works as a signal to investors of what type of securities the fund will invest in. However, past literature on mutual fund styles finds evidence that mutual funds game their stated investment style to achieve better performance through setting a benchmark index that does not reflect their holdings (Sensoy (2009)) or exploiting suboptimal behavior of mutual fund investors by changing the fund name (Cooper, Gulen, and Rau (2005)). The question of whether mutual funds hold stocks that deviate from their stated investment style on a regular basis and, if so, what drives this behavior has not been studied. This paper addresses that gap.

I find that the percentage of mutual fund holdings that are not in line with their stated investment style constitutes a sizable chunk of the combined Total Net Assets of all funds in each cross section. This percentage dropped from around 35% in the early 1990's to around 20% in 1997, and has remained relatively constant since. The fact that mutual funds hold a majority of their assets in their stated investment style is consistent with S.E.C. rule 35d. Moreover, the decrease in the percentage of holdings that are not in line with the stated investment style aligns with a rule change that occurred in 1997. Before 1997, S.E.C. rule 35d required that at least 65% of the mutual funds' assets be invested in their stated investment style. However, this threshold increased to 80% through an amendment to the original S.E.C. Rule in 1997. It seems that while mutual funds on average abide by the S.E.C. ruling, they also try to maximize the unregulated portion, which I call "off-style holdings" going forward.

Given that funds have a limited amount of resources and that they can relatively freely invest with this off-style portion, it is of interest to understand how mutual funds utilize this buffer. Two potential reasons why mutual funds would hold off-style holdings are "co-insurance" and information sharing. Goncalves-Pinto and Schmidt (2015) find that funds in the same fund family participate in what they term a "co-insurance" strategy where other funds in the same family umbrella absorb the negative impact of a fire sale by purchasing stocks that are sold by funds that are distressed. In an effort to mitigate the impact of an asset fire sale, it could be possible that even funds of different investment styles are asked to participate in a co-insurance strategy. The other potential explanation for why mutual funds hold off-style stocks is private information sharing between funds. Hong, Kubik, and Stein (2005) find that there is information flow between neighboring fund managers that leads to positive performance. It could be possible that managers in the same fund family provide private information about stocks that match their investment style to other managers under the same family umbrella.

If co-insurance is reason for funds to hold off-style holdings, I expect the other funds in the same fund family as a distressed fund would utilize their off-style portions to obtain stocks held by distressed funds. Given that this portion of assets is not regulated by the SEC, it provides fund families with a wider range of funds to utilize when some of their funds are experiencing extreme outflow rather than having to use only the same investment style funds.

I first document evidence that is consistent with previous literature that documents the existence of co-insurance in the mutual fund indsutry. If mutual funds utilize their on-style portion of assets as co-insurance while using the off-style portion for other purposes, I expect to see a majority of the increase in holdings of fire sale stocks by funds in the same fund family to be done by funds with the same stated investment style as fire sale stocks. For example, a large cap fund would buy a large cap stock sold by a distressed large cap fund. I find that, on average, 78.8 % of the dollar value of fire sale stocks bought by other funds in the same fund family are from the same investment style. This provides evidence that mutual funds use not only their on-style portion to acquire fire sale stocks from distressed funds, but also use, non-regulated off-style portion.

If information sharing by fund managers is a reason for the holding of off-style stocks, I expect these stocks to outperform on-style stocks that are of a same style. For example, I expect large cap stocks held by mid or small cap fund to outperform large cap stocks held by large cap fund. The idea behind this is that if there is information sharing between funds, information about which large cap stock are expected to outperform will be shared to mid or small cap funds by large cap funds and these would outperform average large cap stocks held by these large cap funds.

I find that while large and mid-cap stock portfolios' performance does not statistically differ between on and off-style portfolios, the performance of the off-style small cap stock portfolio is significantly greater than that of the on-style small cap portfolio. The monthly Four Factor Alpha of the small cap off-style stock portfolio provides 30 basis points on a monthly basis and 3.6% on an annual basis while the small cap on-style stock portfolio provides only 23 basis points on a monthly basis, with the difference being statistically significant at the 10% level. So at least in terms of small cap stocks, there seems to be some information sharing between funds. This is consistent with off-style stocks being used to utilize private information sharing by fund managers.

Overall, I find results that are consistent with off-style holdings being held due to both private information sharing by fund managers, especially for small cap stocks, and co-insurance for the distressed fund in the same fund family. Given that this portion of assets is not regulated by the SEC, mutual funds seem to utilize them for multiple purposes which help increase performance or a buffer for significant underperformance.

My study is related to two strands of literature. First, I contribute to the literature on mutual fund styles. Past literature has documented that there are benefits to style investing such as helping investors evaluate the performance of their mutual funds(e.g., Chan, Chen, and Lakonishok (2002), Barberis and Shleifer (2003)). On the other hand, recent studies find evidence of style gaming by mutual funds either through a fund name change that is not immediately followed by holdings change or through specifying a benchmark index that does not correctly represent their holdings (e.g., Cooper et al. (2005), Sensoy (2009)). Empirical evidence on the performance of funds that deviate from their investment style is mixed (e.g. Wermers (2002), Brown, Harlow, and Zhang (2016)). The paper most closely related to the current study is Cremers and Petajisto (2009), where they use deviation of fund holdings from their closest benchmark index to measure the activeness of each mutual fund. The main difference between my measure and theirs is that I focus on style deviations. For example, their Active Share measure includes all deviations in terms of percentage of assets even in cases where a large cap fund is holding a large cap stock. In this paper, I only focus on stocks that deviate from a fund's stated investment style. Moreover, I document the underlying reasons for why mutual funds hold stocks that are not consistent with their stated investment style.

Second, I contribute to the general literature on fund manager ability. The past literature in fund manager ability finds that fund managers on average do not outperform their benchmarks (e.g., Carhart (1997)). However, more recent studies find evidence of fund manager ability in specific cases where the power of the test in identifying manager ability is greater. Chen, Jegadeesh, and Wermers (2000) find that stocks that are bought by mutual funds outperform stocks that are sold by mutual funds, while stocks that are widely held do not outperform the least widely held stocks. Alexander, Cici, and Gibson (2007) find that stock purchases by fund managers who experience large outflow outperform. Baker, Litov, Wachter, and Wurgler (2010) find evidence of fund manager stock picking ability by looking at holdings and trades prior to earnings announcements. In this paper, I confirm recent findings that while fund managers do not outperform their benchmark on average, they do posses some stock picking ability when focusing on a subset of stocks in which they have high confidence.

The remainder of the paper proceeds as follows. In Section 2, I describe the dataset. In section 3, I provide the main results. Section 4 concludes.

2 Data

2.1 Sample Selection

My primary source of mutual fund data is Morningstar, Inc.[1] I focus on actively managed U.S. domestic open-end equity mutual funds. Following Elton, Gruber, and Blake (1996), I require funds to satisfy a certain lower bound of total net assets (TNA) to alleviate concerns regarding return outliers. I modify Elton et al.'s criteria as described in the first chapter of this dissertation. If a fund has multiple share classes, I value weighted across share classes. In the final sample, I have 4,465 unique funds for the sample period of 1992 to 2014.

Mutual fund holdings data is from Thomson Financial (also known as CDA/Spectrum S12). The main source of Thomson Financial data is periodic filings by mutual funds to the SEC (N-30D form). Prior to 1985, the SEC required each fund to report its portfolio holdings every quarter, but the requirement changed to semiannual starting in 1985. However, the majority of funds continued to report every quarter and the SEC returned to the quarterly reporting requirement in February 2004. Further details on the construction of the Thomson Financial database are available in Wermers (2002).

In order to utilize mutual fund data from Morningstar with holdings data from Thomson Financial, it is necessary that I construct a map between the Morningstar, CRSP and Thomson databases. I follow the methodology provided in Berk and Van Binsbergen (2015) and Patel and Sarkissian (2015) in mapping between Morningstar and CRSP MFDB. I independently mapped CRSP MFDB to Morningstar Principia CDs, and then Morningstar Direct to Morningstar Principia CDs using monthly returns, TNA, CUSIP, Ticker, fund names, and dividends to map the datasets. In the end, I was able to map 90.2% of fund-month

[1]Patel and Sarkissian (2015) find that managerial structure data on Morningstar Direct matches Securities and Exchange Commission (S.E.C.) filings 96% of the time and recommend using Morningstar Direct data for mutual fund manager specific analysis.

observations in Morningstar to the CRSP MFDB. Then, I used MFLINKS from Wharton Research Data Services (WRDS) to map Morningstar Data to the Thomson mutual fund holdings database. I randomly selected funds from my mapping and verified that my mapping is robust. Lastly, I then mapped each holding with CRSP and Compustat data to obtain returns, size, book-to-market ratios, and other firm specific variables.

Given that the S.E.C. regulation is based on the cap size of a fund for U.S. domestic equity funds, I define the percentage of holdings that are not consistent with their stated cap as off-style holdings. For example, the small- or mid-cap stock holdings of a large-cap fund would be categorized as off-style holdings.

2.2 Sample Summary Statistics

Table 26 reports the summary statistics of fund attributes for the funds in my sample. My sample consists of 4,465 unique funds for the sample period of 1992 to 2014. As can be seen from Table 26, more than half of the funds are categorized as large cap funds as Size Category equals 1 for large cap funds, 2 for mid cap funds, and 3 for small cap funds. Both Fund Size and Family Size is similar in magnitude with previous literature. Average Stock held represents the number of stocks held by each fund and this shows that on average each fund holds a little more than 100 stocks in its portfolio. Style Match Pct. is the percentage of assets that match in investment style with stated investment style of each fund. It shows that, on average, 64% of a fund's assets fall in the same investment style as the fund's stated investment style.

3 Results

3.1 Reason for "Off-style Holdings"

S.E.C. Rule 35d and 35d-1 requires that each fund has a significant portion of its assets invested in investments indicated by the name of the fund, with the restriction changing from 65 percent to 80 percent in 1997.[2] In terms of domestic equity mutual funds, the funds that have "cap" based names (Large Cap, Mid Cap and so on) must abide by the rule and specify their definition of "cap" in their prospectus. As a result, I focus on domestic equity funds that have a size categorization.

In this section, I first see whether mutual funds on average abide by S.E.C. Rule 35d rule and how they reacted to the rule change in 1997. I first calculate the dollar value of stocks in each mutual fund's holdings that do not match its stated investment style. Then, for each quarter, I value-weight the percentage of these off-style holdings. The result can be seen in figure 1. As can be seen in the figure, the percentage of off-style stocks held by mutual funds in aggregate hovers around 35% in the early part of the period before 1997, while the percentage drops significantly in 1997 and stays stable at 20% going forward. This shows that, on average, mutual funds are following the rule given by the S.E.C.. However, while they are following the rule, they are utilizing the off-style portion to its maximum. It would be of interest to find out how mutual funds are utilizing this portion.

Two potential reasons why mutual funds would hold off-style stocks are co-insurance and information sharing. Goncalves-Pinto and Schmidt (2015) find that funds in the same fund family participate in a "co-insurance" strategy where other funds in the same family absorb the negative impact of a fire sale by purchasing stocks that are sold by funds that are distressed. In an effort to mitigate the impact of an asset fire sale, it could be possible that even funds of different investment styles are asked to participate in this strategy. If

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[2] A detailed description of how the rule changed over time and how it applies to specific funds can be found in Barnett (2005)

co-insurance is a reason for the holding of off-style stocks, I expect that other funds in the same fund family as a distressed fund would absorb not only the sale of fire sale stocks that do match their stated investment style but also those that do not. This would provide fund families with wider range of funds to utilize when some of its funds are experiencing extreme outflow, rather than having to use only the same investment style funds.

In order to test this, I focus on stocks that were sold by distressed funds. If the holdings of a stock held by a distressed fund decreased in dollar value by more than 25% during the quarter the fund was experiencing an extreme outflow, I categorize them as fire sale stocks. Then I look at other funds in the same fund family as the distressed fund and see if these other funds increase their holdings of these fire sale stocks during the same quarter. I find that 38.3% of the value of fire sale stocks sold by distressed funds are absorbed by other funds in the same fund family during the same quarter. This rate is significantly higher than the rate in which other funds buy stocks of distressed funds that are not fire sale stocks, which is only 9.4%. This provides some evidence of the existence of co-insurance in the mutual fund industry.

If mutual funds utilize their on-style portion of assets to absorb stocks sold by distressed funds while using off-style portion for other purposes, I expect to see majority of increase in holdings of fire sale stocks by funds in the same fund family to be done by funds with the same stated investment style as fire sale stocks. For example, a large cap fund would buy a large cap stock sold by a distressed large cap fund. I find that on average, 78.8% of the dollar value of fire sale stocks bought by other funds in the same fund family are from the same investment style. This provides evidence that mutual funds use both their on- and off-style portions to acquire fire sale stocks from distressed funds.

The other potential explanation for why mutual funds hold off-style holdings is private information sharing between funds. Hong et al. (2005) find that there is information flow between neighboring fund managers that leads to positive performance. Although there is no evidence in the previous mutual fund literature that documents within fund family information sharing, it could be possible that managers in the same fund family provide private information about their own style stocks to other managers under the same family umbrella. If information sharing by fund managers in the same fund family is a reason for the holding of off-style stocks, I expect these stocks to outperform.

In order to test whether holdings of off-style stocks by mutual funds are due to information sharing, I first divide the off-style stocks into three groups, large, mid and small cap based on market capitalization. The large cap group consists of large cap stocks held by mid or small cap funds. Likewise, the mid cap group consists of mid cap stocks held by small or large cap funds, and the small cap group consists of small cap stocks held by mid or large cap funds. As a benchmark with which to compare the performance of these stocks, I construct three additional portfolios of stocks that are in line with the stated investment style of the fund. The idea behind this is that if there is information sharing between funds, large cap stocks held by mid or small cap funds will outperform average large cap stock held by large cap funds.

I find that while large and mid-cap stock portfolios' performance does not statistically differ between on and off-style portfolios, the performance of the off-style small cap stock portfolio is significantly greater than that of the on-style small cap portfolio. As can be seen from Table 27, the monthly Four Factor Alpha of the small cap off-style stock portfolio provides 30 basis points on a monthly basis and 3.6% on an annual basis while the small cap on-style stock portfolio provides only 23 basis points on a monthly basis, with the difference being statistically significant at the 10% level. So at least in terms of small cap stocks, there seems to be some information sharing between funds. This is consistent with off-style stocks being used to utilize private information sharing by fund managers.

4 Conclusion

In this paper, I test whether mutual funds hold stocks that do not match their stated investment style on a regular basis, and explore the motivation behind such holdings. I find that mutual funds hold a significant portion of their assets in stocks that do not match their stated investment style, with the percentage shifting from 35% in 1997 to 20% thereafter in accord with the change in S.E.C. regulations governing where to invest their assets. S.E.C. rule 35d and 35d-1 stipulated that mutual funds invest a majority of their assets in stocks of their stated investment style. With the rule change happening in 1997 that increased the minimum amount required from 65% to 80%, I find that mutual funds in aggregate abide by the rule very strictly, with the off-style portion hovering around 35% pre-1997 and dropping to a stable 20% afterwards.

Given that the investments of mutual funds are highly regulated by the S.E.C., it is of interest to understand how mutual funds utilize their off-style holdings. Two potential reasons why mutual funds would hold off-style holdings are co-insurance and information sharing. If co-insurance is a reason for such holdings, I expect mutual funds to utilize their off-style portion of assets to absorb the sale of fire sale stocks by distressed funds in the same fund family. If information sharing by fund managers in the same fund family is a reason for the holding of off-style stocks, I expect these stocks to outperform. I find results that are consistent with both explanations.

This paper is the first to document that a significant portion of mutual funds' assets are invested in stocks that do not match their investment style. This is by no means illegal and mutual funds on average do strictly abide by the restriction given by the S.E.C.. However, at the same time, mutual funds do invest the maximum portion of their assets allowed by the S.E.C. in stocks that do not match their stated investment style. I document that the reason behind these off-style holdings is due to both co-insurance and information sharing between fund managers, which is a novel finding in relation to previous literature that has mostly focused on competition between managers in the same fund family. Moreover, this paper adds to the recent literature that finds that fund managers do have ability in picking stocks in certain scenarios.

Table 1

Summary Statistics

The table reports the number of observations, mean, median, 25th and 75th percentile and standard deviation of mutual fund manager and mutual fund variables for the sample period of 1992 to 2014. Panel B reports the full sample summary statistics. Panel B (Panel C) reports summary statistics of junior (seasoned) fund managers and funds managed by them. I categorize each mutual fund manager as junior or seasoned using 40th or 60th percentile of each cross sectional age as the cutoff point. Panel D (Panel E) reports summary statistics for fund managers and funds during the Post-2000 period and the Pre-2000 period. All statistics are time-series averages of yearly cross-sectional statistics. A detailed description of each variable is included in Appendix A.

Panel A : Full Sample

	N	Mean	Std. Dev.	25th Pct.	Median	75th Pct.
Age	35398	46.18	9.38	39.08	44.92	52.33
Termination	35398	0.13	0.33	0	0	0
Age60+	35398	0.09	0.29	0	0	0
Female	35398	0.09	0.29	0	0	0
CFA	35398	0.6	0.49	0	1	1
MBA	35398	0.56	0.5	0	1	1
Flow	26413	0.07	0.56	-0.16	-0.05	0.12
Tracking Error - 4 Factor	26413	1.19	0.7	0.71	1.02	1.47
Log Size	26413	19.55	1.64	18.34	19.46	20.69
Fam Size	26413	23.32	2.23	21.99	23.75	24.9

Panel B : Junior Managers

	N	Mean	Std. Dev.	25th Pct.	Median	75th Pct.
Age	13825	37.44	3.79	34.83	37.75	40.08
Termination	13825	0.12	0.33	0	0	0
Female	13825	0.1	0.3	0	0	0
CFA	13825	0.63	0.48	0	1	1
MBA	13825	0.55	0.5	0	1	1
Flow	7206	0.09	0.61	-0.17	-0.04	0.14
Tracking Error - 4 Factor	7206	1.21	0.73	0.7	1.03	1.49
Log Size	7206	19.63	1.61	18.46	19.58	20.74
Fam Size	7206	23.7	2.13	22.48	24.11	25.12

Table 1 (Cont'd)
Summary Statistics

Panel C : Seasoned Managers

	N	Mean	Std. Dev.	25th Pct.	Median	75th Pct.
Age	14213	55.47	6.37	50.42	54.17	59.58
Termination	14213	0.12	0.33	0	0	0
Female	14213	0.09	0.28	0	0	0
CFA	14213	0.55	0.5	0	1	1
MBA	14213	0.56	0.5	0	1	1
Flow	11682	0.06	0.55	-0.16	-0.05	0.11
Tracking Error - 4 Factor	11682	1.18	0.68	0.71	1.01	1.46
Log Size	11682	19.57	1.68	18.31	19.46	20.72
Fam Size	11682	23.14	2.29	21.68	23.56	24.79

Panel D : Pre-2000 Period

	N	Mean	Std. Dev.	25th Pct.	Median	75th Pct.
Age	8249	44.87	9.21	37.5	43.58	52.33
Termination	8249	0.07	0.26	0	0	0
Age60+	8249	0.06	0.23	0	0	0
Female	8249	0.09	0.29	0	0	0
CFA	8249	0.58	0.49	0	1	1
MBA	8249	0.59	0.49	0	1	1
Flow	6512	0.14	0.6	-0.13	-0.01	0.22
Tracking Error - 4 Factor	6512	1.56	0.93	0.9	1.3	1.95
Log Size	6512	19.43	1.6	18.22	19.3	20.54
Fam Size	6512	22.81	2.24	21.4	23.27	24.45

Panel E : Post-2000 Period

	N	Mean	Std. Dev.	25th Pct.	Median	75th Pct.
Age	27149	46.58	9.4	39.58	45.17	52.33
Termination	27149	0.14	0.35	0	0	0
Age60+	27149	0.1	0.31	0	0	0
Female	27149	0.09	0.29	0	0	0
CFA	27149	0.61	0.49	0	1	1
MBA	27149	0.56	0.5	0	1	1
Flow	19901	0.05	0.55	-0.17	-0.06	0.09
Tracking Error - 4 Factor	19901	1.07	0.56	0.66	0.95	1.34
Log Size	19901	19.59	1.65	18.38	19.52	20.74
Fam Size	19901	23.49	2.2	22.23	23.93	25.01

Table 2

Change in Flow-Performance Relationship

The table reports estimates of the growth rate of net new money (FLOW) regressed on fund manager and fund characteristics, similar to the specification of Sirri and Tufano (2002). I use Prospectus Objective group as a group in which performance ranking is calculated for each fund. A fractional performance rank of the fund relative to other funds within the group in the same period(RANK) is categorized either into three (Low, Mid, and High Performer) groups, or into five groups by retaining the Low and High Performer groups and subdividing the Mid group into Mid-Low, Mid-Mid, and Mid-High Performers. Low, Mid, and High Performers are defined as Min(RANK, 0.2), Min(RANK-Low Performer, 0.6), and RANK - Low Performer - Mid Performer respectively. Mid-Low, Mid-Mid, and Mid-High are defined as Min(RANK-Low Performer, 0.2), Min(RANK-Low Performer-Mid-Low Performer, 0.2), and Min(RANK-Low Performer-Mid-Low Performer-Mid-Mid Performer, 0.2). All standard errors are clustered at the fund and year levels, and the resulting standard errors are reported in brackets. A detailed description of each variable is included in Appendix A.

	Pre-2000		Post-2000		Full Sample	
	(1)	(2)	(3)	(4)	(5)	(6)
Flow Cat.	0.7531***	0.7526***	1.1820***	1.1821***	1.0261***	1.0262***
	(0.1219)	(0.1222)	(0.1167)	(0.1166)	(0.1145)	(0.1145)
Ret. Std.	0.0974	0.0952	-0.0476	-0.0480	0.1530	0.1531
	(0.5693)	(0.5746)	(0.5149)	(0.5150)	(0.3989)	(0.3992)
Low Performer	0.2918	0.3553	0.3956***	0.3548***	0.6107**	0.5938**
	(0.2742)	(0.3005)	(0.0964)	(0.0798)	(0.2906)	(0.2911)
Low Perf X Post-2000					-0.2833	-0.2839
					(0.3082)	(0.3083)
Mid Performer	0.4322***		0.2341***		0.2790***	
	(0.0447)		(0.0298)		(0.0296)	
Mid-Low Performer		0.3480*		0.2832***		0.2984***
		(0.2066)		(0.0593)		(0.0649)
Mid-Mid Performer		0.4809***		0.2144***		0.2759***
		(0.1825)		(0.0566)		(0.0639)
Mid-High Performer		0.4388***		0.2171**		0.2651***
		(0.1287)		(0.0905)		(0.0750)
High Performer	2.2176***	2.2003***	1.3294***	1.3489***	2.6204***	2.6326***
	(0.3741)	(0.3654)	(0.3142)	(0.3347)	(0.3158)	(0.3170)
High Perf X Post-2000					-1.4166***	-1.4162***
					(0.4496)	(0.4500)
Post-2000					0.0705	0.0706
					(0.0521)	(0.0522)
Log Size	-0.0481***	-0.0481***	-0.0500***	-0.0500***	-0.0493***	-0.0493***
	(0.0063)	(0.0064)	(0.0076)	(0.0076)	(0.0059)	(0.0059)
Family Size	0.0176***	0.0176***	0.0158***	0.0158***	0.0170***	0.0170***
	(0.0048)	(0.0047)	(0.0033)	(0.0033)	(0.0028)	(0.0028)
Expense	7.4788*	7.4798*	-6.1171***	-6.1087***	-2.1470	-2.1436
	(4.1915)	(4.1892)	(1.0172)	(1.0237)	(2.0323)	(2.0319)
Intercept	0.3071*	0.3031*	0.6041***	0.6074***	0.4283***	0.4296***
	(0.1732)	(0.1761)	(0.1022)	(0.1022)	(0.1095)	(0.1101)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Squared	0.125	0.125	0.076	0.076	0.091	0.091

Table 3

Change in Termination Probability

The table reports logit regressions of termination on fund manager and fund characteristics. I define termination at a fund manager level. If a fund manager manages fewer number of funds the year after or the total TNA of funds the manager manages have decreased by more than 30% in their last year in the data while managing same number of funds, I define this manager as being demoted/terminated and assign 1 for that year and 0 otherwise. However, if the total TNA for the manager increased by more than 30%, I assign 0 to account for the fact that the manager left for better career opportunities. Standard errors are double clustered at at the manager and year levels. Resulting standard errors are reported in brackets. A detailed description of each variable is included in Appendix A.

	Manager Level Termination (Value Weighted)					
	Pre-2000		Post-2000		Full Sample	
	(1)	(2)	(3)	(4)	(5)	(6)
$Alpha_t$	0.0422	-0.0216	-0.2141***	-0.2903***	0.0635	0.0087
	(0.0951)	(0.0641)	(0.0668)	(0.1014)	(0.1018)	(0.0576)
$Alpha_t$ * *Post-2000*					-0.2815**	-0.3063***
					(0.1214)	(0.1117)
Post-2000					0.7076***	0.7464***
					(0.1503)	(0.1527)
$Alpha_{t-1}$	-0.2964***	-0.2660***	-0.3687***	-0.4934***	-0.3558***	-0.4336***
	(0.0717)	(0.0635)	(0.0838)	(0.0746)	(0.0683)	(0.0659)
$Alpha_{t-2}$	-0.2643***	-0.4310***	-0.1889***	-0.3433***	-0.2049***	-0.3546***
	(0.0816)	(0.1564)	(0.0372)	(0.0485)	(0.0360)	(0.0480)
$Alpha_{t-3}$	-0.2997**	-0.1738**	-0.1222***	-0.1401***	-0.1445***	-0.1431***
	(0.1243)	(0.0831)	(0.0383)	(0.0447)	(0.0387)	(0.0421)
Log Size	0.0820*	0.0911*	0.0189	0.0228	0.0284	0.0324
	(0.0425)	(0.0500)	(0.0233)	(0.0225)	(0.0209)	(0.0205)
Fam Size	0.1206***	0.1212***	0.1039***	0.1009***	0.1039***	0.1007***
	(0.0314)	(0.0324)	(0.0216)	(0.0209)	(0.0191)	(0.0185)
$Age60+$	0.2134	0.1710	-0.0116	0.0229	0.0096	0.0378
	(0.2624)	(0.2836)	(0.0891)	(0.0869)	(0.0842)	(0.0821)
Abnormal Flow	-0.1756	-0.1431	-0.2822***	-0.2538***	-0.2592***	-0.2309***
	(0.1228)	(0.1443)	(0.0741)	(0.0592)	(0.0534)	(0.0419)
$Intercept$	-6.6488***	-6.6656***	-4.4460***	-4.3250***	-5.3270***	-5.2436***
	(0.8412)	(0.8916)	(0.5395)	(0.5215)	(0.4602)	(0.4490)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Obj. Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Psuedo R-Squared	0.0431	0.0468	0.0229	0.0356	0.0319	0.0427

Table 4

Change in Risk Taking

The table reports estimates of risk measures regressed on values of time. The dependent variable is Tracking Error. Specifications (1), (3), and (5) use Fama-French-Carhart 4 Factor model and the other three use a combination of the primary prospectus benchmark and the S&P 500 as their benchmarks in calculating Tracking Error. The first two specifications use a time-series regression with Newey-West standard errors with 4 lags. The last four specifications use panel regression with standard errors clustered at year level. Resulting standard errors are reported in parenthesis. A detailed description of each variable is included in Appendix A.

	(1)	(2)	(3)	(4)	(5)	(6)
Quarter	-0.0050**	-0.0093***				
	(0.0020)	(0.0025)				
Post-2000			-0.4909**	-0.5601**	-0.8839***	-0.9641***
			(0.2157)	(0.2304)	(0.3113)	(0.3331)
Constant	1.4978***	1.9985***	1.6088***	2.7209***	2.1565***	3.0438***
	(0.1514)	(0.2050)	(0.2024)	(0.3582)	(0.2970)	(0.4416)
Controls	No	No	No	Yes	No	Yes
Adj. R-Squared	0.1155	0.1952	0.0796	0.1586	0.1186	0.1800

Table 5

Change in Flow-Performance Relationship By Experience

The table reports estimates of the growth rate of net new money (FLOW) regressed on fund manager and fund characteristics, similar to the specification of Sirri and Tufano (2002) for each experience group. I use the Prospectus Objective group as a group in which performance ranking is calculated for each fund. A fractional performance rank of the fund relative to other funds within the group in the same period(RANK) is categorized either into three (Low, Mid, and High Performer) groups, or into five groups by retaining the Low and High Performer groups and subdividing the Mid group into Mid-Low, Mid-Mid, and Mid-High Performers. All standard errors are clustered the fund and year levels and resulting standard errors are reported in brackets. A detailed description of each variable is included in Appendix A.

	Junior		Seasoned	
	(1)	(2)	(3)	(4)
Flow Cat.	1.0036***	1.0067***	1.0811***	1.0812***
	(0.1032)	(0.1028)	(0.1575)	(0.1573)
Ret. Std.	-0.0336	-0.0329	0.0879	0.0886
	(0.4793)	(0.4803)	(0.4750)	(0.4741)
Low Performer	0.9218***	0.7446**	0.5743	0.6301
	(0.3576)	(0.3324)	(0.3891)	(0.4064)
Low Performer X Post-2000	-0.5375	-0.5409	-0.3449	-0.3431
	(0.3867)	(0.3892)	(0.4605)	(0.4591)
Mid Performer	0.2788***		0.2879***	
	(0.0460)		(0.0418)	
Mid-Low Performer		0.5038***		0.2179***
		(0.1221)		(0.0807)
Mid-Mid Performer		0.1772		0.3202***
		(0.1667)		(0.0879)
Mid-High Performer		0.2176		0.3055**
		(0.1632)		(0.1310)
High Performer	2.6791***	2.7529***	2.4706***	2.4479***
	(0.4360)	(0.4470)	(0.5983)	(0.6040)
High Performer X Post-2000	-1.3617**	-1.3589**	-1.3772**	-1.3774**
	(0.5676)	(0.5690)	(0.6566)	(0.6567)
Post-2000	0.0735	0.0744	0.1232	0.1229
	(0.0672)	(0.0677)	(0.0763)	(0.0761)
Log Size	-0.0547***	-0.0546***	-0.0426***	-0.0426***
	(0.0061)	(0.0061)	(0.0069)	(0.0069)
Family Size	0.0152***	0.0151***	0.0141***	0.0141***
	(0.0038)	(0.0038)	(0.0042)	(0.0042)
Expense	0.7110	0.7284	-2.4233	-2.4377
	(2.2480)	(2.2351)	(2.1731)	(2.1699)
Intercept	0.5271***	0.5393***	0.3464***	0.3425***
	(0.1487)	(0.1490)	(0.1224)	(0.1228)
Controls	Yes	Yes	Yes	Yes
Adj. R-Squared	0.0971	0.0975	0.0900	0.0901

Table 6

Relative Difference in Flow-Performance Relationship By Experience

The table reports estimates of the growth rate of net new money (FLOW) regressed on fund manager and fund characteristics, similar to the specification of Sirri and Tufano (2002) for two different sub-periods. I use the Prospectus Objective group as a group in which performance ranking is calculated for each fund. A fractional performance rank of the fund relative to other funds within the group in the same period(RANK) is categorized either into three (Low, Mid, and High Performer) groups, or into five groups by retaining the Low and High Performer groups and subdividing the Mid group into Mid-Low, Mid-Mid, and Mid-High Performers. All standard errors are clustered at fund and year level and resulting standard errors are reported in brackets. A detailed description of each variable is included in Appendix A.

	Pre-2000		Post-2000	
	(1)	(2)	(3)	(4)
Flow Cat.	0.7488***	0.7521***	1.1724***	1.1729***
	(0.1173)	(0.1173)	(0.1205)	(0.1201)
Ret. Std.	0.0486	0.0682	-0.0478	-0.0587
	(0.5656)	(0.5660)	(0.5269)	(0.5270)
Low Performer	0.2929	0.3016	0.3921***	0.3918***
	(0.2734)	(0.2746)	(0.0959)	(0.0962)
Mid Performer	0.4376***	0.4369***	0.2337***	0.2340***
	(0.0443)	(0.0448)	(0.0297)	(0.0296)
High Performer	2.1865***	2.4539***	1.3450***	1.5761***
	(0.3774)	(0.6546)	(0.3157)	(0.3969)
High Performer * $(Exp - \overline{Exp})$	-0.0124		-0.0064	
	(0.0218)		(0.0082)	
*High Performer * Exp Group*		-0.1287		-0.1161
		(0.3321)		(0.1056)
$(Exp - \overline{Exp})$	-0.0023***		0.0010**	
	(0.0008)		(0.0005)	
Exp Group		-0.0303***		0.0071
		(0.0101)		(0.0052)
Log Size	-0.0475***	-0.0477***	-0.0509***	-0.0506***
	(0.0064)	(0.0065)	(0.0076)	(0.0075)
Family Size	0.0155***	0.0156***	0.0164***	0.0159***
	(0.0048)	(0.0047)	(0.0033)	(0.0032)
Expense	7.2050*	7.1948*	-6.1631***	-6.1208***
	(4.2444)	(4.2230)	(1.0340)	(1.0328)
Intercept	0.3554**	0.4184**	0.6072***	0.5986***
	(0.1722)	(0.1811)	(0.1021)	(0.1026)
Controls	Yes	Yes	Yes	Yes
Adj. R-Squared	0.077	0.077	0.085	0.115

Table 7

Change in Termination Probability by Experience

The table reports logit regressions of termination on fund manager and fund characteristics for junior and seasoned managers separately. I define termination at a fund manager level. If a fund manager manages fewer number of funds the year after or the total TNA of funds the manager manages have decreased by more than 30% in their last year in the data while managing same number of funds, I define this manager as being demoted/terminated and assign 1 for that year and 0 otherwise. However, if the total TNA for the manager increased by more than 30%, I assign 0 to account for the fact that the manager left for better career opportunities. Standard errors are double clustered at at the manager and year levels. Resulting standard errors are reported in brackets. A detailed description of each variable is included in Appendix A.

	Manager Level(Value Weighted)			
	Junior Managers		Seasoned Managers	
	(1)	(2)	(3)	(4)
$Alpha_t$	-0.0916	-0.2285***	0.1006	0.1323
	(0.1134)	(0.0709)	(0.1489)	(0.1741)
$Alpha_t$ * *Post-2000*	0.0438	0.0218	-0.3696**	-0.4702**
	(0.1698)	(0.1295)	(0.1576)	(0.2021)
Post-2000	0.5421***	0.5217**	0.7777***	0.8643***
	(0.2013)	(0.2036)	(0.1591)	(0.1443)
$Alpha_{t-1}$	-0.3493***	-0.3954***	-0.4011***	-0.4821***
	(0.0781)	(0.0732)	(0.0783)	(0.0766)
$Alpha_{t-2}$	-0.1923***	-0.2931***	-0.2586***	-0.3483***
	(0.0566)	(0.0608)	(0.0571)	(0.0721)
$Alpha_{t-3}$	-0.1281**	-0.1529***	-0.1098**	-0.1410**
	(0.0614)	(0.0487)	(0.0556)	(0.0656)
Log Size	0.0324	0.0330	-0.0085	-0.0018
	(0.0366)	(0.0370)	(0.0238)	(0.0235)
Fam Size	0.1390***	0.1377***	0.1236***	0.1161***
	(0.0360)	(0.0349)	(0.0186)	(0.0171)
$Age60+$			-0.0181	0.0041
			(0.0970)	(0.0922)
Abnormal Flow	-0.1354**	-0.1228**	-0.2314**	-0.2075***
	(0.0599)	(0.0565)	(0.0946)	(0.0672)
$Intercept$	-6.0769***	-5.9313***	-4.9668***	-4.8662***
	(0.7744)	(0.7660)	(0.4729)	(0.4657)
Controls	Yes	Yes	Yes	Yes
Cat. Dummy	Yes	Yes	Yes	Yes
Psuedo R-Squared	0.0306	0.0375	0.0395	0.0523

Table 8

Relative Difference in Termination Probability by Experience

The table reports logit regressions of termination on fund manager and fund characteristics for the pre- and post-2000 period. I define termination at a fund manager level. If a fund manager manages fewer number of funds the year after or the total TNA of funds the manager manages have decreased by more than 30% in their last year in the data while managing same number of funds, I define this manager as being demoted/terminated and assign 1 for that year and 0 otherwise. However, if the total TNA for the manager increased by more than 30%, I assign 0 to account for the fact that the manager left for better career opportunities. Standard errors are double clustered at at the manager and year levels. Resulting standard errors are reported in brackets. A detailed description of each variable is included in Appendix A.

	Manager Level(Value Weighted)			
	Pre-2000		Post-2000	
	(1)	(2)	(3)	(4)
$Alpha_t$	0.0475	-0.1605	-0.1925***	0.0046
	(0.0873)	(0.1122)	(0.0723)	(0.1672)
$Alpha_t * (Exp - \overline{Exp})$	0.0016		-0.0081**	
	(0.0055)		(0.0035)	
$Alpha_t$ * *Exp Group*		0.1027**		-0.1013*
		(0.0493)		(0.0564)
$Alpha_{t-1}$	-0.3209***	-0.3192***	-0.3608***	-0.3601***
	(0.0721)	(0.0740)	(0.0882)	(0.0885)
$Alpha_{t-2}$	-0.2531***	-0.2520***	-0.1789***	-0.1782***
	(0.0774)	(0.0765)	(0.0361)	(0.0368)
$Alpha_{t-3}$	-0.2794**	-0.2754**	-0.1057***	-0.1052***
	(0.1245)	(0.1245)	(0.0402)	(0.0403)
Log Size	0.1055**	0.1062**	0.0132	0.0127
	(0.0504)	(0.0510)	(0.0251)	(0.0251)
Fam Size	0.1131***	0.1122***	0.1122***	0.1131***
	(0.0348)	(0.0342)	(0.0233)	(0.0232)
$(Exp - \overline{Exp})$	-0.0075		-0.0031	
	(0.0054)		(0.0043)	
Exp Group		-0.1079**		-0.0033
		(0.0536)		(0.0437)
$Age60+$	0.3230	0.2649	0.0070	-0.0429
	(0.3284)	(0.2773)	(0.1145)	(0.0953)
Abnormal Flow	-0.1369	-0.1369	-0.2669***	-0.2667***
	(0.1214)	(0.1209)	(0.0694)	(0.0695)
$Intercept$	-6.9218***	-6.7104***	-4.4785***	-4.4828***
	(0.9322)	(0.8850)	(0.5635)	(0.5236)
Controls	Yes	Yes	Yes	Yes
Cat. Dummy	Yes	Yes	Yes	Yes
Psuedo R-Squared	0.039	0.047	0.026	0.038

Table 9

Risk Taking and Experience

The table reports estimates of risk measures regressed on fund manager and fund characteristics. The dependent variable is Tracking Error. The first three specifications use Fama-French-Carhart 4 Factor model(FFC 4 Factor Model) and the next three use a combination of the primary prospectus benchmark and the S&P 500 as its benchmark in calculating Tracking Error. Exp group equals 1 (2) if a lead manager's age is below (above) the median of each cross section and 0 otherwise. Seasoned (Middle) equals 1 if the fund's lead manager's age is in the top (next) 40th (20th) percentile of each cross section and 0 otherwise. All specifications use panel regressions with Two-way clustered standard errors, clustered in both fund and year. Resulting standard errors are reported in parenthesis. A detailed description of each variable is included in Appendix A.

Panel A: Risk Taking - Post-2000

	FFC 4 Factor Model			Objective Index + S&P 500		
	(1)	(2)	(3)	(4)	(5)	(6)
Exp	-0.0215**			-0.0324**		
	(0.0099)			(0.0150)		
Exp Group		-0.1558**			-0.2589**	
		(0.0695)			(0.1041)	
Middle			-0.1147**			-0.1847***
			(0.0462)			(0.0680)
Seasoned			-0.2287**			-0.3404**
			(0.1095)			(0.1557)
Log Size	-0.0167***	-0.0205***	-0.0206***	-0.0154**	-0.0208***	-0.0211***
	(0.0043)	(0.0041)	(0.0041)	(0.0061)	(0.0059)	(0.0058)
Family Size	-0.0196***	-0.0177***	-0.0177***	-0.0310***	-0.0282***	-0.0284***
	(0.0048)	(0.0047)	(0.0046)	(0.0055)	(0.0053)	(0.0051)
CFA	-0.0424**	-0.0323*	-0.0327*	-0.0835***	-0.0699***	-0.0687***
	(0.0188)	(0.0183)	(0.0181)	(0.0216)	(0.0201)	(0.0197)
MBA	-0.0317	-0.0315	-0.0306	-0.0367	-0.0370	-0.0343
	(0.0227)	(0.0224)	(0.0223)	(0.0244)	(0.0234)	(0.0235)
Female	-0.0488	-0.0400	-0.0399	-0.0960***	-0.0836***	-0.0822***
	(0.0326)	(0.0325)	(0.0325)	(0.0300)	(0.0311)	(0.0307)
Intercept	3.7765***	1.7735***	2.0732***	3.2729***	3.2090***	2.5705***
	(0.8777)	(0.2514)	(0.1904)	(0.5435)	(0.3622)	(0.2051)
Obj. Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Cohort Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Squared	0.085	0.066	0.068	0.101	0.072	0.074

Table 9 (Cont'd)
Risk Taking and Experience

Panel B: Risk Taking - Pre-2000						
	FFC 4 Factor Model			Objective Index + S&P 500		
	(1)	(2)	(3)	(4)	(5)	(6)
Exp	0.0882***			0.1186***		
	(0.0234)			(0.0387)		
Exp Group		0.4276***			0.5791***	
		(0.1279)			(0.1858)	
Middle			0.3202***			0.4548**
			(0.1187)			(0.2106)
Seasoned			0.6657***			0.8368***
			(0.1778)			(0.2819)
Log Size	-0.0378***	-0.0310*	-0.0346**	-0.0554*	-0.0476	-0.0501
	(0.0135)	(0.0166)	(0.0156)	(0.0319)	(0.0365)	(0.0346)
Family Size	0.0138	0.0235*	0.0247*	0.0470***	0.0611***	0.0618***
	(0.0088)	(0.0121)	(0.0127)	(0.0135)	(0.0168)	(0.0168)
CFA	-0.0209	-0.0243	-0.0232	-0.0848	-0.0925	-0.0901
	(0.0350)	(0.0334)	(0.0338)	(0.0575)	(0.0585)	(0.0573)
MBA	-0.0406	-0.0749**	-0.0687**	0.0641	0.0232	0.0237
	(0.0308)	(0.0318)	(0.0308)	(0.0502)	(0.0440)	(0.0442)
Female	-0.0882**	-0.1414***	-0.1208***	-0.1334*	-0.2067**	-0.1799**
	(0.0407)	(0.0407)	(0.0429)	(0.0762)	(0.0822)	(0.0798)
Intercept	-3.8203***	3.0231***	1.7075***	-5.8599*	2.9260***	1.6204*
	(0.6770)	(0.3760)	(0.5188)	(3.1298)	(0.9368)	(0.9436)
Obj. Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Cohort Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Squared	0.224	0.149	0.154	0.158	0.109	0.110

Table 10

Risk Taking and Experience for Different Definitions of Lead Manager

The table reports estimates of risk measures regressed on fund manager and fund characteristics based on different definitions of who the lead manager is. The first three specifications use a different definition on who the lead manager is. Under this assumption, the lead manager is the manager who worked in the fund the longest(Lead II). The next three specifications use subset of funds that are managed by a single manager. The dependent variable is Tracking Error calculated using the Fama-French-Carhart 4 Factor model. Exp Group equals 1 (2) if a lead manager's age is below (above) the median of each cross section and 0 otherwise. Seasoned (Middle) equals 1 if the fund's lead manager's age is in the top (next) 40th (20th) percentile of each cross section and 0 otherwise. All specifications use panel regression with Two-way clustered standard errors, clustered in both fund and year. Resulting standard errors are reported in parenthesis. A detailed description of each variable is included in Appendix A.

Panel A: Risk Taking - Post-2000						
		Lead II			Lead III	
	(1)	(2)	(3)	(4)	(5)	(6)
Exp	-0.0207**			-0.0218**		
	(0.0100)			(0.0107)		
Exp Group		-0.1410**			-0.1880***	
		(0.0685)			(0.0595)	
Middle			-0.1173**			-0.1430**
			(0.0466)			(0.0651)
Seasoned			-0.2201**			-0.2921**
			(0.1117)			(0.1279)
Log Size	-0.0170***	-0.0207***	-0.0207***	-0.0147**	-0.0174***	-0.0193***
	(0.0043)	(0.0041)	(0.0042)	(0.0072)	(0.0064)	(0.0065)
Family Size	-0.0187***	-0.0167***	-0.0165***	-0.0082	-0.0045	-0.0040
	(0.0049)	(0.0048)	(0.0047)	(0.0067)	(0.0060)	(0.0059)
CFA	-0.0405**	-0.0302*	-0.0312*	-0.0441	-0.0358	-0.0377
	(0.0183)	(0.0181)	(0.0178)	(0.0282)	(0.0280)	(0.0271)
MBA	-0.0341	-0.0355	-0.0344	-0.0498	-0.0562*	-0.0526*
	(0.0229)	(0.0229)	(0.0226)	(0.0320)	(0.0324)	(0.0317)
Female	-0.0675**	-0.0583*	-0.0590*	-0.0667	-0.0646	-0.0657
	(0.0330)	(0.0330)	(0.0328)	(0.0425)	(0.0410)	(0.0406)
Intercept	3.7051***	1.8319***	1.7852***	3.3363***	1.4408***	1.5523***
	(0.8875)	(0.2062)	(0.1937)	(0.3860)	(0.1760)	(0.2358)
Cat. Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Cohort Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Squared	0.086	0.068	0.070	0.086	0.070	0.075

Table 10 (Cont'd)

Risk Taking and Experience for Different Definitions of Lead Manager

Panel B: Risk Taking - Pre-2000

	Lead II			Lead III		
	(1)	(2)	(3)	(4)	(5)	(6)
Exp	0.0886***			0.0886***		
	(0.0225)			(0.0225)		
Exp Group		0.4238***			0.4238***	
		(0.1208)			(0.1208)	
Middle			0.3248**			0.3248**
			(0.1273)			(0.1273)
Seasoned			0.6664***			0.6664***
			(0.1727)			(0.1727)
Log Size	-0.0387***	-0.0317*	-0.0351**	-0.0387***	-0.0317*	-0.0351**
	(0.0143)	(0.0171)	(0.0159)	(0.0143)	(0.0171)	(0.0159)
Family Size	0.0132	0.0236**	0.0250**	0.0132	0.0236**	0.0250**
	(0.0085)	(0.0117)	(0.0124)	(0.0085)	(0.0117)	(0.0124)
CFA	-0.0073	-0.0119	-0.0112	-0.0073	-0.0119	-0.0112
	(0.0302)	(0.0275)	(0.0286)	(0.0302)	(0.0275)	(0.0286)
MBA	-0.0432	-0.0703**	-0.0594**	-0.0432	-0.0703**	-0.0594**
	(0.0308)	(0.0309)	(0.0296)	(0.0308)	(0.0309)	(0.0296)
Female	-0.0962**	-0.1478***	-0.1281***	-0.0962**	-0.1478***	-0.1281***
	(0.0416)	(0.0409)	(0.0425)	(0.0416)	(0.0409)	(0.0425)
Intercept	-3.7757**	0.8812	1.7708***	-3.7757**	0.8812	1.7708***
	(1.7215)	(0.6642)	(0.5151)	(1.7215)	(0.6642)	(0.5151)
Cat. Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Cohort Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Squared	0.223	0.149	0.154	0.223	0.149	0.154

Table 11

Robustness Check: Risk Taking Behavior by Fund Size Grouping

The table reports estimates of risk measures regressed on fund manager and fund characteristics for different subsets of groups formed based on fund size. Panel A (B) reports the result for the Pre-2000 and Post-2000 periods. I use the Tracking Error as a measure of risk taking. For each dependent variable, I use the Fama-French-Carhart 4 Factor model(4 Factor) as its benchmark. I group all funds into Large and Mid/Small size groups based on the Morningstar Category. Standard errors are reported in parenthesis. A detailed description of each variable is included in Appendix A.

Panel A: Risk Taking by Fund Size - Post-2000

	Large			Mid/Small Size		
	(1)	(2)	(3)	(4)	(5)	(6)
Exp	-0.0149*			-0.0319**		
	(0.0082)			(0.0128)		
Exp Group		-0.1074**			-0.2235**	
		(0.0490)			(0.1069)	
Middle			-0.0933***			-0.1510**
			(0.0353)			(0.0715)
Seasoned			-0.1649**			-0.3203**
			(0.0810)			(0.1543)
Log Size	-0.0050	-0.0076*	-0.0073	-0.0173***	-0.0249***	-0.0256***
	(0.0046)	(0.0045)	(0.0045)	(0.0067)	(0.0061)	(0.0059)
Family Size	-0.0143***	-0.0127***	-0.0133***	-0.0285***	-0.0250***	-0.0248***
	(0.0044)	(0.0044)	(0.0043)	(0.0072)	(0.0072)	(0.0068)
CFA	-0.0405**	-0.0313	-0.0305	-0.0296	-0.0181	-0.0243
	(0.0199)	(0.0195)	(0.0191)	(0.0268)	(0.0273)	(0.0271)
MBA	-0.0147	-0.0114	-0.0125	-0.0506*	-0.0516*	-0.0519*
	(0.0243)	(0.0240)	(0.0240)	(0.0281)	(0.0285)	(0.0282)
Female	-0.0672**	-0.0556	-0.0569*	-0.0142	-0.0167	-0.0141
	(0.0342)	(0.0346)	(0.0344)	(0.0455)	(0.0456)	(0.0461)
Intercept	2.5867***	1.5346***	1.4075***	3.0372***	2.0121***	2.1100***
	(0.6869)	(0.1825)	(0.1592)	(0.4482)	(0.2557)	(0.2598)
Cat. Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Cohort Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Squared	0.055	0.040	0.044	0.086	0.047	0.050

Table 11 (Cont'd)

Robustness Check: Risk Taking Behavior by Fund Size Grouping

Panel B: Risk Taking by Fund Size - Pre-2000						
	Large			Mid/Small Size		
	(1)	(2)	(3)	(4)	(5)	(6)
Exp	0.0638***			0.1302***		
	(0.0175)			(0.0364)		
Exp Group		0.3449***			0.6706***	
		(0.0855)			(0.2571)	
Middle			0.2566***			0.5221**
			(0.0779)			(0.2294)
Seasoned			0.4641***			1.1345***
			(0.1189)			(0.3484)
Log Size	-0.0105	-0.0063	-0.0094	-0.0559*	-0.0366	-0.0398
	(0.0106)	(0.0123)	(0.0117)	(0.0288)	(0.0361)	(0.0328)
Family Size	0.0094	0.0161	0.0175*	0.0209	0.0346*	0.0345*
	(0.0085)	(0.0101)	(0.0103)	(0.0136)	(0.0188)	(0.0203)
CFA	-0.0173	-0.0238	-0.0240	-0.0183	-0.0063	0.0006
	(0.0340)	(0.0337)	(0.0334)	(0.0595)	(0.0534)	(0.0537)
MBA	0.0216	0.0010	-0.0004	-0.1878***	-0.2475***	-0.2287***
	(0.0293)	(0.0282)	(0.0284)	(0.0529)	(0.0575)	(0.0580)
Female	-0.0593	-0.0928**	-0.0793*	-0.1182*	-0.2238***	-0.1872**
	(0.0411)	(0.0387)	(0.0414)	(0.0693)	(0.0751)	(0.0749)
Intercept	-1.6970	1.2734	3.0251***	-6.5978***	0.1458	1.5398
	(1.3170)	(0.9140)	(0.5716)	(1.3035)	(1.4489)	(1.0637)
Cat. Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Cohort Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Squared	0.169	0.109	0.109	0.203	0.083	0.099

Table 12

Robustness Check: Risk Taking Behavior for Other Risk Taking Measures

The table reports estimates of other measures of risk taking regressed on fund manager and fund characteristics. Panel A (B) reports the results for the Pre-2000 and Post-2000 periods. For each panel, the first two specifications use Amihud and Goyenko (2013) R^2. I use 1 - R^2 of regression of monthly fund returns on its benchmark as the dependent variable to maintain consistency in regression coefficients with Tracking Error. The next two specifications use Return Gap of Kacperczyk et al. (2008). I use the future 12 month average Return Gap measure as the dependent variable. The last two specifications use the Active Share variable of Cremers and Petajisto (2009) and Petajisto (2013) and follow the regression specification in Cremers and Petajisto (2009). Standard errors are reported in brackets. A detailed description of each variable is included in Appendix A.

Panel A: Different Risk Taking Measures - Post-2000

	AG Rsq		Return Gap		Active Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Exp Group	-0.6514*		-0.0644**		-0.0130*	
	(0.3855)		(0.0312)		(0.0068)	
Middle		-0.4684**		-0.0644**		-0.0108
		(0.2224)		(0.0262)		(0.0082)
Seasoned		-1.0771**		-0.0807**		-0.0248**
		(0.4794)		(0.0410)		(0.0109)
Log Size	-0.1042*	-0.1075**	-0.0140***	-0.0142***	0.0022	0.0026
	(0.0536)	(0.0539)	(0.0047)	(0.0048)	(0.0053)	(0.0053)
Family Size	-0.1987***	-0.1982***	0.0122***	0.0122***	-0.0048***	-0.0047***
	(0.0415)	(0.0414)	(0.0026)	(0.0024)	(0.0013)	(0.0013)
CFA	-0.2715**	-0.2634**	-0.0054	-0.0024	0.0045	0.0047
	(0.1255)	(0.1263)	(0.0109)	(0.0101)	(0.0050)	(0.0050)
MBA	-0.2572*	-0.2501*	0.0174	0.0181	-0.0077*	-0.0078*
	(0.1523)	(0.1438)	(0.0113)	(0.0111)	(0.0046)	(0.0046)
Female	-0.4968***	-0.4907***	-0.0226	-0.0221	-0.0033	-0.0029
	(0.1830)	(0.1845)	(0.0147)	(0.0150)	(0.0076)	(0.0076)
Tracking Error					2.1697***	2.1878***
					(0.1932)	(0.1915)
Log $Size^2$					-0.0007	-0.0008*
					(0.0005)	(0.0005)
Intercept	9.8474***	10.9013***	0.0465	-0.0056	0.7828***	0.7403***
	(0.9781)	(1.6540)	(0.1076)	(0.0944)	(0.0410)	(0.0401)
Cat. Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Cohort Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Squared	0.027	0.029	0.006	0.007	0.420	0.417

Table 12 (Cont'd)

Robustness Check: Risk Taking Behavior for Other Risk Taking Measures

Panel B: Different Risk Taking Measures - Pre-2000						
	AG Rsq		Return Gap		Active Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Exp Group	-1.1937		0.0079		0.0120	
	(1.0692)		(0.0534)		(0.0081)	
Middle		-0.8135		-0.0946		-0.0127
		(1.1264)		(0.0701)		(0.0079)
Seasoned		-1.8707		0.0127		-0.0078
		(1.8653)		(0.0883)		(0.0105)
Log Size	-0.4304***	-0.4383***	-0.0734***	-0.0723***	-0.0003	-0.0001
	(0.1148)	(0.1090)	(0.0169)	(0.0166)	(0.0064)	(0.0064)
Family Size	-0.3591***	-0.3445***	0.0157*	0.0153**	-0.0013	-0.0013
	(0.0729)	(0.0708)	(0.0083)	(0.0078)	(0.0015)	(0.0015)
CFA	-0.2417	-0.2507	0.0501	0.0552	0.0061	0.0060
	(0.3235)	(0.3202)	(0.0474)	(0.0476)	(0.0060)	(0.0060)
MBA	-0.1225	-0.1621	0.0103	0.0142	0.0052	0.0056
	(0.2822)	(0.2892)	(0.0370)	(0.0379)	(0.0056)	(0.0056)
Female	-1.5407***	-1.5872***	-0.0560	-0.0548	-0.0102	-0.0097
	(0.4267)	(0.4351)	(0.0550)	(0.0563)	(0.0110)	(0.0108)
Tracking Error					1.3334***	1.3312***
					(0.1046)	(0.1049)
$Log\ Size^2$					-0.0012**	-0.0012**
					(0.0006)	(0.0006)
Intercept	33.3912***	31.4007***	1.1650***	1.1363***	0.8542***	0.9359***
	(5.1830)	(3.6781)	(0.1950)	(0.1899)	(0.0182)	(0.0294)
Cat. Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Cohort Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Squared	0.044	0.046	0.028	0.029	0.395	0.394

Table 13

Robustness Check: Risk Taking and Fund Manager Industry Experience

The table reports estimates of risk measures regressed on fund manager and fund characteristics. The First (Next) four specifications use the sub-periods of Pre-2000 and Post-2000. The dependent variable is the Tracking Error. The first two specifications for each sub-periods use the Fama-French-Carhart 4 Factor model(FFC 4 Factor Model) and the next two use a combination of the primary prospectus benchmark and the S&P 500 (1 Factor) as its benchmark in estimating the Tracking Error. The Industry Exp is based on hand-collected data on when each fund manager started working in the investment industry. The Industry Exp Group equals 1 (2) if a lead manager's industry experience is in the bottom (next) 40th (20th) percentile of each cross section and 0 otherwise. Standard errors are reported in parenthesis. A detailed description of each variable is included in Appendix A.

	Post-2000				Pre-2000			
	FFC 4 Factor Model		1 Factor Model		FFC 4 Factor Model		1 Factor Model	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Industry Exp Group	-0.1261		-0.2394*		0.3861***		0.5029***	
	(0.0995)		(0.1442)		(0.1314)		(0.1837)	
Industry Middle		-0.1559**		-0.2486***		0.1214*		0.1449
		(0.0606)		(0.0861)		(0.0702)		(0.0928)
Industry Seasoned		-0.2147**		-0.3349**		0.6241***		0.8079***
		(0.0905)		(0.1365)		(0.1653)		(0.2537)
Log Size	-0.0245***	-0.0242***	-0.0277***	-0.0270***	-0.0357*	-0.0346**	-0.0618*	-0.0589*
	(0.0046)	(0.0047)	(0.0060)	(0.0060)	(0.0182)	(0.0169)	(0.0362)	(0.0348)
Family Size	-0.0168***	-0.0174***	-0.0276***	-0.0294***	0.0247*	0.0273*	0.0593***	0.0609***
	(0.0054)	(0.0053)	(0.0056)	(0.0055)	(0.0138)	(0.0142)	(0.0170)	(0.0172)
CFA	-0.0530***	-0.0513***	-0.1034***	-0.0988***	-0.0646*	-0.0460	-0.1496**	-0.1250**
	(0.0142)	(0.0144)	(0.0217)	(0.0216)	(0.0370)	(0.0331)	(0.0595)	(0.0526)
MBA	-0.0054	-0.0012	0.0126	0.0187	-0.0332	-0.0309	0.0536	0.0520
	(0.0176)	(0.0175)	(0.0231)	(0.0229)	(0.0277)	(0.0282)	(0.0614)	(0.0634)
Female	-0.0358	-0.0324	-0.1109***	-0.1048***	-0.1583***	-0.1683***	-0.2653**	-0.2764***
	(0.0280)	(0.0289)	(0.0373)	(0.0382)	(0.0413)	(0.0410)	(0.1032)	(0.1034)
Intercept	2.1715***	2.2944***	2.8346***	2.6710***	1.3510*	2.2214***	1.5295	2.0206**
	(0.1838)	(0.2836)	(0.2585)	(0.2679)	(0.7616)	(0.6581)	(1.2014)	(0.9690)
Cat. Dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort Dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.066	0.069	0.073	0.076	0.148	0.157	0.103	0.109

Table 14

Robustness Check: Change in Termination Probability

The table reports logit regressions of termination on fund manager and fund characteristics. I define termination at a fund level, which is similar to specification in Chevalier and Ellison (1999). This dummy variable equals 1 if a fund manager is no longer a fund manager at the fund in year t+1. The first two specifications follow the specifications of Table 3 and compare the earlier period to the recent period. The next four specifications follow the specifications of Table 8 and compare between junior and seasoned managers in two sub-periods. Standard errors are double clustered at the manager and year levels. Resulting standard errors are reported in brackets. A detailed description of each variable is included in Appendix A.

	Fund Level Termination					
	Full Sample		Pre-2000		Post-2000	
	(1)	(2)	(3)	(4)	(5)	(6)
$Alpha_t$	-0.2117***	-0.1962***	-0.2066***	-0.1751	-0.3473***	0.0709
	(0.0617)	(0.0572)	(0.0709)	(0.2164)	(0.0819)	(0.1739)
$Alpha_t$ * *Post-2000*	-0.1190	-0.2491**				
	(0.0998)	(0.0977)				
Post-2000	-0.1241	-0.0790				
	(0.1200)	(0.1236)				
$Alpha_t * (Exp - \overline{Exp})$			-0.0015		-0.0131**	
			(0.0056)		(0.0066)	
$Alpha_t$ * *Exp Group*				-0.0156		-0.2097***
				(0.0941)		(0.0794)
$(Exp - \overline{Exp})$			-0.0054		-0.0065	
			(0.0069)		(0.0063)	
Exp Group				-0.0218		-0.0770
				(0.0821)		(0.0512)
$Alpha_{t-1}$	-0.2737***	-0.3761***	-0.3269***	-0.3271***	-0.2257***	-0.2254***
	(0.0569)	(0.0927)	(0.1095)	(0.1101)	(0.0668)	(0.0669)
$Alpha_{t-2}$	-0.1825***	-0.1647**	0.1193	0.1175	-0.2580***	-0.2595***
	(0.0636)	(0.0673)	(0.0736)	(0.0731)	(0.0711)	(0.0714)
$Alpha_{t-3}$	-0.1503**	-0.2260***	-0.2820***	-0.2822***	-0.1092	-0.1090
	(0.0625)	(0.0564)	(0.0911)	(0.0915)	(0.0801)	(0.0803)
Log Size	-0.2025***	-0.1910***	-0.1453***	-0.1453***	-0.2247***	-0.2245***
	(0.0242)	(0.0263)	(0.0344)	(0.0341)	(0.0356)	(0.0356)
Fam Size	0.1222***	0.1206***	0.1601***	0.1621***	0.0965***	0.0958***
	(0.0154)	(0.0147)	(0.0152)	(0.0158)	(0.0210)	(0.0213)
$Age60+$	-0.1648	-0.1852	0.3501	0.2769	-0.1881	-0.2218
	(0.1374)	(0.1416)	(0.2780)	(0.2533)	(0.1745)	(0.1657)
Abnormal Flow	-0.2512**	-0.2462**	-0.1514	-0.1518	-0.2423**	-0.2404**
	(0.0994)	(0.1006)	(0.2583)	(0.2603)	(0.1126)	(0.1129)
$Intercept$	-0.4895	-0.5345	-2.3341***	-2.3384***	0.4205	0.5935
	(0.4729)	(0.4981)	(0.4691)	(0.5174)	(0.6082)	(0.6359)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Obj. Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Psuedo R-Squared	0.0377	0.0434	0.043	0.0428	0.0434	0.0441

Table A1 : Variable Definitions

The sample includes all open-end U.S. domestic equity funds for the 1992 to 2014 period from Morningstar Direct. Detailed description on the data construction is included in the data section.

Variable	Definition
Abnormal Flow	Abnormal net inflow to each fund after controlling for past performance as suggested by Kostovetsky and Warner (2015). This variable is a residual from a regression of net inflows on past alphas (up to 3 years) and Family Size.
Active Share	Sum of absolute difference in weights between a fund and its benchmark index holdings as introduced by Cremers and Petajisto (2009).
Age60+	Dummy variable that equals 1 if a fund manager is older than 60 and 0 otherwise.
AG Rsq	Amihud and Goyenko (2013) R^2 calculated by subtracting the 4 factor R^2 of fund return for the past 12 months from 1.
Category Flow	Growth rate of net new money (Net Inflow) for all funds in same investment objective category as the fund.
CFA	Dummy variable indicating whether the fund manager has Certified Financial Analyst certificate.
Cohort Dummy	Dummy variable for the decade when each fund manager was born.
Exp	Age of the leading manager of the fund.
Exp Group	Categorical variable that equals 1 for the inexperienced group and 2 for the experienced group based on the cross sectional Exp median.
Expense	Net expense ratio of the fund.
Industry Exp	Number of years each lead manager worked in the investment industry based on hand-collected variables.
Industry Exp Group	Categorical variable that equals 1 for to the inexperienced group and 2 for experienced group based on cross sectional Industry Exp median.
Industry Middle	Dummy variable that equals 1 if a fund manager's Industry Exp is between the 40th to 60th percentile of each cross section and 0 otherwise.
Industry Seasoned	Dummy variable that equals 1 if a fund manager's Industry Exp is in the top 40th percentile in each cross section and 0 otherwise.
Family Size	Natural logarithm of the Total Net Assets (TNA) of the fund family that the fund belongs to.
Female	Dummy variable that equals 1 if the fund is managed by a female manager and 0 otherwise.
Log Size	Natural logarithm of the TNA of each fund.
Middle	Dummy variable that equals 1 if a fund manager's age is between the 40th to 60th percentile of each cross section and 0 otherwise.
MBA	Dummy variable that equals 1 if a fund manager has MBA degree and 0 otherwise.
Objective Dummy	Dummy variable for each investment style group that the fund belongs to.

Table A1 : Variable Definitions (Cont'd)

Variable	Definition
Post-2000	Annual dummy variable that equals 1 for Post-2000 years.
Quarter	Variable that equals 1 for the first quarter of observation and increases by 1 for each quarter.
Ret. Std.	Standard deviation of monthly fund return during the past 12 month horizon.
Return Gap	Unobserved actions of mutual fund managers measured by Kacperczyk et al. (2008). Measured using the difference between actual and hypothetical fund return based on aggregated past reported holdings. I use the average of the future 12 month Return Gap measure.
Seasoned	Dummy variable that equals 1 if a fund manager's age is in the top 40th percentile in each cross section and 0 otherwise.
Tracking Error	Square root of the estimated residual variance from a regression of monthly fund returns on Fama-French-Carhart (FFC) 4 factor model for the next 12 months.

Table 15. Merger Summary Statistics

The table reports summary statistics for the variables employed in our analysis. The sample consists of mergers announced between 1985 and 2014 in which the acquirer is a publicly traded company with common stock data available from the Center for Research in Security Prices (CRSP). The deal value, as reported by Securities Data Company SDC, is required to be at least 1% of the value of the acquirer at the time of the announcement, and acquirer must have data available from Compustat for the fiscal year preceding the merger. Panel A reports the mean, standard deviation, and the 25th, 50th, and 75th percentiles for the full sample of mergers. Panels B through E report statistics for the subset of mergers in which B) the target headquarters is located in the CEO's birth state, C) the target headquarters is located less than 100 miles from the CEO's hometown, D) the target is headquartered outside the acquirer's headquarters state, and E) the target is headquartered more than 100 miles from the acquirer headquarters. A detailed description of each variable is included in Appendix A.

Variable	N	Mean	Standard Deviation	25^{th} Percentile	Median	75^{th} Percentile
Panel A: All Mergers						
Deal Value	8735	849.67	4604.23	22.00	72.90	294.35
Relative Deal Value	8735	0.33	1.31	0.04	0.10	0.28
Price Run-up	8735	0.23	0.90	-0.15	0.06	0.34
Tobin's Q	8735	2.24	5.57	1.09	1.48	2.26
Leverage	8735	0.32	0.86	0.06	0.31	0.53
Δ Income	8735	0.40	0.74	0.00	0.22	0.69
Acq-Target Distance	8012	820.49	819.46	129.06	566.29	1287.99
CEO-Target Distance	8012	949.14	826.59	259.83	701.88	1508.19
Panel B: Home Bias Mergers (State)						
Deal Value	1243	622.28	3929.28	20.00	60.00	213.19
Relative Deal Value	1243	0.31	0.66	0.04	0.11	0.31
Price Run-up	1243	0.21	0.71	-0.15	0.06	0.35
Tobin's Q	1243	2.25	5.14	1.05	1.23	2.01
Leverage	1243	0.36	0.3	0.09	0.36	0.57
Δ Income	1243	0.42	0.74	0.00	0.24	0.68
Acq-Target Distance	1125	820.49	819.46	129.06	566.29	1287.99
CEO-Target Distance	1125	949.14	826.59	259.83	701.88	1508.19
Panel C: Home Bias Mergers (Distance)						
Deal Value	1040	943.13	4871.85	22.10	71.80	283.25
Relative Deal Value	1040	0.36	0.77	0.05	0.13	0.37
Price Run-up	1040	0.18	0.65	-0.15	0.05	0.32
Tobin's Q	1040	2.60	13.91	1.04	1.24	2.02
Leverage	1040	0.36	0.32	0.09	0.36	0.57
Δ Income	1040	0.41	0.71	0.00	0.26	0.68
Acq-Target Distance	1040	328.14	658.67	10.86	36.26	195.5
CEO-Target Distance	1040	35.79	29.37	10.7	27.25	58.52

Table 15. Merger Summary Statistics (continued)

Variable	N	Mean	Standard Deviation	25th Percentile	Median	75th Percentile
Panel D: Cross-State Mergers						
Deal Value	6466	880.18	4773.30	23.00	75.00	307.50
Relative Deal Value	6466	0.32	1.42	0.04	0.09	0.27
Price Run-up	6466	0.22	0.91	-0.15	0.06	0.33
Tobin's Q	6466	2.21	5.86	1.10	1.51	2.27
Leverage	6466	0.32	0.99	0.06	0.31	0.52
Δ Income	6466	0.39	0.74	0.00	0.22	0.69
Acq-Target Distance	5964	1074	801.9	414.22	857.77	1620.17
CEO-Target Distance	5964	1008.56	792.63	368.24	772.22	1526.17
Panel E: Faraway Mergers						
Deal Value	6174	859.79	4457.88	25.00	81.00	327.69
Relative Deal Value	6174	0.31	0.81	0.04	0.09	0.28
Price Run-up	6174	0.22	0.92	-0.15	0.06	0.33
Tobin's Q	6174	2.23	6.03	1.1	1.51	2.28
Leverage	6174	0.31	1.00	0.06	0.30	0.51
Δ Income	6174	0.39	0.73	0.00	0.23	0.69
Acq-Target Distance	6174	1056.31	792.11	388.53	825.72	1584.09
CEO-Target Distance	6174	1026.72	794.71	380.77	789.73	1570.2

Table 16. CEO Home Bias and the Probability of Acquisition

The table reports comparisons of probability of acquisition between actual and hypothetical mergers. We fix the acquirer and choose hypothetical targets that match the size and book to market quintiles of the target and operate in the same Fama-French 48 industry category (from the CRSP-Compustat universe). We also require that that the hypothetical target not to have engaged in a merger within two years before or after the deal announcement date. Panels A and B report the *t*-test statistics for the actual probability of mergers relative to the likelihood of a similar hypothetical merger. *Home Bias* $_{State}$ is a dummy variable that is equal to one if the acquirer firm CEO birth state is equal to target headquarters state. *Cross-State Merger* is a dummy variable that is equal to one if the acquirer headquarters state differs from the target headquarters state. In Panel B, *Home Bias* $_{Distance}$ is a dummy variable that is equal to one when the target headquarters is ≤ 100 miles from the CEO's birthplace. Faraway (Nearby) Mergers are those in which the target and acquirer headquarters are > (≤) 100 miles apart. Panels C and D report the results from probit regressions, where the dependent variable is 1 for actual mergers and 0 for hypothetical mergers, with standard errors reported in parentheses. For the simulation evidence, we randomly select one hypothetical target for each acquirer to create a balanced sample, and we repeat the exercise 1,000 times. The second set of columns reports the average coefficients with empirical p-values in brackets. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively.

Panel A: Merger Likelihood using State Measures of Home Bias

	All Mergers			Cross-State Mergers			In-State Mergers		
	Actual	Hypothetical	Difference	Actual	Hypothetical	Difference	Actual	Hypothetical	Difference
Home Bias $_{State}$	0.147	0.058	0.089***	0.041	0.03	0.011**	0.106	0.028	0.078***
	(0.008)	(0.001)		(0.004)	(0.001)		(0.007)	(0.001)	
Not Home Bias	0.853	0.942	-0.089***	0.7	0.898	-0.198***	0.153	0.044	0.108***
	(0.008)	(0.001)		(0.01)	(0.002)		(0.008)	(0.001)	

Panel B: Merger Likelihood using Distance-Based Measures of Home Bias

	All Mergers			Faraway Mergers			Nearby Mergers		
	Actual	Hypothetical	Difference	Actual	Hypothetical	Difference	Actual	Hypothetical	Difference
Home Bias $_{Distance}$	0.145	0.065	0.080***	0.043	0.036	0.007	0.102	0.029	0.073***
	0.008	0.002		0.005	0.001		0.007	0.001	
Not Home Bias	0.855	0.935	-0.080***	0.708	0.891	-0.183***	0.147	0.044	0.103***
	0.008	0.002		0.011	0.002		0.008	0.001	

Table 16. CEO Home Bias and Probability of Acquisition (continued)

Panel C : Merger Likelihood Regression using State Measure of Home Bias

	Probit				Simulations			
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Home Bias $_{State}$	0.5136***	0.1617***	0.0557	0.0679	0.6395***	0.2549***	0.1705**	0.1939***
	(0.0388)	(0.0420)	(0.0575)	(0.0606)	[0.000]	[0.000]	[0.008]	[0.005]
Cross-State Merger		-0.7310***	-0.7791***	-0.7889***		-0.7737***	-0.8084***	-0.8028***
		(0.0344)	(0.0385)	(0.0399)		[0.000]	[0.000]	[0.000]
Home Bias $_{State}$ × *Cross-State Merger*			0.2229***	0.2161**			0.1679**	0.1396*
			(0.0825)	(0.0861)			[0.026]	[0.066]
|Acq(B/M) – Tgt(B/M)|				-0.0778***				-0.0910***
				(0.0217)				[0.000]
|Acq(ME) – Tgt(ME)|				0.0015***				0.00005
				(0.0003)				[0.373]

Panel D : Merger Likelihood Regression using Distance Measure of Home Bias

Home Bias $_{Distance}$	0.4562***	0.1181***	0.0329	0.0528	0.5303***	0.1796***	0.1504**	0.1890***
	(0.0380)	(0.0435)	(0.0613)	(0.0642)	[0.000]	[0.000]	[0.023]	[0.008]
Faraway Merger		-0.7088***	-0.7477***	-0.7616***		-0.7851***	-0.7978***	-0.7976***
		(0.0365)	(0.0412)	(0.0427)		[0.000]	[0.000]	[0.000]
Home Bias $_{Distance}$ × *Faraway Merger*			0.1696**	0.1646*			0.0546	0.0199
			(0.0858)	(0.0895)			[0.279]	[0.429]
|Acq(B/M) – Tgt(B/M)|				-0.0930***				-0.0984***
				(0.0236)				[0.000]
|Acq(ME) – Tgt(ME)|				0.0010***				-0.0007
				(0.0004)				[0.000]

Table 17. Bidder Announcement Returns for CEO Home Bias Mergers

The table presents regression results for acquirer cumulative abnormal announcement returns. In Columns (1) – (4) , *Home Bias* is a dummy variable that is equal to one when the target headquarters state matches the CEO birth state, and *Cross-State Merger* is equal to one when the acquirer headquarters state differs from the target headquarters state. In Columns (5) – (6), *Home Bias* is equal to one when the target headquarters is within 100 miles of the acquirer CEOs birth town, and *Faraway Merger* is one when the acquirer headquarters is more than 100 miles from the target headquarters. We include year fixed effects, and standard errors clustered by industry are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively. Detailed descriptions of the control variables are presented in Appendix A.

	State Measures of Home Bias				Distance Measures of Home Bias			
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Home Bias	-0.0013	0.0014	-0.0041	0.0015	-0.0049**	-0.0011	-0.0063**	0.0005
	(0.0026)	(0.0041)	(0.0029)	(0.0037)	(0.0023)	(0.0035)	(0.0027)	(0.0034)
Cross-State or Faraway Merger		-0.0021	-0.0069***	-0.0040*		-0.0012	-0.0067***	-0.0035
		(0.0023)	(0.0020)	(0.0022)		(0.0024)	(0.0022)	(0.0024)
Home Bias × Cross State or Faraway		-0.0182***		-0.0181***		-0.0170***		-0.0185***
		(0.0060)		(0.0057)		(0.0057)		(0.0051)
Relative Deal Value			0.0003	0.0003			-0.0216***	-0.0216***
			(0.0005)	(0.0005)			(0.0027)	(0.0027)
Log Total Assets			-0.0017***	-0.0017***			-0.0015	-0.0015
			(0.0004)	(0.0004)			(0.0011)	(0.0011)
Industry Leverage			-0.0002*	-0.0002*			-0.0019***	-0.0020***
			(0.0001)	(0.0001)			(0.0004)	(0.0004)
Industry Tobin's Q			-0.0000	-0.0000			-0.0001	-0.0001
			(0.0000)	(0.0000)			(0.0001)	(0.0001)
Δ Income			-0.0023**	-0.0023**			-0.0000	-0.0000
			(0.0010)	(0.0010)			(0.0000)	(0.0000)
Price Run-up			-0.0024*	-0.0024*			-0.0024***	-0.0024***
			(0.0014)	(0.0014)			(0.0009)	(0.0008)
Cash Deal			0.0042***	0.0043***			-0.0023	-0.0023
			(0.0015)	(0.0015)			(0.0015)	(0.0015)
Stock Deal			-0.0071***	-0.0070***			0.0039**	0.0040**
			(0.0023)	(0.0023)			(0.0015)	(0.0015)
Public Target			-0.0224***	-0.0223***			-0.0082***	-0.0082***
			(0.0026)	(0.0026)			(0.0027)	(0.0027)
R-squared	0.008	0.010	0.047	0.048	0.011	0.012	0.052	0.053

Table 18. Corporate Governance and the Probability of CEO Home Bias Acquisitions

The table presents results from probit regressions for subsets of well and poorly governed acquirer firms. Manager entrenchment is based on the E-index of Bebchuk, Cohen, and Ferrell (2009), with High (Low) Entrenchment denoting firms with E-index greater (less than) 2. Low (High) Institutional ownership is based on the median value of concentrated institutional ownership. Panel A (B) presents State (Distance) based home bias measures. We include year fixed effects and standard errors clustered by industry are reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively.

Panel A : State Based Measures of Home Bias

Variable	High Entrenchment (1)	Low Entrenchment (2)	High Entrenchment (3)	Low Entrenchment (4)	Low Inst Ownership (5)	High Inst Ownership (6)	Low Inst Ownership (7)	High Inst Ownership (8)
Home Bias $_{State}$	1.0376***	0.5404***	0.2853	-0.1963	1.1522***	0.8564***	0.2244	0.0166
	(0.1103)	(0.1755)	(0.1749)	(0.2543)	(0.1020)	(0.1175)	(0.1566)	(0.1806)
Cross State Merger			-1.3419***	-1.2807***			-1.6117***	-1.2415***
			(0.1153)	(0.1524)			(0.1103)	(0.1115)
Home Bias $_{State}$ × *Cross State Merger*			0.4560*	0.5232			0.4281*	0.6784***
			(0.2473)	(0.3933)			(0.2408)	(0.2619)
|Acq(B/M) – Tgt(B/M)|			-0.2928***	-0.6202***			-0.5348***	-0.1514**
			(0.0913)	(0.1673)			(0.1161)	(0.0720)
|Acq(ME) – Tgt(ME)|			0.0009	0.0014*			0.0020***	0.0089***
			(0.0018)	(0.0008)			(0.0008)	(0.0024)

Panel B : Distance Based Measures of Home Bias

Variable	High Entrenchment (1)	Low Entrenchment (2)	High Entrenchment (3)	Low Entrenchment (4)	Low Inst Ownership (5)	High Inst Ownership (6)	Low Inst Ownership (7)	High Inst Ownership (8)
Home Bias $_{Distance}$	1.0611***	0.5795***	0.3646**	-0.0839	0.9818***	0.8933***	-0.0207	0.3435*
	(0.1034)	(0.1706)	(0.1687)	(0.2463)	(0.1030)	(0.1123)	(0.1589)	(0.1785)
Faraway Merger			-1.2213***	-1.4649***			-1.7115***	-1.1142***
			(0.1222)	(0.1631)			(0.1122)	(0.1190)
Home Bias $_{Distance}$ × *Faraway Merger*			0.3541	0.1607			0.5359**	0.2481
			(0.2412)	(0.4163)			(0.2415)	(0.2576)
|Acq(B/M) – Tgt(B/M)|			-0.2926***	-0.5726***			-0.4970***	-0.1444**
			(0.0918)	(0.1671)			(0.1157)	(0.0714)
|Acq(ME) – Tgt(ME)|			-0.0005	0.0011			0.0014*	0.0087***
			(0.0018)	(0.0008)			(0.0008)	(0.0025)

Table 19. Governance and Bidder Announcement Returns

The table presents results from announcement return regressions for subsets of well and poorly governed acquirer firms. Manager entrenchment is based on the E-index of Bebchuk, Cohen, and Ferrell (2009), with High (Low) Entrenchment denoting firms with E-index greater (less than) 2. Low (High) Institutional ownership is based on the median value of concentrated institutional ownership. Panel A (B) presents State (Distance) based home bias measures. We include year fixed effects and standard errors clustered by industry are reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively. A detailed description of each variable is presented in Appendix A.

Panel A : State Measures of Home Bias

Variables	High E-Index	Low E-Index	Low Inst Ownership	High Inst Ownership
	(1)	(2)	(3)	(4)
Home Bias $_{State}$	0.0106**	0.0002	0.0115	0.0053
	(0.0052)	(0.0109)	(0.0071)	(0.0061)
Cross State Merger	0.0036	-0.0038	0.0014	-0.0020
	(0.0037)	(0.0050)	(0.0039)	(0.0031)
Home Bias $_{State}$ × *Cross State Merger*	-0.0242***	0.0015	-0.0303**	-0.0119
	(0.0083)	(0.0147)	(0.0131)	(0.0087)
Relative Deal Value	-0.0102***	0.0023	-0.0009	-0.0049**
	(0.0035)	(0.0041)	(0.0017)	(0.0023)
Log Total Assets	-0.0026***	-0.0004	-0.0030***	-0.0020**
	(0.0009)	(0.0012)	(0.0009)	(0.0008)
Industry Leverage	-0.0004***	-0.0004***	-0.0004***	-0.0004***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Industry Tobin's Q	-0.0000	0.0022**	-0.0000**	0.0000
	(0.0000)	(0.0010)	(0.0000)	(0.0000)
Δ Income	-0.0008	-0.0007	-0.0039***	-0.0024*
	(0.0016)	(0.0028)	(0.0015)	(0.0013)
Price Run-up	-0.0051*	-0.0090	-0.0023	-0.0027
	(0.0026)	(0.0074)	(0.0039)	(0.0023)
Cash Deal	0.0076***	0.0072	0.0085***	0.0054**
	(0.0023)	(0.0061)	(0.0024)	(0.0024)
Stock Deal	-0.0063*	-0.0011	-0.0003	-0.0112***
	(0.0035)	(0.0073)	(0.0028)	(0.0038)
Public Target	-0.0138***	-0.0242***	-0.0215***	-0.0184***
	(0.0024)	(0.0048)	(0.0038)	(0.0025)
Constant	0.0229***	0.0108	0.0301***	0.0256***
	(0.0081)	(0.0122)	(0.0103)	(0.0076)
R-squared	0.076	0.071	0.077	0.063

Table 19. Governance and Bidder Announcement Returns (continued)
Panel B : Acquirer CAR for Distance Variables

Variables	High E-Index	Low E-Index	Low Inst Ownership	High Inst Ownership
	(1)	(2)	(3)	(4)
Home Bias $_{Distance}$	0.0053	0.0007	0.0073	0.0032
	(0.0043)	(0.0083)	(0.0062)	(0.0039)
Faraway Merger	0.0020	0.0005	0.0001	-0.0023
	(0.0035)	(0.0048)	(0.0046)	(0.0032)
Home Bias $_{Distance}$ × *Faraway Merger*	-0.0157**	-0.0044	-0.0351***	-0.0086
	(0.0060)	(0.0156)	(0.0093)	(0.0058)
Relative Deal Value	-0.0086***	0.0031	-0.0227***	-0.0209***
	(0.0029)	(0.0032)	(0.0042)	(0.0029)
Log Total Assets	-0.0019*	-0.0001	-0.0018	-0.0033
	(0.0010)	(0.0011)	(0.0012)	(0.0020)
Industry Leverage	-0.0003***	-0.0002*	-0.0021***	-0.0013**
	(0.0001)	(0.0001)	(0.0006)	(0.0006)
Industry Tobin's Q	-0.0000	0.0026**	-0.0003***	-0.0003***
	(0.0000)	(0.0011)	(0.0001)	(0.0001)
Δ Income	0.0003	-0.0022	-0.0000**	0.0000
	(0.0017)	(0.0029)	(0.0000)	(0.0000)
Price Run-up	-0.0037	-0.0056*	-0.0017	-0.0024*
	(0.0026)	(0.0029)	(0.0017)	(0.0014)
Cash Deal	0.0080***	0.0086	-0.0014	-0.0019
	(0.0027)	(0.0060)	(0.0015)	(0.0030)
Stock Deal	-0.0062	-0.0039	0.0057**	0.0045**
	(0.0040)	(0.0057)	(0.0025)	(0.0022)
Public Target	-0.0149***	-0.0221***	-0.0041	-0.0118***
	(0.0026)	(0.0052)	(0.0032)	(0.0035)
Constant	0.0166*	0.0025	0.0239***	0.0212***
	(0.0089)	(0.0111)	(0.0069)	(0.0056)
R-squared	0.068	0.066	0.058	0.059

Table 20. Strength of Home Bias and Probability of an Acquisition

The table presents the results of probit regressions for subsets of mergers in which acquirer firm CEOs have a strong connection with their birth region. Each regression is run on a different subsample, depending on the level of the strength of Home Bias. In Panel A (B), Home Connection CEOs are classified as those who lived in their birth state (within 100 miles of the target) in adulthood with a resident address listed for at least ten years in the CEO's name. In Panel A (B), Education Connection CEOs are those who obtained undergraduate or graduate degree from their birth state (within 100 miles of the target). We include year fixed effects and standard errors clustered by industry are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively. A detailed description of each variable is included in Appendix A.

Panel A : State Variable Probit Regression

	Home Connection				Education Connection			
	Yes	No	Yes	No	Yes	No	Yes	No
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Home Bias $_{State}$	0.7210***	0.1945*	0.1051	-0.0096	0.5516***	0.4407***	0.2163**	-0.0761
	(0.0759)	(0.1068)	(0.1506)	(0.2870)	(0.0616)	(0.0890)	(0.1092)	(0.1384)
Cross State Merger			-0.8406***	-0.7515***			-0.6182***	-0.7515***
			(0.1278)	(0.0742)			(0.0810)	(0.0747)
Home Bias $_{State}$ × *Cross State*			0.1491	0.2212			0.0672	0.4215**
			(0.2282)	(0.3118)			(0.1529)	(0.2017)
|Acq(B/M) – Tgt(B/M)|			-0.0964*	-0.0481			-0.1063**	-0.2019***
			(0.0551)	(0.0396)			(0.0438)	(0.0570)
|Acq(ME) – Tgt(ME)|			0.0004	-0.0000			0.0047***	0.0004
			(0.0007)	(0.0007)			(0.0012)	(0.0005)

Panel B : Distance Variable Probit Regression

	Home Connection				Education Connection			
	Yes	No	Yes	No	Yes	No	Yes	No
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Home Bias $_{Distance}$	1.1258***	-0.2613***	0.0021	-0.3015	0.5376***	0.3457***	0.2445*	-0.0983
	(0.0891)	(0.0945)	(0.1319)	(0.2233)	(0.0715)	(0.0759)	(0.1288)	(0.1313)
Faraway Merger			-2.0057***	0.1379			-0.6082***	-0.8254***
			(0.1241)	(0.1218)			(0.0958)	(0.0797)
Home Bias $_{Distance}$ × *Faraway*			1.0867***	0.1771			0.0454	0.2181
			(0.2329)	(0.2516)			(0.1796)	(0.1773)
|Acq(B/M) – Tgt(B/M)|			-0.0343	-0.0471			-0.2773***	-0.0816**
			(0.0793)	(0.0328)			(0.0750)	(0.0400)
|Acq(ME) – Tgt(ME)|			0.0036*	0.0042***			0.0058***	0.0005
			(0.0021)	(0.0010)			(0.0014)	(0.0004)

Table 21. Strength of Home Bias and Bidder Announcement Returns

The table presents the results of abnormal return regressions for subsets of mergers in which acquirer firm CEOs have a strong connection with their birth region. Each regression is run on a different subsample, depending on the level of the strength of Home Bias. In Panel A (B) Home Connection CEOs as classified as those who lived in their birth state (within 100 miles of the target) in adulthood with a resident address listed for at least ten years in the CEO's name. In Panel A (B), Education Connection CEOs are those who obtained undergraduate or graduate degree from their birth state (within 100 miles of the target). We include year fixed effects and standard errors clustered by industry are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively. A detailed description of each variable is included in Appendix A.

	Home Connection				Education Connection			
	High	Low	High	Low	High	Low	High	Low
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Home Bias $_{State}$	-0.0095	0.0023			-0.0056	-0.0104		
	(0.0096)	(0.0111)			(0.0072)	(0.0099)		
Cross State Merger	-0.0088	-0.0015			-0.0132	-0.0050		
	(0.0081)	(0.0037)			(0.0089)	(0.0099)		
Home Bias $_{State}$ × *Cross State*	-0.0346*	-0.0129			-0.0034	-0.0010		
	(0.0172)	(0.0112)			(0.0183)	(0.0126)		
Home Bias $_{Distance}$			0.0050	-0.0087*			0.0097*	0.0025
			(0.0055)	(0.0051)			(0.0053)	(0.0063)
Faraway Merger			-0.0002	-0.0047			-0.0001	-0.0011
			(0.0040)	(0.0046)			(0.0046)	(0.0068)
Home Bias $_{Distance}$ × *Faraway*			-0.0235**	-0.0040			-0.0194**	-0.0251
			(0.0113)	(0.0117)			(0.0081)	(0.0151)
Constant	0.0439***	0.0258***	0.0179**	0.0260***	0.0153	0.0373***	0.0200***	0.0221***
	(0.0135)	(0.0084)	(0.0071)	(0.0072)	(0.0113)	(0.0129)	(0.0059)	(0.0061)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.062	0.071	0.067	0.065	0.112	0.061	0.075	0.078

Table 22. Bidder Returns for Public and Private Targets

This table contains regression results for bidder cumulative abnormal returns (CARs) on many controls and the main variables of our interest based on States for public and private targets separately. Home Bias is a dummy variable that is equal to one when the acquirer firm CEO birth state is equal to target headquarters state. Cross State Merger is a dummy variable that is equal to one when the acquirer headquarters state is different from target headquarters state. Home Bias x Cross State Merger is an interaction between Home Bias and Cross State Merger. We include year fixed effects and standard errors clustered by industry are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively. A detailed description of each variable is included in Appendix A.

	Public Targets					Private Targets		
	Bidder CARs			Target CARs		Bidder CARs		
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Home Bias $_{State}$	0.0064	0.0067		-0.0520**		-0.0016	-0.0013	
	(0.0057)	(0.0060)		(0.0214)		(0.0032)	(0.0032)	
Cross State Merger	0.0005	-0.0022		-0.0263		-0.0049*	-0.0051*	
	(0.0036)	(0.0037)		(0.0184)		(0.0026)	(0.0026)	
Home Bias $_{State}$ × Cross State Merger	-0.0227**	-0.0224**		0.0351		-0.0140**	-0.0145***	
	(0.0109)	(0.0111)		(0.0375)		(0.0053)	(0.0051)	
Home Bias $_{Distance}$			0.0070		-0.0251			-0.0025
			(0.0051)		(0.0201)			(0.0043)
Faraway Merger			0.0019		-0.0135			-0.0062**
			(0.0038)		(0.0183)			(0.0024)
Home Bias $_{Distance}$ × Faraway Merger			-0.0134*		0.0340			-0.0200***
			(0.0072)		(0.0283)			(0.0066)
Constant	-0.0155***	-0.0200***	-0.0230***	0.2292***	0.2166***	0.0149***	0.0408***	0.0424***
	(0.0031)	(0.0067)	(0.0072)	(0.0294)	(0.0299)	(0.0029)	(0.0043)	(0.0042)
Controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes
R-squared	0.037	0.066	0.065	0.105	0.104	0.008	0.022	0.035

Table 23. Simulation Evidence for Bidder Returns The table reports the average coefficients and empirical p-values for our regression simulations. For each home bias merger, we find a matching non-home bias candidate merger that satisfies the following criteria: (1) the candidate merger is announced within two years of the actual merger, (2) home bias and candidate bidders reside in the same state, (3) home bias and candidate targets reside in the same state, and (4) home bias and candidate bidders operate in the same industry. From list of candidate mergers, we randomly select one candidate merger for each home bias merger without replacement 1000 times. Then we run regression with same controls as Table 3 for the full sample as well as subsets of close and distant mergers. *, **, and *** represent empirical significance at the 10%, 5%, and 1% level, respectively.

Panel A: State-Based Home Bias Measures

		Mean Coefficient	Empirical p-value	N
Full Sample	*Home Bias* $_{State}$	-0.00250	0.140	1000
Cross-State mergers	*Home Bias* $_{State}$	-0.01185	0.007***	1000
In-State mergers	*Home Bias* $_{State}$	-0.00175	0.249	1000

Panel B: Distanced-Based Home Bias Measures

		Mean Coefficient	Empirical p-value	N
Full Sample	*Home Bias* $_{Distance}$	-0.00374	0.047**	1000
Faraway mergers	*Home Bias* $_{Distance}$	-0.00736	0.028**	1000
Close mergers	*Home Bias* $_{Distance}$	-0.00119	0.327	1000

Table 24. Calendar Time Bidder Returns

This table reports the effects of home bias on long-term performance. We use a calendar time approach and purchase each home bias merger beginning three days after the announcement and holding for 6, 12, and 24 months. We use the Fama-French 3-factor model to risk-adjust returns, and report the monthly alpha for the set of home bias mergers. We also calculate alpha for a randomly drawn set of matched non-home bias mergers based on the location and industry of the merged firms (as in Table 9). We report the average alpha for the 1000 simulated merger portfolios, as well as the empirical p-value that the merger portfolio underperforms the simulated portfolio.

Panel A : State Measures of CEO Home Bias

	Horizon	Home Bias Alpha	p-value	Simulated Alpha	Actual - Simulated	Difference Empirical p-value
All Mergers	6	-0.00633	**0.0143**	0.00058	-0.00691	0.0000
All Mergers	12	-0.00515	**0.0451**	-0.00031	-0.00484	0.0000
All Mergers	24	-0.00361	0.1089	-0.00372	0.00011	0.5010
Cross-State Mergers	6	-0.01455	**0.0021**	0.00285	-0.01739	0.0000
Cross-State Mergers	12	-0.01037	**0.0016**	0.00146	-0.01183	0.0000
Cross-State Mergers	24	-0.00673	**0.0087**	0.00208	-0.00881	0.0000

Panel B : Distanced-Based Measures of CEO Home Bias

	Horizon	Home Bias Alpha	p-value	Simulated Alpha	Actual - Simulated	Difference Empirical p-value
All Mergers	6	-0.00157	0.2585	0.00256	-0.00412	0.0010
All Mergers	12	-0.00375	**0.0441**	0.00345	-0.00720	0.0000
All Mergers	24	-0.00333	**0.0843**	0.00162	-0.00495	0.0000
Faraway Mergers	6	-0.00776	**0.0048**	-0.00133	-0.00642	0.0040
Faraway Mergers	12	-0.00435	**0.0500**	-0.00049	-0.00386	0.0320
Faraway Mergers	24	-0.00324	**0.0708**	-0.00040	-0.00284	0.0300

Table 25. Insider Trading around Home Bias Mergers

The table reports empirical p-value of our test of insider trading using simulations. For each home bias merger, we find a matching candidate merger that satisfies the following criteria: From list of candidate mergers, we randomly select one candidate merger for each home bias merger without replacement 1000 times. Then we count number of mergers in which the net trade was a purchase for the CEO, other Executives, or Board Directors during (-60,-10) and (2,60) trading days relative to the announcement date. Empirical p-value is number of simulations where percentage of purchase is higher for Home Bias Sample compared to that of Matched Sample. *, **, and *** represent empirical significance at the 10%, 5%, and 1% level, respectively.

Panel A : Unconditional Purchases

		State Variables		Distance Variables	
		Trade (t-60,t-10)	Trade (t+2,t+60)	Trade (t-60,t-10)	Trade (t+2,t+60)
CEO Buy	Home Bias Sample	0.0116	0.0367	0.0085	0.0294
	Matched Sample	0.0083	0.0153	0.0046	0.0163
	Empirical P-Value	0.1860	0.004***	0.1170	0.0300**
Director Buy	Home Bias Sample	0.0977	0.0936	0.0567	0.0759
	Matched Sample	0.0750	0.1405	0.0540	0.0979
	Empirical P-Value	0.052*	0.9800	0.3600	0.8780
Executive Buy	Home Bias Sample	0.0217	0.0339	0.0377	0.0315
	Matched Sample	0.0307	0.0740	0.0229	0.0416
	Empirical P-Value	0.6330	0.9940	0.0500**	0.7520

Panel B : Insider Disagreement

CEO Buy, Director not Buy	Home Bias Sample	0.0116	0.0155	0.0059	0.0222
	Matched Sample	0.0019	0.0063	0.0023	0.0090
	Empirical P-Value	0.01***	0.026**	0.0770*	0.0100***
CEO Buy, Executive not Buy	Home Bias Sample	0.0077	0.0225	0.0056	0.0181
	Matched Sample	0.0019	0.0050	0.0022	0.0063
	Empirical P-Value	0.017**	0.001***	0.0910*	0.0080***

Table A2: Variable Definitions:

A.1 Measures of Home Bias and Proximity

- *Home Bias $_{State.}$* – Dummy variable that is equal to one when the acquirer firm CEO birth state is equal to target headquarters state.
- *Cross State Merger.* – Dummy variable that is equal to one when the acquirer headquarters state is different from target headquarters state.
- *Home Bias $_{State}$ x Cross State Merger.* – Interaction between *Home Bias $_{State}$* and *Cross State Merger*.
- *Home Bias $_{Distance}$.* – Dummy variable that is equal to one when the distance between acquirer firm CEO Birth City and target headquarters is less than 100 miles.
- *Faraway Merger.* – Dummy variable that is equal to one when the distance between acquirer headquarters and target headquarters is greater than 100 miles.
- *Home Bias $_{Distance}$ x Faraway Merger.* – Interaction between *Home Bias $_{Distance}$* and *Faraway Merger*.

A.2 Other Variables

- *Δ Income (x100)* – Industry-adjusted three-year income growth used by Morck, Shleifer and Vishny 1990, defined as log(I(t-1)) – \log(I(t-4)), where I(t-1) is the sum of net income, interest, and deferred taxes for the fiscal year preceding the announcement.
- *E-index* – Entrenchment index of Bebchuk, Cohen and Ferrell 2009.
- *Institutional Ownership* – The (industry-adjusted) proportion of shares outstanding (in percent) in the hands of US independent, non-transient, long-term institutional investors, as defined by Chen, Harford, and Li (2007).
- *Industry Leverage* – Acquirer's industry median leverage across all Compustat firms classified using four-digit standard industrial classification (SIC) codes. Leverage is defined as representing the sum of long-term debt (dltt) and debt in current liabilities (dlc) over common equity (ceq).
- *Industry Tobin's Q* – Acquirer's industry median Tobin's Q across all Compustat firms (using four-digit SIC codes) divided by 100. See Tobin's Q.
- *Cash Deal* – Dummy variable that is equal to one when the acquisition is financed entirely with cash.
- *Stock Deal* – Dummy variable that is equal to one when the acquisition is financed entirely with bidder stocks.
- *Public Target* – Dummy variable that is equal to one when the target firm is publicly traded.
- *Leverage* – Sum of long-term debt (dltt) and debt in current liabilities (dlc) over common equity (ceq).
- *Log Total Assets* – Logarithm of total assets (at).
- *Low E-index* – Low entrenchment levels as measured by the E-index of Bebchuk, Cohen and Ferrell 2009. It is equal to one when the E-index is smaller than two.

- *Price Run-up* – Bidder's buy-and-hold abnormal return from 230 to 11 days before the announcement. The CRSP value-weighted index is used the
- *Relative Deal Size* – Value of the deal as reported by Securities Data Company over the market value of the acquirer measured at the end of the fiscal year preceding the announcement.
- *Tobin's Q* – Sum of the market value of book assets (at) and the market value of common equity (csho x prcc) minus the sum of common equity (ceq) and deferred taxes (txdb), all over the sum of 0.9 x book value of assets (at) and 0.1 x market value of assets.
- *Total Assets* – Total book assets (at) in billions of dollars.

Table A3. Home Bias based on MSA and Probability of Acquisition

The table reports probit regression with dependent variable of one if the observation is an actual merger and zero otherwise. We use Metropolitan Statistical Area (MSA) as a geographical unit in defining home bias. Home MSA Bias is a dummy variable that is equal to one when the acquirer firm CEO birth MSA is equal to target headquarters MSA. Cross MSA Merger is a dummy variable that is equal to one when the acquirer headquarters MSA is different from target headquarters MSA. Home MSA Bias x Cross MSA Merger is an interaction between Home MSA Bias and Cross MSA Merger. We include year fixed effects and standard errors clustered by industry are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively. A detailed description of each variable is included in Appendix A.

Variable	(1)	(2)	(3)	(4)
Home Bias $_{MSA}$	-0.0707	-0.0394	-0.0634	-0.1274
	(0.0529)	(0.0633)	(0.0844)	(0.0883)
Cross MSA Merger		0.0473	0.0348	0.0050
		(0.0537)	(0.0609)	(0.0642)
Home Bias $_{MSA}$ × *Cross MSA Merger*			0.0544	0.1229
			(0.1267)	(0.1312)
|Acq(B/M) – Tgt(B/M)|				-0.0986***
				(0.0298)
|Acq(ME) – Tgt(ME)|				0.0010**
				(0.0005)

Table A4. Home Bias based on MSA and Bidder Announcement Returns

This table contains regression results for bidder announcement returns on all control variables in Table 3 and MSA-based variable of interest. *Home Bias $_{MSA}$* is a dummy variable that is equal to one when the acquirer firm CEO birth MSA is equal to target headquarters MSA. *Cross MSA Merger* is a dummy variable that is equal to one when the acquirer headquarters MSA is different from target headquarters MSA. The first two columns (1-2) use same specifications as Table 3. Columns 3-6 use same specifications as Table 5. We include year fixed effects and standard errors clustered by industry are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively. A detailed description of each variable is included in Appendix A.

	CARs		Governance			
Variables	(1)	(2)	(3)	(4)	(5)	(6)
			High E-Index	Low E-Index	Low Inst Ownership	High Inst Ownership
Home Bias $_{MSA}$	-0.0077*	0.0004	-0.0094	0.0168	0.0155*	-0.0045
	(0.0040)	(0.0058)	(0.0102)	(0.0127)	(0.0090)	(0.0066)
Cross MSA Merger	-0.0072**	-0.0030	-0.0087	0.0209**	0.0071	-0.0103**
	(0.0033)	(0.0035)	(0.0066)	(0.0090)	(0.0058)	(0.0047)
Home Bias $_{MSA}$ × Cross MSA Merger		-0.0239*	-0.0099	-0.0467	-0.0735***	-0.0034
		(0.0131)	(0.0153)	(0.0363)	(0.0201)	(0.0114)
Relative Deal Value	-0.0216***	-0.0215***	-0.0115***	-0.0196**	-0.0248***	-0.0179***
	(0.0028)	(0.0028)	(0.0035)	(0.0073)	(0.0052)	(0.0035)
Log Total Assets	-0.0000	0.0000	-0.0075**	0.0077	-0.0009	0.0014
	(0.0012)	(0.0012)	(0.0036)	(0.0047)	(0.0014)	(0.0038)
Industry Leverage	-0.0020***	-0.0020***	-0.0018	0.0003	-0.0027**	-0.0019*
	(0.0006)	(0.0006)	(0.0015)	(0.0013)	(0.0012)	(0.0010)
Industry Tobin's Q	0.0000	0.0000	-0.0007***	0.0000	-0.0006*	-0.0002
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)	(0.0003)
Δ Income	-0.0000**	-0.0000**	-0.0011**	0.0040***	-0.0000*	-0.0008**
	(0.0000)	(0.0000)	(0.0005)	(0.0015)	(0.0000)	(0.0003)
Price Run-up	-0.0014	-0.0014	0.0003	-0.0005	-0.0015	-0.0023
	(0.0012)	(0.0011)	(0.0022)	(0.0033)	(0.0025)	(0.0020)
Cash Deal	-0.0005	-0.0005	0.0004	-0.0075**	-0.0012	-0.0006
	(0.0021)	(0.0020)	(0.0042)	(0.0036)	(0.0028)	(0.0042)
Stock Deal	0.0048**	0.0048**	0.0059	0.0099	0.0076**	0.0044
	(0.0020)	(0.0020)	(0.0049)	(0.0086)	(0.0037)	(0.0032)
Public Target	-0.0104**	-0.0104**	-0.0061	0.0018	-0.0051	-0.0145**
	(0.0044)	(0.0043)	(0.0052)	(0.0086)	(0.0043)	(0.0066)
Constant	0.0304***	0.0268***	0.0260**	-0.0282**	0.0233	0.0306***
	(0.0067)	(0.0070)	(0.0118)	(0.0136)	(0.0146)	(0.0074)
R-squared	0.053	0.054	0.068	0.089	0.085	0.055

Table A5. Bidder Returns with Different Event Windows

This table contains regression results for different bidder announcement returns on all variables in Table 3 and 5. Only our variables of interest are included for brevity. We include year fixed effects and standard errors clustered by industry are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively. A detailed description of each variable is included in Appendix A.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	CAR(-1,0)	CAR(0,1)	CAR(-3,3)	CAR(-3,3)	CAR(-1,0)	CAR(0,1)	CAR(-3,3)	CAR(-3,3)
Home Bias $_{State}$	0.0049	0.0035	0.0089	0.0067				
	(0.0031)	(0.0034)	(0.0072)	(0.0070)				
Cross State Merger	-0.0022	-0.0010	-0.0020	-0.0029				
	(0.0020)	(0.0026)	(0.0036)	(0.0034)				
Home Bias $_{State}$ × *Cross State Merger*	-0.0136***	-0.0120**	-0.0211**	-0.0177*				
	(0.0050)	(0.0059)	(0.0097)	(0.0095)				
Home Bias $_{Distance}$					0.0025	0.0005	-0.0005	-0.0016
					(0.0024)	(0.0024)	(0.0039)	(0.0037)
Faraway Merger					-0.0030	-0.0014	-0.0050*	-0.0052*
					(0.0019)	(0.0018)	(0.0027)	(0.0028)
Home Bias $_{Distance}$ × *Faraway Merger*					-0.0137***	-0.0087	-0.0216**	-0.0185**
					(0.0040)	(0.0062)	(0.0083)	(0.0089)
Constant	0.0171***	0.0299***	0.0420***	0.0379***	0.0182***	0.0310***	0.0447***	0.0401***
	(0.0036)	(0.0054)	(0.0056)	(0.0060)	(0.0036)	(0.0046)	(0.0050)	(0.0056)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.043	0.063	0.05	0.051	0.046	0.067	0.052	0.054

Figure 1

"Off-style" Holdings of Mutual Funds

This figure shows the time series plot of the value weighted percentage of holdings that do not match funds' stated investment styles in the U.S. actively managed equity mutual fund universe. The data is from CRSP Mutual Fund Database with a sample period of 1992 to 2014.

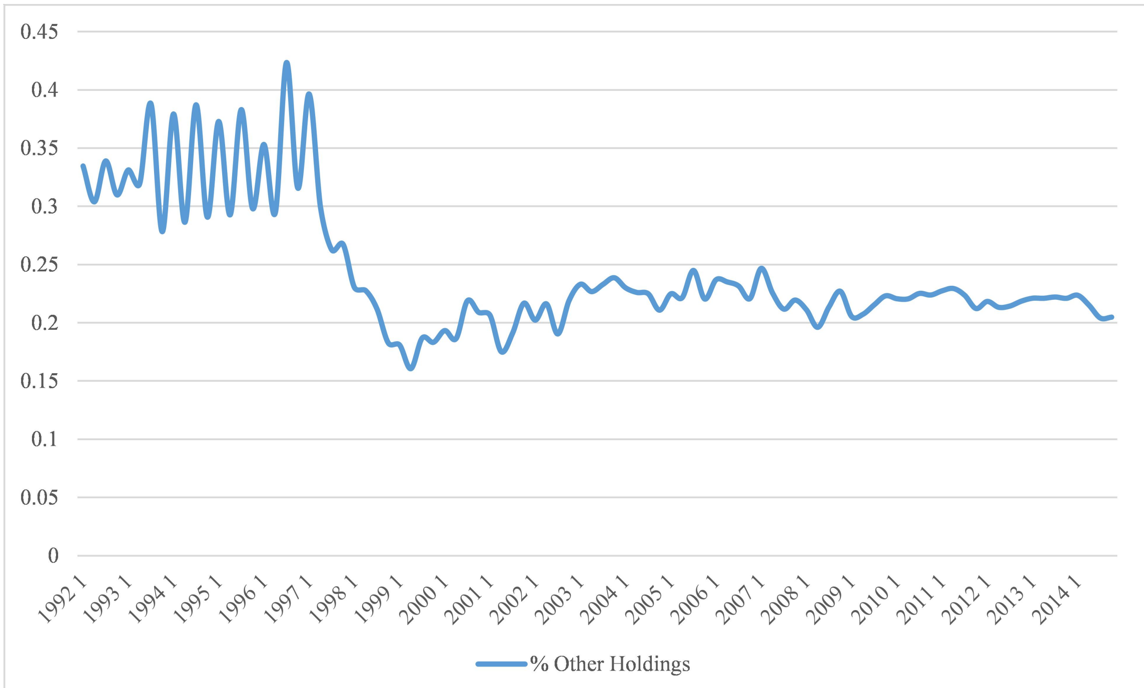

Table 26

Sample Summary Statistics

The table reports the number of observations, mean, median, and standard deviation of mutual fund variables for the sample period of 1992 to 2014. Size Category equals 1 for large cap funds, 2 for mid cap funds and 3 for small cap funds. Fund Size is total net asset of funds in my sample in million dollars. Family Size is total assets managed by fund family in billion dollars. Average Stock held is number of stocks held by each fund. Style Match Pct. is percentage of assets that match in investment style with stated investment style of each fund.

	N	Mean	Std. Dev.	Median
Size Category	4465	1.64	0.79	1
Fund Size	4465	965.46	4834.39	126.46
Family Size	4465	8217.69	67691.96	169.05
Average Stock held	4465	120.06	238.54	60
Style Match Pct.	4465	0.64	0.34	0.73

Table 27

Performance of “Off-style” Stocks

This table reports the monthly portfolio performance of stocks that are not in line with the state investment style of mutual funds. For each month, I equal weight the “non-regulated” stocks and run factor model regressions for the sample period of 1992 to 2014. Standard errors are reported in parenthesis.

	Raw Return	Excess Return	CAPM Alpha	Fama-French Alpha	Carhart Alpha	Carhart Alpha + Liquidity
Large-cap Off-style Stocks	0.0102***	0.0016*	0.0006	0.0001	0.0009	0.0004
	(0.0031)	(0.0010)	(0.0009)	(0.0008)	(0.0008)	(0.0008)
Large-cap On-style Stocks	0.0097***	0.0011*	0.0008	0.0002	0.0011**	0.0007*
	(0.0027)	(0.0006)	(0.0006)	(0.0005)	(0.0004)	(0.0004)
Mid-cap Off-style Stocks	0.0112***	0.0027*	0.0017	-0.0001	0.0010	0.0006
	(0.0033)	(0.0015)	(0.0015)	(0.0009)	(0.0008)	(0.0008)
Mid-cap On-style Stocks	0.0115***	0.0030**	0.0019	0.0004	0.0012	0.0009
	(0.0033)	(0.0015)	(0.0014)	(0.0009)	(0.0009)	(0.0009)
Small-cap Off-style Stocks	0.0137***	0.0052**	0.0037*	0.0014	0.0030***	0.0029***
	(0.0038)	(0.0022)	(0.0021)	(0.0011)	(0.0009)	(0.0009)
Small-cap On-style Stocks	0.0130***	0.0045**	0.0034*	0.0008	0.0023***	0.0022***
	(0.0036)	(0.0020)	(0.0020)	(0.0010)	(0.0008)	(0.0008)

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