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IES Working Paper 40/2020

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Bibliographic information:
de Batz L., Kočenda E. (2020): "Financial Crime and Punishment: A Meta-Analysis" IES Working Papers 40/2020. IES FSV. Charles University.

This paper can be downloaded at: http://ies.fsv.cuni.cz

Financial Crime and Punishment: A Meta-Analysis

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October 2020

Abstract:
We examine how the publication of intentional financial crimes committed by listed firms is interpreted by financial markets, using a systematic and quantitative review of existing empirical studies. Specifically, we conduct a meta-regression analysis and investigate the extent and nature of the impact that the publication of financial misconducts exerts on stock returns. We survey 111 studies, published between 1978 and 2020, with a total of 439 estimates from event studies. Our key finding is that the average abnormal returns calculated from this empirical literature are affected by a negative publication selection bias. Still, after controlling for this bias, our meta-analysis indicates that publications of financial crimes are followed by statistically significant negative abnormal returns, which suggests the existence of an informational effect. Finally, the MRA results demonstrate that crimes committed in common law countries, alleged crimes, and accounting crimes carry particularly weighty information for market participants. The results call for more transparency on side of enforcers along enforcement procedures, to foster timely and proportionate market reactions and support efficient markets.

JEL: C83, G14, G18, K42, N24
Keywords: Meta-Analysis; Event study; Financial Misconduct; Fraud; Financial Markets, Information and Market Efficiency; Returns; Listed Companies

Acknowledgements: We are thankful to Tomáš Havránek for his valuable advice over the project development. We also benefited from helpful comments from Hristos Doucouliagos, Jerome Geyer-Klingeberg, Tom Stanley, and participants at the Thirteenth annual MAER-Net Colloquium (University of Greenwich, 2019). Kočenda acknowledges financial support from the GACR grant No. 20-17044. The usual disclaimer applies.

1. Introduction

Recent in-depth reviews by Amiram et al. (2018) and Liu and Yawson (2020) document a substantial growth of the empirical literature assessing the adverse link between financial crimes and corporate financial performance. This research literature has been fueling regulatory debates on how to enforce financial regulations more efficiently, and specifically on how to deal with financial crimes (La Porta et al., 2006; Jackson and Roe, 2009). The reason is that, amid all corporate crimes, financial crimes trigger the strongest market reactions and subsequently impact corporate reputations severely (Engelen, 2011; Karpoff, 2012 and 2020). For listed firms, the market reaction materializes after a financial crime becomes public and implies that such misconduct should be reflected in the firm's stock prices. In fact, based on the semi-strong efficient market hypothesis, all publicly available information (in this case the financial misconduct(s) of a listed firm) should be fully and immediately incorporated into prices (Fama, 1970). Consequently, when a financial crime of a listed firm becomes public, such firm should experience negative abnormal returns, reflecting the forecasted subsequent cumulated costs (fines, legal fees, compensations, higher costs of doing business, reputational penalty, etc.). Such a market reaction is usually measured with the help of an event study that isolates and quantifies abnormal returns within a specific time interval following public announcement (McKinlay, 1997; Kothari and Warner, 2008). However, such evidence in individual empirical studies (of the abnormal returns following the publication of the financial crime) can be often mixed or less than fully observed (Karpoff et al, 2017). These shortcomings can be surmounted by a quantitative synthesis of the event-studies literature as accentuated by Geyer-Klingeberg et al. (2020). However, to date, no meta-analysis has consolidated, synthesized, and evaluated the empirical findings from event studies assessing whether and to what extent stock markets react to publication of financial misconducts committed by listed firms. In our meta-analysis, we strive to deliver exactly such synthesis.

In line with the literature and with enforcers' practices (such as the American (U.S.) Securities and Exchange Commission (SEC), the French *Autorité des Marchés Financiers* (AMF), or the British (U.K.) Financial Conduct Authority (FCA), we define financial crimes committed by listed firms as the following misconducts: insider trading, price manipulation, dissemination of false information, accounting fraud, and any breach of financial regulation. These misconducts can be alleged, investigated, or sanctioned crimes (see Figure 1). When they are detected, they can lead to regulatory or stock exchange procedures, lawsuits, class actions, or accounting restatements. Once these financial crimes become public, they leave a substantial trace.

The purpose of this study is to systematically and quantitatively synthesize previous empirical results regarding market reactions to intentional financial crimes, specifically when a listed firm (or some of its managers or employees) deliberately cheats on investors. Specifically, we employ a meta-regression analysis (MRA) and investigate the extent and the nature of the effect that materializes on a stock market after intentional financial misconducts become public. Our meta-analysis is unique in that it covers the impacts of the first initial public announcements of financial crimes (either alleged or sanctioned), to the widest possible extent in terms of misconducts, types of enforcement procedures, information canals, and geographic locations by covering all available literature until May 1, 2020. The majority of studies investigate crimes committed in the U.S., given the size of the market and the high regulatory transparency. Still, it is of great interest to put these results into perspective with a wider geographical scope, and for that we also cover European and Asian countries. Meta-analyzing this literature is also a way to challenge the robustness of research on financial misconduct given the pervasiveness of partial observability in research on such misconducts, as developed by Karpoff et al. (2017), and database problems stressed by Amiram et al. (2018).

For our analysis, we surveyed 862 articles published from 1978 to 2020. In the end, we work with a large sample of 439 estimates extracted from 111 articles. The impacts of a total of 31,800 news of financial crimes are estimated, which enlarges considerably the takeaways from individual studies. Despite the richness of this literature, no consensual result can be identified. This is so either regarding the presence of abnormal returns after an intentional financial crime becomes public, or, in terms of magnitude and, to a lesser extent, direction of the stock price reaction. Based on the large number of studies in hand, we ask how important the differences are due to heterogeneity among studies in terms of numerous factors relevant to specific studies. Do the reported impacts of financial misconduct on returns represent the features of the regulatory breach(es)? The data in studies span over a long period – from 1965 to 2018 – and a wide range of financial crimes, at different stages of enforcement. If differences exist in the impact of the stage of the enforcement process (allegation to sanction), are these differences statistically significant? The scope covers 17 countries: most studies cover the U.S., but the data covers also (alphabetically) Australia, Belgium, Canada, China, France, Germany, Japan, Luxembourg, Malaysia, the Netherlands, South Korea, Spain, Sweden, Thailand, Turkey, and the U.K. Hence, in accordance with a general introduction and overview of meta-analysis applications in financial economics (Geyer-Klingeberg et al., 2020), our dataset represents an international sample in terms of market reactions to financial crimes, even though an international evidence is not available at primary study level. Finally, we include articles published in peer-reviewed journals and working papers to investigate the publication selection bias and the sensitiveness of reported effects (abnormal returns) to the research quality. Veld et al. (2018) concluded that articles published in top journals conclude with higher abnormal market reactions than working papers, regarding seasoned equity offerings.

Our meta-analysis is also relevant as it is targeted on the literature that employs one specific methodology: an event study (see Appendix A for details). This means that the studies we survey include a directly available and comparable estimated effect in a form of the abnormal returns due to the financial crime publication, which is crucial for an effective meta-study (Geyer-Klingeberg et al., 2020). The event study methodology, originally outlined in Ball and Brown (1968) and Fama et al. (1969), is widely recognized in the finance and economic literature as an efficient tool to analyze abnormal market reactions to unanticipated news (MacKinlay, 1997). Further, event studies evade the issue of endogeneity and are quite unambiguous with regards to the causal direction of the relationship (Endrikat, 2016). The event study methodology is particularly relevant for the scope of this meta-analysis on financial crime as the event dates are precisely known and are most often communicated *via* official channels, which also facilitates the search for confounding events and their avoidance. The nature of the financial crime news also means that the sample only contains "bad" news that are priced-in more rapidly than good news (Taffler et al., 2004). Additionally, we limit the scope of the surveyed studies to short-term event windows because Kothari and Warner (1997) and Bhagat and Romano (2002a), amid others, raised serious concerns about the specification and explanatory power of an event study with long-term event windows. The key reason is that the noise-to-signal ratio greatly increases as the time distance from the event date becomes larger. In addition, the further from the event, the more likely other confounding events might interfere with the investigated event.

Our contribution to the literature can be summarized in several types of findings that represent the true state of reality assessed *via* a meta-analysis. At first glance, we find that the involvement of a public firm in a financial crime substantially affects the wealth of shareholders quantified as negative abnormal returns over the few days around the event. However, our assessment of the publication selection bias indicates that the collected estimates from the empirical literature are affected by a publication bias, which leads to biased estimates and distorted inferences: negative results are more likely to be published than others. After controlling for this bias, our meta-analysis still evidences informational effect of the intentional financial crimes (statistically and economically significant) but to a lower extent (Karpoff et al., 2017). On average, loss in returns represents minus 1.14% *per* day over the event window following the publication of financial crimes (or a cumulated -3.5% in returns). Our results also indicate that more transparent markets are more responsive to the news of a crime. Accounting fraud also fosters to market corrections. In terms of policy implication, our analysis demonstrates how transparent enforcement actions are priced-in by market participants. Hence, if an enforcer's goal is that markets react to their decisions and communications, then enforcement actions serve as a regulatory tool *per se*.

The rest of the article is structured as follows. We first detail the literature review in section 2, and, based on the analyzed literature, we formulate the hypotheses tested. Information on individual studies constituting the grounds for our analysis is reviewed in the section 3, together with the tools of the meta-analysis used in our study. The assessment of the extent of the publication selection bias and the results of the meta-analysis are presented in section 4. Finally, section 5 concludes and proposes policy-related interpretations.

2. Theoretical and empirical background and hypotheses

2.1 Regulation, enforcement, and deterrence of white-collar crimes

Securities markets are regulated so that all investors, from large institutional to retail investors, have access to quality information about listed firms prior to and after an investment (Black, 2000). The arrangement sets the base for investors' trust. Trust is formed by the *ex-ante* belief that one's counterpart will suffer consequences for opportunistic or fraudulent behavior (Dupont and Karpoff, 2020). Enforcement also aims to provide incentives for market participants' compliance with the law, by detecting breaches, sanctioning violators, and setting example. Violations of securities laws are one of the six possible causes of corporate failures (Soltani, 2010). In that sense, the legal system is fundamental to investors' protection (La Porta et al., 2000).

2.1.1 White-collar crime

Edelhertz (1970; p. 3) defines white-collar crimes as "illegal act(s) or series of illegal acts committed by non-physical means and by concealment or guile, to obtain money or property, to avoid the payment or loss of money or property, or to obtain business or personal advantage". According to Cressey (1950, 1953), three prerequisites can lead to a white-collar crime based on the fraud triangle: 1) a private non-sharable financial problem; 2) contextual opportunities to commit fraud, which would allow the perpetrator to commit the fraud and escape detection; 3) the ability to justify to oneself that the fraudulent actions are not necessarily wrong. Gottschalk (2010) categorizes white-collar crimes into four main forms: fraud, manipulation (on which this article focuses), theft, and corruption. Such crimes can also be classified by victims, as in Karpoff and Lott (1993): 1) fraud of stakeholders (by cheating on implicit or explicit contracts with suppliers, employees, franchisees, or customers); 2) fraud of government (by cheating on contracts with a government agency); 3) financial reporting fraud (by mispresenting the firm's financial condition); and 4) regulatory violations (by violating regulations enforced by federal agencies, mostly financial services agencies). The scope of this study is limited to the last two points, as long as they fall under the scope of supervision of securities market supervisors or central banks, depending on the jurisdictions.

Several specific features of white-collar crimes provide further support for the relevance of our study. Firstly, contrary to many other crimes, white-collar crimes are committed by employees and not by the companies. Still, most frequently, the firms are held responsible, rather than the employees themselves (Choi and Pritchard, 2016), justifying market corrections after a misconduct become public. Secondly, and echoing Becker (1968),[1] a limited share of white-collar crimes is detected (by regulators, analysts, shareholders, stockholders, etc.), with an unknown probability. Alawadhi et al. (2020) assess that only 3.5% of financial mis-presentations are eventually caught and sanctioned. Consequently, Amiram et al. (2018; p. 738) conclude that "our knowledge of financial misconduct comes almost exclusively from firms that were caught, and the characteristics of those firms may differ from firms that commit fraud without detection." This partial observability makes it consequently relevant to enlarge the scope of research by meta-analyzing the existing literature to confirm the relevance of the conclusions of individual studies. Thirdly, corporate frauds can be detected *via* several channels: through the typical corporate governance players (regulators, external auditors, financial analysts) as well as a large network of people interacting with the firms (shareholders, stakeholders, employees, journalists, etc.). The specific channel of detection may impact the subsequent spillovers of the fraud. Finally, acting legally can turn into an economic disadvantage for a firm and/or its management (Hawley, 1991, Aupperle et al., 1985). In fact, the costs for abiding by the law can represent an economic disadvantage when compared to competitors/peers. To state alternatively, and echoing Becker (1968), the expected costs for being sanctioned (fines, litigation costs, reputational penalties, impact on clients and suppliers, HR consequences) can be lower than the benefits from cheating the law (higher returns on assets, lower costs of doing business, etc.).

[1] Becker (1968) models the choice to engage in misbehavior like any other decision involving cost-benefit tradeoffs, in light of the expected profits from fraud, the probability of being caught, and the subsequent sanction.

All in all, it is particularly relevant to enlarge the scope of past studies by meta-analyzing the existing literature to draw more general conclusions on market reactions to white-collar crimes, which occur after such crimes become public (see Figure 2).

2.1.2 Public *versus* private enforcement

Enforcement is always country-specific and can be characterized by various dimensions (see Table 1 for some stylized facts). Similarly, enforcement differs along time and across jurisdictions. Each country has its own enforcement mix, with the different weights given to public (higher in civil law countries) and private (conversely higher in common law countries, typically the U.S.) enforcement, and by difference to self-regulation of the market (Djankov et al., 2008). Financial regulations can be enforced by either several bodies (for example at the federal, province or state levels or depending on the sector with splits between banks, insurance companies, etc.) or one single financial supervisory agency. Enforcement can also rely more on informal discussions and administrative guidance (such as in the U.K., Japan, and France) or on formal legal actions against wrongdoers (like in the U.S.).

A long-time academic debate – at the intersection between accounting, finance, law, and economics – investigates the costs and benefits of public *versus* private enforcements, with proponents on both sides. Both enforcement styles could be more supportive of financial market development (respectively Jackson and Roe (2009) and Johnston and Petacchi (2017) against Becker and Stigler (1974), La Porta et al. (2006), Djankov et al. (2008), and Bai et al. (2010)).

Public enforcement is supported by the existence of externalities, by economy-wide cost savings, by public-regarding and expert-in-their-domains policymakers, by the possibility to cooperate with defendants (Choi and Pritchard, 2016), and by criminal, financial, and reputational penalties that deter wrongdoings. But public enforcement is degraded by the difficulties of implementation of securities regulations. Public enforcers have mixed-to-low incentives (Scholz, 1984): resource constraints, difficult access to information, low competences compared to the industry, corruption and collusion with the industry, and political influence. Conversely, private enforcement actions could be brought by well-informed actors with well-aligned incentives. But, in parallel, private enforcement is subject to collective action and free rider effects among dispersed investors, to slow and inept judiciaries, to lawyers' rent-seeking (costly litigation for investors, commitment problems), to less information than enforcers (Choi and Pritchard, 2016), and to insufficient private monetary penalties.

Our analysis also contributes to the academic debate related to financial crimes of whether markets significantly discriminate between public and private enforcement.

Additionally, given the long timespan of the dataset and the global trend towards regulatory tightening, it is interesting to investigate market reactions along time. Between 1965 and 2018, information channels and quantity of news dramatically increased, to a point that more and more research investigates the consequences of information overload (Ripken, 2006). Is there a time-factor (or a long-rum trend) in market reactions to the publication of financial crimes?

2.2 Intentional financial crimes, not errors

The scope of our analysis on white-collar crimes is limited to violations of securities laws (referred to as "financial crimes"). This scope is supported by the argument of Haslem et al. (2017) that, amid all types of legal corporate violations in the U.S.,[2] securities litigation triggers – by far – the largest (and statistically significant) reactions. Amiram et al. (2018) also stress that financial crimes threaten the existence and efficiency of capital markets, which are based on trust from diverse market participants (investors, stakeholders, financial analysts, etc.). Such crimes cover a wide range of misconducts: financial statement errors,[3] price manipulation (circular trading, reference price influence, improper order handling, boiler-room operation), insider dealing (collusion and information sharing, use of insider information), and dissemination of false/misleading information, etc. Financial crimes can be motivated by the pressure to meet financial targets, the dishonesty of the management, or the search to maximize personal gain (for example, to protect bonuses). When detected, financial crimes can lead to major corrective actions: changes in the financing mix due to higher cost of doing business, changes in the top management, impact on remunerations and teams' commitment, replacement of auditing firms, etc.

The literature (Guy and Pany, 1997; Karpoff et al., 2017; Liu and Yawson, 2020) typically splits financial misconducts between "serious" (*e.g.*, frauds) and "trivial" (*e.g.*, errors). All securities frauds share a common trait: the existence of deliberate or "intentional"

[2] The others being: antitrust, contract, environmental, intellectual property, labor, product liability, personal injury, and civil rights.

[3] The Financial Accounting Standards Board (FASB) in the Statement of Financial Accounting Standards n°154, "Accounting Changes and Error Corrections" (2005) defines errors in previously issued financial statements as "an error in recognition, measurement, presentation, or disclosure in financial statements resulting from mathematical mistakes, mistakes in the application of the Generally Accepted Accounting Principles (GAAP), or oversight or misuse of facts that existed at the time of the financial statements were prepared". Accounting frauds are distinct from aggressive earning management (Desai et al., 2006; Blythe, 2020) propose the following taxonomy of financial statement frauds: 1) falsification, alternation or manipulation of financial records, related documents or business transactions; 2) intentional omissions or misrepresentations of events, transactions, accounts or other information from which financial statements are prepared; 3) deliberate misapplication of accounting principles, policies and procedures used to measure, recognize, report and disclose economic events and business transactions; and 4) intentional omissions of disclosures or presentation of inadequate disclosures pertaining to accounting principles and policies and related financial amounts.

dishonesty or deceit (Sievers and Sofilkanitsch, 2019), which would cause market participants (shareholders, stakeholders, analysts, etc.) to alter their opinion of the firm. Otherwise, they are unintentional errors, which can be corrected (and possibly sanctioned). Such errors can result from new accounting standards (IFRS, U.S. GAAP for example), a new consolidation perimeter (in the aftermath of stock splits, M&As, or divestitures for example), or presentation issues (due to changes of the accounting periods or changes in business segment definitions for example). Hennes et al. (2008) found that 24% of the restatements in the U.S. filed between 2002 and 2005 were intentional frauds, and not errors.

The scope of this analysis is limited to intentional financial crimes (see Figure 3 for a graphical illustration of the scope of the sample), as unintentional errors are unlikely to send a comparable message to the market (Hennes et al., 2008).[4] Lev et al. (2007) demonstrated that restatements involving admitted fraud have considerably more adverse implications for investors than non-fraud restatements.

2.3 Event studies to assess market reactions to the news of financial crimes

The empirical literature typically uses three different methodologies to investigate the spillovers of corporate crimes on corporate financial performance: portfolio analyses, multivariate analyses, and event studies. Our meta-analysis focusses on the event studies, which have proven to be particularly adequate in policy analysis (Fama, 1990; Bhagat and Romano, 2002a, b).

The event study methodology (see appendix A for details) estimates firm-specific movements in security prices (so called abnormal returns) after an unexpected event. The price movements are corrected for recent trends of both the given security and the market. Stock prices reflect the time- and risk-discounted present value of all future expected cashflows for shareholders. Under the semi-strong efficient market hypothesis (Fama, 1970), all publicly-available information (such as a financial crime) is reflected completely and in an unbiased manner in the stock price, such that it is not possible to earn economic profits on the basis of this information. Hence, event studies provide a way for measuring the impact of financial crimes on investors' wealth.

2.4 Spillovers of financial crimes: does it cost to be bad?

A rich literature documents the cost of crime. Shareholders are harmed by the (alleged or sanctioned) misconduct itself and also by the subsequent costs, despite financial misconducts

[4] For example, the authors excluded articles on earning restatements when "fraud", "irregularity", or "investigation" were not mentioned.

being perpetrated by managers. These costs of financial crimes are direct and indirect. The direct costs cover the fines, the compensations, and the legal fees along years-long procedures (Dechow et al., 1996; Palmrose et al., 2004). The indirect costs (Zeidan, 2013; Gatzert, 2015) include lower cash flows expectations (clients) and higher costs of doing business (suppliers, business partners, human resource management) and of capital (downgraded forecasts, risk premia, rating, higher funding costs). The cost of cumulated indirect spillovers can be called "reputational penalty", as described by Engelen and van Essen (2011). The reputational penalty can be proxied by deducting the direct costs from the abnormal market reactions following the publication of the financial crime, estimated with an event study methodology (Cummins et al., 2006; Karpoff et al., 2008; Armour et al., 2017). For allegations of financial crimes, listed firms can endure a pure reputational penalty proportional to the alleged crime. Generally, it reflects revised expectations regarding future cash flows of investors, top management and related parties involved (Karpoff et al., 2008; Armour et al., 2017). In that sense, financial markets are an enforcement canal to induce companies to behave responsibly (Engelen, 2011). Reputational penalties complement enforcement as a tool to deter financial crimes, contrary to, for example, foreign bribery or environmental violations (Karpoff, 2012, 2020).

The spillovers of intentional financial crimes are detailed in the literature which concludes that legal penalties only account for a limited part of the overall market-based consequences incurred for the public firms (Karpoff and Lott, 1993; Alexander, 1999; Karpoff et al., 2005; Murphy et al., 2009; Engelen, 2011; Haslem et al., 2017; Karpoff et al., 2017; Armour et al., 2017). In addition, markets can anticipate the news, following leaks of information over the days preceding the event for example (Bhagat et al., 1994; Pritchard and Ferris, 2001; Djama, 2013; Gande and Lewis, 2009; Dyck et al., 2010; Nainar et al., 2014; Haslem et al., 2017; Armour et al., 2017; de Batz, 2020). As explained by Bhagat et al. (2002b), when information leaks before its public announcement by the regulator or the firm, the event study will understate the damages due to the fraud publication, because part of the impact of the information was already incorporated before its announcement. Extending the event window before the event date controls for possible anticipations or information leaks.

Reactions to financial crimes can differ between countries and regions. Firstly, commercial laws of most countries can be divided between common and code laws (see Table 1, as in Djankov et al., 2008), or by geographical origin (English, French, German, Scandinavian, or socialist), which spread worldwide along history (colonization, wars, voluntary transplantations, etc.). According to La Porta et al. (2006), common laws (typically in the U.S. or the U.K.) are more favorable to stock market development: they put more emphasis on private contracting and standardized disclosure as well as rely on private dispute resolution using market-friendly standards of liability. Secondly, legal origins influence public and private enforcement and, consequently, the outcomes of the publication of financial crimes. Enforcers and regulated entities diverge in terms of disclosure (along the procedures) and liability standards. The literature investigates market reactions to alleged or condemned financial crimes, along the consecutive steps of enforcement (see Figure 1). A fraud can be alleged by newspaper articles or by an official corporate or regulatory communication (see Figure 2 for a graphical illustration). The very first hint of financial crime could trigger the most important and significant abnormal market reaction, even when compared to the sanction publication itself, as demonstrated by Feroz et al. (1991) regarding the U.S. Securities Exchange Commission (SEC) investigations of violations of accounting laws or Pritchard and Ferris (2001) regarding the publication of potential securities frauds followed by the class action filings. Solomon and Soltes (2019; p. 1) stress the difference between "not guilty" and "innocent" for the markets: "even when no charges are ultimately brought [after SEC financial fraud investigations], firms that voluntarily disclose an investigation have significant negative returns, underperforming non-sanctioned firms that stayed silent by 12.7% for a year after the investigation begins."

Additionally, to date, the great majority of the literature on the spillovers of financial crimes investigates the U.S., due to the size of the market, and the higher data availability (along the enforcement process). By using the largest possible scope of results, a meta-analysis can challenge whether patterns observed in the U.S. can be generalized to other regions and jurisdictions. This is all more relevant that Parsons et al. (2018) stress how market reactions to financial crimes can even differ within a given country by comparing major U.S. cities (up to a factor of 3), due to different social attitudes towards right and wrong across cities. This supports for controlling as well for the level of economic and financial development. Shleifer (2005; p. 448) stressed that "regulation – relative to doing nothing – is a more attractive option in richer countries, where the checks on the government are stronger. In contrast, regulation is a particularly poor idea in undemocratic countries and in countries with extremely powerful executives, where the risks of abuse are the greatest." Karpoff et al. (2017) demonstrate how it can even be difficult to compare events (in particular the causes and effects of financial misconducts) within a given country, depending on the datasets used.

Different information channels of financial crimes may influence market reactions. The media coverage of financial crimes typically increases significantly market reactions: the more articles, the stronger markets react (Feroz et al., 1991; Karpoff and Lot, 1993; Nourayi, 1994;

Miller, 2006; Choi and Kahan, 2007; Barber and Odean, 2008; Fang and Peress, 2009; Tibbs et al., 2011; Fang et al., 2014; Peress, 2014). The business media can even be perceived by investors as a watchdog (Miller, 2006), which credibility is supported by more independent sources of information than analysts and corporations (Kothari et al., 2009). Otherwise, enforcers or the defendant firm itself can reveal a financial crime. Still, Karpoff et al. (2017) stressed that all empirical proxies of securities frauds have shortages from newspaper articles reporting frauds, such as the Wall Street Journal typically does in the U.S. (*i.e.* excluding lower-profile crimes which to not grab the media attention), to broader proxies such as public or regulatory datasets compiling all financial reporting errors, securities litigations, or enforcement procedures datasets.

Nonetheless, the net impact of enforcement actions on the market remains –to some extent– controversial (Christensen et al., 2016). Morris et al. (2018; p. 318) stress that "theory suggests that regulator action may result in limited or no benefits, and the empirical evidence to this effect is mixed. If the investigations make investors more concerned about internal problems or future prospects, market quality should deteriorate. However, the SEC's investigation can be an opportunity for the firm to correct internal problems and bad behaviors. Market participants may then respond positively during the investigation thereby revising forecasts to the upside." Christensen et al. (2016) empirically validate the "no-effect" hypothesis of SEC enforcement actions on market quality, presented by Stigler (1964) and Peltzman (1976). Amiram et al. (2018) challenge the rational for the monetary payments by the defendant firm to either the regulators (public enforcement) or to the plaintiffs (private enforcement).

All in all, this meta-analysis aims at systematically understanding the market-imposed sanctions following the publication of intentional financial crimes. Contrary to financial fines which can be observed (if not anonymized), these market abnormal returns are comprised of the legal (if any) and extra-legal (or reputational) sanctions. Hence, they stand for a holistic view of the overall market penalties for financial crimes. This can contribute to regulatory debates on how to come closer to an optimal level of regulation, to deter future crimes. The recent shift towards the "name and shame" mechanism (for accounting standards enforcement in particular in the U.S., Germany, and the U.K.) participates in the same dynamic: it implicitly assumes that investors will react negatively to published findings of erroneous accounting treatments, hence penalizing the firms and incentivizing their peers not to infringe the law.

2.5 Hypotheses of the meta-analysis

In light of the literature review, the following hypotheses are tested using a meta-analysis approach. They all challenge the investors' rationality and the efficiency of financial markets.

Hypothesis 1.1. Markets penalize listed firms for engaging in intentional financial crimes. The initial disclosure of frauds results in significant negative abnormal returns.

Hypothesis 1.2. Published articles suffer from a publication bias, towards negative market reactions to financial crimes.[5]

Hypothesis 1.3. Markets anticipate the events, possibly due to leaks of information.

Hypothesis 2. Public and private enforcements trigger different market reactions.

Hypothesis 3. Markets differentiate between intentional financial crimes (pure accounting frauds, pure violations of securities laws, or both).

Hypothesis 4. Markets account for the suspicion (alleged crimes, lawsuit filings) or for the condemnation of financial crimes (settlements, regulatory sanctions, lawsuits verdicts, and accounting restatements).

Hypothesis 5. Markets differentiate depending on the source of information of the financial crime (media, regulatory, or corporate).

Hypothesis 6. Common law countries (the U.S. in particular), being more transparent, trigger stronger market reactions to financial crimes than in other jurisdictions.

Hypothesis 7. Markets tend to react more along time to the news of intentional financial crimes.

3. Data and methodology

In this section, we describe our procedure for selecting the literature and give an overview of the studies selected for the meta-analysis, complemented by a description of funnel plots. Then, we briefly explain the meta-analysis methodology to be conducted in this paper, based on Havránek et al. (2020), and describe the explanatory variables.

3.1 Selection of the data

We selected the studies chosen by a systematic keyword search that was performed in Google Scholar, which presents the advantage of going through the full text of studies and not only titles, abstracts, or keywords. The search was complemented through other major economic

[5] We expect published studies to be of higher quality on average and to contain fewer typos and mistakes in reporting their results. Still, the inclusion of unpublished papers is unlikely to alleviate publication bias (Rusnák et al., 2013): rational authors draw their conclusions with the intention to publish, by adopting the same preferences as journals. Doucouliagos and Stanley (2013) meta-analyzed 87 meta-analyses and suggest no difference in the magnitude of publication bias between published and unpublished studies.

databases such as JSTOR, Econlit, Science Direct, RepEc (IDEAS), NBER, CEPR, and SSRN. The authors searched for the specific topics related to financial crime and its punishment *via* combinations consisting of relevant keywords including one of the following: *financial crime*, *regulatory breach*, *misconduct*, *fraud*, *sanction*, *penalty*, *class action*, *restatement*, or *lawsuit*, and another one from: *firms*, *financial market*, *event study*, *return*, or *abnormal*. We examined the first 500 papers returned by the searches in Google Scholar.

After this first selection of papers relevant to our study, we systematically inspected the lists of references in these studies and their Google Scholar citations, to check if we can find usable studies not captured by our baseline search. No *a priori* filter was used concerning the date or type of publication. This procedure further increased the number of potential studies. We terminated the search on May 1st, 2020 and did not add any new studies beyond that date. In total, 862 articles were reviewed and analyzed.[6]

In order to obtain our final set of literature, we followed an iterative process of selecting articles that is graphically illustrated by the PRISMA statement in Figure 4, as recommended by Havránek et al. (2020).[7] We form our dataset from studies that strictly satisfy the following six conditions in that they must: 1) use a daily event study methodology; 2) analyze market reactions to (possibly alleged) intentional financial crimes (see Figure 3 for a graphical illustration of the scope of the sample); 3) specify the first public announcement reporting of the (possibly alleged) financial crime, whatever the source of information (newspaper, regulatory or corporate communication, see Figure 2); 4) report (Cumulative) Average Abnormal Returns ($(C)AARs$) and at least an explicit indication of statistical significance (*t*-statistics, *p*-values, *z*-statistic, and/or a significance level (1%, 5%, or 10%)), to calculate standard errors; 5) use short-term event windows. As recommended in Hubler et al. (2019), the dataset is comprised of all short-term $(C)AARs$ reported in each study. We used event windows around the event, centered on $t = 0$, ranging from 10 trading days before the financial crime until 10 trading days after it ($[-10; +10]$, *i.e.* two business weeks before and after the event); and 6) not be master or PhD theses (working papers are included).

Consequently, the most frequent reasons for excluding the selected studies were the following: 1) event studies out of our scope on (partly) unintentional financial crimes, other

[6] We tried to circumvent the fact that language issues can act as a constraint on the scope of meta-analyses. We extended searches to the following languages: English, French, German, Portuguese, and Spanish. Some articles in Chinese, Japanese, and Turkish could not be included in the literature review, though appearing relevant in view of their references. Still, as stressed by Reurink (2018), the representativeness of the presented findings remains skewed heavily towards the Anglo-Saxon world.
[7] Preferred Reporting Items for Systematic Reviews and Meta-Analyses.

corporate scandals or crimes, impact of regulatory changes, impact of financial crimes when committed, over the fraud period (see Figure 2), spillovers of financial crimes on sector peers, or too specific financial crimes (case studies such as Enron or the U.S. stock option backdating scandal in 2006), 2) methodological problems,[8] 3) theoretical articles on financial crimes (models or literature reviews), and 4) experimental articles on financial misconducts.

At the end of our selection process, we formed a set of 111 studies that provide a total of 349 estimates. Out of these studies, 90 were published in academic journals (81%) and the rest are working papers, colloquium proceedings, or chapters of a collective publications. For each study, the complete reference can be found in the Appendix B, and Table 2 describes their main features.

3.2 Descriptive statistics

We employ a meta-regression analysis to examine how and to what extent the publication of intentional financial crimes committed by listed firms impacts stock markets (*i.e.* their returns).

Following Stanley and Doucouliagos (2012) and Hubler et al. (2019), we extract all short-term $AARs$ and $CAARs$ included in the 111 articles in the scope, specifying the event windows (from -10 to +10 days around the event occurring in $t=0$). We obtain a total of 439 effect estimates from 31,800 news of intentional financial crimes committed by listed firms. Including event windows preceding the events controls for market anticipations of the news, resulting from potential corporate or regulatory leaks of information. Complementarily, having 10 trading days after the event controls for the time persistency of the impact and some market inefficiencies, if the reaction is not full and immediate (Fama, 1970). The goal by using all $(C)AARDs$ from the articles is to get as many estimates as possible to account for the variability found across the different studies and between estimates, without introducing potential selection bias, and to properly weight the reported findings. However, it does result in potential interdependence between studies, that must be accommodated by systematically clustering the dataset by studies. $(C)AARDs$ are comparable between articles as, by construction, they all use the same event study methodology (see Appendix 1 for methodological details). Two main methodological differences between (and within) studies are the length of the event windows

[8] For example, higher-than-daily frequencies (weekly, monthly, quarterly, yearly) with usually a longer-term perspective (longer event windows), unpublished estimates of event studies or statistical significance indication, estimations of the costs of financial crimes with other methodologies than event studies (difference in difference with a sector perspective, no specification on the methodology used), and event studies on other variables than returns (volatility, volume of trades, spreads, interest rates, bonds, ratings, bank loans, systemic risk, sales, top management with equity compensation, wages, bonuses, careers, etc.).

and the type of model used to estimate abnormal returns, resulting from an authors' *ad hoc* decision. Contrary to the model used (83% of the articles using a market model), event windows vary significantly across studies (see Table 3). In both the studies under review and the reported estimates, the average lengths are respectively 36 and 4 days, with standard deviations of 83 and 4 days. In fact, there is no standardized way of presenting the results, even though the event day ($t=0$) is at least included in the reported event windows. Consequently, we normalized all $CAARs$ by the length of their respective event windows. We created the following variable to capture the effect of the crime publication: Average Abnormal Return *per* Day ($AARD$), equal to $CAAR$ divided by the length of the event window ($length\_ev\_w\_est$) or to AAR, for one day event windows.[9] $AARDs$ were also winsorized at the 1% level, to ensure that the presence of outliers does not result from mistakes in the original articles.

Event studies typically use hypothesis tests to see if abnormal returns around the event day are statistically significant. Conventionally, the null hypothesis is that $(C)AARs$ equal zero. To test the statistical significance of the abnormal returns, the great majority of studies in the sample use Student's *t*-test (and to a lesser extent *z*-statistics and *p*-values), most frequently with little (or no) information on how the test was run. In some cases, the results of non-parametric tests are also included. The parametric *t*-tests (or statistical significance levels) are provided by the primary studies themselves, under the assumption that the underlying source population is normally distributed. This assumption is never discussed in the literature, as most samples are larger than 30 (293 on average, with a standard deviation of 410). By construction, our sample includes at least a level of significance (1%, 5% or 10%). As done by Frooman (1997), when the *t*-statistics were not published, conservative *t*-statistics (or a worst-case scenario) were obtained as follows: 1) the statistical significance levels were converted into conservative levels of significance;[10] 2) the *z*-statistics were directly changed into *t*-statistics, on the assumption that as sample size increases, the Student's *t* distribution approaches the normal distribution (Marascuilo and Serlin, 1988); and 3) the *p*-values were converted into *t*-statistics by using a *t*-table and the appropriate degrees of freedom. Finally, three studies (Desai et ai. (2006), Nelson et al. (2009), and Goldman et al. (2012)), standing for 7 estimates, mentioned explicitly that the abnormal returns are significant, without including *t*-statistics or the statistical significance. We made the conservative hypothesis that the statistical significance

[9] The $(C)AARDs$ could not be standardized by their standard deviations (Frooman, 1997) as only few event studies report them. Similarly to our $AARDs$, Veld et al. (2018) normalized the reported $CAARs$ by dividing them by the number of days in the event window and included dummy variables for observations with different event windows.
[10] 10% to t = -1.645; 5% to t = -1.96; 1% to t = -2.576; etc.

level was 10% for each. (Conservative) Standard errors were calculated from the conservative t-statistics and the $AARDs$, when they were not included in the study.[11] Standard errors were also winsorized at the 1% level.

Four sets of variables are included in the dataset. Detailed definitions of the variables and their major descriptive statistics are displayed in Table 3, both for the whole sample and for the sub-samples of alleged and condemned financial crimes. More detailed information, or the whole dataset, is available on demand. The scope covers the respective characteristics of the data, the estimation, and the publication, complemented with some control variables (legal, financial, and sectorial characteristics). The articles were split into sub-samples depending on several dimensions, echoing the hypotheses tested: the event windows of the $(C)AARDs$ for hypothesis 1.3, as done in the meta-analysis of Hubler et al. (2019) (strictly before the event (14% of the sample), on the event day (19%), around the event (62% of the sample), and strictly after the event (6% of the sample)); the types of procedure for hypothesis 2 (public or private enforcement procedures); the types of financial crimes for hypothesis 3 (pure accounting frauds, pure violations of securities laws, or both); the steps of the enforcement for hypothesis 4 (alleged financial crimes, *i.e.* revealed by the press or by the ignition of a public or private enforcement procedure, or condemned crimes, in the sense that the guilt of the firm is publicly acknowledged following the verdict of a regulatory procedure, private lawsuit, or class action, or the publication of accounting restatements); the sources of information for hypothesis 5 (newspaper articles *versus* corporate or regulatory information); and the types of laws of the jurisdiction(s) under review for hypothesis 6 (common *versus* civil laws). As in Leuz et al. (2003) and Liang and Renneboog (2017), we assume that the type of commercial laws (common or code) is predetermined and exogenous to our analysis as the legal frameworks were set centuries ago *via* complex interactions (wars, occupations, colonization, amongst others). It is noteworthy that common law countries (and in particular the U.S.) are more transparent along enforcement or legal procedures. Therefore, they stand for a higher share of "alleged crimes" than condemned ones. Complementarily, the average year of sampled data is included to control for the trend in market reactions (hypothesis 7).

Overall, the results compiled from the sample of 111 studies vary. Most frequently, studies investigating the spillovers of financial crime on returns report negative impacts (statistically significant for 73% of the sample or insignificant for 17% of the sample). Conversely, 2% are positive and significant and 8% positive and insignificant. The addendum

[11] Only for 2 studies published standard errors of $(C)AARs$, standing for 10 estimates or 2% of the sample of estimates.

of Table 3 compared different averages of $AARDs$ (not winsorized). Depending on the weights, they range from -1.9% down to -3.3% *per* day over the event window. Table 3 hints that markets would react twice as much to alleged frauds thanks to condemned financial crimes (hypothesis 4). The average sample size (degrees of freedom) is large (293), supporting the significance of the results. The average estimation window covers the period [-153; -21] before the event and the event window [-17; +19] (27% of the articles have event windows beyond the short-term window we focus on, [-10; +10]). More precisely, for the reported $AARDs$, the average event window is [-1.7; +1.4]. 67% of the event studies are complemented with a cross-sectional regression and 12% by an estimation of a subsequent reputational penalty.

The events included in the articles occurred on average between 1994 and 2004 (ranging from 1965 to 2018). Figure 5 depicts graphically the chronological ordering of $AARDs$ for the whole sample, based on the average year of the sample data, ranging from 1973 to 2016. It compares raw data, as extracted from the sampled articles, with winsorized $AARDs$, at the 1% level. Both samples trend upwards along time, hinting that markets tend to react less along time (hypothesis 7).

Finally, the average publication year of the article is 2009 (ranging from 1984 until 2020), most frequently in cross-disciplinary and refereed journals and authored by 2.4 researchers. A third of the latter authored more than one article out of the 111-article sample. In fact, Amiram et al. (2018) stated that studies on financial misconduct belong to three perspectives: law, accounting, or finance (for our sample, by declining order of importance: finance, accounting, business, and law). Articles on condemned crimes appear to be published in better journals and to get higher attention; these features are measured, for example, by the Scopus cite score, by the RePec discounted impact factor, and by the number of Google citations.

3.3 Funnel plots and publication selection bias

We construct funnel plots to investigate the distribution of the reported estimates and a potential publication selection bias (Stanley and Doucouliagos, 2010). This scatter diagram plots the size of the estimated effect ($AARDs$) on the horizontal axis against a measure of the estimate's precision on the vertical axis (the inverse of the conservative estimated standard errors of the $AARDs$). A publication selection bias means that submitted and published manuscripts (*i.e.* the combined actions of researchers, reviewers, and editors) are biased in a direction or in the strength of the study findings (Stanley, 2005). In the absence of publication selection, effect sizes reported by independent studies vary randomly and symmetrically around the "true" value of the effect (Stanley and Doucouliagos, 2012). Additionally, the dispersion of effect sizes should be negatively correlated with the precision of the estimate.

Figure 6 depicts funnel plots of the $AARDs$ for the full 439 sample of financial crimes and for sub-samples, echoing the set of tested hypotheses (except hypothesis 7). All funnel graphs are asymmetrical, skewed to the left (towards negative $AARDs$). They suggest a publication selection bias, under the assumption of a "true" effect holding for the whole sample regardless of the studies' specificities. This skew could indicate a preference in the literature for reporting negative abnormal returns after the announcement of intentional financial crimes committed by listed firms. As recommended by Stanley and Doucouliagos (2012), this hypothesis of publication bias is further investigated, with a meta-regression analysis, to address heterogeneity across studies (time, countries, breaches, procedures, etc.).

3.4 Meta-Regression Analysis (MRA) methodology

We perform a two-step MRA, to explore the publication selection bias demonstrated by the funnel plots and to investigate for the factors causing heterogeneity between the studies in the sample, as in Stanley and Doucouliagos (2012), Askarov and Doucouliagos (2013), and Hubler et al. (2019).

Firstly, we test the presence of a publication selection bias with the Funnel-Asymmetry Test (FAT) and proxy the true impact of the publication of financial crimes on returns with a Precision-Effect Test (PET), as recommended by Stanley (2005) and Stanley and Doucouliagos (2012). Equation (1) is estimated:

$$AARD_{i,j} = \beta_0 + \beta_1 SE_{i,j} + \varepsilon_{i,j}, \quad (1)$$

Where $AARDs$ are the average abnormal returns *per* day (*i.e.* the reported effect), $SE_{i,j}$ are the conservative standard errors of the $AARDs$, β_0 and β_1 are the parameters to be estimated, i and j denote the i^{th} estimate from the j^{th} study ($j \in [\![1; 111]\!]$), and ε are the residuals. A publication selection bias (FAT) is demonstrated by a statistically significant correlation between the reported effects and their standard errors ($\beta_1 \neq 0$), resulting in an asymmetrical funnel plot as previously described. The estimates of β_0 (PET) stand for an unconditional measure of the genuine empirical effect of the publication of financial crimes on the returns of the involved listed firms, corrected for any publication selection bias (Stanley and Doucouliagos, 2012). To estimate Eq. (1), we use the cluster-robust Ordinary Least Squares (OLS), the cluster-robust Weighted Least Squares (WLS), by weighting by squared precision to correct for heteroskedasticity and to account for the quality, the cluster-robust random-effects estimator, and the cluster-robust fixed-effects estimator.

Complementarily, Stanley and Doucouliagos (2012) argue that, when there is an effect, an estimate of the "true" publication-selection-bias-adjusted effect size can be better estimated with the Precision-Effect Estimate with Standard Error (PEESE) approach. They use the estimation of the following equation (which has no intercept) to obtain the coefficient β_0:

$$t\_stat\_wcs_{i,j} = \beta_1 SE_{i,j} + \beta_0\left(1/SE_{i,j}\right) + \varepsilon_{i,j}, \quad (2)$$

Where t_stat_wcs are the estimated (conservative) t-statistics and SE are the conservative standard errors of the $AARDs$. Eq. (2) means that, if the null hypothesis of $\beta_0 = 0$ is rejected, the non-zero effect does actually exist in the literature, and the coefficient β_0 can be regarded as its estimate. To test the robustness of the regression coefficient, we estimate Eq. (2) using not only the cluster-robust OLS and WLS estimators, but also the unbalanced panel estimators, that treat possible heterogeneity among the studies.[12]

Secondly, the generic MRA equation (Eq. (3)), embedding the FAT-PET, is estimated to investigate for and quantify the factors contributing to the heterogeneity of the $AARDs$ results of the sample. The null hypothesis is that the factors related to specific studies are not relevant to the reported outcomes. The following model is estimated:

$$AARD_{i,j} = \beta_0 + \beta_1 SE_{i,j} + \sum \beta_k Z_{i,k} + \varepsilon_{i,j}, \quad (3)$$

Where $AARDs$ are the average abnormal returns *per* day, SE are the conservative standard errors of the $AARDs$, Z is a vector of meta-independent explanatory variables that captures the relevant characteristics of an empirical study and explains its systematic variation from other empirical results in the literature, β_i are the meta-regression coefficient to be estimated, i and j denote the i^{th} estimate from the j^{th} study ($j \in [\![1; 111]\!]$), and ε are the meta-regression disturbance terms (Stanley and Jarrell, 2005). Eq. (3) enables identifying and quantifying the factors that create heterogeneity in the reported estimates, with the vector Z of explanatory variables. Finally, as in Askarov and Doucouliagos (2013), the MRA coefficients are used to calculate the meta-average effect of the publication of intentional financial crimes on returns. The meta-average is the best estimate of the effect of financial crimes as reported by the extant literature, under the following hypotheses: the MRA variables actually quantify the effect of misspecification bias and some MRA variables must be chosen to predict the average effect.

[12] We report the random-effects model estimated by the maximum likelihood (ML) method and the population-averaged generalized estimating equation (GEE) model.

3.5 Definition of the explanatory variables

We build three sets of variables to account for the genuine heterogeneity between studies and the heterogeneity introduced by primary authors' choices (see Table 3) covering the characteristics of the data, of the event study estimation, and of the publication of the study. We also included control variables.

The major features of the data build on the literature of meta-analyses (Havránek et al., 2020). They characterize the major divergences between sampled articles, ranging from the geographical specificities (country(ies) under review, geography, and legal origin) to time specificities (the average year over which crimes where analyzed, and the length of this period under review). Complementarily, they cover the types of financial crimes (pure accounting crime or any violation of securities laws), the media of publication of the financial crime (newspaper articles, corporate or regulatory communication), and the specificities of the enforcement procedures (alleged or condemned crimes; regulatory procedures, private enforcement procedures or accounting restatements). Finally, dummy variables control for the most frequent industrial sectors under review (industry or finance).

The estimation characteristics cover the main possible divergences in event study methodology application (see Appendix A for details): whether the model used to estimate abnormal returns is a market model or not; the characteristics of the estimation and event windows; the sample size (after excluding unintentional crimes and the confounding events in particular);[13] the length of the event window of the estimated $(C)AAR$ (1 day for AAR and 2 to 21 days for $CAAR$, given the limit put on the reported short term estimates); whether abnormal returns were also estimated for longer event windows than $[-10;+10]$; whether an estimation was done for the event day, when the financial crime is revealed (*i.e.* $AAR(0)$); whether the estimation was done around the event (*i.e.* $CAAR[-x;+y]$, with $t=0$ being the event day); and finally if the event study was complemented with cross-sectional regressions and/or reputational penalty estimations.

The publication characteristics used are relevant for a meta-analysis and correspond to those highlighted by Geyer-Klingeberg et at. (2020): the number of authors of the article; if authors were named several times in the sample under review, as a way to assess the level of expertise of the authors of the article; the year and the month of publication; the length of the article; indicators of the quality of the article, of which whether or not the article was published

[13] Contrary to Hubler et al. (2019), we did not include a dummy for the exclusion of confounding events, as it is a prerequisite to building a credible dataset for an event studies, purged from any other significant confounding event.

in a refereed journal (and not a working paper), the journal impact factor, and the number of citations of the article recorded in Google Scholar; and whether the article was published in a cross-disciplinary journal (which could increase the echo of the findings).

Finally, we control for exogenous variables, which are not explicitly accounted for by the authors but can be potential sources of variability in the $AARDs$. In fact, the sample covers a wide range of countries (17) with developed and emerging economies over a long time-span (1965-2018). Consequently, as *per* Hubler et al. (2019), we control for three dimensions of exogenous variability, mostly based on recognized development and governance indicators published by the World Bank: indicators of the level of economic development (based on the GDP or GNI); the level of financial development with the indicators of the stock market (size, liquidity, depth, number of listed firms) and of the banking sector (credit); and the economic freedom in the jurisdiction under review, with the World Bank rule of law index on the average year of the data under review and sub-indexes of the Economic Freedom indicators of the Fraser Institute, as in Hubler et al., 2019.

4. Meta-Regression Analysis results

4.1. Funnel-Asymmetry Test, and FAT-PET-PEESE approach

Eq. (1) is estimated for the whole sample, and then separately for subsamples, corresponding to the six first hypotheses. The results of the FAT-PET are presented in panels (a) of Table 4, sorted according to the specific hypothesis tested. To estimate Eq. (1), we use the following four estimators, as in Iwasaki and Kočenda (2017): 1) OLS clustered by studies (columns [1]), to control for the data dependence within studies (Stanley and Doucouliagos, 2012); 2) WLS clustered by studies, by weighting the standard errors with the inverse of each estimate's variance (columns [2]), to correct for heterogeneity (observations are treated individually and WLS give greater weights to more precise estimates, with lower standard errors); 3) cluster-robust random effects (columns [3]); and 4) cluster-robust fixed effects (columns [4]).

The FAT results confirm a significant publication selection bias in the analyzed literature, towards negative estimates of $AARDs$. All sub-samples but two (event windows before the event and working papers) have highly statistically significant and negative coefficients for standard errors clustered by studies. Additionally, the genuine underlying empirical effect beyond the distortion due to publication selection (PET, the constant) is negative, mostly significant, but much more limited than the naïve averaged estimates (see addendum to Table 3). Averaged across estimators for the full sample and corrected for publication bias, $AARDs$ contract by -0.62% and by a cumulated -1.92% over the average event window [-1.7; +1.4]. This indicates that markets would be much less elastic to the publication of financial crimes than initially thought. Most of the reported $AARDs$ is accounted for by the publication selection bias. Additionally, two sub-samples have statistically significant (and above the average, *i.e.* more negative) constants for the four estimators of Eq. (1): alleged crimes ($AARDs$ of -0.66%), supporting hypothesis 4, and common law countries ($AARDs$ of -0.71%), supporting hypothesis 6.

As the PET suggests that financial crimes significantly impact the returns of firms, though less than initially reported, we report PEESE tests. These tests provide a better estimate of the underlying "true" effect (Stanley and Doucouliagos, 2012). We estimate Eq. (2) based on four estimators, as in Iwasaki and Kočenda (2017): cluster-robust OLS (columns [5]), cluster-robust WLS (columns [6]), random-effects model estimated by the maximum likelihood (columns [7]) and the population-averaged generalized estimating equation (columns [8]).

The PEESE results are presented in panels (b) of Table 4, similarly sorted by hypotheses. We fail to reject the null hypothesis of $\beta_0 = 0$ whatever the estimator. Hence, the PEESE confirms the PET in that a genuine empirical evidence exists in the collected estimates: markets penalize listed firms for engaging in intentional financial crimes (hypothesis 1.1). Still, the PEESE approach concludes with an even lower (by a factor of 4) average effect size adjusted for the publication selection bias (-0.14% *per* day of the event window), which is relatively homogenous across estimates. This implies a -0.43% abnormal contraction in returns cumulated over the average event window [-1.7; +1.4]). The true value of the effect $AARDs$ ranges from -0.26% *per* day to -0.08%. Regarding the PEESE estimates by sub-samples, two main remarks can be made. Firstly, as for the FAT-PET, alleged financial crimes (hypothesis 4) or crimes committed in common law countries (hypothesis 6) trigger the strongest abnormal market reactions (respectively -0.32% and -0.20%).[14] Secondly, regarding the statistically significant estimates across all estimators, published articles conclude with slightly higher abnormal returns (-0.19%, hypothesis 1.2), markets tend to anticipate the news (-0.14%, hypothesis 1.3), accounting crimes are more significant than other securities frauds (hypothesis 3), and the source of the news matters, with two times larger reactions when the crimes are revealed by newspapers than by the firm itself or the regulator (-0.32% against -0.18%, hypothesis 5).

[14] Still, for the common law countries only estimators [5] and [6] are statistically significant.

4.2. The effect of the publication of financial crime on firms' returns

The MRA results of the estimation of Eq. (3) for the impact of the publication of financial crimes on firms' returns are presented in Table 5. As stressed by Askarov and Doucouliagos (2013), modeling heterogeneity across studies with an MRA estimation implies an arbitrage between comprehensiveness and degrees of freedom. Hence, for the sake of clarity and as recommend by Askarov and Doucouliagos (2013), we adopt a parsimonious specification of the MRA, with key variables from Table 3 in view of our set of hypotheses. Additionally, we use a general-to-specific methodology, whereby MRA moderator variables from Table 3 that are neither statistically significant nor relevant to our hypotheses or colinear to other variables are sequentially removed from the model. We use clustered data to adjust standard errors for data dependence resulting from using multiple estimates of abnormal returns *per* study (4 on average). To check the statistical robustness of coefficients, we perform an MRA using the seven estimators presented in Table 5: the cluster-robust OLS estimator, which clusters the collected estimates by study and computes robust standard errors (column [1]); the cluster-robust WLS estimators, which use as an analytical weight either the quality level of the study (number of Google Scholar citations, column [2]), the sample size (column [3]), the precision (the inverse of the squared conservative standard error, column [4]), and the inverse of the number of estimates reported *per* study (column [5]); the cluster-robust unbalanced random effects panel estimator (column [6]); and the cluster-robust study-fixed effect estimator (column [7]), that explores within-study heterogeneity. We present and compare the results for the sake of assessing robustness.

The MRA models capture the heterogeneity in the reported estimates reasonably well. They explain, on average, 58% of the variation in the dependent variable, the reported estimated $AARDs$ (see the adjusted R^2 in Table 5). The meta-regression estimates are, in general, quite consistent across estimators. The MRA estimators detailed in Table 5 support the initial hypotheses: several variables explain the heterogeneity of the reported estimates of $AARDs$ following the publication of a financial crime. The appropriateness of the fixed-effect unbalanced panel estimator for MRA remains unclear.[15] As shown in the table, the coefficients are sensitive to the choice of the estimator. Hence, as in Iwasaki and Kocenda (2017), we will interpret the regression results under the assumption that the meta-independent variables that

[15] Stanley and Doucouliagos (2012) argue that random effects can be quite problematic in MRA, especially if there is publication bias. The Hausman test rejects the null hypothesis that the preferred model is random effects MRA; $\chi 2$ is 29.26 with a p-value of 0.006.

are statistically significant and have the same sign in at least four of seven models constitute statistically robust estimation results.

The constant has a positive sign for all models but one and is statistically significant for four models. Hence, *ceteris paribus*, the constant is not conclusive regarding the impact on returns of the publication of financial crimes. The standard error variable is statistically significantly negative across all models, confirming the publication selection bias in the literature on financial crimes towards negative market reactions.

Complementarily, Table 5 demonstrates that several variables are important in explaining the heterogeneity of the reported estimates. Regarding the characteristics of the data, three features previously observed are confirmed. Firstly, as for the FAT-PET and the PEESE, investigating a common law country (and not a code law) contributes statistically significantly to more negative $AARDs$, *ceteris paribus*, supporting hypothesis 6. 92% of the common law sample is from the U.S. Hence, being more transparent along the enforcement procedure would contribute (negatively) to market reactions and to more efficient markets. Secondly, pure accounting crimes (*versus* violations of securities laws) trigger more negative market reactions, in line with the PEESE. Markets would discriminate depending on the nature of the committed crimes (hypothesis 3), with significantly more negative $AARDs$ for pure accounting frauds, if other research conditions were held constant. This may be accounted for by the fact that such misconducts are more easily understood by investors than price manipulations, insider trading, or breaches to regulatory obligations, and are frequently followed by direct actions with accounting restatements. Thirdly, hypothesis 4 – the very first hint of financial misconduct leads to a stronger market reaction, including an estimation of future sanctions – is supported by the across-the-board negative and statistically significant coefficients of the variable "alleged crime", compared to sanctioned crimes (*i.e.* verdicts). This result confirms the conclusions of the FAT-PET and of the PEESE. Alleged crimes encompass a wide range of situations from newspaper articles mentioning possible frauds to the announcement about early stages of regulatory enforcement or lawsuit/class action filings.

Regarding less (or in-) significant characteristics of the data, the following takeaways can be made. Surprisingly, though in line with the FAT-PET and PEESE, the fact that a breach is subject to a regulatory enforcement procedure – which can point to more serious crimes, as regulators' limited means force them to focus on the worst alleged misdeeds – does not significantly influence market reactions (hypothesis 2). Contrary to hypothesis 5, to the PEESE, and to the literature, the channel through which the financial crime is revealed (newspaper articles *versus* corporate or regulatory communications) does not significantly impact $AARDs$.

Additionally, more recent studies tend to conclude with more negative $AARDs$ (hypothesis 7), given their negative and (to some extent) significant correlation with the average year of the sampled data (which is strongly positively correlated with the publication year). This invalidates the graphical historical retrospectives of $AARDs$ (see Figure 2), with positive slops. Finally, no takeaways can be made from the sectors of incriminated firms.

The following remarks can be made regarding the estimation characteristics. Including the event within the event window, and in particular estimating abnormal market reactions on the day when a financial crime becomes public, is negatively and strongly significantly correlated with the size of $AARDs$ (hypothesis 1.1). This is also confirmed by the fact that using longer event windows, before and after the event, significantly lowers the estimated $AARDs$ (*i.e.* positive coefficient). This supports the fact that markets react rapidly around a crime's publication. *Ergo*, the further from the event, the lower the average cumulated impact. This is echoed by the fact that studies including longer event windows, beyond the [-10;+10] days, do not significantly influence the results. Contrary to the PEESE, the variable controlling for event windows strictly preceding the event was not statistically significant (hypothesis 1.3). Additionally, larger sample sizes also contribute to more negative $AARDs$. Conversely, following the literature methodological standards by using a market model does not impact the results. Finally, the more renowned and in-depth event studies are not limited to estimates of abnormal returns: they are typically complemented with cross-sectional regressions and – to a lesser extent – an estimation of the reputational penalty for financial crime. Still, no information can be derailed from these variables.

Regarding the publication characteristics, the most significant stylized fact is that more negative $AARDs$ attract more attention in literature, with the number of Google citations being negatively and – to some extent – statistically significantly correlated with $AARDs$ (hypothesis 1.2). Conversely, explanatory variables that are not conclusive include: multiple authorships within the sample (*i.e.* the expertise in financial misconducts and possible between-author or between-dataset correlations, stressed by Geyer-Klingeberg et al. (2020)), research undergone by co-authors, publishing in cross-disciplinary journal, and longer articles.

As a robustness check, we added 12 additional studies to the original sample. These additional studies were initially excluded from the scope despite investigating the consequences of intentional financial crimes using an event study methodology, because they either published statistical significance between samples (4 articles), or did not include any information regarding the statistical significance of the results (8 articles). We made the strong hypothesis that all commented estimates of these studies were significant at the 10% level ($t\_stat\_wcs =$

1.645 across the board). Consequently, this complementary sample covers 123 studies, with 460 $(C)AARs$ estimated from 34,550 intentional financial crimes. The sample extension did not alter our findings as all conclusions were confirmed with this larger sample.[16]

4.3.Meta-average effects: the overall reaction of stock markets to the publication of intentional financial crimes

We use the MRA coefficients tabulated in Table 5 to calculate the meta-average effects of financial crimes on stock prices ($AARDs$), as in Askarov and Doucouliagos (2013). In fact, the meta-average is the best estimate of the echo of intentional financial crimes on returns, as provided by the exiting literature. This is under the assumption that the MRA variables quantify the effect of misspecification bias. To do so, *per* Askarov and Doucouliagos (2013), we make the two following assumptions: 1) the MRA variables actually quantify the effect of misspecification bias, and 2) some MRA variables should enter into the MRA prediction, in the absence of theory on the financial crimes. Stanley and Doucouliagos (2012) recommend focusing on the results which are consistent across the multiple WLS, FEML, and cluster-robust MRAs, along with the sample FAT-PET-PEESE MRAs. Hence, in line with our hypotheses, we included the most statistically and economically significant MRA coefficients to predict abnormal market reactions. We assume that a well specified model explaining market reactions to financial crimes should include following variables: the type of crime (accounting or securities regulations), the allegation (or condemnation of crime), the sources of news (newspaper articles or firms/regulators), type of law (common *versus* code), the average year of the sampled data, whether the firms are mostly industrial, the use of market models, the size of the sample of crimes, the length of the event window, whether the event is included in the event window, whether the article included longer-term $(C)AARs$, whether the event study is complemented with cross-sectional regressions, an indication of the echo of the article received by peers (Google citations), the publication in a cross-disciplinary journal, the length of the article, the log GDP growth rate, and market liquidity of the concerned market. Then, meta-averages are constructed as a linear combination of the MRA coefficients for the four cluster-robust WLS estimators, which are the most reliable (Stanley and Doucouliagos, 2012). We also derive statistical significance and 95% confidence intervals. The results are reported in Table 6.

[16] Detailed results are not reported but are available on request.

The publication of intentional financial crimes would impact abnormally negative returns for incriminated firms. Our best average estimate of daily abnormal returns, corrected from the negative publication bias, is -1.14%, with an average 95% confidence interval of -2.49% to +0.47%. Over the average event window investigated in our sample ([-1.7;+1.4] around the event), the $AARD$ implies a cumulated $CAAR$ loss of -3.5%. Additionally, markets would react more to accounting crimes (-1.68% on average, against -0.88% for violations of securities laws), to alleged crimes (-1.61% on average, against -0.41% for condemned crimes), and in common law countries (-1.40% on average, against -0.54% in civil law countries). Such results are much lower than the naïve mean obtained by averaging the reported estimates of $AARDs$ (-1.9% to -3.3%, see addendum of Table 3), as they correct for the negative publication bias.

As a robustness check, we used Andrews and Kasy's (2019) non-linear corrections for publication bias with standard errors clustered by articles. They build a selection model which estimates the probability of publication for each estimate in the literature depending on its *p*-values, based on the observation that standard cut-offs for the *p*-value (0.1, 0.05, and 0.01) are associated with jumps in the distribution of reported estimates. They construct a selection model of publication probability for each estimate in the literature given its *p*-value. The result confirms a strong publication bias in the literature, suggesting a mean impact of the publication of financial crimes corrected for the publication bias of -1.2%, which is statistically significant at the 1% level.[17] This result is coherent with the meta-average previously estimated.

4.4.Comments

The funnel plots are in accord with the FAT and the MRA analyses, in that a highly significant negative publication bias exists for the market reactions to the publication of intentional financial crimes. This is confirmed by the FAT limited to the sub-sample of working papers, which do not suffer from a statistically significant publication bias, contrary to published articles. Still, corrected for the publication bias, we find that the true effect of financial crimes on returns remains negative (ranging from -0.62% for the PET to -0.14% for the PEESE, on average), though being much lower than the naïve initial estimates. Accounting crimes, alleged crimes, and crimes committed in common law countries concur along the different estimations with triggering stronger market reactions.

[17] We chose a symmetric in Z step-function model, with cut-offs corresponding to critical value for two-sided test at the 10% level. By lowering the level to 1%, the average $AARDs$ remain much lower than naïve averages (-1.4%) and highly significant.

We complemented our analysis with an estimation of meta-average effects. Markets negatively interpret the information of intentional financial crimes, leading to a -1.14% abnormal contraction in returns *per* day of the event window (or a cumulated -3.5% contraction over the average event window). Additionally, reactions are stronger for accounting crimes, for alleged crimes (*i.e*. the very first hint of crimes), and for crimes committed in common law jurisdictions (mostly the U.S.). Hence, our results confirm the conclusions of Feroz et al. (1991) and of Pritchard and Ferris (2001): the very first hint of financial crimes triggers the most important and significant abnormal market reaction. This echoes the difference between "not guilty" and "innocent" for the markets stressed by Solomon and Soltes (2019). They stated that being associated to a potential crime will be sanctioned by the market, even when no charges are ultimately brought after an alleged intentional financial fraud.

Further developments and improvements in this study area are desirable to better understand and capture the true effect of financial crimes on returns of listed firms. Firstly, the robustness of the conclusions would gain from being less biased by U.S. studies. In that sense, it is worth stressing two flowing recent trends. The Chinese research on financial crimes has been very dynamic over the last few years. Additionally, the European Securities and Markets Authority (ESMA), the EU's securities markets' regulator, created in 2019 a repository of published sanctions and measures imposed under MiFID II, by National Competent Authorities across Europe. Still, it will take significant resources to build and exploit this subsequent dataset. Secondly, some complementary research could enrich the understanding of some specific crimes, for example the specificities of committing insider trading or price manipulation. Still, given the limited scopes under review, this would imply comparing results from different methodologies, and not only event studies. A major limitation will be the scope and the granularity of the data publicly available. Additionally, for international comparisons, domestic specificities beyond the mere macro-financial specificities (which can be controlled for) curb the relevance of the results, for example with non-synchronized regulatory changes, specificities of enforcement procedures, a different weight given to public and private enforcement, etc. This stresses the interest of European comparison.

The takeaways of this meta-analysis for policy recommendations depend on the agenda of enforcers and regulators. The latter may intend that market participants fear being associated with alleged (and worse condemned) crimes or choose a lighter touch, possibly with anonymized decisions being synonymous to jurisprudence (to set example) or with confidential bilateral procedures. If enforcers intend markets to complement their actions with reputational sanctions, our results point transparency as an efficient regulatory tool. Significant negative abnormal returns follow the publication of alleged crimes committed in common law countries, and conversely no significant reaction follow regulatory procedures and condemned crimes. Enforcers could (for example) communicate along enforcement procedures and substitute sanctions with “name and shame” strategies, at a lower cost. That way, market participants could better price financial crimes, should the enforcers’ objective be that markets account for their work in terms of market supervision, and detection and sanction of financial misconducts. Conversely, if regulators reckon that the regulatory sanction is sufficient (and that markets do not have to double-sentence wrongdoers), anonymization could protect listed firms, and the decisions would still stand for an educational tool.

5. Conclusion

In this paper, we use a total of 439 estimates of abnormal returns following the publication of an intentional financial crime committed by listed firms, extracted from 111 research studies. Under the semi-strong efficient market hypothesis (Fama, 1970), all publicly-available information (here the publication a financial crime) is reflected completely and in an unbiased manner in the stock price, such that it is not possible to earn economic profits on the basis of this information. We perform a meta-analysis to examine the relationship between these abnormal returns and the features of the sample of misconducts under review, of the estimations, and of the publication.

The results of the meta-synthesis reveal a strongly negative publication selection bias in this literature, which is in line with the *a priori* hypothesis of efficient markets and rational investors: markets should react negatively to the publication of financial crimes. When correcting for this publication selection bias, markets on average still react negatively, with a significant -1.14% contraction in abnormal returns *per* day and a cumulated loss of -3.5% over the short-term average event window investigated, which confirms the existence of an informational effect of the publication of financial crimes.

This meta-analysis supports the efficient market hypothesis. An intentional financial crime is bad news regarding the firm, and it potentially leads to substantial costs for listed firms, a feature that justifies a negative market reaction. Additionally, several aspects contribute to materialization of the negative market reactions. Common law countries appear to be more efficient markets, with stronger negative market reactions to the news of (possibly alleged) financial crimes. Further, markets tend to react more to alleged than to condemned financial crimes. Conversely, regulatory enforcement procedures do not significantly impact market reactions.

In terms of policy recommendations, our findings stress how regulatory transparency vis-à-vis the market is an powerful enforcement tool, should the enforcers' objective be that markets account for their actions in terms of market supervision, and of detection and sanction of financial crimes. For example, regulators could improve their communication about enforcement procedures for their actions to be better priced by market participants.

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Table 1: Main Features of Some Securities Enforcers

Table 1 compares the main features of securities law enforcement in the four most frequent countries in the sample: the U.S., China, the U.K. and France.

	U.S.	**China**	**U.K.**	**France**
Securities regulator	Securities and Exchange Commission (SEC)	China Securities Regulatory Commission (CSRC)	Financial Conduct Authority (FCA, FSA until 2012)	*Autorité des Marchés Financiers* (AMF since 2003)
Civil actions can be taken by the securities regulator	Yes	No	Yes	Yes
Major types of sanctions	Cease and desist orders, suspension or revocation of broker-dealer and investment advisor registrations, censures, bars from association with the securities industry, monetary penalties and disgorgements	Warning, fines, disgorgement of illegal gains, banning of market entry, rectification notice, regulatory concern and letter of warning, public statements and regulatory interview	Variation/cancellation /refusal of authorization/approva l/permissions, financial penalties, public censure, prohibition and suspension	Warning, blame, prohibition and suspension from activity, financial penalties
Most frequent type of sanction	Monetary penalties	Non-monetary penalties	Non-monetary penalties	Monetary penalties
Possibility of class actions	Yes	Yes	No	No
Regulatory communication before sanction	Yes	No	No	No
Settlements	Yes	Yes (mediations)	Yes	Yes (since 2012)
Types of laws	Common laws	Code laws	Common laws	Code laws
Legal origins	English	Socialist	English	French

Source: Authors

Table 2: The Meta Dataset

Table 2 describes the main features of the studies included in the meta-analysis. Financial crimes are sorted into three categories: pure accounting frauds, regulatory securities frauds (excluding accounting frauds) and all regulatory securities frauds (including accounting frauds). The country codes are the following, by alphabetical order: AU-Australia, BE-Belgium, CA-Canada, CN-China, DE-Germany, ES-Spain, FR-France, JP-Japan, KR-South Korea, LU-Luxembourg, MY-Malaysia, NL-Netherlands, SW-Sweden, TH-Thailand, TR-Turkey, UK-United Kingdom, US-United States of America. The "sample size" variable is the number of financial crimes which were included in the event study to assess the size effect on returns. The variable "AAR *per* day" is the average of all abnormal returns published, whatever the event window (between -10 to +10 days around the event day), divided by the number of days in the event window. The average AAR *per* day is weighted by the number of estimates *per* study. The variable "Stat. signif." is a dummy variable for statistically significant abnormal returns following the financial crimes. Finally, the variable "Nb. est." stands for the number of estimates included in the dataset *per* study.

Author(s)	Pub. year	Publication outlet	Financial crimes	Countries	Sample period		Sample size	AAR/day	Stat. signif.	Nb. est.
Abdulmanova, Ferris, Jayaraman, Kothari	2019	WP	Regulatory securities frauds	US	2004	2013	462	-0.7%	yes	2
Aggarwal, Hu, Yang	2015	Journal of Portfolio Management	Regulatory securities frauds (incl. accounting frauds)	CN	2001	2011	750	-0.6%	yes/no	5
Agrawal, Chadha	2005	Journal of Law and Economics	Accounting frauds	US	2000	2001	119	-2.0%	yes/no	2
Agrawal, Cooper	2017	Quarterly Journal of Finance	Accounting frauds	US	1997	2002	419	-2.1%	yes	3
Akhigbe, Kudla, Madura	2005	Applied Financial Economics	Accounting frauds	US	1991	2001	77	-3.1%	yes	1
Amoah	2013	Advances in Public Interest Accounting	Regulatory securities frauds	US	1996	2006	301	-7.7%	yes	2
Amoah, Tang	2010	Advances in Accounting	Accounting frauds	US	1997	2002	143	-1.8%	yes/no	2
Andersen, Gilbert, Tourani-Rad	2013	JASSA	Regulatory securities frauds	AU	2004	2012	18	-1.1%	yes	7
Anderson, Yohn	2002	WP	Accounting frauds	US	1997	1999	4	-2.2%	yes	1
Armour, Mayer, Polo	2017	Journal of Financial and Quantitative Analysis	Regulatory securities frauds	UK	2001	2011	40	-0.8%	yes	3
Arnold, Engelen	2007	Management & Marketing	Regulatory securities frauds (incl. accounting frauds)	BE, NL	1994	2003	57	-1.0%	yes/no	4
Baker, Edelman, Powell	1999	Business and Professional Ethics Journal	Regulatory securities frauds	US	1991	1996	14	-0.3%	yes/no	7
Barabanov, Ozocak, Turtle, Walker	2008	Financial Management	Regulatory securities frauds	US	1996	2003	623	-1.6%	yes	1
Bardos, Golec, Harding	2013	Journal of Financial Research	Accounting frauds	US	1997	2002	166	-10.3%	yes	1
Bardos, Mishra	2014	Applied Financial Economics	Accounting frauds	US	1997	2002	24	-5.5%	yes	2
Barniv, Cao	2009	Journal of Accounting and Public Policy	Accounting frauds	US	1995	2003	61	-6.8%	yes	1
Bauer, Braun	2010	Financial Analytical Journal	Regulatory securities frauds (incl. accounting frauds)	US	1996	2007	648	-1.1%	yes	20
Beasley, Carcello, Hermanson, Neal	2010	COSO	Accounting frauds	US	1998	2007	213	-4.1%	yes/no	6
Beneish	1999	The Accounting Review	Accounting frauds	US	1987	1993	50	-4.2%	yes	3
Bhagat, Bizjak, Coles	1998	Financial Management	Regulatory securities frauds	US	1981	1983	46	-1.4%	yes	1

Author(s)	Pub. year	Publication outlet	Financial crimes	Countries	Sample period		Sample size	AAR/day	Stat. signif.	Nb. est.
Billings, Klein, Zur	2012	WP	Regulatory securities frauds (incl. accounting frauds)	US	1996	2008	408	-0.3%	yes	3
Bohn, Choi	1996	University of Pennsylvania Law Review	Regulatory securities frauds	US	1975	1986	103	-1.2%	yes	2
Bonini, Boraschi	2010	Journal of Business Ethics	Regulatory securities frauds (incl. accounting frauds)	US	1996	2005	686	-2.1%	yes	7
Bowen, Call, Rajgopal	2010	The Accounting Review	Regulatory securities frauds (incl. accounting frauds)	US	1989	1996	78	-0.6%	yes	1
Bradley, Cline, Lian	2014	Journal of Corporate Finance	Regulatory securities frauds (incl. accounting frauds)	US	1996	2011	1530	-0.6%	yes	1
Brous, Leggett	1996	Journal of Financial Research	Regulatory securities frauds (incl. accounting frauds)	US	1989	1991	62	-1.0%	yes/no	2
Burns, Khedia	2006	Journal of Financial Economics	Accounting frauds	US	1997	2001	215	-2.0%	yes	4
Callen, Livnat, Segal	2006	Journal of Investing	Accounting frauds	US	1986	2001	385	-2.8%	yes	1
Chava, Cheng, Huang, Lobo	2010	International Journal of Law and Management	Regulatory securities frauds	US	1995	2004	85	-3.1%	yes	1
Chen, Firth, Gao, Rui	2005	Journal of Accounting and Public Policy	Regulatory securities frauds	CN	1999	2003	169	-0.2%	yes/no	10
Choi, Karpoff, Lou, Martin	2019	WP	Regulatory securities frauds (incl. accounting frauds)	US	1978	2015	942	-14.9%	yes	1
Choi, Pritchard	2016	Journal of Legal Studies	Regulatory securities frauds	US	2004	2007	231	-6.5%	yes	3
Christensen, Paik, Williams	2010	Journal of Forensic & Investigative Accounting	Regulatory securities frauds (incl. accounting frauds)	US	2001	2003	151	-2.1%	yes/no	6
Cook, Grove	2009	Journal of Forensic & Investigative Accounting	Regulatory securities frauds (incl. accounting frauds)	US	1984	2005	88	-4.3%	yes	9
Correia, Klausner	2012	WP	Accounting frauds	US	2000	2011	683	-5.0%	yes	2
Cox, Weirich	2002	Managerial Auditing Journal	Accounting frauds	US	1992	1999	27	-4.2%	yes	3
Davidson, Worrell, Lee	1994	Journal of Business Ethics	Accounting frauds	US	1965	1990	34	-0.6%	yes/no	12
Davis, Taghipour, Walker	2017	Managerial Finance	Regulatory securities frauds	US	1996	2013	2153	0.4%	yes	2
de Batz	2020	European Journal of Law and Economics	Regulatory securities frauds (incl. accounting frauds)	FR	2004	2016	52	-0.3%	yes/no	8
Dechow, Sloane, Sweeney	1996	Contemporary Accounting Research	Accounting frauds	US	1982	1992	78	-8.8%	yes	1
Deng, Willis, Xu	2014	Journal of Financial and Quantitative Analysis	Regulatory securities frauds (incl. accounting frauds)	US	1996	2006	156	-1.7%	yes	6
Desai, Hogan, Wilkins	2006	The Accounting Review	Accounting frauds	US	1997	1998	146	-3.7%	yes	1
Djama	2013	Revue Française de Gestion	Accounting frauds	FR	1995	2008	36	-0.9%	yes/no	3
Du	2017	Journal of Business Finance & Accounting	Accounting frauds	US	2001	2011	17	-2.3%	yes	2
Engelen	2009	WP	Regulatory securities frauds	BE, DE, FR, LU, NL, UK	1995	2005	83	-0.8%	yes/no	12

Author(s)	Pub. year	Publication outlet	Financial crimes	Countries	Sample period		Sample size	AAR/day	Stat. signif.	Nb. est.
Engelen	2011	Book chapter	Regulatory securities frauds	BE, DE, FR, LU, NL, UK	1995	2005	101	-1.0%	yes/no	4
Engelen	2012	CESifo Economic Studies	Regulatory securities frauds	US	1993	2008	122	-0.8%	yes/no	2
Eryiğit	2019	Journal of Financial Crime	Accounting frauds	TR	2005	2015	160	-0.1%	yes/no	4
Ewelt-Knauer, Knauer, Lachmann	2015	Journal of Business Economics	Regulatory securities frauds	DE	1998	2014	126	-2.3%	yes	2
Feroz, Park, Pastena	1991	Journal of accounting research	Accounting frauds	US	1982	1989	58	-2.6%	yes/no	11
Ferris, Jandik, Lawless, Makhija	2007	Journal of Financial and Quantitative Analysis	Regulatory securities frauds	US	1982	1999	194	-0.6%	yes	1
Fich, Shivdasani	2007	Journal of Financial Economics	Regulatory securities frauds	US	1998	2002	200	-3.5%	yes	4
Firth, Rui, Wu	2009	Journal of Accounting and Public Policy	Regulatory securities frauds	CN	1999	2005	61	-0.7%	yes/no	10
Firth, Rui, Wu	2011	Journal of Corporate Finance	Accounting frauds	CN	2000	2005	267	-0.1%	yes/no	8
Firth, Wong, Xin, Yick	2016	Journal of Business Ethics	Regulatory securities frauds (incl. accounting frauds)	CN	2003	2010	75	-0.2%	yes	2
Flore, Degryse, Kolaric, Schiereck	2018	WP	Regulatory securities frauds (incl. accounting frauds)	DE, ES, FR, NL, SW, UK, US	2005	2015	251	0.1%	yes/no	5
Gande, Lewis	2009	Journal of Financial and Quantitative Analysis	Regulatory securities frauds	US	1996	2003	605	-1.3%	yes/no	6
Gerety, Lehn	1997	Managerial and Decision Economics	Accounting frauds	US	1981	1987	37	-1.0%	yes	1
Goldman, Peyer, Stefanescu	2012	Financial Management	Accounting frauds	US	1976	2010	444	-8.9%	yes	5
Griffin, Grundfest, Perino	2004	Abacus	Regulatory securities frauds	US	1990	2002	2133	-1.8%	yes/no	4
Griffin, Sun	2016	Accounting and Finance Research	Regulatory securities frauds	US	2001	2007	80	-0.8%	yes/no	4
Haslem, Hutton, Hoffmann Smith	2017	Financial Management	Regulatory securities frauds	US	1995	2006	594	-0.8%	yes	6
Hirschey, Palmrose, Scholz	2005	WP	Accounting frauds	US	1995	1999	405	-7.3%	yes	1
Humphery-Jenner	2012	Journal of Financial Intermediation	Regulatory securities frauds	US	1996	2007	416	-1.1%	yes	5
Iqbal, Shetty, Wang	2007	Journal of Financial Research	Regulatory securities frauds	US	1996	2003	298	-5.2%	yes	8
Johnson, Ryan, Tian	2003	WP	Accounting frauds	US	1992	2005	87	-4.9%	yes	1
Jordan, Peek, Rosengren	2000	Journal of Financial Intermediation	Regulatory securities frauds (incl. accounting frauds)	US	1989	1994	35	-1.7%	yes	1
Karpoff, Koester, Lee, Martin	2017	The Accounting Review	Accounting frauds	US	1978	2011	1052	-15.2%	yes	1
Karpoff, Lee, Martin	2008	Journal of financial and quantitative analysis	Accounting frauds	US	1978	2002	371	-11.2%	yes	6
Karpoff, Lott	1993	Journal of Law and Economics	Accounting frauds	US	1978	1987	4	-1.3%	yes/no	5
Kellogg	1984	Journal of Accounting and Economics	Accounting frauds	US	1967	1979	26	-3.0%	yes/no	2
Kirat, Rezaee	2019	Applied Economics	Regulatory securities frauds (incl. accounting frauds)	FR	2004	2017	54	-0.5%	yes	2

Author(s)	Pub. year	Publication outlet	Financial crimes	Countries	Sample period		Sample size	AAR/day	Stat. signif.	Nb. est.
Klock	2015	Journal of Business & Securities Law	Regulatory securities frauds (incl. accounting frauds)	US	1996	2012	714	-1.0%	yes	3
Kouwenberg, Phunnarungsi	2013	Pacific-Basin Finance Journal	Regulatory securities frauds (incl. accounting frauds)	TH	2003	2010	111	-0.7%	yes/no	4
Kravet, Shevlin	2010	Review of Accounting Studies	Accounting frauds	US	1997	2001	299	-0.8%	yes	1
Kryzanowski, Zhang	2013	Journal of Multinational Financial Management	Accounting frauds	CA	1997	2006	210	-1.9%	yes	4
Kwan, Kwan	2011	International Review of Business Research Papers	Regulatory securities frauds	MY	2005	2009	41	-0.4%	yes/no	3
Lei, Law	2019	WP	Regulatory securities frauds (incl. accounting frauds)	CN	1999	2015	1188	-0.1%	yes/no	7
Liebman, Milhaupt	2008	Columbia Law Review	Regulatory securities frauds	CN	2001	2006	68	-0.7%	yes/no	8
Lieser, Kolaric	2016	WP	Regulatory securities frauds (incl. accounting frauds)	US	1996	2014	1377	-1.3%	yes/no	15
Loh, Rathinasamy	2003	Review of Pacific Basin Financial Markets and Policies	Regulatory securities frauds (incl. accounting frauds)	US	1996	1998	290	-0.5%	yes	2
Marciukaityte, Szewczyk, Uzun, Varma	2006	Financial Analysts Journal	Regulatory securities frauds (incl. accounting frauds)	US	1978	2001	28	-3.9%	yes	1
Marciukaityte, Szewczyk, Varma	2009	Financial Analysts Journal	Accounting frauds	US	1997	2002	187	-3.3%	yes	1
McDowell	2005	WP	Accounting frauds	US	1998	2003	174	-2.1%	yes	1
Muradoglu, Clark Huskey	2008	WP	Regulatory securities frauds (incl. accounting frauds)	US	1995	2004	296	-0.6%	yes/no	12
Nainar, Rai, Tartaroglu	2014	International Journal of Disclosure and Governance	Regulatory securities frauds	US	1999	2007	77	-1.0%	yes/no	5
Nelson, Gilley, Trombley	2009	Securities Litigation Journal	Regulatory securities frauds	US	2002	2007	58	-2.6%	yes	1
Nourayi	1994	Journal of Accounting and Public Policy	Regulatory securities frauds (incl. accounting frauds)	US	1977	1984	82	-0.2%	yes	4
Owers, Lin, Rogers	2002	International Business and Economics Research Journal	Accounting frauds	US	1994	1997	13	-3.8%	yes	4
Ozbas	2008	WP	Accounting frauds	US	1999	2003	75	-2.5%	yes/no	4
Ozeki	2019	Securities Analysts Journal	Accounting frauds	JP	2005	2016	218	-9.1%	yes/no	2
Pereira, Malafronte, Sorwar, Nurullah	2019	Journal of Financial Services Research	Regulatory securities frauds (incl. accounting frauds)	US	2004	2015	1387	-6.4%	yes/no	5
Persons	1997	Journal of Business Research	Regulatory securities frauds	US	1972	1993	95	-0.4%	yes	3
Plumlee, Yohn	2008	WP	Accounting frauds	US	2003	2006	1303	-0.3%	yes	1
Pritchard, Ferris	2001	WP	Regulatory securities frauds	US	1995	1999	89	-3.1%	yes/no	3
Romano	1991	Journal of Law, Economics, and Organization	Regulatory securities frauds	US	1970	1987	66	-0.8%	yes/no	6
Scholz	2008	US Department of Treasury	Accounting frauds	US	1997	2006	264	-6.5%	yes	1
Slovin, Sushka, Polonchek	1999	Journal of Financial Economics	Regulatory securities frauds (incl. accounting frauds)	US	1975	1992	61	-1.8%	yes	2

Author(s)	Pub. year	Publication outlet	Financial crimes	Countries	Sample period		Sample size	AAR/day	Stat. signif.	Nb. est.
Song, Han	2017	Journal of Business Ethics	Regulatory securities frauds (incl. accounting frauds)	KR	2001	2010	220	-0.7%	yes	3
Sun, Zhang	2006	WP	Regulatory securities frauds	CN	1990	2002	144	-0.5%	yes	1
Takmaz, Keles	2017	Journal of Business Research Turk	Regulatory securities frauds	TR	2007	2016	72	-0.2%	yes/no	4
Tanimura, Okamoto	2013	Asian Economic Journal	Accounting frauds	JP	2000	2008	39	-3.1%	yes	1
Tay, Puah, Brahmana, Abdul Malek	2016	Journal of Financial Crime	Regulatory securities frauds (incl. accounting frauds)	MY	1996	2013	17	0.0%	no	2
Wang, Ashton, Jaafar	2019	The British Accounting Review	Accounting frauds	CN	2007	2016	433	-0.1%	yes/no	7
Wang, Wu	2011	China Journal of Accounting Research	Accounting frauds	CN	1999	2005	67	-0.1%	yes/no	5
Wu	2002	WP	Accounting frauds	US	1977	2000	932	-7.7%	yes	1
Wu, Zhang	2014	China Journal of Accounting Studies	Regulatory securities frauds	CN	2002	2011	157	-2.3%	yes	5
Xu, Xu	2020	International Review of Law and Economics	Regulatory securities frauds (incl. accounting frauds)	CN	2014	2018	107	-0.5%	yes/no	6
Yu, Zhang, Zheng	2015	Financial Management	Accounting frauds	CN	1999	2011	195	-0.6%	yes	2
Zeidan	2013	Journal of Business Ethics	Regulatory securities frauds	US	1990	2009	163	-0.4%	yes/no	4
Zhu, Hu	2010	WP	Accounting frauds	CN	2006	2008	88	-0.9%	yes/no	7
Overall	**2009**				**1994**	**2004**	**293**	**-1.9%***		**4**

Source: Authors

Table 3: Variable Definitions and Descriptive Statistics

Table 3 describes most of the variables for the full sample of financial crimes and for the two subsamples of alleged crimes and condemned financial crimes. If not specified, the descriptive statistics are calculated for the sample of articles. When relevant, they are calculated for all estimates included in the meta-analysis (*i.e.* on average 3.95 estimates *per* article). The full sample (All financial crimes) is split between alleged crimes (*i.e.* articles published mentioning a possible crime, regulatory investigations, filling a lawsuit or a class action lawsuit, etc.) and condemned financial crimes (verdicts of regulatory procedures or lawsuits, accounting restatements following the publication of intentional misstatements, etc.). These categories are non-mutually exclusive.

Variables		**Description**	**All financial crimes**				**Alleged financial crimes**				**Condemned financial crimes**			
			Mean	Std dev.	Min.	Max.	Mean	Std dev.	Min.	Max.	Mean	Std dev.	Min.	Max.
Effect:	aard*	Average abnormal returns *per* day ($CAAR/length\_ev\_w\_est$ or AAR).	-1.9%	3.1%	-25.2%	12.2%	-2.4%	3.3%	-25.2%	1.6%	-1.2%	2.6%	-12.5%	12.2%
Std Error:	se*	Standard error of the estimated abnormal returns ((C)AAR divided by the conservative t-statistic: t_stat_wcs).	2.0%	3.1%	0.0%	29.3%	1.9%	2.3%	0.0%	12.4%	2.0%	4.0%	0.1%	29.3%
1. <u>Data characteristics (by studies)</u>														
Geographical scope:	one_country	1 if only one country in the scope.	0.96	0.19	0	1	0.96	0.21	0	1	0.98	0.15	0	1
	US	1 if the estimate's sample is the U.S. (most frequent country in the sample), based on Geyer-Klingeberg et al. (2020).	0.70	0.46	0	1	0.80	0.41	0	1	0.53	0.50	0	1
	Asia	1 if the estimate's sample is an Asian country (China being the 2nd most frequent country in the sample).	0.19	0.39	0	1	0.10	0.30	0	1	0.36	0.48	0	1
	Europe	1 if the estimate's sample is a European country.	0.10	0.30	0	1	0.10	0.3	0	1	0.10	0.29	0	1
	common_law	1 if the legal origin of the commercial law of a country is common law, and zero otherwise.	0.76	0.43	0	1	0.80	0.41	0	1	0.67	0.48	0	1
Period under review:	begin_period	Beginning of period under review.	1994	9.49	1965	2014	1992	10.23	1965	2014	1997	6.77	1975	2007
	end_period	End of period under review.	2004	8.12	1979	2018	2003	8.86	1979	2018	2006	6.37	1991	2016
	length_period	Length of the period under review (end_period - begin_period + 1)	10.8	6.2	2	35	11.7	6.7	3	35	9.2	4.9	2	24
	avg_year	Average year of the period under review ((begin_period + end_period)/2).	1999	8.17	1973	2016	1997	8.83	1973	2016	2001	6.09	1984	2012
Events types:														
Types of regulatory breaches:	account_fr	1 if the scope of crimes is limited to pure accounting frauds.	0.41	0.49	0	1	0.30	0.46	0	1	0.58	0.50	0	1
	secu_fr	1 if the scope of crimes is limited to pure regulatory securities frauds.	0.32	0.47	0	1	0.42	0.50	0	1	0.18	0.39	0	1
	secu_a_fr	1 if the scope of crimes covers regulatory securities frauds (incl. accounting frauds).	0.27	0.45	0	1	0.28	0.45	0	1	0.24	0.43	0	1
Source of the news:	media_com	1 if the crimes were revealed *via* press articles (WSJ in particular in the U.S.).	0.38	0.49	0	1	0.49	0.50	0	1	0.22	0.42	0	1
	corp_com	1 if the crimes were revealed *via* corporate communication.	0.30	0.46	0	1	0.19	0.39	0	1	0.44	0.50	0	1
	reg_com	1 if the crimes were revealed *via* regulatory communication.	0.66	0.48	0	1	0.59	0.49	0	1	0.73	0.45	0	1
Steps of enforcement procedure:	alleged_fr*, [1]	1 if the fraud is alleged (not condemned, covering articles published mentioning a possible crime, regulatory investigations, and filling a lawsuit or a class action lawsuit.	0.61	0.49	0	1	1.00	0.00	1	1	0.07	0.25	0	1
	invest*	1 if the crimes were being investigated.	0.11	0.31	0	1	0.13	0.34	0	1	0.02	0.15	0	1
	settlement*	1 if the crimes went through settlement.	0.04	0.19	0	1	0.01	0.12	0	1	0.04	0.21	0	1
	account_restat*	1 if the crimes led to an accounting restatement.	0.14	0.35	0	1	0.04	0.21	0	1	0.51	0.51	0	1
	condemn*, [1]	1 if the crimes were condemned or recognized (verdicts of regulatory procedures or lawsuits, accounting restatements following the publication of intentional misstatements).	0.41	0.49	0	1	0.04	0.21	0	1	1.00	0.00	1	1

Variables		Description	All financial crimes				Alleged financial crimes				Condemned financial crimes			
			Mean	Std dev.	Min.	Max.	Mean	Std dev.	Min.	Max.	Mean	Std dev.	Min.	Max.
Types of enforcement procedure:	regul_proc[*]	1 if the crimes led to a regulatory procedure.	0.52	0.50	0	1	0.49	0.50	0	1	0.60	0.50	0	1
	stk_exch_proc[*; 2]	1 if the crimes led to a stock exchange procedure.	0.09	0.29	0	1	0.01	0.12	0	1	0.18	0.39	0	1
	class_action[*, 2]	1 if the crimes led to a class action.	0.25	0.43	0	1	0.30	0.46	0	1	0.07	0.25	0	1
	private_lawsuits[*, 2]	1 if the crimes led to a private lawsuit.	0.10	0.31	0	1	0.12	0.32	0	1	0.11	0.32	0	1
Main sectors:	Indus[3]	1 if the most frequent sector involved in financial misconducts is industry.	0.32	0.47	0	1	0.28	0.45	0	1	0.38	0.49	0	1
	Finance[3]	1 if the most frequent sector involved in financial misconducts is finance.[5]	0.17	0.38	0	1	0.16	0.37	0	1	0.18	0.39	0	1
2. <u>Estimation characteristics (by studies if not specified)</u>														
Model:	mkt_model	1 if market model used to estimate abnormal returns (not Fama-French, CAPM or market-adjusted models).	0.83	0.38	0	1	0.86	0.35	0	1	0.80	0.40	0	1
	end_sample_size	Final number of events in the sample (in particular excluding confounding events and events with data problems).	293	410	4	2 153	303	446	4	2 153	261	341	13	1 387
Estimation window:	start_est_w	Beginning of the estimation window (in days, relative to the event in t=0).	-153	165	-1 080	0	-229	110	-750	-61	-258	181	-1 080	-90
	end_est_w	End of the estimation window (in days, relative to the event in t=0).	-21	42	-300	0	-27	39	-250	0	-37	57	-300	-2
Event window:	start_ev_w	Beginning of the event window (in days, relative to the event in t=0).	-17	45	-255	0	-13	36	-250	0	-23	55	-255	0
	end_ev_w	End of the estimation window (in days, relative to the event in t=0).	19	48	0	300	18	49	0	300	19	44	0	255
	length_ev_w	Length of the event window (end_ev_w - start_ev_w +1, in days).	36.4	82.8	1	511	32.0	78.0	1	501	41.0	88.0	2	511
	long_event_w	1 if event windows beyond [-10;+10].	0.27	0.45	0	1	0.26	0.44	0	1	0.27	0.45	0	1
Estimates:	significance[*]	1 if abnormal returns are significant.	0.75	0.43	0	1	0.78	0.41	0	1	0.69	0.46	0	1
	t_stat_wcs[*]	Estimated (conservative) t-statistics, equal to the t-statistic when available or estimated from z-test, p-value, or statistical levels.	-3.60	4.62	-46.8	-0.03	-3.95	5.59	-46.82	7.88	-2.34	3.27	-22.96	3.54
	start_ev_w_est[*]	Beginning of the event window of the estimate (in days, relative to the event in t=0).	-1.7	2.9	-10	6	-1.7	3.0	-10	3	-1.7	2.9	-10	6
	end_ev_w_est[*]	End of the estimation window of the estimate (in days, relative to the event in t=0).	1.4	2.7	-6	10	1.2	2.6	-2	10	1.6	2.8	-6	10
	length_ev_w_est[*]	Length of the event window of the estimate (end_ev_w_est - start_ev_w_est +1, in days).	4.1	4.4	1	21	3.9	4.4	1	21	4.3	4.3	1	21
	nb_est[*]	Number of estimates reported *per* study, to avoid unintentional weighting of articles reporting multiple estimates, as recommended by Havránek and Irsova (2017). We used the raw number of estimates, as most of the articles in the sample did not include the estimate's variances.	3.95	3.34	1	20	4.23	3.73	1	20	3.40	2.51	1	10
	pre_ev_date	The event window is strictly before the event date (t=0), or put it differently start_ev_w_est<0 and end_ev_w_est<0.	0.14	0.35	0	1	0.16	0.37	0	1	0.11	0.32	0	1
	ev_date	The event window is limited to the event date (t=0), or put it differently start_ev_w_est=0 and end_ev_w_est=0.	0.19	0.39	0	1	0.22	0.41	0	1	0.15	0.35	0	1
	around_ev_date	The event window is includes to the event date (t=0), or put it differently start_ev_w_est<0 and end_ev_w_est>0.	0.62	0.49	0	1	0.56	0.50	0	1	0.69	0.50	0	1
	post_ev_date	The event window is includes to the event date (t=0), or put it differently start_ev_w_est>0 and end_ev_w_est>0.	0.06	0.23	0	1	0.06	0.24	0	1	0.05	0.22	0	1
	reput_est	1 if additional estimates of reputational penalties included in the article.	0.12	0.32	0	1	0.12	0.32	0	1	0.13	0.34	0	1

Variables		Description	All financial crimes				Alleged financial crimes				Condemned financial crimes			
			Mean	Std dev.	Min.	Max.	Mean	Std dev.	Min.	Max.	Mean	Std dev.	Min.	Max.
	cross_sect	1 if complementary cross-sectional regressions included in the article.	0.67	0.47	0	1	0.58	0.50	0	1	0.78	0.42	0	1
3. <u>**Publication characteristics (by studies)**</u>														
Characteristics of the article:	nb_authors	Number of authors of the paper.	2.37	0.92	1	4	2.33	0.97	1	4	2.42	0.84	1	4
	multiple_articles	1 if one of the authors of the paper wrote other articles in the sample.	0.33	0.47	0	1	0.32	0.47	0	1	0.33	0.48	0	1
	year_pub	Year of publication.	2009	7.46	1984	2020	2008	8.03	1984	2020	2011	5.93	1996	2020
	month_pub	Month of publication (1 to 12).	5.55	3.71	0	12	5.70	3.68	0	12	5.34	3.60	0	12
	length_art	Length of the article (number of pages).	26.8	14.2	3	80	27.7	15.0	3	80	25.6	13.4	4	56
	cross_disc	1 if published in a cross-disciplinary journal.	0.87	0.33	0	1	0.86	0.35	0	1	0.89	0.32	0	1
	ref_journ	1 if published in a refereed journal or chapter in a book. Otherwise, the article is a working paper.	0.81	0.39	0	1	0.78	0.42	0	1	0.84	0.37	0	1
Quality of the publication:	scopus	1 if published in a Scopus journal.	0.67	0.47	0	1	0.62	0.49	0	1	0.71	0.46	0	1
	cs_2018	Scopus Cite Score in 2018.	1.63	1.84	0	7.34	1.54	1.83	0	7.34	1.67	1.86	0	7.34
	cs_year	Scopus Cite Score of the year of publication (2011 to 2018, otherwise 2011).	1.16	1.37	0	5.58	1.11	1.34	0	5.58	1.17	1.40	0	5.58
	if_repec	Repec Discounted impact factor of the year of publication.	0.39	1.06	0	5.67	0.33	0.91	0	5.67	0.45	1.23	0	5.67
	cit_google	Number of citations in Google Scholar as of May 1st, 2020.	180	544	0	5007	196.3	632	0	5007	145	351	0	1839
4. <u>**Control variables (by studies)**</u>														
Economic development indics.:	log_gdp	Log of nominal current USD GDP mid-period under review (source: World Bank, data available from 1960 to 2018), as in Jackson and Roe (2009).	8.5	1.0	4.9	9.6	8.7	0.8	5.9	9.6	8.3	1.2	4.9	9.6
	log_gni_capita	Log of GNI *per capita* in USD mid-period under review (source World Bank, data available from 1960-2018), as in Hubler et al. (2019) and as in Jackson and Roe (2009).	9.9	1.0	6.5	10.8	10.1	0.8	6.5	10.8	9.5	1.4	6.5	10.8
	log_gdp_capita	Log of GDP *per capita* GDP mid-period under review (source: World Bank), as in Djankov et al. (2008).	3.0	1.1	-0.3	3.9	3.2	0.9	-0.3	3.9	2.7	1.3	-0.3	3.9
Financial market indics.:	mkt_cap_gdp	Domestic market capitalization, as % of GDP mid-period under review (source: World Bank, data available from 1975 to 2018), as in Djankov et al. (2008) and as in Hubler et al. (2019).	97.9	41.9	17.6	168.1	96.1	38.8	29.9	150.4	98.5	46.6	17.6	168.1
	mkt_liquid	Market liquidity indicator mid-period under review (stocks traded, turnover ratio of domestic shares (%) (source: World Bank, data available from 1975 to 2018), as in La Porta et al. (2006).	113.5	47.6	27.3	317.6	113.7	49.7	44.8	317.6	111.1	43.6	27.3	218.0
	log_nb_listed_firms	Log of the average number of domestic listed firms to its population in millions mid-period under review, (source: World Bank, data available from 1975 to 2018), as in Djankov et al. (2008).	2.7	1.2	-0.8	4.5	2.8	1.0	-0.8	3.5	2.4	1.5	-0.8	4.5
	stock_gdp	Total value of stock traded, as % of GDP mid-period under review (current USD, source: World Bank, data available from 1975 to 2018).	0.91	0.88	0.01	2.90	0.88	0.87	0.01	2.90	0.92	0.88	0.01	2.44
	dom_cred_gdp	Domestic credit provided by financial sector (% of GDP) mid-period under review (source: World Bank, data available from 1960 to 2018).	97.9	32.2	64.7	235.7	93.3	32.4	64.7	235.7	107.2	35.0	66.0	223.4
Legal environment:	rule_law	Rule of law mid-period under review (or previous year if not published, or 1996 if before) (source World Bank, data available from 1996 to 2018), supported by the conclusion of La Porta et al. (2006) that financial markets do not prosper when left to market forces alone;	1.2	0.7	-0.6	1.8	1.4	0.5	-0.6	1.8	0.9	0.9	-0.6	1.8

Variables	Description	All financial crimes				Alleged financial crimes				Condemned financial crimes			
		Mean	Std dev.	Min.	Max.	Mean	Std dev.	Min.	Max.	Mean	Std dev.	Min.	Max.
regul_fraser	Regulation sub-index of the economic freedom indicator from the Fraser Institute for the mid-period under review (or the closest-available or average year when not available), with the, data available from 1970 to 2017, as in Hubler et al. (2019).[4]	7.9	1.1	4.2	8.8	8.1	0.9	4.2	8.8	7.5	1.4	4.2	8.8
regul_cred_fraser	Credit market regulation sub-index of the economic freedom indicator from the Fraser Institute for the mid-period under review (or the closest-available or average year when not available), with the, data available from 1970 to 2017, as in Hubler et al. (2019). [4]	9.1	1.1	4.6	10.0	9.3	0.9	4.6	10.0	8.7	1.4	4.6	10. 0
Sample size:		111				69				45			

Sources: Studies, Authors' calculations
[*] *Descriptive statistics for all estimates included in the dataset (not by studies).*
[1] *In some studies, no split was done between alleged and condemned financial crimes. All crimes were treated jointly. Consequently, the sum of the two variables exceeds one.*
[2] *Private enforcement is defined as the combination of the following types of procedures: private lawsuits, stock exchange procedures, and class actions.*
[3] *In half of sample, the main industrial sector (or the split by major industrial sector) was included in the descriptive statistics.*
[4] *The Fraser economic freedom index measures the degree of economic freedom present in five major areas (with 26 components): size of government; legal system and security of property rights; sound money; freedom to trade internationally; and regulation. Each component and sub-component is placed on a scale from 0 to 10 that reflects the distribution of the underlying data.*
[5] *In two articles (Bauer and Braun (2010) and Ozeki (2019), financial firms were excluded from the sample.*

Addendum. As recommended by Stanley and Doucouliagos (2012), weighted averages are recommended to analyze the average effect size. The following table of average abnormal returns *per* day (AARDs) benchmarks the simple average with different weighted averages. Still, in the presence of publication selection bias, all averages are distorted (Stanley, 2008).

	AARDs
Simple (unweighted average)	-1.89%
Weighted by sample size	-2.27%
Weighted by journal impact factors	-1.90%
Weighted by the Google Scholar citations	-3.31%

Sources: Studies, Authors' calculations. *Note: All AARDs are winsorized at the 1% level.*

Table 4: Meta-Regression Analysis of Publication Selection Bias by Hypotheses (FAT-PET and PEESE)

Table 4 details the results of the publication selection bias, based on the FAT-PET test (Eq. (1)) and the PEESE approach (Eq. (2)) for the full sample and sub-samples for the first-six tested hypotheses. The standard errors control for the publication bias (FAT) and the intercepts (PET) or inverse of standard errors (PEESE) for the means beyond bias. For each sample, the first columns present the results using OLS after correcting for data dependence by clustering standard errors by studies (columns [1] and [5]). The second columns (columns [2] and [6]) display the results for clustered weighted least squares, by weighting the standard errors with the inverse of each estimates' variance (Stanley and Doucouliagos, 2012). Finally, the FAT-PAT equation is estimated with cluster-robust random-effects [columns 3] and the cluster-robust fixed-effects [columns 4] while the PEESE equation is estimated with the random-effects model estimated by the maximum likelihood (ML) method [columns 7] and the population averaged generalized estimating equation (GEE) model [columns 8].

Hypothesis 1.1. Markets penalize listed firms for engaging in intentional financial crimes. The initial disclosure of frauds results in significant negative abnormal returns.

(a) FAT-PET test (Equation: $AARD = \beta_0 + \beta_1 SE + \varepsilon$)

	Full sample			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Cluster-robust random-effects panel GLS[a]	Cluster-robust fixed-effects panel LSDV[b]
Model	[1]	[2]	[3]	[4]
Intercept (PET: H_0: β_0=0)	-0.00624*** [1]	-0.000333 [3]	-0.00915***	-0.00922***
	(-4.60)	*-0.818*	*(-3.08)*	*(-4.38)*
SE (FAT: H_0: β_1=0)	-1.576*** [2]	-3.225*** [4]	-1.215***	-1.451***
	(-7.86)	*-9.111*	*(-3.30)*	*(-5.61)*
K	439	439	439	439
R^2	0.517	0.215	0.255	0.5166

(b) PEESE approach (Equation: $t\_stat\_wcs = \beta_1 SE + \beta_0 (1/SE) + \varepsilon$)

	Full sample			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Random effects panel ML	Population-averaged panel GEE
Model	[5]	[6]	[7]	[8]
SE	-67.12***	-1,338***	-34.23*	-45.20***
	(-5.04)	*-5.166*	*(-1.88)*	*(-4.55)*
1/*SE* (H_0: β_0=0)	-0.00262***	-0.000771***	-0.000916**	-0.00134**
	(-5.00)	*-3.943*	*(-2.36)*	*(-2.19)*
K	439	439	439	439
R^2	0.221	0.245	-	-

Hypothesis 1.3. Markets anticipate the events, possibly due to leakages.

	Before event (incl. AAR[-1])			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Cluster-robust random-effects panel GLS[c]	Cluster-robust fixed-effects panel LSDV[d]
Model	[1]	[2]	[3]	[4]
Intercept (PET: H_0: β_0=0)	-0.00761***	-0.000832	-0.00607***	-0.00735***
	(-4.58)	*-1.165*	*(-2.85)*	*(-4.67)*
SE (FAT: H_0: β_1=0)	-0.119	-1.674***	-0.308	-0.101
	(-0.73)	*-5.078*	*(-1.18)*	*(-0.69)*
K	62	62	62	62
R^2	0.015	0.151	0.063	0.0153

	Before event (incl. AAR[-1])			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Random effects panel ML	Population-averaged panel GEE
Model	[5]	[6]	[7]	[8]
SE	-25.21	-1,123**	-13.68	-16
	(-1.61)	*-2.439*	*(-0.48)*	*(-1.28)*
1/*SE* (H_0: β_0=0)	-0.00201**	-0.000619*	-0.00138**	-0.00154**
	(-2.53)	*-1.809*	*(-2.36)*	*(-2.13)*
K	62	62	62	62
R^2	0.235	0.184	-	-

Hypothesis 1.2. Published articles suffer a publication bias, towards negative market reactions to financial crimes.

(a) FAT-PET test (Equation: $AARD = \beta_0 + \beta_1 SE + \varepsilon$)

	Published articles			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Cluster-robust random-effects panel GLS[e]	Cluster-robust fixed-effects panel LSDV[f]
Model	[1]	[2]	[3]	[4]
Intercept (PET: H_0: β_0=0)	-0.00540***	-0.000919	-0.00793**	-0.00656***
	(-3.85)	*-1.457*	*(-2.23)*	*(-3.90)*
SE (FAT: H_0: β_1=0)	-1.605***	-2.657***	-1.322***	-1.553***
	(-7.69)	*-10.16*	*(-3.34)*	*(-6.65)*
K	351	351	351	351
R^2	0.603	0.259	0.334	0.6031

	Working papers			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Cluster-robust random-effects panel GLS[g]	Cluster-robust fixed-effects panel LSDV[h]
Model	[1]	[2]	[3]	[4]
Intercept (PET: H_0: β_0=0)	-0.00988***	0.00124	-0.0154***	-0.0223***
	(-2.88)	*1.694*	*(-11.88)*	*(-3.41)*
SE (FAT: H_0: β_1=0)	-1.289	-5.713***	-0.0581	-0.532
	(-1.43)	*-3.538*	*(-0.20)*	*(-0.96)*
K	88	88	88	88
R^2	0.099	0.218	0	0.0989

(b) PEESE approach (Equation: $t\_stat\_wcs = \beta_1 SE + \beta_0 (1/SE) + \varepsilon$)

	Published articles			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Random effects panel ML	Population-averaged panel GEE
Model	[5]	[6]	[7]	[8]
SE	-58.76***	-1,088***	-25.94*	-41.62***
	(-5.01)	*-5.338*	*(-1.71)*	*(-4.50)*
1/*SE* (H_0: β_0=0)	-0.00320***	-0.00118***	-0.00118***	-0.00191***
	(-4.28)	*-3.189*	*(-3.17)*	*(-3.63)*
K	351	351	351	351
R^2	0.301	0.306	-	-

	Working papers			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Random effects panel ML	Population-averaged panel GEE
Model	[5]	[6]	[7]	[8]
SE	-195.8*	-2,176**	-75.95	-49.81
	(-1.94)	*-2.356*	*(-0.70)*	*(-0.60)*
1/*SE* (H_0: β_0=0)	-0.00171***	-0.000288	-0.000178	0.0000941
	(-3.17)	*-1.366*	*(-0.18)*	*(0.08)*
K	88	88	88	88
R^2	0.138	0.2	-	-

Hypothesis 2. Public and private enforcement triggers different market reactions.

(a) FAT-PET test (Equation: $AARD = \beta_0 + \beta_1 SE + \varepsilon$)

	Public enforcement			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Cluster-robust random-effects panel GLS[i]	Cluster-robust fixed-effects panel LSDV[j]
Model	[1]	[2]	[3]	[4]
Intercept (PET: H_0: β_0=0)	-0.00617***	0.000149	-0.0113***	-0.0113***
	(-3.48)	*0.358*	*(-3.06)*	*(-3.78)*
SE (FAT: H_0: β_1=0)	-1.441***	-2.804***	-0.885**	-1.208***
	(-6.47)	*-8.462*	*(-2.22)*	*(-3.65)*
K	228	228	228	228
R^2	0.499	0.297	0.176	0.4986

	Private enforcement			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Cluster-robust random-effects panel GLS[k]	Cluster-robust fixed-effects panel LSDV[l]
Model	[1]	[2]	[3]	[4]
Intercept (PET: H_0: β_0=0)	-0.00328**	-0.0000639	-0.00282**	-0.00356**
	(-2.08)	*-0.107*	*(-2.19)*	*(-1.97)*
SE (FAT: H_0: β_1=0)	-2.283***	-4.006***	-2.368***	-2.274***
	(-11.59)	*-5.511*	*(-10.21)*	*(-10.93)*
K	182	182	182	182
R^2	0.689	0.203	0.541	0.6892

(b) PEESE approach (Equation: $t\_stat\_wcs = \beta_1 SE + \beta_0 (1/SE) + \varepsilon$)

	Public enforcement			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Random effects panel ML	Population-averaged panel GEE
Model	[5]	[6]	[7]	[8]
SE	-52.92***	-881.3***	-9.787	-40.95***
	(-3.88)	*-3.554*	*(-0.63)*	*(-3.32)*
1/*SE* (H_0: β_0=0)	-0.00184***	-0.000720***	-0.000388	-0.00133***
	(-3.40)	*-3.992*	*(-1.21)*	*(-3.74)*
K	228	228	228	228
R^2	0.194	0.319	-	-

	Private enforcement			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Random effects panel ML	Population-averaged panel GEE
Model	[5]	[6]	[7]	[8]
SE	-117.2***	-2,014***	-71.52	-73.30***
	(-4.79)	*-3.565*	*(-1.62)*	*(-3.02)*
1/*SE* (H_0: β_0=0)	-0.00251***	-0.000608**	-0.000988	-0.00105
	(-3.47)	*-2.521*	*(-1.47)*	*(-1.02)*
K	182	182	182	182
R^2	0.227	0.228	-	-

Hypothesis 3. The market differentiates depending on the types of financial crimes.

(a) FAT-PET test (Equation: $AARD = \beta_0 + \beta_1 SE + \varepsilon$)

	Only accounting frauds			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Cluster-robust random-effects panel GLS[m]	Cluster-robust fixed-effects panel LSDV[n]
Model	[1]	[2]	[3]	[4]
Intercept (PET: H_0: β_0=0)	-0.00837***	-0.000104	-0.00988**	-0.01000***
	(-3.12)	*-0.136*	*(-2.46)*	*(-3.82)*
SE (FAT: H_0: β_1=0)	-1.709***	-3.446***	-1.584***	-1.667***
	(-8.66)	*-6.099*	*(-4.78)*	*(-8.13)*
K	142	142	142	142
R^2	0.615	0.294	0.455	0.6148

	Other securities laws frauds			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Cluster-robust random-effects panel GLS[o]	Cluster-robust fixed-effects panel LSDV[p]
Model	[1]	[2]	[3]	[4]
Intercept (PET: H_0: β_0=0)	-0.00580***	-0.000457	-0.00742**	-0.00742***
	(-4.04)	*-0.92*	*(-2.06)*	*(-2.73)*
SE (FAT: H_0: β_1=0)	-1.357***	-3.096***	-1.092*	-1.232***
	(-4.61)	*-7.036*	*(-1.86)*	*(-2.68)*
K	297	297	297	297
R^2	0.39	0.18	0.178	0.3895

(b) PEESE approach (Equation: $t\_stat\_wcs = \beta_1 SE + \beta_0 (1/SE) + \varepsilon$)

	Only accounting frauds			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Random effects panel ML	Population-averaged panel GEE
Model	[5]	[6]	[7]	[8]
SE	-66.19***	-998.0***	-40.23*	-44.61***
	(-5.14)	*-2.83*	*(-1.76)*	*(-5.03)*
1/*SE* (H_0: β_0=0)	-0.00252**	-0.000901***	-0.00117*	-0.00139***
	(-2.36)	*-2.878*	*(-1.77)*	*(-2.97)*
K	142	142	142	142
R^2	0.194	0.313	-	-

	Other securities laws frauds			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Random effects panel ML	Population-averaged panel GEE
Model	[5]	[6]	[7]	[8]
SE	-68.29***	-1,504***	-39.27	-34.69**
	(-2.70)	*-4.46*	*(-1.44)*	*(-2.01)*
1/*SE* (H_0: β_0=0)	-0.00266***	-0.000717***	-0.00102**	-0.000878
	(-4.40)	*-3.03*	*(-2.15)*	*(-1.09)*
K	297	297	297	297
R^2	0.236	0.23	-	-

Hypothesis 4. The market accounts for the suspicion or the condemnation of financial crimes.

(a) FAT-PET test (Equation: $AARD = \beta_0 + \beta_1 SE + \varepsilon$)

	Alleged crimes and lawsuit filings			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Cluster-robust random-effects panel GLS[q]	Cluster-robust fixed-effects panel LSDV[r]
Model	[1]	[2]	[3]	[4]
Intercept (PET: H_0: β_0=0)	-0.00654*** [5]	-0.00257*** [7]	-0.00785**	-0.00926***
	(-3.82)	*-2.769*	*(-2.44)*	*(-3.80)*
SE (FAT: H_0: β_1=0)	-1.914*** [6]	-3.100*** [8]	-1.760***	-1.863***
	(-10.55)	*-6.745*	*(-4.70)*	*(-7.74)*
K	266	266	266	266
R^2	0.592	0.166	0.418	0.592

	Condemned crimes (settlements, condemnations, accounting restatements)			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Cluster-robust random-effects panel GLS[r]	Cluster-robust fixed-effects panel LSDV[t]
Model	[1]	[2]	[3]	[4]
Intercept (PET: H_0: β_0=0)	-0.00498*** [9]	0.000372 [11]	-0.0123***	-0.0106***
	(-3.36)	*1.114*	*(-6.55)*	*(-3.92)*
SE (FAT: H_0: β_1=0)	-1.142*** [10]	-2.572*** [12]	-0.134	-0.790**
	(-5.04)	*-7.139*	*(-0.52)*	*(-2.49)*
K	179	179	179	179
R^2	0.435	0.256	0.006	0.4348

(b) PEESE approach (Equation: $t\_stat\_wcs = \beta_1 SE + \beta_0 (1/SE) + \varepsilon$)

	Alleged crimes and lawsuit filings			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Random effects panel ML	Population-averaged panel GEE
Model	[5]	[6]	[7]	[8]
SE	-74.65***	-1,295***	-55.88**	-61.63***
	(-5.45)	*-4.852*	*(-2.22)*	*(-4.81)*
1/*SE* (H_0: β_0=0)	-0.00522***	-0.00210***	-0.00259***	-0.00295***
	(-5.69)	*-2.687*	*(-3.87)*	*(-3.47)*
K	266	266	266	266
R^2	0.354	0.353	-	-

	Condemned crimes (settlements, condemnations, accounting restatements)			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Random effects panel ML	Population-averaged panel GEE
Model	[5]	[6]	[7]	[8]
SE	-48.51***	-975.0***	-1.492	-34.78**
	(-2.75)	*-2.997*	*(-0.08)*	*(-2.24)*
1/*SE* (H_0: β_0=0)	-0.00127***	-0.000435**	-0.000106	-0.000775***
	(-2.86)	*-2.274*	*(-0.38)*	*(-3.19)*
K	179	179	179	179
R^2	0.153	0.241	-	-

Hypothesis 5. The market differentiates depending on the source of information of the financial crime.

(a) FAT-PET test (Equation: $AARD = \beta_0 + \beta_1 SE + \varepsilon$)

	Press			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Cluster-robust random-effects panel GLS[u]	Cluster-robust fixed-effects panel LSDV[v]
Model	[1]	[2]	[3]	[4]
Intercept (PET: H_0: β_0=0)	-0.00495**	0.000194	-0.00583	-0.00664***
	(-2.52)	*0.355*	*(-1.35)*	*(-2.76)*
SE (FAT: H_0: β_1=0)	-1.734***	-3.218***	-1.617***	-1.684***
	(-5.27)	*-5.551*	*(-2.82)*	*(-4.74)*
K	175	175	175	175
R^2	0.47	0.213	0.354	0.4701

	Other sources (regulatory and corporate)			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Cluster-robust random-effects panel GLS[w]	Cluster-robust fixed-effects panel LSDV[x]
Model	[1]	[2]	[3]	[4]
Intercept (PET: H_0: β_0=0)	-0.00674***	-0.000616	-0.0116***	-0.0117***
	(-4.33)	*-1.029*	*(-3.30)*	*(-3.81)*
SE (FAT: H_0: β_1=0)	-1.525***	-3.246***	-0.941**	-1.279***
	(-6.44)	*-8.1*	*(-2.25)*	*(-3.86)*
K	264	264	264	264
R^2	0.541	0.219	0.194	0.5407

(b) PEESE approach (Equation: $t\_stat\_wcs = \beta_1 SE + \beta_0 (1/SE) + \varepsilon$)

	Press			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Random effects panel ML	Population-averaged panel GEE
Model	[5]	[6]	[7]	[8]
SE	-74.65***	-1,295***	-55.88**	-61.63***
	(-5.45)	*-4.852*	*(-2.22)*	*(-4.81)*
1/*SE* (H_0: β_0=0)	-0.00522***	-0.00210***	-0.00259***	-0.00295***
	(-5.69)	*-2.687*	*(-3.87)*	*(-3.47)*
K	266	266	266	266
R^2	0.354	0.353	-	-

	Other sources (regulatory and corporate)			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Random effects panel ML	Population-averaged panel GEE
Model	[5]	[6]	[7]	[8]
SE	-55.24***	-1,444***	-28.92	-40.29***
	(-4.10)	*-4.453*	*(-1.40)*	*(-3.48)*
1/*SE* (H_0: β_0=0)	-0.00292***	-0.00102***	-0.00144***	-0.00194***
	(-3.68)	*-3.334*	*(-3.05)*	*(-3.28)*
K	264	264	264	264
R^2	0.261	0.33	-	-

Hypothesis 6. Common law countries are more transparent and efficient markets, with stronger market reactions to financial crimes than in other jurisdictions.

(a) FAT-PET test (Equation: $AARD = \beta_0 + \beta_1 SE + \varepsilon$)

	Common law countries			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Cluster-robust random-effects panel GLS[y]	Cluster-robust fixed-effects panel LSDV[z]
Model	[1]	[2]	[3]	[4]
Intercept (PET: H_0: β_0=0)	-0.00873*** [13]	-0.00163** [15]	-0.0115***	-0.0111***
	(-4.55)	*-2.086*	*(-2.79)*	*(-4.28)*
SE (FAT: H_0: β_1=0)	-1.561*** [14]	-3.240*** [16]	-1.283***	-1.498***
	(-6.91)	*-6.898*	*(-3.11)*	*(-5.35)*
K	304	304	304	304
R^2	0.491	0.199	0.265	0.4911

	Rest of the world			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Cluster-robust random-effects panel GLS[aa]	Cluster-robust fixed-effects panel LSDV[ab]
Model	[1]	[2]	[3]	[4]
Intercept (PET: H_0: β_0=0)	-0.00239* [17]	0.000427 [19]	-0.00420*	-0.00427**
	(-1.77)	*1.147*	*(-2.03)*	*(-2.16)*
SE (FAT: H_0: β_1=0)	-1.203*** [18]	-2.623*** [20]	-0.725	-1.017**
	(-2.96)	*-5.054*	*(-1.33)*	*(-2.12)*
K	135	135	135	135
R^2	0.543	0.284	0.192	0.5435

(b) PEESE approach (Equation: $t\_stat\_wcs = \beta_1 SE + \beta_0 (1/SE) + \varepsilon$)

	Common law countries			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Random effects panel ML	Population-averaged panel GEE
Model	[5]	[6]	[7]	[8]
SE	-65.17***	-1,393***	-37.42*	-48.42***
	(-4.61)	-4.075	(-1.72)	(-4.31)
1/*SE* (H_0: β_0=0)	-0.00404***	-0.00119**	-0.000983	-0.00163
	(-4.46)	-2.427	(-1.61)	(-1.53)
K	304	304	304	304
R^2	0.276	0.243	-	-

	Rest of the world			
Estimator	Cluster-robust OLS	Cluster-robust WLS	Random effects panel ML	Population-averaged panel GEE
Model	[5]	[6]	[7]	[8]
SE	-59.58*	-1,117***	-33.76	-40.50**
	(-1.98)	-4.253	(-1.01)	(-2.26)
1/*SE* (H_0: β_0=0)	-0.00137***	-0.000474***	-0.000988***	-0.00114***
	(-3.77)	-3.701	(-2.60)	(-3.30)
K	135	135	135	135
R^2	0.206	0.435	-	-

*Source: Authors' estimations. Figures in parentheses beneath the regression coefficients are t-statistics using standard errors adjusted for clustering. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.*

Notes:

[a] Breusch-Pagan test: χ^2=40.26, p=0.0000; [b] Hausman test: χ^2=9.88, p=0.0017; [c] Breusch-Pagan test: χ2=4.79, p=0.0143; [d] Hausman test: χ2=0.97, p=0.3244; [e] Breusch-Pagan test: χ2=9.32, p=0.0011; [f] Hausman test: χ2=12.72, p=0.0004; [g] Breusch-Pagan test: χ2=10.61, p=0.0006; [h] Hausman test: χ2=0.45, p=0.5008; [i] Breusch-Pagan test: χ2=32.67, p=0.0000; [j] Hausman test: χ2=0.39, p=0.5307; [k] Breusch-Pagan test: χ2=5.66, p=0.0087; [l] Hausman test: χ2=2.46, p=0.1170; [m] Breusch-Pagan test: χ2=65.10, p=0.0000; [n] Hausman test: χ2=1.15, p=0.2826; [o] Breusch-Pagan test: χ2=0.99, p=0.1601; [p] Hausman test: χ2=50.37, p=0.0000; [q] Breusch-Pagan test: χ2=38.26, p=0.0000; [r] Hausman test: χ2=0.29, p=0.5897; [s] Breusch-Pagan test: χ2=10.54, p=0.0006; [t] Hausman test: χ2=15.75, p=0.0001; [u] Breusch-Pagan test: χ2=27.89, p=0.0000; [v] Hausman test: χ2=4.76, p=0.0290; [w] Breusch-Pagan test: χ2=6.76, p=0.0047; [x] Hausman test: χ2=10.43, p=0.0012; [y] Breusch-Pagan test: χ2=38.80, p=0.0000; [z] Hausman test: χ2=7.11, p=0.0077; [aa] Breusch-Pagan test: χ2=4.99, p=0.0128; [ab] Hausman test: χ2=12.17, p=0.0005.

Wild bootstrap clustering (999 replications) with 95% confidence intervals: full sample: [1] [-0.009 ; -0.003]; [2] [-2.120 ; -1.171]; [3] [-0.005 ; -0.0009]; [4] [-2.356 ; -1.432]; alleged crimes: [5] [-0.010 ; -0.003]; [6] [-2.415 ; -1.529]; [7] [-0.010 ; -0.003]; [8] [-2.46 ; -1.446]; condemned crimes: [9] [-0.007 ; -0.002]; [10] [-1.972 ; -0.159]; [11] [-0.003 ; 0.001]; [12] [-2.159 ; -0.809]; common law countries: [13] [-0.013 ; -0.005]; [14] [-2.207 ; -1.059]; [15] [-0.009 ; -0.002]; [16] [-2.334 ; -1.379]; rest of the world: [17] [-0.006 ; 0.001]; [18] [-1.861 ; -0.105]; [19] [-0.004 ; 0.001]; [20] [-2.634 ; -0.424].

Table 5: Meta-Regression Analysis

Table 5 details the estimates the MRA based on Eq. (1), echoing the hypotheses tested in the article (see reference into parenthesis). All estimations are cluster-robust (Stanley and Doucouliagos, 2012), to account for the data dependence due to having on average 4 estimates *per* study in the sample. Additionally, every column uses a specific estimation method used: Ordinary Least Squares for sample [column 1], Weighted Least Squares (columns [2] to [5]), random effects panel with Generalized Least Squares (column [6]) and fixed effects panel with least squares dummy variable (column [7]). The analytical weights used for WLS are detailed into brackets for every column: the quality level of the article, with the number of Google citations (column [2]), the sample size for every study (column [3]), the precision of the estimates (sample [4]), and the invert of the number of estimates *per* study (sample [5]).

Estimator (Analytical weight in brackets)	**Cluster-robust OLS**	**Cluster-robust WLS [quality level]**	**Cluster-robust WLS [sample size]**	**Cluster-robust WLS [1/SE^2]**	**Cluster-robust WLS [1/nb estimates *per* study]**	**Cluster-robust random effects panel GLS**	**Cluster-robust fixed effects panel LSDV**
	[1]	[2]	[3]	[4]	[5]	[6][a]	[7][b]
Constant *(H1.1)*	1.058**	0.918	0.186	0.450***	0.265	0.834*	-0.115**
	(2.42)	*(1.55)*	*(0.18)*	*(2.75)*	*(0.49)*	*(1.80)*	*(-2.14)*
Standard error *(H1.2)*	-1.393***	-1.780***	-1.421***	-2.578***	-1.471***	-1.363***	-1.115***
	(-8.10)	*(-10.69)*	*(-6.76)*	*(-9.85)*	*(-8.73)*	*(-7.08)*	*(-3.44)*
1. Data characteristics:							
Regulatory procedures *(H2)*	0.000	0.001	-0.003	0.000	-0.001	-0.001	0.001
	(0.15)	*(0.30)*	*(-0.72)*	*(-0.01)*	*(-0.17)*	*(-0.22)*	*(0.24)*
Only accounting crimes *(H3)*	-0.006**	-0.008***	-0.004	-0.002	-0.009***	-0.006*	0.003
	(-2.12)	*(-2.78)*	*(-0.96)*	*(-1.34)*	*(-2.76)*	*(-1.88)*	*(0.45)*
Alleged crimes *(H4)*	-0.011***	-0.007*	-0.015***	-0.006***	-0.011***	-0.010***	-0.012***
	(-4.18)	*(-1.93)*	*(-4.39)*	*(-4.70)*	*(-3.15)*	*(-4.09)*	*(-3.01)*
Crimes revealed by newspaper articles *(H5)*	-0.001	-0.006	0.003	0.001	0.002	-0.002	-0.017*
	(-0.37)	*(-0.94)*	*(0.51)*	*(0.79)*	*(0.58)*	*(-0.55)*	*(-1.75)*
Studies on a common law country *(H6)*	-0.008***	0.000	-0.013**	-0.002	-0.007*	-0.008**	0.006***
	(-2.71)	*(-0.01)*	*(-2.17)*	*(-1.20)*	*(-1.72)*	*(-2.44)*	*(10.74)*
Average year of the data in the sample[d] *(H7)*	-1.040**	-0.875	-0.188	-0.440***	-0.261	-0.819*	dropped
	(-2.41)	*(-1.48)*	*(-0.19)*	*(-2.68)*	*(-0.49)*	*(-1.80)*	
Number of years in the sample[d]	0.002	-0.002	0.001	0.001	0.002	0.002	dropped
	(0.72)	*(-0.46)*	*(0.12)*	*(0.80)*	*(0.45)*	*(0.56)*	
Main sector: industry	-0.003	0.005	-0.002	-0.003*	-0.002	-0.003	dropped
	(-1.18)	*(1.08)*	*(-0.48)*	*(-1.87)*	*(-0.49)*	*(-1.03)*	
Main sector: finance	0.002	0.006	-0.005	-0.001	-0.002	0.002	dropped
	(0.55)	*(1.27)*	*(-0.72)*	*(-0.68)*	*(-0.39)*	*(0.58)*	
2. Estimation characteristics:							
Market model used in the event study	0.000	0.004	0.005	-0.003**	0.006	0.001	dropped
	(-0.15)	*(0.88)*	*(0.91)*	*(-2.37)*	*(1.01)*	*(0.27)*	
Sample size[d]	-0.001*	-0.004***	0.000	0.000	-0.001	-0.002*	-0.003**
	(-1.67)	*(-2.74)*	*(0.49)*	*(0.06)*	*(-0.61)*	*(-1.74)*	*(-2.22)*
Length of the event window of the estimate[d]	0.002***	0.005***	0.002*	0.0006**	0.002	0.002***	0.007
	(3.41)	*(3.89)*	*(1.85)*	*(2.14)*	*(1.62)*	*(3.80)*	*(0.15)*
Long-term estimates (event windows beyond -10 and +10 days)	0.001	-0.017***	0.004	0.002**	0.001	0.000	dropped
	(0.34)	*(-2.82)*	*(1.19)*	*(2.20)*	*(0.32)*	*(0.17)*	
Event window = event day *(H1.3)*	-0.015***	-0.020**	-0.010	-0.002	-0.019***	-0.013***	-0.011***
	(-3.51)	*(-2.07)*	*(-0.96)*	*(-1.50)*	*(-3.34)*	*(-3.37)*	*(-2.75)*
Event window including the event day (but not limited to the event day) *(H1.3)*	-0.013***	-0.017***	-0.013***	-0.002**	-0.013***	-0.013***	-0.012***
	(-4.58)	*(-2.63)*	*(-2.83)*	*(-2.26)*	*(-3.81)*	*(-4.74)*	*(-4.15)*
Cross-sectional regression	-0.005*	-0.001	-0.001	-0.002	-0.002	-0.003	1.865***
	(-1.93)	*(-0.33)*	*(-0.19)*	*(-1.34)*	*(-0.56)*	*(-1.37)*	*(2.78)*
Reputational penalty estimation	0.003	0.003	-0.004	0.002	0.003	0.004	dropped
	(1.12)	*(0.61)*	*(-0.60)*	*(1.10)*	*(0.78)*	*(1.26)*	

Estimator (Analytical weight in brackets)	Cluster-robust OLS	Cluster-robust WLS [quality level]	Cluster-robust WLS [sample size]	Cluster-robust WLS [1/SE^2]	Cluster-robust WLS [1/nb estimates *per* study]	Cluster-robust random effects panel GLS	Cluster-robust fixed effects panel LSDV
	[1]	[2]	[3]	[4]	[5]	[6][a]	[7][b]
3. Publication characteristics:							
Number of authors[d]	-0.002	-0.002	-0.005	0.002	-0.007	-0.003	dropped
	(-0.55)	*(-0.39)*	*(-0.86)*	*(1.30)*	*(-1.50)*	*(-0.82)*	
Multiple authorship in the sample	-0.003	0.002	-0.002	0.001	-0.004	-0.004	dropped
	(-0.96)	*(0.52)*	*(-0.35)*	*(1.02)*	*(-1.40)*	*(-1.30)*	
Publication in a refereed journal *(H1.2)*	0.002	0.010	0.004	0.003	0.001	0.001	dropped
	(0.62)	*(1.55)*	*(0.71)*	*(1.42)*	*(0.25)*	*(0.35)*	
Number of citations in Google Scholar[d] *(H1.2)*	-0.001**		-0.001	-0.001**	-0.000	-0.001**	0.122**
	(-2.38)		*(-1.15)*	*(-2.03)*	*(-0.27)*	*(-2.33)*	*(2.18)*
Publication in a cross-disciplinary journal *(H1.2)*	-0.001	-0.026***	0.007	-0.006*	0.010	0.002	dropped
	(-0.28)	*(-3.03)*	*(0.67)*	*(-1.87)*	*(1.34)*	*(0.33)*	
Number of pages of the article[d]	-0.002	-0.007*	-0.001	-0.002	-0.002	-0.002	dropped
	(-0.82)	*(-1.89)*	*(-0.18)*	*(-1.42)*	*(-0.67)*	*(-0.81)*	
4. Control variables:							
Wealth level (log GDP *per capita*)	-0.001	0.001	-0.001	-0.001	0.000	-0.001*	0.000
	(-1.35)	*(1.10)*	*(-0.94)*	*(-1.65)*	*(-0.66)*	*(-1.92)*	*(0.45)*
Market liquidity[d]	0.007**	-0.005	0.010	0.001	0.007*	0.006**	
	(2.28)	*(-0.88)*	*(1.50)*	*(0.61)*	*(1.81)*	*(2.18)*	
Rule of law	0.000	-0.004	0.005	0.001	0.001	0.000	dropped
	(-0.02)	*(-1.10)*	*(1.52)*	*(1.18)*	*(0.47)*	*(0.13)*	
Adj. R2	0.617	0.833	0.615	0.400	0.647	-	0.373
Number of observations	439	405	439	439	439	439	439

Source: Authors' estimations. See Table 3 for definitions and descriptive statistics of the meta-independent variables.
Notes:
*Figures in parentheses beneath the regression coefficients are t-statistics using cluster-robust standard errors. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.*
[a] *Breusch-Pagan test: $\chi 2 = 4.33, p = 0.0188$.*
[b] *Hausman test: $\chi 2 = 29.26, p = 0.0060$.*
[c] *The limited scope is due to the exclusion of working papers.*
[d] *Normalized variables.*

Table 6: Meta-Average Effects of Financial Crime by Sub-Samples

Table 6 details the estimated $AARDs$ after the publication of financial crimes for the full sample and for three sets of sub-samples (alleged or condemned crimes, common law countries or rest of the world, and accounting frauds or other frauds to securities laws). The first four columns use the most statistically and economically significant MRA coefficients to assess the meta-average effect, from cluster-robust Weighted Least Squares from Table 5 (estimators [2] to [5]). The fifth column is the average of the estimated effects for the four estimators. Statistically significance effects are denoted with stars. The last two columns are the average 95% confidence intervals using cluster-adjusted standard errors.

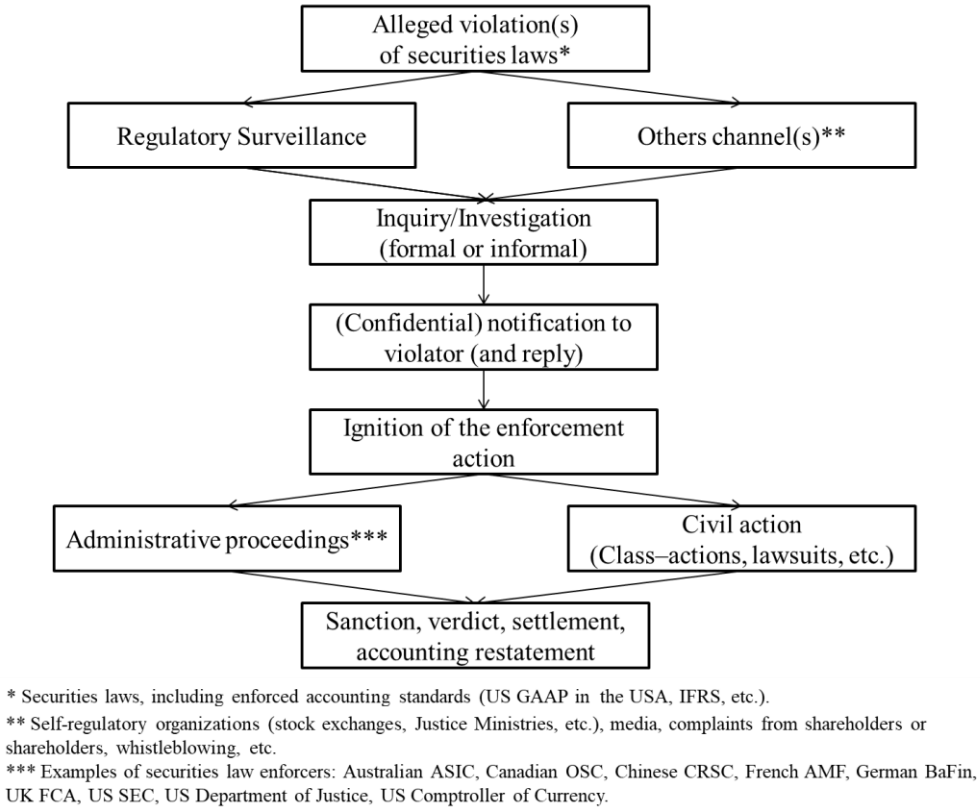
The variables chosen are the following: type of law (common versus code), the average year of the sampled data, the type of crime (accounting or securities regulations), the sources of news (newspaper articles or firms/regulators), the allegation (or condemnation of crime), the use of market models, whether the firms were mostly industrial, the size of the sample of crimes, the length of the event window, whether the event is included in the event window, whether the article included longer-term $(C)AARs$, whether the study included cross-sectional regressions, echo received by peers (Google citations), the length of the article, the publication in a cross-disciplinary journal, the log GDP growth rate, and market liquidity of the concerned market.

	Cluster-robust WLS [quality level]	**Cluster-robust WLS [sample size]**	**Cluster-robust WLS [1/SE^2]**	**Cluster-robust WLS [1/nb estimates *per* study]**	**Averages**	***Average 95% confidence intervals***	
	[2]	[3]	[4]	[5]	[2 - 5]	[2 - 5]	
Full sample	-1.52%**	-1.18%	-0.33%**	-1.52%	-1.14%	*-2.49%*	*0.47%*
	(-2.30)	*(-0.92)*	*(-2.33)*	*(-0.54)*			
Alleged financial crimes	-1.94%***	-1.78%	-0.76%***	-1.94%	-1.61%	*-2.96%*	*0.09%*
	(-2.86)	*(-1.38)*	*(-2.71)*	*(-1.20)*			
Condemned financial crimes	-0.86%	-0.25%	0.32%	-0.86%	-0.41%	*-1.84%*	*1.13%*
	(-1.26)	*(-0.20)*	*(-1.58)*	*(0.51)*			
Financial crimes committed in a common law country	-1.74%**	-1.57%	-0.56%**	-1.74%	-1.40%	*-2.78%*	*0.30%*
	(-2.52)	*(-1.20)*	*(-2.35)*	*(-0.86)*			
Financial crimes committed in a civil law country	-1.01%	-0.30%	0.17%**	-1.01%	-0.54%	*-1.99%*	*0.99%*
	(-1.48)	*(-0.24)*	*(-2.12)*	*(0.28)*			
Accounting frauds only	-2.15%***	-1.46%	-0.97%***	-2.15%*	-1.68%	*-2.81%*	*-0.05%*
	(-3.50)	*(-1.25)*	*(-2.82)*	*(-1.64)*			
Securities frauds	-1.22%*	-1.05%	-0.03%**	-1.22%	-0.88%	*-2.37%*	*0.76%*
	(-1.71)	*(-0.78)*	*(-2.07)*	*(-0.04)*			

*Source: Authors' estimations. Figures in parentheses beneath the regression coefficients are t-statistics using cluster-robust standard errors. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.*

Figure 1: Common features of Financial Crime Prosecution

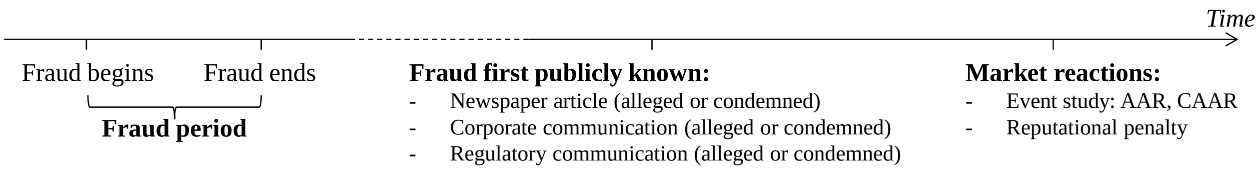
Figure 1 presents a simplified view of the consecutive steps of public or private prosecution for financial crimes. Most of code law countries (France, Germany, Italy, Spain, etc.) do not communicate any information before the sanction is pronounced. Conversely, the common law countries, and most frequently in the U.S., enforcers and defendants can communicate through official ways along the procedures. For example, for the U.S., the following steps were investigated by the literature: Accounting and Auditing Enforcement (AAER), and SEC formal or informal investigations and sanctions, Wells Notice issuance, sanctions by Department of Justice and Securities Exchange Commission, class action filing, and accounting restatement publications.



Source: Authors

Figure 2: Chronology of Financial Crimes

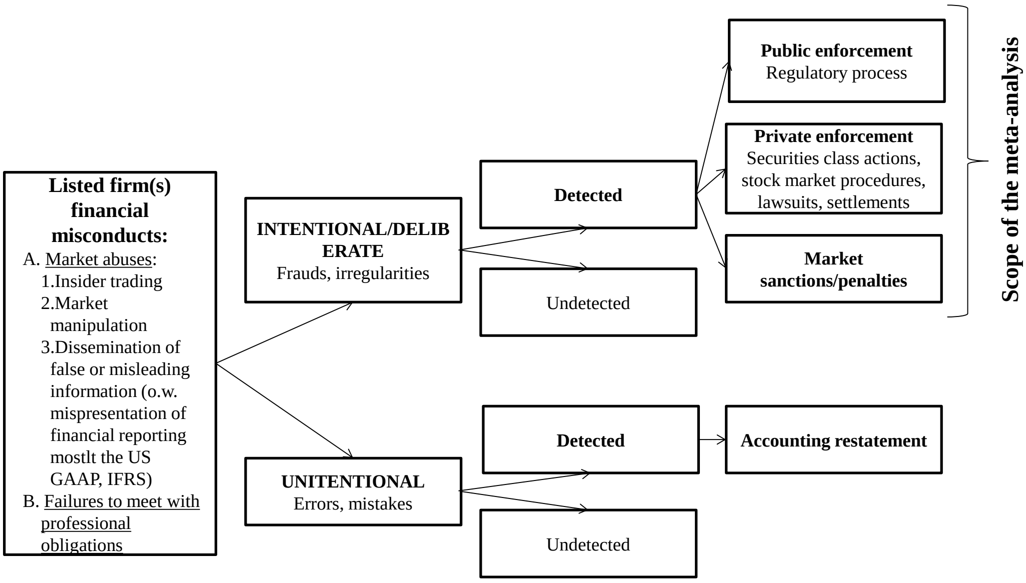
This figure shows the typical succession of events that lead to market reactions when learning about a corporate financial crime. The sequence of events is representative for most crimes in the scope, but may differ in certain cases.



Source: Authors

Figure 3: Graphical Presentation of the Scope of Meta-Analysis

This figure graphically describes the inclusion criteria into the meta-analysis. From a wide range of studies on financial crimes by listed firms, the scope was reduced to the literature investigating detected and intentional crimes and the subsequent market reaction, based on an event-study methodology. Financial crimes cover the following range of misconducts: 3 market abuses with insider trading (insider dealing, soundings, research), price manipulation (spoofing/layering, new issue/M&A support, ramping, squeeze/corner, bull/bear raids, circular trading,[18] improper order handling,[19] and improper price influence[20]), and the dissemination of false information (collusion and information sharing with pools and information disclosure; misleading customers with guarantees, window dressing, mis-presentation), to which add any breach to the regulation enforced and professional obligations for listed firms.



Source: Authors

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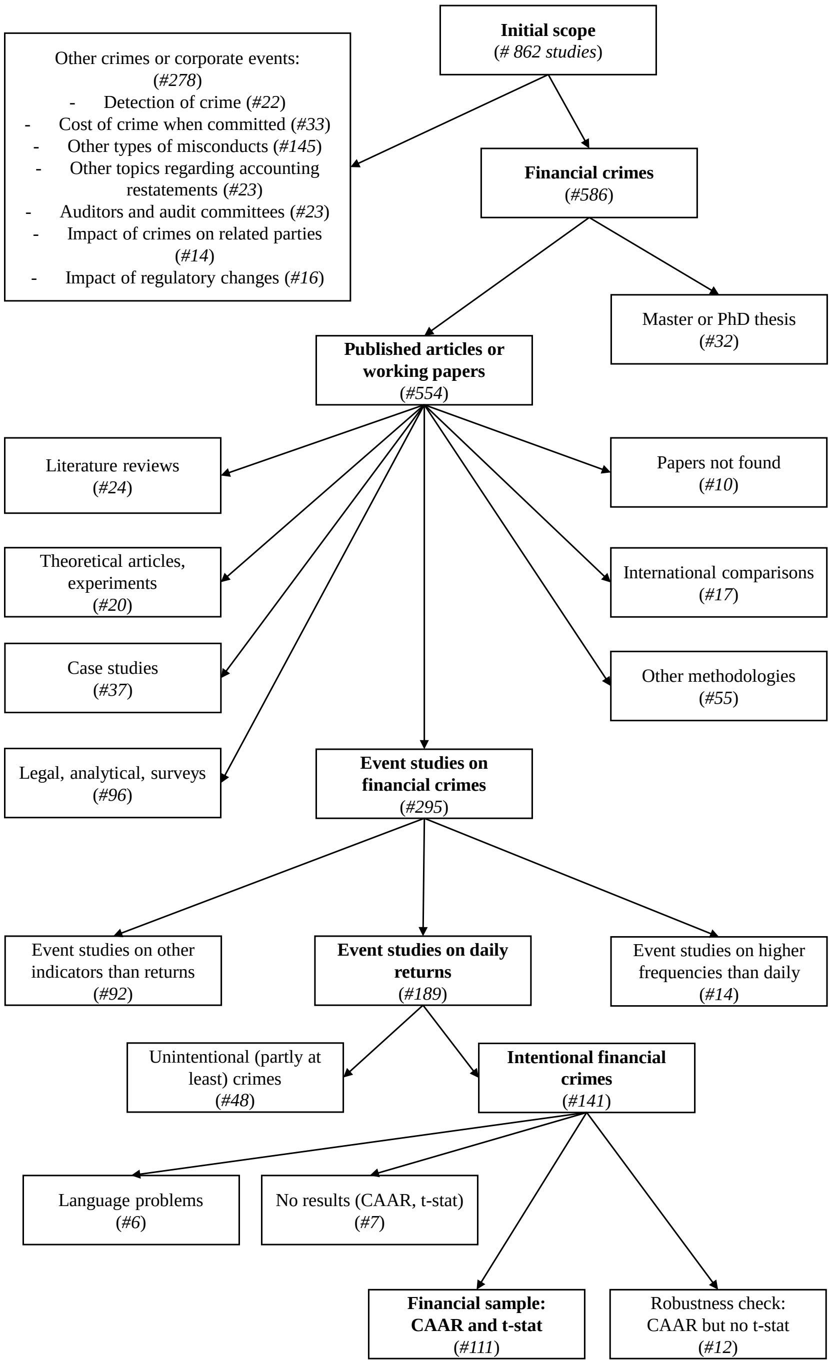
[18] Circular trading includes wash trades, matched trades, money pass and compensation trades, and parking/warehousing.

[19] Front running, cherry picking and partial fills, and stop losses and limits.

[20] Benchmarks, closing prices, reference prices, portfolio trades, and barriers.

Figure 4: PRISMA Statement

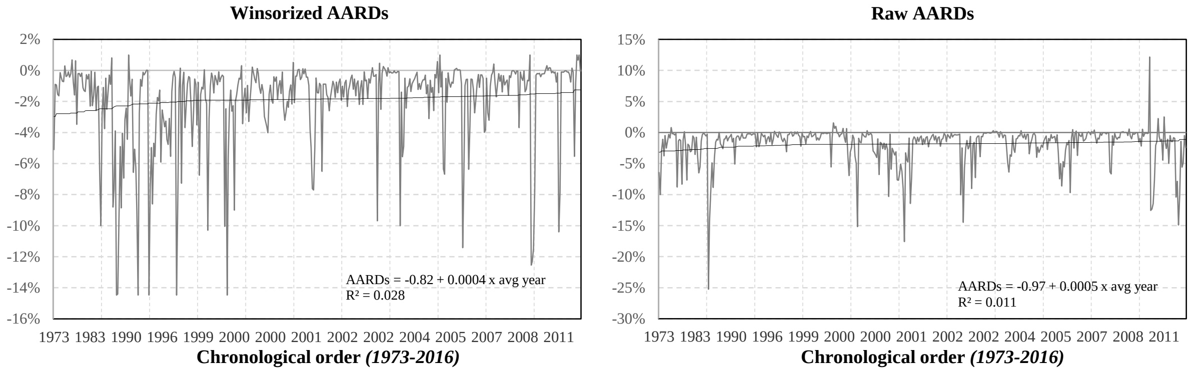
The following PRISMA flow diagram shows the details of the information flow in each stage of literature search in our meta-analysis, as recommended by Moher et al. (2009) and Havránek et al. (2020). From an initial sample of 862 studies reviewed, we end up with a 111 sample of articles, to which add 12 more articles for robustness checks, for which no details were given on the statistical significance. Details of each category is available on demand. Bold titles illustrate how we ended with the final sample. This graphical illustration has its limit as many studies cumulated reasons for being excluded but, for the sake to presentation, they were allocated into one category.



Source: Authors

Figure 5: Chronological Ordering of Winsorized AARDs

Figure 5 depicts graphically the chronological ordering of $AARDs$ for the whole sample, based on the average year of the sample data, ranging from 1973 to 2016. It compares raw data of $AARDs$, as extracted from the sampled articles, with winsorized $AARDs$, at the 1% level.

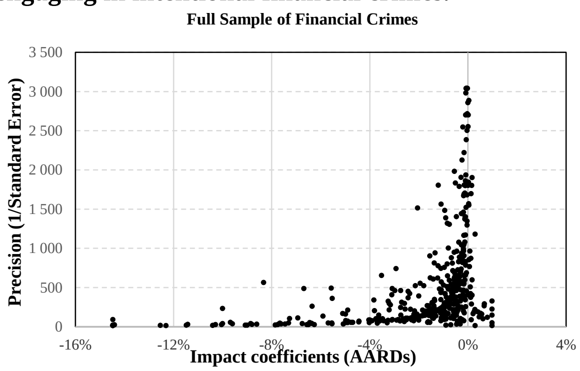


Source: Authors

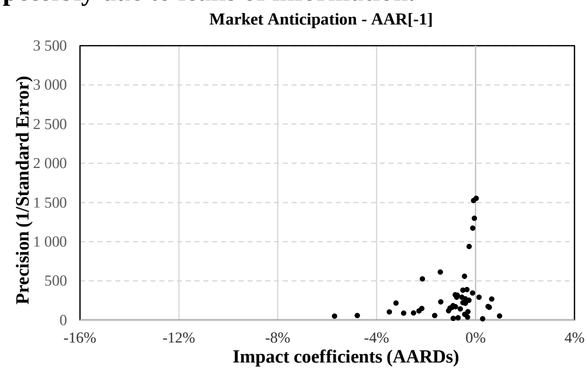
Figure 6: Funnel Graphs in the Impact of Financial Crimes

The following funnel graphs scatter the estimated average abnormal returns *per* day of the publication of financial crimes ($AARDs$) against these estimates' precisions (*i.e.* the inverse of the estimated standard errors). The first funnel graph includes the whole sample of 439 estimates, whereas the rest of the funnel graphs are done for different subsamples, echoing the different tested hypotheses. The distribution is expected to be symmetrical around the true value of the estimate, in the absence of publication bias.

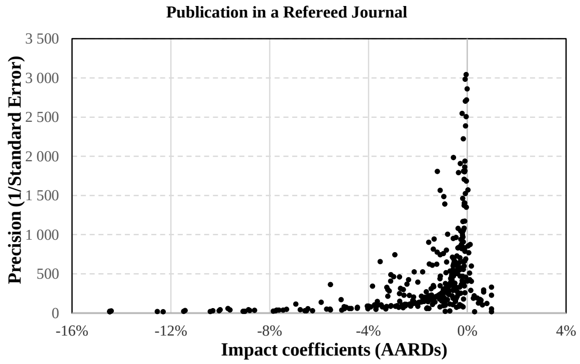
Hypothesis 1.1. Markets penalize listed firms for engaging in intentional financial crimes.

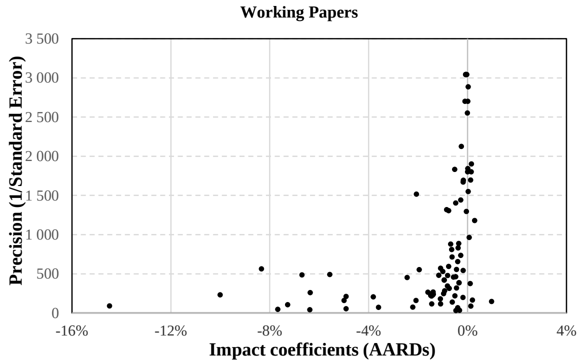


Hypothesis 1.3. Markets anticipate the events, possibly due to leaks of information.

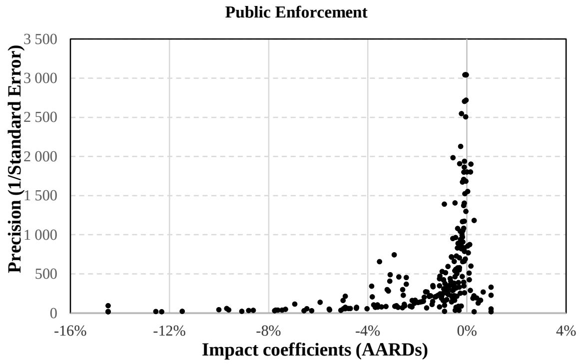


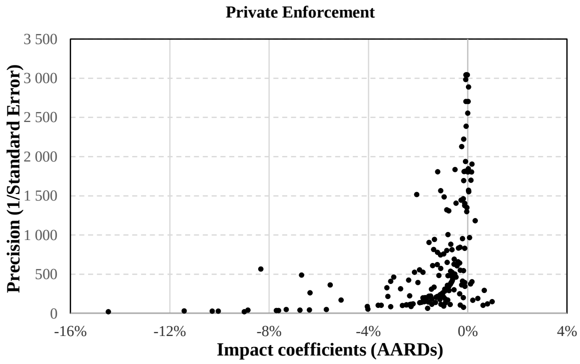
Hypothesis 1.2. Published articles suffer a publication bias, towards negative market reactions to financial crimes.



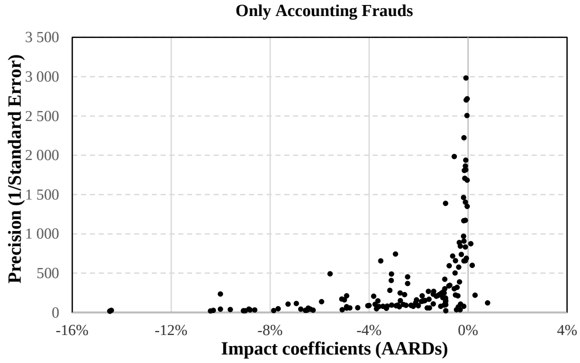


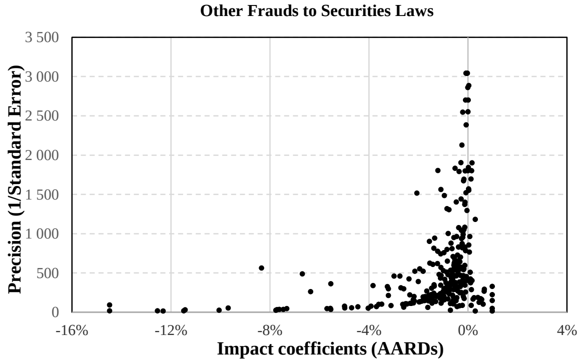
Hypothesis 2. Public and private enforcements trigger different market reactions.



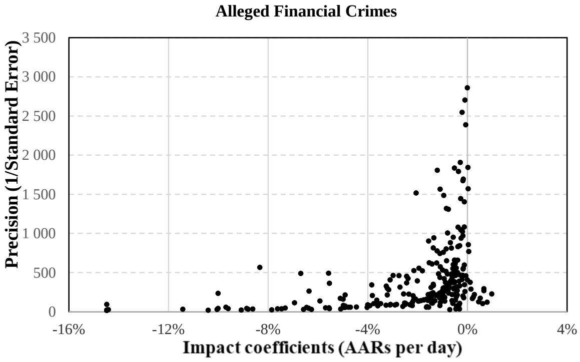


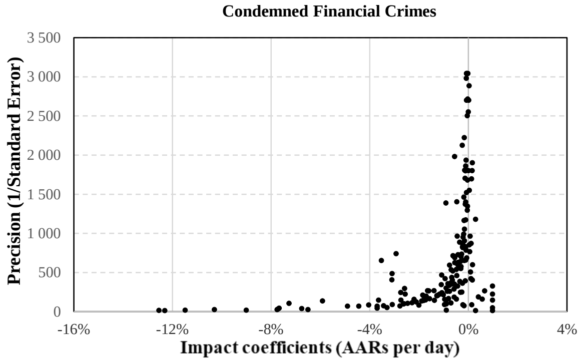
Hypothesis 3. Markets differentiate intentional financial crimes (pure accounting frauds, pure securities frauds, or both).



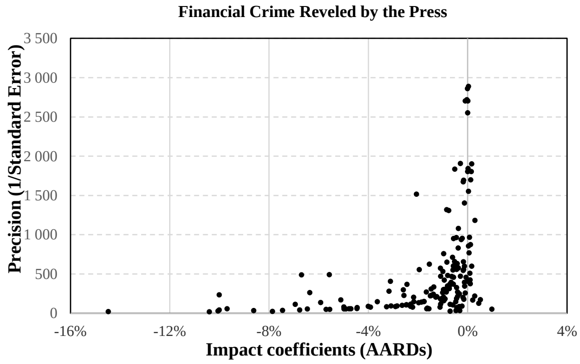


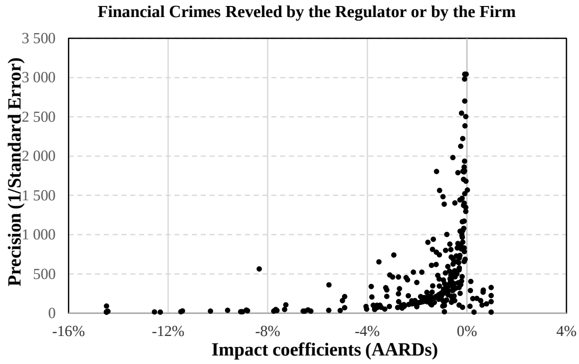
Hypothesis 4. Markets account for the suspicion (alleged crimes, lawsuit filings) or for the condemnation of financial crimes (settlements, condemnations, and accounting restatements).



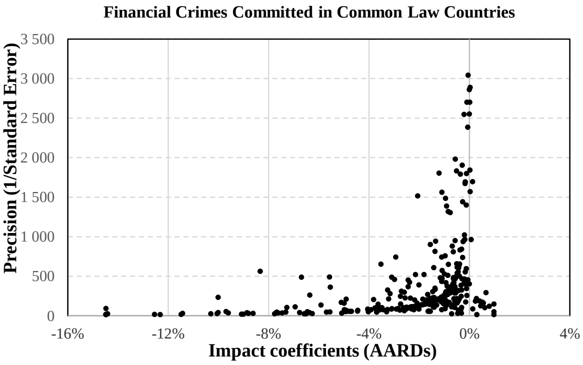


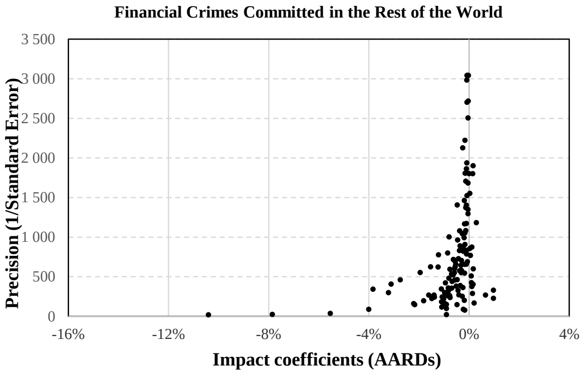
Hypothesis 5. Markets differentiate depending on the source of information (regulatory, corporate, or media) of the financial crime.





Hypothesis 6. Common law countries (the US in particular), being more transparent, trigger stronger market reactions to financial crimes than in other jurisdictions.





Source: Authors

Appendix A: Event Study Methodology

The event studies have been long used to challenge the information content of a wide range of corporate news, called "events" (for example Dolley (1933), MacKinlay (1997), and Kothari and Warner (2008)).[21] The goal is to quantify an "abnormal" market reaction following the event, by deduction estimated "normal" market parameters from "actual" observed market parameters. A wide range of impact measure variables were used: returns (the most frequent, on which this work focuses), bid-ask spread, volatility, turnover, clients, cost of financing (interest rates) and financing mix (debt versus equity), top management turnover, analysts forecasts, etc.

The impact of each event is measured as the abnormal returns. For every "event", the abnormality of daily returns is being tested over an event window, by comparing "actual" *ex-post* returns with "normal" returns. The latter are the expected returns without conditioning on the event occurring, estimated over an estimation window preceding the event window. The abnormal returns consecutive to a given step of the procedure are taken as unbiased estimates of the total financial consequences of the event.

The finance literature has considered several models of expected returns describes the behavior of returns, to sort out, to the maximum possible extent, changes in returns caused by the "event" itself, from those caused by any other unrelated movement in prices. The event is assumed exogenous with respect to the firm. They can be classified between statistical or economic models:

A. Statistical models:

- Constant-mean-return model: $R_{i,t} = \mu_i + \varepsilon_{i,t}$, where $R_{i,t}$ is the returns in *t* for the stock *i*, μ_i is the mean return of stock *i*, and $\varepsilon_{i,t}$ is the disturbance term.
- Market model (or single factor market model): $R_{i,t} = \alpha_i + \beta_i R_{m,t} + \varepsilon_{i,t}$, with $E(\varepsilon_{i,t}) = 0$ and $Var(\varepsilon_{i,t}) = \sigma_\varepsilon^2$, where $R_{i,t}$ and $R_{m,t}$ are the returns in *t* respectively on the stock *i*, and on the market portfolio. $\varepsilon_{i,t}$ is the zero-mean disturbance term. α_i, β_i, and σ_ε^2 are the firm-specific parameters of the model.
- Factor models: adding other factors than the market trend, for example a sector index (Sharpe, 1970).
- Market-adjusted-return model: restricted market model with $\alpha_i = 0$ and $\beta_i = 1$, when no data is available before the event for example.

B. Economic models:

- Capital Asset Pricing Model (CAPM): $R_{i,t} = R_f + \beta_i(R_{m,t} - R_f) + \varepsilon_{i,t}$, with $E(\varepsilon_{i,t}) = 0$ and $Var(\varepsilon_{i,t}) = \sigma_\varepsilon^2$, where R_f is the risk-free rate, $R_{i,t}$ and $R_{m,t}$ are the returns in *t* respectively on the stock *i*, and on the market portfolio. $\varepsilon_{i,t}$ is the zero-mean disturbance term. β_i, is the beta or systemic risk of stock *i*.
- Arbitrage Pricing Theory (Fama-French): $R_{i,t} = \delta_0 + \delta_{i,1}F_{1,t} + \delta_{i,2}F_{2,t} + \cdots + \delta_{i,n}F_{n,t} + \varepsilon_{i,t}$, where $F_{i,t}$, $i \in [\![1;n]\!]$, are the n factors that generate returns and $\delta_{i,y}, y \in [\![1;n]\!]$ are the factor loadings.

In the sample of this meta-analysis, by far the most frequently is the market model. It assumes a stable linear relation between the security return and the market return. It also hypotheses a jointly multivariate normal and temporally independent distribution of returns.

For a firm *i*, over the period τ, the abnormal returns are:

$$AR_{i,\tau} = R_{i,\tau} - E(R_{i,\tau}/X_\tau) \qquad (I)$$

$AR_{i,\tau}$, $R_{i,\tau}$, and $E(R_{i,\tau}/X_\tau)$ respectively capture abnormal, actual, and normal returns on the security *i* over τ, given the conditioning information X_τ for the normal performance model. Equity returns are defined as the daily log difference in value of the equity.

For every security *i* of sector *s*, the market model is in *t*:

$$R_{i,t} = \alpha_i + \beta_i R_{m,t} + \varepsilon_{i,t}, \text{ with } E(\varepsilon_{i,t}) = 0 \text{ and } Var(\varepsilon_{i,t}) = \sigma_\varepsilon^2 \qquad (II)$$

$R_{i,t}$ and $R_{m,t}$ are the returns in *t* respectively on the stock *i*, and on the market portfolio. $\varepsilon_{i,t}$ is the zero-mean disturbance term. α_i, β_i, and σ_ε^2 are the parameters of the model.

Under general conditions, abnormal returns parameters ($\widehat{\alpha_i}$ and $\widehat{\beta_i}$) are estimated for every event using the selected model over an estimation window preceding the event with Ordinary Least Squares, as recommended

[21] Event studies have been used for decades to assess market reactions to corporate misconducts ranging from product unsafety and product recalls (air crashes, drug recalls, product automobile recalls, other product recalls, etc.) to any kind of corporate malfeasance (bribery, criminal fraud, tax evasion, illegal political contributions, criminal antitrust violations and price fixing, employee discrimination, environment accidents, environment and wildlife offenses, business ethics, breach of contracts, misleading advertising, etc.) and financial misconducts (insider trading, accounting frauds, option backdating, etc.).

by MacKinlay (1997). On every day t of the event window, the deviation in an individual stock's daily return (typically including reinvested dividends) from what is expected based on Eq. (II) (*i.e.* the prediction error or "abnormal" returns) is taken as an unbiased estimate of the financial effects of the "event" on the stock *i* in *t:*

$$AR_{i,t} = R_{i,t} - \hat{\alpha}_i - \hat{\beta}_i R_{m,t} \qquad (III)$$

$R_{i,t}$ is the actual returns on the security i in t, and $AR_{i,t}$ is the estimated abnormal returns for the firm i in t. $\hat{\alpha}_i$, and $\hat{\beta}_i$ are the estimates of α_i and β_i, from Eq. (II) over the estimation window. Abnormal returns over the event window capture the impact of the event on the value of the firm, under the assumption that the event is exogenous with respect to the given security. Abnormal returns are calculated over an event window, including the event day ($t = 0$).

The market-adjusted model merely assumes the following: $AR_{i,t} = R_{i,t} - R_{m,t}$.

The event window can start before the event to investigate for potential anticipation by the market (following leaks of information over the days preceding the event for example). Its length can challenge the persistence over time of the price effect. Under the null hypothesis H_0, the "event" has no impact on the distribution of returns (mean or variance effect). Individual parametric t-statistics are calculated for each firm's abnormal return, and for every event day.

Abnormal returns must be aggregated to draw overall inferences for the event of interest, through time and across individual firms. In fact, on a case-by-case basis, the statistical significance is difficult to detect because of the volatility in firms' stock returns. Hence, abnormal returns are then cumulated over time ($CAR_{i,[t_1;t_2]}$) and averaged across the n victims to get the Cumulative Average Abnormal Returns ($\mathrm{CAAR}_{[t_1;t_2]}$) over the period $[t_1; t_2]$, including the event (Eq. (IV)). All events are treated as a group, for which p-value on the constant of the regression for every period gives the significance of the CAR across all sanctions, with robust standard errors.

$$CAAR_{[t_1;t_2]} = \frac{1}{n}\sum_{i=1}^{n} CAR_{i,[t_1;t_2]} = \frac{1}{n}\sum_{i=1}^{n}\sum_{t=t_1}^{t_2} AR_{i,t} \; . \qquad (IV)$$

Appendix B: Studies Included in the Meta-analysis Dataset

Abdulmanova, A., Ferris, S. P., Jayaraman, N., & Kothari, P. (2019). The Effect of Investor Attention on Fraud Discovery and Value Loss in Securities Class Action Litigation. *Georgia Tech Scheller College of Business Research Paper*, (17-34).

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