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

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Abstract

Why do companies voluntarily pledge to reduce their GHG emissions? Existing work shows that, through voluntary environmental programs, firms demonstrate to stakeholders they intend to change behavior and, from that, they extract market and regulatory benefits. The mechanisms by which stakeholders buy into these pledges, however, are unclear. We argue that voluntary emission-reduction targets (VERTs) improve firms' reputation and display an overall image of corporate integrity, so firms use them to restore their reputation following events that damage their image, even those without environmental implications. This happens because stakeholders (the public, investors) infer corporate reputation using heuristics and available reputational information. We theorize that firms adopt more—but less ambitious—VERTs following reputational losses, in fear of public scrutiny. Difference-in-differences models on data about VERTs of 680 US public firms show a 24% increase in VERT adoption after involvement in criminal scandals, particularly among less ambitious targets. A survey experiment featuring 1,752 US respondents provides evidence for our heuristics-based reputational theory. Our results show that public reputational pressures create windows of opportunity for self-regulatory climate action. However, when pledges are aimed at restoring reputation, they result in shallower and easier-to-achieve targets which "cleanse" the negative record of a firm.

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Introduction

Greenhouse gas (GHG) emissions produced by companies are among the main determinants of the unfolding climate crisis. A 2017 report by the Carbon Disclosure Project (CDP) attributed about 70% of global GHG emissions since 1988—52% since the Industrial Revolution—to just 100 firms.[1] Ensuring that a relatively small group of "Carbon Major" companies significantly curb emissions is crucial for climate change mitigation. Although examples exist of ambitious state-led and international regulations mandating corporate emission-reduction, the costs of complying with bold climate policies undermine their efficacy and create political opposition (Genovese and Tvinnereim, 2019; Simmons, 2010).

Instead of being regulated by states, firms predominantly (pledge to) reduce emissions voluntarily (Büthe, 2010; Lerner and Osgood, 2022). Among voluntary programs (Potoski and Prakash, 2005; Malhotra, Monin, and Tomz, 2019), voluntary emission-reduction targets (VERTs), have risen to the core of climate policy since the 2010s (Green, 2013). Given the growing need to curb GHG emissions, VERTs were even given an institutional framework under the Paris Agreement—not just for firms but also for countries and cities (Green, Hale, and Arceo, 2024). This self-regulatory turn is visible across a myriad of corporate emission-reduction commitments, such as Amazon's "The Climate Pledge[2]."

Despite the growing wave of self-regulation, the negative climate consequences of firms' operations and emissions endure. Self-regulation, in fact, often results in modest environmental benefits (Berliner and Prakash, 2015; Green, 2021; Sellare et al., 2022). This private wave of regulation and its limited success raise two interlinked questions: why do firms decide to engage in voluntary self-regulation beyond what is prescribed by law and why are these commitments ineffective? We know that voluntary actions, even when shallow, offer benefits: they pre-empt state regulation (Malhotra, Monin, and Tomz, 2019) and generate market returns (Eberl and Schwaiger, 2005). But we still do not know why voters, policymakers, activists, investors, and consumers buy into such programs, *even if superficial*.

In this paper, we answer that reputational considerations drive the adoption of VERTs and limit their ambition. Expanding on evidence about the reputational benefits of voluntary environmental programs (Potoski and Prakash, 2005), we argue that VERTs improve firms' social reputation by presenting to stakeholders (e.g., the public and investors) a broad culture of corporate integrity. We demonstrate

[1] 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.

[2] See: https://www.aboutamazon.co.uk/planet/the-climate-pledge.

that firms increase their VERT adoption after blows to their reputation, in an attempt to repair it and avoid boycotts (Kam and Deichert, 2020) or disinvestment (Rao, 1997). Firms in reputational distress, moreover, adopt *less ambitious* VERTs, to minimize reputational backlash if they fail to meet pledges.

We show that VERTs are aimed at enhancing corporate reputation by studying a least obvious case: the effect of reputational losses pertaining to *non-environmental* areas of corporate social responsibility (CSR)—e.g., corruption or financial fraud scandals—on VERT adoption. When involved in financial scandals, a firm is exposed to negative media attention (Culpepper, Jung, and Lee, 2022; Dyck, Morse, and Zingales, 2010; Miller, 2006). We argue that defamation attracts the attention of the firm's stakeholders, who negatively update their overall judgment of its corporate social responsibility including their *environmental* evaluation of the firm.

We argue that stakeholders use *heuristics* to negatively update firms' reputation after a CSR violation, resulting in a deteriorated general and environmental reputation. The public uses "availability heuristics" (Tversky and Kahneman, 1973) and draws connections among CSR areas with an implicit reasoning as simple as: "If a firm is doing bad with respect to a social issue (e.g., corruption), it is likely doing as bad in the environmental space." Similarly, investors—who use aggregate indexes as heuristics (Brooks, Cunha, and Mosley, 2015) to guide investment choices (Cormier and Naqvi, 2023)—deduce firms' general image from aggregated scores on environmental, social, and governance (ESG) performance that get negatively affected after a CSR violation.

Anticipating these diffuse reputational blows, a firm embroiled in adverse events attempts to turn around this heuristics-induced reputational damage and restore a reputational premium by pledging emission reductions. That is, VERTs work as assets aimed at reassuring the public that involvement in misconduct is but an exception to an otherwise positive profile, a strategy to cleanse the damaged reputation of a firm.

Reputation does not only determine the decision to adopt VERTs but also their ambition. Companies under reputational distress and public scrutiny promote programs that yield high reputational benefits without creating space for further reputational costs. Firms whose reputation is at stake cannot risk missing such pledges or appearing over-promising, an outcome which would result in further backlash (Tingley and Tomz, 2022). Modest VERTs are a promising solution. They are easily reachable and typically offer leeway for adjustment based on performance, thus limiting the risk of backlash. Even when modest, VERTs are visible, easily advertised, and respond to issue salience.

We provide observational and experimental evidence for our reputational explanation of VERT adoption and our heuristics-based theory of corporate reputation. Observationally, we study the effect of *non-environmental* negative reputational shocks on firms' VERT adoption. We model firms' adoption of VERTs after their involvement in financial corporate criminal scandals with a difference-in-differences design on a sample of 680 US-based firms. Non-environmental scandals allow us to test our heuristics-based reputational theory of VERT adoption and to distinguish it from a regulation-preemption attempt that could follow environmental scandals (see examples in Malhotra, Monin, and Tomz, 2019, 19).

Our sample is made of publicly traded firms participating in the Carbon Disclosure Project (CDP) between 2011 and 2019. It 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, ConocoPhilips, and Occidental Petroleum. We show that firms increase the number of adopted VERTs by about 24% after being involved in a financial scandal. This increase is concentrated among less ambitious VERTs which are easier to meet: intensity targets and pledges on scopes 1 and 2 of the GHG Protocol.[3]

Additionally, we support our heuristics-based reputational theory of VERT adoption with a pre-registered[4] survey experiment that focuses on one of the most influential stakeholder of firms (Baron, 2014; Micheletti and Stolle, 2007; Vogel, 2010): the public. We also offer suggestive evidence that firms use VERTs to mend a damaged reputation towards investors (Appendix section E.2).

We surveyed an online sample of 1,752 US-based respondents which we randomly assigned to read vignettes on the profile of a fictitious US company. The design combines three vignettes manipulating our theoretical conditions: respondents were first assigned to a control vignette describing the firm or a treatment vignette which added information about the involvement of the firm in a corruption scandal. Neither vignette provided *any* environmental information. Next, half respondents were randomly assigned to read a vignette presenting the same firm's adoption of a VERT. We measure respondents' perception of the company's overall and environmental profile. Consistently with our heuristics-based reputational theory, the perception of the firm's *environmental* profile is about 27% worse among respondents exposed to the corruption-scandal vignette. VERT information partly restored this drop in corporate reputation, mitigating the negative effect of the scandal.

We contribute to various strands of political science. First, we offer a reputational explanation of

[3] See: https://ghgprotocol.org.

[4] Anonymous pre-registration is accessible at: 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.

the growing phenomenon of corporate emission-reduction pledges (Green, Hale, and Arceo, 2024). We contribute to a long-standing debate on the motivations behind self-regulation and its efficacy (Bayer and Aklin, 2020; Berliner and Prakash, 2015; Büthe, 2010; Green, 2013, 2021; Potoski and Prakash, 2005; Prakash and Potoski, 2006; Thrall, 2021). We show that environmental programs need not be false and misleading (Nemes et al., 2022) to generate "greenwashing." Firms can simply cleanse their reputation by doing "the bare minimum" and choosing less ambitious and easier to meet pledges (which yield reputational gains) rather than stricter programs with substantial environmental benefits which expose them to reputational risk. Moreover, whereas this literature has mostly studied CSR areas in isolation, we show that the dimensions of corporate reputation are, in fact, connected in the eyes of the public. Such connections prompt firms to alter their behaviors in ways that would seem, otherwise, disconnected.

Second, we contribute to the fundamental political science study on the importance of public opinion (Burstein, 2003). Research has shown that media coverage of politically salient matters affects public opinion (Barnes and Hicks, 2018; Kalatzi Pantera, Böhmelt, and Bakaki, 2023) and that policymakers respond to such salience (Bakaki, Böhmelt, and Ward, 2020; Schaffer, Oehl, and Bernauer, 2022). This mechanism does not just form *policy* opinions, but also views about companies, as shown by growing work on public opinion about firms and its policy implications (Kolcava, Rudolph, and Bernauer, 2021; Malhotra, Monin, and Tomz, 2019). We show that the public outrage at firms involved in financial scandals (Culpepper, Jung, and Lee, 2022) significantly damages their reputation and prompts them to pledge to responsible behaviors to try and restore such image.

Finally, we contribute to a literature in international political economy that studies firms in international regimes (Findley, Nielson, and Sharman, 2015; Simmons, 2010). We offer a reputation-based explanation for corporate behavior under the climate regime, contributing to work on corporate environmental behavior (Bayer, 2023; Colgan, Green, and Hale, 2021; Cory, Lerner, and Osgood, 2021; Genovese and Tvinnereim, 2019; Green et al., 2022; Kennard, 2020; Lerner and Osgood, 2022). We also bridge the corporate environmental politics literature and work on firms' behavior under other regimes—such as anti-corruption (Jensen and Malesky, 2018; Crippa, 2023), money laundering (Morse, 2019), and tax evasion (Arel-Bundock, 2017). We show that public responses to negative events can trigger companies into sustainable behaviors across unrelated regimes. This shows that regulatory areas can be linked not only by the overlap of their rules, as stressed by the regime complexity literature (Alter and Meunier, 2009), but also because their very participants (e.g., companies) are subject to multiple of them.

Public pressure for corporate self-regulation

Regulating firms' behavior has proved to be a Herculean task for states. State-based corporate regulations, often coordinated internationally, have been successful in some instances (e.g., in the case of corporate corruption, see Jensen and Malesky, 2018). However, firms' costs of adapting to such measures risk undermining compliance and turning regulation into empty paper (Simmons, 2010). Climate regulations, and their implied costs to firms, are no exception (Genovese and Tvinnereim, 2019; Green et al., 2022; Grumbach, 2015). As a result, states increasingly delegate regulation to private actors (Avant, Finnemore, and Sell, 2010; Büthe, 2010) in an attempt to exert effective regulatory pressure.

Regulation originating from private actors—also called "civil" regulation—works as a form of "soft law" (Vogel, 2008) consisting of voluntary commitments and initiatives. Private regulatory rules are typically set by a range of non-governmental organizations (NGOs), industry associations, or networks of firms and are voluntary in nature.[5] Civil regulation 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). It can take various forms—e.g., private, non-state, or market-based. Examples include participation in multi-stakeholder initiatives (Berliner and Prakash, 2015; Thrall, 2021), market-lead schemes (Bayer and Aklin, 2020; Green, 2021), and voluntary measures or pledges (Potoski and Prakash, 2005; Green, Hale, and Arceo, 2024)—our focus in this paper. In recent years, voluntary emission-reduction targets (VERTs) have grown in relevance (Green, 2013). According to the Net Zero Tracker, half of the 2,000 largest publicly listed companies in the world had a net zero VERT in 2023.[6]

The public has a pronounced influence in pushing companies to self-regulate (Baron, 2014; Micheletti and Stolle, 2007; Vogel, 2010). Although internal firms' stakeholders, too, have been shown to favor self-regulation (e.g., Lerner and Osgood, 2022; Onkila and Sarna, 2022), the public exercises strong demands for private regulation because it feels that its non-material interests are affected by disrespected or non-existent public policies (Potoski and Prakash, 2005; Prakash and Potoski, 2012).[7] Citizen campaigns, "naming and shaming" (Yadin, 2023), and boycott strategies (Kam and Deichert, 2020) against companies or industries are among the tools the public can adopt for promoting private regulation.

Adopting voluntary commitments, however, entails an opportunity cost to firms. Companies con-

[5] Scholars, however, have argued that the distinction between mandatory and voluntary regulation might be better conceptualized as a continuum rather than a dichotomy (Koenig-Archibugi, 2004).

[6] https://zerotracker.net/analysis/new-analysis-half-of-worlds-largest-companies-are-committed-to-net-zero.

[7] Besides demands for better environmental management, regulation of corporate crime (Culpepper, Jung, and Lee, 2022), and CSR (McBarnet, Voiculescu, and Campbell, 2007) are among the main demands of social activists.

cede to public pressures and self-regulate only if such actions offer them significant benefits. Although public opinion urges for self-regulation other stakeholders, like investors, value it too (Yu, 2012). A growing literature has explored political and economic explanations for companies' voluntary climate action. Improved financial performance (Bardos, Ertugrul, and Gao, 2020), avoided economic losses due to upcoming policy changes (Colgan, Green, and Hale, 2021), imposition of adjustment costs on competitors (Kennard, 2020), consumer loyalty (Park, Kim, and Kwon, 2017), political access (Werner, 2015), and regulatory pre-emption (Malhotra, Monin, and Tomz, 2019) are among the most prominent benefits that firms accrue from self-regulating.

Underpinning these arguments is the expectation that firms' stakeholders—regulators, activists, voters, consumers, or investors—buy into voluntary pledges. However, we still do not know the mechanism by which stakeholders consider firms who make such pledges credible. We expand on the idea that VERTs enhance corporate environmental reputation (Potoski and Prakash, 2005) by suggesting that they do so *across the board*, establishing a general public image of corporate integrity. Such reputational premium offers firms credibility towards stakeholders, allowing them to rip the above advantages.

Theoretical argument

We develop our theory in two steps. First, we explain VERT adoption. We argue that firms adopt VERTs when their reputation is hurt in order to restore a positive image. We theorize that VERTs enhance firms' social image so firms adopt them in response to reputational losses, even those unrelated to their environmental record. Second, we posit that, in the aftermath of adverse shocks, reputational considerations also affect the type of adopted VERTs, leading firms to make more modest pledges.

From corporate reputation to corporate climate pledges

Firms value their reputation. Under incomplete information about firms, stakeholders use the reputation of a company as a "mental image" that helps them to infer its unobservable qualities (Puncheva, 2008). For instance, individuals can use the environmental reputation of a firm to infer the likelihood that it respects climate standards (which is, normally, an unobservable attribute).

Companies cannot directly shape their reputation, since this is an individual-level construct, but they can try to build and sustain a positive image. In order to exercise such indirect influence, firms comply with stakeholders' consensus on the appropriate values and practices. They attempt to establish

legitimacy and increase their trustworthiness among stakeholders. Companies that do not follow social norms and hold a bad reputation, instead, suffer societal opposition in the form of direct mobilization (Micheletti and Stolle, 2007), boycotts (Endres and Panagopoulos, 2017; Kam and Deichert, 2020), and disinvestment on stock markets (Rao, 1997).

Companies' reputation is particularly vulnerable in times of corporate scandals, which generate a disapproval manifested in investors' pressure, intense public scrutiny, and negative media-propagated news that affect public opinion (Clemente and Gabbioneta, 2017; Miller, 2006; Vogler and Eisenegger, 2019).[8] Some firms are more vulnerable to criticism. Publicly-traded or business-to-consumer firms—such as Nike, Nestlé, or the Body Shop—might be more exposed to adverse effects on their brand value. However, reputation is important for virtually any firm: "even global firms that do not market to consumers [...] value public approval and dislike negative media attention" (Vogel, 2010, 77).

We contend that damages to the image of a firm, such as those induced by corporate scandals, propagate to *the entirety* of its social reputation, including its environmental image, creating a diffuse negative reputational shock. This holistic shock exists because stakeholders use various heuristics to create mental images of firms. We focus on two key stakeholders—the public and investors—and analyze the heuristics they use and how these contribute to the spread of reputational damage.

The public negatively updates its environmental view of a firm following corporate scandals. Individuals construct the environmental reputation of a firm, and make up for information scarcity on companies' environmental performance, using "availability heuristics" (Tversky and Kahneman, 1973)—shortcuts used to estimate probabilities of unobservable events by drawing on available information that is associatively proximate enough. Simply put, people weigh the likelihood that a firm respects climate standards by drawing on available news that the same firm has breached other social/governance standards, like anti-corruption or human rights. All areas of CSR pertain to firms' engagement with social norms, therefore such cognitive connections are easily established. Due to these connections, the media coverage devoted to a company's violation of a given CSR norm in a scandal shapes public perceptions concerning its reputation *as a whole* and influences what people think of it (Carroll and McCombs, 2003).

Corporate scandals turn into a diffuse reputational damage also among investors. These stakeholders

[8] The agenda-setting hypothesis (McCombs and Shaw, 1972), which has been widely investigated (Dearing and Rogers, 1996; Murphy and Devine, 2020; Vliegenthart and Boomgaarden, 2010), stresses the pivotal role the media have in crafting the public agenda and shaping public opinion. Similar to the linkage between media content and policy views (Baum and Potter, 2019), there is a linkage between media content and *corporate* views (Culpepper, Jung, and Lee, 2022). The public opinion regarding companies (i.e., their reputation) is informed by news about them.

often use indexes as benchmark for investment choices (Cormier and Naqvi, 2023). Indexes are simple heuristics that synthesize vast amounts of complex information (Brooks, Cunha, and Mosley, 2015). In the CSR space, a multitude of private market analytics agencies (e.g., Bloomberg, Moody's, Refinitiv, RepRisk) supply investors with aggregate indexes measuring firms' ESG performance. These indexes synthesize information pertaining to various CSR behaviors including negative incidents and positive measures. In a sense, an aggregate ESG score represents the social reputation of a firm among investors and guides their financial choices (Choi, Ferri, and Macciocchi, 2023). Similarly to what happens among the public due to availability heuristics, when companies are involved in corporate scandals these indexes get negatively updated, resulting in a diffuse shock among investors.

Following such diffuse reputational shocks, adopting VERTs is a strategic action by companies to anticipate and counter stakeholder opposition. Environmental commitments work as a reservoir of goodwill that insulates firms from (expected) negative reputational losses. Towards the general public, VERTs are a reputational asset because they leverage the very availability heuristics logic that propagates reputational damage. By adopting VERTs, firms attempt to cleanse their record and convince the public that their involvement in misconduct is an exception to an otherwise positive reputation, thus turning the availability heuristics logic around. Similarly, VERTs counterbalance the negative effect of scandals on ESG performance index by restoring a positive, aggregate ESG score.[9]

Among the universe of voluntary environmental programs (Potoski and Prakash, 2005), VERTs are particularly appealing when in reputational distress because they offer an immediate reputational gain. VERTs are pledges referring to a future date. Firms who adopt VERTs surely have to implement behavioral changes if they intend to meet them, but the evaluation of success is pushed to the future. At the moment of the pledge, VERTs provide a straightforward benchmark that can be easily promoted to the public. For example, a company can commit to reducing its carbon footprint by a certain percentage of future output by a specific date.[10]

[9] Such indexes rely on proprietary algorithms that process a myriad of recorded incidents pertaining to CSR. The weights attributed to the individual incidents and how information is processed are generally not public knowledge. However, we consulted the databases supplying information for two widely used indexes, RepRisk's RRI and Refinitiv ESG Scores, and we found that corporate criminal scandals (including corruption, money laundering, and fraud) and VERT adoptions are among the input data. This observation reinforces our interpretation that negative CSR incidents can turn into reputational shocks that affect the reputation of a firm towards investors (its ESG score) and that VERTs can restore such damage.

[10] The first page of Apple's 2020 sustainability report states, in bold, that the company is committed to total carbon neutrality by 2030. See: https://www.apple.com/environment/pdf/Apple_Environmental_Progress_Report_2020.pdf. Similar strategies are also common among "brown" companies. In an ExxonMobil's 2023 sustainability report, one can read (page 6) that the firm is committed to a "reduction in corporate-wide greenhouse gas emissions by approximately 20%." See: https://corporate.exxonmobil.com/-/media/global/files/advancing-climate-solutions-progress-report/2023/2023-advancing-climate-solutions-progress-report.pdf.

Examples of firms adopting these strategies abound. Consider Nike. After accusations for the use of sweatshops and appalling working conditions of garment workers,[11] the company not only pledged to protect labor rights[12] 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.[13] Similarly, the German conglomerate Siemens attempted to foster a "culture of integrity" following its infamous world-wide 2008 corruption scandal.[14] It did so not only by restructuring its anti-corruption policy but also by increasing its environmental commitments and creating a brand new "Environmental Protection, Health Management, and Safety" unit with the power to draft environmental commitments.[15]

To sum up, we expect firms to boost their voluntary climate commitments following reputational losses, even when other areas of their social reputation are damaged. Fearing societal opposition, firms adopt VERTs to cleanse their record and minimize reputational losses by demonstrating that they are responsible actors and that the misconduct was an exception to an otherwise positive record.

Negative reputational shocks and ambition of climate pledges

Here, we move our argument one step further and examine the implications of our logic for the *types of pledges* made. When companies experience reputational damages and are under public scrutiny, they promote VERTs that will yield reputational benefits without creating further reputational costs. In the aftermath of a reputational blow, firms are more sensitive to further reputational risk. Emission-reduction pledges can help ameliorate their public image but they can also backfire if companies appear over-promising or fail to meet their own targets (Tingley and Tomz, 2022; Yadin, 2023). Therefore, firms carefully choose the type of commitment they promote.

We argue that, under reputational distress, firms opt for more risk-averse and easier-to-meet targets. Even when shallow, voluntary environmental programs are considered credible by stakeholders (Malhotra, Monin, and Tomz, 2019). We conceptualize the potential reputational risk implied by climate targets in two different ways, considering the *type* of emission-reduction targets and their *scope*.

[11] Burhan Wazir, "Nike accused of tolerating sweatshops," *The Guardian*, May 20, 2001: https://www.theguardian.com/world/2001/may/20/burhanwazir.theobserver.

[12] David Teather, "Nike lists abuses at Asian factories" *The Guardian*, April 14, 2005: https://www.theguardian.com/business/2005/apr/14/ethicalbusiness.money.

[13] 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/.

[14] 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.

[15] Siemens 2009 Annual Report, page 51: https://www.siemens.com/investor/pool/en/investor_relations/e09_00_gb2009.pdf.

Concerning target *types*, firms typically choose among absolute or intensity targets. Absolute targets are expressed as GHG emissions in a future target year against those in a given base year. For instance, a firm in 2016 might commit to emit, in 2050, half of what it did in 2015. Intensity targets, instead, express the future, pledged emissions as a proportion of the future economic output. Because they benchmark GHG emission reductions against a future economic measure, these target types allow firms to account for the uncertainty of future economic conditions (Ellerman and Wing, 2003).

We expect that firms experiencing reputational damage will adopt more intensity VERTs over absolute ones. An intensity target offers a leeway for readjustment that reduces reputational risk in case the firm failed to meet it. Under intensity targets, in case of an unforeseeable slowdown in future output, the pledge would still be met if the *rate* of GHG emission reduction equaled or exceeded the rate of output slowdown. With unexpected output growth, instead, firms just need not increase their emissions *by a higher rate* than output growth, and the pledge would still be met. That is, even with actual emission growth, an intensity target would still be met if output increased at a higher rate.[16] From the point of view of GHG emissions reduction, absolute targets are thus preferable (Quirion, 2005). But from the point of view of making pledges at lower reputational risk, intensity VERTs provide firms with a more risk-averse solution. Thus, firms that are more sensitive to reputational risk should favor this risk-averse solution over the alternative.

Besides type, firms can pledge to reduce their GHG emissions along several parts of their value chain. Here, we follow the conceptualization of "scopes" offered by the GHG Protocol Corporate Accounting and Reporting Standard.[17] Under this protocol, scope 1 emissions are those occurring directly in the facilities owned or controlled by the firm. Scope 2 emissions are indirectly linked to the firm's activities through its purchase of electricity, steam, heat, or cooling. Scope 3 emissions, instead, result from the firm's activities through assets outside those owned or controlled by it which are in its value chain (both upstream and downstream). This scope includes emissions generated upstream of the value chain for producing goods sourced by the firm or those downstream, generated by the use of purchased goods.

We expect firms under reputational distress to favor emission-reduction targets on lower scopes, which they can more easily control, rather than along their value chains. The different scopes represent different levels of climate ambition and, consequently, reputational risks. The more ambitious the target, the more reputationally risky since high aspirations entail the risk of failing to achieve the

[16] For a primer on absolute and intensity targets, see: https://ccsi.columbia.edu/news/corporate-net-zero-pledges-bad-and-ugly.

[17] See: https://ghgprotocol.org/corporate-standard.

pledged emission-reduction. Reducing emissions along value chains, especially when complex, can be a particularly daunting task for companies and, in some cases, can run directly counter a firm's business model—reducing scope 3 emissions, for instance, is problematic for an oil and gas firm given that this scope includes emissions resulting from the downstream use of the fuel they sell. Therefore, when firms are under reputational distress, they should opt for targets mitigating emissions along more proximate scopes. Reductions at these levels are not reputationally risky but still show an engagement with environmental sustainability.

In sum, our reputational theory of VERT adoption expects firms will make more voluntary climate pledges when they are under reputational distress. In such conditions, they will also favor pledges that are more easily reachable, adjustable to uncertain future conditions, and, overall, less ambitious.

Observational evidence

Data and research design

We provide observational evidence for our argument by studying voluntary corporate GHG emission reduction commitments. We study firms submitting to the CDP climate change survey. The CDP is a voluntary self-disclosure program whose participating companies share information about their environmental performance, risk perspective, and initiatives. It is a convenient data source for us because it standardizes corporate reporting on VERT adoption providing a consistent framework to measure the number of emission-reduction targets, their type, and scope under the GHG Protocol. We draw on nine yearly waves of the "investors" dataset between 2011 and 2019.

Our dataset comprises exclusively publicly listed firms. It considers companies with a public image and for whom reputation, among the general public and investors, is a relevant concern (Vogel, 2010). In Table A.1, we present a basic sample description. We observe a good degree of variation in covariates for firms in our sample, which includes medium-sized firms as well as large multinationals or conglomerates. Companies in our dataset have some degree of public outlook which might not be a representative characteristic among the population of firms, including those that do not participate in the CDP. We consider this public outlook to be an important scope condition of our argument.

However, we have reasons to think the sample is not necessarily skewed with respect to its sustainability behavior. Because most firms voluntarily submit information to the CDP, one could wonder whether firms in our data are "greener" than the rest of the population. We advance three arguments

to defend our data choice. First, we note that, if it were true that only greener firms participated in the CDP, it would be rather difficult to observe changes in their voluntary climate pledges behavior at all because these would already have a rather high level. Such "ceiling effect" (Kane, 2024) would render any observable effect from the sample *stronger* among the general population.

Second, in fact, we note that not only green firms submit to the CDP. Out of the 75 investor-owned firms responsible for about 31% of global GHG emissions since the late 18th Century,[18] 41 submit a response to the CDP, including *all* oil majors (Chevron, ExxonMobil, BP, Shell, ConocoPhilips, Total, Eni) and large coal firms (AngloAmerican, RWE, Westmoreland). Even several state-owned oil firms participate (Gazprom, Pemex, Equinor, Petrobras). About a quarter of our 680 US-based firms, indeed, are classified as having an ESG performance below market averages (Table A.1).

Third, in appendix, we offer an empirical test that reassures us against issues of selection into our sample. We restrict our analysis[19] to firms belonging to the S&P 500 index, which CDP reaches out to and whose vast majority (439 in our data) submit a response. Because these firms do not submit to the CDP by their own initiative, this subgroup should be less impacted by self-selection into the sample. The group can also be considered more homogeneous, allowing us to formulate more proper comparisons.

We build various outcome variables from disclosed CDP data. For each company-year, we measure the total number of adopted GHG emission-reduction targets. This is our main outcome, which we use to test our expectation that VERT adoption increases after reputational damages. We also measure the different types of climate pledges that pertain to different degrees of reputational risk. First, we count the number of absolute and intensity VERTs adopted. Second, we measure the number of targets by their declared scope of application. Targets of scope 3 are considered more ambitious, whereas scope 1 and 2 pledges are less far-reaching. Because some CDP firms declare targets along scope 4, a more recent dimension not defined by the GHG Protocol,[20] we aggregate scopes by summing up two variables: the number of VERTs on scopes 1&2 and those on scopes 3&4.

In order to study the effect of reputational losses on VERT adoption, without incurring likely issues of reverse causality, we leverage shocks to firms' reputation which are plausibly exogenous to VERT adoption choices. We study the effect of corporate criminal scandals—i.e., events where firms violated corporate criminal laws—on VERT adoption, focusing on *financial* scandals.

[18] See the CarbonMajors report: https://carbonmajors.org/briefing/The-Carbon-Majors-Database-26913.
[19] Table D.1.
[20] Jessica Tasman-Jones, "Measuring Scope 4 emissions: what boards need to know," *The Financial Times*: https://professional.ft.com/en-gb/blog/measuring-scope-4-emissions-what-boards-need-to-know/.

Financial scandals provide us with several advantages. The timing of such events, which are not environmental, is likely unrelated to pre-existing corporate plans pertaining to the adoption of environmental programs, reassuring us of their plausible exogeneity. Moreover, our heuristics-based reputational theory supports the expectation that involved firms should respond to such negative events by increasing their VERT adoption. Criminal scandals are also salient enough events—with significant media coverage (Culpepper, Jung, and Lee, 2022)—which should significantly damage a firm's reputation. Finally, focusing on such events (as opposed to environmental scandals) ensures that we can rule out VERT increases due to environmental regulatory-preemption attempts (Malhotra, Monin, and Tomz, 2019).

We gather information on firms' 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. Its limited focus on US corporate criminal policies forces us to consider only American firms from our CDP data. Out of the 3,341 companies submitting to the CDP, we select the 680 US-based ones. We consider violations of laws preventing corporate corruption, money laundering or bank secrecy, improper pharmaceutical/drug-related behavior, anti-competitive business practices, and financial fraud.[21] We then merge the two data sources by relying on a fuzzy-matching algorithm based on company names.[22] We find 69 unique companies from the CDP that were involved in a federal law violation recorded in the CPR. For each match, we code the year that the scandal first became public knowledge.

Because firms who are involved in financial scandals likely differ fundamentally from those that are not, we apply a difference-in-differences design to estimate the effect of involvement in a financial scandal on the number of adopted VERTs. This design removes, by construction, all time-invariant confounders (e.g., size, industry, corporate culture) between "treated" and "control" firms by studying over-time changes in the outcome variable. 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.

Recent literature has highlighted that, in cases of staggered-treatment assignment like ours, the tradi-

[21] More specifically, we consider cases categorized as 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'.

[22] 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.

tional firm and year-fixed effect estimator (or "two-way fixed effect," 2FE) of a difference-in-differences can retrieve a biased estimate of the ATT. This bias is due to "forbidden comparisons" among units treated at different times and heterogeneous effects (Roth et al., 2023, 2219) requiring further, unwarranted, and untestable assumptions of effect homogeneity (Goodman-Bacon, 2021). To overcome these issues, we adopt an estimator proposed by Sun and Abraham (2021).[23] Standard errors are clustered at the company-level.

Results

We report estimated ATTs 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 along scopes 1&2 (4); and along scopes 3&4 (5). The table benchmarks the magnitude of the effect by reporting, for each outcome variable, a baseline representing its average when considering observations for never-treated firms 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 type		Target scope	
	(1) All targets	(2) Absolute	(3) Intensity	(4) Scope 1&2	(5) Scope 3&4
ATT	0.463***	-0.137+	0.599***	0.400***	0.049
	(0.097)	(0.078)	(0.112)	(0.077)	(0.081)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Baseline (average)	1.902	1.007	0.896	1.302	0.301
Num.Obs.	2982	2982	2982	2982	2982
R2	0.623	0.631	0.644	0.589	0.708
R2 Adj.	0.515	0.525	0.543	0.472	0.625

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

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.

Consistently with our argument, the number of adopted VERTs increases as an effect of involvement in a negative reputational shock—a financial criminal scandal. The estimated ATT is such that involvement in federal corporate criminal scandals increases the number of adopted VERTs by 0.463, about a +24% over the baseline average (1.902).

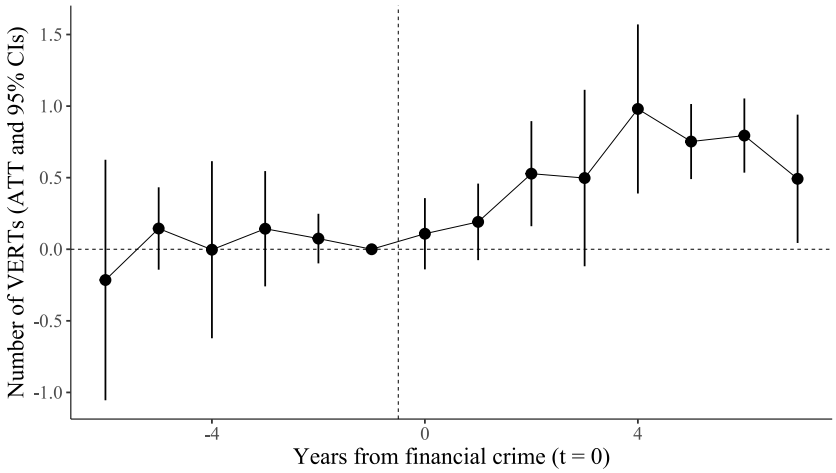
[23] In Appendix, we show that our results are similar when using three other estimators proposed for these setups: those by Borusyak, Jaravel, and Spiess (2024), Callaway and Sant'Anna (2021), and De Chaisemartin and D'Haultfoeuille (2022) which we also compare to the traditional 2FE (Figure B.1).

We also find that the effect is concentrated among reputationally risk-averse targets, as specified by our argument. We observe a significant effect of the reputational shock on the number of *intensity* targets (+0.599, or a +67% over the baseline) which guarantees firms a leeway of adjustment that shelters them from the risk of suffering reputational backlash. Instead, we observe a negative but noisy effect on absolute targets (-0.137, a -14% only significant at a 0.10 conventional level) suggesting a mild substitution effect between the two VERT types. Similarly, we observe an increase in the number of less ambitious VERTs, those defined along scopes 1 and 2 (+0.400, or +31%) while the number of adopted VERTs along the more ambitious scopes 3 and 4 does not change significantly.

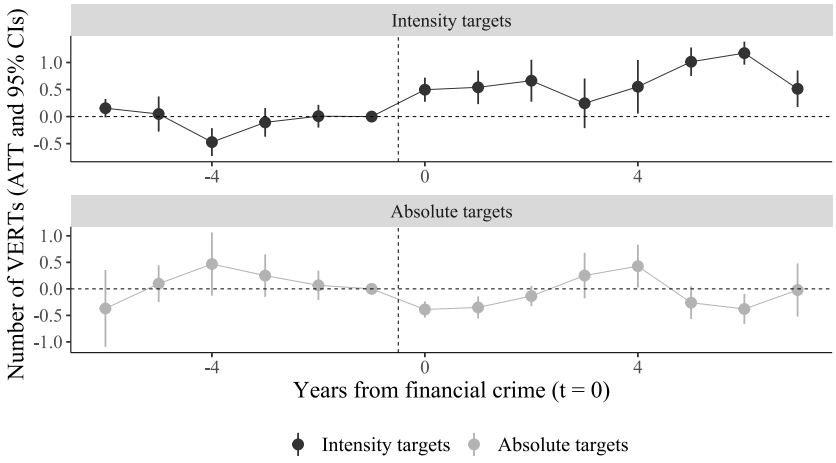
We study dynamic effects for our estimated ATTs, and offer placebo tests to rule out violations of the parallel trends assumption, by performing a fully-fledged event study of the Sun and Abraham (2021) estimator. Figure 1 reports dynamic ATTs for all five outcome variables in separate panels. We start from the total number of VERTs—of any kind—in Figure 1(a). Lending credibility to the parallel trends assumption, we find that companies involved in a scandal do not adopt more or fewer targets than the rest before the treatment. Pre-treatment estimates are all indistinguishable from zero and do not trend in either direction. Instead, involvement in a financial scandal increases companies' adopted VERTs after treatment. The effect is statistically significant at a 0.05 conventional level at least since post-treatment year 2 but trends positively immediately after the scandal. The 2-year lag for a significant effect is due to the fact that this variable pools together types and scopes of targets that have distinct and counteropposed effects, which we investigate below.

When looking at types of targets—Figure 1(b)—we still do not observe any significant divergence before treatment for absolute or intensity VERTs (except for an isolated *negative* effect on year -4 relative to intensity targets). After treatment, we observe an immediate and statistically significant drop in the number of absolute targets following a scandal, which becomes non-significant since year 2. Instead, the number of intensity targets increases significantly immediately after the scandal and remains sizeable and positive for most of the post-treatment period. Although estimates relative to the two variables are not precisely the opposite of each other, such patterns are indicative of a substitution logic between absolute and intensity targets under reputational distress, as implied by our arguments.

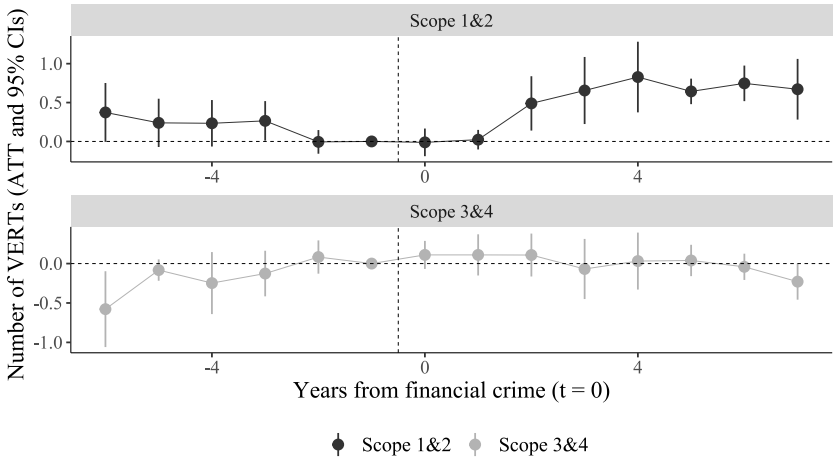
In Figure 1(c), we conclude by looking at the evolution of VERTs by scope. Here, too, we find mostly flat pre-treatment trends in the number of adopted VERTs along the considered scopes. After treatment, instead, we observe an increase in the number of VERTs adopted along scopes 1 and 2



(a) The number of voluntary emission-reduction targets increases steadily



(b) Absolute emission-reduction targets decrease and intensity ones increase



(c) Scope 1&2 emission-reduction targets decrease and scope 3&4 remain unchanged

FIGURE 1: A financial scandal increases VERT adoption, particularly less ambitious targets. Dynamic ATT estimates from Sun and Abraham (2021).

(significant since year 2) whereas targets on scopes 3 do not show any significant change.

These findings bolster our reputational argument: firms respond to adverse reputational shocks by adopting voluntary emission-reduction targets which can counter such damages to their reputation. When doing so, they strategically opt for more risk-averse strategies—like intensity targets—and more modest pledges—like VERTs on scopes 1 and 2—which they can more easily achieve.

Robustness tests

We test the robustness of our findings in Appendix. We find similar ATTs using other difference-in-differences estimators or other operationalizations of our count variables (Section B). We also rule out that our results reflect a general increase in VERTs after the Paris Agreement (Green, Hale, and Arceo, 2024). Although this should drive our estimates to the null (control units would also experience it), we address the concern by: *i*) truncating our panel in 2015 and *ii*) keeping our entire panel until 2019 but excluding firms treated after 2015 (Section C). Next, we find similar effects when considering only S&P 500 firms—a more homogeneous group comprised of large US publicly traded firms; when excluding repeated offenders—firms involved in more than one scandal; when excluding dismissed, acquitted, or dropped cases; when excluding pharmaceutical cases—which might impinge more directly on firms' environmental reputation; and even when excluding every one treated firm at the time (Section D).

We also offer tests to rule out alternative mechanisms (Section E.1). First, we exclude the possibility that financial criminal cases might have had environmental ramifications that we do not observe. Next, we consider whether a scandal increases VERTs because it causes firms to change their executive board, bringing in directors with more pro-environmental demographics—e.g., female or younger individuals (Cosma et al., 2021). We study executive board composition and find no significant changes except that the average executive board member becomes slightly younger after a scandal, albeit very negligibly so (from 73 to 72 years old, on average). Finally, we address the possibility that, after a scandal, board members are replaced by experienced individuals with larger personal networks, who might bring on board environmental practices that exist in their networks (Lerner and Osgood, 2022). We find that boards' network size actually *decreases* after a scandal, likely due to compliance with anti-corruption, anti-collusion, and anti-fraud guidelines which require directors to have fewer personal connections.

Experimental evidence

We also provide experimental evidence for our heuristics-based reputational mechanism explaining VERT adoption. We focus on the public as a relevant stakeholder of firms. In August 2024, we fielded a pre-registered survey experiment on an online sample of 1,780 US-based respondents sampled by Prolific (a survey platform that provides high data quality, see Eyal et al., 2021). The sample is representative of the US population by political affiliation, age, and gender (Table F.1).

The experiment is aimed at testing two core expectations from our theory: first, that individuals have a worse environmental perception of a firm after exposure to information that the company has violated other CSR standards; and second, that information on the adoption of VERTs manages to partly restore such drop in corporate reputation, working as a reputational asset for the company.

Survey design

After collecting pre-treatment information on respondents (and performing attention checks), we presented individuals with vignettes manipulating the theoretical conditions implied by our argument. All vignettes reported information on a fictitious company. First, we randomly assigned individuals to a control or treatment condition (see vignettes in Figure F.1). The control group received information on the company's plans to expand its operations in several locations across the 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 these 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 similar experiments where the order of the two vignettes is randomized, in our case the sequence of the two vignettes 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.

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—such as the perceived level of corruption or the strength of the industry's environmental profile. Appendix Table F.1 shows the company names

and associated industries that we used.[24] 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. 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.

After the vignette presentation, we collected our outcome data. We asked respondents to express, on a scale from 0 to 10, their perception of the company (where 0 indicates "extremely negative" and 10 indicates "extremely positive"). We asked respondents' separate views on the company's overall and environmental reputation.

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 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.[25]

Findings

Our effective sample is 1,752 respondents.[26] In Appendix, we show that pre-treatment covariates are balanced across our treatment groups, including basic demographics, climate change concerns, attitudes

[24] As clarified in our pre-registration, unfortunately, our limited sample prevents us from exploring heterogeneous effects by industry.

[25] 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.

[26] 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).

towards firms and corporate sustainability, previous investment experience, and political affiliation (Tables F.2 and F.3).

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***	-3.269***	-1.348***	-1.463***
	(0.105)	(0.145)	(0.108)	(0.144)
VERT vignette		0.525***		1.355***
		(0.145)		(0.144)
Corruption × VERT vignette		0.799***		0.337+
		(0.205)		(0.204)
(Intercept)	6.520***	6.246***	6.101***	5.395***
	(0.074)	(0.105)	(0.077)	(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.

In Table 2, we report our experimental findings. 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.

Consistently with our expectations, corruption negatively impacts the firms' general *and* environmental reputation. Perhaps unsurprisingly, its effect on the corporate general reputation is large: a 3.269 points average reduction, in the absence of a VERT, below the control group's 6.246 level (-52%). Availability heuristics make the corruption scandal have a significant negative effect also on the *environmental reputation* of the firm: a 1.463 points average reduction, in the absence of a VERT, below the average environmental perception of the firm for the control group (5.395, -27%).

As implied by our theory, VERTs do manage to partly restore such reputation and mitigate the reputational loss. The interaction terms in models 2 and 4 are positive and statistically significant—although only at a 0.10 level of significance in model 4, a weaker effect that is driven by individuals with pre-existing skepticism towards corporations (Table G.2). This implies that the VERT manages to mitigate the negative reputational loss experienced by the firm due to the corruption scandal, partly reassuring individuals about its general or environmental reputation.

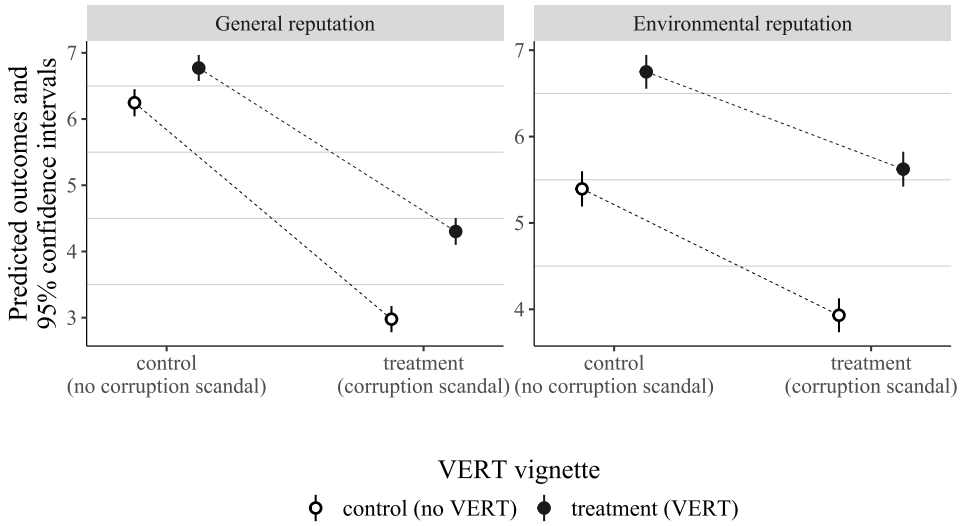


FIGURE 2: Corruption negatively affects firms' general and environmental reputation. VERTs mitigate this damage. Predicted outcomes from models 2 and 4 of Table 2.

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 manage to reduce the slope of this reduction to the point that firms involved in a corruption scandal enjoy a significantly better reputation (general or environmental) with a VERT than what they would have had, absent it. 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.

In Appendix, we present additional findings (Section G). We show that effects are still significant if we adopted heteroskedasticity-robust standard errors. Second, we estimate pre-registered heterogeneous treatment effects as subgroup analyses that consider pre-treatment attitudes towards companies and ESG, investment experience, political ideology, gender, and age. Attitudes towards companies yield perhaps the most interesting finding (Table G.2). We do find that VERTs do not have a reputation-enhancing effect among individuals that have more negative attitudes towards firms to begin with, consistently with an expectation we formulated in our pre-registration. That is, among the general public the less company-skeptic individuals are the likely targets of firms' VERT reputational strategy.

Additional mechanism: Reputation among investors

In Appendix we offer suggestive evidence that firms use VERTs to attempt at restoring a general positive image not only in the eyes of the public but also in the eyes of investors (Section E.2). We document the evolution of two widely used aggregated indexes of companies' ESG performance after financial scandals (from RepRisk and Refinitiv). Among investors, these indexes measure firms' social reputation and guide investment choices (Choi, Ferri, and Macciocchi, 2023). A company's financial performance is thus directly linked to its reputation among investors. When a firm is involved in a scandal, its ESG scores drop, with a potential negative impact on investors' behavior. In such conditions, the firm is in need of a reputational asset that can restore and boost its overall reputational score. VERTs can do just that.

By studying the evolution of these scores after financial scandals, we find evidence consistent with our argument. Immediately following the scandal, reputational risk increases. After that, it constantly improves, reaching a point where it is significantly lower compared to the period before the scandal. We attribute this improvement to the effect of VERTs which manage to reverse the negative reputational shock among investors. Consistently with this interpretation, we find that ESG index providers record a significant improvement specifically in emission-reduction commitments after a financial scandal.

Conclusion

Why do firms voluntarily adopt environmental initiatives and why do such programs yield modest environmental benefits? These questions are increasingly relevant as voluntary emission-reduction targets (VERTs) and "net zero" pledges become central to climate policy (Green, Hale, and Arceo, 2024). Existing work on voluntary environmental programs has shown that firms extract market and regulatory benefits from such pledges (Eberl and Schwaiger, 2005; Malhotra, Monin, and Tomz, 2019) but we still do not know why firms' stakeholders (such as voters, policymakers, activists, investors, and consumers) buy into such programs, even shallow ones. We expand on evidence that firms derive reputational gains from voluntary environmental programs (Potoski and Prakash, 2005) and argue that VERTs are a strategic choice made to present a culture of overall corporate integrity.

We develop a reputational theory that explains VERT adoption and why such targets are modest. We argue that firms increase their VERT adoption when suffering diffuse blows to their reputation. When involved in events that negatively impact their corporate social responsibility (CSR), stakeholders also

negatively update their *environmental* perception of firms by applying "availability heuristics" (Tversky and Kahneman, 1973). In a way, they implicitly conclude that a firm performing poorly with respect to a given social issue (e.g., corruption) is likely also performing poorly in the environmental space. Anticipating such diffuse blows to their reputation, firms attempt to turn this heuristics-based reputational logic around by increasing their emission-reduction pledges, as a reservoir of goodwill and a display of a culture of corporate integrity. We also argue that, when VERTs are adopted under reputational distress, pledges tend to be less ambitious and easier to meet, a strategic choice that minimizes the risk of suffering further reputational backlash from failing to reach goals (Tingley and Tomz, 2022).

Observational and experimental data support our argument. Observationally, we show that US firms involved in financial criminal scandals *with no direct environmental implication* increased their number of adopted VERTs by about 24% over what the rest of the sample did. Such effect is concentrated among shallower targets which are easier to meet and allow leeway for readjustment in order to be met: intensity targets and pledges along emission scopes 1 and 2. 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. We show that 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 the VERT manages 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 preferences (Culpepper, Jung, and Lee, 2022; Kolcava, Rudolph, and Bernauer, 2021; Malhotra, Monin, and Tomz, 2019), investigating whether such diffuse damages to the reputation of a firm also impact public opinion 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 (Section E.2). Finally, in the present work we have deliberately omitted to consider the implications of a diffuse *improvement* to corporate reputation, a phenomenon which likely triggers mechanisms beyond those described here and which could be subject of future work.

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Appendix

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

Contents

A Observational evidence: Sample description

We provide a basic description of our sample of publicly traded US-based firms from the CDP in Table A.1. Our dataset covers 680 firms, observed over the years 2011–2019. As the dataset is unbalanced, we have a total of 3,680 observations but missingness for some variables reduces the effective sample size.

The table first presents our dependent variables: the number of VERTs, the number of absolute or intensity VERTs, and the number of VERTs defined along scopes 1&2 or 3&4. When firms do not report any VERT in their CDP submission, we code the variable as missing, which causes us to consider only 2,983 (out of 3,680) firm-year observations. For these observations, we code the number of VERT by type and scope. Confirming the growing importance of emission-reduction targets (Green, Hale, and Arceo, 2024), we find that VERTs are rather ubiquitous in climate policy: firms that adopt them have almost 2 of them active at any given year, on average.

TABLE A.1: Descriptive statistics of US-based firms participating to the CDP (2011–2019)

	N	Mean	SD	Min	P25	Median	P75	Max
All VERTs (count)	2983	1.901	1.165	1.000	1.000	2.000	2.000	12.000
Absolute VERTs (count)	2983	1.018	1.050	0.000	0.000	1.000	1.000	12.000
Intensity VERTs (count)	2983	0.883	0.779	0.000	0.000	1.000	1.000	8.000
Scope 1&2 VERTs (count)	2983	1.295	0.943	0.000	1.000	1.000	2.000	9.000
Scope 3&4 VERTs (count)	2983	0.303	0.739	0.000	0.000	0.000	0.000	8.000
Assets (billions of USD)	2905	77.125	251.096	0.090	7.121	17.989	44.648	2714.610
Employees (thousands)	2894	59.359	144.304	0.021	9.139	22.500	59.592	2300.000
UNGC environmental violation (binary)	3680	0.048	0.213	0.000	0.000	0.000	0.000	1.000
RepRisk RRI	3134	20.081	12.851	0.000	12.560	19.750	25.537	66.526
Refinitiv ESG score	2058	59.100	14.942	8.410	47.052	60.345	70.410	92.540
Refinitiv ESG Emissions score	2058	70.949	22.297	0.000	58.088	76.140	88.580	99.810
Proportion of female directors	3067	0.212	0.111	0.000	0.133	0.200	0.273	0.722
Proportion of non-US directors	3062	0.082	0.135	0.000	0.000	0.000	0.118	1.000
Average directors' age (year)	3067	72.442	3.831	53.714	69.915	72.556	75.000	86.364
Average directors' network size (count)	3067	2941.205	1362.486	168.071	1986.706	2700.545	3625.618	9955.480

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 $90 million in assets (Motorola Mobility, 2012) or 21 employees (Royal Gold, 2016)—to large multinationals with asset value of about $2.7 trillion (JPMorgan, 2019) or more than 2 million employees (Wal-Mart, 2015).

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 was 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 below to test against the possibility that financial scandals are associated with environmental violations we are not able to observe (Table E.1). We do observe some of these violations in our sample, with about 5% of the observations being characterized by such events. Next, we report aggregated indexes for ESG performance, which we study in Section E.2. 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 more than a fourth of the observations in our sample being classified in what RepRisk considers medium (values between 25-49), high (50–59), or very high (60–74) risk exposure. We confirm these observations with an alternative ESG overall score offered by Refinitiv (available in our sample only for S&P 500 firms) measuring the overall ESG performance of a company. We also report a specific disaggregation of this index pertaining to emission-reduction initiatives. These Refinitiv indexes are reversed with respect to the RRI (lower values indicate worse ESG performance or emission-reduction initiatives). We observe a similar degree

of variation, with about a fourth of our firms being classified as having poor (0–25) or barely satisfactory (25–50) ESG or emission-related performance—Refinitiv considers scores of 50–75 and 75–100 as good and high performance, respectively.

Finally, we present covariates relative to the composition of the board of directors of the companies in our sample, all drown from BoardEx. These variables are used in Table E.1 to rule out alternative explanations relative to the financial scandals changing the composition of the executive board. We report the proportion of female members of the board of directors, which includes firms with no reported female member or with up to 72% of members being female. A similar variation also emerges when looking at the proportion of non-US directors. Finally, we 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 as young as 54 years old or as old as 86. Network sizes also vary considerably.

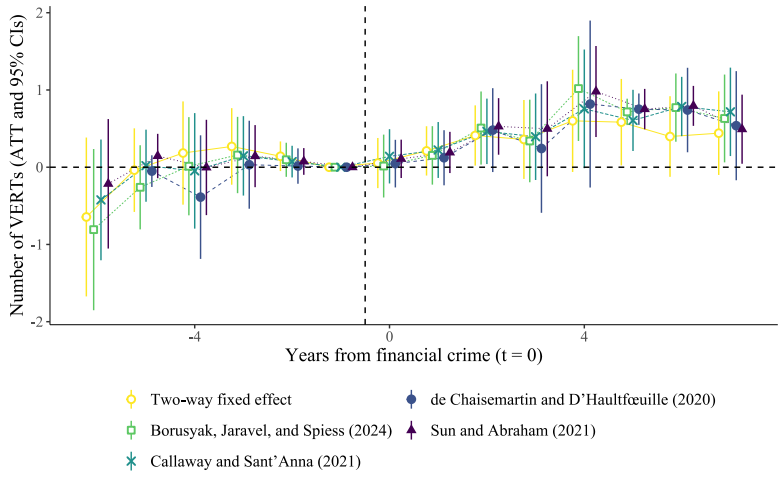
B Observational evidence: Alternative estimators and operationalizations

B.1 All staggered difference-in-differences estimators

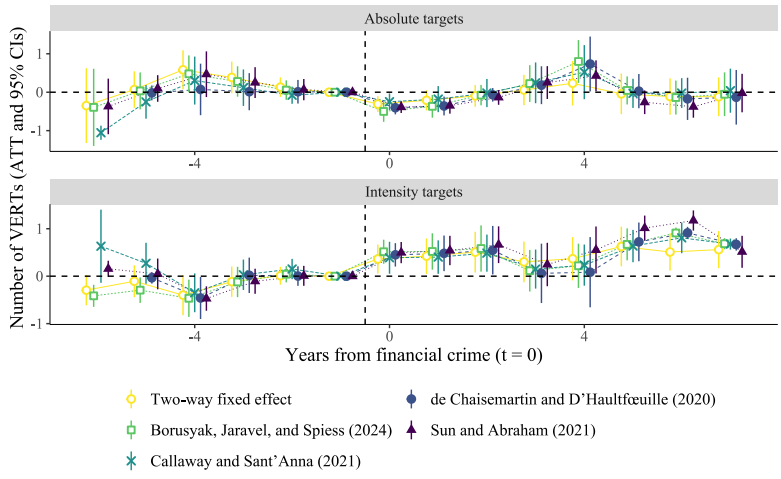
First, we address potential concerns that our estimates are dependent on the chosen Sun and Abraham (2021) estimator, among the various existing for staggered difference-in-differences. In Figure B.1 we replicate our main results, obtained with the estimator by Sun and Abraham (2021), using estimators from Borusyak, Jaravel, and Spiess (2024), Callaway and Sant'Anna (2021), and De Chaisemartin and D'Haultfoeuille (2022). 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 in later time periods—whose performance in staggered-treatment settings is widely debated (see Goodman-Bacon, 2021; Roth et al., 2023)—post-treatment we find consistent positive and significant results for the total number of VERTs as those documented in our main analysis. Next, we apply these very estimators to our other three outcome variables of interest: number of intensity and absolute targets—Figure 1(b)—and VERTs by scope—Figure 1(c). Other estimators report similar dynamic ATTs as those presented in the main text.

B.2 Alternative operationalizations of the outcomes

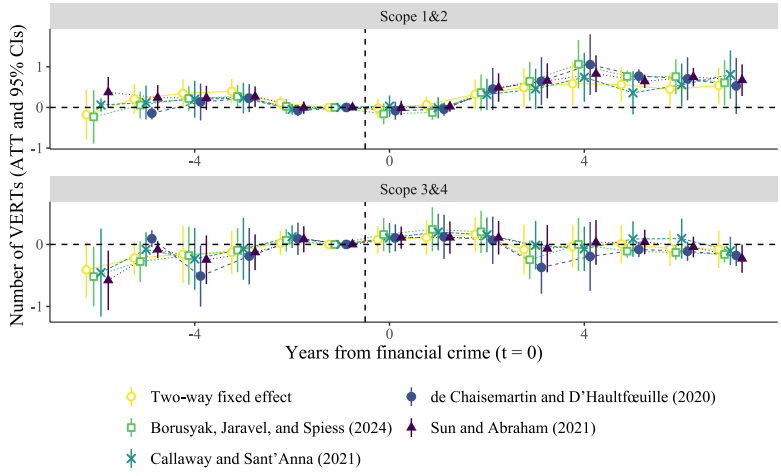
Next, we address potential concerns about the operationalization of our dependent variables. One potential concern is the use of raw counts for the number of (type, scope, and all) targets. One could object that this variable should rather be operationalized using logarithms of counts, to account for decreasing returns to additional climate pledges. In Table B.1 (columns 1, 2, 3, 5, and 6) we do just that, by modelling logarithms of the number of climate pledges +1 as dependent variables of our main models. Finally, in columns 4 and 7 we propose a potential, additional way of studying the substitution between absolute and intensity pledges or scope 1&2 and scope 3&4 VERTs. Instead of modelling the raw number of either type of pledge, we express the number of adopted intensity (scope 1&2) pledges as a share of the total count. We report our overall ATTs in Table B.1. Overall, we find similar, significant results as those presented in the main text. We note that using logarithms makes the substitution logic between the adopted absolute and intensity targets more evident, with the former outcome variable exhibiting a stronger and significant negative effect while the latter showing a positive (and larger) coefficient.



(a) Number of VERTs



(b) Type of VERTs (absolute *vs* intensity)



(c) Scope of VERTs (1&2 *vs* 3&4)

FIGURE B.1: Effect of a financial scandal on outcome variables of interest. All dynamic difference-in-differences estimates

TABLE B.1: The effect of a financial corporate criminal scandal on firms' VERTs. Alternative operationalizations of dependent variables

	Log all targets +1	Log absolute targets +1	Log intensity targets +1	Share intensity targets	Log scope 1&2 targets +1	Log scope 3&4 targets +1	Share scope 1&2 targets
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT of financial crime	0.138***	-0.162***	0.328***	0.399***	0.165***	0.027	0.349***
	(0.032)	(0.039)	(0.062)	(0.112)	(0.030)	(0.047)	(0.090)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline (average)	1.902	1.007	0.896	0.606	1.302	0.301	0.920
Num.Obs.	2982	2982	2982	2103	2982	2982	611
R2	0.647	0.681	0.669	0.696	0.640	0.703	0.741
R2 Adj.	0.547	0.590	0.574	0.570	0.538	0.619	0.601

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

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 logs of counts of targets +1 (models 1–3, 5, and 6); share of intensity targets over absolute ones (model 4); and share of scope 1&2 targets over scope 3&4 ones. Standard errors are clustered at the firm-level and reported in parentheses.

C Observational evidence: 2015 cut-off

Here, we address a potential concern of spuriousness of our results. Of the 69 firms treated in the US CDP sample, 20 were treated 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 would 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, described below.

C.1 Truncating data on 2015

First, we truncate our entire panel data to the year 2015. This choice forces us to work with an extremely limited dataset (just four waves of the CDP: 2011–2014) 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 then replicate our analysis: overall ATTs are in Table C.1. Detected effects have same sign as those in the main text and, in fact, are much larger in size (about a +53% increase for the total number of climate targets and a +101% for the number of intensity targets). However, pre-Paris we observe a significant increase not only for scope 1&2 VERTs, but also for scope 3&4 ones.

C.2 Exclude firms treated after 2015

As a second solution to the Paris threat to identification, we maintain our full panel data but exclude firms that were treated after the year 2015. That is, we consider only treatment cohorts 2011, 2012, 2013, and 2014. This analysis, too, is cleaned of any Paris effect because the treated firms we consider cannot have experienced the renewed reliance on voluntary pledges. Once again, we replicate our analysis (overall ATTs in Table C.2). In this case, too, we detect a positive, sizeable, and statistically significant effect of financial criminal scandals on the total number of emission-reduction targets (+45% over the baseline), in particular intensity ones (+102%) and VERTs on scopes 1&2 (+48%).

TABLE C.1: The effect of a financial corporate criminal scandal on firms' VERTs. Pre-2015 data

		Target type		Target scope	
	(1) All targets	(2) Absolute	(3) Intensity	(4) Scope 1&2	(5) Scope 3&4
ATT of financial crime	0.897***	0.104	0.793***	0.554**	0.332***
	(0.205)	(0.083)	(0.155)	(0.170)	(0.095)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Baseline (average)	1.682	0.897	0.786	1.353	0.308
Num.Obs.	1101	1101	1101	1101	1101
R2	0.791	0.836	0.842	0.773	0.819
R2 Adj.	0.682	0.750	0.760	0.655	0.726

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

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. Panel data truncated at 2015.

TABLE C.2: The effect of a financial corporate criminal scandal on firms' VERTs. Only cohorts treated before 2015

		Target type		Target scope	
	(1) All targets	(2) Absolute	(3) Intensity	(4) Scope 1&2	(5) Scope 3&4
ATT of financial crime	0.861***	-0.055	0.916***	0.633***	0.177
	(0.067)	(0.064)	(0.034)	(0.060)	(0.034)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Baseline (average)	1.906	1.006	0.900	1.306	0.294
Num.Obs.	2848	2848	2848	2848	2848
R2	0.621	0.627	0.639	0.583	0.701
R2 Adj.	0.518	0.526	0.541	0.470	0.619

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

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).

D Observational evidence: Exclusion of companies

D.1 Include only S&P 500 firms

Here, we limit our sample to the sole constituents of the S&P 500. This test serves two purposes. First, it limits our sample to a more comparable group of companies. This mitigates concerns that our main analysis might pool the effect across heterogenous firms with substantially different characteristics (including likelihood of furthering financial crime and climate pledges). Second, 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 mitigates concerns about self-selection into our data. Firms can voluntarily submit to the CDP, a problem which might bias our estimates if 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. Table D.1 reports findings from replicating our analysis on this limited group of firms. Results are consistent with those obtained when using the full US sample of firms.

TABLE D.1: The effect of a financial corporate criminal scandal on firms' VERTs. S&P500 firms only

		Target type		Target scope	
	(1) All targets	(2) Absolute	(3) Intensity	(4) Scope 1&2	(5) Scope 3&4
ATT of financial crime	0.337**	-0.053	0.390**	0.317***	0.013
	(0.113)	(0.087)	(0.135)	(0.090)	(0.097)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Baseline (average)	1.879	1.012	0.868	1.312	0.298
Num.Obs.	2413	2413	2413	2413	2413
R2	0.607	0.625	0.628	0.574	0.700
R2 Adj.	0.513	0.535	0.539	0.471	0.628

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

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.

D.2 Exclude repeated offenders

Here, we exclude from the sample any repeated offender, meaning companies that were involved in corporate criminal scandals multiple time. 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. Aggregated effects, shown in Table D.2, are comparable to our main results.

D.3 Exclude acquitted, dismissed, declined cases

Next, we show that results are substantively unchanged if we exclude from the sample cases that were eventually dismissed, acquitted, or declined. Although companies might suffer reputational costs, at the time of a corporate scandal, regardless of what its ultimate outcome will be, we intend this as a test against the possibility that results are driven by companies that were not eventually found responsible for criminal conduct. Importantly, in this test we *do not* exclude from the sample companies whose cases ended up in "non-prosecution agreements" (NPAs) or "deferred prosecution agreements" (DPAs), two extremely common outcomes in US-lead corporate criminal cases (Garrett, 2011). Notice that, although these judicial outcomes wave (or defer) a prosecution, they always imply that the company admits responsibility for the misconduct and agrees on a version of the illicit action with law enforcers. Results in Table D.3 are, again, comparable to the main ones.

D.4 Exclude pharmaceutical criminal cases

Further, we show that results are robust to the exclusion of companies that violated pharmaceutical laws. In few instances these cases can have environmental ramification. Thus, this test ensures that results on the effect of non-environmental criminal activity on environmental behavior are not driven by events

TABLE D.2: The effect of a financial corporate criminal scandal on firms' VERTs. Remove repeated offenders

		Target type		Target scope	
	(1) All targets	(2) Absolute	(3) Intensity	(4) Scope 1&2	(5) Scope 3&4
ATT of financial crime	0.460***	-0.110	0.569***	0.374***	0.074
	(0.068)	(0.066)	(0.034)	(0.061)	(0.034)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Baseline (average)	1.903	1.008	0.895	1.302	0.301
Num.Obs.	2919	2919	2919	2919	2919
R2	0.626	0.632	0.650	0.591	0.713
R2 Adj.	0.519	0.527	0.549	0.473	0.631

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

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. Data exclude firms involved in multiple corporate criminal events.

TABLE D.3: The effect of a financial corporate criminal scandal on firms' VERTs. Remove dismissed cases

		Target type		Target scope	
	(1) All targets	(2) Absolute	(3) Intensity	(4) Scope 1&2	(5) Scope 3&4
ATT of financial crime	0.619***	-0.126+	0.745***	0.584***	0.028
	(0.097)	(0.067)	(0.098)	(0.065)	(0.089)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Baseline (average)	1.903	1.008	0.896	1.302	0.298
Num.Obs.	2913	2913	2913	2913	2913
R2	0.626	0.630	0.649	0.588	0.710
R2 Adj.	0.519	0.524	0.548	0.470	0.626

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

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. Data exclude firms involved in corporate criminal events that were acquitted, dismissed, or declined.

that have some potential environmental implications. Table D.4, again, shows comparable findings to those detected in the main analysis.

D.5 Exclude one treated firm at the time (jackknife test)

Finally, we ensure that results are not driven by any single outlier treated company. We re-estimate our Sun and Abraham (2021) overall ATT with a jackknife approach. That is, each time we drop one treated unit from the analysis and re-estimate the effect. Figure D.1 shows the results. It reports the overall effects estimated in our main analysis and reported in Table 1 (in red) for comparison. It then reports

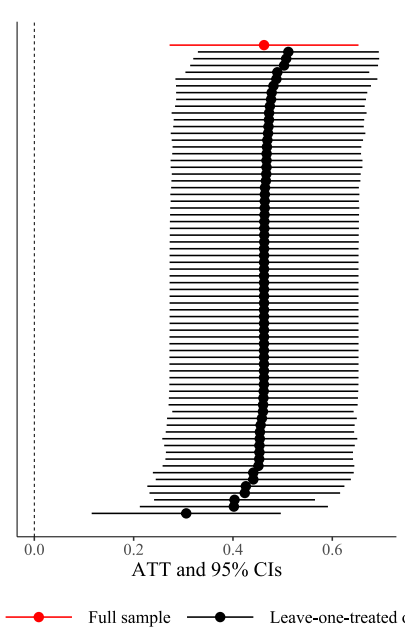
TABLE D.4: The effect of a financial corporate criminal scandal on firms' VERTs. Remove pharmaceutical offenses

		Target type		Target scope	
	(1) All targets	(2) Absolute	(3) Intensity	(4) Scope 1&2	(5) Scope 3&4
ATT of financial crime	0.453***	-0.201*	0.654***	0.378***	0.081
	(0.090)	(0.085)	(0.103)	(0.067)	(0.086)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Baseline (average)	1.904	1.009	0.895	1.301	0.303
Num.Obs.	2919	2919	2919	2919	2919
R2	0.626	0.633	0.647	0.591	0.709
R2 Adj.	0.518	0.527	0.545	0.473	0.625

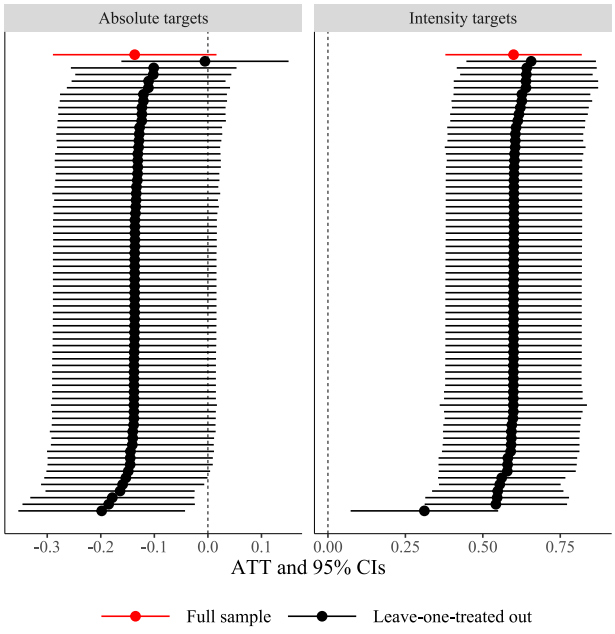
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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. Data exclude firms involved in pharmaceutical corporate criminal events.

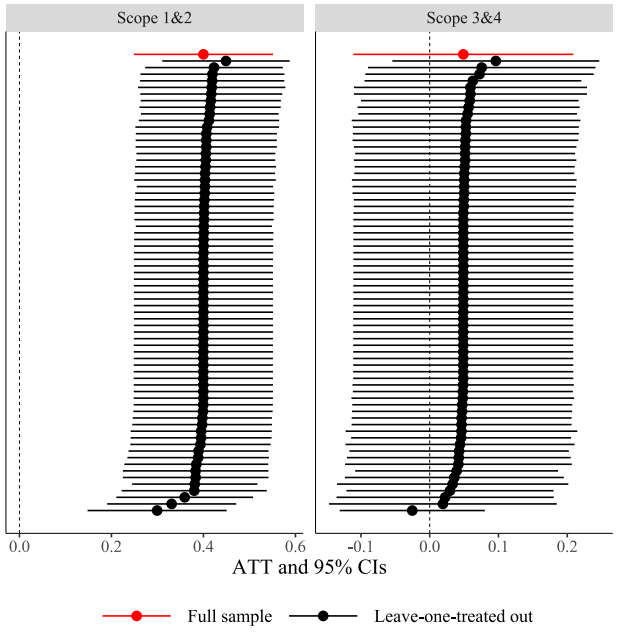
all results from the jackknife approach (black). Regardless of the exclusion of treated companies, the jackknife ATTs are consistent with those presented in the main text.



(a) Number of VERTs



(b) Type of VERTs (absolute *vs* intensity)



(c) Scope of VERTs (1&2 and 3&4)

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

E Observational evidence: Mechanisms

E.1 Non-reputational alternative explanations

In Table E.1 we report tests to rule out alternative mechanisms from our heuristic-based reputational theory of VERT adoption. 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.1: Alternative explanations: the effect of a financial corporate criminal scandal on reported environmental violations and boards of directors' composition

	UNGC violation	Board of directors			
	(1) UNGC	(2) Female	(3) Non-US	(4) Age	(5) Network
ATT of financial crime	0.006	0.005	-0.010	-0.632+	-199.147**
	(0.014)	(0.013)	(0.011)	(0.339)	(69.808)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Baseline (average)	0.044	0.210	0.080	72.517	2849.983
Num.Obs.	3679	3066	3061	3066	3066
R2	0.606	0.777	0.849	0.879	0.935
R2 Adj.	0.506	0.723	0.813	0.849	0.920

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

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, linear probability model); the proportion of the board of directors that is made of female (model 2) or non-US individuals (model 3); the average age of board of directors members (model 4); their average network size (model 5). Outcome data for model 1 come from RepRisk. Data for models 1 – 4 come from BoardEx. 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, thus leading to an increase in VERT adoption. All data for the tests aimed at addressing this argument, in models 2–5, come from BoardEx. First, we consider the proportion of female members of the board, the proportion of non-US members (a test made to account for the possibility that new directors are from more environmentally-friendly countries), and we consider the average age of the board (models 2–4). We model these variables in our difference-in-differences design and do not observe any significant effect, except for the average age. Following a scandal, the average board member becomes slightly younger (from about 73 to 72 years old). Such effect appears substantively minimal: a one-year decrease in average age for the board of directors hardly seems like the change that would explain the shifts observed in our main analysis. Finally, we study how the network size of board members evolves (model 5), in order to rule out the possibility that board of directors' networks increase following a scandal, with a consequent increase in corporate environmentalism that has been shown in similar 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*, likely 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.

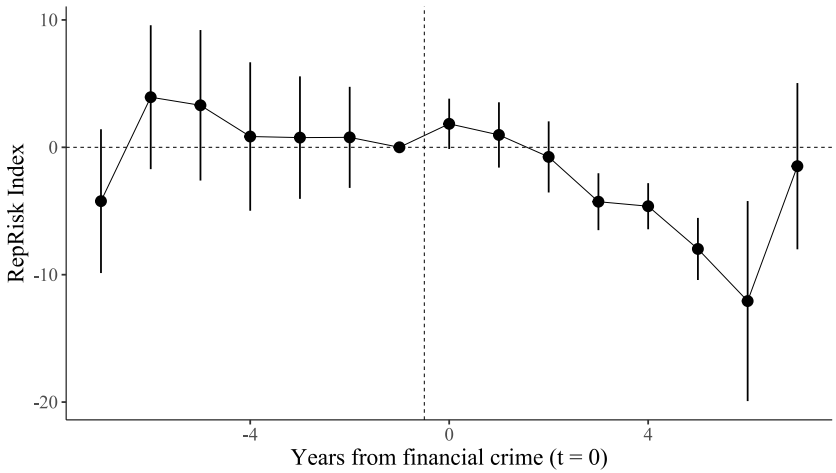
E.2 VERTs as a reputational response aimed at investors

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, following a diffuse reputational shock, firms adopt VERTs as a reservoir of goodwill to restore a culture of integrity across the board. An implication of our argument, in the stock market or investment space, is that firms adopt VERTs after a negative reputational blow to counter an expected deterioration of their environmental, social, and governance (ESG) ratings. Such ratings are important for firms because investors use them to determine investment choices. ESG indexes work as a sort of aggregate reputational heuristic indicating risky firms.

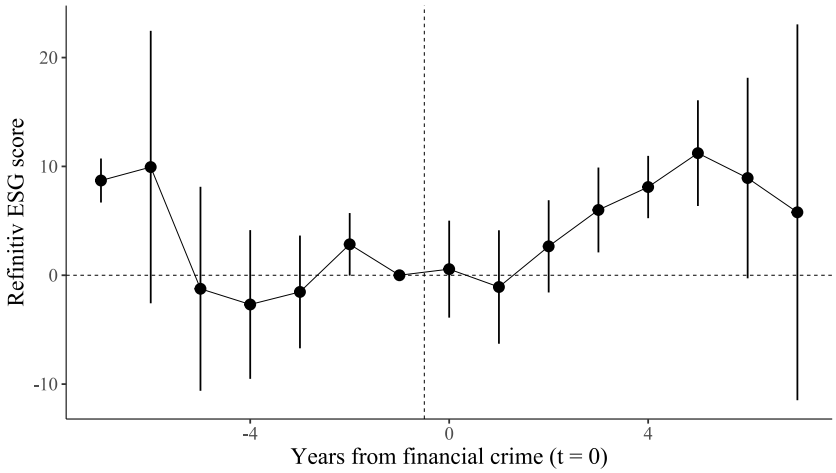
If our logic holds in the investment space too, then it should be observable when modelling ESG ratings themselves. We propose one last event study difference-in-differences to this aim. We model three variables. First, two aggregate indexes measuring overall ESG reputation. We consider the RRI index proposed by RepRisk, one of the most widely regarded 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 ESG score measured by Refinitiv, which synthesizes data about controversies and positive actions entertained by firms across a variety of ESG dimensions, attributing a higher value to firms with a better ESG performance—notice, thus, that *this score is inverted with respect to the RRI*: here, higher values indicate firms with a *lower* reputational risk. Finally, we model the specific ESG score pertaining to emissions, measured by Refinitiv. This is one of the constituents of the overall ESG score previously discussed. It captures the extent to which firms adopt initiatives to reduce their GHG emissions (higher values indicate lower ESG emission risk). We retrieve these variables, where available for firms in our sample, and model them in our event-study difference-in-differences.

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 affect them.

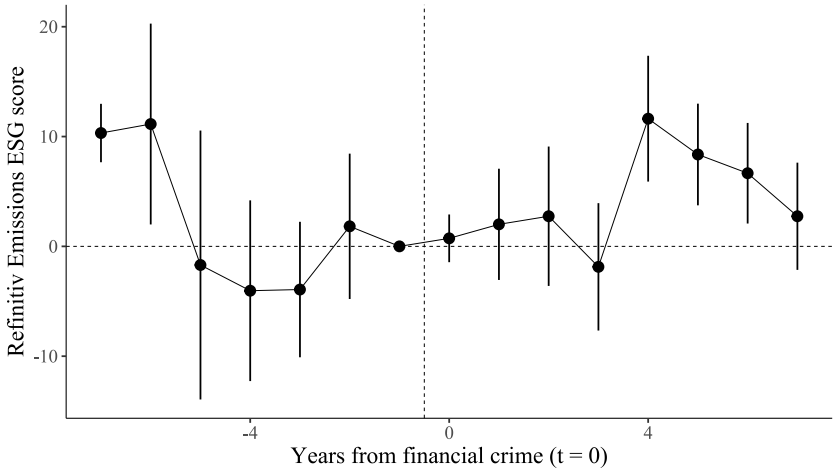
Dynamic ATTs are reported in Figure E.1. The evolution of these ESG scores is consistent with our argument. Pre-treatment, we do not observe any significant trend (except for isolated, higher values of the Refinitiv scores in years -6 and -7 before a scandal). The financial scandal leads to an immediate deterioration of involved firms' ESG rating, as implied by our argument, detected by the RRI—Figure 1(a). This diffuse reputational blow is then followed up by a progressive *improvement* of firms' ESG rating, indicated by estimates observed since year 3 post-treatment for the RRI and for Refinitiv's overall ESG score—Figures 1(a) and 1(b). Our argument contends that this improvement in firms' aggregated ESG metric is an effect of the VERTs that firms adopt when responding to the financial scandal to promote a culture of integrity across the board and compensate for the reputational loss. Figure 1(c) provides



(a) RepRisk's RRI index



(b) Refinitiv ESG score



(c) Refinitiv ESG Emissions score

FIGURE E.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).

evidence that the improvement in the ESG rating is indeed related to emission-reduction programs: the Emissions ESG score improves significantly years after the scandal. That is, VERTs manage to reverse the reputational loss generated by the scandal.

For completeness, we present aggregate ATTs in Table E.2, although dynamic estimates offer more valuable insights for our argument in this case, given that they allow to study how VERTs counterbalance an initial reputational shock. Aggregated ATTs abstract away from such rich dynamics and simply quantify the overall improvement on ESG scores following the financial crime, with RepRisk's RRI and Refinitiv's ESG scores recording a significant improvement of respectively 10% and 6% over the baselines, after the scandal.

TABLE E.2: The effect of a financial corporate criminal scandal on ESG indexes

	RepRisk	Refinitiv	
	(1) RRI	(2) ESG	(3) Emissions
ATT of financial crime	-1.930*	3.435*	2.837
	(0.859)	(1.552)	(1.782)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Baseline (average)	19.274	59.249	70.411
Num.Obs.	3133	2057	2057
R2	0.841	0.683	0.806
R2 Adj.	0.804	0.614	0.764

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

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: RepRisk's RRI score measuring reputational risk associated with ESG violations (model 1, higher values indicate higher reputational risk); Refinitiv's ESG overall score combining ESG controversies and positive actions (model 2, higher values indicate better ESG performance); and Refinitiv's ESG Emissions score measuring actions entertained by firms to reduce their GHG emission (model 3, higher values indicate more positive actions). Outcome data come from RepRisk (model 1) and Refinitiv (models 2–3). Standard errors are clustered at the firm-level and reported in parentheses.

F Experimental evidence: Design

F.1 Survey vignettes

We report the exact wording of the experimental vignettes in Figure F.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 F.1. Each respondent was presented vignettes relative to the same company and industry

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 CO2 emissions in half by 2050."

FIGURE F.1: Experimental vignettes

TABLE F.1: Company names and industries

Company name	Industry
MedTech Nexus	health
Arcadia Information	information technology
Pioneer Fabrications	manufacturing
SilverHaven Resources	mining
Vista Shops	retail

F.2 Sample description

Table F.1 presents the distribution of covariates of the individuals sampled in our survey experiment, representative of the US population by political affiliation, age, and gender.

In Table F.2, 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

TABLE F.1: Prolific US sample representativeness

	Size	Perc.
US Political Affiliation		
Democrat	295	29.5%
Republican	277	27.7%
Independent	428	42.8%
Age Range		
18-24	118	11.8%
25-34	175	17.5%
35-44	170	17.0%
45-54	158	15.8%
55-100	379	37.9%
Gender		
Male	491	49.1%
Female	509	50.9%

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 (binary), any higher education degree (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 F.3, 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 F.2: Balance in covariates relative to the treatment vignette condition.

		Corruption vignette					
		Control (N=873)		Treatment (N=879)			
		Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p
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 F.3: Balance in covariates relative to the VERT vignette condition.

		VERT vignette					
		Control (N=870)		Treatment (N=882)			
		Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p
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 Experimental evidence: Additional results

First, we replicate our results from Table 2 using heteroskedasticity-robust standard errors. This test was not pre-registered but we intend it as a robustness check. Table G.1 reports our findings, which are consistent with the main results. Next, we report pre-registered heterogeneous effects when splitting our sample by pre-treatment covariates: attitudes towards firms (Table G.2), towards corporate sustainability standards (Tables G.3 and G.4), climate change concerns (Table G.5), by previous investment experience (Table G.6), by political affiliation (Table G.7), gender (G.8), and by age (Table G.9).

TABLE G.1: Experimental results. Heteroskedasticity-robust SEs

	General reputation		Environmental reputation	
	(1)	(2)	(3)	(4)
Corruption vignette	-2.899***	-3.269***	-1.348***	-1.463***
	(0.105)	(0.142)	(0.108)	(0.131)
VERT vignette		0.525***		1.355***
		(0.133)		(0.134)
Corruption × VERT vignette		0.799***		0.337+
		(0.204)		(0.203)
(Intercept)	6.520***	6.246***	6.101***	5.395***
	(0.067)	(0.092)	(0.071)	(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 tandard errors in parentheses.

TABLE G.2: Experimental results by attitudes towards firms

	General reputation		Environmental reputation	
	Anti-firm	Pro-firm	Anti-firm	Pro-firm
Corruption vignette	-2.740***	-3.689***	-1.163***	-1.692***
	(0.202)	(0.197)	(0.210)	(0.191)
VERT vignette	0.974***	0.189	1.683***	1.109***
	(0.201)	(0.197)	(0.209)	(0.191)
Corruption × VERT vignette	0.166	1.317***	-0.260	0.844**
	(0.282)	(0.280)	(0.293)	(0.271)
(Intercept)	5.393***	6.965***	4.770***	5.921***
	(0.147)	(0.140)	(0.153)	(0.136)
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
Log.Lik.	-1774.922	-1986.803	-1807.454	-1958.716
F	142.151	179.067	66.870	76.806

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

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.

TABLE G.3: Experimental results by personal importance of corporate sustainability standards

	General reputation		Environmental reputation	
	Anti-ESG	Pro-ESG	Anti-ESG	Pro-ESG
Corruption vignette	-2.830***	-3.642***	-1.200***	-1.682***
	(0.204)	(0.202)	(0.203)	(0.201)
VERT vignette	0.098	0.892***	0.911***	1.736***
	(0.205)	(0.201)	(0.204)	(0.200)
Corruption × VERT vignette	0.982***	0.630*	0.428	0.299
	(0.282)	(0.290)	(0.281)	(0.288)
(Intercept)	6.143***	6.326***	5.286***	5.479***
	(0.149)	(0.144)	(0.149)	(0.143)
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
Log.Lik.	-1736.367	-2058.462	-1733.909	-2053.537
F	101.595	207.490	39.586	99.683

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

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.

TABLE G.4: Experimental results by importance of corporate sustainability standards for prosperity

	General reputation		Environmental reputation	
	Anti-ESG	Pro-ESG	Anti-ESG	Pro-ESG
Corruption vignette	-2.749***	-3.581***	-1.091***	-1.688***
	(0.219)	(0.190)	(0.223)	(0.187)
VERT vignette	0.189	0.745***	0.986***	1.589***
	(0.222)	(0.187)	(0.226)	(0.184)
Corruption × VERT vignette	0.686*	0.876**	0.101	0.497+
	(0.307)	(0.268)	(0.312)	(0.263)
(Intercept)	5.946***	6.410***	5.184***	5.509***
	(0.163)	(0.134)	(0.166)	(0.132)
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
Log.Lik.	-1401.432	-2392.056	-1412.140	-2374.497
F	89.971	215.623	31.560	107.559

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

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.

TABLE G.5: Experimental results by climate change concerns

	General reputation		Environmental reputation	
	Non-concerned	Concerned	Non-concerned	Concerned
Corruption vignette	-2.918***	-3.616***	-1.148***	-1.773***
	(0.185)	(0.221)	(0.186)	(0.217)
VERT vignette	0.324+	0.749***	1.142***	1.591***
	(0.185)	(0.220)	(0.186)	(0.216)
Corruption × VERT vignette	0.600*	0.980**	-0.033	0.682*
	(0.260)	(0.312)	(0.262)	(0.307)
(Intercept)	6.074***	6.407***	5.213***	5.565***
	(0.135)	(0.157)	(0.136)	(0.154)
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
Log.Lik.	-1813.882	-1975.043	-1817.942	-1958.216
F	148.001	159.213	54.104	84.907

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

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.

TABLE G.6: Experimental results by investment experience

	General reputation		Environmental reputation	
	No experience	Experience	No experience	Experience
Corruption vignette	-3.026***	-3.342***	-1.312***	-1.509***
	(0.283)	(0.169)	(0.274)	(0.169)
VERT vignette	1.059***	0.355*	1.801***	1.212***
	(0.284)	(0.168)	(0.275)	(0.169)
Corruption × VERT vignette	0.339	0.950***	0.102	0.407+
	(0.395)	(0.238)	(0.383)	(0.239)
(Intercept)	5.743***	6.407***	5.109***	5.486***
	(0.206)	(0.121)	(0.199)	(0.122)
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
Log.Lik.	-937.414	-2872.547	-923.713	-2879.464
F	84.134	219.204	46.707	90.646

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

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.

TABLE G.7: Experimental results by partisanship

	General reputation		Environmental reputation	
	Democrat	Republican	Democrat	Republican
Corruption vignette	-3.412***	-3.223***	-1.504***	-1.454***
	(0.274)	(0.278)	(0.265)	(0.274)
VERT vignette	0.965***	0.088	1.854***	0.901***
	(0.286)	(0.270)	(0.277)	(0.267)
Corruption × VERT vignette	0.558	1.050**	0.184	0.451
	(0.391)	(0.396)	(0.378)	(0.391)
(Intercept)	6.110***	6.785***	5.237***	5.892***
	(0.203)	(0.190)	(0.197)	(0.187)
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
Log.Lik.	-1137.079	-1049.412	-1120.217	-1043.331
F	102.960	67.602	56.128	24.556

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

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.

TABLE G.8: Experimental results by gender

	General reputation		Environmental reputation	
	Female	Male	Female	Male
Corruption vignette	-3.228***	-3.303***	-1.597***	-1.319***
	(0.198)	(0.212)	(0.198)	(0.210)
VERT vignette	0.859***	0.176	1.621***	1.066***
	(0.196)	(0.213)	(0.197)	(0.211)
Corruption × VERT vignette	0.635*	0.966**	0.299	0.385
	(0.278)	(0.300)	(0.279)	(0.297)
(Intercept)	5.991***	6.507***	5.313***	5.478***
	(0.143)	(0.152)	(0.143)	(0.151)
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
Log.Lik.	-1927.477	-1881.511	-1928.831	-1873.131
F	177.833	129.020	94.989	44.613

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

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.

TABLE G.9: Experimental results by age

	General reputation		Environmental reputation	
	Younger	Older	Younger	Older
Corruption vignette	-2.717***	-3.816***	-1.279***	-1.648***
	(0.206)	(0.203)	(0.207)	(0.202)
VERT vignette	0.710***	0.340+	1.508***	1.199***
	(0.204)	(0.204)	(0.204)	(0.204)
Corruption × VERT vignette	0.478+	1.094***	0.081	0.599*
	(0.287)	(0.289)	(0.288)	(0.288)
(Intercept)	6.038***	6.457***	5.257***	5.534***
	(0.148)	(0.147)	(0.148)	(0.146)
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
Log.Lik.	-1943.201	-1862.780	-1946.843	-1859.177
F	116.525	193.409	64.817	70.142

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

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.