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Activism seeking to improve labor conditions in global supply chains has led transnational corporations to adopt codes of conduct and monitor suppliers for compliance, but it is unclear whether these formal organizational structures raise labor standards. Drawing on thousands of audits conducted by a major social auditor, we identify structural contingencies in the institutional environment and in program design under which codes and monitoring are more likely to be associated with improvements in working conditions. At the institutional level, suppliers improve more when they face greater risk that NGOs and the press will expose harmful working conditions and when their buyers have experienced negative publicity for supply chain working conditions. At the program design level, suppliers improve more when the monitoring regime signals a cooperative approach, when auditors are highly trained, and especially when both are true. These findings should inform strategies for improving working conditions in global supply chains.

Widely reported labor abuses and pressure from consumers and labor activists has prompted high-profile brands to adopt formal organizational structures like codes of labor conduct and social monitoring to address risks in their global supply chains. Nike developed an

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extensive program of codes and monitoring following reports of child labor in its supplier factories; Apple strengthened its supplier monitoring program in response to worker suicides and reports of rampant labor abuses at its Foxconn factory in China; and prominent European apparel brands enhanced their existing programs following the collapse of the Rana Plaza building—in which some of their garments were produced—killing more than 1,000 Bangladeshi factory workers.

Beyond such high-profile cases, codes and monitoring have become ubiquitous in global value chains. To avoid negative publicity generated by accidents and activism, thousands of transnational corporations (TNCs), including all US *Fortune* 500 companies, have adopted codes of conduct that require their suppliers to meet specified workplace standards (McBarnet, 2007) and many conduct "social audits" to monitor and assess suppliers' adherence to those codes (Short, Toffel, and Hugill, 2016). Codes and monitoring are also used by multi-stakeholder initiatives—such as the Roundtable on Sustainable Palm Oil, the Electronics Industry Citizenship Coalition, and the Ethical Trading Initiative—that provide collective fora for private regulation of supply chain practices. Hundreds of thousands of audits are conducted on behalf of TNCs and multi-stakeholder initiatives each year (Gould, 2005), making social auditing an $80 billion industry (AFL-CIO, 2013).

While TNCs generally adopt codes and monitoring for business purposes, including information gathering and reputation management, these organizational structures have been embraced by nongovernmental organizations (NGOs) and other international organizations as part of a broader strategy to improve conditions for supply chain workers (Utting, 2005; LeBaron and Lister, 2015). Codes and monitoring are central to the strategy of NGOs that alternately agitate against and partner with TNCs to encourage ever-stricter standards and more robust

monitoring. United Nations initiatives in the area of business and human rights, like the Global Compact and the Guiding Principles on Business and Human Rights, rely on private standards and monitoring to improve companies' global labor and other human rights practices (Ruggie 2008). Responding to these calls for improvement, leading social auditing firms advertise that their monitoring services will help "both suppliers and customers in implementing sustainable business practices and *improving* workplace conditions in global supply chains [emphasis added]" (UL Responsible Sourcing, 2015; see also Elevate, 2016; Intertek, 2016).

However, it is not clear that codes and monitoring do improve supplier labor practices. Many observers have argued that such organizational structures are, at best, window-dressing (Esbenshade, 2004; Frynas, 2005; Seidman, 2007; Barkemeyer, 2009) or, worse, "organized hypocrisy" (Lim and Tsutsui, 2012: 69)—that is, a calculated ploy to undermine labor organizing efforts (Justice, 2006) and fend off more stringent state regulation (Utting, 2005; Barkemeyer, 2009; Shamir, 2011). Certainly, codes and monitoring bear all the hallmarks of symbolic structures that are likely to be decoupled from actual supplier practices (Meyer and Rowan, 1977).

Nevertheless, we argue that decoupling is not inevitable. We extend the literature on decoupling by investigating the structural contingencies that account for variation in the extent to which supply chain codes and monitoring are associated with improvement in working conditions. Specifically, we ask: how do suppliers' compliance improvement rates differ depending on variations in institutional compliance pressures and in the design of monitoring programs? We also examine the interaction among program design elements. Identifying the conditions under which codes and monitoring are associated with actual improvements in labor conditions is critically important because they are the dominant mode of labor standards

regulation in global value chains. We make no claims about their efficacy relative to other vehicles for improving labor standards, including more robust state-based regulation or worker organizing and empowerment. Rather, our study explores how the efficacy of the existing, highly institutionalized system of codes and monitoring could be enhanced.

We test our hypotheses using a novel dataset of thousands of audits for code-of-conduct compliance conducted at nearly 5,000 factories spanning 13 industries in 66 countries by one of the world's largest supply chain auditing firms. We use regression analysis to estimate a model that predicts a factory's improvement in code compliance based on our hypothesized institutional pressures and program design attributes, controlling for audit and institution characteristics that might also affect improvement. By identifying structural contingencies that favor coupling, our findings (a) challenge the assumption that codes and monitoring will inevitably be decoupled from practices and (b) suggest key considerations that should inform the design and implementation of monitoring strategies to improve conditions in global supply chains.

DECOUPLING OF LABOR CODES AND LABOR PRACTICES

Meyer and Rowan (1977) defined formal organizational structures as sets of practices and procedures that embody rationalized concepts of organizational legitimacy and theorized the conditions under which such structures would be decoupled from actual organizational practice. The decoupling literature strongly suggests that formal organizational structures like labor codes of conduct and social monitoring are likely to be adopted symbolically and decoupled from the suppliers' actual labor practices.

First, there is a strong consensus in the literature that organizational structures adopted to gain legitimacy with external stakeholders rather than to satisfy the task-related efficiency demands of production will be implemented symbolically and decoupled from practices (Meyer

and Rowan, 1977; Boiral, 2007; Bromley and Powell, 2012). It would be difficult to find organizational structures that more "dramatically reflect the myths of their institutional environments instead of the demands of their [organizations'] work activities" (Meyer and Rowan, 1977: 341). Companies initially adopted codes and monitoring to deflect negative publicity and preserve brand reputation (Bartley and Child, 2014). However, the substantive changes in production practices that such structures demand—minimum wage requirements, overtime restrictions, and freedom of association rights—are fundamentally at odds with the economic logic of global value chains that seek to minimize production costs (Locke, 2013).

Second, symbolic structures are more likely to be decoupled in contexts in which efficiency demands are strong and not tempered by countervailing institutional pressures (Meyer and Rowan, 1977; Meyer et al., 1997). Suppliers to global value chains face intense efficiency demands to produce high volume at low cost (Gereffi, Humphrey, and Sturgeon, 2005) and many are in countries with weak regulatory institutions and lax enforcement of labor standards.

Finally, resource constraints impede substantive implementation of formal organizational structures (Meyer et al., 1997; Bromley and Powell, 2012; Lim and Tsutsui, 2012). Suppliers operate on razor-thin margins and many lack the resources to effectively implement code requirements. Thus, the axioms that emerge from the decoupling literature suggest that formal organizational structures like codes and monitoring are likely to be ceremonial window dressing, "implemented, evaluated, and monitored so weakly that they do little to alter daily work routines" (Bromley and Powell, 2012: 489) in ways that improve workers' conditions.

Against this grain, a growing research stream focuses on the conditions under which formal organizational structures adopted symbolically are nevertheless implemented substantively or are "coupled" with organizational practices (Bromley and Powell, 2012; Bartley

and Egels-Zandén, 2015). Consistent with the decoupling literature, most studies that find coupling attribute it to coercive institutional pressures, particularly to forms of state power, such as regulatory inspection and enforcement (Dobbin and Kelly, 2007; Short and Toffel, 2010; Marquis and Qian, 2014). Other studies identifying successful coupling of symbolic structures have been of voluntary programs implemented in the context of broader, legally backed state regulatory regimes such as US antidiscrimination law (Kalev, Dobbin, and Kelly, 2006) and environmental law (Potoski and Prakash, 2005).

Studies specifically investigating suppliers' compliance with labor codes of conduct similarly find that codes and monitoring are associated with better working conditions when combined with government regulatory efforts (e.g., Rodríguez-Garavito, 2005; Seidman, 2007; Amengual, 2010; Locke, Rissing, and Pal, 2013; Toffel, Short, and Ouellet, 2015; Amengual and Chirot, 2016). In addition to the coercive power of the state, studies have found that institutional pressures from unions (Bartley and Egels-Zandén, 2015), a free press (Toffel, Short, and Ouellet, 2015), NGOs (Seidman, 2007; Fransen, 2012; Zajak, 2017), and brands (Oka, 2010a; Bartley and Egels-Zandén, 2015) can induce suppliers to couple their symbolic commitments to codes with their actual labor practices.

Some qualitative studies have expanded the decoupling literature's traditional focus on coercive institutions to investigate how the activities of individual actors inside organizations can create contingencies that promote coupling (Espeland, 1998; Hallett, 2010; Overdevest, 2010; Tilcsik, 2010; Bartley and Egels-Zandén, 2015). For example, in a study of Indonesian apparel and footwear factories, Bartley and Egels-Zandén (2015) find that the coupling of labor codes of conduct and supplier labor practices was contingent on union members' ability to forge and

leverage relationships with brands, international NGOs, and global unions to pressure suppliers to meet their code commitments.

Bartley and Egels-Zandén (2015) highlight the importance of identifying contingencies that can promote coupling in contexts in which theory conventionally predicts a high likelihood of decoupling. To date, such research has focused on the "thorny, on-the-ground processes" (Bartley and Egels-Zandén, 2015: 3) that couple organizational structures and practices in a single firm (Hallett, 2010; Tilcsik, 2010) or in a few firms in the same institutional context (e.g., Bartley and Egels-Zandén, 2015). We extend this research with a large-scale study that examines structural contingencies of coupling operating both inside and outside of supplier organizations across a range of institutional contexts. This builds on existing large-scale studies that examine determinants of coupling at only one level, either institutional (Toffel, Short, and Ouellet, 2015) or organizational (Bird, Short, and Toffel, 2019).

Notably, we measure coupling by the *improvement* in suppliers' compliance with the labor standards contained in codes of conduct. The extent to which suppliers change their practices over time to conform to codes of conduct is a critical marker of whether codes and practices are coupled within supplier organizations. However, in our empirical context, little is known about why some suppliers improve their compliance more than others. Most research in this field has measured levels of supplier compliance at a moment in time rather than improvement over time (e.g., Egels-Zandén, 2007; Locke, Qin, and Brause, 2007; Oka, 2010a, 2010b; Ang et al., 2012; Bartley and Egels-Zandén, 2015; Toffel, Short, and Ouellet, 2015). A few studies have observed that, in the aggregate, supplier compliance with codes has improved over time (Locke, Qin, and Brause, 2007; Shea, Nakayama, and Heymann, 2010; Nadvi et al.,

2011; Ang et al., 2012; Locke, Rissing, and Pal, 2013; Toffel, Short, and Ouellet, 2015). However, these studies have not hypothesized conditions associated with improvement.

We build on a handful of studies that have empirically examined factors associated with code compliance improvement. Some have measured improvement at the national or regional level, leaving open the question of what accounts for variation in individual suppliers' improvement (Weil and Mallo, 2007; Ang et al., 2012). Others identify characteristics shared among small samples of firms that all improved their code compliance, but these studies fail to identify factors that explain why some factories improved more than others (e.g., Egels-Zandén, 2007; Locke, Rissing, and Pal, 2013). Bird, Short, and Toffel (2019) explain variation in improvement across factories based on the internal structural configuration of supplier organizations, but do not look at influences outside the organization. Our study contributes to this literature by identifying both internal and external structural contingencies that explain variation in supplier improvement.

Our hypotheses investigate the coupling potential of structural contingencies at two levels: institutional and programmatic. Specifically, we hypothesize how the coupling of labor codes of conduct and supplier labor practices is likely to be associated with (a) institutional pressure from civil society groups on suppliers and brands and (b) key design features of the monitoring programs that brands adopt. We focus on these structural contingencies for two reasons. First, they are contingencies over which either brands or activists might exercise influence. Brands, for instance, must choose how to design their auditing programs and activists must choose where to target their limited resources and what types of pressure to apply. Our hypothesized variables yield insights that can inform these decisions. Second, with few

exceptions (e.g., Kalev, Dobbin, and Kelly, 2006), little attention has been paid to the influence of program design on coupling outcomes.

Institutional Compliance Pressures

In their fight to improve supply chain labor conditions, "[a]ctivists' main weapon against corporations is their ability to threaten corporate reputations by exposing malfeasance" (King, 2014: 203). Global buyers are particularly sensitive to the possibility that their suppliers' labor abuses will be exposed, because even one incident in the supply chain can damage carefully cultivated corporate reputations (Oka, 2010a). Below, we explore two distinct but related sources of institutional pressure that increase reputational risk for global buyers: (a) the ability of civil society actors to discover and expose supplier abuses and (b) buyers' past exposure to negative publicity about their suppliers' practices.

Institutional pressure on suppliers. Although government inspection regimes are often weak in countries where suppliers are located, research suggests that civil society actors like NGOs and the press can provide monitoring functions and expose wrongdoing (Seidman, 2007; Fransen, 2012; Zajak, 2017). The local press and local NGOs play symbiotic roles in transnational advocacy networks that promote global norms such as labor standards and human rights (Keck and Sikkink, 1998: 3). The high-profile, international NGOs that are often at the center of such networks depend on local NGOs to collect information about violations of global norms by local actors. In fact, some recent studies have found local organizations to be more important than their global counterparts in transnational advocacy campaigns (Zajak, 2017). The strategy of the global anti-sweatshop movement has been to work with local NGO partners to identify which local firms supply targeted global brands and to do the painstaking investigative work required to reveal exploitive labor practices at these suppliers (Bartley and Child, 2014;

Zajak, 2017). Local NGOs, in turn, depend on domestic media and domestic channels of communication to make exploitive practices known to their more powerful international counterparts in the advocacy network (Keck and Sikkink, 1998; Bartley and Child, 2014; Zajak 2017). The more free and open these information channels, the more likely local abuses are to attract local and ultimately international attention, condemnation, and discipline (King, 2014).

Research on civil society pressure and standards compliance has focused on how monitoring by civil society actors affects compliance *levels* at a particular point in time. Toffel, Short, and Ouellet (2015), for instance, demonstrated that suppliers in countries with more press freedom exhibit greater compliance with codes of conduct. However, it is not clear that the mechanisms fostering high compliance *levels* will also foster *improvements* in compliance. For instance, in high-compliance environments, there may be less room to improve due to a lack of low-hanging fruit (Chatterji and Toffel, 2010) or there may simply be less pressure to improve. Similarly, it is not clear that the presence of civil society actors like NGOs will be related to compliance improvement in the same way that it is related to compliance levels. For instance, domestic environments with very low or very high labor-standards compliance might attract more NGOs, but it is a distinct and important question whether NGOs will help *improve* compliance.

We argue that institutional pressure generated by civil society actors like the press and NGOs, particularly those two in combination, will be associated with improvements in supplier compliance. Suppliers whose failure to meet the global norms prescribed by codes of conduct is documented in audits become attractive targets for transnational advocacy networks seeking to

raise international labor and human rights standards.[1] These networks seek to identify violators of global norms and induce them to change (Keck and Sikkink, 1998: 3). Local NGOs in these networks see their role as "promoting change by reporting facts" that can attract the attention and support of international NGOs, press, and policymakers (Keck and Sikkink, 1998: 19). A free local press helps them discover and publicize labor abuses at suppliers, increasing the prospect that the suppliers—and others like them—will be disciplined by their buyers or will suffer domestic political, legal, or economic consequences (Fransen, 2012; Berliner et al., 2015; Zajak, 2017).

We therefore hypothesize:

> Hypothesis 1 (H1): Suppliers will improve more when located in institutional environments in which there is greater potential for civil society monitoring mechanisms to expose noncompliance with codes of conduct.

Institutional pressure on buyers. The impact of the institutional pressures described above depend not only on the probability that suppliers' wrongdoing will be exposed, but also on buyers' reaction to such revelations. The reputational stakes of exposure are higher for some buyers than for others. Buyers with particularly high-value reputations might be acutely sensitive to negative publicity (Abito, Besanko, and Diermeier, 2016). Indeed, in the supply chain labor context, research has demonstrated that highly reputation-conscious buyers are more likely to work with suppliers that better comply with labor standards (Oka, 2010a).

[1] In a very different context, studies have suggested that activists and the press are less likely to target the worst-behaved companies than companies that already have strong reputations for social responsibility performance (Luo, Meier, and Oberholzer-Gee, 2012; Bartley and Child, 2014) or companies that have already adopted extensive organizational structures to implement their corporate social responsibility (CSR) initiatives (McDonnell, King, and Soule, 2015). Those studies focused on activism directed toward branded multinational companies with reputations to protect and argued that activists can exercise more leverage over such firms because they face greater financial consequences of reputational damage. Such is not the case with suppliers in developing countries. They are unbranded, largely invisible to consumers, and thus more insulated from reputational threat. We believe that activists will select their targets very differently in these contexts and will attempt to identify the worst practices by local suppliers in order to gain the most leverage over the global brands that are their ultimate targets.

Less is known, however, about how buyers that have experienced negative reputation events might respond to the threat of additional reputational damage. It has been theorized that firms facing the prospect of reputational threats from activists, as many multinational buyers do, will attempt to forestall trouble by investing in self-regulatory activities (Abito, Besanko, and Diermeier, 2016). Buyers that have already suffered "reputational shocks" through criticism by or confrontation with activists are especially likely to invest in self-regulatory measures to avoid additional reputational harm (Abito, Besanko, and Diermeier, 2016). Because a damaged reputation can invite more activism, firms that have suffered reputational shocks are further incentivized to protect themselves through self-regulation (Abito, Besanko, and Diermeier, 2016; Dorobantu, Henisz, and Nartey, 2017). Empirical studies confirm that companies with more reputational damage in the past are more likely to take actions to protect their reputation (Kotchen and Moon, 2012; McDonnell and King, 2013; McDonnell, King, and Soule, 2015). What these studies do not reveal, however, is whether such protective measures reduce the social harms that gave rise to the reputational harm.

We argue that buyers who have had their reputation compromised by past revelations about their suppliers' harmful practices are more likely than other buyers to make efforts to ensure that their suppliers correct abuses. We therefore hypothesize:

> Hypothesis 2 (H2): Suppliers will improve more when they produce for buyers that have already been publicly exposed for harms to workers in their supply chain.

Design of Monitoring Program

A monitoring system has numerous components, from the stringency of the underlying substantive standards to the frequency and rigor of inspections to the composition of the inspection team. All of these design features have implications for suppliers' social compliance and compliance improvement. We focus on auditor training and the pre-announcement of audits,

features which might facilitate the transfer of compliance-related knowledge from auditors to suppliers, thus promoting compliance.

Auditor training. There is much skepticism about whether social auditing can foster improvement and questions have been raised about the competence of the auditors and the integrity of the auditing process (O'Rourke, 2002; Esbenshade, 2004; LeBaron and Lister, 2015). Critics charge that auditors lack the knowledge and independence to detect labor abuses (O'Rourke, 2002; Esbenshade, 2004; Locke, Amengual, and Mangla, 2009; AFL-CIO, 2013), that they shade their findings depending on the client's perceived interests (LeBaron and Lister, 2015), that they are easily duped by managers who cook the books and coach employees to lie about conditions (AFL-CIO, 2013), and that some are outright corrupt (Clean Clothes Campaign, 2005). Others see auditors stuck in an ever-more-sophisticated cat-and-mouse game with suppliers that maintain fake wage and hours records and coach their workers on how to answer auditors' questions (Power, Ng, and Singh, 2008; LeBaron and Lister, 2015). Some, including critics of social auditing, have suggested that more highly trained auditors could be more effective (Locke, Amengual, and Mangla, 2009; AFL-CIO, 2013) and research has indeed found that better-trained auditors identify more violations (Short, Toffel, and Hugill, 2016).

We argue here that training will likewise enable auditors to help suppliers improve following an audit. Studies have documented that social auditors play an important "pedagogical" role, often instructing factory managers how to remedy the violations discovered (Amengual, 2010). Our interviews with auditors and managers at social auditing firms reveal that the auditors' training typically teaches them how to find violations and what conditions tend to cause them. Such training helps auditors identify root causes and develop compliance solutions. Recent evidence indicates that government inspections can prompt improved working conditions

(Levine, Toffel, and Johnson, 2012), suggesting that inspectors might play a dual role of assessing conditions and suggesting how to improve. Studies in the knowledge transfer literature find that certain types of training can improve the ability to convey information in personal interactions (Thompson, Gentner, and Loewenstein, 2000; Loewenstein, Thompson, and Gentner, 2003; Nadler, Thompson, and Van Boven, 2003) and that information is more likely to be absorbed and acted upon when it comes from a source perceived to have expertise (Borgatti and Cross, 2003; Thomas-Hunt, Ogden, and Neale, 2003; Reinholt, Pederson, and Foss, 2011). We therefore hypothesize:

> Hypothesis 3 (H3): Suppliers will improve more following audits conducted by audit teams that are more highly trained.

Signaling a cooperative approach to social auditing. There is significant debate about the approach buyers should take to audits, including whether they should be conducted in a policing style or a more cooperative style and, relatedly, whether they should be announced in advance. Some argue that pre-announcing gives suppliers time to cover up bad behavior (Clean Clothes Campaign, 2005; AFL-CIO, 2013; LeBaron and Lister, 2015) and there is empirical evidence for that (Gray, 2006; Marks, 2012; Toffel, Short, and Ouellet, 2015). Worker rights advocates therefore have long favored unannounced audits (Frenkel and Scott, 2002).

While it seems clear that unannounced audits will reveal more information about supplier wrongdoing, it is less clear whether they will foster improvement. It is possible that suppliers might be motivated to improve their practices if they know that they can be caught at any time through a surprise audit. However, most buyers do not conduct audits regularly enough for this to be a serious deterrent. Moreover, studies have suggested that a punitive, policing-style approach to monitoring can undermine compliance by dampening intrinsic motivations to comply (Short and Toffel, 2010) and fostering resentment among the regulated community that

can lead to backlash against regulatory requirements (Bardach and Kagan, 1982; Ayres and Braithwaite, 1992). Social auditors report that unannounced audits meant "to catch managers unaware aggravate[] the relationship between buyers and suppliers ... [and] make it difficult to achieve any sustainable change" (Gould, 2005: 28).

A consensus has begun to emerge among academics and practitioners that suppliers are more likely to improve with a less punitive, more cooperative approach to monitoring (Locke, Amengual, and Mangla, 2009). Rather than using audits to detect violations and threaten sanctions, the cooperative approach provides an opportunity "to engage in a process of root-cause analysis, joint problem solving, information sharing, and the diffusion of best practices that is in the mutual self-interest of the supplier, the auditors, and the global corporations for which they work" (Locke, Amengual, and Mangla, 2009: 321). The underlying theory, developed most extensively by Ayres and Braithwaite in *Responsive Regulation* (1992), is that regulators' signals of cooperation will be reciprocated with compliance. Studies have suggested that a cooperative approach to monitoring can help buyers, suppliers, and auditors develop trusting relationships that are more likely than punitive, arms-length approaches to improve compliance (Frenkel and Scott, 2002; Locke and Romis, 2007).

Although we do not observe the micro-level interactions among the buyers, suppliers, and auditors in our sample and so cannot assess whether these parties have trusting or cooperative relationships, we argue that buyers formally signal trust and a cooperative approach to monitoring when they give suppliers advance notice of audits. Our interviews with ethical supply chain managers and social auditors consistently indicated that unannounced audits convey distrust and a punitive or policing approach to monitoring, with auditors sometimes denied entry to factories, whereas announced audits convey a more trusting and cooperative approach. At the

very least, announcing an audit indicates trust in the formal economic sense of making the buyer vulnerable to the possibility of opportunism on the part of suppliers who might use the time afforded by advance notice to hide their misdeeds (Mayer, Davis, and Schoorman, 1995). To be clear, we are not arguing that providing advance notice of audits will *cause* suppliers to improve more rapidly. Instead, our argument is that announcing audits signals to suppliers that they are subject to a more cooperative (less punitive) monitoring regime, that these suppliers are more likely than others to perceive that they are trusted by their buyers and that, consequently, these suppliers will be more willing to reciprocate that perceived trust by investing in improvement. Thus, although an announced audit might uncover fewer violations, we hypothesize:

> Hypothesis 4 (H4): Suppliers that receive advance notice of audits will improve more than other suppliers.

Auditor training in the context of a cooperative approach to auditing. Auditing might be particularly effective in improving supplier practices when more knowledgeable auditors engage with suppliers that are willing and able to receive the information. A substantial literature suggests that individuals and organizations share and absorb knowledge more effectively in collaborative, cooperative, and trusting relationships (Szulanski, 1996; Dyer and Chu, 2003; McEvily, Perrone, and Zaheer, 2003). For instance, Cheng, Yeh, and Tu (2008) show that the transfer of green production practices from buyers to suppliers is most effective when buyers let suppliers participate in decision making *and* when those buyers and suppliers trust one another. Buyers and suppliers surveyed by Oka (2010b) similarly reported more learning about compliance with workplace standards in trusting relationships. As we argue above, announcing audits signals trust in the supplier; we therefore expect suppliers who receive advance notice to be more receptive to the knowledge auditors convey in those audits. Because highly trained auditors likely will have more and/or higher-quality information to convey, we hypothesize the

following moderated relationship:

> Hypothesis 5 (H5): Suppliers audited by highly trained auditors will improve more following announced audits than following unannounced audits.

Recognizing that different violation types may improve at different rates under the hypothesized conditions, we include below an extension to our analysis that disaggregates our dependent variable by violation type to better understand which categories are more likely to improve under which conditions.

DATA AND METHOD

Empirical Context and Sample

We tested our hypotheses using data from code-of-conduct audits conducted by a large social auditing company (henceforth, the "social auditor") that requested anonymity. During our sample period, the social auditor served *Fortune* 500 companies and was accredited to conduct audits of several leading social compliance standards. It operated in more than 100 countries and its staff spoke more than 20 languages. The data include audits conducted from 2004 through 2009, the most recent six-year period for which we could obtain access. Various characteristics of the audits, auditors, and audited suppliers were provided, including unique identifiers (but not names) for the auditors, the suppliers, and the buyers on whose behalf the audits were conducted. While many buyers issue their own supplier codes of conduct, our discussions with the social auditor revealed that the differences between these codes are slight, which gave us confidence in treating all of these audits similarly.

Because our empirical specification requires data from a supplier's focal (current) audit and its prior audit, our sample is limited to those suppliers for which our data includes at least two audits. Our estimation sample consists of 8,677 focal audits conducted at 4,940 suppliers

spanning 13 industries in 66 countries. In our sample, factories are audited an average of every 202 days, with an interquartile range of 83 to 293 days. Those categorized as annual audits are conducted every 344 days on average. The most common industries in our sample are garments, accessories, electronics, and toys (see Table 1). The majority of the audits took place in China; many of the rest took place elsewhere in Asia (Bangladesh, India, Indonesia, the Philippines, and Vietnam) and in North America (Mexico and the United States) (see Table 2). Auditors tend not to specialize by industry, but instead are assigned to audits largely based on their geographic proximity (to minimize travel costs and time) and their availability and to ensure that every audit team includes a trained lead auditor.[2]

[Insert Tables 1 and 2 here]

Dependent Variable

Our dependent variable measures the change in a supplier's compliance between its prior and focal audits. To avoid undue influence of outliers, we used a metric akin to the difference in log violations but is calculable even when violation counts are zero. To calculate *improvement*, we divide the number of violations from the focal audit plus 1 by the number of violations from the previous audit plus 1, take the natural log of that ratio, and then multiply the result by –1 so that higher values reflect greater improvement:

$$y_{i,t} = - 1 \times \ln [(V_{i,t} + 1) / (V_{i,t-1} + 1)],$$

where $V_{i,t}$ is the number of violations for supplier i audited at time t that pertain to child labor, forced or compulsory labor, working hours, occupational safety and health, minimum wage, treatment of foreign workers and subcontractors, and disciplinary practices (there are 75 possible

[2] Nearly 80% of the auditors in our sample conducted audits in just one country. Of those who worked in more than one, most went only to nearby countries. The average auditor in our sample conducted audits of factories in nearly 5 of the 13 industries in our sample and nearly 25% conducted audits in 8 or more industries.

violations across these domains) and where $V_{i,t-1}$ is the comparable figure from the prior audit at time t-1.[3] We add 1, the minimum non-zero value in the data, to both the numerator and the denominator to avoid losing observations in which either the current or prior audit yielded zero violations.[4] This metric, rather than the simple difference in violations, facilitates proportional comparisons between suppliers.[5] It also provides a more reliable estimate than a percent change metric, which can be overly sensitive to outliers and can inflate large changes.[6] Our log form reduces skewness[7] and enables a straightforward interpretation of our coefficients as elasticities. Multiplying the log ratio by –1 results in larger values corresponding to greater improvement, which eases interpretation.

Independent Variables

The risk that the labor abuses documented in a social audit will be exposed and sanctioned depends on press freedom and NGO presence. We measure press freedom using the Press Freedom Index from Reporters without Borders, which reflects the extent to which journalists in a given country faced direct and indirect threats such as imprisonment, physical attacks, and censorship in a given year, a metric used by others for the same purpose (e.g.,

[3] The maximum possible number violations in each category were: child labor (7), forced or compulsory labor (5), working hours (7), occupational safety and health (31), minimum wage (15), treatment of foreign workers and subcontractors (4), and disciplinary practices (6). We excluded violations that, according to our data provider, do not apply to all suppliers (dormitory conditions and canteen violations) or were interpreted differently by auditors in different countries (freedom of association, the right to organize and bargain collectively, legal or client requirements).

[4] Though only 4% of the prior audits in our sample had zero violations, such suppliers might be distinctively capable of exemplary performance and allowing these observations to drop out of the sample risks introducing bias. Adding 0.1 to the numerator and denominator, instead of adding 1, yielded nearly identical results.

[5] For example, our metric considers the proportional reduction from 12 to 6 violations at a large supplier to be equivalent to a small supplier's reduction from 4 to 2 violations, whereas a difference metric would consider the former to be three times the magnitude of the latter.

[6] For example, skewness declines by a factor of 10, from a value of 4.2 for percent change to a mere 0.4, for our *improvement* metric and kurtosis declines by a factor of nearly 7, from 30.5 for percent change to 4.5, for *improvement*.

[7] The simple ratio of violations at an establishment's focal audit to those at its prior audit is highly skewed: it ranges from 0 to 19 and has a mean of 1.1, a standard deviation of 1.3, skewness of 4.2, and kurtosis of 30.5. Models that estimate this simple ratio as a dependent variable would be quite vulnerable to outliers driving their results.

Faccio, 2006; Cannizzaro and Weiner, 2015). We reverse-code the raw Press Freedom Index so that higher values indicate greater press freedom, rescale the result to range from 0 to 1, and take the log (after adding 1) to reduce skew. We measure NGO density as the number of NGOs in the supplier's country per million population—an approach used by others (e.g., Hafner-Burton and Tsutsui, 2005; Chih, Chih, and Chen, 2010)—which we also log to reduce skew. We obtained NGO data from the Union of International Associations and population data from the US Census Bureau's International Data Base. Because both the press and NGOs are critical actors in the transnational advocacy networks that we theorize will generate exposure risks, it is important to include both variables in our analysis. However, press freedom and NGO density are highly correlated (ρ=0.83). We therefore use principle components analysis as a data reduction technique (Hair et al., 1998; Kennedy, 2008), an approach others have used for the same purpose (e.g., Gulati and Sytch, 2007; Perkins, 2014; Guillén and Capron, 2015). The first component's eigenvalue of 1.85 is the only one to exceed the conventional threshold of 1 and it explains 92.3 percent of the variance between press freedom and NGO density. We refer to this first component as *pressure on suppliers.*[8]

Institutional pressure to improve labor standards is directed not only at suppliers, but also—in fact, largely—at multinational buyers. To operationalize that dimension of institutional pressure, we rely on negative media reports, as others have done for similar purposes (Fiaschi, Giuliani, and Nieri, 2013; Kölbel, Busch, and Jancso, 2017). In particular, we consider whether a supplier serves a buyer that had recently been associated with supply chain labor abuses revealed in a news article or NGO report. To measure this, we relied on the database of media articles and reports on supply chain labor abuses compiled by the Business & Human Rights Resource

[8] A robustness test that includes *press freedom* and *NGO density* in our models instead of *pressure on suppliers* yields the same inferences as our primary models.

Centre (BHRRC). BHRRC serves as a labor abuse information clearinghouse and research organization to "track the human rights policy and performance of over 7000 companies in over 180 countries, making information publicly available" (Business & Human Rights Resource Centre, 2017). It gathers news articles from around the world linking companies to human rights abuses and conveys this information to its 177,000 monthly website visitors and via its e-newsletter issued to thousands of subscribers, including activists, businesses, governments, global media, and investors.[9] We measure *pressure on buyers (prior audit)* by taking the number of times the buyer on whose behalf the audit was conducted appeared in articles in this database during the year prior to the audit, adding 1, and logging that sum to reduce skew.[10]

We calculate auditor training as the number of audit training courses an auditor had taken, based on data provided by the social auditor.[11] Because audits are typically conducted by an audit team, we measure *maximum auditor training* as the largest number of training courses that any one team member had undergone by the time the audit was conducted, which we log after adding 1 to reduce skew and then standardize to facilitate interpretation.[12] The maximum number of training courses for audit teams averaged 6.9. We use the maximum training of any one team member because this measures the greatest potential to identify code of conduct violations and to transfer knowledge on how to remediate them.

[9] BHRRC invites companies to respond to any post that names them and reports that 86% of companies do so (Business & Human Rights Resource Centre, 2017), suggesting that companies are attentive to these reports.
[10] Because many buyers appeared in no such articles, we conducted robustness tests that measured this using a dummy (rather than a count) coded 1 for an audit of a supplier whose buyer was featured in at least one article in this database in the prior year, and coded 0 otherwise. Estimates yielded nearly identical results when we added this dummy to our primary specification and when we substituted it for our primary measure of *pressure on buyers (prior audit)*.
[11] Training regarding audit skills helps auditors generally identify violations. Training regarding specific audit topics covers issues relevant to a specific industry, region, or supplier; for example, child labor at a particular supplier. Training on client issues educates auditors about certain codes of conduct the client is particularly concerned about or a specific protocol the client has agreed to comply with, such as SA8000.
[12] Robustness tests (not reported) indicate that using a team's average auditor training instead of its maximum auditor training yields nearly identical results.

Whether an audit was expected or a surprise was measured by *announced*, a dichotomous variable coded 1 when the supplier had advance notice of the audit date and 0 for unannounced audits, based on data from the social auditor. Whether an audit is announced or unannounced is typically determined by the buyer. In our sample, 76 percent of the audits were announced.[13]

Audit-level Control Variables

We control for audit-level factors by constructing variables from data provided by the social auditor. We control for the violations in the prior audit because suppliers whose prior audit yielded many violations face a different opportunity set than those with a "cleaner" history, which may influence their likelihood of improvement. *Violations (prior audit)* is the number of violations from a prior audit, top-coded at the 99th percentile of the sample distribution (25 violations) to reduce the potential impact of outliers and taking the log (after adding 1).[14]

Because prior research indicates that auditing is less stringent when suppliers pay their own auditors (Jiang, Stanford, and Xie, 2012; Duflo et al., 2013; Short and Toffel, 2016), we created three dummy variables to indicate who paid for the audit: *paid by the supplier or third party, paid by the buyer* (on whose behalf the audit was conducted), and *paid by unknown entity* (when we lacked information about who paid).

[13] We found no evidence that a supplier's prior violation count or the duration of the buyer-supplier relationship affected the propensity for a supplier's audit to be announced (versus unannounced). Specifically, we estimated a logistic regression that predicted whether an audit was announced (versus unannounced) based on the duration of the buyer-supplier relationship (proxied by whether an audit was the 2nd, 3rd, 4th, or 5th or more conducted of this supplier for the same buyer), buyer size (log employment), and country (dummies), controlling for supplier industry (dummies) and violations reported in the prior audit. Results indicate that buyer-supplier relationship is not a significant determinant of an audit being announced or unannounced. Larger buyers were more likely to have announced audits and the supplier-country dummies were jointly significant, as were the buyer-country dummies. The regression results that test our hypotheses (reported in Table 4) are unlikely to be contaminated by omitted variable bias associated with factors that predict whether an audit will be announced or unannounced because those regression models control for the statistically significant factors correlated with this decision.

[14] Audits in our sample have a maximum of 75 violations, but such high counts were very rare. Seeking to depict average relationships, we top-code violations at the 99th percentile (25 violations) in order to avoid rare exceptions influencing our results.

Re-audits typically have a more limited scope, tending to focus on concerns raised at the prior audit. Because this could mechanically affect improvement rates, we include three dummy variables as controls: (a) *prior audit was re-audit, but focal audit was not*; (b) *focal audit was re-audit, but prior audit was not*; (c) *prior and focal audit were re-audits*. The baseline condition is that neither was a re-audit.[15]

Our interviews with social auditors—at the firm that provided our data and at others—indicated that the staff hours required to conduct an audit is a reasonable proxy for factory size and complexity, which could be associated with improvement but for which we lack direct measures. In addition, more staff hours in a prior audit might offer more opportunity to transfer information between the audit team and the supplier.[16] We therefore control for *audit duration (prior audit)*, which we calculated by taking the log (after adding 1) of the number of staff hours required to conduct the prior audit.

Audit teams including individuals who had previously audited the supplier have been shown to report fewer violations than teams whose members have no prior history there (Short, Toffel, and Hugill, 2016). We therefore created *previous auditor*, a dummy coded 1 when at least one member of the audit team had participated in the prior audit of that supplier and 0 otherwise. Because suppliers may remediate compliance problems identified at prior audits and thus face increasing mitigation costs, we create *audit sequence* as a count variable to denote each

[15] We have several reasons for believing that this approach adequately controls for the possible influence that the prior or focal audit being a re-audit might have on improvement. We found no evidence that audits categorized as a re-audit averaged fewer violations than routine audits, whether based on a t-test, a simple negative binomial model, or a negative binomial model that also included industry dummies.

[16] We include in our model a dummy variable to denote the nearly 50% of observations for which the number of staff hours required to conduct the prior audit was missing from the database and where we thus recoded *auditor exposure (prior audit)* observations from missing values to 0. This common econometric approach is algebraically equivalent to recoding those missing values with the variable's mean (Greene, 2007: 62).

supplier's first audit in the estimation sample, its second audit, and so on.[17] In our models, we flexibly control for *audit sequence* by including a dummy for each value, which avoids imposing the assumption that audit sequence has a linear influence on *improvement.*

Because an audit team's gender composition has been shown to affect audit results (Short, Toffel, and Hugill, 2016), we include three dummy variables: *all-female audit team*, *mixed-gender audit team*, and *all-male audit team.*[18]

We control for team experience, which has been shown to affect reported violations (Short, Toffel, and Hugill, 2016). We measure the *maximum auditor tenure* of each team as the maximum years of service with the social auditor among all team members. We include in our model both *maximum auditor tenure* and its squared value because the influence of experience on reported violations has been found to be nonlinear (Short, Toffel, and Hugill, 2016).[19]

Institutional Control Variables

Several factors pertaining to the supplier's institutional environment have been shown to affect violation rates (Toffel, Short, and Ouellet, 2015) and could affect improvement rates; we therefore control for them at the prior audit.[20] A supplier country's dependence on foreign direct investment (FDI) might influence the extent to which the supplier perceives the need to respond to international pressure to improve how its factories are managed. We therefore control for each

[17] A supplier's first observation in the estimation sample (*audit sequence* = 1) incorporates information from its focal audit (i,t) and prior audit (i,t-1) because our dependent variable incorporates both of their audit scores. *Audit sequence* = 1 for 57% of the observations in our estimation sample, 2 for 22%, 3 for 10%, and 4 or more for 10%.
[18] We use dummies because 97% of our sample's audit teams were of just three composition mixes: all-female (50%), all-male (32%), or evenly split (15%); the remaining 3% had other mixed ratios.
[19] Robustness tests (not reported) indicate that using teams' *average* auditor tenure yields nearly identical results.
[20] We only include the lagged value for these country-level variables because they are very stable over the period of time between two consecutive supplier audits.

supplier country's percentage of gross domestic product (GDP) made up of FDI (*FDI inflows*) in the year of the prior audit, based on World Bank data.[21]

Because domestic legal protections for labor rights could influence how much pressure to improve suppliers perceive they are under, we obtain *labor laws* scores from Mosley (2011).[22] These scores measure the extent to which domestic law provides collective labor rights such as the rights to join unions and strike, whether government approval is required for collective bargaining, and whether laws restrict worker rights in export processing zones (Greenhill, Mosley, and Prakash, 2009). Because these scores are available only through 2002—before our sample period begins—we use the 2002 values for all years of our analysis. Studies have used this index to measure the stringency of country-level workers' rights protections generally (e.g., Greenhill, Mosley, and Prakash, 2009; Dean, 2015; Toffel, Short, and Ouellet, 2015; Fransen and Burgoon, 2017) on the basis that collective rights are foundational to other workers' rights like wages, benefits, and working conditions (Greenhill, Mosley, and Prakash, 2009).

Because country-level wealth and differences in wealth between supplier and buyer countries could influence improvement rates, we control for *GDP per capita (prior audit)* and *GDP per capita in buyer country (prior audit)*, obtained from World Bank data. We control for potential differences in coercive pressure that buyers might exert based on their size by obtaining

[21] *FDI inflows* measures net inflows of foreign direct investment (that is, inflows less divestment during the previous year) used to acquire a lasting management interest (that is, 10% or more of a company's voting stock was purchased by international entities) in companies in the supplier's country. It is composed of equity capital, earnings reinvestment, and other short-term and long-term capital, as shown in the country's balance of payments.
[22] We find nearly identical results when, as a robustness test, we substitute for labor laws two alternative measures of the stringency of the domestic legal environment: the World Bank rule of law score and the number of ILO labor treaties the country has ratified.

annual values of employment from Amadeus, Capital IQ, Hoovers, or Thomson ONE Banker, with which we create *log buyer employment (prior audit),* logging to reduce skew.[23]

Summary statistics are reported in Table 3.[24]

[Insert Table 3 here]

ESTIMATION AND RESULTS

We test our hypotheses by estimating a model that predicts *improvement* based on the independent and control variables described above and several additional control variables explained below. Our model uses log versions of our continuous independent and control variables (except *pressure on suppliers*, which was created using principle components analysis) both to reflect our sense that they have a diminishing marginal influence on improvement and to diminish the potential impact of outliers. In our specification, each observation includes variables measured at an establishment's focal audit and prior audit; therefore all establishments in our estimation sample have had at least two audits.

Whereas our hypothesized variables pertain to a supplier's prior audit, these same factors pertaining to the focal audit might influence the number of violations reported in that focal audit, which is used to construct our dependent variable. Since failing to account for these factors could bias our estimates, we also control for *maximum auditor training (focal audit)* and *announced (focal audit).* Controlling for maximum auditor training at the focal audit prevents us from misattributing a supplier's reduction of violations to situations in which an establishment's focal audit team was less highly trained than the prior one. Similarly, controlling for whether the focal audit is announced or unannounced prevents us from mistakenly attributing a supplier's

[23] The social auditor enabled us to append these variables to a list of buyer companies (which they provided to us without any other data) and subsequently provided the de-identified dataset.
[24] Correlations are reported in Table A1 in the Appendix.

reduction of focal-audit violations to situations in which advanced warning allowed it to fix or hide problems. We do not include the focal-audit values of *pressure on suppliers* or *pressure on buyers* because they are very stable over time—their respective correlations between prior and focal audits is 0.99 and 0.89—and including them would substantially increase multicollinearity while adding almost no new information.

Because (a) several audit design elements and audit team characteristics at the prior audit could influence violations recorded in that audit and (b) these same factors at the focal audit could influence violations recorded in that audit, we include most audit-level controls—*paid by supplier or third party, paid by unknown entity, re-audit, previous auditor, all-female audit team, mixed-gender audit team,* and *maximum auditor tenure*—in the model twice to control for them at both the prior and focal audits.

We also include industry fixed effects and year fixed effects to control for potential differences in improvement rates between suppliers in different industries and between the years in our sample. Because suppliers might respond differently to buyers in different institutional contexts exerting varying levels of pressure (Toffel, Short, and Ouellet, 2015), we include fixed effects for buyer countries.[25] We log *maximum auditor training (prior audit)* and *audit duration (prior audit)* to reduce skew and then standardize them to facilitate an elasticity interpretation of coefficients in response to a one-standard-deviation change. We use the log form of all other continuous variables to facilitate their interpretations as elasticities.

[25] While we have 17 buyer countries in our sample, 89% of the observations correspond to just two. We therefore pursue a more conservative approach of including buyer-country fixed effects, controlling for differences in prosocial attitude in the buyer country as well as for all other buyer country attributes that are relatively stable during our sample period.

Empirical Results

For context, we note that suppliers in our sample averaged 7.2 violations in their prior audit and 5.6 violations in their focal audit, an average improvement of 1.6 violations. This 22-percent improvement rate (calculated as 1.6 ÷ 7.2) corresponds to the sample average *improvement* rate of 0.22 reported in the summary statistics (Table 3).[26]

We estimate our models using ordinary least squares (OLS) regression, clustering standard errors by the supplier's country, the most aggregated level of our explanatory variables.[27]

[Insert Table 4 here]

We test Hypotheses 1–4 with Model 1 and report results in Table 4. The statistically significant positive coefficient on *pressure on suppliers (prior audit)* ($\beta = 0.089$; $p < 0.01$) indicates that suppliers tend to improve more in countries in which civil society monitoring has greater potential to expose noncompliance with codes of conduct, which supports H1. The coefficient magnitude indicates that a one-standard-deviation increase in *pressure on suppliers (prior audit)* (such as a change from Vietnam to the Philippines) is associated with an increase in *improvement* from an average of 22 percent to 32.6 percent, based on average predictions across our sample.[28] This 32.6-percent improvement from the baseline average of 7.2 violations

[26] 16.3% of the variation in the number of *violations* comes from the factory-country level, 48.3% comes from the factory level, and the remaining 35.4% comes from other factors at the observation level. We also decomposed variation in *improvement* (our dependent variable) by using a mixed model to estimate our primary specification and found that 5.2% of the variation in the number of violations comes from the factory-country level, 5.0% comes from the factory level, and the remaining 89.9% comes from other factors at the observation level.

[27] Our results are robust to several alternative estimation approaches, including (a) two-way clustering standard errors at the supplier-country and buyer-company levels; (b) estimating a cross-sectional model (i.e., one observation per factory) on the factory means of all variables; and (c) estimating a random effects model using factory-level random effects.

[28] 32.6% is calculated by adding to 0.22 the product of 0.089 (the coefficient on *pressure on suppliers*) and 1.19 (the standard deviation of *pressure on suppliers*).

constitutes a reduction of 2.3 violations, which is nearly one and a half times the sample average reduction of 1.6.[29]

The statistically significant positive coefficient on *pressure on buyers (prior audit)* ($\beta = 0.037$; $p < 0.01$) illustrates greater average improvement for suppliers to buyers that have already been publicly exposed for harms to workers in their supply chain, which supports H2. The coefficient magnitude indicates that a one-standard-deviation increase in *pressure on buyers (prior audit)* is associated with an increase in *improvement* from an average of 22 percent to 23.2 percent, based on average predictions across our sample.[30]

The statistically significant positive coefficient on the standardized *maximum auditor training (prior audit)* ($\beta = 0.031$; $p < 0.01$) indicates that greater improvement tends to follow audits conducted by better-trained audit teams, which supports H3.[31] The coefficient magnitude indicates that, on average, suppliers realize an additional 3.1-percentage-point improvement when their prior audit was conducted by a team whose best-trained auditor had one standard deviation more training than the average team's best-trained auditor (that is, 12.7 training courses versus the average of 6.9). Such suppliers average a 25.1-percent reduction (the sum of the 0.22 sample average and the 0.031 coefficient); a reduction of 1.8 violations from the prior to the focal audit, or 0.2 violations more than the average reduction of 1.6 violations.

[29] Support for H1 is robust to replacing *pressure on suppliers (prior audit)* with its two underlying elements, which yields statistically significant positive coefficients on both standardized *press freedom (prior audit)* and standardized *NGO density (prior audit)*. This provides further evidence that suppliers operating in institutional environments with greater press freedom and NGO pressure tend to improve more than suppliers in countries with less of those.
[30] 23.2% is calculated by adding to 0.22 the product of 0.031 (the coefficient on *pressure on suppliers*) and 0.40 (its standard deviation).
[31] Our finding that a better-trained audit team at the prior audit leads to more improvement would risk being driven by regression to the mean if our specification only measured audit team training associated with the prior audit. However, our models *also* control for the focal audit team's training, which mitigates that risk.

The statistically significant positive coefficient on *announced (prior audit)* ($\beta = 0.049$; $p < 0.01$) indicates that greater improvement follows announced audits than unannounced audits, which supports H4.[32] Predictive margins indicate that suppliers whose prior audit was announced experienced an average 23.2-percent improvement, compared to 18.2-percent for suppliers whose prior audit was unannounced. Applied to the average 7.23 violations in the prior audit, this is an average decline of 1.67 violations following announced audits versus 1.34 after unannounced audits. This average differential of 0.33 violations per audit corresponds to one more violation being mitigated after three announced audits than after three unannounced audits. These results are largely identified based on differences between factories because only a small fraction of the factories in our sample have prior audits that are a mix of announced and unannounced audits.

To test Hypothesis 5, we add a term that interacts *maximum auditor training (prior audit)* and *announced (prior audit)* and report the results as Model 2 in Table 4. The statistically significant positive coefficient on the interaction term ($\beta = 0.051$; $p < 0.01$) indicates that better-trained audit teams at prior audits tend to prompt more improvement when those prior audits were announced than when they were unannounced, which supports H5. Figure 1 plots the average predicted effects of *maximum auditor training (prior audit)* on *improvement* for observations in which prior audits were announced or unannounced. The upward-sloped dashed line indicates that for announced audits, better-trained auditors at the prior audit prompt more improvement. The relatively flat solid line indicates that for unannounced audits, suppliers' improvement rates are largely unaffected by how well-trained the prior audit team was.

[32] Note that if announcing the prior audit gave factories time to hide or solve problems, prior audits would yield fewer violations than they otherwise would, which would bias against our hypothesized result; the falsely depressed baseline violation count would make it more difficult to observe subsequent improvement.

Supplementary Analysis

We conducted several additional analyses to assess the robustness of our results. While we believe our dependent variable is a well-designed interpretable metric robust to outliers, we acknowledge its complexity. Therefore, we assessed whether our results were sensitive to this metric by estimating models that instead predict the number of violations cited in the focal audit, controlling for the number of violations cited in the prior audit and including all other independent and control variables from our primary specifications. These negative binomial regression[33] results, reported in Table A2 in the Appendix, confirm all inferences from our primary models and thereby indicate that our results are robust to this alternative specification.[34]

Because improvement might depend on the time between the prior and focal audits, we reestimated our models predicting *improvement rate*, an alternative dependent variable that explicitly accounted for the time lag since the prior audit. *Improvement rate* is calculated by dividing *improvement* (our primary dependent variable) by the log of the number days since the prior audit. These results, too, yield statistically significant support for our hypotheses except that auditor training remains a significant predictor of improvement only in the presence of announced audits.

We coded our variables irrespective of whether the focal and prior audit were conducted for the same brand. Estimating our primary models on the subsample of 6,458 observations in

[33] Because the dependent variable of this model, number of violations, exhibits overdispersion—the ratio of the variance to the mean is 4.4 (that is, 24.7 / 5.6)—we followed the conventional approach of using negative binomial regression rather than Poisson. Moreover, a likelihood-ratio test yields a chi-squared value of 6,931, which indicates that the probability that we would observe these data conditional on alpha equaling 0 (an assumption underlying the Poisson estimator) is virtually zero.

[34] We also estimated OLS models that predicted two other alternative dependent variables: (a) the difference in violations between the focal and prior audit and (b) the difference between the logs of these values. These models yielded results substantially similar to those of our primary models and of the alternative models reported in Table A2. Specifically, they yielded the same inferences for our institutional features and stronger evidence of auditor training, but only evidence of announced having an effect when it was interacted with auditor training.

which an establishment's focal and prior audits were conducted for the same brand yielded insights similar to those of our primary approach. The one difference is that the estimates on this subsample yield evidence of an auditor training effect only in the presence of an announced audit (H5), but not an overall effect (H3).

Because so many of the observations in our sample are from China, we examined whether our primary results held when estimating our models on just the 6,294 observations from the 3,378 factories in China. To do so, we omit our factory-country variables and the country-year-level variables (to avoid multicollinearity concerns, as they changed only slightly over time) and cluster standard errors by factory. All the hypothesized effects continue to yield statistically significant coefficients of the same sign and similar magnitude (though in some cases with larger standard errors, likely due in part to the smaller sample size).

We estimated models that accounted for the fact that some buyers always sought unannounced audits, some always sought announced audits, and some sought a mix. Specifically, we added two control variables to our primary models: one dummy variable indicating audits on behalf of buyers that always specified announced inspections and another indicating audits on behalf of buyers that always specified unannounced inspections.[35] The results are virtually identical to those of our primary models. Overall, our primary results proved markedly stable throughout various robustness tests.

Extensions

Our primary analysis measures improvement based on the difference between the total number of violations cited in the focal audit and in the prior audit, aggregating several types of violation to capture improvement comprehensively. Recognizing that different violation types

[35] Audits conducted on behalf of a buyer that specified a mix was the omitted category.

may improve at different rates under the hypothesized conditions, we disaggregated our dependent variable by violation type to better understand which categories are more likely to improve under which conditions. To explore how our hypothesized variables influence these categories, we estimated models that predicted improvement in each of the violation categories making up our *improvement* variable for which at least 10 percent of audits exhibited variation; these were child labor, working hours, minimum wage, and occupational safety and health (OSH).[36] We created an improvement metric for each category by applying the same formula used to create our primary improvement metric; the correlation between these four variables ranges from 0.20 to 0.34. We report in Table 5 the results of OLS regression models that predict each improvement metric based on the specifications used in our main models, except that our control for a supplier's prior violations corresponds to the specific violation category being predicted. The results broadly validate the mechanisms we theorize above and also indicate that our hypothesized variables are associated with varying degrees of improvement across different violation types, thus highlighting important nuances of our primary results.

First, we find that under conditions of greater institutional pressure on suppliers, there is significantly more improvement in all four subcategories (child labor, working hours, minimum wages, and OSH violations). This confirms the importance of institutional pressure to improvement on all of these dimensions.

Second, we find more improvement in working hours, minimum wage, and OSH violations among suppliers of buyers facing more institutional pressure. These findings support our intuition that reputation-sensitive buyers will be more proactive in seeking improvements to

[36] The scores for *disciplinary practices, forced or compulsory labor*, and *treatment of foreign workers and subcontractors* changed in fewer than 10% of audits. To avoid generalizing from such limited variation, we did not estimate models to predict those three categories.

supply chain working conditions. OSH violations have been a focus of activist organizing and media exposés, making them highly salient to brands with reputation concerns; these brands may apply more pressure to improve supplier conditions. Working hours and wages are dimensions that buyer sourcing practices can influence and buyers facing more institutional pressure might be especially attentive to this. For example, such buyers might be more prone to avoiding frequent change orders or delaying orders, thus reducing suppliers' need to work excessive hours. Such practices can also mitigate the risk of workplace injuries. Similarly, buyers concerned about their reputations might to be more likely to avoid bargaining to minimize suppliers' profit margins to the point that suppliers feel compelled to constrain wages in ways that violate codes of conduct. We were somewhat surprised that we did not find evidence of greater improvements in high-reputation-salience violations like child labor among suppliers of buyers facing more institutional pressure, but we note that the coefficient on this violation type remains positive and its lack of significance may be due to statistical power: factories in our sample were much less likely to exhibit variation over time in the number of child labor violations than in the other types of violations.[37]

Third, we find that visits by highly trained auditors lead to significantly more improvement in child labor, wages, and OSH scores. These findings are consistent with our hypothesis that better-trained auditors can better convey compliance information. Improvements in child labor scores may be attributable to suppliers' greater willingness to receive and follow advice from more highly trained auditors. Payroll and OSH practices can be complex and/or technical and a knowledgeable auditor can provide useful guidance about how to maintain effective record-keeping systems and remedy workplace hazards.

[37] 66% of observations in our sample have the same number of child labor violations in the focal and prior audits, compared to 17–33% for violations of working hours, minimum wage, and OSH.

Finally, we find that supplier improvement following announced audits is driven primarily by improvement in OSH violations—precisely the kind that may require transfer of knowledge between auditors and suppliers regarding buyer expectations and best practices. Notably, compliance with child labor restrictions improves significantly *less* following announced audits than unannounced audits. Because most factory managers are well aware that these are "zero-tolerance" violations that may be grounds for contract termination, an exchange of information is typically not required. We therefore do not expect formal signals of greater trust to foster improvements in this area. In fact, our extension suggests that announcing audits could exacerbate such violations if it is taken as a signal of leniency rather than of trust.

DISCUSSION

Our findings reveal structural conditions under which codes of conduct and monitoring regimes adopted by TNCs are associated with improvements in their suppliers' labor practices. First, suppliers are more likely to improve when local and global institutional pressures generated by civil society activism create greater risk that harms to workers will be discovered and publicized; the more institutional pressure from additional revelations, the more suppliers improve. Second, we find that suppliers improve more not only with external institutional pressures, but also when monitoring programs have features that facilitate knowledge transfer. Suppliers improved more following audits with advance notice, particularly in areas like OSH, where compliance assistance can be most helpful. We also find suppliers more likely to improve when their auditors are highly trained, but only if audits are pre-announced.

In addition to theorizing and identifying conditions under which organizational structures adopted in response to private political activism will be associated with changes in

organizational behavior, our study makes several further important contributions to the literatures on supply chain labor standards regulation, private politics, and decoupling.

First, our finding that highly trained auditors are associated with greater improvement is a corrective to the literature's pervasive auditor-skepticism (O'Rourke, 2002; Esbenshade, 2004; Power, Ng, and Singh, 2008; Locke, Amengual, and Mangla, 2009; AFL-CIO, 2013; LeBaron and Lister, 2015), suggesting the important role auditors can play given the proper tools. At the same time, our finding that even highly trained auditors add no significant compliance improvement through unannounced audits suggests the limitations of training and the need to consider program design holistically rather than piecemeal.

Second, our finding that pre-announced audits were followed by greater improvement in OSH practices but not child labor practices adds nuance to the debate surrounding whether audits should be announced or unannounced. The improvement in OSH practices is consistent with (a) the prediction that compliance can develop iteratively in response to cooperative gestures by those implementing the rules (e.g., Axelrod, 1984; Scholz, 1984; Ayres and Braithwaite, 1992) and (b) qualitative studies finding better compliance with labor codes of conduct by suppliers in trusting and cooperative relationships with buyers (Frenkel and Scott, 2002; Locke and Romis, 2007). But our finding that announced audits impeded improvement in compliance with child labor standards suggests that cooperative signals from buyers may lead some suppliers to believe they can get away with such violations, consistent with research skeptical of the rigor of pre-announced audits (O'Rourke, 2002; Esbenshade, 2004; Clean Clothes Campaign, 2005; Gray, 2006; Power, Ng, and Singh, 2008; AFL-CIO, 2013; LeBaron and Lister, 2015; Short, Toffel, and Hugill, 2016). These findings highlight the difficult tradeoffs in designing monitoring regimes and the imperative that design choices reflect program aims. Monitoring programs

should be structured differently depending on whether they seek to collect the most complete and accurate information, to catch suppliers committing particularly harmful violations, or to improve working conditions by creating conditions for cooperation with suppliers. Our finding that highly trained auditors were associated with accelerated improvement in child labor practices as well as in other violation categories suggests that design tradeoffs could be reconciled to some degree through better training of auditors. Further research should explore how different monitoring approaches can be deployed and combined to leverage their comparative advantages.

Third, the extension disaggregating our dependent variable contributes to the literature demonstrating that labor code compliance tends to improve more rapidly in some categories, like health and safety, than in others, like freedom of association (Barrientos and Smith, 2007; Ruwanpura, 2012) by identifying variation in the conditions under which different violation types improve. Existing research has focused on how unions influence improvement in different categories. Oka (2016) finds that unionized suppliers improve their compliance with wage, hours, and leave standards much more substantially than with OSH standards because unions in developing economies—where most suppliers grappling with code compliance are located—tend to prioritize pocketbook issues over OSH issues. We extend this work by identifying institutional and program design features associated with improvements in OSH violations as well as other categories. These findings suggest the importance of alternative pathways of influence to improve working conditions overall.

Fourth, our finding of greater improvement among suppliers to reputation-compromised buyers suggests that exposure by activists can have substantive impacts beyond the largely symbolic responses identified in the private politics literature. Prior research documents that

activism prompts firms to adopt symbolic structures like "impression management tactics" (McDonnell and King, 2013: 411), public "concessions" to conform to activists' demands (Eesley and Lenox, 2006; King, 2008), and CSR officer positions or board committees (McDonnell, King, and Soule, 2015). However, this research has not revealed whether activism is related to changes in organizational behavior that align it more closely with the activists' normative goals. Our finding that buyers tainted by negative media coverage become especially prone to working with suppliers that are more rapidly improving their working conditions suggests that these reputational risks may prompt firms to take substantive and not merely symbolic measures to avoid further reputation damage. Moreover, our finding that the *more* pressure buyers experience in their institutional environment, the *more* likely their suppliers are to improve is an important strategic insight for activists considering how to target their resources.

Finally, demonstrating the relationship between program design features and performance improvement adds an important dimension to understandings of decoupling. With few exceptions (e.g., Kalev, Dobbin, and Kelly, 2006), program design has been ignored as a coupling determinant and we are aware of no study that investigates how design features create contingencies for one another. We suspect that decoupling studies have devoted little time to the design features of organizational structures adopted in response to activism because this literature has long theorized that such structures are likely to be merely symbolic. Our findings challenge that theoretical premise by suggesting that the difference between symbol and substance may depend, in part, on how organizational structures are designed.

Limitations and Future Research

Our study has limitations but also invites promising future research. First, because all the suppliers in our sample were audited, we address why some audited suppliers improve more

rapidly than others, but not whether auditing is more effective than other interventions such as more stringent government regulation, legally binding international standards, or labor union activities. These are vital research questions.

Second, our findings are subject to several data limitations. We examine factories that faced at least two social audits by a single firm. By omitting those audited just once, we exclude audits that buyers might have initiated as a first step toward establishing a supplier relationship that was subsequently abandoned. Our focus on a single auditing firm has the advantage of providing comparable auditor training data, but does not enable us to compare practices across auditing firms. Omitting certain types of code violation from our analysis enhanced the reliability of our improvement measure, but leaves future research to determine whether the factors we found to predict improvement would also do so with the types of violation we omitted, particularly those concerning freedom of association and collective bargaining. Data limitations also prevented us from controlling for some of the factors that prior studies have found to be predictive of regulatory compliance, such firm size and regulatory enforcement practices. While we believe that our proxies for key independent variables are reasonable, we cannot rule out the possibility that they are imperfect. Finally, it is possible that alternative governance structures might influence improvement, including whether audits are conducted by brand staff or a third-party auditing firm and whether codes are sponsored by brands, multi-stakeholder regimes, or NGOs. We hope that our study paves the way for others to examine these and other factors.

Conclusion

As the anti-sweatshop movement makes the "TNC into the central locus of struggle over labor rights and globalization" (Bartley and Child, 2014: 657), it is crucial to understand whether formal organizational structures like codes of conduct and supplier monitoring can produce

meaningful social change. We identify conditions at the institutional and program design levels under which these formal organizational structures are associated with measurable improvements in working conditions. Our findings suggest key considerations that should inform social monitoring and activist targeting strategies aimed at raising labor standards in global supply chains.

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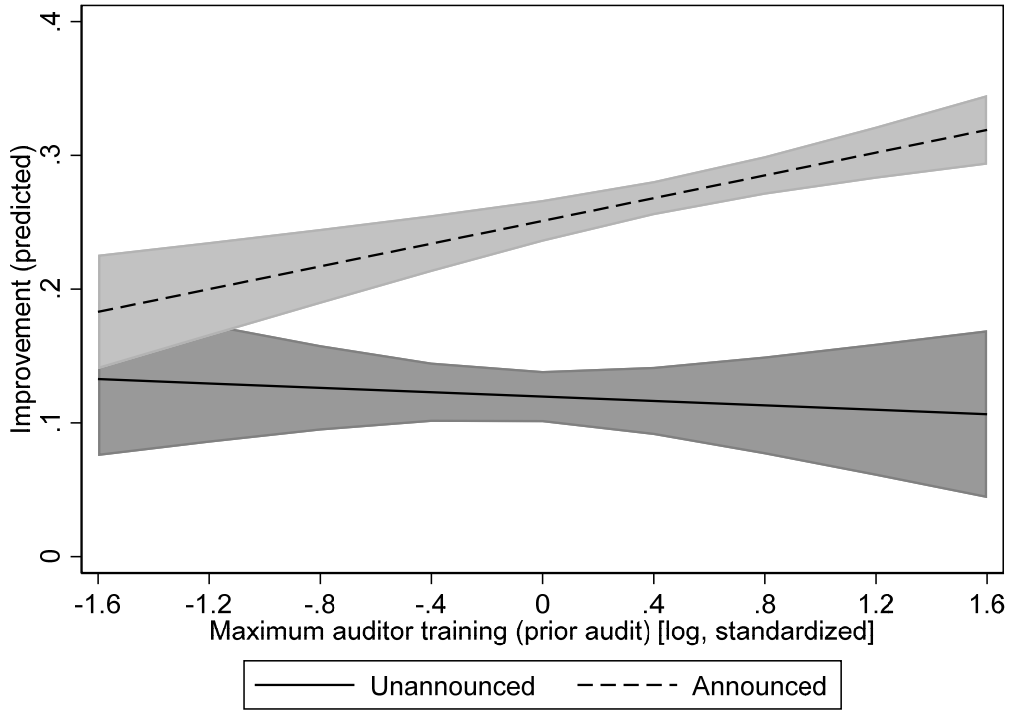
Figure 1. Average Predicted Improvement Values Based on Varying Amounts of Maximum Auditor Training at Prior Audits That Were Unannounced or Announced



Note: Average predicted values of *improvement* from Model 2 of Table 4 when the prior audit was announced (solid line) or unannounced (dashed line), based on varying levels of *maximum auditor training (prior audit)* (logged and standardized) and with all other variables at their actual values.

Table 1. Industry Composition of Audits and Audited Suppliers

	Audits		Suppliers	
Industry	Number	Percent	Number	Percent
Accessories	930	10.7%	506	10.2%
Building materials	143	1.6%	74	1.5%
Chemicals and plastics	47	0.5%	36	0.7%
Electronics	358	4.1%	171	3.5%
Food, agriculture, beverages	73	0.8%	56	1.1%
Footwear	191	2.2%	103	2.1%
Furniture	226	2.6%	103	2.1%
Garment	2,902	33.4%	1,659	33.6%
Metal products	85	1.0%	45	0.9%
Paper, printing, and publishing	117	1.3%	71	1.4%
Services	25	0.3%	15	0.3%
Toys	269	3.1%	139	2.8%
Unknown (other and missing)	3,311	38.2%	1,962	39.7%
Total	8,677	100.0%	4,940	100.0%

Table 2. Location of Audits and Audited Suppliers

	Audits		Suppliers	
Supplier country	Number	Percent	Number	Percent
Bangladesh	129	1.5%	104	2.1%
Brazil	40	0.5%	32	0.6%
Canada	33	0.4%	19	0.4%
China (includes Hong Kong)	6,345	73.1%	3,416	69.1%
Egypt	23	0.3%	13	0.3%
Guatemala	29	0.3%	24	0.5%
India	288	3.3%	207	4.2%
Indonesia	143	1.6%	101	2.0%
Italy	30	0.3%	28	0.6%
Jordan	36	0.4%	22	0.4%
Korea, Republic of (South)	53	0.6%	39	0.8%
Malaysia	40	0.5%	25	0.5%
Mexico	77	0.9%	66	1.3%
Pakistan	75	0.9%	53	1.1%
Peru	35	0.4%	23	0.5%
Philippines	121	1.4%	71	1.4%
Sri Lanka	55	0.6%	40	0.8%
Thailand	41	0.5%	30	0.6%
Turkey	84	1.0%	59	1.2%
United States	629	7.2%	302	6.1%
Vietnam	187	2.2%	121	2.4%
Countries with <20 audits in sample	184	2.1%	145	2.9%
Total	8,677	100.0%	4,940	100.0%

Table 3. Summary Statistics

	Mean	SD	Min	Max
Improvement	0.22	0.86	-3.26	3.26
Pressure on suppliers (prior audit)	-0.34	1.19	-1.14	4.02
Pressure on buyers (prior audit)	0.38	0.90	0	5.55
Maximum auditor training§ (prior audit)	0	1	-1.65	1.64
Announced (prior audit)	0.76	0.43	0	1
Maximum auditor training† (focal audit)	2.04	0.87	0	3.47
Announced (focal audit)	0.77	0.42	0	1
Violations (prior audit)	7.23	5.81	0	25
Violations† (prior audit)	1.84	0.77	0	3.26
Paid by supplier or third party (prior audit)	0.51	0.5	0	1
Paid by the buyer (prior audit)	0.43	0.49	0	1
Paid by unknown entity (prior audit)	0.06	0.24	0	1
Paid by supplier or third party (focal audit)	0.52	0.5	0	1
Paid by the buyer (focal audit)	0.42	0.49	0	1
Paid by unknown entity (focal audit)	0.05	0.22	0	1
Prior audit was re-audit, but focal audit was not	0.1	0.3	0	1
Focal audit was re-audit, but prior audit was not	0.38	0.49	0	1
Prior and focal audit were re-audits	0.19	0.4	0	1
Audit duration† (prior audit)	1.44	0.43	0.03	4.39
Previous auditor (prior audit)	0.2	0.4	0	1
Previous auditor (focal audit)	0.23	0.42	0	1
Audit sequence	1.84	1.31	1	11
All-female audit team (prior audit)	0.53	0.5	0	1
Mixed-gender audit team (prior audit)	0.16	0.37	0	1
All-female audit team (focal audit)	0.5	0.5	0	1
Mixed-gender audit team (focal audit)	0.18	0.38	0	1
Maximum auditor tenure† (prior audit)	1.86	0.28	0.69	2.77
Maximum auditor tenure† (focal audit)	1.78	0.32	0.69	2.77
FDI inflows† (prior audit)	1.54	0.38	-0.45	3.41
Labor laws† (2002)	3.12	0.12	2.3	3.35
GDP per capita† (prior audit)	7.78	1	5.61	10.68
GDP per capita in buyer country† (prior audit)	10.59	0.26	6.59	10.85
Buyer employment† (prior year)	10.32	1.22	1.61	14.35

Notes: † indicates logged. § indicates logged, then standardized. N=8,677 audits, except 7,774 for *pressure on buyers*, 4,338 for *audit duration (prior audit)*, 4,523 for *previous auditor (prior audit)*, 8,668 for *previous auditor (focal audit)*, and 5,355 for *buyer employment*.

Table 4. Regression Results

	(1)	(2)
Dependent variable:	Improvement	
Pressure on suppliers (prior audit)	0.089**	0.088**
	[0.015]	[0.015]
Pressure on buyers (prior audit)	0.037**	0.039**
	[0.010]	[0.010]
Maximum auditor training§ (prior audit)	0.031**	-0.008
	[0.012]	[0.018]
Announced (prior audit)	0.049**	0.049**
	[0.011]	[0.013]
Maximum auditor training§ (prior audit) × Announced (prior audit)		0.051**
		[0.011]
Maximum auditor training† (focal audit)	-0.053**	-0.054**
	[0.016]	[0.016]
Announced (focal audit)	0.048**	0.046*
	[0.017]	[0.018]
Violations† (prior audit)	0.676**	0.679**
	[0.025]	[0.026]
Paid by supplier or third party (prior audit)	-0.016	-0.020
	[0.021]	[0.020]
Paid by supplier or third party (focal audit)	0.136**	0.141**
	[0.017]	[0.016]
Paid by unknown entity (prior audit)	0.009	0.008
	[0.022]	[0.022]
Paid by unknown entity (focal audit)	-0.027	-0.025
	[0.065]	[0.063]
Prior audit was re-audit, but focal audit was not	-0.121**	-0.124**
	[0.035]	[0.033]
Focal audit was re-audit, but prior audit was not	0.277**	0.274**
	[0.016]	[0.016]
Prior and focal audit were re-audits	0.197**	0.194**
	[0.011]	[0.012]
Audit duration† (prior audit)	0.120**	0.126**
	[0.027]	[0.026]
Previous auditor (prior audit)	0.013	0.014
	[0.026]	[0.026]
Previous auditor (focal audit)	0.032+	0.033+
	[0.018]	[0.018]

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Table 4 (continued)

All-female audit team (prior audit)	0.009	0.009
	[0.013]	[0.013]
Mixed-gender audit team (prior audit)	-0.019	-0.019
	[0.025]	[0.025]
All-female audit team (focal audit)	-0.063**	-0.063**
	[0.018]	[0.018]
Mixed-gender audit team (focal audit)	-0.122**	-0.122**
	[0.027]	[0.027]
Maximum auditor tenure† (prior audit)	-0.195	-0.188
	[0.195]	[0.192]
Maximum auditor tenure† (prior audit), squared	0.051	0.048
	[0.051]	[0.050]
Maximum auditor tenure† (focal audit)	-0.574**	-0.584**
	[0.171]	[0.171]
Maximum auditor tenure† (focal audit), squared	0.154**	0.157**
	[0.043]	[0.043]
FDI inflows† (prior audit)	-0.064	-0.063
	[0.040]	[0.040]
Labor laws† (2002)	0.157	0.158
	[0.115]	[0.116]
GDP per capita† (prior audit)	0.051**	0.053**
	[0.016]	[0.015]
GDP per capita in buyer country† (prior audit)	-0.007	0.004
	[0.030]	[0.031]
Buyer employment† (prior year)	-0.043**	-0.045**
	[0.005]	[0.005]
Year fixed effects	Included	Included
Sequence fixed effects	Included	Included
Industry fixed effects	Included	Included
Buyer-country fixed effects	Included	Included
R-squared	0.41	0.42

Notes: Ordinary least squares (OLS) regression coefficients with standard errors clustered by supplier country in brackets. N = 8,677 observations (each based on two consecutive audits) from 4,940 factories. **p<0.01, *p<0.05, +p<0.10. † indicates logged. § indicates logged, then standardized. Baseline (omitted) categories are *paid by the buyer* for focal and prior audit when neither was a re-audit. All models include dummy variables to indicate instances in which the following variables were missing data and thus recoded to 0: *pressure on buyers (prior audit)* (903 audits), *audit duration (prior audit)* (4,339 audits), *previous auditor (prior audit)* (4,154 audits), *previous auditor (focal audit)* (9 audits), and *buyer employment* (3,322 audits).

Table 5. Extension Results

Dependent variable:	(1) Child labor score improvement	(2) Working hours score improvement	(3) Minimum wage score improvement	(4) OSH score improvement
Pressure on suppliers (prior audit)	0.044**	0.034**	0.060**	0.043*
	[0.006]	[0.012]	[0.009]	[0.017]
Pressure on buyers (prior audit)	0.007	0.021**	0.017**	0.033**
	[0.006]	[0.007]	[0.005]	[0.008]
Maximum auditor training§ (prior audit)	0.017*	0.010	0.034**	0.026*
	[0.008]	[0.009]	[0.006]	[0.012]
Announced (prior audit)	-0.013**	0.011	0.010+	0.041**
	[0.004]	[0.010]	[0.006]	[0.011]
Maximum auditor training† (focal audit)	-0.004	-0.045**	-0.015+	-0.044+
	[0.005]	[0.010]	[0.008]	[0.024]
Announced (focal audit)	0.036**	0.014	0.053**	0.043**
	[0.012]	[0.016]	[0.013]	[0.015]
Number of child labor violations (prior audit)	0.383**			
	[0.005]			
Number of hours of work violations (prior audit)		0.315**		
		[0.007]		
Number of minimum wage violations (prior audit)			0.249**	
			[0.010]	
Number of OSH violations (prior audit)				0.121**
				[0.008]
Paid by supplier or third party (prior audit)	-0.001	0.024**	-0.043+	-0.034*
	[0.006]	[0.007]	[0.022]	[0.015]
Paid by supplier or third party (focal audit)	0.028**	0.048**	0.081**	0.034+
	[0.007]	[0.015]	[0.023]	[0.018]
Paid by unknown entity (prior audit)	0.001	0.011	0.004	0.049*
	[0.006]	[0.027]	[0.016]	[0.024]
Paid by unknown entity (focal audit)	0.006	-0.023	-0.018	-0.021
	[0.010]	[0.037]	[0.039]	[0.052]
Prior audit was re-audit, but focal audit was not	-0.010	-0.050**	-0.055**	-0.191**
	[0.011]	[0.013]	[0.010]	[0.054]
Focal audit was re-audit, but prior audit was not	0.029**	0.028**	0.072**	0.304**
	[0.008]	[0.009]	[0.013]	[0.024]
Prior and focal audit were re-audits	0.037**	0.006	0.041**	0.243**
	[0.005]	[0.009]	[0.013]	[0.010]
Audit duration† (prior audit)	0.049**	0.088**	0.084**	0.160**
	[0.016]	[0.017]	[0.015]	[0.040]
Previous auditor (prior audit)	0.007	-0.000	-0.012	0.020
	[0.005]	[0.017]	[0.017]	[0.026]
Previous auditor (focal audit)	-0.011	-0.014	0.017	0.037*
	[0.013]	[0.013]	[0.022]	[0.017]
All-female audit team (prior audit)	0.021**	0.002	-0.009	0.007
	[0.007]	[0.010]	[0.013]	[0.012]
Mixed-gender audit team (prior audit)	0.018*	-0.011	-0.025	0.007
	[0.008]	[0.012]	[0.020]	[0.035]
All-female audit team (focal audit)	-0.004	-0.035*	-0.065**	-0.044*
	[0.009]	[0.015]	[0.011]	[0.020]
Mixed-gender audit team (focal audit)	0.011	-0.051*	-0.067**	-0.127**
	[0.014]	[0.020]	[0.018]	[0.030]

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Maximum auditor tenure† (prior audit)	0.048	0.120	0.057	0.272
	[0.064]	[0.095]	[0.096]	[0.336]
Maximum auditor tenure† (prior audit), squared	-0.006	-0.037	-0.012	-0.071
	[0.017]	[0.028]	[0.029]	[0.092]
Maximum auditor tenure† (focal audit)	-0.175**	-0.269*	-0.145	-0.412*
	[0.050]	[0.114]	[0.101]	[0.172]
Maximum auditor tenure† (focal audit), squared	0.043**	0.062*	0.037	0.125**
	[0.014]	[0.030]	[0.031]	[0.045]
FDI inflows† (prior audit)	-0.026*	-0.067*	-0.030	-0.022
	[0.013]	[0.027]	[0.022]	[0.042]
Labor laws† (2002)	0.010	0.183	0.020	-0.006
	[0.046]	[0.122]	[0.071]	[0.102]
GDP per capita† (prior audit)	0.000	0.044**	0.035**	0.029
	[0.006]	[0.014]	[0.010]	[0.017]
GDP per capita in buyer country† (prior audit)	0.014	-0.042	0.031	0.022
	[0.009]	[0.026]	[0.019]	[0.020]
Buyer employment† (prior year)	-0.009**	-0.023**	-0.004	-0.025**
	[0.003]	[0.005]	[0.004]	[0.007]
Year fixed effects	Included	Included	Included	Included
Sequence fixed effects	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included
Buyer-country fixed effects	Included	Included	Included	Included
R-squared	0.44	0.36	0.32	0.31
Mean y in sample	0.072	0.055	0.081	0.246

Notes: OLS regression coefficients with standard errors clustered by supplier country in brackets. N =8,335 observations (each based on two consecutive audits) from 4,870 factories, slightly smaller than in Table 4 due to a few missing values of violation category counts. **p<0.01, *p<0.05, +p<0.10. † indicates logged. § indicates logged, then standardized. See Table 4 for additional notes.

APPENDIX

Interpreting the Extension Results

This section interprets the magnitude of the extension model results reported in Table 5.

Pressure on suppliers. Average marginal effects indicate that a one-standard-deviation increase in *pressure on suppliers (prior audit)* is associated with a 63% increase in child labor score improvement (calculated by dividing the coefficient, 0.044, by the dependent variable's mean, 0.07), a 68% increase in working hours improvement (0.034 ÷ 0.05), a 74% increase in minimum wage score improvement (0.060 ÷ 0.08), and a 17% increase in OSH score improvement (0.043 ÷ 0.25).

Pressure on buyers. To interpret the magnitude of results for pressure on buyers, we compared the average predicted improvement of audits of suppliers facing *pressure on buyers* half a standard deviation below its mean to that of those facing *pressure on buyers* half a standard deviation above its mean. These predicted values indicate that one standard deviation more *pressure on buyers* was associated with suppliers exhibiting 9% more child labor score improvement (from 6.9% to 7.5%), 39% more working hours score improvement (from 4.6% to 6.4%), 20% more minimum wage score improvement (from 7.4% to 8.9%), and 13% more OSH score improvement (from 23.1% to 26.0%).

Auditor training. Average marginal effects indicate that a one-unit change in *maximum auditor training (prior audit)*—that is, a one-standard-deviation change in this standardized variable—is associated with an average 24% improvement in the child labor score (calculated by dividing the coefficient by the sample-average dependent variable, or 0.017 ÷ 0.07), 43% improvement in the minimum wage score (0.034 ÷ 0.08), and 10% improvement in the OSH

score (0.026 ÷ 0.25). We found no significant association with improvement in the working hours score.

Signaling a cooperative approach. Predictive margins indicate that suppliers' OSH scores improved 25.6% on average following announced audits, 19% more than the 21.6% average improvement following unannounced audits. Minimum wage scores improved 8.4% following announced audits, 15% more than the average 7.3% improvement following unannounced audits, though this effect was only marginally statistically significant. Child labor scores improved 6.9% following announced audits, 17% *less* than the average 8.2% improvement following unannounced audits. We found no association with improvement in working hours scores.

Table A1. Correlations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
(1)Improvement	1.00																	
(2)Pressure on suppliers (prior audit)	-0.03	1.00																
(3)Pressure on buyers (prior audit)	0.04	0.09	1.00															
(4)Maximum auditor training§ (prior audit)	0.06	-0.17	-0.07	1.00														
(5)Announced (prior audit)	0.07	-0.13	-0.04	0.02	1.00													
(6)Maximum auditor training† (focal audit)	-0.02	-0.18	-0.06	0.69	0.06	1.00												
(7)Announced (focal audit)	0.05	-0.17	-0.03	0.02	0.49	0.06	1.00											
(8)Violations (prior audit)	0.44	-0.28	0.03	0.03	0.05	0.03	0.02	1.00										
(9)Violations† (prior audit)	0.57	-0.29	0.03	0.04	0.09	0.04	0.06	0.91	1.00									
(10)Paid by supplier or third party (prior audit)	0.00	-0.28	-0.25	-0.03	0.16	-0.04	0.19	0.04	0.05	1.00								
(11)Paid by the buyer (prior audit)	-0.02	0.26	0.21	0.03	-0.18	0.02	-0.20	-0.07	-0.07	-0.88	1.00							
(12)Paid by unknown entity (prior audit)	0.04	0.05	0.09	0.02	0.04	0.04	0.01	0.06	0.05	-0.26	-0.22	1.00						
(13)Paid by supplier or third party (focal audit)	0.04	-0.26	-0.28	-0.03	0.13	-0.04	0.23	0.05	0.06	0.75	-0.70	-0.10	1.00					
(14)Paid by the buyer (focal audit)	-0.05	0.26	0.25	0.02	-0.14	0.03	-0.24	-0.08	-0.09	-0.70	0.77	-0.12	-0.90	1.00				
(15)Paid by unknown entity (focal audit)	0.01	0.02	0.08	0.02	0.01	0.03	0.01	0.06	0.06	-0.12	-0.12	0.50	-0.25	-0.20	1.00			
(16)Prior audit was re-audit, but focal audit was not	-0.21	-0.03	-0.05	-0.08	-0.02	0.03	0.06	-0.17	-0.18	0.03	-0.02	-0.03	0.01	-0.01	-0.01	1.00		
(17)Focal audit was re-audit, but prior audit was not	0.27	-0.08	0.07	0.05	0.07	-0.06	-0.02	0.32	0.31	0.05	-0.07	0.04	0.04	-0.05	0.03	-0.26	1.00	
(18)Prior and focal audit were re-audits	0.02	-0.15	-0.02	0.04	-0.02	-0.04	0.00	-0.05	-0.02	0.12	-0.09	-0.06	0.11	-0.08	-0.06	-0.17	-0.38	1.00
(19)Audit duration† (prior audit)	0.22	-0.39	0.11	0.24	0.19	0.26	0.22	0.31	0.37	0.04	-0.05	0.02	0.05	-0.06	0.02	-0.03	0.12	0.00
(20)Previous auditor (prior audit)	0.01	0.29	0.03	-0.03	-0.08	-0.06	-0.09	-0.07	-0.07	-0.11	0.10	0.02	-0.11	0.11	0.00	-0.02	-0.03	-0.02
(21)Previous auditor (focal audit)	0.03	0.34	0.03	-0.04	-0.04	-0.09	-0.07	-0.07	-0.08	-0.12	0.10	0.04	-0.10	0.10	0.02	-0.04	0.03	-0.04
(22)Audit sequence	-0.08	0.01	-0.11	0.30	-0.07	0.22	-0.04	-0.27	-0.24	0.03	-0.01	-0.04	0.01	0.00	-0.03	0.13	-0.24	0.15
(23)All-female audit team (prior audit)	0.00	-0.13	0.00	-0.02	0.03	0.01	0.03	0.02	0.02	0.03	-0.03	-0.01	0.03	-0.03	0.00	0.02	-0.01	0.02
(24)Mixed-gender audit team (prior audit)	0.02	-0.01	-0.03	0.09	0.00	0.05	0.00	0.03	0.03	0.01	-0.01	0.01	0.00	-0.01	0.01	-0.01	0.00	0.00
(25)All-female audit team (focal audit)	-0.01	-0.16	-0.03	0.00	0.00	0.00	0.02	0.03	0.02	0.05	-0.05	-0.01	0.05	-0.05	-0.01	0.01	0.01	0.03
(26)Mixed-gender audit team (focal audit)	-0.04	0.01	-0.02	0.01	0.01	0.05	0.02	-0.01	0.00	0.01	-0.01	0.00	0.01	-0.01	0.00	0.03	-0.02	-0.01
(27)Maximum auditor tenure† (prior audit)	-0.01	0.20	0.04	-0.34	-0.07	-0.32	-0.07	-0.03	-0.02	0.00	-0.01	0.03	0.00	0.00	0.02	0.02	-0.04	-0.02
(28)Maximum auditor tenure† (focal audit)	-0.01	0.19	0.02	-0.32	-0.06	-0.27	-0.08	-0.03	-0.03	0.02	-0.02	0.00	0.02	-0.03	0.03	-0.02	0.01	0.02
(29)FDI inflows† (prior audit)	0.03	-0.44	-0.06	0.27	0.11	0.27	0.15	0.16	0.16	0.18	-0.17	-0.01	0.18	-0.18	-0.01	0.01	0.05	0.08
(30)Labor laws† (2002)	0.02	-0.43	-0.02	0.10	0.03	0.13	0.06	0.10	0.09	0.09	-0.09	0.00	0.10	-0.11	0.02	0.00	-0.01	0.05
(31)GDP per capita† (prior audit)	0.00	0.58	0.08	0.05	-0.16	0.03	-0.17	-0.19	-0.20	-0.21	0.19	0.04	-0.21	0.19	0.04	-0.03	-0.09	-0.10
(32)GDP per capita in buyer country† (prior audit)	0.01	0.03	0.02	0.11	0.12	0.09	0.08	-0.10	-0.06	0.00	0.13	-0.25	-0.05	0.10	-0.11	-0.01	0.00	0.01
(33)Buyer employment† (prior year)	0.03	-0.25	-0.06	0.04	0.25	0.06	0.33	0.07	0.09	0.63	-0.65	0.03	0.71	-0.72	0.02	0.03	0.02	0.07

	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)	(33)
(19)Audit duration† (prior audit)	1.00														
(20)Previous auditor (prior audit)	-0.07	1.00													
(21)Previous auditor (focal audit)	-0.07	0.23	1.00												
(22)Audit sequence	-0.07	0.03	0.00	1.00											
(23)All-female audit team (prior audit)	-0.06	-0.11	-0.12	0.00	1.00										
(24)Mixed-gender audit team (prior audit)	0.23	0.04	0.07	0.07	-0.47	1.00									
(25)All-female audit team (focal audit)	-0.03	-0.10	-0.10	0.01	0.14	-0.04	1.00								
(26)Mixed-gender audit team (focal audit)	0.07	0.00	0.03	0.05	-0.02	0.16	-0.47	1.00							
(27)Maximum auditor tenure† (prior audit)	-0.18	0.06	0.04	-0.10	0.02	0.04	0.01	0.00	1.00						
(28)Maximum auditor tenure† (focal audit)	-0.21	0.08	0.09	-0.08	0.02	-0.03	0.04	0.02	0.36	1.00					
(29)FDI inflows† (prior audit)	0.28	-0.18	-0.22	0.08	0.05	0.02	0.09	0.00	-0.19	-0.18	1.00				
(30)Labor laws† (2002)	0.08	-0.23	-0.25	0.05	0.20	0.00	0.21	-0.01	-0.03	-0.04	0.25	1.00			
(31)GDP per capita† (prior audit)	-0.34	0.12	0.13	0.15	0.06	-0.04	0.05	-0.02	0.22	0.23	-0.21	0.10	1.00		
(32)GDP per capita in buyer country† (prior audit)	0.04	0.00	-0.01	0.08	0.01	0.01	0.01	0.00	-0.03	-0.01	-0.01	0.02	0.09	1.00	
(33)Buyer employment† (prior year)	0.14	-0.14	-0.12	0.01	0.03	0.00	0.05	0.00	-0.02	-0.01	0.21	0.13	-0.14	0.03	1.00

Notes: † indicates logged. § indicates logged, then standardized. Piecewise correlations; see Table 3 footer for number of observations per variable.

Table A2. Negative Binomial Regression Results

	(1)		(2)	
Dependent variable:	Number of violations			
	Coef.	AME	Coef.	AME
Pressure on suppliers (prior audit)	-0.131**	-0.73	-0.130**	-0.73
	[0.025]		[0.025]	
Pressure on buyers (prior audit)	-0.049**	-0.28	-0.051**	-0.29
	[0.010]		[0.010]	
Maximum auditor training§ (prior audit)	-0.030*	-0.17	0.008	-0.16
	[0.013]		[0.021]	
Announced (prior audit)	-0.038**	-0.21	-0.037*	-0.20
	[0.013]		[0.015]	
Maximum auditor training§ (prior audit) × Announced (prior audit)			-0.049**	
			[0.014]	
Maximum auditor training† (focal audit)	0.047	0.26	0.048	0.27
	[0.030]		[0.030]	
Announced (focal audit)	-0.102**	-0.57	-0.099**	-0.56
	[0.017]		[0.017]	
Violations† (prior audit)	0.332**	1.86	0.329**	1.85
	[0.020]		[0.020]	
Paid by supplier or third party (prior audit)	0.021	0.12	0.025	0.14
	[0.025]		[0.024]	
Paid by supplier or third party (focal audit)	-0.119**	-0.67	-0.124**	-0.69
	[0.019]		[0.019]	
Paid by unknown entity (prior audit)	-0.002	-0.01	-0.002	-0.01
	[0.031]		[0.031]	
Paid by unknown entity (focal audit)	0.027	0.15	0.025	0.14
	[0.068]		[0.067]	
Prior audit was re-audit, but focal audit was not	0.135**	0.76	0.137**	0.77
	[0.040]		[0.038]	
Focal audit was re-audit, but prior audit was not	-0.253**	-1.42	-0.251**	-1.41
	[0.014]		[0.014]	
Prior and focal audit were re-audits	-0.189**	-1.06	-0.186**	-1.04
	[0.014]		[0.015]	
Audit duration† (prior audit)	-0.164**	-0.92	-0.169**	-0.95
	[0.037]		[0.036]	
Previous auditor (prior audit)	-0.018	-0.10	-0.019	-0.10
	[0.033]		[0.033]	
Previous auditor (focal audit)	-0.031	-0.18	-0.032	-0.18
	[0.021]		[0.021]	

(continued on next page)

Table A2 (continued)

All-female audit team (prior audit)	-0.009	-0.05	-0.009	-0.05
	[0.018]		[0.018]	
Mixed-gender audit team (prior audit)	0.015	0.09	0.015	0.08
	[0.036]		[0.036]	
All-female audit team (focal audit)	0.076**	0.43	0.076**	0.43
	[0.026]		[0.026]	
Mixed-gender audit team (focal audit)	0.155**	0.87	0.156**	0.87
	[0.041]		[0.041]	
Maximum auditor tenure† (prior audit)	-0.157	-0.15	-0.164	-0.13
	[0.221]		[0.218]	
Maximum auditor tenure† (prior audit), squared	0.035		0.038	
	[0.060]		[0.059]	
Maximum auditor tenure† (focal audit)	0.885**	0.27	0.895**	0.27
	[0.180]		[0.180]	
Maximum auditor tenure† (focal audit), squared	-0.236**		-0.239**	
	[0.046]		[0.046]	
FDI inflows† (prior audit)	0.083	0.47	0.082	0.46
	[0.067]		[0.066]	
Labor laws† (2002)	-0.248	-1.39	-0.248	-1.39
	[0.201]		[0.203]	
GDP per capita† (prior audit)	-0.070**	-0.39	-0.072**	-0.40
	[0.026]		[0.026]	
GDP per capita in buyer country† (prior audit)	-0.027	-0.15	-0.038+	-0.21
	[0.020]		[0.021]	
Buyer employment† (prior year)	0.032**	0.18	0.034**	0.19
	[0.006]		[0.006]	
Year fixed effects	Included	Included	Included	Included
Sequence fixed effects	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included
Buyer-country fixed effects	Included	Included	Included	Included

Notes: Negative binomial regression coefficients with standard errors clustered by supplier country in brackets. Average number of violations is 5.64. N = 8,677 observations (each based on two consecutive audits) at 4,940 factories. **p<0.01, *p<0.05, +p<0.10. † indicates logged. § indicates logged, then standardized. Baseline (omitted) categories are *paid by the buyer* for focal and prior audit when neither was a re-audit. All models include dummy variables to indicate instances in which the following variables were missing data and thus recoded to 0: *pressure on buyers (prior audit)*, *audit duration (prior audit)*, *previous auditor (prior audit)*, *previous auditor (focal audit)*, and *buyer employment*. Because the dependent variable in these models is the number of violations, negative coefficients indicate greater improvement (that is, fewer violations), whereas in our primary models, positive coefficients refer to greater improvement.