

Investment Adviser Regulatory Jurisdiction and Reported Misconduct

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Abstract

We compare the ability of state and federal regulators to deter misconduct by investment advisers. Dodd-Frank shifted oversight responsibility for mid-size firms (\$25M-\$100M in assets) from the SEC to state-securities regulators for exogenous reasons. We find that client complaints increased 60% for mid-size advisers, relative to similar advisers continuing under SEC oversight. Complaints increased more in states with less staff-per-adviser. Advisers with histories of misconduct, serving older and less-educated clients, and located farther from regulators misbehaved more. Severity increased as awarded damages doubled and denial rates dropped 10%. The results suggest state regulators deter less misconduct, especially against less-sophisticated clients.

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1 Introduction

Many individuals rely on financial advisers when making important financial decisions. In 2015, investment advisers oversaw \$66 trillion in assets, and the most recent 2013 Survey of Consumer Finances finds 58% of American households are a customer of some investment adviser.¹ Unlike in banking, where customers are provided low-risk products such as checking or savings deposits that are backed to some extent by government guarantees, investment decisions are much riskier with no such guarantees. Although advisers have a fiduciary duty to clients, as a standard and not a rule, fiduciary duty is subject to interpretation and may vary in execution and enforcement quality.

Recently, a new literature has arisen on the determinants and consequences of adviser misconduct. A recent paper by Egan et al. (2017a) documents that a surprisingly large fraction of advisers, 1 in 5, frequently engage in misconduct. Misconduct has direct financial consequences on clients and lowers general trust in the financial sector, which itself can be consequential for the financial sector as a whole Gurun et al. (2017). Egan et al. (2017a) find the adviser market does not to completely filter out misconduct. One reason may be customers may not be able to verify service quality directly due to information frictions, facing search costs (Egan (2017)), and are incapable of monitoring advisers fully. Not only are misconduct rates high, many advisers are repeat offenders. While 50% of advisers lose their jobs after misconduct, 44% of terminated advisers find work within the industry in one year.

In this paper, we exploit the extent to which regulators affect financial misconduct. Regulators may be important in monitoring advisers directly, educating investors, implementing regulation, and identifying and investigating a specific act of misconduct. Oversight of investment advisers is currently divided between a national regulator (the Securities and Exchange Commission) and state regulators, with the SEC overseeing larger and more complex firms (RIAs). A national regulator may have superior human capital, organizational practices, and visibility. In contrast, a state regulator may have better soft information and be more accessible to local constituents. Which regulator is better is an empirical question. In general, assessing the relative performance of regulators is challenging because oversight responsibil-

¹This fraction rises to 75% for households with over \$100,000 in investable assets.

ities rarely change, and when shifts occur, laws tend to change as well.

A unique opportunity to answer this question arose when the Dodd-Frank Act shifted the regulatory landscape, re-assigning a subset of RIAs from SEC oversight to state oversight. More specifically, Dodd-Frank mandated that the SEC transfer oversight of “mid-sized” RIAs (\$25M-100M in assets under management (AUM)) from the SEC to state regulators, except for RIAs located in Wyoming and New York.² This shift occurred quickly, being announced on July 21, 2011, and in effect by January 1, 2012. The size threshold of \$100M AUM was likely chosen because the threshold conveniently reversed a piece of the 1996 National Securities Market Investment Act (NSMIA).³ The objective of the shift was to offset SEC resources newly dedicated to oversight of hedge funds and private equity firms. Therefore, we argue the shift was exogenous to the behavior of advisers working at mid-size RIAs. Also, the states and Dodd-Frank did not alter the legal fiduciary standard advisers are held to. This re-jurisdiction affected over 38% of all existing SEC-registered RIAs.

Using a difference-in-differences design, we study how a shift from SEC to state regulator oversight affects reported complaint rates filed by customers against specific investment advisers. Complaints are an expression of client dissatisfaction with an adviser’s behavior. The shift in oversight did not substantively change the process of filing a complaint, which may be submitted anonymously online for free.⁴ Moreover, complaints are available online for every adviser. Data on the returns advisers earn clients are rarely available and may be confounded by clients’ risk preferences. Using the complaint data, we construct a survivorship-bias-free panel data set at the individual-year level for the years 2009 to 2014.⁵ We assemble these data using a variety of required regulatory disclosures, including nearly 500,000 documents containing full employment histories for individual advisers and all disciplinary and regulatory disclosures. We also gather firm-level annual filings for every RIA, obtained through the Freedom of Information Act, and we hand collected budget data for every state.

Our main finding is that complaint rates increased by 0.5 percentage points per year due to

²Wyoming had no registration requirements for investment advisers at the time, and New York does not examine investment advisers.

³NSMIA had assigned mid-size advisers to the SEC as part of a broader effort to unify state-securities regulations.

⁴Clients must visit their respective state securities regulator’s website.

⁵In this paper, “advisers” refers to individuals, and “RIAs” refers to firms.

the re-jurisdiction, relative to the complaint rate of the control group. This increase is 60% of the complaint rate of the control group in our sample. The increase in complaints was driven by increases in fraud, misrepresentation, and unauthorized activity and not by an increase in frivolous charges.

There are a number of possible explanations for why complaints rose. A reported complaint can be thought of as misconduct times its detection rate, and it is possible that detection rates increased without adviser behavior changing. Adding in the possibility of noise, these complaints may or may not be frivolous. Detection rates could be driven by regulators or customers. It is possible that customers saw an opportunity to seek redress against their investment adviser, perhaps especially since the implementation of this regulatory change probably resulted in an announcement to clients that made state regulators salient. Another possibility is that state regulators themselves are more active than the national regulator, performing more regular or surprise examinations and engaging in other actions that suss out evidence of misconduct. This naturally would result in more complaints. Regulators may possess soft information or be especially motivated to pursue their increased role. Finally, it is possible that actual misconduct itself increased.

We use a variety of evidence to sort these explanations. First, regarding frivolous complaints, we study outcomes of complaint cases. It is important to note that while regulators receive and process complaints, for the vast majority of firms in our sample, the adjudication of the complaint occurs in a Financial Industry Regulatory Authority (FINRA) arbitration panel. FINRA's role was not affected by this re-jurisdiction. We find that in fact the severity of the complaints increased: awarded damages doubled and denial rates declined 10%. This suggests the rise in complaints was not frivolous. Thus either misconduct rates increased or reporting rates did.

Before delving into new cross-sectional tests we develop, we first adapt cross-sectional tests based on Egan et al. (2017a). These tests help us adjudicate our main finding but also provide external validity against prior work. Given that Egan et al. (2017a) find strong evidence of recidivism, we first test whether recidivism is higher among representatives at treated firms. We find indeed that it does. In a cross-sectional test, we find that controlling for prior misconduct, in the period between 2012-2014, reps working at treated firms are more likely to

receive complaints if they received complaints in the past, above and beyond untreated reps. Advisers with histories of misconduct who were treated are 5 percentage points more likely to misbehave than advisers with past misconduct who were not treated, which represents a 50% increase in misconduct relative to the misconduct rates for the untreated RIAs. For untreated reps, the magnitude of recidivism we find is similar to prior work. Next, we test whether client composition interacts with the treatment effect. Egan et al. (2017a) find that misconduct is greatest where clients are less sophisticated. Using our data, we can identify the city name in which the adviser works, which we map to a county. Using population data for a client's county, we find complaints increased more for advisers working in counties with less educated and older populations.

Our cross-sectional tests study the importance of regulator funding. First, we find that the treatment effect is highest in states with the lowest regulator-staff-to-RIA ratios from 1999, when the American Association of Retired Persons conducted a study. The reduced form estimate suggests that a standard deviation movement in budget ratios can explain about 40% of the average treatment effect. Second, we also find that states did not increase resources devoted to the state's regulatory department as states were fiscally constrained in 2012, providing suggestive evidence that regulatory resources did not match the increased workload.

We then test whether physical proximity affects the complaint rates of treated firms after the enactment of the Dodd-Frank Act. If funding were a concern for regulatory quality, then firms farther away in physical proximity to the regulator should receive lower quality regulation. Prior work has suggested that physical distance constrains monitoring ability, affecting the ability of managers to monitor within the firm and of regulators to monitor constituents.⁶ We test this in two ways. First the headquarter distance of the firm to the relevant regulator and secondly the distance of the adviser rep's branch to the relevant state regulator. We find a robust, positive relation between distance and the treatment effect. Using headquarter distance as a measure, the effect is robust controlling for zipcode-by-year fixed effects, state-by-year

⁶Giroud (2013) finds that plane routes affect intra-firm monitoring. It is not just businesses but also regulators who are constrained by physical distance. Nguyen and Nguyen (2017) find that firms increase fraud when further away from a branch of the SEC; closures of these offices appear to increase misconduct by increasing the distance to the closest regulator.

and branch-by-year fixed effects. Magnitudes are such that we find that a standard-deviation increase in distance to the regulator resulted in a 50% greater treatment response.⁷ Second, using branch distance as a measure, where branch is measured as the county-name where the adviser works, we find the same relation *within a county-year*, and *within a firm-year*.

Lastly, regulator involvement likely decreased for the small RIAs with less than \$25 million in assets under management, who were already under state regulation before Dodd-Frank. We find that when the workload of state regulators increases, the complaint rates of always-state-registered small-size RIAs should decrease relative to those who remained registered with the SEC. Finally, using client-type data reported by RIAs in Form ADVs, we also find the treatment effects are lower among advisers serving more sophisticated clients.⁸

After displaying this array of tests, we argue the evidence is incongruent with explanations surrounding customers' or regulators' behavior driving the rise in complaints. On balance, while regulators and customers may have changed their behavior, complaint rates rose in the cross-section where detection rates were likeliest to be weaker. This suggests that we are observing more than simply increases in *reported misconduct*, but rather an increase in *actual misconduct*.

If regulators' behavior somehow drove our findings, we first would expect the effect to be greatest among advisers serving under more well-staffed regulators. Secondly, we would also expect a decrease, not a rise, among already registered advisers. Finally, regulators would likely target advisers closer to them. Yet our cross-sectional evidence points the opposite way on all three counts. This again suggests that increased regulatory strength is unlikely to be driving complaint rates. Finally, any story of regulator strength must also explain why regulators of treated firms are less likely to take regulator action when an adviser rep receives a complaint. A stronger and tougher regulator would presumably be more likely to also take action when a complaint occurs.

If customers were strategically responding to a new regulator, one would not expect the observed rise in complaints to be greatest among less sophisticated individuals. Instead we

⁷This finding is robust to a variety of fixed effects that account for how distance may be correlated with local, time-varying economic conditions around where the RIA is headquartered.

⁸These findings rule out one alternative, which is that clients may be optimally responding to weaker state regulators by monitoring advisers more closely.

find that older and generally less educated customers complained more when considering client demographics, and firms whose clients were non-institutional. If customers were complaining because of an *advertising effect* or because the local regulator appeared more accessible, then the clients of advisers closer to the regulator should see more complaints. Instead we find the treatment effect to be greatest among those further from the regulator. With respect to how the results appear to contradict the regulator-driven and customer-driven explanations, we explain our logic in greater detail in Section 6.

These findings contribute to the literature in two ways. First, we contribute to the large body of work on the optimal design of financial regulation. Our evidence is especially important given the two centuries-long Federalism debates regarding division of responsibilities between local and national governments and a recent sea of regulatory change in the financial industry.⁹ The paper most relevant to ours is Agarwal et al. (2014), who study rotating state and federal supervisors of banks. Like us, they find that federal supervisors are harsher. However, our paper makes new and important contributions. First, our paper provides meaningful external validity in a *different* industry outside of banking. It is not clear ex-ante the results would be the same, as regulators play a different role in banking as they do among financial advisers. explain explain bs not sure yet

In addition, banks are large entities provide capital that is necessary for the local economy to function. This is the principal reason Agarwal et al. (2014) believe state regulators are less harsh. However, the advisers we consider are small, and it is not clear regulators would cater to financial industry participants who are not also not broadly consequential. Given that bank regulators are likely more well-staffed in general than any other financial regulatory agency, and our cross-sectional results on budget and distance, our results instead likely point more to a more meaningful role of regulatory resources. For this reason, our results suggest that for other types of financial regulation, it may be suboptimal to rely on local regulators, even if they can avoid capture or are not subject to local interests. To wit, we cannot comment on whether given the resource-productivity of the current state regulators, whether the current level of

⁹Gopalan et al. (2016) is a recent paper that discusses closures of local field offices regulating banks, suggesting branch closures enable increased risk-taking. Karolyi and Taboada (2015) and Houston et al. (2012) study regulatory arbitrage in cross-border banking. Kondo (2014, 2016) study FINRA as a self-regulatory body and document the role of conflicts of interest and competition between regulators, respectively.

misconduct is optimal against the cost it would require to deter it more effectively. However, to the extent our findings indicate the SEC is more resource-productive, the marginal policy response should be to redistribute state regulatory budgets to the national government. ¹⁰

Second, we contribute to the small but rapidly growing literature on financial advising. One thrust of this literature is that the ability of consumers to trust advisers is consequential for the financial advice industry as a whole. Gennaioli et al. (2015) makes this point theoretically, while Gurun et al. (2017) demonstrates this empirically using the Madoff scandal as a shock to trust among ethnic communities that were most likely to entrust Madoff. Another thrust of this literature is about the quality of advice by advisers. Advisers often times may provide suboptimal advice either due to conflicts-of-interests or limitations to their own investment ability, even if they mean well ¹¹. That said, despite these limitations, advisers play an important role in the information gap between retail investors and the markets. Egan (2017) demonstrates that brokers are able to issue dominated products with identical payoffs, suggesting that customers face search frictions and brokers can on average earn 1.2% higher fees as a result. Relatedly, Chang and Szydlowski (2016) theorize that high adviser fees constitute a fee for information provision, with unsophisticated clients paying higher fees.

Most important to our paper, however, is the new literature on adviser misconduct. Dimmock et al. (2017) study the peer effects of investment adviser fraud after RIAs merge. Egan et al. (2017a) document widespread misconduct and recidivism in the investment adviser industry and suggest market incentives and RIA internal and external governance in their present form do not seem sufficient to eliminate misconduct. Our paper shows that the entity that regulates and the quality of such an entity are meaningful drivers of misconduct rates. The identity of the regulator is certain to matter in the future because by nature of a fiduciary standard, legislation alone cannot collectively, exhaustively enumerate instances of misconduct. In recent debates regarding the Department of Labor Fiduciary standard for brokers,

¹⁰Unlike in Agarwal et al. (2014), regulatory arbitrage is less relevant in our context, as the impact of our regulatory change is meant to be permanent, and the low lead time to implementation reduces concerns about partial anticipation.

¹¹Using a panel of Canadian advisers, Foerster et al. (2017) find that advisers do increase risk-taking by clients, raising expected returns; however, they find limited evidence of customization of advice to clients' risk preferences. The finding that advisers often provide sub-par advice is echoed in a field survey by Mullainathan et al. (2012) and archival studies by Chalmers and Reuter (2013) and Hackethal et al. (2012). Interestingly, requiring consideration of client suitability may reduce bad advice, as found by Chang et al. (2015).

the SEC and state regulators have each opined their own version of the fiduciary law. Our results make the timely suggestion that the assignment of responsibilities for standard-setter and regulator will have a large effect on implementation quality.

Finally, our paper is of relevant to new debates regarding the controversial Department of Labor’s expansion of the fiduciary standard to brokers. Each state, for example, is considering its own implementation of a fiduciary standard for brokers. Our results suggest proper coordination and division of responsibility among regulators is likely to affect outcomes for investors.

2 Background on Investment Advisers

2.1 History of Regulatory Jurisdiction

In the United States, investment advisers are regulated under the Investment Advisers Act of 1940 (Advisers Act). The Advisers Act holds investment advisers to a fiduciary standard, requiring them to act in their clients’ best interests. The legal definition of an investment adviser is broad: an adviser is “*any person who, for compensation, engages in the business of advising others, either directly or through publications or writings, as to the value of securities or as to the advisability of investing in, purchasing, or selling securities, or, who for compensation and as part of a regular business, issues or promulgates analyses or reports concerning securities,*” except when “*solely incidental.*”¹² In addition to providing securities advice, investment advisers may manage investment portfolios, provide financial advice, and offer brokerage services (e.g. buying or selling stock or bonds).¹³ Usually, an investment adviser firm has several investment companies (mutual funds, closed-end funds, unit investment trusts, private funds), and in turn each investment company could offer several different funds. Thus, common names for investment advisers include asset managers, investment counselors, investment managers,

¹²“Solely incidental” in laymen terms is meant to characterize those whose course of business contains content that could be construed as investment advice, but not for the purpose of giving such advice. Business school professors in finance, for instance, do not have to register as investment advisers if discussing efficient market theory or teaching CAPM.

¹³Six years prior to the Investment Advisers Act of 1940, Congress wrote into law the Securities Exchange Act of 1934, which defined a broker-dealer as “any person or company engaged in the business of buying and selling securities on behalf of its customers, for its own account (as dealer) or both.”

portfolio managers, and wealth managers. In this paper, we will term investment adviser firms as “RIAs” and the individuals employed as “investment advisers.”¹⁴

Regulatory oversight of RIAs is divided between the SEC and state regulators. Prior to 1996, the SEC oversaw RIAs managing more than \$100 million in AUM and state regulators oversaw RIAs with less than \$100 million in AUM. The National Securities Markets Improvement Act of 1996 (NSMIA) folded mid-size advisers (\$25-\$100 million AUM) into SEC jurisdiction, as part of a collection of efforts to integrate national securities regulations. Since NSMIA circumscribed state authority, state securities regulators have been aspiring to reclaim their oversight authority. Executive Director Russ Iuculano of the North American Securities Administrators Association (NASAA) stated, “The financial catastrophe of 2008 gave NASAA a great opportunity to make its case that our system of financial services regulation must be strengthened, but only through the combined efforts of state and federal regulators.”

The most recent shift in oversight responsibility resulted from the Dodd-Frank Act. Section 410 of the Dodd-Frank Act shifted oversight of mid-size advisers (\$25-\$100 million AUM) back to the states. While Dodd-Frank was signed into law in July 2010, the shift was not publicly announced until July 21, 2011, and went into effect by January 1, 2012. The primary motivation for this change was that Title IV of the Dodd-Frank Act repealed the “private adviser” exemption, which had exempted hedge funds, private equity firms, and venture capital firms from registering with the SEC. Raising the AUM threshold freed up SEC resources to monitor this new cohort. The Act did not shift oversight of RIAs in Wyoming and New York.¹⁵ Figure 1 depicts the timeline of the events.

[Figure 1 about here]

At the time of the law’s announcement, the SEC estimated that 3,200 RIAs would be delegated to examination by state law. As of 2011, the SEC reported that 3,512 RIAs had filed

¹⁴ RIA employees are also known as investment adviser representatives.

¹⁵RIAs in Minnesota were also not subject to the shift in oversight until 2011, when Minnesota decided to establish an RIA examination program.

with between \$25 million and \$90 million in regulatory AUM, and about 300 of these RIAs would be exempt for other reasons, such as being foreign.

Because investment advisers often help implement their advice, almost 90% of investment advisers are also registered as broker-dealers as of 2012 and under the oversight of FINRA. Oversight by FINRA did not change as a result of the Dodd-Frank Act. Brokers are not held to a fiduciary standard but rather to a “suitability” standard of conduct.¹⁶ Another key difference between brokers and investment advisers is that brokers typically receive commissions and product fees while investment advisers earn fees based on AUM. These differences in compensation incentives may lead to conflicts of interest and thus outcomes that are not in a client’s best interests if clients do not understand when their adviser is acting as a broker or investment adviser.

2.2 Disclosure Requirements for Investment Advisers

Investment advisers and broker-dealers are generally required to maintain updates with regulators of any material events that regulators, clients, or employers may find relevant. Beyond regulatory events and customer complaints, required updates include disclosures of personal bankruptcy, civil suits, or liens on their personal assets. Once these disclosure events are made to regulators, the profiles of RIAs and investment advisers are updated in public databases.

Our workhorse variable is the number of complaints initiated by customers, regardless of their statuses, including complaints that are in progress, settled, denied, or withdrawn.¹⁷ We keep all complaints in order to test both changes in complaint activity and outcomes.¹⁸ More specifically, we are interested in whether client complaint activity differentially increased for

¹⁶The suitability standard requires a broker who recommends buying or selling a security to consider a client’s income and net worth, investment objectives, risk tolerance, and other security holdings. In 2017, for certain activities, the Department of Labor has raised the standard from suitability to fiduciary duty.

¹⁷Customer complaints in BrokerCheck may undergo a variety of status updates. Complaints that are executed fully are often arbitrated through FINRA’s arbitration process or processed by some other formal procedure.

¹⁸This variable is different from the measure of misconduct used in Egan et al. (2017a), who consider the category, “Employment Separation After Allegations” to be part of misconduct, and ignore the category “Customer Dispute - Denied,” as well as other customer disputes. Their purpose is to identify misconduct of any kind, whereas ours is to identify misconduct specifically from the perspective of customers. To the extent our consideration of denied complaints could drive our results, in Section 5, we analyze the propensity for a complaint to be denied.

clients of RIAs that shifted from SEC to state oversight, relative to similar RIAs continuing under SEC oversight. Having found a significant increase in complaints, we then test for changes in the dollar value of alleged damages and the propensity for the case to be settled versus denied.

How costly is filing a complaint? Filing a complaint requires no fee, and complaints are filed electronically. Of course, clients do incur costs in other ways; carrying through with complaints takes time, can lead to alienating a relationship with an existing adviser, and reclaiming damages through arbitration or settlement requires processing fees, as well as potential legal costs. Figure 3 shows the electronic procedure for filing complaints for the state of New Jersey. Methods for other states are nearly identical.

[Figure 3]

3 Data & Methodology

3.1 Investment Adviser Data

Data for this study come from two sources: Form ADV filings and the Investment Adviser Public Disclosure (IAPD) database. Form ADV is filed annually or upon important business updates and indicates details of an RIA's registration - where the RIA operates, management, and basic details about the business operations. We filed a Freedom of Information Act request with the SEC to obtain the universe of SEC-registered Form ADV filings for the years 2000 to 2015.¹⁹ We identify firms that switched from SEC oversight to state oversight in 2012 using Form ADV-W filings, which indicate a partial de-registration filed with the SEC. A partial de-registration indicates the RIA is continuing to operate but under state oversight. A full de-registration indicates an RIA ceases to operate.

We also retrieve the disclosure histories of investment advisers from the IAPD, which the

¹⁹The full universe of Form ADV filings would consist of firms registered with state regulators and would require issuing FOIA requests to every state regulator, or purchasing from a vendor who has done the same. For this study, the full universe is not essential.

SEC maintains. The IAPD draws from the Central Registration Depository (CRD) database, which is maintained by FINRA.²⁰ We obtain these detailed disclosures from the IAPD using a web-scraper that takes a representative’s CRD number and queries the IAPD website. We query all CRD numbers between 1 and 10 million to get the universe of investment advisers.²¹ For each adviser, the IAPD stores all data, including complaints with alleged damages for at least 10 years. Preserved disclosures include customer complaints, criminal actions, regulatory actions, litigation, terminations, civil suits, and other financial matters, such as liens, that might be pertinent to an adviser’s ability to manage money. Often, the customer complaints will contain a product code. For example, 20% of complaints dealt with stocks and 4% with over-the-counter securities. The complaint data contain unstructured text detailing the nature of the allegation from the different participants (e.g., regulator, adviser, client), as well as docket identifiers to track legal proceedings related to a case.

We use the data to construct a person-year panel data set, containing the employing firm, all complaints received, and other individual characteristics. In each representative’s detailed reports from the IAPD, we see the representative’s full employment history at the branch level, including start and end dates.²² Similar to the prior literature, we assign a complaint to the year the complaint was officially received by the CRD. We drop about 7% of disclosures that do not include the date received. We also observe other individual characteristics, such as professional designations and exams passed (Series 66, Series 63, Series 65). For our analysis, we restrict the advisers to be ones employed at the end of 2014. About 12% of person-year observations available from 2009-2014 involve people leaving the industry by the end of 2014. The

²⁰Data for brokers is disseminated through BrokerCheck, which FINRA maintains. As noted in Egan et al. (2017a), BrokerCheck covers all brokers (around half of brokers are not investment advisers) and the vast majority of investment advisers. The difference between the two sources is that about 70,000 investment adviser representatives never registered as brokers and are thus not in FINRA’s database. In the original version of this paper, we used data from BrokerCheck and achieved quantitatively similar results.

²¹We use open-source software to extract details from the queried pdf for each adviser. We extract all fields and normalize across complaint types. The two difficulties are that the field names and data are hand-entered and therefore must be cleaned, and that the different disclosure types have different names for what is effectively the same field (date litigation filed, date complaint received). After this process, we extract the disclosure and assign a date to it.

²²This data are apparently maintained by hand. Sometimes branch locations are misspelled (e.g. ATALANTA, ATLNTA, representing ATLANTA). We normalize the names and remain conservative in grouping branches. For our analyses, this is conservative because an extra branch fixed effect would simply chip away from our point estimates of interest. The second unrelated data issue is that in less than 0.25% of individual years, advisers belong to more than two RIAs, owing evidently to cases where the RIA has multiple CRD numbers assigned. In these cases, we defer to the first method, which assigns individuals to one RIA at a point in time, or assigns the individual to the firm CRD most commonly seen in the sample.

vast majority of people who leave firms leave for reasons unrelated to misconduct. When we include these individuals for analysis, our results are quantitatively similar and more precise due to increased observations. Diagnostic tests suggest that advisers who leave do not seem to exit the sample more frequently whether they are treated or untreated, both unconditionally and conditional on a complaint. Thus we argue survivorship bias is not an issue.

Finally, we also hand-collect data on financial regulator budgets for each state. For all states, we are able to capture the budget back to 2009. However, the data is at different granularity. In some states, there is an identifiable division devoted to adviser enforcement. For some other states, the regulatory efforts are commingled with other functions, such as oversight of banks, insurance, or consumer financial products. That said, within the state, we later present graphical evidence on how state regulator budgets evolved, which one would assume may increase as their workload increased. However, these data are not suitable for a direct cross-sectional comparison given the data arises from different levels of granularity.

3.2 Methodology

We hypothesize that state regulators are less able to deter misconduct than the SEC. The SEC likely attracts higher-human-capital staff and has more experience auditing larger and more-complex RIAs. Alternatively, state regulators may have a local information advantage. However, because state regulators had not monitored mid-size advisers for the prior 15 years, the local-information advantage may be small.

To test the hypothesis, we use the following empirical specification:

$$1\{\text{Complaint}\} = \alpha + \beta_1 \text{Treated} + \beta_2 \text{Post}^{2012} + \beta_3 \text{Post}^{2012} \times \text{Treated} + \varepsilon_{it}$$

where the coefficient of interest is β_3 . The outcome variable is the probability of receiving a complaint (extensive margin). We also examine the effect on the log amount of alleged damages, awarded damages, and denial rates (intensive margin).²³ The specification uses

²³Alleged damages are not associated with every complaint. Additionally, the record-keeping agent may not properly populate the field.

a difference-in-differences design using a person-year panel for the period 2009 to 2014. In robustness, we find similar results using the collapsed-three-year-window approach of Bertrand et al. (2004) and using the annual window. We prefer the annual panel for the main specification because individuals may move across RIAs and we can better control for trends.²⁴ The main specification uses the person-year data set and includes RIA and year fixed effects. We cluster standard errors at the state level.²⁵

3.3 Identifying Treatment and Control Groups

We label RIAs as “treated” if their 2011 AUM is below \$100 million, the RIAs file a Form ADV-W indicating partial de-registration, and the RIAs are located in the affected states (all but New York and Wyoming).²⁶ The SEC projected around 3,200 RIAs would be affected in 2011. Since 2011, the bull market in equities increased the AUM of RIAs, so that in October 2012, the SEC stated that “over 2,300” RIAs made the switch.²⁷ We identify 2,316 treated RIAs, suggesting that we identify well the treated RIAs. Figure 2 shows a large increase in Form ADV filings in 2012. The increase in ADV-W filings drove the spike.

3.4 Summary Statistics

Table 1 displays the breakdown of observation counts for various subsets of the data. We start with 4.6 million person-year observations. We limit the sample to the time period 2009 to 2014. The resulting sample contains 1,791,522 person-year observations across SEC and state-registered advisers. We further limit the sample to RIAs that were SEC registered in 2011 - one year before the Dodd-Frank Act, which results in 1.29 million person-year observations.

[Table 1]

²⁴The collapsed-three-year window around 2012 may have more power as complaints may not be filed immediately when the misconduct occurs. Another advantage of the collapsed-window approach is that the serial correlation of residuals do not mislead inferences. Shortening the window to two years does not affect the inference.

²⁵We also cluster at the RIA level, and the significance does not change.

²⁶Full de-registration implies a business cessation or change of ownership.

²⁷See <https://www.law360.com/securities/articles/388275/sec-counts-1-500-fund-advisers-registered-under-dodd-frank>

Table 2 presents summary statistics at the RIA level. The number of treated RIAs and untreated RIAs is 2,316 and 3,910 respectively, which suggests the change influenced more than a third of RIAs under SEC oversight prior to the shift. Along several dimensions, treated and untreated RIAs are similar. Both types of RIAs are equally likely to report having investment discretion and proprietary conflicts of interest.²⁸ However, compared to non-treated RIAs, treated RIAs are less likely to have custody of assets, less subject to independent audits, less likely to be private funds, less likely to recommend an external broker, and more likely to serve individuals and unsophisticated individuals. These compositional differences in RIA characteristics justify including firm fixed effects in our specifications. In some specifications, we go further and include individual fixed effects. To further ease the concern that treated and untreated RIAs are different, we construct a matched-sample comparison designed to maximize the similarity between individuals and employing RIAs; we compare mid-size RIAs in treated and untreated states (NY and WY); and we restrict the sample to RIAs with assets close to the treatment threshold of \$100 million.

[Table 2]

Table 3 presents information on complaints before and after the Dodd-Frank Act. Panel A summarizes the nature of complaints. The sample probability of receiving a complaint is 1.25% for the full sample. Conditional on having alleged damages, the dollar amount of a complaint is significant with an average over \$200,000. Conditional on restitution, the amount of compensation is around \$200,000. The numbers on settlement and arbitration suggest about half of cases result in restitution. During the financial crisis, alleged damages were much higher.

[Table 3]

Panel B in Table 3 presents complaints by product and allegation type. In terms of product, complaints most frequently involve variable annuities, mutual funds, and equities. Advisers

²⁸Potential proprietary conflicts of interests arise when an RIA and client trading incentives may differ.

may play a bigger role in client investments in these higher-risk products. In terms of allegation types, the most common complaints are for misrepresentation and lack of suitability. The explicit word “fiduciary” is present in about 7% of complaints, suggesting the client is alleging a violation of a broad fiduciary standard. The fourth largest category is “unauthorized,” also at around 7%, suggesting the adviser made a trade the client did not authorize. Fewer complaints allege “churning” and “fees.”

4 Results

4.1 Complaint Rates and Dodd-Frank

Table 4 presents two sets of results. Columns 1 to 3 report the annual sample results. Column 1 reveals that the average annual complaint rate is 0.8% in our sample for the control group. In the post period, advisers working at mid-size RIAs that shifted to state oversight (Treated) experience an increase in complaint rates of 0.53 percentage points (60% of the average annual rate for the control group) in the post period relative to RIAs that remain under SEC oversight. Column 2 adds RIA and year fixed effects, and Column 3 additionally adds individual fixed effects. The magnitudes appear stable as the fixed effects are added, suggesting unconditional differences in investment advisers and RIAs cannot explain our result. Column 4 repeat the exercise using a two-period specification rather than an annual panel. Counting the complaints in the pre-period (2009 to 2011) and post-period (2012 to 2014), we find that the treatment effect again points to an increase of 70%, relative to the three-year complaint rate of 2.5% for the control group.

[Table 4]

One concern is that large-size RIAs (Control) may not be comparable to mid-size RIAs (Treated). For example, the summary data reveal that large-size RIAs engage in somewhat different business practices with different clienteles. Although RIA and individual fixed effects remove unconditional differences between the treatment and control groups, the treatment

and control groups may have different exposures to market conditions. Using annual person-year panels, Table 5 regression (3) compares untreated mid-size RIAs headquartered in New York and Wyoming with treated mid-size RIAs, and the results get stronger.²⁹ Another concern we address is that many RIAs are based in California and New York and may be different. In regressions (1) and (2), we exclude each state respectively and find no meaningful changes in our estimated effect. We also run a placebo test in regression (4) by specifying the treatment year as 2005 to have no overlap with our sample period of 2009 to 2014. The placebo shows no statistically significant results and the point estimate is negative.³⁰ Another way to ensure that we are comparing similar treated and untreated RIAs is to limit the sample to RIAs with similar AUMs. Appendix Table 3 presents the results. Regressions (2) to (8) limit the RIA sample based on 2011 AUM. The effect persists and is stronger when we limit the sample to RIAs that are more similar in size.³¹

[Table 5]

4.2 Parallel trends

Another concern for a difference-in-differences specification is a violation of the parallel-trends assumption, particularly because 2009, the beginning of our three-year panel, in fact coincides with the financial crisis. Visual inspection of parallel trends suggests a clear violation. We also construct a parallel-trends graph, Figure 4. We address this concern in three ways. First, we simply remove 2009 from our analysis, showing formally that this sub-sample does not violate parallel trends. Second, we simply match advisers based on their 2009 and 2010 complaint activity. We do not match on 2011 activity. The modified graph suggests no

²⁹The inferences are identical using a two-period panel. We also present a specification using branch*post fixed effects, which provides a fixed effect for every branch city by time period (treated, untreated) and our estimate remains large and reliable.

³⁰The 2003 to 2006 sample period is more than 10 years before we obtained the data. Because disclosures are maintained for a minimum of 10 years, there may be survivorship bias for this placebo test.

³¹We do not use an RDD because we do not observe the AUM in 2012. We often only observe RIAs' 2011 ADV AUM for RIAs that switch to state oversight and thus cannot observe the forcing variable required for an RDD. Also, the discontinuity is not precise because existing SEC-registered RIAs do not need to deregister unless assets fall under \$90 million but new RIAs have to be above \$100 million to register with the SEC.

violation in the parallel-trends assumption as the matched samples have identical trends in 2011 and divergent trends on treatment in 2012.³² Third, we pick firms who report no more than \$300 million in AUM. One qualitative concern is that larger firms may be more or less exposed to the financial crisis or other time-varying economic conditions than small firms. Therefore sub-sampling on size is likely to compare firms that are composed similarly.

We perform two variants of matched samples. For each investment adviser treated in 2012, we find a matching investment adviser. For the first set of matching, we match on four criteria: indicators for receiving a complaint in 2009 and 2010, the same total complaint count between 2009 and 2010, and working in the same state in 2012. The advisers are also matched on a propensity score of an RIA to be treated based on a vector of RIA characteristics. Note that we do not match on 2011 behavior - the year prior to the treatment year. The matched sample is done with replacement and standard errors are clustered at the match pair, with four matches.³³ The second matching scheme relaxes the constraint that advisers have the exact same number of complaints in 2009 and 2010, but instead that they have the same 2009-2011 complaint count.

We present our tests using these three sets of sub-samples in Table 6. Column 1 shows that the treatment effect when we include is still positive and reliable, and about 80% as large as when including 2009. Column 2 shows that including a dummy the year before for treatment does not load, which suggests that parallel trends is not violated. The treatment effect enlarges to its original magnitude. We repeat this analysis on the 300MM sub-sample. Column 3 shows that on the \$300 million sub-sample, the estimate of the treatment effect is positive and reliable. Column 4 shows that this sub-sample does not statistically violate parallel trends either. Columns 5 and 6 show the treatment effect under matched samples. The result also holds, in regression (5), when we require exact matches on year-by-year complaint counts for 2009 and 2010. Column 6 is different from Column 5 in that we are not exact matching on the 2009 and 2010 trend but rather simply matching on the 2009-2011 complaint count. The magnitudes in

³²In addition to the two main matching algorithms, we also considered other variants: using the same number of complaints total in history (instead of the pre-period), and forcing the same complaint *count* every year in the pre-period. All variants produce similar results that are sometimes stronger. Other restrictions such as forcing zero complaints in the pre-period provide a directionally similar estimate. We also tried matching without replacement, achieving very similar magnitudes.

³³This follows the recommendation of Abadie and Speiss (2016) for clustering adjustments when control observations are drawn with replacement.

both of these specifications are reliable and similar to the original treatment effect. These analyses suggest that parallel trends do not contaminate our inferences. We present two samples that do not formally fail the parallel trends test, and two matched samples that by construction are designed to compare two groups with identical pre-trends. Thus far, however, we argue that we have a robust finding: switching to state regulation resulted in a larger number of complaints for affected investment advisers. Moreover, in upcoming analysis, we present a cross-sectional test interacting treatment with pre-period complaints. Rather than being contaminated by the potential pre-trend, we explicitly take it into account as a source of variation - under which our inference obtains. Second, we present cross-sectional evidence. Any concern about parallel trends must be also simultaneously explain the cross-sectional evidence, which places many restrictions on the alternative hypothesis and renders the alternative of some abstract, time-varying omitted variable significantly less plausible.

[Table 6]

4.3 Which Types of Complaints Increased?

Table 7 shows that the increase in complaints for treated investment advisers increasingly dealt with options, equities, and real estate. Equities and options are risky, complex, and informationally sensitive assets that are likelier to require the regular involvement of the investment adviser. We see null results for private placements, which are primarily products used by sophisticated clients, who are more capable of monitoring their advisers. There also is no evidence of an increase in complaints related to annuity products, which may be because annuities are buy-and-hold products with a long-term horizon. We do not see an increase in churn- or fee-related complaints as a result of treatment. One potential interpretation is that churn- and fee-related complaints are commonly broker-related complaints and oversight of brokers by FINRA did not change as a result of the Dodd-Frank Act. In contrast, fiduciary and unauthorized trading activity differentially increased after treatment. However, there are also very few churning and fee related complaints in our sample.

[Table 7]

We next examine changes in the rates of four other types of disclosures that are not customer complaints. Table 8 presents our results. Regulatory actions exhibit an increase for treated RIAs, but the magnitude is small and statistically unreliable. A relative increase in regulatory actions for the treated is likely due to required re-registration. Financial disclosures, criminal disclosures, and terminations (which could be initiated by the RIA or regulator) similarly are insignificant. The results suggest that treatment is not associated with these other types of disclosures. This provides additional suggestive evidence that regulator involvement does not explain our finding.

[Table 8]

5 Cross-sectional evidence

There are many reasons why complaints may have increased. This section devotes itself to cross-sectional evidence used to understand what interpretation it may be.

There are three branches of reasons why complaints may have increased. Our preferred interpretation is that investment advisers changed their behavior because actual misconduct increased, which suggests regulators became weaker. Second is that clients changed their behavior to strategically complain more. Whether or not regulators were in fact tougher, clients believed they would be more effective operating under the new regulator. Thirdly, regulators may have changed their behavior and become tougher.

It is important to also note the difference between misconduct that is reported and misconduct in reality. Consider the following decomposition:

$$Complaint = \Pr(Detection) \times Misconduct + \Pr(False\ Positive) \times Normal\ Activity.$$

We might observe an increase in complaints for the treated group if the treated advisers misbehave more (*Misconduct* increases). Alternatively, we might observe an increase in complaints by clients if clients increase the probability of detection ($\Pr(\textit{Detection})$ increases). Perhaps, clients view state regulators as being more concerned with local misconduct or more capable than a national regulator, or because the regulator plays a more active role. Also, we might observe an increase in complaints if clients are more likely to file frivolous complaints ($\Pr(\textit{False Positive})$ increases). Forestalling judgement, in this section we present several cross-sectional tests. We collate the evidence in 6 to identify our preferred interpretation.

5.1 Did Frivolous Reporting Increase?

Whether regulators became stronger or weaker, it is necessary to know whether the increased incidence of complaints was an increase in frivolous allegations. Frivolous reporting may increase for a number of reasons. First, clients may perceive the cost of filing complaints with state regulators to be less expensive than filing with the SEC. There is no evidence that the actual cost of filing a complaint online changed, although a physical visit to the local office is less expensive. Second, clients may be opportunistic if clients perceive the state regulator to be more cooperative. To test for frivolous reporting, we examine whether the nature of complaints and the probability of a favorable award for a given severity changed with treatment. We extract three sets of fields: the dollar amount of alleged damages, the amount of monetary compensation (represented in the two fields settlement amount and monetary compensation), and whether the allegation was denied, withdrawn, settled outside of arbitration, or awarded in arbitration.

We first examine whether the severity of complaints increased using the alleged dollars of damages measure. On the one hand, if clients are complaining more, clients may be more willing to complain about smaller misconduct, represented by smaller dollars of alleged damages. On the other hand, if clients are complaining more and the dollars of alleged damages also increase, then misconduct may have also become severe. Alternatively, clients may be filing larger but more frivolous complaints, which should result in higher alleged damages but lower settlement awards. Table 9 regressions (1) to (4) show a positive effect of the shift on

the dollars of alleged damages, with magnitudes suggesting a 20-66% increase in alleged damages; however, the point estimates are not statistically significant.³⁴ Since all the coefficients are positive, there is no evidence in support of the first line of reasoning. Regarding the settlement rates, Table 9 Panel A shows that the log award and the probability of receiving an award significantly increases for cases involving advisers at the treated RIAs. This effect is robust to controlling for first, second, and third-degree polynomials of the log amount of alleged dollar damages, as the amount of award, or even the probability of getting one, should increase in the magnitude of the allegation. This result does support the latter reasoning that complaints both increased and became more severe.³⁵

We next examine whether the probability of a complaint being denied changes for complaints against treated RIAs. If complaints are more frivolous, we would expect the probability of being denied to increase. Instead, Table 9 Panel B shows that the probability of being denied decreases for complaints against treated RIAs.

Overall, the results suggest complaints against treated RIAs increased in count, alleged damages, settlement amounts, and probability of award. Taken together, these three pieces of evidence do not suggest the more frequent complaints were frivolous. If anything, complaints became more *severe*.

[Table 9]

[Table 9]

³⁴After inspecting the data closely, we observe that 1/4th of cases report alleged damages as zero. These fall into two cases: when the customer was awarded a settlement, and a case was denied as generally lacking merit or the case is no longer reportable. The presented results assume use the full data, except it edits the alleged damages to be at least the settlement amount when a non-zero settlement is reported and keeping alleged damages as zero when the case is denied. Another sampling choice is to completely remove these cases, under which our inference that alleged damages did not seem to fall is unchanged. A final sampling choice we made was to aggregate alleged damages by person-year as a handful of financial advisers receive many complaints in the same year. In this case, we can obtain more precise estimates that are significant under some specifications.

³⁵This analysis experiences a drop in sample size because unlike in Table 9, we do not want to control for alleged damages in relation to settlement when we cannot observe the alleged damages. However, including these cases are immaterial to our inference.

5.2 Under-staffed State Regulators

We next investigate the relation between staffing of the state regulator and the treatment effect. Figure 5 shows that although state regulators' workload increased significantly, on average states did not increase regulators' budgets. This suggests that despite the workload increase, state regulatory agencies could not, or neglected to, obtain resources to deal with an increased regulatory burden and the heightened complexity of mid-size advisers. To better identify the impact of regulator budgets on observed misconduct, we study heterogeneous treatment effects across states with different funding.

[Figure 5]

Our main measure of state-regulator resources is staff-per-adviser. To measure staffing, we use a report compiled in 1999 by the American Association of Retired Persons on the regulatory differences for investment advisers in every state.³⁶ We argue the staff-per-adviser devoted to adviser regulation in 1999 is likely correlated with the current level of regulatory oversight. Because it is predetermined, it is not contaminated by reverse causality.³⁷ In some states, the number of staff-per-adviser is not available, attributable to the fact that the regulator is *not* devoted to adviser regulation only, but financial-services regulation overall.

Table 10 shows that states with higher staff-per-adviser saw a smaller treatment response, consistent with more staff at securities regulators deterring more misbehavior. For the purpose of using all available data, the majority of specifications impute zero staff when the regulatory body has a missing value, although our quantitative magnitudes are not sensitive at all to this assumption. Columns 1 and 2 report full-sample analysis with firm and firm and rep fixed effects, reporting near identical magnitudes. Adding state-year fixed effects reduces the magnitude by about 12% to .390 but it remains very reliably estimated, comparing only within-firm changes in the same state at the same time. Thus no economic condition of the state can explain our results unless it is correlated with treatment and time-varying.

³⁶The American Association of Retired Persons has been long involved with consumer advocacy in the financial adviser industry. In addition to producing several reports for public consumption on the financial adviser industry, the AARP has also participated in regulatory discussions through several comment letters.

³⁷As part of this project, we tried surveying regulators today for the same numbers, but obtaining pre-2012 numbers was extremely difficult.

The remainder specifications reveal that magnitudes are similar when considering sub-300 million dollar firms or a matched sample or removing 2009. In fact it appears that when we remove 2009 (a year which challenges our parallel trends assumption) the result actually improves in economic magnitude by 8% to -0.485 to -0.493 considering state-year fixed effects. The standard deviation of the variable is 0.24. Counteracting the treatment effect therefore requires a two-standard-deviations increase above the mean of staff-per-RIA. The result on firms below \$300 million or matched firms suggests it is not likely that differential economic exposures of certain types of individuals or large firms that explains our results.

[Table 10]

5.3 Distance to Regulator and Misconduct

This next section examines complaint rates against the firm or adviser branch's distance to the relevant local regulator. Prior research suggests that travel distance from a governing body to a regulated entity affects the quality of monitoring. Giroud (2013) finds that plane routes affect intra-firm monitoring. It is not just businesses but also regulators who are constrained by physical distance. Nguyen and Nguyen (2017) find that firms increase fraud when further away from a branch of the SEC; closures of these offices appear to increase misconduct by increasing the distance to the closest regulator. In our context, distance may matter because RIAs located farther away require more commute time and other costs to monitor, the regulators have less soft information about more distant RIAs, and the regulators may be less salient to more distant RIAs. Thus, investment advisers located farther away from the regulator may take more advantage of the regulator's weakness. Anecdotal evidence we have collected concurs with this assessment.

To measure distance, we use the longitude and latitude of the zip code of the branch where the investment adviser worked and the location of the adviser's branch, RIA headquarters, nearest SEC office, and nearest FINRA office. Distances are measured using zip-code coordinates from the 2013 Census, although we try other geocoding measures based on Google Maps

and the Bing Maps API achieving similar conclusions quantitatively and qualitatively.³⁸The distances we consider are distances of the branch and distances of the headquarters. Although it may be intuitive we should use the physical location of the representative, we present a greater number of specifications focusing on the location of the firm for two reasons. First, branch cities are self-reported, leading to near-comprehensive but incomplete coverage. Secondly, a branch name is not as precise as the zip code the firm reports, thus requiring us to impute the location. We explain this more in Section 5.4. Second, regulators might be more likely to visit firm headquarters rather than branches. FINRA, for example, does not currently perform branch visits and is reportedly considering doing so.³⁹ However, each regulator may be different in its business practices. These considerations necessitate that we present both sets of results.

[Table 11]

Table 11 shows complaint rates increased more for mid-size RIAs located farther from their appropriate state regulator. Regression (1) shows a positive and significant coefficient on the interaction of treatment and distance using RIA and year fixed effects. An adviser twice as far from a regulator as another adviser has a 0.2-percentage-points-higher probability of receiving a complaint. Column 2 adds state-year fixed effects to account for within-state-time variation. Our estimate improves in magnitude and precision to 0.232. This is sensible because cross-state variation in urban density may confound the estimate of distance. Column 3 shows similar precision and magnitude constraining firms to firms that are small, in case there is a concern that firms of dissimilar size may be dissimilar especially with respect to how their adviser representatives are located. Column 4 adds a great deal of fixed effects showing that controlling for the local economic conditions of where the adviser is located, the firm is located, and distance to the SEC, distance to the relevant state regulator is highly positively related to increased complaint rates. Meanwhile, distance from the SEC does not explain complaint rates.

³⁸The distance measurements are quite similar and results are directionally and quantitatively similar.

³⁹Please see this link, collected October 2017.

One potential concern with the above analysis is that the full-sample does not satisfy the assumption of parallel trends. Columns 5 and 6 show that removing the year 2009 preserves similar economic magnitudes. In Column 5 the estimate actually increases above that of Column 2 by 30% to 0.325, looking within firm and within state-year. Column 6 presents the equivalent of Column 4, showing a slightly reduced magnitude of 0.243 down from 0.281, but with a slightly larger standard error that it just barely misses the 10% significance threshold. The estimate is reliable many specifications that are marginally less stringent, the quantitative magnitude remains robust, and the number of fixed effects in this specification is large. Therefore, we do not feel missing the 10% threshold marginally is surprising or problematic for our inference.

In fact, using this same sample, we next consider what happens when we look inside the firm-year across branches. It appears that within *firm-year*, controlling for *branch-by-year fixed effects*, the distance from the regulator still appears to matter. Column 7 demonstrates that it matters even controlling for distance from the SEC. The estimates are significant at the 5% level. The within-firm-year fixed effects suggests that no matter what the characteristics of the firm are or its differential exposure to economic conditions, the physical distance to the regulator is relevant for complaint rates. The within-branch-year fixed effects rules out local economic conditions, which might be correlated with distance from a firm headquarters, are what explain our results.

5.4 Demographic Composition and Treatment Response

In this analysis, we analyze whether treatment is related to the sophistication of clients. We do not observe clients directly nor their sophistication, but we proxy for it using the demographic composition of the county in which the adviser rep serves (e.g. their branch-level county). We measure client sophistication in two ways: the fraction of the county that is over 60 and the fraction with a bachelor's degree. We assume that client sophistication is decreasing in both of these quantities, as in Egan et al. (2017a).

We first obtain the branch location (city level) in which the individual adviser works and

assign the adviser to a county based on the city name.⁴⁰ After doing so, we obtain county-level characteristics from various government sources. We use the 2011-2015 American Community Survey (ACS) as reported by the U.S. Census Bureau. To ensure look-ahead bias is not contributing to our results due to the 2011-2015 sample overlapping the treatment in 2012, we also use the one-year ACS from 2012 as a robustness check (unreported) and find very similar results.

Table 12 reports our results. Columns 1 and 2 interact the treatment effect with county-level college degree attainment using the full sample. It shows a standard-deviation increase in the county-level percentage of adults without college degrees increases treatment by 18.7 basis points, which is a 33% greater effect than the effect on the mean mid-size RIAs. Column 2 adds a branch fixed effect. Controlling for county-level unconditional variation *increases* the point estimate to 19.4 basis points. Column 3 and 4 reports results based on removing 2009. The point estimate on the interaction with education remains negative but is no longer significant, while the relation with elderly remains significant. Using this same sample, we look within branch-year and within-firm in Columns 5 and 6. In Column 5 we find the same null result regarding treatment and the average population education. However, in Column 6 we find even stronger results for the treatment effect and its relation to elderly clients in Column 6. Column 7 and 8 repeat the analysis for the small, sub \$300 million sample. Columns 9 and 10 repeat the within-firm-year within branch-year analysis for the matched sample, resulting in similar conclusions.

Overall, we interpret this evidence as suggesting the treatment effect increased the most among less sophisticated clients, particularly among the elderly and less reliably among the un-educated. On the one hand, if clients perceive that state regulators are more cooperative than a national regulator, then we would expect more sophisticated clients to be more aware of the shift in regulatory oversight and more likely to increase complaints. On the other hand, if state regulators are weaker than the SEC, then we would expect investment advisers serving

⁴⁰After cleaning branch-city names for misspellings, we assign the city name to all relevant zip codes. Where a branch-name could correspond to multiple counties, we conservatively assign the adviser representative to the largest county. The vast majority of adviser representatives report a branch. However, some observations are lost due to a lack of data, or a branch-city location that can not be disambiguated. In some cases, the adviser representative reports a branch location that is a state or an incomplete city name that does not correspond to a identifiable county.

less sophisticated clients to increase misconduct more, because more sophisticated clients are better at monitoring advisers.

5.5 Client Composition and Treatment Response

We also examine how the treatment correlates with differences in RIA-reported client composition in the annual Form ADV. Individual investment advisers are not required to report their client compositions; only the firm (RIA) reports. Thus, we no longer have branch-level variation in client composition. The Form ADV provides the proportion of clients that are accredited (sophisticated individuals), un-accredited (unsophisticated individuals), government agencies, institutional investors, and private funds. Table 13 regression (5) shows that complaint rates against mid-size RIAs increased less in response to Dodd-Frank for mid-size advisers serving more institutional investors and government agencies. These clients are likely capable of monitoring investment advisers, and RIAs serving these clients may serve more sophisticated individuals in general.

[Table 12]

Columns 6, 7, and 8 present various robustness checks. Columns 6 and 7 use the full-sample adding additional fixed effects, at the individual and state-year level, revealing similar conclusions. Column 8 reproduces the state-year specification removing 2009, achieving similar economic magnitudes.

[Table 13]

5.6 Recidivism

Egan et al. (2017a) document recidivism in the sense that adviser reps who receive complaints are often time repeat offenders. We investigate if the repeat offense is amplified by treatment, using the following cross-sectional specification.

$$Complaint_i = \alpha + \beta_1 Past_i^{Complaint} + \beta_2 Treated + \beta_3 Past_i^{Complaint} \times Treated + \varepsilon_{it}.$$

The variable $Past_i^{Complaint}$ is the number of complaints an investment adviser received from 2009 to 2011, which reflects the extent an adviser misbehaves prior to the treatment. Alternatively, we measure past misconduct using the log number of complaints $\log(1 + \#Complaints)$ and the number of complaints adjusted for characteristics of the RIA, $\epsilon_i^{Complaints}$. Table 14 reports the results.⁴¹

[Table 14]

Regression (1) in Table 14 shows investment advisers that had a complaint during 2009 to 2011 (Pre-Treatment) were 9.4% more likely to have a complaint in 2012 to 2014 (Post-Treatment). The unconditional recidivism we observe is in line with that documented by Egan et al. (2017a). For investment advisers at RIAs that switched to state regulation from SEC oversight, the probability of recidivism during 2012 to 2014 increased 7.2% more, a 77% increase over the control group. In regression (2), we add RIA fixed effects and find very similar results, suggesting investment advisers that misbehaved more at a specific RIA also responded more to the treatment than other advisers at the same RIA. In regression (3), we limit the sample to only mid-size RIAs to compare treated mid-size RIAs with untreated mid-size RIAs located abroad, in Wyoming, and in New York. We find directionally similar results and the treatment response is stronger. Regressions (5) and (6) do tests using the log number of complaints in the past and future and show that representatives that misbehaved relatively more in the past that were also treated received relatively more complaints than similar advisers that were not treated. Regressions (7) to (9) show a similar result using complaint rates adjusted for RIA and state characteristics.

The data support two possible interpretations. On one hand, it is possible clients are simply amplifying their complaints knowing they may be able to recover more losses under a new regulator even if the adviser rep did not change his or her behavior. However, it does also

⁴¹The benchmark is a number of Form ADV characteristics indicating conflicts of interest and AUM, plus state fixed effects. The residual comprises excess complaint variation not attributable to the RIA or state.

support our preferred interpretation. Under the misconduct hypothesis, reps are increasing their bad behavior - when the cats go away the mice come out to play. At the very least, it would contradict our hypothesis if we did not find this result. One final benefit of running this test is that as a cross-sectional test, any parallel trends concern does not apply, beyond the fact we are already modeling it. Yet the test suggests complaint rates moved differentially with treatment.

5.7 Misconduct Rates of Small-Size Advisers (<\$25M AUM)

The Dodd-Frank did not shift oversight of small-size RIAs (less than \$25M AUM), which remained under the oversight of state regulators. On the one hand, clients of small-size RIAs may complain more because state regulators are more concerned with local misconduct than a national regulator. This logic suggests that when the Dodd-Frank expands the oversight responsibility of state regulators to include mid-size RIAs, clients of small-size RIAs should complain less. On the other hand, if state regulators simply become more burdened, then misconduct by advisers may increase, resulting in more complaints. Table 15 regression (2) shows that small-size RIAs saw a significant increase in complaint rates of 0.35 percentage points. The effect is robust to specifications using individual and RIA fixed effects and matched samples, where the match was again done on the pre-trend years of 2009 and 2010 but not 2011.⁴² The magnitude of the increase in complaint rates by always-state-registered advisers appears to be smaller than that of treated mid-size advisers. The evidence overall supports the latter logic that state regulators became more burdened as a result of Dodd-Frank.

[Table 15]

5.8 Regulatory Action Conditional on a Complaint

We next investigate the relation between regulatory action and a complaint. We investigate the interaction between treatment and receiving a complaint and its relation to regulatory action

⁴²We do not observe the ADV data for the never-SEC-registered RIAs, so we cannot do propensity score matching on which RIA characteristics affect the probability of being treated. Instead we do nearest-neighbor matching on historical complaint count and number of years as an adviser representative.

as an outcome. We regress regulatory actions and complaints contemporaneously. To wit, regulatory action may not be initiated synchronously with an observed customer complaint, but we have no reason to believe regulatory action should precede or follow a complaint. The regressions are of the form:

$$1\{regulatoryAction_{ijt}\} = \alpha_{firm} + \alpha_t + \beta_1 treated * post * 1\{complaint_{ijt}\} + cross - terms + \epsilon_{ijt}$$

Table 8 reports the results. Interestingly, the relation between complaints and regulatory action is positive unconditionally. Where there is a complaint, regulatory action is more likely. This relation appears to increase in the post-period. That is likely due to regulatory capacity being constrained during 2009, when overall complaint rates were higher on the tail end of the financial crisis than after. Turning to the coefficient of interest, $treated * post * 1\{complaint_{ijt}\}$, we find a negative relation between regulatory action and complaints for the treated firms. Column 1 suggests that conditional on a complaint, treated firms are 10% less likely to receive a complaint, an estimate that is significant at the 10% level. Column 2 removes 2009 and the magnitude becomes remains reliable. Column 3 displays the sub \$300 million sample removing the year 2009. Column 4 displays the sub-300 million sample using all six years. In three of four columns, the estimate on β_1 remains negative and reliable at the 10% level, the exception being the full annual sample which we view as the least preferred. Column 5 presents a within-firm-year analysis using the annual sample removing the year 2009. It is almost significant at the 10% level with a t-statistic of 1.644, daring to suggest within the same firm at the same point in time, state regulators are less likely to punish the firm, but only among the treated firms.

[Table 8]

Although the overall body of results is not significant in every specification, this is likely due to the fact regulatory actions per se did not increase very reliably as a result of treatment. Regulatory actions are, relative to treatment, quite rare. There is less variation to explain.

However, the cross-sectional relation to complaints if anything has fallen under treatment. Although many possible interpretations could be applied, it does support the claim that state regulators are either slower to respond or overall less likely to take action conditional on a complaint. One other interpretation might be that regulators are no less likely to respond to misconduct, it is just that these complaints we observe under treatment were frivolous. However, this does not square with the evidence we showed earlier regarding complaint outcomes. If anything, complaints became less frivolous.

6 Discussion of interpretations

There are many possible interpretations to our finding that complaint rates increase. We list the leading candidate explanations and argue the cross-sectional tests distinguish our preferred interpretation in the data - that advisers increase their actual, and not just reported misconduct, due to decreased regulatory deterrence under state jurisdiction. Broadly, the two categories of alternatives fall into two types: customers change their behavior or regulators change their behavior. Finally, way, we are careful to distinguish our results from Agarwal, Lucca, Seru and Trebbi (2014). We conclude this section with ancillary robustness checks.

6.1 Clients may have strategically changed their behavior?

First we consider alternatives based on shifts in client behavior. One possible explanation for our main finding is the *advertising effect hypothesis* or the *forum shopping hypothesis*. Under this hypothesis, the transition to state regulators resulted in announcements to clients of a change in regulatory jurisdiction. With state regulators salient, clients might strategically file complaints because they believed they could achieve redress more effectively under a new regulator. In our view, this could explain complaint rates, but not our cross-sectional evidence. Firstly we could expect clients to where regulators have more resources to service them, which contradicts our finding that complaint rates increased less where state budgets were higher (Table 5). Second, regulators should be more salient when closer to the state regulator, which runs counter to what we find in Table 11. Third, we might expect more sophisticated investors to be aware of the change, and our evidence on branch-level demographics on education and age

lean against this. Fourth, it is not clear investors should rationally expect this as treated firms are less likely to see regulatory action in response to a complaint (Table 8). Fifth, any such effect of regulator salience should be temporary, while our parallel trends graphs indicate the opposite.

Another possibility is the *concerned client hypothesis*. If clients were concerned state regulators would be weak, then it is possible clients complain more, particularly when state budgets are weak or clients are further away from their regulator. This is in our view implausible. However, the evidence allows us to dispel this hypothesis directly. This would suggest that the complaints are frivolous. Second, this would require sophistication on the part of clients. Yet our evidence shows the complaints contained more signal, if anything, in the treatment period, and that the adviser reps whose propensity to receive complaints increased dramatically served less sophisticated clients.

6.2 Are we observing a change among regulators?

We next turn to regulator-driven explanations. Our main alternative is the *diligent regulator hypothesis*. Under this hypothesis, the regulator is not the weaker regulator but actually the stronger one. State regulators serve three functions. They perform annual and surprise audits, assist clients when they issue complaints, decide regulatory standards and educate investors and firms under their jurisdiction. A stronger regulator may raise standards against which clients may seek redress or their audits may uncover issues that inform clients. Although the outcome of the complaint is ultimately decided by most often an arbitration panel in front of FINRA the regulator's role in discovery or standard-setting may be instrumental to the outcome.

We find this view to be inconsistent with several pieces of evidence. First, our evidence that advisers in branches or firms headquartered further away from the relevant regulator is incongruent with the view that regulators drive the complaints. Regulatory involvement is constrained physically and therefore their involvement should be greatest closest to their headquarters, but it is the opposite in the data. Second, again, the change in complaint rates seems greatest where state budgets were the smallest per firm, per Table 11. Third, it seems less

likely that unsophisticated clients would take advantage of the stronger local regulator, which is what we find with sorts on firm client composition as in Table 5.4 or branch-demographic composition in Table 12. Of course, regulators may target advertising in places where clients may be relatively less sophisticated or educated. Fourth, while there is some weak evidence that treated advisers receive more regulatory action, with a stronger regulator, we would expect these regulators to issue more regulatory actions where complaints occur. However, treated firms receive less regulatory actions conditional on a complaint, suggesting state regulators are less responsive (Table 8).

The second regulator-driven alternative hypothesis that we consider is the *concavity hypothesis*. Under this narrative, the SEC-jurisdictioned firms are affected in some sense in that the SEC off-loaded firms to the states. By concavity, state regulators became worse and the SEC became better and more attentive to the firms under its purview. Thus we capture not only the weakness of state regulators but the strength of SEC regulators. We view this interpretation as interesting in its own right, but of course it would complicate our interpretation *prima facie*.

However, it is worth noting a few institutional details, which together serve to mitigate or dispel the *concavity hypothesis*. First, even if the SEC became more effective as a result of a decreased workload, this would not explain the cross-sectional variation across state regulators. That is, state regulator weakening must be at least part of the story. In addition, we show that some of the cross-sectional variation can explain the entire magnitude of the treatment effect, suggesting that the role of the SEC strengthening might be relatively small, if it is the case at all. Using Column 1 from Table 11, the results on distance suggest that for a standard deviation movement in log distance from the state regulator, the treatment effect is .334. In addition, the treatment effect for a firm the mean effect for an average firm is roughly .6, in line with the size of the unconditional treatment effect.

Secondly, the SEC intended to off-load mid-size advisers because it reversed a prior regulatory change and was thus a convenient line to re-draw, but it also meant that 3,200 advisers in expectation would be off-loaded at the time this jurisdictional change was conceived. This was almost one-for-one with the number of hedge funds and private equity firms it expected to later regulate. Presumably, these firms are more difficult to regulate. Thus, it is not clear on-

net the SEC actually reduced its workload. Further, although the SEC may have anticipated resources toward these firms, the SEC actually did not off-load as many advisers as it expected to. This is because after the June 2011 announcement, in late 2011 and in early 2012 the US equity market rebounded likely affecting adviser AUM either through inflows or asset values, resulting in 900 fewer than expected jurisdictional changes.

This body of evidence suggests that the concavity hypothesis may not bear out with the reality that the SEC was merely trying to offset its workload through delegation, and if anything likely under-compensated. Thus, in our view, the SEC may have if anything received a greater burden as a result of this regulatory change, on balance. Even if not, the cross-sectional evidence suggests that at least a large part of our observed quantitative magnitudes must be regulatory behavior of state regulators, and not the SEC.

6.3 Detection versus misconduct?

Finally, one possible concern with our analysis is the difference between *detection rates versus true misconduct*. Complaints are detected misconduct. Although true misconduct cannot clearly be observed, we believe our evidence *suggests* actual misconduct has actually increased. Our principal argument rests on the idea two arguments, firstly that conditional on observed misconduct, the success rate of such a complaint has not dropped. Secondly, observed complaints increased where detection rates were most likely to decrease, and thus actual misconduct must have increased if detection rates have strictly fallen on average.

Regarding the first point, we rely on our arbitration analysis. We find that alleged damages increase without a decrease in signal in that the settlement ratios are higher if anything and the denial rates are lower. It is important to note again that FINRA is a third party to the regulatory change we studied. Since FINRA arbitrates complaints, and is not a governmental body directly affected by Dodd-Frank (at least with respect to investment advisers). Thus, we argue the outcome of arbitrations is a signal that the complaints if anything became more severe in that the severity of the allegations increased yet the noise-to-signal ratio of a complaint did not increase.

Second, we observe that complaint rates increase where detection rates likely became weakly

worse. To make this point, we refer back to our cross-sectional evidence on distance to regulator in Table 11, our evidence on less sophisticated clients complaining in Table 12, and our evidence that states with lower budgets in Table 5. The response is evidently rational in two ways: prior bad actors respond differentially (Table 14), and severe regulatory action appears less likely when a customer files a complaint against an adviser rep.

In light of these arguments, and having excluded the two main alternatives, we believe the preferred interpretation of the result that complaint rates increased is that state regulators are viewed as less effective, resulting in increased misconduct rates.

6.4 Differences with Agarwal, Lucca, Seru, Trebbi (2014)

Our paper is superficially similar to Agarwal et al (2014). They find inconsistent and generally “softer” regulation when state regulators hold jurisdiction, which is similar to our principal finding. They attribute the observed behavior primarily to the concern state regulators have because banks drive the local economy. They do not attribute to corruption. There is some evidence regulator human capital and financial budget do make a difference, but secondarily to their preferred interpretation local regulators are concerned about the local economy. However, our mid-size investors first operate in delegated markets and likely their clientele is too small to have an economic footprint that they would be deemed systemically important. Secondly, advisers primarily deal with delegated asset markets which may have zero local footprint. In our untabulated analysis, there is very little correlation between local economic conditions and the treatment effect in a triple difference analysis, suggesting indeed that the role of local considerations is small. Therefore, our main result is more likely due to financial resources and lower regulator quality.

6.5 Robustness checks

The first concern is a potential delay in when the complaint was received after the misconduct. That is, if complaints in 2012 redress prior misconduct, then the increased rise in complaints could reflect opportunistic complaints by customers of re-jurisdictioned mid-size advisers. Figure 4 is therefore helpful in dispelling this one potential alternative. The gap between treated

and untreated appears to persist through 2014, mitigating the alternative that complaints merely reflect a backlog. Cross-sectional evidence also casts doubt on this alternative. For this alternative, it would suggest that customers saw states have immediate capacity to handle complaints. However, we would raise three counter-arguments. First, the states with lower staff-per-RIA had higher complaints, whereas states with capacity would likely accommodate any queue more effectively. Second, firms closer to the regulator saw fewer complaints, whereas these firms would likely be the firms most sought after by the regulator. Third, the less sophisticated investors are the least likely to be aware of regulator capacity, yet as clients of treated firms were the likeliest to complain.

A second alternative is that either the SEC has a more lax or a state regulator has a more stringent reporting standard for complaints. However, we believe this is not an issue for numerous reasons.⁴³ We provide a lengthy discussion in A.3.

A third concern is that the control group may be changing because the SEC becomes busier overseeing hedge funds and private equity firms. We therefore see a drop in complaints for SEC firms, not a rise at the state level. However, this would not explain the results exploiting cross-sectional variation among the re-jurisdictioned mid-size advisers. It would also be inconsistent with results on sub-midsize advisers. If only the SEC changed behavior, then the within-person, within-RIA change in complaint rates would not vary between the mid-size and small-size advisers.

A fourth concern is that some firms may re-register with the SEC. To check this does not influence our results, we define treatment as de-registering partially from the SEC with the requisite AUM, and *never registering* by the end of 2015. When we do this, the results remain similar.

A fifth concern is that our unit of aggregation - at the individual level - is problematic.

⁴³Several regulators we talked to confirm disclosure reporting is handled by FINRA, not by the relevant SEC or state regulator. This division of responsibility suggests that regulator involvement in expungement of records or the general reporting standards is likely not correlated with treatment. Also, to the extent regulator effort is required to intervene in the case of an expungement, for example, our forthcoming result suggests treatment is higher where regulators are less likely to be well-staffed. Moreover, we performed our analysis using data gathered in 2015 and 2016, achieving similar results. There is some delay between reporting a complaint and how long an adviser must wait to redact it. Finally, from inspecting the data by hand, often we actually observe cases that constitute cases that are no longer reportable, and have been asked by the adviser to be removed. In such cases, the complaint details (alleged damages and the case description, for example) are redacted, not the disclosure itself. Therefore, the extensive margin analysis is unlikely to be affected by these concerns.

In the Appendix, we repeat the analysis at the firm level. In both extensive and intensive margin analysis, our main result comes through. We also perform analysis of the percentage of advisers receiving complaints. We take the log percentage and also just the raw ratio as outcome variables. We have no evidence that the the complaints per adviser go down and only statistically reliable evidence this quantity increases, within firm, within year. The meaning of this is significant because one concern might be AUM losses or human capital losses lead to mechanical reductions or increases in complaints. That such a ratio is non-decreasing and if anything increasing suggests that our result is not driven by some mechanical firm down-sizing. Of course, such a finding would be interesting it its own right.

7 Conclusion

We provide evidence that appears consistent with Dodd-Frank weakening oversight of mid-size RIAs, and more generally that national regulators better deter financial misconduct. Specifically, we find the re-jurisdiction of RIAs from SEC to state regulators due to the Dodd-Frank Act increased misconduct by 60% on average. The complaints are not obviously frivolous, but more severe in that they are likelier to result in an award. RIAs in states with fewer regulatory resources and those located farther from the respective state regulator saw the greatest increases in complaints. Investment advisers with histories of greater misconduct appear to incur more customer complaints when under state jurisdiction. More sophisticated investors, like institutions and governments, were less affected. Meanwhile, clients with less education and who are older were more often the victims of misconduct. The evidence contrasts with the plausible alternative hypothesis that a local regulator has advantages deterring financial misbehavior stemming from soft information and a local focus.

We feel our paper is successful in showing the descriptive result that complaints increase, and distinguishing the interpretation in the data that it appears to be best explained increased misconduct. Therefore, our findings suggest state securities regulators may potentially be less of a deterrent than the federal regulator on average. This finding is not necessarily normative, however, in that there may be an optimal level of misconduct versus the cost of deterring it fully.

That said, to the extent welfare is weakly decreasing in more misconduct, all else equal, the optimal allocation of resources for regulation is to reallocate state-level budgets to the SEC. Future research should further isolate the various mechanisms that do and do not matter for a financial regulator's oversight ability and enforcement, such as a regulator's reputation, institutional knowledge and practices, and its human capital. Our results also raise the important question of what is the cost function involved in eliminating misbehavior altogether, and the optimal social tradeoff thereof.

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8 Figures and Tables

Figure 1: Event Timeline

July 21, 2011 to December 31, 2011	January 1, 2012	March 30, 2012	June 28, 2012
New registration thresholds and requirements apply to new applicants, but not to existing SEC-registered advisers until the dates indicated in this table, as applicable.	Each SEC-registered adviser as of July 21, 2011 must remain registered with the SEC until this date (unless relying on an exemption).	Last day for all SEC-registered advisers to file the required Form ADV amendment.	Mid-sized advisers not eligible for SEC registration must file form ADV-W to withdraw by this date.

Figure 2: Annual ADV De-registration Filings

The graph below shows the number of RIAs that de-registered from the SEC with form ADV-W filings.

The Dodd-Frank Act came into effect in 2012. In the graph, the line “ADV-W Filings” refers to the total number of filed Form ADV-Ws. “Partial de-registration” presents the number of Form ADV-Ws for a partial de-registration. An optional field in Form ADV-W allows RIAs to specify the reason for partial de-registration. The line (“# Mention state de-registration”) indicates how many partial de-registrations specifically mentioned the intention to register with state securities regulator.

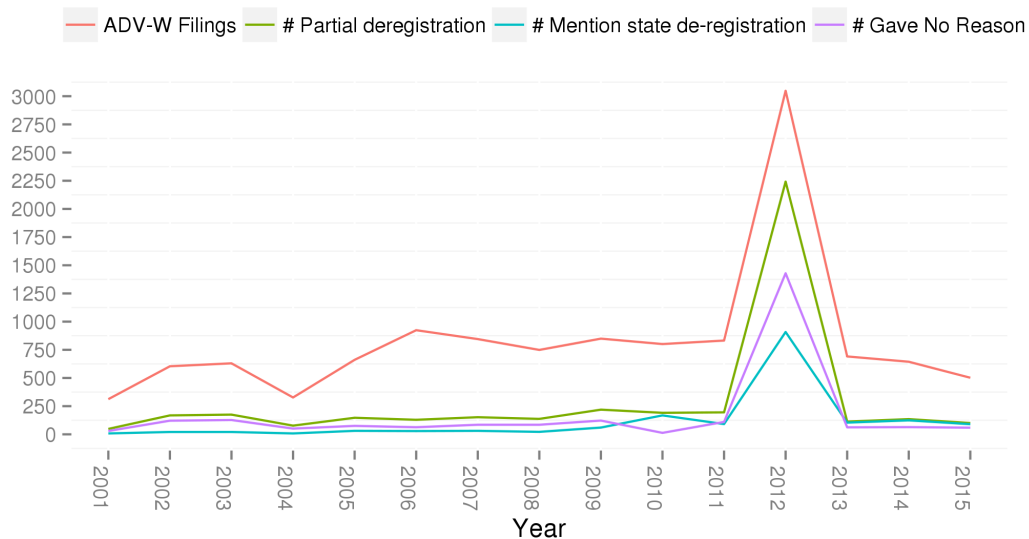


Figure 3: Filing a Customer Complaint

This figure illustrates how customers file a complaint with the New Jersey Securities Regulator.





File a Complaint

The Bureau of Securities investigates complaints against individuals and firms selling securities or offering investment advice as well as companies issuing securities investments. The Bureau is empowered to bring administrative actions or civil law suits to enforce the registration and anti-fraud provisions of the New Jersey Uniform Securities Act. The Bureau may refer certain matters for criminal prosecution.

Please be advised that the Bureau does not have the specific authority to order restitution or the repayment of any monies which you may believe are due you.

Investor Information

Name:

Street Address:

City: State: ZIP Code:

Daytime Number: Evening Number: Fax:

Email Address:

Firm Information

Firm Name:

Street Address:

City: State: ZIP Code:

Telephone Number (1): Telephone Number (2):

Email Address:

Complaint Information

1. Type of firm (if known):

If other, please specify:

2. Name and title of firm's agents or employees with whom you dealt:

Name:

Title:

Figure 4: Parallel Trends

The first figure below shows trends in complaint rates for advisers with more than \$100M and registered with the SEC with those who switched from SEC to state regulation in 2012. The baseline group is formed based on a matched sample, matching on complaint rates in 2009 & 2010 and requiring advisers be in the same state. The second figure shows the residualized complaint rates where the baseline group is defined to be RIAs with assets between \$100M to \$300M. The Dodd-Frank Act shifts oversight of mid-size (\$25M-\$100M in AUM) advisers to state regulators in 2012. This visual evidence is corroborated by a formal statistical test in Table 6.

Dependent variable: $Pr(Complaint_t)$

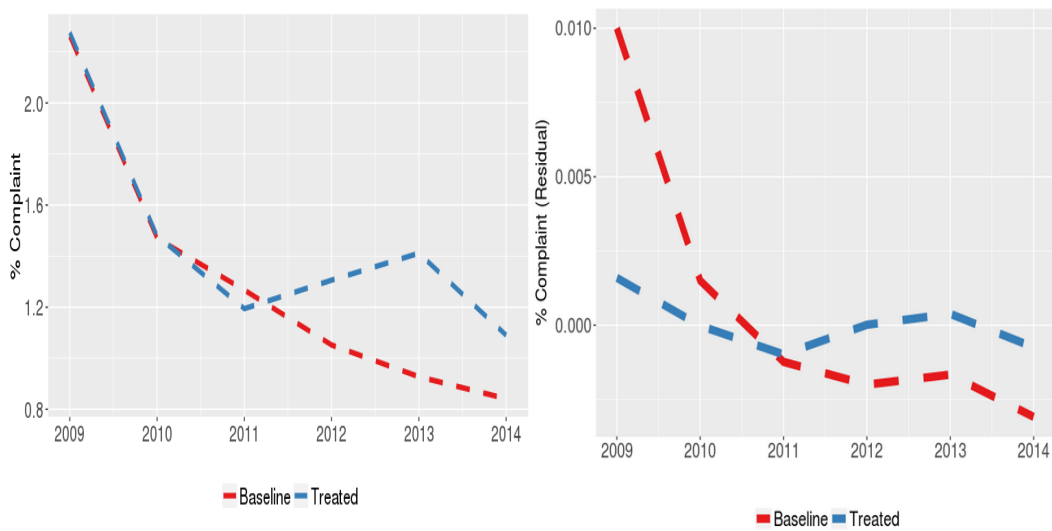


Figure 5: State Regulator Budgets

The figure below shows the normalized proposed state securities regulator budgets. Most states request budgets on a biannual basis. The bold line is the average across all states.

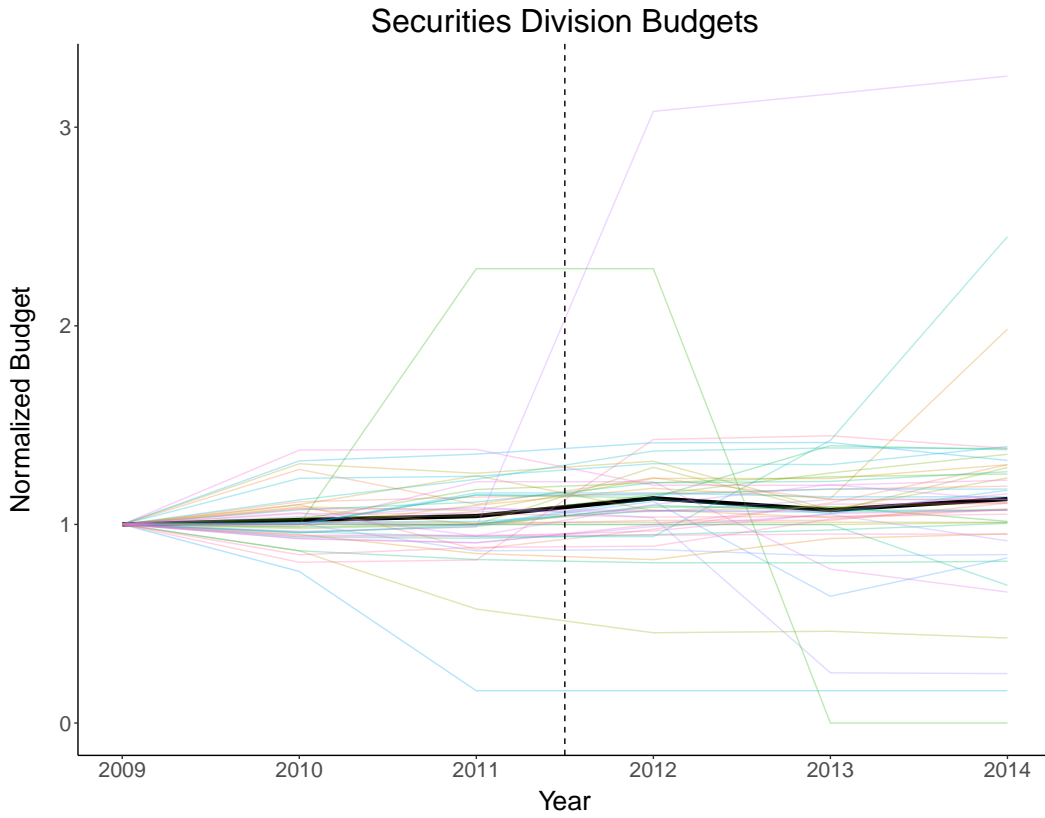


Table 1: Sample Size

The table below shows summary statistics of our sample constructed by merging the universe of RIAs' Form ADV filings and the survivorship-bias free individual data from the Investment Adviser Public

Panel A: Number of...	
Unique representative CRDs in IAPD	492,841
Unique RIA CRDs in IAPD	30,579
Unique CRDs in SEC Form ADV	6,235
Total IA-year Observations in IAPD	4,623,292
Observations(2009-2014)	1,791,522
Observations (Annual Sample)	1,290,043
Observations (3-year window)	382,665
RIAs treated	2,316
Individuals treated	23,547
Individual-year treated	129,171
Always State-registered RIAs	17,821
Always State-registered Individuals	97,264

Database (IAPD).

Panel B: Sample by Year			
Year	# Individuals with Complaints	# Complaints	Pr(Complaint)
2009	7,015	8,551	2.52
2010	4,329	5,303	1.51
2011	3,520	4,084	1.2
2012	3,105	3,569	1.03
2013	3,020	3,470	0.97
2014	2,434	2,969	0.75
Total	23,423	27,946	1.33

Table 2: RIA Summary Statistics

The table provides summary statistics for investment adviser RIAs from Form ADV filings for 2011. RIAs report whether they have custody over assets, independent audits, specific incentive structures, and report the fraction of clients who are institutions, private funds, government, and individuals. Advisers also denote whether individuals are accredited, earning either more than \$200,000 if single or \$300,000 if married, or more than \$1,000,000 in net worth. We denote accredited advisers as sophisticated. Funds are labeled as having a majority of clients in a particular category if the fraction of clients for the adviser is greater than or equal to 50%.

	Total	Untreated	Treated
N with non-missing AUM	6,226	3,910	2,316
Assets:			
AUM 10 th Percentile	35	103	30
AUM 25 th Percentile	57	143	39
AUM 50 th Percentile	128	295	53
AUM 75 th Percentile	422	882	72
AUM 90 th Percentile	1,841	4,090	89
Fraction of AUM with Custody (%)	7	6	14
Fraction of RIAs with Custody (%)	18	24	8
Fraction with Independent Audits (%)	21	29	9
Incentive Structure:			
Private Fund (%)	26	32	15
Other Business (%)	16	15	18
Other Business is Main Business (%)	5	5	6
Recommends a Broker (%)	64	68	58
Have Proprietary Conflicts of Interest (%)	87	88	86
Have Sales Conflicts of Interest (%)	20	27	10
Have Investment Discretion (%)	93	94	90
Client Composition:			
Individuals (%)	69	66	75
Unsophisticated Individuals (%)	35	29	45
Institutions (%)	43	47	36

Table 3: Complaint Case Outcomes

Panel A shows summary statistics for complaints in the pre- and post-treatment periods. For each complaint, we calculate the frequency that it was later withdrawn, closed, denied, settled, or entered arbitration or litigation. Panel B shows the distribution of complaints by product and reasons according to IAPD classifications. The related products and reasons for complaints are not mutually exclusive; a customer may file a single complaint for multiple reasons and associated with multiple products.

Panel A: Complaints in Before and After 2012					
Case Status	Treated	Untreated	Treated	Untreated	
Period:	2009-2011		2012-2014		All
Withdrawn	3%	2%	1.2%	2.4%	2.1%
Closed - No Action	12.2%	6%	12.3%	6.1%	6.4%
Denied	30.5%	47.1%	32.2%	46.6%	46.3%
Settled	40%	37.6%	46.8%	15.1%	38.1%
Arbitration or Litigation	12%	7.3%	7.6%	5.9%	7.1%
\$ Alleged Damages	283,284	311,559.6	179,823	206,377	284,322
Average \$ Settlements	96,840.9	97,127	103,082	82,848	93,996

Panel B: Distribution of Complaints					
Product	Product		Type		
	Number	% of Total	Number	% of Total	
Annuity	7,122	28.32	Suitability	9,955	39.58
Variable Annuity	6,498	25.84	Misrepresentation	8,620	34.27
Mutual Fund	5,371	21.36	Fiduciary	1,943	7.73
Equity	5,040	20.04	Unauthorized Trading	1,827	7.26
Insurance	2,108	8.38	Fraud	1,484	5.90
Debt	2,051	8.16	Fees	1,306	5.19
Real Estate	1,941	7.72	Portfolio Allocation	669	2.66
OTC	1,105	4.39	Churning	567	2.25
Options	591	2.35			
Fixed Annuity	578	2.30			
Private Placements	409	1.63			

Table 4: Baseline Results

The table below shows the difference-in-differences estimates of the impact of switching from SEC to state registration on the propensity to receive a complaint. Columns (1) - (3) uses the annual person-year panels and Columns (4) - (6) collapses the sample to a three-year period around 2012 a la Bertrand et al. (2004). In the latter three columns, all individuals must work the entire three year period to be considered. Robust standard errors clustered by state are shown in parentheses.

<i>Dependent variable:</i>	$1_{\{Complaint_t\}} \times 100$			
Sample	(1)	(2)	(3)	(4)
		Annual sample		Pre-post Sample
Constant	1.266*** (0.168)			
Post ²⁰¹²	-0.802*** (0.088)			
Treated	-0.466*** (0.171)			
Post ²⁰¹² ×Treated	0.529*** (0.161)	0.544*** (0.143)	0.574*** (0.164)	1.653*** (0.726)
Fixed Effects		RIA + Year	RIA + Year +Individual	RIA + Year
Observations	1,299,819	1,299,819	1,299,819	382,665
R ²	0.003	0.011	0.252	0.628

*p<0.1; **p<0.05; ***p<0.01

Table 5: Robustness: Alternative Comparison Groups

The table below shows alternative specifications to address comparison group concerns. Column (1) in the table below excludes California, which is home to the highest number of investment advisers. Column (2) excludes New York and Wyoming because advisers in these two states stayed with the SEC. Column (3) removes advisers with over \$100 million in AUM, forcing the control group to be mid-size advisers in New York and Wyoming. Column (4) presents a placebo estimate defining the treatment year as 2005 using a sample from 2002 to 2008. Robust standard errors clustered by state are shown in parentheses.

<i>Dependent variable:</i>	$1_{\{Complaint_t\}} \times 100$			
	(1)	(2)	(3)	(4)
Sample:	Ex-CA	Ex-NY/WY	≤ 100 MM	Placebo
Post ²⁰¹² × Treated	0.595** (0.177)	0.541*** (0.184)	1.337*** (0.212)	-0.083 (0.313)
Fixed Effects	RIA + Individual + Year	RIA + Individual + Year	RIA + Individual + Year	RIA + Individual + Year
Observations	1,232,293	932,840	66,960	704,235
R^2	0.252	0.276	0.344	0.244

*p<0.1; **p<0.05; ***p<0.01

Table 6: Robustness: Parallel Trends

The table below shows alternative specifications to address parallel trend time-series concerns at an individual level from 2009 to 2014. Column (1) removes 2009, which deviates most from the parallel trends condition. Column (2) removes 2009 and explicitly tests the parallel trends assumption prior to 2012. Column (3) removes 2009 and 2010, using only 2011 as the pre-treatment period. Column (4) considers firms with AUM less than \$300 million. Column (5) considers firms with AUM less than \$300 million and explicitly tests the parallel trends assumption prior to 2012. Column (6) uses a propensity-score matched sample on the probability of being treated after 2012, matching with replacement from the same state, with the same three-year complaint count, and the same pre-trend (whether they received complaints in 2009, 2010, 2011). Column (7) is an alternative matched sample, relaxing the constraint that the individual comes from the same state. All regressions include firm, individual, and year fixed effects. Robust standard errors clustered by state are shown in parentheses for the OLS samples. Robust standard errors clustered by matched pair are shown in parentheses for the matched sample, a la Abadie and Spiess (2016).

<i>Dependent variable:</i>		$1_{\{Complaint_t\}} \times 100$					
Sample	(1) Exclude 2009	(2) Exclude 2009	(3) Exclude 2009/2010	(4) AUM \leq \$300M	(5) AUM \leq \$300M	(6) Match 1	(7) Match 2
Post ²⁰¹² ×Treated	0.409** (0.164)	0.494* (0.254)	0.330** (0.157)	0.491** (0.242)	0.566* (0.298)	0.711*** (0.068)	0.524*** (0.092)
Year ²⁰¹¹ ×Treated		0.153 (0.268)			0.176 (0.254)		
Observations	1,127,588	1,127,588	940,131	126,390	126,390	321,941	241,110
R ²	0.275	0.275	0.323	0.349	0.349	0.250	0.270

*p<0.1; **p<0.05; ***p<0.01

Table 7: Customer Complaint Decomposition

The table below decomposes the percentage probability of receiving complaints at the individual by year level into different types. The types are not mutually exclusive as a complaint may be related to multiple allegations. All regressions include RIA and year fixed effects. Robust standard errors clustered by state are shown in parentheses.

<i>Dependent variable:</i>	$1_{\{Complaint_t\}} \times 100$							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Allegation Type	Churning	Concentrated	Fees	Fiduciary	Fraud	Misrepresentation	Suitability	Unauthorized
Post ²⁰¹² ×Treated	0.010 (0.029)	0.008 (0.014)	-0.011 (0.029)	0.053 (0.054)	0.075* (0.043)	0.163* (0.095)	0.241* (0.124)	-0.004 (0.022)
Observations	1,144,604	1,144,604	1,144,604	1,144,604	1,144,604	1,144,604	1,144,604	1,144,604
R ²	0.231	0.476	0.230	0.289	0.260	0.300	0.328	0.229

*p<0.1; **p<0.05; ***p<0.01

Table 8: Do Other Adviser Disclosures Increase?

The table below shows the impact of state registration on disciplinary actions related to regulatory, financial, criminal, and termination issues. Dependent variables are percentage probabilities. Robust standard errors clustered by state are shown in parentheses.

<i>Dependent variable:</i>	$1_{\{regulatoryAction\}} \times 100$				
	(1)	(2)	(3)	(4)	(5)
Sample	Full	Full	AUM≤\$300M	AUM≤\$300M	Full
Drop 2009	Yes	No	Yes	No	No
$Post^{2012} \times 1_{\{Complaint_t\}} \times Treated$	-4.955 (3.990)	-9.605* (5.430)	-9.209* (5.418)	-12.860* (7.020)	-8.794 (5.35)
$1_{\{Complaint_t\}}$	0.409*** (0.110)	0.560*** (0.151)	1.397 (1.541)	2.516 (2.488)	0.600*** (0.022)
$1_{\{Complaint_t\}} \times Treated$	7.869** (3.540)	12.453** (5.215)	6.837* (4.083)	10.476* (6.216)	12.48*** (5.157)
$1_{\{Complaint_t\}} \times Post^{2012}$	1.175*** (0.429)	1.018** (0.437)	5.469* (2.976)	4.286 (3.392)	0.952*** (0.432)
$Post^{2012} \times Treated$	0.323* (0.180)	0.472*** (0.177)	0.466** (0.231)	0.682*** (0.230)	
Fixed Effects	RIA <i>State×Year</i>	RIA <i>State×Year</i>	RIA <i>State×Year</i>	RIA <i>State×Year</i>	Firm*Year
Observations	1,207,034	1,052,026	111,147	95,290	1,052,026
R^2	0.326	0.016	0.015	0.286	0.06

*p<0.1; **p<0.05; ***p<0.01

Table 9: Complaint Noise-to-Signal Ratio Measured by Case Award and Denial Rates

The table below shows the relations between switching from SEC to state registration and complaint properties. Columns (1) - (2) shows the relation between treatment and alleged damages. Column (3) shows the relation between treatment and the restitution or settlement amount. Column (4) shows the relation between treatment and whether the complaint resulted in a settlement or restitution. Column (5) shows the relation between treatment and whether complaints are withdrawn, denied, or had no action. Cases with no restitution or settle data are included (these are complaints whose settlement is reported as \$0 or missing. If the case is denied, withdrawn or there is no action taken, we code restitution as \$0.). Cubic controls include a 3-rd order polynomial of log(alleged damages) as a control. Pending cases and cases with zero alleged damages reported are removed. Robust standard errors clustered by state are shown in parentheses.

<i>Dependent Variable:</i>	log(1+Alleged Damages)	log(1+Award)	1{Award}	1 _{Withdrawn/Denied/No Action}	
	(1)	(2)	(3)	(4)	(5)
Post ²⁰¹² ×Treated	0.666 (0.420)	0.225 (0.438)	1.126*** (0.346)	0.111*** (0.034)	-0.080*** (0.030)
Fixed Effects	RIA + Branch+ +Year	RIA + <i>State</i> × <i>Year</i>	RIA + <i>State</i> × <i>Year</i>	RIA + <i>State</i> × <i>Year</i>	RIA + <i>State</i> × <i>Year</i>
Cubic Controls	No	No	Yes	Yes	Yes
Observations	18,607	19,355	15,057	15,057	19,355
R ²	0.290	0.175	0.350	0.256	0.329

*p<0.1; **p<0.05; ***p<0.01

Table 10: Staffing of the Investment Adviser Regulatory Office

This table presents the difference-in-difference estimates with triple interactions on the staff-per-RIA

devoted to adviser regulation in 1999 from the American Association of Retired Persons. Staff-per-RIA is the number of oversight employees at the state regulator divided by the number of RIAs. Columns (5) and (7) imputes states with missing data as 0, while all other columns exclude states that have missing data. We do not include a sample with less than \$300 million that does not impute staff since there is little data available. Robust standard errors clustered by state are shown in parentheses.

<i>Dependent variable:</i>	$1_{\{Complaint_t\}} \times 100$					
	(1)	(2)	(3)	(5)	(6)	(7)
Sample	Full	AUM ≤\$300M	Exclude 2009	Exclude 2009	Exclude 2009	Exclude 2009
Impute Staff-per-RIA	Yes	Yes	Yes	No	Yes	No
Post ²⁰¹² ×Treated×Staff-per-RIA	-0.390* (0.179)	-0.442* (0.229)	-0.4846** (0.215)	-0.493* (0.254)	-0.471** (0.170)	-0.479* (0.266)
Post ²⁰¹² ×Treated	0.392*** (0.145)	0.257 (0.205)	0.323*** (0.157)	0.231 (0.182)	0.402** (0.170)	0.277 (0.198)
Fixed Effects	RIA + <i>State</i> × <i>Year</i>	RIA + <i>State</i> × <i>Year</i>	RIA + <i>State</i> × <i>Year</i>	RIA + <i>State</i> × <i>Year</i>	RIA + Indiv <i>State</i> × <i>Year</i>	RIA + Indiv <i>State</i> × <i>Year</i>
Observations	1,297,781	135,284	1,133,172	999,877	1,133,172	999,877
R ²	0.012	0.011	0.010	0.010	0.277	0.284

*p<0.1; **p<0.05; ***p<0.01

Table 11: Distance to Regulator and Complaint Rates

This table presents difference-in-difference estimates using annual panel data to study the impact of regulator distance and complaints. Local offices addresses are retrieved from the NASAA and the SEC website as of 2015. Distances in miles are calculated using coordinates of the zip code of the firm or regulator's address. Robust standard errors clustered by state are shown in parentheses.

<i>Dependent variable:</i>		$1_{\{Complaint_t\}} \times 100$				
Distance from Regulator Measure	Firm HQ Distance					Branch
Sample	(1) Full	(2) AUM \leq \$300M	(3) No 2009	(4) Full	(5) No 2009	(6) No 2009
Post ²⁰¹² × Treated	0.387*** (0.147)	0.303* (0.157)	0.194 (0.179)	-0.112 (0.184)	-0.145 (0.199)	
Post ²⁰¹² × Treated × log(<i>Dist_{State}</i>)	0.238** (0.116)	0.352** (0.138)	0.222** (0.110)	0.233 (0.152)	0.288* (0.167)	0.319** (0.157)
Post ²⁰¹² × log(<i>Dist_{State}</i>)	0.006 (0.065)	-0.06 (0.052)	0.202* (0.118)			-0.017 (0.048)
Post ²⁰¹² × Treated × log(<i>Dist_{SEC}</i>)						
Fixed Effects	RIA <i>State</i> × <i>Year</i>	RIA <i>State</i> × <i>Year</i>	RIA <i>State</i> × <i>Year</i>	RIA <i>HQZip</i> × <i>Post</i>	RIA <i>HQZip</i> × <i>Post</i>	RIA × <i>Year</i> <i>Branch</i> × <i>Year</i>
Observations	1,217,182	1,070,907	128,670	1,217,182	1,070,907	1,052,854
R ²	0.012	0.040	0.010	0.010	0.010	0.063

*p<0.1; **

Table 12: The Treatment Effect and Branch-level Demographics

This table presents the effect of switching from SEC to state regulation interacted with client demographics in the county where an individual adviser representative works. Demographic data are from the 2011-2015 American Community Survey. *% College* is defined as the fraction of the county's adult population from age 24-54 with at least a Bachelor's education and *% Age > 60* is the fraction of the population aged 60 or above. Robust standard errors clustered by state are shown in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
County Type	<i>%Age > 60</i>	<i>% College</i>	<i>%Age > 60</i>	<i>% College</i>	<i>%Age > 60</i>	<i>% College</i>
Sample	Full	Full	No 2009	No 2009	AUM ≤\$300M	AUM ≤\$300M
Post ²⁰¹² ×Treated×County Type	0.355** (0.158)	-0.289* (0.173)	0.376*** (0.135)	-0.069 (0.150)	0.553*** (0.195)	-0.639* (0.369)
Fixed Effects	RIA-by-Year <i>Branch×Year</i>	RIA-by-Year <i>Branch×Year</i>	RIA-by-Year <i>Branch×Year</i>	RIA-by-Year <i>Branch×Year</i>	RIA-by-Year <i>Branch×Year</i>	RIA-by-Year <i>Branch×Year</i>
Observations	1,180,124	1,179,922	1,028,917	1,028,746	109,310	109,276
R ²	0.021	0.021	0.055	0.055	0.024	0.024

*p<0.1; **p<0.05; ***p<0.01

Table 13: Client Composition at the Firm-Level and Client Complaint Rates

This table presents the difference-in-difference estimates sorted by client composition of the RIAs as reported on the firms' 2011 ADV. Indicator variables equal 1 if the firm takes money as a private fund, or from clients who are accredited investors, institutional investors or government agencies. The sample includes only those that report taking money from one of these groups, or unaccredited individuals - those with missing information are excluded. Robust standard errors clustered by state are shown in parentheses.

<i>Dependent variable:</i>			
Sample	Full	300MM	No2009
Post ²⁰¹² ×Treated	42.685** (21.384)	16.734 (21.945)	36.218* (21.750)
Post ²⁰¹² ×Treated×1{Accredited Investors}	76.856 (59.609)	41.243 (81.910)	70.683 (47.994)
Post ²⁰¹² ×Treated×1{Advises Private Funds}	6.643 (34.320)	61.774* (37.475)	11.255 (35.269)
Post ²⁰¹² ×Treated×1{Government}	-50.713 (47.876)	25.03 (31.552)	-68.572 (50.224)
Post ²⁰¹² ×Treated×1{Institutions}	-94.723* (50.092)	-86.717 (62.611)	-89.420** (41.104)
Fixed Effects	RIA <i>State×Year</i>	RIA <i>State×Year</i>	RIA <i>State×Year</i>
Observations	1,299,819	136,159	1,135,001
R ²	0.012	0.04	0.011

*p<0.1; **p<0.05; ***p<0.01

Table 14: The Effect of Regulatory Jurisdiction on Recidivism

This table presents a cross-sectional test of complaints received in the 2012-2014 period. The variable of

interest is $Past \times Treated$, which is the interaction term between a variable describing the individual's past complaint history. Also of interest is the unconditional recidivism term, $Past$, which measures probability an adviser receives a complaint in the 2012-2014 period conditional on the value of $Past$ in 2009-2011. The measures of past activity include whether the individual has received a complaint before $1_{\{Past\ Complaints \geq 0\}}$, the log number of complaints $\log(1 + \#Past\ Complaints)$, and the residual number of complaints ϵ^{Past} . The latter is the residual of a regression based on a benchmark based on the characteristics of RIAs in which the individual worked in the past. Robust standard errors clustered by state are shown in parentheses.

Dependent variable:	$1_{\{Complaint_t\}} \times 100$					
	(1)	(2)	(3)	(4)	(5)	(6)
Past Measure	$1_{\{Past\ Complaints \geq 0\}}$		$\log(1 + \# Past\ Complaints)$			ϵ^{Past}
Constant	2.409*** (0.037)		2.369*** (0.107)		1.380*** (0.053)	
Past	9.346*** (0.367)	8.658*** (0.369)	13.262*** (1.018)	12.139*** (0.533)	30.206*** (1.095)	27.841*** (1.181)
Treated	-0.023 (0.187)		0.075 (0.347)		-0.269 (0.253)	
Past×Treated	7.145*** (2.514)	4.820* (2.513)	5.756** (2.823)	7.114** (3.626)	19.907*** (6.944)	15.581* (7.963)
Fixed Effects		RIA		RIA		RIA
Observations	183,423	183,423	183,423	183,423	183,423	183,423
R^2	0.014	0.038	0.018	0.040	0.016	0.038

*p<0.1; **p<0.05; ***p<0.01

Table 15: Treatment Effect on Existing State-Registered RIAs

The table below shows the treatment effect on RIAs with less than \$25 million in AUM using our

annual panel data. These RIAs were always registered with the state regulator (and never registered with the SEC). The analysis excludes mid-sized advisers (the treated firms). We compare the never-SEC-registered firms to the firms remaining with the SEC. Robust standard errors clustered by state are shown in parentheses.

<i>Dependent variable:</i>	$1_{\{Complaint_t\}} \times 100$					
	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Full	Full	No 2009	No 2009	No 2009,2010	No 2009,2010
Post ²⁰¹² × Never SEC Registered	0.512*** (0.093)	0.345*** (0.110)	0.259*** (0.079)	0.105 (0.089)	0.157** (0.075)	0.003 (0.092)
Year ²⁰¹¹ × Never SEC Registered						
Fixed Effects	RIA + Year	RIA + Indiv. +Year	RIA + Year	RIA + Indiv. +Year	RIA + Year	RIA + Indiv. Year
Observations	1,732,674	1,732,674	1,463,269	1,463,269	1,463,269	1,186,859
R ²	0.018	0.283	.017	.305	.017	0.348

*p<0.1; **p<0.05

A Appendix

A.1 Additional Institutional Details

Figure 2: Assets under management growth

This graph presents the growth in assets under management of RIAs filing Form ADV to the SEC through 2014.

We retrieved the ADV data from the SEC through a Freedom of Information Act request in 2015. For each RIA, we use the latest filing in the calendar year.

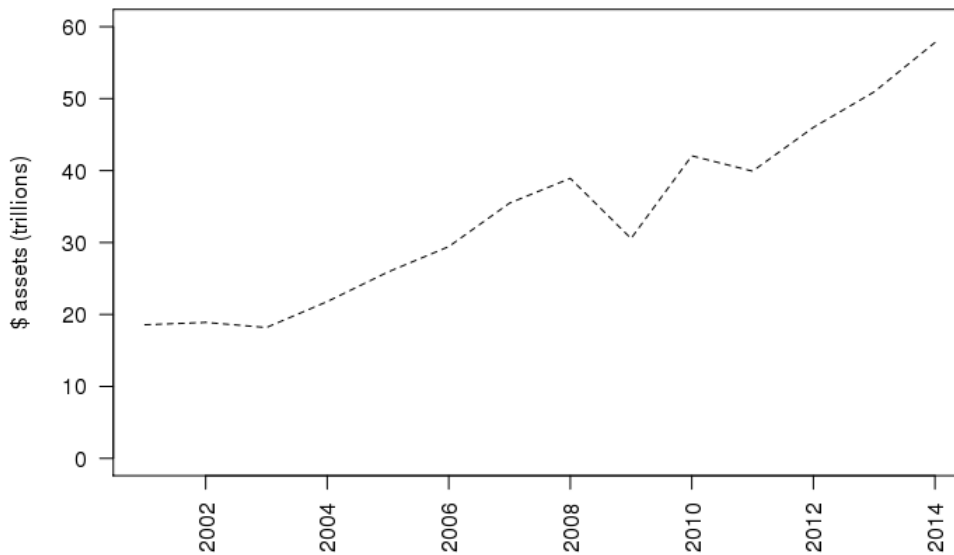


Table 1: Form ADV Information

This table summarizes the components of Form ADV

Item in form ADV	Description
Item 1. Identifying Information	Address, business name, etc. normal business hours.
Item2. SEC Registration/Reporting	States of operation
Item3. Form of Organization	Are you a corporation, LLC? Where are you organized? What country's laws are you subject to?
Item4. Successions	Are you succeeding another business, e.g. inheriting clients?
Item5. Information about your Advisory Business	Regulatory AUM, lines of business, types of clients, compensation arrangements, types of services offered, num. employees, num. clients,
Item 6. Other Business Activities	Non-principal activities your firm engages in
Item 7. Financial Industry Affiliations	Are your related persons part of another financial institution, broker, law firm or accounting firm?
Item 7B Private Fund Reporting	Are you a private fund?
Item 8 Participation or Interest in Client Transactions	Do you have a proprietary interest in client transactions (do you trade your own funds)), a sales interest in client transactions (do you sell your clients stuff), or investment/brokerage discretion? And a lot of related detailed questions.
Item 9 Custody	Do you have custody of assets, of how much? Do your related persons have control? Do you have a qualified custodian, an independent accountant, that overlooks? Do you get surprise audits?
Item 10 Control Persons	Who are the control persons and how much do they own?
Item 11 Disclosures	Criminal, regulatory, civil lawsuits for the firm. Note: Not individual IAR history/roster.
Item 12 Small Business Schedule A/B/D	Various probably irrelevant things about control structure and total firm assets Direct/indirect owners, officers, with ownership stakes. Location of books and records. Other offices.

Figure 1: Regulatory Infrastructure

This graph presents a diagram of the filings made by RIAs and investment advisers (individuals), the relevant receiving entity, the record maintainer, and the points of disclosure through which one can obtain the data used in this paper or related papers. This diagram is based on the authors' own observation.

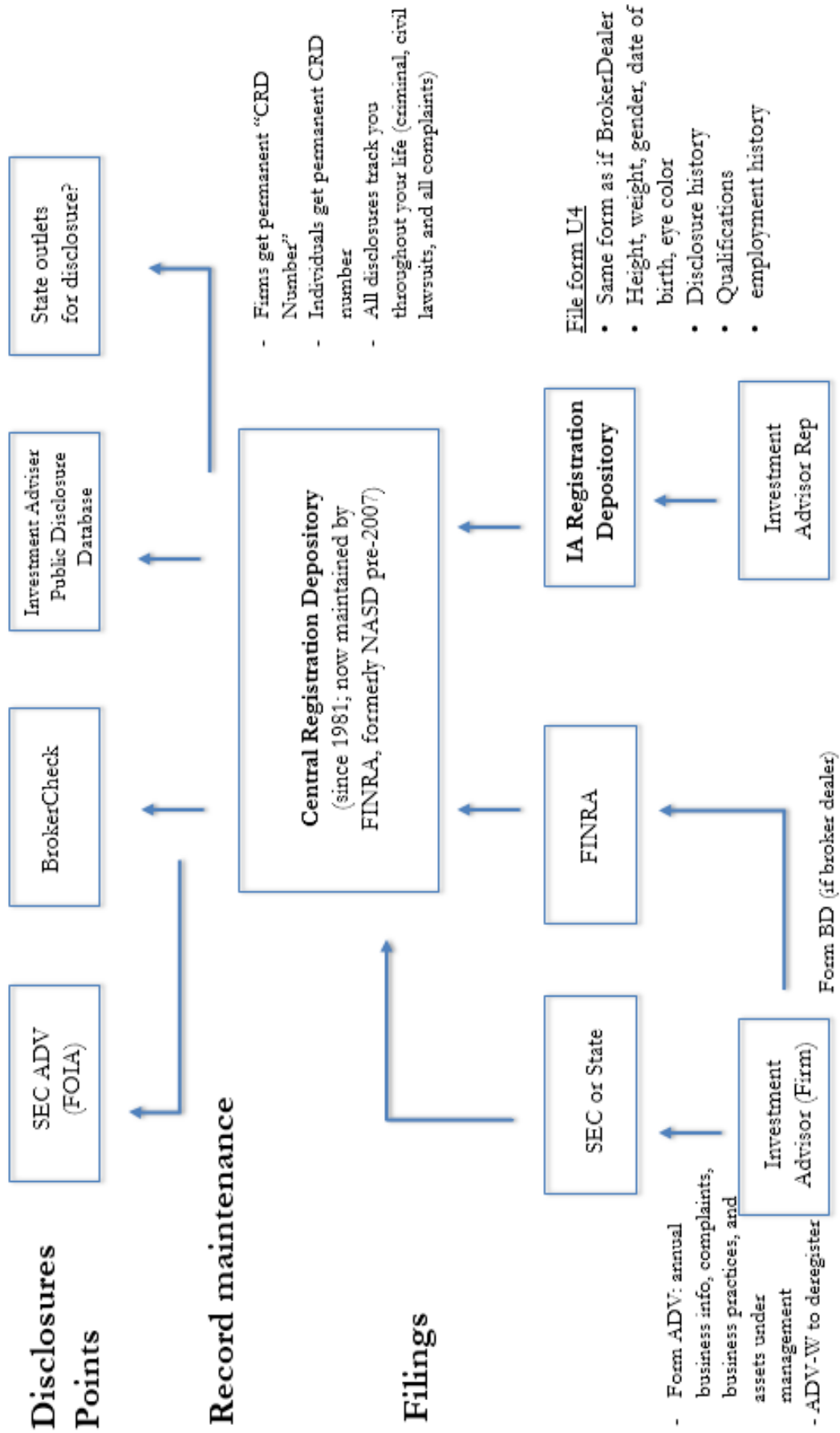


Table 2: Investment Adviser Representative Exits

The table below shows the probability that an investment adviser representative drops out of our sample after receiving a complaint. The dependent variable Penultimate_{*t*} takes the value 1 if year *t* is the penultimate year an IAR exists in our sample. The dependent variable Ultimate_{*t*} takes the value 1 if year *t* is the last year an IAR exists in our sample, limiting it to end in 2014. Receiving a complaint is related to dropping out of our sample.

<i>Dependent variable:</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Penultimate _{<i>t</i>}			Ultimate _{<i>t</i>}						
Treated	2.258*** (0.250)	2.258*** (0.250)				4.637*** (0.247)	4.634*** (0.249)			
$1_{\{Complaint_t \geq 0\}}$	0.485** (0.193)					0.024 (0.037)				
$1_{\{Complaint_t \geq 0\}} \times Treated$	-0.282 (0.931)					1.757 (1.186)				
$\log(1 + \#Complaints_t)$		1.208** (0.302)	1.235*** (0.324)	1.193*** (0.296)	1.024*** (0.266)		0.022 (0.047)	0.031 (0.045)	0.022 (0.047)	-0.017 (0.054)
$\log(1 + \#Complaints_t) \times Treated$		-0.515 (1.448)	-0.642 (1.467)	-0.544 (1.449)	-0.494 (1.513)		2.718 (1.674)	2.721 (1.662)	2.670 (1.673)	2.368 (1.551)
Observations	981,548	981,548	981,548	981,548	981,548	729,258	729,258	729,258	729,258	729,258
Fixed Effects		RIA					RIA			
R ²	0.028	0.028	0.028	0.030	0.074	0.033	0.033	0.032	0.034	0.079

*p<0.1; **p<0.05; ***p<0.01

Table 3: Different AUM Cutoffs

This table presents the main result using different AUM cutoffs. RIAs with 2011 AUM above the stated amount are excluded, such that the control group is all untreated RIAs below the indicated AUM level. Robust standard errors are clustered at the state level.

<i>Dependent variable:</i>	$1_{\{Complaint_t\}} \times 100$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
AUM Cutoff =	100MM	150MM	200MM	250MM	300MM	350MM	400MM
Past×Treated	1.337*** (0.217)	0.823*** (0.294)	0.698** (0.272)	0.640*** (0.236)	0.491** (0.247)	0.479** (0.218)	0.469** (0.214)
Fixed Effects	RIA+Year+ Individual	RIA+Year+ Individual	RIA+Year+ Individual	RIA+Year+ Individual	RIA+Year+ Individual	RIA+Year+ Individual	RIA+Year+ Individual
Observations	66,960	87,047	102,216	113,765	126,390	143,501	153,038
R ²	0.344	0.353	0.353	0.351	0.349	0.341	0.342

*p<0.1; **p<0.05; ***p<0.01

Table 4: Firm-level Analyses

This table presents the main result using different AUM cutoffs. RIAs with 2011 AUM above the stated amount are excluded, such that the control group is all untreated RIAs below the indicated AUM level. Robust standard errors are clustered at the state level.

<i>Dependent variable:</i>	$\log 1[100 * \frac{complaints}{adviser}]$	$\log\{1 + complaint\}$	$\log\{1 + complaint\}$	$1\{complaint\}$	$\log 1[100 * \frac{complaints}{adviser}]$
	(1)	(2)	(3)	(4)	(5)
Post ²⁰¹² ×Treated	0.024** (0.010)	0.015*** (0.005)	0.025*** (0.004)	0.013*** (0.003)	0.024** (0.010)
Treated			-0.018*** (0.006)		
log(AUM in 2012)			-0.015*** [0.002]		
FE?	Firm+Year	Firm+Year	Year	Firm+Year	Firm+Year
Controls for log(#advisers)?	Polynomial 3	No	Polynomial 3	Polynomial 3	Polynomial 3
Observations	33,678	33,854	33,854	33,854	22,712
Sample	All	All	All	All	<=300MM
R ²	0.414	0.892	0.739	0.674	0.342
Adjusted R ²	0.283	0.867	0.739	0.6	0.283

A.2 State Securities Regulators

Table 5: State Securities Regulators

The table below shows the names of state securities regulators, broken up into divisions and departments. Divisions are the smallest organizational entities that oversee securities regulation. For most states, the securities division is a sub-organization of a larger department such as the Secretary of State or the Attorney General’s office. The value is blank when a departmental hierarchy is not provided.

State	Division Name	Department Name
Alabama	Alabama Securities Commission	
Alaska	Banking and Securities Division	Department of Commerce, Community, and Economic Development
Arizona	Securities Division	Arizona Corporation Commission
Arkansas	Arkansas Securities Department	
California	Securities Regulation Division	Department of Business Oversight
Colorado	Division of Securities	Department of Regulatory Agencies
Connecticut	Securities and Business Investment Division	Department of Banking
Delaware	Investor Protection Unit	Attorney General
Florida	Division of Securities	Office of Financial Regulation
Georgia	Division of Securities	Secretary of State Office
Hawaii	Division of Securities	Department of Commerce and Consumer Affairs
Idaho	Securities Section	Department of Finance
Illinois	Securities Department	Secretary of State
Indiana	Securities Division	Secretary of State
Iowa	Securities Bureau	Insurance Division
Kansas	Office of the Securities Commissioner	
Kentucky	Securities Division	Department of Financial Institutions
Louisiana	Securities Division	Office of Financial Institutions
Maine	Office of Securities	Department of Professional and Financial Regulation
Maryland	Securities Division	Attorney General
Massachusetts	Securities Division	Secretary of Commonwealth
Michigan	Corporations, Securities, and Commercial Licensing Bureau	Department of Licensing and Regulatory Affairs
Minnesota	Securities, Franchises, and Subdivided Land	Department of Commerce
Mississippi	Securities Division	Secretary of State
Missouri	Securities Division	Secretary of State
Montana	Commissioner of Securities and Insurance	Office of the Montana State Auditor
Nebraska	Department of Banking and Finance	
Nevada	Nevada Securities Center	Secretary of State
New Hampshire	Bureau of Securities Regulation	Secretary of State
New Jersey	Bureau of Securities	Division of Consumer Affairs
New Mexico	Securities Division	Regulation and Licensing Department
New York	Investor Protection Bureau	Attorney General
North Carolina	The Securities Division	Secretary of State

Table 6: State Securities Regulators (continued)

State	Division Name	Department Name
North Dakota	Securities Department	
Ohio	Division of Securities	Department of Commerce
Oklahoma	Department of Securities	
Oregon	Division of Financial Regulation	
Pennsylvania	Department of Banking and Securities	
Rhode Island	Department of Business Regulations	
South Carolina	Securities Division	Attorney General
South Dakota	Division of Securities	Division of Licensing and Regulation
Tennessee	Department of Commerce & Insurance	Department of Commerce & Insurance
Texas	State Securities Board	
Utah	Division of Securities	
Vermont	Securities Division	Department of Financial Regulation
Virginia	Division of Securities and Retail Franchising	State Corporation Commission
Washington	Division of Securities	Department of Financial Institutions
West Virginia	Securities Commission	State Auditor's Office
Wisconsin	Division of Securities	Department of Financial Institutions
Wyoming	Investing Center	Secretary of State

A.3 Redacting Customer Complaints

One potential concern regarding our results is that redactions increased for RIAs that were treated relative to those that were not treated. First, state regulators may be more susceptible to regulatory capture, seeking to preserve the presence of investment advisers who may otherwise leave the state. Second, the inability of adviser representatives to redact their complaints drives our results, being in a state with more regulatory staff means more resources to process the complaints. Moreover, being farther from the corresponding state regulator would also mean redacting a complaint is more costly. We argue these alternatives are not likely in light of the legal environment through which redactions are processed.

Records are stored at FINRA through the CRD system. Complaints that have alleged damages over \$5,000 or resulted in some legal action are both reported in the system. Upon receiving a complaint, both the investment adviser RIAs and investment adviser representative have to file a complaint disclosure to the CRD system. In August 2010, FINRA began disclosing all historic complaints, regardless of age. In the past, unproven allegations were not disclosed af-

ter two years. Specifically, investment adviser representatives may want to remove over which advisers have little control:

1. **Denied customer complaints.** Although denied customer complaints may seem insignificant, accusations typically are accompanied by harsh words that remain on the CRD for at least two years (since 2009, accusations stay for 10 years). Even if an adviser's record shows patterns of denied rather than arbitrated or settled complaints, RIAs also may be hesitant to affiliate with that adviser. Moreover, whether or not complaints are settled in the first place is mainly up to the RIA, not the representative.
2. **Termination explanations.** Broker-dealers may terminate advisers for any reason. Discrepancies can exist between the self-reported termination explanation and the RIA-reported explanation.

Investment advisers occasionally request expungement of customer complaints. Because the records are stored in the CRD, FINRA handles all expungement requests. Nonetheless, other regulators are involved in the process. FINRA may agree to remove disclosures if brokers obtain a recommendation that is false, erroneous, or that the broker wasn't involved in the alleged misdeed. To obtain this recommendation, representatives must acquire a court confirmation after submitting an expungement request. Upon submission, the corresponding investment adviser regulator (SEC or state regulator) is informed, giving a chance to oppose the expungement. State regulators received a total of 519 requests in 2010, up from 110 in 2009. In total, the process to expunge a complaint typically takes at least one year. Although FINRA claims to have tracked the number of expungements granted, they do not publicly disclose it.

Although the expungement process is fairly difficult, FINRA arbitrations could be settled subject to an agreement that claimants would not oppose the investment adviser representative's subsequent efforts to seek expungement from a court of competent jurisdiction. Subsequently, representatives would initiate unopposed petitions for expungement in state courts that were often rubber-stamped. The judge's order would then be submitted to FINRA, and the arbitration disclosure would be expunged. In response to this practice, FINRA adopted

Rule 2130 in 2004. One of the most significant changes was the need to name FINRA as an additional party challenging expungement. This meant FINRA also receives all appropriate documents with expungement, unambiguously increasing the cost of expungement requests. Moreover, although expungement requests from arbitrated cases are mostly granted, less than 8% of disclosures are expunged. Of the 7,621 arbitration cases from 2012 to 2014, only 563 records were expunged, according to the arbitration bar association.

Some of the surge in requests is also the result of new disclosure demands by FINRA. Until 2009, only brokers who were named as a party to a case had to disclose a customer complaint. Because most investors sue only the brokerage firm, large brokerage firms could shield individual brokers from direct accusations. However, firms might not have this incentive. Larger firms may also be more likely to place blame on an individual whom they could terminate, in order to shift blame to the individual. After 2009, FINRA modified this disclosure practice, requiring all brokers to report complaints regardless of whether they were named directly as a respondent.

The institutional setting suggests deletions of customer complaints is not relevant for the timing of the Dodd-Frank Act. Our phone calls with three regulators: California, Maryland, the SEC also suggest expungement is not an issue. Moreover, the censoring bias from any deleted complaints should not be correlated with treatment either. Finally, our specifications with state-year fixed effects and firm fixed effects absorb a lot of the drivers of expungement.