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Performance Ranks, Conformity, and Cooperation: Evidence from a Sweater Factory

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Abstract

Performance ranking triggers multiple social incentives for workers. On one hand, it offers status rewards to induce the workers to increase their effort. On the other, it introduces risks of social retribution from coworkers for outperforming them. This may make the workers reduce their effort to signal social compatibility instead. This paper uses a field experiment in a sweater factory to disentangle the incentives underlying performance ranks. Treated workers receive ranks either privately or publicly. Private ranks do not have any effect, but public ranks reduce productivity. Additional evidence confirms that the productivity drop is driven by the workers' social concerns, particularly by the desire to conform to the productivity of their friends. Also, cooperation among the workers decreases but with limited effect on productivity. The paper provides evidence showing that inducing worker competition may be counterproductive for firms.

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1 Introduction

Motivating workers is a big challenge for any organization. Existing literature suggests that workers are motivated not only by financial incentives but also by relative and social preferences.[1] These preferences exist in several forms and may interact with financial incentives (Bandiera et al. (2009); Breza et al. (2018)). However, we have little evidence, especially from real workplaces, on how different forms of social preferences interact with each other. Yet, it is critical to design effective incentives for workers.

This paper provides evidence of the interplay of two different forms of social incentives in the context of performance ranking. Seminal models such as Lazear and Rosen (1981) and Green and Stokey (1983) propose the use of performance ranks with monetary rewards to increase worker productivity. More recent papers, on the other hand, propose using ranks as status rewards to exploit the workers' social preferences, namely their desire for social recognition through high ranks (Besley and Ghatak (2008); Ashraf et al. (2014b); Ager et al. (2021)).[2] Empirical evidence from providing ranks, however, is mixed, possibly due to counteracting mechanisms. For instance, workers may value social ties with their coworkers who may negatively reciprocate if they are too keen to outperform them at the management's behest. Instead of signaling high productivity for status, the workers may even reduce their effort to signal social compatibility to their coworkers and avoid social punishment (Bernheim (1994); Akerlof and Kranton (2000)). Performance ranking, therefore, introduces counteracting social incentives to the workers. Its net effect on worker productivity is a-priori ambiguous.

Disentangling the social incentives underlying performance ranking poses several empirical challenges. First, we require a context that provides a measure of the individual performance of workers. Second, the context should be conducive to social incentives for the workers. Third, it is crucial to isolate the effect of social incentives from that of pure information from ranks that may affect the workers' intrinsic incentives. Finally, we must be able to distinguish between responses to the two opposite signaling incentives.

The paper overcomes the empirical challenges through a five-month-long field experiment in the Manual Knitting Department of a large sweater factory in Bangladesh. Workers in this department individually knit batches of 12-48 sweaters (a *job*) and are

[1] See Ashraf and Bandiera (2018) for a review of evidence on workers' social and relative incentives.

[2] It is also considered a good management practice by industry experts. For instance, the World Management Survey (WMS), which assesses the management practices of firms in 35 countries, provides the highest score in a question to firms that use public performance ranking for their workers. See Question 12 in the WMS Manufacturing Survey Instrument 2010 (World Management Survey (2010)).

paid based on piece rates. Administrative records containing job-level knitting times for over 22,000 jobs provide a high-frequency measure of individual productivity to solve the first empirical challenge. The setting solves the second challenge by lending a real-world social network to the experiment. The knitting workers, who worked for an average of five years at the factory before the intervention, report strong social ties with their coworkers. This, along with the long duration of the intervention, provide the stage and time for the workers' social concerns and subsequent responses to realize. I address the third and fourth challenges through a novel experimental design.

The experiment provides monthly performance feedback to 366 workers for five months. In the experiment, *Control* workers receive feedback that does not contain ranks. The first treatment, *Public Treatment*, additionally provides treated workers with information on their ranks as well as that of their coworkers in the Treatment.[3] By making their ranks visible to their coworkers, the treatment introduces social incentives associated with ranks. However, the workers may also respond to intrinsic incentives associated with the information from public ranks. To isolate such effects, workers in a second treatment group, *Private Treatment*, receive information on only their own ranks and the feedback that Control workers receive. In addition, a baseline survey measures the workers' priors about the productivity of their coworkers relative to their own. This helps to test the effect of what they actually learn about their coworkers' relative productivity from the public ranks. Finally, to address the fourth empirical challenge, the survey maps the social network among the workers. This helps to identify workers who compete with their friends and have an incentive to signal social compatibility. In turn, this helps to identify the effects of the two signaling incentives.

The main findings are as follows. First, publicly-ranked workers reduce productivity by 1.5 percent on average due to the intervention. In contrast, there is no change in the average productivity among privately-ranked workers. In other words, learning about ranks in private does not affect productivity on average, but making ranks visible to coworkers does. To understand the magnitudes of the effects, note that the literature usually finds a 0.3-0.8 percent increase in productivity from a 10 percent increase in piece rates.[4]

Second, the productivity effects among publicly-ranked workers *are* driven by social concerns. In particular, the workers' desire for status rewards is overpowered when they

[3]The workers are individually randomized into the experimental arms and ranked only among coworkers receiving the same treatment using their knitting times from the previous month.

[4]For example, see Tonin and Vlassopoulos (2015); DellaVigna and Pope (2018); Goerg et al. (2019). These estimates, however, relate to different kinds of tasks in different contexts.

face risks of social retribution from their social groups. Several pieces of evidence corroborate this interpretation. First, compared to privately-ranked workers, publicly-ranked workers reduce productivity by 1.7 percent if at least one of their friends is randomly assigned to Public Treatment, which makes them directly compete against each other.[5] Workers who do not compete with any friends (weakly) increase productivity instead. Second, the productivity drop is driven by workers who rank relatively high among their competing friends and is triggered by the first feedback letter. Workers who rank relatively low among their friends, on the other hand, do not change productivity.[6] This suggests that the negative productivity effect is associated with concerns about *outranking* friends. Third, a placebo test shows that the workers are not sensitive to outranking coworkers they are not friends with. This highlights the role of social ties in driving the productivity change. Finally, there is no association between productivity and rank-distance with friends in Private Treatment. This suggests that the concerns are salient only when the ranks are publicly visible to the friends. Comparing publicly-ranked workers with privately-ranked workers isolates the effects of public visibility of ranks from that of learning one's ranks. The estimates are unaffected when I additionally control for any new information that the workers learn about their friends' relative productivity from the public ranks. Taken together, the results are consistent with publicly-ranked workers responding positively to status rewards if they do not compete with friends. When they do compete with friends, they reduce productivity to signal social compatibility. Note, by comparing similar workers in the two treatments, the tests hold constant underlying worker characteristics correlated with the likelihood of competing with friends and, conditional on competing, relative ranks with friends. Additional robustness tests confirm that the estimates are not driven by endogenous worker characteristics correlated with their relative rank among their friends, pre-existing differences in productivity, or workers in Private Treatment revealing their ranks.

I rule out alternative explanations for the results using novel data from the production floor. In particular, I rule out complacency, the ratchet effect, and inequity aversion as alternative explanations of the main results. However, a remaining key concern in attributing the workers' productivity drop to social signaling is that it could be driven by coworkers' actions rather than the worker's own effort. In particular, if the workers rely on help from their coworkers for their production, ranking competition might re-

[5] A *friend* is defined as a coworker who a worker socializes with outside the factory.

[6] I measure relative rank with friends using the difference between a worker's rank and the median rank among their friends.

duce their productivity by reducing cooperation from their coworkers. I preemptively address such concerns by measuring cooperation among the knitting workers before and during the intervention. To be concrete, I randomly assign the workers to 20-minute windows, an *observation*, during which their interactions with coworkers are recorded by observing them from a distance (n=480). This provides several valuable insights. First, the intensity of cooperation among the workers is modest at baseline – a worker engages in an act of cooperation with a coworker once in about 100 minutes (five observation slots), for an average duration of 60 seconds. Second, after the ranking begins, the likelihood of cooperation decreases by half and the duration reduces by a third across all the workers. Third, the likelihood of cooperation is lower for the treated workers than for Control workers after the ranking begins. Finally, compared to the pre-intervention period, the share of cooperation that involves help that is harder to refuse (for example, lending a design chart and helping with machine) does not decrease, but the share of voluntary help in the form of offering production tips or suggestions does. The set of evidence suggests that the workers partially withdraw cooperation because of the ranking competition. However, comparing workers with high and low likelihoods of engaging in cooperation with coworkers at baseline suggests that the drop in such interactions has a limited adverse effect on the workers' overall productivity. Importantly, this does not affect the estimated productivity drop associated with relative ranks with friends.[7] The limited effect of changes in cooperation on worker productivity in this factory could be a result of the workers' low dependence on help at baseline. But the general decrease in cooperation because of the ranking competition could be more harmful in contexts where such cooperation among workers is integral to production technology.

There are two subtly different motivations that may drive the productivity response. First, in the context of ranks, a worker's effort imposes a negative externality on his friends when it suppresses their ranks. Second, an enthusiastic effort by a worker to outrank his friends may be interpreted as a desire to distinguish himself from his friends and social group. In both cases, the worker may worry about being perceived as self-centered by his friends and about losing the social ties. Consequently, he may reduce his effort to signal social compatibility to them. However, in the first case, he does it to internalize the negative externality of his effort on his friends. In the second, he does it to socially conform to his friends' productivity.[8]

[7]It is plausible that the workers collaborate less with coworkers in general but increase collaboration with friends. Unfortunately, the sample does not contain sufficient instances of cooperation during the intervention to evaluate this possibility.

[8]For empirical examples on internalizing externality, see Bandiera et al. (2005) and Mas and Moretti

I exploit pair-wise rank-distances between a worker and his friends to distinguish between the two motivations. Notice that the externality of a worker's effort on his friends is the most salient when his friends are ranked close to him so that marginal changes in his effort affect his friends' ranks. Conversely, the worker may worry about being seen as standing out from his friends or social group when he outranks his friends by a substantial distance. The evidence shows that publicly-ranked workers reduce productivity *only* when the distance between them and their outranked friends is substantially high. Moreover, the drop in productivity does not translate into better ranks for their friends; it only reduces the distance between them by lowering the workers' ranks. This is consistent with the workers reducing their effort to conform to group productivity.

The paper contributes to a growing body of empirical literature on the effect of performance ranks on worker productivity. Although theory predicts an unambiguous increase in worker productivity from public ranks because of the workers' status-seeking behavior (Moldovanu et al. (2007); Besley and Ghatak (2008)), empirical evidence has been inconclusive. Delfgaauw et al. (2013), Ashraf et al. (2014b), Song et al. (2017), Ager et al. (2021) and Englmaier et al. (2021) find positive effects, while Ashraf et al. (2014a) and Blader et al. (2020) find negative effects. Understanding the mixed evidence from public ranks becomes further complicated by private ranks also generating mixed effects. Blanes-I-Vidal and Nossol (2011) and Gill et al. (2019) find positive effects while Barankay (2011, 2012) find negative effects, indicating that ranks generate ambiguous effects from underlying intrinsic incentives alone. Using an experiment to carefully disentangle the underlying intrinsic and social incentives, this paper shows that the status-seeking incentives from public performance ranks may be counteracted by workers' social preference for conformity to their social groups in the workplace. This resonates with findings in Blader et al. (2020) who show that workers consider competition with peers inconsistent with a collective spirit in the workplace.

The paper also makes a methodological contribution by measuring cooperation in a typical workplace-setting. Empirical studies that explore such cooperation have been largely limited to laboratory settings (e.g. Carpenter et al. (2010); Harbring and Irlenbusch (2011); Danilov et al. (2019)) or sports (e.g. Deutscher et al. (2013); Garicano and Palacios-Huerta (2014)). Evidence from regular workplaces has been scarce as it is extremely difficult to measure cooperation in such settings. This paper adds to this literature by measuring cooperation among workers in a manufacturing firm and by pre-

(2009). For theoretical models on conformity, see Bernheim (1994) and Akerlof (1997); for empirical example, see Bursztyn et al. (2019).

senting evidence on how such cooperation is adversely affected by an incentive scheme that triggers competition among the workers.

More broadly, the paper contributes to the literature that studies workers' social preferences.[9] A closely related paper is Bandiera et al. (2005) which shows how university students working as temporary fruit pickers on a UK farm withdraw effort under a *relative pay scheme* to internalize the negative externality of their effort on their peers' earnings. Using relative performance ranking, my paper complements Bandiera et al. (2005) in showing how pitting workers against each other may be counterproductive because of their social preferences. Yet, there are two key differences between the two papers. First, the relative incentive scheme in Bandiera et al. (2005) involves pay and introduces a trade-off between monetary incentives and social incentives. In my paper, on the other hand, the trade-off is between two social incentives. In the process, this paper sheds light on how multiple kinds of social preferences can be triggered by the same incentive structure and how such preferences may interact. Second, workers in Bandiera et al. (2005) reduce their effort to internalize its negative externality on their peers. On the other hand, in my paper, the workers reduce their effort to conform to their social group's productivity. Although social conformity has been documented in settings such as investment decisions in education (Bursztyn et al. (2019)), energy consumption (Allcott (2011)), and laboratory experiments (Gächter et al. (2013)), evidence from real workplaces is relatively scarce.[10] This paper shows how social concerns can lead workers to undercut performance and earnings to exhibit conformism on productivity. This has implications for the organization of workers inside firms.

The rest of the paper is organized as follows. Section 2 describes the sweater factory where I conduct the experiment. Section 3 discusses the key incentives underlying performance ranks and describes the experimental design. Section 4 presents the main results on worker productivity. Section 5 tests their robustness while Section 6 explores alternative interpretations. Section 7 examines underlying mechanisms. Section 8 concludes.

[9] A booming literature on workers' social preferences has documented several forms of preferences. For example, workers exhibit warm glow at work (DellaVigna et al. (2022)), enjoy working with friends (Bandiera et al. (2010)), internalize externality of their actions on peers (Hamilton et al. (2003), Mas and Moretti (2009)), and reciprocate to employers positively (Gneezy and List (2006), Jayaraman et al. (2016)) or negatively (Casaburi and Macchiavello (2015), Akerlof et al. (2021)). A related stream of work studies relative incentives among workers. See for examples, Card et al. (2012); Contreras and Zanarone (2017); Breza et al. (2018) and Perez-Truglia (2020).

[10] An exception is Bandiera et al. (2010) where workers in a fruit-picking farm in the UK adjust working speed to be able to spend more time with their friends.

2 Setting

The experiment for this paper takes place in the Manual Knitting Department of a large sweater factory in Bangladesh. Workers in this department knit yarns into sweater parts, which are later passed on to other departments for the remaining production processes. At the start of the experiment in January 2016 (*baseline*), the department consisted of 366 knitting workers. They are all part of the experiment.

The knitting workers knit sweaters through an individual-production process. At a given time, they work on a batch of sweaters or a *job* that usually constitutes 12-48 pieces of sweaters of a particular style and size. Once a worker completes the job in hand, he receives his next. The next job can be sweaters of the same style and size or a different combination.[11] The workers receive a monthly payment depending on the number of sweaters they knit and corresponding piece rates. The rates vary by styles. The workers are organized into 15 administrative groups called *blocks*. Each block is supervised by one supervisor whose role is primarily limited to instructions and troubleshooting.

Several attributes of this department make it a suitable setting for the experiment in this paper. First, the individual-production process makes it possible to measure and compare productivity across workers. Second, the workers exhibit relative preferences concerning productivity. During a baseline survey before the experiment, more than 90% of the workers admit comparing their productivity with at least one coworker in their block. Conditional on making such comparisons, the workers compare themselves to 10 coworkers on average or about 40% of the coworkers in their block. This supports performance ranking as a relevant management tool in this setting. Third, the workers have strong social ties, which lends a rich social environment to the paper to explore social incentives associated with performance ranking. Indeed, during the baseline survey, 345 of the 366 workers report socializing with at least one of their coworkers outside the factory (henceforth, *friends*).[12]

The workers, however, have imprecise information on how their productivity compares with their coworkers'. This is again reflected in the baseline survey. The workers get almost 40% of bilateral comparisons wrong, even concerning coworkers they admit to comparing themselves to.[13] This is possibly because the workers work on multiple styles of sweaters each month. The composition of a worker's styles does not usually match

[11] I use a masculine pronoun as almost all the knitting workers are males.

[12] Like in a regular workplace, the knitting workers had been working at the factory for about five years on average. This could explain the high prevalence of social ties among the workers.

[13] The workers were asked to make comparisons based on production in the previous three months.

his coworkers', making it difficult to compare physical productivity across workers in a meaningful way. The workers could, alternatively, use monthly production earnings for comparison. The earnings use the sweaters' piece rates to translate physical quantities of sweaters into a comparable metric of productivity. However, the rates are determined centrally by the factory's managers and often do not accurately reflect the complexities of the sweaters. Moreover, a given sweater style pays the same rate regardless of the sizes, which often vary from extra-small to extra-large and require different lengths of time to knit. As a result, some styles and sizes become more (or less) rewarding to knit than others.[14] Therefore, the monthly earnings serve only as an imprecise measure of productivity.

In summary, the knitting department provides a context where social comparisons of productivity are common but inaccurate. Performance ranking based on precise measures of individual productivity can provide new information that the workers value. An existing social network allows us to understand how the provision of such information interacts with the workers' social incentives. The following section describes an intervention with performance ranking that this paper introduces in the knitting department.

3 The Experiment

I conduct a five-month-long intervention at the Manual Knitting Department of the sweater factory. All the knitting workers in the Department receive performance feedback, but only treated workers receive performance ranks. Below, I discuss the key incentives underlying such ranks and describe the experimental design that disentangles the effects of these incentives on worker productivity.

3.1 Incentives from Ranks

Performance ranks distinguish relatively more productive workers from less productive ones. Seminal papers such as Lazear and Rosen (1981) and Green and Stokey (1983) suggest that, under certain conditions, tying workers' pay to their performance ranks may create a more efficient incentive than paying them piece rates. More recent theoretical papers such as Moldovanu et al. (2007) and Besley and Ghatak (2008) suggest that, even without monetary rewards, publicly ranking workers on their performance may induce

[14] Anecdotal evidence suggests that the workers are aware of this phenomenon and often express a preference for one style over another.

them to increase their effort. The workers would do so to pursue social recognition or status associated with high ranks. Several papers, such as Ashraf et al. (2014a) and Ager et al. (2021), do provide empirical support for positive productivity effects from public ranks without monetary rewards.[15]

However, signaling high productivity may not be the only signal workers care about. The workers may value social ties with their coworkers and therefore, they may want to signal their social compatibility to their coworkers. The workers may worry about being too keen to outrank their peers, lest they are perceived as self-centered and awarded social sanctions by coworkers they like to share social ties with. There are two reasons why pursuing high ranks may be socially frowned upon. First, in the context of ranks, a worker's effort imposes a negative externality on his peers by suppressing their ranks while improving the worker's own. Second, an enthusiastic effort to outrank one's social group may be interpreted as a desire to distinguish himself from the group, for instance, if the groups form with coworkers with similar productivity. The first concern can lead a worker to decrease his effort to internalize its externality on his peers. Empirical examples of such behavior in real workplaces can be found in Bandiera et al. (2005) and Mas and Moretti (2009). The second concern can lead a worker to decrease his effort to conform to group productivity. Conformism driven by group identity has been modeled by, for example, Bernheim (1994) and Akerlof (1997). Bursztyn et al. (2019) provide empirical evidence for such behavior among students.[16]

Note that public ranks may trigger both social and intrinsic incentives associated with ranks. Intrinsic incentives, unlike signaling motivations, derive from the private consumption of information from ranks. There are two kinds of information that the workers may obtain from the ranks. First, the ranks give the workers information about their own rank. Revelation of this information may have an ambiguous effect on their productivity depending on their intrinsic valuation of the rank and how it revises their corresponding priors. Empirical papers that provide workers with information on ranks in private do find substantial, and sometimes contradictory, effects on worker productivity (see Gill et al. (2019); Blanes-I-Vidal and Nossol (2011), and Barankay (2011, 2012)). Second, public ranks provide the workers with information about their peers'

[15]Workers may also want to signal high productivity to the firm because of career concerns (Holmstrom (1999)). In this paper's context, however, it is unlikely that the workers are driven by career concerns. At baseline, the salary of a supervisor, the next level in the job hierarchy above knitting workers, is less than the earnings of a knitting worker at the 33rd percentile of the production earnings distribution. The best workers are unlikely to desire promotion to a supervisor level.

[16]It is possible that lower-ranked workers, on the other hand, increase effort to conform to group productivity. I will consider this empirically equivalent to increasing effort because of status rewards.

ranks. Depending on how important the coworkers' relative ranks are for their intrinsic motivations and their corresponding priors, the workers may re-optimize their effort.

In summary, public ranks introduce multiple incentives for the workers. By increasing the visibility of ranks, the ranking introduces social incentives to the workers, potentially with a trade-off between signaling high productivity for status and social compatibility. The net effect of these incentives on worker productivity is a-priori ambiguous. In addition, public ranks may also affect the workers' intrinsic motivations by providing them information about their and their coworkers' ranks. The experimental design of the intervention, discussed in the next section, aims to disentangle the effects of these incentives.

3.2 Experimental Design

A five-month-long intervention at the Knitting Department provides the knitting workers monthly feedback on their production in the previous month. Delivered through individually addressed letters, the feedback is first provided at the beginning of February 2016 and continues at monthly intervals till June 2016.[17]

A primary challenge in providing performance feedback to the workers is to measure their performance. The individual production process and the piece-rate-based pay at the factory help to measure individual performance at the monthly level but with imprecision (see Section 2). The intervention builds on it by systematically recording the time it takes a worker to complete a job. This enables accurate productivity measurement at the job level by computing the average time a worker takes to knit a sweater in a job.[18]

Subsequently, the workers receive one of three kinds of feedback. The first provides aggregate information about the workers' production in the previous month. This includes the total number of sweaters they produced, its breakdown by styles, total production time, and a list of all the coworkers who receive similar feedback.[19] The workers who receive this feedback, henceforth *Control* group, do not receive performance ranks.

The second feedback provides the workers with the same information but with ad-

[17]The factory management initially intended to use performance ranking as a permanent management practice. As a result, workers are not told a termination date for the intervention.

[18]The times are recorded centrally by *Distributors* of the jobs, a different group of workers in the knitting department. Prior to the intervention, they recorded various information about the jobs for official records except the times. The records are inaccessible to the workers.

[19]It is plausible that the workers remember the sweaters they knitted in the previous month in any case. The feedback provides an administrative summary of it, a breakdown by styles, and additional information on the total time it took them to produce the sweaters.

ditional information about their and their coworkers' ranks. The workers see the ranks of their peers besides the list of coworkers who receive this feedback. This *Public Treatment* introduces social signaling incentives associated with ranks by making the ranks publicly visible.

Workers may also derive intrinsic utility from their *own* rank. Thus, learning about their ranks could independently lead to additional productivity effects. To estimate and control for such effects, the third feedback provides the workers with ranks in private. Workers who receive this feedback, *Private Treatment*, receive the same information as the Control group but receive additional information about only their own ranks.[20]

A novel characteristic of the experimental design is that it distinguishes between intrinsic and social incentives associated with the revelation of one's ranks. Specifically, Private Treatment triggers intrinsic incentives associated with revealing one's rank to himself. Public Treatment additionally introduces social incentives by revealing a worker's rank to his coworkers. The Control group helps to decompose the total effect from public ranks into the two components.[21]

To estimate the potential productivity effects of information about *peers'* ranks that the workers in Public Treatment receive, the baseline survey measures the workers' priors about how their productivity compares with each of the workers in their blocks (including friends).[22] This helps to identify the priors that are later revised upward or downward because of the information provided through Public Treatment. In turn, this helps to test the effects of such revision on productivity.[23]

To disentangle the productivity effects of the social incentives into the two signaling motivations, notice that the incentive to signal social compatibility is more likely to be triggered when a worker directly competes for ranks with a coworker he is socially connected with. This is because: (i) whether a worker is keen to outrank a particular coworker is only observed when the two compete against each other, and (ii) social sanctions may be especially costly when imposed, individually or collectively, by coworkers they are socially connected with. Therefore, the baseline survey maps the workers'

[20]Following Barankay (2012), I also provide the treated workers with additional information on the ranks they can achieve if they improve knitting time by 5, 10, and 20 percent. This helps them to learn how much faster they would need to work to improve their ranks.

[21]Note that the letters delivered to the workers in Control arm account for potential productivity effects from receiving the letters or concerns about their work being tracked by the management.

[22]A similar exercise with coworkers from outside block was difficult because of the workforce size.

[23]Because the intervention continues for several months, a fourth experimental arm that revealed peers' ranks but kept the workers' ranks private risked a backlash and additional behavioral responses from the workers. This could contaminate the treatment effects. This would have also decreased the design's statistical power to estimate the effects.

social network before the intervention. This later helps to identify the workers who compete with *friends* - coworkers they socialize with outside work - and, therefore, have an incentive to signal social compatibility.

The ranks are computed using the average knitting time per sweater.[24] Workers in each treatment are ranked only against coworkers in the same treatment. Thus, the workers in the Public Treatment receive a self-contained ranking of coworkers.

Productivity of the workers in Public Treatment may also be affected by actions of their coworkers, especially those who are also in Public Treatment. In that case we may observe changes in the workers' productivity even if their own effort does not change. This would be particularly true if the workers depend on help from their coworkers for their production. The ranking competition may induce the workers to help each other less often and lead to a drop in the workers' productivity. To test if this happens, I measure cooperation among the knitting workers before and after the ranking begins. I discuss this in Section 6.2.

Workers are individually assigned to one of the three experimental arms through a public lottery in January 2016.[25] They receive the same kind of feedback throughout the intervention. A concern in this context is contamination of treatment across the arms. For instance, workers in Private Treatment may reveal ranks to each other. To estimate such effects, I experimentally vary the intensity of the most intensive treatment - public ranks - across the blocks. Specifically, before assigning the workers to the experimental arms, I randomly assign eight of of the 15 blocks as Public-Intensive and the remaining seven as Private-Intensive. One-third of the workers in each block are assigned to Control. In Public-Intensive blocks, Public Treatment contains two-thirds of the remaining workers and Private Treatment contains the rest. The weights are reversed in Private-Intensive blocks. This results in an almost equal distribution of workers across the three experimental arms - 125 workers in Control, 117 in Private Treatment, and 124 in Public Treatment.[26] 197 of these workers are in Public-Intensive blocks, and 169 in Private-Intensive blocks.[27]

[24]Section A.1 in the Appendix detail how these ranks are calculated. The letters describe to the workers every month how these ranks are computed.

[25]The workers pick a number to decide what kind of feedback they will receive. This makes it transparent as to why a worker receives a specific type of feedback.

[26]Some minor attrition of workers happens as the intervention progresses, but it is largely balanced across the arms. By the end of the study period, 16 workers leave the factory - five from Control, four from Private, and seven from Public Treatment.

[27]An alternative experimental design could cluster-randomize at the block level. However, the availability of only 15 blocks in total limits the benefits of such a design.

3.3 Data

I use data from three main sources:

1. Administrative Records from Factory: These provide demographic and production-related information for all the knitting workers in the experiment. The records include age, tenure, block assignment, and job-level production details for over 22,000 jobs during the study period (January-June 2016).[28] They also include monthly production earnings, monthly attendance records, and piece rates of sweaters starting from January 2015.

2. Baseline Survey: A baseline survey in October 2015 collects information on the workers' social network in the factory, including information on the peers a worker interacts with inside the factory, the frequency of the interactions, and the peers they socialize with outside the factory. The survey also collects information, on the workers' priors about their productivity relative to their coworkers. It also measures the workers' attitudes towards competition and risk.[29]

3. Cooperation: By directly observing the workers, I measure cooperation among the workers before and during the intervention. More on this in Section 6.2.

Table 1 reports descriptive statistics for the knitting workers in the three experimental arms and verify that they are balanced in observable characteristics at baseline. Columns 2-4 report the means of these characteristics for each arm. Columns 5-7 report p-values from the tests of equality of these means.

The first panel verifies that the mean productivity of workers at baseline, measured in several ways, is similar across the three arms. The most preferred measure is the knitting time per sweater, which is the number of minutes a worker takes to knit a batch of sweaters in a job divided by the number of sweaters. The knitting times are available only for one month before the intervention (January 2016). Therefore, they are susceptible to short-term shocks and potentially unable to capture the long-term productivity of the workers. Hence, I also test alternative productivity measures –

[28] The factory begins recording job-level production time only in January 2016.

[29] I measure attitude towards competition with a lab-in-the-field game in which workers throw balls into a basket (see Gneezy et al. (2009) for an example). The workers choose how to get paid for each successful shot - at a fixed piece rate (non-competitive) or double the rate if they score more than a randomly chosen coworker (competitive). I measure risk using a self-reported assessment of the workers' risk-taking behavior on a scale of 1 to 10.

production earnings, the total number of sweaters produced, and attendance – that are available since January 2015 but only at a monthly interval.

The second panel shows that the workers are also balanced in terms of demographic characteristics. These include the number of years in school, age, tenure at the factory, and attitudes towards competition and risk.

Finally, the third panel shows that the workers are balanced in terms of their social network measured using the number of coworkers in block and the number of friends they report at baseline. The latter is identical between the control and the treatment arms but slightly higher in Public than Private Treatment. Joint-significance tests, however, cannot reject that the variables do not jointly predict treatment status.[30] Finally, the productivity ratio of friends inside and outside a worker's assigned arm is close to one in magnitude in all the arms, indicating that the friends randomized in the arm alongside the worker are representative of his entire network in terms of productivity.

4 Productivity Response To Ranks

I now turn to the main analysis. I begin by estimating the average treatment effects of the ranking on worker productivity. I find that although private ranks do not affect worker productivity on average, public ranks decrease productivity by more than 1.5 percent. In other words, revealing the ranks to only the workers themselves does not affect worker productivity on average. But revealing the ranks to their coworkers leads to a net decrease in their productivity. Subsequent evidence confirms that this is driven by the workers' social concerns, namely their desire to signal social compatibility to their friends.

4.1 Average Treatment Effects

The main specification to estimate average treatment effects on productivity is given by:

$$Y_{isbt} = c + \beta_1(Public_i) + \beta_2(Private_i) + \alpha_i + \lambda_s + \tau_t + X_{isb} + \epsilon_{isbt} \tag{1}$$

Y_{isbt} is worker i's productivity in knitting sweater of style-and-size s in job b in month t. The most preferred measure of productivity is the average time a worker takes to knit a sweater in each job he completes. Indicator variables $Public_i$ and $Private_i$ refer to

[30] Also, specifications in the main analysis with and without the number of friends yield similar results.

worker i's treatment status. I use the logarithmic transformation of the knitting times to interpret the coefficients of key interest β_1 and β_2 as percentage changes in productivity. A positive coefficient indicates lower productivity.

I control for worker i's prior productivity α_i to increase power.[31] The knitting times are available only for one month before the intervention and hence, susceptible to short-term shocks. Therefore, I measure α_i with worker i's average production earnings over January-December 2015.[32] Sweater style-and-size fixed effects λ_s capture variations in knitting times because of sweater characteristics (e.g., sweater complexity or dimensions). τ_t are month fixed effects, and X_{isb} are additional controls for worker, style, and job characteristics. Because treatment intensity varies across blocks which are only 15 in total, I cluster and bootstrap standard errors at the block level.[33]

Column 1 of Table 2 estimates Equation 1 controlling for productivity at baseline and style-and-size fixed effects. This estimates changes in the knitting times of the sweaters while controlling for sweater characteristics and the workers' baseline productivity. The estimated average treatment effect of private ranks is close to zero in magnitude. On the other hand, making ranks public significantly decreases worker productivity. Knitting time per sweater increases by more than 1.5 percent, statistically significant at 10% significance level. This is equivalent to a loss of half a day's production per month per worker on average. In comparison, note that the literature usually finds a 0.3% to 0.8% increase in productivity from a 10% increase in piece rates (Tonin and Vlassopoulos (2015); DellaVigna and Pope (2018); Goerg et al. (2019)).

The next specifications test if the estimates are robust to additional controls. Column 2 adds month fixed-effects to control for seasonality. Column 3 adds controls for the workers' sweater-specific experience measured by the cumulative number of jobs in which the workers worked the same style and size. Column 4 additionally controls for worker characteristics, including tenure at the factory (at baseline), the number of years the workers attended school, self-reported index of their risk-taking behavior, and their attitude towards competition. The estimates remain robust in magnitude and statistically significant at traditional levels of significance.

Figure A1 looks beyond the mean effects of the treatments and investigates how they

[31] This is in the spirit of ANCOVA specifications. See McKenzie (2012) for a formal discussion.

[32] The noise in production earnings as a measure of productivity, stemming from differences in piece rates and allocation of sweaters styles and sizes, is likely to average out over the long run. In any case, the results are similar or higher in magnitude if I use style-and-size adjusted job-level knitting times from January 2016 to control for baseline productivity but slightly noisier.

[33] The results are robust to clustering errors at the worker level instead.

affect the entire distribution of productivity. Because sweaters differ in styles and sizes, differences in the knitting times of sweaters across workers can be due to differences in the workers' productivity as well as differences in the sweaters' styles and sizes. To isolate the shares of worker and sweater characteristics in the knitting times I regress the (job-level) knitting times on the sweaters' style-and-size fixed effects and calculate the mean of the residuals for each worker. Figure A1 plots these mean residuals. The left panel shows that the distributions of the style-and-size adjusted average knitting times per worker are similar across the three arms – as they should be – before the ranking begins. The right panel, on the other hand, shows that the knitting times increase among publicly-ranked workers after the ranking begins. Importantly, it increases all along the productivity distribution. Mirroring earlier results, the distribution for privately-ranked workers remains largely similar to that for Control workers.[34]

Public ranks lead to an economically large decrease in average productivity. In contrast, private ranks have a minimal effect.[35] The difference in the productivity effects of the two treatments, around one percentage point, is negative which indicates that making ranks publicly visible has a net negative effect on worker productivity (mean p-value$\approx$0.15 across the four specifications in Table 2). The next sections investigate if this is truly driven by the workers' social concerns.

4.2 Disentangling Social Incentives from Ranks

I now investigate the extent to which the negative productivity effects from public ranks are driven by the workers' social concerns. To do so, note two issues. First, public ranks confound the social incentives associated with making ranks visible to coworkers with the intrinsic incentives associated with learning own and peers' ranks. It is crucial to control for the latter incentives to isolate the former. Second, public ranks introduce a choice between two social signals: high productivity for social status and social compatibility to friends. Publicly-ranked workers who do not compete with friends do not require to signal social compatibility. They have an unambiguous positive incentive to increase effort to pursue status rewards from ranks. On the other hand, workers who compete with friends must choose between increasing effort for status rewards and decreasing

[34]To be strict, the residuals across jobs also reflect differences in productivity induced by cumulative experience of knitting sweaters of the same style and size (see Column 3 of Table 2). Controlling for job sequence, however, yields identical distributions.

[35]The null effect from private ranks is not due to a lack of new information from the ranks. The first feedback provides ranks that are more than 10 ranks away from the workers' priors (at baseline) for more than 80% of the privately-ranked workers.

effort to confirm social compatibility to friends. Any change in their productivity is likely to be less favorable for the firm. It is essential to distinguish between workers who do and do not compete with friends to understand the effects of the two social concerns.

Taking cue from these observations, Table 3 first distinguishes between workers who do and do not compete with friends. Following Table 2, Column 1 compares Public Treatment with Control. The coefficient for the variable *Public* captures the productivity difference between Public Treatment and Control for workers who have at least one friend randomized into the same arm. The sum of the coefficients for *Public* and *Public*1(Do Not Compete With Friends)* does the same for the workers who do not have any friend in the same arm. The value of this sum suggests that publicly-ranked workers who do not compete with friends respond to the intervention by increasing productivity by more than 4.5 percent (p-value<0.07). In sharp contrast, publicly-ranked workers who compete with friends reduce productivity by more than 2.5 percent (p-value<0.02). This suggests that the negative average treatment effect of Public Ranks reported in Table 2 is entirely driven by the workers who compete with friends.

A concern in interpreting the productivity drop as being driven by competition with friends is that workers who compete with friends are different from those who do not. For instance, the more friends a worker has at baseline, the higher the probability that at least one of them is randomized into his experimental arm. The estimated productivity differences could be driven by differences in worker characteristics. Note that the estimates above compare publicly-ranked workers who compete (res. do not compete) with friends to Control workers who would have competed (res. would not compete) with friends if they were ranked. This holds constant all the underlying factors correlated with the likelihood of competing with friends and exploits the experimentally induced competition among the workers. Column 2 additionally controls the number of reported friends at baseline for completeness. The results remain almost identical. Nonetheless, I will return to the issue of endogenous worker characteristics in Section 5.2.

The estimates above confound the effects of social incentives associated with public visibility of ranks with intrinsic incentives associated with the information that the workers learn from public ranks. I first disentangle the effect of learning one's *own* rank and then test the effect of learning coworkers' ranks.

Recall that productivity effects of learning one's *own* ranks should exist even when workers learn their ranks privately. So, Private Treatment serves as a control for such effects. Before isolating the two sets of incentives, it is useful to first check how workers who do an do not compete with friends respond to private ranks. It is possible that

the two sets of workers respond differently even under private ranks. Column 3 tests how workers who do and do not compete with friends in Private Treatment vary in their productivity responses compared to those in Public Treatment. While the corresponding estimates for Private Treatment share the same signs, they are substantially smaller in magnitude and estimated with a high degree of noise. Public visibility of ranks appears to be the key driver of the productivity responses in Public Treatment.

Column 4 isolates the productivity effects of learning own ranks. It re-estimates the specification in Column 2 but now compares Public Treatment directly to Private Treatment instead of Control.[36] The estimates capture the effects of public ranks above and beyond the impact of learning one's ranks. The estimates show that, compared to similar but privately-ranked workers, publicly-ranked workers who compete with friends reduce productivity by about 1.8 percent on average (p-value$<$0.06). On the other hand, workers who do not compete with friends increase productivity by two percent. The latter, however, is imprecisely estimated.

Next, I estimate the effects of learning new information about coworkers' productivity. The baseline survey records the workers' priors about how they compare with each of their block coworkers in terms of productivity. After the ranks are revealed in the first month, some of these priors are confirmed by the revealed ranks while others are proven incorrect. The latter set of priors provides a proxy for the new information that the workers learn about their coworkers' productivity.[37]

Learning about peers' relative productivity hardly affects the workers' productivity. This is seen in Column 5. It computes the number of incorrect predictions made by the workers at baseline and lets this affect publicly- and privately-ranked workers differently. We expect this to have different effects on the two groups of workers as the former group may update a subset of their priors while the latter does not learn anything new. But the number of incorrect priors at baseline is not correlated with ex-post productivity of either the workers in Private Treatment or those in Public Treatment. In particular, the coefficient for the interaction term is small and imprecisely estimated. The coefficient for the variable *Public* increases slightly in magnitude, but it now reflects the productivity response of only the workers who get all their comparisons correct.

In summary, the negative productivity effect of public ranks is entirely driven by workers who compete with friends. As much as two-thirds of the productivity drop

[36]The counter-factual arms are reported at the bottom of the columns.

[37]This exercise is limited to coworkers in their blocks as doing the same for coworkers outside block was not feasible because of the sheer size of the workforce.

remains after accounting for intrinsic incentives from ranks. The preferred interpretation is that it is driven by the workers' social concerns, in particular, their desire to signal social compatibility. The following section conducts tests to confirm this interpretation.

4.3 Signaling Social Compatibility

The previous section highlights that publicly-ranked workers reduce productivity particularly when they compete against their friends. This response could stem from social concerns but not necessarily from a desire to signal social compatibility to their friends. For instance, this may be driven by a general distaste towards competition with friends rather than concerns about relative rank-differences with them. On the other hand, signaling social compatibility in this context is inherently intertwined with relative rank-differences with friends and becomes especially important when the workers worry that their friends would perceive them as keen to outrank them. It then follows that, if such signaling were indeed at play, we expect the workers to reduce productivity particularly when they fare better in ranks than their friends.

In Table 4, I test if publicly-ranked workers are sensitive to *outranking friends*. I test this in two parts. First, I test if and how a worker's relative ranks among friends affects his productivity response. To do this, I measure the rank-distance between a worker and his friends as the difference (in units of 10 ranks) between a worker's rank and the median rank among all his friends based on the ranks they receive in the first feedback. Second, I test if it is relative ranks only among the friends that matters.

Columns 1-2 test the importance of *outranking* friends. Column 1 tests the workers' response to their rank-distance with their friends, provided the workers rank higher than the median rank among the friends. Indeed, publicly-ranked workers, compared to similar but privately-ranked workers, monotonically decrease productivity as this distance increases. With an increase in the distance by ten ranks, the workers reduce productivity by one percent. I impose a value of zero for this rank-distance for workers with no competing friends. Thus, the coefficient for the variable *Public* jointly captures responses of the workers who rank below the median and those who do not compete with friends.

Column 2, on the other hand, tests the workers' response provided that the workers rank lower than their friends in the first feedback. The estimates show that the workers are insensitive to their rank-distance with their friends if they rank lower than the median. The difference in the estimated productivity responses from above and below

the median is statistically significant at the 5% significance level. Thus, publicly-ranked workers are averse to outperforming their friends in ranks but are indifferent about under-performing them. The absence of an increase in productivity when they rank lower than their friends suggests that high effort might have become stigmatized among the workers from the introduction of public ranking.

Next, I test if it is uniquely the *friends* that the workers worry about. To do so, first, Column 3 tests how the workers respond to rank-distance with the coworkers they are *not* friends with. In sharp contrast to the previous specifications, the interaction term for the rank-distance above median is close to zero in magnitude. Column 4 simultaneously tests the effects of rank-distance with these coworkers and that with friends. While the latter coefficient is large and significant, the former remains small and insignificant; it even has the opposite sign now. Column 5 considers an even narrower reference group that includes only the coworkers who, although not friends, frequently interact with the workers during work. The workers do not respond to rank-distance with these coworkers either but strongly react to that with friends. Thus, the sensitivity to outranking friends does not seem to be an artifact of merely the repeated interactions with their friends at work but is potentially associated with deeper social ties.

The negative response to rank-distance with friends should not exist when the ranks are not publicly visible. Therefore, for completeness, Column 6 estimates the association between productivity and rank-distance with friends separately for publicly- and privately-ranked workers. The counterfactual workers are similar workers in the Control arm. The negative association between productivity and rank-distance with friends prevails for publicly-ranked workers. Reassuringly, no such association exists for privately-ranked workers.

In summary, the productivity drop among publicly-ranked workers is triggered by the public nature of the ranking and is particularly strong when the workers outrank their friends. The results are consistent with the interpretation that the workers worry about upsetting their friends by outranking them and therefore, reduce effort to signal social compatibility.

5 Robustness Tests of Identification

I now conduct several robustness tests to verify the measurement and interpretation of the results in the previous section. The key result of the paper - publicly-ranked workers who outrank their friends at baseline have lower productivity during the intervention

compared to similar but privately-ranked workers - relies on comparing workers who rank high within their networks in the two treatment arms. There are two key concerns in attributing this result to social incentives. First, the subsets of workers in the two experimental arms may differ in productivity already before the intervention. In that case, it can no longer be interpreted as a productivity response induced by the intervention. Second, even if the differences in productivity surface after the intervention, it may be driven by endogenous worker characteristics correlated with whether a worker competes with and outranks his friends. I carefully test and rule out these concerns below. Section A.2 in the Appendix shows that the results are robust to alternative measures of relative productivity. I also probe into possible contamination effects across the experimental arms and confirm that they do not drive the results.

5.1 Pre-existing Differences

The workers in Public Treatment who rank higher than friends at baseline reduce productivity compared to similar workers in Private Treatment. A potential concern is that this difference between the two experimental arms, when distinguishing between workers by their relative productivity within network, predates the intervention. For instance, although the correlation between productivity at baseline and rank-distance with friends is likely to be positive, this correlation could be lower in size among publicly-ranked workers than among privately-ranked workers. In that case, the negative productivity estimates for the high-performing publicly-ranked workers during the intervention would simply reflect the pre-existing productivity differences with privately-ranked workers at baseline rather than a response to the public ranks.

To assuage such concerns, Column 1 of Table 5 tests if the negative association between productivity and rank-distance with friends among publicly-ranked workers, compared to privately-ranked workers, exists before the first treatment letter is delivered. This is a similar specification as Column 4 of Table 3 but with pre-intervention job-level data from January 2016.[38] It is reassuring that no such association exists before the workers learn their actual ranks. This indicates that the productivity differences during the intervention is driven by a reduction in productivity by the publicly-ranked workers in response to the intervention.

[38]A similar but unreported test using monthly production earnings from 2015 yields qualitatively similar results.

5.2 Endogenous Worker Characteristics

Another concern is that the negative association between productivity and rank-distance with friends is driven by endogenous worker characteristics that correlate with whether a worker ranks higher or lower among his friends. We would be particularly concerned about the factors associated with the likelihood of the worker competing with friends and, conditional on competing, ranking higher than them. It is plausible that the workers respond differently to public ranks than to private ranks because of these underlying characteristics instead of the ranking per se.

Social Popularity. A pre-condition for a worker to outrank his friends is to compete with them. The probability of this increases with the number of friends he has at baseline. The higher the number of friends, the higher the probability that he finds at least one of them in his experimental arm. Thus, in principle, the previous results may capture productivity differences between socially popular and unpopular workers triggered by public ranks for reasons unrelated to competition with friends. Indeed, the coefficient for the variable *Public* in Column 1 of Table 4 captures productivity responses of both workers who do not compete with friends and those who do and rank below the median rank among friends. Therefore, at least in part, the interaction term in that specification captures the productivity difference between workers who do not compete with friends and those who do but rank higher than them.

To assuage such concerns, Column 2 of Table 5 re-estimates how publicly-ranked workers respond to rank-distance with friends, but restricts the sample to only the workers who competes with at least one friend. The estimate remains similar to that in the full sample and reassures us that the results are not driven by including in the sample workers who do not compete with their friends. Column 3, on the other hand, does a similar exercise but uses the full sample and controls for the number of friends a worker reports at baseline. Importantly, the specification lets this affect publicly- and privately-ranked workers differently. Again, the key estimates remain robust.

Baseline Productivity. Outranking a friend requires not only competing with them but also being more productive than them. Thus, the productivity response to high rank-distance with friends could reflect response by high productive workers in general, irrespective of their relative ranks with friends. This is unlikely as Columns 3-5 of Table 4 found that the workers do not respond to rank-distance with coworkers they are not friends with. This suggests that the workers do not respond to simply ranking high in general but specifically to ranking high among their friends. Nonetheless, Column

4 of Table 5 lets the workers' baseline productivity affect privately- and publicly-ranked workers differently. Not only the negative association between productivity and rank-distance with friends remains robust, the interaction term between *Public* and *Baseline Productivity* is a precisely estimated zero. In other words, just being high productive at baseline does not drive productivity differences between Private and Public Treatments.

Additional Worker Characteristics. For completeness, Columns 5-6 control for additional worker characteristics. First, Column 5 controls for baseline productivity and social popularity of the workers in the same specification. The estimates remain robust. Column 6 adds additional controls for observable worker characteristics and their interactions with treatment status. These include controls for tenure, education, and attitudes towards competition and risk. Although slightly attenuated, the coefficient of interest remains high and statistically significant.

In summary, Table 5 verifies that the productivity drop we estimate for publicly-ranked workers in earlier specifications is not driven by underlying worker characteristics correlated with how we distinguish between the workers. Our main estimate remains robust to controlling for an array of observable worker characteristics.

5.3 Contamination

Another concern relates to contamination across the experimental arms. We may be especially concerned that privately-ranked workers imitate publicly-ranked workers and share information about ranks with each other.[39] If it indeed happens and the treatment effect on privately-ranked workers is similar to that on publicly-ranked workers, the treatment effect on the latter group of workers estimated by comparing Private and Public Treatments only underestimates the true effects. Instead, it would be more concerning if the contamination somehow leads to an increased productivity among the privately-ranked workers, thereby overestimating the treatment effects.

The experimental design preemptively varies the intensity of Public Treatment across the 15 existing blocks to address such contamination concerns. Eight blocks are randomly selected to contain relatively more publicly-ranked workers than privately-ranked workers (*Public-Intensive Blocks*), while the remaining seven have the opposite (*Private-Intensive Blocks*). The Control arm constitutes a third of the workers in each block.[40] To the extent that the contamination of Private Treatment is correlated with the con-

[39] We may also be worried about the Control workers. I focus the discussion on privately-ranked workers as the relevant comparison for most of the paper is between Public and Private Treatments.

[40] See Section 3.2 for more details on the experimental design.

centration of publicly-ranked workers within blocks, we can exploit the experimental variation in the concentration of publicly-ranked workers to test for contamination.

First, Column 1 of Table A1 compares average treatment effects on privately-ranked workers, in comparison to Control, separately for private- and public-intensive blocks. The interaction term of Private Treatment and block type is positive but highly insignificant. It suggests that, if anything, an increase in the concentration of publicly-ranked workers in a block reduces productivity among privately-ranked workers, somewhat mimicking the productivity response of publicly-ranked workers. The positive coefficient for the variable that indicates the block type reflects low productivity also among Control-arm workers in the public-intensive blocks. Thus, any contamination effects from Public Treatment on privately-ranked workers may only attenuate the differences between the two treatments.

Next, Columns 2 and 3 test how privately-ranked workers in the two sets of blocks respond to their distance to the median rank among their friends. The coefficients that estimate this response in the two sets of blocks are near-zero in magnitude. Clearly, privately-ranked workers in neither private- nor public-intensive blocks are sensitive to the rank-distance with friends. The estimated decline in productivity among privately-ranked workers in public-intensive blocks in Column 1 appears to be a rather general decline than a strategic response to relative ranks among friends.

6 Alternative Explanations

Having established its robustness in the previous section, this section examines alternative explanations for the key result. The preferred interpretation of the productivity drop among publicly-ranked workers is that they do so to signal social compatibility to their friends. However, it is plausible that their productivity changes because of alternative mechanisms such as complacency, reduced cooperation, or the ratchet effect. I discuss and address these concerns below.

6.1 Complacency

Upon finding out that they rank better than their friends, instead of social concerns, publicly-ranked workers might reduce productivity because they become complacent. In other words, the workers might respond to the information about their friends' ranks they learn from the public ranks. Recall that Table 3 finds no effects from the workers'

updating of priors about their friends' relative productivity. However, it is possible that even if revising priors does not have an impact on average, adjusting priors downward has a substantial negative effect that are negated by a positive impact of adjusting priors upward. In other words, to test complacency, one must also consider the direction in which the priors are revised.

Complacency can result from two distinct pieces of information. First, workers can be positively surprised by their *own* ranks, independent of their friends'. But a change in behavior from updating prior about one's ranks should already be accounted for by comparing Public and Private Treatments. Still, it is possible that learning their ranks affects the two groups of treated workers differently. Therefore, Column 1 of Table 6 controls for whether a worker underestimated his actual rank at baseline - measured by the difference between his expected rank reported at baseline and the actual rank that he receives in the first feedback - and lets this affect publicly- and privately-ranked workers differently. The main coefficient of interest, the effect of distance to median rank among friends, remains robust.

More importantly, workers can be surprised by learning their *friends'* ranks. Column 2 of Table 6 controls for this new information and lets this affect the two groups of treated workers differently. It is a similar specification as Column 5 of Table 3 but now accounts for the direction in which the priors are revised. Specifically, it controls for the number of friends a worker predicted to be relatively more productive at baseline but proven otherwise in the first feedback. This is precisely the kind of information that could trigger complacency among the workers, if at all. Again, the coefficient of interest remains robust. The reduction in productivity when publicly-ranked workers outperform their friends does not seem to be driven by complacency.

6.2 Changes in Cooperation Between Workers

Productivity of public-ranked workers' could also decrease due to reduced cooperation among the workers induced by the competition for ranks. The public ranking is a more explicit competition than the private ranking, which would explain why we may observe a more significant decrease in cooperation and productivity from the former. If true, this would imply that the productivity drop is driven by the workers' coworkers' actions rather than the workers' own effort choice. To the extent that cooperation among workers generally increases overall productivity in the firm, ranking competition could undermine potential benefits from ranks by reducing such cooperation and be even detrimental to

the firm.

To investigate how cooperation among the workers changes because of the ranking contest, I measure cooperation among the knitting workers, both before the intervention (December 2015 - January 2016) and during the intervention (February-June 2016). Workers are observed for 20-minutes during 20-minutes-long time slots (*observation set*). Each slot is assigned to a randomly chosen worker (*focal worker*) and three workers surrounding this worker.[41] All acts of cooperation during this time between the workers within or outside the observation set are recorded with details. This leads to 480 unique observation sets containing 1,500 observations of corresponding workers. Table A3 reports the descriptive statistics about these observations and verifies that the numbers of observations and observed workers are balanced across the experimental arms. It also shows that the arms are largely balanced in the workers' characteristics[42] for the 274 unique workers observed before the ranking begins.[43]

I begin by documenting how cooperation changes after the ranking is introduced. Cooperation, an interaction where one worker helps another (henceforth, an *interaction*), involves more than one worker by definition. As this can span across experimental arms (for instance, a publicly-ranked worker helps a privately-ranked worker), changes in behavior in one arm can affect that in another. Therefore, I begin by examining the overall cooperative behavior on the floor.

The top panel in Figure 1 reports the frequency of the workers' interactions with their coworkers before and after the ranking begins. The p-values in each panel is from a test of the null hypothesis that an interaction during an observation is equally likely before and after the ranking begins. The likelihood of an interaction is calculated in three different ways. First, the top-left panel shows the average likelihood of observing at least one interaction during an observation set. This considers all the workers observed during the set as one single unit. This likelihood declines drastically from 45% in the months before the ranking to less than 20% in the months after (p-value$<$0.01). The next panel exploits information at the worker level instead. It estimates the likelihood that any observed worker engages in an interaction during an observation set (that is, it considers each of the observed workers, both focal and surrounding workers, as individual units). Although this panel's likelihoods are lower in magnitudes, 19% and 8% in months before and after the ranking begins, it reveals a similar decline in cooperation - by more than

[41] This increases the number of workers who can be observed within a time frame.

[42] That the differences in means of the characteristics are zero cannot be rejected for all but two out of 36 comparisons. But the characteristics are jointly insignificant in predicting treatment status.

[43] Additional workers could not be covered due to insufficient time before the intervention started.

half.[44] The second panel can double count an interaction if it occurs between two workers in the same set (for example, one helps, and another receives the help). Therefore, the third panel estimates the likelihood of engaging in cooperation considering only the focal workers. The estimates remain robust.

Cooperation among the workers falls not only on the extensive margin but also on the intensive margin. The bottom panel of Figure 1 shows that conditional on an interaction taking place, the average duration of such interactions is lower during the intervention than in the pre-intervention months. The left panel considers interactions by all the workers in an observation set, while the right panel considers only interactions by the focal workers. The results are similar in both the graphs, although slightly noisier in the second, as considering only the focal workers leads to a lower number of observations.

The before-after comparison is helpful to see that the cooperation among the knitting workers decreases after the ranking begins. But this could also be driven by factors unrelated to the intervention, such as changes in work pressure over time. Section A.3 in the Appendix presents evidence to suggest that the ranking competition triggers the decrease in cooperation. In particular, the evidence shows that (i) cooperation is lower among the treated workers than in Control during the intervention, (ii) cooperation between publicly-ranked workers suffers the most, and (iii) cooperation is largely driven by withdrawal of voluntary help. I discuss these in detail in the Appendix.[45]

Does the reduced cooperation drive the productivity response? To answer this, I cannot control for the actual changes in the cooperation in the original specifications as this becomes a "bad control". Instead, for each worker, I calculate the propensity to interact with coworkers during the pre-intervention months. It is simply the share of observations in which the worker interacts with a coworker at least once. If cooperation helps the workers to be more productive, a change in the overall cooperative environment induced by the intervention should affect more the workers with a higher propensity to interact at baseline than those with a lower propensity. Because this approach uses observations at baseline, the following analysis is limited to the 274 workers for whom we have at least one observation before the intervention.

Column 3 of Table 6 re-estimates the productivity changes for the limited sample of 274 workers. The key estimate remains similar to prior estimates with the full sample.

[44] Standard errors are clustered at the observation set level but also work at the individual level.

[45] Notice that any change in the workers' behavior because of simply observing them must be present both before and during the intervention. A comparison of the two time periods should be unaffected by this. Also, comparison of the experimental arms in a given time period controls for such behavioral responses in any given time period.

Next, Column 4 controls for a worker's propensity to interact at baseline and lets it affect privately- and publicly-ranked workers differently. If publicly-ranked workers witness a more significant deterioration in cooperation and thus a larger drop in productivity than others, this will be captured by the interaction term. Indeed, the positive coefficient for this interaction term reflects a larger negative productivity effect from reduced cooperation among publicly-ranked workers. But the estimate is extremely noisy and makes it difficult to draw concrete conclusions. Reassuringly, our estimate of the association between productivity and rank-distance with friends remains robust; if anything, it increases in magnitude. This suggests that while changes in cooperation may affect productivity in general, it does not drive the productivity decline correlated with rank-distance with friends.

The previous specification uses a simple propensity to engage in an act of cooperation at baseline but does not distinguish between the workers who receive help and those who provide help. Clearly, the workers who receive help are more likely to lose from reduced cooperative environment on the production floor.

Therefore, Column 5 repeats a similar exercise as Column 4 but instead of calculating the propensity to simply engage in an act of cooperation, it now uses propensity to receive help at baseline. Indeed, the workers who are more likely to seek help at baseline appear to be generally low productive after the ranking begins. This can be seen in the coefficient for the propensity variable. Note however, this coefficient may capture both a selection effect (less productive workers may seek more help) and the productivity effects of lower cooperation. But, importantly, similar workers in Public Treatment are not likely to be any less productive. Also, the key estimate remains robust.

The reduced collaboration between the workers appears at odds with their desire to prove their social compatibility. But notice that the decrease in cooperation documented earlier is a decline in the average likelihood of collaboration with all coworkers. It is still plausible that publicly-ranked workers reduce overall collaboration with coworkers but provide more help particularly to their friends; that is, some redistribution of help occurs. Unfortunately, the large decrease in cooperation during the intervention constrains a rigorous analysis to test such redistribution.

In summary, the ranking contest reduces cooperation among the workers but this does not explain the productivity decrease earlier associated with signaling social compatibility. It is useful to note that the intensity of cooperation among the workers is only modest at baseline. The likelihood of observing a workers interacting with a coworker during a 20-minute window is one in five. Even when that happens, an interaction lasts

for around a minute on average. The low intensity of cooperation at baseline could explain why a fall in the cooperation does not translate into substantial productivity losses overall. However, the fall in cooperation could be more harmful in contexts where cooperation among workers is an integral component of production technology.

6.3 Other Alternative Explanations

Another interpretation of the main results is that the reduction in productivity is a manifestation of the ratchet effect rather than signaling social compatibility. By collectively slowing down the knitting, the knitting workers could convince the management that the sweaters need longer to knit. This may pave the way for them to ask for higher piece rates. The workers have a private incentive to deviate from the collective undercutting of effort, as doing so increases their individual earnings. Making ranks public may reveal the high-performers and create a pressure on them to reduce speed and comply with the collective cause. However, if this were true, we should observe a reduction in effort when they outrank any coworker, regardless of the workers' social ties with them. But Table 5 shows that the workers do not reduce productivity when they outrank coworkers they are not friends with. This is inconsistent with what we expect to find because of the ratchet effect. Nonetheless, Figure A4 in the Appendix probes further into this. It plots the average treatment effect of the public ranks, relative to that of private ranks, separately for workers who rank at or below the 50th percentile of ranks in the first feedback and those who rank higher. If the ratchet effect were driving the results, the productivity drop would have most likely existed only among the high-ranked workers who knit the sweaters faster than others (in general and not only within networks). But the figure shows that the magnitudes of the average treatment effect is similar for both high and low ranked workers. The standard errors are higher than before as the underlying sample has been split into halves. Still, the estimates rule out the possibility that the productivity drop among publicly-ranked workers reflects the ratchet effect.

Could the results be driven by an aversion to inequality instead? Recall that the workers do not increase productivity when they rank lower than their friends and are indifferent about outranking peers they are not friends with. If aversion to inequality were at play, it is unlikely to be asymmetric and network-specific. Moreover, we find that the workers do not respond when they learn that they overestimated the productivity of some of their friends at baseline (see Table 6). We would expect the workers to respond to this new information if a certain network-specific inequality aversion were at play.

7 Motivations for Signaling Social Compatibility

I now turn to explore the workers' underlying motivation for signaling social compatibility to their friends. There are two related but subtly different reasons why the workers may be concerned about publicly outranking friends. Both of them relate to fears about social punishment from social groups but are motivated by distinct mechanisms. First, in the context of ranks, a worker's high effort may suppress his friends' ranks and thus impose a negative externality on them. Second, an enthusiastic effort to outrank one's friends and proving them to be low productive, especially at the management's behest, may be interpreted as a desire to distinguish himself from them. In both cases, a worker may be perceived as selfish and socially incompatible, leading to him getting socially ostracized. In both cases, he may respond by reducing his effort to signal social compatibility to his friends. However, in the first, the signal is motivated by a desire to internalize the negative externality on friends while in the second, it is a desire for social conformity.

To distinguish between the two mechanisms, notice that a worker imposes a negative externality on his friends when he suppresses their ranks. This is particularly salient when the friends are ranked close to the worker so that marginal changes in the worker's effort can alter their ranks. Conversely, a worker would worry about being perceived as distancing himself from his friends when he outranks them by a big margin.

Figure 2 takes a cue from this observation. It plots the productivity responses of publicly-ranked workers as they outperform an additional friend. The friends are categorized as being outranked by 0-20, 20-40, 40-60, or 60+ rank-distances. The coefficient estimates are relative to privately-ranked workers.

Publicly-ranked workers reduce productivity only when they outrank friends by greater than 40 ranks. They do not decrease productivity when they outrank friends by narrower margins. If anything, they increase productivity with each additional friend ranked lower but within 20 rank-distances. The sample in this figure is limited to workers who rank higher than the median rank among their friends so the results directly speak to the previous results. But the results are very similar even if we consider the whole sample.

Table 7 conducts further tests. First, Column 1 reports estimates from a specification similar to Figure 2 but aggregates rank-distances into just two bins, 0-39 ranks and greater than 39 ranks. As before, outranking an additional friend by at least 40 ranks is associated with a decrease in productivity by about 2.5 percent. Outranking an

additional friend by a lesser margin has an insignificant effect on worker productivity.[46]

It is possible that the productivity drop is still motivated by concerns about externality and, therefore, an attempt to improve the friends' ranks. If that were true, we expect to observe improvement in the workers' friends' ranks relative to their own. Therefore, Column 2 examines whether the productivity drop does translate into better ranks for friends. The outcome variable is the total number of friends a worker outranks in a treated month compared to baseline. The value of this variable may change across months and reflects *within-worker changes* in the number of outranked friends across treated months. Clearly, the productivity drop associated with outranking friends by more than 40 ranks does not translate into better relative ranks for the friends.[47]

The productivity drop does, however, translate into lower ranks for the workers and, therefore, lower average rank-distance with friends. Column 3 shows that for the same workers who reduce productivity in Column 1, there is a drop in ranks by more than four percentiles.[48] The estimates are statistically robust to multiple hypothesis testing, as evident from the sharpened Q-values at the end of the columns.

Column 4 confirms that the lower ranks lead to lower average rank-distances with friends. The outcome variable now captures monthly variations in a worker's average rank-distance with competing friends and is measured in units of 10 ranks. Although slightly noisier in statistical precision (p-value=0.15), the coefficient indicates a reduction in average rank-distance with friends by about four ranks.

In summary, publicly-ranked workers reduce productivity only when they outperform their friends by substantial margins. [49] Subsequent evidence shows that the productivity drop does not translate into improved ranks for their friends but merely reduces their rank-distance with them. The evidence is consistent with the productivity drop being driven by a desire to conform to their social groups' productivity levels rather than internalizing the externality of effort on friends.

[46]Again, the sample includes only the workers who rank higher than the median rank among their friends.

[47]Sharpened Q-values at the end of the columns verify the robustness of the standard errors to multiple hypothesis testing (see Anderson (2008)).

[48]For workers with the same rank-percentile in two experimental arms, the absolute ranks can vary merely because of the differences in the total number of workers in the arms (see Table 1). Therefore, I use rank-percentiles instead.

[49]The results also rule out that workers reduce their effort out of altruism towards their friends. An altruism-driven reduction in effort is more likely to happen when this improves the friends' ranks, that is, when outranked friends are ranked close to the workers. These results indicate the opposite.

8 Conclusion

Using data from a sweater factory this paper shows that workers exhibit social conformism in productivity in their workplace. An experimental design with private and public performance ranking, along with detailed data on the workers' social network, help to show that such conformity can strongly counteract positive effects from performance ranking.

The evidence in this paper suggests that inducing a public competition among workers may be counterproductive for firms if there exists strong social ties among their workers. In a different setting with a different form of competition, Bandiera et al. (2005) show that workers internalize the negative externality of their effort on their coworkers and reduce effort. Blader et al. (2020) show that workers reject public performance ranking because they think it is not consistent with a collective approach to work. This paper, on the other hand, presents evidence that, under public competition, workers may withdraw effort in order to conform to their friends' productivity. Additional evidence exploiting novel data suggests that such competition may also reduce cooperation among the workers. This could further dampen productivity of the firms if cooperation among workers increase overall productivity of the firms. The findings have important implications for designing competition-based incentives for workers.

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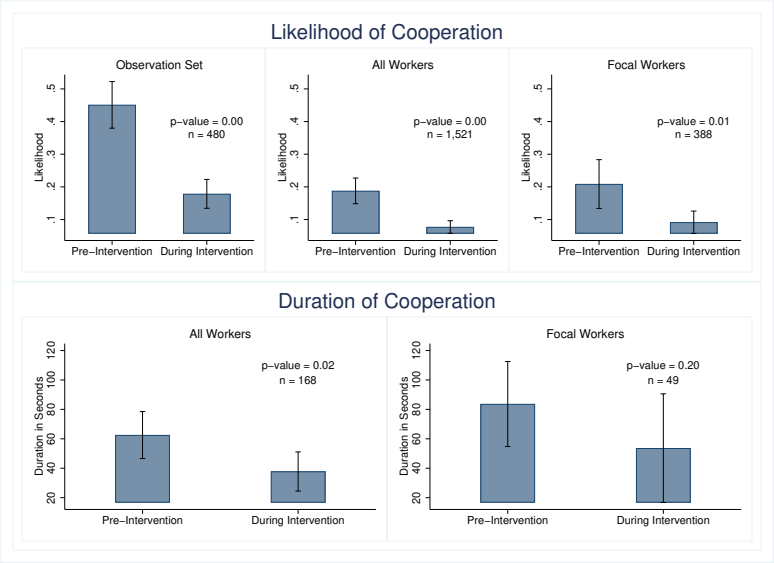
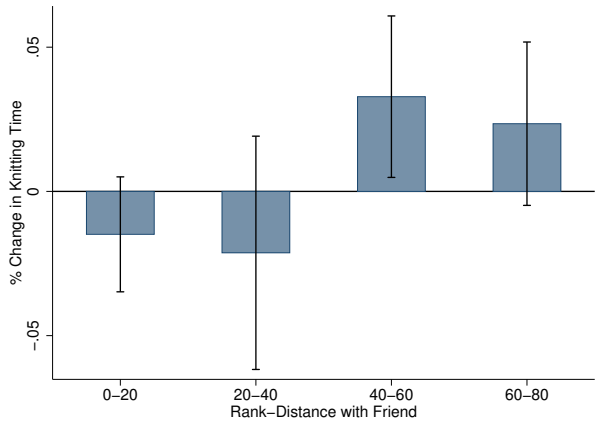
Figure 1: Cooperation Among Workers



Note: The top panel reports the probabilities that a worker interacts with his coworkers during a 20-min observation slot. Each observation in the underlying regression is an observation set containing several workers (left panel), individual workers observed during the observation set (middle panel), or only the focal workers associated with the set (right panel). See Section 6.2 for details. The bottom panel estimates the mean duration of interaction involving a cooperation. Vertical lines over the bars depict 95% confidence intervals around the estimates. The p-value reported in each panel is for the test of equality of the two corresponding bars. The number of observations (n) refers to the total number of observations underlying each panel and thus the number of observations used for the test.

Figure 2: Productivity Effect of an Additional Outranked Friend



Note: The figure shows the difference in average knitting time of sweaters for publicly-ranked workers, relative to privately-ranked workers, in response to outranking an additional friend. Horizontal axis shows the rank-distances by which the friend is outranked, categorized into bins. Vertical axis reports the estimated differences in the knitting times (in percentage) between the two treatments. Vertical lines over the bars depict 90% confidence intervals around the estimates.

Table 1: Descriptive Statistics

	(1) n	(2) Control Mean	(3) Private Mean	(4) Public Mean	(5) (2)-(3)	(6) (2)-(4)	(7) (3)-(4)
Production							
Knitting Time Per Sweater (Jan'16) (mins)	1688	84.88	85.19	82.76	[0.89]	[0.34]	[0.29]
Monthly Production Earnings (Jan'15-Jan'16) (BDT)	4758	11042.93	11038.57	10996.9	[0.97]	[0.66]	[0.7]
Number of Sweaters Produced in a Month (Jan'15-Jan'16)	4755	387.51	394.43	358.88	[0.76]	[0.19]	[0.12]
Monthly Attendance Days (Jan'15-Jan'16) (BDT)	4758	28.38	28.34	28.35	[0.65]	[0.72]	[0.91]
Demographic Characteristics							
Years Attended School	362	7.84	7.56	7.38	[0.39]	[0.17]	[0.59]
Age at Baseline (years)	352	30.61	30.44	30.9	[0.77]	[0.61]	[0.43]
Tenure at Baseline (years)	366	5.32	5.47	5.27	[0.68]	[0.88]	[0.59]
1(Competitive)	363	0.45	0.51	0.41	[0.35]	[0.56]	[0.13]
Risk Scale (1 to 10; 10 highest)	363	5.54	5.5	5.72	[0.91]	[0.51]	[0.46]
Social Network							
Number of Coworkers in Block	363	23.55	23.61	23.63	[0.86]	[0.8]	[0.94]
Number of Friends Reported at Baseline	363	11.46	10.53	12.32	[0.28]	[0.34]	[0.03]
Productivity of Friends in Same Treatment vs. Outside	315	1	1.01	0.99	[0.64]	[0.57]	[0.34]

Note: The table reports means of various baseline characteristics for workers in the three experimental arms. Columns 5-6 test equivalence of the means between control and the treatment arms. Column 7 does the same for the two treatment arms. P-values from the tests are reported in square brackets. *1(Competitive)* is an indicator variable that takes the value 1 if a worker chooses a competitive pay during the lab-in-field game played with them. See Section 3.3. *Productivity of Friends in Same Treatment vs. Outside* is the ratio of average productivity of friends who are randomized into the same experimental arm as a worker and average productivity of all the other friends. By construction, this excludes all the workers who do not have any friends in the same arm. The productivity is computed using their average monthly production earnings over July-September 2015, three months preceding the baseline survey. Three workers could not be surveyed during the baseline survey, which explains the drop in the total number of workers to 363 for some of the survey measures.

Table 2: Average Treatment Effect on Worker Productivity

	(1) Ln(Time)	(2) Ln(Time)	(3) Ln(Time)	(4) Ln(Time)
Public	0.0182*	0.0181*	0.0179*	0.0186*
	(0.0109)	(0.0107)	(0.0103)	(0.0107)
Private	0.0061	0.0055	0.0054	0.0059
	(0.0124)	(0.0124)	(0.0123)	(0.0136)
Job Sequence			-0.0228***	-0.0225***
			(0.0027)	(0.0027)
Observations	22,870	22,870	22,870	22,787
Baseline Productivity	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes	Yes
Month FE	No	Yes	Yes	Yes
Worker Characteristics	No	No	No	Yes

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to knit a sweater in an assigned job. *Public* and *Private* refer to Public and Private Treatments respectively. *Job Sequence* is the cumulative number of jobs in which the worker produced sweaters of a given style and size. *Baseline Productivity* is the average of monthly production earnings in the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. *Worker Characteristics* include tenure at the factory at baseline, number of years attended school, and measures of attitudes towards risk and competition. All regressions include a constant. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 3: Disentangling Social Incentives Underlying Ranks

	(1) Ln(Time)	(2) Ln(Time)	(3) Ln(Time)	(4) Ln(Time)	(5) Ln(Time)
Public	0.0271**	0.0272**	0.0270**	0.0184*	0.0270*
	(0.0114)	(0.0116)	(0.0118)	(0.0095)	(0.0156)
Public * 1(Do Not Compete With Friends)	-0.0753***	-0.0755**	-0.0730**	-0.0391	-0.0385
	(0.0292)	(0.0295)	(0.0290)	(0.0246)	(0.0253)
Private			0.0101		
			(0.0129)		
Private * 1(Do Not Compete With Friends)			-0.0342*		
			(0.0204)		
Public * (# Incorrect Predictions at Baseline)					-0.0028
					(0.0039)
# Incorrect Predictions at Baseline					0.0015
					(0.0017)
Observations	15,573	15,573	22,870	14,950	14,950
Public+1(Do Not Compete With Friends)=0	[0.068]	[0.069]	[0.073]	[0.279]	[0.279]
Public=Private			[0.079]		
Baseline Productivity	Yes	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Number of Friends at Baseline	No	Yes	Yes	Yes	Yes
Counterfactual	Control	Control	Control	Private	Private

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to knit a sweater in an assigned job. *Public* and *Private* refer to Public and Private Treatments respectively. *1(Do Not Compete with Friends)* is a dummy variable that is equal to 1 if a worker does not have any friend randomized in the same experimental arm. *Number of Friends at Baseline* is the number of coworkers a worker reports as a friend during baseline survey. *# Incorrect Predictions at Baseline* is the number of predictions a worker makes during baseline survey about a co-worker's productivity, relative to his own, that are later proved incorrect from the ranking in the first feedback. *Baseline Productivity* is the average of monthly production earnings in the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. All regressions include a constant, and *1(Do Not Compete with Friends)* in an un-interacted form. Column 5 also includes *# Incorrect Predictions at Baseline* in an un-interacted form. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively. Numbers in square brackets report p-values from tests of corresponding null hypothesis.

Table 4: Signaling Social Compatibility

	(1) Ln(Time)	(2) Ln(Time)	(3) Ln(Time)	(4) Ln(Time)	(5) Ln(Time)	(6) Ln(Time)
Public	-0.0001	-0.0031	0.0138	0.0065	-0.0008	0.0071
	(0.0109)	(0.0131)	(0.0091)	(0.0107)	(0.0103)	(0.0123)
Public * Distance to Median Rank Among Friends from Above	0.0099**	0.0103**		0.0128***	0.0093**	0.0085*
	(0.0041)	(0.0041)		(0.0038)	(0.0046)	(0.0048)
Public * Distance to Median Rank Among Friends from Below		0.0005				
		(0.0040)				
Public * Distance to Median Rank Among Interacting Non-Friends from Above			0.0001	-0.0063		
			(0.0030)	(0.0043)		
Public * Dist. to Med. Rank Among Interacting Non-Friends from Ab.					0.0013	
					(0.0048)	
Private						0.0072
						(0.0141)
Private * Distance to Median Rank Among Friends from Above						-0.0011
						(0.0047)
Observations	14,950	14,950	14,950	14,950	14,950	22,870
From Above = From Below		[0.045]				
Baseline Productivity	Yes	Yes	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Friends at Baseline	Yes	Yes	Yes	Yes	Yes	Yes
Counterfactual	Private	Private	Private	Private	Private	Control

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to knit a sweater in an assigned job. *Public* and *Private* refer to Public and Private Treatments respectively. *Dist. to Med. Rank Among Friends (res. Non-Friends) from Above* is the distance to the median rank among friends (res. non-friends) if the worker ranks higher than the median. *Dist. to Med. Rank Among Friends from Below* is the distance to the median rank among friends if the worker ranks lower than the median. *Dist. to Med. Rank Among Interacting Non-Friends from Ab.* is the distance to the median rank among coworkers a worker is not friends with but interacts with frequently during work, provided the worker ranks higher than the median. *Baseline Productivity* is the average of monthly production earnings in 2015, the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. *Number of Friends at Baseline* is the number of coworkers a worker reports as a friend during baseline survey. All regressions include a constant and un-interacted variables measuring distances to median ranks. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 5: Robustness Tests

	(1) Ln(Time) Pre-Interv.	(2) Ln(Time) $\geq$ 1 Friend	(3) Ln(Time)	(4) Ln(Time)	(5) Ln(Time)	(6) Ln(Time)
Public	-0.0112	0.0022	0.0034	0.0195	0.0221	0.0422
	(0.0185)	(0.0137)	(0.0200)	(0.0672)	(0.0699)	(0.0935)
Public * Distance to Median Rank Among Friends from Above	-0.0065	0.0102**	0.0100**	0.0106***	0.0106***	0.0072*
	(0.0060)	(0.0045)	(0.0040)	(0.0039)	(0.0039)	(0.0041)
Public * Number of Friends at Baseline			-0.0003		-0.0003	-0.0010
			(0.0012)		(0.0012)	(0.0015)
Public * Baseline Productivity				-0.0000	-0.0000	0.0000
				(0.0000)	(0.0000)	(0.0000)
Observations	1,046	13,248	14,950	14,950	14,950	14,867
Baseline Productivity	Yes	Yes	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	No	Yes	Yes	Yes	Yes	Yes
Number of Friends at Baseline	Yes	Yes	Yes	Yes	Yes	Yes
Worker Characteristics	No	No	No	No	No	Yes
Public*Worker Characteristics	No	No	No	No	No	Yes
Counterfactual	Private	Private	Private	Private	Private	Private

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to knit a sweater in an assigned job. Column 1 considers data from only January 2016, a month before the intervention starts. Column 3 restricts sample to workers who have at least one friend randomized into the same experimental arm. *Public* and *Private* refer to Public and Private Treatments respectively. *Dist. to Med. Rank Among Friends from Above* is the distance to the median rank among friends if the worker ranks higher than the median. *Baseline Productivity* is the average of monthly production earnings in the year prior to the intervention. *Number of Friends at Baseline* is the number of coworkers a worker reports as friends at baseline. *Worker Characteristics* include tenure at the factory at baseline, number of years attended school, and attitudes towards risk and competition. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. All regressions include a constant and distance to median rank in an un-interacted form. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 6: Alternative Explanations - Complacency, Cooperation

	(1) Ln(Time)	(2) Ln(Time)	(3) Ln(Time)	(4) Ln(Time)	(5) Ln(Time)
Public	0.0118	-0.0034	-0.0048	-0.0070	-0.0045
	(0.0133)	(0.0104)	(0.0119)	(0.0144)	(0.0125)
Public * Distance to Median Rank Among Friends from Above	0.0110**	0.0094**	0.0108*	0.0115**	0.0133**
	(0.0048)	(0.0042)	(0.0057)	(0.0057)	(0.0055)
Public * 1(Underestimated Own Rank)	-0.0502**				
	(0.0208)				
Public * Number of Friends Incorrectly Predicted as More Productive		0.0088			
		(0.0105)			
Public * Propensity to Interact at Baseline				0.0134	
				(0.0512)	
Propensity to Interact at Baseline				0.0211	
				(0.0355)	
Public * Propensity to Receive Help at Baseline					-0.0119
					(0.0560)
Propensity to Receive Help at Baseline					0.0810*
					(0.0454)
Observations	14,714	14,950	11,140	11,140	11,140
Baseline Productivity	Yes	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Number of Friends at Baseline	Yes	Yes	Yes	Yes	Yes
Counterfactual	Private	Private	Private	Private	Private

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to knit a sweater in an assigned job. *Public* and *Private* refer to Public and Private Treatments respectively. *Distance to Median Rank Among Friends from Above* is the distance to the median rank among friends if the worker ranks higher than the median. *1(Underestimated Own Rank)* is a dummy variable that takes value 1 if a worker receives a rank in the first feedback that is better than his prior at baseline. *Number of Friends Incorrectly Predicted as More Productive* is the number of friends a worker incorrectly predicted to be more productive than him at baseline. *Propensity to Interact at Baseline* is the average likelihood that a worker engages in an act of cooperation with his peers during a random observation slot before the intervention. *Baseline Productivity* is the average of monthly production earnings in the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. *Number of Friends at Baseline* is the number of coworkers a worker reports as friends at baseline. All regressions include a constant and *Distance to Median Rank Among Friends from Above* in an un-interacted form. Col. 1 and 2 include *1(Underestimated Own Rank)* and *Number of Friends Incorrectly Predicted as More Productive* in un-interacted forms, respectively. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 7: Tests of Mechanism

	(1) Ln(Time)	(2) Change in Friends Outranked	(3) Rank-Percentile	(4) Rank-Distance w/ All Friends
Public	0.0010	0.1425	0.0228	0.5625
	(0.0213)	(0.2456)	(0.0342)	(1.0123)
Public * Number of Outranked Friends at Rank-Dist. $\leq$40	-0.0161	0.1750*	0.0147	0.1352
	(0.0104)	(0.0951)	(0.0185)	(0.2387)
Public * Number of Outranked Friends at Rank-Dist. >40	0.0296***	-0.1121	-0.0461***	-0.4277
	(0.0099)	(0.1304)	(0.0161)	(0.2979)
Observations	7,007	492	492	492
Adjusted R-squared	0.2649	0.1927	0.3456	0.2667
Sharpened Q-values: Public * Number of Outranked Friends at Rank-Dist. >40	[0.009]	[0.243]	[0.009]	[0.112]
Baseline Productivity	Yes	Yes	Yes	Yes
Style-Size FE	Yes	No	No	No
Month FE	Yes	Yes	Yes	Yes
Number of Friends at Baseline	Yes	Yes	Yes	Yes
Counterfactual	Private	Private	Private	Private

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to knit a sweater in an assigned job. *Public* and *Private* refer to Public and Private Treatments respectively. *Baseline Productivity* is average monthly production earnings in 2015, the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. *Number of Friends at Baseline* is the number of peers a worker reports as friends at baseline. All regressions include a constant and un-interacted variables corresponding to the interactions. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively. P-values are corrected for multiple hypotheses testing using sharpened Q-values following Anderson (2008).

A Appendix

A.1 Computation of Ranks

I compute ranks using the average knitting time of sweaters in the previous month. Knitting times provide cleaner estimates of productivity than, for instance, production earnings which are essentially a product of productivity and piece rates. However, because different workers may work on different styles of sweaters and knitting them may take different lengths of time, we cannot compare the times across workers without accounting for styles.[50] Therefore, I compute ranks in four steps. First, for each style and size a worker works on in a month, I assign him a *percentile-rank* by comparing his average knitting time of the sweaters with that of others in his treatment arm who work on the same style and size. Next, the percentile ranks of each of the styles and sizes are weighted by their share of a worker's total production in the month. In the third step, the weighted percentile ranks for all styles and sizes are added together to derive a single weighted average percentile rank for each worker. The weighted average percentile ranks are used to produce the final ranks.[51]

A.2 Alternative Measurement

The specifications in the paper use median ranks among friends as a benchmark to compare the relative ranks of the workers. Although median rank may appear to be an arbitrary choice, it is the traditional benchmark used in the literature to study conformism. Nonetheless, Column 1 of Table A2 uses an alternative, more general, measure - *average* rank-distance with friends at baseline. To test asymmetric response, Column 2 separately estimates the workers' response to rank-distance with friends they outrank and those they do not. The results are qualitatively similar to earlier results.

A.3 Cooperation Changes Because of Intervention

The before-and-after comparison of cooperation in Section 6.2 addresses the issue that cooperation can span multiple arms and, therefore, changes in one arm may affect another. However, one concern is that collaboration on the production floor may have fallen

[50] A worker works on four different styles in a month on average, the composition of which may be different for other workers.

[51] If two or more workers have the same value for weighted average percentile-ranks they share the same final rank. Such instances, however, are rare.

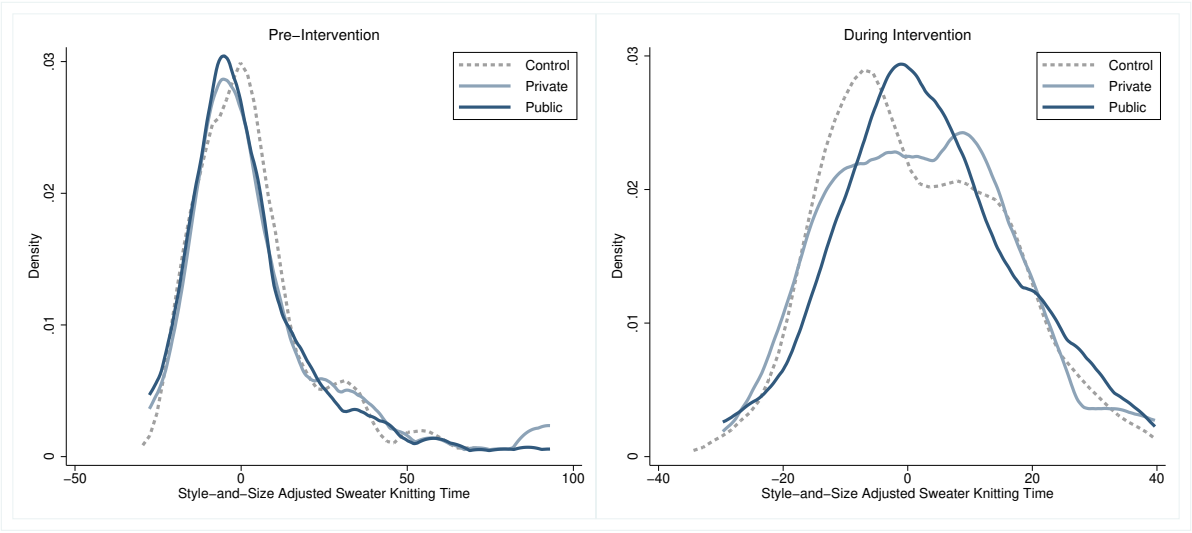
during the intervention because of changes with time (such as changes in workload) that are unrelated to the ranking contest.

Three pieces of evidence suggest that the change in cooperation is driven by the ranking contest. First, there is a lower likelihood of collaboration among the ranked workers during the intervention. This can be seen in Figure A2 which reports the likelihood of a worker engaging in an interaction, conditional on his treatment assignment after the ranking begins. Privately- and publicly-ranked workers are now relatively less likely to engage in collaboration than Control workers. Although the difference in the likelihoods for Control and Private Treatment is small and insignificant (p-value = 0.22), the difference between Control and Public Treatment, arguably the most salient competition format, is marginally significant (p-value = 0.10). The across-arm comparisons, which hold time constant, are not confounded by temporal changes in factors that may correlate with workers' cooperation. We should interpret these estimates with caution for reasons discussed earlier, but the exercise provides valuable insights, nonetheless.

Second, during the intervention, the share of interactions in which publicly-ranked workers interact with fellow workers in Public Treatment is lower relative to pre-intervention months. To illustrate this, consider the unique interactions among the workers before and after the ranking begins. The left panel in Figure A3 reports the shares of such interactions involving pairs of workers with different combinations of treatment statuses. Compared to the pre-intervention months, the shares of interactions that involve two ranked workers are either lower or unchanged. Importantly, the share of interactions where both workers are publicly-ranked declines the most.

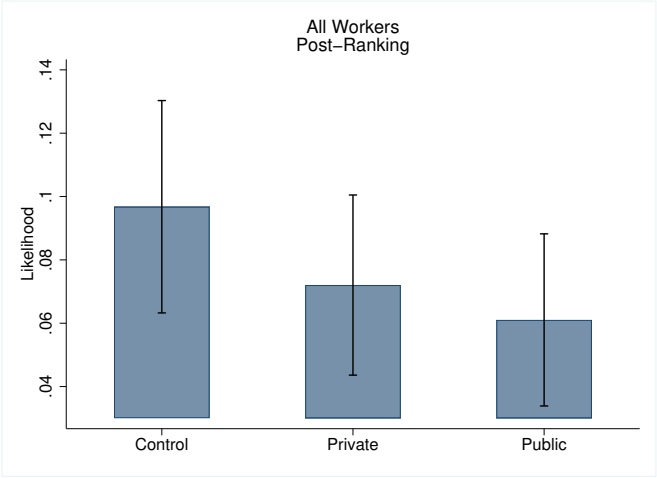
Finally, the right panel reports the cooperation shares by the types of help involved. A worker usually helps another in three ways - with the measurement of sweater parts or helping to set up their machines, lending tools (for example, design chart, pencil), and sharing tips on the production process (for example, suggestions on how to fix errors). During the intervention, there is an increase in the share of help that is possibly harder to refuse (help with measurement or lending), but there is a sharp decline in sharing of knowledge, a help that is relatively more voluntary in nature. In other words, the workers appear to be intrinsically less motivated to help their coworkers after the ranking begins.

Figure A1: Distribution of Average Knitting Time Per Sweater



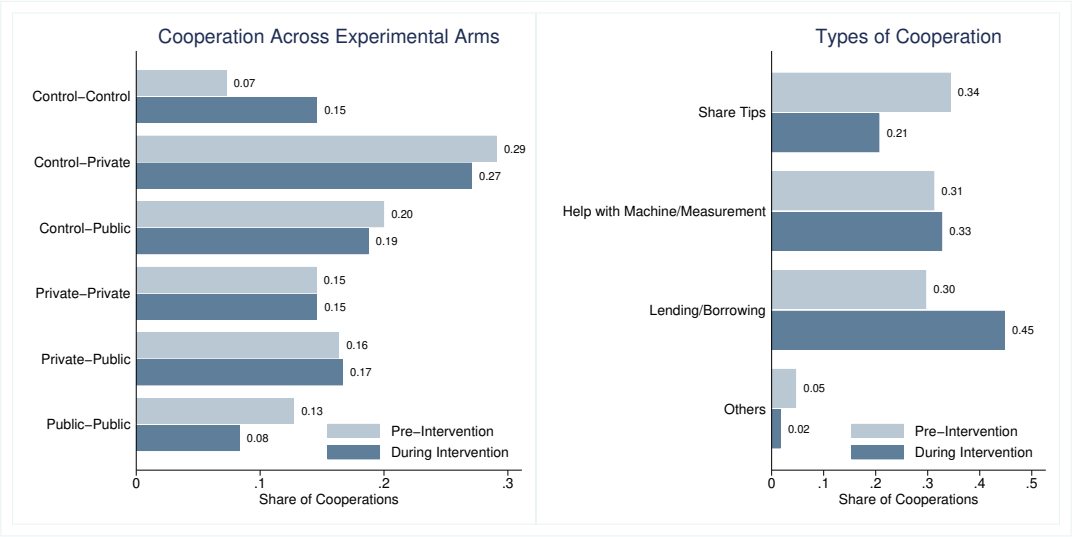
Note: The figure plots residuals from a regression of average knitting time of sweaters on their style-and-size fixed effects. The left panel plots the residuals for the pre-intervention month of January 2016. The right panel does the same for intervention months of February-June 2016.

Figure A2: Cooperation Among Workers in Different Experimental Arms



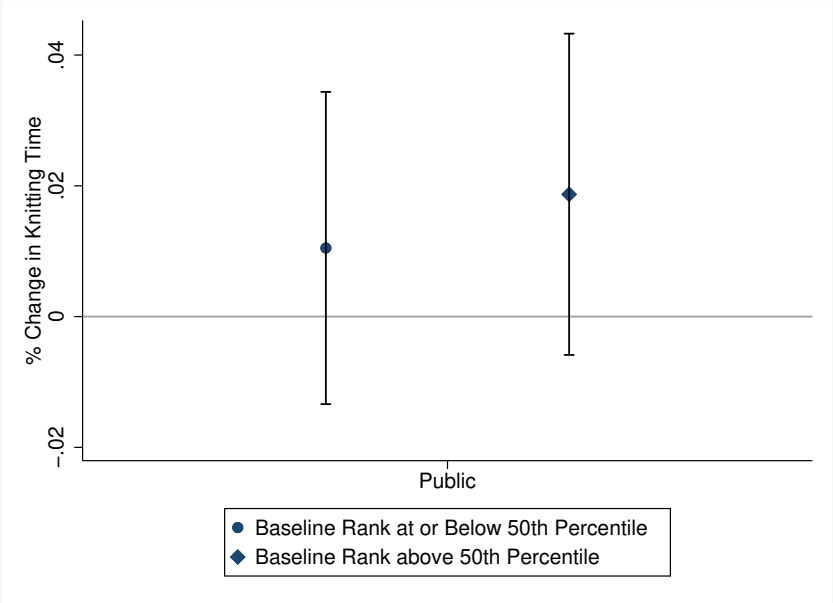
Note: The figure reports the probabilities that a worker interacts with his coworkers after the ranking begins. Vertical axis reports the marginal likelihood that a worker interacts with coworkers during a given observation slot at least once. It is estimated from a linear probability model. Vertical lines depict 95% confidence intervals around the estimates.

Figure A3: Composition of Cooperation Among Workers



Note: The figure breaks down the total number of independent events of cooperation between two workers by the combination of their treatment assignments (left panel) and the types of help involved (right panel). The horizontal axis reflects the shares of such events in a given time period pertaining to corresponding categories.

Figure A4: Treatment Effect of Public Ranks Relative to Private Ranks



Note: The figure reports average treatment effects of public ranks relative to private ranks. The left plot represents the treatment effect on workers who rank low, at or below 50th percentile in the first feedback. The right plot represents the treatment effect on workers who rank higher than 50th percentile. Vertical lines depict 90% confidence intervals around the estimates.

Table A1: Contamination Effects

	(1) Ln(Time)	(2) Ln(Time) Prvt. Inten.	(3) Ln(Time) Pub. Inten.
Private	0.0022	0.0031	0.0253
	(0.0186)	(0.0208)	(0.0260)
Private * 1(Public Intensive Block)	0.0136		
	(0.0252)		
1(Public Intensive Block)	0.0104		
	(0.0159)		
Private * Distance to Median Rank Among Friends from Above		-0.0019	-0.0022
		(0.0070)	(0.0088)
Observations	15,217	8,239	6,978
Baseline Productivity	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Number of Friends at Baseline	Yes	Yes	Yes
Counterfactual	Control	Control	Control

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to knit a sweater in an assigned job. *Private* refer to Private Treatments. *Prvt. Inten.* refer to Private-Intensive blocks which contain relatively more privately-ranked workers than publicly-ranked workers. *Pub. Inten.* or *Public-Intensive Block* refer to the blocks with the opposite. *Distance to Median Rank Among Friends from Above* is the distance to the median rank among friends if a worker ranks higher than the median. *Baseline Productivity* is average monthly production earnings in 2015, the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. *Number of Friends at Baseline* is the number of coworkers a worker reports as friends at baseline. All regressions include a constant and Cols. 2-3 include a control for *Distance to Median Rank Among Friends from Above* in an un-interacted form. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A2: Alternative Measurements

	(1) Ln(Time)	(2) Ln(Time)
Public	0.0130	-0.0110
	(0.0085)	(0.0198)
Public * Mean Rank-Distance with Friends	0.0059**	
	(0.0025)	
Public * Mean Rank-Distance with Outranked Friends		0.0111**
		(0.0045)
Public * Mean Rank-Distance with Outranking Friends		-0.0010
		(0.0036)
Observations	14,950	14,950
Low = High		[0.006]
Baseline Productivity	Yes	Yes
Style-Size FE	Yes	Yes
Month FE	Yes	Yes
Number of Friends at Baseline	Yes	Yes
Counterfactual	Private	Private

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to knit a sweater in an assigned job. *Public* and *Private* refer to Public and Private Rank treatments respectively. *Mean Rank-Distance with Friends* is average rank-distance with friends based on ranks at baseline. *Outranked Friends* refer to friends who are ranked lower than a worker at baseline; *Outranking Friends* are friends ranked higher. *Baseline Productivity* is the average of monthly production earnings in the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. *Number of Friends at Baseline* is the number of coworkers a worker reports as friends at baseline. All regressions include a constant and un-interacted variables corresponding to the interactions. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A3: Descriptive Statistics for Observations Data

	(1) n	(2) Control	(3) Private	(4) Public	(5) (2)-(3)	(6) (2)-(4)	(7) (3)-(4)
At Baseline							
Total Observation Sets	133						
Total Observations at Worker Level	458	149	152	157			
Total Observations of Focal Workers	115	40	35	40			
Number of Unique Workers	274	93	92	89			
During Intervention							
Total Observation Sets	347						
Total Observations at Worker Level	1063	372	347	344			
Total Observations of Focal Workers	273	102	85	86			
Number of Unique Workers	336	114	108	114			
Worker Characteristics of Observed Workers at Baseline	**n**	**Mean**	**Mean**	**Mean**			
Production Time Per Sweater (Jan'16) (mins)	1,262	87.96	85.47	83.11	[0.34]	[0.07]	[0.38]
Monthly Production Earnings (Jan'15-Jan'16) (BDT)	3,562	10,907.77	11,091.15	10,938.24	[0.13]	[0.80]	[0.22]
Number of Sweaters Produced in a Month (Jan'15-Jan'16)	3,559	382.83	398.78	378.38	[0.55]	[0.86]	[0.45]
Monthly Attendance Days (Jan'15-Jan'16) (BDT)	3,562	28.34	28.32	28.33	[0.89]	[0.95]	[0.94]
Years Attended School	271	7.77	7.35	7.39	[0.26]	[0.33]	[0.91]
Age at Baseline (years)	264	30.76	30.22	31.03	[0.43]	[0.71]	[0.24]
Tenure at Baseline (years)	274	5.00	5.34	5.37	[0.43]	[0.38]	[0.96]
1(Competitive)	272	0.49	0.51	0.45	[0.82]	[0.54]	[0.41]
Risk Scale (1 to 10; 10 highest)	272	5.43	5.48	5.73	[0.89]	[0.33]	[0.47]
Number of Coworkers in Block	272	23.44	23.60	23.71	[0.63]	[0.44]	[0.75]
Number of Friends Reported at Baseline	272	11.92	10.28	12.37	[0.10]	[0.68]	[0.03]
Productivity of Friends in Same Treatment vs. Outside	239	1.00	1.01	1.00	[0.51]	[0.89]	[0.61]

Note: The table reports key statistics related to the data on cooperation. The top and middle panels report the number of observations and observed workers before and after ranking begins. The bottom panel reports the means of various baseline characteristics for the workers who were observed before ranking begins. Columns 5-6 test the differences in mean characteristics between control and the treatment arms against nulls of zero. Column 7 does the same for the differences in mean characteristics between the two treatment arms. P-values from the tests are reported in square brackets. *1(Competitive)* is an indicator variable that takes the value 1 if a worker chooses a competitive pay during the lab-in-field game played with them. See Section 3.3. *Productivity of Friends in Same Treatment vs. Outside* is the ratio of average productivity of friends who are randomized into the same experimental arm as a worker and average productivity of all the other friends. By construction, this excludes all the workers who do not have any friends in the same arm. The productivity is computed using their average monthly production earnings over July-September 2015, three months preceding the baseline survey.