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Do Performance Ranks Increase Productivity? Evidence from a Field Experiment

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Abstract

While performance-based ranking may induce workers to increase effort because of status concerns, such information may also demotivate them or make them wary of outperforming peers. This paper disentangles the effects of demotivation, social conformity, and status associated with ranking. I implement a randomized experiment at a Bangladeshi sweater factory that pays employees on piece rates. Treated workers receive monthly information on their relative performance either in private or in public. A simple theoretical framework shows that intrinsic status concerns induce Private Treatment workers to increase or decrease effort depending on the feedback they receive from the intervention. Workers in Public Treatment respond similarly but face two additional incentives - social status (positive effect) and social conformity (negative effect). Empirical evidence shows that Private Treatment workers increased (decreased) effort upon receiving positive (negative) feedback. Public ranking led to lower net effort relative to Private Treatment 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.

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1 Introduction

A growing literature on status concerns suggests that firms can increase the productivity of their workers by providing them with performance-based, relative rankings. Both theoretical research (e.g., Besley and Ghatak [2008]) and empirical evidence (e.g., Vidal and Nossol [2011]; Ashraf et al. [2014a]) suggest that firms benefit from such status incentives. 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? Perhaps yes; some evidence indeed suggests negative average effects from rankings (e.g., Barankay [2011]; Blader et al. [2014]). However, less understood is the key issue of why telling workers their relative positions may induce them to lower their efforts.

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] Thus, this discrepancy 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

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

[2] Indeed, 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.

effects poses empirical challenges. First, clear measures of individual performance 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 combining a novel experimental design with detailed pre-intervention data on workers' self-perceived ranks, and on workers' social networks. Working with a leading sweater factory in Bangladesh, I provide workers with their performance-based ranks. I work with a specific section in the factory that employs 366 workers, all of whom receive payment based on individual production. During the 10-month-long intervention, control-group workers received monthly summary information about their production in the previous month. Treated workers received this same information, and, in addition, they were told their relative ranks. There were two treatment groups; workers in a given treatment group were ranked among co-workers in the same treatment group. In the first group, *Private Treatment*, workers were told only their own ranks. In the second, *Public Treatment*, all the workers were told all rankings - both their own and those of other workers in the Public Treatment. The two treatments allow me to separate the effects of intrinsic and social incentives. A baseline survey conducted prior to the intervention recorded workers' own beliefs about what they expected their position to be in the ranks, and provided a detailed map of their social network. The information on the workers' expected ranks allows me to determine whether the information on their true position provided a positive or negative surprise (feedback) to the worker; thus, this allows me to subsequently identify motivational and demotivational effects. The social network map allows me to disentangle social concerns into social-status and social-conformity components.

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 (more) effort if the marginal disutility from outperforming peers is higher (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 (lower) than their perceived ranks increasing (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, as a larger share of workers received negative feedback ("a negative surprise"). 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.

I also provide additional findings that support the interpretation of the results. 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.

Specifically, with respect to the literature on firms, studies about private feedback on workers' relative ranking 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. Because the wages are determined by the firm, a worker cannot affect this inequality. On the other hand, ranking provides a different context; 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]

[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.

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 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. In this context, 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. To underscore, this is 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. For instance, 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. In the process of identifying the impact of these two channels, this paper also contributes by separating intrinsic and extrinsic incentives within public ranking. Using both private and public rankings in the same

[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.

context, this paper identifies how much of the impact from public rankings is driven by intrinsic motivations. 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 productivity are more common (e.g. Mas and Moretti [2009]), those with negative externalities are relatively rare. One exception is Bandiera et al. [2005], who study a farm that pays workers through a relative pay scheme; that is, a worker's pay depends on the ratio of his own productivity to average productivity among all co-workers. In that context, higher effort by a worker implies lower income for all, and hence generates a negative externality. Bandiera et al provide evidence that workers internalize this negative externality and withhold effort. This paper with rank incentives provides further evidence on the effect of negative externalities. Additionally, this paper reveals 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.[7] These sweater parts are stitched into complete sweaters in the next section, and eventually prepared for shipping in the subsequent steps of the production process. I focus only on the Manual Knitting Section because all the workers in this section produce similar output using almost identical capital input (yarn, manual knitting machine, etc.). Focusing on only one section allows me to measure and compare productivity cleanly across the workers.

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. As mentioned already, the operators are paid piece rates; hence, each operator is responsible for his own production. The process of individual production makes it easy to measure individual productivity.

No promotion opportunities. Operators have no prospect of any kind of promotion. 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

[7]The other two sections produce similar outputs but employ different technologies. So, productivities from these three sections are not directly comparable. One of the other two sections use semi-automatic machines, while the third employs fully automatic machines to knit sweaters.

supervisors' salaries, operators choose to be supervisors only when their productivity falls with age.[8] 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, 236 were hired during the years 2004–2010, 16 over 2011–2012, and 114 in 2014.[9] Thus, most of these operators 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. Indeed, evidence of a strong sense of community among these workers was found in a recent companion paper (Akerlof et al. [2015]) that used data from the same factory to check whether the lay-off of peers had any impact on productivity of retained workers. Similar evidence was 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. The framework constitutes of two stages. In the first stage, I assume that ranks generate only intrinsic status utility for a worker. This utility is intrinsic because it stems from the worker's intrinsic motivation to be good at whatever he does; there are no extrinsic incentives involved. Considering intrinsic status alone lets me explore how workers respond to true rank information in absence of extrinsic incentives. In the process, this also lets me identify the shape of underlying intrinsic status utility curve. In the second stage, I introduce social concerns associated with ranks. Such concerns relate to how a worker wants to be perceived by his peers or other people around him (extrinsic).

To keep the theoretical framework simple, I consider only two periods. In the first period, there are no explicit rank information available at the workplace. In absence of such information, the worker has only a noisy signal of his relative performance. In the second period, the firm introduces rank incentives at the workplace and provides workers with their performance based ranks.

[8]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.

[9]The factory hired another 95 workers during July–August 2016, after the intervention commenced.

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. There are two periods $t \in \{0, 1\}$; in $t = 0$ there are no explicit rank information available, while in $t = 1$ the firm introduces performance based ranks. The ranks can be revealed either privately or publicly; it is inconsequential since the workers do not have any social concerns.

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}. The shock occurs after the worker chooses his effort, but he observes it once it realizes. This privately observed shock can be interpreted as task specific characteristics or unanticipated instances that change the yield of effort e_{it}. ϵ_{it} is i.i.d, $\epsilon_{it} \sim g(\epsilon)$, where $g(\epsilon)$ is the PDF for $\epsilon \in (-\infty, \infty)$, and $E(\epsilon_{it}) = 0$. Utility from wage follows standard concavity assumption, i.e. $W_1(.) > 0$ and $W_{11}(.) \leq 0$.

$C(e_{it}, \alpha_i)$ is cost of effort e_{it} exerted by worker i with skill level $\alpha_i \in (0, \bar{\alpha}]$. Higher α implies higher skill. Cost of effort is convex, i.e. $C_1(.) > 0$ 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, $C_1(0) = 0$, i.e. marginal cost of effort is as low as zero at zero level of effort.

$H(.)$ 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 the total workforce, where n is the total number of workers in the workforce. However, the worker only observes this rank with a noise δ_{it}. For a worker i in $t = 0$, $\delta_{i0} \in (-\infty, \infty)$; but $\delta_{i1} = 0$ for all workers, since they all find out about their true ranks in $t = 1$.

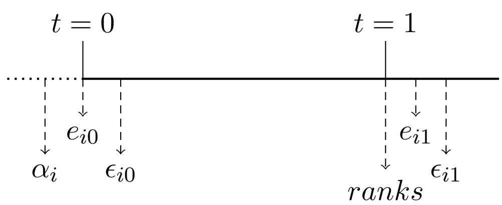
Note that, $\delta_{i0} > 0$ implies that the worker overestimates his rank in period $t = 0$, while $\delta_{i0} < 0$ implies that he underestimates it; $\delta_{i0} = 0$ implies that he observes it perfectly.

Higher the perceived rank, higher is the utility from status; hence $H_1(.) > 0$. I do not impose any restrictions on the second order derivative of $H(.)$; as I will later show, the predictions of this framework will be determined by the curvature of $H(.)$. Also note that, the rank a worker i achieves increases with his own effective effort $\tilde{e}_{it}$, but decreases with that of others $\tilde{e}_{-i,t}$.

The functional form of $z_{it}(.)$ merits a little more discussion. 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. Hence, without loss of generalization, a simplifying assumption made here is that the underlying rank function is linear in worker's own effort and that of others, and thus the effect of marginal effort on rank is constant. This lets me explore $H(.)$ alone. Nonetheless, the main intuition behind the results will be the same even with a more generic rank function.

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. This lets the magnitude of distortion (bias) in perceived rank to be independent of a worker's original 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.

The timeline of the events is given below:



Nature determines skill level α_i of worker i before the start of period $t = 0$. Next, worker i chooses his effort level e_{i0} at the beginning of $t = 0$. Soon after, effort to shock ϵ_{i0} realizes. At the very beginning of $t = 1$, the firm releases precise information on ranks of all the workers based on their performance in $t = 0$. This is unanticipated by the worker at $t = 0$. Subsequently, the worker observes his true rank and chooses his effort e_{i1}, following which, shock ϵ_{i1} realizes.

Since the worker i does not observe shock to effort before determining how much effort to put in, he solves his optimization in expectation. In other words, he chooses e_{it} to maximize his expected utility. Using Leibneiz rule and since $\frac{\partial \tilde{e}_{it}}{\partial e_{it}} = 1$, we get the following first order condition:

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

Assuming an interior solution, we have the following observation for a worker's response in $t = 1$.[10]

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 $H_{11}(.) < 0$, a worker who underestimates his rank in $t = 0$ ($\delta_{i0} < 0$) decreases his effort in $t = 1$, while a worker who overestimates his rank ($\delta_{i0} > 0$) increases his effort in $t = 1$.*

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 (negative) feedback from true rank information increases (decreases) his effort in $t = 1$. Conversely, when intrinsic status utility from perceived rank is concave, a worker who has inaccurate rank information in $t = 0$ but receives positive (negative) feedback from true rank information decreases (increases) his effort in $t = 1$. The above observation is proved 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.

The above framework produces a key insight. If workers do not care about status from ranks (that is, $H(.)$ does not exist in their utility function), they should not respond to rank feedback in $t = 1$, especially in two different directions when they receive two different kind of feedback. Also, conditional on differential response to feedback, the direction of response to a specific kind of feedback can tell us about the curvature of underlying status utility function $H(.)$. In the empirical

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

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

3.2 Social Concerns

Now I introduce social concerns into the worker's utility. Conditional on rank of workers being known to each other, in addition to intrinsic status utility, a worker now also gets utility from his social image associated with his rank. But 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*). Higher the rank, higher is the social status. The worker may therefore be induced 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. Contrary to higher ranks, lower ranks bring greater disappointment and shame. Hence, effort of one worker now imposes a negative externality on other workers; if he tries to increase 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.[11] 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:

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

The interpretation of $W(.)$, $C(.)$ and $H(.)$ are same as before. But 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. I will return to a more detailed discussion on γ_{it}. $s_i \geq 0$ is the weight worker i puts on social-status.

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 when he shames them in ranks (let us call these peers *friends*). This disutility can come

[11]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).

from either real punishment or simply his fear of punishment. Effort of peers is denoted by $\tilde{e}^f$. Disutility from shaming friends does not exist when worker i does not outperform his friends; hence $M(x) = M_1(x) = 0$ if $x \leq 0$. 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 will let it be convex; hence $M_{11}(.) > 0$. However, since public shaming occurs only when ranks are formally made public, $\pi_{it} = 0$ in period t if there is no public ranking, and $\pi_{it} = 1$ otherwise.

Note that in period t a worker feels disutility from outperforming his friends in period $t-1$. This is because the information on whether a worker outperformed his friends in a given month is not available until the next month. As such, any social pressure that a worker feels from his peers is likely to be based on previous month's performance. This requires a slight change in the the timeline of events. Most of the timeline is the same as in the framework with only intrinsic status concerns, except that now I assume the workers expect the ranks to continue beyond $t = 1$. This deviation in timeline is necessary to let workers respond to social conformity in $t = 1$. A worker may be forgiven for outperforming his friends in $t = 0$ since there were no formal ranking in place, but he will be punished if he does it in $t = 1$ when public shaming has come into being. But if there is no ranking in $t = 2$, there will be no way to know how a worker acted in $t = 1$, and hence enforcement of social conformity is not possible.

Let us assume that the firm ranks it workers in one of two ways - privately inform workers of their own rank, or make the whole set of rank information public. γ_{it}, therefore, takes the following values:

$$\gamma_{it} = \begin{cases} 0 \text{ for } t = 0 \\ 0 \text{ for } t{>}0 \text{ \& ranks are private} \\ 1 \text{ for } t{>}0 \text{ \& ranks are public} \end{cases}$$

There are two simplifying assumptions about γ_{it}. First, visibility of ranks is same for all workers, exogenously determined, 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. Alternatively, γ_{it} could take a value between zero and one to allow for some level of visibility of ranks among workers, even without public ranking. The theoretical predictions will still be the same, but I will not be able to empirically separate the effect of intrinsic status incentives (Observation 1) from that of social-status incentives, even when rank information are kept private.[12]

For reasons already discussed earlier, π_{it} takes the following values:

$$\pi_{it} = \begin{cases} 0 \text{ for } t = 0 \\ 0 \text{ for } t{>}0 \text{ \& ranks are private} \\ 1 \text{ for } t{>}0 \text{ \& ranks are public} \end{cases}$$

A key assumption in this framework, however, is that social-status utility function is the same as intrinsic status utility function $H(.)$, and only augmented by a scalar factor. Since both of them are status utility and both depend on how a worker perceives himself (his perceived rank), this is likely to be a reasonable assumption. Also, since we already know how their responses are driven by intrinsic status incentives alone, the assumption makes it easier to understand how workers respond to additional social-status incentives once true ranks are revealed. As a result, I can identify the function $M(.)$ separately from social-status.

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 [13], 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 provided in the appendix. The intuition is the following. When rank information are made public, the visibility of ranks increases, which in turn

[12] Note that it is not absolutely necessary for a worker to know individual effort/ranks of each of his peers to form a perception of his own rank. Given how the rank function has been defined in this framework, the worker only needs to have a perception of the average level of effort among all the other workers.

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

introduces social-status utility attached to ranks. Making ranks public, however, also switches on public shaming. 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. More generally, as long as a worker's rank distance with his friends is not too high, his marginal social-status utility is higher than his marginal disutility from outperforming friends; so, he increases effort. 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

Along the line discussed in the theoretical framework, I designed and implemented a randomized experiment at the sweater factory described in Section 2. The intervention provided treated workers with relative ranks based on their previous month's performance. The research team in the field along with supervisors of the blocks delivered the ranking information to each worker through individually addressed letters at the end of every month, for 9 months after the intervention commenced. 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.[14] 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. Comparing

[14] 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.

the Private and Public Treatment groups allows me to isolate the effect of social concerns.

In terms of the theoretical framework, the experiment does the following. First, by revealing information on true ranks to Private and Public Treatment workers, the experiment eliminates δ_{i0} from their perceived ranks. Second, in the Public Treatment, it increases the visibility of rank information, and hence switches on social concerns among workers only in the Public Treatment. To be more concrete, $z_{it}(.)$ 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 v_{it} , treatment status of worker i, takes the following values:

$$v_{it} = \begin{cases} 0 \text{ if } t = 0 \text{ and } i \in \{Control, Private, Public\} \\ 0 \text{ if } t > 0 \text{ and } i \in \{Control\} \\ 1 \text{ if } t > 0 \text{ and } i \in \{Private, Public\} \end{cases}$$

In addition:

$$\gamma_{it} = \begin{cases} 0 \text{ if } t = 0 \text{ and } i \in \{Control, Private, Public\} \\ 0 \text{ if } t > 0 \text{ and } i \in \{Control, Private\} \\ 1 \text{ if } t > 0 \text{ and } i \in \{Public\} \end{cases}$$

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. To switch off social concerns, γ_{it} and π_{it} are set to zero by making information on ranks private in Private Treatment. Since control group workers do not receive information on their true ranks, $z_{it}(.)$ do not change for them. Therefore, 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 $z_{it}(.)$.

On the other hand, the value of γ_{it} and π_{it} are experimentally changed to one for Public Treatment workers by making their rank information public. In addition to intrinsic effect from changes in perceived rank, this now switches on both social-status and social-conformity mechanisms. Any differential effect in

Public Treatment, relative to Private Treatment, will be driven by the two social concerns induced by increased visibility of ranks.

Observation 1 from the theoretical framework suggests that a worker will respond differently to the intervention depending on whether he overestimated or underestimated his rank prior to the intervention. In other words, how a worker responds depends on δ_{i0}, the ex-ante noise in his perceived rank. To measure δ_{i0}, 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}. This allows me to understand whether the true rank information revealed through the experiment conveyed a positive or negative surprise to a given worker, and also to empirically test Observation 1.

Next, to disentangle social-status and social conformity effects, it is necessary to first identify a reference group that workers may feel social pressure to conform to. In the theoretical framework, this reference group is denoted by f in the superscript of $\tilde{e}^f$. To do this, I compiled a detailed map of the existing social network at baseline. I mapped the network in multiple dimensions. To be concrete, I collected 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. This, along with the fact that the intervention was continued for 10 months, allow me to check how within-worker behavior varied across months in response to ranks of his peers in his reference group, and, in the process, to test Observation 2.

One potential concern in this design is spill-over effect. 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. To check if there was any spill-over effect, the treatments were stratified across blocks. Anecdotal evidence from the factory indicated that the workers were more closely connected socially to workers within their own blocks, and 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). Before randomly assigning workers into experimental arms, first we 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; overall, one-third of operators were in each of the two experimental groups. For each treatment group, the stratification created an exogenous block-level variation in the exposure each treatment group had to the other. The exposure at the block level captured both social proximity and spatial proximity, 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[15] held a *public lottery* within each block. We presented each operator with a bag of tokens with hidden numbers written on them: "1", "2", or "3". The total number of operators in the block and whether the block was in Category A or B determined the composition of tokens for a given block. We explained to the operators that they would receive monthly information based on their production, and that the type of information they would receive depends on the number they picked. 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.

[Table 1 here]

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 group consisted of 117 workers, and the Public Treatment group consisted of 124 workers.[16]

We made it clear to all the operators on the floor that their rank performance would not be rewarded or punished in any other way. There were no monetary rewards for better performance (beyond the higher wages implied by piece rates). There was also no system for promotion of any type. Nonetheless, workers might still have been under the impression that the best performances would somehow be rewarded by the management, and that bad performances would be punished (e.g., they would be fired). In this case, any response from either of the treatment groups could also be driven by incentives for rewards or by fear of punishment. Although such beliefs could not be eliminated completely, even if they existed, they should have existed only in the first months of the treatment, after which

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

[16] 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.

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 concerns for punishment or reward 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. The top management of the factory agreed to implement the experiment as its own management practice, and so they passed down the orders to the production manager on the floor, who in turn conveyed the message to the supervisors. Thus, 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, giving us a total of 9 treatment months (excluding January 2016 when the workers did not receive any rank information but their performance counted towards rank computation). At no point did we mention an end date to the experiment.

Over July–August 2016, the factory hired 95 additional operators to the existing workforce. We also drafted these new operators into the experiment later. However, these workers started working at the factory knowing that there was a ranking system already in place. Hence, their responses to rank incentives might differ from those of the already existing workers. Although this might be interesting to consider in its own right, I leave them out from the analysis.

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 panels. 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 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.[17] 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.

It was important that workers would not be able to compute and compare ranks among themselves when they were not meant to (i.e., in the control group, or across Private and Public Treatments). Because the information on the actual production times was recorded centrally at the Distribution Section, the workers had no access to this information, and hence would be unable to compute their ranks independently. Nonetheless, there could be concerns that because the workers were paid on piece rates, information on total wages could help them deduce their ranks when ranking information was not available to them. However, ranks based on total production wages would not predict time-based ranks for two reasons. First, total production wages depended on the piece rates, which in turn, varied across styles. Moreover, the workers were 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, and these styles would not necessarily match the set of styles produced by another worker. Both of these reasons combined made it more difficult for workers to use wages to deduce time-based ranks.

In addition to the main components (front, back, and sleeve panels), a typical sweater requires a few supplementary parts, for example necks. These supplementary parts are assigned separately from the main panels, and usually have piece rates that allow workers to earn more than they would with other parts. Moreover, because these parts are small, one worker can produce a big batch of them in a short time. So, for a given style of sweater, it is not practical to assign these to more than a few workers. Consequently, the production of these parts for different sweater styles is rotated across different workers in different months, for fairness purposes.

We computed the actual ranks from only main sweater panels, and excluded any supplementary parts that a worker might have worked on. This was necessary because all the supplementary parts of a given sweater style would be produced by a

[17]This computation is shown in detail in the Appendix.

small number of workers, all of whom may not be in the same experimental group, making it impossible to compute style ranks for these parts. Leaving these out meant that the total production wages (which covered all styles and parts) were to yet another degree out of step with any independent predictions that workers might have attempted to make regarding time-based ranks.[18] Moreover, the production time used in ranking computations excluded pre-authorized leaves, but included all unauthorized absences. While this approach served to punish workers for taking unauthorized absences, including these absent times also distanced wage-based ranks from time-based ranks. The treatment letters contained all the details about how ranks were calculated, and this information was repeated every month to serve as reminders.[19]

Note that we did not provide workers with information on the actual time they took to produce sweaters. Hence, a worker from one treatment would not be able to compare himself with a worker from the other treatment. However, not knowing the distance needed to cover to achieve a better rank could have potentially discouraged them from trying in the first place. So, I followed Barankay [2012] and 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 information gave them an idea of how harder they would need to work, but they would not be able to use it to compare themselves with other workers from a different treatment group.

To sum up, the ranks computed from time were different from the ranks that could be computed from wages, and thus prevented workers from predicting their ranks when they were not meant to. Figure 1 shows how these two 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 rank not to be able to precisely predict time-based rank.

[Figure 1 here]

[18] Consequently, workers who worked on only supplementary parts in a given month were excluded from rankings.

[19] In addition, following the delivery of letters in the first few months of treatment, a member of the research team was available in each block at designated times set in advance to answer any question that the workers might have. The letters also announced this time in advance.

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 recorded only the dates a job was distributed and received, not the precise time of the day. The factory started recording the time to help compute ranks for this experiment. To negate any potential responses from the treatment groups just because of receiving the letters, or from the perception that they were being observed, all the workers in the control group also received letters at the end of every production month. These letters contained the following trivial information:

i Number of sweaters the worker produced in the previous month (broken down into styles and sizes)

ii Total time taken to produce the sweaters

iii Names of all the workers in his group, along with their card numbers and block numbers, sorted by first block and then card numbers

While (i) above was already known to the operators, (ii) was new information and important because it helped them to feel included in the experiment and negated any behavioural responses from treatment groups stemming from the feeling that their productions were being timed.

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 the following:

iv The worker's relative rank (in the previous month) among all the workers in the group

v The total number of workers, and the lowest rank in the group

vi What the worker's rank would have been had he improved his time by 5 percent, 10 percent, and 20 percent, ceteris paribus

While (iv) was the core treatment information that helped workers to identify their positions in the distribution, (v) was important because more than one worker could jointly hold ranks; thus, the total number of workers in that case would not be sufficient to identify the worker's relative position in the distribution. Moreover, this could vary across months because workers left or joined the factory, or simply, some workers might not have been ranked in a given month for reasons discussed earlier. Finally, (vi) helped workers to understand how much they needed to improve to progress in the ranking.

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 his group appeared with their respective ranks; this rank was also the variable by which we sorted the list.

To sum up, the control group received letters with information that was not highly informative, but served to produce the same potential effect of receiving letters or being timed as they would in the treatment groups. Treatment groups received the same information as the control group, but the former also received information about their ranks, either privately or publicly, depending on the treatment. 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. Below is a brief discussion of the key data from these sources.

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. These differences are statistically insignificant at traditional significance levels for all of the variables, implying that the groups were well balanced on these characteristics.

Administrative data also contained information on the block in which each worker belonged for each month of the sample period. The data show that it was extremely rare for a worker to change blocks. This implies that workers within a given block had worked in close proximity to each other at least since the start of the sample period in January 2013, or since they joined the factory, whichever came later.

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, the baseline survey collected information on the peers with whom a worker socialized outside the factory, and those with whom the worker regularly interacted while inside the factory. Because anecdotal evidence from the factory indicated that the workers were more closely connected to workers within their own blocks, and because it was impractical to ask about each of the 365 other workers individually, we focused relatively more on the networks within a worker's block. Specifically, we asked the workers to consider each of the other workers in their block, name by name, and tell us how frequently they talked to them and whether they socialized with them outside the factory.

It was not possible to perform the same exercise for all the other workers on the floor, because of the large length of time it would have taken to complete the survey. Instead, we asked them to name 10 workers from outside their own block to whom they talked frequently, or with whom they socialized. Of the 363 workers completing the network survey, 91 said they did not socially engage with anyone outside their block, while only 16 named 10 peers. The left panel in Figure 2 shows that, from within a block, the workers socialized with approximately eight co-workers on average; outside the block this average was approximately three.[20]

[20] Conditional on naming fewer than 10 peers, this number is 2.74.

The right panel of Figure 2 shows that the differences in number of friends is stark when computed as a share of total workers in the block or floor.[21] Consistent with anecdotal evidence, this suggests that the workers were more socially connected with peers inside their block than with those outside their block. Hence, for the rest of the paper, I will focus on this within-block network.

[Figure 3 here]

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. Table 1 indicates that the number of block peers with whom a worker conversed more than three days a week was higher in the Public Treatment group than in the control group. While there is slight imbalance in this measure, 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).

6 Main Results

In line with the theoretical framework discussed earlier, this section reports the main empirical results of the treatment effect. I will start with the average baseline effect, then test the effect of intrinsic status concerns (Observation 1), and finally move on to understand the effect of social concerns (Observation 2).[22]

6.1 Average Treatment Effect

I take advantage of the availability of long-period pre-intervention data for each worker (because workers had been at the factory for a long time) and use a difference-in-difference (DID) strategy to identify the treatment impact.[23] The key baseline specification for our analysis is therefore:

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

[21] The average number of workers in a block was 25, and hence that for the rest of the floor was 341.

[22] The pre-analysis plan mentioned all tests of the various heterogeneity done here.

[23] 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.

where Y_{it} is the outcome variable of interest, and almost always the logarithmic transformation of mean daily wage earned by worker i in month t, and which I use as the measure of worker productivity; mean daily wage is computed as total wages earned from producing all sweater parts divided by the total number of attendance days; 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 used in certain specifications. Given the limited number of workers in each experimental group, the DID approach strengthens the identification of treatment effect. Logarithmic transformation of the outcome variable helps to interpret the coefficients as percentage change in the outcome variable.

It is worth elaborating on X_{it} a little more. As mentioned earlier, production wages not only depend on how productive a worker is, they also depend on the piece rates of the styles on which a worker works. The piece rates may not always reflect the complexity of the styles, and hence wages alone are a noisy measure of productivity. 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. Hence, I use total production wages from all sweater parts and total attendance days to obtain a measure of their mean production wage per working day. This serves as the next-best measure of productivity for both before and after the experiment. However, to reduce the noise stemming from workers producing various styles with various piece rates, in X_{it} I include style fixed effects. These style fixed effects are different for each sweater style and part; thus, they not only control for the varying complexities of sweaters, but also for whether a worker worked on supplementary parts, which usually have higher piece rates. I also include here, depending on specifications, the monthly block size, which varies across months as workers quit or join the factory. However, for a given month it will be the same for all workers from the same block. So, strictly speaking, this varies only at the block and month levels. Ideally, I might have controlled for this with a block fixed effect, but there is little movement of workers across blocks.

I restrict the sample of workers to those who had been at the factory from before the start of the experiment. In other words, I exclude the 95 workers who were hired in the middle of the intervention because they may respond differently to the intervention than the others.

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 without fixed-effect and other controls; 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 the former split the treatment groups 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 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; however, the difference is statistically insignificant and, hence, inconclusive at this stage.

[Table 2 here]

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 fact that the overall treatment effect is close to zero is not entirely surprising. The theoretical framework of this paper did suggest that the treatment effect would vary depending on whether a given worker had overestimated or underestimated his rank prior to the intervention, and also on the shape of the underlying status-utility curve. Such heterogeneous responses could 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} in 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 that the worker had *underestimated* his rank earlier, and then received *positive* feedback.

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. Hence, reported expected rank and true rank in the treatment letter differ in two dimensions. First, the expected rank was based on wages, while true rank was based on time. Second, expected rank relates to a worker's expected rank based on the three months preceding the baseline survey; but true ranks in a given month relate to work in the immediate previous treatment month. Hence, computing δ_{i0} in the way described in the previous paragraph assumes that the expected rank from the baseline survey nonetheless reflects the rank that the worker would expect to receive in the treatment letters. Because rank based on long-term average wages (three months in this context) is a fairly precise measure of productivity, this is a reasonable assumption. 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.

[Figure 2 here]

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.[24] 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 negative positive feedback.[25] Control-group workers are similarly

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

[25]The fact that there were more workers who had previously overestimated their ranks (n=139)

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.

[Table 3 here]

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 did yielded much more 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 in nature, that is $H_{11}(.) > 0$.[26] Because different workers respond in two opposite directions, the net average treatment effect is close to zero, as was seen in Table 2.

I note here that workers were split into two categories based on the feedback they received in the first treatment letter; but the treatment effect was estimated from all the subsequent treatment months considered. Because workers received ranks

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]).

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

in every month of the treatment period, it is possible that they also responded to rank information in subsequent months after the first treatment month. I return to this issue in Section 6.4.

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 social concerns into social-status and conformity incentives, 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 in this experiment. 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. As mentioned earlier, here I consider only friends from one's own block, because baseline data show that workers were more likely to be friends with peers from the same block.

If the social pressure to conform was present, then it would be felt by workers who were relatively more productive than their friends; on the other hand, workers who were relatively less productive than their friends would feel no such pressure. 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 workers who were relatively more productive than their friends from the same block and the same treatment responded after the introduction of the treatment. To do this, I split the publicly ranked workers into two groups - workers whose baseline productivity was higher than the median among their friends, and workers whose baseline productivity was lower than the median among their friends. Baseline productivity is measured by the average daily wage from the whole pre-treatment period. Median is computed from a group that consists of a worker himself and all his friends from the same block and same treatment.

[Table 4 here]

Indeed, in Column 2 we see that publicly ranked workers who were relatively less productive than their friends increased their effort following the introduction of the intervention. Compared to these workers, workers who were relatively more productive decreased their effort by about six p.p. Also, it is not only that these relatively more productive workers decrease their effort compared to relatively less productive workers, the net effort from the former group 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 relatively more productive workers (than their friends) was induced by making the ranks public, we would not expect to see this in the Private Treatment. Indeed, when we do the same exercise with Private Treatment in Column 3, we do not see any such decrease in effort from privately ranked workers who were relatively more productive than their friends. 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.

[Table 5 here]

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.[27]

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

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). Treated workers eventually learned their ranks, but the control-group workers did not. Thus, after controlling for counterfactual responses from control-group workers with similar rank distances from their friends, whatever differential effect I pick up on the treated workers is from the information on ranks made known to them. Further, I use worker fixed effects to pick up within-worker response to monthly variations in ranks; using worker fixed effects ensures that I do not pick up anything that stems from time-invariant issues, such as a worker's baseline productivity (which is likely to be correlated with the worker's rank). I also use year-month fixed effects to cancel out any month-specific common shocks, and 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 response by similar workers in the Private Treatment. The net effect is shown at the bottom of the table, which 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.[28]

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. Indeed, 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

[28] 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.

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 is 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. In other words, a Public Treatment worker who was not ranked higher than his friends very weakly to social-status incentives. Social-status effect is similar in columns 2 and 3 - the differences in Public*Post and Private*Post coefficients are positive but statistically insignificant.

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 the negative effect of conformity present only with respect to friends, or do such effects also exist with respect to other reference groups? 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[29], or (same block and same treatment) peers

[29]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.

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 with whom they were not friends. This is consistent with our 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.[30] 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

In tables 3 and 5, I split workers into two subsets based on the type of feedback they received in the first treatment month. In other words, we interpret the treatment effect from all the intervention months as a response to a single piece of information, ranks in the first treatment month. Rankings were also delivered in each of the subsequent treatment months, raising the question of whether the workers respond to subsequent rank information, too.

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 treatment 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.

[Figure 4 here]

It is not surprising that a worker's response to the first feedback is consistent across all treatment months. A worker's response to ranking information would vary

[30] 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 such conformity in Private Treatment indicates that altruism is less likely.

across months if he learned something new from the latest information. 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 no new information. In column 1 of Table A-1, I check the correlation between workers' expected ranks as reported during the baseline and the true rank they received in the first month of treatment, February 2016. Column 2, on the other hand, reports the correlation between the rank they received in March 2016 and the rank they received in the previous month, February 2016. These are all cross-sectional regressions with only Private Treatment workers. The correlation coefficients indeed show that while the true ranks in February 2016 were only weakly correlated with the workers' expected ranks, the next rank they received in March 2016 was highly correlated with that from February 2016. Ranks in subsequent months are similarly correlated. Column 3 of Table A-1 shows the correlation of ranks between the last two months, June and July, 2016.

To check dynamic responses to this information more rigorously, I also check how workers responded to changes in their ranks across the previous two treatment letters. Table A-2 in the Appendix shows the results. Column 1 considers all workers. Columns 2 and 3 control for the first feedback they received by breaking the workers into subsets based on type of feedback in the first treatment month. In either case, we see little response to changes in their ranks across months.

6.5 Further Tests and Discussions

Previously, I interpreted the differential responses of workers to positive and negative feedback as workers re-optimizing their effort once they had learned about the true return to their effort. In particular, we saw that a worker who received a negative feedback in the first treatment month reduced his effort in all subsequent treatment months. Although it makes sense that, upon receiving negative feedback, a worker would become demotivated, would he then not work harder to achieve his perceived rank?

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 methods. The first method would pay them at a fixed piece rate for each successful shot. The second plan would pay them double that rate, but only if the worker scored more than a randomly chosen peer scored at the same game.[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. Indeed, this is what we see in Table 6.

[Table 6 here]

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 indeed responded more positively than those who were not competitive. The interaction terms are positive in both treatments, but statistically significant only for Public Treatment. The next two columns yield cleaner estimates. When split into subsets of workers 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 indeed reduced their effort upon receiving negative feedback. On the other hand, competitive workers did not give up so easily; to the contrary, they 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

[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].

the differences are not as stark. This would be as expected because, being competitive or not may not 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, there is another subtle difference between the two treatments. It is not the case that Public Treatment was different to Private Treatment only in that a worker's rank was visible to others. Public rankings also allowed the worker 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, I make use of another set of information collected during the baseline survey. More specifically, the baseline survey asked each worker in the factory to compare his own wages (from the previous three months) to those of each of the other workers in the same block. To check whether learning about others' ranks mattered, we check how workers in Public Treatment responded to peers' ranks that were different than the wage comparison made by the workers during the baseline survey. Table A-3 in the Appendix shows the results.

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. These shocks can be of two types: (a) peers that a worker had thought were comparatively less productive, but who, in fact, ranked higher than him in the previous month, or (b) peers the worker had thought were comparatively more productive, but who, in fact, were ranked lower than him in the previous month.

For Public Treatment in column 1 of Table A-3, 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 did indeed share 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 be more likely to share their rankings when they receive good news than when they receive bad news. Finally, in Table A-3 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 fact that Public Treatment workers reduced their effort when they were ranked higher than their friends be explained by complacency? 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 decrease (increase) effort when they receive positive (negative) feedback. This, in fact, can be interpreted as complacency effect. But instead, the empirical evidence suggests the contrary: workers instead increased their effort when given positive feedback, and hence we found empirical support for $H_{11} > 0$ instead.

Could any of the positive effect among workers be explained by fear of getting fired? 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. In any case, we can also check whether the fear of getting fired drove any results. In Figure A-1 in the Appendix, I consider workers whose pre-intervention wages were among the bottom quartile among all workers in the experiment. Then I estimate the treatment effect on these workers in each of the post-treatment months. If it were indeed the case that workers increased their productivity in fear of getting fired, this would be more so for the least-productive workers in the workforce. However, after the treatment had continued for a few months, they would have realized there were no such threat, and, thus, this effect would have faded away. Figure A-1 shows that the average treatment for the least-productive workers was indeed positive, but consistent throughout the intervention period. In other words, the treatment effects we estimated are unlikely to have been driven by the fear of getting fired.

Finally, following the start of the experiment, could there have been a redistribution of sweater styles among workers that might explain the results? This should not be a concern for us because we have almost always controlled for style fixed effects. Nonetheless, if we compare the results with or without the style fixed effects (results omitted for brevity), they remain the same.

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.

It is worth pointing out that the experiment in this paper was conducted in a developing country. While intrinsic motivation and demotivational effects are likely to be the same in developed and developing countries, social conformity, arguably, may be particularly strong in a developing-country context. Because of limited access to financial institutions, social capital may play a bigger role in a developing country to help workers cope with short-term shocks to income. This, in turn, makes social capital much more valuable in such a country. Hence, it is possible that workers in developing countries respond to social-conformity incentives more strongly than workers in a developed country. However, we do not have concrete evidence on this in the existing literature; this may be an avenue for future research.

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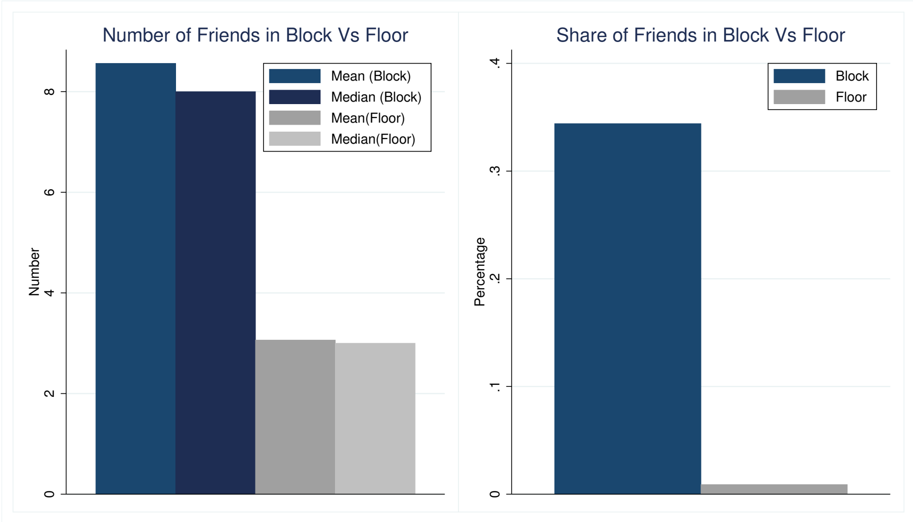
Figure 1: Comparison of Wage based Ranks to Time Based Ranks

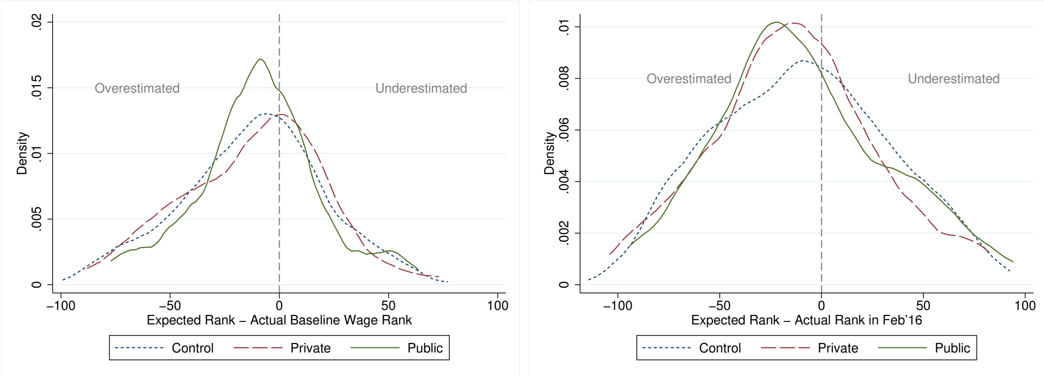
Note: The figure shows how ranks computed from mean production wage per day in each month (horizontal axis) correlates with actual time based ranks that were used in treatment letters (vertical axis). '20160X' refers to calendar month 'X' of year 2016. Time based ranks were fairly correlated with wage based ranks, as the wages were based on piece rates, and hence reflects actual productivity to some extent. Nonetheless, wage based ranks would not be able to precisely predict time based ranks because of additional noise introduced by how time based ranks were computed.

Figure 2: Social Network Within Block and Outside Block



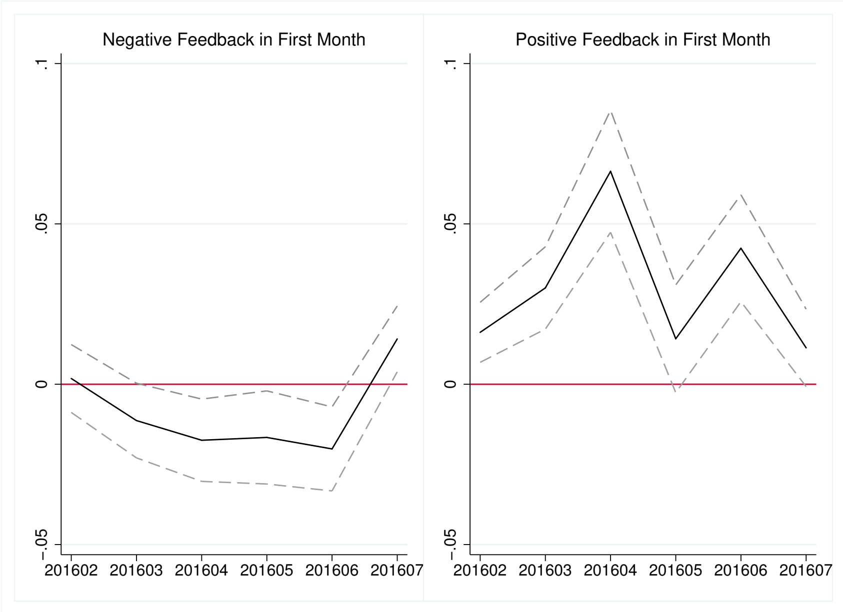
Note: Left panel of the figure above plots the mean and median number of friends workers have within their block, and rest of the floor outside their block. Instead of absolute numbers, the right panel shows the share of workers that a worker is friends with within his block, and rest of the floor. The two panels show that workers are more likely to have social ties with workers within block than outside block.

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. The distribution shows that workers were likely to both overestimate and underestimate their ranks, with a heavier mass for the first group. Separate distribution plots for control and treatment groups show they are largely balanced across experimental arms.

Figure 4: Treatment Effect Across Months (Private Treatment)



Note: The figure above 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, and shown with solid lines. Errors are clustered at both worker and year-month level. 90% confidence intervals around the coefficients are shown with dashed lines. The plots show that the impact of both negative and positive feedback from first treatment month was consistent across the whole treatment period.

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. The table shows that on average, treatment effect in either treatment is close to zero.

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

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(Wage)	Ln(Wage)		Ln(Wage)	Ln(Wage)	
	Positive Feedback ($\delta_0 < 0$)	Negative Feedback ($\delta_0 > 0$)	Cols [1] - [2]	Positive Feedback ($\delta_0 < 0$)	Negative Feedback ($\delta_0 > 0$)	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. The table shows that workers who received positive feedback in the first treatment month increased effort in subsequent months, while those who received negative feedback decreased effort.

Table 4: Conformity towards Friends - With Baseline Productivity

	(1)	(2)	(3)	(4)
	Ln(Wage)	Ln(Wage)	Ln(Wage)	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. The results show that, relative to Private Treatment, workers in Public Treatment reduced their effort whenever they were ranked higher than their friends.

Table 6: Fightback from Competitive Workers

	(1) Ln(Wage)	(2) Ln(Wage)	(3) Ln(Wage)
		Pos.Feed.	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. The results show that among treated workers who received negative feedback in the first treatment month, workers who were more competitive in nature fought back and exerted more effort relative to those who were less competitive in nature.

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. In other words, for worker i in $t = 0$, e^*_{io} sets to zero the following net marginal utility of effort:

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

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. Although the realization for ϵ_{it} might vary across time, while computing expectation over all possible values of ϵ, the time subscript becomes redundant. Also, there is no ϵ_{-i} in z^*_{it} since $E(\epsilon_{it}) = 0$ by assumption.

Once the true ranks are revealed in $t = 1$, if worker i keeps his effort at e^*_{io} and takes everyone else's effort as given at $e^*_{-i,o}$, the net marginal utility evaluated at e^*_{io} is:

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

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:

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

Therefore, in $t = 1$, holding everyone else's effort fixed at $e^*_{-i,0}$ worker i will have the incentive to deviate from his effort from e^*_{i0} if $\Delta_i \neq 0$. But whether he increases or decreases his effort in $t = 1$ depends on the sign of Δ_i. If $\Delta_i > 0$, the marginal benefit at effort level e^*_{i0} in $t = 1$ outweighs the marginal cost of effort at e^*_{i0}. 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, 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 $H_{11}(.)$. I examine these cases below.

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

Under the case where $H_{11} > 0$, the value of the integral in Equation 8 is positive. Then the sign of Δ_i is determined entirely by the sign of δ_{i0}. If worker i underestimated his rank in $t = 0$, that is $\delta_{i0} < 0$, $\Delta_i > 0$ and hence worker i increases his effort in $t = 1$ relative to $t = 0$. Conversely, if the worker overestimated his rank in $t = 0$, that is $\delta_{i0} < 0$, $\Delta_i > 0$ and hence worker i decreases his effort in $t = 1$ relative to $t = 0$.

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

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

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 $t = 0$.

Proof of the above propositions relies on the assumption that a given worker takes everyone else's effort constant, and thus his equilibrium response is solely determined from his first order conditions. To the extent that his equilibrium response considers how other workers might change their behaviour (and hence feedback into $\tilde{e}_{-i,t}$ in his equilibrium response) the proof is an oversimplification.

However, it is indeed more likely that a worker's equilibrium response would hold $\tilde{e}_{-i,t}$ constant. Notice that when we start with e^*_{io}, it already considers an equilibrium response from other workers in period $t = 0$, however that equilibrium is determined. In period $t = 1$, to solve for the equilibrium completely, a worker would need to know $\delta_{-i,0} > 0$ of everyone else so that he can compute e^*_{-i1} and substitute this into his own first order condition. Additionally, he will also need to know α_{-i}. But both of these are private information. So, at least in the first month of treatment, he would not be able to know them. In the subsequent months, he could try to infer $\delta_{-i,0} > 0$ if he had known how others' ranks change, but the scope to learn is limited since worker i observes only his own rank. Any variation in his own rank would be caused by ϵ_{it} as well as $e_{-i,t}$, and $\epsilon_{-i,t}$. So only from his own rank it would be impossible to deduce what $e_{-i,t}$ is, especially when the number of peers is very large. Thus the best worker i can do is to assume everyone else's effort constant.

Alternatively, worker i can form an expectation of $\delta_{-i,0}$ and α_{-i}, and thus solve for equilibrium effort. But this would not only make the model significantly complex, and will also make the predictions vulnerable to what i's expectations about those parameters are, or how such expectations are formed.

A.2 Observation 2

In the instance 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 $t = 1$.

In the instance 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 $\frac{\partial^2 M(.)}{\partial e_{it}^2} > s_i \frac{\partial^2 H(.)}{\partial e_{it}^2}$, which translates to $M_{11}(.) > \left(\frac{n-1}{n}\right)^2 s_i H_{11}(.)$. 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 $x > \tilde{x}$.

B Style Rank Calculation

Ranks for each month were computed in the following three steps. In the first step, for each operator i of treatment group $g \in \{Private, Public\}$, and for each style and size $s \in S_i^g$ where, S_i^g is the set of all style-sizes that i worked on in a given month, his style-rank t_{is}^g is computed by comparing the average time he took to complete one set of sweater parts for style s to that of others in group g who also worked on s. $t_{is}^g \in I$ and $1 \geq t_{is^g \leq n_s^g}$ where n_s^g is the number of operators in group g working on style s, and a higher numerical value for t_{is}^g indicates a worse performance. The total time computed for each job includes the full working hours of a day when an operator was absent without prior notice, but excludes any days he had taken a prior leave for, or days when he is grated a medical leave. Styles where less than three workers (from the same treatment group) worked on were excluded.

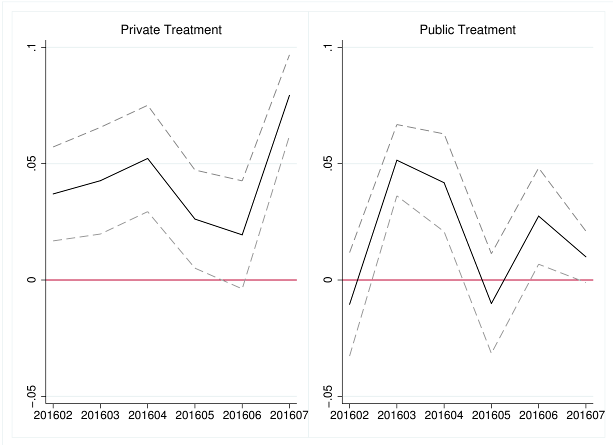
In the second step, a weighted average of normalized style-ranks T_i is computed from all the style-sizes that i worked on. Ignoring the superscript for group, it is given by:

$$T_i = \sum_{s \in S_i} \left[\left(\frac{t_{is}}{m_s} \right) \left(\frac{q_{is}}{q_i} \right) \right] \tag{12}$$

where, m_s is the lowest rank (highest numerical value) for style s, q_{is} is the number of sweater sets of style s that i produced, q_i is the total number of sweater sets that i produced. $\left(\frac{t_{is}}{m_s}\right)$ normalizes each style-rank with respect to the lowest rank in that style, and thus it becomes comparable across styles. On the other hand, $\left(\frac{q_{is}}{q_i}\right)$ weighs each of the normalized style-ranks with respect to the share of a given style in an operator's total production of a month. Thus, the rank puts more weights on styles and sizes for which an operator produced relatively more than those where he produced less. I ignore the cases where only one worker worked on a given style and size. In the final step, the set of final ranks for treatment group g is given by $\{R_1, R_2, R_3, \ldots, R_N\}^g$ where N is the size of group g and $R_i \leq R_j$ *iff* $T_i \leq T_j$ for $i, j \in \{1, 2, \ldots, N\}$.

C Additional Figures & Tables

Figure A- 1: Treatment Effect on Least Productive Workers by Months



Note: The figure above shows treatment effect in each month of the whole treatment period, for worker whose wage in pre-intervention period was in the bottom quartile. Left panel shows monthly treatment effects for Private Treatment while the right panel shows the same for Public Treatment. Underlying regressions are similar to those used for main difference-in-difference analysis and includes worker fixed effects, style fixed effects and block size control. Coefficients are plotted with solid lines. Errors are clustered at both worker and year-month level. 90% confidence intervals around the coefficients are shown with dashed lines. The plots show that the positive treatment effect on least productive workers was consistent across the whole treatment period.

Table A- 1: Correlation Between Expected Rank
True Ranks (Private Treatment)

	(1) Rank Feb'16	(2) Rank Mar'16	(3) Rank Jul'16
Expected Rank	0.327***		
	(0.101)		
Rank in Previous Month		0.681***	0.609***
		(0.0677)	(0.0725)
Constant	43.78***	17.17***	21.12***
	(5.336)	(4.548)	(4.729)
Observations	112	113	110
Adj. R-Sq.	0.0787	0.472	0.390

Note: Column 1 shows correlations between workers' expected ranks as reported during baseline, and the true ranks they received in their first treatment month, February 2016. Column 2 shows correlation between ranks in March 2016 and February 2016, while column 3 shows correlation between ranks in July 2016 and June 2016. All regressions are cross-section regressions based on Private Treatment workers only. *, **, *** indicate statistical significance at 10%, 5% and 1% significance level respectively. The results show that the correlation between true rank and workers' expected rank is weak, while that between true ranks in consecutive months is high.

Table A- 2: Dynamic Response to Ranks

	(1) Ln(Wage)	(2) Ln(Wage)	(3) Ln(Wage)
		Pos.Feed.	Neg.Feed.
Private * Post	-0.00354	0.0197	-0.0176
	(0.00889)	(0.0139)	(0.0126)
Private * Post * Positive Change in Rank (Prev 2 Mths)	0.00787	0.00705	0.00451
	(0.0138)	(0.0138)	(0.0177)
Public * Post	0.00155	0.0176	-0.00723
	(0.00758)	(0.0139)	(0.0105)
Public * Post * Positive Change in Rank (Prev 2 Mths)	-0.00871	-0.0165	-0.00444
	(0.00938)	(0.0106)	(0.0149)
Post * Positive Change in Rank (Prev 2 Mths)	0.00185	-0.00133	0.00453
	(0.00856)	(0.00966)	(0.0135)
Observations	13,876	5,631	8,071
N(Worker)	366	146	216
Constant	YES	YES	YES
FE: Worker, Year-Month, Style	YES	YES	YES
Other Controls: Block Size	YES	YES	YES

Note: *1(Positive Change in Rank (Prev 2 Mths))* refers to a dummy variable that takes the value 1 if a worker observes his rank improve in the previous two months. 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. The results do not provide any evidence of dynamic response to ranks.

Table A- 3: 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. The table shows that conformity behavior among Public Treatment workers were not driven by workers updating knowledge about their peers' relative ranks.