Sotto Voce: The Impacts of Technology to Enhance Worker Voice*

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Abstract

We conducted a randomized controlled trial in which we enabled workers to directly and anonymously communicate with HR through mobile-phone-based technology. Treated workers were much more likely to know about the tool than controls, though usage was low: only 5% of treated workers reported ever using it. Despite this, turnover and absenteeism were 10% and 5% lower, respectively, for treated versus control workers. Together these findings suggest a substantial option value of enhancing worker voice, which can promote positive workplace behaviors even when workers do not directly avail themselves of the technology.

Keywords: voice, exit, turnover, absenteeism, quiet quitting, productivity, readymade garments, India
JEL Classification Codes: D32, J28, M50

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1 Introduction

Relationships between managers and workers are at the heart of much of what we know about the form and function of organizations (Bandiera et al., 2007; Bloom and Van Reenen, 2007; Hoffman and Tadelis, 2021; Ichniowski et al., 1997; Lazear et al., 2015). It is not uncommon for these relationships to become strained from time to time, threatening worker wellbeing as well as firm growth (Akerlof et al., 2020; Coviello et al., 2020; Freeman, 1980; Freeman and Medoff, 1984; Krueger and Mas, 2004; Sandvik et al., 2018). In such instances, both parties face a choice: voice their frustrations and improve relations, or exit the employment relationship (Hirschman, 1970). Given that the costs of separation can be large for firms as well as workers (especially in high-search cost environments), the ability to communicate effectively and induce change in the employment relationship may hold substantial value for both.

Our study asks: does amplifying workers' voices – that is, enabling direct communication of grievances and concerns with employers – causally impact workplace behavior and exit decisions? We report the results of a randomized controlled trial (RCT) in which we sought to enhance worker voice by providing access to a technology that enabled direct, anonymous communication with human resources (HR) representatives. We partnered with Labor Solutions, a technology-focused worker engagement firm, and Shahi Exports, a large garment manufacturer in India, to evaluate the impacts of a mobile phone-based worker voice tool called WOVO.

Prior to the introduction of WOVO, formal channels for Shahi workers to communicate their work-related grievances were limited. WOVO allows workers to communicate anonymously with their factory's HR department via SMS. When a worker sends a message, HR representatives receive the communication via a dashboard, which they can use to sort the message, flag appropriate parties, and send responses to the worker. The key appeals of the tool for workers are its anonymity and its ease of use. We hypothesized that enabling voice among workers might, via the logic of Hirschman (1970), allow the firm to identify workers' concerns and help resolve them, and that these improvements could reduce worker turnover as well as change workplace outcomes like absenteeism and productivity.

We evaluated the impacts of WOVO via a randomized encouragement design. First, the HR departments of two garment factories near Delhi were introduced to and trained in the use of the tool, which was completely new to the firm, in late 2018. Then, workers in the randomly assigned treatment group took part in a training session meant to familiarize them with the tool and its features through real-time demonstrations and hands-on practice. The training also stressed the anonymity of messages, which anecdotal evidence suggested was critical to workers' decisions to share frustrations with managers. Treated workers also received SMS reminders during the course of the evaluation period, which helped to maintain the tool's salience. We find that the randomized training approximately doubled awareness of WOVO and increased trust in the tool (but not other channels of voice). Usage of the tool also increased due to treatment – though overall engagement remained low – from essentially 0 in the control group to approximately 5 percent in the treatment group.

We study three important workplace behaviors by matching workers to the firm's administrative data: retention, absenteeism, and productivity. We find that treated workers were ten percent less likely to quit ($p < 0.1$) by the end of the experiment, relative to control workers whose average attrition was about 16 percent. Enhancing voice also changed worker behavior on the intensive margin: absenteeism declined by approximately 5 percent ($p < 0.05$) relative to the control group. Finally, worker productivity (measured as the quantity of operations completed over the target number per unit time) and wages were essentially unchanged.[1]

Taken together, the magnitudes of the changes in workplace behavior are quite striking, given that actual use of the tool was very low. This suggests either a very large treatment on the treated (TOT) estimate of direct effects of engagement on workplace outcomes, or, alternatively – and perhaps more likely – that treated workers who did not directly engage with the technology were nevertheless impacted by it. This latter case would suggest that the tool created option value. That is, even workers who did not have a specific grievance to address during the study period may have still gained utility from knowing about the existence of a reliable anonymous tool they would be able to use if the need ever arose. This could have increased retention and decreased absenteeism even for workers who did not directly avail themselves of the amenity. A similar result in the context of worker referrals is documented in Friebel et al. (2019): the implementation of an employee referral program in a large grocery chain increased retention even in stores where no referrals were made, most likely because workers valued being involved in the hiring process.

[1] We can reject with statistical confidence that productivity and wages changed by even 2 percent.

Our results add to the rich economics literature on the role of voice in shaping labor market dynamics. An older strand of this literature focuses on voice as represented by collective bargaining via unions (Freeman, 1980). More recent work has emphasized the endogenous emergence of voice – often through "counterproductive" workplace behavior – in response to a negative shock in the employment relationship, e.g., a pay cut (Coviello et al., 2020; Sandvik et al., 2018), a mass layoff (Akerlof et al., 2020), or the hiring of replacement workers (Krueger and Mas, 2004). Several very recent studies, which relate most closely to our work, induce experimental variation in voice for frontline workers in low-income contexts. Adhvaryu et al. (2021) directly elicits voice using employee satisfaction surveys in India; Cai and Wang (2020) makes managers' bonuses an explicit function of worker evaluations in China; and Boudreau (2020) enforces labor regulation related to worker safety committees in Bangladesh.[2]

We build on these recent advances in two ways. First, unlike Adhvaryu et al. (2021) and Cai and Wang (2020), we enhance the worker's *ability* to voice concerns, rather than eliciting or valuing feedback directly. Conceptually, this difference helps us understand whether simply knowing that it is possible to express one's concerns to management (even when concerns come up only sporadically) can increase utility enough to affect workplace behavior. Moreover, in contrast to Adhvaryu et al. (2021), who create a worker feedback mechanism at a "special" (fraught) moment in time for workers, in which annual wage adjustments were far below expectations, we study worker voice in a more business-as-usual context, increasing the generalizability of our results.

Second, while the general idea of opening a conduit for communication to upper management is similar to the worker committees studied in Boudreau (2020) (as well as Jäger et al. (2021) and Harju et al. (2021) in high-income contexts), there are two meaningful differences in the type of channel used in our study versus these recent papers. The aforementioned studies enable voice through committee proxy, i.e., worker representation at the board or upper management level. The technology-enabled increase in worker voice that we study prioritizes anonymity and the creation of a direct link to management. While worker representation via internal committees is typically found to have no significant effects on retention and other workplace outcomes, we hypothesize that the novel features of this technology – anonymity and a direct link – could hold substantial value

[2] Jäger et al. (2021) and Harju et al. (2021) study causal but non-experimental increases in worker voice at the firm level in Germany and Finland, respectively.

to workers, enough to create discernible changes in these outcomes.

We also contribute to the literature on managerial quality, in particular the importance of people management skills (Hoffman and Tadelis, 2021). This literature emphasizes the fact that managers' relationships with their team members are a key driver of firm success (Adhvaryu et al., 2019; Bloom et al., 2019; Bloom and Van Reenen, 2007), and demonstrates that interventions that improve managers' abilities to communicate effectively and better lead their people can generate substantial gains in productivity (Bertrand and Schoar, 2003; Macchiavello et al., 2020). Our study complements this work by showing that technology designed to create an easy-to-use channel of communication between front-line workers and managers can have meaningful impacts on workplace outcomes.

2 Background

This section provides background on our firm partner, Shahi Exports, including an overview of the firm's voice-related mechanisms for workers prior to the intervention. We then describe the details of the new worker communication tool and RCT.

2.1 Context

Shahi is a contract manufacturer that produces apparel orders for large multinational brands. Like most of the ready-made garment industry, frontline workers in Shahi factories are paid close to the minimum wage. Most Shahi employees work in the sewing department, where garments are manufactured in production lines, each of which is managed by a supervisor. In addition to sewing, other departments include cutting (where fabric is cut into parts to be stitched into a garment), finishing (where garment quality is checked and garments are packed for export), and other smaller departments that are not involved in the production process (e.g., maintenance or finance).

In Shahi factories, the most common way workers attempt to address workplace grievances is by directly communicating with supervisors or HR representatives. In addition, Indian law mandates the presence of worker committees, comprising of representatives of workers and the employer (similar to those studied in Boudreau (2020)). Another grievance redressal mechanism is the use of suggestion boxes, where workers can submit complaints anonymously in writing (though these are rarely used in practice). The multinational brands that source from these factories also conduct independent audits (directly or through an external agency) to monitor the adherence of the factory to workplace standards.

2.2 Worker Communication Tool

In late 2018, Shahi launched an SMS-based communication and grievance redressal tool in two of its factories in Delhi National Capital Region.[3] The tool, provided by Labor Solutions (formerly Workplace Options), is an anonymous interactive platform for SMS-based grievance redressal.[4] Each of the two factories received a unique phone number to which workers could send their suggestions or grievances. Workers bore the costs of sending SMSs.[5]

Worker-generated SMSs appear on a dashboard operated by a central HR administrator within each unit. The tool masks the worker's phone number: only information on gender, department, factory floor (pre-filled from the Shahi administrative database), and previous messages from the same number are visible on the dashboard screen. Based on the content of the SMS, the central administrator allocates the case to an HR representative. The HR representative can ask for more information from the workers through the platform via SMS. The HR representative is then responsible for entering details about the case and how it was handled in the platform. Once a case is resolved, the representative closes the case on the dashboard, informs the worker, and asks for feedback. Resolved cases are reviewed and monitored by a central team.

2.3 RCT Timeline

2.3.1 HR Training

The HR representatives were first trained to use the platform in August 2018. They were provided with guidelines about the timeline of a response, interacting with workers, and how to close a case. They were trained to ask for personal identifiers only when it was necessary to solve the grievance (in cases involving a salary error, for example). A refresher training session summarizing this content was conducted just before the launch of the tool in November 2018.

[3]This initiative was funded by The Children's Place, a multinational children's brand.

[4]Website: https://www.laborsolutions.tech/

[5]Most phone carriers include a fixed number of free SMS messages as part of their phone plans. If not part of the bundle, messages typically cost about 1 INR (14 cents in 2019 USD).

2.3.2 Randomization

We randomized the approximately 7000 workers in two experimental factory units into a treatment and control group, stratifying by department or production line, depending on the type of worker. Specifically, for sewing (production-line) workers, each production line represented a unique stratum. For the remaining workers, each department (e.g., cutting, maintenance) was considered a separate stratum. Within each stratum, we randomly assigned half of the workers to treatment and the remaining to control.

2.3.3 Intervention

The tool became operational in November 2018. It was available to all workers in the two experimental units, but we used a randomized encouragement design to generate variation in knowledge and usage. Workers in the randomly selected treatment group were trained in the tool and its features. In the first factory unit, worker training started in late November 2018, and in the second factory, training started in early December 2018. The training took place in batches – approximately two batches of 30 workers per day. During these 45-minute sessions, the tool's anonymity and functionality were demonstrated to workers in order to build trust and familiarity.

Over the course of the trial, treatment workers were sent a total of ten reminder SMS messages. These messages reminded workers about the service, encouraged them to send their suggestions and grievances, asked them to use their registered number to send messages, and provided them with information on the number of cases received and resolved. Due to technical difficulties, each set of SMS reminders was typically only successfully delivered to around 20% to 30% of intended recipients.[6] For this reason, we consider the main component of this treatment to be the initial training session.

The control group received no training or SMS messages. However, many still found out about the tool, most likely through coworkers: at endline, around 45% of the control group reported being aware of it. Control workers had the same access to the tool as treated workers, though nearly no one in the control group reported having used it at endline.

[6] The first SMS was sent on 15 January 2019 and approximately two messages were sent per month. The low success rate was mainly due to incorrect phone numbers of workers in the administrative records and bulk messages getting blocked for some numbers by the telecom service provider.

The platform generated a monthly report that included the number of cases, types of cases, average time to respond, and average time to complete a case. The median HR response time across both factories was 48 hours, while the median time to case closure was 84 hours. Table A1 provides a breakdown of the number of cases received by case type, along with examples for each type. A wide variety of cases were received, ranging from complaints about canteen food to reports of sexual harassment.

The funding for this initiative supported the tool's operation through June 2019, after which it was discontinued.

3 Data

3.1 Administrative Data

Firm administrative data provide us with information about worker tenure, basic demographics (like age and gender), as well as quit dates, attendance, and productivity. We use the attendance data to calculate monthly absenteeism for each worker, from May 2018 (6 months before treatment start) to July 2019 (the month of the tool's discontinuation).

We also have daily individual productivity data for a subset of production line workers. This provides us with, for each line and item on which an individual worked on a given day, a measure of productivity, defined as the number of pieces produced divided by the target number of pieces for that particular clothing item. To drop outliers that are likely the result of reporting error, we trim at the 1st and 99th percentiles of the raw productivity measure. Unlike attendance, collection of productivity data only began in late October 2018 (about one month before treatment start); these data are available through July 2019.

Table 1 reports summary statistics for all workers who were at the firm when the treatment started in November 2018. All workers were randomly assigned to either treatment or control, and there are no statistically significant differences between these groups in terms of tenure, age, gender, education, and absenteeism prior to the introduction of the tool. The average worker is 34 years old, has been at the firm for slightly over 4 years, and has completed 9 grades of schooling. Over half of the workers were female, and average absenteeism was 8.5%. Among workers for whom productivity data were available, Table 1 shows no statistical differences in productivity across treatment and control.

3.2 Survey Data

From the full population of workers in the two factories, we selected approximately 2600 workers to survey. Production line workers were over-sampled: 45% of all production line workers were included in the sample, compared to 30% of non-production workers. We administered a baseline survey to this sample in August 2018. The endline survey was conducted in May/June of 2019.

Both the baseline and endline surveys collected information about perceptions of HR responsiveness and awareness. We asked the following questions to workers: How aware is the HR/management about issues on the floor? How responsive is the HR/management about issues on the floor? Workers responded using a 5-point scale, from "not responsive/aware at all" (1) to "extremely responsive/aware about all issues" (5). For each worker, we averaged the responses to these two questions for a combined measure of responsiveness and awareness, which we refer to as "HR responsiveness" in the remainder of the paper.

Both surveys also included questions on job satisfaction, subjective wellbeing, and mental health. For job satisfaction, we asked workers how satisfied they were with their current job/position, current wage, supervisor, grievance redressal system, and overall workplace environment, to which they responded on a 5-point scale ranging from "extremely dissatisfied" (1) to "extremely satisfied" (5). We averaged the answers to these questions to generate a composite job satisfaction measure. We used the Kessler psychological distress scale (K10) to measure mental health. This score is generated by summing the 5-point responses to 10 different questions. This scale ranges from a minimum of 0 to a maximum of 50, with cutoff scores of 20, 25, and 30 typically used to indicate mild, moderate, and severe psychological distress, respectively. Life satisfaction is measured using Cantril's ladder, in which a response of 10 means the respondent thinks she is living her "best possible life" and a 1 corresponds to the "worst possible life."

In Appendix Table A2, we report summary statistics restricting to workers who completed the baseline survey. The average characteristics of these workers are almost identical to the characteristics of the full sample (and preserve balance across treatment and control), although the surveyed workers are more likely to be female due to the oversampling of (disproportionately female) production line workers. This table also provides summary statistics for the survey measures described above, collected at baseline. None of these survey measures are significantly different across treatment and control. 30% of the baseline survey respondents in the control group and 29% of the treatment group did not complete the endline survey (conducted around 8 months after the baseline). Though the difference is not statistically significant, the magnitude (1.5 percentage points) is consistent with differences in retention that we document in section 4.2.

Finally, the endline survey included questions about workers' knowledge and use of the worker communication tool. The endline also included questions about how much workers trusted various people and systems to address their grievances. Specifically, workers were asked how much they trusted their coworkers, their supervisor, HR, management, the suggestion box, worker committees, and the communication tool to solve their grievances. Workers responded using the following 5-point scale: "do not trust at all" (1), "do not trust very much" (2), "neither trust nor distrust" (3), "trust somewhat" (4), and "trust completely" (5). We look separately at workers' trust in the communication tool and workers' trust in all other channels (for which we average across trust measures relating to coworkers, supervisor, HR, management suggestion box, and worker committees).

4 Results

4.1 Knowledge, Use, and Trust

In Table 2, we report treatment effects on workers' knowledge and usage of the tool, as well as trust in the tool and other grievance redressal mechanisms. We regress these knowledge, use, and trust variables on a treatment dummy, strata fixed effects, and (only in even-numbered columns) demographic controls. Columns 1 and 2 show that workers in the treatment group are 38 percentage points more likely to report being aware of the tool – a statistically significant and large effect relative to the control group mean of 45%. In addition to being more aware of the tool, treatment workers are significantly more likely (over seven times more likely) to have used the tool (columns 3 and 4). However, tool use is indeed quite low in *both* groups: essentially 0% in the control group and about 5% in the treatment group. Conditional on being aware of the tool, treatment workers report stronger trust in the tool than control workers (columns 5 and 6). Trust in other grievance redressal channels is not significantly different in the treatment and control groups (columns 7 and

8).

Together these results suggest that the main "first stage" effect of the treatment was to increase worker awareness of and trust in a new technology to express grievances. Given the low usage rates, any treatment effects we find on workplace outcomes are unlikely to be driven by the actual use of the tool and are more likely the result of option value created by the tool's availability.

4.2 Retention

We next explore treatment effects on retention. Figure 1 separately plots cumulative quit shares among treatment and control workers, starting at the beginning of the experimental period until the end of July 2019, when the intervention ended. Both lines are fairly smooth, but there are occasional discrete increases throughout the study period (at around 100 and 130 days from treatment start, for example), reflecting the fact that quit rates tend to be higher at the end of a month. There is no discrete jump at the time the first SMS reminder was sent out in mid-January, consistent with the fact that only 20% of these messages were successfully delivered.

About a week and a half into the experiment, the solid control line begins to separate from the dashed treatment line. This separation takes place at the end of November. The control line remains above the treatment line throughout the rest of the period, indicating that a larger share quit in the control group compared to the treatment group by the end of the study period.

The early separation of the treatment and control lines is consistent with the idea that the tool may have generated option value and increased workers' utility simply by providing a way for them to communicate concerns effectively. Under this scenario, it is the mere availability of the tool that matters. We would therefore expect treatment-control differences to emerge soon after the introduction of the tool, which appears to be the case in Figure 1.

We evaluate the magnitude and statistical significance of the attrition effect in Table 3, which reports the results of the following hazard regression:

$$\lambda_{ij}(t) = \lambda_0(t) \exp\left(\beta_1 T_i + \gamma X_i + \mu_j\right), \tag{1}$$

where $\lambda_{ij}(t)$ denotes the instantaneous probability of worker i in strata j quitting at time t (measured in days relative to the treatment start), conditional on being still employed at time t. T_i is an indicator variable equal to 1 if worker i belongs to the treatment group and μ_j denotes fixed effects for randomization strata (defined by unit and department or production line). We show results with and without X_i, a vector of basic demographic controls (education, tenure, and age category dummies, including a dummy for those missing the education variable).

In Table 3, estimated coefficients indicate that quit rates were 6 to 10% lower in the treatment compared to the control group (hazard rates are 0.94 and 0.90 for columns 1 and 2 respectively), implying that the technology-enabled increase in voice indeed decreased worker exit. The estimate in column 1 is just shy of statistical significance at conventional levels, though we argue that controls for length of tenure (excluded from this regression) are important because the shape of the hazard function likely varies based on the amount of time a worker has been at the firm. The estimate in column 2, which controls for tenure fixed effects in addition to age and education, is significant at the 10% level. The effect size of 10% in this column corresponds to a treatment-control difference in quit rates of about 1 percentage point. As we discuss in section 6, this magnitude is non-trivial in terms of the implied reductions in firm recruitment costs, but it is not large enough to lead to substantial differences in the composition of treatment and control workers at the end of the study period, as we discuss in section 4.4.

4.3 Absenteeism and Productivity

Next, we estimate treatment effects on worker absenteeism using the following ANCOVA specification:

$$A_{ijt} = \beta_1 \bar{A}_{i0} + \beta_2 T_i + \mu_j + \delta_t + \epsilon_{ijt}. \tag{2}$$

A_{ijt} is the share of days worker i (in strata j) was absent in month-year t and $\bar{A}_{i0}$ is average monthly absenteeism for worker i across the six months before the rollout of the tool. We control for strata and month-year fixed effects (adding demographic controls in even-numbered columns) and cluster standard errors at the worker level. Attendance records are only available for workers still employed at the firm. In order to deal with potentially differential attrition rates by treatment status, we use a similar dynamic inverse probability weighting procedure as used in Adhvaryu et al. (2018).

Specifically, we weight each observation using the inverse of the predicted probability of being in the sample, based on a probit regression using the following as predictors: month-year fixed effects, treatment, demographic controls (education, tenure, age), month-year fixed effects interacted with treatment and each demographic control (separately), and month-year fixed effects interacted with treatment-by-control interactions (for each demographic control).

The first two columns of Table 4, which report the results for regression (2), show that absenteeism during the experimental period is significantly lower for workers in the treatment group. The effect size is 0.44-0.46 percentage points, which represents about a 5% reduction in absenteeism relative to the control group mean.

Next, we study impacts on productivity, using an ANCOVA specification analogous to the one in equation 2. In this regression, a unique observation is defined by a worker, production line, item, and date, since workers sometimes work on more than one line or more than one item on the same day. In addition to strata and month-year fixed effects, we now include item fixed effects, line fixed effects, day of week fixed effects, and line item days (the number of days the line has been working on the particular item) to absorb incidental temporal and team variation in productivity. We cluster standard errors at the line level. Because productivity is missing on days a worker is not present, we use the same inverse probability weighting procedure as described above. The estimates in columns 3 and 4 of Table 4 show that treatment had no discernible effect on worker productivity. In both columns, coefficients are positive, close to zero, and statistically insignificant. Standard errors are small enough to rule out large but imprecisely estimated effects: the upper bound of the 95% confidence interval is less than 0.01 (less than 2% of control group mean). Importantly, it is not the case that production workers were less affected by the intervention in general: in Table A3, we show that the magnitudes of attrition and absenteeism effects among this sub-sample are similar to those in the full sample.

4.4 Salary and Survey Outcomes

We also report treatment effects on worker salary and promotion. We estimate effects on salary using an ANCOVA specification. Results, reported in columns 1 and 2 of Table A4, reveal tightly estimated null effects. We investigate treatment effects on promotion using a cross-sectional regression where the dependent variable is an indicator for whether the worker was promoted between baseline and endline. Once again, both columns 3 and 4 reveal precise zeros. These null effects are perhaps unsurprising given that anonymity was a critical feature of the tool – in most cases those who used the tool remained anonymous to HR, which left little scope for direct positive or negative consequences to that worker. This, paired with the fact that productivity did not change, would imply, consistent with the evidence we find, that promotion and salary (which are tightly linked in this setting) was left unchanged as well.

Finally, in Table A5, we show that the treatment had no effect on other worker survey outcomes: perceived responsiveness and awareness of HR and management, job satisfaction, subjective well-being, and psychological distress. These null effects are unlikely to be due to differential attrition by treatment status. In theory, because quit rates were higher in the control group, this could have led to differences in the composition of treatment and control workers by the end of the study. In particular, if the least motivated, least productive, or least satisfied workers were the ones to exit the control group, this could result in an underestimation of the differences between treatment and control workers. The weighting procedure we use alleviates concerns about differential attrition by observable characteristics. While it does not address differential attrition by unobservable characteristics, we note that the difference in quit rates was small in magnitude and unlikely to result in large compositional differences across treatment and control group. As we show in Table A6, when we restrict to workers still at the firm by the end of the study (columns 1-3), or restrict to surveyed workers who responded to the endline survey (in column 4-6) the differences between treatment and control are small in magnitude and not statistically significant.

5 Alternative Explanations

We interpret our results as evidence that giving workers a means of communicating their concerns to the firm can improve retention and attendance, even if workers do not end up making use of this mechanism. However, the experiences of the treatment and control group differed in other ways – separate from the option value of voice – which could also be responsible for the effects we document.

Treated workers attended a training session that offered them some time away from their typical work activities and allowed them to interact with colleagues and superiors in a different setting.

This could have improved workers' perceptions of the firm, their relationships within the firm, or satisfaction with their jobs. Simply knowing that one was chosen for a special training program could have also driven similar improvements in treatment workers' views and attitudes. At the same time, control workers who discovered they were not chosen for this training program could have experienced the opposite effect – resulting in a gap between treatment and control driven by changes in the control group.

We argue, however, that any such effects are likely to be very small. The training session was a one-time program only 45 minutes in length. Training or informational sessions are not out of the ordinary for Shahi workers as they are sometimes required for various brand compliance initiatives. Given the short, one-time nature of the initial training, we would expect to only see effects immediately after the training. Contrary to this, in Figure 1, the treatment-control gap in retention grows over time.

The SMS reminders sent to the treatment group could have also generated an improved perception of the firm (by signalling to workers, for example, that the firm cared about their wellbeing). However, as discussed above, the SMS reminders were only successfully delivered to a minority of the targeted workers each time. Moreover, there is no indication of any discrete change in retention patterns after the first SMS reminder sent in mid-January (see Figure 1).

It is also worth noting that there were no treatment-control differences in how workers evaluated the responsiveness of HR and management, or their own overall job satisfaction in the endline survey (columns 1 and 2 of Table A5). It seems, therefore, that the effects on retention and absenteeism cannot be solely attributed to treatment workers being more satisfied in general (or control workers being less satisfied in general) with the firm and their jobs.

6 Rate of Return Calculation

To estimate the firm's rate of return on the tool, we combine information on the various costs associated with the intervention and our estimated treatment effects on retention and absenteeism. We consider three types of costs: subscription fees paid to the technology firm; training costs (which include productive time given up to take part in the training – for production workers and HR representatives – and the time of Shahi staff in charge of the training); and charges for reminder

SMSs. Together, these yield a total cost of 16,500 in 2019 USD.[7] Regarding benefits, the treatment reduced absenteeism by 0.44 percentage points (the more conservative estimate from Table 4) which translates into an additional profit of 10,000 USD.[8] In addition, the treatment increased retention by 10%, which translates into 57 fewer workers that needed to be recruited and trained (using the control group quit rate of 0.162 and treatment group sample size of 3,490). Discounted recruitment and training costs per worker are approximately 265.78 USD, which leads to a total of approximately 15,000 USD saved due to the increase in retention.[9] Attendance and retention benefits total about 25,000 USD, implying a net rate of return of approximately 52 percent.

7 Discussion

We report the results of an RCT of worker voice technology within an Indian ready-made garment firm. We show that enabling voice through a simple SMS-based tool reduces worker exit and absenteeism. Given that actual usage of the tool was quite low even among treated workers, our results suggest a substantial option value of voice: simply knowing that it is possible to communicate concerns effectively, even if concerns arise only sporadically, increases worker utility enough to change workplace outcomes. The magnitudes of estimated effects of this "light-touch" treatment are quite striking, particularly when considering that in a nearly identical setting, Adhvaryu et al. (2021) find that a much more intensive treatment – directly eliciting answers regarding satisfaction levels from each treated worker – results in almost the same impacts on both exit and absenteeism.[10]

From the policy perspective, our results demonstrate the promise of SMS-based tools that enhance the voice of frontline workers in low-income settings. Historically, frontline workers – particularly in settings in which collective representation is not the norm, as is the case in many low-income countries – have had very low levels of voice in the workplace. Indeed this is not a problem in low-income contexts only, as evidenced by the recent wave of so-called "quiet quitting"

[7]We purposely do not report costs for these three components separately given the sensitivity of information around the tool's pricing.

[8]We multiply this estimate by 228 (number of working days in our sample period) and 3,490 (number of treatment workers) to obtain a total of 3,501 worker-days gained. Multiplying this by 2.87 (profit per worker per day) yields an additional profit of 10,048 USD.

[9]Recruitment costs total approximately 286 USD per worker. We use an annual interest rate of 5% and assume – conservatively – that all of the benefits are accrued at the end of the sample period, 8 months after the costs were paid.

[10]The impact on exit in Boudreau (2020) is smaller in absolute terms but very similar in percentage terms to the treatment effects shown here, despite quite a large difference in channels across the two experiments.

(low levels of workplace engagement and effort) that has plagued the US workforce. Most of the narrative around labor rights in these contexts characterizes the relationship between management and frontline workers as adversarial. The private sector is nevertheless a key player in promoting widespread access to such tools, given that firms are often the ones who bear the pecuniary and administrative costs of creating technology-enabled channels for voice. Our research shows that firms have a clear incentive to invest in amplifying workers' voices via these technologies that goes beyond public perception and marketing value; such investments have sizable economic returns by reducing costly worker turnover and absenteeism.

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Tables and Figures

Table 1: Summary Statistics

	(1) Full sample	(2) Treatment	(3) Control	(4) Difference
Tenure (months)	48.8	49.3	48.3	-1.04
	(46.9)	(47.2)	(46.6)	(1.12)
Age (years)	34.1	34.1	34.2	0.12
	(7.77)	(7.77)	(7.77)	(0.19)
Female	0.63	0.63	0.64	0.010
	(0.48)	(0.48)	(0.48)	(0.011)
Years of Completed Schooling	8.75	8.78	8.72	-0.055
	(2.34)	(2.40)	(2.27)	(0.069)
Absenteeism	0.085	0.084	0.086	0.0022
	(0.100)	(0.098)	(0.10)	(0.0024)
Productivity	0.53	0.54	0.53	-0.0056
	(0.22)	(0.23)	(0.21)	(0.0090)
Observations	7046	3490	3556	7046

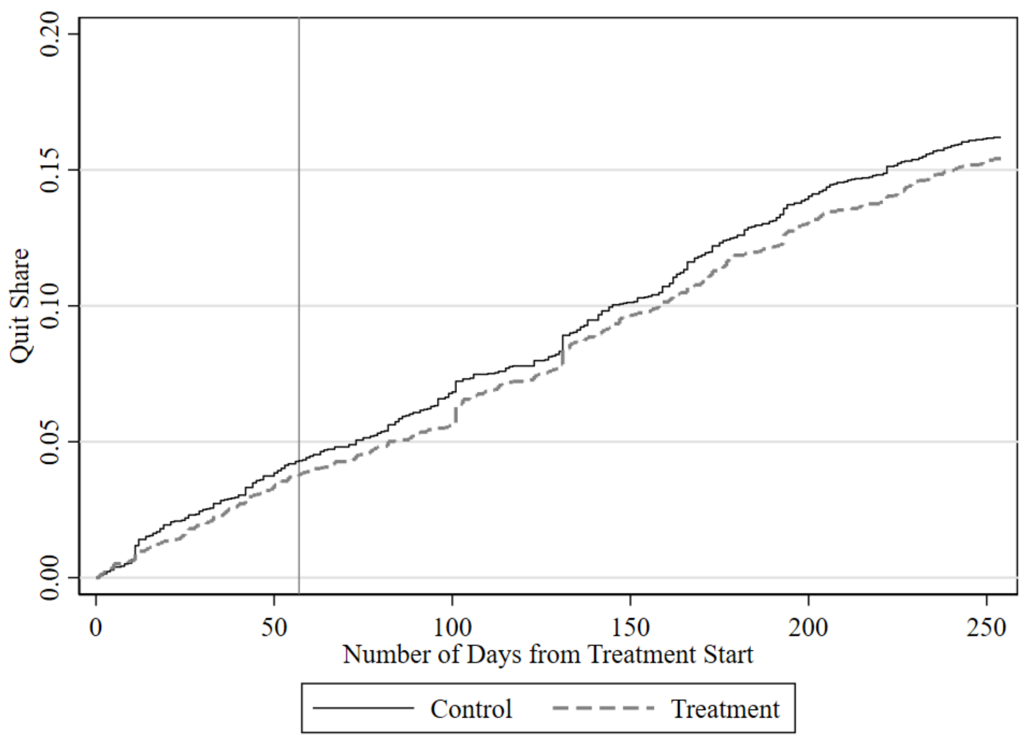
Notes: Standard deviations (in columns 1 to 3) and standard errors (in column 4) in parentheses. Includes all treatment and control workers working at the firm in November 2018. Years of education is missing for 33% of the sample. Absenteeism is averaged across the 6 months prior to the tool's introduction. Productivity data, averaged across the month prior to the tool's introduction, are only available for a subset of production line workers (49% of the sample). All other variables taken from administrative data in the month that randomization took place (August 2018).

Table 2: Effect of Treatment on Tool Knowledge and Use

	(1) Aware of Tool	(2) Aware of Tool	(3) Used Tool	(4) Used Tool	(5) Trust in Tool	(6) Trust in Tool	(7) Trust in Other Channels	(8) Trust in Other Channels
Treatment	0.38***	0.38***	0.045***	0.045***	0.099*	0.091*	0.011	0.0087
	(0.021)	(0.021)	(0.0081)	(0.0079)	(0.055)	(0.055)	(0.030)	(0.030)
Observations	1831	1831	1831	1831	1119	1119	1831	1831
Control Mean	0.45	0.45	0.006	0.006	4.54	4.54	4.39	4.39
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Robust standard errors in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. All regressions control for strata fixed effects. "Individual controls" include fixed effects for age, tenure, and education categories (including a "missing" category). "Control Mean" is the mean of the dependent variable among control group workers. Observations are weighted by the inverse of the predicted probability of being in endline survey, predicted using a probit regression on treatment, demographic controls, and treatment interacted with each demographic control (tenure, age, and education categories).

Figure 1: Quit Rates by Treatment Status



Notes: Vertical line denotes approximate date of first reminder SMS.

Table 3: Effect of Treatment on Retention

	(1)	(2)
Treatment	-0.063	-0.10*
	(0.061)	(0.061)
Observations	7046	7046
Individual Controls	No	Yes

Notes: Coefficients (not hazard ratios) from a Cox proportional hazard model are reported. Robust standard errors in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. All regressions control for strata fixed effects. "Individual controls" include fixed effects for age, tenure, and education categories (including a "missing" category).

Table 4: Effect of Treatment on Absenteeism and Productivity

	Absenteeism		Productivity (Quantity/Target)	
	(1)	(2)	(3)	(4)
Treatment	-0.0046**	-0.0044**	0.0025	0.0027
	(0.0021)	(0.0021)	(0.0037)	(0.0036)
Observations	51943	51943	320115	320115
Control Mean	0.096	0.096	0.51	0.51
Individual Controls	No	Yes	No	Yes

Notes: Standard errors (clustered at worker level in Columns 1-2 and at the line level in Columns 3-4) in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. "Individual controls" include fixed effects for age, tenure, and education categories (including a "missing" category). "Control Mean" is the mean of the dependent variable among control group workers.
Columns 1-2: The unit of observation is the worker-month, restricting to months during the experimental period. Observations are weighted by the inverse of the predicted probability of being in the sample on that month. Both regressions control for the worker's average pre-treatment absenteeism, strata fixed effects, and month-year fixed effects.
Columns 3-4: The unit of observation is the worker-day-line-item, restricting to days during the experimental period. Observations are weighted by the inverse of the predicted probability of being in the sample on that day. Both regressions control for the worker's average pre-treatment productivity, strata, month-year, day of the week, line, and item fixed effects, and line item days.

A Online Appendix

Table A1: Grievance Case Types

Case Type	Total	Examples
Provident Fund	35	Date of Birth mismatch; Provident Fund (PF) withdrawal; Want to check balance in PF account
Banking	30	Did not receive checkbook; need my account number
Canteen food and infrastructure	27	Poor food quality, taste or high price
Compensation and Benefits	23	Did not receive salary/full salary/Over Time (OT); reduction in factory OT
Working Hours and OT	19	Unfair OT, Shift change, night shift, did not receive OT
Welfare schemes	15	Filled the govt scheme form but haven't received the payment
Factory temperature	14	Heat on floor, coolers not working
Conflict with Supervisor	13	Hostile work environment, misbehavior, verbal abuse
Exit procedures	13	Pending final payment after exit from firm or query about date of payment; did not want to leave
Leave	11	Not getting leave /difficulty in getting leaves
Sexual Harassment	8	Several cases against one person leading to investigation and action
Washroom cleanliness	8	No soap; toilet closed; toilet dirty
Others	92	Test messages (Hi/Hello)

Table A2: Summary Statistics: Survey Sample

	(1) Full sample	(2) Treatment	(3) Control	(4) Difference
Tenure (months)	46.8	47.1	46.5	-0.60
	(43.8)	(42.7)	(44.9)	(1.72)
Age (years)	33.7	33.6	33.8	0.19
	(7.47)	(7.37)	(7.58)	(0.29)
Female	0.72	0.72	0.72	-0.0068
	(0.45)	(0.45)	(0.45)	(0.018)
Years of Completed Schooling	8.67	8.68	8.67	-0.018
	(2.26)	(2.23)	(2.28)	(0.11)
Absenteeism	0.073	0.073	0.073	0.00061
	(0.074)	(0.078)	(0.070)	(0.0029)
Perceived HR Responsiveness (5 pt. scale)	3.77	3.76	3.79	0.022
	(1.15)	(1.16)	(1.13)	(0.045)
Job Satisfaction (5 pt. scale)	3.91	3.89	3.92	0.027
	(0.85)	(0.85)	(0.84)	(0.033)
Subjective Wellbeing (Cantril's Ladder 10 pt. scale)	8.02	8.07	7.98	-0.090
	(2.35)	(2.33)	(2.38)	(0.092)
Kessler Psychological Distress Scale (50 pt. scale)	16.3	16.4	16.2	-0.19
	(7.77)	(7.79)	(7.76)	(0.30)
Responded to Endline Survey	0.70	0.71	0.70	-0.015
	(0.46)	(0.45)	(0.46)	(0.018)
Observations	2606	1310	1296	2606

Notes: Standard deviations (in columns 1 to 3) and standard errors (in column 4) in parentheses. Includes all treatment and control workers working at the firm in November 2018 who completed the baseline survey. Absenteeism is averaged across the 6 months prior to the tool's introduction. All other administrative variables are from the month that randomization took place (August 2018). Years of education is missing for 33% of the sample. Survey outcomes summarized here are from the baseline survey. For all variables measured on a 5-point scale, higher values represent higher satisfaction. For the Kessler Psychological Distress Scale, higher numbers represent more psychological distress. For Cantril's ladder of subjective wellbeing, a 10 represents the "best possible life."

Table A3: Effect of Treatment on Retention and Absenteeism, Production Sample

	Retention		Absenteeism	
	(1)	(2)	(3)	(4)
Treatment	-0.087	-0.11	-0.0058*	-0.0054*
	(0.083)	(0.084)	(0.0030)	(0.0030)
Observations	3601	3601	26458	26458
Control Mean			0.085	0.085
Individual Controls	No	Yes	No	Yes

Notes: Standard errors (clustered at worker level) in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. "Individual controls" include fixed effects for age, tenure, and education categories (including a "missing" category).
Columns 1-2: Coefficients (not hazard ratios) from a Cox proportional hazard model are reported.
Columns 3-4: The unit of observation is the worker-month, restricting to months during the experimental period. Observations are weighted by the inverse of the predicted probability of being in the sample on that month. Both regressions control for the worker's average pre-treatment absenteeism, strata fixed effects, and month-year fixed effects.

Table A4: Effect of Treatment on Salary and Promotion

	Salary		Promotion	
	(1)	(2)	(3)	(4)
Treatment	-2.29	0.70	-0.00076	0.0074
	(2.81)	(2.50)	(0.0073)	(0.0066)
Observations	51595	51595	7046	7046
Control Mean	10229.32	10229.32	0.11	0.11
Individual Controls	No	Yes	No	Yes

Notes: Standard errors (clustered at worker level) in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. "Individual controls" include fixed effects for age, tenure, and education categories (including a "missing" category). "Control Mean" is the mean of the dependent variable among control group workers.
Columns 1-2: The unit of observation is the worker-month, restricting to months during the experimental period. Observations are weighted by the inverse of the predicted probability of being in the sample on that month, as described in section 4.3. Both regressions control for the worker's average pre-treatment salary, strata fixed effects, and month-year fixed effects.
Columns 3-4: The unit of observation is the worker. The outcome variable is an indicator equal to 1 if the employee was promoted between baseline and endline.

Table A5: Effect of Treatment on Survey Outcomes

	(1)	(2)	(3)	(4)	(5)
	Perceived Responsive-ness	Job Satisfaction	Subjective Wellbeing	Severe Pyschological Distress	Moderate Pyschological Distress
Treatment	-0.037	-0.0073	-0.100	0.0058	0.0093
	(0.034)	(0.032)	(0.097)	(0.0058)	(0.0088)
Observations	1831	1831	1831	1831	1831
Control Mean	4.36	4.21	8.52	0.01	0.04
Individual Controls	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors (clustered at worker level) in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. These worker-level regressions use endline survey measures as dependent variables and control for the same measures from the baseline survey. All regressions control for strata fixed effects. "Individual controls" include fixed effects for age, tenure, and education categories (including a "missing" category). "Control Mean" is the mean of the dependent variable among control group workers. Observations are weighted by the inverse of the predicted probability of being in endline survey, predicted using a probit regression on treatment, demographic controls, and treatment interacted with each demographic control (tenure, age, and education categories).

Table A6: Summary Statistics: Non-Attrited Sample

	All Non-Attrited Workers			Workers in Endline Survey		
	(1)	(2)	(3)	(4)	(5)	(6)
	Treatment	Control	Difference	Treatment	Control	Difference
Tenure (months)	49.9	49.0	-0.89	48.8	47.8	-1.01
	(48.1)	(47.4)	(1.24)	(43.4)	(44.8)	(2.06)
Age (years)	34.5	34.7	0.12	34.3	34.3	0.027
	(7.74)	(7.70)	(0.20)	(7.39)	(7.47)	(0.35)
Female	0.64	0.65	0.0064	0.75	0.72	-0.024
	(0.48)	(0.48)	(0.012)	(0.44)	(0.45)	(0.021)
Years of Completed Schooling	8.71	8.74	0.027	8.59	8.69	0.11
	(2.38)	(2.22)	(0.075)	(2.13)	(2.22)	(0.12)
Absenteeism	0.080	0.081	0.00051	0.070	0.070	-0.00027
	(0.095)	(0.093)	(0.0024)	(0.075)	(0.068)	(0.0033)
Perceived HR Responsiveness (5 pt. scale)				3.75	3.76	0.0015
				(1.18)	(1.15)	(0.055)
Job Satisfaction (5 pt. scale)				3.89	3.92	0.027
				(0.86)	(0.85)	(0.040)
Subjective Wellbeing (Cantril's Ladder 10 pt. scale)				8.09	7.96	-0.13
				(2.30)	(2.40)	(0.11)
Kessler Psychological Distress Scale (50 pt. scale)				16.6	16.4	-0.22
				(7.83)	(7.84)	(0.37)
Observations	2952	2980	5932	930	901	1831

Notes: Standard deviations (in columns 1, 2, 4, 5) and standard errors (in columns 3 and 6) in parentheses. Columns 1-3 restrict to workers still working at the firm by the end of July 2019. Columns 4-6 restrict to workers who responded to the endline survey. Absenteeism is averaged across the 6 months prior to the tool's introduction. All other administrative variables are from the month that randomization took place (August 2018). Years of education is missing for 33% of the sample. Survey outcomes summarized here are from the baseline survey. For all variables measured on a 5-point scale, higher values represent higher satisfaction. For the Kessler Psychological Distress Scale, higher numbers represent more psychological distress. For Cantril's ladder of subjective wellbeing, a 10 represents the "best possible life."