

Bad Markets, Bad Advisors? Investor Scrutiny and Recorded Financial Advisor Misconduct^Ψ

R. David McLean, Chi Wan, and Mengxin Zhao

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Abstract

Recorded financial advisor misconduct rises sharply when stock market performance is poor. We ask whether this reflects more wrongdoing, investors recasting losses as misconduct, or greater detection of existing misconduct. The evidence is most consistent with detection. Geographic exposure to corporate fraud and the Madoff scandal—trust shocks unrelated to the performance underlying the complaints we study—also increase recorded misconduct. Poor returns also increase directly verifiable violations, such as unauthorized trading, and complaints that yield no adverse finding or payment. Effects are strongest among advisors with high ex-ante misconduct risk. Recorded misconduct thus reflects both wrongdoing and investor scrutiny.

^Ψ Contact information: McLean (dm1448@georgetown.edu), Wan (cwan@sdsu.edu), and Zhao (mmclea2@gmu.edu). We are grateful for helpful comments from seminar participants at UT Dallas, Purdue, Rutgers, Simon Fraser, George Washington, Michigan State, and Kansas.

One in fifteen U.S. financial advisors carries a record of misconduct, and in parts of the country the figure approaches one in six (Egan, Matvos, and Seru (2024)). This matters because financial advisors are relied on by 41% of U.S. adults, and by a majority of upper-income households and of those aged 65 and older (Saad (2025)). Financial advisors oversee trillions of dollars in household savings and exert substantial influence over how those savings are invested (Egan, Matvos, and Seru (2019); Foerster, Linnainmaa, Melzer, and Previtero (2017)). This paper documents a new fact about financial advisor misconduct and asks what it means: recorded misconduct is strongly countercyclical, rising sharply when stock market performance is poor.

We begin by establishing this fact. A year in which the market return is one standard deviation below its mean has a recorded-misconduct rate 28% higher than a year one standard deviation above; the difference rises to 65% for a two-standard-deviation contrast. The fact is not an artifact of the aggregate time series: we find the same pattern in the cross-section using the returns of firms headquartered within 250 miles of the advisor's office. Because those tests compare localities within the same year and control for local economic conditions, aggregate forces such as recessions, litigation waves, and regulatory cycles cannot explain them.

We consider three explanations for why recorded misconduct rises when market returns are poor. First, advisors may actually engage in more misconduct when market conditions deteriorate—for example, because falling income or wealth increases financial pressure (Dimmock, Gerken, and Van Alfen (2021)). This explanation implies variation in underlying advisor conduct. Importantly, however, our measure captures the timing of the complaint rather than the timing of the underlying conduct, which may have occurred earlier.

Second, poor market performance may make pre-existing advisor actions more likely to be perceived as misconduct and generate a complaint. Losses may cause disappointed investors to attribute poor investment outcomes to the advisor's actions and reinterpret advice that turned out poorly as misconduct. Under this explanation, market conditions affect the likelihood that observed advisor actions are attributed to misconduct, rather than whether the underlying conduct occurred or whether investors noticed previously unobserved information.

Third, poor market performance may increase investor attention and scrutiny. Bad performance can undermine confidence in an advisor's competence or integrity (Guiso, Sapienza, and Zingales (2008); Gennaioli, Shleifer, and Vishny (2015)) and erode trust in financial institutions more broadly (Stevenson and Wolfers (2011); Sapienza and Zingales (2012)). These effects may lead investors to examine their accounts and transactions more closely, including aspects of their advisor's behavior that they had previously overlooked. Under this explanation, market downturns increase the probability that pre-existing but previously unobserved misconduct is discovered and recorded, rather than changing how investors interpret conduct that was already observed.

These three channels are not mutually exclusive, and all three may play some role. That said, we find substantial evidence consistent with increased scrutiny. An ideal test for the scrutiny channel would isolate an increase in investor scrutiny without simultaneously affecting either the advisor's behavior or the performance of the investor's portfolio. We use two shocks that plausibly accomplish this: exposure to corporate fraud at firms headquartered in the advisor's state (Giannetti and Wang (2016)) and local exposure to the Bernard Madoff Ponzi scheme (Gurun, Stoffman, and Yonker (2018)). Neither shock is closely tied to losses in the accounts

underlying the complaints—Madoff-related cases are excluded, and direct client exposure to a given fraud firm is limited—so investors’ losses are unlikely to produce the associated increases in recorded misconduct. There is also little reason for a nearby accounting fraud or the Madoff revelation to cause an ordinary financial advisor to begin committing misconduct. Yet we find that both shocks raise recorded financial advisor misconduct, consistent with *recorded* misconduct reflecting both advisor misconduct and investor vigilance.

We also split the Egan, Matvos, and Seru (2019) misconduct measure into two categories: judgment-dependent misconduct, such as unsuitable recommendations, where identifying misconduct requires the investor to evaluate whether the advisor acted appropriately, and directly verifiable misconduct, such as unauthorized trading, churning, and outright fraud. Directly verifiable violations turn on conduct rather than on the quality of advice and are difficult to manufacture by reinterpreting a disappointing outcome. Both types rise during downturns; the rise in directly verifiable violations again points to greater investor scrutiny.

The scrutiny channel also predicts that investors should become more likely to complain, resulting in some cases that ultimately produce no adverse finding against the advisor or payment to the investor. The recorded misconduct measure we use follows Egan, Matvos, and Seru (2019) and includes only cases that resulted in some form of action against the advisor, with settled disputes counted regardless of the settlement amount. We examine two related measures of investor complaints: complaints that were denied, dismissed, withdrawn, or closed with no action, which lie outside the misconduct measure, and complaints from any disclosure category that resulted in a zero-dollar settlement. These complaints also rise when market and local returns are poor, which is consistent with an increase in investors’ propensity to scrutinize and

complain. These tests do not distinguish the scrutiny channel from investors simply recasting losses as misconduct, since both predict more complaints when returns are poor. They do, however, distinguish both channels from advisors committing more misconduct in response to lower returns.

Finally, the effects concentrate where there is more misconduct to find. Exposure to corporate fraud has much larger effects among advisors with prior misconduct records, advisors with Series 65 qualifications (which are associated with investment-advisory activity), and male advisors—the traits that most strongly predict misconduct. An advisor with a prior record is roughly twelve times as sensitive to a corporate fraud in her state as an advisor with a clean history. Because a corporate fraud at a public firm need not create losses in the accounts underlying the complaints we count, this concentration is difficult to attribute to larger damages. Instead, it is the pattern the scrutiny channel predicts: scrutiny uncovers misconduct where misconduct exists.

Our evidence supports a simple insight: recorded financial advisor misconduct is shaped not only by advisor behavior but also by investor trust. When trust declines, scrutiny rises, and misconduct becomes visible. The true prevalence of misconduct may therefore be materially higher than observed in the data, with important implications for regulatory monitoring, investor protection, and empirical research that relies on recorded violations.

Our paper advances the literature on financial advisor misconduct, which has largely studied who commits misconduct and with what consequences (Egan, Matvos, and Seru (2019, 2022); Dimmock, Gerken, and Graham (2018); Dimmock, Gerken, and Van Alfen (2021); Bai, Shang, Wan, and Zhao (2022)). We instead ask when misconduct becomes observable.

Charoenwong, Kwan, and Umar (2019) show that recorded complaints depend on the regulatory-supervision environment; we document a complementary, client-side margin: recorded misconduct also depends on investors' propensity to scrutinize and complain. This changes how misconduct statistics should be interpreted. If observed misconduct reflects wrongdoing interacting with scrutiny, complaint-based measures understate underlying misconduct when scrutiny is low. The one-in-fifteen figure with which we began is therefore likely a lower bound on the prevalence of underlying misconduct.

We also connect the literature on trust in financial markets to the detection of wrongdoing. Trust is fundamental to stock market participation and to the delegation of investment decisions (Guiso, Sapienza, and Zingales (2004, 2008); Gennaioli, Shleifer, and Vishny (2015)), and shocks that erode it change household behavior: households reduce stock market participation after fraud revelations at firms headquartered in their state (Giannetti and Wang (2016)), and investors in communities exposed to the Madoff fraud withdrew assets from investment advisors (Gurun, Stoffman, and Yonker (2018)). We show that declining trust also changes monitoring: rather than only exiting, investors scrutinize their advisors and uncover misconduct.

More broadly, our results speak to settings in which violations become observable only after private parties investigate or complain—corporate fraud, workplace misconduct, whistleblowing. Related research emphasizes both the detection of latent wrongdoing and the difficulty of inferring its prevalence from observed cases (Dyck, Morse, and Zingales (2010, 2024); Beneish (1999); Dechow, Ge, Larson, and Sloan (2011); Wang, Winton, and Yu (2010)). In such

settings, observed wrongdoing reflects both underlying conduct and the intensity of scrutiny. We extend this literature to financial advisors, an important setting given their widespread use.

1. Data, Sample, and Preliminaries

1.1. Misconduct Data

We collect financial advisor misconduct data by scraping the Financial Industry Regulatory Authority's (FINRA) BrokerCheck database and the Investment Adviser Public Disclosure (IAPD) database. BrokerCheck covers brokers registered with FINRA, while IAPD covers investment adviser representatives registered with the SEC or state securities regulators; together, the databases provide data on an advisor's employment history, qualifications, and disclosures, which include any misconduct allegations. Our final sample includes more than 1.4 million financial advisors. The databases place each advisor's disclosures into 23 categories, from criminal offenses to customer disputes. Our sample period is from 2000 through 2020.

Following Egan, Matvos, and Seru (2019), we classify a disclosure as misconduct if it falls into one of the six disclosure categories that they identify as serious enough to be categorized as misconduct: Customer Dispute–Settled, Customer Dispute–Award/Judgment, Employment Separation After Allegations, Regulatory–Final, Criminal–Final Disposition, and Civil–Final; the remaining seventeen of the twenty-three disclosure categories are not counted as misconduct.

Our outcome measures are based on when the complaint against the advisor was initiated, which can differ from when the underlying conduct occurred. The underlying conduct may have occurred months or years earlier. Accordingly, throughout the paper, “recorded

misconduct in year t ” means the complaint was initiated in year t ; it should not be interpreted as misconduct committed in year t .

Table 2 summarizes misconduct cases by complaint reason. The most common category is “Unsuitable Investments,” which accounts for 15.3% of cases. Other frequent categories include “Misrepresentation” (8.02%), “Unauthorized Activity” (5.60%), “Fee/Commission Fraud” (4.77%), and “Breach of Fiduciary Duty” (3.83%). Fraud represents only 3.72% of cases. The Other/Unclassified category comprises 52.28% of cases where our textual analysis could not clearly identify the primary cause or where the conduct did not fit neatly into one of the predefined categories. Table 2 also tags each category as judgment-dependent (J) or directly verifiable (V), a distinction we exploit in Section 3.3.

1.2. Control Variables

Our regressions include control variables measured at the advisor, advisory firm, and county levels.

Advisory Firm Characteristics. The advisory firm traits we use include the number of affiliated advisors and the percentage of advisors with misconduct cases over the last three years.

Advisor Characteristics. For advisor characteristics, we include a dummy variables indicating whether the advisor has a past misconduct case in the last three years, whether the advisor is female, and whether the advisor is Caucasian. We also include dummy variables indicating whether the advisor has passed the Series 63 (Uniform Securities Agent State Law), Series 65 (Uniform Investment Adviser Law), or Series 66 (Uniform Combined Law) examinations.

County Characteristics. At the county level, we include the county population, the average household income, yearly per capita GDP growth, and yearly growth in median home prices (from Zillow).

2. The Stylized Fact: Market Performance and Recorded Misconduct

This section documents the paper's central stylized fact: recorded financial advisor misconduct rises when stock returns are low. We first establish the fact in the aggregate time series using market returns, and then in the cross-section using local stock returns. The cross-sectional tests include year fixed effects, so aggregate forces—recessions, litigation waves, regulatory cycles—cannot explain them. All of the regressions in this section include state and advisory-firm fixed effects.

2.1. Market Returns

Table 3 relates financial advisor misconduct to market returns. Financial advisor misconduct is a dummy variable equal to one if the financial advisor has a misconduct case initiated in year t , and zero otherwise. As we explained above, the misconduct year is the year in which the complaint was initiated, which may be after the year in which the misconduct occurred. We measure aggregate market performance using the CRSP value-weighted index. Returns are measured either in year $t-1$ or year t , while misconduct is measured in year t .

Columns 1, 2, and 3 show that lagged market returns strongly predict misconduct in the subsequent year. The three columns report results using different control variables, and in all cases, the market returns are a robust predictor of financial advisor misconduct. In the third

specification, which includes the complete set of controls, the coefficient is -0.518, and the t -statistic is -11.38. For interpretation, the dependent variable is multiplied by 100. Table 1 shows that the market return has a standard deviation of 0.180, so a one-standard-deviation decline in market returns is associated with a 0.09 percentage-point increase in the likelihood of misconduct. The unconditional probability that an advisor-year observation has a misconduct case is 0.76%, so a one-standard-deviation decline in market returns increases the likelihood of a misconduct case by 12.3 percent. Column 4 replaces the year $t-1$ market return with the contemporaneous, year- t market return. The coefficient is -0.541 (t -statistic = -7.85), close to the lagged estimate.

The control variables behave as in prior work: misconduct is persistent and concentrated among repeat offenders, and it is more common among male advisors, advisors with the Series 65, and advisors at smaller firms and in lower-income counties (Egan, Matvos, and Seru (2019, 2022); Dimmock, Gerken, and Van Alfen (2021)).

The results in Table 3 establish the first half of the stylized fact: recorded misconduct is strongly countercyclical, whether market returns are measured in the year before or in the same year as the recorded case. On their face, these results are consistent with several different mechanisms—advisors behaving worse in downturns, investors complaining more when they lose money, or investors scrutinizing their accounts more closely. Distinguishing among these explanations is the task of Section 3.

2.2. Local Stock Returns

In Table 4, we relate financial advisor misconduct to local stock returns. We measure local stock performance by constructing a value-weighted portfolio of firms headquartered within 250 miles of the advisor's ZIP code. The 250-mile radius follows Ivković and Weisbenner (2005) and Seasholes and Zhu (2010); results are similar with 100- or 500-mile radii (untabulated). This approach generates geographically varying equity returns that capture spatially concentrated shocks to investor experience. We include year fixed effects in these tests, so the effects are orthogonal to the market-return tests we report in Table 3.

A large literature documents that investors exhibit strong local bias and that geographically proximate outcomes are particularly salient. Households overweight nearby firms in their portfolios (Ivković and Weisbenner (2005); Seasholes and Zhu (2010)), discuss investments within local social networks (Hong, Kubik, and Stein (2004)), and are influenced by the financial experiences of neighbors and coworkers (Brown, Ivković, Smith, and Weisbenner (2008)). Thus, many investors are exposed to local stock performance—directly through their own portfolios and indirectly through the experiences of friends, neighbors, and coworkers—so poor local returns are a widely felt and salient event in an area, even for investors who hold no local stocks themselves.

The misconduct data include each advisor's home-office ZIP code but not the complainant's address, so we use the advisor's location to proxy for the location of her clients. Most advisory firms are small and geographically dispersed, and the large national firms maintain dense local office networks to serve nearby clientele. To the extent that some complainants are not local, this measurement error should work against finding a local-return effect.

We test whether advisors located in areas with relatively poor local stock returns are more likely to have misconduct cases in a given year. The first three regressions in Table 4 report results with the local return variable lagged by one year, along with various controls. Whereas Table 3 asked “during a year with poor market performance, is there a greater likelihood of advisor misconduct?”, this table asks “in a given year, do advisors in localities with relatively low stock returns have a greater likelihood of misconduct”?

The third regression has the most complete set of controls. In this regression, the coefficient for local returns is -0.285 (t -statistic = -3.25). The local return variable has a standard deviation of 0.224. Thus, a one-standard-deviation decline in local returns is associated with a 0.06 percentage-point increase in the likelihood of misconduct. The unconditional probability of misconduct is 0.76%, indicating an 8.4 percent increase in the likelihood of misconduct.

In regression 4, we measure the local return contemporaneously, in year t , and the effect is even larger. The local return coefficient is -0.469, and the t -statistic is -4.11. In this specification, a one-standard-deviation decline in local returns is associated with a 13.8 percent increase in the likelihood of misconduct.

Column 5 returns to $t-1$ local returns with the full set of controls and adds $t-1$ nonlocal returns, the value-weighted average return of all stocks not included in the local return portfolio. The local return coefficient is about the same (-0.309; t -statistic = -2.86), while the nonlocal return is not significant (-0.319; t -statistic = -0.33). Because nonlocal returns have little cross-sectional variation within a year, this specification has limited power to distinguish the two coefficients; what it establishes is that the local-return effect survives controlling for nonlocal returns.

Together, Tables 3 and 4 establish the stylized fact: recorded advisor misconduct rises when performance is poor—in the aggregate time series and, within a year, across localities. The within-year evidence, with year fixed effects and controls for local economic conditions, rules out explanations that operate only through aggregate cycles. Before turning to interpretation, we summarize the magnitudes these estimates imply.

2.3. Magnitudes

Figure 1 translates the estimates in Tables 3 and 4 into recorded-misconduct rates. Panel A reports the effects for market returns. Using the market-return coefficient from Table 3 (-0.518), we compute fitted rates around the sample mean of 0.76% for return states one and two standard deviations above and below the mean. Panel A shows that a year in which the market return is one standard deviation below its mean has a fitted misconduct rate of 0.85%, versus 0.67% in a year one standard deviation above, so estimated misconduct is 28% higher in the low return state. For a two-standard-deviation contrast, the rates are 0.95% versus 0.57%, so recorded misconduct is 65% higher in the unfavorable state.

Panel B uses the local-return coefficient from Table 4, regression 3 (-0.285). The local-return contrasts are somewhat smaller, but still economically meaningful. The estimated misconduct is 18% higher for the ± 1 SD contrast (0.82% versus 0.70%) and 40% higher for the ± 2 SD contrast (0.89% versus 0.63%).

Taken in its entirety, Figure 1 shows that returns have large effects on the reporting of financial advisor misconduct. This is true both in the time series (Panel A) and in the cross-section

(Panel B). Whether these patterns reflect changes in advisor behavior, investors recasting poor outcomes as misconduct, or changes in investor scrutiny is the question we address next.

3. Why Does Recorded Misconduct Rise? Evidence on Detection

In this section, we try to better understand why recorded financial advisor misconduct rises when stock returns are lower. An ideal test would isolate an increase in investor scrutiny without simultaneously affecting either the advisor's behavior or the performance of the investor's portfolio. We therefore begin by relating recorded misconduct to two shocks that plausibly have these features: geographic exposure to corporate fraud and to the Madoff Ponzi scheme. These events are salient shocks to investor trust, yet there is little reason for them to cause unrelated financial advisors to change their behavior, and they are largely disconnected from the investment performance underlying the complaints we study.

We then examine alternative measures of misconduct and investor complaints. Some forms of misconduct, such as unauthorized trading, excessive trading, and fraud, are directly verifiable in the sense that their classification does not depend primarily on whether an investment recommendation subsequently performed poorly. We therefore ask whether these cases also become more likely to enter the record following poor market returns. A scrutiny channel also predicts greater complaint activity at the margin. Accordingly, we examine complaints that were ultimately denied, dismissed, withdrawn, closed with no action, or resolved with no monetary payment. These cases do not establish that no misconduct occurred, but their incidence provides a useful measure of investors' propensity to scrutinize and complain. If these

complaints also rise when returns are poor, that pattern is consistent with heightened investor scrutiny contributing to the increase in recorded misconduct.

3.1. Corporate Fraud Exposure

We follow Giannetti and Wang (2016) in treating fraud revelations involving firms headquartered in a state as geographically localized shocks to investor confidence. They show that exposure to corporate fraud reduces household stock market participation, including among households without direct holdings in the fraud firms, and that these effects persist beyond what can be explained by local economic conditions or market returns. This evidence suggests that corporate fraud undermines trust in financial markets more broadly, rather than merely altering beliefs about individual firms.

We measure exposure to corporate fraud using SEC Accounting and Auditing Enforcement Releases (AAERs), which document enforcement actions related to accounting manipulation, financial misrepresentation, and other serious reporting violations. Prior studies use AAERs and other enforcement-based fraud measures to examine the consequences of fraud for firm value, investor behavior, contracting, and market outcomes (e.g., Dechow et al. (2011), Karpoff, Lee, and Martin (2008), Karpoff, Koester, Lee, and Martin (2017), and Dyck, Morse, and Zingales (2010)). Accordingly, AAER-based fraud exposure provides trust-relevant shocks that are distinct from contemporaneous market performance and local economic conditions.

Table 5 examines the relationship between local exposure to corporate fraud and financial advisor misconduct. As in the previous tables, the dependent variable is an indicator for whether a misconduct case, subsequently resolved against the advisor, was initiated in year t . Corporate

fraud exposure is measured using indicator variables that equal one if at least one firm headquartered in the advisor's state is named in an SEC Accounting and Auditing Enforcement Release (AAER) within the past 1, 3, or 4 years. This measure is constructed similarly to Giannetti and Wang (2016). As in the other tables, advisor- and county-level characteristics are included as controls, and all specifications include year and advisory-firm fixed effects, so identification comes from cross-sectional differences in corporate fraud exposure within the same year.

Across specifications, local exposure to corporate fraud is positively and significantly associated with financial advisor misconduct. Advisors operating in states that experienced a recent corporate fraud event are more likely to have a recorded misconduct case. The results are robust across alternative fraud windows, indicating that the relationship is not driven by a particular horizon or isolated event.

In regression 3, which uses the 4-year fraud measure and the full set of controls, the fraud coefficient is 0.041, and the *t*-statistic is 2.24. To put that into context, having a corporate fraud case in the state over the past 4 years increases the likelihood of a financial advisor misconduct case by 5.4%, as the base probability of misconduct is 0.76% (see Table 1). Having fraud in the last 1 or 3 years is also associated with higher misconduct levels, though the effects are strongest with the 4-year measure, likely because it has greater power.

It is difficult to reconcile these findings with corporate fraud causing financial advisors to commit more misconduct. An individual corporate fraud case also does not lower returns for most investors, so the increase in misconduct documented here is unlikely to be caused by fraud-induced losses. Instead, the findings are consistent with a trust-and-scrutiny interpretation: salient corporate scandals reduce investor confidence in financial markets and intermediaries,

leading investors in affected states to reassess financial advice and scrutinize financial advisor behavior more closely.

3.2. Madoff Exposure and Financial Advisor Misconduct

Our second trust shock repeats the same test with a single, salient event. We follow Gurun, Stoffman, and Yonker (2018) in treating the public revelation of the Bernard Madoff Ponzi scheme in December 2008 as a large and sudden shock to investor trust in financial intermediaries. They construct a county-level measure of exposure based on the pre-existing geographic concentration of Madoff victims and show that investors in more exposed areas reduced their use of financial advisors and reallocated assets toward safer alternatives following the fraud's disclosure. These patterns indicate a broad erosion of trust in intermediaries, extending beyond advisors directly connected to the scheme.

The geographic distribution of Madoff victims was largely determined prior to the fraud's revelation and is plausibly orthogonal to contemporaneous local economic conditions. We use this county-level exposure measure to capture geographically varying shocks to investor trust that are unrelated to local market performance or advisor-specific behavior. Because Madoff is a single event rather than a series of shocks across states and years, this design provides less statistical power than the corporate-fraud setting used in the last table.

Table 6 examines the relationship between local exposure to the Bernard Madoff Ponzi scheme and financial advisor misconduct. As with the previous tables, the dependent variable equals one if an advisor has a misconduct case initiated in year t , and the tests reported in this table exclude any Madoff-related cases. The key explanatory variable, Madoff Exposure, is an

indicator for whether an advisor operates in a county with at least one, five, or ten identified Madoff victims. This measure follows Gurun, Stoffman, and Yonker (2018). We examine non-Madoff advisor misconduct initiated within 5 years or 3 years of the public revelation of the fraud in December 2008. Advisor- and county-level characteristics are included as controls, and all specifications include year, state, and advisory-firm fixed effects, so identification comes from cross-sectional differences in exposure within the same year.

Across all specifications, local exposure to Madoff victims is positively and significantly associated with non-Madoff financial advisor misconduct. Advisors operating in counties with at least one Madoff victim experience a higher likelihood of misconduct in the years following the fraud's revelation. The effect is robust across alternative windows and is strongest where local exposure is most severe: the coefficient for counties with ten or more victims (0.103) is roughly 50 percent larger than the coefficient for counties with at least one victim (0.067). The results are similar when restricting the window to three years following the fraud, indicating the effect is already present in the period immediately after the trust shock.

In regression 3, which defines exposure as at least one Madoff victim in the advisor's county, measures misconduct within five years of the fraud's revelation, and includes the full set of controls, the Madoff exposure dummy equals 0.067, and the t-statistic is 2.32. This reflects an 8.8% increase in the likelihood that an advisor has a misconduct case in year t . In regression 6, we use a more stringent exposure threshold of ten or more victims in the advisor's county, and the implied increase in the likelihood of misconduct rises to 13.6%.

Because the Madoff fraud was revealed suddenly and was unrelated to the behavior of most financial advisors, these findings are difficult to reconcile with explanations based on

changes in advisor conduct. Instead, the results support a trust-and-scrutiny mechanism. The public revelation of a large and salient fraud plausibly reduced investor confidence in financial intermediaries more broadly, particularly in areas where local investors were directly harmed. In these locations, investors may have become more vigilant, re-examined prior advice, and scrutinized advisor behavior more closely, increasing the likelihood that existing misconduct was detected and recorded.

3.3. Judgment-Dependent versus Directly Verifiable Misconduct

Most advisor misconduct is not blatant. As shown in Table 2, many cases concern judgment-dependent conduct—suitability, misrepresentation, omission of key facts, breach of fiduciary duty, negligence, and risky-investment recommendations—where identifying wrongdoing requires the investor to evaluate whether the advisor acted appropriately. We label these judgment-dependent cases. We classify the Other/Unclassified cases with this group because they do not fall into the four directly verifiable categories below; the results in this section are similar when these cases are excluded.

In contrast, directly verifiable cases—unauthorized activity, fee or commission fraud, fraud, and churning or excessive trading—turn on conduct rather than on the quality of advice: these allegations are much harder to generate merely by deciding ex post that a recommendation was poor.

The distinction between the two types of misconduct lets us address a concern specific to the judgment-dependent cases: a market downturn could lead investors who lost money to recast

advice that merely turned out poorly as unsuitable—a hindsight effect rather than the detection of pre-existing wrongdoing. Directly verifiable cases are unlikely to be generated this way.

Table 7 re-estimates the market- and local-return specifications separately for judgment-dependent misconduct (columns 1 and 2) and directly verifiable misconduct (columns 3 and 4). Both types respond. For judgment-dependent misconduct, the coefficients on market and local returns are negative and significant (t-statistics = -7.51 and -2.96, respectively), and the results are similar when the Other/Unclassified category is excluded (untabulated). Directly verifiable misconduct also rises when market and local returns are poor (t-statistics = -9.08 and -2.70, respectively).

Because judgment-dependent cases are, under our baseline classification, roughly ten times as common as directly verifiable cases, we compare effects relative to their base rates rather than raw coefficients. A one-standard-deviation decline in the market return (0.180) raises judgment-dependent misconduct by about 0.088 percentage points, or 13% of its mean of 0.69%, and directly verifiable misconduct by about 0.009 percentage points, also about 13% of its mean of 0.07%. The proportional responses are therefore essentially identical. The local-return estimates imply, if anything, a larger proportional response for directly verifiable misconduct.

This comparison is informative because directly verifiable violations—unauthorized trading, fee or commission fraud, outright fraud, and churning—turn on conduct rather than on the quality of advice and are difficult to generate simply by reinterpreting a disappointing outcome. A pure hindsight-relabeling explanation would naturally predict a larger proportional response among judgment-dependent cases. We do not observe that pattern, making hindsight

relabeling less likely to account for the rise in recorded misconduct and leaving the evidence more consistent with increased investor vigilance.

3.4. Complaints Without Adverse Findings or Payments

The trust-and-scrutiny channel suggests an increase in all types of investor complaints, including those that ultimately are not resolved against the advisor. Our primary misconduct measure, by construction, excludes complaints that resulted in no adverse disposition. As a separate and complementary exercise, we turn to complaints that produced no adverse disposition or no payment to the investor.

We measure these in two ways. The first comprises complaints from four disclosure categories defined as denied, dismissed, withdrawn, or closed with no action. These four categories are not included in our primary misconduct measure, which, as we explained above, only includes the six most serious types of disclosures. The second comprises complaints from any of the 23 disclosure categories, not only the six that make up our misconduct measure, that ended with a settlement of zero, in which the investor complained but received no monetary award.

These tests do not distinguish the scrutiny channel from investors simply recasting losses as misconduct, since both predict more complaints when returns are poor; the trust-shock results in Sections 3.1 and 3.2 provide that distinction. The tests here do, however, distinguish both of those channels from advisors committing more misconduct in response to lower returns.

Table 8 relates the return shocks to these complaints. Denied, dismissed, withdrawn, and no-action complaints (columns 1 and 2) rise with poor market returns (t-statistic = -2.20) and poor

local returns (t-statistic = -2.34). Zero-settlement cases (columns 3 and 4) rise with poor market returns (t-statistic = -6.27) and poor local returns (t-statistic = -3.08). Because these complaints produced no award against the advisor, the rise is particularly difficult to reconcile with a pure advisor-behavior explanation, and it cannot reflect investors successfully pursuing compensation. Instead, it speaks directly to the complaint-propensity margin: when trust erodes, investors become more willing to question their advisors and file complaints, even in cases that ultimately produce no payment.

We therefore read Table 8 as direct evidence on the complaint channel, complementing Tables 3 and 4, which count only cases resolved against the advisor. Tables 7 and 8 thus describe two margins of the same response. Heightened scrutiny can uncover genuine misconduct that would otherwise remain hidden, as Table 7 suggests, while also increasing investors' propensity to complain, including complaints that ultimately produce no adverse finding or payment.

4. Where Does Detection Occur? Heterogeneous Effects

Previous studies and our earlier tables established that baseline misconduct risk varies systematically across advisors, reflecting both personal characteristics and the firm and local environments in which advisors operate. This section examines whether the return and trust shocks matter more for advisors with higher baseline misconduct risk. We do so by interacting each shock with characteristics that earlier results show predict misconduct. The intuition is straightforward: if the shocks increase investor scrutiny and lead to the discovery of misconduct, then their effects should be concentrated among advisors who are more likely to engage in

misconduct. Table 9 reports these tests for the market return (columns 1–3), the local return (columns 4–6), and corporate-fraud exposure (columns 7–9).

We focus on three distinct traits that are theoretically motivated and empirically robust predictors of financial advisor misconduct: prior misconduct history, the Series 65 qualification—which may give advisors greater discretion over the investor’s assets—and male gender. These variables are among the strongest predictors of misconduct in our specifications and have been associated with misconduct in earlier studies (Egan, Matvos, and Seru (2019, 2022)).

Because the shocks and the traits enter the regressions with different signs, the expected sign of an interaction depends on both. Return shocks enter negatively—lower returns mean more misconduct—while fraud exposure enters positively; prior misconduct and the Series 65 predict more misconduct, while female gender predicts less. The interactions should therefore amplify each shock’s effect for the high-risk traits and dampen it for female advisors, whatever the shock’s sign.

Columns 1 through 3 of Table 9 interact the market return with each trait. The interaction coefficients show that declines in market returns are associated with larger increases in recorded misconduct for advisors with higher baseline risk. The effect of market returns is greater for advisors with a misconduct history and for advisors holding a Series 65 qualification, and lower for female advisors.

The economic significance of these interactions is large. Because all three traits are dummy variables, adding the shock coefficient to the interaction coefficient gives the with-trait sensitivity, and Panel B of Table 9 reports these sensitivities and the implied multipliers. The effect of market returns is 5.1 times larger for advisors with a past misconduct history (a sensitivity of -

2.460 versus -0.486), 5.2 times larger for advisors with the Series 65, and 4.6 times larger for male than for female advisors. For the female column the multiplier is below one (0.2 in column 3)—the reciprocal of the 4.6 figure—because female advisors are the with-trait group.

Columns 4 through 6 repeat the exercise with local returns, and the patterns closely mirror those for market returns. Poor local returns have a larger effect on advisors with personal misconduct histories, Series 65 qualifications, and on male advisors.

The corresponding multipliers are 8.2 for advisors with a past misconduct history, 6.3 for the Series 65 (for which the shock is insignificant absent the trait), and 3.4 for male relative to female advisors.

Columns 7 through 9 interact corporate-fraud exposure with the same traits, and the pattern is the same. In the prior-misconduct specification (column 7), the fraud coefficient is 0.096, while the interaction is large and highly significant (1.030; t-statistic = 4.30): an advisor with a past misconduct history has a fraud sensitivity of 1.126, 11.7 times that of an advisor with a clean history. The Series 65 multiplier is 6.1, and the effect is essentially absent for female advisors (0.037, versus 0.140 for male advisors).

Across all three shocks, the effects concentrate among advisors with the highest ex-ante misconduct risk—the pattern the scrutiny channel predicts: scrutiny uncovers misconduct where misconduct exists. Interactions with Madoff exposure (untabulated) point in the same direction but are imprecisely estimated, as anticipated in Section 3.2: a binary county-level exposure over a short post-2008 window has limited power.

5. Conclusion

Recorded financial advisor misconduct is strongly countercyclical: it rises when aggregate market returns are poor and, within a year, where local stock returns are poor; recorded misconduct is 28 percent higher in a year one standard deviation below the mean market return than in a comparably good year. Our evidence suggests that changes in investor scrutiny contribute to this pattern. Trust shocks with little connection to the performance underlying the complaints we study—local exposure to corporate fraud and to the Madoff scandal—also increase recorded misconduct. Poor returns increase directly verifiable violations as well as complaints that ultimately yield no adverse finding or payment, and the effects are concentrated among advisors with high ex-ante misconduct risk. Together, the evidence indicates that recorded misconduct reflects wrongdoing interacting with investor scrutiny.

This distinction matters for how misconduct records are interpreted. When trust is high and investors scrutinize less, some wrongdoing may remain outside the observable record; when trust falls, more of it becomes visible. Complaint-based measures can therefore materially understate underlying misconduct, and they depend not only on advisor behavior but also on investors' incentives to investigate and report. It is particularly relevant for regulators who allocate oversight using recorded misconduct, as our findings suggest that observed complaint rates may reflect differences in investor monitoring as well as differences in underlying misconduct. Accounting for this reporting and detection channel can therefore help regulators better interpret misconduct records and target oversight toward advisors who may pose greater underlying risks, even when their recorded complaint histories appear relatively clean. The same

logic may extend to other settings in which violations become observable only after private monitoring, including corporate fraud, workplace misconduct, and whistleblowing.

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Figure 1: Regression-Implied Recorded-Misconduct Rates Across Return States

This figure plots fitted recorded-misconduct rates by return state (Section 2.3). Panel A uses the Table 3 market-return coefficient (-0.518) and the market-return standard deviation (0.180 , Table 1); Panel B uses the local-return coefficient from Table 4, regression 3 (-0.285) and the local-return standard deviation (0.224). Both panels are anchored at the sample-mean misconduct rate of 0.76% . Return states are one and two standard deviations above and below the sample-mean return. These values are linear projections implied by the regression estimates, not raw average misconduct rates within return bins.

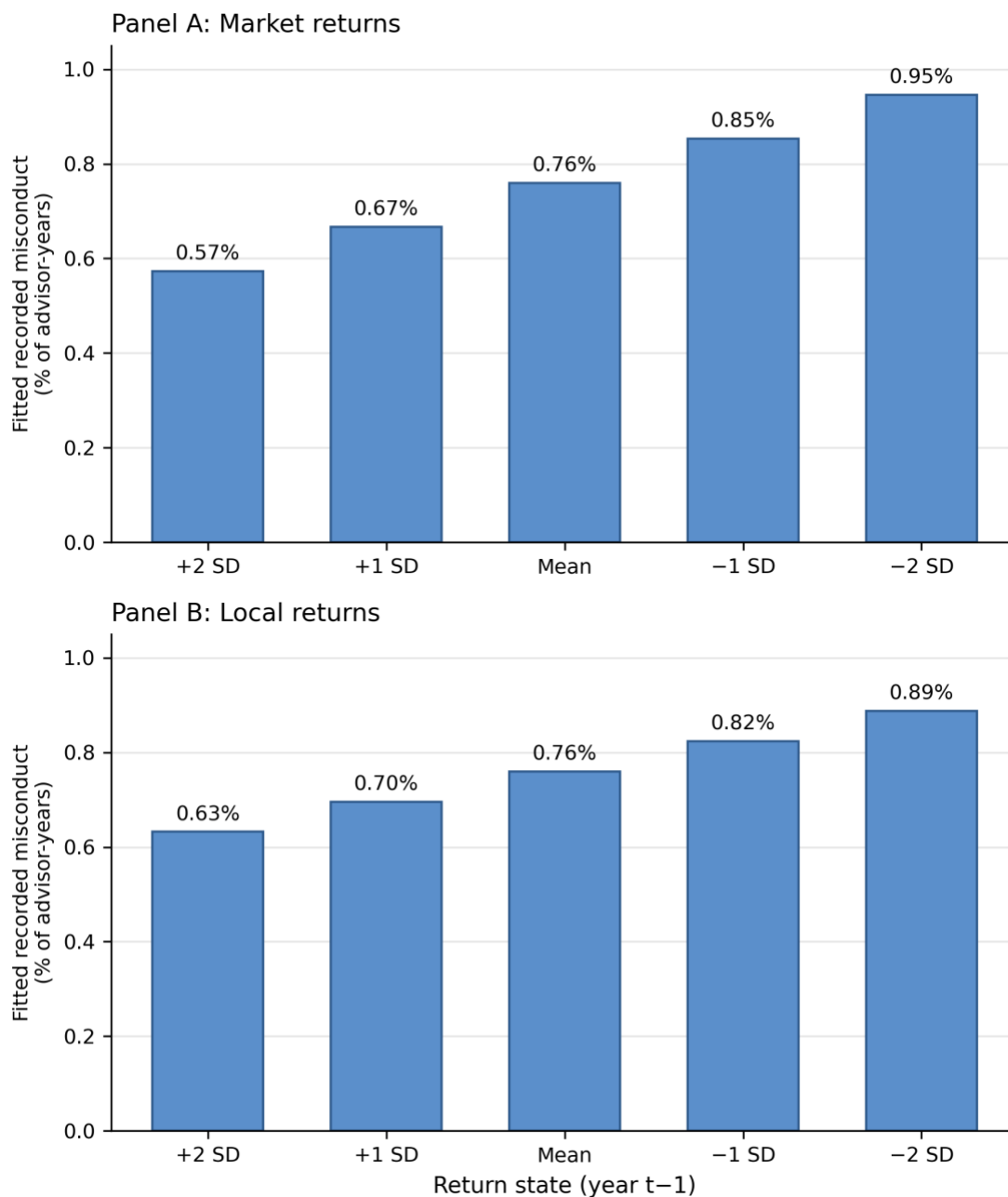


Table 1: Summary Statistics

This table reports summary statistics for the advisor-year panel. Market Return is the annual return on the CRSP value-weighted index. Local Return (Nonlocal Return) is the value-weighted annual return on the portfolio of firms headquartered within (beyond) 250 miles of the advisor's office. Misconduct equals one if a misconduct case, as defined in Section 1.1, was initiated against the advisor in year t (the complaint date). Misconduct (Judgment-Dependent) and Misconduct (Directly Verifiable) split these cases by complaint reason as in Table 2; the former is also reported excluding the Other/Unclassified category. Complaint (No Adverse Disposition) equals one if a customer complaint initiated against the advisor in year t was later denied, dismissed, withdrawn, or closed with no action; Complaint (Zero Settlement) equals one if a complaint in any of the 23 disclosure categories initiated in year t was resolved with a settlement of zero. Prior Misconduct equals one if the advisor had a misconduct case in the prior three years. Firm Size is the log number of advisors at the advisory firm, and Firm Misconduct is the share of the firm's advisors with misconduct over the prior three years. Series 63, Series 65, and Series 66 indicate that the advisor has passed the Uniform Securities Agent State Law, Uniform Investment Adviser Law, and Uniform Combined Law examinations; Female and White are demographic indicators. GDP Growth and Home-Price Growth are annual growth rates of county GDP per capita (BEA) and the county median home-price index (Zillow), and Log Population and Log Income are the logs of county population and average personal income (BEA). Fraud Exposure equals one if a firm headquartered in the advisor's state was named in an SEC Accounting and Auditing Enforcement Release within the past 1, 3, or 4 years. Madoff Exposure equals one if the advisor's county had at least 1, 5, or 10 identified Madoff victims, defined over the 3- or 5-year window following December 2008. Percentiles are omitted for indicator variables. Abbreviations: Mkt Ret, market return; JD, judgment-dependent; DV, directly verifiable; Disp., disposition; Exp., exposure; Madoff Exp. ($\geq k$, n yrs) denotes at least k victims over the n -year window.

Table 1 (Continued)

	N	Mean	SD	p25	Median	p75
Market Return	11,843,723	0.0840	0.1803	-0.0093	0.1254	0.1896
Local Return	11,843,723	0.0927	0.224	-0.0551	0.1297	0.2266
Nonlocal Return	11,843,723	0.0924	0.2218	-0.0677	0.1489	0.1979
Misconduct	11,843,723	0.0076	0.0868	0	0	0
Misconduct (JD)	11,843,723	0.0069	0.0826	0	0	0
Misconduct (JD, excl. Other)	11,843,723	0.0046	0.0678	0	0	0
Misconduct (DV)	11,843,723	0.0007	0.0256	0	0	0
Complaint (No Adverse Disp.)	11,843,723	0.0001	0.0102	0	0	0
Complaint (Zero Settlement)	11,843,723	0.0008	0.029	0	0	0
Prior Misconduct	11,843,723	0.0179	0.1324	0	0	0
Firm Size	11,843,723	1.9642	0.7735	1.3863	2.0794	2.5649
Series 63	11,843,723	0.7075	0.4549	0	1	1
Series 65	11,843,723	0.239	0.4265	0	0	0
Series 66	11,843,723	0.2266	0.4187	0	0	0
Female	11,843,723	0.2505	0.4333	0	0	1
White	11,843,723	0.8661	0.3406	1	1	1
Firm Misconduct	11,843,723	0.0205	0.0216	0.0029	0.0149	0.0311
GDP Growth	11,843,723	0.0089	0.0325	-0.0073	0.012	0.0281
Home-Price Growth	11,843,723	0.0379	0.0782	-0.0023	0.0418	0.0837
Log Population	11,843,723	13.6223	1.1606	13.0516	13.7473	14.2967
Log Income	11,843,723	10.9016	0.4523	10.5781	10.7999	11.1181
Fraud Exp. (4 yrs)	11,843,723	0.790	0.407	1	1	1
Fraud Exp. (1 yr)	11,843,723	0.633	0.482	0	1	1
Fraud Exp. (3 yrs)	11,843,723	0.758	0.428	1	1	1
Madoff Exp. (≥ 1 , 3 yrs)	2,011,366	0.5473	0.4978	0	1	1
Madoff Exp. (≥ 1 , 5 yrs)	3,339,021	0.548	0.4977	0	1	1
Madoff Exp. (≥ 5 , 5 yrs)	3,339,021	0.4381	0.4962	0	0	1
Madoff Exp. (≥ 10 , 5 yrs)	3,339,021	0.3869	0.487	0	0	1

Table 2: Types of Recorded Misconduct

This table reports the percentage of misconduct cases by complaint reason. Categories tagged (J) are judgment-dependent—identifying wrongdoing requires the investor to evaluate whether the advisor acted appropriately—and categories tagged (V) are directly verifiable; see Section 3.3. Other/Unclassified comprises cases whose complaint reason could not be determined from the disclosure text.

Complaint Reason	Share of Cases
Unsuitable (J)	15.3%
Misrepresentation (J)	8.02%
Unauthorized Activity (V)	5.60%
Omission of Key Facts (J)	1.39%
Fee/Commission Fraud (V)	4.77%
Fraud (V)	3.72%
Fiduciary Duty (J)	3.83%
Negligence (J)	2.84%
Risky Investment (J)	0.35%
Churning/Excessive Trading (V)	2.09%
Other/Unclassified	52.28%

Table 3: Market Returns and Recorded Advisor Misconduct

This table reports linear probability regressions in which the dependent variable equals one if a misconduct case was initiated against the advisor in year t (the complaint date, as defined in Section 1.1) and zero otherwise, multiplied by 100 so that coefficients are in percentage points. Market Return is the year $t-1$ return on the CRSP value-weighted index (the year- t return in column 4). Columns differ in the controls included, as indicated at the bottom of the table; all variables are defined in Table 1. t -statistics based on standard errors two-way clustered by year and county are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	1	2	3	4
	Market Return ($t-1$)	Market Return ($t-1$)	Market Return ($t-1$)	Market Return (t)
Market Return	-0.643*** (-13.93)	-0.515*** (-12.75)	-0.518*** (-11.38)	-0.541*** (-7.85)
GDP Growth		-0.292* (-1.90)	-0.338** (-2.01)	-0.160 (-0.32)
Home-Price Growth		-0.559*** (-4.56)	-0.599*** (-6.35)	-0.606*** (-3.30)
Log Population		0.008 (0.76)	0.003 (0.41)	0.003 (0.35)
Log Income		-0.526*** (-12.43)	-0.440*** (-12.64)	-0.441*** (-12.22)
Firm Misconduct			-0.138 (-0.28)	-0.914** (-2.07)
Prior Misconduct			5.112*** (34.12)	5.115*** (34.14)
Firm Size			-0.101*** (-11.82)	-0.100*** (-11.95)
Series 63			-0.014 (-0.97)	-0.013 (-0.89)
Series 65			0.548*** (11.27)	0.549*** (11.30)
Series 66			-0.032 (-0.96)	-0.035 (-1.03)
Female			-0.386*** (-11.39)	-0.386*** (-11.40)
White			0.002 (0.23)	0.002 (0.21)
County controls	No	Yes	Yes	Yes
Advisor/firm controls	No	No	Yes	Yes
Year FE	No	No	No	No
State FE	Yes	Yes	Yes	Yes
Advisory-firm FE	Yes	Yes	Yes	Yes
Observations	12,299,015	11,843,723	11,843,723	11,843,723
Adjusted R²	0.008	0.008	0.016	0.016

Table 4: Local Returns and Recorded Advisor Misconduct

This table reports linear probability regressions in which the dependent variable equals one if a misconduct case was initiated against the advisor in year t (the complaint date, as defined in Section 1.1) and zero otherwise, multiplied by 100 so that coefficients are in percentage points. Local Return is the value-weighted return on a portfolio of firms headquartered within 250 miles of the advisor's office, measured in year $t-1$ (year t in column 4); Nonlocal Return is the value-weighted return on all other firms. Columns differ in the controls included, as indicated at the bottom of the table; all variables are defined in Table 1. t -statistics based on standard errors two-way clustered by year and county are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	1	2	3	4	5
	Local Return ($t-1$)	Local Return ($t-1$)	Local Return ($t-1$)	Local Return (t)	Local and Nonlocal ($t-1$)
Local Return	-0.327*** (-3.50)	-0.295*** (-3.24)	-0.285*** (-3.25)	-0.469*** (-4.11)	-0.309*** (-2.86)
Nonlocal Return					-0.319 (-0.33)
GDP Growth		0.379** (2.18)	0.399** (2.37)	0.424*** (2.71)	0.398** (2.37)
Home-Price Growth		-0.617*** (-3.15)	-0.623*** (-3.71)	-0.628*** (-3.73)	-0.622*** (-3.73)
Log Population		-0.018* (-1.81)	-0.017** (-2.06)	-0.017** (-2.12)	-0.017** (-2.06)
Log Income		-0.197*** (-2.90)	-0.184*** (-3.38)	-0.183*** (-3.37)	-0.184*** (-3.38)
Firm Misconduct			-0.476 (-0.97)	-0.481 (-0.97)	-0.477 (-0.97)
Prior Misconduct			5.106*** (33.93)	5.105*** (33.92)	5.106*** (33.93)
Firm Size			-0.111*** (-13.09)	-0.111*** (-12.95)	-0.111*** (-13.07)
Series 63			-0.020 (-1.39)	-0.020 (-1.37)	-0.020 (-1.39)
Series 65			0.544*** (11.46)	0.544*** (11.47)	0.544*** (11.46)
Series 66			-0.027 (-0.86)	-0.027 (-0.87)	-0.027 (-0.86)
Female			-0.383*** (-11.46)	-0.383*** (-11.46)	-0.383*** (-11.46)
White			0.002 (0.23)	0.002 (0.24)	0.002 (0.23)
County controls	No	Yes	Yes	Yes	Yes
Advisor/firm controls	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Advisory-firm FE	Yes	Yes	Yes	Yes	Yes
Observations	12,299,015	11,843,723	11,843,723	11,843,723	11,843,723
Adjusted R²	0.009	0.009	0.016	0.016	0.016

Table 5: Corporate-Fraud Exposure and Recorded Advisor Misconduct

This table reports linear probability regressions in which the dependent variable equals one if a misconduct case was initiated against the advisor in year t (the complaint date, as defined in Section 1.1) and zero otherwise, multiplied by 100 so that coefficients are in percentage points. Fraud Exposure equals one if at least one firm headquartered in the advisor's state was named in an SEC Accounting and Auditing Enforcement Release within the past 4 years (columns 1–3), 1 year (column 4), or 3 years (column 5), following Giannetti and Wang (2016). Columns differ in the controls included, as indicated at the bottom of the table; all variables are defined in Table 1. t -statistics based on standard errors two-way clustered by year and county are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	1	2	3	4	5
	Past 4 years	Past 4 years	Past 4 years	Past 1 year	Past 3 years
Fraud Exposure,	0.055** (2.41)	0.044** (2.16)	0.041** (2.24)	0.027* (1.66)	0.038** (2.01)
GDP Growth		0.355** (2.03)	0.375** (2.22)	0.368** (2.19)	0.372** (2.18)
Home-Price Growth		-0.626*** (-3.40)	-0.632*** (-4.01)	-0.632*** (-4.04)	-0.630*** (-4.02)
Log Population		-0.017* (-1.77)	-0.017** (-2.01)	-0.017** (-2.01)	-0.017** (-2.01)
Log Income		-0.198*** (-2.92)	-0.185*** (-3.40)	-0.184*** (-3.38)	-0.185*** (-3.39)
Firm Misconduct			-0.471 (-0.96)	-0.465 (-0.94)	-0.471 (-0.95)
Prior Misconduct			5.106*** (33.95)	5.106*** (33.95)	5.106*** (33.95)
Firm Size			-0.111*** (-13.08)	-0.111*** (-13.11)	-0.111*** (-13.08)
Series 63			-0.020 (-1.39)	-0.020 (-1.39)	-0.020 (-1.39)
Series 65			0.544*** (11.46)	0.544*** (11.46)	0.544*** (11.46)
Series 66			-0.027 (-0.85)	-0.027 (-0.86)	-0.027 (-0.85)
Female			-0.383*** (-11.46)	-0.383*** (-11.46)	-0.383*** (-11.46)
White			0.002 (0.23)	0.002 (0.24)	0.002 (0.24)
County controls	No	Yes	Yes	Yes	Yes
Advisor/firm controls	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	No	No
Advisory-firm FE	Yes	Yes	Yes	Yes	Yes
Observations	12,299,015	11,843,723	11,843,723	11,843,723	11,843,723
Adjusted R²	0.009	0.009	0.016	0.016	0.016

Table 6: Madoff Exposure and Recorded Advisor Misconduct

This table reports linear probability regressions in which the dependent variable equals one if a misconduct case was initiated against the advisor in year t (the complaint date, as defined in Section 1.1) and zero otherwise, multiplied by 100 so that coefficients are in percentage points. Madoff-related cases are excluded. Madoff Exposure equals one if the advisor's county had at least 1, 5, or 10 identified Madoff victims, following Gurun, Stoffman, and Yonker (2018); the sample is restricted to cases initiated within 5 years (3 years in column 4) of the fraud's public revelation in December 2008. Because Madoff is a single event with county-level exposure, this design provides less identifying variation than the corporate-fraud shocks in Table 5. Columns differ in the controls included, as indicated at the bottom of the table; all variables are defined in Table 1. t -statistics based on standard errors two-way clustered by year and county are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	1	2	3	4	5	6
Victim threshold	≥1 victim	≥1 victim	≥1 victim	≥1 victim	≥5 victims	≥10 victims
Sample window	5 years	5 years	5 years	3 years	5 years	5 years
Madoff Exposure	0.072* (1.91)	0.098*** (2.88)	0.067** (2.32)	0.068* (1.86)	0.063** (2.01)	0.103*** (3.04)
GDP Growth		-1.026* (-1.93)	-0.859* (-1.85)	-0.547 (-1.44)	-0.856* (-1.83)	-0.843* (-1.80)
Home-Price Growth		-0.108 (-0.61)	-0.134 (-0.80)	0.128 (0.43)	-0.143 (-0.84)	-0.141 (-0.83)
Log Population		-0.030** (-2.00)	-0.023* (-1.69)	-0.009 (-0.55)	-0.023 (-1.64)	-0.034** (-2.29)
Log Income		-0.179*** (-2.59)	-0.180*** (-2.98)	-0.240*** (-3.55)	-0.184*** (-3.01)	-0.170*** (-2.77)
Firm Misconduct			-21.536*** (-15.98)	-49.235*** (-14.85)	-21.536*** (-15.98)	-21.544*** (-15.98)
Prior Misconduct			5.015*** (26.34)	5.177*** (23.06)	5.015*** (26.34)	5.014*** (26.35)
Firm Size			-0.146*** (-7.20)	-0.078*** (-4.08)	-0.146*** (-7.20)	-0.146*** (-7.21)
Series 63			-0.022 (-1.27)	-0.011 (-0.49)	-0.022 (-1.27)	-0.022 (-1.30)
Series 65			0.530*** (13.64)	0.638*** (12.75)	0.531*** (13.69)	0.530*** (13.67)
Series 66			-0.014 (-0.42)	-0.004 (-0.10)	-0.014 (-0.40)	-0.015 (-0.43)
Female			-0.409*** (-9.97)	-0.447*** (-8.79)	-0.409*** (-9.97)	-0.409*** (-9.96)
White			-0.007 (-0.44)	0.009 (0.41)	-0.007 (-0.43)	-0.007 (-0.43)
County controls	No	Yes	Yes	Yes	Yes	Yes
Advisor/firm controls	No	No	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Advisory-firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,135,543	3,059,433	3,059,433	1,831,980	3,059,433	3,059,433
Adjusted R²	0.008	0.008	0.016	0.015	0.016	0.016

Table 7: Judgment-Dependent versus Directly Verifiable Misconduct

This table reports linear probability regressions in which the dependent variable equals one if a judgment-dependent misconduct case (columns 1–2) or a directly verifiable misconduct case (columns 3–4) was initiated against the advisor in year t (the complaint date, as defined in Section 1.1), multiplied by 100 so that coefficients are in percentage points. Judgment-dependent cases comprise Unsuitable, Misrepresentation, Omission of Key Facts, Fiduciary Duty, Negligence, and Risky Investment, plus the Other/Unclassified category; directly verifiable cases comprise Unauthorized Activity, Fee/Commission Fraud, Fraud, and Churning/Excessive Trading (Table 2). Results are similar when the Other/Unclassified category is excluded (untabulated). Return is the year $t-1$ market return (columns 1 and 3) or local return (columns 2 and 4). All specifications include the full set of controls; fixed effects are indicated at the bottom of the table, and all variables are defined in Table 1. t -statistics based on standard errors two-way clustered by year and county are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Judgment-Dependent		Directly Verifiable	
	1	2	3	4
	Market Return	Local Return	Market Return	Local Return
Return	-0.490*** (-7.51)	-0.234*** (-2.96)	-0.051*** (-9.08)	-0.051*** (-2.70)
GDP Growth	-0.200 (-0.43)	0.294** (2.07)	0.040 (0.95)	0.105** (2.21)
Home-Price Growth	-0.637*** (-3.75)	-0.565*** (-3.67)	0.031 (1.63)	-0.058*** (-2.60)
Log Population	-0.002 (-0.30)	-0.018** (-2.42)	0.005*** (3.65)	0.001 (1.19)
Log Income	-0.383*** (-12.01)	-0.174*** (-3.59)	-0.058*** (-9.97)	-0.011 (-1.39)
Firm Misconduct	-0.465 (-1.19)	-0.196 (-0.44)	-0.449*** (-4.84)	-0.280*** (-2.76)
Prior Misconduct	4.484*** (42.48)	4.477*** (42.18)	0.631*** (12.55)	0.629*** (12.48)
Firm Size	-0.090*** (-11.22)	-0.099*** (-12.16)	-0.012*** (-7.48)	-0.012*** (-8.93)
Series 63	-0.011 (-0.77)	-0.015 (-1.14)	-0.003 (-1.09)	-0.005* (-1.87)
Series 65	0.495*** (11.71)	0.492*** (11.85)	0.054*** (7.86)	0.053*** (8.05)
Series 66	-0.022 (-0.73)	-0.016 (-0.57)	-0.013*** (-2.67)	-0.011** (-2.52)
Female	-0.352*** (-11.16)	-0.350*** (-11.20)	-0.034*** (-12.26)	-0.033*** (-12.54)
White	0.002 (0.32)	0.002 (0.33)	-0.001 (-0.29)	-0.001 (-0.26)
Year FE	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes
Advisory-firm FE	Yes	Yes	Yes	Yes
Observations	11,843,723	11,843,723	11,843,723	11,843,723
Adjusted R²	0.013	0.013	0.006	0.006

Table 8: Complaints Without Adverse Findings or Payments

This table reports linear probability regressions in which the dependent variable equals one if a customer complaint initiated against the advisor in year t was resolved as denied, dismissed, withdrawn, or closed with no action (No Adverse Disposition, columns 1–2), or a complaint in any of the 23 disclosure categories initiated in year t was resolved with a settlement of zero (Zero Settlement, columns 3–4), and zero otherwise. Return is the year $t-1$ market return (columns 1 and 3) or local return (columns 2 and 4). All specifications include the full set of controls; fixed effects are indicated at the bottom of the table, and all variables are defined in Table 1. t -statistics based on standard errors two-way clustered by year and county are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels. [NOTE to authors: please state whether the dependent variables in this table are multiplied by 100.]

	No Adverse Disposition		Zero Settlement	
	1 Market Return	2 Local Return	3 Market Return	4 Local Return
Return	-0.004** (-2.20)	-0.019** (-2.34)	-0.066*** (-6.27)	-0.067*** (-3.08)
GDP Growth	-0.032*** (-3.33)	0.006 (0.42)	-0.120 (-1.50)	0.104* (1.65)
Home-Price Growth	0.000 (0.05)	-0.021** (-2.43)	-0.148*** (-4.58)	-0.115*** (-2.72)
Log Population	0.001 (0.83)	-0.001 (-1.02)	0.003** (2.08)	-0.001 (-0.74)
Log Income	-0.016*** (-7.85)	-0.003 (-1.09)	-0.064*** (-10.13)	-0.008 (-1.05)
Firm Misconduct	0.119*** (11.32)	0.119*** (11.25)	-0.284*** (-3.24)	-0.059 (-0.51)
Prior Misconduct	-0.189*** (-5.98)	-0.037 (-1.32)	0.496*** (25.19)	0.494*** (25.09)
Firm Size	0.004*** (4.79)	0.004*** (5.80)	0.011*** (5.05)	0.011*** (5.39)
Series 63	0.001 (0.64)	-0.000 (-0.24)	0.006 (1.57)	0.004 (1.05)
Series 65	0.012*** (5.26)	0.012*** (5.34)	0.104*** (13.23)	0.103*** (13.57)
Series 66	-0.010*** (-6.06)	-0.009*** (-6.14)	0.030*** (4.97)	0.032*** (5.67)
Female	-0.006*** (-4.96)	-0.005*** (-4.96)	-0.046*** (-9.52)	-0.045*** (-9.56)
White	-0.001 (-0.88)	-0.001 (-0.87)	0.006** (2.32)	0.006** (2.33)
Year FE	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes
Advisory-firm FE	Yes	Yes	Yes	Yes
Observations	11,843,723	11,843,723	11,843,723	11,843,723
Adjusted R²	0.001	0.001	0.001	0.001

Table 9: Advisor Traits and Recorded Advisor Misconduct

This table reports linear probability regressions in which the dependent variable equals one if a misconduct case was initiated against the advisor in year t (the complaint date, as defined in Section 1.1) and zero otherwise, multiplied by 100 so that coefficients are in percentage points, on a shock interacted with advisor traits. The shock is the year $t-1$ market return (columns 1–3), the year $t-1$ local return (columns 4–6), or corporate-fraud exposure (columns 7–9); return shocks enter negatively (lower returns mean more misconduct) and fraud exposure enters positively. Panel A reports the shock, the interacted trait, and their interaction; all specifications also include the full set of controls (untabulated), with fixed effects indicated at the bottom of the table. Panel B reports, for each specification, the shock sensitivity without the trait (the shock coefficient), the sensitivity with the trait (the shock coefficient plus the interaction), and their ratio. All variables are defined in Table 1. t -statistics based on standard errors two-way clustered by year and county are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels. [NOTE to authors: please confirm which fraud-exposure horizon (1-, 3-, or 4-year) is used in columns 7–9.]

Panel A: Regression Estimates									
	1	2	3	4	5	6	7	8	9
	Market	Market	Market	Local	Local	Local	Corporate Fraud	Corporate Fraud	Corporate Fraud
Shock	-0.486*** (-10.59)	-0.262*** (-5.29)	-0.646*** (-10.67)	-0.244*** (-2.85)	-0.119 (-1.29)	-0.351*** (-3.82)	0.096*** (4.47)	0.048** (2.33)	0.140*** (5.30)
Prior Misconduct	5.277*** (31.74)	5.112*** (34.11)	5.112*** (34.14)	5.296*** (34.51)	5.108*** (33.90)	5.106*** (33.95)	4.220*** (26.00)	5.113*** (34.25)	5.115*** (34.18)
Series 65	0.548*** (11.26)	0.634*** (11.57)	0.547*** (11.25)	0.544*** (11.46)	0.603*** (11.41)	0.544*** (11.46)	0.549*** (11.24)	0.341*** (10.09)	0.550*** (11.20)
Female	-0.386*** (-11.40)	-0.386*** (-11.45)	-0.428*** (-11.06)	-0.383*** (-11.46)	-0.383*** (-11.47)	-0.406*** (-11.07)	-0.386*** (-11.39)	-0.387*** (-11.43)	-0.299*** (-15.62)
Shock × Prior Misconduct	-1.974*** (-5.15)			-1.757*** (-7.25)			1.030*** (4.30)		
Shock × Series 65		-1.103*** (-8.41)			-0.625*** (-8.60)			0.247*** (5.88)	
Shock × Female			0.505*** (8.83)			0.248*** (5.84)			-0.103*** (-2.68)
Year FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Advisory-firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,843,723	11,843,723	11,843,723	11,843,723	11,843,723	11,843,723	11,843,723	11,843,723	11,843,723
Adjusted R²	0.016	0.016	0.016	0.016	0.016	0.016	0.016	0.016	0.016

Table 9 Continued

Panel B: Economic Significance of the Interactions									
	1	2	3	4	5	6	7	8	9
Shock	Market	Market	Market	Local	Local	Local	Corporate Fraud	Corporate Fraud	Corporate Fraud
Interacted trait	Prior Misconduct	Series 65	Female	Prior Misconduct	Series 65	Female	Prior Misconduct	Series 65	Female
Sensitivity without trait	-0.486	-0.262	-0.646	-0.244	-0.119	-0.351	0.096	0.048	0.140
Sensitivity with trait	-2.460	-1.365	-0.141	-2.001	-0.744	-0.103	1.126	0.295	0.037
Multiplier (with/without)	5.1	5.2	0.2	8.2	6.3	0.3	11.7	6.1	0.3