

Hedge Fund Regulation and Fund Governance: Evidence on the Effects of Mandatory Disclosure Rules

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Received 4 October 2016; accepted 4 March 2019

ABSTRACT

This paper uses three alternating changes in hedge fund regulation to study whether regulation reduces hedge funds' misreporting, and, if so, why regulation is effective. Relative to public companies, hedge fund regulation is relatively light. Much of the regime is a "comply-or-explain" framework that allows funds to forego compliance with governance rules, providing that they disclose their lack of compliance. The results show that regulation reduces misreporting at hedge funds. Further analysis suggests that the disclosure

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Accepted by Christian Leuz. I am greatly indebted to the five members of my dissertation committee: Fabrizio Ferri, Robert J. Jackson, Jr., Wei Jiang, Sharon Katz, and Shivaram Rajgopal. I also wish to extend a special thank you to two anonymous referees, Jennifer Arlen, Bobby Bartlett, Thomas Bourveau, Ryan Bubb, Matt Cedergren, Jim Cox, Konhee Chang, Rob Daines, Miguel Duro, Jacob Goldin, Joe Grundfest, Luzi Hail, Matt Jacob, Mattia Landoni, Gillian Metzger, Josh Mitts, Jim Naughton, Fernan Restrepo, Ethan Rouen, Steven Davidoff Solomon, Randall Thomas, Ayung Tseng, and Forester Wong for their helpful comments and suggestions. I am also very grateful for feedback I received from workshops at the American Accounting Association, the American Law & Economics Association, Columbia Business School, and the Utah Winter Accounting Conference. A legal-oriented version of this paper was previously circulated under the title Disclosure versus Enforcement and the Optimal Design of Securities Regulation. An online appendix to this paper can be downloaded at <http://research.chicagobooth.edu/arc/journal-of-accounting-research/online-supplements>.

requirements led funds to make changes in their internal governance, such as hiring or switching the fund's auditor, and that these changes induced funds to report their financial performance more accurately.

JEL codes: G20; G23; G28; K22; M42; M48

Keywords: mandatory disclosure; hedge funds; SEC regulation; financial misreporting; auditing

1. Introduction

Most hedge funds were not subject to mandatory disclosure or governance requirements until recently. Even when such requirements were imposed, the regime put in place was relatively light. Rather than mandatory rules to which funds must adhere, much of the regulatory framework for hedge funds is a “comply-or-explain” regime—funds are required to disclose whether they comply with a set of governance provisions, but they may forego compliance providing that they disclose their lack of compliance.

The effectiveness of this regulatory regime has been greatly debated—as has the general question of whether hedge funds should be regulated in the first place (Securities Exchange Commission (SEC) [2003]). Many policy makers have argued that regulating hedge funds is unnecessary because funds' investors are sophisticated enough to detect and deter financial misconduct without government assistance (Atkins [2006]). Opponents have questioned this argument, however, pointing out that the majority of hedge funds' investors are institutional investors who may suffer from a “double agency problem” (Karantininis and Nilsson [2007]). According to these opponents, institutional investors may not be incentivized to fully detect and deter wrongdoing because the separation of client (the primary beneficiary) and investor (the investing institution) creates an agency problem that is similar to the agency problem between a firm's managers and its owners (Gilson and Gordon [2013]).

For these reasons, it is unclear *ex ante* whether regulation affects hedge funds' misreporting. To address this question, my analysis exploits three changes in hedge fund regulation. First, in 2004, the SEC adopted a rule requiring that the majority of hedge funds register with a government securities regulator, thus subjecting these funds to mandatory disclosure, government inspections, and compliance rules. Second, in 2006, the courts vacated the SEC's rule, allowing the funds to withdraw from registration. Third, in 2011, the SEC again adopted rules requiring funds to register with a government securities regulator (these rules were adopted in accordance with the Dodd-Frank Act).

This setting is unique for two reasons. First, it allows for stronger inferences on the question of whether hedge fund regulation reduces misreporting because the three events created alternating changes in the regulatory regime. Second, the setting allows for a better understanding of why regulation is effective. Upon registration, funds are typically subject

to a number of concurrent changes, including government inspections, compliance requirements, and mandatory disclosure. However, a unique feature of the Dodd-Frank Act is that it created a secondary classification of hedge funds known as Exempt Reporting Advisers. Unlike the majority of newly registered funds, the funds that became registered under this new classification were exempt from both government inspections and compliance requirements—these funds were *only* subject to the disclosure rules. This setting therefore allows for examination of the disclosure rules in isolation, providing an opportunity to study whether this specific regulatory component is effective.

My study points to three key findings. First, I find that hedge fund regulation reduces misreporting. Misreporting decreased at the funds that were required to register with the SEC, and increased at the funds that withdrew from registration after the courts vacated the SEC's rule (although this result should be interpreted as descriptive because the decision to withdraw is highly endogenous). Second, I provide evidence suggesting that regulation reduced misreporting by spurring funds to make changes to their internal governance. In particular, after the newly registered funds had to publicly disclose whether they were audited and the name of any such auditor, many hired and/or switched auditors. On average, the funds that made such changes experienced greater declines in misreporting. Finally, by examining the funds only subject to disclosure rules, I show that the imposition of mandatory governance disclosures, even without other concurrent regulatory changes, can significantly decrease misreporting.

All tests are difference-in-differences regressions that compare the hedge funds affected by the regulatory changes to a group of control funds that were already registered with the SEC and did not have a change in regulatory status. To address selection concerns, I include a series of robustness tests (e.g., matched samples and placebo tests). Following prior literature, I identify misreporting using two suspicious patterns in the monthly performance returns that hedge funds report to commercial databases. First, I use the incidence of a fund's "kink" at zero—that is, the presence of more small gains than would be expected based on the number of small losses. This measure is thought to capture whether the fund has managed its returns to avoid reporting a loss, and is the best-known predictor of detected fraud at hedge funds (Bollen and Pool [2012]). Second, following Agarwal, Daniel, and Naik [2011], I determine whether the fund engages in "cookie jar" accounting by testing whether the fund reports abnormally high returns in December.

My paper contributes to several areas of literature. First, I contribute to the literature on comply-or-explain disclosure regimes. Long popular overseas, comply-or-explain regimes are becoming increasingly popular in the U.S. as regulators express concern over one-size-fits-all governance regulation. For example, Sarbanes-Oxley (SOX) does not mandate that a "financial expert" sit on the audit committee, but it does require

such a disclosure (see Coates and Srinivasan [2014] for more U.S. examples). However, the prior evidence is mixed on whether (or when) comply-or-explain regimes effectively nudge users toward “best practices.” Some studies, such as Linck, Netter, and Yang [2009], Akkermans et al. [2007], Dharmapala and Khanna [2018], and Manchiraju and Rajgopal [2017], find high rates of compliance, whereas other studies have found comply-or-explain ineffective (e.g., MacNeil and Li [2006], Bianchi et al. [2011], Hooghiemstra and Van Ees [2011], Seidl, Sanderson, and Roberts [2013], Keay [2014]).

My study contributes to this literature in two key ways. First, my results may help to explain some of the inconsistent findings on comply-or-explain. At least in part, these inconsistent findings are driven by different research designs and definitions of compliance, but there is also evidence that comply-or-explain is more effective in some firms and instances than in others. For example, Akkermans et al. [2007] provide evidence that the firms most likely to be scrutinized are those that comply. My results are consistent with this theory; the funds seemingly most likely to incur scrutiny by the SEC and investors—those that disclose significant conflicts of interest or past disciplinary infractions—experience the greatest declines in misreporting. This is consistent with Lehmann [2018], which found that firms with lower-quality governance benefitted most from the initiation of governance analyst coverage. My finding that comply-or-explain is, on average, effective for hedge funds is also notable, as prior literature has suggested that the effectiveness of comply-or-explain regimes is dependent on the ability of investors to enforce them (Bianchi et al. [2011]). Because hedge fund investors are thought to be sophisticated, comply-or-explain regimes could be more successful in deterring misconduct among hedge funds relative to other firms. This is consistent with prior work showing that disclosure laws need enforcement to be effective (e.g., Christensen, Hail, and Leuz [2013, 2016]).

Second, I contribute to the comply-or-explain literature by focusing on the real effects of the disclosure rules. As a first step, I contacted personnel at the funds in my sample to discuss their experience with registration, and many indicated that the new disclosures spurred them to make internal governance changes. I ran empirical tests and found evidence consistent with this feedback. In particular, the newly registered funds were more likely to switch their auditor, and following the Dodd-Frank Act, to hire an auditor. The funds that made these auditor-oriented changes had greater decreases in misreporting. The evidence that disclosure rules led to real changes in governance provides important evidence on a key channel through which disclosure requirements can effect change. In this regard, my paper is similar to Linck, Netter, and Yang [2009], which found that SOX’s financial expert disclosure has led to a doubling of the number of financial experts on audit committees. It is also similar to Christensen et al. [2017], which found decreases in mining-related citations, injuries, and labor productivity after SEC-registered mine owners were

required to include their mine-safety records in their financial reports.¹ By documenting that comply-or-explain leads to changes in auditing, which in turn leads to a reduction in misreporting, I provide evidence of this specific mechanism.² This finding speaks to the real effects of disclosure (Leuz and Wysocki [2016]) and to long-standing research in accounting on the value of auditing (Watts and Zimmerman [1983, 1986])—particularly auditing at entities, such as private firms, for which financial statement audits are not mandated. Upon testing fund inflows following regulation, I find that inflows increased for the funds that initiated audits, a result consistent with prior work on private firms showing that audited financials play a role in capital allocation (Lisowsky and Minnis [2018]) and are associated with benefits such as lower debt pricing (Minnis [2011]).

Finally, I contribute to the hedge fund literature by providing the first evidence of the effectiveness of disclosure to regulate hedge funds. A small number of studies have suggested that hedge fund regulation reduces misreporting (Cumming and Dai [2010], Hoffman [2013], Dimmock and Gerken [2015]), but most of these are cross-country studies comparing misreporting based on regulation in the fund's home country (the one exception is Dimmock and Gerken [2015]).³ More importantly, however, all of these studies have focused on mandatory rules, regulatory examinations, or both. None have studied the use of disclosure to regulate hedge funds. This is a critical omission given the longstanding debate over the effectiveness of mandating disclosure for hedge funds, whose investors already have access to substantial amounts of information (Brown et al. [2008], Cassar et al. [2018]). By providing evidence that disclosure rules can induce governance changes that, in turn, reduce misreporting, my study provides new evidence on this long-standing debate.

¹ Studies on the real effects of disclosures outside the securities context have led to mixed results (see, e.g., Ho, Ashwood, and Handan-Nader [2017] for an overview). Most prominently, Jin and Leslie [2003] found that restaurant hygiene grades reduced hospitalizations for foodborne illnesses by 20%, but this study has been the subject of criticism (Ho, Ashwood, and Handan-Nader [2017]).

² In this regard, my study differs from those such as Krishnan and Visvanathan [2008] or Erkens and Bonner [2013]. These studies focus on how the board's accounting expertise relates to conservatism and firm status, respectively, not how disclosure of board expertise affected reporting quality and/or status. My study also differs from those that examine investor behavior (see, e.g., Defond, Hann, and Hu [2005], which examines market reactions to the appointment of accounting experts) or investor perception (see, e.g., Chang and Sun [2010], which suggests that SOX improved the credibility of accounting earnings).

³ See also Restrepo [2018], which shows financial performance at the newly registered funds declined following the Dodd-Frank Act, and concludes the decline is consistent with increased conservatism.

2. *Institutional Background*

In securities parlance, a fund becomes “regulated” when the fund’s adviser registers with the proper governmental securities authority.⁴ Upon registration, a fund becomes subject to three major regulatory components: compliance requirements, mandatory disclosure rules, and government inspections. Unless stated otherwise, the descriptions below apply to fund regulated under the Hedge Fund Rule and the Dodd-Frank Act.

2.1 COMPONENTS OF HEDGE FUND REGISTRATION

2.1.1. Compliance Requirements. Registered funds are generally subject to a multitude of compliance requirements. The most notable requirements are that the adviser must adopt written compliance policies and procedures, appoint a Chief Compliance Officer, maintain books and records for at least five years, adopt a code of ethics, and follow strict guidelines on sensitive topics such as performance fees and the use of third parties to solicit new clients. Although some states require audits, SEC-registered advisers are only required to produce audited financials (or to have at least one surprise audit each year) if the adviser has control of its clients’ assets. This is referred to as having “custody” of client assets.

2.1.2. Mandatory Disclosure. Registered funds are required to disclose extensive information to the public in a filing known as Form ADV.⁵ Form ADV requires annual disclosure on a wide range of governance-related matters, including the firm’s clients, managers, accounting practices, potential conflicts of interests, and prior disciplinary history. It does not require funds to disclose their financial performance. Further, unlike the mandatory compliance rules, the disclosures in Form ADV do not require the fund to change its behavior—only to disclose the behavior. For example, a fund is not required to eliminate all significant potential conflicts of interest; it

⁴Registration occurs at the investment adviser level, but I use the term funds for ease of exposition. Legally, the fund and investment adviser are separate entities. The fund holds the assets; the investment adviser manages those assets. Hedge funds are commonly defined as funds that utilize the exemptions found in either Section 3(c)(1) or Section 3(c)(7) of the Investment Company Act of 1940. All investors in such funds must be, at a minimum, “accredited investors” as defined by the SEC’s Regulation D, 17 C.F.R. § 230.501(a) (2015) (generally requiring individuals to have at least \$1 million in net worth, or a \$200,000 annual salary, to qualify as an “accredited investor”). Most funds also seek to avoid the costs of Exchange Act regulation. To do so, the funds must have fewer than 2,000 investors (updated by the Jumpstart Our Business Startups Act).

⁵Form ADV is the only mandatory public filing for most hedge funds, but some funds may be required to file two other forms. First, after the Dodd-Frank Act, registered advisers with over \$150 million in U.S. assets are required to disclose portfolio information on Form PF. This form is nonpublic and is exempt from Freedom of Information Act (FOIA) requests. Second, all advisers with over \$100 million in applicable equity securities are required to disclose equity holdings on Form 13F.

must only disclose those conflicts. Form ADV gives investors potentially important information about their advisers. For example, 21% of the advisers in the full data set of firms that filed Form ADV from 2001 to 2015 disclosed a crime or regulatory infraction. Another 28% disclosed that they are not audited at least annually by an independent public accountant. Form ADV is signed under penalty of perjury, meaning that lying is a serious offense.

2.1.3. Government Inspections. Finally, upon registration, advisers are generally subject to compliance examinations, which involve inspections of the fund and its managers by government officials. From 2004 to 2015, the percentage of advisers examined each year ranged from 11% to 18%. (For comparison, 50% of broker dealers are inspected by FINRA together with the SEC each year (SEC [2016])). These inspections can range from simple records requests to onsite exams lasting for several weeks. The exams generally focus on whether the adviser has fulfilled the compliance requirements described above. Following the exams, most advisers receive a deficiency letter and are given the opportunity to address the issues that the SEC has uncovered (Abromovitz [2012]), but some examinations lead to enforcement actions (CBS [2004]).

2.2 CHANGES IN HEDGE FUND REGULATION

Hedge funds have recently been subject to three significant regulatory changes. First, in 2004, the SEC adopted a controversial rule that required the majority of unregistered hedge funds to register with the SEC. Second, in 2006, the courts vacated the SEC's rule mandating registration, thus allowing the newly registered funds to withdraw from SEC registration. Third, in 2011, the SEC again adopted a rule mandating registration for the majority of unregistered hedge funds (this rule was adopted in accordance with the Dodd-Frank Act). In this section, I describe each of these events in detail.

2.2.1. The SEC's "Hedge Fund Rule". The SEC took a largely "hands off" approach to hedge fund regulation until the collapse of Long Term Capital Management L.P. (LTCM), a prominent hedge fund, in 1998. Following the collapse of LTCM, the SEC became concerned that hedge funds could pose systemic risk to the entire financial system. Eventually, after years of debate, the SEC proposed a rule requiring that the vast majority of unregistered hedge funds register with the SEC (SEC [2004]). Nicknamed the Hedge Fund Rule,⁶ the rule was highly controversial and faced

⁶ The Hedge Fund Rule closed a commonly used exemption that many hedge funds relied upon to avoid registration under the Investment Advisers Act of 1940. At the time this rule was proposed, Section 203(b)(3) of the Investment Advisers Act exempted advisers that did not publicly hold themselves out as investment advisers, did not advise a registered investment company, and had fewer than 15 "clients" over the past 12 months. The definition of "client" included only direct investors, allowing funds to avoid registration if investors placed their money in subfunds that invested in the parent fund. The Hedge Fund Rule redefined "client" to include all investors rather than only direct investors.

significant public opposition (CBS [2004]). Nonetheless, despite the objections of many in the hedge fund community and dissenting votes by two of the five SEC commissioners, the SEC adopted the rule in December 2004.

2.2.2. *Goldstein v. SEC.* In response, Phillip Goldstein of Bulldog Investors (a hedge fund required to register) sued the SEC over the Hedge Fund Rule. In a closely watched lawsuit, he alleged that the SEC had overstepped its authority. In June 2006, the DC Circuit agreed with Mr. Goldstein and vacated the Hedge Fund Rule in an unexpected decision. Media coverage following the event described Mr. Goldstein's "surprising victory," and many lawyers criticized the decision as contrary to past precedent.⁷ Even so, in August 2006, the SEC stated that it would not appeal the DC Circuit's decision and allowed the newly registered funds to withdraw without penalty through January 2007 (SEC [2006]).

2.2.3. *The Dodd-Frank Act.* Congress responded in the Dodd-Frank Act. The Dodd-Frank Act mandated that the vast majority of hedge funds again be subject to registration, but that a subset of funds would be treated as "exempt."⁸ Although Congress exempted these funds from standard compliance requirements, it allowed the SEC considerable latitude in determining the examination and disclosure requirements for "exempt" funds (SEC [2011]). The SEC adopted the rules to implement this provision of the Dodd-Frank Act in June 2011. In doing so, it mandated that the vast majority of funds register as nonexempt, but it determined that "exempt" funds, defined largely by assets under management, would also be exempt from the inspection program—these funds would only be required to file Form ADV.

⁷The following quotes reflect coverage of the event: (1) Somers [2006] ("The ruling marked a surprising victory for Goldstein"); (2) Moyer [2006] ("The controversial rule ... was tossed out in June in a surprise ruling"); (3) HLR [2007] ("Under longstanding administrative law doctrine, this change should have survived legal challenge"); and (4) Mann [2008] ("More surprising than the filing of the suit itself was that Mr. Goldstein ... came out victorious"). The DC Circuit has overturned several SEC rules in recent years, but the Hedge Fund Rule in *Goldstein* was only the fourth SEC rule to suffer this fate. The prior cases overturning SEC rules were *Business Roundtable*, 905 F.2d 406, 408-09 (D.C. Cir. 1990); *Teicher v. SEC* 177 F.3d 1016 (D.C. Cir. 1999); and *Chamber of Commerce of U.S. v. SEC*, 412 F.3d 133 (D.C. Cir. 2005) and *Chamber of Commerce of U.S. v. SEC*, 443 F.3d 890 (D.C. Cir. 2006) (the Chamber cases addressed the same rule).

⁸Two other sections of the Dodd-Frank Act could have affected hedge funds. First, Sec. 929P gave the SEC expanded authority to impose administrative fines on persons associated with securities transactions. Second, provisions such as Sec. 922 increased whistleblower incentives and protections. However, I do not think these changes are driving my results because, although there is a significant range of effective dates for these provisions, they largely differ from the effective date for the regulations I study here. Further, these provisions affected all hedge funds, meaning that I would not expect them to have a disproportionate effect on the treatment funds relative to the control funds.

2.3. UNREGISTERED HEDGE FUNDS

Even prior to these mandatory registration rules, an estimated half of funds were registered with the SEC (CBS [2004]). Advisers to companies registered under the Investment Company Act of 1940 (ICA) were required to register (i.e., a hedge fund adviser that also advised a mutual fund registered under the ICA would have to register). Other funds registered voluntarily to achieve Qualified Professional Asset Manager status under the Employee Retirement Income Security Act of 1974. Achieving this status allows fund managers to engage in transactions that would otherwise be prohibited. Further, some funds likely registered for perceived marketing benefits or because their clients demanded they register.

Although unregistered funds were commonly referenced as “unregulated,” they were still subject to antifraud rules. This meant regulators could perform inspections—and bring enforcement actions—if there was reason to believe the fund was committing fraud. However, the effectiveness of the regulatory regime for unregistered funds has been questioned. For example, consider Bernard Madoff’s Ponzi scheme. In total, the SEC received eight separate complaints regarding Madoff from six different sources prior to his eventual confession in December 2008 (all but one occurred while Madoff was unregistered). In response to these complaints, the SEC initiated two investigations and conducted three examinations, but found no evidence of a Ponzi scheme. (SEC [2009]).

3. Methodology

3.1 DATA

To evaluate how regulation affected misreporting by hedge funds, I assembled a data set from two key sources. First, I gathered data on the regulatory history of each fund from historical Form ADV filings. Second, I obtained data on the funds’ financial performance from the Lipper Hedge Fund database.

3.1.1. Form ADV. As noted above, Form ADV is the only publicly available mandatory filing for most hedge funds. Although it is unclear how this disclosure affects fund behavior, prior work has shown that the information is valuable to investors. For example, in the most comprehensive study on the utility of Form ADV, Dimmock and Gerken [2012] find that investors who avoid the 5% of firms with the highest ex ante fraud risk based on Form ADV disclosures can avoid over 40% of the dollar losses due to fraud. And Brown et al. [2008, 2009, 2012] provide evidence that information in Form ADV filings predicts future performance. I obtained Form ADV for this project after a lengthy appeals process with the SEC. Prior to this project, the SEC denied all FOIA requests for

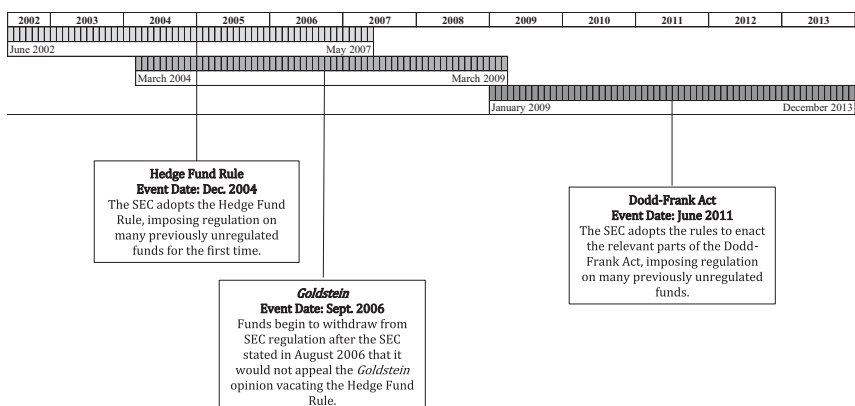


FIG. 1.—Timeline of event windows and dates. This figure shows the event windows and dates used in the analysis. Each window contains 30 months.

historical information, making Form ADV data generally unavailable to academic researchers.⁹

3.1.2. Lipper Hedge Fund Database. I obtained information on hedge funds' financial performance from the Thomson Reuters Lipper Hedge Fund database (also known as the Trading Advisor Selection System (TASS) database). This is a commercial database to which hedge funds report in order to market themselves to potential investors (Agarwal, Fos, and Jiang [2013]). The Lipper Hedge Fund database is recorded at the fund level, whereas Form ADV is filed by the investment adviser. To combine these databases, I performed a one-to-many merge.

3.2 RESEARCH DESIGN

3.2.1. Event Windows. I study each of the three regulatory changes described earlier—the Hedge Fund Rule, *Goldstein*, and the Dodd-Frank Act—using a difference-in-differences design comparing misreporting in the 30 months before and after each change in law. The event dates are described below and summarized in figure 1.

- Hedge Fund Rule: June 2002 to May 2007. The SEC adopted the Hedge Fund Rule in December 2004, so I include the 60 months surrounding this event date. Although funds were not required to register immediately after the SEC adopted the final rules to require

⁹As a result of my endeavors to obtain Form ADV for this project, the SEC now makes historical data available. To my knowledge, the only prior studies that use time-series Form ADV data use the data set described by Dimmock and Gerken [2012], who note that their Form ADV data were not publicly available.

registration, I use the month of rule adoption as the event date to reflect the date when the funds began the registration process.¹⁰

- *Goldstein* decision: March 2004 to March 2009. The first funds to withdraw from registration did so in September 2006 (the month after the SEC announced in August 2006 that it would not appeal the *Goldstein* decision), so I include the 30 months before and after this event date.
- Dodd-Frank Act: January 2009 to December 2013. The SEC adopted the rules to implement the related requirements of the Dodd-Frank Act in June 2011, so I include the 60 months surrounding this event date.

3.2.2. Control and Treatment Samples. In short, the control funds are those that were already registered with the SEC before the change in law, and the treatment funds are those that had a change in registration status due to the change in law. However, in the *Goldstein* setting, the treatment funds are divided into two groups: those that remain registered and those that withdraw. In addition to the below description of the treatment and control funds, a detailed summary is provided in exhibit S1 of the online appendix.

3.2.2.1. Control Funds. I identify the control funds by using the initial registration date included in Form ADV. For example, a fund that first submitted to registration in March 2001 and remained registered through 2013 would have a March 2001 registration date in all future filings, and I would include such a fund as a control fund in all tests. However, a fund that first submitted to registration in April 2005 and remained registered thereafter would have an initial registration date of April 2005, and I would include such a fund as a control fund only in the Dodd-Frank Act setting.

3.2.2.2. Treatment Funds. To identify the funds that had a change in registration status following each change in law, I use each fund's initial registration date in Form ADV and, if applicable, the date the fund ceased filing Form ADV. Because of the aforementioned gap between the date of rule adoption and the date the funds needed to register with the SEC, I conservatively identify the treatment funds as those that registered with the SEC in the six months prior to the deadline imposed by the relevant law. (This six-month cutoff makes little difference because few funds registered immediately after the rules were adopted.) I take this approach because the registration process, like the IPO process, is time consuming. A fund that registered immediately after the Hedge Fund Rule was adopted is likely a

¹⁰ The registration process is cumbersome, so the SEC did not require funds to register immediately after rule adoption. Funds had until January 31st, 2006 for the Hedge Fund Rule and March 31st, 2012 for the Dodd-Frank Act. My conversations with regulators and fund personnel indicated that funds made significant changes during the preparatory period (i.e., the period following rule adoption and before registration), and that studying the funds only after they were registered would ignore these changes. Moreover, because SEC inspectors may demand records from prior to the registration date, funds could expect that the preparatory period would be subject to examination.

voluntary registrant that began the registration process before the rule was adopted. The specific dates are provided below.

- Hedge Fund Rule: funds that submitted to SEC registration from August 2005 through January 2006 (deadline to register of February 1st, 2006).
- *Goldstein*: I split the funds that registered in response to the Hedge Fund Rule into two groups of interest: (1) funds that remained registered with the SEC after *Goldstein* (*Remain*) and (2) funds that withdrew from SEC registration after *Goldstein* (*Withdraw*). The *Remain* funds are those that registered in response to the Hedge Fund Rule and remained registered through March 2009 (the end of the event window), and the *Withdraw* funds are those that registered in response to the Hedge Fund Rule and withdrew at any point from September 2006 (event date) through January 2007 (deadline to withdraw without penalty).
- Dodd-Frank Act: funds that submitted to SEC registration from October 2011 through March 2012 (deadline to register of March 30th, 2012).

The identification of the treatment and control samples means that the same fund can be classified differently in different settings. For example, imagine that Alpha Fund first registered with the SEC in December 2005 and remained registered through the end of 2013. Alpha would be a treatment fund for the Hedge Fund Rule, a *Remain* fund for *Goldstein*, and a control fund for the Dodd-Frank Act. As another example, imagine that Beta Fund registered with the SEC in August 2005, withdrew from registration in October 2006, and never registered again (assume Beta died). Beta would be a treatment fund for the Hedge Fund Rule, a *Withdraw* fund for *Goldstein*, and would not be included in the Dodd-Frank Act analysis.¹¹

I impose three final constraints. First, to have a balanced panel, I only include funds with data throughout an entire event window. For example, a fund that began reporting to the Lipper Hedge Fund database in January 2007 would be included in the Dodd-Frank Act analysis but would not be included in the earlier analyses because it was only present for part of the event window. Second, I drop funds that do not meet the criteria of either the treatment or control samples. For example, a fund that first registered with the SEC in October 2004 (two months before the SEC adopted the Hedge Fund Rule) would be omitted. This fund would not be a control fund because it was not registered throughout the entire period prior to registration, and it would not be a treatment fund because it did not

¹¹ In such a hypothetical, Beta would have an initial registration date of August 2005 in every Form ADV filing from August 2005 through October 2006 but would not appear in the Form ADV filings after October 2006. For funds that disappear from the Form ADV sample, I confirm that the fund withdrew from registration by hand-checking the termination date for each fund using the Investment Adviser Public Disclosure Web site.

become registered due to the Hedge Fund Rule. Third, to be consistent across the settings, I only include first-time registrants as treatment funds. This means, for example, that the treatment group for the Dodd-Frank Act excludes 10 funds that withdrew after *Goldstein* only to register again upon the implementation of the Dodd-Frank Act.

4. Descriptive Statistics

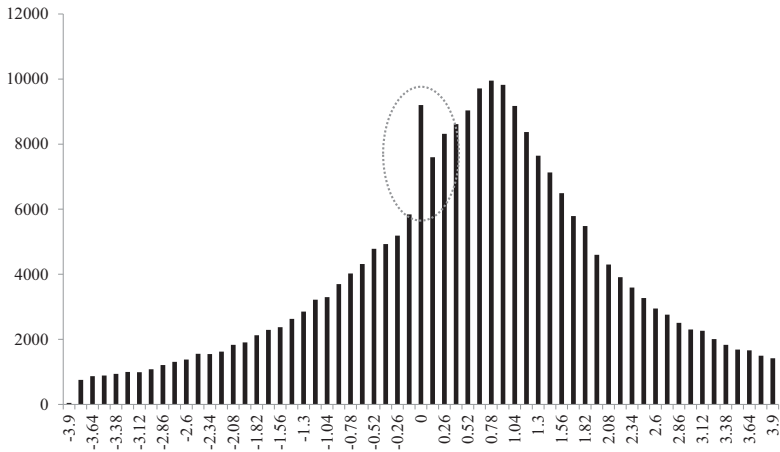
4.1 MEASURES OF MISREPORTING

It is notoriously difficult to measure misreporting by hedge funds. Managers are often thought to have significant discretion in valuing hedge funds' assets because the funds have substantial holdings of level 2 and level 3 assets—assets for which there is no clear pricing benchmark—and it is difficult to obtain detail about these assets because portfolio data are not available. Although some funds seek to minimize managerial discretion through external monitoring mechanisms such as auditing and independent pricing, evidence suggests that these methods reduce, but do not eliminate, misreporting (e.g., Cassar and Gerakos [2010, 2011], Brown et al. [2012]). Moreover, they are not universally adopted—one study found that managers have full discretion to price assets in almost 20% of funds (Cassar and Gerakos [2011]).

Due to these hurdles, academic studies have estimated misreporting at hedge funds by identifying suspicious patterns in the monthly performance returns that hedge funds report to their investors. This approach captures manipulation of the underlying assets, relying on the fact that fund returns are based on the monthly change in net assets (before inflows or outflows from investors and after fees). As described below, I follow prior studies and identify misreporting using two suspicious patterns in reported monthly returns. Examples of the calculations for each measure are provided in exhibit S2 of the online appendix.

4.1.1. Kink at Zero. The first measure captures whether a fund reports fewer monthly returns just below zero than would be expected based on the number of monthly returns just above zero (Burgstahler and Dichev [1997], Beaver, McNichols, and Nelson [2007], Bollen and Pool [2009, 2012]). A visual representation of the kink is presented in panel A of figure 2, which shows the distribution of monthly returns for all funds in the Lipper Hedge Fund database from 2000 to 2013 (the bin width of 13 basis points is set according to the optimal bin width formula described in Silverman [1986]). The intuition is that, absent misreporting, monthly returns will follow a smooth and relatively normal distribution over time. However, because the number of months with positive returns is a significant determinant of fund inflows, fund managers are incentivized to turn small losses into small gains (Agarwal, Daniel, and Naik [2011]). This measure of misreporting was made famous by Bernard Madoff, who violated it widely. Out of 16 years (215 months), Madoff had only 16 months with

Panel A: Kink.



Panel B: “Cookie Jar” Accounting.

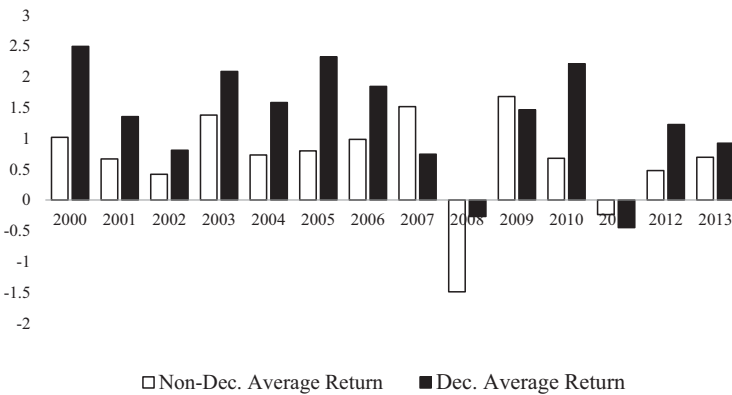


FIG. 2.—This figure shows the measures of misreporting. Panel A describes the “kink” in the distribution of monthly hedge fund returns and indicates that, relative to the surrounding bins, there is a significant spike in the frequency of fund returns reported in the bin just above zero. The bin width of 13 basis points is set according to the optimal bin width formula in Silverman [1986]. Panel B describes mean hedge fund returns in December and non-December months, and indicates that mean fund returns in December were higher than mean fund returns in other months in 10 of the 13 years from 2000 to 2013. Both figures are based on all funds in the Lipper Hedge Fund database from 2000 to 2013.

negative returns (i.e., “down” months)—making for 92.56% winning months (Bernard and Boyle [2009]). Perhaps ironically in retrospect, the Fairfield Sentry Fund, which invested solely in Madoff, used the limited number of down months as a selling point in its marketing materials (Fairfield [2007]).

To test for a kink at each individual fund over the 30-month event window, I create three bins surrounding zero. The first bin includes monthly returns from -1% to -0.50% , the second from -0.50% to 0% , and the third from 0% to 0.50% (I use a bin width of 50 basis points following the fund-specific measure of discontinuity in Bollen and Pool [2009]). All bins include the upper limit. For each 30-month period, I then test whether the number of observations in the bin just below zero is less than expected based on the average of the two surrounding bins. Statistical significance is calculated in accordance with Burgstahler and Dichev [1997], as modified by Beaver, McNichols, and Nelson [2007], and I consider the fund to have misreported if the number of observations in the bin below zero is statistically lower than expected with a t -statistic of 1.96 or greater.

4.1.2. Cookie Jar Accounting. My second measure of misreporting is based on whether a fund uses “cookie jar” accounting—that is, whether the fund accumulates reserves during good times in order to protect against bad times. Prior literature has suggested that one way to test for cookie jar accounting at hedge funds is to look for abnormally high returns in December (Agarwal, Daniel, and Naik [2011]). There are two reasons why funds that have accumulated reserves during the year may recognize these gains in December. First, managers want these returns to be recognized before the year ends for purposes of determining their annual compensation. Second, most hedge fund audits take place at the end of the year, so managers are keen to bring their books into compliance before the audit takes place. Cookie jar accounting is thought to be particularly problematic when the fund’s investors have different investment horizons (e.g., if investor A withdraws her investment in November but investor B does not withdraw until January).

Following Agarwal, Daniel, and Naik [2011], panel B of figure 2 shows the average returns for all hedge funds, both in the month of December and in non-December months, in all years from 2000 to 2013. The figure shows that average returns in December are higher than average returns for other months in 10 out of the 13 years. Notably, the years in which December returns are lower—2007, 2009, and 2011—are years in which cookie jar accounting may not have been an option because of the financial crisis and its aftershocks. To test for cookie jar accounting at the fund level, I regress each fund’s monthly returns for the applicable 30-month period on the seven hedge fund risk factors used by Fung and Hsieh [2004], an indicator for the month of December, and year fixed effects. The seven hedge fund risk factors are included to control for general economic factors that may affect fund returns. I consider the fund to have misreported if the coefficient on the December indicator variable is significantly positive with a t -statistic of 1.96 or greater.

4.2 FREQUENCY OF MISREPORTING

Table 1 shows the frequency of misreporting by the treatment and control funds. If a fund triggers either of the two measures of misreporting, I consider it a “flag” for misreporting.¹² Panel A presents the aggregated number of flags per fund before and after registration, and panel B shows the results for each proxy. Both panels present the results for the full and matched samples. To create the matched sample, I rely on three restrictions. First, each treatment fund must be matched with a control fund that has the same level of misreporting in the period prior to the change in law. Second, U.S. funds must be matched to U.S. funds (and non-U.S. funds to non-U.S. funds). Third, treatment funds must be matched to control funds with the same investment style (e.g., long-equity funds will be matched). Treatment funds without a match along these three criteria are dropped. If a fund has multiple potential matches, I next match treatment and control funds with the most similar propensity to be unregistered, where propensity to be unregistered is determined using a probit model. Each probit model (untabulated) includes monthly returns, performance, age, return volatility, sensitivity to liquidity, and audit history. Because the treatment funds in the *Goldstein* analysis are partitioned into two groups (those that remain registered and those that withdrew after the decision), I drop the funds that remain registered and match only the control funds and the funds that withdrew.

As a general pattern, the frequency of misreporting at the treatment group decreased relative to the control group after registration. For example, 13% of funds that registered in response to the Hedge Fund Rule had a statistically significant kink prior to registration, whereas only 7% of the control funds had such a kink. In the period following registration, however, 6% of the treatment funds and 12% of the control funds had a significant kink.¹³

¹² I treat misreporting as binary and record only whether the fund deviated from the expected distribution in the predicted direction of misreporting—not the severity of the deviation. I follow this approach because not all deviations are equal. For example, if a fund has a significant positive kink above zero, I would consider that misreporting. However, if a fund has a significant negative kink above zero, I have no theoretical explanation for why such a kink reflects misreporting. Hence, treating the variable as binary allows for consistency with the underlying theory.

¹³ One concern is that the control funds may have increased misreporting after the changes in law. Indeed, prior research on SEC enforcement suggests that entities may increase misreporting when the SEC is distracted and unlikely to monitor effectively (Kedia and Rajgopal [2011]). To test for this possibility, I compare the control funds to funds that were completely unconnected to the U.S. regulatory regime (i.e., *Unaffected* funds). My sample of funds completely unconnected to the U.S. regulatory regime includes all funds in the Lipper Hedge Fund database that are located outside the U.S., do not file Form ADV, and report throughout the entire relevant period. Following both the Hedge Fund Rule and the Dodd-Frank Act, the control funds follow the same trend as the *Unaffected* funds, suggesting that changes in misreporting are driven by economic fluctuations rather than a response to regulation. The results are presented in the online appendix in table S6.

TABLE 1
Descriptive Statistics on the Frequency of Flags for Misreporting

	Number of Flag for Misreporting—Full Sample									
	Hedge Fund Rule			<i>Goldstein</i>			DodD-Frank Act			
	Control	Treat.	t-test	Control	Withdraw	Remain	Control	Treat.	t-test	
Before	0.15	0.26	-2.35**	0.22	0.11	0.10	0.32	0.39	-1.41	
After	0.31	0.17	2.28**	0.06	0.09	0.03	0.36	0.13	4.53**	
t-test (after vs. before)	-3.39**	1.57		5.54**	0.32	2.02*	-1.11	4.37**		
Diff. (after - before)	-0.15	0.09	-3.25**	0.17	0.02	0.07	-0.03	0.26	-4.18**	

	Number of Flag for Misreporting—Matched Sample									
	Hedge Fund Rule			<i>Goldstein</i>			DodD-Frank Act			
	Control	Treat.	t-test	Control	Withdraw	t-test	Control	Treat.	t-test	
Before	0.20	0.20	0.00	0.10	0.10	0.00	0.41	0.41	0.00	
After	0.45	0.13	3.62**	0.02	0.10	-1.69*	0.24	0.10	2.44**	
t-test (after vs. before)	-2.77**	1.09		1.69*	0.00		2.30**	4.82**		
Diff. (after - before)	-0.25	0.07	-2.91**	0.08	0.00	1.04	0.16	0.30	-1.47	

(Continued)

TABLE 1—Continued

Panel B:	Frequency of Each Flag—Hedge Fund Rule															
	Before Registration				After Registration				Before Registration				After Registration			
	Full Sample		Matched Sample		Full Sample		Matched Sample		Full Sample		Matched Sample		Full Sample		Matched Sample	
	Control	Treat.	<i>t</i> -test	Control	Treat.	<i>t</i> -test	Control	Treat.	<i>t</i> -test	Control	Treat.	<i>t</i> -test	Control	Treat.	<i>t</i> -test	
Kink	0.07	0.13	-1.72*	0.12	0.06	1.63	0.11	0.10	0.22	0.15	0.04	2.33**	0.15	0.04	2.33**	
Cookie Jar	0.08	0.13	-1.64	0.19	0.11	1.97**	0.09	0.11	-0.27	0.31	0.09	3.37***	0.31	0.09	3.37***	
Frequency of Each Flag—Goldstein																
	Before Court Opinion				After Court Opinion				Before Court Opinion				After Court Opinion			
	Full Sample		Matched Sample		Full Sample		Matched Sample		Full Sample		Matched Sample		Full Sample		Matched Sample	
	Control	Withdraw	Remain	Control	Withdraw	Remain	Control	Withdraw	Remain	Control	Withdraw	Remain	Control	Withdraw	Remain	<i>t</i> -test
Kink	0.09	0.06	0.04	0.03	0.09	0.03	0.04	0.06	0.03	0.04	0.06	0.04	0.00	0.04	0.10	-2.29**
Cookie Jar	0.13	0.05	0.06	0.03	0.00	0.00	0.06	0.04	0.00	0.06	0.04	0.00	0.02	0.00	0.00	1.00

(Continued)

TABLE 1—Continued

Panel B:	Frequency of Each Flag—Dodd-Frank Act											
	Before Registration			After Registration			Before Registration			After Registration		
	Full Sample			Matched Sample			Full Sample			Matched Sample		
	Control	Treat.	t-test	Control	Treat.	t-test	Control	Treat.	t-test	Control	Treat.	t-test
Kink	0.01	0.03	-0.96	0.02	0.01	1.04	0.02	0.02	0.02	0.00	0.00	0.00
Cookie Jar	0.31	0.37	-1.18	0.33	0.12	4.41**	0.38	0.38	0.00	0.24	0.10	2.44**

This table provides descriptive statistics on the frequency of “flags” for misreporting at the treatment and control funds. Panel A shows the aggregate number of flags for the full and matched samples. Panel B presents the disaggregated results for each proxy. The treatment funds are those that had a change in registration status following the change in law, and the control funds are those that were continuously registered with the SEC throughout the entire event window. The treatment funds for *Goldstein* are partitioned into two groups: *Withdraw* and *Remain*. Funds that withdrew from SEC registration are assigned to the withdraw group, and those that remained registered are assigned to the remain group (the funds that remained registered are omitted from the matched sample). Detail on the assignment of treatment and control funds is provided in exhibit S1 of the online appendix.

4.3 FUND CHARACTERISTICS

Table 2 describes other characteristics of the treatment and control funds. The table shows each fund's mean monthly return, mean natural log of net asset value, and mean age over the 30 months prior to the event date. I also include the fund's return volatility over the period, whether the fund is incorporated in the U.S., the sensitivity of the fund to market liquidity, and the average number of months that are audited each year. Sensitivity to liquidity is measured by regressing the fund returns over each period on the Sadka [2006] permanent liquidity variable, where the resulting beta on the Sadka variable is then included in the regressions as a control. Panel A uses the full sample and shows that there are some significant differences between the treatment and control groups. Notably, the treatment funds have better performance and greater return volatility. However, the differences are largely mitigated using the matched sample in panel B.

5. Main Results

This section presents the results of the difference-in-differences multivariate tests. Unless otherwise stated, the dependent variable is the number of flags for misreporting triggered, and controls are included for the variables noted in table 2. As with the descriptive statistics, there is one observation per fund in each period (e.g., the number of observations in column 1 of table 3, panel A, is 722, which is twice the number of funds in the Hedge Fund Rule setting in table 2, panel A). Standard errors are clustered by fund, and all models are run using OLS. Results are presented first using fixed effects for each fund's country of incorporation and investment style, and second using fund fixed effects. All reported p -values reflect two-sided tests. Table S7 in the online appendix compares the baseline standard errors with bootstrapped errors, errors clustered by investment style, and errors clustered by the fund's country of origin (on the whole, the results remain consistent). Table S8 of the online appendix compares the coefficients of interest if I were to use logit instead (the results again remain consistent).

5.1 THE EFFECT OF REGULATION ON MISREPORTING

Table 3 compares misreporting at the treatment and control funds after each change in law. In panel A, columns 1–4 present the results for the Hedge Fund Rule, and columns 5–8 present the results for the Dodd-Frank Act. Panel B presents the results for *Goldstein*. All models in Panel A use the equation below and include the 60 months surrounding the event date. *Post* is set to one in the period after the rule was adopted and to zero in the period before. *Newly Registered Fund* is set to one for all treatment funds and to zero for all control funds (throughout the analysis, *Newly Registered Fund* is omitted from the models with fund fixed effects due to collinearity). As noted previously, results are presented first using fixed effects for

TABLE 2
Descriptive Statistics on Treatment and Control Funds

Variable	Hedge Fund Rule			Dodd-Frank Act			Goldstein					
	Control	Treat.	tstat	Control	Treat.	tstat	Control	Withdraw	tstat	Remain	Withdraw	tstat
Monthly Return	0.79	0.94	-2.03**	1.23	1.67	-3.81***	0.57	0.83	-3.43***	0.77	0.83	-0.54
Ln (Net Asset Value)	6.35	5.71	3.60***	6.01	6.27	-1.80*	6.17	5.60	2.27**	6.06	5.60	1.96*
Age	1.22	1.07	0.66	2.68	2.76	-0.26	1.50	1.97	-1.25	1.20	1.97	-1.99**
Return Volatility	2.02	2.58	-2.89***	2.78	3.53	-3.24***	1.78	2.85	-5.06***	2.69	2.85	-0.48
U.S. Incorporation	0.33	0.20	2.62***	0.30	0.32	-0.51	0.30	0.13	2.72***	0.20	0.13	1.09
Liquidity Sensitivity	84.98	87.88	-0.14	-12.65	-20.62	1.00	-13.12	22.16	-3.98***	19.54	22.16	-0.20
Num. Audited Months	0.02	0.02	-0.97	0.11	0.15	-2.60***	0.06	0.08	-1.01	0.07	0.08	-0.50
<i>Num. Funds</i>	235	126		569	112		289	55		102	55	

(Continued)

TABLE 2—Continued

Variable	Hedge Fund Rule			Dodd-Frank Act			Goldstein		
	Control	Treat.	t-stat	Control	Treat.	t-stat	Control	Withdraw	t-stat
Monthly Return	0.79	1.02	-1.90*	1.24	1.54	-1.69*	0.76	0.80	-0.26
Ln (Net Asset Value)	5.47	6.04	-2.64***	6.66	6.36	1.26	6.01	5.67	0.95
Age	0.93	1.14	-0.70	2.54	2.42	0.27	1.81	1.77	0.06
Return Volatility	2.25	2.58	-1.05	3.06	3.09	-0.10	2.15	2.74	-1.54
U.S. Incorporation	0.28	0.28	0.00	0.30	0.30	0.00	0.14	0.14	0.00
Liquidity Sensitivity	116.81	81.05	0.99	-9.55	-3.23	-0.54	3.62	23.86	-1.37
Num. Audited Months	0.02	0.02	0.16	0.12	0.15	-1.44	0.08	0.09	-0.29
Num. Funds	75	75		86	86		49	49	

This table provides descriptive statistics on the treatment and control funds. Panel A reflects the full sample of funds. Panel B reflects the matched sample. All funds are as defined in table 1. The table shows each fund's mean monthly return, mean log of net asset value, mean age (in years), return volatility (measured as the standard deviation of monthly returns), sensitivity to liquidity, whether the fund is incorporated in the U.S. and mean number of audited months per year. The fund's sensitivity to liquidity is measured by regressing the fund returns on the Sadka [2006] permanent liquidity variable, where the resulting beta on the Sadka variable is then considered to reflect the fund's sensitivity to liquidity. The variables reflect the fund characteristics in the 30 months prior to the event date.

TABLE 3
Registration and Misreporting

	Hedge Fund Rule			Dodd-Frank Act				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Sample		Matched Sample		Full Sample		Matched Sample	
Post	0.188*** (0.051)	0.306*** (0.109)	0.285** (0.113)	0.373* (0.210)	0.026 (0.033)	-0.445 (0.554)	-0.157** (0.076)	0.028 (0.954)
Newly Registered Fund	0.132** (0.056)		0.025 (0.079)		0.121** (0.049)		-0.029 (0.069)	
Post Newly Registered Fund	-0.241*** (0.072)	-0.220** (0.103)	-0.345*** (0.124)	-0.297* (0.174)	-0.288*** (0.065)	-0.285*** (0.094)	-0.207** (0.090)	-0.224* (0.133)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char.	Fund	Char.	Fund	Char	Fund
Observations	722	722	300	300	1,362	1,362	344	344
Rsquared	0.11	0.55	0.28	0.58	0.16	0.56	0.11	0.55

(Continued)

TABLE 3—Continued

	Goldstein					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B:	Full Sample			Matched Sample		
	Full Sample			Three-Period Model		
Post HF Rule-Pre Goldstein						
Post Goldstein	-0.153*** (0.035)	-0.170** (0.077)	-0.083* (0.048)	-0.243* (0.134)	-0.096** (0.043)	-0.057 (0.051)
Withdraw	-0.125** (0.057)		-0.004 (0.065)		-0.307*** (0.054)	-0.338*** (0.088)
Remain	-0.128*** (0.044)				-0.199*** (0.071)	
Post HF Rule-Pre Goldstein * Withdraw					-0.069 (0.079)	
Post HF Rule-Pre Goldstein * Remain					0.053 (0.074)	0.035 (0.088)
Post Goldstein * Withdraw	0.155** (0.069)	0.160* (0.097)	0.064 (0.081)	0.086 (0.108)	0.284*** (0.067)	0.242** (0.079)
Post Goldstein * Remain	0.097** (0.047)	0.086 (0.068)			0.070 (0.076)	0.041 (0.092)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char.	Fund	Char.	Fund
Observations	892	892	196	196	855	855
Adjusted R-squared	0.07	0.09	0.16	0.10	0.13	0.33

This table examines SEC registration and misreporting. All models are run using OLS, and the dependent variable is the number of flags for misreporting. Panel A analyzes the Hedge Fund Rule and Dodd-Frank Act. Panel B shows the analysis for Goldstein. In panel A, the variable *Newly Registered Fund* is set to 1 for the newly registered funds (i.e., the treatment funds), and to 0 for all funds that were continuously registered with the SEC throughout the entire sample period (i.e., the control funds). In panel B, the variable *Withdraw* is set to 1 for all funds that registered in accordance with the Hedge Fund Rule and later withdrew after it was vacated. The variable *Remain* is set to 1 for all funds that registered in accordance with the Hedge Fund Rule and remained registered after it was vacated. Columns 5 and 6 of panel B use an extended model that includes the following three periods: June 2002 to November 2004 (*Before HF Rule* period), December 2004 to September 2006 (*Post HF Rule-Pre Goldstein* period), and October 2006 to March 2009 (*Post Goldstein* period). The *Before HF Rule* period is dropped due to collinearity. All models include the control variables noted in table 2. Fixed effects are included either for the fund's country of incorporation and investment style (fixed effects for fund characteristics, Char.) or for the fund itself (Fund). Standard errors (in parentheses) are clustered by fund. Statistical significance of 10%, 5%, and 1% is indicated by *, **, and ***, respectively.

each fund's country of incorporation and investment style, and second using fund fixed effects.

$$\begin{aligned} \text{Num. Flags} = & \alpha + \beta_1 \text{Post} + \beta_2 \text{Newly Registered Fund} \\ & + \beta_3 \text{Post} * \text{Newly Registered Fund} + \text{Controls} \\ & + \text{Fixed Effects} + \varepsilon. \end{aligned}$$

The variable of interest, the interaction term between *Post* and *Newly Registered Fund*, is negative and statistically significant in all models. The results show that, following the adoption of mandatory registration, the mean fund subjected to registration triggered roughly 0.21–0.35 fewer flags than would have been expected based on the control sample. This large decrease in misreporting is consistent with prior literature. For example, Dimmock and Gerken [2015] used six flags to test the change in misreporting following the Hedge Fund Rule, and they found that the implied estimate of the decline for four of those flags ranged from 37.3% to 57.8%.¹⁴ One explanation for these large magnitudes is that the frequency of flags for misreporting in the sample is low, thus leading to large percentage changes from the pre to post periods.

Using the equation below, panel B turns to the DC Circuit's decision in *Goldstein*, which provides an opportunity to examine the relation between regulation and misreporting in a setting where regulatory oversight is reduced rather than imposed (as noted earlier, the results are descriptive because the decision to withdraw is highly endogenous).

$$\begin{aligned} \text{Num. Flags} = & \alpha + \beta_1 \text{Post} + \beta_2 \text{Withdraw} + \beta_3 \text{Remain} \\ & + \beta_4 \text{Post} * \text{Withdraw} + \beta_5 \text{Post} * \text{Remain} + \text{Controls} \\ & + \text{Fixed Effects} + \varepsilon. \end{aligned}$$

Withdraw is set one if the fund submitted to oversight in accordance with the Hedge Fund Rule and withdrew post-*Goldstein*, and *Remain* is set to one if the fund submitted to oversight in accordance with the Hedge Fund Rule and remained registered. Both are set to zero for the control funds. The primary variables of interest are the interaction terms between *Post* and *Withdraw* and between *Post* and *Remain*, which reflect the change in misreporting, respectively, for the newly registered funds that withdrew and the newly registered funds that remained registered after *Goldstein*, relative to the change in misreporting for the control funds during the same period. Results using the full sample in columns 1 and 2 show that, post-*Goldstein*, the funds that withdrew from SEC registration increased misreporting relative to the control funds. However, as shown in columns 3 and 4, this finding is not robust to the matched sample.

¹⁴ For comparison, if I run panel A of table 3 at the individual flag level—rather than aggregating both flags—I find estimated declines of 10% to 15%.

Columns 5 and 6 use an alternate specification that includes three periods: the 30 months before the Hedge Fund Rule, the 22 months between the adoption of the Hedge Fund Rule and *Goldstein*, and the 30 months after *Goldstein*. The use of this three-period model is meant to address the overlap between the different event windows that is caused by the use of 30-month periods in the original model (the post period for the Hedge Fund Rule ends in May 2007, but funds began to withdraw after *Goldstein* in September 2006). The original model uses this lengthy event window to have sufficient observations to detect a pattern of misreporting and run the regression testing for cookie jar accounting. However, the three-period model shortens the period between the Hedge Fund Rule and *Goldstein* to 22 months to avoid this overlap. In these columns, the interaction term between *Post Goldstein* and *Withdraw* is positive and statistically significant, providing further evidence that the funds that withdrew increased misreporting after *Goldstein*. However, the interactions between the variable reflecting the 22-month period between the Hedge Fund Rule and *Goldstein* (*Post HF Rule-Pre Goldstein*) and the two treatment variables (*Remain* and *Withdraw*) are not significant, a finding inconsistent with columns 1–4 in panel A. One explanation may be the reduced number of observations (22 months) in the period between the adoption of the Hedge Fund Rule and *Goldstein* and the resulting reduction in power.¹⁵

5.2 REAL EFFECTS OF REGULATION

Having provided evidence that regulation reduced misreporting by hedge funds, I now turn to a separate question: *Why* did regulation reduce misreporting? As a preliminary step, I reached out to hedge funds in my sample to discuss their experiences with the registration process. These inquiries provided helpful anecdotal evidence outlining two possible mechanisms through which hedge fund registration may have reduced misreporting. First, upon being required to publicly disclose whether they conformed to best practices, funds indicated that they became more likely to conform to best practices. Second, many respondents indicated that chief compliance officers enjoyed increased status upon SEC registration.

These anecdotes provide plausible explanations for why reporting accuracy increased following registration. However, these explanations are difficult to test empirically because very little information is available on funds' internal governance prior to registration. The commercial databases are largely focused on funds' financial performance, not their governance. However, the Lipper Hedge Fund database notes the most recent audit date and auditor for each fund. Using historical data for these fields, I identified funds that either hired and/or switched auditors after registration.

¹⁵ For example, a fund with ~13% of its observations in each of the top and bottom buckets but zero observations in the middle bucket would have a *t*-stat of 2.15 in a 30-month period, but a *t*-stat of only 1.86 in a 22-month period.

I consider a fund to have hired an auditor if none of the 30 months prior to registration were audited, but one or more of the 30 months after registration was audited. I consider a fund to have switched its auditor if the CompanyID associated with the auditor in the final month of the period prior to registration differs from the CompanyID associated with the auditor in the final month of the period following registration.¹⁶

Panel A of table 4 presents evidence that the newly registered funds were more likely than the control funds to switch auditors after both the Hedge Fund Rule and Dodd-Frank Act. Descriptive statistics show that 18% (17%) of the funds that were audited prior to registration switched auditors following the Hedge Fund Rule (Dodd-Frank Act). The vast majority of these funds switch to a Big4 auditor—all but two funds switched to a Big4 auditor after the Hedge Fund Rule, and all funds switched to a Big4 auditor after the Dodd-Frank Act. By contrast, not a single control fund switched its auditor in either setting. Because the name of the fund's auditor (if one exists) is in a separate data file within the Lipper Hedge Fund database, the first line in panel A presents the number of funds that exist in the separate file (and thus for which I have data for this analysis), the second presents the number of funds that were audited prior to the change in registration (and thus eligible to switch), and the third represents the number that actually switched.

Panel B presents evidence that, after the Dodd-Frank Act, the newly registered funds were also more likely than the control funds to hire an auditor. The table shows the number of funds that were not audited before registration (and thus eligible to initiate audits), the number that initiated audits, and the number that initiated audits but were required to do so because they had custody of client assets. As mentioned earlier, a registered fund that has custody of client assets is required to either produce audited financial statements or to have at least one surprise audit per year confirming the existence of client assets. Hence, hiring an auditor for this subset of funds should not be considered voluntary.

Using a regression model, panel C provides evidence consistent with the descriptive statistics in panels A and B. Columns 1 and 2 include only the subset of funds for which I can identify the auditor prior to registration and show that the newly registered funds were 11–13% more likely to switch auditors than the control funds following each event (statistical significance of 1% in both models). There is one observation per fund, and the dependent variable is set to 1 for the funds that switched auditors. Columns 3 and

¹⁶ As a practical matter, a change in the CompanyID associated with the auditor captures not only switches across audit firms (i.e., PwC to E&Y), but also switches within different segments of the same firm (i.e., E&Y Cayman Islands to E&Y U.S.). Further, the audit date (and auditor) fields are continuously updated to reflect the fund's most recent audit, so this analysis can only be performed with historical data. The field reflects the date of the audit—but does not indicate how many months were audited—so I presume that all audits reflect the prior 12 months (i.e., a date of 12/31/2004 covers 1/1/2004 through 12/31/2004).

TABLE 4
Auditor Changes upon Registration

Panel A:				
	Hedge Fund Rule		Dodd Frank Act	
Treatment Funds				
Num. Newly Registered	118		107	
Funds with Data				
Num. Already Audited	67	57%	46	43%
Num. Switched Auditors	12	18%	8	17%
Control Funds				
Num. Newly Registered	178		473	
Funds with Data				
Num. Already Audited	101	57%	247	52%
Num. Switched Auditors	0	0%	0	0%
Panel B:				
	Hedge Fund Rule		Dodd Frank Act	
Treatment Funds				
Num. Newly Registered	59		65	
Funds Not Audited				
Num. Initiated Audits	31	53%	24	37%
Num. Initiated Audits with Custody	13	42%	11	46%
Control Funds				
Num. Newly Registered	131		256	
Funds Not Audited				
Num. Initiated Audits	83	63%	70	27%
Num. Initiated Audits with Custody	9	11%	46	66%
Panel C:				
	Switch Auditor		Initiate Audit	
	HF Rule	DF Act	HF Rule	DF Act
	(1)	(2)	(3)	(4)
Newly Registered Fund	0.136*** (0.044)	0.118*** (0.038)	-0.032 (0.088)	0.134** (0.068)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Char.	Char.	Char.
Observations	168	293	190	321
Adj. <i>R</i> -squared	0.12	0.37	0.39	0.21

This table provides evidence on the number of newly registered funds that switched or hired auditors. Panel A provides descriptive statistics on auditor switches. Panel B provides descriptive statistics on audit initiations. Panel C presents OLS regressions comparing the behavior of the treatment funds to that of the control funds. Columns 1 and 2 analyze the funds that switched auditors. The dependent variable is set to 1 if the fund switched auditors, and the sample only includes those funds that were audited prior to registration (i.e., those funds eligible to switch their auditor). Columns 3 and 4 analyze the funds that hired auditors. The dependent variable is set to 1 if the fund hired an auditor, and the sample only includes those funds that were not audited prior to registration (i.e., those funds eligible to hire an auditor). All models control for the variables in table 2 (except for the number of months audited), and fixed effects are included for the fund's country of incorporation and investment style (fixed effects for fund characteristics, Char.). Standard errors (in parentheses) are clustered by fund. Statistical significance of 10%, 5%, and 1% is indicated by *, **, and ***, respectively.

4 include only the subset of funds that were not audited prior to registration and therefore eligible to hire an auditor. Column 4 shows that, after the Dodd-Frank Act, the newly registered funds were 13% more likely to hire an auditor (the results for the Hedge Fund Rule are not significant). The equation is the same as that used in columns 1 and 2, except that the dependent variable indicates whether the fund hired an auditor. Controls are included for all variables in table 3, except the number of audited months.

Table 5 provides evidence that the changes in audit behavior reduced misreporting. Columns 1–4 partition the newly registered funds into two groups: (1) those that switched auditors (*Switch*) and (2) those that did not (*No Switch*). These dummy variables are then interacted with the *Post* variable. Only funds that were audited before registration, and thus eligible to switch auditors, are included. Despite the limited number of funds that switched auditors, the results provide evidence that the funds that switched auditors experienced greater declines in misreporting than those that did not. In columns 1–4, the coefficient on *Post*Switch* is negative in all columns and statistically significant at standard levels in three of the four columns. By contrast, the coefficient on *Post*No Switch* is not statistically significant in any models. Controls are included for all variables in table 3, except the number of audited months.

Columns 5–8 examine misreporting at funds that hired an auditor. The newly registered funds are partitioned into three groups: (1) those that hired an auditor following registration (*Initiate Audit*), (2) those that were audited prior to registration (*Audit*), and (3) those that were not audited prior to registration and did not initiate an audit (*No Audit*). These dummy variables are then interacted with the *Post* variable. The results provide evidence that the decrease in misreporting following registration was driven by the funds that hired auditors. In columns 5–6, following the Hedge Fund Rule, only the subset of funds that initiated audits experienced a statistically significant decrease in misreporting in both models. In columns 7–8, following the Dodd-Frank Act, the group of funds that initiated audits experienced the greatest decrease in misreporting, although the funds that were not audited before registration and remained unaudited after registration also experienced a significant decrease in misreporting.

5.3 MANDATING DISCLOSURE

The prior tables provide evidence consistent with the theory that internal governance changes spurred by the disclosure regime induced funds to report more accurately. As a further test of this theory, table 6 examines the change in misreporting for the subset of funds that were only subject to mandatory disclosure rules—that is, the funds “exempt” from regulation.¹⁷ As stated previously, these funds are known as Exempt

¹⁷ State-registered exempt funds file the full Form ADV, and SEC-registered exempt funds file a portion of Form ADV.

TABLE 5
Auditor Changes and Misreporting

	Switch Auditor			Initiate Audit				
	HF Rule (1)	DF Act (2)	DF Act (3)	HF Rule (4)	DF Act (5)	DF Act (6)	DF Act (7)	DF Act (8)
Post	0.023 (0.070)	0.084 (0.219)	0.063 (0.050)	-1.601 (0.979)	0.115** (0.045)	0.294*** (0.109)	0.009 (0.032)	-0.507 (0.542)
Switch	0.180 (0.164)		-0.433*** (0.079)					
No Switch	0.019 (0.082)		0.061 (0.084)					
Initiate Audit					0.036 (0.093)		0.295*** (0.086)	
No Audit					0.159 (0.099)		0.193** (0.084)	
Audit					0.008 (0.093)		-0.082 (0.098)	
Post * Switch	-0.347** (0.169)	-0.328 (0.246)	-0.121** (0.060)	-0.198* (0.119)				
Post * No Switch	-0.103 (0.106)	-0.112 (0.147)	-0.145 (0.119)	0.006 (0.166)				

(Continued)

TABLE 5—Continued

	Switch Auditor				Initiate Audit			
	HF Rule		DF Act		HF Rule		DF Act	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post * Initiate Audit					-0.202** (0.097)	-0.299** (0.147)	-0.583*** (0.120)	-0.555*** (0.172)
Post * No Audit					-0.071 (0.156)	-0.110 (0.228)	-0.318*** (0.083)	-0.348*** (0.113)
Post * Audit					-0.195* (0.108)	-0.104 (0.156)	0.411 (0.281)	0.452 (0.395)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char.	Fund	Char.	Fund	Char.	Fund
Observations	336	336	586	586	722	722	1,362	1,362
Adjusted R-squared	0.06	0.00	0.10	0.13	0.01	0.07	0.10	0.09

This table provides evidence on the effect of switching or hiring auditors on misreporting. The dependent variable is the number of flags for misreporting. All models are run using OLS. Columns 1–4 examine the funds that switched auditors and include only funds that were already audited prior to registration (i.e., those eligible to switch auditors). *Switch* is a dummy variable set to 1 for all newly registered funds that switched auditors. *No Switch* is a dummy variable set to 1 for all newly registered funds that did not switch auditors. Columns 5–8 examine the funds that initiated audits. *Initiate Audit* is set to 1 for all newly registered funds that were not audited prior to registration but hired an auditor thereafter. *No Audit* is set to 1 for all newly registered funds that were not audited prior to registration and remained unaudited. *Audit* is set to 1 for all newly registered funds that were already audited prior to registration. All models control for the variables in table 2 (except for the number of months audited). Fixed effects are included either for the fund's country of incorporation and investment style (fixed effects for fund characteristics, Char.) or for the fund itself (Fund). Standard errors are clustered by fund. Statistical significance of 10%, 5%, and 1% is indicated by *, **, and ***, respectively.

TABLE 6
Disclosure-Only Funds

Panel A:						
Number of Flags for Misreporting						
	Full Sample			Matched Sample		<i>t</i> -Test
	Control	Full Reg.	Disc.-Only	Full Reg.	Disc.-Only	
Before Regulation	0.32	0.39	0.46	0.38	0.38	0.00
After Regulation	0.36	0.13	0.12	0.15	0.13	0.24
<i>t</i> -test (after vs. before)	-1.11	4.37***	5.77***	2.72***	3.08***	.
Diff. (after - before)	-0.03	0.26	0.35	0.23	0.25	-0.14

Panel B:						
Variable	Full Sample			Matched Sample		<i>t</i> -stat
	Full Reg.	Disc.-Only	<i>t</i> -stat	Full Reg.	Disc.-Only	
Monthly Return	1.67	1.01	4.24***	1.56	1.16	1.67*
Ln(Net Asset Value)	6.27	5.65	3.58***	6.03	6.05	-0.11
Age	2.76	1.68	2.76***	2.63	2.41	0.43
Return	3.53	2.38	3.40***	3.60	3.12	1.02
Volatility						
U.S. Incorporation	0.32	0.13	3.62***	0.21	0.21	0.00
Liquidity	-20.62	-9.15	-1.08	-31.84	-10.83	-1.33
Sensitivity						
Num. Audited Months	0.15	0.07	3.73***	0.13	0.07	2.48**
<i>Num. Funds</i>	112	112		61	61	

Panel C:						
	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample		Matched Sample		Quasi-Discontinuity Design	
Post	0.022	-0.741	-0.238**	0.090	-0.442***	2.587
	(0.033)	(0.495)	(0.102)	(1.711)	(0.163)	(4.131)
Full-Regulation	0.117**					
	(0.049)					
Disclosure-Only	0.159***		0.033		-0.357**	
	(0.059)		(0.103)		(0.134)	
Post * Full-Regulation	-0.298***	-0.305***				
	(0.066)	(0.093)				
Post * Disclosure-Only	-0.370***	-0.316***	0.063	0.101	0.076	0.119
	(0.072)	(0.104)	(0.129)	(0.180)	(0.193)	(0.278)

(Continued)

TABLE 6—Continued

Panel C:						
	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample		Matched Sample		Quasi-Discontinuity Design	
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char.	Fund	Fund Style	Fund
Observations	1,586	1,586	244	244	98	98
R-squared	0.10	0.05	0.11	0.04	0.30	0.30

This table shows the change in misreporting for the funds only subject to disclosure requirements. Panel A provides descriptive statistics on the frequency of flags for misreporting. The matched sample matches newly registered *Full-Regulation* and *Disclosure-Only* funds. *Full-Regulation* funds are those subject to SEC inspections, disclosure requirements, and compliance requirements (i.e., Registered Investment Advisers). *Disclosure-Only* funds are those only subject to disclosure requirements (i.e., Exempt Reporting Advisers). Panel B compares fund characteristics for the *Full-Regulation* and *Disclosure-Only* funds. All variables are as defined in table 2. Panel C presents OLS regressions in which the dependent variable is the number of misreporting flags. Columns 1 and 2 compare the change in misreporting at both *Full-Regulation* and *Disclosure-Only* funds relative to the control funds (the control funds are as defined in table 1). The variable *Full-Regulation* is set to 1 for all newly registered funds that became subject to full regulation. The variable *Disclosure-Only* is set to 1 for all newly registered funds that became subject to only disclosure rules. Columns 3 and 4 compare the change in misreporting at the *Disclosure-Only* funds relative to the change at the *Full-Regulation* funds using the matched sample presented in panel A. Columns 5 and 6 compare the *Disclosure-Only* funds to a sample of *Full-Regulation* funds that were close to the eligibility threshold for the disclosure-only regime (i.e., funds managed by advisers with assets under management from \$150 million to \$200 million). Because it is impossible to reliably determine whether foreign funds were close to the threshold, only U.S.-based funds are included. All models control for the variables in panel B. Fixed effects are included either for the fund’s country of incorporation and investment style (fixed effects for fund characteristics, Char.), for the fund itself (Fund), or for the fund’s investment style (Fund Style). Standard errors are clustered by fund. Statistical significance of 10%, 5%, and 1% is indicated by *, **, and ***, respectively.

Reporting Advisers and differ from the funds previously studied because they have to file Form ADV, but are exempt from compliance requirements and government inspections. Advisory firms are eligible for this status if they advise only venture capital funds or only private funds (i.e., hedge funds) with less than \$150 million of U.S. assets. Going forward, I refer to these funds as *Disclosure-Only* funds, and to Registered Investment Advisers (i.e., the funds previously studied) as *Full-Regulation* funds.

Panels A and B present descriptive statistics on the *Disclosure-Only* funds. Panel A shows that the pattern of misreporting at the *Disclosure-Only* funds is very similar to that at the *Full-Regulation* funds. Using the full sample of funds, the frequency of flags decreases from 0.46 to 0.12 (0.39–0.13) at the *Disclosure-Only* (*Full-Regulation* funds). Using the matched sample, which is created using the same procedures as the matched samples presented in table 1, the mean number of flags for the *Disclosure-Only* and *Full-Regulation* samples was the same prior to regulation and comparable after regulation. Panel B presents information on fund characteristics and shows that, on average, the *Disclosure-Only* funds have lower net asset values, lower returns, less return volatility, and are less likely to be audited than the *Full-Regulation* funds. However, the means mask the variability in the *Disclosure-Only* funds:

they have assets ranging from roughly \$64M to \$15B and average monthly returns from roughly -3.86 to 5.51 .

To study the effect of mandating disclosure, panel C of table 6 compares the change in misreporting at the *Full-Regulation* and *Disclosure-Only* funds. The time period, control funds, control variables, and model specifications are the same as those used for the Dodd-Frank Act tests in table 3. Columns 1 and 2 compare the *Disclosure-Only* and *Full-Regulation* funds to the control funds and show that the *Full-Regulation* and *Disclosure-Only* funds had significant declines in misreporting (unreported *F*-tests show that misreporting at the *Full-Regulation* and *Disclosure-Only* funds was not statistically significantly different prior to regulation and that the decreases in misreporting also did not differ significantly). Columns 3 and 4 use the matched sample of *Disclosure-Only* and *Full-Regulation* funds and, as before, show that the decrease in misreporting does not differ significantly across the groups.

Columns 5 and 6 provide further evidence that these groups experienced statistically equivalent declines in misreporting. The columns use a quasi-discontinuity analysis to compare those funds that were eligible for the *Disclosure-Only* regime with those that were almost eligible. Because advisers must manage \$100–\$150 million in U.S. assets to be eligible for *Disclosure-Only* status, I compare these funds to the *Full-Regulation* funds managed by advisers with \$150–\$200 million in assets. The advisers with just over and under \$150 million should be very similar—but only those with less than \$150 million were eligible for the *Disclosure-Only* regime.¹⁸ The results show that the decline in misreporting did not differ across this threshold, providing further confidence that the decrease in misreporting for these two groups was statistically equivalent.

The finding that *Disclosure-Only* funds decreased misreporting is perhaps surprising (e.g., Atkins [2006]). Hedge fund investors are often thought to be highly sophisticated, and Cassar et al. [2018] show that hedge funds disclose substantial financial information in private letters to their investors—far more than is available in Form ADV. Moreover, Brown et al. [2008] suggest that many (presumably most) investors had access to the information in Form ADV before mandatory registration. It is therefore not obvious that the disclosures in Form ADV would have an effect. However, the result is consistent with the prior suggestion that mandatory

¹⁸ Differences in the nature of this cutoff for foreign advisers make it impossible to reliably determine which foreign advisers were eligible for disclosure-only treatment, so I limit the sample to U.S. advisers with between \$100 million and \$200 million in assets. To be eligible for *Disclosure-Only* status, U.S. advisers must advise only venture capital funds or private funds (i.e., hedge funds) with less than \$150 million in total assets—a relatively small sum for an adviser. By contrast, foreign advisers are eligible for *Disclosure-Only* status if they advise funds with less than \$150 million in assets from U.S. investors. However, advisers only disclose total assets, not assets from U.S. investors. As such, for foreign advisers, the available data do not allow me to determine whether an adviser is close to the threshold. Figure S4 in the online appendix shows the countries of incorporation for the *Disclosure-Only* funds.

disclosure led to governance changes. Notably, the *Disclosure-Only* funds displayed similar patterns in auditor changes as the *Full-Regulation* funds: 42% (40%) of the eligible *Disclosure-Only* funds switched their auditor (hired an auditor) after registration.

5.4 FUNDS MOST LIKELY TO BE SCRUTINIZED

Prior literature suggests that comply-or-explain regulation is most effective for those most likely to be scrutinized, suggesting cross-sectional variation in the decline in misreporting based on fund characteristics. To determine whether a fund is more likely to be scrutinized, I consider substantive conflicts of interest and dubious disciplinary history in each fund's Form ADV filing. Prior studies have shown that these characteristics are potentially important determinants of fund performance (Brown et al. [2008]), and the SEC considers these factors in deciding whether to conduct a targeted examination of a particular fund (Abromovitz [2012]). I consider a fund more likely to be scrutinized if it has any of the five following disclosures: (1) principal transactions, (2) cross trades, (3) criminal history, (4) prior investment-related civil litigation, and/or (5) prior regulatory infractions (detail on these five characteristics is provided in exhibit S3 in the online appendix). I consider these five disclosures to be "red flags."

Table 7 provides evidence that the decrease in misreporting is greater for funds with one or more red flags than for those without. Panel A compares the frequency of funds with red flags across the treatment and control samples and shows that the control funds typically have more red flags. One explanation for this trend is that funds with questionable practices may bond to the regulatory regime (Coffee [2002]). Panel B compares the control variables for funds with and without red flags. The funds without red flags are older, have more return volatility, and are more likely to be audited. Panel C presents a regression analysis partitioning the newly registered funds into two groups: (1) *Red Flag*, those with at least one red flag, and (2) *No Flag*, those without a red flag. I interact each of these variables with the *Post* indicator. The decrease in misreporting is statistically significant for the funds with red flags in all four models.¹⁹ The results, which are consistent with prior work, suggest that comply-or-explain regulation is more effective for those more likely to be scrutinized.²⁰

¹⁹ *F*-tests show the decrease for funds with red flags was greater than for those without red flags following the Dodd-Frank Act.

²⁰ These cross-sectional analyses are particularly important because the timing of the regulatory events was not random. The Hedge Fund Rule was adopted in response to concerns raised by the collapse of the prominent hedge fund LTCM (note that the Hedge Fund Rule was adopted in 2004 and LTCM collapsed in 1998, so there was a delay between the two events), and the Dodd-Frank Act was adopted in response to the financial crisis. As such, a concern with testing misreporting before and after these events is that the market conditions driving the adoption of the regulation may have also driven funds to change their behavior (Hail, Tahoun, and Wang [2018]). The use of a control group that should have been similarly

TABLE 7
Funds with Red Flags

Panel A:						
Variable	Hedge Fund Rule			Dodd-Frank Act		
	Control	Treatment	t-stat	Control	Treatment	t-stat
Criminal Infraction	0.01	0.00	1.04	0.04	0.02	1.09
Civil Infraction	0.08	0.01	2.91**	0.18	0.02	4.46***
Regulatory Infraction	0.40	0.06	7.53***	0.32	0.25	1.46
Cross-Trades	0.07	0.02	2.18*	0.12	0.05	2.01*
Principal Transactions	0.16	0.03	3.73***	0.27	0.09	4.18***
Any Red Flag	0.49	0.10	7.81***	0.49	0.38	2.17*
<i>Num. Funds</i>	235	126		569	112	

Panel B:						
Control Variable	Hedge Fund Rule			Dodd-Frank Act		
	No Red Flag	Red Flag	t-stat	No Red Flag	Red Flag	t-stat
Monthly Return	0.89	0.74	1.95	1.27	1.34	-0.81
Ln (Net Asset Value)	6.10	6.18	-0.48	6.20	5.88	2.96**
Age	1.28	0.97	1.39	2.93	2.41	2.13*
Return Volatility	2.50	1.69	4.27***	3.16	2.61	3.13**
U.S. Incorporation	0.29	0.28	0.22	0.30	0.31	-0.33
Liquidity Sensitivity	85.76	86.43	-0.03	-21.43	-5.49	-2.70**
Num. Audited Months	0.03	0.01	2.39*	0.13	0.10	2.45*
<i>Num. Funds</i>	234	127		362	319	

Panel C:				
	Hedge Fund Rule		Dodd-Frank Act	
	(1)	(2)	(3)	(4)
Post	0.188*** (0.051)	0.308*** (0.108)	0.025 (0.033)	-0.432 (0.559)
No Flag	0.077 (0.057)		0.049 (0.064)	
Red Flag	0.597*** (0.134)		0.237*** (0.068)	
Post * No Flag	-0.208*** (0.073)	-0.179* (0.103)	-0.155* (0.081)	-0.158 (0.114)
Post * Red Flag	-0.531** (0.217)	-0.566* (0.294)	-0.500*** (0.086)	-0.486*** (0.120)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char.	Fund

(Continued)

TABLE 7—Continued

Panel C:	Hedge Fund Rule		Dodd-Frank Act	
	(1)	(2)	(3)	(4)
Observations	722	722	1,362	1,362
Adjusted <i>R</i> -squared	0.04	0.09	0.10	0.08
<i>p</i> -value from <i>F</i> -test:				
Post * No Flag v. Post * Red Flag	0.14	0.19	0.00	0.03

This table examines the change in misreporting for funds that have red flags, where all red flags are defined in Exhibit S3 of the online appendix. Panel A provides descriptive statistics on the frequency of red flags. Panel B compares characteristics of funds with and without red flags (all variables are as defined in table 2). Panel C presents regression results. All models are run using OLS. The dependent variable is the number of misreporting flags. *Red Flag* is set to 1 for all newly registered funds with one or more red flags. *No Flag* is set to 1 for all newly registered funds without any red flags. All models control for the variables noted in panel B. Fixed effects are included either for the fund’s country of incorporation and investment style (fixed effects for fund characteristics, Char.) or for the fund itself (Fund). Standard errors are clustered by fund. Statistical significance of 10%, 5%, and 1% is indicated by *, **, and ***, respectively.

5.5 FUND INFLOWS AND OUTFLOWS

Given the seeming benefits of registration, especially for firms that hired auditors, one might wonder whether fund flows from investors changed for these funds. Table 8 examines this question and shows that, following both the Dodd-Frank Act and Hedge Fund Rule, the funds that hired an auditor experienced significant increases in fund flows. This suggests that investors view hiring an auditor as a positive signal, which is consistent with prior literature examining firms for which audits are not mandatory (e.g., Minnis [2011]). As in table 5, the funds are partitioned by whether they hired an auditor following registration, were already audited, or remained unaudited following registration. The dependent variable is the change in net assets from the prior month after adjusting for fund returns (i.e., the inflow/outflow) scaled by the prior month’s net assets. This value is multiplied by 100 so that all coefficients can be interpreted as percentages. The models control for the standard controls in the fund flow literature: the fund’s monthly return, lagged six-month return, net asset value, age, management fee, incentive fee, and lockup period, as well as whether the fund is audited monthly, is subject to a high-water mark provision, is open to the public, and used leverage. All variables are winsorized at the 1st and 99th percentile, and the time periods are the same as those used in table 3.²¹

affected by market conditions helps to alleviate this concern, but cross-sectional tests provide further confidence that the primary results are due to the changes in law rather than general market conditions.

²¹ In panel A of table S9 of the online appendix, I examine the funds that switched auditors. After the Dodd-Frank Act, only funds that did not switch auditors experienced increases in fund flow. Although this is consistent with studies showing a negative market reaction to auditor turnover (e.g., Fried and Schiff [1981], Eichenseher, Hagigi, and Shield [1989]), the results for the Hedge Fund Rule are not significant, as neither funds that switched or did not

TABLE 8
Fund Inflows and Outflows

	Hedge Fund Rule		Dodd-Frank Act	
	(1)	(2)	(3)	(4)
Post	-0.136*** (0.034)	-0.169*** (0.039)	0.614*** (0.037)	0.626*** (0.046)
Initiate Audit	-0.311*** (0.097)		0.038 (0.132)	
No Audit	-0.117 (0.135)		-0.295** (0.147)	
Audit	-0.003 (0.134)		-0.088 (0.157)	
Post * Initiate Audit	0.192* (0.112)	0.228* (0.122)	0.427*** (0.143)	0.503*** (0.158)
Post * No Audit	-0.027 (0.179)	0.011 (0.191)	0.375*** (0.140)	0.452*** (0.153)
Post * Audit	-0.250 (0.165)	-0.255 (0.185)	0.112 (0.141)	0.166 (0.156)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char.	Fund
Observations	16,880	16,880	30,265	30,265
Adjusted <i>R</i> squared	0.44	0.44	0.48	0.48

This table shows the change in fund flows for the treatment funds relative to the control funds. The treatment funds are partitioned by whether they were already audited, hired an auditor, or remained unaudited following registration (all variables are as defined in table 5). The dependent variable is the change in net assets from the prior month after adjusting for fund returns (i.e., the fund inflow/outflow) scaled by the prior month's net assets. This value is multiplied by 100 for ease of interpretation. Controls are included for the fund's monthly return, lagged six-month return, net asset value, age, management fee, incentive fee, and lockup period, as well as whether the fund is audited monthly, is subject to a high-water mark provision, is open to the public, and used leverage. Although all funds are present in both periods, many funds have fewer observations in the period prior to registration because the six-month lagged return is not available in the early part of the sample period (this occurs when the fund did not report in all six months prior to the beginning of the sample period). Fixed effects are included either for the fund's country of incorporation and investment style (fixed effects for fund characteristics, Char.) or for the fund itself (Fund). Standard errors are clustered by fund. Statistical significance of 10%, 5%, and 1% is indicated by *, **, and ***, respectively.

6. Robustness

6.1 SURVIVORSHIP BIAS

For a fund to be included in my sample, I require 30 months of data both before and after each regulatory change. As such, the analysis omits the funds that do not survive at least 60 months—potentially a significant subset of the data. Survivorship bias in hedge fund data has been debated extensively; at the extremes, Barès, Gibson, and Gyger [2001] estimate that

switch experienced a significant change in fund flows. Finally, panel C of table S9 examines fund flows for all funds after each change in law. Most notably, the funds that withdrew after *Goldstein* experienced significant declines in fund flows. These results might explain why many of the *Withdraw* funds dissolved relatively quickly after withdrawing. Of the 55 *Withdraw* funds included in the analysis, only 10 remain in the Dodd-Frank event window (18%). For comparison, of the 102 *Remain* funds, 47 remain for the Dodd-Frank analysis (46%).

the median hedge fund survives for over 10 years, whereas Brown, Goetzmann, and Park [2001] estimate the same figure to be 2.5 years. To address the potential survivorship bias, I consider two approaches. First, table S10 in the online appendix shows the change in misreporting at the treatment and control funds using an unbalanced panel. Second, table S11 examines whether there is a change in the likelihood of fund “death” following registration.

The results provide evidence that the decline in misreporting is not limited to firms that survived at least 60 months. In table S10, the results are consistent with those presented in table 3.²² In table S11, there is evidence that newly registered funds are less likely to “die.” A fund that “dies” is one that no longer self-reports to the hedge fund databases, and prior work has shown that fund death is a very bad signal (Agarwal, Fos, and Jiang [2013]) and is highly correlated with misreporting (Feffer and Kundro [2003]). From an empirical perspective, the advantage of examining fund death is that it can be studied using monthly data—each fund dies in a particular month or it does not. Thus, unlike the analyses using the unbalanced panel, which still require 30 months of data to calculate the measures of misreporting, the analysis of fund death includes all potential treatment and control funds (including the *Disclosure-Only* funds).

6.2 INHERENT LIMITATIONS

6.1.1. Parallel Trends. Data limitations prevent the standard parallel trends analysis. Because of the lengthy period used to estimate misreporting, extending the window creates severe risk of survivorship bias—even one extra observation requires 30 months. To minimize attrition, I use a four-period model that extends 30 months before and after the current windows for the Hedge Fund Rule and Dodd-Frank Act.²³ The results using this model are presented in table S12 of the online appendix. Results are statistically weaker but broadly consistent with those reported in the paper.

6.1.2. Regulatory Avoidance. Funds that successfully evaded federal regulation will not show up in my sample. Prior work has found evidence that some firms evade federal regulation (e.g., Bushee and Leuz [2005], Leuz,

²² Table S10 includes the following additional funds: (1) Hedge Fund Rule: 395 total. 13 prior to registration (1 treatment, 12 control); 382 post registration (78 treatment, 304 control). (2) *Goldstein*: 599 total. 99 prior to registration (2 withdraw, 11 remain, 86 control); 500 post registration (27 withdraw, 80 remain, 393 control). Dodd-Frank Act: 899 total. 567 prior to registration (47 treatment, 520 control); 332 post registration (38 treatment, 294 control).

²³ For the Hedge Fund Rule, the four-period model begins with the three-period model in columns 5–6 of table 3, panel B and adds the 30-month period before the current start date. All models use the same funds as the current analysis, providing those funds are available over the entire period. However, many are not. For example, for the Dodd-Frank Act, there are 112 treatment funds in the original model but only 65 treatment funds in the four-period model. If I were to go back 60 months (rather than extending the sample 30 months in each direction), attrition would be more severe. The timeline is presented in figure S5 of the online appendix.

Triantis, and Wang [2008]), and evasion is a particular concern for hedge funds (FRB [1998]). I reviewed historical data to ascertain whether funds relocated around the time of the legal changes and found no evidence that funds engaged in systematic relocation to avoid regulation, but it is possible that funds opted out of the regulation using other means. For example, in certain circumstances, funds could evade these changes in law by altering the “lockup” period that investors must observe before withdrawing their funds. I note, however, that prior literature found that only 0.5% of domestic funds and 2% of offshore funds changed their lockup periods to evade the Hedge Fund Rule (Aragon, Liang, and Park [2014]).

6.1.3. Proxies for Misreporting. My analysis is based on proxies for misreporting, not incidences of detected misreporting. I analyzed proxies for misreporting for two reasons. First, even if the frequency of misreporting is constant, registration—and government inspections in particular—raises the probability that misreporting will be detected (CBS [2004]). Because the baseline level of detection has changed, comparing the change in enforcement actions before and after registration is problematic. Second, the frequency of detected fraud at hedge funds is very low, especially in the beginning of my sample period. In 2003, for example, the SEC brought a total of six enforcement actions against hedge funds.

7. Conclusion

This paper provides evidence that hedge fund regulation reduces misreporting. Ex ante, the reason for the decline in misreporting is unclear, as hedge fund regulation imposes multiple concurrent changes. In particular, funds become subject to mandatory disclosure, government inspections, and compliance requirements. My findings provide evidence that the mandatory disclosure requirements led funds to make changes in their internal governance, such as hiring or switching the funds’ auditor, and that these changes induced funds to report their financial performance more accurately. The results indicate that disclosure requirements, even those structured as “comply-or-explain” rules, can reduce hedge funds’ misreporting.

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