Trust Busting: The Effect of Fraud on Investor Behavior

Umit G. Gurun
University of Texas at Dallas and NBER

Noah Stoffman
Indiana University

Scott E. Yonker
Cornell University

May 24, 2017

Abstract

We study the importance of trust in the investment advisory industry by exploiting the geographic dispersion of victims of the Madoff Ponzi scheme. Residents of communities that were exposed to the fraud subsequently withdrew assets from investment advisers and increased deposits at banks. Additionally, exposed advisers were more likely to close. Advisers who provided services that can build trust, such as financial planning advice, experienced fewer withdrawals. Our evidence suggests that the trust shock was transmitted through social networks. Taken together, our results show that trust plays a critical role in the financial intermediation industry. (*JEL* D14, G11, G20)

We thank Kenneth Ahern, Geoffrey Booth, Alex Butler, Lauren Cohen, Bill Cready, Francesco D'Acunto, Kent Daniel, Alexander Dyck, Joey Engelberg, Eitan Goldman, Todd Gormley, Byoung-Hyoun Hwang, Christian Leuz, Chris Malloy, Pedro Matos, Chris Parsons, Gus Sauter, Amit Seru, Eric So, René Stulz, Geoff Tate, Anjan Thakor, and Kai Wu and seminar participants at the NBER Behavioral meeting (Fall 2015), the Rodney White Conference on Asset Management (2016), the Texas Finance Festival (2016), the Northeastern Finance Conference (2016), the Western Finance Association annual meeting (2016), the Ohio State Finance Department Alumni Conference (2017), Babson College, Ben-Gurion University of the Negev, Cornell University, City University of Hong Kong, Hebrew University of Jerusalem, Hong Kong University, Nanyang Technological University, National University of Singapore, Singapore Management University, University of Illinois at Chicago, University of Virginia, and Vanderbilt University for helpful comments. We are grateful to Matt Cain for helping us to obtain the Form ADV data.

Send correspondence to Scott E. Yonker, Assistant Professor and Lynn A. Calpeter Sesquicentennial Faculty Fellow in Finance, 201J Warren Hall, Dyson School of Applied Economics and Management, Cornell University, Ithaca, NY 14853; telephone: (607) 255-1378. E-mail: syonker@cornell.edu.

Social scientists have long argued that trust lies at the heart of the success of market economies. In his 1995 bestseller *Trust*, the political scientist Francis Fukuyama argued that countries with high levels of trust would be better positioned to compete in a globalizing world. The gist of this argument was not new to economists, who had understood the importance of trust at least since Arrow (1969), who wrote: "It is useful for individuals to have some trust in each other's word. In the absence of trust it would become very costly to arrange for alternative sanctions and guarantees, and many opportunities for mutually beneficial cooperation would have to be foregone."

The economic importance of trust in many contexts is well understood: it is associated with economic growth (Putnam, Leonardi, and Nanetti 1994; Knack and Keefer 1997; Zak and Knack 2001); the size of firms (La Porta et al. 1997; Bloom, Sadun, and Van Reenen 2012); financial development (Guiso, Sapienza, and Zingales 2004, 2008; D'Acunto, Prokopczuk, and Weber 2015); and international trade and investments (Guiso, Sapienza, and Zingales 2009). The literature has not, however, broadly examined or quantified the effects of trust in the investment advisory industry, an industry in which trust must play a particularly important role in overcoming incomplete contracts. This is the focus of our study.

We quantify the importance of trust by exploiting a large negative shock to investor trust in financial advisers. The trust shock could have led investors to update their beliefs about the risk of having their assets stolen, causing them to withdraw investments from delegated managers in favor of the relative safety of banks. We find strong evidence that investors did precisely that. Importantly, though, we also find that money managers who provide additional services that can build trust, such as financial planning advice, experienced fewer withdrawals. That is, consistent with the prediction of Gennaioli, Shleifer, and Vishny (2015), "money doctors" who build trust with clients by providing "hand-holding" were able to substantially avoid the effects of this reduction in trust.

To identify the causal effects of trust, we exploit the collapse of the multibillion dollar Ponzi scheme orchestrated by Bernard Madoff, which was uncovered in December 2008. The Madoff fraud provides a particularly good testing ground to study trust for a number of reasons. First, the fraud was extremely large, and directly affected many geographically dispersed investors.[1] Second, the fraud was explicitly a shock to trust, as is made clear by the 45 mentions of "trust" in the 113 victim impact statements that were submitted to the court.[2] Third, because the fraud targeted a particular group of investors, we are able to study how the trust shock is transmitted through social networks.

A common factor in the success of a Ponzi scheme is whether an "affinity" link is present between the perpetrator and the targeted victims. In their study of 367 Ponzi schemes, Deason, Rajgopal, and Waymire (2015) find that common religion is one of the most frequent affinity links cited by the SEC. The Madoff scheme was an example of such a fraud, with many victims being Jewish people and organizations. The losses were widely felt in the Jewish community, with a number of charities being forced to cut back operations, and in some cases, close.[3] We therefore refer to the Jewish community as the "affinity group" in the Madoff fraud.

We are interested in the effects of the shock to trust on people other than the direct victims of the fraud, since the behavior of direct victims will also be affected by a confounding wealth effect. Drawing on evidence that social connections and geographic proximity influence investment

[1]The total amount of losses is difficult to determine. The original criminal complaint against Madoff alleged a $50 billion fraud, but later estimates were between $10 and $17 billion, and court-ordered restitution was $17 billion. Calculations vary depending on whether fictitious profits are included, for example. It is estimated that about half of Madoff's clients lost no money.

[2]Guiso (2010) uses survey data to document a large reduction in trust in areas more affected by the Madoff fraud and concludes that his evidence "...proves that the Madoff fraud has lowered trust in financial intermediaries..." (p. 10).

[3]A *New York Times* article notes that "...among the apparent victims of Mr. Madoff were many Jewish educational institutions and charitable causes that lost fortunes in his investments; they include Yeshiva University, Hadassah, the Jewish Community Centers Association of North America and the Elie Wiesel Foundation for Humanity. The Chais Family Foundation, which worked on educational projects in Israel, was recently forced to shut down because of losses in Madoff investments. Many of Mr. Madoff's individual investors were Jewish and supported Jewish causes, apparently drawn to him precisely because of his own communal involvement and because he radiated the comfortable sense of being one of them" (`http://www.nytimes.com/2008/12/24/us/24jews.html`). See also `http://blogs.wsj.com/deals/2008/12/15/madoff-the-atomic-bomb-for-jewish-charities/`.

behavior (Hong, Kubik, and Stein 2005; Ivković and Weisbenner 2007; Pool, Stoffman, and Yonker 2015), we hypothesize that investors who are socially connected to a victim or members of the same affinity group are also likely to suffer a reduction in trust. Moreover, while the Madoff fraud was a major national news event, local media in areas with more victims and more members of the affinity group may have provided more extensive coverage. People in these areas may therefore have been more aware of the fraud and felt its effects more directly, especially if they knew victims personally. Evidence from internet search patterns supports the view that people in areas with more victims were more interested in the fraud: Looking at Google data for the search term "Madoff" in the year after the fraud, the rank correlation between the number of victims in a state and the Google interest index in that state is 0.77.[4] Therefore, we exploit the relative concentration of victims in certain areas to implement difference-in-differences tests that identify the causal effect of trust on investor behavior.

The firm through which the fraud was perpetrated, Bernard L. Madoff Investment Securities (BMIS), was regulated by the Securities and Exchange Commission (SEC) as a registered investment adviser (RIA). Despite a series of red flags, the SEC did not act until Madoff's son turned him in.[5] Thus, in the eyes of some, the Madoff fraud was seen as a failure of the SEC.[6] People lost trust in the system. Indeed, using Gallup survey data, we confirm that people who were more exposed to the Madoff fraud reported larger declines in confidence in the criminal justice system than unaffected people; these results are confined to college-educated people and those with higher levels of income (see Table A.1 of the Internet Appendix).

Investors may have thought that if a former chairman of the NASDAQ could perpetrate such a fraud, then other fraudsters might exist among investment advisers (Guiso 2010). Therefore, in areas

[4]Google constructs a search index for any search term, which is available over time by region. Results for the search term "Madoff" during 2009 are at `https://goo.gl/3QPpoN`.

[5]Markopolos (2010) documents three cases in which he provided evidence of the Madoff fraud to the SEC, beginning in 2000. See Gregoriou and Lhabitant (2009) for additional discussion.

[6]For example, one victim writes in a statement to the court: "In addition to Madoffs [*sic*] actions, our own government has failed us completely. The failure of the SEC to act when they had all the information necessary to stop Madoff in his tracks. Now the SIPC and Mr. Picard is peforming [*sic*] in a manner to deny us our rights they were supposed to protect."

in which investors were more affected by the fraud, we expect to see abnormal outflows from RIAs and inflows into bank deposits. To test this, we use court documents to identify the direct victims of the Madoff fraud by name and address. We then aggregate the number of victims in a particular geographic area, and define the treatment variable as the relative concentration of victims in that area. We construct a panel of investment adviser flows using detailed information on annual assets under management (AUM) and the clientele locations for all RIAs. We also collect branch-level cash deposits at banks from the FDIC. Together, these data enable us to use a difference-in-differences framework to estimate the effects of the shock to trust on both the amount invested through RIAs and in bank deposits.

We find that the shock to trust led investors to move money out of risky investments and into low-risk bank deposits. The results are strongest among those RIAs that are most similar to Madoff's firm—those that invested in pooled investment vehicles and did not provide financial planning services. There is no evidence that the withdrawals are reversed—even up to four years after the fraud was revealed—suggesting that trust shocks have long-lasting effects on investment decisions.

We find that outflows from RIAs led to a significant increase in the number of firm closures: RIAs with clients in regions that were more affected by the trust shock were over 40% more likely to close than control firms following the Madoff event. We also show that bank branches located in ZIP codes with more Madoff victims saw abnormal increases in cash deposits of about 4%.

The economic magnitude of our estimates is large. An adviser with clients in an area with one standard deviation more victims per capita experienced abnormal reductions in their AUM of 4.9%. Aggregating across all RIAs, our estimates indicate that the abnormal outflows due to the trust shock were around $363 billion, meaning that the wealth lost by direct victims can explain less than 5% of the liquidation of assets from RIA accounts. The remaining funds were likely liquidated by investors who were not directly affected by the wealth shock, suggesting that investors updated their beliefs about the probability of fraud after learning about the Madoff Ponzi scheme.

An important welfare implication of these withdrawals could be a reallocation away from risky investments at a time when expected returns are high. Our estimates indicate that at least 27% of RIA withdrawals were deposited into banks. While we can't observe what happened to the other portion of the withdrawals, we conservatively estimate that allocation to cash substantially increased as a result of the fraud.

Our evidence is clear that the trust shock affected many investors indirectly, and also sheds light on the mechanism for the transmission of the trust shock. Abnormal withdrawals from RIAs are concentrated in areas with large populations of the affinity group, suggesting that social networks played a critical role in propagating the effect. At the same time, our analysis of the bank deposit data highlights the localized effect of the shock, which dies out within about ten miles of a bank branch, suggesting that media played less of a role.

Our proxy for a shock to trust is particularly clean, but the empirical setup does pose two challenges. First, the discovery of the Madoff fraud coincided with the 2008 financial crisis. Perhaps people living in areas with many Madoff victims withdrew money from RIAs due to another shock that happened to affect people living in these locations precisely at the same time as the Madoff fraud revelation. For example, Florida—where several hundred Madoff victims resided—felt the subprime mortgage crisis more than other areas (Mian and Sufi 2009). If investors in these areas reduced their holdings at RIAs to cope with potential negative consequences of the subprime mortgage crisis, and not due to the Madoff fraud, that would cast doubt on whether the Madoff trust shock per se generated the effect that we document. Therefore, our regressions include time-varying fixed effects at various geographic levels and a battery of controls (including ZIP-code-level house price appreciation) to control for contemporaneous changes in the economic environment. We also show that our treatment and control groups exhibit parallel trends in observed characteristics prior to the shock and find that our results are robust to different econometric specifications.

A second challenge is that Madoff did not choose clients randomly: He targeted older and wealthier Jewish investors. It is possible that people in the communities in which Madoff victims reside behave relatively more conservatively in the face of a financial crisis. We alleviate this concern by confirming that our results continue to hold in matched samples that have no observable differences between the treatment and control groups along the dimensions of wealth, age, religious composition, or RIA size. Further, we use placebo tests to show that exposure to the Madoff fraud *only* influences RIA flows and bank deposits following the Madoff fraud revelation. This suggests that unobservable differences between the treatment and control groups are likely not driving our main findings. To bolster this point, for a subset of RIAs that disclose their equity holdings, we show that neither the riskiness nor the returns of portfolios managed by RIAs is affected by RIA exposure to the Madoff fraud surrounding the event. To the extent that managers cater to their clients' risk preferences, this result suggests that these particularly important unobservables are not driving our findings.

Our paper is related to a number of others that have studied how trust in financial institutions and advisers affects investor behavior and asset allocation. Choi and Kahan (2007) find that mutual fund investors made substantial withdrawals from funds that were implicated in the 2003 late-trading scandal. Also using mutual fund data, Kostovetsky (2016) argues that a decline in trust leads investors to withdraw investments from funds that announce changes in ownership. More broadly, Guiso (2010) studies the decline of trust in financial institutions following the 2008 financial crisis, while Sapienza and Zingales (2012) argue that it was the trust decline that precipitated the subsequent Great Recession. Giannetti and Wang (2016) find that federal securities enforcement actions against fraudulent firms lead to reduced stock market participation of households in the fraudulent firm's state. Similarly, Georgarakos and Inderst (2014) show that trust in financial advice affects stock market participation, although primarily among less-sophisticated investors. Trust is one of the most-cited features that investors look for in a financial service provider (Hung et al. 2008) and is correlated with the likelihood of following through on advice (Burke and Hung 2016). Investors who do rely on advice end up trading more (Hackethal, Inderst, and Meyer 2012). Our findings complement those of this existing literature, and especially Guiso, Sapienza, and Zingales (2004, 2008), who find that individuals who exhibit a high level of trust invest more than others in risky assets.

We expand on this literature by exploiting an exogenous shock to trust and providing empirical evidence of the importance of trust in the investment advisory industry. Specifically, we show a causal relationship between trust and portfolio allocation and investment flows; that trust is transmitted through social channels; and that trust has real economic effects, in that RIAs with trust-shocked investors were more likely to go out of business.

1 Data and Sample Construction

The analysis relies on three main sources of data: court documents listing the victims of BMIS; the SEC's Form ADV; and the FDIC's Summary of Deposits data. In this section we describe these sources and outline our sample construction. Appendix A provides further detail.

1.1 Madoff victim data

We obtain the list of BMIS clients from court documents released by the U.S. federal bankruptcy court in February 2009.[7] This list contains approximately 14,000 investors, although some investors are mentioned multiple times, as they had more than one account. Victims are identified by name and address. In some cases, multiple victims have the same address, in which case the address is usually the office of a financial adviser. After cleaning the data to identify duplicates, we have

[7] Available at `http://www.scribd.com/doc/11705845/Bernie-Madoff-s-Clients-The-List`.

10,276 unique names at 5,907 unique addresses, which is similar to the 11,374 victims reported by Sander (2009).

Some investments were funneled to Madoff through "feeder" funds set up by investment firms. The largest of these, Fairfield Greenwich Advisors, was reported to have over $7.5 billion invested.[8] In the case of such feeder funds, the funds themselves, and not their individual investors, are listed as the victims. Since it is not possible to know how many investors are represented by these funds, we exclude them by removing corporate entities from our sample using filters on the investor names. We also exclude investors with foreign addresses, thereby removing some large foreign banks and investment funds headquartered in the Bahamas from our sample. Excluding these investors means that we are in some cases underestimating the size of the trust shock in some areas.

Madoff victims were particularly concentrated in certain geographic areas of the country, as shown in Figure 1. While some victims are found throughout the country, they are most concentrated in the Northeast from Philadelphia to Boston, in parts of California, and in southeast Florida, in particular around Miami. As we discussed in the Introduction, we observe the same geographic concentration in internet searches for information about Madoff. Figure 1 shows cities with the highest levels of Google searching in the year after the fraud was revealed; larger circles indicate more intense searching. The strong correlation between the location of victims and the intensity of searching suggests that the effect of the fraud was highest among people who live close to victims, or are socially connected to them.

1.2 Investment adviser data

We collect data on RIAs and their assets under management from Part 1A of SEC Form ADV, which we obtain through a series of Freedom of Information Act (FOIA) requests. The SEC provided us with all filings that were made subsequent to the inception of electronic filing in 2001. The form and

[8] See `http://s.wsj.net/public/resources/documents/st_madoff_victims_20081215.html`.

its schedules include detailed information about investment advisers, including general information about the advisory business, control persons, client composition, conflicts of interest, and criminal behavior.[9] While not all RIAs manage money, we focus on money managers by excluding RIAs who act exclusively as financial planners or investment consultants.

RIAs are required to update their filings, including AUM, annually. We construct a panel of U.S.-based adviser-year observations from 2006 to 2010, which straddles the December 2008 discovery of the Madoff fraud. Since the event occurred so close to the fiscal year-end of many firms, we exclude 2008 from our sample.

While numerous RIAs that invested with Madoff suffered massive outflows,[10] we exclude these firms since we are interested in studying the propagation of the trust shock to non-victims. Therefore we remove from our sample BMIS and any firm that was alleged to have invested with BMIS. In addition, we purge our sample of mutual fund advisers, since they have more stringent regulatory requirements than the typical investment adviser. Our final sample has 3,951 unique RIAs with main offices in every state.

To analyze a real effect of the trust shock, we also identify adviser closures using data on RIA withdrawals from SEC registration. These data are disclosed in form ADV-W (the withdrawal statement) and were obtained by an additional FOIA request. While there are many reasons for withdrawal, we focus our analysis on RIAs that report going out of business.

1.3 Branch-level deposit data

We collect data from the FDIC Summary of Deposits database to measure the spatial distribution of bank deposits. The FDIC collects these data from each institution through a survey. The data

[9] A number of papers have used Form ADV data to study hedge funds. See, for example, Brunnermeier and Nagel (2004), Brown, Goetzmann, Liang, and Schwarz (2008, 2009, 2012), Dimmock and Gerken (2012), Bollen and Pool (2012), and Clifford, Ellis, and Gerken (2016).

[10] An example is the Ivy Asset Management Corp., which lost approximately \$236 million with Madoff and subsequently saw its AUM drop from \$14.3 billion in 2007 to \$206 million in its final SEC filing in 2011. It withdrew its registration in 2012.

contain information about the location, ownership, and deposit amounts booked at all offices of FDIC-insured bank and thrift institutions as of June 30th of each year.

We aggregate deposits across all branches in a ZIP code. We drop observations if the street address cannot be unambiguously matched to a ZIP code. Nguyen (2014) reports that the percentage of unmapped observations is 7.5% in 1999 and declines to less than 1% during the sample period we are using in our study. Most of our analysis is conducted using data from 2007 to 2010. Over this period the data include aggregated deposit data from over 97,000 unique bank branches from 20,602 unique ZIP codes.

1.4 Additional data sources

We use a number of additional data sources to construct geographic control variables. Age, income, and population data are from the 2000 U.S. Census. Data on religious affiliation are from the 2000 Religious Congregations and Membership Study. These data are available at both the county and state level and can be downloaded from the Association of Religious Data Archives (ARDA) Web site.[11]

2 Empirical Methodology

We estimate the importance of trust using a difference-in-differences framework surrounding the Madoff fraud. Since some areas of the country were more exposed to the fraud than others, we are able to estimate differences in the changes in aggregate investment behavior between areas with differential exposure. As discussed in detail in the Introduction, our main hypothesis is that those areas with higher exposure will experience greater effects of the trust shock. We are not interested in the "wealth" effects of the Madoff fraud—that is, the reduction in investments due directly to the destruction of wealth by the fraud—but rather the impact of the reduction in trust induced by

[11] Available at `http://www.thearda.com/Archive/Files/Descriptions/RCMSCY.asp`.

the fraud. Our notion is that knowing others who were affected by the fraud could reduce investors' trust in investment advisers and, in turn, assets will flow from these advisers into safe assets such as bank deposits.

We face two important challenges with this empirical setup. First, Madoff targeted older, wealthier investors within the affinity group, and it is well documented that both age and wealth affect risk-taking (Malmendier and Nagel 2011). Second, the collapse of the fraud coincided with the financial crisis.[12] Because our methodology differences out the time effect, this should not be a major concern unless one believes that the financial crisis, and not the shock to trust, caused people nearer to Madoff victims to respond differently. Considering these issues together, the most plausible alternative that we must be careful to rule out is that differences in age and wealth across areas with differential exposure to Madoff led to differential responses to the financial crisis. This makes it important to control for the effects of demographic characteristics.[13]

We estimate various forms of the regression:

$$\log(\text{AUM})_{i,j,t} = \alpha_i + \gamma_{j,t} + \beta_M \text{Post}_t \times \text{Madoff exposure}_i + \sum_m (\beta_{C,m} \text{Post} \times \text{Control}_{i,m}) + \epsilon_{i,t}, \quad (1)$$

where $\log(\text{AUM})_{i,j,t}$ is the natural logarithm of the assets under management for RIA i with its main office in state j, in year t. "Post" is a dummy variable for the years following the December 2008 event.

Including RIA fixed effects, α_i, allows us to measure the effect on *changes* in AUM. However, these changes are determined not only by investor flows, which we want to identify, but also by the RIA's investment returns. We therefore include controls in most specifications to remove the effect

[12]Since Ponzi schemes rely on new investors to pay off existing investors, such schemes tend to collapse when new investors dry up or when many existing investors want to withdraw funds. Both of these things happened to BMIS in the aftermath of the financial crisis.

[13]Alesina and La Ferrara (2002) show that trust is not only related education, gender, or income of the individual but also to community characteristics, such as the level of income inequality in the location in which the investor lives. These findings are particularly important for our paper, as our interest lies in comparisons of different geographic regions after controlling for community characteristics that can influence the level of trust.

of these investment returns. In particular, we include fixed effects for the filing period to which an adviser's ADV report applies. These fixed effects are determined by the month and year of both the current and previous ADV filings, so they capture the effect of the average investment return on changes in AUM during the period covered by a filing.

The RIA fixed effects absorb unobservable time-invariant characteristics, but time-varying effects such as changing local economic conditions could also have an effect on how much investors contribute to their investment accounts. For example, investors living in locations that suffer adverse economic shocks may be less likely to increase their savings compared to investors living elsewhere. Our specifications therefore include state-year fixed effects to account for such differences across states in each year. Including fixed effects of this nature makes our specification analogous to that recommended by Gormley and Matsa (2014) to control for unobserved heterogeneity. In addition, we interact a number of demographic and firm-specific variables with the post period dummy to control for any observable differences in characteristics related to the treatment that could lead to differences in AUM flows around the event, such as investor wealth and age, or the size of the RIA.

We construct two measures of an RIA's clientele's exposure to Madoff victims ("Madoff exposure"). The first is based on the location of the investment adviser, while the second is based on the location of their clients. For the first measure, we compute the natural log of one plus the average number of victims in the same ZIP code as each reported domestic office. For the second measure, we begin by calculating the number of victims in each state. We then sum the number of victims and the population across all the states with which the advisory firm has at least five clients.[14] The measure of exposure is the average number of victims per 1,000 people in states in which the advisory firm has clients.

The coefficient of interest in Equation (1) is β_M. If a reduction in trust causes investors to move money out of investment adviser accounts, then β_M should be negative, indicating that people who are more exposed to the Madoff fraud abnormally withdrew their RIA investments following the

[14]See Appendix A for details about identifying office and client locations.

Madoff fraud. We then use an analogous specification to test whether investors moved assets into bank deposits.

We verify the parallel trends assumption that there is no treatment effect prior to the Madoff fraud revelation by plotting in Figures 2 and 3 the average change since 2005 in log(AUM) and log(deposits) for groups of RIAs and bank branches that were more exposed (treatment group) and less exposed (control group) to the Madoff fraud, respectively. The plots show that exposure levels do not affect adviser AUM or bank branch deposits until after the Madoff event, confirming the validity of the parallel trends assumption in our tests. In 2009 and 2010, RIAs that are more exposed to the Madoff fraud lose an abnormal percentage of their AUM, while bank branches that are more exposed experience an abnormal increase in deposits compared to the less exposed control group. (Note that the figure plots changes in log(AUM), not flows. AUM also changes with returns, and the return on the CRSP universe was 28% in 2009.)

3 Results

3.1 Investment adviser flows

We begin our main empirical analysis by showing that trust-shocked investors withdrew funds from accounts held at RIAs. First, we report summary statistics for our data in Table 1. Panel A displays these statistics for the investment adviser data. The median RIA has $225 million in assets under management, one office, and clients in 2.5 states. Fifty-two percent of RIAs average at least one Madoff victim in the same ZIP code as their offices. The fact that this is so high indicates that RIAs locate in areas with wealthier investors, which is also where the fraud victims are more likely to be. Financial planning services are provided by 47% of advisers, and 26% charge a performance-based fee. To steal from their clients, advisers must have custody of their cash or securities; approximately 35% do.

Our first set of results is reported in Table 2, where we present results from difference-in-differences regressions of the natural logarithm of assets under management on an adviser's exposure to Madoff victims. In this analysis, we measure Madoff exposure as the log of one plus the average number of Madoff victims in each of the ZIP codes where an adviser has an office. We also create a Madoff exposure indicator variable that is one if an RIA has an average of at least one victim across all office ZIP codes. Our panel includes annual observations during the period 2006–2010, with 2008 excluded, as discussed above. The dummy variable "Post" is an indicator for observations occurring after the Madoff fraud was uncovered in 2008, so it is activated for observations in 2009 and 2010.

To control for differences between ZIP codes that may be correlated with Madoff exposure, we include a number of demographic variables interacted with the post period indicator. These are measured as the average demographic statistic in the ZIP code or county around each of the advisers' offices. We include age, median income, and wealth of high-net-worth investors (calculated as the aggregate assets managed across all RIAs in each ZIP code), as well as the proportion of the population belonging to the affinity group. Similarly, we control for differences between RIAs by interacting various characteristics, such as the size of the adviser's business, with the post period indicator.

These controls are time-invariant, measured as of 2005. In addition, all regression specifications include adviser fixed effects, which absorb any time-invariant variables, so all variables are only included when interacted with the post period indicator. Finally, we include filing period or state–year fixed effects, where the state corresponds to the state in which an adviser maintains its main office and the filing period is described in Section 2. Standard errors are clustered to allow correlation within an adviser and filing period (in models 1 and 2) or by adviser and state-year (models 3–7).

Table 2 shows a consistent negative relation between exposure to Madoff victims and the growth rate of funds invested with RIAs. We begin with no control variables (aside from the adviser and filing period fixed effects), and then add controls. Coefficient estimates on Post$\times$Madoff exposure indicator and Post$\times$Madoff exposure are all negative and significant at the 1% level. The coefficient estimate in model 4 implies that RIAs in areas more exposed to the Madoff fraud experience abnormal reductions in AUM of about 6.7% after the fraud revelation. Using the same specification with the continuous version of Madoff exposure (Column 5) shows that a one-standard-deviation increase in the log number of victims leads to a decrease in AUM of $0.0582 \times 1.52 = 8.8\%$. This is economically large when compared with the median growth rate in AUM of 19% in our sample.

Madoff targeted members of the affinity group, so in model 6 we add controls for the average percentage of the population that is in the affinity group. Including this control has little effect on the Madoff exposure coefficient estimate. Coefficient estimates on control variables in Column 6 indicate that advisers with larger investment balances or fewer locations see reduced flows after 2008. Interestingly, the estimates show that general demographics around the office locations have very little effect on flows, with the exception of our high-net-worth variable (*Log(aggregate RIA AUM)*), which does a much better job of capturing the wealth of RIAs' clients than does average income.

Nonetheless, Madoff did not target victims randomly. He preyed on those with specific demographics. One way to control for this is to estimate the regressions within the subsample of RIAs that had above-normal exposure (Madoff exposure indicator = 1) using the level of exposure as the variable of interest. The results of this estimation are displayed in in Column 7. The coefficient estimate on Madoff exposure is very similar to other estimates and remains significantly negative. This helps to alleviate concerns that differences in demographics between areas with and without Madoff victims are driving the results.

Another way to deal with this issue is to use propensity score matching to generate two very similar groups of treated and control firms and to conduct the tests within this matched sample. We perform this analysis in panel B of the table. Column 1 shows the results of the probit regression predicting the Madoff exposure indicator. RIAs located in wealthier areas, or in areas with larger proportions of the population from the affinity group, are more likely to have Madoff victims nearby.[15] The matched sample is constructed using nearest-neighbor matching without replacement, and requiring that the propensity scores of the matched observations must be within 0.01 of each other.

Columns 2 through 7 present estimates for the matched sample using the same models as those estimated in Columns 1 through 6 of panel A. The estimates are consistent with those in panel A. Each estimate indicates that RIAs that were more exposed to the Madoff fraud saw greater withdrawals following the fraud revelation. In fact, the point estimates are slightly larger using the matched sample, then they are using the full sample.

Panel C of the table illustrates both the importance of matching and how well the matching performs. It shows that prior to matching, RIAs in Madoff-exposed areas are different from those in less-exposed areas: They are located in areas with older, wealthier individuals, and a greater proportion of population in the affinity group. Moreover, exposed RIAs tend to be larger and have more offices than the control group. However, after matching, nearly all of these observable differences are indistinguishable from zero.

We interpret our coefficient estimates on Madoff exposure as the effect on asset flows. However, as discussed in Section 2, changes in AUM are caused both flows and returns. If returns are systematically lower for RIAs in Madoff exposed areas than those in less exposed areas, then we would see similar coefficient estimates. To alleviate this concern, we conduct several additional tests. First, we estimate Equation (1) using the log number of discretionary client accounts as the dependent variable. Unlike assets, the number of accounts does not mechanically change with returns. Results are shown in Table A.3 of the Internet Appendix and are consistent with those in

[15]The results also show that in areas in which RIAs operate, conditional on other factors, the population in areas victimized by Madoff is actually younger. Unconditionally, however, areas victimized by Madoff are indeed older, consistent with anecdotal evidence. The conditional finding stems from the fact that RIAs operate in areas with older individuals and that age is correlated with factors such as wealth. When analogous regressions are estimated for the sample of bank branches, Madoff victimized areas are older, both conditionally and unconditionally (see Table 6).

Table 2. For both the full and matched samples, the reduction in number of client accounts of RIAs that are more exposed to the Madoff fraud is roughly 4% to 7% greater.[16]

Next, we test whether returns or risk-taking vary by Madoff exposure for a subsample of RIAs for which we can estimate returns. A small subsample of RIAs file quarterly 13F holding reports (roughly 225 per year). For this subsample Ng, Wu, and Yonker (2016) construct quarterly holding period returns and aggregate them to form annual equity portfolio returns. Ng, Wu, and Yonker (2016) also calculate the weight on equity as the total value of 13F holdings divided by the RIA's discretionary AUM reported in form ADV. Using these data, we test for differences in equity ratios and equity returns between RIAs that are more exposed or less exposed to the Madoff fraud for each year during the event period and 2008. Results are presented in Table A.4 of the Internet Appendix, and show no significant differences in any event year, suggesting that differences in returns are not likely driving the results in Table 2.[17]

Finally, we compare our estimates of interest using the methodology of Table 2 with those estimated using calculated flows. Again, this is only possible for the subset of RIAs that file quarterly 13F holding reports. Table A.5 in the Internet Appendix reports the results for three different models. We use the methodology of Table 2 and find that the coefficient estimates on the Madoff exposure indicator are all negative (between -0.035 and -0.43), but not statistically different from zero (t-statistics between -1.0 and -1.3) for this small subsample of RIAs. The coefficient estimates when using calculated flows as the dependent variable are similar, but slightly larger in magnitude at around -0.05. Moreover, these estimates are all significant at the 5% level. This provides evidence that the method of flow estimation used throughout this paper generates similar estimates as using calculated flows (at least for this subsample). However, the estimates tend to underestimate the true magnitude of the effect on flows and these estimates, not surprisingly, contain more noise. In

[16]The number of accounts is not the same as the number of clients. One client could have multiple accounts (e.g., an IRA and a Roth IRA). The number of clients is reported on Form ADV, but only in a coarse range, which makes evaluating changes in clients difficult. The number of accounts, however, is reported precisely. The correlation between the log number of accounts and the log number of clients is 0.57, and it is only 0.12 between the log number of accounts and the log AUM.

[17]We obtain similar results using factor loadings and alphas from a three-factor Fama-French model.

other words, when reasonable estimates of returns are available both the economic magnitude and the precision of the estimates of the impact of the Madoff fraud on asset flows increases.

Before focusing on clientele proximity-based measures of Madoff exposure we conduct one final robustness check. To help rule out the possibility that differences in ZIP-code-level economic conditions are driving the results, we add a control to the model estimated in Column 4 of Table 2 for the change in ZIP-code-level high-end home prices from February 2007 to December 2008.[18] The results are displayed in Table A.6 in the Internet Appendix. This control for the effects of the housing crisis is insignificant in the regression and including it has little effect on our coefficient of interest. Similarly, we include as an alternative measure of high net worth the percentage of households in the ZIP code with income over $200,000. This also does not change any of our inferences.

While our office-based measure for Madoff exposure may be good for RIAs whose clients are local, it may not be a good measure for advisers who have clients in many states. This suggests that a clientele-based measure of exposure may be more appropriate. We address this by creating the clientele-based exposure measure, using the number of victims in a state where an RIA has clients (see Section 2 for details). For this analysis, we include a similar set of control variables as those used in Table 2, although we must now aggregate these at the state-level rather than ZIP code or county level. In addition, since our measure is clientele-based, and not based on the location of the RIA, we include county-year fixed effects for the adviser's main office in some specifications, which effectively compares RIAs located in the same county, but with clients located in different states.

The regression results are reported in Table 3. Again, we see strong investment flows out of investment advisers whose clients are more exposed to fraud victims. Taking the standard deviation of "Avg. victims per 1,000 pop. in client states" of 0.05 from Table 1, the magnitude of the effect in model 4 is $-0.97 \times 0.05 = -4.9\%$. Alternative specifications in models 1–3 yield similar results.

[18]We use Zillow's Home Value Index (ZHVI) for top-tier residences by ZIP code, provided by the real estate data firm Zillow. This index uses houses with prices in the top one-third of the price distribution. Data are available at `http://www.zillow.com/research/data/`.

In models 5–7 we see additional evidence that the trust shock was transmitted within the affinity group via social connections or media. This can be seen in the fact that the effect of negative flows is actually restricted to states in which there are both more victims and higher populations of the affinity group. In each of the three specifications the interaction term between Madoff exposure and Pct. affinity group is significantly negative at better than the 5% significance level.

To get a sense of the total economic effect of the trust shock, we aggregate the estimated effect on each RIA as follows. First, for each RIA we multiply beginning assets by the number of victims per 1,000 population in client states, and then multiply the result by the coefficient estimated from model 4. We then sum across all RIAs to get the aggregate change in AUM, which gives an estimate of $363 billion. Using coefficient estimates from other models give results ranging from $318 billion to $430 billion. These estimates clearly dwarf the actual amount of wealth lost in the fraud.

3.2 Adviser characteristics

Gennaioli, Shleifer, and Vishny (2015) observe that many investment managers advertise their services based on trust, experience, and dependability, rather than just past performance. They model a money management industry in which money managers compete for investors not only on the basis of performance, but also of being trustworthy. This model predicts that investors would prefer to use a money manager whom they trust, enabling trusted managers to charge investors a higher fee without having them take their business to a less expensive competitor. The Form ADV data allow us to observe different aspects of RIAs investment advisory business including client type, fee structure, and whether they take custody of assets, or provide financial planning services. In this section, we examine how these RIA characteristics affect their susceptibility to the Madoff trust shock.

RIAs include hedge funds, private equity, real estate, and venture capital advisory firms, but also include what we might consider to be wealth managers. We broadly classify RIAs as either "wealth managers" or "private fund advisers" by defining private fund advisers as those RIAs that disclose that they advise a private fund in Form ADV in 2007. While many RIAs provide both wealth management services and advise private funds, this coarse definition captures two different types of firms, and defining the firms this particular way is more likely to capture "pure" wealth managers. Table A.7 in the Internet Appendix shows large differences in client composition, fee structure, and services provided between these two types of RIAs. Wealth managers have a much greater concentration of individual clients (79% vs. 46%), are much more likely to provide financial planning services (58% vs. 27%), and charge performance-based fees less frequently (6% vs. 57%). They are also much less likely to have custody of client assets (13% vs. 53%). The table also shows that BMIS looked much more like a private fund adviser than a wealth manager: the RIA disclosed few individual clients, did not provide financial planning services, and had custody of client assets.

In Table 4, we report results from regressions following those in model 2 in Table 3, but we add various RIA characteristics interacted with the post period dummy as well as the interaction of the post period dummy, the RIA characteristic, and the measure of Madoff exposure. The coefficient estimate on the RIA characteristic interacted with the post period estimates the abnormal change in AUM over the event period associated with that particular characteristic, while the triple interaction terms estimate the degree to which the characteristic mitigates or exacerbates the effects of the trust shock. For these regressions, we use the client-based measure of exposure.

We focus on characteristics such as whether the RIA provides financial planning services and whether the RIA takes custody of their clients' assets. RIAs that provide financial planning services are likely to build greater trust with their clients. Even if their clients lose trust in regulators, they may not withdrawal their money from their investment advisor if they trust the adviser. Custody, on the other hand, is what allows RIAs to move money into and out of their clients' accounts. In other words, without custody it is much more difficult to steal from clients. Therefore, having custody may exacerbate the Madoff trust shock.

Tests of these hypotheses are shown in Column 1 for the full sample of RIAs. The estimates suggest that RIAs who provide financial planning services saw fewer withdrawals in the post period. The coefficient estimate on Post interacted with Financial planning is 0.12 and significant at the 1% level. Moreover, providing financial planning services almost eliminates the effect of the Madoff fraud on asset outflows as seen by the significantly positive coefficient estimate of 0.93 (significant at the 5% level) on the triple interaction between Post, Madoff exposure, and Financial planning. The coefficient on the triple interaction between Post, Madoff exposure, and Custody is negative as hypothesized, but not statistically different from zero—perhaps because clients do not understand the importance of custody as it relates to fraud.

In Column 2, we test whether wealth managers or private fund advisers (like Madoff) saw greater trust-based withdrawals. The triple interaction between Post, Madoff exposure, and Private fund adviser is significantly negative (both economically and statistically), implying that private fund advisers saw greater trust based withdrawals than did wealth managers. In Columns 3 and 5 we confirm this by estimating the effect of the wealth shock for the subsamples of only wealth managers and private fund advisers, respectively.

Finally, in Columns 4 and 6, we repeat the analysis in Column 1 within the subsamples of wealth managers and private fund advisers. The results indicate that wealth managers who provided financial planning services were able to greatly reduce the adverse effects of the trust shock and that taking custody of assets did not significantly harm wealth managers. No significant effects are found within the set of private fund advisers.

3.3 RIA closures

We now investigate whether RIAs with clients who are more exposed to the trust shock are more likely to go out of business following the Madoff event. Table 5 displays the results of linear probability models predicting the probability that an RIA goes out of business in either 2009 or

2010. The sample is composed of all U.S.-based RIAs in existence in 2007, subject to the filters discussed in Section 1 with one exception: RIAs are not required to exist in 2009, since we are trying to predict whether they go out of business in 2009 or 2010.[19] Data on RIA closures come from Form ADV-W, as discussed earlier in Section 1.2. The dependent variable is an indicator variable that is one if the RIA withdraw from SEC registration due to business closure and the variable of interest is the clientele-based measure of Madoff exposure utilized in Tables 3 and 4.

The table shows that RIAs with clients who are more exposed to Madoff victims are indeed more likely to go out of business following the trust shock. The coefficient estimate for the full sample indicates that a one-standard-deviation increase in Madoff victim exposure increases the probability of going out of business by 0.012, which is a 44% increase in the unconditional probability of closure of 2.7% for RIAs from 2009 to 2010. When splitting the sample between wealth managers and private fund advisers, the primary effect is through the private fund advisers. These advisers had a higher probability of closure during the period at 4.5% and the estimated effect of the trust shock is also much larger. A one-standard-deviation increase in Madoff exposure leads to a 0.022 greater probability of closure, which means firms with greater exposure are about 50% more likely to close. Table A.8 in the Internet Appendix shows that these results continue to hold when estimated using probit regressions.

3.4 Bank deposits

Having shown that funds are withdrawn from investment advisers, we turn next to examine changes in deposits at banks following the Madoff fraud revelation. We use data from 2007 to 2010 to analyze the change in deposits around the trust shock.

[19]We exclude firms that directly invested with Madoff or were in some way related to Madoff. Appendix A provides the details. Nevertheless, it is worth noting that all three of the largest RIAs investing with Madoff subsequently withdrew from SEC registration: Ivy Asset Management, Fairfield Greenwich Advisers, and Maxam Capital Management, which in 2007 managed $14.3 billion, $2.2 billion, and $2.5 billion, respectively.

Summary statistics for the bank data are reported in panel B of Table 1. The median ZIP code has about $80 million in deposits, but there is large variation. Madoff victims are present in about 7% of ZIP codes. This is lower than the proportion for RIAs because bank branches are located in virtually every ZIP code in the country, but investment advisers locate only in high-wealth areas. It is therefore important to control for the presence of high-net-worth individuals, which we do using the natural log of all assets held in RIA accounts in the ZIP code (*Log(aggregate RIA AUM in branch ZIP)*).

Our regressions in this section follow the same structure as those for the RIA data, although we now use ZIP-code-year observations (measured on June 30th). Since we have more ZIP-code-level observations in this analysis, and since individuals are likely to bank at a branch near where they live,[20] we define the exposure to the trust shock for banks in a ZIP code simply as whether any victims live in the ZIP code, or the log of one plus the number of victims. Similar to the numerous fixed effects we include in the RIA regressions, our bank deposit regressions include fixed effects for the ZIP code and either state-year or county-year fixed effects.

Results for our first set of regressions are reported in Table 6, which follows the same general format as Table 2 with panel A showing the results for the full sample of ZIP codes, panel B showing the results for a matched sample of ZIP codes, and panel C showing the differences in characteristics between the treatment and control groups before and after matching. The main variable of interest is Madoff exposure indicator, which is a dummy variable for whether at least one victim of the Madoff fraud lives in the same ZIP code as the bank branch (models 1–4), or Madoff exposure, which is the log of one plus the number of victims in the ZIP code (models 5 and 6). We define the Post indicator variable to take a value of one in 2009 and 2010, and zero in other years.

In panel A, all specifications consistently show greater increases in cash deposits in ZIP codes that suffer the trust shock. In model 1, without controls, the estimate on the Post coefficient indicates that cash deposits increased by 9.5% in the period 2009–2010 across all ZIP codes. Remarkably, we

[20] Gilje (2014) shows that local bank deposits increase with local natural gas discoveries.

see an *additional* 10.0% increase if there is at least one fraud victim in the ZIP code. This coefficient estimate declines to 4.1% when estimating models 2–4, which include additional controls and either state-year or county-year fixed effects. However, all estimates remain statistically significant at better than the 1% level, with t-statistics greater than 3.5.[21] Similar to the RIA regressions above, the control variables interacted with the post period include average age and income in the ZIP code, total assets held at RIAs, percentage of the population that is in the affinity group, and the population in the ZIP code,[22] and cash deposits in 2006. The total wealth measure is a particularly important control in these regressions (t-statistics are around 10) because, in contrast to the RIA sample where investment advisers will tend to be located only in wealth areas, bank branches are located in most ZIP codes in the country.[23]

Using the dose of the treatment as our variable of interest, Madoff exposure, we find that banks in areas with more Madoff victims experienced greater increases in deposits. The coefficient estimate on Madoff exposure is 0.028 in model 5. In Column 6, we restrict the analysis to those ZIP codes where at least one victim of the Madoff fraud resides. Even within these ZIP codes, we continue to see that areas with more victims have a greater increase in cash deposits following the trust shock. And despite a much smaller sample (5,607 observations in 1,402 ZIP codes), the coefficient estimates remain strongly statistically significant. The point estimate of 0.036 indicates that a one-standard-deviation increase in the Madoff exposure (Log(1+num. victims) if Num. victims>0 from panel B of Table 1) is associated with a 3.0% increase in cash deposits.

We can use the estimates in the table to calculate the total economic effect analogously to the calculation we did above for the effect on RIA investments. Using estimates in models 4 and 5, the total increase in bank deposits due to the trust shock is approximately $97 billion. When

[21]Standard errors are clustered by ZIP code in models 1 and 2, by ZIP code and state-year in model 3, and by ZIP code and county-year in models 4 through 6.

[22]Rather than using population to scale the number of victims in the ZIP code, we include it as a control variable. There is not much variation in population between ZIP codes, but some ZIP codes have small populations that make it a poor scaling variable.

[23]As with the RIA results, we also report additional regression results in Table A.9 of the Internet Appendix with additional controls for home price changes and wealth. Our results remain qualitatively unchanged.

compared to our estimates of \$363 billion for the total withdrawals from RIAs, we can account for approximately 27% of the funds that were withdrawn from RIAs as a result of the trust shock. We cannot be certain how the remaining 73% is allocated, but a conservative assumption would be that investors allocated all of it to risky investments. To gauge the change in the allocation to risky assets, we must have an estimate of its value before the trust shock. Table A.4 shows that the average equity ratio in 2006 was about 65%. We do not know how the remaining 35% was allocated among bonds, real estate, cash, and other investments, but if we make the conservative assumption that half was allocated to cash, this would imply an increase in the cash portion of the portfolio from 17.5% to 27%, or a 35% increase. More realistic assumptions would indicate an even larger increase in cash holdings.

Panel B of Table 6 reports results of regressions using the same models as those estimated in panel A using a matched sample that is constructed analogously the methodology used in panel B of Table 2. Specifically, we run a probit regression with 2006 data to predict which ZIP codes will be treated, that is, have at least one victim. All coefficients are significant at the 1% level, and the pseudo-R^2 is 0.39. Our finding that bank deposits increase in treated ZIP codes is confirmed in all specifications.

Panel C shows the importance of matching for the bank branch deposits data. The largest concern for the full sample is the difference in high end wealth as measured by Log(aggregate RIA AUM) between the treatment and control groups. If Madoff afflicted areas are much wealthier then this could drive the differences in bank deposits. However, this concern is alleviated by matching since there are no statistically significant differences in observable characteristics in the matched sample.

Our measure of exposure has so far been based on victims within the ZIP code of each branch. We now investigate the effect of distance between branches and Madoff victims on changes in bank deposits. We use model 3 of Table 6, but also include indicator variables that indicate whether the closest Madoff victim is in the same ZIP code, within ten miles of the ZIP code, from 10 to 25 miles of the ZIP code, or from 25 to 50 miles of the zip code. The coefficient estimates on these indicators, interacted with the post period indicator, are plotted in Figure 4 along with their 95% confidence intervals. The figure shows that deposits abnormally increase with shorter distances from Madoff victims. Banks in ZIP codes with Madoff victims experienced abnormal deposit growth of over 5% following the Madoff event, while those with the nearest Madoff victim within 10 miles saw deposit growth of almost 3%. Banks with Madoff victims greater than 10 miles away did not experience abnormal deposit growth. The fact that the effect is confined to a relatively small area suggests that it is transmitted through social networks more than local media, which would be expected to have an effect over a larger area.

3.5 Placebo tests

In this section, we conduct placebo tests to investigate whether investors in areas that were exposed to the Madoff fraud in December 2008 behaved differently from investors less exposed during other time periods. If, for example, the differential response of Madoff exposed areas in 2009 was due to the market downturn in 2008, and not the revelation of the Madoff fraud, then we might expect to see similar coefficient estimates on the Madoff exposure measures following other market declines, such as the bursting of the technology bubble that began in 2000. If we were to find that investments of trust-shocked areas were disproportionately transferred from risky to less-risky assets during periods such as this, then we would conclude Madoff trust shock per se is not the primary reason of the results we have documented.

To test this, we estimate the effect of Madoff exposure on one year changes in RIA flows and bank branch deposits for each one year horizon by estimating a series of cross-sectional regressions.

Specifically, for each year $t = 2002$ to 2012 we estimate:[24]

$$\Delta\log(\text{AUM})_{i,s,t} = \delta_s + \beta_M \text{Madoff exposure}_i + \sum_m (\beta_{C,m}\text{Control}_{i,m}) + \epsilon_{i,t}, \tag{2}$$

where $\Delta\log(\text{AUM})_{i,s,t}$ is the change in the natural logarithm of AUM for adviser i operating in state s from year $t-1$ to year t, δ_s is an adviser state fixed effect, Madoff Exposure$_i$ is the number of victims per 1,000 population in the states in which adviser i operates, and Control$_{i,m}$ are controls following those used in model 3 of Table 3.

Figure 5 plots the estimates of β_M and their 95% confidence intervals (based on heteroscedasticity-consistent standard errors). Consistent with the Madoff exposure capturing a shock to trust, the coefficient estimate on Madoff exposure is only significantly negative following the revelation of the Madoff fraud in 2009. Moreover the graph indicates the effect of the shock was persistent. As the market recovered following the financial crisis there is no evidence that RIAs with greater exposure to the Madoff fraud experienced abnormal inflows coming back to their advisory businesses.

We repeat the exercise using the bank deposit data by estimating a series of regressions analogous to Equation 2. In these regressions the dependent variable is Δlog(deposits), observations are at the ZIP code level, Madoff exposure is the natural log of one plus the number of victims in the branch ZIP code, controls and fixed effects follow those used in model 4 of Table 6, and the sample includes the matched sample of ZIP codes used in the estimation in panel B of Table 6 from 2000 to 2012. Figure 6 plots the coefficient estimates on Madoff exposure for these regressions. Just like in the previous figure, this figure shows that only after the Madoff fraud did Madoff exposed areas experience abnormal investment behavior. Deposits abnormally increase between June 30, 2008 and June 30, 2009, and continue to do so for the next two years. Additionally, we see no evidence that these abnormal investment behavior reverts.

[24]The RIA data begin in 2001.

Taken together, these placebo tests effectively rule out an alternative explanation for our results: that unobserved heterogeneity causes people in Madoff afflicted areas to respond differently to differing economic conditions, and that this response had nothing to do with Madoff. Clearly this is not the case.

3.6 Instrumental variables regressions

We have thus far addressed endogeneity due to omitted variables using numerous fixed effects specifications. As a final test, we use an instrumental variables approach and verify that our results remain unchanged.

Our main instrument for the number of victims in a ZIP code is simply the average percentage of the county population that is in the affinity group in counties where the RIA has offices (or the bank branch is located), interacted with the post period. Areas with more members of the affinity group are more likely to have victims, making this a good candidate for an instrument. The exclusion restriction requires that the variable also not be related to either RIA flows or bank deposits other than through its effect on the number of Madoff victims. Our placebo analysis suggests members of the affinity group did not behave differently during any year other than following the Madoff fraud. Moreover, the coefficient estimates on the proportion of the population belonging to the affinity group in both Tables 2 and 6 are insignificant. While this evidence helps, it does not necessarily rule out other potential channels through which the affinity group can be related to RIA flows and bank deposits. For example, some of the RIAs may exclusively target the affinity group to grow their AUM. For this reason, in addition to several demographic variables, such as age, income, and size of the community, we include RIA fixed effects in our specification to capture time invariant RIA-specific unobservable factors. We surmise that, after controlling for demographic characteristics and RIA fixed effects, the exclusion restriction is satisfied.

We report results from this IV approach in Table 7. First-stage regressions in Columns (1) and (3) confirm that the instrument is highly correlated with exposure to the trust shock: coefficient estimates on Post $\times$ Pct. affinity group are highly significant ($t > 10$). Consistent with Madoff targeting certain demographics, variables such as wealth, age, and income are important in identifying which areas will have more victims, especially in the bank sample since it is much more geographically heterogeneous than the RIA sample.

Results from the second stage regressions are reported in Column (2) for RIA assets and Column (4) for bank deposits. In both cases, estimates are similar to what we found in the earlier tests, although somewhat larger in magnitude.

4 Conclusion

Using events surrounding the Madoff scandal, we have presented evidence of the importance of trust in the investment advisory industry. We show that a shock to trust led investors to move money from risky to low-risk assets and that this behavior increased the rate of RIA closures in affected areas. In conjunction with previous research that has used cross-sectional variation in measures of trust to identify its effect on a variety of economic behavior outcomes, our results support the view that investor perceptions are important for resource allocation in the economy.

To conclude, we highlight some important considerations in interpreting our results. First, national media coverage of the Madoff fraud may have affected investors, in general, meaning that some of the "untreated" areas in our analysis were actually affected by the trust shock. Since our results are estimated from difference-in-differences regressions, this means that our estimates—despite being quite large in magnitude—may underestimate the true size of the effect.

Second, while our paper relies on identification from one particular ethnic group that comprises the affinity group for the Madoff fraud, the fact that a shock to trust can be transmitted through social networks is likely to apply to any group. The implications for investment behavior, in general, are therefore likely broad.

Third, we have framed our results in terms of a shock to trust, but it is also possible that investors update their beliefs about the distribution of returns in the aftermath of the Madoff fraud. In particular, investors may reevaluate the probability of extreme negative returns due to theft. Such a return may be particularly "salient" in the sense of Bordalo, Gennaioli, and Shleifer (2012) because it is quite different from the rest of the return distribution. To some extent, this distinction may not be economically important. Risk is typically defined in terms of the payoffs from an asset, and including the probability of theft as part of this distribution or excluding it does not change the main implication of our findings: that trust has a large and persistent effect on investor behavior.

Appendix A

SEC Form ADV is also known as the Uniform Application for Investment Adviser Registration. Investment advisers are regulated under the Investment Advisers Act of 1940 and, as a result, must file this form at initial registration, annually in the form of an annual update, and any time there is a material change to their advisory business. An investment adviser is defined as “any person or firm that, for compensation, is engaged in the business of providing advice to others or issuing reports or analyses regarding securities.”[25] In constructing our sample, we exclude financial planners and investment consultants by requiring RIAs to report having discretionary AUM.

To construct the sample, we keep only the “annual updating amendments” filings. Some firms submit numerous updates throughout the year, but only the annual amendment requires firms to update their AUM, and these filings must be made within 90 days of the adviser’s fiscal year-end. We use these filings to construct our panel data set. The period from 2000 to 2014 has 343,691 filings, of which 37.4% are “annual updating amendments.” A few firms submit more than one annual update for a given fiscal year, in which case we keep the one that was submitted first.

We remove stale filings (those of firms whose AUM do not change from one fiscal year to the next) and also those with missing AUM. During the sample period, advisers were required to register with the SEC if they managed over $25 million. Many RIAs have very few assets under management, so we exclude these small firms by requiring advisers to have AUM of at least $50 million in 2005. Mutual fund advisers and subadvisers are purged from the sample. We also require advisers to be based in the United States, to exist in 2006, and to survive until at least 2009. We remove outliers from the sample by removing firms that achieved astronomical growth in any year (the 99th percentile of firm maximum growth). An alternative way of dealing with this is to winsorize the data, but doing so on AUM does not make sense in this setting since firms with the largest assets have their AUM set to some maximum percentile for the entire study period, which would give zero AUM growth throughout the study.

To identify RIAs that invested with BMIS, we use the “Disclosure Reporting Pages” of ADV filings, which requires advisers to report any litigation against them. We also exclude the RIAs of all feeder funds reported by the *Wall Street Journal* to have invested with Madoff.[26]

To calculate investment flows into and out of investment advisers, we use item 5F of Form ADV, which reports assets under management. Because changes in assets under management is a function of both asset flows and investment returns, in most specifications we include controls to remove the effect of investment returns. In particular, we include fixed effects for the filing period to which an adviser’s ADV report applies. These fixed effects are determined by the month and year of both the current and previous ADV filings, so they capture the effect of the average investment return on changes in AUM during the period covered by a filing.

Closures of investment advisers are identified using data from Form ADV-W, which RIAs must file when withdrawing from registration and specify their reason for withdrawal. Firms can withdraw either partially or fully, but since we are interested in firm closures, we only keep full withdrawals from registration. Firms may withdraw from registration for many reasons, but the most common reasons include going out of business, mergers, and relying on an exemption to deregister. We obtained all ADV-W filings from 2001 to 2014 from the SEC through an additional FOIA request.

[25] See p. 2 of “Regulation of Investment Advisers by the U.S. Securities and Exchange Commission” available at `http://www.sec.gov/about/offices/oia/oia_investman/rplaze-042012.pdf`

[26] The list of victims is available online at `http://s.wsj.net/public/resources/documents/st_madoff_victims_20081215.html`

We identify RIAs that went out of business or discontinued their advisory business in 2009 or 2010 by manually examining the stated reason for withdrawal. Almost half of all full withdrawals fall into this category (2.8% overall). The most common withdrawal reasons cited are simply "No longer in business" or "closing business," but other examples include "Winding up investment adviser due to bankruptcy" and "Not enough business."

Three important disclosures are used to capture the exposure of the firm's clientele to Madoff victims. The first is the location of the advisory firm's main office, which is disclosed in item 1F. In Schedule D firms must also disclose the locations of their five largest offices (by number of employees) where their advisory business is conducted. (Many firms choose to disclose more than five.) Finally, in item 2C advisers must provide a list of any states in which they have at least five clients. The SEC then sends a "notice filing" to the securities regulator in each state, except for Wyoming, which has no such regulator.

Bibliography

Alesina, A., and E. La Ferrara. 2002. Who trusts others? *Journal of Public Economics* 85:207–34.

Arrow, K. J. 1969. The organization of economic activity: Issues pertinent to the choice of market versus nonmarket allocation. The analysis and evaluation of public expenditure: The PPB system; a compendium of papers submitted to the Subcommittee on Economy in Government of the Joint Economic Committee, Congress of the United States. 91st Cong, 1st sess.

Bloom, N., R. Sadun, and J. Van Reenen. 2012. The organization of firms across countries. *Quarterly Journal of Economics* 127:1663–1705.

Bollen, N. P., and V. K. Pool. 2012. Suspicious patterns in hedge fund returns and the risk of fraud. *Review of Financial Studies* 25:2673–702.

Bordalo, P., N. Gennaioli, and A. Shleifer. 2012. Salience theory of choice under risk. *Quarterly Journal of Economics* 127:1243–85.

Brown, S., W. Goetzmann, B. Liang, and C. Schwarz. 2008. Mandatory disclosure and operational risk: Evidence from hedge fund registration. *Journal of Finance* 63:2785–815.

———. 2009. Estimating operational risk for hedge funds: The ω-score. *Financial Analysts Journal* 65:43–53.

———. 2012. Trust and delegation. *Journal of Financial Economics* 103:221–34.

Brunnermeier, M. K., and S. Nagel. 2004. Hedge funds and the technology bubble. *Journal of Finance* 59:2013–40.

Burke, J., and A. A. Hung. 2016. Trust and financial advice. Working Paper.

Choi, S., and M. Kahan. 2007. The Market penalty for mutual fund scandals. *Boston University Law Review* 87:1021–57.

Clifford, C. P., J. A. Ellis, and W. C. Gerken. 2016. Hedge fund boards and the market for independent directors. Working Paper.

D'Acunto, F., M. Prokopczuk, and M. Weber. 2015. The long shadow of Jewish persecution on financial decisions. Working Paper.

Deason, S., S. Rajgopal, and G. B. Waymire. 2015. Who gets swindled in Ponzi schemes? Working Paper.

Dimmock, S. G., and W. C. Gerken. 2012. Predicting fraud by investment managers. *Journal of Financial Economics* 105:153–73.

Fukuyama, F. 1995. *Trust: The social virtues and the creation of prosperity.* New York: Free Press.

Gennaioli, N., A. Shleifer, and R. Vishny. 2015. Money doctors. *Journal of Finance* 70:91–114.

Georgarakos, D., and R. Inderst. 2014. Financial advice and stock market participation. Working Paper.

Giannetti, M., and T. Y. Wang. 2016. Corporate scandals and household stock market participation. *Journal of Finance* 6:2591–636.

Gilje, E. 2014. Does local access to finance matter? Evidence from U.S. oil and natural gas shale booms. Working Paper.

Gormley, T. A., and D. A. Matsa. 2014. Common errors: How to (and not to) control for unobserved heterogeneity. *Review of Financial Studies* 27:617–61.

Gregoriou, G. N., and F. Lhabitant. 2009. Madoff: A riot of red flags. Working Paper.

Guiso, L. 2010. A trust-driven financial crisis. Implications for the future of financial markets. Working Paper.

Guiso, L., P. Sapienza, and L. Zingales. 2004. The role of social capital in financial development. *American Economic Review* 94:526–56.

———. 2008. Trusting the stock market. *Journal of Finance* 63:2557–600.

———. 2009. Cultural biases in economic exchange? *Quarterly Journal of Economics* 124:1095–131.

Hackethal, A., R. Inderst, and S. Meyer. 2012. Trading on advice. Working Paper.

Hong, H., J. D. Kubik, and J. C. Stein. 2005. Thy neighbor's portfolio: Word-of-mouth effects in the holdings and trades of money managers. *Journal of Finance* 60:2801–24.

Hung, A. A., N. Clancy, J. Dominitz, E. Talley, and C. Berrebi. 2008. *Investor and industry perspectives on investment advisers and broker-dealers.* Santa Monica, CA: RAND Corporation.

Ivković, Z., and S. Weisbenner. 2007. Information diffusion effects in individual investors' common stock purchases: Covet thy neighbors' investment choices. *Review of Financial Studies* 20:1327–57.

Knack, S., and P. Keefer. 1997. Does social capital have an economic payoff? A cross-country investigation. *Quarterly Journal of Economics* 112:1251–88.

Kostovetsky, L. 2016. Whom do you trust?: Investor-advisor relationships and mutual fund flows. *Review of Financial Studies* 29:898–936.

La Porta, R., F. Lopez-de Silanes, A. Shleifer, and R. W. Vishny. 1997. Trust in large organizations. *American Economic Review* 87:333–8.

Malmendier, U., and S. Nagel. 2011. Depression babies: Do macroeconomic experiences affect risk taking? *Quarterly Journal of Economics* 126:373–416.

Markopolos, H. 2010. *No one would listen: A true financial thriller.* Hoboken, NJ: John Wiley & Sons.

Mian, A., and A. Sufi. 2009. The consequences of mortgage credit expansion: Evidence from the U.S. mortgage default crisis. *Quarterly Journal of Economics* 124:1449–96.

Ng, D., K. Wu, and S. Yonker. 2016. Wealth managers. Working Paper.

Nguyen, H.-L. Q. 2014. Do bank branches still matter? The effect of closings on local economic outcomes. Working Paper.

Pool, V. K., N. Stoffman, and S. E. Yonker. 2015. The people in your neighborhood: Social interactions and mutual fund portfolios. *Journal of Finance* 70:2679–732.

Putnam, R. D., R. Leonardi, and R. Y. Nanetti. 1994. *Making democracy work: Civic traditions in modern Italy.* Princeton: Princeton University Press.

Sander, P. J. 2009. *Madoff: Corruption, deceit, and the making of the world's most notorious Ponzi scheme.* Guilford, CT: Lyons Press.

Sapienza, P., and L. Zingales. 2012. A trust crisis. *International Review of Finance* 12:123–31.

Zak, P. J., and S. Knack. 2001. Trust and growth. *Economic Journal* 111:295–321.

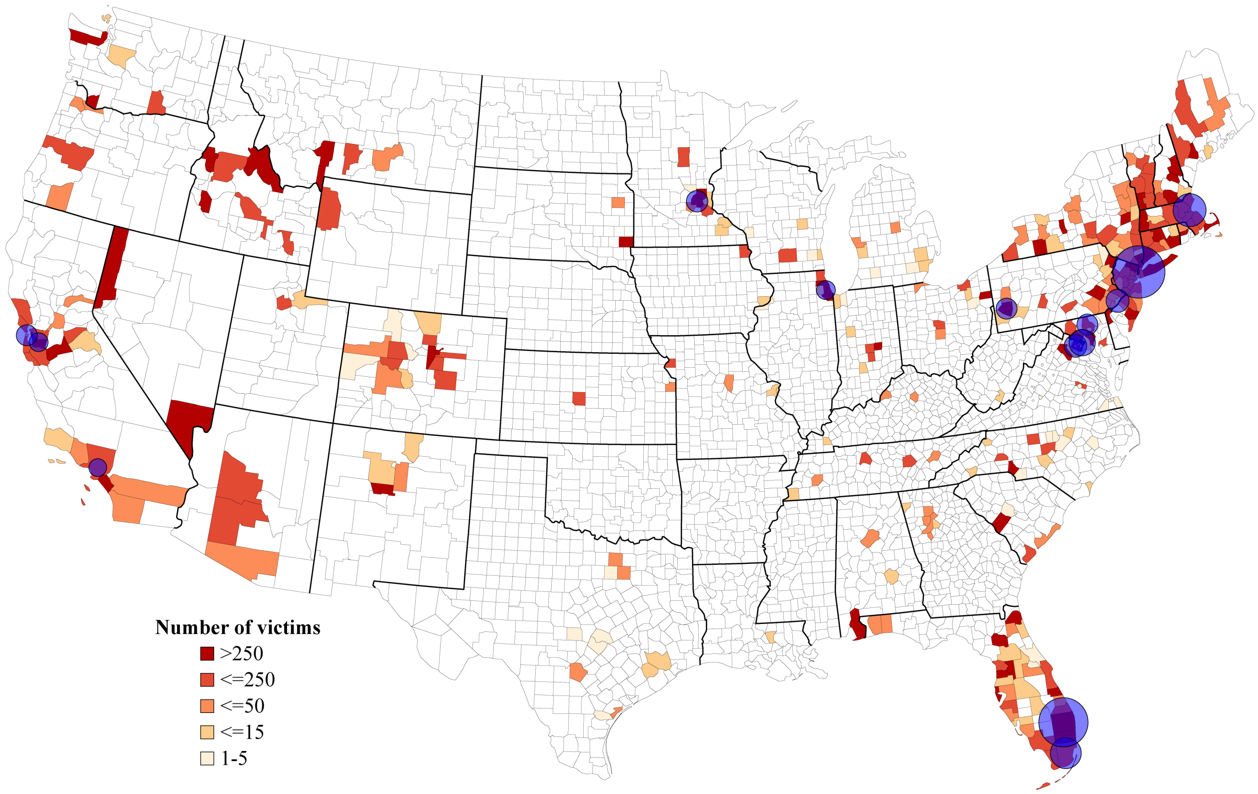

Figure 1: Dispersion of Madoff victims

The map shows the number of victims of the Madoff fraud by county. We count victims as the number of unique names on the list of victims supplied to the court. In some cases, the address corresponds to that of an investment advisory or accounting firm, in which case the victim may reside elsewhere. Counting victims by number of unique addresses—and therefore putting less weight on locations of firms representing victims—provides a very similar picture of the distribution of victims. Cities with high levels of Google searching for the term “Madoff” during 2009 are represented by circles; larger circles indicate more intense search interest. At the state level, the rank correlation between the Google search index and the number of victims is 0.77.

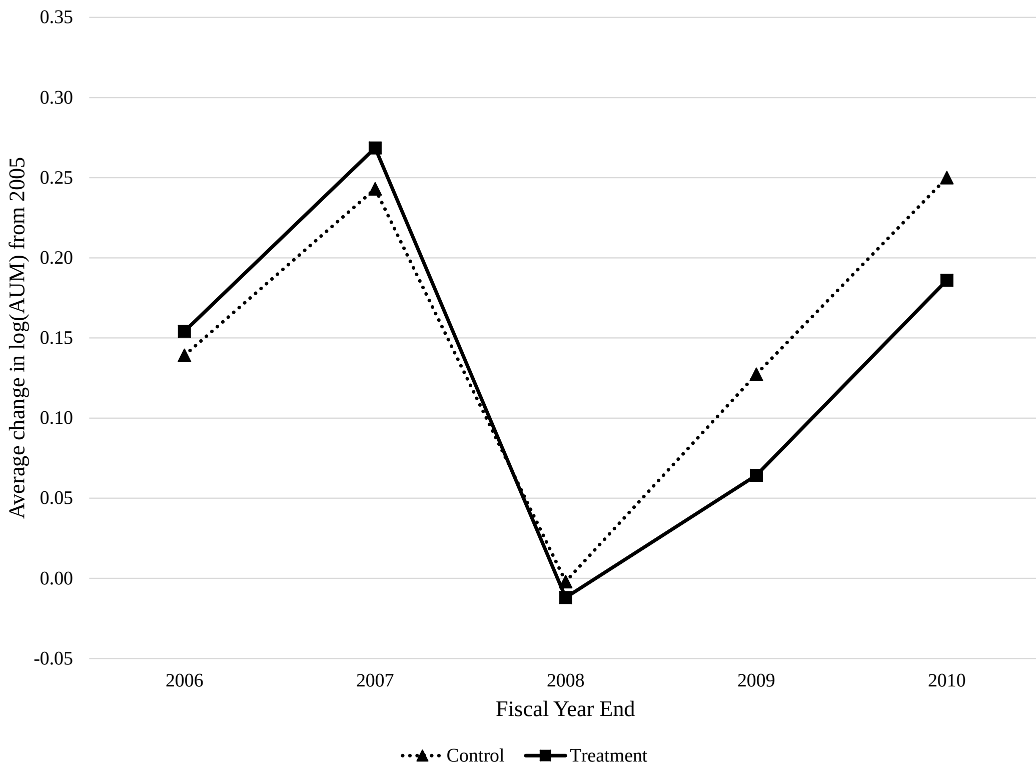

Figure 2: Average cumulative change in RIA log(AUM) by Madoff fraud exposure

The figure displays the average change in the natural log of RIA-level AUM since 2005 for two groups of RIAs based on their exposure to the Madoff fraud. RIAs that are more exposed to the Madoff fraud are those that averaged at least one victim in the ZIP codes where they have offices. This group is labeled the "treatment group." All other RIAs are considered less exposed. This group is labeled the "control group."

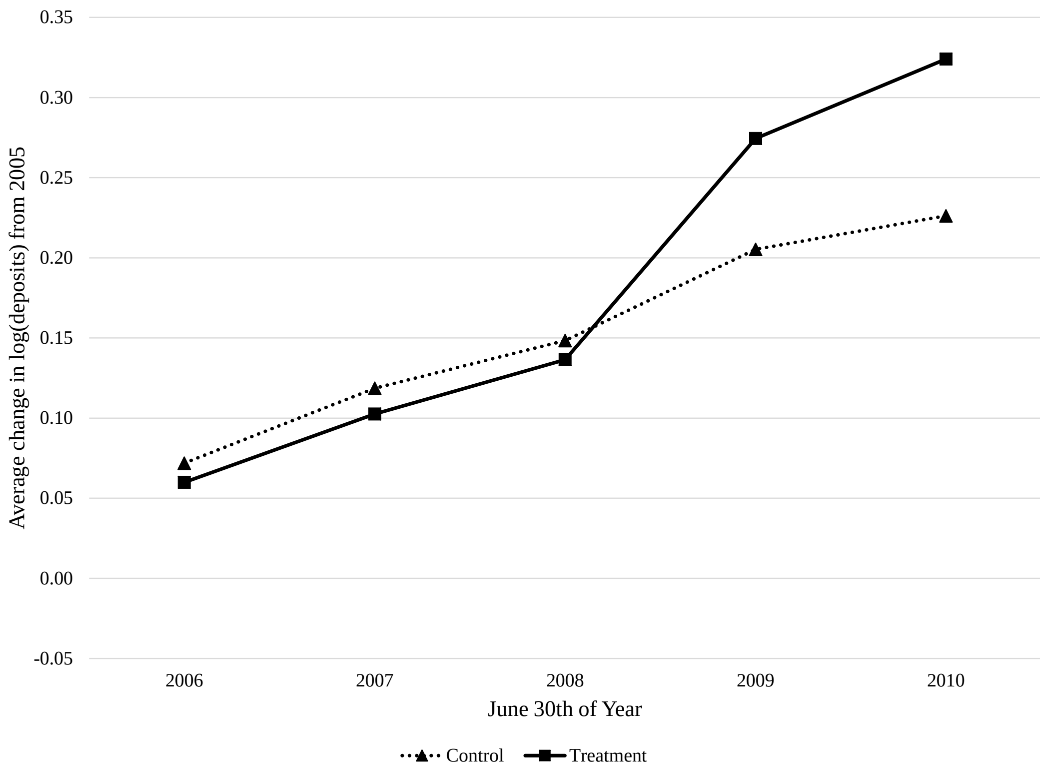

Figure 3: Average cumulative change in log(bank deposits) by Madoff fraud exposure

The figure displays the average change in the natural log of ZIP-code-level aggregate bank branches deposits since 2005 for two groups of bank branches based on their exposure to the Madoff fraud. Bank branches whose clients were more exposed to the Madoff fraud are those with at least one Madoff victim in the ZIP code of the bank branch. This group is labeled the "treatment group." Bank branches with no Madoff victims in their ZIP codes are considered less exposed and is labeled the "control group."

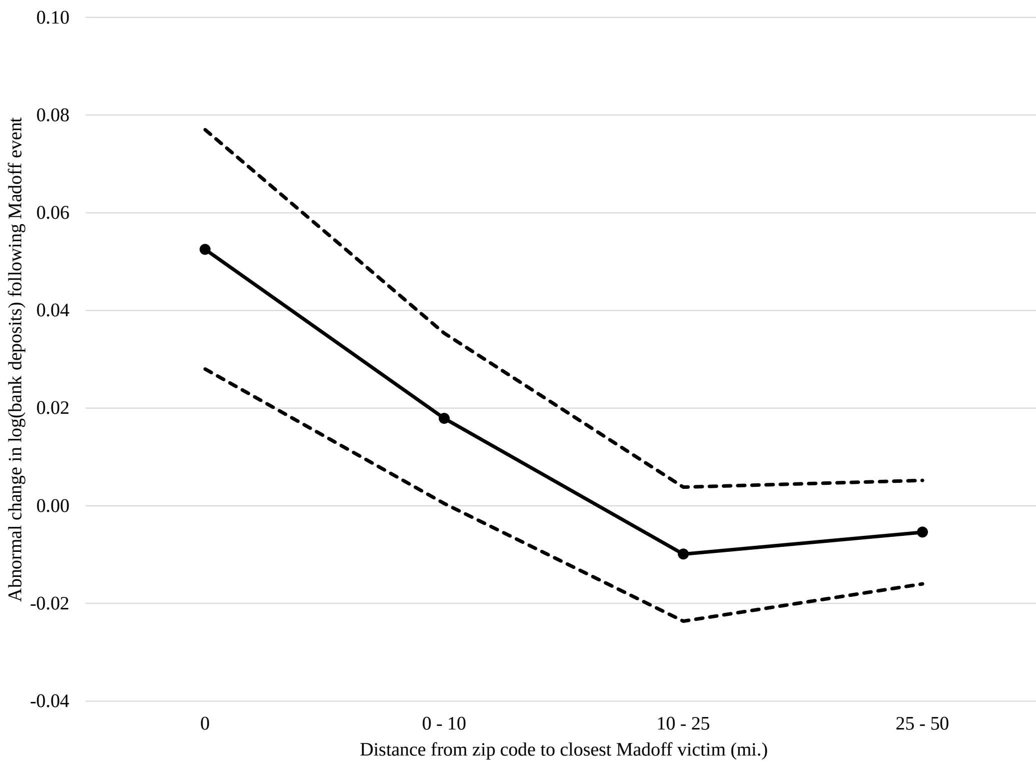

Figure 4: Change in bank deposits and distance from Madoff victims

The figure displays estimates of distance from Madoff victims on changes in bank deposits around the Madoff event. Estimates and standard errors are estimated using the model estimated in Column 3 of Table 6, which includes ZIP code and state-year fixed effects and is estimated using data from 2007 to 2010. Also included in the model are indicator variables that indicate the closest victim to the ZIP code. Specifically, variables that indicate whether the closest Madoff victim is within 10 miles of the ZIP code, from 10 to 25 miles of the ZIP code, or from 25 to 50 miles of the ZIP code. The coefficient estimates on these indicators interacted with the post period indicator are plotted along with their 95% confidence intervals (based on heteroscedasticity-consistent standard errors clustered by ZIP code and state-year).

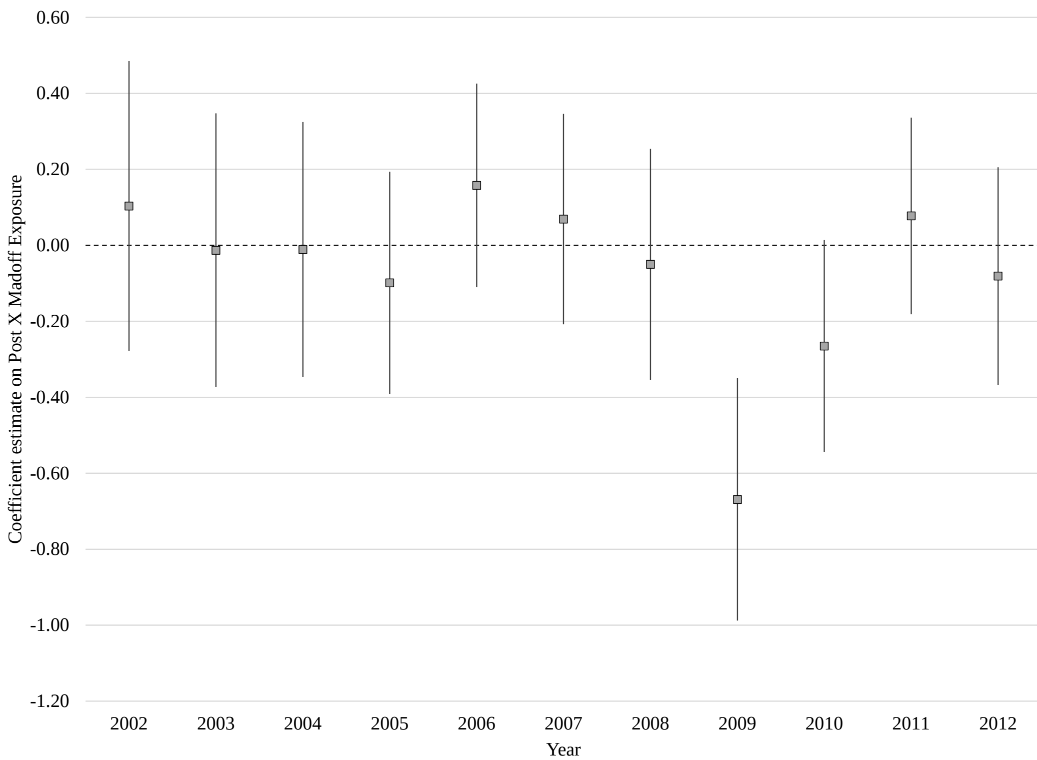

Figure 5: Effect of Madoff exposure on AUM year by year

The figure displays coefficient estimates measuring the difference in one year changes in RIA log (AUM) attributed to RIA exposure to the Madoff fraud. Estimates are for eleven separate cross-sectional regressions for the years 2002 to 2012. Specifically, plotted are the estimates of β_M and their 95% confidence interval (based on standard errors clustered by state) from the following adviser-level regression for each year, where the year indicated in the figure on the x-axis is end of year t:

$$\Delta \log(\text{AUM})_{i,s,t} = \delta_s + \beta_M \text{Madoff exposure}_i + \sum_m (\beta_{C,m} \text{Control}_{i,m}) + \epsilon_{i,t},$$

where $\Delta \log(\text{AUM})_{i,s,t}$ is the change in the natural logarithm of AUM for adviser i operating in state s from year $t-1$ to year t, δ_s is an adviser state fixed effect, Madoff Exposure$_i$ is the number of victims per 1,000 population in the states in which adviser i operates, and Control$_{i,m}$ are controls following those used in model 3 of Table 3. The sample includes all U.S.-based, SEC-registered money managers following the procedure outlined in Cection 1.2 from 2001 to 2012. $\Delta \log(\text{AUM})_{i,s,t}$ is winsorized at the 1% level in both tails.

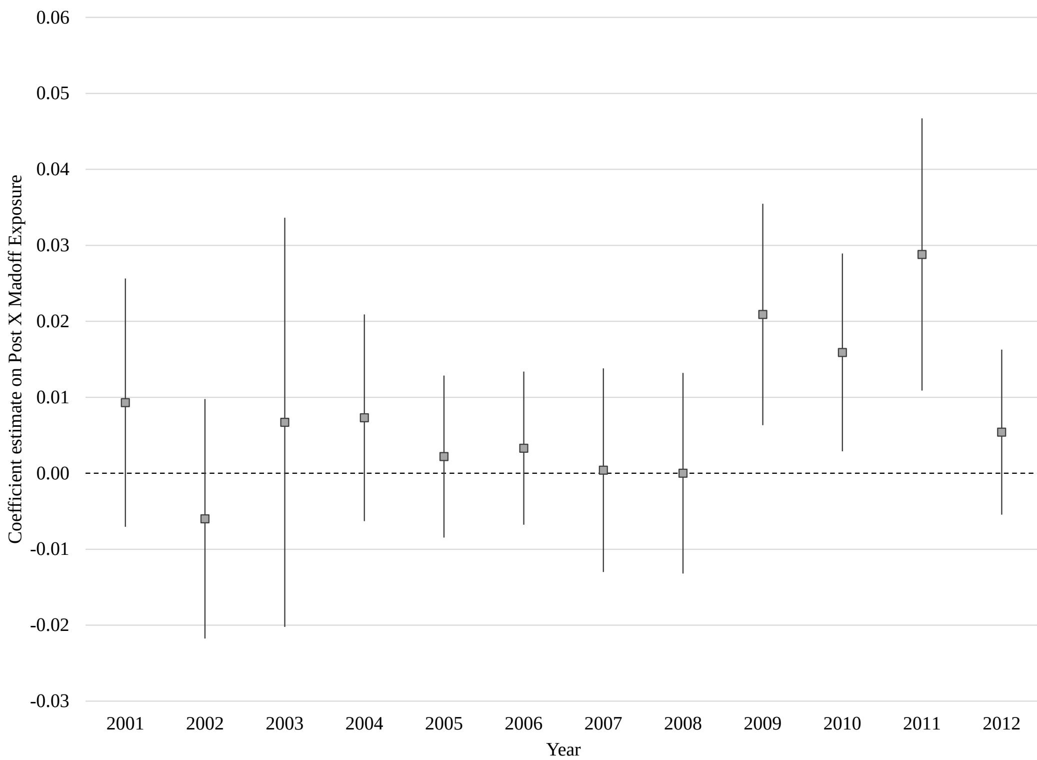

Figure 6: Effect of Madoff exposure on bank deposits year by year

The figure displays coefficient estimates measuring the difference in one year changes in the natural logarithm of ZIP-code-level bank branch deposits attributed to branch exposure to the Madoff fraud. Estimates are for 12 separate cross-sectional regressions for the years 2001 to 2012. Specifically, plotted are the estimates of β_M and their 95% confidence interval (based on standard errors clustered by county) from the following ZIP-code-level regression for each year where the year indicated in the figure on the x-axis is as of June 30th of year t:

$$\Delta \log(\text{deposits})_{i,s,t} = \delta_s + \beta_M \text{Madoff exposure}_i + \sum_m (\beta_{C,m} \text{Control}_{i,m}) + \epsilon_{i,t},$$

where $\Delta \log(\text{deposits})_{i,s,t}$ is the change in the natural logarithm of deposits in bank branches in ZIP code i operating in county s from year $t-1$ to year t, δ_s is a county fixed effect, and Madoff Exposure$_i$ is the natural log of the number of victims in ZIP code i, and Control$_{i,m}$ are controls following those used in model 4 of Table 6. The sample includes the matched sample of ZIP codes used in the estimation in panel B of Table 6 from 2000 to 2012. $\Delta \log(\text{deposits})_{i,s,t}$ is winsorized at the 1% level in both tails.

Table 1: Summary statistics

The table displays summary statistics for the investment adviser data (panel A) and the ZIP-code-level aggregate bank branch data (panel B) used in the study. Summary statistics include data from the years 2006 to 2010, excluding 2008. Bank branch deposit data are reported as of June 30th of each year and are from the FDIC Summary of Deposits database. Investment advisor data are from annual updating amendments to SEC form ADV, which are reported within 90 days of advisers' fiscal year-ends. Data on SEC registration withdrawals and RIA closures are from SEC form ADV-W. Summary statistics for additional variables are reported in Table A.1 of the Online Appendix.

	Mean	Median	SD	N
A. Investment adviser				
AUM ($millions)	1,822.991	225.583	12,577.820	15,512
Log(AUM)	5.740	5.423	1.467	15,512
Number of domestic offices	1.857	1.000	7.678	15,512
Number of client states	6.767	2.500	11.141	15,512
Number of Madoff victims in ZIP code	17.188	1.000	54.948	15,512
Avg. at least one victim in office ZIP	0.524	1.000	0.499	15,512
Log(1 + num. victims)	1.178	0.693	1.516	15,512
Log(1+ num. victims) if Num. victims > 0	2.048	1.609	1.488	8,921
Avg. victims per 1,000 pop. in client states	0.046	0.034	0.051	14,723
Avg. pct. affinity group in office counties	5.155	3.098	5.800	15,508
Log(aggregate RIA AUM in main office ZIP)	24.166	23.769	2.842	15,504
Avg. pct. affinity group in client states	2.413	2.077	1.700	14,723
Pct. clients individuals (not high net worth)	28.883	18.000	29.838	15,512
Pct. clients high net worth individuals	39.038	38.000	31.583	15,512
Provide financial planning services	0.474	0.000	0.499	15,512
Advise a private fund	0.235	0.000	0.424	15,512
Compensated by performance-based fee	0.261	0.000	0.439	15,512
Custody of cash or securities	0.345	0.000	0.475	15,512
2007 RIA filed ADV-W between 2009 and 2010	0.064	0.000	0.244	4,100
2007 RIA closure between 2009 and 2010	0.028	0.000	0.165	4,100
				B. Bank branch sample
Deposits ($thousands)	312,854.600	79,509.990	2,344,202.000	81,194
Log(deposits)	11.301	11.284	1.566	81,194
Number of Madoff victims in ZIP code	0.454	0.000	7.172	81,748
At least one victim in ZIP	0.071	0.000	0.256	81,748
Log(1 + num. victims)	0.096	0.000	0.414	81,748
Log(1 + num. victims) if Num. victims > 0	1.357	1.099	0.841	5,784
Pct. affinity group in county	1.272	0.092	2.821	81,743
Log(aggregate RIA AUM in branch ZIP)	2.843	0.000	7.261	81,748

Table 2: RIA flows and adviser proximity to Madoff victims

The table displays regression results of difference-in-differences regressions estimating the impact of exposure to the Madoff fraud on RIA asset flows using the sample of U.S.-based SEC registered investment advisers (RIAs), excluding mutual fund advisers, for the years 2006, 2007, 2009, and 2010, outlined in Section 1.2 of the text. Variations of the following regression equation are estimated:

$$\log(\text{AUM})_{i,j,t} = \alpha_i + \gamma_{j,t} + \beta_M \text{Post}_t \times \text{Madoff exposure}_i + \sum_m (\beta_{C,m}\text{Post} \times \text{Control}_{i,m}) + \epsilon_{i,t},$$

where $\log(\text{AUM})_{i,j,t}$ is the natural log of the AUM of RIA i, headquartered in state j, at the fiscal year-end of year t. α_i is an RIA fixed effect. $\gamma_{j,t}$ is a state-year fixed effect. These fixed effects are included in models 3 through 8, but models 1 and 2 instead include filing period fixed effects, which are based on pairings between the month and year of the current and previous SEC filings. Post_t is a dummy variable for the years following the December 2008 event (2009 and greater). Madoff exposure_i is measured as the natural log of one plus the average number of Madoff victims in the ZIP codes in which the adviser has offices. The Madoff exposure indicator is a dummy variable that is one if the number of Madoff victims in the RIA's office ZIP codes averages at least one. Control variables interacted with the post period include the natural log of the RIA's beginning assets under management (measured in 2005), the natural log of the number of RIA offices, the average age of the populations in ZIP codes where the RIA has offices, the log of the average of the median income of the populations in ZIP codes where the RIA has offices, log of aggregate RIA AUM in the ZIP code of the RIA's main office, and the average percentage of the population belonging to the affinity group in counties where the RIA has offices. In panel A the analysis is conducted using the full sample of RIAs in Columns 1 through 6, but is limited to only advisors with Madoff exposure indicator = 1 in Column 7. Standard errors are clustered by adviser and filing period in models 1 and 2 and by adviser and state-year in the remaining models. The analysis in panel B uses a propensity score matched sample matched using cross-sectional data from 2006 to estimate the model displayed in Column 1 of panel B. Matched RIAs are then included over the sample period. The control group is matched to "treated" observations (those with Madoff exposure indicator = 1) using nearest neighbor without replacement matching, where the absolute difference in propensity scores between the matched observations is less than or equal to 0.01. Control variables in panel B follow those indicated of the corresponding column in panel A, but their coefficient estimates and standard errors are not reported for brevity. Panel C shows the means of various characteristics for the treatment (Madoff exposure indicator = 1) and control samples and their differences for the full sample and for the matched sample used in panels A and B, respectively. Cross-sectional tests of differences are based on heteroscedastic-consistent standard errors. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

A. Full sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post ×							
Madoff exposure indicator	-0.1177^a	-0.0853^a	-0.0645^a	-0.0672^a			
	(0.0201)	(0.0192)	(0.0198)	(0.0209)			
Madoff exposure					-0.0582^a	-0.0567^a	-0.0598^a
					(0.0118)	(0.0134)	(0.0202)
Log(beg. AUM)		-0.0712^a	-0.0687^a	-0.0720^a	-0.0712^a	-0.0710^a	-0.0905^a
		(0.0096)	(0.0106)	(0.0117)	(0.0117)	(0.0118)	(0.0163)
Log(num. of offices)		0.1135^a	0.1063^a	0.1237^a	0.1206^a	0.1206^a	0.1467^a
		(0.0230)	(0.0202)	(0.0228)	(0.0219)	(0.0219)	(0.0322)
Age				-0.0025^b	-0.0017	-0.0017	-0.0017
				(0.0013)	(0.0013)	(0.0013)	(0.0021)
Log(income)				0.0112	0.0225	0.0234	0.0184
				(0.0254)	(0.0251)	(0.0250)	(0.0437)
Log(aggregate RIA AUM)				0.0028	0.0107^b	0.0110^b	0.0145^b
				(0.0048)	(0.0047)	(0.0047)	(0.0065)
Pct. affinity group						-0.0011	0.0010
						(0.0035)	(0.0047)
Adviser FE	Y	Y	Y	Y	Y	Y	Y
Filing period FE	Y	Y	N	N	N	N	N
Main office state-year FE	N	N	Y	Y	Y	Y	Y
Advisers with at least one victim	N	N	N	N	N	N	Y
R^2	0.94	0.94	0.94	0.94	0.94	0.94	0.93
N	15,416	15,416	15,416	15,278	15,278	15,278	7,991

B. Matched sample

Dependent Variable:	Madoff exp. indicator	Log (AUM)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age	-0.0067[b]						
	(0.0029)						
Log(income)	0.6631[a]						
	(0.0603)						
Log(aggregate RIA AUM)	0.1604[a]						
	(0.0109)						
Pct. affinity group	0.1516[a]						
	(0.0089)						
Log(beg. AUM)	-0.0339[c]						
	(0.0205)						
Log(num. of offices)	0.0506						
	(0.0451)						
Post ×							
Madoff exposure indicator		-0.0703[b]	-0.0717[b]	-0.0738[a]	-0.0758[a]		
		(0.0282)	(0.0281)	(0.0275)	(0.0273)		
Madoff exposure						-0.0662[a]	-0.0652[a]
						(0.0194)	(0.0192)
Model from panel A	NA	1	2	3	4	5	6
Additional controls	N	N	Y	Y	Y	Y	Y
Adviser FE	N	Y	Y	Y	Y	Y	Y
Filing period FE	N	Y	Y	N	N	N	N
Main office state-year FE	N	N	N	Y	Y	Y	Y
R^2	0.30	0.93	0.94	0.93	0.93	0.93	0.93
N	3,813	7,066	7,066	7,066	7,066	7,066	7,066

C. Differences between treatment and control samples

Variable	Sample	Control (1)	Treatment (2)	Diff. (3)
Madoff exposure	full	0.00	2.05	2.05[a]
	matched	0.00	1.33	1.33[a]
Age	full	40.89	44.81	3.92[a]
	matched	42.35	42.92	0.58
Log(income)	full	11.07	11.42	0.35[a]
	matched	11.22	11.22	0.01
Log(aggregate RIA AUM)	full	22.82	25.18	2.36[a]
	matched	23.56	23.54	-0.02
Pct. affinity group	full	2.28	7.33	5.05[a]
	matched	3.24	3.20	-0.04
Log(beg. AUM)	full	5.23	5.85	0.62[a]
	matched	5.40	5.37	-0.03
Log(num. offices)	full	0.14	0.39	0.26[a]
	matched	0.20	0.24	0.04[c]

Table 3: RIA flows and client proximity to Madoff victims

The table displays regression results of difference-in-differences regressions estimating the impact of exposure to the Madoff fraud on RIA asset flows using the sample and general methodology outlined in Table 2. The main difference is the measure of Madoff exposure. In this table Madoff exposure is client proximity based. Specifically, Madoff exposure is measured as the average number of Madoff victims per 1,000 population in the states in which the firm has at least five clients. This measure is based on states in which the RIA "notice files." All regressions include adviser fixed effects. In addition, models include filing-period fixed effects, main office state-year fixed effects, and main office county-year fixed effects where indicated. Demographic control variables are measured as the average demographic in the states in which the firm has at least five clients. Control variables include beginning assets under management (measured in 2005), the log of the number of states in which the adviser has at least five clients, average state-level age, log of the average state-level median income, log of aggregate RIA AUM in the main office ZIP code, and the percentage of the state populations that is in the affinity group. The table reports standard errors clustered by adviser and filing period in models 1, 2, and 5, by adviser and state-year in models 3 and 6, and by adviser and county-year in the remaining models. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post ×							
Madoff exposure	-0.8462^a	-1.1434^a	-0.8557^b	-0.9656^b	-0.2954	0.0409	-0.2969
	(0.2187)	(0.2553)	(0.3425)	(0.3811)	(0.6913)	(0.7314)	(0.9323)
Log(beg. AUM)	0.0833^a	0.0755^a	0.0722^a	0.0730^a	0.0685^a	0.0644^a	0.0683^a
	(0.0125)	(0.0126)	(0.0127)	(0.0139)	(0.0134)	(0.0144)	(0.0170)
Log(num. client states)	-0.0788^a	-0.0802^a	-0.0790^a	-0.0781^a	-0.0789^a	-0.0778^a	-0.0772^a
	(0.0108)	(0.0138)	(0.0129)	(0.0126)	(0.0139)	(0.0130)	(0.0128)
Age		0.0245^a	0.0071	0.0145	0.0191^b	-0.0011	0.0073
		(0.0073)	(0.0124)	(0.0133)	(0.0075)	(0.0126)	(0.0138)
Log(income)		-0.2220^c	-0.2566	-0.2113	-0.4230^a	-0.5321^a	-0.4995^b
		(0.1217)	(0.1662)	(0.1975)	(0.1566)	(0.1839)	(0.2188)
Log(aggregate RIA AUM)		0.0035	0.0020	0.0100^c	0.0038	0.0020	0.0099^c
		(0.0058)	(0.0042)	(0.0053)	(0.0057)	(0.0041)	(0.0053)
Pct. affinity group					0.0267^c	0.0444^b	0.0515^b
					(0.0152)	(0.0192)	(0.0222)
Madoff exposure × Pct. affinity group					-0.2368^b	-0.3137^a	-0.3019^b
					(0.1159)	(0.1151)	(0.1230)
Adviser FE	Y	Y	Y	Y	Y	Y	Y
Filing period FE	Y	Y	N	N	Y	N	N
Main office state-year FE	N	N	Y	N	N	Y	N
Main office county-year FE	N	N	N	Y	N	N	Y
R^2	0.94	0.94	0.94	0.94	0.94	0.94	0.94
N	14,631	14,623	14,623	14,623	14,623	14,623	14,623

Table 4: RIA characteristics and flows

The table displays regression results of difference-in-differences regressions estimating the differential impact of exposure to the Madoff fraud on RIA asset flows based on different RIA characterstics. Regressions follow those in Column 2 of Table 3, but also include RIA characteristics interacted with the post period and the interaction of the post period, the RIA characteristic, and Madoff exposure. Coefficient estimates and their standard errors clustered by RIA and filing period are reported for these variables of interest (coefficient estimates on control variables are not reported for brevity) for three different samples: the full sample, the sample of wealth managers, and the sample of private fund advisers. Private fund advisers are those RIAs that disclosed in 2007 that they advise a private fund. Wealth managers are RIAs that did not make this disclosure. Characteristics included are: "Financial planning," which is a dummy variable indicating whether the RIA provides financial planning services, "Custody," which is a dummy variable that indicates whether the firm has custody of cash or securities, and "Private fund adviser," which is a dummy variable indicating whether the RIA advises a private fund. All characteristics are disclosures made in form ADV during fiscal year 2007. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

	Full		Wealth mgrs.		Prvt. fund advisers	
	(1)	(2)	(3)	(4)	(5)	(6)
Post ×						
Madoff exposure	-1.2114^a	-0.5940^a	-0.5371^b	-0.8874^a	-1.6883^a	-1.4918^c
	(0.3999)	(0.2285)	(0.2351)	(0.3026)	(0.4649)	(0.8385)
Financial planning	0.1165^a			0.0734^a		0.2345^a
	(0.0264)			(0.0274)		(0.0551)
Custody	0.0115			0.0636^b		-0.0221
	(0.0282)			(0.0304)		(0.0494)
Private fund adviser		-0.0249				
		(0.0298)				
Post × Madoff exposure ×						
Financial planning	0.9338^b			0.7690^b		0.4531
	(0.4177)			(0.3833)		(0.8670)
Custody	-0.6422			-0.6147		-0.2670
	(0.5939)			(0.5236)		(0.9549)
Private fund adviser		-1.0618^b				
		(0.4255)				
Additional controls	Y	Y	Y	Y	Y	Y
Adviser FE	Y	Y	Y	Y	Y	Y
Filing period FE	Y	Y	Y	Y	Y	Y
R^2	0.94	0.94	0.94	0.94	0.92	0.93
N	14,623	14,623	9,888	9,888	4,735	4,735

Table 5: RIA Closures and client proximity to Madoff victims

The table displays linear probability regression results predicting the probability of RIAs going out of business following the Madoff event in either 2009 or 2010. The full sample (Column 1) is composed of all U.S.-based RIAs in existence in 2007, subject to the filters discussed in section 1 with one exception—RIAs are not required to exist in 2009 (we are trying to predict whether they go out of business in 2009 or 2010). The regresssions in Columns 2 and 3 include only RIAs that are categorized as "wealth managers" and "private fund advisers," respectively. Private fund advisers are those RIAs that disclosed in 2007 that they advised a private fund. If they did not make this disclosure, the RIA is categorized as a wealth manager. Data on RIA closures come from Form ADV-W, which is the registration withdrawal statement. In this statement, firms list the reason for their withdrawal from SEC registration. The dependent variable is an indicator variable that is one if the RIA withdraw from SEC registration due to business closure. Madoff exposure and control variables follow the client-proximity based measures outlined in Table 3. Coefficients and heteroscedastic robust standard errors are reported as well as the probability of the RIA going out of business. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Sample:	Full	Wealth mgrs.	Prvt. fund advisers
	(1)	(2)	(3)
Madoff exposure	0.2320^{a}	-0.0094	0.4244^{a}
	(0.0835)	(0.0788)	(0.1531)
Log(beg. AUM)	-0.0003	-0.0001	-0.0038
	(0.0023)	(0.0027)	(0.0040)
Log(num. client states)	-0.0034	0.0005	-0.0054
	(0.0032)	(0.0032)	(0.0061)
Age	-0.0043	-0.0026	-0.0038
	(0.0027)	(0.0026)	(0.0061)
Log(income)	-0.0089	-0.0095	-0.0134
	(0.0505)	(0.0523)	(0.1048)
Log(aggregate RIA AUM)	0.0018	-0.0012	0.0040
	(0.0011)	(0.0011)	(0.0026)
Main office state-year FE	Y	Y	Y
Prob. of RIA closure	0.027	0.018	0.045
R^2	0.03	0.02	0.05
N	4,082	2,671	1,411

Table 6: Bank deposits and Madoff victims

The table displays regression results of difference-in-differences regressions estimating the impact of exposure to the Madoff fraud on ZIP-code-level bank branch deposits using the sample of bank deposits for all U.S. ZIP codes within the 50 states that are included in the FDIC Summary of Deposits data for the years 2007 to 2010. Section 1.3 outlines the sample construction. The general methodology and format of the table follows that of Table 2. The dependent variable is the natural log of aggregate ZIP-code-level bank branch deposits. Observations are measured as of June 30th of each year. Madoff exposure is measured as the natural log of the number of Madoff victims in the ZIP code. The Madoff exposure indicator is a dummy variable that is one if the number of Madoff victims in the ZIP code is at least one. Control variables interacted with the post period include the average age of the population in the ZIP code, log of the median income in the ZIP code, log of aggregate RIA AUM in the ZIP code, log of the population in the ZIP code, the natural log of the beginning deposits in branches in the ZIP code (measured in 2006), and the average percentage of the population belonging to the affinity group in the county of the ZIP code. State-year and and county-year fixed effects are included where indicated. In models 1 through 5 of panel A the analysis is conducted using the full sample of ZIP codes, but in model 6 the sample is limited to ZIP codes with Madoff exposure indicator = 1. Standard errors are clustered by ZIP code and state-year in models 1 through 3 and by ZIP code and county-year in the remaining models. The analysis in panel B uses a propensity score matched sample that is matched using the same general methodology used in Table 2. Panel C shows the means of various characteristics for the treatment (Madoff exposure indicator = 1) and control samples and their differences for the full sample and for the matched sample used in panels A and B, respectively. Cross-sectional tests of differences are based on heteroscedastic-consistent standard errors. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

A. Full sample

	(1)	(2)	(3)	(4)	(5)	(6)
Post ×						
Madoff exposure indicator	0.0999[a]	0.0405[a]	0.0412[a]	0.0410[a]		
	(0.0106)	(0.0114)	(0.0102)	(0.0104)		
Madoff exposure					0.0284[a]	0.0362[a]
					(0.0067)	(0.0115)
Log(beg. deposits)		-0.0467[a]	-0.0453[a]	-0.0453[a]	-0.0456[a]	-0.0763[a]
		(0.0044)	(0.0048)	(0.0042)	(0.0042)	(0.0132)
Age		0.0004	0.0006	0.0006	0.0006	-0.0001
		(0.0004)	(0.0004)	(0.0005)	(0.0005)	(0.0016)
Log(income)		0.0949[a]	0.0869[a]	0.0891[a]	0.0884[a]	0.0749[b]
		(0.0074)	(0.0091)	(0.0095)	(0.0095)	(0.0305)
Log(aggregate RIA AUM)		0.0044[a]	0.0042[a]	0.0039[a]	0.0039[a]	0.0033[a]
		(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0009)
Log(population)		0.0473[a]	0.0456[a]	0.0474[a]	0.0477[a]	0.0322[b]
		(0.0042)	(0.0051)	(0.0046)	(0.0046)	(0.0151)
Pct. affinity group		0.0015	0.0016			
		(0.0010)	(0.0013)			
Post	0.0952[a]	-0.8633[a]				
	(0.0025)	(0.0821)				
ZIP code FE	Y	Y	Y	Y	Y	Y
State-year FE	N	N	Y	N	N	N
County-year FE	N	N	N	Y	Y	Y
ZIP codes with at least one victim	N	N	N	N	N	Y
R^2	0.98	0.99	0.99	0.99	0.99	0.98
N	81,194	76,562	76,562	76,569	76,569	5,607

B. Matched sample

Dependent Variable:	Madoff exp. one victim	Log (bank deposits)				
	(1)	(2)	(3)	(4)	(5)	(6)
Log(beg. deposits)	0.2793^a					
	(0.0149)					
Age	0.0270^a					
	(0.0029)					
Log(income)	0.6240^a					
	(0.0473)					
Log(aggregate RIA AUM)	0.0258^a					
	(0.0019)					
Pct. affinity group	0.1422^a					
	(0.0046)					
Post ×						
Madoff exposure indicator		0.0424^a	0.0367^a	0.0462^a	0.0493^a	
		(0.0157)	(0.0142)	(0.0126)	(0.0145)	
Madoff exposure						0.0449^a
						(0.0098)
Model from panel A	NA	1	2	3	4	5
Additional controls	N	N	Y	Y	Y	Y
ZIP code FE	N	Y	Y	Y	Y	Y
State-year FE	N	N	N	Y	N	N
County-year FE	N	N	N	N	Y	Y
R^2	0.39	0.97	0.98	0.98	0.98	0.98
N	19,404	9,121	9,090	9,090	9,090	9,090

C. Differences between treatment and control samples

Variable	Sample	Control (1)	Treatment (2)	Diff. (3)
Madoff exposure	Full	0.00	1.36	1.36[a]
	Matched	0.00	1.20	1.20[a]
Log(beg. deposits)	Full	11.14	12.89	1.75[a]
	Matched	12.81	12.76	-0.06
Age	Full	38.26	40.34	2.08[a]
	Matched	40.61	40.90	0.30
Log(income)	Full	10.80	11.20	0.40[a]
	Matched	11.14	11.14	0.00
Log(aggregate RIA AUM)	Full	2.16	11.79	9.63[a]
	Matched	10.93	10.58	-0.35
Pct. affinity group	Full	0.92	6.00	5.09[a]
	Matched	4.26	4.47	0.20

Table 7: Instrumented Madoff exposure

The table displays instrumental variable regression results of the log of RIA AUM (log of ZIP-code-level aggregate bank branch deposits) on Madoff exposure measures and control variables. The endogenous regressor is "Post × Madoff exposure," which is measured as the average victims per 1,000 population in client states in Columns 1 and 2 and as the natural log of the number of victims in the bank branch ZIP code in Columns 3 and 4. The instrument is the post period interacted with the average of the percentage of the affinity group population residing in the counties in which the firm has offices in Columns 1 and 2 and the the post period interacted with the percentage of population in the ZIP codes' county composed of members of the affinity group in Columns 3 and 4. Columns 1 and 3 show the results of the first-stage regressions. Predicted values of the Madoff victim exposure are then used in Columns 2 and 4 in the instrumented regressions. The table reports standard errors clustered by adviser and filing period in models 1 and 2, and by branch ZIP code and branch state-year in Columns 3 and 4. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

	RIA flows		Bank deposits	
	First stage	Second stage	First stage	Second stage
	(1)	(2)	(3)	(4)
Instrumented Post × Madoff exposure		-2.0579^{a}		0.0476^{b}
		(0.6347)		(0.0200)
Post ×				
Pct. affinity group	0.0037^{a}		0.0633^{a}	
	(0.0002)		(0.0053)	
Age	0.0139^{a}	0.0390^{a}	0.0046^{a}	0.0005
	(0.0005)	(0.0116)	(0.0011)	(0.0004)
Log(income)	-0.0087	-0.2177^{c}	0.1141^{a}	0.0844^{a}
	(0.0085)	(0.1218)	(0.0184)	(0.0093)
Log(aggregate RIA AUM)	0.0002	0.0063	0.0096^{a}	0.0040^{a}
	(0.0003)	(0.0057)	(0.0009)	(0.0004)
Log(beg. AUM)	-0.0061^{a}	0.0694^{a}		
	(0.0009)	(0.0127)		
Log(num. client states)	0.0004	-0.0790^{a}		
	(0.0006)	(0.0139)		
Log(population)			-0.0267^{a}	0.0466^{a}
			(0.0055)	(0.0053)
Log(beg. deposits)			0.0360^{a}	-0.0462^{a}
			(0.0070)	(0.0050)
Adviser FE	Y	Y	N	N
Filing period FE	Y	Y	N	N
ZIP code FE	N	N	Y	Y
State-year FE	N	N	Y	Y
R^2	0.77	0.94	0.68	0.99
N	14,619	14,619	76,625	76,565