

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

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Comments welcome

Abstract:

This paper studies whether hedge fund regulation reduces misreporting, and, if so, why the regulation is effective. The setting is unique in that there were three changes in hedge fund regulation—mandatory regulation was imposed, withdrawn, and then imposed again—and because the regulation mandates the disclosure of governance information rather than financial performance. The results show that regulation reduces misreporting at hedge funds. Further analysis indicates that the disclosure rules 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. The results indicate that comply and explain regimes can reduce hedge funds’ misreporting.

Keywords: Mandatory disclosure, hedge funds, SEC regulation, financial misreporting, auditing

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1. Introduction

Unlike public companies, most hedge funds were not subject to mandatory regulation until recently. Further, even when hedge fund regulation was imposed, the regime put in place was relatively light. Rather than mandatory rules to which funds must adhere, a significant portion of the regulatory regime for hedge funds is structured as 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 the regulatory regime has been greatly debated (SEC Report, 2003). Many policymakers have argued that hedge fund regulation 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, such as pensions and universities,¹ that may suffer from a “double agency problem” (Karantininis and Nilsson, 2011). According to this theory, 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, the question of whether hedge fund regulation reduces misreporting—and, if so, why the regime is effective—requires a careful empirical examination. To this end, my empirical analysis exploits three significant changes in hedge fund regulation. First, in 2004, the SEC adopted a rule regulating the majority of hedge funds. Second, in 2006, the courts vacated the SEC’s rule, allowing the funds to withdraw from regulation. Third, in 2011, the SEC fulfilled the requirements of the Dodd-Frank Act and again adopted rules to regulate the majority of hedge funds.

From a research perspective, 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

¹ Individual investors only account for an estimated 3.6% of total capital (MFA, 2014).

created alternating changes in the regulatory regime. Second, the setting allows for a better understanding of why the regulation is effective. Upon regulation, funds are typically subject to a number of concurrent changes, including government inspections, compliance requirements, and mandatory disclosure. However, a striking feature of the Dodd-Frank Act was that it created a secondary classification of hedge funds known as Exempt Reporting Advisers. Unlike the majority of newly regulated funds, the funds that became regulated 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 became subject to federal regulation, and increased at the funds that withdrew from federal regulation after the courts vacated the SEC's rule. Second, I provide evidence that regulation reduced misreporting by spurring funds to make changes to their internal governance. For example, after the newly regulated funds had to publicly disclose whether they were audited and the name of any such auditor, they were more likely to hire an auditor and/or switch auditors. The funds that made such changes experienced greater declines in misreporting. Finally, by examining the funds only subject to disclosure rules, I show that requiring funds to publicly disclose information about their governance, even without other concurrent regulatory changes, can significantly decrease misreporting. This is consistent with the prior evidence that funds became more likely to adhere to best practices after being required to publicly disclose whether they followed best practices.

All tests are difference-in-differences regressions that compare the hedge funds affected by the regulatory changes to a control group of funds that were already regulated. To address sample selection concerns, I include a series of robustness tests (e.g., placebo tests, matched sample analyses, etc.). In accordance with prior literature, I identify misreporting at hedge funds using two suspicious patterns in the monthly performance returns that hedge funds report to commercial databases. First, I use the size of a

fund's "kink" at zero—that is, the unexpected number of small gains relative to the number of small losses—which is the best-known predictor of detected fraud at hedge funds (Bollen and Pool, 2012). Second, following Agarwal et al. (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 US as regulators express concern over one-size-fits-all governance regulation.² 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 et al. (2009), Akkermans et al. (2007) and Dharmapala and Khanna (2016), find high rates of compliance, whereas other studies have found comply or explain ineffective (MacNeil and Li, 2006; Bianchi et al., 2011; Keay, 2014; Seidl et al., 2013; Hooghiemstra and van Ees, 2011).

My study contributes to this limited, but growing, literature in two key ways. First, my results may help to explain some of the inconsistent findings on comply and 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 and explain is also more effective in some firms and instances than in others. For example, Akkermans et al. (2007) finds that compliance is positively associated with firm size and suggests that the firms most likely to be scrutinized are those that comply. My results support this explanation; the funds seemingly most likely to incur scrutiny by the SEC and investors—i.e., those that disclose potentially worrisome information such as significant conflicts of interest or past disciplinary infractions—are also the funds to experience the greatest declines in misreporting. My finding that comply and explain is, on average,

² For example, although the Sarbanes-Oxley Act (SOX) does not mandate that a "financial expert" sit on a company's audit committee, it does require such a disclosure—and this disclosure requirement has led to a doubling of the number of those experts on audit committees (Linck et al., 2009). As another example, Coates and Srinivasan (2014) note that Section 404 of SOX is effectively a comply or explain regime because it allows for internal control systems to contain weaknesses, provided that such weaknesses are disclosed. The Dodd-Frank Act also has many features typical of comply or explain regimes. For example, Sec. 972 requires that companies disclose whether the same individual serves as both the CEO and chairman of the board.

effective for hedge funds is also notable as prior literature has suggested that the effectiveness of comply and explain regimes is dependent on the ability of investors to enforce them (Bianchi et al., 2011). Because hedge fund investors are thought to be highly 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 et al., 2013; 2016).

Second, I contribute to the comply and explain literature by focusing on the real effects of the disclosure requirements on compliance. I contacted personnel at the funds in my sample to discuss their experiences with the regulatory process, and they indicated that the disclosure requirements spurred them to make internal governance changes. Based on these anecdotes, I ran empirical tests and found evidence consistent with the feedback I received. In particular, I found that the newly regulated funds were more likely to hire auditors and/or to switch auditors, and that the funds that made such audit-oriented changes had greater decreases in misreporting. The evidence that disclosure rules led to real changes in governance provides a missing link in this literature. With the exception of Linck et al. (2009), which looks at real changes in board structure after Sarbanes-Oxley, this literature has focused on market measures such as Tobin's Q and does not explain why, for example, firm performance improves. By providing evidence that comply and explain leads to changes in auditing, which in turn leads to a reduction in misreporting, I provide (to my knowledge) the first evidence of this key channel. 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;).

Finally, I contribute to the hedge fund literature by providing the first evidence of the effectiveness of disclosure to regulate hedge funds. A limited 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-sectional studies using data from several countries (the one exception is Dimmock and Gerken, 2015). 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 are highly sophisticated and already have access to substantial amounts of information (Cassar et al., 2018; Brown et al., 2008; Atkins, 2006). By showing that disclosure rules can induce compliance changes that reduce misreporting, my study addresses this longstanding debate and extends the prior literature in this area.

My paper is organized as follows. The next section reviews the institutional background of hedge fund regulation. Section 3 describes the data sources and research design. Section 4 describes the proxies for misreporting. Section 5 presents the empirical analysis. Section 6 presents the robustness tests, and Section 7 concludes the paper.

2. Institutional Background

As a technical matter, when I say that a fund becomes “regulated,” it means that the fund’s advisor must register with the proper governmental authority—an act that subjects the fund to regulatory requirements.³ A fund that is not registered is still subject to antifraud rules and does not have a free pass to commit fraud, but government enforcement of non-registered funds has been notoriously weak.⁴ For example, as I discuss below, the SEC received eight complaints regarding Madoff (all but one occurred while Madoff was unregistered). Yet, the SEC failed to detect the Ponzi scheme. In this section, I begin with a brief description of the three major components of hedge fund regulation: disclosure rules, compliance requirements, and government examinations. Next, I provide an overview of the legal regime governing hedge fund regulation.

³ Although regulation is applied at the investment advisor level, I use the term funds for ease of exposition. Legally, the fund and investment advisor are separate entities. The fund holds the assets, and the investment advisor manages those assets. A single investment advisor may provide investment advice for multiple funds.

⁴ Antifraud rules allow unregulated funds to be inspected—and regulators to bring enforcement actions—if there is reason to believe the fund is committing fraud. However, as illustrated by Madoff, the antifraud rules may not be enforced even if the fund is examined.

A. Components of Hedge Fund Regulation

1. Compliance requirements. Regulated funds are generally subject to a multitude of compliance requirements. The most notable requirements are that the advisor 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. Regulated advisors who have control of their clients' assets are also generally required either to produce audited financials or to have at least one surprise audit each year.⁵

2. Mandatory disclosure. Regulated funds are required to disclose extensive information to the public in a filing known as Form ADV. 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. Notably, 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 must only disclose those conflicts. Form ADV gives investors potentially important information about their advisors. For example, 21% of the advisors in the full dataset 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.⁶

3. Government inspections. Finally, upon regulation, advisors are generally subject to compliance examinations, which involve inspections of the fund and its managers by government officials. These inspections can range from simple records requests to onsite exams lasting for several weeks. The exams

⁵ US advisors that are not regulated by the SEC may sometimes be required to produce audited financials based on an investor request or state law, but they are not required to do so under federal law

⁶ Form ADV is the only mandatory public filing for most hedge funds, but some funds may be required to file two other forms. First, following the Dodd-Frank Act, regulated advisors with over \$150 million in US assets under management are required to disclose portfolio information on Form PF. This form is not publicly available and is exempt from Freedom of Information Act ("FOIA") requests. Second, advisors with over \$100 million in applicable equity securities are required to disclose equity holdings on Form 13F (this applies even if the advisor is not regulated). However, many advisors are small enough to evade this requirement.

generally focus on whether the advisor has fulfilled the compliance requirements described above. Following the exams, most advisors receive a deficiency letter and are given the opportunity to address the issues that the SEC has uncovered (Abromovitz, 2012). Examinations can reveal unlawful acts that lead to enforcement actions (CBS, 2004).

Although some suggest that SEC enforcement in the form of inspections reduces fraud (HF Rule, 2004), the inspection program has been subject to considerable criticism. First, a limited number of advisors are examined each year. From 2004 to 2015, the percentage examined each year was low and declined over the period (from 18% to 11%). For comparison, roughly 50% of broker-dealers are inspected by FINRA together with the SEC each year (SEC, 2016). Second, there are significant questions as to whether the exams are effective. This is best illustrated by Madoff. In total, the SEC received eight separate complaints regarding Madoff from six different sources prior to his eventual confession in December 2008. The earliest complaint was made in June 1992. In response to these complaints, the SEC initiated two investigations and conducted three examinations, but found no evidence of a Ponzi scheme.⁷

This example illustrates concerns with both SEC inspections and with enforcement of antifraud rules. Antifraud rules exist, but they are not necessarily enforced. Examinations occur, but they are not necessarily effective. This seeming lack of enforcement could impede the effectiveness of the disclosure (Christensen et al., 2013; 2016). However, even if some inspections, such as those involving Madoff, are ineffective, others lead to enforcement actions (CBS, 2004). Further, Form ADV is signed under penalty of perjury, meaning that lying is a serious offense that can result in penalties such as large fines, disbarment, or even prison. Presumably, this threat can be effective even with relatively limited SEC enforcement.

B. Hedge Fund Regulation

⁷ Both investigations suffered similar hindrances: the teams were relatively inexperienced, there was insufficient planning, and the exams were primarily focused on compliance issues rather than fraud prevention. For example, Bernard Madoff Investment Securities, LLC (BMIS) was not initially registered with the SEC (the SEC conducted the exams under its antifraud powers), causing a significant portion of the exam to concern whether BMIS should register rather than whether Madoff was defrauding investors (SEC, 2009).

Hedge funds have recently been subject to three important regulatory changes.⁸ First, in 2004, the SEC adopted a controversial rule that regulated the majority of unregulated hedge funds. Second, in 2006, the courts vacated the SEC’s rule mandating regulation, thus allowing the newly regulated funds to withdraw from SEC regulation. Third, in 2011, the SEC again adopted a rule mandating regulation for the majority of unregulated hedge funds (this rule was adopted in accordance with the Dodd-Frank Act). In this section, I describe each of these events in detail.

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. At the time, it was estimated that fewer than half of hedge funds were regulated by the SEC (CBS, 2004).

After years of debate, the SEC officially proposed to subject the vast majority of unregulated hedge funds to regulation in mid-2004 (HF Rule, 2004). The rule, nicknamed the Hedge Fund Rule, closed a commonly used exemption that many hedge funds relied upon to avoid regulation under the Investment Advisers Act.⁹ The rule was highly controversial and faced significant public opposition, causing many to question whether it would be adopted (CBS, 2004). Nonetheless, despite the objections of many in the

⁸ 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).

⁹ At the time this rule was proposed, Section 203(b)(3) of the Investment Advisers Act exempted advisors that did not publicly hold themselves out as investment advisors, did not advise a registered investment company, and had fewer than 15 “clients” over the past twelve months. Under this exemption, “client” was defined to include only direct investors, allowing funds to avoid regulation by using a legal structure in which investors placed their money in sub-funds that invested in the parent fund rather than investing in the parent fund directly. The Hedge Fund Rule redefined “client” to include all investors rather than only direct investors, thus largely eliminating this commonly used exemption. Although some exemptions to SEC regulation remained (e.g., advisors with less than \$25M were generally left to state agencies rather than the SEC and funds with two-year lockups were also generally exempted), the new rule had widespread effects because unregulated funds generally relied on the “client” exemption.

hedge fund community and dissenting votes by two of the five SEC commissioners, the Commission adopted the rule in December 2004.

2. *Goldstein v. SEC.* In response to the Hedge Fund Rule, the newly regulated hedge funds sued the SEC. In a closely watched lawsuit, Phillip Goldstein of Bulldog Investors alleged that the SEC had overstepped its authority. In June 2006, the DC Circuit agreed with Goldstein and vacated the Hedge Fund Rule in an unexpected decision.¹⁰ Media coverage following the event described Goldstein’s “surprising victory” and the “surprise ruling,” and many lawyers criticized the decision as contrary to past precedent.¹¹ Even so, in August 2006, the SEC stated that it would not appeal the DC Circuit’s decision and allowed the newly regulated funds to withdraw without penalty through January 2007 (Cox, 2006).

3. *The Dodd-Frank Act.* Congress responded in the Dodd-Frank Act. In a section specifically devoted to fund regulation,¹² the Dodd-Frank Act made two very important changes. First, the Dodd-Frank Act mandated that the vast majority of hedge funds again be subject to regulation (DF Rule, 2011). Second, the Dodd-Frank Act created an entirely new category of advisory firms called Exempt Reporting Advisers.

¹⁰ Although recent years have seen a number of SEC rules overturned by the DC Circuit, the Hedge Fund Rule in *Goldstein* was only the fourth SEC rule overturned by the DC Circuit. 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) (both Chamber cases addressed the same rule).

¹¹ The following quotes reflect coverage of the event: (1) Somers (2006) in *Law360* (“The ruling marked a surprising victory for Goldstein”); (2) Staff Article, *Forbes* (2006) (“The controversial rule ... was tossed out in June in a surprise ruling”); (3) Staff Opinion, *Harvard Law Review* (2007) (“Under longstanding administrative law doctrine, this change should have survived legal challenge”); and (4) Mann (2008) in *St. John’s Law Review* (“More surprising than the filing of the suit itself was that Mr. Goldstein and his investment firm came out victorious”).

¹² Although the vast majority of provisions in Dodd-Frank did not affect hedge funds, two other provisions bear mention. First, Sec. 929P gave the SEC authority to impose administrative fines on all persons associated with securities transactions (previously, the SEC could only undertake such actions if the alleged wrongdoer was associated with an SEC-regulated enterprise). Second, the Dodd-Frank Act increased whistleblower incentives and protections. Most notably, Sec. 922 provides whistleblowers 10 to 30 percent of any monetary recovery of over \$1 million that the SEC obtains from an offending party through enforcement actions (previously, the SEC was only allowed to reward whistleblowers in insider trading cases). Although both of these provisions could have affected hedge funds, I do not think either is driving my results. First, the change in administrative proceedings went into effect immediately after President Obama signed the Dodd-Frank Act on July 21, 2010. Because the SEC did not adopt the rules to regulate hedge funds until almost one year later, the timing of the two events differs. Second, the provisions affected all hedge funds, meaning that I would not expect these provisions to have a disproportionate effect on the treatment funds relative to the control funds. Moreover, the Dodd-Frank Act includes multiple whistleblower provisions, and these provisions went into effect on a range of days from July 21, 2010 through August 2011. As such, the timing of many of these provisions also differs from the timing of the specific hedge fund rules I study here.

Following the Dodd-Frank Act, fund advisors could therefore register as either of the two types of advisory firms listed below.

- Type 1: “Registered Investment Adviser.” This fund type already existed before the Dodd-Frank Act, and it is the more common of the two options. Registered Investment Advisers are subject to the mandatory disclosure rules, government inspections, and compliance requirements discussed previously. Going forward, for concision, I refer to all funds regulated as Registered Investment Advisers as “Full-Regulation” funds.
- Type 2: “Exempt Reporting Adviser.” This was the fund type created by the Dodd-Frank Act. Exempt Reporting Advisers differ from Full-Regulation funds because they are only subject to disclosure rules (i.e., they have to file Form ADV but are exempt from compliance requirements and inspections).¹³ 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 US assets. Going forward, I refer to Exempt Reporting Advisers as “Disclosure-Only” funds.

3. Methodology

A. Data

To evaluate how regulation affected misreporting at hedge funds, I assembled a dataset 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.

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.

¹³ State registered ERAs file the full Form ADV and SEC registered ERAs file a portion of Form ADV. ERAs are not required to file Form PF. Congress exempted these funds from the vast majority of compliance requirements, and the SEC exempted these funds from its examination program.

And Brown et al. (2008, 2009, 2012) provide evidence that information in Form ADV filings predicts future performance.

Despite its usefulness, Form ADV has received relatively little attention in academic literature—likely because the SEC historically denied all FOIA requests for the information, making Form ADV data generally unavailable to academic researchers.¹⁴ Moreover, because some advisors are regulated by states and others by the SEC, FOIA requests must be filed separately at each regulator. To obtain the data for this project, I filed FOIA requests with the SEC and sixteen state securities agencies.¹⁵ My original request with the SEC was denied, but I later obtained the data after a lengthy appeals process.

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 et al., 2013). The Lipper Hedge Fund database is recorded at the fund level, whereas Form ADV is filed by the investment advisor. To combine these databases, I performed a one-to-many merge.

B. Research Design

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. This section describes the timeline of the events and the empirical identification of the treatment and control funds.

1. Event windows. As shown in Figure 1A, I use a “two-period” event window that compares misreporting in the thirty months before and after each change in law. The dates are described below.

¹⁴ As a result of my endeavors to obtain Form ADV for this project, the SEC has now begun to make historical data available through FOIA requests. To my knowledge, the only prior studies that use time-series Form ADV data use the dataset described by Dimmock and Gerken (2012), who note that their Form ADV data were not publicly available.

¹⁵ I filed FOIA requests for historical Form ADV filings at sixteen state securities agencies (CA, CO, CT, FL, GA, IL, MA, MD, MN, NJ, NY, OH, PA, TX, VA, and WA). I selected the states by tabulating the percentage of SEC registrants located in each state and submitting FOIA requests in all states with 2% or more of total advisors. I thank Robert Jackson for his very extensive help with the process.

- For the Hedge Fund Rule, the event window extends from June 2002 to May 2007. This is because the SEC adopted the Hedge Fund Rule in December 2004, so I include the 60 months surrounding this event date.
- For the *Goldstein* decision, the event window extends from March 2004 to March 2009. This is because the first funds to withdraw from SEC regulation 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.¹⁶
- For the Dodd-Frank Act, the event window extends from January 2009 to December 2013. This is because the SEC adopted the rules to implement the hedge fund requirements of the Dodd-Frank Act in June 2011, so I include the 60 months surrounding this event date.

In all instances, I use the month of SEC rule adoption as the event date. Although funds were not required to register immediately after the SEC adopted the final rules to implement regulation, I select the month of rule adoption as the event date because I think it is the most accurate measure of when funds began the registration process.¹⁷ The registration process is cumbersome, and the SEC gave funds time to comply. In particular, funds were required to submit to SEC oversight by January 31st, 2006 for the Hedge Fund Rule and by March 31st, 2012 for the Dodd-Frank Act.

One concern with the timeline is that there is overlap across the different events: the Post period for the Hedge Fund Rule ends in May 2007, but funds began to withdraw after *Goldstein* in September 2006. This overlap occurs because all windows in the two-period model are 30 months. This lengthy event window is preferred so that I have sufficient observations to detect a pattern of misreporting—as discussed later, one measure of misreporting relies on regression results. However, to address concerns created by

¹⁶ It seems likely that these funds filed the paperwork to withdraw immediately, but that their withdrawals were not official until September because the SEC had to process the request.

¹⁷ My conversations with numerous 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 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.

this overlap, I add a separate “four-period” model that uses only the 22 months in the period from when the SEC adopted the Hedge Fund Rule until the funds began to withdraw following *Goldstein*. The event windows for the four-period window are shown in Figure 1B and described below (as suggested by the name, the four-period model examines misreporting over four periods).

- For the Hedge Fund Rule, the event window extends from December 1999 to March 2009. The four periods are as follows: December 1999 to May 2002 (“Lagged” period), June 2002 to November 2004 (“Before Regulation” period), December 2004 to September 2006 (“Mandatory Regulation” period), and October 2006 to March 2009 (“Voluntary Regulation” period). All periods are thirty months except for the Mandatory Regulation period, which is 22 months. Due to the timing, the Hedge Fund Rule and *Goldstein* events are in the same four-period model.
- For the Dodd-Frank Act, the event window extends from July 2006 to June 2016. The four periods are as follows: July 2006 to December 2008 (“Lagged” period), January 2009 to June 2011 (“Before Regulation” period), July 2011 to December 2013 (“Mandatory Regulation” period), and January 2014 to June 2016 (“Post Mandatory Regulation” period).¹⁸ All periods are thirty months.

Although I present both options, I view the two-period model as superior to the four-period model for two reasons. First, because I use a balanced panel, the four-period model has a greater risk of survivorship bias as it requires a longer period of data. For example, there are 112 newly regulated funds in the two-period model for the Dodd-Frank Act, but only 65 of those funds are available for the four-period model. Second, prior literature uses a minimum of 24 months to detect misreporting by hedge funds (e.g., Bollen and Pool, 2012), and the four-period model uses fewer in one period.¹⁹ However, a benefit to the

¹⁸ Note that there is minor overlap between the periods: the first three months of the Lagged period are periods of mandatory regulation, whereas the rest of the period is voluntary (post-*Goldstein*).

¹⁹ If anything, I expect that the overlap in the two-period model (e.g., the voluntary months in the mandatory period for *Goldstein*) will add noise. By comparison, the use of only 22 months of data may affect the reliability of the measures of misreporting, and I do not know whether this will bias in favor or against finding a result.

four-period is that it allows me to better examine time-trends—an inquiry that cannot be conducted with the two-period model. As such, I focus on the two-period model, but I present the four-period model for robustness.

2. Control and treatment samples. Here I describe how I identify the control and treatment samples (a summary is provided in Exhibit 1A of the Online Appendix). In short, the control funds are those that were already regulated by the SEC before the change in law, and the treatment funds are those that had a change in regulatory status due to the change in law.

a. Control funds. Empirically, I identify the control funds by using the initial registration date included in Form ADV. For example, a fund that first submitted to regulation in March 2001 and remained regulated 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 regulation in April 2005 and remained regulated 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.

One might wonder why any funds were regulated by the SEC prior to the adoption of the Hedge Fund Rule in 2004. There are a number of possible reasons. First, some funds chose to register to achieve Qualified Professional Asset Manager status under the Employee Retirement Income Security Act of 1974 (ERISA). Achieving this status allows fund managers to engage in transactions that would otherwise be prohibited under ERISA. Second, some funds registered because of perceived marketing benefits. Third, some clients demanded their advisors register. Fourth, advisors to companies registered under the Investment Company Act were required to register (i.e., a hedge fund advisor that also advised a mutual fund registered under the Investment Company Act would have to register).²⁰

²⁰ As an estimate for the number of funds that registered to achieve ERISA benefits, 64% and 83% of the control funds in the Hedge Fund Rule and Dodd-Frank Act settings managed money from pension funds. For comparison, fewer than 10% of the newly regulated funds in both settings managed money from pension funds. Additionally, 53% and 29% of the control funds in the Hedge Fund Rule and Dodd-Frank Act settings managed money from an investment company (however, these figures overstate the percentage of funds required to register because of an affiliation with an investment company because they include all investment companies, not just *registered* investment companies).

b. 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 submitted to federal oversight in the six months prior to the deadline imposed by the relevant law.²¹ I take this approach because the registration process, like the IPO process, is time-consuming. For example, a fund that registered immediately after the Hedge Fund Rule was adopted is likely a voluntary registrant that began the registration process before the rule was adopted. The specific details are provided below.

- For the Hedge Fund Rule, the treatment group includes all funds that submitted to SEC regulation for the first time from August 2005 through January 2006.
- For the *Goldstein* analysis, I split the treatment group into two groups of interest: (1) those funds that chose to remain subject federal oversight (Remain), and (2) those funds that chose to exit the federal regulatory regime (Withdraw). The Remain funds are those that became regulated by the Hedge Fund Rule and remained regulated through March 2009 (the end of the event window), and the Withdraw funds are those that became regulated by Hedge Fund Rule and withdrew at any point from September 2006 (event date) through January 2007 (deadline to withdraw without penalty).
- For the Dodd-Frank Act, the treatment group includes all funds that submitted to SEC regulation for the first time from October 2011 through March 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 regulated through the end of 2013. Alpha would be included as a treatment fund for the Hedge Fund Rule, a Remain fund for the *Goldstein* analysis, and a control fund for the Dodd-

²¹ As a practical matter, this six-month cutoff makes little difference in the results because few funds registered immediately after the rules were adopted.

Frank Act. As another example, imagine that Beta fund first registered with the SEC in August 2005, withdrew from regulation in October 2006, and never registered again (presumably Beta died). Beta would be included as a treatment fund for the Hedge Fund Rule, a Withdraw fund for the *Goldstein* analysis, and would not be included in the Dodd-Frank Act analysis.²²

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 regulated throughout the entire pre-period, and it would not be a treatment fund because it did not become regulated due to the Hedge Fund Rule. Finally, because the four-period event window is meant to be a robustness test of the two-period event window, I use the same firms (when available throughout the 120-month period) and classify them as they are classified in the two-period model.

4. Measures of Misreporting

The variable of interest for my study, misreporting by hedge funds, is notoriously difficult to measure. First, 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. 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., Brown et al. 2012; Cassar and Gerakos, 2010, 2011).

²² 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 regulation (i.e., that the fund did not disappear from my sample due to a data error) by hand-checking the termination date for each fund using the Investment Adviser Public Disclosure website.

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). Second, it is difficult to identify misreporting because the funds’ portfolio data are not publicly available.

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 2A of the Online Appendix.

A. 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 (Bollen and Pool, 2009; 2012; Burgstahler and Dichev, 1997). 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 et al., 2011). This measure was made famous by Madoff, who violated it widely. Out of 16 years (215 months), there were only 16 months with negative returns (i.e., “down” months)—making for 92.56% winning months (Bernard and Boyle, 2009). Perhaps ironically, 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).

Prior empirical work has validated this measure and found that the size of a fund’s kink at zero is the strongest predictor of detected accounting fraud at hedge funds (Bollen and Pool, 2012). Figure 2 provides the distribution of monthly returns for all funds in the Lipper Hedge Fund database from 2000-2013 and shows the kink in the distribution of fund returns at zero. The bin width of 13 basis points in Figure 2 is set according to the optimal bin width formula described in Silverman (1986).

Although Figure 2 presents evidence using all hedge funds, my research design requires that I identify misreporting at each individual fund. To test for a kink at each individual fund, I create three bins surrounding zero. The first bin includes monthly returns from -1% to -.50%, the second from -.50% to 0%, and the third from 0% to .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), 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 significance level of 5% or greater.

B. Cookie Jar Accounting

My second measure of misreporting is based on whether a fund uses so-called “cookie jar” accounting—that is, whether the fund accumulates reserves during good times in order to protect against bad times. In bad times, this practice allows the manager to reach into the “cookie jar” to inflate her reported results. This behavior 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).

Prior literature has suggested that one way to test for cookie jar accounting is to look for evidence of abnormally high returns in December (Agarwal et al., 2011). December returns could be inflated due to cookie jar accounting for two reasons. 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.

Following Agarwal et al. (2011), Figure 3 shows the average returns for all hedge funds, both in the month of December and in non-December months, in all years from 2000-2013. 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. Returning to the Madoff example, although his returns did not trigger this flag, it is interesting to note that December was one of only two months (the other being September) in which Madoff did not record a single negative return over 18 years (Bernard and Boyle, 2009).

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 style factors used by Fung and Hsieh (2004), an indicator for the month of December, and year fixed effects. The seven hedge fund 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 at the 5% level or greater.

5. Empirical Results

A. Descriptive Statistics

Table 1 describes the characteristics for the treatment and control funds used in the two-period event windows.²³ There were more control funds than treatment funds in all analyses. The specific numbers are as follows: (1) Hedge Fund Rule – 235 control funds and 126 treatment funds; (2) DC Circuit's *Goldstein* decision – 289 control funds, 55 funds that withdrew from SEC regulation after the court's decision, and 102 funds that voluntarily remained subject to SEC regulation; (3) Dodd-Frank Act – 569 control funds and 112 treatment funds.²⁴

The table shows each fund's mean monthly return, mean natural log of net asset value, and mean age over the thirty months prior to the event date. I also include the fund's return volatility over the period, whether the fund is incorporated in the US, the sensitivity of the fund to market liquidity, and the average

²³ The corresponding sample sizes for the four-period period are as follows: (1) Hedge Fund Rule/*Goldstein* – 165 control funds and 96 treatment funds (38 funds that withdrew and 58 that remained), and (2) Dodd-Frank Act – 384 control funds and 65 treatment funds. As is standard in the literature, I do not include the backfilled returns in the two-period model. To increase the sample size in the four-period model, however, I include backfilled data.

²⁴ To be conservative, Tables 1-6 only include the Full-Regulation funds—i.e., not Disclosure-Only funds—so that the results will be comparable across the three changes in law, and the Dodd-Frank Act analyses omit all funds that withdrew from SEC regulation after *Goldstein* to ensure that the results from the Hedge Fund Rule and Dodd-Frank Act are comparable (however, the inclusion of these funds does not materially change the findings).

number of months that are audited each year. The fund’s 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. As shown in Table 1, there are significant differences between the treatment and control groups across several observable characteristics. Relative to the control sample, the treatment funds are significantly younger, have better performance, and have greater return volatility. It is for this reason that, when testing the change in misreporting following regulation, I use two control groups: (i) the full sample of eligible control funds, and (ii) a smaller matched sample of control funds.

To create the matched sample, I rely primarily 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 regulation. Second, US funds must be matched to US funds (and non-US funds to non-US 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 unregulated, where the propensity to be unregulated is determined using a probit model. Each probit model (untabulated) includes monthly returns, performance, age, return volatility, sensitivity to liquidity, and audit history (as defined in Table 1). Following these criteria, there are a total of 96 funds in each group in the analysis of the Hedge Fund Rule, 52 funds in each group in the analysis the *Goldstein* opinion,²⁵ and 71 funds in each group in the analysis of the Dodd-Frank Act.

Table 2 provides descriptive evidence on the frequency of misreporting at the control and treatment samples. As explained previously, I use two measures as proxies for misreporting. If a fund triggers either of these measures, I consider it a “flag” for misreporting.²⁶ Panel A presents the average number of flags

²⁵ As noted previously, there are three groups in the *Goldstein* analysis: (1) funds that withdrew from regulation, (2) funds that remained regulated, and (3) control funds. For the matched sample, I matched the funds that withdrew from regulation with the control funds, therefore omitting the funds that remained regulated.

²⁶ 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

per fund for the full sample, and Panel B breaks down the results for the full sample by proxy. Panels C and D present the same descriptive statistics for the matched sample.

As a general pattern, the frequency of misreporting at the treatment group decreased relative to the control group after regulation. For example, 16% of funds that became regulated in response to the Dodd-Frank Act had a statistically significant kink prior to regulation, whereas only 8% of the control funds had such a kink. In the period following regulation, the percentage of funds with kinks did not significantly differ between the two groups (10% and 8% for the control and treatment funds, respectively).

B. The Effect of Regulation on Misreporting

Below I present the results of the difference-in-differences multivariate tests. Across all tests, the dependent variable is the number of flags triggered. As with the descriptive statistics, there is one observation per fund in each period.²⁷ All tests control for the variables noted in Table 1. Standard errors are clustered by fund, but Table 1A in the Online Appendix compares the coefficients of interest if I were to use bootstrapped standard errors, cluster by fund investment style, or cluster by the fund's country of origin (on the whole, the results remain consistent). Similarly, although all models are run using OLS, Table 2A compares the coefficients of interest if I were to use ordinal logit instead (the results again remain consistent). All reported p-values reflect two-sided tests.

1. Two-Period Window. Table 3 tests whether hedge fund regulation reduced misreporting using the two-period event window. Panel A studies the Hedge Fund Rule, Panel B studies the Dodd-Frank Act, and Panel C studies *Goldstein*.

To compare the change in misreporting at the treatment funds relative to the control funds after the two events imposing mandatory regulation (Hedge Fund Rule in 2004 and Dodd-Frank Act rules in 2011), Panels A and B use the following equation:

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.

²⁷ For example, 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 1, Panel A.

$$Num. \text{ Flags} = \alpha + \beta_1 Post + \beta_2 New \text{ Fund} + \beta_3 Post * New \text{ Fund} + Controls + Fixed \text{ Effects} + \varepsilon$$

Each model includes the 60 months surrounding the event date, and the Post variable is set to one in the period after the rule was adopted and to zero in the period before. The New Fund variable is set to one for all treatment funds and to zero for all control funds. The variable of interest capturing the difference-in-differences effect is the interaction term between these two variables. I present the analysis (1) using fixed effects for each fund's country of incorporation and investment style, and (2) using fund fixed effects. The New Fund variable is omitted from the regressions that include fund fixed effects because it is collinear with the fund fixed effects. The results are presented first using the full sample of funds in columns (1) and (2) and then using the matched sample in columns (3) and (4).

The interaction term is negative and statistically significant in all models in Panels A and B, indicating that misreporting decreased for the newly regulated funds. The models show that, following the adoption of the Hedge Fund Rule, the mean fund subjected to regulation triggered roughly 0.25 fewer flags than would have been expected based on the control sample. The economic magnitude for the Dodd-Frank Act is similar or slightly greater (roughly 0.29-0.34 fewer flags than expected).

Using the equation below, Panel C 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 (however, I caution that the results are descriptive rather than causal because the decision to withdraw is highly endogenous).

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

As described previously, 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 regulated. 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 funds that withdrew and the

funds that remained after *Goldstein* relative to the change in misreporting for the control funds during the same period. The Withdraw and Remain indicator variables are omitted from the regressions that include fund fixed effects because they are collinear with the fund fixed effects.

Results using the full sample find that, post-*Goldstein*, the funds that withdrew from federal oversight increased misreporting relative to the control funds. The interaction term between Post and Withdraw is positive and statistically significant. However, this finding should be interpreted with caution, as statistical significance is lower than in the prior two tables and the results for the matched sample are not statistically significant. Nonetheless, the finding complements the prior analysis showing that imposing regulation reduces misreporting.

2. Four-Period Event Window. Table 4 tests whether regulation reduced misreporting using the four-period event window. Panel A studies the Hedge Fund Rule and *Goldstein*, and Panel B studies the Dodd-Frank Act. The equations in both tables use dummy variables for each period and interact those dummies with the New Fund indicator variable. All periods are as defined previously, and each model omits one period due to collinearity.

Panel A uses the equation below to show that misreporting decreased in the period of mandatory regulation between the Hedge Fund Rule and *Goldstein*, and that this decrease was driven by the funds that withdrew post-*Goldstein*.

$$\begin{aligned} \text{Num. Flags} = & \alpha + \beta_1 \text{New Fund} + \beta_2 \text{Before Regulation Period} + \beta_3 \text{Mandatory Regulation Period} + \\ & \beta_4 \text{Voluntary Regulation Period} + \beta_5 \text{New Fund*Before Reg. Period} + \beta_6 \text{New Fund*Mandatory Reg. Period} + \beta_7 \\ & \text{New Fund*Voluntary Reg. Period} + \text{Controls} + \text{Fixed Effects} + \varepsilon \end{aligned}$$

Using the full sample of funds, columns (1) and (2) show that misreporting at the treatment funds did not differ from the control funds prior to regulation or in the period of voluntary regulation (i.e., post-*Goldstein*), but that the treatment funds had lower misreporting during the period of mandatory regulation. Columns (3) and (4) use only the subset of newly regulated funds that remain regulated and find no difference in misreporting between the treatment and control funds after regulation (the coefficients are consistent with greater misreporting prior to regulation and lower misreporting after regulation, but

statistical significance is 15% at best). Columns (5) and (6), however, use only the subset of newly regulated funds that withdrew and find that these funds significantly decreased misreporting upon regulation, but that they did not differ from the control funds in either of the other two periods.

F-tests reported at the bottom of the page provide additional evidence that misreporting declined upon regulation. In columns (1)-(4), the coefficients on the interaction terms New*Mandatory Regulation and New*Voluntary Regulation and are significantly lower than those on New*Before Regulation, suggesting that misreporting decreased after regulation. By comparison, in columns (5) and (6) testing the funds that withdrew, the coefficients on New*Mandatory Regulation are again significantly lower than those on New*Before Regulation, but the coefficients on New*Voluntary Regulation are *higher* than those on New*Mandatory Regulation, suggesting that these funds increased misreporting after withdrawing.

The results provide some interesting takeaways. First, they show that misreporting between the treatment and control funds did not differ significantly prior to regulation, but that misreporting decreased at the treatment funds immediately following regulation. Second, they show that the Withdraw funds were more responsive to regulation—i.e., the firms that presumably incurred the greatest costs of regulation (and thus withdrew) also had the greatest decreases in misreporting upon regulation. One explanation for this result, consistent with the comply and explain literature, is that the Withdraw funds expected greater scrutiny upon regulation. The SEC conducts both random examinations and targeted examinations of higher-risk funds, and these funds may have expected to be targeted by the SEC because they had more warning signs in their Form ADV filings.²⁸

The results in Panel B on the Dodd-Frank Act are directionally consistent with those in Panel A but not statistically significant. For example, in column (1), there does not appear to be a difference in misreporting between the control and treatment funds in the Lagged period, but the treatment funds

²⁸ Of the 126 treatment funds reported in Panel A of Table 12, seven of the 48 that withdrew (14.58%) had at least one red flag. For comparison, only six of the 76 funds that remained (7.89%) had a red flag.

potentially have more misreporting in the period before regulation (20%) and potentially have less misreporting in the period after regulation (20%). All models use the equation below.

$$\begin{aligned} \text{Num. Flags} = & \alpha + \beta_1 \text{New Fund} + \beta_2 \text{Lagged Regulation Period} + \beta_3 \text{Before Regulation Period} + \beta_4 \text{Mandatory} \\ & \text{Regulation Period} + \beta_5 \text{New Fund} * \text{Lagged Reg. Period} + \beta_6 \text{New Fund} * \text{Before Reg. Period} + \beta_7 \text{New} \\ & \text{Fund} * \text{Mandatory Reg. Period} + \text{Controls} + \text{Fixed Effects} + \varepsilon \end{aligned}$$

Although the results are not significant at standard levels, the lack of statistical significance could be due to the sample composition. As mentioned earlier, there are only 65 treatment funds in this analysis, compared with 112 treatment funds in the two-period model. Further, F-tests reported at the bottom of the page suggest that misreporting may have declined, even if the coefficients in the model are not statistically significant. In particular, the coefficients on New*Before Regulation are significantly higher than those on New*Mandatory Regulation. Importantly, however, the coefficients on New*Lagged and New*Before Regulation are not significantly different from one another at standard levels, suggesting that the difference in misreporting during the treatment and control funds was consistent during this limited timeframe.

In sum, Tables 3 and 4 show that hedge fund regulation reduced misreporting. Using the two-period model, Table 3 provides strong evidence that misreporting decreased at the funds that became subject to regulation. Further, using the four-period model, Table 4 provides evidence that misreporting significantly decreased during the period of mandatory regulation between the Hedge Fund Rule and *Goldstein*.

C. 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? To understand why regulation would reduce misreporting, I engaged in a two-step process. First, I reached out to hedge fund compliance officers and other practitioners at the funds in my sample to ask for their perspectives. Second, I ran additional empirical tests based on their anecdotal feedback.

1. Anecdotal evidence. My inquiries provided helpful anecdotal evidence outlining two possible mechanisms through which hedge fund regulation 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. For example, rather than publicly disclose that the fund was not audited regularly by an independent public accountant, some fund managers hired such an auditor (although current investors already have this information, the fund managers wanted to maintain a good image for prospective investors). Second, many respondents indicated that chief compliance officers enjoyed increased status upon SEC regulation. Some funds appointed compliance officers for the first time.²⁹ And funds that already had compliance officers were often thought to place a greater value on the officer's input following regulation.

In sum, anecdotal evidence suggests that the disclosure requirements decreased misreporting by spurring internal governance changes. These governance changes, in turn, induced funds to report their financial performance more accurately. Although this explanation is intuitive, it is difficult to test empirically because very little information is available on funds' internal governance prior to regulation. The commercial databases are largely focused on funds' financial performance, not their governance.

2. Changes in auditing behavior. 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 an auditor and/or switched auditors after regulation. I consider a fund to have hired an auditor if none of the 30 months prior to regulation were audited, but one or more of the 30 months after regulation was audited.³⁰ 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 regulation differs from the CompanyID associated with the auditor in the final month of period following regulation. For example, if a fund lists its auditor as PwC in the Lipper Hedge Fund database when the SEC adopted the rules to implement the Dodd-Frank Act in June 2011 but lists E&Y as its auditor in December 2013 (the final month of the sample period), I would consider the fund

²⁹ The Full-Regulation funds were required to have a compliance officer. However, the Disclosure-Only funds were exempt from this requirement—they were only required to indirectly disclose whether they had such an officer.

³⁰ The audit date field is continuously updated to reflect the fund's most recent audit, so this analysis can only be performed with historical data. The field itself reflects the date of the fund's last audit, but does not indicate how many months were audited. I presume that all audits reflect the prior twelve months (i.e., a date indicating 12/31/2004 would cover 1/1/2004 through 12/31/2004).

to have switched auditors. 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 US).

Table 5 presents evidence that the newly regulated funds were more likely to switch auditors. Panel A provides descriptive statistics and shows that 18% (17%) of the funds that were audited prior to regulation 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 regulation (and thus eligible to switch), and the third represents the number that actually switched.

Panel B examines the frequency of auditor switching more formally using a regression model, and shows that the newly regulated funds were roughly 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 and to 0 otherwise. The control variables are the same as those used in Table 3 (except that I no longer control for the number of months audited), and the control funds include only the subset for which I can identify the auditor.

Table 6 examines whether the change in auditors reduced misreporting. Panel A uses the two-period model and partitions the newly regulated funds into two groups: (1) those that switched auditors (Switch); (2) those that did not (No Switch) and interacts these dummy variables with the Post variable. Panels B and C extend the analysis using the four-period model. In the four-period model, the dummy variable Switch is interacted with dummy variables for each of the three periods included in the model (as

before, one period is dropped due to collinearity). The control variables are those noted in Table 1 (except that the control for the number of audited months is not included).

Despite the limited number of funds that switched auditors, the results provide some evidence that the funds that switched auditors experienced greater declines in misreporting than those that did not. In Panel A, the coefficient on Post*Switch is negative in all columns and statistically significant (or close to significance) in the columns using fund characteristics fixed effects. By contrast, the coefficient on Post*No Switch is not statistically significant in any models. Further, in Panels B and C, there is evidence that the funds that switched had lower levels of misreporting after regulation, but there is no evidence that these funds differed from the control funds prior to regulation. In particular, in Panel B, the funds that switched appeared to have lower levels of misreporting than the control funds after *Goldstein* but not before (perhaps it took some time for reporting quality to improve and the 22-month period from the adoption of the Hedge Fund Rule until *Goldstein* was too short to capture this improvement). Finally, in Panel C, the funds that switched auditors were no different from the control funds in either of the two periods prior to regulation, but experienced a significant decline in misreporting after the Dodd-Frank Act. F-tests reported at the bottom of the table are consistent with this interpretation. In sum, there is evidence that the funds that switched auditors experienced greater declines in misreporting than those that did not. Although statistical significance is low in Panels A and B, this may be a power issue.

Tables 7 and 8 provide similar analysis as Tables 5 and 6, except that these tables examine the funds that hired auditors rather than those that switched auditors. Panel A of Table 7 presents descriptive statistics. Following the Hedge Fund Rule (Dodd-Frank Act), 53% (37%) of the funds that were not audited hired an auditor. The table also shows the number of funds that initiated audits but were required to do so because they had custody of client assets. A regulated 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 (however, as I discuss below, the Disclosure-Only funds were also more likely to switch and/or hire auditors and they are not subject to this requirement).

Panel B examines the likelihood that funds hired an auditor following regulation. The equation is the same as that used in Panel B of Table 5, except that the dependent variable is a dummy variable indicating whether the fund hired an auditor rather than if it switched auditors. The table provides some evidence that the newly regulated funds are more likely to initiate audits (the variable of interest is positive and statistically significant for the Dodd-Frank Act but not the Hedge Fund Rule).

Table 8 studies whether this change in governance reduced misreporting. Using the two-period event window, Panel A partitions the funds into three groups: (1) those that hired an auditor following regulation (Initiate Audit); (2) those that were audited prior to regulation (Audit); and (3) those that were not audited prior to regulation and did not initiate an audit (No Audit). Panels B and C extend this analysis using the four-period event window. In the four-period model, the dummy variable Initiate is interacted with dummy variables for each of the three periods included in the model (as before, one period is dropped due to collinearity). The control variables are those noted in Table 1 (except that the control for the number of audited months is not included).

The results, shown in Table 8, provide evidence that the decrease in misreporting following regulation was driven by the funds that hired auditors. In Panel A, following the Hedge Fund Rule, only the subset of funds that initiated audits experienced a statistically significant decrease in misreporting in both models. Following the Dodd-Frank Act, the group of funds that initiated audits appeared to experience the greatest decrease in misreporting, although the funds that were not audited before regulation also experienced a significant decrease in misreporting.³¹ This is an important point. It seems likely that the funds lacking an auditor had limited governance in other areas as well, so this result suggests that regulation

³¹ Unreported F-tests show that the difference between the coefficients on Post*Initiate and Post*No Audit in the Dodd-Frank setting is statistically significant in OLS models, but not in ordinal logit models

most benefitted the poorly governed funds (even if those funds did not hire an auditor, there are many other potential governance changes).

Panels B and C also indicate that the funds that hired an auditor experienced the greatest declines in misreporting. In both panels, the level of misreporting at the funds that hired an auditor did not differ from the control funds prior to regulation, but was significantly lower after regulation (5% in three models, and 10% in one model). In sum, although I am only able to examine auditing behavior rather than a broader swath of governance changes, Tables 5-8 provide evidence that internal governance changes spurred by the regulatory regime induced funds to report more accurately.

D. Mandating Disclosure

In this section, I study the behavior of the Disclosure-Only funds (i.e., the funds only subject to disclosure rules). If the regulation reduced misreporting by spurring funds to make internal governance changes because of the soft nudge of the disclosure regime, the Disclosure-Only funds should also reduce misreporting. Panel A of Table 9 presents descriptive statistics on these funds. On average, relative to the Full-Regulation funds, the Disclosure-Only funds are smaller, more likely to be audited, have lower returns, and have less return volatility. However, the means mask the variability in the Disclosure-Only funds: they have assets ranging from roughly \$65M to \$12B and average monthly returns from roughly -0.50 to 5.51.

As shown in Figure 4, most Disclosure-Only funds are foreign. This is not surprising given the eligibility restrictions. As mentioned earlier, to be eligible for Disclosure-Only status, US advisory firms must advise only venture capital funds or only private funds (i.e., hedge funds) with less than \$150 million in total assets—a relatively small sum for an advisory firm. By contrast, foreign advisors are eligible for Disclosure-Only status if they advise funds with less than \$150 million in assets *from US investors*. As such, many of the Disclosure-Only funds are foreign funds that are relatively similar in size and age to the Full-Regulation funds—but have limited funding from US investors. Figure 4 shows the specific countries of incorporation for the Disclosure-Only funds; the Cayman Islands and Brazil are the two most common locations.

Panel B of Table 9 presents descriptive statistics on the frequency of misreporting at the control, Full-Regulation, and Disclosure-Only funds. Overall, the pattern of misreporting at the Disclosure-Only funds is very similar to that at the Full-Regulation funds. The frequency of flags decreases from 0.54 to 0.21 (0.59 to 0.18) at the Disclosure-Only (Full-Regulation funds), and unreported tests show the decreases are statistically comparable. Panel B also presents descriptive statistics for the matched sample of Full-Regulation and Disclosure-Only funds. The matched sample is created using the same procedures as the matched samples presented in Table 2.³² After applying these criteria, I have 60 Disclosure-Only funds and 60 Full-Regulation funds. As shown, the mean number of flags for the matched sample was the same prior to regulation and comparable after regulation.

To study the effect of mandating disclosure, Panel C of Table 9 compares the change in misreporting at both the Full-Regulation and Disclosure-Only funds relative to the control funds. Beginning with the equation used in Panel B of Table 3, I add two new variables: (1) Disclosure-Only, a dummy variable reflecting the Disclosure-Only funds, and (2) Post* Disclosure-Only, the interaction between these two variables. 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. The results show that the Disclosure-Only funds decreased misreporting, and that the Full-Regulation and Disclosure-Only funds had statistically equivalent reductions in misreporting.³³

Panel D further supports this inference using the four-period model. Using the same Disclosure-Only variable, I interact this variable with the dummies representing each period. The results show that misreporting between the Disclosure-Only and control funds did not differ in each of the two periods prior to regulation, but that misreporting at the Disclosure-Only funds significantly declined after SEC regulation.

³² All funds must have the same number of flags for misreporting in the period prior to regulation, US funds are matched to US funds (and non-US to non-US), and funds are matched to funds with the same investment strategy. If multiple funds meet these criteria, funds are matched based on their propensity to be a Full-Regulation fund as opposed to a Disclosure-Only fund, where the propensity is based on a probit model that includes the variables noted in Panel A of Table 9.

³³ 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 statistically.

In sum, Table 9 provides strong evidence that mandatory disclosure, even on its own, can reduce misreporting by hedge funds.

This result is perhaps surprising. Hedge fund investors are generally considered 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) hedge fund investors already had access to the information in Form ADV before mandatory regulation. It is therefore not obvious that public disclosure of this information would have this effect.

However, the result is consistent with the prior evidence that regulation spurred governance changes. Further consistent with this prior evidence, Table 10 shows that the Disclosure-Only funds were also more likely to switch and/or hire auditors. Panel A shows that 42% of the funds that were already audited switched their auditor, and 40% of the funds that were previously unaudited hired auditors post-regulation. (These findings are especially notable because Disclosure-Only funds with custody of client assets are not subject to the same audit requirement as Full-Regulation funds. Hence, the change in behavior for this subset of funds is not mandatory.) Using the same equations as in Tables 5 and 7, Panel B formally compares the Disclosure-Only funds to the control funds using a regression model. The results show that the Disclosure-Only funds were more likely to both switch auditors and initiate audits.

E. Fund Inflows and Outflows

Given the seeming benefits of regulation, one might wonder whether fund flows from investors changed for the treatment funds. There is ample reason to think they would. Even aside from the benefits noted here, and as discussed earlier, registration benefits funds that wish to manage pension accounts. Pension assets represent a sizeable portion of total available hedge fund assets, so hedge funds likely open themselves to a wider pool of assets by registering.

Table 11 analyzes the changes in fund flows following regulation using the two-period event window. Panel A shows the results for the Hedge Fund Rule and Dodd-Frank Act, and Panel B shows the

results for *Goldstein*. 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 for ease of interpretation (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 3.³⁴

Panel A shows that fund flow increased following the Hedge Fund Rule and Dodd-Frank Act. Panel B finds that fund flow significantly decreased for the newly registered following *Goldstein*, especially for the funds that withdrew. Although there is a slight decrease in fund flows for the newly regulated funds that remain regulated—perhaps investors lose some interest in the newly regulated funds after the initial increase—the decrease is far greater for the funds that withdraw (unreported F-tests show that the decrease for the funds that withdraw is statistically greater than for the funds that remain regulated). These results might explain why many of the Withdraw funds dissolved relatively quickly after withdrawing. Of the 55 Withdraw funds included in the two-period event window 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%).

My finding that fund flow increased for the newly regulated funds after the Dodd-Frank Act conflicts with the findings of Cumming, Dai, and Johan (2017), which suggests that hedge fund registration caused a decrease in fund flow for long/short funds, funds of funds, and market neutral funds. The difference may result from different research designs. Cumming et al. (2017) uses all funds in the Lipper Hedge Fund database and runs difference-in-differences tests comparing US and non-US funds, assuming that all US funds registered in response to Dodd-Frank and that non-US funds did not register in response to Dodd-

³⁴ Because fund flow is calculated at the monthly level, I drop the relevant funds during the overlap periods (e.g., the analysis of the Hedge Fund Rule drops the funds that withdraw after *Goldstein* in the month they withdrew).

Frank. By comparison, my paper matches the Lipper Hedge Fund data to Form ADV to determine the registration status of each fund.³⁵

6. Robustness

A. Funds Most Likely to Be Scrutinized

Prior literature on comply and explain suggests that this type of regulation is most effective for those most likely to be scrutinized. 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 specifically consider the presence the any of the five following red flags: (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 3A in the Online Appendix). I consider a fund more likely to be scrutinized if it has any of these five red flags.

Panels A and B of Table 12 provide relevant descriptive statistics. 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 possible explanation for this result 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. Across both time periods, the only consistent finding is that funds without red flags have more return volatility.

Using the two-period window, Panel C provides evidence that that funds with one or more red flags had greater decreases in misreporting upon regulation. I partition the newly regulated funds into two groups: (1) Red Flag, those with one or more red flags, and (2) No Flag, those without a red flag, and I interact each

³⁵ Had I followed the approach in Cumming et al. (2017), my sample would have looked very different. First, in my sample, only ~25% of the US funds registered in response to the Dodd-Frank Act—the rest were already registered. Second, in my sample, ~75% of the funds that registered in response to the Dodd-Frank Act were domiciled abroad.

of these variables with the Post indicator. The decrease in misreporting is statistically significant for the funds with red flags in all four models. Further, F-tests show that the decrease is greater for the Red Flag funds than the No Flag funds in all models. In sum, the decrease in misreporting is more pronounced for the funds that should theoretically be most affected.³⁶

B. Endogeneity of Disclosure-Only Funds

As described previously, the assignment of funds to the Full-Regulation and Disclosure-Only categories was not random. Not only was there a cutoff based on assets, but all funds that were eligible for Disclosure-Only status could have elected to be Full-Regulation funds. To address the possible selection bias, I present two robustness tests in Table 3A in the Online Appendix. Columns (1) and (2) create a matched sample of Full-Regulation and Disclosure-Only funds using the matching procedure defined in Table 3, and again show that the decrease in misreporting after regulation was statistically equivalent for both groups. Columns (3) and (4) find the same result using a quasi-discontinuity analysis comparing those funds that were eligible for the Disclosure-Only regime with those that were almost eligible. In particular, because Disclosure-Only advisors must have between \$100 and \$150 million in US assets, columns (3) and (4) compare these funds to the Full-Regulation funds that are managed by advisors with \$150 to \$200 million in assets. The idea behind this test is that the advisors with just over and just under \$150 million should be very similar—but that only those with less than \$150 million were eligible for the disclosure only regime.³⁷ The results provide further confidence that the decrease in misreporting for these two groups was statistically equivalent.

³⁶ These cross-sectional analyses are particularly important in this context given that the timing of the regulatory events studied here 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 that drove the adoption of the regulation may have also driven funds to change their behavior. Although the use of a control group that should have also been affected by market conditions helps to alleviate this concern, cross-sectional tests showing that the funds most likely to be scrutinized appear to experience greater decreases in misreporting provide further confidence that the primary results are due to the changes in law rather than general market conditions.

³⁷ Differences in the nature of this cutoff for foreign advisors make it impossible to reliably determine which foreign advisors were eligible for disclosure-only treatment, so I limited the sample in this test to US advisors with between \$100 million and \$200 million in assets. Foreign advisors are eligible for the disclosure-only regime if they have less

C. Placebo Tests

By definition, the control funds used in the primary analysis were not chosen at random. They were chosen because they had no change in regulatory status following the changes in law—this does not necessarily mean, however, that they were unaffected by the regulation. Prior research on the “constrained cop” theory suggests that managers are more likely to misbehave when they know the “cop” (i.e., the SEC) is distracted (Kedia and Rajgopal, 2011). As applied to my setting, this theory suggests that the control funds may have increased misreporting after the imposition of mandatory regulation. On the one hand, some of the descriptive statistics in Table 2 appear to support this theory, as there appears to be an increase in misreporting at the control funds following the imposition of the Hedge Fund Rule. On the other hand, some changes in the frequency of misreporting are to be expected because misreporting varies with economic cycles.

To test for evidence of the constrained cop theory, I compare the control funds to funds that were completely unconnected to the US regulatory regime (i.e., “Unaffected” funds). My sample of funds completely unconnected to the US regulatory regime includes all funds in the Lipper Hedge Fund database that are located outside the US, do not file Form ADV, and report throughout the entire relevant period.³⁸ Following both the Hedge Fund Rule and the Dodd-Frank Act, the control funds follow the same trend as the Unaffected funds, indicating that changes in misreporting are driven by economic fluctuations rather than the constrained cop theory. The results are presented in the Online Appendix in Table 4A.

than \$150 million in assets from US investors. However, advisors only disclose total assets, not assets from US investors. As such, for foreign advisors, the available data do not allow me to determine whether an advisor is close to the threshold. The resulting sample is limited, but I include the results for completeness.

³⁸ Although I considered using these unaffected funds as the control sample in the main tests, I believe the US-regulated funds are a better control group because there were changes in US regulation beyond the specific provisions I study here. For example, as noted in footnote 12, the Dodd-Frank Act gave the SEC increased authority to pay financial rewards to whistleblowers. This change affected both the newly regulated funds and the funds already regulated by the SEC (i.e., the control funds), but is unlikely to affect the Unaffected funds.

D. Survivorship Bias

At a minimum, for a fund to be included in my sample, I require 30 months of data both before and after each regulatory change. As such, my analysis omits the funds that do not survive at least 60 months—potentially a significant subset of the data. Indeed, survivorship bias in hedge fund data has been debated extensively; at the extremes, Barès, Gibson, and Gyger (2001) estimate that 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 examine whether there is a change in the likelihood that a newly regulated fund “dies” following regulation. A fund that “dies” is one that no longer self-reports to the hedge fund databases, and prior papers have shown that fund death is a very bad signal. For example, Agarwal, Fos, and Jiang (2013) examines what happens to funds after death and note that “the funds that terminated reporting did so for reasons that were overwhelmingly negative ... such as liquidation, fund dormancy, or the data vendor’s inability to contact the fund”. Moreover, fund death is highly correlated with misreporting (Capco, 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, the analysis includes all potential treatment and control funds, not just those with sixty months of data surrounding the event date. The analysis, presented in Table 5A in the Online Appendix, uses the two-period model to show that the newly regulated funds became less likely to “die” following both the Hedge Fund Rule and the Dodd-Frank Act.³⁹ The dependent variable is a dummy set to 1 if the fund disappeared from the Lipper Hedge Fund Database, and to 0 otherwise. The variable of interest is the interaction between Post and New Fund. Columns (1) and (2) present the analysis for the Hedge Fund Rule, and Columns (3) and (4) present the analysis for the Dodd-Frank Act. Because the coefficients on the interaction terms are negative and statistically significant, this test provides evidence that the positive benefits of regulation were not unique to firms that survived at least sixty months.

³⁹ The analysis of the Dodd-Frank Act includes both the Full-Regulation and the Disclosure-Only funds.

E. Inherent Limitations

Finally, I note two methodological limitations to my analysis. First, because I study the change in misreporting at the funds newly subject to federal oversight, I omit funds that evaded federal regulation. Second, my analysis is based on proxies for misreporting, not actual incidences of misreporting.

1. Regulatory avoidance. Prior work has found evidence that some firms evade federal regulation (e.g., Leuz, Triantis, and Wang, 2008; Bushee and Leuz, 2005), and evasion is a particular concern for hedge funds (Greenspan, 1998). Because Greenspan specifically noted that hedge funds are highly mobile and may relocate to avoid regulation, I reviewed historical data to ascertain whether funds relocated around the time of the legal changes. Although I found no evidence that funds engaged in systematic relocation to avoid regulation, I cannot rule out the possibility that funds may have 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 et al., 2014).

2. 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, regulation—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 regulation 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 uses a rich setting to study whether regulation reduced misreporting by hedge funds, and, if so, why the regulation was effective. The setting is unique in that it provides for three changes in law over a relatively short period, and because it allows for focused study on the disclosure of governance

information. My analysis shows that hedge fund regulation reduced misreporting. Further, the results indicate that requiring funds to publicly disclose information about their governance spurred them to make internal changes in governance, such as hiring or switching the fund's auditor, and that these changes ultimately induced them to report their financial performance more accurately. This result indicates that comply and explain regimes can reduce hedge funds' misreporting.

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Table 1. Descriptive Statistics on Treatment and Control Funds. Table 1 provides descriptive statistics for the treatment and control funds in the two-period event windows. Panel A reflects the funds used in the analyses of the Hedge Fund Rule and the Dodd-Frank Act. The treatment funds are those that became regulated following the change in law, and the control funds are those that were continuously regulated by the SEC throughout the entire event window. Panel B reflects the sample used in the *Goldstein* analysis. The control funds are defined as before, whereas the treatment funds are partitioned into two groups: withdraw and remain. Funds that withdrew from SEC regulation are assigned to the withdraw group, and those that remained regulated are assigned to the remain group. The table shows each fund's mean monthly return, mean log of net asset value, mean age, return volatility, sensitivity to liquidity, whether the fund is incorporated in the US, 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 thirty months prior to the event date.

Panel A.

Variable	Hedge Fund Rule			Dodd-Frank Act		
	Treat.	Control	t-stat	Treat.	Control	t-stat
Monthly Return	0.92	0.74	-3.45	1.67	1.23	3.08
Ln (Net Asset Value)	5.73	6.19	3.33	6.27	6.01	-1.80
Age	7.07	7.39	4.86	7.43	7.61	2.52
Return Volatility	2.37	1.88	-3.62	3.53	2.77	-3.24
US Incorporation	0.20	0.33	2.61	0.30	0.32	-0.51
Liquidity Sensitivity	56.02	56.08	0	-20.61	-12.64	0.997
Num. Audited Months	3.89	3.02	-1.58	4.76	7.78	3.14
<i>Num. Funds</i>	126	235		112	569	

Panel B.

Variable	<i>Goldstein</i> Opinion					
	Withdraw	Control	t-stat	Withdraw	Remain	t-stat
Monthly Return	0.74	0.54	-3.63	0.74	0.71	0.37
Ln (Net Asset Value)	5.51	5.98	2.67	5.51	6.05	-2.86
Age	7.36	7.44	0.89	7.36	7.14	2.32
Return Volatility	2.57	1.80	-4.42	2.57	2.53	0.18
US Incorporation	0.13	0.30	2.71	0.13	0.20	-1.08
Liquidity Sensitivity	22.15	-13.12	-3.98	22.15	19.53	0.19
Num. Audited Months	8.09	4.93	-2.87	8.09	5.46	1.93
<i>Num. Funds</i>	55	289		55	102	

Table 2. Descriptive Statistics on the Frequency of Flags for Misreporting. Table 2 provides descriptive statistics on the frequency of “flags” for misreporting at the treatment and control funds. Panels A and B include the full sample, and Panels C and D include only the matched sample. Panels A and C reflect the aggregate number of flags, and Panels B and D present the disaggregated results for each proxy. The treatment and control funds are as defined in Table 1.

Panel A.

	Number of Flags – Full Sample								
	Hedge Fund Rule			<i>Goldstein</i>			Dodd-Frank Act		
	Control	Treat.	t-test	Control	Treat.	t-test	Control	Treat.	t-test
Before	0.17	0.28	-2.16	0.26	0.13	2.01	0.39	0.59	-3.22
After	0.39	0.25	2.15	0.11	0.13	-0.33	0.43	0.18	4.02
t-test (after v before)	4.22	-0.54		4.38	0.00		-1.05	5.1	
Diff. (after - before)	0.22	-0.03	3.24	-0.15	0.00	-1.88	0.04	-0.41	5.23

Panel B.

	Full Sample: Frequency of Each Type of Flag – Hedge Fund Rule						
	Before Regulation			After Regulation			
	Control	Treat.	t-test	Control	Treat.	t-test	
Kink	0.10	0.15	-1.40	0.21	0.14	1.73	
Cookie Jar	0.08	0.14	-1.64	0.18	0.11	1.79	

	Full Sample: Frequency of Each Type of Flag – <i>Goldstein</i>						
	Before Court Opinion			After Court Opinion			
	Control	Treat.	t-test	Control	Treat.	t-test	
Kink	0.17	0.09	1.32	0.09	0.13	-0.96	
Cookie Jar	0.10	0.04	1.52	0.02	0.00	1.17	

	Full Sample: Frequency of Each Type of Flag – Dodd-Frank Act						
	Before Regulation			After Regulation			
	Control	Treat.	t-test	Control	Treat.	t-test	
Kink	0.08	0.16	-2.45	0.10	0.08	0.59	
Cookie Jar	0.31	0.42	-2.18	0.33	0.11	4.36	

Panel C.

Number of Flags – Matched Sample									
	Hedge Fund Rule			<i>Goldstein</i>			Dodd-Frank Act		
	Control	Treat.	t-test	Control	Treat.	t-test	Control	Treat.	t-test
Before	0.25	0.25	0.00	0.13	0.13	0.00	0.49	0.49	0.00
After	0.49	0.24	2.82	0.12	0.13	0.25	0.49	0.20	3.34
t-test (after v before)	-2.73	0.16		0.25	0.00		0.00	3.43	
Diff. (after - before)	0.24	0.01	2.30	-0.01	0	-0.19	0.00	-0.29	2.24

Panel D.

Matched Sample: Frequency of Each Type of Flag – Hedge Fund Rule						
	Before Regulation			After Regulation		
	Control	Treat.	t-test	Control	Treat.	t-test
Kink	0.15	0.15	0.00	0.25	0.11	2.46
Cookie Jar	0.10	0.10	0.00	0.24	0.13	2.07

Matched Sample: Frequency of Each Type of Flag – <i>Goldstein</i>						
	Before Court Opinion			After Court Opinion		
	Control	Treat.	t-test	Control	Treat.	t-test
Kink	0.08	0.10	-0.35	0.08	0.13	-0.95
Cookie Jar	0.06	0.04	0.45	0.04	0.00	1.43

Matched Sample: Frequency of Each Type of Flag – Dodd-Frank Act						
	Before Court Opinion			After Court Opinion		
	Control	Treat.	t-test	Control	Treat.	t-test
Kink	0.10	0.13	0.53	0.14	0.07	1.36
Cookie Jar	0.39	0.37	0.34	0.35	0.13	3.24

Table 3. Regulation & Misreporting: Two-Period Event Window. Table 3 examines the effect of SEC regulation on misreporting using the two-period event window. All models are run using OLS, and the dependent variable is the number of misreporting flags. In all panels, models (1) and (2) use the full sample of funds, and models (3) and (4) use the matched sample. Panel A shows the analysis for the Hedge Fund Rule, Panel B shows the analysis for the Dodd-Frank Act, and Panel C shows the analysis for *Goldstein*. In Panels A and B, the variable *New Fund* is set to 1 for the newly regulated funds (i.e., the treatment funds), and to 0 for all funds that were continuously regulated by the SEC throughout the entire sample period (i.e., the control funds). For *Goldstein*, the variable *Withdraw* is set to 1 for all funds that became regulated in accordance with the Hedge Fund Rule and later withdrew after it was vacated, and to 0 for all other funds. The variable *Remain* is set to 1 for all funds that became regulated in accordance with the Hedge Fund Rule and remained regulated after it was vacated, and to 0 for all other funds. All models include the control variables noted in Table 1. 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 percent is indicated by *, **, and ***, respectively.

	Hedge Fund Rule			
	(1)	(2)	(3)	(4)
	Full Sample		Matched Sample	
Post	0.24*** (0.06)	0.16 (0.12)	0.24** (0.10)	0.46** (0.19)
New Fund	0.17*** (0.06)		0.04 (0.07)	
Post * New Fund	-0.26*** (0.07)	-0.25** (0.11)	-0.26** (0.11)	-0.24* (0.14)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char.	Fund
Observations	722	722	384	384
Adjusted R-squared	0.05	0.16	0.09	0.11

Panel B.

	Dodd-Frank Act			
	(1)	(2)	(3)	(4)
	Full Sample		Matched Sample	
Post	0.01 (0.04)	0.12 (0.08)	-0.01 (0.11)	-0.14 (0.32)
New Fund	0.17*** (0.06)		-0.01 (0.11)	
Post * New Fund	-0.32*** (0.08)	-0.29*** (0.11)	-0.34** (0.13)	-0.33** (0.19)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char	Fund
Observations	1,362	1,362	284	284
Adjusted R-squared	0.08	0.08	0.19	0.23

Panel C.

	<i>Goldstein</i>			
	(1)	(2)	(3)	(4)
	Full Sample		Matched Sample	
Post	-0.17*** (0.04)	-0.14* (0.07)	0.06 (0.09)	-0.03 (0.18)
Withdraw	-0.17*** (0.06)		-0.03 (0.08)	
Remain	-0.09* (0.05)			
Post * Withdraw	0.18** (0.08)	0.18+ (0.11)	0.04 (0.10)	0.09 (0.15)
Post * Remain	0.08 (0.06)	0.09 (0.09)		
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char.	Fund
Observations	892	892	208	208
Adjusted R-squared	0.04	0.08	0.00	0.15

Table 4. Regulation & Misreporting: Four-Period Event Window. Table 4 examines the effect of SEC regulation on misreporting using the four-period event window. All models are run using OLS models, and the dependent variable is the number of misreporting flags. Panel A presents the joint analysis of the Hedge Fund Rule and *Goldstein* using the following four periods: December 1999 to May 2002 (“Lagged” period), June 2002 to November 2004 (“Before Regulation” period), December 2004 to September 2006 (“Mandatory Regulation” period), and October 2006 to March 2009 (“Voluntary Regulation” period). Columns (1) and (2) use the full sample, columns (3) and (4) use only the subset of treatment funds that remained registered after *Goldstein*, and columns (5) and (6) use only the subset of treatment funds that withdrew after *Goldstein*. The Lagged period is omitted due to collinearity. Panel B presents the analysis of the Dodd-Frank Act using the following four periods: July 2006 to December 2008 (“Lagged” period), January 2009 to June 2011 (“Before Regulation” period), July 2011 to December 2013 (“Mandatory Regulation” period), and January 2014 to June 2016 (“Post Mandatory Regulation” period). The Post Mandatory Regulation period is omitted due to collinearity. 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”). All models control for the variables noted in Table 1, and the treatment and control funds are defined as in Table 3. Standard errors are clustered by fund. Statistical significance of 15, 10, 5, and 1 percent is indicated by ⁺, *, **, and ***, respectively.

Panel A.

	Full Sample		Only Newly Regulated Funds that Remain		Only Newly Regulated Funds that Withdraw	
	(1)	(2)	(3)	(4)	(5)	(6)
New Fund	0.08 ⁺ (0.05)		0.05 (0.05)		0.11 (0.09)	
Before Regulation	0.05 (0.06)	0.10 (0.07)	0.10* (0.06)	0.12 ⁺ (0.08)	0.07 (0.07)	0.11 (0.08)
Mandatory Regulation	0.29*** (0.05)	0.32*** (0.06)	0.30*** (0.05)	0.33*** (0.06)	0.30*** (0.05)	0.32*** (0.06)
Voluntary Regulation	0.06 (0.04)	0.08 ⁺ (0.05)	0.08** (0.04)	0.09** (0.05)	0.06 ⁺ (0.04)	0.08 (0.06)
New * Before Regulation	0.08 (0.07)	0.08 (0.07)	0.13 ⁺ (0.08)	0.12 (0.09)	0.03 (0.09)	0.05 (0.10)
New * Mandatory Regulation	-0.20** (0.08)	-0.19** (0.09)	-0.09 (0.08)	-0.10 (0.10)	-0.33** (0.12)	-0.32** (0.13)
New * Voluntary Regulation	-0.08 (0.06)	0.09 (0.07)	-0.07 (0.06)	-0.06 (0.07)	-0.08 (0.11)	-0.09 (0.13)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char.	Fund	Char.	Fund
Observations	1044	1044	892	892	812	812
Adjusted R-squared	0.07	0.14	0.07	0.16	0.07	0.12
F tests: New*Before vs. New*Mandatory	p<.01***	p<.01***	p<.02**	p<.05**	p<.01***	p<.01***
F tests: New*Before vs. New* Voluntary	p<.02**	p<.04**	p<.02**	p<.05**	p<.34	p<.30
F tests: New* Mandatory vs. New*Voluntary	p<.13	p<.20	p<.74	p<.71	p<.02**	p<.07*

Panel B.

	(1)	(2)
New Fund	-0.05 (0.05)	
Lagged Period	0.04 (0.03)	-0.01 (0.04)
Before Regulation	0.30*** (0.05)	0.27*** (0.08)
Mandatory Regulation	0.39*** (0.06)	0.39** (0.09)
New * Lagged Period	-0.04 (0.05)	-0.06 (0.06)
New * Before Regulation	0.10 (0.08)	0.09 (0.09)
New * Mandatory Regulation	-0.11 (0.08)	-0.11 (0.09)
Controls	Yes	Yes
Fixed Effects	Char.	Fund
Observations	1796	1796
Adjusted R-squared	0.09	0.10
F tests: New*Lagged vs. New*Before	p<.11	p<.14
F tests: New*Lagged vs. New*Mandatory	p<.38	p<.58
F tests: New* Before vs. New*Mandatory	p<.03**	p<.07*

Table 5. Auditor Switches Upon Regulation. Table 5 provides evidence on the number of newly regulated funds that switched auditors. Panel A provides descriptive statistics, and Panel B presents OLS regressions comparing the behavior of the treatment funds to that of the control funds. Column (1) of Panel B analyzes the Hedge Fund Rule while Column (2) analyzes the Dodd-Frank Act. All models in Panel B use the same control group and control variables as the models in Table 3. The dependent variable is set to 1 if the fund switched auditors and to 0 otherwise, and the sample only includes those funds that were audited prior to regulation. Fixed effects are included for the fund’s country of incorporation and investment style (fixed effects for fund characteristics, “Char.”). Standard errors are clustered by fund. Statistical significance of 10, 5, and 1 percent is indicated by *, **, and ***, respectively.

Panel A.

	Hedge Fund Rule		Dodd Frank Act	
Treatment Funds				
Num. Newly Registered Funds with Data	118		107	
Num. Already Audited	67	57%	46	43%
Num. Switched Auditors	12	18%	8	17%
Control Funds				
Num. Newly Registered Funds with Data	178		473	
Num. Already Audited	101	57%	247	52%
Num. Switched Auditors	0	0%	0	0%

Panel B.

	Hedge Fund Rule	Dodd-Frank Act
	(1)	(2)
	Switch Auditor	Switch Auditor
Newly Regulated	0.13***	0.13***
	(0.04)	(0.04)
Controls	Yes	Yes
Fixed Effects	Char.	Char.
Observations	168	293
Adjusted R-squared	0.15	0.31

Table 6. Auditor Switches and Misreporting. Table 6 shows the change in misreporting for the funds that switched auditors upon regulation. The dependent variable is the number of misreporting flags. All models are run using OLS, and only include the subset of funds that were already audited prior to regulation and are thus eligible to switch auditors. Panel A uses the two-period event window defined in Table 3, and Panels B and C use the four-period event window defined in Table 4. Switch is a dummy variable set to 1 for all newly regulated funds that switched auditors, and to 0 otherwise. No Switch is a dummy variable set to 1 for all newly regulated funds that did not switch auditors, and to 0 otherwise. All models control for the variables in Table 1. 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 15, 10, 5, and 1 percent is indicated by +, *, **, and ***, respectively.

Panel A.

	Hedge Fund Rule		Dodd-Frank Act	
	(1)	(2)	(3)	(4)
Post	-0.02 (0.08)	0.01 (0.19)	0.04 (0.06)	-0.02 (0.14)
Switch	0.09 (0.16)		-0.30** (0.14)	
No Switch	0.03 (0.09)		0.21* (0.13)	
Post * Switch	-0.27* (0.16)	-0.25 (0.22)	-0.23+ (0.15)	-0.28 (0.25)
Post * No Switch	-0.05 (0.11)	-0.05 (0.15)	-0.10 (0.17)	0.05 (0.26)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char.	Fund
Observations	336	336	586	586
Adjusted R-squared	0.08	0.13	0.08	0.11

Panel B.

	Hedge Fund Rule	
	(1)	(2)
Switch	-0.04 (0.18)	
Before Regulation	-0.05 (0.07)	-0.07 (0.09)
Mandatory Regulation	0.03 (0.07)	0.06 (0.09)
Voluntary Regulation	0.13 (0.07)	0.10 (0.10)
Switch * Before Regulation	-0.12 (0.17)	-0.12 (0.20)
Switch * Mandatory Regulation	-0.05 (0.29)	-0.06 (0.33)
Switch * Voluntary Regulation	-0.30* (0.18)	-0.30+ (0.20)
Controls	Yes	Yes
Fixed Effects	Char.	Fund
Observations	436	436
Adjusted R-squared	0.14	0.14
F tests: New*Before vs. New*Mandatory	p<.72	p<.82
F tests: New*Before vs. New Voluntary	p<.01***	p<.02**
F tests: New* Mandatory vs. New*Voluntary	p<.22	p<.30

Panel C.

	Dodd-Frank Act	
	(1)	(2)
Switch	-0.30*** (0.08)	
Lagged Period	0.00 (0.05)	-0.05 (0.07)
Before Regulation	0.11 (0.10)	-0.01 (0.15)
Mandatory Regulation	0.24** (0.11)	0.14 (0.15)
Switch * Lagged Period	-0.02 (0.06)	0.01 (0.08)
Switch * Before Regulation	0.14 (0.23)	0.21 (0.27)
Switch * Mandatory Regulation	-0.24*** (0.05)	-0.23*** (0.06)
Controls	Yes	Yes
Fixed Effects	Char.	Fund
Observations	792	792
Adjusted R-squared	0.09	0.13
F tests: New*Lagged vs. New*Before	p<.49	p<.44
F tests: New*Lagged vs. New*Mandatory	p<.01***	p<.01***
F tests: New* Before vs. New*Mandatory	p<.10*	p<.10*

Table 7. Audit Initiations Upon Regulation. Table 7 provides evidence on the number of newly regulated funds that hired auditors. Panel A provides descriptive statistics, and Panel B presents OLS regressions comparing the behavior of the treatment funds to that of the control funds. Column (1) of Panel B analyzes the Hedge Fund Rule while Column (2) analyzes the Dodd-Frank Act. All models in Panel B use the same control group and control variables as the models in Table 3. The dependent variable is set to 1 if the fund hired an auditor and to 0 otherwise, and the sample only includes those funds that were not audited prior to regulation. Fixed effects are included for the fund’s country of incorporation and investment style (fixed effects for fund characteristics, “Char.”). Standard errors are clustered by fund. Statistical significance of 10, 5, and 1 percent is indicated by *, **, and ***, respectively.

Panel A.

	Hedge Fund Rule		Dodd Frank Act	
Treatment Funds				
Num. Initiated Audits	31	53%	24	37%
Num. Initiated Audits with Custody	13	42%	11	46%
Control Funds				
Num. Initiated Audits	83	63%	70	27%
Num. Initiated Audits with Custody	9	11%	46	66%

Panel B.

	Hedge Fund Rule	Dodd-Frank Act
	(1)	(2)
	Initiate Audit	Initiate Audit
Newly Regulated	-0.09 (0.09)	0.12* (0.07)
Controls	Yes	Yes
Fixed Effects	Char.	Char.
Observations	190	321
Adjusted R-squared	0.39	0.21

Table 8. Audit Initiations and Misreporting. Table 8 shows the change in misreporting for the funds that hired auditors upon regulation. The dependent variable is the number of misreporting flags. All models are run using OLS. Panel A uses the two-period event window defined in Table 3, and Panels B and C use the four-period event window defined in Table 4. The variable Initiate Audit is set to 1 for all newly funds that initiated audit procedures following regulation, and to 0 otherwise. The variable No Audit is set to 1 for all newly funds that were not audited prior to regulation and did not initiate audit procedures following regulation, and to 0 otherwise. The variable Audit is set to 1 for all newly funds that were already audited prior to regulation, and to 0 otherwise. All models control for the variables in Table 1. 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 15, 10, 5, and 1 percent is indicated by +, *, **, and ***, respectively.

Panel A.

	Hedge Fund Rule		Dodd-Frank Act	
	(1)	(2)	(3)	(4)
Post	0.14*** (0.05)	0.08 (0.11)	-0.00 (0.04)	0.11 (0.08)
Initiate Audit	0.09 (0.10)		0.29** (0.11)	
No Audit	0.14 (0.10)		0.14 (0.09)	
Audit	-0.02 (0.08)		-0.21** (0.10)	
Post * Initiate Audit	-0.26** (0.11)	-0.27* (0.16)	-0.58*** (0.14)	-0.53*** (0.19)
Post * No Audit	0.02 (0.15)	0.03 (0.22)	-0.31*** (0.10)	-0.34** (0.13)
Post * Audit	-0.20* (0.10)	-0.22 (0.16)	0.43+ (0.29)	0.43 (0.37)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char.	Fund
Observations	722	722	1,362	1,362
Adjusted R-squared	0.04	0.14	0.08	0.11

Panel B.

	Hedge Fund Rule	
	(1)	(2)
Initiate	0.00 (0.06)	
Before Regulation	0.10** (0.05)	0.14** (0.07)
Mandatory Regulation	0.24*** (0.04)	0.26*** (0.05)
Voluntary Regulation	0.05 (0.04)	0.07 ⁺ (0.05)
Initiate * Before Regulation	0.02 (0.11)	0.02 (0.12)
Initiate * Mandatory Regulation	-0.20** (0.09)	-0.19* (0.11)
Initiate * Voluntary Regulation	-0.10* (0.06)	-0.10 ⁺ (0.07)
Controls	Yes	Yes
Fixed Effects	Char.	Fund
Observations	1044	1044
Adjusted R-squared	0.09	0.12
F tests: New*Before vs. New*Mandatory	p<.01***	p<.02**
F tests: New*Before vs. New Voluntary	p<.20	p<.29
F tests: New* Mandatory vs. New*Voluntary	p<.23	p<.34

Panel C.

	Dodd-Frank Act	
	(1)	(2)
New Fund	0.06 (0.11)	
Lagged Period	0.04 (0.03)	-0.02 (0.04)
Before Regulation	0.30*** (0.05)	0.27*** (0.08)
Mandatory Regulation	0.38*** (0.06)	0.37*** (0.08)
Initiate * Lagged Period	-0.18 (0.12)	-0.25* (0.14)
Initiate * Before Regulation	0.04 (0.18)	-0.05 (0.21)
Initiate * Mandatory Regulation	-0.26** (0.13)	-0.29** (0.15)
Controls	Yes	Yes
Fixed Effects	Char.	Fund
Observations	1796	1796
Adjusted R-squared	0.09	0.11
F tests: New*Lagged vs. New*Before	p<.13	p<.26
F tests: New*Lagged vs. New*Mandatory	p<.47	p<.78
F tests: New* Before vs. New*Mandatory	p<.09*	p<.26

Table 9. Disclosure-Only Funds. Table 9 shows the change in misreporting for the funds only subject to disclosure requirements. Panel A compares fund characteristics for the 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). All variables are as defined in Table 1, and the variables reflect the value over the thirty months prior to the event date. Panel B provides descriptive statistics on the frequency of “flags” for misreporting. Panels C and D present OLS regressions comparing the change in misreporting at both Full-Regulation and Disclosure-Only funds relative to the control funds using the two-period event window defined in Table 3 and four-period event window defined in Table 4, respectively. The dependent variable is the number of misreporting flags. All models control for the variables in Table 1. 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 percent is indicated by *, **, and ***, respectively.

Panel A.

Variable	Full-Regulation	Disclosure-Only	t-stat
Monthly Return	1.67	1.01	3.98
Ln (Net Asset Value)	6.27	5.66	3.38
Age	7.43	7.39	0.69
Return Volatility	3.53	2.38	3.30
US Incorporation	0.30	0.13	3.29
Liquidity Sensitivity	-20.61	-9.14	-1.13
Num. Audited Months	4.76	7.89	-2.87
<i>Num. Funds</i>	<i>112</i>	<i>109</i>	

Panel B.

	Full Sample		
	Control	Full Reg.	Disc.-Only
Before Regulation	0.39	0.59	0.54
After Regulation	0.43	0.18	0.21
t-test (after vs. before)	-1.05	5.10	4.88
Diff. (after - before)	0.04	-0.41	-0.34

Panel C.

	(1)	(2)
Post	0.01 (0.04)	0.09 (0.08)
Full-Regulation	0.18** (0.06)	
Disclosure-Only	0.15** (0.07)	
Post * Full-Regulation	-0.32*** (0.08)	-0.30*** (0.11)
Post * Disclosure-Only	-0.37*** (0.08)	-0.35*** (0.11)
Controls	Yes	Yes
Fixed Effects	Char.	Fund
Observations	1,580	1,580
Adjusted R-squared	0.08	0.08

Panel D.

	(1)	(2)
Disclosure-Only	0.18 (0.07)	
Lagged Period	0.05 (0.03)	0.02 (0.04)
Before Regulation	0.32*** (0.06)	0.34*** (0.08)
Mandatory Regulation	0.41*** (0.06)	0.46*** (0.09)
Disclosure-Only * Lagged Period	-0.09 (0.10)	-0.03 (0.10)
Disclosure-Only * Before Regulation	0.01 (0.11)	0.07 (0.11)
Disclosure-Only * Mandatory Regulation	-0.40*** (0.10)	-0.34*** (0.10)
Controls	Yes	Yes
Fixed Effects	Char.	Fund
Observations	1836	1836
Adjusted R-squared	0.08	0.10
F tests: New*Lagged vs. New*Before	p<.28	p<.35
F tests: New*Lagged vs. New*Mandatory	p<.01***	p<.01***
F tests: New* Before vs. New*Mandatory	p<.01***	p<.01***

Table 10. Changes in Audit Behavior for Disclosure-Only Funds. Table 10 provides evidence on the number of Disclosure-Only funds that switched auditors and/or hired auditors. Panel A provides descriptive statistics, and Panel B presents OLS regressions comparing the behavior of the Disclosure-Only funds to that of the control funds. In Column (1) of Panel B, the dependent variable is set to 1 if the fund switched auditors and to 0 otherwise, and the sample only includes those funds that were audited prior to regulation. In Column (2) of Panel B, the dependent variable is set to 1 if the fund hired an auditor and to 0 otherwise, and the sample only includes those funds that were not audited prior to regulation. Fixed effects are included for the fund’s country of incorporation and investment style (fixed effects for fund characteristics, “Char.”). Standard errors are clustered by fund. Statistical significance of 10, 5, and 1 percent is indicated by *, **, and ***, respectively.

Panel A.

	Disclosure-Only Funds	
Switch Auditors		
Num. Newly Registered Funds with Data	66	
Num. Already Audited	26	39%
Num. Switched Auditors	11	42%
<hr/>		
Initiate Audit		
Num. Newly Registered Funds with Data	109	
Num. Not Audited	42	39%
Num. Initiated Audits	17	40%

Panel B.

	Disclosure-Only Funds	
	(1)	(2)
	Switch Auditors	Initiate Audit
Newly Regulated	0.33*** (0.12)	0.15* (0.09)
Controls	Yes	Yes
Fixed Effects	Char.	Char.
Observations	126	298
Adjusted R-squared	0.42	0.16

Table 11. Fund Inflows and Outflows. Table 11 shows the change in fund flow for the treatment funds relative to the control funds using the two-period event window. Panel A presents the results for the Hedge Fund Rule and Dodd-Frank Act, and Panel B presents the results for *Goldstein*. 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 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. 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 percent is indicated by *, **, and ***, respectively.

Panel A.

	(1)	(2)	(3)	(4)
	Hedge Fund Rule		Dodd-Frank Act	
Post	-0.11*** (0.01)	-0.20*** (0.03)	0.35*** (0.20)	-0.28*** (0.03)
Newly Regulated Fund	-0.03 (0.02)		-0.15*** (0.05)	
Post*Newly Regulated	0.05** (0.02)	0.05** (0.02)	0.20*** (0.05)	0.20*** (0.05)
Fixed Effects	Char.	Fund	Char.	Fund
Observations	40,511	40,511	58,611	58,611
Adjusted R-squared	0.42	0.41	0.52	0.52

Panel B.

	(1)	(2)
	<i>Goldstein</i>	
Post	-0.22*** (0.03)	-0.22*** (0.03)
Withdraw	0.24*** (0.04)	
Remain	-0.13 (0.02***)	
Post*Withdraw	-0.13*** (0.04)	-0.13*** (0.04)
Post*Remain	-0.05* (0.03)	-0.05* (0.03)
Fixed Effects	Char.	Fund
Observations	51,958	51,958
Adjusted R-squared	0.51	0.51

Table 12. Funds with Red Flags. Table 12 examines the change in misreporting for funds that have “red flags.” Panel A provides descriptive statistics on the frequency of red flags, where all red flags are defined in Exhibit 3A of the Online Appendix. Panel B compares the funds with and without red flags. All control variables are defined as in Table 1. Panel C presents regression results. All models are run using OLS and use the two-period event window defined in Table 3. The dependent variable is the number of misreporting flags. Red Flag is set to 1 for all newly regulated funds with any red flag, and to 0 otherwise. No Flag is set to 1 for all newly regulated funds without any red flags, and to 0 otherwise. 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 percent is indicated by *, **, and ***, respectively.

Panel A.

Variable	Hedge Fund Rule			Dodd-Frank Act		
	Treatment	Control	t-stat	Treatment	Control	t-stat
Criminal Infraction	0%	1%	1.03	2%	4%	1.02
Civil Infraction	1%	10%	2.92	2%	18%	4.33
Regulatory Infraction	7%	48%	7.98	25%	32%	1.39
Cross-Trades	2%	8%	2.18	6%	12%	1.88
Principal Transactions	4%	19%	3.78	9%	27%	4.40
Any Red Flag	10%	49%	7.81	38%	49%	2.13
<i>Num. Funds</i>	126	235		112	569	

Panel B.

Control Variable	Hedge Fund Rule			Dodd-Frank Act		
	Red Flag	No Red Flag	t-stat	Red Flag	No Red Flag	t-stat
Monthly Return	0.70	0.87	3.18	1.33	1.26	-0.74
Ln (Net Asset Value)	6.09	6.01	-0.56	5.88	6.20	2.95
Age	7.38	7.24	-2.04	7.48	7.69	3.96
Return Volatility	1.63	2.29	5.00	2.59	3.16	3.28
US Incorporation	0.28	0.29	0.22	0.31	0.29	-0.38
Liquidity Sensitivity	64.88	51.28	-1.34	-5.39	-21.70	-2.75
Num. Audited Months	1.74	4.26	4.31	10.34	7.24	-4.58
<i>Num. Funds</i>	127	234		320	361	

Panel C.

	(1)	(2)	(3)	(4)
	Hedge Fund Rule		Dodd-Frank Act	
Post	0.24** (0.06)	0.15 (0.12)	0.01 (0.04)	0.11 (0.08)
No Flag	0.10 (0.06)		0.12 (0.08)	
Red Flag	0.69 (0.12)		0.28*** (0.10)	
Post * No Flag	-0.23*** (0.07)	-0.21* (0.11)	-0.18 (0.09)	-0.16 (0.13)
Post * Red Flag	-0.51** (0.22)	-0.55* (0.31)	-0.53*** (0.12)	-0.49*** (0.16)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char.	Fund
Observations	722	722	1,356	1,356
Adjusted R-squared	0.07	0.17	0.08	0.11
F-test:				
Post*No Fund v. Post * Red Flag	<1%	<6%	<2%	<8%

Figure 1A. Timeline of Two-Period Event Windows and Dates. This figure shows the event windows and dates that correspond to the two-period event window. Each window contains 30 months.

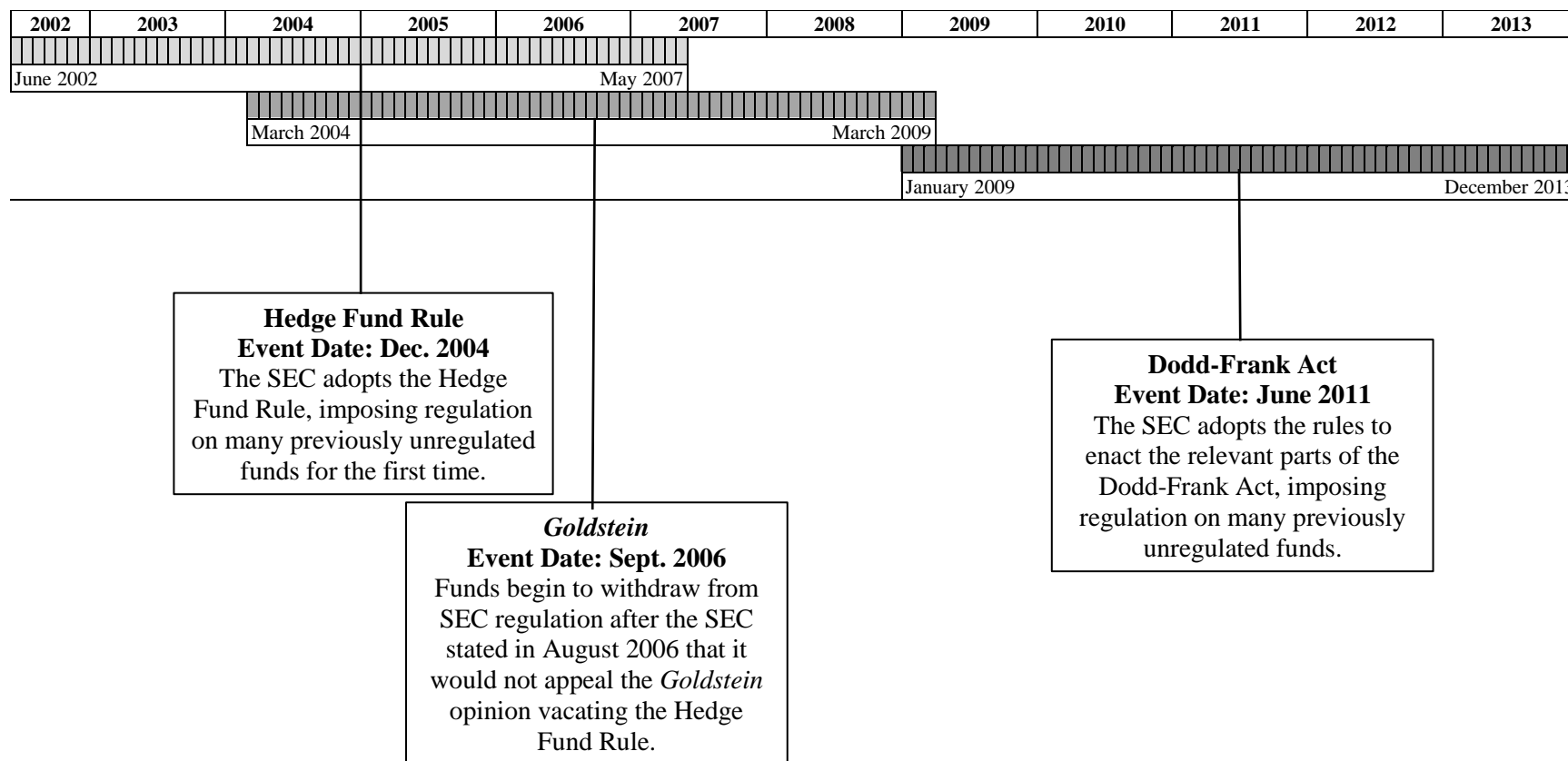
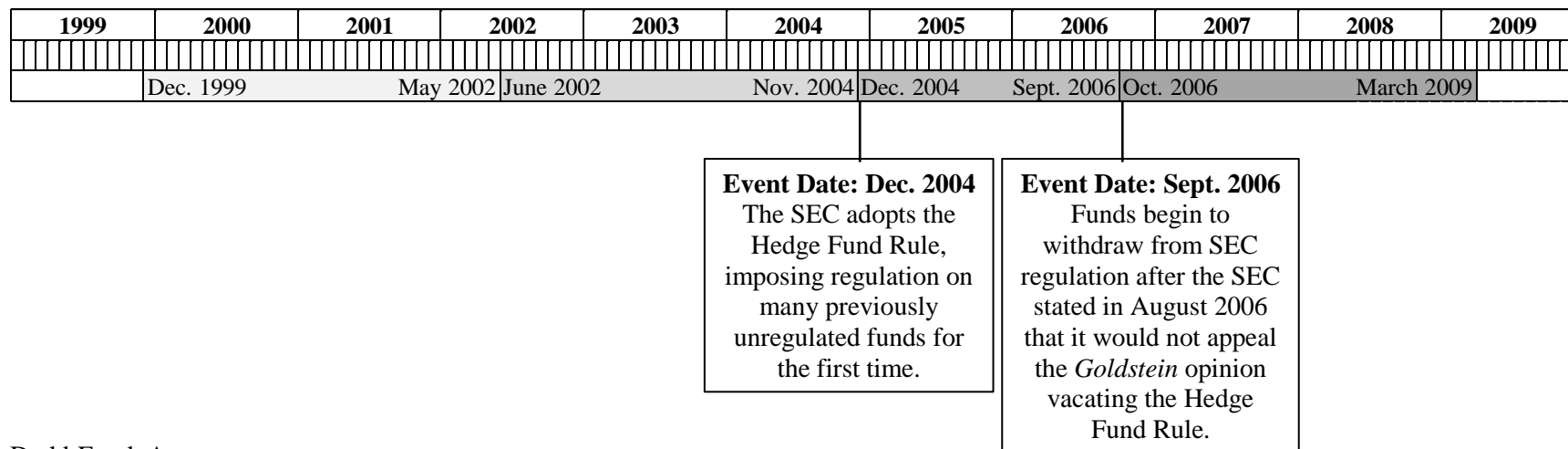


Figure 1B. Timeline of Four-Period Event Windows and Dates. This figure shows the event windows and dates that correspond to the four-period event window. Each window contains 30 months, except that from when the SEC adopted the Hedge Fund Rule to when SEC declined to appeal *Goldstein* (22 months).

Hedge Fund Rule & *Goldstein*



Dodd-Frank Act

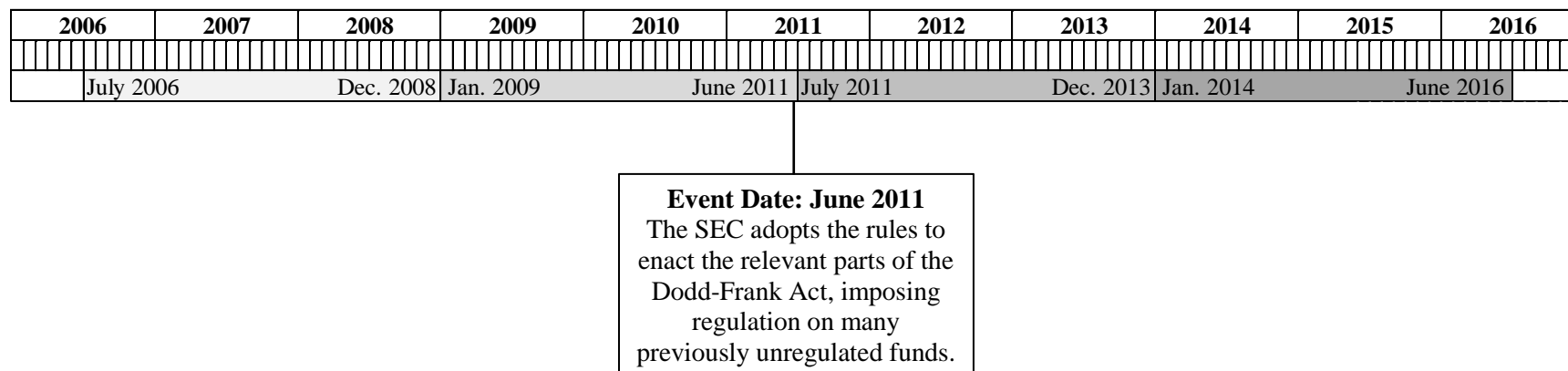


Figure 2. Measure of Misreporting: Kink. This figure describes the distribution of monthly hedge fund returns and indicates that, relative to the surrounding bins, there is a significant spike in the frequency of fund returns reported in the bin just above zero. The figure is based on all funds in the Lipper Hedge Fund database from 2000 to 2013. The bin width of 13 basis points is set according to the optimal bin width formula in Silverman (1986).

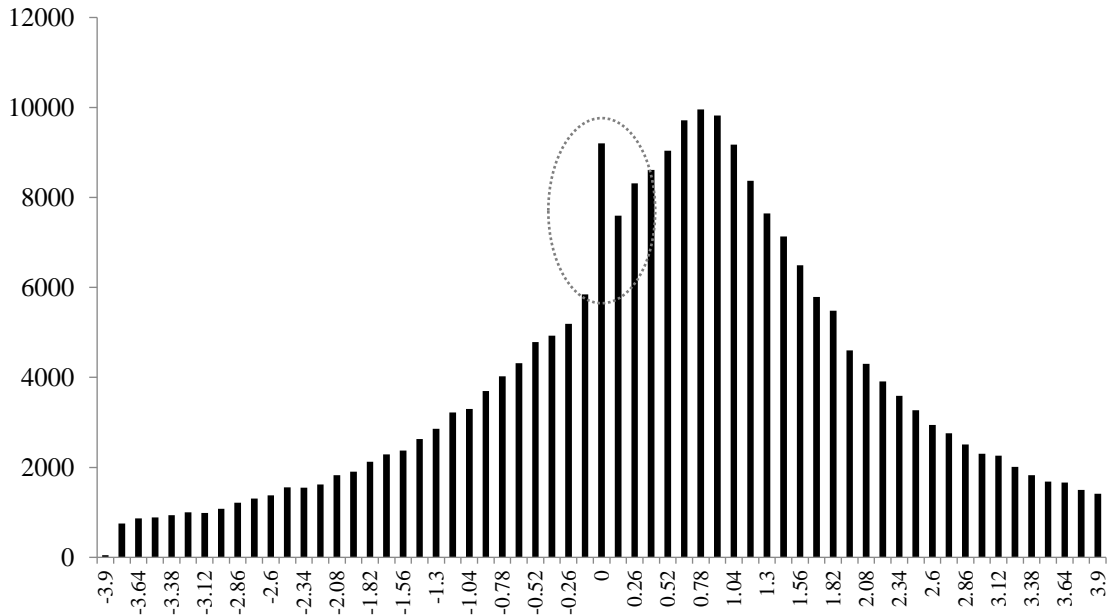


Figure 3. Measure of Misreporting: Cookie Jar Accounting. This figure describes mean hedge fund returns in December and non-December months, and indicates that mean fund returns in December were higher than mean fund returns in other months in ten of the thirteen years from 2000 to 2013. The figure is based on all funds in the Lipper Hedge Fund database.

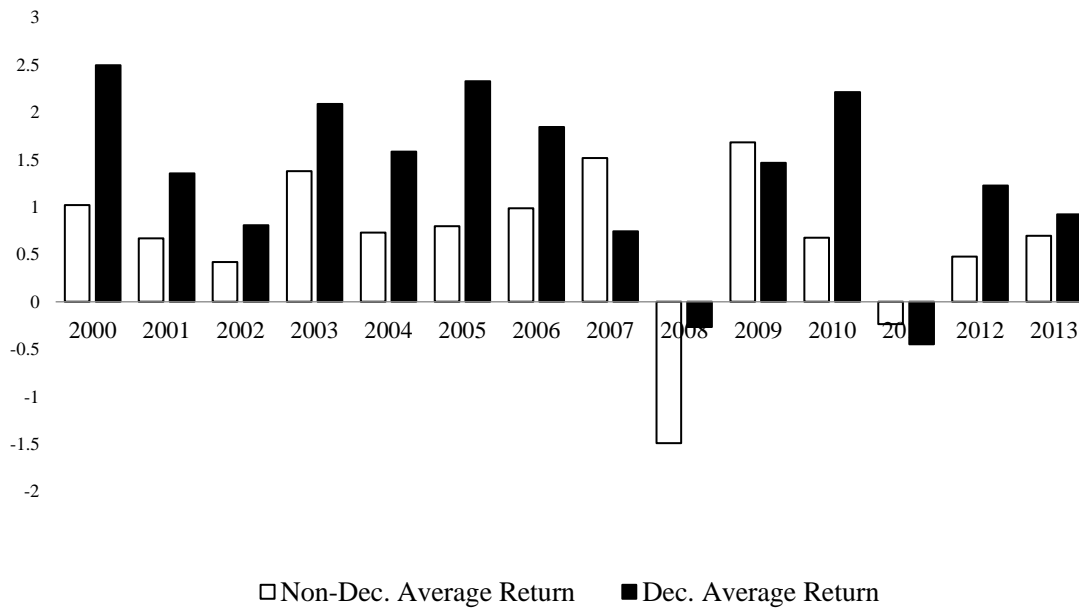


Figure 4. Domicile Country for Disclosure-Only Funds. This figure shows the jurisdictions in which the Disclosure-Only funds are domiciled and the number of Disclosure-Only funds per jurisdiction.

