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Abstract

Performance ranks introduce a trade-off for workers. They have to choose between signaling high productivity or signaling social compatibility to peers. Using a long-term experiment at a sweater factory, this paper disentangles the incentives underlying performance ranks. Treated workers receive either private or public ranks. In response, intrinsic incentives from private ranks do not affect productivity. But publicly-ranked workers reduce productivity to conform to their social groups in the workplace. Additionally, cooperation decreases among the workers, although with limited effect on productivity. The paper shows how inducing competition among workers may be counterproductive for firms.

JEL-Codes: D230, J530, O150.

Keywords: ranks, social conformity, cooperation.

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1 Introduction

Performance ranking is one of the few incentive designs that seek to exploit workers' social and relative preferences to increase their 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 their social and relative preferences (Besley and Ghatak (2008); Ager et al. (2021)).[1] Empirical evidence from providing ranks, however, is mixed, possibly due to contrasting mechanisms. Ranks can increase productivity by inducing workers to increase effort for social recognition or status. Conversely, a ranking contest can hurt firm productivity by reducing cooperation among the workers and potentially altering the relational contracts between the workers. Alternatively, the workers may worry about being perceived as self-serving and getting socially ostracized if they outperform their peers. They may choose to signal compatibility to their social group in the workplace instead, and reduce effort to conform to group productivity (Bernheim (1994); Akerlof and Kranton (2000)). Understanding these trade-offs can help to unravel the complexities underlying workers' social preferences.

This paper 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 these mechanisms poses several empirical challenges. First, we need a precise measure of individual performance. Second, the context of study should be conducive to social incentives present at a typical workplace. Third, we need to isolate the effect of signaling incentives from that of private utility from ranks. Fourth, we need to distinguish between responses to the two opposite signaling incentives. And finally, we must collect data on cooperation among workers to understand how cooperation may change and affect productivity.

The experiment provides monthly performance ranks to 366 workers at the garment factory for five months. The workers individually knit batches of 12-48 sweaters (a *job*) and are paid based on piece rates. The individual-production process and availability of a high-frequency job-level measure of sweater knitting time solves the first empirical challenge. Reflective of a typical workplace, the knitting workers had worked for five years on average at the factory at baseline and report strong social ties with peers. The setting, therefore, lends a real-world social network to the ex-

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

periment and solves the second challenge. The paper addresses the third and fourth challenges through experimental design. Treated workers in the experiment receive performance ranks, based on production in previous month, 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 additional information about ranks of all the workers in Public Treatment.[2] The first treatment introduces intrinsic utility from ranks and 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, *Control*, does not rank workers and helps to estimate the net effect of each treatment. The workers are individually randomized into these arms. A social-network map from baseline helps to distinguish between 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, data on cooperation collected from the production floor measures changes in cooperation among the workers.

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 from the ranks do not affect productivity but signaling motivations do. The direction of the effect suggests that negative signaling incentives from the ranks outweigh the desire to signal high productivity. It is useful to note that, in comparison, the literature usually finds that a 10% increase in piece rates increases productivity by 0.3% to 0.8% (e.g. Tonin and Vlassopoulos (2015); DellaVigna and Pope (2018); Goerg et al. (2019)).[3]

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 friends (weakly) increase productivity instead. Three pieces of evidence suggest that the workers reduce productivity to signal social compatibility to their friends. First, the productivity drop is driven by workers who rank higher than the median among their friends as revealed by the first treatment letter (based on productivity at baseline). Moreover, the magnitude of this drop increases as a worker is ranked farther

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

[3]These estimates, however, relate to different kinds of tasks (e.g. slider task) in different contexts. Unfortunately, it is difficult to derive a viable estimate of the effect of monetary incentives with the data for the sweater factory.

and farther above the median rank. In contrast, workers ranked below the median do not alter their effort choice. This suggests some form of stigmatization of effort among publicly-ranked workers. The estimates are derived by comparing privately- and publicly-ranked workers with similar relative ranks among friends; hence, the comparison exploits only the experimental variation in visibility of ranks and holds constant underlying worker characteristics correlated with the relative ranks. A second test compares privately-ranked workers with Control and verifies that there is no productivity decrease when the ranks are private. This suggests that the reduction in effort is triggered by visibility of ranks which in turn triggers 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 signal is aimed at their social groups. A series of robustness tests verify that the productivity drop is not driven by endogenous worker characteristics (including the workers' social popularity and overall productivity) and contamination effects, and is robust to alternative measurements of relative productivity.

Third, novel data from the production floor reveals that the ranking contest decreases the workers' effort in a second dimension - cooperation with peers. In order to measure cooperation, the workers are randomly assigned into 20-minute *observation* slots during which they are monitored from a distance and their interactions with peers are recorded. These observations span various days before and after the ranking begins. A before-and-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. Additional evidences suggest that the fall in cooperation is driven, at least in part if not full, by the ranking contest. First, compared to Control workers, the likelihood of engaging in an act of cooperation during the intervention months is weakly lower for privately-ranked workers and strongly lower for publicly-ranked workers, for whom the competition is the most salient. Reassuringly, there exists no such differences across the workers before the intervention. Second, in comparison with the pre-intervention months, publicly-ranked workers are relatively less likely to interact with fellow publicly-ranked peers than with other workers during the intervention. Finally, the share of cooperation that involves help harder to refuse does not decrease

(e.g. lending a design chart, help with machine or sweater measurement), but the share of voluntary help in the form of offering production tips or suggestions falls during the intervention. However, the reduced cooperation leads to only weakly lower productivity for workers during the intervention. Importantly, this is independent of the productivity drop we earlier interpreted as signal of social compatibility.

Finally, several tests of mechanism shed light on the nature of the social-compatibility signal. There could be two related but subtly different reasons behind the productivity drop. First, the workers may be wary of being perceived as distancing themselves from their social group in productivity; they may fear that they would lose membership of their group. They may, therefore, reduce effort to conform to the group's productivity level. Second, as the workers' effort in the 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 their friends.[4] Notice that the externality of a worker's effort on his friends is the most salient when his friends are ranked close to him; marginal changes in his effort can affect his friends' ranks. Conversely, a worker's group identity is particularly at risk when he outranks his group by a substantial distance. To distinguish between the two mechanisms I exploit pair-wise rank-differences between a worker and his friends to test how a worker responds when more of his friends are ranked closer or farther away.

The results are consistent with the workers reducing effort to conform to their group productivity. To be specific, the evidences suggest 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 in between them by lowering their own ranks. Additional tests rule out that the productivity drops because workers are complacent after learning new information about their friends' productivity from the public ranks. The results are also inconsistent with alternative explanations such as ratchet effect, inequity aversion, and altruism.

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

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

been inconclusive. Delfgaauw et al. (2013), Ashraf et al. (2014b), Song et al. (2017), Ager et al. (2021) and Englmaier et al. (2021) find positive effects, while Ashraf et al. (2014a) and Blader et al. (2020) find negative effects. Understanding the puzzle of mixed evidence from public ranks becomes further complicated by the finding that private ranks also generate mixed effects. Blanes-I-Vidal and Nossol (2011) and Gill et al. (2019) find positive effects while Barankay (2011, 2012) find negative effects, indicating that ranks may generate ambiguous effects from underlying intrinsic incentives alone. Using an experiment to disentangle intrinsic and social incentives, this paper shows that status seeking incentives from public performance ranks may be counteracted by workers' social preference for conformity to their social groups in the workplace. This resonates findings in Blader et al. (2020) who show 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 the effect of incentives on cooperation among workers. Empirical studies that explore such cooperation 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 regular workplace-settings have been scarce as it is extremely difficult to measure cooperation in such workplaces. This paper adds to this literature 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 the workers.

More broadly, the paper contributes to the literature that studies how workers' social preferences affect their 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 a *relative pay scheme* to internalize the negative externality of their effort on their peers' earnings. Using relative

[5]A booming literature on social preferences of workers has already documented various forms of preferences. For example, workers exhibit warm glow at work (DellaVigna et al. (forthcoming)), enjoy working with friends (Bandiera et al. (2010)), internalize externality of their actions on 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 (Casaburi and Macchiavello (2015), Akerlof et al. (2021)). A related stream of work studies relative incentives among workers by exploring 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)).

performance ranking, my paper complements Bandiera et al. (2005) in showing how pitting workers against each other may be counterproductive because of their social preferences. Yet, there are two key differences between the two papers. First, the relative incentive scheme in Bandiera et al. (2005) involves pay and studies the trade-off between monetary incentives and social incentives. In my paper, on the other hand, the trade-off is between two social incentives. In the process, this paper sheds light on how workers exhibit multiple kinds of social preferences in the same setting (DellaVigna and Pope (2018); DellaVigna et al. (forthcoming)) and how such preferences may interact. Second, workers in Bandiera et al. (2005) reduce effort to internalize the negative externality of their effort on peers. On the other hand, the underlying mechanism for the productivity drop in my paper is the 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 real workplaces is surprisingly scarce. This paper shows how social concerns can lead even experienced workers to undercut performance and earnings to conform to group productivity.[6] Understanding this can have important implications for organization of workers inside firms.

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. Section 6 investigates the effect of the intervention on cooperation among workers. Section 7 tests in detail the underlying mechanism. Finally, 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

[6]These workers are 21-46 years old with tenure at the factory between one and 11 years.

whom are part of the experiment.

At any point in time, a knitting worker is assigned to knit a batch of sweaters, a *job*, which typically constitutes of 12-48 pieces of sweaters of a particular style and size. 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.[7] 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 belong to one of 15 administrative groups called *blocks*, and are 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*). At baseline, the workers had an average tenure at the factory of about five years, which could possibly explain a high number of social ties.

Third, the workers exhibit relative preferences with respect to productivity. Again, the baseline survey helps to illustrate this point. By individually naming the coworkers in their block, the survey asks if the workers compare their own productivity to that of the coworker's.[8] 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.[9] This suggests that performance ranks can provide the workers with new information that they care about.

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

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

[9] This is calculated from a different question. The comparisons are based on production earnings in the last three months.

3 The Experiment

In partnership with the management of the factory, I conduct a randomized experiment at the knitting department described in the previous section. Through the experiment, the knitting workers receive performance feedback in the form of performance ranks. This section discusses the key incentives underlying such ranks and the experimental design that disentangles them.

3.1 Incentives from Ranks

Before describing the experimental design it is useful to first discuss the key incentives that can be triggered by performance ranks. At its core, performance ranks distinguish more productive workers from less productive ones. Therefore, 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)).[10] For instance, higher-ranked workers may earn greater respect from coworkers and even enjoy preferential treatments from the employer. The higher return to effort from status rewards can induce workers to increase effort to signal high productivity.[11]

However, performing well in ranks may invite social sanctions from peers. Workers may be especially concerned about sanctions from their social group (*friends*) as the social capital in such relations is high. They may worry that their group will ostracize them if they are perceived as eager to distinguish themselves from the group in productivity. Keen to retain group identity, they may reduce effort and conform to the group's productivity level. Moreover, in a ranking contest, a worker's effort imposes a negative externality on his peers by suppressing their ranks while improving the worker's own ranks. 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.[12]

[10] Empirical support for this idea can be found in Ashraf et al. (2014b) and Ager et al. (2021) who find positive productivity effects from public ranks.

[11] Workers may also want to signal high productivity to the firm because of career concerns (Holmstrom (1999)). This paper focuses on signaling for status, as it is unlikely that the workers in this setting would be driven by career concerns. 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.

[12] Conformism driven by group identity has been modeled in, for example, Bernheim (1994) and Akerlof (1997). Empirical support can be found in Bursztyn et al. (2019)). On the other hand, Bandiera et al. (2005) and Mas and Moretti (2009) show how workers internalize externality of their

In summary, public ranks introduce a trade-off of social signals for the workers. The trade-off is between signaling high productivity to peers and the firm, or signaling social compatibility to their social group. However, 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 also find strong effects on worker productivity.[13]

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.[14]

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 in previous month. This includes the total of number of sweaters the worker produced, a break-down by styles, total production time in the month, and a list of all the workers who receive similar feedback. The 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 Treatment*.[15] The third feedback contains the same information but the workers who receive this feedback can see the ranks of their peers beside the list that shows all the workers who receive this feedback. In other words, the workers in this *Public Treatment* learn 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.

Importantly, the experimental design makes a distinction between intrinsic and social incentives from ranks. Private Treatment aims to trigger intrinsic incentives

actions on peers.

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

[14] The factory management wished to continue using 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.

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

from ranks by providing them in private. Public Treatment additionally introduces signaling motivations by making the ranks publicly visible. The difference between Public and Private Treatments estimates the net effect of signaling motivations alone. The Control helps to estimate the total effect from each of these treatments.[16]

In order to distinguish between the two signaling incentives underlying public ranks, a baseline survey records details of the workers' social network in the workplace. It contains information about the frequency of interactions between a pair of workers during their stay at the factory, and whether they socialize outside the factory. It helps to distinguish between the workers who compete with members of their social group and those who do not. Any motivation to signal social compatibility will be stronger among the former group of workers than in the latter.

3.3 Randomization

Workers are individually assigned into one of the three experimental arms through a public lottery in January 2016.[17] Once assigned to a particular arm, the workers receive the same kind of feedback for the entire intervention. A particular concern about individual randomization in this context relates to contamination across the arms.[18] To estimate such effects, should they exist, I experimentally vary the intensity of the strongest treatment - public ranks - across the blocks. Prior to random assignment of workers into experimental arms, each of the 15 administrative blocks is randomly chosen as either a Private- or a Public-Intensive block. Control consists of a third of the workers in each block. In Public-Intensive blocks, Public Treatment contains two-thirds of the remaining workers while Private Treatment contains the rest. In Private-Intensive blocks, the weights are reversed. This leads to an almost 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 from Public-Intensive blocks and 169 from Private-Intensive blocks.[19]

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

[17] 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.

[18] An alternative would have been cluster-randomization at the block level. But the availability of only 15 blocks in total limits the benefits from such a design.

[19] By the end of the study period, 16 workers leave the factory. With five of them from Control, four from Private, and seven from Public Treatment the numbers are comparable across the arms.

3.4 Data

The paper uses three sources of data as described below.

(i) Administrative Records: Administrative records from the factory provide demographic and production related information for all the knitting workers in the experiment. This includes 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).[20] This also includes data on monthly production earnings, monthly attendance records, and piece rates of sweaters starting from January 2015.

(ii) Baseline Survey: A baseline survey in October 2015 collects information on the workers' social network in the factory, including information on the peers a worker interacts with inside the factory, how frequently they interact, and who they socialize with outside the factory. The survey also asks the workers about who they compare their productivity with, how do they compare, etc. In addition, the survey measures the workers' attitudes towards competition and risk.[21]

(iii) Cooperation Data: The research team collect first-hand data on cooperation among workers by physically observing them while they work. This data is discussed in Section 6.

Before moving on to the main analysis, Table A1 tests if the knitting workers in the three experimental arms are balanced in observable characteristics at baseline. The Table tests three sets of characteristics - productivity (the key outcome of interest), demographics, and social network. Columns 2-4 report the means of these characteristics for the three arms. Columns 5-7 report p-values from the tests of equality of these means between the different combinations of the 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

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

[21] Following the practice in the literature, attitude towards competition is measured through a game in which workers throw balls into a basket (see Gneezy et al. (2009) for an example). Before throwing the balls, the workers choose how 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). Risk is measured as a self-reported assessment of a worker's risk taking behavior on a scale of 1 to 10.

preferred measure of productivity is average knitting time of a sweater, which is simply the total time a worker takes to complete all the sweaters in a given job divided by the number of sweaters in that job. However, since 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 test alternative productivity measures that are available for a longer period of time, since January 2015 to be precise, but only at a monthly interval. These include monthly production earnings, attendance, and total number of sweaters produced. Reassuringly, the workers in the three experimental arms are balanced on all the productivity measures. 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 the workers' social network along several dimensions. The first measure, the number of coworkers they work beside in their blocks, is similar across the experimental arms. A more direct measure, the number of reported friends at baseline is also similar between control and each of the treatment arms. It is, however, slightly higher in Public Treatment than in Private Treatment. Importantly, though, joint-significance tests cannot reject that the variables do not jointly predict treatment status. Finally, the ratio of productivity of friends in the same arm as a worker and of those who are not is around one in magnitude in all the arms. This confirms that the friends randomized into the same arm as the worker are representative of his social network in terms of productivity.[22]

4 Productivity Response To Ranks

I begin the main analysis by estimating the average treatment effects of the ranking on worker productivity. The results 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, intrinsic incentives from the ranks do not affect worker productivity but signaling incentives from the public ranks lead to net reduction in worker productivity. Subsequent evidences suggest that it is the incentive to signal social compatibility to social groups that drives this drop in productivity. Multiple tests verify the robustness of these findings.

[22]By construction, it is restricted to workers with friends both inside and outside their arm.

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. This is measured 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. Indicator variables $Public_i$ 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 key interest, as percentage changes in productivity. A positive coefficient indicates lower productivity.

To increase power, I control for worker i's long-term productivity α_i.[23] Because data on knitting time is not available for more than a month 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 the block level and bootstrap them as there are only 15 blocks in the factory.[24]

Column 1 of Table 1 estimates Equation 1 while controlling for only the workers' long-term productivity and style-and-size fixed effects. The estimates reveal that the private ranks do not affect the workers' productivity on average, which in other words implies that intrinsic incentives from the 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' sweater-specific experience measured by the cumulative number of jobs in which the workers worked on the same style and size. Column 4 controls for additional worker characteristics

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

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

in the form of tenure at the factory (at 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 A1 plots how the distributions of worker productivity changes because of the intervention. Note that the knitting times of sweaters vary by their styles and sizes. Hence, any difference in the average knitting time of sweaters between two workers can be driven by differences in their productivity as well as differences in the sweaters' styles and sizes. To account for the sweater characteristics, Figure A1 plots the residuals from a regression of job-level sweater knitting times on style-and-size fixed effects.[25] The left panel shows that the distributions of the average knitting time, 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 during the intervention. Moreover, it increases all along the productivity distribution. Mirroring earlier results, the distribution for privately-ranked workers remains similar to that for Control workers.

Strong effects from public ranks and null effect from private ranks imply that the workers are driven by signaling incentives from the ranks more strongly 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 marginally insignificant (average p-value$\approx$0.15 across the four specifications in Table 1). This suggests that negative signaling incentives from the ranks are stronger than any positive signaling incentives triggered by the ranks. The following Section disentangles these incentives.

4.2 Disentangling Signaling Motivations from Ranks

Recall that public ranks trigger two conflicting signaling incentives, one to signal high productivity, and another to signal social compatibility to friends. I conduct several empirical tests in this Section to disentangle the two. The compatibility signals from the workers are meant to reassure their friends that they do not actively pursue outranking them. Thus, the incentive to signal social compatibility is likely to be the strongest precisely for the workers who directly compete with friends for

[25]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 both style-and-size fixed effects and job sequence yields identical distributions.

ranks. Therefore, I estimate productivity responses of the publicly-ranked workers by distinguishing between those who compete with friends 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 friends increase productivity by more than 4.5 percent compared to similar workers in Control. The results are reported in Column 1 of Table 2. Compared to these workers, publicly-ranked workers who 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 the workers in Control who would have also competed with friends had they been ranked. It suggests a 2.7 percent decrease in productivity among the publicly-ranked workers who compete with friends (p-value$<$0.02). Note that this comparison holds constant the underlying worker characteristics correlated with the likelihood of competing with friends. Nonetheless, Column 2 additionally controls for the number of friends a worker reports at baseline, which is positively correlated with the likelihood. Higher the number of friends a worker has, the higher is the likelihood that at least one of them is randomized into the same experimental arm. As expected, this affects the estimates for Control but, the differences in productivity between Public Treatment and Control remain unaffected.

In order to estimate productivity changes from signaling motivations alone it is necessary to compare Public Treatment to Private Treatment instead, and account for the productivity effects from intrinsic incentives underlying the ranks. The estimates from this comparison, in Column 3, reflect the productivity change caused by only the experimental increase in visibility of the workers' signals.[26] 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 the other workers (who do not compete with friends) to increase productivity by two percent, although this estimate is imprecisely estimated.

Although the previous results are consistent with the workers reducing productivity because of competition with their friends, it is not clear if such reduction is really driven by their productivity differences. If the publicly-ranked workers averse to outranking their friends, we expect to observe productivity drop only among the workers who rank better than their friends at baseline. Therefore, Column 4 tests how the

[26]The counter-factual arms are always indicated at the bottom of the corresponding columns.

workers respond when they rank better or worse than their friends in the very first treatment letter, reflected as ranking higher or lower than the median rank among competing friends. Indeed, publicly-ranked workers reduce productivity mostly when they rank higher than their friends at baseline, by about two percent on average.

The next specification takes a step farther and estimates the slope of productivity change with change in the distance to the median rank among friends. The results, in Column 5, show that publicly-ranked workers monotonically reduce productivity when they rank better than the median and the distance to the median increases. The distance is measured in units of 10 ranks. With an increase in distance by 10 ranks above the median, the workers reduce productivity by one percent. A potential concern here is that the workers' baseline productivity may be correlated with their distance to the median rank. Although this is partly true, there is a substantial variation in the workers' rank-distance with friends conditional on their own productivity. Nevertheless, any correlation between baseline productivity and rank-distance with friends is captured by the corresponding coefficient for Private Treatment, and the differential response between Private and Public Treatments (as captured by the interaction) should be unaffected by this correlation. Indeed, when we drop baseline productivity as a control in Column 6 we get a similar estimate. Because we do not control for baseline productivity, there is a slight increase in the standard errors but the estimate remains statistically significant.

Importantly, and in sharp contrast, distance to the median does not alter a workers' productivity when he ranks lower than the median. This is seen in Column 7. The contrast in response to rank-distance above and below the median, statistically significant at 5% significance level, suggests that effort might have become stigmatized for workers who rank high within their social groups.

The results so far suggest that the publicly-ranked workers dislike outranking friends but are indifferent about being ranked lower than them. This outweighs any positive signaling incentives underlying the public ranks. The next section explores if the workers' reduced effort is indeed a signal of social compatibility to their friends.

5 Robustness Tests of Identification

This Section presents a number of robustness tests for the previous results. In particular, a series of placebo tests provide support to the interpretation that the workers in

Public Treatment reduce productivity because of the public visibility of their ranks, and as a signal to their social group. In addition, several tests rule out concerns that the results are driven by underlying characteristics of the workers that are correlated with how we distinguish between them in the previous section. Section A.2 in the Appendix probes into possible contamination effects across the experimental arms and their effect on the main results. Section A.3 tests alternative measures of relative productivity and clustering of errors.

5.1 Placebo Tests

The first placebo test establishes that the reduction in productivity among publicly-ranked workers is triggered by the intervention. To do so, Column 1 of Table 3 tests whether publicly-ranked workers respond to rank-distance with friends any differently than privately-ranked workers, before the first treatment letter is delivered. The test uses production data from January 2016 and confirms that there is no such difference. A caveat is that the ranks used to calculate the rank-distances are computed using production data from January 2016. However, as before, this mechanical correlation should not affect the difference between Public and Private Treatments. Nonetheless, similar tests using monthly production earnings from 2015 yield similar results. The results are not reported for brevity.

The second test shows that it is specifically the visibility of the ranks that triggers the productivity drop. To interpret the productivity change as signals of social compatibility to peers it is necessary that it does not take place when the peers do not observe the workers' ranks. Therefore, Column 2 compares the two Treatment arms against Control. Productivity among publicly-ranked workers show similar reductions as before when compared to Control 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 in Public Treatment is triggered by visibility of the ranks and is meant as a signal to peers.

The next test suggests that the signal is targeted at the workers' friends. To do so, Column 3 tests the workers' response to rank-distance with peers who the workers are not friends with. The comparison once again is between Private and Public Treatments. The interaction term in Column 3 is close to zero indicating that the workers are not sensitive to rank-distance with peers they are not friends

with. Column 4 re-introduces rank-distance with 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.

Column 5, on the other hand, tests how the workers respond to rank-distance with peers they are not friends with but frequently talks to during work. The workers do not care about the rank-distance with this group of peers either, but respond strongly to that with friends. This indicates that the productivity response is not motivated by mere interactions with their peers. Instead, the estimates in Columns 3-6 suggest that there is something unique about the group of peers a worker socializes with, which makes him sensitive to outranking them. Plausibly, the high amount of social capital in the relationships with those peers could make him wary of upsetting them.

The results from the tests in this Section suggest that the productivity drop among publicly-ranked workers is a signal triggered by the public nature of the ranking and the signal is aimed at their friends. Taking a step back, the next Section tests whether this could be driven by endogenous worker characteristics instead.

5.2 Endogenous Worker Characteristics

The workers in Public Treatment who rank higher among their friends at baseline reduce productivity relative to those who rank lower, and importantly, relative to similar workers in Private Treatment. This heterogeneous treatment effect, consistent with signaling social compatibility, could instead be driven by differences in the endogenous characteristics that are correlated with whether a worker ranks higher or lower among his friends. We would be particularly concerned about the characteristics that are correlated with the likelihoods of him competing with friends, and conditional on competing, ranking higher than them.

To assuage such concerns, I first test if the baseline likelihood that a worker competes with a friend during the intervention drives any of the key results. Recall that this likelihood is positively correlated with the number of friends a worker reports at baseline. Higher the number of friends, the more likely it is that the workers find at least one of them in their own experimental arm.

Column 1 of Table 4, therefore, re-estimates how the workers who compete with friends and those who do not differ in their responses to public ranking, but now lets the number of friends affect privately- and publicly-ranked workers differently. The

original estimates remain robust. If anything, it is now larger in magnitude. Next, Column 2 uses the same controls to re-examine how the workers respond to rank-distance with their friends. Again, the estimates are similar to those in Table 2. As an alternative test, Column 3 restricts the sample to workers with at least one friend in their experimental arm and re-estimates the response to the rank-distance. The estimate remains similar to that in the full sample and reassures us that the slope of productivity change in response to rank-distance with friends is valid within the set of workers who compete with friends, and not driven by productivity response of the workers who do not. More generally, Columns 1-3 confirm that the heterogeneous productivity responses is not driven by endogenous selection of workers into who competes with friends and who do not.

Next, I test if the productivity drop is driven by high-productive workers irrespective of their relative ranks in their social group. Response to high rank-distance with friends could simply reflect response by workers with high productivity at baseline. Recall that Columns 5-6 of Table 3 find that the workers do not respond to rank-distance with peers they are not friends with. This already suggests that the workers' baseline productivity matters only so far as it determines their relative ranks within their social network, and not otherwise. Nonetheless, Column 4 lets the workers' baseline productivity affect privately- and publicly-ranked workers differently. The association between productivity and rank-distance with friends remains robust. Column 5 controls for both baseline productivity and social popularity of the workers in the same specification. The estimates, again, remain robust.

For completeness, Column 6 adds additional controls for observable worker characteristics and their interactions with treatment status. These include controls for tenure, education, and attitudes towards competition and risk.[27] Although slightly dented, the coefficient of interest remains high and statistically significant. Finally, Column 7 tests whether the estimates are robust to controlling for treatment-intensity at the block-level, by controlling for whether a block had relatively more publicly-ranked workers than privately-ranked workers. Our key estimate remains robust.

Table 4 verifies that the productivity drop we estimated for publicly-ranked workers in earlier specifications is not driven by underlying worker characteristics correlated with how we distinguish between the workers. Our main estimate remains robust to controlling for an array of observable worker characteristics. Additional

[27] The specification also includes a control for job sequence.

robustness tests are reported in the Appendix. Section A.2 examines and rules out that contamination across experimental arms are a threat to our results. We would be particularly worried about privately-ranked workers imitating publicly-ranked workers and sharing information of their ranks among themselves. Section A.2 assuages such concerns. Section A.3 tests alternative measurements of relative ranks and finds qualitatively similar results. It also confirms the robustness of statistical precision of our key estimates to clustering of errors at worker-level instead of block-level.

Section 7 examines the mechanism behind these productivity changes. But first, we explore how the ranking contest alters the workers' effort in a second dimension.

6 Cooperation Among Workers

We now turn to understanding how the ranking contest may have affected a second aspect of the workers' effort - cooperation with peers. It is possible that the overarching environment of competition from the contest discourages workers from helping their coworkers and even changes the norm related to cooperation in the workplace. This may have direct effect on the overall productivity of the firm.

In order to investigate how cooperation among the workers may change because of the ranking contest, the research team collected primary data on such cooperation from the production floor. This information was collected both prior to the intervention (December, 2015 - January, 2016), and during the intervention (February-June, 2016). Workers were observed for 20 minutes at a time, in 20-minutes-long time slots (*observation set*). Each slot was assigned to a randomly chosen worker (*focal worker*) and three workers surrounding the focal worker. This maximized the number of workers who can be observed during a given slot. During this time, any interaction that involved an act of cooperation between the workers and their peers within or outside the observation set was recorded with details.

The exercise produced 480 unique observation sets containing 1,500 observations of corresponding workers. Table A5 reports descriptive statistics about these observations. The total number of observations at individual- and focal-worker levels, and the number of unique workers observed, are split almost equally between the three experimental arms, both before and after the ranking begins.[28] The Table also tests

[28]The numbers of observations for focal workers are less than that of observation sets as the focal workers can sometimes be absent on the day of the planned sets. The set would, however, be

balance of the worker characteristics, but for only the 274 unique workers observed before the ranking begins.[29] That the differences in means of these characteristics are zero cannot be rejected for all but two (of 36) mean comparisons. But the characteristics are jointly insignificant in predicting treatment status.[30]

6.1 Changes in Cooperation Because of Ranking

I begin by documenting how cooperation changes after ranking is introduced among the knitting workers. Cooperation, an interaction where one worker helps another and referred to as an *interaction* henceforth, involves more than one worker by definition. As a result, it can span across experimental arms (e.g. a publicly-ranked worker helps a privately-ranked worker) and changes in behavior in one arm can affect that in another. Therefore, I first examine the overall cooperative behavior on the floor before analyzing it by arms.

The top panel in Figure 1 reports the frequency of the workers interacting with a peer before and after the ranking begins. The top-left panel shows that the average likelihood of observing at least one interaction during an observation set declines drastically from 45% in the months before the ranking to less than 20% in the months after (p-value<0.01). The next panel exploits information at the worker level instead and estimates the likelihood that any observed worker (including both focal and surrounding workers) engages in an interaction during an observation set. The workers can be either receiving help or providing help. Although the likelihoods are lower in magnitudes in this panel (19% and 8% in months before and after ranking respectively) it reveals a similar decline in cooperation on the floor during the intervention.[31] As the estimates in the second panel can double count an interaction if it takes place between two workers in the same set, the third panel estimates the likelihood of engaging in cooperation counting only the focal workers. The estimates remain robust in magnitude and statistical precision.

recorded with workers surrounding the station where the focal worker would have been located.

[29]There was not sufficient time for more observations before the intervention started.

[30]Knitting time per sweater appears to be slightly lower among publicly-ranked workers than among Control workers in this subset of knitting workers but their production earnings are similar. Therefore, more than productivity differences this perhaps reflects the characteristics of the sweaters they knit in January 2016. Besides, in subsequent across-arm comparisons of cooperation and productivity, we compare Public Treatment to Private Treatment. There is no difference in the average knitting times of sweaters between the workers in these two arms.

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

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

The before-and-after comparison of cooperation addresses the issue that it can span across multiple arms. However, one concern is that cooperation on the production floor may have fallen during the intervention because of other changes with time, such as changes in work load, that are unrelated to the ranking contest.

Three pieces of additional evidence suggest that the changes in cooperation is driven by the ranking contest. First, there is a lower likelihood of cooperation among the ranked workers, especially those in Public Treatment, during the intervention. No such differences exist before the intervention. This can be seen in Figure 2 which reports the likelihoods of a worker engaging in an interaction, conditional on his treatment assignment, before and after the ranking begins. Although cooperation falls drastically for all the experimental arms during the intervention, including in Control, privately- and publicly-ranked workers are now relatively less likely to engage in cooperation than Control workers. Although the difference in the likelihoods for Control and Private Treatment is small and insignificant (p-value = 0.22), the same for Control and Public Treatment, arguably the most salient competition format, is marginally significant (p-value = 0.10). The across-arm comparisons which hold time constant is not confounded by temporal changes in factors that may be correlated with cooperation among workers. We should interpret these estimates with caution for reasons discussed earlier, but the exercise provides useful insights nonetheless.

Second, during the intervention, the share of interactions in which publicly-ranked workers interact with fellow workers in Public Treatment is lower relative to pre-intervention months. To illustrate this, consider the set of unique interactions among the workers before and after the ranking begins. The left panel in Figure 3 reports the shares of such interactions involving pairs of workers with different combinations of treatment-status. During the intervention months and compared to the pre-intervention months, the shares of interactions that involve ranked workers are either

lower or unchanged. Importantly, the share of interactions where both workers are publicly-ranked decline the most.[32]

Finally, the right panel shows the shares of cooperation by the types of help involved. A worker can help another in broadly four ways - helping with measurement of sweater parts (e.g. by holding or stretching the parts), lending tools (e.g. design chart, pencil), helping with machine settings, and sharing tips on the production process (e.g. suggestions on how to fix errors, sharing own experiences with a certain style of sweaters). During the intervention, there is an increase in the share of help that are possibly harder to refuse (help with measurement, lending, or machine), but there is a sharp decline for sharing of knowledge, a help that is relatively more voluntary in nature. In other words, the workers appear to be intrinsically less motivated to help peers after the ranking begins.

To summarize, the evidences in this Section suggest that the ranking contest adversely affects cooperation among the knitting workers. In the next section, I examine how this affects the workers' productivity during the intervention and in particular, if this drives the productivity changes we estimated in Section 4.

6.2 Cooperation and Productivity

How does the reduced cooperation among the knitting workers affect their productivity? To answer this question, I begin by characterizing each worker by their propensity to interact with their peers in the pre-intervention months (baseline). This propensity is simply the share of observations, conducted for a given worker prior to the intervention, in which he interacts with a peer at least once. It summarizes the observation-level data into one summary statistic per worker. Changes in overall cooperative behavior on the floor are likely to affect workers who had a higher propensity to interact before the intervention more than those who had a lower propensity. Because we characterize the workers by their cooperative behavior at baseline, the analysis in this Section uses the sub-sample of 274 workers for whom we have at least one observation before the intervention.

I first examine the effect of changes in cooperation on the productivity of the workers without distinguishing between their treatment assignment. Table 5 pools

[32]Because the probability of cooperation is low in general and additionally falls in the post-intervention period by a substantial margin, we have 122 actual interactions in total which is not sufficient for a rigorous parametric analysis.

all the workers together and, using production data from both pre-intervention and intervention months, examines how productivity of the workers with low and high propensity to interact at baseline evolve during the intervention. First, Column 1 shows that there is a negative (and marginally insignificant) correlation between the workers' productivity and their propensity to interact at baseline. This is captured by the coefficient for propensity to interact without the interaction term.[33] The coefficient estimates the percentage change in average knitting time per sweater if a worker increases his interactions with peers from zero to at least once every time he is observed in a 20-minutes long time slot. To be clear, this correlation can be driven by two factors - selection of workers into the interactions (for instance, low productive workers engage in interactions more often), and the direct effect of diverting time to the interactions from production (the low productive workers are low productive because they spend more time interacting instead of producing). Unfortunately, there is not sufficient power to reliably distinguish between the two.

The product of the propensity at baseline and an indicator that takes the value 1 for intervention months represents a difference-in-difference estimate. It captures the productivity changes between the pre-intervention and intervention months for workers with high propensity to interact at baseline relative to those with low propensity. The coefficient appears small and insignificant, but masks important heterogeneity.

The next test distinguishes between the help providers and the help receivers. Indeed, Column 2 reveals that most of the negative correlation between productivity and propensity to interact at baseline is driven by the help providers. The coefficient for this group of workers is not only substantially higher than the estimate in Column 1 (more than double) but also more precisely estimated. Again, this can reflect both selection and diversion of time from production to interactions. Those who receive help, on the other hand, are also relatively less productive than workers with lower propensity to interact, but the size of the coefficient is much smaller and noisily estimated. Quite intuitively, when cooperation falls during the intervention, the workers with a higher propensity to receive help at baseline suffer from productivity loss. The workers with a higher propensity to help, in contrast, improves in productivity possibly because of the time they save from helping others.

Do the changes in cooperation and associated productivity explain the productiv-

[33]Unlike before, the specification does not control for baseline productivity to ensure that the coefficients capture selection of workers into interactions with peers in terms of their productivity.

ity drop among publicly-ranked workers in Section 4? To explore this, Column 3 first provides a benchmark by re-estimating productivity responses among the sub-sample of workers for whom we have baseline cooperation data. Column 3 is also different than prior specifications because it no longer controls for baseline productivity. The estimate of productivity response to rank-distance with friends is lower in this sub-sample than in the full sample, but is nonetheless positive and economically large in size. Without baseline productivity as a control the standard errors increase too.

Finally, Column 4 controls for the propensity of a worker to interact at baseline. Our key estimate remains robust; if anything, it increases in magnitude. This suggests that while changes in cooperation does affect productivity in general, it does not drive the productivity decline correlated with rank-distance with friends.

In principle, to identify changes in productivity due to the changes in cooperation, we could exploit the fact that cooperation falls relatively more in Public Treatment than in Private Treatment or Control. Indeed, in Column 4, while the selection of workers into interactions is accounted for by the coefficient for propensity alone, the interaction between propensity and Public Treatment captures productivity changes among publicly-ranked workers because of a relatively larger drop in cooperation in Public Treatment than in Private Treatment. This estimates productivity changes from changes in cooperation independently of that associated with rank-distance with friends. The positively signed estimate again reflects a possibly negative productivity effect from reduced cooperation among publicly-ranked workers. It is difficult, however, to draw concrete conclusions from these estimates as the standard errors are very high. In unreported analysis, similar results emerge when comparing Public and Private Treatments to Control.

In summary, the ranking contest reduces cooperation among the workers with imprecisely measured subsequent negative effect on the workers' productivity. Importantly, this is an additional channel of effect on the productivity and independent of the productivity decrease associated with signaling social compatibility. The highly reduced volume of cooperation during the intervention constrains a rigorous analysis to test if the workers redistribute help from general peers to friends, which would be consistent with the workers worrying about sending a signal of social compatibility.

7 Motivations for the Social Signal

We now return to the productivity decrease in Public Treatment associated with rank-distance with friends. There are two related but subtly different reasons why the workers may be concerned about outranking friends. First, an attempt to differentiate themselves from their social group in productivity may lead to social sanctions and loss of membership of the group. Second, improving their own ranks by suppressing their friends' imposes a negative externality on the friends, which can also attract social punishment. In both cases we expect the workers to reduce effort and signal social compatibility to their friends. However, in the first, it is the desire for social conformity to group characteristics that drives the drop while in the second it is the desire to internalize externality. I distinguish between the two mechanisms below.

7.1 Conformity vs Externality

To distinguish between desires for conformity and internalizing externality on peers, I exploit variation in the composition of ranks among friends. Note that a worker imposes a negative externality on his friends when he suppresses their ranks. This is particularly salient when the friends are ranked close to the worker and marginal changes in his effort may alter their ranks. Conversely, a worker would worry about losing group membership when he outranks his friends by a substantial distance.

Therefore, Figure 4 plots productivity of publicly-ranked workers, relative to privately-ranked workers, when the friends they outperform are apart by different rank-distances. The distances are categorized into bins representing rank-distances of 0-20, 20-40, 40-60, or 60 and above. As the Figure illustrates, publicly-ranked workers do not decrease productivity when they outrank friends by narrow margins. If anything, they increase productivity with each additional friend ranked lower but within 20 rank-distances. In contrast, productivity drops substantially as a worker outranks more and more of his friends by greater than 40 ranks.

Table 6 conducts more rigorous tests. Column 1 first reports results from a similar specification underlying Figure 4 but aggregating rank-distances into two bins, 0-40 ranks or more than 40 ranks. Resonating Figure 4, having an additional friend outranked by more than 40 ranks leads to a decrease in productivity by about 2.5 percent. In contrast, having an additional friend outranked by less than that distance leads to insignificant change in a worker's productivity. Column 2 additionally con-

trols for the number of friends a worker competes with. Holding the total number of competing friends constant, Column 2 exploits only the variation in their locations in the rank-distribution around the worker. The results are similar.

Next, Column 3 examines whether the productivity reduction translates into better ranks for friends. If this reduction is driven by concerns about externality and an attempt to better the friends' ranks, we expect to see improvement in their ranks relative to the workers' during the intervention. The outcome variable now is the difference between the total number of friends a worker outranks in a treated month and that at baseline. It reflects within-worker changes in the number of outranked friends across treated months. Clearly, the productivity drop from Column 1 does not translate into better relative ranks for friends, which is inconsistent with the workers worrying about the externality of their effort.[34]

However, the reduction in productivity does translate into lower ranks for the workers and therefore lower average rank-distance with friends. Column 4 shows that for the same workers who reduce productivity in Column 1 there is a drop in ranks by more than 3 percentiles.[35] The estimates are statistically robust to multiple hypothesis testing. Moreover, Column 5 confirms that the lower ranks lead to lower average rank-distance with friends for the same workers. The outcome variable now is a worker's average rank-distance with competing friends in a treated month and measured in units of 10 ranks. Although noisy in statistical precision, the coefficient indicates a reduction in average rank-distance with friends by about two ranks.

To summarize, publicly-ranked workers reduce productivity only when they outperform their friends by substantial margins. This along with additional evidences are consistent with the workers being driven by a desire to conform to the productivity levels of their social groups rather than to internalize externality of effort on friends. I rule out a few alternative explanations for these results in the next Section.

7.2 Alternative Explanations

In this final Section I consider a few alternative explanations to the main results. Because publicly-ranked workers reduce productivity when they receive higher ranks

[34]Sharpened Q-values are reported at the end the columns to show robustness of the standard errors to multiple hypothesis testing (Anderson (2008)).

[35]Note, actual ranks can be different across arms because of differences in the total number of workers in the arms (see Table A1) and yet the rank-percentiles may be same. Therefore, rank-percentiles offer better comparability across arms.

than their friends, one possible explanation for this reduction is complacency. Complacency can result from two distinct pieces of information.

First, workers can be surprised by their *own* ranks, independent of their peers'. Because privately-ranked workers also receive ranks, a change in behavior from updated prior about own ranks should be accounted for by comparing Public and Private Treatments. However, it is possible that this updating affects the two group of workers differently. Column 1 of Table A6, therefore, controls for whether a worker underestimated his true rank at baseline (measured by the difference between his expected rank at baseline and the true rank that he receives in the first treated month) and lets this affect publicly- and privately-ranked workers differently.[36] Our main coefficient of interest, the effect of distance to median rank among friends, remains robust.

Second, workers can be surprised by learning specifically their *peers'* ranks. Indeed, Public Treatment not only makes a worker's rank publicly visible, it also informs him about his peers' ranks. Recall that the baseline survey asks workers to compare their own productivity with that of others in their block. The number of such bilateral comparisons later proved incorrect when actual ranks are provided serve as a measure of the new information that the workers learn from public ranks. Column 2 of Table A6 controls for the effect of this new information among the publicly-ranked workers. In particular, it controls for the number of friends a worker predicted to be relatively more productive at baseline but proven otherwise in the first treatment letter. This is precisely the kind of information that should trigger complacency among the workers, if at all. But the coefficient of our interest remains robust to controlling for the new information. Workers may, however, become complacent only when they rank substantially better than their friends. Therefore, Column 3 retests the specification of Column 1 of Table 6 but controlling for the new information from ranks. Again, we find that our previous estimates are unaffected.

Another possibility is altruism. Workers may feel bad for their friends and reduce effort to improve their ranks. But we expect altruism-driven reduction in effort to be stronger when outranked friends are ranked close to the workers and the workers can affect their ranks. Table 6, however, present results that indicate the opposite.

Could the results be driven by aversion to inequality? Recall that the workers are indifferent about outranking peers they are not friends with and do not increase productivity when they rank lower than their friends. Given our results, such aversion

[36]Information about the workers' expected ranks are collected during baseline survey.

would have to be network-specific and asymmetric. The interpretations of inequality aversion and social conformity are equivalent if there is an (extrinsic) social pressure inside the network to reduce inequality. However, they are different if the aversion is intrinsic in nature. Indeed, such incentives may also be present among privately-ranked workers but they may be unable to respond to the inequality as they do not learn their peers' ranks. Although I cannot rule out this possibility entirely it appears unlikely for a few reasons. First, intrinsic aversion to inequality is likely to be empirically similar to altruism, which appears unlikely for the reasons discussed in the previous paragraph. Second, in Table A6 we do not find that the workers respond strongly to the news that they overestimated the productivity of some of their friends. And finally, in unreported analysis, the workers do not appear to respond to rank-distance with their friends in Private Treatment. All the results combined make it unlikely that the workers are driven by intrinsic desire to reduce inequality in productivity among their friends.

Finally, as a manifestation of *ratchet effect*, the workers could collude to increase the piece rates of their sweaters. By slowing down knitting they could convince the management that the sweaters need longer to knit and therefore higher rates. Since increase in the piece rates is a public good, workers may renege on undercutting their own production and earnings. Making ranks public may help to enforce compliance. Ratchet effect, however, is unlikely to be network specific and inconsistent with the fact that the workers do not reduce productivity when they outrank peers they are not friends with. Moreover, Figure A1 shows that reduction in productivity takes place all along the productivity distribution. Under ratchet effect, we expect it to be stronger among the most productive workers as it is beneficial for the management to use knitting time of precisely these workers to determine the piece rates.

8 Conclusion

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

The paper also provides novel data and evidence from a production setting on

how inducing competition among workers can reduce cooperation among themselves. Although the reduced cooperation had a limited, yet negative, effect on the workers' productivity in this context, it could potentially have a large effect in other contexts.

The findings from this paper suggests that firms should carefully assess how an incentive structure interacts with the social preferences of their workers. In a world with complex web of incentives, social incentives can often counteract positive performance incentives, especially those that induce competition among workers.

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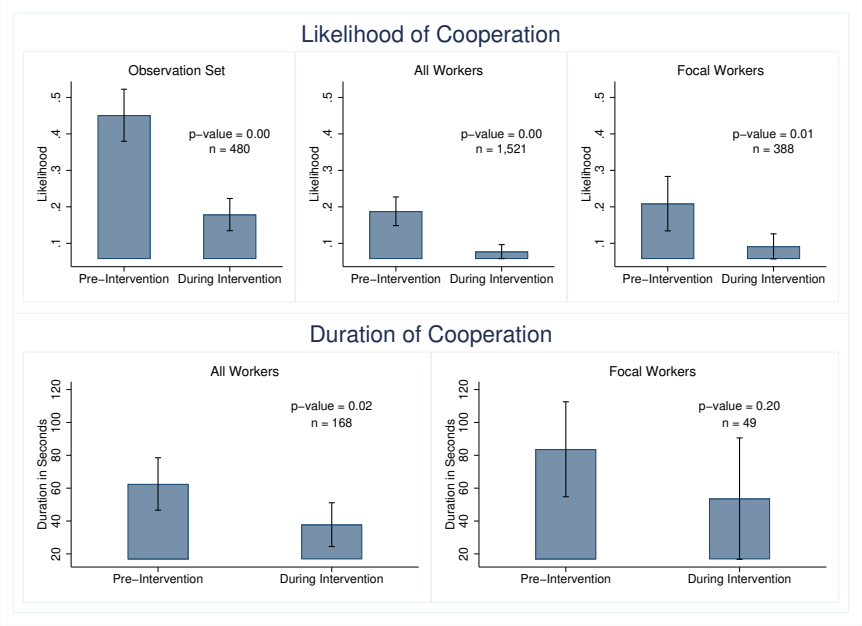
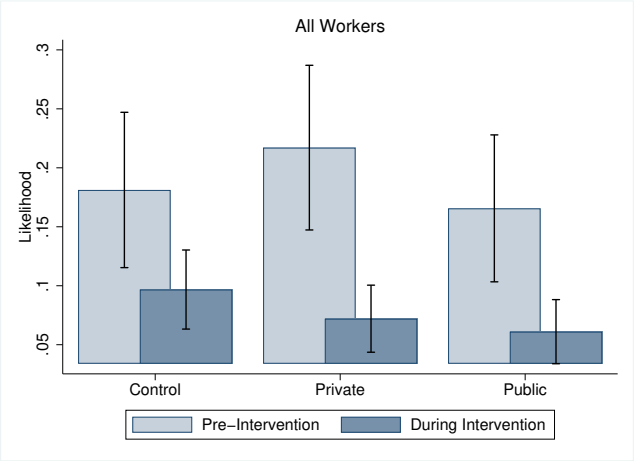
Figure 1: Cooperation Among Workers



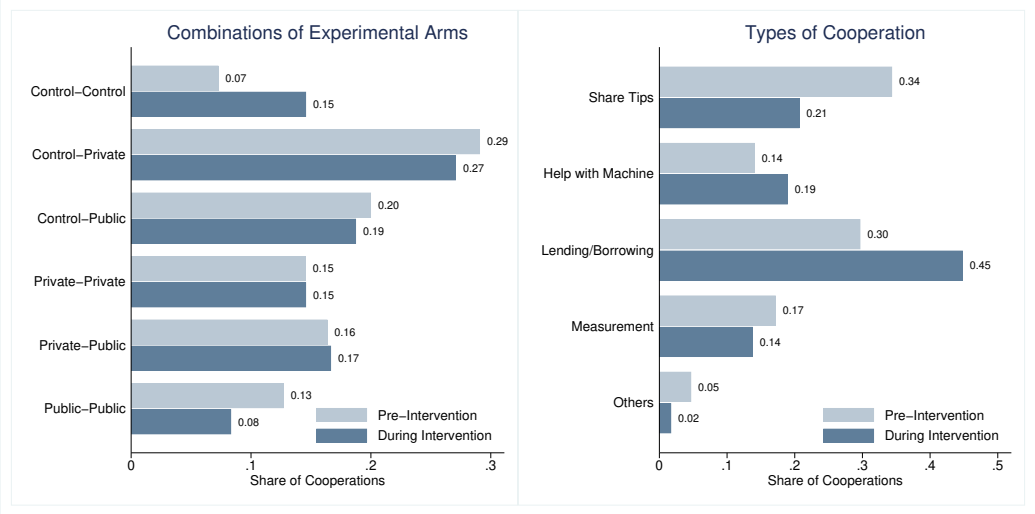
Note: The top panel reports the probabilities that a worker interacts with his coworkers when he is observed. Each observation in the underying regression is an observation set (left panel), individual workers observed during the observation set (middle panel), or only the focal workers associated with the set (right panel). The bottom panel estimates the duration of interaction involving a cooperation. Vertical lines depict 95% confidence intervals around the estimates.

Figure 2: Cooperation Among Workers in Different Experimental Arms



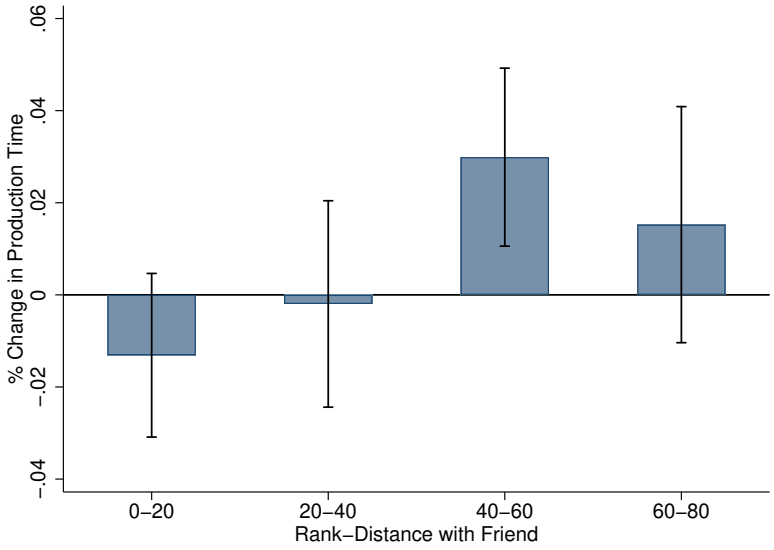
Note: The figure reports the probabilities that a worker interacts with his coworkers when he is observed. Vertical axis reports the marginal likelihood that a worker interacts with coworkers during a given slot at least once. It is estimated from a linear probability model. Vertical lines depict 95%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

Figure 3: Composition of Cooperation Among Workers



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

Figure 4: Productivity Effect of an Additional Outranked Friend



Note: The figure shows the difference in average knitting time of sweaters between publicly- and privately-ranked workers in response to the number of outranked friends at specific rank-distance away. Horizontal axis shows the rank-distances categorized into specific bins. Vertical axis reports the estimated productivity differences between the two treatments. Vertical lines depict 95% confidence intervals around the estimates.

Table 1: Average Treatment Effect on Worker Productivity

	(1) Ln(Time)	(2) Ln(Time)	(3) Ln(Time)	(4) Ln(Time)
Public	0.0182*	0.0181*	0.0179*	0.0186*
	(0.0109)	(0.0107)	(0.0103)	(0.0107)
Private	0.0061	0.0055	0.0054	0.0059
	(0.0124)	(0.0124)	(0.0123)	(0.0136)
Job Sequence			-0.0228***	-0.0225***
			(0.0027)	(0.0027)
Observations	22,870	22,870	22,870	22,787
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 knit a sweater in an assigned job. *Job Sequence* is the cumulative number of jobs in which the worker produced sweaters of a given style and size. *Public* and *Private* refer to Public and Private Treatments respectively. *Baseline Productivity* is the average of monthly production earnings in the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. *Additional Controls* include tenure at the factory at baseline, number of years attended school, and measures of attitudes towards risk and competition. All regressions include a constant. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 2: Social Incentives from Ranking

	(1) Ln(Time)	(2) Ln(Time)	(2) Ln(Time)	(4) Ln(Time)	(5) Ln(Time)	(6) Ln(Time)	(7) Ln(Time)
Public	-0.0482*	-0.0483*	-0.0207	0.0068	-0.0008	-0.0083	-0.0040
	(0.0264)	(0.0266)	(0.0191)	(0.0132)	(0.0113)	(0.0209)	(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**	0.0138*	0.0103**
					(0.0042)	(0.0075)	(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	14,950
Adjusted R-squared	0.3089	0.3089	0.2932	0.2933	0.2933	0.2475	0.2935
Public + Interaction = 0	[0.018]	[0.019]	[0.054]	[0.130]			
From Above = From Below							[0.049]
Baseline Productivity	Yes	Yes	Yes	Yes	Yes	No	Yes
Style-Size FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Counterfactual	Control	Control	Private	Private	Private	Private	Private

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to knit a sweater in an assigned job. *Public* and *Private* refer to Public and Private Treatments respectively. *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 ranks above the median among his friends at baseline. *Distance to Median Rank Among Friends from Above* is the distance to this median if the worker ranks higher than the median. *Distance to Median Rank Among Friends from Below* is the distance to this median if the worker ranks lower. *Baseline Productivity* is the average of monthly production earnings in the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. All regressions include a constant and un-interacted variables corresponding to the interactions. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively. Numbers in square brackets report p-values from tests of corresponding null hypothesis.

Table 3: Placebo Tests

	(1) Ln(Time) Pre-Intervention Jan'16	(2) Ln(Time)	(3) Ln(Time)	(4) Ln(Time)	(5) Ln(Time)
Public	-0.0142	0.0066	0.0130	0.0057	-0.0016
	(0.0187)	(0.0121)	(0.0093)	(0.0108)	(0.0106)
Public * Dist. To Med. Rank Among Friends from Above	-0.0066	0.0084*		0.0128***	0.0092**
	(0.0060)	(0.0048)		(0.0038)	(0.0046)
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)	
Public * Dist. To Med. Rank Among Interacting Non-Friends from Above					0.0015
					(0.0045)
Observations	1,046	22,870	14,950	14,950	14,950
Adjusted R-squared	0.5536	0.2977	0.2938	0.2943	0.2932
Baseline Productivity	Yes	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes	Yes	Yes
Month FE	No	Yes	Yes	Yes	Yes
Counterfactual	Private	Control	Private	Private	Private

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to knit a sweater in an assigned job. *Public* and *Private* refer to Public and Private Treatments respectively. *Dist. to Med. Rank Among Friends (res. Non-Friends) from Above* is the distance to the median rank among friends (res. non-friends) if the worker ranks higher than the median. *Baseline Productivity* is the average of monthly production earnings in 2015, the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. All regressions include a constant and un-interacted variables corresponding to the interactions. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 4: Robustness Tests

	(1) Ln(Time)	(2) Ln(Time)	(3) Ln(Time) At Least 1 Friend	(4) Ln(Time)	(5) Ln(Time)	(6) Ln(Time)	(7) Ln(Time)
Public	-0.0183	0.0034	0.0006	0.0231	0.0221	0.0422	0.0094
	(0.0198)	(0.0200)	(0.0141)	(0.0640)	(0.0699)	(0.0935)	(0.0920)
Public * 1(Competes With at least 1 Friend)	0.0490						
	(0.0301)						
Public * Dist. to Med. Rank Among Friends from Above		0.0100**	0.0098**	0.0107***	0.0106***	0.0072*	0.0077*
		(0.0040)	(0.0046)	(0.0040)	(0.0039)	(0.0041)	(0.0040)
Public * (Number of Friends at Baseline)	-0.0010	-0.0003			-0.0003	-0.0010	-0.0010
	(0.0016)	(0.0012)			(0.0012)	(0.0015)	(0.0016)
Public * 1(Public Intensive Block)							0.0313
							(0.0221)
Observations	14,950	14,950	13,248	14,950	14,950	14,867	14,867
Adjusted R-squared	0.2931	0.2932	0.2936	0.2933	0.2932	0.3039	0.3049
Baseline Productivity	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Public*Baseline Productivity	No	No	No	Yes	Yes	Yes	Yes
Additional Controls	No	No	No	No	No	Yes	Yes
Public*Additional Controls	No	No	No	No	No	Yes	Yes
Counterfactual	Private	Private	Private	Private	Private	Private	Private

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

Table 5: Cooperation and Worker Productivity

	(1) Ln(Time) All Months	(2) Ln(Time) All Months	(3) Ln(Time) Intervention Months	(4) Ln(Time) Intervention Months
Propensity to Interact at Baseline	0.0437			0.0259
	(0.0323)			(0.0243)
Propensity to Interact at Baseline * Post	-0.0051			
	(0.0322)			
Propensity to Receive Help at Baseline		0.0154		
		(0.0335)		
Propensity to Help Peers at Baseline		0.1088*		
		(0.0624)		
Propensity to Receive Help at Baseline * Post		0.0134		
		(0.0296)		
Propensity to Help Peers at Baseline * Post		-0.0455		
		(0.0511)		
Public			0.0089	0.0054
			(0.0276)	(0.0251)
Public * Dist. to Med. Rank Among Friends from Above			0.0062	0.0071
			(0.0098)	(0.0097)
Public * Propensity to Interact at Baseline				0.0207
				(0.0630)
Observations	18,194	18,194	11,140	11,140
Adjusted R-squared	0.2394	0.2398	0.2438	0.2442
Baseline Productivity	No	No	No	No
Style-Size FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Counterfactual	-	-	Private	Private

Ln(Time) is logarithmic transformation of the number of minutes a worker takes to knit a sweater in an assigned job. Columns 1-2 include the pre-intervention month of January 2016. Columns 3-4 include only intervention months. *Post* is a dummy variable that takes the value 0 for months before January 2016, and 1 otherwise. *Public* and *Private* refer to Public and Private Treatments respectively. *Dist. to Med. Rank Among Friends from Above* is the distance to the median rank among friends if the worker ranks higher than the median. *Baseline Productivity* is the average of monthly production earnings in the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. All regressions include a constant and un-interacted variables corresponding to the interactions. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 6: 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	-0.0091	0.1711	0.0189	0.0021
	(0.0158)	(0.0152)	(0.1281)	(0.0274)	(0.3229)
Public * Number of Outranked Friends at Rank-Dist. $\leq$40	-0.0067	-0.0136	0.0775	-0.0063	0.0834
	(0.0081)	(0.0125)	(0.1188)	(0.0138)	(0.1543)
Public * Number of Outranked Friends at Rank-Dist. >40	0.0246***	0.0218**	0.0360	-0.0346***	-0.2131
	(0.0081)	(0.0092)	(0.1082)	(0.0119)	(0.1787)
Public * Number of Competing Friends		0.0058			
		(0.0059)			
Observations	14,950	14,950	1,111	1,145	1,145
Adjusted R-squared	0.2938	0.2939	0.1534	0.4223	0.3175
Sharpened Q-values: Public * Outranked Friends at Rank-Dist. >40	[0.011]	[0.018]	[0.411]	[0.011]	[0.132]
Baseline Productivity	Yes	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	No	No	No
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 knit a sweater in an assigned job. *Public* and *Private* refer to Public and Private Treatments respectively. *Baseline Productivity* is average monthly production earnings in 2015, the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. All regressions include a constant and un-interacted variables corresponding to the interactions. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

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.[37] Therefore, I compute ranks in four steps. First, for each style and size a worker works on in a month, I assign him a *percentile-rank* by comparing his average knitting time of the sweaters with that of others in his treatment arm who work on the same style and size. Next, the percentile-ranks of each of the styles and sizes 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.[38]

A.2 Contamination

A potential concern related to the experimental design discussed in Section 3 is that of contamination across experimental arms. This is particularly relevant since the knitting workers are individually assigned to the experimental arms and they are located on the same production floor.

We may be especially concerned that privately-ranked workers imitate publicly-ranked workers and share information about ranks with each other.[39] But note that if the privately-ranked workers imitate the publicly-ranked workers and the treatment effect among the former follows a similar direction as it does among the latter, it becomes harder to find a difference in outcome between the two arms. It would,

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

[38] 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.

[39] We may also be similarly concerned about workers in the Control arm, but I focus the discussion on privately-ranked workers as the Control-arm workers do not receive any information on ranks, and more importantly, the main results of the paper compare publicly-ranked workers with privately-ranked workers.

however, be concerning if the contamination leads to an increase in productivity among the privately-ranked workers and thus an overestimation of treatment effects.

To address precisely such concerns, the experimental design randomly varies the intensity of Public Treatment across blocks. Eight randomly selected blocks contain relatively more publicly-ranked workers than privately-ranked workers (*Public-Intensive Blocks*), while the remaining seven contain the opposite (*Private-Intensive Blocks*). The Control arm constitutes a third of the workers in each block.[40] To the extent that the density of publicly-ranked workers in the blocks is correlated with the magnitude of contamination effect within the blocks, we can exploit the random variation in this density to test for contamination effects.

Table A2 compares productivity changes among privately-ranked workers in public- and private-intensive blocks by comparing them to Control-arm workers in the same blocks. Column 1 estimates average treatment effect on privately-ranked workers while distinguishing between the two kinds of blocks. The interaction term of Private Treatment and block type is positive, although highly insignificant. It suggests that, if anything, an increase in the density of publicly-ranked workers in a block leads to lower productivity among privately-ranked workers, which goes in the same direction as the treatment effect on public-ranked workers. This comparison would, however, be misleading if both privately-ranked workers and Control-arm workers in public-intensive blocks increase productivity but the Control-arm workers happen to do it by a relatively bigger margin. Reassuringly, a positive coefficient for the dummy variable that indicates the block-type reflects similarly lower productivity among Control-arm workers in public-intensive blocks. Thus, any contamination effects from Public Treatment on privately-ranked workers only attenuates the differences between the two treatments.

Columns 2 and 3 test how privately-ranked workers in the two types of blocks respond to the distance to median rank among friends. Clearly, the privately-ranked workers in neither private- nor public-intensive blocks respond to the rank-distance. The coefficients for the interaction terms in both sets of blocks are small in magnitude. Thus, while there might have been a general decline in productivity among privately-ranked workers in public-intensive blocks, as seen in Column 1, this is hardly a strategic response to ranks as it is among publicly-ranked workers.

More generally, we may be concerned about Hawthorne or John Henry Effects.

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

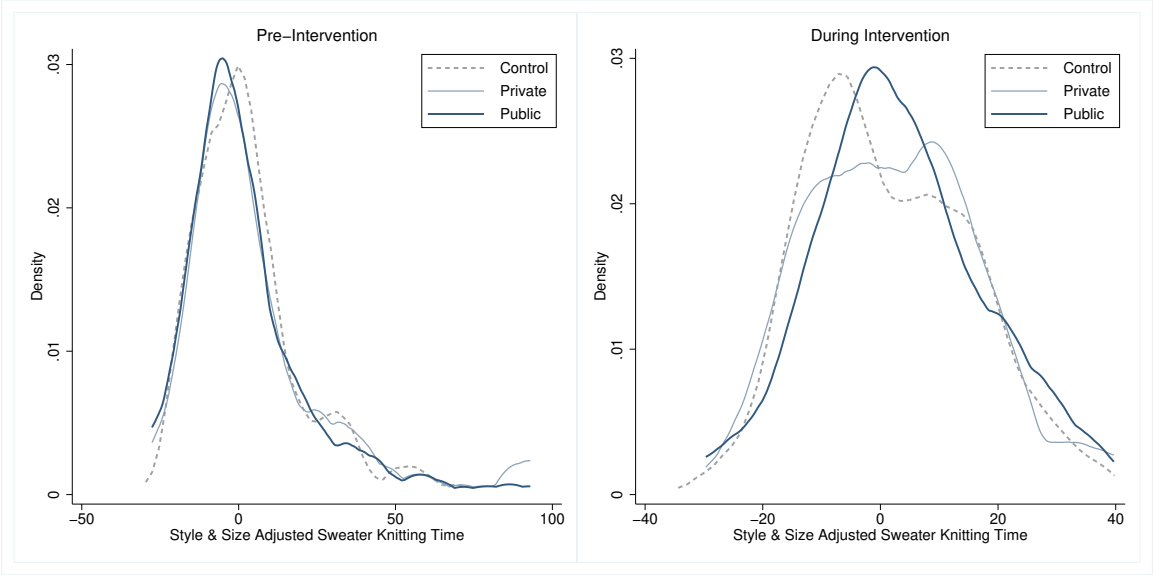
In other words, we may worry that the workers in the different experimental arms behave in particular ways just because they are treated differently to each other in the experiment. However, if either of the effects are present it would affect all workers in a given arm in the same way. This cannot explain the within-arm difference in responses between higher- and lower-ranked friends.

A.3 Alternative Measurement and Clustering of Errors

The specifications in the paper so far use median ranks among friends as a benchmark to compare relative ranks of the workers. Although median rank may appear to be an arbitrary choice it is the traditional benchmark used in the literature to study conformism. Nonetheless, for completeness, Column 1 of Table A3 uses an alternative, more general, measure - *average* rank-distance with friends at baseline. To test asymmetric response, Column 2 separately estimates the workers' response to rank-distance with friends they outrank and those they do not. The results are qualitatively similar to earlier results, when we measure relative ranks using distance to median rank among friends.

In Table A4 I re-estimate the key specifications from Tables 1 and 2 but cluster 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 privately- and publicly-ranked workers respond when competing with at least one friend, in comparison with Control-arm workers. Column 3 compares Public Treatment with Private Treatment instead. Finally, Columns 4-5 test how publicly-ranked workers respond to distance to median rank among friends. Statistical precision of the results remain robust at traditional significance levels, except in Column 3 where the estimates are a little noisier.

Figure A1: Distribution of Average Knitting Time Per Sweater



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

Table A1: 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	[0.89]	[0.34]	[0.29]
Monthly Production Earnings (Jan'15-Jan'16) (BDT)	4758	11042.93	11038.57	10996.9	[0.97]	[0.66]	[0.7]
Monthly Attendance Days (Jan'15-Jan'16) (BDT)	4758	28.38	28.34	28.35	[0.65]	[0.72]	[0.91]
Number of Sweaters Produced in a Month (Jan'15-Jan'16)	4755	387.51	394.43	358.88	[0.76]	[0.19]	[0.12]
Demographic Characteristics							
Years Attended School	362	7.84	7.56	7.38	[0.39]	[0.17]	[0.59]
Age (years)	352	30.61	30.44	30.9	[0.77]	[0.61]	[0.43]
Tenure (years)	366	5.32	5.47	5.27	[0.68]	[0.88]	[0.59]
1(Competitive)	363	0.45	0.51	0.41	[0.35]	[0.56]	[0.13]
Risk Scale (1 to 10; 10 highest)	363	5.54	5.5	5.72	[0.91]	[0.51]	[0.46]
Social Network							
Number of Peers in Block	363	23.55	23.61	23.63	[0.86]	[0.8]	[0.94]
Number of Friends	363	11.46	10.53	12.32	[0.28]	[0.34]	[0.03]
Productivity of Friends in Same Treatment vs. Outside	315	1	1.01	0.99	[0.64]	[0.57]	[0.34]

Note: The table reports means of various baseline characteristics for workers in the three experimental arms. Columns 5-6 test equivalence of these means between control and the treatment arms. Column 7 does the same for the two treatment arms. P-values from the tests are reported in square brackets. Productivity of friends is computed using their average monthly production earnings in July-September 2015, three months preceding the baseline survey.

Table A2: Contamination Effects

	(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 * Dist. to Med. 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 the number of minutes a worker takes to knit a sweater in an assigned job. *Private* refer to Private Treatments. *Prvt. Inten.* refer to Private-Intensive blocks which contain relatively more privately-ranked workers than publicly-ranked workers. *Pub. Inten.* or *Public-Intensive Block* refer to the blocks with the opposite. *Dist. to Med. Rank Among Friends from Above* is the distance to the median rank among friends if the worker ranks higher than the median. *Baseline Productivity* is 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 and un-interacted variables corresponding to the interactions. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A3: Alternative Measurements

	(1) Ln(Time)	(2) Ln(Time)
Public	0.0122	-0.0127
	(0.0091)	(0.0206)
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)
Observations	14,950	14,950
Adjusted R-squared	0.2934	0.2937
Outranked = Outranking		0.00700
Baseline Productivity	Yes	Yes
Style-Size FE	Yes	Yes
Month FE	Yes	Yes
Counterfactual	Private	Private

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

Table A4: 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*	-0.0196	-0.0008	-0.0040
	(0.0108)	(0.0258)	(0.0245)	(0.0134)	(0.0170)
Public * 1(Competes with Friends)		0.0728***	0.0362		
		(0.0281)	(0.0271)		
Private	0.0055	-0.0248			
	(0.0111)	(0.0182)			
Private * 1(Competes with Friends)		0.0359			
		(0.0220)			
Public * Dist. to Med. Rank Among Friends from Above				0.0099**	0.0103**
				(0.0047)	(0.0052)
Public * Dist. to Med. 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	0.2933	0.2935
Public + Interaction = 0		[0.021]	[0.175]		
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 knit a sweater in an assigned job. *Public* and *Private* refer to Public and Private 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. *Dist. to Med. Rank Among Friends from Above* (res. Below) is the distance to the median rank among friends if the worker ranks higher (res. lower) than the median. *Baseline Productivity* is average monthly production earnings in the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. All regressions include a constant and un-interacted variables corresponding to the interactions. 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 A5: Descriptive Statistics for Observations Data

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

Note: The table reports key statistics related to the data on cooperation. The top and middle panels report the number of observations and observed workers before and after ranking begins. The bottom panel reports the means of various baseline characteristics for the workers who were observed before ranking begins. Columns 5-6 test the differences in mean characteristics between control and the treatment arms against nulls of zero. Column 7 does the same for the differences in mean characteristics between the two treatment arms. P-values from the tests are reported in square brackets. Productivity of friends is computed using their average monthly production earnings in July-September 2015, three months preceding the baseline survey.

Table A6: 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 * Dist. to Med. Rank Among Friends from Above	0.0108**	0.0093**	
	(0.0049)	(0.0042)	
Public * (Number of Outranked Friends at Rank-Dist. $\leq$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 the number of minutes a worker takes to knit a sweater in an assigned job. *Public* and *Private* refer to Public and Private Treatments respectively. *Dist. to Med. Rank Among Friends from Above* is the distance to the median rank among friends if the worker ranks higher than the median. *1(Underestimated Own Rank)* is a dummy variable that takes value 1 if a worker receives a rank in the first treated month that is better than what he predicted for himself during baseline survey. *Number of Friends Incorrectly Predicted as More Productive* is the number of friends a worker thought were more productive than him at baseline. *Baseline Productivity* is the average of monthly production earnings in the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. All regressions include a constant and un-interacted variables corresponding to the interactions. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.