The SEC's Publication of an Online Tool for Detecting Firms Doing Business in Countries Designated as State Sponsors of Terrorism

Short-term Investor Reaction and Long-Term Consequences

by

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Abstract. In this paper, we study the impact of moral considerations on the equity market using the example of firms being stigmatised as terror supporting. In summer 2007, the SEC published an online-tool with information on firms doing business in State Sponsor of Terrorism countries. We take sides with those arguing that for moral reasons, investors will have traded on the information provided in the tool by selling stocks of mentioned firms. Hence, we hypothesise that stock prices of stigmatised sample firms will have fallen. Contrary to our expectation, we find no evidence of a negative stock price reaction during the time the tool was posted online but do find such reaction subsequent to the tool's publication. Additionally, we provide evidence for the notion that stigmatisation is not transitory: During a long-time, four year period subsequent to the tool's suspension, investors were able to earn positive abnormal returns from holding an equally weighted portfolio of sample firms.

Keywords: SEC, sin stocks, State Sponsors of Terrorism, stock price reaction

JEL-classification: G11, G12, G14, G18

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1. Introduction

"No investor should ever have to wonder whether his or her investments or retirement savings are indirectly subsidising a terrorist haven or genocidal state," said Christopher Cox, former chairman of the SEC (U.S. Securities and Exchange Commission, 2007a). In reaction to the widespread demand for easy access to information about firms' potential business ties with countries designated as State Sponsors of Terrorism, the SEC provided an online-tool on its webpage in June/July 2007. In this context, State Sponsors of Terrorism are such countries that are determined by the Secretary of State to have repeatedly provided support for acts of international terrorism according to three laws: section 6(j) of the Export Administration Act, section 40 of the Arms Export Control Act, and section 620A of the Foreign Assistance Act (U.S. Department of State, 2011; retrieved at http://www.state.gov/s/ct/c14151.htm).

Before that SEC online tool, other lists compiled by think tanks circulated in the web (Hemphill and Cullari, 2010), but did not receive attention comparable to the SEC's tool. The tool's publication by the SEC on June 25th, 2007, was the first attempt of a federal agency to make information about firms doing business in State Sponsor of Terrorism countries available to investors in a consolidated form. By clicking on the tab "State Sponsors of Terrorism", users were directed to a list of countries designated as State Sponsors of Terrorism by the U.S Department of State (U.S. Securities and Exchange Commission, 2007a). At the time the tool was online, these countries were Cuba, Iran, North Korea, Sudan, and Syria (U.S. Securities and Exchange Commission, 2007b). In the country specific menus, firms with business ties to a respective country were listed. Some firms were listed only once, while others were accused of doing business in all five countries designated as State Sponsors of Terrorism. When clicking on one of the listed firms, users were transmitted to the full text of the firm's latest annual report as filed with the SEC (forms 10-K, 20-F, and 40-F). Of the 90 firms mentioned, most firms were large multina-

tionals with a comprehensive distribution network. They were either active by selling goods in designated countries themselves or third party distributors did the job for them. The industry most prevalent in the sample is the oil industry. Nine sample firms belong to the two-digit SIC code number 13, labelled "oil and gas extraction". Another group of six sample firms is assigned to SIC code 29, named "petroleum refining and related industries". Both codes fall into the oil industry following the Fama and French (1997) classification scheme of 49 industries. Among these firms, involvement in Iran is the predominant reason for list inclusion. This does not come as a surprise as Iran ranks 3rd in the world in natural oil reserves (U.S. Energy Information Administration, 2009).

The Financial Times was the first among a handful of newspapers to report on the tool's existence. In the article "SEC's anti-terror tool angers companies" (Grant, 2007) the author expressed reservation regarding the online tool's general suitability. It was argued that the tool did not encompass any information on whether the firm still operated in one of the designated countries or if it had already abandoned its operations. The second argument put forward was the lack of information on how material the respective firm's involvement in the country actually was. In the tool, the SEC added no information beyond what was written in the annual reports. Some annual reports just included the name of one or more of the designated countries without any quantification on how big the firm's involvement was. Todd M. Malan (2008), president and CEO of the Organisation for International Investment (representing the interests of foreign firms listed at US stock exchanges) feared that investors would unwisely trade on the information that a particular firm had been added to the list:

> "(…) we believe there is a very real risk that investors will see a company name on the SEC's web list and make an investment decision without "clicking through" to

read the context, having made the assumption that the SEC would not have put the company on the list in the first place if there was no reason for concern."

Although the site had been very popular as far as the sheer number of visitors are concerned (according to the SEC, material posted on the tool was clicked on for more than 150,000 times), the SEC took the tool offline on July 15th, 2007, for temporary suspension. According to the SEC, concerns about the fact that information accessible via the tool might not reflect a firm's latest actions in the matter were taken seriously, which is why the tool was supposed to undergo temporary reconstruction (U.S. Securities and Exchange Commission, 2007b). As a next step, on November 16th, 2007, the SEC sent out a concept release, requesting the public to comment on the question whether the SEC should again engage in developing "mechanisms to facilitate greater access to companies' disclosures concerning their business activities in or with countries designated as State Sponsors of Terrorism (U.S. Securities and Exchange Commission, 2007c). Predominantly, respondents argued that a tool like the one posted in June/July 2007 did not appropriately serve its purpose of accurate and unbiased investor information. May it be in lack of a suitable alternative to the online-tool or, as Todd M. Malan (2008) had mentioned, because the entire topic of terror-free investing had shifted to the private sector: To date, we are not aware of any publication that compares to the online-tool published in June/July 2007.

The analysis conducted in this paper will give answer to the concern of many affected firms as it was formulated by Werner Schnappauf (2008), by that time director general and member of the presidential board of the Federation of German Industries (BDI): "References to 'terrorism' and 'State Sponsors of Terrorism' could provoke strong reactions by investors and other users, which could lead to imprudent investment decisions." The concern was that investors might be encouraged by the tool to trade based on the information provided, selling stocks of firms with business ties to State Sponsor of Terrorism countries.

In order to investigate whether such claims were justified, we first use an event study methodology for assessing any abnormal stock price reactions of affected firms following the publication of the online-tool. If the concerns and the anger were appropriate, affected firms should on average have incurred negative abnormal returns when the tool was provided to investors wishing to "divest terror". The event itself is not, in contrast to the majority of event studies conducted, the firms' publication of the information that they do business in these countries. What we define as the event is the SEC, as a federal agency and with the explicit mission to protect investors, stigmatising a group of firms as behaving in a morally questionable way by doing business in countries that the U.S. department of state labels State Sponsors of Terrorism. It is the official labelling by the SEC that we suppose has induced investors to sell stocks, not the bare information disclosed in the respective firms' SEC filings.

Second, we investigate whether trading on the information provided in the tool offers the possibility to earn long-term abnormal returns. If investors sold stocks of firms mentioned in the tool, their prices will subsequently be depressed as compared to their fundamental value. An investor long in the portfolio of stigmatised firms could thus earn positive abnormal returns in the period after the event. We test this hypothesis by conducting an alpha-study in which returns from holding the portfolio of stigmatised stocks are estimated with a global version of the Fama and French (1993) three-factor model (F-F model). If our hypothesis is to be true, the corresponding alpha should be positive for the period succeeding the tool's publication.

Our paper is organised as follows: The next section provides an overview of related literature and sets out our academic contribution. Section 3 describes and summarises the data. In Section 4, we investigate the short-term effect of the tool's publication: In its first part, the event study methodology is described, before results are presented in its second part. In the third part, we check the results for robustness. Section 5 is about the long-term effects of the tool's publication.

In its first part, we briefly describe the methodology used for estimating long-term return performance of the designated firms. In the second part of Section 5, we present outcomes for the long-term effect analysis. Results are checked for robustness in part three. Section 6 concludes.

2. Related literature and contribution

This paper aims at investigating the effects of moral views on the financial performance of a group of firms. Therefore, it is related to studies examining the interplay between social norms and financial markets. Early research on this topic comes from Becker (1957). He sets out a framework in which agents in the market place are assumed to discriminate because of "race, religion, sex, color, social class, personality, or other non-pecuniary considerations". As a result, the discriminating agents reduce their own income as well as that of the people they discriminate against. Arbel and Strebel (1982) specifically apply this theory to financial markets. They find that some firms are much closer followed by analysts than others. Relating to the framework established by Becker (1957), this means that firms less covered by analysts are the ones discriminated against in the sense that they get less attention than other firms. The authors detect a negative relation between excess returns and their measure of analyst coverage.

Probably the most important field of research in the area of discrimination on financial markets is socially responsible investing (SRI), defined as "an investment discipline that considers environmental, social and corporate governance (ESG) criteria to generate long-term competitive financial returns and positive societal impact" (The U.S. Social Investment Forum, 2010). With his guide on how to identify socially responsible stocks, Moskowitz (1972) was among the first scholars to publish studies on SRI investing. Since then, the research field has become increasingly popular: Margolis et al. (2009) count 251 studies analysing the relationship between corporate social performance and corporate financial performance. Of these studies, 106 have been

published in the period between 1998 and 2007. In their meta-analysis, the authors reveal a small positive effect of social responsible behaviour and financial performance. However, the only discernable effect is detected for cases in which firms displayed substantial socially irresponsible behaviour.

While SRI investing either means considering only those stocks that are associated with exceptionally good social responsibility performance (positive screening) or those that are not associated with poor social responsibility performance (negative screening), other academic research as well as popular literature has drawn attention to the reverse side of SRI – sin stocks. Investor guides such as "Stocking up on sin: How to crush the market with vice-based investing" (Waxler, 2004) or "Investing in vice: The recession-proof portfolio of booze, bets, bombs & butts" (Ahrens, 2004) give guidance on successfully investing in firms whose operations are regarded as sinful. In the academic world, the first scholars to relate to potential return advantages of sin stocks are Angel and Rivoli (1997). The authors suppose that stocks avoided by a certain group of investors – as sin stocks are avoided by socially responsible investors – will have higher expected returns than "ordinary" stocks. They further suppose that the return differential is higher, the higher the proportion of sin stock avoiding investors in the market. Essentially, they use the same reasoning as Arbel and Strebel (1982). In a seminal paper, Hong and Kacperczyk (2009) provide empirical evidence for the assertion of Angel and Rivoli (1997). In a long-term return analysis, the authors find significant positive abnormal returns for a portfolio of sin stocks after controlling for usual predictors of return performance. Furthermore, the authors find that stocks of firms operating in sin industries are less held by institutional investors subject to norm constraints (e.g. pension funds or endowment funds) and less followed by analysts, who are believed to primarily cater on such institutional investors. As the reason for the stocks' outperformance, the authors find that the "neglect effect" is causing sin stocks to outperform their peers on a risk-

adjusted basis. Shunned by a large investor basis consisting of norm constrained institutional investors and norm conscious individuals, sin stocks trade cheaply as compared to fundamentals and thus offer superior risk-adjusted returns to the investor.

Also closely related to this paper are studies by Teoh et al. (1999) as well as Rock (2003). Teoh et al. (1999) investigate the impact that the boycott of South Africa in the late 1980's had on the South African financial market. Political and public pressure was exerted on firms investing in South Africa in order to make them withdraw their investments. Contrary to what had been reported in the financial media, the authors find no negative impact of legislative and shareholder boycott on either the valuation of the South African financial market or on firms with operations in South Africa.

Rock (2003) uses an event study methodology in order to examine the effect of public disclosure about sweatshop practices on firms. During a five year period ranging from the year 1996 through 2000, he identifies eight multinational clothing firms for which he assesses the impact of incidents where sweatshop practices become public. Results suggest that firms were indeed punished by investors for relying on such practices.

As the online tool is about firms doing business in terror supporting countries, this paper is also related to research in the field of terrorism and its impact on financial markets. Especially since the September 11th, 2001, attacks, a vast body of literature examines the links between terrorism and the stock market. Chen and Siems (2004) study the U.S. capital market's response to 14 terrorist/military attacks and global capital markets' response to the Iraq's invasion of Kuwait and the September 11th, 2001, terrorist attacks. Karolyi and Martell (2010) investigate stock price reactions to terrorist attacks of 75 U.S. and international firms. Brounen and Derwall (2010) analyse the impact of terrorist attacks on stock prices and compare the reactions following these events to those following earthquakes. Chesney et al. (2010) study the impact of terrorism on the

behaviour of stock, bond and commodity markets over a period of 11 years. Like these studies, the vast majority of studies in the field of terrorism related research in finance are confined to short-term effects.

The paper most closely related to ours and also the sole study to focus on the longer-run consequences of terrorism on investment returns and risks is Karolyi (2008). He investigates two investment strategies based on the S&P 500 investment universe: Investment in firms that operate in countries subject to frequent terrorist attacks on the basis of a terrorism-related risk score on the country level as well as investment in firms that are "terror-free" and therefore do not operate in countries designated as State Sponsors of Terrorism. The results he obtains indicate positive abnormal returns of both investment strategies that are small in economic magnitude and statistically insignificant.

Albeit the vast body of SRI related research, there is still a lot that has to be understood about the reverse side of SRI investing. We are the first to take a look at this reverse side in the special field of terror-free investing. While Karolyi (2008) in his analysis of a "terror-free" portfolio considers a broad equity universe and excludes firms with business ties to "terror countries", we investigate exactly those sinfully behaving firms. Both our investigations, the short term impact and the longer-run consequences of firm stigmatization due to potentially terror related operations have not been analysed, so far. If, as we hypothesise, investors are indeed guided by moral considerations when meeting investment decisions with respect to firms operating in State Sponsor of Terrorism Countries, this is impacting the trade-off a firm faces when confronted with the question of whether or not to operate in such countries. Should reputational risk materialise in poor stock market performance, managerial decision making will be enriched by an additional factor to be considered. That said, we make a completely new contribution to the field of terror-

free investing. Putting this into the big picture, we contribute to the understanding of how social norms affect capital markets.

3. Data

Since the SEC withdrew the online tool from its webpage in July 2007, the list of firms accused of having business ties with State Sponsor of Terrorism countries is no longer available online. For the purpose of this study, QVM group – a private investment advisory firm – generously provided us with the list of firms mentioned in the tool.

The original list as provided by QVM encompasses 90 firms. For the analysis we had to exclude seven firms because of merger, no trading or missing data. Additionally, we excluded two Argentinean firms because the required input for the model parameters was not available for the Argentinean market. Of the remaining 81 firms, 29 are US based and 52 have headquarters domiciled outside the US. While all of the US firms were directly listed, foreign firms predominantly listed equity at US stock exchanges in the form of ADRs (American Depositary Receipts).

Panel A of Table 1 displays an overview of the sample firms' size as measured in market capitalization as well as total assets in the year 2007. The average sample firm has a market capitalization of $ 39 billion, while the median sample firm has a market cap of $ 13 billion. Breaking the sample up by geographical firm origin reveals that non-US sample firms are considerably larger than US sample firms. This observation is in line with the results of Foerster and Karolyi (1999, p. 986), who declare that "cross-listed firms tend to be very large." With a market value of equity of $ 54 billion, the average non-US sample firm is more than four times larger than the corresponding US firm ($ 13 billion). This picture is even more pronounced if one takes a look at total assets. By this measure, the average non-US firm is about 25 times larger than the average US firm in the sample ($ 297 billion and $ 12 billion, respectively). Since banks as an industry

are known to be more asset intensive than others, the large discrepancy when size is measured in assets comes from the fact that all five banks in the sample are non-US banks. Nevertheless, it is worth noting that by both standards, non-US firms are substantially larger than US firms. Non-US firms almost exclusively belong to the category of large multinationals, while many US firms on the SEC's list do not belong to this category. In Panel B of Table 1, we provide an overview of the sample firms' market to book ratios. The average firm in the sample has a market to book ratio of 2.5. Contrary to what we observed in the case of firm size, we do not recognise considerable differences between non-US and US sample firms.

>>> Insert Table 1 about here <<<

Since the SEC as a federal agency is only responsible for firms listed at US American stock exchanges, firms not listed at US exchanges but with business ties to State Sponsor of Terrorism countries would of course not have been on the list. Of the firms on the list, many have more than one listing. This primarily applies to the non-US firms on the list. For these firms, the US equity market represents an attractive opportunity to address investors that would otherwise not have invested in the respective firm. In addition to their listing at a US exchange, these firms are listed at their respective home exchange, which also serves as their primary exchange. As displayed in Table 2, primary exchange also means that most trades are executed via these home exchanges. Trading at US exchanges makes up for a mere 6% of the combined primary exchange and US exchange trading volume for non-US firms. This number of 6% is substantially lower than what has been found in other studies. Marosi and Massoud (2008) report a relative trading volume ratio of 36% for the control sample they use. However, their number is likely to be higher than ours, since the sample also includes stocks with an emerging market as a home exchange. As found by Halling et al. (2008), cross listed firms listing on the US equity markets have relatively more trades executed at the US market in case they are domiciled in a less developed country. The vast

majority of our sample firms have a developed equity market as their home market. Therefore, the relative volume ratio of 17% for such firms as found by Baruch et al. (2007) is better for comparative purposes. Furthermore, 17% is based on a ten year data period beginning in the mid-nineties. As in recent years it became easier to execute direct trades at foreign exchanges for US investors, this number will supposedly further decrease in the subsequent period. Therefore, our number does not seem to contradict the evidence from other studies.

>>> Insert Table 2 about here <<<

With respect to the analysis carried out in this paper, 94% of the combined trading volume at the respective home market means these markets are more liquid and thus provide more efficiency of information incorporation than US equity markets. This does not come to our surprise since research in the field of ADRs has already born similar findings. Ely and Salehizadeh (2001) investigate the degree of integration among international equity markets. They find that home markets generate a higher amount of information and are thus the more important source of information when compared to US equity markets. Gagnon and Karolyi (2010) are engaged in determining the importance of cross-listings. They find that between 2004 and 2008, the number of foreign firms cross-listing at NYSE or NASDAQ has declined. Therefore, our own results and previous research lead us to the conclusion that investigations should focus on the firms' home markets.

Information on the sample firms' price adjusted for capital actions used for calculating stock returns is retrieved from Datastream, as is all other data utilised for constructing the independent variables. The respective market's Datastream total market indices serve as market proxies. We convert prices to USD where necessary. The 3-month US treasury bill is applied as a proxy for the risk-free rate. As control variables, we construct the Fama and French (1993) factors (F-F factors) SMB and HML. Following Lundgren and Olsson (2010), we make use of country specific

portfolios sorted on size and book-to-market. For all the sample companies' home country markets, we use the corresponding large growth (lg), large value (lv), small growth (sg) and small value (sv) indices. As laid out by Lundgren and Olsson (2010, p. 11), the country specific zero-investment portfolios SMB and HML at time t are calculated as follows

$$\text{SMB}_t = \frac{sg_t + sv_t}{2} - \frac{lg_t + lv_t}{2}, \quad (1)$$

$$\text{HML}_t = \frac{lv_t + sv_t}{2} - \frac{lg_t + sg_t}{2}, \quad (2)$$

where sg_t = small cap growth index return at time t,
sv_t = small cap value index return at time t,
lg_t = large cap growth index return at time t,
lv_t = large cap value index return at time t.

4. Short-term effect of the tool's publication

4.1. Short-term effect: Methodology

In our case, the event occurs at the same point in calendar time for all firms affected. As noted e.g. by Binder (1985, 1998) and MacKinlay (1997), the classical event study methodology introduced in particular by Fama et al. (1969) is not suitable if the event takes place at the same point in calendar time for all affected firms. In this case, excess returns are cross-sectional dependent, an effect commonly referred to as "event clustering". Since cross-sectional independence of excess returns is one of the crucial assumptions in conventional methodology, one has to make use of a different approach. As Binder (1998, p.124) shows, the problem of event clustering can be circumvented by integrating so called "event dummies" into the return equations. The model to be estimated is

$$R_{it} = \alpha_i + \beta_i R_{mt} + \gamma_i D_t + \varepsilon_{it}, \quad (3)$$

where R_{it} is the return on security i at time t, R_{mt} is the return on the market portfolio at time t and D_t is a dummy variable that equals one during the event period and zero otherwise. The coefficient of interest is γ_i, a measure of the abnormal return during event time. In contrast to the ap-

proach by Fama et al. (1969), where abnormal returns are defined as residuals from the market model, this approach parameterises the abnormal return in the market model regression equation.

The method of estimating regression equation (3) is twofold: One can either estimate the regression for each firm in the sample separately using OLS (ordinary least squares). Alternatively, one can follow an approach building on Zellner's (1962) seemingly unrelated regressions (SUR). In such a system of equations, regression equation (3) is estimated for each firm separately using GLS (generalised least squares). As pointed out by Binder (1998, p: 124), returns on N securities of firms affected by A events can be estimated separately in the following system of equations:

$$R_{1t} = \alpha_1 + \beta_1 R_{mt} + \sum_{a=1}^{A} \gamma_{1a} \cdot D_{at} + \varepsilon_{1t},$$
$$R_{2t} = \alpha_2 + \beta_2 R_{mt} + \sum_{a=1}^{A} \gamma_{2a} \cdot D_{at} + \varepsilon_{2t},$$
$$\vdots$$
$$R_{Nt} = \alpha_N + \beta_N R_{mt} + \sum_{a=1}^{A} \gamma_{Na} \cdot D_{at} + \varepsilon_{Nt}.$$

The error terms are assumed to be contemporaneously correlated (cross-correlation), exactly what is assumed for the error terms of stock returns in the case of "event clustering". Most commonly in event studies, tests are performed against the null-hypothesis of no average or no cumulative average abnormal returns during the event period (Binder, 1998). The real advantage of the SUR method not only lies in the fact that it is statistically more appropriate and computationally simpler. Rather it is the possibility to extend the range of testable hypotheses beyond what is possible with the standard method: Joint hypotheses about abnormal returns can easily be tested after estimating a SUR system of equations. In this paper, we test two null hypotheses:

(A) H_0: $\sum_{i=1}^{N} \gamma_i = 0$; cumulative abnormal returns across the sample are equal to zero

(B) H_0: Median $\gamma_i = 0$; the median abnormal return of all firms is equal to zero

For our analysis, we adapt regression equation (3) to fit our purpose of analysing sample stock returns at the firms' respective home exchange. Therefore, we estimate stock returns in a SUR system as follows:

$$(4)\ R_{it} - R_{ft} = \alpha_i + \beta_{1i}(R_{imt} - R_{ft}) + \beta_{2i}oilgswd_t + \beta_{3i}smb_{imt} + \beta_{4i}hml_{imt} + \gamma_{ia}D_{at} + \varepsilon_{it},$$

where $R_{it} - R_{ft}$ is the return on sample firm i net of the risk-free rate (3-month US treasury bill) at time t. $R_{imt} - R_{ft}$ is the return on the market net of the risk-free rate at time t. SMB_{imt} is the return on a zero-investment portfolio long in small firms and short in large firms at time t, while HML_{imt} is the return on a zero-investment portfolio long in high book-to-market firms and short in low book-to-market firms at time t. About these terms it is worth noting that they all differ by firm. R_{imt} for example is the return on firm i's respective country market portfolio. For a German firm i in the sample, R_{imt} is the return on the German market portfolio at time t. For an Indian firm, R_{imt} is the return on the Indian market portfolio at time t. The same holds true for SMB_{imt} and HML_{imt}. $Oilgswd_t$ (Datastream World-DS Oil & Gas) is the return on a global portfolio of oil and gas companies. It is introduced in order to control for the high number of oil companies in the sample. D_{at} is the event dummy that equals one during event time and zero otherwise.

We conduct our analysis with the full sample of firms and additionally with two subsamples for which we suppose the hypothesised effect is likely to be even more pronounced. The criteria used for building these subsamples are:

- Firm origin

As noted above, the sample of firms include US based firms as well as non-US based firms. There is reason to believe that the effect will differ by firm domicile: "Home bias" could lead to US firms being more affected by the list's publication than non-US based firms. As documented in various studies on "home bias" (e.g. French and Poterba, 1991), investors tend to mainly invest

in domestic equity. Since the list's publication was primarily directed towards US American investors, the degree to which information on investors' "terror averse" investment preferences will be incorporated into prices will most likely be higher for US firms. In order to grasp a potentially different effect of the tool's publication on US firms and non-US firms, the analysis is carried out with a subsample encompassing US firms, only.

- Operations in Iran

Information on which firms belong to the group being accused of doing business in State Sponsors of Terrorism countries was accessible to users after clicking on the specific country of interest. According to the SEC, information on firms doing business in Iran was most "popular" in the sense that it was the country most often clicked on (U.S. Securities and Exchange Commission, 2007b). One potential interpretation for this fact is that investors were especially feared their investments could enhance the nuclear threat supposedly emanating from Iran. While we do not aim to analyse the exact cause of this finding, we hypothesise negative stock price reactions of firms operating in Iran to be more pronounced than for the sample as a whole.

As event dates, i.e. the time periods tested for abnormal stock price reactions of sample firms, we consider the following periods:

- June 4th 2007 – June 22nd 2007: *Pre event period*

The period has a length of 15 trading days and ends on the last trading day before the tool was published. It therefore measures any information leakage that might have caused stock prices to react abnormally even before the tool was published.

- June 25th and June 26th 2007: *Short event period (two trading days)*

The tool was published on June 25th, 2007. In order to allow for trading of the affected securities on markets that were already closed by the time the information was released (European and Asian markets) June 26th is added to the event period.

- June 25th – July 16th 2007: *Long event period (15 trading days)*

The tool was taken offline on July 16th. This period covers the entire period during which information was online on the SEC's website.

- July 17th – September 25th 2007: *Post event period*

The period starts one day after the tool was taken offline. This 50 trading days post-event period measures any information that was incorporated into prices after the tool's publication.

During these periods, the event dummy is set equal to one. For all other trading days during the estimation period it is set equal to zero. For the pre event, the short event, and the long event period, regressions are estimated using daily returns from July 2006 (one year before the tool was launched online, a sum of approx. 250 trading days) until December 2007 (approx. 100 trading days after the tool was taken offline. Since the post event period is considerably longer, the corresponding estimation period is extended and ends in December 2008.

4.2. Short term effect: Results

In this section, we describe the results obtained from estimating regression (2) with returns from holding the complete sample of firms as well as two different subsamples. Four different specifications of the event dummy D_{at} are defined in order to measure the impact of the pre event period, the short event period, the long event period and the post event period. All tests conducted involve a statement about the coefficient of interest, γ_{ia}, which measures abnormal returns during event time.

4.2.1. Short term effect: Full sample of firms

In this section, we test the two hypotheses described above for the entire sample of firms. Table 3 displays the results. In Panel A we provide sums of coefficient γ_i estimates. The sum of –0.147 for the post event period indicates that on aggregate, excess returns on sample firms were negative in the period succeeding the tool's provision. As indicated by the corresponding chi^2 value, the sum of –0.147 is statistically insignificant at conventional levels of confidence (corresponding p-value = 0.154). Hence, this finding only slightly confirms the notion that investors did indeed act upon the information provided in the tool and sold stocks of firms accused of having business ties to countries designated as State Sponsors of Terrorism. Nevertheless, this interpretation is strengthened by testing the null hypothesis of the median coefficient being equal to zero. Results are displayed in Panel B. We observe a negative median coefficient (–0.001) for the post event period and can reject the null of the median being equal to zero on the 1%-level of confidence.

>>> Insert Table 3 about here <<<

4.2.2. Short term effect: US firms

In light of what has been found for the entire sample of firms, we expect results to be even more pronounced for the subsample of US firms. The results for this subsample, provided in Table 4, do not confirm our expectations. Again, we have to stick to the null for the hypothesis of the sum of the event dummy coefficients being equal to zero regardless of the time period tested. The same holds for the hypothesis that the median event dummy coefficient is equal to zero. Overall, we fail to detect a measurable effect for the sample of US firms.

>>> Insert Table 4 about here <<<

4.2.3. Short term effect: Firms with operations in Iran

Firms with operation in Iran constitute the second subsample of firms for which we suppose the effect of the tool's publication will be more pronounced. In Table 5, we report results of the two hypotheses tested. Again, we fail to reject the null hypothesis of the sum of the event dummy coefficients being equal to zero for all periods tested. Test results presented in Panel B confirm these findings for the pre-, short- and long event period. The sole significant result is obtained for the post event period. We can reject the null of the median event dummy coefficient being equal to zero on the 5%-level of confidence. Since the median coefficient (–0.001) is negative, we reject the null in favour of the alternative hypothesis that the median is smaller than zero. The fact that 35 out of 50 event dummy coefficients are negative constitutes further indicative evidence for the interpretation that stock prices of sample firms depressed subsequent to the tool's availability.

>>> Insert Table 5 about here <<<

4.2.4. Short-term effect: Summary of results

Our hypothesis about negative stock price reactions to the publication of the SEC's online-tool is partially confirmed by the data for the post event period. While we fail to find evidence for this interpretation by using a parametric test, the non-parametric test we conduct renders significant results. We have no reason to believe that the negative information which firms would be mentioned in the tool was incorporated into prices before the tool's release on June 25th, 2007. For all of the samples tested, both hypothesis tests show no significantly negative stock price reactions in the pre event period. A similar finding is true for the short and the long event period. We obtain no significant results regardless of the subsample and hypothesis tested. Therefore, we can assert that investors did by no means quickly react to the information provided in the tool. Both periods do not display any abnormal stock price reactions of sample firms' stocks.

Contrary to what we expected, subsamples did not react more pronounced than the complete sample. Neither the sample of US based firms nor the sample of firms with operations in Iran reacted stronger than the full sample of firms. Although the evidence for investors selling stocks of stigmatised firms is slightly supported by the subsample results, we find no evidence for investors punishing US firms or firms with operations in Iran harder.

The fact that we receive significant results for the post event period strikes us. If financial markets are efficient in the semi-strong form, then why did it take so long until information was incorporated into prices? Should not the short event period be the one for which statistically significant results are obtained? Since we have no information regarding the distribution of the 150,000 clicks during the time the tool was online, we can only guess that clicks were made throughout the entire period the tool was online and that not all investors met their decision to sell at the very first or second day the tool was online. Investors seem to have waited until they sold stocks of stigmatised firms. Another interpretation consistent with the results is that the enormous press coverage caused investors to react. In the press it was argued that information provided in the tool was of poor quality and did not reflect the most recent developments concerning a firm's involvement in the designated countries. If investors had the same views about the information provided, they will not have reacted to the bare information in the tool but to the implicit warnings in the press. As it was put forward that investors might quickly react and sell-off equity of the stigmatised firms, investors might have feared their peers' reaction. By supposing that others might act unwisely, selling stocks could have seemed to be a smart decision.

4.3. Short-term effect robustness check: Portfolio level analysis

So far, we have analysed results on the single stock level. In this section, we aim to test the results for robustness by building portfolios. As modern portfolio theory suggests, exposure to idiosyncratic risk is reduced in a portfolio of assets. Thus, building portfolios is also ideally suited in

an event study: While the return variance unique to individual assets is diversified away, risk common to firms in the sample is not. The first scholar to make use of the portfolio approach in an event study framework was Izan (1978), who used equally weighted returns on a portfolio of firms experiencing the event as the dependent variable.

Similar to the model estimated in the case of individual securities during the preceding sections, we investigate portfolio returns by means of the following regression specification:

$$(5)\ R_{pt} - R_{ft} = \alpha_p + \beta_{1p}(R_{mt} - R_{ft}) + \beta_{2p}\text{oilgswd}_t + \beta_{p3}\text{smb}_t + \beta_{p4}\text{hml}_t + \gamma_{pa}D_{at} + \varepsilon_{pt},$$

where $R_{pt} - R_{ft}$ is the return on portfolio p net of the risk-free rate (3-month US treasury bill) at time t, $R_{mt} - R_{ft}$ is the return on the market (MSCI World) net of the risk-free rate at time t, oilgswd_t is the return on a global portfolio of oil firms at time t, SMB_{pt} is the return on a zero-investment portfolio long in small firms and short in large firms at time t, HML_{pt} is the return on a zero-investment portfolio long in high book-to-market firms and short in low book-to-market firms at time t. D_t is the event dummy which equals one during event time and zero otherwise. Before estimating the regression, we run a test for heteroskedasticity according to Breusch and Pagan (1979) and Cook-Weisberg (1982), commonly referred to as the Breusch-Pagan/Cook-Weisberg test and a test according to White (1980). Test statistics indicate heteroskedasticity for both return series used (one shorter and one longer series). We also run two tests for serial correlation. The first is according to Durbin (1970), the second test conducted is according to Breusch (1978) and Godfrey (1978), generally called the Breusch-Godfrey LM test. The null of no serial correlation cannot be rejected for the shorter return series, while the longer return series exhibits serial correlation. Therefore, we report heteroskedasticity consistent (HC), Huber-White standard errors according to Huber (1967) and White (1980) for the shorter return series. For the longer return series we report Newey and West (1987) heteroskedasticity and autocorrelation consistent (HAC) standard errors.

We are well aware of the studies by Fomby and Murfin (2005) as well as Ford et al. (2010), both referring to the possibility of misinterpreting event study results obtained from careless use of HAC standard errors. Since a necessary condition for drawing misleading conclusions is that the disturbances are independently and identically distributed (i.i.d.), we believe that the rejection of the i.i.d. case for our sample is enough to believe we are amply equipped against the danger of misinterpreting our results.

Table 6 reports results obtained from the estimation of regression equation (5). γ_p, the coefficient of interest, is statistically indistinguishable from zero for all tested periods but the post event period. For the latter time period, the coefficient pertaining to the event dummy is negative (–0.001) and significantly different from zero on the 5%-level of confidence. This result is consistent with the results obtained from estimating the SUR system of equations in the preceding section. Therefore, it supports the notion that investors sold stocks of stigmatised firms but did so with a rather long lag of time.

>>> Insert Table 6 about here <<<

5. Long-term effect of the tool's publication

5.1. Long-term effect: Methodology

In the previous section, we obtained results consistent with our hypothesis that investors did in fact sell stocks of firms mentioned in the tool. In this section, we aim to find an answer to the question whether investors could reap long-term abnormal returns by trading on the information provided in the tool. Subsequent to the tool's publication, prices will have been depressed as compared to fundamentals. This is why we conjecture that long-term abnormal returns could be earned during a long-term period after the publication. The fact that sample firms have been men-

tioned in the tool makes them match the definition of "neglected stocks". Thus, they should subsequently suffer from being shunned by a norm conscious investor base.

As Hong and Kacperczyk (2009) point out, two assumptions are necessary if one wishes to find measurable return differences between "neglected" and "normal" stocks. The first assumption must be that of limits to arbitrage: Capital in the market moved by arbitrageurs does not suffice for the elimination of effects induced by norm constrained, discriminatory investors. The second assumption is that, over time, discriminatory tastes are stable. While the short-term effects discovered in the previous analysis mark light evidence for limited arbitrage, it has to be proven whether discriminatory tastes are in fact stable over time. This could be difficult for two reasons. First, especially the media and affected firms remained doubtful about the tool meeting certain quality standards in the sense that it legitimately sanctioned immoral firm behaviour. Hence, it might be that after some time investors stopped discriminating against sample firms. Second, firms could have reacted to being mentioned in the tool by abandoning operations in the designated countries. On the one hand, these firms would then no longer be regarded as acting immorally, thereby losing the attribute of being a shunned stock and thus stop trading cheaply. On the other hand, it would in this case still remain in doubt whether investors would forgive and stop discriminating against these firms. Answers to these questions will be given by the empirical evidence presented in the next section.

Methodologically, we start out by building an equally weighted portfolio of sample firms. Since we are dealing with a portfolio of firms domiciled in countries all over the world, we proxy for systematic risk using a global equity portfolio. As a second control variable, we introduce returns on a global portfolio of oil and gas firms. With the help of this portfolio, we are able to control for effects induced by the portfolio's high exposure to the oil industry. Returns to holding our equally weighted sample portfolio are estimated with the following regression:

(6) $R_{pt} - R_{ft} = \alpha_p + \beta_{p1}(R_{mt} - R_{ft}) + \beta_{p2}oilgswd_t + \beta_{p3}smb_t + \beta_{p4}hml_t + \varepsilon_{pt}$.

Sample portfolio returns on the left-hand side of the equations (net of the risk-free rate) are explained by $R_{mt} - R_{ft}$, a global equity portfolio (MSCI world) net of the risk-free rate (3-months US treasury bill), $oilgswd_t$, a global portfolio of oil and gas firms (Datastream World-DS Oil & Gas), smb_t, a global zero-investment portfolio mimicking returns to a small firm portfolio as well as hml_t a global zero-investment portfolio mimicking returns on a portfolio of value (high book-to-market) stocks. We estimate the regression with 180 weekly returns ranging from July 27th, 2007, to December 31st, 2010. Thus, the time period of investigation starts in the first week after the tool was abandoned. If our hypothesis holds to be true, a positive alpha is generated by re-gressing the sample firm portfolio returns on the two explanatory variables. For this analysis of long-term effects, we exclude weekly returns above +100% and below −50% to avoid the risk of drawing incorrect inferences based on outliers. As a result, 0.4% of company returns are excluded.

5.2. Long-term effect: Results

Results of the long-term analysis are provided in Table 7. The first model specification merely includes the world market proxy as a single explanatory variable. As we infer from the market beta of 1.198, our sample portfolio is exposed to above average market risk. This does not come to our surprise in this case of an equally weighted portfolio including a substantial amount of smaller firms. In the second model specification, the full regression specification (6) including the portfolio of global oil and gas firms, as well as the smb and hml zero-investment portfolios is estimated. We note that the model fit as measured by the R-squared is slightly better for the full specification ($R^2 = 0.94$) than it is in the case of specification (1), with an R-squared of 0.91. As witnesses by the corresponding t-value of 6.93, the oil and gas firm portfolio adds considerable explanatory power to the model. Furthermore, the sample portfolio loads positively and highly

significant (1%-level of confidence) on SMB, a finding supporting the notion that sample portfolio returns are to a large degree driven by small firms. This is why we draw our attention to results from specification (2). Consistent with our hypothesis of stock prices being depressed after investors sold stocks of stigmatised firms, we find the two-factor alpha of the equally weighted sample firm returns to be positive and significantly different from zero with a corresponding p-value of 0.056. Furthermore, the coefficient (alpha) is economically sizeable as well. The estimate of 0.002 corresponds to a weekly risk-adjusted excess return of 0.15% and an annualised excess return of 8.16%.

>>> Insert Table 7 about here <<<

Although these findings offer some support in favour of our hypothesis, an alternative explanation has to be considered, as well. Possibly, the positive alpha will also be observed in the time period before the tool's publication. If this was the case, our portfolio would offer superior risk-adjusted returns regardless of the time-period investigated. Consequentially, this would rule out the interpretation of the tool having stigmatised sample firms as an explanation for the observed effect. The fact that these firms have also been stigmatised by means of the online-tool could then not serve as an explanation for the observed superior return performance. Rather, over-performance would likely be attributable to pure chance. We will check our results for robustness to this alternative interpretation in the succeeding section.

5.3. Long-term effect robustness check: Variation of the time period

In Table 8, we report the results from estimating regression equation (6) for the 180 week period before the tool's publication (January 16th, 2004 to June 22nd, 2007). Although the four-factor alpha from model specification (2) is positive (0.001), it differs in two respects from the corresponding four-factor alpha in the period subsequent to the event (see Section 5.2.): First, it is statistically indistinguishable from zero at all conventional levels of confidence. Second, its eco-

nomic magnitude is substantially lower: The alpha of 0.001 corresponds to a weekly risk-adjusted excess return of 0.07% and an annualised risk-adjusted excess return of 3.69% - less than half the risk-adjusted excess return in the period subsequent to the tool's publication (0.15% weekly and 8.16% annually). By both these standards, statistical significance and economic magnitude, returns of the sample firm portfolio are considerably lower in the period before the tool's publication than they are in the period subsequent to the tool's publication. Thus, our interpretation of moral stigmatization by means of being mentioned in the tool having caused affected firms' stocks to be depressed is robust to an alteration of time periods.

>>> Insert Table 8 about here <<<

6. Conclusion

In this study, we aimed at investigating the short-term investor reaction and the long-term consequences of the SEC's publication of an online tool for detecting firms doing business in countries designated as State Sponsors of Terrorism. From the results obtained, we draw the following main conclusions: (i) Investors were sensitive to the information provided in the tool, as they sold stocks of firms mentioned. Information was incorporated rather slowly, since prices started falling during a 50 day period after the tool had been available. Moreover, the effect is not more pronounced for US firms and investors did not punish firms harder that operated in Iran. (ii) Investors were able to earn positive abnormal returns from holding an equally weighted portfolio of sample firms during the period starting after the tool's suspension until the end of 2010. From this, we infer that investors were on average not forgetting about the unethical behaviour of certain firms. Findings in this paper shed new light on the question in how far social norms affect capital markets. Investors are in fact guided by moral considerations when making decisions about their investments. As shown, such considerations cause stocks of stigmatised firms to offer

attractive investment opportunities for investors without moral advisements. For the stigmatised firms, findings in this paper offer new insights with respect to the consideration whether to do business in a State Sponsor of Terrorism country. For sure, a firm refraining from business opportunities in the designated countries limits its ability to generate extra revenue. In the light of the evidence presented in this paper, managerial decision making must also consider reputational risks that materialise in poorer stock market performance when balancing the assets and drawbacks of such investment decisions.

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Table 1

Size and market to book of sample stocks

Panel A: Firm size

Market capitalisation ($ million, 2007)

	Average	Median	Obs.
All firms	38.746	13.479	81
US Firms	13.317	1.057	30
Non-US Firms	53.704	31.187	51

Assets ($ million, 2007)

	Average	Median	Obs.
All firms	196.919	9.857	77
US Firms	11.717	772	27
Non-US Firms	296.929	47.734	50

Panel B: Firm market to book (2007)

	Average	Median	Obs.
All firms	2.52*	2,47	72
US Firms	2.50*	2,52	26
Non-US Firms	2.55	2,45	46

This table displays key characteristics of sample firms. Characteristics are presented for the entire sample of firms, for firms domiciled in the US, and for firms domiciled outside the US. Panel A reports firm size as measured in $ million of market capitalisation as well as firm size as measured in $ million of total assets in 2007. Panel B reports market-to-book ratios in 2007. * One firm excluded because of identification as an outlier.

Table 2

Trading volume of sample stocks

Number of shares traded (million, 2007)

	US exchanges	Home exchanges	Obs.
All firms	25.037	211.544	78
US Firms	13.244	13.244	30
Non-US Firms	11.794	186.507	48

This table presents trading volume as measured in million shares traded 2007 for the entire sample of firms, for firms domiciled in the US, and for firms domiciled outside the US.

Table 3

Hypothesis testing: Full sample stock returns				
	Pre Event Period	Short Event Period	Long Event Period	Post Event Period
Panel A				
H_0: $\sum_{i=1}^{N} \gamma_i = 0$	0.026 [1)]	0.068 [1)]	0.071 [1)]	-0.147 [1)]
(Wald test)	(0.03)	(0.02)	(0.32)	(2.03)
Panel B				
H_0: Median $\gamma_i = 0$	0.001 [2)]	0.000 [3)]	0.000 [3)]	-0.001 [3)]
Coef. positive/negative	47/34	41/40	45/36	28/53
(Wilcoxon signed-rank test)	(1.80)*	(0.08)	(0.71)	(-2.79)***
Observations	363	363	363	644

This table displays hypotheses about γi, the event dummy coefficient as estimated in regression equation (4). The sample encompasses a total of 81 firms. Panel A provides results for the hypothesis that the sum over all γi is equal to zero (hypothesis A). Panel B reports results for the hypothesis that the median γi is equal to zero (hypothesis B). The estimation window ranges from 07/2006 to 12/2007 for the pre event period, the short event period and the long event period. For the post event period it ranges from 07/2006 to 12/2008. Chi^2 values for Wald tests and z-values for Wilcoxon signed-rank test are in parentheses.
1) Displayed are sums of coefficient estimates; 2) Displayed is the median coefficient;
* p<0.10, ** p<0.05, *** p<0.01, **** p<0.001.

Table 4

Hypothesis testing: US sample stock returns				
	Pre Event Period	Short Event Period	Long Event Period	Post Event Period
Panel A				
H_0: $\sum_{i=1}^{N} \gamma_i = 0$	0.022 [1)]	-0.041 [1)]	0.046 [1)]	-0.069 [1)]
(Wald test)	(0.02)	(0.02)	(0.16)	(0.60)
Panel B				
H_0: Median $\gamma_i = 0$	0.002 [2)]	-0.006 [2)]	-0.000 [2)]	-0.001 [2)]
Coef. positive/negative	20/9	12/17	13/16	14/15
(Wilcoxon signed-rank test)	(1.61)	(-1.20)	(-1.05)	(-0.88)
Observations	363	363	363	644

This table displays hypotheses about γi, the event dummy coefficient as estimated in regression equation (4). The sample encompasses a total of 29 firms. Panel A provides results for the hypothesis that the sum over all γi is equal to zero (hypothesis A). Panel B reports results for the hypothesis that the median γi is equal to zero (hypothesis B). The estimation window ranges from 07/2006 to 12/2007 for the pre event period, the short event period and the long event period. For the post event period it ranges from 07/2006 to 12/2008. Chi^2 values for Wald tests and z-values for Wilcoxon signed-rank test are in parentheses.
1) Displayed are sums of coefficient estimates; 2) Displayed is the median coefficient;
* p<0.10, ** p<0.05, *** p<0.01, **** p<0.001.

Table 5

Hypothesis testing: Iran operations sample stock returns				
	Pre Event Period	Short Event Period	Long Event Period	Post Event Period
Panel A				
H_0: $\sum_{i=1}^{N} \gamma_i = 0$	-0.047 [2)]	-0.008 [2)]	0.075 [2)]	-0.055 [2)]
(Wald test)	(0.30)	(0.00)	(1.20)	(1.12)
Panel B				
H_0: Median $\gamma_i = 0$	0.001 [3)]	0.001 [3)]	0.000 [3)]	-0.001 [3)]
Coef. positive/negative	27/23	26/24	33/17	15/35
(Wilcoxon signed-rank test)	(1.22)	(0.60)	(1.55)	(-2.41)**
Observations	363	363	363	644

This table displays hypotheses about γi, the event dummy coefficient as estimated in regression equation (4). The sample encompasses a total of 50 firms. Panel A provides results for the hypothesis that the sum over all γi is equal to zero (hypothesis A). Panel B reports results for the hypothesis that the median γi is equal to zero (hypothesis B). The estimation window ranges from 07/2006 to 12/2007 for the pre event period, the short event period and the long event period. For the post event period it ranges from 07/2006 to 12/2008. Chi^2 values for Wald tests and z-values for Wilcoxon signed-rank test are in parentheses.
1) Displayed are sums of coefficient estimates; 2) Displayed is the median coefficient;
* p<0.10, ** p<0.05, *** p<0.01, **** p<0.001.

Table 6

Hypothesis testing: Full sample portfolio returns				
	Pre Event Period	Short Event Period	Long Event Period	Post Event Period
γ_p	0.001	0.000	0.001	-0.001
	(0.55)	(0.17)	(0.54)	(-2.19)**
Market -rf	0.708	0.706	0.707	0.801
	(12.40)****	(12.35)****	(12.39)****	(12.81)****
Oil & Gas	0.341	0.342	0.341	0.275
	(8.79)****	(8.88)****	(8.85)****	(6.16)****
SMB	0.281	0.281	0.282	0.108
	(2.36)**	(2.35)**	(2.36)**	(0.62)
HML	-0.14	-0.14	-0.14	-0.29
	(-0.96)	(-0.98)	(-0.96)	(-1.55)
Observations	363	363	363	644
R-squared	0.66	0.66	0.66	0.80

This table presents results obtained from estimating regression (5). The estimation window ranges from 07/2006 to 12/2007 for the pre event period, the short event period and the long event period. For the post event period it ranges from 07/2006 to 12/2008. The null hypothesis tested is that of the event dummy (y_i) being equal to zero. t-statistics in parentheses. T-statistics for the pre event period, the short event period and the long event period are based on Huber-White standard errors. t-statistics pertaining to the post event period are based on Newey and West (1987) standard errors using five lags ; * p<0.10, ** p<0.05, *** p<0.01, **** p<0.001.

Table 7

Alpha study: Full sample portfolio returns		
	(1)	(2)
α	0.002	0.002
	(2.35)**	(1.92)*
Market-rf	1.198	0.781
	(26.17)****	(10.71)***
Oil & Gas		0.339
		(6.93)***
SMB		0.3812
		(4.49)***
HML		0.1403
		(1.39)
Observations	180	180
R-squared	0.91	0.94

This table presents results obtained from estimating two permutations of regression specification (6). The time period of investigation ranges from 07/2007 to 12/2010. In model (1), the world market proxy (MSCI World) is the sole explanatory variable. Model (2) is the full specification and includes four explanatory variables: the world market proxy (MSCI World), a global oil & gas index, a global zero-investment small firm portfolio (SMB), and a global zero-investment portfolio of value stocks (HML). t-statistics are based on Newey and West (1987) standard errors using five lags in parentheses; * p<0.10, ** p<0.05, *** p<0.01, **** p<0.001.

Table 8

Alpha study: Full sample portfolio returns		
	(1)	(2)
α	0.002	0.001
	(2.23)**	(1.25)
Market-rf	1.104	0.624
	(16.56)****	(9.73)****
Oil & Gas		0.330
		(10.34)****
SBM		0.415
		(3.23)***
HML		-0.257
		(-1.64)
Observations	180	180
R-squared	0.70	0.83

This table presents results obtained from estimating two permutations of regression specification (6). The time period of investigation ranges from 01/2004 to 06/2007. In model (1), the world market proxy (MSCI World) is the sole explanatory variable. Model (2) is the full specification and includes four explanatory variables: the world market proxy (MSCI World), a global oil & gas index, global zero-investment small firm portfolio (SMB), and a global zero-investment portfolio of value stocks (HML). t-statistics are based on Newey and West (1987) standard errors using five lags in parentheses; * p<0.10, ** p<0.05, *** p<0.01, **** p<0.001.