

Mandatory ESG Disclosure and Government Green Procurement

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Abstract

We examine how mandatory ESG disclosure reshapes the way governments, acting as strategic buyers, allocate green public procurement. Using global green government contract data covering 57 countries from 2009 to 2022, we find that the value of green contracts received by affected firms rises significantly following the staggered adoption of disclosure mandates. The contract concentration falls and firms previously outside the procurement perimeter become substantially more likely to win contracts. We label this pattern a democratization of green procurement. The effect is amplified in common-law jurisdictions, under hard-compliance mandates, under the government with less diversity, and where prior ESG information was scarce. Democratization carries real consequences. Treated firms reduce their carbon emission significantly. New green contractors induced by the disclosure mandates divert from general innovation to green innovation. Our findings show that mandatory ESG disclosure functions as an institutional lever of non-market change, generating fiscal spillovers that shift green procurement from relational to rules-based allocation and improving the environmental productivity of public spending without raising aggregate expenditure.

Keywords: Mandatory ESG disclosure, government green procurement, transparency, democratization, non-market institutions

JEL Classification: M48, G38, H57, Q56

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1. Introduction

Sustainable development has become a defining axis of national competitive strategy and a growing arena for state action in international business. Aligned with the United Nations Sustainable Development Goals,¹ governments increasingly use public procurement, which accounts for roughly 12 percent of global GDP and up to 45 percent of government spending in OECD countries, to steer markets toward sustainability. Through Green Public Procurement (GPP),² the state has shifted from a passive regulator into a strategic buyer that reshapes global value chains via its monopsony power (Van Assche and De Marchi, 2024, Belenzon and Cioaca, 2025). Yet the implementation of GPP is constrained by a fundamental information friction. Environmental attributes are credence goods that buyers cannot easily verify either ex ante or ex post (Akerlof, 1970, Even-Tov et al., 2025). Facing this opacity, procurement officials tend toward risk aversion, repeatedly contracting with a narrow set of familiar incumbents whose environmental performance they believe they can predict.

Over the past decade, more than 50 countries have shifted non-financial reporting from voluntary to mandatory regimes, generating an exogenous institutional shock to the corporate information environment (Christensen et al., 2021). By imposing standardized reporting, mandatory ESG disclosure sharply lowers the cost of verifying corporate environmental claims. This raises a question at the intersection of international business, accounting, and public policy.

¹ Goal 12.7 targets to “promote public procurement practices that are sustainable, in accordance with national policies and priorities”. See <https://www.un.org/sustainabledevelopment/sustainable-consumption-production/>

² Green public procurement (GPP) is defined as a process by which public authorities seek to procure goods, services, and works that have a reduced environmental impact throughout their life cycle. See https://green-forum.ec.europa.eu/green-business/green-public-procurement_en#:~:text=Green%20Public%20Procurement%20,that%20would%20otherwise%20be%20procured

When firm-level ESG transparency improves through regulatory mandate, how does the state, acting as a strategic buyer, change the way it allocates green public contracts?

At the theoretical level, two competing institutional logics describe how mandatory ESG disclosure interacts with green public procurement. On the one hand, mandatory disclosure may *complement* GPP. In the absence of reliable information, governments face high adverse selection costs in identifying genuinely green suppliers. Standardized ESG data functions as a public good that lowers search and monitoring costs for procurement officials (Gibbons, 2024), strengthening the state's capacity to expand the scale and scope of green contracting. On the other hand, disclosure mandates may *substitute* for GPP. Because green procurement involves price premia and administrative costs and operates as a corrective tool for environmental externalities, transparent ESG performance may allow capital markets, product markets, and broader stakeholder pressure to discipline polluters and reward green firms directly (Darendeli et al., 2022, Ilhan et al., 2023), thereby diminishing the marginal value of additional procurement-based incentives.

A third possibility, which we develop as our central hypothesis, is that mandatory ESG disclosure produces a *democratization effect* on green public procurement. In opaque environments, procurement officials lean on relational contracting and incumbency to manage accountability risk, generating a "liability of outsidership" (Johanson and Vahlne, 2009) that effectively excludes green-capable new entrants and small and medium-sized enterprises lacking established procurement relationships. Standardized disclosure functions as a depersonalized trust technology that lets outsiders establish legitimacy through verifiable data rather than relational capital. Under this logic, disclosure should reorganize procurement

allocation not by enlarging the budget but by broadening the supplier base and reducing bid concentration. Importantly, this redistribution is not merely a matter of fairness. If the new entrants drawn into the supplier base are more environmentally efficient than the incumbents they displace, dispersion itself generates positive externalities, allowing the same fiscal envelope to deliver greater environmental output.

We test these predictions using global GPP data covering 57 countries from 2009 to 2022, exploiting the staggered adoption of mandatory ESG disclosure as a quasi-natural experiment. At the firm level, the ratio of green contract value to assets rises by about 0.091 percent following a mandate, roughly doubling the pre-regulation level. Event-study estimates show no differential pre-trends, supporting a causal interpretation that the transparency shock, rather than concurrent macroeconomic conditions, drives the procurement outcome.

To unpack this average effect, we examine cross-sectional heterogeneity along institutional and policy dimensions. Three patterns emerge. First, the effect is concentrated in Common Law jurisdictions and weaker in Civil Law countries (La Porta et al., 1998, Liang and Renneboog, 2017), consistent with the view that Common Law systems rely more on public information for coordination, whereas Civil Law systems already channel information through stakeholder networks where the marginal informational gain from a mandate is smaller. Second, the effect is amplified when the reform is implemented directly by the government and when compliance is mandatory rather than “comply-or-explain” (Krueger et al., 2024), confirming that strong enforcement is necessary for disclosure to alter bureaucratic allocation behavior. Third, the effect is attenuated in countries with more diverse governments. The diversity within a government may create more interest conflicts and enhance the power of

anti-climate parties/associations, thus weakens the main effect. Fourth, the effect attenuates in countries with prior voluntary disclosure regimes, suggesting that pre-existing information channels reduce the marginal value of a mandate. Together these patterns indicate that formal institutions and policy design jointly shape how a global accounting reform translates into local procurement outcomes.

We then ask if there is redistribution of the same budget across a broader set of firms. At the country level, contract concentration measured by the Herfindahl-Hirschman Index (HHI) falls significantly. Consistent with this, the probability of initializing green contract increases more significantly than continues winning tender. The pattern is consistent with disclosure functioning as a depersonalized trust mechanism that lowers the liability of outsidership and broadens market access. Governments do not enlarge the budget. They reallocate it across a wider supplier base.

The real impact also caught our attention; we find that firms reduce their emissions (both in absolute value and emission intensity) after the policy, and the green contract dispersion induced by ESG mandatory disclosure promotes the green innovation of the firm. Taken together, the firm-level expansion, the aggregate-level reallocation, and the environmental and innovation outcomes paint a coherent picture. Mandatory ESG disclosure functions as a positive-sum reform that more green innovation, and more inclusive market access. Transparency, in this setting, is not just a screening device but a redistributive institution with technological dividends.

Our study makes three contributions. First, we extend research on government contracting and corporate outcomes. Prior works largely examine firm-level returns to procurement,

including how stable government demand lowers volatility and the cost of capital (Cohen and Li, 2020, Goldman, 2020) and how procurement spurs innovation (Beraja et al., 2023, Belenzon and Cioaca, 2025), as well as how firm-level signals or transparency improve a single firm's chance of winning contracts (Flammer, 2018, Even-Tov et al., 2025). We shift attention from how an individual firm wins contracts to how the state itself reorganizes contract allocation when the information environment changes for all firms simultaneously. Whereas prior work treats disclosure as an endogenous firm strategy to attract government demand (Samuels, 2021, Flammer, 2018), our staggered cross-country setting identifies exogenous variation in the information environment and shows that improved transparency reconfigures the state's allocation logic from narrow relational matching to broader rules-based screening. The shift documented here is not about which firms are smarter signalers but about how the state itself updates its procurement architecture in response to a market-wide informational shock.

Second, we extend the *real effects* literature on mandatory disclosure beyond the firm boundary. Existing works mostly document how disclosure reshapes firms' own investment, financing, and emissions decisions (Christensen et al., 2017, Tomar, 2023). We show that disclosure regulation also reorganizes the behavior of a primary non-market stakeholder, the government, generating fiscal spillovers that travel through public procurement. The same transparency shock that disciplines a firm's reporting widens the set of firms the state is willing to contract with, lowers concentration, and improves the environmental productivity of public spending. This connects to and extends the literature on customers' demand for supplier disclosure (Hope et al., 2021, Samuels, 2021), which has examined how large buyers ask for

more information from their suppliers. Our evidence shows that when transparency is supplied through a regulatory mandate rather than through bilateral negotiation, large public buyers respond by restructuring procurement allocation itself to advance public objectives, not merely by recalibrating supplier screening within an existing supplier set.

Third, we contribute to the international business literature on the state as a strategic buyer and on non-market strategy (Van Assche and De Marchi, 2024, Cuervo-Cazurra et al., 2022). We provide the first large-scale cross-country evidence that a transparency-based institution can reconfigure a state's supplier base by lowering the liability of outsidership (Johanson and Vahlne, 2009) faced by firms without relational capital. The heterogeneity we document across legal origins and enforcement regimes speaks directly to the literature on institutional complementarities. Disclosure-based governance is most powerful where formal institutions privilege public information as the dominant coordination mechanism, and least powerful where stakeholder-oriented relational networks already substitute for it. By linking procurement reallocation to green innovation and to lower aggregate emissions, we further show that corporate accounting regulation can operate as a lever of non-market institutional change in the global pursuit of sustainability, complementing IB debates on institutional voids (Khanna and Palepu, 2010) and on innovation-driven internationalization.

The remainder of the paper proceeds as follows. Section 2 reviews the related literature. Section 3 presents the data and main empirical results. Section 4 examines the reallocation of green procurement and its efficiency implications. Section 5 concludes.

2. Literature Review

We anchor our study in three streams of research that jointly motivate the empirical question. The first stream, on government procurement and information frictions, explains why credence-good problems generate persistent allocation distortions in the absence of standardized information. The second stream, on the real effects of mandatory disclosure, demonstrates that information regulation can reshape the behavior of disclosing firms and the stakeholders that consume their reports, but has not fully examined the public buyer as such a stakeholder. The third stream, on comparative institutional environments in international business, provides the theoretical apparatus for understanding when transparency-based governance can replace relational governance. We discuss each in turn and use them to position our contribution.

2.1 Information Frictions and Allocation in Government Procurement

The traditional IB framing treats the state as a rule-setter whose institutional environment, including the rule of law, property-rights protection, and enforcement quality, conditions the entry modes and operational performance of multinational enterprises (North, 1990). A complementary stream examines state-owned enterprises and how state ownership shapes firms' international expansion. Both perspectives position the state as a background institution rather than as an active economic actor.

Recent research reconceptualizes the state as a strategic buyer whose procurement decisions exercise demand-side industrial policy and shape global value chains (Van Assche and De Marchi, 2024). From a corporate finance perspective, the government functions as a customer of last resort that supplies stable, counter-cyclical demand and lowers firms' cost of capital (Cohen and Li, 2020, Goldman, 2020). Access to this strategic customer, however, is

mediated by a fundamental information friction. Public procurement is a classic principal-agent setting in which the buyer cannot perfectly observe supplier quality (Laffont and Tirole, 1993). The friction is amplified in green procurement, where environmental attributes are credence goods that resist verification both *ex ante* and *ex post* (Even-Tov et al., 2025). Under such opacity, procurement officials manage accountability risk by leaning on familiar incumbents whose performance they believe they can predict, generating a “liability of outsidership” (Johanson and Vahlne, 2009) for entrants that lack established procurement relationships.

Three adaptive responses have been documented in prior work. On the supply side, firms invest in voluntary CSR or signaling activities to demonstrate trustworthiness, especially for complex contracts (Flammer, 2018). On the demand side, large public buyers exercise contractual leverage to require more transparent reporting from their suppliers (Hope et al., 2021, Samuels, 2021, Even-Tov et al., 2025). Most directly related to our setting, Kim, Sun, Xiang, and Zeng (2026) study how mandatory ESG disclosure rules in a supplier’s home country affect that supplier’s success in winning procurement contracts from foreign governments, finding that disclosure mandates ease information frictions across borders and improve foreign procurement access for affected firms. Their evidence highlights the cross-border informational value of ESG mandates from the perspective of an individual foreign supplier-buyer pair.

These mechanisms, however, operate either inside an existing buyer-supplier dyad or along a single cross-border supplier-buyer link. They explain how a particular supplier becomes more visible to a particular buyer once transparency improves, but not how the buyer reorganizes its overall allocation when transparency improves simultaneously for the entire

universe of domestic firms. We complement this literature by shifting the unit of analysis from the dyadic level to the level of the home-country government's full procurement portfolio. We ask whether market-wide transparency dissolves the relational frictions that concentrate green procurement among a narrow set of incumbents and whether it broadens the supplier base in a way that delivers measurable environmental returns. This portfolio-level democratization question, which is distinct from the question of whether any individual firm wins more contracts, is the gap our study addresses.

2.2 The Real Effects of Mandatory Disclosure

A growing real effects literature challenges the view that disclosure is merely a passive veil over fundamentals and shows that information regulation directly alters firm behavior. Early work focused on capital-market outcomes such as the cost of capital and liquidity (Leuz and Verrecchia, 2000). More recent studies document changes in real activity. Christensen et al. (2017) exploit the Dodd-Frank mine safety disclosure rule and find significant declines in violations and casualties through heightened public scrutiny. In the environmental domain, Tomar (2023) shows that mandatory greenhouse gas reporting prompts US facilities to reduce emissions through a benchmarking channel, and Downar et al. (2021) report that UK mandatory carbon reporting lowers regulated firms' emissions without harming financial performance. Bonetti et al. (2023) find that mandatory disclosure of fracking fluid chemicals improves water quality through pressure from social movements and NGOs. Chen et al. (2018) document that China's 2008 CSR mandate reduces industrial wastewater emissions but lowers profitability, illustrating real trade-offs between social and shareholder outcomes. These effects

also propagate along supply chains as firms tighten standards on their partners to avoid reputational contagion (Dai et al., 2021, She, 2022).

Existing real effects research has predominantly examined how disclosure affects private stakeholders such as investors, customers, and supply-chain partners. The public buyer, despite being one of the largest consumers of firm-level information in the global economy, has remained largely outside this research frontier. The closest precedent is Kim, Sun, Xiang, and Zeng (2026), who show that home-country ESG disclosure mandates spill over to the procurement decisions of foreign governments, documenting that the real effects of disclosure regulation cross national borders through the public-buyer channel. Their study confirms that government buyers respond to mandated transparency, but it does so by tracing the effect to a single foreign-buyer outcome at the firm level. We extend this frontier in two complementary directions. First, we examine the home-country government as a buyer and show that disclosure mandates also restructure domestic green procurement, where the public-policy stakes for sustainability are highest. Second, we move from the firm-level outcome of winning contracts to the portfolio-level outcomes of supplier concentration, fiscal allocation, and aggregate environmental productivity, showing that disclosure regulation generates fiscal spillovers that travel through public procurement budgets rather than only through individual contract awards.

A natural question is whether voluntary ESG disclosure could deliver the same institutional function. The evidence suggests it cannot. Theoretically, only firms with favorable news disclose voluntarily, producing selection bias and limited cross-firm comparability (Verrecchia, 1983). Empirically, Krueger et al. (2024) show that the informational gains from mandatory ESG disclosure substantially exceed those of voluntary regimes, and Cui et al. (2025) document

that mandatory disclosure reduces earnings management whereas voluntary disclosure does not. Francis et al. (2008) further show that the apparent cost-of-capital benefits of voluntary disclosure largely disappear once earnings quality is controlled for, indicating that voluntary disclosure is itself an endogenous outcome of underlying firm quality rather than an independent informational channel.

A second limitation of voluntary disclosure is its proprietary cost. Verrecchia (1983) formalizes how voluntarily disclosed information is appropriable by rivals and regulators, attenuating firms' competitive advantage. Consistent with this prediction, Ellis et al. (2012) and Li et al. (2018) find that firms with higher proprietary costs, proxied by R&D and advertising intensity or by trade-secret protection, disclose less. Andrus et al. (2023) further show that greater clarity in voluntary non-financial disclosure can trigger negative market reactions and elevated reputational risk. Voluntary regimes therefore generate a selection barrier that disproportionately deters smaller and less-established firms, precisely the firms whose participation is most valuable for procurement democratization. This asymmetry motivates our focus on mandatory rather than voluntary disclosure as the institutional lever operating on government green procurement.

2.3 From Relational to Rules-Based Governance Across Institutional Environments

A core IB tradition holds that as institutions mature, economic exchange shifts from relational governance, sustained by interpersonal trust and repeated interaction, to rules-based governance, sustained by explicit contracts and verifiable public information (Peng, 2003). Whether disclosure-based governance can affect this transition depends on institutional complementarities. La Porta et al. (1998) and Liang and Renneboog (2017) show that Common

Law systems privilege public information, market-based contracting, and dispersed ownership, whereas Civil Law systems channel coordination through stakeholder networks and state intermediation. We therefore expect the procurement effects of mandatory ESG disclosure to bind more strongly in Common Law settings, where public information is the dominant governance currency, and to bind less in Civil Law settings, where stakeholder-mediated information already partially substitutes for market-wide reporting.

Enforcement and policy design matter symmetrically. Krueger et al. (2024) show that enforcement intensity is a first-order determinant of whether disclosure regulation alters firm behavior, suggesting that mandates implemented directly by the government and structured around hard compliance, rather than around “comply-or-explain” flexibility, should produce stronger procurement effects. The pre-existing information environment also matters. Where voluntary disclosure regimes are already widespread, the marginal informational gain from a mandate is smaller, and the incremental procurement effect should accordingly attenuate. In emerging markets, where institutional voids that take the form of missing intermediaries inflate the cost of verifying supplier quality (Khanna and Palepu, 2010), mandatory disclosure can substitute for missing information infrastructure and allow entrants to establish legitimacy without expensive private certification.

Synthesizing these strands, our study positions mandatory ESG disclosure as an institutional lever that, under the right complementarities, displaces relational procurement networks with rules-based allocation. The transition redistributes contracts toward a broader supplier base, generates measurable environmental and innovation externalities, and offers cross-country evidence on when accounting regulation can function as a form of non-market

institutional change in the global pursuit of sustainability. The next sections describe the data and present the empirical evidence that supports this view.

Overall, our findings carry three implications for international business research and policy. First, they reposition corporate accounting regulation as a lever of non-market institutional change. The same transparency reform that reshapes firms' own behavior also reorganizes the allocation strategy of one of the largest customers in the global economy, generating fiscal spillovers that travel through public procurement budgets rather than through capital markets alone. Second, the heterogeneity across legal origins and enforcement regimes underscores the role of institutional complementarities. Disclosure-based governance is most powerful where formal institutions privilege public information as the dominant coordination mechanism, and least powerful where stakeholder-mediated relational networks already substitute for it. Third, for policymakers, the evidence suggests that mandatory ESG disclosure and green public procurement are institutional complements rather than substitutes. Transparency reforms can be designed not only to inform markets but also to broaden access to public demand, with measurable downstream gains in environmental productivity and in the technological capability of the supplier base.

3. Main Empirical Results

This section describes the data, defines the main variables, presents the empirical strategy, and reports the firm-level evidence on the impact of mandatory ESG disclosure on green contract receipt. Section 4 then turns to the country-level reallocation and efficiency consequences.

3.1 Data and Sample

The dependent variable is constructed from the FactSet TenderAlpha database, which provides contract-level information on government green procurement worldwide, including the awardee's name, ISIN (for listed awardees), the maximum potential tender value in USD, and the tender award and dispatch dates. We use the maximum potential tender value to proxy the total amount of green contracts participated by a firm in a given year³. Although TenderAlpha records extend back to 2001, observations are concentrated after 2009. Our sample period therefore runs from 2009 to 2022, which also avoids the confounding effects of the global financial crisis.

Firm-level financial data come from Compustat and CRSP.⁴ Country-level macroeconomic data comes from the World Bank. The timing of mandatory ESG disclosure adoption in each country is taken from Krueger et al. (2024). We keep the countries with more than 100 observations. We also exclude all U.S. firms for two reasons. First, U.S. firms account for more than half of the global Compustat universe (Krueger et al., 2024), and including them would mechanically tilt the estimates toward U.S. conditions. Second, the U.S. has not adopted a mandatory ESG disclosure regime during our sample period, making it an imperfect control group for a staggered identification strategy. Following standard practice, we drop financial firms (SIC 6000-6999) and utilities (SIC 4900-4949) because their disclosure environment is

³ We utilize “participate” rather than “received” since there is co-awarding green contract in our database, which means that there are more than 1 awardees in the tender. Since the database does not provide the information about the allocation among the contractors, we cannot precisely calculate the value received. In Appendix E, we assume that the contractors take equal proportion of the contract amount and re-run the baseline regression. Our result is robust to this setting.

⁴ The CRSP/Compustat merged database is our complementary data to capture the firms registered outside the U.S. but listed in the U.S. exchanges.

shaped by separate regulatory regimes (Even-Tov et al., 2025). The final sample contains 128,260 firm-year observations across 57 countries from 2009 to 2022.

3.2 Variable Definitions and Summary Statistics

Our main dependent variable is *Dome. GC to asset (%)*, defined as the ratio of the total value of domestic green contracts participated by firm *i* in year *t* to its total assets in year *t-1*⁵. We use total assets as the denominator because it is more stable than sales and less sensitive to short-run revenue volatility. To assess robustness, we construct four alternative measures: *Dome. GC to sale (%)*, *Dome. GC to ave sale (%)*, *Dome. GC to ave asset (%)*, and *lnContract*, the natural logarithm of total green contract value in million USD plus one.

Our key independent variable is *Post*, an indicator equal to one for firm-year observations in or after the year in which the firm's country implements mandatory ESG disclosure, and zero otherwise.⁶ Firm-level controls include *Size* (the natural logarithm of total assets in million USD), *Leverage*, *ROA*, *MTB*, *Cash*, *Sale growth*, *R&D* intensity (with missing values set to zero), and *Capx* (the natural logarithm of capital expenditure per employee in thousand USD plus one, i.e. Compustat item capx divided by Compustat item emp). For missing total assets, we forward-fill from the most recent available observation. For missing book value of equity (Compustat item teq), we apply the same method of Compustat reconstruction using common equity (ceq) plus preferred stock (pstk) plus nonredeemable noncontrolling interests (mibn). The missing value of pstk and mibn is set to 0. In calculation of market value, we use

⁵ We compare the tender country (the ISO country code where the work is performed) in green contract data and fic (the country where the firm is incorporated) to distinguish the domestic and foreign green contract. The Factset database only provides the location where the green contract is performed while do not provide the location of the tender entity.

⁶ For example, if firm *i* received reform in year 2009, its post term in year 2009 and after year 2009 is 1, otherwise is 0. And the dependent variable for firm *i* in year 2009 is matched to its firm characteristic (control variable) in 2008. One concern of our setting is that the implementation of the reform may only cover part of year 2009. Therefore, in Appendix D, we set the reform effect year as the second year of the actual reform year, and perform stagger DID and stack DID to check the robustness of our setting.

Compustat item: *mkvalt* as the proxy for market value of the firm in Compustat/CRSP database. For the firms in Compustat Global, we focus on its primary issue stock based on Compustat item: *prirow* (i.e. we focus on the stock whose *iid* is equal to *prirow* provided by fundamental file). We first calculate the monthly average market value in USD of the primary stock. We then use the latest month value before the fiscal year ending as the proxy of market value in USD of the firm. Finally, we convert the market value back to the native currency and calculate MTB. Country-level controls include the unemployment rate (*Unemp*), the inflation rate (*Inflat*), the GDP per capita growth rate (*GDP growth*), and per-capita carbon intensity (*Carbon intensity*). Variable definitions are summarized in Appendix A. Continuous firm-level variables (except for the variables in logarithms) are winsorized at the 1st and 99th percentiles. Because the cross-sectional distribution of green contract awards is highly skewed across industries and years, we winsorize the dependent variables at the 1st and 99th percentiles by industry-year (two-digit SIC).

Table 1 reports summary statistics for the firm-level and country-level variables. Table 2 reports the correlation matrix of the main variables. Country- and year-level descriptive statistics are reported in Appendix B, and a comparison of pre-event characteristics for contractors versus non-contractors is reported in Appendix C.

[Insert Table 1 here]

The mean of *Dome. GC to asset (%)* is 0.034, indicating that a firm receives green contracts worth on average about 0.034 percent of its total assets in a given year. The 25th, 50th, and 75th percentiles are all zero, reflecting the highly concentrated nature of green procurement, in which only a minority of firms receive contracts in any given year. The mean of *Post* is 0.506,

indicating an approximately balanced split between treated and untreated firm-years. The standard deviations of the remaining controls are within standard ranges.

[Insert Table 2 here]

The correlations between control variables and the treatment indicator are modest, mitigating concerns about multicollinearity in the baseline regressions.

3.3 Baseline Results

We estimate the effect of mandatory ESG disclosure on firm-level green contract receipt using both static and dynamic staggered difference-in-differences specifications. The baseline equation is

$$Dome.GC\ to\ asset(\%)_{i,t} = \beta_0 + \beta_1 Post_{i,t} + \gamma' Controls_{i,t-1} + \mu_t * \delta_s + \theta_i + \varepsilon_{i,t}, \quad (1)$$

where $Dome.GC\ to\ asset(\%)_{i,t}$ represents the ratio of total amount of domestic green contract received by the firm in the fiscal year t+1 to the firms' total asset of the fiscal year t. $Post_{i,t}$ takes value of 1 if the firm i in the countries that implemented ESG mandatory disclosure and takes value of 0 otherwise. $Controls_{i,t}$ is the vector of control variables for firm i in year t. For fixed effects, we control for Industry*Year FE ($\mu_t * \delta_s$) and firm FE (θ_i). $\varepsilon_{i,t}$ denotes the unobservable error term. The industry FEs is denoted by 2-digit SIC code. We cluster the standard error at country level.

[Insert Table 3 here]

Table 3 Panel A reports the static DID estimates. Across all specifications, the coefficient on *Post* is positive and statistically significant at the 5 percent level. In our preferred specification (column 3), the estimated coefficient is 0.091. Relative to the unconditional sample mean of 0.034, the magnitude implies a roughly twofold increase in firm-level green

contract intensity following the mandate, indicating that the result is both statistically and economically meaningful.

This baseline finding admits two non-mutually-exclusive interpretations. The first is that the government expands the total volume of green contracting once disclosure mandates lower the cost of identifying genuinely green suppliers, allocating more contracts to the listed-firm universe overall. The second, which the theoretical framework in Sections 1 and 2 develops as our central hypothesis, is that the government does not enlarge total green spending but reorganizes its allocation. Once disclosure reduces information asymmetry across the universe of firms, procurement officials gain the latitude to extend contracts to a wider supplier base rather than concentrate them within the narrow prior procurement perimeter. Under this interpretation, the firm-level increase reflects redistribution under a fixed budget rather than expansion of the budget itself. We adjudicate between these interpretations directly in Section 4.

Among firm-level controls, *Size* loads negatively, indicating that larger firms receive a smaller share of contracts relative to their asset base. This pattern is consistent with the distinctive logic of green procurement, which prioritizes environmental and social outcomes such as emission reduction over price and scale (Varnas et al., 2009). Larger firms therefore do not enjoy the same scale advantage in green contracting that they typically enjoy in conventional procurement.

Panel B of Table 3 reports the dynamic event-study specification. The pre-treatment indicators $Post(-2)$ and $Post(-1)$ are statistically indistinguishable from zero, supporting the parallel-trends assumption. The post-treatment indicators $Post(1)$ and $Post(2+)$ are

significantly positive, confirming that the firm-level expansion is driven by the mandate rather than by pre-existing trends. The contemporaneous coefficient $Post(0)$ is mildly negative in column 3, consistent with a short-run reallocation effect in which the government re-screens its supplier pool around the implementation year before redirecting contracts to newly visible firms in subsequent periods.

3.4 Identification: Stacked DID with Propensity-Score Matching

A potential concern with the static staggered DID estimator is the “bad comparison” problem (Baker et al., 2022), which arises when already-treated units are used as controls for later-treated units in the presence of treatment-effect heterogeneity over time. To address this concern, we re-estimate the baseline using the stacked DID approach of Gormley and Matsa (2011) and Gormley and Matsa (2016). For each treatment cohort (an event year)⁷, we construct a clean control group composed of never-treated and not-yet-treated units within a $[-3, +3]$ year window around the reform year. Within each cohort window, $Treat$ equals one for treated units and zero otherwise, and $Post$ equals one for years on or after the reform date and zero otherwise. Within each cohort window we apply propensity-score matching (PSM) on the full set of control variables to pair each treated firm with a clean-control firm with the closest propensity score⁸. We then stack the matched cohort samples and identify each subsample with a cohort indicator. Firm-by-cohort and industry-by-year-by-cohort fixed effects are included. Table 4 reports the stacked DID estimates.

[Insert Table 4 here]

⁷ Since our sample starts from 2009, we only focus on the reform implemented on or after 2010.

⁸ Since our covariate set contains country level variable, which makes the perfect prediction in some cohort and cannot generate the PSM score. We drop this cohort before stacking them together. In robustness check, we only utilize the firm level controls as the covariates to solve the perfect prediction problem.

Across specifications without controls, with firm-level controls only, and with the full set of controls, the coefficient on $Treat \times Post$ is positive and significant at the 5 percent level. The point estimates in all columns are still significantly positive, indicating that the baseline result is robust to the bad-comparison problem inherent in conventional staggered DID designs.

3.5 Heterogeneity Across Institutions and Firm Types

Building on the firm-level baseline, we examine cross-sectional heterogeneity along three dimensions theorized in Section 2: the legal-origin and macroeconomic context, the policy design of the disclosure reform, the diversity of government, and the prior information environment in the country. Table 5 reports interactions of *Post* with country- and event-level characteristics. *Civil law* equals one for civil-law countries and zero for common-law countries (La Porta et al., 1998), with China classified as civil law. *Gov. diversity* is defined as the “The probability that two deputies picked at random from among the government parties will be of different parties” for each country-year, which is provided by Database of Political Institutions. The higher the probability, the more diverse of the government. *Vol. ESG dis.* equals one for countries that promoted voluntary ESG disclosure prior to the mandate (Krueger et al., 2024). *Gov. domain* equals one if the reform is administered directly by the government, and *Strong reform* equals one if compliance is mandatory rather than “comply-or-explain”, both following Krueger et al. (2024). For the reform level indicators, we assign 0 to the countries without ESG mandatory disclosure. *GDP growth* captures macroeconomic conditions at the country-year level.

[Insert Table 5 here]

The first four columns of Panel A show that the treatment effect attenuates in countries with prior voluntary ESG disclosure regimes, in civil-law countries, in the countries with more diverse government, and in countries with higher GDP growth. The voluntary-disclosure result confirms an information channel. Where governments already had informal access to ESG information, the marginal informational gain from a mandate is smaller, consistent with Krueger et al. (2024). The civil-law result aligns with the comparative-institutions argument that common-law systems privilege public information and market-based contracting, providing institutional complementarities that reinforce the procurement effects of mandatory disclosure (Beekes et al., 2016, La Porta et al., 1998). In civil-law systems, weaker investor protection and stakeholder-mediated coordination dilute the verifiability of ESG disclosures and attenuate their procurement consequences. In countries with more diverse governments, the administration efficiency may be lower in the implementation of ESG mandatory disclosure since the diverse government may create more internal interest conflict. Also, the diverse government tends to respect the opinions from different parties, including the opinion from the anti-climate/environment association. Thus, the implementation intensity of the countries with diverse government is attenuated. The GDP-growth result is consistent with green procurement functioning in part as a counter-cyclical fiscal instrument. In slower-growing economies, governments are more willing to use green procurement as a policy lever for both environmental and stabilization objectives, amplifying the effect of disclosure mandates on contract reallocation.

The last two columns of Panel A show that the effect is amplified when the mandate is administered directly by the government and when compliance is mandatory rather than

“comply-or-explain”, consistent with the broader finding that enforcement intensity is a first-order determinant of whether disclosure regulation alters bureaucratic and firm behavior (Krueger et al., 2024).

3.6 Robustness

We conduct a battery of robustness tests reported in Tables 6 and 7. Table 6 examines alternative dependent variables, an alternative weighting scheme, and an extended set of controls. Table 7 reports estimates using the heterogeneity-robust DID estimator of Callaway and Sant’Anna (2021).

[Insert Table 6 here]

Panel A of Table 6 replaces the dependent variable with *Dome. GC to sale (%)*, *Dome. GC to ave sale (%)*, *Dome. GC to ave asset (%)*, and *lnDomeGC* (the natural logarithm of domestic tender value in USD plus one). The coefficient on *Post* remains positive and significant across all alternatives. Panel B re-estimates the baseline using entropy balancing as an alternative to PSM. Entropy balancing reweights control observations to match the moments of the treated distribution rather than dropping unmatched units, preserving sample size. The treatment effect estimated on the entropy-balanced sample remains significant at the 5 percent level. Panel C augments the controls with four additional variables that capture firm-level information visibility and country-level climate salience. *ESG cover* is an indicator for Refinitiv coverage, *Prisk_envir* is firm-level environmental policy risk from Hassan et al. (2019), *Env_expen_prop* is the share of environmental protection in GDP, and *Disaster_num* is the number of climate-related disasters, with the latter two from the IMF Macroeconomic Climate Indicators

Dashboard. The treatment effect remains significant after these additions, indicating that the result is not driven by changes in firm-level information visibility or by shifts in country-level climate exposure. In panel D, we re-perform the PSM-stack DID regression utilizing only the firm level control variables as covariates in calculating PSM score. Under the different combination of fixed effects, the coefficients of *Treat*Post* are still significantly positive.

[Insert Table 7 here]

Table 7 reports results from the Callaway and Sant’Anna (2021) DID estimator (CSDID), which is robust to staggered adoption with heterogeneous treatment effects. Whether we include only firm-level controls or the full set of controls, the estimated ATT is positive and statistically significant.

Together, the robustness tests confirm that the baseline finding of a firm-level increase in green contract receipt following mandatory ESG disclosure is not an artifact of variable construction, sample weighting, omitted controls, or estimator choice.

4. Additional Analyses about Contract Reallocation, Efficiency, and Innovation

The firm-level results in Section 3 show that mandatory ESG disclosure raises the green contract value received by affected firms, with the effect concentrated among firms that were previously outside the government’s information set. A natural question follows. Does this firm-level increase reflect an expansion of the government’s total green procurement budget, or a redistribution of a fixed budget across a broader supplier base? This section answers that question and traces the downstream consequences of the answer for environmental and innovation outcomes.

4.1 Reallocation of Green Procurement Within a Country

We decompose the firm-level effect into its extensive and intensive components. The extensive margin captures whether more firms win at least one green contract under the new transparency regime. The intensive margin captures whether the government enlarges the total green procurement budget directed to listed firms. If the firm-level effect operates through the extensive margin, we should observe an increase in the probability that any given firm wins a contract.

We test the extensive margin at the firm level using a linear probability model in which the dependent variable is *Dummy(new GC)*, an indicator equal to one if firm *i* wins a green contract in year *t*. Also, we create two additional dummies for the firm won or not the green contract in the previous year to test the heterogeneous impact on the pre-contractor and new contractor. *Dummy(initiate GC)* is only defined for the firm did not receive green contract in the previous year, and it takes value of 1 the firm received green contract in this year and takes value of 0 otherwise. *Dummy(continue GC)* is only defined for the firm who received green contract in previous year, and it takes value of 1 for the firm continues to receive contract in this year and takes value of 0 otherwise. The specification includes the same controls and fixed effects (industry-by-year and firm) as the baseline. Panel A of Table 8 reports the results. The coefficient on *Post* is 0.008 and statistically significant at the 1 percent level. Relative to the unconditional sample mean of approximately 0.008, this represents a roughly twofold increase in the probability of winning a green contract. In columns 2 and 3, we find that both the probability of obtaining green contracts for pre-contractor or new contractor increase significantly. Although the absolute value of the coefficient is larger in column 3, given that

the mean value of *Dummy(initial GC)* and *Dummy(continue GC)* are 0.003 and 0.646, the economic magnitude is more pronounced for the new contractor. For the new contractor, the probability of obtaining increases about 1 time. While for the existing contractor, the probability increases by about 0.40. The new contractor benefits more from the ESG mandatory disclosure. The extensive margin therefore expands meaningfully after the mandate.

We further test the extensive margin at the country level by computing *Public GC HHI* as the Herfindahl-Hirschman index of contract shares across listed awardees in each country-year. For each country-year we calculate each listed awardee's share of the total green contract value awarded to listed firms, square the shares, and sum them⁹. Higher values indicate more concentrated allocation. Columns 1 and 2 of Panel B show that *Public GC HHI* declines significantly after the mandate. This pattern provides direct evidence that the same fiscal envelope is being divided across a broader set of firms.

[Insert Table 8 here]

Together, the firm-level extensive-margin expansion and the falling concentration index identify a *democratization mechanism*. Mandatory ESG disclosure reallocates that budget across a wider supplier base by lowering the information frictions that previously concentrated awards within a narrow procurement perimeter. This is the central pathway through which a corporate accounting reform produces fiscal-allocation consequences at the country level.

4.2 Real Effects on Firms' Carbon Emissions

Utilizing Trucost database from 2002 to 2021, we discuss the real impact of ESG mandatory disclosure on firm level carbon emission. We focus on the scope 1 (*CO2(scope1)*)

⁹ For the co-awarding contract, we assume that the contractors take the equal weight in the tender.

and scope 2 ($CO_2(scope2)$) emissions of the firm, which is defined as the “Greenhouse gas (GHG) emissions from sources that are owned or controlled by the company” (scope 1) and “Greenhouse gas (GHG) emissions from consumption of purchased electricity, heat or steam by the company” (scope2). Despite these two variables, we also employ the carbon emission intensity of the two scopes ($CO_2\ int.\ scope1$ and $CO_2\ int.\ scope2$), which are the scope 1 and scope 2 emission divided by the company's revenue and directly provided by Trucost¹⁰. In this test, we utilize the same method of PSM-stack DID in Table 5. The result is listed below.

[Insert Table 9 here]

As shown in Table 9, the coefficients of $Treat*Post$ are significantly negative in most of the columns. The first two column shows that the treated firms reduce their carbon emission after the ESG mandatory disclosure after the event, and the effect is slightly pronounced in scope 2 emission. The last two columns show that less emission is not driven by the reducing of revenue, while is driven by the increasing producing efficiency (lower carbon intensity), indicating the reform of production technologies. Overall, our result reveals the real impact of ESG mandatory disclosure in terms of carbon emissions.

4.3 Real Effects on Green Innovation

Beyond the fiscal reallocation of public procurement, an important question for international business policy is whether mandatory ESG disclosure produces real technological consequences. Recent IB research emphasizes that public procurement can function as a

¹⁰ Since the Trucost Environmental file provides the institution-year level measurement of carbon emission, while the institution and firm are not necessarily 1-to-1 corresponded. In the raw data of Trucost, there are about 1.5% observations with 1 institution corresponded to multiple firms, and about 0.06% observations with 1 firm corresponded to multiple institutions. Therefore, for the firms with multiple institutions (i.e. one gvkey corresponds to multiple institution in a year), we aggregate the carbon emission of its institutions as the emission of the firm and aggregate the revenue (in million USD) as the revenue of the firm. We then divided the scope 1 and 2 emission by the firm level revenue to obtain the carbon intensity of the two scopes. For the firm with one institution in a year, we directly use the carbon emission (DI_319413 and DI_319414) and intensity (DI_319407 and DI_319408) of two scope provided by Trucost.

demand-side industrial policy that stimulates corporate R&D and creates lead markets for sustainable technologies (Belenzon and Cioaca, 2025, Van Assche and De Marchi, 2024). If mandatory disclosure reallocates green contracts toward firms that were previously outside the procurement perimeter, and if these firms convert the guaranteed demand into investment in green technology, we should observe an increase in green innovation for treated firms relative to matched controls.

We test this proposition using the PATSTAT Global 2024 database, which provides harmonized cross-country patent records. The dependent variable is *Green inv. Eff.* and *Non-green inv. Eff.*, green patent number or non-green patent number scaled by the weighted sum of R&D (in million USD). The weight of lagged 1 to lagged 5 R&D is 1, 0.8, 0.6, 0.4, and 0.2, which is similar to the setting of Hirshleifer et al. (2013). To isolate the innovation response to the contract-allocation channel, we focus on firms that had not secured any green contract in the two years prior to the disclosure mandate (years $t-3$ to $t-1$) and that obtained a green contract in the year following the event (year t). *Reform_GC* is a dummy variable equal to one for firms that initiate green contracts in the year following the reform and zero for control firms in the same country. We require that both of the treated and control firms should be treated by ESG mandatory disclosure in year $t-1$. This design mitigates reverse-causality concerns, because the award of the green contract is itself attributable to the disclosure shock rather than to pre-existing firm characteristics that would directly confound the patent outcome.

The stacked sample is constructed as follows. (i) For the next year of each reform year (i.e. we set year t as the next year of the reform and the reform took place in $t-1$), we build a cohort covering $[t-3, t+5]$ and retain only firms affected by the mandate in year $t-1$. (ii) We drop firms

that received a green contract in $[t-3, t-1]$, because such awards cannot be attributed to the reform. (iii) Firms that receive a green contract in year t form the treatment group, and the remaining firms form the candidate control pool. (iv) Using firm characteristics in year $t-2$ (i.e. one year before the reform), we perform radius propensity-score matching with a caliper of 0.01 to identify matched controls. (v) The *Post* indicator equals one starting in year t . (vi) We stack cohorts and estimate a stacked DID with firm-by-cohort and industry-by-year-by-cohort fixed effects. Control variables are lagged by one year, and the green patent measure is winsorized by year and industry at the 1st and 99th percentiles.

[Insert Table 10 here]

Table 10 reports the results. In column 1, the coefficient on $Reform_GC \times Post$ is 0.058($t = 2.42$), statistically significant at the 5 percent level. Column 2 reports the parallel test on ordinary (non-green) innovation efficiency. The estimated coefficient is significantly negative (-0.201) ($t = -3.59$), indicating that treated firms expand their green innovation and crowd out their other innovative activity.

These findings are consistent with the interpretation that mandatory disclosure catalyzes a demand-pull for green technology (Belenzon and Cioaca, 2025, Van Assche and De Marchi, 2024). Transparency reallocates public demand toward a broader set of firms, and the guaranteed demand from the state, acting as a strategic buyer, is translated into greater investment in environmental innovation by the new entrants. Combined with the declining of non-green innovation efficiency, we can conclude that the green contract shifts the innovation direction of the firm.

5. Conclusion

This paper studies how an exogenous improvement in firm-level information transparency reshapes the way the state, acting as a strategic buyer, allocates green public procurement. We document a robust firm-level increase in green contract receipt following a disclosure mandate. The result is statistically and economically significant, survives a battery of identification refinements including stacked DID with propensity-score matching, entropy balancing, alternative dependent variables, and the Callaway and Sant’Anna (2021) estimator, and shows no evidence of differential pre-trends in event-study analysis.

We also find that the results is amplified in common-law jurisdictions, in countries with lower GDP growth, in reforms administered directly by the government, and in mandates structured around hard compliance rather than “comply-or-explain”. The effect attenuates where prior voluntary disclosure regimes already supplied an informational channel. The effect is attenuated if the government is more diverse. Together, these patterns identify a clear information-channel mechanism rather than a mechanical expansion of incumbent suppliers.

The country-level evidence then identifies the central mechanism we develop theoretically. Mandatory ESG disclosure redistributes the same fiscal envelope across a broader supplier base, raising the probability that any given firm wins a contract and lowering the Herfindahl-Hirschman concentration of awards. We label this pattern a *democratization* of green public procurement. The additional analysis reveals that the ESG mandatory disclosure indeed reduce the emissions of the firm. Both the absolute emission and the intensity of emissions significantly decrease after the event.

We further document that treated firms expand their green innovation with crowding out their other innovative activity, providing direct evidence that the reallocation of public demand toward newly transparent firms catalyzes a demand-pull for environmental technology. In addition, the procedure is aligned with the shift of innovation pattern.

Taken together, our results show that mandatory ESG disclosure is not merely an instrument of corporate accountability. It functions as an institutional reform that reaches into the public sector, restructures the allocation of fiscal resources, and supports more inclusive and more efficient pursuit of sustainability through the global procurement system.¹¹

¹¹ Our study has limitations that point to fruitful directions for future research. First, our sample focuses on listed firms, while a substantial share of green procurement is awarded to private SMEs that lie outside Compustat. Extending the analysis to private suppliers would help quantify the full magnitude of the democratization effect. Second, our innovation evidence captures the patent-application response, but does not directly trace the diffusion or commercialization of these green technologies. Linking procurement reallocation to downstream technology adoption and emissions outcomes at the project level would deepen the causal chain from disclosure to environmental impact. Third, while we document the cross-border informational value of disclosure mandates indirectly through the global heterogeneity of effects, future work could examine whether home-country mandates differentially reshape multinational firms' allocation decisions across host markets, building on Kim, Sun, Xiang, and Zeng (2026). Finally, as more jurisdictions adopt mandatory sustainability reporting under the ISSB framework, the procurement consequences of converged global disclosure standards represent a natural extension of our analysis.

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Table 1: Summary statistics

This table presents summary statistics of the variables. Variable definitions are provided in Appendix A. We drop observations from the US and from countries with fewer than 100 firm-year observations during the sample period. We also drop firms in the financial and utility industries. Continuous variables (except for those in logarithms) are winsorized at the top and bottom 1 percentiles.

<i>Var.</i>	(1) N	(2) Mean	(3) S.D	(4) P25	(5) P50	(6) P75
Firm's features						
<i>Dome. GC to sale(%)</i>	128,260	0.030	1.499	0.000	0.000	0.000
<i>Dome. GC to asset(%)</i>	128,260	0.034	1.660	0.000	0.000	0.000
<i>Dome. GC to ave asset(%)</i>	128,260	0.033	1.624	0.000	0.000	0.000
<i>Dome. GC to ave sale(%)</i>	128,260	0.028	1.379	0.000	0.000	0.000
<i>lnLocalGC</i>	128,260	0.122	1.415	0.000	0.000	0.000
<i>Dummy(new contract)</i>	128,260	0.008	0.088	0.000	0.000	0.000
<i>Leverage</i>	128,260	0.475	0.204	0.318	0.481	0.628
<i>Size</i>	128,260	5.789	1.994	4.455	5.714	7.041
<i>ROA</i>	128,260	0.082	0.102	0.045	0.084	0.129
<i>MTB</i>	128,260	2.181	2.659	0.712	1.298	2.524
<i>Cash</i>	128,260	0.152	0.143	0.048	0.110	0.208
<i>Sale growth</i>	128,260	0.104	0.364	-0.041	0.049	0.165
<i>Capx</i>	128,260	2.162	1.265	1.251	2.056	2.887
<i>R&D</i>	128,260	0.016	0.036	0.000	0.000	0.016
Country's features						
<i>Post</i>	128,260	0.506	0.500	0.000	1.000	1.000
<i>Unemp</i>	128,260	5.688	3.606	3.657	4.909	6.510
<i>Inflat</i>	128,260	2.068	2.664	0.469	1.509	2.853
<i>GDP growth</i>	128,260	1.588	3.727	0.209	1.537	3.752
<i>Carbon intensity</i>	128,260	7.277	3.444	4.935	8.250	9.530

Table 2: Correlation matrix

This table presents the correlation matrix. *Dome_GC to asset (%)* is the proportion of the green contract to lagged total assets for each firm-year observation. *Post* takes a value of 1 after the implementation of ESG mandatory disclosure and 0 otherwise. All control variables are lagged by one year. Variable definitions are provided in Appendix A.

	<i>Dome_GC to asset(%)</i>	<i>Post</i>	<i>Size</i>	<i>Leverage</i>	<i>ROA</i>	<i>MTB</i>	<i>Cash</i>	<i>R&D</i>	<i>Capx</i>	<i>Sale growth</i>	<i>GDP growth</i>	<i>Inflat</i>	<i>Unemp</i>
<i>Post</i>	0.016	1											
<i>Size</i>	0.007	-0.078	1										
<i>Leverage</i>	0.021	-0.02	0.237	1									
<i>ROA</i>	-0.001	-0.037	0.211	-0.005	1								
<i>MTB</i>	-0.004	0.171	-0.094	0.063	0.097	1							
<i>Cash</i>	-0.002	-0.133	-0.18	-0.326	-0.111	0.198	1						
<i>R&D</i>	-0.009	0.02	-0.088	-0.139	-0.257	0.204	0.26	1					
<i>Capx</i>	-0.015	-0.01	0.394	0.026	0.085	-0.033	-0.129	-0.045	1				
<i>Sale growth</i>	-0.002	0.092	-0.054	-0.042	0.055	0.13	0.044	0.041	0.068	1			
<i>GDP growth</i>	0	0.259	0.001	-0.051	0.043	0.064	-0.051	-0.019	0.013	0.177	1		
<i>Inflat</i>	-0.003	0.23	-0.159	0.008	0.099	0.026	-0.205	-0.107	-0.064	0.112	0.212	1	
<i>Unemp</i>	0.001	0.279	-0.019	0.083	0.025	0.019	-0.134	-0.009	-0.021	0.024	-0.084	0.194	1
<i>Carbon emission</i>	-0.002	-0.216	0.18	-0.034	-0.086	-0.045	0.211	0.097	0.158	-0.03	-0.094	-0.438	-0.148

Table 3: Baseline results and dynamic impact

This table presents the staggered difference-in-differences (DID) results. The dependent variable, *Dome. GC to asset (%)*, is the value of the domestic green contract received by a firm scaled by lagged total asset. Panel A reports the regression results of the static staggered DID. The independent variable is *Post*, which takes a value of 1 on or after the implementation of ESG mandatory disclosure and 0 otherwise. Panel B presents the dynamic analysis for the staggered DID. *Post(i)(-k)* takes a value of 1 if the observation is in the k-th year relative to the country's ESG disclosure reform year (i.e., year 0) and 0 otherwise. *Post(2+)* takes a value of 1 for observations in or after the second year following the country's ESG disclosure reform year and 0 otherwise. All control variables are lagged by one year. The t-values of the coefficients are presented in brackets. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. Standard errors are clustered at the country level. Variable definitions are provided in Appendix A.

Panel A: Staggered difference-in-differences analysis

<i>Dep. Var.</i>	(1)	(2)	(3)
	<i>Dome. GC to asset(%) (t+1)</i>		
<i>Post</i>	0.094** (2.26)	0.095** (2.26)	0.091** (2.53)
<i>Size</i>		-0.017* (-1.74)	-0.018 (-1.62)
<i>Leverage</i>		0.059 (0.79)	0.060 (0.81)
<i>ROA</i>		0.091 (1.40)	0.091 (1.38)
<i>MTB</i>		0.000 (0.08)	0.000 (0.00)
<i>Cash</i>		0.020 (1.54)	0.011 (1.10)
<i>R&D</i>		0.008 (0.19)	0.016 (0.38)
<i>Capx</i>		0.006 (1.18)	0.005 (1.12)
<i>Sale growth</i>		0.004 (0.77)	0.005 (1.01)
<i>GDP growth</i>			-0.000 (-0.45)
<i>Inflat</i>			-0.003 (-1.00)
<i>Unemp</i>			-0.011* (-1.70)
<i>Carbon intensity</i>			-0.015 (-1.02)
<i>Constant</i>	-0.013 (-0.64)	0.033 (0.99)	0.227* (1.70)

Observations	128,260	128,260	128,260
R-squared	0.380	0.380	0.380
Firm FEs	Yes	Yes	Yes
Industry*Year FEs	Yes	Yes	Yes

Panel B: Dynamic staggered difference-in-differences analysis

	(1)	(2)	(3)
<i>Dep. Var.</i>	<i>Dome. GC to asset(%)$(t+1)$</i>		
<i>Post(-2)</i>	0.012 (1.00)	0.011 (0.97)	0.002 (0.24)
<i>Post(-1)</i>	-0.026 (-1.07)	-0.027 (-1.10)	-0.030 (-1.55)
<i>Post(0)</i>	-0.051 (-1.43)	-0.052 (-1.45)	-0.052* (-1.74)
<i>Post(1)</i>	0.059* (1.91)	0.059* (1.91)	0.053* (1.82)
<i>Post(2+)</i>	0.141** (2.17)	0.143** (2.18)	0.135** (2.36)
Controls	No	Firm only	All
Observations	128,260	128,260	128,260
R-squared	0.380	0.380	0.380
Firm FEs	Yes	Yes	Yes
Industry*Year FEs	Yes	Yes	Yes

Table 4: Stacked DID analysis based on a matched sample

This table presents the stacked difference-in-differences (DID) results based on a matched sample. For each treated firm, we find one control firm from a pool of not-yet-treated and never-treated firms based on propensity score matching with a caliper of 0.005. Treated firms are classified into eight cohorts based on their ESG reform years. For the treated and control firms in each cohort, we keep observations within the $[-3, +3]$ year window relative to the ESG reform year (i.e., year 0). *Treat* equals one for treated firms and zero otherwise. *Post* equals one for observations after the ESG disclosure reform in each cohort and zero otherwise. We present the stacked DID regression results based on the matched sample. The *Treat* and *Post* variables are absorbed by the fixed effects. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. We cluster standard errors at the firm level to be robust to heteroscedasticity. Variable definitions are provided in Appendix A.

<i>Dep. Var.</i>	(1)	(2)	(3)	(4)
		<i>Dome. GC to asset(%)$(t+1)$</i>		
<i>Treat*Post</i>	0.065**	0.062**	0.069***	0.068***
	(2.62)	(2.55)	(2.80)	(2.87)
Controls	No	Yes	No	Yes
Observations	15,811	15,811	16,134	16,134
R-squared	0.472	0.472	0.421	0.421
Firm*Cohort FEs	Yes	Yes	Yes	Yes
Year*Cohort FEs	No	No	Yes	Yes
Industry*Year*Cohort FEs	Yes	Yes	No	No

Table 5: Heterogeneity analysis

This table presents five interaction effects of various country-level characteristics. The dependent variable is the proportion of the domestic green contract to lagged total assets (*Dome. GC to asset (%)*) for each firm-year observation. *Vol. ESG dis.* takes a value of 1 if the country had implemented voluntary ESG disclosure before the mandatory ESG disclosure event and 0 otherwise. *GDP growth* is the lagged GDP per capita growth rate (in percentage). *Civil law* takes a value of 1 if the country has a civil law system and 0 for common-law countries, based on La Porta et al. (1998), with China added as a civil law country as a supplement. *Gov. diversity* is defined as the one-year-lagged "probability that two deputies picked at random from among the government parties will be of different parties" for each country-year, as provided directly by the Database of Political Institutions. *Gov. domain* takes a value of 1 if the reform is conducted by the government and 0 otherwise. *Strong reform* takes a value of 1 if the ESG mandatory disclosure is fully compliant and 0 otherwise. For the reform-level variables, we assign a value of 0 to countries without ESG mandatory disclosure. All control variables are lagged by one year relative to the dependent variable. The brackets contain the t-values of the coefficients. We cluster standard errors at the country level to be robust to heteroscedasticity. Variable definitions are provided in Appendix A.

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Dep. Var.</i>	<i>Dome. GC to asset(%)_(t+1)</i>					
<i>Post*Vol. ESG dis</i>	-0.109** (-2.56)					
<i>Post*Civi law</i>		-0.149*** (-3.21)				
<i>Post*GDP growth</i>			-0.003*** (-2.68)			
<i>Post*Gov. diversity</i>				-0.221** (-2.59)		
<i>Post*Gov. domain</i>					0.069* (1.93)	
<i>Post*Strong reform</i>						0.142*** (2.80)
<i>Gov. diversity</i>				0.153** (2.48)		
<i>Post</i>	0.139*** (3.02)	0.166*** (3.65)	0.096** (2.64)	0.179*** (3.01)	0.029 (1.17)	0.015 (0.87)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Moderating Var.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	128,260	128,260	128,260	108,149	128,260	128,260
R-squared	0.380	0.380	0.380	0.393	0.380	0.380
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FEs	Yes	Yes	Yes	Yes	Yes	Yes

Table 6: Robustness tests

This table presents robustness tests. In Panel A, we use the proportion of domestic green contracts to lagged total sales (*Dome. GC to sale (%)*), the proportion of domestic green contracts to the average sales of the current year and the previous year (*Dome. GC to ave sale (%)*), the proportion of domestic green contracts to the average assets of the current year and the previous year (*Dome. GC to ave asset (%)*), and the logarithm of domestic tender value (in USD) plus one (*lnDomeGC*) as alternative dependent variables. In Panel B, we run the regression based on a sample adjusted by the entropy balancing method. In Panel C, we add two additional firm-level and two additional country-level control variables to our regression. *ESG cover* is a dummy that takes a value of 1 if the firm is covered by the Refinitiv ESG database and 0 otherwise. *Prisk_envir* represents the political risk related to environmental policy disclosed by firms, as calculated by Hassan et al. (2019). *Env_expen_prop* is the percentage of environmental protection-related expenditure to total GDP. *Disaster_num* is the number of climate-related disasters for each country-year. These two indicators capture public concern about climate issues, which may affect the probability of implementing ESG mandatory disclosure. They come from the Macroeconomic Climate Indicators Dashboard of the IMF. In Panel D, we re-perform the stacked DID using only firm-level controls as covariates in calculating the PSM score. All control variables are lagged by one year. The t-values of the coefficients are presented in brackets. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. Standard errors are clustered at the country level. Variable definitions are provided in Appendix A.

Panel A: Using alternative dependent variables

<i>Dep. Var.</i>	(1) <i>Dome. GC to sale(%) (t+1)</i>	(2) <i>Dome. GC to ave sale(%) (t+1)</i>	(3) <i>Dome. GC to ave asset(%) (t+1)</i>	(4) <i>lnLocalGC (t+1)</i>
<i>Post</i>	0.090** (2.47)	0.082** (2.54)	0.088** (2.54)	0.113** (2.08)
<i>Size</i>	-0.017 (-1.54)	-0.016 (-1.55)	-0.018 (-1.61)	-0.007 (-0.58)
<i>Leverage</i>	0.035 (0.55)	0.033 (0.53)	0.065 (0.87)	0.039 (1.15)
<i>ROA</i>	0.069 (1.42)	0.073 (1.48)	0.090 (1.34)	0.001 (0.04)
<i>MTB</i>	0.000 (0.13)	0.000 (0.13)	-0.000 (-0.09)	-0.002 (-1.24)
<i>Cash</i>	0.020 (1.52)	0.000 (0.03)	0.009 (0.76)	-0.062 (-1.58)
<i>R&D</i>	0.004 (0.10)	0.006 (0.20)	0.015 (0.37)	0.016 (0.21)
<i>Capx</i>	0.007 (1.13)	0.006 (1.11)	0.005 (1.12)	0.001 (0.18)
<i>Sale growth</i>	0.003 (1.23)	0.003 (1.13)	0.005 (0.94)	0.008 (1.18)
<i>GDP growth</i>	-0.001 (-1.03)	-0.001 (-0.65)	-0.000 (-0.16)	-0.001 (-0.27)
<i>Inflat</i>	-0.002 (-0.78)	-0.002 (-1.12)	-0.003 (-1.03)	0.003 (0.97)
<i>Unemp</i>	-0.010 (-1.63)	-0.008 (-1.61)	-0.011* (-1.70)	0.008 (0.90)
<i>Carbon intensity</i>	-0.012 (-0.92)	-0.011 (-0.97)	-0.015 (-1.02)	-0.030** (-2.01)
<i>Constant</i>	0.186 (1.63)	0.182* (1.74)	0.223 (1.66)	0.269** (2.25)
Observations	128,260	128,260	128,260	128,260
R-squared	0.343	0.344	0.371	0.612
Firm FEs	Yes	Yes	Yes	Yes
Industry*Year FEs	Yes	Yes	Yes	Yes

Panel B: Regression based on the entropy balanced sample

<i>Dep. Var.</i>	(1)	(2)	(3)
	<i>Dome. GC to asset(%) (t+1)</i>		
<i>Post</i>	0.069** (2.34)	0.070** (2.33)	0.067** (2.43)
<i>Size</i>		-0.017* (-1.68)	-0.016 (-1.54)
<i>Leverage</i>		0.032 (0.59)	0.034 (0.62)
<i>ROA</i>		0.108* (1.95)	0.104* (1.86)
<i>MTB</i>		0.001 (0.69)	0.001 (0.58)
<i>Cash</i>		0.010 (0.48)	0.005 (0.25)
<i>R&D</i>		0.044 (1.08)	0.049 (1.17)
<i>Capx</i>		0.008 (1.33)	0.007 (1.31)
<i>Sale growth</i>		0.012 (1.46)	0.012 (1.49)
<i>GDP growth</i>			0.002 (0.98)
<i>Inflat</i>			-0.001 (-0.29)
<i>Unemp</i>			-0.006 (-1.12)
<i>Carbon intensity</i>			-0.007 (-0.73)
<i>Constant</i>	0.003 (0.17)	0.049 (1.61)	0.131 (1.37)
Observations	128,260	128,260	128,260
R-squared	0.390	0.390	0.390
Firm FEs	Yes	Yes	Yes
Industry*Year FEs	Yes	Yes	Yes

Panel C: Controlling for additional control variables

	(1)	(2)	(3)
<i>Dep. Var.</i>	<i>Dome. GC to asset(%)$(t+1)$</i>		
<i>Post</i>	0.094** (2.50)	0.077** (2.10)	0.082** (2.11)
<i>Prisk_envir</i>	-0.018 (-1.09)		-0.018 (-1.03)
<i>ESG cover</i>	-0.013 (-0.93)		-0.020 (-1.18)
<i>Env_expen_prop</i>		-0.184*** (-3.22)	-0.186*** (-3.19)
<i>Disaster_num</i>		-0.006* (-1.85)	-0.006* (-1.92)
Controls	Yes	Yes	Yes
Observations	128,260	90,477	90,477
R-squared	0.380	0.367	0.367
Firm FEs	Yes	Yes	Yes
Industry*Year FEs	Yes	Yes	Yes

Panel D: Using alternative matching variables in the stacked DID analysis

	(1)	(2)	(3)	(4)
<i>Dep. Var.</i>	<i>Dome. GC to asset(%)$(t+1)$</i>			
<i>Treat*Post</i>	0.056*** (3.37)	0.050*** (3.01)	0.073*** (3.46)	0.062*** (3.29)
Controls	No	Yes	No	Yes
Observations	24,861	24,861	25,367	25,367
R-squared	0.465	0.465	0.422	0.422
Firm*Cohort FEs	Yes	Yes	Yes	Yes
Year*Cohort FEs	No	No	Yes	Yes
Industry*Year*Cohort FEs	Yes	Yes	No	No

Table 7: Alternative DID estimator based on Callaway and Sant'Anna

This table illustrates the average treatment effect (ATT) estimated through the method developed by Callaway and Sant'Anna (2021). To prevent already treated individuals contaminating the result, we only employ never treated individuals as control. All the covariates are lagged by one year of the dependent variable. The bracket contains the t-value of coefficient. The *, **, and *** represent the significant level at 10%, 5% and 1% respectively. We cluster the error term in country level to be robust heteroscedasticity. The variable definition is illustrated in Appendix A.

	(1)	(2)	(3)
<i>Dep. Var.</i>	<i>Dome. GC to asset (%)$(t+1)$</i>		
<i>ATT</i>	0.110*** (2.61)	0.122** (2.55)	0.126*** (2.76)
<i>Covariates</i>	No	Firm-only	All

Table 8: Additional analysis - The allocation changes of green contract in the post-period

This table presents additional analyses of the allocation changes of green contracts in the post-period. The dependent variables in Panel A include: *Dummy (new GC)* is an indicator defined for the full sample of firms and it equals one if the firm receives at least one domestic green contracts in year $t+1$, and zero otherwise; *Dummy (initiate GC)* is an indicator defined for a subset of firms that did not received a green contract in the year t , it equals one if the firm receives domestic green contracts in the year $t+1$, and zero otherwise); *Dummy (continue GC)* is an indicator defined for a subset of firms that received a green contract in the year t , it equals one if the firm receives a new domestic green contracts in the current year, and zero otherwise. In Panel B, we implement analysis at country-year level. The dependent variable is the HHI of the direct awardees of contracts awarded to public firms (*Public GC HHI*). All control variables are lagged by one year relative to the dependent variable. The brackets contain the t-values of the coefficients. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. We cluster standard errors at the firm level and the country level for Panel A and Panel B, respectively. Variable definitions are provided in Appendix A.

Panel A: The analysis on the possibility of obtaining green contract

	(1)	(2)	(3)
<i>Dep. Var.</i>	<i>Dummy</i> <i>(new GC)(t+1)</i>	<i>Dummy(initiate GC)(t+1)</i>	<i>Dummy</i> <i>(continue GC)(t+1)</i>
<i>Post</i>	0.008*** (4.20)	0.004*** (3.27)	0.259** (2.38)
<i>Size</i>	-0.001 (-0.71)	-0.000 (-0.48)	0.171 (1.50)
<i>Leverage</i>	0.002 (0.69)	0.000 (0.03)	-0.009 (-0.03)
<i>ROA</i>	-0.001 (-0.22)	-0.001 (-0.29)	1.166* (1.76)
<i>MTB</i>	-0.000 (-1.26)	-0.000 (-0.49)	0.010 (0.63)
<i>Cash</i>	-0.005** (-2.07)	-0.003 (-1.59)	-0.118 (-0.32)
<i>R&D</i>	-0.003 (-0.31)	0.003 (0.35)	1.543 (0.57)
<i>Capx</i>	0.000 (0.05)	-0.000 (-0.17)	0.026 (0.46)
<i>Sale growth</i>	0.001* (1.74)	0.001* (1.96)	-0.062 (-0.61)
<i>GDP growth</i>	-0.000 (-0.42)	-0.000 (-0.18)	-0.011 (-0.83)
<i>Inflat</i>	0.000 (1.24)	0.000 (1.32)	0.033 (1.01)
<i>Unemp</i>	0.000 (1.34)	0.000 (1.10)	0.024 (1.60)
<i>Carbon intensity</i>	-0.002***	-0.001***	-0.036

	(-3.79)	(-3.64)	(-0.65)
<i>Constant</i>	0.019***	0.011***	-1.091
	(3.30)	(2.81)	(-1.20)
Observations	128,260	127,307	753
R-squared	0.582	0.331	0.610
Firm FEs	Yes	Yes	Yes
Industry*Year FEs	Yes	Yes	Yes

Panel B: Country-year level analysis

	(1)	(2)
<i>Dep. Var.</i>	<i>Public GC HHI(t+1)</i>	
<i>Post</i>	-0.120**	-0.085*
	(-2.22)	(-1.91)
<i>TotalGC</i>		-0.058***
		(-4.13)
<i>GDP growth</i>		-0.010**
		(-2.19)
<i>Carbon intensity</i>		-0.006
		(-0.35)
<i>Inflat</i>		-0.001
		(-0.13)
<i>lnGDP</i>		0.248
		(1.35)
<i>Law intensity</i>		-0.083
		(-0.84)
<i>Unemp</i>		0.009
		(1.35)
<i>Constant</i>	0.644***	-1.623
	(22.20)	(-0.85)
Observations	409	409
R-squared	0.656	0.691
Country FEs	Yes	Yes
Year FEs	Yes	Yes

Table 9. The real impact on firms' carbon emission

This table presents the PSM-stacked DID results based on carbon emission. The dependent variables are *CO2 (scope 1)* (scope 1 carbon emissions plus one, taken in logarithms), *CO2 (scope 2)* (scope 2 carbon emissions plus one, taken in logarithms), *CO2 int. scope 1* (scope 1 carbon intensity plus one, taken in logarithms; carbon intensity is defined as carbon emissions scaled by revenue and is provided directly by Trucost), and *CO2 int. scope 2* (scope 2 carbon intensity plus one, taken in logarithms). The method used to construct the PSM-stacked sample is the same as that in Table 5. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. We cluster standard errors at the country level to be robust to heteroscedasticity. The variable definition is illustrated in Appendix A.

<i>Dep. Var.</i>	(1) <i>CO2(scope1) (t+1)</i>	(2) <i>CO2(scope2) (t+1)</i>	(3) <i>CO2 int. scope1(t+1)</i>	(4) <i>CO2 int. scope2(t+1)</i>
<i>Treat*Post</i>	-0.072* (-1.81)	-0.140*** (-4.49)	-0.027 (-0.64)	-0.094*** (-2.96)
<i>Size</i>	0.388*** (3.89)	0.498*** (8.82)	-0.160** (-2.48)	-0.102** (-2.64)
<i>Leverage</i>	0.375 (1.67)	0.255* (1.70)	0.111 (0.97)	0.050 (0.74)
<i>ROA</i>	0.717** (2.05)	0.577 (1.23)	-0.542 (-1.54)	-0.489 (-1.12)
<i>MTB</i>	0.012 (1.09)	0.020*** (2.75)	0.002 (0.31)	0.000 (0.07)
<i>Cash</i>	-0.030 (-0.15)	0.037 (0.21)	0.136 (0.91)	0.182 (0.97)
<i>R&D</i>	-0.208 (-0.09)	1.085 (0.87)	-1.193 (-0.73)	-0.637 (-0.96)
<i>Capx</i>	-0.027 (-0.86)	-0.019 (-0.63)	-0.006 (-0.30)	-0.006 (-0.37)
<i>Sale growth</i>	0.054 (0.99)	0.069** (2.03)	-0.073 (-1.59)	-0.049** (-2.60)
<i>GDP growth</i>	0.003 (0.78)	0.006 (0.90)	-0.000 (-0.16)	0.002 (0.52)
<i>Inflat</i>	0.011 (0.66)	-0.032*** (-2.80)	0.012 (0.69)	-0.027*** (-3.04)
<i>Unemp</i>	-0.010 (-0.98)	-0.009 (-0.92)	0.001 (0.08)	-0.002 (-0.21)
<i>Carbon intensity</i>	-0.014 (-0.34)	0.001 (0.02)	0.006 (0.15)	0.019 (0.69)
<i>Constant</i>	7.592*** (8.37)	6.587*** (14.53)	4.504*** (7.57)	3.904*** (12.35)
Observations	6,019	6,019	6,019	6,019
R-squared	0.963	0.950	0.949	0.893
Firm*Cohort FEs	Yes	Yes	Yes	Yes
Year*Cohort FEs	Yes	Yes	Yes	Yes

Table 10. Evidence from green contract initiation induced by ESG mandatory disclosure

This table illustrates the impact of green contract initiation, induced by ESG mandatory disclosure, on firms' green innovation outcomes. For each next year of reform (i.e. the reform took place in year $t-1$), we first identify firms that initiate green contracts in the year following the reform (i.e., firms that do not receive any green contracts from $t-3$ to $t-1$ but receive green contracts in t). For each initiating firm, we find one control firm that does not have any green contracts during $t-3$ to t (we use a PSM method to identify a control firm that meets this requirement). For each initiating firm and its corresponding control firm, we keep their observations from $[t-3, t+5]$. *Reform_GC* is a dummy variable equal to one for firms that initiate green contracts in the year following the reform and zero for control firms in the same country. *Post* takes a value of one for observations during $[t, t+5]$ and zero for observations during $[t-3, t-1]$. The dependent variables are *Green inv. eff.* and *Non-Green inv. eff.*, which are the number of green patents and non-green patents in year $t+1$, respectively, each scaled by the weighted sum of lagged R&D over the past five years (in millions of USD). Control variables are lagged by one year. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively. We cluster standard errors at the country level to be robust to heteroscedasticity. Variable definitions are provided in Appendix A.

	(1)	(2)
<i>Dep. Var.</i>	<i>Green inv. Eff.(t+1)</i>	<i>Non-green inv. Eff.(t+1)</i>
<i>Reform_GC*Post</i>	0.058** (2.42)	-0.201*** (-3.59)
<i>Size</i>	-0.007 (-1.13)	-0.085* (-1.86)
<i>Leverage</i>	-0.017 (-1.32)	0.063 (0.78)
<i>ROA</i>	-0.014 (-0.64)	-0.273* (-1.93)
<i>MTB</i>	0.000 (0.94)	-0.010 (-1.09)
<i>Cash</i>	-0.029* (-1.78)	0.255* (2.02)
<i>R&D</i>	-0.093 (-1.25)	-1.204** (-2.26)
<i>Capx</i>	0.000 (0.11)	-0.027 (-0.95)
<i>Sale growth</i>	-0.003 (-0.59)	0.116*** (4.20)
<i>GDP growth</i>	0.000 (0.46)	0.010 (1.53)
<i>Inflat</i>	0.003 (1.09)	-0.031** (-2.34)
<i>Unemp</i>	-0.002 (-1.21)	0.019 (0.81)
<i>Carbon intensity</i>	0.010* (1.81)	-0.064 (-1.50)

<i>Constant</i>	0.003 (0.07)	1.074*** (3.75)
Observations	7,395	7,395
R-squared	0.457	0.381
Firm*Cohort FEs	Yes	Yes
Industry*Year*Cohort FEs	Yes	Yes

Appendix A: Variable definitions

This table presents variable definitions.

Variables	Definitions	Data Sources
<i>Dome. GC to asset (%)</i>	The total value of domestic green contract participated by the firm in a year divided by lagged total asset (in percentage)	Factset TenderAlpha
<i>Dome. GC to ave asset (%)</i>	The total value of domestic green contract participated by the firm in a year divided by the average of current and lagged total asset (in percentage)	
<i>Dome. GC to sale (%)</i>	The total value of domestic green contract participated by the firm in a year divided by lagged total sales (in percentage)	
<i>Dome. GC to ave sale (%)</i>	The total value of domestic green contract participated by the firm in a year divided by the average of current and lagged total sales (in percentage)	
<i>lnDomeGC</i>	The logarithms of total participated contract value in USD plus 1	
<i>Dummy (new Dome. GC)</i>	Takes value of 1 if the firm participates the green contract this year and is equal to 0 otherwise.	Krueger et al. (2024)
<i>Public GC HHI</i>	The HH-Index of the direct public awardee of domestic green contract	
<i>TotalGC</i>	The logarithms of total issued green contracts of the countries in million USD plus 1	
<i>Post</i>	A dummy that takes value of 1 after the firms located country implement the mandatory ESG disclosure policy and 0 otherwise	
<i>Vol. ESG dis.</i>	Takes value of 1 if the countries had encouraged voluntary ESG disclosure before the mandatory disclosure and takes value of 0 otherwise.	
<i>Gov. domain.</i>	Takes value of 1 if the mandatory disclosure is implemented by government and takes value of 0 otherwise.	
<i>Strong reform</i>	Takes value of 1 if the disclosure is mandatory compliance and takes value of 0 otherwise (comply-or-explain etc.)	

<i>Gov. diversity</i>	The 1-year lagged value of the probability that two deputies picked at random from among the government parties will be of different parties	Database of Political Institutions
<i>Civi law</i>	Takes value of 1 for the countries with civil law and takes value of 0 otherwise. We add China as civil law countries as a complementary for La Porta et al. (1998)	La Porta et al. (1998)
<i>Size</i>	The logarithms of total asset (in million US dollars) plus 1 of firms	
<i>Leverage</i>	Total liability (Compustat item: lt)/Total asset (Compustat item: at)	
<i>ROA</i>	The operating income before depreciation(Compustat item: oibdp) scaled by total asset(Compustat item: at)	
<i>MTB</i>	The market value scaled by book value of equity (Compustat item: teq).	
<i>Cash</i>	Cash (Compustat item: ch)/Total asset(Compustat item: at)	Compustat
<i>Capx</i>	The natural logarithms of Capital expenditure/Employee in thousand USD(Compustat item: capx divided by Compustat item: emp)) plus 1	
<i>Sale growth</i>	The sale scaled by sale in lagged year and minus 1.	
<i>R&D</i>	R&D(Compustat item: xrd)/Total asset(Compustat item: at). The missing value of R&D expenditure is filled up with 0.	
<i>ESG cover</i>	Takes value of 1 if the firm is covered by Refinitiv ESG database in the given year (i.e. has ESG score in the database) and takes value of 0 otherwise.	Refinitiv ESG database
<i>ESGscore</i>	The ESG combined score	
<i>IO</i>	The fraction of share value held by institutional investors. The denominator is the market value of firm in USD. We replace the missing value with 0.	Thomson Reuters Ownership
<i>Prisk_envir</i>	The political risk related to economic environment or policy disclosed by the firms in earnings conference call transcripts multiplied by 1000. We fill up the missing value with 0.	Hassan et al. (2019)
<i>CO2(scope1)</i>	The logarithms of scope 1 carbon emission plus 1	Trucost
<i>CO2(scope2)</i>	The logarithms of scope 2 carbon emission plus 1	Environmental

<i>CO2 int. scope1</i>	The logarithms of scope 1 carbon emission intensity plus 1	
<i>CO2 int. scope2</i>	The logarithms of scope 2 carbon emission intensity plus 1	
<i>Green inv. Eff.</i>	The green patent number scaled by the weighted sum of the R&D (in million USD) of the past 5 years. The weights from lagged 1 year to lagged 5 year are 1, 0.8, 0.6, 0.4, 0.2.	PATSTAT Global 2024
<i>Non green inv. Eff.</i>	The non-green patent number scaled by the weighted sum of the R&D (in million USD) of the past 5 years. The weights from lagged 1 year to lagged 5 year are 1, 0.8, 0.6, 0.4, 0.2.	
<i>Env_expen_prop</i>	The percentage of environmental protection expenditure to total GDP	IMF
<i>Disaster_num</i>	The number of climate related disasters in each country-year	
<i>Unemp</i>	Unemployment rate (%)	
<i>Inflat</i>	Inflation rate (%)	
<i>GDP growth</i>	The annual growth rate (%) of GDP per capita	
<i>lnGDP</i>	The logarithms of GDP per capita plus 1 (constant 2021 international USD)	World Bank
<i>Carbon intensity</i>	The carbon dioxide emission per capita (tons CO2 per capita)	
<i>Law intensity</i>	The score of rules of law of each country	

Appendix B: The summary statistics by country and year

In this table, we summarize the means of government contract variable (*Dome. GC to asset (%)* and *Dome. GC to sale (%)*) by countries. The last column presents the year of ESG mandatory disclosure reform in each country. “-” denote that the country has never implemented ESG mandatory disclosure reform in the sample period. The timeline of ESG mandatory disclosure is extracted from Krueger et al. (2024).

Country	N	<i>Dome. GC to sale(%)</i>	<i>Dome. GC to asset(%)</i>	Period	Reform
Australia	3516	0	0	2009-2022	2003
Austria	514	0.022	0.027	2009-2022	2016
Belgium	854	0.01	0.007	2009-2022	2009
Bangladesh	239	0	0	2010-2018	-
Bulgaria	173	0.012	0.016	2009-2022	2016
Brazil	498	0	0	2009-2022	-
Canada	518	0	0	2014-2022	2004
Switzerland	1773	0.002	0.002	2009-2022	-
Chile	547	0	0	2009-2022	2015
China	14065	0	0	2009-2022	2008
Cayman Islands	2894	0	0	2011-2016	-
Cyprus	296	0	0	2009-2022	2016
Germany	3985	0.001	0	2009-2022	2016
Denmark	954	0.017	0.02	2009-2022	2016
Spain	922	0.129	0.066	2009-2022	2012
Estonia	162	0.059	0.093	2009-2022	-
Finland	1349	0.042	0.057	2009-2022	2016
France	4917	0.088	0.072	2009-2022	2001
United Kingdom	10135	0.175	0.232	2009-2022	2013
Greece	974	0	0	2009-2022	2006
Hong Kong	1363	0	0	2009-2022	2015
Croatia	379	0	0	2009-2022	-
Hungary	126	0.011	0.008	2009-2022	2016
Indonesia	3724	0	0	2009-2022	2012
India	9123	0	0	2011-2022	2015
Ireland	525	0	0	2009-2022	2016
Iceland	109	0.001	0	2009-2022	-
Israel	1611	0	0	2009-2022	-
Italy	1514	0.018	0.011	2009-2022	2016
Jordan	114	0	0	2009-2022	-
Japan	37634	0.000	0.000	2009-2022	-
Sri Lanka	1593	0	0	2009-2022	-
Lithuania	266	0.084	0.12	2009-2022	-
Luxembourg	356	0	0	2009-2022	-
Latvia	216	0	0	2009-2022	-
Mexico	694	0	0	2009-2022	-
Malta	161	0	0	2009-2022	2016

Mauritius	171	0	0	2009-2022	-
Malaysia	1898	0	0	2009-2022	2007
Nigeria	391	0	0	2012-2020	-
Netherlands	1157	0.026	0.018	2009-2022	2016
Norway	1373	0.014	0.024	2009-2022	2013
New Zealand	458	0	0	2009-2022	-
Pakistan	1895	0	0	2009-2022	2009
Peru	126	0.000	0.000	2009-2022	2015
Philippines	1105	0	0	2009-2022	2011
Poland	1541	0.798	0.824	2009-2022	2016
Portugal	286	0.004	0.002	2009-2022	2010
Romania	363	0.014	0.006	2009-2022	2016
Russia	209	0	0	2009-2022	-
Singapore	1260	0	0	2009-2022	2016
Slovenia	144	0.101	0.151	2009-2022	2017
Sweden	3063	0.011	0.015	2009-2022	2016
Thailand	149	0	0	2009-2022	-
Turkey	1690	0	0	2009-2022	2014
Viet Nam	583	0	0	2010-2022	-
South Africa	1605	0	0	2009-2022	2010

Appendix C: The differences between contractors and non-contractors before ESG mandatory disclosure in the country

This table compares firms that receive green contracts before local ESG reform and firms that do not receive such contracts before local ESG reform. *ESG_Cover* takes value of 1 if the firm is covered by Refinitiv ESG database and takes value of 0 otherwise. *ESG_Score* is the combined ESG of a firm score from Refinitiv ESG database (a higher value indicates that a greener firm). *IO* is the proportion of institutional ownership that is the total market value of share owned by institutional investors divided by the total market value of the firm. The *IO* data comes from Thomson Reuters Ownership.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Firms with green contracts before mandatory ESG disclosure			Firms without green contracts before mandatory ESG disclosure				
Subsample=	[a]			[b]			Diff. in mean	
<i>Var.</i> =	Mean	Median	N	Mean	Median	N	[a] - [b]	t-value
<i>Size</i>	7.866	8.033	538	5.930	5.819	62,808	1.936	24.21***
<i>ESG_Score</i>	0.536	0.533	248	0.446	0.455	8,871	0.089	7.00***
<i>ESG_Cover</i>	0.461	0	538	0.141	0	62,808	0.320	21.11***
<i>IO</i>	0.166	0.158	538	0.072	0.027	62,808	0.094	21.37***

Appendix D: The alternative reform time setting

In this table, we set the next year of reform as the effective year of the policy and perform the test. *Post_alter* is set to 1 after the effectiveness of policy (the second year of reform) and set to 0 otherwise. Panel A presents the result of stagger DID. Panel B presents the result of stack DID. We only use firm level characteristics as the matching variables in this table. And we keep 5 years observations around the event. The *, **, and *** represent the significant level at 10%, 5% and 1% respectively. We cluster the error term at country level to be robust heteroscedasticity. The variable definition is illustrated in Appendix A.

Panel A: Staggered DID

<i>Dep. Var.</i>	(1)	(2)	(3)
	<i>Dome. GC to asset(%)$(t+1)$</i>		
<i>Post_alter</i>	0.141** (2.20)	0.142** (2.20)	0.138** (2.36)
<i>Size</i>		-0.020* (-1.70)	-0.021 (-1.62)
<i>Leverage</i>		0.057 (0.77)	0.058 (0.78)
<i>ROA</i>		0.095 (1.40)	0.094 (1.38)
<i>MTB</i>		0.000 (0.01)	-0.000 (-0.04)
<i>Cash</i>		0.027* (1.69)	0.019 (1.46)
<i>R&D</i>		-0.010 (-0.22)	-0.002 (-0.05)
<i>Capx</i>		0.006 (1.24)	0.005 (1.17)
<i>Sale growth</i>		0.005 (0.91)	0.005 (1.10)
<i>GDP growth</i>			-0.000 (-0.33)
<i>Inflat</i>			-0.002 (-0.82)
<i>Unemp</i>			-0.011* (-1.90)
<i>Carbon intensity</i>			-0.013 (-0.98)
<i>Constant</i>	-0.034 (-1.10)	0.027 (0.87)	0.195* (1.75)
Observations	128,260	128,260	128,260
R-squared	0.380	0.380	0.380
Firm FEs	Yes	Yes	Yes
Industry*Year FEs	Yes	Yes	Yes

Panel B: Stack DID

	(1)	(2)	(3)	(4)
<i>Dep. Var.</i>				
<i>Treat*Post</i>	0.109**	0.097*	0.140***	0.115**
	(2.11)	(1.85)	(2.97)	(2.47)
<i>Controls</i>	No	Yes	No	Yes
Observations	41,668	41,668	42,386	42,386
R-squared	0.411	0.411	0.358	0.358
Firm*Cohort FEs	Yes	Yes	Yes	Yes
Year*Cohort FEs	No	No	Yes	Yes
Industry*Year*Cohort FEs	Yes	Yes	No	No

Appendix E. The result of the re-calculating contract value received

In this table, we re-calculate the *Dome. GC to asset(%) (act. value)(t+1)* considering the co-awarding of green contract. For each co-awarding contract (i.e. green tender with more than 1 contractor), we assume that each contractor takes equal weight in the contract. The actual received tender value is calculated by dividing the tender potential maximum value with number of awardees (identified by direct awardee bizportal id provided by Factset). Then, the awarded contract value for each contractor is the sum of the actual tender value received. Panel A, B, and C present the result of staggered DID, dynamic DID, and PSM-stack DID. In PSM-stack DID, we keep the [-5, +5] years for each cohort around the event year. The other setting is same as previous. The *, **, and *** represent the significant level at 10%, 5% and 1% respectively. We cluster the error term at country level to be robust heteroscedasticity. The variable definition is illustrated in Appendix A.

Panel A: Staggered DID

<i>Dep. Var.</i>	(1)	(2)	(3)
	<i>Dome. GC to asset(%) (act. value)(t+1)</i>		
<i>Post</i>	0.024*** (3.02)	0.025*** (2.99)	0.025*** (2.81)
<i>Size</i>		-0.007 (-1.63)	-0.007 (-1.54)
<i>Leverage</i>		0.010 (0.49)	0.010 (0.48)
<i>ROA</i>		0.013 (1.09)	0.012 (0.99)
<i>MTB</i>		0.000 (0.62)	0.000 (0.61)
<i>Cash</i>		0.016 (1.34)	0.013 (1.26)
<i>R&D</i>		-0.016 (-1.06)	-0.015 (-0.87)
<i>Capx</i>		0.003 (1.29)	0.002 (1.24)
<i>Sale growth</i>		0.002 (0.96)	0.002 (1.00)
<i>GDP growth</i>			0.000 (0.81)
<i>Inflat</i>			0.000 (0.82)
<i>Unemp</i>			-0.003 (-1.25)
<i>Carbon intensity</i>			-0.001 (-0.23)
<i>Constant</i>	-0.002 (-0.38)	0.027 (1.46)	0.048* (1.68)

Observations	128,260	128,260	128,260
R-squared	0.303	0.303	0.303
Firm FEs	Yes	Yes	Yes
Industry*Year FEs	Yes	Yes	Yes

Panel B: Dynamic DID

	(1)	(2)	(3)
<i>Dep. Var.</i>	<i>Dome. GC to asset(%)(act. value)(t+1)</i>		
<i>Post(-2)</i>	-0.004 (-0.96)	-0.004 (-1.00)	-0.004 (-1.17)
<i>Post(-1)</i>	-0.002 (-0.16)	-0.002 (-0.18)	0.000 (0.07)
<i>Post(0)</i>	-0.015 (-0.76)	-0.015 (-0.77)	-0.013 (-0.78)
<i>Post(1)</i>	0.014** (2.12)	0.014** (2.20)	0.016*** (2.74)
<i>Post(2+)</i>	0.036*** (2.77)	0.037*** (2.75)	0.039** (2.53)
Controls	No	Firm only	All
Observations	128,260	128,260	128,260
R-squared	0.303	0.303	0.303
Firm FEs	Yes	Yes	Yes
Industry*Year FEs	Yes	Yes	Yes

Panel C: Stack DID

	(1)	(3)	(4)	(6)
<i>Dep. Var.</i>	<i>Dome. GC to asset(%)(act. value)(t+1)</i>			
<i>Treat*Post</i>	0.030** (2.07)	0.032* (2.00)	0.022** (2.64)	0.025*** (2.81)
Controls	No	Yes	No	Yes
Observations	22,229	22,229	22,719	22,719
R-squared	0.502	0.502	0.440	0.440
Firm*Cohort FEs	Yes	Yes	Yes	Yes
Year*Cohort FEs	No	No	Yes	Yes
Industry*Year*Cohort FEs	Yes	Yes	No	No