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Abstract

Conflicts between management and workers are common and can have significant impacts on productivity. We study how workers in a large Bangladeshi sweater factory responded to management's decision to lay off about a quarter of the workers following a period of labor unrest. Our main finding is that the mass layoff resulted in a large and persistent reduction in the productivity of surviving workers. Moreover, it is specifically the firing of peers with whom workers had social connections – *friends* - that matters. We also provide suggestive evidence of deliberate shading of performance by workers in order to punish the factory's management, and a corresponding deliberate attempt by the factory to win the angry workers back by selectively giving them tasks that are more rewarding. By combining ethnographic and survey data on the socialization process with the factory's internal records, the paper provides a rare glimpse into the aftermath of an episode of labor unrest. A portrait of the firm emerges as a web of interconnected relational agreements supported by social connections.

JEL-Codes: J500, M500, O120.

Keywords: layoffs, productivity, morale, relational contracts.

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1 Introduction

Conflicts between workers and management can severely impact productivity and pose significant challenges to firms. In emerging industrial countries such as Bangladesh, China, and Vietnam, where labor unrest occurs frequently, firms often respond to unrest by laying off workers.[1] The impact of such layoffs on the productivity of surviving workers is, in principle, uncertain. For example, layoffs could raise productivity by removing troublemakers or by intimidating workers with fear of further firing, or they could lower productivity by reducing workers' morale or inducing worker retaliation. Empirical evidence on the impact – and the mechanisms involved – can inform our understanding of worker-management relationships and thus potentially contribute to policy debates on the importance of healthy industrial relations. Despite its relevance, such evidence has proven elusive as it is difficult to access organizations during episodes of conflict and both layoffs and worker productivity are typically hard to measure.[2]

This paper offers a rare glimpse into an episode of labor unrest. We study how workers in the Manual Knitting Section of a large Bangladeshi sweater factory responded to management's decision to lay off about a quarter of the workers following a period of unrest. We combine detailed administrative data (as in the "insider econometrics" approach, Ichniowski and Shaw (2009)) with ethnographic and survey evidence on the socialization process on the production floor. We distinguish workers who were fired by the factory from those who voluntarily quit and exploit features of the production process that allow for accurate individual-level measures of worker productivity.[3]

Our main finding is that the mass layoff was associated with a significant reduction in the productivity of surviving workers which persisted for several months after the firings. It is specifically the firing of peers with whom workers had social connections – *friends* – that is associated with a drop in productivity. Our estimates suggest that each additional fired friend translates into the equivalent of 2-3 days of lost production per month. As workers are paid piece rates, this productivity drop implies a sizeable income loss for the workers as well. We also provide suggestive evidence of deliberate shading of performance by workers in order to punish the factory's management, and a corresponding deliberate attempt by the factory

[1]For example, on China, see The Economist on January 31, 2015, April 26, 2014, and January 28, 2012; on Vietnam, see The Economist on June 7, 2014; on Bangladesh, see The Economist on February 7, 2015. More recently, see Reuters on January 10, 2019 and The Guardian on January 14, 2019 on Cambodia and Bangladesh respectively.

[2]With a rather different focus, Gibbons and Katz (1991) pioneered a literature on layoffs and plant closures to study asymmetric information in the labor market.

[3]The context of this study is also of intrinsic interest. Knitwear is the largest export sector in Bangladesh, having surpassed woven garments in recent years. Relative to woven garments, knitwear production also generates stronger backward linkages for exporting countries. The sector counts around 2,000 plants. The factory in this study is one of the largest exporters in the country.

to win the angry workers back by selectively giving them tasks that are more rewarding. The response to the mass layoffs reveals a workplace in which a web of interconnected relational arrangements (see, e.g., Gibbons and Henderson (2012)) is supported by social connections (Bandiera et al. (2005, 2010)).

We develop our argument in three steps. We first provide detailed background information on the production process, the labor unrest and firing episode, and the socialization process in the factory. We then ask two questions: (1) does the mass layoff decrease a worker's productivity? and (2) if so, why?

In the sweater factory we study, each worker is permanently assigned to one workstation (a machine). Machines are located next to each other and are arranged into "blocks" – sets of adjacent machines that share a common supervisor. The individual nature of production makes it possible to measure and track productivity of individual workers over time.[4] In the spirit of classic observational studies of firms, such as Roethlisberger and Dickson (1939) and Roy (1952), we gathered insights into the typical activities of a worker on the production floor by embedding members of the research team as observers in the factory. Workers were more likely to interact with peers from their own blocks rather than those from outside and, more generally, with peers spatially closer to their own working locations. Among peers located spatially close, we document a discontinuity at the block border in the level of interactions; and we show that workers interact more with peers to their front and to their side than those to their back.[5]

The work environment in the Manual Knitting Section started to deteriorate in February 2014 when workers protested against a change in the location of the factory. This protest precipitated a 17-day shutdown of the section. Almost all workers returned when the section re-opened. In the first week of April 2014, the manual knitting workers staged a second protest against piece rates that were perceived to be low.[6] This protest took a violent turn, with some workers physically injuring a factory upper manager. The factory was shut down for a little more than a month. In the meantime, the management fired 101 of the 406 operators for their alleged involvement in the violence.

We examine how the mass layoff impacted workers' productivity. We define individual measures of exposure to firing. Our baseline measure of a surviving worker's exposure weights

[4] Workers use the same capital (manual knitting machine), inputs (e.g. yarn), and technology for production, which makes production comparable across workers. Although different workers may produce different sweater styles at any given point in time, across worker comparisons are possible by converting physical units of output into a common metric using each sweater part's standard minute values (SMVs).

[5] A detailed workers survey validates the results of these observations and documents how interactions within the factory are associated with social attachments between workers outside the workplace.

[6] Bangladesh increased the minimum wage in the woven garment sector (where workers are generally paid a fixed salary) earlier in January of the same year. Although the factory was not legally required to adjust piece rates, (some) workers might have considered it fair to do so.

each fired worker from the survivor's block by their spatial distance to the survivor. Within a difference-in-difference framework, we document that workers that were more heavily exposed to the mass layoff of peers significantly reduce their productivity in the seven months after the firing episode. We construct more refined measures of exposure that reflect the exact nature of the socialization process on the production floor – within blocks, and with adjacent peers to the front and the sides of the workstation – and find evidence that it is the loss of peers with whom the worker was likely to have socialized (*friends*), rather than simply nearby peers, that drives the loss in productivity.

We consider and rule out a number of alternative explanations for the results. First, we show that there is no negative effect of exposure to workers who voluntarily quit, as opposed to those who were fired by the factory. This evidence, supplemented by additional tests, rules out a number of competing channels, including (but not limited to) loss of help from co-workers, time spent helping new workers that replaced fired ones, and less on-the-job attention while workers look for alternative employment opportunities.

The evidence indicates that the mass layoff of friends led to a drop in productivity; thus, it is inconsistent with *intimidation* (workers raise productivity in response to a more salient and credible threat of being fired). We see two possible explanations for the productivity drop: *demoralization* (workers lower their productivity, as the layoff of friends causes morale to wane); *anger and punishment* (workers lower productivity, as they are motivated, out of anger or a sense that relational contracts have been violated, to shade performance).[7] These explanations are certainly not mutually exclusive; and since they concern workers' mental states, they are intrinsically difficult to distinguish. Nonetheless, the distinction is potentially important. From the factory's point of view, angry workers – in contrast to workers who are simply demoralized – may engage in deliberate acts of sabotage. More broadly, distinguishing *demoralization* from anger and punishment can shed light on the nature of industrial relations in the workplace and inform policy debates, for example, on the role of factory unions.

Evidence on quality flaws suggests that anger accounts for at least part of the productivity drop. There are two types of quality flaws: small flaws that simply require "mending" and more significant flaws ("defects"). Sweaters with small flaws are passed on to a separate group of mending operators; these flaws are costly to the factory but not to the worker. On the other hand, defects must be fixed by the workers themselves before they can move on

[7] See, e.g., Bewley (1999) on morale, Akerlof and Yellen (1990) on anger and morale, Hart and Moore (2008) and Akerlof (2016) on deliberate shading of performance due to contract violations, and Levin (2003) and Li and Matouschek (2013) on relational contracts. Notably, the reason for punishing the firm in this case is mistreatment of peers. This would be consistent with a view of the firm as a web of interconnected relational agreements (see, e.g., Crémer (1986) and Levin (2002)), possibly supported by workers' willingness to punish "altruistically" on behalf of third parties (see Fehr and Gächter (2002); Bernhard et al. (2006)).

to the next job and are thus costly to the workers in terms of lost time and pay. We find that higher exposure to the mass layoff of friends increased the mending rate but not the defect rate. While other interpretations are possible, this evidence suggests that workers might have deliberately tried to punish the firm.[8] Consistent with this story, we also show that the drop in productivity fades over time as the factory tries to win back workers with higher exposure to the firing by allocating jobs that pay relatively higher piece rates.[9] The concluding section discusses implications of these findings for our understanding of industrial relations in developing countries.

This paper contributes to different strands of literature. First, we contribute to the literature on conflict within firms and its effect on firms' performance. For example, Krueger and Mas (2004) document that labor strife at Bridgestone/Firestone's Decatur plant coincided with higher incidence of defective tires.[10] A recent literature has considered changes in pay and pay cuts. For example, Jayaraman et al. (2016) document relatively short-lived positive reciprocity following a wage increase at a tea factory in India; Krueger and Friebel (2019) observe persistent negative reciprocity following unequal pay changes at a German personnel search firm; Sandvik et al. (2020) show higher turnover among the most productive workers following a reduction in commissions at a sales firms; Coviello et al. (2020a) find that workers engage in counter-productive actions after a pay cut; Coviello et al. (2020b) study the impact of minimum wage on worker productivity and termination. We focus on the impact of the mass layoff of co-workers, the subject of extensive but mostly qualitative inquiries in the management literature (see, e.g., Brockner et al. (1987), Cascio (1993), Mishra and Spreitzer (1998)). Brockner et al. (1987) suggest that workers who most closely identify themselves with fired workers, and who think that the layoff was unfair, are most negatively affected (see also Brockner et al. (1993b) and Brockner et al. (1993a)). We contribute by providing quantitative evidence from a workplace.[11]

Our analysis also sheds light on the nature of informal relationships inside the firm. Despite voluminous theoretical research (see, e.g., Baker et al. (1994), Baker et al. (2002); Levin (2002), Levin (2003); Chassang (2010); Li and Matouschek (2013)), evidence on relational contracting within firms remains largely anecdotal since, by definition, they rely on informal exchanges of promises that are rarely recorded anywhere and thus are difficult to measure. Blader et al. (2020) infer the importance of relational contracts in the workplace from work-

[8]This evidence admits alternative interpretations and therefore we complement it with additional pieces of evidence that support our interpretation.

[9]Note that this form of compensation is cheaper than increasing piece rates across the board and it also allows the factory to target workers more discretely, i.e., without angering workers that are left untargeted.

[10]See also Mas (2008), Katz et al. (1983), and Kleiner et al. (2002).

[11]Gerhards and Heinz (2017) provide evidence from the laboratory, while Heinz et al. (Forthcoming) implement a related field experiment among short-term workers in a German call center.

ers' negative response to the introduction of competition-based performance incentives when there exists a strong norm of cooperation between peers. We exploit a period of crisis to infer the existence of multi-lateral relational contracts inside the firm.[12] We provide suggestive evidence consistent with the reduction in workers' effort being part of a punishment strategy (Gibbons and Henderson (2012)) rather than simply reflecting lower morale. Furthermore, the multi-lateral relational contract between workers is supported by personal relationships. The paper thus also contributes to the literature on "social incentives" (see Ashraf and Bandiera (2018)) for an excellent summary).[13]

Finally, our paper is related to a growing empirical literature on human resources management and industrial relations in developing countries. Atkin et al. (2017) document that misaligned incentives between management and workers can slow technology adoption. Macchiavello et al. (2020) show that misaligned beliefs lead to the under-promotion of female operators to supervisory roles in Bangladeshi garment factories, and thus to a misallocation of managerial talent.[14] Breza et al. (2018) find that pay inequality reduces worker productivity and coworkers' ability to cooperate. Breza et al. (2019) show that social norms allow workers in rural villages in India to maintain wage floors in their local labor markets. We offer a rare window into the aftermath of an episode of labor unrest – a characteristic trait of industrial relations in countries with emerging manufacturing sectors.[15]

The paper proceeds as follows. Section 2 provides detailed background information on the factory organization, the production technology, the socialization process, and the labor unrest. Section 3 defines a worker-specific measure of exposure to firing and investigates whether the mass layoff reduced productivity. Section 4 investigates mechanisms. Section 5 concludes.

[12] Macchiavello and Morjaria (2015) exploit a crisis induced by an episode of ethnic violence to infer the existence of relational contracts between – rather than within – firms in the Kenya rose export supply chain.

[13] The paper is also related to the literature on peer effects in the workplace. For example, Bandiera et al. (2005) find that workers reduce productivity when their effort exerts negative externalities on their friends. Mas and Moretti (2009) find positive spillovers from highly productive peers. Unlike these papers – which document spillovers between peers that work together – our evidence indicates spillovers from peers that have left the workplace.

[14] Other recent work in the garment sector includes Boudreau (2020) showing that (buyer enforced) worker-manager safety committees can lead to more compliance with labor laws favoring workers; Tanaka (2020) on how exporting causes firms to improve working conditions; Adhvaryu et al. (2020a) on relational contracts between line supervisors in a large garment factory; and Adhvaryu et al. (2020b) on how providing workers a channel to voice their concerns can encourage them to stay with the firm.

[15] Ashraf et al. (2015) document the prevalence, and study the impact, of labor unrest in Bangladesh. Hjort (2014) finds (ethnic) conflict outside the workplace leads to reduced cooperation between workers along ethnic lines. Like Hjort (2014), we exploit internal records from a workplace, but we focus on conflict between workers and management. Poor relationships are not confined to the industrial sector. In agriculture, plantation workers and smallholder farmers supplying large agribusinesses often face similar struggles (see, e.g., Little and Watts (1994)). Casaburi and Macchiavello (2015) study a Kenyan dairy cooperative trying to (re-)build loyalty among members by threatening to expel non-complying members (the equivalent of layoffs in our context). They find that such threats are hard to enforce in practice.

2 Background

This section describes the context of the study, including a number of distinctive advantages that enable the analysis. We first describe the production process in the factory and how we measure workers' productivity. We then turn to the labor unrest and subsequent firing, and socialization on the production floor. Finally, we describe the data.

2.1 Production

We study the Manual Knitting Section of a large sweater factory in Bangladesh.[16] Workers in this section manually operate machines to knit yarn into sweater parts that are later passed on to the Linking Section to be stitched together. Each worker has a designated machine. The machines are themselves stationed next to each other at designated places on the floor. The total number of workers varies over the sample period because of regular turnover of workers, but for most of the earlier part of the sample period, it is 400 or more. The workers are grouped into "blocks" of about 30 workers, with a supervisor dedicated to each block. The block supervisors are supervised by one Floor-In-Charge who, in turn, is supervised by the Production Manager.

It is important to note that "blocks" are not production teams: while workers within the same block share a common supervisor (whose role is quite limited) and have lunch breaks at the same time, work is done *independently*.[17] Each worker completes the knitting of a sweater all by himself and is paid an individual piece rate. Thus, blocks are not like the sewing lines in woven and light knitwear garment factories studied, for example, by Macchiavello and Morjaria (2015), Menzel (2019) and Adhvaryu et al. (2019).[18]

At any point in time the knitting section works on multiple orders, leading to simultaneous production of multiple styles of sweaters. Whenever a worker becomes available for a new job, he receives one, which means he needs to knit a batch of 12 sweaters of a particular style. A sweater typically consists of four parts (front, back and sleeves), but can vary depending on the style. Completion of a job may take anywhere from a few hours to more than a day depending on the complexity of the style. This allocation of styles is done by "distributors"

[16] At the time of the study the factory also had semi-automatic and automatic knitting sections. These sections have different processes, workforce, and data and were not affected by the unrest and the subsequent layoff. The factory is vertically integrated from yarn winding to packaging of final sweaters for shipment. Knitting is the second stage in the production chain.

[17] Block supervisors are typically former workers who are too old to operate the machines at an appropriate speed. Their role is limited to overseeing and helping to fix machines, and communicating with senior management on behalf of younger workers.

[18] In woven and light knitwear garment factories, production is generally organized along sewing lines in which workers sitting next to each other work on a specific task of a garment before passing it on to the next worker for the next task on the same garment. In other words, it is team production. The workers in such factories are more often women, in contrast to the factory we study where manual knitters are all men.

from the Distribution (sub) Section within the Manual Knitting Section, in consultation with the Floor-In-Charge.

2.2 Measuring Productivity

Several aspects of the production process allow us to measure physical productivity across individual workers and over time. First, each worker is individually responsible for the knitting of a batch of sweaters. The individual nature of production makes it possible to measure and track what each worker produces. Moreover, workers use the same capital (manual knitting machines), inputs (e.g. yarn), and technology for production; this makes production comparable across workers.

Second, although different workers might be producing different sweater styles at any given point in time, across worker comparisons are possible by converting physical units of output into a common metric using each garment's standard minute value (SMV).[19] Every sweater style is accompanied by a "design chart." An example of a chart is shown in Figure A1 in the Appendix. Each chart contains details of the sweater parts, including the yarn type (thin or thick), dimensions, the number of parts necessary to produce the whole sweater, and designs on the sweater. The chart also provides step-by-step instructions for the worker to follow during the process of knitting.

We ask an independent textile engineer to use the factory style charts to calculate SMVs for the corresponding sweaters. The particular factory we work with did not use SMVs. Using a single independent engineer has two further advantages. First, a single engineer provides us with SMVs that are likely more consistent across styles than those produced by different engineers for different sweaters (as is often the case for SMVs estimated by factories themselves). Second, an independent engineer avoids concerns that a factory engineer might strategically adjust SMVs as those are then used to set piece rates (see Gibbons (1987) for a discussion). Indeed, in our context the factory sets piece rates based on estimates from the first month of production whenever a new style is introduced. The correlation coefficient between our estimated SMVs and the piece rates of the corresponding sweaters is 0.9.

We construct a measure of productivity at the worker-month level by weighting production of each style by the style's SMV:

$$MonthlyProduction_{it} = \sum_{s \in S} q_{ist} \times SMV_s,$$

[19]SMV are a widely used measure in the garment industry to benchmark the average time a particular garment should take to produce. This measure has been used to measure line-level (Ashraf et al. (2015), Macchiavello and Morjaria (2015) and worker-level (Adhvaryu et al. (2019)) efficiency in garment factories.

where q_{ist} is the total quantity of sweaters of style s produced by worker i in month t and SMV_s is the estimated SMV of style s. $MonthlyProduction_{it}$ can be interpreted as the number of minutes it would have taken a "typical" worker to produce what worker i produced over the course of month t. $MonthlyProduction_{it}$ serves as our baseline measure of productivity. A complex style has a higher SMV, while a simpler style has a lower SMV. This measure therefore implicitly controls for the complexity of the different styles and yields a measure of physical monthly output that is comparable across workers. Note that we do not divide this measure by the total working hours of a worker. Workers are paid piece rates and are thus free to choose whether they come to work and, conditional on doing so, how fast to work, how many breaks to take, etc.[20]

Monthly earnings from production give us a second, and more traditional, measure of a worker's productivity. Completed sweaters count towards earnings. The factory pays the workers monthly based on the quantities of sweaters they produce and the piece rates for those sweaters. The rates for the sweaters vary across styles and are determined by management.[21]

For a subset of the sample period, we also observe the quality of the sweaters that the workers produce. Before a worker can submit his completed set of sweaters to count towards his monthly earnings, the sweaters are individually checked for flaws. The factory inspects for and records two kinds of flaws that we will exploit later. The first kind consists of more significant flaws, or "defects," that the worker needs to fix himself. The worker takes the faulty sweater parts back to his workstation, fixes the defects, brings them for another round of inspection, and only if he has successfully fixed them is he assigned a new set of sweaters. The second kind are smaller flaws that simply require "mending." These cases are instead passed on to a separate group of mending operators and the worker can move on to his next set of sweaters directly. Defects are thus costly to the worker while mending flaws are not. We observe both the number of sweater parts that were identified as defective and those that were passed for mending.

2.3 Unrest & Layoff

In this sub-section, we describe labor unrest episodes during our sample period. The timeline of relevant events is illustrated in Table A1 in the Appendix. The work environment in the Manual Knitting Section started to deteriorate in February 2014 when the factory's

[20] The analysis explores both monthly production and monthly production divided by days at the factory.

[21] Earnings provide a proxy for productivity that is comparable across workers and over time, but that is nonetheless noisier for two reasons. First, workers work on multiple styles in a month (four on average) that do not necessarily overlap with other workers. Second, piece rates vary across styles but may not perfectly reflect complexity. Earnings will thus confound productivity and complexity-normalized piece rates.

management moved the section from the factory's main compound to a new location about a mile away.[22] The manual knitting workers were unhappy about the move and protested it, which led to a 17-day shutdown of the section. Almost all workers returned when the section re-opened.

A second, more significant, protest – against perceived low piece rates – occurred in the first week of April 2014.[23] This protest turned highly aggressive and violent. At one stage, a group of workers physically injured the Floor-In-Charge. The section was shut down again and re-opened a little more than a month later, in mid May. In the meantime, the management fired 101 of the 406 workers for their alleged involvement in the violence and followed up by filing lawsuits against many of these fired workers. Six supervisors were also fired, allegedly due to their role in the unrest. Working with the factory management, we identify the 101 workers who were fired as opposed to others who voluntarily left the factory right after the protest. The fact that we can identify the workers who were actually fired as opposed to those who voluntarily quit is key to identifying the effect of firing on the remaining workers.

Table A2 in the Appendix reveals that workers whose productivity was low during the period of unrest (February-March 2014) and workers with longer tenures at the factory were more likely to be fired.[24] The table reports linear probability models testing the association between workers' observed characteristics and the probability of being fired in April 2014. Columns 1 and 2 show that workers whose productivity and earnings were below median before the unrest period (June 2013-January 2014) were only slightly more likely to be fired. Columns 3 and 4 reveal that workers with productivity below median during the unrest period were about 15% more likely to be fired. These results, taken together, suggest that the firm mainly fired workers who were disruptive during the unrest – and hence had low productivity over that period - rather than workers with low productivity in general. Columns 5 and 6 show that, conditional on productivity, fired workers were also likely to have been working at the factory for a longer time.

The factory replaced the fired workers and those who quit voluntarily with new workers hired in several phases over July-September 2014. There were no further protests as of the time we stopped working with the factory in November 2016.[25]

[22]The Manual Knitting Section of the factory had experienced some, milder, episodes of labor unrest before our sample period. The exact dates or reasons for these earlier episodes could not be verified.

[23]In January 2014, Bangladesh increased the minimum wage for garment workers on fixed salaries – contracts that are typical in the woven and light knit segments of the garment sector. While the factory was not legally required to increase piece rates, since workers earned significantly more than the minimum wage, some workers may have felt that they deserved an increase. The factory's failure to meet this expectation might have played a role in sparking the unrest.

[24]The similar coefficient regardless of whether we measure productivity using SMVs or piece rates suggests that the lower earnings was driven by lower productivity as opposed to working on styles with less rewarding piece rates.

[25]We do not know whether this was because the factory managed to part with troublemakers, or because

2.4 Socialization Process

This section provides detailed information on the pattern of social interaction among workers. These patterns will later be exploited to assess the impact of the firings on surviving workers.

We gain a comprehensive understanding of the socialization process at the factory through two distinct, complementary routes. First, before the firing episode, in the spirit of Homans (1950), we gathered insights into the typical activities of a worker on the production floor. We sat down at a distance and observed different workers, their work processes, and their interactions with co-workers or supervisors, all the while systematically recording these activities. After the firing episode, we were able to conduct a detailed survey on worker socialization and social connections in the workplace. The survey strongly supports our conclusions from observation of the production floor.

The main finding of these exercises is that workers were more likely to interact with peers from their own blocks and with peers located close by on the factory floor. Among peers located close by, we find a significant discontinuity at the block border in the level of interactions – which suggests that a worker's block identity matters and that the block is an important social grouping within the factory. Within blocks, workers interact more with peers to their front than those to their back. We also document how interactions within the factory translate into social attachments between the workers. Finally, we provide suggestive evidence that the strength of socialization within a block builds over time.[26]

Beginning in January 2014, members of our observation team visited the factory on a weekly basis to closely observe the work processes, work environment, and behaviors of workers in different sections of the factory. We were not anticipating the firings; but this was part of our groundwork for potential interventions in the factory to improve productivity. We compiled detailed qualitative observations of how workers spent their time on the production floor. Tables A3 and A4 in the Appendix show two samples of these observations from the Manual Knitting Section in early January 2014. For instance, we observed the worker in Table A3 between 5.20pm and 7.07pm; over this observation period, the worker went to the distribution section and interacted with several co-workers as well as his supervisor.

Two patterns emerge from our qualitative observations. First, workers frequently converse and engage in social interactions that make work more enjoyable.[27] Second, workers are more likely to interact with peers located close to them. One reason is that workers are stationed at designated machines and movement across the production floor is limited.

the firings served as a deterrent for the remaining workers, or due to other reasons.

[26]We also argue (and provide suggestive evidence) that the allocation of workers to workstations is not driven by pre-existing social ties.

[27]Boredom from exhaustive, repetitive, work is highly demotivating. Workers report rotating styles to reduce boredom despite potential productivity losses when changing styles.

In addition, the floor is noisy due to the simultaneous operation of many machines, which restricts the ability to converse at a distance. These patterns of interaction underpin our definition of exposure to firing in Section 3.2.

We verify our qualitative observations with a survey of the workers' social network in October 2015. Under the assumption that the structure of how workers socialize on the production floor did not change before and after the firing episode, this survey helps to verify the role of spatial distance on the probability of being socially connected.

The social-network survey lines up very well with our qualitative observations: workers interact mostly with proximate peers. The top panel of Figure 1 plots the probability that a worker ever talks or interacts with a peer conditional on the distance between the two. We distinguish between peers from the same block (left sub-panel) and those from outside the block (right sub-panel). Both panels show that the probability a worker talks with a peer is higher when the peer is spatially closer. For instance, with respect to workers from the same block, the probability that a worker talks with a peer one-worker distance away is 0.94 as opposed to 0.85 when the peer is a two-worker distance away. There are similar differences when comparing peers at two-worker distance versus three, or three-worker distance versus farther away. All differences are statistically significant at the 1% level.

A second key finding that emerges from comparing the two panels in the figure is that workers are more likely to talk to peers from their own blocks, conditional on distance. For example, the probability of talking with a peer one-worker distance away is 0.94 if the peer is from the same block but only 0.28 if the peer is from a different block. Therefore, block identity appears to matter and the block appears to be an important social grouping within the factory. To further examine the role of distance, we look into the frequency of interactions among workers inside the same block. Figure A2 depicts the probabilities of a worker speaking with a same-block peer many times a day (left panel), 1-2 days a week (middle panel), or not at all (right panel). Again, this figure confirms the pattern found in our qualitative observations: the closer two workers are located on the production floor, the more likely they are to talk frequently.

Interactions on the production floor are closely associated with deeper forms of social attachment. The bottom panel of Figure 1 shows that the pattern of socializing outside the factory is aligned with the pattern of interaction inside the factory. The probability of socializing with a peer outside the factory is greater if the peer is one-worker distance away rather than two. These probabilities are 0.50 and 0.37 respectively for same-block peers, and 0.11 and 0.03 for outside-block peers; the differences are statistically significant at the 1% significance level. Similar differences arise when comparing peers at other distances (e.g. two-worker distance versus three-worker distance). Conditional on distance, workers are more likely to socialize outside the factory with peers from their own blocks.

Table A5 in the Appendix provides additional evidence on the role of the block as a key driver of socialization. Dyadic regressions in the table take advantage of the arrival of new workers in the factory months after the firings. Columns 1-4 confirm that workers within a block are more likely to socialize outside work. These regressions also show, as one might expect, that new workers are less likely to interact with peers outside the factory. Consistent with the idea that socialization within the block builds over time, the block effect is lower – but still significant – for new workers. In principle, these socialization patterns could be driven by selection: that is, by the process through which new workers are allocated to blocks. However, several considerations suggest that selection is unlikely to be driving the patterns documented in Table A5. First, recruitment and placement of new workers is centralized at the factory level. Management assigns workers to blocks and machines when they first arrive at the factory and neither existing workers and supervisors nor the new workers themselves have much say. Once allocated to blocks and machines, workers stick to them: change of machines is extremely uncommon in the data and change of blocks is altogether non-existent. Second, we would not expect to see a smaller block effect for new workers if it were simply the case that existing workers attract friends to their blocks. Third, the interaction coefficient – between block and being new – is unchanged with the inclusion of both worker and peer fixed effects (see Columns 2 and 3).[28]

Columns 5-7 of Table A5 focus on workers in the same block and show that they are more likely to socialize if they have worked together longer or if they are closer in age. Column 5 shows that a one s.d. increase in tenure overlap at the factory is associated with a 12.5% increase in the probability of socializing.[29] Column 6 shows that a one s.d. increase in the age gap between workers is associated with a 3.5% decline in the probability of socializing. Finally, Column 7 shows that tenure overlap and age gap matter even after controlling for the spatial distance between workers.

We also find that whether a peer is oriented towards a worker's front or back is an important predictor of socialization. This is true even conditional on the peer being one-worker distance away. The left panel of Figure A3 in the Appendix reports the probability of interacting with a peer multiple time a day conditional on the peer's orientation. The right panel reports the equivalent for socialization outside the factory. In both cases, the probability of interaction is significantly lower if the peer is in back of the worker, as opposed to in front or to the side. This result is not surprising since a worker needs to turn around

[28]Unfortunately, we cannot perform the same analysis for workers hired before the unrest. However, conversations with the factory management confirm that the hiring and allocation processes described here applied to those workers as well.

[29]In principle, this positive association could be driven by the hiring of new workers after the unrest; however, we obtain similar results when we exclude new workers.

to interact with a peer behind him, while peers in front or to the side are in his line of sight and can thus be talked to without slowing down work. Our finding that visibility of peers matters resonates with previous studies that also use relative locations of workers (e.g. Mas and Moretti (2009)) – although the underlying mechanism here is different.

To sum up, the relative location of workers and their block identities not only predict the intensity of interactions on the production floor; they also predict more significant social attachments.

2.5 Data

We use administrative data on the monthly production of all workers in the Manual Knitting Section for the period June 2013 to December 2014. The data contain information on the number of sweaters of a given style produced by each worker, the technical specifications of each style (including SMV), and details of the final payments made to each worker. This data is matched to other administrative records about workers, including tenure at the factory, age, attendance records, and, for workers no longer at the factory, the dates of quitting or firing.

Table 1 shows descriptive statistics about the production and firing. We present the statistics at the point of firing (April 2014). As can be seen from the top panel of the table, there were 15 blocks in the Manual Knitting Section at the time of the firings, with a total of 406 workers or an average of 27 workers per block. A total of 101 workers were fired, about 7 workers per block on average; the actual number fired ranged from 2 to 14 per block.

The bottom panel reports statistics about production, attendance, and tenure of the surviving workers. The first row reports average monthly earnings and the second row reports average monthly production (with each style weighted by SMV, as discussed in Section 2.2). Mean monthly attendance is 25.51 days (the factory is open 6 days per week, which is common in Bangladesh). The average worker tenure at the time of the unrest was 63 months (standard deviation of 19 months). We complement the internal production and administrative records with information that we collected from the factory ourselves. Besides the surveys and qualitative observations described above, we code the exact locations of fired and surviving workers.

3 Does the Firing of Peers Lower Productivity?

This section asks whether the firing of peers lowers the productivity of surviving workers. We first discuss why, theoretically, the answer to this question is a priori ambiguous. We then define our measure of exposure to firing for surviving workers and introduce our baseline difference-in-difference specification. We find that a one standard deviation higher exposure to firing reduces productivity by two days' worth of production per month. The next section investigates potential mechanisms underlying this effect and examines how the factory eventually responded.

3.1 Hypothesis

We see three main channels through which the layoffs might have impacted surviving workers' productivity.

1. **Intimidation.** Workers most likely felt pressured and intimidated by the firings. They probably feared more for their jobs and took management's threats more seriously. In response to this intimidation, workers might conceivably have raised their productivity.

2. **Demoralization.** On the flip side, workers may have experienced a loss of morale. Workers in our context had been working at the factory for more than five years on average (see Table 1) and formed strong peer attachments (see Figure 1). Consequently, surviving workers lost a lot of friends as a result of the firings. Their fondness for management also probably declined. If the firings caused workers to value their jobs less, it would be natural for them to respond by working less hard. Stress due to a deteriorating workplace could negatively affect productivity. It is also conceivable that the firings led workers to believe that they would be subjected to arbitrary punishments – and that their effort would not be recognized and rewarded. This too would be a reason to decrease effort.

3. **Desire to punish the firm.** Finally, workers may have been angered by the firings – or felt that the firings violated a relational contract. In response, they may have shaded performance as a means of punishing the firm.[30] Notably, the main reason for punishing the firm in this case is mistreatment of peers. A variety of experiments have demonstrated people's willingness to punish "altruistically" on behalf of third parties (see Fehr and Gächter (2002)). Moreover, there appears to be a particular willingness to punish on behalf of people or groups with whom one identifies (see Bernhard et al. (2006)).[31]

[30] See Gibbons and Henderson (2012) and Li and Matouschek (2013) for relational contracting models where such punishments can arise. Hart and Moore (2008) and Akerlof (2016) develop models where contract violations lead to anger and shading of performance.

[31] The efficiency wage literature argues that firms need to manage both morale and anger; fair treatment of workers is essential both for maintaining morale and for preventing anger (see, e.g., Akerlof and Yellen (1990), and Bewley (1999)).

Demoralization and anger – both potential reasons for a decline in productivity – are intrinsically difficult to distinguish from one another as they concern workers' mental states. Moreover, they are not mutually exclusive. Nonetheless, the distinction is a potentially important one from the factory's point of view as angry workers – in contrast to workers who are simply demoralized – may engage in deliberate acts of sabotage. There is some hope of distinguishing anger from demoralization precisely because sabotage is a distinct characteristic of angry workers.

3.2 Exposure to Firing

Motivated by the contextual evidence in Section 2.4 and the theoretical hypotheses in Section 3.1, we construct a measure of exposure to firing. We define a surviving worker's exposure as the number of (likely) friends fired by the factory. We take our cue from the social-network analysis in Section 2.4 and exploit the fact that workers are more likely to be socially connected to peers from their own blocks and to peers located nearby. Crucially for this exercise, we know which workers were fired (as opposed to voluntarily left the factory).

A first measure of exposure to firing is the total number of workers fired from one's block. The higher the number of workers fired from one's block, the higher is the number of (likely) friends lost. As noted above, however, not all workers from one's block are equally likely to be friends. Furthermore, a block-level measure of exposure makes it hard to distinguish individual responses to firing from block-level effects on productivity (e.g., the general impact on the block of losing workers or changes in block supervisor attitudes).

A second, and preferred, measure weights each of the fired workers from one's block by their spatial distances to a surviving worker. To construct this measure, we take advantage of the production floor map depicting the locations of all workers before the firings.[32] For each surviving worker i, the (weighted) exposure to firing is defined as:

$$E_i = \sum_{j \in \boldsymbol{B_i}} \frac{F_j}{D_{ij}},$$

where $\boldsymbol{B_i}$ is the set of co-workers in the block of worker i, F_j is a binary variable taking value 1 if co-worker j is fired and zero otherwise, and D_{ij} is the Euclidean distance between worker i and co-worker j. The greater the spatial distance between a pair, the lower is the likelihood of social interaction (as well as the expected strength of social attachment). The definition of E_i implies that the measure takes into account both whether a fired peer was from the same block, and how spatially close he was to a surviving worker. The probability

[32] For the floor map see Figure A4 in the Appendix.

that a fired worker was a friend increases on both these dimensions.

Note that while E_i is our baseline measure of exposure to firing, we will investigate alternative measures that weight distance D_{ij} differently, including the total number of workers fired from the block, or the number of workers fired at each distance within the block. Conditional on distance, we will also exploit workers' orientation and discontinuities at block borders.

3.3 Empirical Specification

We estimate within-worker changes in productivity through a difference-in-difference (DID) approach. Our baseline specification is given by

$$y_{it} = \alpha + \beta(E_i \times Post_i) + \theta_i + Month_t + X_{it} + \epsilon_{it}, \tag{1}$$

where y_{it} is productivity of worker i in month t. E_i is the worker-level exposure to firing defined in the previous section. $Post_t$ is equal to zero for months before April 2014, and equal to one for months after. β, the main parameter of interest, is the DID estimate of the effect of exposure to firing on worker productivity. θ_i is a worker fixed effect and $Month_t$ is a month fixed effect. X_{it} includes various controls depending on the specification. In the baseline specification we cluster standard errors at the worker level, but we also explore several robustness checks of this choice.

3.4 Main Results

In this subsection we report our baseline estimates of the effect of firing on the productivity of the surviving workers. We first report the baseline estimates from Equation 1. We then verify that our measure of exposure captures the effect from loss of friends, rule out alternative stories, and explore mechanism in the next section.

We begin by estimating Equation 1 using the total number of peers fired from the block, an unweighted measure of exposure to firing, in Column 1 of Table 2. The outcome variable is monthly production – our main productivity measure. Consistent with our hypothesis, there appears to be a strong negative association between the number of fired peers from the block and the change in productivity between the pre- and post-firing period. An additional worker fired from the block is associated with a drop in productivity of 460 minutes' worth of production per month. This amount is a little less than a day's worth of production (517 minutes, Table 1). Considering the average number of workers fired from each block is 6.7, this is a substantial drop: in the average block, productivity of survivors decreased by an average of 6 days per month worth of output for the six months that followed the firings.

Column 2 introduces our baseline spatially-weighted measure of exposure to firing E_i. We standardize the variable for ease of interpretation. A one standard deviation (s.d.) increase in exposure to firing reduces post-firing productivity of workers by more than 1,400 minutes' worth of production per month; this is equivalent to more than two-and-a-half days' worth of work.[33,34]

Column 3 includes worker fixed effects, ruling out concerns that the effect is driven by selection of less productive workers into higher exposure to firing. The specification also includes additional controls such as year-month fixed effects (that control for seasonality), the number of days in the month that the worker was not given any work (no-work days), and the amount of money earned from producing sample sweaters (sample sweaters are not included in our measure of monthly production). The magnitude of the drop in productivity is slightly attenuated but remains economically and statistically significant.[35]

A concern in interpreting the DID estimates is that workers' exposure to firing might be correlated with other factors that generate the same differential drop in productivity across workers. The most plausible factors are those associated with the initial selection of workers to workstations and, in particular, the kind of workers sitting next to those that end up being fired. For example, workers might sit next to people with whom they are already friends; rabble-rousers might tend to work next to other rabble-rousers. Our understanding of the process through which the factory hires and assigns workers to workstations, supported by evidence on newly hired workers presented in Section 2, suggests that there is little scope, if any, for selection along those lines. We will nevertheless dig deeper into some of these issues after presenting results from the baseline specification. For example, Section 4.1 will indeed show that it is the loss of friends that drives the drop in productivity. From the point of view of our main result, it is not crucial whether workers stationed next to each other were already friends before joining the factory, or whether they became friends socializing at the factory. Section 4.3 will investigate, among other things, whether the workers' similarity to fired workers is associated with a differential drop in productivity.

[33]The negative coefficient on the variable *Post* suggests that this effect is on top of a general decrease in productivity by relatively less exposed workers. Worker productivity might have fallen in the post-firing period for all workers, regardless of exposure. The negative coefficient on *Post*, however, could potentially also be due to confounding factors other than the firings, such as seasonality in production efficiency.

[34]The table reports standard errors clustered at the worker level (p-value $< 1\%$). Results are robust to alternative assumptions regarding the structure of the error term. Unreported estimates yield: wild bootstrap, clustering at Block level (p-value $< 2\%$); Wild bootstrap, clustering at the $Block \times Month$ level (p-value $<2\%$); spatial correlation using Conley (1999) estimators with conservative distance cutoff of 3 (p-value $<1\%$); two-way clustering at the worker and block month (p-value $<0.1\%$). For simplicity, the remainder of the analysis reports only estimates using the clustering at the worker level.

[35]The number of days worked and amount earned on sample styles are potentially "bad controls" because they are endogenous outcomes related to exposure to firing. Specifications in which they are omitted yield slightly larger effects than those reported in the table.

For the time being, we assuage some potential concerns by checking whether productivity evolved differently across workers with high and low exposure to firing before the firing incident. Figure 2 confirms that there was no differential trend in productivity before the firings across workers with different exposure to firing. The figure plots the lead and lag coefficients of exposure to firing for every month from June 2013 until December 2014. Note that we drop the earlier period of unrest, February to March 2014, but we will come back to it later; we also omit the period April to May 2014 when the factory was closed following the unrest. The coefficients of exposure to firing are close to zero in most of the pre-firing months and are always statistically insignificant. As our estimates in Table 2 already revealed, there is a sharp drop in productivity after the firings. This drop persists, largely unabated, for several months after the firing incident. The drop begins to evaporate after December 2014, more than 6 months after the firing incident. We will return to this timing at the end of the next Section, when we try to understand what the factory management did to win back angered workers.

Column 4 of Table 2 investigates how the drop in productivity translates into foregone earnings. Total monthly earnings from production is now the outcome variable. This serves to provide both an estimate of the loss in income that highly exposed workers suffer in the post-firing period (relative to less exposed workers), as well as a robustness exercise had we used a more traditional measure of productivity. Column 4 suggests that a one s.d. increase in exposure to firing led to a drop in wages of 482 Bangladeshi Taka (BDT) per month, slightly more than a day's earnings for a typical worker (395.8 BDT, see Table 1).[36] This impact is about half the one estimated on physical productivity in Column 2, suggesting that the factory might have allocated more remunerative styles to highly exposed workers. We investigate style allocation in the next section. Column 5 additionally includes worker and year-month fixed effects, and controls for no-work days and payment received for sample production. The estimated magnitude is largely unaffected.

Table 2 suggests that higher exposure to firing led to lower productivity in the post-firing period. To what extent is the drop in productivity driven by lower effort at work as opposed to fewer days at work? To answer this question, we check the effect of exposure to firing on the average time-value of production per attendance day (intensive margin) and the total number of days a worker was absent in a month (extensive margin). The results are reported in Table A6 in the Appendix. Column 1 shows that workers who were more exposed to firing were less productive than others even conditional on coming to work (a one s.d. increase in exposure leads to a 41 output-minutes loss in output per day). Column 2 shows that they were also absent more often; a one s.d. increase in exposure to firing leads to a 4%

[36] 1 USD $\approx$ 80 BDT

increase in absenteeism based on their pre-firing mean absent days in a month (2.24 days); this estimate is, however, not statistically significant at conventional levels.

4 Why Does Exposure to Firing Lower Productivity?

Having established that higher exposure to firing reduces productivity, we investigate in this section the mechanisms involved. First, we provide evidence suggesting that it is the loss of peers to whom workers are likely socially connected – in short, *friends* – rather than the loss of peers that are simply located nearby that drives the drop in productivity. Second, we consider (and rule out) several explanations for the loss in productivity of surviving workers: (i) lost opportunities to learn from or receive help from fired workers, (ii) time spent helping newly hired workers, (iii) on-the-job search, and (iv) displeasure carried over from the period of unrest. Our preferred interpretation, supported by the analysis of quality defects, is that the drop in productivity in the post-firing period is driven by surviving workers' desire to punish the factory for firing many of their peers. The section concludes with some additional evidence in support of this interpretation and a discussion of how the factory won back workers in the long-run.

4.1 Is the Effect Driven by Loss of Friends?

Our measure of exposure to firing depends upon both the number of workers fired from a survivor's block and the distances of those workers from the survivor. Instead of social connections, the results in Table 2 might reflect two alternative channels: disruption in production that persisted after the firing episode i) at the block level (e.g., the firing of a block supervisor) and/or ii) spatially clustered (e.g., damage to a group of machines).[37] Table 3 investigates whether the drop in productivity is driven by the loss of peers to whom survivors are likely socially connected (henceforth friends) as opposed to these alternative channels. To do so, we pursue a number of additional tests rooted in the evidence on socialization in Section 2.4: we exploit block boundaries, workstation orientation, and overlap in tenure across workers.

To disentangle social connections from block-level disruption we differentiate fired peers based on their distances from the surviving workers. For each worker i we construct "circles" of nearby workers: "Circle 1" contains all workers who are one-worker distance away from worker i, "Circle 2" contains all workers who are two-worker distance away, and finally,

[37] We specifically check whether any of the drop is driven by the firing of a block's supervisor in Table A8 Column 2. We find no effect of fired supervisors. However, there could be other block-level disruptions (e.g., in distribution).

"Circle 3" contains all other workers in the block.[38] This allows us to test the effect of distance holding the number of same-block peers fired constant.

Column 1 of Table 3 confirms that the effect of firing a peer is largest when the peer is located one-worker distance away. Firing a peer from Circle 1 reduces post-firing productivity of a surviving worker by 900 minutes' worth of production per month, while firing a peer from Circle 2 reduces post-firing productivity by about 400 minutes. Firing a peer from elsewhere in the block leads to a drop of only 200 minutes' worth of production. The difference in effect size between Circle 1 and Circle 2 is not statistically significant (p-value = 0.23), but the difference between Circle 1 and Circle 3 is (p-value = 0.03). Peers from Circle 1 have a 50% chance of being friends (see Section 2.4). The estimate thus suggests that the firing of a friend leads to (900 minutes)/0.5 $\approx$ 3 days of lost work per month. The magnitude is 2 days of lost work per month when using peers from Circle 2.

To disentangle social connections from effects related to physical proximity – for example, damage during the unrest to machines located nearby – we exploit boundaries across blocks and the orientation of workstations. In Section 2 we noted that, holding constant spatial proximity, these dimensions are associated with stronger social ties. In Column 2 we use our spatially-weighted measure of exposure to firing, but now we also compute the measure separately with respect to peers fired from outside the block. Firing peers from outside the block seems to affect a survivor's post-firing productivity adversely, but by less than half as much as firing peers from the same block.[39] As a more rigorous test, in Column 3, we hold constant both the number and distances of fired peers, and vary only their block identities. We test whether the effect of firing a peer from Circle 1 is different when the peer is from the same block as opposed to another block. This, however, restricts the sample to workers who are at the borders of their blocks, and hence had at least one Circle-1 peer from a different block. The effect of firing an outside-block peer from Circle 1 is almost zero, and statistically different from the effect of firing a same-block peer from Circle 1.

A third location-based test differentiates Circle-1 peers in front or to the side from Circle-1 peers behind. Figure A3 revealed that workers were more likely to interact and socialize with peers in their line of sight. So, we now focus only on same-block peers fired from Circle 1 in Column 4. We find that the drop in productivity from firing Circle-1 peers is largely driven by the fired peers who were located in front or to the side – precisely the peers who are more likely to be friends.[40]

[38] A visual representation is provided in Figure A4 in the Appendix.

[39] Notice that we could not have performed this test without an individual measure of exposure, since the sum of the number of workers fired from the block and the number of workers fired from outside the block is constant (hence collinear with Post dummy).

[40] This test excludes workers who are at the very ends of the floor, since they did not have anyone working to their back. Given how machines are distributed on the production floor, every worker has at least one peer working to the front and one to the side.

Finally, Columns 5 and 6 exploit tenure overlap and age distances between fired and surviving workers. Table A5 showed that two workers are more likely to be friends if their tenure overlap is longer or their age gap is smaller. We thus test whether the drop in productivity from spatial exposure to firing is heterogeneous along these dimensions. For each survivor, we compute the average tenure overlap (as of March 2014) with fired peers from the same block; we standardize this average across all surviving workers. Column 5 shows that exposure to firing has almost double the impact on productivity when tenure overlap is one s.d. higher. In Column 6, we perform a similar exercise using average age distance instead of average tenure overlap.[41] The estimated positive coefficient (p-value 0.21) suggests that a survivor's productivity falls more in response to the firing of a peer of similar age.[42]

4.2 A Placebo: Exposure to Fired Workers versus Voluntary Quits

Table 3 suggests that the drop in productivity associated with exposure to firing is driven by the loss of friends. Before we move on to consider different potential explanations for this pattern, Table 4 investigates the extent to which it is the *firing* of friends, as opposed to the *quitting* of friends, that is associated with the decline in productivity.

A notable feature of our context is that we can distinguish workers who were fired from workers who voluntarily quit: alongside the 101 fired workers, 26 workers voluntarily quit in the few months following the unrest. Table 4 investigates placebo specifications where we use a measure of exposure to peers who voluntarily quit. Note that, unlike exposure to firing, exposure to quitting varies over time for a given individual in the post-firing months, as more workers quit. We thus focus on specifications that include both worker and month fixed effects.[43]

Table 4 reports the results. Columns 1 to 4 show no association between a worker's productivity and his exposure to quitting from Circle 1 of his block. The DID estimate of the effect of exposure to quitting (Column 1) is considerably lower and much noisier than the DID estimate of the effect of exposure to firing (Column 2). To hold constant the effect from exposure to firing, Column 3 uses only the post-firing period. The estimate is even smaller.

[41] Age distance between a surviving worker and a fired peer is calculated as the square of the difference in their ages (as of March 2014), divided by the average of their ages. (Standardized) Tenure Overlap and (standardized) Age Distance are fairly orthogonal to (standardized) Exposure; independently regressing them on the Exposure variable yield coefficients of -0.07 and 0.07 respectively. We do not have information on the age of some workers, and hence the sample size drops in Column 6.

[42] The results in Columns 5 and 6 are similar if instead of spatially weighted exposure to firing we simply use the number of workers fired from block.

[43] Unlike the firings, the 26 voluntarily quits are staggered across months. We consider quitting from April 2014 (the unrest month) onwards up until the second-to-last month of the post-firing period (so as to leave at least one month for any effect from voluntary quits to materialize). We set it to zero in the pre-firing months to obtain a DID estimate comparable to the exposure to firing.

Column 4 estimates the effect of exposure to quitting with exposure to firing included as a control. The estimated effect is still insignificant; the estimated effect of exposure to firing is similar to the baseline estimate in Table 2. In sum, we interpret the results in Table 4 in the spirit of a placebo: the negative productivity response documented above stems from peers who were fired, rather than peers who simply left the factory.[44]

To summarize, Tables 3 and 4 suggest that the drop in productivity is driven by the firing of friends. The effect is strong enough to counteract any positive effects the firings might have had on productivity (e.g., due to intimidation as discussed in Section 3.1). We now investigate a number of alternative mechanisms in greater detail. After ruling out certain mechanical channels, we provide evidence suggesting that the loss in productivity is most consistent with a desire to punish.

4.3 Alternative Explanations

In this sub-section, we consider (and rule out) several alternative explanations for the results: (i) lost opportunities to receive help from fired workers, (ii) time spent helping newly hired workers, (iii) resentment predating the firings, and (iv) on-the-job search. We also perform additional robustness tests to the baseline specification. The subsequent subsections investigate mechanisms underlying our results.

Lost Help. Friends might conceivably help each other out on the job (or, relatedly, learn from one another). Therefore, the drop in productivity when friends are fired could be due to a loss of help rather than a loss of social attachment.

Our observations of the production floor suggest that, although many interactions take place between workers, most are social in nature and not many involve production help. It is thus a priori unlikely that the loss of help could explain the significant drop in productivity.[45]

We do not have a direct measure of help between co-workers; therefore we investigate this mechanism indirectly.[46] First, if pre-firing productivity of a surviving worker depended on help from friends, we would expect to find a positive correlation between a worker's productivity and the number of friends around him. Column 1 of Table 5 therefore tests whether surviving workers who had more (same-block) peers surrounding them before the

[44]These results are also indicative that a number of mechanical channels (e.g., loss in help provided by peers, or time spent helping new workers) are also unlikely to be driving our main results. Nevertheless, we investigate more precisely those alternative channels in the next subsections.

[45]A measurement exercise conducted in 2016 reveals that co-operation among workers is quite rare. We conducted several 20-minute long observations of randomly selected groups of four neighboring workers (see Ashraf (2019) for details). More than 2,500 20-minute slots were observed and documented. Help between co-workers was observed in less than 9% of cases. In most of these cases, it was the block supervisor who provided help

[46]Note that to the extent that peers who voluntarily quit also provide help, the analysis above already suggests that loss of help is unlikely to be a driving force in the drop in productivity.

firings were relatively more productive. We focus on same-block Circle 1 peers since this is where help is most likely to come from (as verified by our production-floor observations). Notice that the number of Circle-1 peers varies depending on a worker's location on the floor. The correlation between number of peers around a worker and his productivity in the pre-firing period is small relative to the estimates in Column 1 of Table 3, and statistically insignificant. Of course, workers located at the borders of blocks could be different (e.g., in terms of their own productivity or how social they are) from workers with same-block peers on all sides. So, we conduct additional tests.

Observe that a worker requiring help should suffer a larger drop in productivity when more friends are fired.[47] As such, a worker should be less affected when only one of Circle-1 peer is fired compared to many. To test this hypothesis, we create dummies for each of the different number of peers fired, and estimate the effect on productivity of these dummies separately. The results are shown in Column 2. We find that the detrimental effect of losing one peer from Circle 1 is just as large as the average effect we estimated in Column 1 of Table 3. To go one step further, we restrict the sample in Column 3 to only those workers who had at least five same-block peers in Circle 1 before the firings (the maximum possible number is eight). These workers still had many alternative peers they could turn to for help if one was fired. For this sub-sample we find, if anything, an even larger drop in productivity when a single peer is fired. In Column 4, rather than restricting to a sub-sample, we use the full sample and interact the Post dummy with the number of Circle-1 peers in the pre-firing period. We obtain similar results to those in Column 2. In sum, the loss of friends does not appear to have reduced productivity of surviving workers merely through the channel of lost help.

Time spent helping new workers. Conversely, it is possible that the post-firing productivity of survivors fell because they were helping newly hired operators who replaced fired workers. The greater the number of fired peers, the greater the number of newly hired workers nearby, so survivors who lost more peers in the layoffs might be spending more time helping new co-workers. This could then be misinterpreted as a drop in productivity because of the firings.

The main empirical challenge to ruling out this alternative mechanism is that exposure to newly hired workers is highly correlated with initial exposure to fired workers. We overcome this challenge by taking advantage of the fact that new workers were hired to replace fired

[47]Notice that we do not measure exposure to firing based on the workers' actual friends, but only peers most likely to be friends (Circle-1 peers). Because we are using an implicit probability of friendship, each of the surviving peers in Circle 1 are perfect substitutes for the fired workers from Circle 1, as far as helping is concerned. If we had used a measure based on the number of actual friends who were fired, the remaining workers would probably not be perfect substitutes for fired friends.

workers in two waves over July 2014 to September 2014. We exploit the within-survivor time variation in exposure to new workers in Circle 1 of their own block and check how productivity of surviving workers changes over time as the number of newly hired workers changes. Column 1 of Table 6 shows that as the number of new workers increased over the two waves of hiring, productivity of the surviving workers actually increased. In Column 2, instead of a simple count measure, we use the percentage of new operators in Circle 1 to control for the intensity of any such help provision; the greater the share of new workers around, the more help would need to be provided. The coefficient is again positive, only more imprecisely estimated.

The fact that we see an increase in productivity among surviving workers as new workers get hired raises the possibility that workers were unproductive in the post-firing period simply because they needed other workers around them.[48] To test this hypothesis we check whether the DID estimate is attenuated by the introduction of new workers in the two waves. We first report in Column 3 the DID estimate with respect to only June–September 2014 (as opposed to June-December 2014, as we did previously). Then in Column 4 we introduce changes in the number of new workers around surviving workers. In other words, we exploit the within-worker variation in exposure to new workers over time, holding constant exposure to firing.[49] The estimate of firing exposure's effect on productivity does not decline when we control for exposure to new workers.

We conclude that the drop in productivity associated with having friends fired is not driven by having to spend time helping newly hired workers.

Pre-Existing Resentment and Rabble-Rousers. Another potential concern is that the drop in productivity among workers with high exposure to firing might reflect pre-existing resentment towards the factory – rather than the actual firing of friends. For this to be the case, given our DID strategy, two conditions would need to hold simultaneously: (i) workers with high exposure to firing would need to have had more pre-existing resentments than other workers; (ii) it would need to be the case that pre-existing resentments became more intense in the period immediately following the firings, but not before. This second condition seems quite unlikely on its face; nonetheless, we examine both of these conditions in turn and find no supporting evidence.

We start by examining whether workers with high exposure to firing were particularly resentful prior to the firings. This could have happened if workers sorted spatially, with fired workers locating near other rabble-rousers. Our understanding of how the factory assigns

[48] Alternatively, new workers might have been first placed around surviving workers on better trends.

[49] The variables counting number of new workers are equal to zero in the pre-firing period, so they effectively become DID estimates too.

workers to workstations suggests that there was little to no scope for sorting of this kind (see Section 2.4). Another possibility is that surviving workers with high exposure to firing might have become resentful as a result of social interaction with workers who were subsequently fired during the unrest period.

To shed light on this possibility, we compare the role of spatial and social proximity to fired workers during the unrest and after the firings. The idea is that, if the unrest is responsible for the productivity drop, the impact of spatial proximity to rabble-rousers (proxied by subsequently fired workers) should be enhanced by social proximity during the unrest, but not after the firings. In contrast, if it is the firings themselves, social proximity enhances the impact of spatial proximity after the firings, but not during the unrest.

Column 1 of Table 7 shows that there was indeed a drop in productivity among the spatially exposed surviving workers already during the unrest period in February-March 2014. However, this a period when workers who were later fired were likely already disruptive. It is not surprising that surviving workers around them would have been adversely affected by such disruption.

Columns 2-5 of Table 7 explore social proximity during the unrest and after the firings. Building on Table A5, we exploit tenure overlap as our measure of social proximity. Even columns consider surviving workers with above-median tenure overlap with fired workers in their block; odd columns consider surviving workers with below-median tenure overlap. Columns 2 and 3 consider productivity during the unrest period: the effect of spatial exposure on productivity during the unrest is similar for workers with above- and below-median tenure overlap with fired workers. Columns 4 and 5, however, show that higher tenure overlap exacerbates the effect of spatial exposure after the firings (p-value=0.052). The evidence is thus consistent with the post-firing drop in productivity being driven by the loss of friends after the firings – not with resentment acquired during the unrest period that correlated with subsequent exposure to firing.

Column 6 examines surviving workers who look similar to fired workers – and might have thus shared some of the same resentments. Recall that fired workers were (a) more likely to have had below-median productivity during the unrest period and (b) above-median tenure at the factory (see Table A2). Column 6 tests whether surviving workers with these same characteristics experienced a particularly large drop in productivity in the post-firing period and finds no evidence supporting this hypothesis.[50]

In summary, our analysis suggests that it is the actual firing of friends, rather than

[50] If anything, we find that workers with below-median productivity during the unrest period reduced productivity less after the firings – the opposite of what was hypothesized. This could not have been driven by different levels of exposure among the different groups of workers. Figure A5 shows that the distribution of exposure to firing was similar across the two groups. The evidence might be picking up an intimidation effect.

pre-existing resentment, that drives the productivity drop.

On-the-job Search. Surviving workers who had friends fired might also suffer a decline in productivity because they are searching for new jobs. They might do so for a variety of morale-related reasons: for example, they might find the job less enjoyable after their friends have been fired. On-the-job search could lower productivity either directly (they spend less time and/or they are more distracted) or indirectly (they are less motivated). We present suggestive evidence that this mechanism is unlikely to be quantitatively important.

Notice that we had shown earlier that the drop in productivity exists even conditional on showing up at work (see Table A6). This rules out that the drop is driven by workers not coming to work while looking for new jobs. Nonetheless, we test this hypothesis more systematically in Table A7.

Column 1 of Table A7 suggests that workers who eventually left the factory were indeed more likely to be absent after the firing episode than those who stayed until the end of our sample period. We differentiate between surviving workers who left on or before December 2014 and those who continued at the factory after December 2014 ("stayers").[51] Columns 2 and 3 verify that the drop in productivity among stayers was as large as the average overall drop we estimated in Table 2. We also check in Columns 4 and 5 whether the drop among stayers could be explained by demotivation from failure to find alternative jobs, proxying on-the-job search intensity with the number of days of absence during June-December 2014.[52] If the stayers were demotivated by failure to find jobs, we would expect a stronger drop in productivity among workers who were more likely to have been looking for jobs – whenever they came to work, that is. If anything, we find the opposite. In sum, on-the-job search does not appear to be an important driver of the productivity loss.

Other Tests and Robustness Checks. A potential concern is that our measure of exposure to firing is based on the production floor map after the management moved the knitting section to the new compound. As the move was recent, the map might in principle not capture well relationships that developed long before the unrest. We believe this is not a major concern for the following reasons. First, each machine is ordered on the production floor according to a sequence number. Workers kept their machines and management kept the sequence of machines (and thus the production floor layout) mostly intact after the move to the new compound. Second, to the extent that there were changes in the layout, this should lead to attenuation bias that works against us finding any effect from exposure. Nonethe-

[51] We are aware that this comparison is based on an endogenous choice and thus we present it in the spirit of suggestive evidence that might still be informative about mechanisms.

[52] To avoid a small cluster problem, we split workers based on whether they have 3 or more absent days during the period.

less, we also provide a formal test. Each worker is assigned one manual knitting machine, but each machine is part of a machine station that contains a pair of such machines. The two machines (and hence the workers) face each other. Because each worker is assigned a fixed machine, it is extremely unlikely that the pair of workers assigned to a machine station would change after the move even if the factory had moved around some machine stations. So, instead of defining exposure to firing based on all the original peers around a surviving worker, in Column 1 of Table A8 in the Appendix, we measure exposure based on whether only the front peer was fired or not. We find a similar drop in productivity. Furthermore, the estimated average drop from this front peer being fired, more than 3,000 minutes' worth of production, is much larger than the estimated effect with respect to firing any front peer from Circle 1, a little more than 1,200 minutes' worth of production (Column 4 of Table 3). This could mean that firing the very front peer mattered more than firing any other peer from Circle 1; but the larger effect might also reflect, at least in part, attenuation bias from measurement error in our baseline exposure to firing.

In Column 2 we test whether the supervisor of a block getting fired affects the estimated productivity drop and find that it does not. Finally, Columns 3 and 4 check whether the productivity drop is driven by worker characteristics (age and tenure) that might be correlated with exposure to firing. We find no such evidence.

4.4 Was the Productivity Drop a Morale Effect or Punishment?

Losing friends during the firings is associated with a drop in post-firing productivity. Having ruled out a few alternative explanations, we argue that there are two potential explanations remaining: 1) lower morale; 2) a conscious desire to punish the factory. Demoralization encompasses a number of narrower mechanisms, such as workers perceiving management behavior as unfair (Akerlof (1980); Akerlof and Yellen (1988)) or the workplace becoming less enjoyable (Shapiro and Stiglitz (1984)). If surviving workers were simply demoralized by the firings, they would not deliberately seek to punish the factory; they would only reduce their effort. Workers might follow a punishment strategy, however, if they considered the firings a violation of a relational contract (e.g., Gibbons and Henderson (2012) or Li and Matouschek (2013)) or if they were angered by them (see e.g., Hart and Moore (2008) and Akerlof (2016)). These explanations are not mutually exclusive. Furthermore, they are intrinsically difficult to distinguish from one another as they concern workers' mental states that are not directly observable. We nevertheless provide suggestive evidence of deliberate shading of performance by workers in order to punish the factory. Data on defect rates provides the most compelling evidence; but we also investigate heterogeneous responses across workers with different income levels.

Morale, Punishment and Quality Flaws. We first turn to information on the quality of sweaters that workers produce. As mentioned in Section 2.2, we observe two kinds of quality flaws: minor flaws that only require mending and serious defects. When there is a mending flaw, it is passed on to a separate group of mending operators. The worker can move on directly to a new set of sweaters and his pay is unaffected. When there is a defect, on the other hand, the worker must go back and fix it himself before going on to a new assignment. Therefore, mending flaws only hurt the factory while defects also hurt the worker.

If workers were simply demoralized, we would expect to see a similar increase in defects and mending flaws. If, on the contrary, workers were trying to punish the firm, we would expect to see a greater increase in mending flaws, which are only costly to the factory. We observe quality flaws for every batch of sweaters that workers produce ("tasks"). Unfortunately, we only have data on flaws starting in November 2013. We run similar regressions to those in Figure 2 where we tested for pre-trends; but now the outcome is the fraction of sweaters in a task that need mending on a set of more than 27,000 observed tasks. Again, we cluster errors at the worker level.

The left panel of Figure 3 considers mending. While there is no difference in mending rates between workers with high and low exposure to firing before the firings (no pre-trends), mending rates shoot up among highly exposed workers after the firings. The right panel in Figure 3 considers defects. We confirm the absence of pre-trends. After the firings, defect rates increase by a much smaller amount and only in certain months. The results in Figure 3 are also reflected in Table 8, where we regress the mending and defect rates on exposure to firing, interacted with a dummy for each post-firing month. Column 1 of Table 8 shows that the firings led to an increase in mending rates for highly exposed workers; this effect persists for the first four months after the firings. Column 2 shows that, by contrast, the firings do not have a consistent, positive effect on defect rates; the only two positive and significant coefficients are much smaller in magnitude.

Discussion of Alternative Interpretations. We take this as the first piece of suggestive evidence that workers with higher exposure to firing might be punishing the factory. The evidence is, of course, not fully conclusive as alternative interpretations are, in principle, possible.

First, it is in theory possible that the factory reclassified defects as mending to appease workers. Our understanding of the production process suggests that this is unlikely to be the case. Such reclassification of defects into mending is unlikely because mending flaws and defects are technologically very different from one another. Mending flaws are fixed by hand by different mending operators using single needles while defects are fixed by workers using their knitting machines. If the factory were to reclassify them, they would be taking the risk of delivering faulty sweaters to the buyers – potentially a substantial cost in terms of reputation and future revenues. Note also that, if the factory did reclassify defects, they did so in a way that targeted workers whose productivity fell as a result of the firings. This would support the conclusion we draw in Section 4.5 that the factory took steps to repair its relationship with workers affected by the firings.

A separate question is why workers would be willing to reduce productivity but not be willing to waste more time through defects. A possibility is that management tracks defects in real time (and so knows who the rebel is) while productivity drops are measured at the end of the month and are thus harder to detect and hold against a worker. In that case, the differential behavior of productivity and defects is also consistent with strategic behavior on the part of the worker. This consideration also assuages concerns that mending errors might be more vulnerable to demoralization than defects. It is nevertheless possible that demoralization leads to a loss of attention that results in smaller flaws that require mending, but not in more severe quality defects. We cannot rule out mechanisms along these lines conclusively solely based on evidence about flaws.

Further Evidence. For these reasons, we also perform a second, admittedly more speculative, test. This test is inspired by the observation that demoralization is likely to be an involuntary response while punishment is more deliberate. We investigate whether workers respond differently to exposure to firing depending on how close they are to subsistence earnings. Conditional on exposure to firing, the hypothesis is that demoralization hit all workers equally. On the contrary, punishing the factory is an active choice on workers' part which entails withdrawing effort and foregoing income. Punishment is costlier for workers who earn relatively less (and thus they should punish less).

We split workers into income quartiles in the first three months of the sample period (June - August 2013) and use the remaining period (September 2013 - December 2014) for the subsequent analysis. The bottom quartile received average monthly production earnings ranging from 4,685 BDT to 8,556 BDT (figures for the top quartile are 11,470 BDT to 17,318 BDT). For comparison, the government-set minimum monthly wage in the woven garment sector during the period was 5,300 BDT (Kamal (2017); Paul (2018, September 13)).[53]

Column 1 of Table A9 confirms that the average drop in productivity in the shorter sample is similar to what we found for the full period in Table 2. Column 2 reveals that

[53] All workers received additional allowances and bonuses on top of the production earnings, but such bonuses are a small share of the total monthly income. Also, the earnings considered here include payments for samples, and additional compensation from damaged parts.

workers in the bottom quartile of earnings, closest to subsistence income, did not change their productivity in response to exposure to firing at all. On the other hand, for workers in the middle two quartiles, productivity dropped by 1,241 to 1,802 minutes' worth of production on average. The drop in productivity of the top quartile – about 3,000 minutes' worth of production – is the largest (the comparison is statistically significant). Column 3 runs a similar specification using monthly earnings as the outcome and finds a very similar pattern. The drop in earnings associated with one s.d. increase in exposure is equivalent to about 7% of the mean monthly earnings for the top quartile workers.

In sum, the drop in productivity is stronger for workers that could afford to punish. It is of course possible that poorer workers exerted more effort because they feared more for their jobs or because they differed in how they perceived the firings and/or in their social network. Nevertheless, combined with the evidence on mending and quality defects, we interpret this as suggestive evidence that workers exposed to the firings were, at least in part, withdrawing effort to deliberately punish the factory.

4.5 Factory's Response

We have documented a drop in productivity among surviving workers after the firings and given suggestive evidence that punishment of the factory might be one of the underlying mechanisms. Such punishment could have arisen because workers were angered by the firings or because they considered the firings a violation of a relational contract.

It would be natural for the factory – and most likely in its long-term interest – to try to repair the strained relationship with workers. Figure 2, which shows that productivity gradually increased over the six months following the firings, provides suggestive evidence that the relationship did improve. We briefly explore steps taken by the factory to improve the relationship.

Increasing piece rates is one strategy the factory might have used. However, we do not find any evidence that the factory increased piece rates on average. To check this, we define a measure of how profitable a style is to a worker (style rent) by dividing the style's piece rate by its SMV. Comparing the distributions of style rents before and after the firings, we find no evidence that the factory increased the style rents on average.

Alternatively, the factory could have tried to target higher compensation on workers who were more exposed to the firings. A particular way in which the factory could achieve this is by assigning more profitable styles – i.e., those with piece rates that are high relative to the style complexity – to these workers. Relative to more direct forms of compensation, this particular one has the advantage of being cheaper (piece rates are not increased across the board) and less transparent (i.e., less likely to be detected by workers that are left untargeted).[54]

We find suggestive evidence for this mechanism. We compute an average monthly style rent for each worker (equal to monthly earnings divided by monthly production). We regress the average monthly style rent on exposure to firing, interacted with a dummy for each post-firing month. Column 3 of Table 8 shows that, after the firings, workers that were more exposed received relatively more rewarding styles. Compared to workers with low exposure to firing, workers with high exposure started to receive more rewarding styles more often. The difference continues for most of the post-firing months, although it starts to fade in magnitude during the later months. From the point of view of our test, it is the timing that is particularly striking. While still statistically significant, the estimated effect more than halves *precisely* at the time in which the impact on mending defects fades away. The evidence in Table 8 is thus suggestive that the factory management did make an attempt to repair their damaged relationship with surviving workers. Their effort appears to have paid off as mending defects decreased and eventually, seven months after the firings, productivity almost fully recovered.

5 Conclusion

This paper offers a rare glimpse into the aftermath of an episode of labor unrest – a characteristic trait of industrial relations in countries with emerging manufacturing sectors. Our main finding is that the mass layoff of disruptive workers in a large Bangladeshi sweater factory was associated with a significant reduction in the productivity of surviving workers. We document this novel fact exploiting a combination of detailed personnel and production records from the factory, and ethnographic and survey evidence on the socialization process on the production floor.

The evidence sheds light on the social organization of the workplace and on the importance of healthy industrial relations in emerging markets. With regard to our understanding of workplaces, we found that it is the firing of peers with whom workers had social connections – *friends* – that is particularly associated with a drop in productivity. We also documented evidence consistent with a deliberate shading of performance by workers in order to punish the factory's management, and a corresponding deliberate attempt by the factory to win workers back. The reason for punishing the firm appears to have been *mistreatment of peers*. This would be consistent with a view of the firm as a web of interconnected relational agree-

[54] Fahn and Zanarone (2020) argue that transparency is costly when workers engage in social comparison. Ashraf (2019), studying the same sweater factory, provides evidence that workers in our context do indeed engage in social comparison – and that these comparisons have a significant effect on productivity.

ments supported by workers' willingness to punish "altruistically" on behalf of third parties – a willingness supported by social connections.

Episodes of labor unrest are very common – albeit hard to observe and study – particularly in countries with emerging manufacturing sectors. Our evidence points to the importance of healthy industrial relations for firms' productivity. The evidence is at least in principle consistent with the possibility that factory unions – which are in many countries discouraged if not altogether repressed – might provide a formal institutional means of committing factories to treat workers fairly, thereby avoiding the costs associated with unrest and lost productivity. As new manufacturing hubs emerge, factory unions might also facilitate the establishment of multilateral relational arrangements across workers, like the one identified in this paper. Of course, the establishment of factory unions could alter labor-management relationships – and affect firm performance – in a variety of ways. A better understanding of the effect of unions in emerging economies is a priority for future research.

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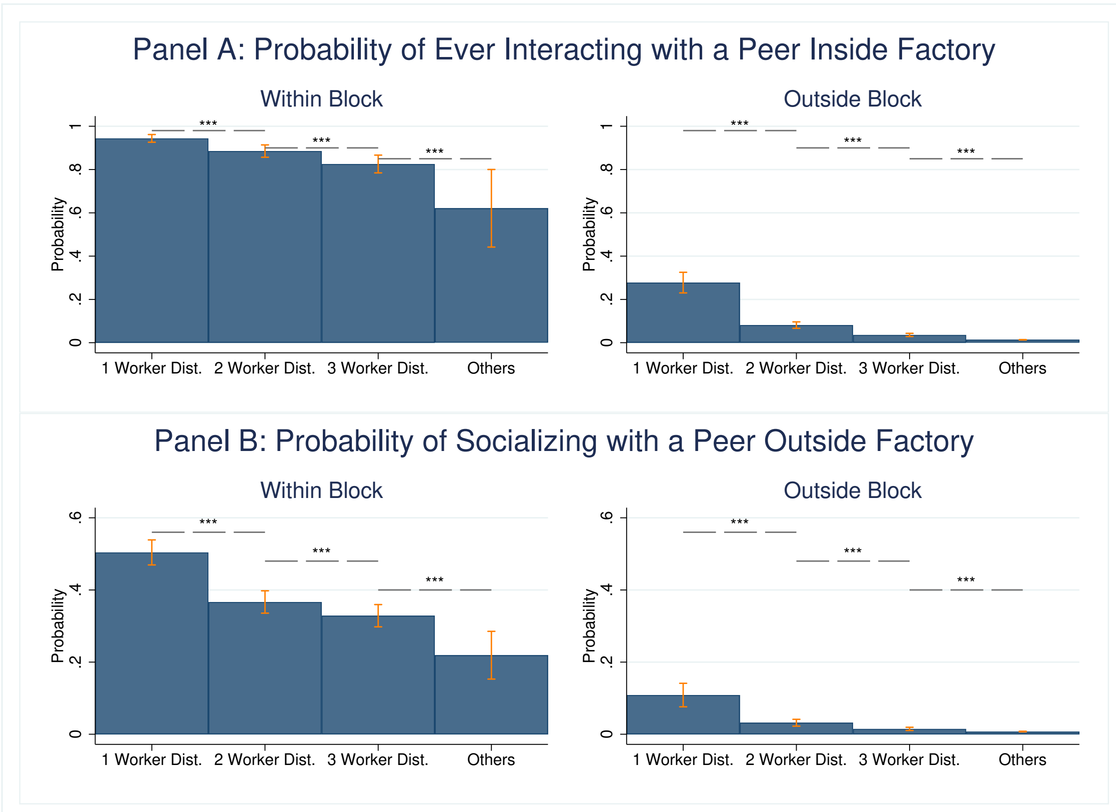
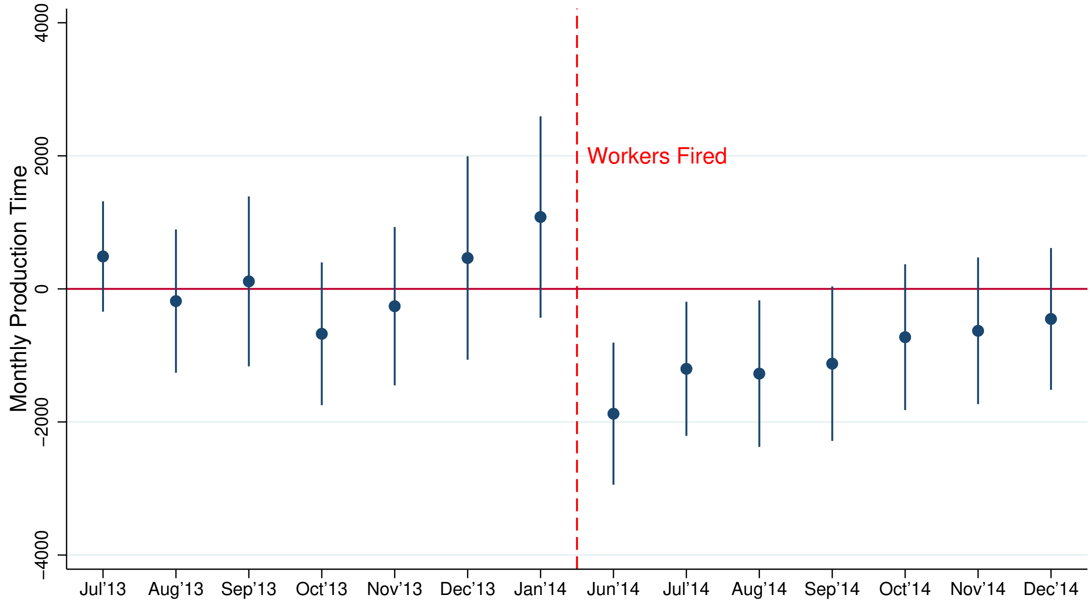
Figures & Tables

Figure 1: Interactions and Socialization Among Workers



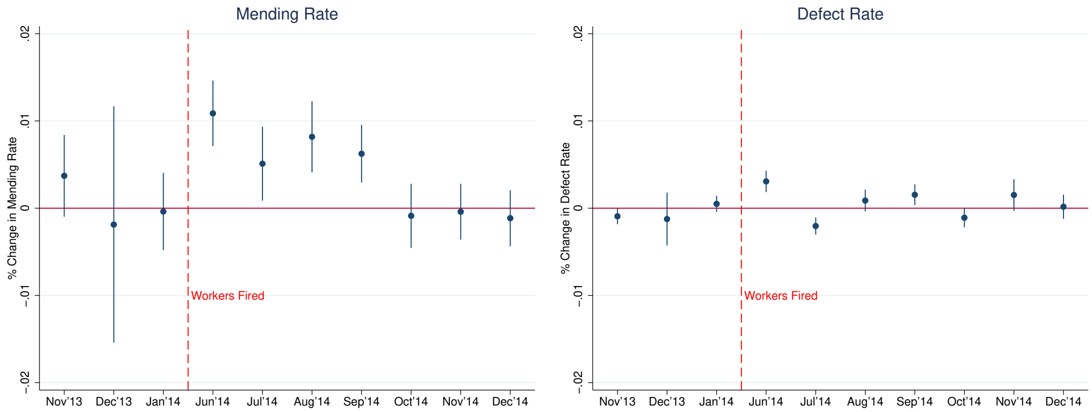
Notes: This figure reports probabilities of a worker ever talking with a peer inside the factory (Panel A) or socializing with a peer outside the factory (Panel B) when the peer is from different worker-distances away from him on the factory floor. The left sub-panels compute the probabilities for peers from one's own block, while the right does it for peers from outside his block. The data used are from a network survey conducted in Oct'15. Underlying regressions are linear probability models with no constants; standard errors are clustered at worker level. Each observation in the regression model is a pair of workers; a total of 9,664 pairs were used in the within-block regressions and 123,926 pairs for the outside-block regressions.

Figure 2: Test of Pre-Trend



Notes: This figure checks how monthly production of surviving workers varies with respect to exposure to firing in months preceding and following the firing of workers in Apr'14. The outcome variable, production, is the total production time of a worker in a month calculated from his total physical output in the month and estimated SMV for the sweaters he worked on. Exposure to firing is standardized spatially weighted exposure to firing. Additional controls include number of days workers were not given any work, total payment for sample production, worker fixed effects, and month fixed effects. The horizontal axis depicts every month in the sample period, except for Feb'14–May'14, when the factory was either going through labour unrest or was closed. The dashed line represents the event of workers getting fired in Apr'14. The vertical lines represent 95% confidence intervals.

Figure 3: Treatment Effect on Quality



Notes: This figure checks how quality of production of surviving workers vary with respect to exposure to firing in months preceding and following the firing of workers in Apr'14. *Mending Rate* (left panel) refers to the share of a worker's total production that had small errors that were instead passed on to mending operators. *Defect Rate* (right panel) refers to the share of a worker's total production that had errors that the worker had to fix himself. Exposure to firing is standardized spatially weighted exposure to firing. Our data on quality consists of a limited set of months shown in the figure. The dashed line refers to Apr'14 when workers were fired. The vertical lines represent 95% confidence intervals.

Table 1: Descriptive Statistics

	n	Mean	Std. Dev.	Min	Max
Number of Workers Per Block	15	27.07	5.3	9	30
Number of Workers Fired Per Block	15	6.73	3.56	2	14
Total Number of Workers Fired in Apr' 2014	101				
Total Number of Workers Retained after Firing	305				
(Non-standardized) Exposure to Firing: Same Block	304	2.57	1.56	0.41	7.02
Survivors	n	Mean	Std. Dev.		
Monthly Production Wages (Jun'13-Mar'14)	2,922	10,097.67	3,822.68		
Time-Value of Monthly Production	2,919	13,198.06	8,885.01		
Total Attendance Days in Month (Jun'13-Mar'14)	2,922	25.51	4.49		
Tenure (months) in Mar'14	305	63.3	19.16		

Notes: The sample period spans from Jun'13 - Dec'14.

Table 2: Effect of Exposure on Firing and Productivity

	(1) Monthly Production	(2) Monthly Production	(3) Monthly Production	(4) Monthly Earnings	(5) Monthly Earnings
Post * (# Fired in Block)	-461.5***				
	(80.80)				
(Exposure: Same Block) * Post		-1,483***	-1,284***	-482.4***	-291.5***
		(255.7)	(256.5)	(85.34)	(72.40)
Exposure: Same Block		866.4***		-143.3	
		(258.3)		(109.9)	
# Fired in Block	181.5**				
	(78.45)				
Post	1,709***	-1,261***		-58.02	
	(598.8)	(261.4)		(87.31)	
Observations	4,134	4,119	4,119	4,123	4,123
Number of Workers	305	304	304	304	304
Worker FE	N	N	Y	N	Y
Year-Month FE	N	N	Y	N	Y
Other Controls	N	N	Y	N	Y

Notes: *Monthly Production* in Cols. 1-3 (res. *Monthly Earnings* in Cols. 4-5) refers to total monthly production time (res. earnings) calculated from total physical output and estimated SMV (res. piece rates and production). *Exposure: Same Block* refers to standardized spatially weighted exposure to firing. *Post* is a dummy variable which is equal to one after Jun'14 and to zero before. *Other Controls* include number of days in a month when a worker was not given work, and total payment for sample production which are not accounted for in a worker's monthly production. All regressions include constants. Standard errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 3: Is the Effect Driven by Social Connections?

	(1)	(2)	(3)	(4)	(5)	(6)
	Monthly Production	Monthly Production	Monthly Production	Monthly Production	Monthly Production	Monthly Production
			Borders	Non-Ends		
(# Fired from Circle 1, Same Block) * Post	-900.2***		-1,218***			
	(277.7)		(388.6)			
(# Fired from Circle 2, Same Block) * Post	-402.2**					
	(196.8)					
(# Fired from Circle 3+, Same Block) * Post	-211.0**					
	(104.1)					
(Exposure: Same Block) * Post		-1,349***			-1,415***	-1,146***
		(258.8)			(233.4)	(261.9)
(Exposure: Other Blocks) * Post		-570.4**				
		(226.2)				
(# Fired from Circle 1, Other Blocks) * Post			363.5			
			(338.4)			
(# Fired from Circle 1 Front, Same Block) * Post				-1,266***		
				(417.5)		
(# Fired from Circle 1 Back, Same Block) * Post				-311.7		
				(442.6)		
(Exposure: SB) * Post * (Tenure Overlap)					-1,090***	
					(315.9)	
(Exposure: SB) * Post * (Age Distance)						326.6
						(261.8)
Observations	4,104	4,119	2,216	2,908	4,119	3,886
Number of Workers	303	304	162	213	304	287
Circle 1 = Circle 2	[0.229]					
Circle 1 = Circle 3 on	[0.020]					
Same Block = Other Blocks		[0.014]	[0.001]			
Front = Back				[0.151]		
Worker FE	Y	Y	Y	Y	Y	Y
Year-Month FE	Y	Y	Y	Y	Y	Y

Notes: *Monthly Production* is total monthly production time calculated from total physical output and estimated SMV. Col. 3 considers workers who are located at the borders of their blocks. Col. 4 considers workers who have peers at their front and back. *Circle 1* refers to peers one-worker-distance away and *Circle 2* refers to two-worker-distance; the rest are pooled together in *Circle 3+*. *Exposure: Same Block* is standardized spatially weighted exposure to firing. *Post* is a dummy variable equal to one after Jun'14 and to zero before. *Tenure Overlap* is the average duration of tenure overlap with all fired workers from the same block, standardized across survivors. *Age Distance* is the difference in ages between a survivor and a fired peer, divided by the average of their ages, and then standardized. All regressions include constants, and control for number of days in a month when a worker was not given work, and total payment for sample production which are not accounted for in a worker's monthly production. Column 5 includes interaction of *Post* dummy with *Tenure Overlap*, Column 6 includes a similar interaction with *Age Distance*. Square brackets at the bottom of the table show p-values for corresponding tests of hypothesis. Standard errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 4: Placebo with Quitting Peers in the Post-Firing Period

	(1) Monthly Production	(2) Monthly Production	(3) Monthly Production Post Period	(4) Monthly Production
# Quitting from Same Block, Circle 1	-230.4		-51.56	199.5
	(916.2)		(388.0)	(933.5)
(# Fired Peers from Circle 1, Same Block) * Post		-1,061***		
		(248.0)		
(Exposure: Same Block) * Post				-1,294***
				(257.2)
Observations	4,116	4,119	1,807	4,101
Number of Workers	305	304	293	304
Worker FE	Y	Y	Y	Y
Year-Month FE	Y	Y	Y	Y

Notes: *Monthly Production* refers to total monthly production time calculated from total physical output and estimated SMV. Col. 3 considers only post-firing period. *# Quitting from Same Block, Circle 1* is the number of peers from same block and Circle 1 who quit during Apr'14–Nov'14. *Circle 1* refers to peers one-worker-distance away. *Exposure: Same Block* refers to standardized spatially weighted exposure to firing. *Post* is a dummy variable which is equal to one after Jun'14 and to zero before. All regressions include constants, and control for number of days in a month when a worker was not given work, and total payment for sample production which are not accounted for in a worker's monthly production. Standard errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 5: Alternative Explanation - Help from Fired Peers

	(1) Mean Production Pre-Unrest	(2) Monthly Production	(3) Monthly Production	(4) Monthly Production
			# C1 Peers>=5	
# Peers in Circle 1 (Pre-Firing), Same Block	97.45			
	(164.6)			
(# Fired from Circle 1, Same Block = 1) * Post		-1,339**	-2,330***	-1,509**
		(600.7)	(818.6)	(610.0)
(# Fired from Circle 1, Same Block = 2) * Post		-2,829***	-2,798***	-3,048***
		(754.8)	(833.5)	(796.4)
(# Fired from Circle 1, Same Block = 3+) * Post		-2,231**	-2,427**	-2,645***
		(884.0)	(1,040)	(1,014)
(# Fired from Circle 2+, Same Block) * Post		-272.2***	-427.4***	-215.5*
		(95.66)	(136.3)	(112.3)
(# Peers in Circle 1 (Pre-Firing), Same Block) * Post				111.6
				(203.1)
(# Peers in Circle 2+ (Pre-Firing), Same Block) * Post				-79.75
				(114.1)
Observations	304	4,119	2,703	4,119
Number of Workers	304	304	200	304
Worker FE	N	Y	Y	Y
Year-Month FE	N	Y	Y	Y

Notes: *Monthly Production* refers to total monthly production time calculated from total physical output and estimated SMV. *Mean Production Pre-Unrest* is the average monthly production during Jun'13-Jan'14. Col. 3 considers only workers who had at least five peers within one-worker distance. *Circle 1* refers to peers one-worker-distance away, while the rest are pooled together in *Circle 2+*. All regressions include constants; Cols. 2-4 include controls for number of days in a month when a worker was not given work, and total payment for sample production which are not accounted for in a worker's monthly production. Standard errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 6: Alternative Explanation - Old Workers Help New Workers

	(1)	(2)	(3)	(4)
	Monthly Production	Monthly Production	Monthly Production	Monthly Production
	Jun'14-Sep'14	Jun'14-Sep'14	Jun'13-Sep'14	Jun'13-Sep'14
# of New Workers in Circle 1	365.1*			287.9
	(206.2)			(207.8)
# of New Wokers in Rest of the Block	159.0			3.431
	(96.90)			(77.86)
% of New Workers in Circle 1		1,365		
		(1,390)		
% of New Workers in block except Circle 1		5,091**		
		(2,074)		
(Exposure: Same Block) * Post			-1,544***	-1,741***
			(270.6)	(308.7)
Observations	1,171	1,159	3,470	3,468
Number of Workers	296	291	304	304
Worker FE	Y	Y	Y	Y
Year-Month FE	Y	Y	Y	Y

Notes: *Monthly Production* refers to total monthly production time calculated from total physical output and estimated SMV.*New Workers* refers to workers hired after firing. *Circle 1* refers to peers one-worker-distance away. *Exposure: Same Block* refers to standardized spatially weighted exposure to firing. *Post* is a dummy variable which is equal to one after Jun'14 and to zero before. All regressions include constants, and control for number of days in a month when a worker was not given work, and total payment for sample production which are not accounted for in a worker's monthly production. Standard errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 7: Alternative Explanation – Pre-existing Resentment

	(1)	(2)	(3)	(4)	(5)	(6)
	Monthly Production	Monthly Production	Monthly Production	Monthly Production	Monthly Production	Monthly Production
	Post= During Unrest	Post= During Unrest	Post= During Unrest	Post= After Unrest	Post= After Unrest	Post= After Unrest
		Tenure Ov. <Median	Tenure Ov. >Median	Tenure Ov. <Median	Tenure Ov. >Median	
(Exposure: Same Block) * Post	-3,025***	-2,760***	-3,115***	-662.9**	-1,643***	-1,780***
	(346.3)	(482.6)	(474.0)	(262.3)	(375.3)	(396.8)
(Exposure: Same Block) * Post * 1(Unrest Period Production <Median)						1,713***
						(484.0)
(Exposure: Same Block) * Post * 1 (Tenure in Mar'14 >Median)						-426.3
						(429.3)
Observations	2,909	1,453	1,456	2,057	2,062	4,119
Number of Workers	304	152	152	152	152	304
Worker FE	Y	Y	Y	Y	Y	Y
Year-Month FE	Y	Y	Y	Y	Y	Y

Notes: *Monthly Production* refers to total monthly production time calculated from total physical output and estimated SMV. *Exposure: Same Block* refers to standardized spatially weighted exposure to firing. *Unrest Period* refers to Feb'14-Mar'14 when there were multiple protests at the factory. Cols. 1-3 consider only the unrest period as post-period in a dif-in-dif setup, while Cols. 4-6 consider Jun'14-Dec'14 as the post-period. Cols. 2 & 4 (res. Cols. 3 & 5) consider workers whose tenure overlap at the factory with fired workers was lower (res. higher) than the median among all surviving workers. *Post* is a dummy variable which is equal to one for months in post-period and to zero for months before. All regressions include constants, and control for number of days in a month when a worker was not given work, and total payment for sample production which are not accounted for in a worker's monthly production. Col. 2 includes interactions of *Post* with dummies *1(Unrest Period Production < Median)* and *1(Tenure in Mar'14 > Median)*. Standard errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

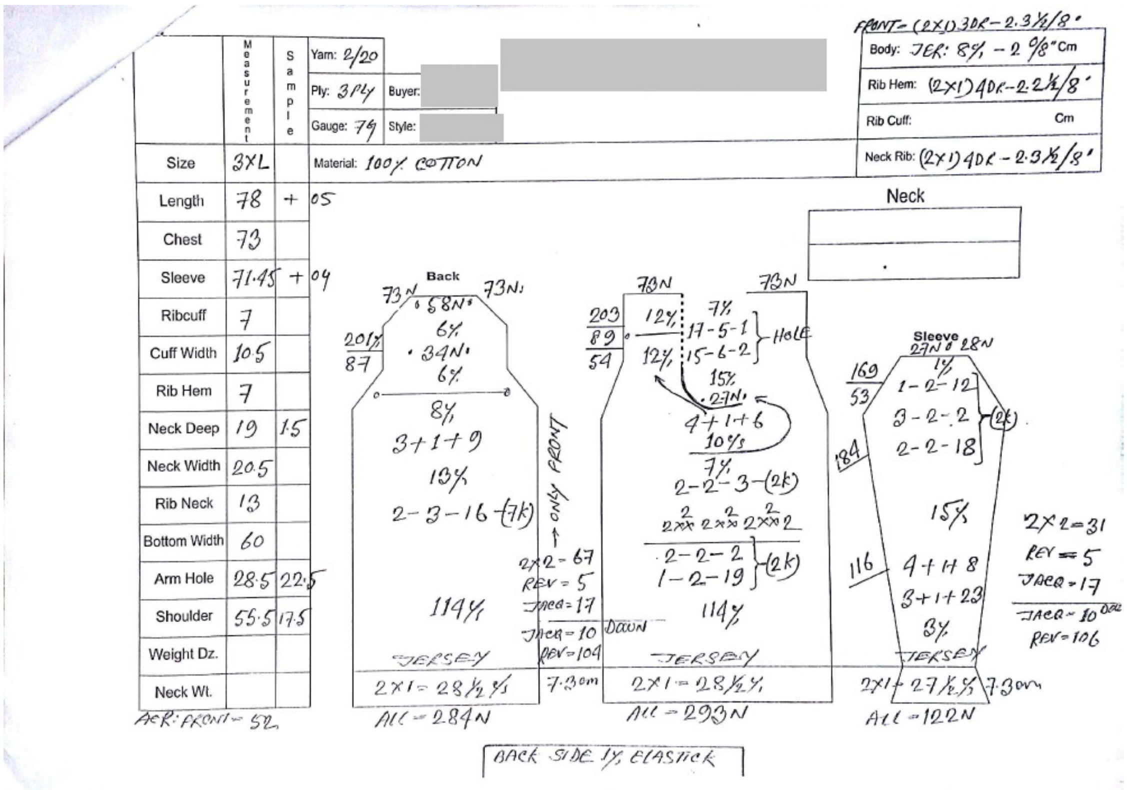
Table 8: Production Quality and Style Rent

	(1) Monthly Mending Rate	(2) Monthly Defect Rate	(3) Monthly Style Rent
Exposure: Same Block	-0.0005	-0.0002	-0.0254***
	(0.0018)	(0.0004)	(0.0038)
(Exposure: Same Block) * 1(Jun'14)	0.0088***	0.0033***	0.0543***
	(0.0019)	(0.0008)	(0.0074)
(Exposure: Same Block) * 1(Jul'14)	0.0036	-0.0019***	0.0606***
	(0.0024)	(0.0006)	(0.0108)
(Exposure: Same Block) * 1(Aug'14)	0.0081***	0.0009	0.0789***
	(0.0023)	(0.0007)	(0.0155)
(Exposure: Same Block) * 1(Sep'14)	0.0055***	0.0016**	0.0520***
	(0.0020)	(0.0006)	(0.0060)
(Exposure: Same Block) * 1(Oct'14)	-0.0014	-0.0008	0.0194***
	(0.0018)	(0.0007)	(0.0058)
(Exposure: Same Block) * 1(Nov'14)	-0.0017	0.0011	0.0179***
	(0.0021)	(0.0009)	(0.0060)
(Exposure: Same Block) * 1(Dec'14)	0.0018	-0.0005	0.0227***
	(0.0019)	(0.0009)	(0.0052)
Observations	27,076	27,076	2,655

Notes: *Mending Rate* refers to the share of a worker's total production that had small errors that were instead passed on to mending operators. *Defect Rate* refers to the share of a worker's total production that had errors that the worker had to fix himself. *Style Rent* is total monthly earnings divided by total monthly production time. *Exposure: Same Block* refers to standardized spatially weighted exposure to firing. *Post* is a dummy variable which is equal to one after Jun'14 and to zero before. The pre-firing months span Nov'13-Jan'14, limited based on availability of quality data. All pre-firing months are omitted category. All regressions include constants and dummies for post-firing months. Standard errors clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

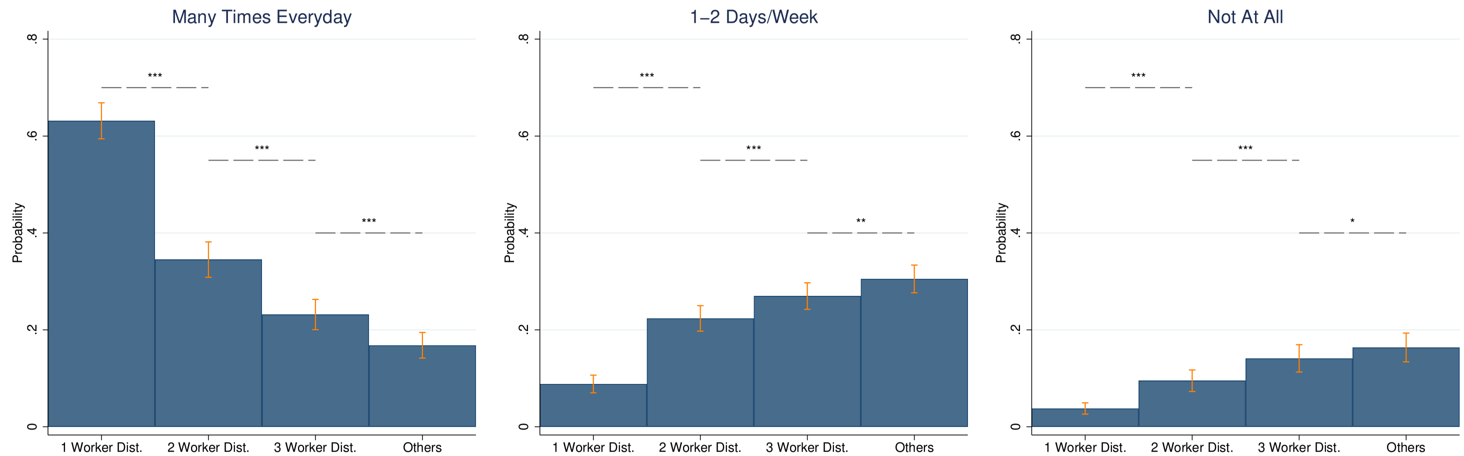
Appendix

Figure A1: Sample Design Chart



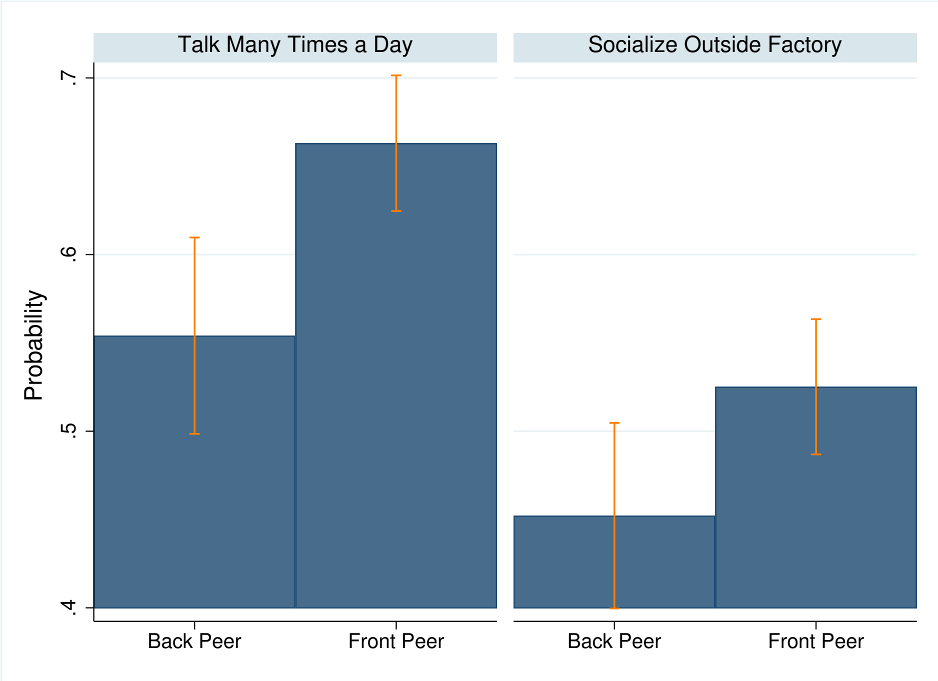
Notes: The figure shows a sample design chart of a sweater that the workers use to knit sweaters, and which we used to estimate SMVs for the corresponding sweaters.

Figure A2: Intensity of Interactions with Same-Block Peers inside Factory



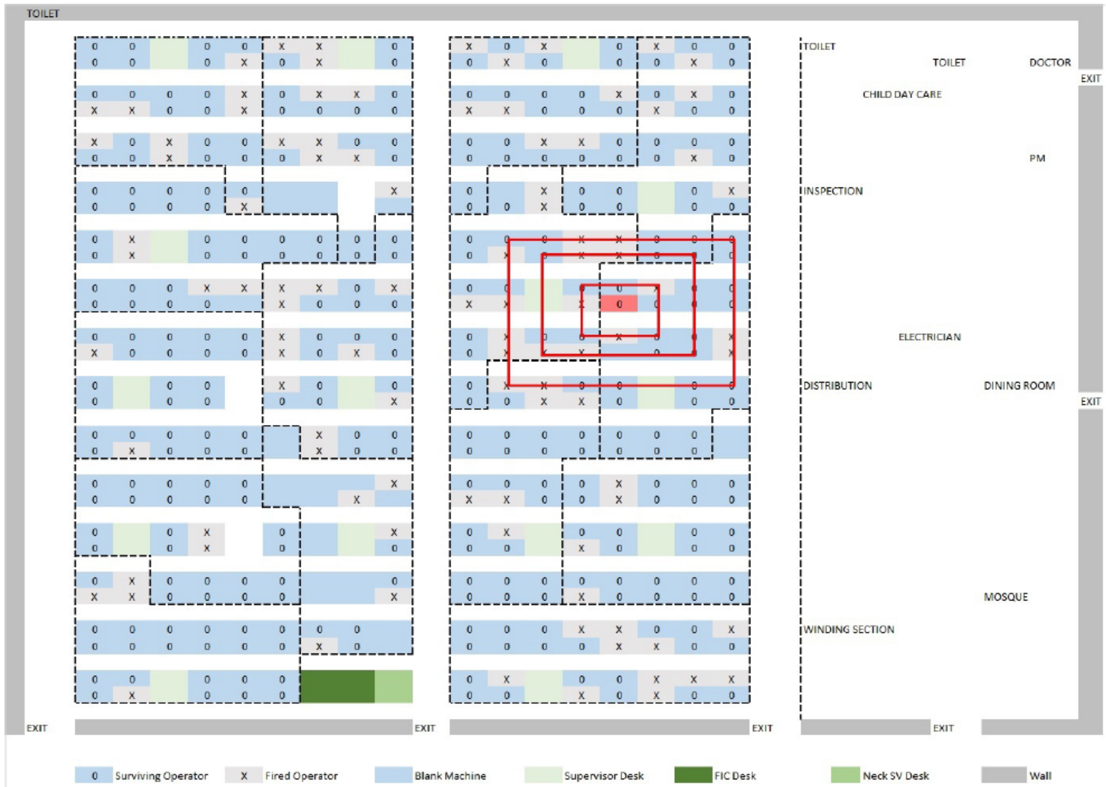
Notes: This figure reports probabilities of a worker talking with different intensities with a within-block peer inside the factory when the peer is from different worker-distances away from him on the factory floor. The probabilities are computed separately for interactions many times a day (left panel), 1-2 days a week (center panel), and not interacting at all (right panel). The probabilities are computed from a linear probability model with no constant and standard errors clustered at worker level, using data from a social-network survey conducted in Oct'15. Each observation in the regression model is a pair of workers, 8,507 in total.

Figure A3: Interactions & Socialization Within Same-Block Circle 1 Peers



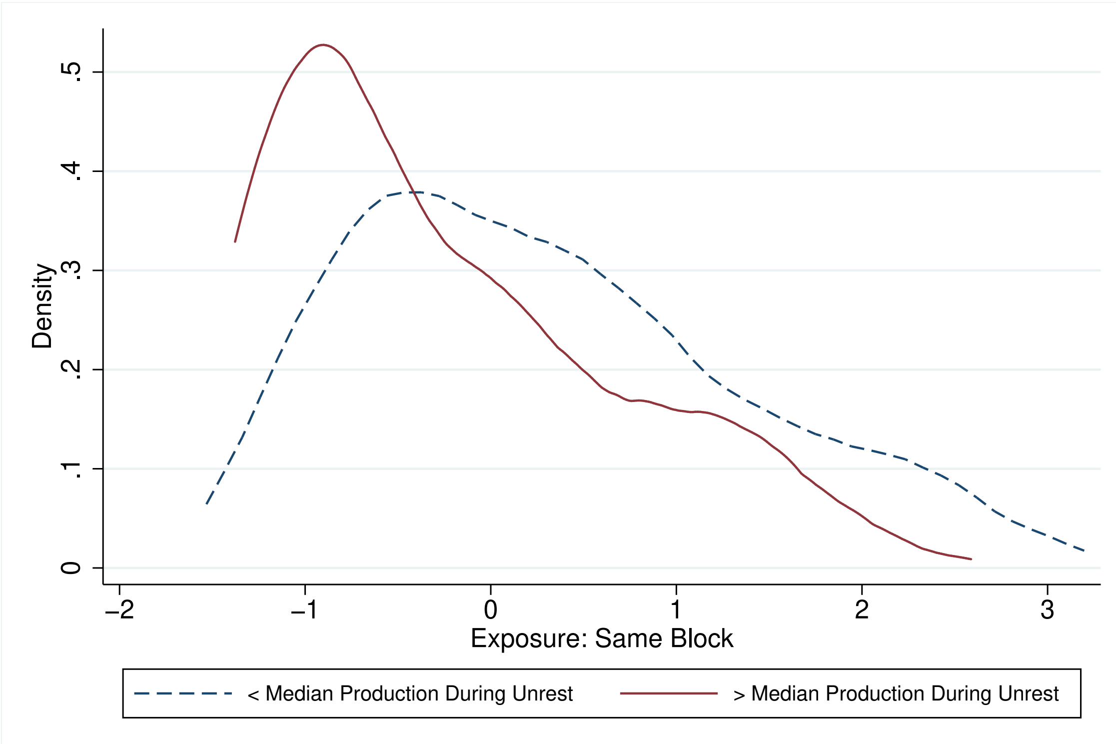
Notes: This figure reports the likelihood that a worker talks with high intensity (left panel) or socializes outside the factory (right panel) with a same-block circle-1 peer when the peer is to the front or back. The reported probabilities are computed from a linear probability model with no constant and standard errors clustered at worker level, using data from a social-network survey conducted in Oct'15. Each observation in the regression model is a pair of workers.

Figure A4: Floor Map



Notes: This figure depicts the floor map of the Manual Knitting section right before the firing of workers in Apr'14. O depict locations of surviving workers while X depict locations of fired workers. Each row of workers in a given (horizontal) shaded strip face workers in the paired row within the same strip. Dashed lines indicate block borders. Consecutive rectangles in solid lines depict the concept of *Circles* of peers around a surviving worker. The right-most side of the map shows locations of other sub-sections on the floor.

Figure A5: Distribution of Exposure to Firing



Notes: This graph shows the distribution of spatially weighted exposure to firing within block for two groups of workers – those who, during the period of unrest, had production lower than the median productive worker, and those who had production higher than the median productive worker. *Production* refers to total monthly production time calculated from total physical output and estimated SMV.

Table A1: Timeline of Key Events

Jun-13	Start of Sample Period
Feb-14	Factory Moves Compound; Protests
Apr-14	Worker Unrest and Firing; Factory Closes
May-14	Factory Re-opens
July-Sep-14	New Workers Hired
Dec-14	End of Sample Period

Table A2: Who Got Fired?

	(1)	(2)	(3)	(4)	(5)	(6)
	P(Fired)	P(Fired)	P(Fired)	P(Fired)	P(Fired)	P(Fired)
1(Pre-Unrest Period Earnings >Median)	-0.0369					
	(0.0431)					
1(Pre-Unrest Period Production >Median)		-0.0369				
		(0.0431)				
1(Unrest Period Earnings >Median)			-0.145***			
			(0.0424)			
1(Unrest Period Production >Median)				-0.165***		-0.144***
				(0.0422)		(0.0412)
1 (Tenure in Mar'14 >Median)					0.0836**	0.0903**
					(0.0417)	(0.0412)
Constant	0.268***	0.268***	0.322***	0.332***	0.177***	0.247***
	(0.0314)	(0.0314)	(0.0301)	(0.0299)	(0.0293)	(0.0352)
Observations	406	406	406	406	390	390

Notes: This table reports probability of a worker getting fired in Apr'14 based on his pre-firing period characteristics. The underlying regression model is a linear probability model. *Pre-Unrest Period* refers to Jun'13-Jan'14. *Unrest Period* refers to Feb'14-Mar'14. *Monthly Production* in refers to total monthly production time (res. earnings) calculated from total physical output and estimated SMV. *Monthly Earnings* refers to earnings calculated from piece rates and production. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A3: Anecdotal Evidence 1

Time	Description of Activity
5:20-5:21 PM	Went to the distribution room to collect elastic yarn.
6:19 PM	Talks to the operator to his back, Just a few words.
6:25 PM	Even though a song is playing on the PA system, one of the operator's cell phone is blaring a different song and 3-4 operators start singing with the song that is playing in the operator's mobile. This lasts approximately 20-30 seconds.
6:35 PM	A lot of short bursts of chitchat going on with and around the subject. The observer could not catch most of it. The work does not stop for these chats.
6:41 PM	Talks to operator to his right. Chitchat.
7:01-7:02 PM	Calls the Supervisor to his machine and supervisor does some adjustment in the machine
7:07 PM	Cleans his machine and leaves the floor for the day.

Notes: Anecdotal evidence shows interactions among workers are largely limited to peers located one-worker distance away. This is partly because the workers are stationed to their machines and partly because the floor is quite noisy from the usage of machines.

Table A4: Anecdotal Evidence 2

Time	Description of Activity
5:09 PM	Subject not in his station
5:30-5:56 PM	Subject arrives at his station and starts setting up his machine for a new style. A lot of non-work related chatting going on with the operator facing him.
6:00 PM	Operator another machine comes to the subject's station and borrows his operation breakdown.
6:12 PM	The operator to the subject's left comes to his station and helps him setup the machine. He gives hands on instruction for approximately 45 seconds.
6:16 PM	More small talk with the operators to his left and front. Subject is still setting up his machine.
6:17 PM	Subject finds that he forgot to change a part in the machine while setting it up for the new style that requires a different gauge. He tells that to the operator in front of him and starts changing it.
6:20- 6:27 PM	Subject fetches the supervisor to his machine. They talk about the technical stuff while the supervisor tries to tune the machine.
6:54 PM	Conversation with an operator to his front. Talks about the trouble he's having with his machine.
6:58 PM	Adjustments done and working with the machine starts.
7:00-7:01 PM	Takes a small sample of cloth he made to the supervisors, comes back in 30 seconds and compares his work with that of the operator to his left who is also doing a neck part.
7:07 PM	Cleans up and leaves the floor for the day.
7:07 PM	Observation Ends

Notes: Anecdotal evidence shows interactions among workers are largely limited to peers located one-worker distance away. This is partly because the workers are stationed to their machines and partly because the floor is quite noisy from the usage of machines.

Table A5: Probability of Socializing with Peers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	P(Soc.)	P(Soc.)	P(Soc.)	P(Soc.)	P(Soc.)	P(Soc.)	P(Soc.)
				1 Worker Dist.	Same Block	Same Block	Same Block
1(Same Block)	0.353***	0.410***	0.399***	0.391***			
	(0.035)	(0.015)	(0.015)	(0.033)			
1(Same Block) * 1(Peer is a New Worker)	-0.140***	-0.134***	-0.133***	-0.102*			
	(0.018)	(0.019)	(0.019)	(0.058)			
1(Peer is a New Worker)	-0.009***			-0.017			
	(0.001)			(0.049)			
Std. Spatial Distance			-0.007***				-0.265***
			(0.001)				(0.024)
Std. Tenure Overlap					0.125***		0.120***
					(0.014)		(0.014)
Std. Age Distance						-0.035**	-0.025*
						(0.014)	(0.014)
Constant	0.012***	0.049***	0.048***	0.132***	0.391***	0.565***	-0.020
	(0.001)	(0.015)	(0.015)	(0.028)	(0.106)	(0.097)	(0.115)
Observations	91,980	91,980	91,980	1,441	8,565	9,664	8,565
Row 1 + 2 =0	[0.00]	[0.00]	[0.00]	[0.00]			
Worker FE	N	Y	Y	N	Y	Y	Y
Peer FE	N	Y	Y	N	Y	Y	Y

Notes: This regression is from Social Network survey done in Oct'2015, and uses worker-worker connections as an observation. *Peer is a New Worker* implies the peer worker was hired after the firing. *Std. Spatial Distance* spatial distance between a pair of workers calculated as an Euclidean distance and then standardized. *Std. Tenure Overlap* refers to the average duration of tenure overlap with all fired workers from the same block, which is then standardized. *Std. Age Distance* refers to a similar measure only with respect to age distance, where age distance is calculated as difference in ages between a survivor and a fired peer, divided by the average of their ages. Standard Errors clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A6: Intensive vs Extensive Margins of Response

	(1) Monthly Production/Day	(2) Monthly Leave+Absent
(Exposure: Same Block) * Post	-41.04***	0.0905
	(7.850)	(0.0834)
Observations	4,116	4,123
Number of Workers	304	304
Worker FE	Y	Y
Year-Month FE	Y	Y

Notes: *Monthly Production/Day* refers to average production time per attendance day. *Absent Days* refers to the sum of pre-authorized and unauthorized absent days. *Exposure: Same Block* refers to standardized spatially weighted exposure to firing. *Post* is a dummy variable which is equal to one after Jun'14 and to zero before. Col. 1 includes controls for number of days a worker was not given work and total payment for sample production. All regressions include a constants. Standard errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A7: Alternative Story - Survivors Look for New Job

	(1)	(2)	(3)	(4)	(5)
	Total Absent Days	Monthly Production	Monthly Production	Monthly Production/Day	Monthly Production/Day
	Jun-Dec'14	Leavers	Stayers	Stayers	Stayers
				Abs+Leave<=3	Abs+Leave>3
1(Left in or before Dec'14)	3.750***				
	(0.531)				
(Exposure: Same Block) * Post		-1,092	-1,287***	-82.79***	-24.87***
		(1,267)	(262.5)	(14.01)	(8.469)
Observations	1,826	299	3,820	542	3,276
Worker FE	Y	Y	Y	Y	Y
Year-Month FE	N	Y	Y	Y	Y
Number of Workers	297	27	277	50	227

Notes: *Total Absent Days* refers to the sum of pre-authorized and unauthorized absent days during Jun-Dec'14. *Monthly Production* refers to total monthly production time calculated from total physical output and estimated SMV. *Monthly Production/Day* refers to monthly production per attendance day. *Leavers* refers to workers who left the factory before the sample period ended in Dec'14, while Stayers refers to those who were there till the end. *Exposure: Same Block* refers to standardized spatially weighted exposure to firing. *Post* is a dummy variable which is equal to one after Jun'14 and to zero before. All regressions include a constants. Col. 2-5 includes controls for number of days a worker was not given work and total payment for sample production. Standard errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A8: Robustness Checks

	(1) Monthly Production	(2) Monthly Production	(3) Monthly Production	(4) Monthly Production
1(Front Worker Fired) * Post	-3,162***			
	(848.8)			
(Exposure: Same Block) * Post		-1,294***	-1,168***	-1,141***
		(257.6)	(218.8)	(258.2)
1(Supervisor Fired) * Post		89.71		
		(531.3)		
Tenure in Mar'14 (Std.) * Post			-1,890***	
			(290.6)	
Age in Mar'14 (Std.) * Post				-910.6***
				(254.0)
Observations	4,044	4,119	4,119	3,886
Number of Workers	299	304	304	287
Worker FE	Y	Y	Y	Y
Year-Month FE	Y	Y	Y	Y

Notes: *Monthly Production* refers to total monthly production time calculated from total physical output and estimated SMV. *1(Front Worker Fired)* is a dummy variable that is equal to one if a peer working right in the front on the same machine-station was fired, and zero otherwise. *Exposure: Same Block* refers to standardized spatially weighted exposure to firing. *Post* is a dummy variable which is equal to one after Jun'14 and to zero before. *1(Supervisor Fired)* is a dummy variable that is equal to one if the supervisor for a worker's block was fired. *Tenure* and *Age* are caculated as of Mar'14 and standardized. All regressions include a constants. Col. 2-5 includes controls for number of days a worker was not given work and total payment for sample production. Standard errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A9: Productivity Drop by Earnings Quartiles

	(1) Monthly Production	(2) Monthly Production	(3) Monthly Production
(Exposure: Same Block) * Post	-1,337***	77.81	30.92
	(311.5)	(434.9)	(131.1)
(Exposure: Same Block) * Post * Mean Jun-Aug'13 Earnings Quartile = 2		-1,802**	-147.2
		(846.9)	(182.8)
(Exposure: Same Block) * Post * Mean Jun-Aug'13 Earnings Quartile = 3		-1,241*	-461.0**
		(645.8)	(195.6)
(Exposure: Same Block) * Post * Mean Jun-Aug'13 Earnings Quartile = 4		-3,004***	-867.4***
		(833.5)	(204.5)
Observations	3,210	3,210	3,211
Number of Workers	304	304	304
Worker FE	Y	Y	Y
Year-Month FE	Y	Y	Y

Notes: *Monthly Production* in Cols. 1-2 (res. *Monthly Earnings* in Cols. 3) refers to total monthly production time (res. earnings) calculated from total physical output and estimated SMV (res. piece rates and production). *Exposure: Same Block* refers to standardized spatially weighted exposure to firing. *Mean Jun-Aug'13 Wages Quartile* refers to splitting workers according to their average monthly wages from Jun-Aug'13; these months are dropped from the subsequent sample over which the regression is run. *Post* is a dummy variable which is equal to one after Jun'14 and to zero before. All regressions include constants, and control for number of days a worker was not given work, and interactions of *Post* dummy with the wage quartiles; Columns 1-2 also control for total payment for sample production. Standard errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.