

## What Works? A systemic review of corporate crime deterrence

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**What Works? A systemic review of corporate crime deterrence**

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

**Abstract**

***Research Summary***

We conducted a meta-analysis of corporate crime deterrence strategies using 80 effect sizes calculated from 58 studies in four treatment areas – Law, Punitive Sanctions, Regulatory Policy, and Multiple Treatments. Of the single treatment strategies, only Regulatory Policy produced a significant deterrent impact at the company level, but the results were not consistent across all study and effect size types. Studies examining multiple treatments, though, produced a consistent significant deterrent effect on offending at both the individual and company-levels.

***Policy Implications***

Our results suggest that regulatory policies that involve consistent inspections and include a cooperative or educational component aimed at the industry may have a substantial impact on corporate offending. However, a mix of agency interventions will likely have the biggest impact on broadly-defined corporate crime. The variety of offenders and behaviors included in corporate crime and its relative complexity likely necessitate multiple intervention strategies, similar to the “pulling levers” approach developed to deter gang violence and the responsive regulation strategy advocated by Braithwaite. However, given some of the shortcomings in the literature, we also recommend that scholars and practitioners more clearly and consistently define the phenomenon of “corporate crime” and target evaluation research toward specific programs and interventions.

## Introduction

Evaluation research in criminal justice has a long history, beginning with the Cambridge-Somerville Youth Study in 1937 (McCord and McCord, 1959). However, concerns regarding program effectiveness reached fever pitch in the 1970s (see Cullen and Gendreau, 2000), and criminal justice scholars were called upon to establish “what works, what doesn’t and what’s promising” across a variety of different spheres (see, e.g., the “Maryland Report”; MacKenzie, Eck, Reuter, and Bushway, 1997). The Campbell Collaboration (C2) was founded to produce up-to-date research summaries about the effects of interventions in education, crime and justice, and social welfare with the proposed intention that such reviews could be used to inform decision-making about programs and policies in these areas (Petrosino, Boruch, Soydan, Duggan, and Sanchez-Meca, 2001). These “evidence-based” efforts to synthesize and decipher knowledge typically utilize a specific methodological technique, meta-analysis, to draw inferences about the merits of assorted programs and policies (Cullen and Gendreau, 2000).

Nearly all systematic reviews in the crime and justice area focus on traditional types of offenders and/or offenses. Yet, newly emerging crime problems such as cross-border human trafficking (Reichel, 2008) and cybercrime (Grabosky, 2014; Maimon, Alper, Sobesto, and Cukier, 2013) as well as old problems, such as white-collar crime, are gaining societal urgency and currency—white collar crime, for example, has re-emerged in light of the recent mortgage meltdown and global financial crisis (Deflem, 2011; Simpson and Yeager, 2015). With a few exceptions (Schell-Busey, 2009; Van Der Laan, Smit, Busschers, and Aarten, 2011), literature summaries of these subjects rely mainly on narrative reviews where “studies are described discursively and afforded relative importance by the reviewer” (Pratt, 2011:1). The research reported here fills this void by offering a quantitative synthesis of research on corporate crime using a meta-analytic approach, focusing on intervention efforts to prevent and control (“deter”) corporate crime. Given that the myriad of possible strategies, programs, and practices designed to achieve this aim would be overwhelming for one systematic review, our efforts concentrate on studies of formal legal interventions and corporate deterrence. In the next section, we define the concept of corporate crime and draw from the literature to describe the theory and mechanisms of corporate deterrence. Then we discuss the rationale and design of the meta-analysis before reporting on the findings.

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With one exception, we find that single treatment strategies (Law, Punitive Sanctions and Regulatory Policy) have minimal to no deterrent impact at the individual and company levels. However, studies examining multiple treatments produce a significant deterrent effect on individual and corporate-level offending.

Moderator analyses generally indicate that the less methodologically rigorous studies and older studies are more likely to produce a significant deterrent effect than the studies employing experimental designs and newer studies. Based on our results, we determine that a mix of agency interventions is apt to have the biggest impact on corporate crime as it is currently defined and measured in the literature. However, because our analysis also reveals a number of shortcomings in the corporate crime literature, we recommend a series of steps that scholars can take to strengthen this body of work. We conclude that it will take careful and systematic research to produce more focused and effective deterrence strategies.

### ***Literature Review***

#### ***Definitions***

The definitional arena of white-collar crime is highly contested ground. Edwin Sutherland (1939), who is credited with creating the concept, is also responsible for generating definitional ambiguity by defining the phenomenon as “a crime committed by a person of respectability and high social status in the course of his occupation” at the same time he focused his empirical research on corporations (1949: 2). Clinard and Yeager (1980: 13) wryly observe that Sutherland’s book should have been entitled *Corporate Crime* instead of *White Collar Crime*. As the first to undertake a systematic review of corporate deterrence, we are unwilling to take a strict stance on the definitional issue because research consistent with the subject matter will be described as white-collar, organizational, corporate, workplace, or business crime (among others).<sup>1</sup> Acts may be committed by top executives, employees, representatives, officials, partners and/or business organizations (Geis, 1962; Blankenship, 1993; Braithwaite, 1984) and can represent a range of behaviors from financial fraud to occupational health and safety offenses (Clinard and Yeager, 1980; Simpson and Schell, 2009). Violators may be pursued and sanctioned administratively, civilly, and/or criminally (Simpson, 2002; Laufer, 2006; Garrett;

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<sup>1</sup> For a more detailed discussion of this definitional ambiguity in the field of corporate crime, see Rorie, Alper, Schell-Busey & Simpson (2015).

2014; Anderson and Waggoner, 2014). In general, we are interested in illegal acts that occur within companies and are undertaken by firm representatives or the company itself to benefit the corporation. Such behavior is differentiated from acts by individuals within the corporation acting in the pursuit of self-interest (Braithwaite, 1984). Importantly, corporate crime depends on and is facilitated by the organizational context in which it occurs (Simpson and Yeager, 2015).

### ***Deterrence***

The deterrence framework in criminology traces its intellectual origins to Beccaria (1963) and Bentham (1948), but its empirical roots to Gibbs (1968) and Tittle (1969). These early scholars used aggregate state-level data to assess whether arrest/incarceration (certainty of punishment) or incarceration length (severity of punishment) for a particular crime were associated with lower crime rates in the future (Paternoster, 1987). This approach to studying the effect of formal legal punishment on offending is characterized as “objective” deterrence because it relies on the actual level of punishment as opposed to “perceptual” deterrence which emphasizes the subjective effect of the threat of punishment on an individual’s criminal participation (Waldo and Chiricos, 1972). Deterrence scholars further differentiate punitive effects on the general population (general deterrence) from the punished individual (specific deterrence), although this distinction is not a tidy one (Stafford and Warr, 1993; Piquero and Pogarsky, 2002).

Deterrence is a well-studied phenomenon (Nagin, 1998), but research in this area has primarily focused on traditional crimes. There are far fewer empirical studies of corporate deterrence (Buell, 2006). Both objective and perceptual approaches are represented in the corporate crime literature (Del Guercio, Odders-White, and Ready, 2013; Braithwaite and Makkai, 1991; Makkai and Braithwaite, 1994), as are investigations that focus on specific and general deterrent outcomes (Simpson and Koper, 1992; Block, Nold, and Sidak, 1981), though it is often difficult to differentiate empirically between the two given measurement limitations (Cohen, 2000: 10246). Recent forays into the literature have honed the deterrent and educative role of vicarious punishments (Thornton, Gunningham, and Kagan, 2005; Yiu, Xu, Wan, 2014: 1568), noting that firms appear more “alert” to punishment of particular kinds of target firms—those that are more prominent and peers within an industry. Research also differentiates targets by unit of analysis and sanction source. For instance, some studies focus on the corporate entity as the subject of sanctions (Cohen, 2000; Buell, 2006; Simpson, Garner,

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and Gibbs, 2007) while others examine how representatives/managers respond to punishment (Simpson, Gibbs, Rorie, Slocum, Cohen, and Vandenberg, 2013). Both the firm and responsible parties may be subject to legal interventions of some sort, ranging from criminal prosecution to civil and/or regulatory actions (Cohen, 2000; Simpson and Koper, 1992; Simpson, 2002).

A notable difference between the corporate and traditional deterrence literatures has been the development of a responsive regulation strategy tied to a regulatory sanctions pyramid (Ayres and Braithwaite, 1992). This model of corporate regulatory enforcement incorporates elements of persuasion compliance (based on a cooperative strategy) and deterrence applied in a hierarchical and iterative process. The strategy encourages cooperative approaches initially (at the base of the pyramid) and escalation to more punitive interventions when cooperative engagement fails. One implication of the model is that compliance and deterrence are not mutually exclusive processes but work together to enhance law-abiding behavior.

Moreover, as Gunningham, Grabosky, and Sinclair (1998) reminded us, prevention and control capacities of different regulatory strategies are often introduced alongside one another. In the United States, for instance, corporations may be simultaneously subject to a variety of enforcement tools (e.g., regulatory agency sanctions as well as criminal prosecution by law enforcement agencies). Such an approach is conceptually similar to the “pulling levers” deterrence strategy employed by law enforcement to reduce gang and group-involved violence (Braga and Weisburd, 2012), which couples problem-oriented policing with community social services and resources. The enforcement pyramid also emphasizes that “regulators should not rush to law enforcement solutions to problems before considering a range of approaches that support capacity-building” (Braithwaite, 2011: 480)..<sup>2</sup>

Like the traditional deterrence literature, empirical findings in the area of corporate deterrence are mixed. In their test of a subjective utility model, Braithwaite and Makkai (1991: 29) report deterrence to be a “stark failure”. Formal sanctions were found to be unrelated to or even counter-intuitively predictive of

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<sup>2</sup> Grabosky (1995; see also Gunningham, Grabosky, and Sinclair, 1998) recommended more third-party involvement in the enforcement pyramid, but practically speaking, these suggestions have not been formally implemented in the United States. Braithwaite (2011) recognizes the important role played by non-legal actors in the regulatory network (the base of his enforcement pyramid emphasized the role of ethics, education, and persuasion) and he has criticized the over-emphasis on deterrence within the pulling levers approach (Braithwaite, 2002).

offending outcomes. Objective (firm-level) studies also refute deterrence. Simpson and Koper (1992) reported that actual punishment levied criminally, civilly, or by the Federal Trade Commission did not significantly reduce the likelihood of offending firms' future anti-competitive acts. However, shifts in law (for instance, changing the offense from a misdemeanor to felony) were associated with lower recidivism risk. Simpson, Garner, and Gibbs (2007), found little evidence that firms respond positively to punitive or cooperative (regulatory) intervention strategies. Firms with the worst environmental records were inspected and sanctioned more often but with little effect on company recidivism.

These mostly dismal results for deterrence are challenged by other studies that find: (1) general deterrent effects associated with punishment of the offending firm (Block, Nold, and Sidak, 1981; Yiu et al., 2014); (2) a reduction in the odds of managers being willing to violate environmental law associated with the threat of formal sanctions (Simpson et al., 2013); and (3) general and specific deterrent effects associated with environmental enforcement (Cohen, 2000). It is hard to unravel the meaning of such outcome variability because the studies themselves are so variable. Inconsistent results may be a function of research quality (design, sample selection, controls) and/or variation might be related to company or offense type, unit of analysis, source of data, and the way in which deterrence is measured. At this point, it is not possible to draw firm conclusions about corporate deterrence from a narrative review of the empirical literature in this area. We turn to meta-analysis as a useful tool for statistically exploring these patterns. Next, we describe the rationale and design of the meta-analysis before reporting our results.

### ***Methods***

This study is the first to assess comprehensively formal legal and administrative prevention and control strategies for corporate crime using meta-analysis. The purpose of a meta-analysis is to summarize the best available research on a specific question using studies as units of analysis. Relevant studies are identified, reviewed, combined, and quantitatively synthesized to compare effects and draw conclusions about the general tendencies revealed in the literature. In this particular case, we are interested in whether formal legal interventions “deter” corporate offending. To determine whether interventions are effective deterrents or not, an effect size is calculated for each study to standardize findings for purposes of comparison.

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Meta-analysis has many benefits to recommend it over traditional qualitative review strategies, but there are some limitations as well (Cullen and Gendreau, 2000; Berk, 2007). The advantages, as stated by Lipsey and Wilson (2001) are fourfold: (1) meta-analysis requires all steps to be “public” and open to scrutiny; (2) the methods are more sophisticated than narrative reviews in that researchers can encode the magnitude and direction of relevant statistical relationships; (3) it is possible to examine the relationship between study findings and study features (e.g., sample characteristics, research design, and measurement procedures); and (4) the systematic coding procedures and database construction provide an organized way of handling information from a large number of study findings, and the database can be updated. There are some negative aspects as well. A meta-analysis can only be as strong as the studies included in the review. If methodologically weak studies are included, they can affect the strength of the conclusions. Researchers also are limited to the information provided in the original studies. In some instances, there are study features that cannot be retrieved and included in the meta-analysis. Another potential problem is publication bias. Research shows that the effects of published studies tend to be larger than those reported in unpublished ones (Lipsey & Wilson, 2001; Wilson, 2009; Dickersin, 2005). Thus, by not including unpublished studies, researchers risk introducing an upward bias in effect sizes, which can result in biased or mistaken conclusions and policy recommendations (Rothstein, 2008; Wilson, 2009).

Some of the potential biases associated with these limitations (e.g., publication bias or the impact of methodologically weak studies) can be assessed empirically as part of a moderator analysis or by statistically checking for bias (described in more detail below). These strategies, coupled with the implementation of careful inclusion criteria with regard to study selection, mitigate several of the methodological limitations of meta-analytic approaches. Further, since the ultimate goal of a meta-analysis is to stimulate evidence-based policy and practice, meta-analysis allows for an examination of what strategies and interventions work and which do not. Given the enormous consequences of corporate crime on society (Cohen, 2000; Liederbach, Cullen, Sundt, and Geis, 2001; Clinard and Yeager, 1980; Barak, 2012), a comprehensive review and assessment of corporate crime deterrence is long overdue.

### *Search Strategy and Inclusion Criteria*

Our search of the literature occurred in two phases; an original search was conducted in 2004 to uncover all eligible documents produced prior to that year, and an update was conducted to find additional studies produced from 2004-2011.<sup>3</sup> To locate these documents, the research team searched 41 online databases and other sources for published and unpublished studies and reports using 69 search terms.<sup>4</sup> For each database and search term, we tracked the number of hits produced by the search (241,014 total hits produced); figure 1 below illustrates the number of studies/cases at each stage of this process. Studies deemed “potentially eligible” (meaning they were both empirical and related to corporate crime deterrence) were moved on to the next phase of coding (4,664 documents) to determine whether the research met our five inclusion criteria. Those that fit the criteria were deemed “Eligible” and included for coding (188 studies). Inclusion criteria included the following:

- 1) The study had to be 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 had to include 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 did not include a treatment *group*, it must report standardized regression coefficients or Pearson correlations if the treatment is measured continuously.
- 3) The study had to report on at least one crime/misconduct outcome. In accordance with our broad definition of corporate crime, the outcome of interest may be one of a wide range of criminal behaviors, regulatory violations, or civil violations.
- 4) The study was written in English, but may be cross-national.
- 5) The study was published before 2012.

Ineligible studies were dropped from further analysis unless the article contained relevant references, in which case it was designated for additional searches, as were the references of our eligible studies. We also increased the breadth of the search by entering all eligible studies into the Web of Science database for forward searching, perusing the contents of leading journals in the field, and—during the original search phase in 2004—checking our work with a group of experts in the corporate crime field who were asked to identify other

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<sup>3</sup> There are slight differences in how the two searches were conducted. After a peer review of the initial protocol, it was recommended that we narrow the focus of the meta-analysis to studies of deterrence efforts only. Search terms included in the initial 2004 search (e.g., “compliance programs,” “training,” and “prevention”) were dropped and the research team focused only on those terms being used that more directly indicated deterrence strategies (e.g., “sanctions,” “fines,” and “regulatory policies”). All databases used in the initial search were used in the second search, with one addition (WorldCat) deemed necessary because of technological changes between the first search and the second. In addition, experts reviewed a list of included articles retrieved from the first search, but we did not obtain expert reviews for the second search. These changes should not have substantively affected the results, as the primary search used many more search terms and thus was more inclusive. In addition, the expert review of the first search resulted in only four articles identified for potential consideration but none of those met the inclusion criteria.

<sup>4</sup> For example, search terms included, “corporate crime AND fines”, “regulatory policy AND antitrust”, “sanction AND white-collar crime”.

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studies that met our criteria. Through all of these processes, we added 77 additional studies to our sample for a total of 265 studies. Unfortunately, fewer than half (106) of our selected studies contained enough information to calculate effect sizes. Ultimately, we ended up with 1,083 unique effect sizes before aggregating.<sup>5</sup> See Figure 1.

### *Coding Protocol*

The explicit focus on legal interventions and deterrence reported in this study is part of a larger effort to document and summarize the entire domain of corporate crime prevention and control. Thus, an initial coding protocol was created to capture this broader focus—one that included internal corporate sanctions, compliance courses, organizational climate, and informal sanctions in addition to formal legal interventions. This larger focus, however, was unmanageable so we restricted our analysis to studies that examined the impact of laws, regulatory policies, punitive sanctions (either civil, criminal, or regulatory), and multiple legal interventions. These categories are described more fully in the results section below. Studies that fell into these categories were coded using the protocol, which captures information on the source and characteristics of the study, sample characteristics, the methods and procedures used by study authors, descriptions of the independent and dependent variables, effect size data, and the conclusions drawn by the study authors. Inter-rater reliability was tested for both phases of the data collection process. During this process, raters first resolved differences and created decision rules for coding, then continued to calculate reliability until acceptable reliability (a Kappa or Pearson Correlation value of at least 0.70) was reached for all items.

### *Statistical Procedures*

Because this systematic review included a wide variety of studies and outcome measures, we calculated multiple types of effect sizes based on the data. Whenever possible, the “standardized mean-difference effect size” was utilized for analyses comparing two groups’ performance. We also used the “product-moment correlation effect size” to estimate the amount of variance explained for relationships in which both the independent variable and dependent variable were continuous. Many studies reported multiple measures of the dependent variable, and a number of authors published more than one article using data from the same sample.

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<sup>5</sup> A list of the studies can be obtained from the authors.

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 effect size (see Lipsey and Wilson, 2001). Using this process, the 1,083 unique effect sizes created 159 aggregate effect sizes in four units of analysis (individual-level, corporate-level, geographic-level, and other). This paper, though, focuses on the 80 effect sizes from 63 studies that were at the individual or corporate levels for four treatment types – Law, Punitive Sanctions, Regulatory Policy, and Multiple Treatments.

Individual study effect sizes were calculated in Microsoft Excel and verified using Wilson’s Effect Size Calculator available on the Campbell Collaboration website.<sup>6</sup> When possible, results are compared across the four treatment groups by sample type (individual v. corporate sample), data type (cross-sectional v. longitudinal), and effect size type (ES-D v. ES-R). Following Lipsey and Wilson (2001), we computed the mean effect sizes<sup>7</sup> and the homogeneity of effects across studies using the inverse variance weight method. A random effects model was assumed and variance components were calculated accordingly. These mean effect computations were run using Stata macros - available on the Campbell Collaboration website (see footnote 6).

We conducted a number of tests for publication bias (Rothstein, 2007) using three different techniques. The first method, Egger’s regression (Egger, Davey-Smith, Schneider, & Minder, 1997), attempts to quantify bias by examining the relationship between the effect sizes and the standard error of effects (Rothstein, 2008). The second method used was “cumulative meta-analysis” in which studies are sorted from largest to smallest and a meta-analysis is run adding one study at a time. A forest plot of the studies helps determine whether there is a shift when smaller studies are added, which would indicate bias (Rothstein, 2008). Finally, Duval and Tweedie’s (2000) trim and fill procedure assesses whether publication bias is affecting the results and estimates what the effect size would be in the absence of publication bias. The adjusted mean effect describes the robustness of the meta-analytic results in the face of publication bias.

There were no treatment types for which all three techniques detected publication bias. Thus, in our results below we discuss the cases where potential bias was indicated in two of the three tests. Finally, we also

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<sup>6</sup> [http://www.campbellcollaboration.org/crime\\_and\\_justice/index.php](http://www.campbellcollaboration.org/crime_and_justice/index.php)

<sup>7</sup> We also examined the data for outliers and recoded them to be 2 standard deviations above/below the mean (Lipsey and Wilson, 2001).

investigated moderator analyses to see whether indicators of study quality explained variability in the observed effects.

### ***Meta-Analysis Results***

Mean effect sizes were calculated for each combination of treatment type, unit of analysis, and time ordering – if there were enough studies within the category to make comparisons possible. For reasons that will be illuminated below, not all of our analyses revealed consistent or conclusive results. Therefore, we provide figures and details for results that are more stable and summarize the findings for other treatment types.<sup>8</sup>

#### **Law and Punitive Sanctions**

The first two treatment types, Law and Punitive Sanctions, produced inconclusive results. Law captures the link between offending and characteristics of, or changes in, law. The five law effect sizes for *company-level* offending produced a marginal deterrent effect (ES=0.021;  $p=0.054$ ), but the results are sensitive to publication bias. Thus, we cannot conclude that Law has a deterrent effect on corporate offending but we do not have enough evidence that the treatment does not work just because results failed to reach significance. We did not have enough studies to examine the effect of law on *individual-level offending* and therefore are unable to draw any firm conclusions about the deterrent effects of law on corporate crime (Weisburd, Lum, and Yang, 2003).

Punitive sanctions include studies that examine the impact of imposed or threatened sanctions including fines, prosecution, conviction, imprisonment or the avoidance of punishment. We analyzed the effect of punitive sanctions for both individuals and firms. Although both levels of analysis showed a slight tendency (albeit insignificant) toward deterrence, again we are hesitant to draw strong conclusions about effects because there is much outcome variation by data type (cross-sectional versus longitudinal). For instance, at the individual level, the studies indicate a tendency toward deterrence (ES-R: ES=0.023;  $p=0.822$  and ES-D: ES=0.056;  $p=0.635$ ). Similarly, punitive sanctions measured cross-sectionally at the firm level suggest a slight (but also insignificant) deterrent effect (ES-R: 0.083;  $p=0.110$  and ES-D: ES=0.204;  $p=0.758$ ). However, the mean effect size for firm-level longitudinal studies was negative (ES=-0.053;  $p=0.12$ ) and all analyses, regardless of unit of analysis or type of data analyzed were subject to publication bias. Based on these results,

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<sup>8</sup> Results for these analyses are available on request.

we do not have enough evidence to conclude that punitive sanctions have a deterrent effect on individual or company-level offending.

### ***Regulatory Policy***

Regulatory policy focuses on the impact of monitoring activity (e.g. inspections), agency resources, and shifts in regulatory policy (such as deregulation) on firm offending. As shown in Figure 1, the regulatory policy studies also produced mixed results. The longitudinal studies produced insignificant and conflicting results, and the mean effect size from the nineteen cross-sectional ES-Rs was negative (ES=-0.061;  $p=0.310$ ) while the mean effect size from the eight cross-sectional ES-Ds showed a significant deterrent effect (ES=0.871;  $p=0.000$ ). The tests for publication bias, though, indicated some concern with the results for the cross-sectional ES-Rs but no issues with the ES-D studies. Because the mean effect size for the eight ES-Ds is large<sup>9</sup> and there is no proof of publication bias, the evidence in favor of deterrence is strong relative to the negative and insignificant relationship revealed by the ES-R studies—even when we removed a possible outlier<sup>10</sup> from the analysis that reduced the effect size (ES=0.40) and level of significance ( $p=0.080$ ) (Weisburd, et al., 2003). See Figure 2.

Overall, the impact of regulatory policy on offending is far from stable. This may be a function of measurement differences between studies. For instance, comparisons in some studies were made between groups that were either subject to the regulation or not. In these cases, the presence of regulatory policy was measured with a dichotomous variable. Others examined the correlation between levels of regulatory policy and offending and so measured the amount of regulatory action using a continuous variable. Studies using the correlational approach show a non-significant mean effect size that is iatrogenic, or negative, while the dichotomous measurement studies (i.e., capturing differences between groups) reveal the largest effect sizes in this study (0.87 and significant in cross-sectional studies, 0.48 and non-significant in longitudinal studies). This indicates that, on average, the presence of regulatory policy leads to a .87 standard deviation decline in offending. We will discuss this finding in more detail below.

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<sup>9</sup> An effect size of .8 for this treatment type is large according to Cohen (1988) and substantial when compared with Lipsey's typical rule of thumb for criminal justice research in which he suggests that an effect size of .10 could be practically significant (2000).

<sup>10</sup> (Ger 1994A)

Another explanation for the inconsistent and contrary regulatory policy findings relates to the idea of a tipping point. At the company level, lower quantities of regulatory action may reduce the risk of offending but more intense regulatory action may encourage defiance of the law instead of adherence to it. That is to say, certain levels of regulatory intervention may be accepted by the regulated community as necessary and legitimate or, at least, tolerable. However, acceptability and tolerance may yield to defiance if the application of the law is seen as particularly burdensome. Although this interpretation is speculative and requires much more evidence to support it, qualitative research has uncovered instances of regulatory defiance (for a summary see Braithwaite, 2009) along with occasions of “creative” compliance—situations where companies find ways to meet the letter of the law while undermining its legitimacy at the same time. (McBarnet, 2004).

### ***Multiple Treatments***

The last type of intervention that we examine is “multiple treatment.” This category measures combination interventions that were not and cannot be separated in the original study. For instance, Gibbs and Simpson (2009) have a variable labeled “total (informal and formal) sanction count” that includes “administrative action, under enforcement review, enforcement conference or letter, penalty, court orders, judicial action, etc.” (p. 101). And at the company level, Almutairi (2000) used an index for political enforcement that included “enforcement of penalties, monitoring, accountability, implementation of pre-auditing notes and recommendations”. When we encountered these multi-treatment studies, we analyzed them as a separate category and calculated mean effect sizes, which are displayed in Figures 2 and 3. All of these studies were cross-sectional. At the individual level, the five ES-R studies indicate a significant deterrent effect for multiple sanctions ( $ES=0.096$ ;  $p=0.045$ ). At the company level, there were six ES-Rs, and this mean effect size also shows a significant deterrent effect ( $ES=0.114$ ;  $p=0.029$ ). Our sensitivity tests reveal no evidence of publication bias at either level of analysis. Thus, we are more confident in the robustness of these results. See Figure 3. See Figure 4.

Overall, the results of the multi-treatment studies show a modest but consistent deterrent effect on offending. The modest size of the effects (.09 and .11) could be construed as statistically significant but relatively meaningless. But, as noted previously, an effect of 0.10 can be meaningful and practically significant, particularly in crime and justice research (Lipsey, 1990; 1999; 2000). Since the financial cost of corporate crime

is typically much greater than that of traditional street crime (Cohen, 2000), a small reduction in offending likely results in large financial savings to victims and society. Moreover in areas such as pollution abatement or employee health and safety, modest changes in company practices and improvements in compliance may have substantial effects on population health. Therefore, we assert that these small but consistent results for multi-treatment studies should be viewed as important until proven otherwise.

### *Summary*

To summarize the main effects of law, punitive sanctions, regulatory policy, and multiple treatments on corporate deterrence, we calculated 12 mean effect sizes of which nine (75%) indicated a deterrent impact. For the most part, the effects were modest and only three out of nine effect sizes were statistically significant at the .05 level (see Table 1 for a summary of the findings). The lack of consistent, significant findings could be due to a lack of available data as most of our analyses were based on fewer than 10 studies. Further, many of those studies did not use rigorous methods that would rule out spurious relationships or establish proper temporal ordering. Given these concerns, we examine whether our results are subject to methodological design and quality issues by conducting moderator analyses. These findings are reported in the next section. See Table 1.

### *Moderator Analyses*

Because of the small number of effect sizes in each of our categories we used a fixed effects (or unpooled) model, as recommended by Williams (2013), to assess whether study “quality” affected our findings. Specifically, the following study characteristics were examined: (1) methodological design, (2) the use of random samples as opposed to non-random samples, and (3) publication year.

Study design can influence the results since more methodologically rigorous studies may be more likely to produce a null result (Weisburd, Lum, and Petrosino, 2001). We conducted a moderator analysis to assess whether results depended on experimental versus quasi-experimental versus non-experimental designs – and further broke down non-experimental designs to compare those with controls to non-experimental studies without controls. We had enough variation to conduct this analysis on punitive sanction studies at both levels, regulatory policy studies at the company level, and multiple treatments at both levels. Table 2 displays these results. See Table 2.

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For punitive sanctions at the individual level, both experimental and non-experimental with controls had an iatrogenic effect, but the negative effect was larger and significant for experimental studies. At the company level, punitive (ES-R) sanction studies that were both non-experimental with controls and without controls had a significant deterrent effect, but the effect was larger for studies without controls. On the other hand, punitive (ES-D) studies showed that non-experimental studies with controls had a significant iatrogenic effect while the studies without controls had a significant deterrent effect.

For regulatory policy (ES-R), studies at the corporate level (both non-experimental with controls and without controls) had a significant iatrogenic effect, and the effect did not differ significantly across the two study types. However, regulatory policy (ES-D) studies demonstrated that both non-experimental studies with controls and without controls had a significant deterrent effect, though the deterrent effect for the studies with controls was greater.

The results for multiple treatments at the individual level demonstrate that both the experimental studies and the non-experimental studies with controls produced a deterrent effect, though the effect for the experimental studies failed to reach significance. The deterrent effect for the non-experimental studies with controls was significantly stronger than the effect for the experimental studies. At the company level, the analysis for multiple treatments showed that non-experimental studies with controls had a significant deterrent effect while the studies without controls had a significant iatrogenic effect. The effects of these two study types were significantly different.

Overall, we see that more rigorous studies found less deterrent impacts of studies and, sometimes, more iatrogenic results except in the case of multiple treatments. This was expected since experimental studies are more likely to control for potentially spurious relationships and are better able to establish that the intervention preceded the outcome (Cullen, Johnson, and Nagin, 2011). Further, this finding is consistent with previous studies. Weisburd, Lum and Petrosino (2001) reported that studies with higher internal validity were more likely to report smaller effect sizes than studies with designs associated with lower internal validity. The authors were examining aggregate treatment outcomes and surmised that if they could break the treatments down, they might see variation by treatment type. Our results support this supposition since the effect was not found for multi-

treatment studies. Still, these results should be interpreted cautiously given the small number of studies available to conduct this analysis.

Another methodological difference between studies included in the meta-analysis was whether the sample was randomly selected. Randomly selected samples also reflect greater methodological rigor because they reduce the likelihood of selection bias. We were able to examine the moderating effect of sample selection for punitive sanctions and regulatory policies, but only for cross-sectional studies. Among Punitive Sanctions studies, use of random samples in individual-level studies produces a greater deterrent effect while at the corporate level, non-random samples and studies missing sample selection information produce a greater deterrent effect than random samples. For regulatory policies, studies missing sample selection information generate a greater deterrent effect than nonrandom samples. Unfortunately, these inconsistent and sparse results do not allow us to conclude much about the impact of sample selection on our results.

We also examined whether year of publication affected the results because 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. We were able to examine moderating effects for seven study types, and in all but one of these, older studies showed a stronger deterrent effect. A summary of these moderator results is available in Table 3. See Table 3.

### ***Discussion and Conclusions***

Inconsistent findings from our meta-analysis of corporate deterrence make it difficult to classify interventions into categories of what works, what doesn't, and what's promising. Generally, there is not enough evidence to conclude that the single strategy treatments (Law, Punitive Sanctions and Regulatory Policy) have a deterrent impact on their own, with the exception of Regulatory Policy at the company-level. On the other hand, the large deterrent impact of the presence of regulatory policies at the firm level is a promising finding and so are the consistent, yet smaller effects, of multiple treatments. To better understand these effects, we unpacked the studies that comprise each treatment category.

#### ***Regulatory policy, company level***

Most studies in the regulatory policy grouping examined environmental regulations, but the positive effects were found across a mix of environmental policies (Krahn, 1998; Gerardu & Wasserman, 1994A&B),

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immigration hiring policies (GAO, 1988) and anti-competition and consumer protection policies (Nielsen & Parker, 2005). The common element that stood out from these studies—and that which produced a significant deterrent effect was the presence of a policy that provided for inspections/inquiries and enforcement. The effectiveness of inspections has been demonstrated elsewhere (research not included in this study). For instance, Occupational Safety and Health Administration inspections significantly reduce workplace injuries and violations (Viscusi, 1986; Gray & Jones, 1991; Cooke & Gautschi, 1981). Such inspections can be successful through both the presence of the policy and threat of inspections (general deterrence) and actual experience with inspections (specific deterrence) and the deterrence message may be even more salient when powerful companies or “like” firms within industries are subject to inspection and sanction (Yiu, Xu, Wan, 2014: 1568)

More specifically, what may be effective is consistent inspections. Krahn (1998) reported the creation of three new regulatory policies backed by consistent inspections produced greater declines in pollution compared to voluntary programs with no inspections. Similarly, in the qualitative examination of inspections, the GAO (1988) determined that more consistent inspections and verification of compliance would be more effective at preventing the hiring of illegal immigrants. Additionally, Gerardu & Wasserman (1994A) showed that standardized inspection checks and consistent follow-ups significantly reduced waste disposal licensing violations and that a compliance citation program allowed inspectors to consistently inspect and uncover more potential violators (Gerardu and Wasserman, 1994B).

Because many of these regulatory programs were created in cooperation with the regulated industry or involved educational initiatives to inform the industry of the new policies, the cooperative component of the program may enhance deterrence. For instance, two of the three programs in Krahn’s (1998) study were developed in cooperation with the industry and the third involved on-site visits and seminars. Similarly, the GAO (1988) discussed the lengthy education process undertaken to inform employers about new regulatory requirements and the Australian Competition and Consumer Commission discussed in Nielsen & Parker’s (2005) study focused on encouraging compliance with the law by educating and informing businesses about their responsibilities under the law. Finally, the citation compliance program in the Gerardu and Wasserman (1994B) study utilized inspections to enforce regulations but also to effectively educate owners and operators.

One observation worth noting is that the studies included in this treatment category were less likely to come from the United States compared to other treatment categories. The inclusion of successful regulatory policies in foreign countries, such as Canada (Krahn, 1998), Australia (Nielsen & Parker, 2005), and Netherlands (Gerardu & Wasserman, 1994A), contributed to the significant deterrent effect. This may point to more successful regulatory policies in these countries compared to the United States, but policies in the United States cannot be dismissed completely since two studies from the U.S. did produce significant deterrent results (GAO, 1988; Gerardu & Wasserman, 1994B). Thus, future research should look to see whether there are certain elements of regulatory policy, within the US and outside of the country, that are associated with driving down the risk of noncompliance.

These findings have implications for the regulatory enforcement pyramid. Given the deterrent impact of regulatory policy and the generally weak deterrent effect of punitive sanctions across the board, it makes sense to focus on regulatory policies at the middle level of the pyramid where persuasion is generally most needed to achieve compliance. Specifically, our findings indicate that policies may be more successful when industry has some input and policies are coupled with education and consistent inspections. More severe strategies (regulatory investigations, penalties, civil suits, and arrest/jail time) should be added where compliance has been difficult to achieve.

### ***Multiple Treatments, individual and company level***

Our most consistent finding (and least subject to publication bias and poor methodological design) is that *multiple treatments*, at the individual and company level, produce a significant deterrent effect. We cannot unpack, however, whether this effect is due to the unique contribution of regulatory actions (non-punitive interventions)<sup>11</sup>, which is mixed in with other treatments, or whether deterrence is achieved via the layered multi-prong approach of the enforcement pyramid. Given that the other interventions provided less consistent results, a combination of treatment may work better than any single treatment method for preventing corporate crime. Although we are unable to test this interpretation with the current data, the notion is consistent with

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<sup>11</sup> Our category “Regulatory Policy” included monitoring/inspections, agency resources, and changes in policy, such as deregulation, but we were unable to examine the effect of “Regulatory Interventions”, such as the use of fines or administrative investigations separately because there were too few studies to generate effect sizes for these kinds of sanctions/interventions by themselves and we could not disentangle them from others in the multiple treatment studies.

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empirical results from other studies of corporate crime control (see Simpson et al., 2013) and with policy recommendations for traditional crime (Braga, Kennedy, Waring & Piehl, 2001; Braga, 2008).

No clear patterns emerge in the individual level studies to provide specific policy recommendations. The only thing these studies had in common was that all of the multi-treatment studies examining individual behavior included measures of regulatory intervention, though one (Ulrich et al., 2003) was more vague than the others. Of these studies, four out of five (Gurley et al., 2007; Piquero, 2012, Ulrich et al., 2003; Simpson & Piquero, 2002) reported a deterrent effect, though the finding in the Simpson and Piquero (2002) study was insignificant. Additionally, two of those four significant studies mixed regulatory sanctions with punishment. For example, Gurley et al. (2007) included a variable for the severity of punishment in each of six scenarios. The punishment in these scenarios ranged from failing a test and a supervisor's reprimand to jail time and fines, including the company receiving a government fine of \$100,000 (the source of the fine was unclear). Their results revealed that severity of punishment was significantly related to ethical decision-making. Piquero (2012) used a "perceived risk" variable that included being arrested, being sued, and undergoing a regulatory investigation. Perceived risk was significantly and negatively related to price fixing intentions at the individual level. In general, these studies demonstrate that actions by administrative agencies such as the SEC or EPA may contribute to deterring corporate crime, particularly when paired with other sanction threats.

The corporate-level studies provide stronger evidence for the potential impact of regulatory interventions absent the potential confound of other kinds of legal sanctions. All studies save one (Gibbs & Simpson, 2009) demonstrated a significant deterrent impact. These studies included clear measures of regulatory interventions but qualified as multi-treatment studies because they typically used scales that included both regulatory policy and regulatory interventions. Of the four studies that produced a significant, positive effect, three focused on a combination of regulatory inspections and enforcement. Thus, it seems that a combination of regulatory monitoring, inspections, and enforcement can produce changes in company behavior.

These studies also may point to the stronger influence of federal regulatory interventions compared to state regulatory interventions. Four studies that showed a deterrent effect for environmental offending (Earnhart, 2007; Hartman et al., 1995) or accounting/budgetary violations (Almutari, 2000; Pellicioni, 2002) focused on federal administrative actions. Only two of these studies also examined state regulatory intervention and,

although significant effects of those regulations were reported (Earnhart, 2007; Hartman et al., 1995), the state effects were relatively modest compared with those of federal agencies.

Once again we note that half the studies driving the multiple treatment effect are of foreign origin – Kuwait (Almutairi, 2000) and South/Southeast Asia (Harman et al., 1995). While the other two significant studies are from the United States, both of the studies producing insignificant or negative results were also from the United States, and it is possible that without these foreign studies, the impact of regulatory interventions would have been less clear. We do not know how different cultures and economic systems affect the ways in which interventions are designed and implemented, and thus we recommend that future studies focus on unraveling these potentially confounding factors.

Multi-treatment studies indicate then that federal regulatory monitoring, inspections, and enforcement will create a modest deterrent impact, and they suggest that policies and standards must be clear, systematic, and comprehensive (Almutari, 2000; Pellicioni, 2002; Earnhart, 2007; Hartman et al., 1995). The multi-intervention studies in the accounting/budgetary area emphasized the need for clear policies and auditing standards that should be systematic and comprehensively implemented (Almutari, 2000; Pellicioni, 2002) but the studies differed regarding the deterrent role of enforcement. In addition, the multi-intervention studies emphasized the importance of cost-effectiveness (Hartman, 1995; Earnhart, 2007). To achieve this aim, agencies should examine the context of the policies in order to get the most out of enforcement. More generally, when regulatory interventions were part of the treatment, there was a more consistent indication of a deterrent effect. For these reasons, regulatory interventions are worth examining on their own to determine whether results are replicated when regulatory interventions are used on their own.

In the meantime, what we can conclude is that multiple treatments create a stronger deterrent impact than most of the single interventions, with the exception of regulatory policy. It seems reasonable that the wide variety of offenders and behaviors included in corporate crime and its relative complexity necessitates multiple intervention strategies. Indeed, the notion of pulling levers developed out of concerns with gang violence and the need to leverage state and federal criminal justice resources with those of other agencies and groups in the community to deter violence (see Kennedy, 1998; Braga, Kennedy, Waring & Piehl, 2001; Braga, 2008). Results offer support for a model of corporate regulatory enforcement that blends cooperation with

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punishment—the type and amount of enforcement response to be determined by the behavior of the manager/company (i.e., responsive regulation). Thus, at the top and even middle levels of the enforcement pyramid, multiple “levers” may need to be pulled to achieve compliance.

### ***Directions for Research***

Our meta-analysis also demonstrates the limitations of the corporate crime literature and can be used to guide a research agenda for corporate crime scholars. This study highlights the well-known fact that there are too few studies in this area; most of our analyses were conducted with ten or fewer studies, and just over half of our analyses included five or fewer studies. Furthermore, there is tremendous variety in the methodological approaches taken by researchers. We suspect that this variation is due, in part, to a lack of systematic data with which to examine corporate offending and the full array of prevention and control strategies. In contrast to more traditional crime research where criminal behavior is measured and dissected via official sources, victimization surveys, cohort studies, and national self-report surveys, corporate offending is not even measured officially in a systematic manner. And, while there are some victimization surveys (National White-Collar Crime Survey, FTC survey) and creative uses of self-report instruments, we have no idea as to the degree to which the different data sources overlap or portray unique incidence and prevalence patterns. Simpson and Yeager (2015: 12) observe that the first step toward solving these problems is to have a sensible, commonly understood and culturally shared definition of the phenomenon (see also, Rorie, Alper, Schell, and Simpson, 2015). But, even if the definitional problem could be put aside, there are other important issues to solve. For instance, there is no official data series that captures the full array of offending behavior for individuals and companies reflected in criminal, civil, and regulatory statistics; there are no measures of key offense and offender characteristics that are commonly defined, counted, and measured as they are for traditional offenses. Nor are there data extracts capable of encapsulating the full array of sanctions brought to bear against violators (Simpson and Yeager, 2015: 12). These deficiencies have created data vacuums, forcing researchers to cull a variety of sources to create unique datasets. It is little wonder that findings lack consistency.

Similarly, too little is known about the successes and failures of specific programs because scientific evaluation is rare and replication absent in this area. Compared with other criminal justice interventions (boot camps, scared straight, Closed Circuit Television cameras (CCTV), and sex-offender treatment), the studies

reviewed in this meta-analysis tend to focus on broad intervention strategies and not on the relative effectiveness of different policy initiatives or specific programs per se (although there are some evaluations within specific regulatory agencies, such as EPA). The paucity of such studies is likely affected by the inaccessibility of companies to outsiders for research purposes, a diffuse set of actors in the deterrence arena willing to cooperate with researchers, and a lack of funding to support evaluation projects. However, even with these difficulties, we encourage researchers to seek partnerships with firms and justice agencies to develop, implement, and evaluate programs that are designed to lower offending risks and/or enhance legal compliance.

Lastly, we need more complete and methodologically rigorous studies. Through our exhaustive search we originally located 265 studies that met our search criteria, but only 106 of those studies contained enough information to calculate effect sizes. In terms of quality, our moderator analyses found that generally the more rigorous studies produced less deterrent impacts of studies and, sometimes, more iatrogenic results (except in the case of multiple treatments). We concur with Weisburd et al. (2001) that such findings could point to the possibility of an overall positive bias in less rigorous corporate crime studies. Even though our results show a consistent deterrent effect for multiple legal interventions, with the general weaknesses in the literature, we are unable artfully to construct the layers and actions of the enforcement pyramid or identify the relevant actors who have a stake in deterrence and a lever to pull.

In sum, there is much work to be done before we can make solid and conclusive evidence-based policy recommendations. Scholars need better data and consistent measures of corporate crime. We need to undertake more focused and high quality studies (particularly randomized experiments or quasi-experiments) focused on program specific interventions (with replications). Until then, the answer to the question of what works, what doesn't, and what's promising in the area of corporate deterrence will remain elusive.

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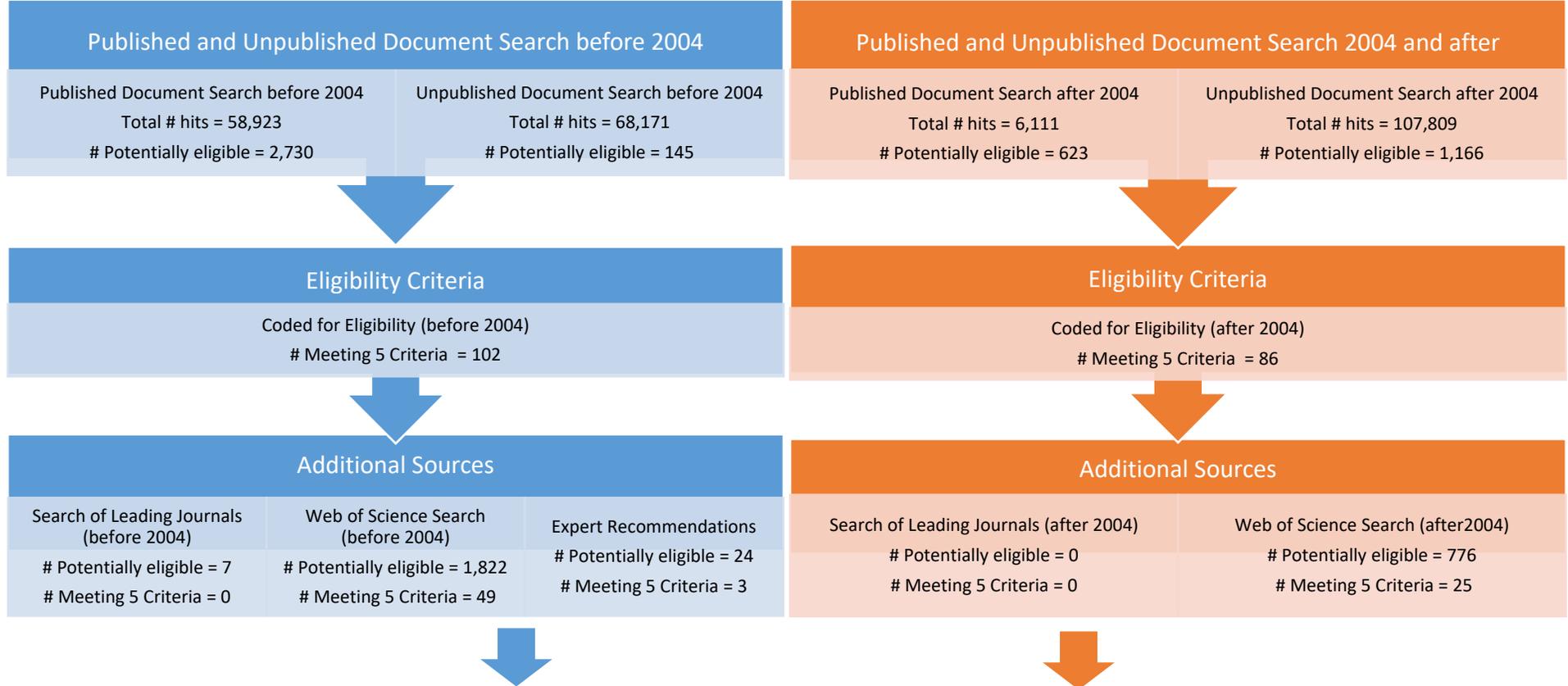
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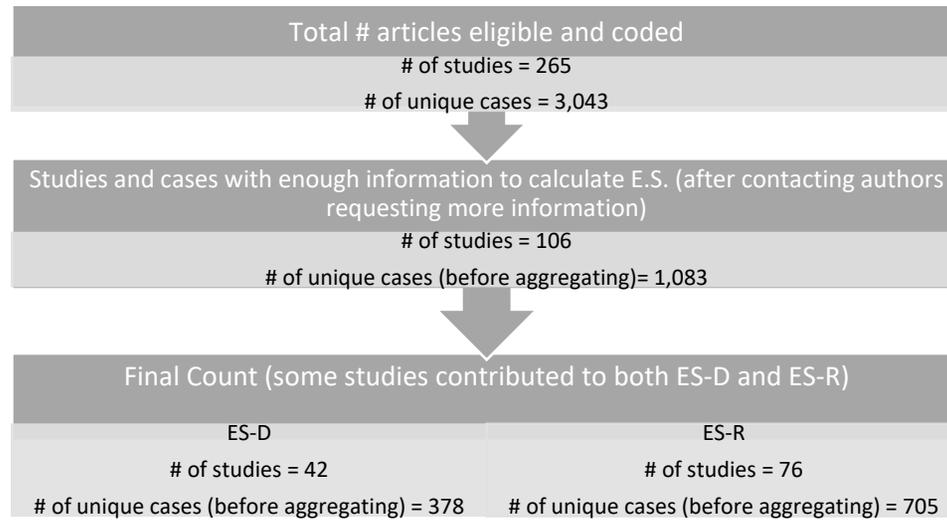
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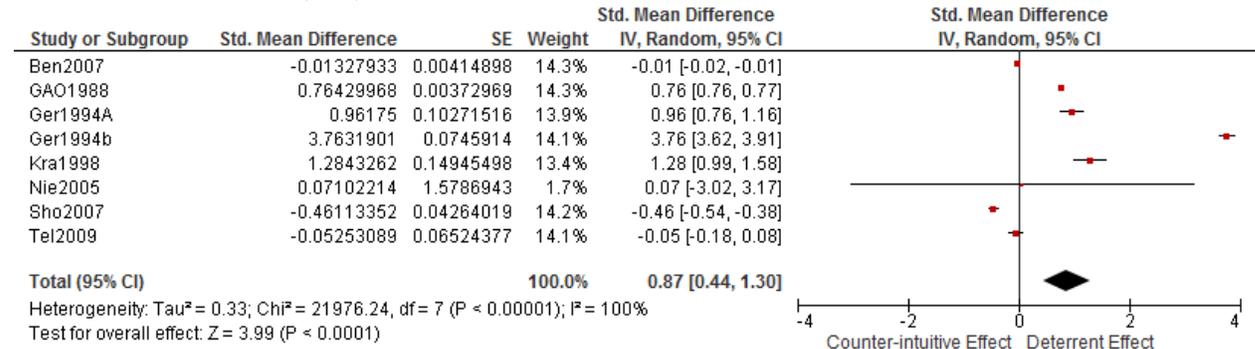
**Figure 1: Number of Studies or Cases at each Stage of the Search and Coding Process**



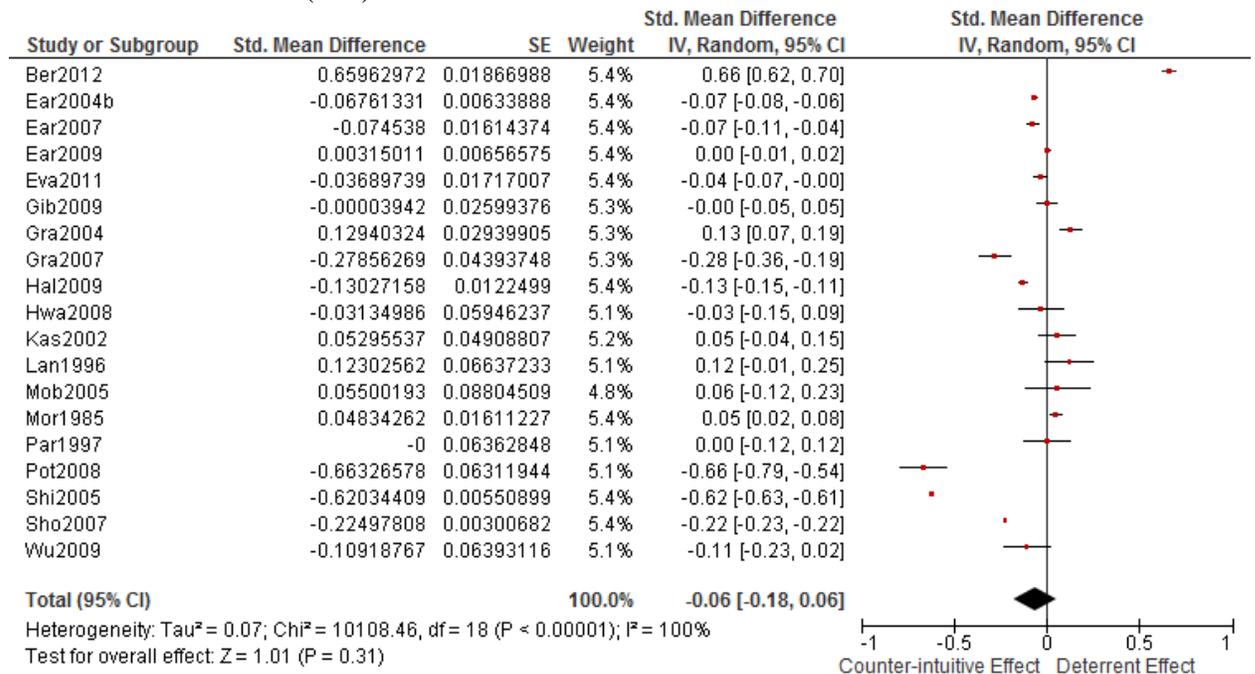
## What Works? A systemic review of corporate crime deterrence



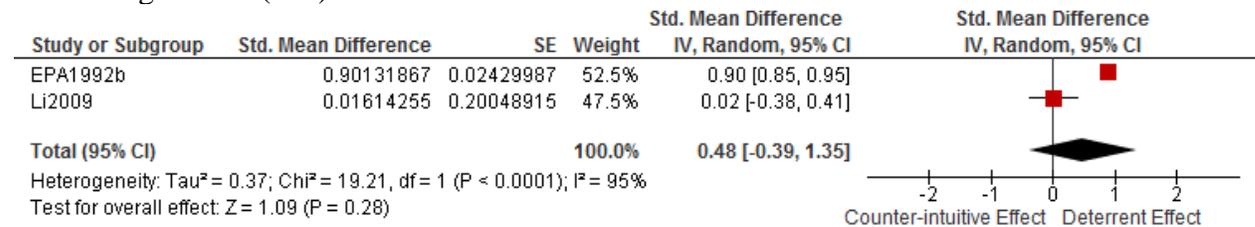
**Figure 2. Regulatory Policies at the Company Level**  
ES-D Cross-Sectional (142)



ES-R Cross-Sectional (142)

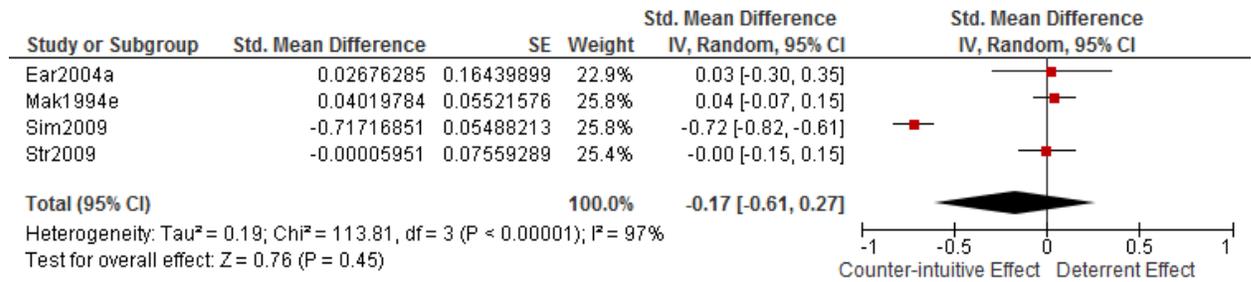


ES-D Longitudinal (242)

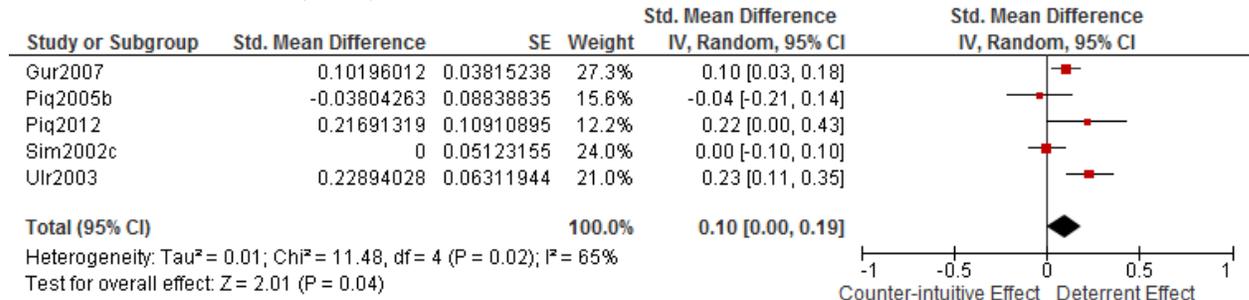


ES-R Longitudinal (242)

# What Works? A systemic review of corporate crime deterrence



**Figure 3. Multiple Treatments at the Individual Level**  
ES-R Cross-Sectional (1151)



**Figure 4. Multiple Treatments at the Company Level**  
ES-R Cross-Sectional (1152)

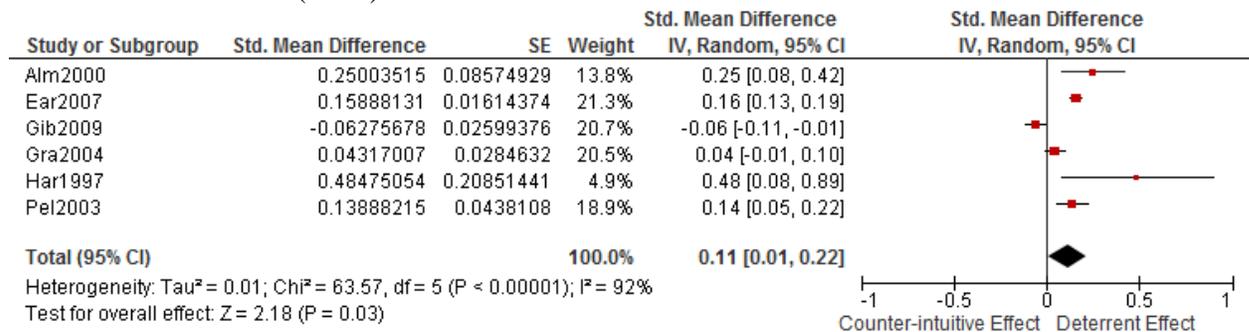


Table 1. Summary of Meta-analysis Results

<b>Treatment Type</b>	<b>Summary of Findings - Individual</b>	<b>Summary of Findings - Corporate</b>
Law	N/A	Marginally significant deterrent impact, but results are sensitive to publication bias.
Punitive Sanctions	Insignificant deterrent effect but evidence of publication bias.	Both cross-sectional and longitudinal studies indicate insignificant results.
Regulatory Policy	N/A	Inconsistent results depending on the measurement of regulatory policy. The presence of regulatory policy has a significant impact on offending, but it may be that more regulatory action encourages defiance instead of compliance.
Multiple Treatments	Significant deterrent effect.	Significant deterrent effect.

Table 2: Categories Examined as Moderators for Deterrence Outcomes

	<b>Study Design:</b> (E=Experimental, QE=Quasi-experimental, NC=Non-experimental with controls, NW=Non-experimental without controls)		<b>Sample Selection</b> (R=Random, NR=Non- Random, M=Missing)		<b>Year of Publication</b> (-96=1996 and earlier, 97-01= 1997-2001, 02-06=2002-2006, 07+=2007 and after)	
	<b>ES-R</b>	<b>ES-D</b>	<b>ES-R</b>	<b>ES-D</b>	<b>ES-R</b>	<b>ES-D</b>
<b>Treatment: Law</b>						
Corporate Samples/Cross- sectional						
<b>Treatment: Punitive Sanctions</b>						
Individual Samples/Cross- sectional	E -0.170 NC -0.031*		R 0.016 NR -0.173*		02-06: 0.121 07+: -0.203*	
Corporate Samples/Cross- sectional	NC 0.074 NW 0.288*	NC -0.130 NW 0.458*	R 0.024 NR 0.072* M 0.370	NR 0.065 M 0.040	-96: 0.019 97-01: 0.276 02-06: 0.084 07+: 0.184*	
<b>Treatment: Regulatory Policies</b>						
Corporate Samples/Cross- sectional	NC -0.210 NW -0.215	NC 0.762 NW 0.623*	NR -0.216 M -0.126*	NR -0.017 M 2.796*	-96: 0.053 02-06: -0.321 07+: -0.176*	-96: 2.796 97-01: 0.765 07+: -0.018*
<b>Treatment: Multiple treatments</b>						
Individual Samples/Cross- sectional	E 0.023 NC 0.136*				02-06: 0.069 07+: 0.114	
Corporate Samples/Cross- sectional	NC 0.134 NW -0.054*				97-01: 0.284 02-06: 0.072 07+: 0.097*	

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

Table 3. Summary of Moderator Analyses

<b>Moderator Variable</b>	<b>Treatment Type</b>	<b>Findings</b>
<b>Research Design Rigor</b>	Punitive Individual Level	Experimental studies have significant iatrogenic effect.
	Punitive Corporate Level (ES-R)	Non-experimental studies with and without controls had a significant deterrent effect, but effect larger for studies without controls.
	Punitive Corporate Level (ES-D)	Non-experimental studies with controls had a significant iatrogenic effect while studies without controls had a significant deterrent effect.
	Regulatory Corporate Level (ES-R)	Non-experimental studies with and without controls had a significant iatrogenic effect.
	Regulatory Corporate Level (ES-D)	Both non-experimental with controls and without had a significant deterrent effect, though effect was stronger with controls.
	Multiple Individual Level	Non-experimental studies with control produced a significant deterrent effect.
	Multiple Corporate Level	Non-experimental studies with controls had a significant deterrent effect; studies without controls had a significant iatrogenic effect.
<b>Random Sample</b>	Punitive Individual Level	Random samples produce greater deterrent effect.
	Punitive Corporate Level	Non-random samples and studies missing sample selection information produce greater deterrent effect.
	Regulatory Corporate Level	Studies missing sample selection information generate greater deterrent effect than nonrandom samples.
<b>Year of Publication</b>	Punitive Individual, Punitive Corporate, Regulatory	Older studies produce stronger deterrent effect.

	Corporate, Multiple Corporate	
	Multiple Individual	Newer studies produce a stronger deterrent effect, but it is not significant.