Layoffs and Productivity at a Bangladeshi Sweater Factory

Robert Akerlof, Anik Ashraf,
Rocco Macchiavello, Atonu Rabbani*

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Conflicts between management and workers are common and can have significant impacts on productivity. Combining ethnographic, survey and administrative records from a large Bangladeshi sweater factory, we study how workers 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 likely had social connections – friends – that matters. Additional evidence on defect rates suggests a deliberate shading of performance by workers in order to punish the factory's management.

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*Akerlof: University of Warwick, r.akerlof@warwick.ac.uk. Ashraf: LMU Munich, anik.ashraf@econ.lmu.de. Macchiavello: London School of Economics and Political Science, r.macchiavello@lse.ac.uk. Rabbani: University of Dhaka and BRAC James P Grant School of Public Health, atonu.rabbani@du.ac.bd. Acknowledgements: We are particularly indebted to Christopher Woodruff, with whom this project was started. We thank Mollah I. Fazlul, Shuvangkor S. Roy, and Tingfang Xiao for their excellent research assistance. We also thank Oriana Bandiera, Jordi Blanes i Vidal, Erika Deserrano, Florian Englmaier, James Fenske, Guido Friebel, Robert Gibbons, Jin Li, Imran Rasul, Eric Verhoogen, Fabian Waldinger, Noam Yuchtman, Giorgio Zanarone, and seminar participants at Barcelona GSE Summer Forum, CERGE-EI, CUNEF, Goethe U. Frankfurt, JKU, LMU Munich, LSE, and U. of Warwick. Financial support from ERC Advanced Grant RMPGG and IPA SME Initiative is gratefully acknowledged.

1 Introduction

On July 18, 2012, following months of labor unrest and strikes, workers at the Maruti Suzuki car plant in Manesar, India erupted into violence, setting fire to the factory and attacking and injuring nearly 100 managers. Serious labor disputes like this one are not uncommon in emerging industrial countries such as Bangladesh, China, India, and Vietnam and they typically result in the mass layoff of workers. Such layoffs may be an effective means of quelling unrest – but they also potentially have a long-term impact on productivity. While layoffs could raise productivity of surviving workers by removing troublemakers or by intimidating workers with fear of further firing, they could also lower productivity, for example by reducing workers' morale or inducing retaliation. Empirical evidence can foster our understanding of how worker-management relationships affect productivity and inform policy debates on industrial relations. Such evidence, however, has proven elusive as it is difficult to access firms during episodes of conflict and both layoffs and worker productivity are typically hard to measure.[1]

This paper offers a rare glimpse into an episode of labor unrest. We study the Manual Knitting Section of a large Bangladeshi sweater factory before, during, and after a period of unrest where management fired roughly a quarter of the workers.[2] Records from the factory, depicted in Figure 1, reveal that production per day in the six months following the layoffs was a quarter lower compared to the same period a year before. Around two-thirds of the observed drop is due to lower productivity of surviving workers, with the rest accounted for by the temporary reduction in the number of workers and the initially lower productivity of newly hired workers. Did the mass firing contribute to the lower productivity of surviving workers? To go beyond the descriptive comparison in Figure 1, we combine detailed individual-level production data with ethnographic and survey evidence on workers' location and socialization processes on the production floor. Through this "insider econometrics" approach (Ichniowski and Shaw (2009)), we define worker-specific measures of exposure to the firing and study how they relate to changes in individual productivity

[1] The events at the Maruti Suzuki plant are described in, e.g., Prasad (2012). For accounts of similar cases of unrest, see, e.g., The Economist on January 31, 2015, April 26, 2014, and January 28, 2012 on China; The Economist on June 7, 2014 on Vietnam; The Economist on February 7, 2015 and The Guardian on January 14, 2019 on Bangladesh; Reuters on January 10, 2019 on Cambodia.

[2] Knitwear is the largest export sector in Bangladesh. The sector counts around 2,000 plants. The factory in this study is one of the largest exporters in the country.

before and after the unrest within a difference-in-difference framework.

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 likely 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 first provide detailed background information on the production process, the labor unrest and firing episode, and the socialization process in the factory. In the 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 productivity of individual workers over time.[3] 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 closely located peers, we find 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.[4]

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. 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

[3]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, across worker comparisons are possible by converting physical units of output into a common metric using each sweater part's standard minute values (SMVs).

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

involvement in the violence.

We define individual measures of exposure to firing to examine how the mass layoff impacted surviving workers' productivity. Our baseline measure of a surviving worker's exposure weights 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 subject these results to a number of additional tests. A first concern is that the drop in productivity is due to the absence of co-workers nearby. We distinguish workers who were fired by the factory from those who voluntarily quit and show that the results are driven by exposure to the former group, not the latter.[5] A second concern is that a worker's position on the production floor might affect (changes in) productivity through channels other than the firing. Inspired by Borusyak and Hull (2021), we simulate mass firings at both the factory and the block level to purge our measure of exposure of the component that is due to the workers' location on the floor and estimate effects that are essentially identical to our baseline specifications. Finally, our measure of exposure could simply capture the long shadow of the unrest: firing of co-workers will be higher among rabble-rousers and/or among those that carry stronger resentment towards the factory from the unrest period. Due to the factory's closure for a large part of the unrest period, we have no information about individual workers' involvement in the unrest (other than the list of fired workers) and it is therefore difficult to decisively rule out this mechanism. To partially assuage concerns, we show that the results are robust when allowing for a worker's own drop in productivity during the unrest period and for his similarity to fired workers to differentially affect the worker's productivity after the unrest.

[5] This evidence, supplemented by additional tests, rules out a number of competing channels, including 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. Given the spatial nature of our analysis and the small number of production blocks, we also show robustness of the results to alternative inference methods.

We also attempt to explore the mechanisms through which the firing of friends could lead to a persistent drop in productivity among surviving workers. Such analysis is, inevitably, suggestive: alternative explanations are certainly not mutually exclusive and workers' mental states are intrinsically difficult to tease apart. Nevertheless, a distinction might be drawn between explanations that presume an intention to harm the firm on the part of the workers from those that do not. Under *demoralization*, workers lower their productivity, as the layoff of friends causes morale to wane. Under *anger and punishment*, workers purposefully lower productivity, as they are motivated, out of anger or a sense that relational contracts have been violated, to shade performance.[6] In contrast to workers who are simply demoralized, angry workers may engage in deliberate acts of sabotage. We find that workers exposed to the firing had higher rates of mending defects (which are fixed by the factory at no cost to the worker) but no higher rates of quality defects (which must be fixed by the worker). Although alternative explanations are certainly possible, this is suggestive of deliberate shading of performance by workers to punish the factory. We also provide suggestive evidence of a corresponding deliberate attempt by management to win the angry workers back by selectively giving them more rewarding tasks.

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.[7] 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. (2021) find that workers engage in counter-productive actions after a pay cut; Coviello et al. (forthcoming) 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 an extensive but mostly qualitative management literature (see, e.g., Brockner et al. (1987), Cascio (1993), Mishra

[6]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), Li and Matouschek (2013), and Breu et al. (2014) on relational contracts.

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

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. (1993a) and Brockner et al. (1993b)). We contribute by providing quantitative evidence from a workplace.[8]

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); Levin (2003)), evidence on relational contracting within firms remains largely anecdotal. By definition, relational contracts rely on informal exchanges of promises that are rarely recorded and thus difficult to measure.[9] Levin (2002) models the trade-off between multilateral relational contracts (in which the firm makes commitments to all workers) versus bilateral relational contracts in which the firm makes commitments to individuals. Multilateral contracts are more effective in binding the firm to its commitments but are difficult to adjust when the environment changes. To the extent that a relational contract was in place, the evidence rejects both the purely bilateral and the fully multilateral relational contract and suggests that the underlying social connections play an important role. Taken together, the evidence is consistent with 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)).[10]

Finally, our paper is related to a growing empirical literature on human resources management and industrial relations in developing countries. Cai and Wang (2020) find that letting workers evaluate managers lowers turnover and increases productivity in an automobile manufacturing firm in China. 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

[8]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.

[9]Blader et al. (2020) infer the importance of relational contracts in the workplace from workers' negative response to the introduction of competition-based performance incentives when there exists a strong norm of cooperation between peers.

[10]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.

to a misallocation of managerial talent.[11] 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.[12]

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 presents robustness checks. Section 5 investigates mechanisms. Section 6 concludes.

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 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.[13] Workers in this section manually operate machines to knit yarn into sweater parts

[11]Examples of other recent work in the garment sector include Boudreau (2020) on safety committees; Adhvaryu et al. (2020) on relational contracts between line supervisors; and Adhvaryu et al. (forthcoming) on workers' voice and retention.

[12]Ashraf et al. (2015) document the prevalence 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 industrial relations are not confined to the manufacturing 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.

[13]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.

that are later passed on to the Linking Section to be stitched together. Each worker has an assigned machine, stationed at a designated location on the factory floor (see Figure A5 for the map). The machines come in pairs and the workers in each pair face each other. 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.[14] The block supervisors are supervised by one Floor-In-Charge who, in turn, is supervised by the Production Manager.

These "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*. Each worker completes the knitting of a sweater all by himself and is paid an individual piece rate.

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" 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

[14]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.

(SMV).[15] Higher SMV reflects more complexity. Every sweater style is accompanied by a "design chart" (see Figure A2 for an example). Each chart contains details of the sweater parts, including the yarn type, 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.

The factory we work with did not use SMVs. We asked an independent textile engineer to use the factory style charts to calculate SMVs for the corresponding sweaters. A single engineer provided 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). The factory sets piece rates based on estimates from the first month of production whenever a new style is introduced. The correlation 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:

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

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. The measure therefore controls for style complexity and yields a measure of physical monthly output that is comparable across workers.[16]

Monthly earnings from production give us a second measure of a worker's productivity. Completed sweaters count towards earnings. The factory pays monthly based

[15]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 efficiency in garment factories at line-level (Ashraf et al. (2015), Macchiavello et al. (2020)) and worker-level (Adhvaryu et al. (2022)).

[16]In the baseline, 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. We show robustness to this choice. Note that we do not know the time a worker actually spends on each style, which bars us from computing productivity at style-level. Instead, we aggregate outputs to compute productivity at month-level so that it is comparable across workers.

on the quantities and piece rates of sweaters produced by the worker. The rates for the sweaters vary across styles and are determined by management.

We also observe the quality of a worker's output. 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 "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 flaws that require "mending." These cases are instead passed on to separate 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.

2.3 Unrest & Layoff

The work environment in the Manual Knitting Section started to deteriorate in February 2014 when the factory's management moved the section from the factory's main compound to a new location about a mile away. The manual knitting workers were unhappy about the move and protested it. This 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.[17] This protest turned 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. From factory records, we identify the 101 workers who were fired as opposed to others who voluntarily left the factory after the protest.

The factory replaced the fired workers and those who quit voluntarily with new workers hired over July-September 2014. There were no further protests as of the

[17] 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.

time we stopped working with the factory in November 2016.

Figure A1 shows average monthly production in the Manual Knitting Section over the period June 2013 to December 2014 for three groups of workers - the surviving workers, the fired workers, and the newly hired workers. Fired workers were relatively less productive than the surviving workers during the unrest period, but not before.[18] Even after the disruptive workers were fired and the factory reopened, the factory's productivity remained below pre-unrest levels. Overall production per day in the six months following the layoffs was 24% lower compared to the same six-month period in 2013 (see Figure 1). The loss of workers – and the initially lower productivity of newly hired workers – explain some of this drop. However, the majority – around two-thirds – is attributable to the lower productivity of surviving workers.

The main goal of the paper is thus to understand the productivity drop among surviving workers. The before-after comparison in Figure A1, however, is unsatisfactory since it confounds a potential effect of the firing with other time-varying factors affecting workers' productivity. To go beyond the before-after comparison, we thus define measures of a worker's exposure to fired workers and investigate the extent to which more exposed workers had larger drops in productivity. We define measures of exposure taking advantage of information on the exact location of workers on the production floor, as well as a detailed understanding of the socialization process at the factory – to which we now turn.

2.4 Socialization Process

Two distinct, complementary exercises inform our understanding of the socialization process at the factory. First, before the firing episode, in the spirit of Homans (1950), we gathered observational insights into the typical activities of workers on the production floor, systematically recording their work processes and interactions with co-workers and supervisors. Second, after the firing episode, we conducted a detailed survey on worker socialization and social connections in the workplace.

The main findings of these exercises are that workers are more likely to interact

[18] Table A1 in the Appendix reports correlations between workers' characteristics and the probability of being fired in April 2014. The table reveals that workers with lower productivity during the unrest period and with longer tenures at the factory were more likely to be fired. A worker's productivity before the unrest period doesn't correlate with the likelihood of being fired. This suggests that the firm mainly fired workers who were disruptive during the unrest rather than workers with low productivity in general.

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 blocks are an important social grouping within the factory. Within blocks, workers interact more with peers to their front than those to their back.

Beginning in January 2014, members of our observation team visited the factory on a weekly basis to observe the work processes, environment, and behaviors. We compiled detailed qualitative observations of how workers spent their time on the production floor. Tables A2 and A3 in the Appendix show two samples of these observations from early January 2014. These observations indicate that workers frequently converse and socially interact to make work more enjoyable.[19] They also suggest that interactions are more likely to take place with nearby peers. One reason is that workers are stationed at designated machines and movement across the production floor is limited. In addition, the floor is noisy due to the simultaneous operation of many machines, which restricts the ability to converse at a distance.

We also conducted a survey of the workers' social network in October 2015. The survey lines up well with the qualitative observations: workers interact mostly with proximate peers. The top panel of Figure 2 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.[20]

A second key finding 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

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

[20] Distance also correlates with the intensity of social interaction. Figure A3 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). The closer two workers are located on the production floor, the more likely they are to talk frequently.

if the peer is from a different block. Therefore, the block appears to be an important social grouping within the factory.

Interactions on the production floor correlate with other forms of social attachment (see the bottom panel of Figure 2). For example, 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. Conditional on distance, workers are more likely to socialize outside the factory with peers from their own blocks.

Table A4 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. As one might expect, new workers are less likely to interact with peers outside work. Consistent with the idea that socialization within the block builds over time, the block effect is lower – but still significant – for new workers.[21,22]

Finally, conditional on distance, a peer's orientation is an important predictor of socialization. Figure A4 shows that the probability of interactions on the production floor (left panel) and socialization outside the factory (right panel) are significantly lower if the peer is to the worker's back, as opposed to the worker's front or side. This is not surprising since a worker needs to turn around to interact with a peer behind him, while peers in front or to the side are in his line of sight and can be talked to without slowing down work.

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

[21] Selection is unlikely to be an important driver of these patterns. First, recruitment and placement of new workers is centralized: management assigns workers to blocks and machines when they first arrive at the factory with no involvement of workers in the process. conversations with the factory management confirm that these hiring and allocation processes were in place before and after the firing. Second, change of machines is extremely uncommon in the data and change of blocks is altogether non-existent. Third, we might not expect a smaller block effect for new workers if it were simply the case that existing workers attracted friends to their blocks.

[22] Within blocks, and conditional on distance, workers are more likely to socialize if they have worked together longer or if they are closer in age (see Columns 5-7 of Table A4).

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 The Effect of Peer Firing on Productivity

This section examines whether the firing of peers lowered the productivity of surviving workers. We define our measure of exposure to firing and introduce our baseline difference-in-difference specification. Estimating this equation reveals that a one standard deviation higher exposure to firing reduces productivity by two days' worth of production per month. We then show that the drop in productivity is specifically related to the loss of peers with whom workers likely had social connections – *friends*. We perform several robustness tests in Section 4.

Before turning to the empirical analysis, it is worth noting that the effect of the firings on surviving workers' productivity is *a priori* ambiguous. On the one hand, surviving workers might feel pressured and intimidated by the firings. They might fear more for their jobs and take management's threats more seriously. In response to this intimidation, workers might conceivably raise their productivity.

On the other hand, it is possible that the firings decreased workers' productivity, either because of a loss in morale or because of anger.[23] There are several reasons morale may have suffered. 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 2). Consequently, surviving workers lost a lot of friends. The firings may also have diminished fondness for management, caused stress, or made workers believe that they would be subjected to arbitrary punishments. For all of these reasons, workers may have felt less motivated to work. Likewise, workers may have been angered by the firings – or felt that they violated a relational contract – in which case they may have lowered productivity as a means of punishing the firm.[24]

3.1 Defining Exposure to Firing

Motivated by the contextual evidence in Section 2.4, 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, we know which workers were fired (as opposed to those who voluntarily left the factory).

The baseline measure of exposure weights each of the fired workers from one's block by their spatial distances to a surviving worker. We take advantage of the production floor map (see Figure A5) depicting the locations of all workers before the firings. 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

[23] 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)).

[24] See Hart and Moore (2008) and Akerlof (2016) for models in which contract violations lead to anger and shading of performance. For relational contracting models, see e.g., Gibbons and Henderson (2012). Notably, the main reason for punishing the firm in this case is mistreatment of peers. Lab experiments have documented people's willingness to punish "altruistically" on behalf of third parties (see Fehr and Gächter (2002)), particularly on behalf of people or groups with whom one identifies (see Bernhard et al. (2006)).

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 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.

3.2 Exposure to Firing and Productivity

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

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

where y_{it} is productivity of worker i in month t. E_i is the worker-level exposure to firing defined above. $Post_t$ 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. In the baseline specification, we cluster standard errors at the worker level but we also explore several robustness checks of this choice in Table A6.

We first estimate 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. We begin our analysis without fixed effects in the specification. 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.

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).

Column 2, therefore, 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.[25]

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 year-month fixed effects that control for seasonality. The magnitude of the drop in productivity is slightly attenuated but remains economically and statistically significant.

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 earnings of 482 Bangladeshi Taka (BDT) per month, slightly more than a day's earnings for a typical worker (395.8 BDT $\approx$ 5 USD, see Table 1). The loss in earnings (one day's earnings) is about half of the loss in production (two-and-a-half days' production) estimated in Column 2, suggesting that the factory might have allocated more remunerative styles to highly exposed workers. We investigate style allocation in Section 5.2. Column 5 additionally includes worker and year-month fixed effects. The estimated magnitude is largely unaffected.

[25] The negative coefficient on the variable *Post* reflects the overall drop in the factory's production in the post-firing period (see Figure 1). While suggestive that the productivity drop associated with exposure to firing is on top of a general decrease in productivity, the estimated level effect could be influenced by confounding factors

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 in Section 4.

For the time being, we assuage potential concerns by checking whether productivity evolved differently across workers with high and low exposure to firing before the firing incident. Figure 3 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 Section 5, when we try to understand what the factory management did to win back angered workers.

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 A5 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 34 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% 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.

3.3 Social Connections: Fired Friends and Productivity Loss

We now investigate the extent to which the effect of exposure to fired workers is driven by the loss of peers with whom workers likely had social connections – *friends*. Our measure of exposure to firing puts weight on firings from one's own block and firings that are spatially close. Therefore, the results in Table 2 might alternatively be driven by: (i) block-level disruption to production (e.g., the firing of a block supervisor) or (ii) spatially clustered disruption to production (e.g., damage to a group of machines) that persisted after the firing episode. In Table 3 we investigate whether the drop in productivity is driven by the loss of likely 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, we construct "circles" of nearby workers: "Circle 1" contains all peers who are one-worker distance away, "Circle 2" contains all peers who are two-worker distance away, and "Circle 3" contains all other peers in the block (see Figure A5 for an illustration). This allows us to test the effect of firing a peer holding distance 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 250 minutes' worth of production. The difference in effect size between Circle 1 and Circle 2 is not statistically significant (p-value = 0.24), 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 $\approx$ 2 days of lost work per month when using peers from Circle 2.

Column 2 controls for block-level changes to productivity by interacting the share of workers from the block who were fired with a post-firing dummy. We confirm that the effect of a fired peer is largest when the peer is located one-worker distance away.

To disentangle social connections from effects related to physical proximity (e.g., 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 3 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.[26] As a further test, in Column 4, 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 A4 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 5. 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.[27]

Finally, Columns 6 and 7 exploit tenure overlap and age distances between fired and surviving workers. Table A4 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

[26] 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 it is collinear with the dummy variable for Post).

[27] 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.

across all surviving workers. Column 6 shows that exposure to firing has almost double the impact on productivity when tenure overlap is one s.d. higher. In Column 7, we perform a similar exercise using average age distance instead of average tenure overlap.[28] The estimated positive coefficient (p-value 0.19) suggests that a survivor's productivity falls more in response to the firing of a peer of similar age.

4 Robustness

We now subject our baseline results to a number of additional tests. First, we conduct a placebo test which shows that it is the loss of *fired* friends – rather than the loss of friends in general – that triggers the productivity drop. Next, we rule out a worker's location on the floor as a potential confounder. Then, we discuss whether our measure of exposure could be capturing the long shadow of the unrest. In Appendix A.1, we also show that our findings are robust to several alternative specifications for standard errors. Section 5 probes further into the mechanism underlying the drop in productivity.

4.1 A Placebo: 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. Table 4 investigates the extent to which it is the *firing* of friends, as opposed to the *absence* of friends, that drives the results.

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. Unlike the firings, the 26 voluntarily quits are staggered across months. Exposure to quitting therefore varies over time for a given individual in the post-firing months. We thus focus on specifications that include both worker and month fixed effects.[29]

[28] 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 uncorrelated with (standardized) Exposure. The sample size drops in Column 7 as age information is missing for some workers.

[29] 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

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 estimated effect of exposure to quitting (Column 1) is considerably lower and noisier than 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. 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 these results in the spirit of a placebo: the negative productivity response stems from peers who were fired, rather than peers who simply left the factory.[30]

4.2 Worker Location as a Confounder

A potential concern is that workers' characteristics could affect their exposure to firing. If so, we might attribute declines in productivity to exposure to firing that are really related to workers' underlying characteristics. Note that we control for worker fixed effects in our analysis, which capture the effect of time-invariant observed or unobserved characteristics that influence worker productivity. However, workers' underlying characteristics might still affect changes in productivity.

There are two ways in which workers' characteristics could affect exposure to firing. First, a worker's characteristics might influence whether they are assigned to a central location on the floor. Workers in central locations have more peers in each circle than workers at block borders – and hence they are more exposed to firing. Second, workers might share characteristics with peers located close to them on the floor – either because of initial selection into locations or as the result of repeated social interaction. If true, workers with high exposure to firing might look like fired workers, in which case our estimates of the effect of firing might be picking up pre-existing resentments formed during the unrest period. We will conduct a few tests to rule out the first issue and then turn to the second issue in Section 4.3.

To deal with the issue that workers in central locations have more peers in each circle, and hence face more exposure to firing, in Column 1 of Table 5, we use the

the exposure to firing.

[30] 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 Appendix.

same specification as Column 1 of Table 3 but now we additionally control for the number of workers in each of the circles (before firing) and interact them with the post dummy. In the process, we estimate the effect of firing by letting underlying worker characteristics that determine workers' locations affect the post-firing productivity. The results remain virtually identical, indicating that the centrality of a worker's location does not drive our estimates.

Inspired by Borusyak and Hull (2021), we further address the issue of location centrality by constructing a measure of *Expected Exposure* for each location on the factory floor. We obtain Expected Exposure by running 500 simulations; in each, workers are fired from 101 random locations on the floor. We define Expected Exposure as a location's average exposure to firing over the 500 simulations. *Recentered Exposure*, defined as the difference between a worker's actual and expected exposure, removes the component of exposure stemming from a worker's location centrality.

Column 2 of Table 5 shows that we find similar drops in productivity with Recentered Exposure as we did with our original exposure measure. In Column 3, we additionally control for Expected Exposure. Result gets stronger (and Expected Exposure has an effect roughly half the size of the Recentered Exposure).

In Columns 2-3 we simulate firings holding constant the total number of workers fired from the whole floor; but we do not hold constant the number of workers fired in each block. In Column 4-5, we redo the simulation exercise, holding constant the number of workers fired from each block. This introduces block-level variation in firing into our simulated measure, Expected Exposure, and leaves only the variation generated from within-block locations in the exogenous component, Recentered Exposure. We thus identify the effect of firing off of specific locations within blocks.

Column 4 confirms that the effects are largest when fired peers are in Circle 1. The effect size dissipates as distance increases. More importantly, the difference in estimates for Circle 1 and Circle 3 is statistically significant (p-value = 0.06). Results are robust controlling for Expected Exposure (Column 5).

4.3 The Long Shadow of the Unrest?

Besides the issue of selection into more and less central locations, another concern is that workers with high exposure to firing could share the characteristics of fired workers. Workers with high exposure might, for instance, have been disruptive during the unrest themselves – or otherwise acquired resentments towards the factory that carried over into the period after the firings. If this is the case, we might misattribute to the firings declines in productivity that really reflect the long shadow of the unrest. Because the factory was closed for a large part of the unrest period, we have no information about individual workers' involvement in the unrest (other than the list of fired workers). It is therefore difficult to decisively rule out these mechanisms; but we conduct a few tests to partially assuage these concerns.

We start by examining whether workers sorted spatially so that *would-be* rabble-rousers located near fired workers. Our understanding of how the factory assigns workers to workstations suggests that there was little to no scope for sorting of this kind (see Section 2.4). Nonetheless, we check this formally. Column 1 of Table 6 repeats the baseline specification of Column 3 in Table 2. To check if the drop in productivity from exposure to firing is driven by rabble-rousing workers pre-selecting themselves into blocks with high numbers of fired workers, in Column 2 we control for the interaction between a Block-specific dummy and the Post dummy, thus controlling for the effect of block-level disruptions (e.g., resentment increasing with the block's supervisor getting fired, or with the number of workers fired from the block). The magnitude of the effect from exposure to firing remains negative and significant.

Next, we check whether the drop in survivors' productivity is driven by similarity to fired workers by controlling for worker characteristics correlated with fired workers. Table A1 reveals that workers with lower productivity during the unrest and with longer tenures at the factory were more likely to be fired. We thus control for the interaction of these variables with the Post dummy. Column 3 of Table 6 reports estimates of the drop in productivity from exposure to firing, omitting the three months preceding the unrest, as we drop these months in the subsequent analysis. Column 4 introduces controls for the fall in productivity during the unrest and the worker's tenure interacted with the Post dummy. Although slightly dented, the estimate of the effect of exposure to firing remains negative and significant.[31]

In sum, while there may have been some carryover of resentment from the unrest period, the firing of friends is a key driver of the productivity drop.

[31] As a further test, we use propensity scores to match survivors to fired workers based on observed characteristics such as fall in productivity during the unrest, tenure, and age. Through this approach, we identify a set of "pseudo-fired" workers: that is, surviving workers with a high likelihood of getting fired in April 2014 based on their similarities to workers who were actually fired. In unreported regressions, we control for whether a survivor was pseudo-fired and find that the estimated effect of exposure to firing remains almost unchanged.

5 Exploring Mechanisms

Our analysis so far suggests that the firing of friends is a key driver of the productivity drop. Several potential mechanisms could account for this fact. In this section, we present suggestive evidence that the drop was driven, at least in part, by surviving workers' desire to punish the factory. We also present evidence that the factory tried to repair strained relationships with workers by offering them more rewarding tasks.

5.1 The Productivity Drop: Morale or Punishment?

Losing friends during the firings is associated with a drop in post-firing productivity. Appendix A.2 considers and rules out several mechanical explanations for the drop: lost opportunities to learn from or receive help from fired workers, time spent helping newly hired workers, and on-the-job search. Two potential explanations seem to remain: 1) loss of worker morale; 2) a conscious desire to punish the factory. Demoralization encompasses a number of 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, which are not directly observable. We nevertheless provide suggestive evidence of deliberate shading of performance by workers in order to punish the factory.

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 directly to a new set of sweaters and his pay is unaffected. When there is a defect, on the other hand, the worker must fix it himself before going on to a new assignment. Therefore, mending flaws only hurt the factory; 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"). We run regressions as in Figure 3 with the fraction of sweaters with mending and defects as outcomes.

The left panel of Figure 4 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 4 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 4 are also reflected in Table A10, 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 A10 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. This is consistent with highly exposed workers punishing the factory.

Alternative interpretations are, in theory, possible. In particular, the factory might have reclassified defects as mending flaws to appease workers. Our understanding of the production process suggests that this is unlikely to have been the case, however. Reclassification is unlikely because mending flaws and defects are technologically very different. 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 defects, they would be taking the risk of delivering faulty sweaters to 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 5.2 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. Evidence on flaws is not sufficient to conclusively rule out such mechanisms.

5.2 Factory's Response

We have documented a drop in productivity among surviving workers after the firings and argued 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 3, 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. The distributions of style rents before and after the firings are nearly identical.

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).[32]

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).

[32]Fahn and Zanarone (forthcoming) argue that transparency is costly when workers engage in social comparison. Ashraf (2022), 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.

We regress the average monthly style rent on exposure to firing, interacted with a dummy for each post-firing month. Column 3 of Table A10 shows that, after the firings, more exposed workers 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. The timing is particularly striking: the estimated effect more than halves *precisely* at the time in which the impact on mending defects fades away. The evidence in Table A10 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.

6 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 workers in a large Bangladeshi sweater factory was associated with a significant reduction in the productivity of surviving workers. We document this 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 agreements supported by workers' willingness to punish "altruistically" on behalf of third parties – a willingness supported by social connections.

With regard to industrial relations in developing countries, episodes of labor unrest are common in countries with emerging manufacturing sectors. Our 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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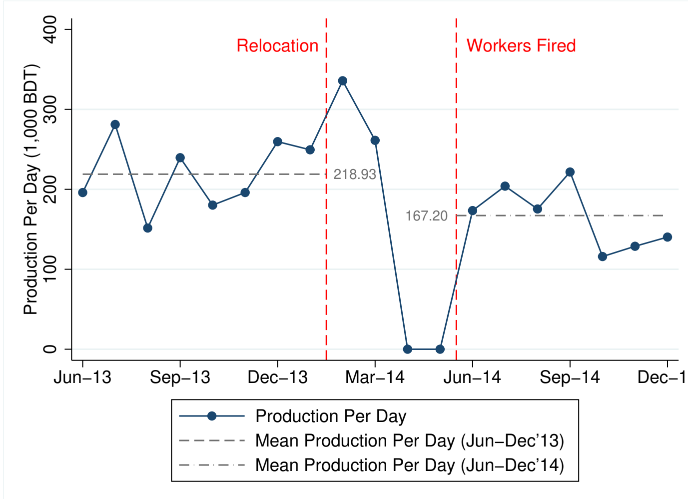
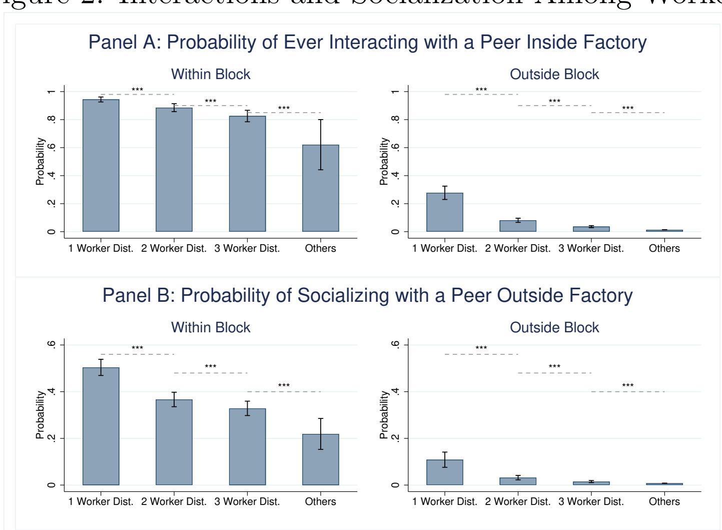
Figure 1: Average Daily Production During Sample Period



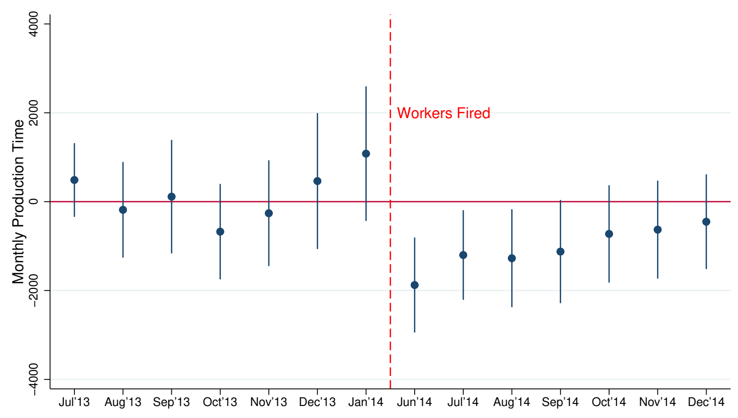
Note: The figure shows average production per operation day in each month. The first vertical line depicts the timing of relocation for the factory compound. The second vertical line depicts the timing of workers getting fired. The horizontal lines represent the average daily production computed from total production and total operation days in Jun'13-Dec'13 (dashed line) and Jun'14-Dec'14 (dash-dotted line).

Figure 2: Interactions and Socialization Among Workers



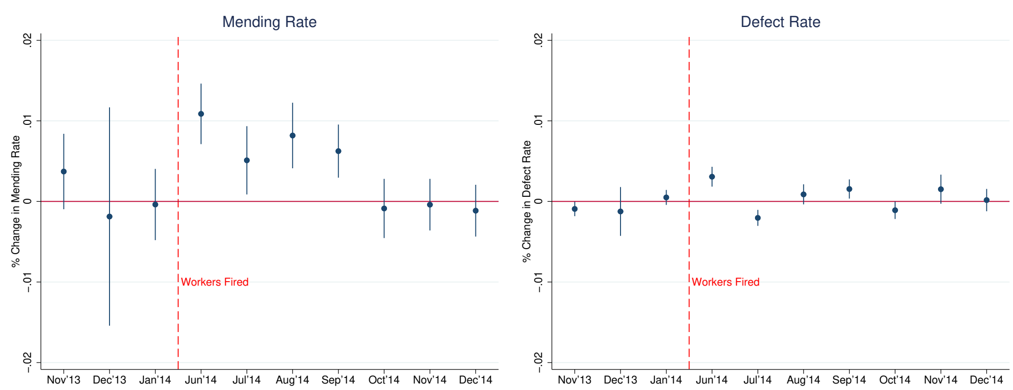
Note: The figure reports probabilities of a worker ever talking with a peer inside the factory (Panel A) or socializing with a peer outside the factory (Panel B). The probabilities are computed for different locations of the peers, and separately for peers from same block (left sub-panels) and different blocks (right sub-panels). Underlying regressions are linear probability models with no constants; standard errors are clustered at worker level. The vertical lines represent 95% confidence intervals. Each observation is a pair of workers.

Figure 3: Test of Pre-Trend



Note: The figure shows 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 is the total production of a worker in a month. 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. Feb'14–May'14 are dropped from the analysis as the factory was either going through labour unrest or was closed in those months. The dashed line refers to Apr'14 when workers were fired. The vertical lines represent 95% confidence intervals.

Figure 4: Treatment Effect on Quality



Note: The figure shows 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

All Workers	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
Exposure to Firing: Same Block (Non-stand.)	304	2.57	1.56	0.41	7.02
Survivors	**n**	**Mean**	**Std. Dev.**		
Monthly Earnings in BDT (Jun'13-Mar'14)	2,922	10,097.67	3,822.68		
Time-Value of Monthly Production (Mins.)	2,919	13,198.06	8,885.01		
Monthly Attendance Days (Jun'13-Mar'14)	2,922	25.51	4.49		
Tenure (months) in Mar'14	305	63.3	19.16		

Note: Sample period spans from June 2013 to December 2014. Out of 406 workers working at the factory in April 2014, 101 were fired and 305 were retained. Bottom panel reports statistics for workers who were retained in April 2014.

Table 2: Effect of Exposure to Firing on 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***	-1,354***	-482.4***	-326.1***
		(255.7)	(256.7)	(85.34)	(72.93)
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, Year-Month FE	N	N	Y	N	Y

Note: *Monthly Production* in Cols. 1-3 (res. *Monthly Earnings* in Cols. 4-5) refers to total monthly production time (res. earnings) calculated from products of total physical output and sweaters' Standard Minute Value (res. piece rates). *Exposure: Same Block* refers to standardized spatially weighted exposure to firing within a worker's own block. *Post* is a dummy variable equal to one in post-firing months. All regressions include a constant. 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)	(7)
	Monthly Production						
				Borders	Non-Ends		
(# Fired, Circle 1, Same Block) * Post	-922.5***	-788.2**		-1,234***			
	(281.5)	(398.1)		(394.5)			
(# Fired, Circle 2, Same Block) * Post	-424.7**	-305.8					
	(198.1)	(304.9)					
(# Fired, Circle 3+, Same Block) * Post	-252.8**	-140.5					
	(104.0)	(238.7)					
(% Workers Fired in Block) * Post		-3,607					
		(6,796)					
(Exposure: Same Block) * Post			-1,420***			-1,481***	-1,218***
			(257.8)			(235.4)	(263.8)
(Exposure: Other Blocks) * Post			-616.7***				
			(228.3)				
(# Fired, Circle 1, Other Blocks) * Post				323.0			
				(351.8)			
(# Fired, Circle 1 Front, Same Block) * Post					-1,319***		
					(421.3)		
(# Fired, Circle 1 Back, Same Block) * Post					-348.0		
					(442.1)		
(Exposure: SB) * Post * (Tenure Overlap)						-1,073***	
						(315.3)	
(Exposure: SB) * Post * (Age Distance)							354.7
							(271.8)
Observations	4,104	4,104	4,119	2,216	2,908	4,119	3,886
Number of Workers	303	303	304	162	213	304	287
Circle 1 = Circle 3 on	[0.026]	[0.035]					
Same Block = Other Blocks			[0.012]	[0.002]			
Front = Back					[0.147]		

Note: *Monthly Production* is time-value of monthly production. *# Fired, Circle 1* refers to number of workers fired from Circle 1, and similarly for other circles. *Circle 1* refers to peers one-worker-distance away, and similarly for others. *Exposure* is standardized spatially weighted exposure to firing. *Same Block* or *SB* (res. *Other Blocks*) refer to firing within (res. outside) a worker's own block. *Post* is a dummy variable equal to one in post-firing months. *Tenure Overlap* is the (standardized) average duration of tenure overlap with all fired workers from the same block. *Age Distance* is the difference in ages between a survivor and a fired peer, divided by the average of their ages, and standardized. All regressions include worker FE, year-month FE, and a constant. Col. 4 considers workers located at block borders. Col. 5 considers workers with at least one peer on their front and back. Col. 6 and 7 include interactions of *Post* dummy with *Tenure Overlap* and *Age Distance* respectively. Square brackets contain 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)	(2)	(3)	(4)
	Monthly Production			
			Post Period	
# Quit, Circle 1, Same Block	-340.7		-122.4	120.4
	(923.3)		(381.9)	(937.8)
(# Fired, Circle 1, Same Block) * Post		-1,099***		
		(250.7)		
(Exposure: Same Block) * Post				-1,361***
				(257.2)
Observations	4,116	4,119	1,807	4,101
Number of Workers	305	304	293	304

Note: *Monthly Production* is time-value of monthly production. *# Quit, Circle 1, Same Block* is the cumulative number of peers at one-worker-distance within the same block who quit up until previous month. Only quits between Apr'14-Nov'14 are counted; the variable is set to zero for earlier months. *# Fired, Circle 1, Same Block* is number of workers fired from Circle 1 of a worker's own block. *Exposure: Same Block* refers to standardized spatially weighted exposure to firing within a worker's own block. *Post* is a dummy variable equal to one in post-firing months. Col. 3 considers only post-firing months. All regressions include worker FE, Year-Month FE, and a constant. Standard errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 5: Selection into Spatial Locations

	(1)	(2)	(3)	(4)	(5)
	Monthly Production				
		Floor Sim.	Floor Sim.	Block Sim.	Block Sim.
(# Fired, Circle 1, SB) * Post	-1,060***				
	(310.6)				
(# Fired, Circle 2, SB) * Post	-454.8**				
	(195.2)				
(# Fired, Circle 3+, SB) * Post	-156.1				
	(126.5)				
(Recentered Exposure: SB) * Post		-1,136***	-1,604***		
		(246.5)	(288.5)		
(Expected Exposure: SB) * Post			-897.6***		
			(332.1)		
(Recentered # Fired, Circle 1, SB) * Post				-1,151**	-1,754***
				(470.5)	(559.4)
(Recentered # Fired, Circle 2, SB) * Post				-1,203***	-1,377***
				(462.0)	(523.8)
(Recentered # Fired, Circle 3+, SB) * Post				-648.5	-892.9**
				(415.9)	(439.7)
(Expected # Fired, Circle 1, SB) * Post					-884.3*
					(467.8)
(Expected # Fired, Circle 2, SB) * Post					-69.58
					(284.6)
(Expected # Fired, Circle 3+, SB) * Post					-105.2
					(236.0)
Observations	4,104	4,119	4,119	4,104	4,104
Number of Workers	303	304	304	303	303
Circle 1 = Circle 2	[0.166]			[0.858]	[0.375]
Circle 1 = Circle 3 on	[0.013]			[0.062]	[0.012]
# Worker in Circle * Post	Y	N	N	N	N
% Workers Fired in Block * Post	N	N	N	Y	Y

Note: *Monthly Production* is time-value of monthly production. *# Fired, Circle 1* refers to number of workers fired from Circle 1, and similarly for other circles. *Circle 1* and *Circle 2* refer to group of peers one- and two-worker-distance away; the rest are pooled together in *Circle 3+*. *SB* refers to firing within a worker's own block. *Post* is a dummy variable equal to one in post-firing months. *Expected Exposure* is the standardized average spatially weighted exposure to simulated firing. *Recentered Exposure* is true spatially weighted exposure to firing less *Expected Exposure*, standardized across survivors. *Expected* and *Recentered* carry the same meaning for other measures of exposure calculated from each circle. Cols. 2-3 show results for simulation holding total number of fired workers constant. Cols. 4-5 show results for simulation holding number of workers fired from each block constant. *# Worker in Circle* refers to total number of workers inhabiting each circle before the firing. All regressions include worker FE, year-month FE, and a constant. Square brackets contain 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 6: Alternative Explanation - Pre-existing Resentment

	(1)	(2)	(3)	(4)
	Monthly Production			
			Early Mths. Dropped	Early Mths. Dropped
(Exposure: Same Block) * Post	-1,354***	-1,456***	-1,496***	-1,112***
	(256.7)	(508.9)	(303.5)	(296.0)
(Own Unrest Period Prod. Drop) * Post				-1,012**
				(391.1)
(Tenure in Mar'14) * Post				-1,443***
				(362.2)
Observations	4,119	4,119	3,210	3,210
Number of Workers	304	304	304	304
Block*Post	N	Y	N	N

Note: *Monthly Production* is time-value of monthly production. *Exposure: Same Block* refers to standardized spatially weighted exposure to firing within a worker's own block. *Own Unrest Period Prod. Drop* refers to standardized drop in productivity during unrest period compared to Jun-Aug, 2015. These months are dropped from analysis in Columns 3-4. *Tenure in Mar'14* refers to standardized tenure of workers in March 2014. *Post* is a dummy variable which is equal to one for months in post-period and to zero for months before. All regressions include worker FE, year-month FE, and a constant. Standard errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

A Appendix

A.1 Inference

In this section we conduct robustness checks of the standard errors of our estimates. The standard errors we report in the paper are clustered at the individual worker level. Instead, it is plausible that the errors are correlated in a more complex way involving blocks, months, workers, or even across space.

Our findings are robust to several alternative specifications for standard errors. We test for correlations within block, correlations within block and month, correlation within the interaction of block and month, and correlation within worker and interaction of block and month. As we have only 15 blocks in our dataset, we implement wild-bootstrap using blocks whenever we do one- or two-way clustering involving blocks. We do not need to bootstrap when we cluster the errors using interactions of block and month. For these tests, we use the estimates proposed by Roodman et al. (2019). Finally, we correct our standard errors for spatial correlations according to Conley (1999). Here, we assume that the errors are spatially correlated up to 3-worker distance, which is highly conservative given that we mostly test exposure within block.

We re-test four previous specifications that are key to establish that surviving workers' productivity decreased because of loss of friends. The results are shown in Table A6. First we test our baseline specification from Column 3 of Table 2. The asterisks in the first row in Column 1 of Table A6 shows that the coefficient was previously found to be statistically significant at 1% significance level when clustering at the worker level. The five following rows report the p-values for the same coefficient but from the alternative assumptions on error correlations. The coefficient remains statistically significant at 2% significance level for all of the alternative assumptions on error structure. Column 2 tests whether the results are robust when we measure productivity using the more traditional measure of earnings (from Column 5 of Table 2). We again find high statistical significance for the coefficient using alternative error structures.

In Column 3 we test the importance of block identity (from Column 3 of Table 3). The coefficient for same-block exposure retains a high level of statistical significance. Also, the difference in coefficients for same-block and outside-block exposure remains statistically significant in most cases, except when we cluster at block, or block and month (p-values for these cases are 0.14 and 0.13, respectively).

Finally, in the last three columns of Table A6 we test whether workers fired from different locations within a block lead to productivity drops of different magnitude among surviving workers (from Column 1 of Table 3). The three columns show results from a single underlying regression, reported horizontally in order to fit into the page. We again find similar statistical precision for estimates regarding the nearest workers – those in Circle 1. The statistical significance of coefficients, however, diminishes as we move farther away from a surviving worker. Importantly, the difference in the productivity drop from the firing of a Circle-1 peer and the productivity drop from the firing of a Circle-3+ peer retains high statistical significance (the largest of the p-values is 0.032).

A.2 Alternative Mechanisms

In this Appendix, we first 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, and (iii) on-the-job search. We also perform additional robustness tests of the baseline specification.

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.[33] It is thus a priori unlikely that the loss of help could explain the significant drop in productivity. Furthermore, 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.

We nevertheless explore the issue more systematically. We do not have a direct measure of help between co-workers throughout the sample period; therefore we investigate this mechanism indirectly. First, if pre-firing productivity of a surviving worker depended on help from friends, we would expect to find a positive correlation

[33] A measurement exercise conducted in 2016, i.e., after the firing, 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 (2022) 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.

between a worker's productivity and the number of friends around him. Column 1 of Table A7 therefore tests whether surviving workers who had more (same-block) peers surrounding them before the 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. We find no correlation between number of peers around a worker and his productivity in the pre-firing period. This is, of course, only a correlation. So, we conduct additional tests.

Next, we measure workers' exposure to peer absenteeism and examine whether the absence of peers affects workers as adversely as the firing of peers. If surviving workers' productivity dropped because of the loss of help from fired peers, we would expect the effect of peer firings and the effect of peer absence to be similar. To make exposure to absenteeism comparable to exposure to fired peers, we sum absent-days across peers in a month and divide by 26 (the average number of working days in a month) to arrive at a normalized measure of average number of peers absent per day in a month. We do this separately for peers in Circle 1 and the rest of the block. Column 2 of Table A7 shows that exposure to absenteeism does not adversely affect a surviving worker's productivity.

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.

Although exposure to newly hired workers is highly correlated with initial exposure to fired workers, we take advantage of the fact that new workers were hired to replace fired 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 3 of Table A7 considers the number of new operators, while Column 4 considers the percentage of new operators in Circle 1. The estimated coefficients are imprecisely estimated but, if anything, positive, rather than negative.

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.[34] 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 5 the DID estimate with respect to only June–September 2014 (as opposed to June-December 2014, as we did previously). Then in Column 6 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.[35] 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.

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 A5). 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 A8.

Column 1 of Table A8 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").[36] Columns 2 and 3 verify that the drop in productivity among stayers was

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

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

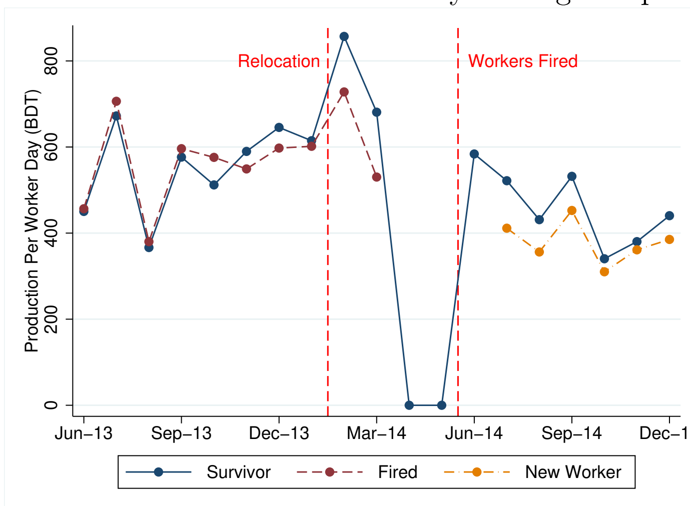
[36] We are aware that this comparison is based on an endogenous choice and thus we present it in the spirit of suggestive evidence that might still be informative about mechanisms.

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.[37] 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.

Robustness to Floor Map 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. Nonetheless, we also provide a formal test. As mentioned in Section 2.1, each knitting machine is part of a pair; the workers assigned to a pair of machines face each other. Even if there were some changes to the layout after the move, it is extremely unlikely that workers in a machine pair would have been broken up. So, instead of defining exposure to firing based on all of the original peers around a surviving worker, in Column 1 of Table A9 in the Appendix, we measure exposure only based on whether the peer in front was fired. 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 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.

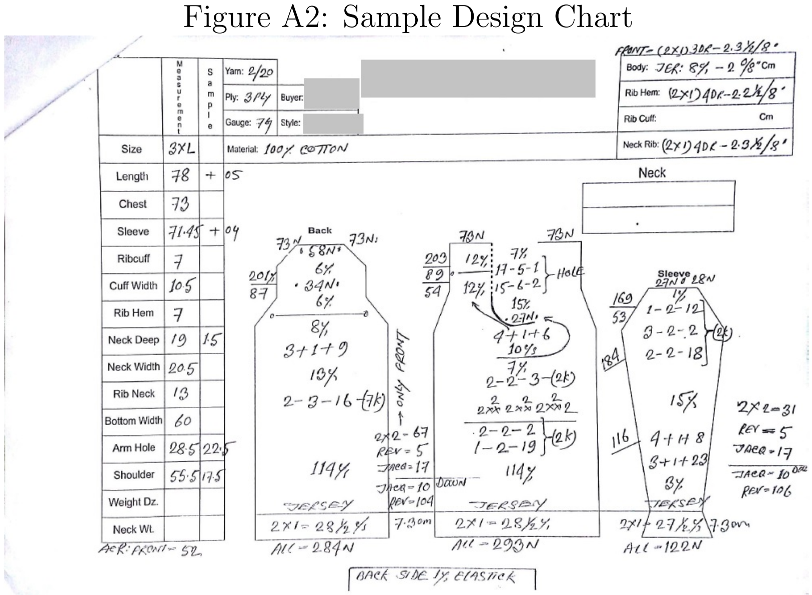
[37] To avoid a small cluster problem, we split workers based on whether they have more than 3 absent days during the period.

Figure A1: Production Per Worker Day During Sample Period



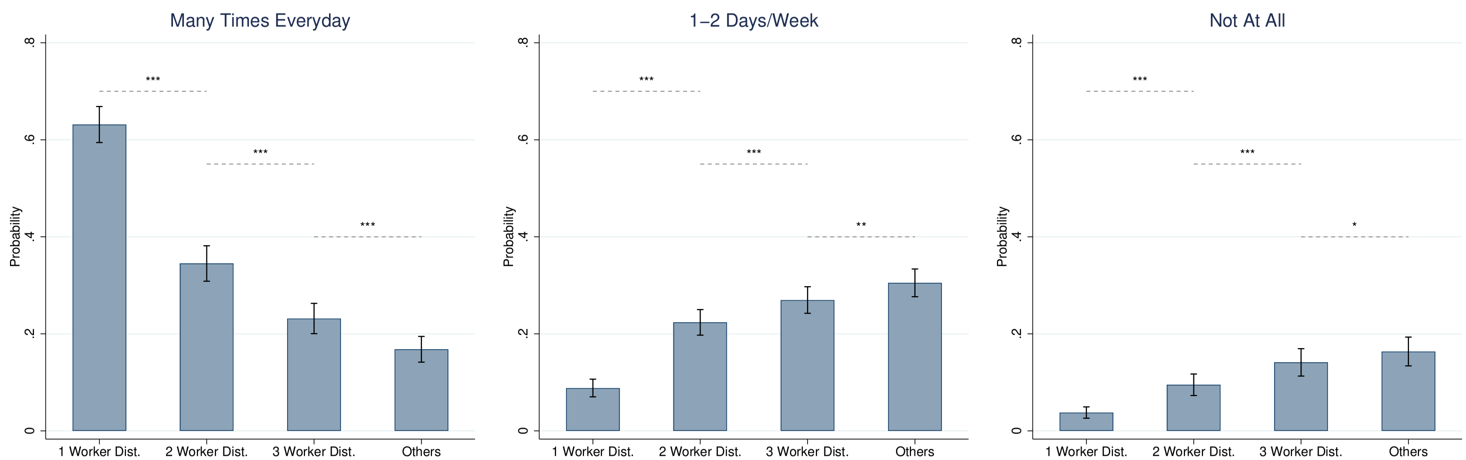
Note: The figure shows average production per worker-day for each of the months in the sample period, broken down by worker types. The first vertical line depicts the timing of relocation for the factory compound. The second vertical line depicts the timing of workers getting fired.

Figure A2: Sample Design Chart



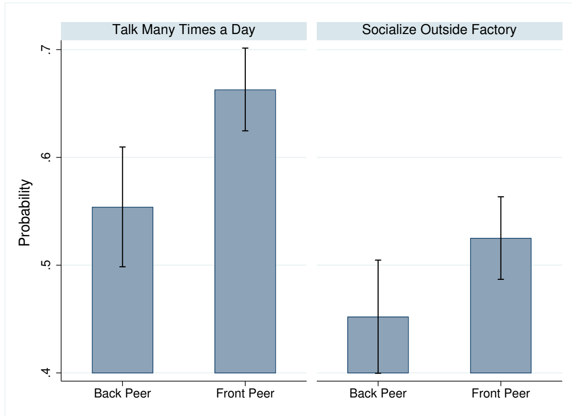
Note: The figure shows a sample design chart of a sweater that the workers use to knit sweaters, and which we used to estimate SMVs for the corresponding sweaters.

Figure A3: Intensity of Interactions with Same-Block Peers inside Factory



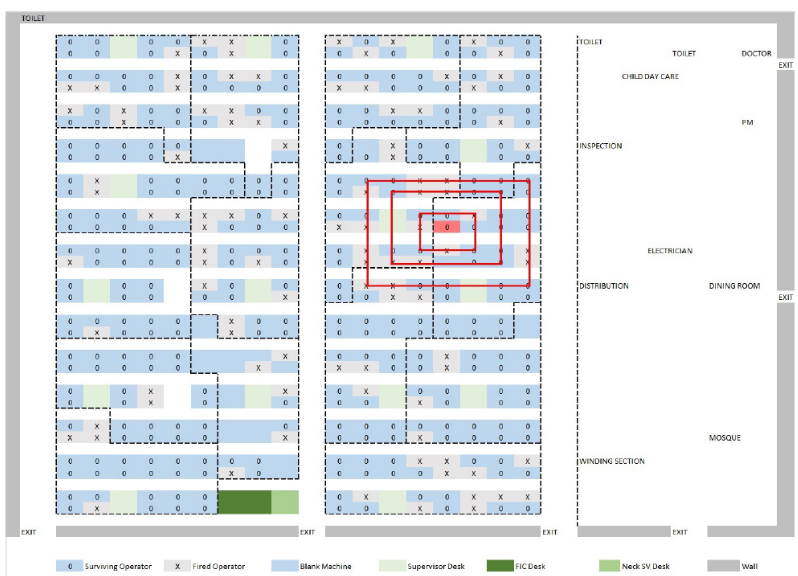
Note: The figure reports probabilities of a worker talking with different intensities with a within-block peer inside the factory when the peer is from different worker-distances away from him. The probabilities are computed separately for interactions frequencies of ”many times a day” (left panel), ”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. Standard errors are clustered at worker level. Each observation in the regression model is a pair of workers.

Figure A4: Interactions & Socialization With Same-Block Peers at One Worker-Distance



Note: The figure reports the likelihood that a worker talks with high intensity or socializes with a same-block peer when the peer is one worker-distance away and is either to the front or back. The reported probabilities are computed from a linear probability model with no constant. Standard errors are clustered at worker level. Each observation in the regression model is a pair of workers.

Figure A5: Floor Map



Note: This is the floor map of the Manual Knitting section right before the firing of workers in Apr'14. *O* depicts locations of surviving workers; *X* depicts locations of fired workers. Every row of workers face workers in the paired row. 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.

Table A1: Who Got Fired?

	(1)	(2)	(3)	(4)	(5)	(6)
	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

Note: 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 A2: 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.

Note: 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 A3: 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

Note: 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: Probability of Socializing with Peers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	P(Socialization)						
				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

Note: 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 A5: Intensive vs Extensive Margins of Response

	(1) Monthly Production/Day	(2) Monthly Leave+Absent
(Exposure: Same Block) * Post	-33.85***	0.0905
	(7.523)	(0.0834)
Observations	4,116	4,123
Number of Workers	304	304
Worker FE; Year-Month FE	Y	Y

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 within a worker's own block. *Post* is a dummy variable which is equal to one in post-firing months. All regressions include constants. Standard errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A6: Robustness to Alternative Assumptions on Standard Errors

	(1) Monthly Production	(2) Monthly Earnings	(3) Monthly Production	(4) Monthly Production		
				Circle=Circle 1	Circle=Circle 2	Circle=Circle 3+
(Exposure: Same Block) * Post	-1,354***	-326.1***	-1,420***			
SE Cluster - Block	[0.018]	[0.045]	[0.012]			
SE Cluster - Block & Month	[0.017]	[0.048]	[0.011]			
SE Cluster - Block*Month	[0.000]	[0.001]	[0.000]			
SE Cluster - Worker & Block*Month	[0.000]	[0.004]	[0.000]			
SE Corrected for Spatial Correlation	[0.000]	[0.000]	[0.000]			
(Exposure: Other Blocks) * Post			-616.7***			
SE Cluster - Block			[0.194]			
SE Cluster - Block & Month			[0.203]			
SE Cluster - Block*Month			[0.007]			
SE Cluster - Worker & Block*Month			[0.031]			
SE Corrected for Spatial Correlation			[0.020]			
(# Fired from Circle, Same Block) * Post				-922.5***	-424.7**	-252.8**
SE Cluster - Block				[0.019]	[0.112]	[0.230]
SE Cluster - Block & Month				[0.011]	[0.132]	[0.218]
SE Cluster - Block*Month				[0.000]	[0.006]	[0.005]
SE Cluster - Worker & Block*Month				[0.002]	[0.040]	[0.031]
SE Corrected for Spatial Correlation				[0.002]	[0.004]	[0.006]
Observations	4,119	4,123	4,119		4,104	
Number of Workers	304	304	304		303	

Note: *Monthly Production* is time-value of monthly production. *Exposure* is standardized spatially weighted exposure to firing. *Same Block* (res. *Other Blocks*) refers to firing within (res. outside) a worker's own block. Numbers in square brackets indicate p-values corresponding to alternative clustering of standard errors (SE). In Col. 4 *# Fired from Circle* refers to count measure of workers fired from a given Circle. *Circle 1* and *Circle 2* refer to group of peers one- and two-worker-distance away; the rest are pooled together in *Circle 3+*. All three sub-columns under Col. 4 are from the same underlying regression. *Post* is a dummy variable equal to one in post-firing months. All underlying regressions include a constant, and use worker and year-month fixed effects except when correcting SE for spatial correlation. The high number of dummy variables are computationally demanding for the codes developed from Conley (1999). *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively when standard errors are clustered at worker level. Standard errors are bootstrapped at block-level whenever they are clustered using blocks.

Table A7: Alternative Explanation - Help

	(1) Mean Production	(2) Monthly Production	(3) Monthly Production	(4) Monthly Production	(5) Monthly Production	(6) Monthly Production
	Pre-Unrest	Pre-Unrest	Jun'14-Sep'14	Jun'14-Sep'14	Jun'13-Sep'14	Jun'13-Sep'14
# in Circle 1: Same Block	97.45					
	(164.6)					
Normalized Man-days Peers Absent in Circle 1, SB		-360.5				
		(1,021)				
Normalized Man-days Peers Absent in Circle 2+, SB		28.13				
		(281.3)				
# of New Workers in Circle 1			367.7*			285.1
			(207.2)			(210.5)
# of New Workers in Rest of the Block			155.9			-6.753
			(99.33)			(78.36)
% of New Workers in Circle 1				1,595		
				(1,422)		
% of New Workers in block except Circle 1				4,834**		
				(2,173)		
(Exposure: Same Block) * Post					-1,639***	-1,820***
					(271.1)	(309.9)
Observations	304	1,821	1,171	1,159	3,470	3,468
Number of Workers	304	304	296	291	304	304
Worker FE, Year-Month FE	N	Y	Y	Y	Y	Y

Note: *Production* is time-value of monthly production. In Column 1 it is calculated as mean of monthly production times over the whole pre-firing period. In Columns 2-6, it is calculated monthly. *Circle 1* refers to peers one-worker-distance away within the same block; *Circle 2+* refers to the rest of the block. *Normalized Man-days Peers Absent* is the sum of absent-days across peers in a month and divided by 26 (the average number of working days in a month). *New Workers* refers to workers hired after firing. *Exposure: Same Block* refers to standardized spatially weighted exposure to firing within a worker's block. *Post* is a dummy variable which is equal to one after Jun'14 and to zero before. All regressions include a constant. Standard errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A8: 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		-1,097	-1,360***	-67.91***	-21.47**
		(1,260)	(262.9)	(12.88)	(8.285)
Observations	1,826	299	3,820	542	3,276
Number of Workers	297	27	277	50	227
Worker FE; Year-Month FE	N	Y	Y	Y	Y

Note: *Total Absent Days* refers to the sum of pre-authorized and unauthorized absent days during Jun-Dec'14. *Monthly Production* is time-value of monthly production time. *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. Standard errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A9: Robustness to Floor Map

	(1)
	Monthly Production
1(Front Worker Fired) * Post	-3,187***
	(851.4)
Observations	4,044
Number of Workers	299
Worker FE; Year-Month FE	Y

Note: *Monthly Production* is time-value of monthly production . *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. *Post* is a dummy variable which is equal to one in post-firing months.The regression includes a constant. Standard errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A10: 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

Note: *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 within a worker's own block. *Post* is a dummy variable which is equal to one in post-firing months. The pre-firing months span Nov'13-Jan'14, limited based on availability of data on quality. All pre-firing months are omitted category. All regressions include a constant and dummies for post-firing months. Standard errors clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.