Passing the Rules Down: How Labor Programs Spread Without Protecting Workers

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Abstract

Private labor programs have expanded across supply chains, yet their effects on working conditions remain mixed. How do labor requirements spread without improving working conditions? I argue that suppliers pass responsibility down the supply chain, and written records shape this process. Buyer firms select suppliers on documented labor policies because they cannot observe working conditions inside suppliers. After securing contracts, suppliers demonstrate oversight by setting labor rules for their own suppliers. Using hazard models and stacked difference-in-differences designs on 822,505 buyer–supplier relationships, I find that suppliers with more extensive environmental, social, and governance (ESG) records are more likely to win a first contract with a buyer firm that runs a labor program, while independently reported labor incidents do not predict these contracts. Suppliers then begin setting such rules for their own suppliers, but only after new relationships. Prohibitions on child and forced labor rise modestly, but collective labor rights and labor incidents do not improve.

Keywords: *global supply chains, labor standards, private regulation, ESG disclosure, decoupling*

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1 Introduction

Firms are held responsible for labor conditions not only within their own operations but also across their supply chains. For example, Nike does not own the factories that make its products, yet it has faced criticism over labor violations at its suppliers. Following sweatshop scandals at its supplier factories in the 1990s, Nike adopted a supplier code of conduct and began disclosing its supplier factories (Locke, 2013). Chains of responsibility did not stop there. In 2020, allegations of forced labor in Xinjiang's cotton industry raised concerns about spinning mills and farms further upstream, beyond the suppliers Nike contracts with directly. Nike stated that its contract suppliers had confirmed they were not using textiles or spun yarn from the region (NIKE, Inc., 2020). This example illustrates how buyer firms take responsibility beyond their direct suppliers and rely on those suppliers to address labor issues further upstream.

Private labor programs have spread across global supply chains over time. Publicly traded firms recorded in `ASSET4`, which provides information on firms' commitments and performance in environmental, social, and governance (ESG) issues, show that about 18% of firms had supply-chain labor programs in 2008, while this share had risen to about 70% by 2022. Yet, labor violations persist, and existing studies find mixed evidence that buyer–supplier relationships improve working conditions within suppliers' own operations (Amengual, Distelhorst and Tobin, 2020, Anner, 2012, Distelhorst, Hainmueller and Locke, 2017, Greenhill, Mosley and Prakash, 2009, Locke, 2013, Malesky and Mosley, 2018, 2021, Vogel, 2008). Much of this research examines compliance within the supplier's operations, taking their relationships with buyers as given. Although studies of multi-tier supply chains recognize that buyers rely on direct suppliers to extend labor requirements further upstream (Bae et al., 2025, Mena, Humphries and Choi, 2013, Tachizawa and Wong, 2014, Wilhelm et al., 2016), less is known about which suppliers take on this role and how they respond. This raises the following question: Do suppliers strengthen commitments to their own workers, extend requirements to their suppliers, or both?

Distinguishing selection from the effects of contractual relationships is a central challenge in studying the diffusion of labor standards, as in research on international compliance (Downs, Rocke and Barsoom, 1996, Simmons and Hopkins, 2005, Von Stein, 2005). Suppliers linked to buyer firms running labor programs may have more extensive labor commitments because these relationships induce them to adopt new policies or because buyers select suppliers that already have such commitments (Greenhill, Mosley and Prakash, 2009). I address this challenge by examining both which suppliers form their first contracts with buyer firms running labor programs and how suppliers' policies change following these contracts.

I argue that documented labor commitments play an important role in both processes. Buyer firms face pressure from investors, regulators, and consumers to address labor risks across their supply chains, but they cannot directly observe working conditions at every supplier. Still, buyer firms need to demonstrate that they have taken action, incentivizing them to provide evidence in the form of codes, audits, and certifications (Gereffi, Humphrey and Sturgeon, 2005, Short, Toffel and Hugill, 2016). Suppliers that already have these records may be more likely to secure contracts because they can demonstrate that they meet buyer firms' labor requirements.

Once suppliers enter these relationships, buyer firms' responsibility for labor conditions extends beyond their direct suppliers. Buyers may be held accountable for conditions further up the chain, even though they have no direct contracts with those firms, making them rely on their suppliers to address these risks. Thus, suppliers can act as double agents (Wilhelm et al., 2016), complying with buyer firms' requirements while extending them to their own suppliers. Before entering such relationships, suppliers may have fewer incentives to establish labor requirements for their own suppliers. Afterward, they face pressure to take on part of the buyer's oversight responsibilities. These requirements can be incorporated into contracts and codes of conduct at relatively low cost, while the costs of implementing them may fall on firms further up the chain.

The timing of suppliers' responses may also depend on when buyer firms introduce labor

requirements. Buyer firms establish expectations through codes of conduct, questionnaires, and contract terms when selecting new suppliers. Introducing new requirements into existing relationships may be more difficult because buyer firms have already invested in these relationships and replacing suppliers can be costly. Thus, requirements that suppliers must meet to enter are easier to enforce than requirements imposed on suppliers a buyer firm already depends on. Building on this, my theory expects suppliers to be more likely to adopt new labor policies following a first contract with a buyer running a labor program than when an existing buyer adopts such a program.

Finally, the diffusion of formal labor requirements does not necessarily lead to improvements in working conditions. Because buyer firms can assess documented commitments more easily than actual labor practices, suppliers have incentives to prioritize formal policies. They can also extend labor requirements to their own suppliers through contracts and codes of conduct, while improving working conditions within their own operations may require greater investment and organizational changes. Building on this, I expect larger changes in documented labor commitments than in actual working conditions.

Together, my argument generates three expectations. First, suppliers with more extensive documented labor commitments should be more likely to form first contracts with buyer firms running labor programs. Second, after these contracts, suppliers should adopt rules for their own suppliers, and they should do so when they enter a new relationship rather than when a buyer firm they already serve adopts a program. Third, documented labor commitments should change more than actual working conditions.

To test my theory, I combine data on firms' positions in supply chains, ESG records, independently reported ESG incidents, public regulation, and firm fundamentals. The resulting data include 822,505 buyer–supplier relationships involving 101,994 suppliers across 185 countries from 2003 to 2022. Using hazard models, I estimate which suppliers win a first contract with buyer firms that operate labor programs. I then estimate changes following suppliers' first contract in the supply chain by using stacked difference-in-differences designs

that compare suppliers with similar prior disclosure. In particular, I examine which dimensions of suppliers' labor records and conduct—formal policies, reported implementation, and independently reported incidents—are most responsive to the first contract with a buyer firm running a labor program.

I find that suppliers with more extensive documentary records are more likely to secure first contracts with buyer firms running labor programs, while independently reported labor incidents do not predict these contracts. Following a first contract, suppliers begin to take on a new role as rule setters for their own suppliers. They expand labor requirements for these firms, while their other governance and environmental policies show little change. Consistent with the theory, this response follows the formation of new relationships rather than the adoption of labor programs by existing buyers. The response is also selective: suppliers show modest increases in prohibitions on child and forced labor but show little change in commitments to freedom of association, which may be more costly to implement because they involve granting workers greater collective power. Despite these formal changes, labor incidents and union representation show no improvement. Together, the findings show that private labor programs extend formal responsibilities across supply chains without necessarily improving working conditions within supplier firms.

This paper contributes to research on private labor regulation and global supply chains in two ways. First, I contribute to research on trade-based norm diffusion by distinguishing supplier selection from changes following contract formation. Existing studies show that trade and supply-chain relationships can transmit labor standards across countries through the linkage of trade to non-trade issues (Cao and Prakash, 2010, Greenhill, Mosley and Prakash, 2009, Neumayer and De Soysa, 2005, Perkins and Neumayer, 2007, Prakash and Potoski, 2006). However, these relationships may also reflect buyer firms' selection of suppliers with existing commitments. By examining both contract formation and subsequent changes in suppliers' policies, I find that documentary records predict which suppliers enter relationships with buyer firms running labor programs, while these relationships are followed

by further adoption of formal labor requirements. This distinction helps explain how private labor standards spread through both supplier selection and changes following contract formation.

Second, I contribute to research on multi-tier supply chains and organizational decoupling by examining how suppliers extend labor requirements to other firms. Previous studies have shown how buyers reach suppliers beyond the first tier (Mena, Humphries and Choi, 2013, Tachizawa and Wong, 2014, Villena and Gioia, 2018). In these relationships, suppliers can act as "double agents," complying with buyer firms' requirements while extending them to their own suppliers (Wilhelm et al., 2016). I show that suppliers expand labor requirements for their own suppliers following first contracts with buyer firms running labor programs. However, although suppliers expand formal policies and report additional implementation measures, labor incidents show little improvement. Building on research showing that formal commitments may remain disconnected from actual practices (Bromley and Powell, 2012, Meyer and Rowan, 1977, Short, Toffel and Hugill, 2016, Wijen, 2014), I show how this gap is relevant to the extension of labor requirements across supply chains. Suppliers take on formal responsibilities for regulating other firms without improving working conditions within their own operations.

2 Labor Programs Across Firm Boundaries

2.1 Responsibility Without Ownership in Supply Chains

Efforts to improve labor practices through trade have emphasized the role of public authority. Governments incorporate non-trade issues, such as labor standards and human-rights provisions, into trade agreements, condition market access on compliance, and use economic leverage to encourage reforms abroad (Hafner-Burton, 2005, 2011, Kelley, 2010). This logic parallels the *California Effect*, which explains how access to an economically important market can encourage firms or jurisdictions elsewhere to adopt more stringent standards (Vogel,

2009). Building on this logic, previous studies in international political economy have shown how trade can transmit labor standards across countries (Distelhorst and Locke, 2018, Greenhill, Mosley and Prakash, 2009, Hafner-Burton, Mosley and Galantucci, 2019, Malesky and Mosley, 2018, Neumayer and De Soysa, 2005). Yet, market competition through trade can also create pressure in the opposite direction. Where monitoring and enforcement are weak, states and firms can lower production costs at the expense of labor protections, leading to a race to the bottom (Mosley and Uno, 2007).

Private actors increasingly set and enforce standards that were once the domain of states (Abbott and Snidal, 2021, Büthe and Mattli, 2011, Green, 2013). As global production networks become more fragmented, firms play a crucial role in transmitting labor standards through supply chains. Downstream buyer firms use supplier codes of conduct, certification, and related labor programs to set standards for their supply chain partners (Berliner and Prakash, 2015, Locke, Amengual and Mangla, 2009, Marx, 2008). As with public rules, compliance can become a condition of winning contracts. Thus, even without public regulations, labor programs can carry labor norms through supply chains.

Nevertheless, spreading rules across firms differs from adopting them within firms. Buyer firms do not own the suppliers that provide parts to them, and those suppliers may have contracts with additional sub-suppliers over which buyer firms have even less direct control. Studies on global value chains identify different forms of interfirm governance, ranging from arm's-length market exchanges to modular, relational, and captive arrangements (Gereffi, Humphrey and Sturgeon, 2005). These relationships vary in the dependence and coordination they involve, but they do not give buyer firms the authority that comes with ownership. Thus, buyer firms can require suppliers to meet labor standards as a condition of obtaining contracts, but they cannot direct how suppliers manage their internal operations (Grosser and Moon, 2005, Ponte, Sturgeon and Dallas, 2019). This raises two important questions: which suppliers gain access to relationships with buyer firms running labor programs and what changes within suppliers once they do?

The first question is a selection problem, which echoes a longstanding debate in the study of international cooperation. Studies on compliance have shown that observed compliance may result from members' changed behavior or the entry of actors already positioned to comply (Downs, Rocke and Barsoom, 1996, Simmons and Hopkins, 2005, Von Stein, 2005). A similar problem arises in private labor regulation in supply chains. Suppliers that sell to buyer firms with labor programs may exhibit stronger labor records either because buyer requirements changed them or because suppliers already better positioned to meet those requirements select into these relationships. Thus, an analysis that begins only after a contractual relationship forms may conflate differences present at contracting with subsequent changes.

Evidence on the second question finds that private labor regulation can influence supplier behavior, but that its effects vary across labor standards and institutional settings (Anner, 2012, Bartley et al., 2015, Distelhorst and Locke, 2018, Locke, 2013, Malesky and Mosley, 2018). Requirements that can be clearly specified and monitored may be easier to enforce than collective labor rights, which can be more costly to enforce and difficult to observe in practice. Moreover, even within the same labor standard category, verifying that suppliers have adopted a policy or code of conduct does not establish whether they follow it. Thus, assessing the effects of private labor regulation requires distinguishing formal commitments from implementation and workplace outcomes.

Previous studies have shown that formal policies do not always change firm practices on the ground. Firms may adopt policies without fully implementing them, or they may implement prescribed procedures without accomplishing the intended outcomes (Bromley and Powell, 2012, Meyer and Rowan, 1977). This decoupling problem arises in supply chains as well. Formal commitments to improve practices in suppliers can fail to induce deeper compliance (Boiral, 2007, Christmann and Taylor, 2006). Moreover, policy implementation varies across institutional settings (Bartley, 2018), generating different patterns of compliance. Auditing does not fundamentally solve this problem because it depends on what

monitoring systems are designed to detect and what firms disclose (Short, Toffel and Hugill, 2016, Toffel, Short and Ouellet, 2015). The responses to the Rana Plaza incident illustrate that a firm-led auditing initiative and a union-backed binding agreement created different forms of accountability for the same factories (Reinecke and Donaghey, 2015).

While extant research explains why written commitments can coexist with limited changes in workplace conditions, it takes supply-chain relationships as given and focuses on compliance within them. However, if buyer firms rely on written records to hold suppliers accountable for labor standards, these records may shape both which suppliers receive contracts and what those suppliers subsequently require of their own suppliers. In the next section, I discuss why buyer firms rely on such records and why these records, rather than verified practices, are sufficient for them.

2.2 Why Buyer Firms Need Records

Buyer firms are held responsible by multiple stakeholders–investors, regulators, civil society, and consumers–for conditions in their supply chains. However, working conditions inside independent suppliers are difficult for external stakeholders to observe. Consequently, accountability often relies on evidence of the steps or procedures buyer firms have taken to manage labor risks, including supplier codes of conduct, monitoring procedures, and documented compliance (Short, Toffel and Hugill, 2016, Toffel, Short and Ouellet, 2015). Hence, buyer firms face two distinct risks. Violation risk arises when a supplier mistreats its workers. Accountability risk arises when the buyer firm cannot show that it took reasonable steps to prevent or address such violations. Although written records cannot eliminate the risk of labor violation, they provide verifiable evidence of oversight and help buyer firms manage accountability risks.[1] Much of this evidence must come from suppliers' answers to surveys, acceptance of codes, and the documentation for audits and reports.

[1]Buyer firms are more likely to implement supply-chain programs after incidents in their suppliers than after other types of incidents (Appendix H).

Why do buyer firms rely on written records to oversee their suppliers? One possibility is that buyer firms *cannot* directly observe working conditions in their suppliers they do not own, especially at their sub-suppliers or sub-sub-suppliers. The other is that written records are what buyer firms *need*. These two reasons have different implications. If buyer firms cannot observe working conditions directly, better monitoring should shift their oversight from records toward practice. If buyer firms rely on written records to demonstrate oversight, monitoring may continue to focus on documentation even as monitoring practices improve, because firms have incentives to show that they have taken action. I argue that the second reason drives buyer firms' behavior. This does not necessarily mean that consumers, regulators, or investors care only about written records. They may want conditions to improve. Yet, they also face the same limitation as buyer firms: observing working conditions inside suppliers is more difficult for them. Buyer firms can more easily observe, compare, and reward documented action than actual labor conditions. ESG disclosures document policies and management systems (Berg, Kölbel and Rigobon, 2022, Chatterji et al., 2016), and supply-chain laws such as the California Transparency in Supply Chains Act and the UK Modern Slavery Act require firms to report what they do, not to demonstrate that conditions have improved. Demand for improvement thus reaches buyer firms as demand for documented action. Much of this documentation must come from suppliers' policies, commitments, and oversight over their own suppliers.

2.3 Selection: Records as the Terms of Access

If buyer firms need documented policies or commitments from their suppliers, suppliers that already hold them are easier to contract with. Before contracting, buyer firms can only evaluate prospective suppliers relying on information that is available and verifiable, such as existing policies, certifications, and disclosures. Such information also reduces the effort required for buyer firms to assess prospective suppliers and obtain the evidence necessary to demonstrate oversight (Gereffi, Humphrey and Sturgeon, 2005, Short, Toffel and Hugill,

2016) as buyer firms cannot directly control suppliers' internal operations (Ponte, Sturgeon and Dallas, 2019). Therefore, suppliers with more developed documentary records are better positioned to satisfy buyer firms' accountability requirements before a contractual relationship forms.

Documentary records may partly reflect better working practices: suppliers with stronger labor conditions may also be more likely to maintain formal policies and reporting systems. However, the two do not necessarily agree closely. Suppliers can adopt written policies without fully implementing them (Baron and Diermeier, 2007, Bromley and Powell, 2012, Christmann and Taylor, 2006, Meyer and Rowan, 1977), while those with good working conditions may not have extensive records. Furthermore, workplace practices are difficult to assess before a contract is formed (Berliner and Prakash, 2015, Locke, Amengual and Mangla, 2009). Labor incidents often surface with a lag, and their reporting depends partly on a supplier's visibility to the media and NGOs (Bartley and Child, 2014). Moreover, some indicators, such as workplace injury rates, are self-reported and difficult to compare across sectors and countries. A supplier with no recorded incidents may be one that has received little scrutiny. By contrast, documentary records can provide evidence of due diligence (LeBaron and Rühmkorf, 2017, Short, Toffel and Hugill, 2016). Even when buyer firms have information about working conditions, they still need to demonstrate how they have managed labor risks. Thus, buyer firms have stronger incentives to select suppliers based on documentary records than on working conditions alone. If records mattered only as a proxy for conditions, workplace practices should predict a first contract once records are held fixed.

This creates a selection mechanism in supply chains. Existing studies examine the diffusion of labor norms within established relationships (Distelhorst, Hainmueller and Locke, 2017, Locke, 2013, Malesky and Mosley, 2018), suggesting that buyer firms reward suppliers with better compliance records with more business (Distelhorst and Locke, 2018). However, existing records can matter before a supply-chain relationship begins. Buyer firms may use them to screen prospective suppliers, while suppliers with more developed records may

be more likely to seek out buyer firms that run labor programs. In order to understand how labor programs spread in supply chains, it is important to examine selection into those relationships as well as changes that follow contracts.

I argue that when buyer firms run supply-chain programs aimed at improving labor conditions, prospective suppliers with more developed records are easier to screen. Such suppliers should be more likely to begin contracts with these buyer firms, even before any programs change supplier behavior. This leads to my first hypothesis:

> **Hypothesis 1:** *Suppliers with more extensive records are more likely to form a first contract with a buyer firm that runs a labor program, while their working conditions do not predict a first contract.*

2.4 After the Contract: Extending Responsibility Upstream

Which aspects of supplier behavior are most likely to change after a first contract with a buyer firm that runs a labor program? Records serve different purposes before and after contracting. Before a contract forms, buyer firms assess whether a supplier can meet their documentary requirements. After contracting, suppliers face continuing obligations to comply with codes of conduct, respond to questionnaires, and provide evidence for buyer firms' reporting. Thus, even suppliers qualified based on their existing records face additional demands after the contract. Because buyer firms cannot directly observe what happens at supplier facilities, suppliers can respond selectively to their demands. I examine whether these responses concern their own suppliers or their own operations, and whether they involve written commitments or actual practices.

First, once suppliers enter relationships with buyer firms running labor programs, they also face expectations to oversee labor conditions among their own suppliers. Buyer firms are held responsible for labor risks beyond their direct suppliers, even though they have no contractual relationships with firms further upstream. To address these risks, buyers rely on their direct suppliers to oversee firms further upstream (Mena, Humphries and Choi, 2013,

Tachizawa and Wong, 2014), and suppliers provide documentation of their own suppliers' labor practices. Thus, suppliers take on what Wilhelm et al. (2016) call a "double agent" role: complying with buyer firms' requirements while extending labor requirements to their own suppliers. In other words, buyer firms' direct suppliers act as both rule recipients and rule setters. Before entering these relationships, suppliers may have fewer incentives to establish rules for their own suppliers. After forming contracts, suppliers are expected to oversee labor risks further upstream and have incentives to set formal rules for their own suppliers.

Suppliers may respond by setting labor requirements for their own suppliers, strengthening their in-house commitments, or both. They may pass obligations upstream to reduce their own compliance costs. One possibility is that suppliers pass obligations upstream to reduce compliance costs. They can add requirements to purchasing contracts, shifting some of the costs to their own suppliers. By contrast, improving working conditions within their operations may require additional investments. Another possibility is that suppliers reproduce the buyer firm's labor program. If buyer firms establish in-house commitments before extending requirements to their suppliers, suppliers may adopt rules for other firms while maintaining similar commitments to their own workers. Under the first explanation, suppliers may adopt rules for their own suppliers without strengthening their in-house commitments. Under the second, both types of commitments are expected to expand together.

Second, the timing of these responses should also depend on when buyer firms introduce labor requirements. Buyer firms establish expectations through codes of conduct and contract terms when selecting new suppliers. In existing relationships, new requirements may be harder to enforce because buyer firms have already invested in these relationships and replacing suppliers can be costly. Therefore, buyers may have greater leverage to establish labor requirements when forming new contracts than when imposing additional requirements on existing suppliers. I expect suppliers to adopt new policies following first contracts with buyer firms running labor programs more readily than when buyers they already serve adopt such programs.

Hypothesis 2A: *A first contract with a buyer firm that runs a labor program increases suppliers' rules for their own suppliers.*

Hypothesis 2B: *The increase in these requirements is larger following a new relationship with a buyer firm that already runs a program than following program adoption by a buyer firm the supplier already serves.*

Beyond these hypotheses, my theory implies which labor standards are more likely to travel. Prohibitions, such as bans on child and forced labor, are relatively easy to state in a code of conduct and are salient in supply-chain due diligence. In contrast, collective labor rights require employers to accept independent worker organization, imposing costs on employers. Moreover, their exercise on the ground is difficult for outsiders to monitor (Anner, 2012, Egels-Zandén and Merk, 2014). Thus, I expect greater improvements in documented policies concerning child and forced labor than in collective labor rights.

2.5 Records Without Conduct

Passing labor rules along supply chains does not necessarily change working conditions. As research on decoupling shows, firms may adopt formal policies without implementing them, and implementation does not always produce the intended outcomes (Bromley and Powell, 2012, Meyer and Rowan, 1977, Wijen, 2014). This problem is particularly relevant in supply chains, where buyers rely on documents and audits to assess practices they cannot directly observe (Short, Toffel and Hugill, 2016). Suppliers may therefore focus on commitments that are easier and less costly to document. Adopting a formal policy is a one-time or short-term act that buyer firms can verify from a document, while reported implementation, such as training and improvement programs, requires sustained activity. Changes in workplace conduct on the ground require improvements in practice that buyer firms can rarely observe and verify, and that require investment in staffing, equipment, and management systems whose cost falls on the supplier itself. Thus, the gap between formal commitments and actual working conditions can emerge in two ways. Within supplier firms, in-house policies may expand without corresponding improvements in working conditions. Further upstream,

suppliers may extend labor requirements to their own suppliers without improving conditions in those firms.

> **Hypothesis 3:** *A first contract with a buyer firm that runs a labor program increases suppliers' records, including written policies and reported implementation, more than their conduct.*

3 Data and Measurement

The study integrates five data sources. I first construct the buyer–supplier universe from `FactSet Revere` relationships observed during 2003–2022. I then link firms to each outcome source separately to avoid conditioning inclusion on whether a supplier has an ESG rating, appears in ESG incidents, or is observed by a regulator.

Buyer-Supplier Relationship `FactSet Revere` identifies buyer–supplier linkages among both public and private firms worldwide from company filings, investor presentations, press releases, and other disclosed sources. For each relationship, the database reports a unique relationship identifier, contract start and end dates (recorded at the daily level), and the headquarters location of both firms. For a subset of relationships, it also records the share of the supplier's revenue attributable to the focal buyer.

I expand each observed relationship into annual buyer–supplier records. The resulting 2003–2022 panel contains 2,721,915 relationship-years, 822,505 unique buyer-supplier pairs, 101,994 suppliers, 162,490 buyer firms across 185 supplier home countries, and 458,028 supplier-years. Relationship coverage expands substantially after 2006. Restricting the sample to 2007–2022 retains 2,527,276 relationship-years, 774,782 buyer–supplier pairs, 99,606 suppliers, 159,437 buyer firms, and 433,057 supplier-years. The first four years therefore account for less than 8% of relationship-years, and excluding them does not affect the results.

The network is highly skewed: 59.8% of suppliers are observed with one buyer firm,

while the mean degree is 8.1.[2] Dyad-level analyses would disproportionately weight highly connected suppliers. Supplier-level outcomes are analyzed at the supplier-year level, while relational outcomes are analyzed at the buyer–supplier-year level.

Supplier Commitments and Records To measure firm-level labor records, I rely on `ASSET4` at the item level: 70 binary indicators of whether a firm has adopted a particular practice. The panel contains over 6,920,000 firm-item-years across 14,004 firms. I use 70 items from 2002 to 2022, although individual items are not populated for every firm-year. The result does not depend on which items enter the measure: excluding the three program items, splitting the 70 items into two disjoint halves, or weighting items by rarity yields coefficients between 0.017 and 0.019 (Table B.1).

I measure suppliers' documentary records in two ways. First, *Items Reported* is the share of the 70 ESG items for which the vendor records any answer in a given year. This measures how much a firm is documented in a given year. Second, *Items Answered Yes* is the mean recorded value across answered items.[3] This measures how much a firm adopts (items answered "yes") among the recorded items. The two measures are distinct. A firm can be extensively documented while having adopted relatively few practices. For the entry analysis, I use both measures.

The main outcome variables are suppliers' records on *Rules for Own Suppliers* and *In-house Policies*. *Rules for Own Suppliers* records include policies, training, improvements, and breaches involving suppliers or contractors. More specifically, I use three health-and-safety items (a supply-chain health-and-safety policy, supply-chain health-and-safety improvements, and supply-chain health-and-safety training) and two human-rights items (human-rights requirements for contractors and reported contractor breaches). On the other hand, *In-house Policies* include corresponding commitments and practices involving the

[2]The median degree of 4 reported in Table A.5 pertains to the 11,418 suppliers ever ESG-assessed, who are substantially better connected than the full supplier universe.

[3]Among firms reporting at least a quarter of the rating items, the two measures correlate at 0.28, while the correlation rises to 0.58 among the most fully rated firms.

firm's own workforce, such as four health-and-safety items (an employee health-and-safety policy, a health-and-safety team, OHSAS 18001 certification, and health-and-safety training) and two human-rights items (a human-rights policy and a commitment to ILO or UN human-rights principles). This distinction helps examine whether a first contract produces rules for own suppliers or broader changes in the supplier's labor records.

ESG measures often differ across data vendors because of differences in scope, measurement, and aggregation (Berg, Kölbel and Rigobon, 2022). I attempt to address these concerns by using individual `ASSET4` items with fixed coding rules rather than composite ESG ratings. This allows me to measure firms' reported commitments and formal policies using binary indicators rather than relying on vendors' weighted evaluations.

Additionally, I measure broader labor-rights records, including prohibitions on child and forced labor and commitments to collective labor rights. While these measures can overlap with own-facing labor policies, child and forced labor tend to be more salient and visible issues. Collective labor rights, by contrast, receive less attention and may be relevant to a smaller set of firms. Treating these variables as secondary benchmarks, I explore whether the documentary response extends to broader labor commitments.

Independently Reported Conduct Measuring conduct on the ground is challenging, requiring a different approach. For documentary outcomes, increased reporting after entry is itself part of the response. For conduct, however, changes in reporting can create a selection problem. Also, although `ASSET4` includes both policy and conduct measures, its conduct variables are more likely to be observed when firms disclose the underlying information. If a first contract changes reporting, these measures may capture changes in observability or visibility rather than shifts in conduct.

To address these concerns, I rely on `RepRisk` to measure conduct. This dataset records ESG incidents identified by media outlets, NGOs, and other external sources. The incidents also include tags for supply-chain incidents and violations of the UN Global Compact

(UNGC) principles. For each firm, I construct the number of social incidents, broader ESG incidents, supply-chain-related incidents, and labor incidents linked to UNGC principles. Of the 101,994 suppliers, 13,913 (13.6%) have at least one recorded `RepRisk` incident. I supplement these data with `ASSET4` conduct measures for which reporting remains stable.

Country-level Labor Rules I measure the country-level ESG instruments using the data collected from *Carrots & Sticks*.[4] The database documents statutory, regulatory, and voluntary instruments through which governments require or encourage firms to report on ESG issues. This includes whether each instrument is mandatory or voluntary and which ESG pillars it covers. Between 2006 and 2022, it records 2,441 instruments across 218 jurisdictions. I hand-code each instrument's ESG criteria from the original reports and classify it across the three ESG pillars. For each supplier-country-year, I construct the cumulative stock of ESG instruments by country based on headquarters information. The resulting panel covers 127 countries represented among suppliers' home countries.

Firm Characteristics I use `Orbis` to measure firm characteristics. Of the 92,642 `FactSet` firms matched to `Orbis`, 45,747 appear as suppliers in the relationship network between 2003 and 2022, yielding 222,847 supplier-years. Firm-level characteristic variables include total assets, return on assets (ROA), solvency, the number of employees, and gearing, which capture firm size, profitability, and indebtedness.[5]

Descriptive Statistics Table 1 summarizes the sample and the coverage of the main data sources. Panel A shows that the supply-chain network data include 101,994 suppliers, 162,490 buyer firms, and 822,505 supplier–buyer relationships across 185 supplier home countries from 2003 to 2022. Among these suppliers, 11,418 are observed in `ASSET4`, and 9,707 have a

[4] https://www.carrotsandsticks.net/

[5] Because the stacked difference-in-differences design absorbs time-invariant firm characteristics through cohort-by-unit fixed effects, firm-level covariates do not enter the primary models.

first contract that can be dated for the analysis. Before a first contract, the annual probability of entering a first contract is 0.120.

Table 1: Sample Scope and Data Coverage

Panel A. Network and estimation units	
Suppliers	101,994
Buyer firms	162,490
Supplier–buyer relationships	822,505
Relationship–years	2,721,915
Supplier home countries	185
Suppliers ever assessed on program items	11,418
Suppliers ever exposed to a buyer with labor programs	37,350
of which in the entry-design event set	9,707
Panel B. Coverage of the supplier universe, by source	
ASSET4 (program items)	11.2%
RepRisk (ESG incidents)	13.6%
Orbis (financials)	43.6%

Notes: The sample spans 2003–2022. Panel A summarizes the supply-chain network. Panel B reports the share of the 101,994-supplier universe observed by each data source at any point through 2022.

Coverage depends on suppliers' network position. Only 1.9% of single-buyer suppliers are `ASSET4` assessed, compared to 50.2% of suppliers serving more than twenty buyer firms. `RepRisk` exhibits a similar pattern. Both of the datasets cover a selected portion of the supply-chain network, with substantially greater coverage among highly connected suppliers (Figures A.1 and A.2). A majority of suppliers assessed are already extensively documented: the median supplier-year has 60 of 70 items answered at the beginning of the panel and 67 by the end (Figure A.3).

Figure A.4 describes the formation of suppliers' first contract with a buyer firm running a labor program. Panel A plots the annual number of first contracts for suppliers, while Panel B distinguishes those arising through new relationships with buyer firms from those generated by program adoption within existing relationships. Most first contracts occur through the formation of new supplier–buyer relationships. I focus on this entry margin in the main analysis and examine program adoption within existing relationships separately.

Additional summary statistics are reported in Appendix A. Tables A.1 and A.2 report

descriptive statistics for the treatments and compare entering suppliers with their not-yet-treated controls before a first contract, respectively. Table A.3 describes the geographic distribution of suppliers, Table A.4 summarizes treatments by cohorts, and Table A.5 describes suppliers' financial characteristics, network position, and sectoral composition.

4 Empirical Strategy

I test my theory using two designs. I estimate discrete-time hazard models of a supplier's first contract with a buyer firm that runs a labor program to examine which suppliers win these contracts (**H1**). Then, I use stacked difference-in-differences designs to examine what changes after the contract (**H2**, **H3**). The treatment is a supplier's first new contract with a buyer firm that already runs a program, meaning that relationship formation and treatment occur at the same time. To test whether the response depends on a new relationship, I also compare this pathway with a second one–existing supplier–buyer relationships in which the buyer later adopts a supply-chain program.

4.1 Supply-Chain-Facing Treatment

The main treatment is a supplier's exposure to a buyer firm that runs a supply-chain labor program. I focus on three supply-chain health-and-safety items. I code a buyer firm as running a program from the first year it is observed holding any of these items and in every year thereafter. This operationalization identifies 5,251 buyer firms in the network. Of these, 4,130 have an observed false-to-true transition, while 1,121 already hold a program when they first appear in the data.

This differs from a launch-only definition restricting programs to buyer firms with an observed transition. Firms enter `ASSET4` when they are first assessed by the vendor, not necessarily when they first adopt an ESG practice. For buyer firms already holding a supply-chain labor program when first observed, the adoption date is unknown, while their program

status is observed. Coding them as not running a program would misclassify their status, especially among firms with more capacity and developed documents that may have adopted these practices before entering the data. I code these buyer firms as running a program from their first observed year, recognizing that the true adoption date may be earlier. This can make measured exposure occur later than the true onset and should, if anything, attenuate the estimated effects. I use observed program status for the main supplier-side analysis and report launched-only coding as a robustness check.[6]

4.2 Selection into Contracts

I estimate a discrete-time hazard of first contract with a buyer firm that runs a program. The risk set consists of supplier-years in the network from a supplier first observed year through the year of its first contract, after which it leaves the risk set. Because both documentary records come from `ASSET4`, this analysis is restricted to supplier-years in which the vendor assessed the supplier in the prior year. Let E_{st} equal one in the year supplier s is first linked to a buyer that holds the supply-chain program.

$$E_{st} = \beta_1 \, Reported_{s,t-1} + \beta_2 \, Yes_{s,t-1} + \mathbf{X}'_{s,t-1}\boldsymbol{\gamma} + \delta_t + \mu_{c(s)} + \eta_{j(s)} + \varepsilon_{st}, \qquad (1)$$

where *Reported* is the share of the 70 items the supplier reports and *Yes* is the share of reported items answered yes, both standardized over the risk set. $\mathbf{X}_{s,t-1}$ includes log total assets, log employees, and the supplier's number of buyer firms. δ_t are year fixed effects, and $\mu_{c(s)}$ and $\eta_{j(s)}$ are fixed effects for the supplier's home country and industry. All covariates are measured before the exposure year by lagging one year. Standard errors are clustered by supplier.

[6] Launched-only coding produces a smaller but still significant effect of a first contract (Table B.2).

4.3 The Effects of a First Contract

I use the first time a supplier forms a relationship with a buyer that already has a supply-chain program as the treatment. I include only suppliers observed before the new relationship forms so that the event does not capture their first appearance in the network. This results in 11,061 first contracts. I then exclude 1,354 suppliers that had already been exposed to another buyer's program. This leads to 9,707 suppliers whose new relationship marks a first contract.

With this sample pool, I construct event cohorts from 2006 to 2017. The endpoints are determined by the support required for the stacked design. On the pre-treatment side, I require my cohort to provide four years of outcomes after the panel begins in 2002. For each cohort k, comparison suppliers remain untreated through $k+4$. I set the last eligible cohort as 2017 because no future-treated controls are available for later cohorts. I apply these restrictions to each cohort stack, so suppliers treated in later years can serve as controls for earlier cohorts. To ensure that no supplier treated within the event window serves as a control, I use not-yet-treated firms that remain untreated through $k+4$ as controls. I exclude never-exposed suppliers to keep the comparison within the population of suppliers that eventually win a first contract with a buyer firm running a labor program. This is crucial given that many never-exposed firms appear sparsely in the network and differ substantially in network embeddedness. Of the 9,707 eligible suppliers, 4,650 have first contracts within the cohort window. 1,884 of them have outcome data within four years of the contract. For the main outcome, the stacked sample contains 1,277 treated and 1,131 comparison suppliers observed in the year before the contract.

I estimate a stacked difference-in-differences model.[7] For each cohort $k \in [2006, 2017]$, treated units are suppliers with $g_s = k$, and controls are suppliers with $g_s > k+4$. The

[7] I assess robustness using cohort-weighted stacked DiD (Wing, Freedman and Hollingsworth, 2024), the Callaway–Sant'Anna estimator with comparisons within prior-disclosure quintiles (Callaway and Sant'Anna, 2021), and the fixed-effects counterfactual estimator (Liu, Wang and Xu, 2024). See Section 7 and Appendix Figure K.1.

cohort-specific stacks are estimated jointly:

$$Y_{skt} \;=\; \alpha_{sk} \;+\; \lambda_{q_{sk}kt} \;+\; \beta\big(D_{sk} \times \mathbf{1}\{e_{skt} \geq 1\}\big) \;+\; \varepsilon_{skt}, \tag{2}$$

where Y_{skt} is the supplier outcome, measured as the mean of the relevant item family, including a formal policy, reported improvements, and reported training related to labor practices. α_{sk} are cohort $\times$ supplier fixed effects, and $\lambda_{q_{sk}kt}$ are baseline-quintile $\times$ cohort $\times$ year fixed effects. The stratifier q_{sk} sorts suppliers by how much they had already documented before the contract. I measure documented records in the year before the contract ($e_{skt} = -1$) as the share of answered assessment items recorded as present. I then divide suppliers into quintiles and compare treated suppliers only with controls in the same quintile. Documentary intensity uses all 70 items, while the outcome uses only the three supply-chain health-and-safety items. Standard errors are clustered by supplier because suppliers appear in multiple years and may enter more than one cohort stack.

4.4 New and Existing Buyer Firms

A supplier can also first become exposed when a buyer firm it already has a contract with adopts a labor program. To compare the two pathways, I estimate them in the same stacked difference-in-differences model. For each supplier with no prior exposure, I identify the first exposure year and whether it occurs through a new relationship with a buyer firm that already runs a program or an existing buyer later adopting a supply-chain program. I assign suppliers whose first exposure runs through both a new and an existing buyer firm in the same year to the new-relationship group. Then, I compare suppliers in either treatment group with the same not-yet-exposed controls using the cohort construction, event window, stratification, and fixed effects of Equation 2, generating the following specification:

$$Y_{skt} = \alpha_{sk} + \lambda_{q_{sk}kt} + \beta_E\big(E_{sk} \times \mathbf{1}\{e_{skt} \geq 1\}\big) + \beta_W\big(W_{sk} \times \mathbf{1}\{e_{skt} \geq 1\}\big) + \varepsilon_{skt}. \tag{3}$$

where E_{sk} and W_{sk} indicate whether first exposure occurs through entry or within an existing relationship. Because both effects are estimated against the same comparison suppliers, the difference $\beta_E - \beta_W$ tests whether the response depends on a new relationship. The design includes 1,277 suppliers first exposed through a new buyer firm and 277 through an existing buyer firm, with 1,379 comparison suppliers.

5 Results

5.1 Who Wins Contracts

I first examine whether suppliers' ESG records predict their first contract with a buyer firm that runs a labor program by using the share of ESG items reported and the share of ESG items answered yes (**H1**). As the relationship between ESG reports and first contracts could partly reflect differences in supplier capacity or network position, I control for supplier size and the number of buyer firms and include year, country, and industry fixed effects.

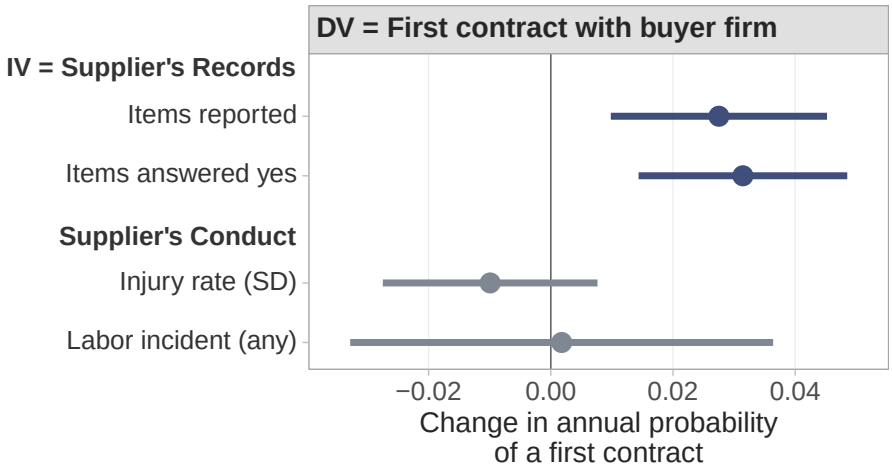
Figure 1 shows that both measures predict a quicker first contract. A one-standard-deviation increase in items reported increases the annual probability of a first contract by 0.028 ($p = 0.002$), and a one-standard-deviation increase in items answered yes raises it by 0.031 ($p < 0.001$).[8] This suggests that suppliers that report more ESG items and have more items answered yes are more likely to contract with buyer firms that have supply-chain health and safety programs. On the other hand, injury rate and labor incidents, which measure supplier conduct, show insignificant coefficients. These patterns are consistent with my argument that buyer firms screen suppliers based on observable evidence of compliance.[9]

One possible explanation for the lack of an association between labor conduct and con-

[8]While I fix the measurement universe at the same 70 items reported in `ASSET4` from 2002 to 2022, the results remain unchanged across alternative definitions of ESG items (Table B.1).

[9]Table C.3 examines public records of conduct focusing on U.S. suppliers by using the Occupational Safety and Health Administration (OSHA) data on inspections and injury. I find that neither violations nor injury rates reported by OSHA predict a first contract, while items answered yes still do.

Figure 1: Records, Conduct, and a First Contract



Note: The figure reports estimates from a discrete-time hazard model of a supplier's first contract with a buyer firm that already runs the three-item supply-chain health and safety program. Items reported is the share of the 70 ESG items for which the supplier records any answer. Items answered yes is the share of answered items recorded as present. Injury rate is the supplier's own-workforce injury index from `ASSET4` and labor incident is any health and safety or employment-conditions incident in `RepRisk` in the three previous years. All variables are measured before the contract year, and the record measures and injury rate are standardized. The model controls for incidents of any kind, assets, employees, and the number of buyer firms, with year, country, and industry fixed effects. Standard errors are clustered by supplier. The full models are reported in Table C.1.

tractual entry is that documentary records already capture some of this information. If suppliers with better working conditions also report more and have more items answered yes, records may partly account for the effect of conduct. Yet, the data show otherwise. Table C.2 shows that neither record measure is correlated with the injury rate from `ASSET4` (−0.09 for items reported and 0.02 for items answered yes). Items answered yes are instead *positively* correlated with labor incidents from `RepRisk` (0.27), and this correlation remains positive net of size, number of buyer firms, and fixed effects (0.13). The share of labor policies a supplier holds is also uncorrelated with its injury rate (−0.09). Thus, documentary records and reported labor incidents need not closely correspond, which may help explain why observed labor conduct plays a limited role in supplier selection.

One concern is that the estimates are driven by a few highly documented suppliers rather than a relationship across the distribution. I replace the two measures with quintile indicators, entered together in the full model with the bottom fifth as the reference (Figure

C.1). The two measures show different patterns. For items reported, the gap emerges at the bottom of the distribution. Suppliers in the second fifth are no more likely to win a first contract than those in the bottom fifth, but suppliers in each of the top three fifths are about 4 to 5 percentage points more likely, roughly half again the bottom fifth's yearly chance of 0.095, and the top fifth does not differ from the third. Because most suppliers report most items, these fifths span a narrow range, from 61 or fewer of 70 items in the bottom fifth to 67 or more in the top. For items answered yes, the chance rises across the distribution and is steepest at the top: suppliers in the fourth fifth are 3.5 percentage points more likely than the bottom fifth, and those in the top fifth 7 points more likely. These results suggest that the estimates are not driven by a few suppliers. Suppliers with gaps in their records fall behind, but once a supplier reports most items, reporting more adds little. Among those suppliers, the ones with more policies in place are more likely to win a first contract.

Overall, these findings demonstrate that ESG records are associated with winning contracts with buyer firms running labor programs. This raises a question of what happens after the contract. This may create a feedback process if contracts lead suppliers to expand their labor records. Alternatively, suppliers may expand only the records their buyer firms can verify and require of them. In the next section, I examine these possibilities.

5.2 What Changes After a First Contract

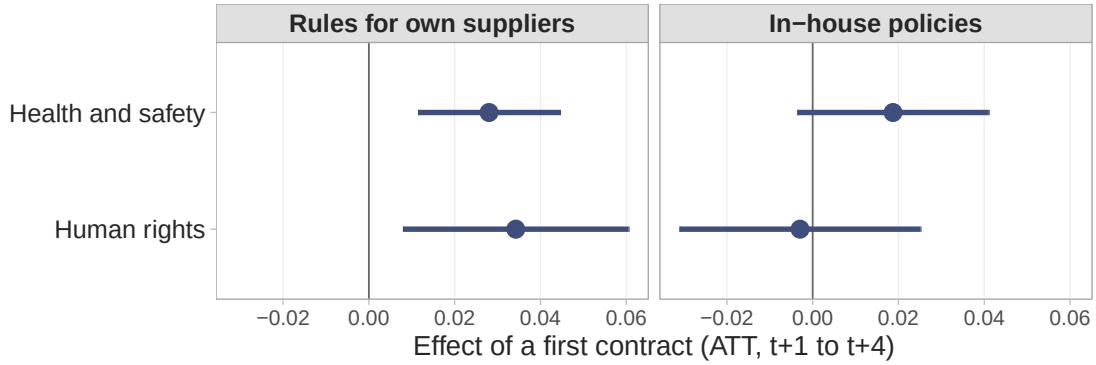
To test **H2A**, I examine how a first contract with a buyer firm that runs a supply-chain health-and-safety program changes two kinds of supplier records: rules for the supplier's own suppliers and policies for its own workplace, each in health and safety and in human rights. The health-and-safety outcomes are the same `ASSET4` items used to define the buyer firm's program, whereas the human-rights outcomes are not used to define treatment.[10]

Figure 2 reports the average effect over the four years after the contract. The left panel

[10]It is worth noting that buyer firms adopt supply-chain human-rights and health-and-safety programs at nearly the same time.

shows that a first contract increases health-and-safety rules for suppliers' own suppliers by 0.028 ($p = 0.001$) and human-rights rules by 0.034 ($p = 0.011$). For most suppliers, this represents a new role: about four in five had no health-and-safety rules for their own suppliers before the contract. Among suppliers with neither these rules nor in-house health-and-safety commitments, 26 percent held rules four years after the contract, compared with 10 percent of comparison suppliers (Figure D.1). The response is specific to labor policies, with no significant changes in governance (0.011, $p = 0.19$) or environmental policies (Appendix Figure D.2). The estimates are similar under three alternative estimators (Section 7).

Figure 2: First Contract, Suppliers' Rules for Own Suppliers, and In-house Policies

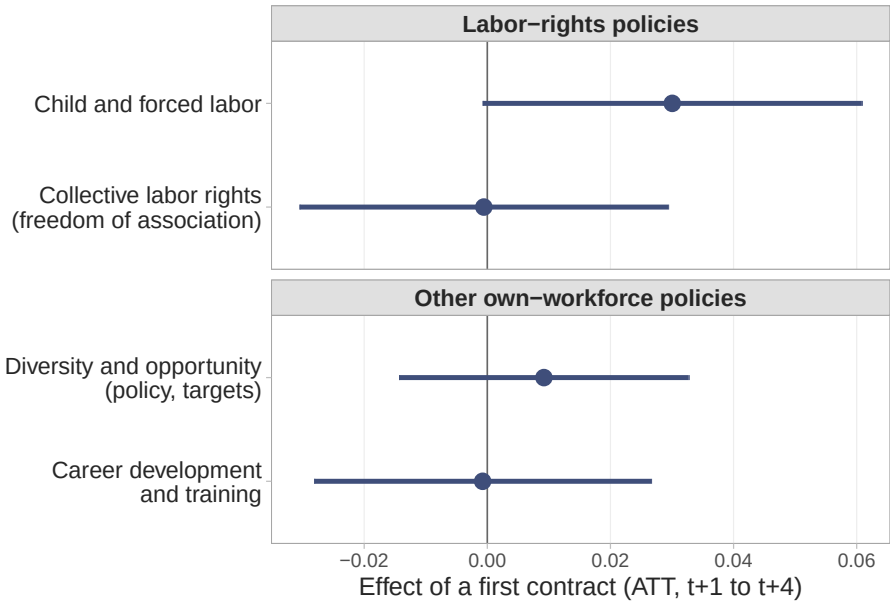


Note: Average effect of a first contract with a buyer firm that runs a labor program over $t+1$ to $t+4$ with 95% confidence intervals. The stacked design includes supplier × cohort and disclosure-quintile × cohort × year fixed effects, with standard errors clustered by supplier. Event plots and additional estimators are reported in Figure D.3 and Table K.1, respectively.

In-house policies show smaller changes. The right panel of Figure 2 and Table D.1 show a positive but statistically insignificant increase in health-and-safety policies and no change in human-rights policies. The weaker in-house response partly reflects where suppliers start. Among suppliers with the same pre-contract adoption levels, in-house health-and-safety policies increase by 0.025 ($p = 0.025$), while the estimates for rules concerning their own suppliers remain similar (Figure D.4). Few treated suppliers hold requirements for their own suppliers without corresponding in-house commitments (1 percent before the contract and at most 3 percent four years afterward). These results suggest that the expansion of rules for suppliers' own suppliers does not appear to come at the expense of in-house commitments.

The above results suggest potential spillovers across labor standards. Which standards are more likely to travel? I next examine other fundamental labor rights, including prohibitions on child and forced labor and freedom of association, and other employee management policies. Figure 3 demonstrates the effects of a first contract on suppliers' policies concerning child and forced labor, collective labor rights, workplace diversity and equal opportunity, and employee training and career development. The first two are related to fundamental labor standards and rights, while the latter two focus on broader employee welfare and development and are less prominent in supply-chain labor programs.

Figure 3: First Contract, Labor Rights, and In-house Workforce Policies



Note: Average effect over $t+1$ to $t+4$ with 95% confidence intervals. Child and forced labor averages the child-labor and forced-labor policy items. Diversity and opportunity averages the diversity policy and diversity targets. Career development and training averages career development, skills training, and training and development policies.

I find suggestive evidence that suppliers expand policies addressing child and forced labor, with marginally significant effects in the stacked DiD and statistically significant effects under the Callaway–Sant'Anna estimator. By contrast, collective labor rights do not shift. This pattern is consistent with the argument that collective labor rights are more difficult to strengthen through private labor programs. Unlike prohibitions on child and forced labor,

freedom of association and collective bargaining require firms to recognize workers' independent bargaining power, which may impose additional costs on employers. Previous studies also find that collective labor rights face greater obstacles from employer resistance and domestic institutional constraints (Egels-Zandén and Merk, 2014, Li, Kuruvilla and Bae, 2025). Moreover, child and forced labor are salient issues in supply-chain due diligence, particularly in upstream production (e.g., sub-suppliers, sub-sub-suppliers), generating incentives for firms to adopt policies addressing supply-chain risks. In addition, there is little evidence in policies related to broader employee welfare and development, which may be less salient in supply-chain labor programs.

Together, these results support **H2A**. Following their first contracts, suppliers begin to set labor requirements for their own suppliers, a role most did not previously hold, without replacing their in-house commitments. Furthermore, the response is selective. Prohibitions on child and forced labor increase marginally, while commitments to collective labor rights and broader workforce policies show limited change.

The pattern remains similar when the first contract is defined with buyer firms' supply-chain human-rights programs (Table E.1). Under this alternative treatment, health-and-safety rules for own suppliers increase by 0.028 ($p = 0.001$) and human-rights rules for own suppliers by 0.032 ($p = 0.019$). In-house human-rights policies do not change significantly, while in-house health-and-safety policies rise by 0.031 ($p = 0.012$). Because the health-and-safety and human-rights programs overlap in their adoption timing, I retain health and safety as the primary treatment and use the human-rights program as a robustness check.

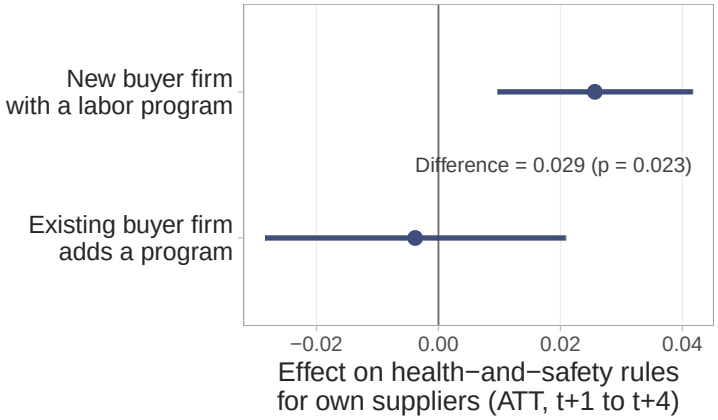
5.3 New or Existing Buyer Firms?

The preceding results focus on a first contract with a new buyer firm that already runs a labor program. I now examine how suppliers respond when existing buyer firms adopt supply-chain labor programs (**H2B**). If the response follows the terms set when a relationship forms, suppliers should respond to the first route rather than the second one. I compare

these two routes in the joint stacked model (Equation 3), which estimates both against the same not-yet-exposed comparison suppliers. In this section, I test this with the results for health-and-safety rules for own suppliers, the outcome of Figure 2.

Figure 4 shows that a first contract with a new buyer firm that runs a labor program increases health-and-safety rules for own suppliers by 0.026 ($p = 0.002$). Yet, I find that the effects are close to zero and not significant when an existing buyer firm adopts a program. The difference between the two routes is 0.029 ($p = 0.023$). Pre-trend tests do not reject parallel trends for either pathway ($p = 0.22$ and $p = 0.39$). The difference is similar when the existing-buyer pathway is estimated with a separate design where exposure is the share of the supplier's buyer firms that adopt a program (0.038, $p < 0.05$, Table F.1). The results point to a stronger role for labor programs when relationships form. Buyer firms can incorporate documentary requirements into a new contract. The same response is not apparent when a program is introduced into relationships already in place.

Figure 4: New and Existing Buyer Firms and Rules for Own Suppliers



Note: Average effect over $t + 1$ to $t + 4$ on the share of the three supply-chain health-and-safety items a supplier holds, with 95% confidence intervals, from the joint stacked model (Equation 3). Both are compared with the same not-yet-exposed suppliers, with supplier × cohort and disclosure-quintile × cohort × year fixed effects and standard errors clustered by supplier. Event-study estimates are reported in Figure F.1.

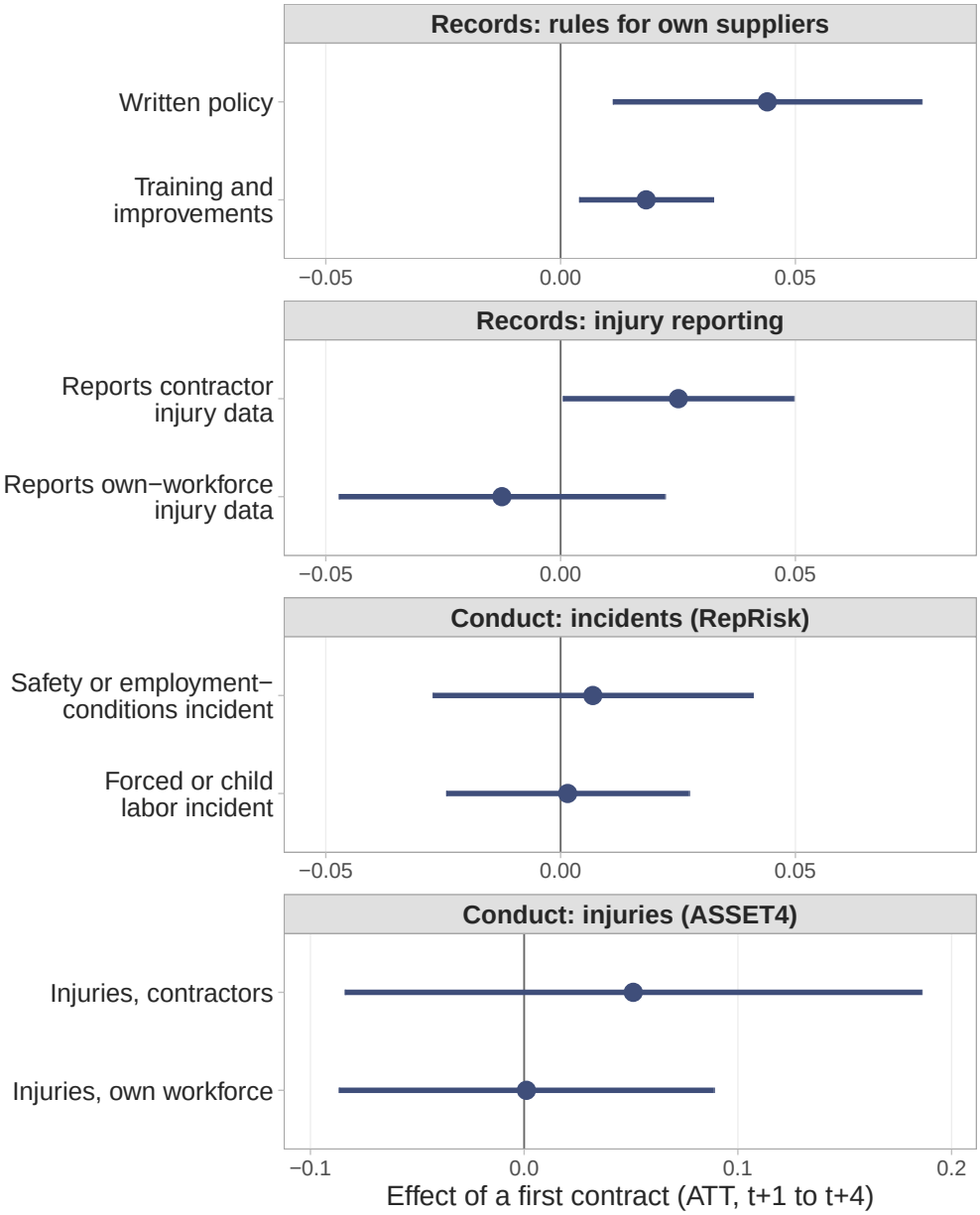
5.4 Records over Conduct

I next examine whether changes in formal labor policies translate into improvements in working conditions (**H3**). The first panel of Figure 5 separates the health-and-safety outcome of Figure 2 into written policies and two implementation measures, including supply-chain health-and-safety training and improvements. A first contract increases the share of suppliers holding the written policy by 0.044 ($p = 0.009$) and implementation records by 0.018 ($p = 0.012$). The written-policy response is more than twice as large, although the difference is not statistically significant. The difference is clearer for first adoption. Among suppliers that had not previously adopted an item, a first contract increases the probability of adopting a written policy by 0.029, compared with 0.006 for an implementation item. The difference is statistically significant ($p = 0.005$, Table G.1). The second panel shows an increase in reporting of contractor injuries (0.025, $p = 0.046$), but no change in reporting of injuries among suppliers' own workers. This indicates greater attention to contractor monitoring after first contracts, as the measures capture whether injury data are reported, not injury rates.

The third panel show little evidence of corresponding improvements in working conditions, which are reported by the media, NGOs, and regulators (`RepRisk`). The probability of any incident in a year does not rise, and neither safety and employment-conditions incidents nor child- and forced-labor incidents shift. The fourth panel examines injury rates reported by suppliers in `ASSET4`. I find no evidence of improvement in contractor injuries or injuries among suppliers' own workers. Union representation also remains unchanged (Figure G.2).

I then examine whether formal requirements lead to further changes among suppliers one tier upstream. Using the same stacked design, I find no significant changes in the in-house labor policies or requirements for their own suppliers among 497 next-tier firms covered by `ASSET4` (Appendix Table G.2). Because these firms are relatively large, the analysis may not capture responses among smaller suppliers targeted by labor programs. I also examine labor incidents among 2,006 next-tier firms covered by `RepRisk`. Safety

Figure 5: First Contract, Written Policy, Implementation, and Conduct



Note: Average effects over $t+1$ to $t+4$ with 95% confidence intervals. The first two panels report records: (1) whether the supplier holds a written supply-chain health-and-safety policy and the share of two implementation items it holds (supply-chain health-and-safety training and improvements), and (2) whether the supplier reports contractor and own-workforce injury data. The last two report supplier conduct: (3) the probability of a `RepRisk` incident of each kind in the year, and (4) `ASSET4` injury indices standardized within year, in standard deviations. Negative estimates indicate fewer incidents. Alternative estimators are reported in Figure G.1.

and employment-conditions incidents increase slightly (0.022, $p = 0.053$), as do child- and forced-labor incidents (0.014, $p = 0.036$), while environmental and governance incidents show no significant changes (Table G.3). These increases may reflect greater scrutiny because reported incidents depend on both underlying violations and their detection. Overall, I find little evidence of further policy adoption or improvements in working conditions among the next-tier firms observed in the data.

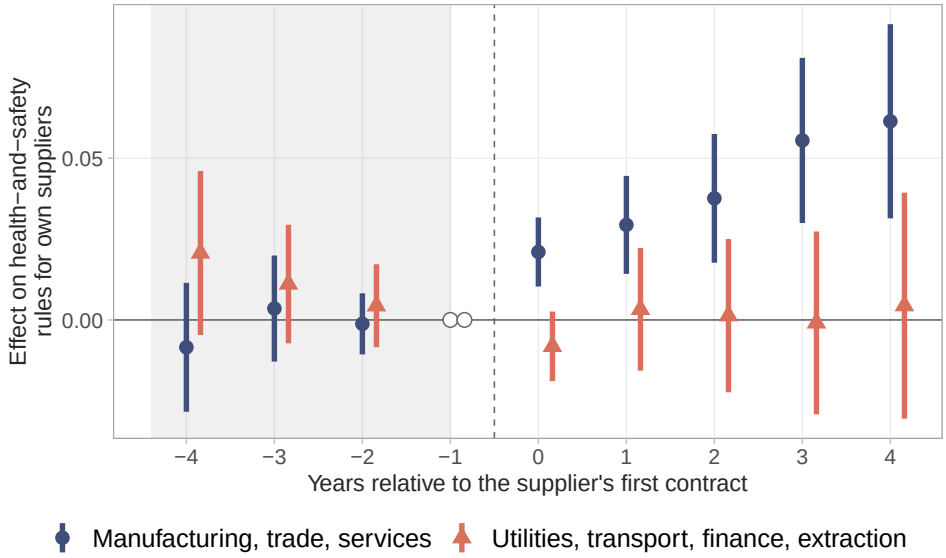
Overall, the findings are consistent with **H3**. First contracts with buyer firms running labor programs increase written policies and reported implementation measures, but show little evidence of corresponding improvements in working conditions or further policy adoption among observed next-tier suppliers.

5.5 Heterogeneous Effects and Additional Exposure Tests

Sector Heterogeneity My theory assumes that buyer firms face risks arising from their suppliers, suggesting that responses should be stronger in sectors where supply-chain labor risks are more salient. Figure 6 estimates the effect of a first contract by the supplier's sector, comparing treated suppliers with comparison suppliers in the same sector. Among suppliers in manufacturing, trade, and services, a first contract increases labor rules for own suppliers by 0.046 ($p < 0.001$) and the written policy by 0.078 ($p = 0.001$). Among suppliers in utilities, transport, finance, and extractive industries, neither changes, and the differences are statistically significant ($p = 0.002$ and $p = 0.006$). In-house health-and-safety policies do not differ by sector. The response is also larger after a first contract with a consumer-facing buyer firm, but this difference is sensitive to pre-contract trends, and I treat it as suggestive (Figure I.1).

Supplier-Country Development I examine whether the documentary response varies with suppliers' home-country context by distinguishing suppliers headquartered in OECD and non-OECD countries. OECD membership provides a proxy for economic and institu-

Figure 6: Rules for Own Suppliers by the Supplier's Sector



Note: Event-time coefficients for health-and-safety rules for own suppliers, estimated separately for suppliers in manufacturing, trade, and services (SIC 20–39, 50–59, 70–89) and suppliers in utilities, transport, finance, and extractive industries (SIC 01–17, 40–49, 60–67). Bars are 95% confidence intervals. Standard errors clustered by supplier.

tional development, which may influence suppliers' documentary records. Table I.1 shows nearly identical estimated responses across the two groups. First contracts with buyer firms operating labor programs increase records of rules for own suppliers by 0.028 among both OECD and non-OECD suppliers. The non-OECD estimate is less precise, reflecting the smaller number of treated suppliers, but the estimated difference between the two groups is statistically insignificant. The documentary response does not appear to be concentrated among suppliers in either OECD or non-OECD countries.

Program Intensity Suppliers may respond to the extent of the buyer firm's program they face rather than the relationship itself. If so, the response should rise with the number of buyer firms' labor programs. To test this, I investigate the effects of the number of the three supply-chain health-and-safety practices held by the buyer firm in the supplier's contract year. Table I.2 shows that the estimated response per additional practice is small and insignificant. The estimate remains similar after controlling for the buyer firm's overall

position, measured across all 70 assessment items. Summing program items across all buyer firms in first contracts shows a larger estimate (0.025, $p = 0.015$), but this measure also may reflect the number of new relationships. Once I control for this number, the estimate becomes smaller and is no longer statistically significant. Thus, I find little evidence that suppliers respond more strongly to more extensive labor programs.

Repeated Exposure I next examine whether suppliers adopt additional policies after contracting with a second buyer firm running a labor program. I re-index the design around the second contract and compare suppliers that form a second relationship with those that have not yet done so. Table I.3 shows the results. The initial estimate is positive (0.0249, $p = 0.017$). However, this result may reflect continued adjustment after the first contract. Once I account for time since the first contract with a buyer firm that runs a labor program, the estimate becomes smaller and insignificant. Matching suppliers on the time between their first and second contract produces the same result. These findings provide limited evidence of additional policy adoption following a second relationship.

Cross-Border Relationships Research on trade and labor standards expects standards to travel through ties across countries, as buyer firms in stricter markets pass their requirements to foreign suppliers (Greenhill, Mosley and Prakash, 2009, Neumayer and De Soysa, 2005). Yet, I find that the response does not depend on whether the relationship crosses a border. Among suppliers whose new buyer firms with programs are all domestic, all foreign, or both, a first contract increases health-and-safety rules for own suppliers by 0.031, 0.021, and 0.042, respectively, and neither the all-foreign nor the mixed group differs significantly from the all-domestic group (Table I.4). Rules for own suppliers travel with the relationship, whether or not it crosses a border.

6 Alternative Explanations

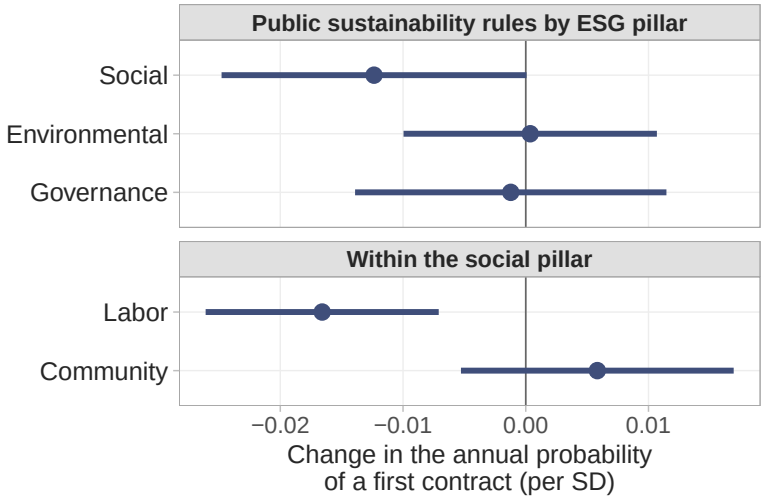
6.1 Who Enters: Country Institutions

Public Labor Rules An alternative explanation is that public rules on labor practices, rather than supplier firms' own records, attract buyer firms that run labor programs. When governments implement labor rules, suppliers may be more closely monitored regardless of their own commitments, which leads buyer firms to prefer them. I test this explanation using the stock of national labor-related instruments in the Carrots & Sticks database of ESG sustainability reporting instruments in the supplier's country.[11] Figure 7 shows that the evidence runs the other way. Among the three ESG dimensions, social instruments, which cover labor, community, and gender, have a marginally negative association with a first contract (-0.012, $p = 0.056$), while environmental and governance instruments show no association. Within the social dimension, the negative association comes from labor rules: a one-standard-deviation larger stock of labor rules lowers the annual probability of a first contract by 0.017 ($p = 0.001$). Importantly, the association is unchanged when suppliers' records are held fixed.

This negative association is concentrated for buyer firms running labor programs. Table J.2 shows that a one-standard-deviation increase in public labor rules is associated with an 11 percent decline in the probability of forming a new tie with a buyer firm that runs a labor program, compared with a 3.9 percent decline for other types of buyers. The difference is statistically significant ($p = 0.001$) and remains similar after controlling for GDP per capita. This suggests that public labor rules do not increase the formation of relationships with buyer firms running private labor programs. One possible explanation is that these buyers face greater reputational exposure when suppliers' labor practices are subject to public scrutiny. However, this mechanism cannot be directly tested with the current data.

[11] Of the 134 labor instruments with a national issuer, about two-fifths are disclosure requirements and the rest impose substantive duties or provide guidance.

Figure 7: Public Sustainability Rules and a First Contract



Note: The figure reports a discrete-time hazard model of a supplier's first contract with a buyer firm adopting a labor program, estimated on supplier-years at risk up to and including the year of first exposure. Firm-level variables, including prior ESG position, ESG items reported, assets, employees, and buyer count, are lagged by one year. All specifications include year fixed effects. Standard errors are clustered by supplier country. Country-level measures are standardized. Full estimates are reported in Table J.1.

State Capacity Another alternative explanation is that buyer firms rely on suppliers' institutional environment, such as state capacity or the quality of public institutions, to assess labor risks of the suppliers. Buyer firms running labor programs may prefer suppliers in countries with stronger public institutions that enforce labor regulations: where states enforce labor regulations more effectively, suppliers may appear less risky regardless of their own policies. In order to test this, I examine how supplier countries' rule of law, government effectiveness, and statistical performance affect their first contracts with buyer firms. Table J.3 shows that although suppliers in countries with stronger institutions are initially more likely to form contracts with buyer firms running labor programs, these associations become small and insignificant after controlling for GDP per capita. More importantly, suppliers' records are uncorrelated with rule of law ($r = -0.04$), and their coefficients remain unchanged across specifications, including models with supplier-country fixed effects.

6.2 What Changes After the Contract

Buyer Firm Leverage Buyer firms may have greater influence over suppliers that are financially vulnerable or economically dependent on them. If so, the response should be stronger among suppliers that are financially weaker or more dependent on the admitting buyer firms. To test this, I use supplier fundamentals–return on assets (ROA), gearing, solvency, firm size, and the number of employees–as proxies for suppliers' capacity to resist buyer demands. I also examine leverage within the relationship more directly using the supplier's revenue dependence on the admitting buyer and relative buyer–supplier size.

Table J.4 provides little support for this mechanism. Panel A shows heterogeneous effects by suppliers' financial measures. Comparing subgroups, I find that the response is larger among suppliers with higher ROA ($p = 0.031$) and larger assets ($p = 0.055$), although the latter is marginally significant. The response does not increase with revenue dependence or the asset gap between buyer firms and suppliers, although I interpret this cautiously due to the limited data availability. The asset-gap estimates lean toward larger responses among suppliers facing buyer firms closer to their own size, although the difference is not significant.

Overall, the response in suppliers' records is not stronger among suppliers facing greater financial vulnerability or buyer leverage. Yet, this does not imply that buyer power is irrelevant to supplier compliance. While threatened penalties enhance suppliers' audit compliance within established relationships (Amengual and Distelhorst, 2025), the outcome here is the documentary response at entry. The response is not stronger where buyer firms have more leverage, and is, if anything, larger among more profitable and larger suppliers.

Any Large New Buyer Firm The response may reflect acquiring a large new customer rather than the buyer firm's labor program. To examine this possibility, I examine suppliers forming their first contracts with buyer firms without the labor-program items (Figure J.1). I find little change in rules for suppliers' own suppliers (0.004, $p = 0.85$), compared with an increase of 0.031 ($p = 0.001$) following contracts with program buyers. It is worth noting that

the difference is not statistically significant ($p = 0.19$), and buyer firms without programs are smaller and have less extensive ESG records. Thus, I interpret these results cautiously. The evidence is consistent with a stronger response to buyer firms running labor programs, although the difference is not statistically significant.

Copying the Buyer Firm's Items One alternative explanation for the findings is imitation. Suppliers may adopt the specific programs already held by their admitting buyer firms. To test this mechanism, I use a supplier–item–year panel, with item × year fixed effects absorbing the common diffusion of each practice. Table J.5 shows the results. *Any Held* indicates that at least one admitting buyer holds the focal item, while *Share Held* measures the share of admitting buyer firms that do so. As shown in the first two columns, I find no significant relationship between the items held by admitting buyer firms and the items suppliers adopt after entry. This suggests that the response is broader within rules for own suppliers rather than copying the buyer firm's items.

Delayed Change Changes in conduct may take longer than policy adoption, obscuring improvements in working conditions. To investigate this possibility, I extend the analysis to eight years. I find that rules for suppliers' own suppliers continue to increase (0.033 in years 1–4, 0.076 in years 5–8, $p < 0.001$), while injuries among suppliers' own workers show no significant decline (Table J.6). Safety and employment-conditions incidents show no significant change. Child- and forced-labor incidents decline in years 5–8 (-0.055), although the estimate is imprecise ($p = 0.10$).

7 Robustness Checks

Estimator choice In the main analyses, I use a stacked difference-in-differences design that compares suppliers with similar prior disclosure. Tables K.1 and K.2 report five alternatives: the stacked design without disclosure groups, the stacked design with cohort

weights (Wing, Freedman and Hollingsworth, 2024), the Callaway–Sant'Anna estimator with and without conditioning on prior disclosure, and the fixed-effects counterfactual estimator. Across all six specifications, health-and-safety rules for suppliers increase (0.024–0.040, all $p < 0.05$). Human-rights rules also increase, although the estimate is insignificant with cohort weights. Several estimators that do not account for prior disclosure show significant pre-trends, supporting the use of baseline disclosure groups in the main specification.

Specification Choice I check whether the pooled entry effect is robust to alternative specifications, event windows, and clustering choices. Figure K.2 compares the baseline-stratified estimate with a specification without disclosure groups and with three- and five-year event windows. I also cluster standard errors by buyer firm, assigning each supplier to one program buyer in its first-contract year. About one-third of suppliers have multiple program buyers that year. Restricting the sample to suppliers with only one program buyer, for whom the assignment is unambiguous, leaves the standard errors unchanged. In addition, using two years before the contract as the reference, or estimating each cohort separately, results in similar estimates (Table K.3 and Figure K.3).

8 Conclusion

As firms are held responsible for labor conditions at suppliers they do not own, buyer firms rely on their direct suppliers to oversee firms further upstream. This paper examines how that responsibility passes across firms and what private labor programs change. I argue that documentary commitments shape both the formation of buyer–supplier relationships and suppliers' responses to them. Suppliers with more extensive records are more likely to form contracts with buyer firms running labor programs, while injury rates and reported labor incidents do not predict these relationships. Following a first contract, suppliers adopt labor rules for their own suppliers. Written requirements can be extended through contracts and assessed by outside actors, but changing workplace practices is more difficult. Suppliers adopt

formal policies and report more implementation, yet injuries and independently reported labor incidents on the ground show no improvement. Among observed next-tier firms, policies do not expand, and labor incidents do not decline. These findings suggest that private labor programs extend formal responsibilities across firms without producing comparable improvements in working conditions.

These results speak to broader debates about the scope of private labor regulation. The spread of supplier codes, reporting requirements, and monitoring systems is often considered evidence of buyer firms expanding responsibility across supply chains. Yet, less is known about how and when suppliers respond to such programs. I show that suppliers become rule setters for firms further upstream by adopting and passing on written requirements. The response comes when relationships form, which suggests that labor programs work through the composition of supply chains as much as through pressure on firms already inside them. Thus, responsibility extends through production networks partly by passing from one firm to the next, as written obligations. While previous studies have focused on decoupling within individual firms, this paper shows how the gap between formal commitments and actual practices can extend across supply-chain relationships.

The findings also have implications for the relationship between public and private labor regulation. Public labor rules in a supplier's country do not make contracts with buyer firms that run labor programs more likely. Suppliers in countries with a larger stock of such rules are less likely to win these contracts, and this association does not run through suppliers' records. Future work can further investigate whether public and private rules substitute for each other where relationships form, or whether buyer firms with programs avoid places where labor practices face public scrutiny.

Although this paper focuses on labor regulation, the theory extends to other regulatory domains. Buyer firms face similar pressures regarding environmental practices, anti-corruption measures, and other standards across their supply chains. In such settings, buyer firms are held responsible and also depend on their direct suppliers to manage risks from

supply chains. I expect suppliers to set rules for their suppliers and formal requirements to spread more easily than changes in the practices they are intended to regulate.

Several questions remain for future research. First, changes in buyer firms' requirements that are unrelated to supplier characteristics could help distinguish the effects of labor programs from supplier selection and the broader effects of acquiring a large customer. Second, the analysis of next-tier firms (e.g., sub-suppliers, sub-sub-suppliers) is more observable among large firms. Additional data on smaller suppliers and their working conditions can help understand how far labor rules spread and how suppliers further upstream respond.

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Appendix

Passing the Rules Down: How Labor Programs Spread Without Protecting Workers

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A Sample and Descriptive Statistics

Figure A.1: ESG Coverage by Supplier Network Position

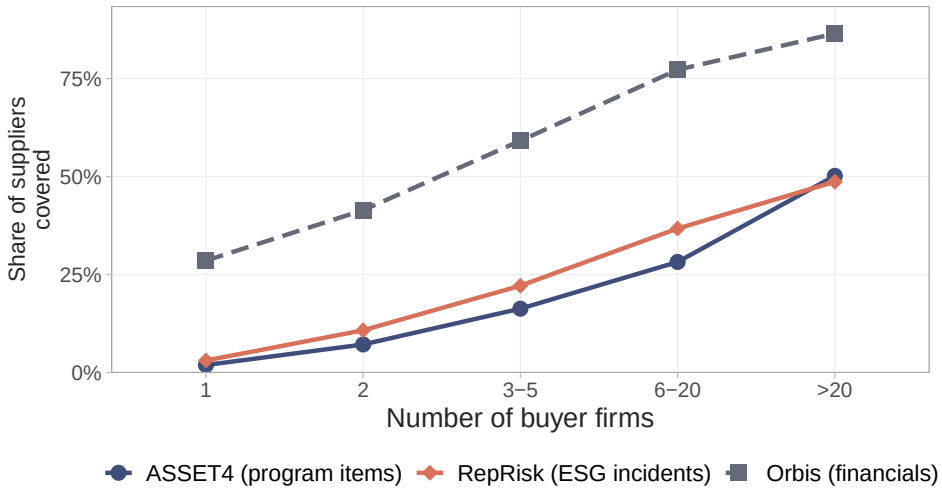
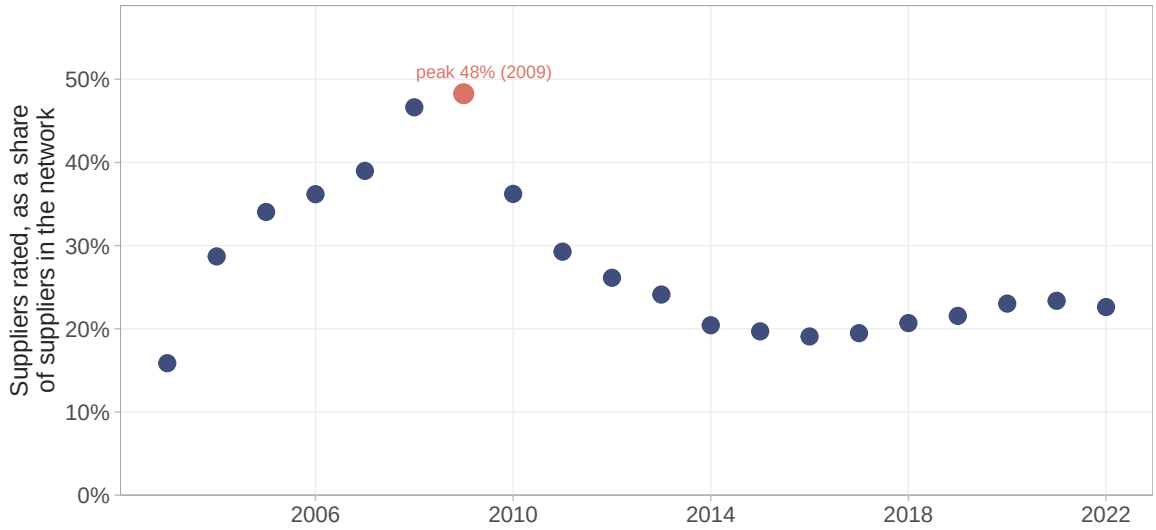
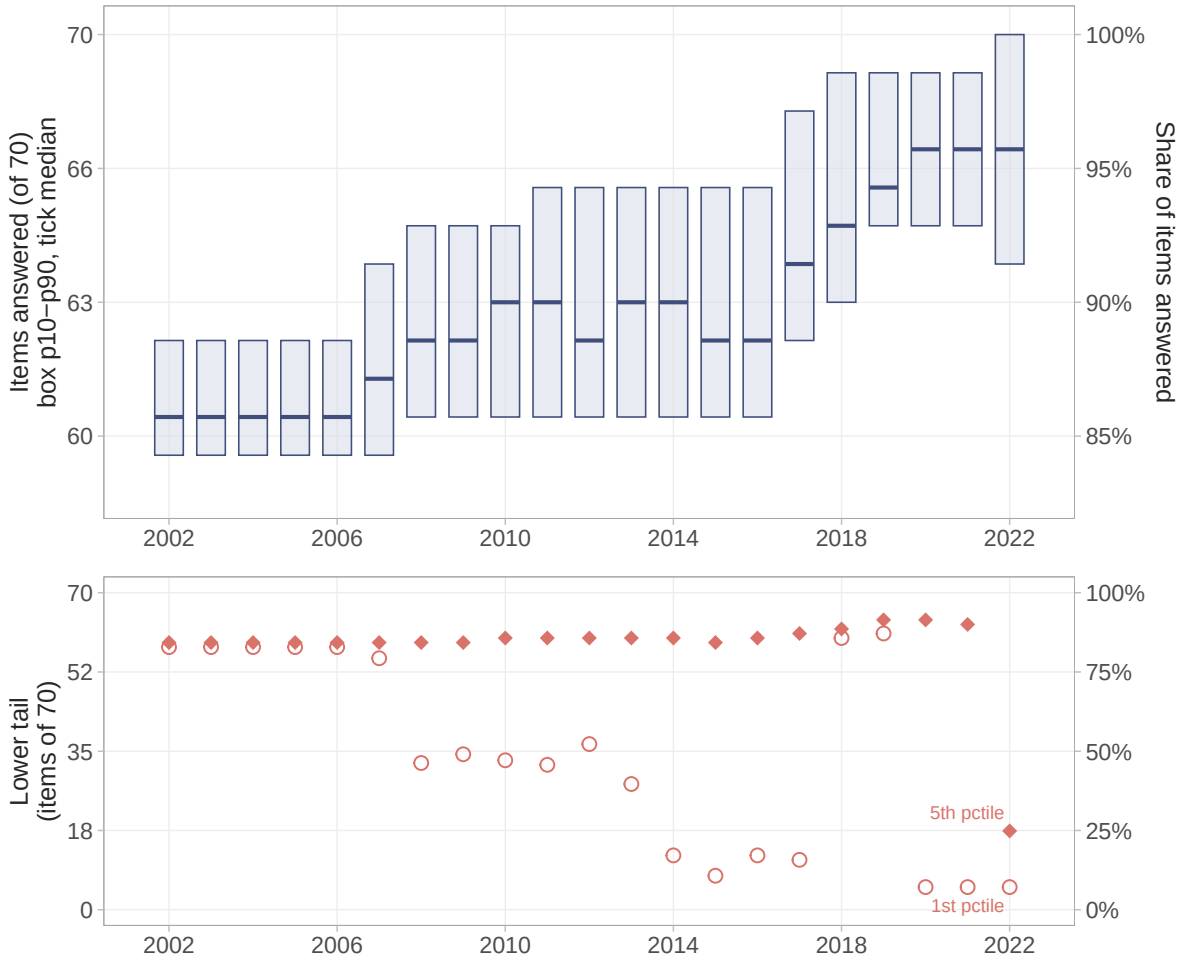


Figure A.2: Vendor Coverage of the Supplier Network



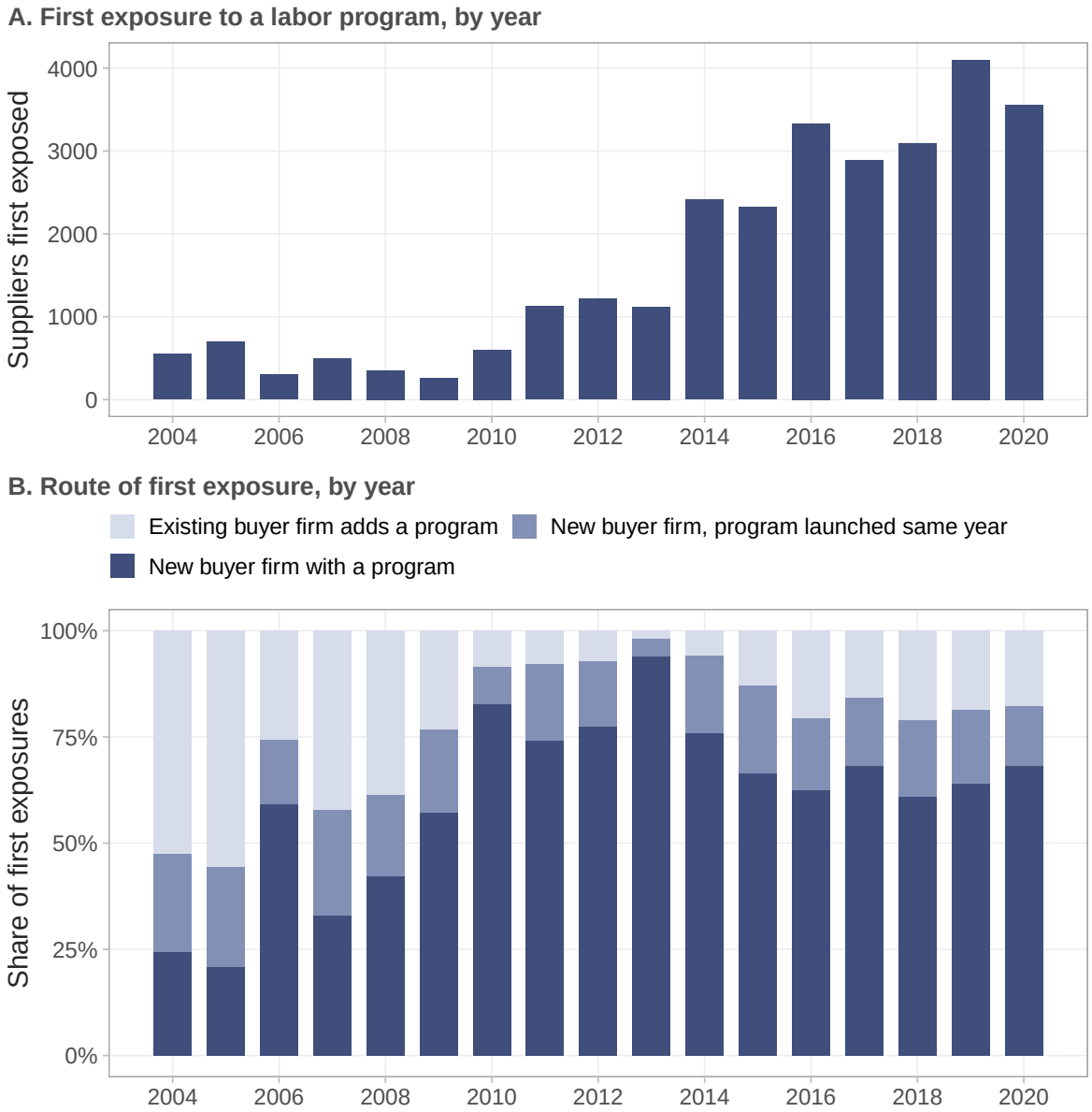
Note: Share of suppliers in the network rated by the vendor in each year. Because the item universe is fixed at 70 items in every year, changes in coverage do not reflect changes in the set of items evaluated.

Figure A.3: The Distribution of Items Reported



Note: The upper panel reports the median number of answered items out of 70, with the 10th–90th percentile range and box limits. The lower panel reports the first and fifth percentiles of the same distribution.

Figure A.4: How Suppliers First Become Exposed to Labor Programs



Note: Panel A reports the annual number of suppliers first linked to a buyer firm that runs a supply-chain labor program. Panel B separates first exposure through a new relationship with such a buyer firm from first exposure through an existing buyer firm that adopts a program.

Table A.1: The Treatment: Buyer Firms Behind a First Contract

Panel A. Buyer firms of suppliers at first exposure	
Buyer firms, mean (median)	7.275 (3.0)
Buyer firms with a program, mean (median)	2.223 (1.0)
Share of buyer firms with a program	0.545
Panel B. The buyer firm with a program	
Items answered yes	0.598
Log assets	22.123
Headquartered in the United States	30.9%
Headquartered in Western Europe	37.2%
Panel C. How suppliers first become exposed	
Through a new relationship: 86% in the cohort window, 82% across 2004–2020 (the period of Figure A.4, which shows the split by year).	

Notes: Panel A covers suppliers first exposed during the cohort window. The analyses of **H2** and **H3** use the subset whose first exposure is a new relationship with a buyer firm that already runs a program. Panel B pools buyer firm–exposure-year observations. Panel C reports how suppliers first become exposed.

Table A.2: Suppliers Before a First Contract and Not-Yet-Treated Comparison Suppliers

Measured at $t-1$	Treated (mean)	Not yet treated (mean)	Normalized difference
Log assets	20.875	20.537	+0.126
Log employees	8.793	8.977	-0.093
Return on assets	4.486	6.082	-0.165
Log number of buyer firms	1.005	0.650	+0.423
Items reported	0.901	0.882	+0.558
Items answered yes	0.388	0.252	+0.713
Health-and-safety rules for own suppliers	0.092	0.043	+0.283
Supplier-cohort observations	1,277	4,528	
Distinct suppliers	1,277	1,131	

Notes: Means in the year before the contract ($t-1$) on the stacked sample of Figure 2. Treated suppliers win a first contract with a buyer firm that runs a labor program in cohort k, and comparison suppliers have no such contract through $k+4$. Means are computed over cohort stacks, so a comparison supplier that serves several cohorts counts once per cohort, as in the regression. The normalized difference is $(\bar{x}_1 - \bar{x}_0)/\sqrt{(s_1^2 + s_0^2)/2}$.

Table A.3: Supplier Home Countries (Top 12)

Country	Suppliers	% of sample	% ESG-assessed
US	27,357	27.0%	13.3%
CN	12,813	12.6%	8.4%
JP	6,821	6.7%	7.4%
GB	4,878	4.8%	14.9%
KR	4,249	4.2%	4.0%
DE	3,634	3.6%	7.4%
CA	3,029	3.0%	14.3%
TW	2,584	2.6%	7.2%
ID	2,440	2.4%	3.2%
IN	2,349	2.3%	24.1%
AU	2,263	2.2%	20.1%
FR	2,144	2.1%	9.7%
Top 12	74,561	73.6%	
All 185 countries	101,301	100%	11.3%

Notes: "% ESG-assessed" is the share of suppliers in each country ever assessed on the program items.

Table A.4: First Contracts by Cohort Year

Cohort	First contracts	Treated suppliers in the stacked sample	Comparison suppliers	Buyer firms with a program
2006	40	26	1,238	40
2007	42	28	1,137	44
2008	48	36	999	44
2009	21	18	894	17
2010	57	46	781	72
2011	184	167	784	257
2012	234	174	691	275
2013	171	140	601	249
2014	223	149	503	310
2015	213	121	407	327
2016	306	182	316	461
2017	345	190	161	496
Total	1,884	1,277	2,010[a]	1,363[a]

Notes: Cohorts span 2006–2017, as in the stacked design. [a]BBecause some suppliers and buyer firms appear in multiple cohorts, the total number of distinct firms is smaller than the sum of the rows.

Table A.5: Characteristics of ESG-Assessed Suppliers and Sectoral Coverage

Panel A. Size, profitability, network, and records			
	p25	Median	p75
Total assets (USD bn)	0.12	0.68	3.19
Employees	470	2,271	9,001
Buyer firms (network degree)	1	4	14
	Mean		(SD)
Return on assets (%)	1.32		(15.89)
Items reported (share of 70)	0.875		(0.248)
Items answered yes (share of answered)	0.505		(0.224)
Suppliers (with financials)			11,418 (10,271)

Panel B. Sector composition (top 10, all suppliers)			
	Suppliers	% of sample	% assessed
Producer Manufacturing	9,455	11.3%	9.8%
Technology Services	8,833	10.6%	10.3%
Finance	8,650	10.3%	20.7%
Electronic Technology	6,501	7.8%	12.3%
Health Technology	6,260	7.5%	17.9%
Process Industries	5,413	6.5%	13.5%
Commercial Services	4,997	6.0%	8.0%
Distribution Services	4,341	5.2%	5.5%
Consumer Services	4,176	5.0%	11.1%
Consumer Non-Durables	3,841	4.6%	14.0%

Notes: Each supplier appears once, using its most recent observed year.

B Measurement and Treatment Coding

Table B.1: Items Reported Across Item Sets

	DV: First contract			
Item set for items reported	Items reported	Items answered yes	Corr.	Obs.
All 70 items (main measure)	0.0182***	0.0368***	−0.04	10,023
	(0.0039)	(0.0051)		
Excluding the three program items (67 items)	0.0180***	0.0365***	−0.03	10,023
	(0.0039)	(0.0050)		
Split half A (35 items)	0.0168***	0.0387***	−0.13	10,020
	(0.0040)	(0.0052)		
Split half B (35 other items)	0.0188***	0.0345***	0.08	10,018
	(0.0039)	(0.0050)		
All 70 items, weighted by rarity	0.0181**	0.0252***	0.54	10,023
	(0.0061)	(0.0055)		
Assets, employees, number of buyer firms (lagged)		✓		
Year, country, industry FE		✓		

Notes: The first row is the main measure and the second removes the three supply-chain health-and-safety items that define the treatment. To examine whether the results are driven by a few items, the third and fourth split the 70 items into two halves with no items in common. The fifth weights each item by one minus its answer rate, so answering a widely answered item counts for less than answering a rare one. "Corr." is the correlation between the row's measure and items answered yes. The item universe is the same 70 items in every year from 2002 to 2022. Standard errors clustered by supplier in parentheses. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table B.2: Coding Buyer Firms from Their First Observed Program against Observed Launches Only

	Observed launches only (robustness)	From first observed program (main)
Panel A. Estimates under each coding		
First contract, disclosure quintiles (main)	0.0179*	0.0280***
	(0.0089)	(0.0084)
First contract, no disclosure quintiles	0.0302***	0.0402***
	(0.0084)	(0.0081)
First adoption	0.0088*	0.0125**
	(0.0042)	(0.0041)
Written policy − implementation	0.0226**	0.0231**
	(0.0081)	(0.0082)
Existing buyer firms adopt a program	−0.0134	−0.0095
	(0.0118)	(0.0121)
Suppliers in the event set	9,478	9,707
Treated in the stacked sample	1,296	1,277
Panel B. Where the codings differ: the event set		
Buyer firms holding a program with no observed launch		1,121
Events with the same year under both codings		8,706
Events dated earlier when coded from the first observed program		246
Events dropped (prior exposure to a buyer firm already holding one)		526
Events added (first tie is to a buyer firm already holding one)		755

Notes: Of the 5,251 buyer firms ever observed with a program, 1,219 (22%) have no observed launch and would be classified as never having a program under launch-only coding. Standard errors are clustered by supplier. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

C Who Wins Contracts

Table C.1: Records, Conduct, and a First Contract

	(1)	(2)	(3)
Supplier's records			
Items reported (SD)	0.0311***		0.0275**
	(0.0089)		(0.0090)
Items answered yes (SD)	0.0361***		0.0314***
	(0.0085)		(0.0087)
Supplier's conduct			
Own-workforce injury rate (SD)		−0.0109	−0.0099
		(0.0089)	(0.0090)
Any labor incident		0.0031	0.0018
		(0.0178)	(0.0177)
Incidents of any kind (visibility control)		✓	✓
Log assets, log employees, number of buyer firms	✓	✓	✓
Year, country, and industry FE	✓	✓	✓
Supplier-years		3,821	
Yearly chance of a first contract		0.167	

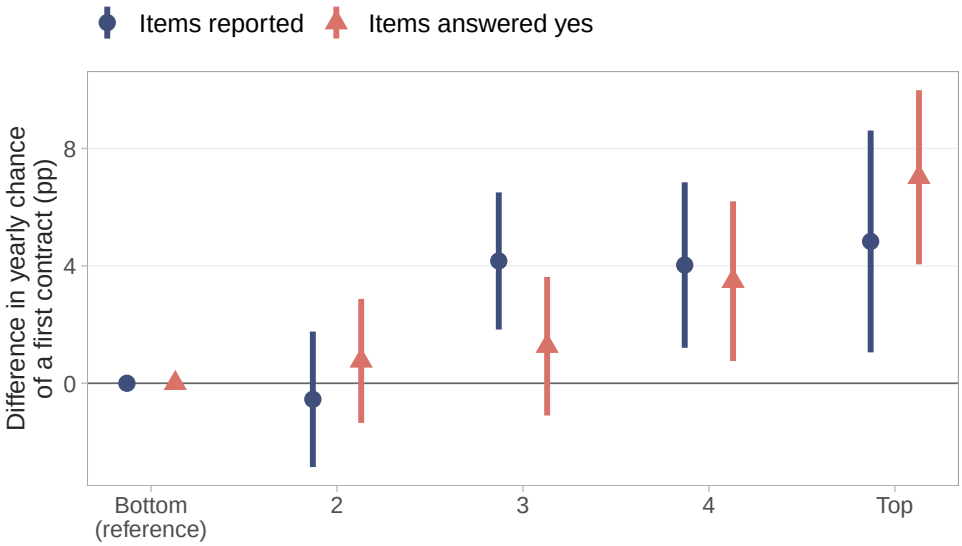
Notes: The event is a supplier's first new contract with a buyer firm that runs a labor program. Standard errors are clustered by supplier. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table C.2: Correlations Between Records, Labor Policies, and Conduct

	Injury Rate		Any Labor Incident	
	Raw	Net	Raw	Net
Items reported	−0.09	−0.01	0.06	0.00
Items answered yes	0.02	−0.01	0.27	0.13
Labor policies held (share of six)	−0.09	−0.02	0.21	0.10
Supplier-years	3,860–3,865		9,410–9,480	

Notes: Correlations between the supplier's records or labor policies at $t-1$ and its conduct, on supplier-years at risk of a first contract with size data. Labor policies held is the share of six answered policy items held: own health and safety, health and safety for own suppliers, human rights, child labor, forced labor, and freedom of association. Net correlations use residuals after regressing both variables on log assets, log employees, the number of buyer firms, and year, country, and industry fixed effects.

Figure C.1: Records and a First Contract by Quintile



Note: Difference in the probability of a first new contract with a buyer firm that runs a labor program, relative to the bottom fifth, in percentage points. Quintiles of items reported and items answered yes are entered together in one model with log assets, log employees, the number of buyer firms, and year, country, and industry fixed effects. Quintiles of items reported are 61 or fewer, 62, 63–64, 65–66, and 67–70 of 70 items.

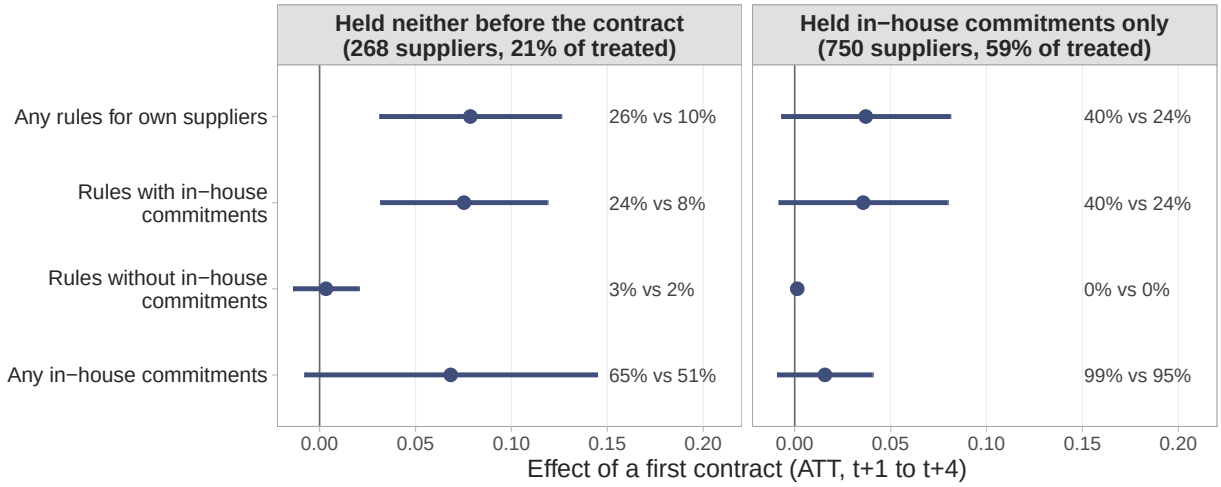
Table C.3: Public Records of US Suppliers' Workplace Conditions and a First Contract

	First contract with a buyer firm			
	(1)	(2)	(3)	(4)
Items answered yes, $t-1$ (SD)	0.0317***	0.0317***	0.0320***	0.0243
	(0.0094)	(0.0093)	(0.0094)	(0.0237)
Items reported, $t-1$ (SD)	0.0100	0.0102	0.0103	0.0235*
	(0.0081)	(0.0081)	(0.0081)	(0.0096)
Any serious OSHA violation, $t-3$ to $t-1$		0.0177		
		(0.0192)		
Log OSHA violations, $t-3$ to $t-1$			−0.0056	
			(0.0069)	
Any OSHA inspection, $t-3$ to $t-1$		−0.0303	−0.0140	
		(0.0197)	(0.0190)	
Injury rate (OSHA ITA), $t-1$ (SD)				0.0082
				(0.0169)
Log assets, log employees, number of buyer firms	Yes	Yes	Yes	Yes
Year and industry FE	Yes	Yes	Yes	Yes
Supplier-years	2,527	2,527	2,527	403
Yearly chance of a first contract	0.125	0.125	0.125	0.124

Notes: U.S. suppliers matched to OSHA records. †p<0.1; *p<0.05; **p<0.01; ***p<0.001

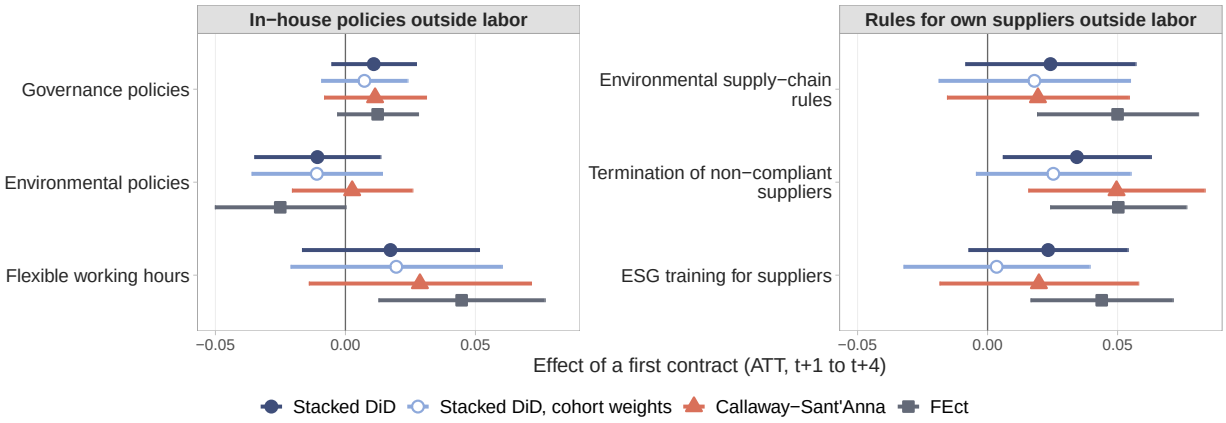
D Rules for Own Suppliers and In-House Policies

Figure D.1: From No Rules to Rules for Own Suppliers



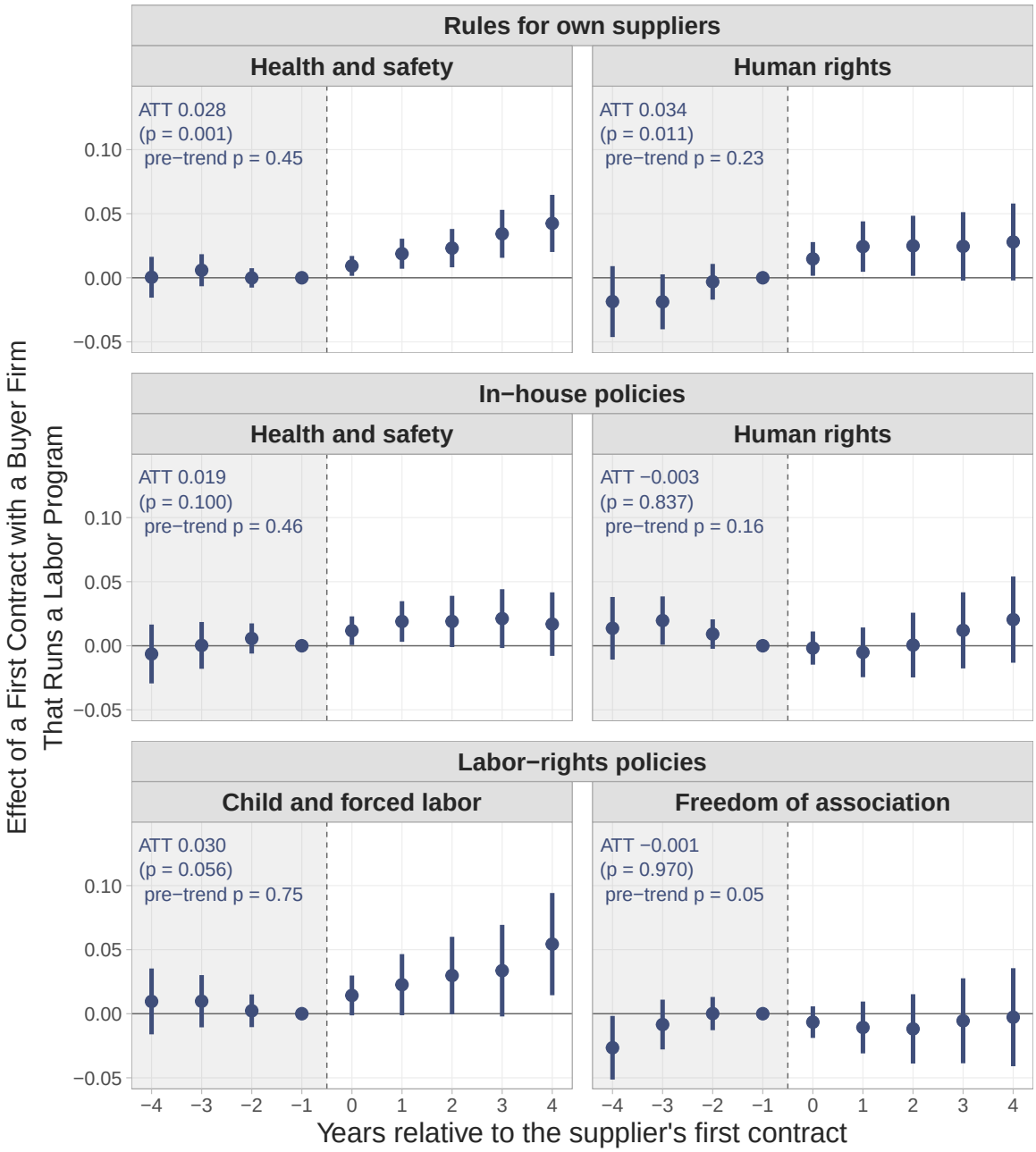
Note: Suppliers are grouped by what they held in the year before the first contract: neither health-and-safety rules for their own suppliers nor in-house health-and-safety commitments (left), or in-house commitments only (right). Points compare suppliers in the same prior-disclosure quintile and standard errors are clustered by supplier. Outcomes are indicators for holding any item.

Figure D.2: Policies outside Labor



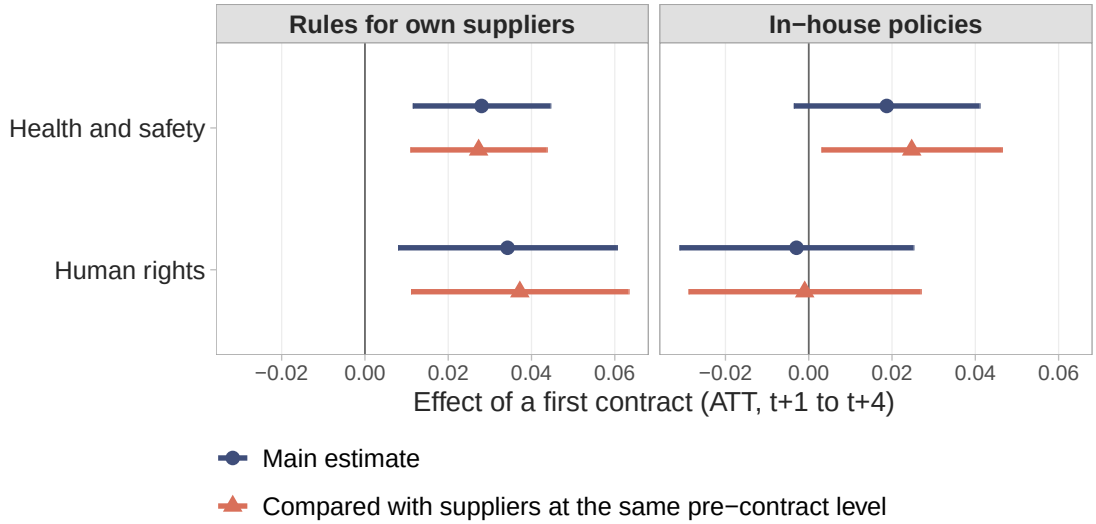
Note: The left panel shows suppliers' in-house policies. Governance policies cover shareholder rights, board functions, fair competition, and data privacy. Environmental policies cover emissions, energy efficiency, and water efficiency. The right panel shows rules for suppliers' own suppliers outside labor. Environmental supply-chain rules cover environmental supply-chain policies and management. Termination of non-compliant suppliers measures stated willingness to end relationships with suppliers that fail environmental criteria. Pre-trend tests are reported in Table K.2.

Figure D.3: Event-Study Estimates for Rules for Own Suppliers and In-House Policies



Note: Event-time coefficients relative to $t-1$ from the stacked design of Figure 2, with 95% confidence intervals and standard errors clustered by supplier. The contract year is estimated separately and is excluded from the average effects in the main text.

Figure D.4: Rules for Own Suppliers and In-House Policies, Compared at the Same Starting Level



Note: The main estimate compares treated suppliers with comparison suppliers in the same quintile of overall disclosure, cohort, and year, as in Figure 2.

Table D.1: Rules for Own Suppliers against In-House Policies

	Rules for own suppliers	In-house policies	Difference	Observations
Health and safety	0.028***	0.019	0.009	83,174
	(0.008)	(0.011)	(0.013)	
Human rights	0.034*	−0.003	0.037*	82,930
	(0.013)	(0.014)	(0.017)	

Notes: The difference comes from a model that stacks the two outcomes and interacts the first contract with the direction of the record, holding supplier, cohort, and issue fixed. All specifications include supplier × cohort and disclosure-quintile × cohort × year fixed effects, with standard errors clustered by supplier. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

E Buyer Firms' Human-Rights Programs as the Treatment

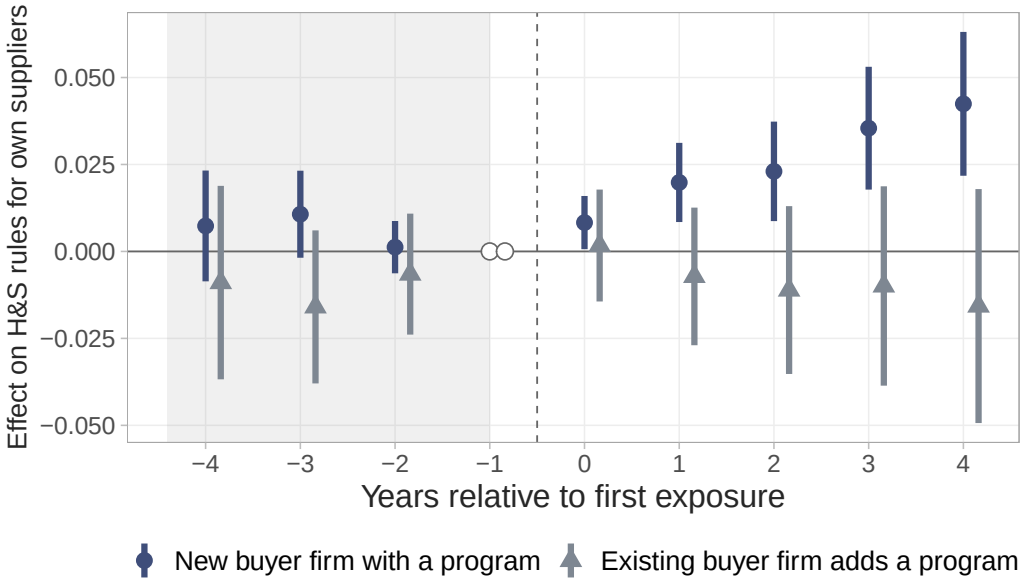
Table E.1: Supplier Records under Two Definitions of the Buyer Firm's Program

	Rules for own suppliers		In-house policies	
	(1) Health and safety	(2) Human rights	(3) Health and safety	(4) Human rights
Panel A. Buyer firm runs a supply-chain health-and-safety program (main)				
First contract	0.0280***	0.0342*	0.0187	−0.0029
	(0.0084)	(0.0134)	(0.0114)	(0.0143)
Mean at $t-1$ (treated)	0.092	0.256	0.584	0.252
Pre-trend p	0.45	0.23	0.46	0.16
Observations	41,342	41,496	41,849	41,452
Treated suppliers	1,277	1,284	1,295	1,281
Comparison suppliers	1,131	1,130	1,134	1,131
Panel B. Buyer firm runs a supply-chain human-rights program				
First contract	0.0283**	0.0323*	0.0314*	0.0102
	(0.0088)	(0.0138)	(0.0125)	(0.0147)
Mean at $t-1$ (treated)	0.087	0.245	0.569	0.241
Pre-trend p	0.43	0.86	0.49	0.35
Observations	37,254	37,379	37,645	37,323
Treated suppliers	1,244	1,249	1,262	1,247
Comparison suppliers	1,052	1,052	1,058	1,052
Supplier × cohort FE	✓	✓	✓	✓
Disclosure quintile × cohort × year FE	✓	✓	✓	✓

Notes: Each cell is a separate stacked difference-in-differences estimate, using the buyer firm's program named in each panel to define the first contract. Standard errors are clustered by supplier. Under both definitions, rules for own suppliers increase in both issues and in-house human-rights policies do not change; in-house health-and-safety policies increase under the human-rights definition. Because the two programs overlap substantially in timing, the human-rights response in Panel A cannot be read as a response beyond the buyer firm's program, and I keep the health-and-safety program as the main treatment. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

F New and Existing Buyer Firms

Figure F.1: New and Existing Buyer Firms, Event-Study Estimates



Note: Event-time coefficients relative to $t-1$ from the joint stacked model of Figure 4 and Table F.1, for health-and-safety rules for own suppliers, with 95% confidence intervals. Both pathways are compared with the same not-yet-exposed suppliers, with supplier × cohort and disclosure-quintile × cohort × year fixed effects and standard errors clustered by supplier. The exposure year ($t = 0$) is estimated separately and is excluded from the average effects.

Table F.1: Rules for Own Suppliers after a New or an Existing Buyer Firm Runs a Program

	(1) Joint model	(2) Separate designs
Panel A. First contract with a new buyer firm that runs a program		
Estimate	0.0256^{**}	0.0280^{***}
	(0.0081)	(0.0084)
Treated suppliers	1,277	1,277
Panel B. An existing buyer firm adopts a program		
Estimate	−0.0038	−0.0095
	(0.0126)	(0.0121)
Treated suppliers	277	—
Panel C. Difference (A − B)		
Estimate	0.0295^{*}	0.0375^{*}
	(0.0130)	(0.0155)
Comparison suppliers	1,379	
Observations	52,667	41,342 / 36,184

Notes: The outcome is the share of the three supply-chain health-and-safety items a supplier holds, as in Figure 2. Column (1) estimates both pathways in one stacked model (Equation 3) against the same not-yet-exposed comparison suppliers, with supplier × cohort and disclosure-quintile × cohort × year fixed effects. Column (2) reports the stacked entry design (Equation 2) in Panel A and the continuous exposure design in Panel B, in which exposure is the share of the supplier's frozen set of buyer firms that adopt a program, with supplier and year fixed effects. Panel B in column (2) is therefore the effect of every buyer firm in the set adopting a program, and the Panel C difference comes from a supplier-level block bootstrap. Standard errors are clustered by supplier. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

G Records and Conduct

Table G.1: First Contract and the First Adoption of Written Policy and Implementation Records

	First adoption			Acquisition		
	(1)	(2)	(3)	(4)	(5)	(6)
First contract	0.0125**			0.0247**		
	(0.0041)			(0.0082)		
First contract × written policy		0.0288**	0.0343***		0.0414*	0.0475**
		(0.0088)	(0.0089)		(0.0170)	(0.0171)
First contract × implementation		0.0056†	0.0061†		0.0169*	0.0205**
		(0.0033)	(0.0032)		(0.0071)	(0.0065)
Written policy − implementation		0.0231**	0.0283***		0.0245	0.0270
		(0.0082)	(0.0083)		(0.0165)	(0.0167)
Supplier × cohort × item FE	✓	✓	✓	✓	✓	✓
Disclosure quintile × cohort × year × item FE	✓	✓	✓	✓	✓	✓
Country and sector × year × item FE			✓			✓
Base rate among comparison suppliers		0.0210			0.0620	
Observations	98,924	98,924	98,710	103,013	103,013	102,813
Treated suppliers		1,270			1,270	

Notes: Stacked design at the supplier × item level, with standard errors clustered by supplier. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table G.2: The Next Tier after a Supplier's First Contract

	Dependent variable: outcome of the supplier's own suppliers			
	In-house labor policies	In-house health and safety	Child and forced labor	Rules for own suppliers, H&S
	(1)	(2)	(3)	(4)
First contract of the supplier they serve	0.0027	0.0287	−0.0169	0.0083
	(0.0171)	(0.0201)	(0.0247)	(0.0151)
Pre-contract mean	0.280	0.493	0.117	0.060
Pre-trend test, p-value	0.785	0.857	0.885	0.911
Treated next-tier firms	497	497	487	488
Comparison next-tier firms	344	344	341	341

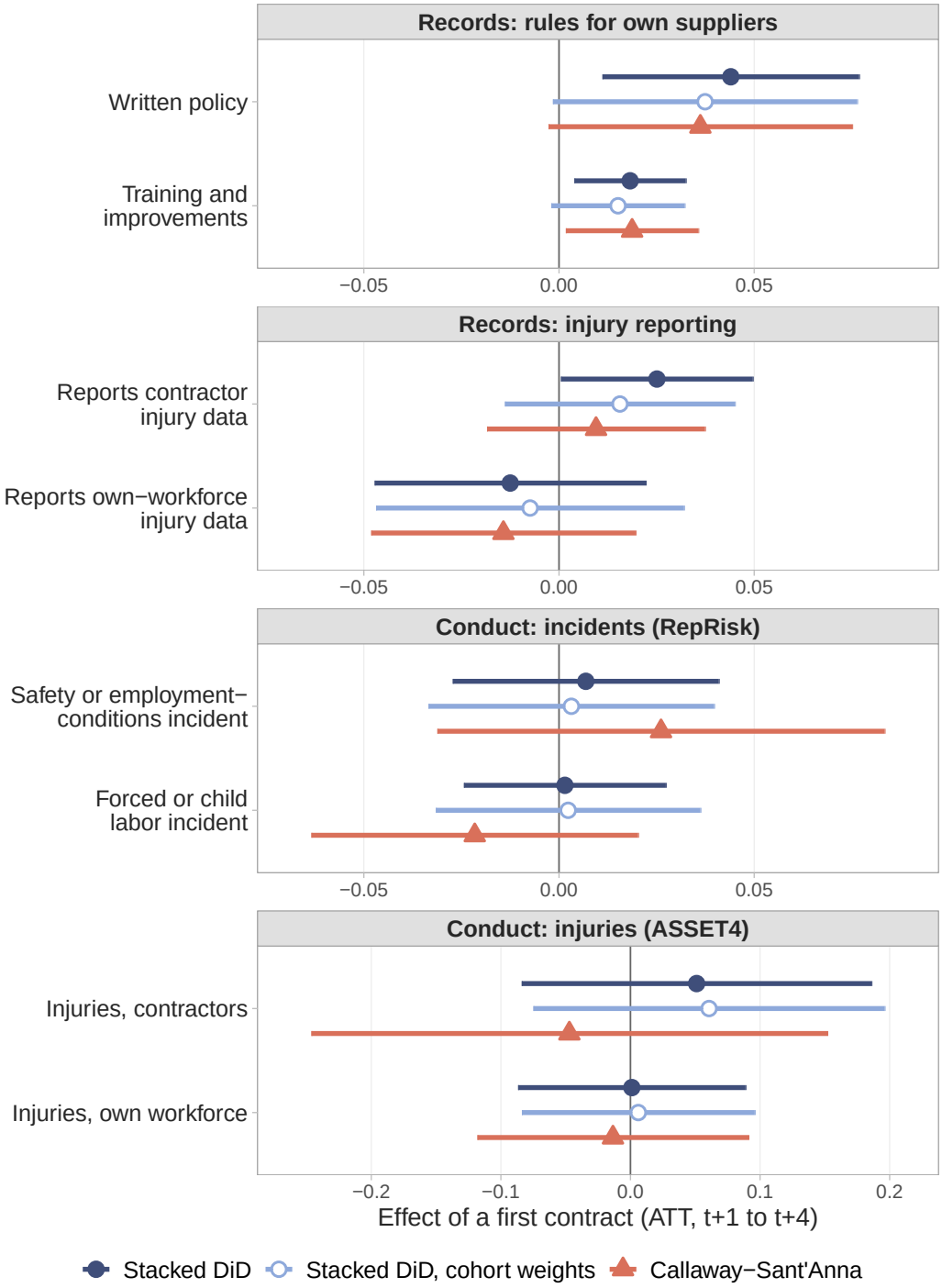
Note: †p<0.1; *p<0.05; **p<0.01; ***p<0.001

Table G.3: Incidents at the Next Tier after a Supplier's First Contract

		Labor		Other	
	Any incident	Safety and employment	Forced or child labor	Environ-mental	Governance
	(1)	(2)	(3)	(4)	(5)
Panel A. All next-tier firms covered by RepRisk; cohort × year fixed effects					
First contract of the supplier they serve	−0.0293†	0.0218†	0.0139*	0.0071	−0.0095
	(0.0155)	(0.0112)	(0.0066)	(0.0109)	(0.0144)
Treated mean at $t-1$	0.222	0.064	0.031	0.112	0.141
Pre-trend test, p-value	0.341	0.414	0.064	0.823	0.405
Treated / comparison firms			2,006 / 2,472		
Panel B. Panel A, also within prior-incident group × cohort × year					
First contract of the supplier they serve	−0.0162	0.0251*	0.0147*	0.0148	−0.0042
	(0.0157)	(0.0114)	(0.0063)	(0.0108)	(0.0145)
Treated mean at $t-1$	0.222	0.064	0.031	0.112	0.141
Pre-trend test, p-value	0.071	0.029	0.102	0.624	0.655
Treated / comparison firms			2,006 / 2,472		
Panel C. Next-tier firms assessed in ASSET4; disclosure-quintile × cohort × year (main design)					
First contract of the supplier they serve	−0.0590†	−0.0091	0.0175	−0.0008	−0.0161
	(0.0344)	(0.0331)	(0.0209)	(0.0276)	(0.0344)
Treated mean at $t-1$	0.540	0.169	0.090	0.296	0.372
Pre-trend test, p-value	0.845	0.156	0.257	0.907	0.432
Treated / comparison firms			433 / 179		

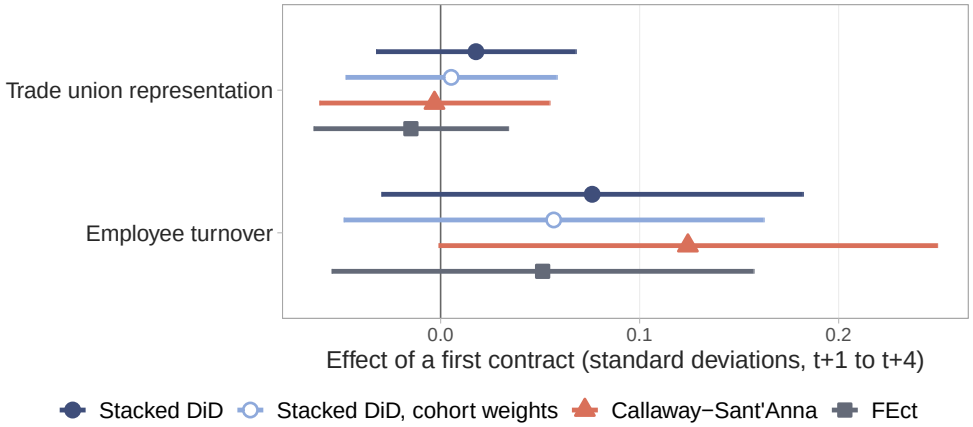
Note: †p<0.1; *p<0.05; **p<0.01; ***p<0.001

Figure G.1: Written Policy, Implementation, Reporting, and Conduct under Alternative Estimators



Note: Outcomes and scales as in Figure 5, estimators as in Figure K.1 except FEct, which is not estimated for the written-policy, implementation, and incident outcomes. FEct estimates for injury reporting and injuries are reported in Table K.2. Incidents are the probability of a `RepRisk` incident of each kind in the year.

Figure G.2: Own-Workforce Conditions



Note: Estimators as in Figure K.1. Trade union representation and employee turnover are `ASSET4` fields standardized within year, in standard deviations. Pre-trend tests are reported in Table K.2.

H When Buyer Firms Adopt Programs

Table H.1: Buyer Firms' Incidents and Supply-Chain Program Launch

DV: Hazard of a buyer firm's first supply-chain program launch	(1)	(2)	(3)
Panel A. Do supply-chain incidents predict launch?			
Any supply-chain social incidents, $t-3$ to $t-1$	0.0269***	0.0177***	
	(0.0049)	(0.0049)	
Any other social incidents, $t-3$ to $t-1$	0.0113***	−0.0040	
	(0.0027)	(0.0028)	
Contrast (supply-chain − other social)	0.0155* (0.0062)	0.0217*** (0.0061)	
Panel B. Volume or kind?			
log(1+total incidents), $t-3$ to $t-1$	0.0089***	0.0009	−0.0044**
	(0.0012)	(0.0012)	(0.0015)
Supply-chain incidents, all pillars			0.0166***
			(0.0034)
In the supply chain:			
Social			0.0145***
			(0.0037)
Environmental			0.0148***
			(0.0041)
Governance			0.0124*
			(0.0050)
Occupational health			0.0184***
			(0.0054)
Labor (UNGC P3–P6)			0.0150**
			(0.0046)
Not in the supply chain:			
Social			−0.0022
			(0.0030)
Environmental			−0.0015
			(0.0027)
Governance			−0.0061*
			(0.0031)
Occupational health			0.0093*
			(0.0037)
Labor (UNGC P3–P6)			0.0047
			(0.0045)
Prior records$_{t-1}$ (SD)		0.0403***	0.0409***
		(0.0014)	(0.0015)
Assessment coverage$_{t-1}$ (SD)		−0.0002	−0.0002
		(0.0011)	(0.0011)
Network size$_{t-1}$ (log suppliers)	0.0134***	0.0074***	0.0073***
	(0.0010)	(0.0010)	(0.0011)
Year and duration-since-risk-entry FE	✓	✓	✓
Total incident volume held constant			✓
Baseline hazard		0.0542	
Buyer firm–years		49,658	

Notes: Panel A uses binary indicators, and Panel B uses log(1 + count). Incident measures count `RepRisk` incidents recorded in $t-3$ to $t-1$. Standard errors are clustered by buyer firm. The sample contains 49,658 buyer firm–years, 8,219 buyer firms, and 2,693 launches. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

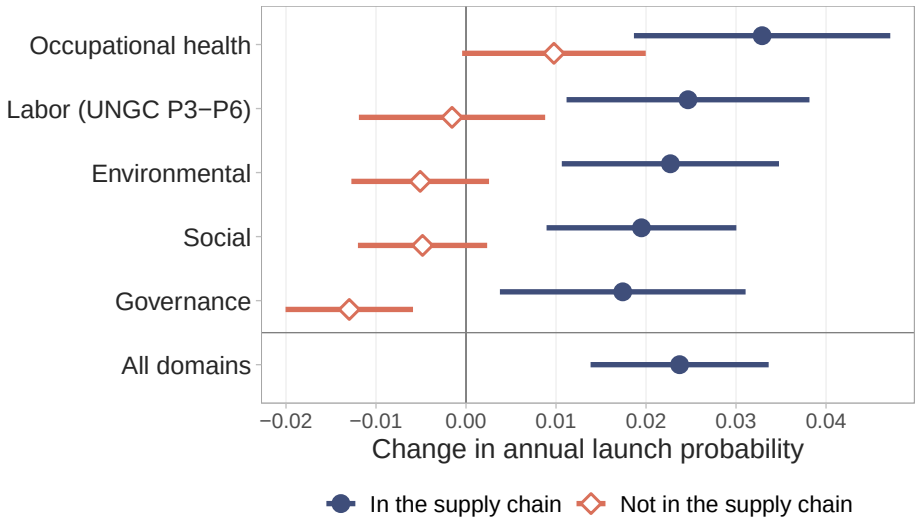
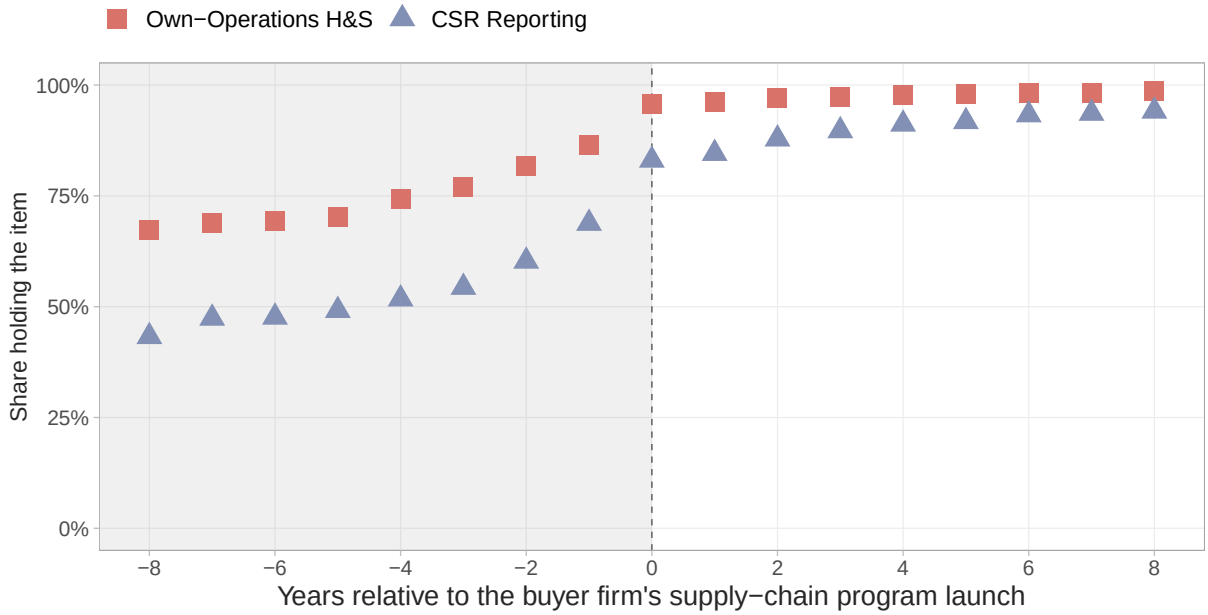


Figure H.1: Buyer Firm Incidents and Supply-Chain Program Launch

Note: Points show the change in launch probability associated with at least one `RepRisk` incident of each type in the previous three years. All models control for total incident volume, network size, documentary maturity, and assessment coverage, as well as year and duration since entering the risk set. Bars show 95% confidence intervals, with standard errors clustered by buyer. Table H.1 reports the estimates using binary indicators and log(1 + count) in Panels A and B, respectively.

Figure H.2: Buyer Firms' Commitments Around a Supply-Chain Program Launch



Note: Share of buyer firms reporting each commitment in the years around their adoption of a supply-chain program, among buyer firms with an observed launch. By the year before launch, 86% have an in-house health-and-safety commitment and 69% already publish Corporate Social Responsibility (CSR) reports. The test is reported in Table H.2.

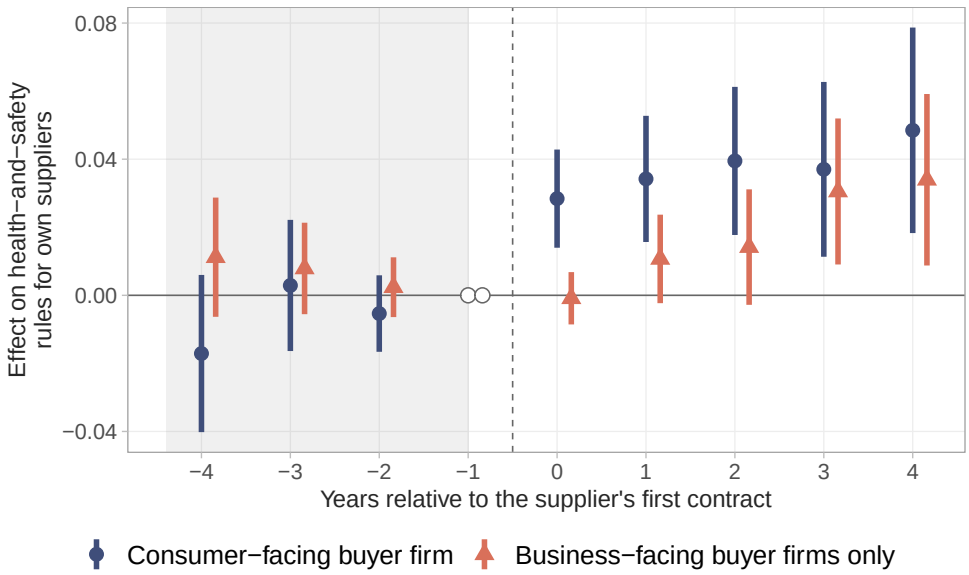
Table H.2: Prior Records and a Buyer Firm's Supply-Chain Program Launch

Panel A. Do prior records predict launch?			
	(1)	(2)	(3)
Prior records$_{t-1}$ (SD)	0.0419***	0.0395***	0.0395***
	(0.0013)	(0.0013)	(0.0013)
as % of baseline hazard	*80%*	*75%*	*75%*
Network size$_{t-1}$ (log suppliers)		0.0080***	0.0080***
Assessment coverage$_{t-1}$ (SD)			−0.0001
Buyer firm–years	54,013	54,013	54,013
Panel B. Does a closely related commitment predict more?			
	(4)	(5)	
In-house health and safety$_{t-1}$	0.0263***	0.0052*	
	(0.0020)	(0.0021)	
Emissions policy$_{t-1}$	0.0335***	0.0068**	
	(0.0024)	(0.0026)	
Contrast (in-house health and safety − emissions)	−0.0072	−0.0016	
	(0.0036)	(0.0036)	
p (difference)	0.043	0.656	
Prior records control (other items)		✓	
Buyer firm–years	54,013	54,013	
Panel C. Records held at entry or accumulated since?			
	(6)	(7)	(8)
Records at risk-set entry (SD)	0.0271***	0.0249***	
	(0.0012)	(0.0012)	
Accumulated since entry (SD)	0.0380***	0.0355***	0.0551***
	(0.0018)	(0.0018)	(0.0029)
Network size and coverage		✓	✓
Buyer firm fixed effects			✓
Buyer firm–years	54,013	54,013	52,678
Year fixed effects		✓	
Duration-since-risk-entry fixed effects		✓	

Notes: Hazard of a buyer firm's first supply-chain program launch. Each model includes year fixed effects and fixed effects for time since entering the risk set. Standard errors are clustered by buyer firm. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

I Heterogeneity and Additional Exposure

Figure I.1: Rules for Own Suppliers, by Whether the Buyer Firm Is Consumer-Facing



Note: Event-time coefficients for health-and-safety rules for own suppliers, estimated separately for suppliers whose first contract includes a consumer-facing buyer firm and suppliers whose first contract is with business-facing buyer firms only. A buyer firm is consumer-facing if its primary industry is retail trade (SIC 52–59), food, tobacco, apparel, furniture, leather, or miscellaneous consumer goods manufacturing (SIC 20, 21, 23, 25, 31, 39), consumer services such as hotels, personal services, film, and recreation (SIC 70, 72, 78, 79), or cosmetics, footwear, computers, consumer electronics, or motor vehicles (SIC 2844, 3021, 3571, 3651, 3711). Bars are 95% confidence intervals; standard errors clustered by supplier.

Table I.1: The Effect of a First Contract by Supplier's Country

	OECD suppliers	Non-OECD suppliers
First contract	0.028^{**}	$0.028^{\dagger}$
	(0.009)	(0.016)
Mean at $t-1$ (treated)	0.082	0.072
Treated suppliers	1,021	256
Difference (non-OECD − OECD)	0.001	($p = 0.970$)
Supplier × cohort FE	✓	
Disclosure quintile × cohort × year FE	✓	
Comparison suppliers	1,131	
Observations	41,342	

Notes: Effect on health-and-safety rules for own suppliers, split by supplier headquarters in OECD and non-OECD countries. Standard errors are clustered by supplier. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table I.2: Program Intensity and the Response to a First Contract

	(1) One buyer firm	(2) + Buyer firm's other items	(3) Pooled	(4) + Number of buyer firms
First contract × program items held by the buyer firm	0.018	0.021	0.025^{*}	0.015
	(0.015)	(0.016)	(0.010)	(0.011)
Supplier–years	37,212	37,212	40,536	40,536
Supplier × cohort FE	✓	✓	✓	✓
Disclosure quintile × cohort × year FE	✓	✓	✓	✓

Notes: Stacked design with standard errors clustered by supplier. Intensity is the number of the three supply-chain health-and-safety items held by the buyer firm in the supplier's contract year, so it is fixed at treatment and cannot respond to the supplier. Column (1) keeps suppliers with one such buyer firm in the contract year. Column (2) adds the buyer firm's mean across all 70 items. Columns (3) and (4) pool items across all such buyer firms, and column (4) controls for their number. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table I.3: A Second Buyer Firm with a Program

	Gap ≥ 3 years			Gap ≥ 1 year		Benchmark
	(1) Naive	(2) + Years-since-first FE	(3) Gap-matched	(4) Naive	(5) + Years-since-first FE	(6) First contract
ATT	$0.0296^{\dagger}$	0.0086	0.0212	0.0249^{*}	−0.0004	0.0333^{***}
	(0.0151)	(0.0236)	(0.0215)	(0.0104)	(0.0151)	(0.0088)
Observations	32,488	32,488	11,526	40,979	40,979	39,603
Treated suppliers	441	441	234	1,246	1,246	1,481
Supplier × cohort FE	✓	✓	✓	✓	✓	✓
Disclosure quintile × cohort × year FE	✓	✓	✓	✓	✓	✓
Years since first contract FE		✓			✓	

Notes: The design is re-indexed around a supplier's second new relationship with a buyer firm that runs a program. Treated suppliers have formed this second relationship, and comparison suppliers have not yet done so. Fixed effects for years since the first contract enter the post-contract average only, not the event-study specification. Standard errors are clustered by supplier. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table I.4: First Contracts Across and Within Borders

	DV: Relationships with buyer firms		
	Domestic	Mixed	Cross-border
First contract	0.0311*	0.0420**	0.0208*
	(0.0132)	(0.0150)	(0.0099)
Treated suppliers	326	217	734
Observations	34,413	33,603	37,484
Difference from domestic		0.0118	−0.0086
		(0.0174)	(0.0135)
Supplier × cohort FE		✓	
Disclosure quintile × cohort × year FE		✓	

Notes: A relationship is cross-border when the buyer firm and supplier are headquartered in different countries. *Cross-border* suppliers are those whose new relationships with buyer firms that run a program all cross a border. Each group is compared with the same comparison suppliers. Event window ±4 years, standard errors clustered by supplier. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

J Alternative Explanations

J.1 Public Labor Rules

Table J.1: Public Rules and a First Contract

	Dependent variable: first contract with a buyer firm that runs a labor program						
		ESG pillars			Within the social pillar		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Social		$-0.0124^{\dagger}$					
		(0.0063)					
Environmental			0.0004				
			(0.0053)				
Governance				-0.0012			
				(0.0065)			
Labor					-0.0166^{**}		-0.0212^{***}
					(0.0048)		(0.0061)
Community						0.0058	$0.0111^{\dagger}$
						(0.0057)	(0.0060)
Gender							0.0037
							(0.0045)
Items answered yes (SD)	0.0282^{***}	0.0269^{***}	0.0283^{***}	0.0281^{***}	0.0280^{***}	0.0296^{***}	0.0298^{***}
	(0.0037)	(0.0037)	(0.0036)	(0.0038)	(0.0033)	(0.0041)	(0.0034)
Items reported (SD)	0.0177^{***}	0.0175^{***}	0.0177^{***}	0.0177^{***}	0.0178^{***}	0.0180^{***}	0.0183^{***}
	(0.0025)	(0.0026)	(0.0026)	(0.0025)	(0.0026)	(0.0026)	(0.0027)
Log assets	-0.0012	0.0001	-0.0013	-0.0011	0.0002	-0.0019	-0.0010
	(0.0029)	(0.0029)	(0.0030)	(0.0028)	(0.0026)	(0.0033)	(0.0030)
Log employees	-0.0031	-0.0028	-0.0031	-0.0032	-0.0028	-0.0032	-0.0026
	(0.0025)	(0.0024)	(0.0026)	(0.0023)	(0.0024)	(0.0024)	(0.0025)
Log number of buyer firms	0.0613^{***}	0.0611^{***}	0.0613^{***}	0.0613^{***}	0.0606^{***}	0.0611^{***}	0.0604^{***}
	(0.0047)	(0.0045)	(0.0048)	(0.0047)	(0.0045)	(0.0048)	(0.0046)
Year fixed effects	✓	✓	✓	✓	✓	✓	✓
Supplier-years	9,611	9,611	9,611	9,611	9,611	9,611	9,611
Suppliers	3,073	3,073	3,073	3,073	3,073	3,073	3,073
Countries	60	60	60	60	60	60	60
Base rate	0.139	0.139	0.139	0.139	0.139	0.139	0.139

Notes: Column (1) includes no public rules, while columns (2)–(6) enter one stock at a time. Column (7) enters the three social-pillar stocks together. Standard errors clustered by supplier country. All models include items reported, items answered yes, log assets, log employees, and the number of buyer firms, lagged by one year, and year fixed effects. Country measures are standardized and measured in the supplier's country at $t-1$. Public rules are cumulative stocks of Carrots & Sticks instruments, hand-coded by ESG pillar and social sub-domain. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table J.2: Public Labor Rules and New Ties, by Buyer Firm Type

	Dependent variable: new tie in year t with a buyer firm that			
	runs a labor program	does not run a program	runs a labor program	does not run a program
	(1)	(2)	(3)	(4)
Public labor rules (SD)	-0.0164^{**}	$-0.0178^{\dagger}$	-0.0153^{**}	$-0.0173^{\dagger}$
	(0.0049)	(0.0101)	(0.0044)	(0.0098)
Log GDP per capita (SD)			0.0157^{***}	0.0066
			(0.0040)	(0.0095)
Base rate	0.138	0.447	0.138	0.447
Labor rules as share of base rate	-11.9%	-4.0%	-11.0%	-3.9%
Equal shares, p-value	0.001		0.002	
Supplier-years	9,496	9,496	9,496	9,496

†p<0.1; *p<0.05; **p<0.01; ***p<0.001

J.2 State Capacity

Table J.3: State Capacity, Income, and First Contracts

	Dependent variable: first contract with a buyer firm that runs a labor program					
	(1)	(2)	(3)	(4)	(5)	(6)
Rule of law	0.014**	−0.004				
	(0.004)	(0.009)				
Government effectiveness			0.015**	0.005		
			(0.005)	(0.009)		
Statistical performance					0.013***	−0.001
					(0.003)	(0.006)
Log GDP per capita		0.019*		0.012		0.018*
		(0.008)		(0.007)		(0.007)
Items answered yes	0.027***	0.030***	0.028***	0.029***	0.026***	0.029***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Items reported	0.017***	0.018***	0.017***	0.018***	0.016***	0.017***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Log total assets	−0.002	−0.003	−0.002	−0.003	−0.002	−0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Log employees	−0.001	−0.001	−0.001	−0.001	−0.001	−0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Log number of buyer firms	0.063***	0.062***	0.063***	0.062***	0.063***	0.063***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,528	9,528	9,528	9,528	9,202	9,202
Countries	62	62	62	62	58	58
Events	1,319	1,319	1,319	1,319	1,283	1,283
Baseline hazard	0.138	0.138	0.138	0.138	0.139	0.139

Note: †p<0.1; *p<0.05; **p<0.01; ***p<0.001

J.3 Buyer Firm Leverage

Table J.4: Supplier Finances, Buyer Firm Leverage, and the Response to a First Contract

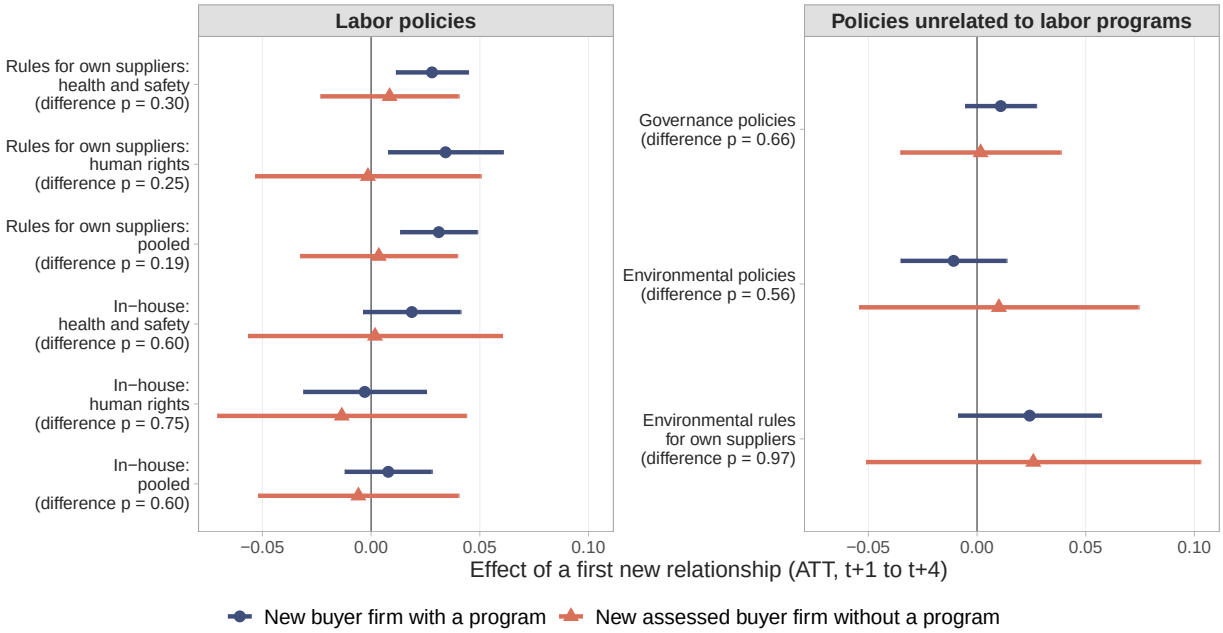
Panel A. Supplier financial position and size	(1) ROA		(2) Gearing		(3) Solvency		(4) Log assets		(5) Log employees	
	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above
Subgroup ATT	0.016	0.041***	0.036**	0.030**	0.023*	0.044***	0.010	0.038***	0.016	0.040***
	(0.011)	(0.010)	(0.012)	(0.010)	(0.010)	(0.012)	(0.014)	(0.009)	(0.016)	(0.010)
Pre-trend p	0.82	0.62	0.46	0.79	0.96	0.07	0.95	0.67	0.84	0.77
Treated suppliers	468	678	406	709	680	470	281	870	159	833
Difference	0.025*		−0.007		0.021†		0.028†		0.024	
	(0.012)		(0.013)		(0.012)		(0.014)		(0.017)	
Continuous, per SD	0.014†		−0.020*		0.011		−0.006		0.010	
	(0.008)		(0.009)		(0.008)		(0.008)		(0.009)	
Observations	40,355		40,143		40,384		40,389		39,282	

Panel B. The relationship with the buyer firm	(6) Revenue share		(7) Asset gap		(8) Employee gap	
	Below	Above	Below	Above	Below	Above
Subgroup ATT	0.025	0.015	0.037***	0.025†	0.040***	0.029*
	(0.022)	(0.027)	(0.010)	(0.013)	(0.010)	(0.014)
Pre-trend p	0.26	0.56	0.30	0.54	0.71	0.35
Treated suppliers	84	56	803	287	720	177
Difference	−0.010		−0.012		−0.011	
	(0.034)		(0.014)		(0.015)	
Continuous, per SD	0.019		0.006		0.002	
	(0.028)		(0.007)		(0.008)	
Observations	33,061		39,949		38,589	

Notes: Stacked design with supplier × cohort and disclosure-quintile × cohort × year fixed effects and standard errors clustered by supplier. Treated suppliers are split at the median, and both halves are compared with the same comparison suppliers. *Difference* is the above-median minus below-median estimate. *Continuous* enters the measure in standardized form. Supplier characteristics and both size gaps are fixed at $t-1$. Revenue share is the relationship's maximum over all observed years and so reflects a lasting feature of the relationship. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

J.4 Any Large New Buyer Firm

Figure J.1: New Relationships with Buyer Firms with and without Labor Programs



Note: Average effect over $t+1$ to $t+4$ of a supplier's first new relationship with a buyer firm that runs a labor program (the main event) and of its first new relationship with a buyer firm assessed by `ASSET4` that year and observed holding none of the three program items, with 95% confidence intervals. Both are estimated in one stacked model with route-specific fixed effects and supplier clustering; row labels give the p-value for the difference.

J.5 Copying the Buyer Firm's Items

Table J.5: Do Suppliers Adopt the Items Their Buyer Firms Hold?

	(1) Share Held	(2) Any Held	(3) Items Not Yet Held
Post × buyer firm holds item	0.005	0.004	0.017
	(0.011)	(0.010)	(0.011)
Placebo: item vectors permuted across suppliers			
Mean permuted estimate	0.003	—	0.021
Permutation p (200 draws)	0.69	—	0.56
Supplier–item–years	34,892	34,892	26,371
Supplier × item FE	✓	✓	✓
Supplier × year FE	✓	✓	✓
Item × year FE	✓	✓	✓

Notes: Supplier × year fixed effects absorb the supplier's overall response to the contract. Standard errors are clustered by supplier. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

J.6 Delayed Change

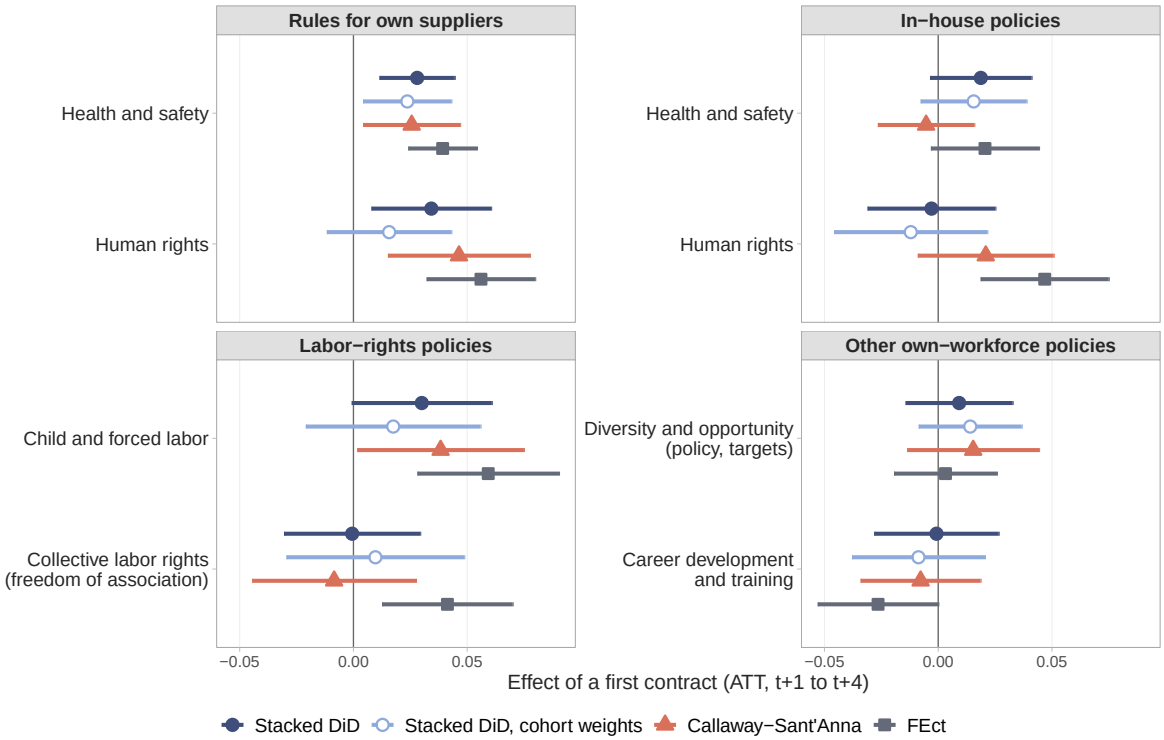
Table J.6: Eight Years after a First Contract

	Rules for own suppliers, H&S	Injuries, contractors (SD)	Injuries, own workforce (SD)	Safety or employment-conditions incident	Forced or child labor incident
	(1)	(2)	(3)	(4)	(5)
First contract, years 1–4	0.033*	0.028	−0.046	0.055†	−0.007
	(0.014)	(0.118)	(0.054)	(0.029)	(0.018)
First contract, years 5–8	0.076***	0.103	−0.071	0.010	−0.055
	(0.020)	(0.170)	(0.084)	(0.041)	(0.034)
Pre-contract mean	0.090	0.055	0.010	0.165	0.090
Treated suppliers	635	89	356	558	558
Treated suppliers observed in years 5–8	577	73	323	532	532

Notes: Stacked difference-in-differences with an eight-year post-contract window: cohorts 2006–2013, comparison suppliers with a first contract after $k + 8$ and no exposure through $k + 8$, supplier × cohort and disclosure-quintile × cohort × year fixed effects, contract year excluded. Separate average effects for $t + 1$ to $t + 4$ and $t + 5$ to $t + 8$ in one model. Standard errors clustered by supplier are in parentheses. †p<0.1; *p<0.05; **p<0.01; ***p<0.001

K Alternative Estimators and Robustness

Figure K.1: Labor Policies under Alternative Estimators



Note: Average effect over $t+1$ to $t+4$ with 95% confidence intervals. Outcomes as in Figures 2 and 3. Stacked DiD is the main design. Cohort weights reweight the stacked design so each cohort counts in proportion to its treated suppliers (Wing, Freedman and Hollingsworth, 2024). Callaway–Sant'Anna compares each cohort with not-yet-treated suppliers in the same prior-disclosure quintile. FEct is the fixed-effects counterfactual estimator (Liu, Wang and Xu, 2024). Estimates and pre-trend tests are reported in Table K.1 and, for other own-workforce policies, Table K.2.

Table K.1: First Contract and Supplier Labor Policies Under Alternative Estimators

	(1)	(2)	(3)	(4)
	Stacked DiD			
	Main	Cohort weights	Callaway–Sant'Anna	FEct
A. Rules for own suppliers				
Health and safety	0.028***	0.024*	0.026*	0.039***
	(0.008)	(0.010)	(0.011)	(0.008)
	[0.45]	[0.40]	[0.44]	[0.04]
Human rights	0.034*	0.016	0.046**	0.056***
	(0.013)	(0.014)	(0.016)	(0.012)
	[0.23]	[0.67]	[0.24]	[0.00]
B. In-house policies				
Health and safety	0.019	0.016	−0.005	0.021†
	(0.011)	(0.012)	(0.011)	(0.012)
	[0.46]	[0.54]	[0.45]	[0.00]
Human rights	−0.003	−0.012	0.021	0.047**
	(0.014)	(0.017)	(0.015)	(0.014)
	[0.16]	[0.42]	[0.87]	[0.03]
C. Labor-rights policies				
Child and forced labor	0.030†	0.017	0.038*	0.059***
	(0.016)	(0.020)	(0.019)	(0.016)
	[0.75]	[0.94]	[0.92]	[0.99]
Freedom of association	−0.001	0.010	−0.008	0.041**
	(0.015)	(0.020)	(0.018)	(0.015)
	[0.05]	[0.02]	[0.28]	[0.12]
Compared within disclosure quintiles	Yes	Yes	Yes	

Notes: Standard errors in parentheses, clustered by supplier (columns 1–2) or from a bootstrap clustered by supplier (columns 3–4). Pre-trend p-values in brackets: joint test of $t-4$ to $t-2$ relative to $t-1$ in columns (1)–(3), and of the pre-contract residuals from $t-4$ to $t-1$ in column (4). Column (1) is the stacked design in Figure 2, with supplier × cohort and disclosure-quintile × cohort × year fixed effects. Column (2) weights comparison suppliers so that each cohort counts in proportion to its treated suppliers (Wing, Freedman and Hollingsworth, 2024). Column (3) is the Callaway–Sant'Anna estimator reported in the text: each first-contract cohort from 2006 to 2017 is compared with suppliers not yet under contract in the same quintile of overall disclosure in the year before the contract, following the same suppliers from that year to each later year. Column (4) is the fixed-effects counterfactual estimator, with suppliers whose first contract comes later as comparisons until their own contract, without disclosure quintiles. The interactive fixed-effects version, with the number of factors chosen by cross-validation, selects zero factors for every outcome and is therefore identical. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table K.2: Other Supplier Policies, Records, and Conduct Under Alternative Estimators

	(1)	(2)	(3)	(4)
	Stacked DiD			
	Main	Cohort weights	Callaway–Sant'Anna	FEct
A. Own-workforce policies				
Diversity and opportunity (policy, targets)	0.009	0.014	0.015	0.003
	(0.012)	(0.012)	(0.015)	(0.012)
	[0.17]	[0.61]	[0.41]	[0.01]
Career development and training	−0.001	−0.009	−0.008	−0.026†
	(0.014)	(0.015)	(0.013)	(0.014)
	[0.25]	[0.13]	[0.11]	[0.00]
B. Other in-house policies				
Flexible working hours	0.017	0.020	0.029	0.045**
	(0.017)	(0.021)	(0.022)	(0.016)
	[0.37]	[0.40]	[0.88]	[0.10]
Governance policies	0.011	0.007	0.011	0.012
	(0.008)	(0.008)	(0.010)	(0.008)
	[0.78]	[0.95]	[1.00]	[0.00]
Environmental policies	−0.011	−0.011	0.003	−0.025†
	(0.012)	(0.013)	(0.012)	(0.013)
	[0.08]	[0.87]	[0.19]	[0.01]
C. Own-workforce conditions (standard deviations)				
Trade union representation	0.018	0.005	−0.003	−0.015
	(0.026)	(0.027)	(0.029)	(0.025)
	[0.43]	[0.53]	[0.34]	[0.24]
Employee turnover	0.076	0.057	0.124†	0.051
	(0.054)	(0.054)	(0.064)	(0.054)
	[0.56]	[0.77]	[0.15]	[0.35]
D. Rules for own suppliers outside labor				
Termination of non-compliant suppliers	0.034*	0.025†	0.050**	0.050***
	(0.015)	(0.015)	(0.017)	(0.013)
	[0.44]	[0.75]	[0.71]	[0.03]
ESG training for suppliers	0.023	0.003	0.020	0.044**
	(0.016)	(0.018)	(0.019)	(0.014)
	[0.80]	[0.86]	[0.64]	[0.06]
Environmental supply-chain rules	0.024	0.018	0.019	0.050**
	(0.017)	(0.019)	(0.018)	(0.016)
	[0.51]	[0.94]	[0.06]	[0.00]
E. Records (probability of reporting)				
Reports contractor injuries	0.025*	0.016	0.009	0.029*
	(0.013)	(0.015)	(0.014)	(0.011)
	[0.63]	[0.62]	[0.59]	[0.01]
Reports own-workforce injuries	−0.013	−0.007	−0.014	0.003
	(0.018)	(0.020)	(0.017)	(0.016)
	[0.02]	[0.01]	[0.05]	[0.00]
F. Conduct				
Injuries, contractors (SD)	0.051	0.061	−0.047	0.039
	(0.069)	(0.069)	(0.102)	(0.073)
	[0.17]	[0.22]	[0.25]	[0.11]
Injuries, own workforce (SD)	0.001	0.006	−0.014	−0.007
	(0.045)	(0.046)	(0.053)	(0.041)
	[0.35]	[0.59]	[0.84]	[0.84]
Any safety or employment-conditions incident (RepRisk)	0.007	0.003	0.026	–
	(0.017)	(0.019)	(0.029)	
	[0.12]	[0.10]	[0.17]	
Any forced or child labor incident (RepRisk)	0.002	0.002	−0.022	–
	(0.013)	(0.017)	(0.021)	
	[0.23]	[0.13]	[0.89]	
Compared within disclosure quintiles	Yes	Yes	Yes	

Notes: $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

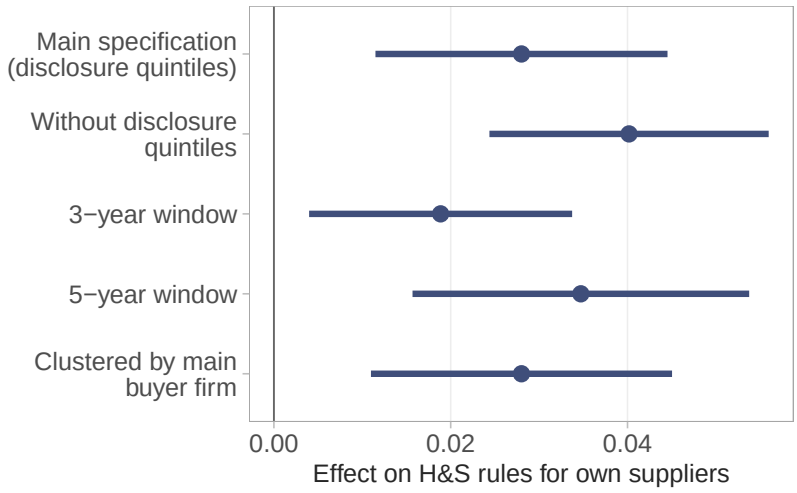
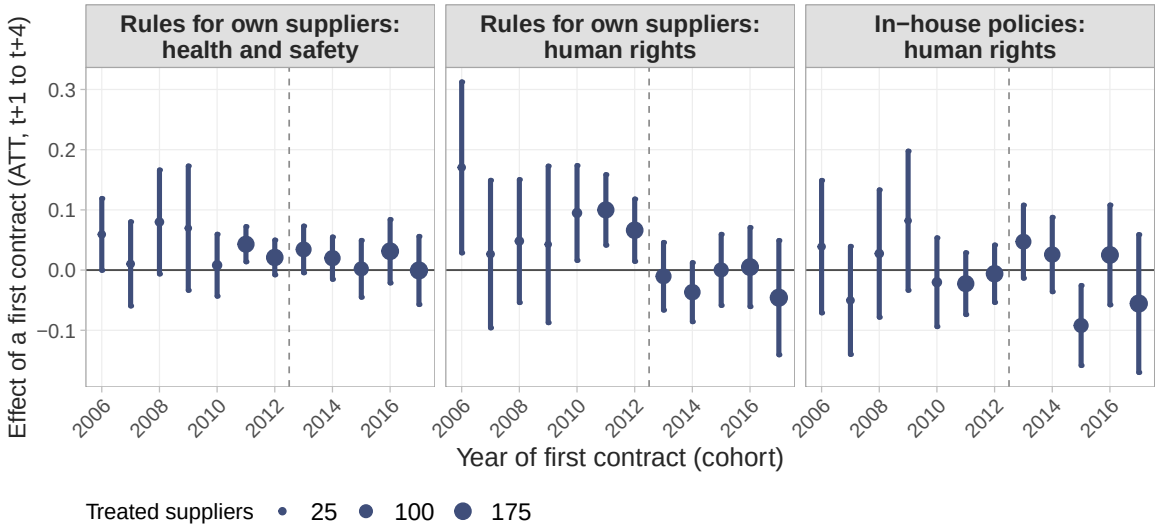


Figure K.2: Robustness of the Entry Effect

Note: The baseline estimate uses baseline-quintile × cohort × year strata. The remaining specifications vary the event window, fixed-effect structure, or clustering of standard errors. Bars report 95% confidence intervals.

Figure K.3: Effect of a First Contract by Cohort



Note: Stacked design of Figure 2 estimated separately for each first-contract year, with 95% confidence intervals. Point size is proportional to the number of treated suppliers in the cohort. The dashed line separates contracts signed through 2012 from later ones.

Table K.3: Anticipation: Using Two Years Before the Contract as the Reference

	(1)	(2)	(3)
Reference year	$t-1$	$t-2$	$t-2$
Disclosure groups cut at	$t-1$	$t-1$	$t-2$
Panel A. Health-and-safety rules for own suppliers			
First contract	0.028**	0.028**	0.031**
	(0.009)	(0.009)	(0.009)
Year before the contract	—	0.000	0.004
		(0.004)	(0.004)
Mean at $t-2$, treated		0.081	
Treated suppliers		1,165	
Panel B. Rules for own suppliers (items, of 5)			
First contract	0.141**	0.145**	0.176***
	(0.043)	(0.046)	(0.047)
Year before the contract	—	0.006	0.034†
		(0.018)	(0.018)
Mean at $t-2$, treated		0.675	
Treated suppliers		1,174	
Panel C. In-house policies (items, of 6)			
First contract	0.030	0.020	0.086
	(0.061)	(0.066)	(0.066)
Year before the contract	—	−0.039	0.034
		(0.026)	(0.026)
Mean at $t-2$, treated		2.719	
Treated suppliers		1,183	

Notes: Stacked difference-in-differences on the first-contract event set, with supplier × cohort and disclosure-quintile × cohort × year fixed effects and standard errors clustered by supplier. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

References for Supporting Information

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