The Surprising Benefits of Mandatory Hedge Fund Disclosure

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Comments welcome

Abstract:

Policymakers have long debated whether regulation would reduce hedge funds' financial misreporting. The traditional argument against regulation is that hedge funds are unlikely to misreport because their investors are highly sophisticated financial players who can detect and deter financial misconduct. However, recent changes in the composition of hedge funds' investors have led many to question this argument. In this paper, I use a quasi-natural experiment—in which a subset of hedge funds was regulated, deregulated, and then regulated again—to test whether hedge fund regulation reduces misreporting. Unique features of the setting permit me to study not only *whether* hedge fund regulation reduces financial misreporting—but, if so, *why*. The results show that regulation reduces misreporting at hedge funds and that the imposition of disclosure requirements, even without other concurrent changes in regulation (e.g., increases in enforcement), can reduce hedge funds' misreporting. This finding seems surprising, because hedge funds' investors are commonly thought to have access to far more information than is required by disclosure rules. Further analyses suggest that the disclosure requirements led funds to make changes in their internal governance, and that such changes induced funds to report their financial performance more accurately.

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

1. Introduction

Hedge fund regulation has long been a controversial topic. Hedge funds' investors are commonly considered to be highly sophisticated financial players, leading many policymakers to argue that hedge fund regulation is unnecessary because these investors will detect and deter financial misconduct without government assistance. The changing nature of the hedge fund industry has caused many to question this argument, however (CBS, 2004). Hedge funds now oversee more than $3 trillion in capital—a sum that has caused regulators to express concern that hedge funds could affect overall financial stability (SEC Report, 2003)—and the majority of hedge funds' investors are now institutional investors, such as pensions and universities, rather than individual investors. Indeed, individual investors only account for an estimated 3.6% of total capital (MFA, 2014).

In contrast to an individual who invests her own capital, institutional investors are often thought to suffer from the "double agency problem" that results when an institution (the "investor") invests on behalf of an individual (the "client") (Karantininis and Nilsson, 2011). According to this theory, institutional investors, such as the pension funds and universities that invest in hedge funds, may not be incentivized to fully detect and deter wrongdoing because the separation of client and investor creates an agency problem that is similar to the agency problem between a firm's managers and its owners. Although both the investor and the client presumably want to avoid fraud, they have different incentive structures that may lead them to place different relative values on, for example, investment in a high risk/high-reward fund. These differing incentives can be exacerbated if the investor needs to meet a performance target.

For these reasons, the question of whether regulation reduces misreporting at hedge funds is not obvious and requires a careful empirical examination. To this end, I exploit a series of legal

changes that took place in response to the developments in the hedge fund industry. In particular, since 2004, three significant changes caused a subset of hedge funds to be regulated, deregulated, and regulated once again. As often the case with public companies, a fund that becomes regulated will be subject to a number of concurrent changes—specifically, regulated funds becomes subject to government inspections, compliance requirements, and mandatory public disclosure. However, a striking feature of hedge fund regulation is that there are different tiers of regulation, and funds regulated under different tiers become subject to different types of regulation. For example, one tier of funds is only subject to mandatory disclosure rules, whereas another must comply with disclosure, government inspections, and compliance requirements. From a research perspective, this setting is unique in two ways. First, it allows for stronger inferences on the question of *whether* hedge fund regulation reduces misreporting, because it relies on three events through which SEC oversight is first imposed, then removed, and then imposed again. Second, and more importantly, it allows for an examination of why and *how* hedge fund regulation reduces misreporting, because different events triggered different types of regulation at different funds.

My study points to three key findings. First, hedge fund regulation reduces misreporting. Across all three legal changes, misreporting decreased upon regulation and increased upon deregulation. Second, the decrease in misreporting was driven by mandatory disclosure requirements. When I group the funds by the type of regulation to which they became subject, I find that misreporting decreased at the groups of funds that became subject to disclosure rules. Notably, misreporting decreased even at the subset of funds *only* subject to disclosure requirements—indicating that disclosure, even without concurrent changes, can have a significant effect. Finally, to understand why mandatory disclosure would decrease misreporting, I contacted personnel at the funds in my sample and ran additional empirical tests based on their feedback.

The evidence indicates that mandatory public disclosure of governance information, such as whether or not the fund is audited or employs a compliance officer, spurred internal governance changes that induced more accurate reporting.

All tests are difference-in-differences regressions that compare the hedge funds affected by the regulatory changes to a control group of unaffected funds (i.e., funds already regulated). I further include a battery of robustness tests to address sample selection concerns. To identify misreporting at hedge funds, I follow prior literature and examine three 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—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. Third, I rely on literature showing that deviations from Benford's Law can be used to predict misreporting (e.g., Amiram et al., 2015) and test whether the fund returns conform to Benford's Law.

My paper contributes to several areas of literature. First, I contribute to the literature on mandatory disclosure—and, in particular, to the literature on the first-time effects of mandatory disclosure. The studies examining the first-time effects of US securities disclosures (e.g., Benston, 1969; 1973; Daines and Jones, 2012) are necessarily limited because disclosure for US public corporations has been federally mandated since the 1930s. As a result, most studies on US firms are forced to examine the effects of mandating *additional* disclosures on firms that are already subject to (often extensive) mandatory disclosure. As noted by Leuz and Wysocki (2008), in these settings, the marginal effect of incremental disclosures is likely to be small, making it difficult to assess the economic magnitude associated with disclosure requirements. By focusing on hedge

funds, I have a unique setting that allows for study on the first-time effects of mandatory securities disclosure in a recent period.[2]

Second, by disentangling the effects of different regulatory components, I contribute to the literature on regulation—in particular, hedge fund regulation. A limited number of prior studies, generally using association designs, have provided evidence that regulation decreases misreporting at hedge funds (Hoffman, 2013; Cumming and Dai, 2010; Dimmock and Gerken, 2015). However, these papers have not attempted to assess the relative effects of different types of regulation. Although it is difficult to isolate different regulatory effects because there are usually multiple changes at once (e.g., Christensen et al., 2013; Barth and Israeli, 2013),[3] my setting allows me to disentangle these factors. My study is therefore focused on *why* regulation is effective and identifies mandatory disclosure—and the ensuing internal governance changes—as the key mechanism.

Finally, I contribute to the literature on the real effects of disclosure. Recent work has called for further analysis on the real effects of disclosure, where real effects are defined as changes made by the reporting entity as a result of the disclosure requirements (Leuz and Wysocki, 2016). To better understand how the disclosure requirements affected the funds, I contacted personnel at the funds in my sample to discuss their experiences with the regulatory process. Based on their responses, which indicated that the funds made internal governance changes, I ran additional

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[2] To study first-time disclosure in a more recent period, some papers have examined OTC firms (e.g., Ferrell, 2003; Bushee and Leuz, 2005; Greenstone, Oyer and Vissing-Jorgensen, 2006). However, these papers do not study the effect of disclosure on misreporting. Additionally, most of these papers examine the effects of the Securities Act Amendments of 1964. Although the imposition of disclosure requirements was the most significant change associated with these Amendments, there were multiple concurrent regulatory changes.

[3] Although some prior literature has examined the relative effects of different types of regulation on public companies, I am not aware of any empirical study of the different regulatory components at investment funds. The need for empirical work in this area is striking because regulators have long questioned whether lessons from public companies can be applied to investment funds (Greenspan, 1998).

empirical tests and provide evidence that these governance changes—and the initiation of audit procedures in particular—decreased misreporting. This finding speaks to long-standing research in accounting on the value of auditing (Watts and Zimmerman, 1983; 1986).

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 showing that hedge fund regulation reduces misreporting. Section 6 examines *why* such regulation reduces misreporting. Section 7 presents the robustness tests, and Section 8 concludes the paper.

2. Institutional Background

A. Setting

The regime regulating hedge funds is distinct from that for public companies and has been recently subject to a series of important changes.[4] Between 2004 and 2010, groups of hedge funds were, ***first,*** made subject to significant regulation by the SEC; ***second,*** relieved by the courts of these regulatory obligations; and ***third,*** again made subject to regulation, this time by Congress. 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 and took actions that set off a series of regulatory changes.

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[4] 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 (recently updated by the Jumpstart Our Business Startups Act).

The first change occurred in 2004, when the SEC proposed to subject the vast majority of unregulated hedge funds to regulation for the first time (HF Rule, 2004).[5] 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 ("IAA").[6] The SEC adopted the Hedge Fund Rule in December 2004, and the newly regulated funds were required to submit to the SEC's authority by February 1st, 2006. The rule was highly controversial and had a widespread effect—indeed, it was estimated that fewer than half of hedge funds were regulated by the SEC before the adoption of the Hedge Fund Rule (CBS, 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 and vacated the Hedge Fund Rule. In August 2006, the SEC stated that it would not appeal the DC Circuit's decision, making clear that the funds subjected to SEC regulation by the Hedge Fund Rule would be allowed to withdraw from such regulation (Cox, 2006).

 3. The Dodd-Frank Act. Congress responded in the Dodd-Frank Act ("DFA"). Although the DFA included a complex series of provisions in this area, for present purposes I only highlight

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[5] It is important to note that there are safeguards in place to prevent even unregulated funds from defrauding their clients. Both regulated and unregulated funds are subject to antifraud rules, meaning that unregulated funds can be inspected—and regulators can bring enforcement actions—if there is reason to believe the fund is committing fraud. Moreover, private investors and their representatives can bring private litigation against unregulated fund managers for wrongdoing.

[6] At the time this rule was proposed, Section 203(b)(3) of the IAA 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.

two changes. First, the DFA mandated that most hedge funds would again be subject to regulation (DFA Rule, 2011). This change required many unregulated funds—including those that had previously withdrawn from regulation after the *Goldstein* decision—to submit to regulation. Second, the DFA created different tiers of regulation, where funds that became regulated under different tiers became subject to different types of regulation. I describe which funds became subject to regulation under each tier in more detail in Section 6.

B. Components of Hedge Fund Regulation

As a technical matter, when I say that a fund becomes "regulated," it means that the fund's advisor must register with the proper authority—an act that subjects the entity to regulatory requirements.[7] Below I describe the three major components of SEC fund regulation: mandatory disclosure rules, compliance requirements, and government inspections.

1. Mandatory disclosure. Regulated investment advisors in the US 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 and compliance matters, including the firm's clients, accounting practices, potential conflicts of interest, and prior disciplinary history. Notably, the information in Form ADV relates to the firm's governance and compliance—not its financial performance.

Although Form ADV has received limited attention from academics, the disclosure gives investors important information about their advisors. For example, 21% of the advisors in my dataset disclosed a crime or regulatory infraction. Another 28% disclosed that they are not audited at least annually by an independent public accountant. And 14% disclosed that the advisor engages

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[7] Although regulation is applied at the investment advisor level, I use the term funds for ease of exposition.

in transactions that cause a significant potential conflict of interest (defined as funds that participate in either principal transactions or agency cross trades).[8]

2. 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 a period of 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 to either produce audited financials or to have at least one surprise audit each year.

3. Government inspections. Finally, upon regulation, advisors are generally subject to compliance examinations, which involve detailed inspections of the fund and its managers by government officials. These inspections, which have been suggested to reduce fraud (HF Rule, 2004), vary substantially in scope, ranging from simple records requests to onsite exams lasting for several weeks. The exams are generally focused on whether the advisor has fulfilled the compliance requirements described above, such as record-keeping and proper client communication. The exams may peripherally address the accuracy of the advisor's Form ADV disclosure, but this is not their primary focus. 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). However, some examinations reveal unlawful acts that lead to enforcement actions (CBS, 2004).

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[8] 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 DFA, 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.

As I describe in Section 6, following the DFA, some unregulated funds became subject to all three of these regulatory components, but others did not. In particular, one group of funds was subjected to only the disclosure requirements discussed above and another to only government inspections.

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. Below I provide details on both sources.

1. Form ADV. As noted above, Form ADV is the only publicly available mandatory filing for most hedge funds and contains important information. Dimmock and Gerken (2012), for example, find that investors who avoid the 5% of firms with the highest ex ante fraud risk based on Form ADV disclosures can avoid over 40% of the dollar losses due to fraud. And Brown et al. (2008, 2009, 2012) provides evidence that the information in Form ADV filings enables investors to identify managers who have better performance in future periods.

Despite its usefulness, Form ADV has received relatively little attention in academic literature—likely because the SEC has denied all FOIA requests for the information, making historical Form ADV data generally unavailable to academic researchers.[9] Moreover, because some advisors are regulated by states and others by the SEC, FOIA requests must be filed

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[9] Only the current versions of Form ADV are available online, and the SEC has traditionally denied all FOIA requests for historical information (although the SEC purports to provide some historical Form ADV data online, the information is notoriously low-quality and not suitable for academic research). 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. 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.

separately at each regulator. To obtain the data for this project, I filed FOIA requests with the SEC and sixteen state securities agencies.[10] 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, notably monthly returns and net asset values, from the Thomson Reuters Lipper Hedge Fund database (also known as the Trading Advisor Selection System ("TASS") database). This database 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. As such, to combine these databases, I performed a one-to-many merge.

B. Research Design

For each of the three regulatory changes described earlier, I employ a "difference-in differences" design. That is: I examine the change in misreporting for the funds affected by the change in the law (the "treatment" group) relative to the change in misreporting for the funds that were *not* affected by that change in the law (the "control" group). This approach requires me to (i) identify the event date (the date of the change in regulation), (ii) determine the period of comparison before and after such event (the "pre" and "post" periods), and (iii) define the treatment and control samples. This information is described below and summarized in the Appendix.

1. Event date. For the Hedge Fund Rule and the DFA, I use the month the SEC adopted the final rule in question as the event date. These months are December 2004 for the Hedge Fund

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[10] 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. Overall, I received Form ADV filings for all SEC registrants from 2001-2014 and some Form ADV data for state registrants from 2006-2014. I thank Robert Jackson for his very extensive help with the process.

Rule and June 2011 for the DFA, respectively. Notably, the funds were not required to register with the SEC immediately after the final rules were adopted. The registration process is cumbersome, and the SEC provided funds with time to prepare. 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 DFA. However, I select the month of rule adoption because the funds knew at this point that they would be regulated, and anecdotal evidence indicates that they made significant changes during the preparatory period. For example, if a fund manager didn't want to publicly disclose that his fund was not audited, he could hire an auditor during the period between rule adoption and official registration. Moreover, because SEC inspectors are allowed to demand records from prior to the registration date, funds could feasibly expect that the period following rule adoption and before official registration would be subject to examination.

For the Court's opinion in *Goldstein* vacating the Hedge Fund Rule, I use September 2006 as the event date. In August 2006, the SEC stated that it would not appeal the *Goldstein* decision and allowed funds to withdraw without penalty through January 2007. I select September as the event date because it is the first month in which any of the funds in my sample actually withdrew (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).

2. "Pre" and "Post" periods. As in Dimmock and Gerken (2015), I examine the 30-month period before and after each event. This lengthy period is necessary to have sufficient observations for some of the regression-based measures of misreporting (discussed later). All funds lacking 30 months of data in both periods are omitted from the analysis so that the sample will not be biased due to attrition.

3. Treatment and control samples. For the Hedge Fund Rule and the DFA, I identify the treatment and control samples as follows. First, conceptually, the treatment funds are those that would not have been regulated but for the Hedge Fund Rule or DFA. Because of the time required to prepare for registration, the funds which registered shortly after the Hedge Fund Rule or the DFA rules were adopted are likely voluntary registrants that began the registration process *before* the event of interest. To avoid their confounding effect, and to ensure that I focus only on funds that became regulated because of the change in law, 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 (most funds indeed registered shortly before the deadline).[11]

Second, conceptually, the control funds are those that were not affected by the regulation because they were already regulated by the SEC. As such, I identify the control funds as those continually subject to SEC regulation during the entire event period (i.e., the thirty months before and after the relevant change in law). Note that the identification of the treatment and control samples requires that a fund can be categorized differently for the different events. For example, if a fund became subject to regulation by the Hedge Fund Rule and remained regulated thereafter, it would be included as a treatment fund in the analysis of the Hedge Fund Rule and a control fund in the analysis of the DFA.

Identification of the treatment funds in the *Goldstein* analysis is more nuanced because there are 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"). I identify the Remain funds as those that became regulated by the Hedge Fund Rule and remained

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[11] As noted earlier, funds were required to submit to SEC oversight by February 1st, 2006 for the Hedge Fund Rule and by March 31st, 2012 for the DFA, so the period used to identify the treatment sample ranges from August 2005–January 2006 for the Hedge Fund Rule and from October 2011–March 2012 for the DFA. See Appendix for details.

regulated through the end of the sample period, and the Withdraw funds as 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).

Finally, funds that do not meet the criteria of either the treatment or control sample are dropped from the analysis. For example, a fund that first registered with the SEC in October 2004 (two months before the Hedge Fund Rule was adopted) would be omitted. This fund would not be considered a control fund because it wasn't regulated throughout the entire pre period, and it would not be classified as a treatment fund because it did not become regulated due to the Hedge Fund Rule.

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 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). 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. Instead, academic studies estimate misreporting by identifying suspicious patterns in the monthly performance returns that hedge funds report to their investors. This approach captures manipulation of the underlying assets because monthly performance returns and fund assets are inherently linked: the fund returns are based on the monthly change in net assets,

before inflows or outflows from investors and after fees, so manipulation of the underlying assets will manifest in the monthly returns. As described below, I follow prior studies and identify misreporting using three suspicious patterns in reported monthly returns.

A. Kink at Zero

The first measure captures whether a fund appears to misreport its monthly returns to avoid reporting a loss—in particular, by reporting fewer monthly returns just below zero than would be expected based on the fund's 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. Fund managers, however, have strong incentives to avoid reporting losses, so they may manage monthly reported returns so as to turn small losses into small gains. Prior empirical work has shown that earnings management of this type is associated with fraud. Bollen and Pool (2012), for example, found that the size of a fund's kink at zero is the strongest predictor of detected accounting fraud at hedge funds.

Figure 1 provides the distribution of monthly returns for all funds in the Lipper Hedge Fund database from 2000-2013 and shows that, consistent with this intuition, there is a "kink" in the distribution of fund returns at zero. That is, there are more than expected gains just above zero. The bin width of 13 basis points in Figure 1 is set according to the optimal bin width formula described in Silverman (1986).

Although Figure 1 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. When bad times arise, this practice allows the manager to reach into the "cookie jar" to inflate her reported results.

Prior literature has suggested that one way to test for "cookie jar" accounting at hedge funds is to consider whether the fund reports abnormally high returns in December (Agarwal et al., 2011). The idea is that funds accumulate reserves in the "cookie jar" throughout the year, and if bad times never arise, managers will recognize excess returns remaining in the "cookie jar" in December 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 2 shows the average returns for all hedge funds, both in the month of December and in non-December months, in all years from 2000-2013. The figure shows that average returns in December are higher than average returns for other months in 10 out of the 13 years, suggesting that some hedge funds indeed use "cookie jar" accounting. Notably, the years in which December returns are lower—2007, 2009, and 2011—are years in

which "cookie jar" accounting may not have been an option because of the financial crisis and its aftershocks.

To test for "cookie jar" accounting at each individual fund, 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 in order to control for general economic factors that may affect hedge 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.

C. Benford's Law

Finally, my third measure of misreporting examines whether the distribution of each fund's monthly returns conforms to Benford's Law. Benford's Law states that, when many distributions are aggregated together, the first digits in the resulting distribution will follow a specific logarithmic curve (Benford, 1938). Benford's Law specifically predicts that the first digit of monthly returns reported by hedge funds should be a one 30.1% of the time, a two 17.6% of the time, and so on, with other digits appearing less frequently until nine, which should only appear as the first digit in 4.6% of all observations.[12] The intuition for the law is that the percentage change needed to "roll over" (i.e., to increase the first digit) of a fund's monthly return is greater for lower numbers. For example, suppose a fund wants to grow its profits from one million dollars to two million dollars. To do so, it must double its profits. But the same fund can increase its profits from eight million dollars to nine million dollars by increasing its profits by just 12.5%. Because of

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[12] The formula for Benford's Law is $P(d)=\log_{10}(1+(1/d))$, where d is the first digit and ranges from one to nine. Thus, the expected distribution for digits one through nine is 30.1%, 17.6%, 12.5%, 9.7%, 7.9%, 6.7%, 5.8%, 5.1%, and 4.6%, respectively. As a general rule, Benford's Law will apply when the distribution of the base-10 log values of the original dataset is smooth, symmetrical, and spread across several orders of magnitude in the log scale (Amiram et al. 2015). To confirm that hedge fund returns will conform to Benford's Law, I graphed the distribution of the base-10 log monthly returns. The distribution is approximately normally distributed and ranges across several orders of magnitude, meaning that the distribution will largely conform to Benford's Law.

these differences in the percentage change necessary to "roll over" from lower digits to higher ones, the typical fund will spend more time with profits between one and two million dollars than with profits between eight and nine million dollars. Thus, more observations of the fund's profits should begin with one or two than with eight or nine.

Benford's Law is widely used to detect financial fraud. Amiram et al. (2015), for example, show that deviations from Benford's Law can be used to predict material misstatements at public companies, and Nigrini (1996; 2012) finds that Benford's Law can be used to detect errors in tax and internal financial reporting. Barring unusual events, such as September 11, 2001, stock returns also conform to Benford's Law (Corazza et al., 2010). In the hedge fund context, in unreported tests I found that 38% of funds in the Lipper Hedge Fund database that the SEC identified as fraudulent deviated from Benford's Law over the final 30 months of their reporting life. By contrast, only 16% of the general population of funds in the database deviated over the same period—a significant difference.

At a summary level, Figure 3 describes (1) the distribution of the first digit of monthly returns predicted by Benford's Law; (2) the actual distribution for the funds in the Lipper Hedge Fund database from 2000-2013; and (3) the actual distribution for Bernie Madoff's infamous investment fund (compiled using a set of returns from Blodget (2008)). Figure 3 suggests that the vast majority of fund returns conform to Benford's Law. For example, Benford predicts that 30.1% of fund returns will start with a one (reflected by the white bar), and the actual distribution shows that 32.3% start with a one (reflected by the grey bar). By contrast, the figure shows a whopping 39.6% of Madoff's returns started with a one—a significant deviation.[13]

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[13] Figure 3 includes negative returns. The first digit of a return of -.012%, for example, would be recorded as one.

To test for deviations from Benford's Law at each individual fund, I rely on the Kolmogorov-Smirnov ("KS") statistic. The KS test is a nonparametric test that relies on the maximum deviation from the expected distribution, where the maximum deviation is determined by the cumulative deviation at each digit (see Amiram et al., 2015 for a discussion of this issue). I consider a fund to have deviated from Benford's Law if the maximum deviation is significant at the 10% level, where the critical value for the 10% threshold is calculated as $1.22\sqrt{N}$.

5. The Effect of Hedge Fund Regulation on Misreporting

My first set of analyses examine whether hedge fund regulation reduced misreporting and provide evidence that it did. This section presents the descriptive statistics and regression results for this inquiry.

A. Descriptive Statistics

1. Comparison of treatment and control funds. There were more control funds than treatment funds in all analyses. The specific numbers are as follows: (1) Hedge Fund Rule – 228 control funds and 124 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;[14] (3) DFA – 552 control funds and 222 treatment funds.[15]

Table 1 describes the fund characteristics. The table shows each fund's mean monthly return, mean log of net asset value, and mean age over the thirty months prior to the event date. I

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[14] The total number of observations in the analyses of the Hedge Fund Rule and *Goldstein* differs slightly because of data availability in the Lipper Hedge Fund database.

[15] Of these 222 treatment funds, 108 were newly subject to full regulation (the others were only subject to partial regulation). Out of an abundance of caution, the DFA analyses in Tables 3 and 5 include only funds subject to full regulation and omit all funds that withdrew from SEC regulation after *Goldstein* to ensure that the initial set of tests use comparable funds across all three settings. I note, however, that the inclusion of these funds does not materially change the findings.

also include the fund's return volatility over the period, whether the fund is incorporated in the US, and the sensitivity of the fund to market liquidity. 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 my 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, more likely to be foreign, have better performance, and have greater return volatility. It is for this reason that, when possible, 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 the following two 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). Treatment funds without a match along these two criteria are dropped. If a fund has multiple potential matches along these criteria, I next match on investment style (e.g., long-equity funds will be matched). If a fund still has multiple potential matches, I 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, and sensitivity to liquidity (as defined in Table 1). Following these criteria, there are a total of 109 funds in each group in the analysis of the Hedge

Fund Rule, 51 funds in each group in the analysis the *Goldstein* opinion,[16] and 101 funds in each group in the analysis of the DFA.

2. Frequency of misreporting. As explained previously, drawing on previous literature, I use three measures as proxies for misreporting. If a fund triggers any one of these three measures, I consider it a "flag" for misreporting.[17] Table 2 presents the frequency of "flags" at the treatment and control funds before and after regulation. Panel A presents the average number of flags 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. For example, 14% of funds that became regulated in response to the DFA 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 (9% and 8% for the control and treatment funds, respectively).

B. Regression Results

Below I present the results of the difference-in-differences multivariate tests. Across all tests, the dependent variable is the number of flags triggered.[18] The results are presented first using

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[16] As noted below, 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.

[17] I treat misreporting as binary and record only *whether* the fund deviated from the expected distribution in the predicted direction of misreporting—not the severity of the deviation. I follow this approach because not all deviations are equal. For example, if a fund has a significant positive kink above zero, I would consider that misreporting. However, if a firm 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.

[18] Although Tables 3-5 aggregate all proxies, the results by individual proxy are presented in Table 10.

the full sample of funds and then using the matched sample. All tests control for the variables noted in Table 1, averaged over the period in question, and standard errors are clustered by fund.

1. Imposition of federal regulation. To compare the change in misreporting at the treatment funds relative to the change in misreporting at the control funds after the two events imposing mandatory regulation (Hedge Fund Rule in 2004 and DFA in 2011), I use the following equation:

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

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.

As shown in Table 3, the interaction term is negative and statistically significant in all models, indicating that misreporting decreased for the newly regulated funds. Although I present the results using ordinal logit models, I run unreported OLS models to better capture the economic magnitude of the effect. The OLS models show that, following the adoption of the Hedge Fund Rule, the mean fund subjected to regulation triggered roughly 0.25 to 0.40 fewer flags than would have been expected based on the control sample (estimates vary across the models). The economic magnitude for the DFA is similar or slightly larger.[19]

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[19] Anecdotal evidence suggests that some funds did not register with the SEC following the Hedge Fund Rule because they were waiting on the outcome of the *Goldstein* case. Hence, the estimated magnitude of the decrease in misreporting following the Hedge Fund Rule may slightly underestimate the actual decrease because the tests omit some funds that, presumably, would have been greatly affected by regulation. Consistent with this possibility, the

To summarize, across both regulatory events, this evidence suggests that the imposition of federal regulation reduced misreporting at hedge funds. For further evidence, I now turn to DC Circuit's decision in *Goldstein*, which vacated the Hedge Fund Rule and thus provides an opportunity to examine the relation between regulation and misreporting in a setting where regulatory oversight is reduced rather than imposed.

2. Removal of federal regulation. As noted earlier, after the *Goldstein* opinion vacated the Hedge Fund Rule, funds that had been regulated by that rule were given a choice; they could voluntarily remain subject to SEC regulation or elect to opt out. To examine whether these funds experienced a change in misreporting relative to control funds, I examine the following equation:

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

In this equation, Withdraw is to 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. As stated previously, I consider a newly regulated fund to have withdrawn if the fund withdrew after the SEC announced, in August 2006, that it would not appeal the *Goldstein* case and on or before January 31, 2007—the deadline to withdraw without penalty after *Goldstein*. The Post variable is set to zero for the thirty months leading to September 2006— the first month in which any of the funds in my sample withdrew from SEC regulation—and to one in the thirty months after September 2006. 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*

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magnitude of the decrease in misreporting appears to be slightly greater for the DFA than for the Hedge Fund Rule when using the full sample (although the estimates are similar using the matched sample).

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.

Table 4 shows 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 (statistical significance is lower than in the prior two tables, but the treatment group has been partitioned into two groups, thereby reducing statistical power). By contrast, the funds that remained subject to federal regulation did not, after *Goldstein*, experience a significant change in misreporting relative to the control funds.[20] This suggests that the initial decrease in misreporting was sticky—after regulation, misreporting at the newly regulated funds remained at the relatively lower level. Taken together with the evidence on the effects of the Hedge Fund Rule and the DFA, these results provide strong evidence that hedge fund regulation reduced misreporting.

6. Disentangling the Effect of Different Regulatory Components

Having provided evidence that mandatory regulation reduced hedge fund misreporting, I now turn to a separate question: *why* did regulation reduce misreporting? As noted earlier, the imposition of federal regulation subjects funds to multiple regulatory elements. The decrease in misreporting could be caused by any one of these elements individually or by their interaction.

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[20] F-tests indicate that the difference between these two interaction terms is directionally consistent with expectations but not statistically significant at standard levels, likely due to the limited power associated with comparing two small subsets of funds.

A. Background

Recall that, as noted in Section 2, the DFA made significant changes to federal law on hedge fund regulation. As described below, these changes effectively created three groups of hedge funds by subjecting different funds to different types of regulation.

- *Full Regulation.* First, as noted previously, the DFA subjected the majority of unregulated funds to full regulation—these were the funds studied in Table 3 above. These funds, known as Registered Investment Advisers ("RIAs"), became subject to mandatory disclosure rules, government inspections, and compliance requirements. Unless a fund is eligible for one of the categories below, it will fall into this group by default.

- *Disclosure-only.* Second, in an effort to reduce the costs associated with hedge fund regulation, a subset of funds was exempted from the vast majority of regulatory requirements—these funds only became subject to mandatory public disclosure (i.e., they had to file Form ADV). These firms, known as Exempt Reporting Advisers ("ERAs"), were eligible for exempt status because they advised only venture capital funds or only private funds (i.e., hedge funds) with less than $150 million of US assets.[21] Because of the asset limitation, most ERAs are foreign. Although US advisors must advise funds with less than $150 million in total assets—a relatively small sum for an advisory firm—foreign advisors are eligible for ERA status if they advise funds with less than $150 million in assets from US investors.

- *Inspection-only.* Third, the DFA subjected a group of funds to SEC inspections. These funds were already complying with disclosure rules and compliance requirements—but were not yet subject to SEC inspections. The funds in this third group were regulated by a state prior to the

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[21] 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 the compliance requirements of the IAA, and the SEC exempted these funds from its inspection program.

DFA, but were required to switch to SEC regulation because of a requirement in the DFA that, subject to limited exceptions, advisors with more than $100 million in assets were required to be regulated by the SEC rather than a state.[22]

The outcome of these changes was that the DFA created three groups of hedge funds, all subject to different changes in regulation, allowing me to disentangle the different regulatory components. However, I note the caveat that funds were not randomly selected for each group. On average, relative to the funds subject to full regulation, the disclosure-only funds were smaller, more likely to be foreign, had lower returns, and had less return volatility. And the inspection-only funds were younger, more likely to be incorporated in the US, and had higher returns than the funds subject to full regulation. I address these sample selection concerns in the robustness tests in Section 7.

B. Isolating the Regulatory Components

To disentangle the different components of regulation, I partition the funds that became subject to federal oversight following the DFA into the three groups previously described. I then conduct difference-in-differences tests comparing the change in misreporting at each of the three groups relative to the control funds. The time period, control funds, and control variables are the same as those used for the DFA tests in Table 3. As before, standard errors are clustered by fund.

$$Num.\ Flags = \alpha + \beta_1 Post + \beta_2\ Full\ Reg. + \beta_3\ Disc.\ Only + \beta_4\ Inspect.\ Only + \\ \beta_5 Post * Full\ Reg. + \beta_6 Post * Disc.\ Only + \beta_7 Post * Inspect.\ Only + Controls + \varepsilon$$

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[22] These funds could theoretically have been subject to state inspections before the DFA, but I consider them to be "inspection-only" funds because almost all the funds in my sample switched from New York—a state that did not conduct inspections—and therefore became subject to government inspections for the first time upon their switch to SEC regulation. Although the switch could activate differences between the federal and state compliance requirements, the differences between these regimes are minor.

The results, presented in Table 5, offer important insights about misreporting at these three groups of funds. First, if we consider misreporting in the 30 months before the DFA, we see that the level of misreporting at the two groups of funds that were not subject to federal disclosure rules was statistically greater than the control funds and statistically comparable to one another (unreported F-tests indicate that misreporting at the full-regulation and disclosure-only funds was not statistically significantly different from one another). By contrast, the level of misreporting at the third group of funds—inspection-only funds—was not statistically different from misreporting at the control funds.

Second, as for the more intriguing question of how misreporting changed *after* the DFA, Table 5 provides evidence that mandatory disclosure, even on its own, can reduce misreporting at hedge funds. The funds subject to *only* disclosure rules significantly decreased misreporting after the change in law, as did those subject to full regulation (F-tests comparing the coefficients from Table 5 show that decreases in misreporting for these two groups were not significantly different from each other). By contrast, the inspection-only funds exhibited no significant change in misreporting relative to the control funds.[23]

This result is perhaps surprising. Hedge fund investors are generally considered to be highly sophisticated, and Cassar et al. (2016) 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 unclear why public disclosure of this information would have a significant effect.

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[23] There are multiple explanations for this result. While possible that inspections do not deter misreporting, it is also possible that the analysis lacks power because I have only a limited sample of funds in this category. As such, I hesitate to draw any firm conclusions on the effect of inspections and instead focus on the effect of disclosure.

C. Disclosure Mechanism

To understand why these disclosure requirements 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 perspective. Second, I ran additional empirical tests to capture the anecdotal feedback.

1. Anecdotal evidence. My inquiries provided helpful anecdotal evidence outlining two possible mechanisms through which the requirement to file Form ADV—and therefore to provide public disclosure of governance information—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. The funds appeared to be more concerned with how prospective investors—rather than current investors—would view the information. Second, many respondents indicated that chief compliance officers enjoyed increased status upon SEC regulation. Some funds appointed compliance officers for the first time.[24] And funds that already had compliance officers were often thought to place a greater value on the officer's input following regulation.

2. Empirical analysis. 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

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[24] The funds that became subject to the full regulation were required to have a compliance officer. However, the disclosure-only firms were exempt from this requirement—they were only required to indirectly disclose whether they had such an officer.

available on funds' internal governance prior to regulation. The commercial databases are largely focused on funds' financial performance, not their governance.

However, the Lipper Hedge Fund database includes a field noting the last official audit date for each fund. Using historical information for this field, I identified the funds that initiated audit procedures following regulation and partitioned the newly regulated funds (all funds that became regulated following the Hedge Fund Rule and all three groups of funds in the DFA setting) into two groups: (1) those that initiated audit procedures ("Initiate") and, (2) those that did not ("No Change"). Using the equation below, I then test whether the funds that initiated audit procedures following regulation had greater decreases in misreporting than funds that did not. The control variables, control sample, and time period are the same as those used in Table 3. As before, standard errors are clustered by fund.

$$Num.\ Flags = \alpha + \beta_1 Post + \beta_2\ Initiate + \beta_3\ No\ Change + \beta_4\ Post * Initiate \\ + \beta_5 Post * No\ Change + Controls + \varepsilon$$

The results, shown in Table 6, provide some support for the notion that the decrease in misreporting following regulation was driven by changes in governance. Following the Hedge Fund Rule, the funds that initiated audit procedures triggered roughly 0.34 fewer flags than would have been expected based on the control funds over the same period.[25] By comparison, the funds that did not change their audit procedures did not have a significant change in misreporting relative to the control funds. The pattern is less pronounced following the DFA, when both groups experienced statistically significant relative decreases in misreporting after regulation. However, the magnitude of the decrease is higher for funds that initiate audits (although unreported F-tests show that the difference is not statistically significant).

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[25] Due to the difficulty of interpreting coefficients from ordinal logit models, this estimate is taken from an unreported OLS model that I ran to better estimate the economic magnitude of the decrease.

Despite obvious limitations, this analysis provides some empirical support for the anecdotal evidence that internal governance changes spurred by the disclosure requirements induced funds to report more accurately. Such findings are consistent with empirical findings on the effects of disclosure in other contexts. For example, Linck et al. (2009) showed that the requirement that public companies disclose whether a "financial expert" sits on the company's audit committee led to a doubling of the number of those experts on audit committees, and Chatterji and Toffel (2010) showed that public disclosure of firms' poor environmental records led those firms to subsequently improve their environmental performance.

7. Robustness

Despite the benefits of the setting, the research design raises some concerns (e.g., sample selection issues). The robustness tests below are designed to address these concerns as best possible.

A. Sample Selection

Although the evidence in Table 5 indicates that the funds subject to full regulation and those subject to only disclosure rules had statistically equivalent decreases in misreporting, the assignment of funds to these different types of regulation was not random. The assignments were primarily based on the value of assets under management. Moreover, firms with between $100 and $150 million in US assets generally have the choice to be subject to the disclosure-only or full-regulation regime. To address the possible selection bias resulting from this setting, I took the steps described below.

1. Matched sample: disclosure-only and full regulation funds. First, I created a matched sample of funds from the disclosure-only and full-regulation groups. As before, all funds must have the same number of flags for misreporting in the period prior to regulation, and US funds are

matched to US funds (and non-US to non-US). If multiple funds meet these criteria, preference is given to funds with the same investment strategy. If there are still multiple potential matches available, 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 fund's mean monthly return, volatility, age, size, and sensitivty to liquidity in the period prior to regulation. After applying these criteria, I have 47 disclosure-only funds and 47 full-regulation funds. As shown in Panel A of Table 7, the mean number of flags for the matched sample was the same prior to regulation and comparable after regulation. Panel B of Table 7 tests the difference more formally by presenting difference-in-differences regressions comparing misreporting between these groups. The analysis again shows that the decrease in misreporting after regulation was statistically equivalent for both groups.

2. Quasi-discontinuity analysis: disclosure-only and full regulation funds. Second, I use a quasi-discontinuity analysis to compare those funds that were eligible for the disclosure-only regime with those that were almost eligible. Because disclosure-only advisors must have between $100 and $150 million in US assets, I compared 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. 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.[26] The resulting sample is limited, but I include

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[26] Foreign advisors are eligible for the disclosure-only regime if they have less than $150 million in assets from US investors (that is, US assets). But advisors only disclose total assets, not US assets. As such, for foreign advisors, the available data do not allow me to determine whether an advisor is close to the threshold.

the results for completeness. The results, shown in Table 8, provide further confidence that the decrease in misreporting for these two groups was statistically equivalent.

B. 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 not have been an adequate control group because they 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. For example, as noted previously, the only years in which mean fund returns in December were lower than mean fund returns in other months were 2007, 2009, and 2011, presumably because cookie jar accounting was not an option due to the Financial Crisis and its aftershocks.

To test for evidence of the constrained cop theory, Table 9 compares the control funds to funds that were completely unconnected to the US regulatory regime. 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 and do not file Form ADV (the "unaffected" funds).[27] As

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[27] 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 the US regulatory landscape that would make it difficult to draw inferences from funds completely unaffected by the US regulatory regime. For example, the DFA

before, the funds must report throughout the entire relevant period. Panel A of Table 9 presents descriptive statistics, and Panel B presents the regression results. All tests use the same time period and control variables as used in Table 3. As before, standard errors are clustered by fund.

Both panels indicate that misreporting at the unaffected funds was similar to that at the control funds. Following the Hedge Fund Rule, unaffected and control funds both significantly increased misreporting at comparable levels. Following the DFA, the change in misreporting was again comparable for the control and unaffected funds. Because the control funds follow the same trend as these unaffected funds, it appears that changes in misreporting are driven by economic fluctuations rather than the constrained cop theory.

C. Inherent Limitations

Finally, I note two methodological limitations to my analysis. First, because I studied the change in misreporting at the funds newly subject to federal oversight, I omitted funds that evaded the federal regulatory regime. 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 mandatory federal regulation (e.g., Leuz et al., 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

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 not unaffected funds.

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 than 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.

There are limitations to the use of proxies, however, and it would be concerning if the results using different proxies were inconsistent with one another. To provide an overview of the consistency across different proxies, Table 10 presents a summary of the main results, broken down by proxy. The kink results in the most consistently significant findings, and the flag for cookie jar accounting, when significant, results in the most statistically significant findings.[28]

8. Conclusion

My evidence indicates that regulation, and particularly rules mandating disclosure of governance and compliance information, has significantly reduced misreporting by hedge funds. The finding that the imposition of mandatory disclosure, even without other concurrent changes in regulation, reduced misreporting by hedge funds is perhaps surprising given that hedge fund investors are generally considered to be highly sophisticated individuals who already have access

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[28] Consistent with the results in the primary analysis, none of the individual flags for misreporting are significant for the Remain funds in the *Goldstein* analysis.

to the information that is being disclosed. To understand why mandated disclosure of this information would lead to a decrease in misreporting, I conduct additional analysis that leads me to conclude that the disclosure rules spurred funds to make internal changes in governance, ultimately inducing them to report their financial performance more accurately.

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Appendix: Timeline of Events

Dec. 2004
The SEC adopts the Hedge Fund Rule, imposing regulation on many previously unregulated funds for the first time.

Sept. 2006
Funds begin to withdraw from SEC regulation after the SEC stated in August 2006 that it would not appeal the *Goldstein* opinion vacating the Hedge Fund Rule.

June 2011
The SEC adopts the rules to enact the relevant parts of the Dodd-Frank Act, imposing regulation on many previously unregulated funds (and those funds that withdrew after the *Goldstein* opinion). The SEC's implementation of the Act creates three groups of funds, each subject to a different change in regulation:

Full-regulation (RIAs)
Subject to a change in disclosure requirements, compliance requirements, and SEC inspections

Disclosure-only (ERAs)
Subject to a change in disclosure requirements

Inspection-only (previous state registrants)
Subject to a change in SEC inspections

Appendix: Fund Variable Definitions

	Theoretical Definition	Empirical Definition
	Hedge Fund Rule	
Treatment Funds	Funds that became regulated because of the Hedge Fund Rule.	Previously unregistered funds that registered as Registered Investment Advisers ("RIAs") with the SEC at any point from August 2005–January 2006.
Control Funds	Funds not affected by the Hedge Fund Rule.	Funds continuously registered with the SEC as RIAs in the months from June 2002 through May 2007 (i.e., the 60-month period around the event date of December 2004).
Regulatory Event	The SEC's adoption of the Hedge Fund Rule in December 2004.	
	Goldstein Opinion	
Withdraw Funds	Funds that became regulated by the Hedge Fund Rule and elected to withdraw from SEC regulation after the Hedge Fund Rule was vacated.	Funds that registered in accordance with the Hedge Fund Rule and withdrew at any point between the day the SEC stated that it would not to appeal the court's decision in August 2006 and Jan. 31st, 2007 (the deadline to withdraw without penalty).
Remain Funds	Funds that became regulated by the Hedge Fund Rule and elected to remain regulated even after the Hedge Fund Rule was vacated.	Funds that registered in accordance with the Hedge Fund Rule and remained registered through March 2009 (i.e., through the 30-month period subsequent to the event date of September 2006).
Control Funds	Funds not affected by the *Goldstein* decision.	Funds continuously registered with the SEC in the months from March 2004 through March 2009 (the 60-month period around the event date of September 2006).
Regulatory Event	September 2006: the first month in which any of the funds in my sample withdrew after the SEC stated in August 2006 that it would not appeal the *Goldstein* opinion.	

Treatment Funds	Funds that became regulated because of the DFA. To make the analysis of the DFA comparable to that of the Hedge Fund Rule, Table 3 includes only Full-Regulation funds (see below) and omits funds that withdrew following *Goldstein*.	Funds that registered with the SEC for the first time as RIAs at any point from October 2011–March 2012.
Control Funds	Funds not affected by the DFA.	Funds continuously registered with the SEC as RIAs in the months from Jan. 2009 through Dec. 2013 (i.e., the 60-month period around the event date of June 2011).
Full-Regulation Funds	Funds that became subject to full SEC regulation (i.e., mandatory disclosure, government inspections, and compliance requirements).	Previously unregistered funds that registered as RIAs with the SEC at any point from October 2011–March 2012.
Disclosure-only Funds	Funds that only became subject to the SEC's public disclosure requirements (i.e., funds that had to file Form ADV for the first time).	Previously unregistered funds that registered as Exempt Reporting Advisers (ERAs) with the SEC at any point from October 2011–March 2012. As relevant to my study, a fund is eligible for ERA status if it advises only private funds with less than $150M in US assets.
Inspection-only Funds	Funds that only became subject to SEC inspections (i.e., funds that were already filing Form ADV and subject to state compliance requirements, but were not subject to government inspections).	Funds that were previously registered as a RIA with a state securities agency but switched to SEC registration at any point from October 2011–March 2012. Subject to limited exception, the DFA mandated that advisers with $100M or more be regulated by the SEC, not a state agency. As a practical matter, most of the funds in my sample that meet this definition switched from New York, a state that does not conduct compliance examinations.
Regulatory Event	The SEC's adoption of the rules to enact the relevant parts of the DFA in June 2011.	

Figure 1. 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).
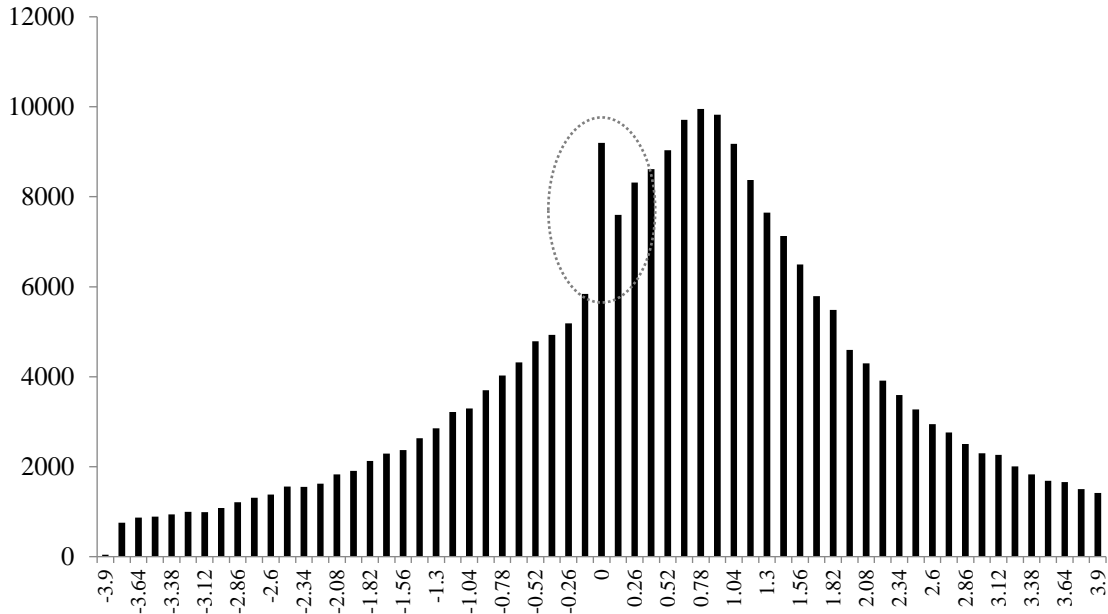
Figure 2. Measure of Misreporting: Cookie Jar Accounting. This figure describes mean hedge fund returns in December and non-December months, and indicates that mean fund returns in December were higher than mean fund returns in other months in ten of the thirteen years from 2000 to 2013. The figure is based on all funds in the Lipper Hedge Fund database.

□ Non-Dec. Average Return   ■ Dec. Average Return
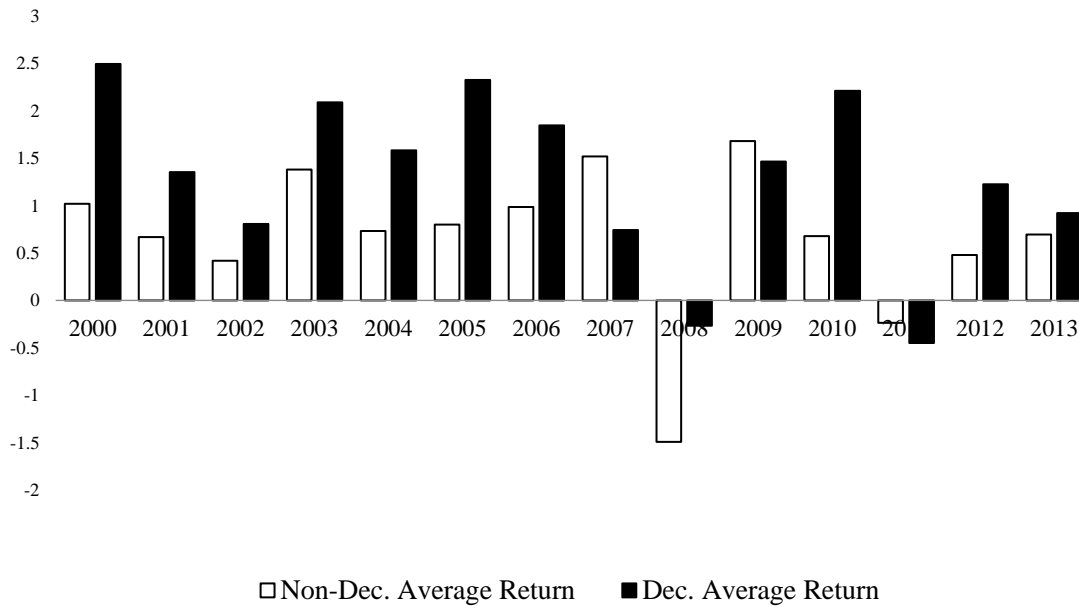
Figure 3. Measure of Misreporting: Benford's Law. This figure describes the distribution of the first digits of monthly hedge fund returns. The white bar indicates the expected distribution based on Benford's Law, the grey bar indicates the actual distribution of first digits of all monthly returns reported to the Lipper Hedge Fund database from 2000-2013, and the black bar indicates the distribution of first digits of all monthly returns reported by Bernie Madoff's investment fund. The first digits of negative returns are included.

Table 1. Descriptive Statistics: All Funds. Table 1 provides descriptive statistics for the treatment and control funds. Panel A reflects the funds used in the analyses of the Hedge Fund Rule and the Dodd-Frank Act ("DFA"). 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 period (i.e., funds that did not have a change in regulatory status following the change in law). Panel B reflects the sample used in the *Goldstein* analysis. The control funds are the same as previously defined, 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, and whether the fund is incorporated in the US. 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.
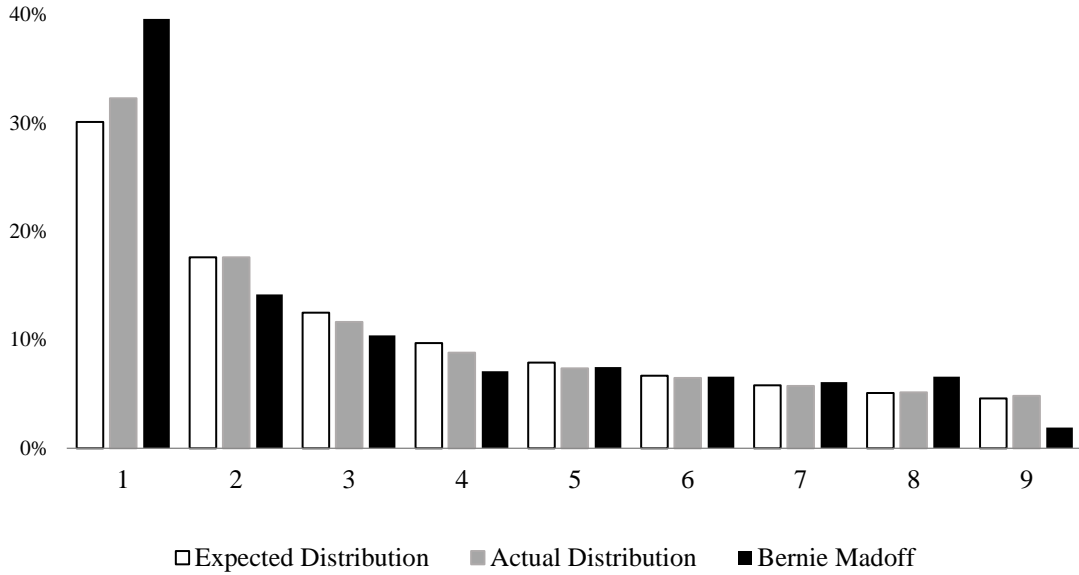
Panel A.

Variable	Hedge Fund Rule			Dodd-Frank Act		
	Treat.	Control	t-stat	Treat.	Control	t-stat
Monthly Return	0.94	0.82	*3.52*	1.06	0.83	*3.15*
Ln (Net Asset Value)	5.86	6.28	*-4.37*	6.37	6.08	*2.73*
Age	7.37	7.62	*-5.82*	7.68	7.83	*-3.22*
Return Volatility	2.24	1.81	*4.87*	3.34	2.53	*5.37*
US Incorporation	0.20	0.33	*-3.71*	0.32	0.30	*0.73*
Liquidity Sensitivity	21.17	4.09	*-2.26*	-27.51	-40.01	*-2.00*
Num. Funds	*124*	*228*		*108*	*552*	

Panel B.

Variable	*Goldstein* Opinion					
	Withdraw	Control	t-stat	Withdraw	Remain	t-stat
Monthly Return	0.36	0.29	*1.13*	0.36	0.45	*-1.26*
Ln (Net Asset Value)	5.61	6.06	*-3.64*	5.61	6.13	*-4.10*
Age	7.60	7.61	*-0.96*	7.60	7.43	*2.80*
Return Volatility	2.99	2.36	*4.29*	2.99	3.08	*-0.48*
US Incorporation	0.13	0.30	*-3.85*	0.13	0.20	*-1.54*
Liquidity Sensitivity	56.46	28.88	*3.69*	56.46	48.88	*0.79*
Num. Funds	*55*	*289*		*55*	*102*	

Table 2. Descriptive Statistics: 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 flag individually. The treatment and control funds are as defined in Table 1.

Panel A.

Number of Flags – Full Sample

	Hedge Fund Rule			Dodd-Frank Act		
	Control	Treat.	t-test	Control	Treat.	t-test
Before Regulation	0.34	0.43	*-1.44*	0.54	0.65	*-1.62*
After Regulation	0.60	0.44	*2.06*	0.56	0.31	*3.86*
t-test (after vs. before)	*4.32*	*0.22*		*0.50*	*4.19*	
Diff. (after - before)	0.26	0.01	*2.74*	0.02	0.34	*4.01*

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*
Benford's Law	0.17	0.15	*0.37*	0.22	0.20	*0.41*

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	0.14	*-1.70*	0.10	0.08	*0.5*
Cookie Jar	0.31	0.36	*-1.18*	0.34	0.13	*4.41*
Benford's Law	0.14	0.15	*-0.07*	0.14	0.11	*0.81*

Panel C.

<div align="center">Number of Flags – Matched Sample</div>

	Hedge Fund Rule			Dodd-Frank Act		
	Control	Treat.	t-test	Control	Treat.	t-test
Before Regulation	0.33	0.33	*0.00*	0.58	0.58	*0.00*
After Regulation	0.66	0.43	*2.45*	0.45	0.30	*1.81*
t-test (after vs. before)	*3.85*	*1.32*		*-1.60*	*-3.46*	
Diff. (after - before)	0.33	0.10	*2.17*	-0.13	-0.28	*1.32*

Panel D.

<div align="center">Matched Sample: Frequency of Each Type of Flag – Hedge Fund Rule</div>

	Before Regulation			After Regulation		
	Control	Treat.	t-test	Control	Treat.	t-test
Kink	0.11	0.13	*-0.46*	0.21	0.15	*1.24*
Cookie Jar	0.06	0.13	*-1.61*	0.23	0.11	*2.37*
Benford's Law	0.16	0.07	*1.92*	0.22	0.17	*0.85*

<div align="center">Matched Sample: Frequency of Each Type of Flag – Dodd-Frank Act</div>

	Before Regulation			After Regulation		
	Control	Treat.	t-test	Control	Treat.	t-test
Kink	0.07	0.11	*-0.92*	0.11	0.07	*1.06*
Cookie Jar	0.32	0.33	*-0.15*	0.19	0.13	*1.15*
Benford's Law	0.19	0.15	*0.93*	0.15	0.10	*1.07*

Table 3. Regression Results: Mandatory Regulation. Table 3 examines the effect of SEC regulation on misreporting. All models are run using ordinal logit models, and the dependent variable is the number of misreporting flags. Panel A shows the analysis for the Hedge Fund Rule, and Panel B shows the analysis for the Dodd-Frank Act. For the Hedge Fund Rule, the Post variable is set to 1 in the 30 months after December 2004, when the SEC adopted the Hedge Fund Rule, and to 0 in the 30 months before the Rule was adopted. For the Dodd-Frank Act, the Post variable is set to 1 in the 30 months after July 2011, when the SEC adopted the rules to implement the DFA, and to 0 in the 30 months before the rules were adopted. 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). All models control for each fund's mean monthly return, mean log of net asset value, mean age over the period, return volatility over the period, sensitivity to market liquidity, and whether the fund was incorporated in the US (the variables are described 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. Models (1) and (2) use the full sample of funds, and Models (3) and (4) use the matched sample. The New Fund indicator is omitted from the regressions that include fund fixed effects because it is collinear with the fund fixed effects. Statistical significance of 10, 5, and 1 percent is indicated by *, **, and ***, respectively.

Panel A.

	Hedge Fund Rule			
	(1)	(2)	(3)	(4)
	Full Sample		Matched Sample	
Post	0.78***	1.37*	0.92***	0.34
	(0.25)	(0.741)	(0.34)	(0.96)
New Fund	0.83***		0.55	
	(0.27)		(0.38)	
Post * New Fund	-1.01***	-1.69**	-0.86**	-2.11**
	(0.33)	(0.70)	(0.42)	(0.93)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char.	Fund
Observations	722	722	436	436
R-squared	0.09	0.50	0.09	0.53

Panel B.	Dodd-Frank Act			
	(1)	(2)	(3)	(4)
	Full Sample		Matched Sample	
Post	-0.02	1.19**	-0.48	1.35
	(0.15)	(0.43)	(0.40)	(0.99)
New Fund	0.88***		0.09	
	(0.21)		(0.34)	
Post * New Fund	-1.67***	-2.64***	-0.89*	-1.76**
	(0.30)	(0.66)	(0.48)	(0.88)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char	Fund
Observations	1,322	1,322	404	404
R-squared	0.08	0.48	0.18	0.55

Table 4. Regression Results: Funds that Withdrew from Regulation. Table 4 examines the effect of SEC regulation on misreporting. All models are run using ordinal logit models and include the control variables noted in Table 3. The dependent variable is the number of misreporting flags. The Post variable is set to 1 in the 30 months after September 2006, when the first funds in my sample withdrew from SEC oversight, and to 0 in the 30 months before September 2006. 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. The control funds are those that were continuously regulated by the SEC throughout the entire sample period. The results are presented first using the full sample and then using only a matched sample of Withdraw and control funds (because there are three groups of funds in the full sample, Remain funds are omitted from the matched sample). 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. The Withdraw and Remain indicators are omitted from the regressions that include fund fixed effects because they are collinear with the fund fixed effects. Statistical significance of 15, 10, 5, and 1 percent is indicated by +, *, **, and ***, respectively.

	(1)	(2)	(3)	(4)
	Full Sample		Matched Sample	
Post	-0.49**	-1.66**	-0.79	-0.74
	(0.22)	(0.72)	(0.83)	(2.69)
Withdraw	-1.15***		-0.54	
	(0.44)		(0.66)	
Remain	-0.48*			
	(0.29)			
Post * Withdraw	0.98*	1.85*	1.77*	3.24+
	(0.51)	(1.01)	(1.02)	(2.10)
Post * Remain	0.49	0.66		
	(0.37)	(0.80)		
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char.	Fund
Observations	892	892	204	204
R-squared	0.05	0.55	0.25	0.58

Table 5. Regression Results: Misreporting and Type of Regulation. Table 5 provides evidence on the effects of different types of regulation. All models are run using ordinal logit models and use the same time period, control group, and control variables as the DFA analysis in Table 3. The dependent variable is the number of misreporting flags. The variable Full Reg. is set to 1 for all funds that became subject to full regulation, and to 0 otherwise. The variable Disc. Only is set to 1 for all funds that became subject to only disclosure rules, and to 0 otherwise. The variable Inspect. Only is set to 1 for all funds that became subject to only a change in government inspections, and to 0 otherwise. 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. The Full Reg., Disc. Only, and Inspect. Only indicators are omitted from the regressions that include fund fixed effects because they are collinear with the fund fixed effects. Statistical significance of 10, 5, and 1 percent is indicated by *, **, and ***, respectively.

	Number of Flags	
	(1)	(1)
	Full Sample	Full Sample
Post	-0.04	1.00**
	(0.15)	(0.41)
Full Reg.	0.81***	
	(0.29)	
Disc. Only	0.88***	
	(0.21)	
Inspect. Only	0.05	
	(1.05)	
Post * Full Reg.	-1.69***	-2.66***
	(0.29)	(0.65)
Post * Disc. Only	-1.59***	-2.84***
	(0.34)	(0.77)
Post * Inspect. Only	-0.23	1.28
	(0.14)	(0.99)
Controls	Yes	Yes
Fixed Effects	Char.	Fund
Observations	1,498	1,498
R-squared	0.09	0.48

Table 6. Regression Results: Auditing and Misreporting. Table 6 provides evidence on the effects of hiring an auditor. All models are run using ordinal logit models and use the same time period, control group, and control variables as the models in Table 3. The dependent variable is the number of misreporting flags. The variable Initiate Audit is set to 1 for all newly regulated funds that initiated audit procedures following regulation, and to 0 otherwise. The variable No Change in Audit is set to 1 for all newly regulated funds that did not initiate audit procedures following regulation, and to 0 otherwise. 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. The Initiate Audit and No Change indicators are omitted from the regressions that include fund fixed effects because they are collinear with the fund fixed effects. Statistical significance of 10, 5, and 1 percent is indicated by *, **, and ***, respectively.

	Hedge Fund Rule		Dodd-Frank Act	
	(1)	(1)	(1)	(1)
	Full Sample	Full Sample	Full Sample	Full Sample
Post	0.75***	1.08	-0.02	1.00**
	(0.25)	(0.76)	(0.15)	(0.41)
Initiate Audit	1.07***		1.24*	
	(0.31)		(0.64)	
No Change in Audit	0.64**		0.87***	
	(0.32)		(0.19)	
Post * Initiate Audit	-1.63***	-3.07***	-2.37**	-4.02*
	(0.42)	(0.89)	(1.06)	(2.15)
Post * No Change in Audit	-0.57	-0.88	-1.66***	-2.75***
	(0.40)	(0.80)	(0.24)	(0.53)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char.	Char.
Observations	722	722	1,516	1,516
R-squared	0.09	0.51	0.09	0.49

Table 7. Robustness: Matching based on Type of Regulation. Table 7 compares the funds that became subject to the disclosure-only regime with a matched sample of funds that became subject to full regulation. Panel A provides descriptive statistics for the matched sample, and Panel B compares the change in misreporting at the disclosure-only funds relative to the change at the full-regulation funds. All models are run using ordinal logit models and use the same time period, control group, and control variables as the DFA analysis in Table 3. The dependent variable is the number of misreporting flags. The variable Disc. Only is set to 1 for all funds that became subject to the disclosure-only regime, and to 0 for all the funds that became subject to full regulation. 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. The Disc. Only indicator is omitted from the regression that includes fund fixed effects because it is collinear with the fund fixed effects. Statistical significance of 10, 5, and 1 percent is indicated by *, **, and ***, respectively.

Panel A.

Matched Sample: Full Reg. and Disc. Only Funds - Dodd-Frank Act			
	Disclosure-Only	Full-Regulation	t-test
Before Regulation	0.49	0.49	*0.00*
After Regulation	0.26	0.32	*-0.55*
t-tests (before vs. after)	*1.45*	*1.85*	
Diff. (after – before)	*-0.23*	*-0.17*	*-0.41*

Panel B.

	(1) Matched Sample	(2) Matched Sample
Post	-1.60**	-1.83
	(0.69)	(2.55)
Disc. Only	0.61	
	(0.55)	
Post * Disc. Only	0.46	1.71
	(0.72)	(1.51)
Controls	Yes	Yes
Fixed Effects	Char.	Fund
Observations	188	188
R-squared	0.16	0.60

Table 8. Robustness: Quasi-discontinuity Design. Table 8 compares the disclosure-only to a sample of full-regulation funds that were close to the eligibility threshold for the disclosure-only regime. All models are run using ordinal logit models and use the same time period, control group, and control variables as the DFA analysis in Table 3. The dependent variable is the number of misreporting flags. Funds are eligible for the SEC's disclosure-only regime if the fund advisor advises funds with $100-$150 million in US assets. If the fund advisor advises funds with more than $150 million in US assets, the fund is automatically subject to full regulation. Hence, the variable Below Threshold is set to 1 for all funds that became subject to only disclosure rules, and to 0 for all funds that just missed the eligibility threshold—that is, funds managed by advisers with assets under management from $150 million to $200 million. Because it is impossible to reliably determine whether foreign funds were close to the threshold, only US based funds are included. Fixed effects are included either for the fund's investment style ("Fund Style") or for the fund itself ("Fund"). Standard errors are clustered by fund. The Below Threshold indicator is omitted from the regression that includes fund fixed effects because it is collinear with the fund fixed effects. Statistical significance of 10, 5, and 1 percent is indicated by *, **, and ***, respectively.

	Number of Flags	
	(1)	(1)
	Full Sample	Full Sample
Post	-1.41	-2.62
	(0.83)	(3.44)
Below Threshold	-1.24	
	(0.70)	
Post * Below	0.49	0.40
	(0.98)	(2.43)
Controls	Yes	Yes
Fixed Effects	Fund Style	Fund
Observations	98	98
R-squared	0.22	0.62

Table 9. Robustness: Placebo Tests. This table provides placebo tests comparing the change in misreporting at the control funds relative to the change in misreporting at funds unaffected by the US regulatory regime. Panel A presents the descriptive statistics, and Panel B presents the regression results. Funds unaffected by the US regime are defined as funds that are located outside the US, have never filed Form ADV, and report to the Lipper Hedge Fund database throughout the entire 60-month period surrounding the change in law. The control funds are the same as those defined in Table 1. All models in Panel B are run using ordinal logit models and use the same time period, control group, and control variables as the models in Table 3. The dependent variable is the number of misreporting flags. The variable Unaffected is set to 1 for all unaffected funds, and to 0 otherwise. 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. The Unaffected indicator is omitted from the regressions that include fund fixed effects because it is collinear with the fund fixed effects. Statistical significance of 10, 5, and 1 percent is indicated by *, **, and ***, respectively.

Panel A.

Number of Flags – Control Funds & Unaffected Funds

	Hedge Fund Rule			Dodd-Frank Act		
	Control	Unaffected	t-test	Control	Unaffected	t-test
Before Regulation	0.34	0.33	*0.11*	0.54	0.46	*1.21*
After Regulation	0.60	0.56	*0.56*	0.56	0.48	*1.27*
t-test (after vs. before)	*4.32*	*2.96*		*0.50*	*0.23*	
Diff. (after - before)	0.26	0.23	*0.43*	0.02	0.02	*0.00*

Panel B.

	(1)	(2)	(3)	(4)
	Hedge Fund Rule		Dodd-Frank Act	
Post	0.59**	1.41**	0.01	1.35***
	(0.25)	(0.71)	(0.15)	(0.45)
Unaffected	-0.13		0.70**	
	(0.31)		(0.31)	
Post * Unaffected	0.10	0.25	-0.01	-0.17
	(0.33)	(0.74)	(0.29)	(0.56)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Char.	Fund	Char.	Fund
Observations	752	752	1348	1348
R-squared	0.11	0.54	0.07	0.47

Table 10. Robustness: Results by Proxy. Table 10 presents the results for each measure of misreporting separately. Panel A replicates Tables 3 and 4, and Panel B replicates Table 5. The table reports the statistical significance of the coefficient on the variable of interest when the dependent variable reflects only the proxy in question. In each panel, the first set of results reports the results using fixed effects for fund characteristics, and the second set of results reports the results using fund fixed effects. Consistent with the results reported earlier, all statistically significant coefficients are negative.

Panel A.

	Kink at Zero	Cookie Jar Accounting	Benford's Law
	Fund Characteristics Fixed Effects		
Hedge Fund Rule	10%	1%	Not. Sig.
DC Circuit Opinion – Withdraw	5%	Not. Sig.	15%
Dodd-Frank Act	1%	1%	10%
	Fund Fixed Effects		
Hedge Fund Rule	10%	5%	Not. Sig.
DC Circuit Opinion – Withdraw	10%	Not. Sig.	15%
Dodd-Frank Act	5%	1%	15%

Panel B.

	Kink at Zero	Cookie Jar Accounting	Benford's Law
	Fund Characteristics Fixed Effects		
Disclosure-only Funds	15%	5%	10%
Full Regulation Funds	10%	1%	Not. Sig.
Inspection-only Funds	Not. Sig.	Not. Sig.	Not. Sig.
	Fund Fixed Effects		
Disclosure-only Funds	5%	5%	15%
Full Regulation Funds	10%	1%	Not. Sig.
Inspection-only Funds	Not. Sig.	Not. Sig.	Not. Sig.