Title

Essays on Working Conditions, Labor Markets, and Multinational Buyers in Developing Countries

Author

Boudreau, Laura Elizabeth

Publication Date

2019

Peer reviewed|Thesis/dissertation

Essays on Working Conditions, Labor Markets, and Multinational Buyers in Developing Countries

by

Laura Elizabeth Boudreau

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Business Administration

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Associate Professor Noam Yuchtman, Chair
Professor Paul Gertler
Associate Professor Reed Walker
Professor Ernesto Dal Bó
Professor Edward Miguel

Spring 2019

Essays on Working Conditions, Labor Markets, and Multinational Buyers in Developing Countries

Abstract

Essays on Working Conditions, Labor Markets, and Multinational Buyers in Developing Countries

by

Laura Elizabeth Boudreau

Doctor of Philosophy in Business Administration

University of California, Berkeley

Associate Professor Noam Yuchtman, Chair

Chapter 1: Western stakeholders are increasingly demanding that multinationals sourcing from developing countries be accountable for labor rights and working conditions upstream in their supply chains. In response, many multinationals privately enforce labor standards in these countries, but the effects of their interventions on local firms and workers are unknown. I partnered with a set of multinational retail and apparel firms to enforce local labor laws on their suppliers in Bangladesh. I implemented a randomized controlled trial with 84 Bangladeshi garment factories, randomly enforcing a mandate for worker-manager safety committees in 41 supplier establishments. The intervention significantly improves compliance with the labor law. It also has a small, positive effect on indicators of safety committees' effectiveness, including measures of physical safety and awareness. Factories with better managerial practices drive these improvements. In contrast, factories with poor managerial practices do not improve compliance or safety, and in these factories, workers' job satisfaction declines.

Chapter 2 (joint with Rachel Heath and Tyler McCormick): Many workers in large factories in developing countries are internal migrants from rural areas. In collaboration with Rachel Heath and Tyler McCormick, I examine the relationship between workers' migration status and the working conditions they face in a household survey of garment workers in Bangladesh. We document that migrants are in firms with higher wages but worse working conditions, but as their careers progress, they have higher mobility than locals as they move towards firms with better conditions. These facts are consistent with a model in which migrants are poorly informed about working conditions upon beginning work but learn more as they gain experience in the industry.

Chapter 3: I test for the presence of compensating wage differentials for factory building safety in Bangladesh's apparel sector. I find no evidence in support of a compensating wage differential for building safety. This descriptive fact is not explained by heterogeneity in workers' or factories' observable characteristics. Instead, I show that workers have incomplete information about factories' compliance with building safety standards, which I argue is difficult for workers to observe. Workers at high compliance factories un-

derestimate their factories' performance on building safety audits, while workers at low compliance factories dramatically overestimate their factories' performance. I implement a pilot field experiment with 308 garments workers in which I randomly intervene to provide workers with information about their factories performance on the audits relative to other factories nearby. The information causes workers to correctly update their beliefs about their factories' safety. It reduces turnover among workers at high compliance factories. Among workers at low compliance factories, turnover is unaffected, but workers are less likely to make referrals to their factory.

To my parents, to my husband, and to the rest of my family.

Contents

List of Figures

List of Tables

Acknowledgments

I have received a great deal of support throughout the researching and writing of this dissertation. I would first like to thank my advisors. The chair of my dissertation committee, Dr. Noam Yuchtman, provided invaluable guidance at every stage of my doctoral studies. My other committee members, Dr. Ernesto Dal Bó, Dr. Paul Gertler, Dr. Ted Miguel, and Dr. Reed Walker, also provided wonderful guidance and insights throughout this process.

I would also like to acknowledge my collaborators on the second chapter of my dissertation, Rachel Heath and Tyler McCormick. Our collaboration helped me to develop my research interests and to generate new research ideas.

I would also like to thank other faculty members at the Haas School of Business and at the University of California, Berkeley (hereafter, UC-Berkeley) who provided excellent feedback throughout my dissertation. In particular, I thank the faculty of the Business and Public Policy (hereafter, BPP) Group at the Haas School of Business and a broader group of faculty at UC-Berkeley, including Fred Finan, David Levine, Jeremy Magruder, and Supreet Kaur.

All three chapters of my dissertation benefited from the financial support of several sponsors. The first chapter is funded by the Alliance for Bangladesh Worker Safety, 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. The second chapter benefited from funding from the International Growth Centre (Small Project Facility Grant) and the National Institutes of Health. The third chapter received funding from the Independent University, Bangladesh (IUB), the Center for Effective Global Action (CEGA) Behavioral Health Grant, and the Berkeley-Haas Center for Responsible Business (CRB). I would also like to thank CEGA for its support for my and other graduate student research in development economics at UC-Berkeley.

In addition, I would like to thank the doctoral students of the BPP Group at Haas. You provided me with invaluable feedback and friendship over the course of multiple years of seminar presentations.

Finally, I would like to thank my family and other friends. My parents have provided me with endless encouragement over the course of my doctoral degree. My husband has been my partner in life and in pursuing a doctoral degree. My sister and other family members have been incredibly supportive. My friends have been fantastic listeners and companions on adventures both big and small.

Chapter 1

Multinational enforcement of labor law: Experimental evidence from Bangladesh's apparel sector

1.1 Introduction

In developing countries, governments often lack the capacity or the political will to update and to enforce regulation (Dal Bó and Finan 2016), including labor regulation. For example, in a 2018 global ranking of labor law and enforcement, 65% of developing countries were found to systematically violate or to provide no guarantee of rights to workers (International Trade Union Confederation 2018).[1] In response, many Western multinationals sourcing from developing countries privately enforce local labor laws on their suppliers through "Corporate Social Responsibility" (CSR) programs (O'Rourke 2014).

In a globalized production system, such CSR programs could in principle provide an important source of enforcement in countries with weak state capacity. It is an open question, however, whether multinationals have the incentives and the capabilities to improve labor standards in developing countries. On one hand, multinational buyers have incentives to prevent industrial disasters in supplier establishments that could pose reputational risks (Tadelis 2002; McDevitt 2011). On the other, enforcement of labor laws may increase labor costs, which suggests that without effective monitoring, multinationals' promises to improve standards may not be credible (Besley and Ghatak 2007). Finally, even if multinationals are motivated to improve labor standards, it's unclear whether they have sufficient bargaining power and monitoring capacity to influence suppliers' practices (Short, Toffel, and Hugill 2016).

Further, if private enforcement of labor law can improve targeted establishments' compliance, it raises the question of whether it generates net benefits or costs to these establishments and to their workers. Existing empirical evidence provides grounds for concern. Botero et al. 2004 examine labor regulation in 85 countries; they show that heavier

[1] 28% of high-income countries included in the report were found to systematically violate or to provide no guarantee of rights to workers (International Trade Union Confederation 2018). Most of these are located in the Middle East.

labor regulation is associated with lower labor force participation and higher unemployment. Besley and Burgess 2004 compare labor regulation across Indian states and find that pro-worker regulation is associated with eroded firm competitiveness and increased urban poverty. Understanding the relationships among labor regulation, firm competitiveness, and worker well-being is of fundamental policy importance in developing countries, but the lack of available causal evidence limits our ability to make informed recommendations.

This paper provides the first experimental evidence on the effects of private enforcement of labor law in a developing country where government enforcement is lacking. I partner with a set of multinational retail and apparel firms, known as the Alliance for Bangladesh Worker Safety (hereafter, the Alliance), that aims to improve the safety performance of its shared Bangladeshi supplier base. The Alliance's membership includes 29 multinational retail and apparel firms representing the majority of North American imports from Bangladesh (e.g., Wal-Mart, Gap, Target).[2] In conjunction with the Alliance, I implemented a randomized controlled trial (RCT) in which I randomly assigned supplier factories to the Alliance's enforcement of a local labor law that requires factories to have worker-manager safety committees (SCs). I estimate the intervention's effects on suppliers' compliance with the labor law and their SCs' effectiveness at improving safety. I also assess the intervention's effects on suppliers' productivity, wages, and employment and on their workers' well-being.

The RCT was implemented over 2017-2018 as part of the Alliance's roll-out of its SC Program. It involved 84 garments and garments-related factories in Bangladesh. The Alliance's SC Program is the "treatment" in this RCT. It is a 6-month enforcement intervention that aims to bring factories into meaningful compliance with Bangladesh's SC law. I randomly assigned 41 factories to immediate participation in the Program (treatment group) and 43 factories to deferred participation in the Program approximately 11 months later (control group).[3] A five-member research team made three full-day visits to factories. The team collected a pre-intervention baseline, a post-intervention round about 5 months later, and a second post-intervention round about 9 months after baseline (see Figure 1.1). For treatment factories, the 5-month data collection visit occurred toward the end of the 6-month enforcement program. I also implemented a retrospective questionnaire to collect production, HR, and other business-related data. Finally, the Alliance provided its own monitoring and administrative datasets. The consolidated datasets are unique in their comprehensiveness and depth. I analyze them according to a pre-analysis plan (PAP), which is registered on the American Economic Association's Social Science Registry.

---

[2] 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; Hudsons 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; Wal-Mart Stores, Inc.; and YM Inc.

[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 naturally aligns with the Alliance's standard operating procedures.

In this draft, I present evidence on the effects of the multinationals' enforcement intervention, measured during the intensive six-month enforcement period. For three of my six primary outcomes, I report final results, and for three, preliminary results. In the spring of 2019, I will finalize the results for factory productivity, employment, and wages, which are the outcomes for which preliminary results are available. I will also test whether the treatment effects persist beyond the period of intensive compliance monitoring using the data from the third round of onsite data collection.

I find that the multinationals' enforcement program significantly increases factories' compliance with Bangladesh's SC labor law, which I measure using a pre-specified index of compliance outcomes. The intervention improves factories' compliance-related outcomes by 0.19 standard deviations (sds) on average. Most factories begin with SCs that are formed correctly but largely inactive. The intervention significantly increases their level of activity; for example, they begin to meet more frequently and are nearly four times more likely to conduct risk assessment. This increase in compliance translates into a statistically significant improvement in a pre-specified index of factory safety indicators. Treatment factories outperform control factories on this index by 0.14 sds on average. This improvement is driven by a statistically significant improvement in treatment factories' performance on an independent spotcheck of safety conditions by the research team. The intervention also improves workers' awareness of the SC.

These findings demonstrate that private enforcement of labor law can significantly improve compliance and contribute to achieving the law's objectives. They provide reason for greater optimism regarding the ability of private enforcement to improve labor standards in developing countries compared to the existing research on private regulation in global supply chains in political science, which is largely skeptical but which lacks causal evidence (Richard M Locke, Qin, and Brause 2007; Richard M Locke, Amengual, and Mangla 2009; Richard M Locke and Romis 2010; Richard M. Locke 2013; Distelhorst, Richard M Locke, et al. 2015; Toffel, Short, and Ouellet 2015). It also provides the first experimental evidence that firms' CSR initiatives can successfully generate public goods/curtail public bads. This finding joins Dragusanu and Nunn 2018 in beginning to build a body of empirical evidence on the efficacy of CSR. It also provides empirical justification for theoretical models of CSR that identify CSR with the private creation of public goods or curtailment of public bads, such as those of Besley and Ghatak 2007 and Lai et al. 2017.

Contrary to my hypothesis, the enforcement intervention significantly decreases workers' job satisfaction. Treatment factories' performance on an index of job satisfaction and mental well-being is -0.15 sds lower than controls'. The index includes self-reported measures of job satisfaction and mental well-being as well as revealed preference measures including absenteeism and turnover. The decline is driven by a reduction in self-reported measures related to job satisfaction. Worker absenteeism also increases at treatment factories relative to controls. Estimated treatment effects from my preferred specification are approximately 0.5 percentage points, which is an increase of about 12%. The third round of data collection will elucidate whether these negative effects on workers are temporary or longer-run in its nature.

Evidence on supplier competitiveness, including productivity. wages, and employment, do not provide conclusive evidence of adverse effects. Preliminary estimated treatment effects on productivity are mostly negative but are not statistically significant and are generally small in magnitude. Estimated treatment effects on wages are similarly negative but small and not statistically significant. Finally, estimated treatment effects on employment are close to zero. Together, the analysis suggests that the intervention significantly improved safety without large, negative effects on suppliers competitiveness. Taken together, the negative estimated treatment effects on productivity and wages suggests that the multinationals' enforcement intervention may generate small productivity costs to factories, which in turn may slightly reduce workers wages, at least in the short-term. Extending the regression panel to incorporate three additional months of data on these outcomes will shed some light on the dynamic nature of these estimates. The productivity results will also be updated.

To further unpack the results, I analyze pre-specified dimensions of heterogeneity. In particular, I consider the role of suppliers' organizational capacity. The existing research on improving adherence to regulation in developing countries focuses on the effects of strengthening state-supplied regulation and enforcement in order to increase firms' incentives for compliance (Duflo et al. 2013; Duflo et al. 2014; Dal Bó and Finan 2016). There has been little to no consideration, however, of whether the organizational capacity of the private sector also contributes to constraining regulatory efficacy. Bloom, Christos, et al. 2010 and Distelhorst, Hainmueller, and Richard M Locke 2017 document positive associations between managerial practices and environmental and social performance, respectively, but this mechanism's importance for improving firms' compliance has not been explored.

I find that factories' baseline managerial practices are an important factor in determining the enforcement intervention's effects.[4] The treatment has large, positive effects on compliance and on measures of SC effectiveness in factories with better baseline managerial practices. In contrast, factories with worse practices do not significantly improve their compliance or measures of SC effectiveness. These results suggest that there may be complementarity between labor regulation and managerial practices. Increasing compliance with labor regulation may depend not only on providing firms with appropriate incentives, but also on their capacity to respond to these incentives.

Further, the negative effects on workers are more pronounced in less well-managed factories. I continue to investigate possible mechanisms for this result. For now, I provide suggestive evidence in favor of a mechanism in which the intervention raises workers' expectations about what SCs will deliver, and in poorly-managed factories, these expectations are not met. This effect would be consistent with recent experimental evidence on low-wage workers' response to an upgrade in employer-provided housing from Adhvaryu, Nyshadham, and H. Xu 2018. While objective measures of housing quality improve, workers' expectations for the improvement are not met, and workers' job satisfaction declines and turnover increases.

This research makes four primary contributions. First, this paper contributes to the literature on labor regulation and economic development, and in particular, their interaction with global trade. Several studies have found that heavier *de jure* labor regulation

[4]Due to power limitations, I am largely unable to reject that the estimated treatment effects are different for the two groups.

is associated with worse economic performance and adverse consequences for workers (Botero et al. 2004; Besley and Burgess 2004; Aghion et al. 2008; Fishback and Kantor 1996). We also know, however, that weak state capacity and political capture by elites results in socially suboptimal enforcement quality in many developing countries (Duflo et al. 2013; Dal Bó and Finan 2016). Fisman and Y. Wang 2015, for example, show that workplace deaths at politically-connected Chinese firms are two to three times higher than at unconnected firms and that this relationship is best explained by firms using political connections to bypass safety regulations. Recent experimental research in Ethiopia finds that forms of industrial work common in developing countries have large, negative health impacts on workers with no compensating increases in income (Blattman and Dercon 2018a). Scholars have raised supply chain linkages as a possible mechanism to bring about improved regulation and enforcement. For example, Harrison and Scorse 2010a show that anti-sweatshop campaigns led the Indonesian government to raise minimum wages, which resulted in a large real wage increases with some costs for firms but no significant effects on employment. This is the first study, however, to test the potential for private enforcement of regulation in a context where state enforcement is lacking. I further contribute by identifying the causal effects of enforcement on factories' productivity and on workers' well-being. Finally, my results suggest an under-explored constraint on regulatory efficacy: Organizational capacity of the private sector.

Second, it contributes to a burgeoning literature on the economics of CSR. Economists have long espoused the Friedman 1970 view that markets should produce private goods and governments should provide public goods and correct failures. Recent theoretical and empirical work, however, highlights two primary reasons why this dichotomy may blur. First, there is significant evidence that governments, particularly in developing countries, frequently fail to fulfill their aforementioned roles; further, governments' jurisdiction is limited to their territories, and they are often constrained in their ability to police production abroad (Besley and Ghatak 2007; Bénabou and Tirole 2010; Dal Bó and Finan 2016). Second, consumers, shareholders, and workers have social and ethical motivations and often value production that occurs socially and environmentally responsible ways (Besley and Ghatak 2007; Dragusanu, Giovannucci, and Nunn 2014; Hainmueller, Hiscox, and Sequeira 2015; Burbano 2016; Hart and Zingales 2017). The existing economic literature on CSR primarily provides the theoretical and empirical bases for its existence and desirability. A recent exception is Dragusanu and Nunn 2018, who consider its efficacy; they show that Fair Trade certification is associated with higher incomes and improved educational outcomes for Costa Rican coffee farmers' families. I study a group of multinationals' CSR program that aims to improve suppliers' compliance with local labor law by leveraging possible monopsonistic power. I provide the first experimental evidence on an important, largely open question: Does private enforcement of *de jure* regulation achieve its stated objective, which, in this setting, is to improve safety?[5] If

[5] An interesting literature spanning political science and management science asks related questions. It largely concludes that multinationals' private regulation programs are generally ineffective at improving compliance and that local context is the main predictor (Richard M Locke, Qin, and Brause 2007; Richard M Locke, Amengual, and Mangla 2009; Richard M Locke and Romis 2010; Richard M. Locke 2013; Distelhorst, Richard M Locke, et al. 2015; Toffel, Short, and Ouellet 2015). As acknowledged in this literature, though, it suffers from a lack of causal identification and a lack of data access. It relies on cross-country and cross-

so, what are the economic consequences?

Third, this paper contributes to the literature on collective worker voice and intra-firm institutions. Economists have long been interested in how increasing workers' collective voice in firms' decision-making impacts firms' economic performance and workers' welfare. The empirical literature on this topic, however, has generally suffered from selection bias, and available causal evidence is limited to marginal firms (Addison, Schnabel, and Wagner 2001; DiNardo and David S Lee 2004; David S. Lee and Mas 2012; Yao and Zhong 2013). My experimental setting improves on previous research by enabling identification of causal effects that are not local to marginal firms. In so far as Western multinationals are driving the effort to increase worker voice, this research also shares commonalities with the literature on Western attempts to introduce Western governance institutions in developing countries (e.g., Casey, Glennerster, and Miguel 2012 and Humphreys, Sánchez de la Sierra, and Van der Windt 2017).

Finally, this paper contributes to the literature on occupational safety and health (OSH), and in particular, on OSH committees. While most of this literature is limited in its ability to make causal statements, Levine, Toffel, and M. S. Johnson 2012 is a recent exception. The authors use a natural experiment in OSH inspections by California state regulators to show that inspections significantly reduce injury rates and costs without negatively affecting establishments' competitiveness. The literature on OSH committees generally examines correlations among the presence and features of OSH committees and injury rates or stakeholder satisfaction with them (see Yassi et al. 2013 for a thorough review). My contribution is to randomize enforcement of OSH committees to provide causal evidence of their effects on factory safety. I also identify complementarity between HR management practices and OSH effectiveness. Further, my main measure of SCs' effectiveness does not rely on injury rates, the reporting of which may be impacted by the treatment. Instead, I use indicators of factory safety, such as performance on spotchecks of factory safety conditions, to measure SCs' effectiveness. I will complement this analysis with an analysis of injury data, which I collect from medical clinic records, workers, and factory management.

The remainder of this paper is organized as follows: Section 1.2 describes the context, including the Alliance and the SC Program. Section 1.3 presents the research design. Section 1.4 presents the preliminary results. Section 1.5 concludes.

1.2 Background

Bangladesh's garments sector

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). Multinational buyers rely on Bangladesh for its combination of low prices and large production capacity (McKinsey & Company 2011).[6]

supplier comparisons that are subject to various forms of omitted variables bias.

[6] A Chief Procurement Officer (CPO) of a major Western retail firm states it simply in a 2011 McKinsey survey, "There is no alternative to Bangladesh."

Apparel is also a critical sector for Bangladesh's economy. Bangladesh is one of the most rapidly industrializing countries in the world (Central Intelligence Agency 2016), and the garments sector has been and continues to be the major driver of its industrial transformation. In 2016, apparel exports constituted 81% of Bangladesh's total exports and 13% of its Gross Domestic Product.[7] The sector directly employs between 4-5 million of Bangladesh's 66.6 million workers.

Bangladesh has been infamous for its weak legal protections for workers, for its lack of enforcement of regulation, and for its low minimum wages for many years.[8] In a 2011 McKinsey survey of western buyers, for example, buyers list lack of social compliance and economic and political instability as two of the top five major risks to sourcing from the country (McKinsey & Company 2011). 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 (see Figure 1.2), that killed at least 1,273 workers and injured at least 3,812 workers at exporting factories (Solidarity Center 2016). In the aftermath of these events, world leaders rebuked the Government of Bangladesh (GoB) for "not taking steps to afford internationally recognized worker rights to workers in that country," and some western governments penalized the country by removing trade benefits (Greenhouse 2013a).

Government and buyer response to the Rana Plaza collapse

Following the collapse of the Rana Plaza Building, the GoB and multinational buyers faced intense political and activist pressure to ensure workers' safety and basic rights. The GoB promised to introduce labor reform and to work with the International Labor Organization (ILO), buyers, factory owners, and worker organizations to prevent another tragedy. European buyers quickly moved to sign an agreement between buyers and labor unions to improve safety and health in Bangladesh's garments sector. This coalition is known as the Accord on Fire and Building Safety in Bangladesh (hereafter, the Accord). Several U.S. retailers refused to sign on to the Accord due to the participation of labor unions and the requirement that buyers are subject to legally-binding arbitration (Greenhouse 2013b; Bhattacharjee 2013). A group of U.S. retailers, led by Gap and Wal-Mart, formed the Alliance for Bangladesh Worker Safety (hereafter, the Alliance) shortly thereafter. I provide more information about the Alliance in the next subsection.

In July 2013, the GoB amended the labor law to improve workplace safety and to strengthen some freedom of association rights. The requirement that factories with 50 or more workers create worker-manager SCs is a key safety provision of the amendment. The GoB also agreed to a a multi-stakeholder action plan to improve safety conditions. The plan includes strengthening the GoB's labor inspection capacity, building and fire safety audits and remediation of the full garments-related sector, safety training, and numerous other actions (Ministry of Labour and Employment 2013). To fulfill the action plan, the GoB is closely coordinating with the ILO, the Accord, and the Alliance. The

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

[8] Garment sector jobs are not without benefits to Bangladeshi society. Heath and Mobarak 2015a, for example, show that the growth in these jobs contributed to decreasing fertility, increasing age at marriage, and increasing educational attainment among Bangladeshi girls in recent decades.

Accord and the Alliance are responsible for overseeing safety for the 60-70% of the sector that they cover. The GoB, with the ILO's support, is responsible for the remaining 30-40% of the sector (International Labor Organization 2017).

The GoB published implementation rules for the SC provision on September 15, 2015. These rules articulate the specific requirements for SCs' formation, operations, and responsibilities. Table 1.1 summarizes key aspects of the requirements. Factories had six months from this date to form and operationalize their SCs. Despite the *de jure* requirement that establishments form and implement SCs, *de facto*, enforcement of the regulation was low. According to an International Labor Organization 2017 report, from 2015-2017, the GoB's focus was primarily on physical safety remediation of garment factories. As of mid-2017, the ILO had supported the GoB to form SCs at 210 of the 1,549 garment factories under the government's purview (i.e., not including Alliance or Accord-covered factories). Unsurprisingly, compliance with the regulation was also low. The title of a news article from late 2017 summarizes the status, *"Half of all apparel units flout needs for safety committees"* (Munni 2017). The article, based on an internal government report, describes the sector's low level of compliance with the regulation.

The Alliance & the SC Program

The Alliance is a coalition of 29 multinational retail and apparel firms (e.g., Wal-Mart, Gap, Target, Costco), which are displayed in Figure 1.3. The Alliance's members represent the majority of North American garment imports from Bangladesh. They committed to a five-year agreement to improve the safety performance of their Bangladeshi supplier bases. The Alliance covers approximately 700-800 garment factories and 1.21 million workers in Bangladesh.[9] In 2013, the GoB, the Accord, and the Alliance formally agreed to share the building safety audit and remediation oversight responsibility for the industry for the Accord and the Alliance's five-year terms (International Labor Organization 2017). The Accord and the Alliance are also enforcing other relevant aspects of Bangladesh's regulations, including the labor law requiring the establishment of SCs.

The Alliance requires all factories in its supplier base to participate in its building safety audit, building remediation, and worker training and empowerment programs. Failure to comply with one or more of these programs results in suspension from the Alliance's supplier base; as of July 2018, the Alliance had suspended 168 factories. The Alliance is also a member of a Private sector-GoB Factory Closure Panel for cases of imminent danger due to structural integrity, which has fully or partially closed 35 factories that supplied to the Alliance.

The Alliance is requiring its suppliers to comply with the 2013 labor law amendment stipulating the establishment of SCs. The Alliance's intervention to enforce the law is its SC Program. This program is the treatment in this study. The SC Program:

1. If necessary, reestablishes SCs through compliant formation processes;
2. Trains SCs' members on their roles and responsibilities, on safety and health topics, and on leadership and communication skills;

[9] More details on the Alliance and its members are available on its website: www.bangladeshworkersafety.org.

3. Raises awareness of workers and managers on the roles and responsibilities of the SC;

4. Requires SCs to complete a series of activities required by law; and

5. Monitors SCs.

The first four activities occur over a period of 3.5 months. The final, intensive monitoring activity continues until six months after the initial engagement. The Alliance then continues to monitor SCs under its general monitoring activities. The Alliance is implementing the SC Program with 200-250 factories that are covered by the Alliance but not covered by the Accord; this is because the Accord is also implementing a SC Program with all of its suppliers, including those that are also covered by the Alliance. The Alliance provides information about its SC Program, including many of the materials used in the program, on its website.

The RCT was built into the Alliance's roll out of the SC 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. Further, for data collection, the Alliance requested factories to cooperate with the research team as part of its general impact evaluation efforts for its programs – factories were not told that the research team was specifically interested in the SC Program.

1.3 Research design

Randomized assignment to the SC Program

This study's goals are first to identify the efficacy of multinationals' efforts to enforce local labor on their suppliers, and second to identify how a change in enforcement affects suppliers' competitiveness and workers' well-being. I address these questions through random assignment of 41 Alliance-covered factories to participate in the SC Program, which entails the Alliance's enforcement of Bangladesh's SC regulation, and 43 Alliance-covered factories serve as controls.[10] The 84 factory sample is drawn from the population of SC Program-eligible supplier establishments. In order to be eligible, factories must have a separate committee that is formed in compliance with Bangladeshi labor law.[11] In most factories, this committee is the Participation Committee (PC), and it is responsible for appointing worker representatives to the SC.[12] Worker representatives on the PC must be elected through free, fair, and competitive elections. In order to be eligible for the SC Program, the Alliance must verify a factory's election process. Often, the brand(s) sourcing from the factory must oversee a new election. Once the Alliance verifies that the PC

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

[11] If a factory has a trade union, then it selects the worker representatives to the SC. Few garments factories in Bangladesh have trade unions. In the 84-factory sample, only two have trade unions.

[12] PCs are legally required for all factories with 50 or more workers located outside of Export Processing Zones (EPZs). EPZ factories are subject to different labor laws. The Alliance implements an analogous process with these factories. The worker representation structure in EPZs is called a Workers' Welfare Association (WWA).

formation process is compliant, a factory becomes eligible for the SC Program.

From January through December 2017, every time the Alliance had a batch of factories that it verified as eligible, it sent the list to me. Within batch, I randomly assigned 50% of factories to the treatment condition and 50% to the control condition. The result is a stratified randomized experiment with six strata, where each strata is a batch of factories. In 11 cases in which multiple factories shared ownership and location (shared building or compound), I randomly selected one factory to participate in the RCT.[13] All other factories at the same location were non-experimental but shared the assignment status of the randomly-selected factory.

Table 1.2 shows baseline balance between control and treatment groups. The sample in each row includes all 80 factories unless otherwise stated. For wages, eight factories were unwilling to provide gross wage data to the research team. For TFP, the sample size will be updated when these results become available. The randomization successfully generated two groups that are well-balanced along observable characteristics. There is a treatment factory that is a negative outlier on worker survey measures. It pulls down the treatment group mean on outcome measures based on worker survey data; these differences are attenuated and no longer statistically significant when this factory is dropped. The results are robust to controlling for the baseline value of the dependent variable and to dropping the outlier factory. Appendix B presents baseline balance and the main results after dropping this factory.

Data collection and measurement

This analysis uses three main sources of data. First, it uses several types of data collected during three day-long visits to factories implemented over nearly one year. Second, it uses monthly production, human resource, and other business performance-related data collected using a retrospective questionnaire administered following the final data collection visit. Third, it uses numerous types of administrative data from the Alliance. The data collection for this project is unique, as the research team had access to factories that likely would not opt into this type of research without the Alliance's requesting their cooperation.

A five-member research team visited factories three times. The 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 SC President, two randomly selected SC worker representatives, 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 factory documentation. It also included photographing factory records for later digitization by the research team. The spotchecks of safety conditions entailed a trained assessor visiting the factory production floor and checking physical safety conditions against a checklist. They were only conducted at the second and third visits. The team leader was an assessor, who was responsible for managing interactions with management, verifying factory

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

documentation, and implementing the safety spotchecks of the factory. A junior assessor supported the team leader. The junior assessor oversaw the the survey process, photographed factory records, and supported survey implementation. Three enumerators implemented surveys.

The first visit established factories' baselines. The second visit, approximately five months after baseline, aimed to measure outcomes immediately after treatment factories completed the most intensive portion of the SC Program. The third visit, approximately 10 months after baseline, aimed to measure outcomes several months after treatment factories completed the SC Program. To minimize experimenter demand effects specific to the SC Program, the research team undertook extensive effort to design protocols for onsite visits to minimize the risks of non-truthful reporting and manipulation of data collection. For example, the research team verified several types of safety-related documentation, such training documents for other Alliance programs and inspection logs for factory machinery, in addition to documentation related to the SC.

The bottom of Table 1.2 presents baseline balance checks for characteristics of randomly selected worker survey participants at treatment and control factories. Workers at treatment and control factories are balanced on observable characteristics.

Figure 1.1 displays the experiment's timeline. As the timeline makes clear, for treatment factories, the onsite data collection is timed such that the intensive 3.5-month treatment phase is completed prior to the second visit. The second visit occurs during the intensive 2.5 month monitoring phase. The third visit is completed approximately four months after treatment factories complete the 6-month SC Program. Control factories do not participate in the SC Program until they have fully completed all activities related to the experiment.

Outcome variables

I pre-specified six primary outcome variables in my PAP. They are:

1. Compliance with Bangladesh SC Regulation (index);
2. SC effectiveness (index);
3. Worker job satisfaction and mental well-being (well-being index);
4. Total Factor Productivity (TFP)
5. Employment;
6. Wages.

Primary outcome variables 1-3 are standardized index variables that are weighted averages of multiple outcomes. I use summary index variables for these outcomes because these are multi-dimensional, multi-measure outcome categories. I aim to capture the intervention's general effect on a set of outcomes related to compliance and SC effectiveness. I would also otherwise need to run many hypothesis tests to test for effects on all of these outcomes. I reduce the risk of overrejection of the null hypothesis by summarizing these outcomes using index variables. Finally, I increase my ability to detect marginally statistically significant effects on multiple outcomes that, aggregated, achieve statistical significance.

For primary outcome 1, compliance with the SC Regulation, I constructed the index by coding the GoB's requirements for SCs enumerated in the 2015 rules issuance. For primary outcome variable 2, I constructed an index of variables that aims to measure SCs' effectiveness at fulfilling the intent of the law, which is to improve factory safety, to increase workers' awareness of safety, and to engender a culture of safety at the factory (with the overarching goal of reducing worker injuries and illnesses). The index includes objective indicators of factory safety, including data from spotchecks of physical safety conditions, as well as survey measures of workers' and senior managers' awareness of the SC. Ideally, I would directly measure SCs' effects on worker injuries and illnesses. The intervention, however, aims to empower workers to raise safety issues and concerns. Consequently, it may result in a net increase reported injuries and illnesses even if the actual number of occurrences decreases.[14] For the well-being index, I construct it using worker survey questions and administrative data on worker turnover and absenteeism. Appendix Figures A1-A3 list all of the variables included in the three primary outcome index variables.

To construct the index variables, I follow Casey, Glennerster, and Miguel 2012 and Haushofer and Shapiro 2016 in using the methodology proposed by Anderson 2008 based on O'Brien 1984. Anderson's approach 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. I accommodate the panel nature of the data by pooling all control group observations across periods when calculating the variance-covariance matrix.

I also pre-specified secondary outcome variables that will allow me to analyze possible mechanisms underlying the effects on my primary outcome variables. For example, I have multiple secondary outcome variables for workers, including workers' perception of their SCs' effectiveness, workers' empowerment to raise safety and other issues, and workers' reported awareness of other types of worker organizations. I do not analyze secondary variable in this draft of the paper but I will incorporate them into the paper in the near future.

I measure TFP assuming a constant returns-to-scale, Cobb-Douglas production technology and competitive output and input markets. I proxy for output elasticities in the Cobb-Douglas production function using industry cost shares, which I obtain from Bangladesh's Survey of Manufacturing Industries (2012). This approach is common in the literature on productivity measurement (Syverson 2011), and it has been used in a recent RCT on firm productivity in India by Bloom, Eifert, et al. 2013. I measure output as physical quantities, labor as person-hours, capital as the number of production machines, and materials as physical material inputs. In the case of capital, I use number of

[14] I am in the process of transcribing factories' injury and medical clinic records. While these are still subject to the same reporting concerns, they are the most comprehensive source of information available. They will be added to the analysis when they become available.

production machines as a proxy for overall capital inputs. For the typical factory, TFP is defined as log(output)-(labor share)*log(man hours)-(materials share)*log(materials)-(1-labor share-materials share)*log(machines). For multi-product and integrated factories, of which there are several in the sample, the definition changes somewhat. See Appendix 3.8 for a detailed explanation of how I define TFP.

Calculating TFP for factories that use multiple types of machinery and/or multiple types of materials is complicated by the fact that I lack data on input-specific cost-shares or output elasticities (i.e., I have an overall capital (materials) elasticity). I measure TFP three ways:

1. Base TFP Measure: Treats all capital (material) inputs as perfect substitutes.
2. TFP Measure 2: Modifies how material inputs enter the production function (see Appendix 3.8 for details).
3. TFP Measure 3: Modifies how capital inputs enter the production function (see Appendix 3.8 for details).

Econometric analysis

Regression models:

I estimate the intervention's average treatment effects using three simple regression models. For the main analysis, I use the following regression model:

$$Y_j = \alpha + \beta\, T_j + \theta\, Y_{j,t=0} + \gamma_j + \epsilon_j \tag{1.1}$$

where Y_j is the outcome of interest 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_j is a stratum indicator, and ϵ_j is the residual. In this model, β is the coefficient of interest. I show results with and without controlling for the baseline value of the dependent variable. All of my statistical tests are two-sided.

For business competitiveness outcomes, which I measure using monthly administrative data, I also show panel regression results. I use the following panel regression model:

$$Y_{jt} = \alpha + \beta_1\, T_j + \beta_2\, Post_t + \beta_3\, T_j * Post_t + \delta_t + \lambda_j + \epsilon_{jt} \tag{1.2}$$

where Y_{jt} is the outcome of interest for factory j at time t. T_j is the treatment indicator. $Post_t$ is a post-intervention indicator equal to 1 when $t > 0$, where $t = 0$ is the baseline data collection month, and otherwise equal to 0. δ_t are calendar month fixed effects and λ_j are factory fixed effects. Note that the treatment indicator, T_j drops from estimation due to the inclusion of factory fixed effects. ϵ_{jt} is the residual, which is clustered by factory. I show results with and without the calendar month fixed effects. In this model, β_3 is the coefficient of interest.

To test for heterogeneous treatment effects, I use the following regression model:

$$Y_j = \alpha + \beta_1\, T_j + \beta_2\, R_j + \beta_3\, T_j * R_j + \theta\, Y_{j,t=0} + \gamma_j + \epsilon_j \tag{1.3}$$

where R_j is an indicator for above median baseline value of a pre-specified interaction variable. The notation for equation 1.3 is otherwise analogous to that for equation 1.1. 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 estimated difference between these two treatment effects. In the heterogeneity analysis, I report β_1 and $\beta_1 + \beta_3$ as well as the p-value for β_3.

Statistical inference:

For statistical inference, instead of using the traditional sampling-based approach, I use randomization inference. Randomization inference is increasingly the recommended way to analyze data from RCTs, in particular for small samples (Athey and Imbens 2016; Young 2015; Heß 2017).

In addition to using summary index variables for multi-measure outcome categories, I also show multiplicity-adjusted p-values. Specifically, across my primary outcome variables, I control the familywise error rate (FWER) - the probability of even one false rejection - using the methodology proposed by List, Shaikh, and Y. Xu 2016. As per my PAP, for sub-index results and secondary outcome variables, I show the individual RI p-values and False Discovery Rate (FDR)-sharpened p-values. FDR-adjusted p-values control the expected proportions of rejections that are false positives. They are less conservative than FWER-adjusted p-values and better suited to analysis of mechanisms, which is the purpose of analyzing these variables (Anderson 2008). This approach is consistent with recent empirical work with index outcome variables (e.g., Haushofer and Shapiro 2016).

Treatment compliance and attrition

Three treatment factories did not receive treatment by the second data collection visit. One of these did not participate due to a critical member of management being on an extended leave of absence at the time that the factory was due to begin. The other two factories are located in the Chittagong Region of Bangladesh, where the Alliance implements the SC Program in batches to ensure cost effectiveness, and it did not have a sufficient number of factories to implement it with these factories. Once we identified this issue, we resolved it for other factories that could have been impacted. A fourth factory began the SC Program less than two weeks before its second round data collection visit. All other factories complied with the treatment. I address the non-compliance issue by presenting Intent to treat (ITT) estimates. I will also present a full set of Local Average Treatment Effect (LATE) estimates, or the effect of treatment on the treated, in the next version of this paper. In this draft, Appendix D Table D1 presents LATE estimates for the primary outcome index variables.

Four factories have attrited from the sample. Two are treatment factories, and two are control factories. Three of the four were suspended by the Alliance due to their failure to make progress with physical building safety remediation. One control factory refused to participate in the second onsite visit. I address attrition by reporting David S. Lee 2009 bounds on the treatment effects. In this draft, I report Lee bounds for the three primary outcome index variables (Appendix Table A2). I will add Lee bounds for other outcomes in a future draft. For all three variables, there is minimal difference between the upper and lower bounds of the treatment effects, and with the exception of the lower bound for the SC effectiveness index, all estimates are statistically significant at the 5% or 10% level.

1.4 Results

This section presents the intervention's effects during the treatment phase, measured at the second onsite data collection visit. It also presents preliminary results on the intervention's short-run effects on supplier establishments' business competitiveness. In the spring of 2019, the remaining competitiveness data and the third-round visit data will become available. The results will be updated accordingly.

Factory compliance with Bangladesh's SC Regulation

How compliant are factories at baseline?

As Table 1.1 summarizes, Bangladesh's SC regulation includes three types of requirements: Requirements for how SCs are formed, for how they operate, and for their responsibilities. Before a factory begins the SC Program, the Alliance aims to verify that a factory's SC has been formed correctly. Specifically, it conducts verification visits to check whether factories' SCs are formed correctly; the Alliance also works with its members to verify that the bodies responsible for nominating worker representatives to the SC are democratically elected. When a factory begins the SC Program, the Alliance again checks that the SC is formed correctly and reforms it if necessary.

For this study, factories needed to be eligible for the SC Program in order to participate. Consequently, all factories in the sample have a SC at baseline, at least on paper. According to the labor law, factories were supposed to establish SCs by March 15, 2016; 20% of factories met this requirement. The median factory formed its SC in November 2016, although formation dates range from October 2015 to December 2017. Relative to its participation in baseline data collection, the median factory established its SC about 5.7 months prior to baseline, although relative formation dates range from less than one month to over two years prior. All factories maintained written lists of SC members, and most SCs were of the correct size and composition.[15] There was also high consistency between factory documentation and SC presidents' reports of SC size and composition ($\rho = 0.94$). Compliance was worse for requirements for democratic selection of worker representatives: 19% of SC presidents and 41% of worker representatives reported non-

[15] In one control factory, the SC was found to be comprised only of managers. In this case, compliance index outcomes related to correct formation of the committee are coded as non-compliant. At the second visit, the same factory provided the names of workers whom it indicated were members of the SC. Through the SC worker representative survey, it emerged that these workers were not members of the SC. Management had instructed them to participate because the composition of SC remained all managers. Again, the compliance index outcomes related to correct formation of the committee are coded as non-compliant.

compliant selection procedures (mainly, selection by management) or did not know how worker representatives had been selected.

While all factories had formed SCs, there was much more variation in the extent to which factories had operationalized them. In 8% of factories, the SC had not yet met; in a further 16%, the SC had met once. 84% of factories SCs' had met at least once in the previous three months, consistent with many factories' SCs recently becoming active. Among the SCs that had met, 88% maintained legally-required meeting minutes. While most factories' SCs were becoming or were already active, 80% of factories had not established a legally-required policy describing their functions and responsibilities. There was also less consistency in the information about SC operations across different sources of information: Presidents' reports matched factory documents and members' reports in about 58% of cases, respectively. There were some reports of management interference with SC operations: 5% of presidents and 7% of worker representatives reported that they were not considered on duty for SC-related activities. 4% of worker representatives reported that management had either offered bribes or otherwise attempted to block SC activities.

Consistent with SCs only recently becoming active, many were not implementing their legally-required safety responsibilities. For example, an important responsibility outlined in the labor law is factory risk assessment. SCs are supposed to regularly inspect factories, to identify risks, and to develop an action plan for their resolution, including making recommendations to senior management. At baseline, only 15% of SCs had ever conducted a factory risk assessment. Relatedly, SCs are required to submit reports/recommendations on safety issues to senior management at least once per 3 months, which 71% of senior managers report receiving. SCs' reported fulfillment of other legally-required responsibilities varied greatly across domains. According to SC presidents' reports, the domain with the highest reported participation is fire prevention and preparedness activities (84%). The domain with the lowest reported participation is accident investigation (54%).

Treatment effects on compliance

Figure 1.4 and Table 1.3 present the results for the pre-specified index of the SC Program's effects on factories' compliance with the SC regulation. Figure 1.4 compares the performance of treatment and control factories on the compliance index at the first and second data collection visits (pre- and post-intervention, respectively). As evident in the figure, both groups start off performing similarly on the compliance index. Control factories' performance improves slightly but is mostly unchanged between the first and second visits. Treatment factories' performance, however, significantly improves compared to controls. At the second visit, treatment factories outperform the control factories by nearly 0.2 sds. The first row of Table 1.3 shows that the ITT effect of the SC Program is 0.19 sds, which is statistically significant according to both RI and FWER p-values (FWER p=0.003). The multinationals' enforcement program is successful at increasing factories' compliance with the labor law above and beyond the effects of state-supplied enforcement and of their other compliance programs.

The SC Compliance index is comprised of three sub-indexes: A formation sub-index, an operations sub-index, and a responsibilities sub-index (see Appendix Figure A1 for index components). Appendix Table A1 shows baseline balance for these and other sub-indexes; treatment and control factories are balanced on all sub-indexes at baseline.

Panel A of Table 1.4 displays the sub-index results for the SC Compliance index. While treatment factories outperform control factories on all SC Compliance sub-indexes at the second visit, by far the largest improvement is on the SC responsibilities sub-index. Treatment factories outperform control factories on this index by 0.34 sds at the second visit, which is statistically significant according to the RI p-value and FDR-sharpened p-value. The large, positive effect on this sub-index is consistent with the Alliance requiring factories to complete a series of activities that are required by law during the SC Program. For example, SCs are legally required to implement a factory risk assessment at least once per quarter. At the second visit, only 17% of control factories' SCs had conducted a risk assessment while 54% of treatment factories' SCs had conducted at least one risk assessment. According to reports by SC presidents, worker representatives, and senior managers, treatment factories' SCs also made more regular safety reports and recommendations to senior management and followed up on these reports more regularly.

In contrast, there is virtually no effect on the SC formation sub-index. The lack of a treatment effect is perhaps unsurprising in light of the Alliance's engagement with factory management on SC formation prior to a factory's becoming eligible for the SC Program. Turning to the operations sub-index, treatment factories outperform control factories by about 0.09 sds, but this difference is not statistically significant. Although the SC Program does not have an overall effect on this sub-index, it does significantly affect one outcome that receives low weight using the Anderson 2008 methodology, which is SCs' meeting frequency. For example, SCs' meeting frequency, which is one outcome included in the operations sub-index, increases by 58%, from an average of 1.27 to 2 meetings per three months. This impact may contribute to improving SCs' effectiveness at fulfilling their legal responsibilities.

SC effectiveness

The multinationals' enforcement program increases factories' compliance with Bangladesh's SC regulation, in particular increasing SCs' fulfillment of legally-required responsibilities. The next critical question is whether these effects translate into improvements in factory safety. Ideally, I would use objective measures of accidents, injuries, and occupational diseases to answer this question. As mentioned in the introduction, however, this intervention is in part aimed at increasing workers' reporting of safety issues and accidents. As a result, even if the intervention reduces accidents and injuries, it may increase the reported number accidents and injuries. For this reason, my primary measure of SCs' effectiveness does not rely on accidents or injuries, but on indicators of factory safety that directly affect the probability of a worker experiencing an accident, injury, or occupational disease. I will supplement this analysis with analysis of worker injuries and occupational diseases, as measured by factories' medical clinic and injury records. These records are currently being digitized, and I will add them to the paper when they become available.

Workers' safety depends on both physical factory safety and safety culture. Accordingly, Bangladesh's SC regulation prescribes responsibilities for SCs related to management of physical factory safety, to training workers, and to safety culture. The effectiveness index includes both physical and awareness/knowledge outcomes that are indicators of SCs' effectiveness at improving factory safety. The index is comprised of the following sub-indexes or, in some cases, unique variables:

- Physical building safety:
  - Performance on an independent spotcheck of factory safety conditions.
  - Progress with required building safety remediation based on Alliance building safety audits (Alliance "Corrective Action Plan (CAP)" completion).[16]
- Factory safety culture:
  - Worker awareness of SC.
  - Worker safety knowledge.
  - Senior management awareness of SC.

Figure 1.5 and Table 1.3 present the results for the SC effectiveness index. As can be seen in Figure 1.5, treatment and control factories perform similarly at baseline. While control factories' performance improves slightly relative to baseline, treatment factories' improvement is more dramatic. Treatment factories outperform control factories by about 0.14 sds at the second visit. Table 1.3 shows that this difference is statistically significant at the 10% level for both the RI and FWER-adjusted p-values (FWER p=0.086). This result provides causal evidence that multinationals' interventions to increase compliance with safety-related labor law can improve factory safety.

Figure 1.6 provides support for the extremeness of the result on SC effectiveness under the null hypothesis of no average treatment effect. The figure plots the joint distribution of compliance treatment effects and SC effectiveness treatment effects under the null hypothesis. The actual parameter estimates are indicated in red. As is evident in the figure, the actual parameter estimates are one of the most extreme points on the joint distribution under the null hypothesis. The chance of jointly observing these effect sizes under the null hypothesis is extremely small.

To unpack the treatment effect, Panel B of Table 1.4 presents results on treatment effects on SC effectiveness for each sub-index. Baseline balance tests for these sub-indexes are presented in Appendix Table A1. There are two baseline imbalances on sub-index variables: Worker awareness of SCs at treatment factories is somewhat lower at treatment factories, although this difference lessens and is not significant at the 5% level when the outlier treatment factory is dropped. Senior managers at treatment factories are more likely to be able to report a specific issue that the SC has identified that the factory has resolved (RI p= 0.076). Estimated treatment effects on these sub-indexes should be interpreted with appropriate caution.

Importantly, the sub-index results show that treatment factories outperform control

[16]Every Alliance-audited factory has a Corrective Action Plan (CAP) based on violations found in the Alliance's building safety audit. The CAP details the remediation actions that the factory will take to address the safety violations. The Alliance monitors factories' progress with implementing remediation and suspends factories that fail to make sufficient progress.

factories on factory safety spotchecks by 0.22 sds. The difference is statistically significant at the 5% level according to the RI p-value (RI p=0.020) and at the 10% level after the FDR p-value adjustment. Table 1.5 shows the treatment effects on each subcomponent of the spotcheck index.[17] Treatment factories outperform control factories on nearly every subcomponent. For example, workers in treatment factories are more likely to be found using machines with appropriate guards for dangerous components and wearing personal protective equipment, which includes equipment such as eye guards, finger guards, chain mesh gloves, goggles, boots, and so on, for their tasks. Although none of the individual differences between treatment and control groups is significant, aggregated, they indicate that the intervention has a small, positive effect on physical indicators of factory safety. This effect is consistent with the SC Program's large effect on SCs' implementation of factory risk assessment, as risk assessment enables SCs to identify safety hazards that need to be resolved.

The SC Program does not increase factories' progress on completing their corrective action plans for building safety violations. These violations often require significant financial investment and time to fix, and if the buyers' intervention increases SCs' ability to push management to make these investments, it may require more time for the effect to materialize; the third round of data collection will be helpful in this regard.

The SC Program does not significantly affect the safety culture sub-indexes, although there is an increase in workers' awareness of SCs compared to controls. For the worker awareness outcomes, the Alliance's Fire Safety and Worker Helpline Training Program, which treatment and control factories are both exposed to, includes training about the factory's SC. This training program likely partially explains workers' high level of baseline awareness of SCs and the null result on worker awareness: At baseline, 81% of workers reported being aware of SCs' general role and responsibilities, and 89% knew that their factory had a SC. As shown in Table 1.6, even with very high baseline awareness of SCs, the SC Program still significantly improves workers' awareness for both of these outcomes and for other measures of worker awareness of the SC.

Worker job satisfaction and well-being

A stated goal of the Alliance's SC Program is to provide workers with a worker-management body with democratically-selected worker representatives that ensures effective identification and resolution of workers' safety concerns. I hypothesized that increasing workers' voice in safety decision-making, and in turn, improving safety inside the factory, would lead workers to feel more satisfied with their jobs, more in control of their safety at the factory, and less stressed. Contrary to my hypothesis, I find that the intervention has the opposite effect, as measured using an index of self-reported job satisfaction and mental well-being.

Figure 1.7 shows treatment and control factories' performance on the worker job satisfaction and mental well-being (well-being index). The figure on the left, sub-figure (a),

[17] Four variables on the spotcheck checklist drop from the analysis because all factories were found to comply with these variables (see Appendix Figure A2).

shows the full sample, and the figure on the right, sub-figure (b), drops the negative outlier in the treatment group. Although the baseline difference including the outlier is not statistically significant, the figure on the right shows that the outlier does not drive the result. Table 1.3 also shows that the estimated treatment effect remains stable when a control for the baseline value of the index is added. Returning to Figure 1.7, it shows a significant decrease in the well-being index at treatment factories relative to controls.

Turning to Table 1.3, the difference at midline is approximately -0.15 sds (FWER p=0.050). The estimate is unchanged if the outlier factory is dropped from the analysis (Appendix Table B2). The negative effect is largely driven by a negative effect on the worker job satisfaction index. Panel C of Table 1.4 shows that worker-reported job satisfaction at treatment factories is -0.39 sds lower than controls (FDR p=0.102). On the other hand, worker-reported mental well-being is only slightly lower than at control factories, and the difference is not statistically significant. Similarly, standardized values of turnover and absenteeism are only slightly more negative, which corresponds to higher rates of turnover and absenteeism, than control factories[18] Appendix Table A4 shows the estimated treatment effect on each sub-variable in the WJMW index. The negative effect on job satisfaction is primarily being driven by a negative effect on workers' referring family and friends to their factory and an increase in the proportion of workers considering leaving their factory for safety-related reasons.

I observe absenteeism and turnover outcomes at the monthly frequency, so I also analyze them using the panel regression model in regression equation 1.2. Table A5 shows estimated treatment effects. The panel regression allows me to identify within-factory changes due to the intervention at treatment compared to control factories. Each regression includes 5 pre-treatment and 5 post-treatment monthly observations. Columns (1) and (2) show that absenteeism at treatment factories increases by about 0.5 percentage points in the five post-treatment months compared to controls. The estimated effect is an 12% increase and is marginally statistically significant (RI p=0.103). Turning to turnover, Table A5, columns (3) and (4) show that the turnover at treatment factories is about 0.34-0.41 percentage points higher in the five post-treatment months compared to control factories, although this difference is not statistically significant.

These results provide suggestive evidence that the intervention increased absenteeism, but it did not significantly affect turnover. Appendix Table A6 also shows that the intervention did not significantly affect workforce composition. The table shows that there are no significant differences in workforce characteristics at treatment factories compared to controls at the second visit. These findings also rule out the possibility that changes in workforce composition are driving the negative effect on workers' job satisfaction.

Why is the Alliance's enforcement intervention negatively affecting workers' job satisfaction? In Section 1.4, I show that the negative effect on job satisfaction is driven by factories with poor managerial practices where the intervention does not improve compliance or safety. I provide suggestive evidence that the negative effect on job satisfaction and the increase in absenteeism is a result of the intervention raising workers' expectations about

[18]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. A higher value of the sub-variable indicates a lower turnover or absenteeism rate, respectively.

what SCs will deliver, and SCs' actual performance not meeting these expectations. This effect would be analogous to the findings of a recent experiment that improved low-wage workers' working conditions in India by Adhvaryu, Nyshadham, and H. Xu 2018, which I discuss in Section 1.4. I have also checked for evidence of other plausible mechanisms for the negative effect, such as workers learning about unsafe conditions at their factories. The data do not provide evidence in favor of learning about unsafe conditions driving the negative effect on job satisfaction (results not reported). I also continue to explore other possible mechanisms.

Business competitiveness

A critical question for this and other forms of labor regulation is what the costs are and who bears them (e.g., Besley and Burgess 2004; Botero et al. 2004). If multinationals' interventions negatively affect targeted suppliers' productivity, then these suppliers are less able to compete against non-targeted alternatives. Unless multinationals reward compliant suppliers through increased prices or other channels, this dynamic would undermine the long-term viability of multinationals enforcing improved standards, as they would have an incentive to source from lower-cost suppliers. Further, if targeted suppliers' productivity falls, and labor markets are competitive, these suppliers may reduce their level of employment. Wages may also fall, in particular if compensation includes production-based incentives.[19]

In this section, I test whether the intervention affects TFP, employment, and gross wages. For TFP, I have the outcome data for the first 64 factories to complete the data collection. For employment and wages, I have the full sample. Out of all sample factories, four factories expanded their use of capital during the observation year.[20] I include these factories in all analyses, but I also show the employment and wage results dropping these factories. See Appendix 3.8 for details on measuring productivity.

Table 1.7 presents the estimated treatment effects on TFP using the panel regression model (equation 1.2). Each regression includes 5 pre-treatment and 5 post-treatment observations, where each observation is one month. In the table, columns (1) and (2) show the results for the base TFP measure, columns (3) and (4) show results for the second TFP measure, which modifies how material inputs are specified, and columns (5) and (6) show the results for the third TFP measure, which modifies how machine inputs are specified. Panel A shows that treatment factories' TFP declines by an estimated 3.6-5.4% compared to control factories' in the post-period, although none of the estimates is statistically significant. Dropping accessories and packaging factories (Panel B), which have more capital-intensive and more variable production, does not significantly affect the results. Panel C shows results for TFP calculated for individual product types, which for multi-product factories, results in multiple observations per factory. As I likely add measurement error when aggregating TFP across product types in multi-product factories,

[19]While it varies across factories, compensation often includes a base wage and some degree of production-based incentives. If the intervention lowers productivity, wages could be directly negatively impacted.

[20]Two treatment and two control factories expanded their use of capital.

these measures may be more precise. The results are qualitatively similar to those in Panel A, however, and suggest a 5.2-6.0% decline in TFP in the post-period compared to control factories. Finally, Panel D tests for effects on TFP for individual product types dropping accessories and packaging factories. Estimated treatment effects in this panel range from 5.3-7.6% declines, although they are not statistically significant.

Table 1.8 shows the estimated treatment effects on TFP using a post-intervention regression model that is analogous to the one used for the analysis of primary outcomes (1) through (3) (equation 1.1). The layout of the table is the same as Table 1.7. The estimated treatment effects are close to zero across almost all regressions. In Panel A, the estimated treatment effect on factory-level TFP range between positive 0.9-1.8% when calendar month fixed effects are included. Estimated treatment effects remain small but turn negative once accessories and packaging factories are dropped (Panel B). The pattern of results is qualitatively similar when factory-product level TFP is the outcome in Panels C and D.

Together, the estimated treatment effects on TFP do not provide conclusive evidence that the enforcement intervention negatively affects factories' productivity. The panel regression model results in Table 1.7 are consistent with the intervention negatively affecting TFP, but the negative effect is not large enough to result in significant differences. Further, the differences between treatment and control factories' TFP disappear using the post-intervention regression model in Table 1.8. Increasing the sample size will improve my ability to determine the intervention's effects on TFP. If I continue to find null results, I will compute the minimum detectable effect size (MDE) that would be detectable ex post under standard assumptions for power calculations.

Table 1.9 presents the estimated treatment effect on employment and on gross wages. For employment, I present results for the the total number of employees at the factory and for the total number of workers. I report both measures because due to variation in how multi-establishment firms count administrative employees, I expect that the number of workers is more accurately measured. In Panel A, Columns (1) and (2) show the estimated treatment effect on overall employment for the full sample. The estimated treatment effect is a 2.3% decline in employment, which is not statistically significant in either regression (RI $p = 0.373$ in column (2)). In columns (3) and (4), the estimated treatment effects on employment of workers is -1.0% (RI $p = 0.680$ in column (4)). These small differences between treatment and control groups disappear using the post-intervention regression model in Table 1.10. In this specification, the estimated treatment effect on overall employment is -0.01% (RI $p = 0.715$) and on employment of workers is -0.04% (RI $p = 0.829$). Evidently, there is not strong evidence that the intervention affects employment.

Turning to wages, columns (5) and (6) of Table 1.9 provide the estimation results for the 72 factories that provided their gross wage data. In column (6) of Panel A, the estimated coefficient is -1.8%, but the difference is not statistically significant (RI $p = 0.557$). The estimated treatment effect is similar using the post-intervention regression model, a decline of 2.4% (RI $p = 0.346$) (Table 1.10, column (4)). The negative effect on wages may be related to the negative estimated treatment effects on TFP, which would decrease wages for workers with production-based incentives. It may also be related to the increase in absenteeism that I find in some specifications, as many factories provide attendance bonuses.

Together, the pattern of estimated treatment effects suggest that the intervention does not have large, adverse effects on productivity, employment, or wages. There is suggestive evidence that there may be some productivity costs of increasing compliance, which may contribute to the small, negative estimated treatment effects on wages (not statistically significant).[21] In the spring of 2019, I will incorporate the final three months of the 13 months of administrative data. These data will provide insight into whether the apparent productivity decline is short-run in nature, for example because there are fixed costs to make a SC operational, or whether they persist. These findings also underscore the importance of benefit-cost analysis to determine the desirability of this enforcement program. In the future, I will incorporate this analysis into this paper.

Heterogeneous treatment effects

In this section, I explore heterogeneity in the intervention's effects along four pre-specified dimensions of heterogeneity: Baseline managerial capacity, compliance with the SC regulation, factory size, and location in an EPZ. For the first three dimensions, I partition the sample into above/below median groups using baseline values of the heterogeneity variable. I use two measures of managerial capacity. First, I create a variable that summarizes senior managers' and lower-level managers' reported frequency of holding production-related meetings with workers. This question is a variant of questions asked in the managerial diagnostics conducted by Bloom, Eifert, et al. 2013 and Macchiavello et al. 2015. I call this variable "production-related" managerial capacity. Second, I create a variable that is an index of worker-reported HR management skills and relations between workers and managers that I pre-specified to measure relations between workers and managers. I call this variable "HR-related" managerial capacity. Table 1.11 shows baseline balance within each interaction-term group for primary outcome variables. Overall, with the exception of factories located in EPZs, treatment and control factories are balanced within subgroups. For the 7 treatment and 7 control factories located in EPZs, the differences are not statistically significant, but they are generally large in magnitude. For this reason, I depart from the PAP and do not analyze this dimension of heterogeneity. Otherwise, there are no statistically significant differences between subgroups.

Table 1.12 shows the results for the three primary outcomes. Each column considers a different dimension of heterogeneity, and each panel considers a different outcome variable. In each panel, the first row displays the estimated treatment effect for the below median group, and the second row displays the treatment effect for the above median group. The final row displays the p-value of the difference in the treatment effects on the subgroups. The regression specification is equation 1.3.

In Panels A and B, production-related managerial practices stand out as the most compelling pattern of effects. Beginning with Panel A, column (6) shows that factories with above median production management practices improve compliance by 0.27 sds (RI $p = 0.002$). In contrast, those with below median production management practices

[21] I will update this paper to report the minimum detectable effect that I would be able to detect given the variation in the data.

improve by about 0.13 sds (RI $p = 0.0170$). I am unable to reject, however, that the estimated treatment effect is equal for both groups. Turning to compliance and size, the compliance effect is slightly larger at factories with initially low compliance as well as at larger factories. While this pattern of results may at first appear surprising, it is consistent with a conceptual framework in which factories have low incentives to comply with the labor law and high powered incentives to produce until the Alliance intervenes. The Alliance's intervention provides strong incentives for compliance, and managers respond to these incentives. Better managers are better equipped to implement the policy and improve compliance more.

The results for SC effectiveness in Panel B also provide support for this hypothesis. Once again, the panel shows a stronger pattern of larger magnitude effects for better managed factories. In particular, Column (6) shows that only better-managed treatment factories improve SC effectiveness; in factories with better practices the estimated treatment effect is 0.27 sds (RI p=0.037), while in below median factories, the estimated treatment effect is approximately zero. Again, the pattern for factories with above median HR practices is similar, although it is less pronounced. For compliance and size, the pattern of heterogeneity is qualitatively similar to that for the compliance index, although the estimated treatment effects are not statistically significant.

Finally, turning to the job satisfaction and mental well-being results in Panel C, column (6) shows that the negative effect on worker job satisfaction and mental wellbeing is about -0.21 sds in factories with worse managerial practices (RI $p = 0.107$); in contrast, the negative effect is attenuated and not statistically significant at better managed factories. Again, this difference is not statistically significant. It does suggest, though, that at better-managed factories that benefit from the improvements in compliance and SC effectiveness, workers do not experience statistically significant declines in job satisfaction and mental well-being. The pattern is similar, although less pronounced, for factory size. Interestingly in light of the results in Panels A and B, there is a large, negative effect on job satisfaction for workers at below median compliance factories, even though they improve compliance, and to some extent, SC effectiveness.

Why does job satisfaction decline at poorly-managed factories?

Why does job satisfaction appear to decline at poorly-managed factories when the intervention is having little to no effect on compliance and SC effectiveness? One plausible mechanism is that the Alliance's intervention raises workers' expectations about what SCs will deliver, but in poorly-run factories, SCs' performance fall short of workers' expectations, and they are disappointed. This effect would be consistent with recent findings from an experiment with low-skill workers in India by Adhvaryu, Nyshadham, and H. Xu 2018. In the experiment, workers were randomly assigned to an intervention that upgraded the quality of employer-provided housing; while the intervention objectively improved housing quality, it reduced workers' job satisfaction and increased turnover. The authors provide evidence that the negative effects were due to the improvement in housing quality falling short of workers' expectations of what it would be.

I cannot directly test that the negative effect on job satisfaction in poorly-managed factories is due to unmet expectations, as I did not collect data on workers' expectations for

SCs. I find support for an important role for workers' expectations and learning about the SCs' role from qualitative evidence gathered from interviews with compliance managers from eight treatment factories. Multiple managers reported that it took several months after their factory's SC became active for workers to understand what issues they could report to the SC and expect to have resolved. In particular, managers reported that it was initially common for workers to raise issues to the SC that were outside of its authority (e.g., working hours or wage-related concerns). In these cases, managers sometimes indicated that the SC relayed the concern to a separate committee responsible for these issues. If these concerns go unresolved, though, it is easy to see why workers may be disappointed, even if their factories' SCs are fulfilling their legal responsibilities. It is also unsurprising that workers may not initially understand the scope of SC authority, as the SC Program is possibly the first time that workers have been informed that there is an institution inside the factory responsible for addressing any type of worker concern. I do not have data with which I can directly test this possibility; while I have access to records of issues raised to the SC, SCs only recorded the safety-related issues in these documents. We did not ask workers about the specific issues that they raised to the SC. If this mechanism contributes to lower job satisfaction at treatment factories, though, it suggests that the negative effect on job satisfaction may be temporary. I will test this using the third data collection round.

Robustness checks for heterogeneity results

There is correlation in factories' characteristics: Better-managed factories tend to be somewhat larger and less compliant. These correlations raise the possibility that only one of these characteristics is actually important in determining 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 variable. This specification demands a lot of the data, but it provides qualitative insight into the relative importance of each dimension. Table 1.13 presents the results. In column (1), in which compliance is outcome variable, the only interaction term that is large in magnitude is above median production management practices (RI p=0.159). In column (2), in which SC effectiveness is outcome variable, the above median production management practices interaction term is again largest in magnitude and statistically significant (RI p=0.008). Finally, the results for worker job satisfaction and mental well-being are less persuasive (column (3)), but the estimated coefficients for the interactions with managerial variables continue to be positive. Together, these results show that managerial practices appear to be an important dimension of heterogeneity after controlling for other factory characteristics and their interaction with the treatment.

A potential concern with this heterogeneity analysis is that there is a small group of factories that is included in three subgroups, above median management ,above median size, and below median compliance, and that I am capturing something singular about these factories, as opposed to differential effects due to the dimensions of heterogeneity that I consider. I will more fully address this concern in a future version of this paper. For now, I mention that each subgroup has 40 factories in the heterogeneity analysis. There are 18 factories, of which eight are treatment, that have above median production management and size and below median compliance. There are six, of which three are treatment, that have above median production management, HR management, and size, and below median compliance. Finally, there is only one that has above median HR management and size and below median compliance and production management. In light of the consistent pattern of heterogeneous treatment effects using the HR management variable, it is unlikely that a specific subgroup of factories of an unobserved "type" are driving the heterogeneous treatment effects.

Finally, one concern about the heterogeneity for less and more compliant factories is that the multinationals differentially monitor these factories. Specifically, one may be concerned that they monitor less compliant factories more than more compliant factories, and that the differential monitoring drives the heterogeneity results. Appendix Table A7 provides evidence against this hypothesis. The table shows Alliance contact with factories through other Alliance programs during the treatment period. In Panel A, the dependent variable is the number of Alliance visits to the factory for remediation verification between the first and second data collection visits. In Panel B, the dependent variable is a dummy variable for participation in the Alliance fire safety training program during the treatment period. In both panels, there are no significant differences in factories' contact with the Alliance for above and below median treatment groups. In short, the Alliance is similarly in contact with both types of factories during the treatment period.

To summarize the results of the heterogeneity analysis, the results show that organizational capacity plays an important role in determining the effect of labor regulation enforcement on factory and worker outcomes. The multinationals' enforcement intervention improves compliance and SC effectiveness only in factories with better managerial capacity. The improvements at these factories do *not* come at the cost of significant negative effects on workers' well-being. For factories with poor management practices, however, the intervention appears to have a marginally statistically significant negative effect on workers with no improvement in safety-related outcomes.

1.5 Conclusion

In this paper, I analyze the effects of a coalition of multinationals' CSR program to enforce a local labor law on their Bangladeshi suppliers. This study is a "first" in multiple streams of literature. It is the first study to provide experimental evidence on whether firms' CSR programs generate meaningful social benefits. It is also the first study to provide experimental evidence on the effects of enforcing labor regulation on factories' competitiveness and workers' well-being. Further, it is the first study to experimentally intervene to increase collective worker voice inside the firm. In addition, through my collaboration with some of the world's largest multinationals, the study has provided unique evidence from a population of factories that would otherwise be unlikely to participate in academic research.

I find that the multinationals' enforcement intervention is successful at increasing factories' compliance with Bangladesh's labor law. Specifically, their intervention to increase suppliers' compliance with a labor law that mandates worker-manager safety committees improves compliance and has a small, positive effect on SCs' effectiveness at improving safety. It improves factories' performance on independent checks of physically-measurable safety conditions and increases workers' knowledge and awareness of SCs. These findings demonstrate that private enforcement of labor law can significantly improve compliance and contribute to achieving the law's objectives. They provide reason for greater optimism regarding the ability of private enforcement to improve labor standards in developing countries compared to the existing research on private regulation in global supply chains in political science, which is largely skeptical but which lacks causal evidence. It also provides the first experimental evidence that firms' CSR initiatives can successfully generate public goods/curtail public bads. Finally, it provides empirical justification for theoretical models of CSR that identify CSR with the private creation of public goods or curtailment of public bads, such as those of Besley and Ghatak 2007 and Lai et al. 2017.

Pre-specified subgroup analysis reveals that the multinationals' enforcement intervention is only effective at improving compliance and safety in better-managed factories. The estimated treatment effects on these factories are large, between 0.25-0.3 sd improvements in compliance and SC effectiveness. In contrast, the intervention does not significantly improve compliance or safety in poorly-managed factories. Further, workers in poorly-managed factories appear to report declines in job satisfaction. I provide suggestive evidence that this result may be due to workers' disappointment with their factories' SCs.

My results have important implications for economic theory, multinational firm strategy, and policymaking. Specifically, they show that organizational capacity in the private sector matters for the efficacy of labor law enforcement in developing countries. Multinationals that aim to enforce local or industry standards on their suppliers need to take into account their suppliers' organizational capacity. Their interventions can have large, beneficial effects when suppliers have capacity to meet higher labor standards. But by raising workers' expectations regarding improvements in factories' that do not have capacity to implement them, their intervention adversely impact workers' job satisfaction, at least in the short-run.

Although preliminary, my analysis of the intervention's effects on factories' productivity, employment, and wages does not provide strong evidence of negative effects. Point estimates for treatment effects on TFP from my preferred estimates are between a 3-5% decline in TFP, although the effects are not statistically significant. Estimated treatment effects on employment and wages are close to zero and not statistically significant. These results will be updated as the remaining factories' TFP outcomes become available and are added to the sample. It is possible that I may still lack statistical power to detect treatment effects on TFP after increasing the sample size; if so, in order to support interpretation of null results, I will report the minimum detectable effect size that I would have been able to detect with the data. Barring drastic changes in the estimated treatment effects, though, they help to allay concerns that enforcement of labor regulation necessarily entails trade-offs between competitiveness and improved working conditions. Further, they can help economists to update their views on enforcement of labor regulation and economic outcomes in developing countries.

The results in paper will be updated during the spring of 2019 to shed light on whether the treatment effects persist beyond the period of intensive enforcement by multinational buyers. Another important question is how the benefits of the multinationals' enforcement intervention compare to its costs. I am seeking program costing data to use this analysis. Ideally, I would use injuries and occupational disease prevalence as the outcomes to measure program benefits. It is possible, though, that reporting of injuries and occupational diseases increase at treatment factories as a result of the intervention, which would confound this analysis. Nevertheless, my research team is coding the medical clinic records for the experimental sample of factories.

My findings raise several important directions for future research. First, this research highlights an important constraint on the efficacy of labor regulation in developing countries, which is the organizational capacity of the private sector. Future research can more fully investigate how firms' organizational capacity supports compliance with labor laws. Second, in the short-run, I do not find evidence of workers differentially sorting in response to improvements in firms' compliance. It is possible, though, that if improvements in compliance are sustained, it may affect workers' mobility and sorting into factories. Boudreau, Heath, and McCormick 2019 provide evidence that garment workers who begin their careers with poor information about factories' working condition exhibit a revealed preference for improving their working conditions compared to their wages. More research is needed, however, on how workers in developing countries make trade-offs between wages and workplace risks. Third, a critical question is what the general equilibrium effects of multinational enforcement of labor law are on compliance and competitiveness of the targeted sector. Finally, there is generally a dearth of empirical evidence in economics on the welfare effects of firms' CSR activities. CSR programs, including private enforcement programs and other types of programs, are becoming increasingly common and increasingly large-scale. These interventions merit more attention.

1.6 Figures and Tables

Figure 1.1: RCT timeline

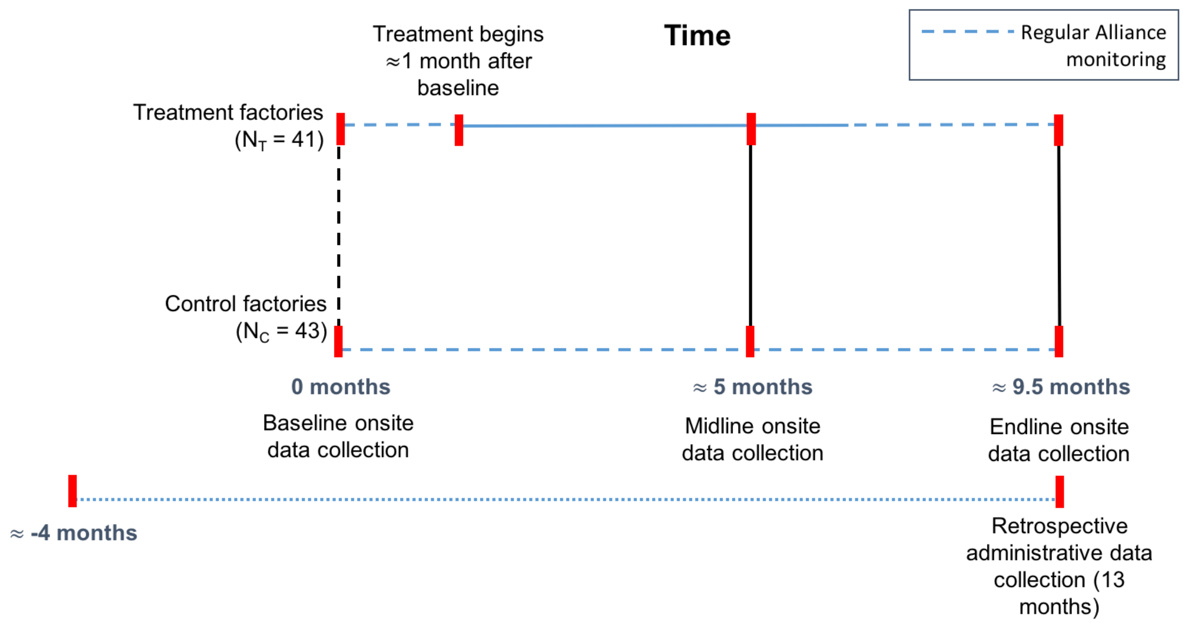
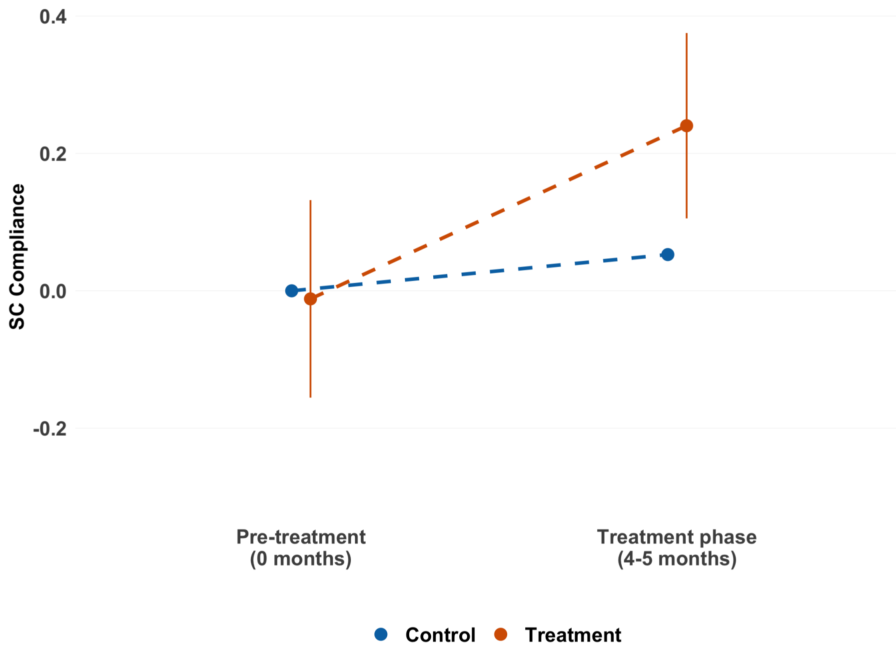


Figure 1.2: Rana Plaza building collapse

Source: Wikipedia.

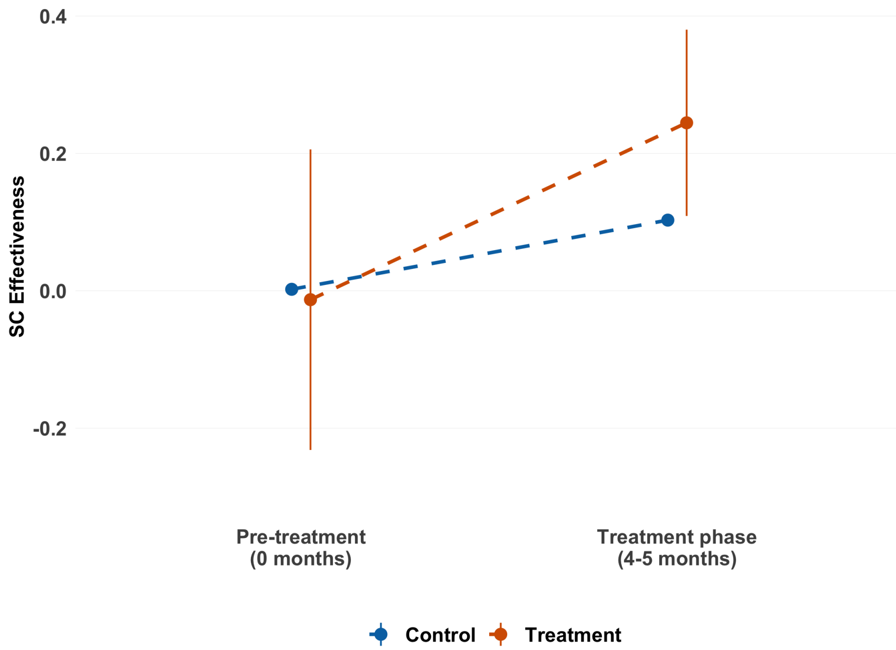
Figure 1.3: Alliance member companies

Figure 1.4: Pre-specified index: SC Compliance



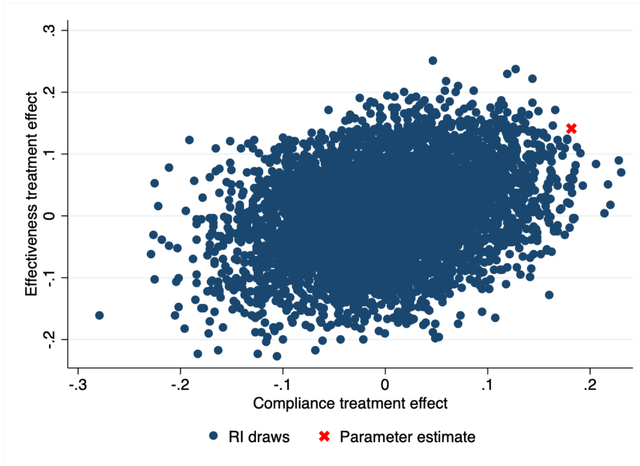
Notes: FWER *p*-val=0.015 for difference in post-treatment means. Whiskers show the the 95% confidence interval calculated from regressions of the outcome variable on a treatment indicator and stratification variables separately for pre-treatment and post-treatment rounds using robust standard errors. Summary index variable is constructed using methodology from Anderson 2008.

Figure 1.5: Pre-specified index: SC Effectiveness



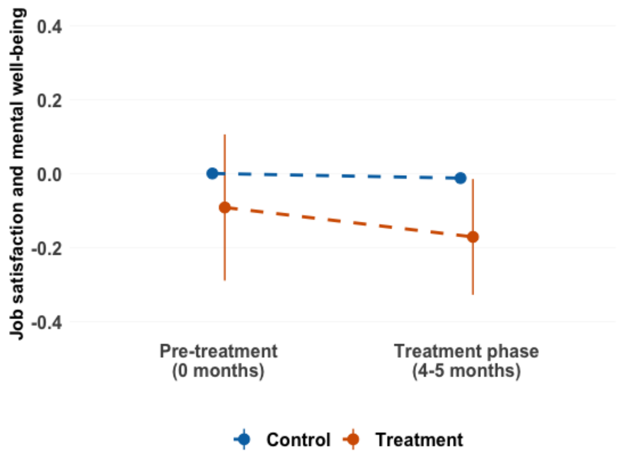
Notes: FWER p-val=0.050 for difference in post-treatment means. Whiskers show the the 95% confidence interval calculated from regressions of the outcome variable on a treatment indicator and stratification variables separately for pre-treatment and post-treatment rounds using robust standard errors. Summary index variable is constructed using methodology from Anderson 2008.

Figure 1.6: Joint distribution of compliance and SC effectiveness treatment effects under the null hypothesis with actual parameter estimates

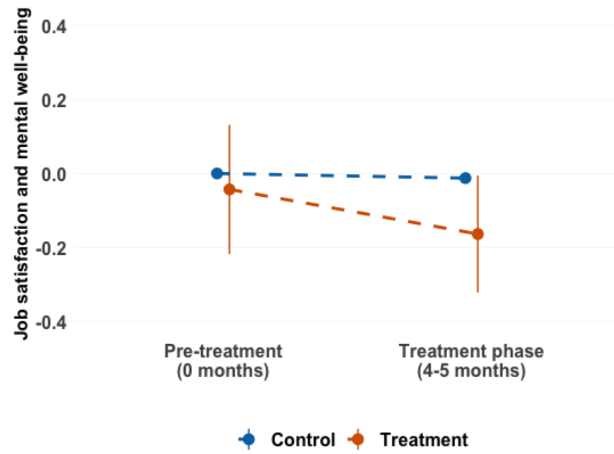


Notes: The figure plots 5000 jointly generated estimates of treatment effects for SC compliance and SC effectiveness under the null hypothesis of no treatment effects. The actual parameter estimates are indicated in red.

Figure 1.7: Worker job satisfaction and mental well-being



(a) Full sample



(b) Dropping 1 outlier

Notes: FWER p-val=0.075 for difference in post-treatment means. Whiskers show the the 95% confidence interval calculated from regressions of the outcome variable on a treatment indicator and stratification variables separately for pre-treatment and post-treatment rounds using robust standard errors. Summary index variable is constructed using methodology from Anderson 2008.

Table 1.1: Key Safety Committee 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* • SC president appointed by management, SC 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 SC • SCs must meet at least once per quarter • SCs must maintain written meeting minutes • Employers must provide SC members adequate time during working hours to fulfill their duties • Employers must provide SC members with occupational health and safety training
Responsibilities	• SCs must implement factory risk assessment at least once per quarter • SCs must make safety-improvement recommendations to the employer • SCs must arrange training and awareness-raising for workers • SCs will participate 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 • SCs will investigate accidents and occupational disease and can submit recommendation to employer for treatment and compensation • SCs will organize regular fire, earthquake, and other disaster management drills

Source: Translation based on Government of Bangladesh 2015.
*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.

Table 1.2: Baseline balance tests

	(1) Control mean n=41	(2) T-C diff n=39	(3) RI p-value
Primary outcome variables			
Compliance index	0.000	-0.012	0.870
Effectiveness index	0.002	-0.015	0.895
Well-being index	0.000	-0.092	0.388
Number employees	1190	-156	0.628
Number workers	1062	-165	0.567
Gross wages (log) (n=72)	15.82	-0.217	0.411
Total factor productivity (log)† (n=56)	2.849	0.155	0.764
Total factor productivity, sewing factories (log) (n=22)	1.867	0.178	0.809
Factory characteristics			
Sewing (only)	0.439	-0.074	0.645
Trade union at factory (1=Yes)	0.049	-0.049	0.294
EPZ(1=Yes)	0.171	0.014	0.888
Monthly absenteeism (%)	4.85	-0.66	0.468
Monthly turnover (%)	3.98	-0.69	0.490
Participation in Alliance training (6 mo pre-baseline)	0.049	-0.020	1.000
Number Alliance remediation visit to factory (6 mo pre-baseline)	0.171	-0.010	1.000
Worker survey respondent characteristics			
Age	27.16	0.40	0.627
Proportion female	0.56	-0.09	0.133
Education (yrs)	6.23	-0.38	0.331
Tenure (yrs)	3.86	-0.21	0.673
Prior industry experience (yrs)	1.52	0.07	0.784

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 in column (1). In column (2), 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 (3), I report the randomization inference (RI) p-value for the coefficient reported in column (2) based on 5000 draws. The regression sample remains the same in all rows unless otherwise indicated. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table 1.3: Treatment effects: Primary Outcome Index Variables

	Control mean	ITT Effect	
	(1)	(2)	(3)
SC Compliance	0.053	0.188	0.193
		[0.008]***	[0.003]***
		{0.015}**	{0.003}***
SC Effectiveness	0.103	0.142	0.144
		[0.050]*	[0.048]**
		{0.049}**	{0.086}*
Worker job satisfaction & mental well-being	-0.013	-0.159	-0.149
(well-being index)		[0.049]**	[0.069]*
		{0.075}*	{0.050}*
Observations		80	80
Stratification variables		Y	Y
Control, base. dep. var.		N	Y

Notes: This table reports OLS estimates of treatment effects on primary outcome index variables. Outcome variables are listed on the left. In all cases, higher values of the index correspond to "positive" outcomes. 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. Column (3) 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. Randomization inference (RI) p-values based on 5000 draws are reported in square brackets. p-values adjusted for multiple hypothesis testing using the method of List, Shaikh, and Y. Xu 2016 are reported in curly brackets. Index variables constructed using Anderson 2008 variance-covariance weighted index. *p<0.1; **p<0.05; ***p<0.01.

Table 1.4: Treatment effects: : Primary outcome sub-indexes and sub-variables

	Control mean	ITT Effect	
	(1)	(2)	(3)
Panel A: SC Compliance			
Formation sub-index	0.070	0.051	0.013
		[0.704]	[0.881]
		{0.448}	{0.416}
Operations sub-index	0.177	0.093	0.092
		[0.206]	[0.189]
		{0.260}	{0.234}
Responsibilities sub-index	-0.054	0.316	0.335
		[0.004]***	[0.002]***
		{0.013}**	{0.007}***
Panel B: SC Effectiveness			
Factory safety spotcheck index	-0.000	0.217	
		[0.020]**	
		{0.087}*	
CAP completion sub-variable	0.314	0.104	0.023
		[0.585]	[0.797]
		{1.000}	{0.917}
Worker SC awareness sub-index	0.073	0.079	0.198
		[0.616]	[0.188]
		{0.969}	{0.603}
Worker safety knowledge sub-index	0.365	-0.069	-0.066
		[0.495]	[0.518]
		{0.969}	{0.917}
Senior manager awareness sub-variable	0.102	0.108	0.075
		[0.656]	[0.759]
		{0.969}	{0.917}
Panel C: Worker Job Satisfaction and Mental Well-being (well-being index)			
Job satisfaction sub-index	-0.156	-0.398	-0.389
		[0.018]**	[0.023]**
		{0.078}*	{0.102}
Mental well-being sub-index	0.040	-0.084	-0.056
		[0.581]	[0.723]
		{1.000}	{0.783}
Turnover sub-variable	0.126	0.072	-0.010
		[0.570]	[0.878]
		{1.000}	{0.783}
Absenteeism sub-variable	0.088	0.030	-0.084
		[0.852]	[0.180]
		{1.000}	{0.370}

Notes: This table reports OLS estimates of treatment effects on primary outcome sub-indexes and sub-variables. Each panel reports the sub-index/sub-variable results for a different outcome variable. Sub-indexes and sub-variables are listed on the left. In all cases, higher values of the index correspond to "positive" outcomes. 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. Column (3) 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. Randomization inference (RI) p-values based on 5000 draws are reported in square brackets. Within-family False Discovery Rate (FDR)-adjusted p-values in curly brackets. Index variables constructed using Anderson 2008 variance-covariance weighted index. *p<0.1; **p<0.05; ***p<0.01.

Table 1.5: Treatment effects: Physical indicators of factory safety

	Control mean	ITT Effect
	(1)	(2)
Factory safety spotcheck index	0.000	0.217 [0.020]** {0.087}*
Sewing: Machines have guards *and* workers wear PPE† for their task	0.500	0.076 [0.621]
Cutting: Machines have knife guards *and* workers wear PPE for their task	0.792	0.071 [0.561]
Dyeing and jobs handling chemicals: Safety masks, goggles, gloves, aprons, and boots worn by workers handling chemicals	0.545	0.102 [0.674]
All PPE appropriate size, functional, and well-maintained	0.951	0.050 [0.260]
Aisles clearly marked and markings visible	0.780	0.052 [0.565]
Aisles clear of sewing scrapes and debris	0.951	0.048 [0.300]
Aisles clear of obstruction	0.854	0.014 [0.867]
Machines in good working order & dangerous parts properly covered	0.927	0.070 [0.153]
Work stations maintained in tidy condition (no loose materials close to electrical appliances)	0.976	0.022 [0.726]
One or more easily accessible first aid kit in section	0.976	0.022 [0.726]
Physical separation between storage & production areas	0.976	-0.005 [0.997]
Drinking water easily accessible for all workers	1.000	-0.025 [0.568]
Drinking water provided appears clean (visual check)	1.000	-0.025 [0.568]
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 Appendix Figure A2). 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. Randomization inference (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. *p<0.1; **p<0.05; ***p<0.01.

Table 1.6: Treatment effects: Worker awareness outcome variables

	Control mean	ITT Effect	
	(1)	(2)	(3)
SC Effectiveness sub-variables			
Aware of SCs & their responsibilities	0.843	0.036	0.053**
		[0.172]	[0.036]
Knows factory has SC	0.945	0.040**	0.040**
		[0.017]	[0.017]
Knows how to report safety concern to SC	0.920	0.009	0.011
		[0.713]	[0.651]
Reported num SC resps	3.060	-0.118	
		[0.345]	
Other worker awareness variables			
Reports SC as channel for raising an issue	0.653	0.018	0.055
		[0.701]	[0.175]
Knows SC members	0.689	0.073**	
		[0.028]	
Observations		80	80
Stratification variables		Y	Y
Control, base. dep. var.		N	Y

Notes: This table reports OLS estimates of treatment effects on all worker SC awareness variables from the baseline and 4-5 month surveys. The first four rows report outcomes included in the SC Effectiveness index (prior to standardization 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. Column (3) 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. Randomization inference (RI) p-values based on 5000 draws are reported in square brackets. *p<0.1; **p<0.05; ***p<0.01.

Table 1.7: Preliminary treatment effects: Productivity

	(1)	(2)	(3)	(4)	(5)	(6)
	Base TFP Measure		TFP Measure 2		TFP Measure 3	
	Panel A: Factory-level TFP					
Treatment x Post	-0.040	-0.041	-0.056	-0.054	-0.035	-0.036
	[0.804]	[0.801]	[0.738]	[0.744]	[0.828]	[0.825]
Factories	56	56	58	58	56	56
Observations	569	568	588	588	568	568
	Panel B: Factory-level TFP *(Dropping accessories & packaging factories)*					
Treatment x Post	-0.052	-0.046	-0.081	-0.080	-0.046	-0.041
	[0.785]	[0.800]	[0.696]	[0.695]	[0.805]	[0.822]
Factories	50	50	50	50	50	50
Observations	499	498	498	498	498	498
	Panel C: Factory-product level TFP					
Treatment x Post	-0.089	-0.052	-0.095	-0.058	-0.091	-0.060
	[0.584]	[0.736]	[0.541]	[0.700]	[0.582]	[0.700]
Factories	56	56	58	58	56	56
Observations	639	638	679	678	637	636
	Panel D: Factory-product level TFP *(Dropping accessories & packaging factories)*					
Treatment x Post	-0.100	-0.053	-0.076	-0.076	-0.064	-0.064
	[0.589]	[0.751]	[0.522]	[0.674]	[0.584]	[0.716]
Factories	50	50	50	50	50	50
Observations	579	578	588	588	576	576
Factory FE	Y	Y	Y	Y	Y	Y
Calendar month FE	N	Y	N	Y	N	Y

Notes: This table reports panel regression estimates of treatment effects on TFP. Each column in the table reports the estimated coefficient from a separate regression. In all panels, the outcome in columns (1) and (2) is the baseline measure of TFP. The TFP measure in columns (3) and (4) allows material inputs to enter the production function differently, and the the TFP measure allows capital inputs to enter the production function differently. Appendix 3.8 explains these differences. In Panel A, TFP is measured at the factory-level, and all factories with TFP measures available are included. In Panel B, TFP is measured at the factory-level, and all factories except accessories and packaging factories are included. In Panel C, TFP is measured at the factory-product level, which is measured more accurately for multi-product factories, and all factories with TFP measures available are included. In Panel D, TFP is measured at the factory-product level, which is measured more accurately for multi-product factories, and all factories except accessories and packaging factories are included. In each panel regression, there are 10 observations per factory, 5 pre-baseline and 5 post-baseline. The dependent variable in each column is regressed on an interaction between the treatment indicator and a post-treatment indicator variable and factory fixed effects. Calendar month fixed effects are included in even-numbered columns. Randomization inference (RI) p-values based on 5000 draws are reported in square brackets. *p<0.1; **p<0.05; ***p<0.01.

Table 1.8: Preliminary treatment effects: Productivity outcomes, model 2

	(1)	(2)	(3)	(4)	(5)	(6)
	Base TFP Measure		TFP Measure 2		TFP Measure 3	
	Panel A: Factory-level TFP					
Treatment	0.024	0.012	0.028	0.018	0.021	0.009
	[0.888]	[0.939]	[0.869]	[0.917]	[0.900]	[0.957]
Factories	56	56	58	58	56	56
Observations	285	285	295	295	285	285
	Panel B: Factory-level TFP *(Dropping accessories & packaging factories)* *(Dropping accessories & packaging factories)*					
Treatment	0.010	-0.013	-0.006	-0.031	0.007	-0.017
	[0.957]	[0.940]	[0.978]	[0.877]	[0.971]	[0.926]
Factories	50	50	50	50	50	50
Observations	250	250	250	250	250	250
	Panel C: Factory-product level TFP					
Treatment	0.008	0.003	0.014	0.011	-0.035	-0.042
	[0.964]	[0.984]	[0.014]	[0.949]	[0.829]	[0.796]
Factories	56	56	58	58	56	56
Observations	320	320	340	340	319	319
	Panel D: Factory-product level TFP *(Dropping accessories & packaging factories)*					
Treatment	-0.005	-0.020	-0.000	-0.016	-0.041	-0.060
	[0.977]	[0.917]	[1.000]	[0.937]	[0.821]	[0.731]
Factories	50	50	50	50	50	50
Observations	290	290	295	295	289	289
Calendar month FE	N	Y	N	Y	N	Y

Notes: This table reports OLS estimates of treatment effects on TFP. Each column in the table reports the estimated coefficient from a separate regression. In all panels, the outcome in columns (1) and (2) is the baseline measure of TFP. The TFP measure in columns (3) and (4) allows material inputs to enter the production function differently, and the the TFP measure allows capital inputs to enter the production function differently. Appendix 3.8 explains these differences. In Panel A, TFP is measured at the factory-level, and all factories with TFP measures available are included. In Panel B, TFP is measured at the factory-level, and all factories except accessories and packaging factories are included. In Panel C, TFP is measured at the factory-product level, which is measured more accurately for multi-product factories, and all factories with TFP measures available are included. In Panel D, TFP is measured at the factory-product level, which is measured more accurately for multi-product factories, and all factories except accessories and packaging factories are included. Each regression includes five post-treatment observations per factory, where each observation is one month. 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. Calendar month fixed effects are included in even-numbered columns. Randomization inference (RI) *p*-values based on 5000 draws are reported in square brackets. *p<0.1; **p<0.05; ***p<0.01.

Table 1.9: Treatment effects: Employment and wages

	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Total employment)		Log(Workers)		Log(Gross wages)	
	Panel A: Full sample					
Treatment x Post	-0.026	-0.023	-0.012	-0.010	-0.016	-0.018
	[0.323]	[0.373]	[0.620]	[0.680]	[0.587]	[0.557]
Factories	80	80	80	80	72	72
Observations	800	800	800	800	719	719
	Panel B: Dropping factories with capital expansion					
Treatment x Post	-0.032	-0.031	-0.018	-0.018	-0.023	-0.028
	[0.216]	[0.225]	[0.445]	[0.444]	[0.444]	[0.362]
Factories	76	76	76	76	68	68
Observations	760	760	760	760	679	679
Factory FE	Y	Y	Y	Y	Y	Y
Calendar month FE	N	Y	N	Y	N	Y

*Notes:*This table reports panel regression estimates of treatment effects on employment and wages. Each column in the table reports the estimated coefficient from a separate regression. In columns (5) and (6), eight factories declined to provide wage information. In each panel regression, there are 10 observations per factory, 5 pre-baseline and 5 post-baseline. The dependent variable in each column is regressed on an interaction between the treatment indicator and a post-treatment indicator variable and factory fixed effects. Calendar month fixed effects are included in the second column for each variable. Randomization inference (RI) p-values based on 5000 draws are reported in square brackets. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table 1.10: Treatment effects: Employment and wages, model 2

	(1)	(2)	(3)
	Log(Total employment)	Log(Workers)	Log(Gross wages)
	Panel A: Full sample		
Treatment effect	-0.008	0.004	-0.024
	[0.715]	[0.829]	[0.346]
Factories	80	80	72
Observations	400	400	360
	Panel B: Dropping factories with capital expansion		
Treatment effect	-0.019	-0.007	-0.027
	[0.381]	[0.612]	[0.301]
Factories	76	76	68
Observations	380	380	340
Stratification variables	Y	Y	Y
Control, baseline dep. var.	Y	Y	Y

Notes: This table reports OLS estimates of treatment effects on absenteeism, turnover, employment, and gross wages. Each column in the table reports the estimated coefficient from a separate regression. The regression sample changes in column (3) because eight factories declined to provide gross wage information. Each regression includes five post-treatment observations per factory, where each observation is one month. 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. Randomization inference (RI) *p*-values based on 5000 draws are reported in square brackets. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table 1.11: Baseline balance tests within subgroups for heterogeneity analysis, primary outcome variables

	(1) Control mean	(2) T-C diff	(3) RI *p*-value	(4) N
Panel A: SC Compliance				
Abv_Med_Compli=0	-0.198	-0.122	0.111	40
Abv_Med_Compli=1	0.208	0.044	0.342	40
Abv_Med_Size=0	0.009	-0.002	0.988	40
Abv_Med_Size=1	-0.007	-0.072	0.536	40
Abv_Med_MGMT=0	0.044	0.051	0.642	40
Abv_Med_MGMT=1	-0.034	-0.116	0.292	40
Abv_Med_MGMT(2)=0	-0.044	0.045	0.675	40
Abv_Med_MGMT(2)=1	0.038	-0.097	0.378	40
EPZ=0	0.037	-0.040	0.624	66
EPZ=1	-0.179	0.362	0.182	14
Panel B: SC Effectiveness				
Abv_Med_Compli=0	-0.028	-0.071	0.666	40
Abv_Med_Compli=1	0.034	0.162	0.322	40
Abv_Med_Size=0	0.133	-0.040	0.815	40
Abv_Med_Size=1	-0.090	-0.076	0.641	40
Abv_Med_MGMT=0	0.087	-0.147	0.403	40
Abv_Med_MGMT=1	-0.064	0.095	0.535	40
Abv_Med_MGMT(2)=0	-0.228	0.068	0.699	40
Abv_Med_MGMT(2)=1	0.201	-0.106	0.473	40
EPZ=0	0.014	-0.070	0.557	66
EPZ=1	-0.053	0.218	0.591	14
Panel C: Worker job satisfaction and mental well-being				
Abv_Med_Compli=0	-0.054	0.020	0.874	40
Abv_Med_Compli=1	0.056	-0.147	0.413	40
Abv_Med_Size=0	0.012	-0.039	0.806	40
Abv_Med_Size=1	-0.009	-0.095	0.585	40
Abv_Med_MGMT=0	0.019	-0.202	0.295	40
Abv_Med_MGMT=1	-0.015	0.007	0.954	40
Abv_Med_MGMT(2)=0	-0.043	-0.222	0.223	40
Abv_Med_MGMT(2)=1	0.037	0.037	0.976	40
EPZ=0	0.023	-0.171	0.158	66
EPZ=1	-0.111	0.511	0.104	14

Note: This table reports OLS estimates of baseline differences between control and treatment groups within each pre-specified subgroup for treatment effect heterogeneity analysis. For the first three dimensions of heterogeneity, compliance, size, and managerial practices, I partition the sample into above/below median subgroups using the baseline value of the variable. For the final dimension of heterogeneity, location in Export Processing Zone (EPZ), I partition the sample using this variable. Each panel reports the within subgroup baseline differences for a different outcome variable. For each outcome, within subgroup, 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 outcome on the treatment indicator and stratification variables within that subgroup. In column (3), I report the randomization inference (RI) p-value for the coefficient reported in column (2) based on 5000 draws. In column (4), I report the number of observations in that subgroup. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table 1.12: Heterogeneous treatment effects: Primary Outcome Index Variables

	Compliance		Size		Mgmt (Prod)		Mgmt (HR)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: *Compliance Index*								
Below median	0.177	0.211	0.201	0.118	0.155	0.127	0.179	0.201
	[0.098]*	[0.052]**	[0.052]*	[0.030]**	[0.153]	[0.170]	[0.095]*	[0.102]
Above Median	0.178	0.161	0.145	0.169	0.214	0.273	0.201	0.243
	[0.007]**	[0.015]**	[0.145]	[0.054]*	[0.033]**	[0.002]***	[0.045]**	[0.013]**
p-val, diff	[0.996]	[0.700]	[0.663]	[0.803]	[0.701]	[0.274]	[0.884]	[0.474]
Panel B: *Effectiveness Index*								
Below median	0.176	0.188	0.076	0.082	-0.002	0.013	0.106	0.092
	[0.175]	[0.145]	[0.372]	[0.332]	[0.979]	[0.878]	[0.396]	[0.493]
Above Median	0.095	0.087	0.212	0.218	0.279	0.267	0.179	0.192
	[0.177]	[0.217]	[0.162]	[0.157]	[0.025]**	[0.037]**	[0.111]	[0.086]*
p-val, diff	[0.601]	[0.500]	[0.508]	[0.516]	[0.071]*	[0.101]	[0.645]	[0.522]
Panel C: *Worker Job Satisfaction and Mental Well-being Index*								
Below median	-0.327	-0.327	-0.241	-0.233	-0.233	-0.213	-0.199	-0.183
	[0.011]**	[0.014]**	[0.064]*	[0.076]*	[0.073]*	[0.107]	[0.117]	[0.150]
Above Median	0.010	0.033	-0.086	-0.071	-0.086	-0.088	-0.101	-0.102
	[0.905]	[0.706]	[0.375]	[0.461]	[0.388]	[0.377]	[0.311]	[0.317]
p-val, diff	[0.045]**	[0.031]**	[0.346]	[0.322]	[0.384]	[0.468]	[0.550]	[0.624]
Observations	80	80	80	80	80	80	80	80
Stratification variables	Y	Y	Y	Y	Y	Y	Y	Y
Control, base. dep. var.	N	Y	N	Y	N	Y	N	Y

Note: This table reports OLS estimates of heterogeneous treatment effects on primary outcome index variables. Each dimension of heterogeneity is indicated at the top of the table. Each panel reports the results for a different outcome variable. 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. For each dimension of heterogeneity, I estimate the treatment effects without and with a control for the baseline value of the dependent variable. All regressions include stratification variables. All subgroups have 40 observations. Randomization inference (RI) *p*-values based on 5000 draws are reported in square brackets. Index variables constructed using Anderson 2008 variance-covariance weighted index. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table 1.13: Heterogeneous treatment effects: Testing the importance of each dimension of heterogeneity

	Dependent variable:		
	Compliance	SC Effectiveness	Job satisfaction & mental well-being
	(1)	(2)	(3)
Treat	0.113	-0.197	-0.517
	[0.521]	[0.210]	[0.062]*
Treat*Abv med Compli	-0.033	-0.048	0.370
	[0.814]	[0.746]	[0.039]**
Treat*Abv med Size	-0.069	0.112	0.199
	[0.620]	[0.514]	[0.324]
Treat*Abv med Mgmt (Prod)	0.190	0.363	0.072
	[0.159]	[0.008]***	[0.663]
Treat*Abv med Mgmt (HR)	0.049	0.184	0.1116
	[0.739]	[0.298]	[0.594]
Observations	80	80	80
Stratification variables	Y	Y	Y

Notes: This table reports OLS estimates of heterogeneous treatment 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. Randomization inference (RI) p-values based on 5000 draws are reported in square brackets. *p<0.1; **p<0.05; ***p<0.01.

Chapter 2

Migrants, information, and working conditions in Bangladeshi garment factories

Joint with Rachel Heath and Tyler McCormick

2.1 Introduction

Working conditions are poor in many industries throughout the world. These poor conditions can culminate in tragedies such as the Rana Plaza collapse in Bangladesh in 2013 – which killed over 1100 workers – and even when not resulting in such visible tragedies, can cause health problems ((Blattman and Dercon 2018a)). A key question that arises is whether workers understand the tradeoffs they are making when they choose to work in such conditions.

We argue that workers do not have full information about working conditions when beginning work, so that the market equilibrium results in an inefficiently low level of working conditions. Our empirical context is the garment industry in Bangladesh, where there has been substantial international attention to working conditions and wage levels. We develop a theoretical model in which firms compete for informed workers (who can observe working conditions upon beginning a job) and uninformed workers. The model illustrates how uninformed workers can end up in firms with inefficiently low investments in working conditions – even in a competitive labor market – as firms compete for workers based on job aspects they can observe (wages) and not on those aspects they cannot observe (working conditions). We then extend the static model to a two period model to derive predictions on workers' mobility as they gain experience in the industry and presumably become better informed about working conditions. If there is a cost to switching factories, workers will do so only if they are sufficiently poorly matched to their current factories. In the context of this model, such workers are more likely to be uninformed workers, who move towards factories with better conditions, even if this improvement comes at the expense of foregone wage gains.

In the context of this model, we consider several potential differences between internal migrants and local workers. Drawing on qualitative evidence that many migrants know very little about the industry when they begin work, we first consider the possibility that they are precisely the workers in the model who are less likely to be informed about working conditions upon beginning work in the industry. However, we also consider several other potential hypotheses:

migrants could have lower costs to moving factories, stronger relative preference for money over working conditions given the desire to send remittances home, or have lower average productivity than local workers.

We look for evidence of each of these possible differences between migrants and locals, using a retrospective panel of the work history of 991 garment workers collected from a household survey of a peri-urban area outside Dhaka, Bangladesh in 2009. We combine workers' reports of problems in the factories, relationship between workers and management, whether the factory provides medical care, and whether the worker has an appointment letter to create an index of working conditions. We compare the working conditions and wages faced by "local" workers originally from the same subdistricts as the survey area (who constitute 14 percent of workers in the sample) to those of internal migrants from rural areas.

We begin by considering differences in labor outcomes across the entire careers of migrants and locals, corresponding to the predictions of the one-period model. Migrants on average work in factories with a 0.29 standard deviation lower index of working conditions than locals. This disparity is not due to observable demographic differences between migrants and locals, and holds when we compare migrants and locals in the same villages. At the same time, migrants are in factories that actually pay higher wages: over the course of their careers, migrants earn 4.9 percent more than other workers, but 1.6 percent less than other workers in their same factories.

We then examine the model's implications for mobility of migrants versus locals as they gain experience. A discrete-time hazard model indicates that a migrant is 1.4 percentage points more likely to leave a factory than a local in a given month. This difference drops in half and become insignificant when we include factory fixed effects, suggesting that the differential mobility of migrants is driven in part by the fact that they end up in the kinds of factories that people want to leave. Finally, we document that the migrants differentially improve their working conditions as they gain experience, compared to locals. Of these baseline models of differences between migrants and locals that we consider, the only one consistent with all four of these empirical facts is the assumption that migrants are more likely to be uninformed upon beginning work in the industry.

At the same time, we also recognize that our empirical results are also consistent with a model in which migrants have a stronger relative preference for wages than locals, but this difference fades over time. For instance, migrants may face moving costs, or lose access to risk sharing networks when they move. While evidence against the differential tendency of migrants to accumulate assets over time, compared to locals, is some suggestive evidence in favor of our information-based model, we acknowledge that a clear delineation between the two models is not possible given our current data.

There is relatively little literature on labor markets in export manufacturing sectors in developing countries, and most of its focus is on the determinants of wages, such as estimating export wage premia (see Harrison and Rodríguez-Clare 2010 for a review) or the effects of anti-sweatshop activism (Harrison and Scorse 2010a). Working conditions – especially subjective measures such as workers' relations with management – have received less attention, likely because collecting credible data is difficult. Firm-level surveys may be subject to misreporting if respondents do not feel comfortable truthfully reporting conditions when interviewed at the firm.[1] Some studies have examined working conditions by using injury or fatality reports at the industry level

[1]**tanaka2016** collected data on fire safety procedures, healthcare management, and freedom of negotiation in garment factories in Myanmar, and demonstrates that the managers' reports of these measures were correlated with enumerators' observations during a factory tour. Still, components of working conditions such as abusive management would still likely not be observed by enumerators during a tour.

((Shanmugam 2001)), but within-industry variance is likely important too. Indeed, Sorkin 2018 finds that nonpecuniary benefits are important in explaining variance in firm-level wages in the United States, and non-wage benefits could be even more important in developing countries given the general scarcity or weak enforcement of formal regulation. While our firm-level measures of working conditions from workers' reports in a household survey are likely imperfect as well – even in the privacy of their homes, workers may be be unwilling to report bad conditions – we argue that these measures provide accurate reports of working conditions across firms with an industry.

The Bangladeshi garment industry in 2009 is a particularly interesting context to examine working conditions in developing countries. The industry had been growing rapidly since the early 1980's, averaging 17 percent yearly employment growth. While NGOs had long been attempting to raise awareness of poor working conditions (see International Restructuring Education Network Europe 1990 for an early example), there was minimal government enforcement of safety standards, so compliance was largely voluntary, often encouraged by Western retailers ((Mahmud and Kabeer 2003); (Ahmed and Nathan 2014)). While there have been recent higher-visibility initiatives in Bangladesh after the Rana Plaza collapse in 2013,[2] reports from other recent industrialized countries report similar lack of enforcement of regulations and resulting intra-industry variation in working conditions, including Robertson et al. 2009 in Indonesia, Oka 2010 in Cambodia, or Tanaka 2015 in Myanmar.

Since neither at the time – nor today – do there exist formal mechanisms to publicize factories' working conditions (to our knowledge), most workers rely on either their own experience or word of mouth to learn about factories upon beginning work ((Amin et al. 1998); (Absar 2009)). Indeed, garment sector jobs can be thought of as "experience goods" whose quality cannot perfectly be observed before purchasing. While there is a long tradition in search models in labor economics of viewing jobs as experience goods (Jovanovic 1979) in which nonpecuniary job characteristics could serve an important role (W. Kip Viscusi 1980), empirical tests of these models have focused on realizations over time of a worker's match-specific productivity (which neither the firm nor the worker knows at the time of hiring). This could be due to data limitations, as these productivity realizations would be likely to show up in a worker's wage trajectory with tenure, which is generally much easier to observe than working conditions.

By contrast, in our model, the firm knows its investment in working conditions, and would like to be able to credibly signal it to the worker. This is a similar context to industrial organization models in which firms know a good's quality but consumers do not. Theoretical models of this scenario have highlighted the potential efficiency gains of market intermediaries (Biglaiser 1993) or sellers' ability to build a reputation (see Mailath and Samuelson 2013 for an overview). Given that we do not see Bangladeshi garment factories engaging in these types of efforts, a natural question is why they don't. While it is generally hard to spread information in the garment industry in Bangladesh – as previously mentioned, we know of no institutions that allow workers to share information about firms with other workers – our model suggests that labor market competition could be a further reason. In particular, if there is a constant stream of new workers, the gains from establishing a reputation fall, since it is plausibly equally profitable to compete for uninformed workers than to invest in quality and then make costly efforts to advertise it. In

[2]Namely, the The Bangladesh Accord on Fire and Building Safety and the Alliance for Bangladesh Worker Safety both work with factories to conduct audits and develop Corrective Action Plans to fix any violations found, including the potential for low interest loans to make these improvements. As discussed in Appendix 3.8, there is substantial variation in factories' performance on these initiatives' physical building safety audits.

section 2.4, we find some evidence that firms with better working conditions are more likely to be still operating under the same management five years after the worker survey, suggesting that eventually, however, a good reputation is important.

Our emphasis on workers' informedness in hiring introduces a new concept to the literature on hiring in developing countries. The existing literature has highlighted factors that affect the workers' future productivity like skill complementarity (De Melo 2009) or the availability of a network member to reduce moral hazard (Heath 2018). Other work has emphasized the role of search frictions (Franklin 2015) and the use of networks as a way of rationing desirable jobs (S.-Y. Wang 2013) or spread information about job openings (J. R. Magruder 2010). More closely related to this paper are Hardy and McCasland 2015 and Bassi and Nansamba 2017b, which focused on asymmetric information about workers' ability. Our focus, by contrast, is on asymmetric information about the job rather than the worker. Given how new an experience a garment factory job is to recent migrants, there is reason to believe that this asymmetry is also important in explaining labor market outcomes.

Our paper also relates to the literature on firm-level heterogeneity, which points out that similar workers receive different compensation in different firms in both developed ((Krueger and Summers 1988); (Brown and Medoff 1989); (Abowd, Kramarz, and Margolis 1999)) and developing ((Teal 1996); (El Badaoui, Strobl, and Walsh 2008)) countries. Indeed, this heterogeneity may be even greater in developing countries, where government interference and market imperfections prop up inefficient firms (Banerjee and Duflo 2005). Minimal workplace safety regulations and other legal protections for workers further contribute to the between-firm variation in non-wage benefits. Given this variation, we document variation in wages and working conditions between firms within an industry, and propose a theory emphasizing the role of matching in explaining how workers are matched to these heterogeneous firms.

Finally, this paper contributes to the literature on rural to urban migration in developing countries. This literature goes back to the canonical models of Lewis 1954 and Harris and Todaro 1970, who argue that workers are on average more productive in urban than rural areas, so that rural to urban migration is a key driver of economic growth. Papers building on this theme have focused on the determinants of the decision to migrate to an urban area ((Marchiori, Maystadt, and Schumacher 2012); (Bryan, S. Chowdhury, and Mobarak 2014); (Kleemans 2014); (Henderson, Storeygard, and Deichmann 2015)) and the effect of migration on the migration household ((Beegle, De Weerdt, and Dercon 2011); (Brauw, Mueller, and Woldehanna 2013); (Kinnan, S.-Y. Wang, and Y. Wang 2015)) and the broader village economy ((Morten 2013); (Munshi and Rosenzweig 2016)). Another strand of this literature examines the effects of internal migrants on wages and other outcomes in urban labor markets ((Kleemans and J. Magruder 2015); (Strobl and Valfort 2015)). This paper brings these two strands of literature together by examining how the characteristics of migrants affect their experience in urban labor markets.

2.2 Data and empirical setting

In this section, we explain the data collection process that provides information on migrants versus local workers, provide some background on the garment industry in Bangladesh and the information that workers plausibly have about factories when choosing a workplace, and describe our method for constructing factory-level measures of working conditions.

Survey and characteristics of respondents

The survey that yields the data we use in this paper was conducted by Rachel Heath and Mushfiq Mobarak between August and November, 2009. The survey consisted of sixty villages in four subdistricts (Savar and Dhamrai subdistricts in Dhaka district and Gazipur Sadar and Kaliakur in Gazipur district) in the peri-urban area surrounding Dhaka. The villages (shown in figure E1) were chosen randomly from three strata of data: 44 villages were chosen from among those considered to be within commuting distance of a garment factory (by an official at the Bangladesh Garment Manufacturers Exporting Association), 12 were chosen from not those considered to be within commuting distance, and 4 from the in between area (to allow the data to be representative at the subdistrict level).[3] The sampling unit was an extended family compound, called a *bari* in Bangla.

In addition to household-level information, each garment worker in a sampled bari filled out a questionnaire asking information about each factory they had worked in since they began working, including information about problems, relationship with management, and other factory characteristics (described more in detail in Section 2.2). Workers were asked the name of each factory, so workers can be matched to other workers in the same factory to create factory-level measures of working conditions. Furthermore, workers were also asked if they ever earned a wage other than the first offer in a factory, and if so, the number of months they received each wage. We can thus construct a retrospective panel of the monthly wage of each worker since she began working, matched to the factory in which the wage was earned.

Several characteristics of the survey area are important in interpreting the results of the paper. First, these villages are near Dhaka, but not in Dhaka. This area was chosen because garment workers in these areas live in residential houses rather than dormitories, where factories tend to limit the access of outsiders and workers may feel less free to truthfully report characteristics of their job. Inasmuch as the typical worker in the survey area has fewer factories within commuting distance of her current residence than a worker in Dhaka, these workers may work in factories with greater monopsony power over their workers than factories in Dhaka. However, the fact that workers tend to move factories frequently – the average worker has worked in 2.3 factories (2.9 among workers in the industry for three years or more) – presents prima facie evidence against complete monopsony power of firms.

Another important characteristic of the firms in the sample is that they hire more males than the typical firm in Bangladesh: 56 percent of the workers in the survey are female, while the national labor force was estimated to be 80 percent female at this time ((Bangladesh Garment Manufacturing Exporters Association 2013); (Saxena 2014)).[4]. The garment factories in the survey area are disproportionately woven factories (compared to the national sample, which has a greater proportional share of knitwear factories). Woven factories, while still conducting the sewing activities that are overwhelmingly female, tend to hire more males to operate the looms, which require upper body strength to operate.

Table 2.1 gives summary statistics of the workers in our sample, broken down by gender and migration status. Because some of our sample began working before moving to their current vil-

[3]These distinctions were very accurate in practice: of the 991 sampled workers, 976 were living in those designated as garment villages, 5 living in those designated as non-garment villages, and 20 living in "in between" villages.

[4]Other sources put the figure at 90 percent female ((N. J. Chowdhury and Ullah 2010); (Ghosh 2014)). Part of the disparity may be the question of whether only sewing-line operators (versus other factory employees) are included (Chris Woodruff, personal communication). This general lack of consensus highlights the general scarcity of detailed information about garment workers and factories.

lage (and we don't know whether they were originally from that village or not), our main measure of migration status is not whether the worker is originally from the village in which she now resides. Instead, we consider whether the worker was originally from Dhaka or Gazipur districts (which incorporate all of the surveyed villages), which we refer to as urban areas, and the workers born there as "locals." By this definition, only 15 percent of male workers and 11 percent of female workers are locals; we consider the rest of workers to be migrants.[5] The greater tendency of women to be migrants is unsurprising, given that women tend to migrate upon marriage in Bangladesh. These migrants were all born in Bangladesh, but they come from all over the country. The largest sending district of Mymensingh, which neighbors Gazipur to the north, constitutes only 13 percent of migrants, and 44 home districts (of the 64 total in Bangladesh) are represented in two or more baris in the sample.

Both groups of workers overall are young (average age 27.9 years for males and 24.4 for females), although they are overwhelming married (79 percent of male workers and 76 percent of females). Male workers have approximately the same education (7.2 years) and experience (4.9 years) regardless of whether they are migrants; female migrants have marginally more education (4.9 years, versus 4.4 years for locals, $P = 0.206$) but less experience (3.5 years, versus 4.5 years for locals). Both male and female migrants came to the village in which they were surveyed on average 4.5 years ago.

Panel B gives a sense of the living conditions of the workers in the sample. Garment workers are better off than the typical Bangladesh household in 2009 in several dimensions; they are likely to live in a house with a cement floor (78 percent of both genders), that has electricity (96 percent of both genders), and possesses a cell phone (77 percent of male workers and 67 percent of female workers). These averages mask substantial divides between urban and local workers: migrant workers are more likely to live in a house with a cement floor or that has electricity, but actually less likely to live in a house with a mobile phone. While only a small minority (4 percent) of migrants own the homes they currently live in, most own a homestead (presumably, in their original village) and around half own agricultural land as well. By contrast, most urban workers own the homes they live in, but are less likely to own agricultural land.

Finally, panel C describes the job characteristics of migrants and local workers. Local male workers were considerably more likely than migrant male workers to have been referred (53 percent of local workers; 37 percent of migrants), whereas 31 percent of both groups of female workers were referred. Local workers tend to have longer commutes; both males and female commute an average of 27 minutes, compared to approximately 18 minutes for male and female migrants. Both genders and migrants groups work on a regular day an average of approximately 8.5 hours and average about 3 hours of overtime in the peak season. Workers from urban areas have a longer tenure with the current firm, 39 months for males and 36 months for females, compared to 25 months for male migrants and 26 months for female migrants.

Overall, while the discussion we have just made highlights several reasons why the workers in the sample are not necessarily representative of workers throughout garment industry in Bangladesh, we posit that this is an important sample in its own right. For one, the workers are heavily migrants, which is a common characteristics of workers throughout the industry; any disadvantages endured by migrants probably highlight a common problem throughout the industry. Secondly, the higher than usual proportion of males in the sample gives us power to detect gender

[5] In Appendix Table E5, we show robustness of our main results to alternative definitions of the the migrant variable.

differences in outcomes, which could potentially be important in understanding the overall labor market outcomes in Bangladesh.

The garment industry in Bangladesh

Figure 2.1 depicts the consistent employment growth in the garment industry between the early 1980's and the 2009 survey; the average yearly employment growth over that period is 17 percent (BGMEA 2013). The high rates of migration in the surveyed villages displayed in table 2.1 are emblematic of the general rates of rural to urban migration that have accompanied the rapid growth of the garment sector. Thus, many workers tend to enter the industry with no experience in the formal sector, and little experience outside the home or village.

As is explained more in detail in Heath 2018 – which uses the same dataset as this paper – hiring is relatively informal. It is common for the firm hiring a worker to receive a referral from one of their current workers (such referrals constitute 32 percent of hires); other workers find out about the job through a personal contact not working in the factory that is hiring (8 percent of hires). It is also common to show up at the factory and ask for work (40 percent of hires). Only 19 percent of workers are hired through more formal means (a written advertisement or recruitment by management). The fact that most hiring is done informally again suggests that workers may know little about a factory when they begin working.

There is anecdotal evidence that the factories these workers enter are quite heterogeneous, both in wages and in working conditions. At the time of the 2009 survey, the minimum wage was 1662.5 taka per month (about 22 US dollars at the time). While the minimum wage did bind in some factories (Heath 2018), others paid substantially more.[6]

Other sources also highlight that there have historically been – and continue even in light of the initiatives to improve safety after the Rana Plaza collapse – wide variation in working conditions across factories. One of the Post-Rana Plaza initiatives of Western retailers conducted building safety audits of 279 exporting factories in the commuting zone for workers in our sample. The audits reveal significant variation in compliance with the initiative's building safety requirements even among 100% export-oriented establishments: Factories ranged from complying with fewer than half of requirements to about 85% of requirements (mean compliance was 63%, with a standard deviation of 7.4%). Appendix 3.8 provides more information about the building safety audits. Interviews Heath conducted with industry officials also underscore the difference between highly visible factories and more "shadowy" factories that try to evade detection from government inspectors and NGO watchdogs. This was relatively easy at the time of the survey (before post-Rana Plaza reforms), given that government inspectors were frequently out-manned. For instance, the European Commission 2014 reports that before Rana Plaza, the Department of Inspection for Factories and Establishments had 76 inspectors for 5000 factories. A private audit market sprung up as retailers sought to reassure their customers they were avoiding unsafe factories, but the results of these audits were rarely transparent, there were accusations of bribery, and even when safety violations were documented there was no mechanism in place to force factories to address the violations (Clifford and Greenhouse 2013).

[6] After the Rana Plaza collapse in 2013, the minimum wage was raised to 5300 taka. While we know of no systematic wage data collected after this hike, anecdotal evidence from conversations from Heath's trip to Dhaka in December 2014 suggest that there is indeed now less variation between factories in wage levels.

Identifying firms with good working conditions

We use workers' reports of problems in the workplace, of the relationship between workers and management, and of services available to measure working conditions in each factory that she or he has worked in. Table 2.2 lists the specific variables. While the unit of observation in the empirical analysis is generally the worker-month level (so that the left column corresponds to the variation we use in the analysis), we also provide the rates of each outcome at the worker-factory level and in the worker's current factory to show how the weighting by time in the factory affects the reporting of conditions and how the conditions on average evolve over a worker's career.

Specifically, the problems that we use to construct the index are: Hours too long (8.2 percent of monthly observations), abusive management (3.2 percent), bad/unsafe working condition (0.8 percent), not paid on time (5.8 percent), unpaid overtime (1.9 percent), fired for sickness (1.7 percent), and "other" (1.6 percent). Note that the reports of problems are somewhat lower in the current factory.[7] Problems were more common when reported at the worker-spell level than the worker-month level, suggesting that workers spend less time in factories when there are problems present.

We also use a worker's categorical response to the question, "Overall, during your time in this factory, did you feel you had good relations with the management?"; options were excellent, very good, good, bad, or very bad. The modal response, given in 67.0 percent of worker-months, was "good". Finally, we use information on whether the factory provides medical care for ill workers (70.5 percent of worker-months) and whether the worker received an appointment letter (37.4 percent of worker-months). Appointment letters lay out the details of employment (such as salary) and say that the worker cannot be dismissed without cause.

We assume that these variables all reflect a single index of firm-level working conditions, independent from the mean wage paid by the factory. For instance, problems in the relationship with the management could reflect management's response to workers' complaints about working conditions. If workers are risk averse, then they also value the stability afforded by appointment letters. Relatedly, while some of the problems relate to wages (late payment or unpaid overtime), they would not be reflected in the base wage but lower the utility the worker gets from a baseline salary by increasing the uncertainty in that salary or decreasing the de facto hourly wage.

We construct a working conditions index variable using the scores on the first principal component of the matrix of working condition variables. Call this variable $\hat{c}_f$. We recode the variables reporting problems to reflect lack of a particular problem, so that higher values indicate more favorable conditions and we created a series of mutually exclusive binary indicators from the categorical variable representing a worker's relationship with management. Accordingly, higher values in our index correspond to better working conditions. This interpretation is not always valid with principal components, even if variables are coded to have the same direction. In our case, however, all variables have the same sign for the loading on the first component. To ensure that this interpretation is robust, we also implemented a non-negative principal components procedure (C. D. Sigg and Buhmann 2008; C. Sigg 2014) and found no substantive (and only minimal numerical) differences. Since all variables are binary, we also implemented non-linear PCA (Gifi 1981; De Leeuw and Mair 2007) and again found no substantive differences in our results.

[7]While this pattern is consistent with our argument that workers move towards factories with better conditions over time, it is also possible that underreporting in overall measures of working conditions is more severe in their current factory if workers fear retaliation if management hears about their responses. While there were no reports from enumerators of workers expressing concern about whether the responses would actually be kept private, we also show in Section 2.5 that the key results on working conditions remain if we discard a respondent's report in her current factory.

In interpreting this index, we also assume that conditions do not change in response to workers' characteristics, so that workers sort based on fixed characteristics of factories, rather than factories offering different conditions to individual workers. We address this concern in several ways. First, in our empirical analysis of worker-level characteristics and working conditions in Section 2.5, we show that our results persist when we reconstruct measures of working conditions that do not use a worker's own report. We also test for within-factory differences in reported working conditions between migrant and local workers employed at the same factory, and find differences that are much smaller in magnitude than between-factory differences.

Second, the possibility that conditions are endogenous to worker-level characteristics may be a particular concern with appointment letters. While there is anecdotal evidence that the decision to offer appointment letters is made at the factory level (the Labour Law of 2006 required them, and before that, it was considered a characteristic of responsible factories), it is possible that some factories offer appointment letters to only their valued workers. Then the interpretation of the relationship between variation in factory quality from appointment letters and a worker-level characteristic such as migration status would reflect the value employers place on this characteristic rather than differences in how workers sort in factories based on working conditions. Accordingly, in section 2.5 we also display the relationship between migrant status and individual measures of working conditions, and show that the results are not driven by appointment letters, or more generally, any single measure of working conditions.

Figure 2.2 shows the estimated distribution in working conditions. The top panel shows the distribution of workers per factory. While the majority of factories in the data have only one worker appear – this is unsurprising, given that this includes any factory in which a sampled worker ever worked, even if they were living in another location – there is a large absolute number of factories with multiple workers in the sample, which is important for our empirical specifications that include wages and firm fixed effects. The bottom panel shows the distribution of working conditions. The long left tail shows that the worst factories tend to have many problems.

Finally, we assess the empirical plausibility of the assumption that factory-level working conditions are stable over time in the top figure in figure E2. If factories were changing their working conditions over time – either improving or regressing – we would expect the slope on the local polynomial smoother to be nonzero. The slope, however, is close to zero throughout the time period. In particular, from about May 1999 to July 2009, which are the 10th and 90th percentiles in the distribution of observations across time (see bottom figure in figure E2), the slope remains approximately zero. Barring the case where factories change conditions in ways that cancel out on average, the figure is consistent with factories maintaining one type of conditions and employing one type of worker over time.

2.3 Model

In this section, we characterize a model of workers' decisions of initial firms and subsequent mobility if they are informed about working conditions when beginning work versus if they are not. We then characterize the model's predictions on migrants' labor outcomes, versus locals, under several plausible assumptions about the differences between migrants and locals. For one, migrants could precisely be the workers who are more likely to be informed. However, we also consider the possibility that migrants have lower mobility costs, greater relative preference for wages over working conditions, and migrants are lower productivity. Out of these scenarios, only the one that migrants are less likely to be informed (but this difference fades with experience) generates the entire set of empirical predictions that we find in section 2.5: migrants are in factories with higher wages but worse working conditions; as they gain experience, they move more than locals and differentially improve their working conditions.

Section 2.4 then shows that the model's main intuition and predictions persist when we consider several extensions: considering workers' participation decisions, allowing labor markets to be imperfectly competitive, allowing for vertical productivity differentiation, and considering the possibility of a turnover cost to firms. We also examine another time-varying difference between migrants and locals – that migrants' relative preference for wages fades with time – and provide some suggestive evidence in favor of our information-based model.

Set-up and baseline results

Workers have marginal revenue product π. They get utility from wages (w) and working conditions (c). Utility is separable in wages and working conditions:[8]

$$u(w,c) = u_w(w) + \beta u_c(c)$$

Some workers observe the working conditions in a firm but others cannot.[9] Firms can pay a per-unit cost of p to improve conditions. Labor markets are competitive, so firms bid the total offer up to the workers' perceived utility.[10] That is, they offer $(\pi, 0)$ to uninformed workers, and to informed workers they offer the (w, c) pair that solves:

$$\begin{aligned}
&\underset{w,b}{max} \quad u_w(w) + \beta u_c(c) \\
&s.t. \quad w + pc = \pi \\
&FOC: \quad u'_w(w) = \frac{\beta}{p} u'_c(\frac{\pi - w}{p})
\end{aligned} \tag{2.1}$$

The FOC indicates that firms offer a level of conditions to informed workers that equates the marginal value of wages with the marginal gains from better conditions, scaled by the cost of

[8]If we relax this assumption – say, the marginal utility of wages could be higher with worse conditions – then there could be firm-level differences in working conditions even without heterogeneity in workers' level of informedness, since workers' utility could either be maximized with a (high wage, low conditions) offer or a (low wage, good conditions) offer. However, absent an additional assumption on migrants versus locals – such as the level of informedness – nonseparability alone wouldn't generate the same pattern of sorting across the firms we see in the data. Do note though that nonseparability would lower the utility loss from the model's predictions on uninformedness. Thus, it would attenuate the testable implications of the model that stem from previously uninformed workers' taking steps to find firms that are better matches, since the uninformed workers would at least value the additional wages that the low-conditions firm is paying them.

[9]There is a close parallel to the IO-behavioral literature on shrouded attributes (Gabaix and Laibson 2006), in which some consumers are Bayesian updaters who infer that hidden attributes of a product are highly priced, whereas "unaware" or myopic consumers do not. These uninformed workers would then represent the unaware consumers in their model. Our theory also parallels Gabaix and Laibson 2006 in demonstrating that competition need not necessarily induce firms to reveal information.

[10]So the uninformed workers' prior is key, since they will infer conditions based on the wage offer they get. In a perfect Bayesian equilibrium where workers know π, they will infer that firms with higher wages can only afford to do so because the conditions are bad. So our assumption that they do not do this is undoubtedly strong, but we think it is reasonable given just how little migrants typically know when first looking for work in a garment factory.

improving conditions. Assume that conditions must be the same for every worker in a firm, so that firms will either specialize in informed or uniformed workers.[11]

Now consider a second period in which previously uninformed workers can now observe working conditions. All workers can choose to switch firms, but would have to pay a mobility cost $m \sim U[0, \bar{m}]$ to do so. So they will switch if they get an offer (w', c') such that

$$u(w', c') - m \geq u(w, c) \tag{2.2}$$

Note that informed workers have no reason to switch firms, since they are already receiving the wage offer that would maximize their utility.[12]

How are migrants different?

There are several potential ways in which (internal) migrants could differ from locals in the above model. We list several possibilities and explain the results that would ensue if each were incorporated into the model.

Migrants are more likely to be uninformed

In the model, workers who are uninformed about working conditions will end up in firms with worse conditions but higher wages. There is indeed reason to believe migrants are less informed than local workers upon beginning work. There is little information about firms in print, so workers tend to rely on word of mouth. Indeed, qualitative evidence has documented that migrants typically know very little about the garment industry overall upon arrival in an urban or peri-urban area, much less about individual firms (Absar 2009). In the extreme, there are anecdotal reports of unscrupulous factories issuing attendance cards without names to newly hired workers so that the workers have no recourse to collect unpaid overtime (Ahmed 2006). Indeed, in our data, table 2.1 demonstrates that migrants are less likely to have received a referral in their current position, and even conditional on receiving a referral, they are less likely to know more than one worker in the firm (48 percent of referred local workers knew at least one other worker in the firm, compared to 36 percent of referred migrants, $P = 0.089$).

Further predictions on migrants will result if the difference in informedness fades with experience in the industry. In the context of the model, assume that all workers can observe working conditions in the second period. Since migrants started off in firms with worse conditions, it is more likely to be worthwhile to pay a cost to move in order to seek out a firm with a preferable balance between conditions in wages. So migrants are more likely to move factories and improve their working conditions with time in the industry than locals, while locals improve their wages more: $\Delta c_{migrant} > \Delta c_{local}$. So migrants' working conditions will improve with time in the industry more than local workers'.

[11] If there are economies of scale in improving conditions, the model would imply that large firms are more likely to specialize in conditions and thus would attract more local workers. So they would then pay lower wages, unless there are firm-level differences in productivity that would imply that more productive firms grow bigger and also pay higher wages.

[12] And even if there are idiosyncratic taste shocks to working in a specific firm that would lead informed workers to switch firms, the uninformed workers would still switch more often unless somehow they receive fewer of these idiosyncratic shocks.

Migrants have lower mobility costs

Another possible difference between migrants and locals is that migrants have lower mobility costs ($\bar{m}_m < \bar{m}_l$), since they have less of a network in any one particular area or factory. If so, then the prediction the migrants have higher mobility that we earlier derived from the assumption that migrants are less likely to be informed upon beginning work could just be because it is easier for migrants to move. However, it would then be easier all along for migrants to seek out factories with good conditions, so they would be in factories with better conditions than locals, whereas locals would be the ones in factories with higher wages.

Migrants have greater relative preference for wages over conditions

Another potential explanation for why migrants are in factories with worse conditions is that they can actually observe working conditions, but they have a higher relative preference for wages over working conditions than do locals ($\beta_m < \beta_l$). For example, if migrants prefer living in their home villages, they would hope to earn a lot of money quickly, even at the risk of their safety or comfort. If so, they would make perfectly well-informed choices to be in firms with worse working conditions but higher wages. But then, if anything, when they move, they would seek out firms with even higher wages (and worse conditions), compared to locals. And this assumption generates the opposite prediction as would the assumption of differences in informedness: the conditions faced by migrants would actually worsen with experience in the industry, compared to those faced by locals.

Migrants are lower productivity

Finally, there could be differences in average productivity (π) between locals and migrants who choose to enter – and stay in – the garment industry. The difference could go in either direction: Migrants could be lower productivity due to worse education or experience with modern technology, or they could be higher productivity given positive selection of migrants. If they are lower productivity, this could explain why they are in factories with worse conditions, but not why they are actually in factories with higher wages. By extension, if they are higher productivity, it is hard to explain why they are in firms with worse working conditions.

Summary of testable implications of different assumptions about migrants

Table 2.3 summarizes the predictions of each of the potential differences between migrants and locals described in Section 2.3. There are many reasons why migrants would be in factories with worse working conditions than locals, including the possibility that they knowingly chose that option because these factories pay higher wages. However, the fact that after they begin working, they differentially move towards better conditions than do locals suggests that they actually do have a preference for better conditions and begin trying to improve their conditions as they learn about the variance of working conditions between firms.

It is possible that several of the potential differences between migrants and locals are present simultaneously. If so, then a finding in line with any given assumption suggests that that particular difference is the strongest. For instance, migrants could be both more poorly informed about conditions and have a higher desire for money over conditions. In this case, a finding that migrants move towards better conditions with time would imply that the difference in informedness (that fades with time) is stronger than migrants' preference for money over conditions, which would (ceteris paribus) tend to say they move towards factories with worse conditions over time compared to locals, who are the ones seeking better conditions in that model.

2.4 Extensions

While the baseline model in the previous section generates key predictions on the wages, working conditions, and mobility of migrant workers versus locals, its simple set-up abstracts away from several realistic features of the garment industry in Bangladesh. In this section we consider several potential extensions to the main model. First we consider extensions related to workers, namely variation in their workers' outside options or the possibility that workers' relative preference for wages versus working conditions can change over time. Then we consider imperfectly competitive labor markets. Finally, we consider extensions related to firms, looking in particular at firm-level variation in productivity, a mobility cost that accrues to firms, and variation in the cost of improving working conditions.

While these extensions are realistic in the context of Bangladesh's garment industry– and indeed, we provide several pieces of ancillary evidence consistent with these models – they do not substantially affect the predictions in the main model. Similarly, while several extensions could generate some of the predictions as the main model even if migrants and locals have the same information about firms when beginning work, most cannot singlehandedly explain the set of empirical results in Section 2.5 without the assumption of differences in informedness between migrants and locals. We acknowledge that the possibility that migrants begin with a higher relative value of wages (compared to working conditions) than locals – but this difference fades with time – can, by contrast, generate all the predictions of the baseline model. We do, however, provide some suggestive evidence that is more consistent with a story of imperfect information than time-variant preferences.

Extensions related to workers

Building in a participation constraint

It is useful to incorporate reservation utility both because it is another potential difference between migrants and locals and to help interpret the retrospective nature of the data. Without variation in workers' productivity or outside option, the possibility that workers drop out if their total compensation is below reservation utility would not fundamentally change the model, since there would be no selection on unobserved characteristics. However, suppose that there is variation in workers' marginal revenue product (π). Since predictions on the change in a worker's wages, working conditions, or mobility between firms can be tested among workers whose utility from the (w, c) offer they receive is at least as high as their outside option in both periods, the relationship between π and the outside option (are better or worse workers more likely to leave the industry?) determines whether the predictions are tested on a group of relatively high or low productivity workers. However, the fundamental predictions of the model – namely, the comparisons between migrants and locals – should still persist in the sample of stayers.

Differences in reservation utility between migrants and locals could, by contrast, generate differences between migrants and locals who stay in the labor market in consecutive periods. Migrants could have a lower reservation utility if they are less aware of non-garment job opportunities in the area, or if their job opportunities at home are inferior. They would thus be more likely to remain in the industry after a bad (w, c) offer than locals. As with the possibility that migrants are low productivity, this could explain why they are in factories with worse conditions, but not why they are actually in factories with higher wages.

Time-varying relative preferences for wages

While the baseline model allows for the possibility that migrant workers have different relative preferences for income (versus working conditions) than locals, these preferences are assumed to be time invariant. However, it is possible that this preference varies over time. Of particular interest is the possibility that migrants initially have higher preference for wages than working conditions than locals, but this difference decreases with experience in the industry. For instance, perhaps migrants have depleted savings or given up on risk-sharing networks as part of their move, and thus they have a particularly high value of income just after moving as they build up savings. This assumption can generate the prediction that migrants move towards better conditions with experience, even in a world of complete information.

Some suggestive evidence against this hypothesis is presented in figure E3, which graphs the current average value of assets of migrants versus locals by experience. If migrants were building up precautionary savings (or replenishing savings after the costs of a move), we would expect the slope of the curve for migrants to be higher than for locals. It does not; the curves are particularly clearly parallel prior to 8 years of experience, where the majority of the support of the distribution of experience is (8 years is the 84th percentile of the experience distribution). Indeed, the difference in slopes is small (0.027 log points per year) and statistically insignificant ($P = 0.434$). While this does not completely rule out other reasons why migrants' preference for wages may diminish over time – say, the marginal value of remittances could drop as their ties to their home community weaken – we still view this suggestive evidence against the most likely stories in which time-varying preferences drive the mobility of migrants towards better working conditions as their careers progress.

Imperfectly competitive labor markets

While the baseline model assumes that firms bid their total spending on wages and working conditions up to the value of the worker's productivity, firms may have some market power in the labor markets in which they operate. However, building this into the model does not substantively change the main predictions as long as the firm's problem is separable in the total compensation they offer workers and the division of this compensation between wages and investments in working conditions. If so, then the main model applies with a total compensation of $\tilde{\pi} < \pi$. For example, consider the opposite extreme from a competitive labor market: the firm has all the bargaining power and thus makes a take-it-or-leave-it offer to the worker. In this case $\tilde{\pi}$ would be set so that the worker's utility from wages and working conditions equals her reservation utility, but again it would still consist of relatively higher wages and lower conditions for the uninformed workers.

Extensions related to firms

Firm-level variation in productivity

Suppose firms vary in productivity, so that workers with the same ability have different marginal revenue products in different firms. These differences could either be permanent (say, due to variation in managerial ability), or temporary (the firm gets a big order that it needs to fill).

We first consider permanent differences in productivity between firms. In the extreme, the dispersion across firms is entirely vertical (so that there are no firms with similar marginal revenue products competing for workers). If so, then firms will set total compensation with some degree of monopsony power (as described in the previous subsection), and the division of this total compensation between wages and investment in working conditions will depend on the relative number of informed and uninformed workers, as in the baseline model. However, unless this monopsony power is complete, total compensation will still positively covary with productivity, as has been demonstrated to be the case in a wide variety of labor markets ((Blanchflower, Oswald, and Sanfey 1996); (Van Reenen 1996); (Budd, Konings, and Slaughter 2005)).

Some evidence for the empirical relevance of this extension is provided in figure E4, which graphs the distribution of wages versus working conditions of firms in the sample. The baseline model in Section 2.3 predicts a negative correlation between wages and working conditions, as firms paying the same total compensation decide to specialize in either wages or working conditions. However, figure E4 shows that there is a net zero relationship between wages and working conditions, suggesting that differing levels of spending on total compensation represents a countervailing force – such as vertical differentiation – that would tend to make total wages and working conditions positively comove.

If the process by which workers are matched into these firms of different tiers is driven at least in part by search frictions (rather than entirely by positive assortative matching based on time-invariant worker characteristics, which leaves no role for workers' mobility between different tiers of firms), this extension can generate the higher mobility of migrants under the assumption that migrants have greater relative preference for wages ($\beta_m > \beta_l$) rather than our key assumption that migrants are more likely to be uninformed. Migrants would be more willing to pay a mobility cost to move to a higher productivity firm than locals. Note, however, that this prediction that migrants have higher mobility is not unambiguous: it is now the locals who are trying to move in order to seek out better conditions, in this case, by finding higher productivity firms that offer better working conditions. So the relative variance in conditions versus wages would determine whether the migrants or locals are more likely to move.

Next, consider the possibility that, due to demand shocks, the worker's marginal revenue product in a specific firm increases at a certain time. If so, then after receiving the positive shock, the firm would increase compensation to entice workers to move there, and workers who move are likely to end up in the firms with positive demand shocks. If migrants have lower mobility costs and there is also a sunk cost to looking at other jobs, then while migrants particularly want to improve their conditions upon moving as predicted by the baseline model, if the demand shock is sufficiently large, they would also improve their wages, which would generate a channel through which migrants earn more with experience. We return to this possibility in Section 2.5 when we discuss the wage trajectory of migrants versus locals with experience.

Additional mobility cost to firms

The model assumes that the cost of mobility is borne by the worker, and since uniformed workers do not anticipate that they will want to move, there is no scope for firms with good conditions to attract uninformed workers with a (w, c) offer that will save workers later moving costs. A related question is whether there are additional costs imparted on the firm to losing workers, which the firm then would internalize when making original wage offers. For instance, there could be costs to hiring or training new workers, or new workers could have initially lower productivity while they grow accustomed to the new firm. If these costs are important, then firms will lower the total value of compensation offered to uninformed workers by the amount of the turnover cost. In the extreme, if the turnover cost is high enough, firms will offer all workers the (w, c) bundle that maximizes the utility of an uninformed worker, and the model in Section 2.3 no longer applies. However, for lower values of turnover costs, some firms will still choose to go after uninformed workers.

Do firms prefer to target migrants or locals?

In the baseline set-up – and even in the above scenario where mobility represents a direct cost to firms – firms still remain indifferent between targeting migrants and non-migrants. With perfectly competitive labor markets, firms bid the expected payment to a worker up to the value of their output (net of any expected mobility costs accruing to the firm), and firms are indifferent between migrants and locals. Even if we relax the assumption of perfectly competitive labor markets, the firms still presumably would not choose to target migrants if doing so was unprofitable.

The above arguments are predicated, however, on the principle that firms are ex ante identical. An interesting alternate possibility to consider is whether there exists fundamental heterogeneity between firms that would lead some firms to target migrants and others to target locals. One possibility is variation in the relative cost of improving working conditions, p. This heterogeneity will prompt firms with lower cost of improving conditions to target locals, and firms with higher costs to target migrants. If there are sufficient firms in each category that firms again bid the value of total compensation up to the worker's productivity, the same argument from earlier applies, but consider instead an alternate extreme where there is just one firm of each category. If firms make a take-it-or-leave-it offer to workers, then firms targeting locals will be more profitable, because it is always weakly cheaper for them to offer a given (w, c) bundle.

Since this result is driven by the fact that firms with lower p will be overall lower-cost producers, in order to isolate the difference in conditions, suppose instead that firms with higher p have a higher output by the differential in the cost of providing the optimal level of conditions c^*, as given in equation 2.1. That is, the productivity of a worker net of the cost of providing the worker's preferred working conditions would be equalized. If so, then firms targeting migrants will instead be more profitable, since they have higher output and spend nothing on working conditions. This profitability advantage could dissipate or reverse, however, in the presence of turnover costs, as described in the previous sub-section.

Overall, then, the model does not give strong predictions on whether firms targeting migrants or locals will be more profitable. While we don't have measures of profitability of the firms, we can explore the relationship between working conditions, wages, the decision to hire migrants, and whether the firms listed by workers in the original 2009 survey were still operational in 2014, when Heath and Mobarak did a follow-up survey of the original firms.[13] At that time, 47 percent

[13] Previous literature has documented a positive correlation between firm-level productivity and survival

of firms were still operating and 40 percent were still operating under the original management. While we cannot rule out the possibility of measurement error (maybe we were unable to locate some firms that were actually operating), other studies have also found high rates of firm-level turnover in the garment sector in Bangladesh (Labowitze 2016).

The results are given in table E1. There is a positive effect on working conditions on firm-level survival when the regression is weighted by the number of observations in a factory, and the effect is relatively large: a one-standard deviation increase in working conditions leads to a 3.4 percentage point increase in the probability of surviving. Firm-level wages are also positive, although statistically insignificant. While the lack of a strong positive relationship may initially seem surprising, note that this pattern fits with the argument of this paper, that higher productivity is not the only reason that firms would pay higher wages. There is also a positive relationship between the percentage of migrants in a factory and the probability it survives, though it is only significant in the unweighted regression. Overall, we interpret these results as providing some evidence in line with the theory that firm that are otherwise more profitable have better ability to improve working conditions.

2.5 Empirical strategy and results

In this section, we explain how we test the results of the model's predictions on the factory level working conditions, wages, and the mobility of migrants versus locals, in the context of the retrospective panel.

Firm-level working conditions

We begin by establishing the differences in the working conditions of migrants versus locals, across their experience in the industry. We estimate a regression that examines the factory-level working conditions $\hat{c}_{ft}$ faced by worker i in factory f at time t as a function of whether that worker is a migrant and other worker-level characteristics (experience, education, gender) assembled in the vector X_{ift}:

$$\hat{c}_{ift} = \beta Migrant_i + \gamma' X_{ift} + \varepsilon_{ift} \tag{2.3}$$

Table 2.4 gives the estimation results. We standardize the outcome variable to have mean zero and standard deviation one. Consistent with the model's main prediction for working conditions, the coefficient on *Migrant* in the first column indicates that over the course of their careers, migrants are in factories with on average 0.29 standard deviations worse working conditions than locals. The second column shows that this effect is not due to differences in experience, education, or gender between migrants and locals; the coefficient on *Migrant* remains unchanged with these controls.

The third through sixth columns focus only on the current observation for each worker to allow for the inclusion of village fixed effects (since we only know the current village of residence of each worker). This sample also facilitate interpretation by including only one observation per worker. The coefficients get smaller when only the current observation is used. This result is consistent with Prediction 1b in Table 2.3 that the difference in informedness between migrant and local workers fades over time. Migrant workers differentially move towards better conditions

among manufacturing firms in developing countries ((Frazer 2005); (Söderbom, Teal, and Harding 2006)), though in Söderbom, Teal, and Harding 2006 the relationship is not present under small firms, which they argue is driven by a positive correlation between productivity and the owner's outside option.

compared to locals. Still, there is a marginally statistically significant difference between the current working conditions of migrants and locals (columns 3 and 4). Columns 5 and 6 show that this difference is unchanged when village fixed effects are included: At the time of the survey, migrants were in factories that had 0.18 standard deviations lower measured working conditions than locals in the same village. So there is no evidence that the tendency for migrants to be in factories with worse conditions is not driven by residential sorting of migrants into areas in which the factories have worse conditions.

The relationship between working conditions and migration is far stronger than the relationship between other worker-level characteristics (namely, experience, education, and gender). Returning to Table 2.4, in the sample that includes past observations (column 2), each year of education is associated with a 0.031 standard deviation increase in working conditions. Male workers are also in factories with an average of 0.12 standard deviations worse working conditions than females, although this is effect is not significant at conventional levels. Both effects also disappear in the current sample of workers, and in neither the full nor current sample is there a relationship between experience and working conditions.[14]

Another implication of migrants' tendency to sort into factories with systematically worse working conditions is that they will sort into factories with other migrants. Figure E5 shows the distribution of the migrant status of other workers in a factory faced by migrants versus locals. While approximately 60 percent of migrants are in factories with only other migrants (among the sampled workers), there is a much wider distribution of the percent migration status among other workers for locals; the differences are indeed highly statistically significant.

In Appendix 3.8, we implement several tests of the robustness of the results in table 2.4. First, table E2 demonstrates their robustness to three important alternate constructions of the working conditions index. Panel A provides reassurance that migrants' tendency to face worse conditions within a factory does not drive their tendency to report worse working conditions; there is an almost identical relationship between migrants and working conditions if we reconstruct the measure of working conditions leaving out the worker's current report. Panel B reconstructs the measure of working conditions leaving out workers' reports from their current factories. If workers are more hesitant to report worse working conditions in their current factory – and differential sorting of workers into factories over time interacts with migration – then it is theoretically possible that this underreporting could driven some of the estimated relationship between migration and working conditions. However, with the exception of the specification that uses only current data and village fixed effects (which asks a lot of the data, given that we're throwing away current reports) the coefficient remains unchanged, suggesting that any differential reporting in the current factory does not drive the estimated migration effect. Finally, Panel C reconstructs the measure of working conditions using only one observation per worker-factory match, as opposed to weighting workers' reports by their tenure at the factory. The results remain unchanged. Table E3 looks at each individual component of the working conditions measure; there is no evidence that the results in table 2.4 are driven entirely by a small number of measures. Moreover, there are particularly strong effects on the measures that may seem to measure bad working conditions particularly well – abusive management, bad/unsafe working conditions, no medical care, and a

[14]Given that the sample consists mostly of migrants, the zero coefficient on experience may at first seem to contradict the model's prediction that migrants move towards better conditions with experience. However, in Section 2.5, we show that a specification with individual fixed effects – our preferred specification for analyzing changes over time – does display a positive overall coefficient on experience, suggesting that changes in the composition of the sample over time may confound the experience estimates in the retrospective panel.

bad relationship with management.

Table E4 directly assesses the validity of the model's assumption that all workers face the same conditions within a factory by comparing working conditions reported by workers at the same factory. The point estimate on migrant is -0.12 standard deviations, but it is statistically insignificant and lower in magnitude that the effect of migrants on the factory-level measure. Thus, even if there are some differences within factory in how migrants are treated, these are considerably smaller in magnitude than the factory-level differences documented in table 2.4.

Table E5 also shows the robustness of the results to alternate definitions of the migrant variable, in particular, defining as a migrant as anyone not from the village in which they currently reside, or anyone not living in the village in which they currently reside by age 10. The point estimates vary with how strictly the migrant variable is defined, but generally support the main results.

Finally, we provide some supplemental evidence for the role of information in determining the level of working conditions faced by workers by examining referrals. While referrals could serve a variety of purposes – and have been argued to increase effort in the context of the garment industry (Heath 2018) – it is also plausible that referrals serve to inform workers about the working conditions in a given factory. Table E6 includes a dummy variable for whether the worker was referred in equation 2.3. In the sample using past observations, workers who are referred are in factories with 0.067 standard deviations better working conditions. While the coefficient rises to 0.10 standard deviations when controls for sex, education, and experience are included, neither coefficient is statistically significant at traditional levels. The effect of referrals becomes borderline significant in the sample of current observations when village fixed effects are included. We thus consider the relationship between referrals and working condition to be additional suggestive evidence of the theoretical model's focus on the importance of information in helping workers assess working conditions at the factories in which they choose to work.

Firm-level wages

We next test the model's prediction on the average wages of factories with and without migrants. To do this, we compare the coefficient on *Migrant* in a wage regression with and without factory fixed effects:

$$log(w_{ift}) = \beta_{ols} Migrant_i + \gamma' X_{ift} + \varepsilon_{ift} \tag{2.4}$$

$$log(w_{ift}) = \delta_f + \beta_{fe} Migrant_i + \gamma' X_{ift} + \varepsilon_{ift} \tag{2.5}$$

Table 2.5 gives the coefficients on *Migrant* and the other worker-level characteristics in regressions with and without firm fixed effects. Consistent with the model's prediction that migrant workers sort into factories with higher wages (but worse conditions) compared to locals, over the course of their careers, migrants earn 4.9 percent more than local workers with the same characteristics, and surveyed migrants were currently earning 8.1 percent more than locals, although neither effect is statistically significant at conventional levels. However, in both cases the coefficient on migrant flips sign when factory fixed effects are added.[15] Indeed, the fact that the coefficients are statistically different from each other confirms that migrants are indeed in firms with higher wages.

[15]This negative within-firm coefficient on migrant suggests that in the context of the discussion in Section 2.3, if anything, migrants are lower average productivity, unless there is a non-productivity-based reason that migrants earn less than others in the same firm (such as lower bargaining power in a noncompetitive labor market).

Educated workers are also in higher-paying firms, but male workers are not. The returns to experience become less concave with firm fixed effects, suggesting that part of the diminishing returns to experience is driven by the sorting of workers across firms.

Mobility

The model's next set of predictions relate to differential mobility of migrants versus locals as they begin to observe working conditions and reoptimize accordingly. Firstly, the model predicts that migrants will have higher mobility than locals. We test this with a discrete-time hazard model, where the outcome is one in months where a worker leaves a factory for another factory and zero in months in which a worker remains in the factory.

$$1(Leave)_{ift} = \beta Migrant_i + \gamma' X_{ift} + \varepsilon_{ift} \tag{2.6}$$

Table 2.6 gives these results. We report average marginal effects from a logit specification. The first column indicates that migrants are 1.4 percentage points more likely to leave one factory for another in a given month than locals; this is a very large effect relative to the average mobility rate of 2.6 percent per month. The second column shows that firm fixed effects decrease the magnitude of the migration coefficient to 0.64 percentage points, which is no longer significant at traditional levels ($p = 0.173$). This is consistent with the model in the sense that the increased mobility of migrants is not driven entirely by a lower mobility costs, rather, they are also more likely to end up in factories that are worth paying a mobility cost to leave.

Changes in conditions and wages with experience

Finally, in table 2.7 we further test the model's prediction that the gap in conditions between migrants and locals fades with time. First we include an interaction between *Migrant* and experience in equation 2.3. When we do this, the results (shown in column 1) are not statistically significant and the point estimate on the interaction of $Migrant \times Experience$ is actually negative. However, the OLS results conflate changes in the composition of the workforce over time with the within-worker changes in improvements suggested by the model. To isolate these within-worker changes, we include worker fixed effects in equation 2.3 and interact migration status (as well as education and gender) with experience. When we do this, we find that while the overall coefficient on experience is small in magnitude and not statistically significant – suggesting that the locals do not change their conditions with experience, migrants do improve their working conditions with experience. Specifically, with every year of experience, the working conditions faced by a migrant improve by 0.031 standard deviations, compared to the trajectory of a local.

In the third and fourth columns, we show the same regressions, but with the outcome as wages rather than conditions. A strict interpretation of the model in which migrants are less likely to be informed would predict that migrants actually lose wages with experience, relative to locals, as they move away from high-wage, low-conditions factories. By contrast, we find no average difference in the within-worker wage trajectory of migrants versus locals. One possible countervailing force to the baseline model's prediction is that migrants are better and learning-by-doing, and they differentially improve their productivity with experience, as suggested by Duleep and Regets 1999 or Berman, Lang, and Siniver 2003.

As explained in Section 2.4, an alternative explanation is that there could be wage gains upon switching factories. Indeed, in the data, there is an average 0.37 percent monthly wage increase among workers if staying in a factory versus a 19 percent increase if changing factories. Then, if migrants have lower mobility costs, they move towards both better conditions (as predicted by the model, even if they have the same mobility cost as locals) and higher wages. Finally, if the mobility cost is sunk (rather than the way it is modeled, when individuals know the options for free and decide whether to move), then after individuals (who are more likely to be migrants) pay the mobility cost, they will then move for both better conditions and better wages.

Finally, we provide two additional pieces of evidence consistent with the tendency of migrants to move towards factories with better conditions as they progress. Table E7 tests the prediction that as migrants move towards factories with better conditions, these factories should employ more local workers. In several regressions with only migrant workers, we find marginally statistically significant evidence that this is the case. Columns (1) and (3) show that each year of experience that a migrant has is associated with a 0.18 percentage point increase in the probability that they work in a factory with at least one other local. The estimate is not statistically significant, which is in part be due to the fact that nearly half of all migrants in our sample, 48%, never work in a factory with a local. Adding worker fixed effects allows us to estimate how experience is correlated with within-worker changes in the likelihood of working a factory with a local. In column (3), we find that an additional year of experience is associated with a 0.67 percentage point increase in this probability ($p = 0.106$). In column (4), we estimate a conditional logit model, which estimates that the probability increases by 1.96 percentage points per year of experience ($p = 0.089$). These two estimates are identified off of workers who switch between factories without and with local workers, which is about 33% of our sample; among this group, migrants are moving from factories with no locals to factories with locals. We also regress the count of locals who work in a factory on migrants' characteristics. In columns (5) and (6), we report the incident rate ratios from the Poisson models. Both regressions suggest that each additional year of experience is associated with an approximately 2% increase in the incident rate for the number of local workers in a migrant's factory. The column (5) estimate is highly statistically significant, but when worker fixed effects are added, the estimate becomes less precise ($p = 0.143$).

Finally, table E8 explores whether migrants are more likely to report having left past factories because of bad conditions. While not statistically significant, a large point estimate indicates that migrants were 5.9 percentage points more likely to have left a past factory because of bad conditions, as reflected in reported reasons for leaving such as "bad relationship with management" or "late payment". Further, the point estimates on the interaction term between migrant and experience is negative (columns 2 and 3), consistent with migrants becoming relatively less likely to report reasons related to working conditions compared to locals.

2.6 Conclusion

Given evidence of poor working conditions in many developing country industries, we propose a theory in which incomplete information leads to workers – and migrants in particular – working in factories with inefficiently low investments in working conditions. We examine this theory in the empirical garment industry in Bangladesh during a period in which rapid growth pulled lots of recent migrants from rural areas into the industry. Using a retrospective panel of the wages and working conditions through the career of 991 workers outside Dhaka collected in 2009, we argue that recent migrants are less able to observe working conditions across firms, and thus end up in firms worse working conditions than local workers. In particular, we show that during the course of their career in the garments sector, on average, migrant workers work at factories with working conditions that are between 0.2-0.3 standard deviations worse than local workers. At the same time, these factories if anything pay higher wages, suggesting that they compete for uninformed migrants by raising wages but not worker conditions. Our findings are consistent with a model in which firms select to specialize in informed or uninformed workers and offer different bundles of wages and working conditions in equilibrium.

As migrants learn about the industry, they demonstrate a revealed preference for improving their working conditions, compared to their wages. In particular, we find that migrant workers are more mobile than locals and that each additional year of a migrant's experience in the garments sector is associated with 0.03 standard deviation greater improvement in working conditions compared to locals. We find no average difference in changes in wages with more experience for migrants compared to locals. While a strict interpretation of our model would predict that, relative to locals, migrants should lose wages with experience, we argue that migrants' greater relative ability to learn by doing or lower mobility costs could represent countervailing forces.

Our findings provide important lessons for those who are interested in migration and manufacturing jobs as pathways to improved welfare for poor populations in developing countries. Previous research affirms the benefits of both internal migration and manufacturing jobs ((Bryan, S. Chowdhury, and Mobarak 2014); (Heath and Mobarak 2015a)); we nuance these findings, however, by documenting how labor market imperfections lessen these benefits. Our results also illustrate that competition for labor does not guarantee efficient investment in working conditions in the presence of imperfect information. Additional research is needed on how alleviating such information asymmetries impacts workers and firms in developing countries. Towards this end, Boudreau 2018 complements this study by experimentally varying workers' information about working conditions and studying the effects on workers' mobility and referrals. Together with the current paper, this body of research aims to provide information both on market frictions that explain how workers end up in jobs with poor working conditions, and what policy can do to minimize these frictions.

2.7 Figures and Tables

Figure 2.1: Garment sector employment

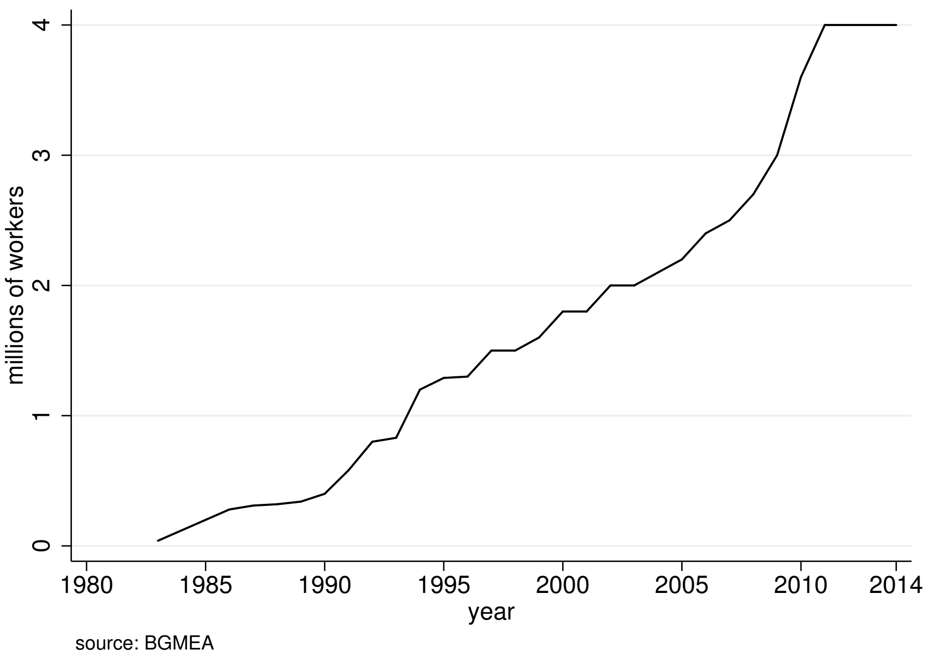


Figure 2.2: Factory-level variation in working conditions

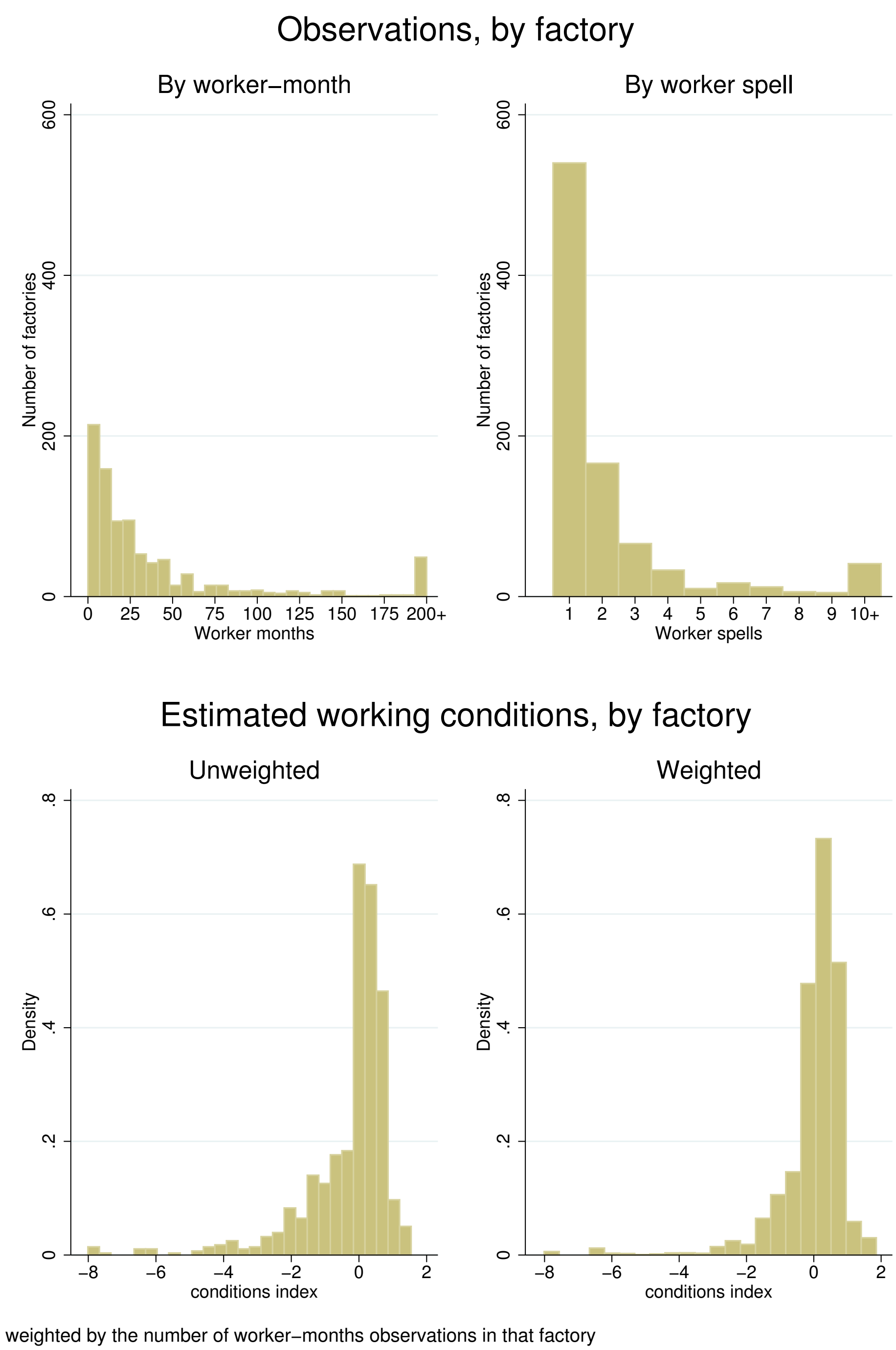


Table 2.1: Summary statistics

	Entire Sample		Migrants		Workers from Urban Areas		P-value of t-test, Migrants vs Urban	
	Males	Females	Males	Females	Males	Females	Males	Females
Panel A: Demographics								
Age	27.93	24.42	28.03	24.49	27.44	23.94	0.577	0.591
Years of Education	7.22	4.86	7.21	4.92	7.24	4.37	0.960	0.206
Years of Experience	4.92	3.57	4.86	3.45	5.26	4.53	0.447	0.014
Married	0.788	0.756	0.805	0.761	0.699	0.714	0.042	0.415
From Urban Area	0.167	0.114						
Originally From Surveyed Village	0.112	0.052	0.000	0.000	0.671	0.460	0.000	0.000
Years Living in Village (If not from Village)	4.21	4.41	4.46	4.49	2.97	3.84	0.040	0.339
Panel B: Socioeconomic Status								
House has Cement Floor	0.781	0.776	0.866	0.822	0.356	0.413	0.000	0.000
House has Electricity	0.966	0.955	0.986	0.969	0.863	0.841	0.000	0.000
Household has a Mobile Phone	0.774	0.673	0.756	0.657	0.863	0.794	0.046	0.030
Household Owns Current Residence	0.146	0.112	0.027	0.045	0.740	0.635	0.000	0.000
Household Owns Homestead	0.902	0.868	0.901	0.857	0.904	0.952	0.943	0.036
Household Owns Agricultural Land	0.553	0.476	0.589	0.494	0.370	0.333	0.001	0.016
Panel C: Job Characteristics								
Referred	0.347	0.317	0.311	0.311	0.528	0.367	0.000	0.380
Commute Time (Minutes)	19.13	19.13	17.56	18.17	26.99	26.90	0.000	0.000
Regular Hours	8.63	8.56	8.67	8.59	8.42	8.33	0.198	0.258
Average Daily Overtime in Peak Season	3.30	3.44	3.30	3.49	3.31	3.03	0.994	0.194
Tenure in Current Factory (Months)	27.22	26.89	24.90	25.70	38.85	36.18	0.000	0.015
N	438	553	365	490	73	63		

Table 2.2: Components of the Working Conditions Index

	All worker-month observations	All worker-factory spells	In current factory
Problems Listed			
hours too long	0.078	0.094	0.060
abusive management	0.033	0.037	0.021
bad/unsafe working conditions	0.009	0.013	0.009
not paid on time	0.059	0.071	0.030
unpaid overtime	0.019	0.024	0.017
fired for sickness	0.017	0.019	0.005
other	0.017	0.024	0.009
Relations with management (worst is "Very Bad")			
"Bad" or better	0.996	0.996	1.000
"Okay" or better	0.970	0.966	0.981
"Good" or better	0.822	0.800	0.830
Excellent	0.154	0.093	0.111
Other proxies			
appointment letter	0.376	0.281	0.345
provide medical care	0.711	0.642	0.753
N	48,687	2,267	991

Table 2.3: Summary of testable implications of different assumptions about migrants

	Migrants in Factories with: Worse Conditions	Migrants in Factories with: Higher Wages	Migrants Higher Mobility	$\Delta c_m > \Delta c_l$
1. More likely to be uninformed about conditions				
a. time invariant	✓	✓		
b. which fades over time	✓	✓	✓	✓
2. Lower mobility costs ($\bar{m}_m < \bar{m}_l$)	(opposite)	(opposite)	✓	✓
3. Greater relative preference for wages ($\beta_m < \beta_l$)	✓	✓	(opposite)	(opposite)
4. Lower productivity ($\pi_m < \pi_l$)	✓	(opposite)		

Note: The predictions of each row in the table assume that the given assumption is the only difference between migrants and locals. For instance, the first two rows assume that migrants are more likely to be uninformed, but have the same mobility costs, preferences, and productivity as locals.

Table 2.4: The relationship between worker-level characteristics and factory-level working conditions

	Dependent Variable = Index of working conditions ($\hat{c}_{ift}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
Migrant	-0.2931***	-0.3127***	-0.1663*	-0.1718*	-0.1801***	-0.1772***
	[0.086]	[0.096]	[0.096]	[0.096]	[0.052]	[0.055]
Male		-0.1153		0.0345		0.0531
		[0.103]		[0.066]		[0.059]
Education (Years)		0.0314**		0.0109		0.0091
		[0.016]		[0.008]		[0.008]
Experience (Years)		-0.005		0.0094		0.0092
		[0.022]		[0.008]		[0.007]
Past observations	Yes	Yes	No	No	No	No
Village fixed effects	No	No	No	No	Yes	Yes
Observations	50,180	50,114	990	987	990	987
R-squared	0.011	0.022	0.006	0.015	0.186	0.197

*Notes: The index of working conditions is described in section 2.4; it is standardized to have mean 0 and standard deviation 1. Migrant = 1 if the individual is was not born in Gazipur or Dhaka districts, as described in section 2.1. "Past observations" refer to any month in which they worker has been in the garment industry since she began working, constructed using the retrospective panel structure of the data, as described in section 2.1. In columns 1 and 2, standard errors clustered at the level of the individual. In columns 3-6, standard errors clustered at the level of the village. *** p<0.01, ** p<0.05, * p<0.1.*

Table 2.5: The effect of factory fixed effects on coefficients in a wage regression

	Dependent Variable = Log wage					
	(1)	(2)	P-value of test BetaFE = BetaOLS	(3)	(4)	P-value of test BetaFE = BetaOLS
Migrant	0.0490	-0.0155	0.0769	0.0806	-0.0436	0.002
	[0.043]	[0.048]		[0.051]	[0.071]	
Male	0.2103***	0.2255***	0.6057	0.2242***	0.2090***	0.571
	[0.034]	[0.032]		[0.029]	[0.039]	
Education	0.0377***	0.0289***	0.0380	0.0272***	0.0208***	0.162
	[0.005]	[0.005]		[0.005]	[0.006]	
Experience	0.1313***	0.1069***	0.0001	0.1100***	0.0986***	0.270
	[0.006]	[0.007]		[0.009]	[0.012]	
Experience squared	-0.0055***	-0.0042***	0.0004	-0.0040***	-0.0032***	0.141
	[0.000]	[0.000]		[0.001]	[0.000]	
Past wages	Yes	Yes		No	No	
Factory fixed effects	No	Yes		No	Yes	
Observations	46,847	46,847		877	877	
R-squared	0.313	0.642		0.361	0.743	

*Notes: Wage expressed in 2009 taka. Migrant = 1 if the individual is was not born in Gazipur or Dhaka districts, as described in section 2.1. Education and experience measured in years. Standard errors clustered at the level of the individual in columns 1 and 2 and the level of the factory in columns 3 and 4. *** p<0.01, ** p<0.05, * p<0.1.*

Table 2.6: Migration and the probability of leaving a factory

Dependent Variable = 1(Leave)	(1)	(2)
Migrant	0.0137***	0.0064
	[0.0030]	[0.0047]
Experience	-0.0008***	-0.0015**
	[0.0003]	[0.0006]
Education	0.0005*	0.0018***
	[0.0002]	[0.0004]
Male	0.0069***	-0.0001
	[0.0019]	[0.0031]
Tenure in Firm	-0.0032***	0.0057***
	[0.0006]	[0.0009]
Factory fixed effects	No	Yes
Observations	48,197	48,197

*Notes: Leave = 1 if the worker left the factory in a particular month and switched to another factory, also in the garment industry. Coefficients are average marginal effects from logit regressions. Migrant = 1 if the individual is was not born in Gazipur or Dhaka districts, as described in section 2.1. Experience, education, and tenure measured in years. Standard errors clustered at the level of the individual. *** p<0.01, ** p<0.05, * p<0.1.*

Table 2.7: Changes in conditions over time

Dependent Variable	Index of working conditions ($\hat{c}$)		Log(wage)	
	(1)	(2)	(3)	(4)
Experience	0.0056	0.0173	0.0247*	0.0152
	[0.032]	[0.021]	[0.013]	[0.013]
Migrant	-0.2495**		0.0297	
	[0.100]		[0.057]	
Migrant X Experience	-0.0222	0.0305*	0.0009	0.0000
	[0.031]	[0.018]	[0.015]	[0.014]
Education	0.0052		0.0135	
	[0.016]		[0.009]	
Education X Experience	0.0069	-0.0055	0.0073***	0.0051**
	[0.007]	[0.005]	[0.003]	[0.002]
Male	0.1172		0.2641***	
	[0.118]		[0.067]	
Male X Experience	-0.065	0.0469	-0.0165	0.0044
	[0.050]	[0.031]	[0.020]	[0.018]
Worker fixed effects	No	Yes	No	Yes
Observations	49,210	49,210	46,847	46,847
R-squared	0.033	0.032	0.294	0.170

*Notes: Wage expressed in 2009 taka. Migrant = 1 if the individual is was not born in Gazipur or Dhaka districts, as described in section 2.1. Education and experience measured in years. Standard errors clustered at the level of the individual. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.*

Chapter 3

Workplace safety and employment decisions: Evidence from an information experiment with Bangladeshi garment workers

3.1 Introduction

Periodically, public debates arise about working conditions in low-skill manufacturing sectors that are common in developing countries. Often, these debates are sparked by industrial disasters, such as the 2013 Rana Plaza factory collapse in Bangladesh, or by exposés of hazardous or unhealthy conditions in factories. Economists typically offer the view, grounded in theoretical and observational empirical literatures (see Blattman and Dercon 2018a for a discussion), that these jobs provide workers with better opportunities than their alternatives. An underlying assumption of this perspective is that while these jobs may have disamenities compared to alternatives, holding all else equal, wages will compensate workers for them (Smith 1979).

A fundamental assumption of the theory of compensating differentials is that workers are informed of their jobs' health and safety hazards. There is very little empirical evidence, however, on the extent to which low-skill workers in developing countries are aware of different jobs' health and safety risks. A body of empirical evidence that documents the presence of information frictions in labor markets in developing countries provides grounds to question this assumption (Jensen 2010; Oster and Steinberg 2013; Beam 2016; Bassi and Nansamba 2017b). Further, we also lack evidence on workers' willingness to trade-off between the wages and the health and safety risks associated with different jobs in developing countries.

In this paper, I test the theory of compensating differentials in a low-skill manufacturing sector in a developing country. I elicit workers' informedness about an important dimension of workplace safety, and I provide experimental evidence that improved information about workplace safety affects workers' beliefs and their employment decisions. My empirical setting is the apparel sector in Bangladesh, which is a low-skill manufacturing sector that is common to many developing countries. I avail of publicly-available building safety audits to create a novel dataset of absolute and relative building safety for a sample of exporting factories in an industrial cluster. I conduct a home-based survey of workers at these factories to elicit their beliefs about their factories' safety. I then randomly provide them with information about their factories' performance on the building safety audits and follow them for seven months to track their employment-related outcomes.

I implemented this pilot study from September 2015-May 2016 with 308 garment workers. The workers lived along pre-determined walking paths through 24 neighborhoods in the Savar-Ashulia area of Bangladesh, which is a peri-urban area near Dhaka that is home to a large cluster of export-oriented garments factories.[1] They were employed at 71 factories that had been audited for building safety by coalition of multinational retail and apparel firms between early-2014 and mid-2015. In the pilot experiment, I randomly provided workers with information about their factories' absolute and relative performance on building safety audits. I emphasized how their factories' performance on the audits compared to the performance of other factories nearby. To mitigate the risk of information spillovers across workers, I randomly assigned the information intervention at the neighborhood level. I analyze the experiment according to a pre-analysis plan (PAP), which is registered on the American Economic Associations Social Science Registry.

I find that factories that sell to similar buyers and that are located in the same geographic area (approximately 130 square kilometers) vary widely in their performance on standardized building safety audits. The factory at the 10th percentile of performance complies with 60% of requirements that are rated "high-priority" for remediation while that at the 90th percentile complies with 80% of these requirements. In contrast with the theory of compensating differentials, there is s slight positive correlation between factories' audit performance and their average log wages. The weighted correlation between audit performance and average log wages is $\rho = 0.16$.[2] The partial weighted correlation after controlling for workers' personal characteristics remains positive ($\rho = 0.14$). These results are concerning because they show that workers in Bangladesh's garments sector are not compensated for bearing building safety risks. This evidence motivates my exploration of the extent to which workers are aware of their factories' building safety.

Following my pre-analysis plan, I divide the sample of factories into three performance bins at the 33rd and 66th percentiles of compliance with "high-priority" building codes. I refer to these groups as "low compliance," "intermediate compliance," and "high compliance."[3] I show that workers at high compliance factories are much more likely to know that their factory had been audited for building safety: 84% of workers at high compliance factories are aware that their factory had been audited, compared to 50% of workers at low compliance factories. I inform workers about the audits and ask them their beliefs about their factories' performance relative to other factories nearby that were audited. Workers at high compliance factories underestimate their factories' performance, while those at low compliance factories dramatically overestimate it. Among workers at low compliance factories, 47% believe that their factory outperforms other factories nearby, 39% believe that their factory performs about the same as other factories, and only 13% believe that their factory performs worse than other factories nearby.

Experimentally intervening to provide workers with information about their factories' performance on the building safety audits causes them to correctly update their beliefs. Further, there is suggestive evidence that it causes workers who learn that their factory is high compliance to remain at their factory longer; treatment workers in this group are 60% less likely to leave their factory in the seven months following the information intervention compared to control workers in this group. This finding suggests that high compliance factories could reduce their turnover if

[1]Enumerators followed a right-hand sampling rule to recruit participants. This approach has been employed in other studies such as Bursztyn et al. 2017.

[2]Factory observations are weighted by the proportion of workers in the sample at the factory.

[3]For conciseness, I focus on comparisons between the high and low compliance groups. In all except one case, the intermediate group falls in between.

they could credibly communicate to their workers that they provide safe conditions. In contrast, providing information about safety does not affect the likelihood that workers at low compliance factories leave their factory. My results provide suggestive evidence, though, that it may reduce their propensity to refer family and friends to their factory. In the future, I aim to implement a larger-scale, improved version of this study in order to validate these results.

This research makes two primary contributions. First, it contributes among the first empirical evidence on the presence of compensating differentials in a labor market in a developing country. To my knowledge, only two other papers, Blattman and Dercon 2018a and Boudreau, Heath, and McCormick 2019, test for the presence of compensating differentials in labor markets in developing countries. Consistent with my finding of a positive correlation between wages and safety, both of these fail to find evidence of compensating differentials. Specifically, Blattman and Dercon 2018a show that industrial jobs in Ethiopia do not provide a wage premium compared to informal alternatives, and alarmingly, that they pose serious health hazards. Boudreau, Heath, and McCormick 2019 document that there is no correlation between worker-reported wages and working conditions in Bangladesh's garments sector. I provide improved evidence by using objective measures of workplace safety instead of relying on workers' reports of conditions. Further, I measure factory safety using severe violations of building standards that pose material risks to workers' safety.

Second, I contribute evidence of information frictions in labor markets in developing countries that can help explain the lack of compensating differentials. In particular, to my knowledge, this is the first study to document the presence of information asymmetry about workplace safety between firms and workers. Further, it provides the first suggestive evidence that alleviating this information asymmetry affects workers' employment outcomes. These findings update and provide an empirical complement to an early theoretical literature on job shopping. This literature models jobs as experience goods, with workers learning about ability-related match qualities, but also about pecuniary and non-pecuniary aspects of the job (W. R. Johnson 1978; W. Kip Viscusi 1979; W. Kip Viscusi 1980).[4] My empirical setting closely mirrors these models and provides the first empirical tests of how improved information about the distribution of safety across establishments affects turnover and other employment outcomes.

The remainder of this paper is organized as follows. Section 3.2 describes the context, including the buyer initiatives that conducted the building safety audits. Section 3.3 presents the study setting and the factory and worker samples. Section 3.4 explains the research design and the experimental intervention. Section 3.5 presents the test of compensating wage differentials, documents information asymmetry about factory safety, and shows how improved information affects workers' beliefs. Section 3.6 presents the pilot's results on how information about safety affects workers' employment decisions. Section 3.7 concludes.

3.2 Context

Bangladesh's garments sector

Bangladesh is one of the most rapidly industrializing countries in the world (Central Intelligence Agency 2016), and the garments sector has been and continues to be the major driver of its industrial transformation. In 2016, apparel exports constituted 81% of Bangladesh's total exports and

[4] W. Kip Viscusi 1979, for example, begins, "Workers seldom have perfect information about the health and safety implications of their jobs...This uncertainty is compounded by uncertainty with regard to the characteristics of the work situation, for example, the concentration of asbestos fibers in the air."

13% of its Gross Domestic Product.[5] The sector is a critically important source of employment in Bangladesh: It directly employs between 4.5-5 million of the 66.6 million person labor force. The majority of garments workers are female (71% of participants in my sample) and most have less than a middle school education (57% of participants in my sample). Many garments workers are internal migrants who move to Dhaka or Chittagong to work in the sector.[6] For example, in a 2009 survey of garment workers conducted in peri-urban region outside of Dhaka by Rachel Heath and Mushfiq Mobarak (discussed in Heath and Mobarak 2015a), 86% of participants were internal migrants.

Garments factories typically hire workers using informal application processes. Anecdotally, the modal approach to hiring is to send managers to the factory's gate or entrance each morning at the beginning of the month. Prospective workers circulate among factories in the area. If a worker fits the factory's desired profile, the manager interviews the worker and conducts a skills test (if required for the position) to determine whether to hire the worker. Factories also frequently hire using referrals. In the same 2009 survey of garment workers mentioned above, 32% of workers received a referral in their current job. As of 2019, there are no centralized sources of information about job openings or about working conditions in Bangladeshi garments factories available to workers.

Working conditions, the Rana Plaza collapse, and initiatives to improve safety

For many years, Bangladesh has been infamous for its weak legal protections for workers, for its lack of enforcement of regulation, and for its low minimum wages.[7] In a 2011 McKinsey survey of western buyers, for example, buyers list lack of social compliance and economic and political instability as two of the top five major risks to sourcing from the country (McKinsey & Company 2011). 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 (see Appendix Figure G1), that killed at least 1,273 workers and injured at least 3,812 workers at exporting factories (Solidarity Center 2016). In the aftermath of these events, world leaders rebuked the Government of Bangladesh (GoB) for "not taking steps to afford internationally recognized worker rights to workers in that country," and some western governments penalized the country by removing trade benefits (Greenhouse 2013a).

Following the collapse of the Rana Plaza Building, two initiatives involving multinational buyers launched with the goal of improving safety in the sector. The first is the Accord on Fire and Building Safety in Bangladesh (hereafter, the Accord), which included 222 European and other retail and apparel firms (e.g., H&M, Inditex, C&A) as well as 10 labor unions. The second is the Alliance for Bangladesh Worker Safety (hereafter, the Alliance), which included 29 primarily North American retail and apparel firms (e.g., Wal-Mart, Gap, Target, Costco). The Alliance formed after several U.S. retailers refused to sign on to the Accord due to the participation of labor unions and the requirement that buyers are subject to legally-binding arbitration (Greenhouse 2013b; Bhattacharjee 2013). Both the Accord and the Alliance signatories committed to five-year

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

[6] Most garments factories in Bangladesh are located in industrial clusters in and around the cities of Dhaka and Chittagong.

[7] Garment sector jobs are not without benefits to Bangladeshi society. Heath and Mobarak 2015a, for example, show that the growth in these jobs contributed to decreasing fertility, increasing age at marriage, and increasing educational attainment among Bangladeshi girls in recent decades.

safety improvement programs. The Accord and the Alliance were both active from 2013-2018; together, they covered approximately 1800-2000 garment factories in Bangladesh.[8]

Accord and Alliance building safety audit and remediation programs

Both initiatives' initial priority was to inspect all buildings in their signatories' supplier bases for compliance with jointly-developed building safety standards. Their standards were harmonized with those developed by the Bangladesh University of Engineering and Technology (BUET) for a government-led national initiative to improve factory safety. The Accord/Alliance building safety standards were founded on the requirements of the 2006 Bangladesh National Building Code (BNBC), with stronger requirements where deemed necessary (Alliance for Bangladesh Worker Safety n.d.).

The standards include requirements for electrical, fire, and structural building safety. Most of the requirements are very technical. For example, the standards require that the electrical system be correctly installed and maintained, paths of egress be of sufficient width for the occupant load, fire doors be installed and of sufficient rating, and so on. Both initiatives publicly post completed building safety audit reports on their websites, which is how I accessed them.

While both initiatives employed the same building safety standards, they formatted their audit reports differently. The Accord's audits contain a list of problems identified at the factory site. The Alliance's audits report report findings as compliance with a standard set of questions (see Figure G2). The format of the Accord audits makes it difficult to compare performance on the audits across factories. In contrast, the format of the Alliance audits makes it possible to compare performance because of the standard question list. For this reason, I only include factories that were audited by the Alliance.

Both initiatives completed a small number of building safety audits in 2013, but they began in earnest in early 2014. Based on the building inspections' findings, both initiatives required factories to develop Corrective Action Plans (CAPs) to remediate safety violations.[9] Both initiatives monitored factories on completion of their CAPs, and they both suspended factories from their supplier bases in case of failure to make sufficient remediation progress.[10]

The Alliance divided its standards, and their associated audit questions, into three categories: "Low," "Medium," and "High" priority standards. While I am unable to find a formal statement of the definition of these priority categories, Alliance staff have indicated in informal interviews that they reflect the risk that the issue poses to human life, with high priority issues posing the greatest risk to human life.

I measure factory safety using compliance with high priority standards. I argue that this measure of factory safety is the most informative for two reasons. First, these safety issues are the most

[8]As of April 2019, the Accord is fighting in Bangladesh's High Court to obtain approval to continue to operate. The Alliance has ceased operations, and many of its members have signed on to support a new initiative called Nirapon that aims to continue certain components of the Alliance's safety improvement programs.

[9]When inspections revealed immediate or imminent danger, both initiatives referred factories to the Government Review Panel (GRP). The GRP can recommend immediate closure to the Inspector General. A total of 35 factories in Bangladesh were referred to the GRP.

[10]Except in rare cases of immediate or imminent danger, the Accord and the Alliance did not begin suspending factories until November 2015, after the experimental intervention.

fundamental for the protection of human life.[11] Second, there was a gap between when the audits were conducted and when I provided the audit information to workers. I expected that the high priority issues would be more difficult and costly to resolve, and as such, that factories would be less likely to remediate them before my information intervention. I have since collected CAP progress data for 62 out of 71 factories. As I expected, remediation prior to the study launch on August 28, 2015, was highest for low priority issues (41% resolved), followed by medium priority issues (34% resolved), and finally, high priority issues (28% resolved).

3.3 Study setting and samples

The setting for this study is Savar-Ashulia, a peri-urban area outside of Dhaka, Bangladesh. Savar-Ashulia is home to a large cluster of export-oriented garments factories that are primarily concentrated around a polygon of roads displayed in Appendix Figure G3. The map shows clusters of garment factories active in the area in 2019.[12] Much of the green area surrounding the main roads on the map is marshland, where there are no factories or neighborhoods. Factories are primarily located along the main roads, in particular the road between Ashulia and Baipayl. At the northernmost tip of the polygon, near the Baipayl area, is the Dhaka Export Processing Zone (EPZ), which houses numerous garments factories. This area is the geographic setting for the study.

Factory Sample

To identify a sample of factories in the area, I collected publicly-posted lists of suppliers, including factory locations, from the Alliance's website. From these lists, I identified 71 factories located in the Savar-Ashulia area for which building safety audits were posted on the Alliance's website by July 2015. Appendix Figure G4 is a map of these factories. I also collected supplier lists for the Accord. Using the Alliance and the Accord supplier lists, I determined all other Alliance and Accord factories in the Savar-Ashulia area. Appendix Figure G5 includes these factories. As can be seen in the map, the spatial distribution of factories in my study closely matches that of the broader population of export-oriented factories in the area.

Figure 3.1 shows the distribution of sample factories' compliance with high priority questions from the Alliance's building safety audits. Even within a limited geographic area and among factories supplying to a similar set of buyers, there is a lot of variation in factories' compliance with building safety standards. The worst and the best performing factories comply with 54% and 89% of high priority standards, respectively. The median (and mean) factory complies with 71% of high priority questions.

One may question whether the factories in my sample are representative of the safety of other export-oriented factories in the area. To partially allay this concern, I analyze CAP data for all factories that appear in the Alliance's publicly-disclosed supplier lists between 2013-2015 and that I can identify as being located in the Savar-Ashulia area. Using this approach, I obtain a list of 49 non-study factories and 62 study factories. In Appendix Figure G6, I plot the distribution of fac-

[11] Performance across categories is very highly correlated: Correlation between performance on high and medium priority questions is 0.73, and between high and low priority questions is 0.58.

[12] A map of garment factories in the area did not exist in 2015.

tories' compliance with high priority questions using this dataset.[13] As can be seen in the figure, the performance distributions are similar; I perform a Kolmogorov-Smirnov test and fail to reject the null hypothesis that the samples are drawn from the same distribution ($p = 0.635$).

Worker Sample

The sampling frame for workers included garments workers who worked at one of the 71 sample factories, who lived in the Savar-Ashulia area, and who were over the age of 18. Habitable land in Savar-Ashulia is densely populated, with a relatively large proportion of the working population employed in the garments sector. In order to recruit participants outside of their factory, I identified 24 walking paths through the area. To do so, I manually gridded out the area and located the first path in the southeast corner. I then placed walking path starting points following two rules: First, walking paths had to be at least one kilometer from each other, and second, they had to be located in habitable area. Following these rules, I obtained fairly complete coverage of the area (see Appendix Figure G7 for walking path starting points).

Participants were recruited on Friday (weekend) mornings when garments workers were most likely to be at home. To recruit participants, enumerators walked along the pre-assigned walking paths. Enumerators were given a target of recruiting 13 participants along their path. To identify prospective participants, they followed a right-hand sampling rule and recruitment protocol (see Appendix Figure G8). The target sample size for the study was 312 workers. Using this approach, 308 participants were recruited.[14] Participant recruitment occurred in two waves, each including twelve starting points. The first wave ran from Friday, August 28, 2015 through September 18, 2015. The second wave started after the Muslim holiday of Eid al-Adha, which fell at the end of September. This wave ran from October 16, 2015 through November 13, 2015.

3.4 Experiment design and empirical strategy

Treatment assignment occurred at the level of the walking path. Twelve walking paths were randomly assigned to the treatment condition, and twleve to the control condition (see Appendix Figure G7).[15] Table G1 displays baseline balance tests for the treatment and control groups. Participants are balanced across the treatment and control conditions, both in terms of the characteristics of the factories where they work and in terms of their personal characteristics.

Participation in the study entailed a baseline survey followed by three rounds of follow-up

[13]For the 62 study factories that I identify in this dataset, their measured compliance is slightly worse using this dataset. The discrepancy may be due to additional safety violations that the Alliance identified during visits to verify remediation actions.

[14]At one starting point, only 11 garment workers could be recruited while maintaining the rule, and at two others, only 12 participants were recruited.

[15]Alternatively, I could have randomized assignment at the factory level. I made this decision because I determined that information spillovers were more likely to occur among family and friends who live nearby to each other than among workers at the same factory. In field visits to the area, garment workers indicated that they primarily socialize with their family and friends in their neighborhood and that workers in the same neighborhood often walk to work together. Further, given the small share of workers treated relative to their factories' size, I deemed contamination through interaction in the workplace to be less of a concern.

phone calls over seven months. The experimental intervention, which I describe in the next subsection, occurred directly after the implementation of the baseline survey. After the experimental intervention, participants received three follow-up phone calls. The first round of phone calls occurred two months after the baseline survey; this round served as a reminder of the information provided during the baseline survey (see Section 3.4). The second round of phone calls occurred 4.5 months after baseline. This phone call included a follow-up survey about workers' employment decisions and other questions from the baseline survey. The third round of phone calls occurred 7 months after baseline. This phone call included a final follow-up survey that was very similar to the survey conducted during the second follow-up call. I conducted three rounds of follow-up phone calls primarily to maintain more constant contact with participants. The phone calls also provided an opportunity to check whether workers had gotten new phone numbers between the baseline survey and the phone call. Appendix A Figure G9 displays the timeline for the study.

Participants were paid 300 Bangladeshi Taka (BDT) for their participation in the baseline survey (plus any additional earnings from incentivized questions). I also incentivized participants to participate in the follow-up phone calls by transferring them phone credit worth 60 BDT for each follow-up phone call that they completed.

Experimental information intervention

The baseline survey instrument included questions on the participant's personal and household characteristics and on employment. It also included one game to measure risk aversion and one to measure ability. The final module asked questions about workers' safety experience and perception of safety in the workplace. In this module, workers were asked whether they had heard of the Accord and/or the Alliance. They were given a basic explanation of the initiatives and told that the initiatives were conducting building safety audits. They were asked whether they knew that the Alliance had audited their factory for building safety. They were asked how they thought that their factory performed on the audits, both in absolute terms and relative to other factories nearby; these two questions each had three response options. Appendix **??** Section I displays a condensed version of this survey module.

At this point, the safety audit information was provided to treatment workers. Enumerators read aloud from a post-survey script and provided the participant with an information flyer, both of which are displayed in Appendix **??** Sections II and III, respectively. To summarize the information intervention, the enumerator informed the worker about the building safety audit again, told the worker how many of the high priority safety questions their factory violated, and how this compared to the performance of the other 70 factories nearby. The enumerator gave the participant a flyer that also contained this information as well as lists of the highest and lowest performing factories. The enumerator told the participant that we obtained the information from the Alliance's website and provided them with the URL. Finally, the enumerator told the worker that we set up a hotline with more information about safety that they could call.

For control workers, the enumerator read aloud a short script informing them that we had set up a hotline that they could call with more information about safety. The enumerator provided the worker with a flyer containing only this information.

The first round of follow-up phone calls was also part of the information intervention. It occurred two months after baseline. In this round, treatment workers were reminded of the information provided at baseline, and control workers were reminded of the helpline number that they could call for more information.

Outcomes

I pre-specified five primary outcome variables in my PAP. They are:

1. Factory building safety perception (index);
2. Employment at baseline factory;
3. Referrals to baseline factory;
4. Reported plans to leave one's job;
5. Calls to the safety information hotline.

The first primary outcome is an index containing one absolute and one relative measure of participants' perceptions of their factories' performance on the building safety audit. This outcome measures the extent to which treatment group participants update their beliefs about their factories' safety after receiving the information intervention; it is only available for wave 2 participants. Primary outcomes 2-4 measure how the information intervention affects participants' employment decisions. The treatment effect on the fourth primary outcome, reported plans to leave one's job, is ambiguous if the treatment affects treatment group participants' probability of leaving their original factory. Participants who leave their original factory to begin work at a new factory or other job may be less likely to report plans to leave their employer because they have recently changed jobs. Therefore, I also report the results separately for those who remain at their original factories and those who report moving to new jobs. These results are speculative, as they are no longer randomized. Finally, primary outcome 5 measures whether providing individuals with basic information about factory safety affects their demand for safety-related information.

At the time that I made the decision to set up the safety information hotline, I was unaware that the Alliance was scaling up its worker safety helpline, *Amader Kotha*. The implementation of this experiment, in the fall of 2015, coincided with the scale-up of the Alliance's helpline. My safety information hotline was not toll-free, but the Alliance's helpline was toll-free. Further, workers could both report safety concerns and receive information about safety from the Alliance's helpline. Ultimately, my helpline received only 9 calls during the study period, five of which were to the treatment line and 4 of which were to the control line. None of the phone numbers could be directly linked to study participants. Evidently, there is not a significant difference between the number of calls to the helpline. It is difficult to interpret this outcome, though, given the concurrent introduction of the Alliance's toll-free helpline. I cannot say with certainty that my safety information line did not receive any calls due to the Alliance's roll-out of the worker helpline, but I believe that it played a role.

Econometric analysis

I test how learning about how one's factory performs on safety audits affects one's employment decisions. I hypothesize that this effect depends on a worker's prior beliefs and on the direction of the information provided. In particular, I provide workers with a signal of their factories' safety that, if perceived to be credible, will lead them to update their beliefs in the direction of the signal.[16] Table 3.1 displays the changes that I expect in workers' beliefs. One caveat to Table 3.1 is that it assumes that workers whose prior belief level matches their factories' performance are unaffected. It is possible, however, that these workers may be affected if the treatment increases their confidence in their beliefs (increases the precision of their posterior beliefs). For this and other reasons, I take a more parsimonious approach and pool individuals by their factories' performance.[17]

Table 3.1: For groups with different priors, the effects of safety audit information on beliefs

	Low compliance	**Intermediate compliance**	**High compliance**
Low prior beliefs	No Δ	Moderate + Δ	Strong + Δ
Intermediate prior beliefs	Moderate - Δ	No Δ	Moderate + Δ
High prior beliefs	Strong - Δ	Moderate - Δ	No Δ

This approach to pooling factories results in three compliance groups: The bottom, middle, and upper terciles of building safety compliance of the 71 factories in the sample (*High compliance*, *Intermediate compliance*, and *Low compliance*, respectively).[18]

Appendix Table G2 shows within-subgroup baseline balance tests for factories' characteristics. Overall, treatment and control group workers in each factory compliance group are well-balanced. There is only one statistically significant difference among treatment and control groups for workers at low compliance factories, which is the number of high-priority issues identified at the factory. Treatment workers in this subgroup are at factories with 8% worse performance on the audits (RI $p = 0.028$). Appendix Table G3 shows within-subgroup baseline balance tests for individuals' characteristics. There are some baseline imbalances within subgroups. For workers at high compliance factories, treatment group workers are slightly older and more educated, and they have been working at their factories for slightly longer than control group workers. Workers at intermediate compliance factories are balanced on all variables except tenure, on which treatment workers have an average of one less year of tenure (RI $p = 0.095$). Finally, for workers at low compliance factories, treatment workers are slightly younger and have slightly more education. I address baseline imbalances by reporting results with and without controlling for individuals' covariates. These baseline imbalances are not surprising because the treatment assignment is not stratified by factory tercile.[19]

[16]See Shrestha 2016 for a simple model of learning from information.

[17]In my PAP, I included an econometric approach that uses participants' prior beliefs about their factories' performance into the analysis. Due to small cell sizes and baseline imbalances, I do not show results for this approach. Appendix Tables I1 through I6 show baseline balance tests for these subgroups.

[18]In my pre-analysis plan, I specified two approaches to grouping the factories into compliance groups. The second proposed approach was to split the sample at the median score, which increases the subgroup sizes. The first approach is more appropriate given the format of and participants' responses to the baseline survey, hence I adopt this approach for the analysis.

[19]This study is a pilot. In a larger study, assignment to treatment will be stratified by factories' compliance.

Regression model:

I estimate the interventions average treatment effects using a simple regression model:

$$Y_{inf} = \beta_0 + \beta_1 T_n + \beta_2 Intermediate_{if} + \beta_3 Low_{if} + \beta_4(T_n \text{ x } Intermediate_{if}) + \\ \beta_5(T_n \text{ x } Low_{if}) + \mathbf{X}_i'\gamma + \phi_w + \eta_f + \epsilon_{inf} \quad (3.1)$$

where Y_{inf} is a given outcome (e.g., participant leaves job) for individual i in neighborhood n working at factory f; T_n is the neighborhood treatment indicator; $Intermediate_{if}$ and Low_{if} are indicators for individual i's factory f belonging to intermediate or low tercile groups, respectively; $\mathbf{X}_i'$ is a vector of individual level controls from the baseline survey; ϕ_w is a wave fixed effect included because the study was implemented in two waves; η_f is an EPZ fixed effect to control for differences between factories located inside and outside of the Dhaka EPZ;[20] and ϵ_{inf} is the idiosyncratic error term. The main parameters of interest are β_1, $(\beta_1 + \beta_4)$, and $(\beta_1 + \beta_5)$. I show results estimated with and without the vector of individual-level controls. These controls include sex, highest educational attainment, age, total monthly wages, and tenure at the time of the baseline survey. When available, I also include a control for the baseline value of the dependent variable.

Statistical inference:

I conduct statistical inference using randomization inference. Randomization inference is increasingly the recommended way to analyze data from RCTs, in particular for small samples (Athey and Imbens 2016; Young 2015; Heß 2017). This approach is a departure from my PAP, in which I specified that I would report t-statistics calculated using the wild cluster bootstrap-t method. At the time that I prepared the PAP in early 2016, randomization inference was less commonly used for analysis of RCTs. In line with the evolution of best practice in the field, though, I have updated my approach.

Attrition:

I present the pilot's main results for the group of workers reached at the final round of phone calls (7 months post-baseline). 215 out of 308 workers were reached in this round. Appendix Table G4 tests for differences in attrition between treatment and control groups. Column 1 shows that attrition is slightly higher in the treatment group, but this difference is not statistically significant. Column 2 shows the differences in attrition within treatment and control groups across terciles. The only group for which there is a marginally statistically significant difference is tercile 3 (low compliance factories) workers. Attrition is 17 percentage points (81%) higher in the treatment compared to the control group. Based on data from the first and second rounds of phone calls, attrition is higher among workers who leave their factories. This may be because these workers are more likely to move or to return to their village. For this reason, I anticipate that this differential attrition biases me toward the null hypothesis for workers at low compliance factories. Appendix Tables G5 and G6 provide support for this possibility. They show the main results for participants

[20] Factories located inside the EPZ are subject to different regulations. They differ, and may attract workers who differ, in systematic ways from factories located outside the EPZ

reached during any round of follow-up phone calls. I will discuss these tables in greater detail in a future version of this paper.

3.5 Compensating differentials, workers' beliefs, and the effects of information about safety

Test of compensating wage differentials

As discussed in Section **??**, factories range widely in their compliance with building safety standards. The worst performing factory in the sample complied with only 54% of high priority standards. In contrast, the best performing factory complied with 89% of high priority standards (Figure 3.1). If the theory of compensating wage differentials holds, more compliant factories should pay lower wages than less compliant factories because they do not need to compensate workers for working in a riskier environment. Figure 3.2 plots factories' compliance and the mean log of total wages for workers in the sample at the factory. Evidently, there is actually a positive correlation between compliance and mean log wages ($\rho = 0.159$). Wages do not appear to compensate workers for building safety risks. I weight factory observations by the number of workers in the sample, which ranges from 1 to 22, and the correlation remains stable ($\rho = 0.164$)

While the sample factories are located in the same geographic area and supply to a similar set of buyers, one may still be concerned that they differ on other dimensions, such as product quality. If so, there may be heterogeneity in the types of workers that they employ that explains the lack of a compensating differential. To test this possibility, I calculate the partial correlation between compliance and wages after controlling for workers' baseline covariates (sex, age, educational attainment, tenure, and tenure squared). The partial correlation between compliance and wages remains positive ($\rho = 0.137$).

Alternatively, perhaps workers are heterogeneous in their degree of risk aversion, and less risk averse workers sort into less safe factories. The data do not support this possibility. First, using both revealed preference and self-reported measures of risk aversion, workers in less safe factories are more, not less, risk averse compared to workers at more safe factories. Second, when asked to rank possible improvements in working conditions, workers at less safe factories do not rank safety improvements differently than workers at more safe factories. Evidently, workers at less safe factories are no less risk averse than workers at safer factories.

It is also possible that less safe factories provide non-wage amenities that compensate workers for safety risks. To test this possibility, I check whether factories systematically vary in other amenities that they provide to workers. There is a positive correlation between compliance and the number of non-monetary benefits provided by the factory ($\rho = 0.207$). The positive correlation is largely driven by two factories that offer very few non-monetary benefits; dropping these factories, $\rho = 0.071$. I also check whether working hours vary with factory compliance. The weighted correlation between compliance and working hours is small and negative, $\rho = -0.076$; it is further attenuated after dropping one outlier ($\rho = -0.052$). Together, this evidence shows that factories do not systematically vary in their monetary and non-monetary compensation of workers in ways that explain the lack of a compensating wage differential.

Workers' beliefs about factory safety

Wages of the Bangladeshi garment factories in my sample do not compensate workers for building safety risks. The lack of a compensating differential does not appear to be explained by workers' characteristics or by differences in employment contracts employed by factories. Instead, I hypothesize that one reason why wages do not compensate workers for these risks is that they are difficult to observe, and workers lack information about them. If so, owners of less safe factories do not need to compensate workers for bearing these risks. In this section, I explore whether asymmetric information about building safety between factory owners and workers helps to explain the lack of a compensating wage differential.

Prior to measuring workers' perception of their factories' building safety, enumerators asked workers about their awareness of the Accord and the Alliance initiatives.[21] 69% of workers at high compliance compliance factories reported being aware of at least one initiative. In contrast, only 41% of workers at low compliance factories reported being aware (Appendix Figure G12).

After briefly explaining the initiatives, enumerators told workers that the Alliance had conducted building safety audits of their factories. They next asked whether workers were aware that their factory had recently been audited for building safety. 84% of workers at high compliance factories were aware that their factory had been audited, compared to only 50% of workers at low compliance factories. Awareness of the safety initiatives is 40% lower among workers at less safe factories. The large gaps in awareness of the buyer initiatives and of the building safety audits among workers at high and low compliance factories is alarming. Workers at the least safe factories are also the least likely to be aware of these buyer/policy initiatives.

Next, workers were asked their beliefs about how their factories performed on the audits. Figure 3.1 displays workers' beliefs about their factories' performance relative to other factories nearby. Among workers at high compliance factories, 51% believed that their factory was high compliance, and about 49% believed that their factories' performance was intermediate. Among workers at low compliance factories, a similar proportion, 47%, believed that their factories were high compliance. Only 13% of workers at low compliance factories correctly believed that their factory was low compliance.

It is striking that across the distribution of compliance, workers hold fairly similar beliefs about their factories' compliance compared to other factories nearby. 87% of workers at low compliance factories overestimated their factories' compliance. Most of these workers severely overestimated their factories' compliance (i.e., they believed that their factories were high compliance). At the same time, 50% of workers at high compliance factories underestimated their factories' compliance.

Finally, workers were asked whether their factory management had informed workers of actions they had taken to improve safety. 99% of workers at high compliance factories compared to 72% of workers at low compliance indicated that management had reported taking actions to improve safety. Workers at high compliance factories were also significantly more likely to report having received training on safety in the past year. It is concerning that in the least safe factories, managers are doing the least to improve safety.

One may wonder why high compliance factories did not more effectively communicate to this fact to their workers. One possibility is that they lack means of credibly communicating to workers that they offer safe conditions. Among workers who reported being aware that their factory had been audited for safety (84% and 50% of workers at high and low compliance factories,

[21]Both initiatives had safety training and other programs for workers that factories were required to participate in.

respectively), 80% of workers at both high and low compliance factories reported that their factories shared the results of the building safety audits with them. These workers' beliefs about their factories' performance on the audits were no different than workers who report being unaware of the audits or not receiving information from management about their factories' performance. The provision of information by management did not affect workers' beliefs about their factories' safety. This suggests that factories may lack the ability to credibly communicate about building safety to workers.

To summarize the findings of this section, across the distribution of factories' compliance with building safety standards, workers misperceive their factories' safety. Workers at low compliance factories dramatically overestimate their factories' compliance, while 50% of workers at high compliance factories underestimate it. Alarmingly, workers at the less safe factories are also significantly less likely to report that their management is taking action to improve safety and that they have received training on safety.

Impacts of safety information on workers' beliefs

If workers perceive the information that I provide to be credible, then I expect them to update their beliefs according to Table 3.1. Tables 3.2 and 3.3 report the results of this analysis, which is only available for wave 2 participants. Panel A of both tables shows the effect of providing information on the standardized of factory safety performance. In this specification, a more positive value of the outcome variable is associated with better compliance. In Table 3.2, the direction of the coefficients are as expected, and the information causes participants at low compliance factories to report much lower beliefs about their factories' compliance. The estimated treatment effect on this group is nearly -0.6 standard deviations (RI $p = 0.072$). In Table 3.3, the slight positive effect on workers in high compliance factories disappears, but the negative effect on workers in low compliance factories is unchanged. Due to power limitations, I am unable to reject that the estimated treatment effects are the same for all groups of workers.

Table 3.4 presents an alternative approach to this analysis. The outcome variable in the analysis is the difference, at Round 3, between the factories' actual tercile of performance and workers' perception of its performance. In both cases, one represents the best performance, and three the worst. In Column (1), workers at high compliance factories still underestimate their factories' performance, but treatment workers do so less than control group workers (not statistically significant). In Column (3), workers at low compliance factories still overestimate their factories' performance, but treatment group workers do so significantly less than control group workers (RI $p = 0.079$). In Panel A, I am able to reject that the treatment effects on workers at high and low compliance factories are the same (RI $p = 0.067$).

3.6 Improved information about safety and employment decisions

Providing workers with improved information about their factories' performance on building safety audits improves the accuracy of their beliefs. How does more accurate information about safety affect their employment decisions? Tables 3.2 and 3.3, Panels B and C, display the results on my preferred outcomes for this question, tenure at the participants' baseline factory and referrals to that factory. In Panel B, column (1), workers who learn that their factory is high compliance are nearly 60% less likely to leave their factory in the seven months after baseline (RI $p = 0.053$). The estimated treatment effect is unchanged after controlling for covariates. This result is striking because it suggests that providing workers with credible, improved information can reduce turnover at safe factories. In contrast, there is no effect on workers' probability of leaving their factory in other groups. In particular, more accurate information about safety does not affect the likelihood that workers at low compliance factories leave their factories. This result raises the question of whether improved information does not lower these workers' expected earnings enough to offset the costs of searching for a new job or whether these workers are less mobile or have worse outside options.

Turning to referrals, Panel C of Tables 3.2 and 3.3 present the results. In Column (1), treatment workers at high compliance factories are 23% more likely to refer family and friends to their baseline factory (not statistically significant). In contrast, treatment workers at low compliance factories are 16% less likely to refer family and friends to their baseline factory (not statistically significant). While these differences are not statistically significant, they are fairly large in magnitude. The directions of these effects are also consistent with improved information having material impacts on workers' employment decisions.[22] In particular, it is noteworthy that workers at low compliance factories make fewer referrals to their baseline factories. This result suggests that these workers may face higher mobility costs or have worse outside options. This preliminary finding merits further research.

Finally, Panel D of Tables 3.2 and 3.3 present the results for workers' plans to leave their factory. As discussed in Section 3.4, this outcome is difficult to interpret in the sense that in the full sample, the treatment effect is ambiguous if the intervention induces workers to leave their factories and move to factories that provide them with greater utility. The first row of Panel D shows the estimated treatment effects on the full sample. The coefficients are negative for all groups, but the only group for which the estimated treatment effect is statistically significant is workers at intermediate compliance factories. The estimated treatment effect on this group is large: Improved information reduces their reported plans to leave their factories by 78% (RI $p = 0.035$). Unsurprisingly, the estimated treatment effect on workers at high compliance factories is also negative, although it is not statistically significant. In contrast, surprisingly, the estimated treatment effect on workers at low compliance factories is also negative.

Subsetting the data to workers who remain at their baseline factory does not dramatically change the estimated treatment effects except for workers at high compliance factories. The estimated treatment effect on this group is a 72% reduction in planning to leave one's baseline factory (RI $p = 0.027$). This effect is consistent with the reduced turnover rate for treatment workers at high compliance factories. Note that this analysis is not experimental and should be interpreted with caution.

There are multiple possible ways to interpret these results. First, for workers at low compliance factories, if these workers have worse outside options, it is possible that they did search for a job and were unable to find one that provides greater utility. Alternatively, perhaps having improved information about the distribution of building safety across factories and the relative performance of one's factory reduces the expected benefits of changing factories, in particular for workers at intermediate compliance factories. This could be the case if these workers had more diffuse benefits about safety, and the intervention improves their precision relative to the control

[22] As shown in Tables G5 and G6, when the analysis is conducted using observations from Round 2 and Round 3 phone calls, the negative effect on referrals to low compliance factories becomes marginally statistically significant (RI $p = 0.084$).

group. In the raw data, this does not appear to be the case. Overall, I do not have great data to dig into this result, but I aim to generate improved evidence in the scaled-up version of this project.

3.7 Conclusion

In this chapter, I test for the presence of compensating wage differentials for factory building safety in Bangladesh's apparel sector. I find no evidence in support of a compensating wage differential for building safety. I show that this descriptive fact is not explained by heterogeneity in workers' or factories' characteristics. Instead, I show that workers have incomplete information about factories' compliance with building safety standards, which I argue is difficult for workers to observe. Workers at high compliance factories underestimate their factories' performance on the audits, while workers at low compliance factories dramatically overestimate their factories' performance.

I experimentally intervene to provide workers with information about their factories' performance on the audits relative to other factories nearby. I find that providing workers with this information leads them to correctly update their beliefs about their factories' safety. It reduces turnover among workers at high compliance factories. Among workers at low compliance factories, turnover is unaffected, but workers are less likely to make referrals to their factory.

My findings provide a basis for scaling up the pilot experiment. First, they indicate the need for additional research on compensating wage differentials in labor markets in developing countries. In particular, the theory of compensating differentials implicitly assumes that workers are informed jobs' safety and health risks. My findings suggest, though, that awareness among workers is lacking, at least for one difficult-to-observe dimension of safety. I will further investigate these empirical facts in a scaled-up version of this research. My findings also speak, in a limited manner, to early theoretical models of job shopping around workplace safety risks. There is space to more directly contribute to this literature by collecting more data about how workers trade-off among wages and safety risks. Related to this point, it is also possible to estimate the value of a statistical life (VSL) for a worker population in a low-skill manufacturing sector that is very common in developing countries.

My findings also have policy implications. In particular, there is a role for entrepreneurs, policy makers, and/or non-governmental organizations to provide workers with credible information about factory safety. This approach will benefit high compliance factories, which appear to be unable to credibly signal their safety to their workers. In contrast, my finding that workers in low compliance factories do not leave, but are less likely to make referrals to their factory, suggests that providing information about factory safety may not be enough. This finding also necessitates further investigation.

3.8 Figures and Tables

Figure 3.1: Sample factories' compliance with high priority questions

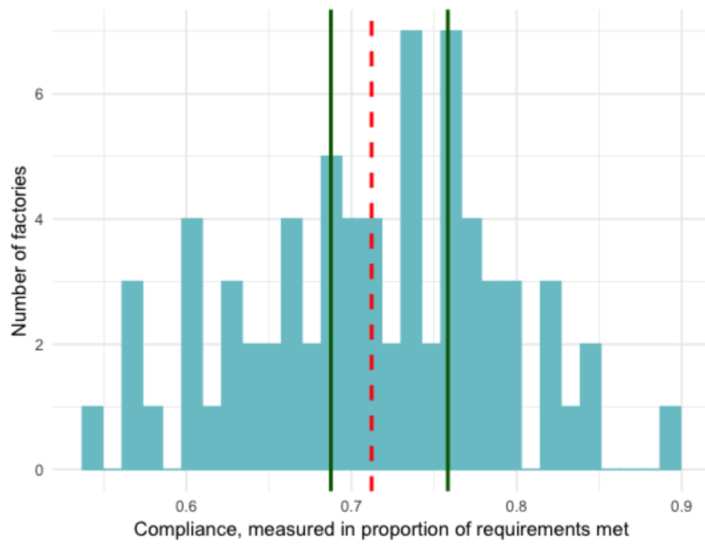


Figure 3.2: Correlation between establishments' compliance with building safety codes and mean log wages

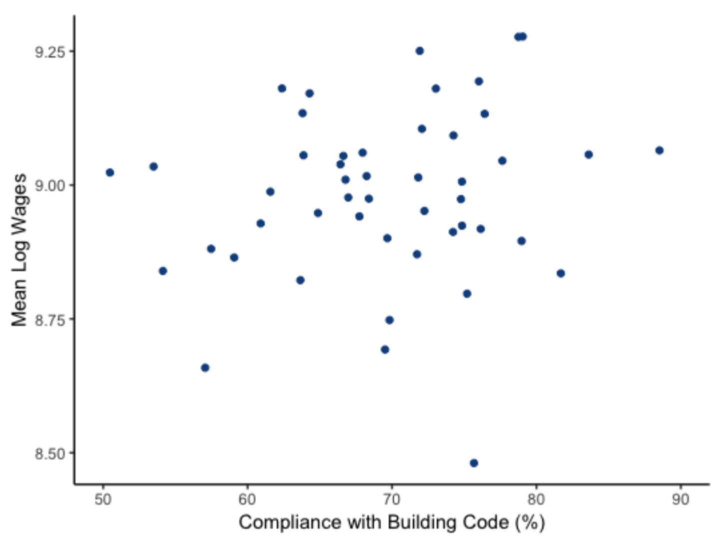


Figure 3.3: Participant awareness of building safety audit by tercile of safety audit performance

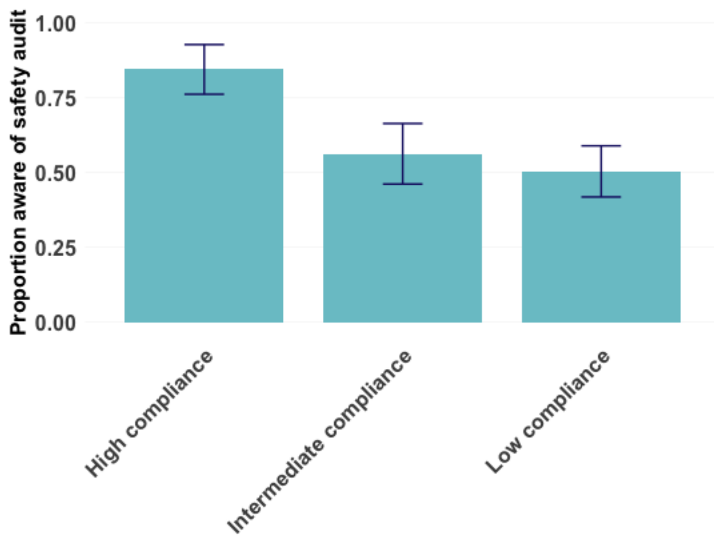


Figure 3.4: Participant perception of building safety audit performance by tercile of safety audit performance

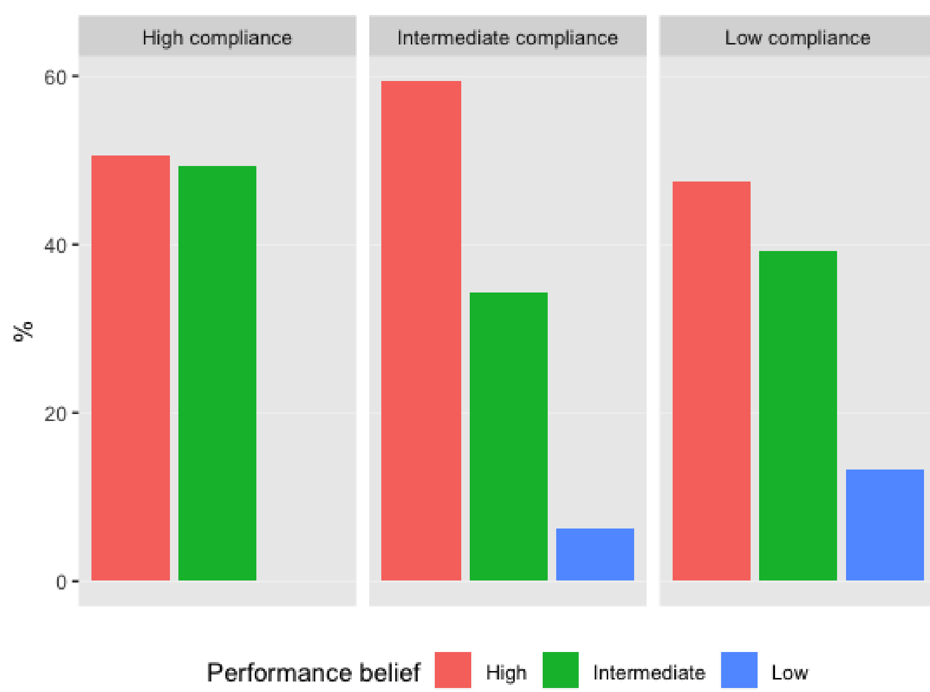


Table 3.2: Main results, no controls; Participant reached at endline

Factory safety:	High	Intermediate	Low	P-val, diff (1), (2)	P-val, diff (1), (3)	P-val, diff (2), (3)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Factory building safety perception (index) (Wave 2 participants only)						
	0.045	-0.304	-0.572			
	[0.894]	[0.772]	[0.072]*	[0.637]	[0.160]	[0.686]
Control group mean	-0.214	0.111	0.116			
Observations (in tercile)	45	23	42			
Panel B: Leaves factory						
	-0.245	0.094	-0.048			
	[0.053]*	[0.563]	[0.655]	[0.083]*	[0.176]	[0.424]
Control group mean	0.410	0.283	0.289			
Observations (in tercile)	52	70	93			
Panel C: Refers family/friends to baseline factory						
	0.103	0.009	-0.111			
	[0.460]	[0.950]	[0.246]	[0.631]	[0.174]	[0.467]
Control group mean	0.455	0.500	0.689			
Observations (in tercile)	52	70	93			
Panel D: Plans to leave baseline factory						
Full sample						
	-0.173	-0.379	-0.150			
	[0.162]	[0.035]**	[0.229]	[0.388]	[0.916]	[0.324]
Control group mean	0.350	0.488	0.310			
Observations (in tercile)	49	66	86			
Among those who do not leave baseline factory						
	-0.240	-0.372	-0.170			
	[0.072]*	[0.090]*	[0.128]	[0.644]	[0.681]	[0.324]
Control group mean	0.333	0.455	0.344			
Observations (in tercile)	36	47	69			

Notes: Two-sided RI p-values based on 5000 draws in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.3: Main results, with controls; Participant reached at endline

Factory safety:	High	Intermediate	Low	P-val, diff (1), (2)	P-val, diff (1), (3)	P-val, diff (2), (3)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Factory building safety perception (index) (Wave 2 participants only)						
	-0.005	-0.322	-0.582			
	[0.986]	[0.763]	[0.067]*	[0.686]	[0.161]	[0.692]
Control group mean	-0.214	0.111	0.116			
Observations (in tercile)	45	23	42			
Panel B: Leaves factory						
	-0.245	0.070	-0.043			
	[0.060]*	[0.699]	[0.693]	[0.140]	[0.170]	[0.565]
Control group mean	0.410	0.283	0.289			
Observations (in tercile)	52	70	93			
Panel C: Refers family/friends to baseline factory						
	0.063	0.068	-0.121			
	[0.630]	[0.722]	[0.183]	[0.983]	[0.203]	[0.356]
Control group mean	0.455	0.500	0.689			
Observations (in tercile)	52	70	93			
Panel D: Plans to leave baseline factory						
Full sample						
	-0.195	-0.408	-0.175			
	[0.135]	[0.026]**	[0.155]	[0.380]	[0.924]	[0.334]
Control group mean	0.350	0.488	0.310			
Observations (in tercile)	49	66	86			
Among those who do not leave baseline factory						
	-0.270	-0.399	-0.190			
	[0.058]*	[0.080]*	[0.104]	[0.667]	[0.657]	[0.334]
Control group mean	0.333	0.455	0.344			
Observations (in tercile)	36	47	69			

Two-sided RI p-values based on 5000 draws in parentheses. All regressions include controls for participants' baseline values of gender, age, years of education, tenure, and the natural log of wages; Panels A, C, and D also include baseline controls for the value of the dependent variable. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.4: Beliefs updating about factory safety (Wave 2 participants only)

Factory safety:	High	Intermediate	Low	P-val, diff (1), (2)	P-val, diff (1), (3)	P-val, diff (2), (3)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: No control variables						
	0.197	-0.081	-0.412			
	[0.450]	[0.903]	[0.079]*	[0.547]	[0.067]*	[0.431]
Control group mean	-0.842	0.571	1.533			
Observations (in tercile)	45	23	42			
Panel B: With control variables						
	0.153	-0.100	-0.443			
	[0.627]	[0.828]	[0.112]	[0.613]	[0.106]	[0.446]
Control group mean	-0.842	0.571	1.533			
Observations (in tercile)	45	23	42			

Two-sided RI p-values based on 5000 draws in parentheses. In Panel B, regressions include controls for participants' baseline values of gender, age, years of education, tenure, and the natural log of wages. *** p<0.01, ** p<0.05, * p<0.1.

Bibliography

Abowd, John M, Francis Kramarz, and David N Margolis (1999). "High wage workers and high wage firms". In: *Econometrica* 67.2, pp. 251–333 (cit. on p. 52).

Absar, Syeda Sharmin (2009). "Problems surrounding wages: the ready made garment sector in Bangladesh". In: *Labour and Management in Development* 2 (cit. on pp. 51, 59).

Ackerberg, Daniel A., Kevin Caves, and Garth Frazer (2015). "Identification Properties of Recent Production Function Estimators". In: *Econometrica* 83.6, pp. 2411–2451 (cit. on p. 125).

Addison, John T., Claus Schnabel, and Joachim Wagner (2001). "Works councils in Germany: their effects on establishment performance". In: *Oxford Economic Papers* 53.4, pp. 659–694 (cit. on p. 6).

Adhvaryu, Achyuta, Anant Nyshadham, and Huayu Xu (2018). "Hostel Takeover: Living Conditions, Reference Dependence, and the Well-being of Migrant Workers". In: *Working Paper* (cit. on pp. 4, 21, 24).

Aghion, Philippe et al. (2008). "The Unequal Effects of Liberalization: Evidence from Dismantling the License Raj in India". In: *The American Economic Review* 98.4, pp. 1397–1412 (cit. on p. 5).

Ahmed, Nazneen (2006). *Bangladesh apparel industry and its workers in a changing world economy*. Wageningen University Thesis (cit. on p. 59).

Ahmed, Nazneen and Dev Nathan (2014). "Improving wages and working conditions in the Bangladeshi garment sector: The role of horizontal and vertical relations". In: *Capturing the Gains Working Paper* (cit. on p. 51).

Alliance for Bangladesh Worker Safety. *Alliance for Bangladesh Worker Safety Building Standards and Inspections*. `http://www.bangladeshworkersafety.org/what-we-do/standards-inspections`. Accessed: 2019-04-01 (cit. on p. 84).

Amin, Sajeda et al. (1998). "Transition to adulthood of female garment-factory workers in Bangladesh". In: *Studies in family planning*, pp. 185–200 (cit. on p. 51).

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". In: *Journal of the American Statistical Association* 103.484, pp. 1481–1495 (cit. on pp. 12, 14, 17, 32, 33, 35, 38, 39, 47, 116, 121, 122, 124, 128).

Athey, Susan and Guido Imbens (2016). "The Econometrics of Randomized Experiments". In: (cit. on pp. 14, 90).

Banerjee, Abhijit V and Esther Duflo (2005). "Growth theory through the lens of development economics". In: *Handbook of economic growth* 1, pp. 473–552 (cit. on p. 52).

Bangladesh Garment Manufacturing Exporters Association (2013). "Ready-made Garment Yearbook". In: (cit. on p. 53).

Bassi, Vittorio and Aisha Nansamba (2017a). "Information Frictions in the Labor Market: Evidence from a Field Experiment in Uganda". In: *University College London. Processed*.

— (2017b). "Information Frictions in the Labor Market: Evidence from a Field Experiment in Uganda". In: *Working Paper* (cit. on pp. 52, 80).

Beam, Emily A. (2016). "Do job fairs matter? Experimental evidence on the impact of job-fair attendance". In: *Journal of Development Economics* 120, pp. 32–40 (cit. on p. 80).

Beegle, Kathleen, Joachim De Weerdt, and Stefan Dercon (2011). "Migration and economic mobility in Tanzania: Evidence from a tracking survey". In: *Review of Economics and Statistics* 93.3, pp. 1010–1033 (cit. on p. 52).

Bénabou, Roland and Jean Tirole (2010). "Individual and Corporate Social Responsibility". In: *Economica* 77 (305), pp. 1–19 (cit. on p. 5).

Berman, Eli, Kevin Lang, and Erez Siniver (2003). "Language-skill complementarity: returns to immigrant language acquisition". In: *Labour Economics* 10.3, pp. 265–290 (cit. on p. 68).

Besley, Timothy and Robin Burgess (2004). "Can Labor Regulation Hinder Economic Performance? Evidence from India". In: *The Quarterly Journal of Economics* 119.1, pp. 91–134 (cit. on pp. 2, 5, 21).

Besley, Timothy and Maitreesh Ghatak (2007). "Retailing public goods: The economics of corporate social responsibility". In: *Journal of Public Economics* 91, pp. 1645–1663 (cit. on pp. 1, 3, 5, 27).

Bhattacharjee, Nivedita (2013). "Gap willing to sign Bangladesh safety accord, if tweaked". In: *Reuters*. URL: `http://www.reuters.com/article/2013/05/21/us-gap-ags-idUSBRE94K15120130521` (cit. on pp. 7, 83).

Biglaiser, Gary (1993). "Middlemen as experts". In: *The RAND Journal of Economics*, pp. 212–223 (cit. on p. 51).

Blanchflower, David G, Andrew J Oswald, and Peter Sanfey (1996). "Wages, profits, and rent-sharing". In: *The Quarterly Journal of Economics* 111.1, pp. 227–251 (cit. on p. 63).

Blattman, Christopher and Stefan Dercon (2018a). "The Impacts of Industrial and Entrepreneurial Work on Income and Health: Experimental Evidence from Ethiopia". In: *American Economic Journal: Applied Economics* 10 (3), pp. 1–38 (cit. on pp. 5, 49, 80, 82).

— (2018b). "The impacts of industrial and entrepreneurial work on income and health: Experimental evidence from Ethiopia". In: *American Economic Journal-Applied* forthcoming.

Bloom, Nicholas, Genakos. Christos, et al. (2010). "Modern Management: Good for the Environment or Just Hot Air?" In: *The Economic Journal* 120, pp. 551–572 (cit. on p. 4).

Bloom, Nicholas, Benn Eifert, et al. (2013). "Does Management Matter? Evidence from India". In: *The Quarterly Journal of Economics* 128.1, pp. 1–51 (cit. on pp. 12, 23).

Botero, Juan C et al. (2004). "The Regulation of Labor". In: *The Quarterly Journal of Economics* 119.4, pp. 1339–1382 (cit. on pp. 1, 5, 21).

Boudreau, Laura (2018). *Workplace safety and employment decisions: Evidence from a field experiment with Bangladeshi garment workers*. Tech. rep. Working Paper (cit. on p. 70).

Boudreau, Laura, Rachel Heath, and Tyler McCormick (2019). "Migrants, Information, and Working Conditions in Bangladeshi Garment Factories". In: *Working Paper* (cit. on pp. 28, 82).

Brandt, Loren et al. (2018). "Ownership and Productivity in Vertically-Integrated Firms: Evidence from the Chinese Steel Industry". In: *Working Paper* (cit. on p. 127).

Brauw, Alan de, Valerie Mueller, Tassew Woldehanna, et al. (2013). "Does internal migration improve overall well-being in Ethiopia?" In: *Ethiopia Strategy Support Program II* 55 (cit. on p. 52).

Brown, Charles and James Medoff (1989). "The Employer Size-Wage Effect". In: *The Journal of Political Economy* 97.5, pp. 1027–1059 (cit. on p. 52).

Bryan, Gharad, Shyamal Chowdhury, and Ahmed Mushfiq Mobarak (2014). "Underinvestment in a profitable technology: The case of seasonal migration in Bangladesh". In: *Econometrica* 82.5, pp. 1671–1748 (cit. on pp. 52, 70).

Budd, John W, Jozef Konings, and Matthew J Slaughter (2005). "Wages and international rent sharing in multinational firms". In: *Review of Economics and Statistics* 87.1, pp. 73–84 (cit. on p. 63).

Burbano, Vanessa C (2016). "Social Responsibility Messages and Worker Wage Requirements: Field Experimental Evidence from Online Labor Marketplaces". In: *Organization Science* 27.4, pp. 1010–1028 (cit. on p. 5).

Bursztyn, Leonardo et al. (2017). "Political Identity: Experimental Evidence on Anti-Americanism in Pakistan". In: *Working Paper* (cit. on p. 81).

Casey, Katherine, Rachel Glennerster, and Edward Miguel (2012). "Reshaping Institutions: Evidence on Aid Impacts Using a Preanalysis Plan". In: *Quarterly Journal of Economics* 127, 1755fffdfffdfffd1812 (cit. on pp. 6, 12).

Central Intelligence Agency (2016). *The World Factbook*. Tech. rep. Central Intelligence Agency (cit. on pp. 7, 82).

Chowdhury, Nazneen Jahan and Md Hafu Ullah (2010). "Socio-Economic Conditions of Female Garment Workers in Chittagong Metropolitan Area–An Empirical Study". In: *Journal of Business and Technology (Dhaka)* 5.2, pp. 53–70 (cit. on p. 53).

Clifford, Stephanie and Steven Greenhouse (2013). "Fast and flawed inspections of factories abroad". In: *The New York Times* September 1 (cit. on p. 55).

Dal Bó, Ernesto and Frederico Finan (2016). "At the intersection: A review of institutions in economic development". In: *Working Paper* (cit. on pp. 1, 4, 5).

De Leeuw, Jan and Patrick Mair (2007). "Homogeneity Analysis in R: The package homals". In: (cit. on p. 56).

De Melo, Rafael Lopes (2009). "Sorting in the labor market: Theory and measurement". PhD thesis. Yale University (cit. on p. 52).

DiNardo, John and David S Lee (2004). "Economic Impacts of New Unionization on Private Sector Employers: 1984fffdfffdfffd2001". In: *The Quarterly Journal of Economics* 119.4, pp. 1383–1441 (cit. on p. 6).

Distelhorst, Greg, Jens Hainmueller, and Richard M Locke (2017). "Does Lean Improve Labor Standards? Management and Social Performance in the Nike Supply Chain". In: *Management Science* 63.3, pp. 707–728 (cit. on p. 4).

Distelhorst, Greg, Richard M Locke, et al. (2015). "Production goes global, compliance stays local: Private regulation in the global electronics industry". In: *Regulation and Governance* (cit. on pp. 3, 5).

Domar, Evsey D (1961). "On the Measurement of Technological Change". In: *The Economic Journal* 71.284, pp. 709–729 (cit. on p. 127).

Dragusanu, Raluca, Daniele Giovannucci, and Nathan Nunn (2014). "The Economics of Fair Trade". In: *Journal of Economic Perspectives* 28 (3), 217fffdfffdfffd236 (cit. on p. 5).

Dragusanu, Raluca and Nathan Nunn (2018). "The Effects of Fair Trade Certification: Evidence from Coffee Producers in Costa Rica". In: *Working Paper* (cit. on pp. 3, 5).

Duflo, Esther et al. (2013). "Truth-Telling by Third Party Auditors and the Response of Polluting Firms: Experimental Evidence from India". In: *The Quarterly Journal of Economics* 128.4, 1499fffdfffdfffd1545 (cit. on pp. 4, 5).

— (2014). "The Value of Regulatory Discretion: Estimates from Environmental Inspections in India". In: *Working Paper* (cit. on p. 4).

Duleep, Harriet Orcutt and Mark C Regets (1999). "Immigrants and human-capital investment". In: *American Economic Review* 89.2, pp. 186–191 (cit. on p. 68).

El Badaoui, Eliane, Eric Strobl, and Frank Walsh (2008). "Is there an informal employment wage penalty? Evidence from South Africa". In: *Economic Development and Cultural Change* 56.3, pp. 683–710 (cit. on p. 52).

European Commission (2014). "Staying Engaged - A Sustainability Compact for continuous improvements in labour rights and factory safety in the Ready-Made Garment and Knitwear Industry in Bangladesh". In: URL: `http://trade.ec.europa.eu/doclib/docs/2014/july/tradoc_152657.pdf` (cit. on p. 55).

Fishback, Price V and Shawn E Kantor (1996). "Did Workers Pay for Workers Compensation Laws?" In: *The Quarterly Journal of Economics* 110.3, pp. 713–742 (cit. on p. 5).

Fisman, Raymond and Yongxiang Wang (2015). "The Mortality Costs of Political Connections". In: *Review of Economic Studies* 82, 1346fffdfffdfffd1382 (cit. on p. 5).

Franklin, Simon et al. (2015). *Location, search costs and youth unemployment: A randomized trial of transport subsidies in Ethiopia*. Tech. rep. Centre for the Study of African Economies, University of Oxford (cit. on p. 52).

Frazer, Garth (2005). "Which firms die? A look at manufacturing firm exit in Ghana". In: *Economic Development and cultural change* 53.3, pp. 585–617 (cit. on p. 65).

Friedman, Milton (1970). "The Social Responsibility of Business is to Increase its Profits". In: *The New York Times Magazine*, pp. 1–19 (cit. on p. 5).

Gabaix, Xavier and David Laibson (2006). "Shrouded Attributes, Consumer Myopia, and Information Suppression in Competitive Markets". In: *The Quarterly Journal of Economics* 121.2, pp. 505–540 (cit. on p. 58).

Ghosh, Palash (2014). "Despite Low Pay, Poor Work Conditions, Garment Factories Empowering Millions Of Bangladeshi Women". In: *International Business Tribune* March 24.1 (cit. on p. 53).

Gifi, Albert (1981). *Nonlinear multivariate analysis*. University of Leiden Leiden (cit. on p. 56).

Government of Bangladesh (2015). *Bangladesh Gazette: Ministry of Labor and Employment Notification*. English translation. S.R.O. No. 291 (cit. on p. 36).

Greenhouse, Steven (2013a). "Obama to Suspend Trade Privileges with Bangladesh". In: *The New York Times*. URL: `https://nyti.ms/14ZF5Qk` (cit. on pp. 7, 83).

— (2013b). "U.S. Retailers See Big Risk in Safety Plan for Factories in Bangladesh". In: *The New York Times*. URL: `http://nyti.ms/13Mb06n` (cit. on pp. 7, 83).

Hainmueller, Jens, Michael J Hiscox, and Sandra Sequeira (2015). "Consumer demand for fair trade: Evidence from a multistore field experiment". In: *The Review of Economics and Statistics* 97 (2), 242fffdfffdfffd256 (cit. on p. 5).

Hardy, Morgan and Jamie McCasland (2015). *Are Small Firms Labor Constrained? Experimental Evidence from Ghana*. Tech. rep. Working paper (cit. on p. 52).

Harris, John R and Michael P Todaro (1970). "Migration, unemployment and development: a two-sector analysis". In: *The American Economic Review* 60.1, pp. 126–142 (cit. on p. 52).

Harrison, Ann and Andrés Rodríguez-Clare (2010). "Trade, Foreign Investment, and Industrial Policy for Developing Countries". In: *Handbook of Development Economics* 5, pp. 4039–4214 (cit. on p. 50).

Harrison, Ann and Jason Scorse (2010a). "Multinationals and anti-sweatshop activism". In: *The American Economic Review* 100.1, pp. 247–273 (cit. on pp. 5, 50).

— (2010b). "Multinationals and anti-sweatshop activism". In: *The American Economic Review* 100.1, pp. 247–273.

Hart, Oliver and Luigi Zingales (2017). "Companies Should Maximize Shareholder Welfare Not Market Value". In: *Journal of Law, Finance, and Accounting* 2, 247fffdfffdfffd274 (cit. on p. 5).

Haushofer, Johannes and Jeremy Shapiro (2016). "The short-term impact of unconditional cash transfers to the poor: Experimental evidence from Kenya". In: *Quarterly Journal of Economics*, pp. 1973–2042 (cit. on pp. 12, 14).

Heath, Rachel (2018). "Why do firms hire using referrals? Evidence from Bangladeshi Garment Factories". In: *Journal of Political Economy* 126.4, pp. 1691–1746 (cit. on pp. 52, 55, 67).

Heath, Rachel and Ahmed Mushfiq Mobarak (2015a). "Manufacturing growth and the lives of Bangladeshi women". In: *Journal of Development Economics* 115, pp. 1–15 (cit. on pp. 7, 70, 83).

— (2015b). "Manufacturing growth and the lives of Bangladeshi women". In: *Journal of Development Economics* 115.

Henderson, V, Adam Storeygard, and U Deichmann (2015). *Has climate change driven urbanization in Africa?* Tech. rep. Working paper, London School of Economics (cit. on p. 52).

Heß, Simon (2017). "Randomization inference with Stata: A guide and software". In: *Stata Journal* 17.3, pp. 630–651 (cit. on pp. 14, 90).

Humphreys, Macartan, Rául Sánchez de la Sierra, and Peter Van der Windt (2017). "Institutional Change by Imitation: Introducing Western Governance Practices in Congolese Villages". In: *Working Paper* (cit. on p. 6).

International Labor Organization (2017). *Towards safer working conditions in the Bangladesh ready-made garment sector*. Tech. rep. International Labor Organization (cit. on p. 8).

International Restructuring Education Network Europe (1990). "Textile and Garment Industry in Bangladesh: Reports from Women Workers' Struggles". In: *South Asia Bulletin* 10.2 (cit. on p. 51).

International Trade Union Confederation (2018). *2018 ITUC Global Rights Index: The World's Worst Countries for Workers*. Tech. rep. Brussels, Belgium: International Trade Union Confederation (cit. on p. 1).

Jensen, Robert (2010). "The (perceived) returns to eduction and the demand for schooling". In: *Quarterly Journal of Economics*, pp. 515–548 (cit. on p. 80).

Johnson, William R. (1978). "A Theory of Job Shopping". In: *The Quarterly Journal of Economics* 92.2, pp. 261–278 (cit. on p. 82).

Jovanovic, Boyan (1979). "Job matching and the theory of turnover". In: *The Journal of Political Economy*, pp. 972–990 (cit. on p. 51).

Kinnan, Cynthia, Shing-Yi Wang, and Yongxiang Wang (2015). *Relaxing Migration Constraints for Rural Households*. Tech. rep. National Bureau of Economic Research (cit. on p. 52).

Kleemans, Marieke (2014). "Migration Choice under Risk and Liquidity Constraints". In: *University of California, Berkeley* (cit. on p. 52).

Kleemans, Marieke and Jeremy Magruder (2015). "Labor market changes in response to immigration: evidence from internal migration driven by weather shocks". In: *Unpublished manuscript* (cit. on p. 52).

Krueger, Alan B and Lawrence H Summers (1988). "Efficiency wages and the inter-industry wage structure". In: *Econometrica: Journal of the Econometric Society*, pp. 259–293 (cit. on p. 52).

Labowitze, Sarah (2016). "New data on the number of factories in Bangladesh". In: *NYU Stern Center for Business and Human Rights; https://bhr.stern.nyu.edu/blogs/-data-on-number-of-factories-bd* (cit. on p. 65).

Lai, Chien-Yu et al. (2017). "The Business of Business is Business: Why (Some) Firms Should Provide Public Goods when they Sell Private Goods". In: *Working Paper* (cit. on pp. 3, 27).

Lee, David S. (2009). "Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects". In: *Review of Economic Studies* 76.3, pp. 1071–1102 (cit. on p. 14).

Lee, David S. and Alexandre Mas (2012). "Long-Run Impacts of Unions on Firms: New Evidence from Financial Markets, 1961fffdfffdfffd1999 *". In: *The Quarterly Journal of Economics* 127.1, pp. 333–378 (cit. on p. 6).

Levine, David I, Michael W Toffel, and Matthew S Johnson (2012). "Randomized Government Safety Inspections Reduce Worker Injuries with No Detectable Job Loss". In: *Science* 336, 907fffdfffdfffd911 (cit. on p. 6).

Lewis, W Arthur (1954). "Economic development with unlimited supplies of labour". In: *The manchester school* 22.2, pp. 139–191 (cit. on p. 52).

List, John A, Azeem M Shaikh, and Yang Xu (2016). "Multiple hypothesis testing in experimental economics". In: *NBER Working Paper* (cit. on pp. 14, 38, 121).

Locke, Richard M. (2013). *The Promise and Limits of Private Power: Promoting Labor Standards in a Global Economy*. Cambridge, United Kingdom: Cambridge University Press. ISBN: 9781107670884 (cit. on pp. 3, 5).

Locke, Richard M, Matthew Amengual, and Akshay Mangla (2009). "Virtue out of Necessity? Compliance, Commitment, and the Improvement of Labor Conditions in Global Supply Chains". In: *Politics and Society* 37.3, pp. 319–351 (cit. on pp. 3, 5).

Locke, Richard M, Fei Qin, and Alberto Brause (2007). "Does monitoring improve standards? Lessons from Nike". In: *Industrial and Labor Relations Review* 61.1, pp. 3–31 (cit. on pp. 3, 5).

Locke, Richard M and Monica Romis (2010). "The promise and perils of private voluntary regulation: Labor standards and work organization in two Mexican garment factories". In: *Review of International Political Economy* 17.1, pp. 45–74 (cit. on pp. 3, 5).

Macchiavello, Rocco et al. (2015). "Challenges of Change: An Experiment Training Women to Manage in the Bangladeshi Garment Sector". In: *Working Paper* (cit. on p. 23).

Magruder, Jeremy R (2010). "Intergenerational networks, unemployment, and persistent inequality in South Africa". In: *American Economic Journal: Applied Economics* 2.1, pp. 62–85 (cit. on p. 52).

Mahmud, Simeen and Naila Kabeer (2003). "Compliance versus accountability: struggles for dignity and daily bread in the Bangladesh garment industry". In: *The Bangladesh Development Studies*, pp. 21–46 (cit. on p. 51).

Mailath, George J and Larry Samuelson (2013). "Reputations in repeated games". In: *The Handbook of Game Theory* 4 (cit. on p. 51).

Marchiori, Luca, Jean-François Maystadt, and Ingmar Schumacher (2012). "The impact of weather anomalies on migration in sub-Saharan Africa". In: *Journal of Environmental Economics and Management* 63.3, pp. 355–374 (cit. on p. 52).

McDevitt, Ryan C (2011). "Names and Reputations: An Empirical Analysis". In: *American Economic Journal: Microeconomics* 3.3, pp. 193–209 (cit. on p. 1).

McKinsey & Company (2011). *Bangladesh's ready-made garments landscape: The challenge of growth*. Tech. rep. McKinsey & Company (cit. on pp. 6, 7, 83).

Ministry of Labour and Employment (2013). *National Tripartite Plan of Action on Fire Safety and Building Integrity in the Ready-Made Garment Sector in Bangladesh*. Tech. rep. Dhaka, Bangladesh: Ministry of Labour and Employment (cit. on p. 7).

Morten, Melanie (2013). "Temporary migration and endogenous risk sharing in village india". In: *Unpublished Manuscript* (cit. on p. 52).

Munni (2017). "Half of apparel units flout needs for safety committees". In: *The Financial Express*. URL: `http://thefinancialexpress.com.bd/print/half-of-apparel-units-flout-needs-for-safety-committees-1506062868` (cit. on p. 8).

Munshi, Kaivan and Mark Rosenzweig (2016). "Why is mobility in India so low? Social insurance, inequality, and growth". In: *American Economic Review* (cit. on p. 52).

O'Brien, Peter C (1984). "Procedures for Comparing Samples with Multiple Endpoints". In: *Biometrics* 40.4, 1079fffdfffdfffd1087 (cit. on p. 12).

Oka, Chikako (2010). "Accounting for the gaps in labour standard compliance: The role of reputation-conscious buyers in the Cambodian garment industry". In: *European Journal of Development Research* 22.1, p. 59 (cit. on p. 51).

O'Rourke, Dara (2014). "The Science of Sustainable Supply Chain". In: *Science* 344.6188, pp. 1124–1127 (cit. on p. 1).

Oster, Emily and Bryce Millett Steinberg (2013). "Do IT service centers promote school enrollment? Evidence from India". In: *Journal of Development Economics* 104, pp. 123–135 (cit. on p. 80).

Robertson, Raymond et al. (2009). "Globalization and working conditions: Evidence from Indonesia". In: *Globalization, Wages, and the Quality of Jobs*, p. 203 (cit. on p. 51).

Saxena, Sanchita (2014). *Made in Bangladesh, Cambodia, and Sri Lanka: the labor behind the global garments and textiles industries*. Cambria Press (cit. on p. 53).

Shanmugam, KR (2001). "Self selection bias in the estimates of compensating differentials for job risks in India". In: *Journal of Risk and Uncertainty* 22.3, pp. 263–275 (cit. on p. 51).

Short, Jodi L, Michael W Toffel, and Andrea R Hugill (2016). "Monitoring global supply chains". In: *Strategic Management Journal* 37, 1878fffdfffdfffd1897 (cit. on p. 1).

Shrestha, Maheshwor (2016). "Get rich or die tryinfffdfffdfffd: Perceived earnings, perceived mortality rate and the value of a statistical life of potential work-migrants from Nepal". In: (cit. on p. 89).

Sigg, Christian (2014). "Package nsprcomp". In: (cit. on p. 56).

Sigg, Christian D and Joachim M Buhmann (2008). "Expectation-maximization for sparse and non-negative PCA". In: *Proceedings of the 25th international conference on Machine learning*. ACM, pp. 960–967 (cit. on p. 56).

Smith, Robert S. (1979). "Compensating wage differentials and public policy: A review". In: *Industrial and Labor Relations Review* 32.3, pp. 339–352 (cit. on p. 80).

Söderbom, Måns, Francis Teal, and Alan Harding (2006). "The determinants of survival among African manufacturing firms". In: *Economic Development and Cultural Change* 54.3, pp. 533–555 (cit. on p. 65).

Solidarity Center (2016). *Bangladesh Garment and Textile Factory Safety Incidents Since Tazreen Fashion Factory Fire, November 24, 2012*. Tech. rep. Solidarity Center (cit. on pp. 7, 83).

Sorkin, Isaac (2018). "Ranking firms using revealed preference". In: *The Quarterly Journal of Economics* 1, p. 63 (cit. on p. 51).

Strobl, Eric and Marie-Anne Valfort (2015). "The effect of weather-induced internal migration on local labor markets. Evidence from Uganda". In: *The World Bank Economic Review* 29.2, pp. 385–412 (cit. on p. 52).

Syverson, Chad (2011). "What Determines Productivity?" In: *Journal of Economic Literature* 49.2, pp. 326–365 (cit. on p. 12).

Tadelis, Steven (2002). "The Market for Reputations as an Incentive Mechanism". In: *Journal of Political Economy* 110.4, pp. 854–882 (cit. on p. 1).

Tanaka, Mari (2015). "Exporting Sweatshops? Evidence from Myanmar". In: *mimeo* (cit. on p. 51).

Teal, Francis (1996). "The size and sources of economic rents in a developing country manufacturing labour market". In: *The Economic Journal*, pp. 963–976 (cit. on p. 52).

The Alliance for Bangladesh Worker Safety (2014). *Alliance Fire Safety and Structural Integrity Standard*. Tech. rep. (cit. on p. 141).

Toffel, Michael W, Jodi L Short, and Melissa Ouellet (2015). "Codes in context: How states, markets, and civil society shape adherence to global labor standards". In: *Regulation and Governance* 9, pp. 206–223 (cit. on pp. 3, 5).

Van Reenen, John (1996). "The creation and capture of rents: wages and innovation in a panel of UK companies". In: *The Quarterly Journal of Economics* 111.1, pp. 195–226 (cit. on p. 63).

Viscusi, W. Kip (1979). "Job hazards and worker quit rates: An analysis of adaptive worker behavior". In: *International Economic Review* 20.1, pp. 29–58 (cit. on p. 82).

Viscusi, W Kip (1980). "A theory of job shopping: A Bayesian perspective". In: *The Quarterly Journal of Economics* 94.3, pp. 609–614.

Viscusi, W. Kip (1980). "A theory of job shopping: A Bayesian perspective". In: *The Quarterly Journal of Economics* 94.13, pp. 609–614 (cit. on pp. 51, 82).

Wang, Shing-Yi (2013). "Marriage Networks, Nepotism, and Labor Market Outcomes in China". In: *American Economic Journal: Applied Economics* 5.3, pp. 91–112 (cit. on p. 52).

World Trade Organization (2017). *World Trade Statistical Review 2017*. Tech. rep. Geneva, Switzerland: World Trade Organization (cit. on p. 6).

Yao, Yang and Ninghua Zhong (2013). "Unions and Workersfffdfffdfffd Welfare in Chinese Firms". In: *Journal of Labor Economics* 31.3, pp. 633–667 (cit. on p. 6).

Yassi, Annalee et al. (2013). "Effectiveness of Joint Health and Safety Committees: A Realist Review". In: *American Journal of Industrial Medicine* 56, 424fffdfffdfffd438 (cit. on p. 6).

Young, Alwyn (2015). "Channeling Fisher: Randomization Tests and the Statistical Insignificance of Seemingly Significant Experimental Results". In: *Working Paper* (cit. on pp. 14, 90).

Appendices

Appendix A

Table A1: Baseline balance tests, sub-index components

	(1) Control mean n=41	(2) T-C diff n=39	(3) RI *p*-value
Panel A: Compliance			
Formation sub-index	0.000	0.055	0.716
Operations sub-index	-0.000	0.006	0.966
Responsibilities sub-index	-0.000	-0.051	0.592
Panel B: SC Effectiveness			
CAP completion sub-variable	0.016	0.106	0.626
Worker awareness sub-index	0.000	-0.511**	0.044
Worker knowledge sub-index	0.000	-0.062	0.755
Senior manager awareness sub-variable	0.000	0.432*	0.076
Panel C: Worker job satisfaction and mental well-being			
Job satisfaction sub-index	0.000	-0.202	0.233
Mental well-being sub-index	0.000	-0.199	0.322
Turnover sub-variable	0.000	0.141	0.474
Absenteeism sub-variable	-0.000	0.148	0.458
Dropping outlier:			
Job satisfaction sub-index	0.000	-0.158	0.333
Mental well-being sub-index	0.000	-0.091	0.560
Turnover sub-variable	0.000	0.141	0.480
Absenteeism sub-variable	0.000	0.141	0.483

Notes: This table reports OLS estimates of baseline differences between control and treatment groups for the sub-indexes and sub-variables that comprise each primary outcome index. Each panel reports the sub-index/sub-variable balance tests for a different outcome variable. For each sub-index or sub-variable, column (1) reports the baseline control group mean. Column (2) 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. Column (3) reports the randomization inference (RI) *p*-value for the coefficient reported in column (2) based on 5000 draws. The regression sample remains the same in all rows. *p<0.1; **p<0.05; ***p<0.01.

Table A2: Lee (2009) bounds for primary outcome index variables

	Lower bound	Upper bound
SC compliance index	0.185	0.186
	(0.073)**	(0.069)**
SC effectiveness index	0.138	0.141
	(0.090)	(0.072)*
Job satisfaction & mental well-being index	-0.158	-0.156
	(0.081)*	(0.084)*

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 A3: Baseline balance tests, alternative measures of factory productivity

	(1) Control mean	(2) T-C diff	(3) RI *p*-value
TFP: Baseline measure			
Total factor productivity (log) (n=50) (dropping accessories and packaging factories)	2.556	0.132	0.795
TFP: Alternative measures 2 and 3			
Total factor productivity (log) (Measure 2) (n=58)	2.923	0.389	0.453
Total factor productivity (log) (Measure 2) (n=50) (dropping accessories and packaging factories)	2.636	0.307	0.538
Total factor productivity (log) (Measure 3) (n=56)	2.942	0.111	0.828
Total factor productivity (log) (Measure 3) (n=50) (dropping accessories and packaging factories)	2.659	0.080	0.875
TFP: Product-level			
Baseline TFP measure			
Total factor productivity (log) (product-level) (n=56)	3.092	-0.072	0.911
Total factor productivity (log) (product-level) (n=50) (dropping accessories and packaging factories)	2.821	0.145	0.839
TFP measure (2)			
Total factor productivity (log) (product-level) (n=58)	3.153	0.125	0.839
Total factor productivity (log) (product-level) (n=56) (dropping accessories and packaging factories)	2.896	0.095	0.896
TFP measure (3)			
Total factor productivity (log) (product-level) (n=56)	3.180	-0.060	0.912
Total factor productivity (log) (product-level) (n=50) (dropping accessories and packaging factories)	2.914	0.154	0.812

Notes: This table reports OLS estimates of baseline differences between control and treatment groups. The number of factories included in the sample to determine the difference is indicated in each row; for each factory, 5 pre-baseline observations are included. For each outcome or covariate, 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 outcome or covariate on the treatment indicator and stratification variables. In column (3), I report the randomization inference (RI) *p*-value for the coefficient reported in column (2) based on 5000 draws. *p<0.1; **p<0.05; ***p<0.01.

Table A4: Treatment effects: Worker job satisfaction & mental well-being sub-variables

	Control mean	ITT Effect	
	(1)	(2)	(3)
Panel A: Worker Job Satisfaction			
Self-reported job satisfaction (qualitative scale, coded 1-5)	4.813	-0.064 [0.205] {0.234}	-0.045 [0.384] {0.345}
Respondent suggested/helped family or friends to get a job at their factory (previous 4 months)	0.573	-0.056 [0.208] {0.234}	-0.049 [0.266] {0.345}
Respondent has thought about leaving their job at factory for safety-related reasons (previous 3 months)	0.024	0.019* [0.063] {0.234}	0.019* [0.064] {0.238}
Panel B: Worker Mental Well-being			
Self-reported level of stress in life (qualitative scale, coded (-1)-(-5))	-1.760	-0.058 [0.476]	-0.059 [0.474]
Self-reported perceived extent of control over their life (qualitative scale, coded 1-5)	4.083	-0.057 [0.333]	-0.037 [0.521]
Self-reported perceived extent of control safety at factory (qualitative scale, coded 1-5)	4.368	-0.045 [0.435]	-0.037 [0.520]
Self-reported stress about experiencing accident or injury at factory (qualitative scale, coded (-1)-(-5))	-1.489	0.041 [0.522]	0.041 [0.526]
Self-reported frequency of feeling unsafe at factory (qualitative scale, coded (-1)-(-5))	-1.236	-0.014 [0.677]	-0.013 [0.691]
Panel C: Turnover and Absenteeism			
Turnover	3.356	-0.360 [0.570]	-0.094 [0.779]
Absenteeism	4.457	-0.139 [0.852]	0.040 [0.898]
Observations		80	80
Stratification variables		Y	Y
Control, base. dep. var.		N	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 and stratification variables. Column (3) 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. Randomization inference (RI) p-values based on 5000 draws are reported in square brackets. *p<0.1; **p<0.05; ***p<0.01.

Table A5: Treatment effects: Absenteeism and turnover

	Dep. variable:			
	Absenteeism		Turnover	
	(1)	(2)	(3)	(4)
	Panel A: Full sample			
Treatment x Post	0.534	0.534	0.336	0.408
	[0.094]*	[0.103]	[0.617]	[0.524]
Factories	80	80	80	80
Observations	799	799	800	800
Control mean, dep. var	4.457	4.457	3.356	3.356
	Panel B: Dropping factories with capital expansion			
Treatment x Post	0.548	0.523	0.266	0.336
	[0.061]*	[0.060]*	[0.733]	[0.647]
Factories	76	76	76	76
Observations	759	759	760	760
Control mean, dep. var	4.414	4.414	3.173	3.173
Factory FE	Y	Y	Y	Y
Calendar month FE	N	Y	N	Y

Notes: This table reports panel regression estimates of treatment effects on worker turnover and absenteeism. Each column in the table reports the estimated coefficient from a separate regression. Four factories that underwent capital expansion during the experimental phase are dropped from the analysis. In each panel regression, there are 10 observations per factory, 5 pre-baseline and 5 post-baseline. The dependent variable in each column is regressed on an interaction between the treatment indicator and a post-treatment indicator variable, calendar month fixed effects, and factory fixed effects. Randomization inference (RI) p-values based on 5000 draws are reported in square brackets. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A6: Treatment effects: Workforce composition

	Dependent variable:				
	Age	Female	Tenure	Prior exp.	Yrs. Education
	(1)	(2)	(3)	(4)	(5)
Treatment effect	−0.200	−0.040	0.233	0.042	0.255
	[0.698]	[0.283]	[0.473]	[0.823]	[0.362]
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 workforce characteristics. 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. Randomization inference (RI) p-values based on 5000 draws are reported in square brackets. *p<0.1; **p<0.05; ***p<0.01.

Table A7: Heterogeneous treatment effects: Alliance contact through other Alliance programs

	Mgmt (Prod)		Mgmt (HR)	
	(1)	(2)	(3)	(4)
Panel A: *Corrective Action Plan (CAP) remediation verification visits*				
Below median	0.260	0.271	-0.071	-0.072
	[0.296]	[0.277]	[0.790]	[0.790]
Control subgroup mean, dep. var.	0.333	0.333	0.409	0.409
Above Median	0.201	0.200	0.394	0.395
	[0.456]	[0.456]	[0.158]	[0.166]
Control subgroup mean, dep. var.	0.826	0.826	0.842	0.842
p-val, diff between rows 1 & 2	[0.870]	[0.845]	[0.238]	[0.244]
Panel B: *Alliance fire safety training program participation*				
Below median	0.134	0.137	0.149	0.154
	[0.246]	[0.238]	[0.188]	[0.167]
Control subgroup mean, dep. var.	0.057	0.057	0.053	0.053
Above Median	0.006	0.000	0.005	-0.004
	[0.866]	[0.999]	[0.911]	[0.918]
Control subgroup mean, dep. var.	0.043	0.043	0.045	0.045
p-val, diff between rows 1 & 2	[0.341]	[0.307]	[0.296]	[0.254]
Observations	80	80	80	80
Stratification variables	Y	Y	Y	Y
Control, base. dep. var.	N	Y	N	Y

Note: This table reports OLS estimates of heterogeneous factory contact/interaction with the Alliance for HR management-related dimensions of heterogeneity. Each dimension of heterogeneity is indicated at the top of the table. In Panel A, the outcome is the number of building safety remediation verification visits that the Alliance made to the factory between the baseline and 4-5 month data collection visits. In Panel B, the outcome is whether the Alliance requested the factory to participate in its fire safety training program between the baseline and 4-5 month data collection visits. 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. For each dimension of heterogeneity, I estimate the treatment effects without and with a control for the baseline value of the dependent variable. All regressions include stratification variables. All subgroups have 40 observations. Randomization inference (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.

Figure A1: Primary outcome 1: SC Compliance Index Components

sub_index	question	variable source 1	variable source 2
formation	Equal worker-manager representation (or more workers than managers)	doc_verification	
formation	Number of members is greater than or equal to mandated number of members	doc_verification	
formation	President is management member and Vice President is worker member	namelist (midline, endline only)	
formation	Compliant worker representative selection process: CBA, PC, or WWA as required	sc president survey	sc worker member survey
formation	Management does not select worker representatives on SC	sc president survey	sc worker member survey
formation	In factories with >= (1/3) female workforce, at least (1/3) worker representatives are female	doc_verification	
formation	Factory maintains list of SC Members	doc_verification	
formation	Correlation between SC President's reports and factory documentation	doc_verification	sc president survey
operations	Factory maintains description of SC Members' roles and responsibilities	doc_verification	
operations	Factory Safety Policy includes a section on the Safety Committee's role and responsibilities	doc_verification	
operations	Safety Committee meets at least 1 time per 3 months	doc_verification	
operations	Frequency of meetings per 3 months	doc_verification	
operations	Meeting minutes are available for all Safety Committee meetings in past three months	doc_verification	
operations	Meeting attendance lists are available for all Safety Committee meetings in past three months	doc_verification	
operations	Safety Committee members have received training in their role on SC	sc president survey	sc worker member survey
operations	Safety Committee members considered on duty during the time they spend on Committee me	sc president survey	sc worker member survey
operations	Safety Committee uses compliant decision rule (unanimous or majority vote)	sc president survey	sc worker member survey
operations	Correlation between SC President's reports and factory documentation	doc_verification	sc president survey
operations	Correlation between SC President's reports and SC worker member reports	sc president survey	sc worker member survey
operations	Management interference in SC operations: Members of management provided any payment:	sc worker member survey	
responsibilities	Safety Committee has completed a risk assessment of the factory.	doc_verification	
responsibilities	Safety Committee has developed an action plan for safety improvements.	doc_verification	
responsibilities	Safety Committee makes regular safety reports/recommendations to management	sc president survey	sc worker member survey
responsibilities	Frequency of follow-up: Regular reports and recommendations to management	sc president survey	sc worker member survey
responsibilities	Senior management frequency of reports from SC (should be minimum 1x month or quarter)	sen_mgmt_surv (all 3)	
responsibilities	Safety Committee organizes training and fire drills	sc president survey	sc worker member survey
responsibilities	Number of fire drills, previous 3 months	doc_verification	
responsibilities	Proportion of workers who report participation in safety-related training	worker survey	
responsibilities	Proportion of workers who report participation in fire drill	worker survey	
responsibilities	Safety Committee regularly inspects the factory's machinery and equipment and make sugges	sc president survey	sc worker member survey (midline, endline only)
responsibilities	Safety Committee participation in the oversight and implementation of the factory's managen	sc president survey	sc worker member survey (midline, endline only)
responsibilities	Safety Committee participation in the oversight and implementation of the factory's fire preve	sc president survey	sc worker member survey (midline, endline only)
responsibilities	Safety Committee participation in the oversight and implementation of the factory's health pr	sc president survey	sc worker member survey (midline, endline only)
responsibilities	Safety Committee investigates accidents and make recommendations to prevent future accide	sc president survey	sc worker member survey
responsibilities	In case of on-the-job worker injury or occupational disease, Safety Committee mediates betwe	sc president survey	sc worker member survey

Figure A2: Primary outcome 2: SC Effectiveness Index Components

sub_index	question	variable source
spot_check	Aisles in section are clearly marked, and markings are easily visible	spot_check (midline, endline only)
spot_check	Aisles in section are clear of obstruction	spot_check (midline, endline only)
spot_check	Aisles in section are clear of sewing scraps or other materials	spot_check (midline, endline only)
spot_check	There is a physical separation between areas where materials are stored and areas where personnel are working (in this section)	spot_check (midline, endline only)
spot_check	Windows, fans, air conditioners or heaters are operational for air circulation, ventilation and provide an acceptable work floor temperature (in this section)	spot_check (midline, endline only)
spot_check	Machines are in good working order and points of operation and other potential dangerous parts are operated with proper machine guards and safety features (i.e., all reeling and dangerous parts of machines are covered) (machines in section)	spot_check (midline, endline only)
spot_check	Individual machines have an individual power shut-off switch within reach of the operator (machines in section)	spot_check (midline, endline only)
spot_check	Fire extinguisher and other fire-fighting materials are in clear view and easily accessible (in section)	spot_check (midline, endline only)
spot_check	Emergency exits are clearly marked with illuminated exit signs (in section)	spot_check (midline, endline only)
spot_check	Evacuation plan is easily visible in all production areas in section	spot_check (midline, endline only)
spot_check	At least one easily accessible first aid kit in section in section	spot_check (midline, endline only)
spot_check	Drinking water is easily accessible for all workers in section *(within 100 meters for all workers in sec*	spot_check (midline, endline only)
spot_check	Visual check of drinking water provided for workers appears clean (in section)	spot_check (midline, endline only)
spot_check	**Sewing:** Sewing machines are equipped with appropriate machine guards and workers wear appropriate PPE for their task (e.g., eye guards for button sewing, finger guards for pocket welt sewing) (in section)	spot_check (midline, endline only)
spot_check	**Cutting:** Cutting machines are equipped with knife guards and workes wear appropriate PPE for their task (e.g., chain mesh gloves for cutting tasks) (in section)	spot_check (midline, endline only)
spot_check	**Dyeing and jobs handling chemicals:** Safety masks, goggles, gloves, aprons, and boots are worn by workers handling chemicals (in section)	spot_check (midline, endline only)
spot_check	All PPE provided are of appropriate size, are functional, and appear well-maintained (in section)	spot_check (midline, endline only)
spot_check	All work stations are maintained in tidy condition, with no loose materials close to electrical appliances (in section)	spot_check (midline, endline only)
spot_check	Machines are appropriatly placed and spaced (1 meter from wall with 1 meter aisles between) (machines in section)	spot_check (endline only)
spot_check	Fire doors are installed, unlocked, and without obstruction (in section)	spot_check (endline only)
spot_check	Toilet facilities in section are clean, functional (clean running water and soap), and provide privacy (stalls with doors)	spot_check (endline only)
spot_check	Chemicals are stored separately from production activites in a well-ventilated room	spot_check (endline only)
spot_check	Chemicals are stored in appropriate containers and containers are stored in an orderly fashion	spot_check (endline only)
spot_check	Material safety data sheets (MSDS) are prominently posted in both storage and use zones, and maintained in languages understood by workers	spot_check (endline only)
spot_check	Chemicals and hazardous substances are properly labelled as per label instructions/MSDS	spot_check (endline only)
spot_check	No loose wiring visible in production area (in section)	spot_check (endline only)
cap	Percent compliant	Alliance CAP data
awareness	Proportion of workers aware that factory has a SC	worker survey
awareness	Proportion of workers aware of SC's function and responsibilities	worker survey
awareness	Proportion of workers aware of how to contact SC member with issue	worker survey
awareness	Proportion of workers aware of SC topic-specific responsibilities	worker survey (midline, endline only)
safety knowledge	Proportion of workers correctly answer fire question	worker survey
safety knowledge	Proportion of workers correctly answer earthquake question	worker survey
senior_mgmt	Senior management can provide at least one example of one issue identified by SC that has been resolved	senior manager survey

Figure A3: Primary outcome 3: Worker Job Satisfaction and Mental Well-being Index Components

sub_index	question	variable source
job_satisfaction	How satisfied are you with your job at your factory?	worker survey
job_satisfaction	Have you suggested to or helped family or friends to get a job at your factory?	worker survey
job_satisfaction	In the past three months or since you began working at this factory if less than three months	worker survey
mental_wellbeing	In general, how stressed are you about things in your life?	worker survey
mental_wellbeing	How much control you feel that you have over the way your life turns out?	worker survey
mental_wellbeing	How much control you feel that you have over your safety at the factory?	worker survey
mental_wellbeing	How stressed are you about the risk of experiencing an accident or injury at your factory?	worker survey
mental_wellbeing	How often do you feel unsafe when you are working at the factory?	worker survey
absenteeism		factory questionnaire
turnover		factory questionnaire

Figure A4: Worker secondary outcome 1: Worker Empowerment Index Components

sub_index	question	variable source
safety_empowerment	How confident are you in your ability to identify unsafe conditions at your factory?	worker survey
safety_empowerment	To what extent do you think that you or workers like you at your factory are capable of contributing ideas that can improve safety at the factory?	worker survey
safety_empowerment	Have you reported a safety concern at your factory in the last year or since you began working at this factory if less than one year ago?	worker survey
safety_empowerment	If you were to have a safety concern, would you report it?	worker survey
safety_empowerment	If you were to get hurt at work, would you report the incident?	worker survey
safety_empowerment	Mean reported comfort sharing safety concern with member of Safety Committee, workers	worker survey
gen_empowerment	To what extent do you think that you or workers like you at your factory are capable of contributing ideas that can improve productivity at the factory?	worker survey
gen_empowerment	Do you feel that if you wanted to change jobs, you could?	worker survey
gen_empowerment	Do you have a goal for job promotion at your factory (e.g., operator level, process supervisor or production line manager)?	worker survey

Appendix B

Table B1: Baseline balance tests (dropping outlier)

	(1) Control mean n=41	(2) T-C diff n=38	(3) RI *p*-value
Primary outcome variables			
Compliance index	0.000	-0.023	0.758
Effectiveness index	0.002	0.006	0.957
Well-being index	0.000	-0.043	0.623
Formation sub-index	0.000	0.041	0.778
Operations sub-index	0.000	-0.008	0.950
Responsibilities sub-index	-0.000	-0.060	0.542
CAP completion sub-variable	0.016	0.109	0.607
Worker awareness sub-index	-0.000	-0.395*	0.065
Worker knowledge sub-index	-0.000	-0.068	0.739
Senior manager awareness sub-variable	0.000	0.406	0.104
Job satisfaction sub-index	0.000	-0.158	0.333
Mental well-being sub-index	0.000	-0.091	0.560
Turnover sub-index	0.000	0.141	0.480
Absenteeism sub-index	0.000	0.141	0.483
Factory characteristics			
Employment	1192	-240	0.459
Trade union at factory (1=Yes)	0.049	-0.049	0.304
EPZ(1=Yes)	0.171	0.014	0.869
Survey respondent characteristics			
Age	27.160	0.478	0.578
Female (Y=1)	0.556	-0.096	0.142
Education (yrs)	6.240	-0.341	0.374
Tenure (yrs)	3.861	-0.184	0.735
Prior industry experience (yrs)	1.516	0.067	0.769

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 in column (1). In column (2), 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 (3), I report the randomization inference (RI) p-value for the coefficient reported in column (2) based on 5000 draws. The regression sample remains the same in all rows. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table B2: Treatment effects: Primary Outcome Index Variables (dropping outlier)

	Control mean	ITT Effect	
	(1)	(2)	(3)
SC Compliance	0.053	0.182	0.193
		[0.011]**	[0.005]***
		{0.028}**	{0.005}***
SC Effectiveness	0.103	0.141	0.140
		[0.058]*	[0.061]*
		{0.059}*	{0.105}
Worker job satisfaction & mental well-being	-0.013	-0.151	-0.147
		[0.069]*	[0.033]**
		{0.108}	{0.060}*
Observations		79	79
Stratification variables		Y	Y
Control, base. dep. var.		N	Y

Notes: This table reports OLS estimates of treatment effects on primary outcome index variables. Outcome variables are listed on the left. In all cases, higher values of the index correspond to "positive" outcomes. 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. Column (3) 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. Randomization inference (RI) p-values based on 5000 draws are reported in square brackets. p-values adjusted for multiple hypothesis testing using the method of List, Shaikh, and Y. Xu 2016 are reported in curly brackets. Index variables constructed using Anderson 2008 variance-covariance weighted index. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table B3: Treatment effects: Primary outcome sub-indexes and sub-variables

	Control mean	ITT Effect	
	(1)	(2)	(3)
Panel A: SC Compliance			
Formation sub-index	0.070	0.015	-0.017
		[0.761]	[0.819]
Operations sub-index	0.177	0.091	0.247
		[0.240]	[0.002]
Responsibilities sub-index	-0.054	0.331	0.175
		[0.006]***	[0.012]
Panel B: SC Effectiveness			
Factory safety spotcheck index	-0.000	0.217	
		[0.018]**	
CAP completion sub-variable	0.314	0.109	0.025
		[0.574]	[0.778]
		{1.000}	{0.904}
Worker SC awareness sub-index	0.073	0.077	0.205
		[0.624]	[0.229]
		{1.000}	{0.263}
Worker safety knowledge sub-index	0.365	-0.058	-0.054
		[0.552]	[0.467]
		{1.000}	{0.713}
Senior manager awareness sub-variable	0.102	0.077	0.054
		[0.740]	[0.823]
		{1.000}	{0.904}
Panel C: Worker Job Satisfaction and Mental Well-being (well-being index)			
Job satisfaction sub-index	-0.156	-0.397	-0.390
		[0.019]***	[0.023]**
		{0.049}**	{0.047}**
Mental well-being sub-index	0.040	-0.057	-0.050
		[0.727]	[0.687]
		{0.364}	{0.493}
Turnover sub-variable	0.126	0.072	-0.011
		[0.573]	[0.886]
		{0.364}	{0.493}
Absenteeism sub-variable	0.088	0.025	-0.084
		[0.875]	[0.183]
		{0.364}	{0.493}

Notes: This table reports OLS estimates of treatment effects on primary outcome sub-indexes and sub-variables. Each panel reports the sub-index/sub-variable results for a different outcome variable. Sub-indexes and sub-variables are listed on the left. In all cases, higher values of the index correspond to "positive" outcomes. 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. Column (3) 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. Randomization inference (RI) p-values based on 5000 draws are reported in square brackets. Within-family False Discovery Rate (FDR)-adjusted p-values in curly brackets. Index variables constructed using Anderson 2008 variance-covariance weighted index. *p<0.1; **p<0.05; ***p<0.01.

Table B4: Baseline balance tests within subgroups for heterogeneity analysis, primary outcome variables (dropping outlier)

	(1) Control mean	(2) T-C diff	(3) RI *p*-value	(4) N
Panel A: SC Compliance				
Abv_Med_Compli=0	-0.198	-0.122	0.111	40
Abv_Med_Compli=1	0.208	0.044	0.342	39
Abv_Med_Size=0	0.009	-0.002	0.988	40
Abv_Med_Size=1	-0.007	-0100	0.381	39
Abv_Med_MGMT=0	0.044	0.033	0.763	39
Abv_Med_MGMT=1	-0.034	0.116	0.292	40
Abv_Med_MGMT(2)=0	-0.044	0.023	0.827	39
Abv_Med_MGMT(2)=1	0.038	-0.097	0.378	40
EPZ=0	0.037	-0.055	0.497	65
EPZ=1	-0.179	0.362	0.182	14
Panel B: SC Effectiveness				
Abv_Med_Compli=0	-0.028	-0.071	0.666	40
Abv_Med_Compli=1	0.034	0.198	0.322	39
Abv_Med_Size=0	0.133	-0.040	0.815	40
Abv_Med_Size=1	-0.090	-0.030	0.849	39
Abv_Med_MGMT=0	0.087	-0.113	0.517	39
Abv_Med_MGMT=1	-0.064	0.095	0.535	40
Abv_Med_MGMT(2)=0	-0.228	0.105	0.546	39
Abv_Med_MGMT(2)=1	0.201	-0.106	0.473	40
EPZ=0	0.014	-0.042	0.730	65
EPZ=1	-0.053	0.218	0.591	14
Panel C: Worker job satisfaction and mental well-being				
Abv_Med_Compli=0	-0.054	0.020	0.874	40
Abv_Med_Compli=1	0.056	-0.066	0.640	39
Abv_Med_Size=0	0.012	-0.039	0.806	40
Abv_Med_Size=1	-0.009	0.010	0.926	39
Abv_Med_MGMT=0	0.019	0.101	0.523	39
Abv_Med_MGMT=1	-0.015	0.007	0.954	40
Abv_Med_MGMT(2)=0	-0.043	-0.134	0.351	39
Abv_Med_MGMT(2)=1	0.037	0.037	0.976	40
EPZ=0	0.023	-0.105	0.291	66
EPZ=1	-0.111	0.511	0.104	14

Note: This table reports OLS estimates of baseline differences between control and treatment groups within each pre-specified subgroup for treatment effect heterogeneity analysis. For the first three dimensions of heterogeneity, compliance, size, and managerial practices, I partition the sample into above/below median subgroups using the baseline value of the variable. For the final dimension of heterogeneity, location in Export Processing Zone (EPZ), I partition the sample using this variable. Each panel reports the within subgroup baseline differences for a different outcome variable. For each outcome, within subgroup, 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 outcome on the treatment indicator and stratification variables within that subgroup. In column (3), I report the randomization inference (RI) p-value for the coefficient reported in column (2) based on 5000 draws. In column (4), I report the number of observations in that subgroup. *p<0.1; **p<0.05; ***p<0.01.

Table B5: Heterogeneous treatment effects: Primary Outcome Index Variables (dropping outlier)

	Compliance		Size		Mgmt (Prod)		Mgmt (HR)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: *Compliance Index*								
Below median	0.177	0.211	0.206	0.201	0.142	0.124	0.167	0.144
	[0.103]	[0.052]*	[0.048]**	[0.030]**	[0.197]	[0.201]	[0.130]	[0.120]
Above median	0.174	0.159	0.126	0.164	0.215	0.273	0.200	0.243
	[0.011]**	[0.018]**	[0.203]	[0.068]*	[0.034]**	[0.002]***	[0.042]**	[0.011]**
p-val, diff between rows 1 & 2	[0.969]	[0.705]	[0.592]	[0.776]	[0.617]	[0.268]	[0.827]	[0.481]
Panel B: *Effectiveness Index*								
Below median	0.176	0.189	0.076	0.082	-0.007	0.005	0.104	0.083
	[0.172]	[0.146]	[0.375]	[0.324]	[0.934]	[0.956]	[0.354]	[0.469]
Above median	0.097	0.083	0.170	0.171	0.279	0.267	0.179	0.193
	[0.180]	[0.246]	[0.180]	[0.184]	[0.019]**	[0.033]**	[0.112]	[0.080]*
p-val, diff between rows 1 & 2	[0.595]	[0.479]	[0.541]	[0.566]	[0.055]*	[0.081]*	[0.637]	[0.490]
Panel C: *Worker Job Satisfaction and Mental Well-being Index*								
Below median	-0.327	-0.327	-0.240	-0.234	-0.218	-0.210	-0.187	-0.181
	[0.016]**	[0.019]**	[0.070]*	[0.080]*	[0.106]	[0.130]	[0.133]	[0.154]
Above median	0.028	0.038	-0.065	-0.063	-0.088	-0.089	-0.101	-0.102
	[0.754]	[0.672]	[0.500]	[0.521]	[0.377]	[0.373]	[0.330]	[0.331]
p-val, diff between rows 1 & 2	[0.033]**	[0.031]**	[0.292]	[0.311]	[0.454]	[0.489]	[0.601]	[0.633]
Observations	79	79	79	79	79	79	79	79
Stratification variables	Y	Y	Y	Y	Y	Y	Y	Y
Control, base. dep. var.	N	Y	N	Y	N	Y	N	Y

Note: This table reports OLS estimates of heterogeneous treatment effects on primary outcome index variables. Each dimension of heterogeneity is indicated at the top of the table. Each panel reports the results for a different outcome variable. 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. For each dimension of heterogeneity, I estimate the treatment effects without and with a control for the baseline value of the dependent variable. All regressions include stratification variables. All subgroups have 40 observations. Randomization inference (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.

Appendix C

Measuring productivity

I make three assumptions in order to measure TFP[23]:

1. Production takes the form of a Cobb-Douglas production function:
$$Y_{it} = \omega_{it} K_{it}^{\beta_k} L_{it}^{\beta_l} M_{it}^{\beta_m} \tag{2}$$
where Y_{it} is physical output of factory i in month t; K_{it}, L_{it}, and M_{it} are inputs of capital, labor, and materials, respectively, and β_k, β_l, and β_m are their output elasticities, respectively. ω_{it} is the factor-neutral (Hicks neutral) shifter.
2. Constant returns to scale:
$$\beta_k + \beta_l + \beta_m = 1$$
3. Perfect competition in the output and input markets.

With these assumptions, I can construct output elasticities for labor, materials, and capital using the share of revenues paid to each input. Denote $\ddot{\beta}_k$, $\ddot{\beta}_l$, and $\ddot{\beta}_m$ the constructed shares. I calculate the output elasticities using cost shares for each factory's relevant industrial classification from Bangladesh's Survey of Manufacturing Industries (2012). Using the constant returns to scale assumption, I calculate capital cost share as $\ddot{\beta}_k = 1 - \ddot{\beta}_l - \ddot{\beta}_m$. This approach results in the following set of constructed shares:

Table C1: Constructed output elasticities by industry classification

Bangladesh Standard Industrial Classification (BSIC)	BSIC Name	Labor Share	Materials Share*	Capital Share
1312	Weaving of textiles (excluding handloom product)	0.068616368	0.714249744	0.217133888
1313	Finishing of textiles (dying, bleaching etc.)	0.107283141	0.668449677	0.224267181
1391	Manufacture of knitted and crocheted fabrics	0.184072524	0.538724705	0.277202771
1392	Manufacture of made-up textile articles, except apparel	0.108182738	0.715834195	0.175983067
1410	Manufacture of wearing apparel, except fur apparel	0.167689376	0.699004718	0.133305906
1430	Manufacture of knitted and crocheted apparel	0.183525689	0.685334685	0.131139626
1520	Manufacture of footwear	0.184997521	0.679515106	0.135487373
1702	Manufacture of corrugated paper, paperboard, and containers of paper and paperboard	0.080221329	0.730260773	0.189517898
1709	Manufacture of other articles of paper and paperboard	0.074541121	0.718162083	0.207296796
2220	Manufacture of plastics products	0.095538477	0.646711619	0.257749904

*Materials share includes all intermediate consumption categories, including: Raw, packing, spare supplies; Fuel; contractual and maintenance; and Overhead rent, interest paid. In practice, materials cost is the large majority of expenditures in this category. Materials share is only available at the 3-digit, not the 4-digit BSIC level.

I measure output and inputs in physical quantities:

- *Output:* Total output produced per month. Measured in physical units of output (e.g., pieces of clothing or kilograms of fabric).

[23]In the future, I will also show results estimating TFP using the approach of Ackerberg, Caves, and Frazer 2015.

- *Capital*†*:* Total number of machines at the factory in that month. For sewing and footwear factories, total number of production lines at the factory that month.

- *Labor:* Total person-hours per month. Man-hours are calculated as total number of worker employees times the average weekly working hours times 4 weeks per month plus total number of management-level employees times average weekly working hours for management staff times 4 weeks per month.

- *Materials*†*:* Total quantity of primary material inputs to a production activity in a month (e.g., kilograms of fabric).

Finally, I construct TFP as follows:

$$TFP = \hat{\omega}_{it} = log(y_{it}) - \ddot{\beta}_k \, log(k_{it}) - \ddot{\beta}_l \, log(l_{it}) - \ddot{\beta}_m \, log(m_{it}) \tag{3}$$

where $\hat{\omega}_{it}$ is log(TFP), which I use in my analysis.

†For machinery and materials, numerous factories use multiple types of machinery and/or material inputs. Currently, I calculate TFP for these factories in three ways:

1. Base TFP Measure: I take the log of the sums of machine and material inputs, respectively. For example, if a washing factory employs two types of machines, washers and dryers, I include them as $\ddot{\beta}_k \, log(k_{1it} + k_{2it})$, where k_{1it} is the number of washers at factory i in month t and k_{2it} is the number of dryers at factory i in month t. This approach assumes that different machines (materials) are perfect substitutes for each other, which is not true in practice.

2. TFP Measure 2: For material inputs, I use $\alpha * \ddot{\beta}_m \, log(m_{1it}) + (1 - \alpha) * \ddot{\beta}_m \, log(m_{2it})$. In the current analysis, $\alpha = 0.5$.

3. TFP Measure 3: For capital inputs, I use $\alpha * \ddot{\beta}_k \, log(k_{1it}) + (1 - \alpha) * \ddot{\beta}_k \, log(k_{2it})$. In the current analysis, $\alpha = 0.5$.

In the future, I will add robustness tests for $\alpha \in (0, 1)$.

Multi-product factories: Nearly 20% of the factories in the sample produce multiple types of products. For example, a factory may produce embroidered tee-shirts and printed tee-shirts, which use different production technologies. For multi-product factories, I have product-specific quantities of output, machines, and material inputs. I use factory-provided employee lists to determine the share of production workers allocated to each production type. I assume that non-production labor is also allocated according to these shares. With this information, I calculate factory-product TFP. As I am interested in factory-level TFP, I need to aggregate TFP across production activities. I lack data on product-specific revenues and costs that I would ideally use to generate product weights. Currently, I assume that labor is perfectly substitutable across products and weigh each product's contribution to TFP by its share of total labor employed at the factory. Factory TFP is the weighted sum of product-specific TFP, where product weights are factory labor-shares. I report results for both factory-level TFP and factory-product level TFP.

Vertically-integrated factories: There are a small number of vertically-integrated factories in the sample. For example, a sweater factory may first knit yarn into material and then sew the material into sweaters. For these factories, I follow Domar 1961 and Brandt et al. 2018 in calculating factory-level TFP. For example, suppose a factory has a two-stage log production system:

$$\begin{cases} y_{1it} = & \omega_{1it} + \beta_{1k}\, k_{1it} + \beta_{1l}\, l_{1it} + \beta_{1m}\, m_{1it} \\ y_{2it} = & \omega_{2it} + \beta_{2k}\, k_{2it} + \beta_{2l}\, l_{2it} + \beta_{2m}\, m_{2it} \end{cases} \tag{4}$$

where the subscripts 1 and 2 denote the stage of production, and

$$y_{1it} = m_{2it}$$

I combine the production across all stages to generate an aggregate log production function:

$$y_{2it} = \omega_{it} + \beta_{2k}\, k_{2it} + \beta_{2m}\, \beta_{1k}\, k_{1it} + \beta_{2l}\, l_{2it} + \beta_{2m}\, \beta_{1l}\, l_{1it} + \beta_{2m}\, m_{2it} + \beta_{2m}\, \beta_{1m}\, m_{1it} \tag{5}$$

I construct input-specific elasticities using the products of the relevant elasticities from Table C1. I then calculate TFP as in equation 3 above.

Appendix D

Table D1: Local Average Treatment Effects: Primary Outcome Index Variables

	Control mean	Local Average Treatment Effect	
	(1)	(2)	(3)
SC Compliance	0.053	0.208***	0.214***
		(0.070)	(0.061)
SC Effectiveness	0.103	0.157**	0.159**
		(0.072)	(0.071)
Worker job satisfaction & mental well-being	-0.013	-0.176**	-0.166**
		(0.082)	(0.082)
Observations		80	80
Stratification variables		Y	Y
Control, base. dep. var.		N	Y

Notes: This table reports two stage least squares (2SLS) estimates of treatment effects on primary outcome index variables. Outcome variables are listed on the left. In all cases, higher values of the index correspond to "positive" outcomes. 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. Column (3) 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 parentheses. Index variables constructed using Anderson 2008 variance-covariance weighted index. *p<0.1; **p<0.05; ***p<0.01.

Appendix E

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Dependent Variable = Measures of conditions leaving out worker's own reports						
Migrant	-0.3111***	-0.3295***	-0.3028*	-0.3053*	-0.1563	-0.1563
	[0.082]	[0.083]	[0.153]	[0.158]	[0.118]	[0.118]
Male		-0.1426**		0.0116		0.029
		[0.069]		[0.101]		[0.092]
Education		0.0295***		0.007		0.0037
		[0.010]		[0.013]		[0.012]
Experience		0.0099		-0.0045		-0.0046
		[0.009]		[0.018]		[0.015]
Observations	39,852	39,788	816	815	816	815
R-squared	0.013	0.025	0.008	0.008	0.153	0.154
Panel B: Dependent Variable = Measures of conditions leaving out reports from current factory						
Migrant	-0.3463***	-0.3778***	-0.3012**	-0.3058**	-0.0872	-0.0957
	[0.083]	[0.092]	[0.119]	[0.115]	[0.138]	[0.132]
Male		0.0041		0.1094		0.1355**
		[0.094]		[0.069]		[0.062]
Education		0.0204		0.006		0.0045
		[0.014]		[0.011]		[0.009]
Experience		-0.0255		-0.0152		-0.0131
		[0.023]		[0.014]		[0.013]
Observations	43,018	42,954	715	714	715	714
R-squared	0.015	0.027	0.012	0.02	0.148	0.157
Panel C: Dependent Variable = Measure of conditions not weighted by tenure						
Migrant	-0.3382***	-0.3625***	-0.2160**	-0.2262**	-0.2286***	-0.2360***
	[0.077]	[0.089]	[0.092]	[0.096]	[0.073]	[0.075]
Male		-0.1375		0.007		0.0327
		[0.092]		[0.094]		[0.069]
Education		0.0290*		0.0112		0.0068
		[0.016]		[0.009]		[0.008]
Experience		-0.0122		0.0073		0.0052
		[0.022]		[0.011]		[0.009]
Observations	50,180	50,114	990	987	990	987
R-squared	0.015	0.026	0.01	0.017	0.224	0.231

*Notes: Migrant = 1 if the individual is was not born in Gazipur or Dhaka districts, as described in section 2.1. "Past observations" refer to any month in which they worker has been in the garment industry since she began working, constructed using the retrospective panel structure of the data, as described in section 2.1. In columns 1 and 2, standard errors clustered at the level of the individual. In columns 3-6, standard errors clustered at the level of the village *** p<0.01, ** p<0.05, * p<0.1.*

Table E2: Alternate constructions of the working conditions measure

	Dependent Variable = 1(Factory Still Operating Under Same Management in 2014)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Index of Working Conditions	0.0158	0.0342**					0.0158	0.0347*
	[0.013]	[0.017]					[0.013]	[0.018]
Factory FE from Wage Equation			0.0258	0.036			0.0233	0.0292
			[0.038]	[0.051]			[0.038]	[0.051]
Fraction Migrants					0.1197**	0.0422	0.1217*	0.0747
					[0.060]	[0.063]	[0.064]	[0.067]
Weighted by Number of Obs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	896	896	812	812	882	882	812	812
R-squared	0.002	0.005	0.001	0.000	0.005	0.001	0.006	0.006

*Notes: Unit of observation is the factory. *** p<0.01, ** p<0.05, * p<0.1.*

Table E1: Correlates of Factory Survival

Outcomes listed below:	(1)	(2)	(3)	(4)	(5)	(6)
Problem:	0.0356	0.0337	0.0349**	0.0338**	0.0172	0.0095
Hours too long	[0.030]	[0.029]	[0.017]	[0.017]	[0.022]	[0.024]
Problem:	0.0337***	0.0366***	0.0075	0.006	0.0162	0.0124
Abusive management	[0.009]	[0.011]	[0.010]	[0.011]	[0.022]	[0.021]
Problem:	0.0090***	0.0088***	-0.0065	-0.0078	-0.0127	-0.0144
Bad/unsafe working conditions	[0.003]	[0.003]	[0.009]	[0.009]	[0.011]	[0.012]
Problem:	-0.0318	-0.0324	-0.0331	-0.0339	-0.0189	-0.0205
Not paid on time	[0.033]	[0.032]	[0.026]	[0.026]	[0.025]	[0.024]
Problem:	0.0067	0.007	0.0028	0.0031	0.0009	-0.0006
Unpaid overtime	[0.013]	[0.013]	[0.011]	[0.012]	[0.010]	[0.010]
Problem:	0.0136**	0.0128**	0.0058*	0.0051*	0.0058*	0.0057
Fired for sickness	[0.006]	[0.006]	[0.003]	[0.003]	[0.003]	[0.003]
Problem:	-0.0007	-0.0016	-0.0236*	-0.0239	-0.0201	-0.0197
Other	[0.008]	[0.008]	[0.014]	[0.014]	[0.014]	[0.015]
Appointment letter	-0.092	-0.0996*	-0.0917	-0.0763	-0.0505	-0.015
	[0.056]	[0.056]	[0.056]	[0.055]	[0.057]	[0.063]
Medical Care	-0.1626***	-0.1740***	-0.0205	-0.036	0.0019	-0.0054
	[0.038]	[0.040]	[0.063]	[0.062]	[0.054]	[0.059]
Relationship with management	-0.4528	-0.4394	-0.4139	-0.3998	-0.4768**	-0.4106
	[0.282]	[0.282]	[0.257]	[0.288]	[0.242]	[0.264]
Past observations	Yes	Yes	No	No	No	No
Village fixed effects	No	No	No	No	Yes	Yes
Controls for sex, education, experience	No	Yes	No	Yes	No	Yes

*Notes: Each cell is the coefficient on Migrant from a separate regression. Regressions for problems, appointment letter, and medical care are OLS and Relationship management (on a five point scale; where 1 = ver y bad; 2 = bad; 3 = okay; 4 = good; 5 = very good) is an ordered logit. Migrant = 1 if the individual is was not born in Gazipur or Dhaka districts, as described in section 2.1. "Past observations" refer to any month in which they worker has been in the garment industry since she began working, constructed using the retrospective panel structure of the data, as described in section 2.1. In columns 1 and 2, standard errors clustered at the level of the individual. In columns 3-6, standard errors clustered at the level of the village *** p<0.01, ** p<0.05, * p<0.1.*

Table E3: Individual Measures of Working Conditions

	Dependent Variable = Person-level index of working conditions					
	(1)	(2)	(3)	(4)	(5)	(6)
Migrant	-0.1224	-0.1116	0.0150	0.0339	-0.0622	-0.0343
	[0.093]	[0.093]	[0.136]	[0.136]	[0.196]	[0.193]
Male		0.0354		0.0425		0.0445
		[0.068]		[0.106]		[0.117]
Education (Years)		0.0084		0.0271***		0.0239**
		[0.009]		[0.010]		[0.011]
Experience (Years)		0.0205***		0.0228**		0.0249**
		[0.006]		[0.010]		[0.011]
Factory FE	Yes	Yes	Yes	Yes	Yes	Yes
Past observations	Yes	Yes	No	No	No	No
Village fixed effects	No	No	No	No	Yes	Yes
Observations	49,206	49,140	977	974	977	974
R-squared	0.598	0.602	0.409	0.422	0.439	0.45

*Notes: The index of working conditions is described in section 2.4; in this analysis, it is standardized to have mean 0 and standard deviation 1 across all workers. Migrant = 1 if the individual is was not born in Gazipur or Dhaka districts, as described in section 2.1. "Past observations" refer to any month in which they worker has been in the garment industry since she began working, constructed using the retrospective panel structure of the data, as described in section 2.1. In column 1, standard errors clustered at the level of the individual. In columns 2-3, standard errors clustered at the level of the village. *** p<0.01, ** p<0.05, * p<0.1.*

Table E4: Within-factory variation in the working conditions measure

Dependent Variable = Index of working conditions ($\hat{c}_{ift}$)						
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Migrant=1 if individual is from outside of current village						
Migrant	-0.3173***	-0.3508***	-0.1807	-0.1973	-0.1708**	-0.1837***
	[0.104]	[0.115]	[0.124]	[0.128]	[0.063]	[0.064]
Male		-0.125		0.0312		0.0491
		[0.104]		[0.068]		[0.061]
Education		0.0317**		0.011		0.0097
		[0.016]		[0.008]		[0.008]
Experience		-0.0041		0.0097		0.0095
		[0.022]		[0.008]		[0.007]
Past observations	Yes	Yes	No	No	No	No
Village fixed effects	No	No	No	No	Yes	Yes
Observations	50,180	50,114	990	987	990	987
R-squared	0.008	0.02	0.004	0.014	0.184	0.195
Panel B: Migrant=1 if individual moved to village after age 10						
Migrant	-0.2513***	-0.2820***	-0.1568	-0.1780*	-0.0982	-0.1178**
	[0.089]	[0.096]	[0.102]	[0.101]	[0.059]	[0.058]
Male		-0.1183		0.0279		0.049
		[0.104]		[0.067]		[0.061]
Education		0.0319**		0.0119		0.0103
		[0.016]		[0.008]		[0.008]
Experience		-0.0034		0.0106		0.0103
		[0.022]		[0.008]		[0.007]
Past observations	Yes	Yes	No	No	No	No
Village fixed effects	No	No	No	No	Yes	Yes
Observations	50,180	50,114	990	987	990	987
R-squared	0.007	0.018	0.005	0.015	0.184	0.195

*Notes: In Panel A, Migrant = 1 if the individual was not born in the village where they reside at the time of survey. In Panel B, Migrant = 1 if the individual moved to the village after the age of 10. "Past observations" refer to any month in which they worker has been in the garment industry since she began working, constructed using the retrospective panel structure of the data, as described in section 2.1. In columns 1 and 2, standard errors clustered at the level of the individual. In columns 3-6, standard errors clustered at the level of the village *** p<0.01, ** p<0.05, * p<0.1.*

Table E5: Robustness of Main Table 4 results to alternative definition of migrant variable

	Dependent Variable = Index of working conditions ($\hat{c}_{ift}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
Referred	0.0663	0.1021	0.0277	0.0487	0.0773	0.0958*
	[0.073]	[0.073]	[0.046]	[0.049]	[0.049]	[0.048]
Migrant	-0.3113***	-0.3266***	-0.1773*	-0.1698	-0.1341*	-0.1098
	[0.080]	[0.089]	[0.105]	[0.105]	[0.069]	[0.066]
Male		-0.164		0.0302		0.0574
		[0.107]		[0.070]		[0.060]
Education (Years)		0.0398**		0.0199**		0.0164*
		[0.016]		[0.009]		[0.009]
Experience (Years)		0.0014		0.0198***		0.0182***
		[0.021]		[0.006]		[0.005]
Past observations	Yes	Yes	No	No	No	No
Village fixed effect	No	No	No	No	Yes	Yes
Observations	49,206	49,140	977	974	977	974
R-squared	0.015	0.033	0.008	0.035	0.208	0.232

*Notes: The index of working conditions is described in section 2.4; it is standardized to have mean 0 and standard deviation 1. Migrant = 1 if the individual is was not born in Gazipur or Dhaka districts, as described in section 2.1. "Past observations" refer to any month in which they worker has been in the garment industry since she began working, constructed using the retrospective panel structure of the data, as described in section 2.1. In columns 1 and 2, standard errors clustered at the level of the individual. In columns 3-6, standard errors clustered at the level of the village. *** p<0.01, ** p<0.05, * p<0.1.*

Table E6: Referrals, Migration Status, and Working Conditions

	Dependent Variable:					
	1(Local works in factory)				Number of locals in factory	
	Linear Probability Model		Logit	Conditional Logit	Poisson IRR	
	(1)	(2)	(3)	(4)	(5)	(6)
Experience	0.0018	0.0067	0.0019	0.0196*	1.0204***	1.0219
	[0.0038]	[0.0041]	[0.0038]	[0.0115]	[0.0024]	[0.0151]
Male	-0.1413***		-0.1407***		0.6003***	
	[0.0390]		[0.0382]		[0.0135]	
Education (Years)	0.0111**		0.0111**		1.0155***	
	[0.0052]		[0.0053]		[0.0033]	
Worker fixed effects	No	Yes	No	Yes	No	Yes
Observations	42,245	42,245	42,245	17,397	42,245	24,619

*Notes: Regression only includes migrant workers. Standard errors clustered at the level of the individual. *** p<0.01, ** p<0.05, * p<0.1.*

Table E7: Migrants' experience and probability that they work at factory that employs 1 or more local workers

Dependent Variable = 1(Reasons for leaving include working conditions)			
	(1)	(2)	(3)
Experience (Years)	0.0012	0.0239	0.1005
	[0.0045]	[0.0162]	[0.1605]
Migrant	0.0587	0.0693	
	[0.0507]	[0.0570]	
Migrant X Experience		-0.0039	-0.1012
		[0.0136]	[0.1602]
Male	-0.0059	0.0159	
	[0.0238]	[0.0311]	
Male X Experience		-0.0079	0.0034
		[0.0077]	[0.0383]
Education (Years)	-0.0036	0.0024	
	[0.0034]	[0.0043]	
Education X Experience		-0.0020*	0.0015
		[0.0011]	[0.0077]
Tenure (Years)	-0.0154*	-0.0182*	-0.0205
	[0.0093]	[0.0093]	[0.0292]
Worker fixed effects	No	No	Yes
Observations	1,254	1,254	314

*Notes: The dependent variable is coded as =1 if worker reports working conditions among reasons for leaving a factory. Two working conditions-related reason categories are "bad relationship with management" and "late payment." Migrant = 1 if the individual is was not born in Gazipur or Dhaka districts, as described in section 2.1. Standard errors clustered at the level of the individual. *** p<0.01, ** p<0.05, * p<0.1.*

Table E8: Reasons for leaving factory include working conditions

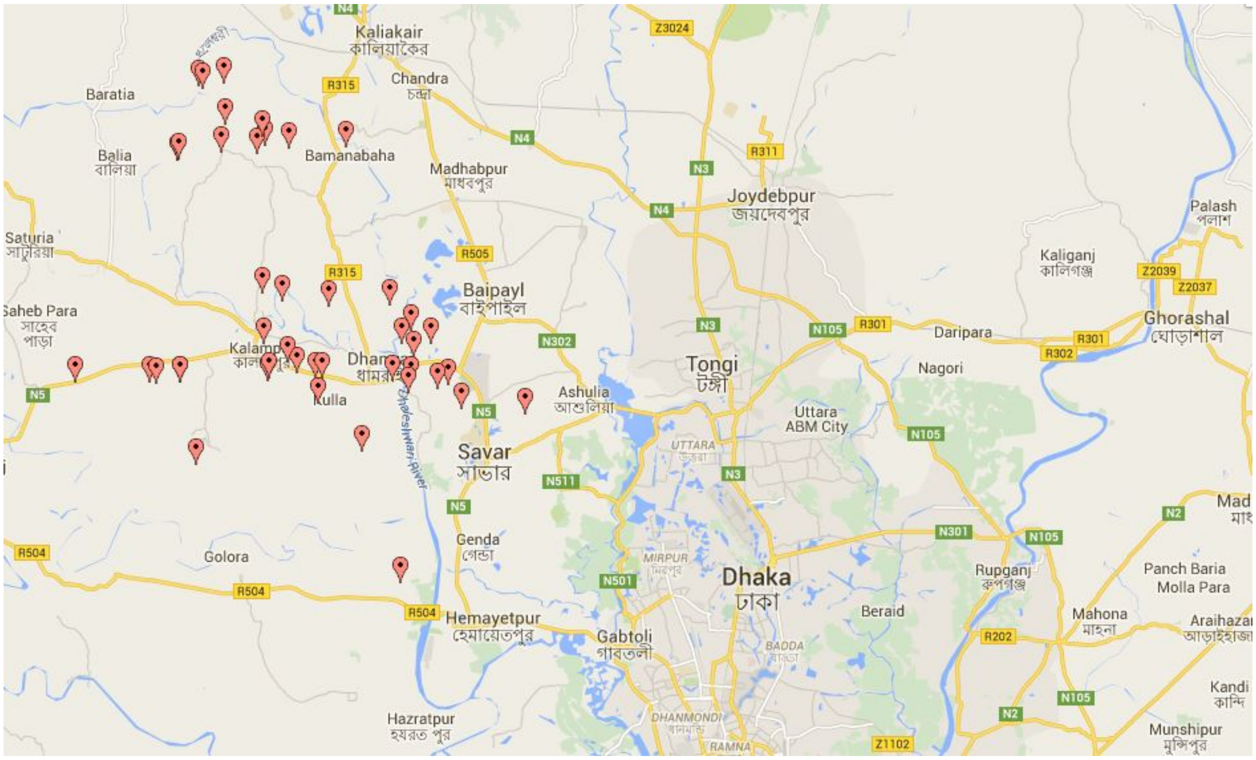


Figure E1: Sample villages

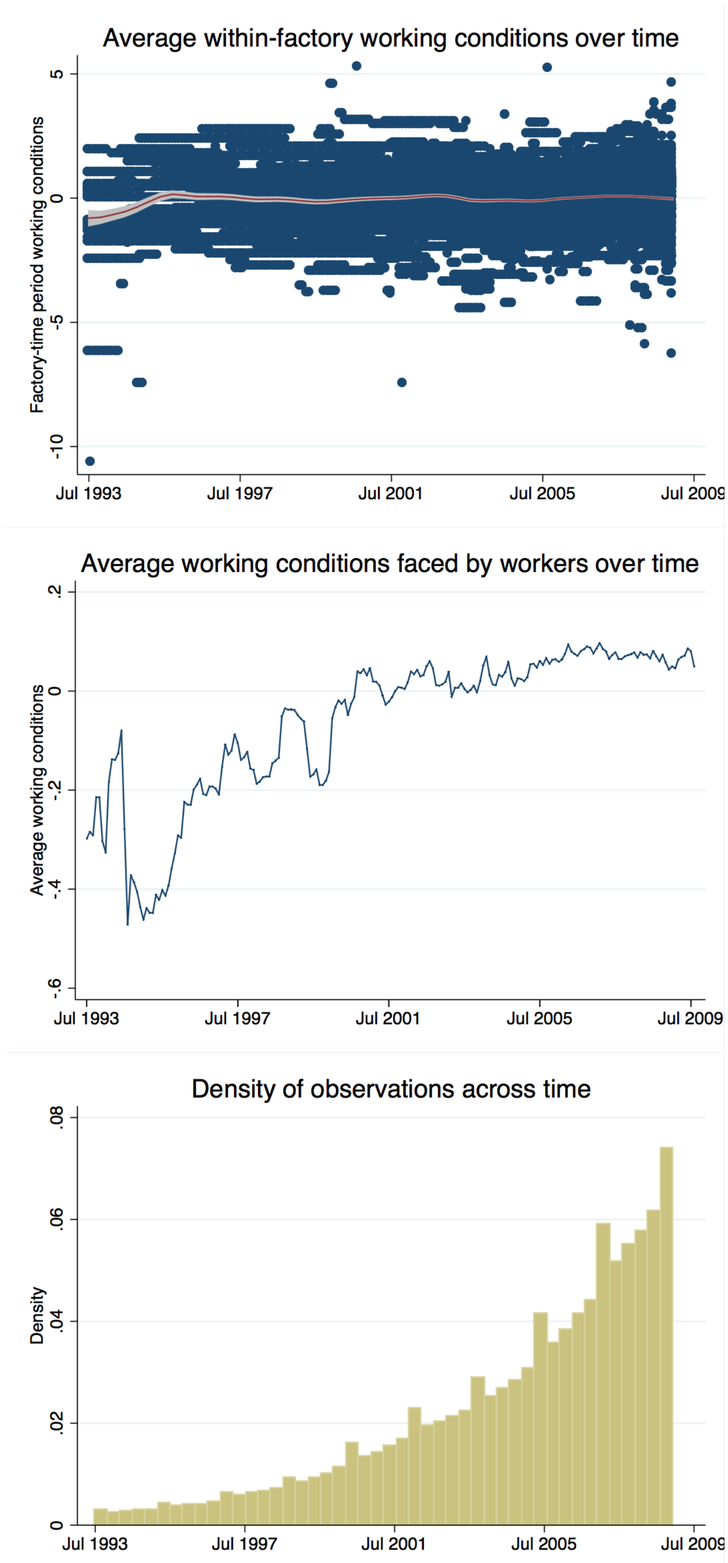


Figure E2: Factory- level working conditions over time

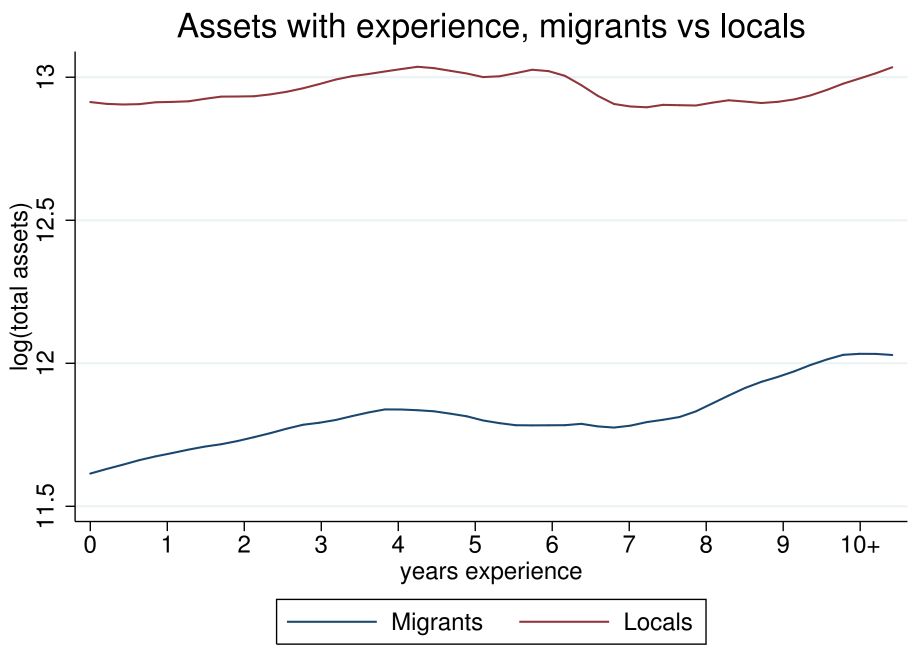


Figure E3: Assets with experience

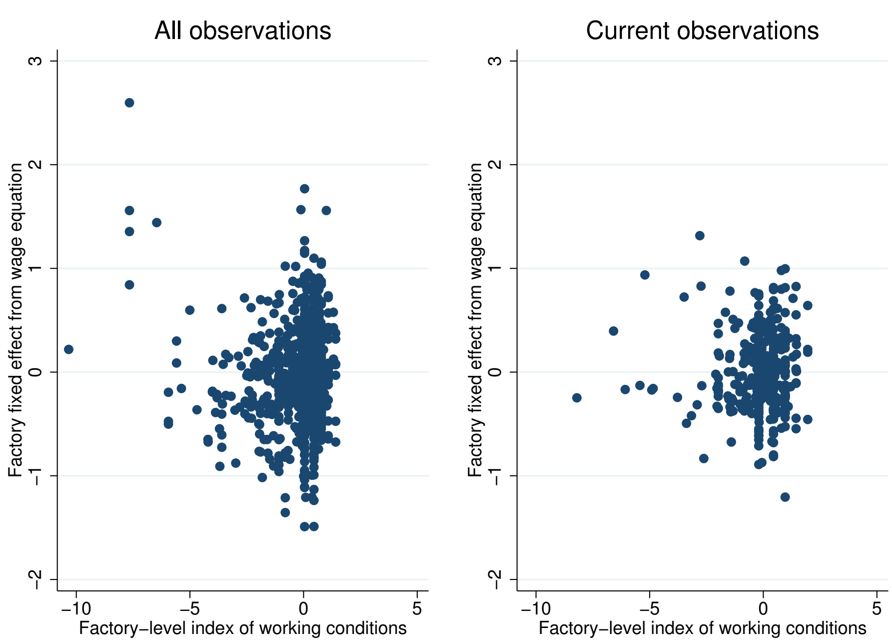


Figure E4: Factory- level working conditions versus factory-level wages

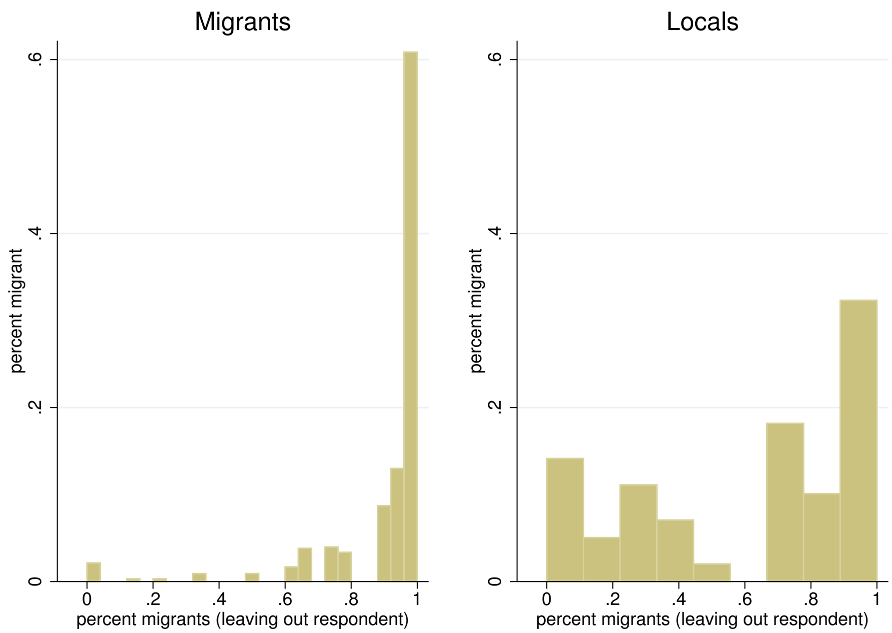


Figure E5: Distribution of other workers in factory, migrants versus local workers

Appendix F

Following the 2013 Rana Plaza collapse, the Bangladesh Accord on Fire and Building Safety (the Accord) and the Alliance for Bangladesh Worker Safety (the Alliance) developed harmonized building safety standards for garment factories in Bangladesh. The building safety standards include requirements for structural, fire, and electrical building safety. The coalitions' building safety standards are largely based on the 2006 Bangladesh National Building Code (BNBC), although in some cases the standards exceed the standards set out by the BNBC (The Alliance for Bangladesh Worker Safety 2014).

In 2013, both initiatives began conducting building safety audits of factories in their supplier bases. Both initiatives make the audits results publicly available on their websites. The Alliance's audits report factories' compliance with a standard set of requirements, which allows us to calculate overall compliance levels for factories audited by the Alliance. Figure F1 displays the distribution of building safety compliance levels for 279 garment factories audited by the Alliance that are located within commuting distance of workers in our sample.

Mean building safety compliance for Alliance-audited factories in this area was 63%, with a standard deviation of 7.4%. The lowest performing factory complied with 46% of the standards, and the highest performing factory complied with 86% of the standards.

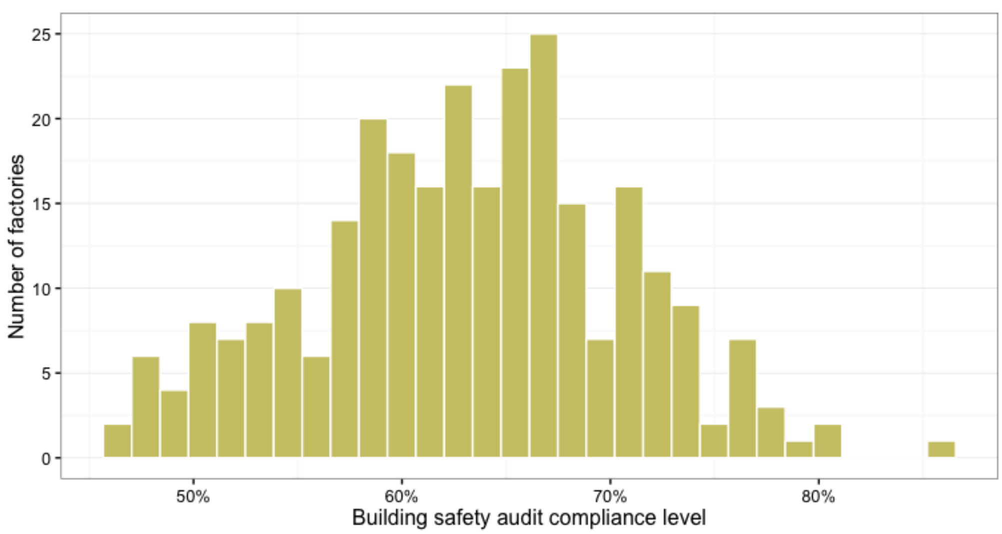


Figure F1: Distribution of building safety compliance of exporting factories in study area

Appendix G

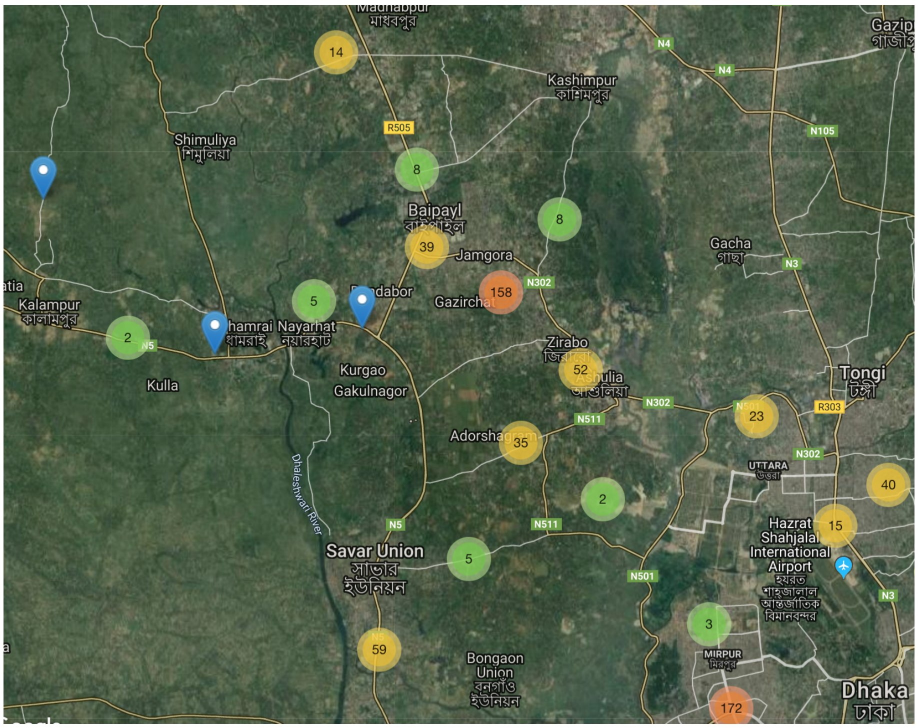
Figures

Figure G1: Rana Plaza factory complex collapse

Figure G2: Alliance building safety audit excerpt

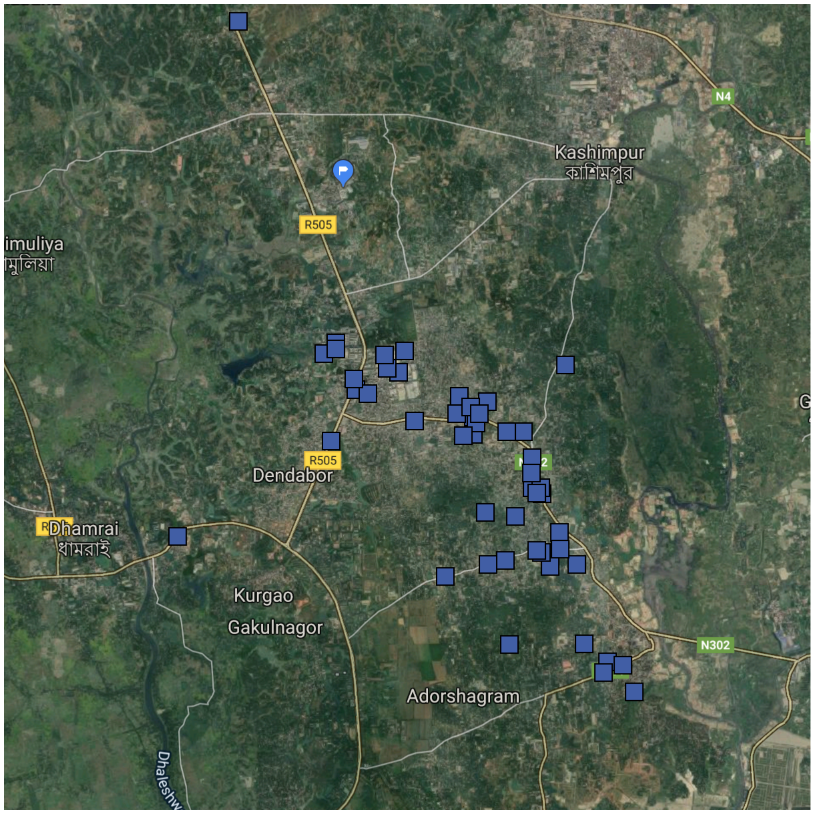
Structural System Design		
Question:	Has evidence of structural integrity been provided using a Preliminary Structural Assessment?	
Priority Level:	High	
Non-Compliance Level:	3	
Description:	There is lack of structural integrity because as per alliance standard section 8.3 the following issues are remarkable in this case: 1. The existing structural documentations are not credible as per BNBC part 6 section 1.9 and as built documents as per alliance standard section 8.20. 2. There are evidence of cracking and beam-column joint repair in few places. (see attached photo) 3. There is wide variation from approved drawing to actual such missing one column in one direction. 4. Floor loading is above 42 psf in 1st floor finished goods areas. 5. Ground floor condition and foundation could not be checked due to lack of lighting or darkness in ground floor.	
Source of Findings:	Photograph: see attached photo	
Suggested Plan of Action:	Ensure structural integrity following alliance standard through detail investigation with help of NDT/SDT by QSEC.	
Suggested Deadline Date:	31 May 2014	
Standard:	Reference Alliance Standards Part 8 Section 8.2 Structural Integrity of Existing Factory Buildings. If a previous assessment has been completed, provide a summary of findings from the report.	

Figure G3: Savar-Ashulia and environs; Numbers represent clusters of factories operational in 2019



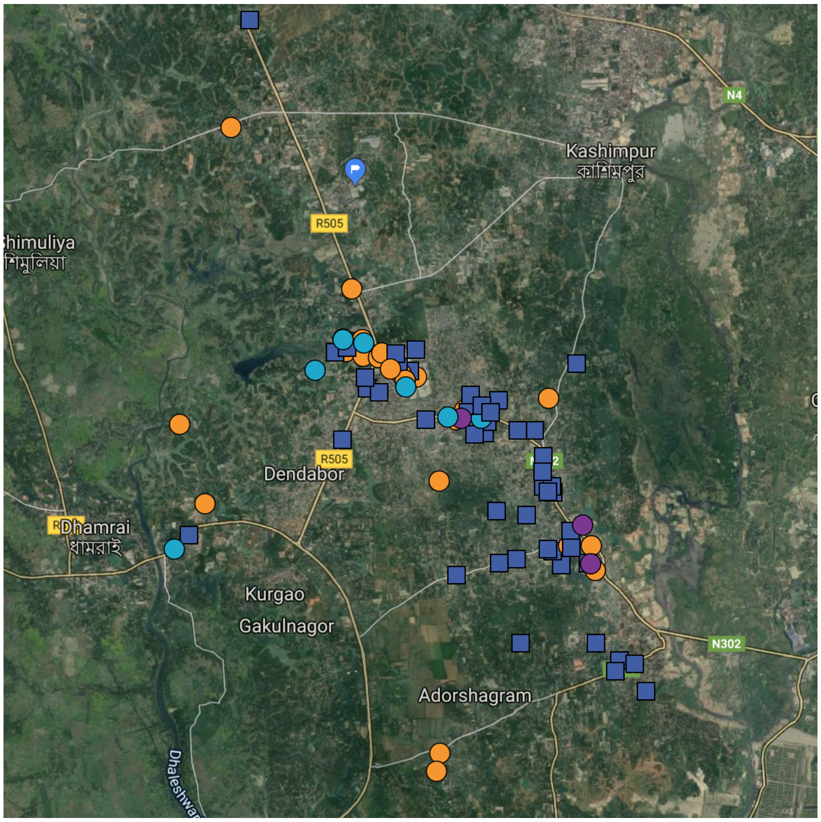
Source: Mapped in Bangladesh (Retrieved April 2019). Map data from Google Maps.

Figure G4: Factories with Alliance building safety audits publicly posted in July 2015



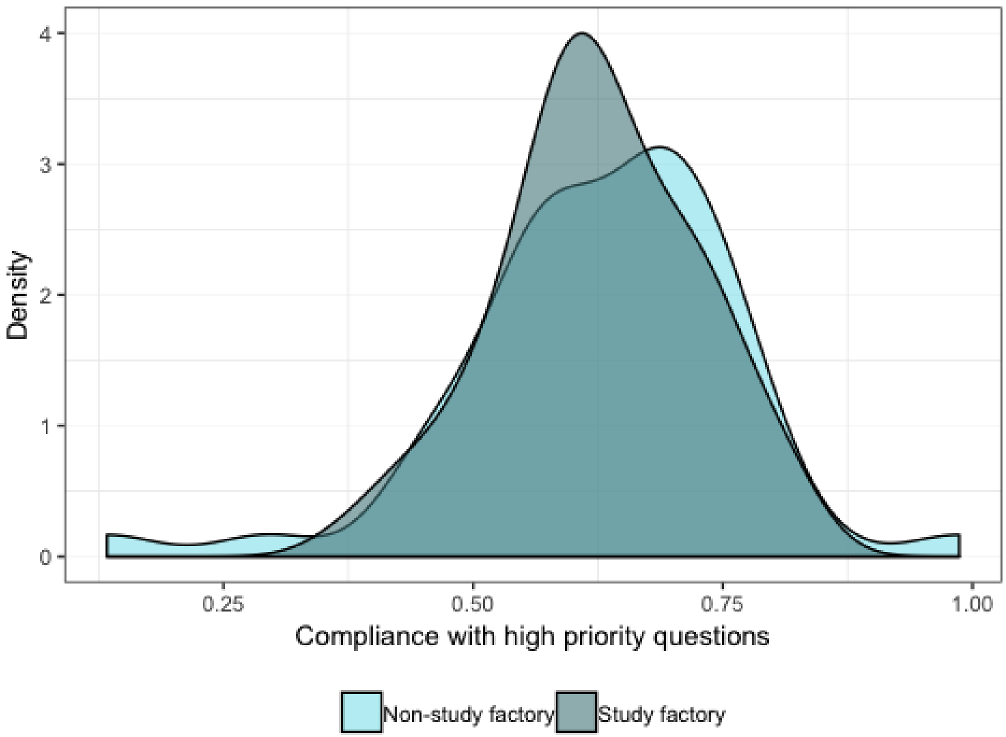
Note: In cases where multiple factories share the same location, they are represented by one point.

Figure G5: All Alliance and Accord factories located in July 2015



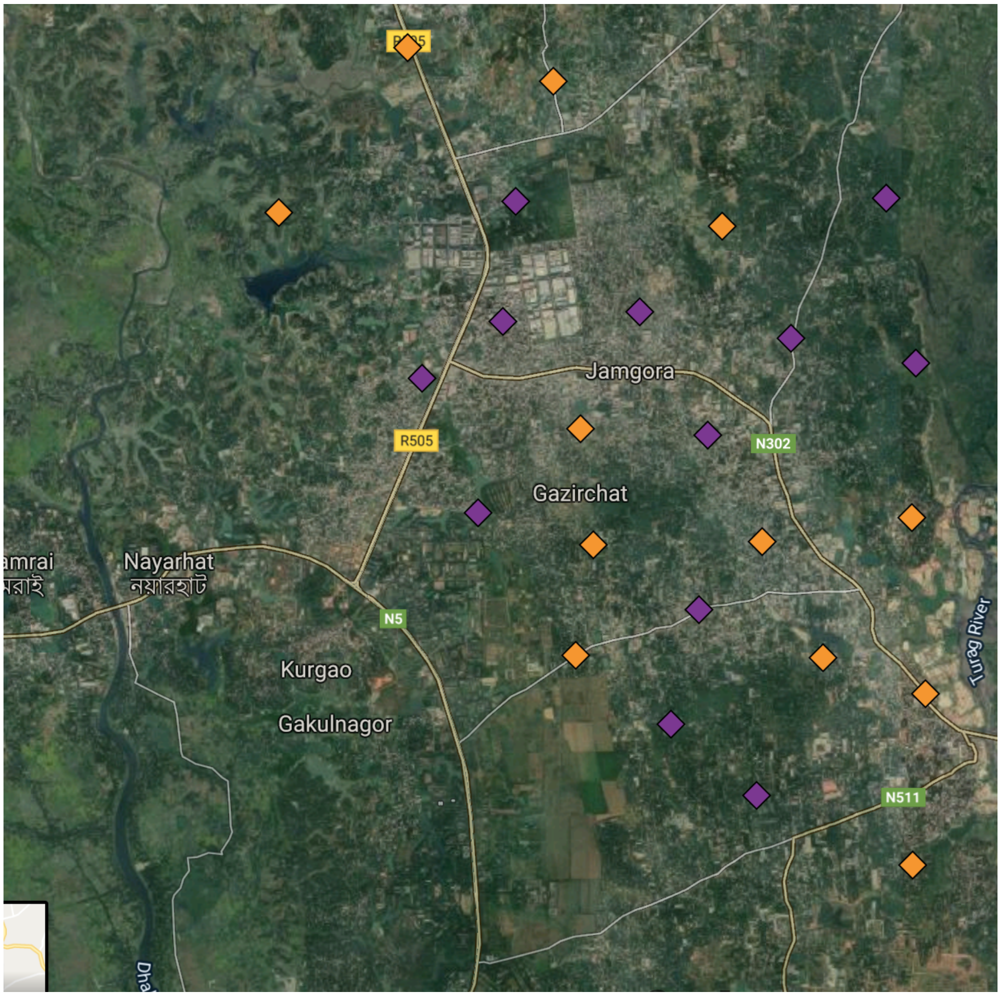
Notes: Blue squares represent study factories. Purple circles represent Alliance factories without publicly-posted building safety audits. Light blue circles represent factories that supply to Alliance members and to Accord signatories but for which a Alliance audit was not available. Orange circles represent factories that supply to Accord signatories but not to Alliance members. In cases where multiple factories share the same location, they are represented by one point.

Figure G6: Distribution of compliance with high priority questions for non-study and study Alliance-audited factories in Savar-Ashulia



Notes: The figure plots the distribution of performance on Alliance building safety audits for non-study and study factories in the Savar-Ashulia area. The non-study sample of factories includes all factories that appear in the Alliance's supplier base between 2013-2015, that are located in the Savar-Ashulia area, and that have a CAP. This sample includes 49 factories. The study sample of factories includes 62 out of 71 factories for which I could locate a CAP.

Figure G7: Neighborhood locations in Savar-Ashulia, Dhaka, Bangladesh; Treatment neighborhoods in purple and control in yellow.



Source: Map data from Google Maps.

Figure G8: Right-hand sampling rule and characteristic neighborhood and dwelling in Savar-Ashulia

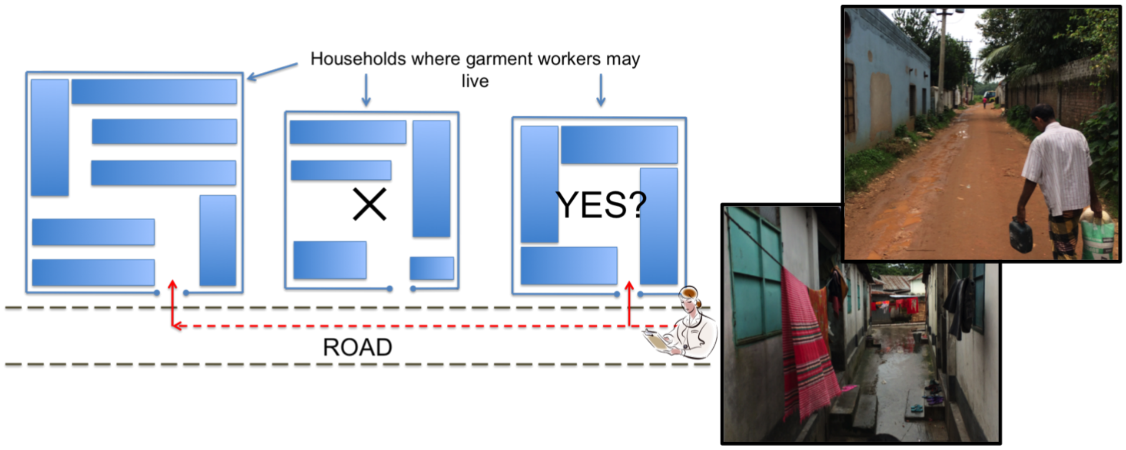


Figure G9: Timeline of interaction with study participants

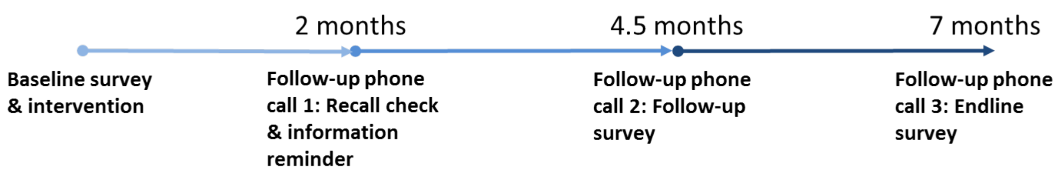
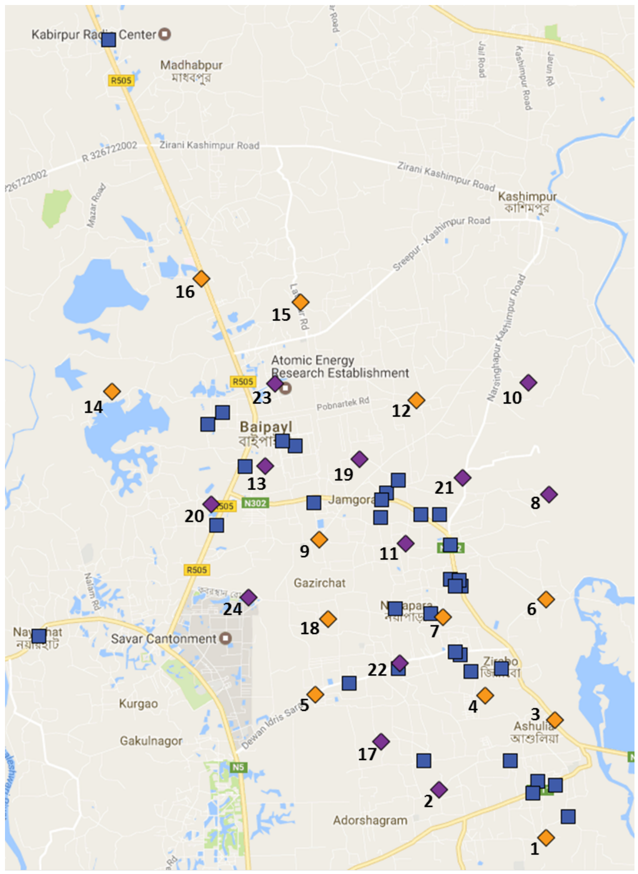


Figure G10: Sample factory and neighborhood locations in Savar-Ashulia, Dhaka, Bangladesh; Treatment neighborhoods in purple and control in yellow.



Source: Map data from Google Maps.

Figure G11: Sample factories' building safety audit performance: Number of high-priority non-compliances

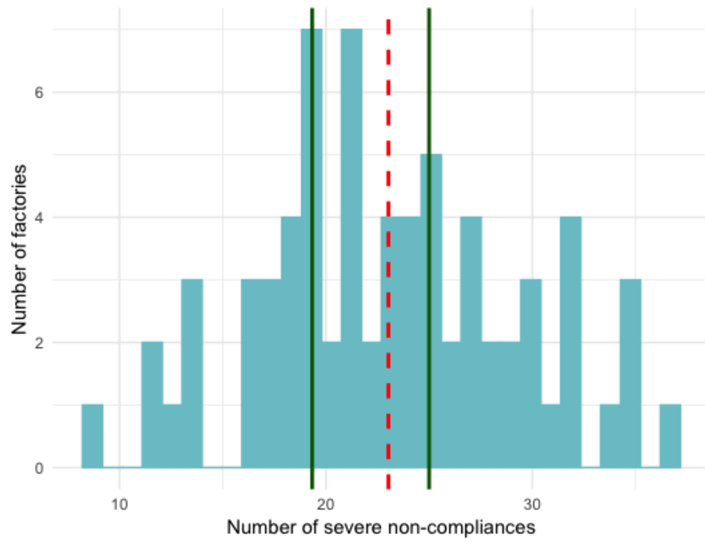


Figure G12: Participant awareness of the Accord/Alliance by tercile of safety audit performance

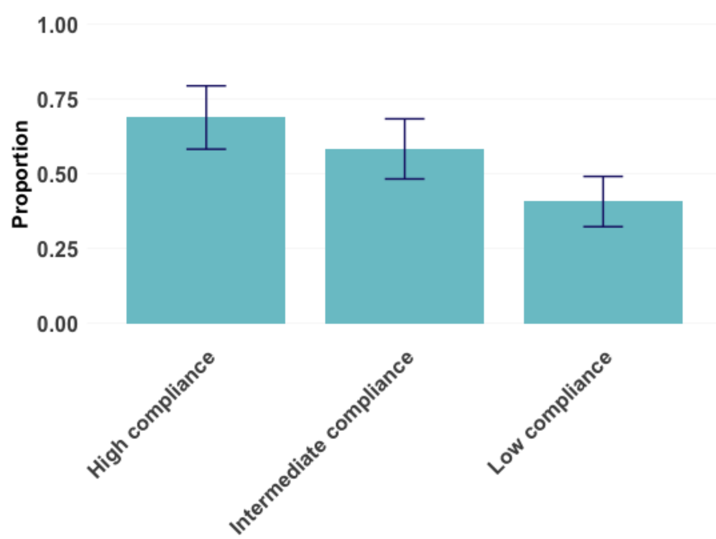
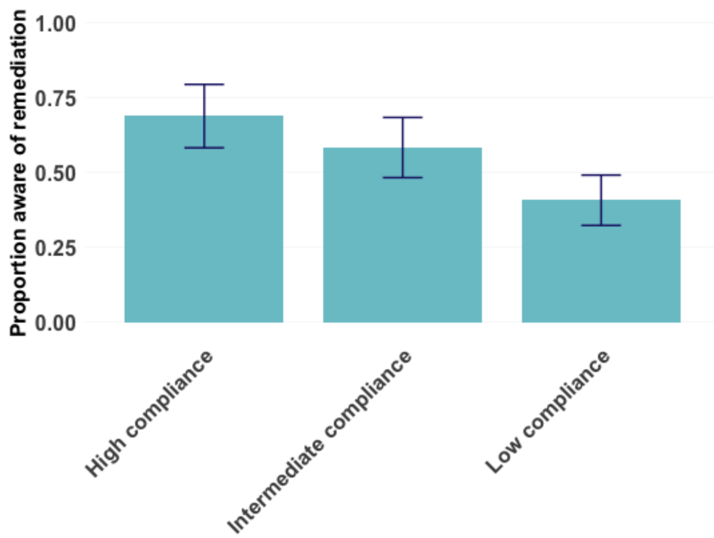


Figure G13: Participant awareness of safety remediation at factory by tercile of safety audit performance, among workers aware of building audit



Tables

Table G1: Test of Treatment-Control Covariate Balance: Full sample

	Control	Treatment	Difference	Randomization inference *p*-value
Panel A: Factory characteristics				
Num. employees	2181	2173	8.00	0.978
EPZ (1=Y)	0.31	0.27	0.04	0.745
High-priority noncompliances	23.66	25.30	-1.66	0.182
Med.-priority noncompliances	36.80	37.25	-0.45	0.840
Low-priority noncompliances	17.33	17.23	0.10	0.916
Mixed-use building (1=Y)	0.07	0.03	0.04	0.314
Panel B: Individual characteristics				
Sex (Female = 1)	0.72	0.70	0.02	0.796
Age	26.41	25.81	0.60	0.413
Education	6.65	7.12	-0.47	0.380
Tenure	2.84	3.11	-0.27	0.621
Total monthly earnings	7944	8533	-588	0.164
Revealed risk aversion	3.412	3.51	-0.09	0.697
Number of observations	153	155		

Notes: Column (4) reports the *p*-values based on a two-sided test statistic for the difference between treatment and control group means using cluster-level assignment to treatment. The *p*-values were computed as the proportion of treatment assignments that yield a difference that is greater or equal to that under the actual treatment assignment, based on 10,000 random draws. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table G2: Test of treatment-control covariate balance for participants at first, second, and third tercile performance: Factory characteristics

	Control	Treatment	Difference	RI p-value
Panel A: Participants at factories in first tercile (high compliance)				
Num. employees	2034.595	2162.000	−127.405	0.959
EPZ (1=Y)	0.730	0.578	0.152	0.570
High-priority noncompliances	16.270	16.711	−0.441	0.675
Med.-priority noncompliances	30.189	30.822	−0.633	0.857
Low-priority noncompliances	14.270	14.244	0.026	0.956
Mixed-use building (1=Y)	0.054	0.022	0.032	0.487
Number of observations	37	45		
Panel B: Participants at factories in second tercile (intermediate compliance)				
Num. employees	1867.100	1926.444	−59.344	0.898
EPZ (1=Y)	0.150	0.083	0.067	0.629
High-priority noncompliances	23.000	23.056	−0.056	0.926
Med.-priority noncompliances	37.983	32.889	5.094	0.271
Low-priority noncompliances	18.267	16.083	2.184	0.118
Mixed-use building (1=Y)	0.150	0.111	0.039	0.784
Number of observations	60	36		
Panel C: Participants at factories in third tercile (low compliance)				
Num. employees	2614.821	2299.230	315.591	0.537
EPZ (1=Y)	0.196	0.162	0.034	0.755
High-priority noncompliances	29.250	31.622	−2.372	0.028**
Med.-priority noncompliances	39.893	43.270	−3.377	0.328
Low-priority noncompliances	18.339	19.595	−1.255	0.292
Mixed-use building (1=Y)	0.000	0.000	0.000	0.909
Number of observations	56	74		

Notes: Column (4) reports the randomization inference p-values based on a two-sided test statistic for the difference between treatment and control group means using cluster-level assignment to treatment. The p-values were computed based on 10,000 random draws. *p<0.1; **p<0.05; ***p<0.01

Table G3: Test of treatment-control covariate balance for participants at first, second, and third tercile performance: Individual characteristics

	Control	Treatment	Difference	Randomization inference p-value
Panel A: Participants at factories in first tercile (high compliance)				
Sex (Female = 1)	0.794	0.767	0.027	0.826
Age	25.06	28.40	-3.34	0.046**
Education	7.51	7.70	-0.18	0.050*
Tenure	2.95	5.12	−2.17	0.093*
Total monthly earnings	8001	9060	-1059	0.322
Revealed risk aversion	3.18	3.79	-0.61	0.238
Number of observations	34	43		
Panel B: Participants at factories in second tercile (intermediate compliance)				
Sex (Female = 1)	0.700	0.556	0.144	0.238
Age	26.62	24.72	1.89	0.215
Education	6.82	7.04	−0.23	0.208
Tenure	2.91	1.94	−0.973	0.095*
Total monthly earnings	8123	8309	-185	0.738
Revealed risk aversion	3.40	3.78	-0.38	0.212
Number of observations	60	36		
Panel C: Participants at factories in third tercile (low compliance)				
Sex (Female = 1)	0.695	0.737	-0.042	0.629
Age	26.98	24.87	2.11	0.078*
Education	5.98	6.82	−0.84	0.071*
Tenure	2.70	2.53	−0.168	0.785
Total monthly earnings	7730	8341	-610	0.323
Revealed risk aversion	3.58	3.22	0.35	0.421
Number of observations	59	76		

Notes: Column (4) reports the p-values based on a two-sided test statistic for the difference between treatment and control group means using cluster-level assignment to treatment. The p-values were computed based on 10,000 random draws. *p<0.1; **p<0.05; ***p<0.01

Table G4: Attrition among treatment and control groups

	Dep var: Attrition from sample	
	(1)	(2)
Treat	0.099	-0.025
	[0.199]	[0.850]
Treat*Tercile 2		0.115
		[0.547]
Treat*Tercile 3		0.198
		[0.240]
Tercile 2		-0.098
		[0.454]
Tercile 3		-0.139
		[0.290]
p-value, diff Tercile 2 T&C		0.579
p-value, diff Tercile 3 T&C		0.109
Control group mean	0.261	
Control group mean (Tercile 1 workers)		0.353
Observations	308	308

Wild cluster-t bootstrap *p*-values with 1,000 replications in brackets. Significance levels for treatment variables' coefficients based on bootstrapped t-stats are indicated. The vector of controls includes the participants' baseline values of gender, age, years of education, tenure and tenure quadratic, and the natural log of wages. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table G5: Main results, no controls; Participant reached any round

Factory safety:	High	Intermediate	Low	P-val, diff (1), (2)	P-val, diff (1), (3)	P-val, diff (2), (3)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Leaves factory						
	-0.207	0.037	0.042			
	[0.074]*	[0.742]	[0.717]	[0.100]	[0.072]	[0.965]
Control group mean	0.462	0.263	0.236			
Observations	269	269	269			
Panel B: Refers family/friends to baseline factory						
	0.057	0.054	-0.137			
	[0.705]	[0.676]	[0.133]	[0.988]	[0.239]	[0.185]
Control group mean	0.480	0.463	0.706			
Observations	247	247	247			
Panel C: Plans to leave baseline factory						
Full sample						
	-0.174	-0.339	-0.170			
	[0.154]	[0.041]**	[0.102]	[0.431]	[0.981]	[0.424]
Control group mean	0.364	0.471	0.313			
Observations	230	230	230			
Among those who do not leave baseline factory						
	-0.270	-0.326	-0.183			
	[0.043]**	[0.082]*	[0.063]*	[0.792]	[0.625]	[0.424]
Observations	178	178	178			

Notes: Two-sided RI p-values based on 5000 draws in parentheses. One-sided RI p-values based on 5000 draws in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table G6: Main results, with controls; Participant reached any round

Factory safety:	High	Intermediate	Low	P-val, diff (1), (2)	P-val, diff (1), (3)	P-val, diff (2), (3)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Leaves factory						
	-0.209	0.014	0.045			
	[0.083]*	[0.914]	[0.686]	[0.161]	[0.068]*	[0.821]
Observations	269	269	269			
Panel B: Refers family/friends to baseline factory						
	0.051	0.106	-0.154			
	[0.726]	[0.512]	[0.084]*	[0.814]	[0.195]	[0.140]
Observations	247	247	247			
Panel C: Plans to leave baseline factory						
Full sample						
	-0.188	-0.360	-0.181			
	[0.132]	[0.029]**	[0.087]*	[0.419]	[0.967]	[0.405]
Observations	230	230	230			
Among those who do not leave baseline factory						
	-0.283	-0.339	-0.191			
	[0.040]**	[0.075]*	[0.065]*	[0.831]	[0.579]	[0.405]
Observations	178	178	178			

Two-sided RI p-values based on 5000 draws in parentheses. One-sided RI p-values based on 5000 draws in brackets. All regressions include controls for participants' baseline values of gender, age, years of education, tenure, and the natural log of wages; Panels A, C, and D also include baseline controls for the value of the dependent variable. *** p<0.01, ** p<0.05, * p<0.1.

Appendix H

I. Baseline survey questions about building safety audits:

42. Have you heard of the Accord and/or of the Alliance? *Response options: YES/NO.*

    Read aloud: “The Accord and the Alliance are both groups of companies that buy from garment factories in Bangladesh that are working to improve safety in garment factories in Bangladesh.”

43. Did you know that the Accord and the Alliance have done audits of many garment factories to see whether they meet the Accord and the Alliance’s rules for safety? *Response options: YES/NO.*

    Read aloud: “The Accord and the Alliance do three types of safety audits of garment factories, fire safety, electrical safety, and structural safety.**”**

44. Did you know that the Alliance audited your factory for safety? *Response options: YES/NO.*

    44a. Only asked if answer=YES:

a. Did your factory share the results of the audit with workers? *Response options: YES/NO.* **i. (IF (a) YES):** How were the results shared with workers (by whom and in what format)? *Open-ended.*

b. How many of the Alliance’s rules for fire, electrical, and structural safety do you think that your factory meets? *Response options: I think that my factory meets most of the rules; I think that my factory meets some of the rules; I think that my factory meets very few of the rules.*

c. Compared to other factories nearby that the Alliance may have audited, how many of the Alliance’s rules for fire, electrical, and structural safety do you think that your factory meets? *Response options: I think my factory meets more of the rules than the other factories nearby; I think that my factory meets about the same number of rules as the other factories nearby; I think my factory meets fewer rules than the other factories nearby.*

45. Has your factory shared information with workers about actions being undertaken to improve safety in your factory? *Response options: YES/NO.*

    3a. Only asked if answer=YES:

a. What actions has your factory reported taking in order to improve safety? *Open-ended.*

Note: Survey formatting has been adjusted for presentation purposes.

II. Treatment group safety information script:

Post-survey script:

"Thank you for being part of our study. We want to share some information about safety at your factory and some of the other garment factories nearby. Last year, the Alliance, a group of companies that buy garments from Bangladesh, audited your factory and many of the other factories nearby on safety. They checked how many times the factories did not meet the Alliance's safety rules. Some of the rules are more important for your safety than others. The Alliance has three groups of rules, less important, more important, and most important. We counted how many times your factory and other factories nearby did not meet the Alliance's safety rules, in particular the MOST IMPORTANT ones. Your factory did not meet the Alliance's most important safety rules ___ times. The factory that performed the best on the audit, Goldtex Garments, did not meet the most important rules 5 times. The factory that performed the worst on the audit, Sterling Styles, did not meet the most import rules 37 times. Out of the 7 factories nearby that we could find audits for, your factory performs worse than ___ other factories nearby. We have put together a list of the top 10 performing and lowest 17 performing factories nearby. I have a copy that I can show you."

(TAKE OUT INFORMATION SHEET AND FILL IN TWO BLANKS ON FRONT. TURN OVER TO THE BACK.)

"Here are the 10 best and 17 worse performing factories nearby based on how many times factories did not meet the Alliance's most important safety rules. These are out of the 71 factories nearby for which we were able to obtain safety audits."

(TURN THE PAPER OVER TO THE FRONT)

"We were able to obtain the safety audits from this website."

(POINT TO WEBSITE LISTED AT THE BOTTOM OF THE PAGE)

"The research team for this study has gathered information about safety for garment workers that could help you. If you want to obtain additional information about safety, call this safety information hotline."

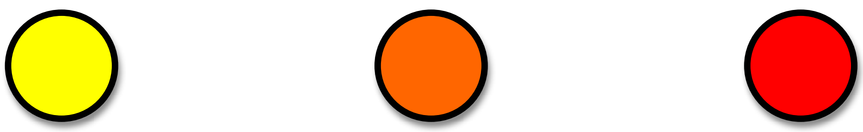
(POINT TO PHONE NUMBER PROVIDED ON THE PAGE)

III. Treatment group safety information flyer (English translation):

How safe is your factory?

The Alliance is a group of companies that buy garments from Bangladesh. The Alliance audited many factories on safety last year. It checked how many times the factories did not meet the Alliance's safety rules.

Some of the rules are more important for your safety than others. The Alliance has three groups of rules, less important, more important, and most important.



Less important   More important   Most important

Your factory did not meet the Alliance's most important safety rules ____ times, which puts it behind ____ of the 71 factories nearby for which audits are available.

Turn this paper over to see the best and worst performing factories nearby based on how many times factories did not meet the Alliance's MOST IMPORTANT safety rules. These factories are out of 71 factories nearby for which safety audits are available from the Alliance.

Factory owners are supposed to fix the safety problems in order to follow the safety rules. Has your factory told you what it is doing to fix safety problems?

For more safety information, call this number:

09604555009

Audit results are publicly available at: http://www.bangladeshworkersafety.org/factory-list-inspection-reports

Top performing factories nearby

Based on how many times factories did not meet the Alliance's MOST IMPORTANT safety rules.

Name	Location	Total
Goldtex Garments Ltd	EPZ	5
Epic Garments Manufacturing Co Ltd (Unit-2)	EPZ-Extension	9
AJ Super Garments Ltd (woven)	Goshbag	12
New World Apparels	Goshbag	12
PKG-Paxar Bangladesh Ltd	EPZ-Extension	12
Apollo Stickers Co [new location]	Zirabo	14
Lenny Apparels Ltd	EPZ	14
Paxar Bangladesh Ltd (Pkg)	EPZ-Extension	14
Mahboob Apparels Ltd	Zirabo	16
Bengal Windsor Thermoplastics Ltd	EPZ-Extension	16

Worse performing factories nearby

Based on how many times factories did not meet the Alliance's MOST IMPORTANT safety rules.

Name	Location	Total
Manta Apparels Ltd	Jamgora	28
Hop Yick (Bangladesh) Ltd (Unit-2)	EPZ	28
Talisman, Ltd	EPZ-Extension	29
Magpie Knit Wear Ltd	Yearpur	29
Fahami Industries Ltd	Baipayl	30
Versatile Attire Ltd	Raj Fulbaria	30
United Trousers, Ltd	Gouripur	30
Samia Garments Ltd	Ashulia	31
Nassa Group	Nischintopur	32
Global Knitwear Ltd	Jamgora	32
Ayesha Clothing Co. Ltd	Jamgora	32
Hop Yick Bangladesh Ltd	EPZ	32
Anzir Apparels Ltd	Ganakbari	34
Landmark Fabrics Ltd-Dinatex	Baro Ashulia	35
Ratul Knitwear	Zirabo	35
Sterling Laundry Ltd	Nayarhat	35
Sterling Styles Ltd	Yearpur Union	37

IV. Control group safety information script:

Post-survey script:

"Thank you for being part of our study. The research team for this study has gathered information about safety for garment workers that could help you. If you want to obtain additional information about safety, call this safety information hotline."

(POINT TO PHONE NUMBER PROVIDED ON THE PAGE)

Appendix I

Balance checks for prior belief-factory performance subgroups

Table I1: Test of treatment-control covariate balance for participants grouped by prior beliefs and actual factory performance: Tercile 1 participants; Factory characteristics

	Control	Treatment	Difference	Randomization inference *p*-value
Panel A: Participants with good priors at factories in first tercile (*good* performance)				
Num. employees	2087	1658	429	0.641
EPZ (1=Y)	0.67	0.63	0.04	0.940
High-priority noncompliances	16.80	16.50	0.30	0.904
Med.-priority noncompliances	29.53	28.46	1.08	0.806
Low-priority noncompliances	14.00	12.67	1.33	0.655
Mixed-use building (1=Y)	0.13	0.04	0.09	0.676
Number of observations	15	24		
Panel B: Participants with okay priors at factories in first tercile (*good* performance)				
Num. employees	1936	2774	-838	0.058*
EPZ (1=Y)	0.74	0.47	0.26	0.326
High-priority noncompliances	17.58	18.11	-0.53	0.714
Med.-priority noncompliances	30.11	33.47	-3.37	0.200
Low-priority noncompliances	14.84	16.47	-1.63	0.265
Mixed-use building (1=Y)	0.00	0.00	0.00	1.00
Number of observations	19	19		

Notes: Column (4) reports the p-values based on a two-sided test statistic for the difference between treatment and control group means using cluster-level assignment to treatment. The p-values were computed based on 10,000 random draws. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table I2: Test of treatment-control covariate balance for participants grouped by prior beliefs and actual factory performance: Tercile 2 participants; Factory characteristics

	Control	Treatment	Difference	Randomization inference *p*-value
Panel C: Participants with good priors at factories in second tercile (*okay* performance)				
Num. employees	1983	2229	-246	0.825
EPZ (1=Y)	0.08	0.05	0.03	0.909
High-priority noncompliances	23.08	23.47	-0.39	0.762
Med.-priority noncompliances	37.92	34.58	3.34	0.641
Low-priority noncompliances	18.24	16.05	2.18	0.463
Mixed-use building (1=Y)	0.16	0.16	0.00	1.00
Number of observations	38	19		
Panel D†: Participants with okay or poor priors at factories in second tercile (*okay* performance)				
Num. employees	1667	1588	79	0.900
EPZ (1=Y)	0.27	0.18	0.16	0.112
High-priority noncompliances	22.86	22.59	0.28	0.690
Med.-priority noncompliances	38.09	31.00	7.09	0.100
Low-priority noncompliances	18.32	16.12	2.20	0.199
Mixed-use building (1=Y)	0.14	0.06	0.08	0.623
Number of observations	22	17		

Notes: † Panel D combines participants with okay and poor priors because there are only 3 participants with poor priors in the treatment and the control groups, respectively, which are too few observations to implement randomization inference. Column (4) reports the *p*-values based on a two-sided test statistic for the difference between treatment and control group means using cluster-level assignment to treatment. The *p*-values were computed based on 10,000 random draws. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table I3: Test of treatment-control covariate balance for participants grouped by prior beliefs and actual factory performance: Tercile 3 participants; Factory characteristics

	Control	Treatment	Difference	Randomization inference *p*-value
Panel F: Participants with good priors at factories in third tercile (*poor* performance)				
Num. employees	2602	2684	-82	0.921
EPZ (1=Y)	0.33	0.26	0.07	0.858
High-priority noncompliances	29.50	31.40	-1.90	0.484
Med.-priority noncompliances	40.23	40.94	-0.71	0.892
Low-priority noncompliances	18.30	19.50	-1.20	0.705
Mixed-use building (1=Y)	0.000	0.000	0.000	1.0
Number of observations	30	34		
Panel E†: Participants with okay or poor priors at factories in third tercile (*poor* performance)				
Num. employees	2606.31	1992.929	613.381	0.135
EPZ (1=Y)	0.14	0.12	0.02	0.929
High-priority noncompliances	28.7	31.5	-2.8	0.036**
Med.-priority noncompliances	38.93	44.71	-5.78	0.000***
Low-priority noncompliances	17.72	19.31	-1.59	0.384
Mixed-use building (1=Y)	0.000	0.000	0.000	1.0
Number of observations	29	42		

Notes: † Panel E combines participants with okay and poor priors because there are only 4 participants with poor priors in the control group and 14 participants with poor priors in the treatment group. The small cell size for this category's control group precludes use of randomization inference. Column (4) reports the *p*-values based on a two-sided test statistic for the difference between treatment and control group means using cluster-level assignment to treatment. The *p*-values were computed based on 10,000 random draws. *p<0.1; **p<0.05; ***p<0.01

Table I4: Test of treatment-control covariate balance for participants grouped by prior beliefs and actual factory performance: Tercile 1 participants; Individual characteristics

	Control	Treatment	Difference	Randomization inference *p*-value
Panel A: Participants with good priors at factories in first tercile (*good* performance)				
Sex (Female = 1)	0.800	0.708	0.092	0.789
Age	25.73	29.00	-3.27	0.495
Education	7.43	7.56	-0.13	0.900
Tenure	3.00	5.22	-2.22	0.581
Total monthly earnings	8641	9637	-996	0.668
Revealed risk aversion	2.87	3.58	-0.72	0.557
Number of observations	15	24		
Panel B: Participants with okay priors at factories in first tercile (*good* performance)				
Sex (Female = 1)	0.789	0.842	-0.053	0.767
Age	24.53	27.63	-3.12	0.134
Education	7.58	7.87	-0.29	0.559
Tenure	2.91	4.99	-2.07	0.263
Total monthly earnings	74968	8332	-835	0.545
Revealed risk aversion	3.42	4.05	-0.63	0.273
Number of observations	19	19		

Notes: Column (4) reports the p-values based on a two-sided test statistic for the difference between treatment and control group means using cluster-level assignment to treatment. The p-values were computed based on 10,000 random draws. *p<0.1; **p<0.05; ***p<0.01

Table I5: Test of treatment-control covariate balance for participants grouped by prior beliefs and actual factory performance: Tercile 2 participants; Individual characteristics

	Control	Treatment	Difference	Randomization inference *p*-value
Panel C: Participants with good priors at factories in second tercile (*okay* performance)				
Sex (Female = 1)	0.632	0.579	0.053	0.821
Age	26.11	23.32	2.79	0.571
Education	6.74	7.63	-0.89	0.605
Tenure	3.21	1.67	1.55	0.422
Total monthly earnings	8345	8047	298	0.796
Revealed risk aversion	3.34	3.21	0.13	0.900
Number of observations	38	19		
Panel D†: Participants with okay or poor priors at factories in second tercile (*okay* performance)				
Sex (Female = 1)	0.818	0.529	0.289	0.026**
Age	27.50	26.29	1.21	0.590
Education	6.95	6.34	0.61	0.193
Tenure	2.39	2.25	0.15	0.872
Total monthly earnings	7740	8601	-861	0.165
Revealed risk aversion	3.50	4.41	-0.91	0.036**
Number of observations	22	17		

Notes: † Panel D combines participants with okay and poor priors because there are only 3 participants with poor priors in the treatment and the control groups, respectively, which are too few observations to implement randomization inference. Column (4) reports the *p*-values based on a two-sided test statistic for the difference between treatment and control group means using cluster-level assignment to treatment. The *p*-values were computed based on 10,000 random draws. *p<0.1; **p<0.05; ***p<0.01

Table I6: Test of treatment-control covariate balance for participants grouped by prior beliefs and actual factory performance: Tercile 3 participants; Individual characteristics

	Control	Treatment	Difference	Randomization inference *p*-value
Panel F: Participants with good priors at factories in third tercile (*poor* performance)				
Sex (Female = 1)	0.633	0.765	-0.131	0.622
Age	27.70	25.62	2.08	0.625
Education	6.53	6.71	-0.17	0.905
Tenure	3.01	2.88	0.13	0.917
Total monthly earnings	7868	8220	-352	0.743
Revealed risk aversion	3.77	3.47	0.30	0.719
Number of observations	30	34		
Panel G: Participants with okay or poor priors at factories in third tercile (*poor* performance)				
Sex (Female = 1)	0.759	0.714	0.044	0.731
Age	26.24	24.26	1.98	0.313
Education	5.41	6.92	-1.51	0.068*
Tenure	2.37	2.25	0.12	0.812
Total monthly earnings	7588	8439	-851	0.053*
Revealed risk aversion	3.38	3.02	0.36	0.314
Number of observations	29	42		

Notes: Column (4) reports the *p*-values based on a two-sided test statistic for the difference between treatment and control group means using cluster-level assignment to treatment. The *p*-values were computed based on 10,000 random draws. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$