Multinational enforcement of labor law: Experimental evidence on strengthening occupational safety and health (OSH) committees

Laura Boudreau*

May 18, 2023

Abstract

Annually, work-related mortality is responsible for 5-7% of all global deaths, and at least 1-in-9 workers experience non-fatal occupational accidents (ILO, 2019*a*,*b*). Occupational Safety and Health (OSH) committees are considered the key worker voice institution through which to improve workplace safety and health (ILO, 1981). I present evidence of OSH committees' causal effects on workers and on factories. To do so, I collaborated with 29 multinational apparel buyers that committed to enforce a local mandate for OSH committees on their suppliers in Bangladesh. With the buyers, I implemented a nearly year-long field experiment with 84 supplier factories, randomly enforcing the mandate on half. The buyers' intervention increased compliance with the OSH committee law. Exploiting the experimental variation in OSH committees' strength, I find that stronger OSH committees had small, positive effects on objective measures of safety. These improvements did not come at a cost to workers in terms of wages or employment or to factories in terms of labor productivity. The effects on compliance, safety, and voice were largest for factories with better managerial practices. Factories with worse practices did not improve, and workers in these factories reported lower job satisfaction; this finding suggests complementarity between external enforcement and internal capacity in determining the efficacy of regulation. *JEL Codes:* F61, J53, J81, L14, O12, O14

Keywords: Worker voice, Safety, OSH Committees, Labor Regulation, Multinational Corporations, Corporate Social Responsibility, Working Conditions, Bangladesh

---

*Columbia University & CEPR (l.boudreau@columbia.edu). I thank Noam Yuchtman, Reed Walker, Ernesto Dal Bó, Paul Gertler, and Ted Miguel for their guidance and encouragement. This research has benefited from comments and suggestions from Matt Amengual, Marianne Bertrand, Nick Bloom, Wouter Dessein, Greg Distelhorst, Fred Finan, John de Figueiredo, Jonas Hjort, Amit Khandelwhal, David Levine, Rocco Macchiavello, Jeremy Magruder, Felix Oberholzer-Gee, Raul Sanchez de la Sierra, Daniela Scur, Abu Shonchoy, Mike Toffel, and numerous seminar and conference participants. This project is funded by the Alliance for Bangladesh Worker Safety, the Private Enterprise Development in Low-income Countries (PEDL) Initiative, the Weiss Family Program Fund for research in Development Economics, the International Growth Centre (IGC), and the Abdul Latif Jameel Poverty Action Lab (J-PAL) Governance Initiative. The Sylff Research Abroad Program also contributed travel funding. None of the sponsors of this research had a right to review the paper before circulation nor to veto its circulation. IRB approval was obtained from the University of California, Berkeley (9/6/2016, 2016-05-8768)

1 Introduction

Annually, work-related mortality is responsible for 5-7% of all global deaths, and at least 1-in-9 workers experience non-fatal occupational accidents; the large majority of this burden is borne by workers in developing countries (ILO, 2019*a*,*b*). In Bangladesh, the Rana Plaza garment factory collapse in 2013 killed 1,134 workers and injured thousands of others (Centre for Policy Dialogue, 2015). Although the collapse is exceptional in its scale, fatalities and injuries in Bangladesh's apparel sector are not; in the year prior to the collapse, labor organizations identified at least 128 fatalities and 1,866 injuries.[1] In response, the ILO, governments, and an expanding set of social partners are working to improve occupational safety and health (OSH) in developing countries. In many countries, however, weak state capacity and corruption make OSH regulations relatively toothless (e.g., Fisman and Wang, 2015). On the private sector side, OSH compliance is also a function of firms' capacity to respond to regulatory incentives, and a growing body of literature suggests that many firms lack the capacity for compliance (Giorcelli, 2019, Bertrand and Crépon, 2020, Distelhorst, Hainmueller and Locke, 2017, Almunia et al., 2021).

This, in part, has led many multinational corporations (MNCs) sourcing from developing countries to attempt to privately enforce local labor laws and to engage their suppliers in capacity building for compliance. It has also led to an increased emphasis on improving labor rights and on increasing workers' voice on safety inside the firm (Hirschman, 1970). Joint worker-manager OSH committees are considered the key worker voice institution through which to achieve this aim (ILO, 1981); their objective is "to provide a platform for good communication, coordination, and cooperation between workers and their representatives and their employers, to ensure that working processes are safe, and workers enjoy optimal physical and mental health" (ILO, 2015). Even MNCs that might be thought to oppose increasing labor costs appear to be turning to OSH committees and to labor unions to help overcome imperfect monitoring in their supply chains (e.g., ACT, 2017). In Bangladesh, in the aftermath of the Rana Plaza collapse, the government mandated OSH committees, and many of the world's largest apparel buyers stated that they would enforce the new law on their suppliers (Alliance, 2013, Accord, 2013). The MNCs' stated goals were to empower workers to take an active role in their safety and to be able to report unsafe conditions without risk of retaliation (Alliance, 2013).

In this paper, I exploit experimental variation arising from MNCs' commitment to enforce Bangladesh's legal mandate for OSH committees to provide evidence of OSH

[1] Author's calculations based on data from the Bangladesh Institute of Labor Studies (BILS) and the AFL-CIO Solidarity Center.

committees' causal effects on workers and on factories in Bangladesh's apparel sector. To generate this variation, I collaborated with a group of 29 multinational apparel buyers, the Alliance for Bangladesh Worker Safety (hereafter, the Alliance), to randomize the timing of the roll-out of their OSH committee enforcement program.[2] The Alliance's intervention entailed a small amount of training for OSH committee members and six months of intensive monitoring of factories' adherence to the law through mandatory reporting and monitoring via email, phone calls, and onsite visits. Over 2017-18, I implemented the experiment with 84 supplier factories that collectively employed nearly 92,000 workers.[3] The research team made three full-day visits to factories: A pre-intervention baseline, a second visit about five months later, and a third visit 9-10 months after baseline. For treatment factories, the second visit occurred after they were exposed to treatment for about 3-4 months and so are subject to possible short-run effects. The third visit occurred 4-5 months later, so I consider possible effects at this visit to be longer-run effects. I also collected factories' business-related data, and the Alliance provided its administrative records. I analyze the data according to a pre-analysis plan (PAP), which is registered on the AEA's Social Science Registry.

I begin by estimating the effects in the short-run. First, I test the effect of the Alliance's intervention on suppliers' compliance with the OSH committee mandate. I find that it significantly increases factories' compliance, which I measure using an index of compliance outcomes. The intervention improves compliance by 0.20 standard deviations (sds) on average. The improvement is driven by OSH committees becoming more active; for example, they meet more frequently and conduct more risk assessments. Workers' assessment of OSH committees' performance also improves.

Next, I examine the consequences of strengthened OSH committees for workers' safety and health. While workplace fatality rates are high relative to industrialized countries, fatalities are still relatively rare, and OSH committees likewise focus on reducing occupational injuries and diseases.[4] These, along with physical safety indicators, including indicators linked to fatality risks, are my primary focus. The concern with testing for effects on injuries and occupational diseases, however, is that theories of voice suggest that strengthening OSH committees may increase workers' willingness to report OSH concerns, injuries, and diseases (Hirschman, 1970, Freeman and Medoff, 1985). On the

[2] The Alliance's membership included the 2 largest global retailers and 8 others in the top 100 (e.g., Walmart, Costco, Target); together, these 10 firms accounted for 23% of the top 100 global retailers' revenue (Deloitte, 2015).

[3] Factories were not aware of their experimental status. Due to logistical constraints, the Alliance rolls out all of its programs in stages, so this design aligns with the Alliance's standard procedures.

[4] My setting is not suitable for examining OSH committees' effects on the most serious threats to workers' welfare, fatalities and severe injuries, which remain important questions for future research.

empirical side, Morantz and Mas (2008) find that workers report fewer injuries when the likelihood of being punished for doing so increases.[5] Further, one of OSH committees' responsibilities is to manage accidents, including investigation of accidents, so strengthening OSH committees may directly increase official accident recording.

Consequently, I test for effects on OSH using three measures that range from least to most subject to reporting effects. The first is an index of observed physical safety and awareness indicators. As the index includes objective measures of OSH, the prediction is that the effect on this index should be unambiguously positive. The second is visitor logs for factories' onsite medical clinics. These clinics are typically the first provider of care for sick or injured workers, and their visitor logs are not used for mandatory injury reporting. While the logs are subject to reporting effects, I expect them to be less exposed compared to factories' injury registers. Finally, I use factories' injury registers, which are maintained for reporting injuries to the government. I argue these registers are more likely to reflect factories' norms around reporting and adherence to reporting requirements, and so changes in voice will affect them.

Beginning with the pre-specified index of observed physical safety and awareness indicators, I find that stronger OSH committees increase factories' performance on this index by 0.15 sds, which is primarily driven by an improvement in their performance on an independently-evaluated OSH checklist that was developed based on global brands' standards. Turning to the medical clinic visitor logs, which are available for 62 factories, I find that on net, stronger OSH committees reduce the proportion of the workforce seeking medical care by 15-16%. In contrast, on net, injuries recorded in factories' injury registers slightly increase, driven by reporting of minor injuries. In total, this evidence suggests that OSH committees improve workers' safety and health and may contribute to increased reporting of injuries.

I then turn to workers' job satisfaction and well-being. In the short-term, I find a small, negative effect on indicators of workers' job satisfaction, which I measure using an index of survey questions, absenteeism, and turnover. The decline is driven by a reduction in some self-reported measures of job satisfaction, while absenteeism and turnover are unaffected. I examine possible channels underlying this result when exploring mechanisms.

Next, I examine wages and employment. I find no evidence of adverse effects on workers' wages or on employment. I also find no effect on an index of non-pecuniary

[5] Reporting also complicates measurement in other economic domains. For example, as Strauss and Thomas (1998) discuss in their review article on health and economic development, health perceptions have been shown to be systematically related to socioeconomic status, possibly because higher-income people have better access to information about their health. Randomized interventions that increase access to healthcare have been found to improve objective measures of health but to worsen self-reported health.

amenities. These results suggest that factories do not offset OSH improvements due to the committees by cutting wages or other amenities for workers, nor do they respond by labor-shedding. Part of the explanation for these null results may be that employers possess labor market power in developing countries, so improvements in labor standards do not necessarily cost workers in terms of wages and employment (Amodio and de Roux, 2021, Brooks et al., 2021). Another contributing factor may be that stronger OSH committees do not appear to hamper labor productivity and may actually improve it; the estimated treatment effects on labor productivity are positive (largely not statistically significant), and with 95% confidence, are *not* more negative than -2.4 to -3.4%. From firms' perspectives, these results also suggest that while OSH committees may not have large positive effects on firms, they also do not have not have large adverse effects on them.[6]

Turning to mechanisms, I test for evidence of channels through which the committees may improve OSH outcomes. The theoretical literature on formal worker voice institutions suggests several, including improving information flows between workers and managers (Hirschman, 1970, Freeman and Lazear, 1995); raising workers' bargaining power over OSH (Viscusi, 1980); and increasing cooperation and coordination between workers and managers (Malcomson, 1983, Freeman and Medoff, 1985, Freeman and Lazear, 1995). I find evidence in favor of increased information flows, including longer and more action-oriented meeting minutes and sizable but imprecise increases in safety-related reporting by workers. I find little evidence in favor of other channels.

Finally, I conduct a pre-specified heterogeneity analysis by the strength of factories' preexisting management practices. This analysis is motivated by the assertion that countries' equilibrium levels of compliance with labor standards depend on the state's willingness and capacity to enforce and on the private sector's capacity to comply. The organizational economics literature identifies firms' capabilities as relying on managerial practices that in turn rely on relational contracts (Gibbons and Henderson, 2011, Blader, Gartenberg and Prat, 2020). Firms in developing countries are lower productivity (Hsieh and Klenow, 2009), which suggests they have worse relational contracting capabilities (Powell, 2019) and is consistent with their adopting fewer management practices (Bloom et al., 2014). I hypothesize that lack of capacity to build relational contracts with workers may contribute to noncompliance with labor regulation Boudreau, Cajal-Grossi and Macchiavello (2023).

I find that the intervention's effects on compliance and on safety indicators are much

[6] The lack of clear benefits to firms may also help to explain why factories had not voluntarily adopted OSH committees, especially given that there are fixed costs of establishing committees and that owners perceived a threat that in the longer-term, increases in workers' bargaining power for OSH may spill over to wages and to other aspects of job quality.

larger for factories with stronger management practices. Worker voice also improves in these factories, but it does not in poorly managed ones. Further, the treatment effects on job satisfaction for workers in better-managed factories are more consistent with the predictions from the theoretical literature on worker voice. While better-managed factories differ in other ways from poorly-managed ones, this evidence suggests a potential direction for future research to explore complementarity between external enforcement and firms' internal managerial capacity in determining the efficacy of regulation.

In the longer-run, I find that the effect of the Alliance's intervention on suppliers' compliance with the OSH committee mandate persists. Treatment factories also continue to outperform controls on the index of observed physical safety and awareness indicators, although the difference is no longer statistically significant. The sign of the effect on the job satisfaction and mental well-being index flips to positive, driven by improvements at better-managed factories, but is only statistically significant in the better-managed group. I continue to fail to reject the null that labor productivity, wages, and employment are unaffected. In sum, in the longer-run, workers appear to be better off due to stronger OSH committees, while factories appear to be no worse off.

This research contributes to the literature on the allocation of decision-making authority in organizations, and in particular the involvement of workers in firms' decision-making. A central question in organizational economics is how the allocation of authority affects organizations' performance; both theoretical and a growing body of empirical work indicates that decentralization is more likely to outperform centralization when employees who are lower in the hierarchy have a greater informational advantage and when their incentives are more aligned with those of top decision-makers (Aghion and Tirole, 1997, Bolton and Dewatripont, 2013, Bandiera et al., 2021, Kala, 2023).

The question of *workers'* involvement in organizations' decision-making is also of paramount importance to labor economists (see Jäger, Schoefer and Heining (2021) for a discussion), but until recently, has been difficult to study empirically due to the challenge of finding settings with exogenous variation in worker participation. Jäger, Schoefer and Heining (2021) and Harju, Jäger and Schoefer (2021) study changes to European countries' laws on worker representation on boards of directors; in both cases, they find very limited effects of worker voice on a wide variety of firm and worker outcomes. The specific institutional context of OSH committees itself is of great interest to economists interested in the economic determinants of worker well-being (e.g., Weil (1999)). To my knowledge, though, the current paper is the first to randomize the allocation of authority between firms and workers across establishments. It provides causal evidence that increasing workers' participation in OSH decision-making leads to small improvements in workers' health and safety, with no detectable adverse effects for firms. It documents that improved information flows between workers and managers is a key mechanism underlying these results. These findings are consistent with workers' having an informational advantage vis-à-vis top decision-makers in at least some OSH-related domains (e.g., due to their proximity to hazards that arise on the production floor) and with the misalignment between workers' and factories' OSH-related incentives not being too great due to the demands of factories' MNC buyers.

This research also contributes to an emerging literature on the impacts of MNCs' sourcing practices related to labor on suppliers and on their workers. Harrison and Scorse (2010) show that anti-sweatshop campaigns against Nike, Adidas, and Reebok led the Indonesian government to raise minimum wages, which caused large real wage increases with some costs for firms but no significant effects on employment. Amengual and Distelhorst (2019) use a regression discontinuity design to study Gap Inc's supplier code of conduct for labor and find that a failing audit grade improves compliance if coupled with the threat of a reduction in orders. Alfaro-Ureña et al. (2021) develop a general equilibrium model to study the incidence of private enforcement on firms and workers in sourcing origin countries; they apply it to MNCs with affiliates in Costa Rica, finding that private enforcement negatively affected the sales and employment of exposed suppliers but positively affected the earnings of their workers, with net positive effects on domestic welfare. I contribute by providing experimental evidence on the effects of MNCs' enforcement of local labor laws in a context where state enforcement is lacking. My findings demonstrate that MNCs' enforcement can improve compliance.

The remainder of this paper is organized as follows: Section 2 describes the institutional context, the Alliance, and the Alliance's OSH Committee Program. Section 3 presents the research design. Section 4 presents the results. Section 5 concludes.

2 Background

2.1 OSH committees in international labor standards

Joint worker-manager OSH committees are a core component of international labor standards for safety. The ILO included OSH Committees in its core Occupational Safety and Health Convention (No. 155, Articles 19-20), which was adopted in 1981.[7] The rationale for OSH committees is to ensure cooperation between managers and workers to achieve a reasonably safe workplace and to ensure that the employer fulfills its obligations (ILO,

[7] ILO Conventions are legally-binding treaties that may be ratified by member states.

1981). The ILO's Recommendation No. 164, a non-legally binding guideline, details how OSH committees should be formed, what types of authority they should have, and what types of legal protection they should enjoy. For example, it specifies that OSH committees should have equal representation of workers and employers and that worker representatives (hereafter, worker reps) should be democratically-elected by their peers. As of 2015, 117 countries had legislation regulating OSH committees (ILO, 2015).

2.2 Bangladesh's apparel sector & labor standards

Bangladesh plays a critical role in the global apparel supply chain. It is the second largest exporter of clothing in the world behind China (World Trade Organization, 2017). Buyers rely on Bangladesh for its combination of low prices and large production capacity (McKinsey & Company, 2011). Apparel has also driven Bangladesh's industrial transformation (Central Intelligence Agency, 2016); in 2016, apparel exports constituted 81% of its exports and 13% of its Gross Domestic Product.[8] The sector directly employs between 4-5 million of Bangladesh's 66.6 million workers.

As with China, Vietnam, and other leading apparel producers, Bangladesh's apparel sector has been criticized for its poor working conditions and limited freedom of association (FOA) rights for years (ILO, 2016). Its labor law has consistently lagged behind international standards for safety and for FOA rights, and collective bargaining has been "virtually nonexistent" in the sector (US Senate Committee on Foreign Relations, 2013). Decades of rapid industrial growth and weak state institutions culminated in a series of high fatality industrial accidents in 2012-13, including the collapse of the Rana Plaza building, that killed at least 1,260 workers and injured at least 4,366 workers at exporting factories.[9,10] The paper's Supplementary Materials provides more information on fatalities and injuries in Bangladesh's apparel sector since the early 2000s. Many observers directly linked the industrial disasters in 2012-13 to workers' inability to organize labor unions and to otherwise hold employers' accountable for unsafe conditions (US Senate Committee on Foreign Relations, 2013).

Under intense pressure, in July 2013, the GoB amended the labor law to strengthen safety and FOA provisions. The requirement for OSH Committees in factories with 50 or more workers was one of the amendment's key provisions. It was strongly resisted by

[8] Author's calculations using data from the World Trade Organization and the World Bank.

[9] Author's calculations based on data from the Bangladesh Institute of Labor Studies (BILS) and the AFL-CIO Solidarity Center.

[10] Following these events, world leaders rebuked the GoB for "not taking steps to afford internationally recognized worker rights to workers in that country," and western governments removed trade benefits from Bangladesh (Greenhouse, 2013*a*).

factory owners, who perceived OSH Committees as a step toward collective bargaining by workers. Reflecting owners' resistance, it took the GoB more than two years to publish implementation rules for OSH Committees, even under intense pressure from the international community (Munni, 2015). The implementation rules articulate requirements for OSH Committees' formation, operations, and responsibilities (see Table I).

Despite the *de jure* requirement to implement OSH Committees, *de facto*, enforcement was low. According to an ILO (2017) report, from 2015-2017, the GoB's focus was primarily on physical safety remediation of the garment sector. Unsurprisingly, compliance with the mandate was also low (Munni, 2017).

Table I: Key OSH Committee Requirements

Category	Requirements
Formation	6-12 committee members depending on factory size
	Equal worker-manager representation
	Appointment of worker representatives by collective bargaining agent or Participation Committee*
	Committee president appointed by management, vice president appointed by worker representatives
	In establishments with >33% female workforce, at least 33% of worker representatives must be female
Operations	Establishments must maintain a written policy on the OSH committee
	Committee must meet at least once per quarter
	Committee must maintain written meeting minutes
	Employers must provide members adequate time during working hours to fulfill their duties
	Employers must provide members with occupational health and safety training
Responsibilities	Committee must implement factory risk assessment at least once per quarter
	Committee must make safety-improvement recommendations to the employer
	Committee must arrange training and awareness-raising for workers
	Committee participates in the oversight of the following safety management systems: Management of equipment and work procedure; Management of dangerous fumes, explosives, or flammable items; Fire safety management; Management of dangerous operations, occupational disease, poisonous disease; Emergency Planning
	Committee investigates accidents and occupational disease and can submit recommendation to employer for treatment and compensation
	Committee organizes regular fire, earthquake, and other disaster management drills

*In factories with a collective bargaining agent (CBA), the CBA selects worker representatives to the safety committee. In factories where there is not a CBA, a Participation Committee (PC) selects worker representatives to the safety committee. A PC is legally required for all factories with 50 or more workers located outside of Export Processing Zones (EPZs). A PC has equal worker-manager representation that aims to promote trust and cooperation between employers and workers. It also aims to ensure application of labor laws.

Source: Translation based on Government of Bangladesh (2015).

2.3 MNCs' enforcement of safety laws

Prior to the Rana Plaza collapse, many large buyers sourcing from Bangladesh monitored their suppliers through their own social compliance programs (Bustillo, Wright and Banjo, 2012).[11] In the aftermath, however, Western buyers faced pressure to take collective action. European buyers signed an agreement with labor unions, which was known as the Accord on Fire and Building Safety in Bangladesh (hereafter, the Accord). Several U.S. retailers refused to sign the Accord; they formed the Alliance shortly thereafter.[12]

The Alliance was a coalition of 29 multinational retail and apparel firms that together represented the majority of North American garment imports from Bangladesh.[13] Its members committed to a five-year agreement to improve the safety performance of their Bangladeshi supplier bases, which included between 600-700 factories and 1.21 million workers.[14] See the paper's Supplementary Materials for an overview of the Alliance's Member Agreement, its programs, and the nature of this research collaboration. As per its Members' Agreement, the Alliance ceased operations on December 31, 2018. Upon its exit, many members supported the formation of Nirapon, an organization tasked with a similar set of safety oversight functions. 22 out of the 29 Alliance Members joined Nirapon, which continues to operate as of mid-2023.

The Alliance required all supplier factories to participate in its building safety audit and remediation and worker training and empowerment programs. Failure to comply with one or more programs resulted in suspension from all Alliance Members' supplier bases; the Alliance suspended 179 factories over its five-and-a-half year term.

[11] While there are no formal statistics on what share of production in global value chains (GVCs) is subject to private enforcement, there is reason to believe that it is substantial. Private enforcement efforts take many forms, including supplier codes of conduct, certification requirements, and, increasingly, multi-firm and multi-stakeholder initiatives ("coalition-based approaches") (de Bakker, Rasche and Ponte, 2019). Coalition-based approaches are organized both at the global- and country- or regional-level and target both broad categories of social and environmental standards (e.g., the Responsible Business Alliance monitors social and environmental standards in electronics, retail, auto and toy GVCs) and specific issues in a given GVC (e.g., the Responsible Mica Initiative aims to reduce child labor in mica mining in India).

[12] Their refusal was due to labor unions' participation and to the clause that buyers are subject to legally-binding arbitration (Greenhouse, 2013*b*).

[13] Alliance Members: Ariela and Associates International LLC; Bon Worth; Canadian Tire Corporation, Limited; Carter's Inc.; The Children's Place Retail Stores Inc.; Costco Wholesale Corporation; Fruit of the Loom, Inc.; Gap Inc.; Giant Tiger; Hudson's Bay Company; IFG Corp.; Intradeco Apparel; J.C. Penney Company Inc.; Jordache Enterprises, Inc.; The Just Group; Kate Spade & Company; Kohl's Department Stores; L. L. Bean Inc.; M. Hidary & Company Inc.; Macy's; Nordstrom; One Jeanswear Group; Public Clothing Company; Sears Holdings Corporation; Target Corporation; The Warehouse; VF Corporation; Walmart Stores, Inc.; and YM Inc.

[14] The Alliance coordinated its activities with the Accord, the GoB, and the ILO. The Accord and the Alliance were responsible for overseeing many aspects of OSH for the 60-70% of the sector that they covered. The GoB, with the ILO's support, was responsible for the remaining 30-40% of the sector (ILO, 2017).

2.4 The Alliance's OSH Committee Program

The Alliance created its OSH Committee Program because it considered credible OSH committees to be vital to achieving worker empowerment and effective communication between workers and managers (Alliance, n.d.). Nirapon continues to require supplier factories to participate in a similar program. The Program included five phases:

1. The Alliance conducted an onsite visit to verify that factories' worker representation body (Participation Committee (PC), Worker Welfare Association (WWA), or trade union) were democratically selected and formed according to the labor law.[15] If these bodies were not democratically formed, which was often the case, the Alliance coordinated with the brand(s) sourcing from the factory to oversee a new election. Once the factory reformed its PC, WWA, or trade union, it submitted evidence of its compliance to the Alliance.

2. The Alliance verified whether the factory had an OSH committee, and if so, whether it was selected and formed according to the law. If necessary, the factory reformed its OSH committee through a compliant process.

3. The Alliance again verified that the OSH committee was formed correctly through email and phone. It then provided two days of training to two worker and two management representatives from the OSH committee. The training covered roles and responsibilities, OSH, and leadership and communication skills. The training also set expectations for the Program's central feature: The preparation and fulfillment of an action plan to achieve compliance with the OSH committee law.

4. The OSH committee prepared an action plan for required activities.

5. Once the Alliance approved the action plan, the Alliance intensively monitored the OSH committee on its completion.

The action plan used an Alliance template and included a detailed schedule of required activities. Several members of management and the OSH committee president and vice president had to sign off on it. Before approving the plan, the Alliance reviewed it and worked with the factory to make revisions. The factory then implemented the plan and provided evidence to the Alliance by e-mail within 2-3 days of each required activity. The Alliance informed factories that repeated failure to submit evidence would result in escalation of the factory's status toward suspension. The Alliance reviewed submissions and investigated by phone calls, e-mails, and onsite audits that were unannounced or announced within a certain time period. At the end of the six months, the Alliance reviewed

[15] PCs are legally-required for all factories with 50 or more workers outside of Export Processing Zones (EPZs). In EPZs, the worker representation structure is a Workers' Welfare Association (WWA). In most Alliance supplier factories, the PC is the body that appoints the worker reps to the OSH committee.

the factory's progress. If the Alliance found it to be insufficient, the factory could be required to repeat parts of the program or its status could be escalated toward suspension. If the Alliance found it to be sufficient, the factory returned to the pool of factories being monitored through the Alliance's general monitoring program.

In the randomized controlled trial (RCT), I experimentally manipulated factories' exposure to phases 3-5 of the OSH Committee Program. This is because the Alliance's authority did not extend to the PC/WWA/trade union, so phases 1-2 depended on Alliance brands' engagement with their suppliers. Organizing free, fair, and contested democratic elections for worker representation bodies often took many months. As such, factories became eligible for the RCT once this process was complete. The treatment effects that I identify are thus the effects of strengthening OSH committees on factories with an OSH committee that exists, at least on paper, at baseline.

2.5 How might stronger OSH committees affect workers and factories?

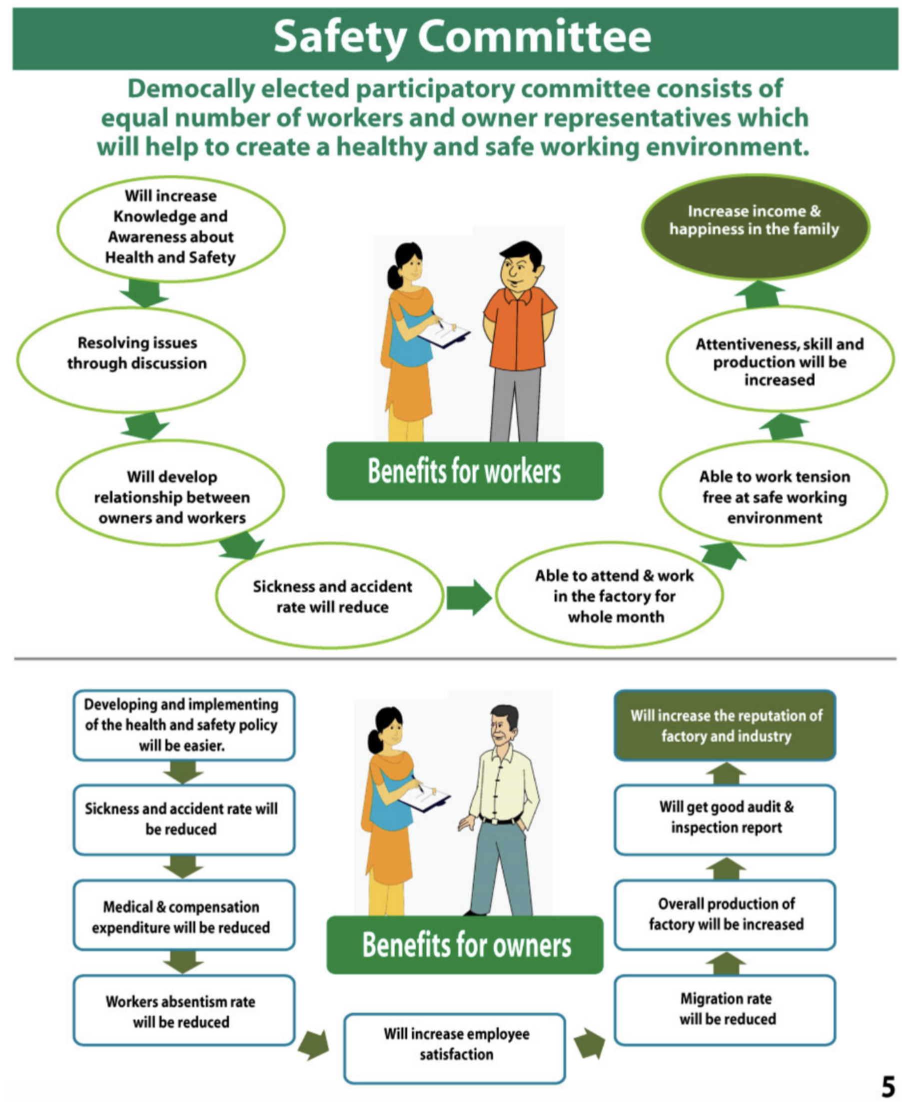
The theoretical literature on formal worker voice institutions suggests several channels through which joint worker-manager OSH committees may affect workers' health and safety and other measures of job quality. One is improving information flows between workers and managers (Hirschman, 1970, Freeman and Lazear, 1995). As OSH is an issue for which information asymmetries and agency problems inside the firm may contribute to inefficient social outcomes, mandated OSH committees may help to overcome communication challenges by more securely aggregating workers' concerns and by improving management's ability to credibly communicate with workers.

Another channel is increasing cooperation between workers and managers (Malcomson, 1983). OSH committees may support increased cooperation due to their authority to investigate whether management deviated from its OSH obligations in the case of accidents and their ability to enforce management's promises through legal recourse to the Labor Inspectorate. OSH committees may also improve coordination between workers and managers (Freeman and Medoff, 1985, Freeman and Lazear, 1995); through increased and more substantive discussion among OSH committee representatives, for example, information may be exchanged that enables new, improved OSH solutions to be identified. Finally, the mandatory nature of the OSH committees, and their legal recourse to the Labor Inspectorate, may increase workers' bargaining power over OSH (Viscusi, 1980).

Through these channels, OSH committees should unambiguously improve *objective* measures of workers' health and safety. They should also, however, increase workers' reporting of OSH concerns, injuries, and diseases. Consequently, it is ambiguous whether stronger OSH committees should decrease or increase *reported* injuries and diseases. I return to this measurement challenge in Section 3.

The first three of these channels, communication, cooperation, and coordination, also appear in the Alliance's causal chain for OSH committees, which is displayed in Figure I. As highlighted by the chain, improvements in workers' health and safety and in their relations with managers should increase workers' comfort in the workplace. Through these mechanisms, OSH committees should increase workers' job satisfaction and may contribute to improving their mental well-being.

Figure I: Alliance's Causal Chain for OSH Committees



Source: Alliance training materials for OSH committee members (English translation).

On the one hand, by improving workers' health and safety, improving the workplace climate, and increasing workers' job satisfaction and mental well-being, OSH committees may increase workers' productivity, as is proposed in the causal chain.[16] On the other, they may hamper workers' productivity through channels such as imposing stringent safety protocols and floor-plan requirements. Given the possibility of positive and negative effects, the effect of stronger OSH committees on labor productivity is ambiguous.

If stronger OSH committees increase workers' non-pecuniary benefits, then the effects on their wages and employment depend on the slope of the labor supply curve facing firms. If the labor market is competitive, then compensating differentials will reduce wages. In this case, if OSH committees' costs to factories exceed workers' valuation of them, employment will eventually fall. If employers face upward-sloping labor supply curves, however, workers may see their overall compensation increase, and wages and employment will not fall.

3 Research design

3.1 Randomized assignment to the OSH Committee Program

The RCT was built into the Alliance's staggered roll out of the OSH Committee Program. The Alliance rolls out all of its programs in phases, so from the experimental factories' perspective, it would not be apparent that the factory was part of a treatment or control group. From January-December 2017, each time that the Alliance had a batch of factories with verified PCs/WWAs/trade unions, it sent me the list. Within batch, I randomly assigned 50% of factories to the treatment group and 50% to the control group. The result is a stratified RCT with six strata (batches), and a total of 41 treatment factories and 43 control factories (84 total factories).[17] In 11 cases in which multiple factories shared ownership and location (building or compound), I randomly selected one factory to participate in the RCT.[18] If the factory was assigned to the control condition, the Alliance did not conduct the OSH Committee Program with any other factories at the same location. OA Table B.I reports summary statistics for the sample.

[16] A growing body of research documents the adverse effects of poor environmental conditions on workers' productivity, including heat (Adhvaryu, Kala and Nyshadham, 2020), noise (Dean, 2021), and air pollution (Adhvaryu, Kala and Nyshadham, 2019).

[17] All control factories were required to participate in the Program after completing the study.

[18] A compound is a plot of land housing multiple factories at the same address.

3.2 Data collection and measurement

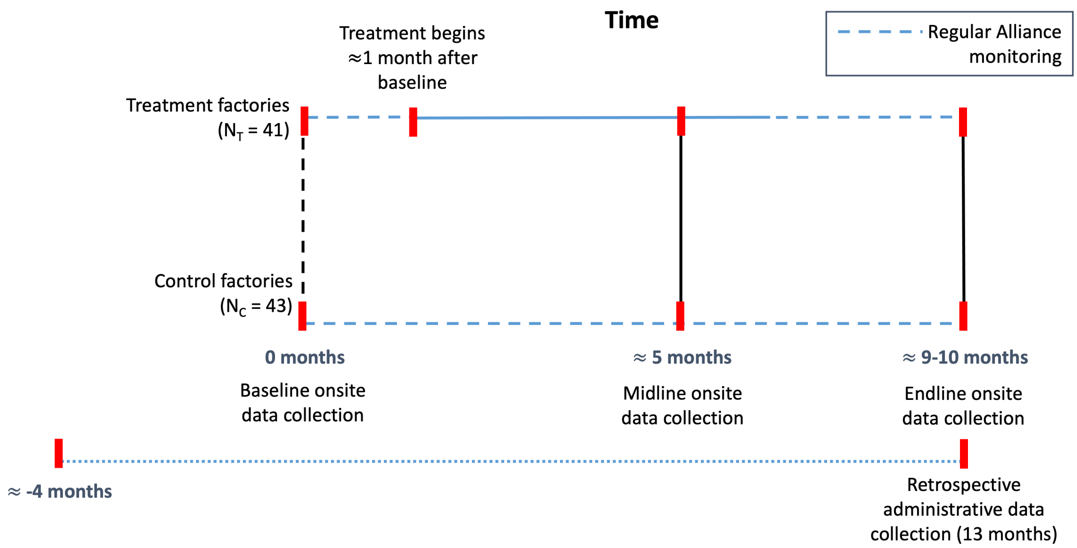
This analysis uses four main sources of data. First, it uses several types of data collected during three separate, day-long visits to factories implemented over nearly one year. Second, it uses monthly production, HR, and other business performance-related data collected using a retrospective questionnaire administered following the final data collection visit. Third, it uses administrative data from the Alliance. Finally, it includes export records from Bangladesh, which provide prices and quantities of produced goods for sample factories that are direct exporters. The Alliance encouraged factories to cooperate with data collection, which was not irregular, as the Alliance monitored of all of its programs and had previously partnered with academic researchers on data collection.

The onsite visits included three types of data collection: Surveys of stakeholders, document collection and verification, and spotchecks of safety conditions. Surveys included 20 randomly selected workers, the OSH committee president, two randomly-selected OSH committee worker reps, the factory's most senior manager, and up to 20 randomly-selected lower-level managers. The document verification process entailed checking legally-required and Alliance-required documentation. It also included photographing records for digitization by the research team. At the second and third visits, a trained assessor visited the production floor to check physical safety conditions using a checklist. The team leader was an assessor, who was responsible for managing interactions with management, verifying documentation, and implementing the safety spotchecks. A junior assessor oversaw the the survey process, photographed records, and supported survey implementation. Three enumerators conducted surveys.

In any study of compliance, one must be concerned about subjects' incentives to misreport. I designed the data collection protocols to minimize experimenter demand effects and the potential for the OSH Committee Program to affect reporting. I also collected data that would allow me to directly test for truth-telling. In OA A, I provide econometric evidence against reporting bias affecting my estimated treatment effects. In the Supplementary Materials, I provide a detailed overview of how the data collection protocol was designed to minimize experimenter demand effects.

Figure II displays the experiment's approximate timeline. The first visit established factories' baselines. The second visit, about five months later, aimed to measure outcomes immediately after treatment factories completed the most intensive phase of the OSH Committee Program. As treatment factories had been exposed to the Alliance Program for 3-4 months by this time, if the intervention has effects, I expect to detect them at this round. I refer to possible effects at this round as "short-run effects." The third visit, about 10 months after baseline, aimed to measure outcomes a few months after treatment factories completed the OSH Committee Program, when they were no longer subject to intensive monitoring. I refer to possible effects at this round as "longer-run effects."

Figure II: RCT timeline



Notes: In the figure, the solid blue line for treatment factories denotes the OSH Committee Program period (intensive monitoring phase). The dashed line for both treatment and control groups denotes regular Alliance monitoring, under which factories receive indirect monitoring through other Alliance programs (e.g., building safety remediation verification visits) and those factories that have completed the OSH Committee Program may be audited through onsite visits. Due to the staggered entry of strata of factories into the RCT, the calendar timeline varies by stratum, with the earliest baseline visits in January 2017.

3.2.1 Pre-specified primary & other outcomes

I analyze six primary outcome variables.[19] My first outcome is an index of compliance with the OSH committee law, which I use to examine whether the Alliance's intervention is successful at increasing factories' compliance with it. I use an index variable because compliance is many-dimensional: The regulation includes requirements on how OSH committees are formed, how they operate, and their responsibilities (see Table I). To determine the variables included in the index, I enumerated the regulation's stipulations. Whenever relevant, I measure a factory's compliance with a stipulation using multiple sources of information. For example, to determine how worker reps to the OSH committee were selected, I put 50% weight on the OSH committee president's report (a member

[19]The Alliance members did not participate in designing the research nor in selecting the outcome variables. Over the course of the research project, I made periodic presentations to Alliance members to update them on the progress, but these were for informational purposes only.

of management) and 50% on the mean of the worker reps' reports.[20] The list of sub-variables in this and other index variables is available in the Supplementary Materials.

Next, I turn to workers' safety and health. I focus on physical safety indicators and on the occurrence of occupational injuries and diseases. Ex ante, I did not focus on fatalities because I expected them to be relatively rare; consistent with my expectation, there were no fatalities at experimental factories during the study period.[21] The concern with testing for effects on occupational injuries and diseases is that OSH committees aim to increase workers' voice on OSH, so strengthening them may increase workers' reporting of injuries and diseases. Consequently, even if OSH committees reduce the true rates of injuries and diseases, I may find that the measured rates increase. In light of this possibility, my primary measure of OSH is an index of observed physical safety and awareness indicators. This index includes objective OSH measures, so the prediction is that stronger OSH committees should improve performance on it. The index components include:

- Physical safety:
  - Performance on an independently-evaluated OSH checklist.
  - Progress with required building safety remediation based on Alliance building safety audits (Alliance "Corrective Action Plan (CAP)" completion).[22]
- Awareness:
  - Workers' awareness of the OSH committee.
  - Workers' safety knowledge.
  - Senior managers' awareness of the OSH committee.

To develop the OSH checklist, I worked with an OSH expert to who helped me to identify critical safety items that could be checked during a 30-minute floor visit. These items were drawn from a checklist for typical OSH audits conducted by major global brands. 23 out of 26 items in the checklist appear in the ILO's Code of Practice on Safety and Health

[20] I use this weighting scheme for all index variables with reports from the OSH committee president and from the worker reps. At baseline, the correlation between presidents' and workers' reports ranges from -0.16 (sub-variable no. 23) to 0.38 (sub-variable no. 3); most correlations are positive.

[21] I collected self-reported data on fatalities from factories and cross-checked it using two different datasets that I constructed using information collected by two independent labor organizations in Bangladesh. Relatedly, the experimental sample experienced few fires during the experiment (three factories reported one or more fires), which are a leading cause of fatalities in the sector. See the Supplementary Materials for more information.

[22] Every Alliance-audited factory had a Corrective Action Plan (CAP) based on violations found in the Alliance's building safety audit. The CAP detailed the remediation actions that the factory would take to address the safety violations. The Alliance monitored factories' remediation progress and suspended factories that failed to make sufficient progress.

in Textiles, Clothing, Leather and Footwear Industries, which is the global standard on practical measures to reduce major risks in these sectors (ILO, 2021).[23,24]

I also measure OSH using two outcomes that are more subject to reporting effects. The first measures visitors to factories' onsite medical clinics. These clinics are typically the first provider of care for sick or injured workers. I collected their visitor logs, which are not used for injury reporting. While the logs are subject to reporting effects, I expect them to be less exposed compared to factories' injury registers. I focus on the best-measured variable available, which is the daily count of visitors to the medical clinic. Using monthly data on the number of employees, I calculate the proportion of the workforce that visits the clinic per day. To smooth noise in the daily records, to address differences in the number of day-level observations per factory, and to present results at the same level as other outcomes measured using factories' administrative data, I average by month. The second outcome is officially-recorded injuries in factories' injury registers. These registers are maintained for reporting injuries to the government. I argue these registers are more likely to reflect factories' adherence to reporting requirements and their norms around reporting, and so changes in voice will affect them.[25] Further, one of the OSH committee's responsibilities is to manage accidents, including investigation of accidents, so strengthening OSH committees may directly increase recording of accidents. The predicted effects on both outcomes are ambiguous, although I identify the medical clinic logs as less subject to the reporting channel.[26]

These sources are only available for 62 and 66 factories, respectively, due to a combination of factors, including not all factories having medical clinics, not all factories maintain-

[23]We excluded items that the OSH committee could not plausibly influence within the study's duration. We also identified several items that the social compliance assessor would only check during the 9-10 month visit. The rationale for this approach was twofold: First, the OSH expert identified eight items that required more than 3-4 months, but plausibly less than 8-9 months, for the OSH committee to address. Second, I wanted to reserve some factory spaces (e.g., bathrooms) to only be visited during the third visit. I aimed to gain insight into the extent to which management was responding to the research team's visits. Due to an administrative error, the eight items were not included in the third visit checklist for 14 out of 80 factories. As such, I depart from my PAP by not including these items.

[24]Four of the OSH checklist items overlap with the best practices for textile factory operations identified by the management consultants in Bloom et al. (2013). This overlap underlines that some OSH practices are aspects of modern management practices.

[25]For medical clinic records, treatment and control factories are balanced at baseline in the average number of daily (p-value of diff.=0.510) and monthly (p-value of diff.=0.296) observations. They are also balanced on the number of monthly injury observations (p-value of diff.=0.628).

[26]Consistent with the OSH checklist and medical clinic records conveying more information about safety, the correlation between factories' performance on the OSH checklist and the proportion of workers who visit the medical clinic is -0.170. In contrast, the correlation between checklist performance and recorded injury rates is 0.024. Medical clinic visitor rates and injury rates are only very weakly positively correlated: 0.046. Finally, the correlation between factories' performance on my OSH checklist and on the Alliance's initial building safety audit is 0.296.

ing medical clinic and/or injury records, limited legibility or information content of the records, and an initial misunderstanding among the research team that resulted in incomplete digitization of records for some factories. Both types of records were photographed during onsite visits for later digitization. For the medical clinic records, this process was time intensive, so social compliance assessors were instructed to photograph a sample of 3-6 days per month when records were large.[27] Due to a misunderstanding about how the records were to be used, assessors sometimes did not photograph complete days. For this reason, and due to the other aforementioned issues, the number of days of observed visitors to medical clinics per factory varies. To address this issue, I present results for this outcome with and without probability weights based on the pre-baseline number of days observed.

Returning to my primary outcomes, I next examine: (3) Workers' job satisfaction and mental well-being (index); (4) wages; (5) employment; and (6) labor productivity.[28] The third primary outcome summarizes the effects of strengthened OSH committees on self-reported and revealed preference measures of workers' job satisfaction and mental well-being. I construct it using survey questions and administrative data on worker turnover and absenteeism. Wages are the log of gross wages paid to all employees in a month. Employment is the total number of people employed at the factory in a month. Wages and employment are measured using administrative data provided by the factories. Labor productivity is measured as the log of the physical quantity of output per person-hour. For the 53 factories that are direct exporters, I measure output using the export records, which I expect to be less subject to measurement error than my self-collected data. There is a lag between when factories produce a good and when it is exported; according to industry experts, it is 3 months on average. I use this lag for my main results and show robustness to 1-6 month lags in OA Table B.XV. For the remaining factories, which produce intermediate inputs into exported products, I use data provided by the factories.[29]

Although not a pre-specified outcome, the export records allow me to observe average unit prices for the 53 direct exporters. I test for effects on prices in subsection 4.2.

To construct the index variables, as per my PAP, I use the methodology proposed by

[27] Assessors were told to photograph the 5th, 15th, and 25th days of month. If these days were weekends or holidays, they were instructed to photograph the preceding and following days.

[28] I pre-specified that I would analyze total factor productivity or labor productivity. I indicated that I would analyze labor productivity if I determined that I could not measure non-labor inputs to production with sufficiently high quality. Ultimately, I could measure labor productivity for more factories and with less measurement error.

[29] In 5 multi-product factories that are not direct exporters, output is measured at the product-level. For these factories, I include the main product in the analysis and determine the share of labor allocated to this product using employee lists. I determine a factory's primary product using quantities of physical output.

Anderson (2008) based on O'Brien (1984).[30] The method entails an average of a family of variables that have each been oriented to be unidirectional, standardized, and weighted by the sum of its row in the inverse variance-covariance matrix calculated using the control group. The weighting maximizes the amount of information captured by the index, as it places less weight on highly correlated outcomes and more on less correlated ones. This approach is well-suited for this study because, due to the staggered roll-out, I was not able to collect a full baseline before committing to the construction of indexes. Summary index variables also reduce the number of hypothesis tests, which reduces the risk of overrejection of the null hypothesis. Finally, they increase my ability to detect effects on multiple outcomes that, aggregated, achieve statistical significance (Anderson, 2008).

I also pre-specified secondary outcome variables to explore possible mechanisms underlying the effects on primary outcome variables. Index components for secondary index variables are available in the paper's Supplementary Materials, OA Table B.II reports baseline balance tests, and OA Tables B.III-B.V present results. I reference these results to support interpretation. In the interest of transparency, I report all deviations from my PAP and their rationales in OA Table B.VI. Overall, I adhere closely to my PAP.

3.2.2 Econometric analysis

I estimate the intervention's average treatment effects using the following specification:

$$Y_j = \alpha + \beta T_j + \theta Y_{j,t=0} + \gamma_w + \mathbf{X}_j' \lambda + \epsilon_j \qquad (1)$$

where Y_j is the outcome for factory j. T_j is the treatment indicator, $Y_{j,t=0}$ is a control for the baseline value of the outcome variable. γ_w is a stratum indicator, and ϵ_j is the residual. β is the coefficient of interest. I report robust standard errors; for outcomes with multiple observations per factory, I report standard errors that are clustered at the factory level. Given my small sample size, I also report randomization inference (RI) p-values for all primary outcomes (Athey and Imbens, 2016). I also estimate effects for primary outcomes using the post double selection (PDS) lasso to select control variables (Belloni, Chernozhukov and Hansen, 2014).[31] The set of potential controls include all variables in Table II Panels B and C that are available for the full sample, the log baseline number of employees, and the baseline value of the outcome variable.

[30] Other recent RCTs that report index results using this methodology include Casey, Glennerster and Miguel (2012) and Haushofer and Shapiro (2016).

[31] This approach has two advantages: It allows me to increase the precision of my estimates while avoiding concerns about specification searching, and it allows me to test my results' robustness to the possibility that chance baseline imbalances between the treatment and control group influence my estimates.

To test for heterogeneous treatment effects (HTEs), I use the following specification:

$$Y_j = \alpha + \beta_1 T_j + \beta_2 R_j + \beta_3 T_j * R_j + \theta Y_{j,t=0} + \gamma_w + \epsilon_j \tag{2}$$

where R_j is an indicator for above median baseline value of a pre-specified interaction variable. The notation for equation 2 is otherwise analogous to that for equation 1. In this specification, β_1 is the estimated treatment effect on factories with a below median baseline value of the interaction variable, $\beta_1 + \beta_3$ is the estimated treatment effect on factories with an above median baseline value of the interaction variable, and β_3 is the difference between these two effects. I report β_1 and $\beta_1 + \beta_3$ as well as the p-value for β_3.

For wages, employment, and labor productivity, to leverage the monthly-frequency of observations, I also present more flexible event study estimates. The model is as follows:

$$Y_{jt} = \sum_{k=a}^{b} \beta_k \, Treatment_{kt}^{k} + \delta_t + \lambda_j + \epsilon_{jt} \tag{3}$$

where t indicates month, $a = -5$ and $b = 7$ indicate five months before and seven months after, respectively, treatment factories begin the OSH Committee Program, δ_t are month fixed effects, and λ_j are factory fixed effects. The omitted term is the interaction of the treatment with the month before treatment factories started the Alliance's intervention.

To account for multiple hypothesis testing, I report multiplicity-adjusted p-values. Across my primary outcome variables, I control the False Discovery Rate (FDR), the expected proportion of rejections that are false positives. I report FDR-sharpened p-values for my preferred specification for all primary outcomes (Anderson, 2008). For index variables, I also show p-values adjusted to control the FDR across each variable's sub-indexes.

3.2.3 Internal Validity

Attrition: Four factories, two treatment and two control, attrited from the sample. Three of the four were suspended by the Alliance for failure to make progress with physical building safety remediation. One control factory refused to participate in the second data collection visit. I address attrition by reporting Lee (2009) bounds on the treatment effects for primary variables with statistically significant treatment effects (OA Table B.IX). There is minimal difference between the upper and lower bounds of the treatment effects, and with the exception of the lower bound for the safety indicators index, all estimates are statistically significant at the 5% or 10% level.

Table II: Baseline balance tests

	(1) Control mean	(2) Control SD	(3) T-C diff	(4) p-value	(5) RI p	(6) Number of factories
Panel A: Primary outcome variables						
Compliance index	-0.010	(0.250)	-0.068	0.389	0.377	80
Safety Indicators index	0.006	(0.403)	-0.050	0.662	0.658	80
Job Satisfaction & well-being index	0.001	(0.375)	-0.095	0.346	0.347	80
Number of employees†	1192	(1206)	-155	0.595	0.609	80
Gross wages (log)†	15.820	(1.044)	-0.190	0.451	0.459	72
Labor productivity (log)†,‡	0.788	(0.918)	0.195	0.378	0.395	77
Labor productivity (log)†,‡, product FE	0.043	(0.473)	-0.109	0.269	0.336	77
Panel B: Factory characteristics						
Trade union at factory	0.049	(0.218)	-0.049	0.165	0.490	80
EPZ(1=Yes)	0.171	(0.381)	0.014	0.873	1.000	80
Sewing (only)	0.439	(0.502)	-0.074	0.511	0.654	80
Number product types	1.171	(0.667)	0.033	0.780	0.846	80
Monthly absenteeism	4.859	(4.581)	-0.677	0.439	0.449	80
Monthly turnover	3.989	(5.003)	-0.704	0.402	0.482	80
Prop. employees visit medical clinic (daily)†	0.011	(0.014)	0.004	0.569	0.665	53
Prop. employees injured (monthly)†	0.003	(0.005)	-0.000	0.798	0.817	66
Prop. employees injured-major (monthly)†	0.000	(0.001)	0.000	0.944	0.949	66
Prop. employees injured-minor (monthly)†	0.002	(0.004)	-0.000	0.850	0.857	66
Participation in Alliance training (6 mo pre-baseline)	0.049	(0.218)	-0.020	0.625	1.000	80
Number Alliance remediation visits to factory (6 mo pre-baseline)	0.171	(0.442)	-0.010	0.908	1.000	80
Panel C: Worker survey respondent characteristics						
Age	27.163	(3.681)	0.402	0.633	0.636	80
Proportion female	0.557	(0.281)	-0.093	0.144	0.140	80
Education (yrs)	6.232	(1.612)	-0.381	0.319	0.316	80
Tenure (yrs)	3.861	(2.461)	-0.213	0.683	0.667	80
Prior industry experience (yrs)	1.516	(0.877)	0.066	0.783	0.779	80

Notes: This table reports OLS estimates of baseline differences between control and treatment groups. For each outcome or covariate, I report the baseline control group mean and SD in columns (1) and (2). In column (3), I report the estimated coefficient for the treatment indicator from a regression of the outcome or covariate on the treatment indicator and stratification variables. In columns (4)-(5), I report the p-value for the treatment indicator calculated using robust standard errors, and the RI p-value for the coefficient reported in column (3) based on 5000 draws. In column (6), I report the number of factories included in the regression. † The regression sample includes all observations in the five pre-treatment months for these variables. Standard errors are clustered by factory for these variables. ‡The regression sample is trimmed at the 99th percentile of all factory-month labor productivity observations. *p<0.1; **p<0.05; ***p<0.01.

Baseline Balance: Table II shows balance tests for primary outcomes and factory and worker characteristics for factories that did not attrite.[32] Three factories that do not appear in the export data declined to provide production data, and eight declined to provide wage data. In sum, the randomization successfully generated two groups that are balanced along observable characteristics. Although the difference is not statistically sig-

[32]The Supplementary Materials reports balance including factories that attrited.

nificant, treatment factories score 0.10 sds lower than controls on the job satisfaction and well-being index. This gap is largely due to a treatment factory whose index value is more than 4 sds below the mean. I adopt a common approach to handling outliers, which is to present results including the outlier and to report baseline balance and the main the results after dropping it (see the Supplementary Materials). The results are robust.

Finally, for labor productivity, although not statistically significant, there is a qualitatively large difference between the groups. This difference is due to small differences in factory types between the groups. The treatment group has somewhat more non-sewing factories (e.g., washing and accessories) that tend to be more capital intensive. For this reason, I deviate from my PAP and show that there are no differences in labor productivity between treatment and control factories that produce the same type of product. I also report results for labor productivity with and without product fixed effects.[33]

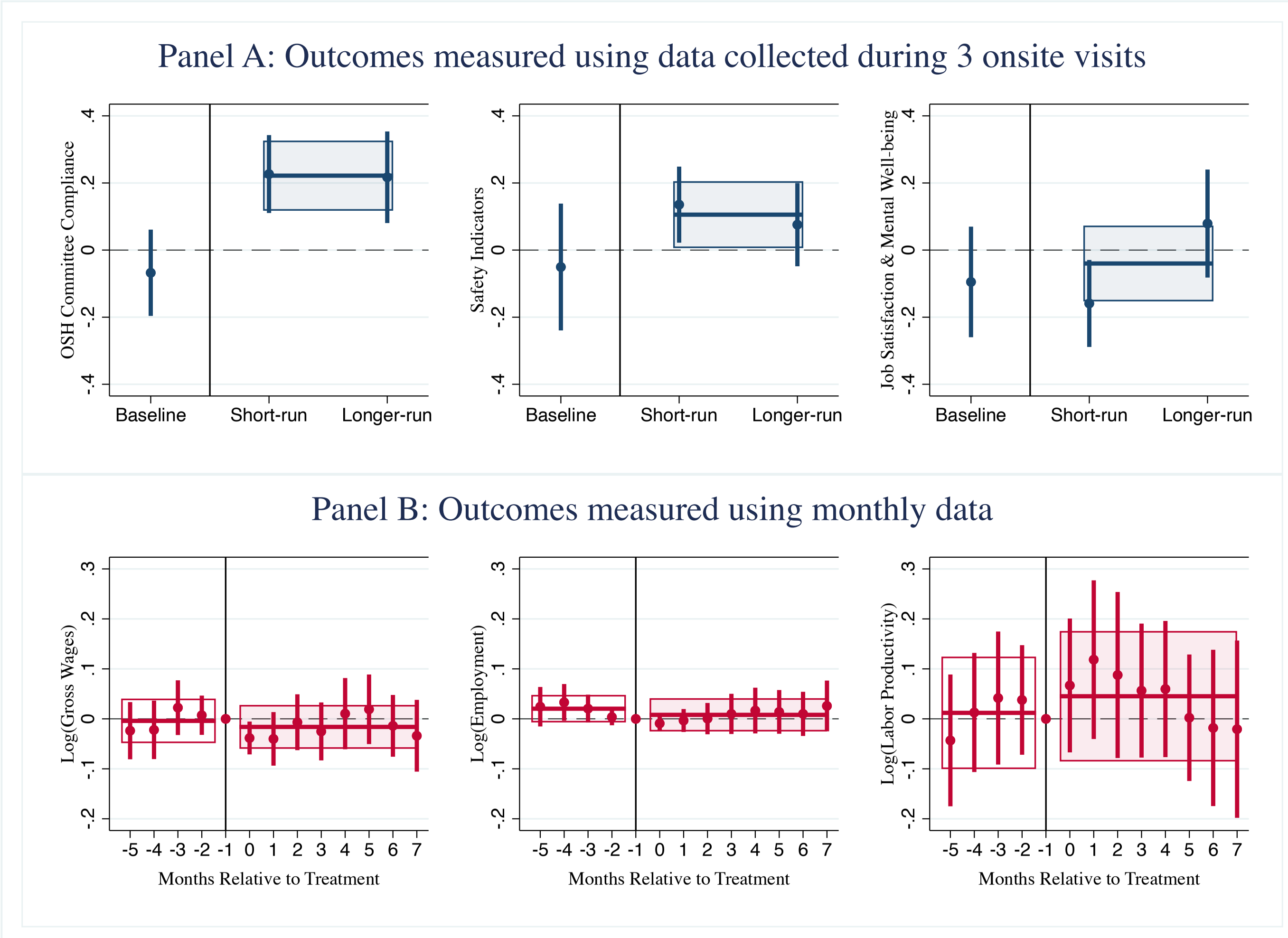
OA Table B.II shows balance tests for OSH committee president and worker reps and senior manager survey participants; variables from these surveys are used to construct certain index variables. The treatment and control groups are balanced on all variables. OA Table B.VII presents baseline balance tests for sub-index variables for primary outcome index variables. For the OSH indicators index, workers' awareness of OSH committees is lower at treatment factories, although this difference lessens and is not significant at the 5% level when the outlier factory is dropped; estimated effects on this sub-index should be interpreted with appropriate caution. Senior managers are marginally more aware of OSH committees' activities at treatment factories ($p < 0.10$).

Compliance: Three treatment factories did not receive treatment by the second data collection visit. Once we identified the reasons for the delays, we resolved them for other factories that could have been impacted.[34] A fourth factory began the OSH Committee Program less than two weeks before its second data collection visit. I address noncompliance by presenting Intent to treat (ITT) estimates and presenting the Local Average Treatment Effect (LATE) estimates for primary outcomes in OA Table B.VIII.

[33] The Alliance did not have data on factories' product types. Because the initiative covered all tiers of suppliers including washing factories, embroidery factories, and accessories factories (e.g., hangers, tags, plastic bags), these factories ultimately comprised a larger share of the sample than I expected.

[34] Two factories are located in Chittagong, where the Alliance implemented the OSH Committee Program in batches to ensure cost effectiveness, and it did not have a sufficient number of factories to implement it with these factories. One factory did not participate due to a critical manager being on an extended leave.

Figure III: Treatment effects on primary outcomes



Notes: In Panel A, each figure reports coefficients from separate regressions of the outcome variable on the treatment indicator and strata fixed effects in each round of data collection for non-attrited factories. In the aggregate specifications, with coefficients represented by the horizontal lines and their 90% confidence intervals indicated as boxes, the short- and long-run rounds of data collection are pooled. 90% confidence intervals (CIs) calculated using robust standard errors are reported. In Panel B, each figure reports coefficients from an event study in which the omitted term is the interaction of the treatment with the month that the Alliance's intervention started. 90% confidence intervals (CIs) calculated using cluster robust standard errors are reported. In the aggregate specifications, the pre-treatment months and post-treatment months, respectively, are pooled. For labor productivity, the regression sample is trimmed at the 99th percentile of all factory-month observations. In all figures, the vertical black line distinguishes measurements that are not (left of line) and that are (right of line) subject to treatment effects. Section 5 of the Supplementary Materials presents this Figure after dropping the outlier factory on worker outcomes and the factory that partially shuts down during the experiment, respectively.

4 Results

I present the results in five sub-sections. First, I present the short-run results. I discuss the effects of MNCs' enforcement on factories' compliance with the OSH committee law. Next, I assess the effects of strengthened OSH committees on workers' safety and health, job satisfaction and well-being, and on their wages, employment, and productivity. I then turn to examine leading possible mechanisms underlying these results. Fourth, I explore a possible role for management practices. Finally, I present the longer-run results, after MNCs cease intensive enforcement.

4.1 MNC enforcement & factories' compliance

4.1.1 Factories' compliance at baseline

First, I examine factories' compliance with the OSH committee law at baseline, as measured by the research team. As explained in Section 2.4, in order to be eligible for this study, factories had to complete phases 1-2 of the Alliance's OSH Committee Program. For the median factory, the baseline visit took place about 3.25 months after the Alliance reported that it verified that the factory's OSH committee was formed as per the law. Perhaps it is unsurprising, then, that all except one factory had an OSH committee at baseline, at least on paper. That said, 80% of factories established their OSH committees *after* the legally-required deadline. 73% of OSH committees were of the correct size and composition, although many presidents and worker reps reported non-compliant selection procedures for worker reps.[35]

In most factories, OSH committees were just becoming active. In 10%, the committee had not yet met; in a further 16%, it had only met once. Many OSH committees were not implementing all of their legally-required responsibilities, such as conducting risk assessments and making recommendations to management to resolve identified issues. At baseline, 15% of OSH committees had ever conducted a risk assessment, and 73% of senior managers reported receiving reports on OSH issues at least once per three months. Committees' reported fulfillment of other responsibilities varied.[36] Finally, there were

[35] In one control factory, the OSH committee was found to be comprised only of managers. As such, compliance index outcomes related to formation are coded as non-compliant. At the second visit, this factory provided the names of workers whom it indicated were members of the committee. Through the worker rep survey, it emerged that management instructed these workers to participate because the committee remained all managers. Again, the formation compliance index outcomes are coded as non-compliant.

[36] According to presidents, OSH committees were most likely to participate in fire prevention and preparedness activities (84%) and least likely to participate in accident investigation (55%). Although 44 presidents reported that the OSH committee was responsible to investigate in case of an accident, only 7 indi-

some reports of management interfering with the committee's activities.[37]

Table III: Short-run treatment effects: Primary outcome index variables

	(1)	(2)	(3)	(4)	(5)	(6)
	OSH committee compliance index		Safety indicators index		Job satisfaction and mental well-being index	
Treatment	0.258***	0.204***	0.144**	0.151**	-0.149*	-0.136*
	(0.060)	(0.068)	(0.067)	(0.065)	(0.079)	(0.071)
	{0.001}	{0.019}	{0.093}	{0.053}	{0.114}	{0.083}
	[0.000]	[0.005]	[0.040]	[0.027]	[0.061]	[0.073]
Control Mean	0.019	0.019	0.109	0.109	-0.013	-0.013
Observations	80	80	80	80	80	80
Stratification variables	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	N	Y	N	Y	N
PDS Lasso Controls	N	Y	N	Y	N	Y

Notes: This table reports OLS estimates of treatment effects on primary outcome index variables. Outcome variables are listed at the top of each column. In all cases, higher values of the index correspond to positive outcomes. Each column reports the estimated ITT effect from a separate regression. Robust standard errors are reported in round brackets. p-values adjusted to control the FDR across primary outcomes are reported in curly brackets. RI p-values based on 5000 draws are reported in square brackets. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$.

4.1.2 The effects of MNC enforcement on compliance

Figure III presents the main results for my primary outcomes. The first figure in Panel A tells us whether the Alliance's intervention increases factories' compliance with the OSH Committee law. It displays the estimated difference between the control and treatment groups' performance on the compliance index at each round of data collection, with 90% confidence intervals (CIs). The figure shows that they performed similarly at baseline but that MNC enforcement improved the treatment group's performance by just over 0.2 sds in the short-run. This result is the first experimental evidence that MNC enforcement can cause factories to increase compliance with the law, above and beyond the effects of state-supplied enforcement.

Table III also presents the short-run results, estimated with a control for factories' baseline value of the index (col. 1) and flexibly selecting controls using the PDS lasso (col. 2). The estimated effect on compliance is 0.2 sds (FDR p=0.019) (col. 2). The control mean

cated that it had actually participated in an accident investigation.

[37] In 10% of factories, at least one worker rep reported that management had offered bribes or attempted to block OSH committees' activities. 5% of presidents and 7% of worker reps reported that they were not considered on duty for OSH committee activities.

of 0.019 indicates that the control group's performance on the compliance index slightly increased by between the baseline and second rounds of data collection.

Table IV: Short-run treatment effects: Sub-indexes of primary outcome index variables

Outcome variable	Control mean	Treatment effect	Robust std. err.	RI *p*	FDR *p*
	(1)	(2)	(3)	(4)	(5)
Panel A: OSH committee compliance index					
Formation sub-index	0.134	0.139*	0.070	0.040	0.053
Operations sub-index	0.214	0.100	0.095	0.288	0.110
Responsibilities sub-index	-0.147	0.383***	0.107	0.001	0.004
Panel B: Safety indicators sub-indexes					
Spotcheck sub-index	-0.000	0.228**	0.089	0.012	0.064
CAP completion sub-variable	0.310	0.047	0.083	0.598	1.000
Worker OSH committee awareness sub-index	0.060	0.202	0.159	0.173	0.713
Worker safety knowledge sub-index	0.477	-0.086	0.140	0.551	1.000
Senior manager awareness sub-index	0.086	0.075	0.237	0.805	1.000
Panel C: Workers' job satisfaction and mental well-being sub-indexes					
Job satisfaction sub-index	-0.130	-0.386**	0.157	0.017	0.069
Mental well-being sub-index	0.011	-0.059	0.159	0.709	0.769
Turnover sub-variable	0.115	-0.010	0.063	0.884	0.769
Absenteeism sub-variable	0.088	-0.084	0.055	0.162	0.236

Notes: This table reports OLS estimates of treatment effects on sub-indexes of primary outcome index variables. Outcome variables are listed in each row. In all cases, higher values of the index correspond to positive outcomes. Each row reports the estimated ITT effect from a separate regression. All regressions include 80 observations. All regressions include stratification variables. With the exception of the spotcheck index, all regressions also include a control for baseline value of the dependent variable. Robust standard errors are reported in column (3). RI *p*-values based on 5000 draws are reported in column (4). *p*-values adjusted to control the FDR across each primary outcome's sub-indexes are reported in column (5).

Panel A of Table IV displays the results for the formation, operations, and responsibilities sub-indexes. While treatment factories outperform control factories on all sub-indexes, the largest treatment effect is on the responsibilities sub-index. Treatment factories outperform control factories on this sub-index by 0.38 sds (FDR p=0.004). The large, positive effect on this sub-index reflects that treatment factories are more likely to conduct legally-required activities. For example, at the second visit, only 15% of control OSH committees had conducted a risk assessment, while 56% of treatment OSH committees had conducted at least one. According to reports by presidents, worker reps, and senior managers, treatment OSH committees also made more regular reports and recommendations to senior management and followed up on these reports more often. Consistent with these effects on objective measures, column (1) of OA Table B.III shows that the intervention improves workers' perception of OSH committees' compliance and effectiveness by about 0.20 sds (p-val=0.112), as measured using a pre-specified index.[38]

Before examining the consequences of stronger OSH committees for workers, it's worth underscoring that, as illustrated by the aforementioned result on risk assessment, the Alliance's Program does not result in suppliers' full compliance with Bangladesh's OSH committee mandate.[39] This suggests that we may not expect MNCs' enforcement interventions to achieve suppliers' full compliance.

4.2 Strengthened OSH committees & workers' outcomes

Having established that the MNCs' intervention significantly increases compliance with the OSH committee law, I next examine the consequences of strengthened OSH committees for workers' outcomes. I begin by analyzing OSH committees' effects on workers' health and safety. I then turn to workers' job satisfaction and well-being, followed by their wages, employment, and productivity.

Health and Safety: Beginning with my most objective OSH measure, the center figure in Panel A of Figure III shows that the treatment and control groups performed similarly on the safety indicators index at baseline, but that the treatment group significantly outperforms the control group once their OSH committees are strengthened. This result provides causal evidence that OSH committees can help to improve safety in factories in a developing country. Columns (3)-(4) of Table III show that the effect is about a 0.14-0.15 sd improvement in indicators of safety (with PDS lasso-selected controls, FDR p=0.053). The table also shows that the control group improves on safety by about 0.11 sds between the baseline and short-run measures, indicating a positive secular trend.

Panel B of Table IV presents the results for each sub-index; row (1) shows that stronger OSH committees improve factories' performance on the OSH checklist evaluated by the research team by 0.23 sds (FDR p=0.064). OA Table B.XI shows the treatment effects on

[38]OA Table B.II presents baseline balance tests for worker secondary outcome variables.

[39]This may be related to imperfect monitoring by the Alliance. For example, of the 36 treatment factories that participated in the Program before the second data collection visit, 13's OSH committees had not conducted a risk assessment by this visit. In a couple of these cases, the research team determined that the factory falsified the record, and in a few others, that managers had conducted risk assessments, but not the OSH committee. In all 13, the Alliance's records show that the committee had conducted a risk assessment.

each item of the checklist.[40] Treatment factories outperform controls on nearly every item. For example, workers are 9-18% more likely to be found using machines with appropriate guards and to be wearing required personal protective equipment (PPE) for their tasks.[41] Increased PPE use is arguably important for injury reduction, as a full 69.2% of non-missing injuries in the injury records (63.2% including missing injuries) refer to injuries to hand(s), finger(s)/thumb(s), palm(s), finger nail(s), or are needle prick(s). This does not include injuries that are likely to the hand but are not explicitly recorded as being to the hand, such as injuries recorded as being cuts from knives or scissors, or injuries to the arm. Although none of the individual differences in the index is statistically significant, when aggregated, they indicate that strengthening OSH committees has a small, positive effect on physical indicators of factory safety. This improvement is consistent with the substantial increase in treatment OSH committees' implementation of risk assessment.

Returning to Panel B of Table IV, row (2) shows that stronger OSH committees do not increase factories' progress on completing their Alliance CAPs for building safety. There are two likely reasons why. First, the pending remediation items often required significant financial investments; even if stronger OSH committees are capable of pushing for these investments, this effect may require more time to materialize. Second, 25% of sample factories had completed 90% or more of required remediation by baseline.

Finally, stronger OSH committees do not significantly affect workers' awareness or OSH knowledge (Panel B, rows 3-4). Workers' awareness of OSH committees increases compared to controls', but the effect is noisy.[42] This result may be related to the fact that all sample factories participated in the Alliance's Fire Safety and Worker Helpline Training Program, which included information about the factory's OSH committee. At baseline, 81% of workers reported being aware of OSH committees' role and responsibilities, and 89% knew that their factory had an OSH committee. As OA Table B.X shows, though, even with this very high awareness, some measures of workers' awareness do improve.

Visitors to medical clinics: Departing from my primary outcomes, I examine effects on workers' safety and health measured using the medical clinic visitor logs. I argue the logs are more subject to reporting effects compared to the observed safety indicators index, but are arguably less subject to them compared to factories' official injury records. As discussed in Section 3.2.1, these results should be interpreted as suggestive, as they are only available for 62 factories. Panel A of Table V presents the results. Column (1), which

[40] Four OSH checklist variables drop from the analysis because all factories were found to comply.

[41] PPE includes equipment such as eye guards, finger guards, gloves, goggles, and boots.

[42] This result should be interpreted with caution due to a baseline imbalance on worker awareness of OSH Committees (OA Table B.VII). Workers' awareness at treatment factories is lower, although this difference lessens and is not significant at the 5% level when the outlier factory on worker outcomes is dropped.

presents weighted results, shows that on average, 1.1% of the workforce visited the medical clinic per day in control factories. In treatment factories, however, the proportion of the workforce visiting the clinic is 16.4% lower (p=0.066). In column (2), the unweighted estimate is very similar, a 15% decrease (p=0.095). While the information on the causes of workers' visits to the medical clinic are not comprehensive enough for me to conduct quantitative analysis on, the causes that I can observe include occupational diseases and minor to more serious injuries (e.g., crushing-related injuries, bone fractures, chemical burns). In sum, there is suggestive evidence that stronger OSH committees reduce the workforce's need for medical attention by between 15-16%, net of possible increases in visitors if committees increase workers' willingness to report injuries and diseases.[43]

Table V: Short-run treatment effects: Visitors to Medical Clinic & Officially-recorded Injuries

	Mean prop. workforce visits medical clinic (daily)		Mean monthly prop. injured	Mean monthly prop. injured- Major injuries	Mean monthly prop. injured- Minor injuries
	(1)	(2)	(3)	(4)	(5)
Treatment	-0.0018*	-0.0018*	0.0005	-0.0002	0.0007
	(0.0010)	(0.0010)	(0.0006)	(0.0002)	(0.0006)
	[0.102]	[0.109]	[0.370]	[0.482]	[0.191]
Control Mean	0.011	0.012	0.002	0.000	0.002
Observations	259	259	309	303	303
Factories	62	62	66	66	66
Stratification variables	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y	Y	Y
Weighted regression	Y	N	N	N	N

Notes: This table reports OLS estimates of treatment effects on the proportion of employees that visit the medical clinic on a daily basis and on the proportion of the workforce with officially-recorded injuries each month. Each column in the table reports the estimated coefficient from a separate regression. The dependent variable in each column is regressed on the treatment indicator, stratification variables, and a control for the baseline value of the dependent variable. Standard errors clustered at the factory level are reported in round brackets. RI p-values based on 5000 draws are reported in square brackets. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Officially-recorded injuries: Finally, I turn to the injury records, which I identify as being the most subject to reporting effects. Panel A of Table V presents the results. Column (3) shows that on average, 0.2% of the workforce is reported to be injured each month in control factories. The estimated effect on reported injuries is positive, a 25% increase, but

[43]In principle, one may be concerned that the reduction in visitors to the medical clinic could be driven by workers in treatment factories, who are more likely to be wearing PPE, being less likely to visit the clinic when injured due to fear of retaliation for not wearing or using PPE properly. The evidence do not support this possibility. After their direct supervisor, onsite medical clinics are the most common way that workers indicate they would report an injury (54.5% of workers). Strengthening the OSH committee has no effect on the share of workers who indicate that they would report an injury to the medical clinic.

is not statistically significant. In columns (4) and (5), I separate major and minor injuries; I expect reporting of minor injuries to be more subject to reporting effects. Consistent with this hypothesis, the estimate for major injuries is negative. In contrast, that for minor injuries is positive and larger in magnitude, consistent with reporting of minor injuries being more responsive to strengthening OSH committees.

In sum, the evidence indicates that strengthening OSH committees contributes to improving workers' health and safety as well as to increasing reporting of accidents and injuries. I find that my most objective measure of health and safety unambiguously improves. My second, less objective measure, workers seeking medical care at onsite clinics, also improves on net. Finally, my measure that is most subject to reporting effects, officially recorded injuries, increases insignificantly, with treatment effect patterns for major and minor injuries that are consistent with increased reporting.

Job Satisfaction and Well-being: Returning to my primary outcomes, the third figure in Panel A of Figure III shows that treatment factories perform somewhat worse on the job satisfaction and mental well-being index at baseline, although the difference is not statistically significant. As discussed in subsection 3.2.3, this gap is largely due a treatment factory that is a negative outlier on this index, and the results are robust to this factory's exclusion (see the Supplementary Materials). The figure also shows that in the short-run, the performance gap on the index between the treatment and the control factories becomes more negative and statistically significant. Column (6) of Table III shows that with PDS lasso-selected controls, the effect is a 0.14 sd decline (FDR p=0.083).

Panel C of Table IV displays the estimated effects for the sub-indexes/variables. It reveals that the negative effect on the primary index is driven by the self-reported job satisfaction sub-index (-0.39 sd effect, FDR p=0.069). The estimated effects on mental well-being, turnover, and absenteeism are all small and are not statistically significant.[44] Consistent with the null effect for turnover, the intervention does not affect workforce composition (OA Table B.X). Together, these results rule out the possibility that changes in workforce composition drive the negative effect on self-reported job satisfaction. The effect is unchanged when the outlier factory is dropped. I examine possible mechanisms underlying the decline in self-reported measures of job satisfaction at treatment factories in Sections 4.3 and 4.4.

[44] For inclusion in the index, the absenteeism and turnover sub-variables are constructed by collapsing five pre- and post-intervention monthly observations into one pre- and post-observation, respectively. They are then multiplied by -1 in order to be unidirectional with other outcomes.

Table VI: Short-run treatment effects: Business competitiveness outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Log(Labor Productivity)					
Treatment	0.101	0.077*	0.101	0.040	0.043	0.046
	(0.063)	(0.045)	(0.064)	(0.037)	(0.034)	(0.038)
				{0.272}		{0.216}
	[0.088]	[0.205]	[0.094]	[0.361]	[0.319]	[0.321]
Control Mean	0.767	0.767	0.767	0.749	0.749	0.749
Factories	77	77	77	76	76	76
Observations	378	378	378	380	380	380
Stratification variables	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	Y	N	Y	Y	N
Product FE	N	Y	N	N	Y	N
PDS Lasso Selected Controls	N	N	Y	N	N	Y
Drop partial shutdown	N	N	N	Y	Y	Y
Panel B	Log(Gross wages)		Log(Employment)			
Treatment	-0.015	-0.004	-0.011	-0.008		
	(0.029)	(0.028)	(0.021)	(0.022)		
	{0.450}	{0.548}	{0.450}	{0.548}		
	[0.612]	[0.913]	[0.655]	[0.742]		
Control Mean	15.865	15.865	6.665	6.665		
Factories	72	72	80	80		
Observations	360	360	400	400		
Stratification variables	Y	Y	Y	Y		
Control, baseline dep. var.	Y	N	Y	N		
PDS Lasso Selected Controls	N	Y	N	Y		

Notes: This table reports OLS estimates of treatment effects on labor productivity, employment, and gross wages. Outcome variables are listed at the top of each column. Each column reports the estimated ITT effect from a separate regression. Panel A reports results for labor productivity. In columns (1)-(3), the sample is trimmed at the 1st and 99th percentile of all factory-month labor productivity observations. Labor productivity is measured as the log of the physical quantity of output per person-hour. Person-hours are calculated as number of workers times the average weekly working hours times 4 weeks per month plus the number of management-level employees times average weekly working hours for managers times 4 weeks per month. In columns (4)-(6), a factory in the control group that partially shut down during the study is dropped. In Panel B, each regression includes five post-treatment observations per factory, where each observation is one month. The regression sample changes across columns due to differential data availability. Standard errors clustered at the factory level are reported in round brackets. p-values adjusted to control the FDR across primary outcomes are reported in curly brackets. RI p-values based on 5000 draws are reported in square brackets. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Wages and Employment: The first and second figures in Panel B of Figure III present coefficients from event study analyses for wages and employment estimated using equation 3. 90% CIs calculated using cluster robust standard errors are reported. The figures show that the difference between treatment and control groups for wages and employment, respectively, is largely stable and statistically insignificant over the course of the experiment. In both cases, one of twelve coefficients is statistically different from zero at the 90% level, no more than would be expected by chance.

Panel B of Table VI presents the effects on gross wages and on employment estimated using equation 1.[45] Columns (1)-(2) show that the estimated effect on wages is a -0.40 to -1.5% decrease (not statistically significant). Columns (3)-(4) show that the estimated effect on employment is a -0.80 to -1.1% decrease (not statistically significant). Taken together, the evidence suggests that stronger OSH committees improve safety without adversely affecting wages or employment. Further, there is no effect on other non-pecuniary amenities (OA Table B.III column 5). These results are consistent with OSH committees imposing only small costs on suppliers. Given the null results on wages and employment, OA Table B.XIV reports the ex post MDEs for them that would be detectable under standard assumptions for power calculations (80% power and 5% statistical significance level).

Labor Productivity: The third figure in Panel B of Figure III reports coefficients from the event study for labor productivity. While noisy, the estimates support the interpretation that the intervention did not significantly affect labor productivity. The jump up in treatment factories' productivity visible in month 1 is due to a control factory that partially suspended production for three months starting in this month; while this type of temporary suspension is part of business, so it does not mean that this factory should be removed, due to the timing of the partial shut down and my smaller sample size, the labor productivity results are sensitive to its inclusion.[46] For this reason, Panel A of Table VI presents results with and without this factory, estimated using equation 1. Columns (1)-(3) present results for the full sample trimming the 1st and 99th percentiles of observations. The estimates range between 7.7-10.1% and are marginally statistically significant. Columns (4)-(6) present results dropping the partial shutdown factory. The estimated effects are much smaller, between 4.0-4.6% increases, and are not statistically significant.[47]

While imprecise, the 95% confidence intervals for these estimates are mostly positive:

[45] OA Table B.XIII reports results for wages, employment, and labor productivity using panel regression.

[46] The factory does not experience dramatic drops in employment or gross wages during the 3-month period. The main results are very similar when this factory is dropped; see the Supplementary Materials.

[47] OA Table B.V shows that there are not short-run effects on quantity of output, working hours, or output quality (defects per 100 units). In the longer-run, working hours at treatment factories decrease slightly.

With 95% confidence, I can rule out effects more negative than -1.3 to -2.5% with the full sample and -2.4 to 3.4% when dropping the factory that shuts down. The results support the interpretation that OSH committees do not negatively affect labor productivity; it is much more likely that their effects are zero or positive. In light of the null results when dropping the factory that shuts down, OA Table B.XIV reports the ex post MDEs.

Finally, the export records allow me to observe average unit prices for the 53 direct exporters in my sample. While the null effects on wages, employment, and labor productivity allay concerns that OSH committees affect factories' labor costs, it is possible that they impose other costs that require suppliers to raise prices. Panel C of OA Table B.XV provides evidence against this possibility. There is no effect on prices, and the estimates are actually negative for most lags, including the preferred 3-month lag in column 3.

4.3 Mechanisms

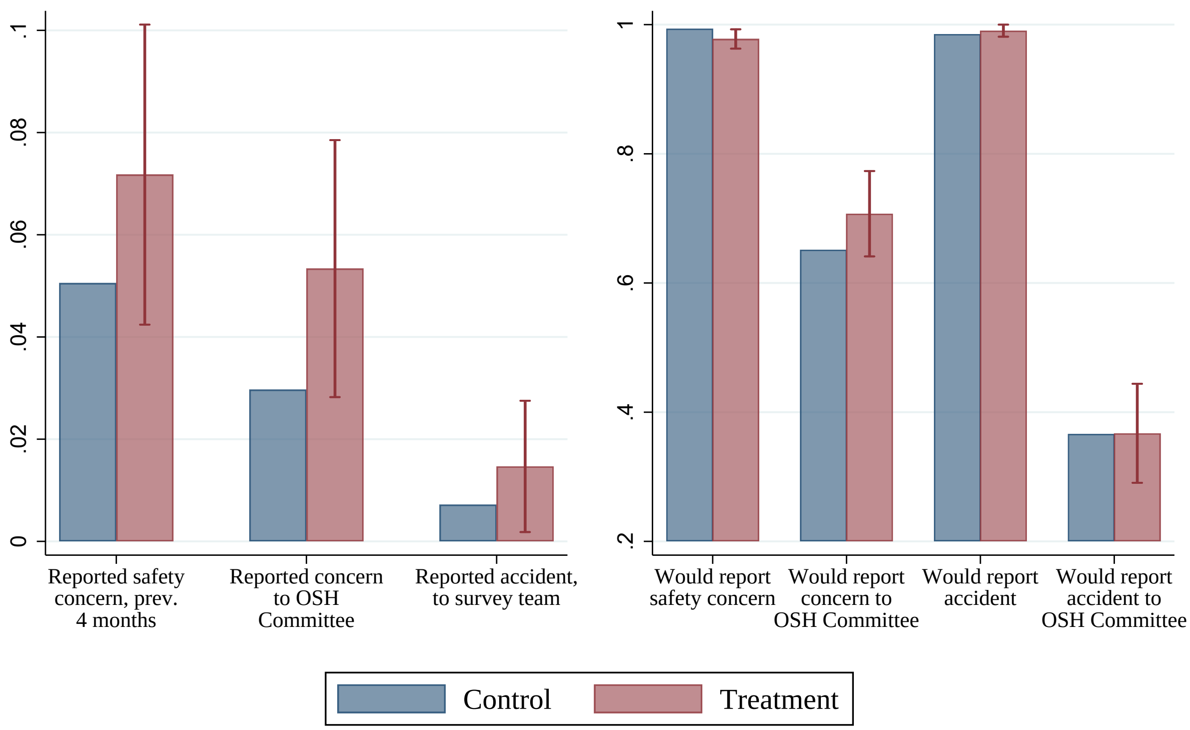
I focus my investigation of mechanisms along two lines of inquiry. First, I explore channels suggested by the theoretical literature on worker voice institutions. Second, I consider two alternatives, which are managerial and OSH training for committee members and preparation of an action plan through the Alliance OSH Committee Program.

4.3.1 Voice-related mechanisms

Improved information flows: To explore the possibility that OSH committees improve information flows around OSH between workers and managers, I first test for effects on communication between representatives on the OSH committee. I measure communication using the number of meetings per three months and the word count of meeting minutes. OA Table B.XVI reports the results. Columns (1)-(2) show that treatment OSH committees hold on average 0.7 more meetings per three months compared to control committees, which meet on average 1.27 times per three months, a 54% increase. Columns (3)-(4) reveal that despite their more frequent meetings, treatment OSH committees' minutes are 23% longer (p=0.103). Given that control committees' meeting minutes are, on average, less than half of a page of text, longer meeting minutes suggest more substantive discussions. It is worth noting, however, that I do not find evidence that worker reps' participation in the meetings increases, as self-reported in surveys, which suggests that much of the additional discussion may be driven by management (columns (5)-(6)).

To gain insight into how the intervention influences the *content* of communication between worker and manager representatives to the OSH committee, I examine the meeting minutes' text. I prepare it using standard methods (Gentzkow, Kelly and Taddy, 2019). I represent discussion content in the form of two-word phrases, known as bi-grams. OA Figure B.I presents the top 15 bi-grams for treatment and control groups before and after OSH committees are strengthened. Treatment OSH committees' shift toward more substantive discussions are reflected in their count distribution's outward shift compared to controls'. Further, more conceptual terms, such as "fire safety," disappear from treatment committees' top bi-grams and are replaced with more action-oriented terms, such as "risk assessment," "discussion decision," and "unanimous consent." In contrast, control committees' minutes remain primarily conceptual. This suggests that treatment committees' discussions are more likely to result in taking action on OSH issues compared to controls'.

Figure IV: Short-run treatment effects: Worker reporting of OSH-related issues



Notes: For each variable, the blue bar is the control group mean in the short-run round of data collection. The red bar is the control group mean plus the estimated coefficient on the treatment indicator from a regression of the variable on the treatment indicator, strata fixed effects, and a control for the baseline value of the variable. Whiskers show the 90% confidence interval from this regression using robust standard errors.

Turning to other measures of information flows, I consider OSH-related reporting by rank-and-file workers, as measured in the worker survey. Figure IV presents the results, which are also reported in OA Table B.XVII. The Figure's left panel shows large, although imprecisely estimated, increases in workers' reporting of safety concerns in their factory, including through the OSH committee. Workers are also 75% more likely to report to the research team that they have experienced an accident at work. Workers' increased willingness to report their experience of accidents is consistent with the increase in officially-recorded injuries from Table V. Figure IV's right panel shows that nearly all workers say that they *would* report a safety concern or an accident in their factory, if they were to have one. Workers at treatment factories are about 9% more likely to list the OSH committee as a channel that they would use to report a safety concern, although they are no more likely to list it as a channel to report an accident.

Other worker voice mechanisms: I examine whether stronger OSH committees increase cooperation around OSH between workers and managers using several survey measures of perceived cooperation with management by committee worker reps and presidents (OA Table B.XVI, columns (7)-(8)) and by rank-and-file workers (OA Table B.XVIII, columns (1)-(3)). I do not find strong evidence of increased cooperation as a key channel. Turning to coordination, I test whether workers at treatment factories report that workers and managers discuss and make plans together to improve safety more than those at control factories. OA Table B.XVIII column (4) shows that workers at treatment factories report slightly more coordination, but the difference is not statistically significant. Finally, if stronger OSH committees increase workers' bargaining power over OSH, I would expect treatment workers to report that their OSH committees are more responsive to workers' concerns and have greater authority over their factories' safety policies compared to control workers. I do not find that this is the case (OA Table B.XVIII, columns (5)-(7)).

In sum, I find the most conclusive evidence in favor of increased information sharing as a key voice-related channel through which OSH committees improve workers' health and safety. There is not strong evidence in favor of other channels, although I return to them in my analysis of HTEs by factories' managerial capacity.

4.3.2 Managerial and OSH training and preparation of an action plan

The Alliance's OSH Committee Program included two days of Alliance-provided training on OSH and management skills for two worker reps and two management reps from the OSH committee. It also required OSH committees to prepare an action plan. It is plausible that one or both of these features improve OSH committees' effectiveness and drive the OSH improvements. I examined both possibilities and did not find evidence in favor of either. For this reason, I include these analyses in the Supplementary Materials.

4.4 HTEs by management practices

In Section 1, I argue that OSH compliance is in part a function of firms' capabilities, which the organizational economics literature identifies as relying on managerial practices that in turn rely on relational contracts (Gibbons and Henderson, 2011, Blader, Gartenberg and Prat, 2020). Firms in developing countries are lower productivity Hsieh and Klenow (2009), which suggests they have worse relational contracting capabilities (Powell, 2019) and is consistent with their adopting fewer management practices (Bloom et al., 2014). I hypothesize that lack of capacity to build relational contracts with workers may contribute to noncompliance with labor regulation (Boudreau, Cajal-Grossi and Macchiavello, 2023). Motivated by this possibility, in this section, I explore heterogeneity in the intervention's effects depending on factories' baseline management practices.[48]

My main measure of management practices summarizes senior and lower-level managers' reported frequency of holding production-related meetings with workers.[49] This is a variant of questions asked in the World Management Survey (WMS) and in studies on managerial practices by Bloom et al. (2013) and Macchiavello et al. (2020). It measures one specific practice, as it was not feasible to conduct a complete management diagnostic. I argue that it is relevant for two reasons. First, it may especially reflect firms' relational contracting capabilities, as regular communication between managers and workers is required to build the trust and clarity that sustain relationships. Second, there is reason to believe it serves well as a proxy for adoption of overall management practices; OA Figure B.II shows that apparel firms' score on the WMS's meeting question is highly correlated with their average overall WMS Index excluding this question.

I partition the sample using the median baseline meeting practice score. I refer to the below median group as poorly-managed and to the above median group as better-managed. OA Table B.VII shows baseline balance within each interaction-term subgroup for primary outcomes for non-attrited factories. There are no statistically significant differences. Table VII presents the results for primary outcomes, estimated using equation 2. Each column considers a different outcome. The first row displays the estimated effect for the below median group, the second row displays the effect for the above median group, and the final row displays the *p*-value of the difference in the effects between the groups.

[48]My PAP included three other dimensions of heterogeneity to explore: Factory size, compliance with the OSH committee regulation, and location in an EPZ. I find the most compelling pattern of HTEs for managerial practices, so I present the results for the other dimensions of heterogeneity in OA C. For space reasons, I omit HTE analysis for these dimensions for business competitiveness variables. For location in an EPZ, as there are large ex ante differences between the seven treatment and the seven control factories in EPZs (OA Table C.II). As such, I depart from the PAP and do not analyze this dimension.

[49]The measure places 25% weight on the factory's most senior manager's report and 75% weight on the lower-level managers' reports. On average, 15 lower-level managers were surveyed.

Table VII: HTEs by managerial practices

	Compliance index	Safety indicators	Job satisfaction & Mental well-being	Log(Labor Productivity)	Log(Wages)	Log(Employment)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Short-run effects						
Below median	0.115	0.052	-0.271**	-0.007	-0.015	-0.010
	(0.079)	(0.084)	(0.125)	(0.045)	(0.037)	(0.031)
	[0.180]	[0.557]	[0.037]	[0.831]	[0.455]	[0.433]
Above median	0.417***	0.235**	-0.030	0.097	-0.018	-0.007
	(0.086)	(0.102)	(0.107)	(0.059)	(0.048)	(0.032)
	[0.000]	[0.046]	[0.763]	[0.001]	[0.470]	[0.694]
p-val, diff	0.011	0.170	0.162	0.181	0.965	0.940
	[0.032]	[0.226]	[0.162]	[0.022]	[0.936]	[0.873]
Control mean, below median	0.080	0.188	0.053	0.691	15.676	6.297
Control mean, above median	-0.029	0.047	-0.065	0.792	16.044	6.953
Factories	80	80	80	76	72	80
Observations	80	80	80	380	360	400
Panel B: Longer-run effects						
Below median	0.142	0.050	0.008	-0.059	-0.004	-0.009
	(0.127)	(0.121)	(0.134)	(0.054)	(0.042)	(0.038)
	[0.298]	[0.692]	[0.957]	[0.455]	[0.927]	[0.785]
Above median	0.318***	0.098	0.239*	0.020	-0.015	0.023
	(0.098)	(0.096)	(0.127)	(0.050)	(0.052)	(0.048)
	[0.004]	[0.328]	[0.073]	[0.750]	[0.772]	[0.627]
p-val, diff	0.266	0.761	0.225	0.282	0.865	0.615
	[0.318]	[0.771]	[0.226]	[0.445]	[0.865]	[0.582]
Control mean, below median	0.178	0.213	-0.047	0.743	15.668	6.298
Control mean, above median	0.101	0.107	-0.140	0.865	16.053	6.961
Factories	80	80	80	76	72	80
Observations	80	80	80	228	216	240
Panel C: Pooled effects						
Below median	0.128*	0.051	-0.132	-0.027	-0.011	-0.010
	(0.076)	(0.072)	(0.092)	(0.040)	(0.036)	(0.033)
	[0.109]	[0.480]	[0.160]	[0.445]	[0.588]	[0.463]
Above median	0.367***	0.167**	0.104	0.068	-0.017	0.004
	(0.063)	(0.068)	(0.091)	(0.049)	(0.042)	(0.037)
	[0.000]	[0.022]	[0.237]	[0.023]	[0.489]	[0.817]
p-val, diff	0.015	0.253	0.075	0.153	0.915	0.782
	[0.028]	[0.278]	[0.066]	[0.039]	[0.847]	[0.535]
Control mean, below median	0.129	0.200	0.003	0.710	15.673	6.298
Control mean, above median	0.036	0.077	-0.103	0.819	16.047	6.956
Factories	80	80	80	76	72	80
Observations	160	160	160	608	576	640
Stratification variables	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y	Y	Y	Y

Notes: This table reports OLS estimates of heterogeneous treatment effects on primary outcome variables. Each outcome variable is indicated at the top of the table. Panel A reports the short-run effects, Panel B reports the longer-run effects, and Panel C presents the pooled effects. In each panel, the below median row reports the estimated treatment effect for the subgroup with below median baseline managerial practices. In each panel, the above median row reports the estimated treatment effect for the subgroup with above median baseline managerial practices. The final two rows in each panel report the p-value of the difference between the estimated treatment effects for below and above median subgroups. RI p-values based on 5000 draws are reported in square brackets. All regressions include stratification variables and a control for the baseline value of the dependent variable. In column (4), product fixed effects are included, and a factory in the control group that partially shut down during the study is dropped. In columns (1)-(3), robust standard errors are reported in round brackets. In columns (4)-(6), standard errors clustered at the factory level are reported in round brackets. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table VIII: HTEs & longer-run treatment effects: Visitors to medical clinics & officially-recorded injuries

	Mean prop. workforce visits medical clinic (daily)		Mean monthly prop. injured	Mean monthly prop. injured-Major injuries	Mean monthly prop. injured-Minor injuries
	(1)	(2)	(3)	(4)	(5)
Panel A: HTEs by managerial practices, short-run					
Below median	-0.0014	-0.0013	-0.0008	-0.0005	-0.0004
	(0.0011)	(0.0014)	(0.0006)	(0.0004)	(0.0005)
	[0.417]	[0.486]	[0.241]	[0.115]	[0.550]
Above median	-0.0024	-0.0022*	0.0018*	0.0001	0.0019*
	(0.0015)	(0.0013)	(0.0010)	(0.0002)	(0.0010)
	[0.175]	[0.133]	[0.047]	[0.502]	[0.031]
p-val, diff	0.541	0.641	0.033	0.173	0.060
	[0.674]	[0.727]	[0.029]	[0.123]	[0.046]
Control mean, below median	0.010	0.014	0.002	0.001	0.002
Control mean, above median	0.011	0.011	0.002	0.000	0.002
Observations	259	259	309	303	303
Factories	62	62	66	66	66
Panel B: HTEs by managerial practices, longer-run					
Below median	0.0008	0.0016	0.0003	0.0001	0.0001
	(0.0011)	(0.0011)	(0.0006)	(0.0002)	(0.0005)
	[0.577]	[0.228]	[0.688]	[0.798]	[0.922]
Above median	-0.0025	-0.0036**	0.0011	0.0000	0.0012
	(0.0015)	(0.0015)	(0.0009)	(0.0001)	(0.0008)
	[0.242]	[0.114]	[0.243]	[0.736]	[0.169]
p-val, diff	0.119	0.006	0.442	0.949	0.254
	[0.206]	[0.046]	[0.509]	[0.953]	[0.328]
Control mean, below median	0.012	0.013	0.002	0.000	0.001
Control mean, above median	0.014	0.015	0.002	0.000	0.002
Observations	147	147	189	186	186
Factories	52	52	65	64	64
Panel C: HTEs by managerial practices, pooled					
Below median	-0.0007	-0.0003	-0.0003	-0.0003	-0.0002
	(0.0009)	(0.0011)	(0.0005)	(0.0003)	(0.0005)
	[0.559]	[0.856]	[0.598]	[0.427]	[0.775]
Above median	-0.0026*	-0.0028**	0.0016*	0.0001	0.0016**
	(0.0014)	(0.0013)	(0.0008)	(0.0001)	(0.0008)
	[0.098]	[0.078]	[0.063]	[0.512]	[0.036]
p-val, diff	0.238	0.119	0.054	0.275	0.058
	[0.413]	[0.283]	[0.080]	[0.281]	[0.085]
Control mean, below median	0.011	0.014	0.002	0.000	0.002
Control mean, above median	0.012	0.012	0.002	0.000	0.002
Observations	406	406	498	489	489
Factories	62	62	66	66	66
Panel D: Main treatment effects, longer-run					
Treatment	-0.0008	-0.0010	0.0007	0.0001	0.0006
	(0.0008)	(0.0011)	(0.0005)	(0.0001)	(0.0005)
	[0.519]	[0.444]	[0.185]	[0.599]	[0.216]
Control Mean	0.013	0.014	0.002	0.000	0.002
Observations	147	147	189	186	186
Factories	52	52	65	64	64
Stratification variables	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y	Y	Y
Weighted regression	Y	N	N	N	N

Notes: This table reports OLS estimates of the longer-run effects on the proportion of employees that visit the medical clinic on a daily basis and on the proportion of the workforce with officially-recorded injuries each month (Panel A). It also reports heterogeneous treatment effects by managerial practices on these outcomes. Each column in the table reports the estimated coefficient from a separate regression. The dependent variable in each column is regressed on the treatment indicator, stratification variables, and a control for the baseline value of the dependent variable. Standard errors clustered at the factory level are reported in round brackets. RI *p*-values based on 5000 draws are reported in square brackets. *p¡0.1; **p¡0.05; ***p¡0.01.

Column (1) of Panel A shows that the effect on compliance depends on factories' management practices: The intervention increases compliance among poorly-managed factories by 0.12 sds and among better-managed factories by 0.42 sds (p-value of difference=0.011). Consistent with the limited increase in compliance among poorly-managed factories, the effect on safety indicators for this group is small. In contrast, OSH indicators improve by 0.24 sds for better-managed factories (p-value of difference=0.170). Together, the results suggest that management matters for the intervention's effects on factories' compliance, and in turn, only better-managed factories' OSH committees are strengthened and meaningfully improve OSH indicators.

Turning to column (3), the effect on workers' job satisfaction and mental well-being for poorly-managed factories is -0.27 sds (p=0.034), while that for better-managed factories is close to zero (p-value of difference=0.162). Evidently, the decline in self-reported job satisfaction is driven by poorly-managed factories, for which the intervention does not meaningfully improve compliance or OSH. I discuss this result in more detail below.

I can try to increase statistical power to detect differences between the effects for each group by pooling both post-treatment rounds of data. Panel C displays the results. Column (1) shows that the effects on compliance remain stable and are statistically different (p=0.015). Column (2) shows that the effects on safety indicators, although attenuated, exhibit the same pattern. I am unable, through, to reject that the effects for both groups are equal (p=0.253). Finally, in column (3), the difference in effects is fairly stable, and I reject the null of equality (p=0.075). In sum, while not conclusive for the safety indicators index, the results provide support for the MNCs' enforcement primarily strengthening committees, and consequently, OSH outcomes, in better-managed factories.

Turning to wages, employment, and labor productivity, I do not find evidence of heterogeneous effects on these outcomes (columns (4)-(6)). This allays concerns that the null average effects mask negative effects for better-managed factories. In Table VII, I include my preferred specification for labor productivity, which drops the control factory that shuts down for part of the sample period and includes product fixed effects.

Consistent with OSH committees only being strengthened at better-managed factories, Panels A-C of Table VIII presents suggestive evidence that visitors to medical clinics decline more among this group compared to less well-managed factories (columns (1)-(2)). Further, officially-recorded injuries only increase among better-managed factories, driven by reporting of minor injuries (columns (3)-(6)). While this evidence should be interpreted as suggestive due to my smaller sample size, the results accord with the pattern of HTEs on my other outcomes, which are measured using different sources.

Finally, OA Tables B.XVII and B.XVIII provide evidence that voice-related channels are key mechanisms underlying these results. Beginning with OSH-related reporting, column (1) of OA Table B.XVII shows that workers at better-managed factories are nearly twice as likely to have reported a safety concern at their factory and are more likely to have reported a concern through the OSH committee (col. 2). Further, OA Table B.XVIII provides some evidence in favor of cooperation, coordination, and bargaining power mechanisms at better-managed factories. In contrast, there is not evidence that the Alliance's managerial and OSH training is improving OSH committees' capacity more at better-managed compared to poorly-managed factories (see Supplementary Materials).

I present robustness checks that support these HTEs by management practices in OA C. Specifically, I show that correlation between factories' management practices and other characteristics does not drive the results. I allay concerns that differential monitoring may generate the heterogeneity. Finally, I implement the analysis using an alternative measure of management practices and find qualitatively similar results.

Why do indicators of job satisfaction decline at poorly-managed treatment factories? Unfortunately, my data are not well-suited to answer this question. It is interesting to note, however, that OA Table B.XVIII shows negative but statistically insignificant effects for workers at poorly-managed factories on all measures of cooperation, coordination, and bargaining power over OSH. OA Table B.XVII column (7) also shows a marginally statistically significant negative effect on workers' willingness to report accidents to the OSH committee. I interpret this as suggestive evidence that an unintended consequences of imposing an institution that relies on relational contract between the employer and workers in a setting where this contract does not exist may be that workers learn that their employer is unwilling or unable to build a relational contract. Ultimately, though, the impact of failed regulatory enforcement on worker outcomes is an area for future research.

4.5 Effects in the longer-run

The Alliance's OSH Committee Program aimed to bring factories into compliance through intensive enforcement for a period of 6 months. The Alliance then monitored factories under its general monitoring activities.[50] Do the effects on compliance and safety persist in the longer-run, even after the intensive monitoring phase ends?

The first figure in Panel A of Figure III shows that the treatment group remains more compliant than the controls by about 0.22 sds in the longer-run, 3-4 months after the end

[50]The Alliance conducted onsite audits during and after factories' participation in the Program. 10% of treatment factories were audited during the Program and 15% were audited in the six months afterward.

Table IX: Longer-run treatment effects, after end of MNCs' intensive enforcement

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	OSH committee compliance index		Safety indicators index		Job satisfaction and mental well-being index	
Treatment	0.234***	0.215***	0.081	0.080	0.114	0.066
	(0.083)	(0.081)	(0.074)	(0.073)	(0.091)	(0.086)
	{0.038}	{0.051}	{0.671}	{1.000}	{0.671}	{1.000}
	[0.005]	[0.013]	[0.293]	[0.297]	[0.210]	[0.458]
Control Mean	0.135	0.135	0.153	0.153	-0.099	-0.099
Observations	80	80	80	80	80	80
Stratification variables	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	N	Y	N	Y	N
PDS Lasso Controls	N	Y	N	Y	N	Y
Panel B	Log(Labor Productivity)					
Treatment	-0.029	-0.010	-0.001	-0.040	-0.022	-0.013
	(0.040)	(0.038)	(0.039)	(0.040)	(0.036)	(0.037)
				{0.671}		{1.000}
	[0.556]	[0.838]	[0.969]	[0.394]	[0.630]	[0.764]
Control Mean	0.821	0.821	0.821	0.813	0.813	0.813
Factories	75	75	75	76	76	76
Observations	215	215	215	228	228	228
Stratification variables	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	Y	N	Y	Y	N
Product FE	N	Y	N	N	Y	N
PDS Lasso Selected Controls	N	N	Y	N	N	Y
Drop partial shutdown	N	N	N	Y	Y	Y
Panel C	Log(Gross wages)		Log(Employment)			
Treatment	-0.009	-0.011	0.003	0.005		
	(0.031)	(0.034)	(0.029)	(0.029)		
	{0.929}	{1.000}	{0.929}	{1.000}		
	[0.799]	[0.757]	[0.934]	[0.881]		
Control Mean	15.866	15.866	6.670	6.670		
Factories	72	72	80	80		
Observations	216	216	240	240		
Stratification variables	Y	Y	Y	Y		
Control, baseline dep. var.	Y	N	Y	N		
PDS Lasso Selected Controls	N	Y	N	Y		

Notes: This table reports OLS estimates of the longer-run effects on primary outcomes, which are measured 3-4 months after the end of intensive enforcement by the MNCs. Outcome variables are listed at the top of each column. For index variables, higher values of the index correspond to positive outcomes. Each column reports the estimated ITT effect from a separate regression. In Panel A, robust standard errors are reported in round brackets. In Panels B and C, standard errors clustered at the factory level are reported in round brackets. In Panel B columns (1)-(3), the sample is trimmed at the 1st and 99th percentile of all factory-month labor productivity observations. In columns (4)-(6), a factory in the control that partially shut down during the study is dropped. Labor productivity is measured as the log of the physical quantity of output per person-hour. Person-hours are calculated as number of workers times the average weekly working hours times 4 weeks per month plus the number of management-level employees times average weekly working hours for managers times 4 weeks per month. p-values adjusted to control the FDR across primary outcomes are reported in curly brackets. RI p-values based on 5000 draws are reported in square brackets. $p^{*} < 0.1$; $p^{**} < 0.05$; $p^{***} < 0.01$.

of intensive enforcement. Columns (1)-(2) of Table IX show the same result.[51]

Do strengthened OSH committees continue to improve workers' health and safety after the intensive monitoring phase ends? The second figure in Panel A (Figure III) and columns (3)-(4) of Table IX show that the treatment group continues to outperform the controls by 0.08 sds. While the treatment effect is no longer statistically significant, it is also not significantly different from the short-run effect. As the control group means for compliance and safety in Table IX show, control factories improved their performance on the compliance and safety indexes by 0.14 and 0.15 sds, respectively, between baseline and the longer-run data collection round. This improvement may be a positive secular trend or may indicate that they were subject to spillover or anticipation effects, which would tend to shrink the treatment effect estimates.[52] The patterns of effects for visitors to medical clinics and injury records are similar to those in the short run analyses (Panel D of Table VIII). Columns (1)-(2) show that the effect for visitors to medical clinics continues to be negative but is no longer statistically significant, possibly due to the fact that 10 more factories drop compared to the short-run analysis.

Interestingly, the effect on job satisfaction and mental well-being turns positive, although it is insignificant (third figure, Panel A of Figure III). Panel B of Table VII shows that the effect for poorly-managed factories is zero, while that for better-managed factories is 0.24 sds (*p*-value of difference=0.225). For better-managed factories, the positive estimate is consistent with workers' valuing OSH committees, but it requiring time for them to learn about OSH committees' role or the benefits they provide.

Finally, I do not find delayed adverse effects on labor productivity, wages, or employment. In most specifications for all variables, the estimates are close to zero.

5 Conclusion

I exploit experimental variation arising from MNCs' enforcement of a recent Bangladeshi mandate for OSH committees to provide evidence of OSH committees' causal effects on workers and on factories in Bangladesh's apparel sector. In doing so, I add to our scarce understanding of how decentralization of decision-making authority through increased worker involvement in firms' decision-making affects workers' and firms' outcomes, in

[51] OA Table B.XIX presents the longer-run results for primary index sub-index variables.

[52] Spillover effects among factories owned by multi-plant firms are plausible, as 69% of sample factories were in this category. Anticipation effects are plausible because the Alliance rolls out its programs in a staggered fashion, and managers generally knew that they would be required to participate. It's possible that control factories expected future enforcement by the Alliance and took actions to prepare. Indeed, the Alliance engaged the control factories in its OSH Committee Program after the end of the data collection.

this case in relation to OSH. I find that greater inclusion of workers through stronger OSH committees results in small improvements in workers' health and safety and may contribute to increased reporting of injuries. While my study's context and design are suited to examine OSH committees' effects on occupational injuries and diseases and physical safety indicators that are linked to fatality risk, there is reason to believe that OSH committees' effects may impact outcomes as extreme as fatality risk, both by changing workplace OSH practices and changing workplace OSH culture through improved information flows. That said, the extent to which OSH committees can mitigate the most serious threats to worker safety, such as large-scale industrial disasters, worker fatalities, and severe injuries remain important questions for future work. I also provide the first experimental evidence that MNCs' enforcement of local labor laws can improve suppliers' compliance in a context with a weak state enforcement.

Another important question for future work is the extent to which my findings can be generalized to other settings. One may be concerned that safety was especially salient in the aftermath of the Rana Plaza collapse and that MNC buyers may have had stronger incentives to enforce than in other countries that they source from. On the one hand, this suggests that the Alliance's intervention may have had more bite than it would in other settings. On the other, the fact that safety may have been more salient in my setting may have led the control group to adopt higher safety standards, reducing the scope for stronger OSH committees to have effects. This suggests that the effects of strengthening OSH committees would be smaller compared to other settings. Consequently, whether we would expect the Alliance's intervention to have larger or smaller effects compared to other settings is ambiguous.

That said, there are aspects of my context that support the relevance of my results for other settings. In particular, Bangladesh's apparel sector shares many features with other low-skill manufacturing sectors in low- and middle-income countries (ILO, 2016). Further, the apparel sector employs many millions of workers, especially women, in countries across Asia; the top 5 exporters are China, Bangladesh, Vietnam, India, and Turkey (World Trade Organization, 2017). Of these, as of 2015 (the most recent year data is available), all except China had legislation regulating OSH committees (ILO, 2015). Finally, the theoretically-motivated worker voice mechanisms that I examine are general in nature and are relevant to consider in any study of worker voice institutions.

Further, in terms of MNCs' enforcement activities, MNCs monitor social and environmental performance throughout their supply chains; in global value chains (GVCs) with Western MNC buyers, participation in social compliance enforcement programs is part of doing business for upstream firms (Locke, 2013).[53] Coalition-based approaches are also a common way of organizing enforcement in GVCs. The Life and Building Safety Initiative, for example, includes several former Alliance members and enforces building safety standards in the apparel and home good sectors in Cambodia, India, and Vietnam.

Another interesting question that arises from this study is what the implications of these findings are for the roles of MNCs and states in governance in GVCs. In contexts in which states are effective at enforcement, it is likely the state that enforces its laws. In weaker states, however, it may be MNCs or multi-stakeholder coalitions with MNCs' participation that provide enforcement. This research suggests that in such contexts, MNCs can contribute to increasing compliance with labor standards. A critical question that is beyond the scope of this study is what the general equilibrium effects of MNC enforcement of labor law are on compliance and competitiveness of the targeted sector; Alfaro-Ureña et al. (2021) takes a step forward by shedding light on these effects in the Costa Rican context. Another crucial question is the longer-term effects of MNC enforcement on state capacity in developing countries, which are a priori ambiguous.

References

Accord. 2013. "Accord on Fire and Building Safety in Bangladesh."

ACT. 2017. "Act on Living Wages Memorandum of Understanding."

Adhvaryu, Achyuta, Namrata Kala, and Anant Nyshadham. 2019. "Management and Shocks to Worker Productivity." *Working Paper*.

Adhvaryu, Achyuta, Namrata Kala, and Anant Nyshadham. 2020. "The Light and the Heat: Productivity Co-benefits of Energy-saving Technology." *Review of Economics and Statistics*, 102(4): 779–792.

Aghion, Philippe, and Jean Tirole. 1997. "Formal and Real Authority in Organizations." *Journal of Political Economy*, 105(1): 1–29.

Alfaro-Ureña, Alonso, Benjamin Faber, Cecile Gaubert, Isabela Manelici, and Jose P. Vasquez. 2021. "Responsible Sourcing? Theory and Evidence from Costa Rica." *Working Paper*.

Alliance. 2013. "Members Agreement."

[53] Alliance members sourced from and very likely monitored suppliers in other countries; for example, from 2015-2019, Gap Inc. conducted fewer than 10% of its supplier audits in Bangladesh (Amengual and Distelhorst, 2019).

Alliance. n.d.. "Safety Committees." *http://bangladeshworkersafety.org/what-we-do/worker-empowerment/sc*, Accessed: 2020-12-04.

Almunia, Miguel, Jonas Hjort, Justine Knebelmann, and Lin Tian. 2021. "Strategic or Confused Firms? Evidence from "Missing" Transactions in Uganda." *Working Paper*.

Amengual, Matt, and Greg Distelhorst. 2019. "Can Sourcing Help Enforce Global Labor Standards? Evidence from the Gap Inc Supply Chain." *Working Paper*.

Amodio, Francesco, and Nicolás de Roux. 2021. "Labor Market Power in Developing Countries: Evidence from Colombian Plants." *Working Paper*.

Anderson, Michael L. 2008. "Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American Statistical Association*, 103(484): 1481 –1495.

Athey, Susan, and Guido Imbens. 2016. "The Econometrics of Randomized Experiments."

Bandiera, Oriana, Michael Best, Adnan Khan, and Andrea Prat. 2021. "The Allocation of Authority in Organizations: A Field Experiment with Bureaucrats." *Quarterly Journal of Economics*, 136(4): 2195–2242.

Belloni, Alexandre, Victor Chernozhukov, and Christian Hansen. 2014. "Inference on Treatment Effects after Selection among High-Dimensional Contr." *Review of Economic Studies*, 81(2): 608–650.

Bertrand, Marianne, and Bruno Crépon. 2020. "Teaching Labor Laws: Evidence From a Randomized Control Trial in South Africa." *Working Paper*.

Blader, Steven, Claudine Gartenberg, and Andrea Prat. 2020. "The Contingent Effects of Management Practices." *Review of Economic Studies*, 87: 721–749.

Bloom, Nicholas, Benn Eifert, Aprajit Mahajan, David McKenzie, and John Roberts. 2013. "Does Management Matter? Evidence from India." *The Quarterly Journal of Economics*, 128(1): 1–51.

Bloom, Nicholas, Renata Lemos, Raffaella Sadun, Daniella Scur, and John Van Reenen. 2014. "The New Empirical Economics of Management." *Journal of the European Economic Association*, 12: 835–876.

Bolton, Patrick, and Mathias Dewatripont. 2013. "Authority in Organizations: A Survey." In *The Handbook of Organizational Economics*. , ed. Robert S. Gibbons and John Roberts, Chapter 9, 342–372. Princeton University Press.

Boudreau, Laura, Julia Cajal-Grossi, and Rocco Macchiavello. 2023. "Global Value Chains in Developing Countries: A Perspective from Coffee and Garments."

Brooks, Wyatt J., Joseph P. Kaboski, Yao Amber Li, and Wei Qian. 2021. "Exploitation of Labor? Classical Monopsony Power and Labor's Share." *Journal of Development Economics*, 150.

Bustillo, Miguel, Tom Wright, and Shelly Banjo. 2012. "For Wal-Mart, Sears, Tough Questions in Bangladesh Fire." *The Wall Street Journal*.

Casey, Katherine, Rachel Glennerster, and Edward Miguel. 2012. "Reshaping Institutions: Evidence on Aid Impacts Using a Preanalysis Plan." *Quarterly Journal of Economics*, 127: 1755–1812.

Central Intelligence Agency. 2016. "The World Factbook." Central Intelligence Agency.

Centre for Policy Dialogue. 2015. "Third Independent Monitoring Report: One Year after the Rana Plaza." Centre for Policy Dialogue.

Dean, Joshua T. 2021. "Noise, Cognitive Function, and Worker Productivity." *Working Paper*.

de Bakker, Frank, Andreas Rasche, and Stefano Ponte. 2019. "Multi-Stakeholder Initiatives On Sustainability: A Cross-Disciplinary Review And Research Agenda For Business Ethics." *Business Ethics Quarterly*, 29.

Deloitte. 2015. "Global Powers of Retailing 2015: Embracing Innovation." London, United Kingdom.

Distelhorst, Greg, Jens Hainmueller, and Richard M Locke. 2017. "Does Lean Improve Labor Standards? Management and Social Performance in the Nike Supply Chain." *Management Science*, 63(3): 707–728.

Fisman, Raymond, and Yongxiang Wang. 2015. "The Mortality Costs of Political Connections." *Review of Economic Studies*, 82: 1346–1382.

Freeman, Richard, and Edward Lazear. 1995. "An Economic Analysis of Works Councils." In *Works Councils: Consultation, Representation, and Cooperation in Industrial Relations*. , ed. Joel Rogers and Wolfgang Streeck, 27–52. Chicago:University of Chicago Press.

Freeman, Richard, and James Medoff. 1985. *What do Unions do?* N.Y. Basic Books.

Gentzkow, Matthew, Bryan T Kelly, and Matt Taddy. 2019. "Text as Data." *Journal of Economic Literature*, 57(3): 535–574.

Gibbons, Robert, and Rebecca Henderson. 2011. "Relational Contracts and Organizational Capabilities." *Organization Science*, 23(5): 1350–1364.

Giorcelli, Michela. 2019. "The Long-Term Effects of Management and Technology Transfers." *American Economic Review*, 109: 121–152.

Government of Bangladesh. 2015. "Bangladesh Gazette: Ministry of Labor and Employment Notification." *English translation*, S.R.O. No. 291.

Greenhouse, Steven. 2013*a*. "Obama to Suspend Trade Privileges with Bangladesh." *The New York Times*.

Greenhouse, Steven. 2013*b*. "U.S. Retailers See Big Risk in Safety Plan for Factories in Bangladesh." *The New York Times*.

Harju, Jarkko, SImon Jäger, and Benjamin Schoefer. 2021. "Voice at Work." *Working Paper*.

Harrison, Ann, and Jason Scorse. 2010. "Multinationals and anti-sweatshop activism." *The American Economic Review*, 100(1): 247–273.

Haushofer, Johannes, and Jeremy Shapiro. 2016. "The short-term impact of unconditional cash transfers to the poor: Experimental evidence from Kenya." *Quarterly Journal of Economics*, 1973–2042.

Hirschman, Albert. 1970. *Exit, Voice, and Loyalty: Responses to Decline in Firms, Organizations, and States.* Vol. 25, Harvard University Press.

Hsieh, Chang-Tai, and Peter J. Klenow. 2009. "Misallocation and Manufacturing TFP in China and India." *The Quarterly Journal of Economics*, 124(4): 1403–1448.

ILO. 1981. "Occupational Safety and Health Convention, 1981 (No. 155)." https://www.ilo.org/dyn/normlex/en/f?p=NORMLEXPUB:12100:::NO:12100:P12100_ILO_CODE:C155:NO.

ILO. 2015. "Joint OSH Committees: Join in building a culture of prevention on OSH." International Labor Organization.

ILO. 2016. "Sectoral Studies on Decent Work in Global Supply Chains." International Labor Organization.

ILO. 2017. "Towards safer working conditions in the Bangladesh ready-made garment sector." International Labor Organization.

ILO. 2019*a*. "Safety and Health at the Heart of the Future of Work." International Labor Organization, Geneva, Switzerland.

ILO. 2019*b*. "World Employment Social Outlook Trends 2019." International Labor Organization.

ILO. 2021. "Safety and health in textiles, clothing, leather and footwear." International Labor Organization, Geneva, Switzerland.

Jäger, SImon, Benjamin Schoefer, and Jörg Heining. 2021. "Labor in the Boardroom." *Quarterly Journal of Economics*, 136(2): 669–725.

Kala, Namrata. 2023. "The Impacts of Managerial Autonomy on Firm Outcomes." Working Paper.

Lee, David S. 2009. "Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects." *Review of Economic Studies*, 76(3): 1071–1102.

Locke, Richard M. 2013. *The Promise and Limits of Private Power: Promoting Labor Standards in a Global Economy.* Cambridge, United Kingdom:Cambridge University Press.

Macchiavello, Rocco, Andreas Menzel, Atonu Rabbani, and Christopher Woodruff. 2020. "Challenges of Change: An Experiment Training Women to Manage in the Bangladeshi Garment Sector." *Working Paper*.

Malcomson, James. 1983. "Trade Unions and Economic Efficiency." *The Economic Journal*, 93: 51–65.

McKinsey & Company. 2011. "Bangladesh's ready-made garments landscape: The challenge of growth." McKinsey & Company.

Morantz, Alison D., and Alexandre Mas. 2008. "Does Post-Accident Drug Testing Reduce Injuries? Evidence from a Large Retail Chain." *American Law and Economics Review*, 246–302.

Munni, Monira. 2015. "Owners-workers' row delays rules forming." *The Financial Express*.

Munni, Monira. 2017. "Half of apparel units flout needs for safety committees." *The Financial Express*.

O'Brien, Peter C. 1984. "Procedures for Comparing Samples with Multiple Endpoints." *Biometrics*, 40(4): 1079–1087.

Powell, Michael. 2019. "Productivity and Credibility in Industry Equilibrium." *The RAND Journal of Economics*, 50(1): 121–146.

Strauss, John, and Duncan Thomas. 1998. "Health, Nutrition, and Economic Development." *Journal of Economic Literature*, 36: 766–817.

US Senate Committee on Foreign Relations. 2013. "Worker Safety and Labor Rights in Bangladesh's Garment Sector." United States Senate.

Viscusi, W. Kip. 1980. "Unions, Labor Market Structure, and the Welfare Implications of the Quality of Work." *Journal of Labor Research*, 1(1): 175–192.

Weil, David. 1999. "Are Mandated Health and Safety Committees Substitutes or Supplements to Labor Unions?" *Industrial and Labor Relations Review*, 52: 424–438.

World Management Survey. n.d.. "World Management Survey Manufacturing Instrument." https://worldmanagementsurvey.org/survey-data/methodology/.

World Trade Organization. 2017. "World Trade Statistical Review 2017." World Trade Organization, Geneva, Switzerland.

Online Appendices

A: Truthful Reporting

In any study of compliance, one must be concerned about subjects' incentives to misreport. In the context of a RCT, in order to bias estimates of treatment effects, the treatment would need to affect subjects' propensity to tell the truth. In designing this RCT, I was keenly aware of these concerns, and I strove to design the data collection protocols in order to minimize experimenter demand effects and the potential for the OSH Committee Program to affect reporting. In this Appendix, I report results for empirical tests of truth-telling by factories during the period of intensive enforcement by the MNCs for the treatment factories, when incentives for misreporting were arguably the highest. In the Supplementary Materials, I provide a detailed overview of how the data collection protocol was designed to minimize experimenter demand effects.

During onsite visits, the research team collected data about other Alliance programs. In addition to shielding my interest in OSH Committees, this approach allows me to test for effects on truth-telling and on "placebo" outcomes that I do not expect to be affected by the OSH Committee Program. Beginning with senior managers, I asked them questions about their factories' progress with building safety remediation under their Alliance CAP. I also asked about their awareness of the Alliance's worker helpline, including the number of recent reports about their factory to the helpline. I can verify the correct answers to these questions using the Alliance's records. Thus, they allow me to test for non-truthful reporting and for managers' awareness of their factories' safety performance.

I also test for effects on three "placebo" outcomes related to factories' compliance with other Alliance programs. First, the Alliance required that all factory personnel carry its worker helpline phone number card with their employee ID card. Survey enumerators were required to verify that survey participants matched the list of randomly-selected participants, which they did by checking the participant's ID card. While checking, they noted whether the participant carried the helpline card (without indicating this to the survey participant). Thus, I can test whether treatment factories differentially respond to being visited by the research team by increasing the share of personnel wearing the cards. I test for effects for workers and for lower-level managers. Second, I test for effects on factories' maintenance of records of Alliance fire safety training implementation. The Alliance used a "train-the-trainer" model and required factories to conduct periodic training with workers and to maintain a training record using a provided template.

Table A.I presents baseline balance for truth-telling variables. In Panel A, variables based on the senior manager survey, there is an imbalance on one variable: Under-reporting of calls to the Alliance worker helpline by senior managers. It is important to note, though, that senior managers at 19 control and 13 treatment factories reported not knowing or were unaware of the Alliance's worker helpline at baseline. In Panel B, there is a marginally statistically significant difference for the share of workers with the Alliance's worker helpline card. This difference shrinks and is no longer statistically significant if the outlier factory on worker variables is dropped.

Table A.II reports the results. Beginning with Panel A, columns (1)-(2) report treatment effects on truth-telling. In column (1), the estimated treatment effect on over-reporting the factory's progress with required building safety remediation is close to zero and not statistically significant. In column (2), managers at treatment factories are actually less likely to under-report calls to the Alliance helpline (not statistically significant). While the treatment does not affect managers' propensity to misreport, columns (3) and (4) show that it appears to increase their awareness of safety issues: Treatment senior managers are significantly more likely to accurately report whether their factory was recently audited by the Alliance on building safety. They are also marginally more likely to be aware of the existence of the Alliance's worker helpline. These findings are consistent with stronger OSH committees' improving senior managers' information - for example, through the committee providing more reports - but not altering their incentives to misreport.

Turning to Panel B, columns (1)-(2) show that there is no difference between treatment or control factories in the share of workers or managers found carrying the Alliance helpline card. Column (3) shows that there is no difference on the Alliance's requirement to maintain safety training records, although compliance with this requirement was already very high at baseline. Together, the results do not provide any evidence that treatment factories differentially respond to the data collection.

Table A.I: Baseline balance tests, truth-telling

	Control mean (SD)	T-C diff	*p*-value [RI *p*]	Number of factories
	(1)	(2)	(3)	(4)
Panel A: Senior Managers				
Over-reports CAP completion	0.275	0.006	0.951	77
	(0.4522)		[0.957]	
Under-reports Alliance helpline calls†	0.478	-0.348***	0.006	50
	(0.5107)		[0.007]	
Correctly reports whether CAP visit	0.195	-0.082	0.299	80
	(0.4012)		[0.362]	
Aware of Alliance helpline	0.927	0.047	0.337	80
	(0.2637)		[0.615]	
Panel B: Compliance with other Alliance Programs				
Share workers with helpline card	0.827	-0.101*	0.096	80
	(0.2220)		[0.098]	
Share lower-level managers with helpline card	0.725	-0.074	0.285	80
	(0.318)		[0.291]	
Alliance Safety Training Record	0.976	0.004	0.916	80
	(0.1562)		[1.000]	

Notes: This table reports OLS estimates of baseline differences between control and treatment groups. For each variable, I report the baseline control group mean in column (1). In column (2), I report the estimated coefficient for the treatment indicator from a regression of the variable on the treatment indicator and stratification variables. In column (3), I report the *p*-value for the treatment indicator calculated using robust standard errors. I also report the RI *p*-value based on 5000 draws. In column (4), I report the sample size for the regression. †Senior managers at 19 control and 13 treatment factories reported not knowing the number of calls or were unaware of the helpline. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A.II: Treatment effects: Truth-telling

	Truth-telling		Awareness	
	Over-reports CAP completion	Under-reports helpline calls†	Correctly reports whether CAP visit	Aware of helpline
	(1)	(2)	(3)	(4)
Panel A: Senior Managers				
Treat	0.028	-0.149	0.251**	0.060
	(0.0742)	(0.1249)	(0.0990)	(0.0414)
	[0.675]	[0.226]	[0.015]	[0.042]
Control Mean	0.220	0.471	0.220	0.951
Observations	78	67	79	79
Strata FE	Y	Y	Y	Y
Control, base. dep. var.	Y	N	Y	Y

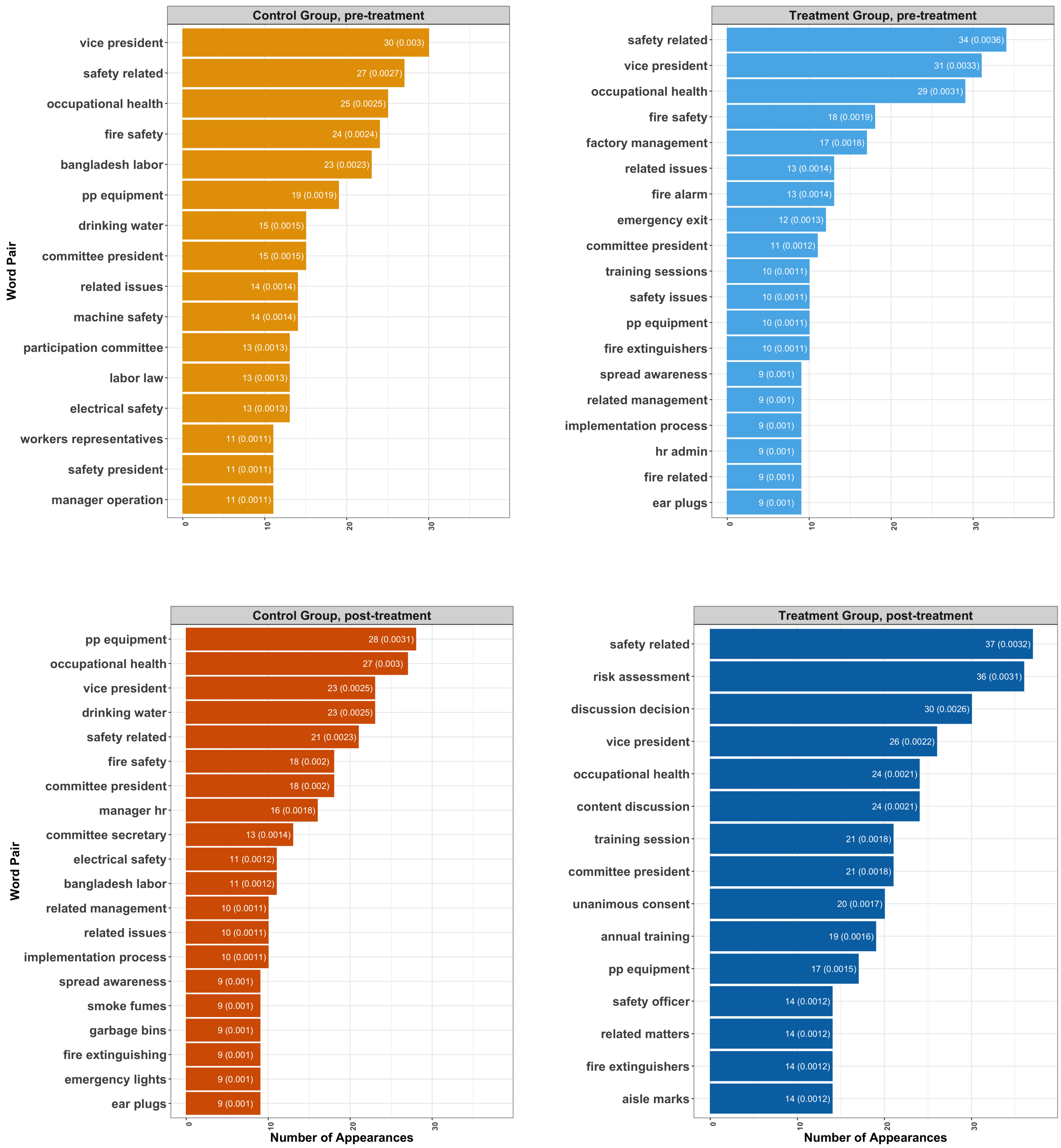
Panel B: Compliance with other Alliance Programs

	Worker Helpline		
	Share workers with card	Share lower-level managers with card	Safety Training Record
	(1)	(2)	(3)
Treat	0.015	-0.065	0.023
	(0.0356)	(0.0515)	(0.0231)
	[0.701]	[0.217]	[1.000]
Control Mean	0.838	0.799	0.977
Observations	80	80	80
Strata FE	Y	Y	Y
Control, base. dep. var.	Y	Y	Y

Notes: This table reports OLS estimates of treatment effects on measures of truth-telling and of awareness. Outcome variables are listed at the top of each column. Robust standard errors are reported in round brackets. RI *p*-values based on 5000 draws are reported in square brackets. †Senior managers at 7 control and 5 treatment factories reported not knowing the number of calls or were unaware of the Alliance's worker helpline at the second data collection visit. *p<0.1; **p<0.05; ***p<0.01.

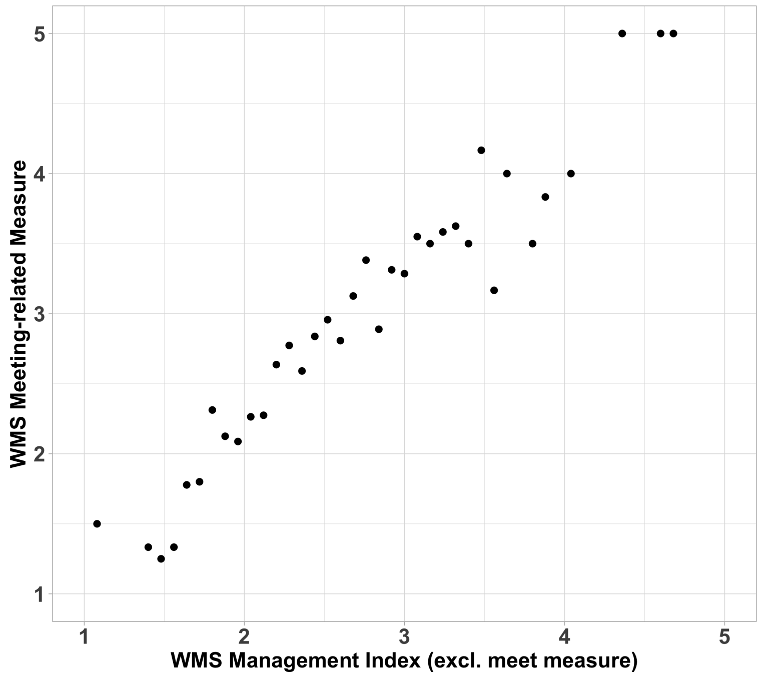
B: Figures and Tables

Figure B.I: Most common two-word combinations in OSH Committee Meetings Minutes



Notes: To prepare the meeting minutes for text analysis, I strip the text of factory and participant names, the phrases "[health and] safety committee(s)" and "meeting(s)," English language stop words, numbers, and punctuation. I also replace the commonly used acronym of "ppe," which stands for personal protective equipment, and the complete phrase, with "pp equipment." Finally, I "stem" words, or replace them with their root, using the Porter stemmer. These approaches are common practice in text analysis (Gentzkow, Kelly and Taddy, 2019).

Figure B.II: Correlation between WMS Management Index (excluding meeting question) and WMS Meeting-related Question, apparel firms in all countries



Notes: This figure presents a binned scatterplot of performance on the WMS excluding the meeting question and performance on the meeting-related WMS question. It includes all apparel manufacturers from all countries included in the WMS. The meeting-related WMS question asks whether performance is reviewed with appropriate frequency and communicated to staff ((World Management Survey, n.d.)). The WMS Management Index is the average score on all other questions.

Table B.I: Sample summary statistics

	(1) Mean	(2) SD	(3) Minimum	(4) Maximum	(5) N
Panel A: Primary outcome variables					
Compliance index	-0.039	0.337	-1.205	0.627	80
Safety Indicators index	-0.020	0.511	-1.309	1.088	80
Job Satisfaction & well-being index	-0.049	0.466	-2.329	0.772	80
Number of employees†	1124	1315	50	7724	400
Gross wages (log)†	15.721	1.089	13.217	18.309	360
Labor productivity (log)†	0.921	1.036	0	4.673	385
Panel B: Factory characteristics					
Trade union at factory	0.025	0.157	0	1	80
EPZ(1=Yes)	0.175	0.382	0	1	80
Sewing (only)	0.400	0.493	0	1	80
Number product types	1.175	0.569	0	4	80
Monthly absenteeism	4.589	3.845	0.074	26.916	80
Monthly turnover	3.606	3.913	0	29.948	80
Prop. employees visit medical clinic (daily)†	0.014	0.022	0.001	0.151	256
Participation in Alliance training (6 mo pre-baseline)	0.038	0.191	0	1	80
Number Alliance remediation visits to factory (6 mo pre-baseline)	0.163	0.404	0	2	80
Panel C: Worker survey respondent characteristics					
Age	27.373	3.585	21.550	40.071	80
Proportion female	0.507	0.282	0	1	80
Education (yrs)	6.055	1.681	2.750	11.300	80
Tenure (yrs)	3.777	2.318	0.429	11.508	80
Prior industry experience (yrs)	1.544	1.034	0.060	5.679	80
Panel D: OSH Committee President survey respondent characteristics					
Age	39.228	8.604	22	62	79
Proportion female	0.114	0.320	0	1	79
Education (yrs)	16.038	1.713	8	18	79
Tenure (yrs)	7.206	6.321	0.083	25	79
Prior industry experience (yrs)	6.090	7.357	0	28.500	79
Panel E: OSH Committee Worker Representative survey respondent characteristics					
Age	27.234	5.153	19.500	48	79
Proportion female	0.449	0.336	0	1	79
Education (yrs)	8.380	2.826	0	14	79
Tenure (yrs)	4.926	4.040	0.375	24.125	79
Prior industry experience (yrs)	1.655	1.875	0	8.500	79
Panel F: Senior Manager survey respondent characteristics					
Age	43.500	8.657	24	68	80
Proportion female	0.025	0.157	0	1	80
Education (yrs)	15.975	1.974	8	18	80
Tenure (yrs)	8.872	8.385	0.083	42	80
Prior industry experience (yrs)	8.741	9.149	0	43	80

Notes: The sample size changes across rows due to differential data availability. † Observations for these variables are at the monthly-level. Employment is available for 80 factories, wages for 72, and labor productivity for 77. In Panels D and E, the sample size is 79 factories because one factory was found not to have a true OSH committee at baseline.

Table B.II: Baseline balance tests, secondary outcome variables, OSH committee presidents and worker representatives, and senior managers

	(1)	(2)	(3)	(4)	(5)	(6)
	Control mean	Control SD	T-C diff	p-value	RI p	Number of factories
Panel A: Secondary outcomes for workers, full sample						
Perceived compliance & effectiveness index	0.000	(0.559)	-0.179	0.177	0.188	80
Perceived worker-manager relations index	0.020	(0.374)	-0.194	0.132	0.123	80
Worker empowerment index	0.022	(0.395)	-0.224*	0.079	0.079	80
Worker organization awareness index	-0.025	(0.726)	-0.112	0.494	0.500	80
Number non-pecuniary benefits	6.492	(0.899)	-0.336	0.102	0.100	80
Monthly safety-related calls (per 1000 workers)	0.057	(0.332)	0.025	0.689	0.875	80
Monthly non-safety-related calls (per 1000 workers)	0.422	(1.532)	0.130	0.719	0.935	80
Panel B: Secondary outcomes for workers, dropping outlier on worker outcomes						
Perceived compliance & effectiveness index	0.000	(0.559)	-0.139	0.278	0.267	79
Perceived worker-manager relations index	0.020	(0.374)	-0.157	0.212	0.197	79
Worker empowerment index	0.022	(0.395)	-0.152	0.152	0.156	79
Worker organization awareness index	-0.025	(0.726)	-0.071	0.657	0.661	79
Number non-pecuniary benefits	6.492	(0.899)	-0.322	0.119	0.110	79
Monthly safety-related calls (per 1000 workers)	0.057	(0.332)	0.025	0.689	0.872	79
Monthly non-safety-related calls (per 1000 workers)	0.422	(1.532)	0.144	0.694	0.919	79
Panel C: Secondary outcomes for factories						
Average Weekly Working Hours	54.367	(5.749)	2.342**	0.037	0.053	79
Efficiency (sewing section)	52.960	(14.020)	7.014	0.165	0.206	33
Defects per hundred units	3.221	(3.143)	-1.010	0.119	0.118	72
Supplier-buyer relations index	0.018	(0.581)	-0.133	0.394	0.448	71
Panel D: OSH Committee Presidents						
Age	40.073	(9.350)	-1.408	0.461	0.479	80
Proportion female	0.073	(0.264)	0.076	0.306	0.320	80
Education (yrs)	16.024	(1.851)	-0.105	0.799	0.797	80
Tenure (yrs)	6.459	(5.566)	1.364	0.334	0.331	80
Prior industry experience (yrs)	7.675	(8.802)	-2.651*	0.095	0.108	80
Panel E: OSH Committee Worker Representatives						
Age	26.888	(4.393)	0.649	0.567	0.584	79
Proportion female	0.488	(0.330)	-0.065	0.401	0.400	79
Education (yrs)	8.394	(2.621)	0.068	0.915	0.918	79
Tenure (yrs)	4.542	(4.109)	0.734	0.406	0.441	79
Prior industry experience (yrs)	1.848	(1.891)	-0.410	0.334	0.340	79
Panel F: Senior Managers						
Age	43.244	(9.497)	0.432	0.823	0.824	80
Proportion female	0.024	(0.156)	0.000	1.000	1.000	80
Education (yrs)	16.000	(1.844)	0.009	0.984	1.000	80
Tenure (yrs)	9.642	(8.998)	-1.864	0.302	0.307	80
Prior industry experience (yrs)	7.593	(9.540)	2.545	0.210	0.228	80

Notes: This table reports OLS estimates of baseline differences between control and treatment groups. For each outcome, I report the baseline control group mean and SD in columns (1) and (2). In column (3), I report the estimated coefficient for the treatment indicator from a regression of the outcome or covariate on the treatment indicator and stratification variables. In column (4), I report the p-value for the treatment indicator calculated using robust standard errors. In column (5), I report the RI p-value for the treatment indicator based on 5000 draws. In column (6), I report the sample size for the regression. *¡0.1; $** < 0.05$; ***¡0.01.

Table B.III: Short-run treatment effects: Secondary worker outcomes

	Perceived compliance& effective index	Perceived worker-manager relations index	Worker empowerment index	Worker organization index	Number non-pecuniary benefits	Safety-related calls per 1k workers Alliance Helpline	Non-safety-related calls per 1k workers Alliance Helpline
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Main treatment effects							
Treatment	0.195	-0.039	0.067	0.016	-0.052	-0.002	-0.030
	(0.122)	(0.081)	(0.091)	(0.079)	(0.168)	(0.075)	(0.101)
	[0.094]	[0.630]	[0.487]	[0.842]	[0.761]	[0.979]	[0.779]
Control Mean	-0.109	0.072	-0.178	0.073	6.802	0.107	0.303
Observations	80	80	80	80	80	400	400
Factories	80	80	80	80	80	80	80
Stratification variables	Y	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y	Y	Y	Y	Y
Panel B: Heterogeneous treatment effects by managerial practices							
Below median	0.015	-0.050	-0.152	-0.132**	-0.032	0.006	-0.028
	(0.143)	(0.117)	(0.115)	(0.065)	(0.273)	(0.121)	(0.151)
	[0.926]	[0.658]	[0.210]	[0.051]	[0.916]	[0.967]	[0.859]
Above median	0.387**	-0.042	0.257*	0.165	-0.072	-0.005	-0.054
	(0.178)	(0.120)	(0.152)	(0.139)	(0.201)	(0.074)	(0.135)
	[0.033]	[0.721]	[0.089]	[0.290]	[0.745]	[0.976]	[0.732]
p-val, diff	0.087	0.963	0.040	0.052	0.910	0.935	0.901
	[0.125]	[0.959]	[0.032]	[0.079]	[0.908]	[0.942]	[0.905]
Observations	80	80	80	80	80	400	400
Factories	80	80	80	80	80	80	80
Control mean, below median	0.012	0.122	-0.015	0.189	6.659	0.123	0.319
Control mean, above median	-0.205	0.034	-0.306	-0.017	6.914	0.094	0.291
Stratification variables	Y	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y	Y	Y	Y	Y

Notes: This table reports OLS estimates of short-run treatment effects on secondary outcome variables for workers. Each column in the table reports the estimated coefficient from a separate regression. The dependent variable in each column is regressed on the treatment indicator, stratification variables, and a control for the baseline value of the dependent variable. The regression sample changes in columns (6) and (7) due to a different source of data for these outcomes. Each regression in columns (1)-(5) includes one post-treatment observations per factory, while those in columns (6)-(7) include five post-treatment observations per factory, where each observation is one month. In columns (1)-(5), robust standard errors are reported in round brackets. In columns (6)-(7), standard errors clustered at the factory level are reported in round brackets. RI p-values based on 5000 draws are reported in square brackets. For index variables, in all cases, higher values of the index correspond to more positive outcomes. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table B.IV: Longer-run treatment effects: Secondary worker outcomes

	Perceived compliance& effective index	Perceived worker-manager relations index	Worker empowerment index	Worker organization index	Number non-pecuniary benefits	Safety-related calls per 1k workers Alliance Helpline	Non-safety-related calls per 1k workers Alliance Helpline
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Main treatment effects							
Treatment	-0.081	0.023	0.288**	-0.009	0.217	-0.035	-0.157
	(0.126)	(0.093)	(0.137)	(0.087)	(0.159)	(0.036)	(0.180)
	[0.522]	[0.791]	[0.037]	[0.916]	[0.189]	[0.359]	[0.403]
Control Mean	-0.064	0.041	-0.412	0.087	6.726	0.071	0.530
Observations	80	80	80	80	80	240	240
Factories	.	.	.	.	.	80	80
Stratification variables	Y	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y	Y	Y	Y	Y
Panel B: Heterogeneous treatment effects by managerial practices							
Below median	-0.200	0.089	0.085	0.007	0.481**	-0.043	-0.287
	(0.182)	(0.124)	(0.174)	(0.125)	(0.234)	(0.061)	(0.224)
	[0.325]	[0.483]	[0.640]	[0.957]	[0.065]	[0.542]	[0.209]
Above median	-0.010	-0.035	0.484**	-0.014	-0.061	-0.026	0.030
	(0.165)	(0.131)	(0.194)	(0.131)	(0.224)	(0.035)	(0.280)
	[0.955]	[0.798]	[0.027]	[0.924]	[0.791]	[0.541]	[0.931]
p-val, diff	0.442	0.462	0.120	0.909	0.104	0.935	0.901
	0.474	0.506	0.168	0.915	0.122	0.835	0.412
Observations	80	80	80	80	80	240	240
Factories	.	.	.	.	.	80	80
Control mean, below median	0.096	0.011	-0.301	0.074	6.551	0.075	0.436
Control mean, above median	-0.189	0.065	-0.499	0.096	6.863	0.068	0.604
Stratification variables	Y	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y	Y	Y	Y	Y

Notes: This table reports OLS estimates of longer-run treatment effects on secondary outcome variables for workers. Each column in the table reports the estimated coefficient from a separate regression. The dependent variable in each column is regressed on the treatment indicator, stratification variables, and a control for the baseline value of the dependent variable. The regression sample changes in columns (6) and (7) due to a different source of data for these outcomes. Each regression in columns (1)-(5) includes one post-treatment observations per factory, while those in columns (6)-(7) include five post-treatment observations per factory, where each observation is one month. In columns (1)-(5), robust standard errors are reported in round brackets. In columns (6)-(7), standard errors clustered at the factory level are reported in round brackets. RI p-values based on 5000 draws are reported in square brackets. For index variables, in all cases, higher values of the index correspond to more positive outcomes. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Table B.V: Treatment effects: Secondary factory outcomes

	Log(Output)	Mean Weekly Working Hours	Efficiency (sewing section)	Defects per 100 units	Supplier-buyer relations index	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Main treatment effects, short-run						
Treatment	0.0638	-0.4418	1.8256	0.2753	0.0467	0.0146
	(0.3138)	(0.5379)	(2.6746)	(0.2366)	(0.0544)	(0.0655)
	[0.846]	[0.502]	[0.487]	[0.267]	[0.437]	[0.843]
Control Mean	11.294	55.100	50.310	3.185	0.120	0.196
Observations	385	395	165	360	355	400
Factories	77	79	33	72	71	80
Panel B: Main treatment effects, longer-run						
Treatment	0.0530	-1.4058**	3.8953	0.1611	-0.0604	-0.1263
	(0.3555)	(0.6079)	(2.7043)	(0.1553)	(0.0813)	(0.1214)
	[0.895]	[0.018]	[0.154]	[0.339]	[0.536]	[0.335]
Control Mean	11.193	55.675	49.475	3.128	0.241	0.379
Observations	231	237	99	216	213	240
Factories	77	79	33	72	71	80
Panel C: HTEs by managerial practices, short-run						
Below median	0.6005	0.8546	3.3656	0.4139	0.0249	-0.0469
	(0.4783)	(0.7208)	(2.7154)	(0.4180)	(0.0747)	(0.0716)
	[0.263]	[0.267]	[0.378]	[0.379]	[0.783]	[0.563]
Above median	-0.5045	-1.7611**	-0.7995	0.1115	0.0749	0.0669
	(0.3992)	(0.8002)	(2.6355)	(0.1921)	(0.0811)	(0.1298)
	[0.199]	[0.112]	[0.823]	[0.599]	[0.418]	[0.622]
p-val, diff	0.090	0.024	0.257	0.526	0.658	0.474
	[0.091]	[0.052]	[0.380]	[0.561]	[0.687]	[0.454]
Observations	385	395	165	360	355	400
Factories	77	79	33	72	71	80
Control mean, below median	10.467	53.733	46.226	3.180	0.145	0.224
Control mean, above median	11.942	56.169	53.651	3.190	0.101	0.175
Panel D: HTEs by managerial practices, longer-run						
Below median	0.5230	-0.7146	7.7301***	-0.0324	-0.1476	-0.3646*
	(0.5084)	(0.6888)	(2.2606)	(0.2163)	(0.1231)	(0.1902)
	[0.399]	[0.390]	[0.037]	[0.898]	[0.372]	[0.087]
Above median	-0.4647	-2.1157**	-0.4091	0.3897*	0.0605	0.1483
	(0.4788)	(0.9015)	(2.4594)	(0.1994)	(0.1102)	(0.1685)
	[0.303]	[0.035]	[0.907]	[0.096]	[0.612]	[0.373]
p-val, diff	0.090	0.024	0.257	0.526	0.658	0.474
	[0.191]	[0.278]	[0.091]	[0.214]	[0.288]	[0.055]
Observations	231	237	99	216	213	240
Factories	77	79	33	72	71	80
Control mean, below median	10.302	54.959	43.270	3.231	0.270	0.416
Control mean, above median	11.890	56.236	54.552	3.025	0.220	0.351
Stratification variables	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y	Y	Y	Y
Product FE	Y	Y	N	N	N	N

Notes: This table reports OLS estimates of treatment effects on secondary outcome variables for factories. Each column in the table reports the estimated coefficient from a separate regression. The dependent variable in each column is regressed on the treatment indicator, stratification variables, and a control for the baseline value of the dependent variable. Columns (1)-(2) also include product FE. The regression sample changes across columns due to different data availability for these outcomes. In columns (5)-(6), for the supplier-buyer relations index, column (5) includes all 3 variables in the pre-specified index, and column (6) drops the third, which is missing for 9 factories. Observations are at the factory-month level in all regressions. Standard errors clustered at the factory level are reported in round brackets. RI p-values based on 5000 draws are reported in square brackets. For index variables, in all cases, higher values of the index correspond to more positive outcomes. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table B.VI: Pre-Analysis Plan Deviations

	Pre-Analysis Plan (PAP)	Modification
1)	The original PAP, which was posted on the AEA Registry in June 2017, included measures from survey question production line-level managers in the index of factory safety indicators (primary outcome variable). It also included survey measures from this group in certain secondary outcome index variables (perceived SC effectiveness and compliance index, worker-manager relations index, perception of worker capabilities index, and a perception of worker well-being index).	During baseline data collection, the research team determined that the line level managers were difficult to engage in surveys during the workday. As Safety Committees mostly aim to serve workers and to support senior management in occupational health and safety policies, after the baseline, I decided to remove variables from lower-level managers' survey data from these indices. I posted an updated PAP reflecting this change in December 2017.
2)	The SC Compliance Index included the following sub-variable: "President is management member and Vice President is worker member."	Due to an oversight, the information about the vice president's status was not collected for the first 57 baseline visits. I do not include this variable in the analysis.
3)	The Safety Indicators Index for the endline round included eight spotcheck variables that were only to be checked at endline.	Due to an administrative error, these additional items were not included in the checklist for the third visit for 14 out of 80 factories. As such, I do not include these variables in the analysis.
4)	The PAP indicated that I would also report outcomes measured at the individual-level at the individual level.	Consistent with best practice in econometric analysis for clusterd RCTs (Athey and Imbens, 2016), as the factory is the unit of analysis that is of interest, I omit individual-level regressions for space reasons.
5)	The PAP indicated that I would test for heterogeneous treatment effects by factories' location inside versus outside of an EPZ.	Ultimately, there were only 14 factories (7 treatment, 7 control) located in EPZs. There are large differences between these groups. As such, while I report baseline balance, I do not test for heterogeneous treatment effects by factory location.
6)	The PAP included hiring and machine downtime as factory-level secondary outcome variables.	I was not able to pilot the factory questionnaire until after I registered the PAP. I learned that many Alliance-covered factories did not systematically track these variables. Many factories in the sample had difficulty reporting them or indicated that they were unable to do. As such, I omit these secondary outcomes.
7)	The worker-manager relations index, a secondary outcome variable, include one variable to measure worker participation in strikes.	I decided that that participation in strikes was too sensitive to credibly measure in my setting, so I do not include the strike variable in the analysis.
8)	The PAP included an index of worker accidents and illness as a secondary outcome variable that included the medical clinic records, factory-reported illnesses and fires, and self-reported accidents and illness from workers.	The research team determined that factories' records of fires and accidents were often incomplete. For this reason, and due to the concern that the intervention may increase reporting of accidents by workers, I determined that the medical clinic records provide the most objective measure of accidents and illnesses.
9)	The PAP did not include using product type fixed effects in the labor productivity analysis.	In the paper, I report results with and without product fixed effects. The reason that I did not include them in the PAP was because the Alliance did not have records of factories' product types, and I did not anticipate that more than half of the factories would produce products other than RMG (e.g., shoes) or would process products (e.g., washing factories).

Table B.VII: Baseline balance tests, Sub-index components of primary outcome index variables & subgroups for heterogeneity analysis

	Control mean	Control SD	T-C diff	p-value	RI p	Number of factories
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OSH Committee Compliance						
Formation sub-index	0.015	(0.5440)	-0.070	0.639	0.636	80
Operations sub-index	-0.029	(0.5540)	0.082	0.511	0.520	80
Responsibilities sub-index	-0.017	(0.4420)	-0.138	0.176	0.177	80
Panel B: Safety Indicators						
CAP completion sub-variable	0.025	(1.0170)	0.126	0.571	0.554	80
Worker awareness sub-index	-0.010	(0.9060)	-0.545**	0.030	0.027	80
Worker knowledge sub-index	0.029	(0.8030)	-0.139	0.492	0.474	80
Senior manager awareness sub-variable	-0.015	(0.9960)	0.430*	0.066	0.077	80
Panel C: Worker Job Satisfaction and Mental Well-being						
Job satisfaction sub-index	0.023	(0.7380)	-0.205	0.219	0.204	80
Mental well-being sub-index	-0.019	(0.5630)	-0.205	0.281	0.294	80
Turnover sub-variable	-0.014	(1.0220)	0.144	0.402	0.482	80
Absenteeism sub-variable	0.000	(1.0000)	0.148	0.439	0.449	80
Panel D: Below-median management subgroup, primary outcomes						
OSH Committee Compliance	0.058	(0.2690)	0.027	0.769	0.775	40
Safety Indicators	0.114	(0.4620)	-0.120	0.473	0.450	40
Job Satisfaction & Mental Well-being	0.070	(0.4040)	-0.198	0.254	0.282	40
Log(Labor productivity)†	0.749	(0.9990)	-0.051	0.665	0.778	195
Log(Wages)	15.625	(1.1290)	0.007	0.983	0.981	190
Log(Employment)	6.297	(1.0740)	0.060	0.851	0.858	200
Panel E: Above-median management subgroup, primary outcomes						
OSH Committee Compliance	-0.063	(0.2260)	-0.184	0.165	0.119	40
Safety Indicators	-0.079	(0.3370)	-0.015	0.926	0.918	40
Job Satisfaction & Mental Well-being	-0.053	(0.3510)	-0.021	0.827	0.847	40
Log(Labor productivity)†	0.818	(0.8560)	-0.254	0.110	0.188	185
Log(Wages)	16.004	(0.9260)	-0.376	0.333	0.369	170
Log(Employment)	6.925	(0.8610)	-0.514	0.162	0.158	200

Notes: This table reports OLS estimates of baseline differences between control and treatment groups. Panels A-C report differences for the sub-indexes and sub-variables that comprise each primary outcome index. Panels D and E report differences between control and treatment groups within above- and below-median management subgroups for the HTE analysis. Columns (1)-(2) report the baseline control group mean and standard deviation. Column (3) reports the estimated coefficient for the treatment indicator from a regression of the sub-index or sub-variable on the treatment indicator and stratification variables. Columns (5) report the p-value calculated using robust standard errors and the RI p-value based on 5000 draws for the coefficient reported in column (3). Column (6) reports the number of observations in the regression. † The regression also includes product-type fixed effects. The trimmed sample drops factory-month observations in the 1st and 99th percentiles of labor productivity. *p¡0.1; **p¡0.05; ***p¡0.01.

Table B.VIII: Local Average Treatment Effects (LATEs): Treatment effects on primary outcomes

	OSH committee compliance index	Safety indicators index	Job satisfaction and mental well-being index
	(1)	(2)	(3)
Panel A: Outcomes measured using data collected during 3 onsite visits, short-run			
LATE	0.279***	0.156**	-0.161**
	(0.060)	(0.068)	(0.079)
Control Mean	0.019	0.109	-0.013
Observations	80	80	80
Panel B: Outcomes measured using data collected during 3 onsite visits, longer-run			
LATE	0.234***	0.081	0.114
	(0.079)	(0.070)	(0.086)
Control Mean	0.135	0.153	-0.099
Observations	80	80	80
Stratification variables	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y
	Log(Labor Productivity)	Log(Wages)	Log(Employment)
Panel C: Outcomes measured using monthly data, short-run			
LATE	0.050	-0.017	-0.012
	(0.037)	(0.031)	(0.023)
Control Mean	0.749	15.865	6.665
Observations	380	360	400
Factories	380	360	400
Panel D: Outcomes measured using monthly data, longer-run			
LATE	-0.023	-0.009	0.003
	(0.035)	(0.032)	(0.029)
Control Mean	0.813	15.866	6.670
Observations	228	216	240
Factories	228	216	240
Stratification variables	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y
Product FE	Y	N	N
Dropping outlier	Y	N	N

Notes: This table reports two stage least squares (2SLS) estimates of treatment effects on primary outcome variables. Each column in the table reports the estimated coefficient from a separate regression. In Panels A and B, higher values of index variables correspond to more positive outcomes. Robust standard errors are reported in parentheses. In Panels C and D, the regression sample changes across columns due to differential data availability. For labor productivity, results are shown dropping the control factory that partially shuts down during the study and including product type FE. Compliance in Panels C and D is coded by month for the 4 factories that started the OSH committee program with substantial delays; the month when they started the program and later months are coded as treated. Standard errors clustered at the factory level are reported in round brackets. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table B.IX: Lee (2009) bounds for effects on primary outcomes

	Lower bound	Upper bound
Panel A: Short-run effects		
OSH Committee Compliance Index	0.223***	0.227***
	(0.077)	(0.074)
Safety Indicators Index	0.135	0.136*
	(0.088)	(0.072)
Job Satisfaction & Mental Well-being Index	-0.159**	-0.158*
	(0.081)	(0.084)
Panel B: Longer-run effects		
OSH Committee Compliance Index	0.213**	0.217**
	(0.092)	(0.087)

Notes: This table reports Lee treatment effect bounds for sample selection. Outcome variables are listed on the left. Column (1) reports the lower bound. Column (2) reports the upper bound. Standard errors are reported in parentheses. *p$<$0.1; **p$<$0.05; ***p$<$0.01.

Table B.X: Short-run treatment effects: Worker awareness & Workforce composition

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Worker awareness						
	Aware of OSH committee & its responsibilities	Knows factory has OSH committee	Knows how to report safety concern to OSH committee	Reported num OSH committee responsibilities	Reports committee as channel for raising issues	Knows OSH committee members
Treatment	0.053**	0.040**	0.011	-0.118	0.063	0.073**
	(0.0249)	(0.0177)	(0.0232)	(0.1228)	(0.0379)	(0.0380)
	[0.036]	[0.017]	[0.651]	[0.345]	[0.101]	[0.028]
Control mean	0.843	0.945	0.920	3.060	0.655	0.689
Observations	80	80	80	80	80	80
Stratification variables	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y	N	Y	N
Panel B: Workforce composition						
	Age	Female	Tenure	Prior exp.	Yrs. Education	
Treatment	−0.171	−0.041	0.251	0.041	0.264	
	(0.5122)	(0.0368)	(0.3204)	(0.1833)	(0.2669)	
	[0.741]	[0.274]	[0.437]	[0.828]	[0.336]	
Control mean	27.667	0.577	3.696	1.507	6.635	
Observations	80	80	80	80	80	
Stratification variables	Y	Y	Y	Y	Y	
Control, baseline dep. var.	Y	Y	Y	Y	Y	

Notes: This table reports OLS estimates of treatment effects on worker OSH committee awareness variables and on workforce characteristics. In Panel A, the first four columns report outcomes included in the Safety Indicators Index (prior to standardization for inclusion in the index). Each column in the table reports the estimated coefficient from a separate regression. The regression sample is the same in all columns. The dependent variable in each column is regressed on the treatment indicator, stratification variables, and a control for the baseline value of the dependent variable (if available). RI p-values based on 5000 draws are reported in square brackets. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table B.XI: Short-run treatment effects: OSH checklist

	Control mean	ITT Effect (SD)	RI p-value
	(1)	(2)	(3)
Factory safety spotcheck index	0.000	0.228** (0.0892)	0.012
Sewing: Machines have guards *and* workers wear PPE† for their task	0.500	0.076 (0.1512)	0.620
Cutting: Machines have guards *and* workers wear PPE for their tasks	0.792	0.071 (0.1173)	0.557
Dyeing and jobs handling chemicals: Safety masks, goggles, gloves, aprons, and boots worn by workers handling chemicals	0.545	0.102 (0.2293)	0.668
All PPE appropriate size, functional, and well-maintained	0.951	0.050 (0.0350)	0.492
Aisles clearly marked and markings visible	0.780	0.025 (0.0908)	1.000
Aisles clear of sewing scrapes and debris	0.951	0.048 (0.0338)	0.503
Aisles clear of obstruction	0.854	0.014 (0.0800)	1.000
Machines in good working order & dangerous parts properly covered	0.927	0.070* (0.0404)	0.247
Work stations maintained in tidy condition (no loose materials close to electrical appliances)	0.976	0.022 (0.0228)	1.000
One or more easily accessible first aid kit in section	0.976	0.022 (0.0228)	1.000
Physical separation between storage and production areas	0.976	0.023 (0.0229)	1.000
Drinking water easily accessible for all workers	1.000	-0.025 (0.0252)	1.000
Drinking water provided appears clean (visual check)	1.000	-0.025 (0.0252)	1.000
Stratification variables		Y	

Notes: This table reports OLS estimates of treatment effects on the spotcheck sub-index and for each variable in the spotcheck index. Four variables on the spotcheck checklist drop from the analysis because all factories were found to comply with these variables (see the Supplementary Materials). Sub-variables are listed on the left. Results are shown for the sub-variables *prior* to standardizing them for inclusion in the index. Column (1) reports the control group mean of the outcome variable. Column (2) reports the estimated ITT effect from a regression of the outcome variable on the treatment indicator and stratification variables. Robust standard errors are reported in round brackets. Column (3) reports RI p-values based on 5000 draws are reported in square brackets. † PPE stands for personal protective equipment. PPE vary by task and include equipment such as eye guards, finger guards, chain mesh gloves, goggles, boots, etc. *¡0.1;**¡0.05;***¡0.01.

Table B.XII: Short-run treatment effects: Workers' job satisfaction & mental well-being sub-variables

	Control mean	ITT Effect
	(1)	(2)
Panel A: Job Satisfaction		
Self-reported job satisfaction (qualitative scale, coded 1-5)	4.813	-0.045 (0.0486) [0.384]
Respondent suggested/helped family or friends to get a job at their factory (previous 4 months)	0.573	-0.049 (0.0428) [0.266]
Respondent has thought about leaving their job at factory for safety-related reasons (previous 3 months)	0.024	0.019* (0.0101) [0.064]
Panel B: Mental Well-being		
Self-reported level of stress in life (qualitative scale, coded (-1)-(-5))	-1.760	-0.059 (0.0755) [0.474]
Self-reported perceived extent of control over their life (qualitative scale, coded 1-5)	4.082	-0.035 (0.0557) [0.534]
Self-reported perceived extent of control safety at factory (qualitative scale, coded 1-5)	4.368	-0.037 (0.0584) [0.530]
Self-reported stress about experiencing accident or injury at factory (qualitative scale, coded (-1)-(-5))	-1.488	0.039 (0.0599) [0.538]
Self-reported frequency of feeling unsafe at factory (qualitative scale, coded (-1)-(-5))	-1.236	-0.013 (0.0317) [0.691]
Panel C: Turnover and Absenteeism		
Turnover	3.356	0.051 (0.3107) [0.884]
Absenteeism	4.457	0.3866 (0.2507) [0.162]
Observations		80
Stratification variables		Y
Control, base. dep. var.		Y

Notes: This table reports OLS estimates of treatment effects on each variable included in the worker job satisfaction and mental well-being index. Each panel reports the sub-variable results for a different sub-index. Sub-indexes and sub-variables are listed on the left. Results are shown for the variables *prior* to orienting them to be unidirectional and standardizing them for inclusion in the index. Column (1) reports the control group mean of the outcome variable. Column (2) reports the estimated ITT effect from a regression of the outcome variable on the treatment indicator, stratification variables, and a control for the baseline value of the outcome variable. Robust standard errors are reported in round brackets. RI p-values based on 5000 draws are reported in square brackets. *p<0.1; **p<0.05; ***p<0.01.

Table B.XIII: Treatment effects: Business competitiveness outcomes, panel regression model

	Log(Labor Productivity)				Log(Gross wages)		Log(Employment)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post, short-run	0.069	0.068	0.028	0.028	-0.017	-0.017	-0.013	-0.013
	(0.046)	(0.046)	(0.042)	(0.042)	(0.029)	(0.029)	(0.022)	(0.022)
	[0.141]	[0.143]	[0.505]	[0.505]	[0.587]	[0.587]	[0.564]	[0.564]
Treatment × Post, longer-run	-0.021	-0.022	-0.054	-0.054	-0.006	-0.006	0.000	0.000
	(0.055)	(0.055)	(0.051)	(0.051)	(0.033)	(0.034)	(0.030)	(0.030)
	[0.695]	[0.689]	[0.246]	[0.246]	[0.859]	[0.859]	[0.994]	[0.994]
Control Mean	0.801	0.801	0.774	0.774	15.865	15.865	6.665	6.665
Factories	77	77	76	76	72	72	80	80
Observations	960	960	988	988	936	936	1040	1040
Factory FE	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	N	Y	N	Y	N	Y	N	Y
Drop partial shutdown	N	N	Y	Y	N	N	N	N

Notes: This table reports OLS estimates of short- and longer-run treatment effects on labor productivity, employment, and gross wages using a panel regression model. Outcome variables are listed at the top of each column. Each column reports the estimated ITT effect from a separate regression. Columns (1)-(4) reports results for labor productivity. In columns (1)-(2), the sample is trimmed at the 1st and 99th percentile of all factory-month labor productivity observations. In columns (3)-(4), a factory in the control group that partially shut down during the study is dropped. Labor productivity is measured as the log of the physical quantity of output per person-hour. Person-hours are calculated as number of workers times the average weekly working hours times 4 weeks per month plus the number of management-level employees times average weekly working hours for managers times 4 weeks per month. The regression sample changes across columns due to differential data availability. Standard errors clustered at the factory level are reported in round brackets. RI p-values based on 5000 draws are reported in square brackets. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table B.XIV: Ex post minimum detectable effect sizes (MDEs): Effects on business competitiveness outcomes

	Control mean (sd)	MDE
	(1)	(2)
Panel A: Short-run effects		
Log(Labor productivity)†	0.767	0.127
	(0.859)	
Log(Labor productivity), dropping factory that partially shuts down	0.749	0.094
	(0.856)	
Log(Gross wages)	15.865	0.081
	(1.080)	
Log(Employment)	6.665	0.060
	(1.038)	
Panel B: Longer-run effects		
Log(Labor productivity)†	0.821	0.107
	(0.851)	
Log(Labor productivity), dropping factory that partially shuts down	0.813	0.101
	(0.918)	
Log(Gross wages)	15.866	0.088
	(1.069)	
Log(Employment)	6.670	0.082
	(1.056)	

Notes: This table reports ex post power calculations and minimum detectable effect sizes for labor productivity, employment, and wage outcome variables with 80% power at the 5% significance level. Outcome variables are listed on the left. Column (1) reports the control group mean and standard deviation in column. Column (2) reports the ex post MDE. †Reported MDE is for sample trimmed at the 1st and 99th percentiles of all factory-month observations for labor productivity.

Table B.XV: Short-run treatment effects: Labor productivity & unit prices, estimated with given number of months lead on observations from customs records

	1	2	3	4	5	6
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log(Labor Productivity)						
Treatment	0.072	0.058	0.043	0.026	0.014	0.005
	(0.035)	(0.034)	(0.034)	(0.032)	(0.032)	(0.034)
Control Mean	0.730	0.738	0.749	0.754	0.769	0.773
Factories	76	76	76	76	76	76
Observations	380	380	380	380	380	380
Stratification variables	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y	Y	Y	Y
Product FE	Y	Y	Y	Y	Y	Y
PDS Lasso Selected Controls	N	N	N	N	N	N
Drop partial shutdown	Y	Y	Y	Y	Y	Y
Panel B: Log(Labor Productivity)						
Treatment	0.067	0.052	0.046	0.031	0.027	0.016
	(0.039)	(0.038)	(0.038)	(0.035)	(0.035)	(0.038)
Control Mean	0.730	0.738	0.749	0.754	0.769	0.773
Factories	76	76	76	76	76	76
Observations	380	380	380	380	380	380
Stratification variables	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	N	N	N	N	N	N
Product FE	N	N	N	N	N	N
PDS Lasso Selected Controls	Y	Y	Y	Y	Y	Y
Drop partial shutdown	Y	Y	Y	Y	Y	Y
Panel C: Log(Average Unit Price)						
Treatment	-0.024	-0.012	-0.019	-0.065	0.003	0.061
	(0.052)	(0.054)	(0.053)	(0.051)	(0.052)	(0.039)
Control Mean	2.336	2.332	2.324	2.330	2.327	2.299
Factories	53	53	52	53	53	53
Observations	257	259	254	253	255	250
Stratification variables	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	N	N	N	N	N	N
Product FE	N	N	N	N	N	N
PDS Lasso Selected Controls	Y	Y	Y	Y	Y	Y

Notes: This table reports OLS estimates of treatment effects on labor productivity and on average unit prices, estimated using leads of 1 to 6 months for observations sourced from the customs records. The number of month leads are listed at the top of each column. In Panels A and B, the outcome is the log of labor productivity. Labor productivity is measured as the log of the physical quantity of output per person-hour. Person-hours are calculated as number of workers times the average weekly working hours times 4 weeks per month plus the number of management-level employees times average weekly working hours for managers times 4 weeks per month. In Panel C, the outcome is the log of the weighted average unit price, where the weights are applied by volume of the HS6 product code. Standard errors clustered at the factory level are reported in round brackets. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Table B.XVI: Short-run treatment effects: OSH committee meetings, OSH committee worker reps, and OSH committee challenges

	Meeting Frequency, prev. 3 months		Log(Word Count)		Worker rep raised issue prev. meeting	Worker reps' participation in meetings	President: Committee needs more support from Management	Worker reps: Committee needs more support from Management
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Main treatment effects								
Treatment	0.738***	0.683***	0.152	0.227	-0.024	-0.043	-0.058	0.019
	(0.194)	(0.185)	(0.174)	(0.137)	(0.127)	(0.113)	(0.070)	(0.074)
	[0.000]	[0.000]	[0.425]	[0.091]	[0.847]	[0.722]	[0.349]	[0.796]
Control Mean	1.268	1.268	5.268	5.268	0.329	2.878	0.146	0.200
Observations	80	80	74	71	80	80	78	79
Stratification variables	Y	Y	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	N	Y	N	Y	Y	Y	Y	Y
Panel B: Heterogeneous treatment effects by managerial practices								
Below median	0.833***	0.839***	0.168	0.185	-0.230	-0.267	-0.176	-0.007
	(0.273)	(0.243)	(0.238)	(0.197)	(0.218)	(0.180)	(0.121)	(0.110)
	[0.006]	[0.003]	[0.460]	[0.381]	[0.318]	[0.212]	[0.136]	[0.945]
Above median	0.622**	0.507*	0.094	0.278	0.127	0.170	0.063	0.047
	(0.289)	(0.288)	(0.269)	(0.198)	(0.102)	(0.138)	(0.098)	(0.106)
	[0.044]	[0.090]	[0.800]	[0.156]	[0.255]	[0.246]	[0.508]	[0.679]
p-val, diff	0.596	0.382	0.843	0.741	0.136	0.074	0.155	0.721
	[0.633]	[0.441]	[0.846]	[0.739]	[0.157]	[0.085]	[0.107]	[0.730]
Observations	80	80	74	71	80	80	78	79
Control mean, below median	1.222	1.222	5.356	5.356	0.583	3.000	0.222	0.176
Control mean, above median	1.304	1.304	5.208	5.208	0.130	2.783	0.087	0.217
Stratification variables	Y	Y	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	N	Y	N	Y	Y	Y	Y	Y

Notes: This table reports OLS estimates of short-run treatment effects on OSH committees' meeting frequency, on the number of words in meeting minutes for OSH committee meetings, and on measures of OSH committee outcomes. Each column in the table reports the estimated coefficient from a separate regression. The dependent variable in each column is regressed on the treatment indicator and stratification variables, and a control for the baseline value of the dependent variable (except columns (1) and (3)). Robust standard errors are reported in round brackets. RI *p*-values based on 5000 draws are reported in square brackets. *p<0.1; **p<0.05; ***p<0.01.

Table B.XVII: Short-run treatment effects: Worker reporting of OSH-related issues

	Reported safety concern, prev. 4 months	Reported concern to OSH Committee	Reported accident, to survey team	Would report safety concern	Would report concern to OSH Committee	Would report accident	Would report accident to OSH Committee
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Main treatment effects							
Treatment	0.021	0.024	0.007	-0.016*	0.056	0.005	0.001
	(0.018)	(0.015)	(0.008)	(0.009)	(0.040)	(0.006)	(0.047)
	[0.261]	[0.117]	[0.340]	[0.078]	[0.169]	[0.384]	[0.982]
Control Mean	0.051	0.030	0.007	0.994	0.652	0.985	0.366
Observations	80	80	80	80	80	80	80
Stratification variables	Y	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y	Y	Y	Y	Y
Panel B: Heterogeneous treatment effects by managerial practices							
Below median	-0.020	-0.000	0.003	-0.022	-0.037	0.003	-0.108*
	(0.023)	(0.021)	(0.005)	(0.016)	(0.049)	(0.009)	(0.062)
	[0.411]	[0.989]	[0.652]	[0.197]	[0.402]	[0.756]	[0.104]
Above median	0.069**	0.051**	0.014	-0.010	0.122*	0.005	0.115*
	(0.031)	(0.024)	(0.015)	(0.010)	(0.064)	(0.007)	(0.068)
	[0.020]	[0.028]	[0.358]	[0.287]	[0.086]	[0.500]	[0.096]
p-val, diff	0.030	0.121	0.454	0.540	0.063	0.854	0.020
	[0.020]	[0.103]	[0.475]	[0.568]	[0.052]	[0.862]	[0.021]
Observations	80	80	80	80	80	80	80
Control mean, below median	0.064	0.033	0.005	0.994	0.763	0.989	0.449
Control mean, above median	0.040	0.027	0.009	0.993	0.565	0.983	0.302
Stratification variables	Y	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y	Y	Y	Y	Y

Notes: This table reports OLS estimates of treatment effects on workers' reporting of OSH-related concerns and accidents. Each column in the table reports the estimated coefficient from a separate regression. The dependent variable in each column is regressed on the treatment indicator, stratification variables, and a control for the baseline value of the dependent variable. Robust standard errors are reported in round brackets. RI *p*-values based on 5000 draws are reported in square brackets. *p¡0.1; **p¡0.05; ***p¡0.01.

Table B.XVIII: Short-run treatment effects: Cooperation, coordination, and bargaining power mechanisms

	Management cares Workers' safety	Management would address unsafe conditions	Management may punish for reporting injury	Workers and Managers improve safety together	Management would pay medical care	OSH Committee can affect OSH policies	OSH Committee responsive to workers' concerns'
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Main treatment effects							
Treatment	0.061*	-0.059	-0.005	0.093	0.006	0.032	0.040
	(0.036)	(0.062)	(0.013)	(0.070)	(0.032)	(0.043)	(0.050)
	[0.098]	[0.398]	[0.732]	[0.184]	[0.847]	[0.463]	[0.396]
Control Mean	4.018	4.316	0.038	3.278	0.809	0.540	4.195
Observations	80	80	80	80	80	80	80
Stratification variables	Y	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y	Y	Y	Y	Y
Panel B: Heterogeneous treatment effects by managerial practices							
Below median	-0.014	-0.161*	-0.020	-0.057	-0.014	-0.039	-0.095
	(0.053)	(0.096)	(0.017)	(0.110)	(0.047)	(0.054)	(0.066)
	[0.804]	[0.151]	[0.269]	[0.640]	[0.766]	[0.477]	[0.170]
Above median	0.137**	0.041	0.012	0.250***	0.016	0.094	0.162**
	(0.052)	(0.083)	(0.022)	(0.078)	(0.043)	(0.067)	(0.068)
	[0.006]	[0.645]	[0.586]	[0.003]	[0.717]	[0.187]	[0.025]
p-val, diff	0.050	0.123	0.266	0.027	0.635	0.133	0.007
	[0.054]	[0.149]	[0.248]	[0.032]	[0.635]	[0.134]	[0.010]
Observations	80	80	80	80	80	80	80
Control mean, below median	4.056	4.383	0.045	3.326	0.851	0.596	4.282
Control mean, above median	3.989	4.263	0.033	3.240	0.775	0.497	4.127
Stratification variables	Y	Y	Y	Y	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y	Y	Y	Y	Y

Notes: This table reports OLS estimates of treatment effects on measures of coordination and cooperation between workers and managers and on workers' bargaining power for OSH. Each column in the table reports the estimated coefficient from a separate regression. The dependent variable in each column is regressed on the treatment indicator, stratification variables, and a control for the baseline value of the dependent variable. Robust standard errors are reported in round brackets. RI *p*-values based on 5000 draws are reported in square brackets. *p¡0.1; **p¡0.05; ***p¡0.01.

Table B.XIX: Longer-run treatment effects: Sub-indexes of primary outcome index variables

Outcome variable	Control mean	Treatment effect	Robust std. err.	RI p	FDR p
	(1)	(2)	(3)	(4)	(5)
Panel A: OSH committee compliance index					
Formation sub-index	0.104	-0.005	0.136	0.971	0.479
Operations sub-index	0.193	0.195*	0.117	0.101	0.110
Responsibilities sub-index	0.126	0.417***	0.111	0.000	0.001
Panel B: Safety indicators sub-indexes					
Spotcheck sub-index	0.028	0.073	0.085	0.421	1.000
CAP completion sub-variable	0.437	0.146	0.087	0.097	1.000
Worker OSH committee awareness sub-index	0.285	0.051	0.113	0.659	1.000
Worker safety knowledge sub-index	0.240	0.177	0.162	0.291	1.000
Senior manager awareness sub-index	0.238	-0.004	0.242	0.962	1.000
Panel C: Workers' job satisfaction and mental well-being sub-indexes					
Job satisfaction sub-index	-0.211	-0.107	0.157	0.492	1.000
Mental well-being sub-index	-0.130	0.351*	0.187	0.065	0.345
Turnover sub-variable	0.083	-0.014	0.064	0.853	1.000
Absenteeism sub-variable	0.051	-0.017	0.052	0.769	1.000

Notes: This table reports OLS estimates of treatment effects on sub-indexes of primary outcome index variables. Outcome variables are listed in each row. In all cases, higher values of the index correspond to positive outcomes. Each row reports the estimated ITT effect from a separate regression. All regressions include 80 observations. All regressions include stratification variables. With the exception of the spotcheck index, all regressions also include a control for baseline value of the dependent variable. Robust standard errors are reported in column (3). RI p-values based on 5000 draws are reported in column (4). p-values adjusted to control the FDR across each primary outcome's sub-indexes are reported in column (5).

C: Robustness checks for HTE analysis

I report robustness checks for the HTE analysis by management practices (Section 4.4). First, there is correlation in factories' characteristics: Better-managed factories tend to be somewhat larger and less compliant. This raises the concern that only one of these characteristics determines the intervention's effects. To examine this possibility, I regress each outcome on the treatment indicator, an indicator for each dimension of heterogeneity, and interactions between each dimension and the treatment. This specification demands a lot of the data, but it provides qualitative insight into the relative importance of each dimension. Table C.I presents the results. For all three primary outcome index variables, management practices remain important. For the safety indicators index, while the interaction term loses statistical significance, it is largest in magnitude.

Another concern is that MNCs may more intensively monitor less compliant factories and that this generates the heterogeneous effects. In this case, one would expect the Alliance to be more likely to audit factories that, at baseline, are less compliant with the OSH committee law. The Alliance audited five treatment factories during the study period, but all of the audits occurred after the 4-5 month data collection visit. As such, differential auditing could not drive the heterogeneous effect patterns in Panel A of Table VII.

Finally, I use an alternative measure of management practices. This measure captures a different dimension of managerial capacity: HR management. I measure HR practices using an index of worker-reported HR practices and relations with managers that I pre-specified as a secondary outcome variable (see the Supplementary Materials for index components). I find a qualitatively similar pattern of heterogeneous effects using this variable as with my main measure. See Tables C.II and C.III below.

Table C.I: Testing the importance of each dimension of heterogeneity, pooled short- and longer-run rounds

	OSH committee compliance index	Safety indicators index	Job satisfaction and mental well-being index
	(1)	(2)	(3)
Treatment	0.244**	0.050	-0.331**
	(0.104)	(0.123)	(0.165)
	[0.049]	[0.639]	[0.028]
Treatment× Abv med Compliance	-0.172	-0.033	0.255
	(0.110)	(0.131)	(0.160)
	[0.130]	[0.758]	[0.072]
Treatment × Abv med Size	-0.065	0.015	0.126
	(0.105)	(0.134)	(0.128)
	[0.563]	[0.877]	[0.306]
Treatment × Abv med Mgmt	0.235**	0.125	0.266*
	(0.109)	(0.121)	(0.142)
	[0.045]	[0.238]	[0.056]
Control Mean	0.077	0.131	-0.056
Observations	160	160	160
Stratification variables	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y

Notes: This table reports OLS estimates of HTEs for the pooled effects, controlling for all dimensions of heterogeneity. Each column in table the reports the estimated coefficients from a separate regression. The regression sample is the same in all columns in a panel. Standard errors clustered by factory are reported in round brackets. RI p-values based on 5000 draws are reported in square brackets. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Table C.II: Baseline balance tests within non-management subgroups for HTE analysis, primary outcome index variables

	Control mean	Control SD	T-C diff	*p*-value	RI *p*	Number of factories
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Factory Size						
Below median subgroup:						
OSH Committee Compliance	0.000	(0.2338)	0.011	0.903	0.908	40
Safety Indicators	0.044	(0.3926)	0.042	0.804	0.816	40
Job Satisfaction & Mental Well-being	0.004	(0.4419)	0.026	0.868	0.868	40
Above median subgroup:						
OSH Committee Compliance	-0.018	(0.2682)	-0.178	0.252	0.206	40
Safety Indicators	-0.027	(0.4183)	-0.147	0.367	0.380	40
Job Satisfaction & Mental Well-being	-0.002	(0.3170)	-0.133	0.367	0.452	40
Panel B: OSH Committee Compliance						
Below median subgroup:						
OSH Committee Compliance	-0.195	(0.1290)	-0.203	0.041	0.014	40
Safety Indicators	-0.002	(0.3489)	-0.029	0.863	0.873	40
Job Satisfaction & Mental Well-being	-0.093	(0.4055)	0.173	0.190	0.192	40
Above median subgroup:						
OSH Committee Compliance	0.185	(0.1900)	0.041	0.484	0.485	40
Safety Indicators	0.014	(0.4625)	-0.036	0.831	0.827	40
Job Satisfaction & Mental Well-being	0.100	(0.3208)	-0.377	0.027	0.018	40
Panel C: Location in EPZ						
EPZ subgroup:						
OSH Committee Compliance	-0.144	(0.2424)	0.140	0.516	0.475	14
Safety Indicators	-0.056	(0.4262)	0.234	0.586	0.590	14
Job Satisfaction & Mental Well-being	-0.104	(0.4362)	0.503	0.076	0.078	14
Non-EPZ subgroup:						
OSH Committee Compliance	0.018	(0.2458)	-0.088	0.344	0.324	66
Safety Indicators	0.019	(0.4037)	-0.105	0.421	0.407	66
Job Satisfaction & Mental Well-being	0.022	(0.3648)	-0.173	0.151	0.137	66
Panel D: HR Managerial Practices						
Below median subgroup:						
OSH Committee Compliance	-0.013	(0.2930)	-0.051	0.667	0.677	40
Safety Indicators	-0.179	(0.4393)	-0.006	0.975	0.972	40
Job Satisfaction & Mental Well-being	-0.011	(0.3929)	-0.250	0.121	0.151	40
Above median subgroup:						
OSH Committee Compliance	-0.007	(0.2130)	-0.112	0.342	0.322	40
Safety Indicators	0.166	(0.2942)	-0.080	0.566	0.567	40
Job Satisfaction & Mental Well-being	0.011	(0.3679)	0.040	0.710	0.738	40

Notes: This table reports OLS estimates of baseline differences between control and treatment groups within non-management subgroups for HTE analysis. In Panels A, B, and D, I partition the sample into above/below median subgroups using the baseline value of the variable. For location in Export Processing Zone (EPZ), I partition the sample using this variable. Each panel reports the within subgroup baseline differences for a different dimension of heterogeneity. For each outcome, I report the baseline control group mean and SD in columns (1)-(2). Column (3) reports the estimated coefficient for the treatment indicator from a regression of the outcome on the treatment indicator and stratification variables. Column (4) reports the *p*-value for the treatment indicator calculated using robust standard errors. Column (5) reports the RI *p*-value for the coefficient reported in column (3) based on 5000 draws. Column (6) reports the sample size for the regression. *p<0.1; **p<0.05; ***p<0.01.

Table C.III: Other heterogeneous treatment effects: Primary outcome index variables, pooled short- and longer-run rounds

	OSH Committeee Compliance	Safety Indicators	Job Satisfaction & Mental Well-being
	(1)	(2)	(3)
Panel A: Baseline Size			
Below median	0.234***	0.083	-0.114
	(0.0743)	(.0767)	(0.0970)
	[0.004]	[0.279]	[0.283]
Above median	0.163*	0.109	0.084
	(0.0824)	(0.0878)	(0.0930)
	[0.060]	[0.236]	[0.314]
p-val, diff	0.516	0.834	0.167
	[0.575]	[0.831]	[0.131]
Panel B: Baseline OSH Committee Compliance			
Below median	0.236**	0.098	-0.045
	(0.0967)	(0.0909)	(0.1001)
	[0.029]	[0.304]	[0.678]
Above median	0.180***	0.099	0.007
	(0.0612)	(0.0737)	(0.0827)
	[0.010]	[0.202]	[0.929]
p-val, diff	0.625	0.991	0.687
	[0.676]	[0.990]	[0.705]
Panel C: Baseline HR Management Practices			
Below median	0.129	0.024	-0.096
	(0.0827)	(0.0685)	(0.1062)
	[0.121]	[0.754]	[0.388]
Above median	0.288***	0.181**	0.064
	(0.0825)	(0.0843)	(0.0746)
	[0.002]	[0.056]	[0.388]
p-val, diff	0.187	0.148	0.231
	[0.220]	[0.208]	[0.221]
Observations	160	160	160
Factories	80	80	80
Stratification variables	Y	Y	Y
Control, base. dep. var.	Y	Y	Y

Note: This table reports OLS estimates of heterogeneous treatment effects on primary outcome index variables, pooling treatment and post-treatment rounds of data. Each outcome variable is indicated at the top of the table. Each panel reports the results for a different dimension of heterogeneity. In each panel, the "Below median" row reports the estimated treatment effect for the subgroup with below median baseline values of the heterogeneity variable. In each panel, the "Above median" row reports the estimated treatment effect for the subgroup with above median baseline values of the heterogeneity variable. The final row in each panel reports the *p*-value of the difference between the estimated treatment effects for below and above median subgroups. All regressions include stratification variables and a control for the baseline value of the dependent variable. All subgroups have 40 factories. Robust standard errors are reported in round brackets. RI *p*-values based on 5000 draws are reported in square brackets. Index variables constructed using Anderson (2008) variance-covariance weighted index. *p<0.1; **p<0.05; ***p<0.01.