

Regulatory Oversight and Return Misreporting by Hedge Funds*

Stephen G. Dimmock¹ and William C. Gerken²

¹Nanyang Technological University and ²University of Kentucky

Abstract

We use Securities and Exchange Commission (SEC) rule changes to show that regulatory oversight reduces return misreporting by hedge funds. Specifically, we use a 2004 rule change that expanded SEC oversight of hedge funds and the 2006 revocation of this rule. Differences-in-differences tests show that, following the rule change, misreporting by newly regulated funds decreased. After revocation, funds that exited the regulatory system increased misreporting relative to funds that remained registered. Placebo tests show no change in misreporting by foreign funds exempt from the rule change. We show that regulatory oversight increased the level of flows and decreased the sensitivity of flows to underperformance.

JEL classification: G20, G23, G28, K22

1. Introduction

In response to several highly publicized cases of return misreporting by hedge funds, the Securities and Exchange Commission (SEC) has advocated greater regulatory oversight of hedge funds.¹ The hedge fund industry has generally opposed greater regulation, arguing that it is costly, ineffective, and unnecessary given the sophistication of hedge fund

* We are grateful to Thorsten Beck (editor), two anonymous referees, Itzhak Ben-David, Nick Bollen, Alice Bonaime, Niki Boyson, Chris Clifford, Jesse Ellis, Brian Henderson, Huasheng Gao, Joseph Gerakos, Jiekun Huang, Chuan Yang Hwang, Ravi Jain, Russell Jame, Petri Jylha, Clive Lennox, Kasper Meisner Nielsen, Wenlan Qian, Veronika Pool, Anand Srinivasan, Yuxi Wang, Chishen Wei, Scott Weisbenner, Lei Zhang, and participants at the American Law and Economics Conference, Berkeley Multidisciplinary Conference on Fraud and Misconduct, CFA-FAJ-Schulich Conference, European Finance Association, Federal Reserve Financial Stability Conference, Financial Management Association, Nanyang Technological University, National University of Singapore, Paul Woolley Conference, and SFS Cavalcade for helpful comments. Clive Lennox deserves special thanks. The authors thank the Paul Woolley Centre for the Study of Capital Market Dysfunctionalities for financial support.

1 See <http://www.sec.gov/news/studies/hedgefunds0903.pdf>.

investors.² Absent from this debate are empirical studies of the causal effects of regulatory oversight on misreporting. In this article, we present empirical evidence that regulatory oversight reduces return misreporting by hedge funds.

Due to concerns about return misreporting, among other issues, in 2004 the SEC proposed Rule IA-2333. This rule required most hedge fund advisors to register with the SEC, and gave the SEC regulatory authority over these advisors. The SEC argued this would deter misreporting as it allowed regulators “... to conduct examinations of the hedge fund advisor... identify practices that may be harmful to investors, and provide a deterrent to unlawful conduct.”³ In addition to permitting SEC enforcement exams, the rule also required hedge funds to document their performance calculations and to follow detailed compliance procedures. Rule IA-2333 caused an immediate increase in the proportion of hedge fund advisors subject to SEC oversight. In June 2006 a Federal Court revoked Rule IA-2333, following which, many hedge fund advisors were permitted to deregister at the end of 2006. Importantly, many of the newly registered advisors voluntarily remained registered after the court decision.

We use the introduction and subsequent revocation of Rule IA-2333 to test whether regulatory oversight affects return misreporting by hedge funds. We generate several misreporting flags based on suspicious return patterns identified in prior studies (Agarwal, Daniel, and Naik, 2011; Bollen and Pool, 2009; Bollen and Pool, 2012). Using these return misreporting flags, we conduct differences-in-differences tests around the introduction of Rule IA-2333, that is, we compare the funds that were forced to register⁴ in response to the rule (IA-2333 funds) with the funds that were required to be registered prior to the rule (Baseline funds). Prior to the rule change, while they were outside the regulatory system, IA-2333 funds had significantly higher misreporting. Following the rule change, misreporting by IA-2333 funds decreased and was not significantly different from that of Baseline funds. Further, the economic magnitudes of the coefficients are large: implying a 30.4% decrease in return misreporting for our composite flag. These results are robust to the inclusion of many control variables, including fund fixed effects.

Next, we extend the differences-in-differences models to include the revocation of Rule IA-2333 and the subsequent deregistration of many funds. For these tests, we divide the IA-2333 funds into those that deregistered after revocation (Deregister funds) and those that did not (Remain funds). The results show that, relative to Baseline funds, return misreporting by Deregister funds was higher before registration. Return misreporting by Deregister funds declined following registration, and then increased following deregistration. Thus, although mandatory oversight imposed costs on all of the IA-2333 funds, the benefits were realized primarily by the investors in the Deregister funds.

We also conduct two placebo tests. First, we show that there are no significant differences-in-differences effects in an earlier period when there was not a regulatory change. Second, we show there are no significant differences-in-differences effects in a placebo sample of foreign funds that are not subject to US regulatory jurisdiction (non-US domiciled funds with non-US advisors). Further, we also show the results are not driven by survival

2 See <http://www.sec.gov/rules/proposed/s73004/mfa101804.pdf>.

3 See <http://www.sec.gov/rules/final/ia-2333.htm#IIA>.

4 Technically, fund advisors, not funds, register with the SEC. For ease of exposition, we sometimes refer to hedge funds as registering.

biases, nor are the results driven by changes in portfolio liquidity or other changes in portfolio strategies.

We next turn to the relation between regulatory oversight and flows, to test whether investors' actions are consistent with the implications of the differences-in-differences results. If investors believed that Rule IA-2333 would decrease return misreporting, we would expect regulated funds to have higher flows. We would also expect greater skepticism of the returns reported by unregulated funds, which would imply lower inflows following good performance and higher outflows following poor performance. Consistent with this intuition, relative to the Baseline funds, IA-2333 funds have lower levels of flows and greater flow-performance sensitivity for poor returns before registration; but these differences disappear during the Mandatory period when the IA-2333 funds are also registered. These results suggest that investors value regulatory oversight and view the returns reported by registered funds as more credible. The results also point, however, to a disadvantage of mandatory oversight; funds can no longer credibly signal their quality to investors, and then benefit from higher inflows.

Our findings have important policy implications, as prior studies show that return misreporting directly harms investors in several ways. First, [Bollen and Pool \(2012\)](#) show that return misreporting is significantly related to both fraud and investor lawsuits. Second, misreporting allows hedge fund advisors to expropriate investors through undeserved fees. Third, misreporting transfers wealth between investors, usually in ways that benefit the fund advisors' carried interest. For example, [Jylha \(2011\)](#) shows that hedge funds strategically misreport returns so that shares are overvalued in periods with net inflows and undervalued in periods with net outflows. Fourth, misreporting distorts the allocation of capital, by inflating investor perceptions of managerial ability. Fifth, [Ben-David, Franzoni, Landier, and Moussawi \(2013\)](#) show that hedge funds manipulate stock prices to improve their reported returns. Thus misreporting distorts asset prices. In addition to its direct harm, misconduct can cause significant indirect harm: [Giannetti and Wang \(2014\)](#) show that misconduct by one firm reduces investors' willingness to invest in other firms. Given the harm caused by return misreporting, understanding whether regulatory oversight affects misreporting is important.

To our knowledge, [Cumming and Dai \(2010\)](#) is the only other paper that studies return misreporting and hedge fund regulation. They explore cross-country variation in restrictions on hedge funds' marketing channels, and find a positive association between misreporting and the use of wrap accounts. Our article differs in that we study regulatory changes that directly affect valuation practices and regulatory examinations. Further, because of the regulatory changes in our sample we can use a differences-in-differences approach to cleanly identify the effects of regulatory oversight on misreporting.

Several prior studies use information from the Form ADV filings required by Rule IA-2333. [Dimmock and Gerken \(2012\)](#) show this information can predict fraud by investment advisors. Unlike [Dimmock and Gerken \(2012\)](#), we show that regulatory oversight reduced return misreporting, rather than testing the usefulness of the required disclosures. [Brown et al. \(2012\)](#) show that information in Form ADV filings is correlated with hedge funds' misreporting of past legal and regulatory violations. [Brown et al. \(2008, 2009\)](#) use information from a February 2006 cross-section of Form ADV filings and show this information can predict returns and fund failures. Our article differs from [Brown et al.](#) in two ways. First, we focus on return misreporting whereas they explore operational risk, which they define using an indicator variable for funds whose advisor reports any prior legal or

regulatory violations. Second, due to the cross-sectional nature of their data, Brown *et al.* are unable to distinguish advisors that registered in response to Rule IA-2333 from those that were already registered. Thus, they are unable to test whether Rule IA-2333 changed hedge funds' behavior. Our panel data allow us to identify when each advisor first registered, and to test whether regulatory oversight caused hedge funds to change their behavior.

This article has implications for ongoing policy issues: Although Rule IA-2333 was revoked, the recent Dodd-Frank Act restored SEC oversight of hedge funds. Despite this revival of the regulatory changes in Rule IA-2333, no prior academic study has tested the effect of the earlier regulatory changes. Empirical support for reviving this regulatory change is important, as many have argued this regulatory oversight is unlikely to decrease misreporting given the sophistication of hedge fund investors. Our results show, however, that regulatory oversight can affect behavior even in a market characterized by sophisticated investors.

2. Background

The Investment Advisers Act of 1940 requires all investment advisors with at least 15 US clients and at least \$25 million in assets under management to register with the SEC. Prior to Rule IA-2333, many hedge fund advisors avoided registration by counting each fund as a client, rather than counting the fund's investors. This was permitted, provided the fund accepted only qualified investors, had fewer than 100 investors, and did not advertise or conduct general solicitations. Despite this exemption, the SEC reported that a majority of hedge fund advisors were registered prior to Rule IA-2333⁵ (advisors were previously registered for reasons such as managing mutual funds, advising 15 or more funds, or voluntarily forgoing the exemption).

In a September 2003 report, the SEC argued for increased oversight of hedge funds, saying "The lack of independent checks on a hedge fund advisor's valuation of a hedge fund's portfolio securities is among the most serious concerns we have identified."⁶ The report also argued that SEC examinations, stricter recordkeeping requirements, and tougher compliance standards would both deter misreporting and facilitate its detection.

In July 2004, the SEC proposed Rule IA-2333, which eliminated the option to count a fund, instead of its investors, as the client (eliminating the registration exemption used by hedge funds). During the comment period that followed the proposal, many people voiced strong objections: The Managed Fund Association reported that 73% of its members opposed the proposal.⁷ Two of the five SEC commissioners publicly opposed the proposal, arguing, among other things, that it was unlikely to reduce return misreporting (see Atkins, 2006). In congressional testimony, Alan Greenspan stated "Even should SEC's proposed risk evaluation surveillance of hedge funds detect possible irregularities, which I doubt frankly, those irregularities will likely be idiosyncratic and of mainly historic interest."⁸ Thus, even from the beginning, many thought the rule would fail to deter misreporting.

5 See Section III.1.A of <http://www.sec.gov/news/testimony/ts051606sfw.htm>.

6 See page 79 of <http://www.sec.gov/news/studies/hedgcfunds0903.pdf>.

7 See <http://www.sec.gov/rules/proposed/s73004/mfa101804.pdf>.

8 See <http://www.gpo.gov/fdsys/pkg/CHRG-108shrg98356/html/CHRG-108shrg98356.htm>.

After the comment period, Rule IA-2333 was passed in October 2004, posted in December 2004, and became effective in February 2005. The rule mandated regular SEC examinations of hedge funds and required advisors to follow strict recordkeeping requirements. In June 2006, a Federal Court revoked Rule IA-2333 in the case *Phillip Goldstein, et. al. v. the Securities and Exchange Commission*. On November 24 2006, the SEC allowed the deadline for appeal to expire. Advisors that had registered in response to Rule IA-2333 were given the option to deregister, with a deadline of January 31 2007; the majority of new registrants declined this option and voluntarily remained registered. SEI (2007) discusses several reasons why these advisors remained registered, including changes in investor expectations and having already paid the fixed costs of registration.

We define the 30-month period from July 2004 through December 2006 as the Mandatory period: the period when we expect Rule IA-2333 to affect return misreporting. The Mandatory period begins in July 2004, the month following the SEC's proposal of Rule IA-2333. Although Rule IA-2333 did not immediately go into effect, we believe the proposal date is the relevant starting point. By July 2004 hedge funds could reasonably anticipate that they would soon face regular SEC compliance exams (and stricter recordkeeping requirements). Given that SEC compliance exams audit all historically reported returns we anticipate that unregistered funds would immediately reduce return misreporting, knowing that their currently reported returns would be audited. Internet Appendix Table 1⁹ shows that the results are similar if we use the October 2004 passage or the February 2005 effective date of the rule as the start date, rather than the proposal date. Because the SEC had publicly discussed some of the regulatory changes before the formal proposal of Rule IA-2333, Internet Appendix Table 1 also shows the results are robust to using an earlier start date, which includes all of 2004 in the Mandatory period, so as to allow for greater anticipation of the rule change. We also note that measurement error in the definitions of the periods would bias against finding significant results.

The Mandatory period ends in December 2006, when the recordkeeping requirements of Rule IA-2333 ended for most funds. After the courts voided Rule IA-2333 in June 2006, the effect on the newly registered advisors was initially unclear; to withdraw from registration advisors must apply for and receive permission from the SEC. Eventually, the SEC announced a simplified deregistration process. This process, however, required advisors to follow the recordkeeping rules until the end of the current fiscal year. As December is the fiscal year end for most funds, they were subject to the recordkeeping requirements until December 2006. The funds were also subject to compliance exams through January 2007 (all results are robust to using January 2007 as the end date).

Rule IA-2333 did not restrict hedge funds' investment activities or trading strategies. Nor did it require disclosure of portfolio holdings. The rule did, however, change the regulatory environment in several ways. First, the SEC regularly examines registered advisors. One of the key justifications for the rule was that "the Commission lacks the authority to examine many hedge fund advisors' books and records or conduct on-site inspections of hedge fund advisor operations, which could reveal instances of mispricing."¹⁰ The US Government Accountability Office reported that in 2006, the SEC examined 321 hedge fund advisors, issued deficiency letters to 294 (91.6%), and uncovered 23 cases of fraud

9 The Internet Appendix is available at <http://tinyurl.com/mxk6yyx>.

10 See page 80: <http://www.sec.gov/news/studies/hedgefunds0903.pdf>.

(7.2%).¹¹ The number of deficiencies and frauds uncovered in this single year suggests that misconduct was common, and also supports the SEC's assertion that examinations accelerate the detection of fraud and thus create an additional deterrent beyond that of preexisting anti-fraud laws.¹²

Second, Registered Investment Advisors are subject to recordkeeping requirements,¹³ which include strict document retention rules and require funds to document performance calculations on a security-by-security basis. Prior studies suggest this focus on performance calculations is important: *Cassar and Gerakos (2011)* show that hedge funds with greater control over valuations have higher return misreporting. *Cici, Kempf, and Puetz (2013)* compare Center for Research in Security Prices (CRSP) stock prices with the prices hedge funds report in their 13F filings. They find significant discrepancies for 25% of hedge funds, with a conditional average overvaluation equal to 2.5% of fund value. Cici et al.'s results are especially striking given that only the largest hedge funds file Form 13F, and there are discrepancies even for highly liquid stocks. Cici et al. also find a 50% decrease in pricing discrepancies following Rule IA-2333, which they interpret as consistent with the main findings of our article. Further, the SEC head of Compliance Inspections and Examinations specifically stated that cherry-picking valuation procedures is one of the most common problems identified during SEC examinations of hedge funds: "We'll see people from quarter to quarter or year to year changing their valuation methodology and managing to pick the one that leads to the highest valuation in their fund."¹⁴

Third, Rule IA-2333 required each advisor to adopt a written compliance code, appoint a Chief Compliance Officer, and implement certain compliance procedures including procedures related to valuation practices. Fourth, Registered Investment Advisors are required to hold clients' assets with a qualified custodian (e.g., a bank or registered broker-dealer). If the advisor or an affiliated firm (i.e., a firm under common control) is the custodian, there are additional requirements, including additional documentation of valuation methods and at least one surprise inspection per year. Finally, Registered Investment Advisors are required to disclose information about their operations, conflicts of interest, and past legal and regulatory violations.

3. Data and Sample Description

This study uses data from a panel of Form ADV filings, and from the TASS and BarclayHedge databases. We merge these data sets and create one observation per fund period. The periods are defined based on the regulatory changes. The Mandatory period includes the 30-month period from July 2004 to December 2006. The Pre-Mandatory (January 2002 to June 2004) and Post-Mandatory periods (January 2007 to June 2009)

11 See <http://www.gao.gov/assets/280/271478.html>.

12 See page 10 of the Executive Summary: <http://www.sec.gov/news/studies/hedgefunds0903.pdf>.

13 The required records include: performance claims, all written communication including internal e-mails, advertising (including reported returns), bank records, bills and statements, financial statements, all personal transactions of representatives and principals, and written supervisory procedures.

14 See <http://www.bloomberg.com/news/2014-09-22/sec-finds-misrepresentations-by-hedge-funds-bowden-says.html>.

include the 30 months before and after the Mandatory period, respectively. Internet Appendix Table 1 shows that the results are robust to alternative definitions of the periods.

3.1 SEC Registration Status and Form ADV Sample

Registered hedge fund advisors must file Form ADV with the SEC. We use a panel of Form ADV filings to determine when each advisor first registered, and whether the advisor de-registered. From these filings, we also obtain information about the advisors' business operations. Historical monthly Form ADV filings, beginning in May 2007, are available on the SEC's website.¹⁵ We combine these publicly available filings with a unique panel of all Form ADV filings from August 2001 to August 2006, described in [Dimmock and Gerken \(2012\)](#), to create a panel covering August 2001 to December 2011. This panel includes the filings of defunct advisors and should not be subject to survivorship bias.

Even following Rule IA-2333, some advisors did not register. First, funds with lockup periods of at least 2 years were exempt. [Aragon, Liang, and Park \(2014\)](#) show that 2% of offshore funds and 0.5% of domestic funds appear to have changed their lockups to avoid registration; this provides some evidence of advisors actively avoiding registration, but the magnitude appears too small to drive our results. Second, some advisors that were required to register failed to comply.¹⁶ Excluding these advisors, however, would only bias toward our findings if, had they registered, they would have significantly increased misreporting. Although we cannot directly disprove this possibility, it seems unlikely.

3.2 Hedge Fund Data

We obtain hedge fund data from the TASS and BarclayHedge databases, and merge share classes between the databases following the algorithm of [Joenvaara, Kosowski, and Tolonen \(2013\)](#). First, we standardize the advisors' names by removing text about share class, domicile, currency, leverage, and legal structure. We then merge the databases using these standardized names, as well as share class, domicile, and currency. Second, for each advisor, we identify share classes whose returns have a correlation of 0.99 or higher, and keep the share class with the longest return history (or by net asset values if the return histories are of equal length). To avoid complications from currency conversions when calculating the misreporting flags, we restrict the sample to funds that report in US dollars. [Joenvaara, Kosowski, and Tolonen \(2013\)](#) report that the levels of return misreporting are similar among the five most commonly used hedge fund databases (which include TASS and BarclayHedge).¹⁷

3.3 Merged Data Set

The BarclayHedge database provides each advisor's SEC number, which can be linked directly to the Form ADV data. For those funds that report only to TASS, we match to Form ADV using the advisors' standardized names, and verify these matches based on location, assets under management, stated client type, and the private fund data from Schedule D of

15 See <http://www.sec.gov/foia/iareports/inva-archive.htm>.

16 Unfortunately, we cannot clearly separate the advisors who failed to register from those who were not required to register (because they had fewer than 15 US-based investors or managed less than \$25 million).

17 Robustness tests show that the results are not significantly different within the TASS and within the BarclayHedge subsamples.

Form ADV. The merged Form ADV–TASS–BarclayHedge data set contains 900 advisors that registered by the end of 2006. Of these 900 advisors, 559 were registered prior to the Rule IA-2333 deadline. To avoid potential selection biases, however, the Baseline sample includes only the 373 advisors that were clearly required to register prior to Rule IA-2333, and exclude the 186 advisors that appear to have voluntarily registered (Table VI shows the results are robust to including these in the analysis). We classify the remaining 341 advisors who registered in response to the rule change as IA-2333 registrants. Of the IA-2333 registrants, 100 chose to deregister after Rule IA-2333 was revoked, whereas 241 remained registered. The proportions in our sample are similar to those reported by the SEC, who also found that a majority of hedge fund advisors were registered prior to the rule change,¹⁸ and that a minority of the IA-2333 advisors deregistered.¹⁹

To ensure a sufficient time series to estimate the misreporting flags, we follow Bollen and Pool (2012) and require at least 24 months of returns per fund period. A potential concern is that excluding fund periods with fewer observations could bias the results. For example, suppose that some IA-2333 funds that misreport chose to exit the return databases after the rule change; this could then decrease the observed misreporting during the Mandatory period. We address this concern in three ways: First, the specifications that include fund fixed effects show that our results occur within fund, not due to changes in the sample of funds. Second, any alternative story based upon non-survival would need to explain both the (relative) decrease in misreporting following registration and the (relative) increase in misreporting following deregistration. Finally, and perhaps most simply, Internet Appendix Table 2 shows that the survival rates of IA-2333 and Baseline funds are not significantly different (nor are the survival rates of Deregister and Remain funds significantly different). After filtering out the funds that do not have sufficient return data, our sample includes 1,535 funds managed by 714 unique advisors.

3.4 Variables and Summary Statistics

Table I summarizes the characteristics of the advisors in the merged data set. The table reports results separately for the IA-2333 advisors and for the Baseline advisors. The third column reports significance tests of the differences between the two groups. There is one observation per advisor, and all variables are reported as of February 2006 (the deadline for newly regulated firms to file Form ADV).

US Advisor is equal to one if the advisor's "principal office and place of business" is in the United States. Advisor Age is based on the earliest reported return date across all of the advisor's funds, and is reported in years. Advisor Total AUM is the total assets under management for the advisor, including non-hedge fund portfolios. Primarily Hedge Fund is equal to one if 75% or more of the advisor's clients are hedge funds. Table I shows that Baseline advisors are more likely to be US based, are older, and manage more funds, total assets, and non-hedge fund assets.

Moving from advisors to funds, Table II compares IA-2333 and Baseline hedge funds. There is one observation per fund, and the values are for the Pre-Mandatory period. There are 650 funds managed by IA-2333 advisors and 885 funds managed by the Baseline

18 See Section III.1.A of <http://www.sec.gov/news/testimony/ts051606sfw.htm>.

19 See <http://www.gao.gov/assets/280/271478.html>.

Table I. Summary statistics of hedge fund advisors

This table presents summary statistics of the 714 hedge fund investment advisors in the merged ADV–TASS–BarclayHedge sample. Averages are reported separately for advisors that registered in response to Rule IA-2333 (IA-2333) and for those that were already registered (Baseline). All values are as of February 2006. US Advisor equals one if the advisor’s principal office and place of business is in the USA. Advisor Age is the advisor’s age in years. Advisor Total AUM is the total assets under management for the investment advisor, including non-hedge fund assets. Primarily Hedge Fund equals one if 75% or more of the advisor’s clients are hedge funds. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels based on Welch’s *t*-test.

	IA-2333	Baseline	difference
Advisors	341	373	
US Advisor	0.664	0.879	−0.215***
Advisor Age (years)	6.7	8.1	−1.4***
Advisor Total AUM (\$mil)	806.3	9,983.4	−9,177.1***
Primarily Hedge Fund	0.708	0.160	0.549***

Table II. Summary statistics of hedge funds

This table summarizes fund characteristics. Averages are reported separately for funds whose advisor first registered in response to Rule IA-2333 (IA-2333) and for funds whose advisor was already registered (Baseline). US Domiciled equals one if the fund is domiciled in the USA. Fund NAV is the net asset value of the funds in millions. Fund Age is the fund age in years. Return is the monthly return. Standard Deviation is the standard deviation of the monthly returns. Alpha is the estimated monthly alpha from the [Fung and Hsieh \(2001\)](#) seven-factor model. Liquidity β is the loading on the liquidity risk factor of [Sadka \(2010\)](#) in the augmented [Fung and Hsieh \(2001\)](#) model. Flows is quarterly imputed flows. There is one observation per fund, and values are for the Pre-Mandatory period (January 2002 to June 2004). The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels based on Welch’s *t*-test.

	IA-2333	Baseline	difference
Funds	650	885	
US Domiciled	0.344	0.486	−0.141***
Fund NAV (\$mil)	104.3	158.3	−54.0***
Fund Age (years)	5.3	5.4	−0.1
Return	0.009	0.008	0.002***
Standard Deviation	0.029	0.026	0.003**
Alpha	0.007	0.005	0.002***
Liquidity β	0.013	0.004	0.009
Flows	0.027	0.001	0.025***

advisors. US Domiciled equals one if the fund is domiciled in the USA.²⁰ Fund Age is reported in years. Fund NAV is the fund reported net asset value. Return and Standard Deviation are the average monthly return and standard deviation. Fund Alpha is the monthly alpha estimated with the [Fung and Hsieh \(2001\)](#) seven-factor model.²¹ Liquidity β is the fund's loading on the liquidity factor of [Sadka \(2010\)](#) in the augmented [Fung and Hsieh \(2001\)](#) model. Quarterly flows are imputed using net asset values and returns. [Table II](#) shows that Baseline funds are larger, but IA-2333 funds have higher returns, standard deviations, alphas, and flows.

4. Return Misreporting Flags

The main dependent variables in this article are a set of return misreporting flags. We use misreporting flags as our dependent variable, rather than observed fraud, because observed fraud depends upon both the (unobservable) occurrence of actual fraud and the rate of fraud detection. If regulatory oversight increases the rate of fraud detection, this would severely bias our tests. As one of the expressed purposes of Rule IA-2333 was to accelerate the detection of fraud, it would not be possible to distinguish whether any relation between fraud and regulatory oversight was due to changes in the occurrence of fraud or changes in detection.

Using the returns within each fund period, we generate misreporting flags based on suspicious return patterns identified in prior studies. Specifically, we generate two flags based on measures of abnormally low correlations with other assets developed by [Bollen and Pool \(2012\)](#), one flag based on the return discontinuity around zero reported by [Bollen and Pool \(2009\)](#), and two flags based on the December return spike reported by [Agarwal, Daniel, and Naik \(2011\)](#). For each flag, we use cutoff values such that, in the absence of any return misreporting, we would expect at most 5% of funds to be misclassified as misreporters. Internet Appendix Table 3 also shows results in which the dependent variable is indicator for an *F*-score calculated from [Bollen and Pool \(2012\)](#) above the 95th percentile of sample funds,²² which predicts fraud based on return misreporting flags; the results are similar.²³

4.1 Low Max R^2 and Low Index β

[Bollen \(2013\)](#) and [Bollen and Pool \(2012\)](#) argue that return misreporting decreases the correlations between reported returns and other assets. They develop two flags, Low Max R^2 and Low Index β , and show that both flags predict hedge fund fraud. The first flag, Low Max R^2 , is based on the maximum proportion of a fund's returns that is explained by hedge

20 The advisor of a non-US domiciled fund must register with the SEC if the fund, or other funds managed by the same advisor, accept money from US investors. The non-US domiciled funds are overwhelmingly located in Caribbean tax havens: 49.0% in the Cayman Islands, 23.3% in the British Virgin Islands, and 11.9% in Bermuda.

21 We are grateful to David Hsieh for providing these factors on his website at: <http://faculty.fuqua.duke.edu/~dah7/DataLibrary/TF-FAC.xls>.

22 In the main article, we do not report results for several of the individual return misreporting flags that are used to create the *F*-Score of [Bollen and Pool \(2012\)](#), because they show that these flags are not significantly related to fraud. Further, in addition to being uncorrelated with fraud, [Cassar and Gerakos \(2011\)](#) argues that asset illiquidity is the major driver of the [Bollen and Pool \(2008\)](#) conditional autocorrelation flag and the [Getmansky, Lo, and Makarov \(2004\)](#) autocorrelation flag.

23 We are grateful to Veronika Pool for providing these data.

fund style factors. For each fund period we find the subset of the Fung and Hsieh (2001) factors that maximizes the fund's adjusted- R^2 . Following Bollen and Pool (2012), Low Max R^2 is set equal to one if the fund's maximum adjusted- R^2 is below the 95th percentile of a fund-specific bootstrap simulation (see Bollen and Pool, 2012 for details). The second flag, Low Index β , is based on the relation between the hedge fund's returns and the returns of its style index. For each fund period we create an adjusted style index, defined as the equal-weighted return of all other funds with the same style. We then regress the fund's returns on the adjusted style index. Following Bollen and Pool (2012), Low Index β is set equal to one if the coefficient on the adjusted style index return is not positive and significant.

4.2 Kink (Return Discontinuity at Zero)

Bollen and Pool (2009) show that reported returns are disproportionately just above zero compared to just below, and argue that funds manipulate returns to avoid reporting losses. We follow Bollen and Pool (2012) and use the histogram approach of Burgstahler and Dichev (1997) to identify the return kink for individual funds. For each fund period, we count the number of monthly returns in three adjacent bins: two to the left of zero and one to the right. Following Bollen and Pool (2012), we set the bin width using the algorithm of Silverman (1986): $1.05846 \times \min(\sigma_i, \frac{Q_3 - Q_1}{1.34}) \times N^{-0.2}$, where σ_i is the standard deviation of the fund's returns, $Q_3 - Q_1$ is the interquartile range, and N is the number of observations. If the return distribution is smooth, the number of observations in the bin just below zero should approximately equal the average of the two adjacent bins. As in Bollen and Pool (2012), the Kink flag equals one if the number of observations in the bin just below zero is significantly below the average of the surrounding bins (i.e., if the difference is negative and smaller than the 5th percentile of the reference distribution).

4.3 December Return Spike and December Residual Spike

Agarwal, Daniel, and Naik (2011) argue that hedge funds overstate December returns because incentive fees are usually calculated at year-end. They show that both raw returns and factor model residuals are abnormally high in December, and this pattern is stronger for funds with greater incentives and opportunities to manipulate returns. Based on their results, we generate two flags. First, for the December Return flag we regress the fund's returns on an indicator variable for the month of December. Second, for the December Residual flag we regress the fund's returns on Fung and Hsieh's (2001) seven-factor model and an indicator variable for the month of December. For both versions, the flag is set equal to one if the December coefficient is positive and significant.

4.4 Any Misreporting Flag

Our final variable is the Any Misreporting flag, which is set equal to one if, during the period, the fund triggers one or more of the flags just described. In Internet Appendix Table 4 we also report results from count models for which the dependent variable is the sum of the flags; the results are similar.

4.5 Summary Statistics: Return Misreporting Flags and Registration Status

Table III summarizes the proportion of funds that trigger each return misreporting flag during the Pre-Mandatory period (January 2002 to June 2004). The results are summarized separately for IA-2333 and Baseline funds. The proportions of funds that trigger the

Table III. Return misreporting flags

This table reports the frequency of the return misreporting flags during the Pre-Mandatory period (January 2002 to June 2004). Averages are reported separately for funds whose advisor first registered in response to Rule IA-2333 (IA-2333) and for funds whose advisor was already registered (Baseline). Low Max R^2 equals one if the maximum adjusted R^2 from regressions on all possible subsets of the [Fung and Hsieh \(2001\)](#) factors is below the 5th percentile of a fund-specific bootstrap simulation (see [Bollen and Pool, 2012](#)) for details. Low Index β equals one if the coefficient from a regression of the fund's returns on its style index is not significant. Kink equals one if the fund's returns exhibit a significant discontinuity at zero. December Return equals one if the fund exhibits a significant positive December return spike. December Residual equals one if the coefficient on the December indicator is significant when we regress the fund's returns on [Fung and Hsieh's \(2001\)](#) seven-factor model and an indicator variable for the month of December. Any Misreporting Flag equals one if the fund triggers any of the return misreporting flags. The final row reports the p -value from Hotelling T^2 -test of the joint difference of the individual flags. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels based on Fisher's exact test.

	IA-2333	Baseline	difference
Low Max R^2	0.116	0.074	0.042***
Low Index β	0.207	0.157	0.050**
Kink	0.118	0.126	-0.008
Dec Return	0.129	0.100	0.029*
Dec Residual	0.189	0.171	0.018
Any Misreporting	0.493	0.456	0.037**
Hotelling T^2 (p -value)			0.001***

misreporting flags are comparable to prior studies. In this simple comparison, which does not account for fund style or other variables, the IA-2333 funds have significantly higher misreporting for four of the six flags, including the Any Misreporting Flag. A Hotelling's T^2 test strongly rejects the multivariate equality of misreporting by these two groups: prior to the rule change, misreporting was higher for IA-2333 funds (p -value < 0.001). Internet Appendix Table 5 shows additional summary statistics of the misreporting flags, including for the Deregister and Remain groups.

Consistent with misreporting being related to advisor-level characteristics such as regulatory oversight, we find significant clustering of flags within advisors. A fund is nearly twice as likely to trigger a flag if another fund managed by the advisor triggers the flag. This relation remains even if we restrict the comparison to include only funds with different styles (i.e., a long/short equity fund is more likely to trigger a flag if the non-long/short equity funds managed by the same advisor also trigger a flag). Thus, this relation is not driven solely by style similarity within the advisor.

The return misreporting flags doubtlessly misclassify some funds. Indeed, even in the absence of any return misreporting, just by construction, we would expect 5% of the funds to trigger each of the individual misreporting flags in each period. Although measurement error in the dependent variable will decrease the power of the tests, it will not bias them; the differences-in-differences methodology will, as its name suggests, difference the

misclassifications from the results. Misclassification would only bias the tests if changes in misclassification were spuriously correlated with regulatory changes, but it is not obvious why this would occur. Nevertheless, to ensure the robustness of our results, Internet Appendix Tables 6 and 7 include results using a stricter cutoff of 1% for triggering the misreporting flags. With the stricter cutoff, the percentage of funds that trigger the misreporting flag decreases, but there is very little change in the difference between the misreporting rates for IA-2333 and Baseline funds. This suggests that the frequencies of misclassification are similar for both IA-2333 and Baseline funds, and the difference between the misreporting rates reflects the actual difference in misreporting. Even with the stricter cutoff the main regression results are essentially the same.

5. Regulatory Oversight and Return Misreporting

Rule IA-2333 changed the regulatory oversight of some, but not all, of the hedge funds in the sample. Thus, we can estimate differences-in-differences models to identify how these regulatory changes affect return misreporting, while controlling for common time series changes. The results presented in the article are estimated using logit models,²⁴ but Internet Appendix Tables 4, 8, and 9 show the results are similar if we use count models of the number of flags or if we use linear models.²⁵

5.1 The Initiation of Rule IA-2333 and Changes in Return Misreporting

In this specification, we consider only the effect of the initial regulatory change when Rule IA-2333 was introduced. As such, we include only data from the Pre-Mandatory (January 2002 to June 2004) and Mandatory periods (July 2004 to December 2006). There is one observation per fund period, and we estimate the following specification:

$$P(Y_{i,t} = 1 | X_{i,t} = x) = F(\alpha + \gamma_1 \cdot I_{IA} + \beta_1 \cdot I_{t=\text{Mandatory}} + \delta_1 \cdot (I_{t=\text{Mandatory}} \times I_{IA}) + \varphi \cdot Z + \epsilon_{i,t}) \quad (1)$$

Where F is the logit function; $Y_{i,t}$ is an indicator variable equal to one if hedge fund i triggers the return misreporting flag during period t ; I_{IA} is an indicator variables equal to one for funds that registered in response to the rule change (IA-2333 funds); $I_{t=\text{Mandatory}}$ is an indicator variable equal to one during the Mandatory period (note that in the specifications reported this variable is subsumed by period-style fixed effects); and Z is an array of additional controls.

The indicator variable for IA-2333 funds removes time-invariant differences in return misreporting between IA-2333 and Baseline funds. The indicator variable for the Mandatory period removes the period-specific level of misreporting common to both groups of funds (e.g., the overall level of misreporting could vary across periods depending upon overall market returns). The key differences-in-differences parameter, δ_1 , is the interaction between IA-2333 funds and the Mandatory period, which measures the change in misreporting for IA-2333 funds, relative to the change for Baseline funds. The identifying

24 Although Ai and Norton (2003) show that there are circumstances in which it is difficult to interpret the coefficients of interaction terms in logit models, Puhani (2012) shows that the interactions in differences-in-differences logit models are not subject to these difficulties. We also note that linear models find similar results.

25 Internet Appendix Table 10 shows the results are similar if we use propensity score matching.

assumption is that, in the absence of regulatory change, both groups would have experienced the same time-series change in misreporting. If this is the case, then given the controls for group effects and period effects, this interaction term measures the change in misreporting due to regulatory oversight.

Panel A of Table IV includes controls for log(total assets managed by the fund's advisor), log(fund NAV), and a dummy for missing NAV, log(fund age), returns, standard deviations, and liquidity β . These variables control for the possibility that both misreporting and regulatory status are driven by some other variable, such as performance, fund size, or changes in portfolio liquidity.

More importantly, from the perspective of avoiding omitted variable bias, we also include style-period fixed effects, that is, all funds in a specific style, such as long/short equity, receive a separate fixed effect for each period. These fixed effects allow each style to have a different period-specific level of return misreporting. As noted earlier, the misreporting flags likely misclassify at least some funds. These fixed effects control for the possibility of time series variation in misclassification across fund styles (that is somehow correlated with regulatory changes). We also include domicile-period fixed effects, that is, all funds domiciled in a specific legal jurisdiction receive a separate fixed effect for each period. These fixed effects guard against the possibility that regulatory changes in other jurisdictions affect the results.

In each column of Table IV, the dependent variable is equal to one if the fund triggers the return misreporting flag during the period. The first six columns show the results for each of the misreporting flags. The Z-scores are reported below the coefficient estimates, and are based on standard errors clustered by fund. Our discussion of the results focuses on the Any Misreporting flag, as it aggregates the information in the other flags. For the sake of completeness, however, Table IV also reports results for the individual flags. The last column shows a joint test of the significance of the coefficients across the five regressions that use the individual misreporting flags. These joint tests account for both across-equation correlations and clustering within fund.

In the first column of Panel A, the coefficient on IA-2333 Fund is positive and significant indicating that, during the Pre-Mandatory period, IA-2333 funds misreport returns significantly more than Baseline funds. The joint significance tests reported in the last column shows that the individual misreporting flags are significantly higher for IA-2333 funds.

Our primary interest is in the (relative) change in misreporting following the increase in regulatory oversight, which is measured by the interaction term $\text{IA-2333} \times \text{Mandatory Period}$. This coefficient is significant and negative for five of the six flags, including the Any Misreporting flag, indicating that the rate of misreporting by IA-2333 funds decreased following Rule IA-2333 (relative to Baseline funds). Further, the implied economic magnitudes of these decreases are large. For example, the coefficient estimate reported in the first column implies that the probability of triggering the Any Misreporting flag decreased by 30.4% for IA-2333 funds.

In the results reported in Table IV, the style-period and domicile-period fixed effects subsume the Mandatory period variable. In robustness tests, we have estimated alternative specifications in which the Mandatory period variable is not subsumed. In those specifications, the Mandatory period variable is never significant, indicating that there is an absolute decrease in the misreporting of IA-2333 funds as well as a decrease relative to that of Baseline funds.

By adding the coefficients on IA-2333 Fund and $\text{IA-2333} \times \text{Mandatory Period}$, we can compare the difference in the levels of return misreporting by IA-2333 and Baseline funds

Table IV. Return misreporting and the introduction of Rule IA-2333

The dependent variable is the return misreporting flag listed at the top of the column. The misreporting flags are calculated separately for each fund period. Panel A reports coefficients from logit models. Panel B reports coefficients from conditional logit models that include fund fixed effects in addition to the control variables used in Panel A. Note that the control variables subsume the period indicator variable. The final column reports the χ^2 -test of joint significance of the individual flags; the p -value is reported beneath the χ^2 -value. IA-2333 Fund is equal to one if the fund's advisor registered in response to the rule change. Mandatory is equal to one if the period is from July 2004 to December 2006. The base period is from January 2002 to June 2004. The models in Panel A include controls for log(total assets managed by the fund's advisor), log(fund NAV), a dummy for missing NAV, log(fund age), returns, standard deviations, liquidity β , style-period effects, domicile-period effects, and constants. Individual coefficients are not reported for style-period effects and domicile-period effects; instead, the p -value of the joint significance of these coefficients is reported in parentheses. Standard errors are clustered by fund, and Z-scores are reported in square brackets. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: logit models

	Any Misreporting	Individual flags			Dec Return	Dec Residual	Joint χ^2
		Low Max R^2	Low Index β	Kink			
IA-2333 Fund	0.278** [2.21]	0.571** [2.53]	0.518*** [2.88]	0.004 [0.02]	0.455** [2.29]	0.272 [1.60]	17.7*** 0.003
IA-2333 \times Mandatory	-0.363** [2.20]	-0.863*** [2.94]	-0.548** [2.48]	0.065 [0.26]	-0.747*** [2.92]	-0.467** [2.09]	20.3*** 0.001
log(Advisor Total AUM)	0.010 [0.72]	0.057* [1.89]	-0.003 [0.14]	0.028 [1.21]	0.008 [0.53]	0.003 [0.19]	
log(Fund NAV)	0.097*** [3.17]	0.038 [0.63]	0.062 [1.23]	-0.059 [1.27]	0.115** [2.55]	0.125 *** [3.14]	
NAV Not reported	1.984*** [3.48]	0.609 [0.53]	1.313 [1.39]	-1.002 [1.17]	2.534*** [3.04]	2.418*** [3.27]	
log(Fund Age)	-0.250*** [3.42]	-0.207 [1.55]	-0.636*** [5.53]	-0.180 [1.55]	-0.009 [0.09]	0.012 [0.12]	
Return	-0.188** [2.39]	-0.371** [2.11]	-1.087*** [7.01]	0.185 [1.47]	0.059 [0.56]	0.120 [1.35]	
Standard deviation	-0.120*** [3.29]	-0.100 [1.10]	-0.043 [0.69]	-0.234*** [3.59]	-0.157** [2.52]	-0.069 [1.63]	
Liquidity β	-0.042 [0.09]	1.759* [1.66]	3.158*** [4.31]	-2.464*** [3.19]	-3.224*** [3.65]	-0.613 [0.94]	
Style-period effects	Yes (0.000)	Yes (0.000)	Yes (0.341)	Yes (0.530)	Yes (0.557)	Yes (0.000)	
Domicile-period	Yes (0.358)	Yes (0.306)	Yes (0.478)	Yes (0.494)	Yes (0.017)	Yes (0.163)	
Observations	2,796	2,796	2,796	2,796	2,796	2,796	

(continued)

Table IV. Continued

Panel B: conditional logit models

	Any Misreporting	Individual flags					
		Low Max R^2	Low Index β	Kink	Dec Return	Dec Residual	Joint χ^2
IA-2333 \times	−0.649***	−1.637***	−1.178**	0.017	−0.740**	−0.603**	21.7***
Mandatory	[3.17]	[3.22]	[2.36]	[0.05]	[2.40]	[2.26]	0.001
log(Fund NAV)	0.012	−0.184	0.162	0.241	0.051	−0.407**	
	[0.10]	[0.57]	[0.55]	[1.20]	[0.25]	[2.33]	
NAV Not reported	0.024	−3.501	2.466	4.803	0.900	−7.305**	
	[0.01]	[0.63]	[0.44]	[1.32]	[0.24]	[2.25]	
log(Fund Age)	−0.793*	−0.656	−0.682	−1.018	0.123	0.593	
	[1.84]	[0.66]	[0.59]	[1.45]	[0.19]	[1.04]	
Return	0.021	−0.098	−1.704***	0.404	0.129	0.152	
	[0.11]	[0.30]	[3.27]	[1.32]	[0.46]	[0.58]	
Standard deviation	−0.070	−0.304*	0.146	−0.149	−0.013	−0.048	
	[0.84]	[1.65]	[0.54]	[0.98]	[0.07]	[0.33]	
Liquidity β	−0.864	0.913	5.907***	−0.517	−4.607***	−2.952***	
	[1.31]	[0.94]	[2.93]	[0.38]	[4.31]	[3.51]	
Style-period effects	Yes	Yes	Yes	Yes	Yes	Yes	
	(0.001)	(0.011)	(0.203)	(0.518)	(0.103)	(0.000)	
Domicile-period	Yes	Yes	Yes	Yes	Yes	Yes	
	(0.154)	(0.269)	(0.881)	(0.236)	(0.188)	(0.073)	
Fund effects	Yes	Yes	Yes	Yes	Yes	Yes	
Observations	2,796	2,796	2,796	2,796	2,796	2,796	

during the Mandatory period (i.e., the sum of the coefficients shows the difference in the levels of misreporting during the Mandatory period, rather than the difference in the change in misreporting between periods). In the first column, this sum is an insignificant −0.085, indicating that the levels of misreporting by IA-2333 and Baseline funds are not significantly different during the Mandatory period.

The results for four of the five individual misreporting flags show significant decreases in misreporting (relative to Baseline funds) during the Mandatory period; this decrease is jointly significant for the individual flags (p -value = 0.001). Further, the implied magnitudes of the decreases in these four flags are large, ranging from 37.3% to 57.8%. Although the results for the Kink flag are insignificant in these fund-level tests, in tests on the pooled distribution of returns as in Jylha (2011), there is a significant decrease in the return kink of IA-2333 funds relative to Baseline funds (see Internet Appendix Table 11).²⁶ The consistent results for the individual misreporting flags provide an important robustness check, as the Low Max R^2 and Low Index β flags are conceptually distinct from the December flags. Thus, any alternative story must explain a decrease in both of these conceptually distinct sets of misreporting flags.

26 Jorion and Schwarz (2014a) show that nonlinear incentive fees can also create a return discontinuity around zero. To the extent that factors other than mispricing affect the return kink, this will reduce the power of our tests.

The differences-in-differences models in Panel B of Table IV include fund fixed effects. In these tests, each fund is essentially benchmarked against itself. Thus, this approach controls for any concerns that the results are driven by time-invariant differences in the characteristics of IA-2333 and Baseline funds, or by changes in the sample of funds (including changes caused by non-survival). Compared to Panel A, the results are similar but slightly stronger: in the Mandatory period the relative rate of misreporting by IA-2333 funds decreased significantly for five of the six misreporting flags. And the economic magnitudes of the coefficients are larger than in Panel A.

The differences-in-differences approach in Table IV removes time series changes common to all funds, and the fund fixed effects in Panel B remove time-invariant fund characteristics. Thus, any alternative explanation of the results must be based on time series variation in misreporting that is limited to IA-2333 funds only. For example, suppose that IA-2333 funds systematically decreased their holdings of illiquid securities following the rule change (perhaps to avoid regulatory scrutiny). Although we include each fund's period-specific factor loading on the Sadka (2010) liquidity factor as a control variable, such a change could, potentially, explain the differences-in-differences results for the Low Max R^2 or Low Index β flags (although not the results for the December return spike flags). To ensure that time series changes in investment strategies or portfolio characteristics do not drive our results, Internet Appendix Table 14 shows differences-in-differences results for the portfolio holdings reported in hedge fund advisors' 13F filings (we caution the reader, however, that less than 20% of the IA-2333 advisors file 13F, and those that do are not representative). There is no evidence that IA-2333 funds switch to more liquid holdings following regulatory changes; if anything the opposite is true. Similarly, Appendix Table 15 does not find any differences-in-differences effects in fund alphas (which are estimated without including a liquidity factor), which is also inconsistent with systematic changes in portfolio liquidity. Finally, we note that average hedge fund returns were similar in both periods, and so it is unlikely that time series changes in market conditions could drive the results.

5.2 Multiple Changes: The Initiation and Subsequent Revocation of Rule IA-2333

In this subsection, we extend the differences-in-differences models to include both the initiation and revocation of Rule IA-2333. These models include three periods: Pre-Mandatory, Mandatory, and Post-Mandatory. In the previous section, we separated funds into those that registered prior to Rule IA-2333 (Baseline) and those that registered in response to the rule (IA-2333). In this subsection, we further separate the IA-2333 funds into two groups: funds that deregistered following revocation (Deregister) and funds that did not (Remain). Separating Deregister and Remain funds allows us to estimate the effects of revocation. Further, it allows Deregister and Remain funds to have different sensitivities to the initial regulatory change, which is potentially important as deregistration reveals something about the funds' underlying type.

We recognize that funds jointly chose whether to misreport returns and whether to deregister after Rule IA-2333 was revoked, and this choice affects the interpretation of the results. Because funds chose to deregister, we cannot interpret the deregistration coefficient as an estimate of the full-sample treatment effect (i.e., the change in return misreporting that would occur if a fund was randomly selected for deregistration). The funds that chose to deregister presumably did so, at least in part, because they were particularly constrained

by Rule IA-2333. As such, it is likely that the return misreporting of Deregister funds is especially sensitive to regulatory oversight, and thus the treatment effect for this subsample is larger than the full-sample treatment effect. From a policy perspective, however, the treatment effect for the Deregister funds, which would not voluntarily register, is of particular interest.

The results reported in Table V are based on the following specification:

$$\begin{aligned}
 P(Y_{i,t} = 1|X_{i,t}) = & F(\alpha + \gamma_1 \cdot I_d + \gamma_2 \cdot I_r + \beta_1 \cdot I_{t=\text{Mandatory}} + \beta_2 \cdot I_{t=\text{Post-Mandatory}} \\
 & + \delta_1 \cdot (I_d \times I_{t=\text{Mandatory}}) + \delta_2 \cdot (I_d \times I_{t=\text{Post-Mandatory}}) \\
 & + \delta_3 \cdot (I_r \times I_{t=\text{Mandatory}}) + \delta_4 \cdot (I_r \times I_{t=\text{Post-Mandatory}}) \\
 & + \varphi \cdot Z + \epsilon_{i,t}
 \end{aligned} \tag{2}$$

Where F is the logit function; $Y_{i,t}$ is an indicator variable equal to one if hedge fund i triggers the Any Misreporting flag during period t ; I_d and I_r are indicator variables equal to one for IA-2333 funds that chose to Deregister and Remain, respectively; I_t are indicator variables equal to one in period t (in the specifications reported, these variables are subsumed by period-style fixed effects). When interpreting the results, our primary interest is in the differences-in-differences coefficients δ_1 , δ_2 , δ_3 , and δ_4 .

Table V reports the results of logit regressions in which the dependent variable equals one if the fund triggers the Any Misreporting flag during the period. The Z-scores are reported below the coefficient estimates, and are based on standard errors clustered by fund. The regressions include the same control variables as in Table IV including the style-period fixed effects,²⁷ and column (2) also includes fund fixed effects.

The results in the first column show that Deregister funds have significantly higher return misreporting: the coefficient of 0.501 implies that, relative to Baseline funds, Deregister funds are 65.0% more likely to trigger the Any Misreporting flag during the Pre-Mandatory period. In contrast, the levels of misreporting by Remain and Baseline funds are not significantly different during the Pre-Mandatory period. The difference between the Deregister and Remain funds is consistent with differences in the underlying types of these two categories of funds; the funds that chose to deregister also chose to misreport returns.

Turning to the differences-in-differences coefficients, both columns show that during the Mandatory period misreporting by Deregister funds decreased relative to that of Baseline funds. Further, the economic magnitudes are large. The coefficient estimates reported in columns (1) and (2) imply decreases in misreporting of 51.3% and 58.5%, respectively.

The sum of the Deregister and the Deregister \times Mandatory coefficients is not significant. Thus, during the Mandatory period, the levels of misreporting by Deregister and Baseline funds are not significantly different. In the Post-Mandatory period the differences-in-differences coefficient is not significantly different from zero (i.e., in the Post-Mandatory period the level of misreporting by Deregister funds is not significantly different from the level during the Pre-Mandatory period). Further, the Deregister \times Post-Mandatory coefficient is

27 The style-period fixed effects are particularly important in this specification. Unlike the Pre-Mandatory and Mandatory periods which both had very similar market conditions, market conditions were quite different during the Post-Mandatory period. The style-period fixed effects, however, remove all common variation in style-specific returns during this period.

Table V. Return misreporting, registration, and deregistration

The dependent variable equals one if the fund triggers any of the return misreporting flags. Deregister is equal to one if the fund’s advisor registered in response to Rule IA-2333 and then deregistered once the rule was revoked. Remain is equal to one if the fund’s advisor registered in response to Rule IA-2333 and voluntarily remained registered after the rule was revoked. Mandatory is equal to one if the period is from July 2004 to December 2006. Post-Mandatory is equal to one if the period is from January 2007 to June 2009. The base period is from January 2002 to June 2004. The models include, but we do not report, controls for log(total assets managed by the fund’s advisor), log(fund NAV), a dummy for missing NAV, log(fund age), returns, standard deviations, liquidity β , style-period effects, domicile-period effects, and constants. Note that the control variables subsume the period indicator variables. The table reports results for logit models. The model in column two also models includes fund fixed effects. Standard errors are clustered by fund, and Z-scores are reported in square brackets. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Deregister	0.501** [2.43]	
Remain	0.116 [0.85]	
Deregister \times Mandatory	−0.720*** [2.67]	−0.880*** [2.70]
Deregister \times Post-Mandatory	−0.398 [1.34]	−0.273 [0.66]
Remain \times Mandatory	−0.211 [1.17]	−0.415* [1.89]
Remain \times Post-Mandatory	−0.058 [0.30]	−0.185 [0.67]
Fund effects	No	Yes
Fund characteristics	Yes	Yes
Style-period effects	Yes	Yes
Domicile-period effects	Yes	Yes
Observations	4,072	4,072

significantly smaller than the Deregister \times Mandatory coefficient. Thus, relative to Baseline funds, misreporting by Deregister funds increased significantly following deregistration.

The results are much weaker for the Remain funds. There is little evidence that these funds had higher return misreporting than Baseline Funds during the Pre-Mandatory period. As such, it is unsurprising that the differences-in-differences coefficients for Remain funds are generally not significant. The weaker effect of regulatory oversight on Remain funds compared to Deregister funds is consistent with different sensitivities to regulatory oversight discussed earlier: The Remain funds are less likely to engage in misreporting even in the absence of oversight, and are thus less affected by regulatory change.

5.3 Placebo Tests of the Parallel Trend Assumption

The key identifying assumption for our differences-in-differences tests is that, in the absence of regulatory changes, any time series changes in misreporting would have been the same for both the IA-2333 and the Baseline funds (the parallel trend assumption). In Table VI, we estimate two placebo tests of this assumption.

Panel A of Table VI shows differences-in-differences results that compare misreporting during the Before Pre-Mandatory period (July 1999 to December 2001) with misreporting during the Pre-Mandatory period. This is a placebo test, as there were no changes in regulatory oversight of hedge funds between these two periods. As the results in Panel A show, the differences-in-differences coefficients are not significant. This provides at least some evidence that the differences in misreporting were stable over time, prior to the change in regulation.

Panel B of Table VI shows a second placebo test, which uses a sample of funds that were not required to register following Rule IA-2333. Specifically, we identify advisors that did not register, are not located in the USA, and do not advise any US domiciled funds (Foreign). The placebo group consists of the funds managed by these foreign advisors. Summary statistics, reported in Internet Appendix Tables 5 and 13, show that IA-2333 and Foreign funds do not significantly differ in their return misreporting or fund characteristics (except for a mechanical difference in where they are domiciled).

Because these Foreign funds were not subject to Rule IA-2333, if the model is properly specified, their return misreporting should not change during the Mandatory period. This is precisely what Panel B of Table VI shows: relative to Baseline funds, Foreign funds have higher rates of return misreporting, but there is no change in misreporting following Rule IA-2333. Further, the placebo results are insignificant because their economic magnitudes are very small, not because of low power: the standard errors for the Foreign \times Mandatory coefficients are smaller than the standard errors for the Deregister \times Mandatory coefficients. Thus, both placebo tests provide support for the parallel trends assumption.

5.4 An Alternative Benchmark Group

As noted in the data section, the Baseline sample includes only those advisors that were clearly required to register during the Pre-Mandatory period (advisors with 15 or more funds or with other business activities that require registration). Thus, to avoid potential sample selection biases, the prior tests do not include funds managed by advisors that voluntarily registered prior to Rule IA-2333. As a robustness test, Panel C of Table VI shows results where these funds (Voluntary) are included. The coefficients for the IA-2333 funds remain essentially the same as in Tables IV and V.

The results for the Voluntary funds also serve as an additional placebo test. There are no time series changes in regulatory oversight for the Voluntary funds, so the differences-in-differences coefficients for these funds should not be significant. This is exactly what Panel C shows. This is important, as the characteristics of the Voluntary and IA-2333 advisors are similar.²⁸ Thus, the finding that the differences-in-differences effects are not significant for Voluntary funds provides some evidence that the results for IA-2333 funds is not driven by time series changes in market conditions that interacted with advisor characteristics.

28 Internet Appendix Tables 12 and 13 show advisor-level and fund-level summary statistics for Voluntary and IA-2333 funds.

Table VI. Robustness tests: alternative time periods and comparison groups

The dependent variable equals one if the fund triggers any of the return misreporting flags. IA-2333 Fund is equal to one if the fund’s advisor registered in response to Rule IA-2333. Deregister is equal to one for IA-2333 funds whose advisor deregistered following revocation. Remain is equal to one for IA-2333 funds whose advisor voluntarily remained registered following revocation. Mandatory is equal to one if the period is from July 2004 to December 2006. Post-Mandatory is equal to one if the period is from January 2007 to June 2009. The base period is from January 2002 to June 2004. Panel A reports results for an extended sample that includes the Before Pre-Mandatory period (July 1999 to December 2001). Panel B reports results for an extended sample that includes foreign funds that were not subject to Rule IA-2333. Foreign is equal to one if the fund’s advisor never registered with the SEC and is located outside of the USA and the fund is not domiciled in the USA. Panel C reports results in which we add an additional group of funds—those that chose to voluntarily forgo the exemption (Voluntary). All results are estimated using logit models. The models include, but we do not report, controls for log(total assets managed by the fund’s advisor), log(fund NAV), a dummy for missing NAV, log(fund age), returns, standard deviations, liquidity β , style-period effects, domicile-period effects, and constants. The even numbered columns of each panel include fund fixed effects. Note that the control variables subsume the period indicator variables. Standard errors are clustered by fund, and Z-scores are reported in square brackets. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Panel A: placebo period		
Deregister	0.456** [2.27]	
Remain	0.093 [0.68]	
Deregister \times before Pre-Mandatory	−0.114 [0.30]	−0.092 [0.24]
Deregister \times Mandatory	−0.693*** [2.60]	−0.911*** [2.82]
Deregister \times Post-Mandatory	−0.325 [1.12]	−0.393 [0.94]
Remain \times before Pre-Mandatory	−0.114 [0.75]	−0.091 [0.36]
Remain \times Mandatory	−0.178 [1.00]	−0.383* [1.79]
Remain \times Post-Mandatory	−0.002 [0.01]	−0.163 [0.62]
Fund effects	No	Yes
Fund characteristics	Yes	Yes
Style-period effects	Yes	Yes
Domicile-period effects	Yes	Yes
Observations	4,965	4,965

(continued)

Table VI. Continued

	(1)	(2)	(3)	(4)
Panel B: placebo group				
Foreign	0.582*** [2.85]		0.388** [2.05]	
Foreign \times Mandatory	-0.136 [0.74]	-0.130 [0.51]	-0.155 [0.86]	-0.203 [0.86]
Foreign \times Post-Mandatory			-0.103 [0.50]	-0.169 [0.59]
IA-2333	0.235* [1.93]			
IA-2333 \times Mandatory	-0.297* [1.85]	-0.597 [2.98]		
Deregister			0.441** [2.19]	
Remain			0.113 [0.83]	
Deregister \times Mandatory			-0.594** [2.27]	-0.770** [2.39]
Deregister \times Post-Mandatory			-0.265 [0.92]	-0.266 [0.66]
Remain \times Mandatory			-0.178 [0.99]	-0.408* [1.89]
Remain \times Post-Mandatory			-0.056 [0.29]	-0.243 [0.92]
Fund Effects	No	Yes	No	Yes
Fund Characteristics	Yes	Yes	Yes	Yes
Style-Period Effects	Yes	Yes	Yes	Yes
Domicile-Period Effects	Yes	Yes	Yes	Yes
Observations	3,809	3,809	5,696	5,696
Panel C: alternative benchmark group				
Voluntary	-0.132 [0.94]		-0.161 [1.15]	
Voluntary \times Mandatory	0.151 [0.79]	0.199 [0.85]	0.158 [0.83]	0.316 [1.32]
Voluntary \times Post-Mandatory			0.063 [0.29]	0.338 [1.15]
IA-2333 Fund	0.257** [2.07]			
IA-2333 \times Mandatory	-0.317* [1.94]	-0.602*** [3.02]		
Deregister			0.478** [2.36]	
Remain			0.100 [0.74]	
Deregister \times Mandatory			-0.674 [2.53]	-0.831*** [2.63]
Deregister \times Post-Mandatory			-0.355 [1.22]	-0.244 [0.60]
Remain \times Mandatory			-0.165 [0.91]	-0.372* [1.71]
Remain \times Post-Mandatory			-0.018 [0.09]	-0.118 [0.44]
Fund effects	No	Yes	No	Yes
Fund characteristics	Yes	Yes	Yes	Yes
Style-period effects	Yes	Yes	Yes	Yes
Domicile-period effects	Yes	Yes	Yes	Yes
Observations	3,491	3,491	5,068	5,068

6. Regulatory Oversight and Investor Flows

In the prior sections, we directly test how misreporting changed following regulatory changes. In this section, we approach this same underlying issue from a different angle. Here, we test whether investors' actions imply they expected Rule IA-2333 would change fund behavior. Specifically, we test whether the levels of flows, or flow-performance sensitivities, changed following regulatory changes.²⁹ If investors expected less-return misreporting following the rule change, the level of flows into IA-2333 funds would increase following the rule change and the sensitivity of flows to poor performance by IA-2333 funds would decrease (because of reduced concerns that poor returns are smoothed or understated). Jorion and Schwarz (2014b) show that the flow-performance relation of hedge fund investors is consistent with rationality. As such, these tests can be viewed as a test of investors' expectations of the effects of the regulatory change.

To estimate the flow-performance sensitivities, we follow the standard method introduced by Sirri and Tufano (1998): in each quarter we sort funds into terciles based on lagged style-adjusted returns and then estimate piecewise linear regressions of the flow-performance relation. Following Ding *et al.* (2009), we control for: standard deviation; closed funds; high watermarks; leverage; fees; net asset values; and lockup, redemption, and subscription periods.

The coefficient in the first row of column (1) of Panel A of Table VII shows that, during the Pre-Mandatory period, the flows into IA-2333 funds were significantly lower compared to Baseline funds. The interaction term, IA-2333 \times Mandatory, shows that following registration there is a significant increase in flows to IA-2333 funds.

Turning to the flow-performance sensitivities, the coefficient for Low Performance \times IA-2333 is significant and positive. This implies that, during the Pre-Mandatory period, flows into IA-2333 funds are more sensitive to poor performance (relative to the Baseline funds). This greater sensitivity does not persist following registration, as shown by the significant negative coefficient on Low Performance \times IA-2333 \times Mandatory.

Panel B of Table VII extends the flow tests to include the Post-Mandatory period. In column (1), the negative coefficients on Deregister and on Remain show that these funds had lower levels of flows during the Pre-Mandatory period. Following registration the levels of flows increased, as shown by the positive coefficients on the interaction terms Deregister \times Mandatory and Remain \times Mandatory. After deregistering in the Post-Mandatory period, the level of flows for Deregister funds is no longer significantly different than during the Pre-Mandatory period. The level of flows for the Remain funds, however, continues to be higher during the Post-Mandatory period. Internet Appendix Table 16 shows the full set of coefficients, including the flow-performance sensitivities for each category of funds.

The results in Table VII suggest that investors value registration. The results, however, also suggest that mandatory regulatory oversight imposes a cost on the funds that were already registered; these funds no longer receive higher flows as they can no longer credibly signal their quality to investors. This points to one advantage of voluntary registration: it

29 Alternatively, investors might pay higher fees for registered funds or accept lower performance. We focus on flows, as the literature generally shows that flows are the margin of adjustment across funds (e.g., Berk and Green, 2004). Internet Appendix Table 15 shows that regulatory changes have little relation with performance, and that fees are actually higher for IA-2333 funds.

Table VII. Hedge fund flows and registration

This table contains regressions of hedge fund flows on past performance. Panel A report results for the Pre-Mandatory (January 2002 to June 2004) and the Mandatory (July 2004 to December 2006) periods. The performance terciles are interacted with IA-2333 Fund, which is equal to one if the fund's advisor registered in response to Rule IA-2333. In Panel B, the sample also includes the Post-Mandatory (January 2007 to June 2009) period. The performance terciles are interacted with Deregister which is an indicator variable equal to one for IA-2333 funds that deregistered after the rule was revoked and Remain which is an indicator variable equal to one for IA-2333 funds that voluntarily remained registered after the rule was revoked. Quarterly flows are regressed on funds' lagged fractional performance rankings over low, medium, and high performance ranges. Net flows are defined as the percentage change in net assets of the fund between the beginning and end of the quarter, net of quarterly returns. Fractional ranks of lagged performance are divided into terciles (Low, Mid, and High Performance). Following [Ding et al. \(2009\)](#), we include controls for standard deviation, open to public, high water mark, leverage, management and performance fees, lockup, redemption period, subscription period, and fund size. Column (2) includes fund fixed effects. Standard errors are clustered by fund. *t*-statistics are reported in square brackets. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Panel A: IA-2333 registration		
IA-2333 Fund	-0.051*** [2.68]	
IA-2333 × Mandatory	0.086*** [3.64]	0.073*** [2.73]
Low Performance	0.304*** [9.10]	0.247*** [7.31]
Low Performance × IA-2333 Fund	0.185** [2.11]	0.244*** [2.71]
Low Performance × IA-2333 Fund × Mandatory	-0.319*** [3.20]	-0.345*** [3.33]
Mid Performance	0.048* [1.80]	-0.005 [0.19]
Mid Performance × IA-2333 Fund	-0.021 [0.28]	-0.104 [1.38]
Mid Performance × IA-2333 Fund × Mandatory	0.044 [0.54]	0.151* [1.87]
High Performance	0.353*** [8.97]	0.328*** [8.53]
High Performance × IA-2333 Fund	0.218** [2.22]	0.003 [0.03]
High Performance × IA-2333 Fund × Mandatory	-0.019 [0.17]	0.091 [0.79]
Fund Control Variables	Yes	Yes
Fund Fixed Effects	No	Yes
Observations	18,333	18,333

(continued)

Table VII. Continued

	(1)	(2)
Panel B: IA-2333 registration and deregistration		
Deregister	-0.062** [2.03]	
Deregister × Mandatory	0.099*** [2.69]	0.097** [2.53]
Deregister × Post-Mandatory	-0.005 [0.16]	0.029 [0.77]
Remain	-0.052** [2.36]	
Remain × Mandatory	0.068** [2.14]	0.069** [2.06]
Remain × Post-Mandatory	0.069** [2.23]	0.055 [1.53]
Performance × period effects	Yes	Yes
Fund control variables	Yes	Yes
Fund fixed effects	No	Yes
Observations	24,907	24,907

allows funds to credibly signal their type to investors. During the Mandatory period, investors could not distinguish Deregister and Remain funds; during the Post-Mandatory period, they could. Prior to the rule change, SEC Commissioner Atkins argued that “Mandating across-the-board registration only serves to eliminate any benefit registered advisors enjoyed in being able to distinguish themselves from unregistered advisors.”³⁰ Our results are consistent with this argument.

7. Conclusion

We use regulatory changes to identify the effect of regulatory oversight on return misreporting by hedge funds. Specifically, we exploit Rule IA-2333, which the SEC passed in 2004 and which extended SEC authority over many additional hedge funds. In 2006, a Federal Court revoked Rule IA-2333. We use differences-in-differences tests to compare changes in return misreporting by funds that registered in response to Rule IA-2333 with that of funds that were registered prior to the rule change. We also test how return misreporting changed following the revocation of the rule.

We show that return misreporting of the newly regulated funds decreased significantly following Rule IA-2333. After the rule was revoked, the funds that deregistered increased their misreporting relative to funds that remained registered. Placebo tests fail to find any significant differences-in-differences tests for a sample of foreign hedge funds that were not subject to this regulatory change.

We also find that regulatory oversight affects fund flows: increased oversight leads to higher overall flows, and also to lower outflows following poor performance. The flow

30 See page 108 of <http://www.sec.gov/rules/final/ia-2333-trans.txt>.

results demonstrate, however, that investors understand the value of oversight and prefer to invest with registered funds. Thus, although mandatory regulatory oversight can reduce return misreporting it may impose a cost on honest funds, by reducing the ability of these funds' to differentiate themselves and to receive higher inflows as a reward.

Understanding the effectiveness of regulatory oversight on return misreporting by hedge funds is important not only because of the direct losses suffered by investors, but also because financial misconduct has spillover effects that reduce investors overall willingness to invest (Giannetti and Wang, 2014). Further, although Rule IA-2333 was revoked, the Dodd-Frank Act restored SEC oversight of hedge funds. Thus, it is important to test whether SEC oversight does reduce return misreporting. In contrast to the predictions of Rule IA-2333's critics, we find that regulatory oversight results in an economically meaningful decrease in return misreporting. When regulatory oversight is voluntary, the funds that most need oversight do not volunteer. Thus, mandatory regulatory oversight curtails misreporting even in a market characterized by sophisticated investors.

References

- Agarwal, V., Daniel, N. D., and Naik N. Y. (2011) Do hedge funds manage their reported returns?, *Review of Financial Studies* **24**, 3281–3320.
- Ai, C. and Norton, E. C. (2003) Interaction terms in logit and probit models, *Economics Letters* **80**, 123–129.
- Aragon, G. O., Liang, B., and Park, H. (2014) Onshore and offshore hedge funds: are they twins?, *Management Science* **60**, 74–91.
- Atkins, P. S. (2006) Protecting investors through hedge fund advisor registration: long on costs, short on returns, *Annual Review of Banking & Financial Law* **25**, 537–556.
- Ben-David, I., Franzoni, F., Landier, A., and Moussawi, R. (2013) Do hedge funds manipulate stock prices?, *Journal of Finance* **68**, 2383–2434.
- Berk, J. B., and Green, R. C. (2004) Mutual fund flows and performance in rational markets, *Journal of Political Economy* **112**, 1269–1295.
- Bollen, N. P. B. (2013) Zero- R^2 hedge funds and market neutrality, *Journal of Financial and Quantitative Analysis* **48**, 519–547.
- Bollen, N. P. B. and Pool, V. K. (2008) Conditional return smoothing in the hedge fund industry, *Journal of Financial and Quantitative Analysis* **43**, 267–298.
- Bollen, N. P. B. and Pool, V. K. (2009) Do hedge fund managers misreport returns? Evidence from the pooled distribution, *Journal of Finance* **64**, 2257–2288.
- Bollen, N. P. B. and Pool, V. K. (2012) Suspicious patterns in hedge fund returns and the risk of fraud, *Review of Financial Studies* **25**, 2673–2702.
- Brown, S., Goetzmann, W., Liang, B., and Schwarz, C. (2008) Mandatory disclosure and operational risk: evidence from hedge fund registration, *Journal of Finance* **63**, 2785–2815.
- Brown, S., Goetzmann, W., Liang, B., and Schwarz, C. (2009) Estimating operational risk for hedge funds: the ω -score, *Financial Analysts Journal* **65**, 43–53.
- Brown, S., Goetzmann, W., Liang, B., and Schwarz, C. (2012) Trust and delegation, *Journal of Financial Economics* **103**, 221–234.
- Burgstahler, D. and Dichev, I. (1997) Earnings management to avoid earnings decreases and losses, *Journal of Accounting and Economics* **24**, 99–126.
- Cassar, G. and Gerakos, J. (2011) Hedge funds: pricing controls and the smoothing of self-reported returns, *Review of Financial Studies* **24**, 1698–1734.
- Cici, G., Kempf, A., and Puetz, A. (2013) The valuation of hedge funds' equity positions, Working paper, The College of William and Mary.

- Cumming, D. and Dai, N. (2010) Hedge fund regulation and misreporting returns, *European Financial Management* **16**, 829–857.
- Dimmock, S. G. and Gerken, W. C. (2012) Predicting fraud by investment managers, *Journal of Financial Economics* **105**, 153–173.
- Ding, B., Getmansky, M., Liang, B., and Wermers, R. (2009) Share restrictions and investor flows in the hedge fund industry, Working paper, University of Maryland.
- Fung, W. and Hsieh, D. (2001) The risk in hedge fund strategies: theory and evidence from trend followers, *Review of Financial Studies* **14**, 313–341.
- Getmansky, M., Lo, A. W., and Makarov, I. (2004) An econometric model of serial correlation and illiquidity in hedge fund returns, *Journal of Financial Economics* **74**, 529–609.
- Giannetti, M. and Wang, T. (2014) Corporate scandals and household stock market participation, Working paper, Stockholm School of Economics.
- Joenvaara, J., Kosowski, R., and Tolonen, P. (2013) Revisiting stylized facts about hedge funds, Working paper, Imperial College.
- Jorion, P. and Schwarz, C. (2014a) Are hedge fund managers systematically misreporting? Or not?, *Journal of Financial Economics* **111**, 311–327.
- Jorion, P. and Schwarz, C. (2014b) The strategic listing decisions of hedge funds, *Journal of Financial and Quantitative Analysis* **49**, 773–796.
- Jylha, P. (2011) Hedge fund return misreporting: incentives and effects, Working paper, Aalto University.
- Puhani, P. A. (2012) The treatment effect, the cross difference, and the interaction term in nonlinear difference-in-differences' models, *Economics Letters* **115**, 85–87.
- Sadka, R. (2010) Liquidity risk and the cross-section of hedge-fund returns, *Journal of Financial Economics* **98**, 54–71.
- SEI (2007) To deregister, or not to deregister? The registered hedge fund advisers dilemma, SEI Knowledge Partnership: Insights for Investment Management, White paper.
- Silverman, B. W. (1986) *Density Estimation for Statistics and Data Analysis*, Chapman and Hall, London.
- Sirri, E. R. and Tufano, P. (1998) Costly search and mutual fund flows, *Journal of Finance* **53**, 1589–1622.