Ashraf, Anik

Working Paper

Do performance ranks increase productivity? Evidence from a field experiment

Discussion Paper, No. 196

Provided in Cooperation with:
University of Munich (LMU) and Humboldt University Berlin, Collaborative Research Center Transregio 190: Rationality and Competition

Suggested Citation: Ashraf, Anik (2019) : Do performance ranks increase productivity? Evidence from a field experiment, Discussion Paper, No. 196, Ludwig-Maximilians-Universität München und Humboldt-Universität zu Berlin, Collaborative Research Center Transregio 190 - Rationality and Competition, München und Berlin

This Version is available at:
https://hdl.handle.net/10419/208096

Do Performance Ranks Increase Productivity? Evidence from a Field Experiment

Anik Ashraf (LMU Munich)

Discussion Paper No. 196

November 4, 2019

Collaborative Research Center Transregio 190 | www.rationality-and-competition.de
Ludwig-Maximilians-Universität München | Humboldt-Universität zu Berlin
Spokesperson: Prof. Dr. Klaus M. Schmidt, University of Munich, 80539 Munich, Germany
+49 (89) 2180 3405 | info@rationality-and-competition.de

Do Performance Ranks Increase Productivity? Evidence from a Field Experiment

Anik Ashraf *

October 31, 2019

Abstract

Can a firm increase its workers' effort by introducing competition through performance-based ranking? On one hand such ranking can increase effort because of individuals' desire for status from high ranks, but on the other, it can demotivate them or make them wary of outperforming peers. This paper disentangles the effects of demotivation, social conformity, and status associated with ranking through a randomized experiment at a Bangladeshi sweater factory. Treated workers receive monthly information on their relative performance either in private or in public. Both a simple theoretical framework and empirical evidence from the field show that workers' intrinsic desire to be good at work induces privately ranked workers to increase effort upon receiving positive feedback, but they get demotivated and decrease effort upon receiving negative feedback. Public ranking lead to lower net effort relative to private ranking because of a strong preference not to outperform friends. The negative effects from demotivation and social conformity may explain why the existing literature finds mixed evidence of impact of ranking workers.

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

*Department of Economics, LMU Munich. Email- anik.ashraf@econ.lmu.de. This research was financially supported by Private Enterprise Development in Low-Income Countries (PEDL). Financial support by Deutsche Forschungsgemeinschaft through CRC TRR 190 (project number 280092119) is also gratefully acknowledged. The experiment for this paper had been registered with AEA RCT Registry on December 17, 2015 with ID-0000981. A. Latif Patwary and Shusmoy Roy provided excellent research assistance. I am grateful to Christopher Woodruff and Rocco Macchiavello for their continuous support, and to Arun Advani, Robert Akerlof, Nava Ashraf, Oriana Bandiera, Nicholas Bloom, Jordi Blanes-i-Vidal, Kristina Czura, Florian Englmaier, James Fenske, Alexandre Mas, Martina Miotti, Roland Rathelot, Simeon Schudy, Giorgio Zanarone, and seminar/conference participants at University of Warwick, PSE, University of Oxford, NEUDC 2017, CUNEF, University of Surrey, and LMU Munich for helpful comments.

1 Introduction

A growing literature suggests that people can be motivated by status rewards, and hence firms can increase the productivity of their workers by introducing competition among them through performance-based relative rankings. Both theoretical research and empirical evidence advocate such a notion (e.g., Besley and Ghatak [2008]; Vidal and Nossol [2011]; Ashraf et al. [2014a]). If this were true, it would be extremely beneficial for firms since this practice can increase their productivity at minimal cost. But if gains are to be had by introducing ranking at the workplace, why don't more firms do it? Is there more to ranking than the simple, positive effect of status concerns?

In this paper, I argue that there are at least two reasons that such rankings may lead to reduced worker effort: One derives from a worker's *intrinsic* motivation for status. A worker who receives information about his relative position may either be motivated or demotivated, depending on whether he previously believed his position to be higher or lower than shown by actual rankings.[1] In turn, this can lead to either an increase or a decrease in his effort. Second, in situations that make ranks public, workers may also be subject to *social* concerns. As the result of being known to others, higher rankings potentially generate higher social status. At the same time, however, a worker who increases his rank imposes a negative externality on the others whom he outperforms. So, the worker may internalize this externality, and may reduce effort to avoid being seen as a self-serving person, and to avoid risking being socially ostracized by co-workers, particularly those with whom he has close interaction.[2]

This paper aims to disentangle the positive effects of status concerns from the negative effects of demotivation and social conformity - all of which may affect the productivity of workers when they are ranked by a firm.[3] Disentangling these effects poses empirical challenges. First, clear measures of individual performance

[1] Recent evidence has shown that workers can become demotivated from relative concerns (Breza et al. [2018]).

[2] For instance, the theoretical literature on conformity (e.g., Akerlof [1997]; Bernheim [1994]) suggests that workers may not want to deviate too much away from their peers, lest they face social punishment.

[3] I use the term *conformity* in a slightly weaker sense than is traditionally used in the literature. In the literature, the term *conformity* refers to people's urge to converge to a single point, whether from below or above. In the context of this paper, however, converging to a rank from below is observationally equivalent to pursuing status incentives, and hence, not empirically identifiable. Hence, conformity can be observed only when it is convergence from above.

must be available. Second, a distinction must be made between intrinsic and social incentives. Third, to understand potential motivational and demotivational effects, workers' prior perceptions about how they rank in comparison with their co-workers must be known. And finally, to test social conformity, a worker's reference group, the network of people with whom he may seek to conform, must be identified.

I overcome these challenges by conducting a randomized control trial experiment in partnership with a leading sweater factory in Bangladesh, where I provided workers with their performance-based ranks. I worked with a specific section in the factory that employed 366 workers, all of whom received payment based on individual production. This helps me to overcome the first empirical challenge. I overcome the second challenge through a 10-month-long experiment with two treatment groups. In the first group, *Private Treatment*, workers were told only their own ranks, based on previous month's performances. In the second, *Public Treatment*, all the workers were told all rankings - both their own and those of other workers in the Public Treatment. Workers in a given treatment group were ranked among co-workers in the same treatment group. The two treatments, along with a control group, allow me to separate the effects of intrinsic and social incentives. Finally, to overcome the third and fourth challenges, a pre-intervention baseline survey recorded workers' own beliefs about what they expected their position to be in the ranks and provided a detailed map of their social network.

I provide a simple theoretical framework to interpret the empirical design and results. There are two key insights from the theoretical framework: First, how a worker responds to the intervention because of intrinsic-status concerns depends on the shape of the underlying intrinsic-status utility curve. If the status utility from rank is convex, a positive feedback will motivate him to increase his effort. This happens as the worker realizes that true marginal utility (now that he is at a higher rank than he had expected) far outweighs the marginal cost of his effort. On the other hand, a worker who receives a negative feedback will be demotivated and decrease his effort. The predictions will be opposite if the status utility from rank is concave. These predictions can be tested empirically to determine the shape of the underlying status-utility curve. Second, while a Public Treatment worker responds to intrinsic status concerns in the same way that a Private Treatment worker does, a Public Treatment worker also responds to social incentives. He faces two additional incentives, social status and social conformity. Relative to a privately ranked worker, social-status incentive will induce the worker to increase

effort in order to achieve a higher rank, but pressure to conform to peers may pull his effort down instead. Hence, relative to a privately ranked worker, a publicly ranked worker will exert more effort as long as he is ranked below the peers who can socially punish him. If he is ranked above them, he will exert relatively less (res. more) effort if the marginal disutility from outperforming peers is higher (res. lower) than the marginal social-status utility.

There are two key empirical findings: First, the response of workers to the private treatment depends on their prior beliefs about their relative positions, with those actually ranked higher (res. lower) than their perceived ranks increasing (res. decreasing) productivity. This suggests that, for these workers, the marginal return to status is increasing with rank (status utility curve is convex). Workers who received positive feedback in the first month of treatment performed more than 2.5 percentage points (p.p.) better than control-group workers who would have received positive feedback had they been ranked. Workers who received negative feedback, however, performed about 4 p.p. worse than those who received positive feedback, and more than 1 p.p. worse than those in the control group. The gain in productivity from one group was offset by the loss in another. Hence, the average treatment effect was positive but statistically insignificant.

Second, making ranks public led to worse outcomes than in making them known in private when workers were ranked higher than their friends. Workers in the Public Treatment group who ranked higher than their *friends* (defined as workers with whom they had social interaction outside the factory, as reported at baseline) reduced their performance by more than 3 p.p. on average compared to those in the Private Treatment group. This conformity occurred only with respect to friends and not with respect to any other peer group, which is consistent with the hypothesis that workers conform out of fear of social punishment. Once the response to social-conformity incentives is accounted for, social status shows a small, positive, but statistically insignificant effect. As a result, average effect of Public Treatment was weakly worse than that of Private Treatment.

However, while negative feedback in the first treatment month had an overall negative effect on workers, not all workers gave up and reduced effort. Conditional on receiving negative feedback in the first month, workers showing more competitive attitudes in a baseline laboratory-in-field experiment performed better after the intervention. While non-competitive workers reduced their effort by about 3 p.p. in response to negative feedback, competitive workers performed about 4 p.p.

better than them. This was true for both treatment groups. This serves as additional evidence that workers cared about their ranks; it also underlines how the same private-ranking treatment elicited opposite responses from different groups of workers.

The subject of providing feedback to workers about their relative ranks has attracted attention across a wide range of fields within economics, including management (Vidal and Nossol [2011]; Kuhnen and Tymula [2012]), education (Azmat and Iriberri [2010]), and public policy (Ager et al. [2017]; Chetty et al. [2015]).[4] Nevertheless, the results from this literature, especially that on firms, are conflicting and remain far from conclusive.

Studies about private feedback on workers' relative ranking within a firm document a wide array of impacts. Vidal and Nossol [2011] find positive impact; Blader et al. [2014] find zero impact; and Barankay [2011, 2012] finds negative impact. However, the source of such variation in impact across these papers is unclear. A possible clue lies in Breza et al. [2018]. From their experimental study, Breza et al. find that workers become demoralized, and reduce effort when they realize that they are paid relatively less than their peers. Can such a demoralization effect explain negative effect from ranks? Possibly yes, but a priori it is not clear. In the context of Breza et al., the demoralization effect stems from wage inequality, which in their context, a worker cannot change. On the other hand, in case of ranking, instead of reducing his effort upon receiving a negative feedback, a worker may increase effort to try to achieve a higher relative position. A formal test of a demoralization effect from ranks has not been done in existing papers.[5]

Studies with public ranking also find conflicting evidence of impacts. Ashraf et al. [2014a] and Delfgaauw et al. [2013] find positive effects; Ashraf et al. [2014b] and Blader et al. [2014] find negative effects; and Bandiera et al. [2013] find no effect. Again, it remains unclear why the evidence is so mixed. The demoralization effect remains one possible explanation; however, comparing the contexts in the papers suggests a second possible mechanism. Delfgaauw et al. [2013] find a positive effect from sales competitions among retail chain stores in Netherlands. On the other hand, Bandiera et al. [2013] find zero effect from rank feedback among workers at a fruit-picking farm who were living in the same quarters for a fair length of

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

[5] Barankay [2011] does raise this issue in his working paper, but cannot provide definitive evidence for the lack of data on workers' prior beliefs about their ranks.

time. A closer inspection reveals that the context in Bandiera et al. [2013] is more conducive for deeper social ties and, hence, stronger incentives to internalize negative externalities than in Delfgaauw et al. [2013].[6] More direct clues lie in Blader et al. [2014]. The study, which involved of truck drivers in a U.S. transport company, took place when the company was in midst of a management intervention that encouraged teamwork and collective effort. Blader and his co-authors find both positive and negative effects of public ranks; positive effects came from sites where the management intervention had not yet taken place, while the negative effects came from sites that had received the intervention. The authors speculate that the intervention may have reinforced social ties among drivers. But this is only a conjecture; the role of social network in rank incentives has not been studied in existing literature.

This paper contributes to the literature and provides new understanding about the dynamics on rank incentives by proposing demotivation and social conformity as two channels that can explain why existing empirical evidence is mixed. Evidence on the demotivation effect found in this paper suggests that the average effect of revealing true ranking information may be positive or negative, depending on whether uninformed workers, on average, overestimate or underestimate their relative performance. Also, the evidence on social conformity suggests that such conformity can further negate positive effects from status motivation if rankings are made public. This paper also contributes by separating intrinsic and extrinsic incentives within public ranking. Except for Blader et al. [2014], existing studies with public ranks do not make this distinction in incentives.

The findings from this paper also add to a few strands of broader literature. As indicated earlier, the evidence of demotivation effect found in this paper relates to the recent empirical literature on how relative concerns demoralize workers (e.g. see Breza et al. [2018]; and Huet-Vaughn [2015]). This very idea that workers' intrinsic relative-concerns can influence their economic decisions has also been explored in theoretical literature covering a wide array of contexts (e.g. Benabou and Tirole [2002, 2003]; Contreras and Zanarone [2017]). The evidence on social conformity, on the other hand, relates to the literature on social incentives in presence of externalities within firms. While studies of the effect of positive externalities on

[6] Bandiera et al. [2005] use a similar context (pickers at a fruit farm in the UK), and indeed find that workers in this setting internalize negative externalities imposed through a relative pay scheme.

productivity are more common (e.g. Mas and Moretti [2009]), those with negative externalities are relatively rare with one exception being Bandiera et al. [2005]. Bandiera et al. provide evidence that workers internalize negative externality they impose on their peers when they are paid through a relative pay scheme. This paper with rank incentives provides evidence that workers reduce effort even when such externalities are non-monetary in nature. Another stream of related literature is that on individuals' social-image concerns in more general settings. Evidence of such social-image concerns and conformity have been found in education (Bursztyn and Jensen [2015]) and in laboratory experiments (Bursztyn et al. [2016]). See Bursztyn and Jensen [2017] for a more detailed discussion on this literature.

In what follows, Section 2 describes the context and setup. Section 3 develops a brief theoretical framework that provides analytical predictions of treatment effects. Section 4 discusses the experimental design. Section 5 discusses the data, while Section 6 discusses the empirical strategy and the main results. In Section 7, I discuss alternative explanations to the findings in this paper. And finally, I conclude in Section 8.

2 Background

The experiment was conducted in partnership with a leading sweater factory in Bangladesh, and implemented in the Manual Knitting Section, one of three knitting sections in the factory. In this section, which is situated on one single floor, workers knit sweater parts using individually assigned manual knitting machines. All the workers in this section produce similar output using almost identical capital input (yarn, manual knitting machine, etc.).

Because the factory takes in multiple orders from multiple buyers at the same time, the Manual Knitting Section can be working on multiple styles (and sizes) of sweaters on a single day. Consequently, at a given point in time, different workers (*operators*, as they are called at the factory) can be working on sweaters of the same style and/or size, or different ones. These styles are assigned to them by *distributors*, based on the production plan. The operators are divided into 15 administrative groups called *blocks*, with each block supervised by one supervisor. The operators are paid based on piece rates and receive their wages at the end of a production month. The complexity of the sweater parts and the corresponding piece rates may vary across styles. A typical sweater contains various knitted components: a front

panel, a back panel, and two sleeves. Usually, an operator is assigned to produce a batch of 12 complete sets of sweater panels. For a style of average complexity, the batch will take a worker around one day to complete.

Three attributes of this Section make it an appropriate setting for the empirical exercise in this paper:

Piece-rate pay. The process of individual production in this setting makes it easy to measure individual productivity.

No promotion opportunities. Operators can move up to the next level, to become supervisors; but because the average take-home wage of a good operator is usually higher than the supervisors' salaries, operators choose to be supervisors only when their productivity falls with age.[7] This rules out the possibility that any ranking intervention would induce the workers to rank well for extrinsic incentives such as promotion.

Workers with long tenures. Among the 366 operators working in this Section at the beginning of the experiment in January 2016, most had been working at the factory for more than six years, which potentially helped them to form expectations of their own ranks, and also to form close social ties with their peers. Evidence of these were also found in a baseline survey conducted prior to the intervention in this paper.

3 Theoretical Framework

In this section I develop a simple theoretical framework to examine how a worker responds to rank incentives once such incentives are introduced by a firm. To keep the theoretical framework simple, let us consider only two periods $t \in \{0, 1\}$. In $t = 0$ there are no explicit rank information available; the worker has only a noisy signal of his relative performance. In $t = 1$ the firm releases the workers' true ranks based on performance in $t = 0$.

3.1 Intrinsic Status Concerns

I start by first considering the case where there are no social concerns and workers are driven by only intrinsic status concerns. This utility is intrinsic because it stems from the worker's intrinsic motivation to be good at whatever he does; there are no

[7] For instance, in January 2016, the average take-home pay of the 15 supervisors on the floor was less than that of a worker in the 33rd percentile.

extrinsic incentives involved. I will introduce social concerns in the next section. To be concrete, worker i in period t chooses effort e_{it} to maximize his utility $U_{it}(.)$ given by the following:

$$U_{it}(.) = W(\tilde{e}_{it}) - C(e_{it}, \alpha_i) + H\big(z_{it}(.)\big) \tag{1}$$

All the functions $W(.), C(.)$, and $H(.)$ are continuously differentiable at least twice. $W(\tilde{e}_{it})$ is utility gained from wage earned through effective effort $\tilde{e}_{it}$. Effective effort $\tilde{e}_{it} = e_{it} + \epsilon_{it}$ is the sum of effort exerted by worker e_{it} and an individual specific and time-variant shock to effort, ϵ_{it}. This shock, observed privately by worker only after choosing e_{it}, can be interpreted as task specific characteristics or instances that change the yield of effort. ϵ_{it} is i.i.d, $\epsilon_{it} \sim g(\epsilon)$, where $g(\epsilon)$ is the PDF for $\epsilon \in (-\infty, \infty)$, and $E(\epsilon_{it}) = 0$. $W_1(.) > 0$ and $W_{11}(.) \leq 0$.

$C(e_{it}, \alpha_i)$ is cost of effort exerted by worker i with skill level $\alpha_i \in (0, \bar{\alpha}]$. Higher α implies higher skill. $C_1(.) > 0$ and $C_{11}(.) > 0$. In addition, $C_{12}(.) < 0$ for $e_{it} > 0$. That is, marginal cost of effort is lower for higher skilled workers at any positive level of effort. Also, $C_1(0) = 0$.

$H(.)$ represents intrinsic status utility derived from the worker's *perceived* rank, $z_{it}(.)$, which is given by:

$$z_{it} = \Big[\tilde{e}_{i,t} - \frac{1}{n}\sum_{j} \tilde{e}_{j,t}\Big] + \delta_{it} \tag{2}$$

The expression inside the braces is worker's true rank, computed as the distance between his effective effort and the mean effective effort of all workers, n in total. However, the worker only observes this rank with a noise δ_{it}. $\delta_{i0} \in (-\infty, \infty)$; but $\delta_{i1} = 0$ for all workers, since they all find out about their true ranks in $t = 1$. Note that, $\delta_{i0} > 0$ implies that the worker overestimates his rank in period $t = 0$, while $\delta_{i0} < 0$ implies that he underestimates it; $\delta_{i0} = 0$ implies that he observes it perfectly.

Higher the perceived rank, higher is the utility from status; hence $H_1(.) > 0$. Also note that, the rank a worker i achieves increases with his own effective effort $\tilde{e}_{it}$, but decreases with that of others $\tilde{e}_{-i,t}$.

One simplifying assumption on $z_{it}(.)$ is that it is linear in worker's own effort and that of others, and thus the effect of marginal effort on rank is constant. It is because since I observe changes in only effort, I cannot separate the effect of

marginal effort on $H(.)$ from that on the underlying rank function. So, I cannot identify the shapes of these two functions at the same time. Nonetheless, the main intuition behind the results will be the same even with a more generic rank function.[8]

The worker chooses e_{it} to maximize his expected utility. Using Leibneiz rule and the fact that $\frac{\partial \tilde{e}_{it}}{\partial e_{it}} = 1$, we get the following first order condition:

$$\int \Big[W_1(\tilde{e}_{it}) + \frac{n-1}{n} H_1(z_{it})\Big] g(\epsilon_{it}) d\epsilon_{it} - C_1(e_{it}, \alpha_i) = 0 \tag{3}$$

At the very beginning of $t = 1$, the firm releases precise information on ranks of all the workers based on their performance in $t = 0$. Assuming an interior solution[9], we have the following observation for a worker's response in $t = 1$.

OBSERVATION 1: *If $H_{11}(.) > 0$, a worker who underestimates his rank in $t = 0$ ($\delta_{i0} < 0$) increases his effort in $t = 1$, while a worker who overestimates his rank ($\delta_{i0} > 0$) decreases his effort in $t = 1$. Conversely, if $H_{11}(.) < 0$, the opposite happens.*

In other words, when intrinsic status utility from perceived rank is convex, a worker who has inaccurate rank information in $t = 0$ but receives positive (res. negative) feedback from true rank information increases (res. decreases) his effort in $t = 1$. Conversely, when intrinsic status utility from perceived rank is concave, a worker who receives positive (res. negative) feedback from true rank information decreases (res. increases) his effort in $t = 1$. The proof is in the Appendix.

Intuitively, a worker increases his effort if true rank information from the firm reveals that his marginal status utility from an additional unit of effort is higher than he thought; he decreases effort if it is the converse. But whether it is the positive feedback or negative feedback that revises his marginal status utility upward depends on the shape of the underlying status utility curve.

In the empirical framework, I will use Observation 1 to test both the existence and the curvature of $H(.)$.

[8] However, a key assumption made in Equation 2 is that the noise in perceived rank of a worker is additively separable from his true rank. On the contrary, if δ_{it} had entered z_{it} in a multiplicative form for instance, a fixed δ_{it} would introduce a higher magnitude of distortion at a higher rank than at a lower rank; this would have been a much stronger assumption to make.

[9] For an interior solution I need the following assumption: $E[W_{11}(.) + (\frac{n-1}{n})^2 H_{11}(.)] < C_{11}(.)$

3.2 Social Concerns

Now I introduce social concerns into the worker's utility. Such concerns relate to how a worker wants to be perceived by his peers or other people around him (extrinsic). Conditional on rank of workers being known to each other, a worker now also gets utility from his social image associated with his rank. There are two types of social image he may care about. The first is his social image as a good worker (i.e. *social status*) associated with a higher rank. This induces him to increase effort to earn higher rank. However, since a rank tournament is a zero-sum game, a higher rank for one worker means a lower rank for another. Hence, effort of one worker now imposes a negative externality on other workers; if he increases effort to earn higher rank he damages his social image as a good person or friend, the second type of social image that he cares about.[10] In fear of being taken as a self-serving person by his peers, a worker may despise getting ranked high, and either not increase effort, or in extreme cases, reduce effort to *socially conform* to that of his peers.

To introduce this trade-off between social status and social conformity, I revise the previous utility function of a worker to the following:

$$U_{it} = W(\tilde{e}_{it}) - C(e_{it}, \alpha_i) + H(z_{it}) + \underbrace{\gamma_{it} s_i H(z_{it})}_{\text{social status}} - \underbrace{\pi_{it} M(\tilde{e}_{i,t-1} - \tilde{e}^f_{i,t-1})}_{\text{social conformity}} \quad (4)$$

Now, social image concerns introduce a social-status component to the utility by augmenting intrinsic status function $H(.)$ with a factor $\gamma_{it} s_i$. γ_{it} represents visibility of i's rank to others - the more visible his rank is to others, stronger is the social status that he derives from his rank.

The last component, $M(.)$, in Equation 4 refers to the disutility worker i gets when his effort is higher than the effort of peers who can socially punish him (let us call these peers *friends*). This disutility can come from either real punishment or simply his fear of punishment. Effort of these friends is denoted by $\tilde{e}^f$. Also, $M(x) = M_1(x) = 0$ for $x \leq 0$. However, this disutility increases with the extent of outperformance; so, $M_1(x) > 0$ for $x > 0$. Since this is a cost of effort, I assume $M_{11}(.) > 0$.

Because of the required condition of visibility, $\gamma_{it} = \pi_{it} = 1$ when ranks are

[10] The worker cares about his image as a good person or friend since this can yield benefits, either monetary (e.g. borrowing money) or non-monetary (e.g. good company during work breaks).

public in $t > 0$ and $\gamma_{it} = \pi_{it} = 0$ otherwise.[11] There are two simplifying assumptions about γ_{it}. First, visibility of ranks is same for all workers and workers do not affect this visibility. This is solely because I intend to focus on changes in effort of workers rather than their behaviour in sharing information. Second, visibility of ranks is zero unless there is a formal public ranking introduced by the firm.

Note that, when rank information are kept private, the above framework with social concerns degenerates to the previous framework with only intrinsic status incentives. When ranks are public on the other hand, assuming an interior solution[12], we have the following observation.

OBSERVATION 2: *Let $x = \tilde{e}_{i,0} - \tilde{e}^f_{i,0}$ be the difference between a worker's own effective effort and that of his friend(s). Worker i exerts more effort in $t = 1$ under public ranking than under private ranking if $x \leq 0$. More generally, there exists a value $\tilde{x} > 0$ such that, worker i exerts more effort in $t = 1$ under public ranking than under private ranking if $x < \tilde{x}$. Alternatively, he exerts less effort in $t = 1$ under public ranking than under private ranking if $x > \tilde{x}$.*

The proof is in the appendix. The intuition is the following. When rank information are made public, the visibility of ranks increases, which in turn introduces social-status utility attached to ranks. Making ranks public, however, also switches on public shaming for low-ranked workers. This introduces disutility from ranking higher than friends. But when a worker is not ranked higher than his friends, he responds positively to social-status incentives, and increase effort relative to under private ranking. On the contrary, when his rank distance with his friends is too high, the marginal disutility from outperforming friends overtakes marginal utility from social-status; so, he decreases effort.

4 The Experiment

4.1 The Design

The intervention for the randomized experiment provided treated workers with relative ranks through individually addressed letters at the end of every month, for

[11] Also, I assume that the workers expect the ranks to continue beyond $t = 1$. This lets workers respond to social conformity in $t = 1$.

[12] For an interior solution, I need the following assumption: for any given i, $\frac{\partial^2 M(.)}{\partial e_{it}^2} > s_i \frac{\partial^2 H(.)}{\partial e_{it}^2}$.

nine months. The control group also received letters, but no information on ranks. The content of these letters is discussed in Section 4.4.

There were two treatment groups. The first, *Private Treatment* Group, received letters that informed workers of only their own ranks, and no one else's; the ranks were computed among workers in this treatment group only. Because the ranks were private, no extrinsic incentives were involved.[13] Hence, the Private Treatment allows me to understand how revelation of true ranking information affects workers because of their intrinsic status incentives alone. Conversely, the second treatment group, the *Public Treatment* Group, received ranking information in such a way that the ranks of all workers were made known to each other; again, these ranks were computed among workers in this treatment group only. The second treatment induced response from intrinsic status concerns just as the first did, but because the ranks were now public, this also induced response to social concerns.

In terms of the theoretical framework, the experiment does the following. By revealing information on true ranks to Private and Public Treatment workers, the experiment eliminates δ_{i0} from their perceived ranks. $z_{it}(.)$ is now redefined as the following:

$$z_{it}(.) = \left[\tilde{e}_{i,t} - \frac{1}{n}\sum_{j}\tilde{e}_{j,t}\right] + \delta_{i0}(1 - v_{it})$$

where treatment status $v_{it} = 1$ for Private and Public Treatment workers in $t > 0$, and $v_{it} = 0$ otherwise. On the other hand, $\gamma_{it} = 1$ for Public Treatment workers in $t > 0$, and $\gamma_{it} = 0$ otherwise.

Thus, the goal is to identify the presence and impact of intrinsic status incentives $H(z_{it})$ by experimentally changing the value of perceived ranks $z_{it}(.)$ among treated workers. γ_{it} and π_{it} are set to zero by making information on ranks private in Private Treatment (and thus switching off social concerns), and set to one by making rank information public in Public Treatment (and thus switching on social concerns). Any differential response in Private Treatment, relative to Control group, will be driven by changes in intrinsic status incentives induced by changes in perceived rank $z_{it}(.)$. Any differential effect in Public Treatment, relative to Private Treatment, will be driven by the two social concerns.

[13] A valid concern here is that workers in the Private Treatment group may have shared rank information among themselves, essentially opening up the door to extrinsic incentives. I will discuss this issue in Section 7.

I used a baseline survey before the intervention to collect information on what each worker expected his rank to be. The difference between his expected rank and his true rank provides a measure of δ_{i0}.

Next, to disentangle social-status and social conformity effects, I compiled a detailed map of the existing social network at baseline, including information on who a worker socialized with outside the factory, who he talked to within the factory, and the administrative block to which he belonged. Potentially, any of these, along with the whole workforce on the floor, could define a worker's reference group to which he might conform.

Now, we may be particularly concerned with spill-over from Public Treatment to Private Treatment, since the former might induce a norm of sharing information in the latter, making the latter less private. Anecdotal evidence from the factory indicated that the workers were more closely connected socially to workers within their own blocks; hence a block encompassed most of a worker's peer connections, regardless of how those connections are defined (e.g. social proximity vs. spatial proximity). To check if there was any spill-over effect, the treatments were stratified across blocks. Before randomly assigning workers into experimental arms, first I randomly selected all 15 blocks of the floor into one of two categories, which I refer to as Category A and Category B. In Category A, 43.33% of operators were assigned to Private Treatment and 23.33% to Public Treatment. In Category B, the public/private weights were reversed. The control group consisted of one-third of the block operators in all blocks; and overall in the whole floor, one-third of operators were in each of the two experimental groups. The stratification created an exogenous block-level variation in the exposure each treatment group had to the other, and helps to identify potential spill-over effects from one treatment group to the other.

Following random assignment of the blocks into the two categories, we[14] held a *public lottery* within each block. A public lottery eliminated the possibility of behavioral responses stemming from suspicions on how they became inducted into one group and not another, but it precluded stratifying treatment on any characteristics other than block.

The top panel of Table 1 shows the final distribution of operators across experimental arms. The control group consisted of 125 workers, the Private Treatment

[14] I switch to "we" to include the field team members of this study.

of 117 workers, and the Public Treatment group consisted of 124 workers.[15]

We told the operators that their performance ranks would neither be rewarded nor punished in any way, monetary or non-monetary. To the extent that the operators might not have believed our assurances, the mechanisms of intrinsic and social concerns get confounded. However, note that even if such beliefs existed, they would have existed only in the first months of the treatment, after which workers would have realized that no such external punishments or rewards were forthcoming. Continuing the treatment for 10 months allows me to check whether such expectations could matter.

4.2 Timeline

In October 2015, we conducted a baseline survey; then, in January 2016, we drafted 366 available workers into experimental groups. It was also only during the drafting that we first informed the workers about the intervention that would follow. Because the top management also wanted to introduce ranking of workers, the experiment was introduced as a new management practice to the floor, rather than as an experiment by an external group of researchers. We delivered the first set of treatment letters in early February 2016, and the final set in early October 2016. At no point did we mention an end date to the experiment.

4.3 Rank Calculation

We computed the ranks provided to treatment workers in five steps. First, for each style and size produced by a worker in the previous month, we computed an average production time per set of sweater parts. In the second step, we compared this average time with the time put in by all the other workers in the same treatment group who also worked on the same style and size, to compute a *style-rank* for each style and size; a higher numerical rank would imply a worse performance. In the third step, for each worker, we normalized each of all the worker's style-ranks by the highest rank value for each of those styles (the worst rank in the treatment group). In the fourth step, we weighted the normalized-style ranks by the share of a given style in the worker's total production in the previous month. Then, we summed all the normalized-and-weighted-style ranks for each worker, to produce a

[15] A slight deviation from one-third of operators in each arm resulted from rounding up of the number of operators for each group in each block.

weighted average of normalized-style ranks. Finally, in the fifth step, we produced a final rank for each worker by comparing this weighted average of style ranks with that of others in the same treatment group.[16]

Because the information on the actual production times was recorded at the Distribution Section, the workers had no access to this information. Nonetheless, there could be concerns that because the workers were paid on piece rates, information on total production wages could help them deduce their ranks even when they were not meant to. However, wage-based ranks would not predict time-based ranks for two reasons. First, wages depended on the piece rates, which in turn, varied across styles. The workers were also aware that the piece rates did not always reflect the complexity or production time of a given style. Second, in a typical month, a worker worked on four different styles, which did not necessarily overlap with other workers. Moreover, in calculating production time we excluded pre-authorized leaves, but included all unauthorized absences in between a batch being assigned and the batch not yet submitted. Additionally, in computing the ranks we excluded production of small supplementary parts that were produced in large batches but not shared between a large number of workers. All these helped to distance wage-based ranks from time-based ranks. The treatment letters contained details about how ranks were calculated.

Figure 1 shows how wage-based ranks and time-based ranks correlate with each other. Because both are measures of productivity, they should be positively correlated, as indeed they are. Nonetheless, there is also sufficient noise for wage-based rank not to be able to precisely predict time-based rank.

Additionally, following Barankay [2012] I provided workers with information on what ranks they could achieve if they improved their average production time by 5 percent, 10 percent, and 20 percent. This gave them an idea of how harder they would need to work to achieve better ranks, but they would not be able to use this information to compare themselves with workers from a different treatment group.

4.4 The Treatment Letters

As stated earlier, we delivered information on ranks to the treated workers through monthly letters. Prior to the intervention, the factory did not record the precise time a job was distributed and received. The factory started recording the time only

[16] In the rare instances when two or more workers had the same value for weighted average style-ranks, we gave them the same final rank.

for the intervention. To control for any potential responses from the time-keeping or receiving the letters, all the workers in the control group also received letters at the end of every production month. These letters contained trivial information such as the total of number of sweaters the worker produced, broken down into styles, total time taken to produce them, and a list of all the workers in the control group.

On the other hand, each worker in the Private Treatment group received a letter with the same information as those in the control group, plus his relative rank among all the workers in the Private Treatment group, the total number of workers as well as the lowest rank in the group, and what the worker's rank would have been had he improved his time by 5 percent, 10 percent, and 20 percent, ceteris paribus.

Each worker in the Public Treatment Group received the same information as those in the Private Treatment group, except that for the former, the names of all the other workers in the Public Treatment group appeared with their respective ranks; this rank was also the variable by which we sorted the list.

To sum up, the difference between the control group and the treatment groups lies in the additional ranking information received, while the difference between the two treatment groups (Private and Public) lies in whether or not other people also knew about their ranks.

5 Data

The data I use for this paper come from two key sources: administrative data from the factory, and a baseline survey conducted in October 2015 before we drafted workers into the experiment.

5.1 Administrative Data

The administrative data from the factory provide detailed information on individual worker-level production wages, attendance, and breakdown of production into sweater styles and corresponding quantities, all compiled at the month level. These are available from January 2013 to October 2016. Starting from January 2016, we also collected the time it took for each operator to complete each of his jobs; we used this to compute the ranks.

The second panel of Table 1 shows the mean values for monthly wage, total days of attendance, average daily wage (total wage per attendance day, which I use as the outcome variable for analysis), age, and tenure at the start of the experiment. Columns 1-3 show the means for each experimental arm, while columns 4 and 5 show the difference in means between the control group and each of the treatment groups. The groups appear well balanced on these characteristics.

5.2 Social Network

We mapped the social network of workers in multiple dimensions. One definition of the network is simply the block to which a worker belongs. However, a more relevant measure in the context of social conformity is the network that can impose social punishments on a worker. Hence, we asked the workers to consider each of the other workers in their block and tell us how frequently they talked to them and whether they socialized with them outside the factory. With respect to the other workers outside their blocks, we asked them to name 10 workers with whom they talked frequently, or with whom they socialized[17].

Figure 2 shows evidence consistent with anecdotal evidence; workers were socially connected with an average of eight peers inside their block as opposed to three from outside their block.[18] The difference is starker when computed as a share of total workers in the block or floor. Hence, for the rest of the paper, I will focus on this within-block network.

The Social Network panel of Table 1 shows that the number of within-block peers with whom a worker socialized outside the factory is well balanced across the experimental groups. While there is slight imbalance in the number of block peers with whom a worker conversed more than three days a week, all the other observed characteristics are well balanced on average. An F-test of the social-network measures as a whole shows that they are jointly insignificant in predicting treatment status either as a combined treatment group or as separate groups (Private and Public).

[17] We did not ask about them individually because of the sheer size of the total workforce. Also, anecdotal evidence suggested that they were friends mostly with workers from their own blocks.

[18] Conditional on naming fewer than 10 peers (which was the case for all but 16 workers), this number is 2.74.

6 Main Results

6.1 Average Treatment Effect

To take advantage of a long period of pre-intervention data I use a difference-in-difference (DID) strategy to identify the treatment impact[19]. The key baseline specification is:

$$Y_{it} = c + \alpha_i + \tau_t + \beta(Treatment_i * Post_t) + \lambda' X_{it} + \eta_{it} \tag{5}$$

where Y_{it} is the outcome variable of interest, and almost always the logarithmic transformation of mean daily wage[20] earned by worker i in month t, and which I use as the measure of worker productivity[21]; α_i is worker fixed effect; τ_t is year-month fixed effect; the DID estimate of treatment effect, β, is the key coefficient of interest; and X_{it} is a vector of additional individual specific time-variant controls that includes, depending on specifications, sweater style FE and monthly block size.[22]

To keep with the standard approach in this literature, and to check how the treatment effects in this paper compare to those found in the existing literature, I start by estimating the average treatment effect of the intervention. Table 2 shows the baseline DID estimates of the average treatment effect. In columns 1 and 2, the treatment groups are pooled together. Column 1 is the simplest baseline specification; column 2 introduces worker, year-month, and style fixed effects, as well as block-size controls. Columns 3 and 4 correspond to columns 1 and 2, respectively, but now split the treatment into Private and Public Treatment groups. Regardless of specifications, columns 1-4 show that, on average, the treatments had no effect. Not only are the estimates statistically insignificant, they are also small in magnitude. A small statistically-insignificant difference emerges between Private and Public Treatment effects, indicating that the workers in the Public Treatment might have performed worse than those in the Private; we will explore this more

[19] An alternative would be Ancova analysis (McKenzie [2012]), but variance calculations show that DID and Ancova analysis are equally efficient with the data in this context.

[20] It is calculated as monthly wage divided by total number of attendance days.

[21] Conversely, the average time a worker takes to produce a sweater would be a more precise measure of productivity (which is what was used to compute ranks), but these times are not available for the pre-intervention period.

[22] Also, I exclude some 95 workers who were hired in the middle of the intervention, because of the concern that they may respond differently to the intervention than the others.

later.

The finding of an overall zero average treatment effect, particularly from Private Treatment, is similar to that found in studies such as Blader et al. [2014]. But, does this arise because the workers did not care about the ranks at all, or were there heterogeneous responses that offset each other? Indeed, the theoretical framework of this paper suggested that the treatment effect would be heterogeneous depending on the nature of feedback. Such heterogeneous responses could potentially offset each other and lead to an overall zero effect. So, in the following section, I empirically test theoretical Observation 1.

6.2 Treatment Effect from Intrinsic Status Concerns

As noted in Section 4, during the baseline survey prior to intervention, we asked workers what they thought their ranks were among all the workers in the whole knitting section. After we randomly grouped them into their corresponding experimental groups, the reported ranks, normalized with respect to the sizes of their experimental groups, serve as an estimation of the ranks they would expect to receive from the treatment (their *Expected Ranks*). Ex post, we revealed their true ranks to them in the first month of treatment. The difference between expected ranks and true ranks as seen on the treatment letter is the "surprise" the workers received. Hence, this difference serves as a measure of δ_{i0} from the theoretical framework. A positive difference $\delta_{i0} > 0$ for a given worker would imply that the worker had *overestimated* his rank earlier, and then received *negative* feedback from the treatment; a negative difference $\delta_{i0} < 0$ would imply the opposite.

It is worth noting one fine point. When we asked a worker about his expected rank during the baseline survey, we asked him to rank himself based on the wages of the previous three months. On the other hand, the ranks in treatment letters were computed from actual production time in immediate previous month. Nonetheless, because rank based on long-term average wages (three months in this context) is a fairly precise measure of productivity, it is reasonable to assume that the expected rank from the baseline survey nonetheless reflects the rank that the worker would expect to receive in the treatment letters. Indeed, distribution of δ_{i0} computed as a difference between expected rank and *true wage rank at baseline* looks very similar to that computed as a difference between expected rank and *time-based rank from the first treatment letter*; it is shown in Figure 3.

Table 3 shows the empirical test of Observation 1. I focus only on the Control and Private Treatment groups to understand intrinsic status concerns because, in the Public Treatment group, social concerns muddle the mechanism.[23] The workers are now split into two subsets. Column 1 refers to all the workers who received positive feedback through the treatment letter in the first month. Column 2 refers to those who instead received negative feedback.[24] Control-group workers are similarly split into these categories. The control-group workers never received any ranking information in practice, but I can nonetheless compute their ranks and, hence, the feedback they would have received had they also been treated. Control subsets serve as more appropriate counterfactuals than the whole control group because they control for any unobserved characteristics that determine whether a worker underestimates or overestimates his rank. Also, note that there is no statistical difference between pre-intervention wages of private- and control-group workers, even after splitting the workers into the two subsets.

The first row of Table 3 shows that there was indeed a differential response to the type of feedback received by workers. Workers who were told that their effort yielded much more (intrinsic) status return than they had previously expected increased effort by about 2.5 p.p., while those who were told that the return was lower than they had previously expected decreased effort by a little more than 1 p.p. However, the difference between the two coefficients is more important. The difference in these coefficients reported in column 3 is almost 4 p.p. in size and statistically significant. I do the same analysis in columns 4–6, but include additional controls. The results are similar.

The findings in Table 3 provide support to this paper's theoretical framework in two ways. First, the fact that we see a differential response to feedback suggests that workers do respond to changes in their perceived rank, z_{it}, that were induced by the experiment. Implicitly, this validates the presence of the function $H(.)$ in worker's utility function; that is, workers do care about intrinsic status. Second, the fact that workers responded positively to positive feedback and negatively to negative feedback suggests that the underlying status utility from ranks is convex

[23] The results for Private Treatment are almost identical when Public Treatment workers are also included in the sample.

[24] The fact that there were more workers who had previously overestimated their ranks (n=139) than who had previously underestimated their ranks (n=82) reflects the distribution plots in Figure 3. Also, this is consistent with findings in existing literature that suggest that people usually overestimate their own performance (e.g. Svenson [1981]; Meyer [1975]).

in nature, that is $H_{11}(.) > 0$.[25] Because different workers respond in two opposite directions, the net average treatment effect is close to zero, as was seen in Table 2.

6.3 Treatment Effect from Social Concerns

Let us now try to understand the treatment effect in Public Treatment. Public Treatment differs from Private in that it introduces social concerns in addition to intrinsic-status concerns. Social concerns, in turn, consist of both social-status and social conformity incentives. To disentangle the latter two, I first check whether there were indeed preferences for such conformity in the Public Treatment group. If a worker faced social pressure to conform to his peers, we would expect the worker to reduce effort when he found himself ranked relatively better than his peers who were *also in the Public Treatment group*. But who were the peers with whom he would care to conform?

Giving in to conformity pressure and reducing effort is costly to the workers. Because they were paid piece rates, reduced effort also implied reduced income. So, for a worker to conform in effort, the return he would receive from conforming would have to offset the income he would lose. Therefore, a strong candidate for the reference group are the co-workers with whom a worker socialized (i.e., *friends* of the worker), because they had the power to impose social costs on him should his ranking shame them. Hence, if there were any effect from conformity, it would be strongest with respect to friends.

If the social pressure to conform was present, we expect it to be felt by workers who were relatively more productive than their friends from the same block and the same treatment (henceforth *high-productive*), and not so much by those who were relatively less productive (henceforth *low-productive*). This is precisely what I test in Table 4. Column 1 is simply average treatment effect in the two treatment groups. In Column 2, I test how publicly ranked high-productive and low-productive workers responded after the introduction of the treatment. To do this, I split the publicly ranked workers into two groups - workers whose baseline productivity (average daily wage from the whole pre-treatment period) was higher than the median among their friends in the same block and same treatment, and workers whose baseline productivity was lower than the median.

[25] Inadequate sample size prevents me from testing whether there is a point of inflection in the underlying status utility curve.

Indeed, in Column 2 we see that publicly ranked low-productive workers increased their effort following the introduction of the intervention. Compared to them, high-productive workers decreased their effort by about six p.p. Also, it is not only that these high-productive workers decrease their effort compared to low-productive workers in the Public Treatment group, the net effort from the former set of workers is also less than similar workers in the control group. This is shown by the sum of Rows B and D at the bottom of the table.

If this behaviour among high-productive workers was induced by making the ranks public, we would not expect to see this in the Private Treatment. When we do the same exercise with Private Treatment in Column 3, we do not indeed see any such decrease in effort from privately ranked high-productive workers. However, to interpret this decrease in effort in Public Treatment as social-conformity effect, we should deduct the response by similar workers in Private Treatment. This is shown by the sum of Rows (B+D) - (A+C) at the bottom of the table, which is also negative and statistically significant. Finally, these results are also robust to including additional controls, as evident from Column 4.

Table 4 shows conformity with respect to baseline productivity. But how did these workers respond to actual rank information? This I test in Table 5.

If the social pressure to conform was present, then it would switch on when a worker is ranked higher than his friends. Hence, as a proxy for this social pressure to conform, I use a time-variant dummy variable that takes the value 1 if, in a given month, the worker found out that he was ranked higher than the median of all ranks among the his friends (in the same Treatment) in the previous month.[26]

As before, to ensure that I use appropriate counterfactuals for treated workers, I also assign ranks to control group workers, and compute their rank distances from their friends (in the control group). This helps to control for control group workers who are of similar distances from their friends. Further, along with worker fixed effects and year-month fixed effects, I also use style fixed effects to control for variation in wage from style-specific characteristics. Table 5 shows the results.

Column 1 of Table 5 shows that, on average, a worker in Public Treatment, relative to a similar control-group worker, reduced effort by about 3 p.p. when the worker was ranked higher than the median of his friends in the previous month (sum of rows B and D). Also, to account for intrinsic responses we need to deduct

[26]Notice that using previous month's rank information also allows me to work around Manski's reflection problem (Manski [1993]).

response by similar workers in the Private Treatment. The net effect, shown at the bottom of the table, is negative and statistically significant. In other words, once a Public Treatment worker found out that he had ranked higher than his friends in the previous month, he reduced his effort by about 2.8 p.p. relative to a Private Treatment worker.[27]

Alternatively, conformity could also be tested using a continuous measure of rank distance with friends instead of a dummy variable as used in column 1 of Table 5. Using a continuous measure of rank distance with friends yields similar results, and shows that the reduction in effort from workers in Public Treatment was more when they are ranked incrementally higher than their friends (i.e. $M_1(.) > 0$), but there were no such effect when they were ranked lower than their friends (i.e. $M(x) = 0$ when $x \leq 0$). These results are omitted for brevity.

To reconcile results from conformity to that from information shocks, in columns 2 and 3 I again split the workers into two subsets based on the feedback they received. We see similar differential responses to feedback (column 4) from Public Treatment as we saw from Private Treatment in Table 3.

Regarding conformity, we find that Public Treatment responded more strongly to conformity pressure even when we split workers based on the type of feedback they received, as is shown by the sum of Rows (E+H)-(A+D) at the bottom of the table. Also, the difference is larger in the case where workers received positive feedback.

Finally, what about social status? Now that we have captured conformity through a dummy variable that switches on when a worker ranks above his friends, Public*Post captures the response when he is *not* ranked better than his friends. In other words, the Public*Post coefficient captures response to intrinsic-status motivations (just like in Private*Post) and response to social-status incentives, but not social conformity. Therefore, the difference between Public*Post and Private*Post coefficients should be driven by social-status incentives. Returning to column 1 which includes all workers, I find that the effect from social status is positive, but very small and statistically insignificant. Social-status effect is similar in columns 2 and 3 - the differences in Public*Post and Private*Post coefficients are positive but statistically insignificant.

[27]Note that the conformity effect with respect to distances from friends' rankings is net of positive status incentives that might also be at work specifically within the network of friends. To that extent, the conformity effect we are capturing is only underestimated.

Note that, while Public Treatment workers who were not ranked higher than their friends exert only weakly more effort than Private Treatment worker, Public Treatment workers who *were* ranked higher than their friends exert significantly less effort than similar Private Treatment workers. In other words, social-conformity effect outweighs social-status effect in the Public Treatment.

Finally, is this conformity behaviour really driven by fear of reciprocation from one's social network? In column 5 of Table 5, I check how the workers responded to getting ranked higher than the median of all the other workers from the same block, who were also in the same treatment, but with whom a worker did *not* socialize outside the factory. The results on conformity to friends still exist (row D), but no such conformity took place with respect to the others (row F).

I repeat the exercise in column 5 by defining the second reference group as either (same block and same treatment) peers of similar productivity but with whom a worker did not socialize with[28], or (same block and same treatment) peers with whom a worker talked inside the factory but with whom the worker did not socialize outside the factory. The results are similar as in column 5, and hence not reported.

Thus, while the Public Treatment workers did exhibit conformity to their friends, they did not show any such behavior with respect to other workers in their block. This is consistent with the hypothesis that a worker would reduce effort to internalize negative externality on peers who can socially punish the worker if he tries to consistently shame them through ranks.[29] Since other workers who are not his friends do not have any strong way to inflict social punishment, the worker does not internalize the negative externality he imposes on them.

6.4 Dynamic Effect

Previous analysis explored how workers responded to the feedback that they received in the first month. But was it consistent across all treated months? To check dynamic responses to ranking information, I first check how Private Treatment workers, conditional on receiving a certain type of feedback in the first treat-

[28] For a given worker, a peer is defined as of similar productivity if the peer's pre-intervention productivity was within the 25 percentile band of the worker's own productivity.

[29] Alternatively, they could also do so for altruistic reasons if they felt guilty about their friends ranking worse than them. Although this alternative reason cannot be ruled out entirely, the fact that we do not see any reduction in effort from positive feedback in Private Treatment indicates that altruism is less likely.

ment month, behaved in subsequent months. I consider only Private Treatment since the effect from information is not muddled by social concerns in this treatment. Figure 4 shows that, conditional on receiving a negative feedback in the first month of treatment, a treated worker consistently reduced effort in all the subsequent treatment months (relative to the control group). Conversely, conditional on receiving a positive feedback in the first month, a worker consistently performed better in all the subsequent months.

It is not surprising that a worker's response to the first feedback is consistent across all treatment months. A worker's response would vary across months if he learned something new from latest feedback. While the information on true rank in the first feedback letter carried enough new information to update a worker's perceived rank, subsequent information was likely to be similar to that received in the first letter, and, hence, carried little new information. A formal analysis bears out this hypothesis. The correlation between workers' true ranks in February 2016 were only weakly correlated with the workers' expected ranks, while the ranks in March 2016 were highly correlated with those from February 2016 (correlation coefficients of 0.33 and 0.68 respectively). Ranks in subsequent months are similarly correlated.

I also check how, over time, workers responded to changes in their ranks across the previous two treatment letters. There were very little response to changes in their ranks across months[30].

6.5 Further Tests and Discussions

The evidence presented so far suggests that a worker who received a negative feedback in the first treatment month became demotivated and reduced his effort in all subsequent treatment months. Why did he not work harder to achieve his perceived rank, instead?

In order to answer the above question, during the baseline survey, we implemented a laboratory-in-the-field game to capture workers' innate competitive nature, thus capturing their willingness to be ranked. In that game, we gave workers 10 ping-pong balls and asked them to throw the balls one at a time into a basket placed 2.5 metres away. We told them they would be paid for each successful shot they made. However, they could choose to be paid through one of two different

[30]Results are available upon request.

methods - either a fixed piece rate for each successful shot, or double that rate but only if the worker scored more than a randomly chosen peer.[31] We asked the worker to select one of the two payment methods only after he saw the setup of the game, so that he could make an informed decision about which payment he wanted to select. We told the worker that his competitor would be picked only after all the workers had played the game, and only then would we decide who had won. Thus, the worker would not know with whom he would be compared. Among 363 workers surveyed, 198 chose the first version, and 165 chose the competitive version of the game. The numbers of workers choosing one or the other version of the game were also balanced between the experimental arms, as shown in Table 1. In the following analysis, I consider the workers who chose the second scheme as having a more competitive attitude than those who did not. If workers cared about their status, workers who were more competitive in nature would perform better than those who were not. This is precisely what we see in Table 6.

In Table 6, Treatment*Post coefficients refer to responses by less- or non-competitive workers, while (Treatment*Post + Treatment*Post*Competitive) tells us the responses of competitive workers. Column 1 shows that competitive workers responded more positively than those who were not competitive. The interaction terms are positive in both treatments, but statistically significant only for Public Treatment. When split into subsets of workers in the next two columns based on the type of feedback received by the workers, a clearer pattern emerges. The difference between competitive and non-competitive workers is stark for workers who received negative feedback, and is almost identical between the two treatments. Non-competitive workers reduced their effort upon receiving negative feedback while competitive workers did not give up so easily; in fact, the latter workers marginally increased their effort compared to control group (rows F and G). Among the workers who received positive feedback, competitive ones increased their effort compared with non-competitive ones, but the differences are not as stark. This is perhaps because being competitive or not is unlikely to make a significant difference when workers find out that they performed better than what they had expected.

The discussion of the differences between Private Treatment and Public Treatment has so far considered only social status and conformity. However, compared to Private Treatment, Public Treatment not only made a given worker's rank vis-

[31] This game is similar to laboratory or laboratory-in-field games used in existing literature to measure an individual's competitive attitude. For example, see Gneezy et al. [2009].

ible to others (and thus introducing social concerns), it also allowed him to know about others' true ranks. Hence, there was more information in public ranks than in private. Could having more accurate knowledge of peers' relative ranks drive any of the results we saw in Public Treatment?

To check whether such learning mattered, we asked each worker during the baseline survey to compare his own wages (from the previous three months) to those of each of the other workers in the same block. To represent new knowledge that was created from knowing peers' ranks in Public Treatment, I use the number of peers whose ranks (in the previous month) were different than a worker's beliefs about his position at baseline. More specifically, this new knowledge realized when peers that a worker had thought were comparatively less productive in fact ranked higher than him in the previous month, or the converse.

Table 7 shows the results from this analysis. For Public Treatment in column 1, the only information shock that seemed to matter surfaced when workers discovered that, contrary to their expectations, their peers ranked lower than themselves. To check how much of this is driven by conformity to friends, in columns 2, as before, I include a dummy to indicate whether or not a worker was ranked higher than the median of the worker's friends. We see that conformity still exists, and clearly was not driven by simply knowing more about other peers' productivity. Nonetheless, peers receiving unexpected relative ranks seemed to matter, but mostly for workers who received negative feedback.

Conversely, for Private Treatment in column 3, we see absolutely no impact of such specific information shocks; both the relevant coefficients are very small in size. This is, of course, what we would expect because the Private Treatment workers never received information about their peers' ranks.

7 Alternative Explanations

One concern in the experimental design of this paper is whether the private ranking treatment indeed remained private, or whether the Private Treatment workers instead shared their rankings with others – effectively making the treatment public. Note that, if Private Treatment workers shared their rankings with others, we would not expect to see any differential responses between Private and Public Treatment workers when they were more highly ranked than their friends (as shown in Table 5). The difference was particularly big and statistically significant when workers

received positive feedback from treatment letters. This is inconsistent with information sharing because we might expect them to share their rankings particularly when they receive good news than when they receive bad news. Finally, in Table 7 column 3, we see that Public Treatment workers responded to new knowledge about peers' productivity, but the Private Treatment workers showed no such response; if the ranking information were being shared, we would expect to see Private Treatment workers responding to new information in a way similar to the responses of the Public Treatment workers.

Could the evidence that we take as social conformity be explained by complacency instead? Note that, in column 5 of Table 5, we found that a Public Treatment worker reduced effort when he was ranked higher than his friends but the worker did not reduce effort when he was ranked higher than peers with whom he did not socialize. If what we interpreted as social-conformity effect was in fact driven by complacency, we would expect to see similar reduction in effort even with respect to peers with whom a worker did not socialize.

Column 5 of Table 5 rules out complacency among Public Treatment workers, but what about in Private Treatment? Again, this is unlikely. In the Private Treatment, we would expect a worker to be complacent when he receives a higher ranking than he had expected (positive feedback). Recall that in Observation 1 we had that conditional on $H_{11}(.) < 0$, workers would decrease effort when they receive positive feedback. This, in fact, can be interpreted as complacency effect. But instead, the empirical evidence suggests the contrary: workers increased their effort when given positive feedback, providing empirical support for $H_{11} > 0$ instead.

Finally, could any of the positive effect among workers be explained by fear of getting fired? Again, note that we saw an increase in productivity only when workers received positive feedback, while we saw a decrease in productivity when workers received negative feedback. If any response were driven by fear of getting fired, we would expect to see the opposite reaction. Moreover, the treatment letters consistently reminded workers that the rankings would not have any effect on their jobs.

8 Conclusion

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

indicates at least two reasons why this may not always be the case. A novel experimental design with private and public ranking, along with detailed baseline data on workers' expected ranks and their social network, help to show that demotivation and social conformity can strongly counteract the positive effects of status motivation.

In particular, the evidence found in this paper indicates that if ranked privately, demotivational effects are likely to offset at least some of the positive effects of intrinsic-status incentives from ranks. If ranked publicly, workers' preferences to socially conform with their friends can lead to even worse results by offsetting the weak positive effect from the additional social-status motivations. Nonetheless, the results from this paper also suggest that rank incentives are more likely to increase productivity on average if workers in a given context are highly competitive in nature. Such competitive attitudes will offset the negative demotivational effect from negative feedback, and, in turn, will complement the positive effect from positive feedback. Similarly, social conformity effects will be diminished if there are thinner social connections at a given workplace.

References

Philipp Ager, Hans-Joachim Voth, and Leonardo Bursztyn. Killer Incentives: Status Competition and Pilot Performance during World War II. *Working Paper*, 2017.

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

Nava Ashraf, Oriana Bandiera, and Kelsey Jack. No Margin, No Mission? A Field Experiment on Incentives for Public Services Delivery. *Journal of Public Economics*, 120:1–17, 2014a.

Nava Ashraf, Oriana Bandiera, and Scott S. Lee. Awards Unbundled: Evidence from a Natural Field Experiment. *Journal of Economic Behavior and Organization*, 100:44–63, 2014b.

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

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

Oriana Bandiera, Iwan Barankay, and Imran Rasul. Team Incentives: Evidence from a Firm Level Experiment. *Journal of the European Economic Association*, 11(5):1079–1114, 2013.

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

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

Roland Benabou and Jean Tirole. Self-Confidence and Personal Motivation. *The Quarterly Journal of Economics*, 117(3):871–915, 2002.

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

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

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

Steven Blader, Claudine Gartenberg, and Andrea Prat. The Contingent Effect of Management Practices. *Working Paper*, 2014.

Emily Breza, Supreet Kaur, and Yogita Shamdasani. The Morale Effects of Pay Inequality. *The Quarterly Journal of Economics*, 133(2):611–663, 2018.

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

Leonardo Bursztyn and Robert Jensen. Social Image and Economic Behavior in the Field: Identifying, Understanding and Shaping Social Pressure. *Annual Review of Economics*, 9(1):131–153, 2017.

Leonardo Bursztyn, Georgy Egorov, and Robert Jensen. Cool to be Smart or Smart to be Cool? Understanding Peer Pressure in Education. *Working Paper*, 2016.

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

Oscar F. Contreras and Giorgio Zanarone. Managing Social Comparison Costs in Organizations. *Working Paper*, 2017.

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

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

Emiliano Huet-Vaughn. Do Social Comparisons Motivate Workers? A Field Experiment on Relative Earnings, Labor Supply and the Inhibitory Effect of Pay Inequality. *Working Paper*, 2015.

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

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

Charles F. Manski. Identification of Endogenous Social Effects: The Reflection Problem. *The Review of Economic Studies*, 60(3):531–542, 1993.

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

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

Herbert H. Meyer. The Pay-for-Performance Dilemma. *Organizational Dynamics*, 3(3):39–65, 1975.

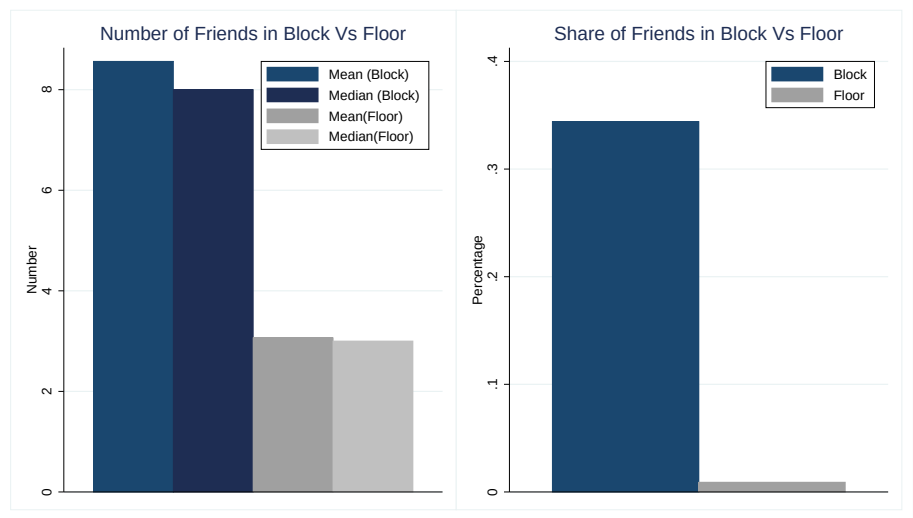
Ola Svenson. Are We Less Risky and More Skillful Than our Fellow Drivers? *Acta Psychologica*, 18:473–474, 1981.

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

Figure 1: Comparison of Wage based Ranks to Time Based Ranks

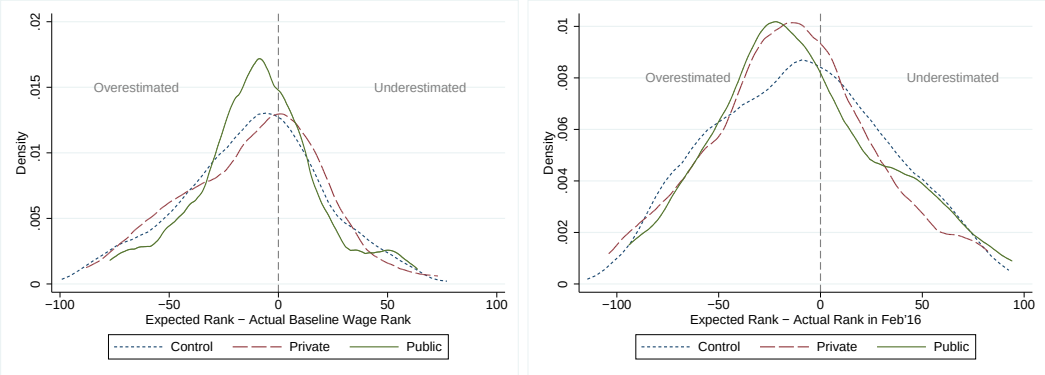
Note: The figure shows how ranks computed from mean production wage per day in each month (horizontal axis) correlates with actual time based ranks that were used in treatment letters (vertical axis). '20160X' refers to calendar month 'X' of year 2016.

Figure 2: Social Network Within Block and Outside Block



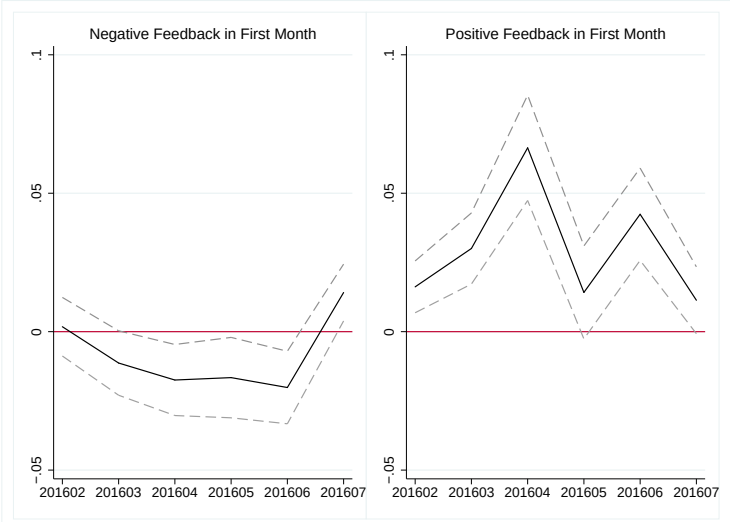
Note: Left panel of the figure above plots the mean and median number of friends workers have within their block, and rest of the floor outside their block. Instead of absolute numbers, the right panel shows the share of workers that a worker is friends with within his block, and rest of the floor.

Figure 3: Difference in Expected Rank and True Rank



Note: The first panel plots the distribution of difference between expected ranks as reported by workers during baseline survey and their actual wage-based ranks during the baseline survey. The second panel plots the distribution of difference between expected ranks as reported by workers during baseline survey and the actual rank provided through treatment letter in first treatment month. A negative value for this difference implies a worker overestimated his rank, while a positive value implies he underestimated it.

Figure 4: Treatment Effect Across Months (Private Treatment)



Note: The figure shows treatment effect in each month of the whole treatment period, conditional on a worker receiving a negative feedback (left panel) or a positive feedback (right panel) in the first treatment month. '20160X' on the horizontal axis refers to calendar month 'X' of year 2016. Underlying regressions are similar to those used for main difference-in-difference analysis and includes worker fixed effects, month fixed effects, style fixed effects and block size control. Coefficients reported are only for Private Treatment. Errors are clustered at both worker and year-month level. 90% confidence intervals around the coefficients are shown with dashed lines.

Table 1: Key Descriptive Statistics

	(1) Control	(2) Private	(3) Public	(4)	(5)
Block Category	n	n	n	Total	
Category A (Private Intensive)	59	71	39	169	
Category B (Public Intensive)	66	46	85	197	
Total	125	117	124	366	
Production	Mean	Mean	Mean	(1)-(2)	(1)-(3)
Pre-Intervention Monthly Production Wage (Tk.)	10504.59	10453.06	10504.44	51.53	0.16
Pre-Intervention Mean Daily Wage (Tk.)	386.34	384.79	386.11	1.55	0.22
Pre-Intervention Monthly Attendance (days)	27.05	27.00	27.08	0.05	-0.02
Age on Jan 1, 2015 (years)	29.61	29.44	29.90	0.17	-0.29
Length of Tenure on Jan 1, 2015 (years)	4.32	4.47	4.28	-0.15	0.05
Social Network	Mean	Mean	Mean	(1)-(2)	(1)-(3)
# of Operators in Block (Drafted)	24.55	24.61	24.63	-0.05	-0.08
# Peers (from block) Socially Interacts with	8.66	7.99	8.98	0.67	-0.32
# Peers (from block) Talks with >=3days/wk	12.65	13.34	14.33	-0.69	-1.68*
	n	n	n	Total	
Chose Competitive Version of Ball-Bucket Game	56	58	51	165	
Chose Non-Competitive Version of Ball-Bucket Game	69	56	73	198	

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

Table 2: Baseline Results

	(1) Ln(Wage)	(2) Ln(Wage)	(3) Ln(Wage)	(4) Ln(Wage)
[A] Treatment * Post	0.000961	-0.000955		
	(0.00329)	(0.00807)		
[B] Treatment	-0.00497			
	(0.0171)			
[C] Private * Post			-0.000519	0.00114
			(0.00391)	(0.0103)
[D] Public * Post			0.00238	-0.00292
			(0.00621)	(0.00858)
[E] Post	0.0278		0.0278	
	(0.0355)		(0.0355)	
[F] Private			-0.00753	
			(0.0200)	
[G] Public			-0.00255	
			(0.0200)	
[H] Block Size		6.65e-05		7.31e-05
		(0.00121)		(0.00121)
Constant	5.918***	5.344***	5.918***	5.344***
	(0.0291)	(0.0450)	(0.0291)	(0.0455)
Observations	14,263	14,251	14,263	14,251
Adj. R-Sq.	0.002	0.796	0.002	0.796
N(Worker)	366	366	366	366
N(Months)	46	46	46	46
FE: Worker, Year-Month, Style	NO	YES	NO	YES

Note: Dependent variable is log of mean daily wage. In Cols 1-2 Private and Public treatment groups are pooled together as one Treatment group, while in Cols 3-4 they are tested separately. Pre-treatment months are Jan'13 - Jan'15, while post-treatment months are Feb'15 - Oct'15. Standard errors are clustered at both worker and month level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance level respectively.

Table 3: Motivation/Demotivation Effect from Revelation of True Ranks

	(1) Ln(Wage) Positive Feedback ($\delta_0 < 0$)	(2) Ln(Wage) Negative Feedback ($\delta_0 > 0$)	(3) Cols [1] - [2]	(4) Ln(Wage) Positive Feedback ($\delta_0 < 0$)	(5) Ln(Wage) Negative Feedback ($\delta_0 > 0$)	(6) Cols [3] - [4]
Private * Post	0.0269***	-0.0130**	0.0399***	0.0230*	-0.0139	0.0398**
	(0.0077)	(0.0057)	(0.0127)	(0.0119)	(0.0103)	(0.0164)
Post	0.0168	0.0350				
	(0.0353)	(0.0356)				
Private	-0.0129	-0.0041				
	(0.0338)	(0.0276)				
Observations	3,279	5,298	8,577	3,278	5,293	8,571
Adj. R-Sq.	0.003	0.003	0.007	0.785	0.779	0.782
N(Worker)	82	139	242	82	139	221
N(Months)	46	46	46	46	46	46
Constant	YES	YES	YES	YES	YES	YES
FE: Worker, Year-Month, Style	NO	NO	NO	YES	YES	YES
Additional Control: Block Size	NO	NO	NO	YES	YES	YES

Note: Dependent variable is log of mean daily wage. *Positive Feedback* (*Negative Feedback*) refers to a worker whose rank in the first treatment month was higher (lower) than his expected rank. δ_0 refers to noise parameter used in the theoretical framework; it refers to the noise in a worker's perceived rank prior to the experiment. Workers from Public Treatment are excluded from the sample. Sample period contains 46 months (pre-treatment: Jan'13 - Jan'15; post-treatment: Feb'15 - Oct'15). Standard errors are clustered at both worker and month level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance level respectively.

Table 4: Conformity towards Friends - With Baseline Productivity

	(1) Ln(Wage)	(2) Ln(Wage)	(3) Ln(Wage)	(4) Ln(Wage)
[A] Private * Post	-0.000519	-0.0030	-0.0004	0.0040
	(0.00391)	(0.0046)	(0.0064)	(0.0106)
[B] Public * Post	0.00238	0.0274**	0.0285***	0.0128
	(0.00621)	(0.0109)	(0.0106)	(0.0085)
[C] Private * Post * 1(Base. Prod. > Median among Friends)			-0.0059	-0.0118
			(0.0144)	(0.0185)
[D] Public * Post * 1(Base. Prod. > Median among Friends)		-0.0594***	-0.0626***	-0.0453***
		(0.0155)	(0.0133)	(0.0146)
[E] Post	0.0278	0.0224	0.0213	
	(0.0355)	(0.0364)	(0.0361)	
[F] Post * 1(Base. Prod. > Median among Friends)		0.0122	0.0153**	0.0194*
		(0.0075)	(0.0071)	(0.0111)
Observations	14,263	14,263	14,263	14,251
Adj. R-Sq.	0.002	0.096	0.097	0.782
N(Worker)	366	366	366	366
Constant	YES	YES	YES	YES
FE: Worker, Year-Month, Style	NO	NO	NO	YES
Other Controls	NO	NO	NO	YES
B + D		-0.0321***	-0.0340***	-0.0325***
		(0.0089)	(0.0071)	(0.0104)
Social Conformity Effect: (B + D) - (A + C)			-0.0278**	-0.0246**
			(0.0118)	(0.0125)

Note: Dependent variable is log of mean daily wage. *1(Base. Prod. > Median among Friends)* is a dummy variable that takes the value 1 if pre-treatment productivity of a worker (measured as mean daily wage over the whole pre-treatment period) is higher than the median pre-treatment productivity among all of his friends (the worker himself included) who are from the same block and in the same treatment. Pre-treatment months are Jan'13 - Jan'15, while post-treatment months are Feb'15 - Oct'15. Standard errors are clustered at both worker and month level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance level respectively.

Table 5: Conformity towards Friends - With Ranks

	(1) Ln(Wage)	(2) Ln(Wage) Pos. Feed.	(3) Ln(Wage) Neg. Feed.	(4) Cols [2] - [3]	(5) Ln(Wage)
[A] Private * Post	-0.0028	0.0297*	-0.0211**	0.0519**	-0.0071
	(0.0083)	(0.0161)	(0.0103)	(0.0188)	(0.0090)
[B] Public * Post	0.0088	0.0306*	-0.0064	0.0392*	0.0064
	(0.0092)	(0.0157)	(0.0120)	(0.0222)	(0.0099)
[C] Private * Post * 1[Rank$_{t-1}$ > Median of Friends in Block]	0.0042	-0.0197	0.0155	-0.0346	-0.0015
	(0.0109)	(0.0212)	(0.0122)	(0.0219)	(0.0122)
[D] Public * Post * 1[Rank$_{t-1}$ > Median of Friends in Block]	-0.0355**	-0.0651***	-0.0142	-0.0489	-0.0399***
	(0.0142)	(0.0216)	(0.0223)	(0.0323)	(0.0146)
[E] Private * Post * 1[Rank$_{t-1}$ > Median of Non-Friends in Block]					0.0157
					(0.0142)
[F] Public * Post * 1[Rank$_{t-1}$ > Median of Non-Friends in Block]					0.0093
					(0.0103)
[G] Post * 1[Rank$_{t-1}$ > Median of Friends of Friends in Block]	0.0274***	0.0309***	0.0224		0.0245***
	(0.0076)	(0.0115)	(0.0146)		(0.0086)
[H] Post * 1[Rank$_{t-1}$ > Median of Non-Friends in Block]					0.0070
					(0.0082)
Observations	13,745	4,654	7,987		13,745
N(Worker)	366	120	216		366
N(Months)	45	45	45		45
Constant	YES	YES	YES		YES
FE: Worker, Year-Month, Style	YES	YES	YES		YES
Other Controls	YES	YES	YES		YES
Social Conformity Effect: (E+H)-(A+D)	-0.0281***	-0.0445**	-0.0149		
	(0.0097)	(0.0184)	(0.0128)		

Note: *1[Rank$_{t-1}$ > Median of Friends in Block]* is a dummy variable that takes the value 1 if a worker was ranked higher than the median rank among his friends (from the same block and same treatment group) in the previous month. *1[Rank$_{t-1}$ > Median of Non-Friends in Block]* is a dummy variable that takes the value 1 if a worker was ranked, in the previous month, higher than the median rank of all same-treatment and same-block peers that he is not friends with. All regressions include constant. Standard errors are clustered at both worker and month level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance level respectively.

Table 6: Fightback from Competitive Workers

	(1) Ln(Wage)	(2) Ln(Wage) Pos.Feed.	(3) Ln(Wage) Neg.Feed.
[A] Private * Post	-0.00657	0.0194	-0.0373**
	(0.0100)	(0.0140)	(0.0149)
[B] Private * Post * Competitive	0.0195	0.0109	0.0449**
	(0.0161)	(0.0236)	(0.0210)
[C] Public * Post	-0.0150*	0.00323	-0.0316***
	(0.00846)	(0.0124)	(0.0115)
[D] Public * Post * Competitive	0.0281*	0.00606	0.0423**
	(0.0151)	(0.0274)	(0.0190)
[E] Post * Competitive	-0.0130	0.0228	-0.0410***
	(0.0105)	(0.0169)	(0.0144)
Observations	14,118	4,813	8,287
N(Worker)	363	120	216
Constant	YES	YES	YES
FE: Worker, Year-Month, Style	YES	YES	YES
Other Controls: Block Size	YES	YES	YES
[A] + [B]	0.0130	0.0303	0.0076
	(0.0121)	(0.0200)	(0.0140)
[C]+[D]	0.0131	0.0093	0.0107
	(0.0108)	(0.0220)	(0.0130)

Note: *Competitive* is a dummy variable that takes the value 1 if a worker chose to get paid through the competitive version of ball-bucket game played during baseline survey. It takes the value 0 if he chose to get paid though uncompetitive piece rate. All regressions include constant, not reported for brevity. Standard errors are clustered at both worker and month level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance level respectively.

Table 7: Knowing Others' Ranks

	(1) Ln(Wage/Day) Public	(2) Ln(Wage/Day) Public	(3) Ln(Wage/Day) Private
Treatment * Post	0.00604	0.0233	0.0118
	(0.0106)	(0.0172)	(0.0127)
Treatment * Post * (# Peers Unexpectedly Ranked Lower in Prev. Mth.)	-0.0130***	-0.0106**	-0.00682
	(0.00497)	(0.00493)	(0.00751)
Treatment * Post * (# Peers Unexpectedly Ranked Higher in Prev. Mth.)	0.000524	-0.00347	-0.00381
	(0.00454)	(0.00547)	(0.00479)
Treatment * Post * 1[Rank$_{t-1}$ > Median of Friends in Block]		-0.0352**	
		(0.0167)	
Post * (# Peers Unexpectedly Ranked Lower in Prev. Mth.)	0.0114***	0.00977**	0.0113***
	(0.00433)	(0.00423)	(0.00401)
Post * (# Peers Unexpectedly Ranked Higher in Prev. Mth.)	-0.00365	-0.000574	-0.00300
	(0.00322)	(0.00395)	(0.00316)
Post * 1[Rank$_{t-1}$ > Median of Friends in Block]		0.0213**	
		(0.00909)	
Observations	9,702	9,361	9,437
N(Worker)	249	249	242
Constant	YES	YES	YES
FE: Worker, Year-Month, Style	YES	YES	YES
Other Controls: Block Size	YES	YES	YES

Note: *(# Peers Unexpectedly Ranked Lower in Prev. Mth.)* refers to number of peers from same block who a worker thought ranked higher than him during baseline survey but got ranked lower than him in previous month's rank during treatment period. Similarly for *(# Peers Unexpectedly Ranked Higher in Prev. Mth.)*. All regressions include constant, not reported for brevity. Standard errors are clustered at both worker and month level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance level respectively.

APPENDIX

A Proof of Theoretical Observations

A.1 Observation 1

Let e^*_{io} represent the equilibrium level of effort exerted by worker i in $t = 0$, which therefore solves Equation 3. Therefore:

$$\frac{\partial U(e^*_{i0},.)}{\partial e_{i0}} = \int \Big[W_1(e^*_{io} + \epsilon_i) + \frac{n-1}{n} H_1(z^*_{i0})\Big] g(\epsilon_i) d\epsilon_i - C_1(e^*_{i0}, \alpha_i) = 0 \quad (6)$$

where, $z^*_{it} = e^*_{io} + \epsilon_i - \frac{1}{n}\sum_j e^*_{j,t} + \delta_{i0}$. I suppress the time subscript in ϵ_{it} since the i.i.d. values for ϵ are drawn from the same distribution of ϵ in each period. While computing expectation over all possible values of ϵ, the subscript becomes redundant.

Once the true ranks are revealed in $t = 1$, if worker i keeps his effort at e^*_{io} and takes as given $e^*_{-i,o}$, the net marginal utility evaluated at e^*_{io} is:

$$\frac{\partial U(e^*_{i0},.)}{\partial e_{i1}} = \int \Big[W_1(e^*_{io} + \epsilon_i) + \frac{n-1}{n} H_1(z^*_{i0} - \delta_{io})\Big] g(\epsilon_i) d\epsilon_i - C_1(e^*_{i0}, \alpha_i) \quad (7)$$

Notice that $H_1(.)$ is now evaluated at $(z^*_{i0} - \delta_{io})$ which is the revised perceived rank for $t = 1$ at e^*_{io}. Letting Δ be the difference of (7)-(6), and using first order Taylor expansion we have:

$$\Delta_i = -\delta_{i0} \frac{n-1}{n} \int H_{11}(z^*_{i0}) g(\epsilon_i) d\epsilon_i \quad (8)$$

Therefore, in $t = 1$, holding everyone else's effort fixed at $e^*_{-i,0}$, if $\Delta_i > 0$, the marginal benefit at effort level e^*_{i0} in $t = 1$ outweighs the marginal cost of effort at e^*_{i0}. [32] Hence, in $t = 1$ worker i will increase his effort from e^*_{i0}. Conversely, if $\Delta_i < 0$, in $t = 1$ he will decrease his effort from e^*_{i0}. But whether Δ_i is positive or negative depends on the signs of both δ_{i0} and $H_{11}(.)$.

Case 1: $H_{11}(.) > 0$

[32] Because of the assumption on interior solution ($E[W_{11}(.) + (\frac{n-1}{n})^2 H_{11}(.)] < C_{11}(.)$) this difference between marginal benefit and marginal cost diminishes as effort goes up.

Under the case where $H_{11} > 0$, the value of the integral in Equation 8 is positive. If $\delta_{i0} < 0$, $\Delta_i > 0$ and hence worker i increases his effort in $t = 1$ relative to $t = 0$. Conversely, if $\delta_{i0} < 0$, he decreases his effort.

Case 2: $H_{11}(.) < 0$

Under the case where $H_{11} < 0$, the value of the integral in Equation 8 is negative. Now, if $\delta_{i0} < 0$, $\Delta_i < 0$ and hence worker i decreases his effort in $t = 1$ relative to $t = 0$. Conversely, if $\delta_{i0} > 0$, he increases his effort.

It is also easy to see that when $\delta_{i0} = 0$, irrespective of the sign of $H_{11}(.)$, $\Delta_i = 0$. Hence, worker i exerts the same level of effort in $t = 1$ that he does in $t = 0$.

The above proofs rely on the assumption that a given worker takes everyone else's effort constant. It is safe to make this assumption because when we start with e^*_{io}, it already considers an equilibrium response from other workers in period $t = 0$, however that equilibrium is determined. Moreover, in $t = 1$, to solve for the equilibrium, a worker would need to know $\delta_{-i,0}$ and α_{-i}, both of which are private information. He could try to infer $\delta_{-i,0} > 0$ if he had known how others' ranks change once the intervention was introduced, but the scope to learn is limited. Any variation in his own rank, the only thing he observes, would be caused by ϵ_{it} as well as $e_{-i,t}$, and $\epsilon_{-i,t}$; hence variation in his own rank alone contains very little information.

A.2 Observation 2

When the firm chooses to rank workers privately, the results are similar as before when workers were driven by only self-image concerns. Therefore, in $t = 1$ the first order condition for a privately ranked worker is given by:

$$\int \Big[W_1(\tilde{e}_{it}) + \frac{n-1}{n} H_1(z_{i1})\Big] g(\epsilon_{it}) d\epsilon_{it} - C_1(e_{it}, \alpha_i) = 0 \tag{9}$$

where, $z_{i1} = z_{i0} - \delta_{i0}$. Let e^*_{pvt} solve the above equation for a privately ranked worker in $t = 1$.

When the firm chooses to rank workers publicly, in $t = 1$ the first order condition is given by:

$$\int \Big[W_1(\tilde{e}_{it}) + \frac{n-1}{n}(1+s_i) H_1(z_{i1}) - M_1(\tilde{e}_{i,t-1} - \tilde{e}^f_{i,t-1})\Big] g(\epsilon_{it}) d\epsilon_{it} - C_1(e_{it}, \alpha_i) = 0 \tag{10}$$

Let e^*_{pub} solve the above equation for a publicly ranked worker in $t = 1$.

Ceteris peribus, the difference between LHS of Equation 10 and LHS of Equation 9 is the difference in marginal incentives between Public and Private ranking. It is given by the following:

$$\hat{\Delta} = \int \Big[\frac{n-1}{n} s_i H_1(z_{i1}) - M_1(\tilde{e}_{i,t-1} - \tilde{e}^f_{i,t-1})\Big] g(\epsilon_{it}) d\epsilon_{it} \tag{11}$$

The first part of RHS in Equation 11 is the change in social-status utility from one additional unit of effort, while the second part is the disutility of outperforming friends from one additional unit of effort.

Let $x = \tilde{e}_{i,t-1} - \tilde{e}^f_{i,t-1}$. Recall that $H_1(.) > 0$ and $M_1(x) = 0$ for $x \leq 0$ by assumption. Also, assumption for interior solution states that $\frac{\partial^2 M(.)}{\partial e^2_{it}} > s_i \frac{\partial^2 H(.)}{\partial e^2_{it}}$, which translates to $M_{11}(.) > \big(\frac{n-1}{n}\big)^2 s_i H_{11}(.)$. Hence, at e^*_{pvt}, by the first two assumptions, $\hat{\Delta} > 0$ for $x \leq 0$. Because of the third assumption which implies that $\hat{\Delta}$ will fall with increase in effort, a publicly ranked worker will exert effort higher than e^*_{pvt}. In other words, when a publicly ranked worker is not ranked higher than his friends in the previous period ($x \leq 0$), he exerts more effort than a privately ranked worker because of social-status return on his effort ($e^*_{pub} > e^*_{pvt}$).

To understand what happens when x increases from $x = 0$, first note that:

$$\frac{\partial \hat{\Delta}}{\partial x} = \int -M_{11}(.) g(\epsilon_{it}) d\epsilon_{it} < 0$$

The last inequality follows from the assumption that $M_{11}(.) > 0$. In other words, comparing across workers when all of them are ranked publicly, $e^*_{pub,i} > e^*_{pub,j}$ when $x_i < x_j$.

Therefore, since $\hat{\Delta}$ is continuously decreasing in x, there exists a value $\tilde{x} > 0$ such that, $e^*_{pub} < e^*_{pvt}$ if $x > \tilde{x}$.