Social Conformity at Work: Evidence from a Field Experiment

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Abstract

When a firm introduces relative performance feedback, workers may increase effort to signal high productivity to their peers and the firm. But social pressure to conform on productivity can decrease worker effort instead. I conduct a long-term field experiment at a sweater factory to disentangle the incentives underlying relative performance feedback using performance ranks. Treated workers receive ranks either privately or publicly. Private ranks do not affect productivity on average. But public ranks have a net negative effect. I show that this effect is driven by desire to signal social compatibility to the workers' social group. They reduce productivity if they outrank friends at baseline, but only when the ranks are visible to coworkers. Additional evidence is consistent with workers having a strong preference for social conformity. Novel data on cooperation show that the ranking contest also decreases cooperation among workers in general and has a further dampening effect on productivity.

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1 Introduction

Relative incentives such as performance ranks can be potentially useful for organizations to increase worker productivity. Although seminal models such as Lazear and Rosen (1981) and Green and Stokey (1983) emphasize monetary rewards with such incentives, workers can be incentivized by ranks even in the absence of such rewards because of pure signaling motivations (Besley and Ghatak (2008)). Empirical evidence from ranks, however, is mixed, possibly due to contrasting mechanisms arising from workers' relative and social preferences.[1] On one hand, workers may increase effort to signal high productivity to their coworkers and managers, who may value high productive workers (e.g. Holmstrom (1999); Besley and Ghatak (2008); Ashraf et al. (2014b)). On the other, workers may worry about being perceived as self-serving and receiving social sanctions if they try to outperform their peers, especially the social groups they identify themselves with. They may, therefore, reduce effort to conform to group productivity and signal social compatibility (Bernheim (1994); Akerlof and Kranton (2000)). It is also possible that the ranking contest decreases cooperation among workers, which may reduce total production.

This paper designs and implements a field experiment in a large garment factory in Bangladesh to disentangle the different mechanisms through which performance ranks may affect worker productivity. Disentangling the mechanisms poses several empirical challenges. First, we need a clear measure of individual performance. Second, the context of study should be conducive to social incentives. Third, we need to isolate the effect of social signaling incentives from that of private utility from ranks. Fourth, we need to distinguish between workers' responses to the audience who values productivity and the audience who values social compatibility. And finally, we must have data on cooperation among workers to understand how that changes.

The experiment provides monthly performance ranks to 366 workers at the garment factory for five months. The workers knit batches of 12-48 sweaters (a *job*) through an individual production process and are paid based on piece rates. I address the first empirical challenge by using job-level sweater knitting times as a high-frequency measure of individual productivity. Having worked for four years on average at the factory the knitting workers report strong social ties with peers at baseline. The setting, therefore, lends a real-world social network to the experiment and solves

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

the second challenge. The paper addresses the third and fourth challenges through experimental design. Treated workers in the experiment receive ranks in one of two ways. In the first, *Private Treatment*, workers receive information only about their own ranks. In the second, *Public Treatment*, workers receive information about their own ranks and that of others in the Public Treatment.[2] The first treatment controls for private utility from ranks, while the second adds signaling motivations by enhancing visibility of the ranks. Comparing the two isolates the effect of signaling motivations alone. A third experimental arm, a Control arm, does not rank workers and helps to estimate the net effect of each treatment. A social-network map from baseline identifies treated workers who do (res. do not) compete with friends, and therefore has (res. do not have) an incentive to signal social compatibility to their group. Finally, first-hand data on cooperation among workers collected from the production floor helps to understand how it changes because of the ranking contest.

The main findings are as follows. First, the intervention does not change average productivity of privately-ranked workers but reduces productivity of publicly-ranked workers by 1.5 percent on average. In other words, intrinsic incentives alone do not affect average productivity but signaling motivations underlying public ranks do. The direction of the effect suggests that the desire to signal high productivity is outweighed by some negative signaling incentives.

Second, publicly-ranked workers reduce productivity to signal social compatibility. Compared to privately-ranked workers, publicly-ranked workers reduce productivity by 1.7 percent when their friends are also randomly assigned into Public Treatment, and therefore they have to compete against friends. Workers who do not compete with any of their friends (weakly) increase productivity instead. Additional tests suggest that the workers reduce productivity to appear less self-serving to their friends. First, the productivity drop is entirely driven by workers who rank higher than the median among their friends in the beginning of the first month of the intervention (based on productivity at baseline). Moreover, the magnitude of this drop increases as a worker is ranked farther and farther above the median rank. In contrast, workers ranked below the median do not alter their effort choice. These estimates are derived by comparing privately- and publicly-ranked workers with similar relative ranks among friends; hence, they exploit only the experimental variation in public visibility of ranks while holding constant underlying worker characteristics correlated with the relative

[2]Workers in a given treatment are ranked among co-workers who are also in the same treatment.

ranks. A second test compares privately-ranked workers with Control arm and verifies that the productivity response is absent when the ranks are private information.[3] This establishes that the reduction in effort is motivated by signaling incentives. Finally, a third test shows that publicly-ranked workers do not reduce effort when they outrank peers they are not friends with. This suggests that the reduction in effort is a signal to their social groups. A series of robustness tests verify that the estimated productivity drop is not driven by endogenous worker characteristics such as workers' social popularity and baseline productivity in general.

The third finding characterizes the nature of the social-compatibility signal. There could be two related but subtly different reasons behind the productivity drop. First, as workers' effort in a ranking contest imposes a negative externality on their friends by pushing their ranks down, the workers may internalize this externality to escape social sanctions from friends.[4] Second, workers may dislike to be seen as eager to distance themselves from the rest of the group in productivity; they may fear that they would lose group membership. They may therefore reduce effort to conform to the productivity of their group. To distinguish between the two mechanisms, notice that externality of a worker's effort on his friends is the most salient when he and his friends are ranked close to each other; marginal changes in his effort can change his friends' ranks. Conversely, a worker's group identity is at higher risk when he outranks his group by a substantial distance.

I exploit variation in the distribution of friends' ranks around a worker's own to distinguish between the two mechanisms. The evidence suggests that publicly-ranked workers reduce productivity when the distance between them and their outranked friends is higher rather than lower. Moreover, the reduction in productivity does not improve their friends' ranks but only reduces the distance between them by lowering their own ranks. The results are consistent with the hypothesis that workers reduce effort to conform to their group productivity. Additional tests rule out that the productivity drops because workers are complacent after learning new information about peers' productivity from public ranks. The evidence is also inconsistent with alternative explanations such as ratchet effect, inequity aversion, and altruism.

Finally, novel data on cooperation from the production floor reveals that the

[3]Additional placebo tests confirm that the productivity drop among publicly-ranked workers realizes only after the ranking starts.

[4]See Bandiera et al. (2005) and Mas and Moretti (2009) for evidence on how workers internalize externality on their peers.

rank-competition adversely affects cooperation among workers. In order to measure cooperation, the workers are randomly assigned into 20-minute *observation* slots during which they are watched from a distance and their interactions with their peers are recorded. These observations span various days before and after the ranking begins. A simple before-after comparison shows that, compared to the pre-intervention period, the likelihood that an observed worker engages in at least one event of cooperation with a peer during an observation is halved after the ranking begins. Cooperation falls not only in frequency but also in duration. The average duration of engagement between workers decreases from about 60 seconds to 40 seconds. This before-after comparison circumvents the issue that changes in cooperative behavior of workers in one arm may affect workers in another. But it may also reflect simple seasonal change in cooperation. However, several tests suggest that cooperation falls, at least in part, because of the ranking contest. First, compared to control-arm workers, the likelihood of engaging in an act of cooperation during an observation in post-intervention months is weakly lower for privately-ranked workers and strongly lower for publicly-ranked workers, for whom the contest is the most salient. Reassuringly, there exists no such differences before the intervention. Second, the reduced likelihood for publicly-ranked workers is driven by reduced interactions with fellow publicly-ranked peers, that is, direct competitors for ranks. Finally, although there is no decrease in the share of cooperation that involves help harder to refuse (e.g. lending a design chart, or help with machine or sweater measurement), the share of voluntary help in the form of offering production tips or suggestions falls. Overall, the reduced cooperation lead to lower productivity for workers who were more likely to seek help in the pre-intervention period. But this is uncorrelated to the productivity drop we earlier interpreted as desire for social-conformity among publicly-ranked workers. Withdrawal of cooperation in the post-intervention months also reassures that the productivity drop among publicly-ranked workers is not a selfless act but possibly driven by social pressure.

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 productivity from public ranks because of workers' status-seeking behavior (e.g. 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 this mixed evi-

dence from public ranks becomes further complicated by the finding that even private ranks generate mixed effects. Blanes-I-Vidal and Nossol (2011) and Gill et al. (2019) find positive effects from private ranks while Barankay (2011, 2012) find negative effects, indicating that ranks may generate ambiguous effects from underlying intrinsic incentives alone. Using an experiment to disentangle intrinsic and social incentives, this paper shows that social-status seeking incentives from public performance ranks may be counteracted by workers' social preference for conformity to social groups in their workplace. This resonates findings in Blader et al. (2020) that workers consider competition with peers inconsistent with a collective spirit toward work.

The paper also presents novel data from a real workplace to contribute to the literature that studies cooperation among workers. Empirical studies that explore cooperation among workers have been largely limited to laboratory experiments. For instance, a number of papers have studied help or sabotage under tournament incentives in laboratory settings (e.g. Carpenter et al. (2010); Harbring and Irlenbusch (2011); Danilov et al. (2019)) or in sports (e.g. Deutscher et al. (2013); Garicano and Palacios-Huerta (2014)). Evidence from typical workplaces have been scarce as it is extremely difficult to measure such cooperation in the workplaces. This paper makes a novel contribution by measuring cooperation among workers in a manufacturing firm and presenting evidence on how such cooperation can be adversely affected by an incentive scheme that triggers competition among workers.

More broadly, the paper contributes to the literature that studies how social preferences affect worker productivity.[5] A closely related paper in particular is Bandiera et al. (2005) which shows how university students working as temporary fruit pickers in an UK farm withdraw effort under *relative pay scheme* to internalize the negative externality their effort imposes on peers' earnings. Using relative performance ranking, my paper complements Bandiera et al. (2005) in showing how relative incentive schemes which pit workers against each other may be counterproductive because of workers' social preferences. Yet, there are two key differences between the papers. First, the relative incentive scheme in Bandiera et al. (2005) involves pay while in

[5]A booming literature on social preferences of workers has already documented various forms of preferences. For example, we know that workers care if their work creates value for their employer (DellaVigna et al. (ming)), they enjoy working beside their friends (Bandiera et al. (2010), internalize externalities of their actions on their peers (Hamilton et al. (2003), Mas and Moretti (2009)), reward employer in return for increase in pay (Gneezy and List (2006), Jayaraman et al. (2016)), but can also punish them at times (Casaburi and Macchiavello (2015), Akerlof et al. (2021)). Ashraf and Bandiera (2018) provides an excellent review.

this paper it is purely non-monetary and social in nature. Second, workers in the fruit-picking farm reduce effort to internalize the negative externality of their effort on peers. On the other hand, the underlying mechanism for productivity drop in my paper is workers' desire to conform to group productivity, a different form of social preferences. Moreover, although social conformity has been documented in settings such as investment decisions in education (e.g. Bursztyn et al. (2019)), energy consumption (Allcott (2011)), and laboratory experiments (e.g. Gächter et al. (2013)), evidence from workers in real workplaces are surprisingly scarce. It is unclear a priori if conformity on effort is relevant when effort directly determines workers' income for living. But it is an important question as it has implications on the organization of workers inside firms. This paper shows how social concerns can lead even mature and experienced workers[6] to undercut their performance and earnings to conform to social groups.[7]

The paper is organized as follows. Section 2 describes the garment factory where the experiment for this paper is conducted. Section 3 discusses the key incentives underlying performance ranks and describes the experiment that builds on it. Section 4 presents the main results on worker productivity. Section 5 tests their robustness and Section 6 explores the underlying mechanism. Section 7 investigates the effect of the intervention on cooperation among workers. Section 8 concludes.

2 Setting

I begin by describing the sweater factory where the experiment for this paper takes place. The experiment takes place at a knitting department of a large sweater factory in Bangladesh. In this department workers knit yarns into sweater parts which are later passed on to other departments for the remaining production processes. In January 2016 (*baseline*), the department consisted of 366 knitting workers, all of whom are part of the experiment.

At any point in time, a knitting worker is assigned to knit a batch of sweaters, a

[6]The workers are between 21 and 46 years old at baseline, with tenure at the factory between one and 11 years.

[7]A related but a different stream of work studies the effect of relative incentives among workers. This stream has explored the effect of information about relative income on worker productivity (Breza et al. (2018)), job satisfaction (Card et al. (2012)), and general welfare (Perez-Truglia (2020) and Culen and Perez-Truglia (2021)).

job, which typically constitutes of 12-48 pieces of sweaters of a particular style and size.[8] The workers work alone using individually assigned knitting machines and raw materials. Once a worker completes the job in hand he receives his next, which can be sweaters of the same style and size or a different combination.[9] The workers receive monthly payment based on the number of sweaters they knit and corresponding piece rates. The piece rates, however, vary by style and size. The workers are grouped into 15 administrative groups called *blocks*, and supervised by one supervisor whose role is largely limited to instructions and troubleshooting.

Several attributes of this department make it a suitable setting for the study. First, production in this department takes place through an individual production process. This makes it possible to measure and compare productivity across workers.

Second, the existing social ties among the knitting workers lend a rich environment to study social incentives. During a baseline survey before the experiment 345 of the 366 workers report socializing with at least one of their coworkers outside the factory (*friends*), possibly because they know each other for a long time. At baseline, the workers had an average tenure at the factory of about five years.

Third, the workers exhibit relative preferences with respect to productivity. The baseline survey helps to illustrate this point. By individually naming all the co-workers in their block, the survey asked if the workers compared their own productivity to that of the co-worker's.[10] More than 90% of the workers admit to comparing their productivity with at least one coworker in their block. Conditional on comparing themselves with at least one coworker, the workers compare themselves to 10 coworkers on average, or about 40% of the coworkers in their block.

Yet, the workers have a noisy idea of how their productivity compares with that of their coworkers. Even with respect to peers they compare themselves to, the workers get almost 40% of bilateral comparisons wrong.[11] This suggests that performance ranks could provide the workers with new information that they care about.

[8] Figure A1 in the Appendix shows pictures of a knitting machine and a sweater part in production.

[9] Almost all the workers in this Section are males. So I use a masculine pronoun.

[10] Doing the same for coworkers outside their block was difficult because of the size of the workforce.

[11] This is calculated from another question which ask the workers to compare their productivity against their coworkers. The comparisons are based on production earnings in the last three months.

3 The Experiment

I conduct a randomized experiment at the knitting department discussed in the previous section. In partnership with the management of the factory, I provide its knitting workers with performance feedback in the form of performance ranks. This section discusses the key incentives underlying performance ranks and the experimental design that disentangles these incentives.

3.1 Incentives from Ranks

Performance ranks distinguish more productive workers from less productive ones. Even without monetary rewards, high ranks may be desirable to workers as it may carry social status in the workplace (Moldovanu et al. (2007); Besley and Ghatak (2008)).[12] For instance, higher-ranked workers may earn greater respect from coworkers and even enjoy preferential treatments from the employer. Status rewards increase the marginal return of effort, which can induce workers to increase effort to signal high productivity.[13]

However, performing well in ranks may also invite social sanctions from peers. Workers may be especially concerned about sanctions from their social group (*friends*) if they are perceived as eager to distinguish themselves from the group in productivity. Moreover, a worker's effort imposes a negative externality on his peers in a ranking contest as the peers may go down the ranks when the worker improves his own. Ascending ranks may, therefore, be considered as selfish and similarly lead to social sanctions. A desire to avoid such sanctions may induce workers to reduce productivity and signal social compatibility to their friends.[14]

In summary, public ranks introduce a trade-off of social signals for the workers.

[12] Empirical support for this idea exists in papers such as Ashraf et al. (2014b) and Ager et al. (2021) who find positive productivity effects of public ranks.

[13] Workers may also want to signal high productivity to their employers because of career concerns (Holmstrom (1999)). This paper focuses on signaling for status, as the empirical setting of this paper eliminates career concerns as an underlying incentive. At baseline, salary of a supervisor in the knitting department is less than the earnings of a knitting worker at the 33rd percentile of earnings distribution. Therefore, the best workers are unlikely to want to get promoted as supervisors.

[14] Conformism driven by group identity has been modeled in several theoretical papers (e.g. Bernheim (1994); Akerlof (1997)). Empirical support exists in, for example, Bursztyn et al. (2019)) who show students under-invest in education to conform to their social groups. Although not in the context of ranks, Bandiera et al. (2005) and Mas and Moretti (2009) show workers internalize externality of their actions on peers.

The trade-off is between signaling high productivity to peers and managers, or signaling social compatibility to their social group. Intrinsic motivations from ranks can affect worker productivity too. Intrinsic motivations, unlike signaling motivations, derive from private consumption of ranks. Workers may value being better at their jobs irrespective of extrinsic benefits. Indeed, empirical papers that reduce signaling utility of ranks by using private ranks find strong effects on worker productivity.[15]

3.2 Treatments

As part of the intervention, the knitting workers receive monthly feedback on their production in previous month. They receive their first feedback at the beginning of February 2016, and continues to receive them monthly until June 2016.[16]

The workers receive one of three kinds of feedback, all of which are delivered through individually addressed monthly letters. The first feedback provides aggregate information about a worker's production. This includes the total of number of sweaters the worker produced in previous month, a break down of his production by styles, total time taken to produce them, and a list of all the workers who receive similar feedback. Workers who receive this feedback are essentially the *Control* group since they do not receive ranks. The second feedback additionally provides information on a worker's rank based on his production in the previous month. This rank is revealed only to the worker and it is therefore a *Private Rank Treatment* (henceforth, Private Treatment).[17] The third feedback contains the same information but workers now see the ranks of their peers beside the list that shows all the workers who receive this feedback. In other words, workers in this *Public Rank Treatment* (henceforth, Public Treatment) know each other's ranks. To be clear, workers in a given treatment are ranked against workers in the same treatment. Section A.1 in the Appendix discusses how the ranks are computed.

The experimental design makes a distinction between intrinsic and social incentives from ranks. By informing workers of their ranks in private, Private Treatment

[15] See Gill et al. (2019), Blanes-I-Vidal and Nossol (2011), and Barankay (2011, 2012).

[16] The factory management wished to continue the performance feedback as a management practice if the results from the intervention proved promising. As a result, workers were not told of a termination date for the intervention.

[17] Following Barankay (2012), I additionally provide the workers with information on the ranks they can achieve if they improve their average production time by 5, 10, and 20 percents. This additional information tells them how much faster they would need to work to improve their ranks.

aims to trigger intrinsic incentives from ranks. Public Treatment additionally introduces signaling motivations by making ranks publicly visible. The difference between Public and Private Treatments estimates the net effect of signaling motivations alone. The Control arm helps to estimate net effect from each of these treatments.[18] In order to identify the workers who may want to signal social compatibility, the baseline survey recorded details of the workers' social network in the workplace. It contains information about the frequency of interactions between a pair of workers while at the factory, and whether they socialize outside the factory. This helps to identify the social group a worker identifies with at the workplace.

3.3 Randomization

Workers are assigned into one of the three experimental arms through a public lottery in January 2016.[19] Once assigned to a particular arm, a worker receives the same kind of feedback for the entire intervention. A particular concern about individual randomization in this context is that of contamination and spillover effects.[20] Nonetheless, to identify spillover effects, I experimentally vary the intensity of the strongest treatment - public ranks - across blocks. To be specific, prior to random assignment of workers into experimental arms, each of the 15 blocks of workers is randomly chosen as either a Private- or a Public-Intensive block. In Private-Intensive blocks, 43.33% of the workers are assigned to Private Treatment and 23.33% to Public Treatment. In Public-Intensive blocks, the weights are reversed. The Control group consists of a third of the workers in each block. This leads to an equal distribution of workers across the three experimental arms, with 125 workers in Control, 117 in Private Treatment, and 124 in Public Treatment. 197 of these workers are located in Public-Intensive blocks and 169 in Private-Intensive blocks.

3.4 Data

The paper uses three sources of data as described below.

[18] Importantly, letters delivered to Control workers account for potential effects from simply receiving the letters or concerns about their production being tracked by the management.

[19] The workers pick a number to decide what kind of feedback they will receive. The numbers, one, two, or three, represent the three experimental arms and are written inside folded papers. This makes it transparent as to why a worker receives a certain kind of feedback during the intervention.

[20] An alternative would have been cluster randomization at the block level. However, with only 15 blocks in total it was not necessarily a better design.

(i) Administrative Records: A set of administrative records provide information about all the knitting workers in the experiment. This include information such as age, tenure, and block assignment but also job-level production details of about 20,000 jobs that the workers completed during the study period (January-June, 2016) and monthly production earnings, monthly attendance records, and piece rates of sweaters starting from January 2015.[21]

(ii) Baseline Survey: A baseline survey conducted in October 2015 collect information on the workers' social network in the factory, including peers a worker interacts with inside the factory, how frequently they interact, and who they socialize with outside the factory. The survey also ask the workers about who they compare their productivity with, how do they compare, etc. In addition, the survey measure the workers' attitudes towards competition and risk.[22]

(iii) Cooperation Data: The research team collect first-hand data on cooperation among workers by physically observing the workers while they work. This data is discussed in Section 7.

Before moving on to the main analysis for the post-intervention months, Table A1 tests if the knitting workers in the three experimental arms are balanced in observed characteristics at baseline. The Table tests three sets of characteristics - productivity (the key outcome variable of interest), demographics, and social network. Columns 1-3 report the means of these characteristics for the three arms. Columns 4-5 test equality of these means between the control and treatment arms and report corresponding p-values. Column 6 does the same for the two treatment arms.

The first panel verifies that the mean productivity of workers at baseline is similar across the arms and cannot be rejected at traditional significance levels. The most preferred measure of productivity is average knitting time of a sweater, which is simply the total time a worker takes to complete a batch of sweaters (a *job*) divided by the number of sweaters in that batch (which usually varies between 12-48). However, since

[21] The factory began recording job-level production time only in January 2016.

[22] Attitude towards competition is measured through a game in which workers throw balls into a basket. Prior to throwing the balls, the workers choose how they want to get paid from each successful shot - either at a fixed piece rate (non-competitive) or double the rate if they compare favorably against a peer to be randomly chosen later (competitive). This game is common in the literature for measuring competitive attitude among individuals (e.g. Gneezy et al. (2009)). Risk is measured as a self-reported assessment of a worker's risk taking behavior on a scale of 1 to 10.

pre-intervention data on the knitting times is available only for one month (January 2016) this may not reliably capture the true productivity of the workers. Therefore, I also report alternative productivity statistics that span a longer period of time (since January 2015) but only at month-level. These include monthly production earnings, attendance, and total number of sweaters produced. Reassuringly, the workers in the three experimental arms are balanced on the alternative productivity measures too.

The second panel tests balance of various demographic characteristics of the workers. The workers are similar in the number of years they attended school, their age, tenure at the factory, and attitudes towards competition and risk.

Finally, the third panel tests balance of workers' social network. The average number of workers they have in their block is similar across the experimental arms. The number of reported friends at baseline is also similar between control and each of the treatment arms, but slightly higher in Public Treatment than in Private Treatment. Separate joint-significance tests for Columns 5-7, however, cannot reject that the variables do not jointly predict treatment status. Finally, the ratio of productivity of friends who are in the same arm as a worker and of those who are not is around one in all the arms. This confirms that the friends randomized into the same arm as a worker are a representative sample of his social network in terms of productivity.[23]

4 Productivity Response To Ranks

This section presents the main results on the effect of ranking on worker productivity. Estimates of average treatment effects suggest that although private ranks do not affect worker productivity on average, public ranks decrease productivity by more than 1.5 percent. In other words, signaling incentives from public ranks lead to net reduction in worker productivity. Next, I provide evidence to suggest that it is the incentive to signal social compatibility to social groups that drives the drop in productivity, and verify robustness of these findings.

[23]This test is restricted to workers who have friends both inside and outside their own treatment arm. Additionally, the baseline survey was conducted in October 2015. This restricts me to using production earnings as a measure of productivity in months preceding the survey.

4.1 Average Treatment Effects

I estimate average treatment effects on productivity using the following specification:

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

Y_{isbt} is worker i's productivity in knitting sweater of style and size s in job b in month t. I measure individual productivity using the average time a worker takes to knit a sweater in each job he completes (see Section 3.4). Apart from being a high-frequency measure of productivity it is also cleaner than alternative measures such as monthly production earnings which is susceptible to variations in piece rates.

$Public_i$ and $Private_i$ refer to treatment status of worker i. I use logarithmic transformation of the knitting time to interpret β_1 and β_2, the coefficients of interest, as percentage changes in productivity. A positive coefficient indicates lower productivity.

To increase power, I include worker i's long-term productivity α_i as an explanatory variable.[24] In absence of data on knitting time for a sufficiently long time before the intervention, I measure α_i with worker i's average production earnings over January-December 2015. 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, I cluster standard errors at block-level and bootstrap them as there are only 15 blocks in the factory.[25]

I begin by estimating Equation 1 while controlling for only the workers' long-term productivity and style-and-size fixed effects in Column 1 of Table 1. The estimated average treatment effects reveal that private ranks do not affect worker productivity, which in other words implies that intrinsic incentives from ranks do not affect productivity on average. In contrast, making ranks public significantly decreases worker productivity; knitting time per sweater increases by more than 1.5 percent. This is equivalent to a loss of half-a-day's production per month per worker on average.

The estimates are robust to additional controls. Column 2 adds month fixed effects to the specification. Column 3 additionally controls for the workers' experience with a particular sweater style and size, measured by the cumulative number of jobs in which the workers worked on the same style and size. Column 4 adds additional controls in

[24] This is in the spirit of ANCOVA specifications (McKenzie (2012)).

[25] I later show that the results are robust to clustering errors at worker level instead.

the form of tenure at the factory as of baseline, the number of years they attended school, self-reported index of risk-taking behavior, and attitude towards competition. The estimates remain robust to all the controls.

Figure 1 plots how the distributions of worker productivity changed because of the intervention. Note that knitting times of sweaters vary by their style and size. Hence, any difference in the average knitting time of a sweater between two workers can be driven by differences in their productivity as well as differences in the sweater styles and sizes that they worked on. Therefore, Figure 1 plots the residuals from a regression of job-level sweater knitting times on style-and-size fixed effects.[26] First, the left panel shows that the distributions of average knitting time of a sweater, adjusted for style and size effects, are identical across the three experimental arms before the ranking begins. The right panel, on the other hand, shows that the knitting time increases among publicly-ranked workers after the ranking begins. Moreover, it increases all along the productivity distribution. Mirroring earlier results, the distribution for privately-ranked workers remains similar to that for Control workers.

Private Treatment triggers only intrinsic incentives from ranks while Public Treatment triggers both intrinsic and signaling incentives. Strong effects from public ranks and null effect from private ranks imply that the workers are driven by signaling incentives from ranks rather than by their intrinsic incentives. The difference in productivity between the two treatments isolates the response to signaling incentives alone. This difference, around one percentage point, is negative and only marginally insignificant (average p-value$\approx$0.15 across the four specifications in Table 1). This suggests that the signaling incentives were overwhelmingly negative in nature and outweighs any positive signaling incentives. The following Section disentangles them.

4.2 Disentangling Signaling Motivations from Ranks

Public ranks trigger two conflicting signaling incentives, one to signal high productivity to peers and managers, and another to signal social compatibility to friends. I conduct several empirical tests in this Section to disentangle the two. First, notice that workers would signal social compatibility only when direct comparisons can be and are made between their productivity and their friends'. This is possible only

[26] To be strict, the residuals reflect variation in productivity across workers and across jobs for a given worker. Alternative estimation of residuals while controlling for style-and-size fixed effects and job sequence yields identical distributions.

when the workers directly compete with their friends for ranks.

Therefore, Column 1 of Table 2 makes a distinction between workers who compete with friends for ranks and those who do not. Indeed, in response to the ranking, publicly-ranked workers who do not have any friends in Public treatment and therefore do not compete with any of them increase productivity by more than 4.5 percent compared to similar workers in Control arm. Compared to them, publicly-ranked workers who do compete with at least one friend are more than seven percent less productive, as indicated by the coefficient of the interaction term. The sum of the two coefficients estimates the causal effect of public ranks on workers who compete with friends, by comparing them to counterfactual workers in Control arm who would have also competed with friends had they been ranked. Thus, this holds constant underlying worker characteristics correlated with the likelihood of competing with friends and exploits only the experimental exposure to ranking. The sum suggests that publicly-ranked workers who compete with friends are 2.7 percent less productive than counterfactual workers in Control arm, and is highly significant (p-value$<$0.02).

To assuage concerns that the previous estimates reflect cross-sectional differences in productivity among workers with low and high chances of having their friends in the same experimental arm, Column 2 controls for the number of friends a worker reports at baseline. Although this affects the estimated coefficients for Control arm, reassuringly, this does not affect the estimated productivity differences between publicly-ranked workers and Control workers.

To interpret the productivity responses among publicly-ranked workers as responses to signaling motivations it is necessary to control for intrinsic incentives from ranks. Therefore, Column 3 compares publicly-ranked workers to privately-ranked workers instead of workers in Control arm.[27] The estimates now reflect productivity change due to experimental increase in visibility of the workers' signals and confirm that signaling motivations from public ranks lead to lower productivity. Compared to similar but privately-ranked workers, publicly-ranked workers who compete with friends reduce productivity by about 1.8 percent (p-value$<$0.06). On the other hand, status rewards drive workers who do not compete with friends to comfortably increase productivity by two percent, although the estimate is imprecisely estimated.

The results in Column 3 are consistent with competition triggering signals of social compatibility, but are essentially reduced form results. To ascertain the link between

[27] The counter-factual arm is shown at the bottom of each column.

social-compatibility signals and ranks, Column 4 tests how the workers respond to specific information from ranks. In particular, Column 4 tests how the workers respond when they rank better or worse than their friends they compete with. As evident from the results, publicly-ranked workers reduce productivity mostly when they rank higher than the median among their friends at baseline.

Alternatively, Column 5 exploits granular variation in rank-differences and estimates the effect of distance to the median rank among friends. The results are similar to that in Column 4 but now it confirms the relationship between rank-distance with friends and productivity changes. Publicly-ranked workers monotonically reduce productivity when they rank better than the median among friends and as distance to the median increases. With an increase in distance of 10 ranks above the median, workers decrease productivity by one percent. In sharp contrast, distance to the median does not alter a workers' productivity when he ranks lower than the median. This can be seen in Column 6. The contrasting effects of rank-distance from above and below the median is statistically significant at 5% significance level.

The results presented so far suggest that publicly-ranked workers dislike outranking friends but are indifferent about being ranked lower than them. This outweighs positive signaling incentives from ranks on average. It appears that high effort may have become stigmatized because of the ranking contest and workers reduce effort to signal compatibility to their friends. The next section provides further evidence in support of this interpretation.

4.3 Tests of Interpretation

To establish that public ranking lead workers to reduce productivity and signal social compatibility to friends, it is first useful to establish that this does not happen before the ranking begins. I begin by testing if the differences in productivity between the treatment arms for workers with relatively lower and higher ranks among friends exist in the pre-intervention months. Column 1 of Table 3 tests this question using production data from January 2016, and confirms that there is no such difference in the pre-intervention month. However, although the test provides a useful comparison of pre- and post-intervention months using the same outcome variable, a caveat is that the overall ranks at baseline are computed using production data from January 2016. This could possibly lead to the positive association we observe in Column 1.

Therefore, I redo the same exercise using production data from 2015 instead. Because sweater knitting times are not available for 2015, I now use monthly production earnings as a measure of productivity. For comparability, Column 2 first re-estimates the treatment effects from post-intervention months using the alternative productivity measure. We find a remarkably similar magnitude for the coefficient; a 10-unit increase in rank-distance above the median rank among friends decreases production earnings by about one percent.[28] Moving on to production in 2015 Column 3 tests a similar specification but with production data from February-June 2015, the same calendar months as the intervention period in 2016, to account for potential seasonality effects and increase comparability of the two time periods. Reassuringly, the estimates show that workers who are later ranked better than their friends at baseline in January 2016 were not likely to be less productive than others before the ranking begins. The coefficient in Column 3 is only a third of the coefficient in the previous column and highly insignificant.

A necessary condition to interpret the productivity change in post-intervention months as signals to peers is that it does not take place when the peers do not observe the workers' ranks. Therefore, instead of comparing publicly-ranked workers against privately-ranked workers, Column 4 now compares the two groups of treated workers against workers in the Control arm. Productivity among publicly-ranked workers show similar reductions as before even when compared to Control group workers. But in sharp contrast, privately-ranked workers do not respond to rank-distance with friends at all. This provides strong support to the interpretation that the productivity drop is triggered by visibility of the ranks and is meant as a signal to peers.

Finally, I test who the signal is targeted at. Instead of rank-distance with friends, Column 5 tests response to rank-distance with peers who the workers are not friends with. The comparison once again is between publicly- and privately-ranked workers. Reassuringly, the interaction term in Column 5 is close to zero indicating that the workers are sensitive only to relative ranks with peers they are friends with, but not others.[29] Column 6 re-introduces distance to median rank among friends into the specification. While the latter coefficient is again large and significant, the previous coefficient remains insignificant and small and, in fact, has the opposite sign now.

[28] The estimate is marginally insignificant but also note that we have lower statistical power because of month-level observations (instead of job-level observations).

[29] Qualitatively similar results emerge with respect to peers the workers often interact with inside the factory but are not friends with outside the factory.

The results in Columns 5 and 6 highlight that the signal that the publicly-ranked workers are sending is aimed at their friends.

In summary, workers reduce productivity only when they rank higher than their friends and when the ranks are visible to their peers. This is consistent with the productivity change being a signal of social compatibility aimed at their friends. The next sections conduct a series of tests to verify robustness of these findings and rule out alternative explanations.

5 Robustness Tests

In this Section I examine robustness of the results in the previous sections. In particular, I rule out concerns that the productivity drop is driven by workers' underlying characteristics, including their social popularity and productivity in general. Next, I probe the existence of spillover effects across experimental arms and examine if it drives any of the original results. Finally, I show robustness of the findings to alternative measures of relative productivity and clustering of errors.

5.1 Endogenous Worker Characteristics

The heterogeneous treatment effects estimated in Section 4.2 can be prone to alternative interpretations. In principle, it could be driven by worker characteristics that determines (a) if a worker finds himself competing with friends, and (b) his relative ranks among the competing friends. In principle, these characteristics could affect our key estimate in two ways. First, they could lead to selection effects. A worker who competes with friends, or a worker who ranks relatively higher among his friends, is different than one who does not. Second, instead of motivations for signaling social compatibility, it could be these underlying workers characteristics that drive the differential productivity responses between privately- and publicly-ranked workers. Notice that selection effects are unlikely to drive our key estimate as we compare privately-ranked and publicly-ranked workers with similar underlying characteristics. Nonetheless, I conduct several robustness tests in Table 4 to assuage these concerns.

Columns 1 and 2 in Table 4 tests if the productivity drop among publicly-ranked workers is driven by worker characteristics that determine if a worker competes with friends to begin with. We would be particularly concerned about the number of

friends a worker has at baseline. Higher the number of friends a worker has higher is the likelihood that at least one of them is randomized into the same experimental arm.

Notice that Columns 2 and 3 in Table 2 already addressed selection concerns of this nature by controlling for the number of friends a worker reports at baseline. Alternatively, Column 1 in Table 4 restricts sample to workers with at least one friend in the same experimental arm and re-estimates the relationship between post-intervention productivity and baseline distance to median rank (from above) among those friends. The key estimate remains the same as in full sample leading to two conclusions. First, the negative relationship between productivity and relatively higher ranks among friends is not driven by high productivity of workers who do not compete with friends. Second, the relationship holds among the subset of workers who do. In other words, the differential productivity drop among publicly-ranked workers is not a result of endogenous selection of workers into who competes with friends and who do not.

The specification in Column 2 additionally tests if productivity drop among publicly-ranked workers is driven by workers' social popularity rather than motivations for signaling. Using the full sample of workers, the specification lets social popularity affect publicly-ranked workers differently than it does privately-ranked workers. Our main estimate remains robust in size and precision, ruling out concerns that workers' social popularity is at the heart of productivity response among publicly-ranked workers.

Similarly, Column 3 investigates whether a worker's prior productivity level matters in explaining our key result (for instance, through complacency). All the specifications in Table 2 already controlled for workers' baseline productivity, partly addressing the concern of selection along this line. Column 3 additionally lets workers' baseline productivity affect publicly-ranked workers differently than privately-ranked workers. Again, the estimate of productivity drop among better-ranked friends remains largely unchanged.

Column 4 and 5 present alternative tests of whether it is worker's general level of productivity that leads to reduction in effort after ranking begins, or it is relative productivity within a worker's social network in particular. Instead of distance from and above median rank among friends, Column 4 tests whether a similar distance but with respect to peers a worker is not friends with (non-friends) also leads to a drop in productivity. If publicly-ranked workers reduce productivity because they

are more productive in general, and not specifically among their friends, we expect to find similar drop in productivity when they rank higher than either kinds of peers. The coefficient of the interaction term in Column 4, however, is close to zero, which is in sharp contrast to what we found earlier in Table 2. Column 5 tests sensitivity to distances from and above median ranks among both friends and non-friends. While publicly-ranked workers reduce productivity when they rank higher than their friends, they do not when they rank higher than other peers.

Column 6 controls for both baseline productivity and social popularity of worker at the same time. Column 7 controls for additional observed worker characteristics and their interactions with *Public*, including controls for tenure at factory, total number of years a worker attended school, attitude towards competition, and a self-reported index (on a scale of 1 to 10) of how risk-loving a worker is. Column 8 controls for whether a block had relatively more publicly-ranked workers than privately-ranked workers, or vice versa. Our key estimate remains robust all throughout, even if occasionally attenuated by a little.

5.2 Spillover Effect on Privately-Ranked Workers

Workers are individually assigned to experimental arms. This along with the fact that they are located spatially close to each other on the production floor could create spillover effects across experimental arms. For instance, we may be concerned that privately-ranked workers share information about ranks among themselves to imitate publicly-ranked workers. However, to the extent that privately-ranked workers behave more like publicly-ranked workers, it becomes only harder to detect a difference in outcome between the two treatment groups. We would be concerned if the spillover effects rather lead to an increase in performance among workers who are not publicly ranked, especially on privately-ranked workers who serve as the counterfactual for most of our analysis, and lead to overestimation of treatment effects.

To address spillover concerns the experimental design had randomly varied across blocks the intensity of Public Treatment, the most intensive treatment. Randomly selected eight blocks have 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.[30] This creates a random variation in the density of publicly-ranked workers across blocks, which is likely to be correlated with the magnitude of spillover effects on privately-ranked workers.

Table A4 tests spillover effects by comparing privately-ranked workers to Control arm workers while exploiting the variation in density of publicly-ranked workers across blocks. Column 1 estimates average treatment effect on privately-ranked workers while distinguishing between Private- and Public-Intensive Blocks. The interaction term of Private Treatment and block type is positive and highly insignificant. It suggests that, if anything, privately-ranked workers with relatively greater share of publicly-ranked workers in their blocks reduce productivity in similar fashion as the latter workers do. A positive coefficient for the indicator variable of block type reflects a similar behavior among Control arm workers. The results suggest that, although highly noisy in precision, any spillover effects from Public Treatment on privately-ranked workers would only attenuate the differences between the two treatments.

Columns 2 and 3 test how privately-ranked workers in the two types of blocks respond to distance from and above median rank among friends - the key behavior that drives productivity drop among publicly-ranked workers. Interestingly, privately-ranked workers in neither Private- nor Public-Intensive Blocks react to rank-distances with friends. The estimated coefficients for the interaction terms with rank-distance in the two sets of blocks are small in magnitude. The coefficient for Private is small in Private-Intensive blocks, but large in Public-Intensive blocks. This indicates that the positive coefficient of the interaction term in Column 1, although noisy in precision, is largely driven by a general decrease in productivity of privately-ranked workers in Public-Intensive blocks, rather than by the strategic decreases in productivity among publicly-ranked workers in response to increasing rank-distances with friends.

6 Tests Of Mechanism

We now turn to exploring the mechanisms behind the productivity drop among publicly-ranked workers. The evidence so far establish that workers reduce productivity when they *publicly* outrank their friends. Because the drop is triggered by public visibility of ranks to their peers, it suggests that the workers might be driven by fear of social sanctions from their peers. Because the workers seem to care only about

[30] See Section 3.2 for more details about the experimental design.

rank-distance with their friends it suggests that they fear sanctions from their friends in particular.

There are two related but subtly different reasons why workers may fear social sanctions. First, an active attempt to differentiate themselves from their social group in productivity may lead to loss of membership in the group. Second, improving own ranks by suppressing their friends' imposes a direct negative externality on them, which could be perceived as selfish and attract social punishment. In either of these cases we expect workers to reduce effort and signal social compatibility to their friends. But in the first case it is driven by a desire for social conformity to group characteristics while in the second it is driven by social pressure to internalize externality. I try to distinguish between the two mechanisms below.

6.1 Conformity vs Externality

A worker's effort imposes a negative externality on his friends when it leads to suppression of their ranks. This is more likely to happen when the worker and his friends are ranked close to each other and marginal changes in the worker's effort can affect their ranks. On the contrary, a worker is more likely to be perceived as keen to distinguish himself from his group when he outranks his friends by substantial distance. Taking cue from this observation I exploit the composition of ranks among friends to understand which of these concerns lead to reduction in productivity among publicly-ranked workers.

Figure 2 plots productivity response of publicly-ranked workers when friends they outperform are different rank-distances apart. Distance with each friend is binned as 0-20, 20-40, 40-60, or 60 and above. Again, the counterfactual workers are privately-ranked workers which allows us to cleanly separate the effect of making ranks public. As evident from the Figure, in response to making ranks public, workers do not decrease productivity when they outrank friends by narrow margins. If anything, when there is an additional friend ranked lower but within 20 rank-distances workers increase productivity. On the other hand, when there are friends that a worker outranks by more than 40 ranks he responds by decreasing his productivity.

Table 5 begins by presenting similar results which paves the way for further tests. Column 1 reports results from a similar specification as Figure 2 but aggregates rank-distances into two bins, 0-40 ranks or more than 40 ranks. Resonating Figure

2 Column 1 shows that most of the productivity drop happens when workers have friends ranked far away from them. Having one additional friend ranked lower by more than 40 ranks leads to a decrease in productivity by about 2.5 percent. In contrast, having one additional friend ranked lower but by less than that distance leads to insignificant change in a worker's productivity. Column 2 additionally controls for the number of friends a worker has in total. This holds constant the total number of friends a worker has and exploits only their locations in the rank-distribution around the worker. The results remain robust. Figure 2 and estimates in Columns 1 and 2 of Table 5 are consistent with workers being concerned about ranking too far away from their social group rather than about affecting their friends' ranks.

Column 3 verify whether the reduction in a worker's productivity observed in Columns 1-2 translate into better ranks for friends. If the productivity drop is nonetheless driven by concerns about externality and aimed at improving friends' ranks, we expect to see relatively less friends ranked lower in treated months than at baseline. The outcome variable in Column 3 is the difference between total number of friends ranked lower in a given treated month and the number of friends ranked lower at baseline, a measure that reflects within-worker changes in number of outranked friends across treated months. The results confirm that productivity drop observed in Columns 1-2 does not translate into better relative ranks for friends.

Is the reduction in productivity really an attempt to reduce rank-distance with friends? If so, is it sufficient in equilibrium when other workers may reduce productivity too? Columns 4 and 5 confirm that the reduction in productivity translates into lower ranks for the publicly-ranked workers in post-baseline months and consequentially lower average rank-distance with friends. Column 4 shows that for the workers who reduce productivity in Column 1 there is a drop in their rank-percentiles by about 3 percentiles.[31] P-value adjusted using Anderson's Sharpened Q-values and reported at the end of the column confirms that this estimate is statistically robust to multiple hypothesis testing (Anderson (2008)). Finally, Column 5 shows that the lower ranks leads to lower average rank-distance with friends for the same workers. The outcome variable in the last column is average rank-distance with all the friends a worker competes with in a given month and is measured in units of 10 ranks. Al-

[31]For a given rank-percentile actual ranks can be different across arms because of differences in total number of workers in each arm (see Table A1). Hence, rank-percentiles offer better comparability across arms.

though noisy in statistical precision, the coefficient indicates a reduction in average rank-distance with friends by about 1.2 ranks.

The evidence in Table 5 shows that workers actively reduce effort to reduce rank-distance with their friends when they substantially outperform the friends. The evidence does not support the hypothesis that workers are concerned about the negative externality that their effort may impose on their friends. But is this behavior really driven by fear of social sanctions? I test a few alternative explanations in the following section.

6.2 Alternative Explanations

A plausible alternative explanation to the results in the previous sections is that the drop in productivity among publicly-ranked workers is driven by complacency. Indeed, Public Treatment not only makes a worker's rank known to others but it also makes others' ranks known to him. It is possible that the new information makes publicly-ranked workers complacent, particularly those who realize that they outperform their social groups by a substantial distance and could in fact rest on their laurels.

There are two sources of complacency. First, workers could be positively surprised by their *own* ranks if the ranks are better than what they expected at baseline. This is independent of how any or some of their peers rank. However, comparing publicly-ranked workers with privately- ranked workers already accounts for this effect as this would be present even when ranks are private. Nonetheless, for completeness, Column 1 of Table 7 controls for a binary variable that indicates whether the first rank that a worker received during the intervention is better than his prior belief (i.e. worker underestimated his true rank at baseline), and let this affect publicly- and privately-ranked workers differently.[32] There is an additional effect of the positive surprise on publicly-ranked workers but our main coefficient of interest, the effect of distance to median rank among friends, remains robust.

Next, I test the effect of positive surprises from *peers'* ranks. During the baseline survey workers were asked to compare their own productivity with that of other workers in their blocks. The number of these bilateral comparisons that were proved wrong by actual ranks in the first month of intervention serves as a proxy measure

[32]Prior beliefs about ranks were recorded during baseline survey.

of the new information that workers receive from public ranks. We are particularly interested in the number of friends a worker had predicted to be relatively more productive at baseline but later found out otherwise from the ranks. Column 2 in Table 7 controls for the effect of this new information among publicly-ranked workers. Again, the coefficient of our interest remains robust. Workers may, however, rest on their laurels only when they rank substantially better than their friends (as in Table 5). Therefore, Column 3 retests the specification of Column 1 of Table 5 but controlling for new information from ranks. Again, we find that our previous estimates are unaffected.

Alternatively, friends in Public Treatment could decide, either independently or jointly, to reduce rank-distances between themselves for intrinsic reasons such as inequality aversion. This would be infeasible among workers in Private Treatment as they cannot observe each others' ranks. I cannot rule out this possibility entirely, but it appears unlikely as publicly-ranked workers who rank below the median among friends do not increase effort. Under inequality aversion we would expect a symmetric conformity - high productive workers reduce effort while low productive workers increase effort. But conformity in our case is asymmetric and appears to be driven only by high productive friends, who sacrifice a part of their income to do so. Another related possibility is altruism. Workers could feel guilty or bad about publicly outranking friends and hence reduce their effort. But altruism-driven motivation to reduce effort would be strongest when workers impose a negative externality on their friends by suppressing the friends' ranks. We do not observe that in Table 5.

Finally, publicly-ranked workers could collude and take longer to knit sweaters to convince their management that their piece rates should be higher - a manifestation of *ratchet effect*. Since higher piece rates are a public good, workers would generally under-sacrifice their production. Making ranks public could help to enforce compliance. If this were true, however, we expect to observe productivity drop more among the most productive workers as it is in the management's interest to use their knitting time to fix piece rates. Instead, Figure 1 shows a reduction in effort all along the productivity distribution. Furthermore, publicly-ranked workers respond to rank-distance only with friends and not peers they are not friends with (see Columns 4 and 5 in Table 4). It is unclear why that would be so under ratchet effect.

7 Cooperation Among Workers

While the previous sections have explored the effect of ranking on worker productivity, ranking could also affect other outcomes that a firm would be interested in. One such outcome that can be affected by competition is cooperation among workers.

In order to explore the effect of ranking on cooperation among workers, ground members of the research team collected first-hand data of cooperation among workers from the production floor. To be precise, the team observed and recorded instances of cooperation among the knitting workers by randomizing which workers were observed in a particular time slot. Each observation time slot lasted 20 minutes and was assigned to a randomly selected worker (*focal worker*) and three other workers surrounding him. The three surrounding workers were always selected following a pre-specified rule - the next worker on the left, right, and front. Selecting multiple workers for a given time slot maximized the number of workers that could be observed in a given period of time.

During the observation slot (*observation set* from henceforth), a member of the research team recorded all the interactions among the selected workers and their peers that involved an act of cooperation. Each instance of cooperation was recorded with additional information about duration of the interaction and what the cooperation was about. A total of 480 observation sets were recorded over December, 2015 to July 2016, leading to a total of about 1,500 observations of individual workers.

Because cooperation can span across experimental arms (e.g. a publicly-ranked worker helps a privately-ranked worker) it is useful to investigate overall cooperative behavior on the floor rather than breaking it down by arms, as changes in behavior in one arm can affect outcome of another. The first graph in the top panel of Figure 3 shows that the average likelihood of at least one instance of cooperation by workers in a given observation set declined drastically from 45% in months before ranking began to less than 20% in months after. The difference is also highly significant statistically (p-value$<$0.01).

The first graph shows cooperation at the level of observation sets but is less informative at worker-level. The second graph in the top panel estimates the likelihood that a given worker engaged in at least one instance of cooperation when he is observed in an observation set. The likelihoods are much lower in magnitudes (19% and 8% in pre- and post-intervention months) but more importantly there is a similar

decline in cooperation on the floor in the post-intervention months even at individual level.[33] Since the estimates in the second graph is vulnerable to double counting if cooperation takes place between two workers in the same set, the third graph instead estimates the likelihood that a focal worker engages in cooperation during an observation set. The estimates remain robust in magnitude and statistical precision.

The bottom panel reports the average duration of an interaction between the workers when they happen. The first graph considers interactions by all workers in an observation set while the second graph considers only interactions by focal workers. The two graphs suggest that cooperation falls not only at the extensive margin (the number of instances) but also at the intensive margin (the duration for which interaction takes place). Instances of cooperation last shorter in the post-intervention months than before.[34]

Although Figure 3 provides an overall picture of changes in cooperative behavior before and after the ranking began, and helps to work around the problem that cooperation can span across multiple arms, it is vulnerable to seasonality effects. It is possible that cooperation fell in the post-intervention months because of other changes in work environment, such as work load, instead of the ranking.

I conduct several tests to check if it is indeed ranking that changes cooperative behavior among workers in the post-intervention months. I begin by estimating the likelihood that a worker engages in an act of cooperation during an observation set conditional on his treatment assignment. I do this for both pre- and post-intervention months. Figure 4 reports the estimates. We should interpret them with caution for reasons discussed above but the exercise is useful nonetheless and should be considered as only suggestive evidence. Three patterns emerge in the Figure. First, there is no significant difference in the likelihood of cooperation across the three experimental arms before ranking is introduced. Second, cooperation drastically drops for all experimental arms in the post-intervention months including the Control arm. And third, both privately-ranked and publicly-ranked workers are less likely to engage in cooperation than Control-arm workers in the post-intervention months. The difference in the likelihood between Control-arm workers and privately-ranked workers is highly insignificant (p-value = 0.22), but that between Control-arm workers

[33] Standard errors are clustered at observation set level, but works equally well at individual level.

[34] The estimates are noisier when we consider only focal workers as there are only 49 instances of cooperation where a focal worker engaged.

and publicly-ranked workers (arguably the most salient competition format) is only marginally insignificant (p-value = 0.10). Note that the across-arm comparisons in post-intervention months holds constant any time fixed-effects and is no longer affected by seasonality. The patterns in Figure 4 are consistent with rank-competition adversely affecting cooperative behavior on the production floor.

Figure 5 considers all the independent events of cooperation and explores changes in composition of cooperation before and after ranking is introduced. The small number of independent events of cooperation in the data constraints any serious exploration (n=64 in pre-intervention and n=58 in post-intervention months), but again the estimates should be considered as suggestive evidence. The left panel in Figure 5 breaks down the events of cooperation by combinations of experimental arms of the involved workers and reports the shares of the combinations in the observed events within each period. The shares of events in which privately- and publicly-ranked workers engage with Control-arm workers appear to fall in the post-intervention months. But the highest decline occurs in share of cooperation among publicly-ranked workers, the most intense competition format.

Finally, the right panel shows the shares of events of cooperation by types of cooperation. There are broadly four types of help that a worker can provide another. They can help each other with measurement of their sweater parts (e.g. by holding or stretching the part), they can lend their tools (e.g. design chart, pencil, etc.), they can help to fix parameters on the knitting machines, and finally they can share tips on the production process (e.g. suggestions on how to fix errors, provide insights from own experience of producing the same style to someone who is just starting the style). While there appears to be a general increase in helps that might be harder to refuse (measurement, lending, and help with machine), there appears to be a sharp decline in sharing of knowledge (tips), a help that is more voluntary in nature. It is suggestive that workers might be intrinsically less motivated to help peers after ranking is introduced.

In summary, evidence suggests that the rank-competition might have reduced cooperation among workers on the production floor. It would have been useful to delve deeper and understand cooperation among the workers in more details, particularly whether workers redistribute help among their peers. Unfortunately the low number of events of cooperation in the data, especially in the post-intervention months, prevents us from doing so.

8 Conclusion

Existing literature suggests that status incentives, in the form of performance-based ranks, can increase worker productivity. However, the evidence in this paper indicates that instead of responding to such incentives workers reduce productivity to conform to the level of productivity of their social groups. A novel experimental design with private and public ranking, along with detailed baseline data on workers' social network, help to show that social conformity can strongly counteract the positive effects of status motivation.

The evidence in this paper suggests that firms should seriously assess how an incentive structure interacts with social preferences of workers in the workplace. In a world with complex combination of incentives, social incentives can often counteract other positive performance incentives and make the latter ineffective.

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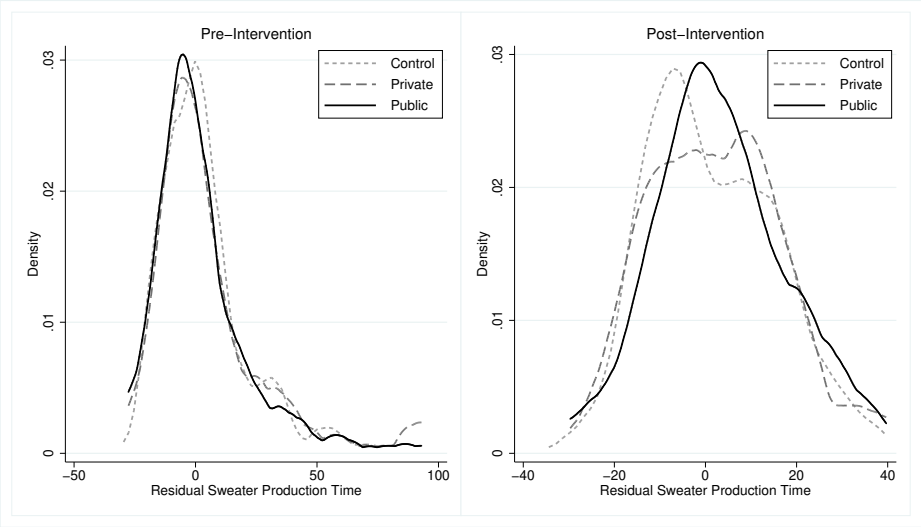
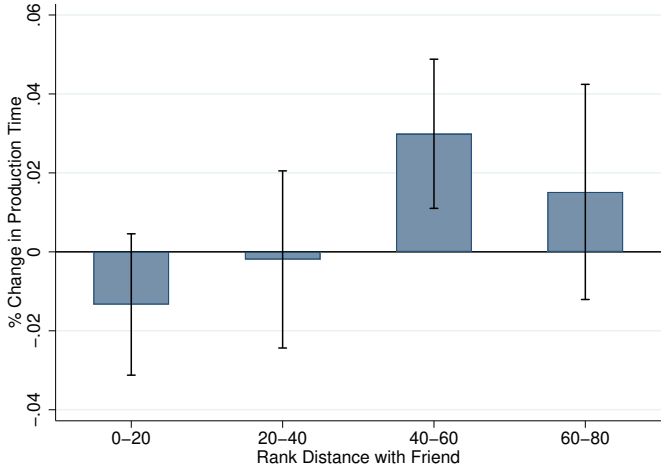
Figure 1: Distribution of Average Production Time Per Sweater



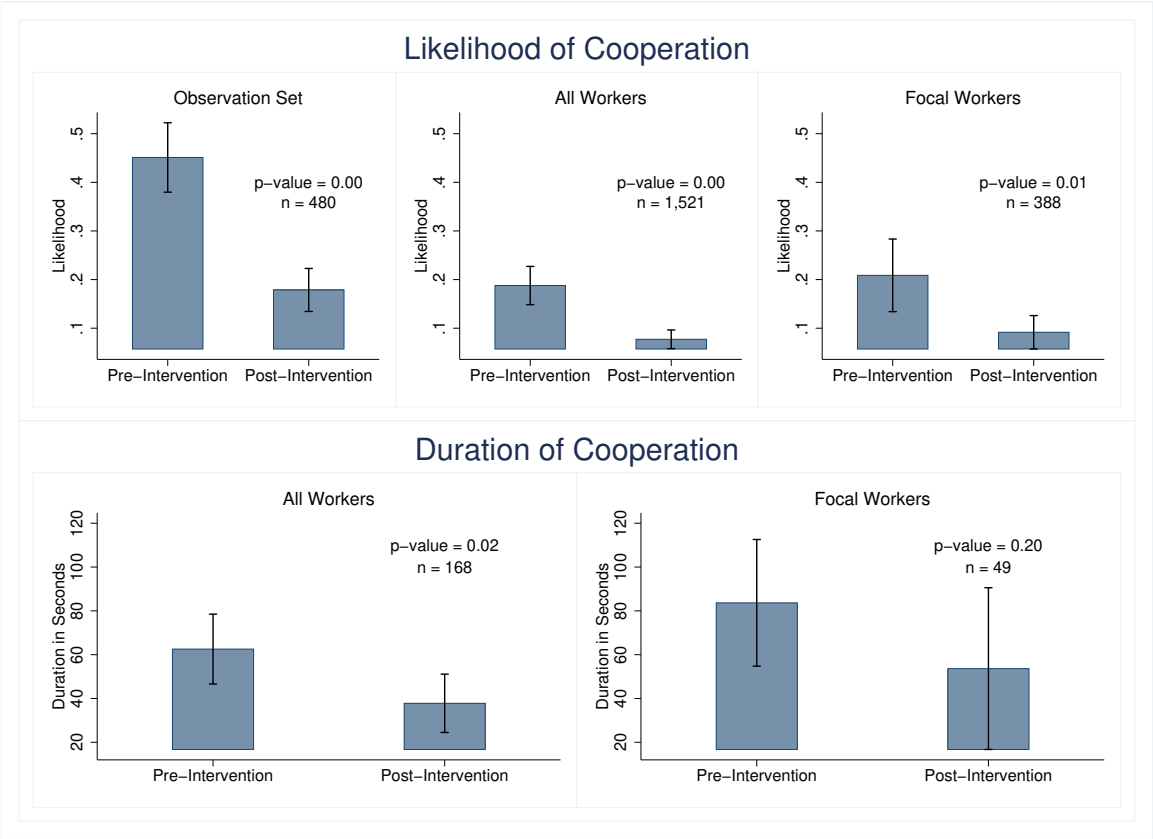
Note: The figure plots residuals from regression of average knitting time of sweaters on style-and-size fixed effects. The residuals reflect style-and-size adjusted variation in production times of sweaters across workers and jobs. The figure plots the residuals separately for the pre-intervention month of January 2016 (left panel) and post-intervention months of February-June 2016 (right panel).

Figure 2: Productivity Effect of an Additional Outranked Friend



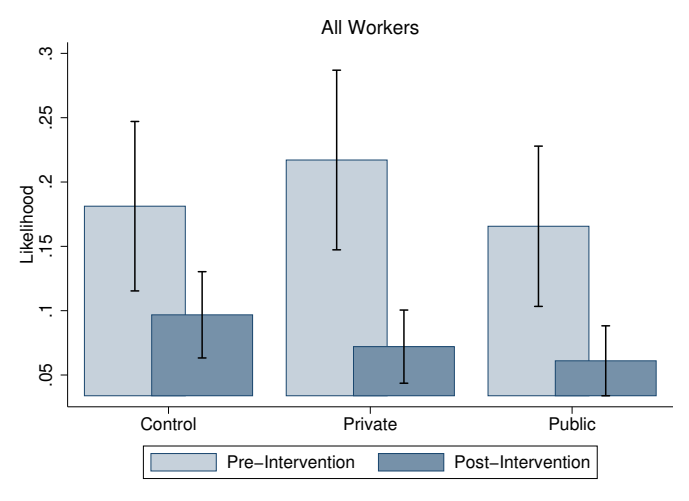
Note: The figure reports changes in average production time of sweaters among publicly-ranked workers, compared to privately-ranked workers, in response to an additional friend ranked lower at baseline. Horizontal axis shows the rank-distances by which the workers outranked their friends. Vertical axis reports the estimated productivity change in response. The estimates are derived from an underlying regression of production times on the number of friends a worker outranks by specific distances, grouped together as bins of rank-distances shown on the horizontal axis. Vertical lines on top of the bars depict 95% confidence intervals around the estimates.

Figure 3: Cooperation Among Workers



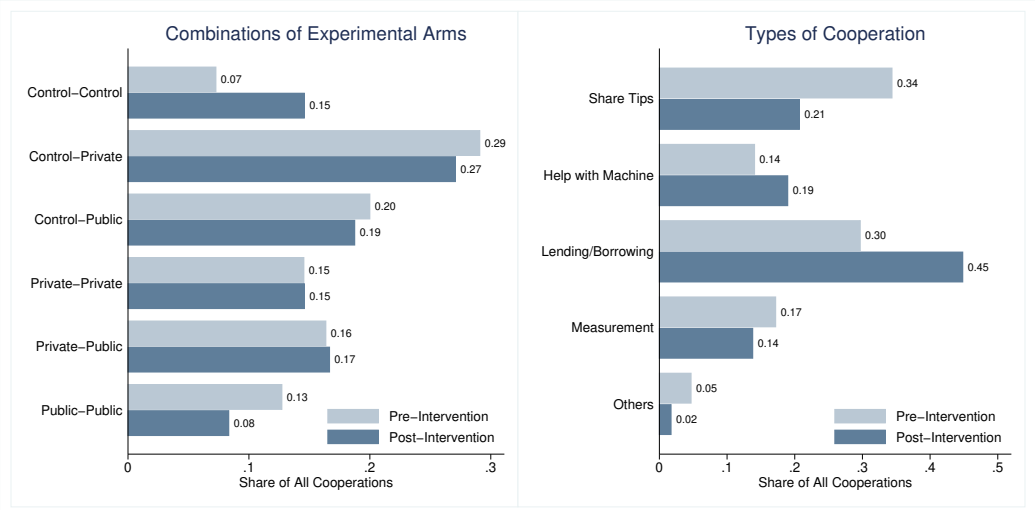
Note: The figure reports the likelihood of a worker engaging in an instance of cooperation with his coworkers. Workers were observed in 20-minute slots, during which the number of instances a worker either helped a peer or was helped by a peer was recorded. Workers were randomized into a given slot. Vertical axis reports the marginal likelihood that a worker engaged in at least one instance of cooperation in a given slot. It is estimated from a linear probability model. Left panel shows the estimates for pre-intervention months (December, 2015 and January, 2016). The estimates are not statistically different across the experimental groups at traditional significance levels. Right panel shows the estimates for post-intervention months (February-June, 2016). The difference in the estimated likelihood between Public Treatment and Control group is statistically significant at 5% significance level. Errors are clustered at worker-level.

Figure 4: Cooperation Among Workers in Different Experimental Arms



Note: The figure reports the likelihood of a worker engaging in an instance of cooperation with his coworkers. Vertical axis reports the marginal likelihood that a worker engaged in at least one instance of cooperation in a given slot. It is estimated from a linear probability model.

Figure 5: Composition of Cooperation Among Workers



Note: The figure reports the likelihood of a worker engaging in an instance of cooperation with his coworkers. Vertical axis reports the marginal likelihood that a worker engaged in at least one instance of cooperation in a given slot. It is estimated from a linear probability model.

Table 1: Average Treatment Effect of Ranking

	(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
Adjusted R-squared	0.2926	0.2972	0.3053	0.3069
Private=Public	[0.182]	[0.142]	[0.127]	[0.160]
Baseline Productivity	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes	Yes
Month FE	No	Yes	Yes	Yes
Additional Controls	No	No	No	Yes

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to produce a sweater in an assigned job. *Public* and *Private* refer to Public and Private Rank 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. *Job Sequence* is the cumulative number of jobs in which a worker produced sweaters of a given style and size. *Additional Controls* include tenure at the factory as of baseline, total number of years a worker 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 2: Social Incentives from Ranking

	(1) Ln(Time)	(2) Ln(Time)	(3) Ln(Time)	(4) Ln(Time)	(5) Ln(Time)	(6) Ln(Time)
Public	-0.0482*	-0.0483*	-0.0207	0.0068	-0.0008	-0.0040
	(0.0264)	(0.0266)	(0.0191)	(0.0132)	(0.0113)	(0.0132)
Public * 1(Competes With Friends)	0.0753***	0.0755**	0.0391			
	(0.0292)	(0.0295)	(0.0246)			
Public * 1(Ranked Above Median Among Friends)				0.0133		
				(0.0202)		
Public * Distance to Median Rank Among Friends from Above					0.0099**	0.0103**
					(0.0042)	(0.0042)
Public * Distance to Median Rank Among Friends from Below						0.0005
						(0.0040)
1(Competes With Friends)	-0.0349**	-0.0249	0.0117			
	(0.0151)	(0.0187)	(0.0157)			
Number of Friends at Baseline		-0.0009	-0.0011			
		(0.0009)	(0.0011)			
Observations	15,573	15,573	14,950	14,950	14,950	14,950
Public + Interaction = 0	[0.018]	[0.019]	[0.054]	[0.130]		
From Above = From Below						[0.049]
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
Counterfactual	Control	Control	Private	Private	Private	Private

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to produce a sweater in an assigned job. *Public* and *Private* refer to Public and Private Rank Treatments respectively. *1(Competes with Friends)* is a dummy variable that is equal to 1 if a worker has at least one friend randomized in the same experimental group. *1(Ranked Above Median Among Friends)* is a dummy variable that is equal to 1 if a worker is ranked above the median of ranks among his friends. *Distance to Median Rank Among Friends from Above* is the rank-distance to this median rank if a worker is ranked higher than the median rank, while *Distance to Median Rank Among Friends from Below* is the rank-distance to median rank if a worker is ranked lower. *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. All regressions include a constant. Col. 4 includes an un-interacted dummy variable *1(Ranked Above Median Among Friends)*. Cols.5-6 include un-interacted variable *Distance to Median Rank Among Friends from Above*; Col. 5 includes an un-interacted variable *Distance to Median Rank Among Friends from Below*. 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 3: Placebo Tests

	(1) Ln(Time)	(2) Ln(Earnings)	(3) Ln(Earnings)	(4) Ln(Time)	(5) Ln(Time)	(6) Ln(Time)
	Pre-Intervention Jan'16	Feb-Jun'16	Pre-Intervention Feb-Jun'15			
Public	-0.0142	0.0143	0.0078	0.0066	0.0130	0.0057
	(0.0187)	(0.0224)	(0.0207)	(0.0121)	(0.0093)	(0.0108)
Public * Dist. to Med. Rank Among Friends from Above	-0.0066	-0.0099	-0.0033	0.0084*		0.0128***
	(0.0060)	(0.0074)	(0.0066)	(0.0048)		(0.0038)
Private				0.0078		
				(0.0141)		
Private * Dist. to Med. Rank Among Friends from Above				-0.0011		
				(0.0048)		
Public * Dist. to Med. Rank Among Non-Friends from Above					0.0002	-0.0062
					(0.0031)	(0.0042)
Observations	1,046	1,152	1,190	22,870	14,950	14,950
Adjusted R-squared	0.5536	0.2797	0.2205	0.2977	0.2938	0.2943
Baseline Productivity	Yes	No	No	Yes	Yes	Yes
Baseline Productivity (2014)	No	Yes	Yes	No	No	No
Month FE	No	Yes	Yes	Yes	Yes	Yes
Style-Size FE	Yes	No	No	Yes	Yes	Yes
Counterfactual	Private	Private	Private	Control	Private	Private

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to produce a sweater in an assigned job. *Earnings* are monthly earnings calculated from total sweaters a worker produced in a month and piece rates for those sweaters. Columns 1-2 limit sample to pre-intervention months, while Columns 3-6 consider treated months February-June 2016. *Public* and *Private* refer to Public and Private Rank Treatments respectively. *Distance to Median Rank Among Friends from Above* is rank-distance to the median rank among a worker's friends when the worker is ranked higher than the median rank. *Distance to Median Rank Among Friends in Private from Above* is a similar rank-distance calculated with respect to friends randomized into Private Treatment. *Baseline Productivity* is average monthly production earnings in 2015, the year prior to the intervention. *Baseline Productivity (2014)* is instead calculated from September-December 2014. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 4: Robustness Tests

	(1) Ln(Time)	(2) Ln(Time)	(3) Ln(Time)	(4) Ln(Time)	(5) Ln(Time)	(6) Ln(Time)
	At Least 1 Friend					
Public	0.0006	0.0034	0.0231	0.0221	0.0422	0.0094
	(0.0141)	(0.0200)	(0.0640)	(0.0699)	(0.0935)	(0.0920)
Public * Dist. to Med. Rank Among Friends from Above	0.0098**	0.0100**	0.0107***	0.0106***	0.0072*	0.0077*
	(0.0046)	(0.0040)	(0.0040)	(0.0039)	(0.0041)	(0.0040)
Public * (# Friends (Jan'16'))		-0.0003		-0.0003	-0.0010	-0.0010
		(0.0012)		(0.0012)	(0.0015)	(0.0016)
Public * 1(Public Intensive Block)						0.0313
						(0.0221)
Observations	13,248	14,950	14,950	14,950	14,867	14,867
Adjusted R-squared	0.2936	0.2932	0.2933	0.2932	0.3039	0.3049
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
Public*Baseline Productivity	No	No	Yes	Yes	Yes	Yes
Additional Controls	No	No	No	No	Yes	Yes
Public*Additional Controls	No	No	No	No	Yes	Yes
Counterfactual	Private	Private	Private	Private	Private	Private

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to produce a sweater in an assigned job. *Earnings* are monthly earnings calculated from total sweaters a worker produced in a month and piece rates for those sweaters. *Public* and *Private* refer to Public and Private Rank Treatments respectively. *Distance to Median Rank Among Friends from Above* is rank-distance to the median rank among a worker's friends when the worker is ranked higher than the median rank. *Number of Friends at Baseline* is the number of peers a worker reports as a friend at baseline. *Distance to Median Rank Among Non-Friends from Above* is a similar measure with respect to competing peers a worker is not friends with. *Baseline Productivity* is average monthly production earnings in 2015, the year prior to the intervention. *Baseline Productivity (2014)* is instead calculated from September-December 2014. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

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Table 5: Tests of Mechanism

	(1) Ln(Time)	(2) Ln(Time)	(3) Change in Friends Outranked	(4) Rank-Percentile	(5) Rank-Distance w/ All Friends
Public	0.0034	0.0161	0.1777	0.0188	0.1034
	(0.0158)	(0.0184)	(0.1156)	(0.0282)	(0.2981)
Public * Number of Outranked Friends at Rank-Dist. <=40	-0.0067	-0.0029	0.0832	-0.0113	-0.0100
	(0.0081)	(0.0098)	(0.1233)	(0.0148)	(0.1422)
Public * Number of Outranked Friends at Rank-Dist. >40	0.0246***	0.0270***	0.0309	-0.0329**	-0.1203
	(0.0081)	(0.0091)	(0.1073)	(0.0147)	(0.2133)
Public * Number of Friends at Baseline		-0.0017			
		(0.0015)			
Observations	14,950	14,950	1,105	1,139	1,139
Adjusted R-squared	0.2938	0.2938	0.1766	0.4289	0.3278
Sharpened Q-values: Public	[0.008]	[0.008]	[0.448]	[0.026]	[0.402]
Baseline Productivity	Yes	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes	Yes	Yes
Month FE	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 produce a sweater in an assigned job. *Public* and *Private* refer to Public and Private Rank 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. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 6: Alternative Explanation - Complacency

	(1) Ln(Time)	(2) Ln(Time)	(3) Ln(Time)
Public	0.0107	-0.0035	0.0021
	(0.0135)	(0.0106)	(0.0154)
Public * Distance to Median Rank Among Friends from Above	0.0108**	0.0093**	
	(0.0049)	(0.0042)	
Public * (Number of Outranked Friends at Rank-Dist. <=40)			-0.0063
			(0.0081)
Public * (Number of Outranked Friends at Rank-Dist. >40)			0.0223**
			(0.0091)
Public * 1(Underestimated Own Rank)	-0.0491**		
	(0.0207)		
Public * Number of Friends Incorrectly Predicted as More Productive		0.0087	0.0064
		(0.0106)	(0.0108)
Observations	14,714	14,950	14,950
Adjusted R-squared	0.2938	0.2936	0.2939
Baseline Productivity	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Counterfactual	Private	Private	Private

Note: *Ln(Time)* is logarithmic transformation of average time (minutes) a worker takes to produce a sweater in a given batch. *Public* and *Private* refer to Public and Private Rank treatments respectively. *Distance to Median Rank Among Friends from Above* is rank-distance to the median rank among a worker's friends when the worker is ranked higher than the median rank. *1(Underestimated Own Rank)* is a dummy variable that takes value 1 if a worker received a rank in the first treated month that was better than what he predicted for himself at baseline. *Number of Friends Incorrectly Predicted as More Productive* is the number of friends a worker thought were more productive than him at baseline. *Dist. from Abv. to Med. Rank Among Non-Fr. Peers Talks with* is the rank-distance to median rank among non-friend peers who a worker frequently interacts with during work, provided the worker is ranked higher than the median rank. *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. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 7: Productivity Changes From Changes in Cooperation

	(1) Ln(Time) Pre-Intervention	(2) Ln(Time) Pre-Intervention	(3) Ln(Time)	(4) Ln(Time)	(5) Ln(Time)	(6) Ln(Time)
P(Engaging in Cooperation at Baseline)	0.0499*				0.0186	
	(0.0298)				(0.0341)	
P(Helped by Peers at Baseline)		0.0234	0.0206			0.0751*
		(0.0227)	(0.0235)			(0.0434)
P(Helping Peers at Baseline)		0.0926*	0.0891*			-0.0729**
		(0.0481)	(0.0488)			(0.0345)
P(Helped by Peers at Baseline) * Post			0.0292			
			(0.0300)			
P(Helped Peers at Baseline) * Post			-0.0803*			
			(0.0427)			
Public				-0.0061	-0.0091	-0.0174
				(0.0118)	(0.0135)	(0.0134)
Public * Rank-Distance to Median Friend from Top				0.0109*	0.0115**	0.0145***
				(0.0058)	(0.0057)	(0.0056)
Public * P(Engaged in Cooperation at Baseline)					0.0178	
					(0.0487)	
Public * P(Helped by Peers at Baseline)						-0.0030
						(0.0555)
Public * P(Helping Peers at Baseline)						0.0891
						(0.0562)
Rank-Distance to Median Friend from Top				-0.0081*	-0.0083**	-0.0102***
				(0.0042)	(0.0042)	(0.0039)
Post			0.0413			
			(0.0289)			
Observations	1,189	1,189	18,075	11,140	11,140	11,140
Baseline Productivity; Style-Size FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	No	No	No	Yes	Yes	Yes
Counterfactual	-	-	-	Private	Private	Private

Note:

A Appendix

A.1 Computation of Ranks

I compute ranks using the average knitting time of sweaters in previous month. Knitting times provide cleaner estimates of productivity than, for instance, production earnings which is 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.[35] Therefore, I compute ranks in four steps. First, for each style and size a worker worked 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 worked on the same style and size. Next, the percentile-ranks of each of the styles and sizes is weighted by its share in 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. Finally, the weighted average percentile-ranks are used to produce the final ranks.[36]

A.2 Alternative Measurement and Clustering of Errors

The results presented in the main paper use median ranks among friends as a benchmark to compare relative ranks of workers. Using the median rank may appear an arbitrary choice but note that distance to median is positively correlated with distance to any other rank-location among a given set of workers (e.g. the very bottom rank among friends). Nonetheless, Column 1 in Table A2 tests a more general measure, *average* rank-distance with friends at baseline. The average rank-distance, measured in units of 10 ranks, is larger for workers who rank higher within their social group. To test asymmetric response, Column 2 separately estimates workers' response to rank-distance with friends they outrank and those they do not. Reassuringly, Columns 1 and 2 produce similar results with average rank-distance as with distance to median rank among friends.

In Table A3 I re-estimate the key specifications from Tables 1 and 2 but clustering

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

[36] 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.

standard errors at the level of workers instead of blocks. Column 1 shows the estimates of average treatment effects from Table 1. Column 2 tests how workers, in both Public and Private Treatments, behave in response to competing with at least one friend, when compared to similar workers in Control arm. Column 3 estimates a similar specification but with a direct comparison of Public and Private Treatments. Finally, Columns 4-5 test how publicly-ranked workers respond to distance to median rank among their friends. All the results remain robust and yield similar statistical precision when standard errors are clustered at worker-level.

Figure A1: Sweater Knitting at Factory

Note: The figure on the left shows a worker operating a manual knitting machine to produce a sweater part. The figure on the right shows a knitted sweater part on a knitting machine.

Table A1: Key Descriptive Statistics

	(1) n	(2) Control Mean	(3) Private Mean	(4) Public Mean	(5) (2)-(3)	(6) (2)-(4)	(7) (3)-(4)
Production							
Production Time Per Sweater (Jan'16) (mins)	1688	84.88	85.19	82.76	-0.31	2.12	2.43
Monthly Production Earnings (Jan'15-Jan'16) (BDT)	4758	11042.93	11038.57	10996.9	4.36	46.03	41.67
Monthly Attendance Days (Jan'15-Jan'16) (BDT)	4758	28.38	28.34	28.35	0.04	0.03	-0.01
Number of Sweaters Produced in a Month (Jan'15-Jan'16)	4755	387.51	394.43	358.88	-6.92	28.63	35.55
Demographic Characteristics							
Years Attended School	362	7.84	7.56	7.38	0.28	0.46	0.18
Age (years)	352	30.61	30.44	30.9	0.17	-0.29	-0.46
Tenure (years)	366	5.32	5.47	5.27	-0.15	0.05	0.2
1(Competitive)	363	0.45	0.51	0.41	-0.06	0.04	0.1
Risk Scale	363	5.54	5.5	5.72	0.04	-0.18	-0.22
Social Network							
Number of Workers in Block	363	23.55	23.61	23.63	-0.06	-0.08	-0.02
Number of Friends	363	11.46	10.53	12.32	0.93	-0.86	-1.79**
Productivity of Friends in Same Treatment vs. Outside	315	1	1.01	0.99	-0.01	0.01	0.02

Note: The table reports key descriptive statistics for each experimental group. The last two columns report the differences in mean between control and the treatment groups. *Production Time Per Sweater at Baseline (mins)* is the average length of time a worker needs to knit one sweater in a job (a batch of sweaters). This is unconditional of style and sizes of the sweaters. *Production Time Per Sweater at Baseline (mins)* is monthly production earnings of a worker averaged over January-December 2015. *, **, and *** indicate that when the difference is tested against a null of zero, the null is rejected at 10%, 5% and 1% significance level respectively.

Table A2: Alternative Measurements & Heterogeneity

	(1) Ln(Time)	(2) Ln(Time)	(3) Ln(Time) n(Friends) ≤Median	(4) Ln(Time) n(Friends) >Median
Public	0.0122	-0.0127	-0.0056	0.0004
	(0.0091)	(0.0206)	(0.0144)	(0.0180)
Public * Mean Rank-Distance with Friends	0.0058**			
	(0.0025)			
Public * Mean Rank-Distance with Outranked Friends		0.0112**		
		(0.0048)		
Public * Mean Rank-Distance with Outranking Friends		-0.0008		
		(0.0037)		
Public * Distance to Median Rank Among Friends from Above			0.0099***	0.0094
			(0.0037)	(0.0089)
Observations	14,950	14,950	7,678	7,272
Adjusted R-squared	0.2934	0.2937	0.2980	0.2881
Outranked = Outranking		0.00700		
Baseline Productivity	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Counterfactual	Private	Private	Private	Private

Note: *Ln(Time)* is logarithmic transformation of average time (minutes) a worker takes to produce a sweater in a given batch. *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 provided in the first treated month. *Outranked Friends* refer to friends who were ranked lower than a worker in the first treated month; *Outranking Friends* were friends who were ranked higher. *Distance to Median Rank Among Friends from Above* is rank-distance to the median rank among a worker's friends when the worker is ranked higher than the median rank. *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. *n(Friends)* is the number of friends who were randomized into the same experimental group. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A3: Alternative Clustering of Standard Errors

	(1) Ln(Time)	(2) Ln(Time)	(3) Ln(Time)	(4) Ln(Time)	(5) Ln(Time)
Public	0.0181*	-0.0459*	-0.0208	-0.0040	-0.0008
	(0.0108)	(0.0258)	(0.0229)	(0.0170)	(0.0134)
Public * 1(Competes with Friends)		0.0728***	0.0376*		
		(0.0281)	(0.0227)		
Private	0.0055	-0.0248			
	(0.0111)	(0.0182)			
Private * 1(Competes with Friends)		0.0359			
		(0.0220)			
Public * Distance to Median Rank Among Friends from Above				0.0103**	0.0099**
				(0.0052)	(0.0047)
Public * Distance to Median Rank Among Friends from Below				0.0005	
				(0.0057)	
Observations	22,870	22,870	14,950	14,950	14,950
Adjusted R-squared	0.2972	0.2975	0.2931	0.2935	0.2933
Public + Interaction = 0		[0.021]	[0.141]		
Private + Interaction = 0		[0.372]			
Baseline Productivity	Yes	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Counterfactual	Control	Control	Private	Private	Private

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to produce a sweater in an assigned job. *Public* and *Private* refer to Public and Private Rank Treatments respectively. *1(Competes with Friends)* is a dummy variable that is equal to 1 if a worker has at least one friend randomized in the same experimental group. *1(Ranked Above Median Among Friends)* is a dummy variable that is equal to 1 if a worker is ranked above the median of ranks among his friends. *Distance to Median Rank Among Friends from Above* is the rank-distance to this median rank if a worker is ranked higher than the median rank, while *Distance to Median Rank Among Friends from Below* is the rank-distance to median rank if a worker is ranked lower. *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. Standard Errors are clustered at worker 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 A4: Spillover Effect

	(1) Ln(Time)	(2) Ln(Time) Prvt. Inten.	(3) Ln(Time) Pub. Inten.
Private	0.0039	0.0058	0.0254
	(0.0191)	(0.0206)	(0.0256)
Private * 1(Public Intensive Block)	0.0115		
	(0.0257)		
1(Public Intensive Block)	0.0107		
	(0.0162)		
Private * Distance to Median Rank Among Friends from Above		-0.0019	-0.0022
		(0.0072)	(0.0086)
Observations	15,217	8,239	6,978
Adjusted R-squared	0.2922	0.3081	0.2721
Baseline Productivity	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Counterfactual	Control	Control	Control

Note: *Ln(Time)* is logarithmic transformation of average time (minutes) a worker takes to produce a sweater in a given batch. *Private* refer to Private Rank treatment. *Pvt. Int.* refers to Private Rank Treatment intensive blocks. *Pub. Int.* refers to Public Rank Treatment intensive blocks. *Distance to Median Rank Among Friends from Above* is the rank-distance to this median rank if a worker is ranked higher than the median rank. *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. Standard Errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.