Journal of Financial Economics 141 (2021) 1218–1243

Contents lists available at ScienceDirect

Journal of Financial Economics

journal homepage: www.elsevier.com/locate/jfec

Unlocking clients: The importance of relationships in the financial advisory industry☆

Umit G. Gurun[a], Noah Stoffman[b], Scott E. Yonker[c,*]

[a] School of Management, University of Texas at Dallas, Richardson, TX 75080, United States
[b] Department of Finance, Kelley School of Business, Indiana University, 1309 East Tenth Street, Bloomington, IN 47405, United States
[c] Dyson School of Applied Economics and Management, Cornell SC Johnson College of Business, Cornell University, Ithaca, NY, 14853, United States

ARTICLE INFO

Article history:
Received 26 September 2019
Revised 21 July 2020
Accepted 28 July 2020
Available online 4 May 2021

JEL classification:
G5
G24
G23
J41
J42
K31

Keywords:
Financial adviser
Relationships
Trust
Clients
Flows
Noncompete agreement
Broker protocol
Misconduct

ABSTRACT

We investigate the importance of client relationships in the financial advisory industry. We exploit firm-level variation in adoption of the Broker Protocol, which enabled clients to follow their advisers to member firms without fear of litigation. We show that advisers' ability to maintain client relationships is a significant predictor of their employment decisions; that about 40% of client assets follow advisers when they move; and that once clients are "unlocked," firms become less willing to fire advisers for misconduct. Firms that unlock their clients subsequently experience higher levels of misconduct and increase their fees, calling into question whether clients are better off.

1. Introduction

Financial advisers in the United States manage $28 trillion of assets for their clients. The relationships between these advisers—numbering over 760,000 in our data—and their millions of clients are critical for supporting the economic activity generated by these investments. Trust is inherent in these relationships, although the literature has yet to distinguish between whom clients trust (Gennaioli et al., 2015; Gurun et al., 2018; Kostovetsky, 2016). Is it the advisory firm that creates advertisements and develops a brand name that prompts clients to walk into a branch? Or

☆ We thank Jawad Addoum, Brian Broughman, Ben Charoenwong, Lauren Cohen, Mark Egan, Zoran Ivkovich, Matthew S. Johnson, Andrew Karolyi, Naveen Khanna, Alan Kwan, Kurt Lavetti, Benjamin Loos, David Ng, Chris Parsons, Andy Puckett, Amiyatosh Purnanandam, Jonathan Reuter, Matthew Serfling, Amit Seru, Merih Sevilir, and Jagadeesh Sivadasan as well as seminar participants at the 9th Conference on Professional Asset Management at Erasmus University, Cornell University, the European Finance Association Conference, FIRS, FTC Micro Conference, Indiana University, North Carolina State University, the Mitsui Finance Symposium at the University of Michigan, the University of Houston, the University of Tennessee Smokey Mountain Finance Conference, and the Western Finance Association Annual Conference for helpful comments.

* Corresponding author.
E-mail addresses: umit.gurun@utdallas.edu (U.G. Gurun), nstoffma@indiana.edu (N. Stoffman), sey8@cornell.edu (S.E. Yonker).

https://doi.org/10.1016/j.jfineco.2021.04.026
0304-405X/

is it the adviser who develops an intimate financial counseling relationship with the client? From the client's perspective, choosing an adviser or advisory firm is no different from choosing a lawyer or a surgeon. Some clients care more about the adviser than the firm, or the surgeon than the hospital, or the lawyer than the law firm. In these industries, where asymmetric information abounds and the potential for client harm is large, the ability to foster personal relationships with clients has important implications for employees, firms, clients, and ultimately, the industry's competitive landscape.

If clients trust their advisers more than their advisory firms, then advisers will have considerable power within their firms, since it is their relationships with clients that constitute the firm's primary asset (Lavetti et al., 2019). Without constraints on their mobility, advisers can move to a competing firm, perhaps taking many of their clients with them and essentially walking out the door with the firm's assets.

To reduce this power imbalance, firms often use non-compete agreements (NCAs) to legally constrain their employees' mobility.[1] In the presence of the trust relationship, however, these agreements not only restrict the adviser's mobility, but to some extent also lock in clients to the firm where their adviser is employed.[2]

In this paper, we investigate the importance of client relationships in the financial advisory industry. We show empirically that these relationships guide adviser employment decisions, that a large portion of client assets follow advisers when they move, and that firms' decisions to discipline advisers are influenced by their amount of control over these relationships. We then study how the power dynamic between firms and their employees induced by these relationships affects clients, firms, and the structure of the financial advisory industry more generally. We conclude that the effects are far-reaching, impacting the prevalence of adviser misconduct, fee rates, and industry competition.

To assess the importance of client relationships in the financial advisory industry, we need a source of variation in their transferability between firms. For this we rely on the 2004 creation of the Protocol for Broker Recruiting (hereafter, "the protocol") by three major brokerage firms, with the encouragement of the Financial Industry Regulatory Authority (FINRA). 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 the litigation expenses that had historically been regularly incurred when an adviser would switch firms.[3] The protocol established a set of rules governing adviser departures. Specifically, it allowed an adviser to take client lists and contact information to their new employer without fear of legal action—effectively unlocking clients from firms.

Importantly for the purposes of our identification strategy, the shield from litigation provided by the protocol applies only when both the previous and new employers are signatories to the protocol. Moreover, the protocol agreement was not restricted to its original signatories; since its inception, over 1500 financial firms have joined in a staggered fashion, and very few have exited.

We combine complete records of all firms joining and leaving the protocol with detailed information on all registered brokers and investment adviser representatives to construct a staggered panel of firm entry into, and exit from, the broker protocol. We then exploit within-firm time series variation in the transferability of client relationships induced by variation in membership in the protocol. Together, these data provide us with a very rich setting in which to study the importance of client relationships in the financial advisory industry.

We begin our empirical analysis by assessing the importance of client relationships to advisers. Typically, advisers are compensated as a percentage of their annual "production," defined as the commissions and fees generated from the clients they service, and this compensation rate can vary across investment advisers. The ability to take clients when moving to another firm is therefore important to adviser compensation, so protocol entry should affect advisers' employment decisions. We find that while the entry into 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 nonprotocol firm. These results provide strong evidence that the transferability of client relationships is indeed a major factor in adviser employment decisions.

If clients trust advisers, then when advisers change firms their clients should follow, inducing a "relationship-based flow." This particular flow mechanism is distinct from those previously studied, such as flows due to past performance (Lynch and Musto, 2003; Huang et al., 2007), expense ratios (Sirri and Tufano, 1998; Bergstresser et al., 2009) or brokers' incentives (Christoffersen et al., 2013). Available data does not allow direct observation of each adviser's book of business, but we do observe each firm's assets under management (AUM) each year, and are therefore able to relate changes in this value to adviser moves. We show that an adviser leaving one protocol member firm to join another brings substantially more assets than if he were to join a firm that is outside the protocol. Our lower bound estimates suggest that, unconstrained, the average adviser takes about 40% of her clients when she changes firms. This value has not been previously estimated in the literature, and could be of interest to market participants, especially in the context of litigation. The estimated elasticity using changes in number of accounts instead of AUM is nearly identical.

[1] 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. NCAs are also known as non-compete clauses, or covenants not to compete.

[2] 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.

[3] The complete text of the 1,200-word agreement is available at http://www.thebrokerprotocol.com/index.php/authors/read-the-protocol.

From a revealed preference perspective, evidence of relationship-based flows suggests that relaxing constraints on the transferability client relationships—what we term "unlocking clients"—makes both advisers and clients better off. However, as noted above, asymmetric information permeates the financial advisory industry, so there is a potential for advisers to take advantage of their clients. Putting clients in unsuitable or high-fee products, shirking by neglecting their advisory duties, or moving clients to higher fee firms are just a few ways that advisers could exploit their clients' trust. Unlocking clients can also weaken firm governance: it tips the balance of power from firms to advisers, which could make firms laxer with respect to punishing advisers when they engage in misconduct to prevent a decline in assets.[4]

We therefore test whether firms are less likely to discipline their advisers following protocol adoption and whether this leads to increased adviser misconduct at member firms. We find that following protocol adoption, firms become more reluctant to fire advisers after they engage in bad behavior. For the sample of advisers working at large firms, engaging in misconduct increases the probability of being fired by about 23%, but that this discipline is effectively undone when firms join the protocol. We also find that protocol adoption is associated with an increase in the propensity to engage in misconduct by about 40%. Together, 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 and that this leads to a higher incidence of misbehavior by advisers.

We next estimate the dynamics of fees following protocol adoption. Firms may increase fees to compensate for the possibility of losing assets to adviser departures or because they realize that the trust underlying relationship-based flows can be easily exploited. Alternatively, firms can decrease their fees to attract new clients to compensate for the loss of AUM that follows adviser departures. For a small sample of brokerage firms, we find that firms do not significantly change their fees in the first year of protocol membership, but in the second year fees go up by about 13% from pre-adoption levels. After three years, fees remain about 18% higher than pre-adoption fee levels. These findings, along with those on higher misconduct rates, call into question whether unlocking clients makes them better off.

What is the effect of unlocking clients on firms? If all firms charged the same fees for identical products, then advisers moving within the protocol member firms should just be a zero sum game. Therefore, to answer this question, we need to think about which firms should gain from joining and which should lose. Prior to the protocol, legal settlements between the former and new employer were the norm when advisers moved clients between firms. The new employer would pay the former some percentage of the adviser's annual production. The large brokerage houses that initiated the protocol along with those that joined in the early years likely anticipated that the agreement would lead to zero net flows, and reduced legal costs. They likely did not anticipate the growth of the protocol to include smaller firms. For these firms, the protocol was an opportunity. The status quo legal settlement process made it extremely difficult for small firms to poach employees from larger firms since they did not have the resources to settle up. Protocol adoption made poaching from large firms "free." With this in mind, we analyze firm outcomes by splitting the sample between large and small firms.

Our empirical findings are largely consistent with these arguments. Adoption of the broker protocol matters much more for smaller firms. Smaller firms see abnormal adviser growth from within the industry of about 8.5% in the year they join the protocol. This is driven by poaching advisers from other member firms. Substantial growth only lasts during the first two years, suggesting that small firms strategically join the protocol to poach advisers and grow their client base. Large firms, by contrast, see no net growth in the number of advisers upon adoption of the protocol, but the long-term effects of protocol adoption for these firms is a decline in advisers. We see similar patterns when investigating the impact of unlocking clients on revenue. For small firms, revenue increases by about 27% upon adoption of the protocol and remains persistently higher. Large firms, however, see a temporary increase of about 7% in the first year, which fades away by the second year. These results indicate that if clients were fully unlocked from firms, the financial advisory industry would become less concentrated, allowing small firms to compete with larger firms.

Tempering the benefit of increased competition, we also find that misconduct rates at small firms increase by more than at larger firms following protocol adoption and remain persistently high. An advisers with a large client base at a small firm will wield much more power than at a large firm since her book of business constitutes a larger percentage of firm assets at the small firm. In a sense the "relationship assets" in small firms are much more concentrated, making them less willing to discipline large advisers.[5]

Our findings contribute to the literature that explores incentives and behavior of financial advisers, who play an influential role in determining their clients' asset choices (Mullainathan et al., 2012; Foerster et al., 2017), despite a failure to deliver tangible benefits (Bergstresser et al., 2009; Chalmers and Reuter, 2020). We extend this literature by highlighting the fundamental importance of the relationships between advisers and their clients in this industry. Charoenwong et al. (2017), Dimmock et al. (2018) and Egan et al. (2019) study misconduct in this industry, whereas Clifford and Gerken (2019) investigate the effect of the broker protocol on investment in human capital. We show that significant adviser power can lead to higher fees, laxer firm governance, and increased adviser misconduct.

[4] A recent literature has found that rates of adviser misconduct are persistent within firms, suggesting that some advisory firms do a poor job of disciplining misconduct (Egan et al., 2019).

[5] This idea is similar to Israelsen and Yonker (2017), who show that firms with concentrated human capital experience large declines in firm value when "key" employees depart.

Our findings are also related to the growing literature on the importance of trust in the financial advisory industry (Gennaioli et al., 2015; Gurun et al., 2018; Germann et al., 2018; Kostovetsky, 2016). We provide the first estimate of the percentage of assets an adviser can expect to take when switching firms, which is direct evidence of the importance of trust-based relationships between clients and advisers. Unlike Gurun et al. (2018), who show the impact on asset flows from clients losing trust in regulators, we show that client trust in advisers—rather than advisory firms—shapes asset flows in the financial advisory market. From this perspective, our study complements Kostovetsky (2016), who studies mutual fund flows around management-company ownership changes, finding evidence that clients also place trust in firms.

Finally, our paper is related to the broad literature in labor economics on the use of NCAs in various industries and how it affects human capital mobility. Starr et al. (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.[6] Studies have generally found that NCAs are an impediment to this mobility (Stuart and Sorenson, 2003; Marx et al., 2009; Marx, 2011), and therefore can affect the growth of both industries and geographic regions (Rosegrant and Lampe, 1992; Saxenian, 1996; Franco and Filson, 2006; Klepper, 2002; Klepper and Sleeper, 2005). 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. In contrast to previous studies, our design allows us to control for geographic differences in local labor market conditions that could be correlated with NCA enforcement.

2. Empirical methodology

We are interested in estimating the importance of client relationships in the financial advisory industry. To do this, we need variation in advisers' ability to move clients when they switch firms. For this purpose, we construct a staggered panel of firm entry and exit into the broker protocol, which relaxed the enforcement of NCAs for advisers moving within member firms, allowing clients to freely follow their advisers to some firms, but not others.

Importantly, there are very few barriers to protocol membership. Firms entering the protocol must only file a joinder agreement and notify the Securities Industry and Financial Markets Association (SIFMA) of their entry. Leaving is also easy, requiring only written notification ten days prior to exit. This ease of entry and exit alleviates the concern that certain types of firms are systematically excluded and that characteristics of those excluded firms could drive our findings.

One challenge to estimating causal effects using the broker protocol as our source of variation is that firms are likely to join the protocol strategically. Indeed, our results show that this is likely the case. In the analysis that follows, we show that adviser 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 Fig. 2). In the Internet Appendix, we identify characteristics that predict a firm's decision to join the protocol, namely firm size, past growth, being a registered investment adviser (RIA), and the amount of competition among local advisers (Table IA.1).

We must therefore consider two potential sources of endogeneity: omitted factors and reverse causality. First we consider omitted factors that predict protocol membership but cannot be included in the model; these could be be static or time-varying at the level of the firm, branch, or local labor market. We address this concern in three ways. First, we include firm-branch fixed effects in our adviser-level regressions, which allows us to control for any time-invariant, firm- and branch-level omitted variables that could 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 can 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 client relationships, essentially transforming what were once firm-specific assets to general assets

[6] Greenhouse (2014) provides examples of non-compete agreements in a surprising range of jobs, including summer camp counselors, event planners, and yoga instructors. In 2016, the sandwich chain Jimmy Johns agreed to stop requiring NCAs with its employees as part of a settlement with the New York attorney general's office.

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, with the assumption that advisers at large firms do not likely influence the decisions of management to join the broker protocol.

3. 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 data sets used in our analysis.

3.1. Financial adviser data

Data on financial advisers are extracted from FINRA's web server, which provides consolidated data from its BrokerCheck web site and the Security and Exchange Commission's Investment Adviser Public Disclosure (IAPD) web site. 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.

3.2. 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 their owners, their clients, and any criminal behavior. Importantly, investment advisory 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 data set.

3.3. 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 web site listing active broker-dealers by month, dating back to 2007.[7]

3.4. 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.[8] 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 1515 unique firms that had joined the broker protocol. Of these, we are able to identify the CRD for 1325 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, 1166 (88.0%) had at least one adviser employed in the year prior to joining the protocol. (The remainder are 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 1166 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 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.

Fig. 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%).

[7] www.sec.gov/help/foiadocsbdfoiahtm.html.

[8] www.thebrokerprotocol.com.

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	2318	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	1302	145	22	41
2013	91	6632	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	4382	32	15	84	5	28	6	0	0
Total	1166	288,691		28.39	97.80	39	2311		18	52
Sample	1151	207,791		27.54	96.94	39	2311		18	52
% total	99	72				100	100			

3.5. Additional data sources

We obtain data on fee-based assets and fee revenue for a subset of large broker-dealers from *InvestmentNews*' B-D Data Center.[9] These data, which cover approximately 75 broker-dealers per year from 2004 to 2016, are compiled from annual surveys of independent broker-dealers. We obtain annual data on revenue for broker-dealers from Audit Analytics' Broker-Dealer Financial and Operational Combined Uniform Single (FOCUS) Report, which collects data from SEC Form X-17A-5 filings, and contains information on the financial and operating conditions of broker-dealers. We summarize these data in Tables IA.7 and IA.8 of the Internet Appendix, respectively. 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).

3.6. Sample construction

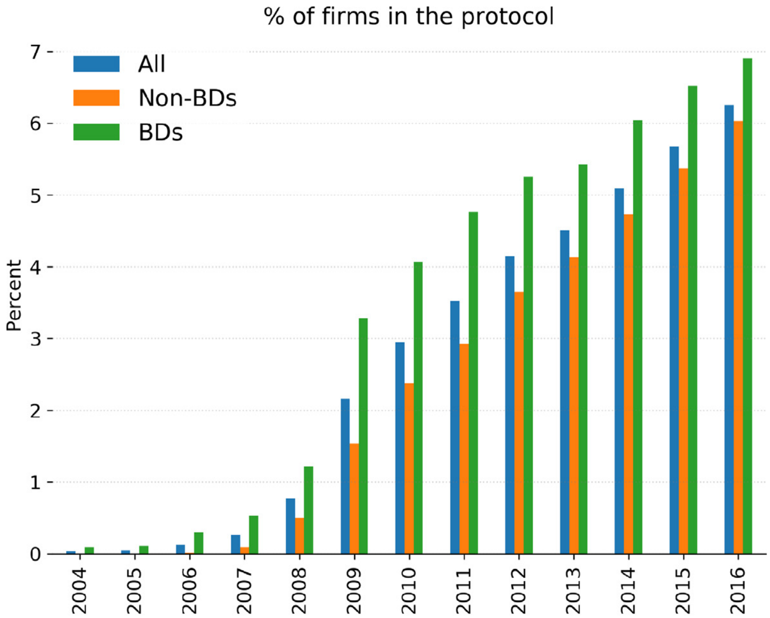
We construct a data set covering advisers beginning in 2003, but show in the Appendix that the data are free of survivorship bias concerns only beginning in August 2007. Our main tests using these data are therefore conducted with annual panel data from the end of 2007 until the end of 2016. This final survivorship-bias-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 the employee-year observations, advisers work for firms in the protocol. Most financial advisers work for broker-dealers (97%). The average financial adviser has 12 years of experience and advisers at firms in the protocol have about three more years of experience, on average, than advisers at firms not in the protocol. The unconditional probability of an adviser leaving for another firm during the year is 0.092. We decompose adviser movements by whether their destination firm is a protocol member. Not surprisingly, the majority of moves are to nonprotocol firms (79%), since there are many more of them.

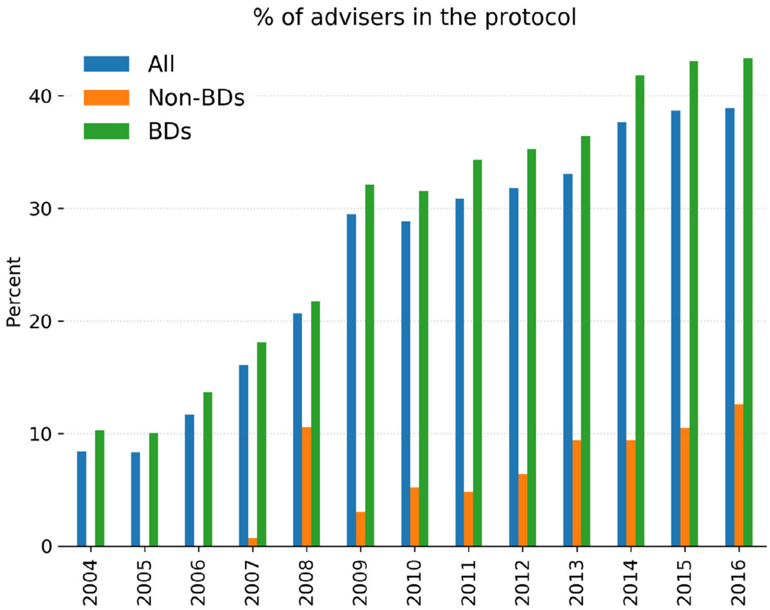
We construct a misconduct indicator variable following Egan et al. (2019). During our sample, advisers engage in misconduct 0.5% of the time, which is slightly less than the 0.6% reported in Table 1 of Egan et al. (2019). Advisers employed by protocol member firms appear to be about 75% more likely to engage in misconduct. We also calculate "Past misconduct," 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% of the firm-years, firms are members of the broker protocol and in 31% 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. Within industry turnover, defined as the average of the percentage of advisers leaving the firm for other firms and the percentage of advisers joining the

[9] http://www.investmentnews.com.



Panel A



Panel B

Fig. 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 (Panel A) and advisers who are employed by members of the broker protocol (Panel B) by year for all firms that 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 the sample.

firm from other firms, is about 6.5% for the average firm, and is predominately driven by turnover with firms that are not in the protocol. At least one misconduct event occurs at firms at a rate of nearly 8% per year and, as previously noted, it is much less frequent among nonprotocol firms.

Our asset flow tests are limited to firms that are 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 firm has $2.2 billion in AUM and 1625 client accounts, but the median AUM is much smaller at $236 million and 450 client accounts. Firms in the protocol manage roughly twice as many assets as those that are not. The average asset and account growth rates are around 7%.

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 employee 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 Tables A.1 and A.2 of the Appendix. 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[a]
Log (years experience)	2.206	2.398	0.970	0.000	3.714	2.110	2.402[a]
Registered investment adviser	0.390	0.000	0.488	0.000	1.000	0.284	0.608[a]
Registered representative	0.994	1.000	0.079	1.000	1.000	0.991	0.999[a]
Gen. sec. rep. (7)	0.669	1.000	0.470	0.000	1.000	0.584	0.846[a]
Inv. co. prod. rep. (6)	0.378	0.000	0.485	0.000	1.000	0.459	0.213[a]
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[a]
Past misconduct	0.068	0.000	0.251	0.000	1.000	0.055	0.095[a]
Absence of NCA enforcement	0.200	0.000	0.400	0.000	1.000	0.190	0.220[b]
Leave for 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[a]
Leave to a nonprotocol firm (%)	5.777	0.000	23.331	0.000	100.000	7.237	2.783[a]
Forced turnover (%)	2.968	0.000	16.969	0.000	100.000	3.329	2.227[a]
Misconduct indicator	0.005	0.000	0.070	0.000	0.000	0.004	0.007[a]
Broker-dealer indicator	0.970	1.000	0.170	0.000	1.000	0.958	0.996[a]
Panel B: Firm level							
Firm in protocol	0.040	0.000	0.195	0.000	1.000	0.000	1.000
Number of advisers	60.097	4.000	670.821	1.000	877.000	42.499	485.065[a]
Log (number of advisers)	1.810	1.386	1.358	0.000	6.777	1.761	2.996[a]
Within industry turnover	6.534	0.000	12.444	0.000	60.000	6.411	9.508[a]
Turnover with firms in protocol	0.967	0.000	2.985	0.000	16.667	0.875	3.201[a]
Turnover with firms not in protocol	5.567	0.000	11.353	0.000	52.632	5.536	6.307[a]
%Δ in advisers	3.947	0.000	24.916	−50.000	100.000	3.825	6.875[a]
%Δ in advisers outside industry	1.178	0.000	16.096	−50.000	57.143	1.196	0.745 [b]
%Δ in advisers within industry	2.769	0.000	17.841	−50.000	78.495	2.629	6.131[a]
%Δ in advisers with protocol firms	0.626	0.000	5.350	−14.286	33.333	0.524	3.082[a]
%Δ in advisers with firms not in protocol	2.143	0.000	16.505	−50.000	66.667	2.105	3.049[a]
% advisers join from outside the industry	6.088	0.000	15.159	0.000	66.667	6.110	5.540[a]
% advisers join from within industry	7.919	0.000	18.421	0.000	100.000	7.726	12.574[a]
% advisers join from protocol firms	1.280	0.000	5.098	0.000	33.333	1.137	4.742[a]
% advisers leave to go outside the industry	4.910	0.000	10.642	0.000	50.000	4.914	4.796
% advisers leave within industry	5.150	0.000	11.380	0.000	50.000	5.096	6.442[a]
% advisers leave for protocol firms	0.654	0.000	2.479	0.000	14.286	0.613	1.660[a]
Misconduct dummy	0.077	0.000	0.267	0.000	1.000	0.068	0.306[a]
Broker dealer indicator	0.311	0.000	0.463	0.000	1.000	0.310	0.347[b]
RIA indicator	0.337	0.000	0.473	0.000	1.000	0.330	0.516[a]
AUM ($ millions)	2,217.889	236.219	8,784.918	11.736	68,754.900	3,729.877	7,601.002
Log (AUM)	5.761	5.465	1.690	2.463	11.313	5.727	6.295[a]
ΔLog (AUM)	0.074	0.082	0.312	−1.180	1.276	0.069	0.143[a]
Number of accounts (thousands)	1.625	0.450	4.763	0.002	34.323	20.750	27.199
Log (Accts)	-0.942	-0.799	1.815	−6.215	3.825	−1.028	0.391[a]
ΔLog (Accts)	0.070	0.041	0.371	−1.345	1.609	0.066	0.124[a]

4. Results

In the following sections, we test the importance of client relationships in the financial advisory industry. We begin with advisers by testing whether unlocking clients affects their employment decisions (Section 4.1). Next we focus on clients by asking, when unlocked, what percentage of client assets follow their advisers when advisers switch firms (Section 4.2). For firms, we estimate how unlocking clients affects their willingness to discipline advisers for bad behavior since unlocking clients transfers bargaining power to advisers (Section 4.3). We then test whether laxer monitoring by firms leads to increased financial misconduct (Section 4.4). The dynamics of fees after firms unlock their clients are then explored (Section 4.5). Finally, we ask which firms are the winners and losers from unlocking clients, and what are the implications for the industry (Section 4.6).

4.1. Adviser employment decisions

We begin by estimating the effect of unlocking clients on advisers' decisions to move to another firm ("turnover").

4.1.1. Adviser-level analysis

We estimate the following linear probability model using our annual adviser-employer matched panel from 2007 through 2016:

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

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).[10] The variable of interest is "Firm in protocol." If unlocking clients increases the propensity of advisers to leave their firms, then the estimate of β_p should be significantly positive.

We estimate regression (1) using three alternative definitions of turnover. First, we use "Leave for 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 nonprotocol firm."

Since all advisers in a firm are treated simultaneously, our empirical design could have what Abadie et al. (2017) call an "assignment" problem. We address this by clustering standard errors by firm throughout the analysis.[11] Sampling problems are not an issue in our study since the "sample" includes the population of financial advisers.

Panel A of Table 3 shows the regression results for these three turnover variables. In column 1, the estimate of β_p is indistinguishable from zero, indicating that unlocking clients does not increase advisers' propensity to switch firms. However, the evidence in columns 2 and 3 shows that unlocking clients redirects advisers toward other protocol firms and away from nonprotocol firms. The estimate of β_p in column 2 is 1.81, indicating that once advisers' firms join the protocol, those advisers are 1.8% more likely to leave for another protocol firm. The unconditional probability of leaving to join a firm in the protocol is 3.5%, 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 nonprotocol firms following their firm joining the protocol declines by about 2.0%. These results are consistent with adviser-client relationships affecting advisers' employment decisions.[12]

To further the argument of causality, in columns 4 through 7 of the table we test whether the effects of unlocking clients due to the relaxation of NCAs are stronger in branches that are located in states that enforce NCAs. 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 β_p 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.

The coefficient estimates indicate that the effects of the protocol are stronger in states that actually enforce NCAs. The estimates of β_p in column 4 are roughly twice the size of those in column 5 and theses differences are significantly different from zero at the 10% level. Similarly, the decrease in the probability of advisers leaving for 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 β_p is −2.14 in column 6 of Panel A, while the coefficient in column 7 is −1.52. This difference is significant at the 10% level.

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 96th 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 unlocking clients affects adviser employment decisions.

4.1.2. Firm-level analysis

We next estimate the dynamics of firm-level turnover and adviser growth following unlocking clients. To ensure that our findings are not driven by outliers, all dependent variables are winsorized at the 1st and 99th percentiles. Specifically, we estimate:

$$\text{Turnover/Growth}_{i,t} = \alpha_i + \gamma_t + \beta_{p,0}(\text{Firm joins protocol})_{i,t} + \beta_{p,1}(\text{Firm joins protocol})_{i,t-1}$$

[10] 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.

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

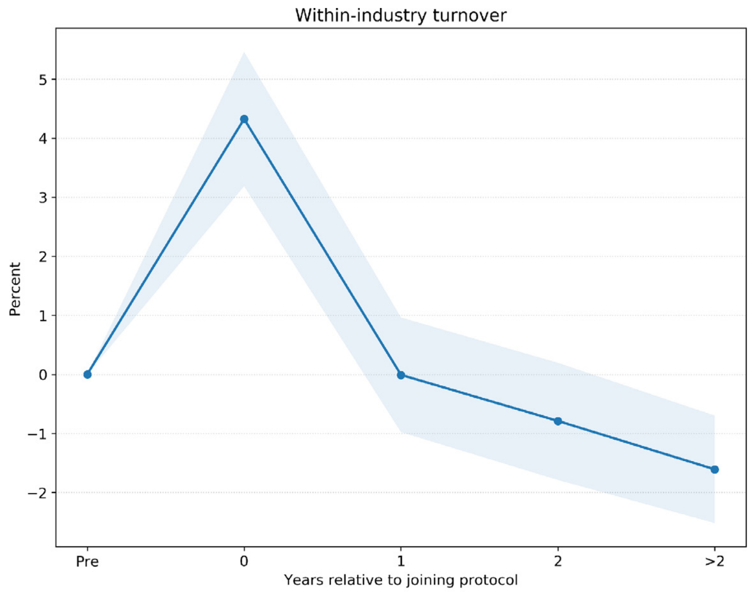
[12] Table IA.3 in the Internet Appendix shows the effect of the inclusion of various fixed effects in our model. Branch fixed effects explain the most variation and have largest effect on the magnitude of the estimates of β_p.

Table 3
Adviser turnover.
The table displays regression results from linear probability models estimated using OLS (Eq. (1) in the text) of various turnover measures 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 a 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. Only coefficient estimates on "Firm in protocol" are displayed in Panel B, but the same control variables used in Panel A are included in the models. The dependent variable in column 1 is "Leave for 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 for a protocol firm" and "Leave for a nonprotocol firm." Columns 4 through 7 show regression results 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. All models include firm-county and county-year fixed effects. 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. Using the same robust standard error estimation we also report $\hat{\beta}_{p,\text{yes}} - \hat{\beta}_{p,\text{no}}$ and the associated standard errors. Significance levels are denoted by c, b, and a, which correspond to 10%, 5%, and 1% levels, respectively.

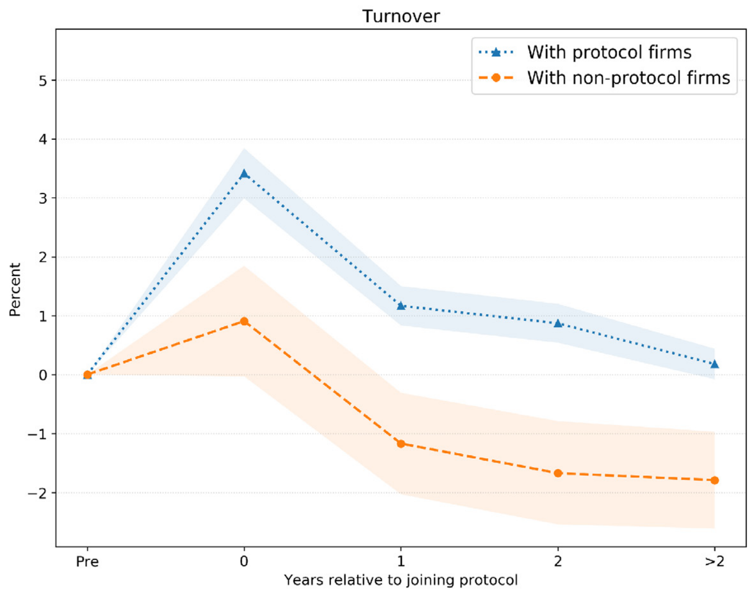
Sample	Leave for another firm	Leave for a protocol firm	Leave for a nonprotocol firm	Leave for a protocol firm	Leave for a protocol firm	Leave for a nonprotocol firm	Leave for a nonprotocol firm
State enforces NCAs?	Full	Full	Full	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: All advisers							
Firm in protocol	−0.183	1.812[a]	−1.995[a]	2.023[a]	1.062[c]	−2.135[a]	−1.523[a]
	(0.885)	(0.592)	(0.502)	(0.636)	(0.596)	(0.537)	(0.489)
Log (number of advisers)	3.359[c]	2.204	1.155	1.957	3.360	1.139	1.231[c]
	(1.896)	(1.673)	(0.882)	(1.583)	(2.132)	(0.996)	(0.667)
Log (years experience)	−1.628[a]	−0.569[a]	−1.059[a]	−0.545[a]	−0.668[a]	−1.056[a]	−1.070[a]
	(0.201)	(0.125)	(0.114)	(0.119)	(0.152)	(0.115)	(0.126)
Investment adviser	0.253	0.819[b]	−0.566[a]	0.720[a]	1.178[b]	−0.504[a]	−0.817[a]
	(0.361)	(0.320)	(0.129)	(0.264)	(0.546)	(0.138)	(0.159)
Gen. sec. rep. (7)	3.300[a]	1.392[a]	1.907[a]	1.290[a]	1.819[a]	1.895[a]	1.963[a]
	(0.215)	(0.137)	(0.160)	(0.107)	(0.292)	(0.163)	(0.209)
Inv. co. prod. rep. (6)	−0.251	−0.096	−0.155	0.016	−0.472	−0.133	−0.246
	(0.323)	(0.210)	(0.193)	(0.163)	(0.372)	(0.208)	(0.177)
Gen. sec. principal (24)	−0.675[b]	−0.399[b]	−0.276[b]	−0.361[b]	−0.567[b]	−0.259[b]	−0.348[b]
	(0.268)	(0.200)	(0.123)	(0.180)	(0.289)	(0.125)	(0.155)
Number of other qual.	0.227[a]	0.119[c]	0.108[a]	0.119[c]	0.126	0.100[b]	0.141[a]
	(0.084)	(0.064)	(0.039)	(0.062)	(0.078)	(0.041)	(0.046)
County-Year FE	Y	Y	Y	Y	Y	Y	Y
Firm-county FE	Y	Y	Y	Y	Y	Y	Y
Mean of the dep. var.	9.169	3.482	5.687	3.441	3.863	5.790	5.349
Adj-R^2	0.10	0.11	0.09	0.11	0.12	0.09	0.10
Observations	5,891,188	5,891,188	5,891,188	4,712,699	1,178,489	4,712,699	1,178,489
$\hat{\beta}_{p,\text{yes}} - \hat{\beta}_{p,\text{no}}$					0.961[c]		-0.612[c]
					(0.534)		(0.369)
Panel B: Sample advisers working at firms with at least 100 advisers							
Firm in protocol	0.007	1.740[a]	−1.733[a]	1.972[a]	0.892	−1.873[a]	−1.256[b]
	(0.947)	(0.664)	(0.543)	(0.707)	(0.695)	(0.585)	(0.499)
Controls	Y	Y	Y	Y	Y	Y	Y
County-year FE	Y	Y	Y	Y	Y	Y	Y
Firm-county FE	Y	Y	Y	Y	Y	Y	Y
Mean of the dep. var.	8.996	3.673	5.323	3.584	4.063	5.407	5.010
Adj-R^2	0.10	0.11	0.09	0.11	0.12	0.08	0.09
Observations	5,221,183	5,221,183	5,221,183	4,180,975	1,040,208	4,180,975	1,040,208
$\hat{\beta}_{p,\text{yes}} - \hat{\beta}_{p,\text{no}}$					1.080[c]		−0.617
					(0.608)		(0.395)

$$+\beta_{p,2}(\text{Firm joins protocol})_{i,t-2} + \beta_{p,>2}(\text{Firm joins protocol})_{i,<t-2} + \Gamma'\text{Controls}_{i,t-1} + \epsilon_{i,t}, \quad (2)$$

where α_i and γ_t are firm and year fixed effects and $(\text{Firm joins protocol})_{i,t}$ is an indicator variable that is one if firm i joins the broker protocol in year t. Therefore, $\beta_{p,s}$ estimates the change in turnover s periods after protocol adoption relative to the firm's average turnover prior to joining the broker protocol. For instance, $\beta_{p,0}$ captures abnormal turnover in the first year of membership. The parameter $\beta_{p,>2}$ captures the average abnormal turnover after three or more years of protocol membership. The lagged log number of advisers is included to control for firm size.



Panel A

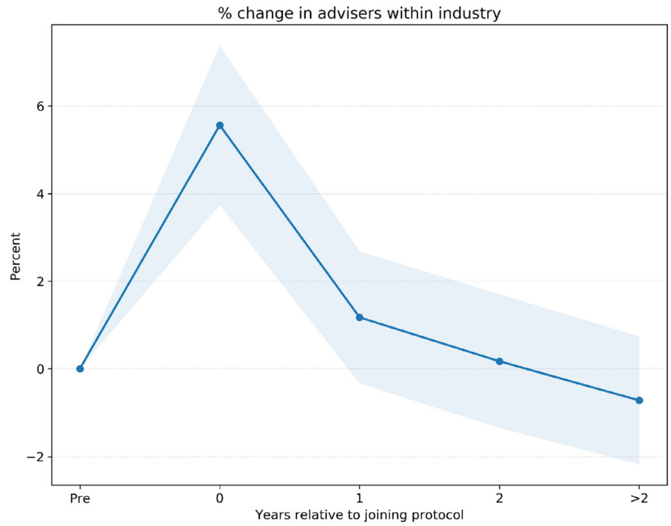


Panel B

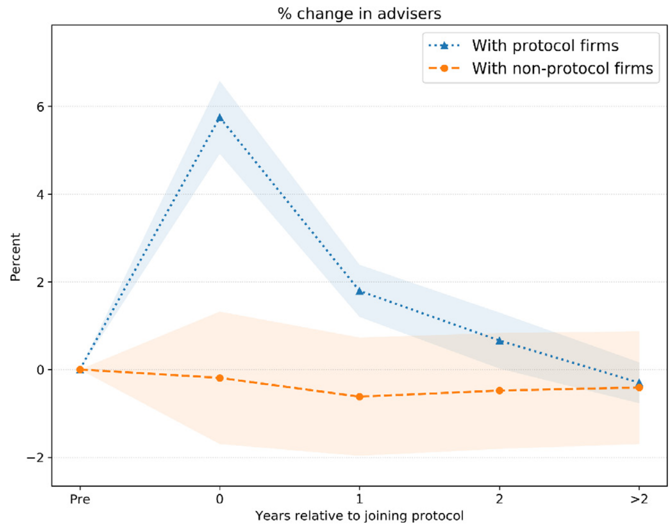
Fig. 2. Adviser turnover: Firm-level dynamics. The figure plots the coefficient estimates and their 10% confidence intervals of the $\beta_{p,t}$'s from Eq. (2), which is a linear probability model with firm and year fixed effects, that regresses various measures of turnover on lags of "Join protocol." Therefore, the coefficient estimates on these indicator variables measure the changes in turnover relative to average turnover 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 within-industry turnover (Panel A), turnover with firms in the protocol and turnover with firms not in the protocol (Panel B), %Δ in advisers within industry (Panel C), and %Δ in advisers with protocol firms and %Δ in advisers with nonprotocol firms (Panel D), where definitions follow those in Table A.2. Confidence intervals are computed using robust standard errors, clustered by firm.

The estimates of the $\beta_{p,s}$'s from the regressions are plotted in Fig. 2.[13] In addition, adviser growth is decomposed into % join and % leave. The endogenous entry of firms into the protocol is evident in the figures. In Panels A and C, we see that within industry turnover and adviser growth spike in the year that a firm joins the protocol, but subsequently reverts to levels observed prior to membership. Panels B and D show that it is the turnover

[13] Coefficient estimates and their standard errors are displayed in the Internet Appendix in Table IA.2.



Panel C



Panel D

Fig. 2. Continued

with other firms in the protocol that drives this the first year spike, 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. Abnormal turnover and adviser growth with protocol firms remains abnormally high for the first years of membership. Turnover with nonprotocol firms does not decline abnormally until the second year of membership, but it remains persistently low thereafter. Adviser growth with nonprotocol firms remains flat through the entire period.

4.2. Relationship-based flows

The results from the previous section are consistent with advisers placing importance on the client relationship. While unlocking clients does not increase adviser propensity to change firms, it does affect the firms that advisers move to. This suggests that clients move with advisers when they switch firms. In this section, we formally test whether clients follow advisers. Finding positive evidence of these relationship-based flows would be consistent with some clients valuing their relationships with advisers more than with their advisory firms.

To test this, we estimate the following fixed effects OLS regression model:

$$\begin{aligned}\Delta\log(\mathrm{AUM})_{i,t} &= \alpha_i + \gamma_t + \beta_{n,o}(\%\Delta\,\text{in adv. outside industry})_{i,t}\\ &\quad + \beta_{n,n}(\%\Delta\,\text{in adv. within industry})_{i,t}\\ &\quad + \beta_{n,p}(\%\Delta\,\text{in adv. with protocol firms})_{i,t}\end{aligned}$$

$$+\beta_{p,o}\text{Protocol}_{i,t} \times (\%\Delta\text{in adv. outside industry})_{i,t}$$
$$+\beta_{p,n}\text{Protocol}_{i,t} \times (\%\Delta\text{in adv. within industry})_{i,t}$$
$$+\beta_{p,p}\text{Protocol}_{i,t} \times (\%\Delta\text{in adv. with protocol firms})_{i,t}$$
$$+\beta_p(\text{Firm in protocol})_{i,t} + \Gamma' Controls_{i,t-1} + \epsilon_{i,t}, \qquad (3)$$

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 α_i and γ_t are firm and year fixed effects, respectively.

The %Δ in adv. variables are various decompositions of the percentage change in the number of advisers at firm i during year t. "%Δ 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 for other firms, regardless of whether those firms are protocol members. "%Δ 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 %Δ in adv.$_{i,t}$=%Δ in adv. outside industry$_{i,t}$+%Δ 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, "%Δ in adv. within industry" can be decomposed into advisers moving between protocol- and nonprotocol firms. "%Δ in adv. with protocol firms" is the difference between the percentage of advisers joining from protocol member firms and those leaving for 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 for protocol and nonprotocol firms.

In regression (3), the coefficients $\beta_{n,o}$, $\beta_{n,n}$, and $\beta_{n,n} + \beta_{n,p}$ capture the elasticities of AUM for nonprotocol firms with respect to outside industry advisers, nonprotocol advisers, and protocol advisers, 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 members of 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 nonprotocol 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 (3) using a firm-level annual panel data set constructed from electronic filings of Form ADV, as described in Section 3.2. 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 advisory firms are RIAs, which are required to make regular filings with the SEC.

Table 4 shows the results of our tests. In column 1, we include only the "%Δ 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 0.107, which implies that a 1% increase in the number of financial advisers at the average firm is associated with about a 10.7 basis point increase in AUM. In column 2, we decompose the change in advisers between outside and inside the industry changes and further decompose inside industry changes into changes with protocol members and non-members. The estimates show that changes within the industry are associated with much larger changes in AUM. A 1% increase in advisers leaving the industry is associated with about a 4 bps decrease in AUM. The same change in advisers leaving for nonprotocol (protocol) firms within the industry leads to a decrease of about 14 (27) bps. Not only do the estimates show that larger changes in AUM are associated with within industry changes in advisers, but they also show that advisers leaving for protocol firms take roughly double the amount of assets with them relative to advisers leaving for firms outside the protocol. This difference is statistically significant.

In column 3, we estimate the full version of Eq. (3). Consistent with our hypotheses, we find that $\beta_{p,p} = 0.185 > 0$ 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 advisers with protocol members, especially when the firm itself is a protocol member. Our estimate of the change in AUM for a 1% increase in the number of advisers leaving a protocol firm for firms in the protocol is $14.3 + 12.4 + 3.9 + 6.9 + 18.5 = 56.0$ bps. In other words, an adviser leaving a protocol member firm for another protocol member firm takes, on average, clients with assets worth about half of the average assets of the firm's existing advisers. It is possible that some of this outflow is due to factors other than advisers taking clients with them, but the 18.5 bps due to protocol-to-protocol firm turnover likely represents a lower bound of the size of the effect, as there is no reason to believe that assets would fall by more for firms in the protocol than those outside it when their advisers leave for protocol firms, other than that between protocol members clients are unlocked.

The regressions estimated in columns 4 through 6 use the change in the natural log of the number of accounts managed by the RIA. While the number of accounts is different from the number of clients, Form ADV does not report continuous values for client counts and the number of accounts is a better predictor of the number of clients than is AUM. The estimates using accounts tell a similar story to those using AUM. The only difference is that our

Table 4
Relationship-based flows.
Panel A of the table displays regression results from fixed effect OLS regressions (Eq. (3) in the text) of changes in log (AUM) (columns 1 to 3) and changes in log(number of accounts) (columns 4 to 6) on contemporaneous changes in the percentage of advisers employed by the firm (%Δ 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 (%Δ in advisers outside industry) or within the industry (%Δ in advisers within industry), which includes moves to both protocol and nonprotocol firms. We add an additional variable that captures the incremental effect of the protocol, the percentage change in advisers to and from other firms that are members of the broker protocol (%Δ in advisers with protocol firms). In column 3, 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 a member of the broker protocol as of the end of the previous calendar year. The analysis in Panel B follows the same pattern, but decomposes each % net change by including separate variables for the percentage of advisers joining and leaving firms. The analysis uses the firm-year observations from the sample described in Panel B of Table 2, which consists of Registered Investment Advisers with the SEC (about 37% of the sample). All continuous variables are winsorized at the 1st and 99th 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.

Dependent variable:	Δlog(AUM)			Δlog(Accts)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
%Δ in advisers	0.107[a]			0.125[a]		
	(0.007)			(0.009)		
%Δ in advisers outside industry		0.037[a]	0.040[a]		0.059[a]	0.059[a]
		(0.009)	(0.010)	(0.012)		(0.013)
%Δ in advisers within industry		0.145[a]	0.143[a]		0.159[a]	0.155[a]
		(0.011)	(0.011)		(0.014)	(0.014)
%Δ in advisers with protocol firms		0.151[a]	0.124[a]		0.161[a]	0.143[a]
		(0.029)	(0.030)		(0.039)	(0.041)
Firm in protocol			0.039[a]			0.063[a]
			(0.014)			(0.018)
Firm in protocol ×						
%Δ outside industry			−0.070			−0.006
			(0.043)			(0.066)
%Δ in advisers within industry			0.069			0.118[c]
			(0.056)			(0.067)
%Δ in advisers with protocol firms			0.185[c]			0.060
			(0.112)			(0.132)
Lagged log(AUM) / log(Acct)	−0.254[a]	−0.253[a]	−0.254[a]	−0.306[a]	−0.305[a]	−0.306[a]
	(0.007)	(0.007)	(0.007)	(0.011)	(0.011)	(0.011)
Year FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Adj-R^2	0.38	0.39	0.39	0.25	0.25	0.25
Observations	43,980	43,980	43,974	43,980	43,980	43,974
Panel B						
% advisers join	0.085[a]			0.113[a]		
	(0.008)			(0.010)		
% advisers leave	−0.196[a]			−0.173[a]		
	(0.014)			(0.017)		
% advisers join from outside the industry		0.027[b]	0.030[a]		0.049[a]	0.049[a]
		(0.010)	(0.011)		(0.014)	(0.014)
% advisers join from within industry		0.107[a]	0.104[a]		0.141[a]	0.136[a]
		(0.013)	(0.013)		(0.016)	(0.016)
% advisers join from protocol firms		0.183[a]	0.169[a]		0.175[a]	0.169[a]
		(0.031)	(0.031)		(0.043)	(0.044)
% advisers leave to go outside the industry		−0.095[a]	−0.096[a]		−0.102[a]	−0.104[a]
		(0.018)	(0.019)		(0.022)	(0.023)
% advisers leave within industry		−0.274[a]	−0.270[a]		−0.224[a]	−0.220[a]
		(0.020)	(0.021)		(0.026)	(0.027)
% advisers leave for protocol firms		−0.175[b]	−0.110		−0.185[b]	−0.120
		(0.072)	(0.073)		(0.093)	(0.096)
Firm in protocol			0.049[a]			0.064[a]
			(0.016)			(0.021)
Firm in protocol ×						
% advisers join from outside the industry			−0.066			0.032
			(0.049)			(0.077)
% advisers join from within industry			0.089			0.143[b]
			(0.059)			(0.069)
% advisers join from protocol firms			0.028			−0.051
			(0.036)			(0.050)

(*continued on next page*)

Table 4 (*continued*)

Dependent variable:	Δlog(AUM)			Δlog(Accts)		
	(1)	(2)	(3)	(4)	(5)	(6)
% advisers leave to go outside the industry			0.057			0.101
			(0.091)			(0.111)
% advisers leave within industry			−0.079			−0.099
			(0.089)			(0.142)
% advisers leave for protocol firms			−0.393[b]			−0.403[c]
			(0.161)			(0.218)
Lagged log(AUM) / log(Acct)	−0.254[a]	−0.253[a]	−0.254[a]	−0.306[a]	−0.305[a]	−0.306[a]
	(0.007)	(0.007)	(0.007)	(0.011)	(0.011)	(0.011)
Year FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Adj-R^2	0.38	0.39	0.39	0.25	0.25	0.25
Observations	43,980	43,980	43,974	43,980	43,980	43,974

test of the coefficient of interest, $\beta_{p,p}$, is not statistically different from zero. AUM fluctuates both with investment performance and flows, while the number of accounts depends only on flows. Indeed, these values in our sample have a correlation of only 0.41. Given this, the fact that our results are consistent across these measures adds to our confidence that we are capturing the common component that drives both, namely client flows.

In Panel B of the table, we decompose each of our measures of the percentage change in advisers into the percentage of advisers joining and leaving, and run regressions analogous to those in Panel A. We do this because we suspect that coefficient estimates on advisers leaving their firms will be much more precise since the average adviser's book of business at their current firm is likely more reflective of their actual book than at the firm to which they move.[14]

The estimates in Panel B show that this is indeed the case. For the most part, the estimates on the coefficients on the "% advisers leave" variables support our main hypotheses. The estimate of $\beta_{p,p}$ using AUM in column 3 is -0.39 and in column 6, using the number of accounts, it is -0.40. Both are similar and statistically different from zero. The coefficient estimates indicate that a 1% increase in advisers leaving for protocol firms from a protocol firm will lead to a decrease in AUM of 80 bps ($= -26.9 - 11 + 4.9 - 7.9 - 39.1$) and a decrease in the number of accounts of 77 bps ($= -21.9 - 11.7 + 6.7 - 9.8 - 39.9$). Of these decreases, we can safely say that half are due to advisers taking clients with them when they switch firms.

In summary, we estimate that when advisers move, that they take about 40% of their book of business with them. This supports the notion that a substantial portion of clients place trust in their advisers over the trust they have in their advisory firms. This has implications for the power dynamic between firms and advisers.

4.3. Disciplining advisers

In the previous section, we showed that unlocking clients leads to a substantial number of clients following their advisers when they switch firms. We therefore ask whether this makes firms reluctant to fire advisers, even when the advisers engage in bad behavior. (Egan et al., 2019), p. 235 find a large presence of repeat offenders among financial advisers and conclude that "this result implies that neither market forces nor regulators fully prevent such advisers from providing services in the future." In other words, clients are ineffective at disciplining "bad" advisers through asset transfers. This is likely drisxsven by information asymmetries. One way firms can mitigate this market imperfection is by disciplining advisers themselves. To test whether unlocking clients reduces firms' incentives to do so, we modify regression (1) to include an indicator variable that is one if the adviser engages in misconduct during year t ("Misconduct"), 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 in 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:

$$\begin{aligned}\text{Turnover}_{j,i,c,t+1} &= \alpha_{i,c} + \gamma_{c,t} + \beta_m(\text{Misconduct})_{j,t} \\ &\quad + \beta_p(\text{Firm in protocol})_{j,i,t} \\ &\quad + \beta_{p,m}(\text{Firm in protocol})_{j,i,t} \times (\text{Misconduct})_{j,t} \\ &\quad + \Gamma'\text{Controls}_{i,t} + \epsilon_{j,i,t}, \end{aligned} \tag{4}$$

where definitions of all variables follow those previously described. β_m measures turnover sensitivity to misconduct, which should be positive, at least in egregious cases of misconduct. β_p measures the difference in turnover propensity for firms once they join the protocol. If firms fear relationship-based outflows, 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.

The results are presented in Table 5 for the full sample and the sample of advisers who work for firms with at least 100 advisers. Following the earlier adviser-level anal-

[14] Suppose an adviser works at a firm with \$100 million in AUM that has ten advisers. On average, each adviser manages \$10 million. If the adviser leaves, taking \$5 million with her to a new firm, then we would estimate that a 10% decrease in advisers leads to a 5% decrease in AUM. If the adviser joins a firm with the same AUM/adviser ratio, then we would get a similar estimate on our coefficient "% advisers join," but if the new firm has a greater (smaller) AUM/adviser ratio the coefficient will be smaller (greater), thereby introducing noise to our estimates.

Table 5
Turnover sensitivity to misconduct.
The table displays regression results from linear probability models estimated using OLS (Eq. (4) in the text) of forced turnover 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 dependent variable is an indicator variable that is one if the adviser joins another firm after 90 days of being unemployed. 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 3. 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			$\geq$ 100 advisers		
	State enforces NCAs?			State enforces NCAs?		
	All	Yes	No	All	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Misconduct	0.458[b]	0.585[a]	−0.009	0.640[a]	0.746[a]	0.274
	(0.183)	(0.187)	(0.397)	(0.211)	(0.206)	(0.460)
Firm in protocol	−0.324[c]	−0.296[c]	−0.422[c]	−0.240	−0.195	−0.401
	(0.176)	(0.174)	(0.246)	(0.175)	(0.170)	(0.252)
Firm in protocol × Misconduct	−0.544[b]	−0.793[a]	0.304	−0.677[b]	−0.899[a]	0.052
	(0.241)	(0.247)	(0.532)	(0.263)	(0.262)	(0.583)
Log (number of advisers)	0.396	0.342	0.644[a]	0.087	−0.006	0.528[c]
	(0.327)	(0.369)	(0.239)	(0.381)	(0.425)	(0.313)
Log (years experience)	−0.699[a]	−0.672[a]	−0.806[a]	−0.748[a]	−0.715[a]	−0.880[a]
	(0.078)	(0.078)	(0.090)	(0.083)	(0.084)	(0.095)
Investment adviser	−0.929[a]	−0.878[a]	−1.136[a]	−0.865[a]	−0.819[a]	−1.059[a]
	(0.092)	(0.095)	(0.116)	(0.097)	(0.099)	(0.121)
Gen. sec. rep. (7)	0.180[c]	0.107	0.484[a]	0.017	−0.052	0.307[b]
	(0.103)	(0.108)	(0.124)	(0.111)	(0.116)	(0.134)
Inv. co. prod. rep. (6)	−0.529[a]	−0.524[a]	−0.549[a]	−0.654[a]	−0.641[a]	−0.701[a]
	(0.160)	(0.170)	(0.146)	(0.178)	(0.188)	(0.164)
Gen. sec. principal (24)	0.205[b]	0.213[b]	0.162	0.328[a]	0.322[a]	0.339[b]
	(0.100)	(0.097)	(0.130)	(0.112)	(0.108)	(0.151)
Number of other qual.	−0.014	−0.032	0.064[c]	−0.008	−0.026	0.070[c]
	(0.029)	(0.030)	(0.035)	(0.033)	(0.034)	(0.039)
Mean of the dep. var.	2.97	3.00	2.82	2.78	2.82	2.64
County-Year FE	Y	Y	Y	Y	Y	Y
Firm-county FE	Y	Y	Y	Y	Y	Y
Adj-R^2	0.03	0.03	0.03	0.02	0.02	0.02
Observations	5,891,188	4,712,699	1,178,489	5,221,183	4,180,975	1,040,208
$\hat{\beta}_{p\times m,\text{yes}} - \hat{\beta}_{p\times m,\text{no}}$			-1.097[b]			-0.951
			(0.559)			(0.599)

ysis 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 estimate of $\beta_{p,m}$ is −0.54 and is significant at the 5% significance level.

Splitting the sample between advisers who work in states that do and do not enforce NCAs (columns 2 and 3), we find that 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 unlock clients for fear of losing AUM.

4.4. Misconduct

Since firms are less likely to discipline their advisers for misconduct, it is natural to ask whether this affects the propensity of advisers to engage in misconduct. We therefore test whether adviser misconduct in-

Table 6
Adviser misconduct.
The table displays regression results from linear probability models estimated using OLS of a measure of adviser misconduct on "Firm in protocol." The analysis 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). 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. 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	All (1)	≥ 100 advisers (2)	All (3)	≥ 100 advisers (4)
Firm in protocol	0.104	0.131[c]	0.148[b]	0.195[a]
	(0.070)	(0.078)	(0.059)	(0.068)
Past misconduct	1.313[a]	1.197[a]		
	(0.068)	(0.077)		
Log (number of advisers)	0.037	0.064	−0.131[a]	−0.157[a]
	(0.047)	(0.061)	(0.011)	(0.019)
Log (years experience)	0.133[a]	0.131[a]	0.365[a]	0.396[a]
	(0.013)	(0.014)	(0.049)	(0.056)
Investment adviser	0.344[a]	0.351[a]	−0.048	−0.019
	(0.033)	(0.035)	(0.031)	(0.030)
Gen. sec. rep. (7)	0.107[a]	0.082[a]	0.194[a]	0.176[a]
	(0.023)	(0.026)	(0.043)	(0.047)
Inv. co. prod. rep. (6)	0.029	0.010	0.062	0.006
	(0.019)	(0.019)	(0.099)	(0.107)
Gen. sec. principal (24)	−0.035[c]	−0.080[a]	0.097[a]	0.095[b]
	(0.018)	(0.019)	(0.036)	(0.041)
Number of other qual.	0.013[c]	0.011	−0.069[a]	−0.081[a]
	(0.007)	(0.007)	(0.016)	(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^2	0.03	0.03	0.05	0.04
Observations	5,862,497	5,197,696	5,706,560	5,043,769

creases once firms unlock clients by joining the protocol. We regress "Misconduct," an indicator variable described in Section 4.3, on "Firm in protocol," controls, and two different specifications 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, so we 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 Table 6. 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-statistics 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 calculated 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. Compared to an unconditional probability of misconduct of 47 bps, this is an increase in likelihood of over 40%. We confirm the robustness of our results to various subsamples as well as extending the sample period back to 2003; these results are presented in Table IA.6 of the Internet Appendix. In all models that include adviser fixed effects, our inferences are unchanged.[15]

4.5. Fee rates

In this section, we investigate the dynamics of advisory fees following protocol adoption. Firms can increase their fees to compensate for the increased probability of relationship-based outflows or because they seek to exploit relationship-based inflows. Alternatively, firms can lower fees to attract relationship-based inflows from advisers seeking lower rates for their clients.

A broker-dealer can generate revenue from two main sources, commissions and fees. Because a commission-

[15] Clifford and Gerken (2019) investigate the relationship between advisers receiving customer complaints and broker protocol membership and find a weak negative relationship (10% significance level). Unlike the misconduct measure of Egan et al. (2019), their measure includes complaints that were dismissed, withdrawn, or are still pending. Since we are interested in adviser malfeasance we do not include disclosures where the adviser is exonerated. In untabulated tests, we find no significant relationship between protocol membership and total customer complaints, but weak evidence that frivolous customer complaints decrease with protocol membership.

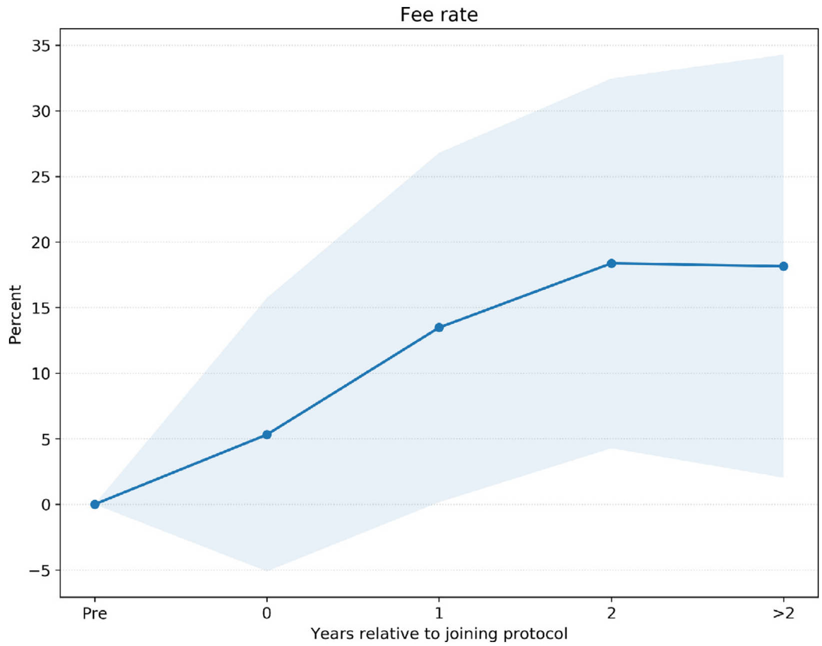


Fig. 3. Fee rates. The figure plots the coefficient estimates and their 10% confidence intervals of the $\beta_{p,t}$'s from Eq. (2), which is a linear regression model with firm and year fixed effects, that regresses "fee rates" on lags of "Join protocol." Therefore, the coefficient estimates on these indicator variables measure the changes in fees rates relative to their average prior to a firm joining the broker protocol. 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 3.5 of the text. The dependent variable is "Fee rate," which is the fee revenues divided by the fee-based AUM. Confidence intervals are computed using robust standard errors, clustered by firm.

based broker derives his income from selling particular investment products (such as mutual funds), a potential conflict of interest can arise between brokerages and their clients. For instance, Mullainathan et al. (2012) find that some advisers in the United States steer investors from well-diversified portfolios to high-fee mutual funds. Such opportunistic behavior has also been found in other financial products (Anagol et al., 2017) and other countries (Bhattacharya et al., 2012; Hackethal et al., 2012). 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 AUM.

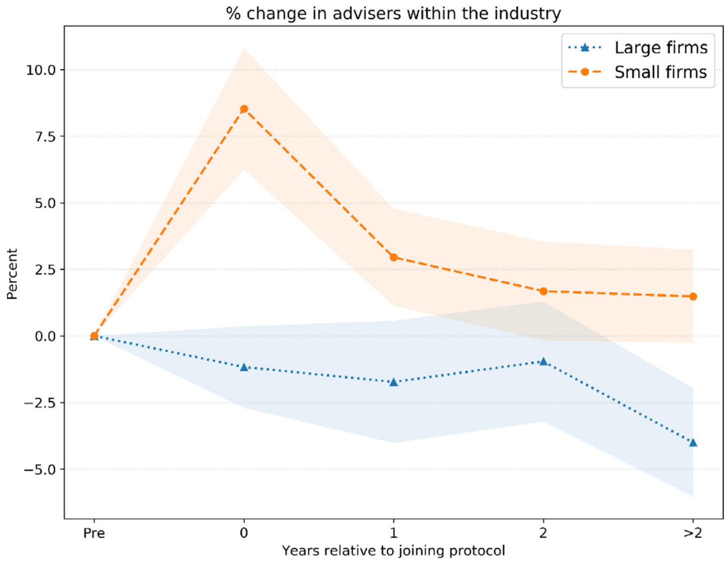
To test our hypotheses, we use broker-dealer revenue breakdown information from the B-D Data Center maintained on the *InvestmentNews* web site. As discussed in Section 3.5, our data set covers 2004 to 2016 and contains approximately 75 large broker-dealers per year. For each of these firms, we observe both the total revenue and fee revenue, as well as the total assets under management that generated the fees. From these, we calculate the "Fee rate", i.e., Fee rate = Fee revenues / Fee-based AUM. Table IA.7 in the Internet Appendix displays summary statistics of this sample. It shows that it is composed of large broker-dealers and the average fee rate is 1.0%.

We calculate how firms adjust their fee rates in response to unlocking clients by estimating Eq. (2) with fee rate as the dependent variable. The model includes firm and year fixed effects, so the coefficients on the β_p's capture the abnormal changes in the fee rate since prior to protocol adoption. Fig. 3 plots the coefficients' estimates on the β_p's and their 10% confidence intervals. It shows that in the year of protocol adoption fee rates do not increase significantly. However, in the second year rates increase about 14 bps and by year three they increase another 4 bps to 18 bps, where they remain significantly higher. Compared to the average fee rate of 100 basis points, the increase is not only statistically significant, but also economically large. These results suggest that unlocking clients led to higher fees, at least at large broker-dealers.

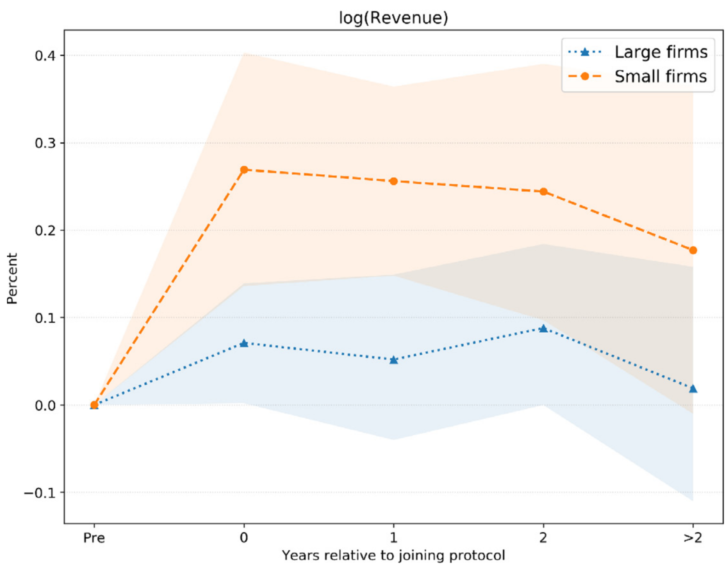
4.6. Winners and losers to unlocking clients

We next investigate the effect of unlocking clients on firms. If all firms within the protocol charge the same fees for identical products, then advisers moving from firm to firm is zero sum game. In fact, this was likely the expectation of the originators and early adopters of the protocol: that net relationship flows with other large brokerages would be small, but that litigation costs would decline. As time went on, however, small firms began joining the protocol, as we show in Table 1. These firms stood to gain from the protocol because it protected them from prohibitively large settlement payouts that they would have had to make if they poached advisers from larger firms in the absence of the agreement. We therefore suspect that small firms are the ultimate beneficiaries of unlocking clients.

We split the sample between small (fewer than 100 advisers) and large (100 or more advisers) firms and explore the firm-level dynamics around protocol adoption on adviser growth, revenue, and misconduct. To do this, we es-



Panel A

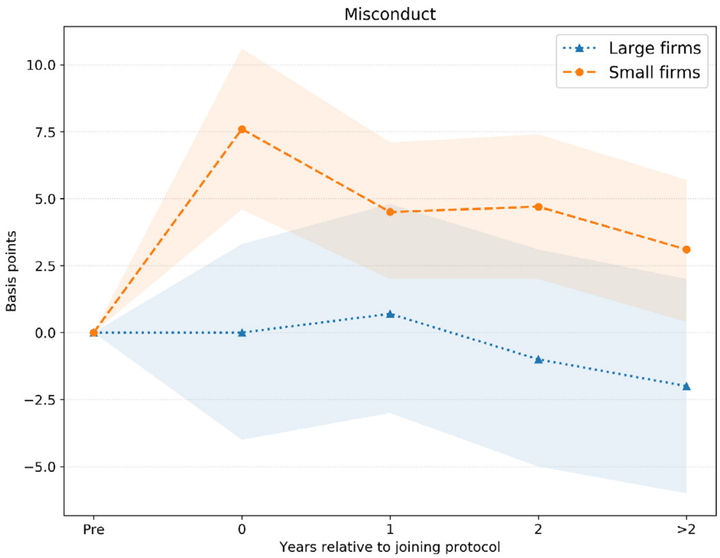


Panel B

Fig. 4. Firm-level outcomes by firm size. The figure plots the coefficient estimates and their 10% confidence intervals of the $\beta_{p,t}$'s from Eq. (2), similar to Figs. 2 and 3 for samples of small (less than 100 advisers) and large firms (100 or more advisers). The dependent variables are within industry turnover (Panel A), the natural log of total revenue (Panel B), and a firm level misconduct dummy (Panel C), where definitions follow those in Table A.2. The analysis and Panels A and C uses the entire firm-level sample described in Panel B of Table 2, split by firm size. The analysis in Panel B uses the sample of broker-dealers covered by the FOCUS data, as described in Section 3.5 and summarized in Table IA.8 in the Internet Appendix. Confidence intervals are computed using robust standard errors, clustered by firm.

timate Eq. (2) with these alternative dependent variables. Fig. 4 plots the coefficient estimates and their 10% confidence bounds. The data on revenue is from FOCUS reports and is only available for broker-dealers. These data are described in Section 3.5 and summary statistics are provided in Table IA.8 in the Internet Appendix.

In Panel A, the dependent variable is "%Δ in advisers within industry." The figure shows that small firms saw massive growth in the first two years following protocol adoption. In the first year, small firms, on average, grew their advisory teams through poaching by over 8%. During the second year of protocol membership, they grew another 3%. In subsequent years, their growth was almost 2% above pre-adoption levels, but not significantly different from zero. Large firms, on the other hand, saw no abnormal growth in the initial years of proto-



Panel C

Fig. 4. Continued

col adoption and by the fourth year these firms began to shrink.

Estimates of changes in revenue, displayed in Panel B, tell a similar story. Small firms saw dramatic increases in revenue following protocol adoption. In the first year of protocol adoption, the revenue of small firms increased by 27%, on average, and this increase remained fairly steady over time. Large firms saw a more muted response. Revenue increased by only 7% in the first year.

In Panel C we also see that the prevalence of misconduct spikes among small firms after unlocking clients, but not among large firms. This may not be surprising. For small firms, each adviser's relationships represent a larger proportion of the firms' total assets. In other words, the relationship assets are much more concentrated for small firms. Therefore, losing one adviser is much more costly to a small firm than a large firm. This makes small firms less likely to discipline advisers for bad behavior, similar to the idea of key human capital put forth by Israelsen and Yonker (2017).[16]

Together, these results suggest that if all clients were unlocked in the industry, small firms would be the beneficiaries, although it is not clear whether this is good or bad for clients.

5. Robustness

5.1. Subsample analysis

To check the robustness of the results, we replicate all adviser-level results (Tables 3, 5, and 6) for three different subsamples. The results are displayed in the Internet Appendix in Tables IA.4, IA.5, and IA.6, respectively.

First, we limit the sample to advisers who are brokers. Several studies of financial advisers (i.e. Egan et al. (2019); Clifford and Gerken (2019)) exclude those who are investment advisers, but not brokers from their samples. To ensure that our results are not driven by these advisers, we exclude them. In general, the main results are unchanged. 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 to 2016, acknowledging that this sample could 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.

5.2. Binary choice models

As an alternative to the linear probability models used to estimate the results displayed in Tables 3, 5, and 6, we estimate our results using binary choice models, but leave the results untabulated.[17] Because maximum likelihood es-

[16] In unreported results, we confirm that unlocking clients leads to laxer discipline among small firms by replicating the analysis in Table 5 for the sample of firms with fewer than 100 advisers.

[17] These results are available upon request.

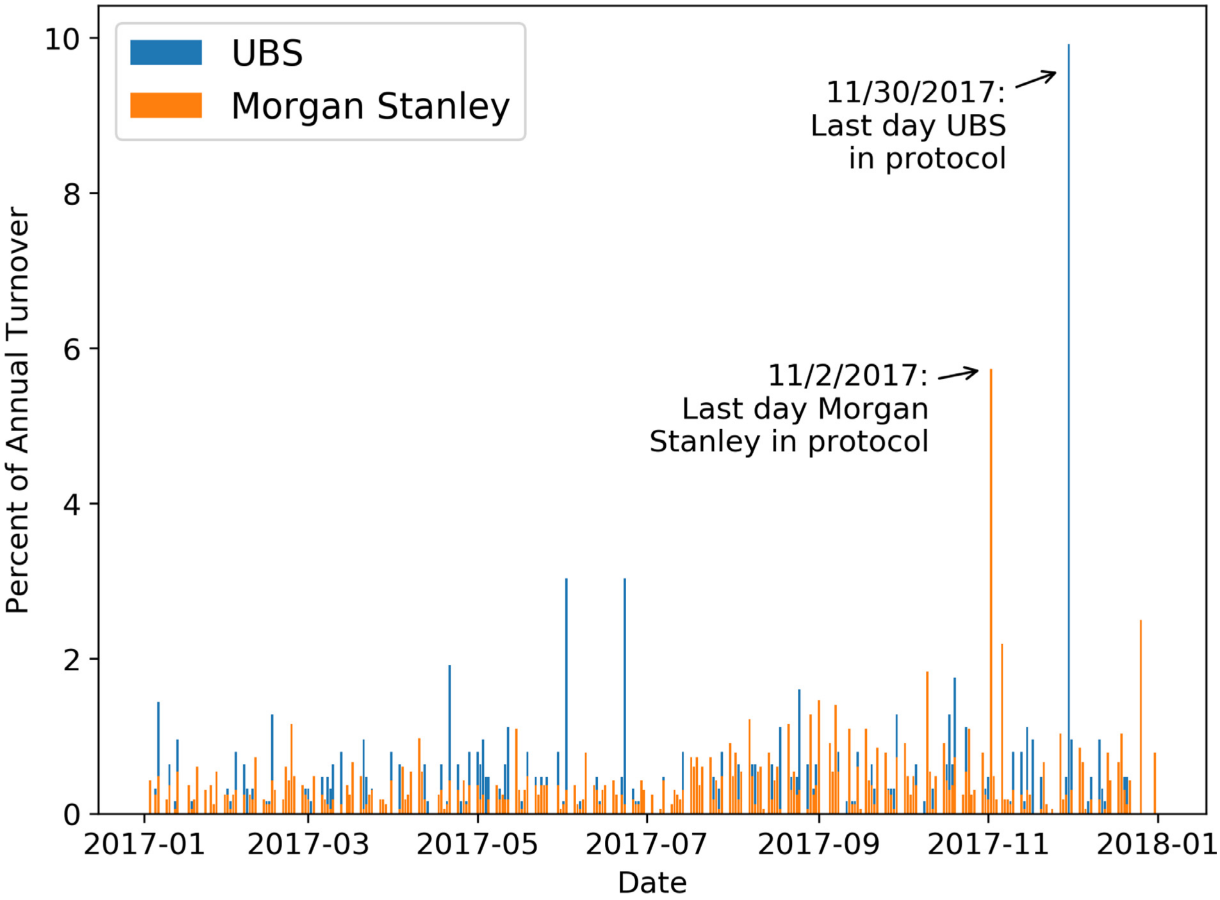


Fig. 5. Protocol withdrawal and adviser exits. The figure plots the percentage of 2017 annual turnover occurring each business day of the year for Morgan Stanley (blue) and UBS Financial Services (orange). On October 24, 2017, Morgan Stanley submitted a letter indicating that it would like to withdrawal from the broker protocol. UBS followed suit on November 20, 2017. It takes ten days for the withdrawal to take effect. Therefore, 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. Those dates are indicated on the graph above. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

timation of nonlinear binary choice models has computational difficulties when the number of fixed effects is large, we first estimate our results using both probit models and linear probability models omitting fixed effects from the models. We find that both models give consistent results across all of our results.

Stammann et al. (2016) develop a package in R, called "bife," that can handle one dimension of high-dimensionality fixed effects. They call their estimator a "bias-corrected logit estimator." We reestimate the results using this estimator, which allows us to include branch fixed effects in our specifications. Since we cannot also include county-year fixed effects, we include yearly fixed effects to absorb general economic conditions. All specifications include the control variables included in our baseline models. Again, we find that all of our earlier results hold. Finally, we include adviser fixed effects in the misconduct regressions analogous to those in Table 6, column 3. We find that the coefficient on "Firm in the protocol" is positive, but not statistically different from zero (p-value$= 0.23$). We conclude that our results are robust to estimation using binary choice models.

5.3. *Out-of-sample evidence: protocol withdrawals and adviser exits*

As an out-of-sample test of the impact of unlocking clients on adviser turnover decisions, 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. 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.

Fig. 5 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 Fig. 5. 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.

6. Conclusion

We demonstrate the importance of client relationships in the financial advisory industry. Our evidence shows that these relationships are critical for clients, advisers, and their firms. They drive advisers' employment decisions: advisers are much more likely to move to firms to which they can freely transfer their clients. Clients value their relationships with their advisers, and follow them to new firms. When unconstrained, advisers move about 40% of client assets with them when they switch firms. These relationships are also important to firms, which become less willing to discipline their advisers for misconduct, for fear of experiencing relationship-based outflows.

An important question is what would be the implication if all clients in the industry were unlocked. How would this affect advisers, clients, firms, and the industry? While we cannot provide a definitive answer, our results give us some clues.

We believe that advisers would stand to gain the most, since they would effectively gain control of a portion of the revenue-generating assets of firms. Unfortunately, we do not observe adviser preferences or wages, but, by revealed preference, advisers would not voluntarily move to another firm unless it makes them better off. Increases in adviser welfare could come through higher wages, better product offerings to clients, or more favorable working conditions.

What about clients? Again, by revealed preference, we suspect that clients believe that they would be better off. After all, why else would they follow their advisers? However, two of our empirical findings question whether that's really the case. First, we find that the lax monitoring of firms induced by unlocking clients leads to a greater incidence of adviser misconduct. Second, we show that following protocol entry, firms raise their fees permanently by about 18 bps, albeit for a small sample of large broker-dealers. Again, our assessment is limited by data. We do not observe the actual products into which clients are allocated, nor do we observe the relevance of clients' allocations to their goals and objectives. The finding that misconduct increases following protocol adoption is somewhat informative, since, as Egan et al. (2019) report, 21% of client complaints are related to the suitability of their investments.

We conclude that unlocking clients would likely benefit small firms over large. We show that protocol adoption affected small firms dramatically more than large firms. It enabled small firms to freely poach advisers from large firms and to gain clients' assets. Essentially, a policy of unlocking clients would level the playing field among firms. A blanket policy could alter the competitive landscape within the industry.

Some legislators in Washington have also expressed concern that NCAs are used by firms to suppress the wages of lower level employees.[18] Theory suggests that employee compensation should be greater in the absence of NCAs because of the creditable threat of employees moving 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 could explore more directly the effects of NCAs on compensation in other industries.

Appendix

A.1. 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 data set. Data from the SEC's IAPD web site 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 web site Terms of Use explicitly provided permission for researchers to download the data for academic purposes.[19] The FINRA web site states that it maintains information on the web site for brokers who have been registered within the last ten years, or possibly longer,[20] indicating that we can have confidence that our sample is free of possible survivorship bias beginning in 2007.

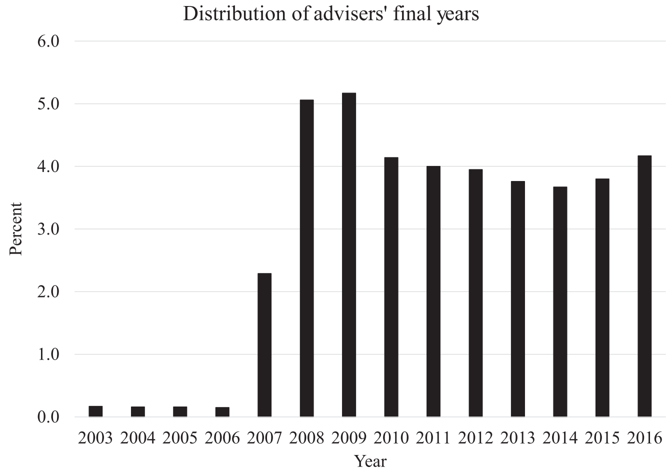
To verify this, we calculate the last year that each financial adviser is included in the data. Panel A of Fig. A.1 shows the distribution of these final years. Almost none of the advisers file their final deregistration prior to 2007, which is ten years prior to when we collected the data.[21] It therefore appears that FINRA deletes entire adviser histories from the publicly available data once they have

[18] See, for example, https://www.c-span.org/video/?c4796572/sen-van-hollen-questions-ftc-chair-joseph-simons.

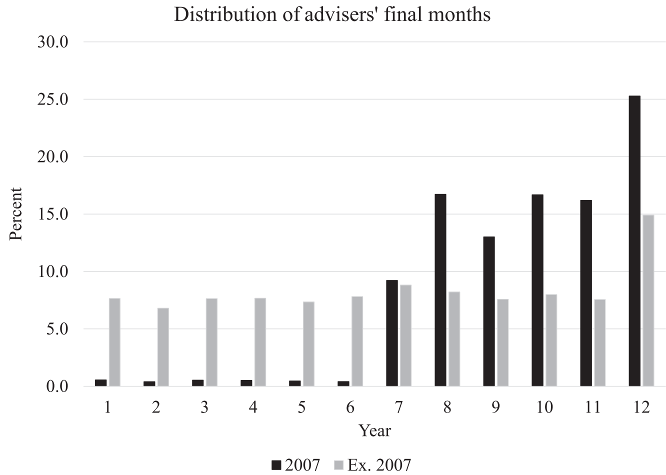
[19] See item 5 of FINRA BrokerCheck Terms of Use, modified July 17, 2017.

[20] See www.finra.org/investors/about-brokercheck.

[21] The figure does not include 2017. About 68% of advisers in the sample are still registered in 2017.



Panel A



Panel B

Fig. A.1. 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 web sites 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 shows the distribution of final months for 2007 and for the years 2003–2016, excluding 2007.

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 there's 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 free of survivorship bias only during the period beginning in August, 2007.

A.2. Additional information on sample construction

One complication in constructing the employee-employer matched data set is that the data provide registration dates, rather than actual employment dates. An adviser could, for example, de-register but stay with a firm in a nonadvisory 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 his registration ends but then begins again at the 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 represents 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 for advisers who 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.3. Variable definitions

Table A.1
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 web site, 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).
Leave for 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 for 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 web site, IAPD, BrokerCheck
Adviser-level variables	Definition	Source
Leave for a nonprotocol 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 web site, IAPD, BrokerCheck
Forced turnover	An indicator variable that is one if "Leave for another firm" is one and the number of days before joining another firm is greater than 90.	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/Judgment; or Civil—Final. These six types of disclosure are selected from a total of 23 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
Primary employer	Employer who has employed the adviser the longest.	IAPD, BrokerCheck

Table A.2
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 web site
Log (number of advisers)	Log of the total number of advisers employed by 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 for 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 for 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 for 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
%Δ in advisers	The percent change in the total number of advisers at the firm.	IAPD, BrokerCheck
%Δ 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 firm's 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
%Δ 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 firm's advisers leaving for 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
%Δ 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 for 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 web site, IAPD, BrokerCheck
Misconduct dummy	An indicator variable that is equal to one if any of the firm's advisers 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
ΔLog (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 3F2c
Log (AUM)	Log of total assets under management at the end of the fiscal year.	SEC Form ADV, Part 1a, Item 3F2c
ΔLog (Accts)	Change in the log of total number of accounts from the end of the previous fiscal year to the end of the current fiscal year.	SEC Form ADV, Part 1a, Item 3F2f
Log (Accts)	Log of total number of accounts at the end of the fiscal year.	SEC Form ADV, Part 1a, Item 3F2f
Fee rate	Fee revenues / fee-based AUM	B-D Data Center maintained on the *InvestmentNews* web site
Log (Revenue)	Log of total revenue end of the fiscal year.	Audit Analytics item "pe_ended_rev"

References

Abadie, A., Athey, S., Imbens, G.W., Wooldridge, J., 2017. When Should You Adjust Standard Errors for Clustering? NBER working paper.

Anagol, S., Cole, S., Sarkar, S., 2017. Understanding the advice of commissions-motivated agents: evidence from the indian life insurance market. Rev. Econ. Stat. 99, 1–15.

Bergstresser, D., Chalmers, J.M.R., Tufano, P., 2009. Assessing the costs and benefits of brokers in the mutual fund industry. Rev. Financ. Stud. 22, 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. Rev. Financ. Stud. 25, 975–1032.

Chalmers, J., Reuter, J., 2020. Is conflicted investment advice better than no advice? J. Financ. Econ. 138, 366–387.

Charoenwong, B., Kwan, A., Umar, T., 2017. Who Should Regulate Investment Advisers? SSRN working paper 2899883.

Christoffersen, S.E., Evans, R., Musto, D.K., 2013. What do consumers' fund flows maximize? Evidence from their brokers' incentives. J. Finance 68, 201–235.

Clifford, C., Gerken, W., 2019. Investment in Human Capital and Labor Mobility: Evidence From a Shock to Property Rights. SSRN working paper 3064204.

Dimmock, S.G., Gerken, W.C., Graham, N.P., 2018. Is fraud contagious? Coworker influence on misconduct by financial advisors. J. Finance 73, 1417–1450.

Egan, M., Matvos, G., Seru, A., 2019. The market for financial adviser misconduct. J. Polit. Economy 127, 233–295.

Foerster, S., Linnainmaa, J.T., Melzer, B.T., Previtero, A., 2017. Retail financial advice: does one size fit all? J. Finance 72, 1441–1482.

Franco, A.M., Filson, D., 2006. Spin-outs: knowledge diffusion through employee mobility. RAND J. Econ. 37, 841–860.

Gennaioli, N., Shleifer, A., Vishny, R., 2015. Money doctors. J. Finance 70, 91–114.

Germann, M., Loos, B., Weber, M., 2018. Trust and Delegated Investing: A Money Doctors Experiment. SSRN working paper 3187189.

Greenhouse, S., 2014. Noncompete Clauses Increasingly Pop up in Array of Jobs. New York TimesSection B, p. 1.
Gurun, U.G., Stoffman, N., Yonker, S.E., 2018. Trust busting: the effect of fraud on investor behavior. Rev. Financ. Stud. 31, 1341–1376.
Hackethal, A., Inderst, R., Meyer, S., 2012. Trading on Advice. SSRN working paper 1701777.
Huang, J., Wei, K.D., Yan, H., 2007. Participation costs and the sensitivity of fund flows to past performance. J. Finance 62, 1273–1311.
Israelsen, R.D., Yonker, S.E., 2017. Key human capital. J. Financ. Quant. Anal. 52, 175–214.
Klepper, S., 2002. The capabilities of new firms and the evolution of the U.S. automobile industry. Ind. Corp. Change 11, 645–666.
Klepper, S., Sleeper, S., 2005. Entry by spinoffs. Manage. Sci. 51, 1291–1306.
Kostovetsky, L., 2016. Whom do you trust?: investor-advisor relationships and mutual fund flows. Rev. Financ. Stud. 29, 898–936.
Lavetti, K., Simon, C., White, W.D., 2019. The impacts of restricting mobility of skilled service workers: evidence from physicians. J. Hum. Resour. 55, 1025–1067.
Lynch, A.W., Musto, D.K., 2003. How investors interpret past fund returns. J. Finance 58 (5), 2033–2058.
Marx, M., 2011. The firm strikes back: non-compete agreements and the mobility of technical professionals. Am. Sociol. Rev. 76, 695–712.
Marx, M., Strumsky, D., Fleming, L., 2009. Mobility, skills, and the Michigan non-compete experiment. Manage. Sci. 55, 875–889.
Mullainathan, S., Noeth, M., Schoar, A., 2012. The Market for Financial Advice: An Audit Study. NBER working paper.
Rosegrant, S., Lampe, D.R., 1992. Route 128: Lessons from Boston's High--Tech Community. Basic Books, Inc..
Samila, S., Sorenson, O., 2011. Noncompete covenants: incentives to innovate or impediments to growth. Manage. Sci. 57, 425–438.
Saxenian, A., 1996. Regional advantage. Harvard University Press.
Sirri, E.R., Tufano, P., 1998. Costly search and mutual fund flows. J. Finance 53, 1589–1622.
Stammann, A., Heiss, F., Mc Fadden, D., 2016. Estimating fixed effects logit models with large panel data.
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. Adm. Sci. Q. 48, 175–201.