

The Surprising Benefits of Mandatory Hedge Fund Disclosure

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ABSTRACT

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Regulators have long disagreed whether regulation would reduce hedge funds' financial misreporting. On the one hand, critics have stated that hedge funds are unlikely to misreport because their investors are highly sophisticated financial players who can detect and deter financial misconduct. On the other hand, recent changes in the composition of hedge funds' investors have led many to question this argument. In this paper, I test whether hedge fund regulation reduces misreporting by using a quasi-natural experiment in which a subset of hedge funds was regulated, deregulated, and then regulated again. Unique features of the setting permit me to study not only whether hedge fund regulation reduces financial misreporting—but, if so, *why* the regulation reduces misreporting. The results show that regulation reduces misreporting at hedge funds and that the imposition of disclosure requirements, even without other concurrent changes in regulation, can reduce hedge funds' misreporting. The result seems surprising, because hedge funds' investors are commonly thought to have access to far more information than is required by disclosure rules. Further inquiries suggest that disclosure requirements led funds to make changes in their internal governance, and that these changes in governance induced funds to report their financial performance more honestly and accurately.

Table of Contents

List of Charts, Graphs, and Illustrations	iii
Acknowledgements	iv
Dedication	v
1. Introduction.....	1
2. Institutional Background.....	5
A. Setting	5
1. The SEC’s “Hedge Fund Rule.”	6
2. Goldstein v. SEC.....	7
3. The Dodd-Frank Act.	7
B. Components of Hedge Fund Regulation	8
1. Mandatory disclosure.....	8
2. Government inspections.....	9
3. Compliance requirements.	9
3. Methodology	10
A. Data	10
1. Form ADV.	10
2. Lipper Hedge Fund Database (TASS).	11
B. Research Design.....	12
4. Measures of Misreporting.....	13
A. Kink at Zero	14
B. Cookie Jar Accounting.....	17
C. Benford’s Law.....	19
5. The Effect of Hedge Fund Regulation on Misreporting	22
A. Descriptive Statistics.....	22
1. Comparison of treatment and control funds.....	22
2. Frequency of misreporting.....	25
B. Regression Results	29

1. Imposition of federal regulation.....	29
2. Removal of federal regulation.	33
6. Disentangling the Effect of Different Regulatory Components	36
A. Background	36
B. Isolating the Regulatory Components	38
C. Disclosure Mechanism	40
1. Anecdotal evidence.....	40
2. Empirical analysis.....	41
7. Robustness	44
A. Non-randomness of Treatment Funds.....	44
1. Matched sample.	45
2. Quasi-discontinuity analysis.	47
B. Non-Randomness of Control Sample.....	48
C. Inherent Limitations	49
1. Regulatory avoidance.....	49
2. Proxies for misreporting.	50
8. Conclusion	52
Reference List	54
Appendix: Fund Variable Definitions.....	59

List of Charts, Graphs, and Illustrations

Figures.

Figure 1. Measure of Misreporting: Kink.

Figure 2. Measure of Misreporting: Cookie Jar Accounting.

Figure 3. Measure of Misreporting: Benford's Law.

Tables.

Table 1. Descriptive Statistics: All Funds.

Table 2. Descriptive Statistics: Frequency of Flags for Misreporting.

Table 3. Regression Results: Mandatory Regulation.

Table 4. Regression Results: Funds that Withdrew from Regulation.

Table 5. Regression Results: Misreporting and Type of Regulation.

Table 6. Regression Results: Auditing and Misreporting.

Table 7. Robustness: Matching based on Type of Regulation.

Table 8. Robustness: Quasi-discontinuity Design.

Table 9. Robustness: Results by Proxy.

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Dedication

My thanks to my husband, Tanner, for his patience with my many years of school. I would not have found my dream job without his support.

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 influential policymakers to argue that hedge fund regulation is unnecessary because their 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. Growth in hedge funds has outpaced growth of investment funds overall, and 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). Furthermore, hedge funds were once considered to cater solely to wealthy individuals, but the majority of hedge funds' investors are now institutional investors such as pensions and universities—individuals 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 invests on behalf of an individual (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 institutional investors and their clients 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 especially exacerbated if the investor needs to meet a performance target.

There have been a series of legal changes in response to the developments in the hedge fund industry. In particular, since 2004, hedge funds have experienced three significant changes that caused a subset of funds to be regulated, deregulated, and regulated once again. As with public companies, a fund that becomes regulated will be subject to a number of concurrent changes—specifically, a regulated fund becomes subject to government inspections, compliance requirements, and mandatory public disclosure. However, an unusual feature of hedge fund regulation is that there are different tiers of regulation, and funds regulated under different tiers became subject to different types of regulation. For example, one tier of funds was subjected to only mandatory disclosure rules. And another became subjected to only government inspections.

This unique setting allows for study on whether hedge fund regulation reduces misreporting—and, if so, *why* it reduces misreporting. 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, I provide evidence that 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 only the funds subject to disclosure significantly decreased misreporting. Notably, funds *only* subject to disclosure requirements decreased misreporting—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 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 at the funds that induced them to report more accurately.

All tests are difference-in-differences regressions that compare the hedge funds affected by the regulatory changes to a control group of funds that were already regulated by the SEC before the adoption of mandatory regulation. I further include a battery of robustness tests to address sample selection concerns. To identify misreporting at hedge funds, I follow prior literature and test for 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—because prior literature has shown that it is the best 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 Benford's Law predicts misreporting (e.g., Amiran 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.¹ Instead, to study the effect of disclosure in recent periods, most studies on US firms are forced to examine the effects of mandating *additional* disclosures on firms that are already subject to mandatory disclosure. Because extensive disclosures are already required, the marginal effect of these additional disclosures is likely to be small, making it difficult to understand the economic magnitude associated with disclosure requirements (Leuz and Wysocki, 2008; 2016). By using hedge funds, I have a unique setting that allows for study on the first-time effects of mandatory securities disclosure in a recent period.

Second, my study is related to the literature disentangling the effects of different regulatory components. Empirical study on the relative effects of different types of regulation is difficult because there are usually multiple changes at once. Moreover, it is difficult to study each change in isolation because different components necessarily depend on one another: The benefits of mandatory disclosure and enforcement, for example, depend on the strength of the other (Barth and Israeli, 2013). Although some prior literature has examined the relative effects of different types of regulation on public companies (e.g., Christiansen et al., 2013), I am not aware of any empirical study of the different regulatory components at investment funds. The need for empirical

¹ There are a limited number of studies that analyze the effect of first-time disclosure for OTC firms, which occurred in a more recent period (e.g., Ferrell, 2003; Bushee and Leuz, 2005). However, the imposition of disclosure requirements in the OTC market, although the most significant regulatory change when it was imposed, occurred concurrent with other regulatory changes.

work in this area is striking because regulators have long questioned whether lessons from public companies can be applied to investment funds (Greenspan, 1998).

Finally, I contribute to the literature on 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). My initial findings showing the regulation reduces misreporting are consistent with these studies, but I extend the initial inquiry of whether regulation reduces misreporting and instead focus on *why* regulation is effective.

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 tumultuous series of changes.² Between 2004 and 2010, groups of hedge funds

² 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

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 these recent changes and provide detail on the major components of hedge fund regulation.

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.³

The first change occurred in 2004, when the SEC proposed to subject the vast majority of unregulated hedge funds to federal regulation for the first time (HF Rule, 2004). The rule, nicknamed the Hedge Fund Rule, closed a commonly used exemption that many hedge funds relied upon to avoid regulation under the Investment Advisers Act ("IAA").⁴ The SEC adopted the

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).

³ The changes in law were imposed on hedge fund advisory firms (i.e., investment advisors) rather than the funds, but I use the term funds for ease of exposition. In my setting, the advisor and fund were substantively the same entity.

⁴ At the time this rule was proposed, Section 203(b)(3) of the IAA exempted advisors that did not hold 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 the exemption.

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 SEC's adoption of 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 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 these different tiers of regulation in more detail in Section 6.

B. Components of Hedge Fund Regulation

To understand what it means for a fund to be “regulated,” I briefly summarize the major components of SEC regulation.⁵ 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.

1. Mandatory disclosure. Most 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 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—not its financial performance.

Although Form ADV has received limited attention from academics, the disclosure gives investors important information about their advisors. For example, some 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 a that the fund

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.

engages in transactions that could cause a significant potential conflict of interest (defined as funds that participate in either principal transactions or agency cross trades).⁶

2. Government inspections. Regulated funds are also 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 below, such as record-keeping and proper client communication. While the exams may peripherally address the accuracy of the advisor's Form ADV disclosure, the accuracy of this information is not generally the 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).

3. Compliance requirements. Finally, upon regulation, advisors are generally subject to a multitude of compliance requirements. The most notable compliance requirements are that the advisor must adopt written compliance policies and procedures, appoint a Chief Compliance

⁶ Form ADV is the only mandatory public filing for most hedge funds, but some funds may be required to file two other forms. First, following the 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 securities will be 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.

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. 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 governance of each fund from historical Form ADV filings; although these data are generally not publicly available, I obtained them by filing Freedom of Information Act ("FOIA") requests with, among others, the SEC. Second, I pulled data on each fund's returns from the Lipper Hedge Fund database, a commercial database to which hedge funds voluntarily report their returns.

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 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 select managers who have better performance in future periods.

Despite the apparent utility of Form ADV, this filing has received little attention in academic literature—likely because historical Form ADV data are extremely difficult to obtain. The current versions of Form ADV are available online, but historical data have traditionally been unavailable to academic researchers.⁷ Moreover, because some advisors are regulated by states and others by the SEC, FOIA requests must be filed separately at each regulator. To obtain the data for this project, I filed FOIA requests with the SEC and sixteen state securities agencies.⁸ My original request with the SEC was denied, but I later obtained the data after a lengthy appeals process.

2. Lipper Hedge Fund Database (TASS). I obtained information on hedge funds’ financial performance, such as monthly returns, 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.

⁷ To my knowledge, the only academic studies that use time-series Form ADV data use the dataset described by Dimmock and Gerken (2012). The authors note in the paper that their Form ADV data were not publicly available.

⁸ 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. In total, 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. The SEC does provide limited historical Form ADV filings online, but these data are notoriously poor quality and would not have permitted me to run the analyses in this paper.

B. Research Design

The unusual nature of my setting permits me to consider the effects both of *imposing* SEC oversight and of *removing* that oversight. All tests use a “difference-in differences” approach. 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). For consistency, I use the standardized methodology described below across all tests.

First, consistent with Dimmock and Gerken (2015), I require each fund included in my sample to have thirty months of observations both before and after the relevant change in law. Such a lengthy period is necessary in order to identify misreporting. For example, as discussed below, one of my measures of misreporting is based on regression estimates, necessitating sufficient observations to run a regression.

Second, I identify the treatment and control samples as follows. If a fund was continually subject to SEC regulation during the thirty months before and after the relevant change in law, I include it in the control sample.⁹ If the fund submitted to federal oversight in the six months prior to the deadline imposed by the relevant law, I consider it to have become regulated in accordance

⁹ I used a control sample of funds that were already regulated by the SEC as opposed to foreign funds because there were changes in the US regulatory landscape that were unrelated to the legal changes I study here. As one such example, the DFA 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 may not have been captured if I had used an alternate control sample such as foreign funds.

with the change in law and include in the treatment sample. Funds were required to submit to SEC oversight by January 31st, 2006 and by March 31st, 2012 for the Hedge Fund Rule and the DFA, respectively, so this period ranges from August 2005–January 2006 for the Hedge Fund Rule and from October 2011–March 2012 for the DFA. Funds that do not meet the criteria of either the treatment or control sample are dropped from the analysis, and all funds lacking full data in both periods are omitted from the analysis so that the sample will not be biased due to attrition.

For the Hedge Fund Rule and DFA, I use the month the SEC adopted the final rule in question as the date of the change in law. These months are December 2004 and June 2011 for the Hedge Fund Rule and the DFA, respectively (the month of the change is included in the pre period). I select the month of rule adoption because the funds knew at this point that they would be regulated, allowing them to prepare for the upcoming changes. For the Court’s opinion in *Goldstein* vacating the Hedge Fund Rule, I use September 2006—the first month in which any of the funds withdrew after the SEC stated that it would not appeal the *Goldstein* opinion in August 2006—as the date of the relevant change in law, and I compare the 30 months before and after that date. The timeline and variable descriptions are summarized in the Appendix.

4. Measures of Misreporting

The variable of interest for my study, misreporting by hedge funds, is notoriously difficult to measure. First, hedge funds have substantial holdings of Level 2 and Level 3 assets. From an outsider’s perspective, it is particularly difficult to identify misreporting of these assets because

there is often no clear pricing benchmark. The lack of an objective benchmark is thought to provide managers with more discretion in valuation, leading some funds seek to minimize managerial discretion through external monitoring such as auditing and independent pricing. Evidence suggests that these methods do reduce misreporting, but that they are not foolproof (e.g., Brown et al. 2012; Cassar and Gerakos, 2010, 2011). Nor are they 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 these prior studies and identify misreporting using three suspicious patterns in the monthly returns that funds report to their investors.

A. Kink at Zero

First, following prior literature, I test 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 manage monthly reported returns so as to turn small losses into small gains. This means that the observations that would naturally have fallen into the bucket below zero instead fall into the bucket above zero, thus allowing the fund to report a small gain instead of a small loss. 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 is the strongest predictor of detected accounting fraud at hedge funds.

Figure 1 provides the distribution of monthly returns for all funds in TASS 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 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).

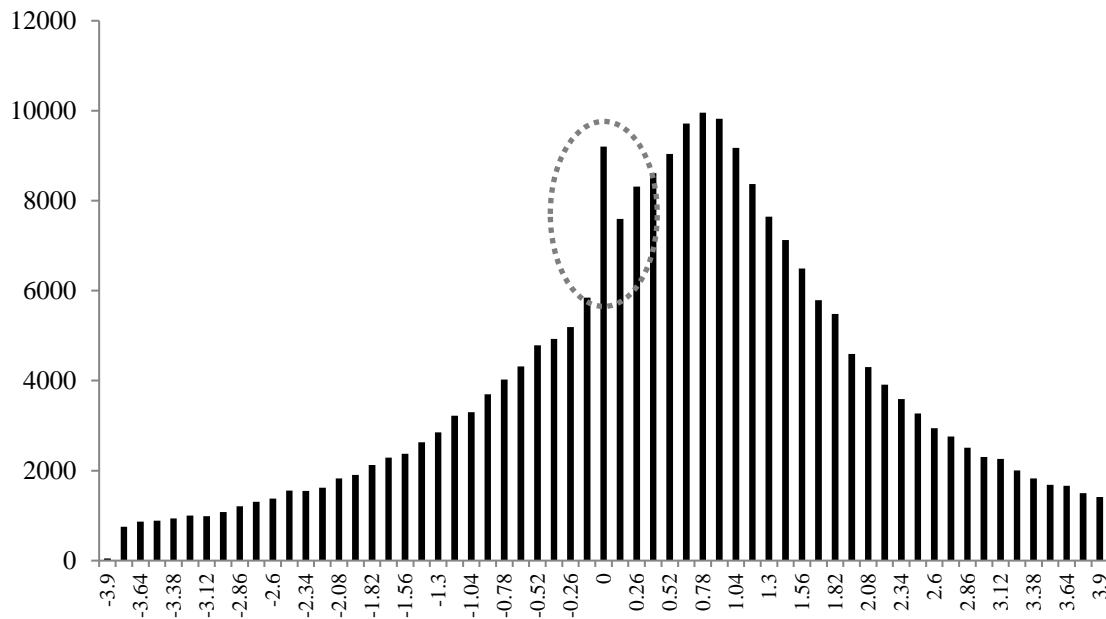


Figure 1. Measure of Misreporting: Kink. This figure describes the distribution of monthly hedge fund returns and indicates that, relative to the surrounding bins, there is a significant spike in the frequency of fund returns reported in the bin just above zero. The figure is based on all funds in the 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).

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. 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 their auditors arrive.

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.

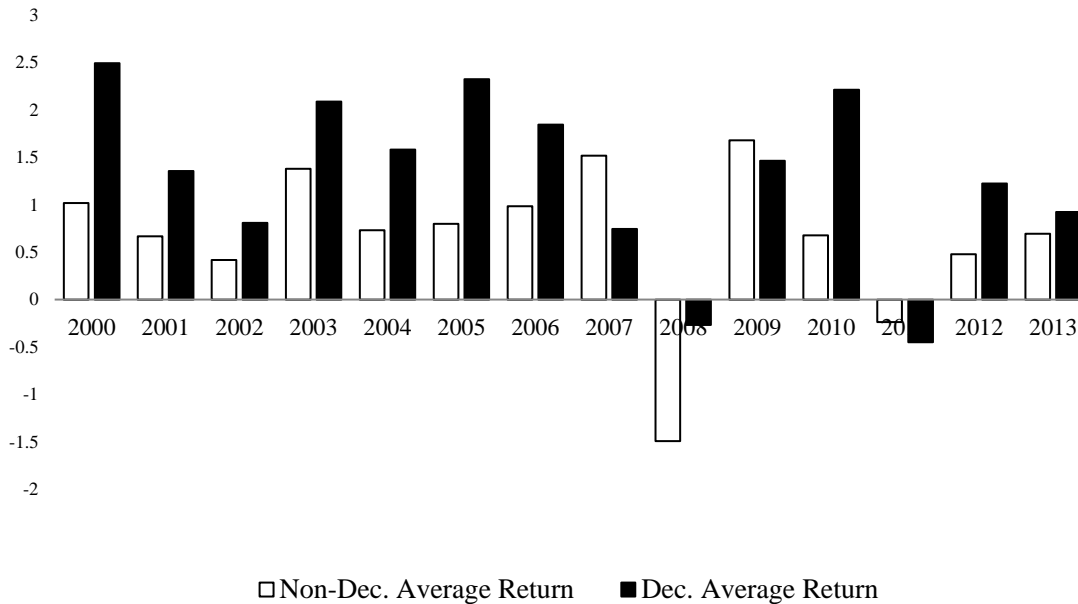


Figure 2. Measure of Misreporting: Cookie Jar Accounting. This figure describes mean hedge fund returns in December and non-December months, and it indicates that mean fund returns in December were higher than mean fund returns in other months in ten of the thirteen years from 2000 to 2013. The figure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

To test for “cookie jar” accounting at each individual fund, I regress each fund’s monthly returns on the seven hedge fund style factors used 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 only appears as the first digit in 4.6% of all observations.¹⁰ 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 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

¹⁰ 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 (Amiran 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.

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. Amiran 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, 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 the distribution of the first digit of monthly returns (1) that is predicted by Benford's Law; (2) of the funds in the Lipper Hedge Fund database from 2000-2013; and (3) of Bernie Madoff's infamous investment fund. 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, using a set of returns compiled from Blodget (2008), the figure shows a whopping 39.6% of Madoff's returns started with a one—a significant deviation.¹¹

¹¹ Figure 3 includes negative returns. The first digit of a return of -.012%, for example, would be recorded as one.

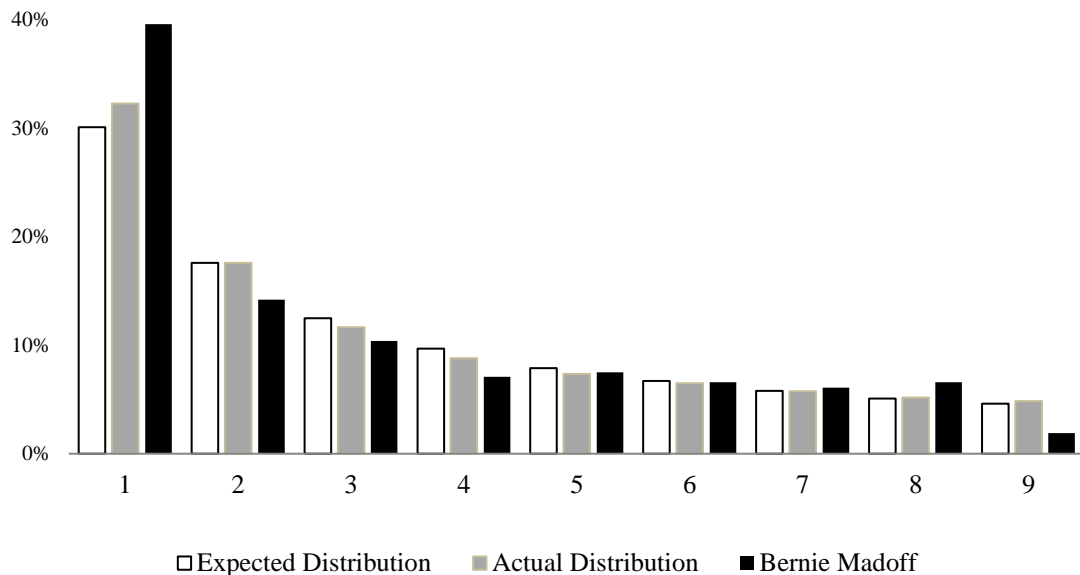


Figure 3. Measure of Misreporting: Benford’s Law. This figure describes the distribution of the first digits of monthly hedge fund returns. The white bar indicates the expected distribution based on Benford’s Law, the grey bar indicates the actual distribution of based on all monthly returns reported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

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 Amiran 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 $1.22\sqrt{N}$.

My analysis includes all three measures as proxies for misreporting by hedge funds. Although the primary results in Tables 3-5 aggregate all measures, Table 9 summarizes these results for each proxy individually.

5. The Effect of Hedge Fund Regulation on Misreporting

As a preliminary inquiry, I ask 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;¹² (3) DFA – 552 control funds and 222 treatment funds.¹³

¹² The total number of observations in the analyses of the Hedge Fund Rule and *Goldstein* differed slightly because of data availability in the Lipper Hedge Fund database.

¹³ 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, I omitted from the DFA analyses in Tables 3 and 5 all funds that withdrew from SEC regulation after *Goldstein* to ensure that the initial set of tests analyzed comparable funds across all three settings. I note, however, that the inclusion of these funds does not materially change the findings.

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 period. I 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 was measured by regressing the fund returns over each period on the Sadka (2006) permanent liquidity variable, where the resulting beta on the Sadka variable was then included in my regressions as a control.

Table 1

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	0.2	0.33	-3.71	0.32	0.3	0.73
Liquidity Sensitivity	21.17	4.09	-2.26	-27.51	-40.01	-2.00
<i>Num. Funds</i>	<i>124</i>	<i>228</i>		<i>108</i>	<i>552</i>	

Panel B.

Variable	<i>Goldstein</i> 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
<i>Num. Funds</i>	<i>55</i>	<i>289</i>		<i>55</i>	<i>102</i>	

Table 1. Descriptive Statistics: All Funds. This table 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 and divides the treatment funds 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 the remain group. The control funds are the same as previously defined.

As shown in Table 1, there were significant differences between the treatment and control groups across several observable characteristics. Relative to the control sample, the treatment funds were significantly younger, more likely to be foreign, had better performance, and had greater return volatility. It is for this reason that, when possible, I use two alternate 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 number of “flags” for 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 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 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.¹⁴ 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, Panel B breaks down the results for the full sample by proxy, and Panel C presents the average number of flags per fund 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 a kink did not significantly differ between the two groups (9% and 8% for the control and treatment funds, respectively).

However, in some instances, it appears that the relative decrease in misreporting by the treatment funds is driven by an increase in misreporting at the control funds rather than a decrease at the treatment funds. On the one hand, changes in the frequency of flags is expected because misreporting by hedge funds varies significantly 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

¹⁴ 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.

was not an option due to the Financial Crisis and its aftershocks. On the other hand, it is also possible that the control funds increased misreporting after regulation—possibly due to the “constrained cop” theory stating that actors are more likely to misbehave when they know the “cop” (i.e., the SEC) is distracted (Kedia and Rajgopal, 2011). If so, perhaps Table 2 provides evidence that regulation actually increased reporting.

To better identify whether movement was occurring at the treatment or control funds, I looked at misreporting by foreign funds, where foreign funds are defined as all funds in the Lipper Hedge Fund database that are located outside the US and do not file Form ADV (as before, the funds must report throughout the entire relevant period). Notably, the trend in misreporting at the foreign funds appears similar to that at the control funds (I confirm that the trend is statistically equivalent in the robustness section). Following the Hedge Fund Rule, foreign and control funds both significantly increased misreporting, whereas misreporting at the treatment funds remained constant. And, following the DFA, there was no significant difference in misreporting at the control and foreign funds, whereas the treatment funds decreased misreporting. Because the control funds follow the same trend as unaffected foreign funds, it appears that the relative decrease in misreporting is driven by changes at the treatment funds rather than the control funds.

Table 2

Panel A.

	Number of Flags – Full Sample					
	Hedge Fund Rule			Dodd-Frank Act		
	Control	Treat.	Foreign	Control	Treat.	Foreign
Before Regulation	0.33	0.43	0.40	0.54	0.65	0.68
After Regulation	0.61	0.44	0.61	0.56	0.31	0.63
t-tests (before vs. after)	-4.32	-0.22	-3.81	-0.50	4.19	1.08

Panel B.

	Flags by Proxy – Hedge Fund Rule					
	Before Regulation			After Regulation		
	Control	Treat.	Foreign	Control	Treat.	Foreign
Kink	0.10	0.15	0.15	0.21	0.14	0.20
Cookie Jar	0.07	0.13	0.08	0.19	0.11	0.20
Benford's Law	0.16	0.15	0.17	0.21	0.20	0.21

	Flags by Proxy – Dodd-Frank Act					
	Before Regulation			After Regulation		
	Control	Treat.	Foreign	Control	Treat.	Foreign
Kink	0.08	0.14	0.22	0.09	0.08	0.19
Cookie Jar	0.31	0.36	0.30	0.34	0.13	0.27
Benford's Law	0.14	0.15	0.16	0.14	0.10	0.17

Panel C.

	Number of Flags – Matched Sample			
	Hedge Fund Rule		Dodd-Frank Act	
	Control	Treat.	Control	Treat.
Before Regulation	0.33	0.33	0.58	0.58
After Regulation	0.66	0.43	0.45	0.30
t-tests (before vs. after)	-3.85	-1.32	1.60	3.46

Table 2. Descriptive Statistics: Frequency of Flags for Misreporting. This table provides descriptive statistics on the frequency of “flags” for misreporting at the treatment and control funds. Panels A and B use the full sample and Panel C includes only the matched sample. Panels A and C reflect the aggregate number of flags, and Panel B presents the disaggregated results for each flag individually. The treatment funds are those that became regulated in accordance with the change in law, the control funds are those that were continuously regulated by the SEC throughout the entire 60-month period surrounding the change in law, and foreign funds are defined as funds located outside the US that do not file Form ADV.

B. Regression Results

Below I present the results of the difference-in-differences regressions showing that regulation reduced misreporting by hedge funds. The dependent variable reflects the number of flags triggered. The results are presented first using the full sample of funds and then using the matched sample. I present the analysis (1) using fixed effects for each fund’s country of incorporation and investment style, and (2) using fund fixed effects.¹⁵ All tests control for the variables noted in Table 1, and standard errors are clustered by fund.

1. Imposition of federal regulation. Using the equation below, I compare the change in misreporting at the treatment funds relative to the change in misreporting at the control funds after

¹⁵ The New Fund variable—and all derivatives thereof, such as the Remain and Withdraw variables in Table 4—is omitted from the regressions that include fund fixed effects because the New Fund dummy is collinear with the fund fixed effects.

the two legal developments that subjected treatment funds to federal regulation: The adoption of the Hedge Fund Rule in 2004 and the adoption of the SEC rules implementing the DFA in 2011. 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 is the interaction term between these two variables.

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

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 also show a significant decrease in misreporting. For example, 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 based across the models). The economic significance for the DFA is similar, but a little greater in some models.

Table 3

	Hedge Fund Rule			
	(1)	(2)	(3)	(4)
	Full Sample		Matched Sample	
Post	0.78*** (0.25)	1.37* (0.741)	0.92*** (0.34)	0.34 (0.96)
New Fund	0.83*** (0.27)		0.55 (0.38)	
Post * New Fund	-1.01*** (0.33)	-1.69** (0.70)	-0.86** (0.42)	-2.11** (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

	Dodd-Frank Act			
	(1)	(2)	(3)	(4)
	Full Sample		Matched Sample	
Post	-0.02 (0.15)	1.19** (0.43)	-0.48 (0.40)	1.35 (0.99)
New Fund	0.88*** (0.21)		0.09 (0.34)	
Post * New Fund	-1.67*** (0.30)	-2.64*** (0.66)	-0.89* (0.48)	-1.76** (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 3. Regression Results: Mandatory Regulation. Difference-in-differences regressions show that the funds subjected to SEC regulation significantly decreased misreporting relative to the control funds. Panel A shows the analysis for the Hedge Fund Rule, and Panel B shows the analysis for the Dodd-Frank Act. All models control for each fund’s mean monthly return, mean log of net asset value, and mean age over the period. I also control for the fund’s return volatility over the period, whether the fund was incorporated in the US, and the sensitivity of the fund to market liquidity. The fund’s sensitivity to liquidity was measured by regressing the fund returns over each period on the Sadka (2006) permanent liquidity variable, and the resulting beta on the Sadka variable was then included as a control. 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. 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, and to 0 for all funds that were continuously regulated by the SEC throughout the entire sample period. All models are run using ordinal logit models, but the results are consistent using OLS. The dependent variable reflects the number of flags triggered. Models (1) and (2) use the full sample of funds, and Models (3) and (4) use the matched sample. Statistical significance of 10, 5, and 1 percent is indicated by *, **, and ***, respectively.

Although the decrease in misreporting for the full sample appears greater after the DFA than the Hedge Fund Rule, it is interesting to note that the magnitude of the decrease in misreporting is more similar using the matched sample. This is consistent with anecdotal evidence that there was a selection effect in the funds that submitted to regulation in accordance with the Hedge Fund Rule; some funds did not submit to oversight because they were waiting on the outcome of the court decision.

To summarize, this evidence suggests that the imposition of federal regulation reduced misreporting at hedge funds. I cannot rule out, however, the possibility that the timing of these regulatory changes happened to coincide with a decrease in misreporting that would have occurred regardless of the regulation. For further evidence, I now turn to DC Circuit’s decision in *Goldstein*.

2. Removal of federal regulation. After the DC Circuit vacated the Hedge Fund Rule in the *Goldstein* case, funds that had been subjected to regulation were given a choice; they could voluntarily remain subject to federal regulation or elect to opt out. I divide these funds into two groups: (1) Those that chose to remain subject federal oversight (“Remain”), and (2) those that chose to exit the federal regulatory regime (“Withdraw”). In the equation below, Remain is set to one if the fund submitted to oversight in accordance with the Hedge Fund Rule and remained regulated, and Withdraw is set one if the fund submitted to oversight in accordance with the Hedge Fund Rule and withdrew post-*Goldstein*. 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 before February 1, 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 Post and Remain, which reflect the change in misreporting for the funds that withdrew (remained) after *Goldstein* relative to the change in misreporting for the control funds during the same period.¹⁶

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

¹⁶ Going forward, I do not include the matched sample because there are more than two groups of funds. I include alternate robustness tests in the final section, however.

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 at 10% in all models. Although statistical significance in this table is lower than in the prior two tables, 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. This suggests that the initial decrease in misreporting was sticky—after regulation, misreporting at the treatment 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.

Table 4

	(1) Full Sample	(2) Full Sample
Post	-0.49** (0.22)	-1.66** (0.72)
Withdraw	-1.15*** (0.44)	
Remain	-0.48 (0.29)	
Post * Withdraw	0.98* (0.51)	1.85* (1.01)
Post * Remain	0.49 (0.37)	0.66 (0.80)
Controls	Yes	Yes
Fixed Effects	Char.	Fund
Observations	892	892
R-squared	0.05	0.55

Table 4. Regression Results: Funds that Withdrew from Regulation. Difference-in-differences regressions show that, after the Hedge Fund Rule was vacated, the funds that withdrew from federal regulation significantly increased misreporting relative to the control funds. All models include the control variables and fixed effects noted in Table 3. Standard errors are clustered by fund. 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 was 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. All models are run using ordinal logit models, but the results are consistent using OLS. The dependent variable reflects the number of flags triggered. Statistical significance of 10, 5, and 1 percent is indicated by *, **, and ***, respectively.

6. Disentangling the Effect of Different Regulatory Components

Having provided evidence that the legal changes I examine here reduced hedge fund misreporting, I now turn to a separate question: *why* did these changes to the law 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.

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.¹⁷ Because of the asset limitation, most ERAs are foreign. Although US advisors must have less than \$150 million in total—a relatively small sum for an advisory firm—foreign advisors need only have less than \$150 million 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 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.¹⁸

The outcome of these changes was that the DFA created three groups of hedge funds, all subject to different changes in regulation. This allows for study of the different regulatory components in isolation. 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

¹⁷ 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.

¹⁸ These funds were already 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 compliance exams for the first time upon their switch to the SEC’s regulatory regime. Each state sets its own compliance requirements, and there are some differences between federal and state compliance requirements. On the whole, however, these differences are minor.

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 to compare the change in misreporting for 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.

$$\begin{aligned} \text{Num. Flags} = & \alpha + \beta_1 \text{Post} + \beta_2 \text{Full Reg.} + \beta_3 \text{Disc. Only} + \beta_4 \text{Inspect. Only} + \\ & \beta_5 \text{Post* Disc. \& Inspect.} + \beta_6 \text{Post*Disc. Only} + \beta_7 \text{Post*Inspect. Only} + \text{Controls} + \varepsilon \end{aligned}$$

The results, presented in Table 5, offer important insights about misreporting at these three groups of funds before the DFA. 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 the full-regulation funds and the disclosure-only funds are not statistically significantly different from each other). Moreover, the level of misreporting at the third group of funds—inspection-only funds—was not statistically different from misreporting at the control funds (confirmed by unreported F-tests).

Table 5

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

Table 5. Regression Results: Misreporting and Type of Regulation. Difference-in-differences regressions show that the funds that became subject to full regulation and those that became subject to only disclosure rules significantly decreased misreporting relative to the control funds. The time period, control group, and control variables are the same as those used for the DFA tests in Table 3. Standard errors are clustered by fund. The variable Full Reg. was set to 1 for all funds that became subject to full regulation, and to 0 otherwise. The variable Disc. Only was set to 1 for all funds that became subject to only disclosure rules, and to 0 otherwise. The variable Inspect. Only was set to 1 for all funds that became subject to only a change in inspections, and to 0 otherwise. All models are run using ordinal logit models, but the results are consistent using OLS. The dependent variable reflects the number of flags triggered. Statistical significance of 10, 5, and 1 percent is indicated by *, **, and ***, respectively.

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 subjected to full regulation (F-tests comparing the coefficients from Table 5 show that decreases in misreporting for these two groups were statistically equivalent). By contrast, the inspection-only funds exhibited no significant change in misreporting relative to the control funds.¹⁹

This result is perhaps surprising. Hedge fund investors are generally considered to be highly sophisticated, and Brown et al. (2008) provided evidence that many—presumably most—hedge fund investors already had access to the information in Form ADV before mandatory regulation. Moreover, the information in Form ADV is related to the advisor’s governance, not its financial performance.

C. Disclosure Mechanism

To understand why disclosure of governance information reduced 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 imposition of governance disclosure requirements may have reduced misreporting. First, upon being required to publicly disclose whether they conformed

¹⁹ 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.

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.²⁰ 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 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. I then partitioned the newly regulated funds into two

²⁰ 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.

groups—those that initiated audit procedures and those that did not—and tested whether the funds that initiated audit procedures following regulation had greater decreases in misreporting than funds that did not. Using the equation below, I compare the two groups of newly regulated funds to the control funds.

$$\begin{aligned} \text{Num. Flags} = & \alpha + \beta_1 \text{Post} + \beta_2 \text{Initiate} + \beta_3 \text{No Change} + \beta_4 \text{Post} * \text{Initiate} \\ & + \beta_5 \text{Post} * \text{No Change} + \text{Controls} + \varepsilon \end{aligned}$$

The results, shown in Table 6, support the theory that the decrease in misreporting following regulation was driven by changes in governance. Following the Hedge Fund Rule, unreported OLS models indicate that 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. By comparison, the funds that did not change their audit procedures did not have a significant change in misreporting relative to the control funds. Although the pattern is less pronounced following the DFA—indeed, the group of funds that initiated audit procedures and the group that did not both experienced statistically significant relative decreases in misreporting after regulation—this is not surprising. Funds can make any number of governance changes, and it is likely that the funds that did not initiate audit procedures made other unobservable changes in behavior.

Table 6

	Hedge Fund Rule		Dodd-Frank Act	
	(1) Full Sample	(1) Full Sample	(1) Full Sample	(1) Full Sample
Post	0.75*** (0.25)	1.08 (0.76)	-0.02 (0.15)	1.00** (0.41)
Initiate Audit	1.07*** (0.31)		1.24* (0.64)	
No Change in Audit	0.64** (0.32)		0.87*** (0.19)	
Post * Initiate Audit	-1.63*** (0.42)	-3.07*** (0.89)	-2.37** (1.06)	-4.02* (2.15)
Post * No Change in Audit	-0.57 (0.40)	-0.88 (0.80)	-1.66*** (0.24)	-2.75*** (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 6. Regression Results: Auditing and Misreporting. Difference-in-differences regressions show that the newly regulated funds that initiated audit procedures significantly decreased misreporting relative to the control funds. The time period, control group, and control variables are the same as those used in Table 3. Standard errors are clustered by fund. The variable Initiate Audit was set to 1 for all newly regulated funds that initiated audit procedures following regulation, and to 0 otherwise. The variable No Change in Audit was set to 1 for all newly regulated funds that did not initiate audit procedures following regulation, and to 0 otherwise. All models are run using ordinal logit models, but the results are consistent using OLS. The dependent variable reflects the number of flags triggered. All models use the full sample of funds. Statistical significance of 10, 5, and 1 percent is indicated by *, **, and ***, respectively.

Despite obvious limitations, this analysis provides important empirical evidence that is consistent with anecdotal evidence that the hedge fund disclosure requirements spurred funds to make internal governance changes, thus inducing them 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 performance. The result is also consistent with experimental studies finding subtle but powerful psychological dynamics triggered by disclosure requirements (Guttentag et al., 2008). Based on recent literature indicating that riskier funds provide their investors with less voluntary disclosure related to fund risk (Cassar et al., 2016), it is also possible that the disclosures on Form ADV were more important for higher risk funds because these funds may have been less likely to previously disclose the information.

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. Non-randomness of Treatment Funds

Although the evidence in Table 5 indicates that the funds subject to full regulation and those subject to the disclosure-only regime 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. 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 sensitivity to liquidity. After applying these filters, I have 50 disclosure-only funds and 50 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-difference regressions comparing misreporting between these groups. The analysis again shows that the decrease in misreporting after regulation was statistically equivalent for both groups.

Table 7

Panel A.

	Dodd-Frank Act	
	Disclosure-Only	Full-Regulation
Before Regulation	0.46	0.46
After Regulation	0.28	0.30
t-tests (before vs. after)	<i>1.56</i>	<i>1.47</i>

Panel B.

	(1) Matched Sample	(2) Matched Sample
Post	-0.69 (0.61)	-2.75 (2.47)
Disc. Only	0.16 (0.55)	
Post * Disc. Only	0.26 (0.72)	0.86 (1.62)
Controls	Yes	Yes
Fixed Effects	Char.	Fund
Observations	200	200
R-squared	0.21	0.71

Table 7. Robustness: Matching based on Type of Regulation. This analysis matches funds that became subject to only disclosure rules with funds that became subject to full regulation. Panel A provides descriptive statistics for the matched sample, and Panel B provides difference-in-difference regressions comparing the change in misreporting for the treatment and control funds. The time period and control variables are the same as those used in the DFA tests in Table 3. Standard errors are clustered by fund. The variable Disc. Only was set to 1 for all funds that became subject to only disclosure rules, and to 0 for all the funds subject to full regulation. The variable of interest, Post*Disc. Only, is not statistically significant in any of the models, indicating that both groups had statistically equivalent decreases in misreporting. The dependent variable reflects the number of flags triggered. Statistical significance of 10, 5, and 1 percent is indicated by *, **, and ***, respectively.

2. *Quasi-discontinuity analysis.* 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 made 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.²¹ The resulting sample is limited, but I include 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.

²¹ Foreign advisors are eligible for the disclosure-only regime if they have less than \$150 million in assets from US investors (“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.

Table 8

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

Table 8. Robustness: Quasi-discontinuity Design. This table compares the disclosure-only and full-regulation funds, but uses only funds that were close to the eligibility threshold for the disclosure-only regime. Funds are eligible for the SEC’s disclosure-only regime if the fund advisor has \$100-\$150 million in US assets. If the fund advisor has more than \$150 million in US assets, the fund is automatically subject to full regulation. Hence, the variable Below Threshold was 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. Only US based funds are included. The analysis indicates that both groups had statistically equivalent decreases in misreporting. The time period and control variables are the same as those used in the DFA tests in Table 3. Standard errors are clustered by fund. All models are run using ordinal logit models, but the results are consistent using OLS. The dependent variable reflects the number of flags triggered. Statistical significance of 10, 5, and 1 percent is indicated by *, **, and ***, respectively.

B. Non-Randomness of Control Sample

By definition, the control funds used in the primary analysis were not chosen at random. They were chosen because they had no change in their regulatory status following the changes in law—this does not necessarily mean, however, that they were unaffected by the regulation. Indeed, as discussed earlier, prior literature on the “constrained cop” theory suggests that these funds may

have actually increased misreporting following regulation, because they knew the SEC was likely to be distracted by the newly regulated funds (Kedia and Rajgopal 2011).

To formally test for evidence of the constrained cop theory, I run placebo tests comparing the changes in misreporting at the control funds relative to the changes in misreporting at foreign funds. All tests, which use the same time period and control variables described previously, compare the control funds used in the primary analysis with the full set of foreign funds in the Lipper Hedge Fund database, where foreign funds are defined as those located outside the US that do not file Form ADV. Although I do not report the results for concision, I find that the change in misreporting for foreign and control funds was comparable following both the Hedge Fund Rule and the DFA. These placebo tests thus provide no evidence that control funds changed misreporting in response to the change in law.

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 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 test for consistency, Table 9 presents a summary of the mains results, broken down by proxy. The kink results in the most

consistently significant findings. By contrast, the flag for cookie jar accounting, when significant, results in the most statistically significant findings.²²

²² Consistent with the results in the primary analysis, none of the individual flags for misreporting are significant for the funds that remain regulated by the SEC in the *Goldstein* analysis.

Table 9

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.

Table 9. Robustness: Results by Proxy. This table 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.

8. Conclusion

My evidence indicates that regulation, and particularly mandatory disclosure rules, 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 to the information in question. To understand why mandated disclosure of governance information would lead to this 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: Fund Variable Definitions

	Theoretical Definition	Empirical Definition
Hedge Fund Rule		
Treatment Funds	Funds that became regulated by the Hedge Fund Rule.	Previously unregistered funds that registered 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 in the months from June 2002 through May 2007.
Regulatory Event	The SEC's adoption of the rules to implement the Hedge Fund Rule (Dec. 2004).	
Goldstein Opinion		
Withdraw Funds	Funds that 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 decided not to appeal the court's decision and Jan. 31st, 2007 (the deadline to withdraw without penalty).
Remain Funds	Funds that 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.
Control Funds	Funds not affected by the <i>Goldstein</i> decision.	Funds continuously registered with the SEC in the months from March 2004 through March 2009.
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 <i>Goldstein</i> opinion.	

Dodd-Frank Act

Treatment Funds	Funds that became subject to full regulation by the DFA. The initial tests only include Full-Regulation funds (see below) so that the DFA analysis will be comparable to the analysis of the Hedge Fund Rule.	Previously unregistered funds that registered as Registered Investment Advisers with the SEC at any point from October 2011–March 2012.
Control Funds	Funds not affected by the DFA.	Funds continuously registered with the SEC in the months from Jan. 2009 through Dec. 2013.
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 Registered Investment Advisers 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 before March 31 st , 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 (i.e., a US adviser can only have up to \$150M in assets, whereas a foreign adviser can only have up to \$150M in assets from US investors).
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 with over \$100M that were previously registered as a Registered Investment Adviser 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 (June 2011).	
