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

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Word count: 8763

Abstract

Firms adopt measures like voluntary emission reduction targets (VERTs) to counter demands for regulation when suffering from a bad environmental reputation among the public. But how does the public form a company's environmental reputation, without observing environmental performance? We propose that it can do so by using *non-environmental* information. Following non-environmental reputation losses, e.g. financial criminal scandals, firms risk increased public demands for environmental regulation because of negative spillovers effects on their environmental reputation. Firms mitigate such public backlash by adopting VERTs. However, they choose more modest and reputationally risk-averse targets. Difference-in-differences models on data about 771 US firms show companies increase (less-ambitious) VERTs by 24% after a financial criminal scandal. A survey experiment featuring 1,752 US respondents backs up our argument. Results show that public reputation pressures create windows of opportunity for environmental self-regulation. However, in such cases, pledged targets are shallower and aimed at "cleansing" damaged reputations.

Firms' greenhouse gas (GHG) emissions are among the main causes of climate change. 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. Both state and private regulation (Bayer, 2023; Büthe, 2010; Lerner and Osgood, 2022; Potoski and Prakash, 2005) have been rising, in an effort to minimize the negative effects of firms' operations on the planet. The role of self-regulation is noteworthy, with voluntary emission-reduction targets (VERTs) rising to the core of climate policy since the 2010s (Green, 2013) and even getting an institutional framework under the Paris Agreement (Green, Hale, and Arceo, 2024).

Among firms' stakeholders—e.g., consumers, investors, lobbying groups—the *public* behaves as a "watchdog" of environmentalism, demanding public and private regulation of firms or industries that have a negative environmental reputation. Their strategies range from organized protests and advocacy campaigns to consumer boycotts (Endres and Panagopoulos, 2017; Kam and Deichert, 2020), all aiming at promoting firms' regulation. For instance, in September 2023 citizens gathered in New York to protest against oil and gas drilling on public land and federal waters.[2] In May 2023, demonstrators in the Hague opposed subsidies to the fossil fuel industry.[3] The public also exerts direct pressure on corporations, targeting companies like Shell,[4] Howden, Axa, and Loyds.[5]

Public demands for regulation are based on available information about firms' misconduct, typically produced by the media (Culpepper, Jung, and Lee, 2022). Information on environmental misconduct deteriorates firms' reputation and prompts the public to demand regulation, which in turn induces firms to self-regulate (Baron, 2014; Malhotra, Monin, and Tomz, 2019). However, information about companies' environmental performance is usually scarce. It is unclear how, in the absence of information on the environmental performance of companies, the public can act as a watchdog of environmentalism.

We propose and test a theory of information spillovers where *non-environmental information* about a firm's behavior shapes its *environmental* reputation among the public, ultimately influencing the firm's behavior. We argue that the public forms its environmental opinion of a company by using availability

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[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.nytimes.com/2023/09/28/climate/biden-drilling-leases.html

[3] See: https://www.euronews.com/2023/09/10/thousands-protest-after-dutch-government-awards-fossil-fuel-subsidies

[4] See:https://www.greenpeace.org.uk/news/activists-install-burning-your-future-sign-outside-shell-hq-as-company-announces-22-4-billion-annual-profits/

[5] See: https://extinctionrebellion.uk/2025/01/30/city-of-london-insurance-ceos-targeted-for-climate-crimes-in-a-second-day-of-action/

heuristics (Tversky and Kahneman, 1973). In the absence of specific information about a firm's environmental performance, stakeholders form their environmental opinion of a firm by relying on available information on its general social behavior. Effectively, the public makes inferences about the firm's environmental reputation with a reasoning as simple as "if a company is performing poorly with respect to one social aspect (e.g., corruption), it is likely doing as bad in environmental terms."

By affecting a firm's environmental reputation, spillovers also impact the company's behavior. Fearing societal opposition and strict state regulation, firms decide to self-regulate by adopting VERTs. Doing so offers them a double reputational advantage. First, VERTs address public demands for sustainability which arise from negative reputation spillovers. While the public might call for both public and private regulation, self-regulatory measures are controllable in terms of stringency and can help the firm preempt more stringent public regulations (Malhotra, Monin, and Tomz, 2019; Kolcava, Rudolph, and Bernauer, 2021), all while demonstrating the firm's "good character". Second, they turn the spillover logic on its head, supplying positive environmental information in the attempt to restore the firm's *social* reputation too—in a sense, "greenwashing" it. At the same time, when choosing to self-regulate, companies suffering negative reputational spillover shocks choose to minimize the risk of failing to meet their own targets, which would lead to intensified public backlash once more (Tingley and Tomz, 2022). They thus set more modest and controllable targets, which are more easily within reach.

Our argument implies two important points. On the one hand, the public can push for climate regulation even when information is scarce. Civil society can intensify its demands for environmentally sustainable plans during moments when a company's reputation is particularly vulnerable. Times of reputational distress can become windows of opportunity for civil society to promote corporate self-regulation since companies respond to spillover effects when managing their reputation. On the other hand, our argument shows that corporate efforts to demonstrate sustainability are largely driven by reputational concerns. Without regulatory oversight, this can lead to greenwashing, where companies adopt easily attainable targets to appear environmentally friendly without making the necessary effort that would likely truly make their behavior sustainable.

We test our reputation theory of information spillovers by focusing on the effect of corporate criminal scandals on involved firms' decision to adopt VERTs. Among the various types of non-environmental negative information that can reach citizens, corporate criminal scandals are likely to have the most significant impact on the public given the extreme salience of such instances (often propagated by the

media, see Culpepper, Jung, and Lee, 2022). These cases thus serve as a proof of concept of our theoretical framework. When a company is accused of financial criminal behavior—e.g., corruption, financial fraud, or money laundering—its environmental image is not directly at stake. However, the public processes media-produced information about the financial misconduct and negatively updates its opinion about the firm's environmental record, too. This generates spillovers damaging the firm's *environmental* reputation, too, increasing demand for environmental regulation. The firm responds to such demands by self-regulating in order to minimize public backlash. However, when a firm chooses to self-regulate, it does so "on its own terms", typically by setting modest targets that align with its interests. Ultimately, firms involved in non-environmental criminal scandals increase their number of adopted (modest) VERTs.

We provide observational and experimental evidence in favor of our argument. Observationally we study the effect of *non-environmental* criminal scandals 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 771 publicly-traded US-based firms that participate in the Carbon Disclosure Project (CDP) between 2010 and 2019.[6] We show that firms increase the number of adopted VERTs by 24% after being involved in a financial scandal with *no direct environmental implication*. This effect is largely concentrated among shallower VERTs which minimize reputational risk.

A pre-registered online survey experiment[7] on 1,752 US-based respondents adds evidence for our proposed mechanism—the use of availability heuristics. We use vignettes to manipulate our theoretical conditions and then 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 a corruption-scandal vignette which did not include any environmental information. A VERT vignette partly restores this drop in corporate reputation, mitigating the damage induced by the scandal.

We advance the political science study of environmental self-regulation by introducing a spillover theory of corporate reputation, demonstrating how actions in one area of corporate social responsibility (CSR) can shape perceptions in other areas. We use this theory to propose a novel explanation of the growing phenomenon of corporate emission-reduction pledges (Green, Hale, and Arceo, 2024). We

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[6] CDP covers more than 30% of the US-based "Carbon Majors" responsible for a significant share of global GHG emissions since the late 1980s—including Chevron (globally, 4th largest historical emitter), ExxonMobil (5th), Marathon, ConocoPhilips, and Occidental Petroleum.

[7] 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.

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). Whereas this literature has mostly studied areas of CSR and reputation in isolation, we show that, in fact, public stakeholders easily connect them. Such linkages prompt firms to alter their behaviors in ways that would seem, otherwise, disconnected.

In linking different areas of CSR and behavior, we contribute to the literature in international political economy that studies firms across regulatory 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 show that public responses to negative events can trigger companies into sustainable behaviors across unrelated regimes, thus bridging the corporate environmental politics literature and work on firms' behavior under other regimes—such as anti-corruption (Jensen and Malesky, 2018; Crippa, 2023). This shows that regulatory areas can be linked because their very participants (e.g., companies) are simultaneously subject to multiple of them.

Finally, we contribute to the 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 We provide additional evidence that public outrage over financial scandals (Culpepper, Jung, and Lee, 2022) significantly harms a firm's reputation. We show that such damage spills over to various aspects of corporate reputation, leading companies to pledge responsible behaviors even across (self-)regulatory areas.

Public pressure for environmental regulation and information spillovers

Regulating firms' behavior is essential for achieving decarbonization. Private regulation developed in addition to state regulation as a form of "soft law" (Vogel, 2008) that is not meant "to replace states, but to embed systems of governance in broader frameworks of social capacity and agency that did not previously exist" (Ruggie, 2004, 519). Various stakeholders—e.g., the public, investors, lobbying groups— advocate for state and private regulation that effectively minimize corporations negative impacts on the

environment (Baron, 2014; Malhotra, Monin, and Tomz, 2019).

Among these stakeholders, the public pushes for climate action either by requesting state interventions (Druckman, Valdes et al., 2019; Werner, 2012) or by demanding corporate action due to inadequate or absent public policies (Potoski and Prakash, 2005; Prakash and Potoski, 2012). Citizens deploy a range of strategies such as demonstrations, public campaigns, "naming and shaming" tactics, and consumer boycotts (Endres and Panagopoulos, 2017; Kam and Deichert, 2020; Yadin, 2023), all of which bring attention to the need to adopt climate policies and initiatives.

For behaving as a "watchdog" of firms' environmental sustainability, the public needs information on whether companies violate or comply with environmental standards. However, this information is usually scarce. Firms are complex organizations performing a plethora of operations. Many of them, particularly negative ones, occur out of sight. Although companies are often mandated to share information—through annual reports, public engagement events, or press releases—this information does not easily reach citizens (Smeuninx, De Clerck, and Aerts, 2020). How does the public behave as a "watchdog" of companies' environmental behavior, absent information on their environmental performance?

Availability heuristics and information spillovers

Absent information about firms' environmental behavior, we argue that the public uses heuristics to formulate judgements about companies' environmental reputation. Heuristics are mental shortcuts, rules of thumb used to formulate expectations on the likelihood of events and behaviors when information is scarce. We focus on a family of shortcuts, "availability heuristics", that draw upon available information to impute missing one (Tversky and Kahneman, 1973). With availability heuristics, people estimate probabilities and formulate judgments by the ease with which related instances or associations can be brought to mind. Such flow of information is enabled by "associative proximity" (Schwarz et al., 1991): the (perceived) likelihood that the missing information co-occurs with the available one. The greater the associative proximity, the easier (and more accurate) the application of heuristics.

We argue that the public forms its environmental reputation of a firm by deploying availability heuristics and borrowing observable information about the company's social behavior. Effectively, people weigh the probability that a company is environmentally "good" or "bad" by looking at available information on the company's performance in other areas of CSR. Such heuristics create spillovers among different areas of a firm's reputation, with information pertaining to one area of sustainability

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propagating to others.

Available information on a firm's social behavior feeds availability heuristics because it is associatively proximate to its environmental behavior. All areas of sustainability, in fact, relate to the firm's engagement with goals beyond profit-seeking and consider the firm's impact on communities, governance practices, and the environment (Ruggie, 2013). Firms themselves advertise sustainable practices simultaneously across a range of environmental, social, and governance standards.[8] Even *violators* of sustainability standards tend to infringe principles across multiple areas at the same time, as exemplified by the prevalence of corruption in the high-emission oil industry (Mahdavi, 2020).

Where does the public obtain information about firms' social behavior? As for many political phenomena (Iyengar and Kinder, 2010; McCombs and Valenzuela, 2020), information about corporate sustainability comes primarily from the media (Aizenberg and Müller, 2021; Deephouse, 2000). The public connects seemingly unrelated areas of corporate reputation by drawing on media coverage of a firm's social attributes to form an opinion of the same firm's environmental behavior.

While the media promote a vast amount of information that reaches the public, we focus on one specific type of highly salient media coverage—corporate criminal scandals. We focus on criminal scandals for two reasons: first, they are highly salient negative events that affect firms' non-environmental reputation; second, they can easily feed the public's heuristics. As such, they provide an ideal case for demonstrating our theory of spillover effects.

VERT adoption in times of corporate criminal scandals

During corporate criminal scandals—e.g., corruption, fraud, tax evasion, or money laundering—media coverage about a firm's misbehavior, and thus salience of its CSR among the public, is at its highest (Carroll, 2011; Kepplinger, Geiss, and Siebert, 2012). In such conditions, public disapproval manifests itself in intensified scrutiny and public opposition (Clemente and Gabbioneta, 2017; Miller, 2006; Vogler and Eisenegger, 2019). The general public often engages in public campaigns and boycotts (Kam and Deichert, 2020) which further reinforce such salience. Some firms are more vulnerable to public criticism. Publicly-traded or business-to-consumer firms—such as Nike, Nestlé, or ExxonMobil—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

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[8] Consider Patagonia's "footprint" webpage: https://www.patagonia.com/our-footprint/; or even ExxonMobil's sustainability reports: https://corporate.exxonmobil.com/sustainability-and-reports/sustainability.

media attention" (Vogel, 2010, 77).

Corporate criminal scandals, with their negative and highly salient impact on a firm's CSR, are situations that facilitate informational spillovers to the firm's environmental reputation. News about a firm's engagement in a scandal—e.g., in corruption—cause public perceptions of the same firm's environmental profile to deteriorate. Put differently, the public responds to the scandal by updating its environmental reputation of the firm with a reasoning as simple as: "if this firm is engaging in this type of misconduct (e.g., corruption), it is likely behaving as poorly environmentally."

We expect that a firm will respond to the blows against its *environmental* reputation, after a corporate scandal, by increasing adoption of voluntary environmental initiatives like VERTs. Availability heuristics turn a scandal into a diffuse reputational shock. In such conditions, adopting VERTs is a strategic action to anticipate and counter stakeholder opposition, and to pre-empt a state response that might lead to stringent corporate regulation (Baron, 2014). Specifically, VERTs offer two advantages. First, they respond to the public's demand for sustainability (Potoski and Prakash, 2005), which is driven by negative reputational spillovers of the scandal. The adoption of VERTs reassures the public that the company is taking firm action against corporate misconduct while also reducing the likelihood of public regulation (Egorov and Harstad, 2017; Kolcava, Rudolph, and Bernauer, 2021; Malhotra, Monin, and Tomz, 2019). Second, they turn the spillover logic around by presenting positive environmental information in an attempt to also restore the firm's social reputation. VERTs thus work as a reservoir of goodwill that mitigates (expected) reputational losses, by leveraging the very availability heuristics logic that propagates reputational damage. With VERTs, firms attempt to cleanse their record and convince the public that their involvement in misconduct is an exception to an otherwise positive profile.

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 but 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, instead, VERTs provide a straightforward benchmark that can be easily promoted.

Examples of firms adopting these strategies in response to corporate criminal scandals—i.e., non-environmental scandals—abound. Consider Nike. After accusations for the use of sweatshops and appalling working conditions of garment workers,[9] the company not only pledged to protect labor rights[10]

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

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

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.[11] Similarly, the German conglomerate Siemens attempted to foster a "culture of integrity" following its infamous world-wide 2008 corruption scandal.[12] It did so not only by restructuring its anti-corruption policy but also by strengthening its climate commitments and creating a brand new "Environmental Protection, Health Management, and Safety" unit with the power to draft climate pledges.[13]

The strategy to use VERTs in times of scandals may not resonate equally with all citizens, particularly those who are skeptical of corporations or CSR (see Goidel et al., 2025). These individuals might find voluntary environmental actions in times of scandals as an unconvincing form of "greenwashing." Nonetheless, from the perspective of firms, promoting VERTs remains a valuable strategy. These individuals are unlikely to be swayed by corporate actions but VERTs offer the opportunity to build a reputational premium that the rest of the public is likely to welcome favorably.

Our framework also allows us to formulate expectations on the *type* of VERTs adopted by a firm involved in the diffuse consequences of a scandal. Because these voluntary programs are adopted under reputational distress, the firm will promote VERTs that will yield reputational benefits without creating further reputational risk. In the aftermath of a reputational blow, the firm is more sensitive to further reputational risk. Emission-reduction pledges can help ameliorate their public image but they can also backfire if the company appears over-promising or fails to meet its own targets (Tingley and Tomz, 2022; Yadin, 2023). Failing to meet one's target can be a serious reputation liability for a firm who is already under intensified public scrutiny. Organizations that promote voluntary regulation often have systems to report companies that fail to meet their own targets. For instance, the data provider which we use in this study, the CDP, publishes a yearly ranking to praise participating firms that meet their own targets and shame those that fail to do so.[14]

As such, we expect that a firm involved in a corporate scandal will adopt more modest and easier-to-meet VERTs. These targets expose the firm to lower risk while also reassuring stakeholders. Stakeholders often consider even shallow voluntary environmental programs as credible and believe they reduce

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2005/apr/14/ethicalbusiness.money.

[11] 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/.

[12] 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.

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

[14] See the CDP "A list:" https://www.cdp.net/en/data/scores.

the need for public regulation (see Malhotra, Monin, and Tomz, 2019). A firm that is under intensified public scrutiny can limit further public backlash by avoiding pledges that would appear over-promising or that are likely not going to be met. The lesser stringency of modest voluntary targets make them more easily controlled by firms. Often, moreover, modest targets offer firms a leeway for readjustment that reduces reputational risk in case of failures to meet them. Conversely, more ambitious targets are also more risky since high aspirations entail the risk of failing to achieve the pledged emission-reduction.

In sum, our reputational spillover theory expects that firms respond to a non-environmental damage to their reputation (such as a corporate criminal scandal) by increasing climate pledges. This is done in an attempt to mitigate diffuse reputational blows, pre-empt public reputation, and turn reputational spillovers around. In such conditions, however, they will favor pledges that are more reputationally risk averse: those that are easily reachable and less ambitious.

Observational evidence

Data collection

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. Participating companies share information about their environmental performance, risk perspective, and initiatives. It is a convenient data source because it standardizes corporate reporting on VERT adoption providing a consistent framework to measure the number of emission-reduction targets and their type over time. We draw on ten yearly waves of the "investors" dataset between 2010 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 revelant concern (Vogel, 2010). In Appendix, 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 argue that the sample is not skewed in terms of sustainability behavior. Although firms voluntarily submit information to the CDP, raising concerns about whether our data reflects "greener"

firms, three reasons justify our claim. First, if only greener firms participated, observing changes in their voluntary climate pledges would be difficult due to a "ceiling effect" (Kane, 2024), making any effects in our sample even stronger in 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,[15] 41 submit a response to the CDP, including *all* oil majors[16] and large coal firms.[17] About a quarter of the 771 US-based firms that we study, indeed, are classified as having a CSR performance below their industry averages (Table A.1). Third, in appendix, we offer an empirical test that reassures us against issues of selection into our sample. We restrict the analysis[18] to firms belonging to the S&P 500 index, which CDP reaches out to and whose vast majority (458, in our data) submit a response. Because these firms do not submit information by their own initiative, this subgroup should be less impacted by self-selection into the sample. The group is also likely more homogeneous, allowing more proper comparisons.

We build various outcome variables from the CDP data. Our main outcome is, for each company-year, the total number of adopted GHG emission-reduction targets. We use this measure to test our expectation that VERT adoption increases after reputational damages. We also count the yearly *types* of climate pledges—i.e., intensity or absolute targets, scope 3 targets—adopted per firm.

The different types of pledges correspond to different degrees of VERT reputational risk. When pledging to reduce emissions, firms typically choose among absolute or intensity targets. Absolute targets express the pledged 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). An intensity target offers a leeway for readjustment that reduces reputational risk in case the firm failed to meet it. Under intensity targets, a firm just needs not to increase emissions *by a higher rate* than output growth, and the pledge would still be met. That is, an intensity target can be met even with actual emission growth.[19] Although not preferable to reduce emissions (Quirion, 2005), intensity targets thus

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[15] See the CarbonMajors report: https://carbonmajors.org/briefing/The-Carbon-Majors-Database-26913.

[16] Chevron, ExxonMobil, BP, Shell, ConocoPhilips, Total, Eni. Even several state-owned oil firms participate to the CDP (Gazprom, Pemex, Equinor, Petrobras)

[17] AngloAmerican, RWE, Westmoreland.

[18] Table D.1.

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

allow the firm to make pledges at lower reputational risk.

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.[20] Scope 1 and 2 emissions are those occurring directly in the facilities owned or controlled by the firm, including due to the 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. 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. Reducing emissions along value chains, especially when complex, can be a daunting task and, in some cases, can run directly counter a firm's business model.[21] We thus consider VERTs on scope 3 emissions as more ambitious types.

We count the number of absolute and intensity VERTs adopted and the number of targets that cover reductions of scope 3 emissions. Because our framework indicates that firms in reputational distress should favor risk-averse targets, we expect that reputational damage will cause firms to pledge to intensity over absolute VERTs. Firms in reputational distress should also not commit to reduce emissions over the ambitious scope 3.

Research design

We operationalize non-environmental reputational losses with spillover effects by considering financial corporate criminal scandals. That is, events where firms violated financial corporate criminal laws. We study the adoption of VERTs as a response to these events.

Financial scandals provide us with several empirical advantages. The timing of such events, which are not environmental, is likely unrelated to pre-existing corporate plans that a firm might have under way pertaining to the adoption of environmental programs. This reassures us of their plausible exogeneity. Moreover, criminal scandals univocally deteriorate firms' social reputation, representing a good test for our heuristics-based theory of environmental reputation and VERT adoption. Criminal scandals are, in fact, salient enough events—with significant media coverage (Culpepper, Jung, and Lee, 2022)—which should significantly damage a firm's reputation.

We gather information on firms' violation of corporate criminal laws from the Corporate Prosecu-

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

[21] 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.

tion 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 (771 US-based firms). 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.[22] We then merge the two data sources by relying on a fuzzy-matching algorithm based on company names.[23] We find 75 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.

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 is appropriate given that firms involved in financial scandals ("treated") likely differ fundamentally from the rest ("control"), but the *timing* of financial scandals is plausibly exogenous to VERT adoption. 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 traditional 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 *even* in case of parallel trends. 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).[24] Standard errors are clustered at the company-level.

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[22] 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'.

[23] 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.

[24] 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) and Callaway and Sant'Anna (2021), which we also compare to the traditional 2FE (Figure B.1).

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 covering scope 3 (4). The table benchmarks the magnitude of the effect by reporting, for each outcome, a "baseline" row representing the outcome variable's average when considering never-treated firms in the full panel and treated ones before the scandal.

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

	(1) All targets	(2) Absolute	(3) Intensity	(4) Scope 3
ATT	0.308*	0.005	0.303*	0.085
	(0.136)	(0.143)	(0.116)	(0.062)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Baseline (average)	1.278	0.695	0.583	0.237
Num.Obs.	4003	4003	4003	4001
R2	0.615	0.625	0.624	0.657
R2 Adj.	0.514	0.526	0.525	0.566

* $p < 0.05$. Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from Sun and Abraham (2021) estimator for staggered-treatment difference-in-differences. Dependent variables in all models are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses.

Consistently with our argument, the number of adopted VERTs increases as an effect of involvement in a *non-environmental* 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 0.308, about a +24% over the baseline average (1.278). That is, firms respond to non-environmental scandals by ramping up their climate pledges.

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 (+0.303, or a +52% over the baseline) which guarantee a leeway of adjustment that shelters firms from the risk of suffering reputational backlash. Instead, we observe a positive but insignificant effect on absolute targets. Similarly, we do not observe an increase in the number of VERTs covering scope 3 emissions.

Parallel trends

Internal validity of the estimates in Table 1 hinges on the untestable parallel trends assumption. Here, we provide evidence for its validity in our context, by presenting dynamic effects for our estimated Sun

and Abraham (2021) ATTs. Pre-treatment estimates, and their direction, are informative of the extent to which the assumption is likely not holding ahead of the treatment.
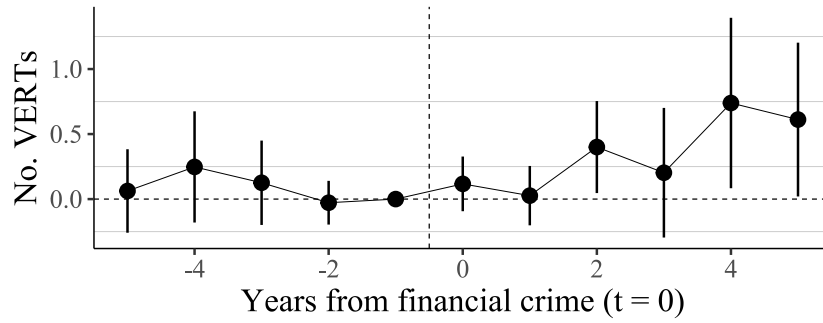
(a) The number of voluntary emission-reduction targets (VERTs) increases steadily
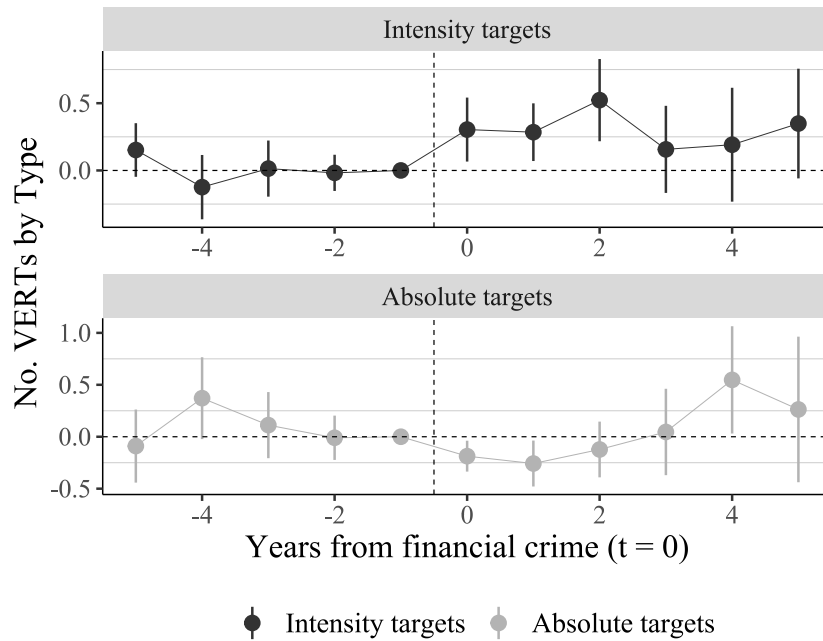
(b) Absolute VERTs decrease and intensity ones increase
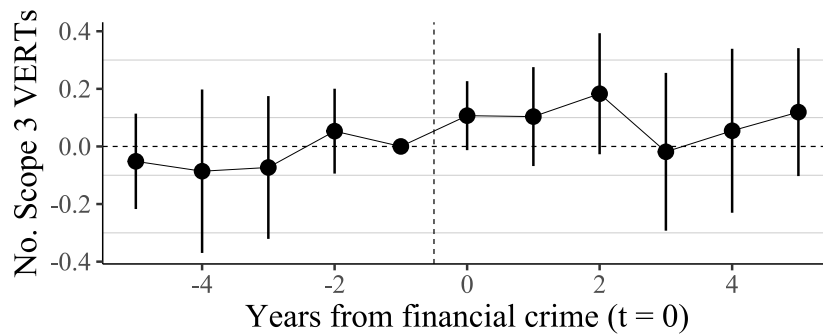
(c) The number of VERTs covering scope 3 remains unchanged

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

We study dynamic effects for our estimated ATTs, and offer placebo tests against 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 four 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 in the five years prior to the treatment. Pre-treatment estimates are all small and statistically indistinguishable from zero. We do observe a modest *decreasing* trend before treatment which, if anything, biases against our argument. Reversing this trend, 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 retrieving a significant effect is due to the fact that this variable pools together types of targets that have distinct and opposing effects, which we investigate next.

When looking at the number of targets by type—Figure 1(b)—there is no statistically significant difference between treated and control firms before treatment. After treatment, we observe an immediate and statistically significant drop in the number of absolute targets following a scandal, which becomes non-significant since year 2. The number of intensity targets increases significantly immediately after the scandal and remains sizeable and positive until year 3. 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 argument.

In Figure 1(c), we conclude by looking at the evolution of VERTs including scope 3. Here, we find a slowly increasing pre-treatment trend that is, however, indistinguishable from zero. After treatment, the difference-in-differences grows but remains insignificant.

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

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 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; and even when excluding every one 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. This is particularly important for us, given that our theory implies *spillover*—and not direct—reputational damages of scandals. 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. 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 conclude by providing experimental evidence for our heuristics-based reputational mechanism explaining VERT adoption. We test two core expectations from our reputational spillover 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 risk-mitigation strategy. We fielded a pre-registered survey experiment in August 2024 on an online sample of 1,780 US-based respondents.[25] The distribution of covariates for political affiliation, age, and gender in this convenience sample mirrors those of the US population (Table F.2).

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[25] The respondents were sampled by Prolific (a survey platform that provides high data quality, see Eyal et al., 2021).

Survey design

After collecting pre-treatment information on respondents (and performing an attention check), we presented individuals with vignettes manipulating the theoretical conditions implied by our argument. All vignettes reported information on a fictitious company. First, we randomly assigned individuals to a control or treatment condition (exact wording is 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 the corruption and VERT vignettes allocated respondents to four groups with the same probability: a control and treatment group unexposed to VERT information and a control and treatment group exposed to the VERT vignette. Unlike common practice in 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.[26] 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.[27]

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

We estimate linear models of the general and environmental reputation outcome variables, featuring

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[26] As clarified in our pre-registration, unfortunately, our limited sample size significantly limits the extent to which we can explore heterogeneous treatment effects by industry.

[27] 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.

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.[28]

Results

Our effective sample is 1,752 respondents.[29] In Appendix, we show that pre-treatment covariates are balanced across our treatment groups, including basic demographics, climate change concerns, attitudes towards firms and CSR, previous investment experience, and political affiliation (Tables F.3 and F.4).

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

* $p < 0.05$. 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

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[28] 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.

[29] 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).

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, however, cause the corruption scandal to also elicit a significant negative effect 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 only in model 2 (the interaction term in model 4 is statistically significant at 0.10 alpha). 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.
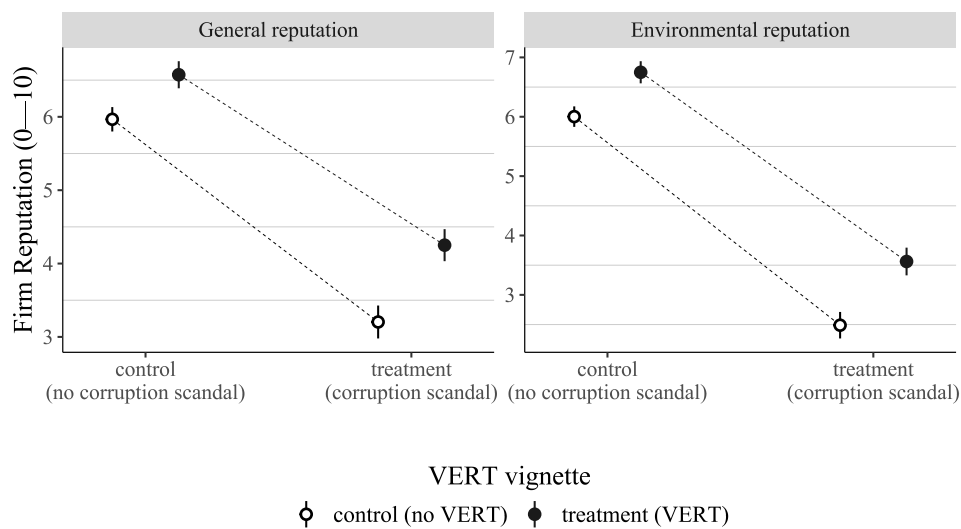
FIGURE 2: Corruption negatively affects firms' general and environmental reputation. VERTs mitigate this damage. Predicted outcomes and 95% confidence intervals from models 2 and 4 of Table 2.

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.

We present additional findings in Appendix (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. In Table G.2, in particular, we find evidence for a pre-registered expectation that the positive effect of VERTs is primarily driven by individuals with pre-existing favorable views of companies. These respondents are persuaded by the adoption of VERTs and show increased confidence in companies. Models in this table report coefficients that are significant and larger than in Table 2 for pro-firm individuals. Individuals with pre-existing anti-firm attitudes, instead, seem not to respond to firms' attempt to improve their reputation by adopting VERTs, consistently with pre-registered expectations.

Additional mechanism: Reputation among investors and ESG indexes

So far, we have considered the general public as a relevant corporate stakeholder. However, based on our theoretical framework we can expect corporate scandals will turn into a diffuse reputational damage also for another crucial corporate stakeholder: investors. Investors often use indexes as benchmark for investment choices (Cormier and Naqvi, 2023). Similarly to availability heuristics, indexes make up for investors' information scarcity by synthesizing vast amounts of complex information (Brooks, Cunha, and Mosley, 2015). In the CSR space, a multitude of private market analytics agencies (e.g., Refinitiv, RepRisk, Bloomberg, Moody's) supply investors with aggregate indexes measuring firms' environmental, social, and governance (ESG) performance. These indexes synthesize information pertaining to various CSR behaviors including negative incidents and positive measures. 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. Among investors, VERTs counterbalance the negative effect of scandals on ESG

performance indexes by restoring a positive, aggregate ESG score.[30]

In Appendix, we offer suggestive evidence that firms use VERTs to attempt at restoring a general positive image 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), for the sample of firms studied in our observational analysis. These indexes measure firms' social reputation and guide investors' choices, creating significant financial repercussions for firms (Choi, Ferri, and Macciocchi, 2023). When a firm is involved in a scandal, its ESG scores drop, leading to potential disinvestment and financial losses. 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

In this paper we have advanced and tested a theory of firms' reputation based on information spillover from corporate criminal scandals. Using this theory, we are able to offer a novel explanation for firms' decisions to self-regulate and pledge voluntary emission-reduction targets (VERTs). We contend that times of intense negative reputational pressure, like corporate criminal scandals (Culpepper, Jung, and Lee, 2022), damage firms' reputation much more broadly than previously acknowledged. Even if these scandals do not directly hinge on firms' environmental reputation, we claim they spill over onto such area of firms' public image. This is caused by the fact that stakeholders apply heuristics to formulate their reputation of firms. Such negative spillover effects cause a corporate criminal scandal to damage firms' environmental reputation, too, prompting stakeholders like the public to demand regulatory initiatives. Attempting to counter such backlash, firms embroiled in criminal scandals adopt VERTs. In particular,

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[30] 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 revert such damage.

they opt for reputationally risk-averse targets—i.e., more modest ones.

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% after the scandal. Such effect is concentrated among shallower targets which are easier to meet and allow leeway for readjustment in order to be met: intensity targets. Consistently, we find no increase in scope 3 emission coverage. Experimentally, we primed a sample of US-based respondents with information on a fictitious company, manipulating its involvement in a corruption scandal and VERT adoption. Individuals presented with corruption information (treated) have an environmental perception of the firm which is about 27% worse than that of the control group, supporting our availability heuristics logic. We also find that 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 initiatives (Culpepper, Jung, and Lee, 2022; Malhotra, Monin, and Tomz, 2019), investigating whether such diffuse damages to the reputation of a firm also impact political elites' view in favor of more stringent state regulation. Second, future studies could investigate whether our heuristics-based reputational theory also translates to other actors than firms, such as states, parties, and local authorities. Third, further research could expand on the initial evidence provided here in favor of our argument for financial investors and ESG index investing (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 should 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

A.1 Data access, availability, and reproducibility of the analysis

Our observational analysis relies on various data, many of which are proprietary. In particular, data from CDP, RepRisk, Refinitiv, and BoardEx are not publicly available. We accessed CDP and Refinitiv data through a subscription of the [ANONYMIZED FOR PEER REVIEW] . RepRisk and BoardEx data were downloaded via Wharton Research Data Services. Different institutions have different data subscriptions on this platform. Data from the Corporate Prosecution Registry (Garrett and Ashley, 2019) are, instead, publicly accessible.[1]

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

A.2 Descriptive statistics

We provide a basic description of our sample of publicly traded US-based firms from the CDP in Table A.1. Our dataset covers 771 firms, observed over the years 2010–2019. As the dataset is unbalanced, we have a total of 4,087 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 including scope 3 emissions. When firms do not report any VERT in their CDP submission, we code the variable as missing, which causes us to consider only 4,004 (out of 4,087) firm-year observations. For these observations, we code the number of VERT by type (absolute and intensity) and the number of VERTs covering scope 3. Confirming the growing importance of emission-reduction targets (Green, Hale, and Arceo, 2024), we find that VERTs are rather ubiquitous in climate policy: firms that adopt them have 1 of them active at any given year, on average.

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).

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

	N	Mean	SD	Min	P25	Median	P75	Max
All VERTs (count)	4004	1.279	1.256	0.000	1.000	1.000	2.000	13.000
Absolute VERTs (count)	4004	0.706	1.061	0.000	0.000	0.000	1.000	13.000
Intensity VERTs (count)	4004	0.573	0.802	0.000	0.000	0.000	1.000	8.000
VERTs that include scope 3	4002	0.239	0.673	0.000	0.000	0.000	0.000	8.000
Assets (billions of USD)	3155	77.463	251.932	0.090	7.198	18.018	44.551	2714.610
Employees (thousands)	3142	59.566	144.768	0.021	9.149	22.950	60.000	2300.000
UNGC environmental violation (binary)	4087	0.047	0.212	0.000	0.000	0.000	0.000	1.000
RepRisk RRI	3400	19.731	12.856	0.000	11.978	19.538	25.224	66.526
Refinitiv ESG score	2243	58.453	15.133	8.410	46.220	59.340	69.965	92.540
Refinitiv ESG Emissions score	2243	70.657	22.463	0.000	57.525	75.740	88.460	99.810
Proportion of female directors	3326	0.209	0.110	0.000	0.133	0.200	0.269	0.722
Average directors' age (year)	3326	72.789	3.929	53.714	70.163	72.936	75.459	86.458
Average directors' network size (count)	3326	2944.380	1377.739	168.071	1979.103	2692.991	3632.413	9955.480

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

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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 (for comparison, 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 drawn 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 in the board of directors. We have firms with *no* reported female board member or with up to 72% of female 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   Other 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) and Callaway and Sant'Anna (2021). We also estimate a traditional 2FE for comparison. Consistently with evidence presented in the main text, there is no strong sign of pre-treatment diverging trends in climate pledges. With the exception of 2FE 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 scope 3 VERTs—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 3) targets. To address this concern we first propose a potential, alternative way of studying the promotion of absolute over intensity targets and the adoption of less ambitious targets that do not cover scope 3. Instead of modelling the raw number of either type of pledge, we express the number of adopted intensity (or scope 3) pledges as a share of the total count. Second, we use binary variables instead of counts to give equal weight to the adoption of VERT types regardless of the number. We report our overall ATTs in Table B.1. Overall, we find similar, significant results as those presented in the main text. We note that using the binary variables for absolute/intensity targets makes the substitution logic between them more evident, with the former outcome variable exhibiting a stronger and significant negative effect. The adoption of VERTs that cover scope 3 still remains insignificant, no matter the operationalization.

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

	Share of intensity targets	Share of scope 3 targets	Intensity binary	Absolute binary	Scope 3 binary
	(1)	(2)	(3)	(4)	(5)
ATT	0.324*	-0.008	0.182*	-0.214*	0.093
	(0.050)	(0.031)	(0.075)	(0.097)	(0.048)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Baseline (average)	0.495	0.142	0.443	0.447	0.159
Num.Obs.	3024	3022	4003	4003	4001
R2	0.766	0.706	0.687	0.654	0.632
R2 Adj.	0.701	0.625	0.604	0.562	0.535

* $p < 0.05$. Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates are from Sun and Abraham (2021) estimator for staggered-treatment difference-in-differences. Dependent variables are share of intensity targets over total number of targets (model 1); share of targets covering scope 3 over total number of targets (model 2); binary variables indicate whether firms had intensity targets (model 3), absolute targets (model 4) or targets of scope 3 (model 5). Standard errors are clustered at the firm-level and reported in parentheses.
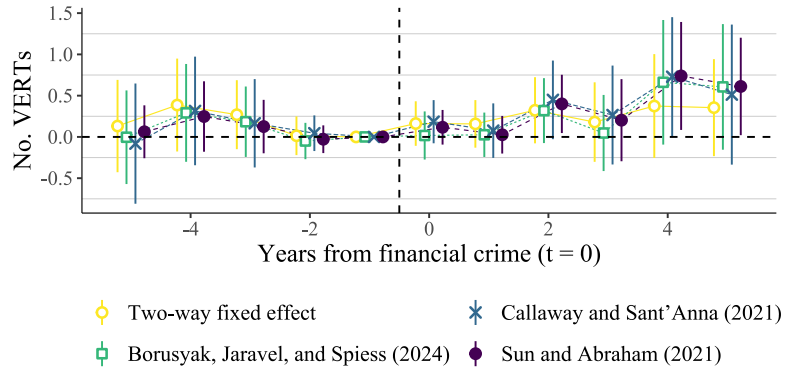
(a) Number of VERTs
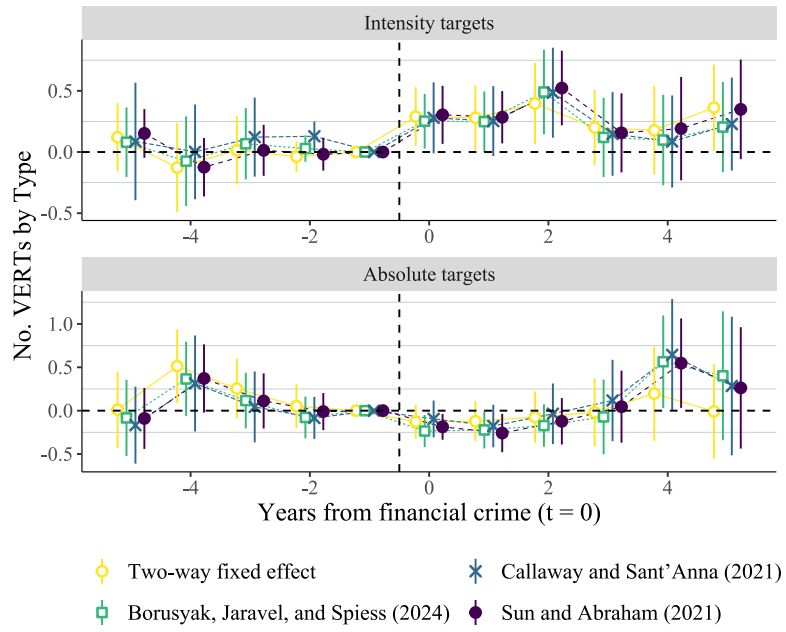
(b) Number of VERTs by type (absolute *vs* intensity)
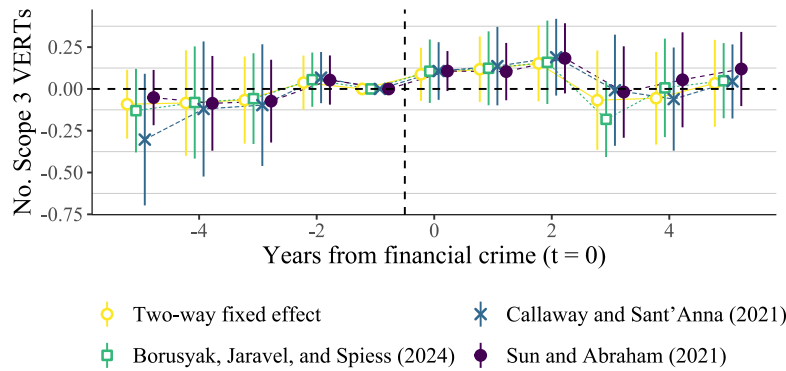
(c) Number of VERTs with scope 3 coverage

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

C Observational evidence: 2015 cut-off

Here, we address a potential concern of spuriousness of our results. Of the 75 firms treated in the US CDP sample, 21 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).

As a solution to the Paris threat to identification, we exclude firms that were treated after the year 2015. That is, we consider only treatment cohorts 2010, 2011, 2012, 2013, and 2014. This analysis 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.1). We detect a positive, sizeable, and statistically significant effect of financial criminal scandals on the total number of emission-reduction targets (+41% over the baseline), and in particular intensity ones (+63%).

Table C.1: The effect of a financial corporate criminal scandal on VERTs adopted by US-based firms participating to the CDP. Pre-2015 treated cohorts.

|	(1) All targets	(2) Absolute	(3) Intensity	(4) Scope 3
ATT	0.522*	0.152	0.370*	0.158
	(0.199)	(0.207)	(0.157)	(0.088)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Baseline (average)	1.268	0.685	0.583	0.229
Num.Obs.	3806	3806	3806	3804
R2	0.605	0.618	0.610	0.648
R2 Adj.	0.505	0.521	0.512	0.558

* $p < 0.05$. 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 making 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 VERTs adopted by US-based firms participating to the CDP. S&P 500 companies

	(1) All targets	(2) Absolute	(3) Intensity	(4) Scope 3
ATT	0.317*	0.054	0.263*	0.062
	(0.153)	(0.115)	(0.108)	(0.050)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Baseline (average)	1.321	0.734	0.587	0.167
Num.Obs.	3148	3148	3148	3148
R2	0.610	0.616	0.624	0.625
R2 Adj.	0.530	0.537	0.547	0.548

* $p < 0.05$. 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 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.2 are, again, comparable to the main ones.

Table D.2: The effect of a financial corporate criminal scandal on VERTs adopted by US-based firms participating to the CDP. Cases where firms were found guilty

	(1) All targets	(2) Absolute	(3) Intensity	(4) Scope 3
ATT	0.408*	0.076	0.332*	0.107
	(0.162)	(0.167)	(0.127)	(0.073)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Baseline (average)	1.270	0.691	0.579	0.233
Num.Obs.	3914	3914	3914	3912
R2	0.618	0.626	0.624	0.656
R2 Adj.	0.515	0.526	0.523	0.564

* $p < 0.05$. Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from Sun and Abraham (2021) estimator for staggered-treatment difference-in-differences. Dependent variables are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses. Data exclude firms involved in corporate criminal events that were acquitted, dismissed, or declined.

D.3 Exclude repeated offenders

Here, we exclude from the sample any repeated offender, meaning companies that were involved in corporate criminal scandals multiple times. The rationale for this test is to ensure that results are not driven by companies that suffer significantly higher reputational costs to mend (whose sustainability actions might thus be less credible), due to the repeated appearance under the spotlight of law enforcers and public opinion. Results, shown in Table D.3, are comparable to our main findings.

Table D.3: The effect of a financial corporate criminal scandal on VERTs adopted by US-based firms participating to the CDP. Non-repeated offenders

	(1) All targets	(2) Absolute	(3) Intensity	(4) Scope 3
ATT	0.362*	0.053	0.309*	0.105
	(0.124)	(0.127)	(0.118)	(0.064)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Baseline (average)	1.277	0.695	0.582	0.236
Num.Obs.	3933	3933	3933	3931
R2	0.621	0.627	0.629	0.663
R2 Adj.	0.520	0.528	0.530	0.573

* $p < 0.05$. Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from Sun and Abraham (2021) estimator for staggered-treatment difference-in-differences. Dependent variables are counts of targets. Standard errors are clustered at the firm-level and reported in parentheses. Data exclude firms involved in multiple corporate criminal events.

D.4 Exclude one treated firm at the time (jackknife test)

Finally, we ensure that results are not driven by any single outlier company. We re-estimate our Sun and Abraham (2021) ATT with a jackknife approach. That is, we drop one firm at the time from the analysis and re-estimate the ATT. Figure D.1 shows the results. It reports the effects from Table 1(in red) for comparison. It then reports all results from the jackknife approach (black) distinguishing between whether the excluded firm is a treated (hollow dot) or control unit (cross). The jackknife ATTs are consistent with those presented in the main text. The only exception is given by the exclusion of the Abbott Laboratories, involved in an FCPA case in 2012. When excluding this firm, the ATT just falls short of statistical significance, returning a p-value of 0.054. Given the proximity to the 0.05 alpha threshold, we consider the exclusion of this single firm not a particular threat to our substantive findings.

(a) Number of VERTs

(b) Number of VERTs with scope 3 coverage

(c) Number of VERTs by type (absolute *vs* intensity)
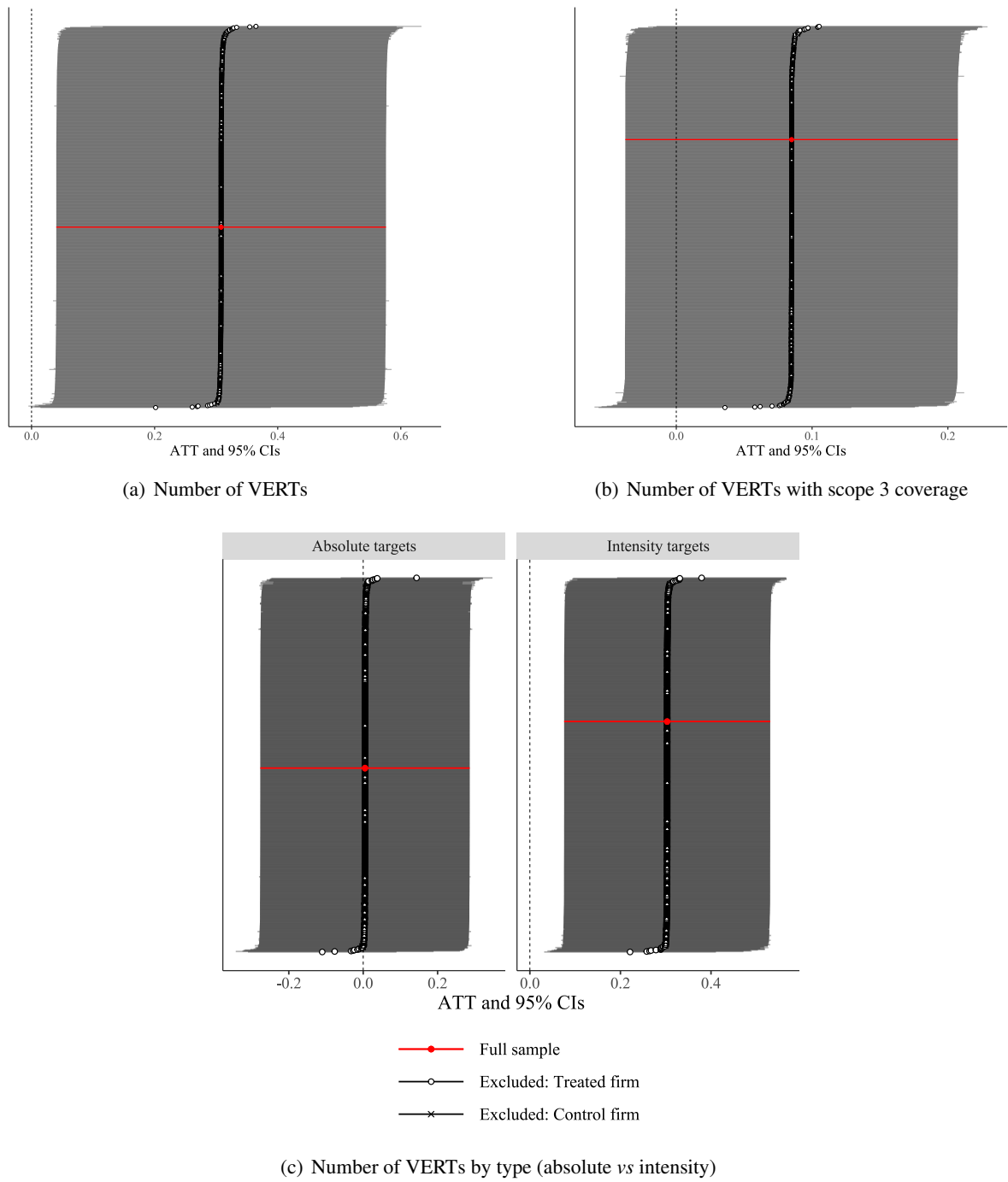
FIGURE D.1: Jackknife exclusion of treated companies from the Sun and Abraham (2021) difference-in-differences estimation

E   Observational evidence: Mechanisms

E.1   Non-reputational alternative mechanisms

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) Age	(4) Network
ATT	-0.037	0.009	-0.040	-160.163
	(0.036)	(0.010)	(0.394)	(89.130)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Baseline (average)	0.044	0.207	72.875	2855.205
Num.Obs.	4086	3326	3326	3326
R2	0.611	0.762	0.878	0.931
R2 Adj.	0.508	0.708	0.851	0.915

* $p < 0.05$. Firms' treatment is defined based on their involvement in a financial criminal event prosecuted under federal US corporate criminal laws. ATT estimates from Sun and Abraham (2021) estimator for staggered-treatment difference-in-differences. Dependent variables are: a binary for whether the firm has (potentially) violated UNGC environmental principles (model 1); the proportion of the board of directors that is made of female (model 2) or the average age of board of directors members (model 3); their average network size (model 4). Outcome data for model 1 come from RepRisk. Data for models 2 – 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 2–4, come from BoardEx. First, we consider the proportion of female members of the board and the average age of the board (models 2–3). We model these variables in our difference-in-differences design and do not observe any significant effect. Second, we study how the network size of board members evolves (model 4), 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*, albeit not significantly, likely as an effect of the attempt by firms to bring on board individuals with smaller personal networks following a financial scandal, to comply with anti-corruption, anti-fraud, and anti-collusion mandates.

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 attempt at improving 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 significant trends. The financial scandal leads to an immediate deterioration of involved firms' ESG rating, as implied by our argument—as shown by the significant and positive effect in the RRI for two years after the scandal, Figure 1(a). This diffuse reputational blow is then followed up by a progressive *improvement* of firms' ESG rating, indicated by estimates trending negative for the RRI and significantly positive for the Refinitiv's overall ESG score since year 3 post-treatment—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 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, driving the positive effect detected in the aggregate.

That is, the positive boost in ESG rating for firms involved in a financial scandal—a few years down the road of this negative event and its adverse reputational effect—is driven by emission-reduction efforts. Consistently with our theory, this is evidence that firms use VERTs to reverse the reputational loss generated by a scandal and that such attempt gets picked up by aggregate ESG indexes, potentially affecting investors' behavior.
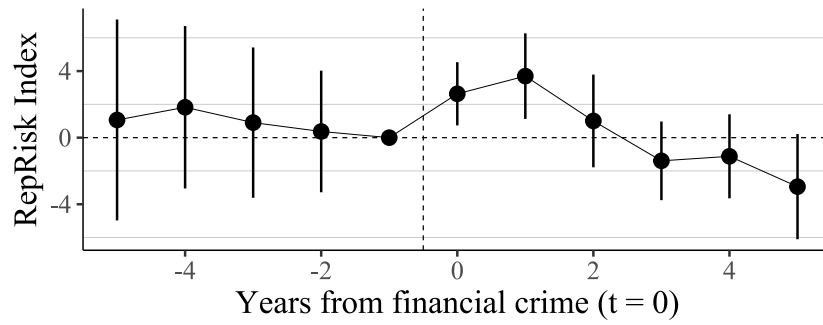
(a) RepRisk's RRI index
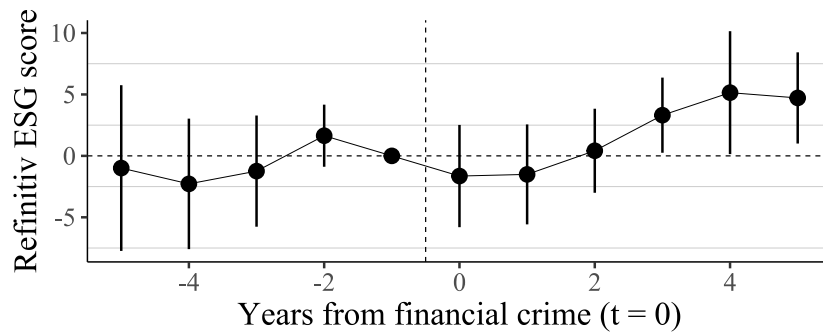
(b) Refinitiv ESG score
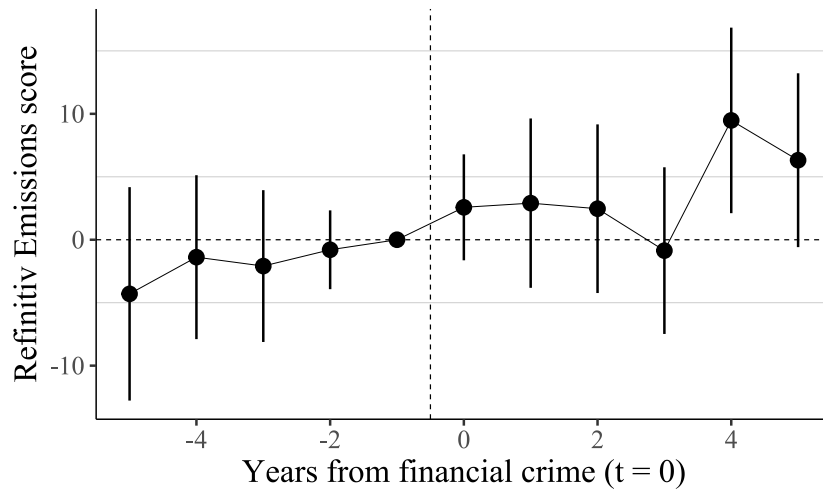
(c) Refinitiv ESG Emissions score

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).

F Experimental evidence: Design

F.1 Research ethics

This study was funded by the [ANONYMIZED FOR PEER REVIEW] and received approval from the [ANONYMIZED FOR PEER REVIEW] Research Ethics Team on 4 March 2024. The Ethics Team confirmed that the project posed no significant research ethics concerns. Detailed information about the survey can be found in our pre-registered report.[2]

The survey was conducted online and distributed to US-based individuals via Prolific.[3] Participants were compensated £1.05 for completing a 7-minute survey, equivalent to an hourly rate of £9.00, which Prolific deems a fair remuneration rate. The survey was fielded on 6 August 2024. Voluntary informed consent was obtained from all participants, who were free to decline participation. Before providing consent, participants were given comprehensive information about the study, including its purpose, the estimated time required to complete it, the fictitious nature of the companies discussed, privacy and data protection under UK GDPR regulation, the confidentiality of the survey, and its voluntary nature. Relevant contact information was also provided.

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

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

F.2 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

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

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[2] Anonymous pre-registration is accessible at: https://osf.io/xvrn3/?view_only=e79862fef77643278979887cae95b2b9.
[3] See: https://www.prolific.com/.

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

F.3 Sample description

Table F.2 presents the distribution of covariates of the individuals sampled in our survey experiment, representative of the US population by political affiliation, age, and gender.

TABLE F.2: 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%

In Table F.3, we test balance in covariates among individuals exposed to the corruption treatment vignette or not. We report difference in means for pre-treatment covariates and associated p-values. We consider individuals' age, gender, level of climate change concerns (on a 0-10 scale), attitudes towards

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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.4, we repeat the same procedure for the VERT vignette. In this case, too, individuals who were exposed to the VERT vignette are not statistically dissimilar from those that were not, before treatment.

Table F.3: 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.4: 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		

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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

* $p < 0.05$. Linear models of the general and environmental reputation of the fictitious firm presented in survey experimental vignettes. Reputation indicators are measured on a 0–10 scale with higher values indicating better reputation. Treatment variables are binary. Heteroskedasticity-robust standard errors in parentheses.

Table 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

* $p < 0.05$. 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.

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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

* $p < 0.05$. 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

* $p < 0.05$. 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

* $p < 0.05$. 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

* $p < 0.05$. 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

* $p < 0.05$. 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

* $p < 0.05$. 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

* $p < 0.05$. 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.

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