

# Greening Criminal Records: How Voluntary Emission-Reduction Targets Restore Corporate Reputation\*

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## Abstract

Why do firms commit to voluntarily reduce greenhouse gas emissions? Existing research explains self-regulation as an effort to restore legitimacy after environmental misbehavior, yet these violations rarely surface. We argue that firms also adopt voluntary emission reduction targets (VERTs) following revelations of more frequent *non-environmental* misconducts that trigger reputational spillovers across responsibility domains. Focusing on financial criminal violations, we theorize that these scandals damage firms' *environmental* reputation, heightening vulnerability to public backlash. To mitigate this risk, firms adopt VERTs—but opt for modest, risk-averse targets. Difference-in-differences models on 770 US-traded firms show that financial scandals increase (less ambitious) VERT adoption by 35%. A survey experiment with 1,752 US respondents supports the reputational spillover mechanism. Our findings show that reputational crises beyond the environmental realm open windows for environmental self-regulation, but resulting commitments might not align with public de-carbonization efforts as they prioritize image repair over ambitious climate action.

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Firms' greenhouse gas (GHG) emissions are major drivers of climate change. A 2017 Carbon Disclosure Project report attributed about 70% of global emissions since 1988 to just 100 firms.<sup>1</sup> Ensuring that this small group of "Carbon Majors" reduces emissions is essential for climate policy. Both public and private regulatory efforts have sought to curb corporate emissions (Bayer, 2023; Büthe, 2010; Lerner and Osgood, 2022; Potoski and Prakash, 2005). In particular, private—or self—regulation via voluntary emission-reduction targets (VERTs) has become a central feature of climate governance since the 2010s (Green, 2013), especially after the Paris Agreement (Green, Hale, and Arceo, 2024).

Why do firms self-regulate? Understanding the motivations for self-regulation is critical for assessing whether private initiatives can align with public decarbonization efforts—especially given that the costs of public regulation often hinder its political feasibility (Gaikwad, Genovese, and Tingley, 2022; Colantone et al., 2024), making self-regulation an appealing alternative. A prominent view explains self-regulation as a response to negative information about firms' environmental performance: firms self-regulate to rebuild public legitimacy (Distelhorst and Locke, 2018; Thrall, 2021) and meet public demands for regulation (Malhotra, Monin, and Tomz, 2019). However, information about firms' environmental misbehavior is normally scarce (Smeuninx, De Clerck, and Aerts, 2020), surfacing usually in times of rare environmental scandals (McGuire, Holtmaat, and Prakash, 2022).

We argue that firms also adopt environmental self-regulation in response to negative information that surfaces in (more common)<sup>2</sup> times of *non-environmental* scandals. We introduce a spillover theory of corporate reputation demonstrating how misconduct in one corporate social responsibility (CSR) area—e.g., violations of corruption or labor rights standards—can shape perceptions in other—e.g., environmental stewardship. Such reputational spillovers create public backlash, leading to negative environmental perceptions (reputation) of the firms responsible for non-environmental misconduct. Expectation of backlash prompts these firms to commit to environmental self-regulation (e.g., VERTs) to signal responsibility.

We focus, in particular, on financial criminal scandals, which are highly salient and come with high media scrutiny and negative publicity—see Culpepper, Jung, and Lee (2022) and our media analysis in

<sup>1</sup> Including public and state-owned firms. CDP report is available at: <https://cdn.cdp.net/cdp-production/cms/reports/documents/000/002/327/original/Carbon-Majors-Report-2017.pdf?1501833772>. For the list of "Carbon Majors," see: <https://carbonmajors.org/Entities>.

<sup>2</sup> Data from Garrett and Ashley (2019) indicate that only 1 in 10 corporate criminal violations by US publicly-listed firms pertain to environmental laws (e.g., the Clean Air Act). The vast majority of corporate criminal violations that surface and that the public is exposed to, instead, concern non-environmental violations: financial crimes like corruption, fraud, tax evasion, or money laundering. Importantly, among the US firms who committed such *non-environmental* violations are large GHG emitters and "Carbon Majors": Chevron, ExxonMobil, ConocoPhillips, Peabody Energy, Anglo American Aviation, Occidental Petroleum, Marathon Oil, or Chesapeake Energy.

Supplemental Information (SI) Figure A.4. Our spillover theory of corporate reputation posits that, when negative non-environmental information surfaces through a firm’s involvement in a financial criminal scandal, the public employs availability heuristics (Tversky and Kahneman, 1973) and forms inferences about its *environmental* reputation, too. The reasoning is as simple as “if a company is performing poorly with respect to one social aspect (e.g., corruption), it is likely doing as bad in environmental terms.” In other words, the negative reputation created by a non-environmental crisis spills over to affect perceptions of the company’s environmental stewardship. The firm is concerned about such reputational damage because it can lead the public (e.g., consumers, activists, or investors) to punish perceived non-compliance with organized protests, advocacy campaigns, boycotts (Endres and Panagopoulos, 2017; Kam and Deichert, 2020).

When a firm is under such multidimensional reputational crises, VERTs offer reputational advantages: they demonstrate the firm’s “good character” and reduce opposition, all while mitigating any demands for public regulation (Kolcava, Rudolph, and Bernauer, 2021). The type of VERTs adopted, also, matters. When in reputational distress, the firm needs to respond to public demands while minimizing the risk of renewed backlash if it fails to meet its commitments (Tingley and Tomz, 2022). Consequently, the firm tends to set more modest and *controllable* targets, which are easier to achieve while still signalling responsiveness to public demands (Malhotra, Monin, and Tomz, 2019). Controllable VERTs do require the firm to change behavior but they are, also, typically less ambitious. Our expectation is that non-environmental scandals should increase the adoption of VERTs, particularly more controllable ones. The mechanism is one of reputational spillovers: non-environmental scandals should undermine individuals’ *environmental* reputation of a firm.

We provide observational and experimental evidence for these expectations. Observationally, we model firms’ adoption of VERTs after their involvement in (non-environmental) financial corporate criminal scandals with a difference-in-differences design on a sample of 770 publicly-traded US-based firms that participated in the Carbon Disclosure Project (CDP) between 2010 and 2019.<sup>3</sup> We show that the average CDP firm increases the number of adopted VERTs by 35% after being involved in a financial scandal with *no direct environmental implication*. This effect is largely concentrated among more controllable VERTs.

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<sup>3</sup> CDP covers more than 30% of the US-based “Carbon Majors” responsible for a significant share of global GHG emissions since the late 1980s—including Chevron (globally, 4th largest historical emitter), ExxonMobil (5th), Marathon, ConocoPhillips, and Occidental Petroleum.

A pre-registered online survey experiment<sup>4</sup> administered to 1,752 US-based respondents adds evidence for our proposed mechanism—the reputational spillover effect. We use vignettes on a hypothetical company to manipulate our theoretical conditions and measure respondents’ perception of the company’s overall and environmental profile. Consistently with our reputational spillover theory, the perception of the firm’s *environmental* profile is about 27% worse among respondents exposed to a corruption-scandal vignette (which included *no* environmental information). A VERT vignette partly restores this drop in corporate reputation, mitigating the damage induced by the scandal.

We draw two implications for climate policy. First, we show that times of reputational distress, due to salient negative information, can become regulatory windows of opportunity. In such times, civil society can intensify its demands for sustainability and call on companies to strengthen their climate commitments, as firms are responsive to negative reputational spillovers. Second, when in reputational distress, voluntary sustainability efforts are tactical moves that serve primarily reputational management purposes. Without regulatory oversight, there is a risk that firms adopt easier initiatives—a practice some may view as greenwashing. Put differently, while reputational crises can induce self-regulation and align with public decarbonization goals, self-regulation can be insufficient to mitigate climate change if companies favor initiatives that are publicly acceptable but controllable and less ambitious.

We contribute to three scholarly literatures. First, we contribute to the study of environmental self-regulation. Prior research has proposed several explanations for why firms adopt environmental initiatives, including regulatory pressure ([Hsueh, 2019](#)), the imposition of adjustment costs on competitors ([Kennard, 2020](#)), political access ([Werner, 2015](#)), regulatory pre-emption ([Malhotra, Monin, and Tomz, 2019](#)), and internal firm characteristics ([Lerner and Osgood, 2022](#)). We extend this literature and highlight that firms *also* self-regulate as a way to manage their reputation. Importantly, we find evidence for it before 2019, i.e. when emission-reduction targets were growing but less common than today.<sup>5</sup>

Specifically, we find large and robust effects of non-environmental criminal scandals—about a 35% increase in VERTs—that exceed those associated with other factors such as internal characteristics, regulatory pressure, and regulatory pre-emption. For example, [Lerner and Osgood \(2022\)](#) showed that a firm’s likelihood of reporting to the CDP rises by 1.4 percentage points when the number of self-regulating firms in the same board member network increases from zero to one. [Hsueh \(2019\)](#) found

<sup>4</sup> Anonymous pre-registration is accessible at: [https://osf.io/xvrn3/?view\\_only=e79862fef77643278979887cae95b2b9](https://osf.io/xvrn3/?view_only=e79862fef77643278979887cae95b2b9). Pre-registration: July 15, 2024, some minor updates introduced on July 17, 2024. The survey was fielded on August 6, 2024.

<sup>5</sup> Green, Hale, and Arceo ([2024](#), Figure 1b) find that Fortune 500 companies began adopting net-zero targets at a much higher rate only after 2020 (i.e., after the end of our data).

that the introduction of the Clean Power Plan in the US increased the likelihood that a company would participate in carbon disclosure by 10%. Finally, unlike regulatory pre-emption which is effective *only* when nearly all firms within an industry participate (Malhotra, Monin, and Tomz, 2019), the reputational effect we illustrate operates at the individual firm level. Empirically, moreover, we advance the study of environmental self-regulation by considering target-setting behavior, following a growing interest by political scientists (e.g., Green, Hale, and Arceo, 2024; Rowan, 2025; Tingley and Tomz, 2022). This moves us past prior studies that have typically used a binary outcome variable—indicating whether a company chooses to disclose information or participate in self-regulatory standards (Hsueh, 2019; Lerner and Osgood, 2022; Prakash and Potoski, 2006).

Second, our paper contributes to research on corporate environmental behavior in international political economy (Bayer, 2023; Colgan, Green, and Hale, 2021; Cory, Lerner, and Osgood, 2021). By revealing how stakeholders link issues across regimes, we explain why firms adopt practices that appear unrelated to the original controversy. This connects work on environmental politics with studies of other regimes (e.g., Findley, Nielson, and Sharman, 2015; Crippa, 2023; Jensen and Malesky, 2018). A key contribution, in this regard, is the insight that firms are simultaneously subject to various (international) regulatory regimes which are less isolated, in practice, than scholars sometimes make them, with consequences for the possibility of fostering self-regulation across domains.

Third, our work contributes to research beyond corporate environmental behavior. Our reputational spillover theory can explain not only why firms adopt *environmental* self-regulation but also why they embrace self-regulation on other CSR issues such as human rights, community engagement, and labor rights (Bright et al., 2020; Distelhorst and Locke, 2018; Malesky and Mosley, 2018; Thrall, 2021). While this paper focuses on environmental responses to non-environmental negative reputational events, the same logic could apply more broadly: reputational harm in any domain can trigger firms to adjust their behavior across multiple, seemingly unrelated CSR areas (provided they are salient and advertizable).

The argument also potentially extends beyond firms, e.g. to political parties or policymakers, whose reputations can be likewise shaped by spillover effects (Leininger and Rudolph, 2025; Schönhage and Geys, 2023; Wolsky, 2022) and whose stakeholders (voters) implement similar heuristics to those we study here (Fortunato and Stevenson, 2013; Duch, Przepiorka, and Stevenson, 2015). By linking public opinion, reputational concerns, and cross-domain accountability, we highlight that negative reputational events might spill over beyond the immediate damage and drive holistic strategies in response.

## **Public demands for environmental self-regulation**

Firms are subject to demands for private and public regulation to reduce their carbon footprint. Private (or self-) regulation complements public regulation as a form of “soft law” ([Vogel, 2008](#)) that is not meant “to replace states, but to embed systems of governance in broader frameworks of social capacity and agency that did not previously exist” ([Ruggie, 2004](#), 519). Various stakeholders—*e.g.*, citizens, consumers investors, and advocacy groups—advocate for private and public regulation to minimize corporations’ negative impact on the environment ([Baron, 2014](#)).

The public has several ways to put pressure on firms and demand regulation. Strategies like demonstrations, advocacy campaigns, “naming and shaming,” and consumer boycotts ([Distelhorst and Locke, 2018](#); [Endres and Panagopoulos, 2017](#); [Kam and Deichert, 2020](#)) bring attention to the need for climate initiatives, self-regulatory or not. Demands for self-regulation are particularly strong when public policies are perceived as absent or inadequate ([Potoski and Prakash, 2005](#)). It is in firms’ best interest to proactively respond to such requests and self-regulate: doing so, even with more controllable and less ambitious initiatives, mitigates demand for (possibly more burdensome, from firms’ perspective) public regulations ([Malhotra, Monin, and Tomz, 2019](#); [Kolcava, Rudolph, and Bernauer, 2021](#)).

However, the public typically demands regulation only when possessing information about firms’ misbehavior ([Culpepper, Jung, and Lee, 2022](#)); information which is, in normal times, limited. Firms are complex organizations. Illicit, socially undesirable, or otherwise negative consequences of corporate activities typically occur out of public sight. Legal mandates to share information about misconduct—*e.g.*, via annual reports ([Smeuninx, De Clerck, and Aerts, 2020](#))—does not easily reach citizens even when companies attend to them. In other words, in normal times there is asymmetry of information about corporate environmental misbehavior between the public and firms ([Kulkarni, 2000](#)).

Such information asymmetry shifts in times of negative events that break the news. In these times of crisis, information on a company’s misbehavior becomes public. Consider environmental scandals. In these situations, information about a firm’s misconduct becomes widely available as public scrutiny intensifies ([Daudigeos, Roulet, and Valiorgue, 2020](#); [Li et al., 2018](#)). Such new information prompts demands for environmental regulatory actions, which the firm has an incentive to meet ([Thrall, 2021](#)). We argue that not only negative environmental events provide the public with information necessary to act, but also *non-environmental* ones.

## Heuristics and negative reputation spillovers

We argue that the public uses *non-environmental* negative information as a heuristic to predict the likelihood of a (normally) less observable behavior: *environmental* misconduct. Heuristics are mental shortcuts used to formulate expectations on the likelihood of unobserved events and behaviors. Availability heuristics, in particular, use available information—e.g., an observed non-environmental misconduct—to predict the likelihood of unobserved behaviors—e.g., environmental misconduct (Tversky and Kahneman, 1973). People use availability heuristics to estimate probabilities and formulate judgments by the ease with which related instances or associations can be brought to mind. Such flow of information is enabled by “associative proximity” (Schwarz et al., 1991): the (perceived) likelihood that the missing information co-occurs with the available one. The greater the associative proximity, the easier (and more accurate) the application of heuristics.

Available information about a company’s poor performance in other CSR areas enables people to guess that the firm is environmentally bad because CSR areas are associatively proximate. All areas of sustainability, in fact, relate to the firm’s engagement with goals beyond profit-seeking and refer to its impact on communities, governance practices, and the environment (Ruggie, 2013). Firms, themselves, often advertise their sustainable practices simultaneously across a range of environmental, social, and governance standards.<sup>6</sup> Even when a firm violates sustainability standards, it tends to infringe principles across multiple areas at the same time, as exemplified by the prevalence of corruption in the high-emission oil industry (Mahdavi, 2020). Because of such associative proximity, CSR violations negatively spill over to a firm’s environmental reputation. This reduces information asymmetry about the likelihood of the firm’s environmental misconduct, not because the public directly observes it, but because heuristics make up for lack of data and lead the public to think less of the firm, environmentally.

We consider one type of particularly negative non-environmental information that is likely to feed availability heuristics: corporate criminal scandals. In these scandals, a firm is found to violate laws against criminal behavior like corruption, fraud, or money laundering. These crimes are generally seen negatively (St-Georges et al., 2023) and they can even be predicate for particularly salient nefarious behavior (e.g., terrorism financing, see Findley, Nielson, and Sharman, 2015).

Unsurprisingly, the media largely cover these misconducts which contravene socially acceptable standards of behaviors, rendering the scandals highly salient (Culpepper, Jung, and Lee, 2022; Carberry,

<sup>6</sup> Consider Patagonia’s “footprint” webpage: <https://www.patagonia.com/our-footprint/>; or ExxonMobil’s sustainability reports: <https://corporate.exxonmobil.com/sustainability-and-reports/sustainability>.

Engelen, and Van Essen, 2018; Kepplinger, Geiss, and Siebert, 2012).<sup>7</sup> With intense scrutiny comes public disapproval of the involved firm (Clemente and Gabbioneta, 2017; Miller, 2006). Such salient and negatively charged information easily reaches the public, feeding heuristics in the manner described above and damaging a firm's social reputation, including its environmental image.

In such times of crises, the public uses heuristics to infer about the likelihood of environmental misbehavior, temporarily overcoming the condition of information asymmetry, and demanding action. Citizens' campaigns and boycotts do not only address the exposed misconduct (a *direct* effect),<sup>8</sup> but also call on the firm for initiatives that demonstrate sustainability across the board (a *spillover* effect induced by negative information, see Kam and Deichert, 2020). In other words, a reputational crisis fuels demands for comprehensive reforms that address the company's overall ethical, social, and environmental conduct.

This pattern, whereby scandals generate publicity that negatively affects one's reputation beyond the specific misconduct, is likely not limited to corporations. It has been observed, for instance, that a scandal involving one politician can tarnish their entire party's image (Leininger and Rudolph, 2025; Schönhage and Geys, 2023; Wolsky, 2022). This occurs, more broadly, because voters use heuristics when adjudicating the behavior of a collective actor like a party (Fortunato and Stevenson, 2013; Duch, Przepiorka, and Stevenson, 2015). Similarly, companies have unified brands: misconduct in one area can trigger broader reputational damage, even in domains unrelated to the original offense.

## **VERT adoption in times of corporate criminal scandals**

We expect a firm to respond to public demands and increase adoption of environmental voluntary initiatives like VERTs, in response to such multi-dimensional reputational crises. After corporate scandals, a reputationally weak firm needs to take steps aimed at restoring public legitimacy, rebuilding trust, and mitigating the negative spillovers. This is because, following a scandal, the firm is under intense public scrutiny and public stakeholders closely watch its sustainability measures (Thrall, 2021). Although in this paper we focus on VERT adoption, our logic expects firms will also adopt sustainability measures in other domains besides environmental protection (e.g., labor rights, community initiatives).

Adopting VERTs is a strategic move that offers three key advantages. First, VERTs address public

<sup>7</sup> See also our own media analysis in SI Figure A.4.

<sup>8</sup> Research has largely shown that firms attempt to repair their reputation due to this direct damage, by improving their behavior in the affected domain (e.g., Distelhorst and Locke, 2018; Thrall, 2021). We take a step further and study *spillover* effects.

demand for sustainability (Potoski and Prakash, 2005) created by the negative reputational spillovers of a scandal. Second, VERTs reverse the heuristics-induced spillover logic: positive environmental information restores the firm's social reputation. VERTs thus act as a reservoir of goodwill that helps to cushion against (anticipated) reputational losses, leveraging the same availability heuristic that fuels reputational damage. Through VERTs, firms attempt to "cleanse" their record and persuade the public that any misconduct represents an exception to an otherwise positive track record. Third, VERTs offer an immediate reputational boost while committing to a goal set for a future date. Although achieving these targets requires real behavioral changes, the evaluation of success is deferred, allowing firms to reap image benefits in the short term. At the moment of the pledge, in fact, VERTs provide a clear and promotable benchmark.<sup>9</sup>

Examples of firms adopting these strategies in response to non-environmental scandals abound. Consider Nike. After public backlash for the use of sweatshops and appalling working conditions of garment workers, the company not only pledged to protect labor rights but also doubled down on its CSR pledges promoting a wider range of sustainable practices—including, pledging to reduce its carbon footprint by launching PVC-free trainers.<sup>10</sup> Similarly, the German conglomerate Siemens attempted to foster a "culture of integrity" following its infamous world-wide 2008 corruption scandal. It did so not only by restructuring its anti-corruption policy but also by strengthening its climate commitments and creating a brand new "Environmental Protection, Health Management, and Safety" unit with the power to draft climate pledges.<sup>11</sup>

Firms with a stronger public image might be more susceptible to the reputational spillovers of a scandal. Publicly traded or business-to-consumer companies—such as Nike, Nestlé, Siemens, or ExxonMobil—tend to be more exposed to brand value damage. They might, therefore, have a stronger incentive to mitigate public backlash. Nevertheless, "even global firms that do not market to consumers [...] value public approval and dislike negative media attention" (Vogel, 2010, 77).

The strategy to use VERTs in times of scandals may not resonate equally with all citizens, particu-

<sup>9</sup> Our emphasis on VERTs, motivated by their growing political relevance (Green, 2013), does not imply that firms avoid other voluntary environmental programs. However, VERTs are interesting as they are uniquely positioned to respond to public pressure while remaining relevant to broader climate change mitigation efforts.

<sup>10</sup> See: David Teather, "Nike lists abuses at Asian factories" *The Guardian*, April 14, 2005: <https://www.theguardian.com/business/2005/apr/14/ethicalbusiness.money>; Julia Day, "Nike launches 'green' trainers". *The Guardian*, January 25, 2002 <https://www.theguardian.com/media/2002/jan/25/marketingandpr>. For a case study of Nike's environmental programs, see: <https://www.vaia.com/en-us/explanations/business-studies/business-case-studies/nike-sweatshop-scandal/>.

<sup>11</sup> See: Siri Schubert and T. Christian Miller, "At Siemens, Bribery Was Just a Line Item," *The New York Times*, December 20, 2008: <https://www.nytimes.com/2008/12/21/business/worldbusiness/21siemens.html>; Siemens 2009 Annual Report, page 51: [https://www.siemens.com/investor/pool/en/investor\\_relations/e09\\_00\\_gb2009.pdf](https://www.siemens.com/investor/pool/en/investor_relations/e09_00_gb2009.pdf).

larly those who are skeptical of corporations or CSR (see [Goidel et al., 2025](#)). These individuals might find voluntary environmental actions in times of scandals as an unconvincing form of “greenwashing.” Nonetheless, from the perspective of firms, promoting VERTs remains a valuable strategy. Skeptical individuals are unlikely to be swayed by corporate actions anyway, but VERTs offer the opportunity to build a reputational premium that the rest of the public can welcome.

### **Corporate scandals and VERT types**

Our reputational framework also allows us to formulate expectations on the *type* of VERTs a firm will adopt when it experiences the spillover effects of a scandal. Because these voluntary programs are adopted under reputational distress, the firm will promote VERTs that will respond to stakeholder backlash and yield reputational benefits *without creating further reputational risk*. In the aftermath of a reputational blow, the firm is more sensitive to further reputational risk because public stakeholders closely follow its sustainability actions.

Emission-reduction pledges can help ameliorate public image but they can also backfire if the company fails to meet its own targets ([Tingley and Tomz, 2022](#); [Yadin, 2023](#)). Failing to meet one’s own target can be a serious further reputation liability. Organizations that promote voluntary regulation often have systems to report companies that (are on track to) fail their own targets, exposing them to further backlash. For instance, the data provider which we use in this study, the CDP, publishes a yearly ranking to praise participating firms that meet their own targets and shame those that fail them.<sup>12</sup>

We expect that a firm involved in corporate scandals will adopt more controllable VERTs—that is, targets that are ultimately easier to meet. Such targets come with higher expectations of success at the time they are set, allowing the firm to reduce reputational risk while still appearing responsive to public calls for regulation ([Malhotra, Monin, and Tomz, 2019](#)). Controllable targets often include flexibility for adjustment, which lowers reputational risk if the firm falls short. In contrast, more ambitious VERTs signal stronger commitment but carry a higher risk of failure and potential reputational damage if the promised reductions are not achieved. While controllable targets may seem less ambitious, we do not claim that they necessarily lack substance or that they do not require substantial changes in corporate behavior. We simply argue that a firm in reputational crises is more likely to choose risk-averse and manageable targets within the broader spectrum of possible voluntary commitments. Importantly, controllable VERTs are often still considered acceptable forms of self-regulation by institutions (e.g., the

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<sup>12</sup> See the CDP “A list:” <https://www.cdp.net/en/data/scores>.

CDP) and the public (Malhotra, Monin, and Tomz, 2019). Because meeting these targets still requires meaningful engagement with self-regulation, the firm leaves little space for civil society actors to present these measures as window-dressing. Finally, we note that the degree of controllability is generally a technical matter (see SI A.1) that the public cannot easily assess. As a result, adopting more controllable targets is unlikely to backfire reputationally.

In sum, we expect that firms involved in highly salient non-environmental negative events (e.g., criminal scandals) will respond to public pressure by increasing their climate-related pledges (e.g., VERTs). When increasing VERTs under reputational damage, moreover, firms are likely to choose pledges that minimize reputational risk: targets that are modest enough to be realistically achieved. This strategy seeks to counteract diffuse reputational harm and convert negative spillovers into positive ones.

## Observational evidence

We first provide observational evidence for our argument by studying how firms' VERTs change following involvement into *non-environmental* criminal scandals. Limited data, especially on firms' involvement in corporate crime<sup>13</sup> and VERT adoption, restricts our analysis to US companies between 2010 and 2019. We focus, in particular, on publicly listed companies since they are most subject to public scrutiny and reputational pressure. If no spillover effect appears in this group, it would be unlikely among less publicity-sensitive firms. Likewise, the post-2010 is a most-likely period of heightened corporate criminal enforcement in the US (Garrett, 2014) and growing salience of climate politics and corporate self-regulation (Green, Hale, and Arceo, 2024). We return to issues of external validity below.

### Data on voluntary emission-reduction targets

We draw data on VERTs from the CDP climate change survey: a voluntary self-disclosure program in which companies participate by sharing information about their environmental performance, risk perspective, and initiatives. It is a convenient data source because it standardizes corporate reporting on VERT adoption providing a consistent framework to measure the number of emission-reduction targets and their type over time. We draw on ten yearly waves (2010 – 2019) of the “investors” dataset, covering

<sup>13</sup> To our knowledge, no systematic cross-crime database of corporate criminal cases exists outside the US. Even for specific crime types (such as corporate corruption, the most common offense in our data) information is not systematically tabulated. Instead, it is available only in textual form—e.g., OECD reports on countries’ enforcement of anti-corruption laws (e.g., OECD, 2012, 85–86). Because these reports aim to evaluate judicial activity rather than document corporate crime, they often anonymize company names, rendering them unusable for our purposes.

only publicly listed firms (770 US firms in total).

Our argument is twofold. First, we contend that firms increase the total number of VERTs following a corporate scandal. Second, we argue that this increase is driven by the adoption of more controllable and less ambitious VERTs. We measure the number of VERTs by firm to test the first expectation. We conceptualize the potential controllability of climate targets in two ways: by the *type* of emission-reduction target and by its *scope*.

When pledging to reduce emissions, a firm typically chooses between two target types: absolute and intensity. We posit that intensity targets are the more controllable type of emission-reduction target. Absolute VERTs commit a firm to reduce its total GHG emissions in a future target year relative to a base year (e.g., committing to emit, by 2050, only 60% of the current CO<sub>2</sub>-equivalent emissions). Intensity targets, by contrast, commit a firm to reduce emissions relative to future economic output. Because intensity targets benchmark future emissions against an economic measure, they let the firm adjust for uncertainty in future conditions (Ellerman and Wing, 2003). An intensity target is, *on average*, easier to meet than a comparable absolute one because it features output at the denominator. Changes in the denominator allow the firm to meet the target regardless of actual changes in emission levels.<sup>14</sup> Intensity targets can even permit absolute emissions *to rise*, if output grows faster. By not benchmarking on output, absolute targets are structurally more binding.

Beyond the type of target, a firm can also pledge to reduce GHG emissions across different parts (*scopes*) of the value chain. Following the GHG Protocol Corporate Accounting and Reporting Standard, scope 1 covers direct emissions from facilities owned or controlled by the firm; scope 2 covers indirect emissions from purchased energy such as electricity, steam, heat, or cooling; and scope 3 covers all other indirect emissions in the value chain, including those generated upstream in the production of purchased goods and downstream through the use of sold goods. Reducing emissions along value chains, especially when complex, can be a daunting task and, in some cases, can run directly counter a firm's business model.<sup>15</sup> We thus consider VERTs on scope 3 emissions as more ambitious and less controllable types.

Ultimately, for CDP-participating firms, we measure the total number of adopted VERTs. We use this measure to test our expectation that VERT adoption increases after reputational damage. We also count the yearly *types* of climate pledges—i.e., intensity, absolute, or scope 3 targets—adopted per firm. We coded company-year observations as 0 VERTs (or VERT-types) when firms explicitly reported not

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<sup>14</sup> A more detailed discussion of the difference in stringency between absolute and intensity targets is included in the SI A.1.

<sup>15</sup> For instance, scope 3 emissions of an oil and gas firm include those from the downstream use of the fuel it sells.

to have any target in their CDP submission. Because our framework indicates that firms in reputational distress should favor risk-averse targets, we expect that reputational damage will cause firms to pledge primarily to intensity rather than absolute or scope 3 VERTs.<sup>16</sup>

Because CDP participation is voluntary, we only observe VERTs when firms submit a response. We cannot see VERTs before firms' first CDP submission, after they stop submitting, or in years when they skip reporting.<sup>17</sup> In our main analysis, we treat these censored cases coding VERT counts as missing, not zero, since climate pledges may still exist. Absence from the CDP does not imply lack of VERTs—indeed, examples exist of non-respondent firms who mention VERTs in their sustainability reports (SI C.2). In SI C, we show that our results are robust to alternative approaches to censoring, including making the conservative assumption of no VERT whenever firms do not submit a CDP response.

Readers might worry that only “greener” firms participate in the CDP, limiting our results’ external validity. We offer three counterarguments. First, even if CDP firms were greener, our focus is on *within-firm* changes in VERTs after reputational shocks. Conditioning on the green-ness of the sample, changes in voluntary climate pledges remain substantively meaningful. If anything, it should be difficult to observe changes in voluntary climate pledges by firms who already have solid climate governance, due to a “ceiling effect” (Kane, 2024). Second, CDP submitters are not necessarily greener. Of the 75 investor-owned firms responsible for about 31% of global GHG emissions since the late 18th century, over half (41) respond to the CDP, including *all* oil majors and large coal firms.<sup>18</sup> Indeed, about a quarter of our sample scores below their industry ESG averages (SI Table A.1). Third, in SI we show robustness to selection bias by restricting the analysis to S&P 500 firms (SI Table C.3), which CDP proactively contacts. Issues of self-selection and group heterogeneity are, arguably, mitigated in this sample.

That said, our observational findings come from a sample most likely to reveal the effect: not because CDP firms are “greener” but because we focus on US publicly traded firms after 2010, a period of salient corporate climate behavior and intense stakeholder scrutiny. We find similar (stronger) effects when joining our data with the universe of Compustat firms (SI Table C.4), suggesting that results generalize

<sup>16</sup> While scope 3 VERTs are certainly ambitious, scope 1 and 2 targets are not necessarily modest. Meeting scope 1 VERTs can be difficult for firms in hard-to-abate sectors like cement, and scope 2 reductions may be beyond a firm’s control in regions where grids are fossil fuel-reliant. We therefore do not treat scope 1–2 VERTs as modest. However, in SI Table B.2 we test this possibility and find an increase in scope 1 and 2 VERTs after scandals—further evidence that, if these targets are indeed easier to meet, reputational damage pushes firms toward less demanding goals.

<sup>17</sup> 95.9% of the CDP submitters that are also recorded in Compustat were in business before and after their first and last CDP submission, indicating that entry/exit from the CDP largely reflects voluntary reporting choices.

<sup>18</sup> Oil majors: Chevron, ExxonMobil, BP, Shell, ConocoPhilips, Total, Eni. Even several state-owned oil firms participate to the CDP (Gazprom, Pemex, Equinor, Petrobras). Coal firms: AngloAmerican, RWE, Westmoreland. For the full CarbonMajors report: <https://carbonmajors.org/briefing/The-Carbon-Majors-Database-26913>

to the population of US publicly traded firms in this same time period. We stress, however, that they may not transport<sup>19</sup> to non-publicly traded firms, firms in other countries, or other time periods. Lacking data to test the validity of our results in these contexts, we present this study as a first examination of our theorized reputational spillovers and their impact on corporate self-regulation, leaving external validity inferences for future research.

## Research design

We operationalize non-environmental reputational losses by considering a firm's involvement into financial corporate criminal scandals, i.e. events where a firm violated financial corporate criminal laws. Financial scandals provide us with several empirical advantages. The timing of such events, which are not environmental, is likely unrelated to pre-existing corporate plans on VERT adoption. This reassures us of their plausible exogeneity. Moreover, criminal scandals univocally deteriorate a firm's social reputation (we only consider cases where the firm is found guilty), representing a good test for our heuristics-based theory of environmental reputation and spillovers on VERT adoption. Criminal scandals are, in fact, salient events which affect the public ([Culpepper, Jung, and Lee, 2022](#)).

We gather information on firms' (or their employees') violation of corporate criminal laws from the Corporate Prosecution Registry (CPR, [Garrett and Ashley, 2019](#)). The CPR reports the universe of corporate criminal prosecutions initiated since 1992 by US federal authorities for violations of federal corporate laws. We consider exclusively violations of *non-environmental* laws, those against corporate corruption, money laundering or bank secrecy, improper pharmaceutical/drug-related behavior, anti-competitive business practices, and financial fraud.<sup>20</sup> We then merge CDP and CPR data by relying on a fuzzy-matching algorithm based on company names.<sup>21</sup> We find 65 unique companies from the CDP that were involved in a federal law violation recorded in the CPR. For each match, we code the relevant scandal year from CPR, i.e. the date of the court judgment, the date of the prosecutorial agreement, or the

<sup>19</sup> On the distinction between generalizability and transportability, see [Findley, Kikuta, and Denly \(2021\)](#).

<sup>20</sup> More specifically, any of the following: 'FCPA', 'FDCA / Pharma', 'Fraud - General', 'Fraud - Health Care', 'Import / Export', 'Bribery', 'Immigration', 'Kickbacks', 'Antitrust', 'Bank Secrecy Act', 'Fraud - Tax', 'Money Laundering', 'Fraud - Securities', 'Controlled Substances / Drugs / Meth Act', 'Gambling', 'Fraud - Accounting'. We do not consider instances in the CPR of criminal cases being dismissed, declined, or resulting in an acquittal. We show robustness to this choice in Tables B.5 and D.1.

<sup>21</sup> We largely follow the procedure by [Lerner and Osgood \(2022\)](#). We increase the similarity between same-company name pairs by operating a light pre-processing: we lower-case names, remove symbols, and remove typical business suffixes ("Inc.", "Corp.", "Group.") Next, we compute term frequency-inverse document frequency (TF-IDF) similarity for all possible name combinations between the two sources and keep only the top two nearest matches for each company. We extensively check for false positives among matches with high similarity scores and for false negatives among the non-matches with lower scores.

date of the plea—depending on the outcome of the case.<sup>22</sup> A potential limitation is that the CPR omits non-illegal misbehavior which might negatively affect a firm’s reputation. Nonetheless, our focus on criminal events (where the firm has been found guilty) provides a clear, objective basis for reputational harm, removing the need for subjective decisions about what is a reputationally relevant event or not.

In SI Figure A.3, we describe the scandals involving CDP firms, showing a rising trend in corporate criminal cases since the early 2000s and a sufficient number of events during our CDP observation period. The median number of yearly cases is 4, with a maximum of 7 in 2013. Cases span primary, secondary, and tertiary sectors, with the majority (27) involving corporate corruption. Using penalty size as a proxy for public resonance, many cases appear high-profile, with an average settlement of \$115M. A descriptive newspaper analysis (SI Figure A.4) further shows that media coverage of firms increases following a scandal, especially for publicly visible firms, confirming that CPR-coded events are salient and widely reported (see also [Culpepper, Jung, and Lee, 2022](#)).

We apply a difference-in-differences (DID) design to estimate the effect of involvement in such financial scandals on the number of adopted VERTs. In this design, a firm is considered “treated” after it has been involved in a financial corporate criminal scandal or as “control” if it has not been involved in one (yet).<sup>23</sup> We adopt an estimator proposed by [Sun and Abraham \(2021\)](#) for staggered-treatment settings, which overcomes issues of effect heterogeneity and “improper comparisons” highlighted in recent literature ([Goodman-Bacon, 2021](#); [Roth et al., 2023](#), p. 2219).<sup>24</sup> With this estimator, we do not need to specify a given post-treatment time-window in which companies are considered “treated” or not, as the design can flexibly study effect dynamics without such arbitrary choices. All that the estimator needs is the treatment “cohort” (i.e., the year when a firm experiences a scandal, if ever). Standard errors are clustered at the company-level.

A DID design is appropriate given that firms involved in financial scandals (“treated”) likely differ fundamentally from the rest (“control”), but the *timing* of financial scandals is plausibly exogenous to VERT adoption. By comparing changes in the outcome variable between treated and control firms,

<sup>22</sup> Although the resolution of a corporate criminal case comes with intense media coverage (SI Figure A.4), and thus facilitates negative reputational spillover effects, it is possible that allegations for some cases were already known ahead of this year. To account for the possibility that the CPR incorrectly codes the relevant treatment year, we searched the earliest news we could find for all 65 cases of interest. In 5 cases, the CPR year comes after the earliest media occurrence by a median of 1 year. In SI Table B.4, we replicate our analysis by recoding treatment timing with this earlier date.

<sup>23</sup> Seven firms in our sample have been involved in multiple criminal scandals. In our main analysis, we consider them as “treated” after their first scandal. In SI Table B.3, we find some evidence of a cumulative effect of involvement in multiple criminal scandals. In SI Table D.2 we show that, in any case, our main results are robust to their exclusion.

<sup>24</sup> In SI Figure B.1, we show that our results are similar when using a traditional firm-year fixed-effect (two-way fixed-effect, 2FE) and estimators by [Borusyak, Jaravel, and Spiess \(2024\)](#) and [Callaway and Sant’Anna \(2021\)](#).

the DID removes all time-invariant confounding differences between these groups (e.g., size, industry, corporate culture). It identifies an average treatment effect of the treated (ATT) companies under “parallel trends,” *i.e.* the assumption that, had a scandal not materialized, the number of VERTs for treated companies would have run parallel to that observed for control companies. This assumption would be violated in case of time-varying confounders, *i.e.* features which affect VERT adoption and whose variation timing coincides with the scandal timing. We take several steps to assess the plausibility of our identifying assumption. First, we offer placebo tests that indicate parallel trends of VERT adoption prior to the treatment, between treated and control firms. Second, in SI Table E.2 we offer placebo tests on a range of potential time-varying confounders. Third, we add them as controls in SI Table E.3.

## Results

We report estimated overall ATTs from [Sun and Abraham \(2021\)](#) in Table 1. Each column focuses on a different outcome variable: the number of emission targets (column 1); of absolute targets (2); of intensity targets (3); of targets covering scope 3 (4). The table benchmarks the magnitude of the effect by reporting, for each outcome, a “baseline” row representing the outcome variable’s average when considering never-treated firms in the full panel and treated ones before the scandal.

Table 1: The effect of a financial corporate criminal scandal on VERTs adopted by US-based firms participating to the CDP

	Target types			
	(1) All targets	(2) Intensity	(3) Absolute	(4) Scope 3
ATT	0.444* (0.175)	0.375** (0.141)	0.069 (0.164)	0.149 (0.099)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Baseline (average)	1.279	0.583	0.696	0.238
Num.Obs.	4003	4003	4003	4001
R2	0.615	0.622	0.622	0.656
R2 Adj.	0.514	0.523	0.523	0.566

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms’ treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from [Sun and Abraham \(2021\)](#) estimator for staggered-treatment difference-in-differences. Dependent variables in all models are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses.

Consistent with our argument, involvement in a non-environmental reputational shock—a financial criminal scandal—increases the number of adopted VERTs by 0.44 (+35% over the baseline average of 1.28). In other words, firms respond to non-environmental scandals by expanding their climate pledges. The effect is concentrated among reputationally risk-averse targets that are more controllable, as speci-

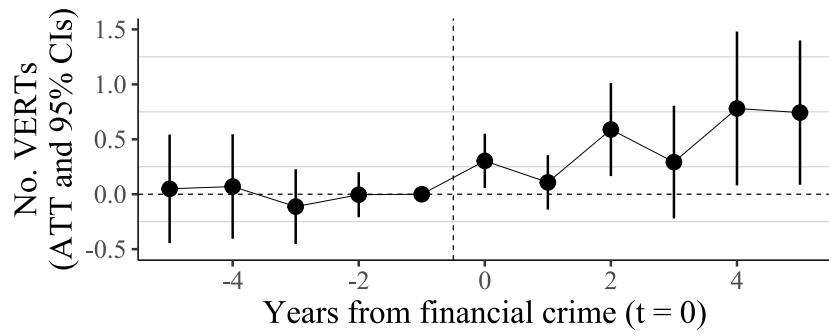
fied by our argument. Criminal scandals significantly increase the number of *intensity* targets (+0.38, or a +64% over the baseline) which guarantee a leeway of adjustment that shelters firms from the risk of suffering reputational backlash while responding to public pressures. Instead, we observe an insignificant effect on absolute targets and VERTs covering scope 3 emissions.

We test the robustness of our findings in SI. Results hold across alternative DID estimators, different operationalizations of our count variables, analyses of scope 1 and 2 VERTs, when accounting for industry-specific effects and trends, multiple criminal scandals, and when correcting CPR treatment timing with the first media mention of the corporate criminal violation (SI B). We find similar effects when addressing issues of selection into CDP data and our resulting coding of missing VERT observations—including by linking our data to the universe of US Compustat or S&P 500 firms (SI C). We also exclude (groups of) firms which could bias our findings and find similar effects (SI D). Finally, we rule out alternative explanations, such as a general VERT increase after the Paris Agreement ([Green, Hale, and Arceo, 2024](#)), or effects via composition of the boards of directors ([Lerner and Osgood, 2022](#)), financial fundamentals, or firm competitiveness (SI E).

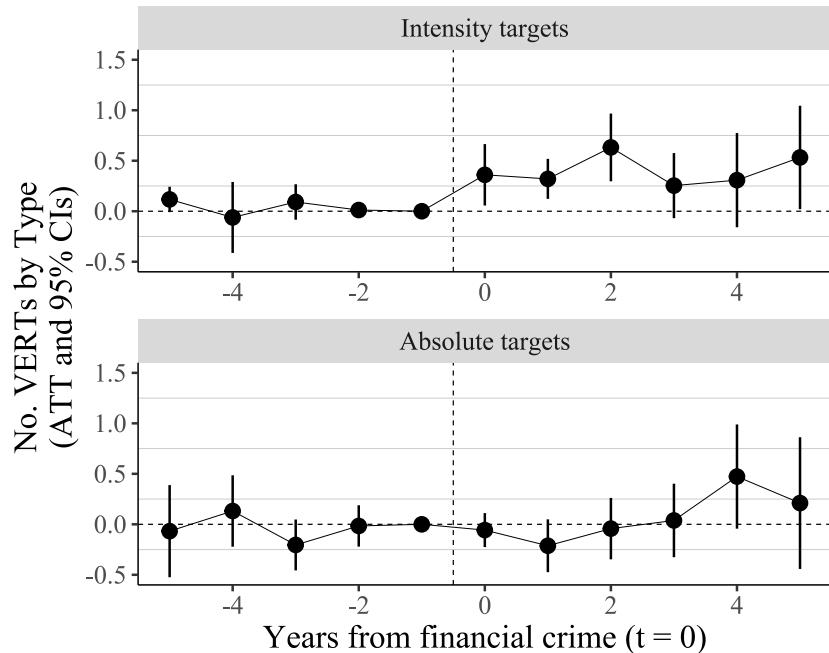
### Parallel trends

Internal validity of our estimates hinges on the untestable parallel trends assumption (PTA). We provide evidence for its validity by presenting dynamic effects from estimated [Sun and Abraham \(2021\)](#) ATTs via an event study. Pre-treatment estimates, and their direction, are informative of the extent to which the assumption is likely not holding ahead of the treatment.

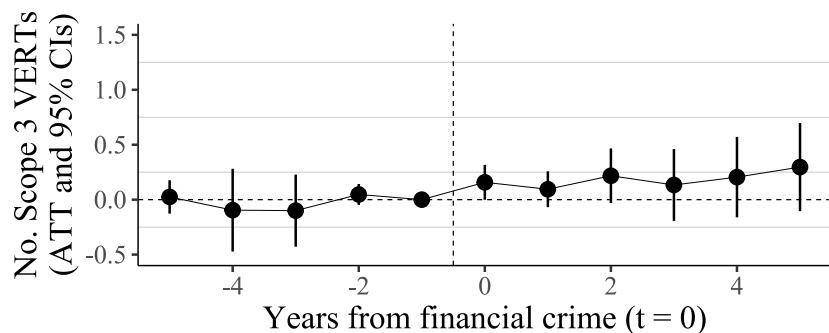
Figure 1 reports dynamic ATTs for all four outcome variables. Looking at the total number of VERTs—Figure 1(a)—we find that companies involved in corporate crime do not adopt more or fewer targets than the rest in the five years prior to it, which lends credibility to the PTA. Involvement in a financial scandal increases companies' adopted VERTs immediately after treatment (year 0). Reflecting the fact that target-setting is not a behavior that firms easily switch on and off, we find long-term effects on years +2, +4, and +5, too. When looking at the number of targets by type—Figure 1(b)—there is no statistically significant difference between treated and control firms before treatment. After treatment, we observe an immediate and statistically significant increase in the number of intensity targets (year 0), detected until year +2 and again on year +5. There seems to be a modest, albeit insignificant, post-criminal scandal drop in absolute targets, instead. Such patterns indicate that firms in reputational



(a) Number of VERTs adopted of any kind



(b) Number of intensity and absolute VERTs adopted



(c) Number of scope 3 VERTs

**FIGURE 1:** A financial scandal increases VERT adoption, particularly less ambitious targets. Dynamic ATT estimates from [Sun and Abraham \(2021\)](#) and 95% confidence intervals

distress favor controllable and reputationally risk-averse intensity targets, as implied by our argument. In Figure 1(c), we conclude by looking at the evolution of VERTs covering scope 3. Here, we find a small increase in the number of scope 3 VERTs post-treatment, against our argument. However, we note that the overall ATT on scope 3 VERTs is insignificant in Table 1 and across virtually all our robustness tests, including the use of alternative DID estimators (SI Figure B.1).

These findings bolster our information spillover argument: firms respond to *non-environmental* reputational damage by adopting voluntary emission-reduction targets which can counter (and potentially reverse) such diffuse damages. When doing so, they strategically opt for more risk-averse and more modest pledges—choosing intensity targets over absolute ones.

### Mechanism evidence

In SI F we provide evidence that supports core elements of our mechanism. First, we support our contention that more public-facing firms, involved in more high-profile scandals, experience a stronger reputational spillover effect, which causes them to increase VERT adoption to a larger degree. Indeed, we find that the post-criminal scandal increase in (less ambitious) VERTs is driven by firms that are public-facing—those that market directly to consumers (+0.593, p-value = 0.009), rather than to other businesses—and by high-profile scandals (+0.482, p-value = 0.021)—which we proxy as being above the median of the payment imposed.

We also show that corporate scandals damage reputation not only among the public but among investors, too. Investors often rely on ESG indexes as benchmarks for financial decisions (Cormier and Naqvi, 2023). Similar to availability heuristics, these indexes condense vast and complex information into a single metric, compensating for investors' limited information (Brooks, Cunha, and Mosley, 2015). Private agencies like Refinitiv, RepRisk, Bloomberg, and Moody's provide aggregate ESG scores capturing a firm's environmental, social, and governance performance, effectively reflecting its reputation among investors (Choi, Ferri, and Macciocchi, 2023). Scandals trigger downward revisions in these scores, transmitting reputational shocks across the investor community.<sup>25</sup>

Examining RepRisk and Refinitiv scores after financial scandals, we find a sharp rise in reputational risk, exposing firms to potential disinvestment (SI Figure F.1). Over time, risk declines, in RepRisk's case below pre-scandal levels. While we cannot isolate VERTs as the sole driver, this aligns with Figure

<sup>25</sup> Our review of RepRisk's RRI and Refinitiv's ESG Scores confirms that both corporate criminal scandals (e.g., corruption, fraud, money laundering) and voluntary emissions-reduction targets (VERTs) are included as inputs, supporting the interpretation that scandals can damage—and VERTs can repair—a firm's reputation among investors.

<sup>1</sup> and suggests that VERTs—and possibly other CSR initiatives—help reverse reputational damage. Though our analysis focuses on climate targets, our logic can extend to other CSR domains. Firms involved in financial criminal scandals might not only adopt VERTs but also voluntary actions in other domains (e.g., labor rights and broader sustainability measures, see Nike and Siemens examples above). Lacking data on these outcomes, we leave it for future work to study these effects.

This additional test provides ancillary and suggestive evidence of reputational repair among investors. Now, we turn to a more rigorous test of our heuristics-based argument where the public is the primary stakeholder.

## Experimental evidence

We test experimentally two core expectations from our reputational spillover mechanism: first, that individuals view a firm less favorably, environmentally, after learning it violated other CSR standards; and second, that adopting VERTs can partially restore its reputation, acting as a reputational risk-mitigation strategy. We fielded a pre-registered survey experiment in August 2024 on an online sample of 1,780 US-based respondents.<sup>26</sup> The distributions of covariates for political affiliation, age, and gender roughly match the US population, though our respondents are more educated (59% with a college degree, see SI Table G.2). Given that we intend experimental evidence as a proof of concept for internal validity and a test of mechanism for the observational evidence presented above, we are less concerned about representativeness than in normal survey research.

## Survey design

The survey is reported in SI section G. After collecting pre-treatment information on respondents (and performing an attention check), we presented individuals with vignettes manipulating the theoretical conditions implied by our argument. All vignettes reported information on a fictitious company. Although we designed our vignettes to be realistic, and based them on the reporting of real corruption cases, we favored presenting a fictitious company, as opposed to a real case, to minimize individuals' previous exposure to real corporate scandals and related opinions on famous firms.

First, we randomly assigned individuals to a control or treatment condition. The control group received information on the company's plans to expand its operations in several locations across the

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<sup>26</sup> The respondents were sampled by Prolific (a survey platform that provides high data quality, see [Eyal et al., 2021](#)).

US. Treated respondents were additionally shown text on the involvement of the company in a foreign corruption scandal. Following [Kane \(2024\)](#), we designed the treatment to be sufficiently salient in order to elicit a strong response. After this vignette, we further manipulated information provision and randomly assigned half respondents to see an additional vignette describing a VERT adopted by the same company.

Random combinations of the corruption and VERT vignettes allocated respondents to four groups with the same probability: a control and treatment group unexposed to VERT information and a control and treatment group exposed to the VERT vignette. Unlike common practice in full factorial experiments where the order of the vignettes is randomized, in our case their sequence matters. In order to mimic the real-life scenario of a company adopting an emission-reduction plan in response to a scandal, we imposed that the VERT vignette necessarily followed the treatment or control one.<sup>[27](#)</sup>

For every respondent, we randomized the company name and industry. We did so to ensure that results are not driven by a given industry and its characteristics. SI Table G.1 shows the company names and associated industries. We chose five industries—health, information technology, manufacturing, mining, and retail—representing a good degree of variation in levels of involvement in corruption scandals and environmental performance.<sup>[28](#)</sup>

After the vignette presentation, we collected our outcome data. We asked respondents’ separate views on the company’s overall and environmental reputation. Particularly, we asked respondents to express, on a scale from 0 to 10, their perception of the company (where 0 indicates “extremely negative,” 5 indicates “neither negative nor positive,” and 10 indicates “extremely positive”).<sup>[29](#)</sup>

We estimate linear models of the general and environmental reputation outcome variables, featuring two binary indicators for whether the respondent was presented with the corruption scandal treatment or

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<sup>27</sup> Some readers may worry about demand effects from vignette order for the 25% of respondents exposed to both. Two possibilities exist: first, the corruption vignette could negatively skew responses to the subsequent VERT vignette. This would bias *against* our hypothesis by making VERTs appear less effective. Second, the VERT vignette could positively influence recall of the preceding corruption vignette. Rather than a demand effect, this is *exactly* the type of effect that we theorize and that we intend to deliver experimental evidence for—VERTs act as a reservoir of goodwill that follows and partially mitigates a scandal.

<sup>28</sup> Two of these industries—mining and health—are more typically involved in corruption scandals for large public procurement but vary in their degree of environmental performance. The remaining three are somewhat less common among cases of corruption but vary in terms of their environmental records. All company names are fictitious and we ensured no real, prominent company exists with the same names. As clarified in our pre-registration, unfortunately, our limited sample size significantly limits the extent to which we can explore heterogeneous treatment effects by industry.

<sup>29</sup> Readers may worry that the survey induced reputational spillovers by design, since respondents could not select “I don’t know” and question order was fixed. We have limited leeway to address this concern, that we did not anticipate before fielding the survey. However, we note that all respondents saw the same vignette and question order, so any such spillovers by design should be evenly distributed across treatment arms. Consequently, our estimated treatment effects—based on differences *between* groups—should remain valid even if question order influenced absolute levels of evaluation.

VERT vignettes, and their interaction. The coefficient of the un-interacted corruption treatment quantifies the effect of corruption on the general or environmental image of the fictitious company among individuals *unexposed* to VERT information. We pre-registered an expectation that this coefficient would be negative. Because of availability heuristics, individuals exposed to negative news about the company (and not exposed to VERTs) negatively evaluate the firm in terms of its general *and* environmental image.

The coefficient of the interaction term, instead, represents the difference in the effect of the corruption scandal among individuals exposed or not to VERT information. It quantifies the degree by which exposure to the VERT mitigates the hypothesized negative reputational effect of corruption. Our pre-registered expectation is that this quantity would be positive: information on firms' VERTs should reduce the negative impact of a corruption scandal on firms' general and environmental image.<sup>30</sup> We deviate from our pre-registration and estimate robust standard errors to account for heteroskedasticity—results are comparable if we report regular standard errors (SI Table H.1).

## Results

In Table 2, we report our experimental findings.<sup>31</sup> In SI Tables G.3 and G.4, we show that pre-treatment covariates are balanced across our treatment groups. First, we report the unconditional effect of the corruption vignette and quantify the average effect of the corruption information on general (model 1) and environmental (model 3) reputation. In models 2 and 4 we report the interaction models.

We find evidence consistent with our argument and expectations: corruption scandals negatively impact the firm's reputation (including the environmental one) but VERTs mitigate such reputational loss. The negative effect of corruption on *environmental* reputation, in particular, illustrates the functioning of availability heuristics, as respondents exposed to corruption had no additional environmental information than the control group. Our expectations are confirmed by the consistently negative and statistically significant coefficients of the corruption variable and the positive coefficient of the interaction term (which is smaller and only significant at alpha of 0.10 in model 4, p-value = 0.08).

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<sup>30</sup> We did not pre-register any hypothesis on the relative size of the two terms, i.e. on whether the VERT treatment would be strong enough to *completely* offset the hypothesized negative effect of corruption.

<sup>31</sup> Our effective sample is 1,752 respondents. Following our pre-registration guidelines, we discarded 16 individuals who failed our pre-treatment attention check and, in addition, 12 individuals who attempted to take the survey multiple times (providing different answers).

Table 2: The effect of corruption and VERT vignettes on firm’s reputation

	General reputation		Environmental reputation	
	(1)	(2)	(3)	(4)
Corruption vignette	-2.899*** (0.105)	-3.269*** (0.142)	-1.348*** (0.108)	-1.463*** (0.131)
VERT vignette		0.525*** (0.133)		1.355*** (0.134)
Corruption × VERT vignette		0.799*** (0.204)		0.337+ (0.203)
(Intercept)	6.520*** (0.067)	6.246*** (0.092)	6.101*** (0.071)	5.395*** (0.085)
Num.Obs.	1752	1752	1752	1752
R2	0.304	0.340	0.082	0.187
R2 Adj.	0.303	0.339	0.081	0.186

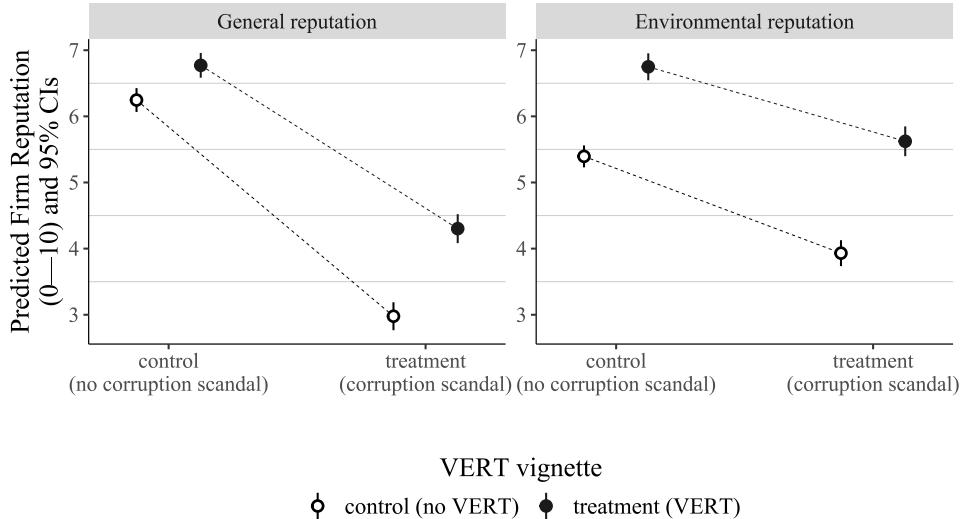
+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Linear models of the general and environmental reputation of the fictitious firm presented in survey experimental vignettes. Reputation indicators are measured on a 0–10 scale with higher values indicating better reputation. Treatment variables are binary. Heteroskedasticity-robust standard errors in parentheses.

Model 2 shows that a corruption scandal reduces a firm’s general reputation by 3.27 points (52% below the control average of 6.25) for respondents unexposed to VERTs, but only by 2.47 points for those who saw VERT information—a 0.80-point (24%) mitigation. Similar, though weaker, effects occur for environmental reputation (model 4): corruption lowers it by 1.46 points (-27% from the control average of 5.40) without VERTs, versus 1.13 points with VERTs (0.33-point difference, 23% smaller). As pre-registered, smaller mitigating effects of VERTs may reflect survey context demand effects, where individuals (particularly those with pre-existing negative views towards firms) would suspect that a firm adopts VERTs to “greenwash” a corruption scandal and thus the VERT vignette would fail for these subjects. Indeed, in SI Table H.3, we perform a pre-registered test that shows a positive and larger effect of VERTs for individuals who do not hold negative pre-dispositions towards firms.

We plot predicted effects from models 2 and 4 in Figure 2 when moving from the control to the treatment corruption-scandal condition, based on whether respondents were exposed to VERT information (solid dot) or not (hollow). Evidencing availability heuristics, the corruption scandal significantly reduces firms’ reputation—even the environmental one—in all cases, as indicated by the downward sloped lines connecting treatment and control points. However, VERTs moderate the slope of this reduction. That is, VERTs work as reservoirs of goodwill, a reputational asset that companies can use to mitigate the negative reputational consequences of the scandal and, in a way, “cleanse” their record.

We present additional findings in SI (Section H). We estimate pre-registered heterogeneous treatment effects as subgroup analyses that consider pre-treatment attitudes towards companies, towards



**FIGURE 2:** Corruption negatively affects firms’ general and environmental reputation. VERTs mitigate this damage. Predicted outcomes and 95% confidence intervals from models 2 and 4 of Table 2.

ESG, investment experience, political ideology, gender, and age. Consistently with our pre-registered expectations, we find that VERTs elicit a stronger moderating effect on individuals with pre-existing more favorable attitudes towards firms, more right-leaning, and older.

## Conclusion

In this paper, we developed and tested a theory of reputational spillovers from corporate criminal scandals. We argued that such scandals harm firms’ reputations more broadly than previously recognized, spilling over into areas like environmental performance even when the scandal is unrelated to it. This occurs because stakeholders rely on heuristics when forming judgments about firms. The resulting negative spillover pressures firms to respond, leading them to adopt voluntary emission-reduction targets (VERTs). In doing so, firms tend to select more modest targets, to mitigate further reputational risk.

Observational and experimental data supported our argument. Observationally, we showed that US firms involved in financial criminal scandals *with no direct environmental implication* increased their number of adopted VERTs by about 35% after the scandal. Such effect was concentrated among more controllable targets which are easier to meet and allow leeway for readjustment in order to be met: intensity targets. Consistently, we found no increase in absolute targets or scope 3 emission coverage. Experimentally, we primed a sample of US-based respondents with information on a fictitious company, manipulating its involvement in a corruption scandal and VERT adoption. Individuals presented with

corruption information (treated) have an environmental perception of the firm which is about 27% worse than that of the control group, supporting our availability heuristics logic. We also find that VERT information managed to partly restore this reputational loss, turning the availability heuristics logic on its head.

Several future lines of research can be developed from our work. First, future work can combine our takeaway with those linking firm behavior and regulatory initiatives ([Culpepper, Jung, and Lee, 2022](#); [Malhotra, Monin, and Tomz, 2019](#)), investigating whether such diffuse damages to the reputation of a firm also impact political elites' view in favor of more stringent state regulation. Second, future studies could investigate whether our heuristics-based reputational theory also translates to other actors than firms, such as states, parties, and local authorities. Third, further research could expand on the initial evidence provided here in favor of our argument for financial investors and ESG index investing (SI section E.2). Relatedly, future work could investigate whether our theory yields observable implications with respect to other corporate social responsibility programs, beyond VERTs. Finally, in the present work we have only considered the spillover effects of an event which is detrimental to a firm's reputation (e.g., a scandal) and have deliberately overlooked the case of an event which *improves* it, a phenomenon which likely triggers mechanisms beyond those described here and which should be subject of future work.

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# Supplemental Information

## Greening Criminal Records: How Voluntary Emission-Reduction Targets Restore Corporate Reputation

### Contents

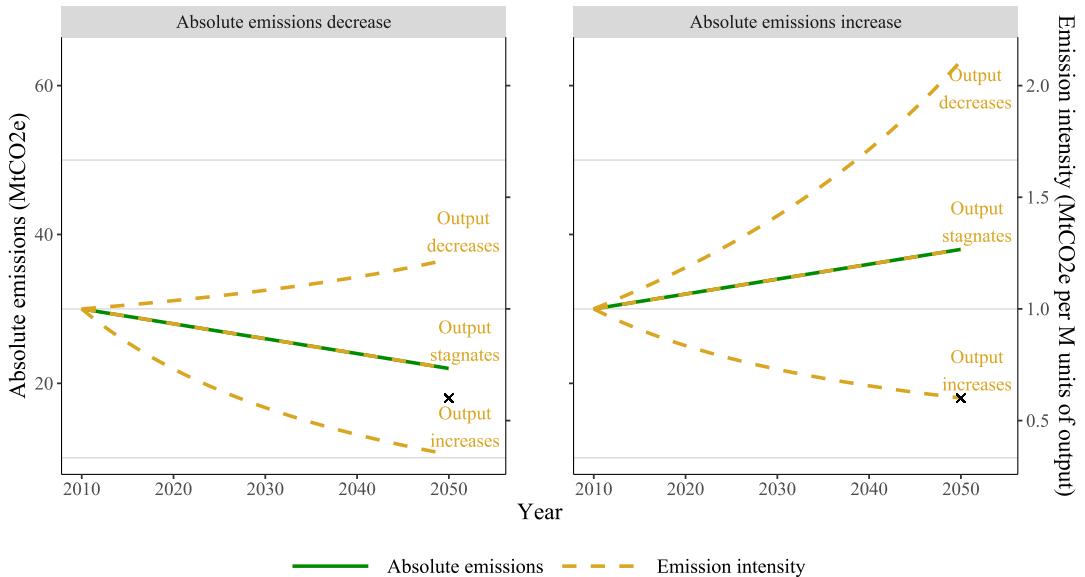
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## A Observational evidence: Setup and data description

### A.1 Absolute vs intensity targets

Firms can measure their emissions, and set their voluntary emission-reduction targets (VERTs), in absolute or intensity terms. Absolute emissions consider the actual quantity of emitted gas, typically in mega-tonnes of CO<sub>2</sub>-equivalent (MtCO<sub>2</sub>e). With an absolute VERT, a firm commits to reduce its amount of emitted gas by a given target year. Emission intensity, instead, is measured as a ratio between absolute emissions and some measure of economic activity, e.g. output production. With an intensity VERT, a firm commits to reduce the amount of emitted gas *per unit of output produced*, in a given year.

Intensity VERTs are more easily within a firm's control because they are benchmarked against an activity measure. This allows the firm to account for the uncertainty of future economic conditions (Ellerman and Wing, 2003). But it also allows the firm to meet a target *even when absolute emissions increase*, provided output grows sufficiently.<sup>1</sup> For this reason we argue that, when setting a target, intensity VERTs are favorable if in reputational distress, as they minimize the risk of future reputational backlash.



**FIGURE A.1:** Example of absolute and intensity emissions of a firm who sets an emission-reduction target in 2010 for 2050

We illustrate that intensity VERTs are easier to reach with output growth, and thus provide a reputational advantage, in Figure A.1. Imagine a firm which, in 2010, emits 30 MtCO<sub>2</sub>e (absolute) or, with 30 million units of output, 1 MtCO<sub>2</sub>e per M units of output (intensity). In 2010 the firm commits to reduce its emissions by 40%, before 2050. We represent this target in Figure A.1 as a cross. If expressed in absolute terms (left-hand side y-axis), this VERT implies a reduction from 30 to 18 MtCO<sub>2</sub>e by 2050. If expressed in intensity terms (right-hand side y-axis), this VERT implies a reduction from 1 to 0.6 MtCO<sub>2</sub>e per million units of output, by 2050. In the left panel, imagine the firm reduces its absolute emissions by 2050 (green line, left-hand side axis), but not enough to meet the target (by 2050, the green line is above the cross). How does emission intensity (golden dashed lines, right-hand side axis) move? If output stagnates (does not grow), the two lines overlap: emission intensity decreases by the same rate

<sup>1</sup> For a primer on absolute and intensity targets, see: [https://ccsi.columbia.edu/news/corporate-netzero-pledges-bad-and-ugly](https://ccsi.columbia.edu/news/corporate-net-zero-pledges-bad-and-ugly).

as absolute emissions and the intensity target is also not met. But if output increases, the intensity target might be met, even if the company has not reduced absolute emissions by the required degree. A similar case could occur, e.g., with technological breakthroughs which increase efficiency and output. Even if emissions *increase* in absolute terms (right panel) and the firm emits more gas, higher output growths can put an intensity target within reach—e.g., if the firm expands production, increasing absolute emissions but increasing output by a much larger rate. Growth in output thus causes emissions to decrease more when they are measured in intensity terms, making intensity VERTs easier to meet (for this reason, absolute VERTs are favored by initiatives like the Science-Based Targets Initiative). Only when output *decreases* is an intensity VERT more difficult to achieve than an absolute one, as exemplified in both panels by the dashed golden line at the top.

We argue that, at the time of setting a target (e.g., in 2010) intensity VERTs are more controllable (and thus reputationally safer) than absolute ones, *on average*. Consider conditions of decline in output, which make intensity VERTs more difficult to meet than absolute ones (top golden dashed lines in Figure A.1). If absolute emissions grow (or do not decrease enough), *both* types of targets would be out of reach by 2050 and expose the firm to reputational backlash. Only if absolute emissions *decrease* while output decreases, a firm could meet an absolute VERT but not an intensity one. We note that it will be uncommon for a firm to plan for such a scenario at the time of setting a target, as it implies the expectation of a future contraction in output. While decarbonization processes (e.g., electrification or renewable energy uptake) can create expectations of emission reduction, it is rare for firms (especially publicly traded ones) to plan for output contraction. We leave it to future work to fully unpack theoretically and empirically whether rare cases of firms that expect output contraction favor absolute VERTs when in reputational distress. Here, we just note that, *on average*, when setting a target a firm will likely consider intensity VERTs as more easily within its control. We also emphasize that meeting an intensity VERT *still* requires that the firm implements positive environmental measures (e.g., to not increase emissions by as much as output is growing). We only make a case that intensity targets should be more controllable, as output growth magnifies emission intensity reduction.

## A.2 Data access, availability, and reproducibility of the analysis

Our observational analysis relies on various data, many of which are proprietary. In particular, data from CDP, RepRisk, Refinitiv, and BoardEx are not publicly available. We accessed CDP and Refinitiv data through a subscription of the [ANONYMIZED FOR PEER REVIEW]. RepRisk and BoardEx data were downloaded via Wharton Research Data Services through a subscription of the same institution. Different institutions have different data subscriptions on this platform. Data from the Corporate Prosecution Registry ([Garrett and Ashley, 2019](#)) are, instead, publicly accessible.<sup>2</sup>

Legal restrictions prevent us from sharing the raw data used to support our observational analysis. For this reason, we provide a clean replication dataset for the analysis which is anonymized of any company name or identifier. Instead, we create an arbitrary and internal company ID. In order to fully replicate our results, we provide instructions on how to access the data and we provide full R code used for cleaning, merging, and analyzing them.

## A.3 Descriptive statistics

We provide a basic description of our sample of publicly traded US-based firms from the CDP in Table A.1. Our dataset covers 770 firms, observed over the years 2010–2019. Because firms voluntarily decide if and when to submit information to the CDP, we observe them for a total of 4,087 observations (we address the problem of censoring in Appendix section C). Missingness for some covariates further alters the effective sample size.

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<sup>2</sup> See: <https://corporate-prosecution-registry.com/>.

The table first presents our dependent variables: the number of VERTs, the number of absolute or intensity VERTs, and the number of VERTs including scope 3 emissions. When firms submit information to the CDP but do not report any VERT information, we code the variable as missing, which causes us to consider only 4,004 (out of 4,087) firm-year observations. We code as 0 VERTs those cases where firms explicitly report not to have any target. We similarly code the number of VERT by type (absolute and intensity) and the number of VERTs covering scope 3. Confirming the growing importance of emission-reduction targets (Green, Hale, and Arceo, 2024), we find that VERTs are rather ubiquitous in climate policy: CDP respondent firms have 1 of them active in any given year, on average. We also report outcomes that we use in Tables B.1 and B.2: the share of intensity VERTs over the total (which is the inverse of the share of absolute VERTs over the total); the share of scope 3 VERTs over the total; binaries for intensity, absolute, and scope 3 VERT adoption; and the number of VERTs covering scopes 1, 2, or both.

Next, we report variables relative to environmental, social, and governance (ESG) performance drawn from RepRisk and Refinitiv. First, a binary for whether, in a given year, a firm is reported for a (potential) violation of an environmental UNGC principle (either in its direct organization or along its supply chain, or both), which we use to test against the possibility that financial scandals are associated with environmental violations that we do not observe (Table E.2). We do see some environmental violations in our sample, with about 3% of the observations characterized by such events. Next, we report aggregated indexes for ESG performance, which we study in Section F.3. We report the RRI—an overall ESG rating computed by RepRisk to quantify the overall reputational risk exposure of a company to ESG matters—which ranges from 0 (lowest ESG reputational risk) to 100 (highest). Our sample includes a good degree of variation in terms of ESG risk, with about a fourth of the observations classified in what RepRisk considers medium, high, or very high risk (values above 25). These observations are confirmed when considering an alternative CSR score (where lower values indicate *worse* ESG performance) offered by Refinitiv (available in our sample only for S&P 500 firms).

Next, we present covariates relative to the composition of the board of directors of the companies in our sample, all drawn from BoardEx. We use these variables in Table E.2 to rule out alternative explanations relative to the financial scandals changing the composition of the executive board. We report the proportion of female members in the board of directors. We have firms with *no* reported female board member or with up to 73% female directors. We also look at the average age of the board of directors and at their average network size (defined as the average number of overlaps through employment, other activities, and education for board members). From this point of view, too, our sample covers a good degree of variation, with firms whose average director is aged between 48 and 77. Network sizes also vary considerably.

Table A.1: Descriptive statistics of US-based firms participating to the CDP (2010–2019)

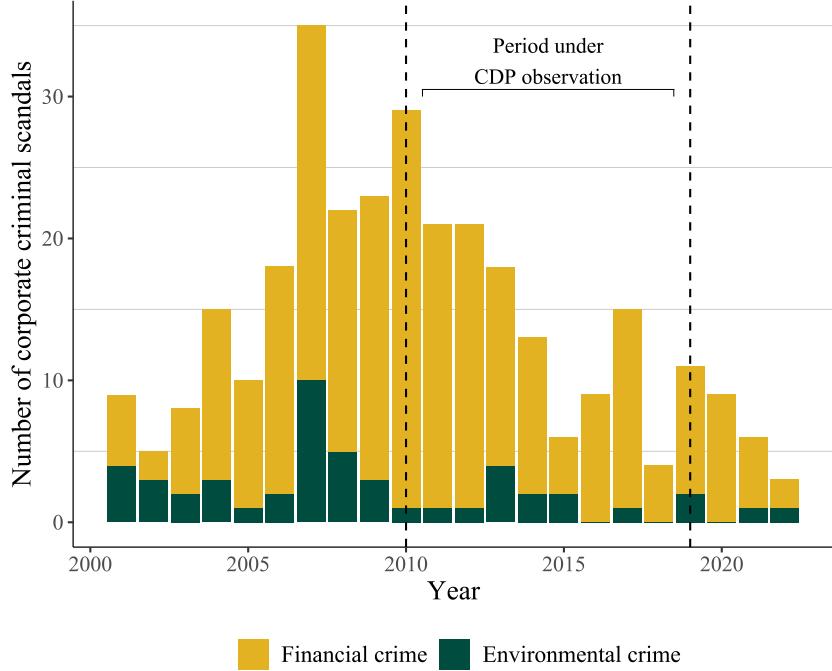
	N	Mean	SD	Min	P25	Median	P75	Max
All VERTs (count)	4004	1.279	1.256	0.000	1.000	1.000	2.000	13.000
Absolute VERTs (count)	4004	0.706	1.061	0.000	0.000	0.000	1.000	13.000
Intensity VERTs (count)	4004	0.573	0.802	0.000	0.000	0.000	1.000	8.000
Scope 3 VERTs (count)	4002	0.239	0.673	0.000	0.000	0.000	0.000	8.000
Share of intensity VERTs (proportion)	3024	0.484	0.455	0.000	0.000	0.500	1.000	1.000
Share of scope 3 VERTs (proportion)	3022	0.145	0.309	0.000	0.000	0.000	0.000	1.000
Intensity VERTs (binary)	4004	0.437	0.496	0.000	0.000	0.000	1.000	1.000
Absolute VERTs (binary)	4004	0.457	0.498	0.000	0.000	0.000	1.000	1.000
Scope 3 VERTs (binary)	4002	0.162	0.369	0.000	0.000	0.000	0.000	1.000
Scope 2 VERTs (count)	4002	0.855	0.891	0.000	0.000	1.000	1.000	7.000
Scope 1 VERTs (count)	4002	0.911	0.941	0.000	0.000	1.000	1.000	7.000
Scope 1 or 2 VERTs (count)	4002	1.027	1.002	0.000	0.000	1.000	1.000	9.000
UNGC environmental violation (binary)	5200	0.045	0.206	0.000	0.000	0.000	0.000	1.000
RepRisk RRI	5200	16.740	12.819	0.000	6.446	17.221	23.349	66.526
Refinitiv CSR score	2892	56.997	32.453	0.000	35.398	68.920	84.750	99.960
Proportion of female directors (proportion)	4878	0.197	0.115	0.000	0.119	0.188	0.265	0.733
Average directors' age (year)	4878	63.858	3.407	48.000	61.800	63.973	65.950	76.909
Average directors' network size (count)	4878	2733.776	1334.856	168.071	1791.175	2509.597	3446.446	9955.480
Assets (billions of USD)	4712	58.554	210.263	0.051	5.025	12.227	34.865	2714.610
Employees (thousands)	4658	48.278	123.115	0.013	7.295	17.000	48.000	2300.000
Return on Assets (ratio)	4706	0.095	0.077	-1.056	0.048	0.084	0.130	0.561

Finally, when looking at fundamental covariates (financial value of assets and number of employees, both downloaded from Compustat), we see that the sample includes a rather wide range of firms, including medium-sized companies—with as little as \$51 million in assets (ServiceNow Inc, 2010) or 13 employees (Atlantic Power Corporation, 2010)—to large firms with asset value of about \$2.7 trillion (JPMorgan, 2019) or more than 2 million employees (Walmart, 2015–2017). We also measure return on assets (ROA) at the firm level (defined conventionally as the ratio between net profits and total assets) which we use in Table E.2 to proxy for competitive pressures at the firm-level and rule out alternative explanations whereby corporate scandals would increase competitive pressures to firms and cause an increase in VERT adoption.

#### A.4 Description of corporate scandals

In Figure A.2, we use CPR data to describe the frequency of corporate criminal scandals involving US publicly listed firms (participating in the CDP or not) since the year 2000. These scandals, in which firms have violated US corporate criminal laws, are quite common. US publicly listed firms have been involved in a median of 12 yearly scandals and up to 34 on a single year (2007). The overwhelming majority of these scandals are not environmental: they involve violations of *financial* corporate criminal laws (e.g., anti-bribery, money-laundering, tax evasion, fraud, antitrust policies). The graph demonstrates this by distinguishing between financial crimes—those that we consider as “treatments” in our paper—and environmental ones—which we do not consider (the CPR classifies these events as violations of “Environmental,” “Wildlife,” or “Act to Prevent Pollution from Ships” laws). Especially in the time period of highest climate salience (post-2010), environmental scandals make for less than 10% of the yearly recorded offences.

Next, we describe the scandals involving the firms in our sample: US publicly-listed CDP respondents. We begin by showing the frequency of such scandal “treatments” over time, in Figure A.3, colored by the NAICS-2 industry code of the companies involved in the scandals. We follow our coding in the main text and report the earliest year (if ever) a CDP firm is involved in a corporate criminal event in the CPR. As in the main text, we consider the date of the agreement, the date of the judgment, or the date of the plea—depending on whether the case is one of deferred or non-prosecution agreements (DPA or NPA), trial conviction, or plea agreement. A few of the CDP submitters experienced a corporate criminal scandal in the early 2000s, but we observe the bulk of the scandals starting from the late 2000s



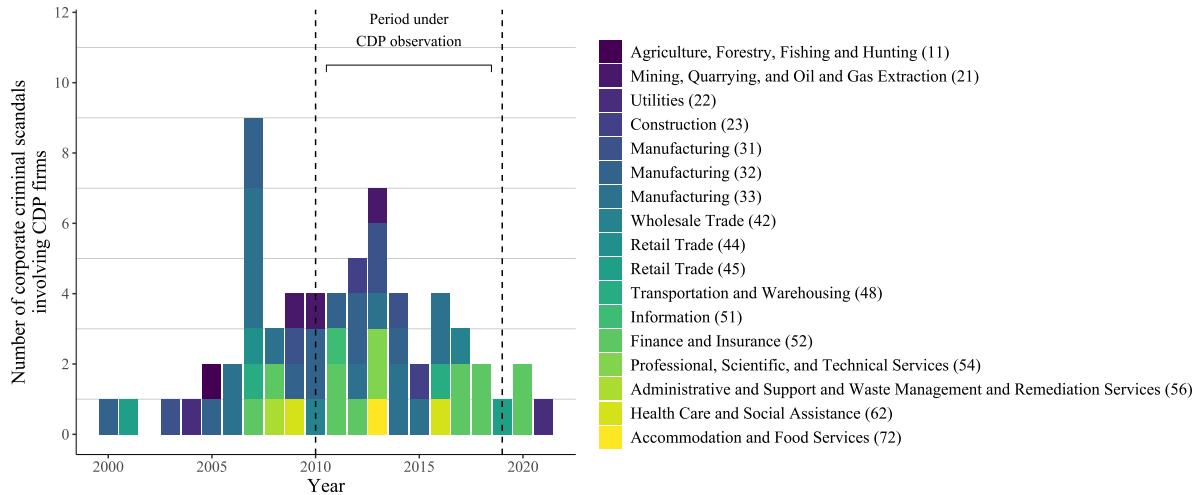
**FIGURE A.2:** The frequency of scandals in the Corporate Prosecution Registry involving US publicly listed firms (2000–2022). We distinguish between environmental and financial crimes.

and during our 2010–2019 period of CDP observation (highlighted in the plot).<sup>3</sup> This is consistent with observations made by corporate law scholars who have studied the enforcement of corporate criminal statutes by US authorities (Garrett, 2011). We also note that scandals do not seem to cluster on specific industries at given points in time, given that we observe a good share of cases involving firms in the primary (e.g., NAICS-2 codes 11, 21), secondary (e.g., codes 31–33), or tertiary sectors (e.g., codes 51, 52, 54) since the early 2000s and until the end of the period under representation.

As a proxy for the resonance of these scandals, we take the size of the total payment imposed by authorities (inclusive of fine, forfeiture, and restitution amounts). The average (median) fine or monetary settlement for CDP firms involved in a corporate criminal scandal before 2019 was \$115,606,446 (\$17,700,000). The maximum was \$900,000,000. These large payments correspond to high-profile cases. For instance, one of the criminal files in our data is a case of international corruption involving Walmart’s operations across Brazil, China, India, and Mexico which was settled in 2019 for criminal activity allegedly occurring in the years 2000–2011.<sup>4</sup> In Table F.2, we leverage the intuition that higher payments proxy for more high profile cases to probe the validity of our findings: we show that our results are stronger in more high profile cases, i.e. those in which corporate criminals are charged with relatively larger payments.

<sup>3</sup> Firms that experienced a scandal before the beginning of our CDP data are considered as “always treated” in our analysis. The staggered-treatment difference-in-differences estimators that we consider remove them from the control group, thus preventing “improper comparisons” (see Roth et al., 2023). Firms that experienced a scandal after the end of our CDP data are considered as “not yet treated” and are thus rightfully included in the control group.

<sup>4</sup> See: <https://edition.cnn.com/2019/06/20/business/walmart-bribery-mexico-brazil-fcpa-sec>.



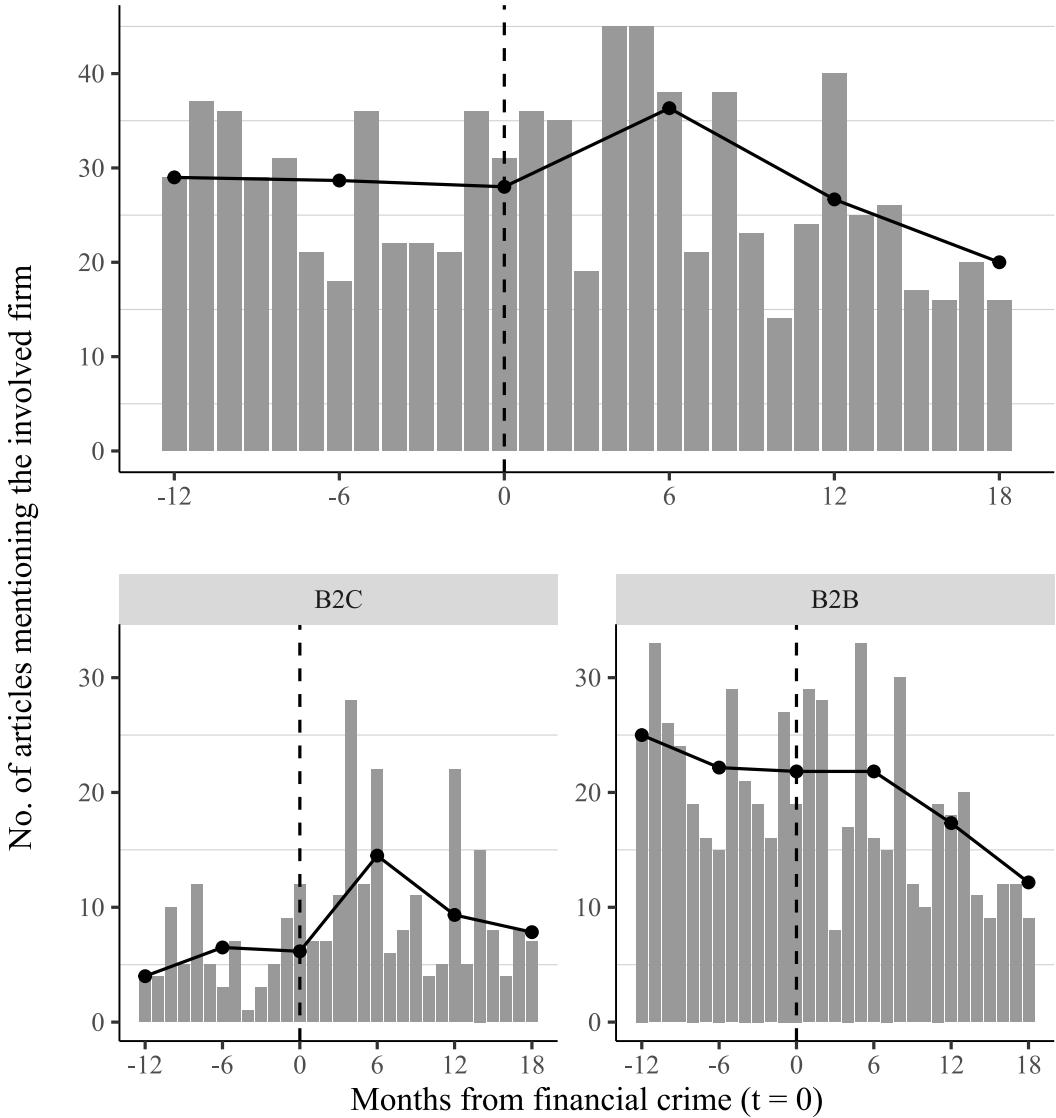
**FIGURE A.3:** The frequency of corporate criminal scandals involving CDP submitters, colored by involved industries. Data from the Corporate Prosecution Registry ([Garrett and Ashley, 2019](#)). Only the first corporate criminal scandal per firm is reported

In terms of types of events, the majority of corporate criminal scandals involving CDP firms are cases of corporate corruption (27 cases). These are followed, in the order, by violations of pharmaceutical laws (10) and fraud (9). The rest of the cases are rather equally split among money laundering, violation of trade laws, antitrust, tax evasion, or (in just a few cases) a combination of these types of crime. As per the outcomes of these scandals, most cases are resolved out of court, by means of non-prosecution (28 cases) or deferred prosecution agreements (20), as it typically happens in American corporate law ([Garrett, 2014](#)). They are followed by plea deals (9) or by a combination of these three tools.

To confirm that these are high-profile events, with significant media resonance, we conducted a descriptive media analysis which shows that newspaper coverage of companies involved in financial crime increases after a scandal. This confirms, descriptively, that financial crime increases public scrutiny of involved companies. For each CDP company involved in a financial criminal scandal, we searched ProQuest for articles appearing in *The Wall Street Journal* (either in print or online) containing the company's name in the title or subtitle (we considered only firms who experienced a financial criminal event within our CDP window of observation, i.e. between 2010 and 2019). We consider this journal as it is the main US media source on firms that is available to a specialist and general audience. We considered a time window of up to 12 months before and 18 after each criminal event.<sup>5</sup> The top panel of Figure A.4 reports our results. We present the monthly coverage of companies, before and after a financial criminal scandal (bins), as well as six-month aggregated moving averages (lines and dots). We observe an increase in the coverage of companies after a financial scandal (from about 28 average mentions in the six months before a scandal to 36 in the six months after, i.e., about a 29% increase).

Unsurprisingly, we also find evidence that this increase in coverage is particularly experienced by firms who are, to a larger degree, public-facing. In the bottom panel, we split our data according to whether the firm operates in a primarily business-to-consumer (B2C) industry or not (business-to-business, B2B)—see section F.1 for details on how we coded this variable. Firms who operate in B2C industries, who are more exposed to public scrutiny by virtue of their operations, experience significantly higher coverage after a criminal scandal than before (from about 6 average mentions in the six months before a scandal to 14 in the six months after, i.e., about a 133% increase). Such coverage remains at a

<sup>5</sup> We could access ProQuest data only after 2010. For the four companies that experienced scandals in 2010 (earliest date is May 2010), we are forced to consider fewer pre-scandal months. However, the figure provided remains practically unchanged when we consider only firms with scandals occurring on or after 2011, for whom we have one full years of pre-scandal coverage, reassuring us that the jump in coverage is not an artefact of pre-scandal data availability.



**FIGURE A.4:** Coverage of companies involved in financial criminal scandals in *The Wall Street Journal* (top panel), monthly counts and six-month moving averages. Bottom panel splits the sample based on whether the involved company operates in an industry that is typically business-to-consumer (B2C) or not—business-to-business (B2B).

higher level than previously, after a scandal. Their B2B counterparts, instead, do not seem to experience the same type of coverage jump. Consistently, in Table F.1 we show that corporate criminal scandals' effects on VERTs are driven by firms in public-facing (B2C) industries.

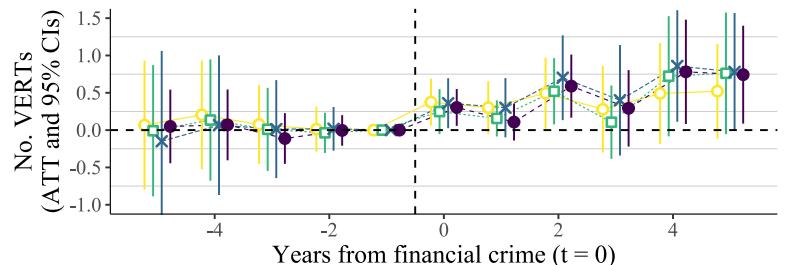
## B Observational evidence: Alternative estimators and operationalizations

### B.1 Other staggered difference-in-differences estimators

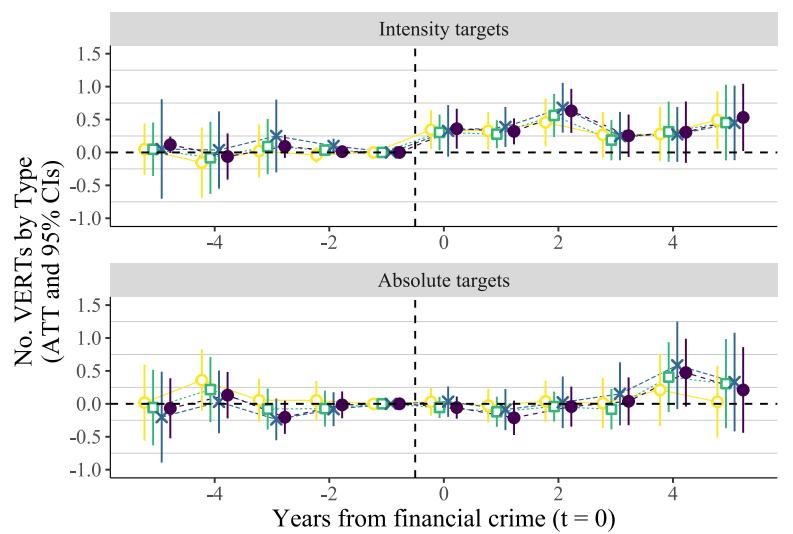
Here, we address potential concerns that our estimates are dependent on the chosen [Sun and Abraham \(2021\)](#) estimator, among the various existing for staggered difference-in-differences. In Figure B.1 we replicate our results using estimators from [Borusyak, Jaravel, and Spiess \(2024\)](#) and [Callaway and Sant'Anna \(2021\)](#). We also estimate a traditional 2FE for comparison. Consistently with evidence presented in the main text, there is no strong sign of pre-treatment diverging trends in climate pledges. With the exception of 2FE after year +4 (whose use is discouraged in staggered-treatment settings like ours, see [Goodman-Bacon, 2021; Roth et al., 2023](#)), post-treatment we find consistent positive and significant effects on the total number of VERTs as those documented in our main analysis. This is true for the [Callaway and Sant'Anna \(2021\)](#) estimator and for that by [Borusyak, Jaravel, and Spiess \(2024\)](#), albeit for the latter only at a level of significance of 0.10 on years +4 and +5 (p-values 0.08 and 0.07, respectively). Next, we apply these very estimators to our other three outcome variables of interest: number of intensity and absolute targets—Figure 1(b)—and scope 3 VERTs—Figure 1(c). Other estimators report similar dynamic ATTs as those presented in the main text. We stress that scope 3 VERTs estimates are largely insignificant using alternative estimators.

### B.2 Alternative operationalizations of the outcomes and effects on other scopes

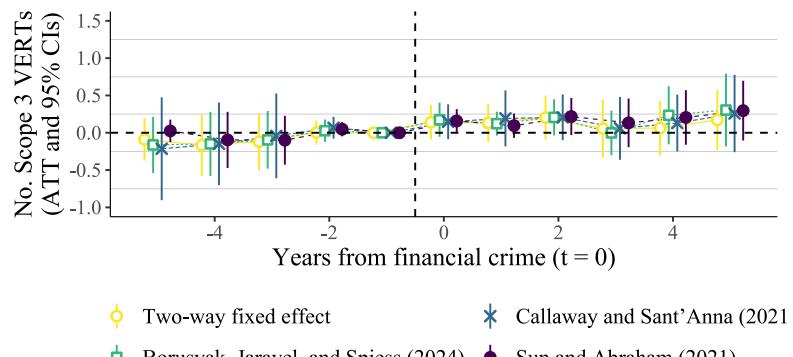
Next, we address potential concerns about the operationalization of our dependent variables. One potential concern is the use of raw counts for the number of (type, scope 3) targets. To address this concern we first propose a potential, alternative way of studying the promotion of absolute over intensity targets and the adoption of less ambitious targets that do not cover scope 3. Instead of modelling the raw number of either type of pledge, we express the number of adopted intensity (or scope 3) pledges as a share of the total count. Second, we use binary variables instead of counts to give equal weight to the adoption of VERT types regardless of the number. We report our overall ATTs in Table B.1. Overall, we find similar, significant results as those presented in the main text: a scandal increases the share of intensity targets and increases the probability of adopting an intensity target. The effect on the share of scope 3 targets is insignificant and small. We observe a positive effect significant at an alpha of 0.10, against our theory, on the probability of adopting at least one scope 3 VERT.



(a) Number of VERTs



(b) Number of VERTs by type (absolute  $\nu$  intensity)



(c) Number of VERTs with scope 3 coverage

**FIGURE B.1:** Effect of a financial scandal on outcome variables of interest. All dynamic difference-in-differences ATTs and 95% confidence intervals

Table B.1: Robustness tests to account for alternative operationalizations of the dependent variables

	Share of intensity VERTs (1)	Share of scope 3 VERTs (2)	Intensity binary (3)	Absolute binary (4)	Scope 3 binary (5)
ATT	0.357*** (0.046)	0.005 (0.040)	0.222* (0.088)	-0.171 (0.110)	0.115+ (0.061)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Baseline (average)	0.495	0.143	0.444	0.447	0.161
Num.Obs.	3023	3021	4003	4003	4001
R2	0.761	0.702	0.683	0.651	0.631
R2 Adj.	0.696	0.621	0.599	0.559	0.534

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates are from [Sun and Abraham \(2021\)](#) estimator for staggered-treatment difference-in-differences. Dependent variables are share of intensity targets over total number of targets (model 1); share of targets covering scope 3 over total number of targets (model 2); binary variables indicate whether firms had intensity targets (model 3), absolute targets (model 4) or targets of scope 3 (model 5). Standard errors are clustered at the firm-level and reported in parentheses.

We also show results when considering three alternative outcomes: the number of VERTs defined alongside scope 1 or 2, those defined exclusively alongside scope 1, and those defined exclusively alongside scope 2. At least for some firms, reducing emissions along these scopes (particularly scope 1) corresponds to less ambitious targets. We stress, however, that this is only true to a certain extent as for some hard-to-abate sectors (e.g., cement), reducing scope 1 emissions can be a very costly endeavor. That notwithstanding, we use this test to further probe our argument that, when in reputational distress, firms opt for less ambitious (and thus, more reputationally risk-averse) targets. In Table B.2 we apply our [Sun and Abraham \(2021\)](#) models to estimate the effect of corporate criminal scandals on the number of VERTs covering scope 1 or 2 (column 1), the number of VERTs covering scope 1 (column 2), and the number of VERTs covering scope 2 (column 3). We also report results for the number of VERTs covering scope 3 (column 4), which we presented in the main text, for comparison.

Table B.2: Results for the number of VERTs defined on other emission scopes

	(1) Scope 1+2	(2) Scope 1	(3) Scope 2	(4) Scope 3
ATT	0.271* (0.114)	0.266** (0.100)	0.206 (0.133)	0.149 (0.099)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Baseline (average)	1.028	0.907	0.847	0.238
Num.Obs.	4001	4001	4001	4001
R2	0.591	0.632	0.622	0.656
R2 Adj.	0.484	0.535	0.522	0.566

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from [Sun and Abraham \(2021\)](#) estimator for staggered-treatment difference-in-differences. Dependent variables in all models are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses.

Consistently with our argument, we find that corporate criminal scandals increase the number of least ambitious VERTs (those on scope 1 or 2), an effect which is particularly driven by an increase in targets on scope 1. We also note that, as ambition increases (i.e., as scope numbers increase), the point estimate for the effect of corporate criminal scandals becomes smaller, which is consistent with our argument that firms opt for more reputationally risk-averse targets when in reputational distress.

### B.3 Effect of multiple corporate criminal scandals

Seven firms in the CDP sample experienced more than one corporate criminal scandal. With repeated offence, one could expect companies would reasonably be under intensified pressure to adopt VERTs for reputational reasons. This is a plausible expectation, within our framework, but one that we cannot directly test with our data. Due to the limited number of firms experiencing multiple events, we are severely limited in the extent to which we can just replicate our analysis with such a smaller treated group. We thus proceed differently to probe this expectation. We substitute our treatment variable with a cumulative indicator measuring, at each point in time, the number of corporate criminal scandals that each company has experienced until that point in time. For firms that experienced only one scandal, this cumulative indicator equals the regular binary treatment variable that would be included in a two-way FE model. For those experiencing more than one scandal, this indicator captures the cumulative effect of additional scandals. Because this design is not a difference-in-differences anymore, we just estimate it by means of regular two-way FE. Table B.3 shows that, consistently with our main findings, each additional corporate criminal scandal indeed increases the number of intensity VERTs by 0.272 and decreases the number of absolute VERTs by 0.072 (although insignificant). The result of these opposite pulls and substitution effects is a null estimate on the total number of VERTs.

Table B.3: Robustness to using number of corporate criminal scandals

	(1) All targets	(2) Intensity	(3) Absolute	(4) Scope 3
Corporate crime scandals (no.)	0.200 (0.159)	0.272* (0.137)	-0.072 (0.115)	0.117 (0.126)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Baseline (average)	1.279	0.583	0.696	0.238
Num.Obs.	4004	4004	4004	4002
R2	0.600	0.607	0.611	0.643
R2 Adj.	0.507	0.516	0.520	0.560

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Estimates from a two-way fixed effect model. Dependent variables in all models are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses.

### B.4 Alternative operationalizations of treatment and treatment timing

In our main analysis, we use the date reported by the CPR to define treatment timing. This corresponds to the year that a corporate criminal case was resolved, via judgment, out-of-court settlement, or plea. This is a moment of high salience for a company, one that newspapers pick up on (see Figure A.4). However, it is possible that corporate criminal violations were known to the wider public already before the case resolution. We contend that this is not a severe issue for our argument, considering that resolution of a criminal case comes with intensified media coverage: we should expect a VERT effect from this point onward. That notwithstanding, here we account for this potential issue by recoding all instances of treatment timing in our data as defined by the CPR. With web searches, we code the earliest year a corporate criminal violation is mentioned in news sources. We find 5 companies, out of 65 treated ones, for whom the earliest media record precedes the CPR, with a median of 1 year anticipation. We then recode our treatment schedule, accordingly, and replicate our models. Results, in Table B.4, are consistent with our main text findings.

Table B.4: Robustness to recoding treatment timing as earliest news

	(1) All targets	(2) Intensity	(3) Absolute	(4) Scope 3
ATT	0.400* (0.185)	0.233* (0.111)	0.166 (0.132)	0.121 (0.099)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Baseline (average)	1.279	0.583	0.696	0.238
Num.Obs.	4003	4003	4003	4001
R2	0.608	0.617	0.619	0.652
R2 Adj.	0.507	0.517	0.520	0.561

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from [Sun and Abraham \(2021\)](#) estimator for staggered-treatment difference-in-differences. Dependent variables in all models are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses.

Next, we assess robustness to our choice of assigning to the “control” group those firms involved in criminal cases that were acquitted, dismissed, or declined. This choice makes substantive sense given that firms that undergo these events are effectively cleared of criminal allegations at the time of the relevant CPR event. But readers may wonder whether the reputation of acquitted firms is also tarnished, notwithstanding the acquittal. In Table B.5, however, we show that we obtain similar (albeit smaller) effects when we remain agnostic on the nature of these events and consider these firms as “treated” on the CPR year (i.e., the year of the acquittal, dismissal, or declination) as much as we do for the other firms that undergo prosecutorial agreements, court judgments, or that sign plea agreements. Effect sizes are significantly smaller and noisier when we include these firms in the treatment group, as it is expectable given that acquittals, dismissals, or declinations are *not* reputationally negative events as much as the actual attribution of a criminal responsibility is. In Table D.1, we show robustness to the exclusion of these firms from the control group, too.

Table B.5: Robustness to recoding treatment assignment to include dismissed, declined, or acquittals

	(1) All targets	(2) Intensity	(3) Absolute	(4) Scope 3
ATT	0.336* (0.147)	0.340** (0.126)	-0.004 (0.141)	0.120 (0.083)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Baseline (average)	1.279	0.583	0.696	0.238
Num.Obs.	4003	4003	4003	4001
R2	0.615	0.623	0.625	0.655
R2 Adj.	0.513	0.523	0.526	0.564

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from [Sun and Abraham \(2021\)](#) estimator for staggered-treatment difference-in-differences. Dependent variables in all models are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses.

## B.5 Account for industry × year effects

It is plausible that the timing of scandals and of VERT adoption cluster within specific industries. There might be differential trends in VERT adoption between industries; at the same time, certain industries might be more exposed to prosecutorial activity than others at specific points in time. If these timings coincide, the parallel trends assumption might be violated. For instance, if the energy sector was already ramping up VERT adoption faster than manufacturing and scandals disproportionately happened in the

energy sector, the control group of the difference-in-differences could mistakenly attribute that industry-specific dynamic to the effect of scandals.

Although scandals do not seem to cluster on specific industries at specific points in time (Figure A.3), here we account for such a possibility. We re-estimate our [Sun and Abraham \(2021\)](#) design after introducing industry  $\times$  year FEs, where industry is defined at the NAICS-2 level. We note that the time-invariant industry-level variation is already absorbed by the individual company-level FE, instead, so it cannot be included on its own in the models. Results, reported in Table B.6, are comparable to our main findings.

Table B.6: Robustness to including industry  $\times$  year FE

	(1) All targets	(2) Intensity	(3) Absolute	(4) Scope 3
ATT	0.498** (0.175)	0.355* (0.159)	0.142 (0.169)	0.163 (0.105)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry $\times$ year FE	Yes	Yes	Yes	Yes
Baseline (average)	1.279	0.583	0.696	0.238
Num.Obs.	3993	3993	3993	3991
R2	0.638	0.645	0.645	0.673
R2 Adj.	0.512	0.522	0.522	0.559

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from [Sun and Abraham \(2021\)](#) estimator for staggered-treatment difference-in-differences. Dependent variables in all models are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses.

## C Observational evidence: Selection into CDP data

### C.1 Account for censoring of CDP data

Here, we address issues of data censoring due to firms entering or exiting the CDP, which causes us not to observe a balanced panel of VERT adoption for the 770 firms, for the full period under study. Our unbalanced panel of VERTs is characterized by left-censoring (we observe firms only after they begin their CDP submissions), right-censoring (we observe firms only until they stop their CDP submissions) and, less frequently, what we call “intermission”—missingness in-between years due to a firm skipping its CDP submissions on a given year but continuing them later.

Instances of censoring are mostly due to companies deliberately deciding to start/end/intermit their CDP submission, rather than going in/out of business. To determine this, we link our CDP data with Compustat, leveraging the idea that a firm that is present in Compustat indicates the firm is certainly active at that point in time (absence, on the other hand, could indicate the firm is out of business or has not began operations yet). Out of the 508 US CDP submitters who are included in Compustat, 95.9% were present in Compustat both before and after their last CDP submission, indicating that the decision to start/stop CDP participation is almost entirely a voluntary choice in our data. A negligible minority (3.5%) was present in Compustat before but not after their last CDP submission (indicating a small number of firms that might stop CDP submission after going out of business), while only 0.6% were not present in Compustat before but were present after a CDP submission, indicating that a very negligible share could have began their submission right after commencing operations.

Because voluntary censoring by firms prevents us from observing VERTs, one might be concerned that our main results do not properly take into account firms ceasing or continuing VERTs when not submitting to the CDP. The issue is not easily solvable: absence from a CDP submission does not necessarily imply that (in that year) a company has no VERTs in place. We address this problem, in this

section, with two batteries of tests that make the best usage of the data we have available.

First, we propose two tests that address the problem of intermission—missingness in between years of CDP submissions. First, we assume that a company that intermits its CDP submissions maintains the same number of VERTs that it reported in previous years. Second, we assume that a company that intermits its CDP submissions has *no* active VERTs at the time of intermission. Although we believe the first to be a more plausible possibility, we report both results in Table C.1. Regardless of whether we extend previously observed VERTs (columns 1–4) or assume zero VERTs in case of intermissions (columns 5–9), we find a positive and significant increase in VERTs, and particularly intensity targets, following a financial scandal, comparable to those presented in the main text.

Table C.1: Robustness tests to account for intermission of CDP responses

	Extend previous VERTs				Assume zero VERTs			
	(1) All targets	(2) Intensity	(3) Absolute	(4) Scope 3	(5) All targets	(6) Intensity	(7) Absolute	(8) Scope 3
ATT	0.390* (0.159)	0.352** (0.136)	0.038 (0.150)	0.114 (0.085)	0.396* (0.159)	0.356** (0.136)	0.040 (0.150)	0.114 (0.084)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline (average)	1.244	0.574	0.669	0.229	1.204	0.549	0.655	0.224
Num.Obs.	4251	4251	4251	4250	4246	4246	4246	4244
R2	0.622	0.623	0.621	0.657	0.605	0.600	0.617	0.647
R2 Adj.	0.529	0.531	0.528	0.574	0.508	0.502	0.523	0.561

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from Sun and Abraham (2021) estimator for staggered-treatment difference-in-differences. Dependent variables in all models are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses.

We offer a second battery of tests to address the problems of left and right truncation. Similarly to what we did above, we propose two ways to mitigate left and right-censoring concerns. In our first test, we account for right-censoring (and intermission) by generating an unbalanced panel of the 770 firms, observed from the first time they submit a CDP response until 2019 and, for years where we do not observe a CDP response, we just extend the last observed VERT value. In the second test, we account for all issues of censoring and intermission. We generate a balanced panel of the 770 firms between 2010 and 2019 and assume that a firm has exactly zero VERTs whenever they do not respond to the CDP. We replicate our difference-in-differences on this balanced panel. Results, reported in Table C.2, show a significant and positive increase in VERTs, particularly intensity targets, following a financial crime, regardless of whether we adopt the first (columns 1–4) or second approach (columns 5–8).

Table C.2: Robustness tests to account for truncation of CDP responses

	Extend previous VERTs				Assume zero VERTs			
	(1) All targets	(2) Intensity	(3) Absolute	(4) Scope 3	(5) All targets	(6) Intensity	(7) Absolute	(8) Scope 3
ATT	0.391* (0.152)	0.326** (0.125)	0.065 (0.143)	0.104 (0.078)	0.441** (0.151)	0.251** (0.096)	0.190+ (0.114)	0.092 (0.071)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline (average)	1.095	0.491	0.604	0.202	0.648	0.295	0.352	0.121
Num.Obs.	5772	5772	5772	5771	7700	7700	7700	7700
R2	0.682	0.671	0.673	0.688	0.556	0.521	0.533	0.569
R2 Adj.	0.628	0.616	0.618	0.635	0.500	0.460	0.474	0.515

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from Sun and Abraham (2021) estimator for staggered-treatment difference-in-differences. Dependent variables in all models are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses.

The reader should interpret these results with caution: whenever firms do not submit to the CDP, we are fundamentally incapable to observe whether they have no VERTs or an unchanged number of targets from previous time points. Nevertheless, these tests reassure us that our main results do not suffer from selection bias induced by data censoring.

## C.2 S&P 500 analysis

Here, we limit our sample to the sole constituents of the S&P 500. This test serves two purposes. First, it limits our analysis to a group of companies that are directly contacted by the CDP for submitting survey responses. This is very important because it further mitigates concerns about self-selection into our data. Firms can voluntarily submit to the CDP, a problem which might bias our estimates if time-varying unobservable determinants of selection into the CDP positively correlated also with the timing of a financial scandal and with the intensity of climate pledges. Because S&P 500 constituents represent the core CDP sample since the survey inception, this problem is mitigated when focusing on this subsample. Second, this test limits our sample to a more comparable group of companies. This mitigates concerns that our main analysis might pool the effect across heterogeneous firms with substantially different characteristics (including likelihood of furthering financial crime and making climate pledges).

Similarly to what we did in the previous section, we present two batteries of tests in Table C.3. First, we limit our CDP observations to the sole S&P 500 companies that have responded to the CDP and replicate our analysis (columns 1–4). We find a statistically significant increase in the number of VERTs, and particularly intensity targets, following financial scandals.

Second, we construct a full balanced panel of the 640 firms that have been S&P 500 constituents between the years 2010 and 2019 (the S&P 500 index is updated every year as companies' capitalization changes), observed in this entire time period. We merge it with our CDP data and assume our dependent variables for the number of VERTs (and VERT types) to be exactly zero when an S&P 500 company does not report a CDP answer. This test introduces 183 companies (all S&P 500 constituents) which we did not observe in our previous tests as they have never submitted to the CDP. For this reason, we make sure to merge these companies' names with the CPR data, in order to be able to tell if and when these companies were involved in financial scandals. We find that 18 S&P 500 companies out of the 183 that have never submitted to the CDP (9.8%) have been involved in a financial scandal—as opposed to 57 of the 457 S&P 500 that have submitted a CDP response (12.5%).

We report results when replicating our difference-in-differences models on this balanced panel, in columns 5–8 of Table C.3. We note that this is a very conservative test as it basically imposes that 18 treated units have exactly zero post-treatment VERTs (against our argument) in the entire time-period following the treatment, just because they do not submit to the CDP. A cursory web search would show that this is, of course, false in several instances. For example, among the S&P 500 firms that are involved in a scandal but do not report to the CDP is Ingersoll Rand (involved in an anti-corruption case in 2007). The company's sustainability reports mention emission reduction targets at least as early as 2016, when the firm reported a VERT of a 35% reduction in emissions alongside scopes 1 and 2 by 2020.<sup>6</sup> To make another example of a non-CDP submitter, Kinder Morgan, a US energy infrastructure firm involved in a corporate criminal scandal in 2010, committed "to achieve a methane emissions intensity target for [their] natural gas transmission and storage operations by 2025" in its 2017 sustainability report.<sup>7</sup> Gathering information on VERT adoptions across years of textual sustainability reports for all S&P 500 that do not participate in the CDP would be an infeasible data collection effort. In fact, the advantage of CDP data is precisely that they report VERT information consistently for participating firms. So we retain the very conservative approach to assume zero VERTs for all these firms. Net of this conservativeness, we still detect an increase in the number of VERTs (and intensity targets, in particular) following the financial scandal (columns 5–8).

<sup>6</sup> See page 20 of the 2016 sustainability report: [https://www.tranetechnologies.com/content/dam/cs-corporate/pdf/sustainability/annual/2016\\_Sustainability\\_Report.pdf](https://www.tranetechnologies.com/content/dam/cs-corporate/pdf/sustainability/annual/2016_Sustainability_Report.pdf).

<sup>7</sup> See page 12 of the 2017 sustainability report: [https://www.kindermorgan.com/WWWKM/media/Safety-Environmental/documents/2017\\_ESG\\_Report.pdf](https://www.kindermorgan.com/WWWKM/media/Safety-Environmental/documents/2017_ESG_Report.pdf).

Table C.3: Robustness tests to consider only S&amp;P 500 companies

	Only submitted VERT responses				Assume zero VERTs with no response			
	(1) All targets	(2) Intensity	(3) Absolute	(4) Scope 3	(5) All targets	(6) Intensity	(7) Absolute	(8) Scope 3
ATT	0.486*	0.341*	0.145	0.126	0.343*	0.228*	0.115	0.078
	(0.205)	(0.138)	(0.128)	(0.116)	(0.149)	(0.091)	(0.089)	(0.070)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline (average)	1.323	0.588	0.735	0.244	0.637	0.283	0.354	0.118
Num.Obs.	3148	3148	3148	3148	6400	6400	6400	6400
R2	0.610	0.621	0.612	0.666	0.618	0.568	0.579	0.620
R2 Adj.	0.531	0.544	0.533	0.598	0.569	0.512	0.525	0.571

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from [Sun and Abraham \(2021\)](#) estimator for staggered-treatment difference-in-differences. Dependent variables are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses. Standard and Poor's 500 constituents only.

### C.3 Compustat analysis

We conclude with one further test that addresses issues of selection into the treatment. We complement our sample of 770 CDP firms with 12,693 US firms who are publicly traded and present in Compustat (but not in the CDP). We construct a balanced panel of the resulting list of 13,463 firms observed between 2010 and 2019. Whenever each firm does not report to the CDP, we assume exactly zero VERTs. We fuzzy-merge the 12,693 firms with data from the Corporate Prosecution Registry (CPR) to measure if/when each of these Compustat firms has been involved in a corporate criminal scandal. We, then, replicate our staggered difference-in-differences analysis on this balanced panel and report the results in Table C.4. Consistently with our previous findings, we observe a significant increase in the number of (intensity) VERTs following a corporate criminal scandal. There is, in this case, also a positive effect on the number of absolute VERTs, which goes against our expectations.

Table C.4: Robustness tests to consider all Compustat companies

	(1) All targets	(2) Intensity	(3) Absolute	(4) Scope 3
ATT	0.366*** (0.107)	0.191** (0.065)	0.175* (0.077)	0.076 (0.046)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Baseline (average)	0.035	0.016	0.019	0.007
Num.Obs.	134630	134630	134630	134630
R2	0.653	0.588	0.591	0.584
R2 Adj.	0.615	0.541	0.546	0.537

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from [Sun and Abraham \(2021\)](#) estimator for staggered-treatment difference-in-differences. Dependent variables in all models are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses.

## D Observational evidence: Exclusion of companies

### D.1 Excluded acquitted, dismissed, declined cases from the control group

In Table B.5, we have shown that our results do not hinge on the choice of considering firms that were involved in the dismissal, acquittal, or declination of a criminal case as “control”—as opposed to “treated.” Here, we show that results are substantively unchanged if we exclude them from the control group (and from the analysis) altogether. The inclusion of these firms in the control group might confound it, as firms who experience a case dismissal, declination, or acquittal might experience complex reputational dynamics whose exploration goes beyond the scope of our article. Importantly, we still consider as “treated” companies whose cases ended up in “non-prosecution agreements” (NPAs) or “deferred pros-

ecution agreements” (DPAs), two extremely common outcomes in US-lead corporate criminal cases ([Garrett, 2011](#)). Notice that, although these judicial outcomes waive (or defer) a prosecution, they always require that the company admits responsibility for the misconduct and agrees on a version of the illicit action with law enforcers. Results in Table D.1 are, again, comparable to the main ones.

Table D.1: Robustness to excluding firms which were not found guilty

	(1) All targets	(2) Intensity	(3) Absolute	(4) Scope 3
ATT	0.439* (0.175)	0.376** (0.141)	0.063 (0.164)	0.149 (0.099)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Baseline (average)	1.268	0.577	0.691	0.231
Num.Obs.	3917	3917	3917	3915
R2	0.617	0.622	0.626	0.654
R2 Adj.	0.514	0.521	0.526	0.562

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms’ treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from [Sun and Abraham \(2021\)](#) estimator for staggered-treatment difference-in-differences. Dependent variables are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses. Data exclude firms involved in corporate criminal events that were acquitted, dismissed, or declined.

## D.2 Exclude repeated offenders

Here, we exclude from the sample any repeated offender, meaning companies that were involved in corporate criminal scandals multiple times. The rationale for this test is to ensure that results are not driven by companies that suffer significantly higher reputational costs to mend (whose sustainability actions might thus be less credible), due to the repeated appearance under the spotlight of law enforcers and public opinion. Results, shown in Table D.2, are comparable to our main findings.

Table D.2: Robustness to excluding repeated offenders

	(1) All targets	(2) Intensity	(3) Absolute	(4) Scope 3
ATT	0.509** (0.165)	0.400** (0.144)	0.108 (0.141)	0.158 (0.107)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Baseline (average)	1.279	0.584	0.695	0.238
Num.Obs.	3944	3944	3944	3942
R2	0.616	0.621	0.622	0.657
R2 Adj.	0.514	0.520	0.522	0.566

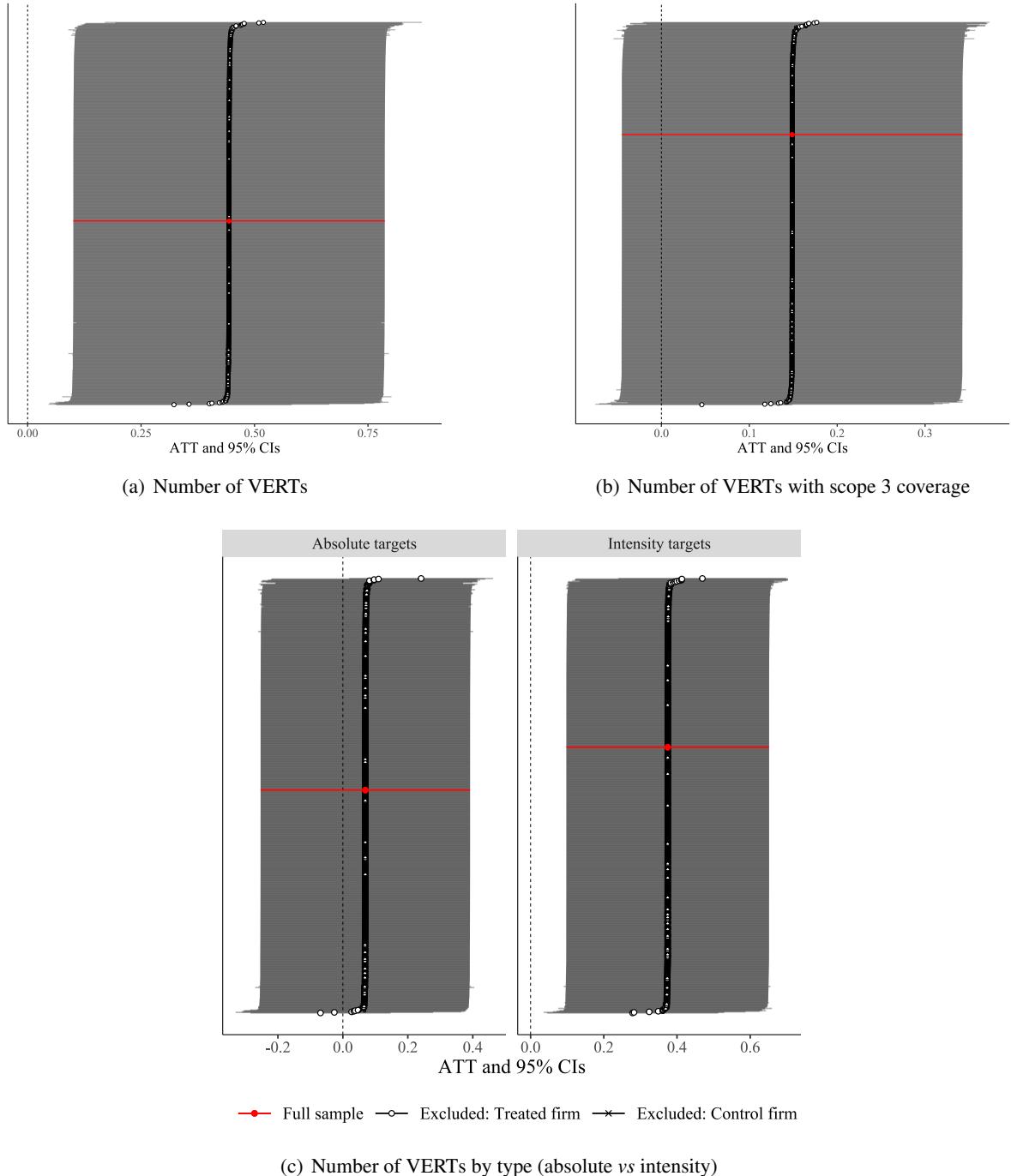
+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms’ treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from [Sun and Abraham \(2021\)](#) estimator for staggered-treatment difference-in-differences. Dependent variables are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses. Data exclude firms involved in multiple corporate criminal events.

## D.3 Exclude one firm at the time (jackknife test)

Finally, we ensure that results are not driven by any single outlier company. We re-estimate our [Sun and Abraham \(2021\)](#) ATT with a jackknife approach. That is, we drop one firm at the time from the analysis and re-estimate the ATT. Figure D.1 shows the results. It reports the effects from Table 1(in red) for comparison. It then reports all results from the jackknife approach (black) distinguishing between

whether the excluded firm is a treated (hollow dot) or control unit (cross). The jackknife ATTs are consistent with those presented in the main text.



**FIGURE D.1:** Jackknife exclusion of treated companies from the [Sun and Abraham \(2021\)](#) difference-in-differences estimation

## E Observational evidence: Ruling out alternative explanations

### E.1 The Paris Agreement as an alternative explanation

Here, we address a potential concern of spuriousness of our results. Of the 65 firms treated in the US CDP sample, 15 were treated on or after 2015, a date coinciding with the UNFCCC negotiations that would eventually lead to the 2016 Paris Agreement. Central to these negotiations (and to the agreement) were voluntary climate pledges by states and organizations. Because corporations have important stakes in climate negotiations ([Genovese, 2019](#)) it is possible that some of the firms we consider as treated increased their climate pledges as a response to Paris, rather than to financial scandals. In other words, our estimated ATTs might simply reflect a changed international framework with increased reliance on voluntary climate targets on the side of organizations (like firms). We address this concern in two ways.

First, we exclude firms that were treated on or after 2015. That is, in this test we consider only treatment cohorts 2010, 2011, 2012, 2013, and 2014. This analysis is cleaned of a Paris effect because the treated firms we consider cannot have experienced the renewed reliance on voluntary pledges at least at the time of their treatment. Once again, we replicate our analysis (columns 1–4 of Table E.1). We detect a positive, sizeable, and statistically significant effect of financial criminal scandals on the total number of (intensity) VERTs.

Table E.1: Account for confounding role of the Paris Agreement

	Only pre-2015 treated firms				Truncate data at 2015			
	(1) All targets	(2) Intensity	(3) Absolute	(4) Scope 3	(5) All targets	(6) Intensity	(7) Absolute	(8) Scope 3
ATT	0.564** (0.215)	0.423* (0.174)	0.141 (0.202)	0.209+ (0.121)	0.346+ (0.198)	0.329* (0.152)	0.017 (0.128)	0.117 (0.077)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline (average)	1.278	0.588	0.691	0.235	1.132	0.520	0.612	0.144
Num.Obs.	3892	3892	3892	3890	1883	1883	1883	1883
R2	0.603	0.610	0.615	0.648	0.719	0.733	0.735	0.717
R2 Adj.	0.504	0.512	0.518	0.561	0.592	0.611	0.614	0.588

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from [Sun and Abraham \(2021\)](#) estimator for staggered-treatment difference-in-differences. Dependent variables are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses. Only cohorts treated before 2015 (or never treated).

However, these results might still suffer the Paris effect if treated firms experienced the renewed self-governance push in later time points. To entirely clear up such suspicions, we proceed at a second, stricter test. We right-truncate our entire panel data to the year 2015. This choice forces us to work with an extremely limited dataset (just five waves of the CDP from 2010 to 2014, included) but, by construction, results in an analysis which is cleaned of any Paris effect. In a way, by doing so we force the clock to stop right before Paris was negotiated. We stress that this is a very demanding test, given that we drop more than half of our observations. We replicate our analysis in columns 5–8 of Table E.1. Detected effects are consistent with those documented in the main text in sign and size (positive for all VERTs and intensity targets). Although the point estimates are comparable to those in the main text, the ATT for the total number of VERTs is noisier here, likely a result of the lower sample size.

### E.2 Corporate environmentalism, board characteristics, and industry-level pressures

Does a corporate scandal increase VERTs via the diffuse reputational pressure that we describe in our theory, or does it do so because it causes other internal changes that lead to VERT adoption? In Table E.2 we report tests to rule out these alternative mechanisms. First, we address the possibility that financial scandals have environmental ramifications that we do not directly observe. This would undermine our heuristic-based theory of reputation, as VERTs might simply be a direct response to these ramifications. To rule out this possibility, we use RepRisk data on whether a (potential) violation of an environmental principle of the UN Global Compact (UNGC) was reported along the firms' operations or their supply

chain. We model this binary dependent variable in our difference-in-differences design and find no significant effect (model 1).

Table E.2: Account for several alternative explanations: reported environmental violations, boards of directors' composition, financials, and return-on-assets

	Anti-environmentalism	Board of directors			Financials		
	(1) UNGC	(2) Female	(3) Age	(4) Network	(5) Assets	(6) Employees	(7) ROA
ATT	-0.021 (0.042)	0.027** (0.010)	0.698 (0.452)	-71.872 (77.342)	24.818 (18.486)	-13.894 (10.297)	-0.004 (0.009)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline (average)	0.042	0.196	63.849	2665.317	52.625	47.274	0.095
Num.Obs.	5199	4877	4877	4877	4711	4657	4705
R2	0.551	0.743	0.776	0.913	0.991	0.981	0.727
R2 Adj.	0.491	0.705	0.743	0.901	0.990	0.979	0.687

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from [Sun and Abraham \(2021\)](#) estimator for staggered-treatment difference-in-differences. Dependent variables are: a binary for whether the firm has (potentially) violated UNGC environmental principles (model 1); the female proportion of the board of directors (model 2) or the average age of board of directors members (model 3); their average network size (model 4); the firm's asset value (model 5); number of employees (model 6); or return on assets to the single firm (model 7). Outcome data for model 1 come from RepRisk. Data for models 2 – 4 come from BoardEx. Data for models 5 – 7 come from Compustat. Standard errors are clustered at the firm-level and reported in parentheses.

Next, we address the possibility that the financial scandal leads firms to change the composition of their boards of directors, bringing on board individuals with characteristics that make them more pro-environmental (e.g., younger or female), thus leading to an increase in VERT adoption. All data for the tests aimed at addressing this argument, in models 2–4, come from BoardEx. We first consider the proportion of female members of the board and the average age of the board (models 2–3). We model these variables in our difference-in-differences design and find that corporate criminal scandals lead to a small increase in the share of female board members (+0.027, over a pre-scandal average of 0.196, amounting to a small, 14% increase). A change in the proportion of female directors from 0.196 to 0.223 does not appear strong enough to justify the large VERT effect we document in the main text. To make sure this effect is not confounding our estimates, in the next table we control for this covariate (and others). Moving on, we find no significant change in the age of the average board member (model 3). We also study how the network size of board members evolves (model 4). This test is meant to account for the possibility that board of directors' networks increase following a scandal, with a consequent increase in corporate environmentalism that has been shown in expanded networks (see [Lerner and Osgood, 2022](#)). Contrary to this alternative explanation for our results, we find that the average network size for board members hit by financial scandals actually *decreases*, albeit insignificantly so, likely as an effect of the attempt by firms to bring on board individuals with smaller personal networks following a financial scandal, to comply with anti-corruption, anti-fraud, and anti-collusion mandates.

We conclude by investigating whether the scandal changes companies' fundamentals and financials which might, in turn, affect VERT adoption. In models 5–6, we model the value of firms' assets and the number of employees. We find that neither of them changes significantly after a scandal. Finally, we study the effect of financial scandals on returns on assets (ROA) to each individual firm (model 7), defined conventionally as the ratio between net income and total assets to the company. We take this variable, which measures the efficiency of a firm, as a proxy for the competitive pressure that a single firm experiences (less efficient firms face stronger competitive pressures), which might be affected by a scandal and simultaneously affect a company's willingness to adopt VERTs. We find no significant effect of scandals on these variables.

The above discussion indicates that the post-scandal VERT adoption is likely not attributable to other internal changes that occur post-scandals. But is it possible that pre-existing *trends* in those covariates—e.g., a *trend* towards hiring more female directors—correlate with the timing of a scandal and, simultaneously, affect VERTs? If so, our parallel trends assumption would be violated and the internal validity of our estimates compromised. To dispel such concerns, here we include the above variables as controls. This causes our estimates to rely on a less strict version of the parallel trends

assumption: a *conditional* parallel trends assumption—i.e., that post-treatment trends between treatment and control units would have been parallel absent the treatment, *conditional on covariates*. The recent DID literature has demonstrated that adding control variables in a DID model requires further unwarranted assumptions about parallel trends in covariates and no effect heterogeneity conditional on covariates (Roth et al., 2023). To overcome these problems, we adopt the inverse probability weighting estimator originally proposed by Abadie (2005) for a 2-group, 2-period DID and adapted by Callaway and Sant’Anna (2021) to staggered-treatment settings. This estimator accommodates covariates, using them to explain the treatment timing. That is, we are able to account for the common timing, spurious or not, of covariates trends and of scandal occurrence. We include covariates stepwise. First, we include the binary for violation of environmental UNGC principles. Then, we add board of directors’ characteristics (proportion of female directors, average age, and average board member’s network size). Finally, we add fundamental financial variables (asset value, number of employees, and ROAs for a firm and NAICS-2 industry). Results, reported in Table E.3 show positive, large, and statistically significant effects of corporate criminal scandals on the total number of targets (columns 1–3) and particularly intensity targets (4–6). We find no consistent effect on absolute VERTs (7–9). When looking at scope 3 VERTs (10–12), we find a small positive effect, contrary to our argument but significant only at an alpha of 0.10 when including at least board characteristics.

Table E.3: Robustness to controlling for time-varying covariates using Callaway and Sant’Anna (2021) estimator

	All targets			Intensity			Absolute			Scope 3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
ATT	0.475*	0.804**	0.811***	0.460**	0.745***	0.671***	0.015	0.059	0.140	0.171	0.316+	0.351+
	(0.202)	(0.270)	(0.229)	(0.142)	(0.178)	(0.192)	(0.176)	(0.203)	(0.158)	(0.131)	(0.175)	(0.189)
UNGC env. violation	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Board controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Financial controls			Yes		Yes				Yes			Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline (average)	1.279	1.279	1.279	0.583	0.583	0.583	0.696	0.696	0.696	0.238	0.238	0.238
Num.Obs.	3151	2957	2760	3151	2957	2760	3151	2957	2760	3149	2955	2758

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms’ treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from Callaway and Sant’Anna (2021) estimator for staggered-treatment difference-in-differences with inverse probability weighting to accommodate for covariates in first stage (treatment model). Dependent variables in all models are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses.

## F Observational evidence: Mechanisms

### F.1 Heterogeneous effects: B2B vs B2C firms

To probe the validity of our theory, here we present heterogeneous effects distinguishing between firms who primarily market to business—business-to-business (B2B)—or consumers—business-to-consumer (B2C). As discussed in the main text, not *exclusively* B2C firms face reputational pressures: several B2B firms also do, especially when publicly traded. Nevertheless, plausibly B2C firms will experience a stronger reputational effect. Here, we test such expectation.

We classify firms as B2B or B2C using industry codes, following an established practice in business and management studies (e.g., Lev, Petrovits, and Radhakrishnan, 2010; Flammer, 2015). We employ companies’ 2-digit North American Industry Classification (NAICS-2) code, which we retrieve from Compustat and from manual internet searches for a minority of firms that do not report information on Compustat. We classify as B2B the 228 firms active in any of the following sectors: 11 Agriculture, Forestry, Fishing, and Hunting; 21 Mining; 23 Construction; 42 Wholesale Trade; 49 Warehousing; 52 Finance and Insurance; 53 Real Estate Rental and Leasing; 54 Professional, Scientific, and Technical Services; 56 Administrative and Support and Waste Management and Remediation Services; 62 Health Care and Social Assistance; and 81 Other Services (except Public Administration). Instead, we classify as B2C the 542 firms active in any of the following sectors: 22 Utilities; 31–33 Manufacturing; 44–45 Retail Trade; 48 Transportation; 51 Information; 71 Arts, Entertainment, and Recreation; and 72

Accommodation and Food Services (three firms miss a NAICS-2 code from Compustat so we are unable to code them as either).

Although rough, the classification has good face validity. Among the firms classified as B2C, we find notorious examples of companies that, although not exclusively, market to consumers: NextEra, Coca-Cola, ExxonMobil, Levi's, Apple, Walmart, American Airlines, Google, Starbucks, McDonald's. Among the B2B ones, we find firms that typically market to other businesses or bid for large government contracts: Monsanto, Freeport-McMoRan, Schlumberger LTD, Valeant Pharmaceuticals, Aon Corporation, Iron Mountain, Cognizant, Medtronic.

We then replicate our analysis by splitting the data in the two subsamples and report our results in Table F.1. Consistently with our expectations, we find that the effect of scandals on VERT adoption (all targets and intensity ones) is mainly experienced by B2C firms. B2B ones, instead, experience a small and statistically insignificant effect. The fact that consumer-oriented firms experience the largest effect confirms our argument that the effect of scandals on VERT adoption is primarily reputational.

Table F.1: Heterogeneous effects by B2B vs B2C firms

	All targets		Intensity		Absolute		Scope 3	
	(1) B2C	(2) B2B	(3) B2C	(4) B2B	(5) B2C	(6) B2B	(7) B2C	(8) B2B
ATT	0.593** (0.228)	0.223 (0.170)	0.383* (0.162)	0.188 (0.150)	0.210 (0.128)	0.035 (0.197)	0.180 (0.148)	0.093 (0.064)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline (average)	1.381	1.015	0.664	0.376	0.718	0.639	0.263	0.175
Num.Obs.	2866	1137	2866	1137	2866	1137	2866	1135
R2	0.625	0.590	0.610	0.638	0.632	0.606	0.687	0.588
R2 Adj.	0.526	0.458	0.508	0.522	0.536	0.479	0.605	0.455

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from Sun and Abraham (2021) estimator for staggered-treatment difference-in-differences. Dependent variables in all models are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses.

## F.2 Heterogeneous effects by size of payment imposed

Here, we use information on the size of the payment imposed by authorities to corporate criminals (inclusive of fines, forfeitures, restitutions, and other mandated settlements) to proxy for whether a scandal was particularly high profile (thus, salient) or not. We subset our treated firms based on whether the imposed total payments are above or below the observed median of total payments for treated firms (\$17,700,000) and replicate our analysis in these two subsets (for both subsets we maintain all firms that have never experienced a scandal as control units). If VERT adoption follows scandals due to a reputational logic, one should reasonably expect that more high profile cases (i.e., those resulting in higher settlements) will experience a stronger effect. Results in Table F.2 show exactly that: the effect of a scandal on VERT (and intensity VERT) adoption is large and significant for high-profile cases (those with larger total payments, models 1–4). When looking at low-profile ones (models 5–8), we find a positive effect that is smaller and only significant at an alpha of 0.10. In this subsample, we also find a significant effect on *absolute* VERTs.

Table F.2: Heterogeneous effects by size of total payments from criminal event

	Total payment above median				Total payment below median			
	(1) All targets	(2) Intensity	(3) Absolute	(4) Scope 3	(5) All targets	(6) Intensity	(7) Absolute	(8) Scope 3
ATT	0.482*	0.472**	0.009	0.192	0.330+	0.004	0.326*	0.024
	(0.208)	(0.169)	(0.171)	(0.136)	(0.187)	(0.070)	(0.144)	(0.045)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline (average)	1.395	0.430	0.965	0.333	1.505	0.697	0.807	0.450
Num.Obs.	3799	3799	3799	3797	3736	3736	3736	3734
R2	0.618	0.622	0.626	0.658	0.610	0.616	0.617	0.654
R2 Adj.	0.516	0.521	0.526	0.566	0.511	0.518	0.520	0.566

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from [Sun and Abraham \(2021\)](#) estimator for staggered-treatment difference-in-differences. Dependent variables in all models are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses.

### E.3 VERTs as a reputational response aimed at investors

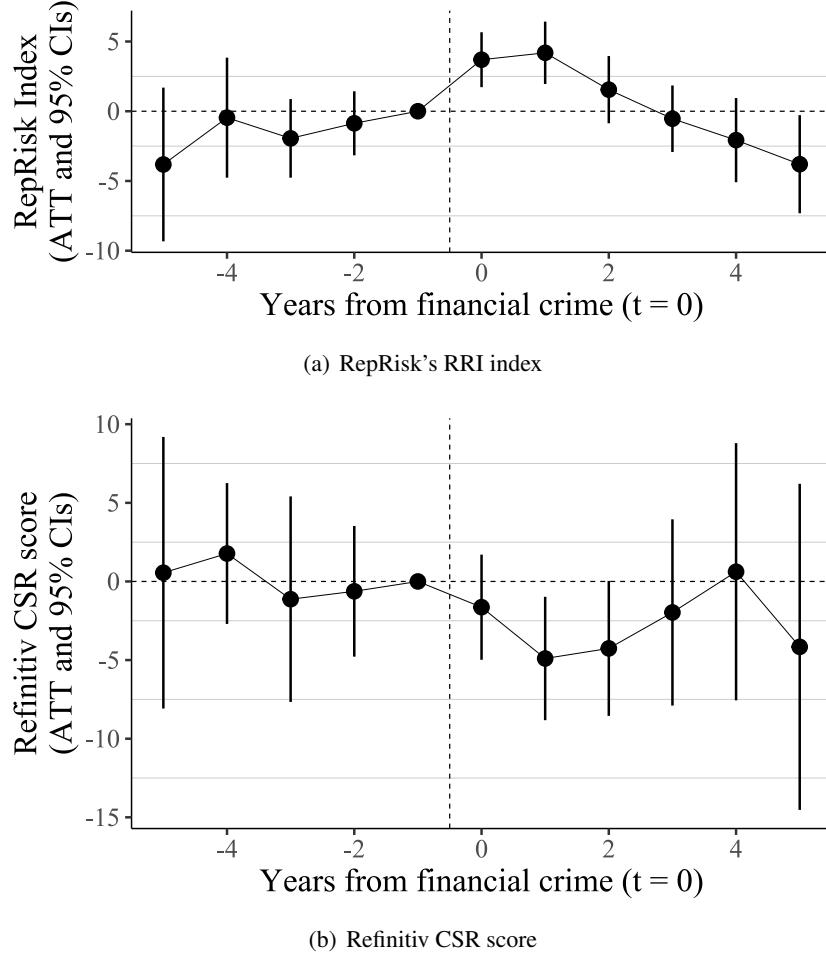
We conclude with one last test for our reputational theory of VERT adoption that is specific to one group of stakeholders to publicly traded firms: investors. Our theory states that a corporate criminal scandal turns into a diffuse reputational shock which negatively affects firms' overall public reputation. But a (publicly traded) firm is not only facing the general public: it also faces investors. In the stock market or investment space, this implies that firms suffer a negative reputational blow to their environmental, social, and governance (ESG) ratings following a scandal. Such ratings are important for firms because investors use them to determine investment choices. ESG indexes work as a sort of aggregate reputational heuristic for risky firms.

We propose one event study difference-in-differences to show this effect. We model two aggregate indexes measuring overall ESG reputation. We consider the RRI index proposed by RepRisk, one of the most widely used measurement of ESG reputational risk exposure. RepRisk builds this index starting from a proprietary algorithm which synthesizes information along several ESG issues. The score ranges from 0 (lowest ESG reputational risk) to 100 (highest risk). Second, we model the CSR score measured by Refinitiv, which synthesizes data about controversies and positive actions entertained by firms across a variety of CSR dimensions, attributing a higher value to firms with a better performance—notice, thus, that *this score is inverted with respect to the RRI*: here, higher values indicate firms with a *lower* reputational risk.

We make one important observation here, about the validity of these indexes: we take these ESG metrics as indicating the CSR reputation of a firm on financial markets, and *not* its “true” CSR performance. Taking these indexes at face value would require us to delve into the specifics of how the indexes are constructed—for instance studying how single, complex information about ESG controversies or positive actions are considered, coded, aggregated up, and weighted by these proprietary algorithms, whether the algorithms are reasonable or whether they introduce bias. Such insightful effort lies outside the scope of our work. Instead we note that, regardless of whether these ESG metrics correctly account for real CSR information—that is, regardless of whether the image that they present of a firm is correct or biased—indexes direct investment choices ([Choi, Ferri, and Macciocchi, 2023](#); [Cormier and Naqvi, 2023](#)) and thus the image that they present will have important consequences for firms, who will have an incentive to try and improve them.

Dynamic ATTs are reported in Figure F.1. The evolution of these ESG scores is consistent with our argument. Pre-treatment, we do not observe significant effects. The financial scandal leads to an immediate deterioration of involved firms' ESG rating, as implied by our argument—as shown by the significant and positive effect in the RRI for two years after the scandal and by the negative trend for the Refinitiv CSR score. This diffuse reputational blow is then followed up by a progressive *improvement* of firms' ESG rating, indicated by estimates trending negative for the RRI and positive for the Refinitiv's score since year 3 post-treatment. Although we cannot attribute such effect to VERTs exclusively, given that companies might be implementing additional programs to mitigate the negative reputational blow, we speculate that (part of) this counterbalancing effect is driven by firms using VERTs to compensate

for the reputational losses. These results confirm that corporate criminal scandals turn into negative reputational blows that negatively affect the general reputation of a firm among investors—as measured by aggregate ESG metrics.



**FIGURE F.1:** A financial scandal increases aggregate ESG risk rating immediately. As time passes, ESG risk rating improves as firms adopt VERTs to balance the reputational shock. Dynamic ATT estimates from [Sun and Abraham \(2021\)](#).

## G Experimental evidence: Design

### G.1 Survey design and ethics

In this section, we report the exact wording used in our survey instrument. Survey items were presented to the respondents in exactly the order reported here.

#### G.1.1 Pre-treatment questions and attention check

Here, we report the exact wording of our pre-treatment survey questions. Information on individuals' age, gender, ethnicity, and political affiliation is collected automatically by Prolific and it was retrieved via participants' anonymous Prolific IDs.

**Q1.** On a scale from 0 to 10, where 0 is ‘not at all’ and 10 is ‘a great deal’, how concerned are you about climate change?

- 0 (not at all)
- 1
- 2
- 3
- 4
- 5 (somewhat)
- 6
- 7
- 8
- 9
- 10 (a great deal)

**Q2.** On a scale from 0 to 10, where 0 is ‘not at all’ and 10 is ‘a great deal’, how much do you think firms positively impact the prosperity of your society?

[Same scale as for **Q1** displayed]

**Q3.** On a scale from 0 to 10, where 0 is ‘not at all’ and 10 is ‘a great deal’, how important do you think it is that firms comply with corporate sustainability standards (such as environmental, social, and governance ones)?

[Same scale as for **Q1** displayed]

**Q4.** On a scale from 0 to 10, where 0 is ‘not at all’ and 10 is ‘a great deal’, how important do you think corporate sustainability standards (such as environmental, social, and governance ones) are for the prosperity of your society?

[Same scale as for **Q1** displayed]

**Q5.** Have you ever invested in stock markets, either in a personal or professional capacity?

- 1. Yes, in a personal capacity
- 2. Yes, in a professional capacity
- 3. Yes, in both capacities
- 4. No

**Q6.** On a scale from 0 to 10, where 0 is ‘not at all’ and 10 is ‘a great deal’, please select number 4 to demonstrate you are paying attention to the survey questions.

[Same scale as for **Q1** displayed]

### G.1.2 Survey vignettes

We report the exact wording of the experimental vignettes in Figure G.1. The top panel reports wording of the control and treatment (red text) vignettes. The blue text refers to the randomized industry and company names, which are listed in Table G.1. Each respondent was presented vignettes relative to the same company and industry. The corruption treatment/control vignettes (top panel of Figure G.1) necessarily preceded the VERT vignette (bottom) for the respondents who were presented it, in order to mimic the realistic scenario of a company adopting a VERT *in the aftermath* of a scandal, and not ahead of it. Random assignment allocated our respondents into four approximately equal-sized groups: one group shown the corruption control vignette and no VERT vignette (24% of respondents), one group shown the corruption treatment vignette and no VERT vignette (26%), one group shown the corruption control vignette and the VERT vignette (26%), and one group shown the corruption treatment vignette and the VERT vignette (24%).

### **Corruption treatment and control vignettes:**

#### **[Firm name] announces significant expansion [amidst corruption scandal]**

[Firm name], leading firm in the global [Industry] sector, has announced the opening of five new facilities across the United States. The expansion aims at increasing the company's production capacity and is expected to generate significant returns for its shareholders. This move is part of [Firm name]'s broader strategy to enhance its market presence across the country and streamline its production processes.

[These good news, however, come at a turbulent time for the company. The Department of Justice (DOJ) is investigating into a large-scale corruption scheme allegedly operated by [Firm name] to secure billion-dollar-worth public contracts abroad.]

#### **VERT adoption vignette:**

#### **[Firm name] to cut down emissions in half by 2050**

[Firm name] held a press release event, yesterday evening, at its headquarters. CEO Benjamin Colegrave presented the company's new plan to mitigate its carbon footprint. "On top of our current actions to tackle climate change, today we set a more ambitious path forward." said Colegrave. "With our 'Green Restructure Plan', we will implement new production processes and diversify our sources of energy intake. We commit to slice our current CO<sub>2</sub> emissions in half by 2050."

**FIGURE G.1:** Experimental vignettes

**TABLE G.1:** Company names and industries

<b>Firm name</b>	<b>Industry</b>
MedTech Nexus	health
Arcadia Information	information technology
Pioneer Fabrications	manufacturing
SilverHaven Resources	mining
Vista Shops	retail

#### **G.1.3 Post-treatment questions**

Here, we report the verbatim wording of the post-treatment questions in our survey.

**Y1.** The article you just read talked about an organization. What type of organization was it?

1. A trade union
2. A company
3. A political party
4. The US army

**Y2.** On a scale from 0 to 10, where 0 is ‘extremely negative’ and 10 is ‘extremely positive’, what is your perception of the firm you just read about?

- 0 (extremely negative)  
1  
2  
3  
4  
5 (neither positive nor negative)  
6  
7  
8  
9  
10 (extremely positive)

**Y3.** On a scale from 0 to 10, where 0 is ‘extremely negative’ and 10 is ‘extremely positive’, what is your perception of the integrity of the firm you just read about?

[Same scale as for **Y2** displayed]

**Y4.** On a scale from 0 to 10, where 0 is ‘extremely negative’ and 10 is ‘extremely positive’, what is your perception of the environmental profile of the firm you just read about?

[Same scale as for **Y2** displayed]

**Y5.** On a scale from 0 to 10, where 0 is ‘extremely negative’ and 10 is ‘extremely positive’, what is your perception of the social profile of the firm you just read about?

[Same scale as for **Y2** displayed]

**Q7.** In which industry are you currently employed?

1. Construction
2. Education and Health Services
3. Financial Activities
4. Information
5. Leisure and Hospitality
6. Manufacturing
7. Natural Resources and Mining
8. Professional and business services
9. Public Administration
10. Trade, Transportation, Utilities
11. Unemployed
12. Other – Please specify

**Q8.** What is the highest level of education you achieved?

1. No schooling completed
2. Regular high school diploma
3. GED or alternative credential
4. Some college credit, but less than 1 year of college
5. 1 or more years of college credit, no degree
6. Associates degree (for example: AA, AS)
7. Bachelor’s degree (for example: BA, BS)
8. Master’s degree (for example: MA, MS, MEng, MEd, MSW, MBA)
9. Professional degree beyond bachelor’s degree (for example: MD, DDS, DVM, LLB, JD)
10. Doctorate degree (for example, PhD, EdD)

## G.2 Sample description and balance in covariates

Table G.2 presents the distribution of individuals by political affiliation, age range, gender, education, and ethnicity. In Table G.3, we test balance in covariates among individuals exposed to the corruption treatment vignette or not. We report difference in means for pre-treatment covariates and associated p-values. We consider individuals' age, gender, level of climate change concerns (on a 0-10 scale), attitudes towards firms (opinion on whether firms contribute to the prosperity of their society, 0-10), opinion on whether corporate sustainability standards are important (0-10), opinion on whether corporate sustainability standards contribute to the prosperity of their society (0-10), any past investment experience (recoded binary), any higher education degree (recoded binary, considering Bachelor, Master, Doctorate, or other professional degree like MD, DDS, DVM, LLB, JD), and political affiliation. The covariates are rather equally balanced, with differences that are overall negligible across the board and large p-values. In Table G.4, we repeat the same procedure for the VERT vignette. In this case, too, individuals who were exposed to the VERT vignette are not statistically dissimilar from those that were not, before treatment.

Table G.2: Distribution of US respondents by co-variate groups

	Size	Perc.
<b>US Political Affiliation</b>		
Democrat	515	29.4%
Republican	480	27.4%
Independent	757	43.2%
<b>Age Range</b>		
18-24	206	11.8%
25-34	304	17.4%
35-44	297	17.0%
45-54	276	15.8%
55-100	669	38.2%
<b>Gender</b>		
Male	855	48.8%
Female	897	51.2%
<b>Education</b>		
Has higher educ. degree	1027	58.6%
Has no higher educ. degree	725	41.4%
<b>Ethnicity</b>		
White	1184	67.6%
Black	342	19.5%
Asian	72	4.1%
Mixed	114	6.5%
Other	35	2.0%

Table G.3: Balance in covariates relative to the treatment vignette condition.

	Corruption vignette					
	Control (N=873)		Treatment (N=879)		Diff. in Means	p
	Mean	Std. Dev.	Mean	Std. Dev.		
Age	46.25	15.75	46.25	16.09	-0.00	1.00
Female	0.52	0.50	0.51	0.50	-0.01	0.70
Environmental concern	6.80	2.94	6.80	2.88	0.00	0.99
Firms prosperity	5.91	2.36	5.83	2.29	-0.09	0.43
ESG importance	7.25	2.67	7.18	2.56	-0.07	0.58
ESG prosperity	6.96	2.65	6.86	2.62	-0.10	0.43
Any investment experience	0.76	0.43	0.74	0.44	-0.01	0.49
Higher education degree	0.59	0.49	0.58	0.49	-0.01	0.75
	N	Pct.	N	Pct.		
Political affiliation	Democrat	238	27.3	277	31.5	
	Independent	379	43.4	378	43.0	
	Republican	256	29.3	224	25.5	

Table G.4: Balance in covariates relative to the VERT vignette condition.

	VERT vignette					
	Control (N=870)		Treatment (N=882)		Diff. in Means	p
	Mean	Std. Dev.	Mean	Std. Dev.		
Age	46.69	15.77	45.82	16.06	-0.88	0.25
Female	0.51	0.50	0.51	0.50	0.01	0.82
Environmental concern	6.75	2.95	6.85	2.86	0.10	0.45
Firms prosperity	5.96	2.30	5.78	2.34	-0.18	0.11
ESG importance	7.24	2.65	7.19	2.58	-0.06	0.65
ESG prosperity	6.89	2.67	6.92	2.60	0.03	0.83
Any investment experience	0.75	0.43	0.75	0.44	-0.01	0.70
Higher education degree	0.59	0.49	0.59	0.49	-0.00	1.00
	N	Pct.	N	Pct.		
Political affiliation	Democrat	264	30.3	251	28.5	
	Independent	362	41.6	395	44.8	
	Republican	244	28.0	236	26.8	

### G.2.1 Research ethics

This study was funded by the [ANONYMIZED FOR PEER REVIEW] and received approval from the [ANONYMIZED FOR PEER REVIEW] Research Ethics Team on 4 March 2024. The Ethics Team confirmed that the project posed no significant research ethics concerns. Detailed information about the survey can be found in our pre-registered report.<sup>8</sup>

The survey was conducted online and distributed to US-based individuals via Prolific.<sup>9</sup> Participants were compensated £1.05 for completing a 7-minute survey, equivalent to an hourly rate of £9.00, which Prolific deems a fair remuneration rate. The survey was fielded on 6 August 2024. Voluntary informed consent was obtained from all participants, who were free to decline participation. Before providing consent, participants were given comprehensive information about the study, including its purpose, the estimated time required to complete it, the fictitious nature of the companies discussed, privacy and data protection under [ANONYMIZED FOR PEER REVIEW] regulation, the confidentiality of the survey, and its voluntary nature. Relevant contact information was also provided.

<sup>8</sup> Anonymous pre-registration is accessible at: [https://osf.io/xvrn3/?view\\_only=e79862fef77643278979887cae95b2b9](https://osf.io/xvrn3/?view_only=e79862fef77643278979887cae95b2b9).

<sup>9</sup> See: <https://www.prolific.com/>.

To facilitate the experiment, deception was employed in describing the companies. Participants were presented with information about a fictitious company, including details about its involvement in a corruption scandal and its environmental profile. This approach was chosen to prevent any potential harm to real companies from the fictitious information. Participants were informed about the use of deception prior to providing their consent. To further mitigate any risk, a reminder was included at the conclusion of the survey, clarifying that the company described was fictitious and that any resemblance to real companies or events was purely coincidental. Contact details were also provided for participants who might had questions or concerns about the study.

Throughout the storage and analysis of data, participants' identities remained confidential, in line with the guarantees outlined during the consent process.

## H Experimental evidence: Additional material

### H.1 Robustness tests

First, we replicate our results from Table 2 using independently identically distributed standard errors (as this was the standard error choice that we pre-registered). Table H.1 reports our findings, which are consistent with the main results.

Table H.1: Experimental results, i.i.d. SEs

	General reputation		Environmental reputation	
	(1)	(2)	(3)	(4)
Corruption vignette	-2.899*** (0.105)	-3.269*** (0.145)	-1.348*** (0.108)	-1.463*** (0.144)
VERT vignette		0.525*** (0.145)		1.355*** (0.144)
Corruption × VERT vignette		0.799*** (0.205)		0.337+ (0.204)
(Intercept)	6.520*** (0.074)	6.246*** (0.105)	6.101*** (0.077)	5.395*** (0.104)
Num.Obs.	1752	1752	1752	1752
R2	0.304	0.340	0.082	0.187
R2 Adj.	0.303	0.339	0.081	0.186

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Linear models of the general and environmental reputation of the fictitious firm presented in survey experimental vignettes. Reputation indicators are measured on a 0–10 scale with higher values indicating better reputation. Treatment variables are binary. Standard errors in parentheses.

We conducted a pre-treatment and a post-treatment attention check. Our results already discard all individuals who failed the pre-treatment attention check, as we pre-registered. Because selecting observations based on a post-treatment attention check can introduce post-treatment bias (Kane, 2024), we use this piece of information differently. We show that rates of failures for the post-treatment checks were unaffected by the treatments (or their combinations). This reassures us that individuals were equally attentive to the vignettes across all treatment groups, thus differences in attentiveness do not explain the detected effects. In other words, the (low) rates of in-attentiveness only introduce noise, rather than bias. We provide evidence for this in Table H.2, where we fit linear probability models for the likelihood of passing the post-treatment checks on our treatment indicators. Model 1 introduces solely the corruption binary, while model 2 introduces the interaction term with the VERT vignette. We find that the rather high baseline rate of attentiveness to the vignettes (96% of individuals responded correctly, see intercepts) was not significantly affected by any treatment (nor their combinations).

Table H.2: Likelihood of passing post-treatment attention check

	Correct attention check answer	
	(1)	(2)
Corruption vignette	0.008 (0.008)	0.003 (0.012)
VERT vignette		-0.001 (0.013)
Corruption $\times$ VERT vignette		0.011 (0.017)
(Intercept)	0.963*** (0.006)	0.964*** (0.009)
Num.Obs.	1752	1752
R2	0.001	0.001
R2 Adj.	-0.000	-0.001

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Linear probability models for the likelihood of passing post-treatment attention check.  
Treatment variables are binary. Heteroskedasticity-robust standard errors in parentheses.

## H.2 Pre-registered heterogeneous effects

Here, we report pre-registered heterogeneous effects when splitting our sample by pre-treatment covariates. We pre-registered all tests reported in this section, but only formulated clear pre-registered hypotheses about the directionality of heterogeneous treatment effects where individuals' demographics motivated clear *a priori* expectations. To recap our expectations, we expected the moderating effect of VERT adoption to be stronger for individuals who hold more favorable predispositions towards companies, are more right wing, and are older. We find evidence of heterogeneous effects in these hypothesized directions.

To foreshadow one consistent finding, across all our splits we regularly find that the corruption treatment significantly damages a firm's general *and* environmental reputation. For this reason, and because we did not formulate heterogeneous effect expectations with respect to the main treatment variable, we limit our discussion below to findings with respect to the interaction term (which quantifies the extent to which a VERT mitigates the negative and diffuse reputational effect of a scandal). Finally, we caution the reader that these pre-registered splits force us to slice our samples significantly. This approach limits our statistical power (some models include as little as 438 observations) and, in some of the below tables, prevents us from estimating significant effects for the interaction term. For this reason, we refer the reader to the main text results as the most relevant experimental findings and treat these heterogeneous tests as suggestive.

First, we split our sample based on individuals' response with respect to the median recorded value for the attitudes towards firms (Table H.3). As pre-registered, we find that VERT adoption is effective in mitigating the negative effect of a corruption scandal on firms' general and environmental reputation *only* among individuals who are more pre-disposed towards business to begin with (here defined as being above the median on the question about whether firms positively contribute to society's prosperity). Among these individuals, effects are stronger than those for the whole sample reported in our main text.

Table H.3: Heterogeneous effects by attitudes towards firms

	General reputation		Environmental reputation	
	Anti-firm	Pro-firm	Anti-firm	Pro-firm
Corruption vignette	-2.740*** (0.183)	-3.689*** (0.199)	-1.163*** (0.177)	-1.692*** (0.181)
VERT vignette	0.974*** (0.177)	0.189 (0.177)	1.683*** (0.185)	1.109*** (0.184)
Corruption $\times$ VERT vignette	0.166 (0.279)	1.317*** (0.279)	-0.260 (0.290)	0.844** (0.271)
(Intercept)	5.393*** (0.111)	6.965*** (0.123)	4.770*** (0.113)	5.921*** (0.112)
Num.Obs.	835	917	835	917
R2	0.339	0.370	0.194	0.202
R2 Adj.	0.337	0.368	0.192	0.199

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Linear models of the general and environmental reputation of the fictitious firm presented in survey experimental vignettes. Reputation indicators are measured on a 0–10 scale with higher values indicating better reputation. Treatment variables are binary. Heteroskedasticity-robust standard errors in parentheses.

Next, we operate a similar split with respect to the responses about corporate sustainability standards. At the time of our pre-registration we did not draw specific expectations about effect heterogeneity with respect to ESG standards. For this reason, we treat this test as explorative. We split our sample based on whether individual responses were above or below the median value for the question about the importance of firm's compliance with ESG standards (Table H.4) and about individual believes that ESG standards contribute to society's prosperity (Table H.5). In this case, we do not find strong differences between the two groups: in both tables, we find a positive mitigating effect of VERT adoption, which falls short of statistical significance for environmental reputation in Table H.4.

Table H.4: Heterogeneous effects by importance of ESG standards

	General reputation		Environmental reputation	
	Anti-ESG	Pro-ESG	Anti-ESG	Pro-ESG
Corruption vignette	-2.830*** (0.194)	-3.642*** (0.202)	-1.200*** (0.169)	-1.682*** (0.194)
VERT vignette	0.098 (0.185)	0.892*** (0.183)	0.911*** (0.189)	1.736*** (0.183)
Corruption $\times$ VERT vignette	0.982*** (0.279)	0.630* (0.292)	0.428 (0.276)	0.299 (0.290)
(Intercept)	6.143*** (0.128)	6.326*** (0.130)	5.286*** (0.113)	5.479*** (0.122)
Num.Obs.	820	932	820	932
R2	0.272	0.401	0.127	0.244
R2 Adj.	0.269	0.400	0.124	0.241

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Linear models of the general and environmental reputation of the fictitious firm presented in survey experimental vignettes. Reputation indicators are measured on a 0–10 scale with higher values indicating better reputation. Treatment variables are binary. Heteroskedasticity-robust standard errors in parentheses.

Table H.5: Heterogeneous effects by importance of ESG standards for prosperity

	General reputation		Environmental reputation	
	Anti-ESG	Pro-ESG	Anti-ESG	Pro-ESG
Corruption vignette	-2.749*** (0.197)	-3.581*** (0.194)	-1.091*** (0.177)	-1.688*** (0.181)
VERT vignette	0.189 (0.199)	0.745*** (0.170)	0.986*** (0.204)	1.589*** (0.172)
Corruption $\times$ VERT vignette	0.686* (0.303)	0.876** (0.269)	0.101 (0.309)	0.497+ (0.264)
(Intercept)	5.946*** (0.129)	6.410*** (0.122)	5.184*** (0.121)	5.509*** (0.112)
Num.Obs.	668	1084	668	1084
R2	0.289	0.375	0.125	0.230
R2 Adj.	0.286	0.373	0.121	0.228

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Linear models of the general and environmental reputation of the fictitious firm presented in survey experimental vignettes. Reputation indicators are measured on a 0–10 scale with higher values indicating better reputation. Treatment variables are binary. Heteroskedasticity-robust standard errors in parentheses.

We continue by splitting the sample based on individuals' response to the climate change concerns question, with respect to its median value. In this case, too, we did not pre-register a specific expectation. Results (in Table H.6) show that the moderating effect of the VERT vignette on a firm's environmental reputation is positive only for individuals who are more concerned about climate change, thus showing that these individuals are more prone to respond to firms' efforts at mending their reputation via VERT adoption. Although we did not pre-register any expectation for this heterogeneous test, we speculate that this effect is reasonable considering that these are precisely the audience of public climate pledges aimed at mending reputation.

Table H.6: Heterogeneous effects by climate change concerns

	General reputation		Environmental reputation	
	Non-concerned	Concerned	Non-concerned	Concerned
Corruption vignette	-2.918*** (0.176)	-3.616*** (0.221)	-1.148*** (0.151)	-1.773*** (0.212)
VERT vignette	0.324+ (0.170)	0.749*** (0.199)	1.142*** (0.166)	1.591*** (0.205)
Corruption $\times$ VERT vignette	0.600* (0.258)	0.980** (0.313)	-0.033 (0.258)	0.682* (0.307)
(Intercept)	6.074*** (0.111)	6.407*** (0.144)	5.213*** (0.084)	5.565*** (0.143)
Num.Obs.	876	876	876	876
R2	0.337	0.354	0.157	0.226
R2 Adj.	0.335	0.352	0.154	0.223

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Linear models of the general and environmental reputation of the fictitious firm presented in survey experimental vignettes. Reputation indicators are measured on a 0–10 scale with higher values indicating better reputation. Treatment variables are binary. Heteroskedasticity-robust standard errors in parentheses.

Next, we split our sample by whether individuals have previous investment experience in any capacity (Table H.7). For this test, too, we did not pre-register any specific expectations in advance. We find a mitigating effect of VERT adoption only for individuals with previous investment experience. When seen against the light of observational evidence from Appendix Section F.3, this result provides

further suggestive evidence that investors are a significant target of firms' VERT adoption for reputation management.

Table H.7: Heterogeneous effects by investment experience

	General reputation		Environmental reputation	
	No experience	Experience	No experience	Experience
Corruption vignette	-3.026*** (0.268)	-3.342*** (0.165)	-1.312*** (0.250)	-1.509*** (0.153)
VERT vignette	1.059*** (0.252)	0.355* (0.155)	1.801*** (0.244)	1.212*** (0.159)
Corruption × VERT vignette	0.339 (0.391)	0.950*** (0.238)	0.102 (0.379)	0.407+ (0.239)
(Intercept)	5.743*** (0.166)	6.407*** (0.108)	5.109*** (0.156)	5.486*** (0.099)
Num.Obs.	438	1314	438	1314
R2	0.368	0.334	0.244	0.172
R2 Adj.	0.363	0.333	0.239	0.170

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Linear models of the general and environmental reputation of the fictitious firm presented in survey experimental vignettes. Reputation indicators are measured on a 0–10 scale with higher values indicating better reputation. Treatment variables are binary. Heteroskedasticity-robust standard errors in parentheses.

Next, we split our results by political affiliation (Republican *vs* Democrat, Table H.8). Consistently with our pre-registered expectation, we find that liberal individuals are less responsive to firms' usage of VERTs for mitigating reputational damage from a scandal, given that the interaction coefficient is only significant in the model of general reputation for republican respondents. That is, only individuals on the political right are prone to positively respond to a VERT as a way to mitigate the reputational damage induced by a scandal.

Table H.8: Experimental results by partisanship

	General reputation		Environmental reputation	
	Democrat	Republican	Democrat	Republican
Corruption vignette	-3.412*** (0.264)	-3.223*** (0.276)	-1.504*** (0.245)	-1.454*** (0.250)
VERT vignette	0.965*** (0.272)	0.088 (0.242)	1.854*** (0.258)	0.901*** (0.258)
Corruption × VERT vignette	0.558 (0.388)	1.050** (0.402)	0.184 (0.375)	0.451 (0.394)
(Intercept)	6.110*** (0.182)	6.785*** (0.169)	5.237*** (0.164)	5.892*** (0.160)
Num.Obs.	515	480	515	480
R2	0.377	0.299	0.248	0.134
R2 Adj.	0.373	0.294	0.243	0.129

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Linear models of the general and environmental reputation of the fictitious firm presented in survey experimental vignettes. Reputation indicators are measured on a 0–10 scale with higher values indicating better reputation. Treatment variables are binary. Heteroskedasticity-robust standard errors in parentheses.

Finally, we split our results by gender (male *vs* female, H.9), and by age (below or above the median age, Table H.10). We only pre-registered an expectation for the age variable: that the moderating effect of VERT adoption would be positive only for older individuals. We do not observe heterogenous effects

by gender but, as pre-registered, we observe that older individuals are those for whom VERT adoption is effective at mitigating the diffused reputational damaged induced by a corruption scandal.

Table H.9: Heterogeneous effects by gender

	General reputation		Environmental reputation	
	Female	Male	Female	Male
Corruption vignette	-3.228*** (0.190)	-3.303*** (0.209)	-1.597*** (0.184)	-1.319*** (0.186)
VERT vignette	0.859*** (0.181)	0.176 (0.193)	1.621*** (0.183)	1.066*** (0.197)
Corruption $\times$ VERT vignette	0.635* (0.278)	0.966** (0.299)	0.299 (0.279)	0.385 (0.296)
(Intercept)	5.991*** (0.127)	6.507*** (0.131)	5.313*** (0.125)	5.478*** (0.114)
Num.Obs.	897	855	897	855
R2	0.374	0.313	0.242	0.136
R2 Adj.	0.372	0.310	0.239	0.133

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Linear models of the general and environmental reputation of the fictitious firm presented in survey experimental vignettes. Reputation indicators are measured on a 0–10 scale with higher values indicating better reputation. Treatment variables are binary. Heteroskedasticity-robust standard errors in parentheses.

Table H.10: Heterogeneous effects by age

	General reputation		Environmental reputation	
	Younger	Older	Younger	Older
Corruption vignette	-2.717*** (0.207)	-3.816*** (0.191)	-1.279*** (0.195)	-1.648*** (0.174)
VERT vignette	0.710*** (0.187)	0.340+ (0.188)	1.508*** (0.189)	1.199*** (0.191)
Corruption $\times$ VERT vignette	0.478+ (0.287)	1.094*** (0.289)	0.081 (0.287)	0.599* (0.288)
(Intercept)	6.038*** (0.132)	6.457*** (0.127)	5.257*** (0.128)	5.534*** (0.110)
Num.Obs.	892	860	892	860
R2	0.282	0.404	0.180	0.197
R2 Adj.	0.280	0.402	0.177	0.195

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Linear models of the general and environmental reputation of the fictitious firm presented in survey experimental vignettes. Reputation indicators are measured on a 0–10 scale with higher values indicating better reputation. Treatment variables are binary. Heteroskedasticity-robust standard errors in parentheses.

### H.3 Results for other reputational profiles

We conclude by reporting results when modelling the two additional measures of firms’ reputation, as assessed by individual respondents. First, a measure of the reported integrity of the firm. Second, a measure of the firm’s social profile. We re-estimate our models with heteroskedasticity-robust standard errors in Table H.11. Evidencing that the corruption scandal results in a diffuse reputational shock, we find that it has a consistently negative effect on individuals’ assessments of the firm’s integrity *and* social reputation. We also find that VERT adoption is able to recover the damage suffered by the firm in terms of its social reputation, thus confirming that they are a strategy aimed at cleansing the firm’s reputation

across the board. We notice that VERTs do not significantly improve the perceived integrity of the firm, likely as a result of the high salience of the corruption scandal on this very proximate CSR aspect (especially in a survey context where attention to the treatment is heightened than in natural settings).

Table H.11: Experimental results. Social and governance reputation

	Integrity reputation		Social reputation	
	(1)	(2)	(3)	(4)
Corruption vignette	-3.382*** (0.107)	-3.513*** (0.144)	-2.571*** (0.104)	-2.763*** (0.143)
VERT vignette		0.747*** (0.130)		0.607*** (0.127)
Corruption $\times$ VERT vignette		0.326 (0.210)		0.440* (0.204)
(Intercept)	6.392*** (0.066)	6.002*** (0.088)	6.283*** (0.065)	5.967*** (0.085)
Num.Obs.	1752	1752	1752	1752
R2	0.362	0.389	0.258	0.286
R2 Adj.	0.362	0.388	0.257	0.285

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

Linear models of the social and governance reputation of the fictitious firm presented in survey experimental vignettes. Reputation indicators are measured on a 0–10 scale with higher values indicating better reputation. Treatment variables are binary. Heteroskedasticity-robust standard errors in parentheses.

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