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# **Corporate Crime Deterrence: A Systematic Review**

Sally S. Simpson, Melissa Rorie, Mariel Alper, Natalie  
Schell-Busey

With William S. Laufer and N. Craig Smith



**THE CAMPBELL COLLABORATION**

# Colophon

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# Executive summary/Abstract

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## BACKGROUND

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Corporate crime is a poorly understood problem with little known about effective strategies to prevent and control it. Competing definitions of corporate crime affect how the phenomenon is studied and implications for reducing it. Therefore, in this review, we use John Braithwaite's definition (1984: 6) which specifies that corporate crime is "the conduct of a corporation, or of employees acting on behalf of a corporation, which is proscribed and punishable by law." Consistent with this approach, this review focuses on various legal strategies aimed at companies and their officials/managers to curtail corporate crime. Interventions may be punitive or cooperative, but the goal is to prevent offending and increase levels of corporate compliance.

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## OBJECTIVES

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Our overall objective is to identify and synthesize published and unpublished studies on *formal legal and administrative* prevention and control strategies—i.e., the actions and programs of government law enforcement agencies, legislative bodies, and regulatory agencies on corporate crime. We then assess the impact of these strategies on individual and company offending. Included are legal and administrative interventions such as new laws or changes in laws, inspections by regulatory agencies, punitive sanctions and non-punitive interventions aimed at deterring or controlling illegal behaviors.

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## CRITERIA FOR INCLUSION OF STUDIES

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We were highly inclusive in our selection criteria, including studies that encompass a wide variety of methodologies: experimental (e.g., lab studies or vignette surveys), quasi-experimental (e.g., pre/post-tests), and non-experimental (e.g., correlational statistics using secondary data). The studies included also contained a wide variety of

data (e.g., data from official agencies, corporate reports, individuals' survey responses, etc.). Our search included published and unpublished articles, reports, documents, and other readily available sources. The outcome of interest, corporate offending, could reflect actual behavior or behavioral intentions as reported by respondents.

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## **SEARCH STRATEGY**

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Our search strategy included numerous online databases and other sources (41 in total) to identify published and unpublished studies consistent with our definition of corporate crime. Sixty-nine search terms were generated to conduct the search, including specific terms associated with corporate offending (misconduct, corruption, unethical conduct, and organizational crime), offense type, and legal/regulatory strategies. For each database and search term, we tracked the number of “hits” gleaned from the search and determined whether the article related to corporate crime deterrence (broadly defined) and if there was quantitative evidence that might be coded for a systematic review. Studies meeting both criteria were considered “potentially eligible” and went through the next phase of eligibility coding.

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## **DATA COLLECTION AND ANALYSIS**

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This research proceeded in two phases—the first phase (through 2003) collected studies of any intervention type (legal and otherwise) that focused on corporate crime prevention and control. The second phase of the project focused exclusively on prevention and control strategies solely in the legal area. Reflecting the broader focus of the first phase, we created a coding protocol that included the entire domain of corporate crime prevention/deterrence research which included the specific treatment variables we are interested in here—legal restraints. Two hundred and sixty five studies were eligible for inclusion. From each included study, we coded various forms of data classified into and calculated multiple types of effect sizes. These included 1) the “standardized mean-difference effect size” for analyses comparing two groups’ performance on a continuous outcome, 2) the “product-moment correlation effect size” for relationships in which both the independent variable and dependent variable are continuous (or assumed to represent a continuous construct), and 3) the “Odds-Ratio Effect Size” when both the independent and dependent variables involved are dichotomous. Interventions were classified into six treatments and broken down by subgroups (unit of analysis and whether the design was cross-sectional or longitudinal). We also conducted moderator analyses.

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## MAIN RESULTS

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Out of the 40 possible treatment categories, we were able to calculate a mean effect size for 19. Although most showed a positive albeit non-significant treatment effect, some (including a significant effect) were iatrogenic. Looking at the specific mechanisms, the impact of **law** on corporate crime showed a modest deterrent effect at the firm and geographical level of analysis (there was not enough data to calculate effect sizes for individuals). However, this finding is limited to cross-sectional studies. For **punitive sanctions**, where there was substantially more data from which to calculate effect sizes, we observe a similar pattern: A tendency toward deterrence across units of analysis, with relatively few significant effects regardless of whether data are cross-sectional or longitudinal. The one area where there appears to be a consistent treatment effect is in the area of **regulatory policy**, but only at the individual level. Effects for other levels are contradictory (with some positive and others iatrogenic) and none are statistically significant. Regarding moderator effects, the least methodologically rigorous designs— those that were not experimental versus experimental designs and those without statistical control variables versus controls were associated with a treatment effect. We also found that older studies were associated with stronger deterrent effects—perhaps because the older studies are less methodologically rigorous than those that are newer. Other moderator results were less clear (publication bias, country bias, disciplinary bias; offense type), but given how few of the analyses revealed strong treatment effects overall we think it is premature to draw any conclusions from these findings and call instead for more methodologically rigorous and focused studies particularly in the punitive sanction and regulatory policy areas.

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# 1 Background for the Review

Few subject areas span as many disciplinary boundaries as does corporate crime. Since Sutherland's famous Presidential Address to the American Sociological Association in 1939 (1940) and subsequent publication of White Collar Crime ten years later, business scholars, economists, sociologists, political scientists, lawyers, and psychologists, and criminologists have speculated not just about the etiological origins of corporate crime but about the success of various strategies for its prevention and control. The recent global financial crisis has, once again, brought the problem of white-collar crime to the attention of the media, general public, and policy-makers, but there is still surprisingly little systematic and comprehensive data to properly theorize and empirically assess the phenomena or to recommend evidence-based policies to prevent and control it. This systematic review is the first of its kind to report and evaluate the empirical evidence on corporate crime deterrence strategies.

By corporate crime we mean “the conduct of a corporation, or of employees acting on behalf of a corporation, which is proscribed and punishable by law” (Braithwaite, 1984:6). Corporate crime, therefore, encompasses a wide array of illegal activities that are criminally, civilly, and administratively proscribed *and* which may be undertaken by individual managers/employees as well as by the firm (as an organizational actor) to achieve organizational goals. Corporate crimes are generally distinguished from other types of white-collar offenses by the use of organizational resources and by who gains from the offense. Thus, when Raymond Scott Stevenson, head of Tyco's tax department, directed a series of transactions designed to reduce Tyco's state tax liability by back-dating transactions to avoid reporting a \$170 million dollar federal capital gain, he used organizational resources to “benefit” the company's bottom line but not himself.<sup>1</sup>

This distinction between white-collar and corporate offending is by no means unambiguous. For instance, a top manager may utilize organizational resources to enrich him or herself--described as “collective embezzlement” by Calavita and Pontell

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<sup>1</sup> <http://www.usdoj.gov/usao/fls/PressReleases/060921-04.html>

(1991). In addition, although many acts of corporate crime are undertaken to achieve organizational goals, such acts may indirectly benefit the individual through promotion or salary bonuses. However, in accordance with Braithwaite's definition and consistent with our focus on corporate deterrence, we are interested in the kinds of behaviors typically characterized as "corporate" and not offenses where the motivation for offending largely is personal instead of organizational.

The kinds of offenses that meet our definitional criteria can include the following categories of offenses: administrative noncompliance, environmental violations, financial violations, labor violations, manufacturing violations, and/or unfair trade practices<sup>2</sup> (Clinard & Yeager, 1980:113-116). Similar to classifications of street crimes (e.g., violent crimes), each category contains a variety of specific offenses, often with distinct statutes that define illegalities and provide remedies and sanctions for violators. For instance, unfair trade practices include monopolization, price-fixing, unfair advertising, and price discrimination, among other illegal activities (Simpson, 1986). The Federal Trade Commission Act, Robinson-Patman Act, and the Sherman-Clayton Antitrust Act are some of the more significant pieces of legislation that define what constitutes unfair trade practices and the range of penalties for violators. Environmental violations are classified by different media (e.g., air, water, land) and statutes (e.g., Clean Water Act, Clean Air Act, Resource Conservation and Recovery Act). Similar to anti-competitive illegality, some of these practices are defined as criminal offenses while others fall within the civil and administrative realms. While many corporate offenses are financial, others are "violent" in nature where human lives may be lost and individuals injured (for instance, some Occupational Health and Safety Administration violations).<sup>3</sup>

A key feature of corporate offending is crime complexity. Although some offenses may be quite simple (bribery or Medicare fraud), others often involve multiple interconnected actors and organizations, occur over long periods of time, and entail manipulating shell companies and billions of dollars (such as Enron). Corporate crimes also vary by seriousness. Egregious offenses can carry substantial criminal and civil

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<sup>2</sup> These categories of offenses are particular to the United States. Other countries, especially in the west, may have similar classifications, but the laws and punishments are not necessarily comparable. In a systematic review, it is common that studies from a variety of different countries are included in the literature search and subsequent analysis. This is appropriate since a key goal of meta-analysis is generalization over studies to summarize outcomes. Although our analysis draws from different places with distinct laws and practices, our purpose is to evaluate and assess the general impact of laws and sanctions on corporate crime. To gauge whether studies conducted outside of the United States might explain differences across studies in the observed outcomes, we conduct a moderator analysis (Section 3.4.3). Results of that investigation are described in section 4.3.5.

<sup>3</sup> Workplace safety offenses range from relatively minor violations including failure to report an accident or hang a proper safety sign to more serious incidents (e.g., exposure to known toxic substances in the workplace without proper training or equipment).

sanctions while others are fairly minor “technical” violations such as when a business fails to submit a report to a regulatory agency.

Definitional murkiness, breadth, and complexity have made the phenomenon difficult to study, but other barriers exist as well. Perhaps the most salient lies with the lack of high quality data. There is no UCR-like national data base that can be used to “measure” the corporate crime problem, nor are there any systematic procedures for identifying the “hidden” figure of these crimes (i.e., there is nothing comparable to the NCVS). Most studies of corporate offending are qualitative, *ex post facto* case study investigations of often sensational events. There are only a handful of systematic scientific studies of corporate offending (including Sutherland’s original study) because most federal agencies that fund criminological research historically have focused on “street” crime. These agencies are also more apt to fund evaluation research on programs and policies in these same areas. We therefore have learned a great deal about the successes or failures of drug courts, pulling levers deterrence strategies, or gun seizures, but relatively little about whether corporate crime is deterred through criminal prosecution or other intervention strategies.

Because a review of this type has never been conducted, our approach has proceeded with a different logic than most systematic reviews. Instead of beginning with a particular intervention in mind (e.g., pulling levers, boot camps, CCTV), it was necessary first to examine and assess the kinds of studies that had been conducted to determine whether a critical mass of studies could be targeted for assessment and evaluation.<sup>4</sup> To that end, we conducted an extensive interdisciplinary search of the white-collar and corporate offending literatures without regard to specific types of corporate crime prevention or intervention strategies (see, Simpson, Laufer, Smith, Schell, Powers, Richmond, & Bears, 2008).<sup>5</sup> From this effort, we observed that much of the extant literature focused on the legal realm--on the influence or effectiveness of legal restraints on corporate crime. Consequently, a systematic review of the empirical evidence on the effects of legal remedies (broadly defined) is the subject of this report.

There are many ways in which the legal domain can prevent and deter corporate crime: (1) Laws educate the public about appropriate behavior and dictate the punishments that will be meted out should transgressions and discovery occur. In the corporate crime domain, laws such as the Sherman Antitrust Act, Sarbanes-Oxley and the Clean

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<sup>4</sup> van der laan, Smit, Busshers, and Aarten (2011) adopted a similar approach in their systematic review of cross-border trafficking in humans.

<sup>5</sup> This initial study culled documents related to any type of crime control or treatment program, including extra-legal strategies such as ethics training and internal compliance programs (see Schell-Busey, 2009), the effect of reputational damage (e.g., market penalties) and other extralegal consequences on violations, as well as the legal and administrative domains included in the current protocol.

Air Act prohibit certain behaviors and set standards for corporate practices. Violations can prompt official reaction (filing a case, prosecution, conviction) and the application of sanctions such as fines and corporate probation or, in some instances, the avoidance of punishment. (2) Regulatory policies delimit what firms can and cannot do within a particular sphere and compel particular types of behaviors. When firms fail to meet regulatory standards, administrative and civil sanctions may be brought against the company. Although there is sometimes overlap between the different systems of justice (criminal, administrative, and civil), regulatory sanctions are brought forth outside of the formal criminal justice system by administrative agencies such as the Environmental Protection Agency or the Securities and Exchange Commission. Potential actions by such agencies can be grouped into two different types of strategies. The first is “regulatory policy” which includes inspections, resources available to the agency, as well as deregulation. The second group includes “non-punitive actions” that such agencies take (e.g., warning letters, cease and desist orders).

The variety of constructs in the legal domain serve to control corporate illegality through both prevention and deterrence. Laws prevent crimes by telling potential offenders which behaviors are acceptable or prohibited—what is right and wrong. Regulatory actions such as inspections and warning letters can also educate corporations by identifying areas of non-compliance to the company. Laws may also deter crime by increasing informal monitoring of behaviors—when people know that a behavior is illegal they may be more likely to report it to formal control agents such as police or regulators. Official sanctions deter crimes generally (i.e., non-offenders will not offend because they do not want to be caught and punished) as well as specifically (i.e., offenders will not offend again because the pains of punishment were costly the first time). In addition, regulatory actions and official sanctions increase the risk of reputational damage which, in the case of public companies, can serve as a powerful deterrent.

Prior research on legal interventions has produced inconsistent results as to their effectiveness. Deterrence theory posits that fear of detection, coupled with certain and severe sanctions for offending, will promote compliance. However, in reality, offenders generally face a low risk of detection and punishments tend not to be severe (for example, fines are often smaller than the harm caused by the offense; Cohen, 1989). Additionally, government agencies employ many different strategies to promote compliance—some based in deterrence and others in cooperative strategies. Certain approaches may be more effective than others or the effect of certain strategies may be context-specific (Axelrad, 2000; Gezelius & Hauck, 2011; May, 2005; May & Winter, 1999; Welles & Enge, 2000). To better understand whether the legal system matters as a whole, or whether certain strategies are more effective than others, the present review examines a variety of interventions.

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## 2 Objectives of the Review

Our overall objective is to identify and synthesize published and unpublished studies on *formal legal and administrative* prevention and control strategies—i.e., the actions and programs of government law enforcement agencies, legislative bodies, and regulatory agencies on corporate crime. This review has considered all types of legal and regulatory practices as long as corporate crime prevention and control was part of the outcome. Based on the evidence, we provide a systematic assessment of the effectiveness of the identified strategies and programs. Generally, we assess the kind of research that is being done and its quality. Specifically, we address the following questions:

- Which kinds of legal and administrative interventions lower the risk of corporate offending?
- Do effects vary by unit of analysis (e.g., individuals vs. firm)?
- How do study characteristics influence observed outcomes?

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## 3 Materials and Methods

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### 3.1 INCLUSION AND EXCLUSION CRITERIA

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#### 3.1.1 Types of Studies

To identify the universe of studies that examined the impact of legal and administrative deterrence strategies on the occurrence of corporate crime, we searched for articles using a broad set of search terms (see section 3.2.). We then extracted studies from various sources that involved corporate crime behaviors *and* were empirical (broadly defined) in nature.

Given our objective (which, as stated above, is to identify and synthesize published and unpublished studies on *formal legal and administrative* prevention and control strategies on corporate offending) this review is limited to examining the impact of such interventions on noncompliance. Nonetheless, we acknowledge a developing literature on the subject of firm over-compliance (extreme volunteerism). Most formal legal strategies are meant to prevent offending, not promote beyond-compliance behaviors. We have included articles that look at beyond-compliance outcomes, but only calculated effect sizes representing the relationship between the independent variable and offending.

Aside from limiting our outcome, we were highly inclusive in most other aspects of the review. Given the dearth of rigorous empirical research on corporate crime, we included studies that encompass a wide variety of methodologies: experimental (e.g., lab studies or vignette surveys), quasi-experimental (e.g., pre/post-tests), and non-experimental (e.g., correlational statistics using secondary data). The studies included also contained a wide variety of data (e.g., data from official agencies, corporate reports, individuals' survey responses, etc.). Our search included published and unpublished articles, reports, documents, and other readily available sources. The outcome of interest, corporate offending, could reflect actual behavior or behavioral intentions as reported by respondents.

### **3.1.2 Types of Units of Analysis**

There were no restrictions on the unit of analysis. Included in our analyses are studies that use individual respondents, data on corporations, data on geographic areas (e.g., state emissions, cartels in various countries) and studies using other units of analysis (e.g., court cases or company-years).

### **3.1.3 Types of Interventions**

This study focused on corporate crime deterrence stemming from any form of legal interventions. We consider interventions to include any action that could ostensibly impact the decision-making of corporations. Such legal actions include: laws (e.g., the passing of Sarbanes-Oxley), punitive sanctions (e.g., arrest, fines, or likelihood of prosecution), non-punitive actions by regulatory agencies (e.g., cease and desist order) and regulatory policies (e.g., number of times a company was inspected).

There were many studies that examined more than one type of intervention—for example, the passing of a law *and* the number of regulatory inspections may have been included in a multiple regression model. When these interventions were included as separate constructs, an effect size was calculated to represent each one's relationship with offending. When a study combined constructs (e.g., by using an index), we coded the intervention as “multiple treatments involved” and calculated such effect sizes separately from the other types of actions.

### **3.1.4 Constructs Comprising Dependent Variables**

The outcome of each study is one of a variety of corporate illegal behaviors. Some examples of the outcomes include: variations in pollution emissions, official records of compliance with regulations (e.g., environmental, employment, OSHA), recidivism, safety violations/compliance, number of financial transactions, perceived intentions to offend, injuries from safety violations or environmental accidents, convictions, citations, noncompliant inspections, compliance measures (e.g., self-ratings), accuracy of regulatory records, complaints (e.g., about consumer fraud), and perceptions of enforcement effectiveness. All measures included in the study fall under one of these descriptions.

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## **3.2 SEARCH STRATEGIES FOR IDENTIFICATION OF STUDIES**

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### **3.2.1 Keyword Search on Online Abstract Databases and Internet Databases**

The first step in finding studies to be included was an exhaustive search of multiple online databases and other sources. Within each source, we employed 69 search strings

(given below). Published articles were found by applying each search string to the following databases:

- Social Work Abstracts
- ABI
- PsycINFO
- Sociological Abstracts
- ERIC
- CJA
- Worldwide Political Science Abstracts
- BSP
- EconLit
- PAIS International
- WorldCat

In addition to searching for published documents, we conducted a search of unpublished and additional published documents in the following sites:

- Google Scholar
- Digital Dissertation databases
- Department of Justice website
- Securities and Exchange Commission website
- Federal Trade Commission website
- Occupational Safety and Health Administration website
- European Corporate Governance Institute website
- DLA Piper website
- International Chamber of Commerce website
- National White Collar Crime Center website
- Financial Crimes Enforcement Network website
- Ministry of Finance Netherlands website
- United Nations Office on Drugs and Crime website
- Royal Canadian Mounted Police website
- European Commission—Company Law, Corporate Governance and Financial Crime Unit website
- American Prosecutors Research Institute: White Collar Crime Unit website
- Association of Inspectors General website
- Commodity Futures Trading Commission website
- U.S. Department of Justice Tax Division website
- U.S. Department of Justice Criminal Division: Fraud Section website
- U.S. Secret Service Financial Crimes Division website
- Ethics Resource Center website
- International Association of Financial Crimes Investigators website
- Transparency International website
- World Trade Organization website
- British Home Office of Foreign and Commonwealth website
- Department for Business Enterprise and Regulatory Reform website
- Crime Research Centre website
- Australia Institute of Criminology website
- The World Bank website

A complete list of the exact search strings used to collect studies from the above databases is given below:

- Sanction and Accounting Fraud
- Sanction and Anti-competitive Behavior
- Sanction and Antitrust
- Sanction and Business Corruption
- Sanction and Business Crime
- Sanction and Business Misconduct
- Sanction and Business Violations
- Sanction and Corporate Corruption
- Sanction and Corporate Manslaughter
- Sanction and Corporate Crime
- Sanction and Corporate Misconduct
- Sanction and Corporate Violations
- Sanction and Environmental Crime
- Sanction and Health Care Fraud
- Sanction and Organizational Corruption
- Sanction and Organizational Crime
- Sanction and Organizational Misconduct
- Sanction and Organizational Violations
- Sanction and Securities Fraud
- Sanction and Ethical Business Culture
- Sanction and Unethical Conduct
- Sanction and Unethical Behavior
- Sanction and White Collar Crime
- Fine and Accounting Fraud
- Fine and Anti-competitive Behavior
- Fine and Antitrust
- Fine and Business Corruption
- Fine and Business Crime
- Fine and Business Misconduct
- Fine and Business Violations
- Fine and Corporate Corruption
- Fine and Corporate Manslaughter
- Fine and Corporate Crime
- Fine and Corporate Misconduct
- Fine and Corporate Violations
- Fine and Environmental Crime
- Fine and Health Care Fraud
- Fine and Organizational Corruption
- Fine and Organizational Crime
- Fine and Organizational Misconduct
- Fine and Organizational Violations

- Fine and Securities Fraud
- Fine and Ethical Business Culture
- Fine and Unethical Conduct
- Fine and Unethical Behavior
- Fine and White Collar Crime
- Regulatory Policy and Accounting Fraud
- Regulatory Policy and Anti-competitive Behavior
- Regulatory Policy and Antitrust
- Regulatory Policy and Business Corruption
- Regulatory Policy and Business Crime
- Regulatory Policy and Business Misconduct
- Regulatory Policy and Business Violations
- Regulatory Policy and Corporate Corruption
- Regulatory Policy and Corporate Manslaughter
- Regulatory Policy and Corporate Crime
- Regulatory Policy and Corporate Misconduct
- Regulatory Policy and Corporate Violations
- Regulatory Policy and Environmental Crime
- Regulatory Policy and Health Care Fraud
- Regulatory Policy and Organizational Corruption
- Regulatory Policy and Organizational Crime
- Regulatory Policy and Organizational Misconduct
- Regulatory Policy and Organizational Violations
- Regulatory Policy and Securities Fraud
- Regulatory Policy and Ethical Business Culture
- Regulatory Policy and Unethical Conduct
- Regulatory Policy and Unethical Behavior
- Regulatory Policy and White Collar Crime

For each database and search string, we tracked the number of “hits” gleaned from the search (see Figure 1). For each citation, we reviewed the abstract to determine 1) whether the article related to corporate crime deterrence and 2) if there was quantitative evidence of some kind that might be coded for a systematic review. Studies meeting both of those criteria were considered “potentially eligible” and went through the next phase of eligibility coding, described below.

### **3.2.2 Relevant Articles Coded for Inclusion**

After gathering the “potentially eligible” articles as described above, coders examined each document in its entirety and decided whether it would be included in the final dataset. Inclusion depended on whether the study met five criteria:

- 1) The study was an evaluation of a corporate crime prevention/control strategy in the legal or administrative domains (i.e., deterrence resulting from effective regulations, fines, regulatory inspections, etc.).
- 2) The study includes a comparison group (or a pre-intervention comparison period in the case of pre-post studies) that did not receive the treatment condition. Studies may be experimental, quasi-experimental, or pre-post evaluations. If the study does not include a treatment *group*, does it report standardized regression coefficients/Pearson correlations if the treatment is measured continuously?
- 3) The study reports on at least one crime/misconduct outcome. In accordance with our broad definition of corporate crime (see Section 1), the outcome of interest may be one of a wide range of criminal behaviors, regulatory violations, or civil violations.
- 4) The study is written in English, but may be cross-national.
- 5) The study was published before 2012. (Plans to update the study after this current review are described in Section 5.)

During this process, studies could be coded as “Eligible,” “Not Eligible,” or as a “Relevant Review.” Articles meeting all five criteria were gathered for further coding (described in Section 3.3.). A document deemed as a “relevant review” meant that although it did not meet all 5 criteria for inclusion, there were other studies cited within the piece that needed to be examined for potential inclusion.

### **3.2.3 Web of Science Search**

All articles deemed “eligible” were entered into the Web of Science database for forward searching.

### **3.2.4 Search of Leading Journals**

In addition to searching through bibliographies of studies that were found during our search, we also perused the contents of leading journals in the field. Specifically, we examined the Table of Contents for all years of *The Journal of Law and Economics* and *The Journal of Human Resources*. These journals were chosen because many of the studies already deemed eligible were published in those outlets.

### **3.2.5 Requested Studies from Experts**

After coding all articles, we compiled a preliminary bibliography of included studies. We e-mailed this list to a group of experts in the corporate crime domain, some of whom provided us with additional studies that they thought met our criteria. Note that this was only done after the initial study search described in Section 1 (for articles published before 2004) and was not completed for the second round of searching.

### **3.2.6 Conclusion of the Search**

Our search for all legal deterrence studies in the corporate crime domain was completed in the summer of 2012. All citations determined to be eligible were coded into a Microsoft Excel database for the purpose of calculating effect sizes (see Section 3.3.). During this coding process, we discovered that many studies did not include the necessary data to calculate effect sizes, so an effort was made to contact the lead author of each study and request the necessary information.

Figure 1 provides the number of cases at each step in the process, to more fully illustrate how studies were funneled out of the current meta-analysis. It also demonstrates how many cases were lost due to missing data. It is important to reiterate that the present report actually combines the efforts of two separate searches. The first search sought out articles on all corporate crime prevention efforts prior to 2004 (but eventually dropped any not relevant to legal deterrence) while the second search sought out studies specific to legal deterrence in the corporate crime domain. Figure 1 therefore starts off with 2 different branches of the search that eventually merge as all cases were combined for analyses. As is apparent from the number of “hits” from the relevant search terms, both before and after 2004, the winnowing process was extremely arduous and time consuming.

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## **3.3 DETAILS OF STUDY CODING CATEGORIES**

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### **3.3.1 Coding Protocol**

For the initial study, we created a coding protocol that included the entire domain of corporate crime prevention/deterrence research. As previously noted, this protocol included the specific treatment variables we are interested in here—legal restraints. The entire coding protocol is attached as Appendix VI. In this document, the variable named “TREATMENT” (p. 12-13 of the current document) provides all potential descriptions of the treatment program—including those not used in the current study. The interventions that are relevant to the current discussion are those categorized as:

- 2) Law—any formal statute that prohibits or restricts corporate behavior, such as environmental laws, financial crime laws, antitrust laws, etc.
- 3) Official Sanction/Fine—any formal punishment that a corporation or individual manager acting on behalf of the company can receive, including a guilty verdict, criminal prosecution, a jail sentence, or monetary fines,
- 4) Regulatory Policy—normal regulatory procedures designed to increase the likelihood that corporate crime is discovered (e.g., the number of

inspections by agency personnel, agency budget, the number of regulatory personnel, or a lack of regulatory stringency),

- 14) Other—any strategy that didn't fall under one of these other categories
- 15) Multiple Treatments—as previously mentioned, some studies created a construct that incorporated more than one approach where separate effect sizes could not be calculated for each intervention (e.g., combining the scores on questions about the perceived likelihood of prosecution as well as the perceived likelihood of regulatory inspection),
- 16) Non-punitive Action by Regulatory Agency—administrative actions taken in reaction to knowledge of noncompliance but does not entail a formal punishment (e.g., warning letters, cease-and-desist orders).

The protocol includes codes used to describe the source of the study (Section I of Appendix VI; e.g., country of publication, journal's disciplinary area), characteristics of the study (Section II; e.g., randomized experiment or not, start/end date of data collection, concerns about validity), sample characteristics (Section III; e.g., whether individuals or corporations), the methods and procedures used by the study authors (Section IV; e.g., use of a control group), descriptions of the independent variable (Section V; e.g., construct and operationalization), descriptions of the dependent variable (Section VI; e.g., construct and operationalization), effect size data (Section VII; i.e., coding the data provided that will be employed to calculate an effect size), and then conclusions made by the study authors (Section VIII). There are also shaded boxes at the very end that describe the various types of effect sizes and relevant statistics needed for future analysis.

### **3.3.2 Inter-rater Reliability**

Two members of the research team entered all data from the eligible articles into a Microsoft Excel spreadsheet. There were two phases of data collection, described above—one collecting articles published before 2004 and another collecting and coding articles published from 2004 – 2011. We conducted two separate inter-rater reliability tests since one of the initial coders was replaced in the second data collection. For the pre-2004 articles, an initial coding session was completed in which 80 articles collected at that point were used for inter-rater reliability testing. Coders went through 20 articles at a time and resolved differences between the two databases. Often, this collaboration would result in decision rules (provided as endnotes in the codebook). After reviewing approximately 80 articles in this manner, an acceptable inter-rater reliability was established for most variables (those not reaching either a Kappa value or Pearson correlation value of 0.70 were not be used in further analyses). The same process took place for the post-2003 articles, although in this phase acceptable reliabilities were established after 20 articles. The coders split the rest of the articles for

independent coding. No changes were made to the coding sheet after an acceptable inter-rater reliability was established and no additional decision rules were necessary.

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## 3.4 STATISTICAL PROCEDURES

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### 3.4.1 Calculating Effect Sizes

Our systematic review included a wide variety of studies and outcome measures. As such, we coded various forms of data and calculated multiple types of effect sizes. These included 1) the “standardized mean-difference effect size” for analyses comparing two groups’ performance on a continuous outcome, 2) the “product-moment correlation effect size” for relationships in which both the independent variable and dependent variable are continuous (or assumed to represent a continuous construct), and 3) the “Odds-Ratio Effect Size” when both the independent and dependent variables involved are dichotomous.<sup>6</sup> When reporting the results, we only compare similar effect sizes—for example, a standardized mean-difference effect size from Study A was NOT included in the same analysis as a product moment correlation effect size in Study B. We calculated effect sizes both by hand in Microsoft Excel as well as using David B. Wilson’s Effect Size Calculator available on the Campbell Collaboration website. Based on Lipsey and Wilson’s (2001) recommendations, the mean-difference effect sizes were transformed using Hedges’ small sample bias correction and all product-moment correlation effect sizes were converted to Fisher’s z for analyses.

Note that it is inappropriate to create an average effect size when the unit of analysis differs among studies, when the constructs differ, or when some studies are cross-sectional while others are longitudinal. Therefore, we attempted to calculate separate effect sizes for 40 different groups. However, we do not report results for categories where there are too few studies from which to draw reasonable conclusions. For example, we are unable to report results for non-punitive sanctions (data are available on request); there are also many subgroups with too few cases. The treatments and subgroups for which we do report are bolded in the following list<sup>7</sup>:

- 1) Treatment: Law
  - a. Individual Samples/Cross-sectional
  - b. Corporate Samples/Cross-sectional (122)**
  - c. Geographic Areas/Cross-sectional (123)**
  - d. Other units of analysis/Cross-sectional

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<sup>6</sup> There was not enough data to conduct a meta-analysis on Odds-Ratio effect sizes.

<sup>7</sup> The numbers in parentheses are a reference code identifying the subgroup’s location in the forest plots (see Appendix IV).

- e. Individual Samples/Longitudinal
  - f. Corporate Samples/Longitudinal
  - g. Geographic Areas/Longitudinal (223)**
  - h. Other units of analysis/Longitudinal
- 2) Treatment: Punitive Sanctions
- a. Individual Samples/Cross-sectional (131)**
  - b. Corporate Samples/Cross-sectional (132)**
  - c. Geographic Areas/Cross-sectional (133)**
  - d. Other units of analysis/Cross-sectional (134)**
  - e. Individual Samples/Longitudinal (231)**
  - f. Corporate Samples/Longitudinal (232)**
  - g. Geographic Areas/Longitudinal (233)**
  - h. Other units of analysis/Longitudinal
- 3) Treatment: Non-punitive Sanctions
- a. Individual Samples/Cross-sectional
  - b. Corporate Samples/Cross-sectional
  - c. Geographic Areas/Cross-sectional
  - d. Other units of analysis/Cross-sectional
  - e. Individual Samples/Longitudinal
  - f. Corporate Samples/Longitudinal
  - g. Geographic Areas/Longitudinal
  - h. Other units of analysis/Longitudinal
- 4) Treatment: Regulatory Policies
- a. Individual Samples/Cross-sectional (141)**
  - b. Corporate Samples/Cross-sectional (142)**
  - c. Geographic Areas/Cross-sectional (143)**
  - d. Other units of analysis/Cross-sectional (144)**
  - e. Individual Samples/Longitudinal (241)**
  - f. Corporate Samples/Longitudinal (242)**
  - g. Geographic Areas/Longitudinal
  - h. Other units of analysis/Longitudinal
- 5) Treatment: Multiple treatments
- a. Individual Samples/Cross-sectional (1151)**
  - b. Corporate Samples/Cross-sectional (1152)**
  - c. Geographic Areas/Cross-sectional
  - d. Other units of analysis/Cross-sectional
  - e. Individual Samples/Longitudinal
  - f. Corporate Samples/Longitudinal
  - g. Geographic Areas/Longitudinal
  - h. Other units of analysis/Longitudinal

Following Lipsey and Wilson (2001), we computed the mean effect sizes<sup>8</sup> and the homogeneity of effects across studies using the inverse variance weight method. We assumed a random effects model and calculated variance components accordingly, using a method of moments estimator. Computations were run using Stata macros provided by D.B. Wilson (available on the Campbell Collaboration website).

### **3.4.2 Assumption of Independence**

Many studies reported more than one outcome that is relevant to our domain of interest and a number of authors published more than one article using data from the same sample. Proper analysis of the data requires that the effect sizes we calculate come from independent samples. When there were multiple effect sizes describing the same relationship that came from the same sample, the effect sizes were averaged so that each sample only provided one (see Lipsey & Wilson, 2001).

### **3.4.3 Moderator Analyses**

When calculating the average effect size for each construct of interest, we tested whether the studies within each category were homogenous—that is, whether the variability in effect sizes was due only to random sampling error or if there was something about the studies that caused the effect sizes to differ. When variability in the statistic could not be dismissed as random sampling error, we conducted moderator analyses to determine whether certain study or sample characteristics could explain the variability in effects. The measures included:

- Whether the study was published/unpublished
- The discipline of the journal where the study was published or (if not determinable) the discipline of the lead author
- The year of publication
- Whether the design was experimental/quasi-experimental as opposed to non-experimental
- Whether the study was conducted in the United States
- Whether the independent variable was constructed using official data (e.g., court records, regulatory agency records, legislative records), self-report data, observations/site visits, or “other”
- Whether the dependent variable was constructed using official data (e.g., regulatory agency records, court records), self-report data, observations/site visits, or “other”
- Whether the sample was drawn from more than one organization
- The mean age of the sample

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<sup>8</sup> We also examined the data for outliers and recoded them to be 2 standard deviations above/below the mean (Lipsey and Wilson, 2001).

- The predominant race of the sample
- The predominant management level of the sample
- The type of participants included in the sample (e.g., students, professionals)
- The level of education of the sample
- The industry from which the sample came
- The average number of employees in the companies in the sample
- The average profit of the companies in the sample
- Whether the sample was randomly selected or not
- Offense type

Many of the studies were missing information on these methodological or sample characteristics, which limited our ability to conduct moderator analyses on the full sample. We followed Williams (2013), who recommends using a fixed effects (or unpooled) model as opposed to a mixed effects model when cell sizes are small.

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## 4 Results

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### 4.1 COMPUTATION OF EFFECT SIZES

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The studies included in the meta-analysis were heterogeneous, utilizing different units of analysis, types of treatments, and temporal ordering structures. In order to compute average effect sizes based on a meaningful, more homogenous group of studies, separate mean effect sizes were calculated for each combination of treatment type (law, punitive sanction, regulatory policy, non-punitive sanction, other sanction, and multiple treatments), unit of analysis (individual-level offending, company-level offending, geographic area offending rates, or other units of analysis), and time ordering (cross-sectional, longitudinal, and both cross-sectional and longitudinal; see Section 3.4.1.). All of these categories were described above in the methods section and the frequency of effect sizes calculated for each category is presented in Appendix I.

The computation of effect sizes from the studies was often indirect, reflecting a broader problem in the crime and justice research of poor descriptive validity (Farrington, 2006). Descriptive validity is the “adequacy of reporting of key features of evaluations (e.g., design, sample sizes, characteristics of experimental units, descriptions of experimental and control conditions, outcome measures, effect sizes)” (Farrington, 2006, p. 335). This information is necessary to carry out meta-analyses and systematic reviews. Standardized effect sizes were not provided in most of the articles reviewed, and the effect sizes for the present review were calculated indirectly from a variety of statistics. The frequencies of the statistics from which effect sizes were calculated is presented in Appendix I. In addition, a table that shows the data available from each study as well as other study characteristics is presented in Appendix II. Also, due to the fact that a heterogeneous group of studies were examined with different measures of the treatments and offending outcomes, ES-Rs (correlations) were calculated from some studies and ES-Ds (standardized mean differences) were calculated for others, depending on whether the independent variable was continuous or dichotomous. Results for each of the two effect size types were calculated separately for each of the above categories (see Appendix II). Although this has the disadvantage of reducing the number of studies in each category, it is preferable to conduct separate analyses for the

two type of effect sizes to maintain statistical purity (Lipsey & Wilson, 2001, p. 57). The steps used to compute the effect sizes are described above in the methods section. The effect sizes for all of the studies are listed in Appendix III and forest plots are presented in Appendix IV. The results for each of the treatments are discussed individually below.

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## **4.2 META-ANALYSIS RESULTS**

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### **4.2.1 Law**

Sufficient numbers of effect sizes were available to examine three types of studies: cross-sectional studies examining the effect of law on company-level offending (based on ES-Ds), cross-sectional studies examining the effect of law on geographic-level offending (based on ES-Rs and ES-Ds), and longitudinal studies examining the effect of law on geographic-level offending (based on ES-Rs). We were unable to evaluate the effect of law on individual offending.

#### ***Company-level***

Five ES-Ds were calculated for cross-sectional studies at the company-level unit of analysis. These studies showed a marginal deterrent effect (ES=0.021;  $p=0.054$ ; 95% confidence limits -0.00, 0.04). All five studies had a positive effect size (indicating a deterrent effect) and the 95% confidence intervals did not intersect zero for three of the five studies.

#### ***Geographic-level***

Of the seven cross-sectional studies examining the effect of laws on geographic-level offending, three were ES-Rs and four were ES-Ds. For the former, the mean effect size showed a positive but non-significant deterrent effect (ES=0.301;  $p=0.244$ ; 95% confidence limits -0.21, 0.81). Two of the three studies had effect sizes that were directionally positive with confidence intervals that did not cross zero (indicating statistical significance). The remaining study showed a counter-intuitive (iatrogenic) effect size. However, the confidence interval crossed zero suggesting that this effect could be due to chance.

For the ES-Ds, the mean effect size also showed a non-significant deterrent effect (ES=0.10;  $p=0.17$ ; 95% confidence limits -0.04, 0.23). Although three of the four studies showed a positive effect size, the confidence interval crossed zero for three of the four studies.

There were very few longitudinal studies examining the effect of laws. However, ES-Rs were able to be calculated for two studies examining the effect of laws on geographic-

level ending. The mean effect size from these studies shows a non-significant iatrogenic effect on offending (ES= -0.018; p=0.883; 95% confidence limits -0.25, 0.22).

### ***Conclusion***

Overall, studies examining the impact of the law on corporate crime show a small and non-significant deterrent effect for cross sectional measures. When examined using longitudinal studies, however, this relationship becomes counterintuitive (but remains non-significant). Because the evidence is limited, it is imprudent to draw firm conclusions about the deterrent impact of law on corporate crime. The tendency in the findings is to find a modest directional leaning toward deterrence in the cross sectional but not longitudinal studies. These results may imply that the implementation of law has a short-term deterrent impact on offending that is not borne out over the long term or that changes in law might promote “defiance” for some firms under particular circumstances. At this point, however, such interpretations are mere speculation. Additional research (using more rigorous methodology) is needed to tease out the actual impact of legislation on corporate crime.

### **4.2.2 Punitive Sanctions**

Studies of the effect of punitive sanctions on corporate offending were more common than those that examined the impact of law. Consequently, we were able to calculate effect sizes for almost all of the different groups. Characteristics of all of these studies are listed in table X.

#### ***Individual-level***

Seventeen (17) effect sizes were calculated from cross-sectional studies examining the effect of punitive sanctions on individual-level offending (11 ES-Rs and 6 ES-Ds). The mean effect size of the eleven ES-R studies showed a non-significant deterrent effect (ES=0.029; p=0.600; 95% confidence limits -0.08, 0.14). Of these 11 effect sizes, seven were positive (indicating a deterrent effect) and four of these did not intersect zero. The mean effect size of the six ES-D studies showed a non-significant effect as well (ES=0.03; p=0.59; 95% confidence limits -0.08, 0.15). Four of the six effect sizes were positive, three of the four intersected zero.

For the longitudinal studies, two effect sizes could be used to calculate a mean effect size (from ES-Ds). The mean effect size was also positive but non-significant (ES=0.034; p=0.893; 95% confidence limits -0.46, 0.52).

#### ***Company-level***

Ten ES-Rs and five ES-Ds were available from cross-sectional studies to calculate mean effect sizes. Both the mean effect size for the ES-Rs (0.083; p=0.110; 95% confidence limits -0.02, 0.18) and the ES-Ds (ES=0.204; p=0.758; 95% confidence limits -1.10, 1.50) were non-significant. Two longitudinal ES-Rs could be used to calculate a mean

effect size, and the mean effect size was also non-significant (ES=-0.053; p=0.12; 95% confidence limits -0.12, 0.01).

### ***Geographic-level***

Analyses at the geographic level were less common among studies examining the effect of punitive sanctions compared to those examining the effect of laws. Three cross-sectional ES-Rs were available from which to calculate a mean effect size. The effect was not significant (ES=0.008; p=0.923; 95% confidence limits -0.16, 0.18) nor was the average effect size for the two longitudinal ES-Rs (ES=-0.001; p=0.999; 95% confidence limits -0.20, 0.20).

### ***Other unit of analysis***

Four ES-Rs were available from cross-sectional studies using an “other” unit of analysis. The mean effect size showed a non-significant deterrent effect (ES=0.250; p=0.222; 95% confidence limits -0.15, 0.65). However, since this is not a homogenous group of studies and the level of analysis may not be the same between the studies, these results should be interpreted cautiously. All four studies showed a positive effect, but only two of the studies had statistically significant results.

### ***Conclusion***

The evidence fails to show a consistent deterrent effect of punitive sanctions on individual offending, company-level offending, geographic-level offending, or offending among studies using an “other” unit of analysis. Not only did the effect sizes fail to reach significance, they were small in magnitude. This result holds regardless of whether the study is longitudinal or cross-sectional.

### **4.2.3 Regulatory Policies**

There were a substantial number of studies that examined the effect of regulatory policy on corporate crime and, as was the case for punitive sanctions, we were able to calculate effect sizes across many of the different groups although studies are sparse in some cases (e.g., individual level offending, geographic-level offending and other units of analysis).

### ***Individual-level***

There were only two ES-Rs and no ES-Ds from cross-sectional studies examining the effect of regulatory sanctions at the individual level. The mean effect size of the two ES-Rs showed a significant deterrent effect (ES= 0.095; p=0.039; 95% confidence limits 0.00, 0.19). Additionally, there were two ES-Ds available to calculate a mean effect size from longitudinal studies. The mean effect size from these studies was marginally significant and positive (1.320; p=0.084; 95% confidence limits -0.18, 2.82).

### ***Company-level***

There were numerous studies examining the effect of regulatory sanctions at the company level. The mean effect size from eight cross-sectional ES-Ds showed a significant deterrent effect (ES= 0.871;  $p=0.000$ ; 95% confidence limits 0.44, 1.30) while the mean effect size from eighteen cross-sectional ES-Rs was not significant and negative (ES= -0.061;  $p=0.310$ ; 95% confidence limits -0.18, 0.06). There were six effect sizes from longitudinal studies: four ES-Rs and two ES-Ds. These studies showed contradictory directional mean effects (i.e., one was negative and the other positive), but neither was significant (ES= -0.169;  $p=0.449$ ; 95% confidence limits -0.61, 0.27) versus (ES= 0.48;  $p=0.276$ ; 95% confidence limits -0.39, 1.35).

### ***Geographic-level***

A mean effect size could only be calculated for cross-sectional studies at the geographic level of analysis. The mean effect size for the six ES-Rs (ES= -0.086;  $p=0.198$ ; 95% confidence limits -0.22, 0.05) and the two ES-Ds (ES= -0.05664;  $p=0.740$ ; 95% confidence limits -0.39, 0.28) were not significant.

### ***Other Unit of Analysis***

Seven cross-sectional ES-R studies used another unit of analysis. However, the mean effect size for these studies was not significant (ES= 0.006;  $p=0.952$ ; 95% confidence limits -0.18, 0.19).

### ***Conclusion***

Overall, the impact of regulatory sanctions on offending is inconsistent and seems dependent on how the treatment is measured. The results suggest a deterrent effect of regulatory sanctions on individual offending; there is a significant deterrent effect based on cross-sectional studies (though there are only two) and a marginal deterrent effect based on longitudinal studies (though there are only two of these as well). At the company level, the mean effect size from studies using correlational measures show a non-significant iatrogenic effect while the studies examining differences between groups show a deterrent effect (significant in cross-sectional studies, non-significant in longitudinal studies). It is possible that, at the company level, there is a tipping point whereby a certain *amount* of regulatory action encourages defiance instead of compliance. Although this interpretation is purely conjecture and requires much more evidence to support it, qualitative research has uncovered instances of regulatory defiance along with occasions of “creative” compliance (McBarnet, 2004). When examining regulatory sanctions by geographic area, more stringent regulations seem to promote offending although the effects are small and non-significant. Taken as a whole, it is unclear what we can infer about the impact of regulatory sanctions on corporate behavior.

#### **4.2.4 Multiple Types of Sanctions**

This category includes studies in which the treatment included multiple types of sanctions that fit into more than one of the above categories. 14 ES-Rs and two ES-Ds were available for this category. From these effect sizes, there was sufficient data to calculate mean effect sizes for cross-sectional studies at the individual and the company levels of analysis using ES-Rs.

##### ***Individual-level***

The mean effect size from the five ES-R studies indicates a significant deterrent effect for this category of sanctions (ES= 0.096;  $p=0.045$ ; 95% confidence limits 0.00, 0.19). Three of the five effect sizes were positive and none of the confidence intervals for these three effect sizes crossed zero. For the two non-positive effect sizes, confidence intervals for both crossed zero.

##### ***Company-level***

Six ES-Rs were able to be calculated for company-level studies. The mean effect size from these studies also indicates a significant deterrent effect (ES= 0.114;  $p=0.029$ ; 95% confidence limits 0.01, 0.22). Five of the six effect sizes are positive and the confidence intervals for all but one of these do not cross zero.

##### ***Conclusion***

While mean effect sizes can only be calculated for cross-sectional studies which examine a treatment that include multiple types, the results show a significant deterrent effect of multiple types of sanctions on individual- and company-level offending. It's interesting that this category demonstrates a small, yet consistent deterrent effect on offending whereas the other interventions do not. This may be due to the characteristics of the studies included in this category, but it would be interesting for future researchers to investigate whether a combination of programs works better than a particular method in isolation (see Simpson, et al, 2013).

#### **4.2.5 Summary of Results**

Out of the 25 effect sizes calculated, 16 (64%) indicated a desirable (i.e., deterrent) impact. However, none of the effects were strong in magnitude and only 4 of those effect sizes were statistically significant. It seems reasonable to argue that there just is not enough data available—most of our analyses are based on less than 10 studies. Furthermore, most of the studies included do not use rigorous methods that would rule out spurious relationships or establish proper temporal ordering. The next section examines how study characteristics may impact the relationships found.

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### 4.3 MODERATOR ANALYSES

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Moderator analyses were performed for ten methodological variables in categories where there was sufficient data and variation. These moderators included whether the study was published, the discipline of the study, publication year, whether the study was experimental/quasi-experimental as opposed to non-experimental, the country where the study was conducted, whether the study includes controls, the data source of the independent variable, the data source of the dependent variable, whether the sample was randomly selected, and the type of offense being studied. Unfortunately, few studies reported information on sample demographics or corporation characteristics, so moderator analyses on these substantive variables could not be performed. The full list of moderators explored is presented in the methods section. The results below discuss the nine methodological variables which are of theoretical interest and have sufficient data to explore. It was necessary to have at least two studies in each category of the moderator variable in order to calculate a meaningful mean effect size for that category. Due to the small number of effect sizes in each category (cell sizes of less than five), we followed the recommendation of Williams (2013), and used a fixed effects (or unpooled) model.

#### 4.3.1 Publication Bias

Although we recognize that including unpublished studies often fails to fully ameliorate biases and may introduce additional concerns (Ferguson & Brannick, 2011), numerous steps were taken during the systematic review to locate unpublished studies as a way to lower the risk of the results being affected by publication bias. Publication bias is a serious challenge to any review and it is important to understand the potential impact on one's results. One way to investigate potential publication bias is to compare the mean effect sizes for published and unpublished studies. Sufficient data and variation was present to examine the moderating effect of publication status for nine categories of ES-R and ES-D studies. The categories are listed in the table in Appendix V. 108 (67.9%) of the effect sizes came from published studies. In seven of the nine categories for which a moderator effect was examined, including studies examining the effect of punitive sanctions or regulatory policies, non-published studies showed a stronger deterrent effect than published studies which sometimes showed counter-intuitive effects. Non-published studies showed a significantly stronger deterrent effect in five of the seven categories. Studies examining the effect of law or multiple treatments, on the other hand, showed a stronger deterrent effect for published studies, though the moderator effect is only significant for studies examining multiple treatments. Given that the differences between published and unpublished studies are inconsistent, publication bias is unlikely to have seriously impacted our findings.

### 4.3.2 Discipline of Study

The studies used in the meta-analysis came from a wide range of disciplines. The following six categories of disciplines were examined where possible: studies published in human behavioral disciplines (including criminology, sociology, and psychology), financial disciplines (including business, marketing, accounting, or economics), political disciplines (including political science or public policy), environmental disciplines (environmental science or biology), other disciplines, and inter-disciplinary areas. 23 (14.5%) of the effect sizes came from human behavioral disciplines, 29 (18.2%) came from financial disciplines, 23 (14.5%) came from political disciplines, 7 (4.4%) came from environmental disciplines, 44 (27.7%) came from inter-disciplinary areas, and 13 (8.2%) came from other disciplines.

Sufficient data was available to examine the moderating effect of discipline for nine categories of studies, including those examining the effect of laws, punitive sanctions, regulatory policies, and multiple treatments. There was evidence of significant moderating effects in three of the study types. Among the ES-Rs examining *punitive sanctions, cross-sectional studies at the individual level of analysis* showed a significantly stronger deterrent effect in human behavioral studies compared to financial studies (which showed a counter-intuitive effect). On the other hand, ES-D studies showed similar effect sizes for human behavioral and financial studies. Studies at the *corporate level of analysis* showed a significantly stronger deterrent effect in financial studies compared to inter-disciplinary studies. Overall, however, there does not appear to be a consistent set of findings by the disciplinary background of the study and therefore we conclude that academic discipline does not explain or help to understand differences across studies in observed outcomes.

### 4.3.3 Year of Publication

The year of publication may also affect the results as more recent studies may be more methodologically rigorous. The following categories of years were compared: 1996 and earlier, 1997-2001, 2002-2006, 2007 and after. Sufficient data was available to examine moderating effects for eight study types. In all but one study type examined, older studies showed a stronger deterrent effect.

### 4.3.4 Study design

The study design can also influence the results, as more methodologically rigorous experimental studies may be more likely to show a null result since they control for potentially spurious relationships and are better able to establish that the intervention preceded the outcome (Cullen and Jonson, add date). While only two categories of studies had sufficient data and variation to conduct a moderator analysis (*cross-sectional, individual-level studies of punitive sanctions or multiple sanctions*), both of

these confirmed that non-experimental studies show a stronger deterrent effect than experimental studies. Although this result is consistent with our expectation that more rigorous methodological designs would be less apt to find deterrent effects, these results should be interpreted cautiously given the small number of studies available to conduct this analysis.

#### **4.3.5 Country**

Studies used in these analyses came from many different countries, but the large majority of effect sizes (78%) were calculated from studies conducted in the United States. There was sufficient data and variation to examine the moderating effect of country origin (US versus other) for four categories of studies. The analysis revealed that studies from outside the United States showed a stronger deterrent effect associated with *punitive sanctions* which may indicate more effective regulatory strategies outside of the United States but, given the paucity of studies from which to examine country moderator effects, this conclusion should be interpreted cautiously.

It is also important to note that we did not include studies written in languages other than English, as we were limited by available resources. This suggests that, despite the indication that control of corporate crime outside of the United States may be different, we are unable to assess a truly comparative perspective. Although academics may publish in English-speaking journals, we are certain to have missed documents produced by the regulatory or law enforcement agencies in other countries. Future research should attempt to locate these records to better understand how corporate crime is conceptualized as well as controlled in different countries.

#### **4.3.6 Controls**

Similar to our expectation that research design could moderate the relationship between treatment and outcome, we also expected that studies that included statistical control variables would also be less apt to find a deterrent effect. Two-thirds of the effect sizes (106) came from studies that used controls in their analyses. Though there were few categories in which there was sufficient data and variation to perform a moderator analysis, the evidence from studies of *punitive sanctions* was consistent with this expectation. Studies that do not include controls show a stronger deterrent effect than those without controls. This implies that deterrence may be confounded with other factors that come along with punitive sanctions (for example, it may be that reputational damage stemming from being prosecuted promotes deterrence, not the prosecution itself; Williams and Hawkins, 1986).

#### **4.3.7 Data source of independent variable and dependent variables**

The treatment variable was most commonly measured using official data sources (60.4%), with 27.0% of the independent variables measured from self-reports. The remaining 12.6% came from other sources. Official data was more commonly used when the level of analysis was the corporation or geographic area and self-report data was more common when the unit of analysis was the individual. There was sufficient data to examine the moderating effect of the data source for five categories of studies, including *cross-sectional studies of regulatory policies and official sanctions*. For all but one category, self-report studies showed a stronger deterrent effect than studies using data from official sources or other data.

A similar pattern was found for the three categories for which there was sufficient data to calculate the moderating effect of the data source of the dependent variable. Again, self-report data showed a stronger deterrent effect than official data sources, as studies using official data showed a small or counter-intuitive result.

It is not unusual in the deterrence literature to find different outcomes by data type (self-report versus official data), especially considering that the different types of data generally are tied to different conceptualizations of deterrence. In the case of official data, we presumably are measuring “objective” deterrence. Self-report data capture deterrence as a perceptual process (Gerken & Grove, 1975). Although self-report data are subject to erroneous perceptions and reporting, official data sources, because they depend on formal discovery of illegal acts, may be less able to discern deterrent effects. Our results suggest that deterrent processes are more apparent in the case of corporate crime when treatment (i.e., the application of sanctions) and reported offending is measured through self-reports compared with “objective” measures.

#### **4.3.8 Sample Selection**

Another methodological difference between the studies included in the meta-analysis was whether the sample was randomly selected. Randomly selected samples reflect greater methodological rigor (i.e., are less subject to selection biases), and these studies are expected to be more likely to show a null result. The moderating effect of sample selection was able to be examined for five types of *cross-sectional studies*: ES-R and ES-D studies examining the effect of *punitive sanctions on individual offending*, ES-R studies examining the effect of *punitive sanctions on company-level offending*, and ES-R and ES-D studies examining the effect of *regulatory policies on company-level offending*. The two categories where there were sufficient numbers of effect sizes to compare random and non-random sample (cross-sectional punitive sanctions on individual and corporate-level offending) demonstrate inconsistent differences. For

corporate-level studies, non-random samples (and those studies that don't report sample type) show a stronger deterrent effect than do random samples. Yet, at the individual level, non-random samples show a counter-intuitive effect while random studies show a deterrent effect. Based on this sparse information, the impact of having a random sample is unclear.

#### **4.3.9 Type of Offense**

It is also possible that different offenses are more or less impacted by legal interventions—for example, offenses that carry less chance of reputational damage to the company (e.g., bribery) may be influenced more by legal sanctions than offenses more subject to public outrage (e.g., environmental offending). To assess the potential for differential effects, we coded the outcome variables into one of six categories: 1) financial offending (including antitrust and trade violations; 30.2% of effect sizes), 2) environmental crimes (47.8% of effect sizes), 3) Occupational Safety and Health Act violations (6.9% of effect sizes), 4) consumer safety violations (0.6% of effect sizes), 5) multiple outcomes combined (5.0% of effect sizes), and 6) other types of crime (9.4% of effect sizes). There were sufficient numbers of effect sizes to compare outcomes for 5 ES-r types and 2 ES-d types.

For the impact of law, we were able to compare the cross-sectional corporate-level ES-ds for financial offending and environmental offending, both of which demonstrate a significant deterrent effect. Thus, laws appear to have a consistent effect on corporations for the two types of offenses we were able to compare. Consistent with a crime prevention and control argument, this may be due to the educative effect of law (i.e., laws tell corporations how to behave and what will happen if laws are violated) or general deterrence—the threat of punishment and moral stigma associated with breaches of law.

The effect of punitive sanctions was examined for individual-level cross-sectional ES-rs and ES-ds. For the ES-rs, punitive sanctions are associated with a significant counterintuitive effect on financial offending, a significant deterrent effect on scales combining multiple outcomes, and a non-significant deterrent effect on other types of outcomes. Looking at the ES-ds, punitive sanctions are associated with a non-significant deterrent effect on financial crime and multiple outcomes. While multiple types of crimes showed a significant deterrence effect and antitrust/financial offending showed a significant counterintuitive effect in the ES-R studies, the effect was not significant in the ES-D studies. It may be that the mere presence of punitive sanctions is not enough to deter individual offenders generally, but increasing the number of sanctions pushes financial offenders to defy the law while encouraging compliance of corporate crime overall. We were also able to compare the impact of punitive sanctions for corporate-level cross-sectional ES-rs, which showed that punitive sanctions

significantly deter financial and environmental crimes and have a non-significant deterrent effect on other types of crimes. Our results hint at the possibility that punitive sanctions against the corporations responsible for financial crimes are more effective than those brought against individual offenders.

The impact of regulatory policies on various outcomes could be examined using the ES-rs at the cross-sectional corporate and cross-sectional geographic levels. At the corporate level, regulatory policies exhibit a significant deterrent effect on financial crimes but appear to have a significant counterintuitive effect on environmental offending. When looking at geographic areas, regulatory policies are associated with a counterintuitive effect on financial offending but a non-significant deterrent effect on environmental crimes. It may be that a more general approach to improving environmental outcomes over a widespread area is more effective, whereas a more specific strategy for financial crimes is necessary.

The effect of multiple treatments could be examined using ES-rs on cross-sectional corporate-level outcomes. Here, multiple treatments have a significant deterrent effect on both financial offending and environmental offending. This implies that applying multiple legal strategies has a consistent effect regardless of the type of crime being targeted.

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## 5 Conclusions

The available scientific evidence on the deterrent value of law, sanctions, and regulatory policies on corporate crime is widely scattered and methodologically varied. Given that the subject matter is of great interest to scholars and policy-makers across many different disciplines, this is not surprising. However, the broad scope of the literature has made this systematic review less parsimonious than we would have preferred and, although this review represents the first attempt to examine this literature in a systematic fashion, it is limited by the paucity of strong empirical studies on corporate deterrence.

Taken as a whole, the basic findings of the review are inconclusive. Out of the 40 possible treatment categories, we were able to calculate a mean effect size for 19. Although most showed a positive albeit non-significant treatment effect, some (including a significant effect) were iatrogenic. Looking at the specific mechanisms, the impact of **LAW** on corporate crime showed a modest deterrent effect at the firm and geographical level of analysis (there was not enough data to calculate effect sizes for individuals). However, this finding is limited to cross-sectional studies. For **PUNITIVE SANCTIONS**, where there was substantially more data from which to calculate effect sizes, we observe a similar pattern: A tendency toward deterrence across units of analysis, with relatively few significant effects regardless of whether data are cross-sectional or longitudinal. The one area where there appears to be a consistent treatment effect is in the area of **REGULATORY POLICY**, but only at the individual level. Effects for other levels are contradictory (with some positive and others iatrogenic) and none are statistically significant.

It is not unusual to find deterrent effects at one level of analysis but not at another (Simpson, 2009). As noted previously, this result may be an artifact of the type of data utilized at these different levels (perceptual self-report data versus official statistics—we did find a moderator effect for data type). It may also be a function of how deterrence is conceptualized and measured—are the studies capturing a specific or general process? But, it might also reflect critical differences in how deterrence works for individuals versus organizations. Boulding (1953), in his characterization of bureaucratic inertia, observed that a corporation is like an elephant, plodding along, oblivious to its

assortment of riders (see also, Fisse & Braithwaite, 1993). The company might not respond quickly to changes in regulation or may be defiant toward existing regulation—especially given the politicized nature of much regulatory policy. Individual managers, on the other hand, often have more personal relationships with regulators and thus are sensitive to the social opprobrium associated with violating that trust or as part of their corporate role, they may be responsible for particular components of regulatory compliance in which case failure to comply can result in an internal reprimand (including the possibility of dismissal). A responsive regulatory strategy acknowledges and builds from these kinds of personal relationships (Nielsen & Parker, 2009). But, responsive regulation also recognizes the need for different types of compliance and deterrence strategies because one size does not fit all and, as Braithwaite reminds us (2002:4), the same person or firm can *at the same time* have multiple motivations to comply or not. Consistent with this point, in our systematic review **MULTIPLE INTERVENTIONS** is the only treatment category to demonstrate a small but consistent treatment effect at both the individual and company level as well as for multiple offense types. Although there are not enough studies to thoroughly test this approach as it applies to corporate deterrence, recognizing the need for different types and levels of interventions is consistent with the enforcement pyramid developed by Braithwaite and the pulling levers “varied menu” of sanctions (Kennedy, 1997) which have showed promise in the prevention and control of violence (Braga & Weisburd, 2012).

Although the systematic review did not reveal strong or dependable patterns across groups, we had enough information from various studies to explore potential moderator effects. In particular, we examined whether our results could have been affected by publication bias, research quality, and the year in which the research was conducted. Similar to findings reported in other crime and justice research and systematic reviews (weaker designs are associated with stronger effects, Weisburd, Lum, & Petrosino, 2001; Welsh, Peel, Farrington, Elffers, & Braga, 2011; Braga & Weisburd, 2012), the least methodologically rigorous designs— those that were not experimental versus experimental designs and those without statistical control variables versus controls were associated with a treatment effect. We also found that older studies were associated with stronger deterrent effects—perhaps because the older studies are less methodologically rigorous than those that are newer. Other moderator results were less clear (publication bias, country bias, disciplinary bias; offense type), but given how few of the analyses revealed strong treatment effects overall we think it is premature to draw any conclusions from these findings at this point.

As already mentioned, this evaluation was less focused than we would have liked. But, it was a critical first step toward establishing the intellectual and empirical terrain of corporate deterrence. Now that this task has been accomplished, updates can be more

attentive to specific categories. Because most of the relevant empirical literature is found in the punitive sanctions and regulatory policy categories, we recommend that future updates concentrate on these areas and that researchers design more rigorous studies to examine whether certain policies and interventions promote specific and/or general deterrence for companies and the managers working within them. We also would call upon researchers to design future studies that test the promising multiple interventions (“pulling levers”) approach. For too long, academics and policy-makers have speculated about the best means to prevent and control corporate crime absent well-designed and executed research. Hopefully, this review will now form the basis of systematic research that will inform evidence-based practice.

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## **6 Plans for Updating the Review**

The authors plan to update the review every 5 years.

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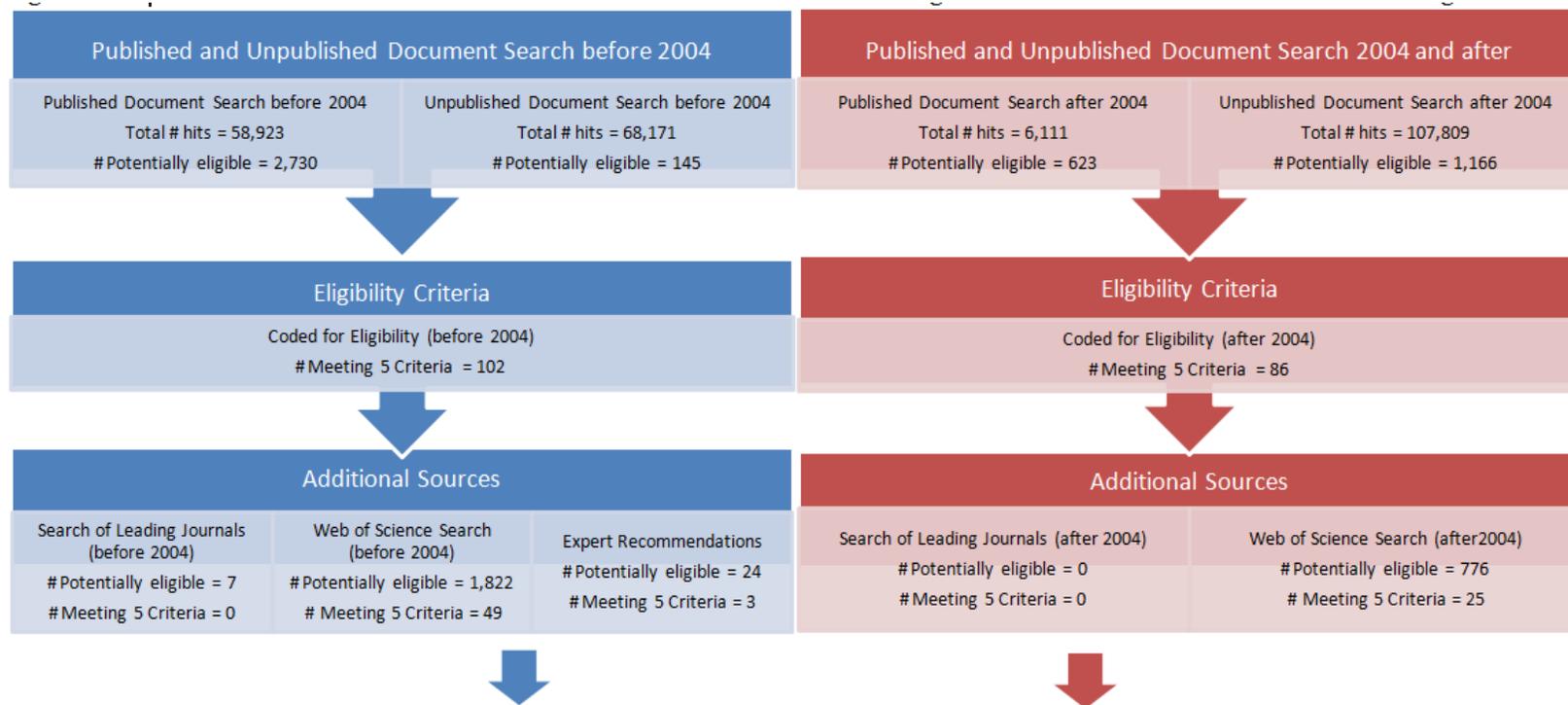
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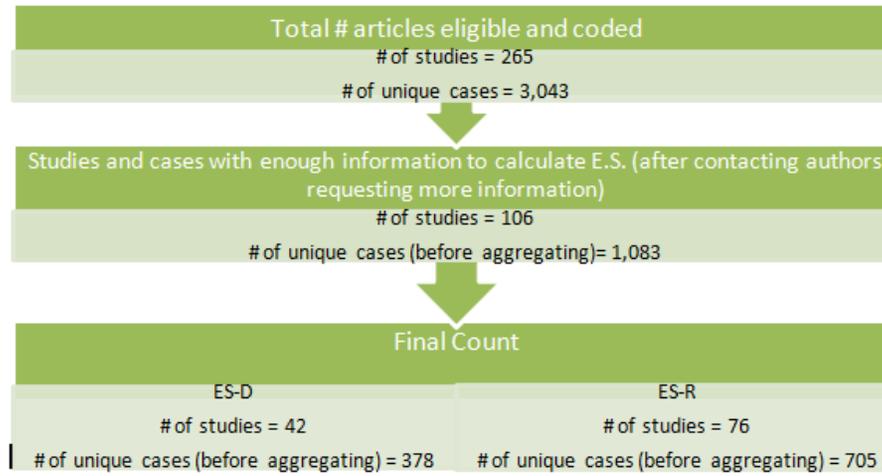
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# 9 Tables and Figures

**9.1 FIGURE 1: NUMBER OF STUDIES OR CASES AT EACH STAGE FIGURES OF THE SEARCH AND CODING PROCESS**





# Appendix I: Frequencies of Statistics Used to Calculate Effect Sizes

Data Type	Count of Effect Sizes	Percentage of Effect Sizes
ES-D from Chi-Square statistics and sample size	2	1.26%
ES-D from correlations	7	4.40%
ES-D from group means and standard deviations	5	3.14%
ES-D from proportions	7	4.40%
ES-D from standardized regression	1	0.63%
ES-D from t-test	4	2.52%
ES-D from t-test and f-test	1	0.63%
ES-D from unstandardized regression	21	13.21%
ES-R from correlations	30	18.87%
ES-R from correlations and unstandardized regression	1	0.63%
ES-R from frequencies	1	0.63%
ES-R from standardized regression	14	8.81%
ES-R from standardized regression and correlations	5	3.14%
ES-R from t-test	3	1.89%
ES-R from t-test and correlations	1	0.63%
ES-R from t-test and standardized regression	1	0.63%
ES-R from unstandardized regression	46	28.93%
ES-R from unstandardized regression and correlations	9	5.66%
Total:	159	100.00%

## Appendix II: Number of Effect Sizes for each Study Type

Treatment: Law	Number of Effect Sizes		
	ES-Rs	ES-Ds	Total
Individual Samples/Cross-sectional	1	0	1
Corporate Samples/Cross-sectional	1	5	6
Geographic Areas/Cross-sectional	3	4	7
Other units of analysis/Cross-sectional	1	1	2
Individual Samples/Longitudinal	0	0	0
Corporate Samples/Longitudinal	0	1	1
Geographic Areas/Longitudinal	2	1	3
Other units of analysis/Longitudinal	0	1	1
<b>Treatment: Punitive Sanctions</b>			
Individual Samples/Cross-sectional	11	6	17
Corporate Samples/Cross-sectional	10	5	15
Geographic Areas/Cross-sectional	3	0	3
Other units of analysis/Cross-sectional	4	1	5
Individual Samples/Longitudinal	0	2	2
Corporate Samples/Longitudinal	3	1	4
Geographic Areas/Longitudinal	2	0	2
Other units of analysis/Longitudinal	0	1	1
<b>Treatment: Regulatory Policies</b>			
Individual Samples/Cross-sectional	2	1	3

Corporate Samples/Cross-sectional	19	8	27
Geographic Areas/Cross-sectional	6	2	8
Other units of analysis/Cross-sectional	7	0	7
Individual Samples/Longitudinal	0	2	2
Corporate Samples/Longitudinal	4	2	6
Geographic Areas/Longitudinal	1	0	1
Other units of analysis/Longitudinal	0	1	1
<b>Treatment: Non-punitive actions</b>			
Individual Samples/Cross-sectional	0	0	0
Corporate Samples/Cross-sectional	2	0	2
Geographic Areas/Cross-sectional	0	0	0
Other units of analysis/Cross-sectional	0	0	0
Individual Samples/Longitudinal	0	0	0
Corporate Samples/Longitudinal	0	0	0
Geographic Areas/Longitudinal	0	0	0
Other units of analysis/Longitudinal	0	0	0
<b>Treatment: Multiple treatments</b>			
Individual Samples/Cross-sectional	5	0	5
Corporate Samples/Cross-sectional	6	1	7
Geographic Areas/Cross-sectional	1	0	1
Other units of analysis/Cross-sectional	0	0	0
Individual Samples/Longitudinal	0	0	0
Corporate Samples/Longitudinal	1	1	2
Geographic Areas/Longitudinal	1	0	1
Other units of analysis/Longitudinal	0	0	0

## Appendix III: Study Characteristics and Outcomes

### A: Treatment-Law

Authors	Year	Time Ordering	Treatment	Unit of Analysis	Data Type	Effect Size	Lower CI	Upper CI
Short and Toffel	2007	Cross-sectional	Law	Corporation	ES-D from correlations	0.021	0.009	0.033
Bolotova	2006	Cross-sectional	Law	Corporation	ES-D from unstandardized regression	0.490	0.171	0.810
Guerrero	2011	Cross-sectional	Law	Corporation	ES-D from unstandardized regression	0.003	-0.014	0.020
Mishra & Ray	2010	Cross-sectional	Law	Corporation	ES-D from unstandardized regression	0.031	0.006	0.057
Sam	2010	Cross-sectional	Law	Corporation	ES-D from unstandardized regression	0.027	-0.025	0.080
Jaffe	1974	Longitudinal	Law	Corporation	ES-D from group means and standard deviations	0.541	-4.816	5.898
Greco	1987	Cross-sectional	Law	Geographic	ES-D from t-test	1.404	0.512	2.297
Ashenmiller & Normal	2011	Cross-sectional	Law	Geographic	ES-D from unstandardized regression	0.000	-0.065	0.065
Barker	2008	Cross-sectional	Law	Geographic	ES-D from unstandardized regression	0.122	0.122	0.122

Payne & Gainey	2004	Cross-sectional	Law	Geographic	ES-D from unstandardized regression	0.090	-0.520	0.700
Alberini and Austin	1999	Longitudinal	Law	Geographic	ES-D from unstandardized regression	0.008	-0.546	0.563
Guerrero	2011	Cross-sectional	Law	Other	ES-D from unstandardized regression	0.008	-0.029	0.045
Connor	2003	Longitudinal	Law	Other	ES-D from group means and standard deviations	-0.274	-0.964	0.416
Gao	2010/ 2011	Cross-sectional	Law	Individual	ES-R from correlations and unstandardized regression	-0.086	0.087	-0.259
Mishra & Ray/ Chatterjee & Ray	2010/ 2009	Cross-sectional	Law	Corporation	ES-R from unstandardized regression	0.250	0.266	0.233
Broadman and Recanatini	2002	Cross-sectional	Law	Geographic	ES-R from correlations	0.761	1.169	0.352
Wenner	1972	Cross-sectional	Law	Geographic	ES-R from correlations	-0.171	0.115	-0.457
O'Toole et al.	1997	Cross-sectional	Law	Geographic	ES-R from standardized regression and correlations	0.360	0.646	0.074
Grant	1997	Cross-sectional and Longitudinal	Law	Geographic	ES-R from standardized regression	-0.052	0.110	-0.214
Rousso & Steves	2006	Longitudinal	Law	Geographic	ES-R from correlations	-0.092	0.317	-0.500
Maxwell et al.	2000	Longitudinal	Law	Geographic	ES-R from unstandardized regression	0.019	0.304	-0.267
Burby and Patterson	1993	Cross-sectional	Law	Other	ES-R from standardized regression	0.094	0.290	-0.102

### **B: Treatment- Punitive Sanction**

Weisburd, Waring, and Chayet	1995	Cross-sectional	Punitive Sanction	Individual	ES-D from Chi-Square statistics and sample size	-0.052	-0.310	0.207
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Bigoni, Fridolfsson, Le Coq, and Spagnolo/ Bigoni	2009/2011	Cross-sectional	Punitive Sanction	Individual	ES-D from group means and standard deviations	0.432	0.102	0.762
Simpson	2002	Cross-sectional	Punitive Sanction	Individual	ES-D from group means and standard deviations	0.105	-0.188	0.398
Wade	2004	Cross-sectional	Punitive Sanction	Individual	ES-D from t-test and f-test	0.059	-0.174	0.291
Alm, Jackson, & McKee	2009	Cross-sectional	Punitive Sanction	Individual	ES-D from unstandardized regression	0.040	-0.017	0.098
Bruner et al.	2008	Cross-sectional	Punitive Sanction	Individual	ES-D from unstandardized regression	-0.137	-0.266	-0.008
Weisburd, Waring, and Chayet	2001	Longitudinal	Punitive Sanction	Individual	ES-D from Chi-Square statistics and sample size	-0.190	-0.501	0.121
Wade	2004	Longitudinal	Punitive Sanction	Individual	ES-D from t-test	0.315	-0.142	0.772
Gibbs & Simpson	2009	Cross-sectional	Punitive Sanction	Corporation	ES-D from correlations	-0.275	-0.743	0.192
Short and Toffel	2007	Cross-sectional	Punitive Sanction	Corporation	ES-D from correlations	-0.466	-0.508	-0.424
EPA	1997	Cross-sectional	Punitive Sanction	Corporation	ES-D from proportions	2.039	1.984	2.094
Bolotova	2006	Cross-sectional	Punitive Sanction	Corporation	ES-D from unstandardized regression	-0.166	-0.437	0.104
Chatterji, Levine, & Toffel	2009	Cross-sectional	Punitive Sanction	Corporation	ES-D from unstandardized regression	-0.126	-0.212	-0.039
Earnhart	2004	Longitudinal	Punitive Sanction	Corporation	ES-D from unstandardized regression	-0.276	-2.065	1.513
Burby and Patterson	1993	Cross-sectional	Punitive Sanction	Other	ES-D from standardized regression	0.021	-0.431	0.473

Connor	2003	Longitudinal	Punitive Sanction	Other	ES-D from group means and standard deviations	-2.457	-3.411	-1.503
Shafer and Morris	2004	Cross-sectional	Punitive Sanction	Individual	ES-R from correlations	0.143	0.261	0.024
Smith et al.	2007	Cross-sectional	Punitive Sanction	Individual	ES-R from correlations	0.236	0.365	0.107
Ugrin	2008	Cross-sectional	Punitive Sanction	Individual	ES-R from correlations	0.230	0.409	0.051
Piquero, Tibbetts, & Blankenship	2005	Cross-sectional	Punitive Sanction	Individual	ES-R from standardized regression and correlations	0.142	0.314	-0.030
Wenzel	2004	Cross-sectional	Punitive Sanction	Individual	ES-R from standardized regression and correlations	0.095	0.148	0.043
Akir & Malie	2010	Cross-sectional	Punitive Sanction	Individual	ES-R from unstandardized regression	-0.241	-0.073	-0.409
Alm, Jackson, & McKee	2009	Cross-sectional	Punitive Sanction	Individual	ES-R from unstandardized regression	-0.006	0.022	-0.035
Bruner et al.	2008	Cross-sectional	Punitive Sanction	Individual	ES-R from unstandardized regression	-0.311	-0.262	-0.360
Piquero & Piquero	2006	Cross-sectional	Punitive Sanction	Individual	ES-R from unstandardized regression	0.016	0.230	-0.198
Pryor et al.	2008	Cross-sectional	Punitive Sanction	Individual	ES-R from unstandardized regression	0.068	0.156	-0.019
Gao	2010/ 2011	Cross-sectional	Punitive Sanction	Individual	ES-R from unstandardized regression and correlations	-0.016	0.157	-0.188
Bolotova	2006	Cross-sectional	Punitive Sanction	Corporation	ES-R from correlation	-0.045	0.083	-0.173
Chatterji, Levine, & Toffel	2009	Cross-sectional	Punitive Sanction	Corporation	ES-R from correlations	-0.134	-0.098	-0.169
Fowler-Rians	1997	Cross-sectional	Punitive Sanction	Corporation	ES-R from correlations	0.052	0.230	-0.125

Gibbs & Simpson	2009	Cross-sectional	Punitive Sanction	Corporation	ES-R from correlations	-0.023	0.028	-0.074
Hallward-Driemeier	2009	Cross-sectional	Punitive Sanction	Corporation	ES-R from correlations	0.385	0.409	0.361
Almutairi	2000	Cross-sectional	Punitive Sanction	Corporation	ES-R from standardized regression	0.477	0.645	0.309
Langbert	1996	Cross-sectional	Punitive Sanction	Corporation	ES-R from t-test	-0.050	0.080	-0.180
Braithwaite and Makkai	1991	Cross-sectional	Punitive Sanction	Corporation	ES-R from unstandardized regression	0.058	0.155	-0.039
Earnhart	2004/ 2009	Cross-sectional	Punitive Sanction	Corporation	ES-R from unstandardized regression	-0.003	0.009	-0.016
Shimshack and Ward	2005	Cross-sectional	Punitive Sanction	Corporation	ES-R from unstandardized regression	0.150	0.161	0.139
Gray and Scholz	1993	Cross-sectional and Longitudinal	Punitive Sanction	Corporation	ES-R from correlations	-0.182	-0.159	-0.206
Jesilow, Geis, and O'Brien	1985	Longitudinal	Punitive Sanction	Corporation	ES-R from frequencies	-0.087	0.024	-0.199
Makkai and Braithwaite	1994	Longitudinal	Punitive Sanction	Corporation	ES-R from standardized regression	-0.049	0.048	-0.146
Stretesky and Lynch	2009	Longitudinal	Punitive Sanction	Corporation	ES-R from unstandardized regression	0.000	0.148	-0.148
Broadman and Recanatini	2002	Cross-sectional	Punitive Sanction	Geographic	ES-R from correlations	0.361	0.770	-0.048
May and Winter	1999	Cross-sectional	Punitive Sanction	Geographic	ES-R from standardized regression	-0.096	0.039	-0.230
Rickman & Witt	2007	Cross-sectional	Punitive Sanction	Geographic	ES-R from unstandardized regression	0.007	0.153	-0.138
Alberini and Austin	1999	Longitudinal	Punitive Sanction	Geographic	ES-R from unstandardized regression	0.005	0.288	-0.278

Maxwell et al.	2000	Longitudinal	Punitive Sanction	Geographic	ES-R from unstandardized regression	-0.005	0.281	-0.291
Burby and Patterson	1993	Cross-sectional	Punitive Sanction	Other	ES-R from standardized regression	0.085	0.281	-0.111
Sam	2010	Cross-sectional	Punitive Sanction	Other	ES-R from unstandardized regression	0.001	0.024	-0.021
Short and Toffell	2008	Cross-sectional	Punitive Sanction	Other	ES-R from unstandardized regression	0.876	0.907	0.844
Vidovic and Khanna	2012	Cross-sectional	Punitive Sanction	Other	ES-R from unstandardized regression	0.031	0.049	0.012
Foulon et al.	2002	Cross-sectional and Longitudinal	Punitive Sanction	Other	ES-R from unstandardized regression	0.012	0.578	-0.553
Lanoie	1992	Cross-sectional and Longitudinal	Punitive Sanction	Other	ES-R from unstandardized regression	-0.002	0.165	-0.170
Meadow	1993	Cross-sectional and Longitudinal	Punitive Sanction	Other	ES-R from unstandardized regression and correlations	-0.122	-0.016	-0.227

### **C: Treatment- Non-punitive Sanction**

Gibbs & Simpson	2009	Cross-sectional	Non-punitive Sanction	Corporation	ES-R from correlations	-0.097	-0.046	-0.148
Shimshack and Ward	2005	Cross-sectional	Non-punitive Sanction	Corporation	ES-R from unstandardized regression	-0.256	-0.246	-0.267

### **D: Treatment- Regulatory Policy**

Alm, Jackson, & McKee	2009	Cross-sectional	Regulatory Policy	Individual	ES-D from unstandardized regression	-0.022	-0.092	0.047
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Boardman et al.	1998	Longitudinal	Regulatory Policy	Individual	ES-D from t-test	2.093	1.577	2.609
Wade	2004	Longitudinal	Regulatory Policy	Individual	ES-D from t-test	0.566	0.163	0.968
Nielsen and Parker	2005	Cross-sectional	Regulatory Policy	Corporation	ES-D from correlations	0.071	-3.023	3.165
Short and Toffel	2007	Cross-sectional	Regulatory Policy	Corporation	ES-D from correlations	-0.461	-0.545	-0.378
US GAO	1988	Cross-sectional	Regulatory Policy	Corporation	ES-D from proportions	0.764	0.757	0.772
Gerardu and Wasserman	1994	Cross-sectional	Regulatory Policy	Corporation	ES-D from proportions	0.962	2.059	2.462
Gerardu and Wasserman	1991	Cross-sectional	Regulatory Policy	Corporation	ES-D from proportions	3.763	3.617	3.909
Krahn	1998	Cross-sectional	Regulatory Policy	Corporation	ES-D from proportions	1.284	0.991	1.577
Bennear	2007	Cross-sectional	Regulatory Policy	Corporation	ES-D from unstandardized regression	-0.013	-0.021	-0.005
Telle	2009	Cross-sectional	Regulatory Policy	Corporation	ES-D from unstandardized regression	-0.053	-0.180	0.075
Chang	1993	Cross-sectional and Longitudinal	Regulatory Policy	Corporation	ES-D from unstandardized regression	-0.274	-0.964	0.416
Li	2009	Longitudinal	Regulatory Policy	Corporation	ES-D from correlations	0.016	-0.377	0.409
EPA	1992	Longitudinal	Regulatory Policy	Corporation	ES-D from proportions	0.901	0.854	0.949
O'Toole et al.	1997	Cross-sectional	Regulatory Policy	Geographic	ES-D from correlations	0.251	-0.329	0.831
Klein	2010	Cross-sectional	Regulatory Policy	Geographic	ES-D from unstandardized regression	-0.151	-0.232	-0.070

Yang	2007	Longitudinal	Regulatory Policy	Other	ES-D from unstandardized regression	-0.009	-0.110	0.092
Shafer & Morris	2004	Cross-sectional	Regulatory Policy	Individual	ES-R from correlations	0.162	0.280	0.043
Alm, Jackson, & McKee	2009	Cross-sectional	Regulatory Policy	Individual	ES-R from unstandardized regression	0.063	0.092	0.035
Gibbs & Simpson	2009	Cross-sectional	Regulatory Policy	Corporation	ES-R from correlations	0.000	0.051	-0.051
Hallward-Driemeier	2009	Cross-sectional	Regulatory Policy	Corporation	ES-R from correlations	-0.130	-0.106	-0.154
Short and Toffel	2007	Cross-sectional	Regulatory Policy	Corporation	ES-R from correlations	-0.225	-0.219	-0.231
Pargal et al.	1997	Cross-sectional	Regulatory Policy	Corporation	ES-R from standardized regression	0.000	0.125	-0.125
Shimshack and Ward	2005	Cross-sectional	Regulatory Policy	Corporation	ES-R from standardized regression	-0.620	-0.610	-0.631
Langbert	1996	Cross-sectional	Regulatory Policy	Corporation	ES-R from t-test	0.123	0.253	-0.007
Moran	1985	Cross-sectional	Regulatory Policy	Corporation	ES-R from t-test	0.048	0.080	0.017
Kassinis and Vafeas	2002	Cross-sectional	Regulatory Policy	Corporation	ES-R from t-test and correlations	0.053	0.149	-0.043
Berg et al.	2012	Cross-sectional	Regulatory Policy	Corporation	ES-R from unstandardized regression	0.660	0.696	0.623
Earnhart	2004	Cross-sectional	Regulatory Policy	Corporation	ES-R from unstandardized regression	-0.068	-0.055	-0.080
Earnhart	2007	Cross-sectional	Regulatory Policy	Corporation	ES-R from unstandardized regression	-0.075	-0.043	-0.106
Earnhart	2009	Cross-sectional	Regulatory Policy	Corporation	ES-R from unstandardized regression	0.003	0.016	-0.010

Evans et al.	2011	Cross-sectional	Regulatory Policy	Corporation	ES-R from unstandardized regression	-0.037	-0.003	-0.071
Gray & Shadbegian	2004	Cross-sectional	Regulatory Policy	Corporation	ES-R from unstandardized regression	0.129	0.187	0.072
Gray & Shadbeguan	2007	Cross-sectional	Regulatory Policy	Corporation	ES-R from unstandardized regression	-0.279	-0.192	-0.365
Hwang	2008	Cross-sectional	Regulatory Policy	Corporation	ES-R from unstandardized regression	-0.031	0.085	-0.148
Pothukuchi	2008	Cross-sectional	Regulatory Policy	Corporation	ES-R from unstandardized regression	-0.663	-0.540	-0.787
Wu	2009	Cross-sectional	Regulatory Policy	Corporation	ES-R from unstandardized regression	-0.109	0.016	-0.234
Mobus	2005	Cross-sectional	Regulatory Policy	Corporation	ES-R from unstandardized regression and correlations	0.055	0.228	-0.118
Braithwaite, Braithwaite, Gibson, and Makkai	1994	Cross-sectional and Longitudinal	Regulatory Policy	Corporation	ES-R from correlations	0.191	0.288	0.093
Gray and Scholz	1993	Cross-sectional and Longitudinal	Regulatory Policy	Corporation	ES-R from correlations	-0.151	-0.128	-0.175
Helland and Whitford	2003	Cross-sectional and Longitudinal	Regulatory Policy	Corporation	ES-R from unstandardized regression	-0.043	0.023	-0.109
Makkai and Braithwaite	1994	Longitudinal	Regulatory Policy	Corporation	ES-R from standardized regression	0.040	0.148	-0.068
Earnhart	2004	Longitudinal	Regulatory Policy	Corporation	ES-R from unstandardized regression	0.027	0.349	-0.295
Stretesky and Lynch	2009	Longitudinal	Regulatory Policy	Corporation	ES-R from unstandardized regression	0.000	0.148	-0.148
Simpson & Schell	2009	Longitudinal	Regulatory Policy	Corporation	ES-R from unstandardized regression and correlations	-0.717	-0.610	-0.825
Jing & Graham	2008	Cross-sectional	Regulatory Policy	Geographic	ES-R from correlations	-0.637	-0.345	-0.930

Wenner	1972	Cross-sectional	Regulatory Policy	Geographic	ES-R from correlations	-0.232	0.053	-0.518
May and Winter	1999	Cross-sectional	Regulatory Policy	Geographic	ES-R from standardized regression	0.097	0.232	-0.037
O'Toole et al.	1997	Cross-sectional	Regulatory Policy	Geographic	ES-R from standardized regression and correlations	0.122	0.408	-0.164
Barker	2008	Cross-sectional	Regulatory Policy	Geographic	ES-R from unstandardized regression	0.001	0.110	-0.108
Klein	2010	Cross-sectional	Regulatory Policy	Geographic	ES-R from unstandardized regression	-0.089	-0.051	-0.127
Maxwell et al.	2000	Longitudinal	Regulatory Policy	Geographic	ES-R from unstandardized regression	0.026	0.311	-0.260
Burby and Patterson	1993	Cross-sectional	Regulatory Policy	Other	ES-R from standardized regression	0.073	0.269	-0.123
Burby et al.	1998	Cross-sectional	Regulatory Policy	Other	ES-R from standardized regression	0.084	0.152	0.015
Guerrero	2011	Cross-sectional	Regulatory Policy	Other	ES-R from unstandardized regression	0.008	0.026	-0.009
Telle	2009	Cross-sectional	Regulatory Policy	Other	ES-R from unstandardized regression	-0.060	0.004	-0.124
Toffel and Short	2011	Cross-sectional	Regulatory Policy	Other	ES-R from unstandardized regression	0.277	0.301	0.254
Vidovic and Khanna	2012	Cross-sectional	Regulatory Policy	Other	ES-R from unstandardized regression	0.018	0.036	-0.001
Short and Toffel	2008	Cross-sectional	Regulatory Policy	Other	ES-R from unstandardized regression and correlations	-0.351	-0.334	-0.368
Klahsen	1999	Cross-sectional and Longitudinal	Regulatory Policy	Other	ES-R from correlations	0.034	0.135	-0.066
Grau and Groves	1997	Cross-sectional and Longitudinal	Regulatory Policy	Other	ES-R from t-test and standardized regression	0.217	0.379	0.056

Lanoie	1992	Cross-sectional and Longitudinal	Regulatory Policy	Other	ES-R from unstandardized regression	0.114	0.281	-0.054
Meadow	1993	Cross-sectional and Longitudinal	Regulatory Policy	Other	ES-R from unstandardized regression and correlations	-0.146	-0.041	-0.251

**E: Treatment- Multiple Sanction Types**

Chatterji, Levine, & Toffel	2009	Cross-sectional	Multiple Sanction Types	Corporation	ES-D from unstandardized regression	-0.128	-0.217	-0.040
EPA	1992	Longitudinal	Multiple Sanction Types	Corporation	ES-D from proportions	0.341	0.122	0.559
Ulrich et al.	2003	Cross-sectional	Multiple Sanction Types	Individual	ES-R from correlations	0.229	0.353	0.105
Simpson and Piquero	2002	Cross-sectional	Multiple Sanction Types	Individual	ES-R from standardized regression	0.000	0.100	-0.100
Piquero et al	2005	Cross-sectional	Multiple Sanction Types	Individual	ES-R from unstandardized regression	-0.038	0.135	-0.211
Piquero	2012	Cross-sectional	Multiple Sanction Types	Individual	ES-R from unstandardized regression	0.217	0.431	0.003
Gurley et al	2007	Cross-sectional	Multiple Sanction Types	Individual	ES-R from unstandardized regression and correlations	0.102	0.177	0.027

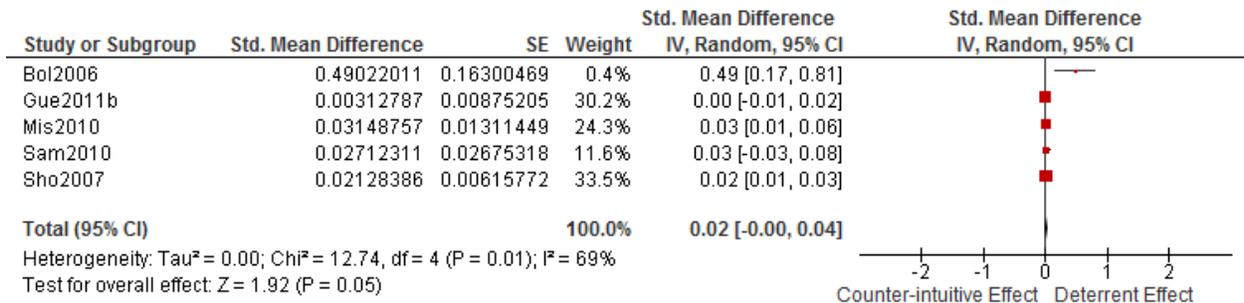
Gibbs & Simpson	2009	Cross-sectional	Multiple Sanction Types	Corporation	ES-R from correlations	-0.063	-0.012	-0.114
Hartman et al.	1997	Cross-sectional	Multiple Sanction Types	Corporation	ES-R from correlations	0.485	0.893	0.076
Pellicioni	2002	Cross-sectional	Multiple Sanction Types	Corporation	ES-R from correlations	0.139	0.225	0.053
Almutairi	2000	Cross-sectional	Multiple Sanction Types	Corporation	ES-R from standardized regression	0.250	0.418	0.082
Earnhart	2007	Cross-sectional	Multiple Sanction Types	Corporation	ES-R from unstandardized regression	0.159	0.191	0.127
Gray & Shadbegian	2004	Cross-sectional	Multiple Sanction Types	Corporation	ES-R from unstandardized regression and correlations	0.043	0.099	-0.013
Earnhart	2004	Longitudinal	Multiple Sanction Types	Corporation	ES-R from unstandardized regression	0.172	0.494	-0.150
Anderson & Gray	2006	Cross-sectional	Multiple Sanction Types	Geographic	ES-R from correlations	0.149	0.715	-0.417
Rousso & Steves	2006	Longitudinal	Multiple Sanction Types	Geographic	ES-R from correlations	0.097	0.506	-0.312

# Appendix IV: Forest Plots

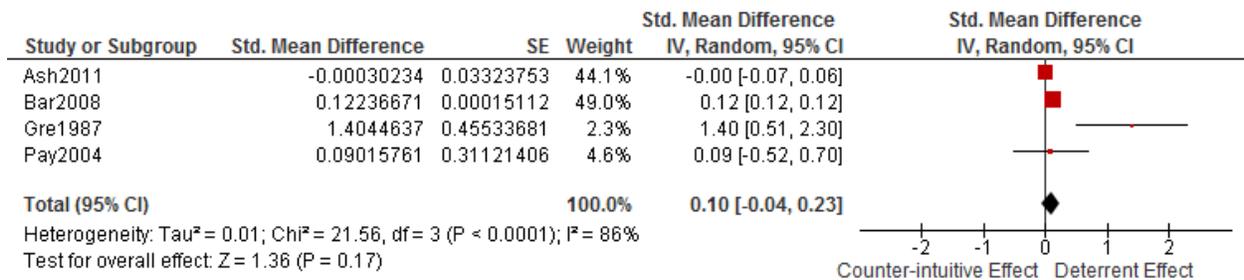
## ES-D FOREST PLOTS

### Law

#### Corporate Cross-Sectional (122)

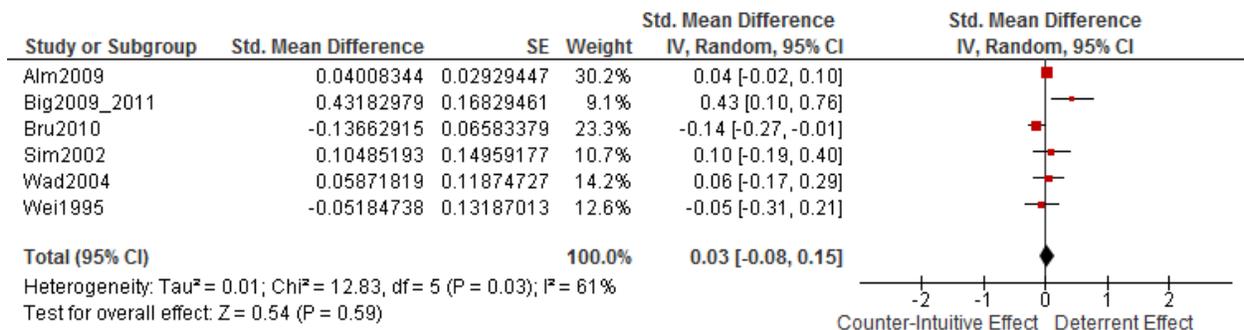


#### Geographic Cross-Sectional (123)

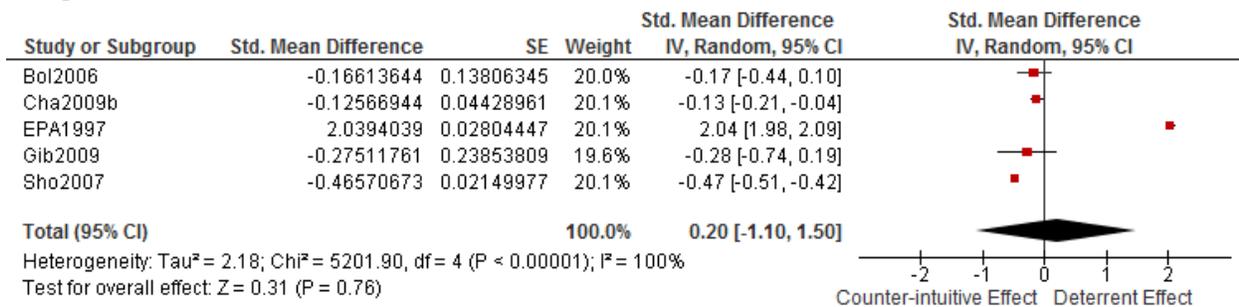


### Punitive Sanctions

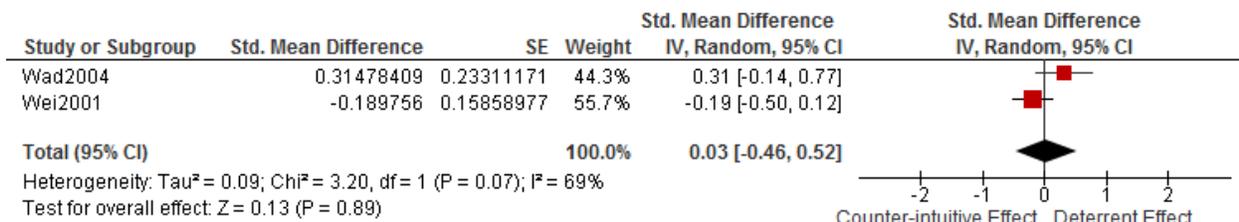
#### Individual Cross-Sectional (131)



## Corporate Cross-Sectional (132)

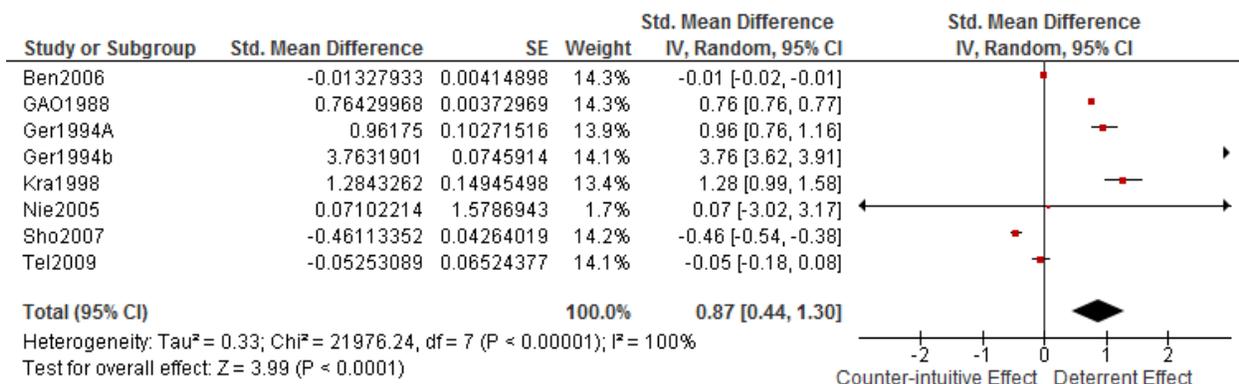


## Individual Longitudinal (231)

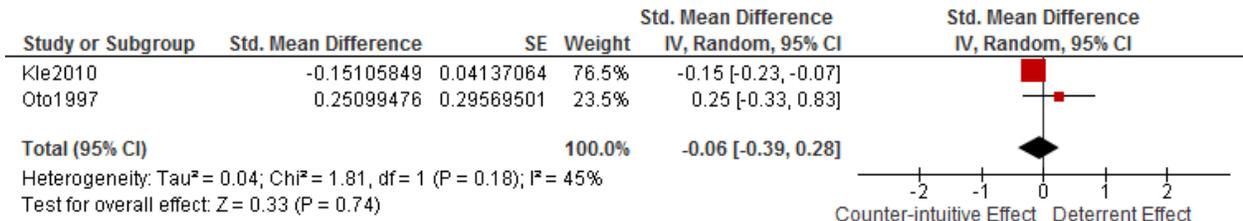


## Regulatory Policies

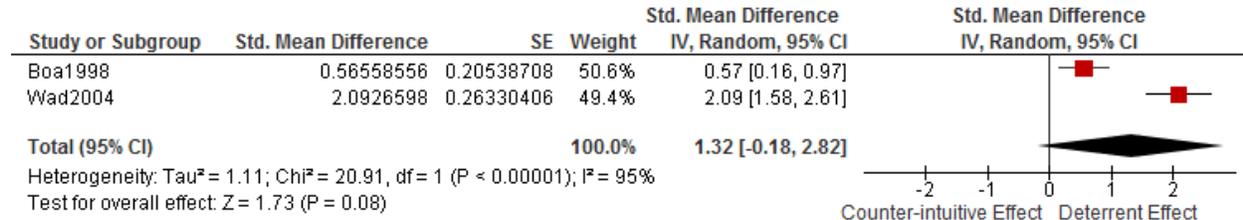
### Corporate Cross-Sectional (142)



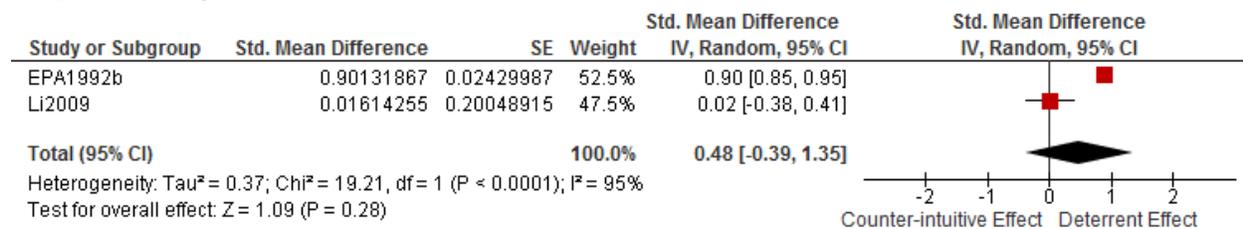
### Geographic Cross-Sectional (143)



### Individual Longitudinal (241)



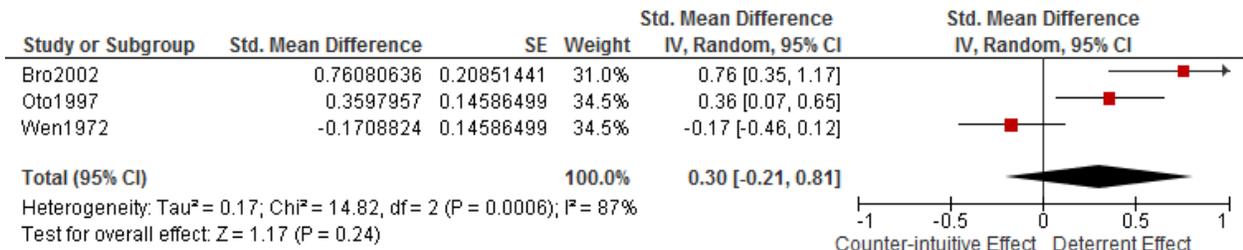
### Corporate Longitudinal (242)



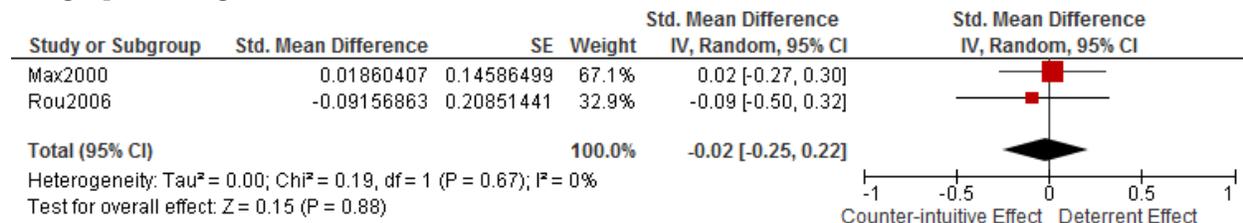
## ES-R FOREST PLOTS

### Law

#### Geographic Cross-Sectional (123)

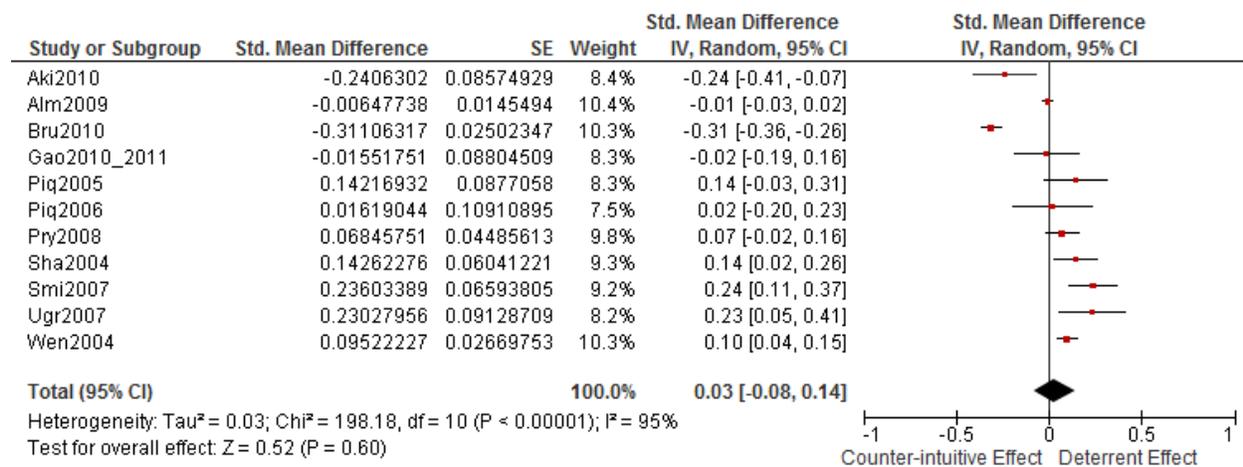


#### Geographic Longitudinal (223)

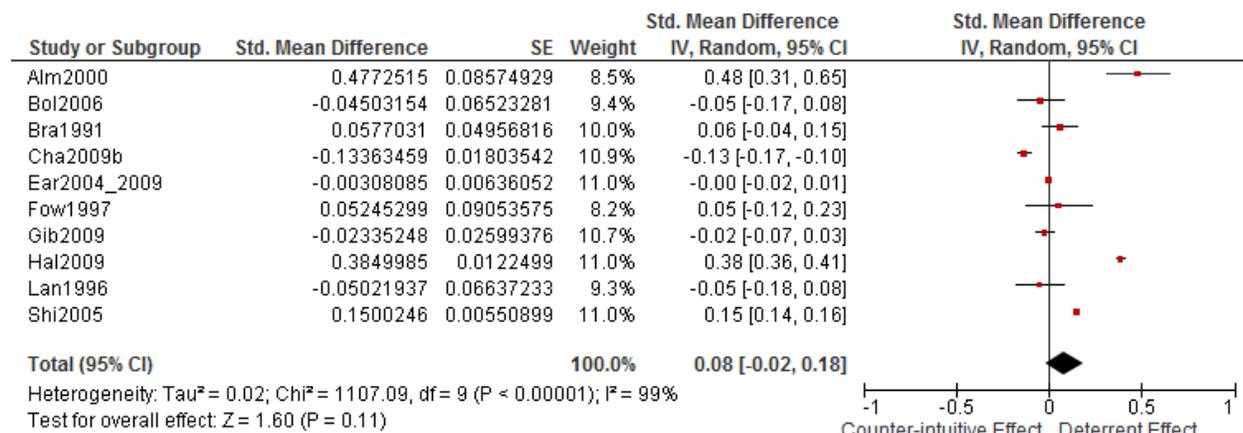


### Punitive Sanctions

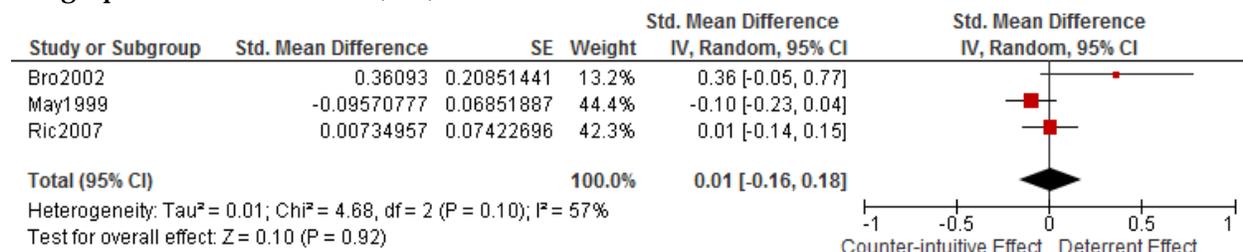
#### Individual Cross-Sectional (131)



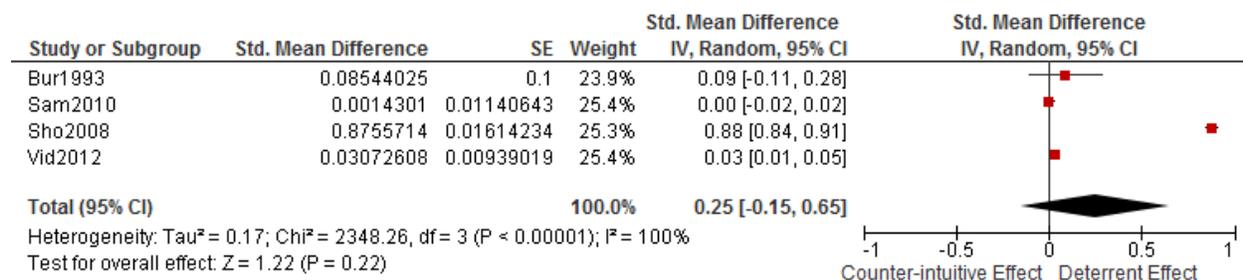
### Corporate Cross-Sectional (132)



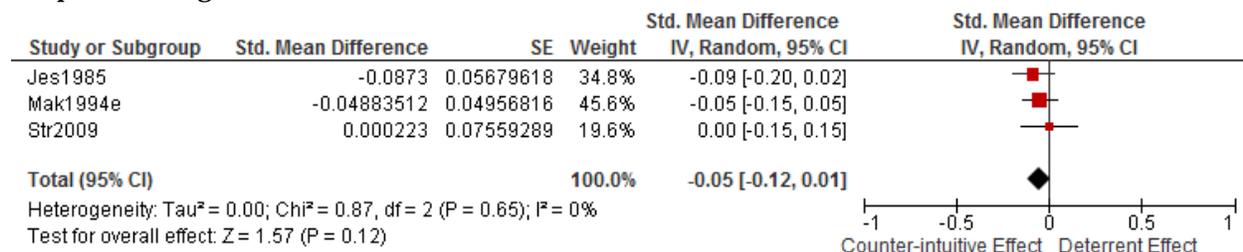
### Geographic Cross-Sectional (133)



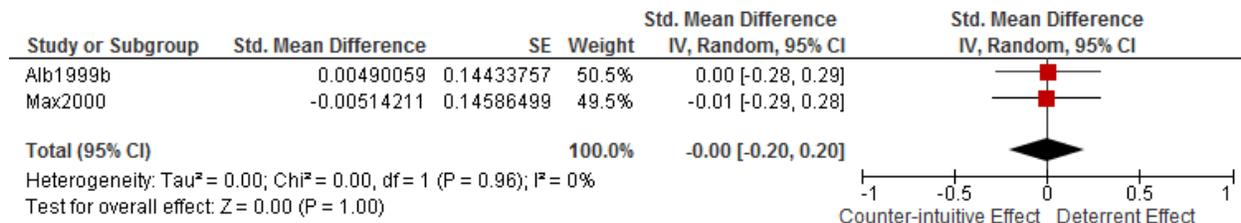
### Other Cross-Sectional (134)



### Corporate Longitudinal (232)

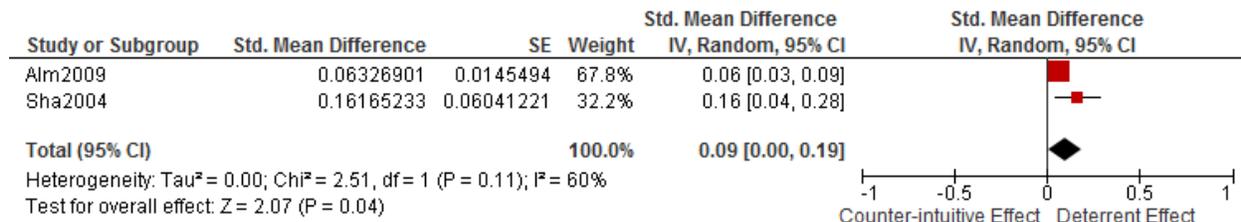


## Geographic Longitudinal (233)

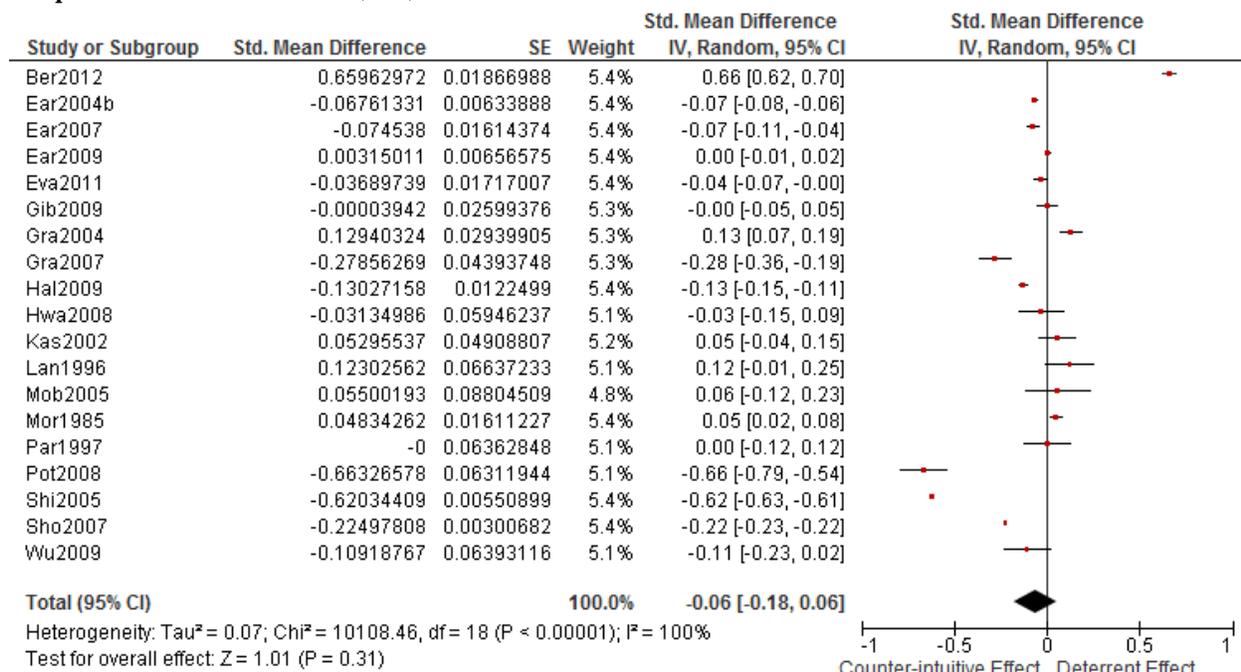


## Regulatory Policies

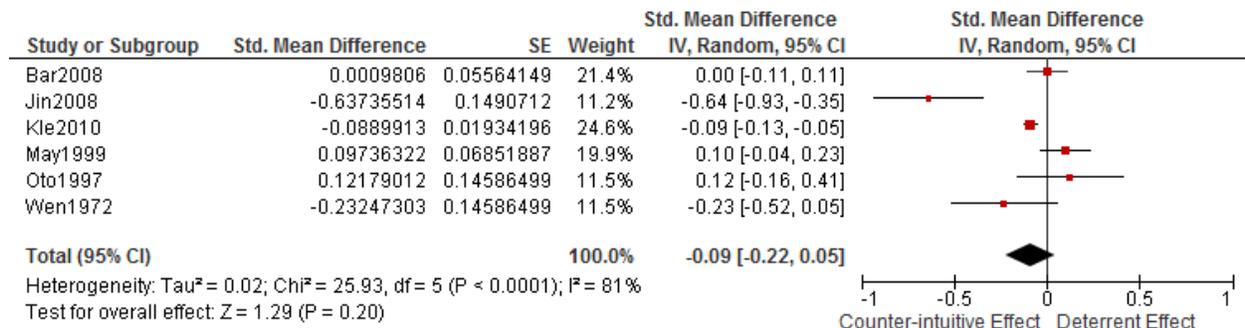
### Individual Cross-Sectional (141)



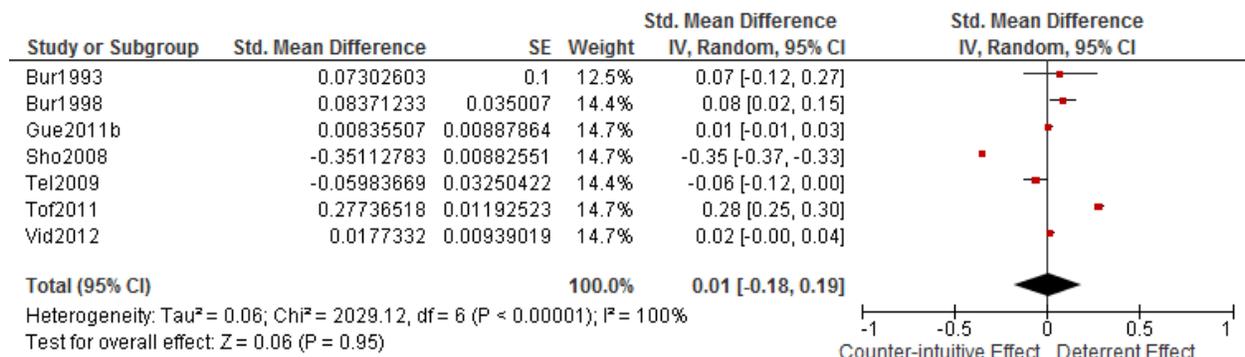
### Corporate Cross-Sectional (142)



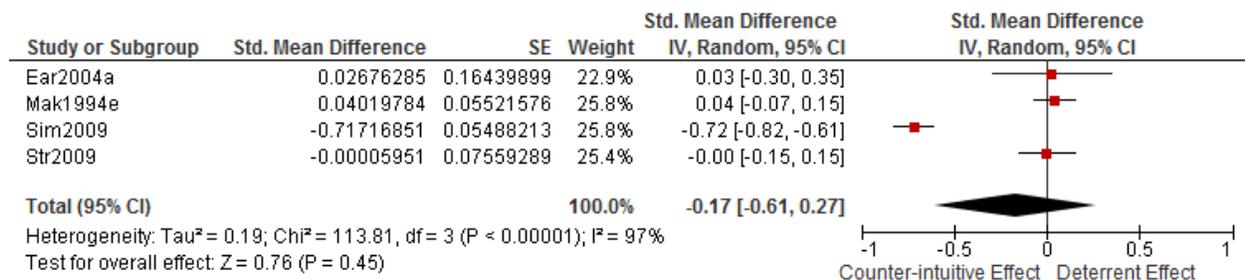
## Geographic Cross-Sectional (143)



## Other Cross-Sectional (144)

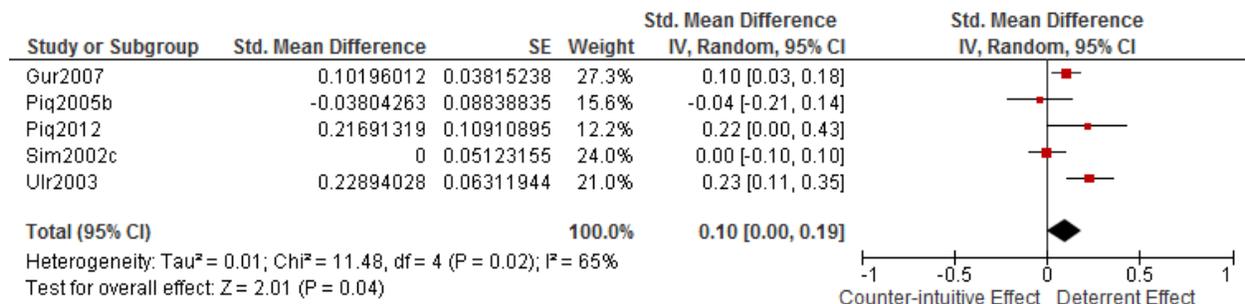


## Corporate Longitudinal (242)

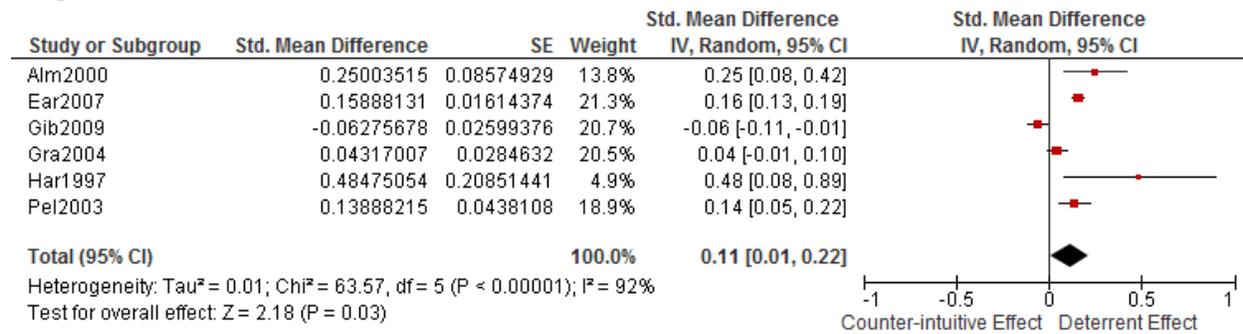


## Multiple Treatments

### Individual Cross-Sectional (1151)



## Corporate Cross-Sectional (1152)



# Appendix V: Categories examined as Moderators for Deterrence Outcomes

	Publication Bias: Published vs. Unpublished		Discipline		Year of Publication		Study Design: Experimental vs. Non-Experimental		Country: USA vs. Non-USA	
	ES-R	ES-D	ES-R	ES-D	ES-R	ES-D	ES-R	ES-D	ES-R	ES-D
<b>Treatment: Law</b>										
Individual Samples/Cross-sectional										
Corporate Samples/Cross-sectional		0.022 0.013		Financial 0.012 Multi 0.022						0.016 0.034
Geographic Areas/Cross-sectional										
Other units of analysis/Cross-sectional										
Individual Samples/Longitudinal										
Corporate Samples/Longitudinal										
Geographic Areas/Longitudinal										
Other units of analysis/Longitudinal										
<b>Treatment: Punitive Sanctions</b>										
Individual	-0.0499	0.011	Human	Human	2002-	02-06	-0.059			-0.060

Samples/Cross-sectional	0.078*	0.061	bx 0.113 Financial -0.073*	bx 0.017 Financial 0.011	2006: 0.0102	0.077 07+	0.064*	0.059*
					2007+ -	0.011		
								0.060*
Corporate Samples/Cross-sectional	0.071 0.367*	-0.400 1.952*	Financial 0.365 Multi 0.073*		-96 0.019 97-01 0.276 02-06 0.084 07+ 0.184*			0.071 0.355*
Geographic Areas/Cross-sectional								
Other units of analysis/Cross-sectional								
Individual Samples/Longitudinal								
Corporate Samples/Longitudinal								
Geographic Areas/Longitudinal								
Other units of analysis/Longitudinal								
<b>Treatment: Regulatory Policies</b>								
Individual Samples/Cross-sectional								
Corporate Samples/Cross-sectional	-0.238 0.073*	-0.018 0.772	Other 0.049 Financial 0.018 Political -0.059 Enviro - 0.390 Multi - 0.316*	Enviro 2.584 Political 0.417 Multi - 0.339	-96 0.053 02-06 -0.321 07+ - 0.176*	-96 2.796 97-01 0.765 07+ - 0.018*		0.418 0.364
Geographic Areas/Cross-sectional	-0.003 0.092*		Political 0.051 Financial -0.077		97-01 0.102 07+ - 0.088*			

Other units of analysis/Cross-sectional		Political 0.083 Multi 0.108		
Individual Samples/Longitudinal				
Corporate Samples/Longitudinal				
Geographic Areas/Longitudinal				
Other units of analysis/Longitudinal				
<b>Treatment: Non-punitive actions</b>				
Individual Samples/Cross-sectional				
Corporate Samples/Cross-sectional				
Geographic Areas/Cross-sectional				
Other units of analysis/Cross-sectional				
Individual Samples/Longitudinal				
Corporate Samples/Longitudinal				
Geographic Areas/Longitudinal				
Other units of analysis/Longitudinal				
<b>Treatment: Multiple treatments</b>				
Individual Samples/Cross-sectional		Human bx 0.063 Other 0.066	02-06 0.069 07+ 0.114	0.023 0.136*
Corporate Samples/Cross-sectional	.09719 .08922*		97-01 0.284 02-06 0.072 07+	

---

0.097\*

---

Geographic  
Areas/Cross-  
sectional

---

Other units of  
analysis/Cross-  
sectional

---

Individual  
Samples/Longitudinal

---

Corporate  
Samples/Longitudinal

---

Geographic  
Areas/Longitudinal

---

Other units of  
analysis/Longitudinal

---

\*= $Q_{\text{between}}$  significant at .05

## Appendix V: Moderators Continued

	Controls vs. No Controls		Data Source for IV		Data Source for DV		Sample Selection (R=Random, NR=Non-Random, M=Missing)		Type of Crime	
	ES-R	ES-D	ES-R	ES-D	ES-R	ES-D	ES-R	ES-D	ES-R	ES-D
<b>Treatment: Law</b>										
Individual Samples/Cross-sectional										
Corporate Samples/Cross-sectional									Financial: 0.03 Env.: 0.02	
Geographic Areas/Cross-sectional										
Other units of analysis/Cross-sectional										
Individual Samples/Longitudinal										
Corporate Samples/Longitudinal										
Geographic Areas/Longitudinal										
Other units of										

analysis/Longitudinal						
<b>Treatment: Punitive Sanctions</b>						
Individual Samples/Cross-sectional	-0.043 0.194*	SR 0.372 Other 0.285*		R 0.075 NR -0.173*		Financial:- 0.04 Multiple: 0.24 Other: 0.03* Financial:0.01 Multiple: 0.02
Corporate Samples/Cross-sectional	0.074 0.288*	SR 0.363 Official 0.070*	SR 0.300 Official 0.073*	R 0.024 NR 0.072* M 0.370	NR 0.065 M 0.040	Financial: 0.37 Env.: 0.07 Other: 0.02*
Geographic Areas/Cross-sectional						
Other units of analysis/Cross-sectional						
Individual Samples/Longitudinal						
Corporate Samples/Longitudinal						
Geographic Areas/Longitudinal						
Other units of analysis/Longitudinal						
<b>Treatment: Regulatory Policies</b>						
Individual Samples/Cross-sectional						
Corporate Samples/Cross-sectional	-0.210 -0.215	SR 0.096 Official -0.228*	SR 0.087 Official -0.230*	NR -0.216 M -0.126*	NR -0.017 M 2.796*	Financial:0.11 Env.: -0.23*
Geographic Areas/Cross-sectional		SR -0.082 Other 0.250	SR 0.102 Official -0.090*			Financial:- 0.09 Env. 0.05*
Other units of analysis/Cross-						
		SR 0.083				

sectional	Official -0.051*
Individual Samples/Longitudinal	
Corporate Samples/Longitudinal	
Geographic Areas/Longitudinal	
Other units of analysis/Longitudinal	
<b>Treatment: Non-punitive actions</b>	
Individual Samples/Cross-sectional	
Corporate Samples/Cross-sectional	
Geographic Areas/Cross-sectional	
Other units of analysis/Cross-sectional	
Individual Samples/Longitudinal	
Corporate Samples/Longitudinal	
Geographic Areas/Longitudinal	
Other units of analysis/Longitudinal	
<b>Treatment: Multiple treatments</b>	
Individual Samples/Cross-sectional	
Corporate Samples/Cross-sectional	Financial:0.16 Env.:0.09
Geographic Areas/Cross-sectional	

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Other units of analysis/Cross-sectional

---

Individual Samples/Longitudinal

---

Corporate Samples/Longitudinal

---

Geographic Areas/Longitudinal

---

Other units of analysis/Longitudinal

---

\*= $Q_{\text{between}}$  significant at .05

# Appendix VI: Coding Protocol

## CC Meta-Analysis Coding Sheets: Study-Level Coding Protocol

**Bibliographic Reference (APA format):** \_\_\_\_\_  
 \_\_\_\_\_  
 \_\_\_\_\_  
 \_\_\_\_\_

### **I. Source Descriptors**

<b><u>Variable Name</u></b>	<b><u>Code</u></b>	<b><u>Item</u></b>												
ID		1) Study ID number: <ul style="list-style-type: none"> <li>- First 3 letters of first author’s last name followed by year</li> <li>- If duplicates, add an “A” or “B” based on alphabetical order of titles</li> </ul>												
PUBTYPE		2) Type of Publication: <table style="width: 100%; border: none;"> <tr> <td style="width: 50%;">1. Book</td> <td style="width: 50%;">2. Book chapter</td> </tr> <tr> <td>3. Journal article</td> <td>4. Thesis or dissertation</td> </tr> <tr> <td>5. Government report (state/local)</td> <td>6. Government report (federal)</td> </tr> <tr> <td>7. Working paper</td> <td>8. Conference paper</td> </tr> <tr> <td>9. Regulatory Agency report</td> <td>10. Corporate Report</td> </tr> <tr> <td colspan="2">11. Other (specify)</td> </tr> </table>	1. Book	2. Book chapter	3. Journal article	4. Thesis or dissertation	5. Government report (state/local)	6. Government report (federal)	7. Working paper	8. Conference paper	9. Regulatory Agency report	10. Corporate Report	11. Other (specify)	
1. Book	2. Book chapter													
3. Journal article	4. Thesis or dissertation													
5. Government report (state/local)	6. Government report (federal)													
7. Working paper	8. Conference paper													
9. Regulatory Agency report	10. Corporate Report													
11. Other (specify)														
PUBTYPE_OTH		2b) Type of Publication—specify other publication type:												
YEAR		3) Year of Publication												

DISCIPLINE		<p>4) Disciplinary Affiliation of Publication/Journal:</p> <ol style="list-style-type: none"> <li>1. Criminology</li> <li>2. Sociology</li> <li>3. Business/Marketing</li> <li>4. Political Science</li> <li>5. Environmental Science/Biology</li> <li>6. Psychology</li> <li>7. Public Policy</li> <li>8. Economics</li> <li>9. Other</li> <li>10. Multiple disciplines (list under DISC_OTH)</li> </ol>
DISC_OTH		<p>4b) Disciplinary Affiliation of Publication/Journal—specify other discipline:</p>
FUNDING		<p>5) Source of funding for the research:</p> <ol style="list-style-type: none"> <li>0. No funding/None reported</li> <li>1. Government agency</li> <li>2. University</li> <li>3. NGO/Non-profit</li> <li>4. Private business</li> <li>5. Other (specify)</li> </ol>
FUND_OTH		<p>5b) Source of funding for the research—specify other:</p>
NAT_PUB		<p>6) Country of Publication</p>
DATE		<p>7) Date coded</p>
CODER ID		<p>8) Coder:</p> <ol style="list-style-type: none"> <li>1. Natalie</li> <li>2. Patricia</li> <li>3. Melissa</li> <li>4. Mariel</li> </ol>

---

## **II. Study Characteristics**

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STUDYTYPE		<p>1) Type of study:</p> <ol style="list-style-type: none"> <li>1. Randomized experiment (in-basket or lab; e.g., conditions are randomized at the individual level or everyone receives the same survey)</li> <li>2. Randomized experiment (vignette survey; e.g., conditions within scenarios are randomized)</li> <li>3. Nonequivalent control group (quasi-experimental)—has a comparison group that is not randomly assigned (e.g., matched pairs comparison or propensity score matching)</li> <li>4. Time-series/pre-posttest (no control group)</li> <li>5. Time-series/pre-posttest (with control group)</li> <li>6. Non-experimental (i.e., multiple regression or correlation)</li> <li>7. Other (specify)</li> </ol>
STTYPE_OTH		1b) Type of study—specify Other:
STARTDATE		2) Date Range of Research: First year of data
ENDDATE		3) Date Range of Research: Last year of data
NAT_STUD		4) Country where study conducted:
NUMOUT		5) Number of crime/misconduct outcomes reported in study <sup>ii</sup>
UOA		<p>6) What is the unit of analysis in this study (i.e., the type of outcome)?</p> <ol style="list-style-type: none"> <li>1. Individual decision-making/behavior</li> <li>2. Company decision-making/behavior</li> <li>3. Geographic area (e.g., state, country)</li> <li>4. Other (specify)</li> </ol>
UOA_OTH		6b) What is the unit of analysis in this study? Specify other:
DATARLBTY		<p>7) Did the researcher empirically assess the reliability of the data collected?</p> <ol style="list-style-type: none"> <li>1. Yes</li> <li>0. No</li> </ol>

DATAVLDY		8) Did the researcher assess the validity of the data collected (e.g., discussed whether measures used accurately represented the construct of interest)? 1. Yes 0. No
DATAPROB		9) If the researcher noted any concerns about the data, please describe here:

### **III. Sample Descriptors**

SAMPLEN		1) Sample size
SAMP_INDCOR		2) Does the sample consist of individuals or corporations? 1. Individuals 2. Corporations 3. Other (specify; e.g., court cases)
SAMP_OTHER		2b) Does the sample consist of individuals or corporations? Specify other:
SAMP_MIX		3) Was the sample drawn from more than one organization? 1. Yes 0. No

**If the sample consists of individuals, answer the following questions. Otherwise, skip to question #9:**

AGE		4) Mean Age of Sample (if mean age cannot be determined, enter 888)
-----	--	---

RACE		<p>5) Predominant Race of sample</p> <ol style="list-style-type: none"> <li>1. Mostly white</li> <li>2. Mostly black</li> <li>3. Mostly Hispanic</li> <li>4. Mostly Asian</li> <li>5. Mixed, none more than 50%</li> <li>6. Mixed, cannot estimate proportion</li> <li>888. Unknown/Not reported</li> </ol>
SEX		<p>6) Predominant Sex of sample</p> <ol style="list-style-type: none"> <li>1. 60% or more male</li> <li>2. 60% or more female</li> <li>3. Even mix of male and female</li> <li>888. Unknown/Not reported</li> </ol>
MGMT		<p>7) Predominant management level of sample:</p> <ol style="list-style-type: none"> <li>1. 60% or more non-managerial employee</li> <li>2. 60% or more middle managers or supervisors</li> <li>3. 60% or more CEO/Executives (or highest-level employees such as law firm partners)</li> <li>4. Even mix of multiple levels</li> <li>5. Other (Specify)</li> <li>888. Unknown/Not reported</li> </ol>
MGMT2		<p>7b) Management level of sample—specify other:</p>
PRTCNT		<p>8) Who were the participants of the study?</p> <ol style="list-style-type: none"> <li>1. Unemployed students</li> <li>2. Working students</li> <li>3. Both unemployed and working students</li> <li>4. Professionals</li> <li>5. Both students and professionals</li> </ol>

EDUCATION		<p>9) Predominant education level of sample</p> <ol style="list-style-type: none"> <li>1. 60% or more: High school degree or less</li> <li>2. 60% or more : Some college education (or currently in college)</li> <li>3. 60% or more: College graduates</li> <li>4. 60% or more: Some graduate education (or currently in graduate program)</li> <li>5. 60% or more: Completed graduate degree</li> <li>6: Even mix of multiple education levels</li> <li>888. Unknown/Not reported</li> </ol>																												
EMPLENGTH		<p>10) Length of employment of the target population:_____</p> <ol style="list-style-type: none"> <li>1. No work experience</li> <li>2. Less than 5 years</li> <li>3. Between 5 – 10 years</li> <li>5. More than 10 years</li> <li>6. Multiple levels of experience included in sample</li> <li>888. Unknown/Not reported</li> </ol>																												
INDUSTRY		<p>11) From what industry was the sample drawn? (choose all that apply)</p> <table style="width: 100%; border: none;"> <tr> <td style="width: 50%;">1. Agriculture</td> <td style="width: 50%;">2. Accounting</td> </tr> <tr> <td>3. Advertising</td> <td>4. Airline</td> </tr> <tr> <td>5. Banking</td> <td>6. Biotechnology</td> </tr> <tr> <td>7. Computer/Technology</td> <td>8. Consumer products</td> </tr> <tr> <td>9. Defense</td> <td>10. Education</td> </tr> <tr> <td>11. Energy</td> <td>12. Food, beverage, or tobacco</td> </tr> <tr> <td>13. Health care</td> <td>14. Investment banking</td> </tr> <tr> <td>15. Legal</td> <td>16. Manufacturing</td> </tr> <tr> <td>17. Marketing/Business</td> <td>18. Pharmaceuticals</td> </tr> <tr> <td>19. Real Estate</td> <td>20. Retail</td> </tr> <tr> <td>21. Securities and Commodities</td> <td>22. Service</td> </tr> <tr> <td>23. Telecommunications</td> <td>24. Transportation</td> </tr> <tr> <td>888. Unknown/Not reported</td> <td>25. Other (specify)</td> </tr> <tr> <td colspan="2">26. Multiple categories (list under IND_OTH)</td> </tr> </table>	1. Agriculture	2. Accounting	3. Advertising	4. Airline	5. Banking	6. Biotechnology	7. Computer/Technology	8. Consumer products	9. Defense	10. Education	11. Energy	12. Food, beverage, or tobacco	13. Health care	14. Investment banking	15. Legal	16. Manufacturing	17. Marketing/Business	18. Pharmaceuticals	19. Real Estate	20. Retail	21. Securities and Commodities	22. Service	23. Telecommunications	24. Transportation	888. Unknown/Not reported	25. Other (specify)	26. Multiple categories (list under IND_OTH)	
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26. Multiple categories (list under IND_OTH)																														

IND_OTH		11b) From what industry was the sample drawn? Specify other:
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**If the sample consists of corporations, please answer the following questions:**

COMPSIZE		12) Average number of employees in sample companies (if UOA is firm and information is not given, record 888)
COMPPROF		13) Average profit of companies in sample (not given = 888)
COMPSALES		14) Average annual sales of companies in sample (not given = 888)

**IV. Methods and Procedures**

RANDOM		1) Was the sample randomly selected? 1. Yes 0. No 888. Unclear or not reported
SAMPPROC		2) Sampling procedures 1. Random probability sample 2. Stratified random sample 3. Matched-pairs 4. Snowball sampling 5. Convenience sample (drawn from individuals to which researchers have easy access) 6. Secondary data analysis (without specification of sampling procedures) 7. Other (specify)
SAMPPR_OTH		2b) Sampling procedures—specify other:
SURVEY		3) Survey design 1. Mail 2. Phone 3. Face-to-face Interview 4. Other (specify) 777. Not applicable (not a survey)

SURVEY_OTH		3b) Survey design—specify other:
CROSSEC		4) Is the research design cross-sectional or longitudinal? 1. Cross-sectional (including pooled cross-sections or lagged independent variables) 2. Longitudinal (i.e., the dependent variable is measured at more than one point in time to assess change over time)
BIAS		5) Did the authors assess the differences between survey respondents' and non-respondents' background characteristics? 1. Yes 0. No 777. Not applicable (not a survey)
BIAS_YES		5b) If yes, were significant differences found between responders' and nonresponders' background characteristics? 1. Yes 0. No 777. Not applicable
BIAS_ADD		5c) If yes, what did the authors do to address these differences?
RESPRATE		6) Response rate to survey (777 if not a survey)
ATTRITION		7) If longitudinal, rate of attrition (put 777 if not a longitudinal panel survey)
SIGLEVEL		8) Level of statistical significance used (usually .05)
CONTROL		9) Nature of control group 1. Randomly assigned—no treatment 2. Randomly assigned—alternative treatment 3. Natural experiment—no treatment 4. Natural experiment—alternative treatment 5. Time-series—pre/post 6. Propensity-score matching/Matched pairs 7. No control group (not an experimental design or not a listed QED)

PRETEST		<p>10) Did the authors assess pre-test differences between TX/control groups?</p> <p>1. Yes</p> <p>0. No</p>
PRTST_DIFF		<p>10b) If so, were differences found between groups?</p> <p>1. Yes</p> <p>0. No</p>
PRTST_ADD		<p>10c) If yes, what did the researchers do to address these differences?</p>

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**V. Description of Independent Variable**

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TREATMENT		<p>1) What form did the treatment take?<sup>iii</sup></p> <ol style="list-style-type: none"> <li><del>1. Class/training on compliance with standards or procedures</del></li> <li>2. Law (including case law)</li> <li>3. Official sanction/Fine (e.g., conviction, prosecution, prison) OR punishment avoidance (e.g., acquittal)</li> <li>4. Regulatory Policy (e.g., inspections, agency resources, deregulation)</li> <li><b>16.</b> Non-punitive action by regulatory agency (e.g., warning letter, cease and desist order)</li> <li><del>5. Corporate policy (e.g., procedures, employee participation)</del></li> <li><del>6. Corporate structure (e.g., corporate governance initiative, percent outside members on board)</del></li> <li><del>7. Compliance program (e.g., internal monitoring, voluntary organizational programs involving more than education)</del></li> <li><del>8. Membership in external professional organization</del></li> <li><del>9. Consultant participation (e.g., accounting firm or others)</del></li> <li><del>10. Unionization/Employee Committees</del></li> <li><del>11. Workers' benefits (e.g., workers' compensation)</del></li> <li><del>12. Organizational climate (e.g., supervisory support or punishment by the company)</del></li> <li><del>13. Informal sanctions (e.g., bad publicity)</del></li> <li>14. Other (specify)</li> <li>15. Multiple treatments involved</li> </ol>
TREAT_DES		1b) Brief description of treatment:
TREAT_BIN		<p>1c) Was the independent variable binary or continuous?</p> <ol style="list-style-type: none"> <li>1. Binary</li> <li>2. Continuous</li> </ol>
TREATCON_DES		2) Description of continuous independent variable measurement:

IV_SOURCE		<p>4) What data sources were used to measure the independent variables? (Select all that apply)</p> <ol style="list-style-type: none"> <li>1. Official data</li> <li>2. Self-report data (e.g., surveys or interviews)</li> <li>3. Observations/site visits of places or environments</li> <li>4. Other (specify)</li> </ol>
IVSRCE_OTH		<p>4b) What data sources were used to measure the independent variables? Specify other:</p>
CONTROLS		<p>5) Did the authors control for potentially spurious variables?</p> <ol style="list-style-type: none"> <li>1. Yes</li> <li>0. No</li> </ol>

#### **VI. Dependent Variable Descriptors**

OUTCM_ACT		<p>1) Did the outcome describe <i>actual</i> behavior (e.g., arrests) or <i>intentions</i> (e.g., hypothetical situations)?</p> <ol style="list-style-type: none"> <li>1. Actual behavior</li> <li>2. Intentions/Opinions about behavior/Lab setting</li> <li>3. Both</li> </ol>
OUTCM_DSC		<p>1b) Brief description of outcome<sup>iv</sup>:</p>
OUTCMDTA		<p>2) What type of data was used to measure the outcome covered on this coding sheet?</p> <ol style="list-style-type: none"> <li>1. Official data</li> <li>2. Self-report data (e.g., surveys or interviews)</li> <li>3. Observations/site visits of places or environments</li> <li>4. Other (specify)</li> </ol>
OUTDTA_OTH		<p>2b) What type of data was used to measure the outcome covered on this coding sheet? Specify other:</p>

OUTMSRE	3) How was the DV measured?  1. Scale—1 item 2. Composite 3. Raw number of violations (or rates: #/unit of opportunity) 4. Dichotomous measure 5. Other (specify)—e.g., dollar amounts
OUTMSRE_OTH	3b) How was the DV measured? Specify other:
OUTMSRE_DES	4)Description of continuous outcome measure:
ILL_UNETH	5) Is the DV measured using illegal or unethical behavior? <sup>v</sup>  1. <b>Illegal</b> (e.g., can be sanctioned by law enforcement or regulatory sanctions, or is subject to auditing) 2. <b>Unethical</b> (morally ambiguous but not subject to sanctions) 3. <b>Both</b> 4. <b>Other (specify):</b> (Unclear whether sanctionable/only related to company policies) <b>888. Unknown/Not reported</b>
ILLUNETH_OTH	5b) Is the DV measured using illegal or unethical behavior? Specify other:
COMP_SOC	6) Is the victim of the offense the company or general society?  1. Company 2. Society 3. Other (specify)
COMP_SOC_OTH	6b) Does the behavior affect the company or society, according to Akers' (1977) list? Specify other:

## **II. Effect Size Data<sup>vi</sup>**

ATT_PROB		<p>1) Was attrition a problem for this outcome?</p> <p>1. Yes 0. No 777. Not Applicable (not a panel survey) 888. Not reported/unknown</p>
ATT_CASES		<p>2) If attrition was a problem, how many cases were lost?</p>
ATT_REAS		<p>3) If attrition was a problem, why were cases lost?</p>
RAWDIFF		<p>4) Raw difference (i.e., means or frequencies) favors (i.e. shows more success for):</p> <p>1. Treatment group (or post period) 2. Control group (or pre period) 3. Neither (exactly equal) 888. Unknown 777. Not applicable</p>
SIGDIFF		<p>5) Did a test of statistical significance indicate statistically significant differences between either the control and treatment groups or the pre and post tested treatment group?</p> <p>1. Yes 0. No 888. Unknown 777. Not applicable</p>
STANDES		<p>6) Was a standardized effect size reported?</p> <p>1. Yes 0. No</p>
ES		<p>7) If yes, what was the effect size</p>
ES_PAGE		<p>8) If yes, page number where effect size data is found</p>
NOES		<p>9) If no, is there data available to calculate an effect size?</p> <p>1. Yes 0. No</p>

NOES_DATA	10) Type of data effect size can be calculated from: <sup>vii</sup> <ol style="list-style-type: none"> <li>1. Means and standard deviations</li> <li>2. <i>t</i>-value or <i>z</i>-value</li> <li>3. <i>F</i>-value</li> <li>4. Chi-square (df=1)</li> <li>5. Frequencies or proportions (dichotomous)</li> <li>6. Frequencies or proportions (polychotomous)</li> <li>7. Pre and post</li> <li>8. Standardized regression coefficients</li> <li>9. Unstandardized regression coefficients</li> <li>10. Correlations (Pearson's <i>r</i>)</li> <li>11. Other (specify)</li> </ol>
NOES_OTH	10b) Type of data effect size can be calculated from— specify other:
NOES_REG	10c) If the data presented is an <i>unstandardized</i> regression coefficient, what type of regression was used? <ol style="list-style-type: none"> <li>1. OLS</li> <li>2. Logistic</li> <li>3. Tobit</li> <li>4. Poisson</li> <li>5. Other (specify)</li> <li>6. Ordered logit</li> </ol>
NOES_REG2	10d) If the data presented is an <i>unstandardized</i> regression coefficient, what type of regression was used? Specify other:
TX_N	11) Treatment group sample size <sup>viii</sup>
CON_N	11b) Control group sample size
TX_propN	12) Proportion of sample in treatment group (Tx/Tx+Control)
CON_propN	12b) Proportion of sample in control group (Con/Tx+Control)
TXMEAN	12) Treatment group mean (dependent variable)

CONMEAN		12b) Control group mean (dependent variable)
TXSD		13) Treatment group standard deviation (dependent variable)
CONSD		14) Control group standard deviation (dependent variable)
SUCCTX_N_a		15a) <i>n</i> of treatment group with successful outcome
SUCCCON_N_c		15b) <i>n</i> of control group with successful outcome
FAILTX_N_b		16a) <i>n</i> of treatment group with unsuccessful outcome
FAILTX_N_d		16b) <i>n</i> of control group with unsuccessful outcome
PROPTX_SUCCa		17) Proportion of treatment group with successful outcome
PROPCON_SUCCc		18) Proportion of control group with successful outcome
TVALUE		19) <i>t</i> -value (for independent/dependent-samples means comparisons only)
TVALUE_P		19b) <i>t</i> -test <i>p</i> value
ZVALUE		20) <i>z</i> -value
ZVALUE_P		20b) <i>z</i> -test <i>p</i> value
FVALUE		21) <i>F</i> -value
FVALUE_P		21b) <i>F</i> -test <i>p</i> value
CHISQ		22) Chi-square value (df=1)

CHISQ_P		22b) Chi-square <i>p</i> value
SD_X		23) Standard deviation of the independent variable
SD_Y		24) Standard Deviation of the dependent variable (note: for dichotomous dependent variables, this can be calculated using the formula $\sqrt{p(1-p)}$ )
UNSTNDRGS		25) Unstandardized regression coefficient
STNDRGSS		26) Standardized regression coefficient
PRSONR		27) Pearson's <i>r</i>
OTHDATA		28) Type of data effect size can be calculated from: (specify other—actual data)

### **III. Conclusions made by the author**

CNCLS_IMM		<p>1) Did the assessment find evidence for the effectiveness of the treatment? (e.g., significant statistical test in the hypothesized direction)</p> <p>0. No 1. Yes 2. Not tested</p>
CNCLS_REL		<p>2) Did the author(s) conclude there a relationship between the corporate crime prevention technique and a reduction in illegal corporate activities/violations, regardless of significant finding?</p> <p>0. No 1. Yes 2. Can't tell/Author did not discuss</p>
CNCLS_ADD		3) Additional notes about conclusions:
UNIQESAMPLE		4) Was this sample used in this study used in another article included in this meta-analysis? If yes, list other study IDs that use this sample.

#### **DECISION RULES AND NOTES ABOUT VARIABLES**

<sup>1</sup> If book or unclear, code from author bio

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<sup>ii</sup> For our purposes, we will include studies that examine criminal and regulatory violations by corporations or their employees. The majority of corporate offenses are handled by regulatory agencies, like the EPA & OSHA. Thus, a focus on strictly criminal behaviors would limit this study and miss a great deal of corporate misconduct. According to Clinard and Yeager (1980), corporate crime is “any act committed by corporations that is punished by the state, regardless of whether it is punished under administrative, civil, or criminal law” (p. 16). **This offense-based definition encompasses a wide range of behaviors such as antitrust offenses, intentionally polluting the environment, unsafe labor practices, and tax and securities violations.**

<sup>iii</sup> We are looking for variables that measure:

- Extralegal or legal interventions and that are policy-relevant (i.e., can be the subject of an intervention).
  - o “General organizational climate” is not relevant unless this includes specific policies in the organization that affect compliance.
  - o We are NOT interested in personality characteristics (e.g., morality) or a person’s approval of the law, job, policy, etc.
- Things we ARE interested in include
  - o Civil or criminal laws or sanctions (including civil cases)
  - o Ethical or safety **policies** within the company
  - o Internal compliance/monitoring programs
  - o Market devices such as shaming (e.g., bad publicity)
  - o Membership in external professional organizations that can sanction members
  - o Internal/external audits
  - o Corporate structure, including
    - Insider vs. outsider members on the board of directors (including gray and independent directors)
    - Public vs. private ownership
    - Whether CEO is head of the board of directors
  - o People’s perceptions of risks (e.g., of getting caught or being sanctioned either formally or informally)
- We are mainly interested in the **presence vs. absence** of such variables, not descriptions about these IVs or gradations/dosage of the treatment (e.g., we are not interested in the size of the auditing company).
  - o If the independent variable is related to corporate compliance programs or something that seems to be of interest, include it only if you can dichotomize it and if there is not already a dichotomous variable of interest (e.g., company expenditures on compliance—could be dichotomized if companies report \$0 versus non-zero values).
- When an intervention includes multiple components but only has one data point, just record one case and list all of the categories of the treatment variable under which it could fall, separate by commas.

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<sup>iv</sup> Regarding measures of the dependent variable, we are not looking at overcompliance in and of itself.

- If overcompliance is measured, it can be used if compared to noncompliance (and should be combined with compliance if applicable).
- We ARE interested in severity measures (e.g., the amount of money lost, number of injuries) as well as compliance vs. noncompliance.

<sup>v</sup> An illegal act is one that has been formalized as a law or regulatory statute—i.e., you can be sued, cited, or arrested for it.

- Unethical practices are those that are not punishable under the law but are morally questionable.

<sup>vi</sup> Decision rules on including ESs:

- If two or more tables/models are presented on the same IV and same operationalization of the DV, include all **unique measures** of the variables of interest.
- Prioritizing the table/model that 1) includes more IVs of interest and 2) has the full (more final) model.
- If alternative modeling strategies (e.g., OLS as well as Poisson) are used and there is no significant difference between the two use the simpler model.
- After including all of the variables of interest from the final model, include any other (not already included) variables of interest from other models that may have been dropped from the final model.

<sup>vii</sup> Anytime an article has more than one model, NOES\_DTA should only have one value and there needs to be another case. **There needs to be a new case anytime you have a new independent variable, dependent variable, or model (e.g., anytime you have data coming from a different place).**

- When both unstandardized and standardized coefficients, just record that you have standardized coefficients in NOES\_DTA but record both in their appropriate places
- When both means and t-tests, just record that you have a t-test in NOES\_DTA but record both in their appropriate places
- If you have a regression coefficient and descriptive statistics (means, SD), just record the regression in NOES\_DTA but give all of the information in the appropriate place

<sup>viii</sup> For time-series, the baseline/pre-intervention numbers belong under the “control group.” The post-test is the treatment group.