

# Causal evidence on the effects of enforcement investigations\*

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## Abstract

We provide causal evidence on the effects of financial reporting enforcement investigations. Using a proprietary data set on randomly selected and risk-based selected enforcement investigations of listed German firms from 2005 to 2018, we find that risk-based selected firms face a significant abnormal reduction in daily returns of -8.6 basis points during enforcement investigations. Randomly selected firms, however, do not show significant changes in firm value during investigations. Consistently, we find that risk-based selected firms are less transparent during investigations, and change their financial reporting behavior, whereas randomly selected firms show no change in transparency or financial reporting behavior. Overall, our study suggests that the negative firm-level outcomes of enforcement investigations as shown by prior research are likely a result of the selection process. When we avoid the endogenous selection and focus on randomized investigations, we document that firms are largely unaffected.

*JEL classification:* G14, G18, G38, K22, M41

*Key words:* Financial reporting enforcement, net effect of enforcement, costs and benefits of enforcement.

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# 1 Introduction

A cornerstone of many financial reporting enforcement systems worldwide are pro-active enforcement investigations, which are reviews of financial reports of publicly listed firms without the need of specific complaints. While we know plenty about other aspects of enforcement such as imposing sanctions or broader market effects of installing or strengthening enforcement regimes, much less is known about the effect of enforcement investigations at the firm-level. Concurrent work by Florou et al. (2020) and Christensen et al. (2020) suggests that an increased likelihood of enforcement investigations is related to an increase in audit fees and a decrease in shareholder wealth. Moreover, Blackburne et al. (2020a) and Blackburne et al. (2020b) use novel data from undisclosed SEC investigations and find that, during investigations, firms switch from accrual to real earnings management, insider trades spike, and that investigations can predict declines in future returns.

An important limitation of these prior studies, however, is that they rely on endogenously selected investigations. Specifically, enforcement institutions such as the Securities Exchange Commission (SEC) usually start investigations based on a ‘lead’ (e.g., whistleblower information, press reports, surveillance activity) that suggests a possible violation (Blackburne et al., 2020b). Moreover, given limited resources, the SEC is likely to only pursue cases with low ambiguity and that are easier to win (Dechow et al., 2010). Therefore, firms that are under investigation are a highly selective group, and any outcome measure may be correlated with the reason the firm was selected to be investigated. For example, if the selection process was based on a qualified audit opinion or looming financial distress, firms might have shown these negative market effects or increases in audit fees even in absence of an investigation. Hence, it remains unclear whether any effect can be attributed to the investigation, the reason of the selection, or a combination of both.

We avoid the limitations of prior research by using a proprietary data set and a unique in-

stitutional setting. Specifically, we obtain publicly unavailable data on enforcement investigations from the Federal Financial Supervisory Authority in Germany (Bundesanstalt für Finanzaufsicht (BaFin)), which is comparable to the SEC. The BaFin provides us with detailed data on all enforcement investigations of German public firms starting from the initiation of the enforcement system in 2005 until 2018. The data includes information about the reason (random selection vs. risk-based selection), the exact time span of the investigation, and its outcome. The German setting provides another important advantage. In contrast to most other countries, more than 80% of enforcement investigations are based on a random sampling mechanism.<sup>1</sup>

Our identification strategy exploits this setting in two ways. First, we use the randomized nature of investigations and combine the randomization with a tight research design to determine the causal effects of investigations. Specifically, we test whether certain consequences occur during investigations, but not before or after investigations. In our regression models, we include investigation-, firm-, and industry-period fixed effects. This approach allows us to use each firm's investigation as its own control. Thereby, any effects that we identify stem from the within-investigation variation over the pre-, during-, and post-period, while we also control for the general level of our outcome measures at the firm-level. Moreover, the industry-period fixed effects absorb any industry-specific time trends over the pre-, during-, and post-period. This approach allows us to draw causal inferences about the firm-level effects of enforcement investigations. Second, because we also have risk-based investigations in our sample, we are able to differentiate which effects stem from the risk-based selection model and which are driven by the investigation itself. Thereby, we can reconcile our findings with prior literature.

Using a sample of 1,080 investigations, we analyze the net effect of enforcement investigations on firm value. Specifically, we gauge the change in the daily abnormal returns compared to the

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<sup>1</sup> In the EU, the mean proportion of issuers that are selected either randomly or by rotation is 36%. Hence, the German setting with a large proportion of randomly sampled firms (80% to 85%) is particularly useful for our identification strategy.

two-digit SIC-industry from the pre- to the during-investigation period. The cumulative abnormal return during enforcement investigations reflects the firm-specific net benefits or costs of on-going enforcement investigations. We find that randomly selected firms do not show any significant firm value changes due to enforcement investigations. By contrast, risk-based selected firms show a negative net effect during investigations. This effect is also economically relevant with a daily decrease of 8.6 basis points or a total negative cumulative abnormal return of -12.73 percent for the median investigation. This result for risk-based selected firms is consistent with Christensen et al. (2020), who show a negative effect on firm value for endogenously selected target sector firms.

Because of our null result for randomly selected firms, it is likely that the results for risk-based selected firms are driven by the endogenous nature of the risk selection.<sup>2</sup> However, it is also possible that risk-based selected firms experience different costs and benefits during enforcement investigations. For example, the enforcement institution could adapt its enforcement intensity for risky firms because of an ex ante suspicion. To shed more light on the different explanations, we employ a Butterworth high-pass filter, which is a filter technique borrowed from macroeconomics. Specifically, this filter cancels out any short- or long-term (multi-order) trends in firms' returns that exceed the length of investigations. When applying this filter, the negative return effects for risk-based selected firms disappear. This result suggests that our findings related to risk-based selected firms are likely a consequence of selection bias, hence unlikely driven by different costs and benefits of enforcement investigations.

Next, we attempt to identify whether the null result for randomly selected firms' market values is because of costs and benefits offsetting each other, or no significant costs and benefits related to on-going enforcement investigations. Specifically, we identify channels potentially un-

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<sup>2</sup> Even though we can rule out a time-invariant selection bias (for example, firms with a steady downward trend or a generally higher risk of erroneous financial reporting), because we do not find abnormal changes in firm value after an investigation has ended, the results could be the consequence of a temporary selection bias that is related to the risk factors (among these risk factors could be a closed merger or firms being particularly affected by a major GAAP change).

derlying market value effects such as level of transparency or financial reporting behavior during investigations. We find no significant changes for randomly selected firms for any of the channels, suggesting no significant costs and benefits. By contrast, consistent with the negative market value effects, we find risk-based selected firms abnormally changing their transparency and related financial reporting behavior. Because these effects are sustained as they persist even after investigations have ended, it is likely the result of the FREP's selection mechanism.

Overall, our study suggests that the negative firm-level outcomes of enforcement investigations as shown by prior research are likely a result of the selection process. When we avoid the endogenous selection and focus on randomized investigations, we show that firms are largely unaffected. Thereby, we contribute to the literature by shedding light on the general cost-benefit trade-off of financial reporting enforcement. Understanding the costs and benefits is particularly important for regulators in the case of enforcement. In other aspects of financial reporting or corporate governance, firms usually have the choice, for example, to voluntarily provide additional disclosures, hire higher quality auditors, or impose other self-binding mechanisms. In the case of enforcement, it is unlikely that a market solution will emerge.<sup>3</sup> Hence, firms must rely on the solution provided by a regulator. We also contribute to the stream of literature that exploits regulator-induced randomization (e.g., Bushee and Leuz, 2005; Diether et al., 2009; Fang et al., 2016). While the literature that uses quasi-natural experiments to make causal claims is often limited to very specific and narrow settings that do not allow generalizability (Leuz and Wysocki, 2016), regulator-induced randomization allows inferences that may extend to a broader population. In fact, our study includes all firms that are listed on a regulated market in Germany. This broad sample with a randomization of enforcement investigations allows us to draw causal inferences for the population of public firms in Germany.

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<sup>3</sup> Such a market solution would require that a firm establishes an enforcement body on its own. The independence of such a body, however, would be questionable. Independence would increase when more firms join the establishment. However, coordination costs would also increase and, thus, mitigate a market solution.

While the German setting with our access to the BaFin provides us with a unique research opportunity, it also limits the generalizability of our findings. The fact that we are able to replicate the negative net effects for risk-based selection found in other settings, however, alleviates this concern. Moreover, several countries such as Sweden or Austria follow a similar two-tier enforcement mechanism as Germany. Furthermore, the EU has installed a joint European System of Financial Supervision including the European Securities and Markets Authority (ESMA) to promote further supervisory convergence within the EU. In addition, the general costs and benefits of enforcement investigations may be more general economic links that are valid regardless of the specific institutional context. Therefore, we believe that our findings may also be useful outside the German context.

## 2 Background and Hypotheses

### 2.1 Institutional Background and Legal Framework

#### *Institutions*

In 2005, Germany introduced a two-tier enforcement system. The Financial Reporting Enforcement Panel (FREP) is the main operating body that primarily examines audited financial statements and management reports of publicly listed firms (Sec. 342b German GAAP).<sup>4</sup> The second tier, the BaFin, comes into play when firms disagree with the FREP's findings (Sec. 108 Securities Trading Act). If an enforcement investigation concludes with a material error finding, the firm has the obligation to publish the error finding in the German Federal Gazette ('Bundesanzeiger') to ensure adverse disclosure ('name and shame') and foster compliance. In case of non-error conclusions, usually, no public announcements are made or required. All publicly listed companies, that is, firms based in Germany that issue equity or debt instruments that are publicly

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<sup>4</sup> Because the FREP is a private body, firms' agreement to investigations is theoretically voluntary. Yet, we are unaware of any cases where firms refused to be investigated by the FREP because the BaFin would mandate a firm to cooperate in these cases.

traded on a regulated market are subject to enforcement investigations by the FREP or the BaFin (Sec. 342b (2) s. 2 German GAAP).

### *Selection mechanism*

The selection mechanism for enforcement investigations consists of three steps.<sup>5</sup> First, the FREP selects firms based on risk factors, which accounts for approximately 10% to 15% of all investigations. Risk factors comprise indications of erroneous financial reporting, such as qualified audit opinions, public media reports, whistle-blowing, or examinations requested by the BaFin. Second, the remaining non-risk firms are subject to a stratified sampling approach based on three strata. The number of firms randomly drawn from the first stratum is determined in such a way that firms listed on one of the major indices are on average investigated once in a 4 to 5 years cycle, and the remaining listed firms once in a 8 to 10 years cycle (second stratum). Additionally, the FREP randomly draws 40% of the firms of a smaller third stratum that contains firms from the other two strata that particularly match to the European enforcement priorities, that have not been investigated for longer periods, or firms with other characteristics that might increase financial reporting complexity.<sup>6</sup> 85% to 90% of the FREP's investigations are based on this stratified random sampling mechanism.<sup>7</sup> Finally, to ensure that each firm could be selected at any time, the FREP randomly draws ten firms that were not selected in the former two steps and picks three of them in such a way that no firm faces an excessive enforcement burden (FREP, 2018b).<sup>8</sup> Overall, there

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<sup>5</sup> For a detailed overview see FREP (2018b) or Barth (2018).

<sup>6</sup> As a result, firms from the third stratum have a slightly higher likelihood of being selected by the FREP, thus weighting our random sample somewhat toward these firms. However, given that this stratum only makes up a small portion of randomly drawn firms (approximately 10%) and the fact that the criteria for a firm being part of the third stratum are rather abstract, the overall influence on our findings should be limited. Moreover, until 2016, the FREP has only drawn 30% of the firms that are part of the third stratum (Barth, 2018).

<sup>7</sup> By removing firms that were investigated from the relevant stratum for the rest of a cycle period, the FREP ensures that all firms are selected within specified time frames. Firms, however, do not know when the next cycle begins. Hence, it is possible that firms may be under investigation in back-to-back years. By contrast, some firms may not be under investigation for up to 18 years when not being listed on a major index and drawn in the first year of a cycle and in the last year of the next cycle.

<sup>8</sup> This third step was added to the selection mechanism in 2016 (FREP 2016). Besides this amendment, there were only minor changes in the partitioning of the sample into strata.

are thus two types of reasons for an investigation. First, risk factors identified by the FREP or the BaFin and, second, a random selection.

The large fraction of more than 80% of randomly selected investigations is exceptional compared to most other countries. For example, in the EU, the mean proportion of issuers that are selected either randomly or by rotation is 36% (ESMA, 2017). Hence, the German setting is particularly useful for our identification strategy. However, note that the specifics of the German selection mechanism as described above have two important drawbacks. First, in our main tests, the risk-based pre-sampling mechanism limits the generalizability of our results to ‘normal’ (i.e., non-risk-based) investigations. However, the risk-based selection only pertains to approximately 15% of all investigations, and, thus, the vast majority are investigations of inconspicuous firms. Second, the stratum-based mechanism that ensures that firms will be investigated within a certain cycle (either 8 or 18 years) provides room for firms to anticipate investigations with a higher degree of certainty compared to a pure random selection.<sup>9</sup> Nonetheless, this only affects firms in a way that they know they will certainly be investigated after a specific period. ‘Surprising’ investigations that occur quicker than these rotation cycles are, of course, possible.

### *Investigation process*

The FREP describes the investigation procedure as follows (FREP, 2005, 2018a): Each selected firm is assigned to a FREP examiner, who will be the examiner-in-charge for the investigation. However, the examiner-in-charge will request additional internal or external workforce if necessary to ensure an appropriate and timely investigation. Depending on the time resources available, the examiner-in-charge decides when to start an investigation after the selection. However, firms selected because of the FREP or the BaFin having indications of erroneous financial reporting are usually prioritized and investigations of these firms start immediately. The thereupon starting

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<sup>9</sup> In this regard, our results for randomly selected firms may reflect lower-bound estimates, because firms may be, to some extent, better prepared, which may allow them to avoid some of the costs.



investigation procedure is highly standardized, independent of the reason for selection. First, the FREP will contact the firm, ask for cooperation and whether the auditor shall be involved. Furthermore, the FREP requests the annual financial report, management report, auditor's report, a list of unadjusted (immaterial) audit differences, and, depending on the scope of the investigation, the quarterly or semi-annual financial reports. For firms selected based on the random sampling mechanism, the FREP will decide on the scope of the investigation after reviewing these documents. For firms selected based on indications of erroneous financial reporting, the FREP initially limits the scope of the investigations to the areas under suspicion, but might extend the scope depending on its findings. After reviewing the documents, the FREP will send questionnaires with tight deadlines for responding and ask for additional documentation necessary for analyzing the subject matters. These inquiries are repeated until all questions are clarified. Afterwards, a second FREP examiner will critically review the investigation report. When the investigation is closed, the FREP informs the BaFin about the outcome, which, in the case of an error finding, mandates the firm to publish it in the German Federal Gazette. Moreover, IAS 8 requires the firm to correct the error in its subsequent annual financial report.

Overall, the FREP's financial reporting enforcement system is similar to that of the SEC with some minor differences. The SEC, on one hand, investigates firms because of a 'lead' (Blackburne et al., 2020b) or, on the other hand, as part of its periodical or transactional filing review process, where firms are selected risk-based (Cunningham and Leidner, 2020).<sup>10</sup> Whereas the FREP selects firms also based on risk factors or a 'lead', the vast majority of FREP investigations originate from a random selection (FREP, 2018b). The investigation process itself is similar again. During the filing review process, the SEC determines a SEC staff examiner or several examiners, who review a firm's disclosures by screening them for deficiencies and, in case of irregularities, send questions

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<sup>10</sup>Investigations based on a lead are largely under the responsibility of the SEC's Division of Enforcement (SEC, 2020a) and the filing review process under that of the Division of Corporation Finance (SEC, 2020b).

and requests for clarifications (Cunningham and Leidner, 2020). However, whereas such FREP investigations only become public in case of error findings, SEC investigations are disclosed in any way if the SEC has written a comment letter, which includes cases the SEC had a suspicion that was clarified during the investigation process. Hence, there might be a barrier for the SEC staff to contact the firm and clarify potential irregularities (Cunningham and Leidner, 2020), which does not exist for the FREP. The SEC Enforcement Division’s enforcement process is organized slightly different<sup>11</sup> but its responsibilities are similar to that of the FREP and the BaFin.<sup>12</sup> Last, unlike the FREP, the SEC has federal authority, which could result in firms taking SEC investigations more seriously. However, given that the FREP can bring the BaFin into action if a firm refuses to cooperate (Sec. 108 (1) no. 1 Securities Trading Act), the FREP acts with de facto federal authority. Altogether, besides differences in the selection and disclosure of investigations, the FREP’s and SEC’s investigation processes are similar.

## 2.2 Prior research

Prior research suggests that enforcement is an important factor to determine financial reporting outcomes (Holthausen, 2009). For example, several studies show that key regulatory changes such as the introduction of International Financial Reporting Standards (IFRS) or security market regulations are only effective in strong enforcement environments (e.g., Daske et al., 2008; Christensen et al., 2013). However, less is known about the direct costs and benefits of enforcement. Some studies document benefits of enforcement in the form of increases in liquidity (Ernstberger et al., 2012; Christensen et al., 2016), and increases in forecast accuracy (Hope, 2003). Silvers (2016) finds that investors of non-US firms that are listed in the US perceive an increase in the likelihood of SEC enforcement as net beneficial. On the other hand, Ewert and Wagenhofer (2019) suggest

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<sup>11</sup>See Zheng (2020) for details.

<sup>12</sup>Other than the SEC, the FREP’s focus lies solely on financial reporting enforcement. The enforcement of other topics related to the Securities Trading Act, such as insider trading, is the responsibility of the BaFin and, similar to the US, of courts (Sec. 6 Securities Trading Act).

that financial reporting quality does not always improve with greater enforcement strength. While these studies pertain to the general effects of installing or changing enforcement regimes, some studies focus on specific aspects of enforcement regimes.

Specifically, a large body of literature analyzes the ex-post remedial actions such as name-and-shame mechanisms for errors from enforcement investigations or disclosure of comment letters. Generally, the results show that error announcements are effective in penalizing transgressing firms (e.g., Feroz et al., 1991; Dechow et al., 1996; Beatty et al., 1998; Bonner et al., 1998; Beneish, 1999; Hines et al., 1999; Peasnell et al., 2001; Farber, 2005; Karpoff et al., 2008; Hitz et al., 2012). Moreover, comment letter disclosures seem to be effective in providing new information to the market (e.g., Dechow et al., 2016; Cunningham et al., 2017; Johnston and Petacchi, 2017; Bens et al., 2016) and change firms' future financial reporting behavior (e.g., Kubick et al., 2016; Brown et al., 2018).

The firm-level consequences of pro-active enforcement investigations, a cornerstone of many financial reporting enforcement systems, has just recently begun to receive attention. Generally, the consequences of investigations have been difficult to study because only enforcement investigations that lead to error findings are usually observable, while one cannot identify investigations without an error finding.<sup>13</sup> Non-error conclusions, however, are usually more frequent than errors. For example, in our setting, most error rates for random investigation are in the low 10 or 20 percents.<sup>14</sup> Moreover, an analysis based on observable enforcement actions with an error finding makes it impossible to disentangle the impact of the error itself from the investigation.

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<sup>13</sup>For a sample of German firms from 2006 to 2016, Hitz and Schnack (2019) find that approximately 5.6% of firms voluntarily disclose ongoing investigations, and 15.8% disclose information about concluded investigations. Similarly, Blackburne et al. (2020b) report that only 19% of US-firms disclose information about investigations at the outset.

<sup>14</sup>Specifically, in Germany, the error rate for all 84 (99) investigations in 2018 (2017) was 15% (15%). When only considering random-based investigations, the error rate was 11% (12%) in 2018 (2017) (FREP, 2019). ESMA (2017) reports the error rates for all European enforcers in 2014 and 2015. The weighted average rate for errors that lead to corrective notes or reissuances was 10%.

Prior research has used two ways to circumvent this issue. First, Christensen et al. (2020) and Florou et al. (2020) exploit a setting in the UK from 2004 to 2011, where the Financial Reporting Review Panel (FRRP) announced priority industry sectors that are subject to enforcement investigations with a higher likelihood (increase from 4.4% to 23.4%). Christensen et al. (2020) find that investors of priority industry sector firms react negatively to the announcement. The reaction is more negative for firms with abnormally high profits. The authors also find that transparency increases for firms in priority sectors. Overall, Christensen et al. (2020) interpret their results that the decline in market value is partly driven by an increase in transparency beyond the value-maximizing level. Florou et al. (2020) find that audit fees temporarily increase, and, accruals become more conservative. Second, Blackburne et al. (2020a) and Blackburne et al. (2020b) use novel data from the SEC on all investigations closed between 2000 and 2017. Blackburne et al. (2020b) find that enforcement investigations predict future declines in market values. Moreover, corporate insiders exploit this information by conducting insider trades that are on average profitable for their personal gain. Blackburne et al. (2020a) find that firms under investigation switch from accrual to real earnings management, increase conservatism, and show a reduced risk of accounting irregularities.

A drawback of both the UK and US settings is that they rely on endogenously selected investigations. In particular, the selection of target industry sectors by the FRRP is not random. The FRRP may choose sectors where they already expect issues. In fact, Christensen et al. (2020) find that low market returns, negative media coverage, and disclosure of accounting deficiencies are important determinants of the choice of target industry sectors. Hence, any results may be driven by these factors or the revelation of the FRRP's private information about reporting quality in target industry sectors.<sup>15</sup> Similarly, in the US, the SEC usually starts investigations based on a 'lead' (e.g., whistleblower information, press reports, surveillance activity) that suggests a possible

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<sup>15</sup>In addition, the selection of firms within these sectors may also be endogenous.

violation (Blackburne et al., 2020b). Given limited resources, the SEC likely pursues cases that do not involve much ambiguity and that are easier to win (Dechow et al., 2010). Hence, firms that are under investigation are a highly selective group, and any outcome measure may be correlated with the reason the firm was selected to be investigated in the first place. Another limitation of the SEC data is that the nature of each investigation, the outcome, the dates of communications with the target, and the dates of any Wells Notices are unknown (Blackburne et al., 2020b). Hence, the exact time span when the investigation is active cannot be determined.

With our setting, we overcome these drawbacks. In particular, the proprietary data allows us information about the start and conclusion date of the investigation, the reason of selection (random vs. risk-based selection), and the outcome of the investigation. Hence, we can determine the exact time span that a firm was under investigation. Moreover, in contrast to most other countries, more than 80% of enforcement investigations are based on a random sampling mechanism in Germany.<sup>16</sup> Hence, by exploiting the availability of randomly selected investigations, we avoid the majority of selection problems that prior studies suffer from. Thereby, we are the first to provide causal evidence on the effects of enforcement investigations. Moreover, because we also have risk-based investigations in our sample, we can determine their firm-level effects to reconcile our findings with prior literature.

### **2.3 Costs and benefits of financial reporting enforcement investigations**

Enforcement investigations potentially cause costs and benefits for firms that are under investigation.<sup>17</sup> These costs and benefits may be direct or indirect, and they can occur during

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<sup>16</sup>In the EU, the mean proportion of issuers that are selected either randomly or by rotation is 36%.

<sup>17</sup>Note that these costs and benefits pertain to the firm-level effects of investigations, which may differ from those of general financial reporting enforcement or securities regulation. The latter include potential positive externalities and cost savings, circumventing the limitations of private enforcement, and creating a binding commitment mechanism (e.g., Christensen et al., 2020; Coffee, 1984; Easterbrook and Fischel, 1984; Zingales, 2009; Johnson et al., 2002; Leuz and Wysocki, 2016). The costs include direct costs of establishing an enforcement system, difficulties of ensuring effectiveness of the system, and potential regulatory capture (e.g., Christensen et al., 2020; Stigler, 1971; Posner, 1974; Peltzman, 1976; Becker, 1983).

investigations or extend to after investigations are closed. Direct investigation costs occur when managers and staff have to spend time and other resources to deal with the investigation. Investigations consist of several rounds of inquiries. These inquiries usually focus on the enforcement priorities of the current year, but they often expand to other areas if the enforcement agent finds inconsistencies or suspects irregularities. Enforcement agents set deadlines of two to four weeks for firms to respond to these inquiries. Responses to the inquiries often require detailed argumentation for the accounting of complex transactions. Handling these inquiries consumes management and staff time, and potentially requires investments in information systems.

Other direct costs of enforcement investigations may come from firms' auditors. First, auditors may experience an increased engagement risk, which they may address with increased testing or other audit procedures. Moreover, given the potential reputation damages for auditors in case of error findings, auditors likely charge a risk premium. Second, because some of the investigator's inquiries require complex argumentation, clients told us in informal conversations that they regularly rely on their auditors for support. Similarly, some of the Big4 audit firms have created new enforcement investigation consulting branches that are specialized in consulting firms under investigation. Ultimately, enforcement investigations may increase audit fees, non-audit fees, or other consulting charges.

Enforcement investigations may also have indirect effects. For example, responding to investigations may distract management from focusing on important strategic or operational decisions, which may cause indirect costs by impacting firm investments and operational performance. Similarly, managers may experience an elevated risk when under investigation, which may cause them to delay investments or other important decisions. Moreover, managers may deviate from their 'normal' reporting behavior when being under the scrutiny of an enforcement institution. For example, if managers are aware of the investigation, they may want to appease the enforcement institution

with less aggressive accounting choices or additional transparency or other disclosures to minimize potential penalties or avoid negative investigation outcomes. By contrast, firms may restrict their provision of disclosures when they are under investigation to avoid any adverse consequences related to the investigation's outcome. For example, timely information about current developments may allow enforcement agents to better assess the plausibility of assumptions of certain accounting choices (e.g., expected growth rates in impairment tests). In addition, firms may delay the disclosure of news during investigations so that they can release the news bundled with a potentially negative outcome of the investigation. The extra workload related to ongoing investigations may also bring firms to neglect voluntary disclosures. Overall, on the one hand, changes in reporting may make some firms more transparent, which could be beneficial for shareholders if the transparency benefits are not outweighed by proprietary costs. On the other hand, any changes in the reporting behavior could mean a deviation from the value-maximizing level of a firm's transparency level. In this regard, firms under investigation could experience net costs if they deviate from the optimum level of transparency.

Furthermore, Blackburne et al. (2020b) suggest that corporate insiders may exploit insider information with regard to the enforcement investigation. Specifically, highly profitable insider trades spike during investigations. Hence, these insider trades potentially represent opportunistic actions at the cost of outside shareholders. By contrast, being under investigation could also discipline managers so that they abstain from opportunistic actions, which may benefit shareholders.

Several reasons exist why enforcement investigations may have *no* sustained effects on firm value. For example, managers are already under substantial scrutiny by equity holders (e.g., Niehaus and Roth, 1999), debt providers (e.g., Asquith et al., 2005), financial analysts (e.g., Mayew, 2008), and other regulators or tax authorities (e.g., Bozanic et al., 2017; Amiram et al., 2018). Hence, the additional scrutiny of an ongoing investigation may not have an incremental effect on managers'

behavior. Similarly, the fact that managers know that they will eventually be under investigation may already have a preemptive disciplining effect so that the actual investigation does not change their behavior. Moreover, because outsiders are generally not aware of ongoing investigations, managers may not feel the necessity to change anything. Hence, firms may not experience any costs or benefits except from the direct costs of the investigation.

Overall, the discussion above suggests that net costs and benefits are unclear. Hence, we ultimately consider the effect of enforcement investigations on firm value as an empirical question.<sup>18</sup>

### 3 Identification Strategy and Data

#### 3.1 Identification Strategy

For our identification strategy, we focus on the pre-investigation, during-investigation, and post-investigation period of each enforcement investigation. Specifically, by using a tight fixed effects structure, we identify effects that stem from the within-investigation variation over the pre-, during-, and post-period. Including the post-investigation period allows us to analyze whether a potential effect continues or attenuates after the investigation has ended, which helps us to separate temporary from more sustained enforcement effects. Furthermore, related to risk-based selected firms, considering the post-period allows us to differentiate between selection bias, which should persist after an investigation has ended, and a different exposure (i.e., different costs and benefits during investigations) of risk-based selected firms compared to randomly selected firms.

[Insert Figure 1 about here]

Figure 1 illustrates the general timeline and the different periods for an illustrative firm with

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<sup>18</sup>Note that we do not rely on that information about ongoing investigations is leaked to market participants for the costs and benefits to manifest in firm value changes. While some leakage may be going on, several indirect ways exist. For example, changes in firm's reporting and disclosure behavior can indirectly impact firm value through liquidity and the cost of capital. Moreover, many of the direct costs of audit fees or consulting fees are reflected in expenses, which ultimately affect profitability or other performance indicators. These will become known to the market via interim reports or 8K-type disclosures. Moreover, insider trades are another way how outside shareholders may become aware of firm value changes, which may trigger price corrections.



two investigations. Some of our proxies such as returns or daily spreads relate to the exact time span of enforcement investigations (see upper part of Figure 1). In these cases, the during-period covers the exact time a firm is being investigated, and the pre-period (post-period) covers 150 trading days before (after) the investigation has started (ended).<sup>19</sup> Other proxies such as discretionary accruals relate to fiscal periods. In these cases, the pre- and post-period represent the fiscal periods before and after the investigation period (see lower part of Figure 1). If an enforcement investigation spans over two fiscal periods, we classify both periods as during-periods (see investigation 2 in Figure 1).<sup>20</sup> In our empirical tests, we use the following generalized model:

$$\begin{aligned} Consequences_{i,t} = & \alpha + \beta_1 * DURING_{i,t} + \beta_2 * POST_{i,t} + \sum \beta_j Controls_{i,t} + \sum \beta_i FirmFE \\ & + \sum \beta_k InvestigationFE + \sum \beta_t TimeFE + \varepsilon_{i,t} \end{aligned} \quad (1)$$

Where  $Consequences_{i,t}$  is one of several potential consequences variables that are discussed below in more detail. Our main variables of interest are  $DURING_{i,t}$  and  $POST_{i,t}$ .  $DURING_{i,t}$  is a dummy variable that is one for the period during an enforcement investigation, and zero otherwise. If firms show an abnormal level of the consequences variable compared to before the investigation started, we predict  $DURING_{i,t}$  to be significantly different from zero.  $POST_{i,t}$  is a dummy variable that is one for the period after an enforcement investigation, and zero otherwise. If firms continue to show an abnormal level after the investigation is concluded, we expect  $POST_{i,t}$  to be significantly different from zero.  $Controls_{i,t}$  is a vector of  $j$  control variables related to the dependent variable.

Because firms can have multiple investigations, we use investigation fixed effects in addition to firm fixed effects (see Figure 1). The firm fixed effects  $Firm_i FE$  absorb the baseline level of the consequences variables for each firm, and the investigation fixed effects  $Investigation_k FE$  absorb the specific level of the consequences variables at the pre-, during-, and post-period for each

<sup>19</sup> Results and inferences are not sensitive to the exact length of the pre- and post-period. For example, the inferences remain similar using 100, 200, 300 days, or the same number of days as the respective investigation took.

<sup>20</sup> The results and inferences are similar if we require that firms need to be under investigation for at least 5% (10%; 20%; 30%) of days in a fiscal period for the classification as a during-period.

investigation.<sup>21</sup> *Time<sub>t</sub> FE* are two-digit SIC-industry-specific daily, semi-annually or yearly fixed effects, depending on the periodicity of the dependent variable. Specifically, for daily outcome variables, such as market returns or liquidity, we use industry-calendar-day-year as the period, where each day in each year for every two-digit SIC-industry has a specific fixed effect. For other measures that refer to the fiscal period such as audit fees or filing lags, we use industry-fiscal period fixed effects. By including *Time<sub>t</sub> FE*, the within-investigation variation is filtered for any industry-wide trends in the consequences variables.

Ultimately, this approach allows us to use each firm’s investigation as its own control. Thereby, any effects that we identify stem from the within-investigation variation over the pre-, during-, and post-period. Moreover, combining this approach with the random sampling of enforcement investigations used by the FREP allows us to draw causal inferences about the consequences of enforcement investigations.<sup>22</sup>

To distinguish between risk-based and randomly selected investigations, we interact the dummies for the during- and post-investigation periods with the dummy variable *RANDOM<sub>i,t</sub>*, which indicates whether a firm was randomly selected or not. Thereby, the during- and post-investigation dummies show the enforcement effect for risk-based selected firms, and the interaction variables show how this effect is different for randomly selected investigations.

$$\begin{aligned}
Consequences_{i,t} = & \alpha + \beta_1 * DURING_{i,t} + \beta_2 * POST_{i,t} + \beta_3 * DURING \times RANDOM_{i,t} \\
& + \beta_4 * POST \times RANDOM_{i,t} + \Sigma \beta_j Controls_{i,t} + \Sigma \beta_i FirmFE \\
& + \Sigma \beta_k InvestigationFE + \Sigma \beta_t TimeFE + \varepsilon_{i,t}
\end{aligned} \tag{2}$$

*RANDOM<sub>i,t</sub>* is a dummy variable that is one for the pre-, during-, and post-investigation

<sup>21</sup>In Figure 1, the investigation fixed effects are illustrated for the case with consequences variables that relate to fiscal periods. For those consequences variables that relate to the exact time span of enforcement investigations, the investigation fixed effects cover the pre-, during-, and post-period as illustrated in the upper part of Figure 1.

<sup>22</sup>We acknowledge that the results for the randomized firms likely only generalize to ‘normal’ (i.e., non-risk-based investigations) firms, because the risk-based selected firms are drawn from the pool before the randomization. However, we believe that this bias is limited given that the vast majority of firms (85% to 90%) are classified as normal.

periods if an investigation was randomly selected, and zero otherwise.<sup>23</sup> Equation (2) allows us to compare the results with prior research that has solely used risk-based selected firms.

## 3.2 Data

### *Sample*

Our initial sample comprises all public firms subject to enforcement between 2005 and 2018. The BaFin provides us with data on all enforcement investigations carried out during that time period. This data includes the exact start and end date of an investigation, the reason a firm was selected (i.e., random- or risk-based), and whether the investigation concluded with an error finding. We drop investigations that have not ended at the time we received the data from the BaFin. Furthermore, we merge investigations that overlap with their start and end dates.<sup>24</sup> This is the case, for example, if the FREP investigates an annual or semi-annual report and the investigated firm publishes a more recent annual or semi-annual report during the investigation, which the FREP decides to investigate as well.<sup>25</sup> This procedure leads to a final sample of 1,080 investigations or 632 firms.

### *Descriptive statistics*

Table 1 shows the descriptive statistics for the 632 firms over the period 2005 to 2018. 64% of the sample firms are traded on the regulated market of the Frankfurt stock exchange, and the remaining 36% at the regulated market of regional stock exchanges in Germany. The mean of total assets is approximately € 13 billion. Furthermore, 88.9% of the investigations are based on a random selection, whereas only 11.1% of the investigations are selected risk-based. Overall, the FREP concludes an investigation in 13.8% of the cases with an error finding, and the mean

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<sup>23</sup>Note that the main effect of  $RANDOM_{i,t}$  is absorbed by the  $Investigation_i$  fixed effect.

<sup>24</sup>Our results are not sensitive to the exclusion of these observations.

<sup>25</sup>Note that we assume that any merged investigations are risk-based selected and/or concluded with a finding if this is true for at least one of the overlapping investigations. However, our findings are not sensitive to that research design choice and remain similar if we drop all overlapping investigations.

(median) investigation length is approximately 251 (208) calendar days, which equals 178 (148) trading days.

[Insert Table 1 about here]

Table 2 shows the descriptive statistics separately for randomly (Columns 3 to 5) and risk-based selected firms (Column 6-8).<sup>26</sup> The firm fundamentals in Panel A show that randomly selected firms are highly similar to the population and that the FREP’s sampling mechanism randomizes successfully.<sup>27</sup> However, in line with our expectations, the significantly lower mean value of  $FINDING_{i,t}$  for randomly selected firms in Panel B shows that these firms have fewer error findings than the mean firm of the population.<sup>28</sup> At the same time, risk-based selected firms have significantly longer investigations, more investigations that conclude with error findings, and tend to be different in terms of their financials although most of these differences are not significant because of a high variation in the data.

[Insert Table 2 about here]

## 4 Results

### 4.1 Net Effect of Enforcement Investigations

To assess the firm-level net effect of enforcement investigations, we analyze firms’ market value of equity changes due to investigations. In Figure 2, we plot firms’ cumulative industry-adjusted abnormal returns separately for randomly and risk-based selected firms around investigations. No-

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<sup>26</sup>In addition to the investigation years, Panel A includes the pre- and post-investigation years for randomly and risk-based selected firms.

<sup>27</sup>For calculating the mean values for randomly selected firms in Panel A, we weight firms from the second stratum double because the likelihood of being selected is half compared to firms in the first stratum (refer to section 2.1 for details on the selection mechanism). Moreover, for calculating the significance of the difference between the population and randomly selected firms (Column 5), we adjust the population by exempting risk-based selected firms as they are drawn from the pool before the randomization.

<sup>28</sup>The significantly higher portion of firms listed on the regulated market of the Frankfurt stock exchange stems from firms that joined or left the regulated market during the sample period, resulting in firm years where these firms were not subject to the FREP’s enforcement.

tably, risk-based selected firms (red line in Figure 2) show abnormal reductions in their equity values during investigations. This decrease in equity values, however, already begins before investigations have started and continues, to some extent, after investigations have ended. These results suggest that risk-based selected firms may, to some extent, already be on a certain downward trend, which might have triggered the risk-based selection in the first place. By contrast, randomly selected firms (blue line in Figure 2) do not show any discernible cumulative abnormal returns.

[Insert Figure 2 about here]

We test the significance of these results by estimating the regression model introduced in section 3.1. To determine abnormal returns, we use  $RETURN_{i,t}$ , which is firm  $i$ 's return on day  $t$ , as the dependent variable. By including  $Industry \times Date_t$  fixed effects in the regressions, we determine abnormal returns adjusted for the normal return in the two-digit SIC-industry.<sup>29</sup> If ongoing enforcement investigations affect firms' equity values,  $DURING\_INVESTIGATION_{i,t}$  should be significantly different from zero.

Table 3 shows the results separately for randomly and risk-based selected firms. In Column 1 of Panel A, we analyze risk-based selected firms.<sup>30</sup> The coefficient of  $DURING\_INVESTIGATION_{i,t}$  is significantly negative with a value of 8.6 basis points, which means that risk-based selected firms' daily abnormal returns relative to their industry peers decrease, on average, by 8.6 basis points during investigations. This decline equals a negative cumulative abnormal return of 12.73% over the median investigation period length of 148 trading days. At the same time, the coefficient of  $POST\_INVESTIGATION_{i,t}$  is not significantly different from zero, indicating that the decline in market value does not continue to the same extent after an investigation has ended. At the same

<sup>29</sup>The results and inferences are similar when we use industry-adjusted returns as the dependent variable in the regression model.

<sup>30</sup> $DURING\_INVESTIGATION_{i,t}$  and  $POST\_INVESTIGATION_{i,t}$  are zero for randomly selected firms so that the dummy variable only reflects the effect of risk-based selected firms being investigated. Note that our results and inferences are similar if we drop all randomly selected firms.

time, the insignificant coefficient of  $POST\_INVESTIGATION_{i,t}$  also means that the lower market value is sustained and does not reverse afterwards. Hence, risk-based selected firms experience reductions in market values during enforcement investigations.

In Column 2, we run the same regression for randomly selected firms. Here, neither the coefficient of  $DURING\_INVESTIGATION_{i,t}$ , nor the coefficient of  $POST\_INVESTIGATION_{i,t}$  are significantly different from zero. We find similar results in Panel B of Table 3, where we include risk-based and randomly selected investigations in the regression, and differentiate between them by interaction terms. Here,  $DURING\_INVESTIGATION_{i,t}$  indicates the negative abnormal return for risk-based selected firms, and  $DURING\_INVESTIGATION \times RANDOM_{i,t}$  that this effect is close to zero for randomly selected firms. Consequently, enforcement investigations do not affect randomly selected firms' market value.

[Insert Table 3 about here]

Overall, these results are consistent with Christensen et al. (2020), who find negative abnormal returns for firms in industries that the British enforcement institution announces to be under increased enforcement scrutiny in the subsequent year. Their results, however, may not be driven by the increased enforcement intensity itself, but by the risk-based focus industry and firm selection. Our findings for risk-based selected firms and the null result related to randomly selected firms support this notion. In the next section, we shed further light on the mechanisms behind these findings.

## 4.2 Different Costs and Benefits or Selection Bias?

The null result for randomly selected investigations suggests that the negative firm value effect for risk-based selected firms is either a consequence of different costs and benefits related to enforcement investigations for these firms, or driven by the FREP's selection mechanism and thus

unrelated to the enforcement investigation itself. Specifically, risk-based selected firms may experience different costs and benefits during investigations, because FREP examiners may challenge those firms more or firms act differently during these investigations. By contrast, the negative consequences at the time of the investigations might have even occurred in the absence of an investigation given the risk-based selection is based on indications of erroneous financial reporting. In the following, we attempt to disentangle these alternative explanations by shedding more light on the role of selection.

Selection of risk-based firms can be based on 1) time-invariant factors, such as memberships in more risky industries; and 2) time-variant factors, such as temporary negative trends in KPIs or events that spike in short intervals, such as incidences of M&As or whistle-blower cases. The graphical inspection in Figure 2 indicates that the downward development for risk-based selected firms already begins before investigations start, which is consistent with a selection based on time-variant factors (2)). Alternatively, it is also possible that this trend is driven by a time-invariant difference (1)) that is also a priced risk factor that is not accounted for in the abnormal returns. With the research design in our main regression analysis, we can rule out 1) because of the firm-fixed effects. However, to assess the relative magnitude of 1), we repeat our main analysis without firm and investigation fixed effects in Table 4. In Column 1, we find that the negative firm value effect is approximately 50% higher compared to our main results. Hence, these additional 50% can be attributed to 1) and a less stringent research design would have allocated this selection-driven increment to the effect of enforcement investigations.

[Insert Table 4 about here]

To shed more light on to what extent our results may be driven by 2), we employ a filter technique borrowed from macroeconomics. Specifically, we use a Butterworth high-pass filter to

cancel out any short- or long-term trend effects in firms' returns. Such trend components do not need to be linear, but can be of any order. By specifying a minimum period of oscillation that should be retained in the time series, we can filter out any trends that exceed this minimum period. We set the minimum period of oscillation equal to the length of a firm's investigation.<sup>31</sup> Thereby, we only retain return movements that that can be related to the investigation itself.<sup>32</sup> For example, assume that the reason for a risk-based selection was a dubious M&A-deal a few weeks before the investigation starts. This M&A-deal might trigger a downward development in returns similar to the length of the investigation. However, by using the Butterworth high-pass filters, our treatment variable  $DURING\_INVESTIGATION_{i,t}$  would ignore such return effects. Hence,  $DURING\_INVESTIGATION_{i,t}$  only captures firm value effects during the ongoing investigation.

Column 2 of Table 4 reports the results after adjusting each firm's return with the Butterworth filter. Interestingly, the negative returns for risk-based selected firms disappear, suggesting that after filtering out trends that exceed the investigation period, there is no significant change in returns during investigations. This result suggests that our findings related to risk-based selected firms are likely a consequence of selection bias, and it is unlikely that it is driven by different costs and benefits during these investigations.

### 4.3 Are randomly selected firms entirely unaffected by investigations?

The prior analyses suggest that randomly selected firms may not be affected at all by ongoing enforcement investigations. In this section, we attempt to shed more light on this. Particularly, we test whether the null result in returns for randomly selected firms may be due to benefits and costs offsetting each other in their effect on firm value. To test this, we identify channels potentially underlying market value effects. As elaborated in section 2.3, one channel is changes in firms'

<sup>31</sup> For firms that have multiple investigations we use the mean investigation length. Results and inferences, however, are similar when we use the minimum or maximum investigation length.

<sup>32</sup> This also means that we remove trends that persist after investigations have ended but might be related to the investigation. However, this concern is attenuated by Figure 2 showing only weak changes in abnormal returns in the post-investigation period.



transparency, which affect information asymmetries between insiders and outsiders. Information asymmetries regularly result from firms not providing all information to the capital market in a timely manner. Hence, to more directly capture how enforcement investigations could affect a firm’s transparency, we also measure changes in firms’ financial reporting behavior that are likely related to information asymmetries. Specifically, firms could change the timing of disclosures and the quantity of firm-related news because of the additional workload and uncertainty related to the enforcement investigation, thereby decreasing the information supply. Moreover, enforcement investigations could also affect the contents of financial reports at the time of an investigation. This could be the case, for example, if the FREP questions certain prior accounting choices or a firm stretched discretion related to them, which then triggers changes in a firm’s financial reporting and related uncertainty during an investigation. We investigate these potential changes by measuring three dimensions of financial reporting behavior, that is, firms’ filing lags of their annual and semi-annual reports (i.e., timing), the number of press releases a firm discloses (i.e., quantity), and how reported accruals change during investigations (i.e., content).

To measure information asymmetries, we follow prior research (e.g., Daske et al., 2008; Lang et al., 2012) and specify  $SPREAD_{i,t}$ , which is firm  $i$ ’s relative bid-ask spread on day  $t$ . We measure the three dimensions of a firm’s financial reporting behavior as follows: First, for the timing dimension, we specify the variable  $FILING\_LAG_{i,t}$ , which reflects firm  $i$ ’s filing lag in log-days for its annual and semi-annual financial reports after the corresponding closing dates.<sup>33</sup> Because of systematic differences between annual and semi-annual reports, we include a dummy variable that equals one for filing lags of annual reports, and zero for filing lags of semi-annual reports. Second, for the quantity dimension, we specify  $NO\_PRESS\_REL_{i,t}$ , which is the mean number of

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<sup>33</sup>In the case a firm publishes both, an annual and a semi-annual report in the pre-, during-, and post-investigation periods, respectively, we consider the filing lag of the annual report and if there is more than one annual report filed in one of the periods, we calculate the period’s mean filing lag. Because we have ultimately only three values of  $FILING\_LAG_{i,t}$  per investigation, we control for industry-specific time trends with two-digit SIC-industry-by-year fixed effects.

daily published press releases over the pre-, during-, and post-investigation period, respectively. These press releases are firm-initiated and comprise, for example, information on firms' business development or earnings news.<sup>34</sup> Finally, besides uncertainties leading to changes in the timing and quantity of information, firms financial reporting could also change in terms of content, that is, firms could adapt certain accounting or valuation methods that the FREP questions in the report being investigated. In this case, a firm's financial statement numbers would change abnormally during investigation years compared to the annual financial report of the pre-investigation year. Therefore, similar to Louis and White (2007), we measure financial reporting behavior by abnormal accruals  $|DACC_{i,t}|$  as firm  $i$ 's absolute abnormal accruals in year  $t$  by the ROA-adjusted modified Jones model (Kothari et al., 2005). If enforcement investigations affect firms' transparency, we expect the sum of  $DURING\_INVESTIGATION_{i,t}$  and  $DURING\_INVESTIGATION \times RANDOM_{i,t}$  to be significantly different from zero.

Panel A in Table 5 reports the results for the relative bid-ask spreads. We use the same research design as in our main tests, where  $DURING\_INVESTIGATION_{i,t}$  and  $POST\_INVESTIGATION_{i,t}$  show the changes in bid-ask spreads for risk-based selected firms relative to their industry peers with the pre-investigation period as the reference, and the interaction term with  $RANDOM_{i,t}$  how this effect is different for randomly selected firms. In Column 1, the interaction term  $DURING\_INVESTIGATION \times RANDOM_{i,t}$  is significantly negative with a magnitude that is offsetting the positive coefficient of  $DURING\_INVESTIGATION_{i,t}$ .  $POST\_INVESTIGATION \times RANDOM_{i,t}$  indicates the same for the post-investigation period, suggesting that randomly selected firms' bid-ask spreads are unaffected by enforcement investigations. The coefficient of  $DURING\_INVESTIGATION_{i,t}$  reflects the changes in bid-ask spreads of risk-based selected firms. In particular, it is significantly positive and indicates an increase of risk-based selected firms' relative bid-ask spreads from the pre-

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<sup>34</sup>Note that we drop duplicate press releases that Ravenpack stores because of different sources providing the same press releases published by a firm.

to the during-period of approximately 1 percentage point when compared to their industry peers. Given a mean relative bid-ask spread of 3.7%, this effect is also economically meaningful. Because the  $POST\_INVESTIGATION_{i,t}$  dummy is also significantly positive with a similar magnitude, this effect seems to persist even after an investigation has ended. This persistence of the effect is again an indicator that the change in spreads may not directly be related to the investigation, but rather driven by other sustained changes that potentially triggered the investigation in the first place.

Panel B in Table 5 shows the results for the transparency-related financial reporting behavior. Consistently, in all three dimensions, we do not find significant changes in randomly selected firm's financial reporting behavior. These results are also consistent with the null result for randomly selected firms' bid-ask spreads, suggesting that firms' transparency and financial reporting behavior are largely unaffected by enforcement investigations. At the same time, the significant coefficients on  $DURING\_INVESTIGATION_{i,t}$  in Columns 1 and 3 indicate an increase in risk-based selected firms' filing lags (13% or 7 days) and abnormal changes in their reported discretionary accruals during investigations. Considering that investors are sensitive to abnormally high filing lags (e.g., Bagnoli et al., 2002) and that there seem to be abnormal changes in risk-based selected firms' financial reporting behavior, this result is in line with our findings for the bid-ask spreads and, ultimately, the firm value effect. However, as our analysis in section 4.2 suggests, these results are likely the result of the FREP's risk-based selection mechanism.

Overall, consistently with our null result for firm values, we find that enforcement investigations do not affect firms' transparency or related dimensions of financial reporting behavior. These results support the notion that on-going enforcement investigations do not create significant benefits and costs for firms being investigated, hence shifting the question on whether financial reporting enforcement is net beneficial or costly rather to time-invariant factors, such as its prevention function or fees for financing it.

[Insert Table 5 about here]

#### 4.4 Further Analyses on the Effects of Enforcement Investigations

*In the next steps of our analysis, we will shed further light on potential channels underlying the firm value effect. In particular, we plan to analyze real effects, audit fees, and insider trading. We apologize that this is still work-in-progress.*

## 5 Conclusion

Understanding the effects of financial reporting enforcement has been of considerable interest in the literature. Prior research has used settings with a changing enforcement intensity or proactive investigations based on risk-based selection, and finds various benefits and costs related to financial reporting enforcement. These research designs, however, have the disadvantage of being either broad in capturing enforcement effects (enforcement intensity settings), and/or suffer from selection biases (risk-based selected enforcement investigations). Hence, prior research lacks a setting with exogenous enforcement investigations. We overcome this issue by taking advantage of our access to proprietary data of the German enforcement system, which comprises a random combined with a risk-based selection mechanism.

By investigating enforcement effects for risk-based selected firms, we largely confirm the results of prior research. Specifically, we find a negative market value effect for these firms, indicating that enforcement investigations are net costly to those firms. We also find that this could be a consequence of a worse transparency that is reflected in increased bid-ask spreads during investigations that might be related to a higher filing lag of annual and semi-annual reports, and a change in firms' financial reporting behavior. However, when investigating randomly selected investigations, that is, investigations where we can rule out selection bias, we find that none of the results hold.

Our study is subject to two caveats. First, prior research regards the German enforcement

mechanism as relatively weak in its intensity (Brown et al., 2014; Hitz et al., 2012). Hence, any null-finding we report for the randomly selected firms could be a consequence of a low enforcement intensity, and, thus not extend to settings with stronger enforcement such as the SEC in the US. However, our findings of relatively strong effects for the risk-based selected sample firms do not support this concern. Second, we only measure costs and benefits that emerge during actual enforcement investigations. Besides this, financial reporting enforcement also causes costs and benefits that are time-invariant. For example, the effect of firms and auditors knowing that they could be investigated at any time likely creates the benefit of more compliance. Consequently, for a comprehensive judgment of financial reporting enforcement effects, studies investigating time-invariant costs and benefits need to be taken into account.

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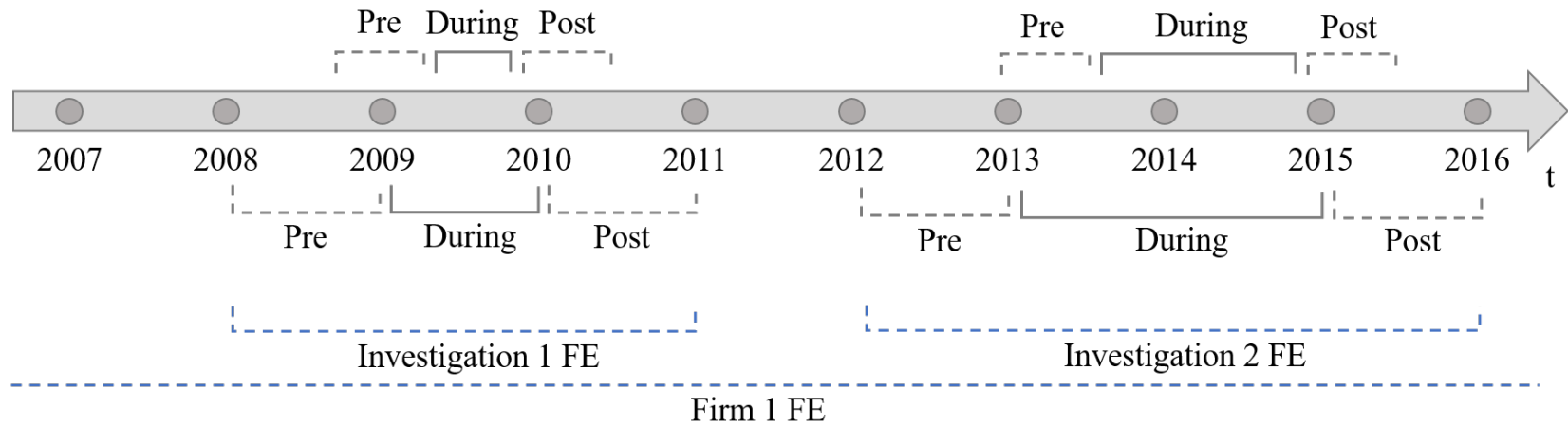
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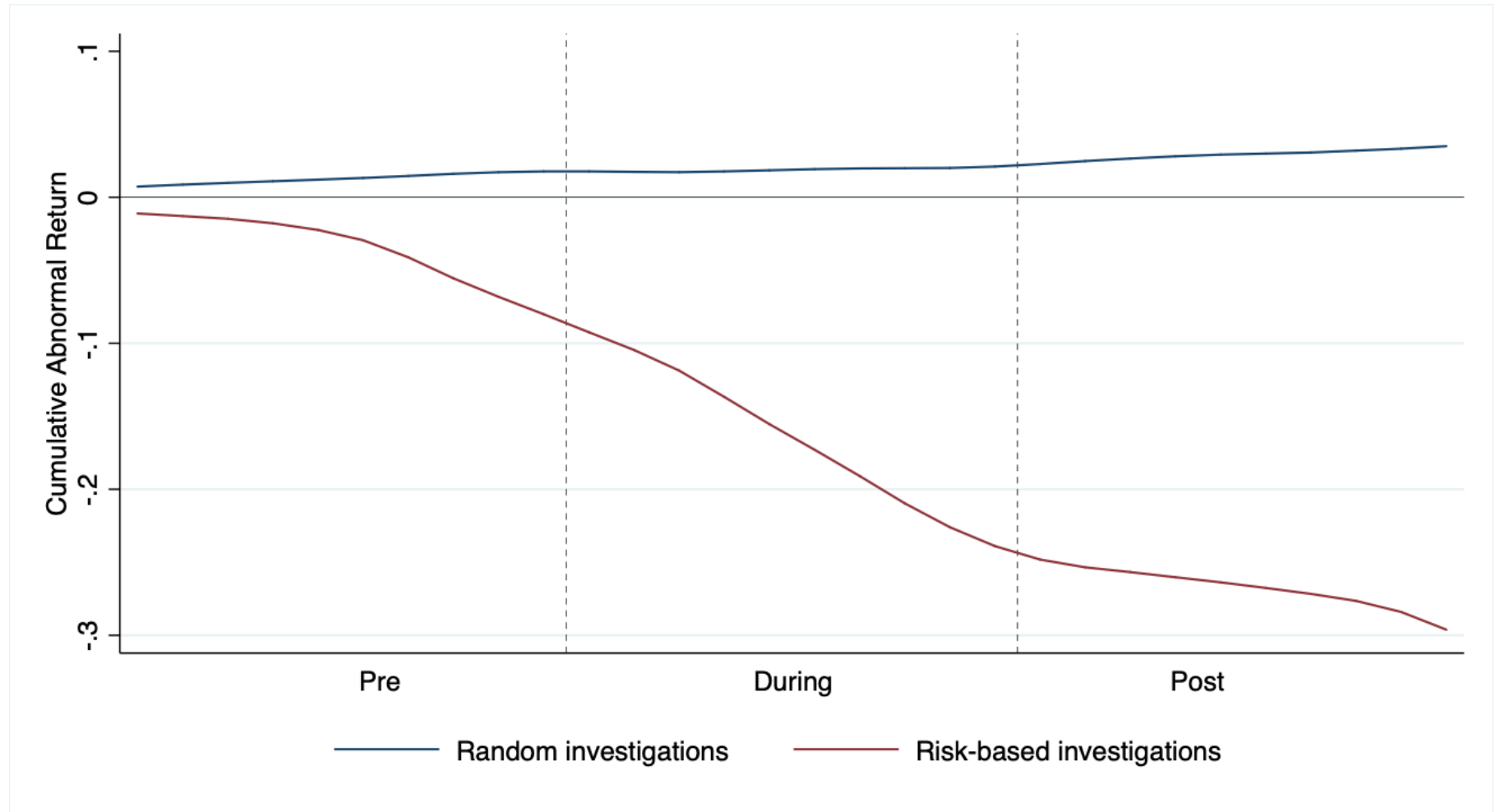
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**FIGURE 1**  
**Research Design and Fixed Effects Structure**



This Figure illustrates the general timeline and the different periods for an illustrative firm with two investigations. Some of our proxies such as returns or daily spreads relate to the exact time span of enforcement investigations (see upper part of the figure). In these cases, the during-period covers the exact time a firm is being investigated, and the pre-period (post-period) covers 150 trading days before (after) the investigations has started (ended). Other proxies such as discretionary accruals relate to fiscal periods. In these cases, the pre- and post-period represent the fiscal periods before and after the investigation period (see lower part of the figure). If an enforcement investigation spans over two fiscal periods, we classify both periods as during-periods (see investigation 2). Because firms can have multiple investigations, we use investigation fixed effects in addition to firm fixed effects. The firm fixed effects  $Firm_i FE$  absorb the baseline level of the consequences variables for each firm, and the investigation fixed effects  $Investigation_k FE$  absorb the specific level of the consequences variables at the pre-, during-, and post-period for each investigation. This allows us to investigate the within-variation over the pre-, during-, and post-period, i.e., the change of an outcome variable from the pre- to the during- and post-investigation period, respectively. Note that the investigation fixed effects are illustrated for the case with consequences variables that relate to fiscal periods. For those consequences variables that relate to the exact time span of enforcement investigations, the investigation fixed effects cover the pre-, during-, and post-period as illustrated in the upper part of Figure 1.

**FIGURE 2**  
**Net Effect of Financial Reporting Enforcement on Firm Value**



This Figure illustrates the cumulative abnormal returns for randomly and risk-based selected firms around investigations. The x-axis shows the time separated into the pre-investigation period (150 trading days before the start of an investigation), the during-investigation period, and the post-investigation period (150 trading days after the end of an investigation). Because investigations differ in length, we split the pre-, during-, and post-investigation periods into quantiles in a way so we obtain comparable time intervals. The y-axis shows the cumulative industry-adjusted abnormal return. The blue (red) line shows the mean cumulative abnormal returns for randomly (risk-based) selected firms.

**TABLE 1**  
**Descriptive statistics**

**Panel A: Sample firm characteristics (firm year-level)**

Number of distinct firms: 632

	<b>N</b>	<b>Mean</b>	<b>STD</b>	<b>Q1</b>	<b>Median</b>	<b>Q3</b>
TOTAL_ASSETS <sub><i>i,t</i></sub> (€ million)	7003	13 215.382	92 923.191	48.080	187.189	1243.400
NET_SALES <sub><i>i,t</i></sub> (€ million)	7013	3792.144	14 805.887	31.810	147.315	946.713
ROA <sub><i>i,t</i></sub>	6370	0.037	0.148	0.000	0.031	0.072
MARKET_CAP <sub><i>i,t</i></sub> (€ million)	7691	2368.302	9181.888	20.590	99.120	599.040
REG_MARKET_FRANKFURT <sub><i>i,t</i></sub>	9044	0.639	0.480	0.000	1.000	1.000

**Panel B: Investigation-related variables (investigation-level)**

Number of distinct investigations: 1080

	<b>N</b>	<b>Mean</b>	<b>STD</b>	<b>Q1</b>	<b>Median</b>	<b>Q3</b>
RANDOM <sub><i>i,t</i></sub> (%)	1080	0.889	0.312	1.000	1.000	1.000
FINDING <sub><i>i,t</i></sub> (%)	1080	0.138	0.345	0.000	0.000	0.000
LENGTH <sub><i>i,t</i></sub> (calendar days)	1080	251.008	163.311	150.000	208.000	302.000
LENGTH <sub><i>i,t</i></sub> (trading days)	1080	178.800	116.331	107.000	148.000	215.000

This table presents the descriptive statistics. Panel A shows the sample firm characteristics on the firm year-level and Panel B the investigation-related variables on the investigation-level.  $TOTAL\_ASSETS_{i,t}$  is firm  $i$ 's total assets of firm  $i$  in year  $t$  in million euros.  $NET\_SALES_{i,t}$  is firm  $i$ 's net sales in year  $t$  in million euros.  $ROA_{i,t}$  is firm  $i$ 's return on assets in year  $t$ , calculated as net income divided by total assets at the beginning of the period.  $MARKET\_CAP_{i,t}$  is firm  $i$ 's market capitalization in year  $t$  in million euros.  $REG\_MARKET\_FRANKFURT_{i,t}$  is an indicator variable that equals one if firm  $i$  is listed in the regulated market of Frankfurt stock exchange, and zero if it is listed at a regulated market of a regional stock exchange in Germany.  $RANDOM_{i,t}$  is an indicator variable that equals one in the pre-, during-, and post investigation period if firm  $i$  is being investigated as a result of a random selection, and zero for a risk-based selection.  $FINDING_{i,t}$  is an indicator variable that equals one in the pre-, during-, and post-investigation period if the investigation at firm  $i$  is concluded with a finding, and zero if the investigation is concluded without a finding.  $LENGTH_{i,t}$  (calendar days) is the number of calendar days from the beginning to the end of an investigation, and  $LENGTH_{i,t}$  (trading days) the number of trading days from the beginning to the end of an investigation.

**TABLE 2**  
**Descriptive statistics for random vs. risk-based selected firms**

**Panel A: Sample firm characteristics (firm year-level)**

	Population		Randomly selected			Risk-based selected			Random-risk
	N	Mean	N	Mean	Diff. to pop.	N	Mean	Diff. to pop.	Diff.
TOTAL_ASSETS <sub>i,t</sub> (€ million)	7003	13 215.382	3029	11 600.080	-1615.302	340	21 274.751	8059.369	-9674.671*
NET_SALES <sub>i,t</sub> (€ million)	7013	3792.144	3033	3206.077	-586.067	339	6931.531	3139.389***	-3725.455***
ROA <sub>i,t</sub>	6370	0.037	2919	0.065	0.028	313	-0.026	-0.063	0.090
MARKET_CAP <sub>i,t</sub> (€ million)	7691	2368.302	3016	2215.435	-152.867	396	2557.690	189.390	-342.255
REG_MARKET_FRANKFURT <sub>i,t</sub>	9044	0.639	3289	0.812	0.173***	422	0.796	0.157***	0.016

**Panel B: Investigation-related variables (investigation-level)**

	Population		Randomly selected			Risk-based selected			Random-Risk
	N	Mean	N	Mean	Diff. to pop.	N	Mean	Diff. to pop.	Diff.
FINDING <sub>i,t</sub>	1080	0.138	961	0.103	-0.035**	119	0.420	0.282***	-0.317***
LENGTH <sub>i,t</sub> (calendar days)	1080	251.008	961	243.745	-7.263	119	309.660	58.652***	-65.915***
LENGTH <sub>i,t</sub> (trading days)	1080	178.800	961	173.626	-5.174	119	220.580	41.780***	-46.953***

This table presents the descriptive statistics separately for randomly and risk-based selected firms. Panel A shows the sample firm characteristics on the firm year-level and, for randomly and risk-based selected firms, only includes the pre-, during-, and post-investigation years. Panel B presents the investigation-related variables on the investigation-level. Columns 1-2 show the descriptive statistics for the population and Columns 3-5 (6-8) show them for the randomly (risk-based) selected firms and the corresponding differences to the population means, respectively. Column 9 shows the differences between randomly and risk-based selected firms.  $TOTAL\_ASSETS_{i,t}$  is firm  $i$ 's total assets of firm  $i$  in year  $t$  in million euros.  $NET\_SALES_{i,t}$  is firm  $i$ 's net sales in year  $t$  in million euros.  $ROA_{i,t}$  is firm  $i$ 's return on assets in year  $t$ , calculated as net income divided by total assets at the beginning of the period.  $MARKET\_CAP_{i,t}$  is firm  $i$ 's market capitalization in year  $t$  in million euros.  $REG\_MARKET\_FRANKFURT_{i,t}$  is an indicator variable that equals one if firm  $i$  is listed in the regulated market of Frankfurt stock exchange, and zero if it is listed at a regulated market of a regional stock exchange in Germany.  $RANDOM_{i,t}$  is an indicator variable that is equal to one in the pre-, during-, and post investigation period if firm  $i$  is being investigated as a result of a random selection, and zero for a risk-based selection.  $FINDING_{i,t}$  is an indicator variable that equals one in the pre-, during-, and post-investigation period if the investigation at firm  $i$  is concluded with a finding, and zero if the investigation is concluded without a finding.  $LENGTH_{i,t}$  (calendar days) is the number of calendar days from the beginning to the end of an investigation, and  $LENGTH_{i,t}$  (trading days) the number of trading days from the beginning to the end of an investigation. For calculating the means for randomly selected firms in Panel A, we weight firms from the second stratum double because the likelihood of being selected is half compared to firms in the first stratum (refer to section 2.1 for details on the selection mechanism). Moreover, for calculating the significance of the difference between the population and randomly selected firms (Column 5), we adjust the population by exempting risk-based selected firms as they are drawn from the pool before the randomization. \*, \*\*, \*\*\* denote statistical significance at the 0.10, 0.05, and 0.01 levels (two-tailed tests).

**TABLE 3**  
**Net Effect of Financial Reporting Enforcement on Firm Value**

**Panel A: Separate regressions**

	Risk-Based Selected	Randomly Selected
DURING_INVESTIGATION <sub><i>i,t</i></sub>	-8.627** (-1.97)	-0.510 (-0.34)
POST_INVESTIGATION <sub><i>i,t</i></sub>	-2.135 (-0.41)	-1.168 (-1.27)
Firm <sub><i>i</i></sub> FE	Yes	Yes
Investigation FE	Yes	Yes
Industry×Date <sub><i>t</i></sub> FE	Yes	Yes
Adjusted <i>R</i> <sup>2</sup>	0.109	0.109
Observations	1,068,858	1,068,858

**Panel B: Regression with interaction terms**

	RETURN <sub><i>i,t</i></sub>
DURING_INVESTIGATION <sub><i>i,t</i></sub>	-8.665** (-1.97)
POST_INVESTIGATION <sub><i>i,t</i></sub>	-2.299 (-0.42)
DURING_INVESTIGATION×RANDOM <sub><i>i,t</i></sub>	8.124* (1.82)
POST_INVESTIGATION×RANDOM <sub><i>i,t</i></sub>	1.248 (0.22)
Firm <sub><i>i</i></sub> FE	Yes
Investigation FE	Yes
Industry×Date <sub><i>t</i></sub> FE	Yes
Adjusted <i>R</i> <sup>2</sup>	0.109
Observations	1,068,858

This table presents the effect of enforcement investigations on firms' equity values. Panel A shows the results separately for randomly and risk-based selected firms. The dependent variable  $RETURN_{i,t}$  is firm  $i$ 's stock return at day  $t$  in basis points. In Column 1,  $DURING\_INVESTIGATION_{i,t}$  is an indicator variable that equals one if firm  $i$  is investigated at day  $t$  as a result of a risk-based selection, and zero otherwise.  $POST\_INVESTIGATION_{i,t}$  is an indicator variable that equals one for a risk-based selected firm  $i$  for the 150 trading days after the end of an investigation, and zero otherwise. In Column 2,  $DURING\_INVESTIGATION_{i,t}$  is an indicator variable that equals one if firm  $i$  is investigated at day  $t$  as a result of a random selection, and zero otherwise.  $POST\_INVESTIGATION_{i,t}$  is an indicator variable that equals one for a random selected firm  $i$  for the 150 trading days after the end of an investigation, and zero otherwise. Panel B shows the results for all investigations, that is, randomly and risk-based selected investigations.  $RANDOM_{i,t}$  is an indicator variable that equals one in the pre-, during-, and post-investigation period if firm  $i$  is being investigated as a result of a random selection, and zero for a risk-based selection. In all regressions, we include firm  $i$ , investigation, and two-digit SIC-industry×date  $t$  fixed effects. We cluster standard errors at the firm-level. Robust t-statistics are reported in parentheses. \*, \*\*, \*\*\* denote statistical significance at the 0.10, 0.05, and 0.01 levels (two-tailed tests).

**TABLE 4**  
**Net Effect of Financial Reporting Enforcement on Firm Value**

	RETURN <sub><i>i,t</i></sub>	FILTERED_RETURN <sub><i>i,t</i></sub>
DURING_INVESTIGATION <sub><i>i,t</i></sub>	−13.164*** (−3.66)	−0.573 (−0.32)
POST_INVESTIGATION <sub><i>i,t</i></sub>	−3.390 (−0.83)	0.719 (0.32)
DURING_INVESTIGATION×RANDOM <sub><i>i,t</i></sub>	12.685*** (3.40)	0.160 (0.09)
POST_INVESTIGATION×RANDOM <sub><i>i,t</i></sub>	2.822 (0.67)	−1.212 (−0.53)
RANDOM <sub><i>i,t</i></sub>	1.212 (1.62)	
Firm <sub><i>i</i></sub> FE	No	Yes
Investigation FE	No	Yes
Industry×Date <sub><i>t</i></sub> FE	Yes	Yes
Adjusted $R^2$	0.109	0.106
Observations	1,068,858	1,068,858

This table presents the effect of enforcement investigations on firms' equity values with two different specifications. Compared to our main model in Table 3, Column 1 excludes firm and investigation fixed effects. Column 2 includes firm and investigation fixed effects, but uses filtered daily returns after applying a Butterworth high-pass filter following Pollock (2000). In Column 1, the dependent variable  $RETURN_{i,t}$  is firm  $i$ 's stock return at day  $t$  in basis points. In Column 2, the dependent variable  $FILTERED\_RETURN_{i,t}$  is firm  $i$ 's stock return at day  $t$  in basis points filtered with a Butterworth high-pass filter (Pollock, 2000).  $DURING\_INVESTIGATION_{i,t}$  is an indicator variable that equals one if firm  $i$  is investigated at day  $t$ , and zero otherwise.  $POST\_INVESTIGATION_{i,t}$  is an indicator variable that equals one for the 150 trading days after the end of an investigation, and zero otherwise.  $RANDOM_{i,t}$  is an indicator variable that equals one in the pre-, during-, and post-investigation period if firm  $i$  is being investigated as a result of a random selection, and zero for a risk-based selection. In all regressions, we include two-digit SIC-industry×date  $t$  fixed effects. We cluster standard errors at the firm-level. Robust t-statistics are reported in parentheses. \*, \*\*, \*\*\* denote statistical significance at the 0.10, 0.05, and 0.01 levels (two-tailed tests).

**TABLE 5**  
**Information Asymmetries and Enforcement**

**Panel A: Information asymmetries**

	<u>SPREAD<sub>i,t</sub></u>
DURING_INVESTIGATION <sub>i,t</sub>	1.047* (1.74)
POST_INVESTIGATION <sub>i,t</sub>	1.044* (1.76)
DURING_INVESTIGATION×RANDOM <sub>i,t</sub>	−1.015* (−1.69)
POST_INVESTIGATION×RANDOM <sub>i,t</sub>	−1.019* (−1.71)
Firm <sub>i</sub> FE	Yes
Investigation FE	Yes
Industry×Date <sub>t</sub> FE	Yes
Adjusted $R^2$	0.559
Observations	1,045,262

**Panel B: Financial reporting behavior**

	<u>FILING_LAG<sub>i,t</sub></u>	<u>NO_PRESS_REL<sub>i,t</sub></u>	<u> DACC<sub>i,t</sub> </u>
DURING <sub>i,t</sub>	0.1299** (2.12)	−0.0002 (−0.19)	0.029* (1.90)
POST <sub>i,t</sub>	0.0726 (0.71)	−0.0007 (−0.64)	0.016 (0.86)
DURING×RANDOM <sub>i,t</sub>	−0.1377** (−2.08)	−0.0011 (−1.01)	−0.033** (−2.06)
POST×RANDOM <sub>i,t</sub>	−0.0695 (−0.68)	−0.0008 (−0.67)	−0.013 (−0.70)
Control annual vs. semi-annual	Yes	No	No
Firm <sub>i</sub> FE	Yes	Yes	Yes
Investigation FE	Yes	Yes	Yes
Industry×Year <sub>t</sub> FE	Yes	Yes	Yes
Adjusted $R^2$	0.777	0.787	0.560
Observations	1,615	3,235	4,770

This table presents the effect of enforcement investigations on firms' transparency (Panel A) and reporting behavior (Panel B). In Panel A, the dependent variable  $SPREAD_{i,t}$  is firm  $i$ 's relative bid-ask spread at day  $t$  in percent. In Column 1 of Panel B, the dependent variable  $FILING\_LAG_{i,t}$  is the natural logarithm of one plus the mean filing lag in days of firm  $i$ 's annual or semi-annual reports after the closing date in year  $t$  in the pre-, during-, and post-investigation period, respectively. In Column 2, the dependent variable  $NO\_PRESS\_RELEASES_{i,t}$  is the mean number of firm  $i$ 's published press releases in the pre-, during-, and post-investigation period, respectively, scaled by the length of the pre-, during-, and post-investigation period. In Column 3, the dependent variable  $|DACC_{i,t}|$  is firm  $i$ 's absolute discretionary accruals in year  $t$ , scaled by total assets and calculated with the ROA-adjusted modified Jones model following Kothari et al. (2005).  $DURING\_INVESTIGATION_{i,t}$  is an indicator variable that equals one if firm  $i$  is investigated at day  $t$ , and zero otherwise.  $POST\_INVESTIGATION_{i,t}$  is an indicator variable that equals one for firm  $i$  for the 150 trading days after the end of an investigation, and zero otherwise.  $RANDOM_{i,t}$  is an indicator variable that equals one in the pre-, during-, and post-investigation period if firm  $i$  is being investigated as a result of a random selection, and zero for a risk-based selection. In all regressions, we include firm  $i$ , investigation, and two-digit SIC-industry×date  $t$  (Panel A) or SIC-industry×year  $t$  (Panel B) fixed effects. We cluster standard errors at the firm-level. Robust t-statistics are reported in parentheses. \*, \*\*, \*\*\* denote statistical significance at the 0.10, 0.05, and 0.01 levels (two-tailed tests).