



Are a Few Huge Outcomes Distorting Financial Misconduct Research?

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[LINK TO ABSTRACT](#)

In generating meaningful empirical research, one problem is the existence of a few observations with extreme values, often referred to as outliers. Including extreme values in data is apt to produce misleading results. In his textbook *Econometric Analysis*, William Greene writes:

Even in the absence of multicollinearity or other data problems, it is worthwhile to examine one's data closely for two reasons. First, the identification of outliers in the data is useful, particularly in relatively small cross sections in which the identity and perhaps even the ultimate source of the data point may be known. Second, it may be possible to ascertain which, if any, particular observations are especially influential in the results obtained. As such, the identification of these data points may call for further study. (Greene 2002, 60)

In financial misconduct research, a leading dataset of more than 1,100 observations is often used, and in that dataset, Enron, WorldCom, Cendant, Colonial Bancgroup, and a dozen or so more are especially influential observations.

Researchers deal with the extreme-value problem in different ways. Some drop outliers from the data, while others cap extreme values at a certain level. If researchers decide simply to keep the observations as they are, they need to alert readers to them, clearly and early in their analysis. Also, they should report how the extreme values drive any of their results. In literature on corporate governance,

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accruals, and risk premia, for example, we find papers that show that extreme observations produce misleading empirical findings (Guthrie et al. 2012; Kraft et al. 2006; Knez and Ready 2012). Furthermore, extreme-value observations call for qualitative investigation to understand whether the variable in question really seemed to play a crucial role.

In the 2018 *Journal of Accounting Research* article “Whistleblowers and Outcomes of Financial Misrepresentation Enforcement Actions,” Andrew Call, Gerald Martin, Nathan Sharp, and Jaron Wilde (2018a) investigate whether the participation of a whistleblower affects the severity of enforcement outcomes. Enforcement outcomes are categorized into several forms, most notably (1) firm penalties, (2) other-penalties,² (3) employee penalties, and (4) employee prison sentences. Call, Martin, Sharp, and Wilde (2018a)—which I abbreviate CMSW and treat grammatically as plural—find that enforcement outcomes are more severe in enforcement actions that are associated with whistleblowers. But the *top one percent* of those enforcement outcomes (11 observations) in their sample of 1,133 enforcement actions influence their results. Table 1 shows the top observations (numbering either 11 or 12) for the top percentile of each category of penalty as a percentage of total penalties among the 1,133 enforcement actions. The top 11 observations in firm penalties constitute 66 percent of the total (all 1,133 observations) firm penalties; the top 11 observations in other-penalties make up 92 percent of the total other-penalties; the top 12 observations in employee penalties make up 84 percent of the total employee penalties; and the top 11 observations in employee prison sentences make up 26 percent of the total employee prison sentences. In addition, all of the nine most extreme observations for the largest firm penalties have the variable in question, the *Whistleblower* dummy, coded as one.

TABLE 1. Top 11 observations as a percentage of total penalties in the 1,133 enforcement actions

Type of Penalties	Total Penalties	Top 11 Observations (Top 1 percent)	Top 11 Observations as a Percentage of Total Penalties
Firm Penalties (\$ Million)	\$14,496	\$9,500	65.5%
Other-Penalties (\$ Million)	\$13,552	\$12,419	91.6%
Employee Penalties (\$ Million)	\$25,710	\$21,521	83.7%
Employee Prison Sentences (Months)	24,247	6,394	26.4%

I also provide in Table 2 the same analysis but restricted to the post-Sarbanes-Oxley Act (SOX) sample of 658 cases focused on by CMSW, and the extreme-values problem there is only slightly less pronounced.

2. When I use the hyphenated “other-penalties” I am referring specifically to the set of “other penalties” that Call, Martin, Sharp, and Wilde analyze, as defined in Section 5.3 of their paper (2018a, 151).

TABLE 2. Top 6 observations as a percentage of total penalties in the 658 enforcement actions

Type of Penalties	Total Penalties	Top 6 Observations (Top 1 percent)	Top 6 Observations as a Percentage of Total Penalties
Firm Penalties (\$ Million)	\$13,578	\$7,396	54.5%
Other-Penalties (\$ Million)	\$11,107	\$9,684	87.2%
Employee Penalties (\$ Million)	\$21,176	\$16,383	77.4%
Employee Prison Sentences (Months)	17,181	3,658	21.3%

Yet CMSW do not mention this extreme-values issue in their abstract or introduction. They do not bring up the issue until the twelfth page of the article. In a lengthy post at a Columbia University blog they make no mention whatever of the issue (Call et al. 2018b). I suggest that in both pieces they should have been upfront about the problem of the extreme enforcement outcomes, and that CMSW should have shown how outliers affect their results.

On the twelfth page of their article, CMSW state:

Two challenges that arise when estimating outcomes of regulatory enforcement actions are the large number of zero-valued observations (i.e., enforcement actions without any resultant penalties or criminal prison sentences) and the severe positive skewness in the dependent variable (i.e., some extremely large penalties). Whereas other regression techniques using a log-transformed dependent variable plus a constant (e.g., Tobit or log-linear regression) suffer from potentially severe bias when estimating regressions using data with these attributes, prior research shows that the Poisson pseudo-maximum likelihood (PPML) estimator is a particularly effective modeling technique for data distributions characterized by a disproportionate number of zeros and severe skewness. (CMSW, 134, citations omitted)

Eight pages later they write:

a potential problem that arises when estimating outcomes of regulatory enforcement actions is the combination of a large number of zero-valued observations with a severe positive skew in the dependent variable (e.g., many observations with no penalties and a nontrivial number of very large penalties). In our sample, 474 (72.0%) of the enforcement actions have no penalties assessed against the firm, while each of the largest 20 actions has \$100 million or more in firm penalties (with three actions exceeding \$1 billion). Further, 208 (31.6%) actions have no penalties assessed against employees, while the largest 22 each exceed \$100 million in employee penalties and the largest four each exceed \$1 billion. Finally, 506 (76.9%) have no prison sentences assessed against employees, while 25 exceed 20 years. Notably, we find that 105 (16.0%)

actions result in no penalties (firm, employee, or agent firm/employee) and no prison sentences. These distributions (i.e., severe skewness and many observations with zeros) suggest that PPML is the best estimator for our regression analyses. (CMSW, 142ff.)

Later, in a footnote, they write:

In terms of economic significance, we find that whistleblowers are associated with an increase in predicted firm penalties from \$8.7 million (without a whistleblower) to \$30.5 million (with a whistleblower), an increase in predicted employee penalties increase from \$22.8 million to \$69.4 million, and an increase in predicted prison sentences increase from 22.5 months to 41.9 months. However, these estimates should be interpreted with caution because of severe skewness in distribution of both the outcome variables (firm penalties, employee penalties, prison sentences) and several of the control variables associated with outcomes of enforcement actions (e.g., Bribery, Organized crime). (CMSW, 146 n.17)

And finally, later they state that “the distribution of regulator penalties (monetary fines and prison sentences) exhibits severe skewness, which limits our ability to reliably quantify the economic impact of our findings” (CMSW, 164).

Thus CMSW do mention the extreme-values issue. But those mentions begin too late and are perhaps rather peripheral and brief, and merely cautionary. The authors do not identify the extreme observations, such as WorldCom and Enron, in their manuscript. The term “outlier” is not found in the paper. As for the morpheme “extreme,” it is found only once. They do repeatedly use the term “severe,” in connection to the word “skew.” But to speak of “severe skewness” does not clearly convey the simple fact of a few extreme observations. Also, although they mention “severe skewness” by the by, they nowhere investigate and show how extreme observations affect their results. We all understand that authors do not wish to accentuate possible weakness of their papers. Still, the authors could have spoken about the extreme-values issue from the start and shown how outliers affect the results.

Several matters of context

Before looking into the results of CMSW, I provide some background discussion regarding: (1) policy and research on financial misconduct, (2) examples of other financial-misconduct studies vulnerable to the extreme-values problem, (3) government programs that incentivize whistleblowing, (4) how I came to write

this paper, (5) the leading dataset on enforcement outcomes, and (6) the *Whistleblower* coding in the dataset used by CMSW.

Remarks on policy and research on financial misconduct

Since 1933, there have been several major laws and regulations made in hopes of reducing financial misconduct at publicly traded firms, including these:

- Securities Act of 1933
- Securities and Exchange Act of 1934
- Foreign Corrupt Practices Act of 1977
- Insider Trading and Securities Fraud Enforcement Act of 1988
- Securities Enforcement Remedies and Penny Stock Reform Act of 1990
- U.S. Sentencing Commission’s Organizational Sentencing Guidelines of 1991
- Private Securities Litigation Reform Act of 1995
- Sarbanes-Oxley Act of 2002
- Dodd-Frank Act of 2010

Financial misconduct research has become an attractive research area for many finance and accounting researchers, as the findings can hold significance for public policy. A study with even a remote chance of influencing policy can gather significant interest. The recent availability of financial misconduct databases from such sources as the Government Accountability Office, Audit Analytics, Stanford’s Securities Class Action Clearinghouse, and the Securities and Exchange Commission’s Accounting and Auditing Enforcement Releases have made it easier for researchers to pursue this line of research. Jonathan Karpoff, Allison Koester, D. Scott Lee, and Gerald Martin (2017) report that “more than 150 papers have been published in top accounting and finance journals that use one or more of these data sources.” Searching with Google Scholar for the term “financial misconduct” (with the quotation marks) delivers about 4,250 hits (as of January 17, 2019). One team of authors write: “The growth in datasets and strong industry and public policy implications for research on financial market misconduct and corporate fraud suggests that the demand for high quality research on corporate fraud and financial market misconduct will continue to grow significantly in the future” (Cumming, Dannhauser, and Johan 2015, 165). Recent findings have attracted great attention not only from media but from lawmakers and regulators such as the Securities and Exchange Commission (SEC). Findings are often used to initiate new policies or justify existing policies. I’ve toiled extensively in the

literature, and I really cannot name a single financial-misconduct paper in a top accounting or finance journal that reflects badly on a current law or regulation.

Other financial misconduct research with extreme-values problems

In this subsection I briefly discuss a few other papers that might well suffer from the extreme-values problem, to indicate that the issue raised in this paper extends beyond CMSW.

A paper by Frank Yu and Xiaoyun Yu (2011), which has 295 Google Scholar citations (as of January 18, 2019), looks at the relationship between corporate lobbying and fraud detection. Yu and Yu find that firms' lobbying activities make a significant difference in the number of days between the commencement at a firm of a later-perceived fraud and the commencement of the detection of the fraud:

Our study sheds light on the recent debate about whether to improve the transparency in corporate political spending. Many firms have argued against detailed disclosure of political spending, citing objections such as the possibility of revelation of corporate strategy to competitors, distractions to management, and negligible impact on shareholder values. Our results suggest that political spending does affect the welfare of investors and that there is a need for more transparency in corporate political spending. (Yu and Yu 2011, 1867)

In other words, the study suggests reform to require firms to disclose their political spending in greater detail. The sample of their study, however, includes extreme observations such as Enron and WorldCom. Including the extreme observations might generate misleading results, as for example Enron³ and WorldCom spent large amounts of money in lobbying activities and also avoided fraud detection for a very long time.

In 2010, the SEC's Enforcement Division announced a new policy named the "Cooperation Program." The program includes various measures designed to encourage greater cooperation by individuals and companies in SEC investigation and enforcement action. It provides incentives to individuals and companies who come forward and offer valuable information to SEC investigators. On its website, the SEC asserts: "There is a spectrum of tools available to the Commission and its staff for facilitating and rewarding cooperation by individuals and entities. These benefits to cooperators can range from reduced charges and sanctions in enforcement actions to taking no enforcement action at all" ([link](#)).

3. On Enron, see Tran 2002.

Given the existence of that program, the paper by Rebecca Files, Gerald Martin, and Stephanie Rasmussen (2018) set out to examine the benefits of cooperation. Files et al. (2018) look at the association between the severity of enforcement outcomes and firm cooperation in the enforcement action. They find that a firm's credit for having cooperated is negatively associated with firm monetary penalties assessed by the SEC and the Department of Justice (DOJ). Their estimates suggest that firms with cooperation credit realize an average penalty reduction of \$23.8 million. The authors state "our results provide important insight into what constitutes meaningful cooperation with regulators and suggest that the benefits can be substantial for firms deemed to be cooperative" (2018, 1).

Although Files et al. (2018) try to give us insights about the usefulness of cooperation for the cooperators, they do a less than satisfactory job of addressing the extreme-value problem in their results. Further investigation of the effect of extreme firm penalties on the relation between the cooperation and firm penalties is warranted, as the study can be interpreted as justifying the existence of the current SEC's Cooperation Program. The study also uses the same enforcement action data as CMSW.

Maria Correia (2014) finds that firms with long-term political connections, as measured by contributions and lobbying, face lower monetary penalties when they are prosecuted by the SEC. Correia suggests that an increase of \$100,000 in political action committee money in the five pre-violation years is linked to an 11 percent decrease in monetary penalties by the SEC. However, this paper also has not considered the effect of the extreme observations on its results. The *Los Angeles Times* published an article based on this study titled "Politically Connected Companies Get a Break from the SEC, Study Says" (Hiltzik 2014). The *Times* article suggests "some (SEC) chairs have tried to get permission from Congress to self-fund fees, but Congress isn't that dumb," and proposes that this is because self-funding will eliminate Congress's ability to pressure the SEC as suggested in the study. Correia (2014) also uses the same enforcement action data as CMSW.

Ed deHaan, Simi Kedia, Kevin Koh, and Shivaram Rajgopal (2015) look at the association between enforcement outcomes and career opportunities for SEC trial lawyers in civil cases involving accounting misrepresentation. They find that revolving-door incentives do not appear to undermine the prosecution of civil cases against accounting misrepresentations. They write:

[T]hese results provide preliminary input to the discussion among the press, policy makers, and Congress about whether revolving doors are detrimental to the SEC's regulatory efforts. In our particular setting, future job prospects, on average, appear to make SEC lawyers increase their enforcement efforts in trying civil cases. These results can potentially inform the SEC's policy on

revolving doors. (deHaan et al. 2015, 92)

However, the SEC regards the further harm to injured shareholders as an important consideration in the determination of whether or not to impose monetary penalties. That means zero or low monetary damages do not imply lax enforcement by the SEC lawyers. That makes the deHaan et al. (2015) dependent variable of monetary penalty a questionable measure of enforcement effort. Their measure of the monetary penalty variable also comes from the same enforcement action data as CMSW.

Government programs that incentivize whistleblowing

Section 806 of the Sarbanes-Oxley Act of 2002 was enacted to protect any employee of a publicly traded company or subsidiary who provides evidence of fraud. It authorizes the U.S. Department of Labor to protect whistleblowers against employers who retaliate. Section 1107 of the Act permits the Department of Justice to criminally charge those responsible for the retaliation. On the surface, this make sense. For instance, Alexander Dyck, Adair Morse, and Luigi Zingales (2010) find that fraud detection does not rely on standard corporate governance actors such as the SEC and auditors, but rather it takes a village, including several nontraditional actors such as employees and media. Given the perception that whistleblowing is an effective way to expose fraud, the Dodd-Frank Act of 2010 created the SEC Whistleblower Program. The program rewards individuals who submit tips related to violations of the federal securities laws, with awards in the range of 10 to 30 percent of the monetary sanctions collected. It also provides whistleblowers with employment protection and allows them to report the wrongdoings anonymously. The program is managed by the newly established SEC Office of the Whistleblower ([link](#)).

Now comes the focus of my paper: CMSW—that is, Call, Martin, Sharp, and Wilde (2018a). The study has been considered as providing indirect evidence of the effectiveness of whistleblower programs. CMSW investigate whether a whistleblower’s participation affects the severity of enforcement outcomes such as firm penalties, employee penalties, other-penalties, and employee prison sentences. They find enforcement outcomes are more severe in enforcement actions associated with whistleblowers. The authors state:

Examining the role of whistleblowers in securities enforcement is important because policy makers continue to enact legislation attempting to encourage whistleblower involvement and because regulators dedicate significant resources to promoting and rewarding whistleblowing activity. For example, the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010

(Dodd-Frank Act) requires the SEC and the Commodity Futures Trading Commission (CFTC) to establish whistleblower offices that provide a formal venue through which whistleblowers can voice complaints and share evidence with regulators. Rewards for whistleblowers who come forward with original information about corporate misconduct can be large, ranging from 10% to 30% of monetary sanctions over \$1 million stemming from investigations facilitated by whistleblowers' information, documentation, or cooperation. (CMSW, 124, citations omitted)

CMSW write: "Our findings are important to legislators considering the efficacy of current whistleblower policies and the determination of budgets for whistleblower programs, to regulators who design enforcement programs, to SEC and DOJ prosecutors evaluating the merits of using information from whistleblowers in their investigations, and to firms in assessing the consequences of potential enforcement actions" (CMSW, 128; see also 164).

The CMSW study attracted a lot of attention. The *Wall Street Journal* ran an article based on it titled "Firms Hit With Bigger Penalties When Whistleblowers Involved," which never mentions the problem of extreme observations. One of the co-authors of the paper, Nate Sharp, is quoted as saying: "Even after holding all those things constant, we see that whistleblowers have a very big effect" (Ensign 2014).⁴

Recently, the authors wrote a blog post on Columbia Law School's Blog on Corporations and the Capital Markets titled "Financial Enforcement Actions and the Role of Whistleblowers" and based on the CMSW study. There they write:

Our findings are relevant and timely in light of the U.S. federal government's extensive investments in whistleblowing programs. Section 922 of the Dodd-Frank Wall Street Reform and Consumer Protection Act offers significant monetary incentives (10 percent to 30 percent of monetary sanctions collected via criminal or civil proceedings) to prospective whistleblowers, and also established the SEC Investor Protection Fund to provide funding for this program. As of the end of 2017, the balance in this fund was \$321 million, and the government had paid out a total of \$160 million to 46 different whistleblowers since the passage of Dodd-Frank. In addition, while the U.S. generally offers the most aggressive whistleblowing rewards, other countries are following suit. For example, in 2016, the Ontario Securities Commission adopted a whistleblowing program and began offering financial incentives to prospective whistleblowers in Canada. As such, large-scale evidence on the usefulness of whistleblowers in the enforcement process (at least in terms of

4. Much of the attention paid to the study occurred in 2014, shortly after it was posted as a working paper ([link](#)).

enforcement outcomes) is relevant to regulators and legislators who continue to promote whistleblowing programs and reward those who assist in enforcement actions. (Call et al. 2018b)

The intention of CMSW is noble, as policymakers have made a significant attempt to establish or increase rewards for whistleblowers in the area of financial misconduct. Few studies have examined the benefits of those whistleblower programs. However, CMSW should not be interpreted as providing solid support for the whistleblower programs, as I find that the top one percent (11 observations) of those enforcement outcomes in CMSW's large sample of 1,133 enforcement actions influence their results. By illustrating the effect of extreme observations on CMSW's findings, I show that further investigation is warranted. This is important because policies such as the Whistleblower Program also have negative consequences, and policy judgments should consider both benefits and costs.

The SEC asks for comment letters when it proposes a new rule. For the implementation of the Whistleblower Provisions of Section 21F, the SEC received a comment letter from the U.S. Chamber of Commerce. The letter makes it obvious that companies are concerned about the unintended consequences and the cost of whistleblower programs. The letter states:

If implemented as proposed...the rule would have a number of harmful consequences, including eviscerating corporate compliance and reporting programs; giving rise to unjustified negative publicity about, and unnecessary SEC investigations of, a large number of innocent companies; and overwhelming the Commission with an avalanche of poor-quality information. These results are directly contrary to the well-documented fact that companies and employees benefit, and scarce government enforcement dollars are preserved, when companies have the first chance to address financial wrongdoing. These outcomes would also fly in the face of the legislative purpose reflected in Section 301 of SOX, which *requires* public companies to develop sophisticated internal reporting programs. (Hirschman and Rickard 2010, 2–3, emphasis in original)

The list of possible harmful effects could be extended much further; here I relegate mention of some to a footnote.⁵

5. In addition to possible consequences mentioned in the Chamber of Commerce quotation, here are some others: a culture of suspicion amongst firm employees and a chilling effect on communication within the firm; disgruntled employees abusing this power by whistleblowing anonymously to disrupt the company; employees abusing this power by threatening to whistleblow and shakedown the firm (this threat could be effective even if the firm were to know itself innocent); overregulation opens the door to political abuse or shakedown of disfavored firms; competitors accusing a company, or putting someone up to it, wasting the company's time and resources; and suffocation/crowding out of other private-sector means of

How I came to this project

My research is in the political economy of finance, especially financial misconduct and related policy issues, including a piece (Kuvvet 2015) that used the aforementioned leading dataset. I became interested in the literature on the severity of enforcement outcomes. But I notice that the extreme-values problem is often buried. I have searched for papers that address it rigorously and have found only one (Files 2012). In 2018, I came across CMSW in the *Journal of Accounting Research*. The journal requires researchers to publish their data online upon acceptance of their paper, and so the data used in that paper are now publicly available there ([link](#)). Using that data I explored whether the extreme-value problem has any influence on CMSW's results. I found that it does, wrote a comment paper on CMSW, and submitted it to the *Journal of Accounting Research*. I received a rejection letter but also two fruitful referee reports that helped me to make improvements reflected in the present paper. The earlier version of my paper focused only on firm penalties. After receiving the comments from the referees I expanded my analysis to address employee penalties, other-penalties, and employee prison sentences. The journal did not, however, indicate an interest in receiving a revised version, so I tried *Econ Journal Watch*.

The Karpoff, Koester, Lee, and Martin dataset

The leading financial misconduct dataset, used by CMSW in their study, comes from Jonathan Karpoff, Allison Koester, D. Scott Lee, and Gerald Martin (hereafter KKLM). The data have been hand-collected by Martin, also one of the coauthors of CMSW. The data include financial misrepresentation enforcement actions under Section 13(b) of the Securities Exchange Act, created by the 1977 Foreign Corrupt Practices Act between 1978 and 2012. The first publications to use the data were authored by Karpoff, Lee, and Martin (2008a; 2008b).

To access the full KKLM dataset or any part that has not been publicly released one must contact its creators. If they agree to share their data with the researcher, the researcher has to sign a one-time use-of-data agreement, meaning that the researcher can only use the data only once for a study as stated in the

correcting the problem. It is important to avoid double standards by recognizing that government officials, just like private-sector actors, have limitations and imperfections: they wield great power but face little accountability, and their conduct can be capricious, self-interested, unreasonable, or politically biased. I acknowledge, however, that the \$1 million penalties threshold for payouts to whistleblowers helps to mitigate some of these possible pathological consequences. Additional commentary on these matters has been published by think tanks and other sources skeptical of government activism (see, e.g., Calomiris, Scott, and Spatt 2011; Katz 2011).

agreement. The agreement includes the following terms of use: (1) “The data will be used only for the research project described below (please provide a short description and title);” (2) “List all coauthors and their affiliations on the research project below;” and (3) “The data will not be shared with any other person without prior written consent” from Karpoff, Lee, and Martin.

The data have been widely used in the corporate misconduct literature. I found more than 20 published papers that use the KKLM database, including papers in top-tier journals such as the *Journal of Accounting Research*, *Journal of Accounting and Economics*, *Journal of Finance*, *Journal of Financial Economics*, and *Journal of Financial and Quantitative Analysis* (e.g., CMSW; Call et al. 2016; Correia 2014; Files 2012; Kedia and Rajgopal 2011; Karpoff et al. 2008a; 2008b). Many published and working papers attempting to find the determinants of the severity of the enforcement outcomes use KKLM’s data.

The KKLM data has become rather the gold standard for corporate misconduct data after Karpoff, Koester, Lee, and Martin’s 2017 article in *Accounting Review*. The article compares their KKLM database to those from four popular sources (Government Accountability Office, Audit Analytics, Securities Class Action Clearinghouse, and Accounting and Auditing Enforcement Releases) used in the corporate misconduct literature. They suggest that one can get different results from empirical tests depending on which of those four databases is used in a study. Although Karpoff et al. (2017) do not explicitly say that their KKLM data is superior to the four other databases, one can clearly infer that conclusion from the article. They claim that the KKLM data, unlike the others, does not suffer from the following issues: misidentified event dates, missing relevant information, errors of omission, duplicate events for the same instance of misconduct, and inclusion of events unrelated to misconduct. Unlike Audit Analytics data, KKLM data are not available for researchers to purchase, and unlike GAO data, KKLM data are not freely available to the public.⁶ But today if a researcher uses one of the other four popular databases, referees are prone to bring up Karpoff et al.’s (2017) *Accounting Review* paper and point to the weaknesses of the database as grounds for rejection.

The *Whistleblower* and *Tipster* variables

The KKLM data goes back to 1978 and contains 1,133 enforcement actions. We now turn to CMSW’s primary analysis which focuses on the 658 enforcement actions in the post-SOX period (2002–2012); they confined their primary analysis to that period because most of the cases coded as whistleblowing took place during

6. As I noted above (p. 11), in 2018 some of the KKLM data became publicly available, to meet the requirement of the *Journal of Accounting Research*, upon the publication of CMSW.

that period.

CMSW's basis for coding the existence (and timing) of whistleblowing is complicated and less than clear. Figure 1 reproduces a portion of their Table 2 (CMSW, 139). One sees the 658 post-SOX "Total enforcement actions." Of those, 148 have been coded Whistleblower. Those codings are arrived at by three types of sources: 110 are attributed to "OSHA FOIA", 13 to "*Qui tam*," and 25 to "As noted in enforcement proceedings." I begin by explaining those attributed to the OSHA source.

Figure 1. Partial reproduction of Table 2 from CMSW (p. 139)

Panel A: Source of whistleblower action	
Type	<i>N</i>
OSHA FOIA whistleblower complaints received	934
Total enforcement actions	658
No whistleblower	510
Whistleblower	148
Whistleblower Cases by Source:	
OSHA FOIA	110
<i>Qui tam</i>	13
As noted in enforcement proceedings	25
Whistleblower Cases by Type:	
Tipster	74
Nontipster	74

The OSHA source does not refer to any direct report from SEC about whether whistleblowing was involved in the investigation, since SEC keeps that secret. Rather, the coding is based on an inference from complaints by employees of having been discriminated against for having blown a whistle. After the passage of Sarbanes-Oxley Act, the Occupational Safety and Health Administration (OSHA) became responsible for handling employee complaints of having been discriminated against for having blown the whistle on alleged financial misconduct. OSHA is required to communicate those discrimination complaints to the SEC. Given the fact that OSHA stores all those employee complaints of discrimination for whistleblowing, Andrew Call, one of the coauthors of CMSW, used Freedom of Information Act (FOIA) requests to obtain those complaints from each of the OSHA offices across the nation. Each of the ten OSHA offices responded to Call's request with the information for that particular region.

The information requested by Call only concerned complaints for discrimination-due-to-whistleblowing that fall under Section 806 of the Sarbanes-Oxley Act of 2002. Call requested all such complaints contained in OSHA's database. The information provided by OSHA includes the date the employee filed the complaint with OSHA and the name of the firm complained about. The

data obtained by Call consist of 934 unique complaints, related to 619 companies, filed from October 2002 through December 2010. From the complaints of discrimination, the researchers infer allegations of financial misconduct.

The OSHA data was used a couple of times prior to CMSW. Call used the OSHA data in a 2010 paper in *Accounting Review* coauthored with Robert M. Bowen and Shiva Rajgopal. Also, he shared the OSHA data with Jaron Wilde, one of the coauthors of CMSW, for the sole-authored paper by Wilde published in *Accounting Review* in 2017.

The main variable of interest in CMSW is *Whistleblower*. It is an indicator variable equal to one if the researchers deem a whistleblower to have been associated with the enforcement action, and zero otherwise. CMSW also examine the association between whistleblowing and the severity of enforcement outcomes conditional on the timing of the whistleblower's discrimination complaint. They create two additional whistleblowing variables, namely *Whistleblower (Tipster)* and *Whistleblower (Nontipster)*. CMSW use the filing date of complaints of discrimination-due-to-whistleblowing as the relevant date for determining whether the whistleblower is a tipster or a nontipster. They consider whistleblowers to be nontipsters if the complaint date is after the earliest known regulatory investigation or enforcement inquiry date.

CMSW (p. 169) treat an OSHA complaint as a tipster if:

1. the complaint date is unknown,
2. the complaint date precedes the end of the violation period (that is, the period which, according to SEC's determinations, the firm engaged in the misconduct), or
3. the complaint date precedes the earliest known investigation, by the SEC or DOJ, into the misconduct.⁷

To generate the codings, CMSW merge the OSHA data with KKLM enforcement action data. The filing date of complaint comes from Call's OSHA data. The end of the violation period, the earliest known regulatory investigation, and the enforcement inquiry date come from the KKLM enforcement action data. Merging those datasets with those mentioned dates creates major issues, some of which are acknowledged by CMSW.

Let me first point out a matter of possibly confusing terminology. CMSW

7. CMSW also mention the date of an enforcement inquiry as a basis, that is, that the complaint is coded as tipster if the complaint date precedes that date, but it seems me that point (3) in my listing renders that criterion redundant, since an enforcement inquiry presuppose that an investigation is not yet begun. See also their mention of "informal inquiry" on page 131—presumably, after an investigation has begun, no subsequent communication is "informal."

refer to allegations of discrimination-due-to-whistleblowing as “whistleblowing allegations.” What they really mean here is discrimination complaints, but specifically complaints of having been discriminated against for having blown a whistle on firm financial misconduct. The one making the complaint/“allegation” is saying that what motivated the discrimination was her having blown a whistle. The data do not establish that she did in fact blow a whistle, nor what conduct on the part of the firm any such whistleblowing may have concerned, nor that actual discrimination occurred. Such is the nature of what CMSW call a “whistleblowing allegation.” Empirical research is not always sensitive to fuzziness in data, but the research in question here, exhibiting the extreme-values problem, can be highly sensitive to such fuzziness: The miscoding of just a few of the extreme values can change the results. Researchers should be clear and upfront about such sensitivity.

On page 126, CMSW state that “because we cannot directly observe whether regulators actually used the information from each OSHA whistleblower, these whistleblower allegations [read: complaints of discrimination due to whistleblowing] reflect only potential whistleblower involvement in an enforcement action.” On page 164, CMSW also state that “most of the whistleblower allegations in our sample are obtained from OSHA, and we cannot directly observe whether the SEC or DOJ actually used the information from each OSHA whistleblower. As a result, these cases represent potential whistleblower involvement in an enforcement action.” In other words, even though OSHA is required to communicate the employee discrimination complaint for blowing the whistle on alleged financial misconduct to the SEC, the SEC is not required to act on any such allegation. The SEC is especially likely not to act if it regards the allegation as frivolous.

Here is something that confuses me: Call published a paper in *Journal of Accounting and Economics* with Simi Kedia and Shivaram Rajgopal in 2016. They find that firms grant more rank-and-file stock options when involved in financial reporting violations, consistent with management’s incentives to discourage employee whistleblowing (blowing a whistle would reduce the value of the stock). That paper does not use the OSHA data. Instead:

We use a LexisNexis search to construct our sample of whistle-blowing firms. We follow Bowen et al. (2010) and search every combination of the following sets of terms: (1) ‘whistle,’ ‘whistle-blowing,’ ‘whistleblower,’ and ‘whistleblower’ and (2) ‘financial,’ ‘accounting,’ and ‘fraud.’ We perform this search over the calendar years 1992 through 2010. We augment the sample with the employee-based whistleblowing events identified by Dyck et al. (2010), yielding a total of 153 whistle-blowing events. (Call, Kedia, and Rajgopal 2016, 286)

Thus, Call and his 2016 coauthors do not use his already-available OSHA data

for that paper. Instead, they rely on LexisNexis to collect whistleblowing involvements. In a footnote, the authors say why, citing Bowen, Call, and Rajgopal (2010): “Bowen et al. (2010) evaluate the efficacy of whistleblowing complaints filed with OSHA and conclude that these complaints are generally frivolous. Hence, we do not employ OSHA-related whistle-blowing events in our data analysis” (Call, Kedia, and Rajgopal 2016, 287 n.20). Yet, CMSW uses the OSHA data.

Another problem arises when CMSW merges the OSHA data with the KKLM enforcement actions data. CMSW merge the two by using the date the employee filed the complaint with OSHA. If the date falls between the beginning of the violation period and the last regulatory proceedings of an enforcement outcome for that company, CMSW consider the enforcement action as associated with that particular complaint. That is, even if that date is just one day before the final regulatory proceedings of an enforcement outcome, CMSW treats that enforcement outcome as if a whistleblower played a role, albeit, in that case, as a nontipster. Second, as Call, Kedia, and Rajgopal (2016, 287, n20) point out, such OSHA complaints are often frivolous. Thus, they are not likely to be used by the SEC.

Other issues arise when CMSW try to classify whistleblowers as tipsters versus nontipsters based on the filing date of the complaint with OSHA. First of all: Of the 148 whistleblowing cases in CMSW, 13 do not have the date for the whistleblowing (CMSW, 147 n.18). One might think that such cases should simply be excluded. But CMSW count them as tipsters, without explaining why they do so. Second, the filing date of the OSHA complaint is not necessarily the same as the date the complainer began to assist the SEC investigation even if we assume that the complaint is used by SEC for the investigation.

Also, it is also not clear from the manuscript what happens if there is more than one complainer/inferred-whistleblower. For instance, suppose there is a tipster and then over the course of the investigation a nontipster also provides information. CMSW do not explain how to code such a case—which certainly seems a plausible scenario for major misconduct that catches fire.

But my greatest concern with the OSHA-derived data of CMSW is this: Their whistleblower identities for many observations conflict with Dyck et al. (2010)’s whistleblower data. For instance, the Dyck et al. (2010) data suggest that the ‘whistleblower’ for Enron is a newspaper, but CMSW’s coding shows an employee as a tipster whistleblower. In other words, the CMSW data says that the investigation of Enron was started because of an employee allegation. Again, the dubious coding of one case can make a huge difference when the data suffers from an extreme-values problem.

In addition to the 110 complaints to OSHA for alleged discrimination due to

whistleblowing, CMSW also add 13 *qui tam* whistleblowing cases to their sample. Under the False Claims Act of 1863, also known as the Lincoln Law, people who are not affiliated with the government, known as “relators” under the law, can file actions on behalf of government against persons and companies that defraud government programs and be paid a percentage of the settlement. However, a relator does not have to be an employee of the fraudulent company; any person with the knowledge of a company defrauding government program can file a *qui tam* lawsuit. CMSW do not state, and it is not otherwise clear, whether their 13 *qui tam* cases are all related to employee whistleblowing.

CMSW also add 25 additional whistleblowing cases directly referred to in administrative and legal proceedings in the enforcement actions. However, CMSW do not specify whether those whistleblowing cases are all employee-related or what those administrative and legal proceedings are.

Of the 148 cases coded *Whistleblower* in CMSW, 13 do not have the date for the whistleblowing. CMSW count them as tipsters. It is not clear from the manuscript whether those 13 cases come from OSHA cases, *qui tam* cases, or those additional 25 cases. We also do not know whether any of those 13 cases without the whistleblowing dates is an extreme observation. Again, these uncertainties are important, because a few codings can matter a lot when a few extreme observations drive results.

For any given case, we simply do not know the lineage of the coding. As we have seen, uncertainties abound in CMSW’s inferring of whistleblowing and of tipster status. The extreme-values problem calls for such disclosure, because, again, a few dubious codings could make a big difference in the results. It is puzzling that CMSW do not tell the lineage for each of their 148 whistleblowing codings. They do not say why they do not tell the lineage.

Results when extreme observations are removed

A total of 1,133 enforcement actions are included in the KKLM database, 658 of which occurred after the passage of the Sarbanes-Oxley Act in 2002. One of the types of enforcement action is firm penalties, which are fines levied against the fraudulent firm. Table 3 shows the largest 11 firm penalties (top one percent) in CMSW’s sample of SEC and DOJ enforcement actions between 1978 and 2012 that are associated with alleged financial misrepresentation. The sum of the three largest firm penalties is \$5,459 million. The sum of the firm penalties for those 11 enforcement actions is \$9,500 million. The sum of all firm penalties in the

entire sample of 1,133 enforcement actions is \$14,496 million. Thus, those 11 enforcement actions constitute 66 percent of the total firm penalties in the sample. (The number of enforcement actions which resulted in \$0 in firm penalties was 911, or 80 percent.) Thus, the few extreme observations are likely to have a significant effect on the severity of firm penalties if whistleblowers are also associated with them. Indeed, among those 11 observations, nine (81 percent) of the largest firm penalties have a value of one for the *Whistleblower* dummy. Again, we do not know how those nine codings were arrived at.

TABLE 3. Largest firm penalties (Top 1 percent) in CMSW's sample of SEC and DOJ enforcement actions between 1978 and 2012 associated with alleged financial misrepresentation

Record ID	Company Name	Firm Penalties (\$ Million)	Whistle-blower	Whistle-blower (Tipster)	Whistle-blower (Nontipster)
582	WorldCom Inc	2,278	1	0	1
991	Siemens AG	1,659	1	1	0
566	Enron Corp (2)	1,522	1	1	0
662	American International Group Inc. (1)	825	1	0	1
970	Halliburton Co	600	1	0	1
586	Reliant Resources Inc	512	1	1	0
735	Time Warner Inc. (2) [America Online Inc. (1)]	510	1	0	1
710	Bristol Myers Squibb Co	450	1	0	1
811	Federal National Mortgage Association (Fannie Mae)	400	1	0	1
228	National Medical Enterprises Inc	379	0	0	0
1116	ENI SpA	365	0	0	0
<i>Note:</i> Record ID is an SEC identifier that relates to the specific action in the SEC enforcement action dataset.					

In Table 4, the dependent variable is *Firm Penalties*, in increments of millions of dollars. Firm penalties are the total civil and criminal monetary penalties assessed against the firm, its parent, and its subsidiaries, and they consist of disgorgement, prejudgment interest, civil and criminal fines, and criminal restitution. Following CMSW, Table 4 uses the Poisson pseudo-maximum likelihood regressions. Model 1 in Table 4 successfully replicates CMSW's principal results for firm penalties using the full sample of 1,133 enforcement actions. The coefficients and χ^2 -statistics for Model 1 are the same as those of Model (1) in Table A2 in CMSW's online appendix.

TABLE 4. Replicating the main results of CMSW without the 11 largest firm penalties (Top 1 percent) by using the 1,133 SEC and DOJ enforcement actions between 1978 and 2012 associated with alleged financial misrepresentation (Full Sample)

	Model [1]	Model [2]	Model [3]	Model [4]
	Coefficient ($\hat{\alpha}$)			
Intercept	-8.751*** (-4.45)	-6.941*** (-5.67)	-7.268*** (-5.81)	-8.402*** (-6.03)
Whistleblower	0.995* (1.67)	-0.329 (-1.08)		0.237 (0.54)
Whistleblower (Tipster)			-0.871** (-2.23)	
Whistleblower (Nontipster)			0.018 (0.06)	
Self-dealing	-1.546* (-1.82)	-0.208 (-0.66)	-0.334 (-1.02)	-0.512 (-0.96)
% Blockholder ownership	-1.855 (-1.19)	-1.246*** (-2.79)	-1.063** (-2.29)	-0.444 (-0.48)
% Initial abnormal return	0.996 (0.93)	0.176 (0.15)	0.107 (0.09)	0.597 (0.55)
Violation period	0.738*** (3.66)	0.616*** (3.08)	0.724*** (3.41)	0.726*** (4.17)
Bribery	0.411 (1.01)	0.86 (1.58)	0.872* (1.74)	0.474 (1.04)
Organized crime	-3.998*** (-2.89)	-2.773** (-2.39)	-2.615** (-2.26)	-3.11*** (-2.65)
Deterrence	-0.078 (-0.18)	0.334 (1.08)	0.243 (0.83)	0.083 (0.2)
# C-level respondents	0.515 (1.34)	0.756** (2.47)	0.783** (2.57)	0.663* (1.96)
# Code violations	1.422*** (2.97)	0.501* (1.83)	0.398 (1.52)	0.905** (2.39)
Fraud	-0.658* (-1.75)	-0.229 (-0.52)	-0.187 (-0.47)	-0.201 (-0.5)
Misled auditor	0.556 (1.45)	0.076 (0.24)	0.065 (0.21)	-0.013 (-0.04)
Big N auditor	2.939** (1.99)	0.645 (1.36)	0.654 (1.41)	1.521** (2.3)
Exec respondent terminated	-0.201 (-0.39)	-0.799** (-2.23)	-0.814** (-2.3)	-0.727 (-1.7)
Cooperation	0.542 (1.59)	0.651* (1.71)	0.633* (1.7)	0.432 (1.44)
Impeded investigation	0.022 (0.03)	1.966*** (4.22)	2.056*** (4.65)	0.756 (1.6)
% Independent directors	-0.584 (-0.83)	0.917 (1.5)	1.179* (1.96)	1.077 (1.54)
Recidivist	-0.112 (-0.33)	-0.387 (-1.45)	-0.224 (-0.83)	-0.054 (-0.16)
Market capitalization	0.261*** (2.74)	0.461*** (6.32)	0.442*** (6.09)	0.400*** (5.2)
Market-to-book assets	-0.445** (-2.2)	-0.300** (-2.24)	-0.314** (-2.43)	-0.197 (-1.54)
Leverage ratio	0.579*** (3.06)	0.200 (1.38)	0.207 (1.28)	0.420*** (3.06)
Distance from regulator	-0.079 (-1.36)	-0.003 (-0.06)	0.000 (0.01)	-0.058 (-1.02)
Post-Sarbanes Oxley	0.016 (0.03)	-0.312 (-0.48)	-0.253 (-0.4)	-0.417 (-0.82)
FF 12 Industry Fixed Effect	Yes	Yes	Yes	Yes
N	1,133	1,122	1,122	1,133

Notes: The dependent variable is *Firm Penalties*. My model specification is the same as that in Table 4 of CMSW (p. 145). Model [1] uses the full sample of 1,133 enforcement actions. Model [2] and Model [3] omit the top one percent of *Firm Penalties* from the sample (11 observations). Model [4] replaces each of those 11 extreme observations with the 99th percentile value (\$338 million) of *Firm Penalties* for the full sample. Variables are as defined in CMSW's Appendix. These are exponential regression results, with $\hat{\alpha}$ -statistics shown, using robust standard errors. *, **, and *** denote statistical significance at the 10-percent, 5-percent, and 1-percent levels, respectively.

However, after omitting the top one percent of firm penalties from the sample (11 observations) and rerunning the regression, in Model 2, the estimated coefficient for the *Whistleblower* dummy is statistically insignificant ($\alpha = -1.08$). In contrast to CMSW's findings, the sign of the *Whistleblower* dummy is also negative. Model 3 examines the association between *Firm Penalties* without the 11 largest penalties and both *Whistleblower (Tipster)* and *Whistleblower (Nontipster)*. In Model 3, the estimated coefficient for *Whistleblower (Tipster)* is negative and statistically significant ($\alpha = -2.23$) and the coefficient for *Whistleblower (Nontipster)* is statistically insignificant ($\alpha = 0.06$), in contrast to CMSW's findings of a statistically significant positive association between *Firm Penalties* and both *Whistleblower (Tipster)* and *Whistleblower (Nontipster)*. Model 4 replaces each of those 11 extreme observations with the 99th percentile value (\$338 million) of *Firm Penalties* for the full sample, and the *Whistleblower* results remain statistically insignificant ($\alpha = 0.54$).

CMSW's primary analysis focuses on the 658 enforcement actions in the post-SOX period, because most of the whistleblowing allegations took place during that period. Therefore, Model 1 in Table 5 reproduces the main results for *Firm Penalties* of CMSW's Model (1) in their Table 5 (p. 145) without the extreme observations for *Firm Penalties* by using the 658 enforcement actions that occurred between 2002 and 2012, after the passage of SOX. Models 1 and 2 exclude the 10 post-SOX extreme observations from the 11 pre- and post-SOX cases used in Table 3.⁸ In Model 1, the estimated coefficient for the *Whistleblower* dummy is statistically insignificant ($\alpha = -0.63$), which suggests that whistleblowers have no effect on the severity of firm penalties. In Model 2, the estimated coefficient for *Whistleblower (Tipster)* is negative and statistically significant ($\alpha = -1.86$). Again, this contrasts with CMSW, who find a statistically significant positive association between *Whistleblower (Tipster)* and the severity of *Firm Penalties*. The coefficient for *Whistleblower (Nontipster)* in Model 2 is again statistically insignificant ($\alpha = 0.14$). Model 3 replaces each of the ten extreme observations with the 99th percentile value (\$338 million) of *Firm Penalties* for the full sample. The *Whistleblower* dummy remains statistically insignificant.

One might argue that I should use the extreme observations (top one percent) in the sample of the 658 enforcement actions in Table 5, based on the post-SOX period, rather than the full sample with 1,133 enforcement actions, as CMSW's main analysis (their Table 4) uses the post-SOX sample of 658 enforcement actions. To address this argument in Models 4 and 5, I show the outcome when I omit only the six largest firm-penalty cases, that is, the 99th percentile value for *Firm Penalties* (\$510 million) based on the 658-observation

8. One of the 11 extreme observations for firm penalties, National Medical Enterprises, is not in the 658-observation sample because its enforcement action is pre-SOX.

sample. In addition, I show the results using alternative models, rather than using the Poisson pseudo-maximum likelihood. Model 6 is an OLS model, and Model 7 is a Tobit model. The *Whistleblower* dummy remains statistically insignificant after excluding those 10 extreme observations in Models 6 and 7.

CMSW also examine the relationship between whistleblowers and the severity of other enforcement outcomes such as other-penalties, employee penalties, and employee prison sentences, and find statistically significant but slightly weaker results. Part A of Table 6 shows the largest 11 values (top one percent) for the *Other-Penalties* variable in CMSW's sample of SEC and DOJ enforcement actions between 1978 and 2012 associated with alleged financial misrepresentation. Other-penalties are the total civil and criminal monetary penalties assessed against the agent firms and/or respondents (e.g. audit firm, bankers, suppliers) in connection with the financial misrepresentation of the target firm, in increments of millions of dollars. The largest amount of other-penalties (Colonial BancGroup Inc.) is \$7,532 million. The sum of the other-penalties for those 11 enforcement actions is \$12,419 million. The sum of all other-penalties in the entire sample of 1,133 enforcement actions is \$13,552 million. Those 11 enforcement actions constitute 92 percent of the total other-penalties in the sample.

Part B of Table 6 shows the largest 12 values (top one percent) for *Employee Penalties* in CMSW's sample of SEC and DOJ enforcement actions between 1978 and 2012 associated with alleged financial misrepresentation. Employee penalties are the total civil and criminal penalties assessed against all employees—consisting of disgorgement, prejudgment interest, civil fines, criminal restitution, and criminal fines—in increments of millions of dollars. The largest employee penalty (Cendant Corp) is \$6,557 million. The sum of the employee penalties for those 12 enforcement actions is \$21,521 million. The sum of all employee penalties in the entire sample of 1,133 enforcement actions is \$25,710 million. Thus, those 12 enforcement actions constitute 84 percent of the total employee penalties in the sample.

TABLE 5. Replicating CMSW's main results without the largest firm penalties (top 1 percent) using the 658 SEC and DOJ enforcement actions between 2002 and 2012 associated with alleged financial misrepresentation (Post-SOX)

	Model [1]		Model [2]		Model [3]		Model [4]		Model [5]		Model [6]		Model [7]	
	Coefficient	(<i>z</i>)	Coefficient	(<i>z</i>)	Coefficient	(<i>z</i>)	Coefficient	(<i>z</i>)	Coefficient	(<i>z</i>)	Coefficient	(<i>t</i>)	Coefficient	(<i>t</i>)
Whistleblower	-0.185	(-0.63)			0.615	(1.55)	0.064	(0.22)			2.920	(0.86)	7.430	(1.12)
Whistleblower (Tipster)			-0.636*	(-1.86)					-0.570*	(-1.88)				
Whistleblower (Nontipster)			0.042	(0.14)					0.350	(1.16)				
Control Variables	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
FF 12 Industry Fixed Effect	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
N	648		648		658		652		652		648		648	
<i>Notes:</i> The dependent variable is <i>Firm Penalties</i> . Model 1 and Model 2 exclude the 10 post-SOX extreme observations that are among the 11 pre- and post-SOX cases used in Table 3. Model [3] replaces each of those ten extreme observations with the 99th percentile value (\$338 million) of <i>Firm Penalties</i> for the full sample. Model 4 and Model 5 omit only the six largest firm-penalties cases, that is, the top one percent of <i>Firm Penalties</i> from the post-SOX sample. Exponential regression results are shown for Models 1, 2, 3, 4, and 5. Model 6 and Model 7 exclude the same 10 extreme observations excluded as in Models 1 and 2; Model 6 is an OLS model, and Model 7 is a Tobit model. Z- or t-statistics are shown, using robust standard errors. *, **, and *** denote statistical significance at the 10-percent, 5-percent, and 1-percent levels, respectively.														

TABLE 6 Part A. Largest other-penalties (top 1 percent) in CMSW's sample of SEC and DOJ enforcement actions between 1978 and 2012 associated with alleged financial misrepresentation

Record ID	Company Name	Other-Penalties (\$ Million)	Whistle-blower	Whistle-blower (Tipster)	Whistle-blower (Nontipster)
1217	Colonial BancGroup Inc.	7,532	1	1	0
148	American Continental Corp. (Lincoln Savings & Loan)	2,215	0	0	0
909	Refco Inc/Refco Group Ltd.	689	0	0	0
566	Enron Corp. (2)	518	1	1	0
531	Franklin American Corp.	466	0	0	0
706	Allou Health & Beauty Care Inc.	326	0	0	0
682	Suprema Specialties Inc.	153	1	1	0
970	Halliburton Co.	149	1	0	1
971	KBR Inc.	149	0	0	0
575	PNC Financial Services Group Inc.	122	1	0	1
123	Crazy Eddie Inc.	100	0	0	0

TABLE 6 Part B. Largest employee penalties (top 1 percent) in CMSW's sample of SEC and DOJ enforcement actions between 1978 and 2012 associated with alleged financial misrepresentation

Record ID	Company Name	Employee Penalties (\$ Million)	Whistle-blower	Whistle-blower (Tipster)	Whistle-blower (Nontipster)
442	Cendant Corp.	6,557	1	0	1
909	Refco Inc/Refco Group Ltd.	4,887	0	0	0
148	American Continental Corp. (Lincoln Savings & Loan)	3,476	0	0	0
584	Adelphia Communications Corp.	1,477	0	0	0
589	Mortgage Corporation of America (MCA Financial Corp.)	1,217	0	0	0
684	Computer Associates International Inc.	889	1	0	1
531	Franklin American Corp.	850	0	0	0
170	Sahlen & Associates Inc.	532	1	1	0
1217	Colonial BancGroup Inc.	506	1	1	0
706	Allou Health & Beauty Care Inc.	500	0	0	0
553	Lason Inc.	325	0	0	0
400	Centennial Technologies Inc.	305	0	0	0

TABLE 6 Part C. Largest employee prison sentences (top 1 percent) in CMSW's sample of SEC and DOJ enforcement actions between 1978 and 2012 associated with alleged financial misrepresentation

Record ID	Company Name	Employee Prison Sentences (Months)	Whistle- blower	Whistle- blower (Tipster)	Whistle- blower (Nontipster)
996	Satyam Computer Systems Ltd.	840	1	1	0
32	Flight Transportation Corp.	732	1	1	0
566	Enron Corp. (2)	700	1	1	0
504	Tyco International Ltd. (1)	636	1	0	1
1207	Novaferon Labs Inc.	612	0	0	0
202	ZZZZ Best Co.	540	1	1	0
576	Enterasys Networks Inc.	513	0	0	0
797	Aprisma Management Technologies Inc.	513	0	0	0
1186	Sterling Financial Corp.	456	1	1	0
531	Franklin American Corp.	436	0	0	0
571	Hamilton Bancorp Inc.	416	0	0	0

Part C of Table 6 shows the largest 11 values (top one percent) for *Employee Prison Sentences* in CMSW's sample of SEC and DOJ enforcement actions between 1978 and 2012 associated with alleged financial misrepresentation. Employee prison sentences are the total incarcerations, consisting of jail, prison, home detention, and halfway house sentences, in increments of months imposed upon employee respondents named in the enforcement action. The sum of the employee prison sentences for those 11 enforcement actions is 6,394 months. The sum of all employee prison sentences in the entire sample of 1,133 enforcement actions is 24,247 months. Those 11 enforcement actions constitute one-fourth of the total employee prison sentences in the sample.

In Part D of Table 6, I examine the relationship between *Whistleblower* and the severity of *Other-Penalties*, *Employee Penalties*, and *Employee Prison Sentences*, without using those extreme observations stated above. I use Poisson pseudo-maximum likelihood regressions in the Table, and I show that *Whistleblower* is not associated with the severity of *Other-Penalties*, *Employee Penalties*, and *Employee Prison Sentences* after omitting those extreme observations.

TABLE 6 Part D. Replicating the results of CMSW without extreme (top 1 percent) other-penalties, employee penalties, and employee prison sentences using the 1,133 SEC and DOJ enforcement actions between 1978 and 2012 associated with alleged financial misrepresentation

	Other-Penalties (\$ Million)		Other-Penalties (\$ Million)		Employee Penalties (\$ Million)		Employee Penalties (\$ Million)		Employee Prison Sentences (Months)		Employee Prison Sentences (Months)	
	Model [1]		Model [2]		Model [3]		Model [4]		Model [5]		Model [6]	
	Coefficient	(z)	Coefficient	(z)	Coefficient	(z)	Coefficient	(z)	Coefficient	(z)	Coefficient	(z)
Whistleblower	-0.162	(-0.33)			0.208	(0.67)			0.254	(0.89)		
Whistleblower (Tipster)			-0.589	(-0.69)			0.280	(0.63)			0.331	(0.90)
Whistleblower (Nontipster)			0.146	(0.31)			0.131	(0.47)			0.173	(0.49)
Control Variables	Yes		Yes		Yes		Yes		Yes		Yes	
FF 12 Industry Fixed Effect	Yes		Yes		Yes		Yes		Yes		Yes	
N	1,122		1,122		1,121		1,121		1,122		1,122	
<i>Notes:</i> These are exponential regression results, with z-statistics shown, using robust standard errors. *, **, and *** denote statistical significance at the 10-percent, 5-percent, and 1-percent levels, respectively.												

TABLE 7. Alternative models

	Other-Penalties (\$ Million)		Other-Penalties (\$ Million)		Employee Penalties (\$ Million)		Employee Penalties (\$ Million)		Employee Prison Sentences (Months)		Employee Prison Sentences (Months)	
	Model [1]		Model [2]		Model [3]		Model [4]		Model [5]		Model [6]	
	Coefficient	(t)	Coefficient	(t)	Coefficient	(t)	Coefficient	(t)	Coefficient	(t)	Coefficient	(t)
Whistleblower	-0.075	(-0.14)	0.83	(0.58)	2.575	(1.12)	3.553	(1.37)	4.657	(0.75)	10.659	(0.57)
Control Variables	Yes		Yes		Yes		Yes		Yes		Yes	
FF 12 Industry Fixed Effect	Yes		Yes		Yes		Yes		Yes		Yes	
N	1,122		1,122		1,121		1,121		1,122		1,122	
<i>Notes:</i> Models 1, 3, and 5 are OLS models, and Models 2, 4, and 6 are Tobit models. T-statistics are shown, using robust standard errors. *, **, and *** denote statistical significance at the 10-percent, 5-percent, and 1-percent levels, respectively.												

I also demonstrate the results by using alternative models. Models 1, 3, and 5 in Table 7 are OLS models, while Models 2, 4, and 6 are Tobit models. The key results remain statistically insignificant.

Although it would be considered uncommon, it is also possible to remove extreme observations of one enforcement-outcome variable to investigate the robustness of the results on another enforcement-outcome variable. For instance, one can remove the extreme observations of *Firm Penalties* from the sample and show the robustness of CMSW's results for *Employee Penalties* and *Employee Prison Sentences*. Models 1 and 2 of Table 8 show those results. If we drop the extreme observations this way, the results of CMSW remain robust. There is certainly not one right way to do robustness checks. But we need to observe that none of the 11 extreme observations of *Firm Penalties* matches with any extreme observations of *Employee Penalties* and only one of the extreme observations of *Firm Penalties* and *Employee Prison Sentences* is the same.

As for results on mere incidence, that is, yes/no on the involvement of a whistleblower on enforcement outcomes, CMSW write:

We find that whistleblower involvement is positively associated with the incidence of firm penalties ($p < 0.05$) and prison sentences ($p < 0.10$), but we do not find a significant result for the incidence of employee penalties. These results suggest that whistleblower involvement is associated with an 8.58% increased likelihood that the SEC imposes monetary sanctions on the firm and a 6.64% increased likelihood of criminal sanctions against the targeted employees. (CMSW, 157)

Given the natural correlations we would expect among severity of misconduct, likelihood of penalties, and whistleblowing, somewhat like the correlations among the severity of health emergencies, the likelihood of medical interventions, and calls to 9-1-1, it is surprising that CMSW did not find a statistically significant correlation for one of the enforcement-outcome categories. The other correlations are perhaps less than one would expect. I examine whether whistleblowing is associated with the incidence of firm penalties after dropping the extreme values of *Firm Penalties*. I use a logit model with a binary dependent variable indicating whether a firm penalty was assessed. Model 3 of Table 8 shows, unsurprisingly, since we just are dropping a few cases in the large sample of yes-or-no data, that the findings of CMSW for the incidence of *Firm Penalties* remain.

TABLE 8. Replicating the results of employee penalties, employee prison sentences, and the incidence of firm penalties without extreme (top 1 percent) firm penalties using the 1,133 SEC and DOJ enforcement actions 1978–2012 associated with alleged financial misrepresentation

	Employee Penalties (\$ Million)		Employee Prison Sentences (Months)		Pr (Firm Penalties)	
	Model [1]		Model [2]		Model [3]	
	Coefficient	(z)	Coefficient	(z)	Coefficient	(Chi-squared)
Whistleblower	1.295***	(2.87)	1.056***	(3.29)	0.867***	(9.55)
Control Variables	Yes		Yes		Yes	
FF 12 Industry Fixed Effect	Yes		Yes		Yes	
N	1,122		1,122		1,122	
<i>Notes:</i> Exponential regression results are reported for Models 1 and 2. Model 3 is a logit model. Z-statistics and chi-squared are shown, using robust standard errors. *, **, and *** denote statistical significance at the 10-percent, 5-percent, and 1-percent levels, respectively.						

I also acknowledge the difficult issue involved in dropping extreme observations from any data. Some might liken that to throwing the baby out with the bathwater. Extreme observations in some settings can be highly economically important. In terms of frequency among firms, both whistleblowing and enforcement action cases would be considered uncommon in the population of public firms. The combination of these events can have big economic implications. Extreme observations can also help us understand a unique phenomenon. Although many extreme-setting studies in accounting and finance such as those by Merle Erickson, Michelle Hanlon, and Edward L. Maydew (2004), John R. Graham and Alan L. Tucker (2006), and Karthik Ramanna and Sugata Roychowdhury (2010) are non-generalizable to all firms, those studies still provide us some insights. We can learn something valuable from extreme settings. Most extreme observations may be especially important in helping us to understand a phenomenon. Nine of the 11 extreme observations for firm penalties in CMSW are purported to involve whistleblowers. Thus, one might argue that whistleblowers are particularly important in helping uncover the most severe violations, thus supporting the conclusion of CMSW's study.

It is possible, however, that characteristics such as whistleblowing just happen to be present in extreme observations. One would want to look, by qualitative investigation, at the extreme observations and see whether whistleblowing was crucial. Katherine Guthrie, Jan Sokolowsky, and Kam-Ming Wan (2012) did such an investigation in their 2012 *Journal of Finance* paper. Guthrie et al. (2012) reexamine the results of Vidhi Chhaochharia and Yaniv Grinstein (2009). Chhaochharia and Grinstein (2009) find that CEO pay decreases by 17 percent more in firms whose boards were not compliant with the recent NYSE/NASDAQ independence requirements than in firms that were compliant. How-

ever, Guthrie et al. (2012) investigate two extreme observations in Chhaochharia and Grinstein's (2009) sample and prove that in neither of the two cases could the board structure have been a reason that CEO compensation changed. After dropping those observations, Guthrie et al. (2012) find that board independence has no effect on the level of CEO pay. A similar type of investigation for those extreme observations, that is whether whistleblowing played an important role in those enforcement actions, would be ideal for CMSW. On page 164, however, CMSW state "most of the whistleblower allegations in our sample are obtained from OSHA, and we cannot directly observe whether the SEC or DOJ actually used the information from each OSHA whistleblower. As a result, these cases represent potential whistleblower involvement in an enforcement action." In other words, we may not be able to investigate whether whistleblowers played a crucial role in those nine extreme cases given the nature of CMSW's whistleblower data, though ideally we would like to see a qualitative investigation. We do not know whether whistleblowing in the few extreme observations was like 9-1-1 calls following a heart attack.

However, we can rely on the extensive analysis of whistleblowing cases for the alleged corporate misconduct by Dyck et al. (2010) to do our qualitative investigation for some of those extreme observations of CMSW. In this excellent and well-known study (957 Google Scholar citations as of January 25, 2019) Dyck et al. (2010) look at what actors bring corporate fraud to light by gathering data on a comprehensive alleged corporate fraud that took place in the U.S. firms with 750 million dollars in assets between 1996 and 2004. Dyck et al. (2010)'s data includes cases such as Enron and WorldCom, which are also extreme observations in CMSW. Thus, we can compare the codings for some of CMSW's extreme observations with Dyck et al. (2010)'s classification of actors bringing fraud to light. Dyck et al. (2010)'s classifications include analyst, auditor, client or competitor, employee, equity holder, firm, industry regulator/government agency or self-regulatory organization, law firm, newspaper, SEC, and short-seller. Dyck et al. (2010) is an excellent example of how qualitative investigation should be conducted. I shall provide a lengthy quotation from the paper to show their way of clarifying and explaining how they classify each fraud case:

Our key variable is the identity of the actor who brings each fraud to light. To uncover the fraud detectors for each of our 216 cases, we search Factiva for news wires and articles over the period from 3 months prior to the class action period (defined as the period over which the suit claims misbehavior) to the settlement date or the current date, if the case is still pending. Our searches return approximately 800 articles per case. The point to reading so many articles for each case is to understand, as much as possible, the circumstances of the alleged fraud and the detector who reveals the

information.

In a number of cases, we find that the whistleblower is not the person labeled by the media as such. A chain of events initiated by another party may already be forcing the scandal to light when an individual expedites the process by disclosing internal information. For instance, Enron's whistleblower by our classification is the Texas edition of the *Wall Street Journal*, not Sherron Watkins, who is labeled the Enron whistleblower. Of course, we do not wish to undercredit the importance of individuals who contribute details as the fraud emerges. However, our aim is to identify the initial force that causes a scandal to come to light.

To mitigate potential concerns about subjectivity in identifying the first actor to bring each fraud to light, we implement a meticulous procedure. The initial coding of each case was done by a research assistant (a law student) and, independently, by at least one of the authors. Where judgment was required, all three authors analyzed the case until a consensus was reached. A year after the initial coding we divided the cases into thirds and each of the authors recoded cases without referencing the prior coding. Again, when the coding was at all unclear, all three authors read the case to ensure consistency in interpretation. In the process of verifying our coding, we went back to our sources and created a list of the news article(s) that were most informative in pointing to which player was the whistleblower.⁹ We sent this document to academic colleagues who work in corporate governance and to the NBER corporate governance list soliciting comments regarding the details of particular cases. (Dyck et al. 2010, 2218–2222)

The online appendix of Dyck et al. (2010) shows the summaries of 216 fraud cases in their sample ([link](#)). In each of the summaries, they identify the responsible actor along with a representative quote from the available evidence. Table 3 of my paper shows the nine largest firm penalties of CMSW with employee whistleblowers. Six out of those nine cases are also in Dyck et al. (2010)'s sample; the three that are not are Siemens, AIG, and Fannie Mae.

Let's take a closer look at those six cases: Enron, WorldCom, Halliburton, Reliant Resources, America Online, and Bristol Myers Squibb. For Enron, CMSW data show an employee as a tipster whistleblower. In other words, CMSW data claims that because of the employee allegation, the investigation for Enron was started. However, if we look at the sample of Dyck et al. (2010), we see that a newspaper brought the fraud to light and not the employee as suggested by CMSW's whistleblowing data. In their online appendix, Dyck et al. (2010) provides qualitative evidence for a newspaper having been the 'tipster' in the Enron case:

9. I have omitted from the quotation here a parenthetical pointer to the appendix containing the list. Now the appendix is available on Adair Morse's website ([link](#)).

The Texas version of the Wall Street Journal publishes a story in the fall of 2000 asking whether the profits from companies like Enron are just artifacts of the firms' manipulation of marking assets to market. A few months later, Fortune and the New York Times publish articles questioning the ultimate origin of value in the stock run-up of Enron and covering Enron's incredulous behavior in the California energy crisis, respectively. Meanwhile, following the Texas WSJ article, short sellers begin increased scrutiny of the firm and, in particular, into Enron's financing entities. Not long after the CEO resigns in August 2001, information comes to light that the firm had misrepresented the value of its assets by billions of dollars, and related party transactions were siphoning value from the firm to the benefit of executives. A number of other improprieties emerge. Shareholders have claimed \$30 billion in damage from now-defunct Enron; litigation continues. Officers settle with SEC for \$64.4 million and Auditors and investment banks settle with SEC for \$7.3 billion. (Dyck et al. 2010, online appendix p. 21)

Similarly, Dyck et al. (2010) indicate that their classifications for WorldCom, Halliburton, Reliant Resources, America Online, and Bristol Myers Squibb were, respectively: SEC, Newspaper, Industry Regulator, Auditor, and Firm. Thus, Dyck et al. show no *employee* tipsters for these five cases. CMSW code all five of these cases as having an employee whistleblower.

Only 26 out of 216 cases classified in Dyck et al. (2010) data are employee whistleblowers. Andrew Call, one of the coauthors of CMSW, used those 26 employee-based whistleblowing events as identified by Dyck et al. (2010) in Call, Kedia, and Rajgopal (2016), so he knew of Dyck et al. (2010). In CMSW he should have at least mentioned the inconsistencies. Dyck et al. (2010)'s data only goes through 2004, but some of CMSW's cases, which go through 2012, such as Enron, are coded as employee tipster by CMSW but not by Dyck et al., which instead cites a newspaper as the tipster.

Conclusion

With the abundance of data in our time, the number of empirical research studies in accounting and finance that show some statistically significant relationship has increased substantially. Many policy makers use those findings to initiate new policies or justify existing policies. Government laws and regulations that aim to prevent corporate misconduct can burden the economy and do little good for their intended purpose. Some researchers even question the efficacy of the government's enforcement of securities laws relative to private enforcement. For instance, Rafael La Porta, Florencio Lopez-De-Silanes, and Andrei Shleifer

(2006) find that private enforcement is more important than public enforcement for financial market development.

In this paper, I suggest that my investigations and considerations should move all of us further toward doubt and skepticism about the findings of CMSW. We should not reach a definitive conclusion about the value of whistleblowing programs based on CMSW.

Unfortunately, the extreme-values problem is not unique to CMSW. This issue is very common in the financial misconduct literature (e.g., Files, Martin, and Rasmussen 2018; Correia 2014; Yu and Yu 2011). Therefore, I recommend six practices that financial misconduct researchers should implement in their future studies to address the problem and related issues:

1. *Be upfront about the extreme-values problem.* Financial misconduct scholars should give adequate attention to the extreme-values issue in their analysis. Extreme values in the data should be identified and readers should be alerted clearly and early in the analysis about the identities and directions of those extreme observations. Mentions of extreme values in the study that are brief, peripheral, and too late can be considered as deceptive. Scholars can show the extreme observations such as WorldCom and Enron, together with the main variable of interest, in a table as I have done in Table 3.
2. *Show the results without extreme observations in an analysis:* Extreme observations such as Enron, WorldCom, Cendant, Colonial Bancgroup are here to stay. Scholars should investigate and show how extreme observations influence their main findings. I could find only one paper (Files 2012) in the literature that shows in an analysis that the main findings of a paper remain robust after excluding the top one percent of enforcement outcomes from the data.
3. *Be clear about the lineage of each and every coding.* Especially when plagued by the extreme-values problem, it is crucial to reduce and resolve any fuzziness about how the coding of each observation was arrived at.
4. *Do a qualitative investigation of extreme-value observations:* Financial misconduct scholars should do a qualitative investigation of extreme-value observations to understand whether the variable in question really play a crucial role (e.g., Guthrie et al. 2012). For instance, the variable in question in CMSW is *Whistleblower* and CMSW's data show that the top nine observations for firm penalties have employee whistleblowers. If CMSW had done a qualitative investigation, they might have seen that it was other actors (e.g., the SEC, newspapers, industry regulators, auditors, and firms) and not employees that played a crucial role for

some of those extreme cases (e.g., Reliant and Enron).

5. *Be cautious in identifying implications of findings*: Financial misconduct scholars should realize that because of the extreme-value problem in enforcement outcomes, the findings of the literature cannot be generalized. Scholars should not present (either implicitly or explicitly) their findings as providing solid support for government policies. They need to be cautious not to overstretch their findings. For instance, in the third paragraph of their conclusion, CMSW accurately state that “our study should not be interpreted as an examination of the efficacy of...any whistleblowing program,” but in the final paragraph they state that “this study makes important contributions...to policy discussions on the efficacy of...formal whistleblowing programs.” Which one of those statements should we take seriously? Scholars should avoid this type of ambiguous statement of the implications of an analysis. When scholars give an interview about their studies to the media, they should also mention the extreme-value problem and other limitations of their studies.
6. *Avoid one-sided analysis*: Policies such as the SEC Whistleblower Program have negative as well as positive consequences. Financial misconduct scholars should consider both benefits and costs of policy judgments in their analysis. We should never suppose that private enterprise and free markets work perfectly, but even less should we assume that government does.

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