Do Shocks to Personal Wealth Affect Risk Taking in Delegated Portfolios?*

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Abstract

Using exogenous wealth shocks stemming from the collapse of the housing market, we show that managers who experience substantial losses in their home values subsequently reduce the risk in their funds. The decline in fund risk is seen in total risk, idiosyncratic risk, systematic risk, and in tracking error. Our paper provides evidence that the idiosyncratic personal preferences of mutual fund managers affect their professional decisions and offers a methodology for testing for manager effects that is not subject to the selection critique of Fee, Hadlock, and Pierce (2013).

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While mutual fund managers are hired to act in the sole interests of their clients, there is mounting evidence that differences in the personal backgrounds and characteristics of fund managers are reflected in their investment decisions. For example, Greenwood and Nagel (2009) show that young managers invest more in technology stocks prior to the technology bubble. Hong and Kostovetsky (2012) find that mutual fund managers are motivated by political ideology in their portfolio management decisions and Pool, Stoffman, and Yonker (2012) show that managers over-invest in companies headquartered in their home states. In the broader literature, Bertrand and Schoar (2003) has spawned a cottage industry of research into how managers "matter."[1]

But do managers matter? An alternative explanation, which poses a challenge to research that tries to link manager characteristics to delegated portfolio decisions, is selection. Instead of managers imprinting their own preferences on the fund, they may be selected to be managers precisely because their personal traits and characteristics make them best able to implement the preferences of their clients. Since those "selected" characteristics of the manager are typically thought to be time-invariant, researchers have relied on manager turnover to identify the "manager effects." But turnover is not usually exogenous, and selection of managers is certainly not random. If managerial characteristics can be observed by the researcher, then surely they can be uncovered during a lengthy interview process and used to select the manager who is most likely to follow the fund's preferred policies. Indeed, in the corporate finance literature Fee, Hadlock, and Pierce (2013) provide evidence that CEO's are selected *because* of their personal style.[2]

Given this challenge, a more powerful test of the existence of idiosyncratic manager effects is to study changes in fund policies that occur after an idiosyncratic shock to a manager's preferences or beliefs. This allows the analysis to be conducted *within* managers, thereby differencing out each manager's fixed style, leaving only the effects of idiosyncratic changes in attitudes. We adopt this strategy by examining the effects of shocks to managers' personal wealth on professional risk taking. The mutual fund industry provides a nice setting for this research as managers regularly make investment decisions, which are observable at a relatively high frequency. Additionally, the amount of risk taking can easily be measured.

[1]In corporate finance, numerous papers find that managers' personal traits are reflected in corporate decisions as well (for example, Malmendier and Tate (2005, 2008) and Cronqvist, Makhija, and Yonker (2012)).

[2]In addition, Bodnaruk and Simonov (2014) argue that mutual fund managers self-select into fund styles based on their loss-aversion.

Shocks to wealth provide a useful source of identification - and present an interesting economic question in and of themselves. Such shocks can be measured at the manager level over time, and have been shown to induce changes in risk attitudes (Guiso, Sapienza, and Zingales, 2014). Therefore, finding that personal wealth shocks lead to changes in risk taking at the fund provides important evidence that managers affect firm decisions and are not merely selected to implement predetermined investment strategies.

To conduct our tests, we use the largely unexpected bursting of the real estate bubble in 2007–2008 as an exogenous shock to the personal wealth of fund managers.[3] Our detailed data on the values of managers' homes allow us exploit the large variation in house price changes during this period. This is an ideal setting to utilize the differences-in-differences framework, enabling us to test whether idiosyncratic changes in managers' personal risk attitudes lead to changes in risk taking in their professional portfolios.

Malmendier and Nagel (2011) argue that changes in risk attitudes can stem from either changes in *preferences* or changes in *beliefs*. In the absence of agency conflicts, even if wealth shocks influence managers' risk preferences, they should not influence managers' *professional* decisions since managers are hired to invest for the benefit of their clients. But given the ample evidence of agency conflicts in the mutual fund industry (Chevalier and Ellison, 1997; Davis and Kim, 2007; Guercio and Reuter, 2014), changes in risk preferences could affect risk taking in the professional domain. For example, if managers have an ownership stake in the funds they manage,[4] they may view their funds as a simple extension of their personal portfolio. If managers' risk attitudes change following a negative shock to personal wealth, they may reduce risk taking in the professional domain simply to reduce the risk of their personal portfolios. Alternatively, managers may change the risk profile of their funds in response to negative wealth shocks—even if they do not have an ownership stake—because it directly affects the riskiness of managers' returns to human capital. For example, Chevalier and Ellison (1999) show that managerial turnover is sensitive to recent performance in the mutual fund industry, especially for younger managers, and that these managers respond by taking less risk in

[3] Cheng, Raina, and Xiong (2014) provide evidence that the financial crisis was largely unexpected, even among people working in the area of financial securitization.

[4] Fund managers report an ownership stake in over 50% of U.S. equity funds (Khorana, Servaes, and Wedge, 2007). Such ownership is encouraged by the S.E.C., which argues that the level of a manager's ownership is "a direct indication of his or her alignment with the interests of shareholders in that fund." (See `http://www.federalregister.gov/a/04-19575/p-101`.)

their portfolios. Therefore, career concerns may drive managers to dial back risk in their funds following a shock to their personal wealth.[5]

Experiencing a shock may also affect beliefs, causing managers to update their expectations of future economic outcomes.[6] Wealth-shocked managers may become more pessimistic, leading them to take less risk in *both* their personal and professional portfolios, even in the absence of any agency conflicts. This behavioral consistency across domains has been one mechanism through which the traits and characteristics of managers have been shown to influence both corporate and portfolio management decisions (Malmendier and Tate, 2005, 2008; Cronqvist et al., 2012; Greenwood and Nagel, 2009; Hong and Kostovetsky, 2012).

To translate house price changes into wealth shocks in an economically meaningful way, we need to evaluate these price changes relative to each manager's total wealth. (A manager whose house loses $100,000 of value probably will not be too upset if his net worth is in the tens of millions of dollars.) Therefore, we begin our empirical analysis by estimating the wealth of fund managers in our sample of actively managed U.S. domestic equity mutual funds. To do so, we follow Dittmann and Maug (2007) to develop an estimation technique that uses information about managerial employment and income history. We also extend their model by incorporating a corresponding history of market index returns. We estimate that the median mutual fund manager has $2.3 million in financial assets at the end of 2006, but the variation across managers is large, with an interquartile range of $5.5 million. After generating these wealth estimates, we validate them by testing how well they can predict the cross section of manager housing wealth, which is estimated using an unrelated data set. We find that log wealth is significantly positively associated with log housing wealth—even within manager home zip codes, so our wealth estimates have power to predict which managers own which homes within small geographic areas. This is an especially tough test, and provides strong evidence for the validity of our wealth measure.

Having estimated wealth and validated the estimates, we next define the treatment for our differences-in-differences setup. First, we collect data on personal real estate holdings during the

[5]In addition to the threat of termination, another source of risk concerning mutual fund managers' future labor income is adverse fund flows. However, due to the convex flow-performance relationship (Sirri and Tufano, 1998), we would not expect managers to try to avoid adverse fund flows by reducing fund risk.

[6]Malmendier and Nagel (2011) show that experience affects personal portfolio decisions, and relate this to the psychology literature showing that experiences influence personal decisions, especially those that are more recent (Nisbett and Ross, 1980; Weber, Böckenholt, Hilton, and Wallace, 1993; Hertwig, Barron, Weber, and Erev, 2004).

collapse of the housing market for our sample of fund managers. We then compute the percentage change in wealth due to housing (hereafter, "wealth change"), which is defined as the dollar change in the value of a manager's homes during 2007–2008 (the "event period") divided by estimated wealth. Managers in our sample are assigned to the treatment group if their wealth change is below a given threshold. For example, in our baseline regressions, treated managers are those whose wealth change is less than the sample median (−3.5%), while the control group consists of those whose wealth change is above the median.[7]

We then compute fund risk measures prior to, and after, the event period and take differences within manager-fund pairs. Finally, we estimate the impact of idiosyncratic changes in manager personal wealth on risk taking in delegated portfolios by regressing these differences on a treatment group dummy variable along with a variety of control variables. This regression framework effectively implements a manager fixed effects specification, while eliminating dependence among variables in the pre- and post-event periods, as advocated by Bertrand, Duflo, and Mullainathan (2004).

We find robust evidence that managers who suffer a shock to their personal wealth subsequently reduce the risk of their delegated portfolios. This is consistent with shocks to personal wealth inducing an increased distaste for risk, which then translates into lower risk taking in the fund. In our baseline model, wealth-shocked managers lower the total risk of their funds by about 11% relative to the control group of managers who are not shocked. This reduction in risk is economically large, given that it is induced by about a 20% reduction in total wealth. By comparison, an agent with log utility who suffers a 20% reduction in wealth would reduce risk by 20% in his *personal* portfolio.

Our finding that managers reduce risk taking following wealth shocks is consistent across numerous specifications, including alternative definitions of the wealth shock, various event windows, and a variety of control variables. We also find that the decline in total fund risk is due to reductions in both idiosyncratic risk and systematic risk. Additionally, mangers lower the tracking error of their funds.

[7] While −3.5% is the threshold, it is important to note that treated managers in this baseline model experienced reductions in wealth of approximately 20%, on average, and those in the control group experienced almost no wealth change.

We provide several additional tests to support these results. First, since our identification strategy relies on the assumption of parallel trends before the event, we also estimate a dynamic model to confirm that there are no differences in risk taking between treated and untreated managers in the pre-event window. Second, the results are robust to using characteristics-matched samples of treated and untreated managers using propensity score matching. Third, to address the concern that our results could be driven by our wealth estimates, we rerun the analyses by decomposing our wealth change measure into the change in the value of housing and the fraction of wealth that is attributed to housing. We also conduct a placebo test by randomizing housing price changes across managers. Fourth, we rule out a number of alternative explanations for the effects of the wealth shock, such as mutual fund shareholders experiencing correlated wealth shocks ("clientele effect"), or other local effects such as a change in the risk-return characteristics of local stocks. Fifth, we re-estimate our results including a control for leverage for the subsample of managers for whom we have mortgage data. Finally, we show that our results are not driven by fund family, location, or fund strategy effects.

Having shown that managers reduce risk taking in their delegated portfolios after suffering a personal wealth shock, we next turn to an analysis of *how* they do it. We show that treated managers increase the number of stocks in their portfolios, reduce the concentration of their holdings in any one stock, diversify their holdings across industries, reduce the active share of the portfolio, and switch to stocks that are, *ex ante*, less risky.

Finally, while our primary objective in this paper is to test for manager effects, we provide preliminary tests using manager characteristics such as age, experience, and the amount of personal money they invest in the fund, to explore the alternative channels through which wealth shocks would affect risk taking, which we outlined earlier. Our results are inconsistent with the notion that fund managers reduce risk because of their ownership stake in their funds, but we do find weak evidence consistent with career concerns.

Our paper contributes to the emerging literature that argues that personal traits and biases of managers affect their professional decisions. We show that these influences could occur not just through behavioral biases, but through such primitives as manager risk preferences and beliefs. And, in contrast to previous papers, our study is explicitly designed to address selection: the concern that managers are merely selected by the fund's board to best execute the preferences of their shareholders.

1 Data and sample construction

1.1 Mutual fund data

We obtain information on fund managers from Morningstar, which reports the name of each manager of a fund (including individuals on team-managed funds), and their start and end dates with the fund. We also use Morningstar for information on funds' cash holdings. We limit the sample to actively managed U.S. equity funds by filtering Morningstar style categories and manually screening fund names.

We use fund CUSIP and ticker information to establish an initial match between Morningstar and the CRSP Survivor Bias-Free Mutual Fund Database. We then screen these initial matches by requiring that manager names are the same in both databases (when managerial information is available in CRSP), inception dates for the same share classes are the same in both databases, and fund names are sufficiently similar.

We get fund characteristics and returns from the CRSP mutual fund database. We combine share classes in CRSP into fund-level variables using MFLINK, so fund age is calculated as the age of the oldest share class, fund size is the sum of the total net assets (TNA) of all share classes, and fund returns and expense ratios are calculated as the TNA-weighted average returns and expense ratios of the share classes, respectively.

For our main analysis, we measure risk as the quarterly standard deviation of daily fund returns. We also use the holdings-based measures of risk developed by Huang, Sialm, and Zhang (2011). These measures are computed using the Thomson Financial CDA/Spectrum Mutual Fund database, which contains the quarter-end holdings reported by mutual funds in mandatory SEC filings. Thomson uses two date variables, RDATE and FDATE, which refer to the actual date for which the holdings are valid and the Thomson vintage date on which the data were cut, respectively. We follow standard research practice and restrict our sample to those observations where FDATE and RDATE reference the same quarter to avoid the use of stale data in our analysis. We drop observations when the stock price, CUSIP, or the number of shares held are missing.

To focus on funds for which we can properly control for investment style, we restrict our sample to those funds with a Morningstar category in the 3-by-3 size/value grid (US Large Blend, US Large Growth, US Large Value, US Mid-Cap Blend, US Mid-Cap Growth, US Mid-Cap Value, US Small Blend, US Small Growth, or US Small Value). We also remove funds with fewer than 20 holdings, more than ten managers, total net assets below $20 million at the end of 2004, and those that do not exist for at least one quarter during our pre-event and post-event periods (defined later).

We augment these standard mutual fund databases with the home addresses of fund managers following the method of Yonker (2010) and Pool, Stoffman, and Yonker (2012) to identify managers in the LexisNexis Public Records database using name, age, and location searches.[8] These data include a history of all addresses associated with each person.The data are extensive, drawing on public records from county tax assessor records, state motor vehicle registrations, reports from credit agencies, court filings, and post office records, among other sources. In addition to home addresses, the LexisNexis Public Records database also contains mortgage information for a subsample of managers.

1.2 Housing wealth

We measure the value of fund manager homes using data collected from Zillow, a web site that compiles data from county assessor records on home size, home characteristics, and lot size. Zillow uses a proprietary method to generate estimates of home values (Zestimates®) for over one hundred million homes in the U.S., including single-family homes and condominiums. Rental properties are not generally included in the data that we downloaded, so an apartment rented by a fund manager is unlikely to be included in our sample.[9] For each manager home identified in LexisNexis, we search for the address on Zillow, and download the history of monthly Zestimates. These are available over a ten-year history, and since we conducted our data collection in early 2014, the address-level home price data go back to 2004.

[8] See Pool, Stoffman, and Yonker (2012) for a detailed explanation of the matching procedure.
[9] For a detailed description of how Zestimates are calculated, see `http://www.zillow.com/research`.

We are able to gather residential addresses and corresponding home values for 737 managers in the database. Our final sample includes 778 funds and 1,151 manager–fund observations. The sample coverage is very similar to that of Pool, Stoffman, and Yonker (2014), who show that their sample is representative of the broader sample of mutual funds, covering over 80% of actively managed mutual funds by assets under management.

1.3 Total manager wealth

To translate home price changes into wealth shocks in an economically meaningful way, we need to scale these changes by each manager's total wealth. Information on the personal wealth of mutual fund managers is not readily available however. We therefore estimate a fund manager's total wealth by extending the wealth equation proposed by Dittmann and Maug (2007). Our algorithm is described in detail in the Internet Appendix. To summarize the approach, we start by estimating each manager's income each year, and then use an iterative procedure to estimate their wealth each year. We run our iterative procedure without any data restrictions, using all fund and manager histories from the CRSP and Morningstar databases.

In Figure 1 we plot the distribution of estimated manager wealth at the beginning of the event period. Median wealth is $2.3 million, although the distribution is highly skewed, with about 10% of managers having wealth greater than $15 million. While we must make a number of assumptions to estimate wealth, our results do not appear to be sensitive to particular values of these parameters.

Since our algorithm introduces an approach to the mutual fund literature for assessing the personal wealth of mutual fund managers, it is important to provide external validation that the estimates are reasonable. We summarize the results of these tests in Table 1. We show that our wealth estimate is highly correlated with housing wealth, which is estimated using a data source that is completely unrelated to the data we use to estimate manager wealth. We measure total housing wealth as the sum of the value of all properties owned by the manager at the end of 2006 using Zillow estimates. We then regress log total housing wealth on our standardized manager wealth estimates as well as additional standardized control variables. Columns 1 and 2 confirm that manager total wealth is significantly related to housing wealth, even after controlling for the variables that are used to estimate total wealth. In column 3 we add fixed effects for the zip code where the fund management company is located, and find similar results. In column 4, we include manager home zip code fixed effects, which, not surprisingly, dramatically increases the adjusted R^2.[10] In this specification, wealth remains significantly related to housing wealth while the other variables are no longer significant. The last column adds income, which provides no additional explanatory power. These last two specifications show that even *within a particular zip code*, managers with higher estimated total wealth own more expensive homes. This is an especially tough test, and provides strong evidence that our wealth estimation procedure produces a useful measure.

2 Methodology

2.1 Empirical setup

We are interested in whether changes in fund manager personal wealth affect risk taking in their delegated portfolios. To test this question, we use a differences-in-differences framework. In our experiment, treated managers receive a negative shock to their personal wealth, while untreated managers do not. Formally, we estimate regressions of the form

$$\Delta\bar{\sigma}_{i,j} = \gamma_w WealthShock_i + \Gamma' \Delta\overline{Controls}_{i,j} + \epsilon_{i,j}, \tag{1}$$

where $\Delta\bar{\sigma}_{i,j} = \bar{\sigma}^{post}_{i,j} - \bar{\sigma}^{pre}_{i,j}$ is the difference in the average risk between the post-event period and the pre-event period for fund j over those quarters in which manager i is a manager of the fund. $WealthShock_i$ is a dummy variable that is one if manager i is treated with the wealth shock, and $\Delta\overline{Controls}_{i,j}$ is a vector of changes in pre- and post-event average manager- and fund-specific control variables.

A common approach in a differences-in-differences setting would be to include all quarterly observations of risk in both the pre- and post-event periods for each fund manager, but Bertrand et al. (2004) show that serial correlation leads to downward biased standard errors in traditional differences-in-differences analyses. We therefore follow the simple but effective solution that they

[10]For managers with multiple homes, the zip code of the primary home is used, where the property closest to the management company is considered the primary home.

propose, which is to eliminate the time series variation by collapsing the sample to one observation each from the pre- and post-event periods by taking averages in each period.[11]

Because we are interested in manager behavior and define the treatment at the manager level, we take differences at the manager-fund level. As noted in the introduction, this also has the benefit that any "selected" characteristics of managers are differenced out. Of course, this means that when a fund has multiple managers, there will be repeated observations of fund-specific variables, so we cluster standard errors by fund throughout our empirical analyses. (We also report results using alternative standard errors in the Internet Appendix.)

The primary coefficient of interest in regression (1) is γ_w, which measures the effect on fund risk of an idiosyncratic shock to manager personal wealth. As discussed in the introduction, in the absence of manager effects, an idiosyncratic change in wealth will not affect risk taking in the fund, and γ_w would be zero. Importantly, the regression effectively includes manager fixed effects, since we take differences at the manager-fund level. Our estimate of γ_w therefore measures how managers change their individual risk taking behavior at the same fund following the treatment—we are not merely looking at group averages, but within actual individuals and funds.

2.2 Implementation

To implement the empirical methodology we need to identify a subsample of our managers that has been treated with a personal wealth shock that would induce a change in risk attitudes. Ideally, the treatment should be random, exogenous, and unexpected. We use changes in managers' real estate wealth during the recent housing crisis to define our treatment and control groups. While one may think that professional managers anticipated the housing collapse and adjusted their real estate holdings accordingly, Cheng et al. (2014) provide evidence that the financial crisis was largely unexpected. They show that even managers in securitized finance were largely unaware of the looming end of the housing bubble.

The left panel of Figure 2 shows the time series of aggregate housing prices from January, 1999 through December, 2013 using the seasonally adjusted house price index of the Federal Housing Finance Agency. There is a large decline in home prices from the end of 2006 through the end of

[11] The results are qualitatively similar when we do not collapse the sample.

2008. We call this period the "event period" and define housing wealth shocks over this period. During the event period prices fell 12.5% on average. The pre- and post-event periods in our experiment are defined as the eight quarters immediately preceding and following the event window, respectively.

To be useful as a treatment, the housing crisis must affect some managers differently than others. The right panel of Figure 2, which shows the distribution of home price changes in 10,000 zip codes tracked by Zillow, confirms that there is large variation in home price changes during the event period. Homeowners in zip codes in the top quartile of the distribution of home price changes experience an average drop of approximately 7% in value, while those in the lowest quartile experience average declines of 25%. These cross-sectional differences in house price changes are the one of the sources of variation that defines our treatment.

A potential concern with relying on cross sectional differences in house prices to define our treatment is that it could lead to systematic geographic clustering in the treatment and control groups. This is not the case, however. Home price changes vary considerably during the crisis even within major metropolitan areas, as shown in Figure 3, where we plot the distribution of these changes during the event window across zip codes for each of the six metropolitan areas with the largest number of mutual fund managers in our sample (Boston, New York, Washington, San Francisco, Minneapolis, and Houston). Within all six metropolitan areas, some zip codes experienced price increases of over 5% during the crisis, while others experienced declines of more than 15%. As we will show later, this—along with the fact that many managers own multiple homes in multiple locations in our sample—results in shocked managers residing in nearly every locale.

To formally define our treatment, we calculate the wealth change for each manager during the event period. This is defined as the dollar change in the value of the manager's portfolio of homes during the event period divided by the manager's wealth at the beginning of the event period. To construct the numerator of this measure we use the housing wealth measure described in Section 1.2 above. To calculate the change in the value of those homes that managers own over the entire event period, we simply take the difference between the corresponding end and beginning period home values. For homes that are purchased during the event period, we use a similar calculation but replace the beginning period home value with the value at the time of purchase to calculate the change. Analogously, for properties that were sold prior to the end of the event period, we substitute the end-of-period value with the value at the time of the sale. For each manager, we then aggregate all dollar changes in the value of their homes during the event period. For all values, we use address-level home values from Zillow as described in Section 1.2. Finally, the denominator of our measure of the wealth change is our estimate of 2006 year-end manager wealth, computed using the methodology described in Section 1.3. We winsorize the home price changes, the wealth estimates, and the final ratio at the 1st and 99th percentiles.

Figure 4 shows the distribution of the wealth change in the sample. The average change is -9.7% and the corresponding standard deviation is 20.3%. Regression (1) relies on an indicator ($WealthShock_i$) that captures whether a manager is treated with a wealth shock. We therefore define $WealthShock_i$ as a dummy variable that takes a value of one if manager i's wealth change is below the median, which is -3.5% in the sample. In robustness tests, we also define treatment cutoffs based on the bottom third and bottom quarter of the distribution of wealth change. These cutoffs correspond to decreases in housing wealth of -7.9% and -10.8%, respectively, and are indicated in the figure. In these specifications the control groups are defined as managers in the top third and quarter of the distribution, respectively.

It is important to note that the large mass just to the left of zero in Figure 4 is not driven simply by the numerator of our wealth change measure. Rather, since housing wealth represents a small proportion of total wealth for some managers, even large percentage changes in housing value correspond to relatively small changes in total wealth for these managers.

Finally, as mentioned earlier, it is important to verify that treated managers are not geographically concentrated in just a few cities. Therefore, in Figure 5 we show the cities in which the managers in our sample own homes, and the proportion of managers in each city who meet each wealth shock cutoff. For the figure, we define cities using an agglomerative hierarchical clustering algorithm that begins with each zip code as a singleton, and continues to join zip codes into clusters until each cluster is at least 50 miles apart. Each circle denotes a location where managers have homes, with the size of the circle indicating the number of unique managers with homes in the area. The proportion of managers who are shocked at each treatment cutoff is shown as shaded slices of the circle. As is clear from the figure, treated managers are widely dispersed. Most cities have managers who experience a wealth shock as well as managers who do not.

3 Results

3.1 Summary statistics

Summary statistics for our variables of interest are presented in Table 2. Median housing wealth is \$1.4 million, compared to median total wealth of \$2.3 million. As noted above, these variables have highly skewed distributions with large outliers. The median manager is 43.5 years old and has about 6 years of experience, which is calculated as the number of years since the manager first began working as a portfolio manager at any fund. Looking at the risk taking measures, the effects of the financial crisis are clear: during the pre-event period, the median fund–manager pair has average total risk (measured as the quarterly standard deviation of daily returns) of 0.74, which almost doubles during the event period.

Differences between the treatment and control groups are presented in Panel B of the table. Since the treatment is determined based on change in wealth due to housing, there is of course a large and significant difference in this variable during the event period. On average, managers in the control group experience almost no change in wealth due to changes in their home values, while the wealth of those in the treatment group is reduced by 20%. The remaining fund and manager characteristics, computed based on the pre-event period, reveal that treated managers are, on average, younger, less experienced, manage smaller funds, and are part of larger management teams. These differences are to be expected, since age and fund size are positively related to manager wealth. Wealthier managers are less likely to be classified as treated because even large changes in the value of their homes can be small relative to their total wealth. Managers in the treated group also have higher fund turnover and somewhat higher expense ratios, but their funds perform similarly to those in the untreated sample. To verify that these differences do not drive our results, we show in Section 3.3.2 that our results continue to hold in a matched sample constructed using propensity score weighting, which removes these differences between the treatment and control groups.

3.2 Wealth shocks and risk taking

If idiosyncratic changes in manager risk attitudes do not affect their professional decisions, then there should be no relation between idiosyncratic changes in manager personal wealth and changes in risk taking in their delegated portfolios. We test this *lack of idiosyncratic style* hypothesis by estimating equation (1) using the methodology described in Section 2. The dependent variable in the model is the difference in the average realized risk between the post-event period and the pre-event period. To calculate this difference, we first compute daily total fund return volatility during each quarter in the pre- and post-event periods in which the manager is with the fund, measured as the standard deviation of daily fund returns. We then take the average of these quarterly volatility observations over the pre- and post-event periods, respectively, as described in Section 2.1. Our dependent variable is the difference between the post- and pre-event period averages. Under the lack of idiosyncratic style hypothesis, the estimate of γ_w should be indistinguishable from zero.

Table 3 reports the estimation results using alternative treatment cutoffs and definitions of the event period. In all specifications, the tests reject the lack of idiosyncratic style hypothesis. The estimate of γ_w is significantly negative in each column of the table. This means that, on average, managers who lose a substantial portion of their wealth due to a decline in the value of their homes, subsequently lower the risk in their professional portfolios relative to those who do not experience a housing wealth shock. This is consistent with shocks to wealth increasing managers' distaste for risk, which translates to risk taking in the managers' delegated portfolios.

Column 2 reports the coefficients for our baseline model, which includes controls for changes in performance, fund size, expenses, and turnover. The estimate of γ_w is -0.084 and is significant at better than the 1% level. Since average total volatility during the pre-event period is 0.79%, this estimate suggests that wealth-shocked managers reduce risk by about 10.6% relative to managers in the control group. This 10.6% reduction in risk is induced by a 20% reduction in wealth (see Panel B of Table 2).

Admittedly, defining our treatment by splitting the sample by the median change in wealth is somewhat arbitrary. Therefore, in columns 3 and 4 of the table we estimate our results using alternative definitions of the treatment and control groups. A manager is in the treated group in column 3 (4) if his wealth change is in the bottom third (quarter) in our sample. The corresponding control groups in these regressions include those managers whose wealth change is in the top third (quarter) in the sample. Using these definitions the average difference in wealth change between the treated and control groups is -29% and -47% for the two alternative treatment definitions, respectively. Consistent with our baseline results, γ_w is estimated to be significantly negatively in both columns. Additionally, the estimated γ_w's increase monotonically as we require larger wealth shocks for our treatment cutoffs. The estimates suggest that the personal wealth elasticity of total fund risk (% change in total fund risk scaled by the % change in personal wealth) is between 0.42 and 0.53.

Since our event period coincides with the financial crisis, we ensure that the results are not sensitive to our definition of a particular event window by using two alternative event periods over which we calculate the wealth changes. Specifically, in column 5 we use June 30, 2008 as the end date of our event period and in column 6 we set the end date to September 30, 2008. While the results are somewhat smaller in magnitude, the estimates of γ_w are still negative and statistically significant at better than the 1% level.

Similarly, using eight quarters around the event period to define our pre- and post-event windows is also somewhat arbitrary. *Ex ante*, it is hard to know how long the effect of a wealth shock may last. Thus, in columns 7 and 8 of the table we alter the length of the pre- and post-event periods. In column 7, we use four quarters for both the pre- and post-event windows, while in column 8 we use two. In both specifications the estimates are consistent with the previous findings: wealth-shocked managers lower their funds' risk relative to managers who are not shocked.

3.2.1 Risk components

We next investigate whether the reduction in total risk is due to changes in idiosyncratic risk or in systematic risk. To do this, we decompose fund risk in each quarter into a systematic and an idiosyncratic component by estimating a market model using daily fund returns. Daily returns on the market portfolio are computed as the value-weighted returns of the stocks in the CRSP universe. We measure the fund's systematic risk for the quarter as the estimate of β from this model and refer to it as "market risk." We then use the volatility of the estimated residuals to capture idiosyncratic risk. Finally, since tracking error is an important metric of portfolio risk in the mutual fund industry, we also compute a tracking error measure. We define tracking error as the standard deviation of daily fund returns in excess of the return on the market portfolio. We compute market risk, idiosyncratic risk, and tracking error for each quarter of the pre- and post-event periods in which the manager is at the fund.

To test whether idiosyncratic shocks to manager wealth lead to changes in the market risk of the fund, we estimate the regression described in equation (1) using the difference in the fund's average market risk between the post-event period and the pre-event period as our dependent variable. Analogous to Table 3, we use a collapsed sample approach and calculate this variable as the difference in average market risk across the post- and pre-event periods.

Panel A of Table 4 reports the results. The estimate of γ_w is significantly negative in each column of the panel indicating that managers lower systematic risk in their portfolios in response to their real estate losses. In our baseline specification reported in column 2 the coefficient estimate is -0.047, which is statistically significant at better than the 1% level. Since average fund beta in the pre-event period is 1.10, this translates into a 4.3% decrease in the fund betas of wealth-shocked managers relative to the control group. Moreover, the behavior of the coefficient estimates across columns 1–8 is similar to that in Table 3. For example, the magnitude of the estimates increases as we increase the threshold for our treatment cutoff and decreases when we change the definition of our event-period.

Interestingly, the decline in total risk we document in Table 3 is not simply due to a decline in systematic risk. Following a wealth shock, managers also reduce the idiosyncratic risk of their funds. We tabulate these results in Panel B of the table.[12] The estimated wealth effect is negative in each specification and statistically significant in all but one case. Our baseline estimate in column 2 indicates that shocked managers reduce idiosyncratic risk by 0.02% more, on average, than their peers who are not shocked, which is about a 6.1% reduction in idiosyncratic risk. Given the estimates, about 40% of the 10.6% reduction in total risk can be attributed to reductions in systematic risk, while about 60% is due to relative reductions in idiosyncratic risk.

Finally, in Panel C we show strong evidence that managers lower the tracking error of their funds as a result of their personal housing shocks. This indicates that the wealth shock prompts managers to move their portfolios closer to those of passively managed funds.

[12] We follow the same collapsed sample methodology to calculate our dependent variable in this panel as well as in Panel C below.

3.3 Robustness

3.3.1 Parallel trends

Since our identification strategy relies on the assumption of parallel trends before the event, we now confirm that there are no differences in risk taking between treated and untreated managers in the pre-event window. To do so, we abandon the collapsed sample approach and measure the effect of the idiosyncratic wealth shock in each quarter during the pre- and post-event windows using the following dynamic model:

$$\sigma_{j,t} = \delta_{i,j} + \sum_{k=2}^{16} (\gamma_k Qtr_{k,t} + \gamma_{w,k} Qtr_{k,t} \times WealthShock_i) + \Gamma' Controls_{j,t-1} + \epsilon_{i,j,t}, \tag{2}$$

which includes separate dummy variables for each pre- and post-event quarter. In particular, $Qtr_{k,t}$ is an indicator variable that is one if quarter t is the k^{th} quarter of the 16 quarters that comprise the pre- and post-event periods. Interacting these quarter dummies with $WealthShock_i$ then allows us to estimate the effect of the idiosyncratic shock to manager wealth in each quarter. The $\gamma_{w,k}$ coefficients reflect these effects.

We report the results of this analysis in Figure 6, which plots the coefficient estimates from the dynamic model. The figure confirms that risk taking by treated and untreated managers exhibits parallel trends in the pre-event period, so our results in Table 3 are not driven by pre-event differences across these groups. Figure 6 also gives us an idea of how long the effects of wealth shocks persist on professional managers. The figure shows that the effects of the shock dissipate after six quarters. These findings are in contrast to those of Malmendier and Nagel (2011), who show that market return experience has long lasting effects on the personal portfolio decisions of individuals. These differences may arise due to differences in market sophistication and career concerns. As the market began to rebound in the second quarter of 2009, wealth-shocked managers began to increase risk-taking back toward normal levels.

3.3.2 Matched samples

One potential concern about our γ_w estimates is that managers may differ along a number of dimensions across the treated and untreated groups. Indeed, Panel B of Table 2 shows that treated managers (as defined the median cutoff) are significantly younger, less wealthy, and have less experience as portfolio managers. They are also part of larger management teams and manage funds that are smaller and have higher turnover rates.

We address this concern by using a matched sample approach. We apply a nearest neighbor propensity score matching algorithm without replacement to create alternative treated and untreated groups. Given the differences seen in Panel B of Table 2, we match by manager wealth, fund size, fund turnover, manager age, manager tenure, and the size of the management team during the pre-event period. We also require that matched pairs are not co-managers of the same fund. Since the matching algorithm does not use replacement, the treated and untreated groups would not be different from those in the previous tables if we used our baseline median treatment cutoff. Therefore, in this analysis treated managers are defined as those whose wealth change is in the lowest tercile of our sample, and untreated managers are drawn from the top two terciles using propensity scores.

Panel A of Table 5 presents the results of our matched sample tests. The first four columns summarize our results based on our first matching criterion. The coefficient estimates of the interaction term (γ_w) remain negative and significant for both total risk and for our three alternative risk components, though the magnitude of the estimates is somewhat smaller than that reported in column 3 of Tables 3 and 4. Panel B validates our matching approach by showing that the matched groups are no longer different in terms of observable fund or manager characteristics, with the exception of turnover.

3.3.3 Fund and manager characteristics

Another way of dealing with the observable differences between the treatment and control groups is to control for these differences in the regressions. In Table 6 we report the results from the OLS estimation of differences-in-differences regressions for changes in various measures of realized fund risk. The models follow the baseline model, estimated in column 2 of Table 3, but also include fund and manager characteristics. The fund characteristics considered are fund size, turnover, and performance. Manager characteristics include age, experience, and wealth. Each control variable is defined as a dummy variable relative to the median of that variable in the sample.

The results are consistent with our previous analyses, indicating that even after controlling for these characteristics wealth-shocked managers significantly lower risk in their delegated portfolios relative to their peers. Interestingly, none of these fund or manager characteristics are consistently related to changes in risk taking across the different risk measures. When comparing the estimates of γ_w in the first column (-0.057) with that in column 1 of Table 5 (-0.059), we see that both methods yield similar results. This is also true for our other measures of risk.

3.3.4 The role of wealth

Another important concern about our approach is that our measure of wealth may proxy for some unobserved managerial characteristic, such as skill. By construction, more experienced managers or those who manage larger funds will have greater estimated wealth, and both experience and fund size could indicate manager skill. Greater wealth also means that for a given home price change, these managers are less likely to fall into our treated group. If skilled managers are untreated and unskilled managers are treated, and if skilled managers respond to wealth shocks differently, then our results could be driven by manager sophistication and not the wealth shock itself.

Subsection 3.3.2 above helps to alleviate this concern by adopting a propensity score matching algorithm that requires treated and untreated managers to have similar levels of wealth (as well as similar sized funds and experience). Nevertheless, in this section we provide three additional tests to address this concern. First, in our baseline analyses we use a wealth shock dummy by applying different cutoffs to the distribution of the wealth change variable. We now rerun our model by replacing the wealth shock indicator with the logarithm of wealth change,[13] a continuous variable. Additionally, wealth change (i.e., total house price change divided by total wealth) is simply the product of percentage house price change (i.e., total house price change divided by the total value of homes) and percent of wealth allocated to housing (i.e., total value of homes divided by total wealth). Therefore, we can further decompose our continuous wealth change variable—into

[13]Since wealth change can take a negative value, we add one before taking the log.

a variable solely driven by the housing shock and another that is driven by wealth—to examine the effect of wealth on the aggregate coefficient estimate.

Table 7 reports the results. The odd-numbered columns in the table summarize the coefficient estimates of the continuous wealth change variable, while the even-numbered columns contain results for the decomposition. The table reveals that our results are robust to replacing our indicator variable with a continuous measure: in all cases our coefficient estimates are positive and highly significant, indicating that managers who suffer larger negative shocks subsequently reduce fund risk. Our decomposition results also confirm that wealth is not driving the aggregate coefficient. Managers who lose more of their housing wealth during the financial crisis exhibit lower risk-taking at their fund. With the exception of beta, these results are also statistically significant.

Our second approach to address the wealth concern is a bootstrap procedure. We rerun the regression in column 2 of Table 3, but create new, fictitious wealth shock measures. In particular, we first assign a new wealth change variable to our managers by randomly drawing house price changes but keeping each manager's own estimate of wealth as a denominator. This allows us to randomly treat the managers only with respect to their house price depreciation and leave wealth (and all other characteristics) unaltered. Once the wealth change values are assigned, we follow the baseline procedure and create the treatment group by using the sample median as the cutoff. We conduct 5,000 such simulations.

Figure 7 plots the distribution of the wealth shock coefficient based on the 5,000 simulations. The mean coefficient estimate from the simulations is -0.028, with a standard deviation of 0.017. This mean is a little less than zero, suggesting that wealth does play some role in the results, but the magnitude is nowhere near the size of our coefficient estimate of -0.084 (column 2 of Table 3). This point estimate is more than three standard deviations below the mean under the null, and in fact lies to the left of almost the entire distribution (only one draw is smaller). This provides strong evidence that our results are not driven by our wealth estimates.

Finally, while economic theory proposes that the correct scaler is wealth, to further alleviate the concern that our results are affected by our estimate of wealth, in Table IA.II of the Internet Appendix we show that the main results continue to hold when we use current income instead of wealth to scale the change in housing wealth to create our wealth shock variable. Income is much easier to estimate than wealth and it does not require assumptions that would affect cross-sectional variation. In the second row of the table we provide an additional robustness test by defining wealth shocks by the average percent change in the price of all homes owned by the manager.

3.3.5 Local clientele

In the previous sections we consider wealth shocks to be idiosyncratic to the manager, which is an important assumption for the validity of our tests. It is possible however that the mutual fund industry is geographically segmented and that many fund investors are local to the fund. Therefore, manager shocks could coincide with client wealth shocks and funds may lower risk not because of the former but merely to cater to their clients' changing economic circumstances.

This concern is mitigated by the fact that managers do not always live close to fund headquarters in our sample and frequently own multiple homes in different locations. Additionally, fund complexes—especially those that are large—often have a geographically diverse shareholders. Nonetheless, the catering story suggests that there may be an omitted factor that could drive our results and we provide several tests in this section to address it. To do so, we create a number of alternative shock measures to capture client wealth shocks. We then individually add these alternative shocks to the baseline model estimated in column 2 of Table 3.

Our goal is to capture shocks that are local to fund company headquarters. We therefore define alternative home price shock variables at the level of the zip code, MSA, and state, where the fund complex is headquartered. As before, the shock is defined at the sample median. The results are reported in Table 8. The table shows that adding these local housing shock controls to our model does not affect our estimates of γ_w, which remain negative and significant in all specifications. In contrast, the coefficients on the alternative shocks are never significantly different from zero. This indicates that wealth-shocked managers are not lowering the risk of their funds to cater to their clients.

In addition to the local housing-related measures, we also define shocks based on fund flows, changes in state-level income, and state-level stock returns over the event period. We use fund flows as a proxy for clients' financial wealth and, as is standard in the literature, we calculate these flows using information on monthly fund TNA and return. However, controlling for flows could also be important because investor flows are a primary driver of funds' cash holdings. For instance, funds facing large redemptions need to allocate a larger portion of their portfolios to cash or near-cash assets to mitigate the cost of meeting these withdrawals. This could, in turn, mechanically lower fund risk.

Another proxy for client wealth is state-level income, which is based on the state of the fund company's headquarters. We obtain data to calculate this measure from the Bureau of Economic Analysis. Similarly, we define state-level stock returns as the returns of value-weighted portfolios of stocks that are headquartered in the state of the fund company (i.e., "local" stocks). We include state-level stock returns because local clients may overinvest in local stocks (Coval and Moskowitz, 1999), so a shock to local stock returns could affect client wealth.

Finally, in addition to using the three new shock measures, we also run three specifications in which we include region, division, and state fixed effects. All three fixed effects are defined based on the headquarters of the fund company. Region and division fixed effects denote Census Bureau regions and Census Bureau divisions, respectively, and are shown in Figure 5.

The results are reported in Table 8. The table shows that adding these additional controls to our model does not affect our estimates of γ_w, which remain negative and significant in all specifications. Consistent with the local housing-based measures, the coefficients on these additional alternative shocks do not play a significant role in explaining fund risk with the exception of fund flow on tracking error.

3.3.6 Additional specifications

In Table 9 we report results from additional specifications for our main results. In columns 1-3 we estimate our baseline regression using subsamples of managers whose primary residence is in the New York City area, the Boston area, or all other areas, respectively. In column 4 we estimate the regression using the entire sample, but adding city fixed effects. In column 5 and 6 we add fund family and strategy fixed effects, respectively. Strategy fixed effects are based on Morningstar categories in the 3-by-3 size/value grid. These specifications, which address a potential ommitted variable at the local, family, or strategy levels, yield similar results to those reported earlier.

In column 7, we control for the manager's housing leverage. We do so by hand collecting mortgage information from LexisNexis Public Records Database. For each manager we record information on the most recent primary mortgage since the home purchase, but prior to the end of 2006. When available, we use interest rate and term information to calculate the mortgage balance as of the end of 2006 under the assumption of no principal prepayment.[14] We then scale the mortgage balance by the home purchase price to arrive at the leverage ratio. The results in column 7 reveal that controlling for home leverage only enhances the magnitude and precision of the effect of the wealth shock.

We summarize additional robustness analyses in the Internet Appendix. Table IA.III reports results from our baseline regression, estimated using standard errors that are clustered at either the city, management company zip code, or manager primary home zip code level. Our results remain unchanged. Finally, Table IA.IV reports results from regressions where we replace the dependent variables with the ratio of the risk measures, rather than the difference. These results are consistent with our main results, with stronger statistical significance.

3.4 How do wealth-shocked managers lower risk?

Our results thus far provide strong evidence that idiosyncratic shocks to managers' personal wealth affect fund investors as these shocks prompt managers to change the fund's risk profile. We now ask how treated managers lower risk. There are several mechanisms through which managers can lower fund risk. Managers can increase the number of stocks they hold in their delegated portfolios or increase the share of the portfolio invested in cash or near-cash securities. Funds with high portfolio or industry concentration can also decrease their allocations in the most heavily weighted securities or diversify across a larger set of industries, while those that overweight local stocks can reduce the portfolio weights of their local holdings, since geographically proximate stocks tend to comove (Pirinsky and Wang, 2006). Additionally, funds may also move their holdings closer to those in their passive benchmark portfolios, thus reducing their active share (Cremers and Petajisto, 2009).

[14]When interest rate information is not available we use the prevailing mortgage rates as of the contract date for mortgages with the same ammortization period. If term information is unavailable, then we assume the contract has a 30 year term. For managers with mortgages without amounts we drop the observations, since we cannot compute the mortgage balance. For managers for which no mortgage is found, we set their mortgage balance to zero.

To examine the role of these alternative mechanisms, we compute the number of stocks held, portfolio concentration, industry concentration, and the weight in local stocks for each fund. We use fund holdings from Thomson and quarterly cash holdings from Morningstar. Our portfolio concentration measure is a Herfindahl index of the fund's holdings, and industry concentration follows that proposed by Kacperczyk, Sialm, and Zheng (2005). As before, we define local stocks as those that are headquartered in the same state as the fund's management company. Finally, we obtain data on active share from Antti Petajisto's website.

Columns 1–6 of Table 10 report the coefficient estimates from the model described in equation (1) using the alternative mechanisms managers may follow to change fund risk as dependent variables. As before, we create these dependent variables using the collapsed sample approach. The mechanisms are not mutually exclusive; indeed managers may pull several of these levers simultaneously to alter fund risk. Columns 1–6 of the table show that three of the commonly used mechanisms involve diversification: wealth-shocked managers significantly increase the number of stocks in their portfolios and reduce portfolio and industry concentration relative to untreated managers in the sample. Additionally, treated managers also lower the active share of their funds. These results are not surprising. Managers may find it hard to justify an increase in cash holdings, and by holding cash they also limit returns. There is also a constraint with respect to local stocks, because portfolios must be overweighted in local stocks to make reducing local weights an option.

The portfolio and industry concentration measures capture the idea that managers can change fund risk by altering the mix of stocks they hold. We investigate this idea in columns 7–10 in the table by examining the risk profile of the fund's holdings. That is, we investigate changes in risk due to active changes in portfolio weights. In particular, we calculate the total risk, market risk, idiosyncratic risk, and tracking error of the fund's holdings in each quarter. To do so, we first compute daily quarter t holdings returns for each fund j as follows:

$$R^t_{j,d} = \sum_{s \in \mathcal{N}} w_{s,j,t} R_{s,d}, \tag{3}$$

where $w_{s,j,t}$ is the weight fund j places on security s at the beginning of quarter t and $R_{s,d}$ is the return on security s on day d in the last quarter of 2006. That is, our quarter t holdings returns are computed using quarter t weights; however, stock returns are held constant at their daily values in the last quarter of 2006. (We use these Q4/2006 returns to ensure that subsequent changes in stock return characteristics do not drive changes in the results.)

Next, we calculate holdings-based risk measures for each quarter in the pre- and post-event periods in which the manager is at the fund using the daily holdings returns we generate from equation (3). Accordingly, total holdings risk in quarter t is the standard deviation of the daily holdings returns. Following Section 3.2.1, we compute the systematic and idiosyncratic risk components by estimating a market model in each quarter. We use the estimate of β from this model as our holdings-based systematic risk measure for the quarter and refer to it as "holdings-based market risk." We then compute the volatility of the estimated daily residuals to capture holdings-based idiosyncratic risk. Finally, we also compute a holdings-based tracking error measure. We define holdings-based tracking error as the standard deviation of the daily excess returns of the fund's holdings. Daily excess returns are calculated as in equation (3) with the exception that daily stock returns ($R_{s,d}$) are replaced by the daily return of the stock in excess of the market portfolio. We then convert these quarterly holdings-based risk measures into dependent variables for the model described in equation (1) using the collapsed sample approach.

For instance, in column 7 in the Table, our dependent variable is the difference between the average holdings-based total volatility of the fund in the post-period and the pre-event period. Note that since we use the same return series to calculate the holdings-based daily returns for each quarter, we filter out changes in risk due to changes in stock characteristics or market conditions. Accordingly, differences in our average holdings-based risk measures across the two periods are only driven by changes in portfolio weights.

Columns 7–10 of Table 10 show that managers who receive an idiosyncratic wealth shock significantly change the composition of their portfolios compared to those peer managers who do not experience a shock. The coefficient estimates on three of the four holdings-based measures are negative and statistically significant. While these findings could come from an increase in the number of stocks held by the fund or a decrease in portfolio or industry concentration (and thus are consistent with columns 1, 3, and 4 of the table), they are also consistent with a broad range of decisions to shift toward less risky assets in general. Specifically, following severe real estate loses, managers can increase their delegated portfolio weights in stocks that have less volatility, lower idiosyncratic risk, lower systematic risk, and/or lower tracking error.

3.5 Why do personal wealth shocks lead to lower fund risk?

While the focus of the paper is to test the lack of idiosyncratic style hypothesis, it is interesting to try to understand why fund managers lower risk following wealth shocks. In the introduction we discuss a number of reasons why risk taking in delegated portfolios may be affected by changes in fund manager risk attitudes. We now investigate these hypotheses further.

Recall that if agency conflicts exist then a wealth shock to a manager can lead them to change their risk taking in their professional portfolio, either because they treat their professional portfolio as an extension of their personal portfolio, or because of career concerns due to changing riskiness of their human capital. And even without agency conflicts, a wealth shock may alter the manager's beliefs about future investment returns, and therefore lead the manager to reduce risk taking in both the personal and professional portfolio.

While these hypotheses are not mutually exclusive and are difficult to test, we present results in Table 11 to try to understand the relative importance of some of these channels. If career concerns are driving the results, then managers who have the greatest career concerns should be most responsive to shocks in wealth. Younger managers and those with less experience should be more worried about their employment opportunities compared to managers who are older and more established. Accordingly, we split our sample of managers by the median age and median experience and estimate our baseline regressions within these samples. The table shows that younger managers reduce total risk more than do older managers and that inexperienced managers reduce total risk more than do experienced managers. However, the estimates of γ_w are significantly negatively estimated in all specifications and the differences between the corresponding subsamples are not significant. Thus the results provide only weak evidence for our career concerns story.

If managers are dialing back risk in their professional portfolios because they treat their professional portfolios as personal portfolios, then we should see that the effect is strongest among managers who invest a substantial portion of their wealth in their funds. In columns 5 and 6 of the table we estimate our baseline differences-in-differences regressions for samples split based on the percentage of wealth the manager invests in his fund. The table shows that managers with little wealth invested in their funds dial back risk following wealth shocks, while those with substantial wealth do not. This finding goes against the prediction of the personal portfolio hypothesis but may support the idea that ownership is a governance mechanism. Observing that only managers with weak incentives reduce fund risk in response to wealth shocks, is suggestive that this behavior is suboptimal, which could be driven by a behavioral bias.

While we do not want to draw too strong of conclusions from this analysis, we believe that we can rule out the personal portfolio hypothesis as a potential channel and that the career concerns hypothesis shows some merit.

4 Conclusion

Empirical finance research has been subject to changing constraints on data availability and computing power over time. In early studies, the unit of observation was a country. As data became more available, empiricists began conducting research at the firm level. Ultimately, economic decisions are made by individuals and so in the last decade an explosion in the availability of personal information has allowed researchers to focus on managers. There is a debate as to whether the traits, styles, and biases of managers impact firm- or fund-level decisions. A challenge to this research is that managers may be "selected" to implement the preferences of the board or the fund and not that managers imprint their preferences on their funds or corporations (Fee et al., 2013).

To address this challenge, we investigate the impact of wealth shocks on managers preferences and beliefs. This allows us to conduct tests within managers, which differences out selected styles. Specifically, we investigate how shocks to fund manager personal wealth translate into risk taking in mutual fund portfolios.

We provide robust evidence that managers who experience exogenous shocks to wealth from falling home prices subsequently lower the risk of their funds relative to managers who do not experience shocks. This finding supports the view that shocks to wealth decrease managers' appetite for risk, who lower the risk of their delegated mutual funds in response. Not only does this finding allow us to reject the *lack of idiosyncratic style* hypothesis for managers in the mutual fund industry, but it also suggests that idiosyncratic events in managers' personal lives can have a substantial impact on their professional decisions. We find that a 20% decrease in personal wealth leads to about an 11% decrease in risk taking in delegated portfolios. While shocks to real estate assets are one type of wealth shock, there are many other life events that can shock personal wealth, such as divorce or a personal lawsuit. Our research suggests that these personal life events would have a similar affect on delegated risk taking.

Finally, our ability to speak directly to the importance of idiosyncratic shocks more generally is perhaps limited by differences between the mutual fund industry and other corporations in terms of the roles of managers, organizational structures, and governance mechanisms. Nevertheless, we provide a path for future research in corporate finance by presenting a methodology for identifying the presence of idiosyncratic manager style effects that is not subject to the "selected" style critique.

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Table 1:
Validating wealth estimates

The table reports the coefficient estimates and standard errors from OLS estimations of cross sectional regressions explaining the natural logarithm of manager total housing wealth at the end of 2006. The procedure for estimating fund manager wealth, our key independent variable, is outlined in the Internet Appendix. This wealth estimate is used in the denominator of the treatment variable of interest in the paper; percentage change in wealth due to housing. The other independent variables are defined in the Internet Appendix. Independent variables are standardized to have means of zero and standard deviations of one. The regression in column 3 includes management company zip code fixed effects and regressions in columns 4 through 6 include manager home zip code fixed effects. Manager home zip codes are the zip code of the manager's home that is closest to the zip code of the management company for which he works. Standard errors, clustered by fund manager, are reported in parentheses. Significance levels are denoted by *a*, *b*, *c*, which correspond to 1%, 5%, and 10% levels, respectively.

Dependent variable:	Log (total housing wealth)				
	(1)	(2)	(3)	(4)	(5)
Log manager wealth	0.365^a	0.229^a	0.171^b	0.236^a	0.308^a
	(0.049)	(0.064)	(0.066)	(0.077)	(0.104)
Log fund size		0.023	-0.052	-0.004	0.007
		(0.033)	(0.039)	(0.017)	(0.017)
Expense ratio		0.015	0.022	0.026	0.022
		(0.036)	(0.042)	(0.022)	(0.023)
Number of managers		0.057	0.119^b	-0.024	-0.026
		(0.040)	(0.053)	(0.032)	(0.031)
Manager age		0.049	0.021	0.041	0.025
		(0.054)	(0.061)	(0.084)	(0.084)
Manager experience		0.172^a	0.161^a	0.034	0.027
		(0.055)	(0.056)	(0.083)	(0.083)
Log manager income					-0.098
					(0.091)
Management company zip code FE	N	N	Y	N	N
Manager home zip code FE	N	N	N	Y	Y
Observations	1,151	1,150	1,124	1,139	1,139
Adj-R-squared	0.13	0.15	0.33	0.83	0.83

Table 2:
Differences in differences sample statistics

Panel A of the table reports summary statistics for sample of active, U.S. equity funds included in the differences-in-differences analysis. The sample includes 1,151 manager-fund observations, which are constructed by differencing the averages of quarterly, manager-fund observations over the pre-event period (2005 and 2006) and the post-event period (2009 and 2010). In total there are 15,933 quarterly, manager-fund observations used in the construction of the cross sectional differences. We exclude funds from the sample if: 1) their Morningstar category is outside the 3-by-3 size/value grid (US Large Blend, US Large Growth, US Large Value, US Mid-Cap Blend, US Mid-Cap Growth, US Mid-Cap Value, US Small Blend, US Small Growth, or US Small Value), 2) they have fewer than $20 million in assets under management at the end of 2004, 3) they have more than ten managers, or 4) if they do not exist for at least one quarter during the pre-event period and post-event period. The event period is from the end of 2006 through the end of 2008 and is the time period over which the change in manager housing wealth is measured. Statistics on the pre-event averages and changes from the pre-event to the post-event period are reported for most variables. Panel B shows the average pre-event characteristics of funds in the treatment and control groups, as well as a test of their differences. Managers are in the treatment group if the percentage change in wealth due to housing during the event period is less than the median in the sample. Remaining managers are in the control group. Significance levels are denoted by a, b, c, which correspond to 1%, 5%, and 10% levels, respectively.

Panel A: Summary statistics

				Percentile				
Variable	Mean	Median	St. Dev.	1	25	75	99	N
Manager characteristics								
Total housing wealth ($1,000s)	2,406.53	1,372.62	3,176.25	149.49	666.51	2,780.46	21,100.00	1,151
Change in housing wealth during event period ($1,000s)	-174.14	-83.62	342.35	-1,756.12	-237.19	-18.62	678.24	1,151
Manager wealth ($1,000s)	7,888.89	2,283.52	16,800.00	33.96	873.70	6,407.69	119,000.00	1,151
Pct. change in wealth due to housing during event period	-9.66	-3.48	20.27	-119.71	-10.78	-0.43	18.32	1,151
Pre-event manager age	44.97	43.50	10.18	28.60	37.50	50.00	74.50	1,151
Pre-event manager experience	7.65	6.29	5.97	0.42	3.05	10.88	24.17	1,151
% wealth in fund	8.72	1.34	16.22	0.00	0.00	9.34	84.99	733
Fund characteristics								
Pre-event log fund age	2.23	2.25	1.01	0.00	1.70	2.74	4.55	1,151
Pre-event performance percentile	0.57	0.58	0.24	0.06	0.42	0.73	1.38	1,149
Change in performance percentile	-0.04	-0.05	0.29	-0.69	-0.23	0.15	0.66	1,088
Pre-event log fund size	5.86	5.79	2.05	1.18	4.50	7.01	11.21	1,151
Change in log fund size	0.12	-0.07	1.07	-1.97	-0.51	0.60	3.55	1,101
Pre-event turnover	0.96	0.57	1.60	0.01	0.28	1.00	8.25	1,149
Change in turnover	-0.02	0.07	0.99	-3.94	-0.14	0.25	1.51	1,094
Pre-event monthly expense ratio	0.11	0.10	0.04	0.01	0.08	0.13	0.25	1,150
Change in monthly expense ratio	-0.01	0.00	0.01	-0.05	-0.01	0.00	0.03	1,095
Pre-event number of managers	3.41	3.00	1.98	1.00	2.00	4.13	10.00	1,151

Table 2 continues on the following page.

Table 2 continued from the previous page.

				Percentile				
Variable	Mean	Median	St. Dev.	1	25	75	99	N
risk taking measures and mechanisms								
Pre-event total risk	0.79	0.74	0.21	0.45	0.64	0.90	1.64	1,151
Change in total risk	0.83	0.73	0.37	0.25	0.62	0.90	2.28	1,151
Pre-event market risk	1.10	1.04	0.26	0.60	0.92	1.25	2.25	1,151
Change in market risk	-0.04	-0.03	0.18	-0.51	-0.16	0.09	0.41	1,151
Pre-event idiosyncratic risk	0.28	0.25	0.12	0.06	0.19	0.35	0.73	1,151
Change in idiosyncratic risk	0.10	0.07	0.11	-0.10	0.03	0.14	0.48	1,151
Pre-event tracking error	0.33	0.28	0.17	0.08	0.20	0.42	1.04	1,151
Change in tracking error	0.12	0.09	0.15	-0.14	0.03	0.17	0.72	1,151
Pre-event number of stocks	139.53	84.88	180.03	15.00	53.50	139.50	775.00	1,118
Change in number of stocks	-0.08	-0.75	40.95	-145.17	-9.63	5.50	235.25	1,070
Pre-event percent of portfolio in cash	3.55	2.47	3.74	0.00	1.16	4.66	20.04	1,081
Change in percent of portfolio in cash	-0.47	-0.13	2.85	-10.67	-1.59	0.92	9.17	972
Pre-event portfolio holdings concentration	2.00	1.70	1.47	0.18	1.16	2.41	9.03	1,130
Change in portfolio holdings concentration	0.08	0.04	0.54	-1.67	-0.12	0.26	2.60	1,089
Pre-event portfolio industry concentration	5.15	3.84	4.97	0.19	1.97	6.41	27.98	1,131
Change in portfolio industry concentration	0.25	0.20	2.75	-9.26	-1.03	1.45	10.08	1,090
Pre-event weight in local stocks	7.07	3.93	8.97	0.00	1.03	9.25	40.08	1,078
Change in weight in local stocks	0.01	0.01	0.06	-0.18	-0.03	0.04	0.20	1,094
Pre-event holdings-based total risk	0.66	0.63	0.21	0.41	0.51	0.79	1.58	1,128
Change in holdings-based total risk	0.02	0.01	0.10	-0.30	-0.04	0.07	0.30	1,094
Pre-event holdings-based market risk	1.19	1.16	0.35	0.72	0.94	1.41	2.82	1,128
Change in holdings-based market risk	0.00	0.00	0.06	-0.17	-0.03	0.03	0.22	1,094
Pre-event holdings-based idiosyncratic risk	0.27	0.25	0.13	0.06	0.17	0.34	0.66	1,128
Change in holdings-based idiosyncratic risk	0.00	0.00	0.06	-0.18	-0.03	0.03	0.23	1,094
Pre-event holdings-based tracking error	0.30	0.27	0.15	0.07	0.18	0.41	0.80	1,128
Change in holdings-based tracking error	0.40	0.07	4.59	-14.50	-1.28	2.06	19.54	837

Table 2 continues on the following page.

Table 2 continued from the previous page.

Panel B: Pre-event characteristics of treatment and control groups

	Control	Treatment	Diff
Pct. change in wealth due to housing during event period	0.30	-19.80	-20.10^{a}
Pre-event manager wealth ($1,000s)	13,119.00	2,575.00	-10,544.00^{a}
Pre-event manager age	46.74	43.17	-3.57^{a}
Pre-event manager experience	9.33	5.94	-3.39^{a}
Pre-event log fund age	2.25	2.21	-0.04
Pre-event performance percentile	0.57	0.57	-0.01
Pre-event log fund size	6.12	5.59	-0.54^{a}
Pre-event turnover	0.81	1.11	0.30^{a}
Pre-event monthly expense ratio	0.10	0.11	0.01^{b}
Pre-event number of managers	3.30	3.53	0.23^{b}

Table 3:
Personal wealth shocks and risk taking

The table reports the coefficient estimates and standard errors from the OLS estimation of the difference in difference regression,

$$\Delta\bar{\sigma}_{i,j} = \gamma_w WealthShock_i + \Gamma' \Delta\overline{Controls}_{i,j} + \epsilon_{i,j},$$

using the sample of active U.S. domestic equity mutual funds outlined in Table 2, where $\Delta\bar{\sigma}_{i,j} = \bar{\sigma}^{post}_{i,j} - \bar{\sigma}^{pre}_{i,j}$ is the difference in the average realized risk between the post-event period and the pre-event period for fund j over those quarters in which manager i is a manager of the fund. $WealthShock_i$ is a dummy variable that is one if manager i is treated with the wealth shock, and $\Delta\overline{Controls}_{i,j}$ is a vector of changes in pre- and post-event average manager- and fund-specific control variables. Quarterly total fund risk is measured as the standard deviation of daily fund returns during the quarter. Control variables are defined in the Appendix. In all columns except 3 and 4, treated managers are those whose percentage change in wealth due to housing during the event period is less than the median in the sample. Treated managers in column 3 (4) are those whose percentage change in wealth due to housing during the event period is in the bottom third (fourth) in the sample. Control groups in these regressions include managers whose percentage change in housing wealth was in the top third (fourth) in the sample. The event period (period over which the percentage change in wealth due to housing is measured) is from the end of 2006 until the end of 2008 in all regressions except those in column 5 and 6, which use the alternative event period end dates indicated. The regressions in columns 7 and 8 include only the indicated number of quarters before and after the event period in the construction of the pre- and post-event averages. All other regressions include 8 quarters on either side of the event period. Standard errors, clustered by fund, are in parentheses. Significance levels are denoted by a, b, c, which correspond to 1%, 5%, and 10% levels, respectively.

			Alt. shock percentile		Alt. event end period		Alt. pre/post length (qtrs)	
			33^{rd}	25^{th}	Jun. '08	Sep. '08	4	2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wealth shock	-0.062^a	-0.084^a	-0.109^a	-0.158^a	-0.070^a	-0.069^a	-0.069^a	-0.071^a
	(0.023)	(0.022)	(0.027)	(0.041)	(0.021)	(0.021)	(0.021)	(0.021)
Change in past performance percentile		-0.009	-0.023	-0.032	-0.010	-0.008	-0.077^c	-0.253^a
		(0.050)	(0.062)	(0.080)	(0.050)	(0.050)	(0.040)	(0.038)
Change in log fund size		-0.007	0.005	0.032	-0.007	-0.006	-0.018	-0.053^a
		(0.011)	(0.014)	(0.020)	(0.011)	(0.011)	(0.012)	(0.015)
Change in expense ratio		1.790^b	2.177^b	3.497^b	1.667^c	1.671^c	3.013^a	1.860^c
		(0.868)	(1.089)	(1.448)	(0.878)	(0.875)	(0.902)	(1.042)
Change in turnover		0.010	0.011	0.111^b	0.010	0.010	-0.015	-0.019
		(0.014)	(0.014)	(0.049)	(0.014)	(0.014)	(0.014)	(0.013)
Constant	0.857^a	0.846^a	0.866^a	0.883^a	0.839^a	0.839^a	1.048^a	1.583^a
	(0.018)	(0.019)	(0.024)	(0.033)	(0.019)	(0.019)	(0.019)	(0.019)
Observations	1,151	1,084	720	354	1,079	1,078	1,040	1,009
Adj-R-squared	0.01	0.02	0.02	0.07	0.01	0.01	0.03	0.11

Table 4:
Personal wealth shocks and risk taking - Risk components

The table reports the coefficient estimates and standard errors from the OLS estimation of the difference in difference regressions outlined in Table 3. Regressions specifications reported in the columns of Panels A, B, and C are the same as those in Table 3, but are estimated for alternative measures of fund risk. The dependent variables in these regressions are fund realized market risk, idiosyncratic risk, and tracking error in Panels A, B, and C, respectively. Market risk is measured as the fund's beta from the CAPM, idiosyncratic risk is the standard deviation of residuals from the CAPM, and tracking error is measured relative to the market return. Each measure is estimated using daily fund returns during the quarter of the observation, averaged over the pre- and post-event periods, and then differenced. Control variables that are included in column 2 of Table 3 are included in the regressions where indicated, but their coefficient estimates and standard errors are not reported. Standard errors, clustered by fund, are in parentheses. Significance levels are denoted by a, b, c, which correspond to 1%, 5%, and 10% levels, respectively.

			Alt. shock percentile		Alt. event end period		Alt. pre/post length (qtrs)	
			33^{rd}	25^{th}	Jun. '08	Sep. '08	4	2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: ΔMarket risk								
Wealth shock	-0.049^a	-0.047^a	-0.062^a	-0.077^a	-0.033^a	-0.043^a	-0.052^a	-0.067^a
	(0.011)	(0.011)	(0.014)	(0.021)	(0.011)	(0.011)	(0.015)	(0.018)
Adj-R-squared	0.02	0.04	0.06	0.10	0.03	0.03	0.02	0.02
Panel B: ΔIdiosyncratic risk								
Wealth shock	-0.011^c	-0.017^a	-0.024^a	-0.034^a	-0.017^a	-0.015^b	-0.016^b	-0.009
	(0.007)	(0.006)	(0.008)	(0.012)	(0.006)	(0.006)	(0.007)	(0.008)
Adj-R-squared	0.00	0.04	0.04	0.05	0.03	0.03	0.04	0.07

Table 4 continues on the following page.

Table 4 continued from the previous page.

			Alt. shock percentile		Alt. event end period		Alt. pre/post length (qtrs)	
			33^{rd}	25^{th}	Jun. '08	Sep. '08	4	2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel C: ΔTracking error								
Wealth shock	-0.022[b]	-0.030[a]	-0.037[a]	-0.062[a]	-0.026[a]	-0.026[a]	-0.034[a]	-0.038[a]
	(0.009)	(0.009)	(0.011)	(0.017)	(0.009)	(0.009)	(0.010)	(0.012)
Adj-R-squared	0.00	0.03	0.03	0.05	0.03	0.03	0.04	0.05
Additional controls	N	Y	Y	Y	Y	Y	Y	Y
Observations	1,151	1,084	720	354	1,079	1,078	1,040	1,009

Table 5:
Personal wealth shocks and risk taking - Characteristic-matched sample

Panel A of the table reports the coefficient estimates and standard errors from the OLS estimation of the difference in difference regressions outlined in Table 3 for changes in various measures of realized fund risk using a matched sample of funds from the sample outlined in Table 2. The matched sample is created using a nearest neighbor without replacement propensity score matching algorithm based on fund size, fund turnover, manager wealth, manager age, manager experience, and number of fund managers during the pre-event period. Matched pairs are prohibited from being from the same fund. Treated managers in the analysis are defined as those whose percentage change in wealth due to housing during the event period is in the bottom tercile. Untreated managers are chosen from the top two terciles based on propensity score. Control variables that are included in column 2 of Table 3 are included in all regressions, but their coefficient estimates and standard errors are not reported. Panel B shows the average pre-event characteristics of funds in the treatment and control groups, as well as a test of their differences. Significance levels are denoted by a, b, c, which correspond to 1%, 5%, and 10% levels, respectively.

Panel A: characteristic-matched sample: regressions

Dependent variable:	ΔTotal risk	ΔMarket risk	ΔIdio. risk	ΔTrack. error
	(1)	(2)	(3)	(4)
Wealth shock	-0.062^a	-0.021	-0.014^c	-0.022^b
	(0.023)	(0.013)	(0.007)	(0.010)
Additional controls	Y	Y	Y	Y
Observations	699	699	699	699
Adj-R-squared	0.01	0.06	0.03	0.03

Panel B: characteristic-matched sample: characteristics of treatment and control groups

	Control	Treatment	Diff
Pct. change in wealth due to housing during event period	-0.71	-27.29	-26.58^a
Pre-event manager wealth ($1,000s)	2,270.00	1,950.00	-320.00
Pre-event manager age	42.84	42.21	-0.63
Pre-event manager experience	5.69	5.48	-0.21
Pre-event log fund age	2.15	2.10	-0.05
Pre-event performance percentile	0.55	0.55	0.00
Pre-event log fund size	5.36	5.33	-0.03
Pre-event turnover	0.91	1.15	0.24^b
Pre-event monthly expense ratio	0.11	0.11	0.00
Pre-event number of managers	3.51	3.54	0.03

Table 6:
Personal wealth shocks and risk taking - fund and manager characteristics

The table reports the coefficient estimates and standard errors from the OLS estimation of the difference in difference regressions outlined in Table 3 for changes in various measures of realized fund risk. Models estimated follow the baseline model, estimated in column 2 of Table 3, but also include fund and manager characteristics. A fund is considered a small fund if the total net assets of the fund (TNA) are less than the median in the sample during the pre-event period. A fund is a low turnover fund if fund turnover is less than the median in the sample during the pre-event period. A fund is a low performing fund if the fund's performance during the pre-event period is less than the median in the sample. A manager is a young manager if the manager's age at the end of 2006 is less than the median in the sample. A manager is an inexperienced manager if the portfolio management experience of the manager at the end of 2006 is less than the median in the sample. A manager is a wealthy manager if the end of 2006 wealth of the manager is greater than the median in the sample. Reported are the coefficient estimates and standard errors on the wealth shock and the fund and manager characteristics. Control variables that are included in column 2 of Table 3 are included in all regressions, but their coefficient estimates and standard errors are not reported. Standard errors, clustered by fund, are in parentheses. Significance levels are denoted by *a*, *b*, *c*, which correspond to 1%, 5%, and 10% levels, respectively.

Dependent variable:	ΔTotal risk	ΔMarket risk	ΔIdio. risk	ΔTrack. error
	(1)	(2)	(3)	(4)
Wealth shock	-0.057^a	-0.025^b	-0.017^a	-0.027^a
	(0.022)	(0.011)	(0.007)	(0.009)
Small fund	-0.056^c	0.007	-0.006	0.004
	(0.030)	(0.014)	(0.009)	(0.013)
Low turnover fund	-0.017	0.096^a	-0.012	-0.011
	(0.027)	(0.014)	(0.008)	(0.012)
Low performing fund	0.001	-0.006	0.025^a	0.026^b
	(0.025)	(0.013)	(0.008)	(0.011)
Young manager	0.035	-0.002	0.015^c	0.016
	(0.025)	(0.012)	(0.007)	(0.011)
Inexperienced manager	-0.046^c	-0.005	-0.011	-0.009
	(0.027)	(0.012)	(0.008)	(0.011)
Wealthy manager	-0.025	0.041^a	-0.021^b	-0.013
	(0.030)	(0.014)	(0.009)	(0.013)
Constant	0.908^a	-0.092^a	0.112^a	0.131^a
	(0.045)	(0.019)	(0.012)	(0.018)
Observations	1,148	1,148	1,148	1,148
Adj-R-squared	0.01	0.10	0.03	0.01

Table 7:

Personal wealth shocks and risk taking - decomposing wealth change

The table reports the coefficient estimates and standard errors from the OLS estimation of the difference in difference regressions outlined in Table 3 but replaces *Wealth Shock* in columns 1, 3, 5, and 7 with the log of one plus wealth change (i.e., total house price change divided by total wealth). In columns 2, 4, 6, and 8 we decompose the continuous wealth change variable into total house price change divided by the total value of homes (i.e., percent house price change) and the total value of homes divided by total wealth (percent of wealth allocated to housing), and replace the *Wealth Shock* dummy with the log of one plus these measures. Control variables that are included in column 2 of Table 3 are included in all regressions, but their coefficient estimates and standard errors are not reported. Standard errors, clustered by fund, are in parentheses. Significance levels are denoted by a, b, c, which correspond to 1%, 5%, and 10% levels, respectively.

Dependent variable:	ΔTotal risk		ΔMarket risk		ΔIdio. risk		ΔTrack. error	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log wealth change	0.106^a		0.063^b		0.021^a		0.037^b	
	(0.035)		(0.028)		(0.008)		(0.017)	
Log % house price decline		0.272^b		0.023		0.090^b		0.118^b
		(0.131)		(0.059)		(0.036)		(0.050)
Log % wealth in housing		-0.051^b		-0.064^a		0.001		-0.012
		(0.020)		(0.010)		(0.006)		(0.009)
Additional Controls	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,064	1,084	1,064	1,084	1,064	1,084	1,064	1,084
Adj-R-squared	0.01	0.02	0.03	0.07	0.03	0.03	0.03	0.03

Table 8:
Personal wealth shocks and risk taking - alternative clientele shocks

The table reports the coefficient estimates and standard errors from the OLS estimation of the difference in difference regressions outlined in Table 3 for changes in various measures of realized fund risk. Models follow the baseline model, estimated in column 2 of Table 3, but also include alternative shock measures that proxy for the wealth shocks of the fund's clients. We use three alternative shocks based on changes in home prices during the event period around the mutual fund company's headquarters at the zip code level, MSA level, and state level. Data on home price changes are from Zillow. Funds with shocked clients are those whose local house price changes during the event period are less than the median in the sample. Our other alternative shocks are based on fund flows, changes in state-level income, and state-level stock returns over the event period. Clients experience the alternative shock under these measures if changes in the alternative variable over the event period are less than the median in the sample. In addition, where indicated, specifications include fixed effects based on Census Bureau regions, Census Bureau divisions, and U.S. states. Data on state-level income are from the Bureau of Economic Analysis. State-level stock returns are the returns of value-weighted portfolios of stocks constructed based on firm headquarters state. Reported are the coefficient estimates and standard errors on the wealth shock and the alternative shock. Control variables that are included in column 2 of Table 3 are included in all regressions, but their coefficient estimates and standard errors are not reported. Standard errors, clustered by fund, are in parentheses. Significance levels are denoted by *a*, *b*, *c*, which correspond to 1%, 5%, and 10% levels, respectively.

Dep. var.	Alternative shock	Wealth shock	Alternative shock
ΔTotal risk	Company zip home price	-0.079[a]	-0.024
		(0.021)	(0.024)
	Metro home price	-0.085[a]	0.028
		(0.022)	(0.025)
	State home price	-0.085[a]	0.028
		(0.022)	(0.025)
	Flow	-0.085[a]	-0.044
		(0.021)	(0.028)
	State income	-0.082[a]	0.019
		(0.022)	(0.025)
	State stock returns	-0.082[a]	0.003
		(0.022)	(0.023)
	Region FE	-0.081[a]	
		(0.022)	
	Division FE	-0.082[a]	
		(0.021)	
	State FE	-0.077[a]	
		(0.021)	

Table 8 continues on the following page.

Table 8 continued from the previous page.

Dep. var.	Alternative shock	Wealth shock	Alternative shock
ΔMarket risk	Company zip home price	-0.046^{a}	-0.020
		(0.012)	(0.014)
	Metro home price	-0.051^{a}	0.011
		(0.012)	(0.016)
	State home price	-0.051^{a}	0.011
		(0.012)	(0.016)
	Flow	-0.048^{a}	-0.025
		(0.011)	(0.018)
	State income	-0.049^{a}	-0.018
		(0.012)	(0.014)
	State stock returns	-0.049^{a}	0.002
		(0.012)	(0.014)
	Region FE	-0.049^{a}	
		(0.011)	
	Division FE	-0.050^{a}	
		(0.011)	
	State FE	-0.044^{a}	
		(0.011)	
ΔIdio. risk	Company zip home price	-0.018^{a}	0.005
		(0.006)	(0.008)
	Metro home price	-0.017^{a}	0.000
		(0.006)	(0.008)
	State home price	-0.017^{a}	0.000
		(0.006)	(0.008)
	Flow	-0.018^{a}	-0.010
		(0.006)	(0.009)
	State income	-0.017^{a}	0.001
		(0.006)	(0.008)
	State stock returns	-0.017^{a}	0.004
		(0.006)	(0.008)
	Region FE	-0.018^{a}	
		(0.006)	
	Division FE	-0.017^{a}	
		(0.006)	
	State FE	-0.015^{b}	
		(0.006)	

Table 8 continues on the following page.

Table 8 continued from the previous page.

Dep. var.	Alternative shock	Wealth shock	Alternative shock
ΔTrack. err.	Company zip home price	-0.030[a]	-0.009
		(0.009)	(0.011)
	Metro home price	-0.031[a]	0.008
		(0.009)	(0.013)
	State home price	-0.031[a]	0.008
		(0.009)	(0.013)
	Flow	-0.031[a]	-0.036[b]
		(0.009)	(0.015)
	State income	-0.030[a]	0.009
		(0.009)	(0.012)
	State stock returns	-0.031[a]	0.011
		(0.009)	(0.012)
	Region FE	-0.031[a]	
		(0.009)	
	Division FE	-0.031[a]	
		(0.009)	
	State FE	-0.030[a]	
		(0.009)	

Table 9:
Personal wealth shocks and risk taking - alternative specifications

The table reports the wealth shock coefficient estimates and standard errors from the OLS estimation of the difference in difference regressions outlined in Table 3 for various measures of changes in realized fund risk for alternative specifications. The first four columns use different samples based on the manager's primary residence. The sample includes all managers whose primary residence is in the New York City area, Boston area, and neither the New York nor Boston areas in columns 1 through 3, respectively. Column 4 includes all observations and also includes city cluster fixed effects. Cities are defined as in Figure 5. Models in columns 5 and 6 include fund family and strategy fixed effects, respectively. Strategies are defined as the relevant Morningstar nine box grid style. The model in column 7 controls for home leverage. Wealth shocks in this table are defined as below the median percentage change in wealth during the event period. Control variables that are included in column 2 of Table 3 are included in all regressions, but their coefficient estimates and standard errors are not reported. Standard errors, clustered by fund, are in parentheses. Significance levels are denoted by *a*, *b*, *c*, which correspond to 1%, 5%, and 10% levels, respectively.

	Wealth shock coefficients and standard errors						
Sample:	NYC (1)	BOS (2)	OTH (3)	City FE (4)	Family FE (5)	Strategy FE (6)	Lev. control (7)
Δ Total risk	-0.096^{c}	-0.108^{b}	-0.079^{a}	-0.076^{a}	-0.077^{a}	-0.071^{a}	-0.111^{a}
	(0.056)	(0.052)	(0.024)	(0.021)	(0.022)	(0.020)	(0.024)
Δ Market risk	-0.015	-0.105^{a}	-0.044^{a}	-0.049^{a}	-0.032^{a}	-0.030^{a}	-0.051^{a}
	(0.029)	(0.030)	(0.013)	(0.012)	(0.009)	(0.009)	(0.012)
Δ Idiosyncratic risk	-0.019	0.012	-0.020^{a}	-0.015^{b}	-0.007	-0.016^{a}	-0.021^{a}
	(0.016)	(0.014)	(0.007)	(0.006)	(0.006)	(0.006)	(0.007)
Δ Tracking error	-0.028	-0.015	-0.031^{a}	-0.028^{a}	-0.019^{b}	-0.025^{a}	-0.040^{a}
	(0.023)	(0.018)	(0.011)	(0.009)	(0.008)	(0.008)	(0.010)
Additional controls	Y	Y	Y	Y	Y	Y	Y
Observations	182	141	761	1041	1002	1084	878

Table 10:
How do wealth-shocked managers lower risk?

The table reports the coefficient estimates and standard errors from the OLS estimation of the difference in difference regressions outlined in Table 3 for changes in various mechanisms used to change fund risk. In column 1, the dependent variable is the change in the log of the number of stocks in the fund's portfolio. In column 2, the dependent variable is the change in the percentage of the portfolio invested in cash or cash equivalents. Data on cash holdings are from Morningstar. In column 3, the dependent variable is the change in the holdings concentration of the portfolio. In column 4, the dependent variable is the change in the industry concentration of the portfolio. In column 5, the dependent variable is the change in the portfolio weight on local stocks, where a stock is local if the firm and management company are headquartered in the same state. In column 6, the dependent variable is the change in activeshare, where activeshare is defined as in Cremers and Petajisto (2009). The dependent variables in columns 7 through 10 are changes in holdings-based measures of total risk, market risk, idiosyncratic risk, and tracking error. These measures calculate the risk of the portfolio for quarter t based on the holdings of the fund at the beginning of quarter t but using daily stock returns during the fourth quarter of 2006. Reported are the coefficient estimates and standard errors on our wealth shock variable. Control variables that are included in column 2 of Table 3 are included in all regressions, but their coefficient estimates and standard errors are not reported. Standard errors, clustered by fund, are in parentheses. Significance levels are denoted by a, b, c, which correspond to 1%, 5%, and 10% levels, respectively.

							Holdings-based			
Dependent variable:	ΔLog # stocks	Δ% cash	ΔHolding concent.	ΔIndustry concent.	ΔLocal hldg wt.	ΔActive share	ΔTotal risk	ΔMarket risk	ΔIdio. risk	ΔTrack. error
Panel A: Wealth shock defined below the 50th percentile										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Wealth shock	0.031^b	0.053	-0.087^b	-0.403^b	0.157	-0.011^b	-0.008^c	-0.010	-0.008^b	-0.010^b
	(0.014)	(0.195)	(0.035)	(0.188)	(0.323)	(0.004)	(0.004)	(0.006)	(0.004)	(0.004)
Observations	1,039	944	1,057	1,058	820	639	1,053	1,053	1,053	1,053
Adj-R-squared	0.01	0.03	0.01	0.01	0.00	0.01	0.07	0.06	0.05	0.05
Panel B: Wealth shock defined below the 33rd percentile										
Wealth shock	0.049^a	-0.001	-0.118^a	-0.497^b	0.413	-0.015^b	-0.008	-0.008	-0.010^b	-0.011^b
	(0.019)	(0.247)	(0.042)	(0.235)	(0.401)	(0.006)	(0.005)	(0.008)	(0.005)	(0.005)
Observations	692	640	702	703	552	421	699	699	699	699
Adj-R-squared	0.01	0.02	0.01	0.01	0.00	0.02	0.07	0.06	0.05	0.06

Table 11:
Why do wealth-shocked managers lower risk?

The table reports the coefficient estimates and standard errors from the OLS estimation of the difference in difference regressions outlined in Table 3 for changes in total realized fund risk for samples split by median manager age, median manager experience, and median manager percentage of wealth invested in the fund. The percentage of manager wealth invested in the fund is computed as the midpoint of the manager ownership range (in dollars) at the end of 2008, scaled by 2008 estimated wealth. Models estimated follow the baseline model estimated in column 2 of Table 3. Control variables that are included in column 2 of Table 3 are included in all regressions, but their coefficient estimates and standard errors are not reported. Standard errors, clustered by fund, are in parentheses. Significance levels are denoted by a, b, c, which correspond to 1%, 5%, and 10% levels, respectively.

	Age		Experience		% wealth in fund	
	Young	Old	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
Wealth shock	-0.107^a	-0.066^b	-0.108^a	-0.053^c	-0.077^b	-0.020
	(0.029)	(0.030)	(0.031)	(0.028)	(0.038)	(0.030)
Additional controls	Y	Y	Y	Y	Y	Y
Observations	542	542	551	533	326	355
Adj-R-squared	0.02	0.02	0.01	0.02	0.01	0.01

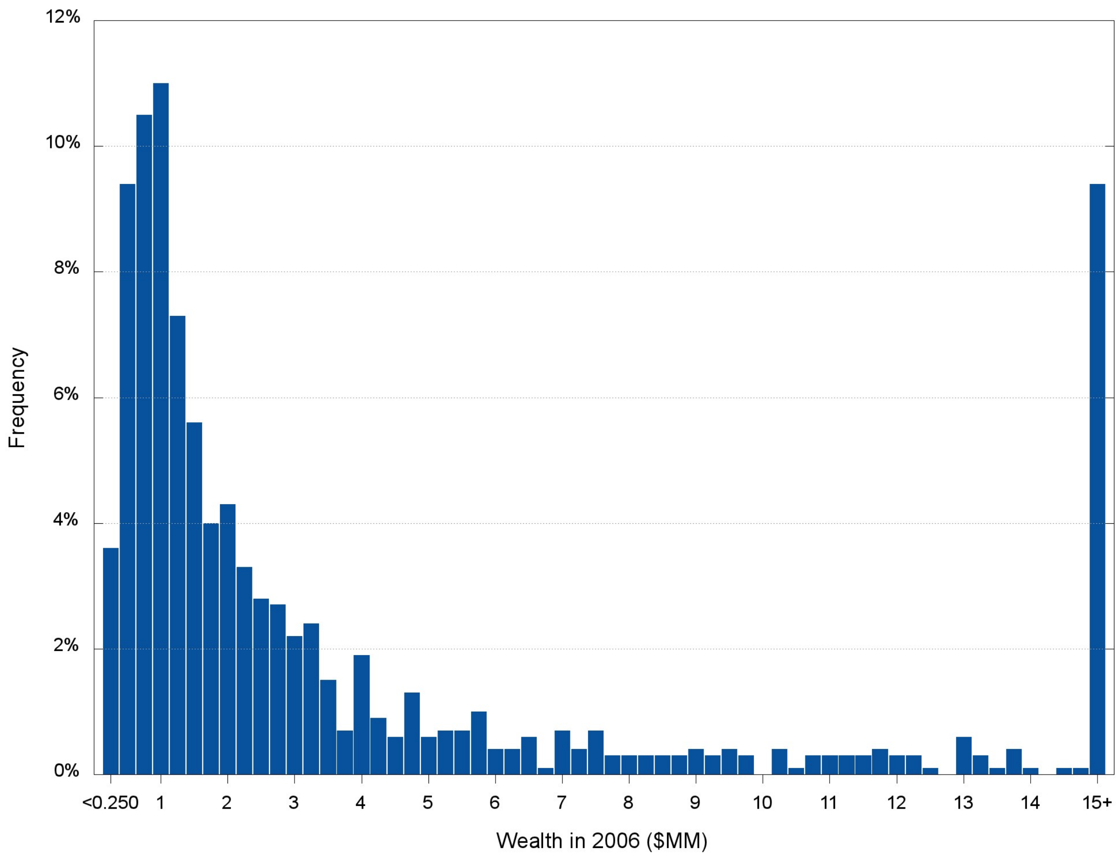

Figure 1: Distribution wealth in 2006

The figure shows the distribution of manager wealth at the end of the pre-event period (December, 2006). Wealth is estimated using the method described in Section 1.3.

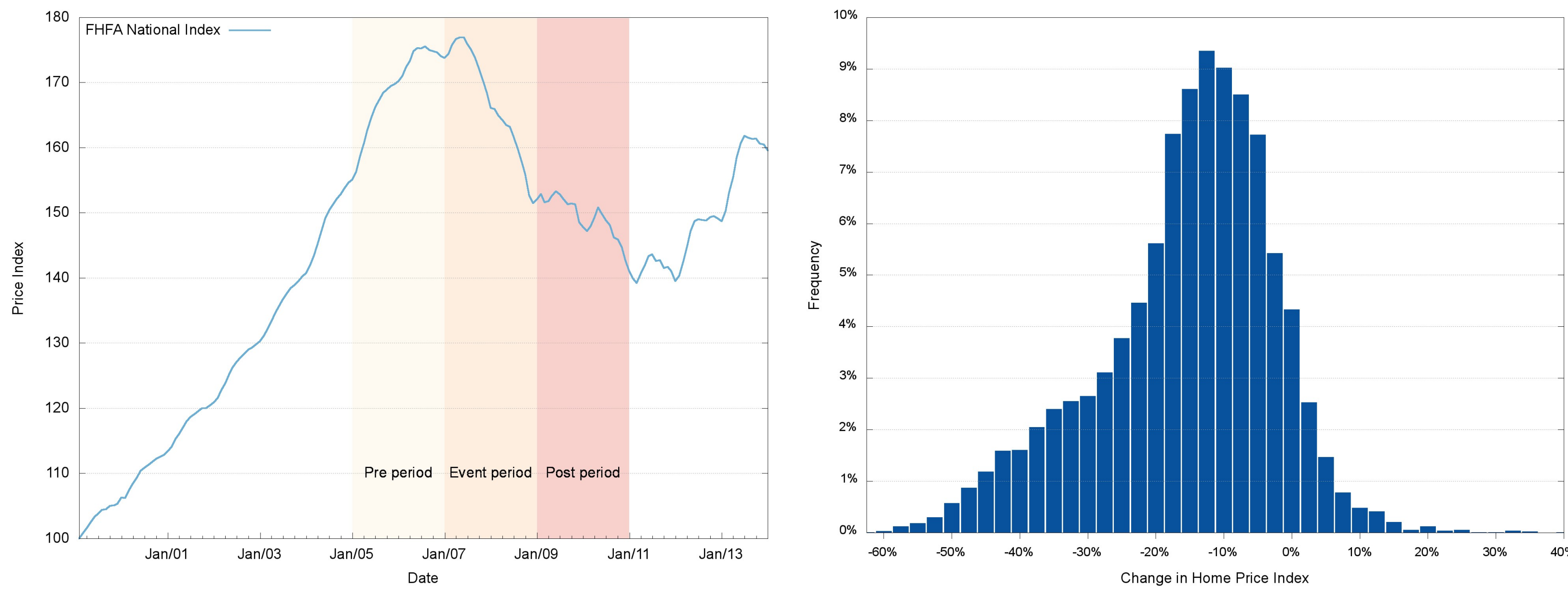

Figure 2: U.S. home prices

The left panel of the figure shows the Home Price Index created by the Federal Housing Finance Agency, which uses seasonally-adjusted sale price data. The event period used in the study, as well as the “pre” and “post” periods are shown with shading. The right panel shows a histogram of price changes during the event period, using the Zillow Home Value Index (ZHVI) created by Zillow for 10,000 zip codes each month.

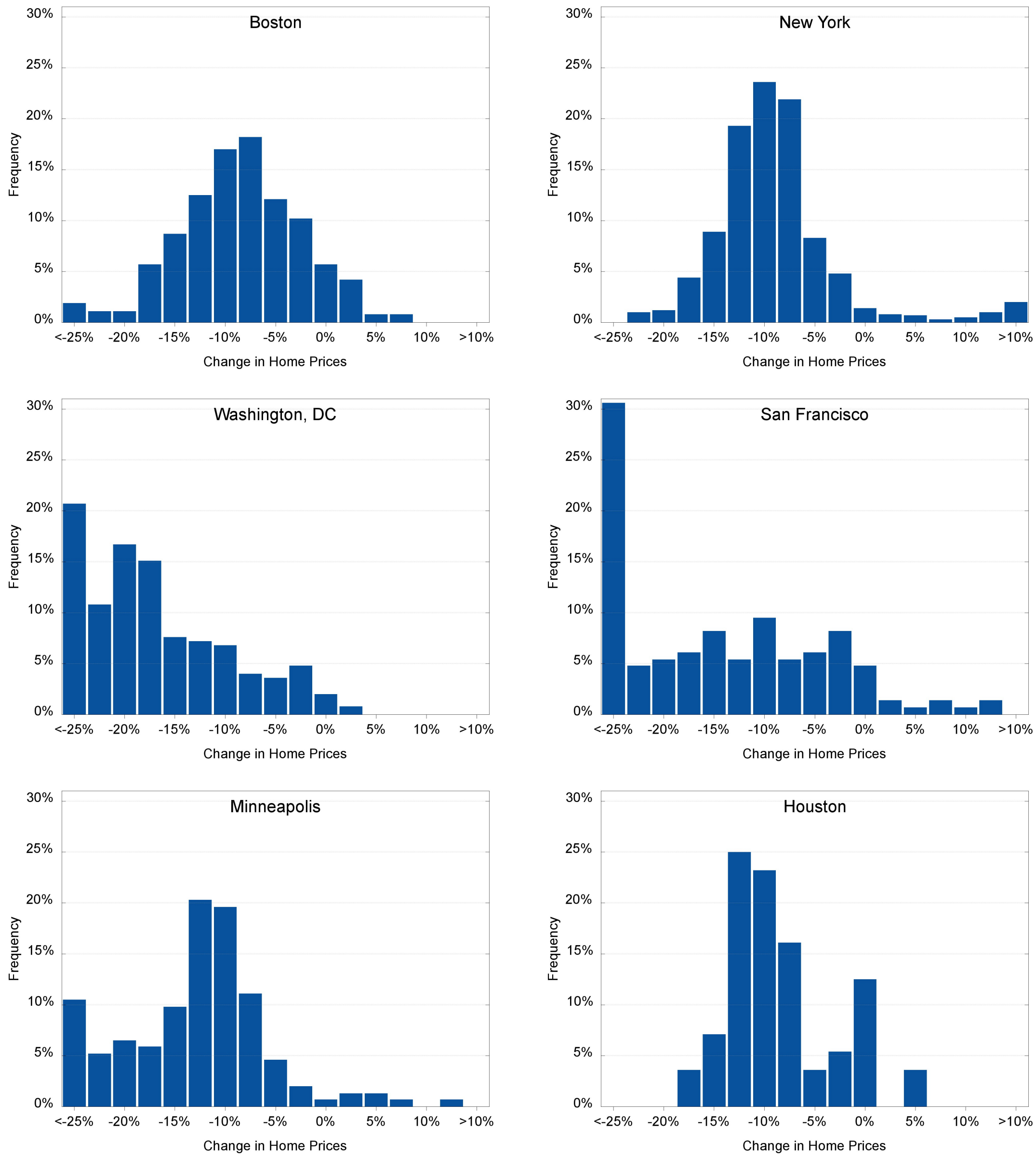

Figure 3: Distribution of housing price changes

The figure shows the distribution of home price changes for the six metropolitan areas with the highest number of fund managers in our sample. Home price changes are calculated over the event period (January 2006 to December 2008) using all zip codes in a metropolitan area in the Zillow data.

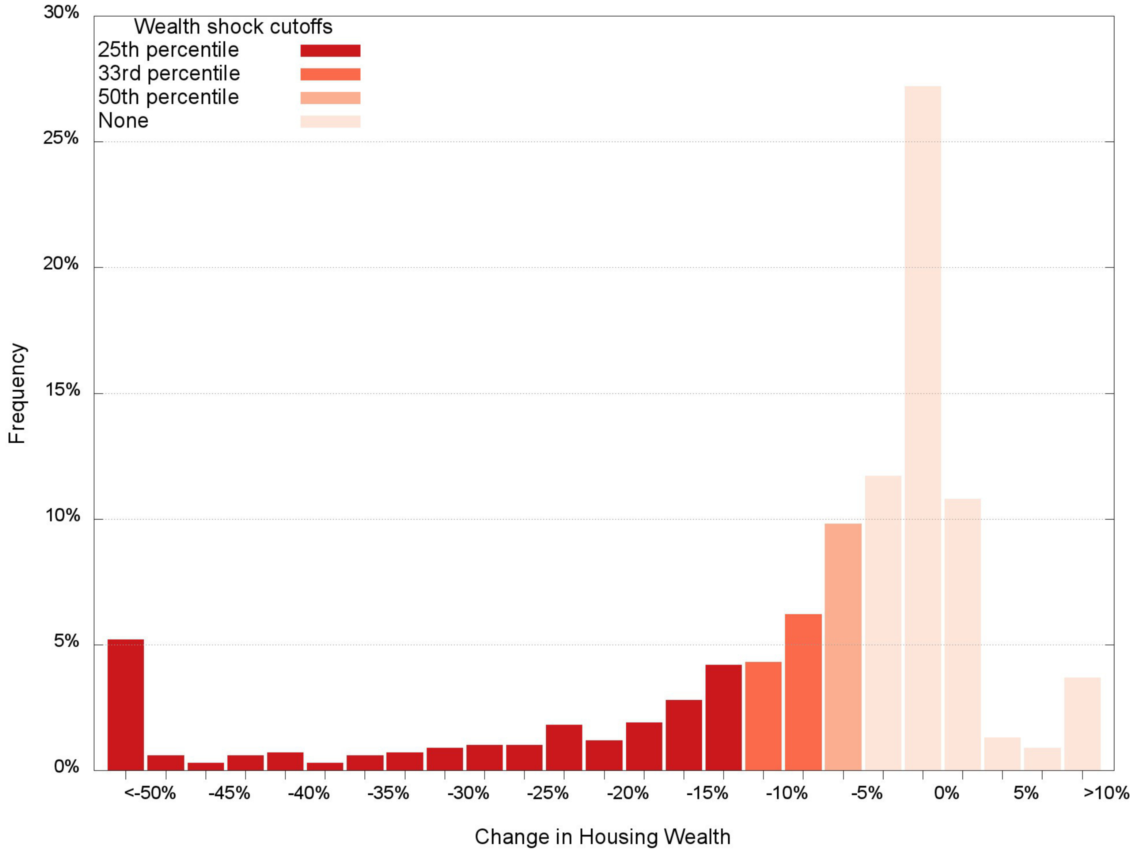

Figure 4: Distribution the percentage change in housing wealth

The figure shows the distribution of estimated housing wealth shocks during the event period. We calculate the total change in value for all homes owned by each manager, and scale by estimated wealth. Both home price changes and wealth are winsorized at 5% and 95% prior to calculating the wealth shock ratio. The histogram is plotted using 2.5% bins. Wealth shock cutoffs are determined at the 25th, 33rd, and 50th percentiles of the housing wealth change distribution for the entire sample, is are shown with increasingly dark shading.

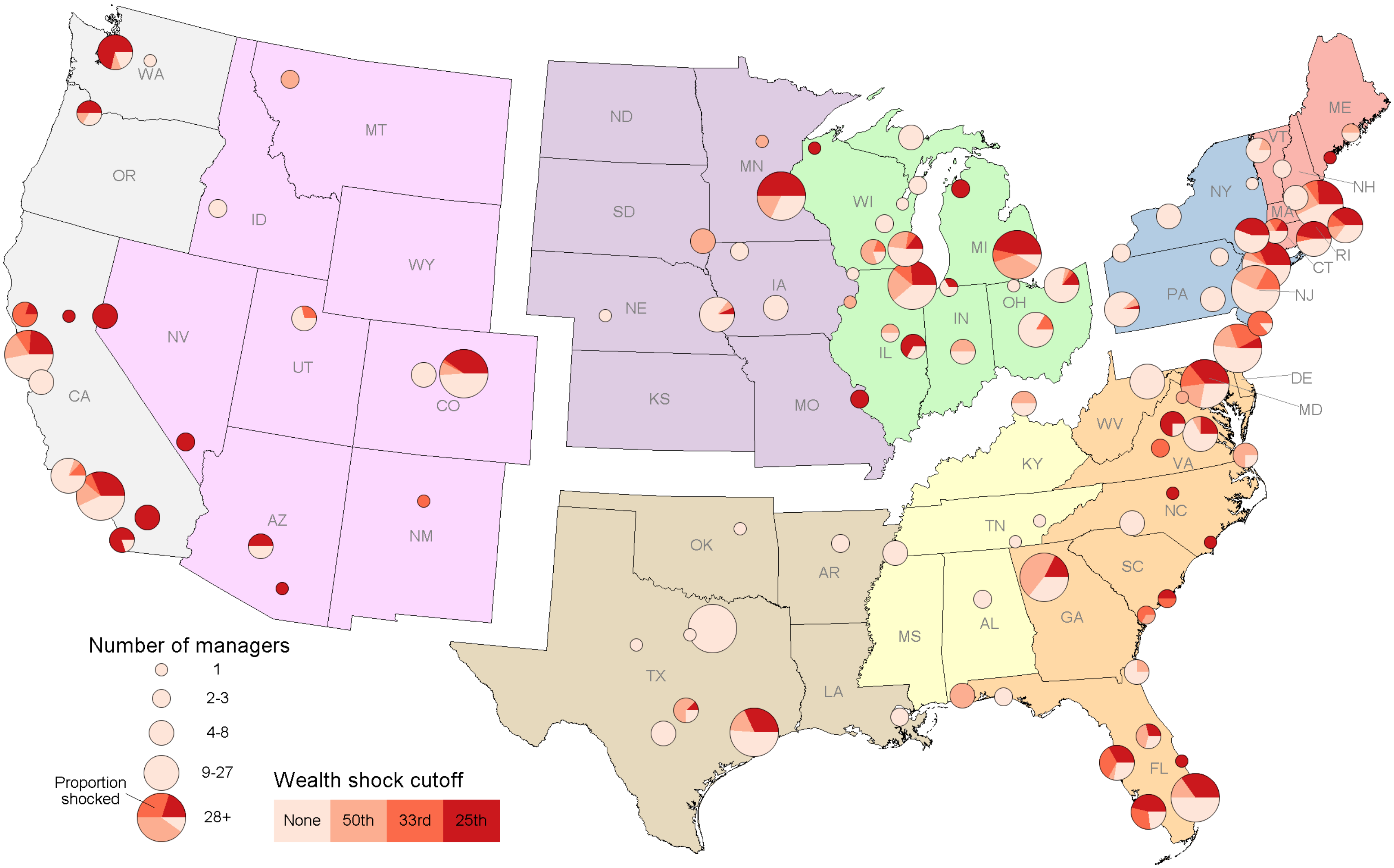

Figure 5: Manager homes and wealth shocks

The figure shows the location of managers in the sample. All homes are assigned to a zip code cluster ("city"), denoted by a circle. The size of the circle corresponds to the number of managers who live in each city. We define cities using an agglomerative hierarchical clustering algorithm that begins with each zip code as a singleton, and continues to join zip codes into clusters until each cluster is at least 50 miles apart. The proportion of managers in each city who experience wealth shocks at the 50th, 33rd, or 25th percentile, respectively, are denoted by pie slices with increasingly dark shading. U.S. census divisions are shown with different colors, and census regions are separated.

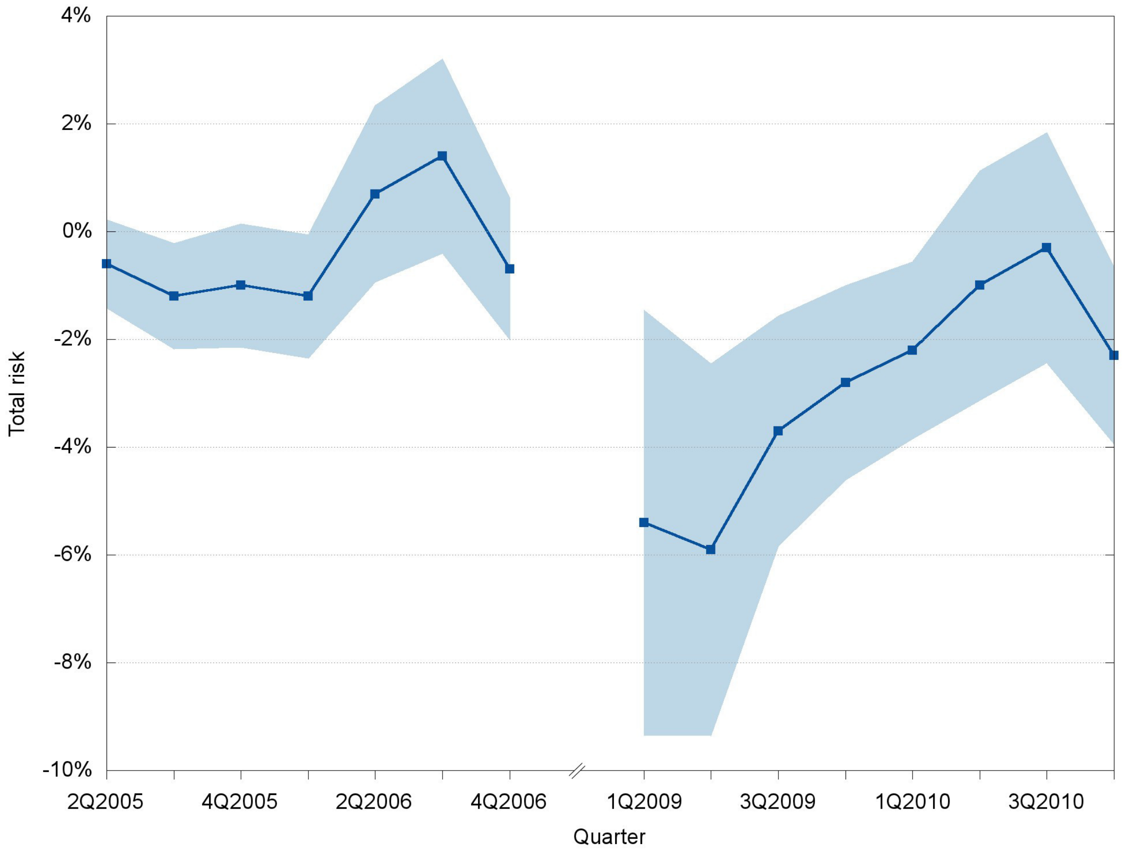

Figure 6: Differences in changes in total risk

The figure shows coefficient estimates and standard errors from the OLS estimation of the dynamic version of the difference in difference regression,

$$\sigma_{j,t} = \delta_{i,j} + \sum_{k=2}^{16}[\gamma_k Qtr_{k,t} + \gamma_{w,k} Qtr_{k,t} \times WealthShock_i] + \Gamma' Controls_{j,t-1} + \epsilon_{i,j,t},$$

total realized risk using the quarterly, manager-fund observation sample (15,933 observations) of active U.S. domestic equity mutual funds. $\delta_{i,j}$ is a manager-fund fixed effect, $Qtr_{k,t}$ is an indicator variable that is one if quarter t is the k^{th} quarter, and $WealthShock_i$ is an indicator variable that is one if the manager of the fund was treated with a shock to his wealth. The coefficients of interest that are plotted in the figure are the $\gamma_{w,k}$'s, which tell us the effect on risk taking of the wealth shock in the k^{th} quarter. Control variables that are included in column 2 of Table 3 are included in all regressions. Standard errors, clustered by fund, are shown by shading.

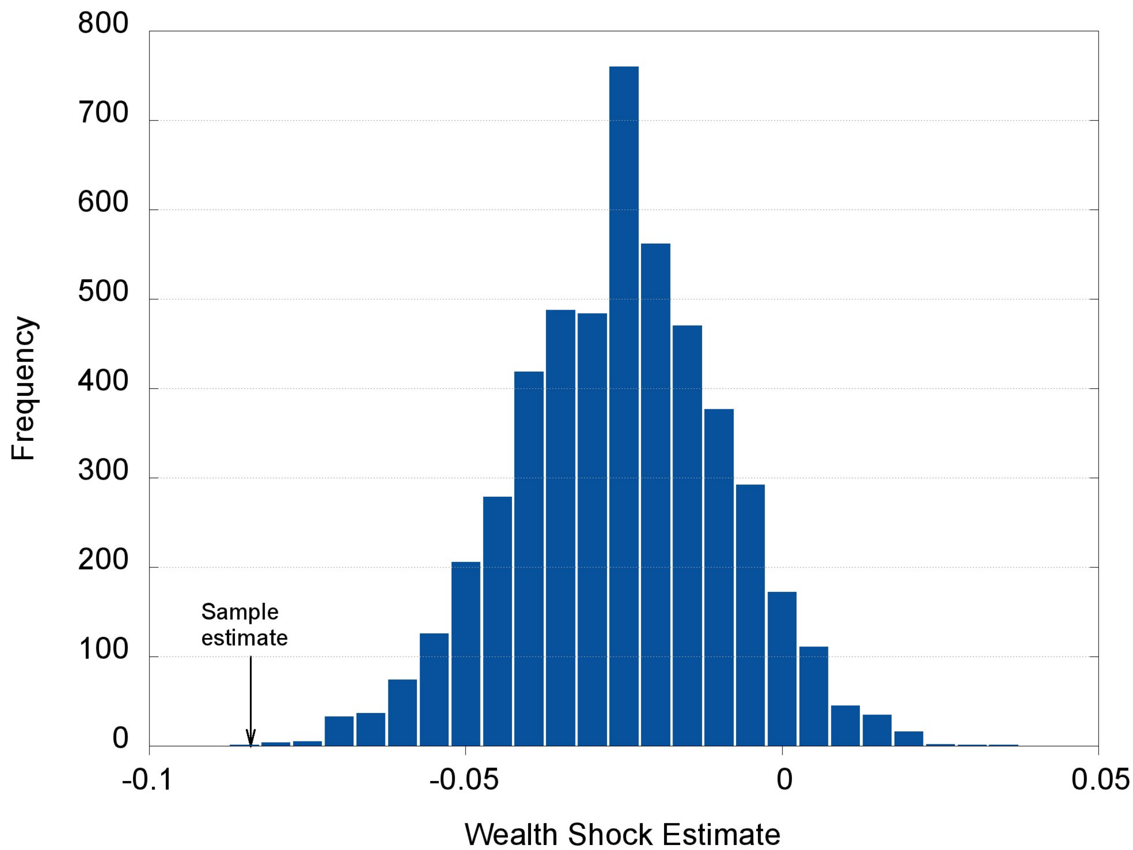

Figure 7: Distribution of coefficient estimates under placebo

The figure shows the distribution of coefficient estimates for the wealth shock in regressions using placebo data in 5,000 simulations. We rerun our main regression using fictitious wealth shock measures. We first assign a new wealth change variable to each manager by randomly drawing house price changes but keeping the manager's true wealth estimate as the denominator. This allows us to randomly treat the managers only with respect to their house price depreciation, while leaving wealth (and all other characteristics) unaltered. Once the wealth change values are assigned, we follow the baseline procedure and create the treatment group by using the sample median as the cutoff. The mean of the distribution is -0.028 with a standard deviation of 0.017. For comparison, the sample estimate from column 2 of Table 3 is also shown.

Internet Appendix to

“Do Shocks to Personal Wealth Affect Risk Taking in Delegated Portfolios?”

This appendix provides additional results that are not included in the main text of the article:

- List of variable definitions
- The algorithm for estimating total manager wealth
- Understanding wealth estimates
- Measuring shocks using alternative measures
- Alternative standard error estimate
- Percentage changes in risk
- Fund-level analysis

Variable definitions

Manager characteristics

Housing wealth. Sum of the dollar value of all homes owned by a manager (Source: Zillow).

Change in housing wealth during event period. Dollar amount of the change in housing wealth from January, 2006 to December, 2008 (Zillow).

Manager wealth. Total wealth of a manager, estimate using the technique explained in section 1.2 (Morningstar, CRSP, Zillow).

Manager age. Age of manager in years (LexisNexis).

Manager tenure. Number of years a manager has been a manager of a particular fund (Morningstar).

% wealth in fund. Midpoint of the manager fund ownership range (in dollars) at the end of 2008, scaled by 2008 estimated wealth (Morningstar).

Fund characteristics

Fund age. Number of years since fund inception date (CRSP).

Performance percentile. Percentile ranking of raw fund performance in previous year within each of the Morningstar 9-box style grid (CRSP, Morningstar).

Fund size. Total net assets (TNA) under management (CRSP).

Turnover. Annual fund turnover (CRSP).

Monthly expense ratio. Fund expense ratio (CRSP).

Number of managers. Number of managers in a fund at a given quarter-end (CRSP).

Fund risk taking measures and mechanisms

Total risk. Standard deviation of daily fund returns, measured quarterly (CRSP).

Market risk. Market beta estimated from regression of daily fund returns on CRSP value weighted market return, measured quarterly (CRSP).

Idiosyncratic risk. Standard deviation of daily return residuals from CAPM model, measured quarterly (CRSP).

Tracking error – market. Standard deviation of excess daily fund returns over the CRSP value weighted market return, measured quarterly (CRSP).

Tracking error − style. Standard deviation of excess daily fund returns over the TNA-weighted return of other funds in the same 9-box Morningstar style category (CRSP, Morningstar).

Number of stocks. Number of stocks held by fund at end of quarter (Thomson).

Percent of portfolio in cash. Percentage of total fund assets held in cash (Thomson).

Holdings concentration. Herfindahl index of holdings weights at end of quarter (Thomson).

Industry concentration. Herfindahl index of industry weights at end of quarter using the ten industries classification adopted by Kacperczyk et al. (2005) (Thomson, CRSP).

Holdings composition-based risk measures. Total risk, market risk, idiosyncratic risk, and tracking error measures calculated using stock return measures for stocks held by the fund at the beginning of the period. Stock returns are taken from the last quarter of the pre-event period. (Thomson, CRSP).

Estimating total manager wealth

We begin by estimating each manager's annual income as a fraction of the total expenses charged at all funds they manage. Each year, we calculate the average of each fund's TNA, reported in CRSP. This is typically reported monthly in the later part of the sample, but only quarterly or semiannually in the earlier periods. We then calculate total fund expenses by multiplying average TNA by the fund's expense ratio.

We assume that 50% of this quantity is available to pay manager salaries, and we allocate an equal share to all managers who work at the fund for at least six months in a given year. For each manager, we then sum income earned across all funds they manage to get a total estimated income each year.

We assume that end-of-period wealth evolves according to the equation

$$W_{i,t} = W_{i,t-1} \times R^p_{i,t} + \theta(1-\tau)I_t, \tag{4}$$

where I_t denotes income, $\tau = 0.33$ is the tax rate, $\theta = 0.2$ is the marginal propensity to save, and manager i's personal investment return in year t is $R^p_{i,t} = \omega_{i,t-1}R^E_t + (1-\omega_{i,t-1})R^B_t$, and R^E and R^B are the annual returns on the S&P 500 index and a Fama-Bliss Treasury bond portfolio, respectively. We assume the manager follows a portfolio allocation rule of $\omega_{i,t} = (100 - \text{age}_{i,t})/100$.

When a manager first enters the sample, we must assign her an initial level of wealth, W_0. We do so using the following iterative procedure. In the first step, we assume that managers have no previous wealth when they enter the sample, and use equation (4) to generate annual wealth values for each manager. We then calculate the median wealth by age (adjusted using CPI data to be in 2006 dollars), which we use in the next iteration as the initial wealth for a manager, after converting the value back to nominal terms. For example, a manager who is 43 years old when first appearing in the data in 1996 is assigned an initial wealth equal to the median wealth of all 43-year-old managers (whichever year they appear in the data), adjusted to 1996 dollars. In addition, we assume that managers previously had less lucrative jobs so we scale the initial wealth by 60%. (Admittedly, this is arbitrary, but this assumption does not affect our results.)

We repeat this procedure ten times, which is sufficient to allow convergence of the wealth function. In each iteration, we use the median wealth by age from the previous iteration to generate initial wealth levels for managers as they first enter the sample. In Figure IA.I we show median fund manager wealth by age after zero, one, and ten iterations. We initially restrict the sample to people older than 25 years. In addition, there are relatively few managers who are younger than 30, and the distribution is sensitive in this range to outliers. Therefore, we apply a final restriction to arrive at a relatively clean proxy for initial wealth. In particular, we fit a regression using only those managers between the ages of 30 and 85. The estimated model is

$$\text{median wealth} = -532741 + 174697(\text{age} - 25) - 8105(\text{age} - 25)^2 + 230.4(\text{age} - 25)^3 - 1.93(\text{age} - 25)^4. \quad (5)$$

All the coefficients are significant, and $R^2 = 0.99$. In the final wealth estimation, we use the fitted values from this regression as the initial wealth value for all managers, including those who are younger than 30 years old. We replace any negative predicted wealth levels with zero. This function is plotted as a solid line in the figure, and is used to determine initial wealth for all managers in the sample.

Table IA.I summarizes the relative importance of the factors that go into our wealth estimates.

Table IA.I:
Understanding wealth estimates

The table reports the coefficient estimates and standard errors from OLS estimations of cross sectional regressions explaining the natural logarithm of estimated manager wealth at the end of 2006. This wealth estimate is used in the denominator of the treatment variable of interest in the paper; percentage change in wealth due to housing. Independent variables are defined in the Internet Appendix. Independent variables are standardized to have means of zero and standard deviations of one. Significance levels are denoted by *a*, *b*, *c*, which correspond to 1%, 5%, and 10% levels, respectively.

Dependent variable:	Log (manager wealth)			
	(1)	(2)	(3)	(4)
Log fund size	0.420[a]		0.044	
	(0.042)		(0.038)	
Expense ratio	-0.078[b]		0.005	
	(0.039)		(0.030)	
Number of managers	-0.059		-0.043	
	(0.041)		(0.030)	
Manager age	0.264[a]		0.372[a]	0.372[a]
	(0.077)		(0.057)	(0.058)
Manager experience	0.727[a]		0.483[a]	0.487[a]
	(0.075)		(0.055)	(0.055)
Log manager income		1.038[a]	0.852[a]	0.871[a]
		(0.059)	(0.062)	(0.052)
Observations	1,150	1,151	1,150	1,151
Adj-R-squared	0.50	0.48	0.73	0.73

Table IA.II:
Personal wealth shocks and risk taking - alternative shock measures

The table reports the coefficient estimates and standard errors from the OLS estimation of the difference in difference regressions outlined in Table 3 for various measures of changes in realized fund risk. Models estimated follow the baseline model estimated in column 2 of Table 3. Wealth shocks in this table are defined by the change in total housing wealth during the event period relative to 2006 estimated *income* in row 1 and the average of the percent change in home price of all homes owned by the manager in row 2. For each measure of realized risk, treatment and control groups are defined in three different ways. In columns 1, 4, 7, and 10 managers are wealth shocked if the change in total housing wealth relative to income is less than the median in the sample and are included in the control group otherwise. In columns 2, 5, 8, and 11 the treated managers are in the bottom third of the wealth change and control groups are in the top third. In columns, 3, 6, 9, and 12 wealth shocked managers are in the bottom quarter of the wealth change and control group members are in the top quarter. Control variables that are included in column 2 of Table 3 are included in all regressions, but their coefficient estimates and standard errors are not reported. Standard errors, clustered by fund, are in parentheses. Significance levels are denoted by a, b, c, which correspond to 1%, 5%, and 10% levels, respectively.

Dependent variable:	ΔTotal risk			ΔMarket risk			ΔIdio. risk			ΔTrack. err.		
Percentile defining housing wealth shock:	50^{th}	33^{rd}	25^{th}	50^{th}	33^{rd}	25^{th}	50^{th}	33^{rd}	25^{th}	50^{th}	33^{rd}	25^{th}
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(1) Wealth shock (Δ Housing wealth / income)	-0.094^a	-0.108^a	-0.141^a	-0.036^a	-0.045^a	-0.052^a	-0.019^a	-0.029^a	-0.034^a	-0.030^a	-0.043^a	-0.047^a
	(0.021)	(0.027)	(0.033)	(0.012)	(0.015)	(0.017)	(0.007)	(0.008)	(0.009)	(0.009)	(0.011)	(0.013)
(2) Wealth shock (Avg. % Δ house price)	-0.050^b	-0.053^b	-0.063^b	-0.002	-0.001	-0.015	-0.018^a	-0.023^a	-0.026^a	-0.028^a	-0.035^a	-0.036^a
	(0.021)	(0.027)	(0.031)	(0.011)	(0.014)	(0.016)	(0.007)	(0.008)	(0.009)	(0.010)	(0.012)	(0.014)
Additional controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,084	721	542	1,084	721	542	1,084	721	542	1,084	721	542

Table IA.III:
Personal wealth shocks and risk taking - alternative standard error estimates

The table reports the wealth shock coefficient estimates and standard errors from the OLS estimation of the difference in difference regressions outlined in Table 3 for various measures of changes in realized fund risk for three alternative methods of estimating standard errors. Standard errors are clustered by city, management company zip code, and fund manager home zip code in columns 1 through 3, respectively. We define cities using an agglomerative hierarchical clustering algorithm that begins with each zip code as a singleton, and continues to join zip codes into clusters until each cluster is at least 50 miles apart. Models estimated follow the baseline model estimated in column 2 of Table 3. Wealth shocks in this table are defined as below the median percentage change and housing wealth during the event period. Control variables that are included in column 2 of Table 3 are included in all regressions, but their coefficient estimates and standard errors are not reported. Standard errors are in parentheses. Significance levels are denoted by a, b, c, which correspond to 1%, 5%, and 10% levels, respectively.

	Wealth shock coefficients and standard errors		
Standard errors clustered by:	City (1)	Mgt. zip (2)	Mgr. zip (3)
Δ Total risk	-0.086^{a}	-0.081^{a}	-0.082^{a}
	(0.023)	(0.026)	(0.027)
Δ Market risk	-0.051^{a}	-0.049^{a}	-0.049^{a}
	(0.018)	(0.014)	(0.015)
Δ Idiosyncratic risk	-0.018^{a}	-0.017^{b}	-0.017^{b}
	(0.006)	(0.007)	(0.008)
Δ Tracking error	-0.030^{a}	-0.030^{a}	-0.030^{b}
	(0.009)	(0.011)	(0.012)
Additional controls	Y	Y	Y
Observations	1041	1065	1075

Table IA.IV:
Personal wealth shocks and risk taking - percentage changes in risk

The table reports the coefficient estimates and standard errors from the OLS estimation of the difference in difference regressions outlined in Table 3 for various measures of percentage changes in realized fund risk. Models estimated follow the baseline model estimated in column 2 of Table 3. Control variables that are included in column 2 of Table 3 are included in all regressions, but their coefficient estimates and standard errors are not reported. Wealth shocks in this table are defined as below the median percentage change in wealth. Standard errors, clustered by fund, are in parentheses. Significance levels are denoted by a, b, c, which correspond to 1%, 5%, and 10% levels, respectively.

Dependent variable:	%ΔTotal risk	%ΔMarket risk	%ΔIdio. risk	%ΔTrack. error
	(1)	(2)	(3)	(4)
Wealth shock	-17.70^{a}	-3.90^{a}	-11.40^{a}	-16.10^{a}
	(3.50)	(1.10)	(2.80)	(3.60)
Additional controls	Y	Y	Y	Y
Observations	1,084	1,084	1,084	1,084
Adj-R-squared	0.04	0.03	0.04	0.02

Table IA.V:

Personal wealth shocks and risk taking - fund-level analysis

The table reports the coefficient estimates and standard errors from the OLS estimation of the difference in difference regressions outlined in Table 3 for various measures of changes in realized fund risk. Models estimated follow the baseline model estimated in column 2 of Table 3. Observations are at the fund level and not the manager-fund level as in earlier tables. Control variables that are included in column 2 of Table 3 are included in all regressions, but their coefficient estimates and standard errors are not reported. Wealth shocks in this table are defined as below the median average percentage change in wealth of all managers of the fund. Standard errors, clustered by fund, are in parentheses. Significance levels are denoted by *a*, *b*, *c*, which correspond to 1%, 5%, and 10% levels, respectively.

Dependent variable:	ΔTotal risk	ΔMarket risk	ΔIdio. risk	ΔTrack. error
	(1)	(2)	(3)	(4)
Wealth shock	-0.073^a	-0.042^a	-0.015^b	-0.021^b
	(0.024)	(0.013)	(0.007)	(0.010)
Additional controls	Y	Y	Y	Y
Observations	729	729	729	729
Adj-R-squared	0.01	0.03	0.04	0.03

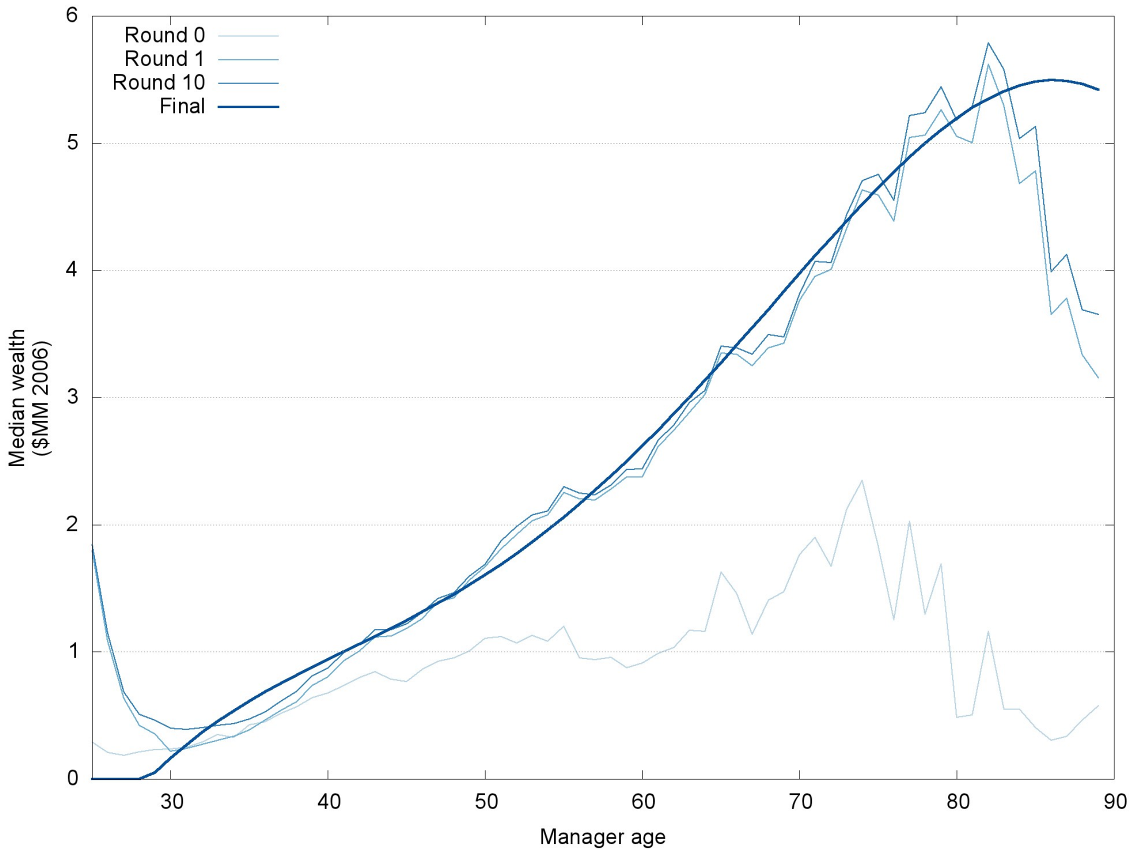

Figure IA.I: Fund manager wealth

The figure shows median fund manager wealth, determined using the technique described in the paper. We plot median wealth (in millions of 2006 dollars) at three stages in our iterative estimation procedure: the initial round (Round 0), the next round (Round 1), and the last round (Round 10). We also plot the predicted values from regression (5), which we use as initial wealth for managers when they enter the sample (Final).