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Is Fraud Contagious? Co-Worker Influence on Misconduct by Financial Advisors

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ABSTRACT

Using a novel data set of U.S. financial advisors that includes individuals' employment histories and misconduct records, we show that co-workers influence an individual's propensity to commit financial misconduct. We identify co-workers' effect on misconduct using changes in co-workers caused by mergers of financial advisory firms. The tests include merger-firm fixed effects to exploit the variation in changes to co-workers across branches of the same firm. The probability of an advisor committing misconduct increases if his new co-workers, encountered in the merger, have a history of misconduct. This effect is stronger between demographically similar co-workers.

Keywords: Financial advisors, Financial misconduct, Fraud, Social networks, Peer effects, Career networks

JEL Codes: D18, G20, G24, G28, K22

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Is financial misconduct contagious? The media and a large academic literature document widespread financial misconduct in many different contexts,¹ and both academics and policy makers have stressed the importance of understanding why financial misconduct occurs and how it spreads. In this paper we show that financial misconduct is contagious, that is, that the ethical conduct of many individuals is malleable, with their misconduct behavior influenced by the misconduct behavior of their co-workers. Specifically, we show that following a shock to their co-worker group, the misconduct behavior of financial advisors² becomes more like that of their new co-workers.

Co-workers – advisors who work in the same branch of a firm – can influence each other’s misconduct behavior in several ways. First, co-worker influence may occur through social learning (e.g., Banerjee, 1992; Ellison and Fudenberg, 1995). By interacting with their co-workers, advisors can learn about the profitability of misconduct as well as techniques for committing misconduct or reducing the risk or detection (e.g., Sah, 1991). Second, co-worker influence may occur through social utility, where an advisor’s utility from an action depends on the actions of his peers. By interacting with their co-workers, advisors can absorb the branch’s culture and social norms, and conform their behavior accordingly (e.g., Bernheim, 1994). Such social norms may be directly related to misconduct, but may also include indirectly related social norms (e.g., a branch culture of intense competition or a lack of respect for clients). Advisors may also mimic their co-workers’ behavior due to concerns about their relative position (e.g., Bursztyn, Ederer, Ferman, and Yuchtman, 2014; DeMarzo, Kaniel, and Kremer, 2004).

Empirically identifying peer effects such as co-worker influence on misconduct is challenging.

¹Dyck, Morse, and Zingales (2014) summarize the extent and cost of corporate fraud, Griffin and Maturana (2016) and Piskorski, Seru, and Witkin (2015) document misconduct for residential mortgages, Duffie and Stein (2015) review LIBOR manipulation, and Dimmock and Gerken (2012) document fraud by asset managers.

²Throughout the paper “financial advisor” and “advisor” refer to individuals who register with FINRA as “registered representatives.” This includes individuals commonly referred to as brokers or financial planners.

First, co-worker groups are endogenously formed, as individuals select to work with others who share similar characteristics. Egan, Matvos, and Seru (2018), for example, show that dishonest financial advisors choose to work together. Second, within a firm, co-workers face similar economic incentives and monitoring procedures. Distinguishing co-worker influence from selection, common incentives, shared supervision, or the external environment is challenging and requires very specific data features.

We test whether co-workers influence the propensity to commit misconduct using data on financial advisors for several reasons. First, data on the financial advisory industry have unique features that allow us to overcome the identification challenges above. Second, the financial advisory industry is economically important. In 2013 for instance, there were more than 635,837 financial advisors in the U.S.,³ and a sizable majority of U.S. investors used an advisor for investment decisions (see Hung et al., 2008). Third, the compensation structures used in this industry create incentives for misconduct, as the models of Inderst and Ottaviani (2009) and Stoughton, Wu, and Zechner (2011) show. Consistent with the implications of these models, misconduct occurs frequently in this industry: during the 1999-2011 period our sample includes more than 26,000 substantiated cases of misconduct.⁴

We define an advisor's co-workers as the other advisors employed at the same branch of the firm at the same time. To avoid the problem of endogenous co-worker selection, we use changes to co-worker groups caused by mergers of financial advisory firms. Of course, while merger decisions are themselves endogenously determined at the firm level, our data allow us to overcome this issue by observing misconduct at the individual advisor rather than firm level. In particular, because we observe information about advisory firms, the geographically distinct branches of each firm, and the individual advisors of each branch, our empirical

³See <http://www.finra.org/Newsroom/Statistics/>.

⁴The cases of misconduct in our study are substantiated customer complaints related to financial misconduct by advisors, most of which fall under the concept of civil fraud.

tests focus on the within-firm variation by including merger-firm fixed effects. The key to our identification is the fact that mergers occur at the firm level, but changes to co-worker groups occur within a firm at the branch level. Our empirical tests thus exploit across-branch variation and the impact of combining branches during a merger, while removing all effects at the firm level, which addresses the most obvious endogeneity concerns.

To illustrate, consider a merger between Target Firm, with branches in Atlanta and Boston, and Acquirer Firm, with branches in Atlanta, Boston, and Chicago. Suppose that the advisors at the Atlanta branch of Target Firm have a history of misconduct, while the advisors at all other branches of both firms have clean histories. When the firms merge, the branches in Atlanta and Boston are combined but the branch in Chicago remains unchanged. Thus, following the merger, there are changes to the co-worker groups at the Atlanta and Boston branches (of both Target Firm and Acquirer Firm), but only the co-worker group of advisors from the Atlanta branch of Acquirer Firm has changed to now include individuals with a history of misconduct (the advisors from the Atlanta branch of Target Firm). The empirical question, then, is whether the advisors from the Atlanta branch of Acquirer Firm are now more likely to commit misconduct *relative to advisors from the Boston branch of Acquirer Firm*. The comparison between the two branches removes any variation common to all branches of the pre-merger firm and any common time-series changes in the propensity to commit misconduct.

Our measure of co-worker behavior avoids the reflection problem described in Manski (1993) by observing co-worker and individual behavior in separate time periods. We define *Introduced Branch* as the individuals that an advisor encounters due to the merger (i.e., individuals who worked for the other firm before the merger but work in the combined branch after). Our key measure of co-worker behavior, *Introduced Branch High Misconduct*, captures misconduct committed by *Introduced Branch* co-workers *before* the merger, and thus before the advisor meets the co-worker group. We use this measure to explain individual misconduct

committed *after* the advisor meets these new co-workers in the merger.

We find evidence of co-worker influence on misconduct committed by financial advisors. Controlling for merger-firm fixed effects and using changes to an advisor’s co-workers due to a merger, we show that an advisor is 37% more likely to commit misconduct if his *Introduced Branch* co-workers have a history of misconduct. We further show that the results are robust to using observations at the branch rather than individual level, and that the magnitude of the effect is similar for advisors from the target firm and from the acquirer firm.

Additional tests show that co-worker influence is asymmetric: there is evidence of contagion in misconduct but no significant evidence of contagion in good conduct. These results help distinguish the mechanism through which peers influence misconduct. This asymmetry is consistent with social learning about misconduct. For example, an advisor can learn about misconduct from meeting dishonest co-workers, but does not forget information because of meeting honest co-workers. In contrast, mechanisms based on social utility are more consistent with symmetric effects. For example, an advisor with a preference for conforming to social norms would conform to either a norm for honesty or a norm for dishonesty.

The key assumption for our identification strategy is that the merger-firm fixed effects eliminate selection bias. That is, conditional on a merger occurring, the firm does not systematically select to combine branches or advisors based on characteristics that are correlated with misconduct. We conduct several tests that suggest selection bias does not drive the results. First, we show that, once merger-firm fixed effects are included, misconduct at an advisor’s *Introduced Branch* is unrelated to that advisor’s misconduct *before the merger occurred*. Second, we undo any possible selection by firms or individuals by using simple rules based on distance to “assign” advisors to the *Geographically Closest Branch* of the other firm involved in the merger. The results in this alternative sample are similar to those in the main sample. Third, we limit the sample to include only acquirer-target branch pairs for which there is a single target branch and a single acquirer branch within a specific geographic area

(such as a 50 mile radius), which removes the ability of the merging firms to assortatively combine branches. The results based on this limited sample are similar to those for the main sample. Thus, there is no evidence that selection drives the results.

Prior studies such as Bertrand, Luttmer, and Mullainathan (2000) and Pool, Stoffman, and Yonker (2015) show that social influence is typically stronger between individuals with the same ethnicity. Using advisor ethnicity, we show that co-worker influence on misconduct is stronger for advisors who share the same ethnicity as an *Introduced Branch* co-worker with a history of misconduct.

In a robustness test, we estimate more restrictive models to ensure that our main results are not driven by exposure to geographic variation (e.g., biases occurring because the merged branches are exposed to the same local economic or demographic conditions). These models include merger-firm-state-year or merger-firm-county-year fixed effects, and thus these tests compare advisors to individuals from different branches of the same pre-merger firm who work in the same state or county (and in the same year). Even with these more restrictive fixed effects, the results are similar to those found using the main specification.

A potential concern with our identification is that the relation between an advisor's misconduct and *Introduced Branch High Misconduct* could occur because, following a merger, the advisor is exposed to the same branch supervisor as the *Introduced Branch* co-workers. Supervisors can influence misconduct by setting the general ethical tone for the branch or by their implementation and enforcement of monitoring procedures. Accordingly, we conduct tests designed to remove the influence of supervisors. To do so, we identify the subset of advisors who do not change supervisors in the merger and do not encounter supervisors from the other firm. Even for this no-change-in-supervisor subsample, we find that post-merger misconduct is positively related to *Introduced Branch High Misconduct*. This result suggests that *peer* co-workers influence misconduct behavior.

Our paper relates to the literature, beginning with Glaeser, Sacerdote, and Scheinkman

(1996), that finds evidence of peer effects in street crime. Ballester, Calvó-Armengol, and Zenou (2010) and Bayer, Hjalmarsson, and Pozen (2009) argue that peer effects are particularly important in the context of crime because no formal schools teach the necessary skills, in which case social interaction is the most plausible means through which criminal behavior proliferates. Similarly, in our setting of financial misconduct committed by white-collar professionals, no formal training programs teach advisors how to commit misconduct. Due to the need for secrecy, misconduct techniques and social norms can only be transmitted through informal channels such as social interactions.

Our paper contributes to the literature that find evidence of commonality in misconduct among connected firms, such as Bizjak, Lemmon, and Whitby (2009), Brown (2011), Chiu, Teoh, and Tian (2013), Kedia, Koh, and Rajgopal (2015), and Parsons, Sulaeman, and Titman (2016). These papers observe misconduct by firms; in contrast, we observe misconduct by individual employees. A key advantage of our more granular data is that they allow us to identify causal peer effects in financial misconduct. Establishing that there are peer effects in financial misconduct has important policy implications for both the type and the severity of the optimal penalties (Ballester, Calvó-Armengol, and Zenou, 2010; Glaeser, Sacerdote, and Scheinkman, 1996; Kahan, 1997; Posner, 1997).

Finally, our paper also contributes to the literature on the financial advisory industry. Despite the economic importance of this industry, empirical studies generally find that advisors harm performance by steering clients towards high-fee products (e.g., Bergstresser, Chalmers, and Tufano, 2009; Chalmers and Reuter, 2015; Hackethal, Haliassos, and Jappelli, 2012; Hoechle, Ruenzi, Schaub, and Schmid, 2014; Mullainathan, Noeth, and Schoar, 2012). The paper most directly related to ours is Egan, Matvos, and Seru (2018), who use similar data and show that misconduct by advisors is common and that certain firms specialize in misconduct. Given these dismal empirical findings, Gennaioli, Shleifer, and Vishny (2015) and Georgarakos and Inderst (2014) argue that a key function of advisors is to convince

households to trust the financial system enough to participate in the market. Giannetti and Wang (2016) and Gurun, Stoffman, and Yonker (2018) show that even indirect exposure to misconduct destroys trust and results in nonparticipation. Thus, misconduct by advisors harms households’ financial well-being and potentially raises the cost of capital. In this paper, we highlight one of the causes of misconduct by advisors. Understanding the causes of misconduct is important, so firms can design effective internal controls and regulators can limit the harm from misconduct.

The remainder of the paper is organized as follows. In Section I we discuss our data and variables. In Section II we examine co-worker influence on misconduct. In Section III we investigate the issue of selection bias. In Section IV we explore similarity and co-worker influence on misconduct. In Section V we address the issue of confounding variation. In Section VI we explore supervisors and co-worker influence on misconduct. Finally, in Section VII we conclude.

I. Data and Variables

Our primary data on financial advisors come from historical Form U4 filings, which provide information on advisors’ characteristics, employment histories, and misconduct. All registered representatives (the legal term for what we call “financial advisors”) in the U.S. are registered with FINRA when their employer files a Uniform Application for Securities Industry Registration or Transfer (Form U4). An updated Form U4 must be filed following any material change in an advisor’s information (including change in employment or disclosure of misconduct). The Form U4 data are collected in the Central Registration Depository (CRD), which assigns each advisor a unique identification number that remains constant throughout the advisor’s career. FINRA discloses much of this information to the public through its BrokerCheck system and also shares the information with state regulatory agencies.

We obtain the Form U4 data from Meridian-IQ’s Registered Representative Database and from Freedom of Information Act requests to state regulators. Meridian-IQ also obtain their data from state regulators, but because not all state regulators supply Meridian-IQ with a complete set of variables, our universe of financial advisors consists of those registered in 32 states. A large majority of advisors register in multiple states,⁵ and so we have data for many advisors in the states that do not supply information. The only advisors we do not observe are those who never register in a reporting state (approximately 22% of advisors in 2012). Our merger sample starts in August 1999, the month in which the CRD transitioned to an Internet-based system. We conclude our merger sample in 2011 because we require at least three years after the merger to measure misconduct (see Section I.C). During the sample period, we have data for 522,363 unique individuals. We also obtain data directly from cooperating state securities regulators⁶ and cross-validate the Meridian-IQ data. As some states do not provide Meridian-IQ with certain personal information (e.g., date of birth), we fill in missing variables using data provided by state regulators. Our data set, summarized in Table I, is generally similar to that of Egan, Matvos, and Seru (2018), who provide a detailed summary of the financial advisory industry in their study of the labor market for advisors who commit misconduct.

A. Financial Advisory Firms, Branches, and Financial Advisors

During our sample period, the number of registered broker-dealer firms ranges from 5,482 in 1999 to 4,456 in 2011. But because our identification strategy is based on mergers, our analyses focus on the firms involved in one of the 477 mergers that meet our criteria during the sample period (and that have enough variation to allow our fixed effects strategy).

⁵This is because an advisor must be registered in each state in which he does business.

⁶We thank the securities regulators of Alabama, Arkansas, California, Connecticut, Florida, Kentucky, Maryland, Michigan, Minnesota, Nevada, New York, Pennsylvania, Texas, Vermont, and Washington for providing data.

A financial advisory firm can have one or more branch offices (defined by FINRA as any distinct business address where “one or more associated persons of a member regularly conduct the business of effecting any transactions in, or inducing or attempting to induce the purchase or sale of, any security, or that is held out as such”). Using the unique CRD branch number, we identify each advisor’s co-workers as the other advisors who work at the same branch 30 days before the merger. The majority of firms have only a single branch (but single-branch firms employ only a small minority of advisors). To enter our merger sample, however, we require that both the target and the acquirer have multiple branches. As a result, the firms in our merger sample are substantially larger than the median firm.

Following the merger, on average 17.3% (17.1%) of the target (acquirer) advisors work at a branch with at least one acquirer (target) advisor. The remaining advisors continue to work at the same branch without meeting anyone from the other firm or leave the firm (12.9% of target advisors and 0.3% of acquirer advisors leave the firm). Starting with the full sample, we retain the set of target and acquirer advisors that are introduced due to the merger. For an average (median) merger, this set includes 124.2 (22) target advisors and 192.0 (35) acquirer advisors mixed across 11.6 (3) unique post-merger branches.

As Manski (2000) discusses, it is important to accurately classify group membership. A branch of an advisory firm provides a clearly defined group, which is well suited for identifying co-worker influence. In U4 filings, each advisor is required to list the address of his office of employment as well as its unique official CRD branch number. All advisors with the same CRD branch number share a single office address, and the average branch size is quite small, with an average of 16.0 advisors at an acquirer branch, 7.7 advisors at a target branch, and 27.2 advisors at the combined branch.⁷ Given the shared office location and the small average

⁷The average post-merger branch is slightly larger than the sum of the average pre-merger branches, as in some cases multiple branches (usually from the target firm) are merged with a single branch of the other firm.

branch size, it is reasonable to assume that nearly all group members know and interact with each other. Several prior studies also define group membership based on within-firm groups of co-workers (e.g., Duflo and Saez, 2002, 2003; Hvide and Östberg, 2015; Ichino and Maggi, 2000). We are the first study, however, to identify shocks to groups based on the combining of branches that occurs following mergers.

The characteristics of the financial advisors in our merger sample are similar to those in the full sample. As Table I shows, in the merger sample the average advisor is about 40 years old and has about 10 years of industry experience. (In the full sample, the average advisor is 42.6 years old and has 11.1 years of experience.) Compared to acquirer advisors, target advisors are slightly older, more experienced, and have more assets under management.⁸

B. Mergers of Financial Advisory Firms

We identify mergers of financial advisory firms using advisors’ employment histories in the Form U4 data. Each advisor must disclose the reason for leaving each prior job. If the advisor left because his firm (the target) was acquired, the reason is given as “Mass Transfer.”⁹ We use mass transfers to identify mergers and classify firms as targets or acquirers. When we cross-check our merger sample with news stories and with the mergers listed in Hong and Kacperczyk (2010), we find that this method reliably identifies and classifies merging firms.

We define the merger date as the date of the earliest mass transfer between a target-acquirer pair (many mergers involve several mass transfers at slightly different dates). To avoid errors due to variation in filing dates (many target advisors appear to be employed at both the target and the acquirer for several weeks), we measure pre-merger employment 30

⁸Meridian provides the assets under management (AUM) for 58.2% of the financial advisors in the sample, but this variable is available only as a cross-section observed at the end of the sample period.

⁹The mass transfer program simplifies the bulk transfer of registration data following a merger, consolidation, or reorganization. A mass transfer must involve at least 50 individuals to qualify, and thus our merger sample implicitly conditions on the target firm having at least 50 employees.

days before the merger date. Because we use the earliest mass transfer date and some advisors are not reported as transferred for several weeks, we measure post-merger employment 100 days after the merger. We define the pre-merger period as the three-year period ending 30 days prior to the merger date, and the post-merger period as the three-year period beginning 100 days after the merger date.¹⁰ To avoid survival bias, the sample retains each advisor who joined the merged branch even if he leaves the branch during the post-merger period.

C. Financial Misconduct: Measurement and Definitions

We identify cases of financial misconduct using the mandatory disclosures made in Form U4. These disclosures include customer complaints, regulatory actions, civil cases, criminal charges, and terminations; however, for reasons explained shortly, we limit our sample to customer-initiated complaints.¹¹ For a subset of the data, we observe the reason for the complaint. The most common complaints include: unsuitability, unauthorized trading, churning, and misrepresentation or omission.¹² FINRA Rule 3070 requires firms to report all written customer complaints to the appropriate regulator through the CRD system; customers can also file complaints directly with FINRA or their state regulator.

If the complaint alleges damages of \$5,000 or more, it appears in the advisor’s publicly available disclosures and is labeled as “Pending.” A pending complaint is resolved in one of three ways: (1) through arbitration, where the case is denied, dismissed, or upheld, (2)

¹⁰In our main analyses, we include observations from firms with multiple mergers even if the pre- or post-periods overlap. Internet Appendix Table IAI reports results from subsamples of only those branches and firms without any overlap. The results are similar to those in the main specification. The Internet Appendix is available in the online version of the article on the *Journal of Finance* website.

¹¹Egan, Matvos, and Seru (2018) define misconduct to include customer complaints, regulatory actions, civil cases, criminal charges, and terminations. Internet Appendix Table IAI shows that if we use their broader definition of misconduct, the results are similar to those reported in the main specification.

¹²Unsuitability occurs when an advisor recommends assets that are unsuited for the client’s risk tolerance or goals. Unauthorized trading occurs when an advisor fails to obtain permission for a trade or acts against a client’s instructions. Churning is excessive trading of securities to generate commissions. Misrepresentation or omission occurs when an advisor knowingly misstates or omits material information.

through a negotiated settlement, or (3) through the customer withdrawing the complaint. Nearly all cases are resolved within two years.¹³ Although the Meridian-IQ data includes complaints filed through 2014, our main sample includes only mergers that occurred before the end of 2011 to ensure we can screen out cases that are still pending.

From the resolved complaints, we identify cases of misconduct as follows. For cases resolved through arbitration, we include only those that are upheld (i.e., we exclude those that are denied or dismissed). For cases that are withdrawn or settled, the financial advisor can cease disclosing the case if the settlement was less than \$10,000 (before May 18, 2009) or less than \$15,000 (on or after May 18, 2009).¹⁴ We exclude cases that the advisor ceases to disclose, as these may be merely “nuisance” settlements rather than legitimate complaints. Finally, FINRA Rule 2080 allows an advisor to request that a complaint be expunged if: “(1) the claim, allegation or information is factually impossible or clearly erroneous; (2) the registered person was not involved in the alleged investment-related sales practice violation, forgery, theft, misappropriation or conversion of funds; or (3) the claim, allegation or information is false.” If the request is approved by a FINRA arbitration panel and confirmed by a court order, the advisor may cease to disclose the case. After applying these screening rules, the remaining cases either are upheld in arbitration or involve a significant monetary payment and have not been expunged.¹⁵ These screening rules should therefore eliminate spurious complaints and “nuisance” settlements paid only to avoid legal fees.

As stated above, we identify misconduct using only customer complaints. That is, we do not use disclosed events that were initiated by regulators or financial advisory firms.

¹³The median complaint is resolved in less than six months, with over 90% of complaints resolved within two years. We thank the Attorney General’s Office of Florida for providing the data used to calculate resolution times.

¹⁴Internet Appendix Table IAI shows that the results are stable across subperiods.

¹⁵Internet Appendix Table IAI shows that our results are similar if we use all customer complaints, instead of the screened version defined in this section. The results are also robust to excluding all complaints settled for less than \$50,000.

Regulator- or firm-initiated events could be correlated at the branch level (e.g., because of branch-level regulatory examinations), which could lead to the spurious appearance of co-worker similarity. Customer complaints are less likely to be subject to such a bias. Using customer complaints also reduces the likelihood of bias due to undetected cases of misconduct. Clearly, not all cases of misconduct are detected. This will add noise to our tests, but will only bias the results if (1) the detection rate changes following a merger, (2) there is across-branch variation in these changes, and (3) this across-branch variation is correlated with misconduct at the *Introduced Branch*. It is not obvious why such a correlation pattern would occur; neither FINRA nor state regulators provide publicly accessible information at the branch level.

D. Financial Misconduct: Variable Definitions and Summary Statistics

Using the definition of misconduct described above, we create two variables: *Pre-Merger Individual Misconduct* is an indicator equal to one if a (substantiated) complaint was filed against the advisor during the three-year period ending 30 days before the merger, and *Post-Merger Individual Misconduct* is an indicator equal to one if a (substantiated) complaint was filed against the advisor during the three-year period beginning 100 days after the merger.¹⁶ The post-merger period begins 100 days after the merger to avoid cases that are filed after the merger but that occurred before the merger; Internet Appendix Table IAI shows that the results are similar if the post-merger period begins two years after the merger. Using data available only to FINRA employees, Qureshi and Sokobin (2015) find that the lag between actual misconduct and the associated complaint is usually quite short.

Panel A of Table II shows that 1.02% of advisors commit misconduct during the pre-merger period and 1.40% commit misconduct during the post-merger period. This increase

¹⁶Internet Appendix Table IAIII shows that the results are similar if we use windows of one, two, five, or 10 years.

in misconduct between the pre-merger and post-merger periods is consistent with a general upward trend in the overall (i.e., non-merger) sample and is not specific to the merger sample.

For each advisor, we identify two co-worker groups. The *Original Branch* co-workers are the advisor’s co-workers at the pre-merger branch as of 30 days before the merger date. Note that an advisor is not included in his own *Original Branch* co-worker group, as this would create a mechanical statistical bias (Manski, 1993). The *Introduced Branch* co-workers are the new co-workers that the advisor encounters due to the merger (i.e., employees of the other firm who, prior to the merger, worked at the branch that the advisor is merged into). Having defined these groups, we then measure the group members’ misconduct during the pre-merger period.

For each of the two co-worker groups, we create a *High Misconduct* indicator variable. Within each co-worker group, we calculate the proportion of advisors for whom *Pre-Merger Individual Misconduct* is equal to one. If this proportion is greater than 1.23% (the full-sample average), we set *High Misconduct* equal to one. Internet Appendix Table IAIV shows that our results are similar for alternative definitions of the co-worker misconduct variables: (*Misconduct Dummy* is equal to one if any member of the co-worker group has a history of misconduct; *Misconduct Rate* is the percentage of group members with a history of misconduct; *Misconduct 5% Cutoff* uses a cutoff of 5% instead of 1.23%).¹⁷

As shown in Panel B of Table II, *Original Branch High Misconduct* is equal to one for 12.5% of the pre-merger branches and *Introduced Branch High Misconduct* is equal to one for 14.7% pre-merger branches.¹⁸ The key independent variable, *Introduced Branch*

¹⁷Although simple, *Misconduct Dummy* has the disadvantage of a mechanical positive relation with the number of advisors in the branch, and *Misconduct Rate* has the disadvantage that it is sensitive to branch size for small branches. *High Misconduct* has only a small correlation with *Introduced Branch* size ($\rho = 0.085$).

¹⁸The variables *Original Branch High Misconduct* and *Introduced Branch High Misconduct* differ slightly for two reasons. First, in some cases two branches (usually from the target) are merged with a single branch of the other firm. Second, each individual’s own history of misconduct is excluded when calculating his *Original Branch High Misconduct*, while no such exclusion occurs in calculating *Introduced Branch High Misconduct*.

High Misconduct, has a standard deviation of 0.31 and a within-standard deviation of 0.24, indicating that sufficient variation remains after including merger-firm fixed effects. Indeed, the between-firm standard deviation is 0.16, indicating that there is greater variation across advisors within a firm than there is across different firms.

Panel C of Table II presents summary statistics for the settlements and awards. The average payment is \$430,338 and the median is \$50,000. Note that for 7.5% of the cases we do not observe the amount. The settlements and awards generally compensate the customer only for actual losses and do not include punitive damages or compensation for emotional distress.

II. Co-Worker Influence on Misconduct

In his classic paper, Manski (1993) discusses three major difficulties in identifying social influence: (1) selection, (2) correlated environment, and (3) the reflection problem.

The advisors in our sample are not randomly assigned to branches. As a result, behavioral similarity within a branch could merely reflect similar individuals selecting to work together. Thus, identifying co-worker influence requires an exogenous change to an advisor’s co-workers. We use changes caused by mergers of financial advisory firms. Of course, mergers occur for endogenous reasons, potentially even for reasons related to misconduct, but these reasons operate at the firm level, not at the branch or individual level. In our tests, we include merger-firm fixed effects, which eliminate variation at the firm level, including the (endogenous) reasons for the merger. Our analyses, therefore, use only the residual variation that remains at the branch or individual level. This removes the effect of selection, subject to the assumption that firms do not systematically make merger decisions based on within-firm, branch-specific characteristics that are correlated with misconduct. Because this assumption is critical for our analyses, in Section III we test it and find no evidence of selection bias after including

merger-firm fixed effects. The merger-firm fixed effects also remove any factors common to the institutional practices of all advisors at a firm, such as from formal oversight procedures, commission schedules, firm culture, etc.

Co-workers typically share a similar economic environment. If they respond in similar ways to common shocks, this could falsely create the appearance of co-worker effects. Because we regress post-merger behavior on pre-merger behavior, the observed relation cannot be caused by common shocks occurring after group formation. In the tests reported in this paper, we focus on filtering out the effect of common shocks rather than explaining them. In additional tests discussed in the Internet Appendix we further explore the issue of sharing a common environment and facing common shocks. The results, reported in Internet Appendix Table IAXII, suggest that common shocks have large effects on misconduct.

The reflection problem of Manski (1993) refers to the fact that, during any given time period, the average behavior of a group is a mechanical function of the behavior of the individual group members. In our study, the key measure of group behavior, *Introduced Branch High Misconduct*, avoids this mechanical relation. First, an advisor’s own behavior is not included in the measurement of group behavior. Second, we measure *Introduced Branch High Misconduct* before the merger (i.e., before the advisor meets the *Introduced Branch* co-workers) and use this to explain individual behavior after the merger. This approach follows Ahern, Duchin, and Shumway (2014), Lerner and Malmendier (2013), Sacerdote (2001), and Shue (2013), who also avoid the reflection problem by measuring group and individual behaviors in separate periods.

A. *Introduced Branch Co-Workers and Misconduct*

In this subsection, we examine whether misconduct by financial advisors during the post-merger period is related to misconduct committed by *Introduced Branch* co-workers during the pre-merger period. We estimate a conditional logit model in which there is one observation

per advisor-merger. Specifically, we use the following specification:

$$Pr(y_{i,m} = 1|x_{i,m}) = F(\beta \cdot \textit{Introduced Branch High Misconduct}_{i,m} + \mathbf{X}_{i,m} \cdot \sigma + \alpha_{m,f}), \quad (1)$$

where $y_{i,m}$ is an indicator equal to one if financial advisor i commits misconduct in the three-year period following merger m , $F(\cdot)$ indicates the logit function, *Introduced Branch High Misconduct* $_{i,m}$ equals one if an above-average fraction of the advisor's new co-workers at the merged branch committed misconduct in the pre-merger period, $\mathbf{X}_{i,m}$ is a vector of control variables, and $\alpha_{m,f}$ indicates a separate fixed effect for each merger-firm combination (separate fixed effects for the target and the acquirer in each merger). Note that the merger occurs at a fixed point in time, and thus the merger-firm fixed effects subsume any time-period-specific effects. Because we estimate the conditional logit model of Chamberlain (1980), the $\alpha_{m,f}$ coefficients are conditioned out of the model and are not estimated. For ease of interpretation, we report odds ratios instead of coefficients. The significance tests reported in Table III are based on standard errors clustered by merger.

All specifications reported in Table III control for (1) *Original Branch High Misconduct*, which equals one if an above-average fraction of the individuals in the advisor's pre-merger branch committed misconduct in the pre-merger period, and (2) *Pre-Merger Individual Misconduct*, which is set to one if the advisor committed misconduct in the pre-merger period. The specifications also control for advisor age, experience, gender, and assets under management. We also control for whether the advisor's pre-merger branch was the firm's main office (headquarters), an indicator for a solo-person branch, and $\log(\textit{Original Branch Size})$ and $\log(\textit{Introduced Branch Size})$, which are the natural logarithms of the number of co-workers in the advisor's pre-merger branch and the branch into which the advisor is merged, respectively. In addition, the specifications control for characteristics of the area around the branch: the natural logarithm of population, five-year population growth, and

population density, which are taken from the American Community Survey and are measured at the county level. The final control, *Political Corruption*, is the state-level corruption rate based on federal convictions of politicians as reported in Glaeser and Saks (2006), which Parsons, Sulaeman, and Titman (2016) show is related to financial statement misreporting. We also include indicator variables for missing demographic, local, and political corruption data.¹⁹

Column (1) of Table III reports an odds ratio of 1.370 for *Introduced Branch High Misconduct*, which is significant and greater than one. Given the merger-firm fixed effects, the result can be interpreted as follows. Suppose that before a merger two identical advisors work at different branches of the same firm, but after the merger one advisor is merged into a new branch with a history of high misconduct and the other is merged into a new branch with a history of low misconduct. The advisor merged into the branch with a history of low misconduct is 37% more likely to commit misconduct in the next three years *relative to the other advisor*. Given that the baseline misconduct rate is 1.4%, a 37% increase implies a 0.52 percentage point increase in the probability of misconduct. Following the method of Glaeser, Sacerdote, and Scheinkman (2003), this implies a social multiplier of 1.59. That is, a case of misconduct by an advisor results in an additional 0.59 cases of misconduct at that branch.

The results for the control variables are intuitively reasonable. The odds ratio for *Original Branch High Misconduct* is greater than one, indicating that advisors are more likely to commit misconduct if their pre-merger co-workers have a history of misconduct. This should not be interpreted as evidence of co-worker influence, however, as advisors self-selected their *Original Branch* co-workers, and thus this odds ratio captures both self-selection and co-worker influence. Indeed, consistent with the importance of self-selection, the odds ratio

¹⁹Age, gender, and assets under management are missing for some advisors. Population data are unavailable for some counties. The political corruption rate of Glaeser and Saks (2006) is unavailable for advisors in Puerto Rico. For observations with missing data, we set the variable to zero and the missing data indicator to one. There are three missing data indicator variables (demographic, local, and political corruption).

for *Original Branch High Misconduct* is significantly higher than the odds ratio for *Introduced Branch High Misconduct*. The results further show a positive relation between post-merger misconduct and *Pre-Merger Individual Misconduct*, consistent with Egan, Matvos, and Seru (2018), who also find strong individual persistence in misconduct. The results also show that misconduct is positively related to local population growth and political corruption, consistent with the findings of Parsons, Sulaeman, and Titman (2016) for financial statement misreporting.

Columns (2) and (3) of Table III report results for alternative specifications. Column (2) shows the results of a linear fixed effects model. Column (3) shows the results of a negative binomial fixed effects model in which the dependent variable is the number of misconduct cases that occur for the advisor during the post-merger period. The results, including the implied economic magnitudes, are similar across specifications – after a merger, the propensity to commit misconduct varies with the characteristics of the *Introduced Branch* co-workers.

The baseline specification uses a group-level independent variable and an individual-level dependent variable. The error terms for individuals within the same branch are likely correlated because they all experience the same treatment. If there is no adjustment, such as clustering, the standard errors will be understated (see Moulton, 1990). In our baseline specification, we cluster the standard errors, which Wooldridge (2006) and Donald and Lang (2007) suggest will work well given the number of groups. Nevertheless, as a robustness test, column (4) of Table III reports results for a specification in which the unit of observation is a pre-merger branch group merged with a specific branch of the other firm. The control variables are branch-group averages, and the dependent variable equals one if the branch-group has an above-average rate of misconduct in the post-merger period. The odds ratio for *Introduced Branch High Misconduct* remains significantly greater than one. Internet Appendix Tables IAVII to IAX report similar group-level results for all other specifications in the paper.

B. Contagion of Misconduct, Good Conduct, or Both?

The finding in column (1) of Table III is a *relative* result – it compares advisors from the same firm, and shows how the relative probability of committing misconduct changes if an advisor encounters an *Introduced Branch* with a high rate of misconduct. Because of the merger-firm fixed effects, however, the regression in column (1) cannot show whether the change in the relative probability of misconduct is due to an increase by advisors who encounter dishonest *Introduced Branch* co-workers, a decrease by advisors who encounter honest *Introduced Branch* co-workers, or both. Disentangling these possibilities requires a benchmark comparison group of advisors who do not encounter new co-workers in the merger.

We use propensity score matching to create a benchmark sample of advisors who were not affected by the merger. For each observation in the merger sample, we find a matched observation as follows. First, at the time of the merger, the potential match observation worked at the same firm and in the same state as the advisor, but did not encounter new co-workers in the merger.²⁰ Second, we estimate a logit model using the same independent variables as in column (1) of Table III (excluding *Introduced Branch High Misconduct* and $\log(\text{Introduced Branch Size})$). Third, based on the propensity score from the logit model, we match each observation in the main sample (with replacement) to its nearest neighbor. We then append the matched observations to the main sample and estimate a conditional logit model that includes both *Introduced Branch High Misconduct* and *Introduced Branch Low Misconduct* as independent variables (the matched benchmark sample is the excluded group).

The results in column (5) of Table III show a statistically significant odds ratio of 1.401 for *Introduced Branch High Misconduct*, but an insignificant odds ratio for *Introduced Branch Low Misconduct*. Thus, the results provide evidence of contagion in misconduct but no evidence of contagion in good conduct. The asymmetry of co-worker influence provides

²⁰We drop observations with fewer than 10 possible match observations within the merger-firm-state.

suggestive evidence regarding the mechanism of peer effects. Specifically, an asymmetric effect is consistent with social learning: an advisor can learn new techniques from dishonest co-workers, but does not forget techniques due to meeting honest co-workers. In contrast, alternative mechanisms based on social utility generally imply symmetric effects. For example, a preference to conform to social norms (e.g., Bernheim, 1994) implies symmetric effects, with advisors increasing misconduct if they encounter dishonest co-workers but decreasing misconduct if they encounter honest co-workers.

C. Randomization of Branch Assignment

As a robustness test, we use a bootstrap procedure to impose the null of no co-worker effects by randomizing the combination of branches following a merger. For each merger-firm, we counterfactually match each branch with a randomly selected branch from the other firm to create a pseudo post-merger branch. This procedure retains the pre-merger branch groups, but creates random pairings between branches of the target and acquirer. We then calculate the counterfactual variable *Pseudo-Introduced High Misconduct*. All other variables are unchanged (including *Individual Post-Merger Misconduct*). Using *Pseudo-Introduced High Misconduct*, we estimate a regression otherwise identical to that in column (1) of Table III. We repeat this procedure 10,000 times. Conceptually, this procedure is similar to that of Guryan, Kroft, and Notowidigdo (2009), although we match branches instead of individuals to account for possible intra-branch correlations (see Cameron, Gelbach, and Miller, 2008).

Figure 1 plots the distribution of the odds ratios for the counterfactual variable *Pseudo-Introduced Branch High Misconduct*. The figure clearly shows that the actual odds ratio from column (1) of Table III lies well above the mass of the distribution of estimates from the placebo test. The actual odds ratio of 1.370 is over four standard deviations above the mean of the simulations, and only two of the 10,000 placebo estimates are greater than the actual odds ratio. The key takeaway is that the actual branch assignments, and thus the actual

co-workers from the *Introduced Branch*, are crucial for generating the observed results.

D. *Pre-Merger and Post-Merger Trends in Misconduct*

Our identification strategy assumes that, after including the fixed effects and controls, the pre-merger trends in misconduct are the same for the “treated” (*Introduced Branch High Misconduct* = 1) and “untreated” observations. As a visual check of this assumption, Figure 2 plots the residual rates of misconduct for the “treated” and “untreated” groups for the three years before and after the merger. The residuals are taken from annual regressions in which the dependent variable equals one if the advisor discloses misconduct during the year (i.e., misconduct is measured over one year, rather than over three years as in the main specification). Because it is difficult to interpret residuals from conditional logit models, we estimate linear fixed effect models similar to that in column (2) of Table III, except these regressions do not include *Introduced Branch High Misconduct*.

For each of the six annual regressions, Figure 2 shows the average residuals for those advisors for whom *Introduced Branch High Misconduct* = 1 and for the other advisors. Figure 2 also shows the 95% confidence intervals for each group based on standard errors clustered by merger. Prior to the merger, the average residuals are close to zero for both groups, and there is no evidence of different pre-merger trends. Following the merger, the level of the residuals jumps for the *Introduced Branch High Misconduct* group. The similarity of the residuals prior to the merger suggests that, within a merger, firms do not select branch combinations based on branch characteristics that are correlated with misconduct.

III. Empirical Tests of Selection Bias

The key assumption for our identification strategy is that the merger-firm fixed effects eliminate selection bias. That is, we assume that, conditional on a merger occurring, the firm

does not systematically select to combine branches or advisors based on characteristics that are correlated with misconduct. This assumption is critical, because such selection could lead to the spurious appearance of co-worker influence. In this section, we examine whether selection bias affects the results.

A. *Do Future Co-Workers Predict Current Misconduct?*

An empirical implication of our identifying assumption is that, after controlling for merger-firm fixed effects, there will be no correlation between *Pre-Merger Individual Misconduct* and *Introduced Branch High Misconduct*. That is, we should not observe “co-worker influence” before the advisor meets the co-workers. In columns (1) and (2) of Table IV, the dependent variable is *Pre-Merger Individual Misconduct* (not post-merger, as in the main empirical tests).²¹ To establish a baseline, column (1) does not include merger-firm fixed effects; in this regression, during the pre-merger period there is a significant positive relation between misconduct by individual advisors and advisors at the *Introduced Branch*. This relation disappears, however, after including merger-firm fixed effects in column (2). Given the findings in Table IV, these results indicate a time-series change in misconduct for those advisors exposed to misconduct.

B. *Individual Selection Following Mergers*

The next test considers selection bias operating at the individual level. The potential concern is that, conditional upon two branches combining following a merger, an advisor could decide whether to remain with the merged firm based upon his perceived fit with the new branch. For example, an unethical advisor might choose to remain with the merged firm only if his

²¹Unlike in the main specification, in columns (1) and (2) of Table IV the dependent and independent variables are measured during the same time period. As a result, the coefficient estimate of interest is likely positively biased (Angrist, 2014), which works against the insignificant finding in column (2) of Table IV.

Introduced Branch co-workers appear tolerant of unethical behavior. To ensure selective exit does not drive the results, we identify advisors who meet the following criteria: (1) they worked at either the target or the acquirer 30 days before the merger, (2) their branch merged with a branch of the other firm, and (3) they did not work at the merged firm 100 days after the merger. Advisors who meet these criteria are defined as exiting advisors. In this robustness test, we counterfactually assign each exiting advisor the *Introduced Branch High Misconduct* of the branch he would have been merged into had he not exited. We combine the exiting advisors with the main sample and estimate a conditional logit model. The results in column (3) of Table IV show that the odds ratio for *Introduced Branch High Misconduct* is similar to that reported in the main specification.²²

C. Match Advisors to the Geographically Closest Branch

After a merger occurs, the acquiring firm has discretion with respect to which specific branches to combine. Obviously, geography constrains this decision. Indeed, for 83.4% of the advisors in the main sample their *Introduced Branch* is the geographically closest branch.²³ However, particularly in dense urban areas, the precise combination of branches contains an element of choice. In this subsection, we create a sample that removes the element of choice by assigning branch “combinations” using simple rules based on geography. The assignment procedure begins with the full sample of all branches and advisors associated with either the target or the acquirer 30 days before the merger (including branches and advisors that were not combined

²²As a simple check of selective exit, Panel A of Internet Appendix Table IAVI cross-tabulates exit rates by *Pre-Merger Individual Misconduct* and *Introduced Branch High Misconduct*. If selective exit occurs, the off-diagonal elements will be larger, as these indicate mismatches (i.e., when an advisor with (without) a history of misconduct matches to an *Introduced Branch* with low (high) misconduct). The off-diagonal elements are not significantly larger (p -value = 0.850). Panel B shows the cross-tabulation of *Original Branch High Misconduct* and *Introduced Branch High Misconduct*. Again, there is no evidence of selective exit.

²³Internet Appendix Table IAXI shows that the results are robust to dropping those advisors whose *Introduced Branch* is not the geographically closest branch.

following the merger). For each target branch, we consider all possible target-acquirer branch pairs within the same state and within a fixed geographic distance (within the same zip code or within 10, 25, or 50 miles, depending upon the specification). If there are multiple possible target-acquirer matches, we choose the geographically closest match. If there are multiple equidistant matches, we choose the acquirer branch with the largest number of advisors. Multiple target branches can match to a single acquirer branch. We use this assignment procedure to define each advisor’s *Geographically Closest Branch* co-workers. Note that this assignment procedure is entirely rule based and removes any selection by the acquiring firm, as the matching procedure includes all branches and advisors even if they were not directly affected by the merger. We then re-run the tests using *Geographically Closest Branch High Misconduct* as the key independent variable.

Panel B of Table IV reports the results. In column (1), *Geographically Closest Branch* is restricted to include only those acquirer-target pairs that are within the same zip code, which results in a smaller sample than in the main tests.²⁴ The 10-, 25-, and 50-mile matches are more permissive and result in larger samples, because fewer actual matches are screened out by the distance restriction²⁵ and the number of counterfactual branch “assignments” increases with the maximum allowable distance. In all four columns, the results are consistent with the baseline specification: the relation between *Geographically Closest Branch High Misconduct* and individual misconduct is positive and significant.

²⁴The restriction that the *Geographically Closest Branch* must be within the same zip code screens out 35.2% of the observations from the merger sample used in Table III. However, this sample also includes advisors from geographically proximate branches that were not combined following the merger.

²⁵The restrictions that the target-acquirer branch pair must lie within 10, 25, or 50 miles screen out, respectively, 12.7%, 8.8%, and 6.6% of the merger sample used in Table III.

D. Branches with Limited Options for Merging

In the prior subsection, we matched advisors to *Introduced Branches* using rules based on distance and branch size. In this subsection, we avoid the need for such rules by focusing on more restrictive matching criteria. The samples for the tests presented in Panel C of Table IV include only acquirer-target branch pairs for which there is a single target branch and a single acquirer branch within a specific geographic area. For example, the sample in column (1) includes only acquirer-target branch matches for which there is a single target branch and a single acquirer branch in the same zip code. Columns (2), (3), and (4) require a one-to-one pairing within a radius of 10, 25, and 50 miles, respectively. We use this assignment procedure to define each advisor’s *One-to-One Introduced Branch* co-workers. As the area for which we require only a single possible one-to-one match grows, the sample size necessarily decreases. In all four cases, the results are similar to the baseline results – there is a positive and significant relation between *One-to-One Introduced Branch High Misconduct* and individual misconduct following the merger. Thus, consistent with the other tests presented in this section we find no evidence of selection bias.

IV. Similarity and Co-Worker Influence on Misconduct

To separate peer effects from other branch environment effects, we exploit within-branch variation in social connections. Prior studies show that individuals with similar characteristics interact more, and that this increased interaction strengthens social influence. Thus, we expect a stronger relation between *Post-Merger Individual Misconduct* and *Introduced Branch High Misconduct* when the individual is similar to the *Introduced Branch* advisors who committed misconduct. Bertrand, Luttmer, and Mullainathan (2000) and McPherson, Smith-Lovin, and Cook (2001) show that ethnicity is an important factor determining social interactions, and Pool, Stoffman, and Yonker (2015) show that a shared ethnicity strengthens the effects

of social interactions in the mutual fund industry. Based on these studies, we use shared ethnicity as a proxy for the strength of the connection between two advisors.

Because we do not directly observe ethnicity, we map advisors' names to ethnic categories using the classification algorithm of Ambekar et al. (2009). We then create the variable *Same Ethnicity Misconduct*, which is equal to one if any of the advisor's *Introduced Branch* co-workers committed misconduct and shares the same ethnicity as the advisor. The specification reported in column (1) of Table V includes an interaction term between *Same Ethnicity Misconduct* and *Introduced Branch High Misconduct*. The specification also includes *Same Ethnicity Misconduct* and an indicator for the advisor's ethnic category,²⁶ which absorbs any differences across ethnic groups in the rate of misconduct (or in the propensity of customers to file complaints against certain groups). Apart from these changes, the specification is identical to that in column (1) of Table III.

The estimated odds ratio for *Introduced Branch High Misconduct* remains significant and greater than one, implying that the results documented earlier are not driven entirely by advisors with the same ethnicity. The odds ratio on the interaction term, *Introduced Branch High Misconduct* $\times$ *Same Ethnicity Misconduct*, is also greater than one and significant, which implies that the relation is stronger between individuals of the same ethnicity. Further, the implied magnitude of the relation is large – even after controlling for *Introduced Branch High Misconduct*, an advisor is 26% more likely to commit misconduct if his *Introduced Branch* includes a co-worker with a history of misconduct who shares the advisor's ethnicity.

The same-ethnicity result is consistent with co-worker influence: the *specific people* that an advisor works with affect misconduct, and behavioral similarity is strongest between co-workers who likely interact the most. In contrast, this result is not an obvious implication of alternative explanations unrelated to the specific people that comprise an advisor's co-

²⁶The algorithm classifies names into one of 13 categories such as Anglo, Indian, Jewish, and Nordic. We drop observations for which we cannot reliably assign an ethnicity or that lack the necessary input data.

workers. In addition to the across-branch results documented above, the same-ethnicity result shows there is *within-branch* variation in co-worker influence: even within the same branch under the same supervisor, co-worker influence is stronger between more socially connected advisors.

V. Confounding Variation and Co-Worker Influence on Misconduct

The results above show that, after a merger, advisors are more likely to commit misconduct if their *Introduced Branch* co-workers have a history of misconduct. The robustness tests in this section address possible sources of confounding across-branch variation that are not directly related to the misconduct of *Introduced Branch* co-workers.

A. Geographic Variation

Following a merger, the pairing of branches for combination is based primarily on geographic proximity. Thus, *Introduced Branch* co-workers are people who live and work in the same area. This raises the concern, that due to their shared location, advisors and their *Introduced Branch* co-workers are exposed to common economic, demographic, or regulatory factors that are not removed by merger-firm fixed effects. To address this possibility, we include merger-firm-geographic area fixed effects.

The specification reported in column (2) of Table V includes merger-firm-state fixed effects, which implies that a firm with branches in say 15 states would now have 15 distinct fixed effects. Column (3) includes merger-firm-county fixed effects, which implies that a firm with branches in say 35 counties would now have 35 distinct fixed effects. These specifications limit the comparison groups to other branches of the firm in the same state or in the same county, respectively. Because not all firms have multiple branches within the same state or

county, the conditional logit model drops some observations, resulting in smaller sample sizes. These fixed effects also subsume the geographic control variables (population, population growth, population density, and local political corruption), and so these controls are dropped from these models.

Even with these more stringent fixed effects, columns (2) and (3) of Table V report odds ratios for *Introduced Branch High Misconduct* that are similar to the baseline estimate in Table III. These results rule out multiple categories of alternative explanations. For example, government regulation of financial advisors is conducted primarily at the state level: the results in column (2) suggest that common exposure to state regulation is unlikely to drive the results. Further, the merger-firm-county results in column (3) suggest that local economic or demographic conditions are unlikely to drive the results.

B. Co-worker Influence in Complex Product Types or Productivity

Another potential explanation for our findings is based on co-worker influence in product type or productivity, rather than co-worker influence in misconduct. For example, advisors at a branch with a high misconduct rate could have more complaints because they sell more complex financial products or have more assets under management. If co-workers influence the type or amount of financial products sold and these positively correlate with misconduct, this could result in co-worker influence in misconduct.

To address the concern related to the sale of complex financial products, in column (4) of Table V we restrict the definition of misconduct to customer complaints related to mutual funds. The odds ratio for *Introduced Branch High Misconduct* is significant and greater than one, indicating that there is co-worker influence in misconduct even for a relatively noncontroversial and noncomplex financial product.

To address possible concerns related to co-worker influence on productivity combined with a positive correlation between productivity and customer complaints, we examine particularly

egregious cases of misconduct that are unlikely to have occurred simply because of the volume of business conducted. In column (5) of Table V, the dependent variable is equal to one if the advisor pays a penalty greater than \$100,000 for a single case. In column (5) of Internet Appendix Table IAI the dependent variable is equal to one if the advisor is sanctioned by a regulatory agency – regulatory sanctions are less common than customer complaints and generally indicate unambiguously bad behavior. For both dependent variables, the odds ratio for *Introduced Branch High Misconduct* is significant and greater than one.

C. Regulatory Actions

Another possible source of confounding across-branch variation comes from common exposure to regulators. The effects of any time-series or even geographic-area-time-series commonality in regulatory actions would be removed by the fixed effect specifications reported earlier. Further, we do not include regulatory actions in our misconduct variable (precisely to avoid such concerns). Nevertheless, as a robustness test, we estimate the baseline specification on a restricted sample. This sample excludes all post-merger branches at which any advisor (from either the target or the acquirer) was subject to a regulatory action during the pre-merger period. Although regulatory actions are less common than substantiated customer complaints (substantiated customer complaints are more than four times as common), this restriction substantially reduces the sample size as large branches are often screened out. As column (6) of Table V shows, the results are essentially the same as in the baseline specification. Further, these results cannot be driven by common shocks in regulatory actions, because the sample does not include any regulatory actions whatsoever.

VI. Supervisors and Co-Worker Influence on Misconduct

The prior results show that co-workers influence the misconduct of financial advisors. Co-workers, defined broadly, could include both peer co-workers and supervisors. Indeed, supervisors could disproportionately influence misconduct through their implementation and enforcement of monitoring procedures. Co-worker influences from supervisors versus from peers are conceptually different, however, and have different implications for how a firm or regulator might prevent misconduct. In this section we attempt to remove co-worker influence from supervisors and isolate the influence from peer co-workers.

A. Target and Acquirer Branches

Following a merger, target advisors generally move into the acquirer’s branches. Therefore it seems reasonable to assume that the acquirer’s institutional practices, and supervisory implementation of these practices, will persist after the merger.²⁷ This implies that supervisor influence should be stronger for target advisors, because the supervision of target advisors becomes more similar to that of their *Introduced Co-workers* at the acquirer branch.

The specification reported in column (1) of Table VI includes an interaction term between *Introduced Branch High Misconduct* and *Acquirer* (an indicator variable equal to one for advisors from the acquiring firm). If supervisor effects are larger for target advisors, implying that supervisor effects drive the results, the interaction term should have an odds ratio less than one. We find, however, that the odds ratio is greater than one and insignificant.

²⁷As a simple test of this proposition, we use data from AuditAnalytics to tabulate which firm’s auditor is retained following mergers. The acquirer’s auditor is retained in 87% of cases and the target’s auditor in only 9.5% of cases.

B. Co-Worker Influence When the Supervisor is Unchanged

In this subsection we focus on two subsamples of advisors for which we can remove influence from supervisors and isolate the influence from peer co-workers. We first identify all advisors licensed to serve as supervisors (advisors who have passed the Series 9 or 10 exams). We then limit the sample to only those advisors (1) who are not supervisors, (2) who work for the same branch-level supervisor before and after the merger, and (3) whose *Introduced Branch* co-workers do not include any supervisors.²⁸ These advisors meet new peer co-workers because of the merger but do not experience a change in supervisor. In this sample, *Introduced Branch High Misconduct* excludes supervisors and captures only the influence from peer co-workers. The sample for column (3) further restricts the sample to only those advisors who worked for the acquiring firm before the merger, as advisors from the acquiring firm likely experience little change in firm-level supervisory protocols.

The results for these restricted samples are reported in columns (2) and (3) of Table VI. In both columns, the odds ratio for *Introduced Branch High Misconduct* is significant and greater than one (in column (3) the odds ratio is significant at only the 10% level). Thus, advisors are more likely to commit misconduct if they encounter new co-workers with a history of misconduct, even if there is no change in supervisor. This result supports the view that there is co-worker influence from peer co-workers, in addition to any influence from supervisors.

VII. Conclusion

In this paper, we show that the misconduct behavior of financial advisors is malleable and that co-workers influence an individual's propensity to commit misconduct. We define co-workers as the advisors working at a single branch of a multi-branch firm. To minimize selection bias

²⁸The *Introduced Branch* co-worker group does not always include a supervisor, because solo and small branches are sometimes supervised remotely.

from advisors choosing their co-workers, we use changes to co-workers caused by mergers between firms. The results show that an advisor is 37% more likely to commit misconduct if he is merged into a new branch that includes individuals with a history of misconduct (relative to an advisor from the same firm who is merged into a branch with no history of misconduct). This implies a social multiplier of 1.59, meaning that a case of misconduct by an advisor results in an additional 0.59 cases of misconduct by that advisor's co-workers.

Establishing that co-workers influence misconduct committed by financial advisors has important policy implications. Co-worker influence implies that misconduct by one advisor creates a negative externality, which implies in turn that penalties for misconduct should be more severe than if there were no externalities. Such externalities also affect the type of punishment that should be applied, making suspensions or expulsions from the industry more effective than monetary penalties (e.g., Kahan, 1997). Further, co-worker influence suggests that changes to the current disclosure regime could benefit investors. Currently, investors can only view information about an individual advisor. Requiring clear disclosure of information about an advisor's co-workers could provide investors with useful information, while also creating a reputational penalty for the negative effects of co-worker influence.

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Table I. Summary Statistics: Financial Advisory Firms, Branches, and Financial Advisors

This table provides summary statistics on firm and advisor characteristics for the merger sample. The sample consists of 477 mergers during the period August 1999 to December 2011. Statistics are reported separately for acquirer and target firms. *Assets under Management (AUM)* is summarized using only those observations for which Meridian IQ has data.

	Acquirer	Target
<i>Financial Advisory Firms</i>		
Number of Firms	477	477
Average Financial Advisors per Firm	192.0	124.2
<i>Branches</i>		
Number of Branches	5,737	7,693
Average Branches per Firm	12.0	16.1
Average Financial Advisors per Branch	16.0	7.7
<i>Individual Financial Advisors</i>		
Number of Financial Advisors	91,569	59,264
Experience	9.8	11.0
Assets Under Management (AUM) in \$ millions	80.0	88.1
Male	71.0%	71.1%
Age	39.3	41.6

Table II. Misconduct by Financial Advisors

This table provides summary statistics on misconduct by financial advisors in the merger sample. Panel A reports the number of misconduct events and percentage of advisors that commit misconduct in the three-year windows before and after the merge: the pre-merger period is the three years prior to the merger, while the post-merger period is the three years after the merger. Panel B reports misconduct statistics at the branch level. *Original Branch High Misconduct* equals one if an above-average percentage of the advisor's *Original Branch* co-workers committed misconduct during the pre-merger period. *Introduced Branch High Misconduct* equals one if an above-average percentage of the advisor's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period. Panel C presents the dollar value distribution of settlements and awards. Settlements are not disclosed for 7.5% of complaints.

Panel A: Individual Misconduct Summary Statistics		
Pre-Merger Individual Misconduct	1,867	1.02%
Post-Merger Individual Misconduct	2,812	1.40%
Panel B: Branch Misconduct Summary Statistics		
Original Branch High Misconduct		12.5%
Introduced Branch High Misconduct		14.7%
Panel C: Dollar Value Distribution of Settlements and Awards		
Mean	\$430,338	
10 th Percentile	\$12,500	
25 th Percentile	\$22,826	
Median	\$50,000	
75 th Percentile	\$150,000	
90 th Percentile	\$425,294	
% Settlement Not Disclosed	7.5%	

Table III. Mergers, Introduced Co-Workers, and Financial Misconduct

Columns (1), (4), and (5) report odds ratios from a conditional logit model grouped by merger firm. In Columns (1), (2), (3), and (5), the unit observation is the advisor; in column (4) the unit of observation is the branch and variables are based on branch-level averages. Column (2) reports estimates from a linear model with merger-firm fixed effects. Column (3) reports estimates from a negative binomial model with merger-firm fixed effects. In columns (1), (2), and (5), the dependent variable equals one if the advisor commits misconduct in the post-merger period (three-year window). In column (3), the dependent variable is the count of misconduct events. In columns (1), (2), and (3), the sample includes all advisors who work for a merged branch. In column (5), the sample also includes a propensity score matched sample of advisors that do not encounter any new co-workers in the merger. *Introduced Branch High Misconduct* equals one if an above-average percentage of the advisor’s new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period (three-year window). *Introduced Branch Low Misconduct* equals one if a below-average percentage of the advisor’s new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period. *Original Branch High Misconduct* equals one if an above-average percentage of the advisor’s *Original Branch* co-workers committed misconduct during the pre-merger period. *Pre-Merger Individual Misconduct* equals one if the advisor committed misconduct during the pre-merger period. The models also include controls for advisor age, experience, gender, assets under management, branch type, the number of advisors in the *Original Branch* and *Introduced Branch*, county population, population growth, population density, and *Political Corruption*, the state-level corruption rate based on federal convictions of politicians. Indicators for missing data are included but not reported. *Test of difference: High vs. Low* reports the test of the difference in the coefficients on *Introduced Branch High Misconduct* and *Introduced Branch Low Misconduct* in column (5). Z-scores based on standard errors clustered by merger are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Introduced Branch High Misconduct	1.370*** [4.25]	0.006*** [3.29]	0.267*** [3.71]	2.351*** [6.33]	1.401** [2.47]
Introduced Branch Low Misconduct					1.068 [0.79]
Original Branch High Misconduct	1.613*** [7.00]	0.010*** [5.93]	0.470*** [9.03]	3.872*** [15.10]	1.675*** [4.49]
Pre-Merger Individual Misconduct	3.983*** [11.90]	0.077*** [8.48]	1.326*** [15.96]		4.183*** [15.80]
Standardized Age	1.043 [1.64]	0.003*** [5.06]	0.035 [1.13]	1.025 [0.49]	1.081 [1.30]
Standardized Experience	1.228*** [7.68]	0.003*** [6.15]	0.206*** [7.73]	1.115** [2.17]	1.276*** [4.03]
Male Dummy	3.691*** [20.23]	0.014*** [7.56]	1.282*** [16.62]	1.336*** [3.87]	3.605*** [14.15]
Standardized AUM	1.023** [2.53]	0.002** [2.15]	0.019* [1.85]	0.952 [0.85]	1.042* [1.88]
Main Office Dummy	0.522** [2.42]	-0.003** [2.29]	-0.610*** [3.46]	0.308 [1.62]	0.246*** [3.42]
Solo Original Branch	0.778 [1.39]	-0.004 [1.39]	-0.257* [1.65]	1.008 [0.07]	0.799 [0.99]
log(Original Branch Size)	0.772*** [8.92]	-0.003*** [3.97]	-0.248*** [11.10]	1.126*** [3.48]	0.788*** [7.15]
log(Introduced Branch Size)	0.829*** [5.81]	-0.002*** [5.39]	-0.179*** [8.29]	1.188*** [2.75]	0.901** [2.14]
log(Population)	1.050 [1.54]	-0.001 [0.75]	0.049* [1.78]	1.080 [1.51]	0.973 [0.56]
Population Growth	2.959* [1.79]	0.029* [1.84]	0.704* [1.96]	1.345 [0.44]	2.952** [2.49]
Population Density	1.006* [1.71]	0.000** [2.04]	0.004** [2.09]	1.000 [0.07]	1.008*** [2.94]
Political Corruption	2.177* [1.81]	0.012 [1.29]	0.883*** [3.51]	1.573 [0.83]	2.134* [1.91]
<i>Test of difference: High vs. Low</i>					0.272*** [2.81]
Missing Variable Indicators	Yes	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Includes Non-merging Advisors	No	No	No	No	Yes
Observations	113,963	150,746	113,963	12,233	210,332

Table IV. Tests of Selection Bias

In Panel A, the table reports three tests for selection bias. Column (1) reports odds ratios from a logit model of pre-merger misconduct. Column (2) reports odds ratios from a conditional logit model of pre-merger misconduct with merger-firm fixed effects. In columns (1) and (2), the dependent variable equals one if the advisor committed misconduct in the pre-merger period (three-year window). In column (3), the dependent variable equals one if the advisor committed misconduct in the post-merger period (three-year window). In column (3), the sample also includes all advisors from the merger sample pre-merger branches. *Introduced Branch High Misconduct* equals one if an above-average percentage of the advisor's *Introduced Branch* co-workers committed misconduct during the pre-merger period. *Original Branch High Misconduct* equals one if an above-average percentage of the advisor's *Original Branch* co-workers committed misconduct during the pre-merger period. *Before Pre-Merger Individual Misconduct* equals one if the advisor committed misconduct during any year prior to the beginning of the pre-merger period, $(-\infty, -3)$. In Panel B, the sample consists of the geographically closest match for all possible target-acquirer branch pairs (within the same state and within a fixed geographic distance) for the full sample of branches and advisors associated with either the target or the acquirer 30 days before the merger. *Geographically Closest Branch High Misconduct* equals one if an above-average percentage of the potential co-workers in the geographically closest branch committed misconduct during the pre-merger period. In Panel C, the sample comprises all branches for which there is a single target-acquirer branch pair within a certain geographic distance. In column (1), we restrict attention to targets in the same zip code of the acquirer branch. In columns (2) through (4), the sample comprises all branches in which there is a single target-acquirer branch pair within X miles of each other. *One-to-One Introduced Branch High Misconduct* equals one if an above-average percentage of the potential co-workers in the single branch within the radius committed misconduct during the pre-merger period. The models also include controls for advisor age, experience, gender, assets under management, branch type, the number of advisors in the *Original Branch* and *Introduced Branch*, county population, population growth, population density, and political corruption. Z -scores based on standard errors clustered by merger are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Selection Bias Tests			
	Placebo Test of Selection Bias		Include Exiting Advisors
	(1)	(2)	(3)
Dependent Variable Measured:	Pre-Merger	Pre-Merger	Post-Merger
Introduced Branch High Misconduct	1.796*** [7.65]	1.122 [1.47]	1.438*** [4.34]
Original Branch High Misconduct	3.466*** [9.09]	1.696*** [3.58]	1.640*** [7.81]
Before Pre-Merger Individual Misconduct	3.221*** [11.69]	2.794*** [12.51]	
Pre-Merger Individual Misconduct			3.982*** [11.33]
Advisor, Branch, and Local Controls	Yes	Yes	Yes
Merger-Firm Fixed Effects	No	Yes	Yes
Number of Observations	150,746	102,691	116,223

Panel B: Match Advisors to Geographically Closest Branch				
	Same Zip	10 mile	25 mile	50 mile
	(1)	(2)	(3)	(4)
Geographic Closest Branch High Misconduct	1.489*** [3.94]	1.264*** [3.30]	1.269*** [3.51]	1.257*** [3.38]
Original Branch High Misconduct	1.448*** [4.32]	1.499*** [6.53]	1.500*** [7.30]	1.491*** [7.73]
Pre-Merger Individual Misconduct	4.106*** [12.40]	3.936*** [14.24]	3.878*** [16.66]	3.793*** [16.35]
Advisor, Branch, and Local Controls	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes
Number of Observations	108,257	173,091	192,487	203,830

Panel C: Branches with Limited Options for Merging				
	Same Zip	10 mile	25 mile	50 mile
	(1)	(2)	(3)	(4)
One-to-One Introduced Branch High Misconduct	1.373*** [3.63]	1.621*** [5.27]	1.587*** [3.01]	1.645* [1.82]
Original Branch High Misconduct	1.486*** [4.65]	1.531*** [2.97]	1.359* [1.65]	1.351 [1.48]
Pre-Merger Individual Misconduct	3.631*** [8.65]	3.178*** [8.18]	1.488*** [6.92]	3.839*** [5.23]
Advisor, Branch, and Local Controls	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes
Number of Observations	80,411	35,818	24,317	18,917

Table V. Branch Environment and Co-Worker Influence on Misconduct

This table reports odds ratios from conditional logit models in which the dependent variable equals one if the advisor commits misconduct in the post-merger period (three-year window). *Introduced Branch High Misconduct* equals one if an above-average percentage of the advisor's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period (three-year window). *Original Branch High Misconduct* equals one if an above-average percentage of the advisor's *Original Branch* co-workers committed misconduct during the pre-merger period. *Pre-Merger Individual Misconduct* equals one if the advisor committed misconduct during the pre-merger period. *Same Ethnicity Misconduct* equals one if an advisor's new colleagues of the same ethnicity (classified using the approach from Ambekar et al., 2009) committed misconduct during the pre-merger period. $\log(\text{Introduced Branch Same Ethnicity Size})$ is the logarithm of the number of advisors of the same ethnicity in the *Introduced Branch*. In column (1), *Same Ethnicity Misconduct*, $\log(\text{Introduced Branch Same Ethnicity Size})$, and ethnicity fixed effects are included as controls but not reported. In column (2), the model includes merger-firm-state fixed effects (separate fixed effects for each state in which branches are located for each firm in each merger). In column (3), the model includes merger-firm-county fixed effects (separate fixed effects for each county in which branches are located for each firm in each merger). In column (4), we restrict the definition of misconduct (for both dependent and independent variables) to customer complaints regarding mutual funds. In column (5), the dependent variable is complaints with settlements or fines of \$100,000 or greater. In column (6), the sample excludes all branches with any history of regulatory actions (in the pre-merger window). The models also include controls for advisor age, experience, gender, assets under management, branch type, the number of advisors in the *Original Branch* and *Introduced Branch*, county population, population growth, population density, and political corruption. *Z*-scores based on standard errors clustered by merger are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Similarity	Geographic		Mutual Fund	\$100k+	Reg. Action
	(1)	(2)	(3)	(4)	(5)	(6)
Introduced Branch High Misconduct	1.328** [2.26]	1.320*** [2.94]	1.378** [2.08]	1.641** [2.34]	1.377** [2.12]	1.317*** [2.88]
IBHM $\times$ Same Ethnicity Misconduct	1.259** [2.10]					
Original Branch High Misconduct	1.538*** [5.65]	1.645*** [6.19]	1.770*** [4.71]	1.431** [2.30]	1.541*** [5.00]	1.514*** [4.81]
Pre-Merger Individual Misconduct	3.567*** [8.71]	4.184*** [11.33]	4.447*** [11.05]	6.442*** [6.67]	4.809*** [15.39]	3.950*** [12.51]
Advisor, Branch, and Local Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ethnicity Fixed Effects	Yes	No	No	No	No	No
Merger-Firm Fixed Effects	Yes	Subsumed	Subsumed	Yes	Yes	Yes
Merger-Firm-State Fixed Effects	No	Yes	No	No	No	No
Merger-Firm-County Fixed Effects	No	No	Yes	No	No	No
Exclude when History of Reg. Actions	No	No	No	No	No	Yes
Number of Observations	78,234	84,457	70,614	86,339	88,885	77,023

Table VI. Across-Branch Variation in Monitoring and Supervision

This table reports odds ratios from conditional logit models in which the dependent variable equals one if the advisor commits misconduct in the post-merger period (three-year window). *Introduced Branch High Misconduct* equals one if an above-average percentage of the advisor's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period (three-year window). *Original Branch High Misconduct* equals one if an above-average percentage of the advisor's *Original Branch* co-workers committed misconduct during the pre-merger period. *Pre-Merger Individual Misconduct* equals one if the advisor committed misconduct during the pre-merger period. Column (1) includes the interaction between *Introduced Branch High Misconduct* and a dummy equal to one if the advisor initially worked for the acquiring firm (the *Acquirer* dummy is subsumed by the merger-firm fixed effects). In column (2), the sample includes only those advisors that were not introduced to new supervisors during the merger. In column (3), the sample includes only those acquirer advisors that were not introduced to new supervisors during the merger. The models also include controls for advisor age, experience, gender, assets under management, branch type, the number of advisors in the *Original Branch* and *Introduced Branch*, county population, population growth, population density, and political corruption. *Z*-scores based on standard errors clustered by merger are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
Introduced Branch High Misconduct	1.263** [2.17]	1.331** [2.55]	1.529* [1.78]
Original Branch High Misconduct	1.620*** [7.01]	1.627*** [4.53]	1.564*** [3.72]
Pre-Merger Individual Misconduct	3.994*** [11.80]	3.626*** [12.68]	3.082*** [5.62]
Introduced Branch High Misconduct $\times$ Acquirer	1.209 [1.03]		
Advisor, Branch, and Local Controls	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes
Number of Observations	113,963	61,493	36,758

Fig. 1. Odds ratios of *Introduced Branch High Misconduct* in placebo samples.

The figure shows a histogram of *Pseudo-Introduced Branch High Misconduct* odds ratios from 10,000 cluster bootstrap iterations of the model in Table III, column (1). For each iteration, within each merger, each branch is introduced to a randomly selected branch from the other firm in the merger (creating new counterfactual values for *Introduced Branch High Misconduct* and *Introduced Branch Size*). The model is reestimated using the counterfactual *Introduced Branch* values. All other advisor characteristics remain the same.

Fig. 2. Pre- and post- trends.

This figure plots residuals from annual linear regressions similar to the main specification in Table III, column (2). Because the figure plots the average residuals grouped by *Introduced Branch High Misconduct*, the specification does not include *Introduced Branch High Misconduct* as an independent variable. The dashed lines show the 95% confidence intervals based on standard errors clustered by merger.

Internet Appendix for “Is Fraud Contagious? Co-Worker Influence on Misconduct by Financial Advisors”

STEPHEN G. DIMMOCK, WILLIAM C. GERKEN, and NATHANIEL P. GRAHAM*

This appendix contains two sections. Section I discusses the effects of common environments in our setting. Section II provides robustness results referenced in the main paper.

I. Common Environment

The tests in the paper are designed to filter out the effects of common environments. However, it is interesting to evaluate the relative magnitudes of peer effects and a common environment. Among other methodological issues, Angrist (2014) compares “peer effects” estimated with (1) an independent variable based on pre-meeting behavior and (2) an independent variable based on post-meeting behavior. Angrist argues that the larger coefficients found when using an independent variable measured after the group members met, indicates the additional effect of a common environment.

In Table IAXII, we estimate a specification similar to that in column (1) of Table III. The difference is that we include *Post-Merger Branch High Misconduct* as an additional independent variable. This variable is an indicator set equal to one if an above-average proportion of an advisor’s post-merger co-workers commit misconduct during the post-merger period. (The advisor’s own post-merger misconduct is not included when calculating *Post-Merger Branch High Misconduct*, as that would create a mechanical relation with the dependent variable.)

The odds ratio for *Post-Merger Branch High Misconduct* is 1.728, which is significantly larger than the odds ratio for *Introduced Branch High Misconduct* of 1.266 (the p -value of the difference in coefficients is 0.046). The larger magnitude of the coefficient on contemporaneous peer behavior (relative to pre-merger peer behavior) suggests that there exist common environment effects that are potentially large. Although this evidence is suggestive, we note that our test does not provide a clean decomposition between peer effects and common environmental effects, as it is possible that peer effects are larger for contemporaneous peer behavior than for lagged peer behavior.

II. Robustness Results

*Dimmock, Stephen G., William C. Gerken, and Nathaniel P. Graham, Internet Appendix for “Is Fraud Contagious? Co-Worker Influence on Misconduct by Financial Advisors” [DOI String]. Please note: Wiley-Blackwell is not responsible for the content or functionality of any additional information provided by the authors. Any queries (other than missing material) should be directed to the authors of the article.

Table IAI. Alternate Timing Robustness Tests

This table reports odds ratios from conditional logit models in which the dependent variable equals one if the advisor commits misconduct in the post-merger period (three-year window). *Introduced Branch High Misconduct* equals one if an above-average percentage of the advisor's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period. *Original Branch High Misconduct* equals one if an above-average percentage of the advisor's *Original Branch* co-workers committed misconduct during the pre-merger period (three-year window). *Pre-Merger Individual Misconduct* equals one if the advisor committed misconduct during the pre-merger period. In column (1), the sample comprises all branches that do not experience another merger for three years. In column (2), the sample comprises all firms that do not experience another merger for three years. In column (3), we include only those observations from mergers in the 1999 to 2005 period. In column (4), we include only those observations from mergers in the 2006 to 2011 period. In column (5), the dependent variable is misconduct in a three-year window that starts two years and 100 days after the merger. The models include merger-firm fixed effects. The models also include controls for advisor age, experience, gender, assets under management, branch type, the number of advisors in the *Original Branch* and *Introduced Branch*, county population, population growth, population density, and political corruption. Z-scores based on standard errors clustered by merger are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	No Overlap at Branch Level (1)	No Overlap at Firm Level (2)	Early Sample (3)	Late Sample (4)	Delayed Post Period (5)
Introduced Branch High Misconduct	1.403*** [4.02]	1.367*** [2.68]	1.268** [2.17]	1.441*** [3.56]	1.397*** [3.83]
Original Branch High Misconduct	1.547*** [5.70]	1.584*** [4.43]	1.655*** [4.48]	1.615*** [5.61]	1.553*** [6.02]
Pre-Merger Individual Misconduct	3.802*** [13.86]	3.807*** [8.80]	5.743*** [14.34]	2.931*** [12.89]	3.132*** [9.51]
Advisor, Branch, and Local Controls	Yes	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Number of Observations	80,713	34,151	48,340	65,623	105,126

Table IAIL. Alternative Definitions of Misconduct

In column (1), we define misconduct as customer complaints, employment separations after allegations, and regulatory, civil, and criminal disclosures as in Egan, Matvos, and Seru (2018). In column (2), we include validated customer complaints and append denied complaints during the three-year post-merger window. In column (3), the dependent variable is misconduct defined as customer complaints, employment separations after allegations, and regulatory, civil, and criminal disclosures as in Egan, Matvos, and Seru (2018) as well as denied customer complaints. In column (4), we exclude from our definition of validated customer complaints any complaints with settlements of less than \$50,000. In column (5), the dependent variable is regulatory action in the post-merger period. *Introduced Branch High Misconduct* equals one if an above-average percentage of the advisor's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period (three-year window). *Original Branch High Misconduct* equals one if an above-average percentage of the advisor's *Original Branch* co-workers committed misconduct during the pre-merger period. *Pre-Merger Individual Misconduct* equals one if the advisor committed misconduct during the pre-merger period. The models also include controls for advisor age, experience, gender, assets under management, branch type, the number of advisors in the *Original Branch* and *Introduced Branch*, county population, population growth, population density, and political corruption. *Z*-scores based on standard errors clustered by merger are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Egan, Matvos, and Seru (2016) Definition (1)	Customer Complaints Incl. Denied (2)	Egan, Matvos, and Seru (2016) Incl. Denied (3)	Exclude Small \$ Settlements (4)	Regulatory Actions (5)
Introduced Branch High Misconduct	1.307*** [3.49]	1.370*** [4.06]	1.308*** [3.60]	1.302*** [2.99]	1.682** [2.44]
Original Branch High Misconduct	1.549*** [7.49]	1.467*** [5.15]	1.492*** [6.54]	1.628*** [8.53]	1.238 [0.92]
Pre-Merger Individual Misconduct	4.124*** [17.34]	4.005*** [14.96]	4.067*** [19.40]	4.527*** [12.45]	8.385*** [8.39]
Advisor, Branch, and Local Controls	Yes	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	128,820	115,442	129,077	113,963	77,618

Table IAIII. Alternate Window Lengths

This table reports the *Introduced Branch High Misconduct* odds ratios from models of post-merger misconduct using alternative pre-merger and post-merger windows. The models and variable definitions are based on the specification in Table III, column (1) and are identical except for the window size. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Post-Merger Window Length			
	1-year	2-year	3-year	5-year
1-year Pre-Merger Window	1.239 [1.47]	1.334*** [2.76]	1.376*** [3.55]	1.376*** [3.85]
2-year Pre-Merger Window	1.376*** [2.97]	1.266** [2.57]	1.292*** [3.11]	1.370*** [3.76]
3-year Pre-Merger Window	1.352*** [2.73]	1.309*** [3.36]	1.370*** [4.25]	1.410*** [4.60]
5-year Pre-Merger Window	1.471*** [3.60]	1.348*** [3.30]	1.258** [2.40]	1.253** [2.21]
10-year Pre-Merger Window	1.420*** [3.21]	1.383*** [3.29]	1.342*** [2.85]	1.299*** [2.66]
Full Pre-Merger Window	1.502*** [3.27]	1.485*** [4.45]	1.508*** [4.68]	1.470*** [5.20]

Table IAIV. Alternative Functional Forms

This table reports odds ratios from conditional logit models of post-merger misconduct (three-year window). *Introduced Branch High Misconduct* equals one if an above-average percentage of the advisor's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period (three-year window). *Original Branch High Misconduct* equals one if an above-average percentage of the advisor's *Original Branch* co-workers committed misconduct during the pre-merger period. *Introduced Branch Misconduct Rate* equals the percentage of the advisor's new co-workers from the other firm in the merger who committed misconduct during the pre-merger period. *Original Branch Misconduct Rate* equals the percentage of advisors at the advisor's *Original Branch* who committed misconduct during the pre-merger period. *Introduced Branch Misconduct 5% Cutoff* equals one if the percentage of the advisor's new colleagues from the other firm in the merger who committed misconduct during the pre-merger period is above 5%. *Original Branch Misconduct 5% Cutoff* equals one if the percentage of advisors at the advisor's *Original Branch* who committed misconduct during the pre-merger period is above 5%. *Pre-Merger Individual Misconduct* equals one if the advisor committed misconduct during the pre-merger period. The models also include controls for advisor age, experience, gender, assets under management, branch type, the number of advisors in the *Original Branch* and *Introduced Branch*, county population, population growth, population density, and political corruption. *Z*-scores based on standard errors clustered by merger are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	(1)	(2)	(3)
Introduced Branch Misconduct Dummy	1.240** [2.07]		
Original Branch Misconduct Dummy	1.382*** [4.32]		
Introduced Branch Misconduct Rate		1.836** [2.08]	
Original Branch Misconduct Rate		1.933*** [3.52]	
Introduced Branch Misconduct 5% Cutoff			1.300** [2.54]
Original Branch Misconduct 5% Cutoff			1.264*** [2.40]
Pre-Merger Individual Misconduct	4.069*** [12.02]	4.022*** [12.08]	4.017*** [11.86]
Advisor, Branch, and Local Controls	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes
Number of Observations	113,963	113,963	113,963

Table IAV. Branch Level Placebo

In columns (1) and (2), the dependent variable equals one if the branch commits misconduct at an above-average rate in the pre-merger period (three-year window). In columns (3) and (4), the dependent variable is the average branch-level misconduct rate in the pre-merger period (three-year window). *Introduced Branch High Misconduct* equals one if an above-average percentage of the *Introduced Branch* committed misconduct during the pre-merger period. *Introduced Branch Level of Misconduct* is the percentage of advisors from the *Introduced Branch* who committed misconduct during the pre-merger period. The models also include controls for the number of advisors in the *Original Branch* and *Introduced Branch*, local population and corruption. Z-scores based on standard errors clustered by merger are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Logit (1)	Logit (2)	Linear (3)	Linear (4)
Introduced Branch High Misconduct	2.186*** [12.84]	0.894 [1.55]		
Introduced Branch Level of Misconduct			0.030*** [4.48]	-0.007 [1.07]
Controls	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	No	Yes	No	Yes
Observations	17,783	12,447	17,783	17,783

Table IAVI. Survival and Misconduct: Addressing Assortative Matching Concerns

This table shows the exit rates from pre-merger branches (advisor leaves firm) tabulated by *Introduced Branch* type. Panel A reports exit rates at the individual level. Panel B reports exit rates at the branch level.

Panel A: Non-survival by Introduced Branch: Individual Matching			
	Introduced Branch High Misconduct		
	No	Yes	
Pre-Merger Individual Misconduct	No	3.14%	3.19%
	Yes	3.52%	2.89%
$\chi^2=0.798$, p -value = 0.850			
Panel B: Non-survival by Introduced Branch: Original Branch Matching			
	Introduced Branch High Misconduct		
	No	Yes	
Original Branch High Misconduct	No	3.14%	3.06%
	Yes	3.15%	3.47%
$\chi^2=2.120$, p -value = 0.548			

Table IAVII. Mergers, Introduced Co-Workers, and Financial Misconduct

Columns (1) and (4) report odds ratios from a conditional logit model grouped by merger-firm. Column (2) reports estimates from a linear model with merger-firm fixed effects. Column (3) reports estimates from a negative binomial model with merger-firm fixed effects. In columns (1), (2), and (4), the dependent variable equals one if an above-average percentage of the branch's advisors committed misconduct during the post-merger period (three-year window). In column (3), the dependent variable is the branch-level count of misconduct events. In columns (1), (2), and (3), the sample includes all branches that are introduced to advisors from the other firm in the merger. In column (4), the sample also includes a propensity score matched sample of branches that do not encounter any new co-workers in the merger. *Introduced Branch High Misconduct* equals one if an above-average percentage of branch's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period. *Introduced Branch Low Misconduct* equals one if a below-average percentage of the branch's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period. *Original Branch High Misconduct* equals one if an above-average percentage of the branch's advisors committed misconduct during the pre-merger period. The models also include controls for branch-level average of advisor age, experience, gender, assets under management, branch type, the number of advisors in the *Original Branch* and *Introduced Branch*, county population, population growth, population density, and *Political Corruption*, the state-level corruption rate based on federal convictions of politicians. Indicators for missing data are included but not reported. *Test of difference: High vs. Low* reports the test of the difference between coefficients of *Introduced Branch High Misconduct* and *Introduced Branch Low Misconduct* in column (4). Z-scores based on standard errors clustered by merger are in brackets in columns (1), (2), and (4). Z-scores without clustering are reported in column (3). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Cond. Logit (1)	Linear F.E. (2)	Count (3)	Cond. Logit (4)
Introduced Branch High Misconduct	2.351*** [6.33]	0.112*** [5.72]	1.559*** [9.73]	4.395*** [6.52]
Original Branch High Misconduct	3.872*** [15.10]	0.217*** [15.42]	2.483*** [17.94]	3.792*** [14.55]
Introduced Branch Low Misconduct				2.204*** [4.37]
Standardized Age	1.025 [0.49]	0.004 [0.92]	1.037 [1.10]	1.011 [0.18]
Standardized Experience	1.115** [2.17]	0.008** [2.15]	1.069** [2.40]	1.150** [2.25]
Male Dummy	1.336*** [3.87]	0.020*** [3.39]	1.212*** [3.53]	1.193** [2.53]
Standardized AUM	0.952 [-0.85]	-0.003 [-0.96]	0.997 [-0.10]	0.969 [-0.52]
Main Office Dummy	0.308 [-1.62]	-0.094 [-1.39]	0.536*** [-7.11]	0.217** [-2.11]
Solo Original Branch	1.008 [0.07]	0.009 [1.44]	1.110 [1.57]	0.922 [-0.55]
log(Original Branch Size)	1.126*** [3.48]	0.012*** [3.87]	1.069*** [4.16]	1.008 [0.28]
log(Introduced Branch Size)	1.188*** [2.75]	0.011** [2.06]	1.383*** [21.70]	1.233*** [4.48]
log(Population)	1.080 [1.51]	0.009** [2.05]	1.094*** [4.02]	1.123** [2.40]
Population Growth	1.345 [0.44]	0.008 [0.14]	1.789* [1.80]	1.520 [0.76]
Population Density	1.000 [-0.07]	0.000 [0.26]	1.005*** [3.19]	1.004 [0.58]
Political Corruption	1.573 [0.83]	0.004 [0.06]	1.082 [0.36]	1.950 [1.35]
<i>Test of difference: High vs. Low</i>				0.690***
Missing Variable Indicators	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes
Includes Non-merging Advisors	No	No	No	Yes
Observations	12,233	17,602	12,233	14,719

Table IAVIII. Tests of Selection Bias

In Panel A, the table reports three tests for selection bias. Column (1) reports odds ratios from a logit model of pre-merger misconduct. Column (2) reports odds ratios from a conditional logit model of pre-merger misconduct with merger-firm fixed effects. In columns (1) and (2), the dependent variable equals one if an above-average percentage of the branch's advisors committed misconduct during the pre-merger period (three-year window). In column (3), the dependent variable equals one if an above-average percentage of the branch's advisors committed misconduct during the post-merger period (three-year window). In column (3), the sample also includes all advisors from the merger sample's pre-merger branches. *Introduced Branch High Misconduct* equals one if an above-average percentage of the branch's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period. *Original Branch High Misconduct* equals one if an above-average percentage of the branch's advisors committed misconduct during the pre-merger period. In Panel B, the sample consists of the geographically closest match for all possible target-acquirer branch pairs (within the same state and within a fixed geographic distance) for the full sample of branches associated with either the target or the acquirer 30 days before the merger. *Geographically Closest Branch High Misconduct* equals one if an above-average percentage of the potential co-workers in the geographically closest branch committed misconduct during the pre-merger period. In Panel C, the sample comprises all branches for which there is a single target-acquirer branch pair within a certain geographic distance. In column (1), we restrict pairs to a target in the same zip code as the acquirer branch. In columns (2) through (4), the sample comprises all branches in which there is a single target-acquirer branch pair within X miles of each other. *One-to-One Introduced Branch High Misconduct* equals one if an above-average percentage of the potential co-workers in the single branch within the radius committed misconduct during the pre-merger period. The models also include controls for branch-level average of advisor age, experience, gender, assets under management, branch type, the number of advisors in the *Original Branch* and *Introduced Branch*, county population, population growth, population density, and political corruption. Z -scores based on standard errors clustered by merger are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Selection Bias Tests				
	Placebo Test of Selection Bias		Include Exiting Advisors	
	(1)	(2)	(3)	
Dependent Variable Measured:	Pre-Merger	Pre-Merger	Post-Merger	
Introduced Branch High Misconduct	1.796*** [7.65]	0.879 [1.00]	1.951*** [5.87]	
Original Branch High Misconduct			1.592*** [7.02]	
Avg. Advisor, Branch, and Local Controls	Yes	Yes	Yes	
Merger-Firm Fixed Effects	No	Yes	Yes	
Number of Observations	17,602	12,024	13,913	
Panel B: Match Advisors to Geographically Closest Branch				
	Same Zip (1)	10 mile (2)	25 mile (3)	50 mile (4)
Geographic Closest Branch High Misconduct	3.476*** [4.42]	2.142** [2.61]	1.735* [1.66]	1.649 [1.53]
Original Branch High Misconduct	5.484*** [21.65]	4.750*** [14.67]	4.661 [15.65]	4.611*** [16.84]
Average Advisor, Branch, and Local Controls	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes
Number of Observations	13,936	28,319	33,442	36,776
Panel C: Branches with Limited Options for Merging				
	Same Zip (1)	10 mile (2)	25 mile (3)	50 mile (4)
One-to-One Introduced Branch High Misconduct	2.741*** [6.53]	2.883*** [3.88]	2.781*** [3.43]	3.071*** [2.96]
Original Branch High Misconduct	3.413*** [9.01]	3.218*** [6.69]	3.105*** [4.82]	3.574*** [3.95]
Average Advisor, Branch, and Local Controls	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes
Number of Observations	5,897	2,805	1,661	1,062

Table IAIX. Branch Environment and Co-Worker Influence on Misconduct

This table reports odds ratios from conditional logit models in which the dependent variable equals one if an above-average percentage of the branch's advisors committed misconduct during the post-merger period (three-year window). *Introduced Branch High Misconduct* equals one if an above-average percentage of the branch's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period (three-year window). *Original Branch High Misconduct* equals one if an above-average percentage of the branch's advisors committed misconduct during the pre-merger period. *Same Ethnicity Misconduct* is the branch-level percentage of advisors whose new colleagues of the same ethnicity (classified using the approach of Ambekar et al., 2009) committed misconduct before the merger (three-year window). $\log(\text{Introduced Branch Same Ethnicity Size})$ is the logarithm of the branch-level number of advisors of the same ethnicity in the *Introduced Branch*. In column (1), $\log(\text{Introduced Branch Same Ethnicity Size})$ and ethnicity fixed effects are included as controls but not reported. In column (2), the model includes merger-firm-state fixed effects (separate fixed effects for each state in which branches are located for each firm in each merger). In column (3), the model includes merger-firm-county fixed effects (separate fixed effects for each county in which branches are located for each firm in each merger). In column (4), we restrict the definition of misconduct (for both dependent and independent variables) to customer complaints regarding mutual funds. In column (5), dependent variable is complaints with settlements or fines of greater than \$100,000. In column (6), the sample excludes all branches with any history of regulatory actions (in the pre-merger window). The models also include controls for branch-level average of advisor age, experience, gender, assets under management, branch type, the number of advisors in the *Original Branch* and *Introduced Branch*, county population, population growth, population density, and political corruption. Z-scores based on standard errors clustered by merger are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Similarity	Geographic		Mutual Fund	\$100k+	Reg. Action
	(1)	(2)	(3)	(4)	(5)	(6)
Introduced Branch High Misconduct	2.061*** [4.66]	2.757*** [6.37]	3.870*** [5.80]	1.814*** [2.83]	2.238*** [4.03]	2.096*** [5.78]
IBHM $\times$ Same Ethnicity Misconduct	1.479** [2.22]					
Same Ethnicity Misconduct	0.951 [0.26]					
Original Branch High Misconduct	3.815*** [15.12]	3.875*** [12.59]	4.550*** [9.39]	2.896*** [6.37]	3.627*** [9.61]	4.020*** [9.94]
Average Advisor, Branch, and Local Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ethnicity Fixed Effects	Yes	No	No	No	No	No
Merger-Firm Fixed Effects	Yes	Subsumed	Subsumed	Yes	Yes	Yes
Merger-Firm-State Fixed Effects	No	Yes	No	No	No	No
Merger-Firm-County Fixed Effects	No	No	Yes	No	No	No
Exclude Branches with History of Reg. Actions	No	No	No	No	No	Yes
Number of Observations	11,663	6,761	2,885	9,403	9,664	8,560

Table IAX. Across-Branch Variation in Monitoring and Supervision

This table reports odds ratios from conditional logit models in which the dependent variable equals one if an above-average percentage of the branch's advisors committed misconduct during the post-merger period (three-year window). *Introduced Branch High Misconduct* equals one if an above-average percentage of the branch's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period (three-year window). *Original Branch High Misconduct* equals one if an above-average percentage of the branch's advisors committed misconduct during the pre-merger period. Column (1) includes the interaction between *Introduced Branch High Misconduct* and an indicator equal to one if the branch is part of the acquiring firm before the merger (the *Acquirer* indicator is subsumed by the merger-firm fixed effects). In column (2), the sample includes only those branches that were not introduced to new supervisors during the merger. In column (3), the sample includes only those acquirer branches that were not introduced to new supervisors during the merger. The models also include controls for branch-level average of advisor age, experience, gender, assets under management, branch type, the number of advisors in the *Original Branch* and *Introduced Branch*, county population, population growth, population density, and political corruption. *Z*-scores based on standard errors clustered by merger are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
Introduced Branch High Misconduct	2.587*** [4.96]	2.261*** [2.99]	2.915*** [4.02]
Original Branch High Misconduct	3.858*** [15.12]	4.109*** [9.65]	5.948*** [6.61]
Introduced Branch High Misconduct $\times$ Acquirer	0.734 [1.33]		
Average Advisor, Branch, and Local Controls	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes
Number of Observations	12,233	3,795	1,461

Table IAXI. Geographically Closest Branch Only Sample

This table reports odds ratios from a conditional logit model in which the dependent variable equals one if the advisor commits misconduct in the post-merger period (three-year window). *Introduced Branch High Misconduct* equals one if an above-average percentage of the advisor's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period (three-year window). *Original Branch High Misconduct* equals one if an above-average percentage of the advisor's *Original Branch* co-workers committed misconduct during the pre-merger period. *Pre-Merger Individual Misconduct* equals one if the advisor committed misconduct before the merger. The sample includes only those branches that merge to the geographically closest branch in the merger. The models include merger-firm fixed effects. The models also include controls for advisor age, experience, gender, assets under management, branch type, the number of advisors in the *Original Branch* and *Introduced Branch*, county population, population growth, population density, and political corruption. *Z*-scores clustered by merger are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)
Introduced Branch High Misconduct	1.526*** [4.71]
Original Branch High Misconduct	1.574*** [6.23]
Pre-Merger Individual Misconduct	3.816*** [12.16]
Advisor, Branch, and Local Controls	Yes
Merger-Firm Fixed Effects	Yes
Observations	92,421

Table IAXII. Post-Merger Branch Misconduct

This table reports odds ratios from conditional logit models in which the dependent variable equals one if the advisor commits misconduct during the pre-merger period (three-year window). *Post-Merger Branch High Misconduct* equals one if an above-average percentage of the advisor's post-merger co-workers committed misconduct during the post-merger period. *Introduced Branch High Misconduct* equals one if an above-average percentage of the advisor's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period. *Original Branch High Misconduct* equals one if an above-average percentage of the advisor's *Original Branch* co-workers committed misconduct during the pre-merger period. *Pre-Merger Individual Misconduct* equals one if the advisor committed misconduct before the merger. The models include merger-firm fixed effects. The models also include controls for advisor age, experience, gender, assets under management, branch type, the number of advisors in the Original Branch and Introduced Branch, county population, population growth, population density, and political corruption. *Z*-scores clustered by merger are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Dependent Variable Measured:	Post-Merger	Post-Merger
Post-Merger Branch High Misconduct (Post-Merger)	1.757*** [5.15]	1.728*** [4.89]
Introduced Branch High Misconduct (Pre-Merger)		1.266*** [3.06]
Original Branch High Misconduct (Pre-Merger)	1.427*** [4.90]	1.431*** [4.99]
Pre-Merger Individual Misconduct (Pre-Merger)	3.942*** [11.78]	3.942*** [11.78]
Advisor, Branch, and Local Controls	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes
Observations	113,963	113,963