

# Unlocking Clients: Non-compete Agreements in the Financial Advisory Industry

Umit G. Gurun  
University of Texas at Dallas

Noah Stoffman  
Indiana University

Scott E. Yonker  
Cornell University

May 30, 2019

## Abstract

We investigate the effects of non-compete agreements (NCAs) in the market for financial advice using variation in firm-level adoption of the Broker Protocol, which relaxed enforcement of NCAs between member firms. We show that overall adviser departures do not increase, but advisers move strategically to member firms and take client assets with them, generating significant relationship-based flows. When NCAs are relaxed, firms become less willing to fire advisers for misconduct and advisers' propensities to engage in misconduct and client fees increase. Our findings question whether the costs of "unlocking" clients from their advisory firms outweigh the benefits.

We thank Jawad Addoum, Brian Broughman, Mark Egan, Zoran Ivkovich, Andrew Karolyi, Alan Kwan, Kurt Lavetti, David Ng, Chris Parsons, Andy Puckett, Jonathan Reuter, Amit Seru, Matthew Serfling, Jagadeesh Sivadasan, and Merih Sevilir as well as seminar participants at the 9th Conference on Professional Asset Management at Erasmus University, Cornell University, Indiana University, the Mitsui Finance Symposium at the University of Michigan, the University of Tennessee Smokey Mountain Finance Conference, and FIRS for helpful comments.

Contact information: Umit G. Gurun, Professor of Accounting and Finance, University of Texas at Dallas, Telephone: (972) 900-5409. E-mail: [umit.gurun@utdallas.edu](mailto:umit.gurun@utdallas.edu); Noah Stoffman, Associate Professor of Finance, Indiana University, Telephone: (812) 856-5664. E-mail: [nstoffma@iu.edu](mailto:nstoffma@iu.edu); Scott E. Yonker, Associate Professor and Lynn A. Calpeter Sesquicentennial Faculty Fellow in Finance, 201J Warren Hall, Dyson School of Applied Economics and Management, Cornell University, Ithaca NY, 14853. Telephone: (607) 255-1378. E-mail: [syonker@cornell.edu](mailto:syonker@cornell.edu). Please send all correspondence to Scott Yonker.

The mobility of human capital is critical for allocating skilled labor to its most efficient use (Becker, 1962). Non-compete agreements (NCAs), which legally constrain employee mobility, provide an important impediment to such reallocation (Marx, Strumsky, and Fleming (2009) and Marx (2011)), although they can also provide an incentive for firms to invest in their employees' human capital (Rubin and Shedd, 1981) or mitigate holdup problems between entrepreneurs and investors (Hart and Moore, 1994; Kaplan and Strömberg, 2003).<sup>1</sup> These contracts are particularly important in client-facing service industries, where it is the relationship between the employee and the client that is the primary asset of the firm (Lavetti, Simon, and White, 2019). In such industries, employees without any constraints on mobility can move to a competing firm, perhaps taking many of their clients with them, essentially walking out the door with the firm's assets. Restrictions on the mobility of employees in these industries are therefore also restrictions, to some extent, on the mobility of clients.

The financial advisory industry is one such industry. Millions of investors, particularly those who are unsophisticated, trust their advisers to help them take on risk that they otherwise wouldn't have the confidence to handle (Gennaioli, Shleifer, and Vishny, 2015). Because of the central importance of trust in this industry, the relationships between clients and advisers are key to understanding individual investor welfare and decision-making.

In this paper, we study the effects of non-compete agreements in the financial advisory industry. This industry is large, with over 760,000 financial advisers managing over \$28 trillion of assets.<sup>2</sup> Extant research on non-compete agreements has generally focused on workers in technology-intensive or high-skilled industries, where the purpose of the NCA is to restrict technology transfer between competing firms. In contrast, the financial advisory industry is one where the primary goal of NCAs is to prevent the loss of clients. The use of NCAs for this purpose has increased dramatically in

---

<sup>1</sup>We refer to non-compete agreements, but include also non-solicit agreements, which allow employees to move to competing firms, but not to solicit former clients to move their business. A non-compete agreement is also known as a non-compete clause, or a covenant not to compete.

<sup>2</sup>These figures are calculated from our data, which covers the universe of financial advisers.

recent years,<sup>3</sup> and with it the need for policy makers to understand the implications of such use has become even more important.

In order to protect firms’ “relationship assets,” an adviser’s departure had historically often led to litigation by their former employer.<sup>4</sup> This changed in 2004, when three major brokerage firms agreed to the Protocol for Broker Recruiting (hereafter, “the protocol”). The stated purpose of the agreement was to “further clients’ interests of privacy and freedom of choice in connection with the movement of their Registered Representatives between firms,” although it was also seen as a way to reduce litigation expenses.<sup>5</sup> The protocol established a set of rules governing the departure of advisers, allowing an adviser to take client lists and contact information to their new employer without fear of legal action. Importantly for our purposes, this shield from litigation applies only when both the old and new employers are signatories to the protocol. The agreement was not restricted to the original signatories, and since its inception over 1,500 financial firms have joined the agreement in a staggered fashion, and until recently very few had exited.

Complete records of all firms joining and leaving the protocol are publicly available. We combine these with detailed information on all registered brokers and investment adviser representatives obtained from FINRA and the SEC. Together, these data provide us with a very rich setting in which to study the impact of loosening constraints on adviser mobility. In sharp contrast to previous research about labor mobility and non-compete agreements, we are able to exploit time-series variation in the enforcement of NCAs at the firm level, rather than relying on state-level proxies. This allows us to control for unobservable time-varying local labor market effects. In addition, our data include observations of employment records for the entire industry: every financial adviser at every firm, regardless of whether their employer is a member of the protocol.

---

<sup>3</sup>Starr, Prescott, and Bishara (2018) find that 18% of employees report being bound by non-compete agreements—including 20% of employees with less than a high school education—while 38% of employees report having signed a non-compete agreement at some point in the past. Greenhouse (2014) provides examples of non-compete agreements being required in a surprising range of jobs, including summer camp counselors, event planners, and yoga instructors.

<sup>4</sup>Throughout the paper, we use the term “advisers” to refer to both registered investment advisers and registered representatives employed at broker-dealers, who may or may not also be registered investment advisers.

<sup>5</sup>The complete text of the agreement is available at <http://www.thebrokerprotocol.com/index.php/authors/read-the-protocol>.

We exploit this setting to investigate the direct effects of non-compete agreements on the labor market for financial advisers as well as resulting spillover effects on client welfare. Specifically, we use firm-level variation in NCAs stemming from adoption of the broker protocol to ask how these restrictions are related to the mobility of financial advisers, and the extent to which clients move assets with their advisers. We also study how clients may be harmed as a result of adviser mobility by examining changes in client fees, changes in firms' willingness to discipline advisers for misconduct, and changes in advisers' propensities to engage in misconduct.

We carefully construct a staggered panel of firm entry into, and exit from, the protocol. We then exploit within-firm time series variation in membership in the protocol along with a key feature of the protocol—that only its members benefit from its protection, so the enforceability of an NCA varies for a particular adviser over time, as the set of firms party to the protocol changes. This allows us to test, for example, whether NCAs influence which firms advisers move to, and whether they are able to take more of their clients' assets with them when they move. Our empirical approach, which we discuss in detail in the next section, is designed to rule out endogeneity due to either omitted factors that may be correlated with the decision to join the protocol, or to reverse causality.

We begin studying adviser turnover by looking at the propensity to switch firms. While the protocol is not associated with a significant change in the overall likelihood of an adviser's departure, there is a substantial shift in the firms that advisers move to. Specifically, the probability of leaving for another firm in the protocol increases by approximately 50%. This effect is offset by a decline in the probability of going to a non-protocol firm. Similar results obtain when we restrict the sample to firms with more than 100 advisers, where there is less of a possibility that the firm's decision to join the protocol is related to any individual adviser's decision to leave the firm. We also exploit state-level variation in NCA enforcement and include a battery of fixed effects to further mitigate

possible endogeneity concerns.<sup>6</sup> We show that the effects of protocol membership are much weaker within the same firm in branches located in states that do not enforce NCAs.

These initial results provide strong evidence that enforcement of non-compete agreements has an economically large effect on labor mobility. Firm-level analysis yields similar results, and allows for additional tests. Although total turnover does not increase following protocol adoption, the substitution in turnover that we observe to protocol firms from non-protocol firms suggests that the average cost of turnover likely does increase, since advisers can more easily take clients with them when they move to other firms in the protocol.

To test this, we examine the effect on a firm's assets under management (AUM) when an adviser leaves. While our data do not allow us to directly observe each adviser's book of business, we do observe each firm's AUM each year, and are therefore able to relate changes in this value to adviser moves. We first show that changes in AUM are positively related to contemporaneous adviser turnover, and that this relationship is stronger for turnover in advisers within, rather than outside, the financial industry. In other words, advisers take clients with them. Separating this test by whether advisers move to another protocol firm, we find significantly larger effects when they do: an adviser leaving one protocol firm to join another, brings a client list worth almost half the average client list of the firm's existing advisers.

A recent literature has found that rates of adviser misconduct are persistent within firms, suggesting some advisory firms do a poor job of disciplining misconduct (Egan, Matvos, and Seru, 2019) and that misconduct can be contagious among co-workers (Dimmock, Gerken, and Graham, 2018). In light of our evidence that advisers take more clients with them if their firm is in the protocol, it is natural to ask whether these protocol firms become laxer with respect to punishing misconduct in order to prevent a decline in assets. To explore this, we test whether, following protocol adoption, firms become more reluctant to fire advisers after advisers engage in bad behavior.

---

<sup>6</sup>Our regression models include both branch and county-year fixed effects to account for any branch-level time-invariant unobservable characteristics and any time-varying local effects such as changes in local labor market characteristics.

For the sample of advisers working at large firms (100 advisers or more), we find that engaging in misconduct increases the probability of being fired by about 23%, but that this discipline is effectively undone when firms join the protocol. These findings support the notion that firms are indeed reluctant to fire employees once they have entered the protocol for fear of losing the assets of those advisers' clients.

Next, we test whether this reduced discipline leads advisers to engage in misconduct more frequently. In adviser-level regressions that include adviser and county-year fixed effects, we find that joining the protocol is associated with an increase in the propensity to engage in misconduct by 40%. Tests at the firm-level yield even higher estimates of over 60%.

Finally, we test whether firms pass on the increased cost of adviser turnover to their customers by increasing fees. We analyze a small sample of brokerage firms for which we have detailed information about the breakdown of revenue, and find that firms increase their fees by 14% after joining the protocol. Such a fee increase is consistent with firms recouping the costs associated with attracting new advisers. This finding highlights a potential dark side of the increased labor market mobility, as loyal investors stick with their advisers and could end up incurring higher fees.

Our paper contributes to a literature spanning finance, economics, and law examining the implications of human capital mobility in the economy. We contribute to an active literature in finance exploring incentives and behavior in the market for financial advisers, who play an influential role in determining their clients' asset choices (Mullainathan, Noeth, and Schoar, 2012; Foerster, Linnainmaa, Melzer, and Previtro, 2017), despite a failure to deliver tangible benefits (Bergstresser, Chalmers, and Tufano, 2009; Chalmers and Reuter, 2018). Gennaioli et al. (2015) argue that investment advisers, who advertise on the basis of trust, experience, and dependability, provide intangible benefits to investors. Consistent with this prediction about the importance of trust in this industry, Gurun, Stoffman, and Yonker (2018) find that the clients of investment advisers respond dramatically to a shock to trust in their advisers. Several recent papers have investigated

the importance of misconduct in this industry (Charoenwong, Kwan, and Umar, 2017; Dimmock et al., 2018; Egan et al., 2019). In a contemporaneous working paper, Clifford and Gerken (2017) investigate the effect of the Broker Protocol on investment in human capital. We contribute to this literature by providing evidence of the importance of adviser mobility on asset flows, misconduct, and fees. Consistent with adviser-client trust being of central importance, we show that a substantial portion of client assets follow advisers when they switch firms. To our knowledge, we provide the first empirical estimate in the literature of these relationship-driven flows.

Another stream of literature explores the labor market and wage premium in the financial sector (Oyer, 2008; Goldin and Katz, 2008; Philippon and Reshef, 2012; Bond and Glode, 2014; Axelson and Bond, 2015; C el erier and Vall ee, 2018; Glode and Lowery, 2016). We provide empirical support to some of the predictions generated in this literature. For example, Axelson and Bond (2015) argue that some workers in the financial industry are hard to manage because their outside options make them insensitive to threat of dismissal, a prediction consistent with our findings about the relation between rates of misconduct and forced turnover.

More broadly, the mobility of human capital has been shown to be related to growth of both industries and geographic regions (Rosegrant and Lampe, 1992; Saxenian, 1996; Franco and Filson, 2000; Klepper, 2002; Klepper and Sleeper, 2005). Studies have generally found that NCAs are an impediment to this mobility (Stuart and Sorenson, 2003; Marx et al., 2009; Marx, 2011), although Barnett and Sichelman (2016) dispute the interpretation that this leads to a reduction of innovation or causes other economic harm. Balasubramanian, Chang, Sakakibara, Sivadasan, and Starr (2018) argue that NCAs suppress wages. With the exception of Lavetti et al. (2019), who use a survey of physicians in five states, this literature has relied on state-level variation in enforcement of NCAs. We contribute to this literature by providing the first large-scale evidence of the effects of NCAs on labor mobility and bargaining power using firm-level variation in NCAs. Unlike previous studies our design allows us to control for geographic differences in local labor market conditions that could be correlated with NCA enforcement.

# 1 Empirical methodology

We are interested in understanding the effect of non-compete agreements on the market for financial advice. This includes how these agreements affect the allocation of labor, as well as effects on firm and product market outcomes. To assess the impact of NCAs we use the staggered panel of firm entry and exit into the broker protocol, which relaxed the enforcement of NCAs for advisers moving to another protocol firm.

There are very few barriers to a firm wishing to join or leave the protocol. Firms entering the protocol need only file a joinder agreement and notify the Securities Industry and Financial Markets Association (SIFMA) of their entry. Exiting requires written notification ten days prior to leaving the protocol. These low costs alleviate any concern that certain types of firms are systematically excluded from joining and that characteristics of those excluded firms could drive our results.

A challenge of estimating the causal effect of the broker protocol is that firms are likely to join strategically. Indeed, our results show that this is likely the case. In the analysis that follows, we show that total advisor turnover increases significantly in the year a firm joins the protocol, and remains high in the year following protocol membership, but then converges to pre-membership levels, suggesting that firms enter the protocol to poach advisers (see Figure 2). Investigating which firm characteristics predict protocol membership decisions, we identify firm size, past growth, being an RIA, and the amount of competition among local advisers as particularly important (Table IA.I in the Internet Appendix).

These results imply that we need to consider two sources of endogeneity. The first is endogeneity due omitted factors that predict protocol membership but cannot be included in the model. The omitted factors that we worry about can be static or time-varying at the firm, branch, and local labor market levels. We address this concern in three ways. First, we include firm–branch fixed effects in our adviser-level regressions. This approach helps us capture all time-invariant, firm- and branch-level omitted variables that may drive protocol adoption. Second, we include county–year



fixed effects to remove the effect of any time series trends that could be due to changing local economic conditions or the increasing number of firms entering the protocol across geographies, for example. The inclusion of these fixed effects rules out the possibility that either static or time-varying omitted variables at the local level influence our estimates. Third, while we control for observable firm and branch characteristics that could vary through time, this cannot account for time-varying *omitted* firm and branch characteristics that could drive protocol entry. For example, a firm may adopt a more aggressive corporate strategy that includes aggressive recruiting. This strategy could simultaneously affect many firm-level policies, as well as leading to the firm's decision to join the protocol. Such changes in firm policies would be correlated with protocol adoption, but are not a result of protocol adoption. We deal with this by exploiting several facts: (i) Protocol adoption is a firm-level decision that applies to all firm branches regardless of their location; (ii) many firms have branches in different states; and (iii) there is substantial heterogeneity in the level of enforcement of NCAs by state. Therefore, looking within a firm, protocol entry should have stronger effects on branches located in states that have stronger NCA enforcement. Throughout the analysis we test this hypothesis.

The second possible source of endogeneity is reverse causality. When regressing adviser turnover on protocol membership, for example, it is difficult to determine whether firms join the protocol because they seek to poach advisers, or whether joining the protocol causes turnover to increase. We argue that while this source of endogeneity is certainly present at the firm level, firm entry into the protocol acts as an exogenous positive shock to the transferability of the advisers' relationship assets, essentially transforming what were once firm-specific assets to general assets that advisers can take with them if they leave. This is especially true for advisers at large firms. We therefore conduct all of our analysis both with the full sample of observations as well as a subset of advisers who work for large firms, arguing that advisers at large firms do not likely influence the decisions of management to join the broker protocol.

## 2 Data and sample construction

In this section we discuss the four main data sources utilized in the study and how we use them to construct the adviser-level and firm-level datasets used in our analysis.

### 2.1 Financial adviser data

Data on financial advisers are extracted from FINRA’s web server, which provides consolidated data from their BrokerCheck website and the SEC’s Investment Adviser Public Disclosure (IAPD) website.<sup>7</sup> These data include information on all registered representatives (brokers) and investment adviser representatives (investment advisers). Following Egan et al. (2019), we refer to these two groups collectively as “financial advisers.” Data extracted from this source include the histories of broker and investment adviser registrations with firms, locations of employment, customer complaints and dispute resolutions, and industry examinations. The data are similar to that used in the main analysis of Egan et al. (2019), but also include advisers working for registered investment advisers that are not also broker-dealers.

### 2.2 Registered investment adviser data

Data on registered investment advisory firms are from Part 1A of SEC Form ADV, the Uniform Application for Investment Advisor Registration, which we obtained through a series of Freedom of Information Act (FOIA) requests. The SEC granted us all electronic filings made since the electronic filing mandate began in 2001, through the first quarter of 2017. These data include detailed information about investment advisory firms, including information about the advisory business, their owners, their clients, and any criminal behavior. Importantly, investment advisory

---

<sup>7</sup>[brokercheck.finra.org](http://brokercheck.finra.org) and [www.adviserinfo.sec.gov](http://www.adviserinfo.sec.gov), respectively. Individuals are identified by a CRD identifier, but since we don’t have a comprehensive list of all valid CRDs we begin by querying the database for all possible CRDs.

firms are required to update their filings annually, including assets under management (AUM). Using these data, we follow Gurun et al. (2018) in constructing an advisory firm-year panel dataset.

### 2.3 Broker-dealer data

Broker-dealers are identified using Form BD, which is filed by all registered broker-dealers. The data were obtained through a FOIA request to the SEC and are augmented with additional information from the SEC's website listing active broker-dealers by month, dating back to 2007.<sup>8</sup>

### 2.4 Broker protocol data

Entry and exit dates to the broker protocol are collected from a web site maintained by the law firm Carlile, Patchen, and Murphy, LLP.<sup>9</sup> The site includes a directory of all firms that have ever entered the broker protocol, and provides legal names of firms, their dates of entry and exit, and contact information. We match these firms to FINRA's unique firm-level CRD identifier by matching legal names of these entities to those in the SEC and FINRA databases. This matching is extremely precise because the protocol web site uses legal names of firms.

As of the end of 2016, there were 1,515 unique firms that had joined the broker protocol. Of these, we are able to identify the CRD for 1,325 firms, or 87.5% of the initial sample. Most firms that we are unable to match appear to be banks or trusts and are therefore not included in the adviser data. Of the matched firms, 1,166 (88.0%) had at least one adviser employed in the year prior to joining the protocol. (The remainder is firms that were established and joined the protocol prior to commencing operations or having any registered advisers.)

Table 1 reports firm entry and exit by year into the protocol. The table shows that by December 2016, only 39 of the 1,166 firms that had entered the protocol had subsequently left. Entry by number of firms peaked at 214 in the aftermath of the financial crisis, in 2009. Looking at the

---

<sup>8</sup>[www.sec.gov/help/foiadocsbdfoiahtm.html](http://www.sec.gov/help/foiadocsbdfoiahtm.html)

<sup>9</sup>[www.thebrokerprotocol.com](http://www.thebrokerprotocol.com)

number of advisers added to the protocol, the two highest years were 2004 and 2009, each with over 57,000 advisers joining. The table also shows that in the early years of the protocol entry was dominated by large broker-dealers, but that smaller registered investment advisers have made up the majority of entrants since 2010. For example, the average firm joining in 2004 had 14,323 advisers, while at the end of our sample period this number had declined to just 32.

Our analysis uses only the period of 2007 onward because of a possible survivorship bias present in our data prior to 2007, which we discuss in detail below. The table shows that our sample includes 99% of the staggered firm entries, 100% of the exits, and 207,791 advisers that were employed when their firms joined the protocol, which is 72% of the population.

Figure 1 shows the percentage of firms and advisers in the protocol by year. Panel A of the figure shows that protocol membership by firm has steadily increased over the period. By the end of 2016, 6.3% of firms with more than one adviser were party to the protocol. These rates are slightly higher for broker-dealer firms than for non-broker-dealer firms. Turning to the number of financial advisers employed at firms in the protocol, Panel B of the figure shows that by the end of 2016, 38.9% of advisers were employed by firms in the protocol. A much larger proportion of advisers employed by broker-dealers than those employed by non-broker-dealers were covered (43.3% vs. 12.6%).

## 2.5 Additional data sources

We obtain data on fee-based assets and fee revenue for a subset of large broker-dealers from the B-D Data Center on the *InvestmentNews* website<sup>10</sup>. These data, which cover approximately 75 broker-dealers per year from 2004 to 2016, come from the *InvestmentNews*' annual independent B-D surveys. We also construct a measure of state-level NCA enforceability, "Absence of NCA enforcement," based on data presented in Table 1 of Stuart and Sorenson (2003).

---

<sup>10</sup><http://www.investmentnews.com>

## 2.6 Sample construction

We construct a data set covering advisers beginning in 2003, but show in the appendix that the data are free of any survivorship bias concerns beginning in August 2007. Our main tests using these data are therefore conducted with annual panel data from the end of 2007 until the end of 2016. This final survivorship-free sample includes 5,902,522 employee-year observations. We run robustness tests using all available data back to the beginning in 2003, but acknowledge that a possible survivorship bias exists in this extended sample.

Summary statistics for the adviser-panel are displayed in Panel A of Table 2. Also shown are the subsamples based on whether the adviser is employed by a firm that is a member of the protocol during the year or not. The table shows that for 33% of employee-year observations advisers who work for firms in the protocol. Most financial advisers work for broker-dealers (97.0%). The average financial adviser has 12.1 years of experience and advisers at firms in the protocol have about 3 more years of experience, on average, than advisers at firms not in the protocol. The unconditional probability of an adviser leaving to another firm in the sample is 0.092 and the probability that they leave the profession is 0.075. We decompose observations where advisers move to other firms by whether their destination firm is a member of the protocol. Not surprisingly, the majority of moves are to non-protocol firms (79%), since there are many more of them.

We construct a misconduct indicator variable following Egan et al. (2019). During our sample, advisers engage in misconduct 0.5% of the time, which is slightly smaller than the 0.6% reported in Table 1 of Egan et al. (2019). Advisers employed by protocol member firms appear to be about 75% more likely to engage in misconduct. We also calculate a past misconduct variable, which indicates if an adviser has ever engaged in misconduct in the past. Its average is 6.8%, matching the 7% reported by Egan et al. (2019). More generally, our summary statistics closely match those of Egan et al. (2019).

We construct a firm-level sample by collapsing the adviser-level data each year. This gives us 133,519 firm-year observations from 2007 until 2016, for about 13,350 firms per year. In 4.0% of the firm-years, firms are members of the broker protocol and in 31.1% firms are broker-dealers. The average firm has 59 advisers, but this distribution is highly skewed with a median of only four. Moreover, broker protocol members have many more advisers than firms that are not members. Turnover, defined as the percentage of advisers leaving the firm during the year plus the percentage of advisers joining the firm during the year divided by two, is about 12.6% and for the average firm in the sample the number of advisers grows by about 4.2% per year. Misconduct per adviser occurs at a rate of 0.83% in the sample, and as previously noted it is much less frequent among non-protocol firms.

Part of our analysis relies on estimating the effect of the protocol on asset flows across firms. We can perform these analyses only for firms that are also registered investment advisers with the SEC. The RIA sample indicator reveals that firms in about 33.7% (44,995 firm-years) of the firm-years also file Form ADV and report AUM. The average AUM for these firms is \$3.96 billion, but the median is much smaller at \$236 million. Firms in the protocol manage roughly twice as many assets as those that are not. The average asset growth per year is 7.3%.

### 3 Results

In the following sections, we test whether NCAs restrict the mobility of workers and their relationship assets. We begin by investigating the direct effects of NCAs by testing whether the relaxing of NCAs affects financial adviser mobility (Section 3.1) and advisers' abilities to move relationship-assets (Section 3.2). We then turn to estimating the indirect effects of relaxing NCAs, which effectively increases the bargaining power of advisers with their firms. Here, we test whether the relaxation of NCAs changed firms' willingness to discipline their advisers for bad behavior and whether this in turn influenced advisers' propensities to engage in financial misconduct (Sections 3.3 and 3.4). We

then test whether firms pass any added costs associated with their loss of control of their relationship assets on to their clients in the form of higher fees (Section 3.5).

### 3.1 Adviser turnover

We begin by estimating the relationship between broker protocol membership and turnover.

#### 3.1.1 Adviser-level analysis

To test whether protocol membership increases adviser turnover, we estimate the following linear probability model using our annual adviser-employer matched panel from 2007 until 2016:

$$\text{Turnover}_{j,i,c,t+1} = \alpha_{i,c} + \gamma_{c,t} + \beta (\text{Firm in protocol})_{j,i,t} + \gamma' \text{Controls}_{i,t} + \epsilon_{j,i,t}, \quad (1)$$

where  $\text{Turnover}_{j,i,c,t+1}$  is an indicator that is one if individual  $j$ 's employment at firm  $i$  in a branch located in county  $c$  ends during year  $t + 1$ ,  $(\text{Firm in the protocol})_{i,t}$  is an indicator variable that is one if firm  $i$  is in the broker protocol by the end of year  $t$ , and  $\alpha_{i,c}$  and  $\gamma_{c,t}$  are branch (firm–county) and county–year fixed effects, respectively. Control variables include the log of the number of advisers employed at firm  $i$  at the end of year  $t$ , the log of the number of years of experience of adviser  $j$  by the end of year  $t$ , and a series of dummy variables indicating the exams/qualifications of the financial advisers, which follow the definitions used in Egan et al. (2019). One exception is that we include a dummy variable “investment adviser” that indicates whether the adviser is currently registered as an investment adviser. Egan et al. (2019), instead use data on exams passed to infer registration as an investment adviser. The variable of interest is “Firm in protocol.” If protocol membership increases the propensity of advisers to leave their firms, then the estimate of  $\beta$  should be significantly positive.

We estimate regression (1) using four alternative definitions of turnover. First, we use “Leave to another firm,” an indicator variable that is one if an adviser leaves one firm and joins another. We further decompose this variable into two categories: whether the firm that the adviser joins is a member of the protocol or not, creating the indicator variables “Leave to a protocol firm” and “Leave to a non-protocol firm.” Our final measure of turnover is “Leave profession,” which includes turnover events where the adviser departs the firm and never rejoins another firm during our sample period.

Since all advisers in a firm are treated simultaneously, our empirical design may have what Abadie, Athey, Imbens, and Wooldridge (2017) call an “assignment” problem. We address this by clustering standard errors by firm throughout the analysis.<sup>11</sup> (Sampling problems are not an issue in our study since the sample essentially includes the population of financial advisers.)

Panel A of Table 3 shows the regression results for these four turnover variables. In column 1, the estimate of  $\beta_p$  is indistinguishable from zero, indicating that advisers do not abnormally depart their firms once their firms become a part of the broker protocol. However, the evidence in columns 2 and 3 shows that protocol membership redirects advisers toward other protocol firms and away from non-protocol firms. The estimate of  $\beta_p$  in column 2 is 1.81, indicating that once advisers’ firms join the protocol, those advisers are 1.8% more likely to leave to another protocol firm. The unconditional probability of leaving to join a firm in the protocol is 3.4%, so the economic magnitude of this effect is substantial, increasing the probability by over 50%. The estimate in column 3 indicates that the probability that advisers leave to join non-protocol firms following their firm joining the protocol declines by about 2.0%, which essentially offsets the increase in movement toward protocol firms. Finally, in column 4, we see that advisers are slightly less likely to leave the profession after their firms join the protocol. This could be a result of increased bargaining power, which ultimately leads to better employment terms and overall satisfaction.

---

<sup>11</sup>Two-way clustering by firm and year is not appropriate, since we only have nine years of data. Typical advice is that there should be at least 50 clusters to make clustering the standard errors appropriate.



Panel B shows the results when the sample is restricted to advisers who work for large firms (those with 100 advisers or more). The sample averages about 590 of these firms per year, which is about the 96 percentile of firm size. These results are less susceptible to reverse causality since individual advisers are less likely to be able to influence their firms' decisions to join the protocol. The results confirm that advisers are more likely to leave to other firms that are members of the protocol and less likely to leave to firms not members of the protocol once their firms become protocol members. These changes almost exactly offset one another with the probability of moving to another firm in the protocol increasing by 1.7%, while leaving to a firm not in the protocol decreases by about 1.7%. Interestingly, the protocol membership has no effect on whether advisers leave the profession in this subsample. Table IA.II in the Internet Appendix shows the effect of the inclusion of various fixed effects in our model. Branch fixed effects explain the most variation and have largest effect on the magnitude of the estimates of  $\rho$ .

To further the argument of causality, we next test whether the effects of protocol membership are stronger in branches that are located in states that enforce non-compete agreements. To do this, we estimate regression (1) separately for advisers working at branches located in states that enforce NCAs and for those working in states that do not. We then test whether  $\rho$  is larger in magnitude for the sample of advisers working in states that enforce NCAs. If advisers are aware of the state-level enforceability of these agreements, then the protocol should have more of an effect on turnover in states that enforce NCAs. Of course, broker protocol can still influence adviser mobility in states where NCAs are not influenced if advisers are unaware of the strength of enforceability in their states. Our state-level measure of NCA enforcement is based on the "Absence of NCA enforcement" from Table 1 of Stuart and Sorenson (2003), which is also used by Samila and Sorenson (2011). In the analysis, we label "State enforces NCAs? - Yes" if "Absence of NCA enforcement" = 0 in the state where the adviser works and we label "State enforces NCAs? - No" otherwise.

Table 4 shows the results of these tests. For both the full sample (Panel A) and the sample of advisers working for large firms (Panel B) the coefficient estimates indicate that the effects of

the protocol are stronger in states that actually enforce NCAs. The estimates of  $\beta_p$  in column 1 are roughly twice the size of those in column 2 and these differences are significantly different from zero at the 10% level. Similarly, the decrease in the probability of advisers leaving to firms that are not in the protocol following protocol membership is larger in magnitude for advisers working in states that enforce NCAs. The estimate of  $\beta_p$  is 2.14 in column 3 of Panel A, while the coefficient in column 4 is 1.52. This difference of 0.61, significant at the 10% level. In Panel B, the difference in  $\beta_p$  estimates is of similar magnitude, but standard errors are slightly larger, resulting in a two-sided tests statistic with a  $p$ -value = 0.12.

### 3.1.2 Firm-level analysis

We next estimate the relationship between broker protocol membership and turnover at the firm level. Specifically, we estimate:

$$\text{Turnover}_{i,t} = \alpha_i + \beta_t + \beta_p(\text{Firm in protocol})_{i,t} + \beta' \text{Controls}_{i,t-1} + \epsilon_{i,t}, \quad (2)$$

where  $\text{Turnover}_{i,t}$  is the adviser turnover at firm  $i$  during calendar year  $t$ ,  $(\text{Firm in protocol})_{i,t}$  is a dummy variable that is equal to one if firm  $i$  is a member of the broker protocol at the end of year  $t$ . As controls, we include the log of the number of advisers employed by the firm at the end of year  $t-1$ . If relaxing NCAs increases turnover, then the coefficient estimate on  $\beta_p$  should be significantly greater than zero. To ensure that our findings are not driven by outliers, all dependent variables are winsorized at the 1st and 99th percentiles.

Table 5 reports results from this regression. The dependent variable in column 1 is within-industry turnover, which is defined as the average of the percentage of advisers who either join from or leave to firms within the financial advisory industry during the year. Percentages are calculated based on the number of advisers at the end of year  $t-1$ . This measure of turnover can be decomposed into two components: turnover with firms in the protocol; and turnover with firms

that are not in the protocol. Outside industry turnover is the average of the percentage of rookie advisers who join a firm and the percentage of advisers who leave a firm to retire. Results for each of these categories of turnover are presented separately in columns 2, 3, and 4, respectively. Finally, in columns 5 and 6, we investigate the relationship between protocol membership and firms hiring new (rookie) advisers and also the percent growth in all advisers. Detailed definitions of all variable are presented in the Appendix.

The estimates in column 1 indicate that protocol membership is associated with 1.2 percentage points higher turnover. Given that the average level of turnover is 12.6%, this implies a 10% increase in total adviser turnover. The source of this increase in total turnover provides further evidence that it is the protocol that accounts for at least part of this increase: turnover with firms in the protocol increases by about 136% ( $1.35/0.99$ ) after joining the protocol, while turnover with non-protocol firms declines. This evidence is consistent with the earlier adviser-level tests. As mentioned earlier, firms choose whether and when to join the protocol, so the significantly positive coefficients on “Firm in the protocol” in columns 5 and 6 both indicate that firms tend to join the protocol when they are expanding or looking to poach advisers from other firms.

We next explore the dynamics of these findings by estimating the following regression:

$$\begin{aligned}
\text{Turnover}_{i,t} = & \alpha_{i,c} + \beta_{c,t} + \beta_{p,2}(\text{Firm joins protocol})_{i,t+2} + \beta_{p,1}(\text{Firm joins protocol})_{i,t+1} \\
& + \beta_{p,0}(\text{Firm joins protocol})_{i,t} + \beta_{p,1}(\text{Firm joins protocol})_{i,t-1} \\
& + \beta_{p,2}(\text{Firm joins protocol})_{i,t-2} + \beta_{p,>2}(\text{Firm joins protocol})_{i,<t-2} \\
& + \gamma' \text{Controls}_{i,t-1} + \epsilon_{i,t},
\end{aligned} \tag{3}$$

where  $(\text{Firm joins protocol})_{i,t}$  is an indicator variable that is one if firm  $i$  joins the broker protocol in year  $t$ . Therefore, the  $\beta_{p,s}$  coefficients estimate the changes in turnover relative to average turnover three years or more prior to joining the broker protocol. For instance,  $\beta_{p,1}$  estimates any anticipatory effect of protocol membership on turnover in the year prior to membership, while

$p_{,0}$  captures abnormal turnover in the first year of membership. The parameter  $p_{,>2}$  captures the average abnormal turnover after three or more years of protocol membership.

The estimates of the  $p_{,s}$ 's from the regressions are plotted in Figure 2. The endogenous entry of firms into the protocol is evident in the figures. In panel A, we see that total turnover spikes by seven percentage points, an increase of over 50%, in years that a firm joins the protocol, and remains significantly abnormally high in the second year of protocol membership, but subsequently reverts to levels observed prior to membership. Panel B shows that it is the turnover with other firms in the protocol that generates the initial jump in turnover, consistent with firms entering the protocol to poach advisers. Also consistent with this is that there is no initial effect on turnover with firms that are not protocol members (Panel C). Abnormal turnover with protocol firms remains abnormally high in the years after joining, 1.12 percentage points or about double pre-protocol levels. Turnover with non-protocol firms (Panel C) does not decline abnormally until the second year of membership, but it remains persistently low thereafter. The figure indicates that three years of protocol membership is sufficient to change the turnover path that advisers take, moving to and from other firms in the protocol rather than outside of the protocol.

### 3.2 Asset flows

The firm-level results from section 3.1 show that turnover increases after firms join the protocol, but that this increased turnover seems to be driven by a firm's choice to enter the protocol, which is endogenous. It appears that firms seeking to grow join the protocol to poach advisers. This increased hiring shows up in firm-level turnover. Our adviser-level analysis, which we argue estimates the causal effect of the broker protocol, does not indicate that their firms joining the protocol causes advisers to be more likely to leave their firms for other firms in general. However, both the firm- and adviser-level results show that turnover tilts toward other firms in the protocol once a firm joins.

These findings suggest that while total turnover does not increase, the average cost of turnover likely does for firms once they join the protocol.

The finding that advisers increasingly move to protocol firms is consistent with the idea that they do so because they can take clients with them when they depart. In this section we test whether AUM follow advisers and in particular, whether more assets follow advisers from firms in the protocol, especially when they join other firms in the protocol. To test this we estimate the following fixed effects OLS regression model:

$$\begin{aligned}
\Delta\log(\text{AUM})_{i,t} = & \alpha_i + \gamma_t + \beta_{n,o}(\% \Delta \text{ in adv. outside industry})_{i,t} \\
& + \beta_{n,n}(\% \Delta \text{ in adv. within industry})_{i,t} \\
& + \beta_{n,p}(\% \Delta \text{ in adv. with protocol firms})_{i,t} \\
& + \beta_{p,o} \text{Protocol}_{i,t} \times (\% \Delta \text{ in adv. outside industry})_{i,t} \\
& + \beta_{p,n} \text{Protocol}_{i,t} \times (\% \Delta \text{ in adv. within industry})_{i,t} \\
& + \beta_{p,p} \text{Protocol}_{i,t} \times (\% \Delta \text{ in adv. with protocol firms})_{i,t} \\
& + \beta_p (\text{Firm in protocol})_{i,t} + \gamma' \text{Controls}_{i,t-1} + \epsilon_{i,t},
\end{aligned} \tag{4}$$

where  $\Delta\log(\text{AUM})_{i,t}$  is the change in the log of AUM of firm  $i$  during year  $t$ ,  $(\text{Firm in protocol})_{i,t}$  is an indicator variable if firm  $i$  is a member of the broker protocol by the end of year  $t$ , and  $\alpha_i$  and  $\gamma_t$  are firm and year fixed effects, respectively.

The  $\% \Delta$  in adv. variables are various decompositions of the percentage change in the number of advisers at firm  $i$  during year  $t$ . “ $\% \Delta$  in adv. within industry” is the percentage change in advisers to and from other firms in our sample. Therefore, it is the difference between advisers joining from other firms and advisers leaving to other firms, regardless of whether those firms are protocol members. “ $\% \Delta$  in adv. outside industry” is the percentage change in advisers entering or leaving our sample. This includes the difference between advisers who enter our sample for the first time and those that leave the profession (i.e., they never show up in our data again) and also the difference between

advisers joining after being unemployed for at least a year and those leaving and being unemployed for at least a year. These two components sum to the total percentage change in advisers at the firm during the year, so  $\% \Delta \text{ in adv.}_{i,t} = \% \Delta \text{ in adv. outside industry}_{i,t} + \% \Delta \text{ in adv. within industry}_{i,t}$ , where the scaling factor in all measures is the number of advisers at the end of year  $t - 1$ . We separate these components because we hypothesize that advisers moving to or from other firms in the industry are more likely to move assets with them than are rookie advisers, or those who leave the industry. This leads to the prediction that  $\beta_{n,n} > \beta_{n,o}$ .

Finally, “ $\% \Delta \text{ in adv. with protocol members}$ ” is the difference between the percentage of advisers joining from protocol member firms and those leaving to protocol member firms. As before, the scaling factor is the total number of advisers at the end of year  $t - 1$ . Constructing our variables this way allows us to test for differences in the elasticities of AUM to advisers for those joining from or leaving to protocol and non-protocol firms.

In regression (4), the coefficients  $\beta_{n,o}$ ,  $\beta_{n,n}$ , and  $\beta_{n,n} + \beta_{n,p}$  capture the elasticities of AUM with respect to outside industry advisers, non-protocol advisers, and protocol advisers for firms not in the protocol, respectively. The coefficients  $\beta_{p,o}$ ,  $\beta_{p,n}$ , and  $\beta_{p,p}$  capture the incremental effect on those elasticities due to firms being in the protocol.

Recall that in order for financial advisers to move assets from one firm to another without legal repercussions, both firms must be in the protocol. Therefore, our main hypothesis is that changes in AUM should be most sensitive to the changes in advisers at protocol firms moving to and from other protocol firms, or  $\beta_{p,p} > 0$ . In addition, there is no reason to believe that the change in AUM should be any more sensitive to changes in non-protocol advisers or changes in advisers from outside the industry if the firm is a protocol member, implying that  $\beta_{p,o} = 0$  and  $\beta_{p,n} = 0$ .

We estimate various forms of regression (4) using a firm-level annual panel data set constructed from electronic filings of Form ADV as described in section 2.2. In Table 2 we showed that this sample covers roughly 34% of firm-year observations in the sample. This decline in sample size is

due to the fact that not all firms that employ financial advisers are registered investment advisers, which are required to make regular filings with the SEC.

Table 6 shows the results of our tests. In column 1, we include only the “% $\Delta$  in advisers” as our variable of interest in order to test the general contemporaneous relationship between changes in AUM and changes in advisers. The coefficient estimate is 0.104, which implies that a 1% increase in the number of financial advisers at the average firm is associated with about 10.4 basis points greater AUM. In column 2, we decompose the change in advisers between those coming within and outside the industry. The estimates show that changes within the industry are associated with much larger changes in AUM. A 1% increase in advisers coming from outside the industry is associated with a 6 bps increase in AUM, while the same change in advisers coming from other firms within the industry leads to an increase of about 15 bps. This is consistent with the idea that advisers do move assets with them when they change firms.

In column 3, we test whether changes in AUM are more sensitive to changes in within-industry advisers when those changes are due to advisers either arriving from or leaving to firms in the protocol. The evidence indicates that there is such a difference. The coefficient estimate on “% $\Delta$  in advisers with protocol firms” is 0.169 and significant at better than the 1% level. This indicates that a 1% increase in advisers joining from firms in the protocol is associated with 16.9 bps greater increase in AUM than the same increase in advisers coming from firms that are not in the protocol, and along with the coefficient estimate of 0.128 on “% $\Delta$  in advisers within industry” it implies that a 1% increase in advisers joining from protocol firms is associated with a 29.7 bps increase in AUM.

In column 4, we include a control for firms being protocol members and also interact protocol membership with changes in within and outside industry hires. The results indicate that the AUM of firms that are protocol members grow by about 3.8% more per year than that of non-members. More importantly, the coefficient estimate on the interaction term between protocol membership and changes in within industry advisers is 0.154 and is statistically different from zero at better than

the 1% significance-level. So while a 1% increase in advisers from within the industry is associated with a 14 bps increase in AUM for non-protocol firms, it is associated with about a 30 bps increase for firms that are protocol members. Importantly, being a protocol member does not change the sensitivity of changes in AUM to changes in advisers from outside the industry.

Finally, in column 5, we estimate the full version of equation 4. Consistent with our hypotheses, we find that  $\beta_{p,p} = 0.184 > 0$  and we fail to reject the hypotheses that  $\beta_{p,o} = 0$  and  $\beta_{p,n} = 0$ . These findings indicate that changes in AUM are particularly sensitive to changes in adviser going to and joining from firms that are protocol members, especially when the firm itself is a protocol member. Our estimate of the change in AUM for a one percent change in the number of advisers coming from protocol firms to a firm in the protocol is  $12.6 + 14.1 + 7.7 + 18.4 = 52.8$  bps. In other words, a new adviser joining a firm in the protocol from another firm in the protocol, on average, brings clients with assets worth about half of the average assets of the firm’s existing advisers. It is possible that some of this 53 bps of AUM growth is due to factors other than new advisers bringing assets with them, but the 18.4 bps due to protocol-to-protocol firm turnover likely represents a lower bound of the size of the effect, as there is no other reason to believe that assets would grow by more for firms in the broker protocol than those that are not when they hire advisers from other protocol firms.

### 3.3 Disciplining advisers

In the previous section, we provided evidence that advisers move more of their clients’ assets with them once their firm joins the protocol. This means that losing advisers is more costly for firms in the protocol. We therefore ask whether this makes firms reluctant to fire advisers, even when the advisers engage in bad behavior. To test this we modify regression (1) to include an indicator variable that is one if the adviser engages in misconduct during year  $t$  (“Misconduct”), using the definition of Egan et al. (2019), and the interaction of “Misconduct” with whether the firm is a member of the protocol. Our dependent variable is forced turnover, which is defined as turnover



for which the adviser is subsequently unemployed for at least 90 days on the assumption that few individuals would choose to be unemployed for that long. Formally, we estimate:

$$\begin{aligned} \text{Turnover}_{j,i,c,t+1} = & \alpha_{i,c} + \gamma_{c,t} + \beta_m(\text{Misconduct})_{j,t} + \beta_p(\text{Firm in protocol})_{j,i,t} \\ & + \beta_{p,m}(\text{Firm in protocol})_{j,i,t} \times (\text{Misconduct})_{j,t} + \gamma' \text{Controls}_{i,t} + \epsilon_{j,i,t}, \end{aligned} \quad (5)$$

where definitions of all variables follow those previously described.  $\beta_m$  measures turnover sensitivity to misconduct, which should be positive, at least in egregious cases of misconduct.  $\beta_p$  measures the difference in turnover propensity for firms once they join the protocol. If turnover is more costly for firms, then they may be more reluctant to fire advisers following protocol entry, implying that this coefficient could be negative.  $\beta_{p,m}$  captures the difference in turnover sensitivity to misconduct attributable to firms being protocol members. If the increased cost of turnover for protocol firms makes them less willing to discipline their advisers for bad behavior, then the estimate of  $\beta_{p,m}$  will be negative.

The results are presented in Table 7 for the full sample and the sample of advisers who work for firms with at least 100 advisers. Following the earlier adviser-level analysis on turnover, both of these samples are further split by state-level NCA enforcement and we test whether protocol membership has a larger impact on turnover sensitivity to misconduct in states that enforce NCAs. The results from the full sample (column 1), indicate that engaging in misconduct increases the probability of being fired by 46 bps, which is about a 15% increase in the unconditional probability of forced turnover. In the same sample, being a member of the protocol essentially undoes this discipline. The estimates of  $\beta_{p,m}$  is -0.54 and is significant at the 5% significance level. When this sample is split between advisers who work in states that do and do not enforce NCAs (columns 2 and 3), we find an interesting result. Advisers who work in states that enforce NCAs are more likely to be fired for engaging in misconduct, but advisers at firms that relax the enforcement of NCAs by being members of the protocol are not more likely to be fired for engaging in misconduct. This suggests that both state-level enforcement of NCAs and firm-level enforcement are important

to the balance of power between firms and advisers. In the sample of advisers who work in states that do not enforce NCAs, we find that engaging in misconduct does not increase the probability of being fired irrespective of whether the advisers firm is a protocol member or not. Focusing on the sample of advisers working for firms with at least 100 advisers, we find similar results. In general, these results are consistent with firms being more reluctant to fire employees once they enter the protocol for fear of losing AUM.

### 3.4 Misconduct

Since firms are less likely to discipline their advisers for misconduct, it natural to ask whether this affects the propensity of advisers to engage in misconduct. We therefore test whether adviser misconduct increases once firms join the broker protocol. We conduct both adviser- and firm-level tests. In the adviser-level tests, we regress “Misconduct,” an indicator variable described in Section 3.3 on “Firm in protocol,” controls, and two different specification of fixed effects. In the first specification, we include firm-county and county-year fixed effects. Egan et al. (2019) show that advisers’ past misconduct is a strong predictor of future misconduct. We therefore add “Past misconduct” as a control in these regressions. In the second specification we include adviser fixed effects, instead of firm-county. Adviser fixed effects could be important to include to control for any time-invariant, unobservable, individual characteristics of managers.

The results of these tests are presented in Panel A of Table 8. In columns 1 and 2 of the Table, which uses the model with firm-county and county-year fixed effects, the coefficient estimates on “Firm in protocol” are both positive, but only significantly statistically different from zero in the sample of advisers working for large firms ( $t$ -stats of 1.5 and 1.7). Once adviser fixed effects are included in the model, the coefficient estimates on “Firm in protocol” become both statistically and economically significant. The estimate in column 4, which is estimated using the sample of advisers working for employers with at least 100 advisers, indicates that the probability that an adviser

engages in misconduct increases by 20 bps once his employer joins the protocol. On a unconditional probability of misconduct of 47 bps, this is an increase in likelihood of over 40%.

In Panel B, of Table 8 we present the firm-level regression results. In this regression, we regress “Misconduct per 100 advisers,” defined as the number of advisers who engage in misconduct during the year divided by the number of advisers at the end of the previous year times 100, on “Firm in protocol”, control variables, and firm and year fixed effects. The results presented in column 1 indicate that misconduct, on average, significantly increases for firms after joining the protocol. The magnitudes are again large and consistent with the adviser-level analysis — “Misconduct per 100 advisers” increases by 0.53 on an unconditional mean of 0.83, implying an increase in misconduct of over 60%.

We note that Clifford and Gerken (2017) run similar regressions and come to the opposite conclusion. We show in Table IA.VI in the Internet Appendix the robustness of our results to various subsamples and also from extending the sample period back to 2003. In all models that include adviser fixed effects our inferences are unchanged.

### **3.5 Commissions and fees**

In this section, we investigate whether composition of broker revenues changed over the years in response to the turnover induced by the protocol. A broker–dealer can generate revenue from two main sources, commissions and fees. Because a commission-based broker derives his income from selling particular investment products (such as high-fee mutual funds), a potential conflict of interest can arise between brokerages and their clients. A brokerage fee, on the other hand, is a flat rate that customers pay brokers to manage money regardless of the type of investment the client has in her portfolio. This flat rate is generally expressed as a percentage of asset under management. Previous research has found that financial advisors play an influential role in determining their clients’ asset choices. For instance, Foerster et al. (2017) show that a large portion of the advisors’

personal portfolios look similar to client portfolios, suggesting advisor’s personal behavior explains a substantial amount of variation in client behavior. Given the influence of the broker dealer on clients’ assets, a natural question is whether the protocol created an incentive for firms to change their revenue structure in order to offset the costs firms experience from increased protocol-induced turnover.

We investigate this issue in two steps. We begin by examining whether commission-based revenue increased relative to fee-based revenue. We focus on the commission-based component because studies have also found that some advisors steer customers into particular financial products that may be not in their customers’ best interests. For instance, Mullainathan et al. (2012) find that some advisors in the U.S. steer investors from well diversified portfolios to high fee mutual funds. Such opportunistic behavior has also been found in other financial products (Anagol, Cole, and Sarkar, 2017) and other countries (Bhattacharya, Hackethal, Kaesler, Loos, and Meyer, 2012; Hackethal, Inderst, and Meyer, 2012). In our second analysis, we investigate whether firms pass part of the protocol-related costs on to customers through an increase in the flat-fee rate.

To test our hypotheses, we use broker-dealer revenue breakdown information from the B-D Data Center maintained on the *InvestmentNews* website. As discussed in Section 2.5, our dataset covers 2004 to 2016 and contains approximately 75 large broker-dealers per year. For each of these firms, we observe both the amount of commission revenue and fee revenue, as well as the total asset under management that generated the fees. From these, we calculate two variables. The first one is the percentage of commission fee in total revenue, i.e. “Commission share” = Commission fee revenue / Total revenue. The second variable is “Fee rate”, i.e. Fee rate = Fee Revenues / Fee-based AUM.

We estimate the relationship between broker protocol membership and commission share and fee rate by regressing these measures on “Firm in protocol,” control variables, and year and firm fixed effects. Table 9 reports the regression results.

The evidence in column 1 and 2 shows that while the commission-based revenues reduced slightly, 2.26%, after the joining the broker but the reduction is small compared to the average commission share, 75%. The evidence in column 3 and 4 shows that fee rate increased by 14.4 basis point after the joining the broker protocol. Off of the mean fee rate, 100 basis points, the increase is not only statistically significant, but also economically large. These results collectively suggest that broker-dealers do not change their revenue composition after joining the protocol, but they increase the fee derived from fee-based accounts significantly.

## 4 Robustness

### 4.1 Subsample analysis

To check the robustness of the results, we replicate all adviser-level results (Tables 3, 4, 7, and 8) for three different subsamples. The results are displayed in Tables IA.III, IA.IV, IA.V, and IA.VI, respectively, in the Internet Appendix.

First we limit the sample to advisers who are brokers. Several studies of financial advisers (i.e. Egan et al. (2019); Clifford and Gerken (2017)) do not include financial advisers who are investment advisers, but not brokers in their samples. To ensure that our results are not driven by these advisers we exclude them. In general, the main results are unchanged by excluding these advisers from the sample. This is not that surprising since the majority of financial advisers are registered brokers.

Next we estimate our results for the subsample of advisers who work for only one firm. When advisers are registered with multiple firms simultaneously a choice must be made about which firm is the main employer. Again we do our best by basing our choice on the initial registration date, but other choices could be made. The main results do not materially change when limiting the analysis to this sample.

Finally, we reproduce the results for the extended sample from 2003-2016, acknowledging that this sample may have a survivorship bias. This bias is particularly important for analysis including forced turnover and misconduct, since advisers who are either fired or engage in misconduct are likely to disappear from the sample. Indeed, both the turnover sensitivity to misconduct and misconduct results are weaker in this sample. However, the results on turnover are in line with the main analysis.

## 4.2 Protocol withdrawals and adviser exits

As an out-of-sample test of the impact of NCAs on adviser turnover, we take advantage of two recent events that followed our initial data collection. In October and November of 2017 two major financial advisory firms exited the broker protocol. In order to withdraw from the broker protocol firms must submit a letter of their intent, but the actual withdrawal does not become effective for ten business days. We therefore examine whether an abnormal percentage of advisers leave these firms during the nine day window after the withdrawal submission, but prior to the withdrawal taking effect.

Figure 3 plots the percentage of 2017 annual turnover occurring each business day of the year (daily number of advisers leaving the firm scaled by total number advisers leaving the firm during 2017) for Morgan Stanley and UBS Financial Services. Morgan Stanley submitted its withdrawal notice on October 24, 2017 and UBS followed suit on November 20, 2017. Because of the ten day grace period, the last days that Morgan Stanley and UBS Financial Services were members of the broker protocol were November 2, 2017 and November 30, 2017, respectively. These dates are indicated in Figure 3. The average percentage of annual turnover per day is 0.39% ( $=1/257$ ) during 2017. On the final days that Morgan Stanley and UBS were members of the protocol, they experienced 5.73% and 9.92%, respectively, of their daily attrition for the entire year. That is, on November 2, 62 advisers left Morgan Stanley and on November 30, 94 advisers left UBS. While

we do not conduct formal statistical tests, note that the standard deviation of daily turnover for Morgan Stanley and UBS in 2017 was 0.48% and 0.73%, respectively. This indicates that exits were over ten standard deviations from the mean for both brokerages on their last days in the broker protocol and in both cases they were the maximum for the year. It is also worth noting that, of those advisers who left either Morgan Stanley or UBS on those dates, only two (1.3%) joined firms that were not members of the broker protocol. This evidence points toward a strong causal relationship between NCAs and adviser turnover.

## 5 Conclusion

Financial advisers have historically been routinely bound by non-compete agreements, prohibiting them from soliciting clients if they were to leave their current employer. This made it difficult for clients to follow their advisers if they switched employers. Encouraged by FINRA, which wanted to increase client freedom of choice, and in an effort to reduce legal costs, this arrangement changed in 2004 with the signing of the Broker Protocol by several major brokerage firms. The agreement has subsequently been joined by over 1,500 firms.

Using variation in the adoption of the protocol, we investigate the effects of the relaxation of non-compete agreements on the market financial advice. We identify several important effects of this relaxation. First, adviser turnover temporarily increases after firms join the protocol; this appears to be mainly driven by firms joining the protocol in order to poach advisers from other firms. More importantly, the pattern of adviser mobility changes significantly: following protocol adoption, advisers increasingly move to other firms that are members of the protocol rather than those that are not party to the agreement. Second, client assets follow advisers. Changes in assets under management for firms in the protocol are much more sensitive to net adviser moves with other protocol firms than they are to net adviser moves with non-protocol firms. This is not true for firms that are not in the protocol, indicating that relaxing NCA enforcement allows advisers to

take their clients with them. Third, firms in the protocol are substantially less likely to fire advisers for engaging in misconduct following protocol adoption and the incidence of adviser misconduct also increases. Finally, client fees increase by about 14% following protocol adoption, suggesting that the increased costs associated with adviser turnover are passed on to consumers.

Our findings have important policy implications. For example, our results suggest that investors could be made better off with more disclosure on the changing nature of broker incentives. A recent regulatory trend in consumer markets is the notion that consumers are better off if they are made aware of their options prior to borrowing or signing a financial contract. For example, in the mortgage market, the new TILA–RESPA Integrated Disclosure rules require mortgage providers to supply easy-to-understand disclosure statements. Similarly, the “Schumer box” requires credit card terms to be presented in a standardized format. The aim of these disclosures is to simplify credit terms and help consumers understand rates and fees. Our analysis provides evidence that some investors could incur higher fees as a result of adviser transitions, suggesting that additional disclosure to clients may be particularly useful at the time of such transitions. While FINRA Rule 2273 “Educational Communication Related to Recruitment Practices and Account Transfers,” which became effective in 2016, mandates that clients be notified of potential conflicts of interests that could stem from these transitions, our results call into question whether the rule went far enough. Informing clients of signing bonuses paid to their advisers, for example, might lead clients to ask how firms afford to pay such bonuses.

Some legislators in Washington have also expressed concern that NCAs are used by firms to suppress the wages of lower-level employees.<sup>12</sup> Theory suggests that employee compensation should be greater in the absence of NCAs because of the creditable threat of employees to move to competitors. While we cannot directly observe compensation data, our results suggest that the relaxation of NCA enforcement leads to a significant increase in the bargaining power of financial advisers. Future research may be able to explore the effects of NCAs on compensation more directly.

---

<sup>12</sup>See, for example, <https://www.c-span.org/video/?c4796572/sen-van-hollen-questions-ftc-chair-joseph-simons>.



## Bibliography

- Abadie, A., Athey, S., Imbens, G. W., Wooldridge, J., 2017. When should you adjust standard errors for clustering? Tech. rep., National Bureau of Economic Research.
- Anagol, S., Cole, S., Sarkar, S., 2017. Understanding the advice of commissions-motivated agents: Evidence from the indian life insurance market. *Review of Economics and Statistics* 99 (1), 1–15.
- Axelson, U., Bond, P., 2015. Wall street occupations. *Journal of Finance* 70 (5), 1949–1996.
- Balasubramanian, N., Chang, J. W., Sakakibara, M., Sivadasan, J., Starr, E., 2018. Locked in? the enforceability of covenants not to compete and the careers of high-tech workers. University of Michigan Working Paper.
- Barnett, J., Sichelman, T. M., 2016. Revisiting labor mobility in innovation markets. SSRN working paper 2758854.
- Becker, G. S., 1962. Investment in human capital: A theoretical analysis. *Journal of political economy* 70 (5, Part 2), 9–49.
- Bergstresser, D., Chalmers, J. M. R., Tufano, P., May 2009. Assessing the costs and benefits of brokers in the mutual fund industry. *Review of Financial Studies* 22 (10), 4129–4156.
- Bhattacharya, U., Hackethal, A., Kaesler, S., Loos, B., Meyer, S., 2012. Is unbiased financial advice to retail investors sufficient? answers from a large field study. *The Review of Financial Studies* 25 (4), 975–1032.
- Bond, P., Glode, V., 2014. The labor market for bankers and regulators. *Review of Financial Studies* 27 (9), 2539–2579.
- Célérier, C., Vallée, B., 2018. Returns to talent and the finance wage premium. Forthcoming, *Review of Financial Studies*.
- Chalmers, J., Reuter, J., 2018. Is conflicted investment advice better than no advice?
- Charoenwong, B., Kwan, A., Umar, T., 2017. Who should regulate investment advisers? SSRN working paper 2899883.
- Clifford, C., Gerken, W., 2017. Investment in human capital and labor mobility: Evidence from a shock to property rights. University of Kentucky Working Paper.
- Dimmock, S. G., Gerken, W. C., Graham, N. P., 2018. Is fraud contagious? Coworker influence on misconduct by financial advisors. *Journal of Finance* 73 (3), 1417–1450.
- Egan, M., Matvos, G., Seru, A., 2019. The market for financial adviser misconduct. *Journal of Political Economy* 127 (1), 233–295.
- Foerster, S., Linnainmaa, J. T., Melzer, B. T., Previtro, A., 2017. Retail financial advice: does one size fit all? *Journal of Finance* 72 (4), 1441–1482.
- Franco, A. M., Filson, D., 2000. Knowledge diffusion through employee mobility. Tech. rep., Claremont Colleges Working Papers in Economics.

- Gennaioli, N., Shleifer, A., Vishny, R., 2015. Money doctors. *Journal of Finance* 70 (1), 91–114.
- Glode, V., Lowery, R., 2016. Compensating financial experts. *Journal of Finance* 71 (6), 2781–2808.
- Goldin, C., Katz, L. F., 2008. Transitions: Career and family life cycles of the educational elite. *American Economic Review* 98 (2), 363–69.
- Greenhouse, S., 2014. Noncompete clauses increasingly pop up in array of jobs. *New York Times* 8.
- Gurun, U. G., Stoffman, N., Yonker, S. E., 2018. Trust busting: The effect of fraud on investor behavior. *The Review of Financial Studies* 31 (4), 1341–1376.
- Hackethal, A., Inderst, R., Meyer, S., 2012. Trading on advice. SSRN working paper 1701777.
- Hart, O., Moore, J., 1994. A theory of debt based on the inalienability of human capital. *Quarterly Journal of Economics* 109 (4), 841–879.
- Kaplan, S. N., Strömberg, P., 2003. Financial contracting theory meets the real world: An empirical analysis of venture capital contracts. *Review of Economic Studies* 70 (2), 281–315.
- Klepper, S., 2002. The capabilities of new firms and the evolution of the us automobile industry. *Industrial and corporate change* 11 (4), 645–666.
- Klepper, S., Sleeper, S., 2005. Entry by spinoffs. *Management science* 51 (8), 1291–1306.
- Lavetti, K., Simon, C., White, W. D., 2019. The impacts of restricting mobility of skilled service workers: Evidence from physicians. *Journal of Human Resources*, 0617–8840R5.
- Marx, M., 2011. The firm strikes back: non-compete agreements and the mobility of technical professionals. *American Sociological Review* 76 (5), 695–712.
- Marx, M., Strumsky, D., Fleming, L., 2009. Mobility, skills, and the Michigan non-compete experiment. *Management Science* 55 (6), 875–889.
- Mullainathan, S., Noeth, M., Schoar, A., 2012. The market for financial advice: An audit study. Tech. rep., National Bureau of Economic Research.
- Oyer, P., 2008. The making of an investment banker: Stock market shocks, career choice, and lifetime income. *Journal of Finance* 63 (6), 2601–2628.
- Philippon, T., Reshef, A., 2012. Wages and human capital in the us finance industry: 1909–2006. *Quarterly Journal of Economics* 127 (4), 1551–1609.
- Rosegrant, S., Lampe, D. R., 1992. *Route 128: lessons from Boston’s high-tech community*. Basic Books, Inc.
- Rubin, P. H., Shedd, P., 1981. Human capital and covenants not to compete. *Journal of Legal Studies* 10 (1), 93–110.
- Samila, S., Sorenson, O., 2011. Noncompete covenants: Incentives to innovate or impediments to growth. *Management Science* 57 (3), 425–438.

Saxenian, A., 1996. *Regional advantage*. Harvard University Press.

Starr, E., Prescott, J., Bishara, N., 2018. Noncompetes in the US labor force. SSRN working paper 2625714.

Stuart, T. E., Sorenson, O., 2003. Liquidity events and the geographic distribution of entrepreneurial activity. *Administrative Science Quarterly* 48 (2), 175–201.

## Appendix

### A.I Verifying the survivorship-bias free sample

We use historical brokerage and investment adviser registration dates for advisers to construct a survivorship-bias free adviser-firm-year panel dataset. Data from the SEC's IAPD website provides historical beginning and ending investment adviser registration dates, while FINRA's BrokerCheck web site provides beginning and ending registered representative (broker) registration dates. Financial advisers can be dually registered, or registered only as a broker or investment adviser. When constructing the employment spells we use the union of dates spanned by broker and investment registrations to determine the dates of employment of dually registered financial advisers with their firms.

We downloaded these data in July 2017, after an update to FINRA's website Terms of Use explicitly provided permission for researchers to download the data for academic purposes.<sup>13</sup> The FINRA website states that it maintains information on the website for brokers who have been registered within the last ten years or possibly longer,<sup>14</sup> indicating that we can have confidence that our sample is free of possible survivorship bias beginning in 2007.

To verify this, we calculate the last year that each financial adviser is included in the data. Panel A of Figure A.I shows the distribution of these final years. Almost none of the advisers file their final deregistration prior to 2007, which is ten years prior to when we collected the data.<sup>15</sup> It therefore appears that FINRA deletes entire adviser histories from the publicly-available data once they have been de-registered for ten years. Panel B provides additional support for this claim by comparing the distribution of an adviser's final month of registration in 2007 to all other years. The typical distribution is fairly even across all months, although with an uptick in December. But in 2007 the sample is completely different—there are almost no final de-registrations until July in that year, which is precisely ten years before we downloaded the data.

In light of this evidence, we conclude that our data are survivorship-bias free only during the period beginning in August, 2007.

---

<sup>13</sup>See item 5 of FINRA BrokerCheck® Terms of Use, modified July 17, 2017.

<sup>14</sup>See [www.finra.org/investors/about-brokercheck](http://www.finra.org/investors/about-brokercheck).

<sup>15</sup>The figure does not include 2017. About 68% of advisers in the sample are still registered in 2017.

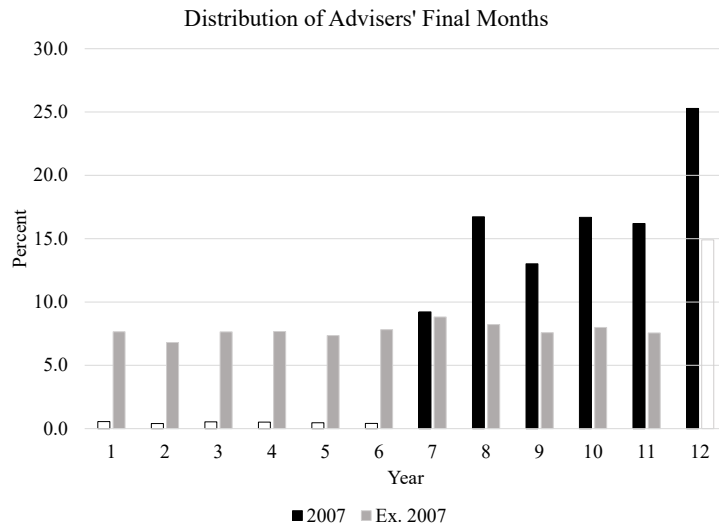
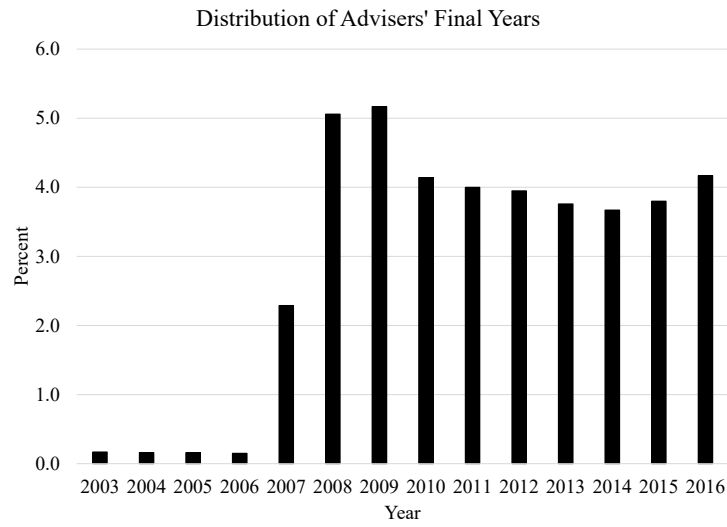


Figure A.I: Distribution of advisers' final years and months

Panel a of the figure displays the distribution of the advisers' final years of registration for data extracted from the BrokerCheck and IAPD websites in July of 2017 for the years 2003 to 2017. The year 2017 is not included in the graph, but accounts for 68% of the observations. Panel b of the figure shows the distribution of final months for 2007 and for the years 2003–2016, excluding 2007.

One complication in constructing the employee–employer matched dataset is that the data provide registration dates rather than actual employment dates. An adviser could, for example, de-register but stay with the firm in a non-advisory role. This is unlikely to be much of an issue, however, because the cost of maintaining registration is low relative to the potential benefits, so even if financial advisers move into different roles, they will most likely keep their registrations active. Nevertheless, we assume that an adviser is continually employed with a firm if an advisers’ registration ends at a firm but then begins again at that same firm within 365 days, provided that the adviser has not registered with another firm during the intervening period. We also remove registrations lasting less than two weeks.

A second complication is that many financial advisers are registered simultaneously with multiple firms. In our sample, 91.9% of advisers–year observations are from advisers registered with one firm, while the corresponding numbers for those registered at two firms is 7.7%. The remaining 0.4% of observations represent advisers simultaneously registered at more than two firms. In cases of multiple employment, we assume that the primary employer is the firm with which the adviser has been registered the longest. We provide evidence of robustness to this assumption by showing that our main results hold when focusing only on observations where advisers work for a single employer.

Finally, we limit our sample to firms with at least two advisers located within the United States, since we are interested in the effects of non-compete agreements.

## A.II Variable definitions

Table A.I: Adviser-level variable definitions

Adviser-level variables	Definition	Source
Firm in protocol	An indicator variable that is one if any of the adviser's employers are members of the protocol as of the end of the calendar year.	Broker protocol website, IAPD, BrokerCheck
Log (number of advisers)	Log of the total number of advisers employed by the adviser's primary employer at the end of the calendar year.	IAPD, BrokerCheck
Log (years experience)	Log of the number of years since the adviser is first registered as a financial adviser at any firm.	IAPD, BrokerCheck
Investment adviser	An indicator variable that is one if the adviser is registered as an investment adviser during the year.	IAPD
Sec. agent st. law (63)	An indicator variable that is one if the adviser passed the Series 63 exam by the end of the year.	IAPD, BrokerCheck
Gen. sec. rep. (7)	An indicator variable that is one if the adviser passed the Series 7 exam by the end of the year.	IAPD, BrokerCheck
Inv. co. prod. rep. (6)	An indicator variable that is one if the adviser passed the Series 6 exam by the end of the year.	IAPD, BrokerCheck
Gen. sec. principal (24)	An indicator variable that is one if the adviser passed the Series 24 exam by the end of the year.	IAPD, BrokerCheck
Number of other qual.	The number of exams passed other than Series 6, 7, 24, 63, 65, or 66 by the end of the year.	IAPD, BrokerCheck
Past misconduct	An indicator variable that is one if the adviser has a misconduct record as of the previous year, where misconduct is defined according to Egan et al. (2019).	IAPD, BrokerCheck
Absence of NCA enforcement	An indicator variable that is one if the state where the adviser works does not enforce non-compete agreements.	Table 1 of Stuart and Sorenson (2003); Samila and Sorenson (2011).

Table A.I continues on the following page.

Adviser-level variables	Definition	Source
Leave to another firm	An indicator variable that is one if the adviser leaves his/her firm during the year and subsequently joins another firm in the data.	IAPD, BrokerCheck
Leave to a protocol firm	An indicator variable that is one if the adviser leaves his/her firm during the year and subsequently joins a firm that is a member of the protocol.	Broker protocol website, IAPD, BrokerCheck
Leave to a non-protocol firm	An indicator variable that is one if the adviser leaves his/her firm during the year and subsequently joins a firm that is not a member of the protocol.	Broker protocol website, IAPD, BrokerCheck
Leave profession	An indicator variable that is one if the adviser leaves his/her firm during the year and never registers with another financial firm.	IAPD, BrokerCheck
Forced turnover	An indicator variable that is one if “Leave to another firm” is one and the number of days before joining another firm is greater than 90.	IAPD, BrokerCheck
Days unemployed	The number of days between deregistering with the adviser’s current employer and registering with the adviser’s new employer when “Leave to another firm” is one.	IAPD, BrokerCheck
Misconduct indicator	Following Egan et al. (2019), this is an indicator variable that is one if any of the following disclosures appear for an adviser during the year: Customer Dispute—Settled; Employment Separation After Allegations; Regulatory—Final; Criminal—Final Disposition; Customer Dispute—Award/Judgement; or Civil—Final. These six types of disclosure are selected from a total of twenty-three categories.	IAPD, BrokerCheck
Broker-dealer indicator	An indicator variable that is one if the adviser’s primary employer is a registered broker-dealer.	Form BD, IAPD, BrokerCheck
RIA indicator	An indicator variable that is on if the adviser’s primary employer is a registered investment adviser.	SEC Form ADV, IAPD, BrokerCheck
Primary employer	Employer who has employed the adviser the longest.	IAPD, BrokerCheck



Table A.II: Firm-level variable definitions

Firm-level variables	Definition	Source
Firm in protocol	An indicator variable that is one if any of the firm is a member of the protocol as of the end of the calendar year.	Broker protocol website
Log (number of advisers)	Log of the total number of advisers employed the firm at the end of the calendar year.	IAPD, BrokerCheck
Within industry turnover	The average of the percentage of the firm's advisers leaving to other firms and the percentage of the firm's advisers joining from other firms, where percentages are calculated based on the number of advisers at the firm at the end of the previous calendar year.	IAPD, BrokerCheck
Turnover with firms in protocol	The average of the percentage of the firm's advisers leaving to firms in the protocol and the percentage of the firm's advisers joining from firms in the protocol, where percentages are calculated based on the number of advisers at the firm at the end of the previous calendar year.	IAPD, BrokerCheck
Turnover with firms not in protocol	The average of the percentage of the firm's advisers leaving to firms not in the protocol and the percentage of the firm's advisers joining from firms not in the protocol, where percentages are calculated based on the number of advisers at the firm at the end of the previous calendar year.	IAPD, BrokerCheck
Outside industry turnover	The average of the percentage of the firm's advisers leaving the firm and the industry (last time of registration in the sample) and the percentage of the firm's advisers joining the firm who are joining financial firm for the first time (first time registering with a firm in the sample), where percentages are calculated based on the number of advisers at the firm at the end of the previous calendar year.	IAPD, BrokerCheck
% Rookie advisers	The percentage of firm's advisers hired during the year who are joining a financial firm for the first time (first time registering with a firm in the sample), where percentages are calculated based on the number of advisers at the firm at the end of the previous calendar year.	IAPD, BrokerCheck
% in advisers	The percent change in the total number of advisers at the firm.	IAPD, BrokerCheck
% in advisers outside industry	The difference in the percentage of rookie advisers hired by the firm (registering for the first time) and the percentage of the firms advisers leaving the industry (deregistering for the last time), where percentages are scaled by the total number of advisers at the firm at the end of the previous calendar year.	IAPD, BrokerCheck

Table A.II continues on the following page.

Firm-level variables	Definition	Source
% in advisers within industry	The difference in the percentage of advisers hired from other firms within the industry by the firm and the percentage of the firms advisers leaving to other firms in the industry, where percentages are scaled by the total number of advisers at the firm at the end of the previous calendar year.	IAPD, BrokerCheck
% in advisers with protocol firms	The difference in the percentage of advisers hired from protocol member firms and the percentage of the firm's advisers leaving to protocol member firms, where percentages are scaled by the total number of advisers at the firm at the end of the previous calendar year.	Broker protocol website, IAPD, BrokerCheck
Misconduct per adviser ( $\times 100$ )	The percentage of the firm's advisers who engaged in misconduct, as defined by the "Misconduct indicator," during the calendar year.	IAPD, BrokerCheck
Broker dealer indicator	An indicator variable that is one if firm is a registered broker-dealer.	Form BD, IAPD, BrokerCheck
RIA indicator	An indicator variable that is on if the firm is a registered investment adviser.	SEC Form ADV, IAPD, BrokerCheck
Log (AUM)	Change in the log of total assets under management from the end of the previous fiscal year to the end of the current fiscal year.	SEC Form ADV, Part 1a, Item 3F1c
Log (AUM)	Log of total assets under management at the end of the fiscal year.	SEC Form ADV, Part 1a, Item 3F1c

Table 1: Entry and exit in the broker protocol

The table shows the number of firms and advisers that entered or exited the broker protocol each year. The number of advisers is the total number of advisers registered with the firm as of the end of the calendar year prior to the entry or exit year. The table also reports the percentage of entering or exiting firms that are registered broker dealers and the percentage of advisers who work for registered broker dealers. Also reported are the total number of entries/exits (“Total”) and the total number covered for our sample period (“Sample total”), as well as the percentage of the total covered by our sample, which begins in 2007.

Year	Entry					Exit				
	Number			% BD		Number			% BD	
	Firms	Advisers	Adv./ Firm	Firms	Advisers	Firms	Advisers	Adv./ Firm	Firms	Advisers
2004	4	57,290	14,323	100	100					
2005	1	432	432	100	100					
2006	10	23,178	2,318	90	100					
2007	18	17,968	998	67	97					
2008	71	26,769	377	46	100					
2009	214	57,596	269	44	99					
2010	135	15,196	113	29	96	3	133	44	0	0
2011	119	12,530	105	27	98	5	48	10	20	35
2012	110	11,127	101	23	84	9	1,302	145	22	41
2013	91	6,632	73	14	91	5	447	89	40	91
2014	134	43,659	326	17	98	5	70	14	0	0
2015	124	11,932	96	20	96	7	283	40	29	86
2016	135	4,382	32	15	84	5	28	6	0	0
Total	1,166	288,691		28.39	97.80	39	2,311		18	52
Sample	1,151	207,791		27.54	96.94	39	2,311		18	52
% total	99	72				100	100			

Table 2: Summary statistics

The table displays summary statistics for variables used in the analysis. Reported in Panel A are summary statistics for the survivorship bias-free adviser-level panel of advisers who work for employers that employ at least two financial advisers, which includes 5,902,522 adviser-year observations from the end of 2007 through the end of 2016. Reported in Panel B are summary statistics for the firm-level panel of all firms that employ at least two financial advisers, which includes 133,519 firm-year observations from the end of 2007 through the end of 2016. All variables are defined in the Appendix in Tables A.I and A.II. Also reported are means of the sample split by whether the employer (adviser-panel) or the firm is a member of the broker protocol at the end of the calendar year and the significance levels of univariate  $T$ -tests testing the differences in these means.  $T$ -statistics are computed using robust standard errors, clustered by firm. Significance levels are denoted by  $c$ ,  $b$ , and  $a$ , which correspond to 10%, 5%, and 1% levels, respectively. Data on AUM is available only for firms that register as investment advisers with the SEC. For about 37% of the firm-year observations the firm is registered as an investment adviser.

	Mean	Median	St. Dev.	1st Per.	99th Per.	Not in Protocol Mean	In Protocol Mean
Panel A: Adviser-level							
Firm in protocol	0.328	0.000	0.469	0.000	1.000	0.000	1.000
Years experience	12.070	10.000	9.653	0.000	40.000	10.979	14.306 <sup>a</sup>
Log (years experience)	2.206	2.398	0.970	0.000	3.714	2.110	2.402 <sup>a</sup>
Investment adviser	0.390	0.000	0.488	0.000	1.000	0.284	0.608 <sup>a</sup>
Registered representative	0.994	1.000	0.079	1.000	1.000	0.991	0.999 <sup>a</sup>
Sec. agent st. law (63)	0.737	1.000	0.440	0.000	1.000	0.742	0.727
Gen. sec. rep. (7)	0.669	1.000	0.470	0.000	1.000	0.584	0.846 <sup>a</sup>
Inv. co. prod. rep. (6)	0.378	0.000	0.485	0.000	1.000	0.459	0.213 <sup>a</sup>
Gen. sec. principal (24)	0.139	0.000	0.346	0.000	1.000	0.139	0.138
Number of other qual.	0.469	0.000	0.860	0.000	4.000	0.393	0.625 <sup>a</sup>
Past misconduct	0.068	0.000	0.251	0.000	1.000	0.055	0.095 <sup>a</sup>
Absence of NCA enforcement	0.200	0.000	0.400	0.000	1.000	0.190	0.220 <sup>b</sup>
Leave to another firm (%)	9.221	0.000	28.933	0.000	100.000	9.116	9.438
Leave to a protocol firm (%)	3.444	0.000	18.236	0.000	100.000	1.878	6.654 <sup>a</sup>
Leave to a non-protocol firm (%)	5.777	0.000	23.331	0.000	100.000	7.237	2.783 <sup>a</sup>
Leave profession (%)	7.498	0.000	26.337	0.000	100.000	8.437	5.575 <sup>a</sup>
Unforced turnover (%)	6.254	0.000	24.213	0.000	100.000	5.787	7.211
Forced turnover (%)	2.968	0.000	16.969	0.000	100.000	3.329	2.227 <sup>a</sup>
Days unemployed	26.582	0.000	55.436	0.000	266.000	33.744	16.672 <sup>a</sup>
Misconduct indicator	0.005	0.000	0.070	0.000	0.000	0.004	0.007 <sup>a</sup>
Become entrepreneur (%)	0.039	0.000	1.977	0.000	0.000	0.033	0.051 <sup>a</sup>
Broker-dealer indicator	0.970	1.000	0.170	0.000	1.000	0.958	0.996 <sup>a</sup>
RIA indicator	0.636	1.000	0.481	0.000	1.000	0.486	0.941 <sup>a</sup>

Table 2 continues on the following page.

Table 2 continued from the previous page.

	Mean	Median	St. Dev.	1st Per.	99th Per.	Not in Protocol Mean	In Protocol Mean
Panel B: Firm-level							
Firm in protocol	0.040	0.000	0.195	0.000	1.000	0.000	1.000
Number of advisers	59.116	4.000	659.252	2.000	865.000	41.810	477.051 <sup>a</sup>
Log (number of advisers)	1.806	1.386	1.334	0.693	6.763	1.758	2.961 <sup>a</sup>
Turnover	12.596	0.000	20.038	0.000	100.000	12.461	15.859 <sup>a</sup>
Turnover with firms in protocol	0.989	0.000	3.048	0.000	16.667	0.895	3.242 <sup>a</sup>
Turnover with firms not in protocol	5.096	0.000	10.794	0.000	50.000	5.095	5.119
Other turnover	5.965	0.000	10.790	0.000	50.000	5.950	6.313 <sup>b</sup>
% Rookie advisers	3.964	0.000	11.680	0.000	50.000	3.982	3.531 <sup>a</sup>
% $\Delta$ in advisers	0.042	0.000	0.277	-0.500	1.333	0.041	0.087 <sup>a</sup>
% $\Delta$ in advisers outside industry	1.339	0.000	19.529	-50.000	66.667	0.013	0.029 <sup>a</sup>
% $\Delta$ in advisers within industry	2.906	0.000	17.341	-35.714	86.770	0.028	0.057 <sup>a</sup>
% $\Delta$ in advisers with protocol firms	0.629	0.000	5.365	-13.230	33.333	0.005	0.031 <sup>a</sup>
Misconduct per adviser ( $\times 100$ )	0.830	0.000	6.021	0.000	25.000	0.801	1.517 <sup>a</sup>
Broker dealer indicator	0.311	0.000	0.463	0.000	1.000	0.310	0.347 <sup>b</sup>
RIA indicator	0.337	0.000	0.473	0.000	1.000	0.330	0.515 <sup>a</sup>
AUM (\$ millions)	3,964.780	236.243	32,883.510	11.852	81,884.300	3,730.438	7,601.002
Log (AUM)	5.714	5.417	1.689	2.407	11.281	5.683	6.170 <sup>a</sup>
$\Delta$ Log (AUM)	0.073	0.082	0.310	-1.179	1.270	0.069	0.143 <sup>a</sup>

Table 3: Adviser turnover and the protocol - adviser level evidence

The table displays regression results from linear probability models estimated using OLS (Equation 1 in the text) of various measures turnover in the next year on “Firm in protocol,” which is an indicator variable that is one if the financial adviser is employed by a firm that is member of the broker protocol as of the end of the calendar year. The table reports the results using two samples. In Panel A, the analysis uses the entire adviser-level sample described in Panel A of Table 2. In Panel B the sample is restricted to employees who are employed by firms with at least 100 advisers. The dependent variable in column 1 is “Leave to another firm,” which is an indicator variable that is one if the adviser who departs in year  $t + 1$  joins another firm by August of 2017 (the time of download for our data). We further decompose this variables by whether the firm that the adviser joins is a member of the protocol or not, creating the indicator variables “Leave to a protocol firm” and “Leave to a non-protocol firm.” Our final measure of turnover is “Leave profession.” This includes turnover events where the adviser departs the firm, but never rejoins another firm by August of 2017. All models include firm-county and county-year fixed effect. County is based on the primary branch where the adviser works.  $T$ -statistics are computed using robust standard errors (reported in parentheses), clustered by firm. Significance levels are denoted by  $c$ ,  $b$ , and  $a$ , which correspond to 10%, 5%, and 1% levels, respectively.

	Leave to another firm (1)	Leave to a protocol firm (2)	Leave to a non-protocol firm (3)	Leave profession (4)
Panel A: Full sample				
Firm in protocol	-0.183 (0.885)	1.812 <sup>a</sup> (0.592)	-1.995 <sup>a</sup> (0.502)	-0.667 <sup>b</sup> (0.275)
Log (number of advisers)	3.359 <sup>c</sup> (1.896)	2.204 (1.673)	1.155 (0.882)	0.734 <sup>b</sup> (0.341)
Log (years experience)	-1.628 <sup>a</sup> (0.201)	-0.569 <sup>a</sup> (0.125)	-1.059 <sup>a</sup> (0.114)	-2.163 <sup>a</sup> (0.134)
Investment adviser	0.253 (0.361)	0.819 <sup>b</sup> (0.320)	-0.566 <sup>a</sup> (0.129)	-4.139 <sup>a</sup> (0.218)
Gen. sec. rep. (7)	3.300 <sup>a</sup> (0.215)	1.392 <sup>a</sup> (0.137)	1.907 <sup>a</sup> (0.160)	-3.610 <sup>a</sup> (0.281)
Inv. co. prod. rep. (6)	-0.251 (0.323)	-0.096 (0.210)	-0.155 (0.193)	-0.272 <sup>b</sup> (0.131)
Gen. sec. principal (24)	-0.675 <sup>b</sup> (0.268)	-0.399 <sup>b</sup> (0.200)	-0.276 <sup>b</sup> (0.123)	-1.159 <sup>a</sup> (0.112)
Number of other qual.	0.227 <sup>a</sup> (0.084)	0.119 <sup>c</sup> (0.064)	0.108 <sup>a</sup> (0.039)	-0.047 (0.065)
County-Year FE	Y	Y	Y	Y
Firm-county FE	Y	Y	Y	Y
Mean of the dep. var.	9.221	3.444	5.777	7.498
Adj-R-squared	0.10	0.11	0.09	0.06
Observations	5,891,188	5,891,188	5,891,188	5,891,188

Table 3 continues on the following page.

Table 3 continued from the previous page.

	Leave to another firm (1)	Leave to a protocol firm (2)	Leave to a non-protocol firm (3)	Leave profession (4)
Panel B: Sample of firms with at least 100 advisers				
Firm in protocol	0.010 (0.948)	1.743 <sup>a</sup> (0.664)	-1.733 <sup>a</sup> (0.543)	-0.279 (0.280)
Log (number of advisers)	3.208 (2.480)	2.931 (2.143)	0.277 (1.050)	0.333 (0.445)
Log (years experience)	-1.730 <sup>a</sup> (0.217)	-0.594 <sup>a</sup> (0.136)	-1.136 <sup>a</sup> (0.123)	-2.273 <sup>a</sup> (0.146)
Investment adviser	0.378 (0.368)	0.836 <sup>b</sup> (0.324)	-0.457 <sup>a</sup> (0.133)	-3.962 <sup>a</sup> (0.225)
Gen. sec. rep. (7)	3.159 <sup>a</sup> (0.239)	1.434 <sup>a</sup> (0.154)	1.726 <sup>a</sup> (0.176)	-3.873 <sup>a</sup> (0.308)
Inv. co. prod. rep. (6)	-0.459 (0.352)	-0.134 (0.223)	-0.325 (0.214)	-0.292 <sup>b</sup> (0.136)
Gen. sec. principal (24)	-0.486 (0.307)	-0.405 <sup>c</sup> (0.231)	-0.080 (0.139)	-0.910 <sup>a</sup> (0.120)
Number of other qual.	0.248 <sup>a</sup> (0.095)	0.123 <sup>c</sup> (0.071)	0.125 <sup>a</sup> (0.045)	-0.016 (0.074)
County-Year FE	Y	Y	Y	Y
Firm-county FE	Y	Y	Y	Y
Mean of the dep. var.	9.032	3.676	5.356	7.330
Adj-R-squared	0.10	0.11	0.09	0.06
Observations	5,217,482	5,217,482	5,217,482	5,217,482

Table 4: Adviser turnover, the protocol, and state-level enforcement

The table displays regression results from linear probability models estimated using OLS (Equation 1 in the text) of various measures of turnover in the next year on “Firm in protocol” for subsamples of advisers split by state-level NCA enforcement. We categorize state-level enforcement of NCAs based on the variable “Absence of NCA enforcement” which is a dummy variable that indicates that the state where the adviser works does not enforce non-compete agreements. This variable is based on Table 1 of Stuart and Sorenson (2003) and used in Samila and Sorenson (2011). We categorize states that do not enforce NCAs as those where “Absence of NCA enforcement”=1 and those that do enforce NCAs as states where “Absence of NCA enforcement”=0. The table displays regression results for the full sample of observations (Panel A) and for the sample of advisers working for firms with at least 100 advisers (Panel B). In both cases these samples are split by advisers working in states where NCA are enforced (columns 1 and 3) and where they are not (Columns 2 and 4). All models include firm-county and county-year fixed effects and the controls included in Table 3. *T*-statistics are computed using robust standard errors (reported in parentheses), clustered by firm. Using the same robust standard error estimation we also report  $\hat{p}_{p,yes}$   $\hat{p}_{p,no}$  and the associated standard errors. Significance levels are denoted by *c*, *b*, and *a*, which correspond to 10%, 5%, and 1% levels, respectively.

Dep. variable	Leave to a protocol firm		Leave to a non-protocol firm	
	State enforces NCAs?		State enforces NCAs?	
Sample	Yes	No	Yes	No
	(1)	(2)	(3)	(4)
Panel A: Full sample				
Firm in protocol	2.023 <sup>a</sup> (0.636)	1.062 <sup>c</sup> (0.596)	-2.135 <sup>a</sup> (0.537)	-1.523 <sup>a</sup> (0.489)
Adj-R-squared	0.11	0.12	0.09	0.10
Observations	4,712,699	1,178,489	4,712,699	1,178,489
$\hat{p}_{p,yes}$ $\hat{p}_{p,no}$		0.961 <sup>c</sup> (0.534)		-0.612 <sup>c</sup> (0.369)
Panel B: Sample of firms with at least 100 advisers				
Firm in protocol	1.976 <sup>a</sup> (0.708)	0.888 (0.697)	-1.874 <sup>a</sup> (0.585)	-1.252 <sup>b</sup> (0.499)
Adj-R-squared	0.11	0.12	0.08	0.09
Observations	4,177,988	1,039,494	4,177,988	1,039,494
$\hat{p}_{p,yes}$ $\hat{p}_{p,no}$		1.088 <sup>c</sup> (0.610)		-0.622 (0.396)
Controls	Y	Y	Y	Y
County-Year FE	Y	Y	Y	Y
Firm-county FE	Y	Y	Y	Y



Table 5: Adviser turnover and the protocol - firm-level evidence

The table displays regression results from fixed effect OLS regressions (Equation 2 in the text) of various measures of turnover on “Firm in protocol,” which is an indicator variable that is one if the firm is member of the broker protocol as of the end of the calendar year. The analysis uses the entire firm-level sample described in Panel B of Table 2. The dependent variable in column 1 is within industry turnover, which is defined as the average of the percentage of advisers joining and leaving the firm from/to other firms in the industry during the year. Percentages are calculated based on the number of advisers at the end of year  $t - 1$ . Within industry turnover can be decomposed into two components, turnover to and from firms in the protocol (column 2), and turnover to and from firms not in the protocol (column 3). The dependent variable in column 4 is outside industry turnover, which is the average of the percentage of rookie advisers hired and the percentage of retiring advisers. In columns 5 and 6, we investigate the relationship between protocol membership and firms hiring new (rookie) advisers and also the percent growth in all advisers. All models include firm and year fixed effects.  $T$ -statistics are computed using robust standard errors (reported in parentheses), clustered by firm. Significance levels are denoted by  $c$ ,  $b$ , and  $a$ , which correspond to 10%, 5%, and 1% levels, respectively.

	Within industry turnover (1)	Turnover with firms in protocol (2)	Turnover with firms not in protocol (3)	Outside industry turnover (4)	% $\Delta$ Rookie advisers (5)	% $\Delta$ advisers (6)
Firm in protocol	1.243 <sup>c</sup> (0.734)	1.349 <sup>a</sup> (0.142)	-1.350 <sup>a</sup> (0.396)	1.042 <sup>a</sup> (0.328)	1.726 <sup>a</sup> (0.356)	5.178 <sup>a</sup> (1.175)
Lagged log (number of advisers)	-11.718 <sup>a</sup> (0.344)	-0.606 <sup>a</sup> (0.045)	-5.455 <sup>a</sup> (0.186)	-4.490 <sup>a</sup> (0.184)	-6.994 <sup>a</sup> (0.219)	-34.447 <sup>a</sup> (0.631)
Mean of the dep. var.	12.596	0.989	5.096	5.965	3.964	4.245
Adj-R-squared	0.40	0.20	0.34	0.30	0.19	0.22
Observations	130,990	130,990	130,990	130,990	130,990	130,990
Year FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y

Table 6: Asset flows, advisor turnover, and the protocol

The table displays regression results from fixed effect OLS regressions (Equation 4 in the text) of changes in log (AUM) on contemporaneous changes in the percentage of advisers employed by the firm (% $\Delta$  in advisers) in column 1. In column 2, we decompose the percentage change in managers, by whether they are leaving or joining from outside the industry (% $\Delta$  in advisers outside the industry) or within the industry (% $\Delta$  in advisers within the industry). In column 3, we add an additional variable, which captures the percentage change in advisers to and from other firms that are members of the broker protocol (% $\Delta$  in advisers with protocol firms). In columns 4 and 5, we interact these measures of percentage changes in advisers with “Firm in protocol,” which is an indicator variable that is one if the firm is member of the broker protocol as of the end of the calendar year. The analysis uses the firm-year observations from the sample described in Panel B of Table 2 that are Registered Investment Advisers with the SEC (about 37% of the sample). All continuous variables are winsorized at the first and ninety-ninth percentiles to remove the effects of outliers. All models include firm and year fixed effects.  $T$ -statistics are computed using robust standard errors (reported in parentheses), clustered by firm. Significance levels are denoted by  $c$ ,  $b$ , and  $a$ , which correspond to 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
% $\Delta$ in advisers	0.104 <sup>a</sup> (0.007)				
% $\Delta$ in advisers outside industry		0.064 <sup>a</sup> (0.009)	0.058 <sup>a</sup> (0.009)	0.062 <sup>a</sup> (0.009)	0.058 <sup>a</sup> (0.009)
% $\Delta$ in advisers within industry		0.150 <sup>a</sup> (0.011)	0.128 <sup>a</sup> (0.012)	0.143 <sup>a</sup> (0.011)	0.126 <sup>a</sup> (0.012)
% $\Delta$ in advisers with protocol firms			0.169 <sup>a</sup> (0.029)		0.141 <sup>a</sup> (0.030)
Firm in protocol				0.038 <sup>a</sup> (0.014)	0.038 <sup>a</sup> (0.014)
Firm in protocol $\times$ % $\Delta$ outside industry				0.002 (0.036)	-0.030 (0.038)
% $\Delta$ within industry				0.154 <sup>a</sup> (0.051)	0.077 (0.061)
% $\Delta$ in advisers with protocol firms					0.184 <sup>c</sup> (0.111)
Lagged log(AUM)	-0.252 <sup>a</sup> (0.007)	-0.252 <sup>a</sup> (0.007)	-0.252 <sup>a</sup> (0.007)	-0.252 <sup>a</sup> (0.007)	-0.253 <sup>a</sup> (0.007)
Year FE	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y
Adj-R-squared	0.38	0.38	0.38	0.38	0.38
Observations	43,966	43,966	43,966	43,966	43,966

Table 7: Turnover sensitivity to misconduct and the protocol

The table displays regression results from linear probability models estimated using OLS (Equation 5 in the text) of forced in the next year on “Misconduct,” which is an indicator variable if the adviser engaged in misconduct, as defined by Egan et al. (2019), during the year, “Firm in protocol,” which is an indicator variable that is one if the financial adviser is employed by a firm that is member of the broker protocol as of the end of the calendar year, and the interaction of the two. The table reports the results using two large samples and two subsamples of each. In columns 1 through 3, the analysis uses the entire adviser-level sample described in Panel A of Table 2. In columns 4 through 6, the results are reported for the sample of advisers employed by firms with at least 100 advisers. Each of these samples is split by state level NCA enforcement using the variable “Absence of NCA enforcement,” as outlined in Table 4. The dependent variable is “Forced turnover,” which is an indicator variable that is one if the adviser joins another firm after 90 days of being unemployed. All models include firm-county and county-year fixed effects.  $T$ -statistics are computed using robust standard errors (reported in parentheses), clustered by firm. Using the same robust standard error estimation we also report  $\hat{\beta}_{p \times m, \text{yes}}$   $\hat{\beta}_{p \times m, \text{no}}$  (the difference between the coefficient estimates on the interaction term of “Firm in the protocol” and “Misconduct” between the “yes” and “no” samples.) and the associated standard errors. Significance levels are denoted by  $c$ ,  $b$ , and  $a$ , which correspond to 10%, 5%, and 1% levels, respectively.

Sample	Full sample			100 advisers		
	All (1)	State enforces NCAs?		All (4)	State enforces NCAs?	
		Yes (2)	No (3)		Yes (5)	No (6)
Misconduct	0.458 <sup>b</sup> (0.183)	0.585 <sup>a</sup> (0.187)	-0.009 (0.397)	0.641 <sup>a</sup> (0.211)	0.746 <sup>a</sup> (0.206)	0.276 (0.460)
Firm in protocol	-0.324 <sup>c</sup> (0.176)	-0.296 <sup>c</sup> (0.174)	-0.422 <sup>c</sup> (0.246)	-0.239 (0.175)	-0.195 (0.170)	-0.402 (0.252)
Firm in protocol × Misconduct	-0.544 <sup>b</sup> (0.241)	-0.793 <sup>a</sup> (0.247)	0.304 (0.532)	-0.678 <sup>b</sup> (0.263)	-0.899 <sup>a</sup> (0.262)	0.050 (0.584)
Log (number of advisers)	0.396 (0.327)	0.342 (0.369)	0.644 <sup>a</sup> (0.239)	0.079 (0.380)	-0.016 (0.423)	0.533 <sup>c</sup> (0.316)
Log (years experience)	-0.699 <sup>a</sup> (0.078)	-0.672 <sup>a</sup> (0.078)	-0.806 <sup>a</sup> (0.090)	-0.749 <sup>a</sup> (0.083)	-0.717 <sup>a</sup> (0.084)	-0.881 <sup>a</sup> (0.095)
Investment adviser	-0.929 <sup>a</sup> (0.092)	-0.878 <sup>a</sup> (0.095)	-1.136 <sup>a</sup> (0.116)	-0.865 <sup>a</sup> (0.097)	-0.819 <sup>a</sup> (0.099)	-1.055 <sup>a</sup> (0.121)
Gen. sec. rep. (7)	0.180 <sup>c</sup> (0.103)	0.107 (0.108)	0.484 <sup>a</sup> (0.124)	0.018 (0.111)	-0.050 (0.117)	0.301 <sup>b</sup> (0.134)
Inv. co. prod. rep. (6)	-0.529 <sup>a</sup> (0.160)	-0.524 <sup>a</sup> (0.170)	-0.549 <sup>a</sup> (0.146)	-0.654 <sup>a</sup> (0.178)	-0.641 <sup>a</sup> (0.188)	-0.705 <sup>a</sup> (0.164)
Gen. sec. principal (24)	0.205 <sup>b</sup> (0.100)	0.213 <sup>b</sup> (0.097)	0.162 (0.130)	0.326 <sup>a</sup> (0.112)	0.320 <sup>a</sup> (0.108)	0.336 <sup>b</sup> (0.151)
Number of other qual.	-0.014 (0.029)	-0.032 (0.030)	0.064 <sup>c</sup> (0.035)	-0.008 (0.033)	-0.025 (0.034)	0.070 <sup>c</sup> (0.039)
County-Year FE	Y	Y	Y	Y	Y	Y
Firm-county FE	Y	Y	Y	Y	Y	Y
Mean of the dep. var.	2.97	3.00	2.82	2.78	2.82	2.64
Adj-R-squared	0.03	0.03	0.03	0.02	0.02	0.02
Observations	5,891,188	4,712,699	1,178,489	5,217,482	4,177,988	1,039,494
$\hat{\beta}_{p \times m, \text{yes}}$ $\hat{\beta}_{p \times m, \text{no}}$			-1.097 <sup>b</sup> (0.559)			-0.949 (0.600)

Table 8: Adviser misconduct and the protocol

The table displays regression results from linear probability models (Panel A) and fixed effect OLS regressions (Panel B) estimated using OLS of measures of misconduct on “Firm in protocol.” The analysis in Panel A uses the adviser-level data described in Panel A of Table 2 and the dependent variable is “Misconduct” multiplied by 100. “Misconduct” is an indicator variable that is one if the adviser engaged in misconduct during the year, as defined by Egan et al. (2019). Panel B uses the firm-level data described in Panel B of Table 2 and the dependent variable is “Misconduct per 100 advisers,” which is the number of the firm’s advisers who engaged in misconduct during the year, per 100 advisers working at the firm at the end of the previous year. In Panel A, the results are reported for two different fixed effect models for the full sample and the samples financial advisers working for firms with at least 100 advisers. The models estimated in columns 1 and 2 include county-year and firm-county fixed effects and those in columns 3 and 4 include county-year and financial adviser fixed effects. In Panel B, the model includes firm and year fixed effects. *T*-statistics are computed using robust standard errors (reported in parentheses), clustered by firm. Significance levels are denoted by *c*, *b*, and *a*, which correspond to 10%, 5%, and 1% levels, respectively.

Panel A: Adviser-level evidence				
Sample	All (1)	100 advisers (2)	All (3)	100 advisers (4)
Firm in protocol	0.104 (0.070)	0.131 <sup>c</sup> (0.078)	0.148 <sup>b</sup> (0.059)	0.196 <sup>a</sup> (0.068)
Past misconduct	1.313 <sup>a</sup> (0.068)	1.197 <sup>a</sup> (0.077)		
Log (number of advisers)	0.037 (0.047)	0.064 (0.061)	-0.131 <sup>a</sup> (0.011)	-0.158 <sup>a</sup> (0.019)
Log (years experience)	0.133 <sup>a</sup> (0.013)	0.131 <sup>a</sup> (0.014)	0.365 <sup>a</sup> (0.049)	0.397 <sup>a</sup> (0.056)
Investment adviser	0.344 <sup>a</sup> (0.033)	0.351 <sup>a</sup> (0.035)	-0.048 (0.031)	-0.018 (0.030)
Gen. sec. rep. (7)	0.107 <sup>a</sup> (0.023)	0.082 <sup>a</sup> (0.026)	0.194 <sup>a</sup> (0.043)	0.175 <sup>a</sup> (0.047)
Inv. co. prod. rep. (6)	0.029 (0.019)	0.010 (0.019)	0.062 (0.099)	0.006 (0.107)
Gen. sec. principal (24)	-0.035 <sup>c</sup> (0.018)	-0.080 <sup>a</sup> (0.019)	0.097 <sup>a</sup> (0.036)	0.097 <sup>b</sup> (0.041)
Number of other qual.	0.013 <sup>c</sup> (0.007)	0.011 (0.007)	-0.069 <sup>a</sup> (0.016)	-0.081 <sup>a</sup> (0.016)
County-Year FE	Y	Y	Y	Y
Firm-county FE	Y	Y	N	N
Adviser FE	N	N	Y	Y
Mean of the dep. var.	0.494	0.472	0.494	0.472
Adj-R-squared	0.03	0.03	0.05	0.04
Observations	5,862,497	5,194,488	5,706,560	5,040,460

Table 8 continues on the following page.

Table 8 continued from the previous page.

---

Panel B: Firm-level evidence

---

	(1)
Firm in protocol	0.532 <sup>a</sup> (0.205)
Log (number of advisers)	0.144 (0.088)
Year FE	Y
Firm FE	Y
Mean of the dep. var.	0.830
Adj-R-squared	0.28
Observations	130,990

---

Table 9: Commissions and fees and the protocol

Panel A of the table displays regression results from fixed effect OLS regressions of measures of fee composition and fee rates on “Lagged Firm in protocol,” which is an indicator variable that is one if the firm is member of the broker protocol as of the end of the previous calendar year. The analysis uses the sample of firms covered by the *InvestmentNews* annual independent B-D surveys from 2004 to 2016 with complete data as outlined in Section 2.5 of the text. The dependent variable in columns 1 and 2 is “Commission share,” which captures the percentage of revenue made up by commissions and is equal to the commission fee revenue divided by total revenue. In columns 3 and 4, the dependent variable is the “Fee rate,” which is the fee revenues divided by the fee-based AUM. Panel B of Table shows summary statistics for this sample. All dependent variables are winsorized at the first and ninety-ninth percentiles to remove effects of outliers. All models include firm and year fixed effects. *T*-statistics are computed using robust standard errors (reported in parentheses), clustered by firm and year. Significance levels are denoted by *c*, *b*, and *a*, which correspond to 10%, 5%, and 1% levels, respectively.

---

Panel A: Regression results

---

Dep. var.	Commission share		Fee rate	
	(1)	(2)	(3)	(4)
Lagged firm in protocol	-2.255 <sup>c</sup> (1.137)	-2.255 <sup>c</sup> (1.137)	14.393 <sup>b</sup> (7.156)	14.393 <sup>b</sup> (7.160)
Lagged log (number of advisers)	1.633 (2.027)	1.876 (3.138)	21.993 <sup>b</sup> (10.807)	22.351 (14.009)
Lagged Log (number of representatives)		-0.256 (3.135)		-0.377 (11.315)
Year FE	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Adj-R-squared	0.89	0.89	0.37	0.37
Observations	804	804	804	804

---

Table continues on the following page.

Table continued from the previous page.

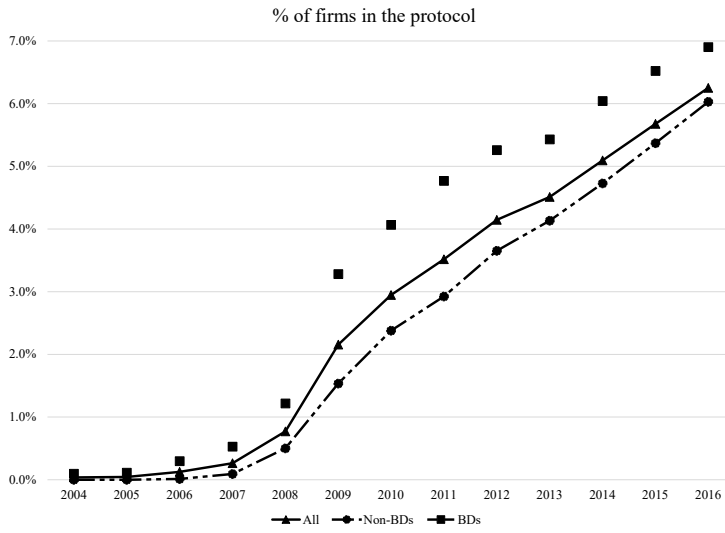
---

Panel B: Sample summary statistics

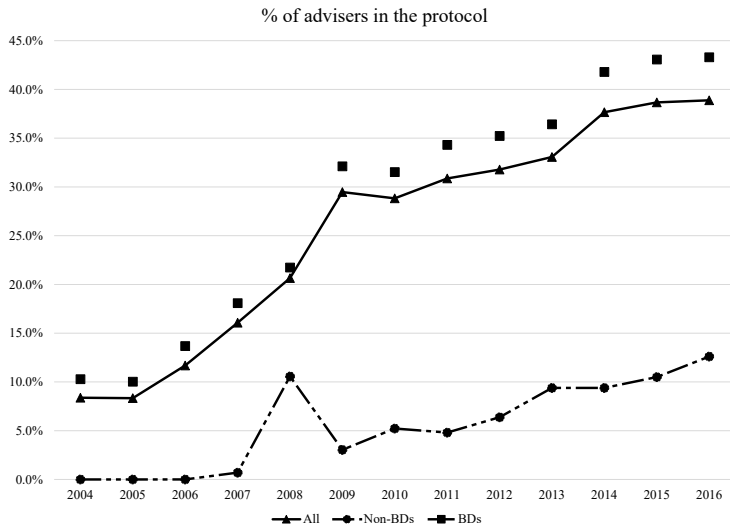
---

	Mean	Median	St. Dev.	1st Per.	99th Per.
Commission share	75.357	77.544	13.990	34.328	97.691
Fee rate	100.362	95.146	43.909	18.243	311.661
Firm in protocol	0.343	0.000	0.475	0.000	1.000
Number of advisers	1,576.101	822.500	2,272.871	23.000	13,518.000
Log (number of advisers)	6.682	6.712	1.241	3.135	9.512
Number of representatives	1,368.819	730.500	1,810.633	31.000	7,795.000
Log (number of representatives)	6.608	6.594	1.156	3.434	8.961

---



(a)

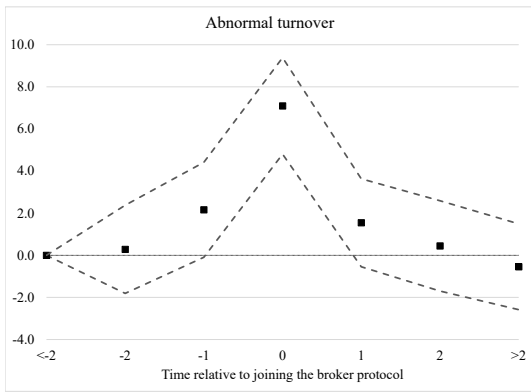


(b)

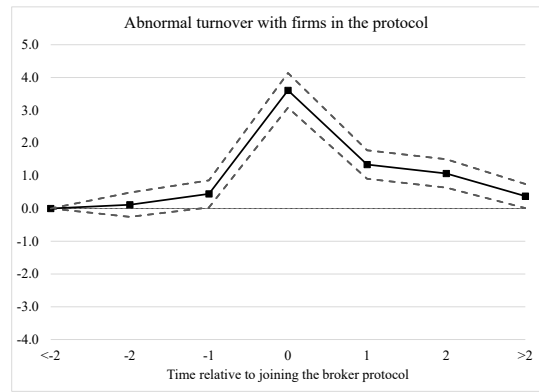
Figure 1: Percentage of firms and advisers in the protocol by year

The figure shows the percentage of financial firms that are members of the broker protocol (a) and advisers who are employed by members of the broker protocol (b) by year for all firms who employ at least two financial advisers between 2004 and 2016. These percentages are also decomposed into firms (employers) that are not broker-dealers and firms that are broker-dealers. The survivorship-bias-free sample begins in August of 2007. Advisers who retire prior to August 2007 are missing from sample.

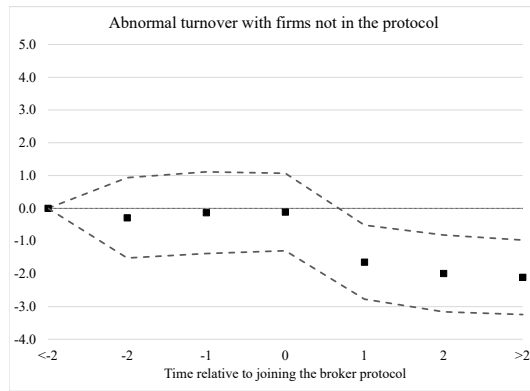




(a)



(b)



(c)

Figure 2: Adviser turnover and the protocol - firm-level evidence dynamics

The figure plots the coefficient estimates and their 10% confidence intervals of the  $\beta_{p,t}$ 's from Equation 3, which is a linear probability model with firm and year fixed effects, that regresses various measures of turnover on leads and lags of “Join protocol.” Therefore, the coefficient estimates on these indicator variables measure the changes in turnover relative to average turnover three years or more prior to a firm joining the broker protocol. The analysis uses the entire firm-level sample described in Panel B of Table 2. The dependent variables are total turnover, turnover with other firms in the protocol, and turnover with firms not in the protocol, where definitions follow those in Table 5. Confidence intervals are computed using robust standard errors, clustered by firm and year.

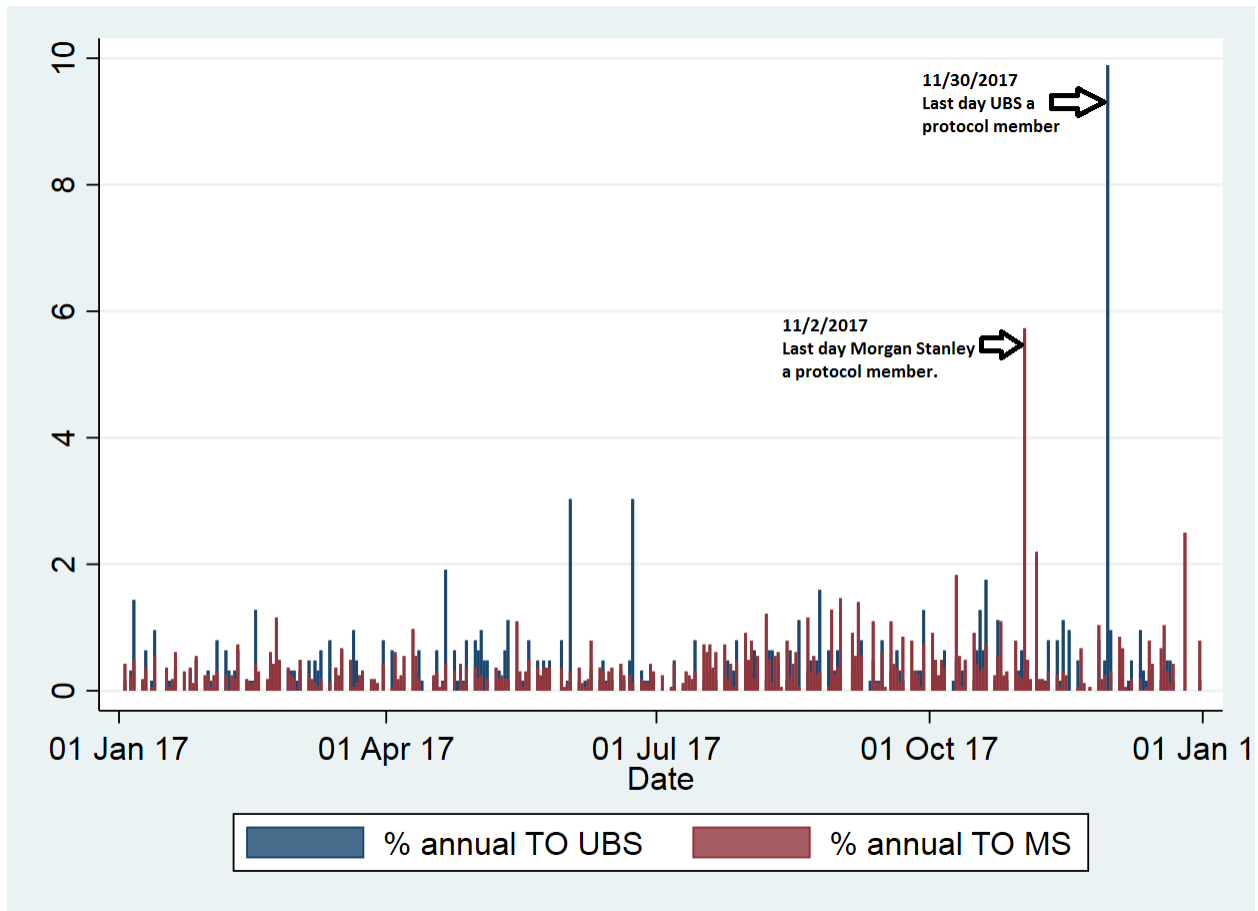


Figure 3: Protocol withdrawal and adviser exits

The figure plots the percentage of 2017 annual turnover occurring each business day of the year for Morgan Stanley (blue) and UBS Financial Services (red). On October 24, 2017, Morgan Stanley submitted a letter indicating that it would like to withdrawal from the broker protocol. UBS followed suit on November 20, 2017. It takes 10 days for the withdrawal to take effect. Therefore, the last days that Morgan Stanley and UBS Financial Services were members of the broker protocol, where November 2, 2017 and November 30, 2017, respectively. Those dates are indicated on the graph above.

## Internet Appendix

Table IA.I: Predicting protocol membership

The table displays the results from predictive regressions of firms joining the broker protocol using firm-year observations for the sample period 2004-2016 (Column 1) and 2007-2016 (Column 2). The dependent variable is an indicator variable that is 1 if the firm joins the broker protocol and is zero otherwise. All independent variables are lagged by one year and their definitions are found in the Appendix except for “% county advisers in protocol,” which is the percentage of the advisers at other firms that are members of the protocol in the counties where the firm has branches.  $T$ -statistics are computed using robust standard errors (reported in parentheses), clustered by firm and year. Significance levels are denoted by  $c$ ,  $b$ , and  $a$ , which correspond to 10%, 5%, and 1% levels, respectively.

Sample:	2004- 2016 (1)	2007- 2016 (2)
Log (number of advisers)	0.361 <sup>a</sup> (0.027)	0.410 <sup>a</sup> (0.032)
Percent change in advisers	0.373 <sup>a</sup> (0.066)	0.455 <sup>a</sup> (0.079)
Broker-dealer indicator	0.060 (0.047)	0.057 (0.056)
RIA indicator	0.392 <sup>a</sup> (0.051)	0.421 <sup>a</sup> (0.058)
% of county advisers in the protocol	0.376 <sup>c</sup> (0.193)	0.473 <sup>b</sup> (0.205)
Year FE	Y	Y
Mean of the dep. var.	0.527	0.696
Adj-R-squared	0.009	0.009
Observations	125,989	108,628

Table IA.II: Adviser turnover and the protocol - alternative models

The table displays regression results for four different models using the sample and methodology outlined in Table 3 of the main text. In Panels A, B, and C the dependent variables are “Leave to another firm”, “Leave to a protocol firm”, and “Leave to a non-protocol firm,” respectively. None of the models include controls other than the fixed effects that are indicated at the bottom of the table. *T*-statistics are computed using robust standard errors (reported in parentheses), clustered by firm. Significance levels are denoted by *c*, *b*, and *a*, which correspond to 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Panel A: Dep. var. = “Leave to another firm”				
Firm in protocol	0.322 (1.451)	0.852 (1.081)	0.631 (0.838)	0.050 (0.862)
Constant	9.116 <sup>a</sup> (0.560)			
Adj-R-squared	0.00	0.02	0.09	0.10
Observations	5,902,522	5,900,371	5,893,392	5,891,188
Panel B: Dep. var. = “Leave to a protocol firm”				
Firm in protocol	4.776 <sup>a</sup> (1.511)	5.032 <sup>a</sup> (1.067)	2.542 <sup>a</sup> (0.511)	1.997 <sup>a</sup> (0.554)
Constant	1.878 <sup>a</sup> (0.190)			
Adj-R-squared	0.02	0.03	0.10	0.11
Observations	5,902,522	5,900,371	5,893,392	5,891,188
Panel C: Dep. var. = “Leave to a non-protocol firm”				
Firm in protocol	-4.454 <sup>a</sup> (0.500)	-4.180 <sup>a</sup> (0.517)	-1.912 <sup>a</sup> (0.440)	-1.946 <sup>a</sup> (0.508)
Constant	7.237 <sup>a</sup> (0.553)			
Adj-R-squared	0.01	0.02	0.09	0.09
Observations	5,902,522	5,900,371	5,893,392	5,891,188
County-Year FE	N	Y	N	Y
Firm-county FE	N	N	Y	Y

Table IA.III: Adviser turnover and the protocol - robustness

The table displays regression results for various subsamples using the model and methodology outlined in Table 3. In Panels A and B the samples are restricted to advisers who are brokers and advisers who are registered at only one firm, respectively. In Panel C, The sample is expanded to include all available data from 2003 until 2016. All models include firm-county and county-year fixed effect and the controls included in Table 3. *T*-statistics are computed using robust standard errors (reported in parentheses), clustered by firm. Significance levels are denoted by *c*, *b*, and *a*, which correspond to 10%, 5%, and 1% levels, respectively.

	Leave to another firm (1)	Leave to a protocol firm (2)	Leave to a non-protocol firm (3)	Leave profession (4)
Panel A: Sample of brokers				
Firm in protocol	-0.218 (0.889)	1.808 <sup>a</sup> (0.595)	-2.026 <sup>a</sup> (0.505)	-0.708 <sup>b</sup> (0.277)
Adj-R-squared	0.10	0.11	0.09	0.06
Observations	5,854,012	5,854,012	5,854,012	5,854,012
Panel B: Sample of advisers registered with only one firm				
Firm in protocol	0.022 (1.021)	1.943 <sup>a</sup> (0.689)	-1.921 <sup>a</sup> (0.576)	-0.593 <sup>c</sup> (0.318)
Adj-R-squared	0.11	0.11	0.10	0.06
Observations	5,414,700	5,414,700	5,414,700	5,414,700
Panel C: Extended sample 2003-2016				
Firm in protocol	0.047 (0.577)	2.355 <sup>a</sup> (0.435)	-2.308 <sup>a</sup> (0.372)	-1.396 <sup>a</sup> (0.225)
Adj-R-squared	0.10	0.08	0.11	0.06
Observations	8,224,925	8,224,925	8,224,925	8,224,925
County-Year FE	Y	Y	Y	Y
Firm-county FE	Y	Y	Y	Y

Table IA.IV: Adviser turnover, the protocol, and state-level enforcement - robustness

The table extends the analysis from Table 4 from the main text by estimating the results using various subsamples. In Panel A, only advisers who are brokers are included in the sample. In Panel B, only advisers who are employed by only one firm are included in the sample. In Panel C, the sample is from 2003-2016, but the years 2003-2006 suffer from survivorship bias. All models include firm-county and county-year fixed effects and the controls included in Table 3.  $T$ -statistics are computed using robust standard errors (reported in parentheses), clustered by firm. Using the same robust standard error estimation we also report  $\hat{p}_{,no}$   $\hat{p}_{,yes}$  and the associated standard errors. Significance levels are denoted by  $c$ ,  $b$ , and  $a$ , which correspond to 10%, 5%, and 1% levels, respectively.

Dep. variable	Leave to a protocol firm		Leave to a non-protocol firm	
	State enforces NCAs?		State enforces NCAs?	
Sample	Yes	No	Yes	No
	(1)	(2)	(3)	(4)
Panel A: Sample of brokers				
Firm in protocol	2.020 <sup>a</sup> (0.639)	1.051 <sup>c</sup> (0.600)	-2.164 <sup>a</sup> (0.539)	-1.559 <sup>a</sup> (0.491)
Adj-R-squared	0.11	0.12	0.09	0.10
Observations	4,685,166	1,168,846	4,685,166	1,168,846
$\hat{p}_{,yes}$		0.969 <sup>c</sup> (0.536)		-0.605 (0.370)
$\hat{p}_{,no}$				
Panel B: Sample of advisers registered with only one firm				
Firm in protocol	2.170 <sup>a</sup> (0.741)	1.126 (0.689)	-2.065 <sup>a</sup> (0.616)	-1.430 <sup>b</sup> (0.561)
Adj-R-squared	0.11	0.12	0.10	0.10
Observations	4,328,580	1,086,120	4,328,580	1,086,120
$\hat{p}_{,yes}$		1.044 <sup>c</sup> (0.625)		-0.635 (0.432)
$\hat{p}_{,no}$				

Table IA.IV continues on the next page.

Table IA.IV continued from the previous page.

Dep. variable	Leave to a protocol firm		Leave to a non-protocol firm	
	State enforces NCAs?		State enforces NCAs?	
Sample	Yes	No	Yes	No
	(1)	(2)	(3)	(4)
Panel C: Extended sample 2003-2016				
Firm in protocol	2.434 <sup>a</sup> (0.450)	2.040 <sup>a</sup> (0.501)	-2.451 <sup>a</sup> (0.413)	-1.815 <sup>a</sup> (0.492)
Adj-R-squared	0.08	0.09	0.11	0.12
Observations	6,574,983	1,637,294	6,574,983	1,637,294
$\hat{p}_{,yes}$		0.394 (0.392)		-0.636 (0.529)
$\hat{p}_{,no}$				
Controls	Y	Y	Y	Y
County-Year FE	Y	Y	Y	Y
Firm-county FE	Y	Y	Y	Y

Table IA.V: Turnover sensitivity to misconduct and the protocol - robustness

The table extends the analysis from Table 7 from the main text by estimating the results using various subsamples. In Panel A, only advisers who are investment advisers (investment adviser=1) are included. In Panel B, only advisers who are not investment advisers (investment adviser=0) are included in the sample. In Panel C, only advisers who are employed by only one firm are included in the sample. In Panel D, the sample is from 2003-2016, but the years 2003-2006 suffer from survivorship bias. All models include firm-county and county-year fixed effects and the controls included in Table 3. *T*-statistics are computed using robust standard errors (reported in parentheses), clustered by firm. Using the same robust standard error estimation we also report  $\hat{\beta}_{p \times m, \text{yes}} - \hat{\beta}_{p \times m, \text{no}}$  (the difference between the coefficient estimates on the interaction term of “Firm in the protocol” and “Misconduct” between the “No” and “Yes” samples.) and the associated standard errors. Significance levels are denoted by *c*, *b*, and *a*, which correspond to 10%, 5%, and 1% levels, respectively.

Sample	All (1)	State enforces NCAs?	
		Yes (2)	No (3)
Panel A: Sample of brokers			
Misconduct	0.446 <sup>b</sup> (0.184)	0.571 <sup>a</sup> (0.187)	-0.015 (0.399)
Firm in protocol	-0.335 <sup>c</sup> (0.176)	-0.307 <sup>c</sup> (0.174)	-0.433 <sup>c</sup> (0.246)
Firm in protocol × Misconduct	-0.534 <sup>b</sup> (0.242)	-0.780 <sup>a</sup> (0.248)	0.308 (0.533)
Adj-R-squared	0.03	0.03	0.03
Observations	5,854,012	4,685,166	1,168,846
$\hat{\beta}_{p \times m, \text{yes}} - \hat{\beta}_{p \times m, \text{no}}$			-1.088 <sup>c</sup> (0.561)
Panel B: Sample of advisers registered with only one firm			
Misconduct	0.446 <sup>b</sup> (0.201)	0.580 <sup>a</sup> (0.204)	-0.056 (0.425)
Firm in protocol	-0.351 <sup>c</sup> (0.190)	-0.313 <sup>c</sup> (0.186)	-0.483 <sup>c</sup> (0.274)
Firm in protocol × Misconduct	-0.507 <sup>b</sup> (0.255)	-0.740 <sup>a</sup> (0.263)	0.307 (0.549)
Adj-R-squared	0.04	0.04	0.04
Observations	5,426,832	4,338,375	1,088,457
$\hat{\beta}_{p \times m, \text{yes}} - \hat{\beta}_{p \times m, \text{no}}$			-1.047 <sup>c</sup> (0.578)

Table IA.V continues on the next page.



Table IA.V continued from the previous page.

Sample	All (1)	State enforces NCAs?	
		Yes (2)	No (3)
Panel C: Extended sample 2003-2016			
Misconduct	0.308 <sup>b</sup> (0.138)	0.333 <sup>b</sup> (0.152)	0.229 (0.293)
Firm in protocol	0.036 (0.177)	0.021 (0.177)	0.080 (0.270)
Firm in protocol × Misconduct	-0.239 (0.216)	-0.355 (0.221)	0.096 (0.469)
Adj-R-squared	0.04	0.04	0.04
Observations	8,235,921	6,583,754	1,639,413
$\hat{p} \times m, \text{yes}$			-0.451 (0.490)
$\hat{p} \times m, \text{no}$			
Controls	Y	Y	Y
County-Year FE	Y	Y	Y
Firm-county FE	Y	Y	Y

Table IA.VI: Misconduct and the protocol - robustness

The table displays regression results for various subsamples using the model and methodology outlined in Table 8 of the main text. The dependent variable is “Misconduct” multiplied by 100. In columns 1 and 2 the samples are restricted to advisers who brokers and advisers who are only registered with on firm, respectively. In column 4, the sample is expanded to include all available data from 2003 until 2016, but the years 2003-2006 suffer from survivorship bias. In panel A all models include firm-county and county-year fixed effects and in Panel B they include adviser and county-year fixed effects. In both panels the control variables included in Table 8 of the main text are included, but their coefficients are not reported. *T*-statistics are computed using robust standard errors (reported in parentheses), clustered by firm. Significance levels are denoted by *c*, *b*, and *a*, which correspond to 10%, 5%, and 1% levels, respectively.

Sample	Brokers (1)	One firm (2)	2003-2016 (3)
Panel A: firm-county fixed effects			
Firm in protocol	0.114 (0.072)	0.122 (0.074)	-0.014 (0.041)
Past misconduct	1.290 <sup>a</sup> (0.069)	1.294 <sup>a</sup> (0.067)	1.342 <sup>a</sup> (0.059)
County-Year FE	Y	Y	Y
Firm-county FE	Y	Y	Y
Adj-R-squared	0.03	0.04	0.03
Observations	5,692,393	5,365,813	8,362,697
Panel B: adviser fixed effects			
Firm in protocol	0.155 <sup>b</sup> (0.060)	0.157 <sup>b</sup> (0.066)	0.100 <sup>a</sup> (0.036)
County-Year FE	Y	Y	Y
Adviser FE	Y	Y	Y
Adj-R-squared	0.05	0.04	0.02
Observations	5,538,200	5,365,813	8,362,697