

The Effect of Deferred Prosecution Agreements on Firm Performance

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Preliminary
August 19, 2019

We thank the University of Toronto, Tulane University, and the Social Sciences and Humanities Research Council of Canada for financial support. We appreciate the helpful comments from Eugene Soltes.

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Abstract

The recent increase in the use of deferred and non-prosecution agreements (DPAs) by government agencies as a mechanism to hold a firm accountable for having engaged in wrongdoing and to reform the firm's practices has given rise to a vigorous debate regarding the merits and drawbacks of such arrangements, compared with the alternative of prosecuting these firms. We find that firms subject to DPAs experience significantly lower buy-and-hold returns in the one- to three-year period following the DPA compared with prosecuted firms. These results are consistent with shareholders experiencing a wealth loss when a firm enters into a DPA. We also show that DPA firms experience negative real consequences following the initiation of a DPA, relative to prosecuted firms, as measured by decreases in both sales and the number of employees. These results are inconsistent with the idea that DPAs reduce the collateral damage to stakeholders who are not responsible for the crimes committed by the organization (i.e., innocent parties).

Keywords: Non-prosecution agreements, deferred prosecution agreements, prosecution, litigation, financial performance.

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1. Introduction

Government agencies, such as the Securities and Exchange Commission (SEC), Department of Justice (DOJ), and Department of Labor (DOL), have traditionally either prosecuted firms for criminal misconduct or settled wrongdoing through plea deals. More recently, the use of a third option—deferred and non-prosecution agreements (DPAs, hereafter)—has become increasingly popular. With the agreement of the firm alleged to be involved in wrongdoing, DPAs allow government agencies to impose a customized set of conditions, often including changes to governance and compliance, as well as financial penalties, on the firm. In return, the government agrees to defer or decline prosecuting the firm in the future. Since 2000, government agencies have entered into over 300 agreements with various corporations, of which more than half have been executed in 2010 or later (Dunn et al., 2014). Neither the factors that have led to an increase in the use of the DPAs, nor the choices of the conditions and compliance stipulated in the agreements, are clear. While anecdotal evidence suggests that the cost of litigation and the fear of unintended consequences has been the driving force behind the proliferation of this alternative mechanism to prosecution, to the best of our knowledge, ours is the first empirical study to examine the impact of DPAs on longer-term firm performance.

Although the use of DPAs has increased significantly in the past decade, the practice has not been without controversy (Kaal and Lacine, 2014). Proponents of their increased use argue that the agreements are a more efficient method to discipline firms for failing to comply with laws. Given the bargaining power residing with the government in a DPA, corporate changes can be enacted without: 1) the government incurring costs associated with litigation while obtaining the burden of proof to secure criminal conviction; and, 2) creating unintended collateral damage to the

economy and other stakeholders, similar to those resulting from the indictment of Arthur Andersen.

Those opposing the use of DPAs, however, point out that these agreements are extrajudicial contracts that operate outside of the regular legal system and potentially undermine the rule of law. Further, the extent of bargaining power available to the government agencies allows them to potentially impose overly strict conditions, which may be more costly to firms' stakeholders than any collateral damage arising from prosecution (Greenblum, 2005). Additionally, opponents raise the issue that such agreements may be ineffective due both to the variation in their implementation and difficulty in assessing the extent to which firms comply with each condition set forth in such agreements. In essence, DPAs are viewed by critics as a more lenient way to deal with corporate crime that may be more detrimental to shareholders and other stakeholders than litigation.

Ultimately, it is an open question as to whether DPAs are a more or a less effective method compared with the alternative of prosecution. Corporate governance and conduct improvements associated with DPAs are mostly internal to the firm and difficult to measure directly. Accordingly, we examine firms' longer-term stock market performance to capture the relative net benefit of being subjected to a DPA as opposed to prosecution.

We gather a sample of 109 DPA firms for which all financial variables are available from the privately gathered collection of DPAs, generously made available by Professor Brandon Garrett of Duke University. We compare our sample of DPA firms to 496 firms prosecuted by the government. In our main set of tests, we find that DPA firms experience significantly lower stock market performance relative to the prosecuted firms. By the end of the first year following the DPA, we find that buy-and-hold abnormal returns (*BHAR*) are 15.5 percentage points lower for DPA firms relative to prosecuted firms. By the end of the third year, *BHAR* is 21.4 percentage

points lower for DPA firms relative to prosecuted firms. Our finding is robust to various measures of stock market performance.

To further explore whether DPAs potentially shield other stakeholders from collateral damages, we examine the effect these agreements have on the change in sales levels, the number of employees, and total assets relative to firms that are prosecuted. If the agreements are enacted to shield other stakeholders from the potential fallout that comes with the litigation, we would expect to see the benefits accruing to such stakeholders. Interestingly, we do not observe the collateral benefits of such arrangements. Specifically, we find that DPA firms have lower sales and fewer employees following a DPA. By the end of the third year, we find that the change in sales levels is 11.2 percentage points lower for DPA firms relative to prosecuted firms. The change in employee levels for DPA firms show a similar trend with a decline of 11.0 percentage points by the end of the third year relative to prosecuted firms. Overall, our results do not lend support for the idea that DPAs are more beneficial to a firm's stakeholders.

Our study makes several contributions to the literature. First, the study is one of the earliest to explore the implications of deferred and non-prosecution agreements for investors and other stakeholders, such as employees and governments, both in the U.S and abroad.¹ While legal scholars have examined the nature of the prosecution agreements descriptively, there are no studies, to our knowledge, that empirically examine whether such agreements are beneficial to the

¹ A recent high-profile scandal involving SNC-Lavalin, a Canadian firm accused of paying millions of dollars in bribes to conduct business abroad, is one example of the controversy surrounding DPAs outside of the U.S. The firm is alleged to have issued a "threat" to the Canadian government that it would be forced to eliminate a significant portion of its workforce should it be prosecuted, effectively lobbying the government to grant it leniency through a DPA. According to allegations, SNC-Lavalin argued that the DPA would allow it to remediate its procedural deficiencies while saving the jobs of thousands of Canadians. The government defended its position to eventually deal with the SNC-Lavalin corruption case through a DPA rather than through a more traditional court process stating that such an agreement is not a "get-out-of-jail-free-card" and any firm involved in such an agreement is subject to significant fines and sanctions.¹ Despite reassurances by the government that SNC-Lavalin's wrongdoing was being appropriately punished, many disagreed with the idea that such agreements are equivalent to the regular judicial process and characterized the government's use of such an agreement as an attempt to sweep the scandal under the rug.

firm and other stakeholder groups or whether, alternatively, they lead to worse outcomes relative to litigation.² To that end, we document that DPAs seem to result in the worsening of corporate performance compared with prosecution. Furthermore, contrary to popular belief, we document that such arrangements do not appear to provide the benefit of protecting employees and other constituencies from collateral damages imposed by prosecution. This result has important policy implications, as it adds to the debate on whether there are tangible benefits or costs to these extrajudicial agreements, and whether the increased use of DPAs as witnessed during the past decade is warranted. Second, the study contributes to the literature examining the impact of litigation on corporate reform. The findings in our study add further credence to the idea that litigation acts as a corporate governance mechanism benefiting shareholders (Appel, 2016) and may be superior to other arrangements (e.g., DPAs) that plaintiffs may explore to “punish” the firm for corporate misconduct. Third, our study contributes to our understanding of corporate governance by exploring an alternative channel through which changes to corporate conduct can be induced and documenting the effectiveness of such a channel in reforming corporate behavior and, ultimately, improving firm performance.

The rest of the study is organized as follows. Section 2 describes the institutional background and provides greater insight into the nature of DPAs. Section 3 reviews related literature while Section 4 develops our hypotheses. Section 5 describes the data used and the research design. Section 6 discusses our findings. Section 7 concludes.

2. Institutional Background

Traditionally, a firm found to have engaged in unlawful behavior was subject to

² One related working paper that we are aware of is Kaal and Lacine (2015). They study the short-term stock market response to DPAs and observe mixed results. The working paper, however, is incomplete and lacks statistical tests.

government prosecution. The outcome of such prosecution may have resulted in a lengthy legal trial or a settlement through a plea deal. Prosecution can have serious consequences for the accused firm, however, as witnessed by the Enron scandal and subsequent prosecution of Arthur Andersen, leading to the collapse of the accounting firm and eventual layoff of 28,000 of its employees (Garrett, 2014). To avoid this costly process and mitigate similar possible unintended consequences of prosecution to various stakeholders, government agencies in the United States, U.K., and, more recently, Canada, have begun entering into non-prosecution or deferred prosecution agreements (DPAs, hereafter) instead of prosecuting firms accused of wrongdoing.³

In the U.S., DPAs have been in the existence since the early 1990s. Such agreements, however, were infrequently used until 1999, when the Department of Justice implemented guidelines regarding the prosecution of business organizations. The original guidelines, drafted by the then Attorney General Eric Holder, Jr., stipulated nine factors to be considered when deciding which course of action to pursue when dealing with corporate crimes.⁴ While the guidelines make no specific mention of DPAs as a possible mechanism for dealing with corporate offenders, they provide criteria as to when some type of “alternative” to prosecution may be considered. Among the nine factors specified, accused firm’s cooperation with the investigation is often considered the most important deciding factor as to whether a government agency should pursue an alternative to prosecution. Other notable factors include the adequacy of the offender’s compliance system, prior history of wrongdoing, self-reporting of the underlying issue, proactive undertaking of remedial actions, waiver of client-attorney privileges, and the likelihood of collateral consequences

³ While we use the DPA abbreviation through the study, our intent is to capture both types of agreements: the non-prosecution and deferred prosecution agreements.

⁴ The original guidelines are often referred to as the Holder Memo, because they were created by the then Attorney General, Eric Holder Jr. History available in “The Principles of Federal Prosecution of Business Organizations: A Ten-Year Anniversary Perspective”, which can be found at https://www.paulweiss.com/media/1497187/pw_nysba_oct09.pdf.

to the stakeholders.⁵

Despite the availability of this alternative mechanism to deal with corporate crime since the 1990s, the use of the agreements increased significantly only in the most recent years. Between 1993 and 2005, there were only 34 DPAs negotiated between various government agencies and firms accused of wrongdoing (Barkow and Barkow, 2011). Most of the agreements occur after Attorney General Larry Thompson revised the Holder Memo and issued it in a form of new guidance entitled “The Principles of Federal Prosecution of Business Organizations” in 2003. The updated guidelines, which explicitly named deferred prosecution as one of the options to punish and reform corporations following wrongdoing, were issued as a response to the criticism of the Department of Justice’s (DOJ) handling of the Arthur Andersen prosecution (Uhlmann, 2013), leading to the ultimate collapse of the firm.

Issuance of the new guidelines and two subsequent revisions—one in 2006 and another in 2008—resulted in a significant spike in the use of the agreements as an alternative disciplining mechanism used by government agencies dealing with corporate crime. Since 2000, the Department of Justice (DOJ) and the Securities and Exchange Commission (SEC) have entered into over 300 agreements with various corporations, of which more than half have been executed in 2010 or later (Dunn et al., 2014). While many DPAs have been initiated by the DOJ or the SEC, these are not the only two agencies using similar types of agreements in place of prosecution. The agreements are normally formed between a firm accused of misconduct and the regulatory agency that has a jurisdiction over the matter in which the firm is alleged to have behaved unlawfully.

While the U.S. has a relatively longer history of DPA use, other countries have recently adopted similar practices. For instance, Canada introduced similar agreements as recently as in

⁵ Original memo available at the U.S. Department of Justice website: <https://www.justice.gov/sites/default/files/criminal-fraud/legacy/2010/04/11/charging-corps.PDF>.

2018 by means of a modification to the Criminal Code. The Criminal Code defines such remediation agreements as “an agreement between an organization accused of having committed an offence and a prosecutor, to stay any proceedings related to that offence if the organization complies with the terms of the agreement.”⁶ While multiple countries presently use DPAs, the implementation of the DPA mechanism differs across different jurisdictions in certain aspects. For example, unlike in the U.S. where the role of the courts is limited, Canadian implementation of DPAs stipulates that a superior court must first judicially approve any such agreement before it is considered valid.⁷

Regardless of the jurisdiction, DPAs are similar along many dimensions. Such agreements usually impose a monetary penalty on the accused firm. In addition, they customarily stipulate a number of conditions, which the accused firm must meet in order to avoid further legal action (hence, the term “deferred prosecution”). Compliance with the conditions is monitored over the length of the agreement either directly by the government agency involved, or by appointing an independent third party (i.e., an external monitor) to oversee the compliance process. While traditional prosecution is considered the proverbial “stick” of the legal system, the DPA is the “carrot.” In return for some combination of cooperation, payment of fines, and admission of guilt, the government agency spares the firms from a lengthy trial, possible criminal conviction, and the associated reputational damage that can ensue following prosecution.⁸

3. Literature Review

The rapid rise in the use of DPAs as an alternative to prosecution leads to a natural question

⁶ Full text of criminal code available at: <https://laws-lois.justice.gc.ca/PDF/C-46.pdf>.

⁷ “Deferred Prosecution Agreements are Coming to Canada” — https://blg.com/en/News-And-Publications/Publication_5406.

⁸ “Government of Canada Announces Public Consultation regarding Deferred Prosecution Agreements” found at <https://mcmillan.ca/government-of-canada-announces-public-consultation-regarding-deferred-prosecution-agreements-dpas>.

of whether such agreements are effective at enacting corporate governance changes and subsequently leading to improvement in performance of firms entering into such agreements. Popular opinion regarding the effectiveness of such arrangements vis-à-vis prosecution, the alternative disciplining mechanism, varies greatly with pundits on both sides proposing compelling arguments as to why DPAs may or may not lead to the desired improvement in corporate conduct and, ultimately, firm performance.

While little evidence exists on the costs and benefits of DPAs, a significant literature studies the effects of prosecution (i.e., government prosecution and private litigation) on firms as well as the individual actors accused of corporate misconduct. In order to hypothesize about the relative strength of the two disciplining mechanisms in improving firm performance, one has to understand possible differences between the two as well as prior findings regarding the impact of prosecution on firm outcomes.

Prior literature posits that prosecution can act as either an ex-ante or an ex-post governance mechanism. Thompson (1999) argues that through the threat of litigation, shareholders and other stakeholders can ex-ante affect firm's corporate governance and, ultimately, its conduct. Further, Appel (2016) posits that the mere threat of securities litigation can deter firms from engaging in wrongdoing in the first place. Personal reputation costs borne by directors and officers associated with litigated firms motivates such individuals to not engage in behaviors that may lead to litigation (Fich and Shivdasani, 2007; Karpoff et al., 2008; Brochet and Srinivasan, 2014). Using state-level staggered adoption of universal demand laws, which make litigation significantly more difficult, Appel (2016) finds that reducing the threat of litigation leads to worse financial performance and increased entrenchment at firms affected by the passage of such laws. Similarly, Donelson and Yust (2014) document that an exogenous decrease in litigation risk adversely affects operating

performance and leads to an increase in restatements. In essence, easy access to litigation as a potential disciplining mechanism leads firms, *ex ante*, to behave in a way that is beneficial to shareholders. While litigation is often studied empirically in the context of securities class action lawsuits, government agencies can also bring lawsuits against corporations and, presumably, also affect corporate governance through similar channels.

Conditional on having engaged in wrongdoing, prosecution can also help restore firms' corporate governance *ex-post*. Litigation settlements often specify changes to governance practices that firms should implement (Appel, 2016). Erickson (2010) estimates that around 80% of litigation judgments include provisions specifically aimed at improving corporate governance. Ferris et al. (2007) and Cheng et al. (2010) document that litigated firms experience a significant increase in the number of outsiders on their boards along with other positive changes to their board characteristics. Even without specific requirements to change governance, firms often make changes to the board and the executive team following a high-profile negative event to signal a commitment to improved behavior as well as improve their legitimacy (Liu, 2013). As improvements to governance are virtually certain post-litigation, it is plausible that litigation will ultimately result in improved future performance because good governance leads to better financial performance. Certainly, numerous studies show that governance structure affects firm performance. For instance, increased board independence (Weisbach, 1988, Yeh and Woidtke, 2005; Adams and Ferreira, 2007; Choi et al., 2007; Black and Kim, 2012), board expertise (Krishnan and Visvanathan, 2008; Goh, 2009), and removal of entrenchment provisions (Bebchuk and Cohen, 2005; Faleye, 2007) are shown to improve firm performance along different dimensions and lead to an increase in firm value.

4. Hypothesis Development

Based on the discussion above, the question arises as to how DPAs compare to prosecution in their perceived ability to ex-post improve firm governance and ultimately performance. On one hand, it is possible that DPAs can be as effective, if not more, in achieving the desired outcomes following prosecution. By virtue of government agencies' bargaining power, corporate changes can be forced upon a target firm.⁹ Should the firm not comply with the terms of the enacted agreement, the government agency in question may elect to pursue further action and eventually prosecute the firm. As firms are unlikely to risk an indictment, prosecutors can expect compliance with every demand (Markoff, 2013). In the case of those agreements that defer prosecution, given that the threat of litigation is not removed but rather merely delayed for a period of time conditional on firms meeting the demands set forth, these agreements have the potential to more readily bring about change than traditional prosecution.

Further, prosecution is costly, not only to the government, but also to the accused firm. The legal fees can be considerable, and even if the financial settlement or judgement is relatively low, the financial benefits may accrue primarily to the lawyers, rather than to the firm's shareholders (Romano, 1991). By entering into a DPA, the firm avoids prolonged litigation, which can consume financial and other resources (Coffee, 1986; Romano, 1991; Lowry and Shu, 2002; Haslem, 2005; Simmons and Ryan, 2007), and have negative consequences in terms of reputation (Karpoff and Lott, 1999; Atanasov et al., 2007; Franz et al., 2015), access to capital (Autore et al., 2014) and firm value (Cutler and Summers, 1988; Bizjak and Coles, 1995). In some instances, the cost of prosecution may be so large that it leads to significant financial distress and increased likelihood of bankruptcy (Bhagat et al., 1994). Lawsuits brought forward by government agencies, in

⁹ Assistant Attorney General, Lanny A. Breuer, U.S. Department of Justice at an address to the New York City Bar Association. Available at <http://www.justice.gov/criminal/pr/speeches/2012/crm-speech-1209131.html>.

particular, are associated with some of the largest wealth losses (Bhagat et al., 1998). By avoiding government-initiated prosecution, firms also avoid the potential large negative payoff associated with such an event. Consequently, it is possible that DPAs lead to better firm performance in the period following the unlawful event compared with prosecution due to the lower costs incurred when disciplined through a DPA.

On the other hand, opponents consider a DPA to be a weaker form of disciplining—the legal equivalent of a “slap on the wrist.” They claim that by entering into an agreement, the government agencies, in essence, allow for preferential treatment of some firms, indicating the government’s lack of appetite to discipline such firms, and sowing doubt regarding the government’s commitment to deal with corporate crime (Uhlmann, 2013; Markoff, 2013). For example, following the U.S. DOJ’s deferred prosecution agreement with HSBC for money laundering and violation of sanctions, the *New York Times* ran an editorial claiming that it was a “dark day for the rule of law” and noted that government has “bought into the notion that too big to fail is too big to jail.” (See Markoff (2013) for the full reference to the case.)

Knowing that the government is willing to cut a deal, such firms may be reluctant to fully implement the required changes. Variation in the conditions stipulated in the agreements as well as variation in the enforcement and monitoring of the compliance with such conditions may render such agreements ineffective. In essence, those opposed to these arrangements posit that such deals undermine the rule of law as they exist outside the realm of the regular legal system (Uhlmann, 2013). If DPAs are ineffective at enacting meaningful improvements to corporate governance and corporate conduct, there is little reason to believe that firms subject to such agreements would experience better future firm performance.

Even if the DPAs are not a weaker form of disciplining, from the firm’s perspective, it is

possible that DPAs are as costly, if not more costly, than prosecution. As the government agency has greater bargaining power, for DPA firms the punishment enacted through fines may be greater in magnitude and compliance requirements placed on the firms more burdensome and costly to implement relative to those determined by litigation. Further, compliance with the agreements mandate certain corporate changes and on-going compliance with the conditions set forth in the agreement. The process of complying with the conditions may be more than just financially costly; it could be time-consuming as well, draining corporate resources throughout the duration of the agreement.

In light of these competing views, we argue that it is ultimately an empirical question as to whether the impact of DPAs is comparable to that of prosecution. We examine the impact of DPAs on firm performance for the following two reasons. First, while DPAs are meant to lead to corporate reform through changes in corporate governance, any changes in corporate conduct are internal to the firm and, therefore, difficult to observe and measure directly. In fact, the Government Accountability Office issued a report on the use of DPAs and noted the following:¹⁰

"DOJ cannot evaluate and demonstrate the extent to which DPAs and NPAs—in addition to other tools, such as prosecution—contribute to the department's efforts to combat corporate crime because it has no measures to assess their effectiveness. Specifically, DOJ intends for these agreements to promote corporate reform; however, DOJ does not have performance measures in place to assess whether this goal has been met."

Second, even if we could observe and measure changes to conduct, we would not be able to capture the costs associated with these changes by simply examining whether a firm implements them or not. By associating firm performance with the choice of the disciplining mechanism, we are better

¹⁰ DOJ Has Taken Steps to Better Track its Use of Deferred and Non-Prosecution Agreements, but Should Evaluate Effectiveness, US Government Accountability Office, December 2009. Available at: <https://www.gao.gov/new.items/d10110.pdf>.

able to capture the relative net impact of the mechanism on firm value. If reforms resulting from the agreements are net beneficial, any benefits derived should be reflected in the firm performance in subsequent periods. Hence, we examine firms' longer-term stock market performance to capture the relative net benefit of being subjected to a DPA as opposed to prosecution. Accordingly, our first hypothesis (in null form) is stated as follows:

H1: A firm disciplined through a DPA for an unlawful event experiences post-event firm stock market performance similar to that of a firm subjected to prosecution.

One of the main arguments put forth by government agencies justifying the alternative use of DPAs as disciplining mechanisms is that they shield the firm and ultimately its stakeholders, namely employees, from the unintended consequences of prosecution. Prosecuted firms may incur significant reputational damage, such as the potential loss of clients, customers, and contracts, which consequently leads the firm to shutting down or downsizing. Such outcomes may eliminate jobs and negatively impact the local economy. Further, prior studies document that the negative wealth effects associated with prosecution are not limited to the defendant; instead, negative consequences can spill over to industry peers as well (Gande and Lewis, 2009). The sentiment is echoed in the guidelines, issued by Attorney General Thompson and further revised in 2008, which state: "Obtaining a conviction may produce a result that seriously harms innocent third parties who played no role in the criminal conduct." Anecdotally, the litigation of Arthur Andersen led to the corporate collapse and loss of 28,000 jobs across the country. To the extent that DPAs can prevent collateral damage to the employees and other stakeholders resulting from downsizing or shutting down business segments, there may be value to such arrangements even if firm performance does not differ based on the choice of the disciplining mechanism.

While those in favor of DPAs point to Arthur Andersen as an example of litigation that led

to a massive economic impact, it is important to note that the Andersen case was unusual in that respect. Most firms involved in high-profile prosecutions resulting from corporate wrongdoing are still in business (Markoff, 2013). Furthermore, the alternative to prosecution is not costless to the firm either. A firm entering into a DPA has to pay a fine and invest significant resources into complying with the conditions of the agreement. The financial and management time burden associated with the enactment of the DPA may be severe enough to lead to the streamlining of operations or implementation of cost-cutting measures, ultimately having a negative impact on the firm's ability to keep the operations intact and leading to similar downsizing and negative consequences to stakeholders. Further, it is not clear that firms entering into DPAs are fully shielded from the reputational damage that comes from being identified as a corporate offender. As many of the DPAs are highly publicized, even though the content of the DPA is often not disclosed, it is generally known that a firm breached the law. The fact that the firm was allowed to enter a DPA instead of being prosecuted may not lead to a more positive perception of the firm, or limit the reputational damage stemming from being associated with illegal activity. Therefore, it is not clear, *ex ante*, whether a DPA can shield a firm's stakeholders from the negative consequences of the firm's involvement in wrongdoing and subsequently, limit the collateral damage. Accordingly, our second hypothesis (in null form) is stated as follows:

H2: A firm disciplined through a DPA for an unlawful event experiences post-event collateral damages (e.g., downsizing, employee loss) similar to that of a firm subjected to prosecution.

5. Data and Research Design

5.1. *Data and Sample Construction*

We begin by creating a list of all companies that enter into a DPA. To do so, we use the

privately gathered collection of DPAs and docket sheets generously made available by Professor Brandon Garrett of Duke University. As there is no public repository of DPAs, he compiled the initial set of companies through FOIA requests (Garrett, 2014). We compare this set of firms with data from the International Association of Independent Corporate Monitors (IAICM) to ensure that we have as complete a set of agreements as possible. Using the two repositories as the starting point yields a total of 467 firms for which such agreements are available. We perform secondary searches using Google and the Bloomberg Business Week website to identify the parent company at the time of the agreement and hand match the parent company name to CRSP. This initial screening results in 184 DPAs between the years 2001 and 2017. We require that all observations have a full set of dependent variables and control variables, restricting the final DPA sample to 109 unique agreements, as shown in Table 1. One limitation of our study is that we cannot observe what performance of DPA firms would have looked like had these firms been prosecuted. To the extent that DPA firms are particularly vulnerable to collateral damages arising from prosecution, we may not be able to fully capture the extent to which these firms benefit from DPAs relative to prosecution. That said, to partially address the limitation, we use a matched control sample of firms that do not enter into a DPA where the control firms are matched on industry, year, and closest size in the year prior to the date of the DPA.

To construct our sample of prosecuted firms, we start with litigation data from Audit Analytics, which contains various types of litigation. Since we are interested in litigation in which the plaintiff is a government organization (as opposed to shareholder litigation), we retain observations in the litigation sample only if one of the plaintiffs identified is a government agency. We impose the same data requirements for prosecutions as we do for DPAs, resulting in a final sample of 496 unique government prosecutions. As with the DPA sample, we identify a set of

control firms that are not prosecuted by the government, matching on industry and closest size in the year prior to initiation of the prosecution.

Table 2 provides descriptive information our DPA sample. Panel A shows the sample composition by industry, which is defined as the two-digit SIC code. Manufacturing is the most highly represented industry in our sample with 49 observations, followed by finance with 28 observations.

The remaining panels provide additional hand collected terms of the DPA agreements. Panel B lists the frequency of the primary offense category. “FCPA/Bribery/Kickbacks” is the most common primary offense with 46 observations, followed closely by “Fraud” with 44 observations. Other offenses include antitrust, environmental, food and drug, and labor violations.

In lieu of prosecution, companies are commonly forced to allocate additional resources to compliance controls, often under the oversight of an independent monitor. When selecting an independent monitor, the government, company, or both parties suggest a list of potential candidates. According to John Hanson of the International Association of Independent Corporate Monitors:

“The best person for this role is an expert on corporate compliance and ethics programs. That can involve a lawyer or not, someone who has previously worked for the federal government or not. The trick is to find someone who can effectuate a plan that he or she had no input on developing.”

Panel C shows that in our sample, an independent monitor is appointed in 34% (37/109) of the cases.¹¹ The panel also documents that of the agreements, 88 of the 109 agreements refer to specific

¹¹ While about a third of DPAs require that the firm use an independent monitor to devise the plan to improve processes and procedures and provide oversight over the implementation, the role of a monitor has not been without controversy. In some instances, monitors have been crucial in ensuring a firm’s remediation of problems (e.g., Standard Chartered Bank’s monitor was instrumental in finding loopholes in company’s software). Equally, however, there are documented cases of corporate monitors acting in a way that violated purported independence. Adding to the complexity, corporate monitors are paid by the company which they are in charge of overseeing, casting some doubt

pre-agreement compliance actions taken by the offending firm. Examples of pre-agreement compliance include extraordinary cooperation by the company, voluntary disclosure of criminal activities, remedial actions taken to improve compliance procedures, the addition of board members, and the removal of individuals directly involved in the criminal activities. The last column of Panel C shows that 86 DPAs require a compliance program, which typically involve periodic reporting on compliance improvements taken by the offending firm.

From Panel D, we observe that the mean (median) probationary period of our sample is 3.6 (3.0) years. The mean (median) total monetary fine imposed in our sample is \$96.2 million (\$13.4 million). The fine imposed ranges from a minimum of \$0 to a maximum \$1.7 billion.

Table 3 provides characteristics of the DPA, prosecuted, and two separate matched control samples. All variables are measured in the fiscal year prior to the event year, where the event year is defined as the fiscal year that contains the date of the DPA or the initiation of the government litigation. The Appendix provides definitions for all variables used in our tests.

As shown in column (iv), DPA firms and their matched control firms are similar across multiple dimensions. However, we do find that DPA firms exhibit lower sales growth (*Sales Growth*) and capital intensiveness (*PPE*) than control firms at the 10% significance level. We find that the prosecuted firms have lower market-to-book ratios (*MB*), capital intensiveness (*PPE*), sales growth (*Sales Growth*), returns (*Return*), and equity issuance (*Equity Issue*) than their matched control firms. However, prosecuted firms have higher analyst following (*Coverage*) on average than control firms. Column (ix) compares DPA firms with prosecuted firms. We find that DPA firms are larger at the 1% level, consistent with Garrett's (2014) premise that larger firms are

on the notion that monitors can remain independent during the oversight process (Thomson Reuters, "Deferred Prosecution Agreements: Working with the Independent Monitor", available at: <https://blogs.thomsonreuters.com/answeron/deferred-prosecution-agreements-working-independent-monitor/>).

more likely to receive a DPA than face prosecution. We also find that DPA firms have lower market-to-book ratios, are less capital intensive, have lower sales growth than prosecuted firms. Other differences are statistically insignificant.

5.2. Research Design

5.2.1. *Stock Market Performance.* We test the prediction that DPAs differentially affect stock market performance (H1) by estimating the following regression:

$$\begin{aligned}
 BHAR_{t+n} = & \beta_0 + \beta_1 DPA_t + \beta_2 Size_{t-1} + \beta_3 MB_{t-1} + \beta_4 PPE_{t-1} + \beta_5 Sales\ Growth_{t-1} \\
 & + \beta_6 ROA_{t-1} + \beta_7 Return\ Std_{t-1} + \beta_8 Equity\ Issue_{t-1} \\
 & + \beta_9 Going\ Concern_{t-1} + \beta_{10} Coverage_{t-1} + Year\ FE + \varepsilon_{t+n}
 \end{aligned} \tag{1}$$

$BHAR_{t+n}$ is the market-adjusted buy-and-hold abnormal return measured from the beginning of fiscal year t (i.e., the event year) through the end of year $t+n$. DPA is an indicator variable set to 1 if the firm entered into a non-prosecution of deferred prosecution agreement in year t , zero otherwise. We include a vector of control variables measured in the year prior to the event. Our controls include variables which could be correlated with assignment to our treatment or control group. Drawing on prior research by Kim and Skinner (2012), we control for the following firm characteristics associated with the propensity to face litigation: 1) lagged size ($Size$), 2) lagged market-to-book (MB), 3) lagged capital intensity (PPE), 4) lagged change in sales ($Sales\ Growth$), 5) lagged market performance ($Return$), and 6) lagged return volatility ($Return\ Std$). We also control for the demand of capital ($Equity\ Issue$ and $Going\ Concern$) and monitoring of the firm ($Coverage$). Finally, we include year fixed effects in all of our empirical analyses to control for variation in our outcome variables over time and to control for time variation in the annual operating budgets of government prosecutors. Our coefficient of interest in Eq. (1) is β_1 , which captures the incremental difference in market performance of DPA firms relative to the benchmarked sample of firms (i.e., matched control firms or prosecuted firms). A positive

(negative) β_1 would be consistent with DPA firms having higher (lower) market performance after receiving an DPA relative to benchmark firms.

4.2.2. *Real Effects*. In our second set of tests, we test the prediction that DPAs differentially affect real outcomes relative to a benchmarked sample (H2). To test for differences in real outcomes, we estimate the following regression:

$$\begin{aligned} \text{Real Effect}_{t+n} = & \beta_0 + \beta_1 \text{DPA}_t + \beta_2 \text{Size}_{t-1} + \beta_3 \text{MB}_{t-1} + \beta_4 \text{PPE}_{t-1} + \beta_5 \text{Sales Growth}_{t-1} \\ & + \beta_6 \text{ROA}_{t-1} + \beta_7 \text{Return Std}_{t-1} + \beta_8 \text{Equity Issue}_{t-1} \\ & + \beta_9 \text{Going Concern}_{t-1} + \beta_{10} \text{Coverage}_{t-1} + \text{Year FE} + \varepsilon_{t+n} \end{aligned} \quad (2)$$

We use several proxies for real effects: 1) the percentage change in annual sales levels from year $t-1$ to year $t+n$ (ΔSales_{+n}), 2) the percentage change in employees from the end of year $t-1$ to the end of year $t+n$ (ΔEmp_{+n}), and 3) the percentage change in total assets from the end of year $t-1$ to the end of year $t+n$ (ΔTA_{+n}). Other variables are as previously described. Our coefficient of interest in Eq. (2) is β_1 , which captures the incremental difference in real effects for DPA firms relative to the benchmark firms.

6. Results

6.1. Tests of H1: Stock Market Performance

Table 4 presents results of our univariate tests of stock market performance for DPAs and prosecuted firms. As shown in column (i), *BHAR* is negative and statistically different from zero as of the end of the first, second, and third years following a DPA. We do not see a similar decline in stock market performance for prosecuted firms, as shown in column (ii). In column (iii), we find that *BHAR* is significantly lower for DPA firms relative to prosecuted firms as of the end of the first, second, and third years following the event. We plot *BHAR* for DPA firms and prosecuted firms in Figure 1, which graphically demonstrates the results from Table 4. In year t (i.e., the year

of either the DPA or prosecution), we note little difference in *BHAR* between DPA and prosecuted firms. However, for DPA firms, *BHAR* continues to drift lower relative to prosecuted firms over the next three years following the year the agreement was entered into.

The results of our stock market performance tests are reported in Table 5. In models (i) through (iv), we examine the difference in *BHAR* for DPA firms relative to a matched control sample, where *BHAR* is measured cumulatively over years t through $t+3$, respectively. In model (ii), β_1 is negative and marginally significant at the 10% level. In models (iii) and (iv), β_1 is consistently negative, but no longer significant at conventional levels. In models (v) through (viii), we examine the relative difference in *BHAR* for prosecuted firms relative to a matched control sample. *DPA* is replaced by *Prosecuted*, an indicator variable set to 1 for prosecuted firms, zero otherwise. We find no statistical difference in *BHAR* between prosecuted firms and a matched sample of firms either in the event year or cumulated over the next three years.

Finally, in models (ix) through (xii), we benchmark DPA firms to prosecuted firms, given the important policy implications and debate within legal circles regarding the relative efficacy of DPAs relative to the more traditional method of prosecuting corporate crime. In model (ix), we find no difference in *BHAR* between DPA firms and prosecuted firms ($\beta_1 = 0.031, p = 0.355$). In model (x), we find that *BHAR* is 15.5 percentage points lower for DPA firms relative to prosecuted firms ($\beta_1 = -0.155, p < 0.01$) by the end of the first year following the event. By the end of the second and third years, *BHAR* is 20.5 percentage points ($\beta_1 = -0.205, p < 0.01$) and 21.4 percentage points ($\beta_1 = -0.214, p = 0.019$) lower, respectively, for DPA firms relative to prosecuted firms. In untabulated tests, we reestimate models (ix) through (xii), using the same sample of DPA and prosecution firms but use entropy balancing to ensure that our DPA firms and prosecuted firms are similar across all control variables measured prior to the event year. Results and inferences

are qualitatively similar.¹²

In general, the results from Table 4 and Table 5 provide strong evidence that buy-and-hold abnormal returns are significantly lower, both economically and statistically, for DPA firms relative to prosecuted firms in the years following the event. These results cast doubt on the notion that DPAs are more beneficial to a firm's shareholders than traditional prosecution.

6.2. *Tests of H2: Real Effects*

In Section 4, we posit that DPAs impact firms' operations differentially relative to prosecution. We further examine this claim, which forms the basis for the arguments put forth by proponents, namely that DPAs may prevent collateral damages to employees and other stakeholders. We begin by examining the effect on changes in sales levels, shown in Table 6. Models (i) through (iv) examine the difference in $\Delta Sales$ for DPA firms relative to a matched control sample. β_l is negative and marginally significant at the 10% level in year t ($\beta_l = -0.059$, $p = 0.067$). In models (ii), (iii), and (iv), β_l is increasingly negative and statistically significant at conventional levels. By the end of the third year following the DPA, the change in sales levels is 23.7 percentage points lower for DPA firms relative control firms ($\beta_l = -0.237$, $p < 0.01$). In models (v) through (viii), we find no statistically significant difference in $\Delta Sales$ for prosecuted firms relative to a matched set of controls firms. In models (ix) through (xii), DPA firms are benchmarked against prosecuted firms. Sales levels decline throughout the three years following the DPA. By the end of the third year, we find that the change in sales levels for DPA firms is 11.2 percentage points lower compared with prosecuted firms ($\beta_l = -0.112$, $p = 0.034$).

Table 7 reports tests examining the effect of DPAs on changes in employee levels (ΔEmp).

¹² All results and inferences for the tests in the remaining analyses in which we compare DPA firms with prosecution firms are insensitive to the use of entropy balancing.

As in Table 6, models (i) through (iv) benchmark DPA firms to a matched control sample. Our coefficient of interest, β_1 , is negative in models (iii) and (iv), consistent with employee levels declining for DPA firms. By contrast, we find no statistically significant difference between prosecuted firms and matched control firms. Finally, in models (ix) through (xii), DPA firms are benchmarked against prosecuted firms. By the end of the second and third years, the change in employee levels is 6.4 percentage points ($\beta_1 = -0.064, p = 0.059$) and 11.0 percentage points ($\beta_1 = -0.110, p < 0.01$) lower, respectively, for DPA firms compared with prosecuted firms. In total, the evidence provided in Table 7 is inconsistent with the use of DPAs mitigating unintended consequences of prosecution accruing to employees of the prosecuted firm. Rather, our results indicate that DPAs have a negative impact on employees in the years following the unlawful behavior relative to prosecution of the firm.

In Table 8, we report results of our tests in which we examine the effect of DPAs on changes in asset levels. Relative to the control sample, the change in total assets for DPA firms is 11.8, 16.1, and 20.9 percentage points lower by the end of the first, second, and third years following the DPA, respectively. We find no difference between prosecuted firms and matched control firms for any of the years following the event. In tests benchmarking DPA firms against prosecuted firms, β_1 is not statistically significant. When examining the DPA source documents, we find that certain agreements explicitly discuss the closure of segments or branches associated with the corporate wrongdoing. Overall, the results in Table 8 provides modest empirical evidence consistent with DPA firms downsizing.

7. Conclusion

The recent increase in the use of DPAs as a mechanism to hold firms accountable for having engaged in wrongdoing and to reform its conduct has given rise to a vigorous debate regarding the

merits and drawbacks of such alternative disciplining arrangements. While those in favor of DPAs argue that the agreements provide the benefits of prosecution without the unintended consequences to other stakeholder groups, opponents posit that replacing prosecution with extrajudicial agreements diminishes the rule of law and allows for discretion in their application, which may favor certain firms and create an uneven playing field. Currently, anecdotal evidence supports both sides. Our study is one of the first to provide systematic empirical evidence on whether the claims in favor of this new method to deal with corporate crime are substantiated.

We begin by descriptively documenting the nature of DPAs. To provide evidence on the consequences of this increasingly popular disciplining mechanism, we then document the stock market performance of firms subject to such agreements relative to their peers (matched controls) and relative to firms subject to prosecution. We find that DPA firms have lower buy-and-hold returns compared to a matched sample of control firms. Prosecuted firms, on the other hand, do not exhibit lower returns compared to a matched sample of control firms. Comparing the two groups of firms subject to different disciplining mechanism directly, DPA and prosecuted firms, we find that, on average, firms subject to DPAs perform worse in the post-period relative to the prosecuted sample. Such firms experience significantly lower buy-and hold-returns in the one to three years following the event.

Further, we examine the claim that DPAs prevent collateral damages to innocent parties who are not responsible for the crimes committed by the organization. We find no evidence that the agreements protect firms' stakeholders from negative consequences. In fact, our results document negative real consequences accruing to the stakeholders of DPA firms in the post-DPA period relative to control firms, as well as relative to the prosecuted firms, as measured by the changes in both sales levels and the number of employees.

Overall, our results do not lend credence to the notion that DPAs are associated with superior market performance or less collateral damage to other stakeholders. By documenting these findings, our study sheds light on the controversial debate regarding the merits of shielding firms from prosecution by providing early empirical evidence on this subject and may be able to provide insights to regulators in other countries considering introducing these or similar agreements to deal with corporate crime.

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Appendix
Variable Descriptions

<i>DPA_t</i>	An indicator variable equal to 1 for firms that enter into either a non-prosecution or deferred prosecution agreement in year t and 0 otherwise.
<i>Prosecuted_t</i>	An indicator variable equal to 1 for firms that are prosecuted by the government in year t and 0 otherwise.
<i>BHAR_{t+n}</i>	A firm's market-adjusted buy-and-hold abnormal return cumulated from the beginning of event year year t to the end of year $t+n$, calculated as $e^{\sum_{m=1}^{(t+n) \times m} \ln(1+ret)} - e^{\sum_{m=1}^{(t+n) \times m} \ln(1+vwretd)}$ where m is the month in year $t+n$ and ret and $vwretd$ are the monthly return and value-weighted market returns, respectively, obtained from the CRSP monthly stock file.
<i>ΔSales_{t+n}</i>	The percentage change in annual sales levels (<i>sale</i>), measured as the difference in sales measured in year $t+n$ less sales in year $t-1$ scaled by sales in year $t-1$.
<i>ΔEmp_{t+n}</i>	The percentage change in employees (<i>emp</i>), measured as the difference in employees measured in year $t+n$ less employees measured in year $t-1$ scaled by employees in year $t-1$.
<i>ΔTA_{t+n}</i>	The percentage change in total assets (<i>at</i>), measured as the difference in total assets measured in year $t+n$ less total assets measured in year $t-1$ scaled by total assets in year $t-1$.
<i>Size_{t-1}</i>	The natural log of a firm's market value of equity ($csho \times prcc_f$) measured in the year prior to the event year.
<i>MB_{t-1}</i>	The firm's market value of equity divided by the book value of equity measured in the year prior to the event year ($csho \times prcc_f / ceq$).
<i>PPE_{t-1}</i>	Property, plant, and equipment (<i>ppegt</i>) measured in the year prior to the event year scaled by lagged assets (<i>at</i>).
<i>Sales Growth_{t-1}</i>	Change in sales (<i>sale</i>) in the year prior to the event year scaled by lagged sales.

<i>ROA_{t-1}</i>	Net income (<i>ni</i>) measured in the year prior to the event year scaled by lagged assets (<i>at</i>).
<i>Return_{t-1}</i>	The annualized return (<i>ret</i>) measured in the year prior to the event year.
<i>Return Std_{t-1}</i>	The standard deviation of returns (<i>ret</i>) measured in the year prior to the event year.
<i>Equity Issue_{t-1}</i>	The firm's sale of common and preferred stocks (<i>sstk</i>) scaled by lagged assets (<i>at</i>).
<i>Going Concern_{t-1}</i>	Indicator variable equal to 1 if a firm receives a going concern opinion in the year prior to the event year.
<i>Coverage_{t-1}</i>	A firm's analyst coverage measured in the year prior to the event year.

Figure 1: Market-Adjusted Buy-and-Hold Abnormal Returns

This figure plots the buy-and-hold abnormal returns (*BHAR*) from the beginning of the event year t to the end of the third year after year t for DPA firms (shown by the solid red line) and prosecuted firms (shown by the blue dashed line).

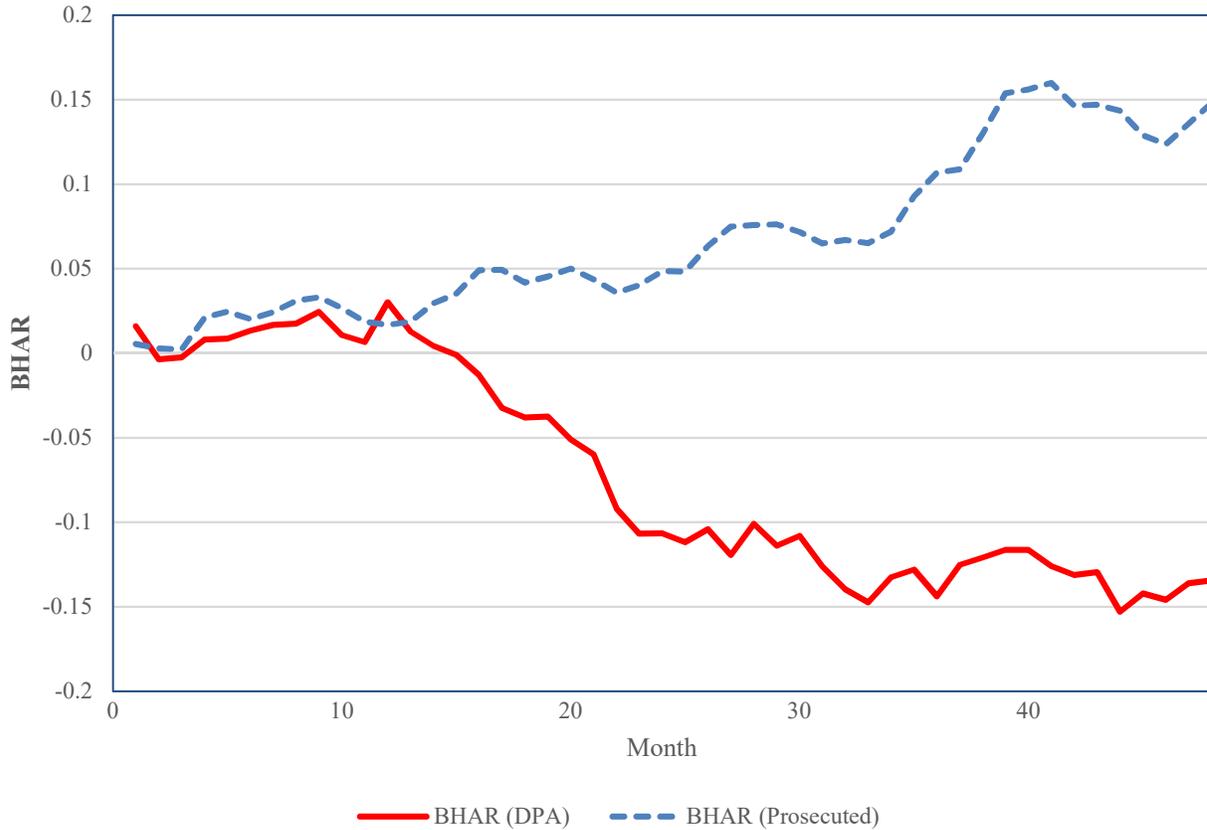


Table 1: Sample Construction

This table presents the construction of the final DPA sample.

DPA's identified through FOIA requests	467
Less: Firms with unavailable CRSP price data	283
Less: Firms missing full set of control variables	<u>174</u>
Final sample of DPA firms	109

Table 2: Description of DPA Firms

This table describes our sample by industry, primary offense, and agreement terms. Panel A presents the number of DPAs by industry. Panel B presents the number DPAs by primary offense. Panel C presents the number of DPAs by binary agreement terms we hand collected. Panel D presents the number of DPAs by continuous agreement terms.

Panel A: By Industry			
	<i>N</i>		
Mining (10-14)	8		
Construction (15-17)	2		
Manufacturing (20-39)	49		
Transportation, Communications, Electric, Gas and Sanitary Service (40-49)	7		
Wholesale Trade (50-51)	5		
Retail Trade (52-59)	2		
Finance, Insurance and Real Estate (60-67)	28		
Services (70-89)	6		
Other (99)	2		
Total	109		

Panel B: By Primary Offense			
	<i>N</i>		
Antitrust	4		
Environmental	1		
FCPA/Bribery/Kickbacks	46		
Food and Drug	9		
Fraud	44		
Labor	2		
Other	2		
Safety	1		
Total	109		

Panel C: By Agreement Terms (Binary)			
	<i>Independent Monitor</i>	<i>Pre-Agreement Compliance</i>	<i>Compliance Program</i>
Yes	37	21	23
No	72	88	86
Total	109	109	109

Panel D: Agreement Terms (Continuous)		
	<i>Probationary Period (Years)</i>	<i>Monetary Fine (\$millions)</i>
Mean	3.62	\$96.16
Min	1.00	\$0
25th Percentile	3.00	\$1.10
Median	3.00	\$13.44
75th Percentile	5.00	\$50.00
Max	7.00	\$1,700.00

Table 3: Sample Characteristics

This table presents a comparison of firm characteristics as of the most recent fiscal year-end before the DPA or prosecution. We provide results for separate control samples consisting of firms that do not enter into a DPA or who are not prosecuted by the government that are closest in size to the DPA or prosecuted firm in the same 2-digit SIC industry. All variables are defined in the Appendix. Significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

	DPA				Prosecuted				DPA - Prosecuted
	N (i)	Treat (ii)	Control (iii)	Diff (iv)	N (v)	Treat (vi)	Control (vii)	Diff (viii)	Diff (ix)
<i>Size_{t-1}</i>	109	9.423	9.357	0.066	496	8.711	8.541	0.170	0.712***
<i>MB_{t-1}</i>	109	2.753	3.082	-0.329	496	3.515	4.080	-0.565*	-0.762*
<i>PPE_{t-1}</i>	109	0.180	0.244	-0.064*	496	0.269	0.312	-0.043**	-0.089***
<i>Sales Growth_{t-1}</i>	109	0.044	0.096	-0.052*	496	0.093	0.150	-0.057***	-0.049**
<i>ROA_{t-1}</i>	109	0.037	0.044	-0.007	496	0.053	0.052	0.001	-0.016
<i>Return_{t-1}</i>	109	0.130	0.153	-0.023	496	0.179	0.284	-0.105**	-0.049
<i>Return Std_{t-1}</i>	109	0.101	0.089	0.012	496	0.109	0.112	-0.003	-0.008
<i>Equity Issue_{t-1}</i>	109	0.012	0.016	-0.004	496	0.021	0.031	-0.010*	-0.009
<i>Going Concern_{t-1}</i>	109	0.009	0.009	0.000	496	0.012	0.006	0.006	-0.003
<i>Coverage_{t-1}</i>	109	18.266	17.339	0.927	496	18.452	13.938	4.514***	-0.186

Table 4: Univariate Tests

This table presents univariate tests of *BHAR*. Column (i) presents the mean *BHAR* for 109 DPA firms and tests whether it differs from zero. Column (ii) presents the mean *BHAR* for 496 prosecuted firms and tests whether it differs from zero. Column (iii) presents tests comparing the mean *BHAR* for DPA firms to the mean *BHAR* for prosecuted firms. Significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

	DPA Firms (i)	Prosecuted Firms (ii)	DPA – Prosecuted Firms (iii)
<i>BHAR_t</i>	0.030	-0.002	0.032
<i>BHAR_{t+1}</i>	-0.106***	0.032	-0.138**
<i>BHAR_{t+2}</i>	-0.143**	0.080**	-0.223***
<i>BHAR_{t+3}</i>	-0.141*	0.117***	-0.258***

Table 5: BHAR

This table presents results of our tests of H1. In columns (i) through (iv), DPA firms are benchmarked against a matched control sample. In columns (v) through (viii), prosecuted firms are benchmarked against a matched control sample. In columns (ix) through (xii), DPA firms are benchmarked against prosecuted firms. All tests are two-sided. Standard errors are clustered at the firm level. All variables are defined in the Appendix. Significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

	DPA vs. Control Firms				Prosecuted vs. Control Firms				DPA vs. Prosecuted			
	$BHAR_t$	$BHAR_{t+1}$	$BHAR_{t+2}$	$BHAR_{t+3}$	$BHAR_t$	$BHAR_{t+1}$	$BHAR_{t+2}$	$BHAR_{t+3}$	$BHAR_t$	$BHAR_{t+1}$	$BHAR_{t+2}$	$BHAR_{t+3}$
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)	(x)	(xi)	(xii)
DPA_t	-0.009 (0.864)	-0.148* (0.100)	-0.144 (0.117)	-0.174 (0.172)					0.031 (0.355)	-0.155*** (0.001)	-0.205*** (0.001)	-0.214** (0.019)
$Prosecuted_t$					-0.016 (0.557)	0.003 (0.939)	0.064 (0.157)	0.093 (0.113)				
$Size_{t-1}$	-0.029 (0.121)	-0.003 (0.903)	-0.031 (0.322)	-0.060 (0.183)	-0.013 (0.110)	-0.028** (0.037)	-0.029* (0.064)	-0.047** (0.023)	-0.022* (0.084)	-0.011 (0.592)	-0.028 (0.238)	-0.046 (0.162)
MB_{t-1}	0.010 (0.306)	-0.005 (0.715)	-0.016 (0.264)	-0.025 (0.251)	-0.003 (0.453)	-0.001 (0.931)	-0.007 (0.232)	-0.005 (0.550)	0.003 (0.471)	-0.004 (0.586)	-0.010 (0.232)	-0.011 (0.326)
PPE_{t-1}	0.075 (0.506)	-0.113 (0.446)	-0.284 (0.105)	-0.340 (0.146)	0.057 (0.292)	0.063 (0.430)	0.127 (0.161)	0.180 (0.130)	0.085 (0.278)	-0.084 (0.478)	-0.082 (0.545)	-0.074 (0.685)
$Sales Growth_{t-1}$	0.106 (0.569)	0.233 (0.532)	0.151 (0.614)	0.200 (0.652)	-0.046 (0.496)	-0.084 (0.355)	0.063 (0.529)	0.043 (0.724)	-0.049 (0.585)	-0.086 (0.459)	0.058 (0.692)	0.033 (0.865)
ROA_{t-1}	-0.254 (0.482)	-0.531 (0.509)	0.203 (0.754)	0.214 (0.818)	0.204 (0.301)	0.209 (0.399)	0.463* (0.055)	0.471 (0.140)	0.223 (0.468)	0.770*** (0.006)	1.158*** (0.001)	1.112** (0.030)
$Return_{t-1}$	-0.055 (0.619)	-0.048 (0.709)	-0.041 (0.800)	-0.068 (0.803)	-0.072** (0.014)	-0.123*** (0.001)	-0.124*** (0.003)	-0.151*** (0.009)	-0.098** (0.014)	-0.120** (0.011)	-0.169*** (0.002)	-0.173* (0.064)
$Return Std_{t-1}$	2.740*** (0.001)	1.531 (0.224)	0.975 (0.510)	0.689 (0.707)	0.863** (0.023)	0.526 (0.337)	0.824 (0.189)	1.373* (0.092)	1.292*** (0.001)	1.432** (0.026)	1.875** (0.026)	2.339** (0.032)
$Equity Issue_{t-1}$	-1.718*** (0.002)	-1.734* (0.071)	-1.235 (0.121)	-1.212 (0.355)	-0.172 (0.481)	-0.118 (0.771)	-0.498* (0.097)	-1.135*** (< 0.001)	-0.363 (0.116)	-0.702** (0.026)	-0.904*** (0.004)	-1.202*** (0.007)
$Going Concern_{t-1}$	-0.348** (0.048)	-0.268 (0.379)	-0.449 (0.143)	-0.825* (0.057)	-0.038 (0.818)	-0.207 (0.337)	-0.215 (0.567)	0.029 (0.962)	-0.266* (0.054)	-0.366 (0.150)	-0.421 (0.380)	-0.266 (0.751)
$Coverage_{t-1}$	0.004 (0.104)	0.006 (0.122)	0.010*** (0.008)	0.014*** (0.009)	0.001 (0.570)	0.001 (0.666)	-0.001 (0.607)	0.000 (0.932)	0.003 (0.186)	0.001 (0.842)	0.001 (0.754)	0.003 (0.524)
Constant	0.008 (0.968)	-0.145 (0.682)	0.007 (0.984)	0.321 (0.522)	-0.325*** (0.003)	-0.300* (0.071)	-0.034 (0.853)	0.009 (0.968)	-0.414*** (0.002)	-0.482** (0.040)	-0.062 (0.808)	0.049 (0.876)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	218	218	218	218	992	992	992	992	605	605	605	605
Adj. R^2	0.150	0.002	0.005	0.012	0.082	0.075	0.034	0.042	0.162	0.102	0.069	0.050

Table 6: Change in Sales

This table presents results of our tests of H2, examining the change in sales levels. In columns (i) through (iv), DPA firms are benchmarked against a matched control sample. In columns (v) through (viii), prosecuted firms are benchmarked against a matched control sample. In columns (ix) through (xii), DPA firms are benchmarked against prosecuted firms. All tests are two-sided. Standard errors are clustered at the firm level. All variables are defined in the Appendix. Significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

	DPA vs. Control Firms				Prosecuted vs. Control Firms				DPA vs. Prosecuted			
	$\Delta Sales_t$	$\Delta Sales_{t+1}$	$\Delta Sales_{t+2}$	$\Delta Sales_{t+3}$	$\Delta Sales_t$	$\Delta Sales_{t+1}$	$\Delta Sales_{t+2}$	$\Delta Sales_{t+3}$	$\Delta Sales_t$	$\Delta Sales_{t+1}$	$\Delta Sales_{t+2}$	$\Delta Sales_{t+3}$
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)	(x)	(xi)	(xii)
DPA_t	-0.059*	-0.108**	-0.214***	-0.237***					0.002	-0.041	-0.105**	-0.112**
	(0.067)	(0.030)	(0.002)	(0.003)					(0.941)	(0.219)	(0.023)	(0.034)
$Prosecuted_t$					0.013	-0.010	-0.017	-0.025				
					(0.454)	(0.735)	(0.691)	(0.625)				
$Size_{t-1}$	-0.011	-0.023	-0.040*	-0.034	0.002	-0.004	-0.011	-0.017	-0.005	-0.006	-0.025*	-0.038**
	(0.306)	(0.121)	(0.070)	(0.149)	(0.730)	(0.628)	(0.419)	(0.323)	(0.399)	(0.530)	(0.084)	(0.034)
MB_{t-1}	-0.008	0.000	-0.001	-0.003	0.003	0.010	0.021**	0.026**	0.003	0.009	0.012	0.016
	(0.324)	(0.974)	(0.932)	(0.869)	(0.247)	(0.118)	(0.049)	(0.037)	(0.480)	(0.257)	(0.365)	(0.299)
PPE_{t-1}	0.110	0.303**	0.169	0.253	0.087**	0.097	0.068	0.119	0.213***	0.324***	0.298**	0.266
	(0.136)	(0.015)	(0.399)	(0.316)	(0.049)	(0.188)	(0.553)	(0.376)	(0.002)	(0.002)	(0.049)	(0.125)
$Sales Growth_{t-1}$	0.243	0.218	0.291	0.189	0.187***	0.315**	0.498**	0.694***	0.090	0.182	0.260	0.260
	(0.210)	(0.364)	(0.345)	(0.568)	(0.002)	(0.020)	(0.035)	(0.008)	(0.280)	(0.186)	(0.161)	(0.186)
ROA_{t-1}	-0.512	-0.660	-0.685	-0.323	0.064	-0.329	-0.785*	-0.560	0.206	0.302	0.269	0.768***
	(0.222)	(0.179)	(0.333)	(0.673)	(0.653)	(0.277)	(0.097)	(0.279)	(0.385)	(0.419)	(0.493)	(0.008)
$Return_{t-1}$	0.116**	0.130*	0.186*	0.223*	0.057***	0.087**	0.082*	0.088	0.082***	0.048	0.054	0.018
	(0.047)	(0.095)	(0.058)	(0.081)	(0.008)	(0.013)	(0.084)	(0.107)	(0.002)	(0.312)	(0.364)	(0.795)
$Return Std_{t-1}$	-0.254	-0.816	-1.003	-0.547	-0.025	-0.815***	-1.313***	-1.258**	0.075	-0.238	-0.447	-0.428
	(0.606)	(0.229)	(0.262)	(0.571)	(0.912)	(0.011)	(0.004)	(0.015)	(0.784)	(0.486)	(0.314)	(0.420)
$Equity Issue_{t-1}$	1.228*	0.632	1.087	1.612	0.682***	0.628**	0.940**	1.564**	1.138***	0.614	0.406	1.831*
	(0.070)	(0.442)	(0.335)	(0.170)	(< 0.001)	(0.031)	(0.027)	(0.012)	(< 0.001)	(0.233)	(0.483)	(0.053)
$Going Concern_{t-1}$	-0.122	-0.273	-0.670***	-0.709***	-0.059	-0.127	-0.073	-0.264	-0.127	-0.143	-0.145	-0.313
	(0.470)	(0.125)	(< 0.001)	(< 0.001)	(0.747)	(0.522)	(0.765)	(0.225)	(0.561)	(0.508)	(0.539)	(0.267)
$Coverage_{t-1}$	0.000	0.002	0.004	0.005	0.000	0.001	0.000	0.000	0.001	0.003	0.006*	0.008**
	(0.933)	(0.451)	(0.230)	(0.151)	(0.835)	(0.763)	(0.979)	(0.988)	(0.519)	(0.292)	(0.093)	(0.048)
Constant	0.235**	0.453**	1.002***	1.268***	0.001	0.298**	0.992***	1.074***	-0.053	0.123	0.956***	0.983***
	(0.041)	(0.014)	(< 0.001)	(< 0.001)	(0.987)	(0.043)	(< 0.001)	(< 0.001)	(0.634)	(0.475)	(0.002)	(0.005)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	218	218	218	218	992	992	992	992	605	605	605	605
Adj. R^2	0.156	0.122	0.114	0.131	0.191	0.127	0.136	0.169	0.232	0.140	0.156	0.176

Table 7: Change in Employees

This table presents results of our tests of H2, examining the change in employee levels. In columns (i) through (iv), DPA firms are benchmarked against a matched control sample. In columns (v) through (viii), prosecuted firms are benchmarked against a matched control sample. In columns (ix) through (xii), DPA firms are benchmarked against prosecuted firms. All tests are two-sided. Standard errors are clustered at the firm level. All variables are defined in the Appendix. Significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

	DPA vs. Control Firms				Prosecuted vs. Control Firms				DPA vs. Prosecuted			
	ΔEmp_t	ΔEmp_{t+1}	ΔEmp_{t+2}	ΔEmp_{t+3}	ΔEmp_t	ΔEmp_{t+1}	ΔEmp_{t+2}	ΔEmp_{t+3}	ΔEmp_t	ΔEmp_{t+1}	ΔEmp_{t+2}	ΔEmp_{t+3}
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)	(x)	(xi)	(xii)
DPA_t	0.005 (0.832)	-0.039 (0.291)	-0.077* (0.073)	-0.108** (0.025)					-0.001 (0.943)	-0.016 (0.565)	-0.064* (0.059)	-0.110*** (0.007)
$Prosecuted_t$					0.029* (0.071)	-0.026 (0.363)	-0.040 (0.265)	-0.036 (0.379)				
$Size_{t-1}$	-0.010 (0.217)	-0.016 (0.151)	-0.013 (0.327)	-0.020 (0.188)	-0.003 (0.513)	-0.019** (0.019)	-0.027** (0.013)	-0.029** (0.026)	-0.009 (0.102)	-0.017* (0.055)	-0.028** (0.018)	-0.033** (0.022)
MB_{t-1}	0.013** (0.011)	0.018** (0.023)	0.018** (0.016)	0.012 (0.150)	0.004 (0.141)	0.011* (0.086)	0.017** (0.041)	0.022 (0.024)	0.006 (0.189)	0.011 (0.181)	0.018* (0.091)	0.021 (0.106)
PPE_{t-1}	0.064 (0.332)	0.024 (0.799)	0.023 (0.834)	-0.049 (0.667)	-0.051 (0.121)	-0.097 (0.125)	-0.144* (0.096)	-0.125 (0.200)	0.026 (0.637)	0.089 (0.301)	-0.024 (0.809)	-0.031 (0.777)
$Sales\ Growth_{t-1}$	0.101 (0.269)	0.127 (0.261)	0.260* (0.073)	0.298* (0.084)	0.152*** (0.000)	0.336*** (0.002)	0.537*** (0.001)	0.633*** (0.001)	0.185*** (0.006)	0.323*** (0.002)	0.436*** (< 0.001)	0.439*** (< 0.001)
ROA_{t-1}	0.280 (0.122)	0.491** (0.029)	0.546** (0.045)	0.814** (0.017)	0.097 (0.412)	-0.100 (0.722)	-0.100 (0.786)	0.342 (0.352)	-0.054 (0.783)	0.005 (0.989)	0.145 (0.682)	0.885*** (< 0.001)
$Return_{t-1}$	0.039 (0.407)	0.131* (0.075)	0.121 (0.121)	0.143* (0.090)	0.086*** (0.001)	0.101*** (0.002)	0.081** (0.025)	0.081* (0.065)	0.074** (0.015)	0.081* (0.088)	0.062 (0.203)	0.042 (0.485)
$Return\ Std_{t-1}$	0.364 (0.272)	0.258 (0.525)	0.092 (0.849)	0.095 (0.863)	-0.140 (0.321)	-0.657*** (0.007)	-0.851*** (0.009)	-0.664* (0.087)	-0.195 (0.228)	-0.267 (0.304)	-0.368 (0.227)	-0.097 (0.797)
$Equity\ Issue_{t-1}$	-0.243 (0.429)	-0.640 (0.106)	-0.218 (0.605)	-0.035 (0.943)	0.358** (0.014)	0.361 (0.114)	0.434 (0.170)	0.705** (0.087)	0.495** (0.049)	0.308 (0.301)	0.193 (0.618)	0.707 (0.192)
$Going\ Concern_{t-1}$	-0.284*** (< 0.001)	-0.382*** (0.002)	-0.602*** (< 0.001)	-0.659*** (< 0.001)	-0.129* (0.083)	-0.024 (0.863)	-0.088 (0.592)	-0.310* (0.050)	-0.190*** (0.009)	-0.098 (0.433)	-0.108 (0.483)	-0.295* (0.084)
$Coverage_{t-1}$	0.000 (0.959)	0.003 (0.169)	0.003 (0.202)	0.003 (0.144)	0.000 (0.712)	0.001 (0.658)	0.000 (0.974)	0.000 (0.948)	0.000 (0.813)	0.003 (0.187)	0.003 (0.257)	0.004 (0.166)
Constant	0.039 (0.618)	0.117 (0.343)	0.197 (0.164)	0.262 (0.113)	0.100 (0.169)	0.432*** (0.002)	0.708*** (< 0.001)	0.723*** (< 0.001)	0.217** (0.045)	0.224 (0.119)	0.468 (0.011)	0.505** (0.020)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	218	218	218	218	992	992	992	992	605	605	605	605
Adj. R^2	0.118	0.156	0.161	0.179	0.204	0.150	0.147	0.172	0.200	0.171	0.149	0.198

Table 8: Change in Assets

This table presents results of our tests H2, examining the change in total asset levels. In columns (i) through (iv), DPA firms are benchmarked against a matched control sample. In columns (v) through (viii), prosecuted firms are benchmarked against a matched control sample. In columns (ix) through (xii), DPA firms are benchmarked against prosecuted firms. All tests are two-sided. Standard errors are clustered at the firm level. All variables are defined in the Appendix. Significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

	DPA vs. Control Firms				Prosecuted vs. Control Firms				DPA vs. Prosecuted			
	ΔTA_t	ΔTA_{t+1}	ΔTA_{t+2}	ΔTA_{t+3}	ΔTA_t	ΔTA_{t+1}	ΔTA_{t+2}	ΔTA_{t+3}	ΔTA_t	ΔTA_{t+1}	ΔTA_{t+2}	ΔTA_{t+3}
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)	(x)	(xi)	(xii)
DPA_t	-0.003 (0.941)	-0.118** (0.047)	-0.161** (0.021)	-0.209** (0.013)					0.022 (0.536)	-0.036 (0.360)	-0.043 (0.309)	-0.068 (0.201)
$Prosecuted_t$					0.060*** (0.009)	-0.003 (0.930)	-0.035 (0.431)	-0.062 (0.284)				
$Size_{t-1}$	-0.013 (0.267)	-0.035* (0.066)	-0.036 (0.111)	-0.056** (0.035)	-0.002 (0.747)	-0.010 (0.333)	-0.021 (0.148)	-0.038** (0.041)	-0.010 (0.257)	-0.015 (0.254)	-0.035*** (0.042)	-0.058*** (0.009)
MB_{t-1}	0.019** (0.028)	0.030** (0.023)	0.030** (0.025)	0.026* (0.082)	0.011** (0.012)	0.018** (0.024)	0.023** (0.037)	0.033** (0.016)	0.011 (0.129)	0.018* (0.074)	0.018 (0.147)	0.019 (0.226)
PPE_{t-1}	0.165 (0.137)	0.131 (0.372)	0.121 (0.474)	0.094 (0.636)	0.000 (0.995)	-0.063 (0.382)	-0.090 (0.336)	-0.029 (0.803)	0.105 (0.241)	0.020 (0.856)	-0.100 (0.454)	-0.064 (0.685)
$Sales\ Growth_{t-1}$	0.122 (0.405)	0.104 (0.542)	0.239 (0.336)	0.152 (0.561)	0.107 (0.111)	0.235** (0.049)	0.351** (0.011)	0.533*** (0.001)	0.171* (0.093)	0.335*** (0.007)	0.415*** (0.003)	0.380** (0.023)
ROA_{t-1}	0.509* (0.070)	0.701* (0.070)	0.799 (0.106)	1.449*** (0.007)	0.251 (0.214)	0.134 (0.654)	0.498 (0.147)	0.833** (0.050)	0.395 (0.267)	0.631** (0.037)	1.209*** (< 0.001)	1.778*** (< 0.001)
$Return_{t-1}$	0.099 (0.170)	0.202* (0.063)	0.136 (0.195)	0.179 (0.144)	0.130*** (0.003)	0.160*** (< 0.001)	0.105* (0.050)	0.106 (0.116)	0.091** (0.020)	0.134*** (0.003)	0.086 (0.136)	0.057 (0.429)
$Return\ Std_{t-1}$	0.658 (0.234)	1.206* (0.091)	1.025 (0.197)	0.936 (0.273)	0.022 (0.926)	-0.387 (0.219)	-0.704* (0.057)	-0.485 (0.348)	-0.110 (0.718)	-0.093 (0.780)	-0.467 (0.201)	-0.208 (0.717)
$Equity\ Issue_{t-1}$	-0.655 (0.205)	-1.365** (0.024)	-0.800 (0.287)	-1.132 (0.124)	0.358 (0.100)	0.386 (0.345)	0.767 (0.149)	1.140* (0.085)	0.943*** (0.002)	0.024 (0.935)	0.265 (0.587)	0.400 (0.508)
$Going\ Concern_{t-1}$	-0.435*** (< 0.001)	-0.725*** (< 0.001)	-0.866*** (< 0.001)	-0.961*** (< 0.001)	-0.129 (0.536)	-0.173 (0.412)	-0.352* (0.085)	-0.475* (0.081)	0.073 (0.762)	0.070 (0.742)	-0.049 (0.817)	-0.120 (0.725)
$Coverage_{t-1}$	0.001 (0.593)	0.003 (0.185)	0.005* (0.051)	0.008** (0.012)	-0.001 (0.228)	0.000 (0.890)	0.001 (0.665)	0.004 (0.216)	0.001 (0.758)	0.002 (0.384)	0.006* (0.070)	0.011*** (0.007)
Constant	0.091 (0.377)	0.493** (0.025)	0.647** (0.010)	0.990*** (0.002)	0.164* (0.081)	0.521*** (0.001)	0.858*** (< 0.001)	1.189*** (< 0.001)	0.256* (0.091)	0.478*** (0.008)	0.903*** (< 0.001)	1.255*** (< 0.001)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	218	218	218	218	992	992	992	992	605	605	605	605
Adj. R^2	0.098	0.134	0.144	0.164	0.177	0.141	0.128	0.161	0.158	0.173	0.194	0.191