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EXPECTATIONS, WAGE HIKES AND WORKER VOICE*

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Enabling worker voice could improve worker retention and effort by providing workers the chance to improve their situation or an outlet to express discontent. We provide a test of this hypothesis via a randomised controlled trial in Indian garment factories. Just after what proved to be a disappointing wage hike, workers were chosen at random to participate in an anonymous survey in which they were asked for feedback on job conditions, supervisor performance and overall job satisfaction. Enabling voice in this manner reduced turnover and absenteeism after the hike, particularly for the most disappointed workers.

The way in which firms manage workers has profound implications for performance by way of worker attendance, retention and productivity (Bloom and Van Reenen, 2007; Lazear and Oyer, 2007; Lazear and Shaw, 2007; Bloom *et al.*, 2010; Bloom and Van Reenen, 2011). For example, studies have documented the importance of the structure of incentives (Bandiera *et al.*, 2007; 2013; Gosnell *et al.*, 2019); the allocation of workers to teams and tasks (Hjort, 2014; Amodio and Martinez-Carrasco, 2018; Adhvaryu *et al.*, 2020; 2022); monitoring, learning about and evaluating performance (Lange, 2007; Kahn, 2013; Kahn and Lange, 2014; Frederiksen *et al.*, 2017; Gosnell *et al.*, 2019); and interpersonal skills and relationships among co-workers (Bandiera *et al.*, 2009; 2010; Hoffman and Tadelis, 2018; Adhvaryu *et al.*, 2021).

Another important component of the worker-firm relationship is the extent to which workers feel their voices are valued and accounted for in managerial decision-making. Providing workers with voice could improve the employment relationship through two channels. First, dissatisfied workers might be less likely to quit if they have the opportunity to voice their discontent to create improvement. We refer to this as the 'instrumental' value of voice, which was first championed in Hirschman's 1970 seminal work on exit and voice. While indirect tests of Hirschman's theory, based on associations between measures of voice and firm outcomes, abound in the economics literature (see, e.g., Freeman, 1980; Batt *et al.*, 2002; Beard *et al.*, 2015; Cottini *et al.*, 2011; Watkins and Hyclak, 2011; Gans *et al.*, 2017), direct rigorous evidence on the impacts of increased voice on worker turnover is quite limited.[1]

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The data and codes for this paper are available on the Journal repository. They were checked for their ability to reproduce the results presented in the paper. The authors were granted an exemption to publish parts of their data because access to these data is restricted. However, the authors provided the Journal with temporary access to the data, which enabled the Journal to run their codes. The codes for the parts subject to exemption are also available on the Journal repository. The restricted access data and these codes were also checked for their ability to reproduce the results presented in the paper. The replication package for this paper is available at the following address: https://doi.org/10.5281/zenodo.5790160.

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[1] Experiment-based analyses from psychology have reached similar findings, confirming Hirschman's theory in the realm of consumer relationships (see, e.g., Maute and Forrester, 1993; Divett *et al.*, 2003). On the other hand, evidence

In addition to instrumental value, voice also has inherent value (Ong *et al.*, 2012). That is, simply being able to communicate dissatisfaction to one's employer might increase a worker's utility. Because of its instrumental and inherent value, voice could essentially function as non-wage compensation, resulting in lower turnover and increased worker effort. Our study provides a test of this hypothesis via a randomised controlled trial in which we enabled greater voice for workers just after what proved to be a disappointing scheduled wage hike.

The State Governments of India revise their wage floors each year; the increase in the minimum wage is generally linked to expected inflation (Anand *et al.*, 2014). In low-skill industries, in which wages for a majority of workers are often closely benchmarked to the (sector-and locality-specific) minimum wage, the annual wage hike is highly anticipated by both employers and workers. The period of time leading up to and following the annual hike is often marked by widespread labour unrest (Justino, 2006; ILO, 2018).[2]

To understand the impacts of increasing voice on worker turnover, and in particular the role of voice in mitigating the effects of wage-related disappointment, we partnered with the largest ready-made garments firm in India. Just before the 2016 wage hike was implemented by the firm, data were collected on a random sample of workers regarding their current wages, expectations about changes due to the upcoming wage increment and other opportunities available to them in the labour market. These data reveal that workers' expectations were substantially higher than the realised wage hike: workers expected a hike that was roughly three times the size of the realised increase.

Directly following the wage hike, the firm randomised half of the surveyed sample to an intervention designed to enhance worker voice. Workers in the treatment group were invited to take part in a survey asking about their job satisfaction (related to various components of the job), and their opinions on various statements about the work environment. The results of this survey are themselves telling. Many workers used the survey to express their dissatisfaction with various aspects of their jobs. On average, satisfaction with wage levels was particularly low, highlighting the salience of wages as a potential driver of exit.

We find that this voice intervention reduced the probability of quitting by 20% in the months following the wage hike. This effect is strongest for the most disappointed workers, whose expectations were farthest from the realised wage hike. We also see a similar pattern in absenteeism, a proxy for effort provision on the job. The negative interaction between voice and disappointment persists even when we allow for heterogeneous voice effects across individuals with different outside options and other worker-level characteristics.

This paper provides a field-experiment-based evaluation of Hirschman's seminal thesis on exit and voice as it pertains to the employment relationship. Work in labour economics on this issue has focused on the effects of voice through collective action (e.g., Freeman, 1980). Our paper is most closely related to the recent work of Coviello *et al.* (2022), who examined the incidence of counterproductive worker behaviour and exit (employee turnover) after an unexpected reduction in pay for sales workers, and Cai and Wang (2020), who studied the impacts of the incorporation of worker feedback into manager incentive pay in a Chinese automobile factory. Our study complements these papers by demonstrating the impacts of a randomised voice treatment (independent of managerial incentives) on worker turnover, and specifically the way in which access to voice mitigates disappointment.

from a natural experiment in Finland reveals no effect of the representation of workers on boards on separation rates (Harju *et al.*, 2021).

[2] The popular press has widely reported on this phenomenon; see, for example, Reuters (2016) and Bengali (2016).

Our study is among the first to examine voice as it relates to low-income workers in manufacturing contexts, where voice tends to be particularly limited and exit is common (Rees *et al.*, 1991; Dundon and Gollan, 2007; Macey and Schneider, 2008). As the low-skill workforce in many developing countries transitions rapidly from agriculture to industrial work, employers often face high worker turnover due to poor working conditions, low pay and restricted worker rights (Boudreau *et al.*, 2021; Bossavie *et al.*, 2019; Boudreau, 2020). Our study affirms the value of providing voice to vulnerable workers in exactly these high-intensity environments.

1. Context and Intervention

1.1. *Context*

We partnered with Shahi Exports, Private Limited, the largest ready-made garments (RMG) exporter in India. As is the case in many manufacturing firms in low-income contexts, turnover is high at Shahi: 5% of our study sample quits by the end of the first month of the study and 18% quits by the end of the fifth month. The costs of turnover, as emphasised by Shahi's upper management, pose a significant challenge, leading to persistently high recruitment and training costs and underutilisation of capital.

In the RMG sector, wages for frontline workers are influenced heavily by government minimum wage policy, which is largely determined at the state level. In the state of Karnataka, where the majority of Shahi's factories (and the entirety of this study's sample) are located, the minimum wage schedule specifies different minimum wages across geographic areas, industries and skill levels within each industry.

The minimum wage is comprised of two parts—a 'basic' portion and a 'dearness allowance', which is intended to allow for cost of living adjustments. Every year, the state government makes adjustments to minimum wage schedules by changing the dearness allowance to account for inflation. In addition, adjustments to the 'basic' wage level are made every five years or so, commonly resulting in larger increases than the more frequent inflation adjustments. The last such increase preceding our study period was in 2014. As shown in Online Appendix Figure A1, the tailoring industry's minimum wage increased substantially more in 2014 than in subsequent years, due to the basic wage hike that happened in that year (Government of Karnataka, 2016).[3]

After the wage hike announcement made by the government every year, Shahi revises its wages to comply with the stated increases. Firms have discretion, however, and Shahi can choose to raise wages by more than the minimum wage policy requires (though this is rare). Overall, there is substantial uncertainty about the size of these annual wage increases from the point of view of workers, due to the fact that both government as well as firm decision-making is not predictable.

Anecdotal evidence suggests that worker dissatisfaction is especially high after these annual firm-wide wage increases, a fact that may in part be explained by disappointment brought about by wage-related uncertainty. Even though minimum wage changes are determined by the government, employees are aware that the firm has discretion in wage setting. Because the state minimum wage schedule specifies a different wage level for different skill levels, the firm can (and does) raise wages for individual workers by promoting them to a higher skill level: median salary for the lowest skill level in our sample is 137 USD, while median salary for the highest

[3] This was also true for other industries with large female shares, but the 2014 increase was much larger in tailoring than in other industries because the baseline tailoring wage was lower compared to comparable sectors.

skill level is 155 USD. In addition, even within skill levels, we still do see some variation in wage levels. For example, for the workers in our sample in the highest skill level, the 25th and 75th percentile monthly salaries are 150 and 159 USD. For workers in the lowest skill level, the corresponding values are 132 and 139. The knowledge that the firm does not always have to pay exactly the minimum wage could lead workers to blame the firm for lower-than-expected wage increases.

1.2. *Intervention*

The intervention we consider is an employee satisfaction survey (Shahi Exports, 2016a). The survey questions, copied in full in Online Appendix Section A.1, created an opportunity for respondents to express their (dis)agreement with various statements about their job: whether it is difficult to ask others for help and whether supervisors encourage learning, for example. Respondents were also asked about their general satisfaction with their job, wage, supervisor and overall work environment.

In the consent script read to each respondent before each survey was administered (copied in full in Online Appendix Section A.1), several important points were made clear. First, respondents knew that their individual responses were confidential. Second, respondents were aware that the survey was being conducted because Shahi was interested in learning about the satisfaction of its workers. Therefore, they knew that the survey results would be communicated in some way to the firm, even though the surveys were not being conducted by Shahi employees. Finally, they were told that their names had been selected at random, which should have minimised the potential for respondents to perceive themselves as singled out by their employer.

An employee satisfaction survey could embody both the instrumental and inherent value of voice. The work of Hirschman (1970) and many others emphasises the instrumental value of voice, based on the basic premise that individuals have two main options in unsatisfactory situations: they can quit without trying to improve their situation at work (exit), or they can stay, speak up and try to remedy the situation (voice). The workers in our study context do not typically have many opportunities to voice concerns about their working conditions and may therefore have no option but to exit, which may in part explain the firm's high turnover. An employee satisfaction survey, by providing a way for workers to communicate dissatisfaction about their jobs, could reduce exit by making the voice channel a possibility.

In addition to the instrumental value of voice, evidence from lab experiments demonstrates that voice also has inherent value (Ong *et al.*, 2012). In our setting, an employee satisfaction survey demonstrates that the firm is interested in worker satisfaction and allows respondents to communicate their (dis)satisfaction, both of which could directly provide utility to workers.[4] This voice instrument, therefore, has the potential to reduce exit not only by creating expectations of constructive changes in the work environment (instrumental value), but also by directly improving a worker's utility (inherent value).

The responses to this employee survey instrument reveal that many workers did in fact use it to express dissatisfaction with various aspects of the job. As we show in Online Appendix Table A1, over 20% of workers agreed with the statements that mistakes were held against them and

[4] Although both treatment and control workers were surveyed at baseline, the statement that the firm was interested in the responses was only made prior to the employee satisfaction survey and therefore only read to treatment workers. For this reason, the 'intervention' should be thought of as one that informed workers of the firm's interest in their satisfaction and also provided a way to express their (dis)satisfaction.

that asking for help was difficult. Smaller proportions (ranging between 6% and 16%) provided negative evaluations of their supervisor, indicating their supervisor was not encouraging, not someone they could trust or indifferent about helping solve problems. Over 50% of the sample responded negatively to at least one of the six statements.

Though average satisfaction levels with respect to the job, supervisor and workplace environment were quite high (over half reported being extremely satisfied), satisfaction with wage levels was much lower—with over half either somewhat or extremely dissatisfied. This highlights the salience of wages as a potential driver of exit.

2. Data

2.1. *Baseline and Intervention Surveys*

In May 2016, before workers were made aware of how the annual minimum wage hike would translate into an increase in their take-home pay at Shahi, a baseline survey was conducted to elicit worker expectations about the pending wage hike (Shahi Exports, 2016b). Workers were asked how much they expected take-home wages to increase next month, along with questions about wages at their best outside option—the job they would most likely have if they did not work at Shahi. A randomly selected sample of approximately 2,000 workers from 12 factory units located in the Indian state of Karnataka were surveyed.

Using these data, we construct a measure of disappointment, which combines data on ex ante worker expectations and ex post wage increases. Specifically, we calculate the difference between the wage hike an individual was expecting to receive in June and the wage hike she actually received, which turned out to be a 398 rupee increase (approximately 6 USD in 2016 dollars) for all individuals in our sample.[5] Another important variable is the outside option wage. Workers were first asked what job they would most likely have or would be easiest to get if they did not work at Shahi, and then asked for the wage they would earn at the specified job.

Of the baseline sample, approximately half were randomly selected for the voice intervention. We stratified by factory unit and job type (there are twelve factory units and five different job types: tailor, checker, helper, operator or other). Individuals assigned to the treatment group were given the satisfaction survey described in Subsection 1.2, after the wage hikes were implemented at the beginning of June 2016. These surveys were administered from the end of June to the beginning of July 2016.

2.2. *Administrative Data*

From the firm's administrative data (Shahi Exports, 2017), we are able to observe the dates that an individual joins and leaves the firm. Because of stringent labour laws, firing is very uncommon in this setting, which means that almost all job separations are due to quitting rather than firing. We also observe daily attendance and can calculate the share of days (in a given time period, during which a worker was still employed by the firm) a worker was absent. We also obtain a set of

[5] The size of the wage increase is not always the same for all workers because the government sometimes dictates different wage increases for workers of different skill levels and across different geographic zones. Shahi also has the discretion to raise wages more for different workers as long as it complies with the new minimum wage laws. It is not uncommon, however, for Shahi and other firms to implement a uniform wage increase for all workers in all factories across the state of Karnataka.

Table 1. *Summary Statistics.*

	(1) Full sample mean/SD	(2) Voice group mean/SD	(3) Control mean/SD	(4) Difference diff/SE
Disappointment	1.69	1.69	1.70	0.018
	(2.37)	(2.38)	(2.38)	(0.11)
Monthly salary	10.4	10.2	10.5	0.28
	(4.69)	(4.26)	(5.06)	(0.22)
Outside option salary	15.2	15.1	15.2	0.11
	(7.36)	(7.12)	(7.58)	(0.34)
Tenure (in years)	1.92	1.90	1.94	0.038
	(1.68)	(1.62)	(1.74)	(0.078)
Female	0.71	0.71	0.70	−0.0038
	(0.46)	(0.45)	(0.46)	(0.021)
Years of education	8.54	8.62	8.46	−0.16
	(3.57)	(3.49)	(3.65)	(0.16)
Speak Kannada	0.68	0.66	0.71	0.047**
	(0.47)	(0.47)	(0.46)	(0.022)
Bangalore	0.68	0.67	0.68	0.0060
	(0.47)	(0.47)	(0.47)	(0.022)
Sewing department	0.54	0.54	0.54	−0.0086
	(0.50)	(0.50)	(0.50)	(0.023)
Tailor	0.42	0.43	0.42	−0.0080
	(0.49)	(0.49)	(0.49)	(0.023)
Checker	0.072	0.072	0.072	0.00020
	(0.26)	(0.26)	(0.26)	(0.012)
Helper	0.13	0.12	0.13	0.0076
	(0.33)	(0.33)	(0.34)	(0.015)
Operator	0.043	0.045	0.042	−0.0029
	(0.20)	(0.21)	(0.20)	(0.0094)
Other job	0.34	0.34	0.34	0.0031
	(0.47)	(0.47)	(0.47)	(0.022)
Pre-treatment absenteeism	0.11	0.12	0.11	−0.0062
	(0.13)	(0.13)	(0.13)	(0.0061)
Joint test *p*-value				0.676
Observations	1,871	916	955	1,871

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Wage disappointment is the difference between expected and actual wages after the wage hike, reported in 10 USD increments. Monthly salary and outside option salary are also reported in 10 USD increments. For the 4% of respondents who are missing the outside option salary variable, we assign them with the mean value. After doing this, all variables are non-missing for all 1,871 individuals.

individual-level controls, including tenure at the firm, gender, education, hometown, department and job type.

2.3. *Summary Statistics*

Table 1 reports summary statistics for our study sample: starting with the full sample, the treatment group, the control group and then differences between the two.[6] Wage disappointment, defined as the difference between expected and actual monthly wages after the hike, is high. On average, individuals were expecting to earn 16.9 USD more (which is approximately 17% of average monthly wages) than they actually ended up earning after the hike. In other words, workers were expecting a wage hike of approximately 23 USD on average (approximately 22% of monthly salary), a value that lies in between the 2014 minimum wage hike (which corresponded to a 35%

[6] Our sample includes individuals in the baseline survey who were present at the firm when the intervention took place (in June 2016) and who were not missing any demographic covariates.

increase) and the 2015 wage hike (which corresponded to a 7% increase), both depicted in Online Appendix Figure A1.

The sample is balanced on important observables, like salary, outside option salary, tenure, education and job type across treatment and control groups.[7] Language is the only variable for which there is a (small) statistically significant difference across treatment and control groups. The joint test, however, cannot reject the null that all covariates are balanced across treatment and control groups (with a p-value of 0.676).[8]

Wage disappointment, a key variable in this analysis, is correlated with many individual characteristics, like education and tenure (see Online Appendix Table A2 and Section A.2 for a longer discussion). Interestingly, it also appears to be influenced by previous experience with wage hikes. Specifically, workers who were present for the large wage hike in 2014, and those who work on the same line as workers who were present during this hike, report higher levels of disappointment.

3. Empirical Strategy

We base our empirical strategy on the conceptual framework described in Online Appendix Section A.3, which is an extension of the canonical model of the reference-dependence set in the context of wage determination (Tversky and Kahneman, 1991; Kőszegi and Rabin, 2006; Barberis, 2013).[9] Summarising briefly, this framework proposes that, after learning about the size of a wage hike, a worker will choose to quit if the utility at her current job (after the wage increase) is lower than the utility she would have at her next best option. This decision, therefore, depends on her wage prior to the hike (inclusive of job-specific amenities), the realised wage hike at her current job, the wage and amenities at her best outside option and the realised wage hike at her best outside option.[10] Importantly, one variable that factors into her utility at her current job and therefore her quit decision is the wage hike she *expected* from her current firm prior to the announcement.[11] The discrepancy between the realised wage hike and the worker's expected wage hike is what we refer to as wage disappointment, which is captured in our data.

We model quitting as a function of current wages, outside option wages, disappointment and exposure to the voice intervention. A voice intervention could affect the quit decision independently of disappointment or by mitigating disappointment. To allow for this, we estimate a Cox proportional hazard model of the form

$$\lambda_i(t) = \lambda_0(t)\exp(\kappa_1 W_i + \kappa_2 \underline{W}_i + \kappa_3 D_i + \kappa_4 T_i + \kappa_5 D_i T_i + \gamma X_i), \tag{1}$$

[7] While it may seem counterintuitive that average outside option salary is higher than monthly salary, this is likely because respondents' outside options include agricultural jobs that are seasonal, or factory jobs at smaller firms with worse amenities (i.e., unregulated hours, no pension, etc.)

[8] We perform this Wald test of joint significance by estimating a system of equations, regressing each characteristic onto treatment status, using seemingly unrelated regression.

[9] A large body of empirical work shows the importance of reference points in determining a wide range of outcomes in the real world (see, e.g., Bartling *et al.*, 2015; Ockenfels *et al.*, 2015; Backus *et al.*, 2017; DellaVigna *et al.*, 2017; Adhvaryu *et al.*, 2018; O'Donoghue and Sprenger, 2018). Lab-experimental evidence confirms that reference points can be determined by expectations of future outcomes (Marzilli Ericson and Fuster, 2011).

[10] We later assume that the difference between the wage hike in the current job and outside option is approximately 0 or exogenous to other regressors.

[11] Though relative pay (inequality between own wages and peer wages) has been shown to be an important driver of job separations in the United States (Dube *et al.*, 2019), we abstract away from that here because the wage hike we study resulted in a uniform increase in wages across workers.

where $\lambda_i(t)$ denotes the instantaneous probability of individual i quitting at time t (measured in days relative to her start date) conditional on being still employed at time t, W_i is the individual's current wage, $\underline{W}_i$ is the outside option wage given in the survey, D_i is disappointment and T_i is an indicator for those assigned to the voice intervention treatment.[12]

Although random assignment ensures the exogeneity of T_i, wage disappointment is not exogenous. In fact, Online Appendix Table A2 illustrates that wage disappointment is correlated with several potentially important drivers of quitting. We therefore include a vector of controls, X_i: gender, years of tenure indicators, years of education, an indicator for speaking Kannada, an indicator for being from Bangalore and an indicator for being part of the sewing department. We estimate the model with and without fixed effects for job type and factory unit (our stratification variables).

The inclusion of the interaction between voice and disappointment allows for the intervention to have heterogeneous effects by the level of disappointment. If providing workers with voice helped mitigate their disappointment, we should expect to see a negative coefficient on the disappointment-voice interaction (κ_5).

The potential endogeneity of disappointment, discussed above, complicates the interpretation of κ_5 in (1), even with controls for covariates. For example, a statistically significant κ_5 could be an indication that the effect of voice varies by salary (or some other correlate of disappointment) and not actually by disappointment. We therefore also estimate several specifications that control for T_i interacted with a host of other covariates. Even in these specifications, we acknowledge that disappointment could still be correlated with other qualities—like loyalty—that we do not measure. However, given that we directly measure disappointment, we argue that this is likely to be the primary factor driving any effects we find, especially in our most rigorous specifications (which control for interactions with observed covariates). It is also reassuring to note that disappointment does appear to be driven by quasi-exogenous factors—for example, whether a worker happens to be working on the same production line as people who experienced the large wage hike in 2014 (see Online Appendix Table A2).

Finally, we also run ordinary least squares (OLS) regressions using the same independent variables as in (1); dependent variables we consider include separate indicators for having quit by the end of July, August and every month until November, as well as rates of absenteeism across various combinations of months.

4. Results

4.1. *Retention*

In Figure 1, we plot the cumulative share of the sample that has left the firm, starting in July 2016 (the first month after the voice intervention treatment) until the end of November. The dashed line (representing the treatment group), starts separating from the solid line (the control group) after about two weeks, and remains lower than the solid line throughout the entire time period. By the end of November, quit shares are approximately 2 percentage points lower in the voice intervention group than in the control group.

[12] Of those assigned to the treatment group, 90% actually completed the employee satisfaction survey. Compliers and non-compliers are similar across observable characteristics, except that compliers had significantly lower disappointment and higher salary. We use the random assignment dummy variable to estimate Intent-to-treat effects in all specifications. Unsurprisingly, given that the vast majority of treatment workers complied, our results are preserved when we drop non-compliers.

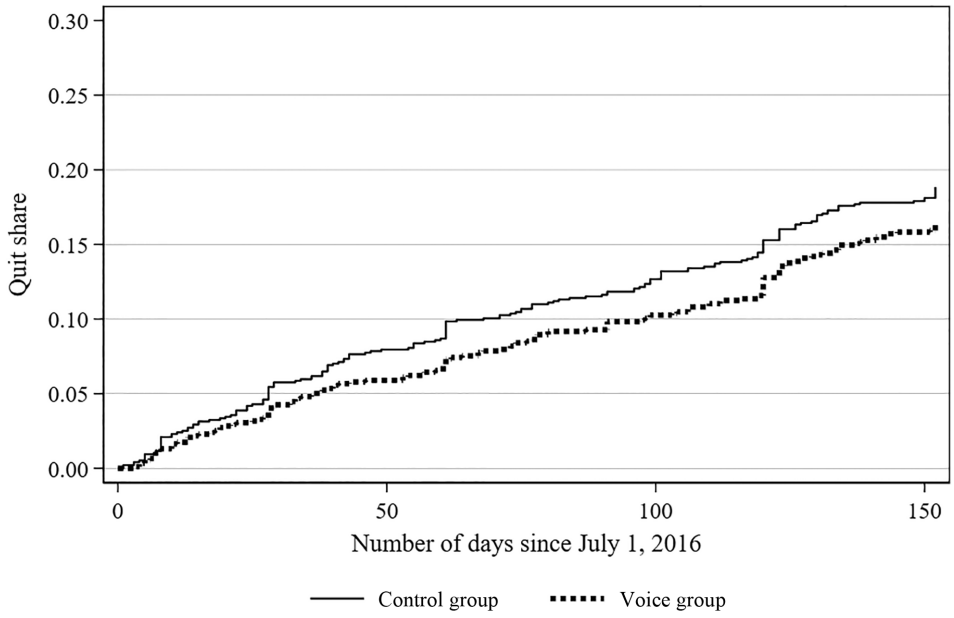

Fig. 1. *Quit Rates by Treatment Status.*

Table 2. *Hazard Model Estimates of the Effects of Disappointment and Voice on Quitting.*

	(1)	(2)	(3)	(4)
Voice group	−0.23**	0.046	−0.23**	0.036
	(0.11)	(0.14)	(0.11)	(0.14)
Disappointment	0.038	0.087***	0.051*	0.096***
	(0.025)	(0.027)	(0.027)	(0.029)
Disappointment × voice		−0.13***		−0.13***
		(0.044)		(0.045)
Monthly salary	−0.077***	−0.079***	−0.079***	−0.082***
	(0.024)	(0.024)	(0.028)	(0.028)
Outside option	0.021**	0.023**	0.017	0.019*
Salary	(0.010)	(0.010)	(0.011)	(0.011)
Observations	1,871	1,871	1,871	1,871
Fixed effects	None	None	Unit & job	Unit & job
p-value of sum		0.28		0.48

Notes: Robust SEs are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Coefficients (not hazard ratios) from a Cox proportional hazard model are reported. All regressions control for years of tenure indicators, years of education and indicators for Kannada (language), Bangalore (hometown) and sewing department. Individuals who are missing the outside option salary variable are assigned the sample average, and an indicator for those missing this variable is included. The last row reports the *p*-value of the test of the null that the sum of the disappointment and disappointment-voice coefficients is equal to zero.

In Table 2, we begin by estimating the hazard model described in (1), excluding the interaction term in column 1. A negative coefficient of −0.23 on the voice intervention indicator indicates that those in the treatment group are on average 20% less likely to quit than those in the control group. To put this magnitude in perspective, the voice intervention effect is equivalent to the change in retention associated with a 30 USD increase in monthly salary.

In column 2, we allow for the treatment to interact with wage disappointment. The coefficient on this interaction is negative and significant. The main effect of voice, now representing the impact of voice on those whose expectations were exactly met by the wage hike, is small in magnitude and not significant. In addition, the main effect of wage disappointment is positive and significant: in the control group, more disappointed workers are more likely to quit. The sum of the coefficients on the disappointment main effect and the disappointment-voice interaction captures the relationship between disappointment and quitting for the voice group. While this sum is negative, the *p*-value reported at the bottom of the table shows that it is not statistically significant. In columns 3 and 4, we show that the inclusion of factory unit and job type fixed effects has little effect on the coefficient estimates.

In short, individuals who were disappointed by the wage hike were more likely to quit, but the voice intervention was particularly able to lower quit rates among the disappointed. At the average level of wage disappointment (17 USD), treatment workers were 16% less likely to quit than control workers. For those who were not disappointed at all, the intervention had no statistically significant effect. Online Appendix Table A4 shows that the differential effects of treatment appear to be driven by workers in the top quartile of the disappointment distribution.

Because the model in Online Appendix Section A.3 predicts that current wages and outside option wages should be important determinants of quitting, the preferred specification we present in Table 2 controls for both of these variables, which do affect quitting in the expected directions.[13] However, we show in Online Appendix Table A3 that coefficient estimates for voice and disappointment are very similar when these variables are excluded (columns 1 and 2). This table also shows that our results are robust to other specifications of the model. Columns 3 and 4 show results without any individual-level controls.[14] In columns 5 and 6, we allow for factory unit-level frailty, which allows for a factory unit-specific random effect that enters multiplicatively in the hazard function.

In Table 3, we investigate *when* the effects of the voice intervention started to kick in, and how persistent these effects were. We run OLS regressions using the same set of independent variables as in the hazard models above. The five dependent variables of interest are dummy variables for having quit by July, August, September, October and November. In Table 3, we show that the main effect of disappointment is small in column 1, but larger and significant in the remaining columns. Disappointment effects do not kick in until August, perhaps because workers—many of whom are migrants—need time to discuss quit decisions with their families, or save enough money for the trip home. August is also when the effect of the voice intervention on these disappointed individuals is first observed. The magnitudes of the wage disappointment main effect and the disappointment-voice interaction are similar in the remaining columns, suggesting that the voice intervention did more than just temporarily delay quitting (at least within our window of analysis)—the effects of the voice intervention persisted for several months after the wage hike. Online Appendix Table A5 reports specifications that exclude the

[13] These coefficients also have the expected signs in the vast majority of the regressions in the remainder of the paper, though they are not always statistically significant in regressions that control for many additional correlated variables (e.g., job type fixed effects, which explain 14% of the variation in the monthly salary variable and outside option wage variable).

[14] While the interaction specification (column 4) is almost identical to previous results, the treatment dummy coefficient in the basic specification (column 3) is slightly smaller and no longer statistically significant. Additional analyses (available on request) reveal that the coefficient magnitude reduction is driven by the exclusion of the tenure dummies, specifically. We argue that their inclusion is important given that the quitting hazard likely varies for different levels of tenure.

Table 3. *Effects of Disappointment and Voice on Quitting by Month.*

	Quit by the end of . . .				
	(1) July	(2) Aug	(3) Sept	(4) Oct	(5) Nov
Voice group	−0.0044	0.0028	0.015	0.012	0.025
	(0.013)	(0.016)	(0.017)	(0.019)	(0.022)
Disappointment	0.0069	0.018***	0.021***	0.020***	0.022***
	(0.0047)	(0.0058)	(0.0063)	(0.0065)	(0.0066)
Disappointment × voice	−0.0077	−0.019***	−0.023***	−0.025***	−0.031***
	(0.0056)	(0.0069)	(0.0075)	(0.0079)	(0.0085)
Monthly salary	−0.0038**	−0.0059**	−0.0071***	−0.0083***	−0.011***
	(0.0016)	(0.0023)	(0.0025)	(0.0026)	(0.0027)
Outside option salary	0.0016	0.0021	0.0026*	0.0012	0.0030*
	(0.00098)	(0.0013)	(0.0014)	(0.0015)	(0.0016)
Observations	1,871	1,871	1,871	1,871	1,871
Mean of dependent var.	0.050	0.086	0.11	0.14	0.18
Fixed effects	None	None	None	None	None
p-value of sum	0.84	0.80	0.70	0.38	0.11

Notes: Robust SEs are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions control for years of tenure indicators, years of education, and indicators for Kannada (language), Bangalore (hometown) and sewing department. Individuals who are missing the outside option salary variable are assigned the sample average, and an indicator for those missing this variable is included. The last row reports the *p*-value of the test of the null that the sum of the disappointment and disappointment-voice coefficients is equal to zero.

interaction term, add unit and job fixed effects, and drop monthly salary and outside option controls.

4.2. *Absenteeism*

Next, we examine absenteeism—a less extreme version of exit and potential proxy for reduced on-the-job effort. We repeat our regressions above using absenteeism as our outcome variable—specifically, the share of days (over various time periods) that an individual did not attend work, conditional on still being employed at the firm.[15] In all of these regressions, we adopt an ANCOVA specification that controls for the share of days in the months of April and May (prior to the June voice treatment) that an individual was absent from work.

For individuals in the control group, disappointment drives up absenteeism. This is clear in columns 1 through 3 of Table 4, which suggests that these effects start kicking in immediately, unlike in the quit regressions in Table 3. However, as was the case for quitting, the voice intervention mitigates these effects entirely (starting in August).

Online Appendix Figure A2 plots treatment-control differences in absenteeism in the months leading up to the experiment, separately for those below and above the 75th percentile of disappointment. There are no significant pre-trends in absenteeism for either group. In addition, we report additional specifications in Online Appendix Table A6: excluding the interaction term, adding unit and job fixed effects, and dropping monthly salary and outside option controls.

[15] Each absenteeism variable calculates the share of days absent divided by the number of days the worker was still employed at the firm, over a time period starting in June until the specified month. Days after a worker quits are not included in the calculation. This ensures that we maintain the full sample in all regressions.

Table 4. *Effects of Disappointment and Voice on Absenteeism.*

	Share of days absent in . . .				
	(1) July	(2) July–Aug	(3) July–Sept	(4) July–Oct	(5) July–Nov
Voice group	0.0055	0.0081	0.0086	0.0081	0.0076
	(0.0092)	(0.0087)	(0.0085)	(0.0084)	(0.0085)
Disappointment	0.0064*	0.0078**	0.0061*	0.0054	0.0047
	(0.0037)	(0.0036)	(0.0037)	(0.0036)	(0.0035)
Disappointment × voice	−0.0064	−0.0081*	−0.0083**	−0.0092**	−0.0090**
	(0.0042)	(0.0041)	(0.0041)	(0.0039)	(0.0039)
Monthly salary	−0.00078	−0.00075	−0.0015	−0.0016	−0.0020*
	(0.0010)	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Outside option salary	0.00034	0.00018	0.00069	0.00051	0.00069
	(0.00067)	(0.00066)	(0.00066)	(0.00065)	(0.00066)
Pre-treatment absenteeism	0.19***	0.16***	0.21***	0.23***	0.24***
	(0.049)	(0.048)	(0.048)	(0.046)	(0.045)
Observations	1,871	1,871	1,871	1,871	1,871
Mean of dependent var.	0.100	0.11	0.12	0.12	0.13
Fixed effects	None	None	None	None	None
p-value of sum	1.00	0.90	0.42	0.16	0.12

Notes: Robust SEs are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the share of work days in the specified period that an individual was reported absent, out of all days an individual was still employed at the firm. All regressions control for years of tenure indicators, years of education, and indicators for Kannada (language), Bangalore (hometown) and sewing department. Individuals who are missing the outside option salary variable are assigned the sample average, and an indicator for those missing this variable is included. The last row reports the *p*-value of the test of the null that the sum of the disappointment and disappointment-voice coefficients is equal to zero.

4.3. *Threats to Identification and Alternative Interpretations*

Although the voice intervention was assigned randomly, wage disappointment might be endogenously determined. In Online Appendix Section A.4, we conduct additional analyses to ensure that other correlates of disappointment are not responsible for the negative interaction term reported in previous tables. When we include interactions between the treatment dummy and other worker characteristics (demographics as well as outside options), the estimate of the disappointment-treatment interaction remains unchanged (see Online Appendix Tables A7 and A8).

We also provide evidence that the firm did not differentially reward treatment and control workers via promotion or salary increases (Online Appendix Table A9), which could have provided an alternate explanation for the treatment effects. This is consistent with the fact that the survey was anonymous and suggests it was unlikely that the firm discovered the identity of workers through informal channels.

Finally, because this experiment was not registered with a pre-analysis plan prior to its implementation, we conduct additional robustness checks to show that disappointment is indeed one of the most important sources of heterogeneity in treatment effects. In Online Appendix Section A.5, machine learning methods provide evidence that disappointment is the main driver of heterogeneity.

4.4. *Mechanisms*

How did the employee satisfaction survey change worker behaviour? One key possibility is the inherent value of voice. Workers who took the survey could have simply appreciated

being given the opportunity to voice their opinions. An improved perception of the firm, or a feeling of satisfaction after taking the survey, could have led to lower quit rates and absenteeism.

Summary results of the survey were not shared with firm management until after the evaluation period, i.e., the period in which retention and attendance data were compiled on the experimental sample. This means that the effects we document could not have been driven by any action taken by the firm in response to the surveys specifically. This does not, however, rule out the possibility that general actions taken by the firm or supervisors (not in direct response to the survey results) could have been perceived differently by treatment and control workers. Specifically, treatment workers could have interpreted positive changes as a result of their survey responses.

Finally, the satisfaction survey could have changed behaviour by generating expectations of future improvements among the treatment group. As Hirschman (1970) hypothesised, workers who have limited opportunities for voice have no choice but to exit. By providing a potential channel for communication and future improvements, the survey could have given workers a voice alternative, preventing them from exercising their exit option.

In sum, the effects we find are likely driven by a combination of the following mechanisms: treatment workers having higher firm-specific utility (due to the inherent value of voice), interpreting changes differently from control workers or being more optimistic about improvements to come.

5. Conclusion

In this study, we provide real-world experimental evidence on the effect of voice on worker retention and effort. A randomly assigned employee satisfaction survey, administered to Indian garment workers shortly after a disappointing wage hike, reduced quit rates by 20%. Importantly, the effects of this voice intervention were strongest among those most disappointed by the wage hike. The same pattern of results is apparent when we look at worker absenteeism.

A conservative cost benefit analysis, which only takes into account the direct retention benefits (money saved on recruitment and training costs), reveals a very high return on investment for this intervention.[16] The survey cost USD 3.75 per respondent and was administered to 916 workers. It also cost about 15 minutes of productive time for each worker, which corresponds to 0.09 USD in lost profits per worker,[17] yielding a total cost of USD 3,517. In terms of the benefits, the firm estimates that recruitment and training costs are approximately USD 300 per worker. By the end of November, attrition was 2.8 percentage points lower in the treatment than the control group,[18] which translates to 25 fewer workers that needed to be recruited and trained—a total discounted benefit of USD 7,246,[19] which is more than

[16] Although the firm may have suspected that lower-than-expected wage increases could have demotivating effects, lack of data on the magnitudes of these effects could be why the firm typically benchmarked their wage hikes close to the minimum wage schedule.

[17] In one eight-hour work day, an average worker makes about 8.2 garments per day, which sell for 7 USD, of which the firm earns 5% in profit.

[18] This is the coefficient on the voice treatment dummy in a regression excluding the disappointment-treatment interaction; see Online Appendix Table A5, column 5.

[19] This assumes an annual interest rate of 7% and that all of the benefits are realised six months after the intervention, which is a conservative assumption because some recruitment likely had to happen earlier than this.

double the total cost. Not including the benefits of reduced absenteeism and not incorporating the lower productivity of new workers (which would make total cost of recruitment, and therefore the benefits of the intervention, even higher), this implies a net return on investment of 106%.

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Additional Supporting Information may be found in the online version of this article:

Online Appendix
Replication Package

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