Crime, Punishment and the Value of Corporate Social Responsibility*

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Abstract

Using enforcements of the Foreign Corrupt Practices Act, we test the hypothesis that socially responsible (ESG) firms receive lower sanctions from prosecutors. Since virtually all cases are settled by bargaining, we estimate sanction specifications derived from a Nash Bargaining model. To account for ESG firm bribes potentially being unobservably less egregious, we instrument ESG with legal code pages in the state of the firm's headquarters. Our instrumented estimates point to ESG firms receiving $14.3 million or 65% lower sanctions, all else equal. Consistent with our exclusion restriction, pages of state laws are uncorrelated with revenues from bribes.

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1 Introduction

According to a 2009 McKinsey Survey, two-thirds of CFOs and three-quarters of investment professionals embraced the notion that pro-social business behavior adhering to environmental, social and governance (ESG) criteria adds to shareholder value. In particular, they believed that the value added is tied to promoting a positive corporate reputation. Despite considerable academic and practitioner attention, evidence regarding the usefulness of sustainable investments have been mixed at best (see Margolis, Elfeinbein, and Walsh (2009) for a meta-analysis).

The traditional research approach has been to take a large cross-section of firms and test if firm ESG scores, typically produced by a large commercial vendor such as MSCI-KLD, are correlated with or predict firm financial performance. Besides the usual omitted variables or selection concerns (Hong, Kubik, and Scheinkman (2011)), the economic magnitudes of correlations between ESG scores and expected stock returns have not been robustly large to begin with.

One reason perhaps for why this is the case is that regulatory risks, which a number of theories suggest that a corporate socially responsible reputation is likely meant to address (Baron (2001), Benabou and Tirole (2010)), have been small in sample for a number of reasons. For instance, economists point to the unprecedented deregulation of the US economy since the 1970s (Peltzman, Levine, and Noll (1989), Alesina, Ardagna, Nicoletti, and Schiantarelli (2005)). Moreover, several high profile attempts by governments at setting a price for carbon emissions have failed. However, such regulatory risks might increase considerably in the future as household opinions regarding environmental and social issues shift. Therefore, it would be valuable to assess the extent to which firm ESG scores ameliorate such potentially important risks.

Rather than focusing on ESG effects for a large cross-section of firms where empirical challenges such as selection are large and where the direct effect of ESG is likely to be small, our approach is to focus on a narrow subset of firms where ESG effects (particularly

associated with corporate reputation) are likely to be large and where we might have a reasonable identification strategy.

To this end, we estimate the value of firm ESG for bargaining settlements of the Foreign Corrupt Practices Act (FCPA). The FCPA was passed in 1977 in response to the realization that bribery of foreign officials by US firms was prevalent and that such bribery was detrimental to the reputation of US firms overall.[1] The FCPA made it illegal for any US issuer, domestic concern, or other person to bribe a foreign official in order to influence his acts or decisions or those of his government or political party.

Bribery and FCPA penalties are a significant source of corporate tail risk. Even though the odds of being ensnared in an FCPA case are relatively small, the largest fines can be on the order of billions of dollars, a sizeable sum even for shareholders of large corporations. The number of cases prosecuted under the FCPA have grown rapidly in recent years, prompting academics in law and economics (Choi and Davis (2013)) to name the anti-bribery provisions of the FCPA as the most important rules in the regulation of US business abroad.

Firms that have been affected by FCPA cases are firms for which sustainability issues had a first-order effect on near-term profits, and the affected firms may provide some foreshadowing on the role of ESG considerations for many more firms going forward. We therefore focus on the effect of ESG ranking on the settlements exacted from these firms, which translate directly into shareholder returns.

We gather data on bargaining settlements for cases from 1989-2016. FCPA case data released during settlements include key information such as the amount of sanctions, revenues obtained from bribes, bribe size, and the number of years bribes were paid. For reasons of comparability and identification, we focus on firms headquartered in the US. We measure corporate social responsibility using the widely-used MSCI-KLD scores of ESG. In total, we

[1]The report to the House of Representatives that initially introduced the FCPA outlined the reasoning behind this legislation: http://www.justice.gov/criminal/fraud/fcpa/history/1977/houseprt-95-640.pdf. Bribery was thought to undermine the free market system championed by the US and harm foreign policy by lowering its credibility. Not only were these actions judged as harmful, but a survey of corporations cited in the report indicated that bribery was not deemed necessary by companies in a variety of industries and of various sizes.

have 131 cases in our sample. Firms ensnared in the FCPA are typically large exporters that come from a broad cross-section of industries. As such, our analysis is important on a value-weighted basis because it applies to the largest firms in the corporate landscape.

Moreover, virtually all cases are settled via bargaining between the parent company and prosecutors from the Department of Justice (and often the SEC as well) over the size of the sanctions (Reilly (2013)). According to US Justice department guidelines, the basis for these negotiations depends critically on the ill-gotten revenues from bribes by the company. Hence, a theoretically motivated baseline empirical specification comes from a Nash-Bargaining set-up (Binmore, Rubinstein, and Wolinsky (1986)) whereby firms and prosecutors bargain over sanctions proposed by guidelines that can be multiples of revenues that were obtained from bribery. Such a simple theoretically tight analysis would be more difficult for a large cross-section of firms.

We expect, for a number of reasons, high ESG parent firms to receive lower sanctions, all else equal. First, the well-known halo effect (Thorndike (1920), Efran (1974), Nisbett and Wilson (1977)), where juries extrapolate guilt or innocence based on a defendant's reputation in another domain, ought to lead to better trial outcomes for high ESG firms. Our premise is that such a halo effect which typically involve trials of individuals should nonetheless apply to trials of companies. Consistent with our premise, Cohen and Gurun (2018) find that companies facing upcoming jury trials strategically target advertising and ESG activities to the area where their jury pool is likely to reside. Because both parties understand the possibility of this halo effect, ESG firms should have more bargaining power (or a better threat point should negotiations fail) with prosecutors and receive lower sanctions as a result.

Second, the prosecutors themselves might also give high ESG firms the benefit of the doubt, which of course will make settling the case much easier and result in lower sanctions due to reduced investigation costs (Becker, 1974; Polinsky and Shavell, 1992; Arlen, 1994; Arlen and Kraakman, 1997). These two rationales are reinforcing because firms given the

benefit of the doubt will also be more cooperative and be on better behavior.

Given the functional form of the Nash-Bargaining solution, our simple model points to working with log sanctions and log bribery revenues. This specification also fortuitously addresses the distributions of sanctions and bribery profits being right skewed. We document a strong linear relationship between log sanctions and log bribery profits, with an OLS coefficient close to one, consistent with our framework. To avoid measurement error issues involving bribe revenues, we use the log of the ratio of sanctions to bribery revenue as our dependent variable of interest.

Consistent with our hypothesis, a one point increase in a firm's ESG score leads to a decrease in the log ratio of sanctions to bribery revenues that is nearly 27% of the standard deviation of the dependent variable. Another way to frame the economic effect is that a one point increase in ESG leads to a 30% or $6.6 million lower sanction, all else equal. We control for industry effects to adjust for any differences in ESG scores due to the nature of the industry; i.e. the oil industry will naturally have worst scores than media firms.

Of course, an important concern in interpreting this OLS estimate as causal involves omitted variables bias. This bias might come from classical measurement error or endogeneity, where a firm caught in a FCPA case might try to improve its ESG score as in Cohen and Gurun (2018). But our main concern is that the subsidiaries of high ESG firms, for whatever reason, might commit less egregious foreign bribes that are not completely captured by case data on profits.

To address these concerns, we instrument firm sustainability scores using a state-level measure of kilobytes of pages of laws or legal code developed by Mulligan and Shleifer (2005). The idea is that these legal codes are designed to promote more pro-social behavior of firms in that state. Since ESG scores are designed to quantify such pro-social behavior, we expect and find empirically that firms headquartered in states with more laws, conditional on state population, do have on average higher ESG scores. Due to fixed cost of creating laws, Mulligan and Shleifer (2005) point out that kilobytes of laws increase with population.

Because we do not want to make comparisons between states of different population, we use a residual variation in kilobytes of law pages controlling for state population.[2]

The original Mulligan and Shleifer (2005) dataset only covered 37 states. Some of the firms in our sample are headquartered elsewhere. So we expand their sample to cover all the states where we have firm headquarters. We obtain a strong first-stage: kilobytes of laws in the firm's state headquarters predicts the firm ESG z-score with a t-statistic of 5.14. Our instrumented results are 2.5 times larger than our OLS estimates and reduced-form estimates are also economically and statistically significant. A one point increase in ESG leads then to a $14.3 million or 65% lower sanctions, all else equal. The large increase caused by instrumenting likely reflects that ESG scores are noisy measures of actual corporate reputation for goodness, and hence biases down the OLS estimate. Instrumenting using state laws helps with this measurement error.

The exclusion restriction of this instrumental variables strategy is that the egregiousness of bribes by employees at foreign subsidiaries is uncorrelated with this state-level kilobyte measure. As we discuss below, there are a number of reasons for why our exclusion restriction is plausible a priori. For instance, the state code pages do not govern bribes done by foreign subsidiaries since they apply to all firms in a state, most of which do not have any exports or any foreign businesses to begin with. Consistent with our exclusion restriction, firm ESG and state legal code pages are not correlated with bribery revenues or the size of bribes in our sample. In fact, if anything the point estimates go the wrong way: i.e., firms located in states with higher kilobytes of laws have larger bribes and more revenues from these bribes.

Another worry with our exclusion restriction is that egregiousness of bribes might not be captured by bribery revenues per se but by other attributes of the case. We also gather data on whether the FCPA case was voluntarily brought by management to prosecutors and the prosecutorial attention paid to the case (as measured by the number of named prosecutors on the case). The underlying premise for this analysis is that more egregious cases are less

[2]Our first-stage regression is related to work showing using cross-country data that the type of legal system can affect ESG outcomes (Liang and Renneboog (2017)).

likely to be self-reported and likely to attract more prosecutorial attention. However, we find little significance between these other measures of egregiousness and a firm's ESG score or the kilobytes of law where the firm is headquartered. In other words, our narrow focus on the FCPA setting also allows us to address omitted variables concerns in a reasonable way that would be otherwise difficult in a general cross-sectional setting.

However, our state legal codes pages measure is not a generic instrument to identify the effects of ESG on firm revenues, as is the focus of the literature. One possible reason is if only more profitable firms can afford to locate or remain in states with more legal code pages. This would thereby violate the exclusion restriction. But we do not think that firms with a particular propensity to bribe would locate in certain states. This is the crux of why our narrower setting is valuable for identification purposes.

Of course, we are not the first paper to study the FCPA or related bribery laws in other countries. Earlier finance papers examined whether bribery is ex-post profitable for corporations net of the penalties (Karpoff, Lee, and Martin (2015), Zeume (2017)). Law and economics papers (see, e.g., Choi and Davis (2013)) have examined different determinants of sanctions using an optimal fines framework but without an instrumental estimation strategy. Our Nash-Bargaining set-up along with our identification strategy are novel.

Moreover, our evidence contributes to the literature on measuring the reputation effects of ESG. The best evidence on the reputational effects of ESG thus far has come from experiments. For instance, Elfenbein, Fisman, and McManus (2012) study eBay sellers to isolate a product signaling effect and Smith, Read, and Lopez-Rodriguez (2010) use student experiments to show that CSR might engender a halo effect for consumer products. But the extrapolative relevance of these experiments for large corporations has not been established, though product market signalling has been shown to be important in recent panel studies (Servaes and Tamayo (2013), Albuquerque, Koskinen, and Zhang (2018)). Otherwise, field and case studies such as Vanhamme and Grobben (2009), who study corporate crises, and Barrage, Chyn, and Hastings (2014), who focus on British Petroleum's oil spill, establish the

effectiveness of advertising in countering negative consumer perceptions.

Our evidence can be interpreted as ESG offering a firm tail risk protection against regulatory risks, similar to findings in Lins, Servaes, and Tamayo (2017) that high ESG firms did better during the Financial Crisis. Our findings also inform the debate on what institutional owners value — be it non-pecuniary (Hong and Kostovetsky (2012a), Hong and Kacperczyk (2009), Cheng, Hong, and Shue (2013), Hartzmark and Sussman (2018)) or pecuniary (Dyck, Lins, Roth, and Wagner (2019)).

2 Data and Summary Statistics

2.1 FCPA Case Data

The data on FCPA cases are taken from the website on FCPA enforcements maintained by Stanford Law School. To be in our sample, we require a firm to have FCPA case information on sanctions, revenues or bribes. We require the firm to be a publicly traded company in the Russell 3000 index so that we can obtain basic financial information about the company. In total, we have 131 cases in our sample as a result of these filtering rules.[3]

In Figure 1, we plot the the number of cases over time. There were quite few cases against corporations in the 1990s and early 2000s. FCPA enforcements ramped up considerably after 2007 during the Bush administration. This ramp up is generally attributed to a few reasons, including the passage of Sarbanes-Oxley in the early 2000s leading to more financial reporting and increased anti-corruption and cooperation efforts by other countries as more companies started doing business abroad.

Tables 1 and 2 summarize the types of industries and countries involved in these 131 cases. We use the Fama-French 17 industry portfolios to classify firms. The most commonly represented industries are business equipment and manufacturing; however, offenses do not

[3]There are a total 328 cases on the Stanford website over the time period studied, but a number of these are private firms, foreign corporations or small companies.

appear to be concentrated in any one industry. There is also a good deal of variation across countries, with the largest number of bribes taking place in China (36 cases) and Iraq (23 cases). In this table, we do not display all countries but just those with at least 8 FCPA violations. The total number of observations is greater than the 131 cases in our sample because each FCPA case may involve multiple countries.

The majority of the cases are settled through deferred prosecution or non-prosecution agreements (DPAs and NPAs); therefore, the sanction amounts include civil and criminal penalties, disgorgement of profits (including pre-judgement interest), as well as any fines paid. The characteristics of these cases are summarized in Table 3. The mean sanction is $21.98 million and the median is $6.57 million. The mean raw revenue generated from bribe is calculated to be $16.8 million with a median of $8.95 million. The mean bribe is $34.63 million. The median payment is $1.25 million. We only have raw bribe revenue data for 83 of the 131 cases. We have the bribe amount for 117 of the 131 cases.

Because bribe revenue is a key variable in the negotiation process, we will infer the bribe revenue for cases where it is missing via a regression model. In Figure 2, we plot the log bribe revenue against log bribe amount for cases where both pieces of information are available. The regression line that fits the data has a coefficient of .72 with a t-statistic of 10.81. The constant is 1.38 with a t-statistic of 10.00. We can then use this regression model to predict bribe revenue for cases where we have missing data. The expanded data is labeled bribery revenue, which has a mean of $26.26 million.

The ratio of sanctions to bribe revenue has a mean of 1.25 with a standard deviation of 1.14. The number of years of bribery (i.e. how long the bribes went on) has a mean of 5.7 years and a median of 5 years.[4] In our sample, 62% of the cases are voluntarily disclosed by the parent firm, and in 15% of the cases there is no prosecutor identified. We will use these additional pieces of case information in our discussion of the exclusion restriction below.

[4]There is one FCPA case in our sample where the years of bribery is not described.

2.2 Measure of ESG

Our annual firm ESG scores come from Kinder, Lydenberg and Domini (KLD) Research & Analytics, Inc. These scores were first collected in 1991 for 488 firms and coverage grew over the years to include 2,894 firms in 2009. After 2009, the calculations of ESG scores changed. Therefore we use the ESG score in the year of the FCPA settlement to measure firm goodness if the FCPA settlement was before 2009. If the settlement date is in 2009 or later, we use the ESG score from 2009.

KLD scans public databases, such as those on employee strikes and Environmental Protection Agency (EPA) violations, and uses a team of analysts to measure these and other social responsibility dimensions of firm production. Firms are graded on roughly 60 indicators. Each indicator represents a strength or a concern in one of six major areas: community, corporate governance, diversity, employee relations, environment, and product. The total strengths, net of the total concerns, are summed together to calculate a single ESG score.[5]

According to KLD guidelines, a one point increase in ESG requires a firm to change one corporate social responsibility indicator from a concern to neutral or from neutral to a strength. For example, a company would need to "consistently [give] over 1.5% of trailing three-year net earnings before taxes (NEBT) to charity" to get a strength. Many of the indicators such as having a funded retirement plan involve resources. Another such indicator score is on firm philanthropy. A company would have to donate around a few percent of its capital expenditures each year to rank highly when it comes to philanthropic giving. Among the 60 indicators, there are some less costly than retirement plan funding or philanthropic giving, but presumably every firm can score well on the less costly indicators. Hence the dispersion of scores we are picking up reflect the more costly measures, which can be easily in the millions of dollars.[6]

[5]There is also a subcategory for human rights, which we exclude because it went through a major overhaul in 2002 and is therefore not consistent throughout our sample period.

[6]ESG scores have been shown to influence mutual fund managers' portfolios and in particular the portfolios of mutual funds marketed as being socially responsible (Hong and Kostovetsky (2012b)). Socially responsible funds typically own stocks with the highest ESG scores within an industry.

In Table 4, we list the firms with the highest and lowest ESG scores in the FCPA sample. Many of the companies on this list are well-known to consumers. There is a positive correlation between ESG score and the list of top brands compiled by publications such as Businessweek and Forbes. The correlation is roughly 0.36, suggesting that ESG captures the types of firm characteristics that influence consumer and investor sentiment about the firm, much in the way that is suggested by Cohen and Gurun (2018).

In Table 3, the mean and median ESG score are both around -1. In contrast, the average ESG score across all firms surveyed in similar years is 0.1 and the median is 0. Notice that the ESG scores of firms in the FCPA sample are slightly lower than those of other firms. This suggests that higher ESG firms are less likely to be prosecuted under the FCPA. This could be due to a number of different factors, one of which is an ESG effect in the selection of firms to prosecute. Therefore, it is crucial we have an instrumental variables strategy to address potential selection issues.

In our analysis, we will work with ESG z-score, where the z-score is calculated within industry/year cells, using the entire KLD data set. We do this because ESG scores differ across industries: industries such as manufacturing or chemicals will have lower scores than industries such as media or finance. We do not want to identify the effect of ESG using these industry differences; therefore, we will always look for ESG effects within industry. The ESG z-score has a mean of -0.48. There is a large standard deviation for the ESG z-score of 1.49. That is, within industry-year, we can have significant variation in ESG among our subset of prosecuted firms.

The average firm involved in one of these FCPA cases has a market capitalization (Market Cap) of $25.6 billion, with a median of $4.5 billion. These are large firms, consistent with the fact that large multinational firms have more opportunities to engage in foreign bribery. We also report the distribution for return on assets (ROA) for these firms. The mean ROA is three percent with a standard deviation of eight percent.

2.3 Kilobytes of Legal Codes by State

Our measure of the amount of legal code in a state, the instrument for firm ESG scores, comes from Mulligan and Shleifer (2005). They measure the size (in kilobytes) of a state's legal code in the early 2000s. They download from the internet the legal code of every state. They then format the files of each state so that they are comparable. They then measure the size of the files of each state. Thirteen states did not have their legal codes in a format that they could analyze, so their data set contains information on only 37 states.

We have 19 FCPA cases in our data set where the firm is headquartered in a state not covered by their data. These cases involve seven states: Colorado, Georgia, Ohio, Oklahoma, Maryland, Virginia and Wisconsin. We can extend the Mulligan and Shleifer (2005) data to include these states using Justia.com, which keeps histories of the websites of state legal codes online. Most of these remaining states have their legal codes up on this site starting in the mid 2000s.[7] Using the legal codes from the year nearest to the early 2000s, we follow the crawling protocol of Mulligan and Shleifer (2005) to gather the kilobytes data for these additional seven states. One caveat is that legal codes can change from year to year. Since the codes for these seven states were from later years, this might create measurement error issues. So we also consider a robustness analysis using just the original Mulligan and Shleifer (2005) sample of states.

Our measure of state population comes from the 2000 Census, and is available in the Mulligan and Shleifer (2005) data set.[8] They also provide other measures of state characteristics such as the mean earnings of workers by state in 2000 and the salary of judges in each state in 2002.

In Table 3, the state where the typical firm in our sample is headquartered has 75,087 kilobytes of laws with a standard deviation of 28,963 kilobytes. The mean population of

[7]The two exceptions are Wisconsin and Colorado which have their legal code on this site for 2010 and 2016, respectively.

[8]Data can be found here: https://scholar.harvard.edu/shleifer/publications/extent-Market-And-Supply-Regulation.

the state where the typical firm is headquartered is 15.06 million people with a standard deviation of 9.8 million people.

3 Bargaining for FCPA Settlements and Regression Specifications

3.1 Negotiating Guidelines

Virtually all cases are settled via bargaining between the parent company and prosecutors from the Department of Justice and/or the SEC over the size of the sanctions. The details of the bargaining process are described in Reilly (2013). Rough sentencing guidelines, the basis of which frame the negotiations, are detailed in *A Resource Guide to the U.S. Foreign Corrupt Practices Act*, published in the Criminal Division of the U.S. Department of Justice and the Enforcement Division of the U.S. Securities and Exchange Commission. Sentencing guidelines suggest that the fine sought by prosecutors should scale with and be multiple times the actual bribe revenue. However, there is substantial leeway for bargaining between the firm and prosecutors as to the actual imposed fine or sanction.

The prosecutor's opinion is particularly influential for the enforcement of the FCPA. This is because most cases are decided by the prosecutor rather than a judge. The prevalent use of DPAs and NPAs in the criminal charges handled by the Department of Justice means that charges are not actually filed against many companies. In the cases when companies are actually charged, they are likely to be resolved through a plea agreement. The civil cases handled by the Securities and Exchange Commission follow a similar theme, with most resolved through a settled civil complaint. These policies give prosecutors a good deal of discretion in setting sanction amounts.

3.2 Regression Specifications

Following a standard bargaining setup, we assume both parties are risk neutral. If S is the sanction agreed on by the parties, then the payoff to the government is S and the payoff to the firm is $-S$. Let Z^* be the amount set by sentencing guidelines. We assume that if there is no agreement then the government would collect, net of legal costs, an expected payment of $\mu^p Z^*$, where $\mu^p > 0$, and the firm would pay an expected fine plus legal costs of $Z^* - \mu^f Z^*$ with $1 > \mu^f > 0$. That is, in disagreement, the firm would have to pay some sanction but it would be less than Z^*. We assume that $Z^* = kZ$ where k is some multiple that can be greater than one since sentencing guidelines suggest not only clawing back ill-gotten gains but also imposing additional punishments.[9] The presence of legal costs means that $\mu^p Z^* < Z^* - \mu^f Z^*$ or $\mu^p + \mu^f < 1$.

The Nash bargaining solution is the sanction S that maximizes the Nash product, i.e. it solves

$$\text{Max}_S(S - \mu^p Z^*)(Z^* - \mu^f Z^* - S). \tag{1}$$

The first order condition is given by

$$Z^* - 2S + \mu^p Z^* - \mu^f Z^* = 0. \tag{2}$$

The Nash bargaining solution is then

$$S = \frac{1 + \mu^p - \mu^f}{2} Z^*. \tag{3}$$

Let $\lambda \equiv \frac{1+\mu^p-\mu^f}{2} > 0$, since $\mu^f < 1$. Thus the fraction of sanction to bribe revenues is

$$\frac{S}{Z} = \lambda k, \tag{4}$$

[9]Indeed, as shown in Table 3, the mean of the ratio of sanctions to bribe revenues is 1.25, but there is a large standard deviation of 1.14 which points to the potential importance of bargaining and underlying firm characteristics. We assuming k is common across all firms but we could also allow k to depend on particular case conditions and our estimation strategies would be similar.

where recall again that Z^* sentencing guideline is some multiple k of actual bribe revenue Z. λ captures the differences in disagreement payoffs of the two counter-parties $\mu^p - \mu^f$. This difference should depend on firm characteristics such as firm ESG score for the reasons articulated above, such as better outcomes due to cooperation with prosecutors or halo effects with juries.

This Nash-bargaining solution gives us a natural baseline empirical specification for modeling data for a set of N cases involving sanctions S_i and Z_i. We assume that $\lambda_i k$ has the following functional form

$$\lambda_i k = \exp(\alpha + \beta_0 ESG\ z\text{-}score_i + \beta_1 X_i + \epsilon_i), \tag{5}$$

where determinants of the sanction outcome depend on the firm's ESG score, other firm characteristics X_i such as the industry of the firm or other details of the bribe and and an unobservable term ϵ_i.

Then a natural empirical specification for our dependent variable of interest (the log of sanctions over bribe revenues) is given by:

$$\log(Sanctions/Bribe\ Revenues) = \log\lambda_i k = \alpha + \beta_0 ESG\ z\text{-}score_i + \beta_1 X_i + \epsilon_i. \tag{6}$$

where the parameter of interest is β_0, which we expect to be negative for a number of reasons as we alluded to in the Introduction.

3.3 Specification Check

The model implies that the relationship between $log(Sanctions)$ and $log(Bribe\ Revenues)$ is linear with a coefficient of one. We show this relationship in Figure 3, where we plot on the y-axis $log(Sanctions)$ and on the x-axis $log(Bribe\ Revenues)$. There is a strong pronounced linear relationship with a coefficient of around .95 and t-statistic of 12.82. Therefore, we work with as our dependent variable of interest $log(Sanctions/Bribe\ Revenues)$. We discuss

alternative specifications where we put $log(Bribe\ Revenues)$ on the right hand side below and to estimate it simultaneously along with β_0. As we demonstrate below the correlation between firm ESG scores and revenues from bribes is close to zero and is actually slightly negative. So our results will not change much if we use this alternative. But we prefer the specification implied by the model because it mitigates some the usual concerns with measurement error regarding profits.

4 Empirical Findings

4.1 OLS Results

We show in Table 5 the relationship between our dependent variable of interest $log(Sanctions/Bribe\ Revenues)$ and our independent variable of interest firm *ESG z-score*. These OLS regressions include year and industry (Fama-French 17) effects. We control for industry effects to adjust for any differences in ESG scores due to the nature of the industry; i.e. the oil industry will naturally have worst scores than media firms. In column (1), the coefficient of interest is -.25 with a t-statistic of 1.99. A one standard deviation increase in *ESG z-score* (1.49) decreases $log(Sanction/Bribe\ Revenues)$ by .37, which is around 27% of a standard deviation of the dependent variable of interest. In Figure 4, we show the scatter-plot of this relationship; it indicates that this pronounced negative relationship is not driven by by outliers.

In column (2), we estimate the alternative specification where we have as the dependent variable $log(Sanctions)$ and on the right-hand side the *ESG z-score* and $log(Bribe\ Revenues)$. The coefficient on our independent variable of interest is -.30 with a statistic -2.48. The coefficient on $log(Bribe\ Revenues)$ is .83 with a t-statistic of 11.46, which is similar to the results in Figure 3. Another way to frame the economic effect is that a one point increase in *ESG z-score* lowers sanctions by about 30%. The mean sanction is $22 million. So 30% of this mean is around $6.6 million lower sanctions, all else equal.

We can also include other bribe characteristics such as length of the bribe and the OLS results remain similar. We address this issue below after we have discussed our instrumental variables strategy and exclusion restriction.

4.2 IV Results

The main omitted variables concern for interpreting the OLS estimates as causal is that the subsidiaries of high ESG firms, for whatever reason, might commit less egregious foreign bribes that are not completely captured by case data on profits. This would create a negative correlation between firm ESG scores and sanctions that is not causal.

We address this potential issue with an instrumental variables strategy. We instrument firm sustainability scores using a state-level measure of legal oversight developed by Mulligan and Shleifer (2005). The argument is that more legal oversight and regulation is designed to promote pro-social behavior of firms in that state, providing potentially exogenous variation in firm ESG scores that we can use to identify our model.

We implement our identification strategy by using Mulligan and Shleifer (2005)'s measure of state legal code pages, kilobytes of laws in a state measured from 2001 to 2003. We use their original data with information on 37 states and add the extra set of states we need as described above. Mulligan and Shleifer (2005) show that there is a strong positive relationship between state legal code using their measure and state population. State population might affect the behavior of firms for reasons other than the extent of legal code. For example, more populous states are more urban. Firms in urban versus rural areas might behave differently. Therefore, we measure the legal environment of a state using their measure conditional on state population. That is, a state has more legal codes and hence regulations if it has more kilobytes of state laws than other states of similar populations.

Hence, our first stage is a regression of *ESG z-scores* on the $log(Kilobytes\ of\ Laws)$, $log(State\ Population)$ and year and industry effects. Table 6 show the estimates of this first stage. The coefficient on $log(kilobytes\ of\ laws)$ is 1.25 with a statistic is 5.14, so we are not

concerned about weak instrument issues. Figure 5 plots that strong relationship between firm ESG scores and kilobytes of state laws.

The two stage least squares results using this instrument are presented in Table 7. In the column (1) with $log(Sanctions/Bribe\ Revenues)$ as the dependent variable, the coefficient on *ESG z-score* is -0.65 with a t-statistic of 3.37. Notice that this coefficient is about 2.5 time larger than the OLS estimate found in Table 5. State population is in the second-stage regression, but does not have any explanatory power for sanctions to profits. A one point increase in *ESG z-score* leads to on average a $14.3 million or 65% lower sanctions, all else equal. The large increase likely reflects that KLD scores are noisy measures of actual corporate reputation for goodness, and hence biases down the OLS estimate; instrumenting using state laws helps with this measurement error.

In column (2), where the dependent variable $log(Sanctions)$, our coefficient of interest is -0.44 with a t-statistic of 4.33. This coefficient is about 50% larger than the analogous OLS estimate. The coefficient on $log(profit)$ is .81, very similar as what we found in the OLS specification, and state population again has no explanatory power for sanctions. So overall, the IV results lead us to conclude that there is a causal effect from firm ESG to lower sanctions or better bargaining outcomes with prosecutors.

4.3 Reduced-form Estimates

In column (1) of Table 8, we present reduced-form estimates of how $log(Sanctions/Bribe\ Revenues)$ varies with $log(Kilobytes\ of\ laws)$. The coefficient on kilobytes of state laws is -.82 with a t-statistic of 2.24. In column (2), we estimate the reduced form for the alternative specification by controlling for $log(Bribe\ Revenues)$ on the right hand side. The coefficient on kilobytes of state laws is -0.68 with a t-statistic of -1.88. So the reduced-form and the 2SLS results point to the same conclusion that firms with higher ESG receive lower sanctions relative to the bribe revenues.

4.4 Examining the Exclusion Restriction of the IV Strategy

The exclusion restriction of the IV strategy is that the unobservable egregiousness of bribes by employees at foreign subsidiaries is uncorrelated with the state legal code pages measure. There are a number of reasons to believe this is a plausible exclusion restriction. States have commercial anti-bribery laws such as illegality of kickbacks, but these state-level laws or regulations are unlikely to determine the behavior of firms abroad. Most firms in a state do not have any exports or have any foreign businesses to begin with. This is the hole in laws that the FCPA was meant to address. In other words, it is not likely that the kilobyte law measure is picking up that certain states have harsher foreign bribery statues than other states.

And unlike other types of corporate infractions such as accounting fraud, which almost always involve the CEO, CFO or other upper management, bribes usually do not involve top firm executives. Instead, the fraud is often committed by foreign employees abroad that are removed from firm headquarters. However, a number of bribes are then detected by management at headquarters during audits, while others are detected through whistleblowers who report the bribes to management or prosecutors.

Consistent with this exclusion restriction, we show in Table 9 that state legal code pages and firm bribe characteristics are not correlated. In column (1), we regress the log of bribe revenues on log of kilobytes fo state laws. The coefficient is 1.40 but is statistically insignificant. The worry of the plausibility of the exclusion restriction was that firms headquartered in states with more laws would do less egregious bribes. According to this bribe revenue metric, the opposite if anything seems to be the case. In column (2), we use the raw bribe revenues instead of our expanded bribe revenue measure and the coefficient is 1.41 but again not statistically significant.

In column (3), we use the size of the bribe as the dependent variable. We find that firms headquartered in states with more state laws have a positive correlation to bribe size but again it is statistically insignificant. In column (4), we consider years of bribes. Here we find

no relationship at all.

Another way of examining the validity of the exclusion restriction of the IV strategy is to run a balance test. For our sample, we estimate a regression of the *log*(*Kilobytes of laws*) on *log*(*2000 State Population*) and year effects. We then recover the residual of that regression. We create a dummy variable that indicates whether the value of the residual is positive or not. For the balance test, we examine whether other firm characteristics are different for positive residual firms versus negative residual firms.

The results of this balance test are presented in Table 10. As in Table 9, we see that there is little difference in bribe revenues and the years in bribes for high and low legal code page state firms. For other firm financial characteristics such as market capitalization and ROA, we similarly see little difference between the two types of firms.

We do find, perhaps unsurprisingly, substantial differences in state-level characteristics of high and low legal code page firms. States with high residual legal code pages have higher median earnings. They are substantially less likely to be located in the South, and they pay their state judges higher salaries. The concern would be if these differences in state characteristics say something about the unobservable egregiousness of bribes by firms in those states. One way to see if this is an issue is to include these state characteristic controls in the IV specification and see if their inclusion substantially changes the results.[10]

We show what happens when these characteristics are included in the reduced form and IV specifications in Table 11. The specifications are identical to the base cases, except that the three state characteristic variables from the balance test are included. Column (1) shows the results of adding these extra controls to the reduced form. The coefficient on *log*(*Kilobytes of Laws*) is even larger in absolute value than the estimate of the specification not including these extra controls. Column (2) shows the equivalent IV results. Again, the coefficient of interest, on *ESG z-score*, is higher in absolute value, although in this case

[10]There are some differences in industry classification, especially in the energy sector. A large number of energy firms are located in Texas, a low legal code pages state. This is one of the motivations for the industry effect controls in our base specifications.

somewhat less precise.

4.5 Other Case Characteristics

We have framed our discussion of the exclusion restriction by focusing on revenues derived from bribes as the sole measure of bribe egregiousness. But in reality, egregiousness might not be one dimensional. For instance, some cases might be more complex, which might lead to higher sanctions as a result. Alternatively, some cases might be more publicized and have more prosecutors involved, which might also lead to higher sanctions.

To the extent our instrument state regulatory pages is correlated with this complexity, our exclusion restriction might be invalidated. To get at this issue, we use case information on the number of prosecutors assigned to the case and whether the case was voluntarily disclosed. The idea is that more complicated and egregious cases draw more attention and are less likely to be voluntarily disclosed.

The enforcement actions of some cases have names of prosecutors listed, including well-known US District Attorneys while other cases have no named prosecutors. We view this measure as capturing information regarding the complexity or publicity attached to the case. In Table 12, we examine how *ESG z-score* influences the propensity for a case to have no named prosecutors. In column (1), we estimate a probit model with *ESG z-score* as the key variable of interest. We see that there is no relationship. In column (2), we estimate the reduced-form probit replacing firm *ESG z-score* with the legal code pages in the firm's headquarter state. Again, we find that that there is no relationship. We view these results as generally supportive of our conclusions on the plausibility of the exclusion restriction in Table 9.

In columns (3) and (4) of Table 12, we examine how *ESG z-score* affects the propensity for firms to voluntarily disclose their crimes to authorities. The probit estimate in column (1) is insignificant. In column (2), we implement the reduced-form probit using state regulatory pages in the firm's headquarter instead of firm *ESG z-score*. We see again that there is no

relationship. So we conclude that our findings are not connected to or simply a result of high ESG firms being more likely to voluntarily disclose.

4.6 Subsample of States with Measure of Early 2000s Legal Code Pages

Finally, we examine what happens when we limit our sample to the 112 FCPA cases that have firms that are headquartered in the states covered by the original Mulligan and Shleifer (2005) data set. The results are presented in Table 13. In general the results are very similar to our main results. In column (1), the simple OLS coefficient is -.29 with a t-statistic of 2.15. This is similar to the -.25 coefficient using the full sample. In column (2), we present the first-stage regression. The coefficient of regressing ESG on state law pages is 1.97 with a t-statistic of 4.81. This is also similar to the first-stage using the full sample. In column (3), the coefficient on the reduced form specification is -.75 with a t-statistic of 1.63. The coefficient is slightly smaller than that obtained in the full sample and the t-statistic in this case is only marginally significant. The same is true for the IV estimate in column (4). That the reduced form and IV estimates are less precise than the full sample is probably not surprising, given that the sample is about 15% smaller.

5 Conclusion

Corporate social responsibility is becoming an increasingly important part of corporate strategy. As a result, it is valuable to quantify the benefits of ESG to companies. In contrast to traditional approaches looking at an entire cross-section of firms where sustainability risks might be low on average, we focus on a subset of firms where ESG is likely to play a major role in outcomes — that is, those firms caught in the Foreign Corrupt Practices Act and negotiating with prosecutors on their settlement.

We compare the punishment of crimes by more and less socially responsible corporations.

To account for ESG firm bribes being unobservably less egregious, we instrument ESG with law pages in the firm headquarter's state. We find economically significant estimates that point to the value of ESG. Our instrumented estimates point to ESG firms receiving $14.3 million or 65% lower sanctions, all else equal.

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Figure 1: FCPA Actions by Year

Note: The number of FCPA cases of firms with KLD and sanction/bribe information headquartered in the U.S. each year are displayed. There are 131 total cases.

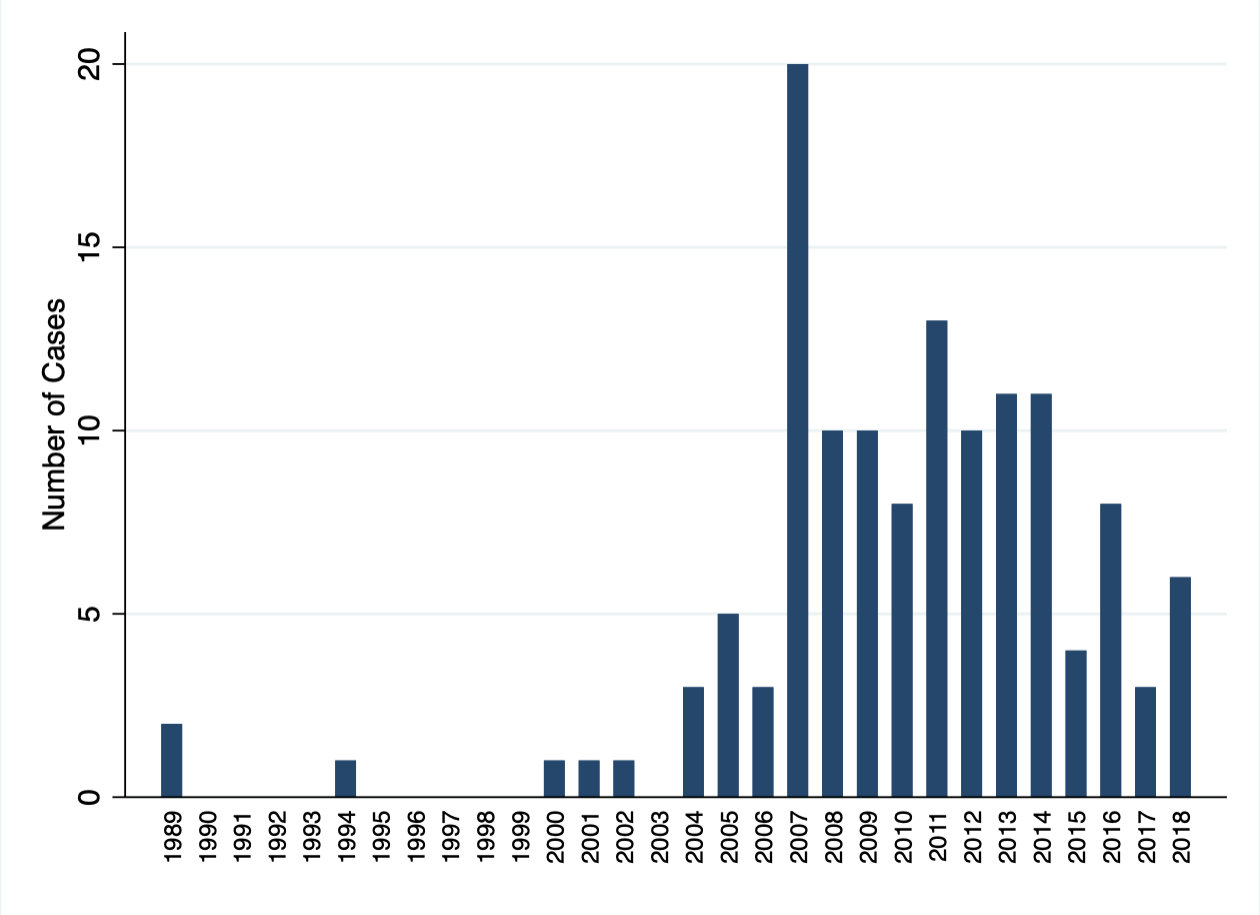

Figure 2: The Relationship Between Size of Bribe and Revenues of Bribe

Note: For each FCPA case where information is available (69 cases), the size of the bribe (in logs) is plotted against the amount of the raw revenues from the bribe (also in logs). The regression line that fits the data has a coefficient of .72 with a t-statistic of 10.81. The constant is 1.38 with a t-statistic of 10.00.

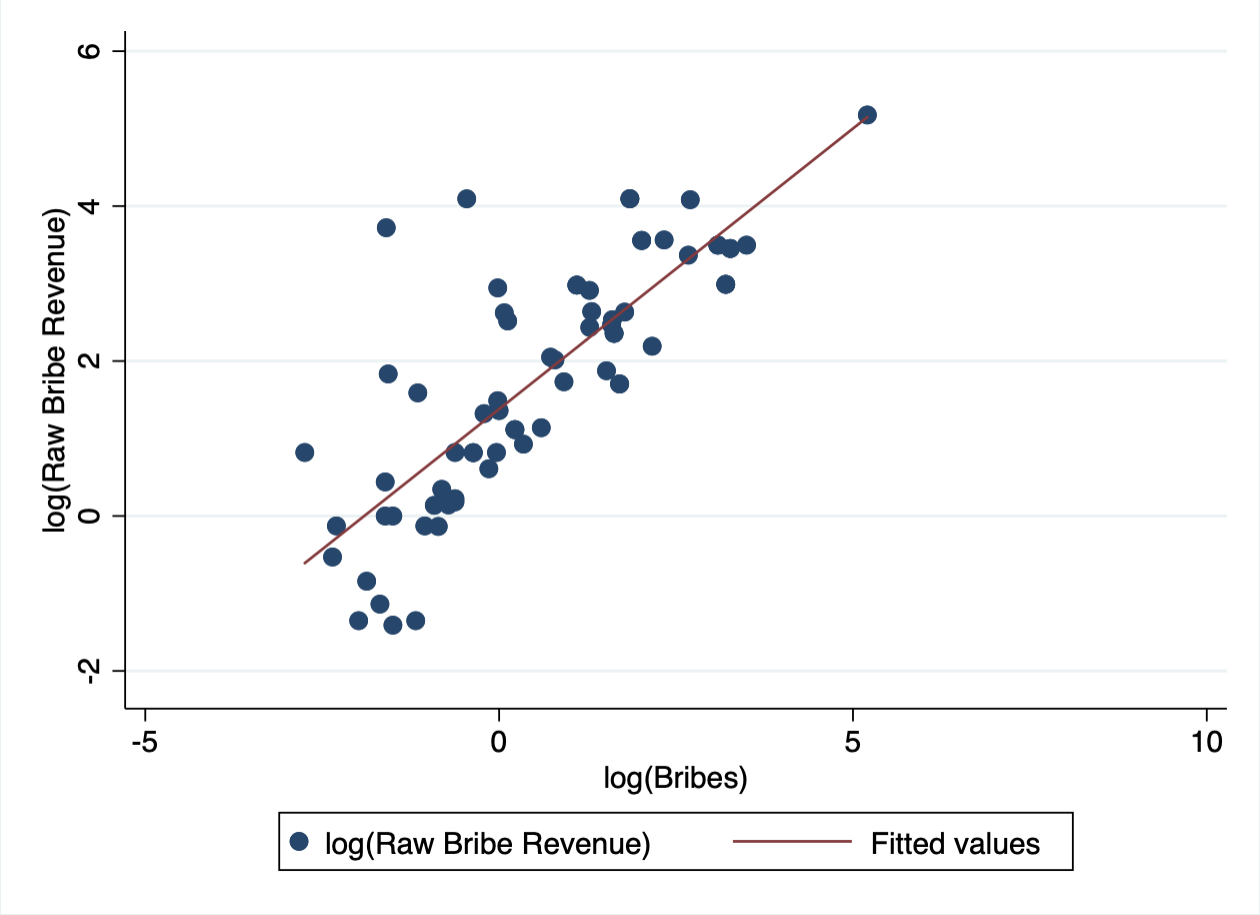

Figure 3: The Relationship Between Sanctions and Bribe Revenues for High and Low ESG Firms

Note: Scatter plot of log(Sanctions) and log(Bribe Revenues) (residualized with year effects and industry effects). Only firms in the top and bottom 25% of the ESG score distribution (and residualized with year effects and industry effects) are included. The OLS line is included, using all observations. The OLS coefficient is 0.95 with a t-statistic of 12.82. 64% of the low ESG score firms have positive residuals. 66% of high ESG score firms have negative residuals.

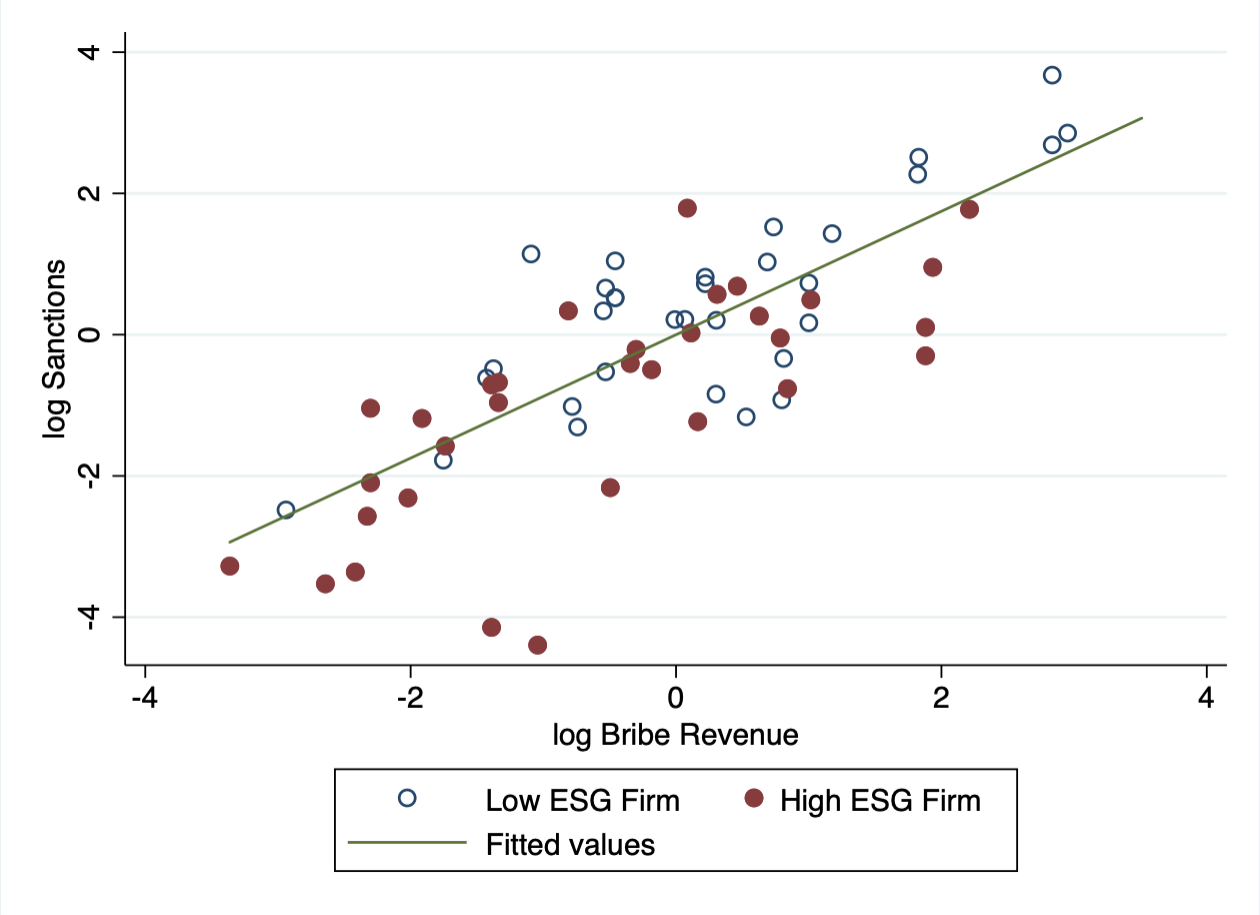

Figure 4: The Relationship Between Sanctions and Firm ESG Score

Note: Scatter plot of log(Sanctions/Bribe Revenue) and firm ESG z-score (residualized with year effects and industry effects).

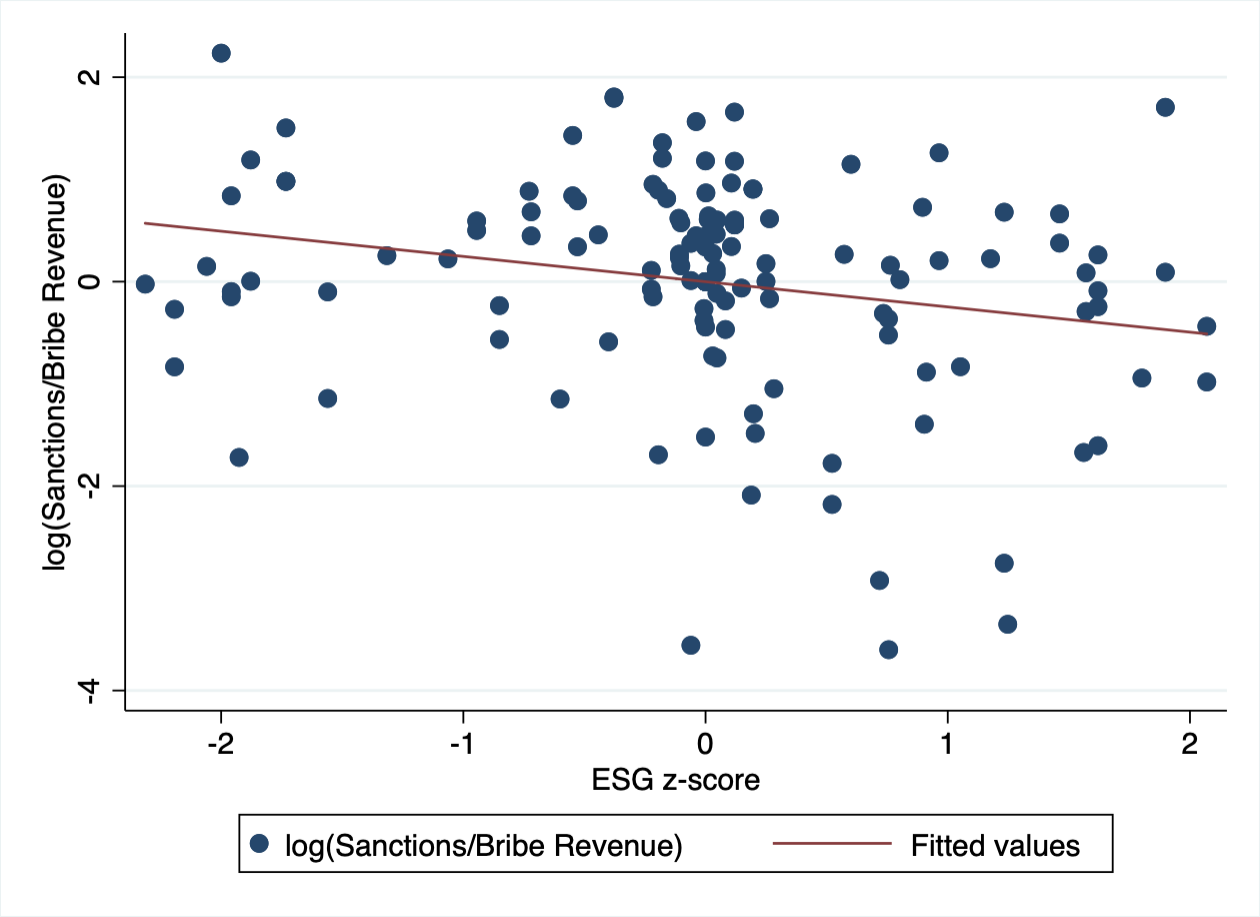

Figure 5: The Relationship Between the Size of State Legal Codes and Firm ESG Scores

Note: Scatter plot of log(Kilobytes of Laws) and firm ESG z-scores (residualized with year effects and industry effects).

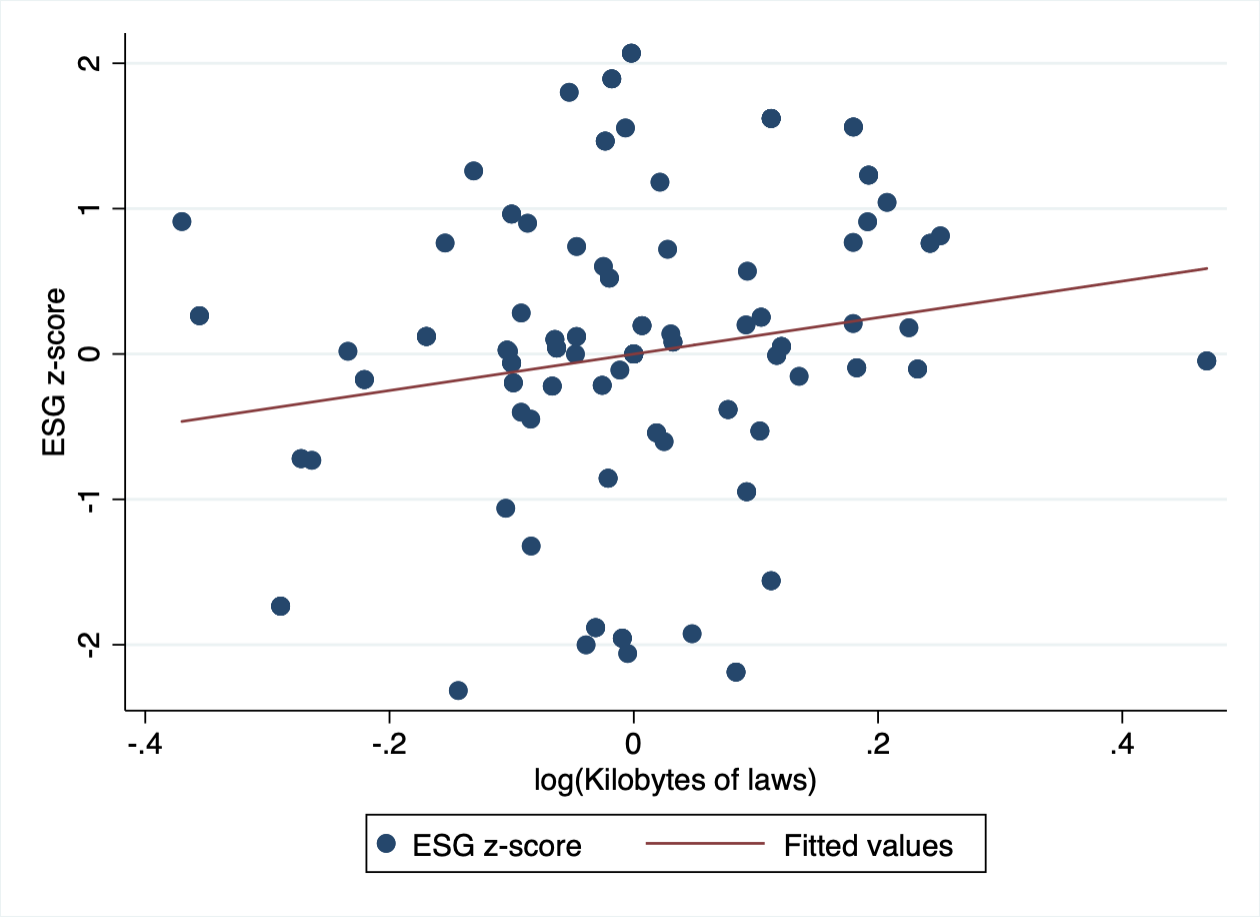

Figure 6: The Relationship Between the Size of State Legal Codes and FCPA Sanctions

Note: Scatter plot of log(Kilobytes of Laws) and firm log(Sanctions/Bribe Revenue) (residualized with year effects and industry effects).

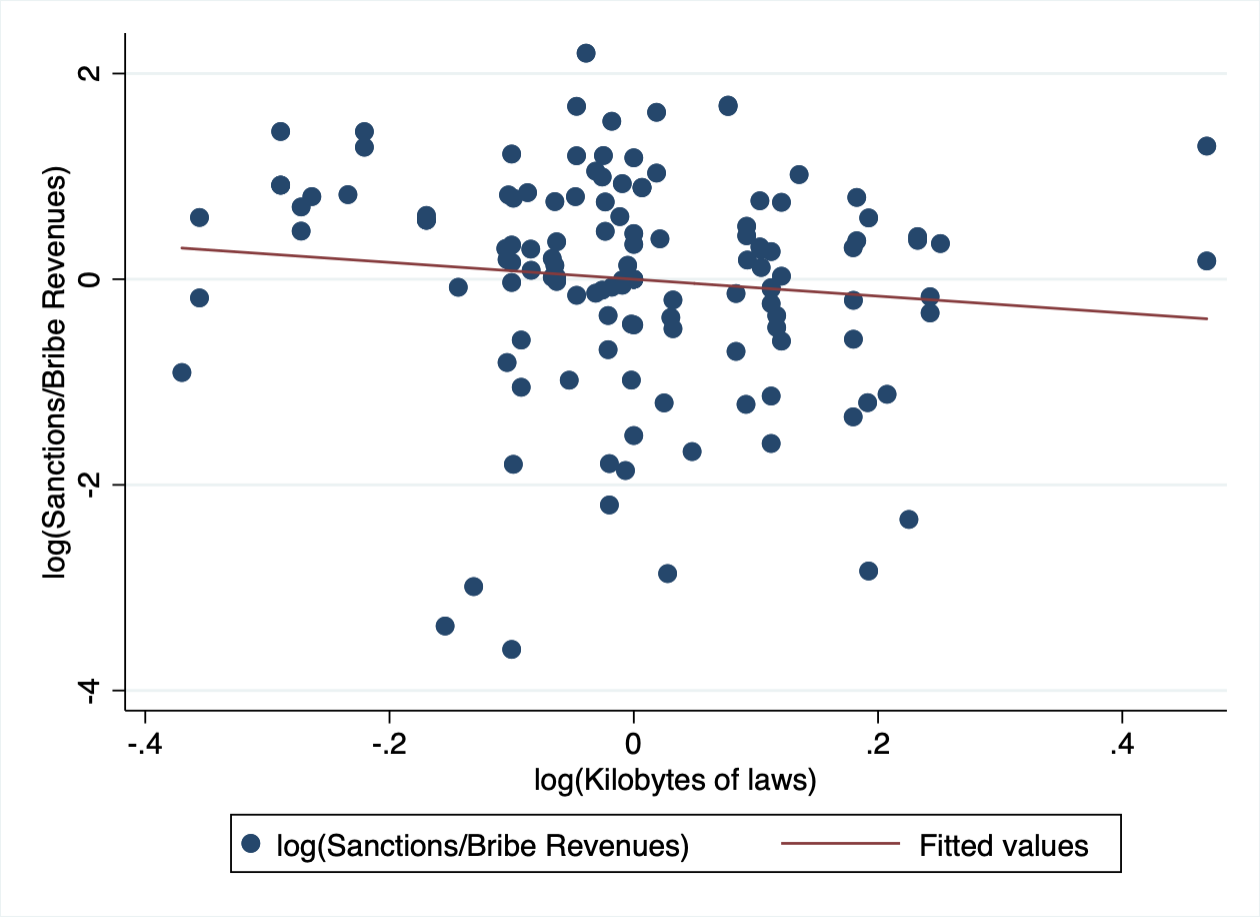

Table 1: Industry Distribution of FCPA Cases

Note: The number of FCPA cases from each industry are displayed. The industry classification is the Fama-French 17.

	Number of Cases
Food	14
Oil and Petroleum Products	17
Chemicals	3
Drugs, Soap, Perfumes and Tobacco	10
Construction	3
Machinery and Business Equipment	26
Transportation	10
Utilities	2
Finance	7
Other	38

Table 2: Countries Most Involved in FCPA Cases

Note: The countries with the most FCPA cases are listed.

	Number of Cases
China	36
Iraq	23
Indonesia	13
Russia	11
Nigeria	11
India	10
Mexico	10
Thailand	9
Egypt	9
Vietnam	8
Poland	8

Table 3: Summary Statistics

Note: Summary statistics are shown for the 131 FCPA cases that match to KLD data. Sanctions, bribe revenue, bribes and market capitalization are measured in millions of dollars. Raw Bribe Revenue is the bribe revenue for the cases with actual bribe revenue information. Bribe Revenue adds the predictions of bribe revenue for cases with missing information. State population is measured in millions of people. Mean earnings and judge pay are in dollars. Standard deviations are in brackets.

	Mean	10th Percentile	Median	90th Percentile	Observations
Sanctions	21.98	.33	6.57	48.67	131
	[48.73]				
Raw Bribe Revenue	16.80	.88	8.95	35.29	83
	[24.11]				
Bribes	34.63	.15	1.25	24.61	117
	[250.61]				
Bribe Revenue	26.27	.87	7.50	38.23	131
	[94.93]				
Sanctions/Bribe Revenue	1.25	.12	1.15	2.09	131
	[1.14]				
Years of Bribes	5.7	2	5	10	130
	[3.3]				
Voluntary Disclosure	.62				131
Prosecutor Not Identified	.15				131
ESG score	-.98	-4	-1	1	131
	[2.79]				
ESG z-score	-.48	-2.11	-.46	.73	131
	[1.49]				
Market Capitalization	25672	470	4544	87644	131
	[52407]				
ROA	.03	-.07	.04	.13	131
	[.08]				
Kilobytes of State Laws	75087	42927	73910	132862	131
	[28963]				
State Population	15.06	3.42	12.42	33.87	131
	[9.80]				
Mean Earnings by State	23480	20716	22828	26595	131
	[2720]				
Average Judge Pay by State	117294	101700	116000	136700	131
	[15184]				

Table 4: High and Low ESG Firms

Note: The FCPA firms with the highest and lowest KLD scores are displayed.

High ESG Firms		Low ESG Firms	
IBM	8	Goodyear	-9
Hewlett-Packard	6	Tyson Foods	-7
Avon Products	6	El Paso Corporation	-7
Johnson & Johnson	5	Halliburton	-5
Nature's Sunshine Products	4	United Technologies	-5

Table 5: OLS Relationship Between Firm ESG Score and Sanctions

Note: OLS estimates of the effect of firm ESG scores on FCPA sanctions are shown. In column 1, the dependent variable is log(Sanctions/Bribe Revenue). In column 2, the dependent variable is log(Sanctions). t-statistics based on standard errors clustered by industry and year are shown in parentheses.

	log(Sanctions/Bribe Revenue)	log(Sanctions)
	(1)	(2)
ESG z-score	-.25	-.30
	(-1.99)	(-2.48)
log(Bribe Revenue)		.83
		(11.46)
Year Effects	Yes	Yes
Industry Effects	Yes	Yes
Observations	131	131

Table 6: First Stage Relationship Between the Size of State Law Codes and Firm ESG Scores

Note: OLS estimates of the effect of size (measured in kilobytes) of a state's legal code on the ESG z-score of a firm in that state. The dependent variable is the ESG z-score of the firm in the FCPA sample. t-statistics based on standard errors clustered by industry and year are shown in parentheses.

log(Kilobytes of Laws)	1.25
	(5.14)
log(State Population)	-.52
	(-0.94)
Year Effects	Yes
Industry Effects	Yes
Observations	131

Table 7: 2SLS Estimates of the Relationship Between Firm ESG Scores and Sanctions

Note: Two stage least squares estimates of the effect of a firm's ESG Score on FCPA sanctions, using the size of the state's legal code as an instrument. The dependent variable in column (1) is log(Sanctions/Bribe Revenues). In column (2), the dependent variable is log(Sanctions). t-statistics based on standard errors clustered by industry and year are shown in parentheses.

	log(Sanctions/Bribe Revenue)	log(Sanctions)
	(1)	(2)
ESG z-score	-.65	-.44
	(-3.37)	(-4.33)
log(Bribe Revenue)		.81
		(13.99)
log(State Population)	-.22	-.16
	(-1.06)	(-0.72)
Year Effects	Yes	Yes
Industry Effects	Yes	Yes
Observations	131	131

Table 8: Reduced Form Estimates of the Relationship Between the Size of State Legal Codes and Sanctions

Note: OLS estimates of the effect of the size of a state's legal code on FCPA sanctions of a firm in that state. The dependent variable in column (1) is log(Sanctions/Bribe Revenue). In column (2), the dependent variable is log(Sanctions). t-statistics based on standard errors clustered by industry and year are shown in parentheses.

	log(Sanctions/Bribe Revenue)	log(Sanctions)
	(1)	(2)
log(Kilobytes of State Laws)	-.82	-.68
	(-2.24)	(-1.88)
log(Bribe Revenue)		.90
		(6.17)
log(State Population)	.13	.10
	(0.25)	(0.18)
Year Effects	Yes	Yes
Industry Effects	Yes	Yes
Observations	131	131

Table 9: Examining the Exclusion Restriction

Note: OLS estimates of the effect of the size of the state legal code on various measures of the illegal activity of the firm. The dependent variable in column (1) is log(Bribe Revenue), including the predicted observations. In column (2), the dependent variable is log(Raw Bribe Revenue), not including the predicted observations. The dependent variable in column (3) is log(Bribe), and the dependent variable in column (4) is the Number of Years of Bribes. t-statistics based on standard errors clustered by industry and year are shown in parentheses.

	log(Bribe Revenues)	log(Raw Bribe Revenues)	log(Bribe)	Years of Bribes
	(1)	(2)	(3)	(4)
log(Kbs of State Laws)	1.40	1.41	1.38	.00
	(1.01)	(0.94)	(0.72)	(0.00)
log(State Population)	-.29	-.09	-.12	.23
	(-0.33)	(-0.11)	(-0.10)	(0.21)
Year Effects	Yes	Yes	Yes	Yes
Industry Effects	Yes	Yes	Yes	Yes
Observations	131	83	117	130

Table 10: Balance Test

Note: Estimates of the differences in the observable characteristics of firms with high versus low legal code page residuals. We estimate a OLS model of log(Kbs of State Laws) on log(State Population) and year effects. We then recover the residual. Observations with residuals greater than zero are classified as high legal code page residual observations; those with residuals less than zero are classified as low legal code page residual observations. Means of firm characteristics for high and low residual firms are presented in columns (1) and (2). Column (3) presents the t-statistic of the difference of the two means.

	High Residual (1)	Low Residual (2)	t-Statistic of Difference (3)
log(Bribe Revenues)	2.17	1.68	1.05
Years of Bribes	6.1	5.4	0.73
log(Market Capitalization)	8.6	8.4	0.34
ROA	.037	.033	0.24
Indicator for Southern State	.15	.58	-4.80
log(State Median Earnings)	10.1	10.0	1.80
log(State Judicial Pay)	11.7	11.6	2.23

Table 11: Adding Additional State Controls to Reduced Form and IV Specifications

Note: OLS and IV estimates of the base specifications including additional state controls. Column (1) shows the results of estimating the reduced form specification (column (1) of Table 8, including the state characteristics shown in the balance test. Column (2) shows the result of the IV specification (column (1) of Table 7) also including those controls. t-statistics based on standard errors clustered by industry and year are shown in parentheses.

	Reduced Form	IV
	(1)	(2)
log(Kbs of State Laws)	-1.30	
	(-2.39)	
ESG z-score		-1.14
		(-1.78)
log(State Population)	.32	-.27
	(0.76)	(-1.28)
log(State Median Earnings)	-1.76	-.96
	(-0.48)	(-0.28)
Indicator for Southern State	-.38	-.43
	(0.50)	(-0.60)
log(State Judicial Pay)	.90	.83
	(0.41)	0.46)
Year Effects	Yes	Yes
Industry Effects	Yes	Yes
Observations	131	131

Table 12: Other Measures of the Seriousness of Bribes

Note: Probit estimates of the effect of firm ESG scores on measures of prosecutor and firm behavior. The dependent variable in columns (1) and (2) is Prosecutor Not Identified, an indicator that the prosecutors were not named in the case. The dependent variable in columns (3) and (4) is Voluntary Disclosure, an indicator that the firm turned itself in to authorities. The marginal effects for the probits in brackets. The odd columns show the estimates the effect of ESG scores on prosecutor behavior. The even columns show the estimates the reduced-form effect of state regulation on behavior. t-statistics based on standard errors clustered by industry are shown in parentheses.

	Prosecutor Not Identified		Voluntary Disclosure	
	ESG Scores	Reduced Form	ESG Scores	Reduced Form
	(1)	(2)	(3)	(4)
ESG z-score	.14		-.04	
	(1.09)		(-0.20)	
	[.03]		[-.01]	
log(Kbs of State Laws)		-.71		2.53
		(-1.00)		(1.72)
		[-.15]		[.82]
log(Bribe Revenues)	-.19	-.22	-.22	-.27
	(-1.66)	(-1.65)	(-1.86)	(-2.97)
	[-.04]	[-.05]	[-.07]	[-.09]
log(State Population)		.57		-1.27
		(1.28)		(-1.21)
		[.12]		[-.43]
Year Effects	Yes	Yes	Yes	Yes
Industry Effects	Yes	Yes	Yes	Yes
Observations	131	131	131	131

Table 13: Using Subsample of States with Measure of Early 2000s Legal Code Pages

Note: These are estimates of previous specifications using only firms headquartered in states covered by the original Mulligan and Shleifer (2005) sample. Column (1) shows the OLS relationship between ESG scores and log(Sanctions/Bribe Revenue). Column (2) shows the first stage relationship between firm ESG scores and state legal code size. Column (3) shows the reduced form relationship between log(Sanctions/Bribe Revenue), and column (4) shows the IV results. t-statistics based on standard errors clustered by industry and year are shown in parentheses.

	Simple OLS	First Stage	Reduced Form	IV
	(1)	(2)	(3)	(4)
ESG z-score	-.29			-.38
	(-2.15)			(-1.77)
log(Kbs of State Laws)		1.97	-.75	
		(4.81)	(-1.63)	
log(State Population)		-.69	.08	-.18
		(-3.01)	(0.15)	(-0.71)
Year Effects	Yes	Yes	Yes	Yes
Industry Effects	Yes	Yes	Yes	Yes
Observations	112	112	112	112