

Hostel Takeover: Living Conditions, Reference Dependence, and the Well-being of Migrant Workers*

Achyuta Adhvaryu[†] Anant Nyshadham[‡] Huayu Xu[§]

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Abstract

We report impacts of a randomized housing quality improvement intervention among Indian migrant workers. Despite modest improvements in conditions, respondents experienced a decline in satisfaction and a large increase in psychological distress as a result of treatment. In contrast, residents who faced the same treatment-induced variation in living conditions as the original sample, but who arrived after treatment had already been initiated, had *increased* satisfaction. Impacts on turnover echo these patterns. We interpret this as evidence of reference dependence: residents who were primed to expect larger-than-realized improvements in living conditions suffered utility losses, while exposed but unprimed residents experienced gains.

JEL Codes: J28, J32, D9, I31

Keywords: worker satisfaction, reference dependence, personnel management, migrant workers, dormitories, India

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[†]University of Michigan, NBER, BREAD; adhvaryu@umich.edu; achadhvaryu.com

[‡]Boston College & NBER; nyshadha@bc.edu; anantnyshadham.com

[§]University of Michigan; xuhy@umich.edu

1 Introduction

Worker satisfaction is highly correlated with productivity, turnover, and the growth of firms (Edmans, 2012; Freeman, 1977; Hamermesh, 1977; Miller and Monge, 1986). Influential studies in economics have revealed a key causal role for reference points in the determination of worker satisfaction; much of this work has focused on wages, for which reference points are often determined by peer comparisons (Breza et al., 2017; Card et al., 2012; Clark and Oswald, 1996; Mas, 2006; Ockenfels et al., 2015). Non-wage compensation, e.g., fringe benefits and workplace amenities, which plays an increasingly central role in total compensation packages and has been shown to be an important part of workers' perceptions of job offers, has received far less attention (Budd, 2004; Hart, 2010; Schnake, 2016; Simon and Kaestner, 2004; Woodbury, 1983).

The focus of this study is the role of reference dependence regarding non-wage amenities in determining worker satisfaction. We study the impacts of a change in the management of hostels for migrant garment workers in Bengaluru, India. This is an important context in which to study non-wage amenities, given the sea change in population employment shares happening in many low-income countries, in which millions of workers annually are moving away from agricultural production and into manufacturing and services sectors (World Bank, 2014). Since these growing sectors are largely located in urban areas, this process generates a large influx of rural migrants into cities (United Nations, 2008).¹ Due to soaring rents, migrants often avail themselves of subsidized housing in hostels operated by the firms at which they work. Life in hostels is in general characterized negatively – cramped quarters, a lack of cleanliness, insufficient access to basic utilities, and risk of theft and violence (Kirk, 2015; Mahadevia et al., 2012). There may thus be relatively large gains in satisfaction from even small improvements in living conditions in urban hostels.

At baseline, the hostels we study were employer-managed. In two phases, hostel management was transferred to a local NGO specializing in women's empowerment with specific experience managing migrant worker hostels. Hostels were randomized into either phase 1 or phase 2 of the transfer process. There was a gap of approximately 5 months between phases, during which phase 1 hostels were under the new (NGO) management and phase 2 hostels were still managed by the

¹The resulting high demand for urban real estate has been a key topic of interest for policymakers and academics alike (Duflo et al., 2012; Galiani et al., 2017; Garriga et al., 2017; Hsieh and Moretti, 2018).

employer. At the end of this 5-month gap, we surveyed a random sample of workers from all hostels to study differences in living conditions and the subjective well-being of workers generated by the change in management. Phase 2 hostels were then transferred as well to the new management, and the study period ended.

We document some rather surprising impacts of the intervention. Survey enumerators' blinded evaluations of the hostels find that treatment improved living conditions in several key dimensions (particularly related to cleanliness and safety). Yet, despite this, workers report being less satisfied with their living situation, their job, and their salary, and report substantial decreases in subjective well-being (measured via Cantril's Ladder and Kessler's depression-anxiety scale) as a result of treatment. Impacts on worker turnover, measured in the firm's administrative data, echo this pattern of results. There is an initial increase in retention in the first month of treatment, which quickly disappears and gives way to (imprecisely estimated) negative impacts for the remainder of the study period.

Discussions with hostel residents led to the suggestion that "disappointment" with the actual changes that occurred in hostels was the leading explanation for the negative effects on satisfaction and subjective well-being. Hostel residents were sensitized to the management transfer and told that the new management would be an organization whose goals are broadly aligned with worker welfare, with a track record of running high quality hostels. They were also told that a large cash transfer would be made by the employer so as to substantially improve the housing quality, including repainting walls in the bedding area, replacing old facilities in kitchen, bathroom, and toilet, and increasing manpower to improve sanitation and security. However, data from the human resources department of the firm suggest that transfers made to the new management were fairly small and mainly used for increasing manpower.² Enumerators' blinded evaluations of housing quality are consistent with this statement, suggesting that treatment only induced improvements in cleanliness and safety and had little impact on other aspects of quality. In qualitative follow-ups, many hostel residents complained that the improvements that occurred were far below their expectations, and did not make meaningful differences in their everyday lives.

This qualitative evidence suggests that negative effects on satisfaction and subjective well-

²Operating cost for the employer before the transfer is roughly INR 1,325 per resident per month. The employer pays the new management INR 1,475 per month for each worker, wherein INR 600 is deducted from worker salary. This includes water, electricity, rent, staff salaries etc.

being were due to expectation-based reference dependence among hostel residents. When utility is anchored to a reference point determined by the expectation of a future outcome, falling short of that expectation, even if this entails an increase in consumption, can cause utility declines (Delquié and Cillo, 2006; Gul, 1991; Kőszegi and Rabin, 2006, 2007; Loomes and Sugden, 1986). Imagine, for example, if one expected a \$1,000 bonus but ended up with only \$500; the disappointment this causes may induce a loss in subjective well-being. Results from laboratory experiments suggest that individuals do indeed form reference points based on the expectation of future outcomes (as opposed to status quo outcomes) (Abeler et al., 2011; Gill and Prowse, 2012; Loomes and Sugden, 1987; Marzilli Ericson and Fuster, 2011). We argue that, in the context of our intervention, the modest improvements in living conditions that we document may not have measured up to the high expectations regarding the magnitude of quality improvements in hostels, generating a loss in utility for the hostel residents.

To evaluate this hypothesis, we turn to a second sample of “joiners”, i.e., workers who, as a result of their date of joining, moved into the hostels after phase 1 hostels had been transferred to the new management, but before phase 2 began. This group received the same treatment related to improved living conditions as did the original sample (who were present before phase 1 began), but they were not exposed to the expectation manipulation that occurred in the lead-up to the phase 1 transfer. Rather, when they arrived in Bengaluru, they simply happened to be placed in a treatment or control hostel, and experienced the living conditions at that hostel as *status quo*. This implies that the joiners should not have anchored their utility based on expectations of large changes in quality, and thus should not exhibit the same declines in subjective well-being documented for the original sample.

This is indeed what we find. Unlike the original sample, “joiners” in fact experienced higher satisfaction and weak increases in subjective well-being as a result of treatment, consistent with the treatment effects seen on enumerators’ evaluations of living standards discussed earlier. We show that in a pooled specification, impacts on joiners are indeed statistically significantly different from impacts on the original sample. This second set of results, combined with the time pattern of retention impacts, helps us identify the particular mechanisms underlying the negative impacts on subjective well-being for the original sample.

We contribute to the understanding of the determinants of worker satisfaction. Much of this

literature in economics has focused on the role of reference dependence as it pertains to wages (Adhvaryu et al., 2019; Breza et al., 2017; Card et al., 2012; Clark and Oswald, 1996; Mas, 2006; Ockenfels et al., 2015).³ Non-wage compensation, though it plays a central role in driving labor market decisions, has not to our knowledge received the same attention (Budd, 2004; Hart, 2010; Simon and Kaestner, 2004; Woodbury, 1983). We add to existing studies by demonstrating how reference dependence around employer-sponsored living conditions for migrants can lead to substantial changes in worker satisfaction.

Our paper is also related to the large literature on the impacts of living standards improvements in low-income contexts. Most of this work focuses on policies related to slum upgrading programs (see excellent reviews of this literature in Brakarz and Jaitman (2013); Lilford et al. (2017)). Subjective wellbeing of residents is often a primary outcome in randomized evaluations of these programs, and most of this literature finds substantial short-term increases in wellbeing as a result of better housing quality (Cattaneo et al., 2009; Devoto et al., 2012; Galiani et al., 2017), though a recent study following up on one of these randomized evaluations found so-called hedonic adaptation, i.e., that happiness reverts over time to a stable reference point (Galiani et al., 2015). Also related is the work evaluating the landmark Moving to Opportunity housing voucher program in the United States, which finds substantial increases in mental health as well (Kling et al., 2007).

Our work builds on these studies in two main ways. First, we focus on migrant workers and living conditions in hostels (as opposed to lifelong urban residents in slums), an important and growing population that has not received adequate attention in previous work. Second, we focus on an indirect intervention, in that the management transfer that was randomized was one stage removed from actual living standards improvements. This distinction is important because it allows more room for the “disappointment” effect that we document than an intervention in which, say, all treated households receive a new, high-quality dwelling.

The remainder of the paper is organized as follows. Section 2 provides background and discusses experimental design. Section 3 discusses the data and provides summary statistics. Section 4

³Reference-dependent preferences have been a mainstay of behavioral economics theory for decades (Barberis, 2013; Delqu e and Cillo, 2006; Gul, 1991; Kahneman and Tversky, 1979; K szegi and Rabin, 2006, 2007, 2009; Loomes and Sugden, 1986). Evidence from laboratory and field settings has corroborated the real-world importance of reference dependence (O’Donoghue and Sprenger, 2018). For some recent examples of this work, see, e.g., Abeler et al. (2011); Allen et al. (2017); Backus et al. (2017); Bartling et al. (2015); Card and Dahl (2011); Crawford and Meng (2011); DellaVigna et al. (2017); Gill and Prowse (2012); List (2003); Marzilli Ericson and Fuster (2011); Pope and Schweitzer (2011).

describes the estimation strategy, shows the results, and evaluates possible mechanisms. Section 5 concludes.

2 Experimental Design

2.1 Context

We focus on the housing experiences of migrant workers working in ready-made garments production. The apparel sector employs a large share of low-income workers in many developing country labor markets, due in part to its labor-intensive production process. Since most garment manufacturing hubs are located in urban areas, the apparel sector represents important migration and employment opportunities for rural populations, especially for women, who comprise the majority of the garments workforce. Our firm partner, Shahi Exports, Private Limited, is the largest exporter of ready-made garments in India, and one of the four largest in the world. Shahi employs majority women (roughly 80% of tailors and production helpers) and a large proportion of the workforce is made up of migrants from rural areas (roughly 40% of the workforce of each factory unit on average). Like low-skill manufacturing firms the world over, Shahi faces high rates of turnover, especially among its migrant workers. On average, the firm replaces 75% of its workforce every year, which adversely impacts productivity and leads to high recruitment and training costs.

There are several hypothesized reasons for particularly high turnover among migrant workers. First, low-income workers may take up jobs as a safety net to cope with adverse shocks or temporary unemployment spells, rather than as longer-term careers (Blattman and Dercon, 2018). For example, frequent worker separation can be driven by seasonal migration, wherein rural households send migrants to urban factories during “lean” season, and these migrants subsequently return during and after the harvest seasons (Bryan et al., 2014). Second, migrant workers may lack the incentives to permanently settle in cities because doing so may isolate them from family and social networks (Barnhardt et al., 2017). Moreover, migrant workers may have imperfect information about job conditions before migrating to cities and may leave due to dissatisfaction. For migrant female workers, the potential barriers to assimilating into life in cities may be even stronger, given early marriage norms and other norms against women’s labor force participation in South Asia (Bernhardt et al., 2018; Chari et al., 2017; Field and Ambrus, 2008), lack of control over their

own earnings (Field et al., 2016), and competing demands on time from non-market work such as domestic chores and home production (Afridi et al., 2018).

We focus on housing quality for migrant workers. Due to high rents in megacities in many low-income countries, many migrant workers have little choice but to live in employer-subsidized hostels. Living conditions in these hostels are usually characterized by overcrowding, lack of security, and lack of sanitation and facilities. These poor living conditions could translate into dissatisfaction and a broad range of health issues among migrant workers, which in turn may lead to reduced tenure and increased turnover. In this study, we investigate how improved living conditions in hostels affect migrant workers' satisfaction and turnover.

2.2 Intervention

As of April 2016, the partner firm, Shahi Exports, owned and directly managed 80 hostels in the Bengaluru area, housing in total about 7,500 employees working in 19 factory units. Each hostel was managed by one live-in caretaker appointed by the employer. The average capacity per hostel was approximately 100 residents, with 6 to 8 residents living in each housing unit (which was similar to a one-bedroom apartment). Each resident paid about 10 USD (600 INR) per month in rent, which constituted about 10% of wages and was directly deducted from salary. The average operating costs of each hostel for the employer were about 1,370 USD per month, including utilities, rent, staff salaries etc.

In 2016, the employer decided to outsource the management of its hostels to Janodaya, a Bengaluru-area NGO specializing in women's empowerment and housing services for migrants. According to the agreement between the two organizations, the employer paid Janodaya an average of 1,500 USD per month for each hostel under its management. Janodaya assigned two trained social workers to each hostel to undertake day-to-day management (in place of the live-in caretakers who were employed by the firm). One social worker was in charge of sanitary conditions and the other was charged with security and utility maintenance. Janodaya also bore the entirety of utilities costs, as well as any other costs of running the hostels. The NGO also promised to provide free language and cooking training and other programming designed to enhance the well-being of residents. Appendix B details the full list of changes promised by Janodaya in the hostels. To provide some background on changes in hostel conditions due to the intervention, in Figure 1 we

include a set of photographs of the hostels before and after they were transferred.



Figure 1: Hostel Conditions Before and After Treatment

2.3 RCT Design

We studied the impacts of this changeover in management on living conditions in the hostels; residents' perceptions of hostel conditions; measures of residents' subjective wellbeing; as well as workplace outcomes. In order to estimate causal treatment effects, we convinced Shahi Exports to roll out the management changeover across units in two phases, with factory units (and their corresponding hostels) assigned randomly to either the first or the second phase of changeover.

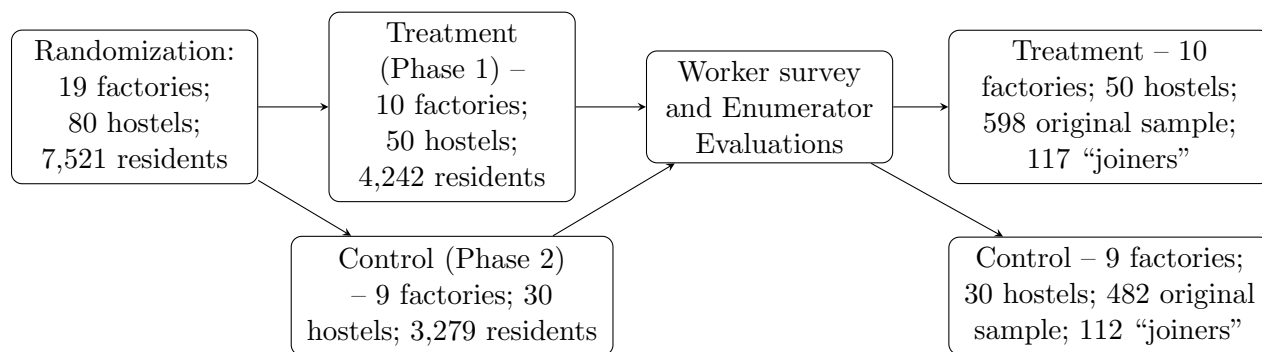


Figure 2A: Experimental Design

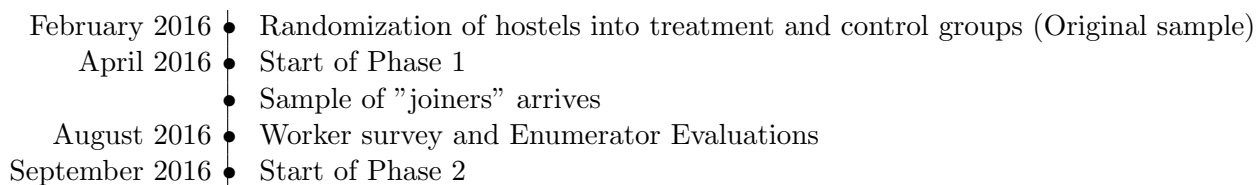


Figure 2B: Timeline of Experiment and Data Collection

In total, 80 hostels linked to 19 factory units were handed over to the NGO in these two phases. Ten factories were randomized to phase I and the remaining 9 to phase II. Phase I began on April 1, 2016, wherein 50 hostels corresponding to 10 factories were treated, while phase II took effect 5 months later, in September 2016, wherein the remaining 30 hostels corresponding to 9 factory units were transferred to the new management. Residents of the 50 hostels in phase I serve as our treatment group and residents of the 30 hostels in phase II constitute the control group. Hostel residents were notified about and sensitized to the shift in management about two weeks in advance of the changeover.

Our research design takes advantage of the gap of approximately five months between phases I and II, during which treatment hostels were under the new (NGO) management while control hostels were still managed by the employer. Near the end of this five-month gap, we conducted a survey among a random sample of workers from all hostels to study differences in satisfaction, subjective well-being, and perceived changes in hostel conditions among workers. At the same time, we administered a blinded enumerator evaluation survey for all 80 hostels in order to form

“objective” measures of changes in housing quality. We describe these survey efforts below.

3 Data

Two surveys were conducted to measure perceived changes in living conditions as well as satisfaction and subjective well-being among residents. We are also able to track retention of migrant workers, using the firm’s administrative data.

3.1 Worker Survey

We randomly sampled workers from the full roster of residents from each hostel and surveyed them in August 2016, approximately five months after treatment hostels had been transferred to new management (control hostels were still under employer management until the following month). We compiled a roster of all residents in the 80 hostels under study in February 2016 (these were the latest rosters available to us prior to the management changeover in Phase I hostels), and surveyed a random sample of (1,080).⁴ We refer to these respondents as the “original sample” hereafter in the paper. Summary statistics and balance checks between treatment and control groups in the survey respondent sample are reported in Table 1.⁵

We also surveyed an additional randomly selected sample of 229 hostel residents who joined the firm between April and July 2016, of whom 117 and 112 were living in treatment and control hostels, respectively. Since all workers in this sample joined the firm after Phase I hostels had been transferred (and before Phase II began), we refer these respondents as “joiners”. Summary statistics and balance checks between treatment and control groups in this additional sample are reported in Table A2. The worker survey includes, among other things, questions related to subjective well-being (satisfaction with hostel conditions, job position, and payment), perceived hostel conditions in various dimensions (cleanliness, congestion, safety, utility access, etc.), and physical and mental health (psychological distress and self-esteem).

⁴There was attrition between the set of workers sampled from the baseline hostel roster and the set of survey respondents. However, attrition across treatment and control groups was not differential; and balance is preserved across treatment and control in the respondent sample as shown in Table 1.

⁵Summary statistics and balance checks for the whole populations of hostel residents are reported in Table A1.

3.2 Enumerator Evaluation Survey

We also undertook a second data collection effort to elicit blinded evaluations of living conditions by survey enumerators. The evaluation covered questions related to hostel conditions in several important dimensions. Each hostel was visited by two enumerators, who were asked to rate similar measures by observation (cleanliness, congestion, safety and comfort, etc.) on a Likert scale, and to gauge access to utilities, including working toilet, bathroom, and kitchen. To make sure that the evaluations were not biased, the survey was contracted out to a third-party survey firm, whose enumerators were unaware of the intervention or the treatment status of dorms. The site visits were also done unannounced so that hostel managers were unable to manipulate living conditions right before the visits.

3.3 Firm Administrative Data

Using employee identifiers, we match data from the worker survey to administrative data from Shahi Exports. We focus on data on workers' demographic characteristics and retention. The variables available in demographic data include gender, age, date on which the worker joined the firm, and job type. We also observe monthly salary data for all workers from which we can obtain monthly worker retention.⁶

3.4 Summary Statistics and Balance Checks

Table 1 presents summary statistics as well as balance checks for worker characteristics and baseline values of workplace measures at the time of the hostel resident survey. We look at attendance rate, salary (available for original sample only), age, years of tenure with the firm, and indicators for gender, marriage, and children. Tests of differences in means across treatment and control groups are presented. We fail to reject that the difference between means for treated and control workers for any of these measures at baseline is zero.

23% of those surveyed were male workers. The average worker was about 23 years old. Average tenure with the firm was slightly less than 1 year. About 78% of these migrants work as tailors. Only about 8% of the sample are ever married, and about 6% have children.

⁶We also observe the attendance patterns of employees, recorded on a daily basis. We present results from analysis of this data in the appendix only, as the pattern resembles that of the retention data but estimates are less precise.

Table 1: Summary Statistics and Balance Checks for Original Sample

| Variables | Control | | Treated | | Difference | |
|----------------------------------|---------|-------|---------|-------|--------------------|----------------|
| | 482 | | 598 | | Mean Difference | <i>p</i> value |
| | Mean | SD | Mean | SD | | |
| Attendance Rate (March 2016) | 0.942 | 0.008 | 0.939 | 0.007 | 0.003 | 0.764 |
| Log(Salary) | 8.919 | 0.008 | 8.921 | 0.007 | -0.002 | 0.853 |
| Gender | 0.230 | 0.051 | 0.239 | 0.046 | -0.008 | 0.901 |
| Age | 23.09 | 0.196 | 23.19 | 0.176 | -0.100 | 0.706 |
| Years of Tenure | 0.925 | 0.095 | 0.961 | 0.086 | -0.036 | 0.782 |
| Tailor | 0.743 | 0.047 | 0.808 | 0.042 | -0.065 | 0.317 |
| Ever Married | 0.072 | 0.016 | 0.095 | 0.014 | -0.023 | 0.312 |
| Have Children | 0.054 | 0.012 | 0.063 | 0.011 | -0.009 | 0.570 |
| Household Engages in Agriculture | 0.777 | 0.034 | 0.790 | 0.030 | -0.013 | 0.772 |
| Household Owns Land | 0.704 | 0.030 | 0.716 | 0.027 | -0.012 | 0.764 |

Note: Standard errors are clustered at the unit level.

4 Results

4.1 Treatment effects on hostel conditions from enumerators' blinded evaluations

We begin by comparing several important dimensions of hostel conditions across treatment and control hostels, based on enumerators' blinded evaluations. The estimating equation is given as follows:

$$Y_{hue} = \alpha + \beta T_u + \lambda_e + \epsilon_{hue}, \quad (1)$$

where Y_{hue} is an outcome of hostel h , belonging to factory unit u , and evaluated by enumerator e . We had two enumerators visit each of the 80 hostels so we can account for heterogeneity across enumerators by including enumerator fixed effects in each regression. Standard errors are clustered by factory unit, the level at which the randomization was conducted. Given the small number of clusters (19 units), we report p -values obtained from the wild cluster bootstrap procedure developed in Cameron et al. (2008).

Table 2 presents the results. We measure five key dimensions of hostel conditions: cleanliness, safety, access to toilet and bathroom, access to kitchen, and bedding area conditions. Cleanliness and safety are measured by enumerator ratings on a 1-to-5 scale, with 5 being the highest possible rating.

Estimates in columns 1 and 2, related to cleanliness and safety, are both positive and precisely estimated, indicating modest improvements in important dimensions of living conditions in treatment hostels. Compared with the control hostels, treatment hostels experienced roughly a 10% (or .32 standard deviations) increase in both cleanliness and safety scores as a result of treatment.

The summary index related to toilets in column 3 averages across effects on four components – access to working toilets, cleanliness of toilets, access to working bathrooms, and cleanliness of bathrooms – as a measure of overall toilet and bathroom condition (normalized so that the index has mean 0 and SD 1). The treatment effect on this variable is not statistically significant ($p = .144$), though it is positive and quite large, suggesting that treatment increased toilet and bathroom conditions by .31 SD.

Summary indices for kitchen and bedding conditions are constructed in the same fashion and

Table 2: Hostel Conditions: Enumerator Evaluations

| VARIABLES | Overall Cleanliness- 1-5 rating | Overall Safety- 1-5 rating | Toilet & Bathroom- Index | Kitchen - Index | Sleeping Area-Index |
|-------------------|------------------------------------|-------------------------------|-----------------------------|--------------------|---------------------|
| | (1) | (2) | (3) | (4) | (5) |
| Treatment | .381 (.088) | .417 (.032) | .312 (.144) | .060 (.689) | .111 (.577) |
| Mean of dep. var. | 3.613 | 4.193 | 0 | 0 | 0 |
| Observations | 160 | 160 | 160 | 160 | 160 |

Note: Overall cleanliness and safety are rated on a 1-5 scale; Summary index in column 3: access to working toilets, cleanliness of toilets, access to working bathrooms, and cleanliness of bathrooms; Summary index in column 4: access to a working kitchen, cleanliness of kitchen, and safety of kitchen; Summary index in column 5: cleanliness, comfort, and spaciousness of the bedding area. All variables have been converted so that a larger value is a better outcome. The models control for enumerator fixed effects. *P*-values obtained via wild bootstrap as in Cameron et al (2008) with clustering at the factory unit level, based on 499 repetitions, appear in parentheses.

results are presented in column 4 and 5, respectively. Again, both estimates are positive but not statistically different from zero. Overall, we interpret the results from enumerators' evaluations as indicative of modest improvements in hostel living conditions due to treatment.⁷

4.2 Subjective Wellbeing

4.2.1 Original Sample

Next we investigate the effects of the treatment on subjective wellbeing among migrant workers. In particular, we explore the impacts of treatment on worker satisfaction and mental health. We focus

⁷To measure the impact of treatment on perceived changes in hostel conditions among residents, we also estimate regression models using measures of hostel conditions based on residents' evaluations in the survey, along the same dimensions assessed in the enumerator evaluations. We estimate those models separately for the original sample and for the sample of "joiners". We do not find any significant effects of treatment on residents' evaluations, suggesting that residents did not internalize the modest improvements documented by enumerators (reported above). Nevertheless, we note that the original sample tends to report slightly negative impacts; while the "joiners" report slightly positive impacts. Results are reported in panels A and B of Appendix Table A3. This pattern is consistent with treatment impacting the subjective thresholds against which the hostel residents assess their satisfaction levels with each dimension. This phenomenon has been documented and studied extensively in the health and subjective well-being literature (see, King et al. (2004), Kapteyn et al. (2007), and Molina (2016).)

on subjective wellbeing as a key outcome for two reasons. First, the intervention was originally designed by the partner firm to improve worker welfare and satisfaction, which may, consequently, matter for important workplace outcomes such as retention and productivity. Second, as suggested by prior studies (Kling et al., 2007), subjective wellbeing can be more sensitive to changes in living conditions than economic and physical health outcomes. Because the intervention was randomly assigned, the research design used in this paper is based on comparisons of treatment and control group means. For each worker-level outcome, we estimate the following regression specification:

$$Y_{iu} = \alpha + \beta T_u + \gamma X + \epsilon_{iu}, \quad (2)$$

where Y_{iu} is one of the measures of subjective wellbeing for worker i in factory unit u ; T_u is an indicator equal to 1 for individuals living in treatment hostels; and X is a vector of control variables, which includes gender, marital status, an indicator for having children, year of birth fixed effects, year-month of joining fixed effects, production-division fixed effects, and enumerator fixed effects. We cluster standard errors at the factory unit level and report p -values obtained from a wild cluster bootstrap procedure.

We begin by showing treatment effects on satisfaction among original sample. Those workers all joined before the start of phase 1 and thus, were fully exposed to the intervention including all messaging from the firm regarding the upcoming changes. Table 3 presents estimates of equation 2 with 4 measures of general satisfaction as dependent variables. In column 1-3, the outcomes are general satisfaction regarding the respondent’s overall dorm situation, job position, and monthly pay, respectively. The original measure of satisfaction is on a Likert scale, ranging from “extremely dissatisfied” to “extremely satisfied”. To capture the key variation in this measure, we construct a binary variable that equals 1 if a worker is “extremely satisfied”.

The results are quite striking. Columns 1–3 show a strong *negative* effect of treatment on worker satisfaction. Migrant workers living in treatment hostels were 7.4 percentage points less likely to be satisfied with their hostel situation. They are 9.2 and 7.9 percentage points less likely to be satisfied with their job position and monthly pay. All three coefficients are large in magnitude and significant at conventional levels. In column 4, we follow Kling et al. (2007) and Anderson (2008)

and estimate a summary standardized index that aggregates information over multiple treatment effect estimates. Specifically, we create an index of overall satisfaction that averages together three measures of satisfaction in columns 1–3. The summary index is defined to be the simple average across standardized z -score measures of each component. The z -score is calculated by subtracting the mean and dividing by the standard deviation. All components have been “realigned,” so to speak, whenever necessary, so that a higher score is a better outcome. The summary index has mean 0 and standard deviation 1; therefore, the estimates are interpreted in terms of standard deviation units. Results in column 4 indicate a fairly large negative effect on overall worker satisfaction. For the summary index that averages together all three measures, the estimate is consistently negative (with $p = 0.02$).

To check the robustness of these results, we estimate corresponding ordered probit models using the original measures of satisfaction as dependent variables, which are on a 1-5 scale. The results are highly consistent and are reported in Appendix Table A4. All signs of coefficients on treatment indicator are negative and statistically significant at the 5% or 1% level. The marginal effects of treatment on worker satisfaction related to hostel, job, and monthly pay are 7.8%, 7.9%, and 5.8%, respectively, which are very similar to estimates from the linear probability models.

Table 3: General Satisfaction- Original Sample

| VARIABLES | Dorm Situation – Very Satisfied | Job Position – Very Satisfied | Monthly Pay – Very Satisfied | Mean Effect (1) – (3) |
|-------------------|------------------------------------|----------------------------------|---------------------------------|--------------------------|
| | (1) | (2) | (3) | (4) |
| Treatment | -.074 (.064) | -.092 (.004) | -.079 (.080) | -.253 (.020) |
| Mean of dep. Var. | .663 | .606 | .159 | 0 |
| Observations | 1,080 | 1,080 | 1,080 | 1,080 |

Note: The models control for gender, marital status, an indicator for having children, enumerator fixed effects, division fixed effects, year of birth and month of joining fixed effects. P -values obtained via wild bootstrap as in Cameron et al (2008) with clustering at the factory unit level, based on 499 repetitions, appear in parentheses.

We further investigate the impacts of the treatment on psychological wellbeing of workers,

by estimating equation 2 with measures constructed from Cantril’s ladder and the Kessler 10 (K10) psychological distress scale (Andrews and Slade, 2001; Kessler et al., 2002). Consistent with the findings on satisfaction, results in Table 4 show adverse effects of treatment on psychological wellbeing. The treatment effect estimate in column 1 is quite strongly negative (with $p < 0.01$) indicating migrant workers in treatment hostels systematically report being at a lower step in Cantril’s imagined life ladder. Column 2 reports the estimate for the K10 psychological distress index, which is on a 10-50 scale, where smaller scores indicate less likelihood of psychological distress. Again, the estimate is statistically significant and shows an adverse effect on psychological wellbeing of workers.

Table 4: Cantril’s Ladder and Psychological Distress (K10 Score) - Original Sample

| VARIABLES | Step of Ladder 0-10 Scale | K10 Score | Log of K10 Score | Moderate K10 \geq 25 |
|-------------------|------------------------------|-----------------|---------------------|---------------------------|
| | (1) | (2) | (3) | (4) |
| Treatment | -.563 (.004) | 1.002 (.044) | .061 (.044) | .045 (.000) |
| Mean of dep. var. | 5.9 | 14.55 | 2.633 | .045 |
| Observations | 1,080 | 1,080 | 1,080 | 1,080 |

Note: The models control for gender, marital status, an indicator for having children, division indicators, enumerator fixed effects, year of birth and month of joining fixed effects. *P*-values obtained via wild bootstrap as in Cameron et al (2008) with clustering at the factory unit level, based on 499 repetitions, appear in parentheses.

We construct two measures of psychological distress based on the K10 index, namely the log of K10 index and an indicator for moderate distress. The results are presented in column 3 and 4, respectively. Both estimates are in the same direction and of similar significance as former estimates. The result in column 4 shows that the probability of moderate distress increased by 4.5 percentage points due to treatment.

In sum, our results show that the treatment modestly improved the housing quality, especially in several key dimensions. However, original sample workers living in treatment hostels did not perceive these modest improvements and tend to report negative effects. In addition, their subjective well-being significantly declined relative to residents in control hostels. In particular, we document a 0.15 standard deviation decrease in satisfaction with housing quality, a 0.31 standard deviation decrease in life satisfaction, and a 0.21 standard deviation increase in psychological distress. These effects are economically meaningful compared with other studies on subjective well-being. For example, Haushofer and Shapiro (2016) estimate that an average cash transfer of \$709 PPP in rural Kenya increased happiness and life satisfaction by 0.16 and 0.17 standard deviations, respectively, and decreased stress by 0.26 standard deviations. Galiani et al. (2017) find that providing better houses to the extremely poor in Latin America increased satisfaction with housing quality by between 0.5 and 0.63 standard deviations and satisfaction with quality of life by almost 0.4 standard deviations.

Puzzled by these negative treatment effects, we conducted follow-up interviews with a few residents living in treatment hostels. Responses of hostel residents pointed to “disappointment” as a leading explanation behind the results. Two weeks prior to the intervention, residents in treatment hostels were told by managers that the hostel management would be transferred to an experienced local NGO whose aim is to improve the welfare of migrant workers and a large amount of money would be transferred from the employer so as to increase manpower, repair and replace old facilities, paint walls etc. They were also told several welfare programs will be introduced by the new management, including free language and skill training, regular access to free supplements and fruits etc. However, during our interviews many respondents reported that most things that had been promised were not provided and little change had occurred in their hostels. Others thought that there were changes made by the new management but they were not enough to make meaningful differences to their lives.

In light of this, we propose that the decline in subjective well-being is related to workers’ expectations of changes in living conditions relative to the actual changes that occurred as a result of the intervention. In particular, we argue the results are consistent with a reference-dependence hypothesis, under which residents’ utility is anchored to a reference point determined by their expectation of future housing quality. When the actual changes did not measure up to these

expectations, even if they were mildly positive, residents would have experienced a loss in utility.

To test the hypothesis, we turn to our sample of “joiners,” who arrived at the hostels after phase 1 hostels (treatment) have been transferred to the new management, but before phase 2 began. When these workers joined the firm, they were randomly assigned to a hostel and experienced the same treatment-induced variation in living conditions that the original sample did. However, they were not exposed to the expectations manipulation that occurred before phase 1 began. If the reference-dependence hypothesis is true, the joiners should not exhibit the same decline in subjective well-being as documented for the original sample.

4.2.2 Results for Joiners

Appendix Table A2 presents the summary statistics and balance checks for the sample of “joiners.” Overall, this sample is balanced across treatment and control groups except that workers in treatment hostels tend to join the firm slightly earlier (less than 3 weeks). We control for the month of tenure fixed effects in all of our regressions and also examine the treatment effect by tenure month later in this paper. In short, our results do not seem to be driven by this difference.

We study the same measures of subjective well-being for the sample of “joiners” using the same regression specification as in equation 2. Table 5 presents the results using measures of satisfaction as dependent variables. Consistent with the prediction of the reference-dependence hypothesis, we do find that joiners in treatment hostels experienced increases in housing- and job- related satisfaction. All of the three estimates in column 1-3 are positive and economically meaningful in magnitude. Joiners in treatment hostels were 9.1 percentage points more likely to be satisfied with overall dorm condition, although this point estimate is not statistically significant ($p = 0.18$). They were also 14 percentage points more likely to be satisfied with their job positions and monthly pay and both coefficients are statistically different from 0 at conventional levels. The “mean effect” estimate in column 4 indicates that on average, treatment increased the overall satisfaction of a resident by 0.37 SD. Corresponding estimates from ordered probit models are consistently positive and similar in significance and can be found in Appendix Table A5. Estimates related to psychological wellbeing for joiners are presented in Table 6. Overall, the estimates are small in magnitude and imprecisely estimated. We turn next to a pooled specification to compare treatment effects between original sample and “joiners”.

Table 5: General Satisfaction - Joiners

| VARIABLES | Dorm Situation – Very Satisfied | Job Position– Very Satisfied | Monthly Pay – Very Satisfied | Mean Effect (1) - (3) |
|-------------------|------------------------------------|---------------------------------|---------------------------------|--------------------------|
| | (1) | (2) | (3) | (4) |
| Treatment | .091 (.180) | .149 (.036) | .141 (.056) | .376 (.012) |
| Mean of dep. var. | .567 | .537 | .196 | 0 |
| Observations | 229 | 229 | 229 | 229 |

Note: The models control for gender, marital status, an indicator for having children, enumerator fixed effects, division fixed effects, year of birth and month of joining fixed effects. *P*-values obtained via wild bootstrap as in Cameron et al (2008) with clustering at the factory unit level, based on 499 repetitions, appear in parentheses.

Table 6: Cantril's Ladder and Psychological Distress (K10 Score) - Joiners

| VARIABLES | Step of Ladder 0-10 Scale | K10 Score | Log of K10 Score | Moderate K10 \geq 25 |
|-------------------|------------------------------|-----------------|---------------------|---------------------------|
| | (1) | (2) | (3) | (4) |
| Treatment | .097 (.774) | -.149 (.773) | -.016 (.593) | .004 (.853) |
| Mean of dep. var. | 5.768 | 14.32 | 2.629 | .017 |
| Observations | 229 | 229 | 229 | 229 |

Note: The models control for gender, marital status, an indicator for having children, enumerator fixed effects, division fixed effects, year of birth and month of joining fixed effects. *P*-values obtained via wild bootstrap as in Cameron et al (2008) with clustering at the factory unit level, based on 499 repetitions, appear in parentheses.

4.2.3 Difference in Treatment Effect between Original Sample and Joiners

In this subsection, we test whether the treatment effects for the original sample are statistically different from those for the joiners. Let J_i be an indicator equal to 1 for joiners and T_u be an indicator for the treatment status. The following regression equation is estimated using the entire

sample that pools the original sample and joiners sample:

$$Y_{iu} = \alpha + \beta_1 T_u + \beta_2 T_u \times J_i + \gamma X + \epsilon_{iu} \quad (3)$$

where a measure of subjective wellbeing for individual i in factory unit u is regressed on the explanatory variable of interest $T_u * J_i$, main effect T_u , and a series of control variables. The control variables are the same as in equation 2, with the only difference that the vector X now also allows for the impact of baseline characteristics to vary across the original sample and “joiners” and also includes the main effect of J_i . This mitigates concerns that joiners are responding differently to the treatment because they might be systematically different in baseline characteristics. The coefficient β_2 delivers the difference in treatment effects between the original sample and the joiners. The results for worker satisfaction and psychological well-being are presented in Table 7 and Table 8, respectively.

Consistent with the previously presented results, estimates of main effects in the first rows of Table 7 and Table 8 show that original sample experienced substantial declines in worker satisfaction and mental wellbeing. Estimates on the interaction term are also economically meaningful and statistically significant. Results in column 1 of Table 7 show that joiners in treatment hostels were 16.4 percentage points more likely to be very satisfied with the overall dorm condition than original sample residents in the same hostel. They were also 24 and 22 percentage points more likely to be very satisfied with their job position and monthly pay, respectively. Mean effect estimates in column 4 of Table 7 indicate that the treatment effect on overall satisfaction for joiners is .6 SD greater than that for the original sample. Estimates in Table 8 consistently indicate that joiners in treatment hostels reported being at a significantly higher step in Cantril’s imagined life ladder and were less likely to experience psychological distress.

The above pattern of results allows us to rule out several competing hypotheses. For example, one concern is that despite the increase in housing quality along important dimensions as a result of treatment, housing quality may have declined in some other dimensions that we were not able to measure in the survey. If those unobserved aspects were more important to residents, overall satisfaction and subjective well-being could have declined as a consequence. Another concern is that the transfer of management to another organization allowed the employer to free human

resources from the treatment hostels so that they could be spent on control hostels. The decline in subjective well-being among treatment residents may actually reflect an increase in subjective well-being among control residents. While these hypotheses are consistent with the negative effects that are documented for the original sample, they are inconsistent with the positive effects for the joiners. Since “joiners” experienced the same treatment-induced variation in living conditions as the original sample did, if either of these alternative explanations were true, we should see similar treatment effects for the two samples.

Table 7: General Satisfaction - Pooled Specifications

| VARIABLES | Dorm Situation – Very Satisfied | Job Position– Very Satisfied | Monthly Payment – Very Satisfied | Mean Effect (1) - (3) |
|-----------------------|------------------------------------|---------------------------------|--|--------------------------|
| | (1) | (2) | (3) | (4) |
| Treatment | -.073 (.064) | -.092 (.004) | -.078 (.092) | -.250 (.024) |
| Treatment*1 (Joiners) | .164 (.028) | .241 (.000) | .219 (.012) | .644 (.000) |
| Mean of dep. var. | .647 | .594 | .166 | 0 |
| Observations | 1,309 | 1,309 | 1,309 | 1,309 |

Note: The models include a full set of controls and their interactions with a dummy for “joiner”. *P*-values obtained via wild bootstrap as in Cameron et al (2008) with clustering at the factory unit level, based on 499 repetitions, appear in parentheses.

4.2.4 Heterogenous Effects by Tenure Month

Here, we expand the above analysis by allowing treatment effect to vary by months of tenure of the hostel resident. In particular, we interact an indicator for treatment status with a series of dummies for the following tenure month bins (1-2, 3-4, 5-6, 7-10, 11-18, >18). Notice that a joiner typically has a tenure month between 1 and 4 at the survey time. Original sample workers have tenure that are longer than 4 months. This heterogeneity analysis allows us to check the robustness of our results and more importantly, to speak to two other competing explanations which are in

Table 8: Cantril’s Ladder and Psychological Distress (K10 Score) - Pooled Specifications

| VARIABLES | Step of Ladder 0-10 Scale | K10 Score | Log of K10 Score | Moderate K10 >= 25 |
|-----------------------|------------------------------|------------------|---------------------|-----------------------|
| | (1) | (2) | (3) | (4) |
| Treatment | -0.563 (.004) | 1.000 (.044) | .061 (.044) | .045 (.008) |
| Treatment*1 (Joiners) | .661 (.076) | -1.150 (.064) | -.076 (.048) | -.041 (.100) |
| Mean of dep. var. | 5.877 | 14.508 | 2.632 | .040 |
| Observations | 1,309 | 1,309 | 1,309 | 1,309 |

Note: The models include a full set of controls and their interactions with a dummy for “joiner”. *P*-values obtained via wild bootstrap as in Cameron et al (2008) with clustering at the factory unit level, based on 499 repetitions, appear in parentheses.

some sense consistent with the pattern of impacts we have documented so far.

Specifically, the disutility experienced by original sample workers in the treatment group might arise from so-called betrayal aversion (Bohnet et al., 2008; Bohnet and Zeckhauser, 2004). That is, hostel residents might have experienced utility losses because they felt “betrayed” by the firm, which did not keep its promise of substantially improving hostel conditions. Another possibility is that the change in hostel management was disruptive to the original sample of workers, either because individuals are intrinsically resistant to change (Oreg, 2003), or because they have become accustomed to old practices and changes within the organization make them worse off (Atkin et al., 2017; Dow and Perotti, 2013). The argument would be that “joiners” are not affected in the same way simply because they arrived after the management handover had occurred, and thus experienced the new management as *status quo*.

Figure 3 plots the coefficients and 95% confidence intervals for each tenure month bin using the mean effect of satisfaction as the dependent variable. Each coefficient represents the estimate of the treatment effect on overall satisfaction for residents in that tenure month bin. Two important features stand out. First, there is a sharp jump in treatment effect from a positive value to a

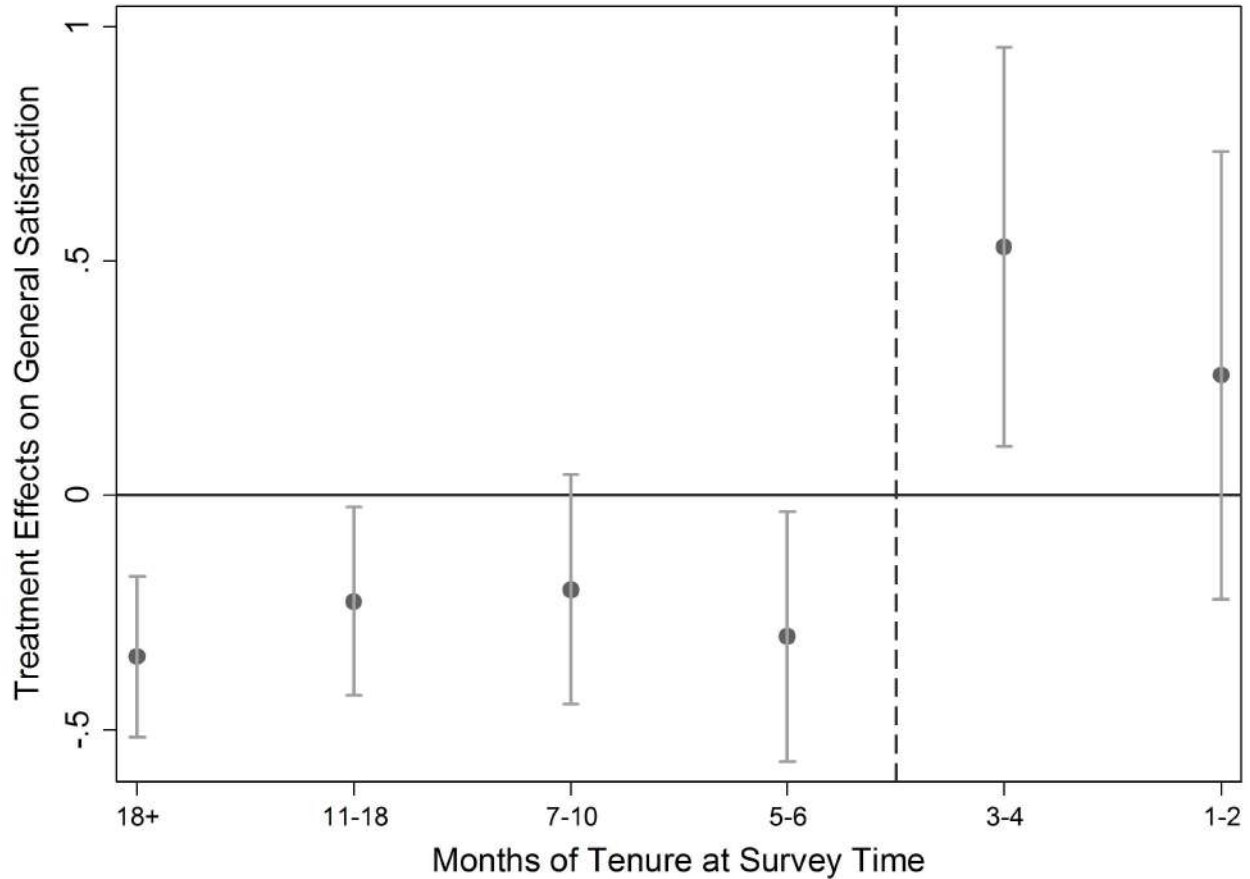


Figure 3: Treatment Effects on Overall Satisfaction, by Tenure Month

Note: This figure plots coefficient estimates and 95% confidence intervals for treatment dummy interacted with the tenure month indicators listed on the x-axis. The regression includes a full set of controls and their interactions with a dummy for “joiners”. Dependent variable is the mean effect of satisfaction.

negative when tenure month increases from 4 to 5, suggesting that whether or not a resident has been exposed to the old management determined the sign of the treatment effect.

Second, the treatment effect does not appear to vary by tenure month for the original sample. If betrayal and aversion to change were driving mechanisms for the treatment effect, one might expect to see a stronger sentiment of betrayal and more aversion to change the higher the worker’s tenure. The fact that we do not find this, of course, does not provide dispositive evidence refuting the importance of betrayal and change aversion, it suggests that perhaps these mechanisms, if they were indeed at play, are not fully driving the results.

4.3 Worker Retention

In this section, we use the firm’s administrative data linked to hostel residents by their worker IDs to investigate the treatment effect on worker retention at the firm. Monthly payroll data allow us to track all residents living in hostels at the baseline (February 2016) and know exactly when they leave the firm. We investigate treatment effects on worker retention by estimating the following regression specification on all residents living in hostels at the baseline:

$$Y_{iut} = \sum_{k=4}^{12} \beta_k T_u \times 1(\gamma_t = k) + \lambda_u + \mu_{gt} + \epsilon_{iut} \quad (4)$$

where the outcome is an indicator variable that takes the value 1 if worker i from factory unit u was retained in month t and 0 otherwise. T_u is a dummy variable that takes the value 1 if the worker is from a treatment factory unit and 0 if she is from a control and it is interacted with monthly dummies from April 2016 onwards (i.e., $1(\gamma_t = k)$ is an indicator equal to 1 for the k -th month of 2016). We use data from February to December 2016. Dummies for February and March are omitted to make treatment relative to the pre-treatment period. Each regression includes unit fixed effects λ_u (which absorb the main effect of the treatment indicator) and month by gender fixed effects μ_{gt} (which absorbs gender specific time-variant determinants of retention common to all units). This specification allows the coefficient on the treatment indicator to vary by month. β_k are the key coefficients of interest, representing the treatment effects on retention in a given month k .

We report the results in column 1 of Appendix Table A6 and plot the month-by-month treatment impacts on (cumulative) retention rate in Figure 4. Results show that migrant workers living in treatment hostels were 3.2 percentage points more likely to be retained in the first month of treatment and the estimate is statistically significant at the 5% level. However, this impact quickly diminished and gave way to (imprecisely estimated) negative impacts for the remainder of the study period.

This pattern of retention contributes additional evidence in support of the reference-dependence hypothesis and against the change-aversion story. In particular, if the change in management is disruptive to hostel residents, we should observe decline in retention rate right after the change

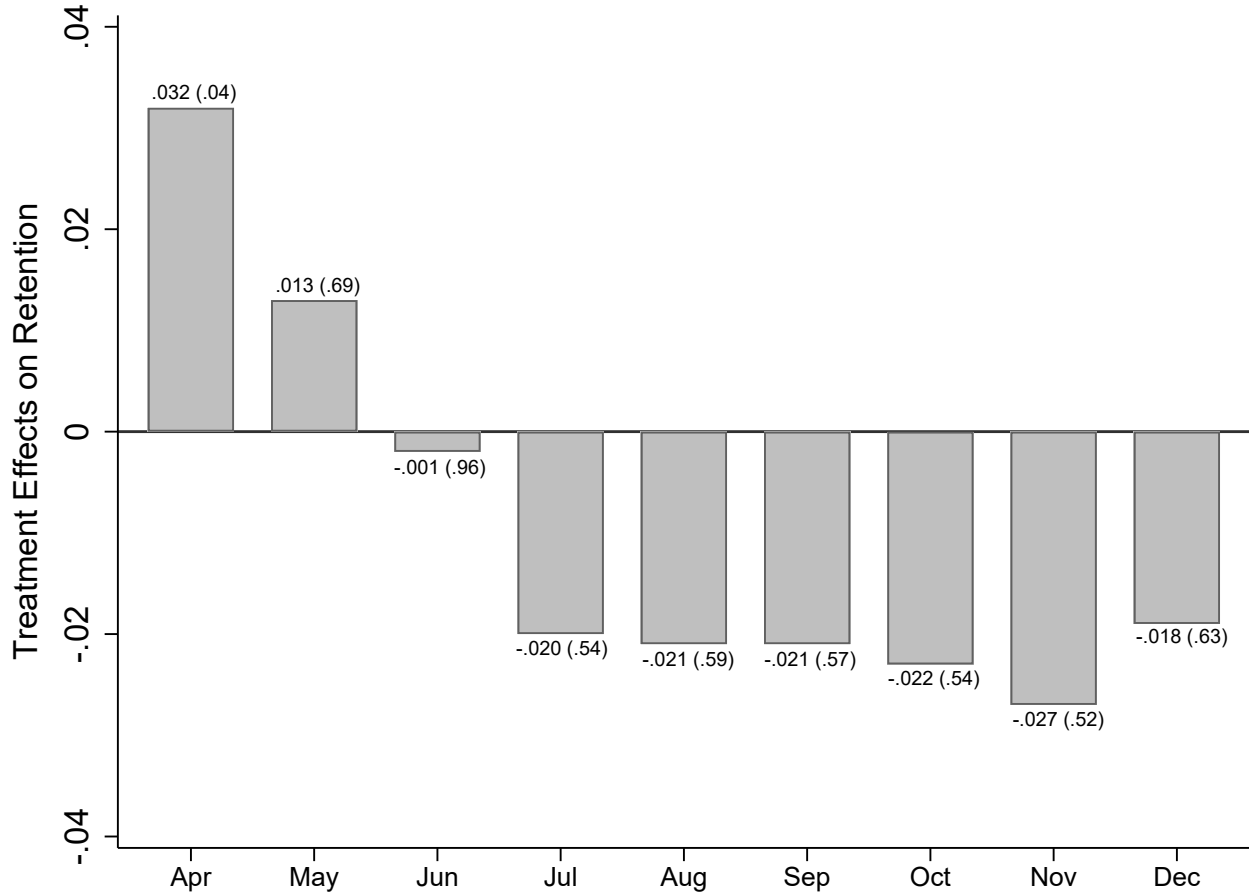


Figure 4: Treatment Effects on Worker Retention by Month

Note: This figure plots coefficient estimates for monthly treatment impacts on worker retention. The regression uses data for all residents living in hostels at the baseline (February 2016) and includes unit fixed effects and gender by month fixed effects. Sample period is February–December 2016. *P*-values obtained via wild bootstrap as in Cameron et al (2008) with clustering at the factory unit level, based on 499 repetitions, appear in parentheses.

occurred when the disruption was most intense. Rather, these results are consistent with residents holding high expectations for improvements in hostel conditions when the management was first transferred in April and thus being more likely to be retained. When the modest improvements that actually occurred fell below their expectations, worker separation increased due to disappointment.⁸

⁸A similar pattern emerges when using a variable measuring both retained and present at work on a given day (using administrative data on attendance) as the outcome, but this analysis yields less precisely estimated coefficients. We present these results in column 2 of Appendix Table A6.

5 Conclusion

This study documents the impacts of a change in the management of hostels housing garment workers in urban Bengaluru, India. Despite evidence of modest improvements in cleanliness and safety, two key determinants of hostel quality, we find that residents were substantially less satisfied with their housing and job situations, and reported higher levels of psychological distress, as a result of treatment.

We provide evidence supporting the idea that reference-dependent utility, in which reference points were anchored to high expectations of housing quality following the transfer of hostel management, could explain the surprising results we find. First, we study a sample of “joiners,” residents who arrived at the hostel after the first randomized phase of management transfer had taken place. This sample received all the benefits of improved living conditions without the expectations “manipulation” that may have occurred with the original sample. If our hypothesis related to reference dependence is correct, these joiners should not exhibit the same decreases in subjective wellbeing observed for the original sample. In line with this, we find indeed that joiners actually show increases in most measures of subjective wellbeing, hand in hand with the modest housing quality differential across treatment and control hostels. Second, we study the impacts of treatment on worker separation among the original sample and find that residents in the treatment hostels were more likely to be retained in the first month of treatment and then became more likely to leave in later period. This pattern further supports the reference dependence hypothesis and suggests that hostel residents held high expectations immediately following the transfer of management but were disappointed by the modest improvements that actually occurred.

Our results are important for policymaking in low-income country contexts because they emphasize the crucial role that properly setting expectations – and implementing policy that lives up to those expectations – can play in determining the success or failure of policies. The political economy of policymaking often necessitates that the potential benefits of proposed policies be widely disseminated, and the potential costs hidden, so that policies are most effectively “sold” to the public and its elected representatives. Our work points out that doing this comes at an inherent cost: the more a policy is oversold, the less likely it is that its effects will live up to expectations. If the gap between expectations and reality is large enough, even objectively successful programs

may fall prey to reference dependence, and subjective wellbeing may decline.

This does not necessarily imply that the returns to policymakers setting expectations low are large. If gains and losses relative to a reference point result in asymmetric changes in utility, it is likely that setting expectations extremely low would have only modest returns in terms of impacts on subjective wellbeing. Benchmarking expectations to the most likely policy outcome (with perhaps, at most, a slight undersell) could be roughly optimal in a world with implementation uncertainty.

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Appendix: Not for publication.

A Additional Results

Table A1: Summary Statistics and Balance Checks for the Whole Populations of Hostel Residents

| Variables | Control | | Treated | | Difference | |
|------------------------------|---------|-------|---------|-------|--------------------|----------------|
| | 3,279 | | 4,242 | | Mean Difference | <i>p</i> value |
| | Mean | SD | Mean | SD | | |
| Attendance Rate (March 2016) | 0.906 | 0.012 | 0.918 | 0.011 | -0.012 | 0.472 |
| Log(Salary) | 8.919 | 0.007 | 8.921 | 0.006 | -0.001 | 0.879 |
| Male | 0.287 | 0.050 | 0.309 | 0.047 | -0.022 | 0.754 |
| Age | 22.94 | 0.218 | 23.32 | 0.202 | -0.388 | 0.209 |
| Years of Tenure | 0.809 | 0.076 | 0.830 | 0.071 | -0.021 | 0.846 |
| Tailor | 0.717 | 0.050 | 0.756 | 0.047 | -0.039 | 0.574 |

Note: Standard errors are clustered at the unit level.

Table A2: Summary Statistics and Balance Checks for the Sample of “Joiners”

| Variables | Control | | Treated | | Difference | |
|----------------------------------|---------|-------|---------|-------|-----------------|----------------|
| | 112 | | 117 | | Mean Difference | <i>p</i> value |
| | Mean | SD | Mean | SD | | |
| Male | 0.313 | 0.085 | 0.359 | 0.077 | -0.046 | 0.691 |
| Age | 22.32 | 0.683 | 22.17 | 0.624 | 0.150 | 0.873 |
| Years of Tenure | 0.151 | 0.015 | 0.206 | 0.014 | -0.056 | 0.015 |
| Tailor | 0.598 | 0.104 | 0.735 | 0.092 | -0.137 | 0.340 |
| Ever Married | 0.107 | 0.036 | 0.120 | 0.034 | -0.013 | 0.802 |
| Have Children | 0.036 | 0.036 | 0.103 | 0.033 | -0.067 | 0.193 |
| Household Engages in Agriculture | 0.732 | 0.042 | 0.752 | 0.041 | -0.020 | 0.735 |
| Household Owns Land | 0.652 | 0.050 | 0.684 | 0.048 | -0.032 | 0.652 |

Note: Standard errors are clustered at the unit level.

Table A3: Perceived Hostel Conditions- Original Sample & Joiners

| VARIABLES | Overall Cleanliness 1-5 | Safety- Index | Toilet & Bathroom- Index | Kitchen- Index | Sleeping Area-Index |
|-------------------|-------------------------------|------------------|--------------------------------|-------------------|------------------------|
| | Panel A: Original Sample | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| Treatment | -.037 (.601) | -.101 (.277) | -.085 (.388) | -.100 (.413) | -.008 (.897) |
| Mean of dep. var. | 4.438 | 0 | 0 | 0 | 0 |
| Observations | 1,080 | 1,080 | 1,080 | 1,080 | 1,080 |
| Panel B: Joiners | | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| Treatment | .057 (.517) | .038 (.633) | .106 (.513) | .174 (.240) | .151 (.456) |
| Mean of dep. var. | 4.336 | 0 | 0 | 0 | 0 |
| Observations | 229 | 229 | 229 | 229 | 229 |

Note: Overall cleanliness is rated on a 1-5 scale; Summary index in column 2: An indicator for ever feeling unsafe in hostel and an indicator for ever feeling unsafe walking nearby hostel; Summary index in column 3: access to working toilets, cleanliness of toilets, access to working bathrooms, and cleanliness of bathrooms; Summary index in column 4: access to a working kitchen, cleanliness of kitchen, and safety of kitchen; Summary index in column 5: cleanliness, comfort, and spaciousness of the bedding area. All variables have been converted so that a larger value is a better outcome. The models control for gender, marital status, an indicator for having children, enumerator fixed effects, division fixed effects, year of birth and month of joining fixed effects. *P*-values obtained via wild bootstrap as in Cameron et al (2008) with clustering at the factory unit level, based on 499 repetitions, appear in parentheses.

Table A4: General Satisfaction- Original Sample (Ordered Probit model)

| VARIABLES | Dorm Situation – Very Satisfied | Job Position – Very Satisfied | Monthly Pay – Very Satisfied |
|--|------------------------------------|----------------------------------|---------------------------------|
| | (1) | (2) | (3) |
| Treatment | -.296 (.023) | -.285 (.000) | -.300 (.031) |
| Marginal effect for being "Very Satisfied" | -.078 (.023) | -.079 (.000) | -.058 (.054) |
| Mean of dep. var. | 4.494 | 4.450 | 2.986 |
| Observations | 1,080 | 1,080 | 1,080 |

Note: The models control for gender, marital status, an indicator for having children, enumerator fixed effects, division fixed effects, year of birth and month of joining fixed effects. *P*-values appear in parentheses. Standard errors are clustered at the unit level.

Table A5: General Satisfaction- Joiners (Ordered Probit model)

| VARIABLES | Dorm Situation – Very Satisfied | Job Position – Very Satisfied | Monthly Pay – Very Satisfied |
|--|------------------------------------|----------------------------------|---------------------------------|
| | (1) | (2) | (3) |
| Treatment | .118 (.653) | .544 (.006) | .549 (.005) |
| Marginal effect for being "Very Satisfied" | .027 (.652) | .141 (.004) | .110 (.001) |
| Mean of dep. var. | 4.331 | 4.397 | 3.257 |
| Observations | 229 | 229 | 229 |

Note: The models control for gender, marital status, an indicator for having children, enumerator fixed effects, division fixed effects, year of birth and month of joining fixed effects. *P*-values appear in parentheses. Standard errors are clustered at the unit level.

Table A6: Retention and working

| VARIABLES | Retained | Working |
|-------------------|------------------------------------|---|
| | 1(Workers Still on Payroll Roster) | 1(Worker Retained and Present in Factory today) |
| | (1) | (2) |
| Treatment*April | .032 (.044) | .013 (.657) |
| Treatment*May | .013 (.689) | .006 (.785) |
| Treatment*June | -.001 (.962) | -.012 (.709) |
| Treatment*July | -.020 (.537) | -.020 (.601) |
| Treatment*Aug | -.021 (.589) | -.027 (.597) |
| Treatment*Sep | -.021 (.569) | -.012 (.861) |
| Treatment*Oct | -.022 (.537) | -.028 (.653) |
| Treatment*Nov | -.027 (.521) | -.019 (.749) |
| Treatment*Dec | -.018 (.629) | -.019 (.725) |
| Mean of Dep. Var. | .659 | .554 |
| Observations | 75,878 | 1,986,624 |

Note: The models control for unit fixed effects and gender by month fixed effects. *P*-values obtained via wild bootstrap as in Cameron et al (2008) with clustering at the factory unit level, based on 499 repetitions, appear in parentheses.

B Intervention Details

- Two well-trained social workers will be appointed to each hostel, with one working as caretaker and the other as security guard.
- Caretakers will be responsible for personnel management, sanitary conditions, and coordination between Shahi and Janodaya. Security guards are in charge of security and utility maintenance. Regular checks on hostel conditions will be done by Janodaya.
- Free language, cooking, knitting, and other skill training will be provided to residents on a regular basis.
- Hostel residents will also be provided with nutritional amenities, including supplements and fruits.
- Recreational activities, including singing and dancing, will be held in hostels for interested residents.
- Grievances committee, Works committee, and Prevention of Sexual Harassment committee will be formed to help resolve disputes and conflicts between residents and to protect women from sexual harassment.
- Residents will be given more freedom of movement. Restrictions on times at which residents are allowed to enter and exit the hostels will be relaxed.