Is Pollution a Sin? A Study on the Institutional Ownership of Polluter Stocks*

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Abstract

This study examines whether stocks of polluter firms behave like sin stocks. I test whether the aggregate institutional holdings of polluters are constrained by social norms of environmental responsibility. Using U.S. data from the Toxic Release Inventory, I identify the firms that release the greatest amount of pollutants. Consistent with expectations of increasing environmental awareness, institutional ownership of polluters relative to non-polluters is reduced and decreasing over the sample period of 1987-2014. I additionally find that institutions display the greatest aversion to dioxin polluters. After grouping institutions based on trading strategies, I find that institutions with passive buy-and-hold strategies display the greatest reluctance to own polluters, which are instead disproportionately owned by institutions with shorter investment horizons. Further tests reveal that polluters also receive less analyst coverage than comparable stocks. Finally, I examine polluter performance with a long-short portfolio, which provides no evidence of abnormal returns.

1. Introduction

Due to the impact of industrial pollution on the environment and human health, many investors and institutions increasingly include non-financial environmental information in their portfolio formation process. According to The Forum for Sustainable and Responsible Investment (2016), at the beginning of 2016 more than one-fifth of professionally managed portfolios in the U.S. were invested according to socially responsible investment (SRI) strategies, equivalent to $8.72 trillion or more.[1] The importance of environmental performance as a subset of overall corporate social responsibility (CSR) is also frequently highlighted in the reports of professional services firms, illustrating the perceived relevance of environmental governance on economic performance.[2]

The literature has primarily focused on the performance of SRI in financial markets,[3] however this study examines specific environmental interactions with institutional ownership. I deviate from topics on SRI and positive environmental performance, and instead choose to focus on the implications of socially *irresponsible* behaviour on investment; I target environmental 'sin' stocks by examining polluting firms. Polluters generate negative externalities in markets, impose costs on society, and therefore may be discriminated against during the investment decision making process. Using data from the Toxic Release Inventory (TRI), I identify the largest relative polluters in the sample and examine the relationship between toxic releases and aggregate institutional ownership.

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[1] The Forum for Sustainable and Responsible Investment (2016) finds that for institutional investors, climate change and carbon emissions are the second biggest socially responsible investing criteria following conflict risk, with $2.51 trillion institutional investor funds tied into related assets in 2016.

[2] For example, a report from Deloitte (2013) states that in order to generate value, firms should be "positioning themselves to anticipate the drivers of regulatory and stakeholder expectations", and "evaluate the company's readiness to respond to the implications of environmental performance", specifically in relation to "operations, brand image, compliance structures and even company valuations" (p. 1). Similarly, a more recent report by Ernst & Young (2017) finds that it is "commonly understood that serious reputational and environmental risks can and do surface, and they can have very real impacts on the bottom line"; investors who use environmental, social and governance screens in their investments point to both the "long-term benefits" and "lower investment risk" of these investments (p. 3).

[3] For example, see Hamilton, Jo, & Statman (1993), Geczy, Stambaugh, & Levin (2005), Galema, Plantinga, & Scholtens (2008), Renneboog, Ter Horst, & Zhang (2008) and Derwall & Koedijk (2009).

Prior literature does not focus on polluting as an explicit criterion in the classification of sin stocks;[4] this research aims to fill that gap.

The main contribution of this study is to examine whether firm pollution levels in the U.S. are associated with reduced institutional equity ownership. I test for evidence of a social norm against the ownership of polluter stocks, and primarily examine whether institutional investors have lower equity ownership of public firms with greater toxic releases, after controlling for other firm level characteristics. There is plenty of anecdotal evidence of increasing investor awareness of environmental performance and climate change, illustrated by the rising prevalence of 'green funds' and pollution divestment campaigns. Social pressures for SRI and discrimination against polluters may restrict ownership of polluter stocks for the average institutional investor; I hypothesise a negative relationship between polluters and institutional ownership. Following the logic of Hong & Kacperczyk (2009), financial institutions that have diverse constituents, publicly known positions in stocks, or are easily exposed to public scrutiny are more likely to be constrained by social norms. In contrast, individual investors and inside owners can keep their stock positions relatively opaque, and are thus less likely to be affected by social norms. Due to an expected increasing awareness of the costs of pollution on human health and the environment,[5] I additionally hypothesise a negative trend in the difference between institutional ownership of polluter and non-polluter stocks over time. The hypotheses also imply that polluter stocks should be less followed by sell-side analysts, as their services tend to cater to institutional investors (Hong & Kacperczyk, 2009).

Results are consistent with the hypothesis that polluter stocks are associated with reduced institutional ownership. I find that on average, institutional investors have approximately 4.5% reduced investment of firms that pollute in the top yearly quintile of polluters after controlling for

[4] In the literature, sin stocks are usually defined as stocks in the alcohol, gambling and tobacco industries; see Salaber (2007), Fabozzi, Ma, & Oliphant (2008), Hong & Kacperczyk (2009), Salaber (2009) and Liston (2016). Some SRI funds may also exclude weapon and armament producers.

[5] For example, Flammer (2013) provides empirical evidence of increasing external pressures over time on firms to be environmentally friendly.

ownership trends and various firm and stock characteristics. There is a positive trend on overall institutional ownership; however, when interacted with the pollution variable the coefficient becomes negative, revealing that institutions are increasingly reluctant to invest in polluting firms. Upon disaggregating toxic releases by chemical classification, I find that the dioxins and dioxin-like compounds are most significantly associated with reduced institutional ownership.

I consider the relationship between social norms and the different types of institutional investors. I repeat ownership tests by disaggregating institutions based on the institutional investor classifications of Bushee (1998). Results reveal that all three Bushee institutional investor groups have reduced ownership of polluter stocks; however, estimates vary by group, suggesting that the effects of social norms are heterogeneous among institutions. Institutions characterised by passive, diversified buy-and-hold strategies have the most reduced ownership of polluters. A similar result is found if ownership is separated based on 13F institution type; banks, insurance companies, endowments and pension funds on average have a reduced ownership of polluters, while mutual funds and independent investment advisers do not. Using data on analyst coverage, I also find a reduced level of analyst coverage for polluter stocks.

I further test whether the institutions that own polluter stocks are more likely to have shorter investment horizons due to their role as market arbitrageur. Using a firm level quarterly churn variable as a proxy for average investor horizons, results reveal that polluter stocks are indeed disproportionately held by institutions with shorter investment horizons. Finally, I test whether polluter firms earn abnormal returns due to the shunned-stock effect (Angel & Rivoli, 1997). I create a long-short polluter portfolio and test for abnormal returns benchmarked against popular risk factor models, however I find no evidence of a long-short polluter portfolio generating abnormal returns.

2. Literature review

I review the literature by first defining societal discrimination within an economic context. I then examine the relationships between social norms and SRI; lastly, I review studies relevant to sin stocks.

2.1. Societal discrimination

In a seminal contribution, Becker (2010) describes how market participants are willing to incur financial costs in order to avoid things they dislike. This discriminatory behaviour is linked to disutility generated through contact or association. Social norms refer to the behaviour of avoiding things which are discriminated against by society. Akerlof (1980) and Romer (1984) show that social norms are insensitive to arbitrage gains; social norms may be found to persist if it results in a loss of reputation for the party engaging in discriminatory behaviour that outweighs any benefits of the discriminatory behaviour itself. In the context of this study, societal discrimination refers to public pressure on institutional investors to divest from polluting firms.

2.2. Socially responsible investing

While pollution is modelled as a negative externality in economics, financial literature has generally aggregated the study of socially responsible investments as a subset of CSR. A large portion of the literature is dedicated to the returns of SRI; studies are currently inconclusive on the relationship between SRI and portfolio returns (Galema, Plantinga, & Scholtens, 2008). For example, studies by both Hamilton, Jo, & Statman (1993) and Renneboog, Ter Horst, & Zhang (2008) reveal that socially responsible funds do not generate risk-adjusted abnormal returns that are significantly different to conventional funds. Similarly, Derwall & Koedijk (2009) find that socially responsible fixed-income bond funds perform approximately the same as their peers. However, other studies such as Geczy, Stambaugh, & Levin (2005) show that imposing SRI constraints on investments reduce returns, consistent with the logic of reduced opportunity sets in standard portfolio theory.

Investment strategies are influenced by various types of social norms. For example, Ivković & Weisbenner (2005) find that individual investors exhibit a preference for local investments. Different groups of discriminatory investors have varying preferences for specific stock characteristics; women are found to place greater weighting on stocks with progressive gender policies, while younger investors are found to avoid firms with poor environmental records (Hood, Nofsinger, & Varma, 2014). These results indicate that socially responsible investors are not homogeneous as a group, or in their discriminatory behaviour. Similarly, in a study focusing on the importance of political values as a cultural factor that influence asset allocation, Hong & Kostovetsky (2012) find that institutions with liberal values are more likely to discriminate against investments that are not socially responsible, such as tobacco, natural resources, and guns and defence. Using a measure of investor holdings turnover as a proxy for investor horizons, Starks, Venkat, & Zhu (2017) also find that institutional investors with longer-term horizons prefer firms with higher ESG scores. Societal discrimination has thus been found to affect the investment decisions of not only particular groups of individual investors, but also institutions.

2.3. Sin stocks

The study of investment ‘sin’ is a relatively new branch of the literature in economic discrimination; examining social perceptions of ‘bad’ activities or participants, the subject area is inversely related to CSR. Within the subtopic of finance, the literature focuses on the performance and behaviour of financial assets that are perceived as sinful, such as the shares of tobacco or gaming firms. Sin stocks are more likely to be scrutinised by society and attract a disproportionate level of discrimination; for example, Kim & Venkatachalam (2011) find that financial reporting quality is greater for sin firms, possibly to compensate for their poor public perception. Additionally, audit and consulting fees are found to be higher for companies that deviate from social norms (Leventis, Hasan, & Dedoulis, 2013), suggesting that discriminated firms are penalised with various additional costs.

Institutional ownership studies have investigated the drivers of institutional interest in equity investments. In their seminal paper, Gompers & Metrick (2001) find a set of financial variables that explain variation in total institutional ownership of a stock; I use these variables in my models. Hong & Kacperczyk (2009) specifically examine the effect of sin on institutional ownership. Identifying alcohol, gambling and tobacco as sin stocks, they find that both the institutional ownership and analyst coverage of sin stocks is reduced due to social norms that target specific sub-industries. Specifically, the effects of social norms are found to constrain passive investors that include banks, insurance companies and endowments; however, there is no evidence to suggest that institutions playing the role of market arbitrageurs are similarly constrained in their investments. Arbitrageurs may be less likely to care about social norms, nor be willing to sacrifice good investment opportunities to satisfy society. Hong & Kacperczyk (2009) further test the returns of a portfolio of sin stocks and find evidence of abnormal returns. Consistent with the hypothesis of Becker (2010), their finding supports the theory that institutions incur financial opportunity costs in abstaining from stocks which are shunned by society. Liu, Lu, & Veenstra (2014) also explore the interactions between sin stocks and investment, finding that the institutional ownership and analyst coverage of sin stocks is positively correlated with the degree of social acceptance of the specific sin. This is relevant to environmental sin stocks, as social acceptance of polluters is likely to diminish as environmental issues become more pertinent over time.

Polluter stocks are not included in the set of sin stocks within the study by Hong & Kacperczyk (2009), nor in Liu et al. (2014). Unlike firms operating in the alcohol, gambling and tobacco industries, polluters can alter their pollution levels over time, generating time-variation in their sinner status. Fernando, Sharfman, & Uysal (2010) consider the effects of ‘greenness’ on institutional ownership. Using KLD data, firms that are identified as both ‘green’ and ‘toxic’ are found to have lower institutional ownership. I similarly consider environmental performance in relation to institutional ownership; however, I avoid using data from KLD due to the nature of the dataset. The discrete KLD environmental scores have limited variation and as a result cluster firms based on estimated environmental scores. I instead opt for continuous, audited and more objective data on pollution, sourced from the Toxic Release Inventory, which allows for greater analysis.

Data from the TRI is used by Kim, Wan, Wang, & Yang (2014) in an ownership study. Their results reveal that a prevalence of local institutional investors leads to reduced pollution at the facility level; pollution abatement efforts are also found to increase firm value when there are an increased proportion of local investors relative to firm facilities. Their study considers the opposite causal driver, and is accordingly conducted at the facility level. I argue that investment decisions are more likely to be made at the firm level, and therefore environmental screens are more likely to drive lower investment in polluting firms, rather than local shareholder pressure reducing aggregate firm level pollution. I do however test the latter channel in three robustness tests of reverse causality, but find no evidence of institutional ownership driving firm pollution.

The shunned-stock hypothesis assumes that the shortage of demand for sin stocks will impact the behaviour of their prices (Derwall, Koedijk, & Ter Horst, 2011). Angel & Rivoli (1997) extend Merton's (1987) segmented markets theory to argue that shunned controversial stocks generate higher expected returns in proportion to the level of socially responsible investors in the market. Hong & Kacperczyk (2009) examine the performance of sin stocks, and argue that institutional aversion to sin stocks causes their prices to be relatively cheaper, and equivalently generate higher expected returns. Derwall et al. (2011) also find that controversial stocks produce abnormally high returns, and explain the lack of abnormal performance by the average SRI mutual fund as a result of a mixture of both positive and negative screens. Given the evidence in the literature, I later test for evidence of polluter abnormal returns with a portfolio of polluter stocks.

Institutions are hypothesised to be constrained by social norms, and are expected to abstain from investing in firms that are discriminated by society as environmental sinners. In contrast, individual investors can hide their holdings with relative ease (Hong & Kacperczyk, 2009), and as a result are able to own more equity of polluter firms. Due to reduced institutional interest in these stocks, I also expect reduced analyst coverage of polluters. I further hypothesise that institutions investing in polluters have shorter investment horizons, and that polluter stocks earn abnormal returns through the shunned stock effect.

3. Data

Data used in this study is sourced from four primary sources: the TRI, Compustat, CRSP and Thomson Reuters institutional holdings. I use IBES for data on analyst coverage.

3.1. Pollution and fundamentals data

I obtain data on firm releases of pollutants from the Environmental Protection Agency's Toxic Release Inventory database.[6] The TRI contains information on the releases of toxic chemicals in the U.S. that may damage the environment and human health. Toxic releases include carcinogens and persistent bio-accumulative toxic chemicals.[7] Release disclosure via TRI is a mandatory program that covers over 50,000 industrial facilities and 500 different chemicals. Firms are required to disclose annual releases of toxic chemicals to the EPA if they employ 10 or more full-time employees, operate in a pollution prone industry, and handle or manufacture a TRI-listed chemical above threshold levels in any given year. The EPA releases the yearly TRI National Analysis dataset during December or the following January. The toxic releases data dates back to 1987 and covers most major industries, including mining, utilities, manufacturing, publishing, and hazardous waste. TRI data has been used by regulators, media, and environmental activists (Hamilton, 1995), while the economic and financial literature has also used toxic releases from the database to study CSR activity.[8]

The sum of total facility on-site releases, off-site releases, and transfer of releases to public owned treatment works is stored as *Total Releases*, measured in millions of pounds. I aggregate *Total Releases* by firm and year.

I use the Compustat database to source an assortment of fundamental accounting variables as at the end of year *t*. I merge firm TRI data with Compustat data, and create a dummy variable named

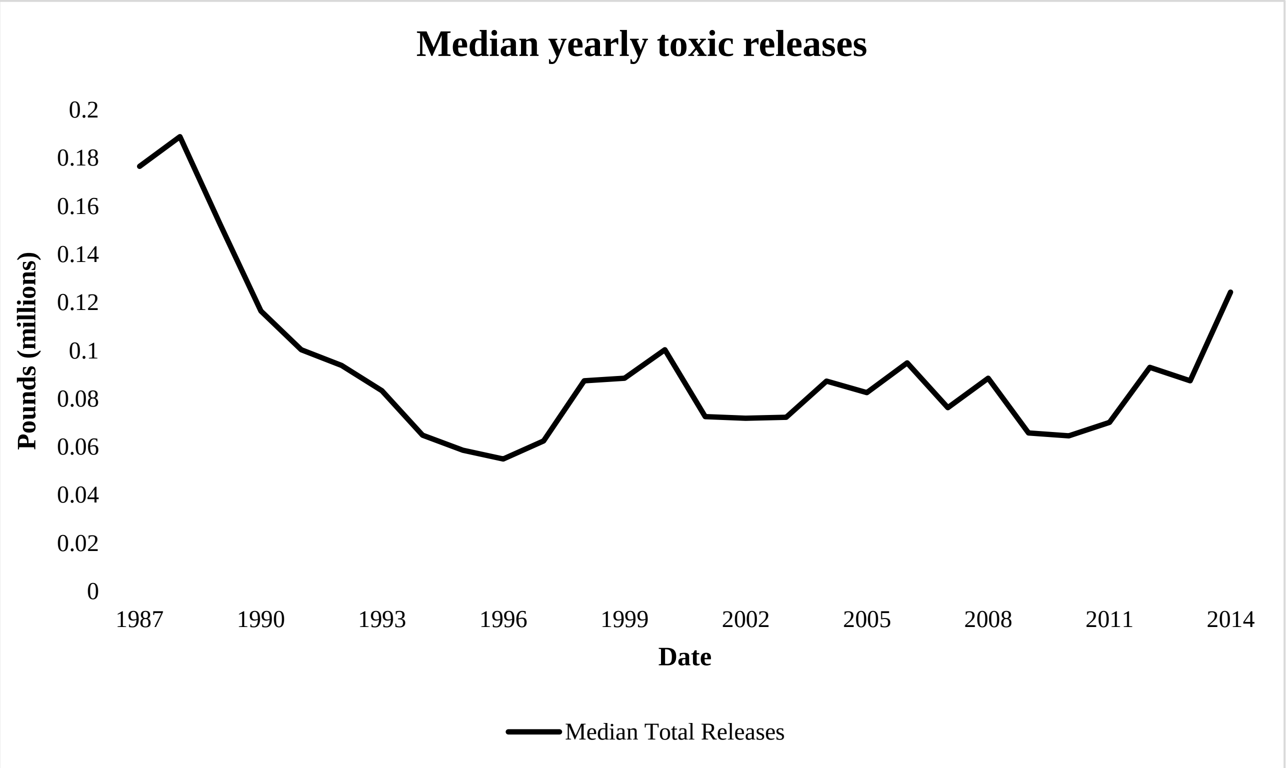
[6] The TRI was initially established under the 1986 Emergency Planning and Community Right-to-Know Act of 1986, and later expanded in the Pollution Prevention Act of 1990. The TRI was initially established in reaction to an industrial disaster in Bhopal, India in December 1984, along with a similar chemical release that occurred in West Virginia, 1985.

[7] The chemicals covered by the TRI program are generally those that are linked with cancer and other chronic health effects, significant acute health effects or significant environmental damages. Greenhouse gas emission is not specifically covered by the TRI program which instead focuses on toxic chemicals, though is some overlap between the two.

[8] For example, see Dooley & Lerner (1994), Hart & Ahuja (1996), Maxwell, Lyon, & Hackett (2000), and King & Lenox (2001).

Polluterdummy$_{i,t}$ to identify the largest relative polluters. *Polluterdummy*$_{i,t}$ is activated if firm i is in the top quintile of polluters in year t. The primary advantage of using a dummy variable is to avoid imposing any assumptions about the structural relationship between pollution and institutional ownership; instead my main models simply focus on the behaviour of the worst polluters. Using CRSP, I then merge market data on firm equities with the TRI-Compustat dataset. I only include securities with CRSP share codes of 10 or 11, and store returns in percentage format. To be included in the final sample, a firm must exist on all three of these databases. I drop firms that operate in the financial services industries with one digit SIC codes of 6 from the sample (Hong & Kacperczyk, 2009).[9] This provides a total of 8,953 firm-year observations over the final sample period of 1987 to 2014. I illustrate the average firm pollution in the sample by plotting the median *Total Releases* for each year in figure 1 below.

Figure 1: The time series of the median yearly values of *Total Releases*. *Total Releases* represents total toxics released by a firm in a single year, measured in millions of pounds. I source data from the Toxic Release Inventory Program, URL: *https://www.epa.gov/toxics-release-inventory-tri-program/tri-basic-data-files-calendar-years-1987-2016*.



[9] I find that main results are consistent if firms with book values of less than $10m are excluded.

Figure 1 reveals that the median toxic releases fall sharply in the early 1990's following the introduction of the Toxic Release Inventory program, and then becomes relatively stationary. This is possibly due to increasing environmental regulation, increasing abatement efficiency or a different sample of firms in the data.

3.2. Institutional ownership and analyst coverage data

I obtain data on institutional holdings of equity from the Thomson Reuters Institutional 13F Holdings database.[10] The 13F database contains information about institutional investors with $100 million or more in assets under management. Institutions comprise of banks, insurance companies, mutual funds, investment advisers, and others.[11] $IO_{i,t}$ measures the percentage of firms *i*'s shares outstanding that are owned by an institution at the end of year *t*. Firms with a missing value for *IO* are assumed to have 0 institutional ownership.

I follow Hong & Kacperczyk (2009) in their choice of control variables used in institutional ownership regressions. $INDBETA_{i,t}$ is the CAPM beta for firm *i*'s industry and controls for industry level market risk.[12] $LOGSIZE_{i,t}$ is calculated by taking the natural logarithm of firm *i's* market capitalisation plus 1 at the end of year *t*, and is a measure of firm size. $LOGMB_{i,t}$ is calculated by taking the natural logarithm of 1 plus firm *i's* market capitalisation divided by book value at the end of year *t*. $STD_{i,t}$ is the standard deviation of daily returns for the shares of firm *i* for year *t* and measures return volatility. $PRINV_{i,t}$ is the inverse of firm *i's* share price at the end of year *t* and controls for raw price effects. $RET_{i,t}$ is the average monthly return for firm *i's* shares during year *t*. I also use two

[10] The database contains a number of issues that are highlighted in Geertsema (2014). I follow his methodology in addressing these issues.

[11] 'Investment advisers' includes hedge funds. 'Others' includes pension funds, foundations and universities.

[12] Industry betas are calculated by regressing each of the Fama-French 49 industry portfolio excess returns (Fama & French, 1997) against market excess returns in 60-month rolling window regressions. The monthly betas of each industry are then averaged by year to convert *INDBETA* to a yearly frequency. I choose industry betas over firm betas to present results that are robust to the controls used in Hong & Kacperczyk (2009), and because industry betas improve the sample size as some individual firms have been listed for less than 60 months in the sample. However, I still find that main results are consistent when *INDBETA* is replaced with firm-specific market betas.

dummy variables, $NASD_{i,t}$ and $SP500_{i,t}$ which are respectively activated if firm i is listed on the Nasdaq or is a constituent of the S&P 500 index, during year t.

In tests examining analyst coverage, I use a panel dataset of the number of analysts that cover a particular stock. I obtain analyst coverage data from IBES. $LOGCOV_{i,t}$ is the natural logarithm of 1 plus the average number of analysts who cover firm i during year t. Again, stocks that are not listed on IBES are assumed to have 0 average analyst coverage. I use the same control variables from institutional ownership tests in analyst coverage regressions. Summary statistics of ownership variables and analyst coverage are presented in Table 1.

Table 1: Summary statistics for the sample panel data used in institutional ownership and analyst coverage regressions. Variable means are presented for the full sample and for the subset of polluters and non-polluters. Polluters are identified with the *Polluterdummy* variable. Explanatory variable are reported below the dependent variables. Significant differences in averages between polluters and non-polluters at the 10% level is denoted with *, at the 5% level with ** and at the 1% level with ***.

Sample summary statistics

Variable	Full sample	Polluters	Non-polluters	Difference
IO	0.58	0.58	0.58	0.00
LOGCOV	1.72	1.94	1.66	0.29***
INDBETA	1.04	0.89	1.08	-0.19***
LOGSIZE	20.88	21.90	20.63	1.27***
LOGMB	1.15	1.11	1.16	-0.05***
STD (%)	2.58	2.25	2.67	-0.42***
PRINV	0.08	0.06	0.09	-0.03***
RET (%)	1.31	1.24	1.33	-0.09
NASD	0.17	0.06	0.20	-0.14***
SP500	0.36	0.60	0.30	0.29***
N	8,953	1,809	7,144	

In a simple test of averages, there is no difference between institutional ownership based on polluter status, however polluters have greater analyst coverage. In line with expectations, polluter firms are larger in size and are also more likely to be listed on the S&P500. Polluter firms also have lower standard deviation of returns, and operate in industries with lower market betas. The price of polluter stocks also tends to be higher, however there is no significant difference between the returns of polluter firms and non-polluters.

3.3. Polluter performance data

To test the validity of the shunned-stock hypothesis for polluter stocks, I examine the performance of polluter firm portfolios. I source stock return data from CRSP. I exclude returns on non-domestic equities. I follow Shumway (1997) in correcting for delisting biases, and exclude micro-caps from the portfolio test.[13] Benchmark factor models used include the Capital Asset Pricing Model, the Fama-French 3-factors (Fama & French, 1993) and the Carhart 4-factors (Carhart, 1997). I source data on risk factor models from Kenneth French's data library.[14] Realised stock returns and benchmark risk factors are of monthly frequency in portfolio tests.

4. Main results

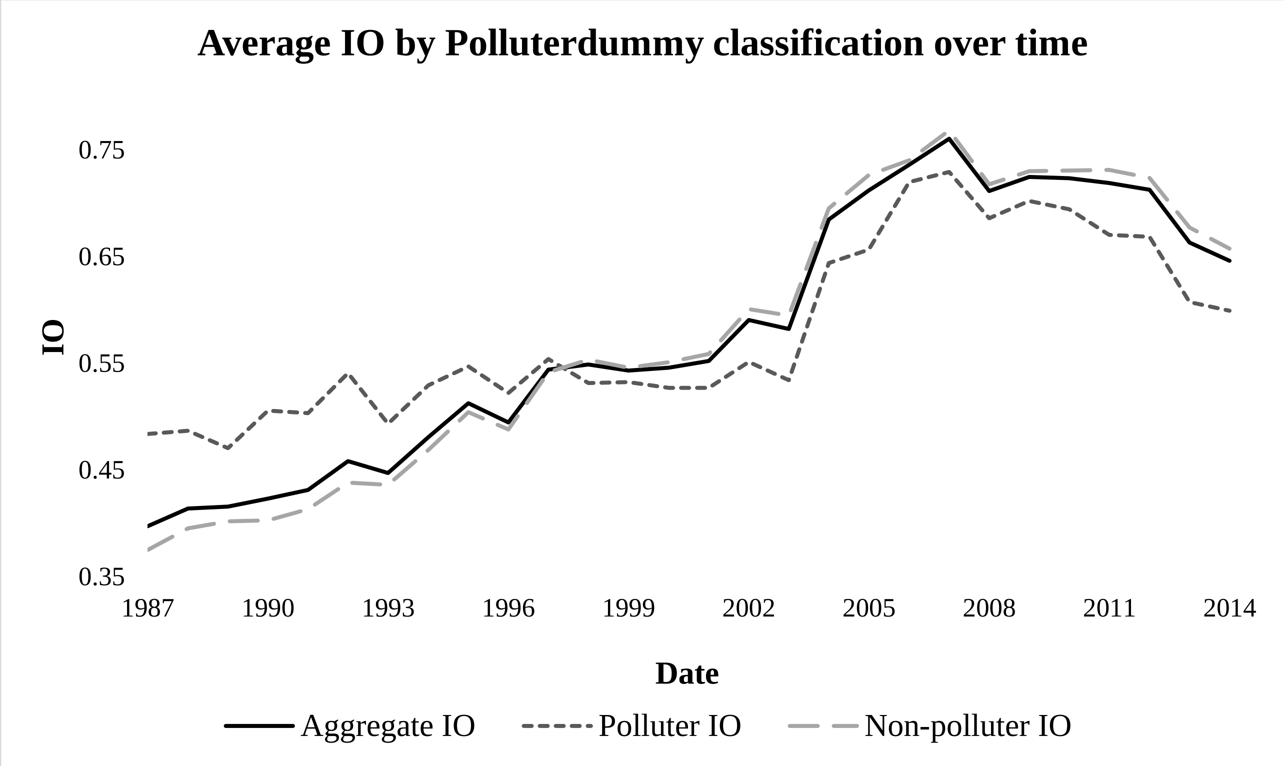
4.1. Institutional ownership of polluter stocks

I primarily consider whether firm pollution is negatively associated with institutional ownership of the polluters stocks. I hypothesise that institutional investors shy away from investing in firms that pollute heavily due to social norms. Also, with increasing environmental concerns among the public and institutions, societal discrimination against pollution is likely to increase over the sample period; I therefore expect a reduced level of institutional ownership in polluter stocks over time. In a preliminary test, I use a simple yearly average of the *IO* variable for polluters and non-polluters classified by *Polluterdummy*. I also generate a yearly average *IO* for the aggregate sample. I present the time series of averages across groups in figure 2

[13] If delisting returns in the panel data have a delisting stock code of 500, 520, between 551 and 573 inclusive, 574, 580 or 584, returns have been set at a value of -30%; while a missing delisting return with an available delisting code has returns set to -100%. Micro-caps are defined as shares of firms with very low market capitalisations of less than $250m (Lins, Servaes, & Tamayo, 2017).

[14] Kenneth French data library URL: *http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html*.

Figure 2: A time series of the average institutional ownership of firms by *Polluterdummy* classification. The trendlines are generated by taking an average of *IO* for both polluters and non-polluters, as well as the entire aggregated sample.



The average *IO* for both the non-polluters and the aggregate sample is increasing over the sample period. These two time series follow each other closely as there are far more non-polluters in the sample that contribute to aggregated sample averages. There is a large increase of ownership leading up to the GFC (2008), which then levels off following the aftermath. Interestingly, the institutional ownership of polluter stocks starts off higher than non-polluter stocks and is also increasing over the time period, however at a slower rate than that of non-polluters and the aggregate sample. 1998 appears to be the point of intersection between the series, one year prior to the Kyoto Protocol. Note that the average time series is generated without controlling for firm size or stock returns, and hence may be the result of a confounding variable driving differences in the time series.

I run the following panel regression to estimate the negative relationship between institutional ownership and *Polluterdummy*, after accounting for ownership control factors used in the literature.

$$IO_{i,t} = \alpha + \beta^{polluter} * Polluterdummy_{i,t} + \boldsymbol{\beta^{control}} * \boldsymbol{CONT_{i,t}} + \varepsilon_{i,t} \qquad (1)$$

Institutional ownership of a stock is regressed against the *Polluterdummy*, which is equal to 1 if the firm is in the top quintile polluters in the sample in a given year, and is equal to 0 otherwise. ***CONT*** is a vector of control variables that include *LOGSIZE*, *LOGMB*, *STD*, *INDBETA*, *PRINV*, *RET*, *NASD* and *SP500*.[15] The parameter of interest is $\beta^{polluter}$, which is the estimated impact on institutional ownership of being a large polluter. Under the alternative hypothesis, $\beta^{polluter}$ is negative. On average, aggregate *IO* has a positive trend; I account for time heterogeneity in the dependent variable by including yearly fixed effects (Gormley & Matsa, 2013). Hong & Kacperczyk (2009) use the "ultra-conservative" (p. 24) approach of adjusting standard errors by clustering on Fama-French (1997) industry groups. I follow their approach and additionally include clustering by year in a pooled panel regression, therefore making significance estimates even more conservative (Petersen, 2009).

I repeat the initial regression using *Total Releases* as an independent variable in place of *Polluterdummy*. Using *Total Releases* tests for a linear relationship between pollution and institutional ownership. Using *Polluterdummy*, I also implement an industry fixed effects panel regression based on the Fama & French (1997) industries; this model highlights whether polluters have reduced institutional ownership within their industry groups.

For robustness, I repeat the initial regression with but with a trend variable as an alternative control for linear time-varying heterogeneities. I then interact the linear trend with *Polluterdummy* to test whether institutional reluctance to invest in the largest polluters has changed over time. If societal discrimination of polluter stocks has increased over the sample period, the estimated interaction coefficient should be negative. Though yearly fixed effects provide greater flexibility in the model specifications, using a simple yearly trend is useful in estimating a smoothed average in ownership

[15] I avoid including corporate governance variables in main tests due to concerns of simultaneity with *IO* and reduced sample size, however in a robustness test I find a consistent relationship between *IO* and *Polluterdummy* after controlling for KLD governance variables, which include managerial compensation, low governance reporting transparency, total number of governance strengths and total number of governance concerns. I report the results of the robustness test in table 14 in the appendix.

trends along with interactions with *Polluterdummy*. I present the results of all five regression specifications in Table 2.

Table 2: Results of the institutional ownership panel regressions where the dependent variable is *IO*. I present regression coefficient estimates with t-statistics in brackets below. Standard errors are adjusted by using two-way clustering on industry and year. There are 8,953 firm-year observations in the sample for each specification. Significance at the 10% level is denoted with *, at the 5% level with ** and at the 1% level with ***.

Institutional ownership panel regression results

Variable	(1)	(2)	(3)	(4)	(5)
Polluterdummy	-0.0440***		-0.0279	-0.0461***	0.0123
	(-3.28)		(-1.61)	(-3.49)	(0.84)
Total Releases		-0.0003			
		(-1.38)			
t				0.0098***	0.0107***
				(6.41)	(7.04)
*Polluterdummy * t*					-0.0040***
					(-5.57)
INDBETA	0.1050***	0.1109***	0.0304*	0.0899***	0.0864***
	(3.67)	(3.51)	(1.67)	(3.59)	(3.60)
LOGSIZE	0.0432***	0.0413***	0.0488***	0.0471***	0.0466***
	(6.41)	(5.94)	(6.01)	(6.66)	(6.43)
LOGMB	-0.0119	-0.0078	-0.0220**	-0.0128	-0.0135
	(-1.19)	(-0.70)	(-2.52)	(-1.20)	(-1.29)
STD	-0.0129*	-0.0132*	-0.0143**	-0.0029	-0.0029
	(-1.73)	(-1.70)	(-2.27)	(-0.40)	(-0.42)
PRINV	-0.1094**	-0.1114**	-0.0901*	-0.1338**	-0.1339**
	(-2.22)	(-2.21)	(-1.92)	(-2.55)	(-2.56)
RET	-0.0013	-0.0014	-0.0008	-0.0027**	-0.0027**
	(-1.32)	(-1.37)	(-0.94)	(-2.00)	(-1.99)
NASD	-0.0550***	-0.0527***	-0.0559***	-0.0569***	-0.0571***
	(-3.86)	(-3.69)	(-3.88)	(-4.03)	(-4.04)
SP500	-0.0248	-0.0267	-0.0286	-0.0320	-0.0324*
	(-1.28)	(-1.35)	(-1.45)	(-1.63)	(-1.66)
Fixed effects	Year	Year	Year & Industry	None	None
Adjusted R^2	0.4070	0.4028	0.4507	0.3740	0.3768

Results are consistent with hypotheses of societal discrimination against polluters and increasing environmental awareness. In regressions (1) and (4), the estimated *Polluterdummy* coefficients are significantly negative, revealing that institutions are less likely to own the stocks of firms that pollute the largest quantities in a year, as expected. Polluting firms are associated with approximately 4.5% less institutional ownership of their stocks on average compared to the institutional ownership of non-polluters within any year. Interestingly, the effect of pollution on ownership appears to be non-linear; results of the *Total Releases* regression does not generate a statistically significant coefficient. I explore this non-linearity in the next section.

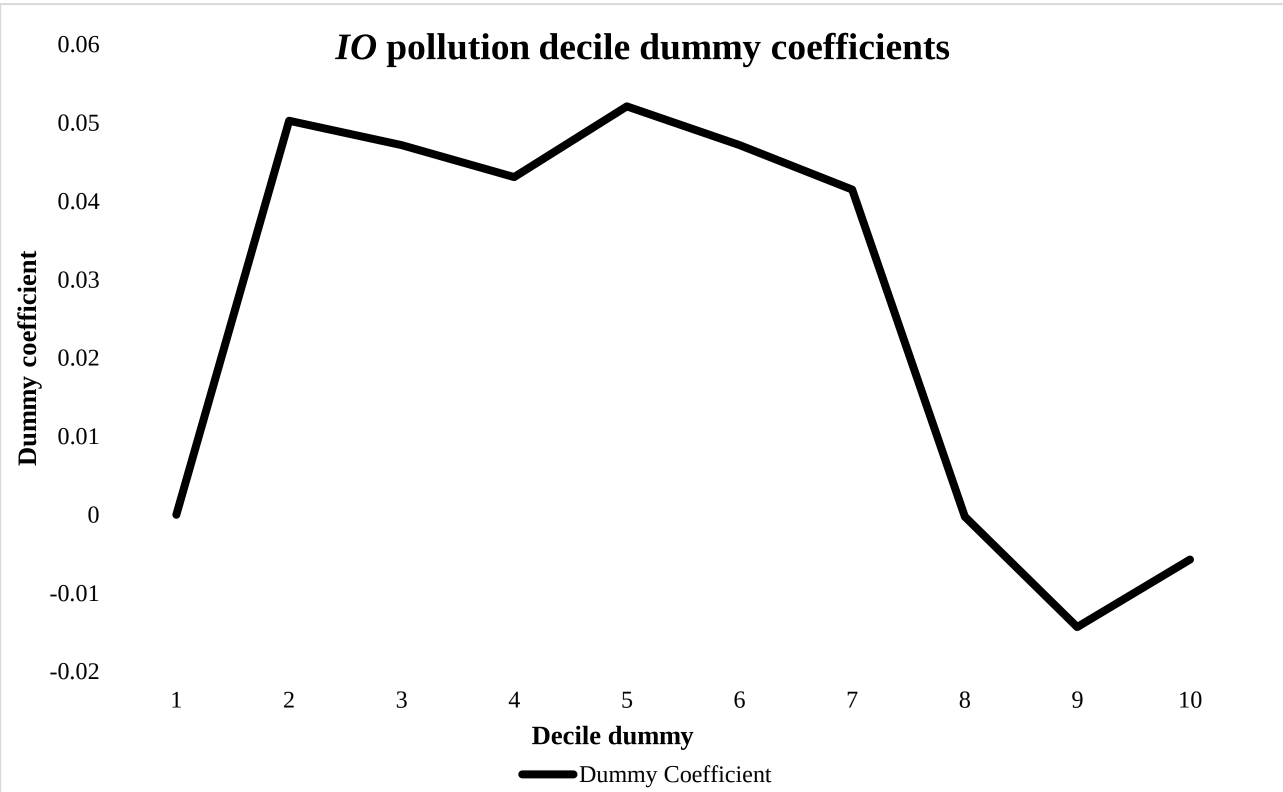
Using yearly fixed effects generates a statistically significant coefficient for *Polluterdummy*, however once industry fixed effects are included the significance disappears. The industry and yearly fixed effects model provides no evidence that polluters have reduced institutional ownership once industry averages are accounted for, such that institutional ownership of polluters within an industry is not significantly reduced. This finding suggests that pollution matters at the industry level, and that the within-industry effects of pollution are too small to overcome noise in the regression.

Consistent with Gompers & Metrick (2001), the linear trend in regression (4) and (5) is positive and statistically significant; indicating that after controlling for firm characteristics, institutions are increasing their proportional holdings of equity by approximately 1% each year on average. However, the polluter-time interaction is significantly negative, revealing that institutions are reducing their holdings of polluters relative to non-polluters by approximately 0.4% each year on average; or equivalently, institutional ownership of polluters is also increasing but at a slower rate than that of non-polluters. This interaction effect absorbs significance from the *Polluterdummy* coefficient variable, and with 28 years in the sample, reverses the positive coefficient too. The interaction estimate is consistent with figure 2, where initially the ownership of polluter stocks is higher than non-polluters, but then reverses around the late 90's.

Consistent with the literature, industry beta and size appear to have a positive effect on institutional ownership, while being listed on the NASDAQ has negative effects. *STD* has a negative coefficient, revealing that volatile stocks are associated with reduced institutional ownership on average; however the effect is only significant if fixed effects are included. Share price is found to have a positive association with institutional ownership, as the coefficient of *PRINV* suggests that institutions hold more expensive shares. Ownership is also found to be negatively related to the stock's average performance in the past year. The main results are overall in line with the hypothesised societal discrimination on polluter stocks, and provide evidence of a reduced institutional ownership of polluter firms.

I examine the association between *IO* and *Total Releases* by repeating the fixed effects panel regression but with 9 polluter decile dummy variables; one for each yearly decile of *Total Releases*. Firms that pollute in the lowest decile in a year have no dummy variable activated, while firms that pollute in the 2^{nd} decile (*Total Releases* is between the 10^{th} and 20^{th} percentile) have the corresponding 2^{nd} decile dummy variable activated, and so on. I use the complete set of control variables shown in table 2, and use the yearly fixed effects model. I graph the estimated dummy coefficients from this test in figure 3. This test highlights the average relation between *Total Releases* and institutional ownership.

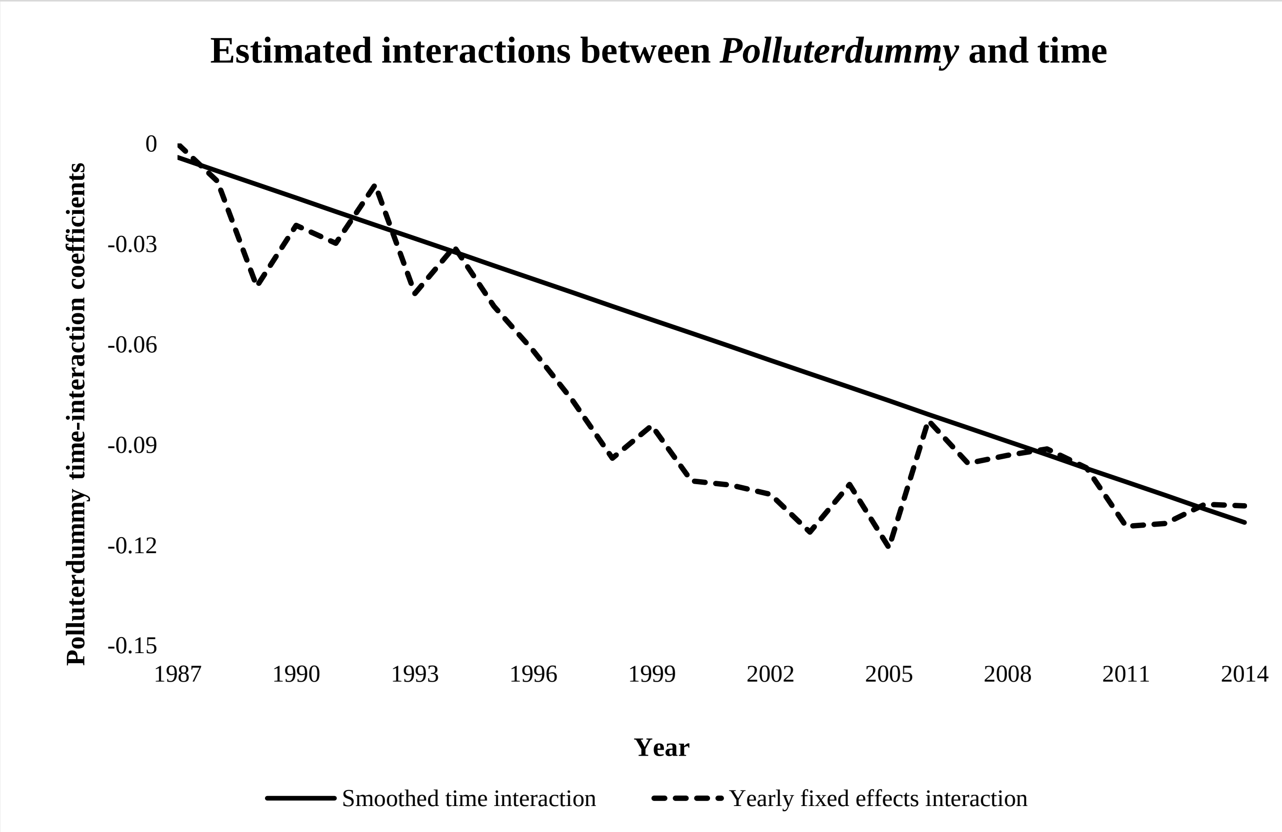
Figure 3: Pollution decile dummy coefficients, estimated from a yearly fixed effects panel regression with *IO* as the dependent variable. Decile 1 has no active dummy, and is therefore the benchmark from which the following dummy coefficients are compared against.



Results reveal a non-monotonic relationship between toxic releases and institutional ownership, explaining the non-significance in the estimated coefficient for *Total Releases* in table 2 regression (2). Figure 3 depicts how institutional holdings are more sensitive to pollution once toxic releases exceed a threshold, shown by the sharp decrease in coefficients following decile 7 firms. This relationship suggests that the greatest polluters are disproportionately discriminated against relative to firms that pollute in quantities just below the threshold. Consistent with Fernando et al. (2010), firms in the lowest decile of pollution also appear to have reduced institutional ownership relative to the middle deciles, suggesting a negative institutional reaction to firm pollution being on either extreme of the spectrum.

As seen in the table 2 regression (5), the polluter-time interaction is significantly negative, revealing that institutions have reduced their ownership of polluter stocks over time relative to their holdings of non-polluters. However, the interaction is a smoothed slope and does not explicitly show yearly changes in the gap between the institutional ownership of polluters and other firms. I estimate the annual changes in the ownership of polluters relative to non-polluters by conducting the yearly fixed effects panel regression and interacting each of the yearly dummy variables with *Polluterdummy*. The year 1987 has no dummy and is therefore the benchmark from which the fixed effect interaction coefficients are compared against. I plot the interaction coefficients generated by both the linear trend and yearly fixed effects models in a time series, illustrated in figure 4. The estimated coefficients can be interpreted as the yearly effects on the difference in institutional ownership between non-polluters and polluters.

Figure 4: Estimated interaction coefficients between *Polluterdummy* and time, where the dependent variable is *IO*. Coefficients are estimated using the *Polluterdummy*-time interaction trend in the main regression, as well as *Polluterdummy*-yearly fixed effects interactions. 1987 has no active dummy, and is therefore the benchmark from which the dummy interaction coefficients are compared against. The estimated *Polluterdummy* coefficient is 0.0123 when using the linear trend interaction model, and is 0.0319 when using the fixed effects interaction model.



Results reveal that the largest decrease in the estimated interaction coefficients occurred in the period between 1994 and 2003. This is roughly consistent with figure 2. Institutions decreased their holdings of polluter firms relative to non-polluters in the early 1990's, and then maintained the ownership gap following the mid 2000's. From the mid 2000's onwards, the difference in institutional ownership between polluter and non-polluters is more stationary, with the estimated *Polluterdummy* and yearly fixed effects interaction coefficients falling within the approximate band of -0.09 and -0.12. These estimates reveal that after the early broadening of the ownership gap between polluters and non-polluters, there has not been much of a further decline from 2005 onwards.

In table 2 I find that institutional ownership of polluters is reduced relative to non-polluters. However, within the subset of polluter firms, some firms may be perceived to generate enough positive economic value to offset the social externalities of pollution. Some firms may also operate in industries that generate valuable output but cannot avoid polluting in their operations. In contrast, some inefficient polluting firms may pollute at high levels which cannot be justified by their low levels of positive economic output. I therefore test whether *inefficient* polluters are associated with reduced institutional ownership.

I proxy for polluter efficiency by scaling yearly *Total Releases* by the net sales of the firm. Dividing *Total Releases* by net sales produces a ratio of negative to positive outputs. An inefficient polluter will have a high ratio, while an efficient polluter will have a low ratio. I create a new dummy variable labelled *Inefficientpolluter*, which is activated if a firm has a pollution to sales ratio that is within the top quintile for a year. *Inefficientpolluter* and *Polluterdummy* are positively correlated, with a Pearson correlation coefficient of 0.60 significant at the 1% level,[16] indicating that the largest absolute polluters are also likely to be the least efficient polluters. I run the yearly fixed effects panel regression used in the primary ownership test with *Inefficientpolluter*; this regression estimates whether institutions hold fewer stocks of firms that are relatively inefficient in their toxic releases. A negative estimated coefficient for *Inefficientpolluter* would imply that society discriminates against polluters after considering the economic value that these firms may otherwise generate. I also include *Polluterdummy* and industry fixed effects in two additional regression specifications. Results of the three regressions are presented in table 3.

[16] Pearson and Spearman coefficients are identical when estimating the correlation between two dummy variables.

Table 3: Results of the polluter efficiency fixed effects panel regressions where the dependent variable is *IO*. I present regression coefficient estimates with t-statistics in brackets below. Standard errors are adjusted by using two-way clustering on industry and year. There are 8,953 firm-year observations in the sample for each specification. Significance at the 10% level is denoted with *, at the 5% level with ** and at the 1% level with ***.

Polluter efficiency panel regression results

Variable	(1)	(2)	(3)
Inefficientpolluter	-0.0423**	-0.0258	-0.0153
	(-2.76)	(-1.36)	(-0.81)
Polluterdummy		-0.0267	-0.0187
		(-1.62)	(-0.96)
INDBETA	0.1039***	0.1028***	0.0301*
	(3.70)	(3.70)	(1.66)
LOGSIZE	0.0395***	0.0415***	0.0478***
	(5.56)	(6.21)	(6.06)
LOGMB	-0.0088	-0.0110	-0.0214**
	(-0.83)	(-1.08)	(-2.42)
STD	-0.0131*	-0.0129*	-0.0143**
	(-1.77)	(-1.76)	(-2.29)
PRINV	-0.1115**	-0.1103**	-0.0909*
	(-2.24)	(-2.23)	(-1.94)
RET	-0.0012	-0.0012	-0.0008
	(-1.28)	(-1.29)	(-0.92)
NASD	-0.0567***	-0.0565***	-0.0567***
	(-3.95)	(-3.95)	(-3.98)
SP500	-0.0263	-0.0252	-0.0286
	(-1.34)	(-1.29)	(-1.44)
Fixed effects	Year	Year	Year & Industry
Adjusted R^2	0.4070	0.4080	0.4509

Results indicate that the institutional ownership of inefficient polluters is also reduced, by approximately the same level as that of the greatest absolute polluters. When *Polluterdummy* is included as an explanatory variable in the regression, both the estimated coefficients of *Polluterdummy* and *Inefficientpolluter* are negative, however neither has statistical significance. This is likely a result of the high correlation between these two variables; both explanatory variables are competing to explain the same variation in institutional ownership and lose significance. Though results find a negative relationship between inefficient pollution and institutional ownership, they are unable to differentiate between the effects of absolute pollution and inefficient pollution on ownership. Similar to results found in the main regression, I find no evidence to indicate that either *Polluterdummy* nor *Inefficientpolluter* are associated with reduced within-industry institutional ownership.

4.2. Disaggregated toxic releases test

An advantage of using data from the Toxic Release Inventory is the data granularity; unlike environmental scores such as from KLD, the dataset breaks down the various types of releases by chemical group. Using the disaggregation of *Total Releases* by toxic classification, I test which toxic substances in particular have the greatest negative association with institutional ownership.

Total Releases is disaggregated into one of three mutually exclusive chemical groups. The first classification consists of standard TRI chemicals, which comprise of toxic chemicals such as by-products and certain forms of ammonia, aluminium, phosphorus and zinc. These chemicals may significantly damage human health. The second category consists of persistent bio-accumulative chemicals (PBT) such as lead or mercury compounds, which accumulate in body tissue over time, cause lasting damage to the environment and are not easily destroyed. The last category of chemicals are separately identified persistent toxics labelled as dioxin and dioxin-like compounds. These are trace level by-products of combustion or industrial processes. Dioxins are extremely toxic and human exposure is mostly through food products.

I re-run the institutional ownership model with *TRI*, *PBT* and *Dioxin* as individual explanatory variables measuring releases of their respective chemical classifications at the firm-year level. All variables are stored in millions of pounds, except for *Dioxin* releases which are stored in pounds. Data on *Dioxins* only begins from the year 2000 onwards and therefore reduces the panel size. The average values for *TRI*, *PBT* and *Dioxin* releases are 3.47, 0.26 and 0.03 respectively, and the Pearson correlations between the three variables ranges from 0.02 to 0.54. I use the same control variables in prior ownership tests and include yearly fixed effects. This test serves to examine the varying effects of toxic chemical groups on institutional ownership; I hypothesise a negative coefficient for all three chemical classifications. Results are presented in table 4.

Table 4: Results of the institutional ownership fixed effects panel regression disaggregated by chemical classification, where the dependent variable is *IO*. *TRI*, *PBT* and *Dioxin* measure the toxic releases of their respective chemical classification. *TRI* and *PBT* are measured in millions of pounds, while *Dioxin* is measured in pounds. I present regression coefficient estimates with t-statistics in brackets below. Standard errors are adjusted by using two-way clustering on industry and year. There are 4,777 firm-year observations in the sample. Significance at the 10% level is denoted with *, at the 5% level with ** and at the 1% level with ***.

	Toxic releases disaggregated by chemical classification
Variable	Coefficient estimate
TRI	-0.0022
	(-0.60)
PBT	-0.0012
	(-0.75)
Dioxin	-0.0425***
	(-2.86)
INDBETA	0.0970***
	(3.42)
LOGSIZE	0.0433***
	(5.09)
LOGMB	0.0122
	(1.04)
STD	-0.0013
	(-0.14)
PRINV	-0.1128*
	(-1.94)
RET	-0.0029*
	(-1.92)
NASD	-0.0751***
	(-4.02)
SP500	-0.0814***
	(-3.06)
Fixed effects	Year
Adjusted R^2	0.2641

Results reveal a negative association between institutional ownership and releases of all three chemical categories, however only *Dioxin* is statistically significant. Aside from the statistical significance, the magnitude of the *Dioxin* coefficient is much larger than that of the other two groups, given that *Dioxin* is measured in pounds while the other two chemicals are measured in millions of pounds. Results indicate that despite having the lowest average releases, the marginal impact of *Dioxin* releases are significantly greater than the other two chemical groups.

4.3. Disaggregated ownership tests

In the following two tests I break down the dependent variable *IO* by institution type; in the first test I separate institutions by their Bushee (1998) classification, while in the second test I disaggregate institutions based on their 13F classes. These tests reveal which institution groups are most associated with reduced ownership of polluter firms relative to their holdings of non-polluters. I hypothesise that institutions that are aggressive and attempt to arbitrage price inefficiencies in the market are less likely to be constrained by social norms.

I first consider Bushee (1998) institutional investor classifications to disaggregate institutions into separate groups.[17] Institutions are grouped by Bushee (1998) using a cluster analysis on a set of factors that measure past characteristics of investment behaviour. These three factors comprise of the level of portfolio concentration, the degree of portfolio turnover, and institution trading sensitivity to current earnings. Disaggregation by these three factors allows for tests to estimate the association between firm pollution and ownership from differing institutional groups.

Institutions are separated into three groups. Institutions are classified as either 'dedicated', 'quasi-indexer' or 'transient'. By construction, dedicated institutions have the highest portfolio concentration, low portfolio turnover and almost no sensitivity to current earnings (Bushee, 1998). Dedicated investors invest large amounts in a small number of firms and have a 'relationship' approach to their investments; dedicated investors are not frequent traders and have stable equity

[17] I thank Brian J. Bushee for access to his institutional investor database, which includes data on investor type and a permanent investor unique identifier. I avoid using the permanent investor type classification and instead opt for the dynamic classification to account for changes in investor behaviour. Not all investors in the database are given a classification, and therefore some manager holdings observations are excluded from all samples. Investor classification data URL: *http://acct.wharton.upenn.edu/faculty/bushee/IIclass.html*.

holdings in relatively fewer firms (Bushee & Noe, 2000). Quasi-indexers hold large, diversified portfolios with low turnover and have contrarian trading tendencies, all of which is consistent with index-like, buy-and-hold, value strategies (Bushee, 1998). Quasi-indexers are the largest class of institutional investors. Finally, transient institutions have the greatest portfolio turnover and use of momentum strategies, and exhibit relatively high diversification (Bushee, 1998). Transient institutions trade aggressively on short-term strategies and use corporate disclosures as a low-cost source of information for their holdings. Transient ownership is positively associated with future changes in volatility, which along with their high portfolio turnover suggests a short-run investment horizon (Bushee & Noe, 2000).

These institution types have varied trading strategies that may have an impact on their holdings of polluter stocks. I hypothesise that due to their short-term strategies and aggressive trading behaviour, transient institutions are more likely to play the role of arbitrageur in the market, and therefore be relatively indifferent to social norms. Quasi-indexers are more likely to avoid polluter stocks for ethical and reputational reasons; due to their long-term, diversified, passive buy-and-hold strategies they may be more sensitive to social norms. Similarly, dedicated institutions also have long-term investment horizons which may influence their preference for non-polluting firms. I repeat the yearly fixed effects panel regression for each subcategory of institution by regressing the ownership variable on *Polluterdummy* and the set of control variables in the subsamples, and present the results in table 5.

Table 5: Results of the institutional ownership fixed effects panel regressions disaggregated by Bushee institution group, where the dependent variable is *IO*. I present regression coefficient estimates with t-statistics in brackets below. Standard errors are adjusted by using two-way clustering on industry and year. There are 8,953 firm-year observations in the sample for each specification. Significance at the 10% level is denoted with *, at the 5% level with ** and at the 1% level with ***.

Institutional ownership disaggregated by Bushee institution group

	Institution groups		
Variable	Dedicated	Quasi-indexer	Transient
Polluterdummy	-0.0115***	-0.0560***	-0.0091*
	(-2.65)	(-5.41)	(-1.72)
INDBETA	0.0130***	0.0800***	0.0380***
	(2.86)	(3.46)	(4.07)
LOGSIZE	-0.0035**	0.0114**	0.0070***
	(-2.08)	(2.15)	(3.00)
LOGMB	0.0035	0.0263***	0.0123***
	(0.82)	(2.77)	(2.93)
STD	-0.0072***	-0.0211***	0.0005
	(-3.00)	(-4.88)	(0.24)
PRINV	-0.0131*	-0.0703*	-0.0366***
	(-1.74)	(-1.81)	(-3.20)
RET	-0.0002	-0.0020**	0.0025***
	(-0.76)	(-2.31)	(4.44)
NASD	-0.0064	-0.0274**	-0.0105
	(-1.30)	(-1.99)	(-1.55)
SP500	0.0140**	0.0419*	-0.0137
	(1.97)	(1.92)	(-1.44)
Fixed effects	Year	Year	Year
Adjusted R^2	0.2085	0.4264	0.2678

The results reveal that all three institutional investor have significantly reduced ownership of polluters. Estimates show that quasi-indexers are the most averse to polluter stocks, followed by dedicated and then transient institutions. Though both dedicated and transient institutions have a similar estimated coefficient for *Polluterdummy*, the estimate is more significant for the dedicated institution subsample. Some control variables, such as *LOGSIZE* and *RET*, change significance and sign dependent on the institutional group; this is expected due to the differences in trading strategies between institutional groups. As expected, the adjusted R^2 is much higher for quasi-indexers than transient investors, likely due to the diverse trading strategies used by the latter. Dedicated investors have the lowest adjusted R^2 implying greater variation in their relatively smaller breadth of niche investments.

Following Hong & Kacperczyk (2009), I again disaggregate institutions in the next test, but this time based on 13F classes. The 13F dataset classifies institutions into five types; type 1 represents banks, type 2 insurance companies, type 3 mutual funds, type 4 independent investment advisors, and type 5 as other, which includes universities, other endowments, and pension funds. I restrict my Thomson Reuters institutional holdings sample to 1987 to 1997 due to mapping issues in the data post 1997, where many institutions are incorrectly stored as type 5 (Hong & Kacperczyk, 2009). I then classify type 1, 2 and 5 as 'Type A' institutions, and 3 and 4 as 'Type B' institutions. On average, Type A institutions are more likely to be passive investors with greater public accountability, while Type B institutions are less likely to be constrained by social norms and act as arbitrageurs if polluter stocks are ignored by other market participants (Hong & Kacperczyk, 2009). I hypothesise a negative coefficient on *Polluterdummy* for Type A institutions only, due to their greater sensitivity to social pressures. I repeat the institutional ownership fixed effects panel regressions for both the subsample institution groups. Results of the regressions are presented in table 6.

Table 6: Results of the institutional ownership fixed effects panel regressions disaggregated by institution type, where the dependent variable is *IO*. 'Type A' consists of banks, insurance firms, pension plans, endowments, universities and employee-ownership plans. 'Type B' consists of mutual funds, independent investment advisors and hedge funds. I present regression coefficient estimates with t-statistics in brackets below. Standard errors are adjusted by using two-way clustering on industry and year. Data is limited to the range 1987 to 1997. There are 5,425 firm-year observations in the sample. Significance at the 10% level is denoted with *, at the 5% level with ** and at the 1% level with ***.

Institutional ownership disaggregated by type

Variable	Type A	Type B
Polluterdummy	-0.0335***	0.0627**
	(-3.29)	(2.13)
INDBETA	0.0161	0.0055
	(0.67)	(0.09)
LOGSIZE	0.0054	0.0572***
	(1.12)	(4.58)
LOGMB	0.0226*	-0.0705**
	(1.95)	(-2.02)
STD	-0.0182***	0.0010
	(-4.67)	(0.07)
PRINV	-0.0180	0.2050**
	(-0.91)	(2.15)
RET	-0.0020***	0.0022
	(-2.64)	(0.90)
NASD	-0.0230*	0.0207
	(-1.83)	(0.42)
SP500	0.0663***	-0.1282***
	(3.67)	(-3.19)
Fixed effects	Year	Year
Adjusted R^2	0.2004	0.0563

The estimated coefficients reveal that on average, Type A investors hold reduced levels of polluter stocks while the Type B investors hold more; coefficients are estimated at the 1% and 5% significance levels respectively. Due to the lack of clean data post 1997, the effects of more recent environmental awareness and anti-polluter sentiment are not seen in the results. Despite this, the estimated coefficients themselves are in line with expectations. Type A investors are generally more passive in their trading strategy and may be scrutinised more, whereas Type B investors are more likely to have relatively lower public scrutiny on their holdings, attempt to arbitrage any price inefficiencies in the market, and be indifferent to social norms. Type B investors are also more likely to trade more aggressively and have more varied trading strategies within the group, supported by a dramatically lower adjusted R^2 compared to Type A. In this reduced sample, results provide evidence of polluter stocks being ignored by passive investor groups, and in contrast are invested in by aggressive institutions.

4.4. Analyst coverage

Following the logic of Hong & Kacperczyk (2009), analyst coverage of sin stocks should be reduced due to the relationship between sell-side analysts and institutional investors; if institutions are reluctant to own polluter stocks, there will be reduced demand for coverage of polluters. I test whether analyst coverage is negatively associated with total firm pollution with the following pooled panel regression.

$$LOGCOV_{i,t} = \alpha + \beta^{polluter} * Polluterdummy_{i,t} + \boldsymbol{\beta^{control}} * \boldsymbol{CONT_{i,t}} + \varepsilon_{i,t} \qquad (2)$$

LOGCOV is regressed against the same control variables in institutional ownership regressions and the *Polluterdummy* variable. I first employ a yearly fixed effects model to test the association between analyst coverage and pollution using *Polluterdummy* and *Total Releases* separately. I then include industry fixed effects in the *Polluterdummy* model to test for within-industry relationships between pollution and institutional ownership. In my final two model specifications, I use an independent trend variable *t* instead of fixed effects, and then include a polluter-time interaction effect between *Polluterdummy* and *t*.

I control for the same variables used in ownership regressions (Hong & Kacperczyk, 2009), represented by the vector ***CONT***. T-stats are calculated using two-way clustered standard errors, with clustering on year and industry. I present the results of all five regression specifications in table 7.

Table 7: Results of the analyst coverage panel regressions where the dependent variable is *LOGCOV*. I present regression coefficient estimates with t-statistics in brackets below. Standard errors are adjusted by using two-way clustering on industry and year. There are 8,953 firm-year observations in the sample for each specification. Significance at the 10% level is denoted with *, at the 5% level with ** and at the 1% level with ***.

Analyst coverage panel regression results

Variable	(1)	(2)	(3)	(4)	(5)
Polluterdummy	-0.1328*		-0.1263	-0.1302*	-0.2405*
	(-1.89)		(-1.37)	(-1.91)	(-1.70)
Total Releases		-0.0012			
		(-0.48)			
t				-0.0063	-0.0079**
				(-1.50)	(-2.21)
*Polluterdummy * t*					0.0076
					(1.14)
INDBETA	0.2830***	0.3006***	0.0918	0.3378***	0.3343***
	(6.59)	(6.23)	(1.53)	(7.01)	(6.74)
LOGSIZE	0.3250***	0.3192***	0.3526***	0.3196***	0.3206***
	(10.38)	(9.68)	(12.19)	(10.30)	(10.45)
LOGMB	0.0699	0.0819	0.0553	0.0514	0.0528
	(1.44)	(1.63)	(1.02)	(1.09)	(1.11)
STD	0.0221*	0.0214	0.0230**	0.0136	0.0137
	(1.68)	(1.59)	(2.16)	(1.09)	(1.10)
PRINV	-0.0286	-0.0348	0.0101	0.0066	0.0068
	(-0.35)	(-0.41)	(0.14)	(0.09)	(0.09)
RET	-0.0304***	-0.0307***	-0.0314***	-0.0269***	-0.0269***
	(-9.43)	(-9.66)	(-9.68)	(-9.14)	(-9.19)
NASD	0.0592	0.0659	0.0331	0.0543	0.0546
	(0.73)	(0.80)	(0.40)	(0.67)	(0.67)
SP500	0.2625**	0.2572**	0.2204**	0.2907***	0.2914***
	(2.48)	(2.43)	(2.14)	(2.72)	(2.72)
Fixed effects	Year	Year	Year & Industry	None	None
Adjusted R^2	0.4182	0.4164	0.4568	0.4127	0.4131

Results are consistent with the hypothesised relationship between polluter firms and analyst coverage. I find that after controlling for various firm level variables, firms that pollute in the top quintile in a year have reduced analyst coverage on average. Estimates generated with using the linear trend and yearly fixed effects in regressions (1) and (4) are similar and significant at the 10% level.

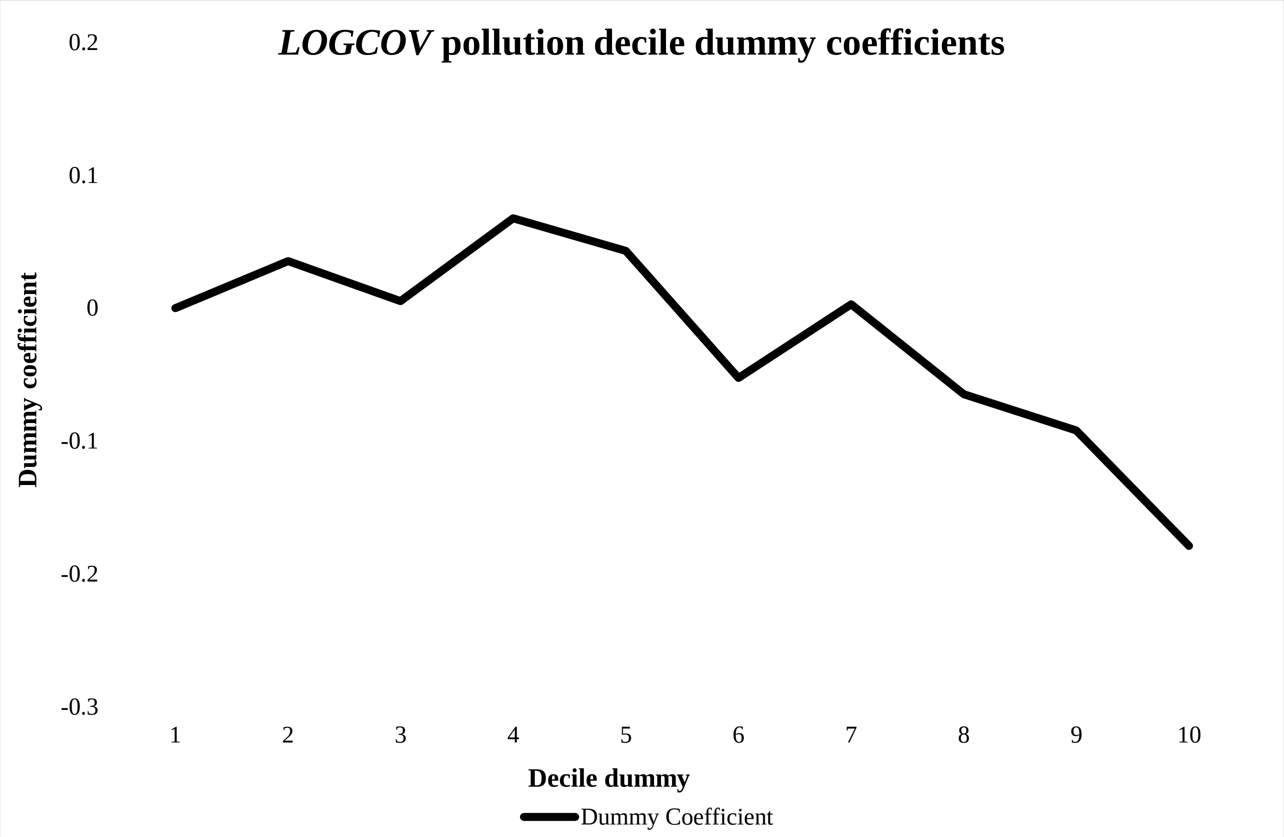
Like estimates from ownership regressions, the estimated coefficient of *Total Releases* is insignificant, suggesting a non-linear relationship between analyst coverage and pollution levels. The estimated coefficient of *Polluterdummy* is statistically insignificant when industry fixed effects are included, providing no evidence of a within-industry relationship between polluters and analyst coverage, similar to results from the primary institutional ownership test. The polluter-time interaction coefficient is also insignificant, providing no evidence that analyst coverage for polluters is diverging from that of other stocks.

Though average *LOGCOV* is increasing with time in the sample, the regression coefficient of the linear trend is negative; this is a result of the marginal effect of time after controlling for other factors such as firm size.[18] The coefficient of *LOGSIZE* is positive and estimated with significance, and along with the coefficients of *INDBETA* and *SP500*, reveals that firms that are large, operate in risky market-sensitive industries, or are listed on the S&P500 receive greater analyst coverage. The coefficient of *RET* is also estimated with significance, however has a negative sign, indicating that firms that have generated higher returns receive reduced analyst coverage. Overall estimates are consistent with the findings of Hong & Kacperczyk (2009) and support the hypothesis that polluters have reduced analyst coverage, albeit with weaker significance than in ownership tests.

To illustrate the relationship between pollution and analyst coverage, I recreate figure 3 except with analyst coverage as the dependent variable. Specifically, I regress *LOGCOV* on 9 pollution decile dummies, with yearly fixed effects and the same control variables used in table 6. I graph the estimated polluter decile coefficients in figure 4.

[18] The Pearson correlation coefficient between *LOGSIZE* and the trend is 0.30, and is statistically significant at the 1% level. This is not high enough to create severe multicollinearity problems in regressions, however note that if *LOGSIZE* is removed from the vector of control variables, the estimated sign of the trend coefficient reverses.

Figure 5: Pollution decile dummy coefficients, estimated from a yearly fixed effects panel regression with *LOGCOV* as the dependent variable. Upper and lower bounds of the estimated 95% confidence intervals are generated with standard errors adjusted for clustering on year and industry. Decile 1 has no active dummy, and is therefore the benchmark from which the following dummy coefficients are compared against.



Like figure 3, figure 4 depicts a non-monotonic relationship between the LOGCOV and pollution levels on average. Unlike figure 3, however, figure 4 portrays a relatively flatter relationship, without an abrupt reduction in analyst attention at the higher pollution deciles. This is consistent with a weaker significance of *Polluterdummy* coefficient estimates from table 7 as compared to the estimates obtained from the main ownership regressions in table 2.

5. Additional tests

5.1. Investor churn

Institutional ownership of polluting firms may be influenced specifically by institutional investor horizons. In the main results I find that quasi-indexers, which are associated with long-term investment horizons, are more reluctant to own polluter stocks compared to transient firms, which are associated with aggressive short-term trading strategies. I therefore test whether polluter firms are owned by investors with relatively short-term investment horizons. Compared to the disaggregated ownership tests, this additional test serves to explicitly estimate the relationship between institutional investment horizon and polluter stocks. This test also indirectly examines which institutions hold the most polluter stocks, as opposed to disaggregated institutional tests which instead examine which investors own the least amount of polluter stocks relative to their other holdings.

I hypothesise that institutions with short-term horizons are more likely to exhibit arbitrage strategies for sin stocks, and exploit mispricing from the shunned stock effect. Due to the potential for tail event regulatory shocks to cash flows, institutions may also be reluctant to hold polluter stocks over long periods, and instead use these stocks in a 'hot-potato' momentum strategy. If true, these channels will inflate the quarterly trading churn of polluter stocks, ceteris paribus, which will be disproportionately held by institutions with relatively short-term investment horizons.

I test the short-horizon investor hypothesis by aggregating average investor horizons at the firm level. I use a churn variable to proxy for investor horizons as a function of their trading activity. Following Gaspar, Massa, & Matos (2005) I first generate an investor churn variable to measure average institutional investment horizons.

$$IChurn_{j,q} = \frac{\sum_{i\in I}\left|Shares_{i,j,q} * Price_{i,q} - Shares_{i,j,q-1} * Price_{i,q}\right|}{\sum_{i\in I}\left(Shares_{i,j,q} * Price_{i,q} + Shares_{i,j,q-1} * Price_{1,q-1}\right)/2} \quad (3)$$

$IChurn_{j,q}$ is a weighted average measure of the turnover of institution *j* at quarter q. *$Shares_{i,j,q}$* represents institution *j*'s holdings of firm *i*'s shares at the end of quarter q. *$Price_{i,q}$* represents the price of firm *i*'s shares at the end of quarter *t*. The ratio is bounded by 0, with a higher ratio indicating a greater turnover of holdings. I then aggregate the *IChurn* ratio at the firm-quarter level with the following equation.

$$Firmchurn_{i,q} = \frac{\sum_{j\in J}\left(IChurn_{j,q} * Shares_{i,j,q}\right)}{\sum_{j\in J} Shares_{i,j,q}} \quad (4)$$

Firmchurn$_{i,q}$ is a weighted average measure of the institutional churn of the shares of firm *i* at the end of quarter *q*, with weightings proportional to the number of shares held by institution *j* as a percentage of total shares of firm *i* held by institutions. Finally, I take a yearly average of *Firmchurn* for each firm to use as the dependent variable in tests.

$$Firmchurn_{i,t} = \frac{\sum_{q=1}^{4} Firmchurn_{i,q,t}}{4} \qquad (5)$$

In a panel setting, I regress *Firmchurn* on the *Polluterdummy* dummy variable and on the set of control variables used in prior institutional ownership tests.

$$Firmchurn_{i,t} = \alpha + \beta^{polluter} * Polluterdummy_{i,t} + \boldsymbol{\beta^{control}} * \boldsymbol{CONT_{i,t}} + \varepsilon_{\boldsymbol{i,t}} \qquad (6)$$

I control for the firm level independent variables used in ownership regressions, represented by the vector ***CONT***. I also include *DIVYIELD*$_{i,t}$ as an additional control variables in the test (Starks et al. 2017). *DIVYIELD*$_{i,t}$ is the annual dividend yield for firm *i* during year *t*.[19] To control for the impacts of yearly business cycle fluctuations and industry averages, I incorporate yearly and industry fixed effects in the model. I adjust standard errors with two-way clustering on Fama-French industry and year.[20] Results of the regression are presented in table 8.

[19] *DIVYIELD*$_{i,t}$ is generated by dividing the total dividends paid by firm *i* in year *t* by the closing share price at the end of the year. Starks et al. (2017) also include *TURNOVER*$_{i,t}$ as an explanatory variable, which is the average monthly stock turnover ratio in year *t*, calculated by dividing the monthly trading volume of stock *i* by the shares outstanding at the end of the month. I omit *TURNOVER* from the set of control variables due to potential simultaneity concerns; however I find a positive coefficient for *Polluterdummy* that is significant at the 10% level if *TURNOVER* is used as the dependent variable instead of *Firmchurn* in the following regression.

[20] I also find the coefficient estimate for the *Polluterdummy* variable is significant if industry fixed effects are included.

Table 8: Results of the investor horizons fixed effects panel regression, where the dependent variable is *Firmchurn*. I present regression coefficient estimates with t-statistics in brackets below. Standard errors are adjusted by using two-way clustering on industry and year. There are 7,490 firm-year observations in the sample. Compared to main tests, the sample is reduced as I only include stocks that have institutional ownership greater than 0 and use the Bushey permanent key to uniquely identify institutions, which is missing for some data. Significance at the 10% level is denoted with *, at the 5% level with ** and at the 1% level with ***.

Investor horizons panel regression results

Variable	(1)	(2)
Polluterdummy	0.0080**	0.0086***
	(2.22)	(2.97)
INDBETA	0.0058	-0.0004
	(1.47)	(-0.11)
LOGSIZE	0.0027*	0.0021
	(1.66)	(1.14)
LOGMB	0.0021	0.0020
	(0.77)	(0.73)
STD	0.0101***	0.0089***
	(4.81)	(4.18)
PRINV	-0.0367*	-0.0322*
	(-1.83)	(-1.66)
RET	0.0012**	0.0013**
	(2.18)	(2.26)
DIVYIELD	0.0060	0.0064
	(0.43)	(0.48)
NASD	-0.0007	-0.0018
	(-0.18)	(-0.41)
SP500	-0.0307***	-0.0297***
	(-8.69)	(-7.56)
Fixed effects	Year	Year & Industry
Adjusted R^2	0.8030	0.8074

Results for the *Polluterdummy* coefficient estimate is consistent with both a priori expectations and the findings of Starks et al. (2017). Polluter firms have significantly higher institutional investor quarterly churn on average compared to non-polluter firms, indicating that institutions that invest in polluter stocks generally have shorter investment horizons and higher average holdings turnover. The estimated coefficient of *Polluterdummy* rivals that of *INDBETA*, *LOGSIZE*, and *STD*, however as *Polluterdummy* is a binary variable its maximum economic effect is less than that of the mentioned variables. The estimated *Polluterdummy* coefficient remains significant in both models used, suggesting that firm pollution is associated with both within-year and within-industry higher ownership from short-horizon investors. The control variables themselves are revealing; *STD*, *RET* and *SP500* are highly significant explanatory variables, while in contrast to Starks et al. (2017), *DIVYIELD* is insignificant. Overall, results are consistent with the hypothesised relation between polluter sin stocks and short-term investor horizons.

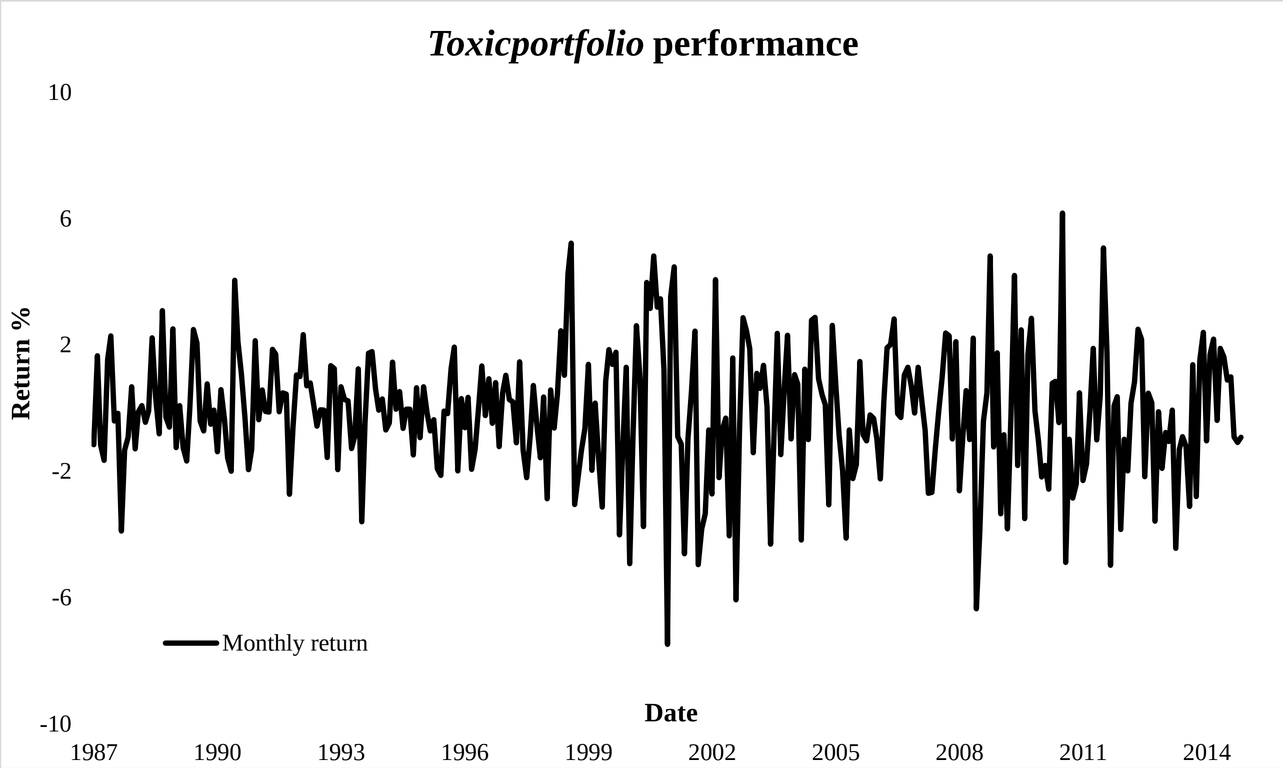
5.2. Polluter portfolio returns

Hong & Kacperczyk (2009) find that a portfolio of sin stocks outperform comparables. Following the prior findings of reduced institutional ownership and analyst coverage of polluters, similar to traditional sin stocks, I test whether polluter stocks also outperform due to the shunned-stock effect. If arbitrage of mispricing for sin stocks is truly limited due to societal discrimination (Akerlof, 1980), polluter stocks should generate abnormal returns over time.

I test whether polluter stocks outperform in the market on average using a portfolio approach. I create a long-short, equal-weighted *Toxicportfolio* that is long on the stocks with the highest quintile of toxic releases and short on the remaining stocks in a year, and is rebalanced every month.[21] Inconsistent with expectations, I find that the *Toxicportfolio* generates -0.138% average returns a month with a Newey-West t-stat of -1.25 over the 335 months in the sample. The time series of *Toxicportfolio* monthly returns are illustrated in figure 5.

[21] I motivate my choice of equal-weights based on the expected equivalence of sin status among polluters regardless of their size. In unreported results I repeat these tests with a value-weighted strategy, and again find non-results. Portfolio sorting and weights are calculated based on one-month lagged information.

Figure 6: A time series of the monthly holding period returns of the long-short *Toxicportfolio* over the sample period.



The average returns of the portfolio strategy are volatile, and do not show any clear evidence of positive abnormal performance. The time series of *Toxicportfolio* performance is not adjusted for common risk factors; I therefore regress the returns of *Toxicportfolio* against three popular benchmark risk models in the following time series regression to test for abnormal returns.

$$R_t = \alpha + \boldsymbol{\beta}^{\boldsymbol{control}} * \boldsymbol{CONT_t} + \varepsilon_t \qquad (7)$$

The returns of *Toxicportfolio, R_t*, are regressed against three sets of benchmark models which consist of the CAPM, the Fama-French 3-factors and the Carhart 4-factors. The variable of interest is α, which is a measure of abnormal returns generated by the *Toxicportfolio* in excess of the benchmark predictions. I adjust standard errors using Newey-West corrections for 5 month lags.[22] Results of the portfolio regression are shown in table 9.

[22] Following the literature I set the lag equal to $4(T/100)^a$ where T = 335 time periods and a = 4/25 using the quadratic spectral kernel. The output equals 4.85, which I round up to 5.

Table 9: Regression results for equal-weighted long-short *Toxicportfolio* average returns. I present the abnormal return estimates along with factor sensitivities to the benchmark models. Standard errors are presented in brackets below. Standard errors are adjusted for Newey-West 5 month lags. There are 335 monthly observations. Significance at the 10% level is denoted with *, at the 5% level with ** and at the 1% level with ***.

***Toxicportfolio* regression results**

	CAPM	FF 3-factors	Carhart 4-factors
α	-0.073	-0.093	-0.139
	(-0.62)	(-0.88)	(-1.22)
MKT	-0.105**	-0.062	-0.047
	(-2.48)	(-1.54)	(-1.15)
SMB		-0.204***	-0.210***
		(-4.89)	(-5.49)
HML		0.063	0.082
		(1.15)	(1.51)
MOM			0.058
			(1.71)
Adjusted R^2	0.0503	0.1667	0.1809

Results provide no evidence of polluter outperformance on average. All three benchmark models provide insignificant estimates of abnormal returns. Contrary to the hypothesis, estimates of portfolio alpha are all negative. The long-short portfolio loads negatively and significantly on the market and SMB factors. Negative factor loadings indicate that polluter firms have lower market betas and behave more like large-cap firms. This is both intuitive and consistent with the summary statistics presented in table 1. On average, polluter firms in the sample have lower market sensitivity and are larger in size. The market beta of the portfolio becomes statistically insignificant as additional risk factors are added. Results are overall inconsistent with the shunned-stock hypothesis. This may be due to weak limits to arbitrage of polluter stocks. There is no evidence to suggest that Type B investors from disaggregated ownership tests are constrained in their investment of polluter firms; similarly, transient and dedicated institutions are not as reluctant to hold polluter stocks as quasi-indexers. These investor groups may contribute to the lack of outperformance of polluter stocks through their arbitrage efforts.

5.3. Robustness reverse causality tests

I consider potential reverse causality in my model. Reverse causality may pose problems of simultaneity in models that estimate the relationship between institutional ownership and firm pollution. One may argue that it is not firm pollution that drives institutional ownership, but instead institutional ownership that affects overall releases through institutional oversight and pressures that may encourage a firm to adopt greener policies; however, this is unlikely to invalidate results for the following reasons. As there may also be pressure from retail or public sector owners to reduce pollution, there should be no causal flow from *IO* to pollution, as *IO* measures the ratio of institutional to total ownership. Furthermore, because *IO* is representative of total institutional ownership, aggregate institutional pressures on polluters depend on the average institution, which may promote or discourage pollution based on its individual incentives. Due to the high dollar and time costs required to develop efficiency in pollution, potential institutional pressure to reduce toxic releases are also more likely to occur over a longer time-frame compared to a contemporaneous year. *IO* is measured at year end, whereas *Total Releases* is based on pollution throughout the year, further weakening the likelihood of the ownership variable having a causal relationship with the firm pollution measure used in tests. For robustness however, I use a PVAR, a change-on-change analysis, and a natural experiment to test the reverse causality hypothesis.

I first estimate the following PVAR model to test for simultaneity in my main results. To account for non-stationarity, I use first order differences of all variables. The PVAR simultaneously estimates the effects of lagged changes in institutional ownership on changes in toxic releases and vice versa, whilst controlling for the effects of lagged changes in each variable.

$$\boldsymbol{Y_{i,t}} = \boldsymbol{\alpha} + \boldsymbol{\beta_1} * \boldsymbol{Y_{i,t-1}} + \boldsymbol{\beta_2} * \boldsymbol{Y_{i,t-2}} + \boldsymbol{\beta_3} * \boldsymbol{Y_{i,t-3}} + \boldsymbol{\beta_4} * \boldsymbol{X_{i,t}} + \boldsymbol{\varepsilon_t} \qquad (8)$$

The matrix $\boldsymbol{Y_{i,t}}$ consists of the variables $\Delta IO_{i,t}$ and $\Delta TRI_{i,t}$, while the exogenous $\boldsymbol{X_{i,t}}$ consists of the first order differences in control variables used in ownership tests. I drop $\Delta NASD$ from the set of control variables as it does not vary within the sample. I control for 3 lags of dependent variables in the model. If reverse causality is present, coefficients of ΔIO or its lags should be estimated with statistical significance, and the IRF should show statistically significant effects of shocks in ΔIO on ΔTRI. I illustrate the results of the PVAR and corresponding IRF's in the following tables and figures.

Table 10: PVAR regression results testing for reverse causality in the institutional ownership regressions. I use STATA code for the PVAR developed in Abrigo & Love's (2015) working paper. Dependent variables are IO and TRI, with 3 lags. The exogeneous *NASD* dummy is omitted from the regression due to no variation in the reduced sample of 5,538 observations. I present PVAR coefficient estimates with t-statistics in brackets below. Significance at the 10% level is denoted with *, at the 5% level with ** and at the 1% level with ***.

PVAR estimates

Independent variables	Dependent variables	
	ΔIO	*ΔTotal Releases*
$\Delta IO_{(-1)}$	-0.1278***	0.3219
	(-5.20)	(0.85)
$\Delta IO_{(-2)}$	0.0158	0.2157
	(0.85)	(0.51)
$\Delta IO_{(-3)}$	0.0180	0.5837
	(0.81)	(1.49)
$\Delta Total\ Releases_{(-1)}$	-0.0004**	0.1186
	(-1.99)	(0.55)
$\Delta Total\ Releases_{(-2)}$	0.0000	-0.0816
	(0.24)	(-0.54)
$\Delta Total\ Releases_{(-3)}$	-0.0001	0.1015
	(-1.43)	(1.53)
$\Delta INDBETA$	0.0969***	0.5048
	(6.68)	(0.61)
$\Delta LOGSIZE$	0.0817***	0.0088
	(10.99)	(0.04)
$\Delta LOGMB$	0.0026	-0.0274
	(0.56)	(-0.36)
ΔSTD	-0.0028*	-0.0283
	(-1.81)	(-0.53)
$\Delta PRINV$	0.0080	0.0435
	(0.50)	(0.13)
ΔRET	-0.0035***	0.0005
	(-7.23)	(0.03)
$\Delta SP500$	0.0127	-0.5872
	(0.92)	(-1.38)

Table 11: Impulse response results of the PVAR model in tabulated format.

Impulse response function		
Response variable and forecast horizon	**Impulse variables**	
Δ*IO*	Δ*IO*	Δ*Total Releases*
0	1	0
1	-0.1278	-0.0004
2	0.0319	0.0000
3	0.0118	-0.0001
4	-0.0036	0.0000
5	0.0012	0.0000
6	0.0000	0.0000
7	-0.0001	0.0000
8	0.0000	0.0000
9	0.0000	0.0000
10	0.0000	0.0000
Δ*Total Releases*		
0	0	1
1	0.3219	0.1186
2	0.2127	-0.0678
3	0.5653	0.0837
4	0.0184	0.0272
5	-0.0023	-0.0105
6	0.0621	0.0050
7	0.0076	0.0042
8	-0.0037	-0.0010
9	0.0052	0.0000
10	0.0017	0.0005

Figure 7: Impulse response function for the PVAR model. The impulse variable is *ΔTRI* while the response variable is *ΔIO*. The dark bands around the impulse response estimate represent 95% confidence intervals generated with bootstrapped standard errors from 1000 random draws.

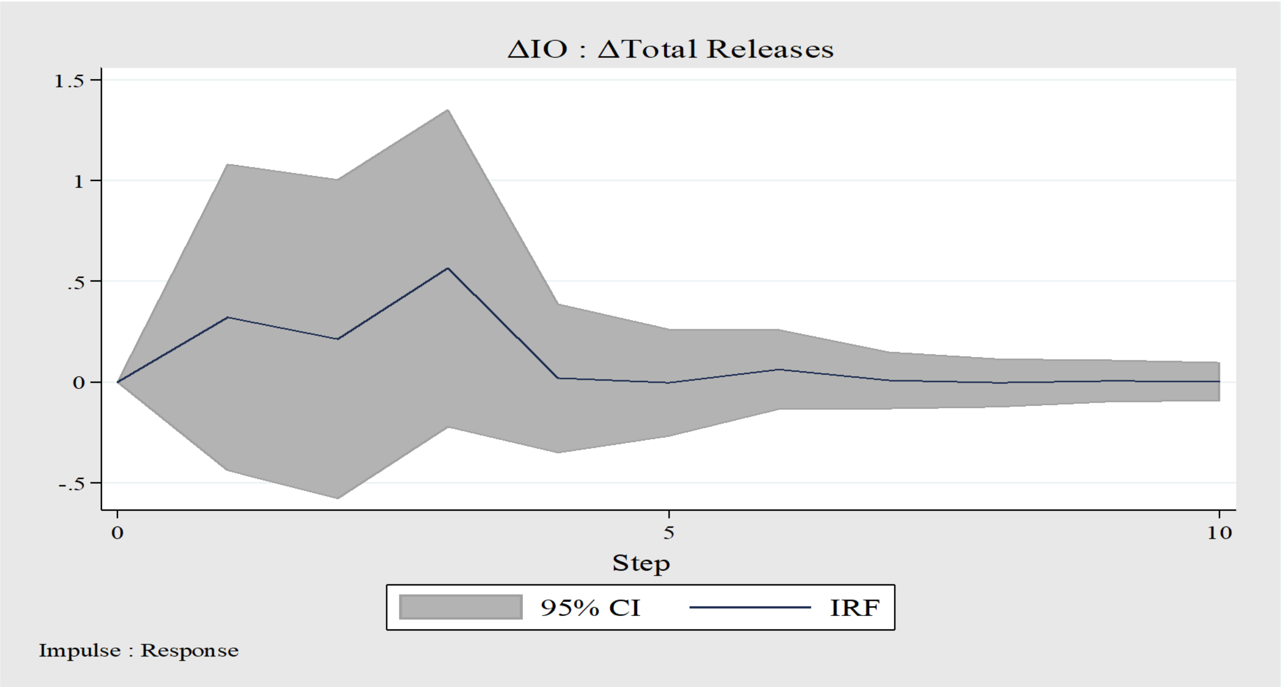
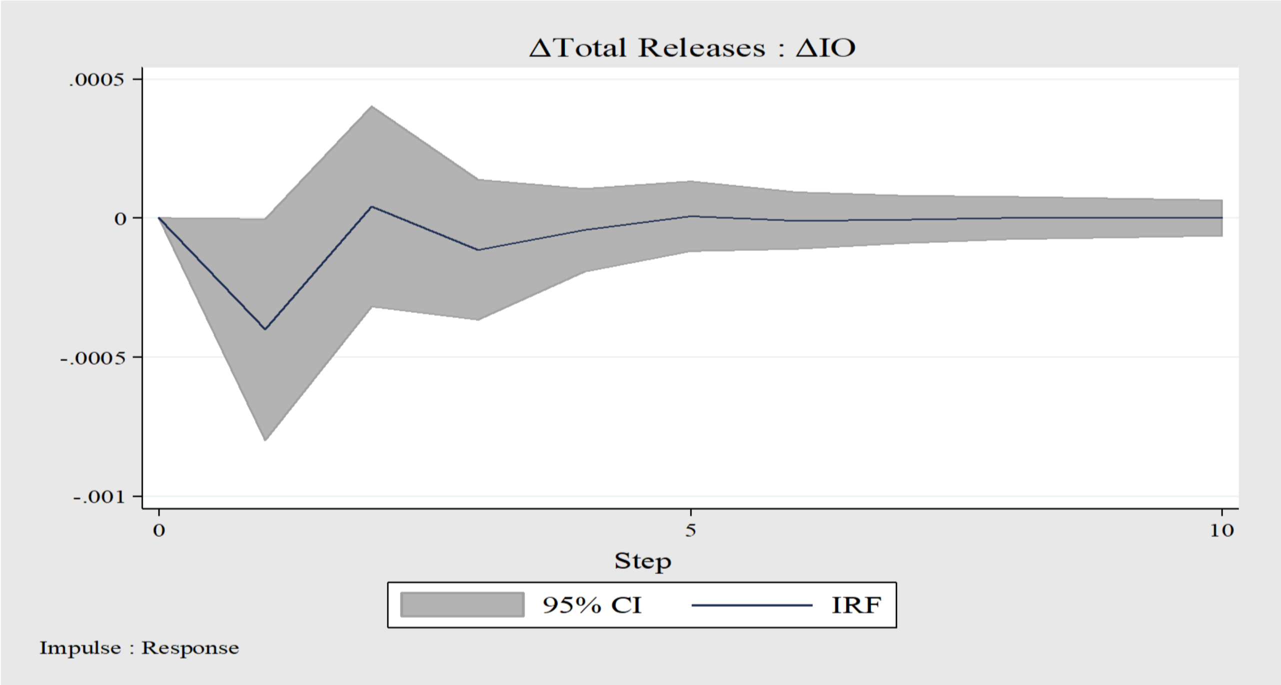


Figure 8: Impulse response function for the PVAR model. The impulse variable is *ΔIO* while the response variable is *ΔTRI*. The dark bands around the impulse response estimate represent 95% confidence intervals generated with bootstrapped standard errors from 1000 random draws.



Results provide evidence that Δ*IO* is positively affected by one-year lagged values of Δ*Total Releases*, suggesting the presence of short-run causality in favour of my primary hypothesis. More importantly, there is no evidence of a relation between Δ*Total Releases* and lagged values of Δ*IO*. The t-stat of the estimated effect of one-year lagged Δ*Total Releases* on Δ*IO* is much higher than vice versa. Furthermore, figure 6's IRF graph illustrates that the 95% confidence interval contains 0, and therefore provides no evidence of a causal short-term effect of Δ*IO* shocks on Δ*Total Releases*. For robustness I conduct the same test using only the subsample of firms that have a value of 1 for the *Polluterdummy* dummy variable, and again find no evidence of reverse causality.

I additionally use a change-on-change analysis to test for Granger causality, and estimate whether lagged changes in *IO* lead to changes in *Total Releases.*[23] I conduct this test with the following specification.

[23] For example, see Aggarwal, Erel, Ferreira, & Matos (2011), Chhaochharia, Kumar, & Niessen-Ruenzi (2012) or Kim et al. (2014)).

$$\Delta Y_{i,t} = \alpha + \beta^{x} * \Delta X_{i,t-1} + \boldsymbol{\beta^{cont}} * \Delta \boldsymbol{CONT_{i,t-1}} + \varepsilon_{i,t} \tag{9}$$

The change in the dependent variable from *t-1* to *t* is regressed on the change in the independent variable and vector of control variables from *t-2* to *t-1*. I run this regression twice, once with the change in *IO* as the dependent variable and the lagged change in *Total Releases* as the independent variable, and vice versa. I also include yearly fixed effects to control for time-varying heterogeneities. I present results of the two regressions in table 12.

Table 12: Change-on-change analysis fixed effect panel regression results. Changes in the dependent variables *IO* are regressed against lagged changes in *TRI* and vice versa, controlling for lagged changes in prior ownership variables. The exogeneous *NASD* dummy is omitted from the regression due to no variation in the reduced sample of 7,360 firm-year observations. I present coefficient estimates with t-statistics in brackets below. Standard errors are calculated with clustering on industry and year. Significance at the 10% level is denoted with *, at the 5% level with ** and at the 1% level with ***.

Change-on-change analysis

Independent variables	Dependent variables	
	ΔIO_t	$\Delta Total\ Releases_t$
$\Delta Total\ Releases_{t-1}$	-0.0001**	
	(2.42)	
ΔIO_{t-1}		0.1244
		(0.99)
$\Delta INDBETA_{t-1}$	0.0062	0.8684
	(0.39)	(1.01)
$\Delta LOGSIZE_{t-1}$	-0.0021	-0.2483
	(-0.51)	(-1.00)
$\Delta LOGMB_{t-1}$	-0.0008	-0.0124
	(-0.20)	(-0.11)
ΔSTD_{t-1}	0.0003	0.0007
	(0.10)	(0.01)
$\Delta PRINV_{t-1}$	-0.0400**	-0.4024
	(-2.67)	(-0.65)
ΔRET_{t-1}	0.0011*	0.0042
	(2.02)	(0.26)
$\Delta SP500_{t-1}$	-0.0044	-0.1811
	(-0.50)	(-1.34)
Fixed effects	Year	Year
Adjusted R^2	0.1152	0.0011

Results again do not provide any evidence of a relationship between lagged changes in ownership and current changes in firm pollution. While the negative effect of lagged changes in pollution on contemporaneous changes in institutional ownership is estimated with significance at the 5% level, estimates of the reciprocal relationship are insignificant. For increased robustness, I also repeat this test with the subsample of firms identified as polluters with the *Polluterdummy* variable, but find a consistent lack of results.

In a final robustness test, I use a natural experiment which causes independent variation in *IO*. Specifically, I examine whether firms that have been added to or removed from the S&P500 index have significant changes in their pollution. I hypothesise that inclusion or exclusion from the index affects the institutional ownership of the stock, and test for a corresponding effect on *Total Releases* which would provide evidence of reverse causality. Filtering by firms that have had a change in the value of *SP500* from the previous year, I generate a sample of 55 firms that have been included in the index in the previous year and 22 firms that have been removed as index constituents since the previous year, for a total of 77 firm-year observations. I then compare the changes in institutional ownership to changes in pollution levels with the following panel regression.

$$\Delta Total\ Releases_{i,t} = \alpha + \beta * \Delta IO_{i,t} + \varepsilon_{i,t} \qquad (10)$$

I regress contemporaneous changes in pollution levels on the changes in institutional ownership following this exogeneous shock on the S&P500 index constituent status. If there is a causal relationship between *IO* and *Total Releases*, I expect a statistically significant negative coefficient estimate. In order to capture time heterogeneities I include yearly fixed effects, and use two-way clustered standard errors on year and industry.[24] I present the results of this regression in table 13, along with the average changes in *IO* and *Total Releases* following inclusion or exclusion from the S&P500.

[24] I find similar non-results if I include industry fixed effects or drop fixed effects from the panel altogether.

Table 13: S&P500 constituent change analysis. I limit the sample to firms that have been recently included or excluded as constituents of the S&P500 index in the prior year, grouped as recent inclusions, exclusions or both. I present the mean ΔIO and Δ*Total Releases* for the year following the status change. In the bottom half of the table are regression coefficients generated by regressing *ΔTotal Releases* on contemporaneous *ΔIO* with yearly fixed effects. I present coefficient estimates with t-statistics in brackets below. Standard errors are calculated with clustering on industry and year. Significance at the 10% level is denoted with *, at the 5% level with ** and at the 1% level with ***.

S&P500 constituent change analysis

	Recently included	Recently excluded	Total sample
Mean ΔIO_t	0.0394**	-0.0103	0.0252*
	(2.36)	(-0.36)	(1.72)
Mean Δ*Total Releases*$_t$	-0.3160	-0.1741	-0.2754
	(-0.61)	(-0.60)	(-0.73)
ΔIO_t	-6.3445	1.0342	-1.1323
	(-1.25)	(0.69)	(-0.40)
N	55	22	77
Fixed effects	Year	Year	Year

Following shocks to firm S&P500 constituent status, I find no evidence of any effect of a change in institutional ownership on firm pollution levels. This test provides no evidence of reverse causality between the variables. The estimated coefficient sign is negative for only two out of the three samples, and in the 'recently excluded' subsample the sign is positive; all three estimated regression coefficients are insignificant. In an unreported test, I find that replacing the regression dependent variable with any one of Δ*Total Releases*$_{t+1}$, Δ*Total Releases*$_{t+2}$ or Δ*Total Releases*$_{t+3}$ also generates insignificant results.

Overall the results of these tests fail to find evidence of a relationship flowing from *IO* to *Total Releases*; no model finds evidence of a causal effect of institutional ownership on firm level toxic releases.

6. Conclusion

I argue that institutional investors are constrained through discriminatory social norms, which limit investments in polluter stocks. This is more likely to be the case for institutions due to their large public profiles which are more easily exposed to public scrutiny compared to that of individual investors, who are more capable of keeping their positions in sin stocks out of the public eye. Overall, results reveal that polluter stocks are shunned by institutional investors, similar to the sin stocks of tobacco, alcohol and gambling (Hong & Kacperczyk, 2009). Results also reveal that while institutional investment in equities is increasing over the sample period, ownership of polluter firms is increasing at a significantly slower rate. I attribute these findings to growing environmental sentiment, which include concerns relating to human health and environmental damage. When disaggregated by trading strategy based on Bushee (1998) groups, all three institution groups are found to have reduced holdings of polluter stocks at varying levels. Similarly, disaggregation by 13F institution type reveals that institutions more likely to be constrained by society also have reduced polluter ownership, while institutions with relatively more opaque or aggressive strategies have increased ownership. Following findings of reduced institutional ownership, I also find that polluter stocks receive less analyst coverage than their comparables. Auxiliary tests reveal that polluter stocks are held by investors with shorter investment horizons, measured through the quarterly churn in their holdings, however tests do not provide evidence of either underperformance or outperformance of polluter stocks. The results of this study are consistent with the theory that society shuns environmental sin stocks as a reaction of their costs to social welfare; the ownership of polluter stocks generates disutility or costs from association that exceed their benefits.

Topics on the impacts of environmentalism on corporate finance are gaining momentum as a literature. Further research could incorporate high frequency pollution and investor trading data to estimate causal drivers in investor decision making, or alternatively to examine potential window dressing in holdings disclosures. The reduced institutional ownership of polluter firms implies that the remainder of the stocks are held by either retail investors, the public sector or insiders, however these ownership channels are outside the scope of this paper and should be investigated. Further research could also study how the physical consequences of pollution (i.e. smog, health problems, toxic spills) affect the decision making of the investors that suffer the consequences. Finally, the role of regulation is crucial to assessing the performance of both polluter and green stocks; research into environmental-regulatory risk channels and expected stock returns is currently a developing field of asset pricing.

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Appendix

Table 14: Robustness test results, where regression (1) is repeated with additional corporate governance control variables based on KLD ratings. Governance control variables include limited managerial compensation (cgov_str_a), excessive managerial compensation (cgov_con_b), investment in other meritable companies (cgov_str_c), strong reporting quality (cgov_str_d), weak reporting quality (cgov_con_h), the and the total number of governance strengths and concerns (cgov_str_num and cgov_con_num respectively). More information on these variables can be found through the WRDS MSCI ESG KLD STATS variable description page. I present regression coefficient estimates with t-statistics in brackets below. Standard errors are adjusted by using two-way clustering on industry and year. There are 1,115 firm-year observations in the sample for each specification. Significance at the 10% level is denoted with *, at the 5% level with ** and at the 1% level with ***.

Robustness test of model (1) with corporate governance control variables

Variable	(1)	(2)	(3)
Polluterdummy	-0.0497**	-0.0311	-0.0487**
	(-2.18)	(-0.74)	(-2.13)
INDBETA	0.0886***	0.0120	0.0915***
	(4.72)	(0.74)	(4.66)
LOGSIZE	-0.0120**	-0.0086	-0.0121
	(-1.00)	(-0.67)	(-0.99)
LOGMB	0.0008	-0.0103	0.0030
	(0.05)	(-0.60)	(0.19)
STD	-0.0116	-0.0131	-0.0145**
	(-1.27)	(-1.32)	(-2.16)
PRINV	-0.2389***	-0.2485***	-0.2317***
	(-4.87)	(-5.59)	(-4.18)
RET	0.0004	0.0008	0.0003
	(0.22)	(0.27)	(0.60)
NASD	-0.0522**	-0.0647**	-0.0514**
	(-2.45)	(-2.54)	(-2.44)
SP500	0.0022	0.0119	-0.0005
	(0.09)	(0.42)	(-0.02)
cgov_str_a	-0.0680	-0.0750	-0.0688
	(-1.44)	(-1.27)	(-1.44)
cgov_con_b	0.1004***	0.0850***	0.1011***
	(4.03)	(2.81)	(4.14)
cgov_str_c	-0.1012***	-0.1316***	-0.1041***
	(-4.30)	(-4.33)	(-3.87)
cgov_str_d	-0.0620	-0.0580	-0.0640
	(-0.96)	(-0.79)	(-1.00)
cgov_con_h	0.0910***	0.0958***	0.0854***
	(3.93)	(3.62)	(3.59)
cgov_str_num	0.0064	0.0085	0.0076
	(0.15)	(0.16)	(0.18)
cgov_con_num	-0.0301	-0.0266	-0.0291
	(-1.49)	(-1.38)	(-1.42)
Fixed effects	Year	Year & Industry	None
Adjusted R^2	0.1451	0.1934	0.1435