Social Conformity at Work: Evidence from a Field Experiment

Anik Ashraf *

October 23, 2021

Abstract

Relative performance incentive can increase workers' productivity by inducing them to signal high productivity to their peers and employer. On the other hand, desire to signal social compatibility to their social groups in the workplace can push workers to conform to group productivity, and potentially reduce effort. Using a five-month long field experiment with performance ranks at a sweater factory I study how workers respond to the two-audience signaling problem. Treated workers receive ranks either privately or publicly. In response, average productivity remains unchanged among privately-ranked workers. But signaling motivations from public ranks reduce average productivity among publicly-ranked workers. Exploiting the composition of peers a worker competes with I show that workers choose to signal social compatibility over high productivity when they rank higher than friends at baseline, but only when the ranks are visible to coworkers. This is driven by a preference for social conformity than for internalizing externality on friends. Additional evidence shows that the ranking competition decreases overall cooperation among workers.

Keywords: Social Incentives, Peer Effects, Rank
JEL Classification: D23, J53, O15

*Department of Economics, LMU Munich. Email- anik.ashraf@econ.lmu.de. Acknowledgements: I am grateful to Christopher Woodruff and Rocco Macchiavello for their continuous support with this project. A. Latif Patwary and Shusmoy Roy provided excellent research assistance. I thank Arun Advani, Robert Akerlof, Nava Ashraf, Oriana Bandiera, Iwan Barankay, Jordi Blanes-i-Vidal, Luca Braghieri, Kristina Czura, Pascaline Dupas, Florian Englmaier, James Fenske, Martina Miotto, Andrea Prat, Roland Rathelot, Simeon Schudy, Yves Le Yaouanq, Giorgio Zanarone, and numerous seminar/conference participants for helpful comments. Private Enterprise Development in Low-Income Countries (PEDL) financially supported this research. AEA RCT Registry ID for the experiment in this paper is ID-0000981.

1 Introduction

Workers face a complex mix of incentives at work. Some incentives such as performance and career incentives are formally or informally contracted upon with the firm. Others such as intrinsic, relative, and social incentives are more personal and can be intricate. This complex web of incentives may at times find a worker having to simultaneously please distinct audiences who have conflicting interests in the worker's action. Understanding how workers respond when confronted with multiple audiences is key to designing effective incentive schemes within organizations.

Relative performance incentive illustrates an incentive scheme that encapsulates multiple incentives and audiences for a worker. Seminal models on relative performance incentives such as Lazear and Rosen (1981) and Green and Stokey (1983) propose to use them to pay workers. But such incentives can also affect worker productivity through pure signaling motivations. Consider relative performance ranking for an example. Driven by status incentives associated with high ranks, workers may want to signal to their coworkers, managers, or whoever values high productivity that they are among the more productive workers in their firm (e.g. Holmstrom (1999); Besley and Ghatak (2008); Ashraf et al. (2014a)). For instance, the workers may like to be considered as role-models by their peers or receive greater respect and preferential treatments from their managers and employers.[1] On the other hand, any attempt at racing ahead under such incentives can be perceived by peers as self-serving, an undesirable attribute in coworkers, especially in fellow members of a social group. When productivity plays a role in forming social groups (e.g. friends) in the workplace, increasing effort to perform better than the group a worker identifies with may be perceived as an attempt to disassociate himself from the group in productivity. Concerns about losing group identity and facing social sanctions may lead workers to reduce effort and conform to group productivity to signal social compatibility (Bernheim (1994), Akerlof (1997), Akerlof and Kranton (2000), and Akerlof (2016)).[2]

[1] Workers may also be driven by career concerns and pursue high ranks to get pay raise or promotions. This paper will not focus on career incentives and, in fact, will be able to shut down career concerns as an underlying incentive.

[2] A similar behavior in the context of education is known as aversion to *acting white*. Austen-Smith and Fryer (2005) and Fryer (2007) model a two-audience signaling problem for students who have to choose between obtaining more education to signal high productivity to potential employers and conforming to lower education levels of their social groups to signal social compatibility. Investing in education when the norm in one's social group is the opposite is known as *acting white*. Bursztyn and Jensen (2015) and Bursztyn et al. (2019) empirically confirm that students under-invest in education

This paper studies how workers respond to a two-audience signaling problem created by relative performance ranking. Performance ranking has garnered much interest in recent literature.[3] However, the evidence has been mixed. For instance, Delfgaauw et al. (2013), Song et al. (2017), and Ager et al. (forthcoming) find positive effects from performance ranks, but Ashraf et al. (2014b) and Blader et al. (2020) find negative effects. In this paper, I test whether motivation to signal high productivity can be undermined by workers' desire to signal social compatibility to their social group, a particular subset of their coworkers who value social compatibility of the workers.

Disentangling social incentives underlying performance ranks poses several empirical challenges. First, we must have clear measures of individual performance. Second, there must be credible threats and consequences of social sanctions. Third, we must be able to isolate social signaling incentives from the effect of private consumption of information on ranks (*intrinsic incentives*).[4] Finally, we must be able to distinguish between workers' responses to the two audiences - the audience who values productivity and the audience who values social compatibility.

I implement a randomized control trial experiment at a large sweater factory in Bangladesh where I provide workers with performance ranks. The experiment involves 366 knitting workers who knit sweaters through an individual production process. Workers are paid based on piece rates and the number of sweaters they knit. The sweaters are assigned to them in batches of 12-48 sweaters, where each batch is called a *job*. Using administrative records from 22,000 such jobs I use average knitting time of a sweater in each job as a measure individual productivity of the workers. This helps me to solve the first empirical challenge. The knitting workers had been working together at the factory for more than four years on average and report strong social ties with peers at baseline. Thus, the field-setting lends a real-world social network

to signal social compatibility to their peers. Although developed in the context of education, the model in Austen-Smith and Fryer (2005) has wide-ranging applications.

[3]It is also considered a cheap and productive management practice by industry experts. For instance, in one of its questions the World Management Survey, administered in 35 countries to assess management practices of firms, provides the highest score to a firm if they use public performance ranking for their workers. See Question 12 on Clarity and Comparability of Goals in the World Management Survey 2010's Manufacturing Survey Instrument (World Management Survey (2010)). 'Employee of the Month' awards at restaurants, call centers, and other organizations are also a common form of performance ranks, although simpler.

[4]Indeed, existing evidence suggest that ranks affect productivity even when they are not visible to coworkers (e.g. Barankay (2011, 2012); Blanes-I-Vidal and Nossol (2011); Gill et al. (2019)).

and social incentives to the experiment and solves the second challenge. I overcome the third and fourth challenges through experimental design. Treated workers in the experiment receive information about their relative ranks in one of two ways. In the first, *Private Treatment*, workers receive information about only their own ranks. In the second, *Public Treatment*, workers receive information about their own ranks as well as that of other workers in the Public Treatment.[5] Private ranks control for intrinsic incentives from ranks, while public ranks enhances visibility of signals and introduces signaling motivations. Comparison of the two treatments isolates the effect of the latter. A third experimental arm, a Control arm, does not rank workers and therefore helps to estimate the net effects from the two treatments. Finally, a social network map constructed at the beginning of the intervention identifies the social relationship between a worker and the peers he competes with. Social compatibility is judged when workers directly compete with their friends. So, the social network map helps to distinguish between workers with and without an incentive to signal social compatibility. The intervention continued for five months, which repeatedly reinforces the social incentives and enables us to observe workers' responses beyond short run.

There are three key findings. First, the intervention does not change average productivity of privately-ranked workers. On the other hand, publicly-ranked workers reduce productivity by about 1.5 percent on average over the five-month long intervention. In other words, intrinsic incentives alone do not affect worker productivity on average. But signaling motivations underlying public ranks strongly affect worker behavior. The direction of the average effect suggests that the desire to signal high productivity is outweighed by negative signaling incentives, possibly the desire to signal social compatibility.

Second, compared to privately-ranked workers, publicly-ranked workers reduce productivity by 1.7 percent when their friends are also randomly assigned into Public Treatment. Workers who do not compete with any of their friends (weakly) increase productivity instead. Additional tests suggest that the reduction in effort is driven by social pressure to signal social compatibility. The first test shows that the productivity drop is entirely driven by publicly-ranked workers who receive a rank higher than the median among their friends in the very beginning of the intervention (at baseline). The second test shows that the magnitude of the drop increases linearly as

[5] Workers in a given treatment arm are ranked among co-workers in the same arm.

a worker is ranked farther and farther above the median rank. In contrast, workers who are ranked below the median do not alter their effort choice. These tests compare workers with similar distance to the median rank among their friends, but who were randomly determined to receive either private or public ranks. In other words, the tests estimate the effect of experimental variation in public visibility of ranks while holding worker characteristics constant, including those that are potentially correlated with workers' rank-differences with friends at baseline. The two tests combined suggest that publicly-ranked workers reduce productivity when they find themselves performing better than their group, but lower performing workers do not alter their behavior. A third test comparing privately-ranked workers with Control arm verifies that better performing workers do not react to differences between their rank and their friends' when information about ranks are kept private.[6] In other words, the reduction in effort is motivated by signaling incentives. Finally, a fourth test shows that publicly-ranked workers do not reduce effort when they outrank peers they are not friends with. This suggests that the reduction in effort is a signal to their social groups.

A series of robustness tests verify that the heterogeneous treatment effects are not driven by endogenous worker characteristics. We may be concerned about two characteristics in particular - a worker's social popularity and his productivity in general. Higher the number of friends a worker has, greater is the likelihood that some of his friends are assigned the same treatment and the worker has to compete with them. In addition, the likelihood that a worker is ranked higher than his friends is positively correlated with his own productivity at baseline. Instead of signaling incentives from public ranks, it could be these underlying characteristics that drive the productivity drop among publicly-ranked workers. However, multiple tests rule out this concern. Moreover, the results are robust to controlling for additional worker characteristics and controls.

The third finding characterizes the signal that the workers send to their social groups. There could be two related but subtly different signals that motivate workers to reduce productivity. Notice that a worker's effort in a ranking contest imposes a negative externality on his friends if by working harder the worker pushes his friends'

[6]Additional placebo tests confirm that the productivity drop among publicly-ranked workers realizes only after the ranking starts, and that the workers do not reduce effort when they rank better than their friends they do not compete with.

ranks down. A worker who pursues high ranks at the cost of his friends' reputation can portray an image of a selfish individual, leading to getting socially ostracized. The worker may, therefore, choose to reduce effort to internalize this externality.[7] Alternatively, workers may want to avoid being perceived as different than the rest of the group, and even worse, being perceived as keen to distance himself from the group (in productivity). Driven by concern about losing group identity, the worker may reduce effort to conform to the productivity of his group, now made salient by the ranking. To distinguish between the two mechanisms observe that concerns about externality would induce workers to reduce effort with the aim of stooping their own ranks below their friends'. The likelihood that the marginal reduction in a worker's effort improves a friend's rank is higher when the friend is closer than when he is farther away in ranks. Thus, if workers are concerned about the externality they are more likely to reduce effort in the former case than in the latter, when the cost of improving a friend's rank is very high. Conversely, concerns about conformity would lead workers to reduce effort when they are ranked far away from their social group. Exploiting variation in pair-wise rank-differences with friends, I find that publicly-ranked workers reduce productivity only when their friends are ranked lower and far away from them. They do not reduce productivity when their friends are ranked lower but closer. Additional tests confirm that the realized reduction in productivity is not sufficient to change any of their friends' ranks; but it does reduce the rank-distance between a worker and his friends by lowering the workers' own ranks. The results provide suggestive evidence that workers reduce productivity to conform to their social groups.

I test alternative explanations for the results. One key concern in particular is that publicly-ranked workers, unlike privately-ranked workers, learn about their peers' relative productivity through public ranks. In principle, the results could be driven by potentially new information they learn from the public ranks (for instance, the workers become complacent after learning their peers' ranks). To test this hypothesis I use workers' prior knowledge about each other's productivity to measure the extent of new information the workers receive from public ranks. I find no empirical support that this new information drives the results. I can also eliminate ratchet effect, collusion, and altruism as alternative explanations for the results.

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

Finally, there is an additional finding. Data on cooperation between workers reveals that workers are less likely to help each other once the ranking starts. Starting from a little before the intervention, members of the research team quantitatively measured cooperation on the production floor through direct observations of workers. Observers watched workers from a distance for 20 minutes in a row (an *observation*) and recorded the number of instances when the workers were engaged in cooperative behavior with their peers. The observations followed a pre-set fixture in which workers were randomly assigned into 20-minute slots. About 1,500 such observations were completed in total. Analysis of this data shows that, before the intervention, there is a 15% chance that a worker in an experimental group is engaged in at least one instance of cooperation during a 20-minute observation slot. After the intervention starts, the likelihood of cooperation falls by at least half for any experimental group (including Control arm). In other words, the ranking contest reduces overall cooperation among all workers on the floor. While the before-after comparison is prone to seasonality effects, comparison of experimental arms in a given time period can account for them. Reassuringly, average likelihood of cooperation prior to the intervention shows no significant differences between arms. In the post-intervention period, however, the likelihood that a worker directly involved in a ranking contest engages in cooperative behavior on the floor is lower than that for a Control arm worker. The decline for privately-ranked workers is small and statistically insignificant. But the probability that a publicly-ranked worker engages in at least one instance of cooperation is nearly half of that for a Control arm worker. Again, the pattern of cooperation for workers in different arms suggest that the ranking contest adversely affects cooperative behavior among workers. Reduction in overall cooperation among publicly-ranked workers appear counter-intuitive as workers in this context appear keen to signal social compatibility. It is, however, plausible that workers now provide less help to non-friend peers and more help to friends; the former change may outweigh the latter. Unfortunately, because cooperation among workers is a low-frequency event before the intervention and substantially drops in the post-intervention period, the recorded instances of cooperation in the post-intervention period are not sufficient to test redistribution of help.

The paper contributes to three strands of literature. The first is a growing body of literature that has explored the potential of relative performance ranks to increase worker productivity through signaling incentives. In fact, ranking has attracted much

attention across a wide range of fields, including management (Blanes-I-Vidal and Nossol (2011); Kuhnen and Tymula (2012)), education (Azmat and Iriberri (2010)), and public policy (Chetty et al. (2015)).[8] Although theory predicts an increase in productivity with public performance ranks (see Besley and Ghatak (2008)) empirical evidence, especially from firms, has been inconclusive. While Ashraf et al. (2014a), Delfgaauw et al. (2013), Song et al. (2017), and Englmaier et al. (2021) find positive effects from public ranks, Ashraf et al. (2014b) and Blader et al. (2020) find negative effects. It remains unclear why public ranks are counterproductive in some cases. A clue, however, lies in Blader et al. (2020). The paper studies truck drivers in a U.S. transport company that underwent a management intervention to encourage teamwork and collective effort. Blader et al. (2020) document positive effects from public ranks in sites where the management intervention had not yet taken place. On the other hand, the ranks had a negative effect in sites that had received the intervention. One explanation is that the intervention reinforced social ties among drivers which in turn led to a negative effect from ranking. This was not formally tested in the paper.

In this paper, I propose one explanation of the mixed evidence from public ranks in the literature.[9] In particular, I provide direct evidence of how workers' preference for social conformity with their social group counteract positive signaling incentives from public ranks and lead to an overall negative effect. A closely related paper is Bandiera et al. (2005). They show that university students working as temporary fruit pickers in an UK farm withdraw effort under *relative pay scheme* to internalize the negative externality their effort exerts on their peers' earnings. Using relative performance ranking, my paper complements Bandiera et al. (2005) in showing how relative incentive schemes, a scheme that pits workers against each other, may become counterproductive because of workers' social preferences. The underlying mechanism in Bandiera et al. (2005) is workers' desire not to hurt their peers' pay when they are observed. In my paper it is asymmetric conformity to group productivity possibly to preserve one's group identity. With experimental evidence from relative performance ranks at a manufacturing firm, this paper highlights how social concerns can lead even

[8] See Kluger and DeNisi (1996) for a discussion of findings in the field of psychology.

[9] Evidence is also mixed with ranks provided in private. Blanes-I-Vidal and Nossol (2011) and Gill et al. (2019) find positive effects from private ranks, but Barankay (2011, 2012) find negative effects. However, I am able to rule out that the negative effects from public ranking is driven by private consumption of ranks as I compare publicly-ranked workers to privately-ranked workers.

older and more experienced workers to undercut their performance and earnings.[10]

More generally, this paper contributes to the literature that study how social preferences exhibit themselves at workplaces. Social preferences affect worker behavior in many different ways. For instance, workers may choose to internalize externalities of their actions on their peers (Hamilton et al. (2003), Mas and Moretti (2009), Bandiera et al. (2005)), misallocate resources to help friends (Bandiera et al. (2009)), reward employer in return for increase in pay (Gneezy and List (2006), Jayaraman et al. (2016)), increase effort when it is meaningful for the employer (DellaVigna et al. (2019)), but also punish employer for taking stern measures (Casaburi and Macchiavello (2015), Akerlof et al. (2021)).[11] My paper provides evidence on conformist behavior from a real workplace, and shows how this can render performance incentives ineffective. Although such conformity has been modelled in theory (Bernheim (1994) and Akerlof (1997)), and shown to exist in non-work settings (e.g. educational settings in Bursztyn and Jensen (2015) and Bursztyn et al. (2019)) empirical evidence for conformism at real workplaces has been relatively scarce.

The paper also relates to the literature on workers' preference for inequity aversion. A number of recent papers have explored pay transparency (see Perez-Truglia (2020) and Culen and Perez-Truglia (2021)) and shown that workers do not like when their coworkers are paid relatively more (see Card et al. (2012) and Breza et al. (2015)). While the inequity aversion in this literature relates to pay and is intrinsic in nature, in my paper on the other hand, workers dislike being more productive than their peers because of social pressure to conform to group level productivity. This suggests that inequity aversion may not only be an intrinsic trait but also socially desirable at workplaces.

The rest of the paper is organized as follows. Section 2 offers a brief discussion of the incentives underlying performance ranks. Section 3 describes the setting in which the experiment was conducted. Section 4 describes the experiment. Section 5 presents the main results on the effect of the intervention on worker productivity; Section 6 tests their robustness. Section 7 explores the mechanism behind these results. Section 8 investigates the effect of the intervention on social cohesion among workers. Section 9 concludes.

[10] To be concrete, as of the beginning of the intervention, the workers in the Knitting Section were aged between 21 and 46 years, with tenure at the factory between one and 11 years.

[11] See Ashraf and Bandiera (2018) for an excellent review on different kinds of social incentives that play a role in the workplaces.

2 Incentives from Ranks

The paper aims to learn how workers respond to a two-audience signaling problem created by performance ranking. The following section discusses the incentives from performance ranking. Section 4 will build on this discussion to present the experiment.

Performance ranks are inherently designed to distinguish more productive workers from less productive ones. Even when ranks do not earn monetary rewards they can be useful incentives for workers. Moldovanu et al. (2007) and Besley and Ghatak (2008) suggest that performance ranks can trigger status-seeking behavior among workers.[12] Better ranked workers, synonymous to better performers, may command greater respect from peers and managers alike, considered role-models by their peers, and enjoy preferential treatments. Pursuit for such status can induce workers to increase effort. Empirical support for this idea exists in Ashraf et al. (2014a), Ager et al. (forthcoming), and Englmaier et al. (2021) that find positive effect of public ranks on worker effort. Beside signaling for status, workers may also want to signal high productivity to their employers because of career concerns (Holmstrom (1999)). This paper will, however, speak more towards signaling for status than for career concerns as the context in which the paper is set is more conducive to the former than the latter (I return to this issue in Section 3).

On the other hand, trying too hard to distinguish oneself from the rest may trigger backlash from peers. In particular, workers may be concerned about social sanctions from their social group or friends if they are perceived as eager to distinguish themselves from the group in productivity, and possibly shame the rest in the process. In addition, one worker ascending in ranks means someone else gets demoted; effort imposes a negative externality on peers in a ranking contest. Therefore, working hard to ascend ranks may be considered as selfish and socially incompatible, and lead to social sanctions. In such situations, workers may reduce productivity to signal social compatibility to their social group (the second audience) in fear of social sanctions. Indeed, a number of theoretical papers suggest that individuals proactively conform to social groups they want to identity with (e.g. Bernheim (1994); Akerlof (1997)). Evidence of social conformity in the context of real workplaces is rare. But Bursztyn and Jensen (2015) and Bursztyn et al. (2019)) provide evidence with students in

[12] See Bagwell and Bernheim (1996), Benabou and Tirole (2003), Gaspart and Seki (2003), Rege (2008), Charles et al. (2009) and Bursztyn et al. (2018)) for theory and evidence on status-seeking behavior in general.

education settings. They show how the students do not want to be perceived by their peers as *acting white* and studying too hard, and therefore under-invest in education. As for externality, although not in the context of ranks, Hamilton et al. (2003), Bandiera et al. (2005) and Mas and Moretti (2009) show how workers internalize externality of their actions on peers in different contexts. In Bandiera et al. (2005), in particular, workers sacrifice income to internalize the negative externality they impose on their peers in a relative-incentive-pay scheme.

To be clear, intrinsic motivations from ranking can also affect worker productivity. Intrinsic motivations, in contrast to signaling motivations, derive from private consumption of ranks. Empirical papers that provide workers with ranks in private, and hence reduce the signaling utility of ranks, find mixed evidence. Gill et al. (2019) find positive effects in a laboratory experiment; Blanes-I-Vidal and Nossol (2011) and Blader et al. (2020) report similar findings in field settings. Barankay (2011) and Barankay (2012), in contrary, find negative effects. Although it is difficult to difficult to determine the causes behind mixed evidence in these papers, intrinsic motivations from ranks may clearly matter.

In summary, ranks introduce a trade-off to the workers. On one hand they might want to increase effort to signal productivity to peers and managers. On the other hand, they might want to reduce effort to signal social compatibility to their social group. The rest of the paper explores how workers respond to this trade-off.

3 Setting

We now turn to the sweater factory where the experiment for this paper takes place. The experiment was conducted at the Manual Knitting Section of a leading sweater factory in Bangladesh. Workers in this section use individually assigned knitting machines to knit sweater parts that are later stitched together in a different section of the factory. In January 2016, at the beginning of the intervention, 366 workers were working at this Section. These workers are divided into 15 administrative groups called blocks. Each block is supervised by one supervisor, whose role is largely limited to instructions and troubleshooting. Workers in a particular block locate close to each other on the floor.

Several attributes of the Knitting Section make it a suitable setting for the study. First, production in the Knitting Section takes place through an individual produc-

tion process.[13] The individual production process makes it possible to measure and compare productivity across workers.

To be specific, knitting workers are individually assigned to produce a batch of sweaters, a *job*, which typically constitutes of 12 or 24 pieces of sweaters of a particular style and size. Once a particular job is completed the workers receive their next job. The next job can be sweaters of the same style and size as the previous job or a different combination. Workers receive payment at the end of a production month based on the number of sweaters they produce and the piece rates for those sweaters. The piece rates, however, vary by style and size. The mix of sweater styles and sizes that a worker works on in a month may be different than that of another worker.

Second, repeated interactions and existing social network among workers in this Section provide a rich environment for social incentives to thrive. Indeed, during a baseline survey conducted at the beginning of the experiment 345 of the 366 workers report that they socialize with at least one of their coworkers outside the factory (*friends*). This is not surprising as most of the workers in this Section had been working together for at least six years prior to the start of the intervention.

Finally, workers care about relative productivity among themselves. Again, the baseline survey helps to illustrate this point. By individually naming every single co-worker in their block, the survey asked the workers if they compare their productivity to the named co-worker.[14] More than 90% of the workers admit to comparing their productivity with at least one coworker in their block. This is shown by the first bar in Figure 1 based on the workers' responses. The second bar shows that conditional on drawing comparisons with at least one coworker, the workers compare themselves to 10 coworkers on average, or about 40% of the coworkers in their blocks.

Yet, workers have a noisy idea of how their productivity compares with that of their coworkers. In a different question, the baseline survey asked the workers to make a comparison of their productivity against their coworkers'. The third bar in Figure 1 shows that even with respect to peers they admit comparing themselves to in the previous question, they get almost 40% of the bilateral comparisons wrong.[15]

Another important characteristic of the Section is that its workers are not keen

[13]Figures A1 and A2 in the Appendix show pictures of the production process and a sample sweater part produced at the factory.

[14]The sheer size of the full workforce made it infeasible to repeat this exercise with coworkers outside block.

[15]Workers ranked themselves based on production earnings in the three previous months.

to get promoted. The knitting workers can get promoted to become supervisors. But the knitting workers' take-home earnings from pre-intervention period reveals that a supervisor's salary is less than the earnings of a knitting worker at the 33rd percentile of earnings distribution. Clearly, the best workers do not have an incentive to be promoted. This helps to eliminate career concerns as a mechanism underlying any worker response to performance ranks, and ensures that any incentive to increase productivity is driven by signaling motivations to earn status.

In summary, the Manual Knitting Section offers a context where social comparisons of productivity is common. Nonetheless, there is significant noise in the information that workers have on their relative productivity. Physical distances between the workers, large size of the workforce, and lack of information about peers' production earnings are the likely factors that contribute to this noise. Even when peers' production earnings are known, the basket of sweaters that a worker works on may differ from that of another. This, combined with the fact that different styles have different piece rates, make it difficult to precisely deduce relative productivity by comparing production earnings.[16] Therefore, performance ranking is likely to provide workers with new information that they care about.

4 The Experiment

Building on the discussion of incentives in Section 2, I design and implement a randomized experiment at the Manual Knitting Section. I partner with the management of the factory to introduce performance feedback for the knitting workers. The first feedback is provided at the beginning of February 2016, based on performance in January 2016. Feedback continues until June 2016.[17] In what follows, I first explain the experimental design, and then the data that I use in the analysis.

[16] Anecdotal evidence confirm that the workers are aware that the piece rates do not accurately reflect complexity of sweaters across different styles.

[17] The intervention was introduced to the workers as a new management practice at the factory. The factory management was keen to continue ranking workers if the results from the intervention proved promising. Therefore, workers were not told of any termination date of the intervention.

4.1 Treatments

The intervention provides workers at the Manual Knitting Section with individually addressed monthly letters containing feedback on their production in previous month. The workers receive one of three kinds of feedback.

The first feedback provides a worker with monthly letters that contain aggregate information about his production.[18] The letters contain information on the total of number of sweaters the worker produced in previous month, a break down of his total production into styles, total time taken to produce them, and a list of all the workers who receive similar feedback. Workers who receive this feedback are essentially a *Control* group since they do not receive information on ranks.

The second feedback additionally provides information on rank based on a worker's production in the previous month. It, however, does not contain information on anyone else' rank. This is *Private Rank Treatment* (henceforth, Private Treatment).[19]

The third feedback contains the same information as the second, but workers are also told the ranks of their peers who receive this feedback. Ranks of peers are shown beside their names in the list of workers receiving this feedback. In other words, workers in this *Public Rank Treatment* (henceforth, Public Treatment) know each other's rank. Workers in a given treatment arm are ranked against workers in the same treatment arm.

The experimental design makes a distinction between intrinsic and social incentives from ranks. As earlier highlighted in Section 2, workers may be driven by intrinsic incentives, such as a distaste for bottom ranks and desire for high ranks, even in absence of social concerns (e.g. Gill et al. (2019)). Private Treatment capture precisely these responses.

The Public Treatment makes ranks visible to peers, and introduces motivations for signaling. The difference between Public Treatment and Control estimates the total effect of public ranks (intrinsic and signaling motivations). The difference between Public and Private Treatments estimates the net effect of signaling motivations alone. In addition, in order to make a distinction between the two audiences a worker may be signaling to, a baseline survey mapped the existing social network among the workers.

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

[19] Following Barankay (2012), I also provide workers with information on the ranks they can achieve if they improve their average production time by 5, 10, and 20 percents. This additional information gives them an idea of how harder they would need to work to improve ranks.

It records the nature of relationship between pairs of workers.

Workers get randomly assigned to the three experimental arms through a public lottery in January 2016. Once assigned to a particular feedback, a worker receives the same kind of feedback for the entire period of the intervention.[20]

A particular concern is that workers in Private Treatment may imitate Public Treatment workers and share information about their ranks within themselves. If that happens, ranks of workers in Private Treatment may also become public knowledge. To be clear, individual sharing information on ranks is unlikely to create strong social status or social conformity effects as formal public ranking does. Moreover, even if it does this only makes it difficult to find a difference between worker responses in Public and Private Treatments. Any observed effect, ex-post, is therefore a lower bound of the effect of signaling motivations.

Nonetheless, I vary the intensity of Public Treatment across blocks to measure possible spill-over effect from Public to Private Treatments. Prior to random assignment of workers into experimental groups, each of the 15 blocks of workers is randomly chosen as either a Private- or a Public-Intensive block. In Private-Intensive blocks, 43.33% of the workers are assigned to Private Treatment and 23.33% to Public Treatment. In Public-Intensive blocks, the weights are reversed. The Control group consists of a third of the workers in each block.

Overall, about a third of the workers in the floor are randomized into each experimental group. Panel A of Table 1 verifies that a total of 366 workers are almost equally divided between Control (n=125), Private (n=117), and Public (n=124) treatments from the random assignments. In total, 197 of these workers are located in Public-Intensive blocks and 169 in Private-Intensive blocks.

4.2 Computing Ranks

I compute a worker's rank using average production time of sweaters in previous month. The alternative would be to compute the ranks based on workers' production earnings, the product of total pieces of sweaters the workers produce and the piece rates for those sweaters. I, however, prefer average production time for two reasons. First, compared to earnings, production time is a physical measure of productivity

[20]The workers picked a folded paper containing the numbers one, two, or three. This determined what kind of feedback they would receive. That they are choosing between different kind of feedback was explained to them.

that is not affected by piece rates of sweaters. Because piece rates vary across styles and may not accurately reflect the complexity of the styles, ranks computed based on earnings may not be completely credible to the workers.[21] Second, because production times are recorded centrally at the Knitting Section, the workers cannot use it to compute ranks among themselves when they are not meant to (e.g. in the Control group). The first month that information on actual production time is available is January 2016, based on which the first ranks are provided in February 2016.

Because different styles and sizes of sweaters take different lengths of time to be completed, it is important that ranks account for these differences. It is particularly important since the basket of styles and sizes a worker produces may not completely overlap with that of another.[22] Therefore, I compute ranks in four steps. First, for each style and size a worker worked on in previous month, I assign him a *percentile-rank* by comparing his average production time with that of all workers in his treatment group who worked on the same style and size. Next, I weight percentile-ranks of each of the styles and sizes by its share in a worker's total production in the previous month. In the third step, I add together the weighted percentile-ranks for all styles and sizes to derive a single weighted average percentile-rank for each worker. Finally, the weighted average percentile-ranks are ordered in a descending order to produce the final ranks.[23]

4.3 Data

The primary source of data is detailed administrative records obtained from the factory for all workers in the Manual Knitting Section. This data includes sweater production time for each worker for each job starting in January 2016 and covering the entire intervention period till June 2016. This information exists for about 1,200 jobs in January 2016 (baseline) and more than 22,000 jobs during February-June, 2016 (intervention period). In addition, the data also provides monthly production earnings of all the workers for several years prior to the intervention.

I use average production time of a sweater in a job as the most preferred measure of worker productivity during the intervention period. The second panel of Table 1

[21] Piece rates are determined by the management based on sweater designs.

[22] On an average, a worker works on four different styles in a month.

[23] 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.

shows that the average production time per sweater (unconditional of style and size) is well balanced across experimental groups. Columns 1-3 show the means for the experimental arms, while columns 4 and 5 show the difference in means between the control arm and each of the treatment arms. Since production time varies by sweater styles and sizes, the unconditional averages reflect differences in both worker productivity and differences in styles and sizes that the workers worked on. In the main analysis of the paper I will control for the latter differences. As an alternative measure of productivity for periods when production time is not available I use monthly production earnings averaged over a number of months 2015.[24] Table 1 shows that average production earnings is also balanced across experimental arms.

The second panel of Table 1 tests balance of worker characteristics. A few characteristics like age and tenure are observed in administrative records. A few other characteristics were collected through a baseline survey conducted in October 2015. These include the number of years the workers attended school, how risk-loving they think they are (on a scale of 1 to 10), and included a lab-in-the-field game to evaluate their attitude towards competition. The lab-in-field game asked workers to throw 10 ping-pong balls into a bucket placed 2.5 metres away from them. The workers could choose to get paid either through a fixed piece rate for each successful shot, or through a competition in which they get paid only if they scored more than a randomly chosen peer.[25] In a dummy variable, I label workers who chose the second scheme as being more competitive than those who did not. Table 1 confirms that workers across experimental arms are also balanced on available worker characteristics.

The most important component of the baseline survey was information on the workers' social network. In particular, the survey collected information on which of the peers a worker socializes with outside the factory, who they talk with during work, and how frequently they talk. The Social Network panel of Table 1 shows that the number of peers that a worker has in his block, and the total number of peers he socializes outside the factory (friends) are balanced across the experimental

[24]Depending on the context, it is averaged over either January-December 2015 or February-June 2015. It is noted accordingly. Monthly earnings averaged over a long period of time help to attenuate any noise created by differences in piece rates across styles.

[25]The rate for each successful shot in the competitive version of the game is twice that in the other as the ex-ante chance of winning the competition for a given worker is one-half. The expected pay-off is therefore same in both the schemes of payment. This game is similar to laboratory or laboratory-in-field games used in existing literature to measure an individual's competitive attitude (e.g. see Gneezy et al. (2009)).

arms. However, workers in Public Treatment have more friends in their own arm than workers in other experimental arms. This is by construction. There were eight Public-Intensive blocks as opposed to seven Private-Intensive blocks. Because Public-Intensive blocks randomize a larger share of a block into Public Treatment, and workers are more likely to be friends with peers in their blocks, workers in Public Treatment ended up having more of their friends in the same treatment.

A final set of data consists of information on cooperation across workers. In order to collect this information, ground members of the research team physically observed workers while they were working on their knitting machines. Workers were randomly selected into observation windows of 20 minutes, which were then completed at different times and days before and during the intervention period. During each of these observation windows, the team members collected information that includes who a target worker interacts and cooperates with, who initiates the interaction, who helps and who is helped, and finally, what the help involved (e.g. lending design chart, giving tips, helping with measurement etc.).

5 Productivity Response To Performance Ranks

This section presents main results on the effect of ranking on worker productivity. First, average treatment effects reveal that public ranking decreases worker productivity by more than 1.5 percentage points (p.p.) when compared to Control group. Private ranking does not change productivity on average. The negative difference between the two treatments suggests that workers reduce productivity when their ranks are made publicly known, as opposed to when they are kept private.

The workers' social network map helps to understand the source of the productivity change. First, I exploit the variation in social relationships that a worker has with peers he is ranked against. I find that the productivity drop among publicly-ranked workers is entirely driven by workers who compete with friends for ranks. Next, I exploit the variation in differences in ranks with friends. This shows that the productivity drop is driven mainly by workers who rank relatively higher within their social group. Publicly-ranked workers, who receive a rank higher than the median rank among all of their friends in the first treated month, lower productivity compared to similar but privately-ranked workers by nearly 2 p.p. on average. Moreover, the productivity drop increases as workers are ranked farther away from the median.

Workers who are ranked lower than the median do not respond to ranking.

5.1 Average Treatment Effect

I begin by estimating average treatment effects from Public and Private Treatments. To increase precision of estimates I conduct an ANCOVA analysis; I estimate differences in productivity across experimental groups in post-intervention period while controlling for pre-intervention productivity (McKenzie (2012)).

I use the following specification to estimate average treatment effects on productivity:

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

where Y_{isbt} is the logarithmic transformation of average time worker i takes to produce a sweater of style and size s in assigned job b in month t. Y_{isbt} is the key measure of productivity. $Public_i$ and $Private_i$ refer to treatment-assignment status of worker i. The coefficients of key interests are β_1 and β_2 which estimate percentage changes in productivity because of the corresponding treatments. A negative coefficient indicates higher productivity, while a positive coefficient indicates lower productivity.

α_i is worker i's *baseline productivity* i.e. productivity before the intervention. To measure baseline productivity I use average production earnings of the workers over the entire year of 2015.[26] λ_s, sweater style-and-size fixed effects, help to cleanly estimate treatment effects on productivity by isolating differences in sweater production times due to differences in sweater allocation.[27] τ_t are month fixed effects. Finally, X_{isb} are additional controls for worker, style, and job characteristics. The standard errors are clustered at block level, and bootstrapped to avoid small-cluster problem.[28]

Table 2 reports estimates of average treatment effects from the intervention. First, Column 1 estimates Equation 1 controlling for only workers' baseline productivity and style-and-size fixed effects. Interesting results emerge. Productivity of workers in

[26] Production times of sweaters are available only for one single month before the intervention making production times a less suited option to measure a worker's skill or long-term productivity.

[27] An underlying assumption is that sweater allocations were not affected by treatment status of workers.

[28] Bootstrapping is necessary as there are only 15 blocks at the factory. Alternatively, standard errors can be clustered at worker level. In Section **??** I show that clustering errors at worker level lead to similar results. I report block-clustered standard errors as default since the intensity of individual treatment assignment varies across blocks.

Private Treatment do not change on average in response to private ranking. Making ranks public, on the other hand, significantly lowers productivity; sweater production time among publicly-ranked workers increases by more than 1.5 p.p. on average. This is equivalent to an average loss of about 5 hours of production per month per worker during the entire intervention period.[29]

The estimates of productivity changes are robust to additional controls. Column 2 adds month fixed effects to the specification in Column 1. The estimates remain almost unchanged. I introduce further controls in Columns 3-4. Column 3 controls for a worker's experience with a particular sweater style and size. It is measured by cumulative number of jobs in which the worker worked the same sweater style and size (including the current job). The negative coefficient for this variable indicates learning by doing. The more jobs a worker completes for the same style and size, the less time he needs to produce a similar sweater in the next job. Column 4 adds additional controls on worker characteristics. These controls include workers' tenure at the factory (in years, as of the beginning of the intervention), the number of years they attended school, self-reported index of risk-taking behavior, and attitude towards competition. Again, the estimates remain robust to the various worker characteristics reflecting well-balanced randomization.

Figure 2 shows how the intervention affected the distribution of sweater-production times. Because sweater production times vary by style and size of sweaters, it is difficult to interpret and compare simple average of sweater production times across workers. Some styles may take longer to knit than others; bigger sizes are also likely to take longer than smaller sizes. As a result, average production time of sweaters can vary across workers because of either or both of worker productivity and allocation of styles and sizes. To account for the variation in production times because of sweater characteristics, I regress sweater production times in a given job on style-and-size fixed effects. The variation in the residuals from this regression reflect job-level variation in production time per sweater due to variation in productivity across workers.[30]

The left panel in Figure 2 shows the distribution of residuals in the pre-intervention

[29]Production times increase by about 1.8 minutes per sweater. On average, workers produce 168 pieces of sweaters during the intervention period.

[30]To be precise, the residuals reflect variation in productivity across both workers and jobs. The residuals can alternatively be estimated while controlling for both style-and-size fixed effects and job sequence, like in Column 3 of Table 2. This yields almost identical distributions.

month of January 2016.[31] Productivity distributions are identical across the three experimental arms before the workers are ranked. The right panel, on the other hand, shows the productivity distributions during the intervention period. Compared to that for the Control arm, there is a clear rightward shift of the distribution for Public Treatment. In other words there is an increase in production time, and hence a decrease in productivity, among publicly-ranked workers all along the productivity distribution curve. Mirroring results in Table 2, the distribution for Private Treatment remains largely similar to Control arm.

The difference in productivity between publicly- and privately-ranked workers is around one p.p. reduction on average. Statistically, the difference is only marginally insignificant; average p-value ≈ 0.15 across the four specifications in Table 2. Importantly, the treatments vary in visibility of ranks which in turn makes any signals that the workers intend to send more visible and prominent. Potential effects from intrinsic incentives or motivations from ranks are already accounted for by Private Treatment.

The strong response to public-ranking, in contrast to private-ranking, suggests that public nature of the ranks might have induced signaling among publicly-ranked workers. The net negative difference between the two treatments indicates that strong negative effects from signaling outweighs any potential positive effects. I now turn to exploring the source of this negative effect in the following section.

5.2 Two-Audience Signaling

Ranking introduces motivations for two-audience signaling among workers. On one hand, they may want to signal to the firm and peers that they are high productive workers. On the other hand, they may worry about being perceived as selfish or keen to distinguish themselves from their social group if they outrank (and shame) their group in ranks. They may worry that their social group or friends will impose social sanctions on them. In order to avoid this, the workers may reduce productivity, lower their ranks, and signal to their friends that they are socially compatible.

To understand if desire to signal social compatibility explains the productivity drop among publicly-ranked workers, I first distinguish between workers who have a need to send this signal and those who do not. To be specific, workers who do not

[31] As mentioned earlier, production times are available only from January 2016.

compete with anyone from their social group do not need to worry about outranking their group. They have only one audience to signal to - the audience who values high productive workers. Thus, they are likely to work harder to prove that they are high productive. Workers who do compete with their friends, on the contrary, have to simultaneously worry about the second audience - the audience who values their social compatibility. Therefore, we would expect this latter set of workers to exert less effort than the former.

Column 1 of Table 3 distinguishes between workers who do not compete with any of their friends from those who compete with at least one partly by virtue of random assignment into experimental arms. Compared to similar workers in Control arm, publicly-ranked workers who do not compete against any friends increase productivity by more than 4.5 p.p. in response to the ranking. On the other hand, workers who compete with at least one of their friends are much less productive in response. They are more than seven p.p. less productive than publicly-ranked workers who do not compete with a friend.

However, this heterogeneous response among publicly-ranked workers can be driven by underlying differences in worker characteristics. For instance, workers who are more social and thus more likely to get a friend randomized into their experimental arm may also be less productive in general.

For a causal interpretation of the results we must consider the sum of coefficients for *Public* and the interaction term. The sum estimates net causal effect of public ranks on workers who compete with their friends by comparing them with workers in Control arm who would have also competed with their friends had they been ranked. The sum reveals that publicly-ranked workers who compete with friends are not only less productive than publicly-ranked workers who do not compete with friends, they are also 2.7 p.p. less productive than similar workers in Control arm (p-value$<$0.02). Note, we now compare workers of similar characteristics, who are either ranked or not ranked by chance. Also notice, concerns about social-compatibility signals not only negate but outweigh the positive impact of productivity signals.

Comparisons of publicly-ranked workers with workers in the Control arm provides estimates for the total effect of public ranking. The difference in responses from the two experimental arms may not only be driven by signaling motivations but also intrinsic motivations from ranks. It is, therefore, more useful to compare publicly-ranked workers with privately-ranked workers to estimate the effect of sig-

naling motivations alone. This essentially exploits variation in visibility of ranks and hence visibility of signals. In the process it accounts for any potential effect from intrinsic motivations.

Column 2 repeats the exercise in Column 1 but now additionally compares privately-ranked workers to Control arm. The results indicate that privately-ranked workers who do not compete against friends increase productivity compared to Control arm, but by much less than publicly ranked workers. Importantly, workers who compete with friends do not significantly decrease productivity compared to similar workers in Control arm.

To be more concrete, Column 3 compares publicly-ranked workers with privately ranked-workers and directly estimates the effect of making ranks public.[32] Estimates confirm that publicly-ranked workers exempt from social-compatibility concerns increase productivity by two p.p. compared to similar workers who are privately-ranked instead. In other words, the incentive to produce a visible signal of high productivity had a positive effect on workers who need not worry about social-compatibility signals. In sharp contrast, publicly-ranked workers who might worry about social-compatibility signals reduce productivity by about 1.7 p.p. compared to similar but privately-ranked workers.

Is the productivity drop really driven by concerns from outranking friends? Column 4-6 investigate this question in details. First, Column 4 shows that most of the productivity drop among publicly-ranked workers happens among workers who find themselves ranked high among their friends in the first month of treatment. Workers ranked higher than the median rank within their social group reduce productivity by nearly two p.p. on average during the entire intervention. The sum of the coefficients is only marginally insignificant.[33]

Column 5 provides a more revealing test. Instead of making a binary distinction between workers who rank above or below median rank among friends, Column 5 tests the effect of distance to the median rank from either above or below. The results verify that workers are sensitive to rank-distance to the median rank but only when they are ranked above and not when they are ranked lower.[34] Moreover, size of

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

[33]To be clear, the ranks on right hand side of the regressions are from the first treatment letter that the workers receive at the very beginning of the treatment period (February 2016). The treatment effects are estimates of productivity changes during the entire intervention period that follows the letter.

[34]The difference in the estimated effects of rank-distance from above and below is statistically

the productivity drop increases with rank-distance. The evidence is consistent with higher ranks being stigmatized within a social group, while there is no stigma to lower ranks. Finally, to ensure that the results in Column 5 is not driven by small cell sizes, Column 6 estimates the effect of rank-distance to median rank on workers who are ranked higher than the median, by comparing them to workers who are ranked at the median, ranked lower, or do not compete against friends. Reassuringly, we find a similar effect of the rank-distance.

The specifications in Columns 4-6 assume that workers care about being close to the median rank within their social group. Although this is the traditional way to test conformism in the literature, it requires an assumption that workers want to conform to the *median*. Note that the assumption is innocuous since distance to median is positively correlated with distance to any other location in ranks (e.g. the very bottom rank among friends). Nonetheless, I test two alternative specifications that do not require this assumption. First, Column 1 in Table A1 tests how workers respond to *average* rank-distance with friends. The higher they are ranked within their social group, the higher would be this average rank-distance. The alternative measurement yields similar result - workers reduce productivity when they are ranked relatively higher than their friends. To test asymmetric response to rank-distance with friends, Column 2 estimates workers' response to rank-distance with friends they outrank and with friends who outrank them. Reassuringly, the results confirm that workers respond only to rank-distance with friends who are ranked lower than them.

Public ranks introduce two contradicting signaling options for the workers. Induced by motivations to signal productivity to their firm and peers, workers increase productivity. But this happens only among workers who do not compete with any of their friends. Workers who compete with friends, on the other hand, reduce productivity to signal social compatibility to their friends. In particular, these workers reduce productivity when they outrank at least half of their friends in the first treated month. Interestingly, not increasing productivity should have been sufficient for the latter workers to avoid being perceived as socially incompatible. After all, the ranks in the first treatment letter reflects the status-quo on productivity that existed prior to the intervention. But in reality, the workers go further and reduce productivity which suggests the workers had strong social concerns about proving compatibility to their social group.

significant at 5% significance level.

6 Robustness Tests

In this Section I conduct several robustness tests to verify that the productivity drop among publicly ranked workers is indeed in response to getting ranked higher than their friends. First, a series of placebo tests confirm that (a) productivity among publicly-ranked workers fall only after ranking starts (b) it is triggered by making ranks public, and (c) it does not happen when friends they do not compete with (i.e. friends in Private Treatment) are ranked lower. In other words, the evidence is consistent with the intervention triggering a two-audience signaling problem and the workers responding accordingly. The next set of results rule out concerns that the productivity drop is driven by workers' relative productivity in general, and establish that the productivity drop is network-specific. The drop is also not explained by underlying differences in several other worker characteristics. All together, these tests suggest that the workers reduce productivity in response to getting ranked better than their friends publicly.

6.1 Placebo Tests

I begin by testing if the heterogeneity in productivity among workers publicly ranked higher and lower within their social group precedes the intervention. To be specific, Column 1 of Table 4 tests if workers with a public rank higher than the median among their friends (in February 2016) have lower productivity than the rest of publicly-ranked workers in the pre-intervention month of January 2016. This is the same specification as Column 6 of Table 3 but with pre-intervention production data.[35] Reassuringly, the results indicate that there is no association between distance to median rank among friends and productivity in pre-intervention period. This is in stark contrast to the strong negative association as seen earlier in Table 3.

Column 1 compares pre- and post-intervention periods using the same outcome variable (production times). A caveat, however, is that the ranks that workers receive in February 2016 and are used on the right-hand side of the specification in Column 1, are computed from production times in January 2016. Although the rank-distance computed on the right-hand side are those within a worker's social group, and not overall ranks, there is an element of mechanical correlation between

[35]Because production time of sweaters are available only from January 2016, this restricts us to only one month of pre-intervention data. Hence, I exclude month fixed effect from this specification.

left- and right-hand sides of the specification. Nonetheless, we do not observe any positive association between rank-distance to friends and production times, in the pre-intervention data as we do in post-intervention data. If anything, the association is negative reflecting positive correlation between higher productivity and favorable rank-distance to friends prior to the intervention.

Column 2 uses production earnings from a year predating the intervention as an alternative measure of pre-intervention productivity. This solves the problem of mechanical correlation discussed in the previous paragraph. In particular, Column 2 runs the same specification as Column 1 but on monthly production earnings over February-June 2015.[36] Note, February-June 2015 match the calendar months of the intervention period (February-June 2016). This eliminates the risk of seasonality invalidating our comparison, in case production differs across the experimental arms in different months of the year.

Column 2 reaffirms the findings in Column 1. Even with an older and longer set of pre-intervention months there is again no negative association between productivity and better ranks among friends. Column 3 tests the same specification but with monthly production earnings from the post-intervention months. There now appears to be a stronger negative association from the post-intervention months, mirroring results from Table 3. Note, since the outcome variable is production earnings, a negative coefficient indicates lower productivity. The estimate in Column 3 is, however, noisier than the estimates Table 3, as the total number of observations is lower at month level than at job level.

A second placebo test verifies that productivity drops among better-ranked friends only when ranks are made public. To do so, Column 4 compares publicly- and privately- ranked workers against counter-factual workers in Control group. This is a similar test as Column 2 of Table 3 but now focusing on behavior of better-ranked friends. Column 3 confirms that privately-ranked workers do not respond to distance above median rank among friends in the way that publicly-ranked workers do.

A final placebo test shows that publicly-ranked workers are sensitive to outranking friends only when they directly compete with them. In the two-audience signaling problem, workers' social concerns stem from outranking friends they compete against.

[36] As a result, baseline productivity is estimated using production earnings from September-December 2014 instead of January-December 2015.

If that is true, workers should not respond to differences in ranks with friends they do not compete with and hence do not outrank. Therefore, Column 5 tests how publicly-ranked workers respond to distance above median rank *among friends in Private Treatment*. The interaction term is small and insignificant. Column 5 adds rank-distance with competing friends as a control. But the results remain the same. Estimates in Columns 4 and 5 confirm that publicly ranked workers do not respond to rank-distance with "outranked" friends in Private Treatment.

6.2 Endogenous Worker Characteristics

The negative association between productivity and better ranks among friends can be prone to alternative interpretations. The heterogeneous productivity response among worse and better ranked friends could be driven by underlying worker characteristics that is unrelated to signaling social compatibility. I conduct a number of robustness tests in Table 5 to assuage these concerns.

A primary concern relates to underlying workers characteristics that determine which workers have only one audience to signal to and those who have two. To be concrete, a worker has a second audience and therefore needs to signal social compatibility only when he competes with friends. In turn, whether a worker competes with friends depends on both a random chance that his friends were assigned into the same experimental arm, and how many friends a worker has to begin with. The more friends he has, the more likely it is that some of them are assigned into the same experimental arm.

Table 5 presents a number of tests to check if the aforementioned endogeneity drives our results. First, Column 1 restricts sample to workers who have at least one friend randomly assigned into the same experimental arm. It then re-estimates the linear relationship between distance to median rank (from above) among those competing friends and post-intervention productivity. Reassuringly, the negative association between such rank-distance and productivity is similar to our previous estimate, suggesting that our estimate is not driven by endogenous selection of workers playing to one- or two-audience.

Column 2 uses full sample of workers instead and controls for the total number of friends a worker reports to have in the baseline survey. It also lets this number affect publicly-ranked workers differently than it does privately-ranked workers. As

mentioned earlier, the more friends a worker has the more likely he competes with some of them. Again, our main estimate remains robust in size and precision. Finally, Column 3 controls for the total number of friends a worker competes against. It is a test similar in spirit to the previous test but controls for the actual number of friends a worker competes with. The estimate of productivity drop remains robust. Columns 2-3 reconfirm that social popularity of workers by itself does not drive the productivity drop we observe among publicly-ranked workers.

A second concern is that rank-distance with friends is correlated with a worker's own productivity. It is possible that better public ranks led high productive workers to become less productive for reasons unrelated to signaling social compatibility, such as complacency. Therefore, in Column 4 of Table 5 I let baseline productivity affect publicly-ranked workers differently than privately-ranked workers. In particular, I control for an interaction of workers' baseline productivity and workers' treatment assignment. The estimate of productivity drop among better-ranked friends remains largely unchanged.

Column 5 presents an alternative test. If publicly-ranked workers reduce productivity because they are ranked high in general and not because they are ranked high within their social group in particular, we should observe similar drops in productivity when the difference in their ranks and that of non-friend peers increases. Column 5 tests this. The estimates, however, suggests that publicly-ranked workers are not sensitive to rank-distance with peers they are not friends with. The coefficient is close to zero and insignificant. Column 6 simultaneously tests sensitivity to rank-distance with both friends and non-friends. While we see a strong negative response to higher rank-distance with friends, there is no such association with peers workers are not friends with; if anything there appears to be an inverse relationship.

Figure 3 plots a worker's baseline productivity against average baseline productivity of his friends.[37] Although there is some variation, the figure suggests that there is a positive correlation between productivity of a worker and that of his social group. High productive workers are friends with other high productive workers, and similarly for low productive workers. This positive correlation implies that it is not only the high productive workers who find themselves ranked better than their friends; relatively low productive workers may find themselves in a similar situation. In other words, workers across the whole productivity distribution may find themselves out-

[37] As before, baseline productivity is average monthly production earnings of workers in 2015.

ranking (at least some of) their friends. This also explains why there is not a strong correlation between productivity in general and rank among friends.

Column 7 controls for both baseline productivity and social popularity of worker at the same time. Our main estimate remains robust. Finally, to assuage concerns that the main estimate is driven by other underlying worker characteristics, Column 8 controls for additional observed worker characteristics and their interactions with *Public*. These controls include tenure at factory, total number of years a worker attended school, attitude towards competition, and a self-reported index (on a scale of 1 to 10) of how risk-loving a worker is. Again, our key estimate remains robust, although slightly attenuated.

6.3 Alternative Clustering of Standard Errors

The results presented so far in the paper use standard errors clustered at block level. An alternative possibility is that the standard errors are correlated at worker level.

In Table A2 I re-estimate the key specifications from Tables 2 and 3 clustering standard errors at worker level instead of blocks. Column 1 shows the estimates of average treatment effects from Table 2. Column 2 tests how workers, in both Public and Private Treatments, behave in response to competing with at least one friend, when compared to similar workers in Control arm. Column 3 estimates a similar specification but with a direct comparison of Public and Private Treatments. Finally, Columns 4-5 test how publicly-ranked workers respond to distance to median rank among their friends. All the results remain robust and yield similar statistical precision when standard errors are clustered at worker level.

6.4 Spillover Effect on Privately-Ranked Workers

Because workers were randomly assigned to experimental arms at an individual level, and workers work at close proximity to each other, a legitimate concern is about spillover effects across experimental arms. Because we compare publicly-ranked workers with privately-ranked workers for most of our analysis, we may be particularly concerned about spillover effect on the latter. For instance, we may be concerned that privately-ranked workers may share information about their ranks with their peers to imitate publicly-ranked workers, and as a result behave more like publicly-ranked workers. Note, ex ante, this would only make it more difficult to find a difference in

treatment effects between publicly- and privately-ranked workers ex post. As such, the treatment effects estimated in earlier tables are likely to be lower-bound estimates of the true effect.

However, it would be a concern if there were positive spillover effects from publicly-ranked workers to privately-ranked workers. In other words, if privately-ranked workers increase productivity because they work beside publicly-ranked workers, we would be over-estimating the productivity drop among publicly-ranked workers in response to ranking. To test precisely this concern, the experimental design instilled a random variation in the density of publicly-ranked workers within each block. Eight of the 15 blocks were randomly selected to have a higher share of workers assigned to Public Treatment (*Public-Intensive Blocks*), while the remaining seven would have a higher share assigned to Private Treatment (*Private-Intensive Blocks*). The Control arm had a third of the workers in each block.[38] If there were spillover effects from Public Treatment to Private Treatment, we would be able to test this by comparing responses of privately-ranked workers when they have a high number of publicly-ranked peers around them and that when relatively less of their peers are publicly-ranked.

Column 1 of Table A3 estimates average treatment effect on privately-ranked workers while distinguishing between workers in Private- and Public-Intensive Blocks. The counterfactual workers in this test are the Control arm workers. A positive but highly insignificant coefficient for the interaction term of Private and an indicator variable for Public-Intensive Block suggests that privately-ranked workers in Public-Intensive Blocks behave almost similarly to those in Private-Intensive Blocks. If anything, privately-ranked workers with relatively higher number of publicly-ranked workers in their blocks reduce productivity just like the latter. A positive coefficient for the indicator variable reflects a similar behavior among Control arm workers in Public-Intensive Blocks. Overall, Column 1 suggests that there are no strong spillover effects from Public Treatment to the other arms. If anything, having more publicly-ranked workers around led to an overall decrease in productivity among privately-ranked workers, which only attenuates the differences between publicly- and privately-ranked workers.

Column 2 and 3 test if privately-ranked workers in the two types of blocks respond differently to rank-distance to median rank among friends - the key behavior that drives productivity differences among privately- and publicly-ranked workers. The

[38] See Section 4.1 for more details about the experimental design.

evidence suggest otherwise. Compared to similar workers in Control arm, privately-ranked workers in neither Private- nor Public-Intensive Blocks react to rank-distances with friends. In fact, the estimated coefficients in the two sets of blocks are very low and similar in magnitude to each other. Again, this points against any spillover effect of Public Treatment on privately-ranked workers.

7 Tests Of Mechanism

Having established that workers reduce productivity in response to publicly outranking their friends, we now turn to investigating the reason behind it. Absence of such behavior among privately-ranked workers point towards concerns about perceived social compatibility as the underlying reason. Workers may worry that publicly outperforming their friends, and perhaps shaming them in the process, may make them look selfish and socially incompatible and lead to social sanctions from their friends.

There are two reasons why workers may fear social sanctions. First, a perceived attempt at differentiating themselves from their friends ("acting white") in terms of productivity may offend their friends. A second but somewhat related concern is that of imposing a direct and negative externality on their friends by suppressing their ranks. Because ranking is a zero-sum game, a worker ranked higher means someone else is ranked lower. Workers may fear that imposing this negative externality on their friends will make them look selfish. In either of these cases, relatively higher ranked workers among their friends would reduce productivity to signal that they are not selfish or not trying to stand out from their social group; in other words, workers would reduce productivity to signal social compatibility.

I exploit variation in the composition of a worker's network to distinguish between the two mechanisms. Using variation in the rank-location of friends, relative to a worker's own, I show that workers reduce productivity only more of their friends are located farther down from them in ranks. On the other hand, the workers increase productivity when more of their friends are located closer to them, precisely when a reduction in productivity could improve their friends' ranks. Also, the reduced productivity among publicly-ranked workers does not translate into better ranks of their friends. All the evidence suggest that workers are not concerned about the externality their effort imposes on their friends but are rather worried about ranking too far better than their friends. It is this latter concern that makes them reduce

their effort and productivity and *conform* to the ranks of their friends.

7.1 Conformity vs Externality

To understand whether publicly-ranked workers reduce effort to conform to the ranks of their friends or to internalize the negative externality they impose on them, I exploit composition of a worker's network. A worker imposes negative externality on a friend only when he suppresses his friend's rank. This is more plausible when his friend is ranked close enough that marginal changes in effort can affect the friend's rank. In other words, if a worker is concerned about imposing negative externality on his friends he would be concerned more when he has friends ranked lower and close to his own rank. On the contrary, if a worker is concerned about being perceived as distinguishing himself from his group, he would be more concerned when his rank is better than his friends by a substantial margin. I take cue from this observation and exploit the composition of a worker's social group to understand which of the two mechanisms is at play.

To be specific, Figure 4 plots how publicly-ranked workers respond when friends they outrank are ranked closer to them as opposed to farther. The figure plots productivity changes among publicly-ranked workers, compared to privately-ranked workers, when they outrank a friend by a rank-distance of 0-20, 20-40, 40-60, or 60 and above. Interestingly, a publicly-ranked worker does not decrease productivity when he outranks an additional friend by a close margin - precisely when he could have affected his friend's rank relatively easily. If anything, having additional friends ranked lower but within 20 rank-distances tends to increase productivity. On the other hand, as he outranks more and more friends by a bigger margin, beyond 40 ranks to be precise, he starts to respond by decreasing his productivity.

Column 1 of Table 6 presents the same results but aggregating outranked friends into only two groups - friends ranked either within a distance of 40 ranks or at a distance farther away. In line with Figure 4, most of the productivity drop happens when workers have friends ranked far away from them. Column 2 additionally controls for the number of peers a worker has in total. This ensures that we exploit changes in number of friends in the two bins while holding total number of friends constant. We get virtually similar results.

Columns 1-2 suggests that workers reduce productivity only when they outrank

their friends by big margins. The fact that they do not reduce productivity when they compete neck-to-neck with their friends suggests that workers may not care about the externality they impose on their friends. But in principle, reduction in productivity when outranking friends by big margins could also be consistent with internalizing externality. When they outrank their friends by big margins, they would have to reduce their productivity by big margins to overturn rank-differences with their friends.

Column 3 therefore tests whether the reduced productivity shown in Columns 1-2 translate into more friends getting ranked higher. The outcome variable is the difference between number of friends who get ranked lower in each treated month and the number of friends who got ranked lower in the very first treated month (February 2016).[39] Taking the difference computes within-worker changes in the number of outranked friends (baseline versus later months) and allows us to control for cross-sectional differences in the number of outranked friends. The results in Column 3 suggest that productivity drop did not in fact translate into better relative ranks for friends on an average.

Nonetheless, the reduced productivity did translate into lower ranks for the workers. Column 4 shows that the reduction in productivity with each additional outranked friend at rank-distance higher than 40 leads to a drop in rank-percentile by about 5 percentiles. I use rank-percentile instead of actual ranks as the outcome variable since, for the same rank-percentile, actual ranks can differ because of differences in size of experimental arms (see Table 1). Anderson's Sharpened Q-values, reported at the end of the column, show that this result is robust to multiple hypothesis testing (Anderson (2008)).

Finally, Column 5 shows that lower productivity and lower rank-percentiles did lead to lower average rank-distance with friends during the intervention period. The outcome variable is average rank-distance with all competing friends in a given treated month. Although a little noisy, the estimates show that each additional friend ranked lower and farther away leads to a decrease in rank-distance with all friends.

In summary,the evidence in Table 6 is consistent with workers reducing their productivity only when they are ranked distant from their social group. Because similar response does not occur when friends are ranked lower but close, combined

[39]This includes the ranks computed based on production in June 2016. Hence, we have five months' of data as we did with productivity as outcome.

with the fact that the workers do not reduce productivity enough to go below their friends' ranks suggest that the productivity drop among publicly-ranked workers is not driven by concerns about externality. Furthermore, the productivity drop do translate into lower ranks and hence lower rank-distance with friends. This again supports the hypothesis that workers reduce effort to merely lessen the gap with in ranks with their friends.

7.2 Alternative Explanations

It is difficult to imagine many alternative explanations of the results that fit all the evidence presented so far. Nonetheless, there remains a few that require careful consideration.

A prime concern stems from the fact that Public and Private Treatments vary not only by making a worker's rank known to others, but also by making others' ranks known to him. A plausible alternative explanation, therefore, is that the drop in productivity among publicly-ranked workers is driven by new information about *peers'* relative productivity. It is plausible that upon knowing peers' ranks, the relatively high productive friends realize that they are doing unexpectedly better than their social group. More generally, complacency from getting a better-than-expected rank may lead to resting on their laurels.

If workers were complacent from positive surprises after learning their own ranks (irrespective of their peers') this behavior would also be present among privately-ranked workers. Since we take the difference in responses between publicly- and privately- ranked workers, we account for any effect from updating one's belief about his own rank. Nonetheless, Column 1 of Table 7 controls for the difference between the workers' prior beliefs about their rank and the actual rank that they received at the beginning of the intervention. The former information was collected during baseline survey. In particular, I control for a binary variable that identifies whether a worker received a rank better than his prior belief, and let this affect publicly- and privately-ranked workers in different ways. Although there is an additional effect of the positive surprise on publicly-ranked workers, our main coefficient of interest remains robust.

Next, I test the effect of new information about peers' ranks. During the baseline survey workers made a comparison of their productivity with respect to each of the

other workers in their blocks. This helps to compute the number of friends a worker had predicted to be relatively more productive at baseline, but later found out otherwise from the ranks. The more comparisons a worker got wrong during the baseline survey, more is the new information that he receives from public ranks. Column 2 in Table 7 controls for this new information. Our key estimation of effect from distance to median rank remains robust. However, in line with the results in Table 6, workers may rest on their laurels only when their friends are ranked really far away. Column 3 in Table 7 retests the specification of Column 1 of Table 6 but controlling for new information from ranks. Again, we find that our previous estimates are unaffected.

Alternatively, is it possible that workers colluded between each other to reduce productivity? One reason why it would be of general interest to reduce productivity is to bargain for higher piece rates for the sweaters. This is known as ratchet effect. Making ranks public would certainly make such collusion easier. However, if workers reduced productivity to affect piece rates we are likely to observe this drop only among the most productive workers. Instead, Figure 2 and Table 5 document a drop all along the productivity distribution. Furthermore, publicly-ranked workers respond negatively only when they are ranked higher within their social group, and are not sensitive to outranking peers they are not friends with. If ratchet effect were at play it is unlikely to depend on social relationships.

Nonetheless, social relationships may be linked with ratchet effect in that it might be easier to coordinate and enforce collusion within social groups. For instance easier communication among friends may help to verify and enforce collusion. To test if that is the case, I check how workers respond to outranking peers that they are not friends with, but they often interact with during work. If communication with social group is the underlying reason that drives the productivity drop we would expect to observe somewhat similar patterns with peers workers often interacts with. Therefore, Column 4 of Table 7 computes distance to median rank among such peers, and finds no negative effect from that distance when workers rank higher than this median. Column 5 re-introduces rank-distance with friends in this specification. The original key estimate remains robust, whereas the coefficient for rank-distance to non-friend peers a worker frequently interacts with becomes further attenuated.

Finally, altruism is also an unlikely underlying mechanism driving our results. Altruism in this context would reflect workers feeling bad about their outranked peers. However, if altruism were the driving mechanism it would affect the workers

more when they impose a negative externality on their friends by suppressing the friends' ranks. Results in Table 6 are inconsistent with this pattern.

8 Cooperation Among Workers

We have so far explored the effect of ranking on worker productivity. But it is conceivable that ranking may affect other outcomes that may also be of interest to the firm. One such outcome that can be strongly affected by competition among workers is social cohesion.

To understand whether competition among workers affect social cohesion among workers, I test how cooperation among workers change because of the intervention. Ground members of the research team observed and recorded instances of cooperation among the knitting workers starting from shortly before the intervention. Workers were randomly selected into 20-minute windows during which a research team member observed the selected workers and their interactions with their peers. The member observed the workers from a distance in order not to affect the workers' behavior. If a worker engaged into an interaction with a peer in order to either provide help or receive help regarding sweater production, it is recorded as an instance of cooperation.

Figure 5 plots the probability that a worker engaged in at least one instance of cooperation with his peers during a 20-minute slot in which he was observed. An underlying linear probability model predicts the probabilities conditional on the treatment assignment of the workers. The left panel in the figure reports the estimated likelihood of cooperation during the pre-intervention period while the right panel does the same for post-intervention period.

The left panel in Figure 5 shows that there was about 15% chance that a worker in any experimental group would be engaged in at least one instance of cooperation during an observed slot. The probabilities are computed from a total of 458 observed slots observed during late December 2015 and January 2016. Although there is some variation in estimated likelihood of cooperation across the experimental arms the differences are statistically highly insignificant.

The right panel stands in stark contrast. The ranking clearly had an effect on cooperation among workers. There are two patterns in the data that deserves a note. First, the intervention reduces likelihood of cooperation among workers by at least half for any experimental group, *including the Control arm*. The reduction of

cooperation among Control group workers is particularly interesting, as it implies that cooperation fell among all workers and not just within those engaged in competition for ranks. One explanation for this pattern might be that treated workers reduce cooperation in general (and not only with other competing workers) and thus they cooperate less with Control workers too. In any case, competition on the work floor seems to have undermined cooperation among workers.

A before-after comparison of cooperative behavior is prone to seasonality effects. Differences across experimental arms in the post-intervention period, however, can shed further light on the effect of the intervention while holding time-effects constant.

The right panel in Figure 5 illustrates the effect of the intervention on the workers who compete on ranks. The probability that a publicly-ranked worker engages in at least one instance of cooperation is nearly half of that for a Control group worker in the post-intervention period. The difference is statistically significant at 5% significance level. Privately-ranked workers also show a decline but not by as much as publicly ranked workers do. The estimates are derived from a total of 1039 observations made during the treated months. It is important to note that the reduction in cooperation induced by ranking intervention is likely to be a lower-bound of the true effect as cooperation among Control-arm workers were also clearly reduced because of the intervention.

Evidence from cooperative behavior among workers suggest that the social cohesion between workers might have been adversely affected by making workers compete with one another. It is surprising as, on the other hand, workers sacrifice productivity, and hence part of their income, to appear socially compatible in the post-intervention months. It is possible in principle that workers still help their friends as much as they did before (or even more), but reduce cooperation with other peers. In other words, workers might redistribute help from non-friend peers to friends. Insufficient frequency of cooperation before the intervention combined with a further reduction in cooperation among publicly-ranked workers in the post- intervention period make it difficult to probe this any further.

9 Conclusion

Existing literature suggests that status incentives, in the form of performance-based ranks, can increase worker productivity. However, the evidence in this paper indicates

that instead of responding to such incentives workers reduce productivity to conform to the level of productivity of their social groups. A novel experimental design with private and public ranking, along with detailed baseline data on workers' social network, help to show that social conformity can strongly counteract the positive effects of status motivation.

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

References

Ager, Philipp, Leonardo Bursztyn, Lukas Leucht, and Joachim Voth, "Killer Incentives: Rivalry, Performance and Risk-Taking Among German Fighter Pilots, 1939-45," *Review of Economic Studies*, forthcoming.

Akerlof, George A, "Social Distance and Social Decisions," *Econometrica*, 1997, *65* (3), 1005–1027.

_ **and Rachel E Kranton**, "Economics and Identity," *The Quarterly Journal of Economics*, 2000, *115* (3), 715–753.

Akerlof, Robert, ""We Thinking"and Its Consequences," *American Economic Review: Papers and Proceedings*, 2016, *106(5)*, 415–419.

_ **, Anik Ashraf, Rocco Macchiavello, and Atonu Rabbani**, "Layoffs and Productivity at a Bangladeshi Sweater Factory," *Working Paper*, 2021.

Anderson, Michael L., "Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects," *Journal of the American Statistical Association*, 2008.

Ashraf, Nava and Oriana Bandiera, "Social Incentives in Organizations," *Annual Review of Economics*, 2018, *10*, 439–463.

_ **,** _ **, and Kelsey Jack**, "No Margin, No Mission? A Field Experiment on Incentives for Public Services Delivery," *Journal of Public Economics*, 2014b, *120*, 1–17.

_ , _ , **and Scott S. Lee**, "Awards Unbundled: Evidence from a Natural Field Experiment," *Journal of Economic Behavior and Organization*, 2014a, *100*, 44–63.

Austen-Smith, David and Roland G. Fryer, "An Economic Analysis of Acting White," *The Quarterly Journal of Economics*, 2005, *120*, 551–583.

Azmat, Ghazala and Nagore Iriberri, "The Importance of Relative Performance Feedback Information: Evidence from a Natural Experiment Using High School Students," *Journal of Public Economics*, 2010, *94*, 435–452.

Bagwell, Laurie Simon and B. Douglas Bernheim, "Veblen Effects in a Theory of Conspicuous Consumption," *The American Economic Review*, 1996, *86* (3), 349–373.

Bandiera, Oriana, Iwan Barankay, and Imran Rasul, "Social Preferences and the Response to Incentives: Evidence from Personnel Data," *The Quarterly Journal of Economics*, 2005, *120* (3), 917–962.

_ , _ , **and** _ , "Social Connections and Incentives in the Workplace: Evidence from Personnel Data," *Econometrica*, 2009, *77* (4), 1047–1094.

Barankay, Iwan, "Rankings and Social Tournaments: Evidence from a Crowd Sourcing Experiment," *Working Paper*, 2011.

_ , "Rank Incentives: Evidence from a Randomized Workplace Experiment," *Management Science (Revise and Resubmit)*, 2012.

Benabou, Roland and Jean Tirole, "Intrinsic and Extrinsic Motivation," *Review of Economic Studies*, 2003, *70*, 489–520.

Bernheim, Douglas B., "A Theory of Conformity," *Journal of Political Economy*, 1994, *102* (5), 841–877.

Besley, Timothy and Maitreesh Ghatak, "Status Incentives," *American Economic Review*, 2008, *98* (2), 206–211.

Blader, Steven, Claudine Gartenberg, and Andrea Prat, "The Contingent Effect of Management Practices," *Review of Economic Studies*, 2020, *87* (2), 721–749.

Blanes-I-Vidal, Jordi and Mareike Nossol, "Tournaments without Prizes: Evidence from Personnel Records," *Management Science*, 2011, *57* (10), 1721–1736.

Breza, Emily, Supreet Kaur, and Yogita Shamdasani, "The Morale Effects of Pay Inequality," *The Quarterly Journal of Economics*, 2015, *133* (2), 611–663.

Bursztyn, Leonardo and Robert Jensen, "How Does Peer Pressure Affect Educational Investments?," *The Quarterly Journal of Economics*, 2015, *130* (3), 1329–1367.

_ , Bruno Ferman, Stefano Fiorin, Martin Kanz, and Gautam Rao, "Status Goods: Experimental Evidence from Platinum Credit Cards," *The Quarterly Journal of Economics*, 2018, *133* (3), 1561–1595.

_ , Georgy Egorov, and Robert Jensen, "Cool to be Smart or Smart to be Cool? Understanding Peer Pressure in Education," *Review of Economic Studies*, 2019, *86* (4), 1487–1526.

Card, David, Alexandre Mas, Enrico Moretti, and Emmanuel Saez, "Inequality at Work: The Effect of Peer Salaries on Job Satisfaction," *American Economic Review*, 2012, *102* (6), 2981–3003.

Casaburi, Lorenzo and Rocco Macchiavello, "Loyalty, Exit and Enforcement: Evidence from a Kenya Dairy Cooperative," *American Economic Review Papers Proceedings*, 2015.

Charles, Kerwin Kofi, Erik Hurst, and Nikolai Roussanov, "Conspicuous Consumption and Race," *The Quarterly Journal of Economics*, 2009, *124* (2), 425–467.

Chetty, Raj, Mushfiq Mobarak, and Monica Singhal, "Increasing Tax Compliance through Social Recognition," *Working Paper*, 2015.

Culen, Zoe and Ricardo Perez-Truglia, "How Much Does Your Boss Make? The Effects of Salary Comparisons," *Journal of Political Economy*, 2021.

Delfgaauw, Josse, Robert Dur, Joeri Sol, and Willem Verbeke, "Tournament Incentives in the Field: Gender Differences in the Workplace," *Journal of Labor Economics*, 2013, *31* (2), 305–326.

DellaVigna, Stefano, John A. List, Ulrike Malmendier, and Gautam Rao, "Estimating social preferences and gift exchange at work," *Working Paper*, 2019.

Englmaier, Florian, Dominik Grothe, Stefan Grimm, David Schindler, and Simeon Schudy, "Tournament Incentives and Team Performance in Non-Routine Analytical Team Tasks," *Working Paper*, 2021.

Fryer, Roland G., "A Model of Social Interactions and Endogenous Poverty Traps," *Rationality and Society*, 2007, *19*, 335–366.

Gaspart, Frédéric and Erika Seki, "Cooperation, Status Seeking and Competitive Behaviour: Theory and Evidence," *Journal of Economic Behavior and Organization*, 2003, *51* (1), 51–77.

Gill, David, Zdenka Kissová, Jaesun Lee, and Victoria Prowse, *First-Place Loving and Last-Place Loathing: How Rank in the Distribution of Performance Affects Effort Provision*, Vol. 65 2019.

Gneezy, Uri and John A. List, "Putting behavioral economics to work: testing for gift exchange in labor markets using field experiments," *Econometrica*, 2006, *74*, 1365–84.

_ , Kenneth L. Leonard, and John A. List, "Gender Differences in Competition: Evidence From a Matrilineal and a Patriarchal Society," *Econometrica*, 2009, *77* (5), 1637–1664.

Green, Jerry R. and Nancy L. Stokey, "A Compariosn of Tournaments and Contracts," *Journal of Political Economy*, 1983, *91* (3), 349–364.

Hamilton, Barton H., Jack A. Nickerson, and Hideo Owan, "Team Incentives and Worker Heterogeneity: An Empirical Analysis of the Impact of Teams on Productivity and Participation," *Journal of Political Economy*, 2003, *111*, 465–497.

Holmstrom, Bengt, "Managerial Incentive Problems: A Dynamic Perspective," *The Review of Economic Studies*, 1999, *66* (1), 169–182.

Jayaraman, Rajshri, Francis de Vericourt, and Debraj Ray, "Anatomy of a Contract Change," *American Economic Review*, 2016, *106*, 316–358.

Kluger, Avraham N. and Angelo DeNisi, "The Effects of Feedback Interventions on Performance: A Historical Review, a Meta-Analysis, and a Preliminary Feedback Intervention Theory," *Psychological Bulletin*, 1996, *119* (2), 254–284.

Kuhnen, M. Camelia and Agnieszka Tymula, "Feedback, Self-Esteem, and Performance in Organizations," *Management Science*, 2012, *58* (1), 94–113.

Lazear, Edward P. and Sherwin Rosen, "Rank-Order Tournaments as Optimum Labor Contracts," *Journal of Political Economy*, 1981, *89* (5), 841–864.

Mas, Alexandre and Enrico Moretti, "Peers at Work," *American Economic Review*, 2009, *99* (1), 112–145.

McKenzie, David, "Beyond Baseline and Follow-up: The Case for More T in Experiments," *Journal of Development Economics*, 2012, *99*, 210–221.

Moldovanu, Benny, Aner Sela, and Xianwen Shi, "Contests for Status," *Journal of Political Economy*, 2007, *115* (2), 338–363.

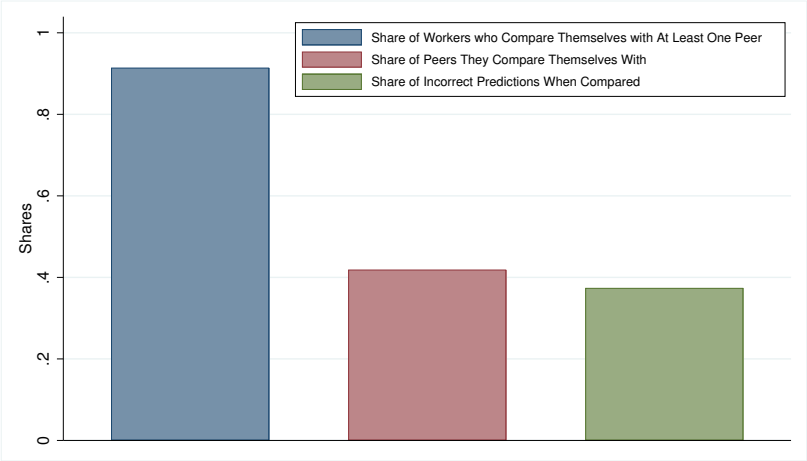
Perez-Truglia, Ricardo, "The Effects of Income Transparency on Well-Being: Evidence from a Natural Experiment," *American Economic Review*, 2020, *110* (4), 1019–54.

Rege, Mari, "Why Do People Care About Social Status?," *Journal of Economic Behavior and Organization*, 2008, *66*, 233–242.

Song, Hummy, Anita L. Tucker, Karen L. Murrell, and David R. Vinson, "Closing the Productivity Gap: Improving Worker Productivity Through Public Relative Performance Feedback and Validation of Best Practices," *Management Science*, 2017.

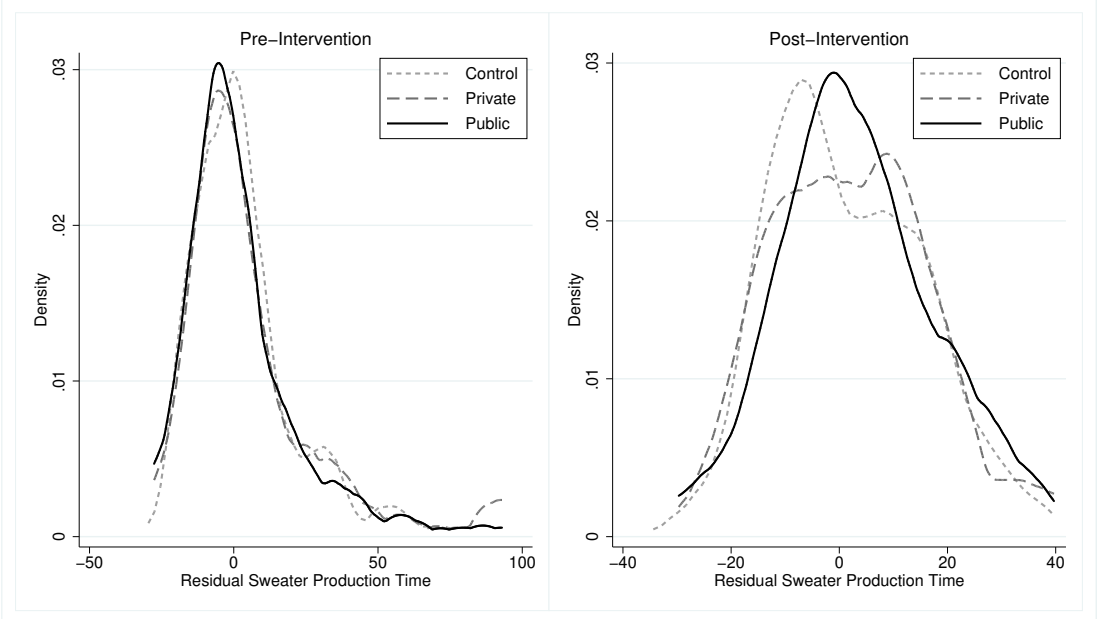
World Management Survey, "2010 Manufacturing Survey," *Questionnaire*, 2010.

Figure 1: Workers Compare Productivity



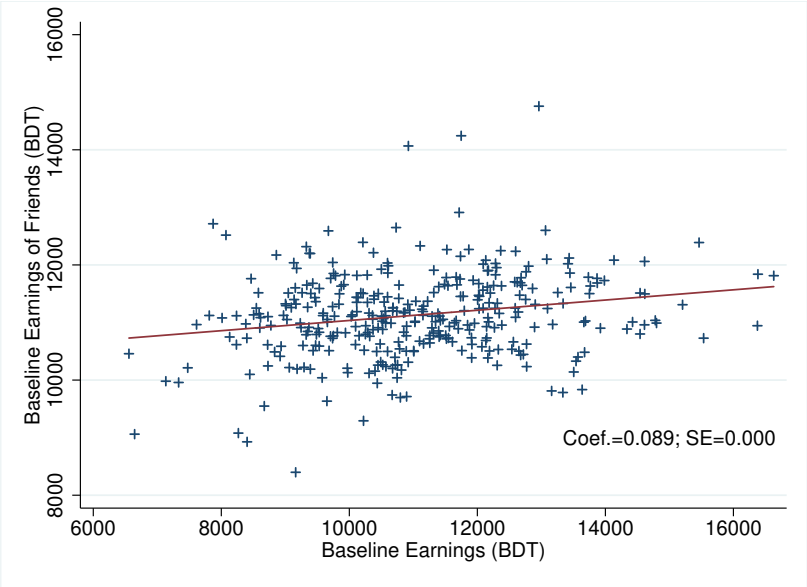
Note: The figure shows that it is common for knitting workers to compare their productivity with their peers'. It reports worker responses from a baseline survey, which asked workers if they compared themselves with their peers in terms of productivity, and their predictions about relative productivity with those peers.

Figure 2: Average Production Time Per Sweater



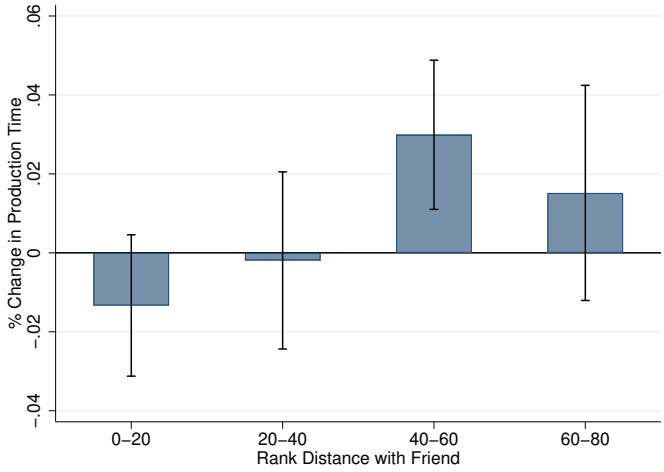
Note: The figure reports residuals from regression of average knitting time of sweaters while controlling for style-and-size fixed effects. The residuals reflect style-and-size adjusted variation in production times of sweaters across workers and jobs. Left panel in the figure shows distribution of the residuals for each experimental arm in the pre-intervention month of January 2016. Right panel shows the same for post-intervention months of February-June 2016.

Figure 3: Average Production Earnings of Workers and their Friends



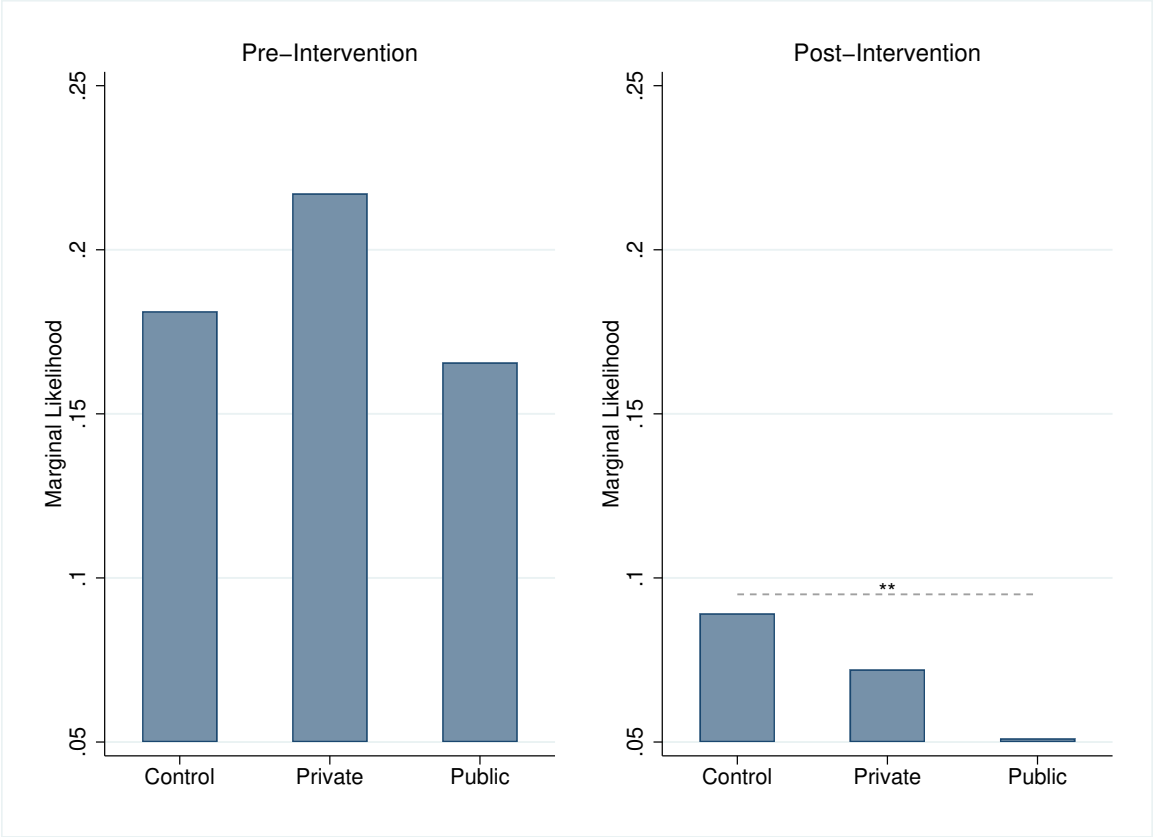
Note: The figure shows that a worker's own productivity (horizontal axis) is positively correlated with average productivity of his friends (vertical axis) at baseline. Productivity is measured with average monthly production earnings over 12 months in the year before the intervention (January-December 2015).

Figure 4: Productivity Effect of an Additional Outranked Friend



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

Figure 5: Cooperation Among Workers



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

Table 1: Key Descriptive Statistics

	(1) Control	(2) Private	(3) Public	(4) (1-2)	(5) (1-3)
Number of Workers	125	117	124		
	Mean	Mean	Mean		
Worker Characteristics					
Age	30.63	30.47	30.93	-0.15	0.3
Years Attended School	7.82	7.6	7.4	-0.22	-0.42
Tenure	5.34	5.45	5.29	0.11	-0.05
Risk	5.54	5.57	5.68	0.03	0.14
Competitive	0.45	0.5	0.42	0.05	-0.03
Production					
Production Time Per Sweater at Baseline (mins)	86.51	88.37	85.46	1.86	-1.05
Monthly Production Earnings at Baseline (BDT)	11039.91	11019.75	11070.69	-20.16	30.78
Social Network					
# Workers in Block	24.55	24.61	24.63	-0.05	-0.08
# Friends	11.48	10.49	12.44	-0.99	0.96

Note: The table reports key descriptive statistics for each experimental group. The last two columns report the differences in mean between control and the treatment groups. *, **, and *** indicate that when the difference is tested against a null of zero, the null is rejected at 10%, 5% and 1% significance level respectively.

Table 2: Average Treatment Effect of Ranking

	(1) Ln(Time)	(2) Ln(Time)	(3) Ln(Time)	(4) Ln(Time)
Public	0.0182*	0.0181*	0.0179*	0.0186*
	(0.0109)	(0.0107)	(0.0103)	(0.0107)
Private	0.0061	0.0055	0.0054	0.0059
	(0.0124)	(0.0124)	(0.0123)	(0.0136)
Job Sequence			-0.0228***	-0.0225***
			(0.0027)	(0.0027)
Observations	22,870	22,870	22,870	22,787
Adjusted R-squared	0.2926	0.2972	0.3053	0.3069
Private=Public	[0.182]	[0.142]	[0.127]	[0.160]
Baseline Productivity	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes	Yes
Month FE	No	Yes	Yes	Yes
Additional Controls	No	No	No	Yes

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to produce a sweater in an assigned job. *Public* and *Private* refer to Public and Private Rank Treatments respectively. *Baseline Productivity* is average monthly production earnings in 2015, the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. *Job Sequence* is the cumulative number of jobs in which a worker produced sweaters of a given style and size. *Additional Controls* include tenure at the factory as of baseline, total number of years a worker attended school, and measures of attitudes towards risk and competition. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 3: Social Incentives from Ranking

	(1) Ln(Time)	(2) Ln(Time)	(3) Ln(Time)	(4) Ln(Time)	(5) Ln(Time)	(6) Ln(Time)
Public	-0.0482*	-0.0459*	-0.0208	0.0068	-0.0040	-0.0008
	(0.0264)	(0.0255)	(0.0169)	(0.0132)	(0.0132)	(0.0113)
Public * 1(Competes with Friends)	0.0753***	0.0728**	0.0376*			
	(0.0292)	(0.0288)	(0.0214)			
Private		-0.0248				
		(0.0183)				
Private * 1(Competes with Friends)		0.0359*				
		(0.0192)				
Public * 1(Ranked Above Median Among Friends)				0.0133		
				(0.0202)		
Public * Distance to Median Rank Among Friends from Above					0.0103**	0.0099**
					(0.0042)	(0.0042)
Public * Distance to Median Rank Among Friends from Below					0.0005	
					(0.0040)	
Observations	15,573	22,870	14,950	14,950	14,950	14,950
Adjusted R-squared	0.3089	0.2975	0.2931	0.2933	0.2935	0.2933
Public + Interaction = 0	[0.018]	[0.021]	[0.078]	[0.130]		
Private + Interaction = 0		[0.398]				
From Above = From Below					[0.049]	
Baseline Productivity	Yes	Yes	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Counterfactual	Control	Control	Private	Private	Private	Private

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to produce a sweater in an assigned job. *Public* and *Private* refer to Public and Private Rank Treatments respectively. *1(Competes with Friends)* is a dummy variable that is equal to 1 if a worker has at least one friend randomized in the same experimental group. *1(Ranked Above Median Among Friends)* is a dummy variable that is equal to 1 if a worker is ranked above the median of ranks among his friends. *Distance to Median Rank Among Friends from Above* is the rank-distance to this median rank if a worker is ranked higher than the median rank, while *Distance to Median Rank Among Friends from Below* is the rank-distance to median rank if a worker is ranked lower. *Baseline Productivity* is average monthly production earnings in 2015, the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively. Numbers in square brackets report p-values from tests of corresponding null hypothesis.

Table 4: Placebo Tests

	(1) Ln(Time) Pre-Intervention	(2) Ln(Earnings) Pre-Intervention	(3) Ln(Earnings)	(4) Ln(Time)	(5) Ln(Time)	(6) Ln(Time)
Public	-0.0142	0.0078	0.0143	0.0066	0.0158	0.0070
	(0.0187)	(0.0207)	(0.0224)	(0.0121)	(0.0117)	(0.0129)
Public * Distance to Median Rank Among Friends from Above	-0.0066	-0.0033	-0.0099	0.0084*		0.0117**
	(0.0060)	(0.0066)	(0.0074)	(0.0048)		(0.0058)
Private				0.0078		
				(0.0141)		
Private * Distance to Median Rank Among Friends from Above				-0.0011		
				(0.0048)		
Public * Distance to Median Rank Among Friends in Private from Above					0.0013	-0.0047
					(0.0053)	(0.0051)
Observations	1,046	1,190	1,152	22,870	15,573	15,573
Adjusted R-squared	0.5536	0.2205	0.2797	0.2977	0.3087	0.3092
Baseline Productivity	Yes	No	No	Yes	Yes	Yes
Baseline Productivity (2014)	No	Yes	Yes	No	No	No
Style-Size FE	Yes	No	No	Yes	Yes	Yes
Month FE	No	Yes	Yes	Yes	Yes	Yes
Counterfactual	Private	Private	Private	Control	Control	Control

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

Table 5: Robustness Tests

	(1) Ln(Time) At Least 1 Friend	(2) Ln(Time)	(3) Ln(Time)	(4) Ln(Time)	(5) Ln(Time)	(6) Ln(Time)	(7) Ln(Time)	(8) Ln(Time)
Public	0.0006	0.0034	0.0231	0.0130	0.0057	0.0221	0.0422	0.0094
	(0.0141)	(0.0200)	(0.0640)	(0.0093)	(0.0108)	(0.0699)	(0.0935)	(0.0920)
Public * Distance to Median Rank Among Friends from Above	0.0098**	0.0100**	0.0107***		0.0128***	0.0106***	0.0072*	0.0077*
	(0.0046)	(0.0040)	(0.0040)		(0.0038)	(0.0039)	(0.0041)	(0.0040)
Public * (Number of Friends at Baseline)		-0.0003				-0.0003	-0.0010	-0.0010
		(0.0012)				(0.0012)	(0.0015)	(0.0016)
Public * Distance to Median Rank Among Non-Friends from Above				0.0002	-0.0062			
				(0.0031)	(0.0042)			
Public * 1(Public Intensive Block)								0.0313
								(0.0221)
Observations	13,248	14,950	14,950	14,950	14,950	14,950	14,867	14,867
Adjusted R-squared	0.2936	0.2932	0.2933	0.2938	0.2943	0.2932	0.3039	0.3049
Baseline Productivity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Public*Baseline Productivity	No	No	Yes	No	No	Yes	Yes	Yes
Additional Controls	No	No	No	No	No	No	Yes	Yes
Public*Additional Controls	No	No	No	No	No	No	Yes	Yes
Counterfactual	Private	Private	Private	Private	Private	Private	Private	Private

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

,

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	0.0161	0.1777	0.0188	0.1034
	(0.0158)	(0.0184)	(0.1156)	(0.0282)	(0.2981)
Public * Number of Outranked Friends at Rank-Dist. <=40	-0.0067	-0.0029	0.0832	-0.0113	-0.0100
	(0.0081)	(0.0098)	(0.1233)	(0.0148)	(0.1422)
Public * Number of Outranked Friends at Rank-Dist. >40	0.0246***	0.0270***	0.0309	-0.0329**	-0.1203
	(0.0081)	(0.0091)	(0.1073)	(0.0147)	(0.2133)
Public * Number of Friends at Baseline		-0.0017			
		(0.0015)			
Observations	14,950	14,950	1,105	1,139	1,139
Adjusted R-squared	0.2938	0.2938	0.1766	0.4289	0.3278
Sharpened Q-values: Public	[0.008]	[0.008]	[0.448]	[0.026]	[0.402]
Baseline Productivity	Yes	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Counterfactual	Private	Private	Private	Private	Private

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to produce a sweater in an assigned job. *Public* and *Private* refer to Public and Private Rank treatments respectively. *Baseline Productivity* is average monthly production earnings in 2015, the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 7: Alternative Explanations

	(1) Ln(Time)	(2) Ln(Time)	(3) Ln(Time)	(4) Ln(Time)	(5) Ln(Time)
Public	0.0107	-0.0035	0.0021	0.0072	-0.0016
	(0.0135)	(0.0106)	(0.0154)	(0.0081)	(0.0106)
Public * Distance to Median Rank Among Friends from Above	0.0108**	0.0093**			0.0092**
	(0.0049)	(0.0042)			(0.0046)
Public * (Number of Outranked Friends at Rank-Dist. <=40)			-0.0063		
			(0.0081)		
Public * (Number of Outranked Friends at Rank-Dist. >40)			0.0223**		
			(0.0091)		
Public * 1(Underestimated Own Rank)	-0.0491**				
	(0.0207)				
Public * Number of Friends Incorrectly Predicted as More Productive		0.0087	0.0064		
		(0.0106)	(0.0108)		
Public * Dist. from Abv. to Med. Rank Among Non-Fr. Peers Talks with				0.0051	0.0015
				(0.0041)	(0.0045)
Observations	14,714	14,950	14,950	14,950	14,950
Adjusted R-squared	0.2938	0.2936	0.2939	0.2929	0.2932
Baseline Productivity	Yes	Yes	Yes	Yes	Yes
Style-Size FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Counterfactual	Private	Private	Private	Private	Private

Note: *Ln(Time)* is logarithmic transformation of average time (minutes) a worker takes to produce a sweater in a given batch. *Public* and *Private* refer to Public and Private Rank treatments respectively. *Distance to Median Rank Among Friends from Above* is rank-distance to the median rank among a worker's friends when the worker is ranked higher than the median rank. *1(Underestimated Own Rank)* is a dummy variable that takes value 1 if a worker received a rank in the first treated month that was better than what he predicted for himself at baseline. *Number of Friends Incorrectly Predicted as More Productive* is the number of friends a worker thought were more productive than him at baseline. *Dist. from Abv. to Med. Rank Among Non-Fr. Peers Talks with* is the rank-distance to median rank among non-friend peers who a worker frequently interacts with during work, provided the worker is ranked higher than the median rank. *Baseline Productivity* is average monthly production earnings in 2015, the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Appendix

Figure A1: Sweater Knitting at Factory

Note: The figure shows a worker operating a manual knitting machine to produce a sweater part.

Figure A2: Sweater Knitting at Factory

Note: The figure shows a knitted sweater part in the process of production.

Table A1: Alternative Measurements & Heterogeneity

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

Note: *Ln(Time)* is logarithmic transformation of average time (minutes) a worker takes to produce a sweater in a given batch. *Public* and *Private* refer to Public and Private Rank treatments respectively. *Mean Rank-Distance with Friends* is average rank-distance with friends based on ranks provided in the first treated month. *Outranked Friends* refer to friends who were ranked lower than a worker in the first treated month; *Outranking Friends* were friends who were ranked higher. *Distance to Median Rank Among Friends from Above* is rank-distance to the median rank among a worker's friends when the worker is ranked higher than the median rank. *Baseline Productivity* is average monthly production earnings in 2015, the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. *n(Friends)* is the number of friends who were randomized into the same experimental group. Standard Errors are bootstrapped and clustered at block level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table A2: Alternative Clustering of Standard Errors

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

Note: *Ln(Time)* is logarithmic transformation of the number of minutes a worker takes to produce a sweater in an assigned job. *Public* and *Private* refer to Public and Private Rank Treatments respectively. *1(Competes with Friends)* is a dummy variable that is equal to 1 if a worker has at least one friend randomized in the same experimental group. *1(Ranked Above Median Among Friends)* is a dummy variable that is equal to 1 if a worker is ranked above the median of ranks among his friends. *Distance to Median Rank Among Friends from Above* is the rank-distance to this median rank if a worker is ranked higher than the median rank, while *Distance to Median Rank Among Friends from Below* is the rank-distance to median rank if a worker is ranked lower. *Baseline Productivity* is average monthly production earnings in 2015, the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. Standard Errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively. Numbers in square brackets report p-values from tests of corresponding null hypothesis.

Table A3: Spillover Effect

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

Note: *Ln(Time)* is logarithmic transformation of average time (minutes) a worker takes to produce a sweater in a given batch. *Private* refer to Private Rank treatment. *Pvt. Int.* refers to Private Rank Treatment intensive blocks. *Pub. Int.* refers to Public Rank Treatment intensive blocks. *Distance to Median Rank Among Friends from Above* is the rank-distance to this median rank if a worker is ranked higher than the median rank. *Baseline Productivity* is average monthly production earnings in 2015, the year prior to the intervention. *Style-Size FE* are fixed effects for combinations of styles and sizes of sweaters. Standard Errors are clustered at worker level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.