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Permalink

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Journal

Journal of Legal Studies, 53(2)

Author

Restrepo, Fernan

Publication Date

2024

The economic consequences of hedge fund regulation: An analysis of the effect of the Dodd-Frank Act

Fernán Restrepo*

Abstract

This paper exploits registration data administered by the Securities and Exchange Commission to examine the effect of the Dodd-Frank Act (the “DFA”) on profitability, risk-taking, and capital formation in the hedge fund industry. The data show that after the DFA was implemented, there was a significant decline in investor profitability – which can be at least partially attributed to direct compliance costs. However, compliance costs do not seem to fully explain the results: part of the decline seems to be driven by collateral effects of compliance, and particularly by diversion of managerial attention from core business activities and/or adjustments to financial valuation or reporting practices. The data also show that risk-taking did not change significantly, and that although fund managers closed funds and launched less funds in response to the law, that behavior did not result in less capital formation (in the form of assets under management).

JEL Classification: G23, G24, G28, K22.

* Assistant Professor of Law, University of California – Los Angeles; Affiliated Fellow, Rock Center for Corporate Governance, Stanford Law School. Email: fernán.restrepo@law.ucla.edu. For extremely helpful comments, I am very grateful to the editors of the Journal of Legal Studies (Ed Morrison and William Hubbard), an anonymous referee, Joseph Grundfest, Colleen Honigsberg, Michael Klausner, Alison Morantz, Lisa Ouellette, Guhan Subramanian, Randall Thomas, George Triantis, and workshop participants at Brooklyn, Chicago, Fordham, Georgetown, Michigan State, Pennsylvania State, University of California – Irvine, University of California – Los Angeles, University of San Diego, Vanderbilt, and Wisconsin.

The economic consequences of hedge fund regulation: An analysis of the effect of the Dodd-Frank Act

1. Introduction

Hedge funds are controversial investment vehicles. Historically, these vehicles were exempt from federal oversight because their funding comes from relatively sophisticated investors¹ who are generally thought to be able to “fend for themselves” and who, therefore, should not need government assistance to protect their investments.² But as demonstrated repeatedly over the last decades, investors in hedge funds are not immune from fraud and the financial system as a whole can be significantly affected by this subset of the industry. A salient example of this was Bernard Madoff’s Ponzi scheme, which involved a \$50 billion fraud in which, for years, Madoff paid returns to certain investors out of the principal received from other investors (Securities and Exchange Commission v. Bernard L. Madoff and Bernard L. Madoff Investment Securities LLC (S.D.N.Y. Civ. 08 CV 10791 (LLS)), Litigation Release No. 20834, December 19, 2008.). And the collapse of Long-Term Capital Management L.P., whose high leverage and large size ultimately required a bailout organized by the Federal Reserve Bank of New York in 1998, illustrates how the stability of the entire financial system can be affected by liquidity problems in the hedge fund industry (see, e.g., The President’s Working Group on Financial Markets, 1999).

In response to the 2007 financial crisis and other problems in the financial system (including the problems posed by hedge funds), in July 2010, Congress enacted the Dodd-Frank Wall Street Reform and Consumer Protection Act (hereinafter, the “DFA” or “Dodd-Frank”). Dodd-Frank is one of the most significant pieces of legislation ever affecting the financial sector and the hedge fund industry in particular. Broadly speaking, Dodd-Frank subjects most hedge funds to government inspections, registration with the Securities and Exchange Commission

¹ Hedge funds are commonly defined as funds that rely on the registration exemptions provided by either Section 3(c)(1) or Section 3(c)(7) of the Investment Company Act of 1940. See 15 U.S.C. § 80a-3(c)(1) and (7) (2019). To qualify for the exemption under Section 3(c)(1), the fund must have fewer than 100 beneficial owners and all the investors must be “accredited investors,” as defined by the SEC’s Regulation D, 17 C.F.R. § 230.501(a) (2019). To qualify for Section 3(c)(7), all the fund’s investors must be “qualified” investors. Broadly speaking, “accredited” investors are investors that have a minimum net worth or salary, and “qualified” investors are investors that have a minimum amount of investable assets.

² See, e.g., Staff Report to the United States Securities and Exchange Commission (2003, pp. 14, 87) (discussing the principle, set forth by the U.S. Supreme Court in the *Ralston Purina* decision, that securities sold to investors who “can fend for themselves” do not require federal registration). See also Prohibition of Fraud by Advisers to Certain Pooled Investment Vehicles; Accredited Investors in Certain Private Investment Vehicles, 72 Fed. Reg. 404, n.34 (proposed Jan. 4, 2007); SEC v. Ralston Purina Co., 346 U.S. 119, 125–27 (1953).

(“SEC”), and a number of disclosure and compliance obligations.³ Under the DFA, for example, hedge funds are required to maintain and report information on conflicts of interest, ownership structure, clients and officers, disciplinary history, assets under management, performance, risk exposure, and financing (17 C.F.R. § 275.203-1; Release No. I.A.-3308 (Oct. 31, 2011); 15 U.S.C. § 78m(f)(1)).⁴ And in the area of inspections, the newly registered funds became subject to the Presence Exam Initiative, a comprehensive federal examination program of the funds’ valuation methodologies, allocation of expenses to investors, and other business practices (Bowden, 2014; SEC, 2012; Wyatt, 2015).

According to the SEC and other commentators, the new obligations under the DFA were necessary to protect investors (especially from hedge fund valuation practices that result in inflated profits) and facilitate the control of systemic risk (Staff Report to the United States Securities and Exchange Commission, 2003; Aguilar, 2009; Department of the Treasury, 2009). But the hedge fund industry criticized the regulatory initiative quite vigorously, claiming that the new compliance requirements were not only unnecessary (due to the sophistication of hedge fund investors), but also very costly (see, e.g., Kaal, 2013 (reviewing these criticisms)). After the passage of the law, however, a large number of hedge funds also stated that they would absorb the new compliance costs and that, as a result, the DFA would not affect investors (KPMG, 2016).

The tension between hedge funds and public authorities over the passage of the DFA raises various questions. First, was Dodd-Frank associated with a meaningful reduction in profitability (and, if it was, what were the drivers of the association and was the reduction borne by investors)? Second, if there was a decline in profits, was the decline compensated by reduced risk-taking (i.e., reduced return volatility)? And third, did the law lead to less capital formation (i.e., less resources being channeled from sectors with excess of liquidity to sectors in need of liquidity) because managers closed funds or launched less funds in response to the regulatory change? Some of these issues have attracted the attention of other researchers, who have reached different conclusions (see Kaal et al., 2014; Cumming, Dai, and Johan, 2017). But as discussed in more detail in Section 2, when those papers were written, it was difficult to

³ Unless noted otherwise, when referring to *hedge funds*, I refer more specifically to the investment advisor of the fund. These terms are used here interchangeably for ease of exposition, but the fund and the advisor are separate legal entities. The fund is the vehicle holding the assets and the investment advisor manages those assets and designates other entities that provide additional services related to the operation of the fund.

⁴ Section 2 discusses the regulation of these disclosures in more detail.

distinguish which funds actually became subject to regulation from those that did not because the key data source to identify the regulatory status of a fund (namely, Form ADV) was not publicly available. With the availability of Form ADV today, however, it is now possible to estimate more precisely the effect of the law – so I provide such an analysis.⁵

The data show that, relative to a control group of funds that were already registered with the SEC or that were largely unregulated, the net profits going to investors in the funds that registered in response to Dodd-Frank decreased immediately after the implementation of the law. This decline can in fact be partially attributed to *direct compliance costs* (i.e., expenses that are directly necessary to meet regulatory obligations), but those costs do not seem to fully explain the results; part of the decline seems to be driven by collateral effects of compliance, which include diversion of managerial attention from core business activities and/or adjustments to financial valuation or reporting practices that ultimately result in revisions of performance estimations. The data also show that risk-taking did not change significantly after the DFA, and that although fund managers closed funds and launched less funds in response to the law, that behavior did not result in less capital formation (as measured by total assets under management).

To the extent that part of the decline in profits documented in this paper appears to stem from direct compliance costs and diversion of managerial time from a fund's business, then this paper documents an important cost of the DFA for the purposes of the cost/benefit analysis of the law. However, since part of the decline could be explained by adjustments to valuation or reporting practices, then that portion of the decline should actually be interpreted as a benefit of the law. In addition, on a more concrete policy level, the fact that some hedge funds predicted that direct regulatory expenses would not be passed on to investors suggests that it is worth examining whether the industry has been disclosing accurately how it allocates those expenses – and if the disclosures have not been accurate, then the results would lend support to current proposals that seek to require greater transparency in this area and additional controls (in the form of mandatory annual audits) (SEC, 2022). And this conclusion is bolstered by the finding that enhanced oversight post-DFA may have prompted funds to improve their valuation and reporting practices; that result was concentrated among funds that were not previously

⁵ I obtained historical Form ADV data for this project by filing a Freedom of Information Act (FOIA) request with the SEC. The data are now publicly available on the SEC's website.

audited, so the data suggest that the lack of audits can have a negative effect on reporting practices.

It is worth emphasizing that although I provide some suggestions on the possible magnitudes of the individual drivers behind the decline in performance, establishing exactly those magnitudes would require a fund-by-fund analysis. However, even without that specific information, this paper moves the literature forward because it shows that, inconsistent with some predictions, investors experienced a significant reduction in profitability following the DFA; because it documents the likely drivers of that decline; and because it shows that all those drivers have important implications and therefore merit further examination on a fund-by-fund basis.

The remainder of this paper is organized as follows. Section 2 provides background; Section 3 describes the methodology and data sources; Section 4 presents the results; Section 5 discusses the results; and Section 6 concludes.

2. Background

2.1. Institutional background

Historically, hedge funds were unregulated investment vehicles in the United States (Staff Report to the United States Securities and Exchange Commission, 2003, p. 21). The first attempt to change this came in December 2004 when, following a series of scandals involving excessive risk-taking, misrepresentation of profits, and improper valuation of assets, the SEC subjected most hedge funds to registration (Registration Under the Advisers Act of Certain Hedge Fund Advisers, Proposed Rule, Release No. IA-2266, File S7-30-04, RIN 3235-AJ25 (June 2004); Securities and Exchange Commission, Registration Under the Advisers Act of Certain Hedge Fund Advisers, 17 CFR Parts 275 and 279, Release No. IA-2266; File No. S7-30-04, RIN 3235-AJ25, July 20, 2004.). But this obligation had a short duration. In response to the new rule, popularly known as the Hedge Fund Rule, members of the hedge fund industry sued the SEC, alleging that the Commission did not have authority to regulate them; and shortly thereafter, the D.C. Circuit agreed with the plaintiffs, thus vacating the rule in *Goldstein v. SEC*, 451 F.3d 873, 884 (D.C. Cir. 2006).

The safe harbor created by *Goldstein*, however, only lasted a few years. In July 2010, the Dodd-Frank Act again subjected most hedge funds to regulation— and, this time relying on an express

legislative mandate, the SEC implemented the new rules in June 2011 (Rules Implementing Amendments to the Investment Advisers Act of 1940, 76 Fed. Reg. 42,950, 42,963-64, 42,982 (June 22, 2011); Exemptions for Advisers to Venture Capital Funds, Private Fund Advisers With Less Than \$150 Million in Assets Under Management, and Foreign Private Advisers, 76 Fed. Reg. 39,646, 39,666 (June 22, 2011)). The implementation rules provided that investment advisors managing hedge funds would have to register as either of two types of firms: *Registered Investment Advisors* (“RIAs”), the type of registered firm that already existed before Dodd-Frank, or *Exempt Reporting Advisors* (“ERAs”), a new type of registration category created by the act.

An investment advisor must register as an RIA if the advisor has over \$150 million in assets under management or if the state in which the advisor operates does not impose registration obligations or examinations, and this kind of registration generates three types of obligations:⁶ compliance requirements, government inspections, and disclosure obligations. *Compliance requirements* mainly include maintaining books and records for at least five years, the adoption of written compliance rules, the obligation to appoint a chief compliance officer, and the obligation to observe certain guidelines on performance fees and solicitation of new clients. *Government inspections* are onsite exams in which the SEC assesses whether a firm is observing its compliance requirements, but they can also take the form of simple information requests. And *disclosure obligations* mainly include filing Form ADV, in which all investment advisors must disclose extensive information about their structure and operation – including conflicts of interest, ownership structure, officers, clients, assets under management, accounting practices, and disciplinary history. Some advisors also must file Form PF and Form 13F.⁷

ERAs only have to file some portions of Form ADV and are exempt from inspections and compliance requirements (Dodd-Frank Act, § 407, § 408; Exemptions for Advisers to Venture Capital Funds, Private Fund Advisers With Less Than \$150 Million in Assets Under

⁶ At the time the DFA was implemented, Wyoming did not have registration requirements and New York did not examine investment advisors. Foreign investment advisors must register as RIAs if they have more than \$150 million in assets under management *from U.S. investors*.

⁷ Form PF is a new form created by the DFA. The form must be filed by investment advisors with U.S. assets under management over \$150 million and focuses on the advisor’s portfolio information – including performance, risk, and financing data. This form is confidential and is exempt from FOIA requests. Form 13F, in which all advisors with over \$100 million in applicable equity securities must disclose basic information about such holdings, was required before the DFA and is mandatory even if the advisor is not registered.

Management, and Foreign Private Advisers, 76 Fed. Reg. 39,646, 39,666, Release No. IA-3222; File No. S7-37-10 (July 6, 2011)). Investment advisors are eligible for this registration type if they only advise (1) venture capital funds or (2) private funds (hedge funds) with aggregate assets not exceeding \$150 million. Since only investment advisors with over \$100 million in assets under management are eligible to register with the SEC, ERAs' assets must be above that threshold. Foreign investment advisors can also register with the SEC as ERAs if they have less than \$150 million in assets under management *from U.S. investors* (17 C.F.R. § 275.203(m)-1).

2.2. Prior literature

The previous section illustrates that mandatory disclosure and public enforcement are the two main tools that Dodd-Frank employed to regulate hedge fund managers – an approach that is grounded on at least two theoretical arguments. On the one hand, disclosure requirements can deter misconduct (or excessive risk-taking) *ex ante* because insiders likely anticipate that their actions will be visible to investors and public authorities, and because if misconduct actually occurs, then the disclosures make it easier for investors to sue (e.g., Eisenberg, 1976; Mahoney, 1995; Armour, Hansmann, and Kraakman, 2009). And on the other hand, public enforcement complements disclosure rules because the prospect of a regulatory action raises the expected cost of wrongful actions and because that prospect can also prompt firms to improve the quality of their disclosures (see, e.g., Coffee, 2007; Karpoff, Lee and Martin, 2008, p. 582; Jackson and Roe, 2009).⁸ The SEC, in fact, explicitly relied on these justifications from the first moment it tried to regulate the hedge fund industry, predicting that greater transparency and the prospect of surprise examinations would “encourage a culture of compliance” and lead to an easier and earlier detection of intentional and inadvertent irregularities – especially in the area of performance reporting and valuation of assets (SEC Staff Report, 2003, pp. 76-77, 93).⁹

⁸ The empirical literature on public enforcement and mandatory disclosure is vast (see, e.g., Leuz and Wysocki, 2016; Christensen, Hail, and Leuz, 2013, 2016). But specifically in the areas of hedge fund regulation and the kinds of disclosures contained in Form ADV (i.e., information on voluntary corporate governance), there are various studies that lend support to the arguments described above. On the relationship between hedge fund regulation and various positive outcomes, see Cumming and Dai (2010); Hoffman (2013); Dimmock and Gerken (2015); and Honigsberg (2019a). On the relationship between disclosure of corporate governance information and positive outcomes, see Akkermans et al. (2007); Armour (2008); Arcot, Bruno, and Faure-Grimaud (2010).

⁹ According to staff members of the SEC, “The lack of independent checks on a hedge fund adviser’s valuation of a hedge fund’s portfolio securities is among the most serious concerns we have identified (...) Hedge fund advisers have powerful incentives to achieve superior (and positive) performance. The requirement to achieve positive performance in order to receive a performance allocation (and to remain in business) is just one such incentive (...) Hedge fund advisers that perform well find it easier to retain investors and raise additional capital. A hedge fund adviser has broad discretion to value these securities; hedge funds need only value its portfolio securities in a manner consistent with the valuation policies and guidelines they disclose to their investors (...) Because many hedge funds invest in highly illiquid securities, these concerns are heightened (...) Our

The benefits of hedge fund regulation are controversial, though, because investors in hedge funds are often thought to be relatively sophisticated and thus able to “fend for themselves” without the need of regulatory disclosures; because the SEC has limited resources to conduct systematic and successful inspections; and because producing regulatory disclosures and responding to inspections is costly – which, according to this line of commentary, can even push funds to leave the industry. For example, former SEC Commissioner Paul Atkins argued that “Form ADV is unlikely to provide any new information to investors who performed even the most minimal level of due diligence,” that “overseeing an ever-growing body of registrants with a small pool of examiners [is] a Herculean task,” and that the registration mandate might lead investment advisors to shelve plans to join the industry (Atkins, 2006). And the business press expressed as follows the concern about the costs generated by the DFA: “[f]rom the perspective of hedge fund managers, the Dodd-Frank Act is, in so many ways, a huge drag. The law requires them to register with the Securities and Exchange Commission. To supply reams of sensitive data on trading positions. To screen potential investors more carefully. To hire compliance officer after compliance officer. How could all of this not eat into profits and hamstring competitiveness?” (Summers [2012]). Several members of the industry also stated, however, that they would absorb the regulatory expenses created by the DFA and that, as a result, the law would not affect the investors in the industry.¹⁰

But despite the significance of the controversy on the costs and benefits of hedge fund regulation, the empirical evidence on the topic remains scarce. There are two prior papers that examine the effect of the DFA on hedge fund performance and, in the case of one of them, on

examinations confirmed that in many cases controls at some hedge fund advisers are very informal. We believe that the prospect of a compliance examination by the Commission staff will result in the adoption of procedures and controls designed to fulfill the hedge fund adviser’s fiduciary responsibilities to the hedge fund and its investors...” (Staff Report to the United States Securities and Exchange Commission [2003, pp. 79-80]). Similarly, after the DFA was implemented, the SEC continued to reiterate that preventing portfolio misrepresentation would be one of the commission’s guiding principles to determine how to allocate its enforcement resources. In the words of Robert Khuzami, former Director of the SEC’s Division of Enforcement, “[a]nybody who is beating the market indexes by 3 percent and doing it on a steady basis, we are going to look for them.” See Hearing before the Subcommittee on Capital Markets and Government Sponsored Enterprises of the Committee on Financial Services, U.S. House of Representatives, 112th Congress (March 10, 2011) (statement of Robert Khuzami, SEC Director of Enforcement); Walter (2013).

¹⁰ Kaal (2016, p. 307) finds that approximately 76% of the respondents said that Dodd-Frank would not affect the returns of investors, but 78% said that the profits of the manager were affected. In addition, he noted that “[i]t is unclear whether and how the increased expenses will be passed on to investors over time.” In a separate survey, KPMG (2013) similarly found that approximately 70% of the respondents said that they would absorb the cost of compliance, and concluded that “overwhelmingly, managers are absorbing [such costs] rather than passing [them] on to their funds.”

volatility. The first paper compares funds with assets under management (“AUM”) above \$150 million (which, as mentioned in Section 2.1, are generally required to register with the SEC) and funds with AUM below \$150 million (which are generally exempt from federal registration), and finds that the DFA did not have a negative effect on hedge fund returns (see Kaal et al., 2014). The second study compares U.S. funds (which the authors define as the treatment group) and non-U.S. funds (the control group), and finds that Dodd-Frank appears to have caused a reduction in performance and volatility. Based on this result, the authors conclude that although the financial system benefited from the regulatory change in the form of less risk-taking, “the costs of the regulation are substantial and may have outweighed the benefits (...) thus representing a partial government failure” (Cumming, Dai, and Johan, 2017).

One important limitation of these studies, however, is that their classifications of the treatment and control groups do not necessarily capture the effect of Dodd-Frank. In the case of the study by Kaal et al. (2012), there are investment advisors with more than \$150 million in AUM that were already registered with the SEC prior to March 2012,¹¹ and there are also investment advisors with less than \$150 million in AUM that must register with the commission.¹² As a result, the treatment group is over-inclusive and the control group is under-inclusive.¹³ In the case of Cumming, Dai, and Johan’s (2017) study, the treatment group is also over-inclusive because, as mentioned, there are many U.S. funds that were already registered with the SEC prior to the DFA;¹⁴ and the control group is under-inclusive because there are foreign funds that have to register (namely, the funds whose investment advisors have over \$150 million in AUM from U.S. investors).¹⁵

¹¹ This is the case because investment advisors could submit to registration voluntarily and because some of them advise mutual funds (in which case registration is mandatory). Investment advisors could submit voluntarily to regulation before the DFA, for example, to signal safety to investors, to satisfy demands of registration made by investors, or to achieve the status of Qualified Professional Asset Manager under the Employee Retirement Income Security Act of 1974 (ERISA), which allows fund managers to engage in certain types of transactions that would otherwise be prohibited under ERISA (see, e.g., Atkins [2006a, p.548-9]).

¹² As discussed in Section 2, this occurs when investment advisors operate in states that do not require registration or examination of investment advisors.

¹³ This study also has two additional limitations. One is that investment advisors with assets under management between \$100 million and \$150 million must register with the SEC but they are eligible to register as ERAs; however, the study does not take into account this form of registration. The other limitation is that the authors appear to classify funds as treatment or control observations based on AUM at the fund level, not at the investment advisor level. The DFA, however, requires federal registration of an investment advisor *if the advisor* (not each individual fund) has \$150 million or more in AUM.

¹⁴ In addition, the authors do not take into account that U.S. advisors with AUM below \$150 million were required to register as ERAs, not as RIAs, and that advisors with AUM below \$100 million were generally exempted from federal registration.

¹⁵ To illustrate the implications of these observations, only 12% of the U.S funds in my sample registered as RIAs in response to Dodd-Frank. Another 6% registered as ERAs and the rest were already registered. Similarly, only

Besides these considerations, there are two additional aspects that could affect the results in the two studies. First, in both of them, the cutoff for the difference-in-differences analysis is March 2012, the deadline to register with the SEC under the DFA. Using this date, however, ignores both the fact that some funds that registered in response to the DFA did so before the registration deadline and the fact that funds managers might have changed their reporting behavior when they began preparing for registration. And second, comparing U.S. with non-U.S. funds has the drawback that the two types of funds are subject to different regulatory environments and may also be affected by different economic fluctuations, which might again confound the estimations. Cumming, Dai, and Johan (2017, p.15), in fact, note this limitation, emphasizing that “[t]he interpretation of economic conditions in the US versus other parts of the world as a parallel trends counter-argument to our Dodd-Frank results warrants additional scrutiny in future research.” In this study, I respond to that call for additional scrutiny – and address additional questions that were not addressed in the previous studies.¹⁶

3. Data and methodology

To examine the effect of the DFA, this work employs a difference-in-differences (“DiD”) methodology that compares changes over time (before and after the DFA) in the performance, volatility, and structure of the funds that became subject to full federal oversight (the treatment group) with a group of funds unaffected by the law (the control group).

Following Honigsberg (2019a), the treatment group includes all the funds that submitted to SEC oversight within the six months preceding the deadline imposed by the DFA. I take this approach because registration requires preparation and therefore funds that registered immediately after the DFA was implemented are likely to be voluntary registrants that began registration before the implementation rules were adopted. The results are qualitatively similar, however, if the treatment group includes all the funds that submitted to federal oversight between the implementation date and the registration deadline.

25% of the funds that registered as RIAs were domiciled in the U.S. and only 15% of the funds whose investment advisor had AUM over \$150 million were new registrants (this figure considers only RIAs because ERAs do not report their AUM on Form ADV).

¹⁶ It is worth emphasizing that all these observations about the previous papers are not meant to criticize them but rather to explain why there is still a gap in the literature in this area. As mentioned earlier, the key data source used here to identify the actual registration status of a fund (i.e., Form ADV) was not publicly available when those papers were written.

The baseline control group includes (1) all the funds that were continuously registered with the SEC during the entire sample period (30 months before and after the implementation of the DFA), and (2) funds that registered with the SEC as *Exempt Reporting Advisers* – which, as mentioned in Section 2, are subject to relatively light disclosure obligations and are exempt from government inspections and compliance requirements. Since even the general disclosure obligations imposed on ERAs might have affected those funds, however, I also run the regressions using the two subsets of the control group separately.

The DiD estimations are in the form of panel regressions with fund fixed effects and all the inferences are based on standard errors clustered by fund.¹⁷ In particular, the regressions are based on the following equation:

$$Performance_{it} = \phi_i + \lambda_t + \beta Treat_i \times Post_t + \varepsilon_{it}, \quad (1)$$

where $Performance_{it}$ is the performance of fund i in month t ; $Treat_i \times Post_t$ is the difference-in-differences estimator (that is, an interaction between $Treat_i$, an indicator variable for funds in the treatment group, and $Post_t$, an indicator variable for post-DFA observations); ϕ_i is a vector of fund fixed effects (which account for unobserved time-invariant fund characteristics and therefore ensure that β captures within-fund changes in performance); and λ_t is a vector of month fixed effects (which account for overall time trends in performance). The time cutoff to define an observation as pre- or post-DFA in the $Post_t$ variable is the date the SEC implemented the DFA (June 2011).¹⁸

This paper studies two measures of $Performance_{it}$. The first is the net-of-fees monthly return and the second is the eight-factor alpha (which accounts for general market fluctuations). I

¹⁷ The results are similar, however, if the regressions are run using other types of errors, including Driscoll-Kraay (1998) errors (which account for cross-sectional dependence), Newey-West errors (1997) (which account for autocorrelation), or bootstrap errors (which do not assume that the theoretical distribution of the test statistics is known).

¹⁸ The stand-alone $Treat_i$ and $Post_t$ variables are not included in the regression because they are collinear with the fund and time fixed effects, respectively. Similarly, other time-invariant fund-level characteristics are collinear with the fund fixed effects and therefore they are also excluded from the model. The sensitivity analyses, however, include random-effects specifications with controls for time-invariant fund characteristics, including, in particular, the following characteristics: management fees, incentive fees, lockups, leverage, high-water marks, location (U.S. funds versus foreign funds), investment strategy, average fund age, and pre-DFA volatility, net asset value, and sensitivity to market liquidity (measured by the beta with respect to the Pástor and Stambaugh [2003] market-wide liquidity measure). The investment strategies are described in Appendix 1.

compute the alphas using the augmented Fung and Hsieh (2004) risk factors,¹⁹ which I obtain from the following regression:

$$R_{it} - R_{ft} = \alpha_i + \sum_{j=1}^n \beta_{ij} F_{ij} + \varepsilon_{it}, \quad (2)$$

where R_{it} is the return of fund i in month t , R_{ft} is the risk-free rate in month t , β_{ij} is the factor loading of the returns of fund i on the j -th risk factor, F_{ij} , during the 30-month window preceding the month of the event, and ε_{it} is the error term. The monthly alpha of fund i , α_i , is calculated as the difference between realized returns and model-fitted returns.²⁰

As mentioned before, I estimate equation 1 using a time window of 30 months before and after the month in which the SEC adopted the rules implementing Dodd-Frank.²¹ To mitigate bias resulting from changes in the composition in the sample, I only include in the regressions funds that survived during the entire sample period.²² However, as part of the sensitivity analyses (as well as in the analysis of capital formation), I also run the regressions using all the funds in the sample.

The methodology to estimate changes in volatility following the DFA is similar to the methodology presented in equation 1 and therefore this part of the analysis is based on the following equation:

$$Risk_{it} = \phi_i + \beta_1 Treat_i \times Post_t + \varepsilon_{it}, \quad (3)$$

where $Risk_{it}$ is the risk of fund i in period t (before or after Dodd-Frank), and the remaining variables are defined as in equation 1. As in previous literature, I employ two proxies for $Risk_{it}$: *Total volatility* and *Idiosyncratic volatility* (see, e.g., Cumming, Dai, and Johan, 2017). *Total volatility* is the standard deviation of monthly returns and works as a proxy for the overall risk

¹⁹ The Fung and Hsieh (2001, 2004) risk factors, F_{ij} , include two equity-oriented risk factors, two bond-oriented factors, three trend-following factors, and an emerging markets factor. For additional details, see also Hsieh (2016).

²⁰ Unreported analyses show that the results are qualitatively similar with other performance evaluation methods, including the Bali et al. (2012) nine-factor alpha, the Capital Asset Pricing Model (CAPM), the Fama-French (1993) three factor model, and the Carhart (1997) four-factor model.

²¹ The total number of months used in the analysis is actually 61 because the month of the event is added to the post-DFA observations. The results are similar, however, if this month is removed from the analysis or if it is added to the pre-DFA observations.

²² Therefore, I exclude from the baseline estimations funds that stopped reporting to the Lipper database (the data source on monthly performance), funds that withdrew from registration with the SEC, and funds that changed their registration status (from ERA to RIA or vice versa).

exposure of a hedge fund. *Idiosyncratic volatility* is the standard deviation of the residuals from the Fung and Hsieh (2004) eight-factor model and works as a proxy for the risk exposure of the fund that is not attributable to general market fluctuations. To account for potential heteroskedasticity in the time series component of the data, I examine not only changes in *unconditional* volatility using the baseline fixed effects model, but also changes in *conditional* volatility using Autoregressive Conditional Heteroskedastic (ARCH) models (see, e.g., Engle, 1982; 2001).²³

The methodology to estimate changes in the capital formed by the industry is based on an analysis of changes in the industry's structure and is also similar to equation 1, so the regression equation takes the following form:

$$CapitalFormation_{it} = \beta_0 + \beta_1 Treat \times Post_t + \varepsilon_{it}, \quad (4)$$

where *CapitalFormation_{it}* is one of three variables: closing a fund, launching a new fund, or assets under management. I estimate these regressions using ordinary least squares (OLS) when the dependent variable is fund exits or fund entries, and a fixed-effects model when assets under management is the dependent variable.²⁴ In addition, as mentioned, I use the full sample in these estimations (that is, not just the funds that survived during the 60-month period surrounding the DFA).

The estimations of equations 1 and 3 are based on the entire sample and also on a matched sample. The matching procedure matches treatment funds with control funds on the basis of their returns and volatility at the beginning of the sample period, and on the basis of other observable characteristics, including investment style, location, leverage, age, management fee, incentive fee, net asset value, sensitivity to market liquidity, and the type of registration of

²³ The ARCH estimations allow me to examine how volatility changes in the current period after conditioning on volatility in previous periods. The basic intuition behind the model is that the variance of the returns within each hedge fund may not be constant over time (that is, it may be heteroskedastic) and therefore the model treats heteroskedasticity as a variance to be modeled. I employ an ARCH model with one autoregressive lag (i.e., an ARCH(1) model) in all the reported estimations, but the results are similar if I employ a GARCH(1,1) model (that is, a model with one autoregressive lag and one moving average component). I use the ARCH(1) model as the baseline estimation because the time series component of my panel is not very long and, as a result, the model converges more frequently than the GARCH(1,1) model – which is more demanding computationally.

²⁴ In all these analyses, a fund exits or enters the market if the fund leaves or is added to the Lipper database. I use OLS in the analysis of fund exits and entries because previous literature suggests that the magnitude of the bias in fixed-effects models can be larger than the bias in OLS models in discrete-time hazard settings (Tauchmann, 2019). In addition, I do not use proportional hazard models for ease of interpretation – but those models yield similar conclusions.

the investment advisor with the SEC. The analyses based on equation 4 employ the full sample because the relevant unit of analysis in that case is the universe of funds that exited or entered the market at any time during the sample period.

I use various sources of data for this project. The data on monthly fund performance and general characteristics of the funds come from the Thomson Reuters Lipper database. The information on the regulatory history of each investment advisor comes from the Form ADV database. The data on risk factors to compute the alphas come from David Hsieh's data library, the Federal Reserve Bank of St. Louis, and Datastream.

4. Results

4.1. Summary statistics

Table 1 provides summary statistics and Appendix 1 defines all the variables. As shown in the table, there are significant differences between the treatment and control groups over some dimensions (i.e., NAV, incentive fees, leverage, redemption notices, age, and audits). As mentioned in Section 3, therefore, I mitigate these differences by using not only the full sample but also a matched sample in all the estimations. In addition, the sensitivity analyses include random effects regressions with explicit controls for all the observable characteristics.

Table 2 provides univariate tests on the performance and risk variables, as well as on the variables capturing structural changes in the industry. As shown in the tables, monthly returns and alphas decreased in the treatment group relative to the control group after the DFA. Both performance variables actually declined in the two groups, but the decline was larger for the treatment funds. Monthly returns decreased 0.79 percentage points in the treatment group and 0.41 in the control group, implying a relative decline of 0.38 percentage points among the treated observations. Similarly, alphas decreased 0.45 percentage points in the treatment group and 0.17 points in the control group, implying a relative decline of 0.28 percentage points in the treatment group. Since, as mentioned in the previous section, these computations focus on changes in net-of-fees performance, the results imply that the relative reduction in the performance of the newly regulated funds affected not just the fund managers – it also affected the investors.

The risk variables also declined after the DFA, but the changes in the treatment and control groups were generally similar. With respect to changes in the composition of the industry, fund

exits in the treatment group increased in relative terms, and although the incidence of entries declined significantly in both groups, this decline was stronger among the treatment funds. However, the overall capital formed by the industry does not seem to have been affected by the DFA, as the relative assets of the treatment funds actually increased after the law. These results then provide preliminary evidence that fund managers anticipated that the DFA would be a costly event and therefore they reacted to the law by changing their business structure; however, these changes generally resulted in greater consolidation but not in less resources being channeled from sectors with excess of liquidity to companies in need of liquidity.²⁵

4.2. Regression results

Table 3 presents the regressions on the association between the DFA and the performance and risk-taking of the sample funds. Panel A reports the results for raw returns and alphas, and Panel B presents the results for volatility. Models 1 and 2 of each panel present the regressions using the full sample and Models 3 and 4 present the results for the matched sample. As shown in Panel A, the difference-in-differences estimator was negative and statistically significant (generally at the 1% level) in all the estimations – therefore confirming that, after controlling for general economic fluctuations and time-invariant fund characteristics, the reported performance of the treatment group declined in relation to the control group. And the magnitude of this decline was similar to the one produced by the univariate tests: 0.37 and 0.28 percentage points for raw returns and alphas, respectively (when the full sample is considered), and 0.36 points for both returns and alphas (when the matched sample is considered). In contrast to these results, Panel B shows that there was no significant change in the relative volatility of the treatment group post-DFA.

Whether or not the relative decline in the treatment group's performance presented in Table 3 can be attributed to the DFA depends in large part on the validity of the parallel trends assumption – that is, the assumption that the treatment and control groups were following similar trajectories before the DFA. Figure 1, which shows the trajectories of the performance variables over time, suggests that the assumption in fact appears to be satisfied (as the two groups exhibit similar movements).²⁶ But to examine more formally this observation,

²⁵ This result is to some extent similar to reorganization patterns identified by the SEC in the context of the private equity industry. The composition of the funds marketed changed in that industry between 2012 and 2014, but the money raised actually increased. According to Mark Wyatt, Director of the Office of Compliance and Examinations of the SEC, these facts “appear to be a reflection of the natural maturing and consolidation of the industry” (Wyatt, 2016).

²⁶ The data in the figure are smoothed by a moving average of nine periods, and the reference line is June 2011.

Appendix 2 shows analytical tests, which confirm that the assumption is reasonably satisfied: the performance of the treatment group only declined in relation to the control group after the implementation of the DFA, and this effect was sharp around that date.²⁷

Table 4 presents the regressions on the association between the DFA and the measures for changes in the composition of the industry. In line with the univariate results, the regressions show that the treatment group experienced a relative increase in fund exits and a relative decline in fund entries after the DFA – and Figure 2 further illustrates that these results took place immediately after the registration deadline under the act.²⁸ However, as mentioned earlier, these changes did not result in lower aggregate assets under management.

Since Table 3 shows that there was a significant post-DFA decline in performance and the decline was consistent across all the specifications, I ran various sensitivity analyses on that result, which are presented in Table 5. First, to ensure that the results are not driven by the selection of the control group, I ran the regressions using pre-existing RIAs and ERAs as separate control groups. Second, I used random effects instead of fixed effects regressions, which allows me to include explicit controls for fund characteristics that do not change over time.²⁹ Third, to mitigate the potential effect of factors unrelated to the DFA, I used a shorter time window around the implementation of the act.³⁰ And fourth, I ran the regressions using all the funds available instead of only the funds that survived during the entire sample period, which mitigates potential survival bias. As shown in the table, the results from all these

²⁷ Figure 1 and Table A2.1 show that the decline in reported profits in the treatment group became especially pronounced approximately nine months after the implementation of the DFA. This result is probably explained by the fact that although the law was implemented in June 2011, the treatment funds registered with the SEC in the period between that month and March 2012 – the deadline to register with the commission. In fact, 88% of the treatment funds registered in the period between February 2012 and March 2012. In this sense, the data suggest that although the treatment funds started to become affected by the DFA after the implementation of the law, their actual registration was an important determinant of the effects of the law.

²⁸ The figure shows the monthly incidence of fund exits and entries in the treatment group and the control group. I define the monthly incidence of the variables as the number of exits or entries within each group in a given month, divided by all the exits or entries within the corresponding group – but the basic patterns are similar if I use all the funds available in a month as the denominator, or if I use all the exits or entries across the two groups as the denominator.

²⁹ As mentioned earlier, time-invariant characteristics are omitted from the fixed-effects model due to collinearity with the fixed effects. These characteristics include the following: the size of the management fee and the incentive fee; investment style; sensitivity to market liquidity; and indicator variables for high-water marks, leverage, lockups, and fund domicile in the U.S.

³⁰ These estimations are based on a window of 21 months before and after the implementation date in order to include at least one year after the deadline *to register* (March 2012) – not just after the implementation date.

estimations were qualitatively consistent with the baseline estimations, which underscores the stability of the results.³¹

4.3. The mechanisms behind the drop in performance

The decline in performance documented in the previous sub-section raises the question of what caused that decline. One explanation is compliance costs, as hedge funds must necessarily spend resources on compliance. As mentioned in Section 2, after the DFA, the industry must appoint a compliance officer, prepare written compliance rules, implement recordkeeping procedures, prepare new performance and risk disclosures, implement new guidelines on performance fees and solicitation of new clients, and, in general, ensure that all the financial and governance disclosures would withstand an inspection by the SEC – all of which is likely to affect the profitability of a fund. Some commentators emphasized that these obligations generate “real costs,” especially because they are prepared in the shadow of scrutiny by the SEC and private litigants;³² and despite the industry’s claims that most hedge funds would absorb the costs, some observers anticipated that they would be “passed on to investors in one way or another” (Atkins, 2006). This possibility is feasible especially in light of the fact that the SEC has found that hedge funds’ disclosures on how fees and expenses are calculated and allocated “are often opaque” and “do not provide investors with sufficiently detailed information,”³³ and the commission has in fact pursued various enforcement actions for false and misleading information on these aspects.³⁴

³¹ As discussed in Section 3, untabulated sensitivity analyses also include alternative methods to compute the standard errors, which are intended to account for cross-sectional dependence and autocorrelation. In addition, the untabulated analyses include alternative risk models to compute the alphas, including the Capital Asset Pricing Model (“CAPM”), the Fama-French (1993) three factor model, and the Carhart (1997) four-factor model. In all these estimations, the difference-in-differences estimator remained negative and statistically significant at conventional levels, which confirms the stability of the results.

³² For instance, in the words of former commissioner Paul Atkins, “[t]he registration mandate ... is likely to impose real costs on investors. Proponents of mandatory registration have insisted that registration imposes relatively few costs on advisors. After all, registration supposedly is nothing more than filing out a form. But even filing out a form can be expensive if it will be submitted to a regulator and scrutinized by the trial bar... [And on top of registration,] advisors face numerous substantive requirements ... all of which impose costs” (Atkins, 2006).

³³ For example, the SEC has found that hedge funds are sometimes unclear on the methodology they use to compute their fees, that they do not explain the labels they use for various items, and that they provide long lists of potential fees and expenses “without giving details on [their] magnitude and scope” (SEC, 2022, p. 10). Similarly, practitioners have noted that hedge funds sometimes “deal with expenses” by developing “arrangements that permit the manager to pass [those] expenses through to [investors] as add-ons to the management fee” (Griffith, 2009).

³⁴ See, e.g., *SEC v. Joseph W. Daniel*, Litigation Release No. 19427 (Oct. 13, 2005) and *In re Joseph W. Daniel*, Investment Advisers Act Release No. 2450 (Nov. 29, 2005); *In the Matter of Swapnil Rege*, Investment Advisers Act Release No. 5303 (July 18, 2019).

However, even if direct costs are a driver of the post-DFA decline in performance, those costs do not seem to fully account for the results, as the drop in profits that I estimate here is considerably larger than what survey studies estimated to be the cost of compliance. KPMG (2013), for example, estimated that hedge funds would spend between 0.10% and 0.40% of their assets under management per year on direct compliance costs, and another study estimated these costs to be 1% of AUM (KPMG, 2013, p. 15; Kaal, 2016). In contrast, I estimate that the average *monthly* performance of the treatment group declined by 0.373 percentage points after the DFA – approximately 4.5% of AUM per year.³⁵

A potential explanation for this gap is that collateral effects of compliance explain part of the decline in performance. One of such effects is the opportunity cost associated with the diversion of managerial time and expertise from core business activities to regulation. Commentary by practitioners and members of the industry in fact predicted that overseeing compliance with Dodd-Frank would demand considerable time and effort from managers,³⁶ and at least one regulator anticipated that “the indirect costs [of the DFA] on U.S. investors might be even more significant than the direct costs” (Atkins, 2006). In addition, prior literature also suggests that the costs of managerial “distractions” like the ones caused by regulatory compliance are not trivial in terms of lost profitability and market value (Core et al., 1999; Fich and Shivdasani, 2006; Field et al, 2013; Falato et al., 2014; Masulis and Zhang, 2019). Some studies, for example, estimate that “distractions” affecting the directors of public companies are associated with an annual profitability reduction of three percentage points (Masulis and Zhang, 2019),³⁷ and other estimations suggest that unexpected events causing “distraction” are associated with a stock market decline of around 1.74%. These results are in line with theoretical predictions, which hold that diverting managerial talent to activities that are outside the core business of a firm imply that the business will receive less monitoring and industry-specific knowledge, which should in turn result in lower profitability (Masulis and Zhang, 2019).

³⁵ KPMG also predicted that the size of direct compliance costs would change with the size of the investment advisor, with small advisors spending 0.40%, medium-size advisors spending 0.30%, and large advisors spending 0.10% of their AUM on compliance costs. However, in line with subsequent survey data (e.g., Kaal 2016), I do not find an inverse relationship between the size of the investment advisor and the magnitude of compliance costs.

³⁶ See, e.g., Kaal (2016, p. 208) (documenting that hedge funds predicted that “opportunity costs because of distraction from core fund management” would be an important cost of the DFA).

³⁷ For other evidence consistent with this argument, see Bennedson et al. (2010); Malmendier and Tate (2009), and Hauser (2018). Some studies find, however, that the number of directorships held by a director (which some authors use as a proxy for director busyness) is not related to negative outcomes. See Ferris, Jagannathan, and Pritchard (2003); Perry and Peyer(2005); Field, Lowry, and Mkrtchyan(2013).

Another collateral effect of compliance is the possibility that hedge funds revised their financial valuation and reporting practices, which in turn translated to lower reported profits. As mentioned earlier, one of the main goals of the DFA (and one of the enforcement priorities of the SEC after the implementation of the law) was the prevention of inadvertent and intentional misreporting (SEC, 2003), so it is likely that as part of the adjustment to the new regulatory obligations, hedge funds paid particular attention to this area. This is actually an effect that the industry anticipated and welcomed,³⁸ and a possibility that is supported by prior empirical evidence, which has found a negative relationship between public oversight of the industry and some misreporting proxies (Dimmock and Gerken, 2015; Honigsberg, 2019a).³⁹ It is worth emphasizing, however, that although the record of SEC enforcement actions illustrates that intentional inflation of profits can (and does) occur,⁴⁰ the data do not necessarily suggest that the industry was systematically engaging in intentional misreporting; in fact, to the extent that part of the post-DFA reduction in reported profits was the product of adjustments to reporting practices, it seems reasonable to assume that those adjustments were most likely corrections of inadvertent mistakes or weaknesses caused by informal or insufficient internal controls, or just improvements in the methodologies or assumptions to estimate the returns.

³⁸ For example, Rob Mirsky, then global head of KPMG's hedge fund practice, stated that shortly after the implementation of the DFA, some hedge fund managers with which KPMG has spoken thought that the DFA brought "some improvement to process." See Lovell (2013).

³⁹ The proxies are the "discontinuity around zero" or "kink at zero," and the "December spike." The "kink at zero" measures whether a fund reports fewer monthly returns just below zero than would be expected based on the number of monthly returns just above zero. The intuition behind this proxy is that fund managers have incentives to turn small losses into small gains because the number of months with positive returns influences fund inflows (see Burgstahler and Dichev, 1997; Bollen and Pool, 2009, 2012; Agarwal, Daniel, and Naik, 2011). The "December spike" attempts to capture whether a fund underreports positive performance throughout the year in order to have a buffer for periods of bad performance. Managers have incentives to recognize this buffer in December for two reasons: because better annual performance increases managers' compensation and also attracts investor inflows; and because, if a hedge fund is audited, the audit usually takes place at the end of the year, implying that managers will want to bring their financial statements into compliance before the year closes (see Agarwal, Daniel, and Naik, 2011; Honigsberg, 2019a). Dimmock and Gerken (2015) and Honigsberg (2019a) examine the relationship between regulatory change in the United States and misreporting proxies. In a related study, Leuz, Wanda, and Wysocky (2013) studied various misreporting proxies across several countries, and found that indications of misreporting were less common in countries that appear to have stronger investor protection.

⁴⁰ See, e.g., SEC v. Michael Lauer, Lancer Management Group, LLC, and Lancer Management Group II, LLC, Litigation Release No. 18247 (July 23, 2003); SEC v. Peter W. Chabot, Chabot Investments, Inc., Sirens Investments, Inc., Sirens Synergy and the Synergy Fund, LLC, Litigation Release No. 18214 (July 3, 2003); SEC v. David M. Mobley, Sr., Litigation Release No. 18150 (May 20, 2003); In the Matter of Charles K. Seavey, Advisers Act Release No. 2119 (Mar. 27, 2003); SEC v. Hoover and Hoover Capital Management, Inc., Litigation Release No. 17981 (Feb. 11, 2003); SEC v. Beacon Hill Asset Management LLC, Litigation Release No. 17831 (Nov. 7, 2002); SEC v. Edward Thomas Jung, Litigation Release No. 17417 (Mar. 15, 2002); SEC v. Jerry A. Womack, Litigation Release No. 17293 (Jan. 2, 2002); SEC v. Michael W. Berger, Manhattan Investment Fund, Ltd., Manhattan Capital Management Inc., Litigation Release No. 17193 (Oct. 16, 2001).

All these explanations for the post-DFA decline in performance are consistent with the restructuring efforts undertaken by the hedge fund industry shortly after the DFA. As mentioned earlier, even though Dodd-Frank did not result in less capital formation, the law did exhibit a significant association with increased fund exits and less fund entries, which can be interpreted as evidence that fund managers anticipated significant costs related to the law and demands in terms of time and effort – all of which ultimately prompted managers to consolidate their businesses. However, to test more formally the association between the post-DFA decline in profits and the direct and collateral effects of compliance, I performed various analyses of heterogeneity in the treatment effects of the law.

The first tests examines whether the newly regulated funds that were not audited before the DFA experienced a stronger decline in performance than the funds that were audited. The funds that were not audited were probably less prepared to file financial and governance disclosures that would withstand an inspection by the SEC, so it is likely that those funds had to invest more on compliance (directly and in the form of managerial oversight) and that they were more exposed to the need of revising their valuation or reporting practices.

The second test explores whether, within the subset of unaudited funds, the reduction in the profits of the newly regulated funds was stronger among the funds that had a higher concentration of hard-to-value assets – that is, assets whose value is not determined by general market benchmarks. If the value of the assets of a fund is not determined by general market benchmarks, then the cost of identifying and monitoring that value will be higher, and fund managers will also have greater discretion in the task of valuation – which increases the risk of misreporting (Cassar and Gerakos, 2011; Dietrich, Harris, and Muller, 2001).⁴¹

The third test examines whether the reduction in the returns of the newly regulated funds was stronger among investment advisors that managed a relatively high number of funds – that is, a number above the median value. A higher number of funds could make a business structure more complex and difficult to manage, which can amplify the direct and indirect costs of compliance, and which can make the tasks of valuation and reporting more difficult. In fact,

⁴¹ My proxy for hard-to-value assets, *LowCommonRisk*, is an indicator variable that takes the value of one if a fund's returns have a correlation with general market benchmarks that is below the median value in the sample and zero otherwise. I define a fund's correlation with general market benchmarks as the R^2 from regressing the funds' returns on the Fung and Hsieh (2004) eight common risk factors.

survey evidence has documented a positive relationship between the costs of compliance and the size of the investment advisor (Kaal, 2016).

Table 5 presents the results. As shown in the table, all the triple interactions that measure heterogeneity in the treatment effects (i.e., $Treat \times Post \times NoAudit$, $Treat \times Post \times LowCommonRisk$, and $Treat \times Post \times HighFundsManaged$) were negative and statistically significant, which confirms that the post-DFA decline in the performance of the treatment group was stronger among the unaudited funds, the funds with a high concentration of hard-to-value assets, and the investment advisors that manage a relatively high number of funds. This in turn confirms, as mentioned, that direct and indirect effects of compliance are plausible explanations for the changes in profitability.

As mentioned earlier, survey studies estimate that the direct costs of compliance are between 0.1% and 1% of AUM per year, but I estimate that the DFA was associated with a reduction in reported profits of approximately 4.5% – which in turn suggests, as Atkins (2006) predicted, that most of the decline is explained by indirect effects of compliance. However, it is difficult to determine how exactly the different types of indirect effects contribute to that magnitude, and it is even difficult to determine the exact impact of the direct costs because some managers might have partially absorbed those costs and because they might have incentives to invest in compliance even more than the estimated magnitudes suggested by the survey studies.⁴² Assuming that the direct costs of compliance are between 0.1% and 1% of AUM, and assuming that the opportunity cost of diversion of managerial time is 3% (as Masulis and Zhang, 2019 suggest), then adjustments in valuation and reporting practices would account for 0.5% to 1.4% of AUM. However, these are just suggestive estimations and therefore a precise decomposition of the direct and indirect effects of compliance would require a fund-by-fund analysis.⁴³

⁴² The effectiveness of investments in compliance are controversial – because, for example, compliance functions are discharged by individuals who may feel compelled to be loyal to their managers (e.g., Bainbridge, 2008; Coffee, 2003; Kim, 2005; Langevoort, 2017). But despite this, managers may have incentives to spend “immense and growing” amounts on compliance because they may “fear exposing their organizations to greater liability should they fail to spend enough” (Chen and Soltes, 2018). And the fear of personal liability might reinforce this incentive: if it is discovered that the firm fails to comply with the law, then the presence of a strong compliance program will protect managers from claims that they failed to supervise the firm and that they were co-authors of false or misleading statements under securities law (Gadinis and Miazad, 2019). These ideas are in line with Paul Atkin’s argument mentioned earlier that “even filing out a form can be expensive if it will be submitted to a regulator and scrutinized by the trial bar” (Atkins, 2006) – and, taking together these arguments, they provide support to the notion that the survey figures might underestimate the direct costs of compliance.

⁴³ As mentioned in Section 2, one of the new obligations that the DFA imposed on certain investment advisors (those with AUM over \$150 million) was to file Form PF, which focuses on performance and risk information. Some commentators argue that this form can be especially costly to produce, so I examined the impact of that

It is worth noting that the suggestion that part of the post-DFA reduction in profits is explained by revisions to valuation and reporting practices raises a question: why did investors not push managers to make those revisions earlier? As mentioned before, at least some hedge fund investors are relatively sophisticated, which suggests that those investors should have monitored the financial practices of their managers. However, there are various possible answers to that question. First, even if hedge funds disclose data voluntarily or in response to investors' demands, information asymmetries between managers and investors will likely remain⁴⁴; and even if, in an ideal world, those asymmetries could be completely eliminated, investors might not monitor managers due to the transaction and opportunity costs of doing so.⁴⁵ Second, some hedge fund investors are unsophisticated or apathetic (e.g., Black, 1990; Easterbrook and Fischel, 1991, p. 66),⁴⁶ and even when they are sophisticated institutions, they may still fail to monitor hedge funds because institutional investors have their own agency problems: the institution's monitoring decisions are not made by its beneficial owners but rather by its managers, who may have little incentives to supervise the companies in which the institution invests (i.e., the "portfolio companies").⁴⁷ And third, although, in theory, hedge fund

particular form in Tables OA.2 and OA.3 of the Online Appendix. The estimations do not show any meaningful effect on performance, although there was a decline in volatility. However, I interpret all these results only as suggestive evidence for two reasons. First, the SEC does not disclose Form PF data, so it is not possible to know exactly which funds filed the form and which did not; as a result, I have to rely on a broader proxy (i.e., whether or not a fund is managed by an advisor with AUM over \$150 million) to classify the fund as a filer or as a non-filer. And second, the analysis of volatility suggests that there is a violation of the parallel trends assumption. The violation occurs when I use the entire sample. Although the matched sample corrects the violation in the models of unconditional volatility, this is not the case in the AR(1) models.

⁴⁴ In fact, although some of the information in Form ADV might have been often disclosed to investors in the fund's prospectus and other marketing materials (e.g., Atkins, 2006, p. 541), there is indirect evidence suggesting that prospectuses did not include all the information contained in Form ADV (and particularly the information in the full version of Form ADV that RIAs are required to file) (see, e.g., Brown et al., 2008, p. 2813). In this sense, even assuming that investors were generally informed before the DFA, the regulatory disclosures might still have reduced information gaps.

⁴⁵ For an illustration of misreporting practices that were not detected in large part because of the lack of monitoring by investors, see, e.g., SEC v. Beacon Hill Asset Management LLC, Civil Action No. 02-8855 (LAK), Amended complaint (June 15, 2004).

⁴⁶ As mentioned earlier, in fact, securities laws set wealth floors for individuals to invest in hedge funds, but those floors do not necessarily imply sophistication. In Bernard Madoff's Ponzi Scheme, for example, many of the defrauded investors were individuals with no connection at all to the financial industry. See Affidavit of Mailing, SIPC v. Bernard L. Madoff Investment Securities LLC, No. 08-01789 (Bankr. S.D.N.Y. Feb. 4, 2009). And compounding this problem, as mentioned in Section 2, there is evidence from other areas of commercial transactions and financial markets indicating that individuals pay little attention to corporate disclosures (see, e.g., Ben-Shahar and Schneider, 2011, pp. 665-679 (reviewing the literature); Nili and Kastiel, 2016, p. 64).

⁴⁷ These weak incentives may be caused by three factors. First, in some cases (for example, in the mutual fund industry), the remuneration of institutional investors' managers is a small percentage of their assets under management and therefore they retain only a small fraction of the gain associated with monitoring the portfolio companies – if any. Second, monitoring activities by institutional investors that increase the value of a portfolio company will benefit rival institutional investors that track the same portfolio companies; consequently, institutional investors seeking to differentiate themselves from other institutional investors with similar

investors could discipline managers by simply exiting the fund if the investors disagree with a practice, this enforcement mechanism is not always available due to the presence of lockup periods.

Before concluding this section, it is also worth noting that I also examined other potential sources of heterogeneity in the treatment effects, including the presence of leverage or lockups, fund age, and the investment strategy of a fund. However, as shown in Table OA.4 of the Online Appendix, those variables do not meaningfully explain the effects of the DFA.⁴⁸

5. Discussion

The significant decline in profits documented in this paper has various policy implications. First, to the extent that part of the decline seems to be driven by direct compliance costs and by the opportunity costs associated with managerial distraction from core business activities, then that portion of the decline should be counted as one of the costs of Dodd-Frank in the estimation of the net economic effect of the law. In contrast, the law did not result in less resources being channeled from sectors with excess of liquidity to sectors in need of liquidity, so reductions in capital formation should not be counted as a cost.

Second, as mentioned earlier, following the enactment of the DFA, several hedge funds stated that they would absorb the compliance costs created by the law and that, as a result, the law would not affect investors. However, this paper shows not only that the DFA was associated with lower profits going to investors, but also that the reduction may be partially attributable to direct compliance costs. This inconsistency therefore suggests that it is worth examining on an individual basis whether hedge funds have been disclosing accurately how they allocate their DFA-related regulatory costs, and if the disclosures have been inaccurate (which, as mentioned before, is something that has already triggered enforcement actions by the SEC), then that finding would provide support to two recent regulatory initiatives: requiring hedge funds to provide investors with periodic statements with detailed information about how fees

investment strategies may not achieve that goal by investing in monitoring. And third, institutional investors' managers may want to be excessively deferential to the managers of portfolio companies if non-deference would undermine other business relationships between the two managers. See Bebchuk and Hirst (2019) (focusing on the agency problems of index funds). For analyses of other contexts, see also: Black (1990, 1992); Rock (1991); Coffee (1991); Gilson and Gordon (2013); Strine (2010, 2014); and De Fontenay (2018).

⁴⁸ The only exception was one of the investment strategies (long-short equity), which was associated with a larger decline in performance in the treatment group. One possible explanation for this result is that the value of equities might be more difficult to monitor than the value of the assets in which other fund strategies invest (such as debt).

and expenses are calculated and allocated, and mandating annual audits of that information (SEC, 2022).⁴⁹ In addition, although the SEC also has proposed prohibiting hedge funds from passing on to investors expenses related to SEC inspections and examinations, the findings in this paper suggest that, at the very least, the industry should disclose whether investors are ultimately bearing those expenses. All these measures would enhance the ability of investors (and auditors) to monitor the fees, which is likely desirable.

The third implication is that, assuming that it is correct that part of the reduction in profitability documented here could be explained by hedge funds' revisions of financial reporting and valuation practices, then that portion of the reduction should most likely be interpreted as a benefit of the law rather than a cost.⁵⁰ This finding supports the prediction that the prospect of SEC examinations would have a positive effect on the reporting processes, but the finding also has a specific implication: since the adjustments were concentrated among the funds that were unaudited, then the results provide additional support to the initiative of requiring annual audits of performance computations (SEC, 2022). As even critics of hedge fund regulation have noted, annual audits are a "straightforward method" to enhance the monitoring of financial disclosures while at the same taking into account that the SEC has limited resources to inspect the industry (Atkins, 2006).

It is important to note that the data presented in this paper have various inherent limitations. One is that I do not capture funds that evaded federal regulation – an end that could be achieved if the investment advisor changes its location or reduces its assets under management below the cutoff that triggers registration (\$150 million). I did not find evidence of any of these specific forms of regulatory avoidance,⁵¹ but if fund managers avoided registration through other means, then the results presented here will not capture those managers.

⁴⁹ Although some hedge funds disclose this information on a quarterly basis, it is unclear whether all of them do – and, in addition, the SEC has found that those disclosures are "often opaque" (SEC, 2022, p. 10).

⁵⁰ Of course, the quality of financial reports is not necessarily better because the reported performance is lower. However, it seems unlikely that funds would make downward revisions to their performance estimations without believing that the adjustment was necessary.

⁵¹ To examine the possibility of asset manipulation, I examined whether there was an abnormally high incidence of funds *just below* the cutoff that triggers registration with the SEC (i.e., \$150 million in AUM). Unfortunately, performing this test in a clean manner is not possible because investment advisors disclose their AUM in Form ADV and therefore I do not have AUM data for funds that never registered or, in the case of the treatment group, for the sample period preceding registration. However, I built a proxy for the investment advisors' AUM based on Lipper data by summing the assets of the individual funds that report to the database. Using this proxy, I then created bins of \$50 million to classify the funds and tested whether the incidence of funds in the \$100-\$150 million bin was significantly higher than the incidence of funds in the bin just above the threshold or the average of the two neighboring bins. However, as mentioned, I did not find evidence of "bunching." It is worth emphasizing that

The other limitation is that the Lipper database is an imperfect source of information on hedge fund performance because hedge funds report voluntarily to Lipper and therefore the database may lead to bias. As mentioned earlier, I mitigated this concern by running the regressions not only with a balanced panel (which is unaffected by changes in the composition of the sample), but also with all the funds in the sample (which mitigates potential bias resulting from the fact that the funds that remain in the sample might have characteristics that are related to the impact of the DFA). However, these measures do not fully eliminate the possibility that there are unobservable characteristics associated with the likelihood of reporting to Lipper.

In addition to these inherent limitations, this paper also leaves open various questions for future research. Two of them illustrate this point. First, this paper suggests that mandatory disclosure and public oversight were associated with a decline in performance, but there is no direct empirical evidence on what information included in the new regulatory filings was not already being disclosed to investors in the fund's prospectus and other marketing materials.⁵² Studying that aspect would shed light on how exactly the DFA supplements voluntary disclosures and, therefore, on how exactly federal regulation provides a differentiated layer of investor protection.

And second, related to the previous point, this paper examines the impact of specific types of mandatory disclosures: SEC registration and Form ADV. However, there are other types that are worth examining. For example, in an attempt to help retail investors make more informed choices in financial markets, the SEC recently required investment advisors to produce a new form, Form CRS ("Client Relationship Summary"), which provides a simplified version of key data about the advisor and its relationship with the client (see, e.g., SEC, 2020). Studying this

the proxy for AUM I employ is imperfect because an investment advisor may advise hedge funds that are not included in Lipper or funds that are not hedge funds. To mitigate this limitation, therefore, I also examined, *using Form ADV data*, whether there was an *abnormally low* number of funds in the bin *just above the \$150 million cutoff* (i.e., the \$150 - \$200 million bin) *among the treatment funds only*. The idea behind this test is that if the funds with low enough assets managed systematically their AUM to avoid registration, then the number of funds in the bin just above the \$150 million cutoff should be lower than expected *when only the funds that actually registered are considered*. Once again, however, I did not find support for this hypothesis. All these results then show that even though some hedge funds predicted that they would reduce their AUM in response to Dodd-Frank (Kaal, 2016, pp. 301-302), that does not seem to have been a systematic response to the law; and this result may not be surprising, as it might be more costly for hedge funds to reduce their AUM than register with the SEC.

⁵² There is in fact *indirect* evidence suggesting that Form ADV contained information that was not generally available to investors – but that study does not compare the actual data disclosed in regulatory filings with the data disclosed by the industry (Brown et al., 2008, p. 2813).

form would be interesting because that would provide evidence on the impact of simplification on the type of information that is disclosed to investors and the contractual terms that govern the relationships between investors and their financial advisors – all of which sheds light on how disclosure rules can produce more efficient contracting.⁵³

6. Conclusion

This paper examines the effect of Dodd-Frank on hedge fund performance, risk-taking, and capital formation. The results show that the DFA was not associated with a significant change in risk-taking (as measured by volatility), and even though there was a significant increase in fund exits and a significant reduction in fund entries after the law, those changes did not result in less capital formation. In contrast, the performance of the funds that became regulated by the DFA declined in relation to the funds that were already registered with the SEC or that were largely unaffected by the legal change, and this result seems to be explained by direct compliance costs and indirect effects of compliance – including especially opportunity costs and revisions of valuation and reporting practices. Since this paper focuses on changes in net-of-fees performance, the results suggest that, contrary to predictions made by the industry, the DFA affected the ultimate investors. This conclusion in turn has implications for the cost/benefit analysis of the law and for current regulatory initiatives that propose greater transparency and annual audits in the areas of financial reporting and allocation of expenses.

⁵³ There is, in fact, preliminary evidence indicating that a large fraction of filers disclosed false and misleading information in the first batch of submissions of Form CRS in June 2020. See Zweig and Fuller (2020).

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Table 1
Descriptive statistics on treatment and control fund characteristics

Table 1 presents descriptive statistics for the treatment and control funds. The variables are defined in Appendix 1. The control group consists of 469 RIAs that were already registered with the SEC and 86 ERAs. Detailed descriptive statistics on these two subsets of the control group are reported in Table OA.1 of the Online Appendix. The statistical significance of the mean differences is based on *t*-tests. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Variable	Treatment group				Control group				Difference of means
	Mean	Median	SD	N	Mean	Median	SD	N	
Log(NAV)	6.76	7.10	2.22	85	5.94	5.93	1.98	555	0.82***
Management fee	1.43	1.50	0.59	85	1.41	1.50	0.51	555	0.03
Incentive fee	14.24	20.00	7.55	85	11.56	15.00	9.38	555	2.68**
High water mark	0.58	1.00	0.50	85	0.55	1.00	0.50	555	0.03
Leveraged	0.61	1.00	0.49	85	0.48	0.00	0.50	555	0.14**
Lockup	0.26	0.00	0.44	85	0.19	0.00	0.39	555	0.07
Redemption notice	0.93	1.00	0.26	85	0.83	1.00	0.37	555	0.10**
US fund	0.26	0.00	0.44	85	0.30	0.00	0.46	555	-0.04
Age	7.11	5.16	4.68	85	8.70	7.19	4.97	555	-1.59***
Liquidity sensitivity	4.75	4.31	13.66	85	5.70	4.28	12.65	555	-0.96
Audited	0.38	0.00	0.49	85	0.47	0.00	0.50	555	-0.09

Table 2
Univariate analyses

Table 2 presents univariate analyses on all the outcome variables. Returns and alphas are winsorized at 5% to mitigate the influence of outliers (but the results are similar if I winsorize at 1%). The assets under management are in logarithmic form. The exit and entry rates are the total number of exits or entries within the treatment or the control group, divided by all the entries or exits. Δ_t is the difference in the outcome variables by period, and the bottom cell of that column is the difference-in-differences coefficient.

Panel A. Performance

Group	Raw returns			Alpha		
	Pre-DFA	Post-DFA	Δ_t	Pre-DFA	Post-DFA	Δ_t
Treatment	1.32	0.54	-0.79***	0.84	0.39	-0.45
Control	0.95	0.53	-0.41***	0.47	0.30	-0.17
Treatment - Control	0.38***	0.00	-0.37***	0.37***	0.09**	-0.28***

Panel B. Volatility

Group	Total volatility			Idiosyncratic volatility		
	Pre-DFA	Post-DFA	Δ_t	Pre-DFA	Post-DFA	Δ_t
Treatment	1.71	1.52	-0.20***	1.61	1.44	-0.16***
Control	1.67	1.52	-0.15***	1.54	1.35	-0.19***
Treatment - Control	0.04	0.00	-0.05	0.07**	0.09***	0.03

Panel C. Exits, entries, and assets under management

Group	Exit rate			Entry rate			Assets under management		
	Pre-DFA	Post-DFA	Δ_t	Pre-DFA	Post-DFA	Δ_t	Pre-DFA	Post-DFA	Δ_t
Treatment	0.002	0.012	0.009***	0.012	0.000	-0.012***	17.835	18.119	0.284***
Control	0.011	0.014	0.003***	0.002	0.000	-0.002***	17.944	17.851	-0.093***
Treatment – Control	-0.009***	-0.002	0.007***	0.010***	0.000	-0.010***	-0.109***	0.268***	0.377***

Table 3
Dodd-Frank and hedge fund performance and volatility:
Baseline regressions

Table 3 examines the effect of Dodd-Frank on hedge fund performance. All the regressions are run using fixed effects at the fund level and clustered standard errors by fund (in parentheses). Time fixed effects are at the month level for the estimations in Panel A. The stand-alone *Treat* and *Post* variables are not included in Panel A because they are collinear with the fund and time fixed effects, respectively. Only funds that survived during the entire sample period (30 months before and after June 2011) are used in the analysis, but the actual number of months per fund used in Panel A is 61 because the month of the event is added to the post-DFA period. The R-squared is the within R-squared. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Performance analysis

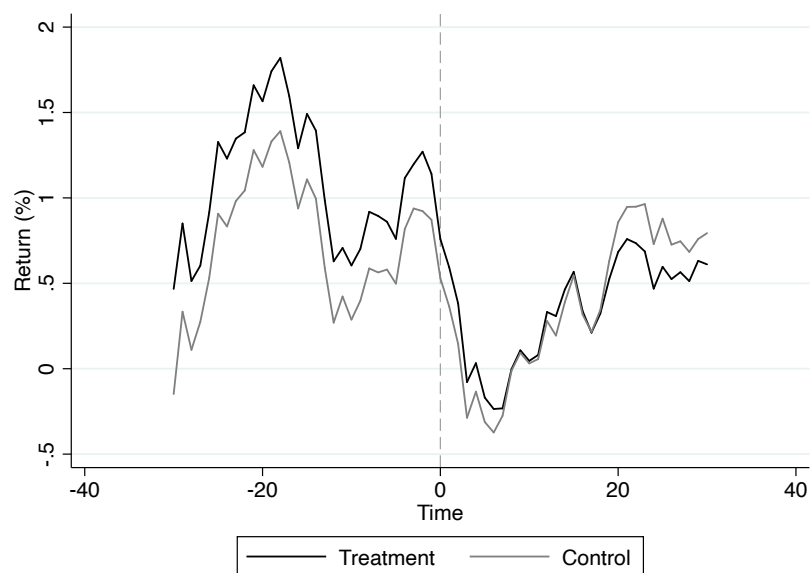
Sample	Full sample		Matched sample	
Variable	Returns	Alpha	Returns	Alpha
Model	(1)	(2)	(3)	(4)
Treat×Post	-0.373*** (0.08)	-0.278*** (0.08)	-0.357*** (0.10)	-0.365*** (0.10)
Fund fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
R-squared	0.28	0.10	0.26	0.10
Number of funds	640	640	170	170
N	39040	39040	10370	10370

Panel B. Volatility analysis

Sample		Full Sample			Matched sample			
Variable	Total risk	Total risk (ARCH(1))	Idiosyncratic risk	Idiosyncratic risk (ARCH(1))	Total risk	Total risk (ARCH(1))	Idiosyncratic risk	Idiosyncratic risk (ARCH(1))
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat×Post	-0.045 (0.05)	-0.023 (0.02)	0.025 (0.04)	0.011 (0.01)	-0.043 (0.07)	-0.002 (0.03)	-0.005 (0.05)	0.000 (0.02)
Fund fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.16	0.10	0.12	0.11	0.14	0.10	0.10	0.10
Number of funds	640	631	640	630	170	168	170	168
N	39040	38491	39040	38430	10370	10248	10370	10248

Figure 1
Hedge fund performance before and after Dodd-Frank

Panel A. Raw returns over time



Panel B. Eight-factor alpha over time

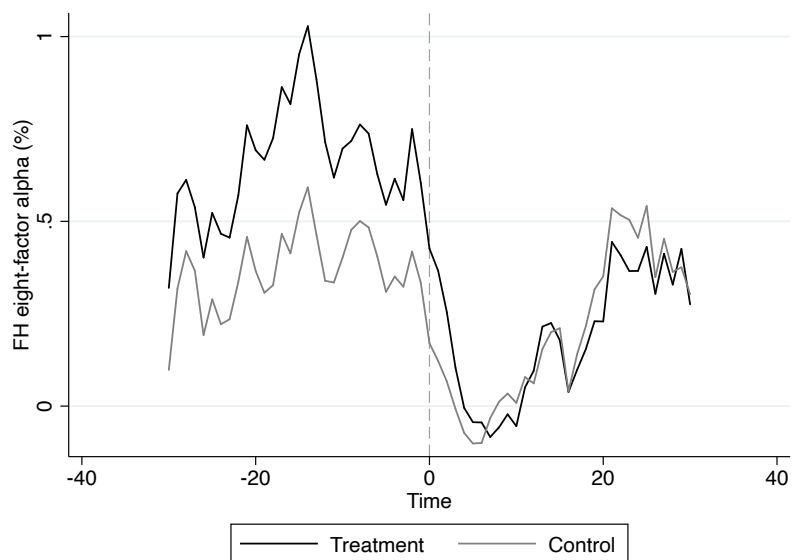


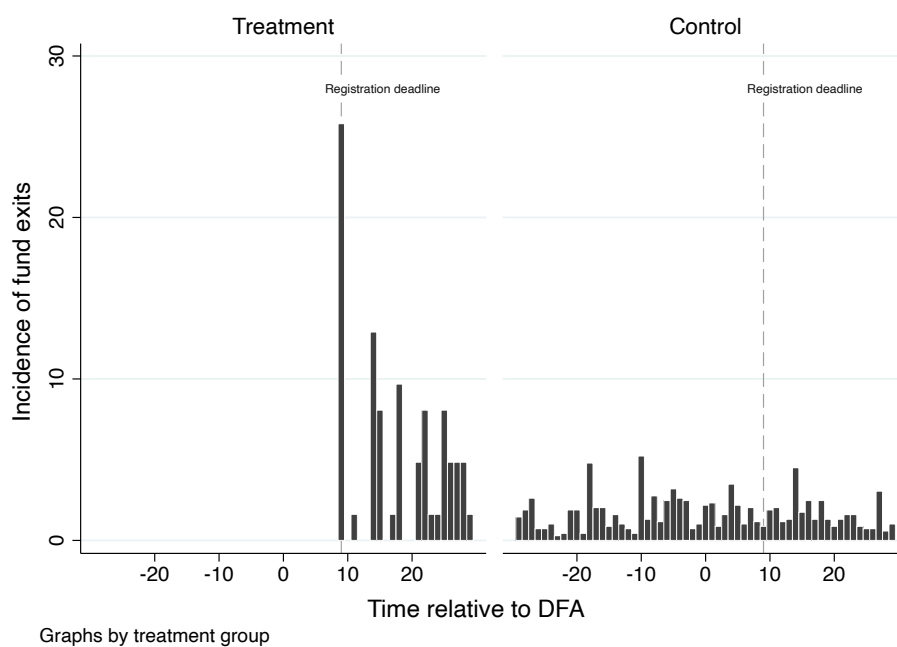
Table 4
Exits, entries, and assets under management

The table presents the analysis of fund exits, fund entries, and assets under management. In the analyses of fund exits and entries, the dependent variable takes the value of 1 if a fund exits or enters the sample in a specific month and 0 otherwise. I used pooled OLS instead of fund fixed effects in these analyses because previous literature suggests that the magnitude of the bias in fixed-effects models can be larger than in pooled OLS in discrete-time hazard settings (Tauchmann, 2019). In addition, I did not use proportional hazard models for the analysis of fund exits for ease of interpretation. Because the graphical evidence shows that the effect of the DFA on exits and entries is sharp around the registration deadline, I take that date as the cutoff. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Variable	Exit rate		Entry rate		Assets under management	
Model	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	0.007*** (0.00)	0.006*** (0.00)	-0.010*** (0.00)	-0.011*** (0.00)	0.560*** (0.12)	0.558*** (0.12)
Treat	-0.009*** (0.00)	-0.010*** (0.00)	0.010*** (0.00)	0.009*** (0.00)		
Fund FE	No	No	No	No	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
R-squared	0.005	0.007	0.007	0.011	0.019	0.022
Number of funds	1616	1616	1616	1616	1101	1101
N	70659	70659	70659	70659	41350	41350

Figure 2
Fund exits and entries in the sample period

Panel A. Exits



Panel B. Entries

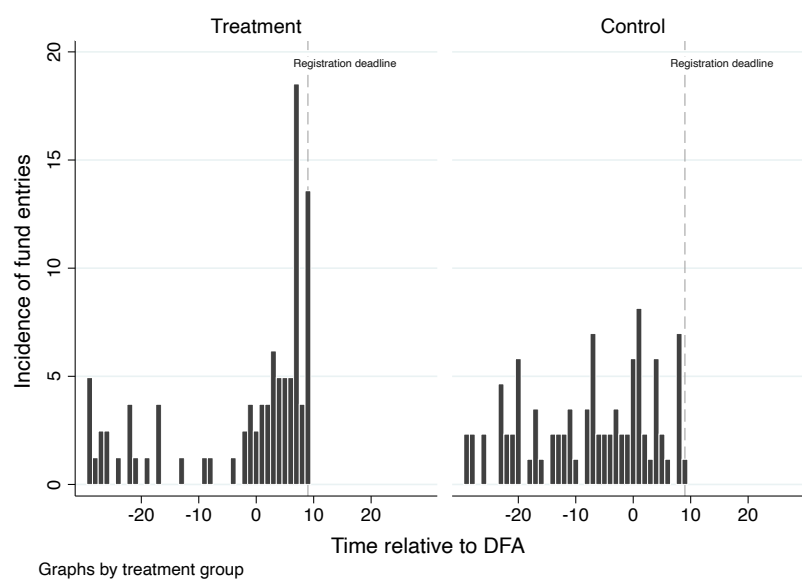


Table 5
Sensitivity analysis on the post-DFA reduction of performance

The table presents sensitivity analyses. The control group of pre-existing RIAs includes all funds managed by RIAs that were continually registered during the entire sample period. The control group of ERAs includes all funds that registered as ERAs in response to Dodd-Frank. Models 1-8 are based on the baseline sample (i.e., funds that survived during the entire sample period) and Models 9-10 include all the funds available (including those that failed or entered the market in the sample period). In Models 1-6, the regressions are based on the entire sample period (i.e., 30 months before and after the event date). In Models 7-8, the time window is 21 months before and after June 2011. In all the fixed effects regressions (Models 1-4, and 7-8), the fixed effects are at the fund level and at the month level. In the random effects specifications (Models 5-6), the random effects are at the fund level and all the regressions include month fixed effects and controls for the following fund characteristics: management fee, incentive fee, high water mark, investment style, leverage, lockup, redemption notice, dummy for U.S. funds, age, and pre-DFA NAV, volatility, and sensitivity to market liquidity. The stand-alone *Treat* variable is included in the random effects regressions because, unlike in the fixed effects models, the variable is not collinear with the fund fixed effects. The stand-alone *Post* variable, in contrast, is not included because it is collinear with the month fixed effects. The R-squared is the within R-squared for the fixed effects estimations and the overall R-squared for the random effects estimations. The standard errors (in parentheses) are clustered by fund. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Sample	Baseline (funds that survived in the entire sample period)						All funds			
Control group	RIAs only		ERAs only		Pooled		Pooled		Pooled	
Time window	Full		Full		Full		Reduced		Full	
Dependent variable	Return	Alpha	Return	Alpha	Return	Alpha	Return	Alpha	Return	Alpha
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat×Post	-0.359*** (0.08)	-0.275*** (0.08)	-0.446*** (0.11)	-0.297*** (0.10)	-0.373*** (0.08)	-0.278*** (0.08)	-0.335*** (0.07)	-0.312*** (0.08)	-0.348*** (0.06)	-0.317*** (0.07)
Treat					0.330*** (0.07)	0.322*** (0.07)				
Fund FE or RE	FE	FE	FE	FE	RE	RE	FE	FE	FE	FE
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No	No	No	No	No
R-squared	0.32	0.12	0.17	0.07	0.28	0.10	0.31	0.10	0.25	0.08
Number of funds	554	554	171	171	640	640	640	640	1616	1497
N	33794	33794	10431	10431	39040	39040	27520	27520	70659	68632

Table 6
Heterogeneity in the treatment effects of the DFA

The table examines heterogeneity in the treatment effects. The stand-alone *Treat*, *NoAudit*, *LowCommonRisk*, and *HighFundsManaged* variables (as well as the interaction between those variables and *Treat*) are collinear with the fund fixed effects and are therefore excluded from the regressions. The *Post* variable is also excluded because it is collinear with the time fixed effects. Only funds that survived during the entire sample period (30 months before and after June 2011) are used in the analysis. Columns 1-2 and 5-6 are based on the entire sample, and columns 3 and 4 use only unaudited funds. All the regressions are run using fixed effects at the fund level and clustered standard errors by fund (in parentheses). Time fixed effects are at the month level. The R-squared is the within R-squared. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Analysis	Lack of audits		Hard-to-value assets		Number of funds managed	
Dependent variable	Returns	Alpha	Returns	Alpha	Returns	Alpha
Model	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post×NoAudit	-0.375*** (0.14)	-0.529*** (0.16)				
Post×NoAudit	-0.143** (0.06)	0.017 (0.05)				
Treat×Post×LowCommonRisk			-0.667** (0.30)	-0.676** (0.27)		
Post×LowCommonRisk			0.396*** (0.10)	-0.051 (0.09)		
Treat×Post×HighFundsManaged					-0.320** (0.16)	-0.438*** (0.17)
Post×HighFundsManaged					0.572*** (0.07)	0.371*** (0.06)
Treat×Post	-0.126 (0.10)	0.050 (0.12)	-0.235** (0.11)	-0.235** (0.10)	-0.107 (0.12)	0.030 (0.13)
Fund fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.22	0.01	0.28	0.08	0.28	0.1
Number of funds	640	640	349	349	640	640
N	39040	39040	21289	21289	39040	39040

Appendix 1

Variable definitions

Variable	Description
<i>Alpha</i>	Alpha estimated using the Fung & Hsieh (2004) eight-factor model, as described in the text.
<i>Age</i>	Mean age of a fund during the 60-month period around the event date.
<i>AUM₁₅₀₊</i>	Funds whose investment advisors are registered with the SEC as RIAs and have over \$150 million in assets under management. This variable works as a proxy for investment advisors that are required to file Form PF.
<i>Fund entry</i>	Indicator variable that equals 1 if a fund enters the sample in a particular month and 0 otherwise.
<i>Fund exit</i>	Indicator variable that equals 1 if a fund exits the Lipper database in a particular month and 0 otherwise.
<i>Incentive fee</i>	Percentage of a fund's profits charged by the investment advisor.
<i>High fee</i>	Indicator variable that takes the value of 1 if a fund charges an incentive fee above the midpoint of the 10% - 20% range (that is, 15%) and 0 otherwise.
<i>HighFundsManaged</i>	An indicator variable that takes the value of one if an investment advisor manages a number of funds that is above the median number in the sample.
<i>High volatility</i>	Indicator variable that takes the value of 1 if a fund has "high volatility" (i.e., above-the-median standard deviation of returns) and 0 otherwise. I employ two versions of this proxy: high <i>total</i> volatility (" <i>HighVolatility_{Total}</i> ") and high <i>idiosyncratic</i> volatility (" <i>HighVolatility_{Idiosyncratic}</i> ").
<i>High water mark</i>	Indicator variable that equals 1 if a fund employs a high water mark provision and 0 otherwise. High water mark provisions restrict incentive fees if a fund has losses during a pre-defined period.
<i>Leverage</i>	Indicator variable that equals 1 if a fund allows leverage and 0 otherwise
<i>Lockup</i>	Indicator variable that equals 1 if the fund has a lockup provision and 0 otherwise. Lockups provide a minimum period during which an investor must remain in a fund.
<i>Low common risk</i>	Indicator variable that takes the value of 1 if a fund's returns have low correlation with general market benchmarks (i.e., the fund has below-the-median correlation with general market benchmarks) and 0 otherwise. I define a fund's correlation with general market benchmarks as the R^2 from regressing the funds' returns on the Fung and Hsieh (2004) eight common risk factors.
<i>Management fee</i>	The fixed fee charged by an investment advisor for the management of the fund. The fee is expressed as a percentage of the assets under management.
<i>NAV</i>	A fund's average net asset value (NAV) during the 30-month period preceding the event date. NAV is computed as the ratio of net assets (total assets minus liabilities) to shares outstanding.
<i>No Audit</i>	Indicator variable that takes the value of 1 if a fund was not audited at least once within a year before the registration year under the DFA and zero otherwise.
<i>Post</i>	Indicator variable that equals 1 for observations after June 2011 (the month in which the SEC adopted the rules implementing Dodd-Frank) and 0 otherwise. In the analysis of the Hedge Fund Rule (unreported), the variable equals 1 for observations after December 2004 (the month in which the rule was adopted) and 0 otherwise.
<i>Redemption notice</i>	Indicator variable that equals 1 if investors must give an advance notice to the fund manager to redeem their shares.
<i>Sensitivity to market liquidity</i>	Beta estimated by regressing a fund's returns on the Pástor and Stambaugh (2003) innovation in liquidity measure. The regressions employed to obtain the beta include the Fung and Hsieh (2004) eight risk factors and are estimated using the last 30 months of data prior to the event.
<i>Small_{qi}</i>	Indicator variable for funds managed by investment advisors with AUM below the median value. This paper examines two variations of this variable: <i>Small_{q2}</i> , which uses the median value of the distribution to classify a fund as small or large; and <i>Small_{q4}</i> , which uses the smallest quartile to classify a fund as small.
<i>Strategy</i>	The main investment strategy of a fund as reported to Lipper. The investment strategies in the sample are: convertible arbitrage, emerging markets, equity market neutral, event-driven, fixed income arbitrage, fund-of-funds, global macro, long-short equity hedging, multi-strategy, and "other." As explained in the text, in some of the regression analyses, I collapse the strategies into three categories in order to have at least ten treatment funds in each category: fund-of-funds, long-short equity hedging, multi-strategy, and "other."
<i>Treat</i>	Indicator variable that equals 1 for funds advised by investment advisors that became registered as Registered Investment Advisors (RIAs) with the SEC in response to the Dodd-Frank Act and 0 otherwise. Investment advisors are assumed to have registered with the SEC in response to the Dodd-Frank Act if they registered for the first time as RIAs at any point between October 2011 and March 2012 (that is, at any point during the 6-month period preceding the registration deadline). The control group is the group of funds that (1) were continuously registered with the SEC as RIAs during the entire 60-month period around the event date, and (2) funds that registered as ERAs with the SEC at any point between October 2011 and March 2012. In the analysis of the Hedge Fund Rule (unreported), the treatment funds are the funds that registered with the SEC as RIAs at any point between August 2005 and January 2006. The control is the group of funds that were continuously registered with the SEC as RIAs during the 60-month period around December 2004.
<i>U.S. fund</i>	Indicator variable that equals 1 if the fund is domiciled in the U.S. and 0 otherwise
<i>Volatility (idiosyncratic)</i>	Standard deviation of the residuals from the Fung & Hsieh (2004) eight-factor model.
<i>Volatility (total)</i>	Standard deviation of monthly returns.

Appendix 2

Parallel trends analysis: Analytical tests

This appendix presents analytical tests of the parallel trends assumption. Similar to the methodology proposed by Mora and Reggio (2017), I first divide the sample by 6-month subperiods and then estimate the baseline fixed-effects regressions with interactions between the treatment variable and each 6-month subperiod. The interactions of the *Treat* variable with each subperiod measure the relative change in performance that the treated funds experienced in the corresponding subperiod and the joint significance of the interactions preceding the event date capture differences in pre-treatment dynamics.

This part of the analysis therefore estimates the following equation:

$$y_{it} = \phi_i + \sum_{\tau=t_2}^T \delta_{\tau} I_{\tau}^t + \sum_{\tau=t_2}^T \gamma_{\tau} I_{\tau}^t \times Treat_i + \varepsilon_{it}, \quad (1)$$

where y_{it} is the performance of fund i at period t , and I^{τ} is a dummy for subperiod τ . ϕ_i is a vector of fund fixed effects and ε_{it} is the error term.

As shown in the table, the interactions between the treatment variable and the dummies for each 6-month subperiod preceding the event month are generally indistinguishable from zero, suggesting that the parallel trends assumption is satisfied. The tests also show that the interactions were not jointly significant for raw returns, which indicates similar pre-treatment dynamics and, therefore, parallel trends. In the case of alphas, the variables were jointly significant; however, that was corrected in the matched sample. These results therefore suggest that the relative reduction in the treatment group's performance following the DFA, including the reduction in alphas, was not simply driven by a declining trend that predated the event date.

Table A2.1
Parallel trends analysis for baseline difference-in-differences analysis

The table presents analytical tests on the parallel trends assumption. The variables are defined in Appendix 1. All the regressions are run using fixed effects at the fund level and clustered standard errors by fund. Besides the interactions displayed in the table, the regressions include the stand-alone *Treat* variable and dummy variables for each 6-month period within the 60 months surrounding the implementation date of Dodd-Frank. Those variables are omitted from the regression output for conciseness. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Tests for each 6-month period

Variable	Full sample		Matched sample	
	Returns	Alpha	Returns	Alpha
	(1)	(2)	(3)	(4)
Treat×Period _{[-24,-18)}	-0.106 (0.13)	0.015 (0.11)	0.082 (0.19)	0.009 (0.15)
Treat×Period _{[-18,-12)}	-0.031 (0.14)	0.050 (0.11)	0.196 (0.20)	0.079 (0.15)
Treat×Period _{[-12,-6)}	-0.219 (0.15)	-0.219* (0.13)	-0.082 (0.20)	-0.159 (0.17)
Treat×Period _{[-6,0)}	-0.178 (0.15)	-0.090 (0.11)	0.188 (0.21)	0.007 (0.15)
Treat×Period _{[0,6)}	-0.241 (0.19)	-0.253* (0.15)	-0.122 (0.25)	-0.291 (0.19)
Treat×Period _{[6,+12)}	-0.467*** (0.15)	-0.409*** (0.11)	-0.352* (0.20)	-0.489*** (0.15)
Treat×Period _{[+12,+18)}	-0.550*** (0.14)	-0.464*** (0.13)	-0.343* (0.19)	-0.465*** (0.17)
Treat×Period _{[+18,+24)}	-0.611*** (0.19)	-0.269** (0.13)	-0.276 (0.24)	-0.287 (0.18)
Treat×Period _[+24,+30]	-0.522*** (0.16)	-0.253** (0.10)	-0.303 (0.21)	-0.362** (0.14)
Fund fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
R-squared	0.06	0.02	0.06	0.02
Number of funds	640	640	170	170
N	39040	39040	10370	10370

Panel B. Tests on common pre-treatment dynamics

Variable	Full sample		Matched sample	
	F-statistic	p-value	F-statistic	p-value
Returns	0.88	0.47	0.17	0.33
Alpha	4.15	0.00	1.54	0.20

Online Appendix for *The economic consequences of hedge fund regulation: An analysis of the effect of the Dodd-Frank Act*

Table OA.1
Additional descriptive statistics including all the subsets of the control group

The table presents descriptive statistics for the treatment and control funds. The table is similar to Table 1 in the body of the text, but it displays additional information on the two subsets of the control group. The table includes *t*-tests of means differences between the treatment group and the pooled control group (“T – P”), the treatment group and the subset of control RIAs (“T – R”), and the treatment group and the subset of control ERAs (“T – E”). ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Group	Treatment group				Control group												Differences of means		
					Pooled (RIAs & ERAs)				RIAs				ERAs						
	Mean	Media n	SD	N	Mean	Median	SD	N	Mean	Median	SD	N	Mean	Median	SD	N	T – P	T – R	T – E
Log(NAV)	6.76	7.10	2.22	85	5.94	5.93	1.98	555	6.28	6.50	1.62	469	4.07	4.76	2.63	86	0.82***	0.48**	2.69***
Management fee	1.43	1.50	0.59	85	1.41	1.50	0.51	555	1.45	1.50	0.43	469	1.19	1.25	0.80	86	0.03	-0.01	0.24**
Incentive fee	14.24	20.00	7.55	85	11.56	15.00	9.38	555	11.42	15.00	9.15	469	12.33	20.00	10.59	86	2.68**	2.82***	1.91
High water mark	0.58	1.00	0.50	85	0.55	1.00	0.50	555	0.58	1.00	0.49	469	0.41	0.00	0.49	86	0.03	0.00	0.17**
Leveraged	0.61	1.00	0.49	85	0.48	0.00	0.50	555	0.49	0.00	0.50	469	0.42	0.00	0.50	86	0.14**	0.13**	0.19**
Lockup	0.26	0.00	0.44	85	0.19	0.00	0.39	555	0.20	0.00	0.40	469	0.12	0.00	0.32	86	0.07	0.05	0.14**
Redemption notice	0.93	1.00	0.26	85	0.83	1.00	0.37	555	0.89	1.00	0.32	469	0.55	1.00	0.50	86	0.10**	0.04	0.38***
US fund	0.26	0.00	0.44	85	0.30	0.00	0.46	555	0.33	0.00	0.47	469	0.13	0.00	0.34	86	-0.04	-0.07	0.13**
Age	7.11	5.16	4.68	85	8.70	7.19	4.97	555	9.03	7.55	5.09	469	6.90	5.87	3.84	86	-1.59***	-1.92***	0.21
Liquidity sensitivity	4.75	4.31	13.66	85	5.70	4.28	12.65	555	6.36	5.37	12.77	469	2.09	-0.97	11.40	86	-0.96	-1.62	2.66
Audited	0.38	0.00	0.49	85	0.47	0.00	0.50	555	0.51	1.00	0.50	469	0.21	0.00	0.41	86	-0.09	-0.14**	0.17**

Table OA.2.
Form PF, performance, and volatility

This table examines the effect of the obligation to file Form PF on hedge fund performance and volatility. AUM_{150+} is an indicator variable that takes the value of 1 for funds whose investment advisors are registered with the SEC as RIAs and have over \$150 million in assets under management. I use this variable as a proxy for the obligation to file Form PF. The control group in this analysis includes all RIAs with assets under management below \$150 million and funds whose investment advisors are registered with the SEC as ERAs. The stand-alone AUM_{150+} and $Post$ variables are not included in Panel A because they are collinear with the fund and time fixed effects, respectively. In Panel B, only the AUM_{150+} variable is collinear with the fixed effects and therefore the $Post$ variable is included in the regressions. Only funds that survived during the entire sample period (30 months before and after June 2011) are used in the analysis. All the regressions include fixed effects at the fund level and clustered standard errors by fund (in parentheses). Time fixed effects are at the month level in Panel A. The R-squared is the within R-squared. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Performance analysis

Sample	Full sample		Matched sample	
Variable	Returns	Alpha	Returns	Alpha
Model	(1)	(2)	(3)	(4)
$AUM_{150+} \times Post$	-0.005 (0.08)	-0.031 (0.06)	-0.133 (0.10)	-0.052 (0.09)
Fund fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
R-squared	0.28	0.10	0.24	0.07
Number of funds	640	640	234	234
N	39040	39040	14274	14274

Panel B. Volatility analysis

Sample	Full Sample				Matched sample			
Variable	Total risk	AR(1) total risk	Idiosyncratic risk	AR(1) idiosyncratic risk	Total risk	AR(1) total risk	Idiosyncratic risk	AR(1) idiosyncratic risk
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
AUM ₁₅₀₊ ×Post	-0.127*** (0.04)	-0.058*** (0.01)	-0.140*** (0.03)	-0.037*** (0.01)	-0.180*** (0.05)	-0.044** (0.02)	-0.164*** (0.04)	-0.034** (0.01)
Fund fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.16	0.09	0.12	0.12	0.13	0.09	0.12	0.11
Number of funds	640	631	640	630	234	234	234	229
N	39040	38491	39040	38430	14274	14274	14274	13969

Table OA.3.
Parallel trends for the analysis of the impact of Form PF on performance and volatility

Panel A. Parallel trend tests for performance

The table presents analytical tests on the parallel trends assumption. The variables are defined in Appendix 1. All the regressions are run using fixed effects at the fund level and clustered standard errors by fund. Besides the interactions displayed in the table, the regressions include the stand-alone AUM_{150+} variable and dummy variables for each 6-month period within the 60 months surrounding the implementation date of Dodd-Frank. Those variables are omitted from the regression output for conciseness. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A.I. Tests for each 6-month period

Variable	Full sample		Matched sample	
	Returns	Alpha	Returns	Alpha
	(1)	(2)	(3)	(4)
$AUM_{150+} \times \text{Period}_{[-24,-18]}$	0.229 (0.14)	0.243** (0.10)	-0.323* (0.18)	-0.286** (0.14)
$AUM_{150+} \times \text{Period}_{[-18,-12]}$	-0.109 (0.12)	0.256*** (0.10)	-0.549*** (0.17)	-0.170 (0.13)
$AUM_{150+} \times \text{Period}_{[-12,-6]}$	-0.033 (0.13)	0.115 (0.11)	-0.507*** (0.18)	-0.316** (0.14)
$AUM_{150+} \times \text{Period}_{[-6,0]}$	-0.050 (0.15)	0.024 (0.11)	-0.589*** (0.21)	-0.448*** (0.15)
$AUM_{150+} \times \text{Period}_{[0,+6]}$	-0.163 (0.18)	-0.111 (0.14)	-0.889*** (0.23)	-0.695*** (0.17)
$AUM_{150+} \times \text{Period}_{[+6,+12]}$	-0.065 (0.18)	0.064 (0.13)	-0.636*** (0.21)	-0.354** (0.16)
$AUM_{150+} \times \text{Period}_{[+12,+18]}$	0.109 (0.15)	0.204* (0.11)	-0.341* (0.19)	-0.120 (0.15)
$AUM_{150+} \times \text{Period}_{[+18,+24]}$	0.125 (0.16)	0.308** (0.13)	-0.382* (0.22)	-0.086 (0.17)
$AUM_{150+} \times \text{Period}_{[+24,+30]}$	0.006 (0.15)	0.028 (0.12)	-0.405** (0.20)	-0.237 (0.15)
Fund fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
R-squared	0.06	0.02	0.05	0.02
Number of funds	640	640	234	234
N	39040	39040	14274	14274

Panel A.2. Tests on common pre-treatment dynamics

Variable	Full sample		Matched sample	
	F-statistic	p-value	F-statistic	p-value
Returns	2.44	0.05	2.90	0.02
Alpha	3.42	0.01	3.15	0.01

Panel B. Parallel trend tests for volatility

Sample	Full sample				Matched sample			
Variable	Total risk	Total risk (ARCH(1) model)	Idiosyncratic risk	Idiosyncratic risk (ARCH(1) model)	Total risk	Total risk (ARCH(1) model)	Idiosyncratic risk	Idiosyncratic risk (ARCH(1) model)
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
AUM ₁₅₀₊ ×Period _{[-24,-18)}	0.148* (0.08)	0.046* (0.02)	0.127* (0.07)	-0.078*** (0.03)	0.084 (0.10)	-0.005 (0.03)	0.206** (0.10)	-0.044 (0.04)
AUM ₁₅₀₊ ×Period _{[-18,-12)}	0.000 (0.07)	-0.064** (0.03)	-0.134* (0.07)	-0.076** (0.03)	-0.006 (0.10)	-0.091** (0.04)	-0.003 (0.10)	-0.018 (0.04)
AUM ₁₅₀₊ ×Period _{[-12,-6)}	-0.166* (0.09)	-0.037 (0.03)	-0.153* (0.08)	-0.105*** (0.03)	-0.165 (0.11)	-0.049 (0.04)	-0.101 (0.10)	-0.082* (0.04)
AUM ₁₅₀₊ ×Period _{[-6,0)}	-0.054 (0.09)	-0.088*** (0.03)	-0.115 (0.07)	-0.087*** (0.03)	-0.121 (0.11)	-0.105** (0.04)	-0.059 (0.10)	-0.063 (0.04)
AUM ₁₅₀₊ ×Period _{[0,+6)}	-0.090 (0.09)	-0.029 (0.03)	-0.397*** (0.08)	-0.168*** (0.03)	-0.022 (0.11)	0.041 (0.04)	-0.266*** (0.10)	-0.122*** (0.04)
AUM ₁₅₀₊ ×Period _{[+6,+12)}	-0.167** (0.08)	-0.106*** (0.03)	-0.240*** (0.08)	-0.134*** (0.03)	-0.267*** (0.10)	-0.128*** (0.04)	-0.171* (0.10)	-0.115*** (0.04)
AUM ₁₅₀₊ ×Period _{[+12,+18)}	-0.021 (0.10)	-0.085*** (0.03)	0.090 (0.08)	-0.076** (0.03)	-0.108 (0.12)	-0.111** (0.05)	0.093 (0.11)	-0.050 (0.04)
AUM ₁₅₀₊ ×Period _{[+18,+24)}	-0.190** (0.09)	-0.108*** (0.04)	-0.161** (0.08)	-0.067** (0.03)	-0.336*** (0.12)	-0.151*** (0.05)	-0.167 (0.11)	-0.049 (0.05)
AUM ₁₅₀₊ ×Period _[+24,+30]	-0.224** (0.09)	-0.102*** (0.03)	-0.257*** (0.09)	-0.090*** (0.03)	-0.354*** (0.12)	-0.118*** (0.04)	-0.250** (0.11)	-0.047 (0.04)
Fund fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.05	0.06	0.07	0.07	0.06	0.06	0.06	0.07
Number of funds	640	631	640	630	234	234	234	229
N	39040	38491	39040	38430	14274	14274	14274	13969

Panel B.2. Tests on common pre-treatment dynamics

Variable	Full sample		Matched sample	
	F-statistic	<i>p</i> -value	F-statistic	<i>p</i> -value
Total risk	3.36	0.01	1.74	0.14
ARCH(1) total risk	6.64	0.00	2.51	0.04
Idiosyncratic risk	5.24	0.00	3.97	0.00
ARCH(1) idiosyncratic risk	3.15	0.01	2.32	0.06

Table OA.4.
Other heterogeneous effects of the DFA on performance

The table presents additional analyses of the heterogeneity in the treatment effects of the DFA. *Levered* is an indicator variable for funds that employ leverage. *Young* is an indicator variable for funds whose mean age in the sample period was below the median value. *Lockup* is an indicator variable for funds with lockup clauses. *Strategy* classifies funds into one of the following investment strategies: fund-of-funds, multi-strategy, long-short equity, and other strategies. I collapse into the “other” category the strategies that have a low incidence in the sample (that is, the strategies with less than 10 funds). All the regressions include fixed effects at the fund level and clustered standard errors by fund. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Variable	Return	Alpha	Return	Alpha	Return	Alpha	Return	Alpha
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat×Post×Levered	-0.017 (0.15)	-0.055 (0.16)						
Post×Levered	-0.189** (0.08)	0.022 (0.07)						
Treat×Post×Young			0.179 (0.17)	-0.118 (0.18)				
Post×Young			-0.100* (0.06)	0.012 (0.05)				
Treat×Post×Lockup					-0.251 (0.19)	0.022 (0.22)		
Post×Lockup					-0.400*** (0.08)	-0.314*** (0.07)		
Treat×Post×Strategy _{FundOfFunds}							-0.156 (0.16)	-0.043 (0.17)
Post×Strategy _{FundOfFunds}							0.341*** (0.07)	0.038 (0.05)
Treat×Post×Strategy _{LongShort equity}							-0.639** (0.28)	-0.633* (0.35)
Post×Strategy _{LongShort equity}							0.400*** (0.09)	0.301*** (0.08)
Treat×Post×Strategy _{Multi-strategy}							-0.131 (0.23)	-0.250 (0.18)
Post×Strategy _{Multi-strategy}							0.081 (0.11)	-0.111 (0.08)
Fund fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.28	0.10	0.28	0.10	0.28	0.10	0.28	0.10
Number of funds	640	640	640	640	640	640	640	640
N	39040	39040	39040	39040	39040	39040	39040	39040