

The Externalities of Mandatory ESG Disclosure

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Abstract

We study the potential negative externalities of mandatory environmental, social, and governance (ESG) disclosure. Our analysis exploits a unique regulatory change in China that requires a subset of firms to report their contributions to poverty alleviation—on top of reporting general ESG issues—using a difference-in-differences design. We find that treated firms significantly increase their anti-poverty spending, but also increase their pollution, after the regulatory change came into force. The negative environmental externality is more concentrated in firms that are more financially constrained, as well as firms that are facing fiercer market competition. We further show that this effect is driven by a firm's incentive to strategically cater to politicians' agenda in order to obtain preferential treatment. These findings suggest that mandating ESG disclosure in selected areas may induce firms to trade off different ESG goals by prioritizing more conspicuous ESG issues at the cost of trivializing other, longer-term, issues.

Keywords: mandatory disclosure, ESG, poverty alleviation, pollution, political incentives-mandatory disclosure, ESG, poverty alleviation, pollution, political incentives

JEL Classifications: G18, G32, G38, K22, K32, M14, M41, M48

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Abstract

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The Externalities of Mandatory ESG Disclosure

1. Introduction

The disclosure and reporting of environmental, social, and corporate governance (ESG) information has become increasingly prevalent around the world. Today, almost all public companies regularly publish ESG reports alongside their standard financial reports or annual reports. Most of these disclosures are made on a voluntary basis, following frameworks developed by international organizations such as the Taskforce for Climate-Related Financial Disclosure (TCFD), the Global Reporting Initiative (GRI), the Sustainability Accounting Standards Board (SASB), and the newly created International Sustainability Standards Board (ISSB). This global movement is largely driven by institutional investors' demand for more reliable data on major ESG issues, to guide their investments (Ilhan et al. [2021]), and the desire by the broader society to more closely monitor corporate ESG actions.

To address such investor and societal demands, many jurisdictions have enacted ESG disclosure requirements. Several countries, such as the UK and Singapore, have prescribed ESG-related disclosures on specific topics, such as climate-related issues, for all public companies, while stock exchanges in other countries have also enacted non-prescriptive regulations that require some companies to issue ESG reports. More recently, the European Commission's proposal for a Corporate Sustainability Reporting Directive (CSRD), new legislation that aims to replace the Non-Financial Reporting Directive (NFRD), envisages the adoption of mandatory reporting of broad ESG issues across all large companies in the EU. The US Securities and Exchange Commission (SEC) is also contemplating mandating climate-related disclosures for all listed companies, which has triggered fierce debates.¹ Despite these regulatory actions, it remains unclear whether mandatory ESG disclosure will achieve the intended goal of promoting corporate engagement in specific ESG issues, and whether it will create

¹ For example, see: <https://www.sec.gov/comments/climate-disclosure/cll12.htm>, and: <https://corpgov.law.harvard.edu/wp-content/uploads/2022/06/Comment-Letter.docx.pdf>

unintended externalities, given the one-size-fits-all nature of most regulation in this area. These are the questions we aim to answer in this paper.

Motivating our empirical investigation is the notion that mandatory ESG disclosure, especially disclosure implemented in a staggered way to cover specific ESG issues or firms, incentivizes reporting firms to focus more on the mandated issues than on other ESG issues. Mandating disclosure on selected ESG issues (e.g., greenhouse gas (GHG) emissions) usually increases their saliency, putting them under the spotlight. However, this may distort firms' incentives, especially for those firms that face resource constraints, leading them to trade off more salient, shorter-term ESG goals with longer-term goals.

While studies on voluntary ESG disclosure are abundant, the extant literature on mandatory ESG disclosure is rather limited ([Christensen et al. \[2021\]](#)) and is predominantly focusing on the direct effect of climate-related disclosure (e.g., [Jouvenot and Krueger, \[2020\]](#); [Tomar \[2021\]](#); [Grewal \[2021\]](#); [Downar et al. \[2021\]](#)) or to a lesser extent workplace safety issues ([Christensen et al. \[2017\]](#)).² However, these studies remain silent on the potential externalities of mandatory ESG reporting. In addition, there lacks a setting in this literature investigating multiple specific ESG issues that apply to a subset of firms. Existing policy frameworks based on mandatory disclosure either focus on one single aspect of ESG for all firms (e.g., in the UK and Singapore) or do not differentiate between general ESG issues and specific ESG issues (e.g., the EU NFRD/CSRD). Thus, policy changes do not engender a meaningful control group, making it difficult to examine whether and how firms trade off different “E”, “S”, or “G” efforts.

We aim to address the gaps in the literature by exploiting the unique setting of China, where the regulator requires a set of listed firms to disclose their donations to poverty alleviation efforts, and by studying the real effect of this requirement on firms' behavior. In 2016, the China Securities and Regulatory Commission (CSRC) started mandating some listed firms to disclose—in their

² Regarding broader ESG issues, [Ioannou and Serafeim \[2019\]](#), [Krueger et al. \[2021\]](#) and [Fiechter et al. \[2022\]](#) provide international evidence that ESG-related mandatory disclosure requirements are associated with greater corporate social responsibility (CSR) engagement, greater firm value and other beneficial real outcomes.

ESG reports—detailed quantitative information on their pecuniary and non-pecuniary contributions to the “targeted poverty alleviation” (TPA) program, which is a political campaign launched by China’s paramount leader Xi Jinping. The mandate aims to encourage corporate engagement in poverty alleviation in rural areas in China.

In particular, we investigate whether and to what extent the TPA disclosure mandate generates negative environmental externalities. Our focus on environmental issues in the analysis is motivated by their long-term nature. Because of the long time span involved in the quest to solve environmental problems and the lack of verifiable immediate outcomes of such efforts, firms may be incentivized to deprioritize this issue and to direct resources to TPA, which is more salient and for which there is a clear timeline for achieving goals. Indeed, the literature documents that firms facing tradeoffs between short-term capital market goals and long-term sustainability tend to prioritize the former by sacrificing the latter (e.g., [Thomas et al. \[2022\]](#)).

The TPA disclosure mandate setting offers several advantages for this study. First, China’s regulatory regime with regard to ESG disclosure has evolved from covering all aspects of ESG (similar to EU NFRD), starting in 2008, to focusing on specific social dimensions (i.e., poverty alleviation). In particular, firms targeted by the 2016 TPA disclosure mandate were already subject to a regulation passed in 2008 by CSRC mandating them to disclose general ESG issues in CSR reports, but without detailed guidance being provided. Therefore, China’s ESG reporting regime is broader in scope as compared to the mandatory disclosure on firms’ carbon footprint that exists in other countries (e.g., in the UK and Singapore, and the recent US SEC proposal), and also provides variation in its emphasis on different “E” and “S” aspects over time. This allows us to study how firms trade off different ESG goals—a subject that is missing in the ESG literature. Second, as the list of firms that are subject to the TPA disclosure mandate was determined long before the enactment of the TPA disclosure mandate, i.e., as early as 2008, when China first mandated general ESG disclosure, there is less concern that these firms are “selected” into the treatment group due to unobservable characteristics. In addition, the co-existence of both mandatory disclosure and

voluntary disclosure requirements in China helps us to form a better-defined control group. Third, the TPA disclosure mandate was motivated by the Chinese top leader's political agenda, rather than by investors' concerns about corporate financial performance. This alleviates the endogeneity concerns that a firm's TPA contributions, and the disclosure of them, are driven by its financial performance, or vice versa. Instead, we are able to causally identify a firm's tradeoff between short- and long-term ESG goals as triggered by the TPA disclosure mandate.

Using a sample of firms listed on the Shanghai and Shenzhen stock exchanges over the period 2013–2019 and applying a difference-in-differences (DiD) methodology, we examine the effects of the TPA disclosure mandate on firms' donations to poverty alleviation, and on environmental externalities.

We first find that treated firms on average increased their TPA donations by 500,000 USD after the enactment of the 2016 reporting mandate, as compared with control firms not subject to the mandate. The direct effect is economically sizable and is likely an intended consequence from the regulator's perspective.

Next, we find that in response to the mandate, treated firms on average released 41.8% more pollutants than control firms, translating to 5,258 tons of toxic pollutants. These pollutants cover a wide range of toxicants, including sulfur dioxide and heavy metals, even tiny amounts of which are found to be hazardous to public health ([Currie and Schmieder, \[2009\]](#); [Currie et al. \[2014\]](#)). Alternative specifications show that the results are not driven by unobserved firm-specific factors or time-varying industry shocks, but more likely signify the negative externalities of the TPA disclosure mandate on the environment. This finding suggests a reallocation of corporate resources across different ESG categories, from the environmental ("E") dimension to the more regulatorily salient social ("S") dimension—thus reflecting a tradeoff of different ESG goals induced by the requirement of mandatory ESG disclosure. Interestingly, we do not find similar results for other ESG concerns, such as product quality and employee wellbeing, which are regular issues with immediate outcomes, and which do not have the long timespan that is involved in achieving environmental protection goals.

To substantiate the interpretation on externalities and to rule out alternative explanations, we perform several additional tests. First, we show that the findings are robust to alternative data on firm-level pollution. Second, we show that the increased level of pollution is not driven mechanically by the expansion of business operations. Third, we show that treated firms, while polluting more, are also more likely to receive regulatory penalties in the case of any severe environmental violation. This finding points to the real adverse consequence for firms of prioritizing TPA donations over environmental protection, and may explain why firms differ in the degree to which they commit to TPA donations. It also suggests that firms may derive other benefits from donating that outweigh the costs of underinvesting in pollution abatement. Fourth, we show that the TPA–environment tradeoff appears to take place within firms’ fixed budgets, as we do not find a significant difference in treated firms’ capital expenditure after the mandate, as compared to those of control firms. In other words, the TPA disclosure mandate has changed how the pie of ESG spending is split but not the total size of the pie. Finally, in a dynamic analysis testing the pre-trend, we do not find differences in donations and pollution levels between treated and control firms in absence of the mandate.

We then conduct several cross-sectional analyses to shed light on the potential mechanisms by which firms’ cost–benefit tradeoffs between TPA donations and pollution abatement efforts are altered. First, financially constrained firms are more prone to making such a tradeoff as they need to prioritize their spending on ESG activities that are under the spotlight, i.e., that are subject to mandatory disclosure, and thus cut spending on other less salient, but costly, ESG issues, such as pollution reduction. Consistent with this prediction, we find that negative environmental externalities are concentrated in firms that are more financially constrained. Second, firms facing fiercer market competition are more incentivized to prioritize regular spendings that do not cause significant cash flow fluctuations (commonly seen in expenditures on long-term environment abatement). They are also more likely to cater to the agenda of local politicians, who are similarly motivated to prioritize poverty alleviation to be aligned with Xi’s agenda, in exchange for receiving potential preferential treatment by politicians,

which help them survive and gain a competitive edge. Supporting these arguments, we find greater environmental externalities of the TPA disclosure mandate for firms with higher levels of product market competition, as measured by (the inverse of) their industry Herfindahl—Hirschman Index (HHI) and a regional product market development index.

We next investigate whether the tradeoff between different ESG goals, as triggered by mandatory disclosure, stems from a managerial agency problem or is a strategic choice by firms. On the one hand, managers may disproportionately divert corporate resources toward TPA to signal their commitment to local politicians and the public through their firms' ESG reports, thus boosting their personal reputation, but at the cost of sacrificing shareholders' values. On the other hand, such increased emphasis on TPA may represent a firm's strategic choice to cater to politicians' agenda in exchange for receiving economic favor, which could lead to greater profitability ([Bertrand et al. \[2020\]](#)). To disentangle these two mechanisms, we test whether treated firms gained more preferential treatment after the TPA disclosure mandate came into force. We show that treated firms that donated more—which presumably increased their pollutions more—obtained more loans from state-owned banks and received more government subsidies, and achieved greater operating performance and valuation. These results indicate that the tradeoff behavior is driven by firms' strategic choice to pursue economic favor, instead of reflecting a managerial agency problem. We further find that political leaders in cities with more TPA donations by listed firms are more likely to be promoted to higher positions, suggesting that TPA contributions are indeed associated with local politicians' agenda, and thus firms are incentivized to pander to this political priority. Collectively, these results suggest that mandatory ESG disclosure on selected ESG issues may create negative externalities in regard to other ESG commitments, through altering firms' priorities and inducing them to trade off different ESG goals within a fixed budget.

Our findings add to the extant literature in several ways. First, we contribute to the growing literature on the real effects of mandatory ESG disclosure ([Christensen et al. \[2021\]](#)). On the one hand, it is not clear whether such

reporting mandates have any real effect, as many have low standards and loose guidelines, and some firms may choose to comply only superficially with any disclosure requirements (Krueger et al. [2021]). On the other hand, extant studies on this topic do find evidence of the real impacts of reporting mandates, but this is usually limited to specific sectors or outcomes. For example, Christensen et al. [2017] study the real effects of mandatory CSR disclosure in the US mining sector, Jouvenot and Krueger [2020], Downar et al. [2021] and Tomar [2021] study disclosure on GHG emissions, and Rauter [2020] studies the effect of mandatory extraction payment disclosure on corporate payment and investment policies. Krueger et al. [2021] investigate mandatory disclosure on broader ESG issues, but mostly focus on the implications for shareholders (e.g., stock crash risks), instead of the real effects on firms and their stakeholders. Fiechter et al. [2022] also take a broad ESG perspective by exploring the EU’s NFRD (2014/95), but their setting does not allow for a well-defined control group as all firms within the EU are affected. Moreover, these studies do not study the potential negative externalities of mandatory ESG disclosure—a question that is of paramount importance as such reporting mandates usually have specific goals but largely ignore their unintended consequences. Perhaps the closest study to ours is Chen et al. [2018], which also exploits a Chinese setting but focuses on an earlier (2008) mandate which requires some companies to issue CSR reports. Chen et al. [2018] explore the *generic* ESG reporting mandate but their setting does not allow for studying the tradeoff among different ESG goals. Our study is a direct response to the call by Christensen et al. [2021] to investigate mandatory ESG reporting in specific areas by focusing on its perils.

Second, we add to the nascent but emerging literature on how firms trade off different ESG goals (see Bénabou and Tirole [2010], Freeman [2010], Kitzmueller and Shimshack [2012], and Bridoux and Stofberg [2015], for a review and detailed discussions). Existing studies mostly focus on how firms trade off shareholder interests and stakeholder welfare, especially when faced with budget constraints. For example, Xu and Kim [2022] document that US firms release more toxic pollutants when they are more financially constrained, to avoid potential legal liabilities. Thomas et al. [2022] find that firms pollute more to meet

earnings targets, signifying a negative externality of financial reporting incentives on the environment and society. [Liu et al. \[2021\]](#) and [Chen et al. \[2018\]](#) document a similar tradeoff between short-term capital market pressure and long-term environmental sustainability goals, using a sample of Chinese firms. Unlike these studies, our study provides evidence on the tradeoff across different non-shareholding stakeholders as triggered by an ESG reporting mandate. We show how such a tradeoff can be driven by firms' strategic choice to pander to politicians' agenda in exchange for favorable treatment when facing resource constraints, which might be the root of such externality. This finding also sheds light on the different implications of peripheral (or decoupled) and integrated ESG activities, with the former being peripheral to a firm's main business, whereas the later can be integral to its main revenues and costs ([Weaver et al. \[1999\]](#); [MacLean and Behnam, \[2010\]](#)). We show that when facing fierce business competition and pressure to survive, firms are incentivized to trade off different ESG activities without increasing their overall capital expenditures, probably because they realize that these activities (poverty alleviation and pollution reduction) are not an integral element of their success.

2. Institutional Background and Hypothesis Development

2.1 THE TPA DISCLOSURE MANDATE

Despite China's remarkable achievement in combating poverty, which has been recognized by the United Nations, a large part of the population had been in extreme poverty until recently. In 2013, one year after taking over the presidency, Xi Jinping put forward the concept of "targeted poverty alleviation" (TPA), which is central to China's anti-poverty strategy and to the centenary goal of the Communist Party of China (CPC) to build a "moderately prosperous society". The TPA aims to accurately identify impoverished areas and populations, and to allocate resources toward them ([Liu et al. \[2018\]](#)).³ In pursuit of the TPA agenda, a national poverty registration system has been established, leading groups on

³ For more detailed information, see the official document *Opinion on Promoting Poverty Alleviation through Innovative Mechanisms*, issued in 2013 by the State Council of China.

poverty alleviation have been set up at all administrative levels, clear guidelines have been developed, and target populations and timelines have been selected.⁴

Against the backdrop of this political campaign, mandatory disclosure of corporate contributions to poverty alleviation was introduced in 2016 by the two major stock exchanges in China, the Shanghai Stock Exchange (SSE) and the Shenzhen Stock Exchange (SZSE), at the instruction of CSRC. Both exchanges issued an announcement mandating detailed disclosure of corporate anti-poverty contributions and achievements in corporate ESG reports, for a subset of listed firms.⁵ These firms include those listed in the “Corporate Governance Sector” of SSE, financial firms, firms with overseas listings, and firms included in the “Shenzhen 100 Index”.⁶ Centrally administered state-owned enterprises (SOEs) are also included in the TPA disclosure mandate. In addition, SSE and SZSE issued a series of guidelines on the format of reporting, to ensure that the information provided by firms is not boilerplate language, which is a typical issue in ESG reporting mandates (Crilly et al. [2016]). As an illustrative example, we provide a sample of TPA donation disclosure in Appendix A. A firm that is subject to the TPA disclosure mandate needs to provide information on the monetary amount of pecuniary donations, the number of poor people being helped, and the number of jobs created for people living in rural areas, all of which is quantitative information and thus less subject to discretion.

There is a unique feature of the TPA disclosure mandate that makes it an ideal setting for studying the real effect of mandatory disclosure and its externalities. The specific reporting mandate in China examined in this study has

⁴ In 2021, Xi declared a complete victory in the campaign to eradicate extreme poverty, attributing this victory to the eight-year TPA campaign, which lifted nearly 100 million people out of poverty (<https://www.bbc.com/zhongwen/simp/chinese-news-56194835>).

⁵ Although both exchanges also require the disclosure of poverty alleviation engagement in the “Major Events” section of financial reports, such disclosure is not mandatory. On top of that, the most important source of ESG information is a firm’s standalone ESG report, which should provide detailed and comprehensive information on ESG activities.

⁶ For example, the Notice on Periodic Reports by Listed Companies issued by SSE states that “A subset of firms, including firms listed in its “Corporate Governance Sector”, financial firms as well as firms with overseas listed shares, are required to issue CSR reports when issuing annual report... Those who issue CSR reports should emphasize information on corporate engagement in social poverty alleviation and disclose it separately in their CSR reports... CSR reports should be approved by the board of directors... Other listed firms are encouraged to issue CSR reports.” SZSE simultaneously issued a similar note stating that “Firms included in “Shenzhen 100 Index” are required to issue CSR reports which should be approved by the board of directors... CSR reports should separately cover corporate involvement in social poverty alleviation...”.

evolved from a generic ESG disclosure regime. Figure 1 illustrates the evolution of mandatory ESG disclosure policies in China over time. In 2008, CSRC and the two stock exchanges first introduced the ESG disclosure mandate for the subset of firms listed on SZSE and SSE.⁷ In 2013, all centrally administered SOEs were further included in this mandatory reporting scheme. The 2008 mandate does not specify the topics and formats of ESG disclosure, and is instead a general framework requiring firms to provide disclosures on a broad range of issues. Starting from 2016, the same set of firms that are subject to the 2008 reporting mandate have additionally been required to disclose their TPA contributions in their ESG reports. The evolution from the 2008 mandate to the 2016 mandate, and the fact that the same firms and sectors continue to be affected, mean we have data on TPA donations both for firms that are subject to the mandate and for firms that voluntarily donated before and after 2016, albeit the information provided after 2016 is more detailed. This feature allows us to examine issues and draw causal inferences in a way that earlier studies (e.g., [Chen et al. \[2018\]](#)) have not been able to do.

2.2 HYPOTHESIS DEVELOPMENT

2.2.1 Direct Effect of Mandated TPA Disclosure

To shed light on potential externalities of the TPA disclosure mandate, we first examine whether it has a direct effect on corporate engagement in poverty alleviation. In general, a reporting mandate facilitates the disclosure and dissemination of information on a firm’s endeavors in a specific area, which reduces the cost for stakeholders of processing information and increases public awareness of the firm’s behavior ([Kanodia and Lee, \[1998\]](#); [Blankespoor et al. \[2020\]](#)). With regard to ESG disclosure, this process enables stakeholders to engage in more active monitoring and to put greater pressure on the reporting firm in their ESG practice ([Christensen et al. \[2021\]](#); [Houston and Shan, \[2022\]](#)). Greater engagement in ESG issues further helps the firm to receive increased stakeholder support ([Christensen et al. \[2017\]](#)). In our setting, such stakeholder

⁷ The enforcement of the mandated ESG disclosure is nontrivial in that firms that fail to provide a report are subject to delisting ([Chen et al., \[2018\]](#)).

monitoring and support are manifested by a reporting firm’s relationship with local politicians. Political attention and scrutiny by public authorities motivate firms to devote resources to things local politicians care about, so as to gain political influence and politicians’ favor. Such behavior is often dubbed strategic CSR ([Liston-Heyes and Ceton, \[2007\]](#); [Lin et al. \[2015\]](#)). That is, a firm may strategically respond to the TPA disclosure mandate by committing more resources toward poverty alleviation so as to cater to politicians’ interests, in return for favorable regulatory treatment.

Based on the analysis above, we first posit that firms that are subject to the 2016 TPA disclosure mandate increased their spending toward poverty alleviation more after the mandate came into force than did other firms.

2.2.2 Environmental Externalities of Mandated TPA Disclosure

We next conjecture that the TPA disclosure mandate may create negative externalities in relation to other less prioritized, longer-term, ESG dimensions. Specifically, we focus on environmental externalities, for at least three reasons. First, the environment is a well-defined public good but one that is lacking clearly identified private ownership, which leads to lower pollution abatement costs incurred at firm level than the marginal cost borne by the whole society ([Xu and Kim, \[2022\]](#)). Second, while being important, environmental issues are long-term in nature, with the time horizon well beyond the length of politicians’ careers, which makes them of less regulatory salience.⁸ Third, from a reporting perspective, while it is not difficult to mandate that firms disclose their pollution abatement investment, it is extremely challenging to link such disclosures to well-defined long-term targets, such as carbon neutrality at the societal level. As a result, firms may have incentives to cut spending on environmental issues when they need to disclose short-term-oriented and regulatorily salient TPA spending.

Such environmental externalities of the TPA disclosure mandate reflect the tradeoff between poverty alleviation and pollution abatement. For one thing, both

⁸ For example, during the most recent 20th CPC Congress, environmental issues were set aside amid other hotly discussed issues, such as targeted poverty alleviation, the “dynamic zero” strategy to combat Covid-19, anti-corruption, the Russia–Ukraine War, tensions with the West (especially the United States), and “Chinese-style modernization”. The timeline for achieving “carbon neutrality by 2060” is far beyond the political lives of Xi and most governmental officials.

donations and pollution abatement investments are costly, and the resource constraints that firms constantly face leads them to prioritize one over the other (Wang et al. [2016]). Dealing with toxic pollutants requires substantial inputs of materials, labor and financial resources. Absent any regulatory change, firms in equilibrium internalize the pollution abatement costs in the production process to achieve the long-term goal of environmental sustainability. The TPA disclosure mandate, by drawing public and regulators' attention to poverty alleviation, increases the benefit of engaging in poverty alleviation, which has traditionally been part of the government's responsibilities. On top of that, financial constraints increase the marginal cost of pollution abatement efforts (Xu and Kim, [2022]). As a result, marginal firms are likely to reallocate their resources toward TPA-related donations and away from investing in pollution abatement. For another, the 2016 reporting mandate, by facilitating greater transparency, enables managers to learn about their peers' performance through other companies' disclosures: the visibility of peer firms' disclosures facilitates benchmarking and incentivizes managers to adjust their own investment policies (Beatty et al. [2013]). Regarding mandatory ESG disclosure, investors and stakeholders can better distinguish genuine ESG behavior from greenwashing when information quality is higher. The disclosure of more unambiguous, quantitative information on corporate TPA donations allows for direct comparison among reporting firms and their product market competitors. The peer pressure incentivizes firms to prioritize regular spendings that do not cause significant cash flow fluctuations over longer-term spendings with greater uncertainties (such as environmental R&Ds). In addition, the pressure from peers is strengthened when the government is the stakeholder and recipient of the information. That is, product market peers have incentives to compete for preferential treatment by the government, as described in Flammer [2018], by catering to politicians' primary interests via prioritizing TPA donations over environmental protection.⁹

Based on the above arguments, we hypothesize that treated firms will spend more on their TPA contributions and will simultaneously cut back on

⁹ Flammer [2018] documents that US firms that invest more in ESG win more government procurement contracts relative to their competitors that invest less in ESG.

spending in other less salient, but capital-intensive, ESG projects—those related to pollution abatement and environmental innovation. As a result, they will produce more pollution, resulting in negative and unintended environmental externalities. Such negative externalities can be intensified when firms are more constrained in their resources and face intense competition, which creates stronger incentives for them to trade off different ESG goals.

Hypothesis: There is a negative environmental externality in regard to firm-level pollution after the introduction of the 2016 TPA reporting mandate.

It is worth pointing out that if the net benefit of TPA donations is large enough, or if firms are able to internalize any potential cost associated with it, we might not expect the reporting mandate to have any negative externalities in regard to firms' involvement in any other ESG dimension, including environmental protection. In the following sections, we formally test whether such negative externalities indeed exist.

3. Research Design

3.1 DATA AND SAMPLE SELECTION

Our study combines data from several sources:

(1) *Donations and TPA contributions data.* Data on a firm's total donations are obtained from the China Securities Market and Accounting Research (CSMAR) database. Since the enactment of the 2008 reporting mandate, firms that are subject to the mandate (treated firms) disclose the *total* amount of their donations in their mandated CSR reports, whereas firms that are not subject to the mandate (control firms) may choose to issue CSR reports on a voluntary basis, in which they disclose their social donations.¹⁰ The total donation amount does not differentiate between poverty alleviation-related donations and non-poverty alleviation-related ones. CSMAR records all donation data from all firms with donation information

¹⁰ Prior to the 2008 disclosure mandate, information on social donations was accessed from footnotes in financial reports.

from the year 2008 onwards. However, data on donations to poverty alleviation efforts are not readily available in the CSMAR database for the period before the enactment of the 2016 TPA disclosure mandate, which requires treated firms to report their TPA donations and other donations separately. CSMAR provides data on TPA donations from 2016 onward. For the purpose of our study, we supplement the data for the pre-2016 period by hand-collecting data on poverty-alleviation donations from CSR reports for all treated firms and control firms.

(2) *Environmental pollution data.* Data on firm-level environmental pollution come from multiple sources, including CSMAR, the Trucost Environmental Dataset from Standard and Poor's, and manual collection. CSMAR provides data on the quantity of a firm's major pollutant emissions, which are retrieved from the firm's standalone ESG reports and annual reports. We supplement the CSMAR pollution data with data from Trucost on various costs of air pollutants, GHG emissions, pollutants released on the land and in water and waste disposed.¹¹ We also hand-collect data on penalties for environmental violations from the websites of local environmental protection authorities.

(3) *Other data.* Data on other aspects of corporate ESG performance, including firms' involvement in regulatory actions or lawsuits related to products and services, employee health and safety-related issues, or corporate misconduct relating to corporate governance, are obtained from the Chinese Research Data Services Platform (CNRDS), which complements the CSMAR database and is increasingly used in China-related research (e.g., [Dong et al. \[2021\]](#)). Regional economic development data, such as city-level GDP and GDP per capita, are obtained from the National Bureau of Statistics of China. Historical information on the turnover and promotion of local politicians is hand-collected via internet searches. All corporate financial data are obtained from CSMAR.

¹¹ Another source of data on firm-level pollutants is the Environmental Survey and Reporting database of China (ESR), which has been used in prior literature (e.g., [He et al. \[2020\]](#); [Liu et al. \[2021\]](#)). However, the data are only available until 2014, and so are not applicable in our setting. While ESR covers pollutants including sulfur dioxide, the data used in our study have a wider coverage of pollutants, including both sulfur dioxide and heavy metals. Nevertheless, in untabulated analysis, we compare the data used in this study with ESR data for the two overlapping years 2013–2014 and find that the firm-level pollution measures in the two datasets are positively correlated.

Our original sample consists of all firms listed on the SSE and SZSE from 2013 to 2019. We then restrict our sample to non-financial firms and we exclude firm-years with missing financial information or negative book value of equity or total assets.¹² Applying these filters, our final sample consists of 14,143 firm-year observations, corresponding to 2,376 unique firms.

We obtained the list of treated firms by collecting four types of firms that are subject to the 2016 TPA disclosure mandate: SSE-listed firms in the “Corporate Governance Sector” or with overseas listings, SZSE-listed firms that are listed on the “Shenzhen 100 Index”, and all centrally administered SOEs listed on both stock exchanges.¹³ Note that locally administered SOEs are not subject to the disclosure mandate. In total, 432 unique firms are identified as treated firms. Control firms are those that are not subject to the mandate.

3.2 EMPIRICAL SPECIFICATION

We first estimate the following DiD model to investigate the direct effect of the 2016 TPA disclosure mandate on a firm’s own donations.

$$Donation_{i,t} = \beta_1 Treat_i \times Post_t + \theta Controls_{i,t-1} + d_i + l_t + \varepsilon_{i,t}, \quad (1)$$

in which i and t denote firm and year, respectively. The dependent variable, *Donation*, is measured in two ways. The first measure is a firm’s targeted poverty alleviation-related donations (*TPADonation*), defined as the logarithm of one plus charitable money in dollars donated toward poverty alleviation. The second measure is a firm’s total donations (*TotalDonation*), measured as the logarithm of one plus all charitable giving in a monetary unit. d_i and l_t indicate firm and year fixed effects. *Treat* is an indicator variable that equals one if a firm is subject to the TPA disclosure mandate, as discussed in Section 3.1, and zero otherwise. *Post* is an indicator variable equaling one for years after 2016 and zero otherwise.

¹² The mandate was announced officially on 30 December 2016. Since all listed firms in China have a fiscal year ended on 31 December, the passage of the mandate one day before the fiscal year end makes any effect from the year 2016 obscure. We thus exclude the data in 2016 from our sample of analysis. We find that our main finding holds if we keep the data and treat 2016 as the first year in the post-mandate period.

¹³ In China, there are two types of SOEs: centrally administered SOEs that are supervised and monitored by the central government, and locally administered SOEs that are subject to the supervision of local governments. There are around 100 centrally administered SOEs, comprising 10% of all SOEs.

Controls is a vector of control variables, following [Campbell \[2007\]](#) and [Lys et al. \[2015\]](#), including firm size (*LnAsset*), firm age (*FirmAge*), leverage (*Leverage*), profitability (*ROA*), cash flow from operations (*CFO*), assets turnover ratios (*ATO*), investment opportunity as measured by Tobin’s Q (*TobinQ*), the total ownership of a firm’s 10 largest shareholders (*Top10*), and whether the firm is a state-owned entity (*SOE*). Detailed variable definitions are presented in Appendix B. All continuous variables are winsorized at the top and bottom one percentile of their distributions.

To investigate the externalities of the TPA disclosure mandate, we next run a similar DiD model

$$Pollution_{i,t} = \beta_2 Treat_i \times Post_t + \theta Controls_{i,t-1} + d_i + l_t + \varepsilon_{i,t}, \quad (2)$$

in which all variables are defined similarly as in Equation (1) except that we replace the dependent variable with environmental pollution (*Pollution*), which is defined as the logarithm of one plus the total volume of a firm’s major pollutants released into the air, on the land and in water in a year.¹⁴ These pollutants include liquid waste material, industrial sewage/garbage, and poisonous heavy metals, such as arsenic (As), chromium (Cr), mercury (Hg), cadmium (Cd), thallium (Tl), and lead (Pb) etc. With their relatively high density and due to the fact that they are hazardous at very low concentrations, a small amount of metallic chemical elements can be detrimental to public health, causing, for instance, brain damage and mental retardation ([Currie and Schmieder, \[2009\]](#); [Currie et al. \[2014\]](#)). With this measure, we aim to capture the overall environmental externalities as a result of the enactment of the 2016 TPA disclosure mandate. We follow prior studies and use the level of a firm’s pollution as our dependent variable for our main analysis ([Akey and Appel, \[2021\]](#); [Xu and Kim, \[2022\]](#); [Thomas et al. \[2022\]](#)). To ensure the robustness of our results, we also scale the total emission volumes by the firm’s revenue (*PollutionRev*) or the cost of goods sold (*PollutionCogs*) to measure the firm-level pollution intensity and to take into account the size effect. Furthermore,

¹⁴ The distribution of the volume of pollutants is right-skewed ([Thomas et al. \[2022\]](#)). Thus, we take the logarithm of the release of pollutants.

we use alternative data sources for corporate environmental activities and performance, as discussed in Section 3.1.

The coefficients β_1 in Equation (1) and β_2 in Equation (2) capture the average change in corporate donations toward poverty alleviation and the average change in corporate engagement in environmental pollution abatement efforts after 2016 for treatment firms relative to control firms. Based on our hypothesis, a positive and significant β_1 suggests that the disclosure mandate has an intended real effect in regard to promoting a firm's contribution to poverty alleviation, and a positive (negative) and significant β_2 indicates a negative (positive) externality of the TPA disclosure mandate in relation to the environment.

3.3 DESCRIPTIVE STATISTICS

Panel A of Table 1 presents the summary statistics for the main variables for all firms. The average values of a firm's annual total donations (*TotalDonation*) and TPA contributions (*TPADonation*) are 2.65 million yuan and 1.34 million yuan, respectively. On average, a firm in the sample releases 12,579 tons of toxic pollutants (*Pollution*) into the environment per year. A firm has, on average, a logarithm of total assets of 22.23 (i.e., 14,449 million yuan), a logarithm of firm age of 2.84 (i.e., 19 years), book leverage (total liabilities to total assets) of 0.44, an ROA of 0.03, cash flow to total assets of 0.04, operating revenue to total assets of 0.61, a Tobin's Q of 2.29, and an ownership concentration of 57%. About 39% of the sample firms are SOEs. In Panel B of Table 1, we partition the sample into treated and control groups and report the descriptive statistics separately. We find that treated and control groups differ significantly in their donations and pollutant emissions. Treated firms, on average, donate more in terms of both total amount and TPA contributions. By contrast, treated firms and control firms are similar in regard to a variety of firm fundamentals, like firm age, size, leverage, profitability and ownership concentration. It is worth pointing out that the classification of the treatment and control groups does not perfectly overlap with that of SOEs vs. non-SOEs: SOEs account for 72% of treated firms and 32% of control firms.

4. Main Empirical Results

4.1 DIRECT EFFECT OF TPA DISCLOSURE MANDATE ON POVERTY ALLEVIATION DONATIONS

We first examine the direct effect of the TPA disclosure mandate on a firm's donations by estimating Equation (1). Table 2 reports the results using different specifications. In Columns (1) and (4), we include industry-by-year fixed effects to account for time-varying shocks within industries which may be correlated with corporate donations. In Columns (2) and (5), we include both firm and year fixed effects to control for unobserved firm-specific factors and time-varying macroeconomic factors. Columns (3) and (6) control for firm and industry-by-year fixed effects altogether.

In Columns (1)—(3), where the dependent variable is the total donation amount (*TotalDonation*), the coefficients on $Treat \times Post$ are positive and statistically significant across all specifications, indicating that firms increase their total spending on charitable giving in response to the 2016 TPA disclosure mandate. The point estimate in Column (2) suggests that the average treated firms increase their total donation by 79.5% ($=\exp(0.585)-1$), or about a 2.11 million RMB (approximately 293,000 USD) increase in the average amount of donations, relative to control firms that are not subject to the disclosure mandate. This is an economically significant increase given that the average annual profits for our sample firms in China in the pre-mandate period is about 230 million RMB (89 million USD). It is worth noting that this effect likely captures the *lower* bound of the mandate's real effect on a firm's total donations, as treated firms may cut corners on other social donations in order to allocate more funds to TPA contributions.¹⁵

In Columns (4)—(6), we replace the dependent variable with a firm's TPA-related donations (*TPADonation*) and re-estimate Equation (1). We again find positive and significant coefficients on $Treat \times Post$ across all specifications, corroborating the results in Columns (1)—(3). These results are also suggestive

¹⁵ We confirm this conjecture in the results shown in Section 4.5.

that the increase in treated firms’ total donations after treatment is likely to be driven by the increase in their TPA contributions. In terms of economic magnitude, Column (5) suggests that treated firms experience a 269% ($=\exp(1.306)-1$) increase in anti-poverty contributions—amounting to 3.60 million RMB (approximately 500,000 USD)—after the 2016 TPA disclosure mandate, relative to that of the control firms. This estimate is likely to be the *upper* bound of the real effect to the extent that hand-collected data on poverty alleviation donations prior to 2016 are understated. Though far less often observed in our sample, some firms provide qualitative or less detailed information in their CSR reports prior to 2016. Collectively, the results in Table 2 provide consistent evidence that mandating corporate TPA disclosure has a direct real effect on both total corporate donations and contributions to poverty alleviation, which are likely to be the intended consequences of the 2016 mandate.

4.2 ENVIRONMENTAL EXTERNALITIES OF TPA DISCLOSURE MANDATE

We next investigate the externalities of the TPA disclosure mandate by focusing on firms’ environmental performance to better understand how firms trade off different ESG goals. We argue that while firms spend more on anti-poverty contributions after the 2016 TPA disclosure mandate, they may spend less on environmental protection, by cutting back on pollution abatement costs, leading to more pollution.¹⁶ To this end, we examine whether and to what extent treated firms that are subject to the mandate release more poisonous pollutants into the environment compared with their counterparts.

Table 3 presents the results. As some industries are more polluting, we control for industry-by-year fixed effects at the minimum, or more preferably firm fixed effects. We report in Column (1) the within-industry analysis by including industry-year fixed effects, while leaving out any firm-level controls.¹⁷ The coefficient on $Treat \times Post$ is positive and significant, suggesting that after the introduction of the TPA disclosure mandate in 2016, treated firms release more

¹⁶ In Table C1 of Appendix C, we show that the change in TPA donations is positively correlated with the change in pollutant emissions.

¹⁷ We report the regression results without covariates to address the concern raised by [Gormley and Matsa \[2014\]](#) that covariates that are correlated with the treatment may prevent us from drawing reliable causal inferences.

hazardous pollutants into the environment compared with control firms in the same industry. Column (2) reports the results of estimating Equation (2), which includes both firm and year fixed effects, as well as firm-level controls, to account for any observed time-varying and unobserved time-invariant firm characteristics. The coefficient on $Treat \times Post$ remains positive and significant. The magnitude of effect is economically sizable as well: treated firms on average release 41.8% ($=\exp(0.349)-1$) more pollutants than control firms after the TPA disclosure mandate, translating to 5,258 ($= 12,579 \times 41.8\%$) tons of toxic pollutants. Considering that those toxic pollutants include heavy metals which are associated with the poisoning of human beings, even in very small amounts, this scale of pollution presents significant public health concerns. Column (3) repeats the analysis in Column (2) by also including industry-year fixed effects and shows consistent results.

4.3 ROBUSTNESS TESTS: ALTERNATIVE DATA AND MEASURES

To ensure our results indeed reflect the environmental externalities of the TPA disclosure mandate, we run several robustness tests. First, we use the Trucost Database as an alternative source of data on firm-level environmental pollution and show that our baseline findings are upheld. Using both public and private information, Trucost systematically estimates the costs associated with GHG emissions, air pollutants, land and water pollutants, as well as resource consumption, for firms around the world. The data are widely used in the finance and accounting literature (e.g., [Bolton and Kacperczyk \[2021, 2022\]](#); [Dai et al. \[2021\]](#)). Panel A of Table 4 reports the results by replacing the dependent variable in Table 3 with the costs associated with air pollutants (*AirPol*) in Column (1), greenhouse gas emissions (*GHG*) in Column (2), land and water pollutants (*LWP*) in Column (3), and waste production (*Waste*) in Column (4). Those cost measures are all log-transformed, thus the coefficients can be interpreted as percentages. The coefficients on $Treat \times Post$ remain positive and significant, providing additional support to our baseline finding in Table 3 that treated firms generate more negative environmental externalities after the mandate. In terms of economic magnitude, the environmental costs associated with the pollutants rise by 15%–40%.

In addition, to mitigate the concern that the increase in pollution by treated firms in Table 3 is driven by an expansion of business production, as opposed to the tradeoff of different ESG goals, we use a scaled measure of *Pollution* as the dependent variable. In Panel B of Table 4, we repeat the analysis in Column (2) of Table 3 by replacing the dependent variable with *PollutionRev* (*PollutionCogs*), which is defined as the total amount of pollutants scaled by total revenue (cost of goods sold). The positive coefficients on $Treat \times Post$ suggest that our main finding is not sensitive to business expansion.

Moreover, the within-ESG tradeoff argument underneath our main finding hinges on the assumption that any environmental violation is associated with significant costs. We therefore investigate whether treated firms that pollute more in response to the 2016 mandate are more likely to be penalized by the environmental protection authorities due to environmental violations. To this end, we construct an indicator variable, *EnvirPenalty*, that is equal to one if a given firm receives any administrative punishment for a violation of the environmental protection law in China.¹⁸ Panel C of Table 4 presents the results using a linear probability model. The point estimate in Column (1) suggests that treated firms are 1.7% more likely to be penalized for environmental violations in the post-2016 mandate period. The increase is economically sizable as the unconditional probability of receiving an environmental penalty is 3%. We further show that the result is robust when we replace the binary variable *EnvirPenalty* with the logarithm of one plus the number of penalties received by a firm in a year (*EnvirPenaltyNum*) in Column (2).

Lastly, to sharpen our identification strategy and address potential selection biases, we adopt a propensity score matching approach to match treated firms with control firms based on a few observable characteristics, as set out in Tables 2 and 3, to ensure that the two groups of firms are comparable along those dimensions. In particular, we match a treated firm to its three closest peer firms that have comparable firm characteristics.¹⁹ We then repeat our baseline analysis

¹⁸ The types of penalties imposed by the local environmental protection authorities include fines and temporarily suspending business operations etc.

¹⁹ The results are robust when we use alternative matches, such as 1:4 or 1:5 matches.

in Table 3. Table C2 of Appendix C presents the results estimated from the matched sample, which shows a qualitatively similar DiD estimator on pollution, further corroborating the finding in Table 3.

4.4 PARALLEL TRENDS ASSUMPTION

The DiD estimation in Table 3 relies on the parallel trend assumption that treated and control firms follow similar pre-trends before the 2016 TPA disclosure mandate. Therefore, we next test the pre-trend by examining the dynamic effects of the mandate by estimating the following model:

$$y_{i,t} = \sum \beta_{(\tau)} Treat_i \times Year_{(t-\tau)} + \theta Controls_{i,t-1} + d_i + l_t + \varepsilon_{i,t}, \quad (3)$$

in which τ denotes year τ relative to the year 2016. The dependent variable y is either the *Donation* measure as used in Equation (1) or the *Pollution* measure as used in Equation (2). Table 5 reports the dynamic effects estimated from Equation (3). The coefficients on $Treat \times Year_{(t-2)}$ and $Treat \times Year_{(t-1)}$ in all columns are statistically indistinguishable from zero and are economically trivial. This suggests that the differences in donations and pollution levels between treated firms and control firms are unlikely to be driven by any time trend. In contrast, coefficients on $Treat \times Year_{(t+1)}$, $Treat \times Year_{(t+2)}$, and $Treat \times Year_{(t+3)}$ in the post-mandate period are all significantly positive and sizable in terms of economic magnitude. For instance, these coefficients range from 1.1 to 1.5 in Column (2) (from 0.3 to 0.6 in Column (3)), which is quantitatively similar to those in the baseline models in Table 2 and Table 3. The sizable point estimates on these terms suggest that following the introduction the 2016 TPA disclosure mandate, treated firms contribute more to poverty alleviation while generating more pollution, confirming the negative externalities of the mandate.

A potential concern is that the selection of firms into the treated group, albeit being predetermined eight years prior to the 2016 mandate, may be correlated with unobserved factors that have long-lasting effects. While we acknowledge that the assignment of treated firms may not be entirely random, we address this concern by conducting a falsification test. In particular, we randomly assign a firm to the treatment or control group and repeat the baseline analysis in

Table 3 by replacing the original sample with this pseudo-matched sample. We repeat this process 1,000 times, which provides us with pseudo 1,000 DiD estimators (coefficients on $Treat \times Post$). We then plot the distribution of the pseudo DiD estimators and present these in Figure 2. If any other unobserved random factor were to drive our finding, we would expect to see the distribution centered at the actual coefficient, represented by the line. However, the distribution of the pseudo coefficients is centered at zero, which significantly deviates from the actual effect.

4.5 EFFECTS ON OTHER DIMENSIONS OF CORPORATE ESG AND NON-ESG ACTIVITIES

In this section, we examine whether the 2016 TPA disclosure mandate has any spillover effect on other aspects of corporate ESG besides environmental engagement, such as non-TPA donations, product or service quality, employee wellbeing and safety, and corporate misconduct, as well as non-ESG activities. We have shown in Section 4.1 that there is an increase in total social donations among treated firms after the introduction of the mandate, and that the increase is largely driven by the increase in TPA contributions. It is interesting to investigate whether treated firms substitute other donations with TPA contributions. Column (1) of Panel A, Table 6 shows the result of replacing the dependent variable in Table 2 with *OtherDonation*, measured by the logarithm of one plus non-TPA donations. The coefficient on $Treat \times Post$ is significantly negative, and the point estimate indicates that treated firms cut back their spending on non-TPA donations by 32.3% ($=\exp(0.28)-1$).

We then test the effect on other non-environmental dimensions of ESG engagement by exploring whether a firm has experienced regulatory actions or lawsuits. The last three columns of Panel A in Table 6 report the results from estimating a linear probability model, with the dependent variable being a binary indicator for whether a firm has been subject to any regulatory actions or lawsuits related to (a) products or services (Column (2)), (b) employee health and safety (Column (3)), and (c) corporate misconduct (Column (4)). The coefficients on $Treat \times Post$ in all three columns are statistically indistinguishable from zero, suggesting

that the negative externalities are mostly manifested in environmental issues, and not other ESG issues.²⁰

As a final test, we examine whether the disclosure mandate triggers externalities in relation to other non-ESG activities that are related to core corporate business operations. Panel B of Table 6 reports the results of repeating the baseline analysis in Tables 2 and 3 by replacing the dependent variables with operating expenses scaled by lagged total assets (*OperExp*) and capital expenditure scaled by lagged total assets (*Capex*). The empirical results suggest that spending on non-ESG activities is not affected by the reporting mandate.

It is imperative to discuss why one observes negative externalities in relation to environmental performance, but not other ESG dimensions. Unlike other ESG issues, environmental issues are long-term in nature, and are difficult to measure and regulate at the individual firm level (such as Scope 3 emissions). From politicians' perspective, it is also challenging to evaluate whether environmental performance meets specific targets in the long run. For example, one can only gauge whether environmental improvement is in line with China's pledge in regard to the Paris Agreement that it will achieve carbon neutrality by 2060, which is far beyond the length of Xi's political career. In contrast, in 2021 Xi declared a complete victory in the fight to end extreme poverty in China, which he called a miracle and said would "go down in history," after an eight-year campaign, right before his re-election for an unprecedented third term. Indeed, Xi also emphasized other aspects of ESG, such as environmental protection, for which CSRC also requires mandatory reporting but which have not been elevated to the level of a national strategy and made a top political priority.

²⁰ This inference holds to the extent that there is no differential change in regulatory enforcement related to products/services, employee wellbeing or corporate governance for treated firms vs. control firms in the post-2016 period. To the best of our knowledge, we do not observe such changes in our sample period.

5. The Channels

5.1 THE ROLE OF FINANCIAL CONSTRAINTS

The environmental externalities of the mandate reflect a tradeoff between short-term social donations and long-term environmental investments, which is based on the assumption that firms have limited resources and they allocate their resources toward the aspects of ESG that are most salient for their important stakeholders (Wang et al. [2016]). Such a tradeoff should be particularly prominent when firms are financially constrained and thus have limited resources. To test this channel, we exploit the within-treated variation in financial constraints by partitioning our sample by the widely used SA index for firm-level financial constraints (Hadlock and Pierce, [2010]). Using within-treated estimations also helps rule out any confounding effect that is specific to treated firms (Boissel and Matray, [2022]).

Table 7 reports the results. We sort the sample of treated firms into terciles based on their average SA index values three years prior to the mandate, and classify those in the bottom tercile as less constrained firms. *LowFinCons* is an indicator variable which is equal to one for less financially constrained firms and zero otherwise. In Column (1) (Column (2)), we retain (drop) firms in the middle tercile. The variable of interest in Table 7 is the interaction $Treat \times Post \times LowFinCons$, which captures the marginal difference in pollution between financially less constrained treated firms relative to more constrained treated firms. The significant negative coefficients on this triple interaction term suggest that the negative environmental externalities are attenuated by the relaxation of financial constraint. This finding points to the role of financial constraints in driving a corporate tradeoff among different ESG goals, and echoes the findings of some recent studies (e.g., Hong et al. [2012]; Xu and Kim, [2022]; Bartram et al. [2022]).

5.2 THE ROLE OF PRODUCT MARKET COMPETITION

Another potential channel through which mandatory ESG disclosure generates externalities is competition among peer firms (Cao et al. [2019]; Christensen et al. [2021]). Firms facing fierce competition have greater incentives

to cater to stakeholders' interest, and the disclosure of information makes it easier for peers to infer corporate performance, which further intensifies competition. As a result, one would expect the negative environmental externalities to be stronger for firms facing greater competition. We use two proxies to capture the intensity of product market competition and interact the measures with $Treat \times Post$ to test this channel. The first one is the Herfindahl-Hirschman Index (HHI) for a firm's industry, which is calculated using the pre-mandate period average HHI value for each industry and sorting industries into terciles based on the average HHI. As a higher HHI value indicates greater concentration, we consider a firm to be facing low industry competition (*LowIndCom*), if the HHI of its industry falls into the top tercile. Columns (1) and (2) of Table 8 present the results without and with industry-year fixed effects, respectively. The regression estimate suggests that the triple term is statistically negative at the 1% significance level, showing that the negative environmental externalities become weaker when firms are in less competitive product markets relative to firms in more competitive markets.

The second proxy for product market competition is the regional market development index developed by [Fan et al. \[2017\]](#). This regional index explores the geographic variation at the provincial level in product market development in China and has been widely used in prior studies (e.g., [Li et al. \[2011\]](#); [Berkowitz et al. \[2015\]](#)). We compute the average rank of the product market index in the three years prior to 2016. We define a province-level indicator variable, *HighMktIndex*, which is equal to one if the average rank of market index of a province is in the top tercile in China and zero otherwise. We present the cross-sectional results in Column (3) of Table 8. The significant coefficient on $Treat \times Post \times HighMktIndex$ suggests that the negative environmental externalities are mostly driven by treated firms located in more competitive local markets, corroborating the finding in the first two columns. The result is robust to the inclusion of province-year fixed effects in Column (4), indicating that the finding is not driven by unobserved time-varying factors pertaining to firm location.

6. Uncovering the Root of Mandate Externalities: Agency Problem vs. Strategic Behavior

A remaining question is whether the negative externalities of mandatory ESG disclosure reflect an agency problem of managers misusing corporate funds for private benefits, or a strategic choice by firms to pander to politicians' agenda. Our results thus far do not clearly distinguish between these two explanations. On the one hand, both financial constraints and the misallocation of resources may simply reflect poor governance within a firm, which usually leads to value destruction for the firm (Masulis and Reza, [2015]). On the other hand, firms facing financial constraints and fierce competition may reallocate their limited resources to seek preferential treatment from politicians in order to survive. The latter may result in greater shareholder value through preferential treatment, at the cost of other stakeholders. In this section we try to disentangle the two motivations of this TPA–pollution abatement tradeoff triggered by the disclosure mandate by directly testing its value implications and whether it is related to receiving government support.

6.1 FINANCIAL PERFORMANCE AND FIRM VALUE

We start the analysis by studying the effect of the disclosure-induced TPA–pollution tradeoff on a firm's financial performance and valuation. Table 9 presents results on financial performance (Panel A) and firm value (Panel B). In Panel A, we measure financial performance using return on assets (*ROA*) in Column (1), return on equity (*ROE*) in Column (2), operating margin (*OperMargin*) in Column (3) and net profit margin (*NetProfitMargin*) in Column (4). To link the differences in financial performance to the tradeoff, we create an additional variable capturing the within-treatment variations. Specifically, *HighTPA* is a binary indicator for treated firms with a higher increase in TPA donations after the mandate. To construct this variable, we first calculate the change in a firm's TPA contributions after the regulation and then sort treated firms into terciles based on the magnitude of their TPA contribution changes. Firms falling into the top tercile are classified as *HighTPA* firms. The variable of interest $Treat \times Post \times HighTPA$ in Panel A of Table 9 captures the difference in financial performance

between treated firms with larger TPA donation increases—which presumably pollute more—and treated firms with smaller TPA donation changes. Both the coefficients on $Treat \times Post \times HighTPA$ and on $Treat \times Post$ in all four columns are significantly positive (with the magnitudes of the latter being smaller). These results provide robust evidence that a larger improvement in short-term financial performance is mostly concentrated among treated firms that donate more and likely pollute more.

In order to understand whether the effects on short-term financial performance translate into long-term value, we further examine the change in a firm’s Tobin’s Q, measured as the market-to-book ratio of total assets, following the mandate. Using *TobinQ* as the proxy for firm value, we find in Panel B that the Tobin’s Q of treated firms that donate more increases more significantly relative to control firms after 2016, as evidenced by the positive coefficient on the triple term $Treat \times Post \times HighTPA$. The positive valuation effect of the TPA disclosure mandate does not support the explanation based on managerial agency problem but is consistent with the notion that firms strategically prioritize TPA to maximize profits by catering to local politicians’ interests, in exchange for receiving preferential treatment from them. In Table C3 of Appendix C, we find no significant difference in environmental externalities between treated firms with a higher level of institutional ownership, a proxy for the monitoring intensity in relation to a given firm. This result further rules out the agency-based explanation. We further test the “strategic CSR” motive in the following sections.

It is important to reconcile our findings on financial performance and firm value with those of prior studies which show that mandated ESG disclosure generally has an adverse effect on firm value (e.g., [Manchiraju and Rajgopal, \[2017\]](#); [Chen et al. \[2018\]](#); [Grewa et al. \[2019\]](#)). Extant studies mostly focus on how mandatory ESG disclosure induces a firm’s tradeoff between ESG and non-ESG activities, or between its shareholders and stakeholders ([Christensen et al. \[2021\]](#)). In contrast, the tradeoff in our setting is between different ESG goals, and a firm is incentivized to strategically reallocate resources within ESG activities to cater to one important stakeholder (the government), at the cost of other stakeholders.

6.2 ACCESS TO GOVERNMENT FINANCING

We next examine whether treated firms that donate more have better access to government financing after the mandate, as an ex ante political incentive. We replace the dependent variable in Equation (1) with a variable of firm-level government financing. Specifically, we use two different measures to capture and triangulate the government financing received by a firm, including the amount of loans received from state-owned banks (*SOEBankLoan*) and government subsidies (*GovSubs*). Table 10 presents the results. *SOEBankLoan* in Column (1) is defined as the logarithm of one plus the total loan amount obtained from state-owned banks. *GovSubs* in Column (2) is calculated as the logarithm of one plus the subsidies received from the government. The coefficients on the triple interaction term $Treat \times Post \times HighTPA$ are significantly positive in Column (1) and (2), indicating that treated firms that make more TPA donations indeed receive a higher amount of loans from state-owned banks and larger government subsidies in the aftermath of the 2016 mandate. The economic magnitudes are also nontrivial. To put these numbers into perspective, an average treated firm that donates more in TPA receives 126.4% ($=\exp(0.817)-1$) more state-bank loans, as well as 181.5% ($=\exp(1.035)-1$) more government subsidies, compared with the sample mean. Collectively, these results are consistent with the notion that the 2016 TPA disclosure mandate motivates firms to strategically cater to local politicians' top priorities in terms of their endeavors to alleviate poverty, in exchange for better access to government financing. These findings resonate with the growing literature on the economic consequences of governments being stakeholders in influencing corporate outcomes ([Liston-Heyes and Ceton, \[2007\]](#); [Marquis and Qian, \[2014\]](#); [Bertrand et al. \[2020\]](#)).

6.3 ALIGNMENT WITH POLITICIANS' AGENDA

Finally, to confirm that the value-enhancing effects are indeed due to firms' catering to politicians in exchange for government support, we test whether TPA contributions are associated with the advancement of local officers' political careers. We posit that under the pressure of Xi's TPA campaign since 2013, local politicians are more likely to be promoted to higher-ranked positions in the

political hierarchy if they make efforts in regard to alleviating poverty.²¹ Consequently, they place more pressure on treated firms to increase, and to report, their TPA contributions.

To shed light on this political incentive that may lie underneath our results, we examine the extent to which local government leaders are likely to be promoted when treated firms in their jurisdiction increase TPA donations after 2016. We focus on a city’s CPC Secretary, who is usually the paramount leader of the city. Table 11 present the results, in which we aggregate firm-level data to the city level. To perform the city-level analysis, we first identify all cities in which our sample firms are headquartered. For each city, we hand-collected information on its CPC Secretary over the period 2013–2019. On average, a CPC Secretary stayed in her position for two to three years before experiencing political turnover. We then manually checked whether a given political turnover indicates a promotion or not. We define the dependent variable in both Panel A and Panel B of Table 11 as an indicator variable that is equal to one if a city’s CPC Secretary is promoted to a higher position in the political hierarchy, and zero otherwise. In Panel A, we construct a binary variable, *Treat_City*, to indicate cities in which at least one treated firm is located. We end up with 126 unique treated cities and 184 control cities. One of the most important factors that determines political promotion is one’s political tenure. Thus, we control for *Tenure*, measured by the number of years a city CPC Secretary has been in place, in all specifications. To further rule out city-specific characteristics that are associated with the likelihood of being promoted, we control for city fixed effects in Columns (3) and (4). We also add year fixed effects in Columns (2), (3) and (4) to account for macroeconomic confounding factors. The significantly positive coefficients on $Treat_City \times Post$ in Panel A of Table 11 indicate that cities’ CPC Secretaries are more likely to be promoted when there is at least one treated firm that is subject to the disclosure mandate. The point estimates suggest a sizable (approximately 10%) higher chance of promotion. It is worth pointing out that the inference still holds with the inclusion of the time-

²¹ We acknowledge that local politicians’ incentives may change over time. For instance, numerous studies have documented how the tournament incentives of prioritizing local GDP growth across different regions can explain politicians’ promotion to higher political positions (Li and Zhou, [2005]; Chen and Kung, [2016]).

varying city GDP per capita (*GDP_percap*). The coefficient on this variable is indistinguishable from zero, suggesting economic growth may not be the top priority of city CPC Secretaries during the period of Xi’s political campaign, as compared to TPA contributions.

As a further robustness check, we replace the binary variable *Treat_City* in Panel A with a continuous variable *Treat_City_Amt*, which is the aggregated TPA donation amount of all treated firms in a city in a year. We report the results in Panel B of Table 11. We find consistent and robust results, evidenced by significant and positive coefficients on *Treated_City_Amt* $\times$ *Post*.

7. Conclusions

This paper provides robust evidence on the negative externalities of mandatory ESG disclosure using a unique setting in China, in which a subset of firms that were initially subject to mandatory CSR reporting are now further required to disclose their contributions to poverty alleviation efforts. This setting embodies what a mandatory ESG disclosure typically entails, namely the political agenda of regulators, which is an important feature that is often overlooked in the literature. In China, TPA is widely viewed as a political campaign that signifies the top priority of—and achievement of—the country’s paramount leader. In this regard, our findings demonstrate how the Chinese setting—including the unique regulatory framework and politician–firm dynamics—can potentially help researchers answer important questions in the accounting and ESG literature (Cheng et al. [2022]; Lennox and Wu, [2022]).

Using a DiD approach, we find that firms that are subject to mandatory TPA disclosure significantly increased their social donations towards anti-poverty purposes after the passage of the TPA disclosure mandate. Notably, these firms also increased their emissions of major pollutants. This finding of negative environmental externality is robust to alternative measures of firm-level pollution. The cross-sectional analyses suggest that the negative environmental externalities are concentrated in firms that are more financially constrained, as well as firms that are facing fiercer market competition.

We also find that treated firms with more TPA contribution receive more government subsidies and loans from state-owned banks, and achieve greater operating performance and valuation. These results suggest that the above environmental externality effect is driven by a firm’s strategic behavior in regard to catering to politicians’ preferences, in exchange for favorable treatment. Further corroborating this conjecture is evidence that political leaders in cities with more TPA donations by listed firms are more likely to be promoted to higher positions. Overall, our findings suggest a tradeoff between different ESG goals, induced by mandatory ESG disclosure.

Perhaps the most significant implication of our findings is that mandatory ESG disclosure, especially that focusing on specific issues or the welfare of *some* stakeholders, can have significant negative externalities in regard to other stakeholders and the society at large. In this regard, we join the recent debates on the real effects of ESG reporting mandates ([Krueger et al. \[2021\]](#); [Fiechter et al. \[2022\]](#)), and, more specifically, how such mandates can alter corporate behavior and induce a tradeoff between shareholders and stakeholders (e.g., [Chen et al. \[2018\]](#)). We not only document such a tradeoff, but also unpack firms’ motivations from the perspective of political incentives—a phenomenon that is prevalent around the world. Our study is therefore generalizable to other settings and can inform regulators who are contemplating introducing a CSR/ESG reporting mandate about potential negative effects on stakeholders, which are often overlooked in current policy debates.

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Figure 1. The Evolution of ESG Disclosures in China over Time

The figure above illustrates the timeline, major milestones, and affected firms of the mandatory ESG disclosure landscape in China, and how our sample coverage differs from that in Chen, Hung, Wang (2018).

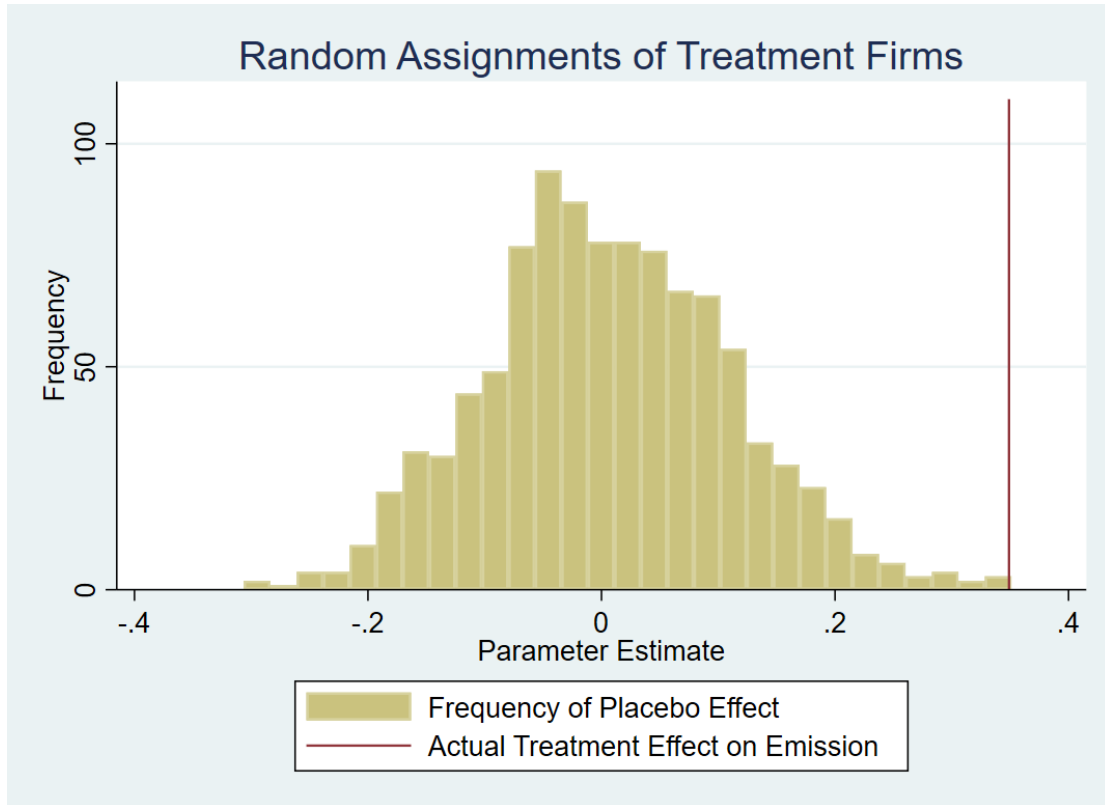


Figure 2. Distribution of the Results Estimated from Placebo Tests

The figure above compares the actual treatment effect with placebo effects. We keep the treatment year unchanged and randomly assign “placebo treatments” to our sample firms. Based on this pseudo treatment-control sample, we estimate the coefficient on $Treat \times Post$. We repeat this practice 1000 times and plot the distribution of these coefficients. The red line represents the actual coefficient on $Treatment \times Post$ estimated from Equation (2).

TABLE 1

Summary Statistics

This table shows the descriptive statistics for the main variables. Panel A reports the statistics for all observations in our sample. Panel B presents the statistics for treated firms and control firms separately. Appendix B presents a detailed description of variable construction.

<i>Panel A: Summary Statistics for All Firms</i>								
Variables	Obs	Min	P25	Median	Mean	P75	Max	SD
<i>TotalDonation</i>	14,143	0.00	0.00	0.00	1.27	1.90	10.42	2.33
<i>TPADonation</i>	14,143	0.00	0.00	0.00	0.79	0.00	9.57	1.90
<i>OtherDonation</i>	14,143	0.00	0.00	0.00	0.68	0.00	8.52	1.76
<i>Pollution</i>	14,143	0.00	0.00	0.00	0.51	0.00	14.56	2.13
<i>PollutionRev</i>	14,143	0.00	0.00	0.00	1.05	0.00	161.29	10.29
<i>PollutionCogs</i>	14,143	0.00	0.00	0.00	1.44	0.00	202.37	14.05
<i>EnvirPenalty</i>	14,143	0.00	0.00	0.00	0.03	0.00	1.00	0.18
<i>EnvirPenaltyNum</i>	14,143	0.00	0.00	0.00	0.03	0.00	1.39	0.16
<i>AirPol</i>	14,143	0.00	0.00	0.00	0.32	0.01	9.28	1.00
<i>GHG</i>	14,143	0.00	0.00	0.00	0.45	0.07	10.05	1.27
<i>LWP</i>	14,143	0.00	0.00	0.00	0.14	0.00	6.82	0.55
<i>Waste</i>	14,143	0.00	0.00	0.00	0.14	0.02	7.87	0.51
<i>ProCon</i>	14,143	0.00	0.00	0.00	0.01	0.00	1.00	0.08
<i>EmpCon</i>	14,143	0.00	0.00	0.00	0.00	0.00	1.00	0.04
<i>CgovCon</i>	14,143	0.00	0.00	0.00	0.02	0.00	1.00	0.15
<i>ROE</i>	14,143	-5.44	0.02	0.06	0.02	0.10	0.36	0.33
<i>OperMargin</i>	14,142	-3.20	0.02	0.06	0.05	0.14	0.70	0.29
<i>NetProfitMargin</i>	14,142	-4.19	0.02	0.06	0.04	0.12	0.61	0.32
<i>LoanAmt</i>	14,143	0.00	0.00	0.00	0.65	0.00	12.96	2.56
<i>GovSubs</i>	14,143	0.00	0.00	0.00	3.32	7.21	11.56	3.83
<i>LnAsset</i>	14,143	19.32	21.32	22.08	22.23	23.00	26.38	1.31
<i>Leverage</i>	14,143	0.04	0.27	0.43	0.44	0.60	0.91	0.21
<i>ROA</i>	14,143	-0.63	0.01	0.03	0.03	0.06	0.21	0.06
<i>CFO</i>	14,143	-0.20	0.00	0.04	0.04	0.08	0.27	0.07
<i>ATO</i>	14,143	0.04	0.32	0.50	0.61	0.75	2.73	0.44
<i>TobinQ</i>	14,143	0.38	1.28	1.78	2.29	2.66	15.89	1.68
<i>Top10</i>	14,143	0.20	0.46	0.58	0.57	0.68	0.96	0.15
<i>SOE</i>	14,143	0.00	0.00	0.00	0.39	1.00	1.00	0.49
<i>FirmAge</i>	14,143	1.61	2.64	2.89	2.84	3.09	3.53	0.35
<i>LowFinCons</i>	14,143	0.00	0.00	0.00	0.06	0.00	1.00	0.24
<i>LowIndCom</i>	14,143	0.00	0.00	0.00	0.05	0.00	1.00	0.22
<i>HighMktIndex</i>	14,143	0.00	0.00	0.00	0.40	1.00	1.00	0.49

TABLE 1 (Continued)*Summary Statistics*

<i>Panel B: Summary Statistics for Treated and Control Groups</i>						
Variables	Treated Firms			Control Firms		
	Obs	Mean	SD	Obs	Mean	SD
<i>TotalDonation</i>	2,588	2.74	3.01	11,555	0.94	2.00
<i>TPADonation</i>	2,588	1.74	2.67	11,555	0.58	1.61
<i>OtherDonation</i>	2,588	1.61	2.53	11,555	0.48	1.46
<i>Pollution</i>	2,588	1.17	3.33	11,555	0.37	1.72
<i>PollutionRev</i>	2,588	2.55	15.56	11,555	0.71	8.64
<i>PollutionCogs</i>	2,588	3.44	21.12	11,555	0.99	11.86
<i>EnvirPenalty</i>	2,588	0.04	0.19	11,555	0.03	0.18
<i>EnvirPenaltyNum</i>	2,588	0.03	0.17	11,555	0.03	0.15
<i>AirPol</i>	2,588	0.91	1.65	11,555	0.19	0.72
<i>GHG</i>	2,588	1.24	2.01	11,555	0.28	0.95
<i>LWP</i>	2,588	0.39	0.95	11,555	0.08	0.39
<i>Waste</i>	2,588	0.46	0.98	11,555	0.07	0.29
<i>ProCon</i>	2,588	0.03	0.16	11,555	0.00	0.05
<i>EmpCon</i>	2,588	0.01	0.07	11,555	0.00	0.03
<i>CgovCon</i>	2,588	0.05	0.22	11,555	0.02	0.13
<i>ROE</i>	2,588	0.07	0.13	11,555	0.01	0.36
<i>OperMargin</i>	2,588	0.09	0.19	11,554	0.04	0.31
<i>NetProfitMargin</i>	2,588	0.08	0.19	11,554	0.03	0.34
<i>LoanAmt</i>	2,588	1.26	3.57	11,555	0.51	2.26
<i>GovSubs</i>	2,588	3.61	4.29	11,555	3.26	3.71
<i>LnAsset</i>	2,588	23.45	1.39	11,555	21.96	1.12
<i>Leverage</i>	2,588	0.51	0.19	11,555	0.42	0.21
<i>ROA</i>	2,588	0.04	0.05	11,555	0.03	0.07
<i>CFO</i>	2,588	0.05	0.07	11,555	0.04	0.07
<i>ATO</i>	2,588	0.65	0.47	11,555	0.60	0.43
<i>TobinQ</i>	2,588	1.85	1.34	11,555	2.39	1.73
<i>Top10</i>	2,588	0.59	0.16	11,555	0.57	0.15
<i>SOE</i>	2,588	0.72	0.45	11,555	0.32	0.47
<i>FirmAge</i>	2,588	2.91	0.31	11,555	2.83	0.35

TABLE 2

Anti-Poverty Contribution after TPA Disclosure Mandate

This table reports the results of testing whether treated firms, which are subject to TPA disclosure mandate, donate more to TPA and any charitable causes after the reporting mandate. The dependent variable in Column (1) to (3), *TotalDonation*, is the natural logarithm of one plus all charitable donations in dollars. The dependent variable in Column (4) to (6), *TPADonation*, is the natural logarithm of one plus charitable donations toward poverty alleviation in dollars. Appendix B contains detailed descriptions of all variables. Standard errors are clustered by firms. ***, **, and * denote significance at the 1%, 5%, and 10%, respectively.

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	<i>TotalDonation</i>			<i>TPADonation</i>		
<i>Treat</i> × <i>Post</i>	0.453*** (0.130)	0.585*** (0.132)	0.468*** (0.130)	1.181*** (0.132)	1.306*** (0.137)	1.185*** (0.132)
<i>Treated</i>	1.487*** (0.112)			0.483*** (0.058)		
<i>LnAsset</i>		0.200*** (0.066)	0.275*** (0.063)		0.062 (0.061)	0.166*** (0.058)
<i>Leverage</i>		-0.061 (0.194)	0.022 (0.196)		-0.125 (0.179)	-0.069 (0.180)
<i>ROA</i>		1.920*** (0.270)	1.544*** (0.283)		1.452*** (0.243)	0.982*** (0.253)
<i>CFO</i>		0.454* (0.268)	0.373 (0.268)		0.319 (0.230)	0.273 (0.230)
<i>ATO</i>		0.012 (0.108)	0.054 (0.105)		-0.053 (0.101)	0.023 (0.098)
<i>TobinQ</i>		0.031** (0.015)	0.032** (0.016)		0.001 (0.013)	0.005 (0.013)
<i>Top10</i>		0.817*** (0.294)	0.281 (0.294)		0.942*** (0.278)	0.341 (0.265)
<i>SOE</i>		0.156 (0.197)	0.164 (0.193)		0.166 (0.185)	0.172 (0.179)
<i>FirmAge</i>		-0.535 (0.437)	-0.373 (0.434)		-0.216 (0.426)	-0.015 (0.418)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	Yes	No	Yes	Yes
Ind×Year FE	Yes	No	Yes	Yes	No	Yes
Obs.	14,143	14,143	14,143	14,143	14,143	14,143
Adj R-sq	0.28	0.53	0.54	0.28	0.46	0.49

TABLE 3*Environmental Externality of TPA Disclosure Mandate*

This table reports the results of testing whether treated firms, which are subject to TPA disclosure mandate, release more hazardous pollutants into the environment. The dependent variable, *Pollution*, is the logarithm of one plus total volume of major pollutants in tons. Appendix B presents detailed descriptions of all variables. Standard errors are clustered by firms. ***, **, and * denote significance at the 1%, 5%, and 10%, respectively.

VARIABLES	(1) <i>Pollution</i>	(2) <i>Pollution</i>	(3) <i>Pollution</i>
<i>Treat</i> × <i>Post</i>	0.396*** (0.140)	0.349** (0.142)	0.384*** (0.141)
<i>Treated</i>	0.503*** (0.097)		
<i>LnAsset</i>		-0.194*** (0.049)	-0.067 (0.051)
<i>Leverage</i>		-0.039 (0.178)	0.088 (0.178)
<i>ROA</i>		1.193*** (0.266)	0.559** (0.252)
<i>CFO</i>		0.322 (0.249)	0.333 (0.245)
<i>ATO</i>		0.002 (0.086)	-0.008 (0.087)
<i>TobinQ</i>		-0.020* (0.012)	-0.008 (0.012)
<i>Top10</i>		0.113 (0.236)	0.036 (0.243)
<i>SOE</i>		-0.093 (0.143)	-0.037 (0.152)
<i>FirmAge</i>		-0.399 (0.387)	-0.340 (0.398)
Year FE	Yes	Yes	Yes
Firm FE	No	Yes	Yes
Ind×Year FE	Yes	No	Yes
Obs.	14,143	14,143	14,143
Adj R-sq	0.12	0.37	0.39

TABLE 4

Additional Evidence on Environmental Externality

This table reports the results of various robustness tests on environmental externalities of the TPA disclosure mandate. In Panel A, we use alternative data on pollution from Trucost Database. The dependent variables in Column (1) to Column (4) of Panel A capture the estimated costs (in dollars) of direct air pollutants (*AirPol*), greenhouse gas emission (*GHG*), land and water pollutants (*LWP*) and waste production (*Waste*), respectively. In Panel B, we use a scaled version of the pollutant emissions to take into consideration the effects of possible business expansion. *PollutionRev* is total volume of major pollutants scaled by operating revenue while *PollutionCogs* is total volume scaled by cost of goods sold. In Panel C, we report the results of whether treated firms are more likely to be penalized by the local Environmental Protection Agency regarding environmental issues. *EnvirPenalty* is a dummy variable that indicates whether a firm receives environmental penalty while *EnvirPenaltyNum* is the logarithm of one plus the number of penalties a firm receives. Control variables in all panels are the same as those in Table 3. Appendix B presents detailed descriptions of all variables. Standard errors are clustered by firms. ***, **, and * denote significance at the 1%, 5%, and 10%, respectively.

<i>Panel A: Evidence from Trucost</i>				
VARIABLES	(1) <i>AirPol</i>	(2) <i>GHG</i>	(3) <i>LWP</i>	(4) <i>Waste</i>
<i>Treat</i> × <i>Post</i>	0.263*** (0.059)	0.366*** (0.069)	0.137*** (0.032)	0.214*** (0.035)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Obs.	14,143	14,143	14,143	14,143
Adj R-sq	0.74	0.75	0.74	0.75
<i>Panel B: Evidence from Accounting for Scale of Production</i>				
VARIABLES	(1) <i>PollutionRev</i>	(2) <i>PollutionCogs</i>		
<i>Treat</i> × <i>Post</i>	1.232*** (0.405)	1.740*** (0.587)		
Controls	Yes	Yes		
Year FE	Yes	Yes		
Firm FE	Yes	Yes		
Obs.	14,143	14,143		
Adj R-sq	0.16	0.16		
<i>Panel C: Evidence from Environmental Penalty</i>				
VARIABLES	(1) <i>EnvirPenalty</i>	(2) <i>EnvirPenaltyNum</i>		
<i>Treat</i> × <i>Post</i>	0.017** (0.008)	0.014* (0.007)		
Controls	Yes	Yes		
Year FE	Yes	Yes		
Firm FE	Yes	Yes		
Obs.	14,143	14,143		
Adj R-sq	0.23	0.25		

TABLE 5*Dynamic Effects*

This table reports the results of testing the dynamic effects of the TPA disclosure mandate on treated firms' spending on total donations, TPA donations and pollutions over time. $Treat \times Year_{(t-\tau)}$ is the interaction of $Treat$ and $Year_{(t-\tau)}$ in which τ denotes year τ relative to the year 2016. Control variables are the same as those in Table 2 and 3. Appendix B presents detailed descriptions of all variables. Standard errors are clustered by firms. ***, **, and * denote significance at the 1%, 5%, and 10%, respectively.

VARIABLES	(1) <i>TotalDonation</i>	(2) <i>TPADonation</i>	(3) <i>Pollution</i>
$Treat \times Year_{(t-2)}$	0.008 (0.106)	-0.015 (0.070)	0.073 (0.114)
$Treat \times Year_{(t-1)}$	-0.091 (0.112)	0.002 (0.078)	0.161 (0.125)
$Treat \times Year_{(t+1)}$	0.330** (0.161)	1.094*** (0.149)	0.601*** (0.198)
$Treat \times Year_{(t+2)}$	0.774*** (0.165)	1.310*** (0.158)	0.336* (0.203)
$Treat \times Year_{(t+3)}$	0.562*** (0.166)	1.500*** (0.160)	0.346* (0.204)
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Obs.	14,143	14,143	14,143
Adj R-sq	0.53	0.46	0.37

TABLE 6

Externalities on Other ESG Dimensions and Non-ESG Activities

This table reports the results of testing whether treated firms, which are subject to TPA disclosure mandate, are more concerned about other donations, as well as product, employee relations, and corporate governance related controversies (Panel A) and non-ESG activities including operating expenses and capital expenditures (Panel B). The dependent variable in Column (1) of Panel A, *OtherDonation*, is the natural logarithm of one plus the amount of social donations other than poverty alleviation contributions. The dependent variable in Column (2), *ProCon*, is a dummy variable that indicates whether a firm is subject to regulatory actions or lawsuits related to product or services. The dependent variable in Column (3), *EmpCon*, is a dummy variable that measures whether a firm involves in disputes or is penalized regarding employee health and safety related issues. The dependent variable in Column (4), *CgovCon*, is a binary indicator for whether a firm engages in financial misconduct. *OperExp* in Panel B is operating expenses scaled by lagged total assets while *Capx* is changes in fixed assets scaled by one-year-lagged total assets. Control variables are the same as those in Table 3. Appendix B contains detailed descriptions of all variables. Standard errors are clustered by firms. ***, **, and * denote significance at the 1%, 5%, and 10%, respectively.

<i>Panel A. Externality on Other ESG Activities</i>				
VARIABLES	(1) <i>OtherDonation</i>	(2) <i>ProCon</i>	(3) <i>EmpCon</i>	(4) <i>CgovCon</i>
<i>Treat</i> × <i>Post</i>	-0.280*** (0.096)	-0.006 (0.005)	-0.002 (0.003)	0.015 (0.010)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Obs.	14,143	14,143	14,143	14,143
Adj R-sq	0.44	0.17	0.07	0.11
<i>Panel B. Externality on Non-ESG Activities</i>			(1) <i>OperExp</i>	(2) <i>Capx</i>
<i>Treat</i> × <i>Post</i>			-0.010 (0.011)	0.002 (0.003)
Controls			Yes	Yes
Year FE			Yes	Yes
Firm FE			Yes	Yes
Obs.			14,143	14,143
Adj R-sq			0.80	0.11

TABLE 7

Environmental Externality and Financial Constraints

This table reports the results of testing whether the environmental externality is more pronounced among treated firms that are more financially constrained. *LowFinCons* is a binary indicator that equals one if treated firms are less financially constrained before the mandate and zero otherwise. We calculate the average value of the SA index from Hadlock and Pierce (2010) during the pre-mandate period and then sort all treated firms into terciles based on the index value. Treated firms in the bottom tercile are considered as being less constrained. In Column (1) (Column (2)), we retain (drop) firms in the middle tercile. Control variables are the same as those in Table 3. Appendix B presents detailed descriptions of all variables. Standard errors are clustered by firms. ***, **, and * denote significance at the 1%, 5%, and 10%, respectively.

VARIABLES	(1) <i>Pollution</i>	(2) <i>Pollution</i>
<i>Treat</i> × <i>Post</i> × <i>LowFinCons</i>	-0.559** (0.239)	-0.703** (0.351)
<i>Treat</i> × <i>Post</i>	0.536*** (0.192)	0.679** (0.321)
Controls	Yes	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
Obs.	14,143	13,281
Adj R-sq	0.37	0.37

TABLE 8

Environmental Externality and Competition

This table reports the results of testing whether the environmental externality is more concentrated among treated firms that are faced with a higher level of product market competition. We use two proxies to indicate product market competition. *LowIndCom* is an indicator variable that equals one for less competitive industries. We use operating revenue to calculate the Herfindahl Index for each industry every year and then average the index during the pre-regulation period. Then we sort industries into terciles based on the average HHI index. We consider a firm as facing low industry competition if the HHI of its industry falls into the top tercile. *HighMktIndex* is an indicator variable that equals one for provinces with a higher score in the marketization index (suggesting better product market development) during the pre-mandate period. We calculate the average ranks of product marketization index during the pre-mandate period and then sort provinces into terciles based on the ranks. We consider a firm facing high competition in a region if the average rank falls into the top tercile. Control variables are the same as those in Table 3. Appendix B presents detailed descriptions of all variables. Standard errors are clustered by firms. ***, **, and * denote significance at the 1%, 5%, and 10%, respectively.

VARIABLES	(1) <i>Pollution</i>	(2) <i>Pollution</i>	(3) <i>Pollution</i>	(4) <i>Pollution</i>
<i>Treat</i> × <i>Post</i> × <i>LowIndCom</i>	-1.212*** (0.342)	-1.149*** (0.341)		
<i>Treat</i> × <i>Post</i> × <i>HighMktIndex</i>			0.754** (0.316)	0.648** (0.320)
<i>Treat</i> × <i>Post</i>	0.403*** (0.147)	0.425*** (0.145)	0.116 (0.161)	0.214 (0.161)
<i>Post</i> * <i>LowIndCom</i>	-0.153 (0.173)			
<i>Post</i> * <i>HighMktIndex</i>			0.041 (0.080)	
Controls	Yes	Yes	Yes	Yes
Ind×Year FE	No	Yes	No	No
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Province×Year FE	No	No	No	Yes
Obs.	14,143	14,143	14,143	14,143
Adj R-sq	0.37	0.39	0.37	0.37

TABLE 9

Firm Profitability and Firm Value after Disclosure Mandate

This table reports the results of testing the change in firm profitability (Panel A) and firm value (Panel B) after the TPA disclosure mandate. *ROA* is defined as the net income scaled by total assets. *ROE* is defined as net income scaled by shareholder's equity. *OperMargin* is the operating profits scaled by total operating revenue. *NetProfitMargin* is the net profits scaled by total operating revenue. *TobinQ* is measured by total assets minus book value of equity plus market value of equity, divided by total assets. *HighTPA* is an indicator variable that equals one for treated firms with relatively more increases in TPA donations after the regulation. We first calculate the changes in TPA donations after the regulation and then sort the treated firms into terciles based on the change in TPA donations. Then we classify a firm into the top tercile as a *HighTPA* firm. Appendix B presents detailed descriptions of all variables. Standard errors are clustered by firms. ***, **, and * denote significance at the 1%, 5%, and 10%, respectively.

<i>Panel A: Firm Profitability after Disclosure Mandate</i>				
VARIABLES	(1) <i>ROA</i>	(2) <i>ROE</i>	(3) <i>OperMargin</i>	(4) <i>NetProfitMargin</i>
<i>Treat</i> × <i>Post</i> × <i>HighTPA</i>	0.011*** (0.004)	0.025** (0.010)	0.029** (0.012)	0.037*** (0.014)
<i>Treat</i> × <i>Post</i>	0.006* (0.003)	0.043*** (0.011)	0.022* (0.012)	0.029** (0.014)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Obs.	14,143	14,143	14,143	14,143
Adj R-sq	0.28	0.12	0.22	0.16

<i>Panel B: Firm Value after Disclosure Mandate</i>	
VARIABLES	<i>TobinQ</i>
<i>Treat</i> × <i>Post</i> × <i>HighTPA</i>	0.497*** (0.081)
<i>Treat</i> × <i>Post</i>	0.201*** (0.076)
Controls	0.779*** (0.242)
Year FE	Yes
Firm FE	Yes
Obs.	14,143
Adj R-sq	0.65

TABLE 10*Loan Amount from SOE banks and Government Subsidies Received after Disclosure Regulation*

This table reports the results of testing whether treated firms, which are subject to TPA disclosure mandate, receive more loans from state-owned banks (SOE banks) and more government subsidies after the regulation. The dependent variable *SOEBankLoan* in Column (1) is the logarithm of one plus total loan amount obtained from SOE banks. The dependent variable *GovSubs* in Column (2) is calculated by the logarithm of one plus the amount of subsidies received from the government. Control variables are the same as those in Table 3. Appendix B presents detailed descriptions of all variables. Standard errors are clustered by firms. ***, **, and * denote significance at the 1%, 5%, and 10%, respectively.

VARIABLES	(1) <i>SOEBankLoan</i>	(2) <i>GovSubs</i>
<i>Treat×Post×HighTPA</i>	0.817**	1.035***
	(0.400)	(0.243)
<i>Treat×Post</i>	0.535***	0.533***
	(0.200)	(0.135)
Controls	Yes	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
Obs.	14,143	14,143
Adj R-sq	0.39	0.83

TABLE 11

Political Promotion Incentive

This table reports the city-year level results of testing whether the Communist Party of China (CPC) secretaries of treated cities are more likely to be promoted after the TPA reporting mandate. *Treat_City* in Panel A is an indicator variable which is equal to one for cities with at least one treated firm and zero otherwise. In Panel B, we replace *Treat_City* with *Treat_City_Amt* which is the aggregated anti-poverty donation amount by all treated firms in a year. The dependent variable *Promoted* is an indicator variable which is equal to one if a city experiences CPC secretary turnover and the prior secretary is promoted to a higher position, and zero otherwise. *Tenure* is the number of years a given CPC secretary has been in that position in a city. *GDP_percap* is the natural logarithm of city-level GDP per capita. Standard errors are clustered at the city level. ***, **, and * denote significance at the 1%, 5%, and 10%, respectively.

<i>Panel A: Treatment Defined as Cities with at Least One Treated Firm</i>				
VARIABLES	(1) <i>Promoted</i>	(2) <i>Promoted</i>	(3) <i>Promoted</i>	(4) <i>Promoted</i>
<i>Treat_City</i> × <i>Post</i>	0.105*** (0.035)	0.103*** (0.035)	0.113*** (0.039)	0.128*** (0.045)
<i>Treat_City</i>	-0.027 (0.024)	-0.027 (0.024)		
<i>Post</i>	0.012 (0.021)			
<i>Tenure</i>	0.055*** (0.010)	0.055*** (0.010)	0.125*** (0.010)	0.121*** (0.013)
<i>GDP_percap</i>				0.001 (0.014)
Year FE	N	Y	Y	Y
City FE	N	N	Y	Y
Obs.	1,686	1,686	1,686	1,234
Adj R-sq	0.044	0.079	0.122	0.116
<i>Panel B: Treatment Defined as the Aggregate Donation Amount of a City</i>				
VARIABLES	(1) <i>Promoted</i>	(2) <i>Promoted</i>	(3) <i>Promoted</i>	(4) <i>Promoted</i>
<i>Treat_City_Amt</i> × <i>Post</i>	0.017** (0.007)	0.017** (0.007)	0.019** (0.010)	0.025** (0.011)
<i>Treat_City_Amt</i>	-0.009 (0.006)	-0.009 (0.006)	-0.011 (0.012)	-0.013 (0.014)
<i>Post</i>	0.028 (0.019)			
<i>Tenure</i>	0.054*** (0.010)	0.054*** (0.010)	0.125*** (0.010)	0.121*** (0.012)
<i>GDP_percap</i>				-0.001 (0.014)
Year FE	N	Y	Y	Y
City FE	N	N	Y	Y
Obs.	1,686	1,686	1,686	1,234
Adj R-sq	0.041	0.077	0.119	0.114

Appendix A. An Illustrative Sample of TPA Disclosure

The mandated TPA disclosure regulation requires treated firms to disclose quantitatively detailed information on TPA donations. We provide an example of the TPA disclosures as follow (in translated version):

<i>Indicators</i>	<i>Unit</i>	<i>Quantity/Progress</i>
<i>1. Total contribution</i>	-	-
Fund	'0000 RMB	700.29
Material	'0000 RMB	250
<i>2. Disaggregated contribution</i>	-	-
(1) Industrial development	-	-
(2) Improving employment	-	-
(3) Relocating the poor	-	-
(4) Education	-	-
4.1 Fund to poor students	'0000 RMB	320.39
4.2 Number of poor students	Number	2,207
4.3 Fund to improve educational resources	'0000 RMB	629.9
(5) Healthcare	-	-
(6) Ecological protection	-	-
(7) Guarantee for basic living standard	-	-
(8) Social assistance	-	-
(9) Other programs	-	-
<i>3. Awards</i>	-	-

Appendix B. Variable Definitions

Variable Name	Definition
<i>Key variables of interests</i>	
<u><i>Dependent variables</i></u>	
<i>TotalDonation</i>	The logarithm of one plus all charitable money in dollars
<i>TPADonation</i>	The logarithm of one plus charitable money in dollars donated toward poverty alleviation
<i>Pollution</i>	The logarithm of one plus total volume of major pollutants in tons; These pollutants include liquid waste material, industrial sewage/garbage, and poisonous heavy metals, such as arsenic (As), chromium (Cr), mercury (Hg), cadmium (Cd), thallium (Tl), and lead (Pb) etc.
<i>PollutionRev</i>	Total volume of major pollutants (tons) divided by operating revenue (millions)
<i>PollutionCogs</i>	Total volume of major pollutants (tons) divided by cost of goods sold (millions)
<i>OtherDonation</i>	The logarithm of one plus the amounts of all social donations in dollars other than poverty alleviation donations
<i>EnvirPenalty</i>	Dummy variable that indicates whether a firm receives any administrative punishment for a violation of the environmental protection law
<i>EnvirPenaltyNum</i>	The logarithm of one plus the number of environmental penalties by a firm
<i>AirPol</i>	The logarithm of one plus the direct costs of air pollutants in million US dollar. The direct costs of air pollutants refer to the cost of pollutants released to air by the consumption of fossil fuels and production processes which are owned or controlled by the company
<i>GHG</i>	The logarithm of one plus the direct costs of greenhouse gas emission in million US dollar
<i>LWP</i>	The logarithm of one plus the direct costs of land and water pollutants in million US dollar
<i>Waste</i>	The logarithm of one plus the direct costs of waste production in million US dollar
<i>ProCon</i>	Dummy variable that indicates whether a firm has been subject to any regulatory actions or lawsuits related to products or services
<i>EmpCon</i>	Dummy variable that measures whether a firm has been subject to any regulatory actions or lawsuits related to employee health and safety related issues
<i>CgovCon</i>	Dummy variable that captures whether a firm has been subject to any regulatory actions or lawsuits related to corporate misconduct
<i>ROE</i>	Net income divided by the shareholder's equity
<i>OperMargin</i>	Operating profits divided by total operating revenue
<i>NetProfitMargin</i>	Net profits divided by total operating revenue
<i>SOEBankLoan</i>	The logarithm of one plus the loan amount borrowed from SOE banks
<i>GovSubs</i>	The logarithm of one plus the government subsidies received by a firm
<u><i>Independent variables</i></u>	
<i>Treat</i>	Indicator variable that equals one if a firm is subject to the TPA disclosure mandate
<i>Post</i>	Indicator variable that equals one for years after 2016
<i>Treat×Post</i>	Interaction of <i>Treat</i> and <i>Post</i>
<i>Treat × Year_(t-τ)</i>	Interaction of <i>Treat</i> and <i>Year_(t-τ)</i> in which τ denotes year τ relative to the year 2016
<i>LowFinCons</i>	Indicator variable that equals one for treated firms that are less financially constrained before the reporting mandate. We calculate average SA index during the pre-regulation period and then sort treated firms into terciles based on the index. Treated firms in the bottom tercile are considered to be less financially constrained

<i>LowIndCom</i>	Indicator variable that equals one for industries that are less competitive. We use operating revenue to calculate the Herfindahl Index for each industry every year and then average the index during the pre-regulation period. Then we sort industries into terciles based on average HHI. We consider a firm facing low industry competition, if the HHI of its industry falls into the top tercile
<i>HighMktIndex</i>	Indicator variable that equals one for provinces with a better product market development. We compute the average rank of the product market index in the pre-mandate period and sort provinces into terciles based on the average ranks. We consider a firm facing high competition in a province which falls into the top tercile in the ranking
<i>HighTPA</i>	Indicator variable that equals one for treated firms with relatively more increases in TPA donations after the regulation. We first calculate the changes in TPA donations after the regulation and then sort the treated firms into terciles based on the change in TPA donations. Then we classify a firm falling into the top tercile as a <i>HighTPA</i> firm
<u><i>Control variables</i></u>	
<i>LnAsset</i>	The logarithm of total assets
<i>Leverage</i>	Total liabilities divided by total assets
<i>ROA</i>	Net income divided by total assets
<i>CFO</i>	Cash flow from operations divided by total assets
<i>ATO</i>	Operating revenue divided by total assets
<i>TobinQ</i>	Total assets minus book value of equity plus market value of equity, divided by total assets
<i>Top10</i>	Total equity holdings by top 10 shareholders
<i>SOE</i>	Indicator variable that equals one is a firm is a state-owned entity
<i>FirmAge</i>	The logarithm of firm age

Appendix C.

Table C1. Changes in TPA Donations and Changes in Pollutant Emissions

This table reports the results of testing whether changes in anti-poverty contributions are positively associated with changes in pollutant emissions after the regulation. To construct *Pollution_change*, we first take the average of the total volume of pollutant emissions in the pre-mandate period and the post-mandate period, respectively. We then take the natural logarithm of one plus the absolute value of average volume change (post-mandate — pre-mandate) and multiply the log-transformed amount by -1 if the change is negative. *TotalDonation_change* and *TPADonation_change* are calculated in a similar way. Other control variables are constructed by calculating the difference between their post-mandate average values and pre-mandate average values. Standard errors are clustered by firms. ***, **, and * denote significance at the 1%, 5%, and 10%, respectively.

VARIABLES	(1) <i>Pollution_change</i>	(2) <i>Pollution_change</i>
<i>TotalDonation_change</i>	0.056* (0.031)	
<i>TPADonation_change</i>		0.072* (0.038)
<i>LnAsset_change</i>	0.040** (0.018)	0.039** (0.018)
<i>Leverage_change</i>	-1.080** (0.439)	-1.073** (0.439)
<i>ROA_change</i>	1.051 (1.148)	1.145 (1.141)
<i>CFO_change</i>	2.341** (0.919)	2.323** (0.918)
<i>ATO_change</i>	0.275 (0.254)	0.267 (0.255)
<i>TobinQ_change</i>	0.037 (0.048)	0.039 (0.048)
<i>Top10_change</i>	0.260 (0.607)	0.277 (0.609)
Year FE	Yes	Yes
Firm FE	Yes	Yes
Obs.	2,376	2,376
Adj R-sq	0.06	0.06

Table C2. Additional Robustness Test from a Matched Sample

This table reports the results of testing whether treated firms, which are subject to TPA disclosure mandate, pollute more after the mandate using a matched sample. We adopt a propensity score matching approach to match treated firms with control firms based on a few observable characteristics set out in Table 3 to ensure that the two groups of firms are comparable along those dimensions. In particular, we match a treated firm to its three closest peer firms that have comparable firm characteristics. Appendix B presents detailed descriptions of all variables. Standard errors are clustered by firms. ***, **, and * denote significance at the 1%, 5%, and 10%, respectively.

VARIABLES	(1) <i>Pollution</i>	(2) <i>Pollution</i>	(3) <i>Pollution</i>
<i>Treat</i> × <i>Post</i>	0.371** (0.150)	0.381** (0.151)	0.315** (0.149)
<i>Treat</i>	0.392*** (0.099)	0.126 (0.103)	
<i>Controls</i>	No	Yes	Yes
Year FE	Yes	Yes	Yes
Firm FE	No	No	Yes
Ind×Year FE	Yes	Yes	No
Obs.	8,577	8,577	8,577
Adj R-sq	0.11	0.13	0.38

Table C3. Environmental Externality and Institutional Holdings

This table reports the cross-sectional results of testing whether the environmental externality of the TPA disclosure mandate vary across firms with different levels of institutional holdings. *HighInsOwn* is an indicator variable that equals one for treated firms with a higher level of ownership by institutional shareholders. In Column (1) (Column (2)), we retain (drop) firms in the middle tercile. Standard errors are clustered by firms. ***, **, and * denote significance at the 1%, 5%, and 10%, respectively.

VARIABLES	(1) <i>Pollution</i>	(2) <i>Pollution</i>
<i>Treat</i> × <i>Post</i> × <i>HighInsOwn</i>	0.253 (0.308)	0.414 (0.325)
<i>Treat</i> × <i>Post</i>	0.264* (0.158)	0.107 (0.191)
<i>Controls</i>	Yes	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
Obs.	14,143	13,281
Adj R-sq	0.37	0.35

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