

# Does Food Insecurity Hinder Migration?

## Experimental Evidence from the Indian Public Distribution System

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**WORLD BANK GROUP**

Poverty and Equity Global Practice

August 2023

## Abstract

People may avoid migrating if they cannot insure themselves against the risk of a bad outcome. Governments can reduce the consumption risk faced by migrants by allowing them to access social protection programs in the destination. This study randomly informed around 62,000 households across 18 Indian states about a new program allowing migrants to

collect their food ration across the country, together with information about practical barriers to using the program. Four months later, treated households held lower beliefs about food ration portability, and were less likely to migrate to cities. The findings indicate that food insecurity risk reduces urban migration.

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# Does Food Insecurity Hinder Migration? Experimental Evidence from the Indian Public Distribution System\*

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JEL CLASSIFICATIONS: J61, R23, R12, I38

KEYWORDS: urban migration, risk, social protection, food security

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# 1 Introduction

Developing economies typically feature persistent spatial gaps in real income, which are substantial even after accounting for workers’ skills (Caselli, 2005, Gollin et al., 2014). Why don’t more people migrate to higher-income areas? A possible explanation is that migration is risky. Prospective migrants may be unable to manage the financial risk involved with job search in a distant location, and may anticipate losing access to basic necessities such as food or housing in the destination.<sup>1</sup> This risk is especially acute in cities, where households do not typically grow their own food and so must find stable employment to meet their food needs (IFPRI, 2017). Because most lucrative jobs are located in cities, food insecurity risk may have significant implications for aggregate productivity (Lagakos, 2020). Social protection programs have the potential to facilitate migration by providing access to basic necessities, but it is unclear which—if any—constraints are holding migrants back. Knowing which constraints affect potential migrants has important implications for public policy, as programs should target households facing the constraints they are designed to alleviate.

This paper studies whether improving food security in the destination facilitates migration using a cluster-randomized controlled trial. We randomly informed around 62,000 Indian households across 18 states about a recent scheme to make their food entitlements portable. The scheme, called “One Nation, One Ration Card” (ONORC), was introduced in 2019 by the Government of India to incorporate migrants into the Public Distribution System (PDS), a social protection program used by 63% of the Indian population to obtain subsidized food grain (MicroSave, 2020). The PDS is one of the largest food subsidy programs in the world, costing more than 1 percent of GDP each year (World Bank, 2018), and is likely the single biggest transfer program for most households in India (Gadenne et al., 2021). However, until recently migrants were excluded from this scheme, as beneficiaries were required to claim ration in a designated PDS shop in their home locality. The ONORC initiative, introduced in August 2019, allows beneficiaries to collect food ration across the entire country. Data we collected in January 2021 suggest that awareness of the program was low: in states that had joined ONORC by that time, only 28% of eligible households believed they could claim

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<sup>1</sup>A sizable minority of emigrants do not have jobs lined up when they arrive in the destination. In our data, 27% of emigrants—excluding those who emigrated for marriage—reported searching for a week or more after arriving before finding their first job. Eight percent searched for two weeks, and another 8 percent searched for three or more weeks.

ration in a PDS shop other than their home shop. Only 8% believed they could claim ration outside their home state.

We embedded an information experiment into a panel survey, the Consumer Pyramid Household Survey (CPHS), carried out three times a year by the Center for Monitoring Indian Economy (CMIE). During the survey wave from September–December 2021, enumerators read a script explaining the ONORC scheme to households in clusters—corresponding approximately to villages in rural areas and towns in urban areas—randomly assigned to the treatment group. The script included information about barriers that migrants might face when attempting to claim ration at a non-designated shop.<sup>2</sup> This information was based on pre-experimental research we conducted into the program’s implementation frictions, including sending migrant mystery shoppers to test the ONORC program and phone surveys with PDS shop owners. We also provided treated households with a toll-free phone number they could call to reach an information hotline which we staffed for five months. Our staff were trained to answer questions about the ONORC program or to find ration shops across 29 states using a database we created of ration shop addresses and phone numbers.

The information we provided immediately increased beliefs about ration portability within district, across district, and across state lines, indicating that many households were unaware of the ONORC program. Four months later, treated households had significantly *lower* beliefs about ration portability compared to control-group households. We show that concurrent awareness campaigns by state governments—which informed households about the ONORC scheme but did not provide the same information about barriers to accessing it—explain the higher beliefs about ration portability in the control group four months after our experiment.<sup>3</sup> To measure the intensity of government awareness campaigns, we exploit additional, out-of-sample data on beliefs about ration portability which we collected both before and after our information experiment. We find that the reduction in perceived portability caused by our experiment was driven by states with more intensive government

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<sup>2</sup>Specifically, the script told households that not all ration shop owners are aware of the ONORC program, that they should bring their unique ID card called *Aadhaar* along with a copy of their household’s ration card—which must be linked to their *Aadhaar*—that the shopkeeper may ask to see additional ID cards, that older versions of ration cards may not be accepted or may require manual adjustment to the ID, that the shop must be equipped with an electronic point-of-sale system.

<sup>3</sup>Under the PDS, state governments are responsible for grain distribution, portability implementation, and publicity and awareness campaigns. See, for example, publications by the Ministry of Consumer Affairs, Food & Public Distribution [here](#) and the Ministry of Information and Broadcasting [here](#).

campaigns.

Our experiment led to a significant reduction in urban emigration over the four months following our intervention. We observe a corresponding increase in emigration to rural destinations, with only a slight—and statistically insignificant—reduction in emigration overall. These treatment impacts are driven by states with more intensive government awareness campaigns. The shift from urban to rural destinations is concentrated among households reporting at baseline that finding food after migrating to a city would be a major challenge, consistent with food security concerns hindering urban migration. The shift from urban to rural destinations is not pronounced among poor households, poor households without access to credit, or households with low assets at baseline. In fact, households reporting that food is a migration barrier are *less* likely to be members of either of these three groups. This implies that programs not directly targeting destination food security—such as cash transfers to poor households—would be unlikely to substitute for food ration portability in insuring these prospective migrants.

Eight months after our intervention, treatment impacts on emigration disappear, suggesting that concerns about ration access deterred short-term—rather than long-term—urban migration. We find small and mixed initial impacts on income and consumption, and modestly positive impacts eight months after the experiment. These findings are suggestive of labor market disruptions in the control group, possibly due to difficulty finding food in urban destinations.

Overall, our results indicate that the risk of food insecurity in the destination acts as a barrier to urban migration in India. We do not find that this barrier operates through credit constraints. Emigration rates in this context are highest among poor households and those experiencing a negative consumption shock, whereas a credit constraints model would predict the opposite pattern. Moreover, migration responses to our experiment are not stronger among poor or credit-constrained households. Rather, our data suggest that households use emigration to cope with negative shocks,<sup>4</sup> and that food ration access in the destination offers insurance against bad migration outcomes. However, although ONORC has in principle been implemented across all of India, reports of transaction failures are

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<sup>4</sup>This finding is consistent with the results in Lagakos et al. (2018).

common, and are pronounced among migrant beneficiaries.<sup>5</sup> Our pre-experimental research confirms that significant implementation frictions remain, and the reduction in perceived portability caused by our experiment suggests that concerns over these frictions were central to households’ beliefs. The corresponding reduction in urban emigration suggests that ration access in the destination influences households’ decisions about where to emigrate.

This paper contributes to the literature studying barriers to internal migration in low-income settings. These barriers distort workers’ location decisions and likely contribute to the large sectoral productivity gaps present in nearly all developing economies (Caselli, 2005, Gollin et al., 2014, Bryan and Morten, 2019). This literature, reviewed by Lagakos (2020), has focused largely on inadequate information (Baseler, 2022), financial constraints (Bryan et al., 2014, Cai, 2020), costs of migrating (Lagakos et al., 2018, Imbert and Papp, 2020, Morten and Oliveira, 2023), cultural differences (Atkin, 2016), and land market frictions (De Janvry et al., 2015). Our results highlight an additional barrier: uninsured consumption risk in the destination. While other studies have suggested that the lack of portability of social protection programs may deter internal migration in India (Kone et al., 2018, Nayyar and Kim, 2018),<sup>6</sup> we are the first to provide causal evidence of this.

We also contribute to the literature studying the interaction between income risk and migration. As emphasized by the classic paper by Harris and Todaro (1970), migrating to search for a job can be risky, as unemployment rates—especially in urban areas—are often high in developing economies. Migration is also a form of insurance, allowing migrating households to diversify their income sources across space (Rosenzweig and Stark, 1989) and acting as a substitute for informal insurance mechanisms (Munshi and Rosenzweig, 2016, Morten, 2019).<sup>7</sup> Social protection programs can reduce migration if they increase the value of remaining at the origin (Imbert and Papp, 2019, 2020). To our knowledge, this paper is the first to study the effects on migration of a social protection program that—in principle—reduces the risk incurred by migrating. While most studies of risk and migration examine

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<sup>5</sup>Common reasons for transaction failures are authentication issues when attempting to use the biometric identification system and stock-outs. See, for example, [this report](#).

<sup>6</sup>The rapid decrease in migration costs in China from 2000–2005, which Tombe and Zhu (2019) show was responsible for a significant share of aggregate labor productivity gains over the same period, may in part reflect increasing portability of social services due to relaxation of *hukou* restrictions, although employment restrictions likely also played a major role.

<sup>7</sup>Migration can also improve informal risk sharing at the origin by increasing the resources available to the village (Meghir et al., 2021).

risk sharing at the origin, we examine the role of insurance provided by social protection programs at the destination.

Our findings also relate to the literature studying the role of information in migration decisions, which focuses mainly on international migration. Several papers have found information gaps between perceived and actual potential earnings abroad (McKenzie et al., 2013, Shrestha, 2020) or between international migrants and their home-country family members (Ambler, 2015, Ashraf et al., 2015, Batista and Narciso, 2016, Seshan and Zubrickas, 2017, Joseph et al., 2018). Experiments attempting to facilitate international migration by providing information about the destination or the migration process have generally not found impacts on migration (Beam, 2016, Beam et al., 2016), although information about the risks *en route* was found to reduce intentions to migrate from The Gambia to Europe (Bah et al., 2022), and information about mortality and wages in the destination affected migration decisions out of Nepal (Shrestha, 2020). In Kenya, Baseler (2022) found that rural households hold downward-biased beliefs about big-city incomes, and that providing information about urban incomes increases migration to the capital city. In this paper, we show that information about access to food ration in the destination affects decisions about whether to migrate to a city.

We also contribute to the literature studying the design of social protection programs in developing countries, recently reviewed by Banerjee et al. (2022). One puzzle in this literature is the low take-up of social programs among eligible households (Bhattacharya et al., 2015, Demirguc-Kunt et al., 2017), which appears to be partly due to hassle and information costs (Carneiro et al., 2018). We also document low take-up rates of the ONORC program, due in part to households' concerns about practical barriers to using the program. Another central focus of this literature is beneficiary targeting. We show, somewhat surprisingly, that PDS access in the destination does not appear to be decision-relevant for households that are poor or credit-constrained by conventional measures. Instead, self-reported concerns about finding food in the destination strongly predict a migration response to our information treatment. While these households would be unlikely to be targeted by cash transfer programs, they would benefit from improving access to the ONORC program, most likely by reducing administrative barriers.

Finally, we contribute to understanding the puzzle of low migration rates in India in the



face of substantial spatial income gaps (Munshi and Rosenzweig, 2016). Informal insurance provided by rural networks appears partly responsible, either because it can substitute for migration as a risk-coping strategy or because migrants lose access to the network (Munshi and Rosenzweig, 2016, Morten, 2019). We offer an additional explanation: that the importance of social protection programs in India—combined with significant administrative barriers, especially across district or state lines—increases the relative value of staying home. Our study also offers a rare look into short-term work migration. Most of the migration literature studies long-term migration—which is captured in population censuses—or uses smaller or more localized surveys (Banerjee and Duflo, 2007, Morten, 2019, Imbert and Papp, 2020). The only other large-scale study of short-term migration in India we are aware of is Imbert and Papp (2019), which uses National Sample Survey (NSS) data from 1999–2000 and 2007–2008, which capture migration spells between two and six or one and six months, respectively. We collected detailed information on all work migration spells in our sample, regardless of duration or destination, and show that short-term migration to nearby locations is the most common form of work migration.

This paper proceeds as follows. Section 2 provides background information about the ONORC scheme and our pre-experimental research into awareness of ONORC and barriers to using it. Section 3 describes our experimental design, including details on sample selection, the information intervention, and our empirical strategy. Section 4 provides a descriptive analysis of emigration patterns in our setting, and Section 5 presents results from our experiment. Section 6 concludes.

## 2 Background

This section summarizes the One Nation, One Ration Card (ONORC) program and describes our pre-experimental research into awareness of ONORC and barriers to its implementation.

### 2.1 India’s Public Distribution System and ONORC Scheme

The largest social protection scheme in India is the Public Distribution System (PDS), through which ration card holders are entitled to quotas of food grain at a subsidized price. The program alone costs 1 percent of GDP and benefits an estimated 152 million individuals

(World Bank, 2018).<sup>8</sup> Because PDS transfers are in-kind, they also protect beneficiaries from price risk.<sup>9</sup>

Until recently, migrants were excluded from the ONORC scheme, as beneficiaries were required to claim ration in a designated PDS shop near their home. Recognizing this constraint to labor mobility, the government of India has recently worked toward greater portability of social welfare schemes. In 2019, it introduced One Nation, One Ration Card (ONORC), a scheme that aims to ensure the portability of ration cards across the country. The initiative, introduced in four states in August 2019, was subsequently rolled out across the entire country. In principle, the ONORC scheme allows households with migrants to alternate claiming ration across months, or to split a given month’s ration. Our data indicate that, as of early 2022, many migrants are using the PDS: in our sample, 46% of migrants had claimed ration at some point in the destination.<sup>10</sup>

## 2.2 Pre-Experimental Research

**Research Into Awareness of Ration Portability.** In January 2021, we launched an exploratory module to assess awareness of and interest in ration portability. The survey was conducted with 28,066 ration card holders.<sup>11</sup> Results from the exploratory module are shown in Appendix B. We found that awareness of portability was very low: in states that had joined ONORC by October 2020, only 28% of households were aware that they could claim ration at any shop other than their local, designated ration shop, as shown in Appendix Figure B1. Only 8% reported that it would be possible to claim ration outside their state.<sup>12</sup> The most common answer given for why they believed their ration was not portable, as shown in Figure B2, was that it was not permitted by the government (about 60% of answers), suggesting

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<sup>8</sup>Gadenne et al. (2021) report that the transfer value of the rice subsidy alone represents 4.9% of the average monthly expenditure for beneficiary households, and find substantial effects of program expansion on households’ caloric intake. As a benchmark, the National Rural Employment Guarantee Scheme (NREGS) made up 1.8% of beneficiaries’ expenditure in Andhra Pradesh (Muralidharan et al., 2017), a state with relatively generous NREGS benefits.

<sup>9</sup>Gadenne et al. (2021) find that in-kind transfers through the PDS improve welfare relative to cash transfers, though Banerjee et al. (2021) find that the introduction of vouchers improved targeting over in-kind transfers in Indonesia.

<sup>10</sup>Choudhury et al. (2020) find that ONORC led existing migrants to stay in the destination longer.

<sup>11</sup>These households were not included in the experimental study.

<sup>12</sup>If we restrict to states that had joined ONORC by the onset of Covid-19 in March 2020, the share of households aware of ration portability rises only slightly to 34% (10% for interstate portability).

that many households are not aware of the ONORC program at all. However, other common answers included that the shop owner would not allow it (about 20% of answers) and that there would be technical issues (about 7% of answers), pointing to existing concerns about implementation frictions. Only a tiny fraction of households reported attempting to use their ration card at a non-designated shop, as shown in Appendix Figure B1: 8% had tried to do so anywhere, and 2% had tried to do so in another state. When asked why they had not tried to claim their ration elsewhere, about 70% of households reported that they either do not claim ration, or are happy claiming from their designated ration shop, as shown in Appendix Figure B3. However, about 19% of households report that they have not tried to claim ration at another shop because they believe their card would not be accepted, or that the government would not allow them to, providing initial evidence that many households would be interested in take-up of the ONORC program.

**Research Into Barriers to Ration Portability.** Before launching our experiment, we gathered information on *de jure* eligibility requirements as well as *de facto* barriers to using the ONORC scheme from Indian government websites and mobile applications, phone surveys of 2,000 ration shop owners in 20 states, and mystery shoppers sent to test the ration system in 8 states. Appendix Table B1 summarizes this research. Surveyed ration shops are representative of the population of ration shops in 11 states, and a convenience sample identified through web searches in 9 states. The ration shop survey included questions on the process of claiming ration for out-of-district and out-of-state migrants. We also collected basic data from 500 migrants living and working in major cities. Additionally, mystery shoppers—who were eligible to claim ration—traveled to the same cities to visit ration shops to test whether they could claim ration with an out-of-state or out-of-district ration card.

Our ration shop owner surveys and mystery shopper activity confirmed at least partly successful implementation of ration portability in these 20 states. However, they also indicate sizable frictions. In our survey of PDS shop owners, most, but not all—74% overall, and 80% in urban areas—reported that migrants can claim ration in their shop as long as their ration card is read by the ePoS machine. In our mystery shopper activity, 50% of transactions were approved. The mystery shoppers visited several PDS shops within each city

(median of 9 per city), and 75% succeeded in claiming ration at least once in a given city. As our mystery shoppers were likely better-equipped than most migrants to successfully claim ration—they were familiar with ration portability policies and carried the proper identification documents—these results point to potential difficulties faced by emigrants attempting to claim ration even when the ONORC scheme is, in principle, in force.<sup>13</sup>

### 3 Experimental Design

We use a cluster-randomized controlled trial to induce random variation in beliefs about ration portability. Our treatment sample is a subset of the survey sample covered by the Centre for Monitoring Indian Economy (CMIE).

#### 3.1 Sample Selection and Data Collection

We restricted our intervention to states satisfying the following criteria: 1) they had adopted ONORC by August 2021, and 2) we were able to confirm through state-level transaction data and either or both of A) ration shop owner surveys and B) mystery shopper surveys that claiming ration with an out-of-district or out-of-state ration card was possible in that state (see Section 2.2). We decided to exclude the remaining states from the treatment sample because we could not directly confirm whether ONORC had been successfully implemented in those states, and the great majority of migration within India occurs within state rather than across states, as shown in Appendix Figure B4.

We conducted our intervention with a subset of the survey sample covered by CMIE, which has collected panel data on approximately 170,000 households across almost all states in India since 2014 (Centre for Monitoring Indian Economy, 2022).<sup>14</sup> Our sample consists of the approximately 62,000 households in the CMIE sample that 1) reside within the 18 states covered by our project, 2) responded to the CMIE 2021 wave 3 survey, which forms our

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<sup>13</sup>A case study of PDS transactions in Chhattisgarh and Jharkhand found similar barriers to PDS access stemming from biometric failures or errors in *Aadhaar* data such as misspelling of names and errors in date of birth, sex, address, or phone number (Panda, 2022).

<sup>14</sup>CMIE’s sampling and survey methodology are described in greater detail in Vyas (2021a,b). Comparisons with other benchmark national representative surveys can be found in Sinha Roy and van der Weide (2022).

baseline survey, and 3) have a ration card.<sup>15</sup> Appendix Figure A1 summarizes our sample selection process.

Our information intervention was embedded into our baseline survey, which ran from October 2021 through December 2021. We collected basic data on perceptions about ration portability immediately after the intervention. Our primary follow-up data were collected from February 2022 through April 2022, approximately four months after the information intervention: we refer to these as our 4-month follow-up survey. We collected a second round of follow-up data, with more limited outcomes, from June through August 2022, which we refer to as our 8-month follow-up survey.

Out of the 62,130 households surveyed at baseline, we successfully surveyed 52,902 (85%) at the 4-month follow up, and 45,351 (73%) at the 8-month follow up. Attrition is not correlated with treatment, as shown in Appendix Table C9.

Shortly after the 4-month follow-up survey, we conducted additional surveys with emigrant household members by phone. We attempted to survey all new emigrants—defined as members who were listed as emigrants in the 4-month survey but were not listed as emigrants in the baseline survey—and a random 10% sample of existing emigrants. Altogether, we successfully surveyed 6,483 out of 8,993, or 72%, of these individuals.<sup>16</sup> Attrition is not differential by treatment status, as shown in Appendix Table C9. We use data on emigrant outcomes gathered from phone surveys with emigrants in place of reports from household surveys when available.

## 3.2 Randomization

We divided our experimental sample into a single treatment group and a control group. Assignment to the treatment group was randomized at the level of CMIE’s primary sampling unit (PSU), which corresponds roughly to a village in rural areas and a town or city in urban areas. Cluster randomization minimizes the possibility of information spillovers from treated

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<sup>15</sup>In the most recently available data, about 87% of households in our sample have a ration card (Bhattacharya and Sinha Roy, 2021). The difference between this figure and the 66% population coverage mandated by the National Food Security Act (NFSA) is likely due to varying definitions of “ration card.” For example, in certain states households may have non-NFSA food ration cards, or non-food ration cards. We chose to use an inclusive definition of “ration card” as a condition of assignment to the experimental sample.

<sup>16</sup>Of these 6,483 individuals surveyed, 1,285—or 20%—had returned to the household by the time of the phone survey.

to control households.

Treatment status was assigned using a stratified permutation method. Strata were formed from the following features: state identifier, an urban dummy, a dummy for clusters with an above-median share of households that had sent an emigrant anywhere since 2017, and a dummy for clusters with an above-median poverty rate (defined as being below the 40th percentile of per-adult-equivalent household consumption). The Stata command *randtreat* was used, assigning misfits using the global method.

### 3.3 Information Intervention

Households in our treatment group were read a script during the baseline survey. The script included basic information about the ONORC scheme, including that their ration allowance is portable across district and state lines; information on which states had joined the ONORC program by the time of the survey; information on the documents required to claim ration outside of their designated PDS shop; several caveats about practical barriers to using ONORC (see footnote 2 for a list); and information on government offices responsible for resolving ration access issues and the phone number of a government helpline dedicated to ration portability. The script was read by an enumerator, and an information sheet was left with the respondent. In cases where respondents had clarifying or follow-up questions, enumerators were instructed to answer them if possible or to direct the respondent to our information hotline if not. All respondents were provided access to an information hotline which they could call to obtain local information (phone numbers and/or addresses) on ration shops across 29 states. We processed and uploaded individual ration shop information to Mapbox and Google Earth so that hotline staff could search for ration shops within or near the caller's locality of interest. The hotline was staffed by research assistants, who could also answer basic questions about ration portability. Hotline access lasted for 5 months after the beginning of the intervention (from October 2021 to March 2022). Our staff reported receiving 390 total calls over that period. Households in the control group did not receive the script, sheet, or hotline access. The information script, information sheet, and a description of the information provided through the hotline are available in Appendix A.

### 3.4 Outcomes and Estimating Equations

This section explains how we construct outcome variables and estimate treatment impacts. Additional details are available in our pre-analysis plan [here](#).

**Households and Emigrants.** Throughout this paper, we use the CMIE definitions of a household, an emigrant, and an immigrant. A household is composed of “individuals who are usually residents of the household at the time of the survey interview and have been residents of the household over the four months preceding the date of the interview.” An emigrant is “a person who was a member of the household in the previous interview [but] no longer resides as a member of the household and is not deceased” and whose reason for emigration is not “Shifted to in-laws/new residence after marriage.” After emigrating, individuals remain emigrants until they immigrate back to the household. Immigrants are “those who have migrated into the household since the last CPHS interview with the household” (CMIE Member Roster). We use the term *baseline emigrants* to refer to individuals listed as emigrants at the time of intervention and *new emigrants* to denote household members who emigrated after the intervention. We capture short-term emigration with the question “Since our last visit, have any members of your household migrated for work or in search of work and returned to the household?” and collect responses at the individual level. We use the term *family* to refer to the union of household members, immigrants, and emigrants. Our primary outcome of interest is the number of emigrants each family has, including current emigrants and returned (short-term) emigrants who left after the beginning of the experiment.<sup>17</sup>

**Portability Beliefs.** To measure perceived ration portability, we asked households a series of Yes/No questions about whether they can use their ration card at a shop other than their designated shop, outside their home district, and outside their home state.<sup>18</sup> If a person

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<sup>17</sup>As described in our pre-analysis plan, we also separately analyze emigration effects among a set of households we identified as being likely to send emigrants in response to the information we provided. These households satisfied the following criteria: they had used their ration card within the month preceding the baseline survey, they were in the bottom 40% of per-adult-equivalent consumption in our sample, and they had at least one male household member living at the origin aged 18–45 at baseline.

<sup>18</sup>We began the baseline survey with a single, multiple-choice question, “Which fair price shops are you eligible to claim your ration from?” and instructed enumerators not to read the options aloud (“fair price shop” refers to ration shops). Partway through the survey, we became aware that some respondents understood the question to be asking where they actually claim ration. We therefore switched to the series of Yes/No questions described above, and estimate impacts on perceived portability at baseline using the

answers “No” to one of these questions, we code the following questions as “No.”

**Income and Consumption.** We measure impacts on family income by adding monthly individual wage income for all household members, agricultural self-production, and household business profit (averaged over the preceding four months) to wage, salary, casual labor earnings, and business profits earned by emigrants over the preceding month. Total consumption and food consumption are measured through expenditure questions, adding the average monthly value (over the preceding four months) of household expenditure to the preceding month’s value of each emigrant’s expenditure.<sup>19</sup> We also assess impacts on the occupational income score of Acemoglu and Autor (2011), a subjective financial well-being indicator denoting whether the household’s finances have improved from the prior year, and remittances received by the household over the previous month. We assess impacts among baseline emigrants on ration claiming, food security (whether they had skipped meals recently), and job search efforts (number of hours per week spent looking for a job). Finally, we combine all our income, consumption, subjective well-being, and food security measures into an index of economic well-being following the methodology of Anderson (2008).

**Estimating Equations.** We measure intent-to-treat effects on perceived ration portability and emigration using the following specification:<sup>20</sup>

$$y_{it} = \beta T_i + \gamma y_{i0} + \eta X_i + \theta_t + \alpha_i + \epsilon_{it} \quad (1)$$

where  $y_{it}$  is an outcome for family  $i$  measured at time  $t$  with  $t = 0$  corresponding to baseline (pre-treatment) values,  $T_i$  is a treatment assignment dummy,  $X_i$  is a vector of baseline

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new questions. Combining the questions produces estimates that are smaller, but still significantly different from zero. In follow-up surveys, we use the series of Yes/No questions only.

<sup>19</sup>Because emigrants may join new households in the destination, we ask about emigrants’ total household earnings and then divide that report by the number of adult-equivalents represented in that expenditure, which is assessed directly through survey questions.

<sup>20</sup>As described in our pre-analysis plan (Baseler et al., 2022), we had originally planned to estimate treatment impacts using ANCOVA regression as described in McKenzie (2012). However, the sign of our treatment impact on portability beliefs changed over time due to concurrent government awareness campaigns (see Section 5.2), making ANCOVA estimates less interpretable. We therefore focus on treatment impacts estimated separately by survey wave. Our main analysis uses unweighted results to produce internally valid estimates. Weighted results, which estimate average treatment impacts for the population of Indian households across the 18 states in our sample, are similar, though slightly noisier. Appendix C presents the full set of pre-specified analysis, including ANCOVA estimates and weighted results.



controls chosen through double lasso,<sup>21</sup>  $\theta_t$  is a survey-round fixed effect,  $\alpha_i$  is a randomization-stratum fixed effect, and  $\epsilon_{it}$  is an error term.<sup>22</sup> Standard errors are clustered at the primary sampling unit level, corresponding to the unit of treatment randomization.

**Pre-Analysis Plan.** This study was pre-registered in the AEA RCT Registry (Baseler et al., 2022), and the pre-analysis plan can be found [here](#). The main text of this paper presents a subset of pre-specified results together with new analysis. The full set of pre-specified results is presented in Appendix C, including sharpened  $q$ -values computed within three outcome domains—emigration, economic well-being, and heterogeneous treatment impacts—to control the false discovery rate, following the methodology described in Anderson (2008).

### 3.5 Summary Statistics and Balance

Summary statistics for our sample, and tests of randomization balance, are shown in Table 1. The average household has about 3.7 members and earns \$257 per month. Forty-one percent of adults (aged 18 or over) list their status as employed at the time of the baseline survey. The highest-educated person in the average household has about 11 years of education. Nearly every adult is literate.

Fifty-six percent of households have had an emigrant at some point since 2014 (including all forms of emigration), and 33% have an emigrant (excluding emigrants for marriage) at the time of the baseline survey. About two-thirds (0.61/0.94) of these emigrants are in cities.

Randomization appears to have successfully created balanced groups, as shown in Table 1. Across 15 baseline variables summarizing demographic, migration experience, and economic outcomes, only one is statistically significantly different at the 10% level—and none is statistically significantly different at the 5% level—in the treatment group compared to the control group, similar to expectation under balanced groups.

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<sup>21</sup>We estimate post-double-lasso coefficients using the Stata command *pdslasso*, and include in the lasso all possible controls from the baseline survey. We convert categorical variables to a set of dummies, and compute averages over family members for variables defined at the individual level.

<sup>22</sup>Relative to the estimating equation specified in our pre-analysis plan, Equation 1 omits the variable  $M_{i0}$ , indicating missing values of  $y_{i0}$ . This is because we have no missing values for  $y_{i0}$ .

Table 1: Baseline Sample Statistics and Randomization Balance

Variable	(1) Control Mean/(SD)	(2) Treatment Mean/(SD)	(1)–(2) Adjusted t-test <i>p</i> -value
Highest Education (Years)	11.32 (18.31)	11.18 (16.99)	0.82
Household Size	3.72 (8.44)	3.77 (7.99)	0.76
# of Adult-Equivalents	3.51 (7.53)	3.55 (7.14)	0.80
OBC/ST/SC Castes, % of Household	0.67 (2.59)	0.68 (2.15)	0.74
Literacy, % of Adults	0.99 (0.17)	0.99 (0.38)	0.12
Ever Migrated	0.56 (1.98)	0.56 (1.92)	0.94
Any Current Migrant	0.32 (1.83)	0.33 (1.87)	0.92
# of Current Migrants	0.94 (6.25)	0.91 (5.82)	0.33
# of Current Urban Migrants	0.63 (6.83)	0.56 (6.23)	0.12
# of Current Inter-State Migrants	0.02 (0.51)	0.03 (1.52)	0.08*
Household Income (USD/Month)	261 (1499)	252 (1176)	0.75
Total Consumption (USD/Month)	158 (579)	157 (615)	0.59
Food Consumption (USD/Month)	45 (146)	45 (167)	0.12
Employment, % of Adults	0.40 (0.90)	0.41 (0.80)	0.80
Has a Bank Account	1.00 (0.01)	1.00 (0.01)	0.38
Observations	31,456	30,674	62,130
Clusters	848	892	1,740

First two columns show means within control and treatment households, respectively. Third column shows *p*-values from a two-sided t-test of equivalence of means, controlling for a randomization-stratum fixed effect and clustering standard errors at the village/town (primary sampling unit) level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

## 4 Descriptive Analysis of Emigration Patterns

Table 2 summarizes emigration patterns during our study period using data from our baseline and follow-up surveys. Among household members—that is, non-emigrants—in the survey wave covering September–December 2021 (our baseline survey), 2% emigrate at some point over the following four months, and 3.1% emigrate at some point over the following eight months. Among those who emigrated over the following four months, 32% go to urban areas.<sup>23</sup> Most of these migrants do not travel far: only 32% cross district lines, and only 9% cross state lines. Many also do not stay away for long: more than half of those who emigrated over the following four months have returned home during that four-month period. Only 39% of these emigrants are still away eight months later. Compared to statistics from the NSS 2007–2008, as reported by Imbert and Papp (2019), the overall migration numbers are qualitatively similar, although our data show a much higher share of within-state migration. This is possibly due to nearby, within-state trips of under one month—which the NSS does not capture—though it may also reflect changing migration patterns over time.

Table 2: Migration Patterns During Our Study Period

	% of Individuals	N
<b>Among Household Members at Baseline:</b>		
Emigrated Over Following 4 Months	0.020	98,030
Emigrated Over Following 8 Months	0.031	106,763
<b>Among 4-Month Emigrants:</b>		
Emigrated to Urban Area	0.32	1,791
Emigrated to Different District	0.32	1,788
Emigrated to Different State	0.09	1,791
Still Emigrated 4 Months Later	0.46	1,791
Still Emigrated 8 Months Later	0.39	1,246

Data from baseline surveys. Sample includes individuals ages 18–45 living in the household as of our baseline survey. Emigration excludes marriage emigration and includes short-term emigrants (who leave and return to the household within the same 4-month period). Estimates are weighted to account for sampling methodology and non-response.

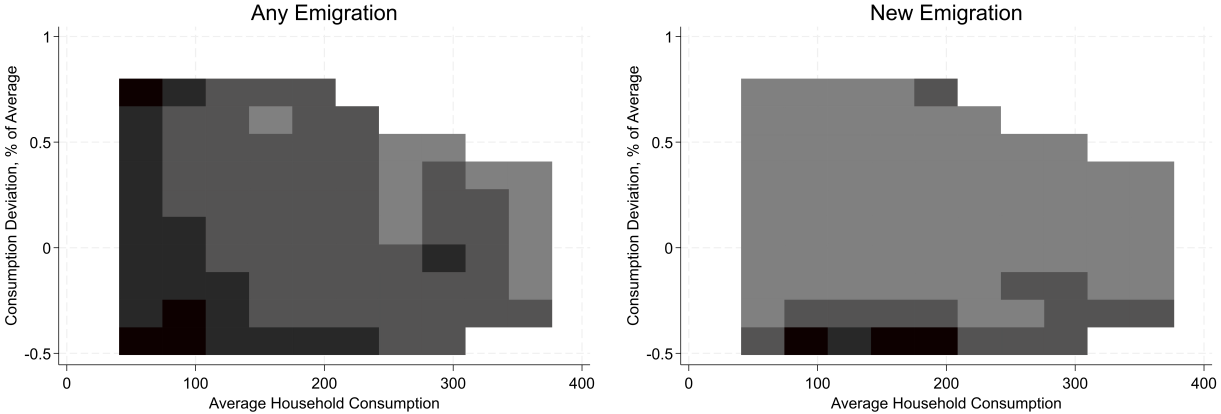
Emigrants tend to come from households with low levels of average consumption—defined over the year preceding the baseline survey—as shown in Figure 1. They also tend to come

<sup>23</sup>The lower urban share among new emigrants, shown in Table 2, compared to the higher urban share among all emigrants, shown in Table 1, suggests that urban migration episodes are longer than rural ones.

from households experiencing a negative consumption shock, defined as the percent deviation of baseline consumption from its average over the previous year. The role of consumption shocks in driving new emigration is even more apparent, as shown in the right panel of Figure 1: new emigrants come almost entirely from households experiencing large negative shocks, representing 25%–50% of average consumption levels.

These patterns provide *prima facie* evidence that credit constraints are not the predominant barrier to migration in this setting: a credit-constraint model would predict higher emigration rates among richer households, or households experiencing positive shocks. Rather, these patterns suggest that households use emigration to cope with negative shocks, similar to the findings of Lagakos et al. (2018). Because new emigrants are coming largely from households experiencing negative consumption shocks, the value of insurance against poor outcomes in the destination is likely to be high.

Figure 1: Emigration Rates Are Highest Among Poor Households With Recent Negative Consumption Shocks.



Data from baseline surveys. Each rectangle shows the average emigration rate among households in a given cell. Darker colors indicate higher emigration rates. Horizontal axis shows average monthly household consumption over the year preceding the baseline survey. Vertical axis shows the consumption deviation from that average, expressed as a share of the average, at baseline. New emigration is defined as emigration among individuals who were household members in the previous survey wave. Excludes marriage emigration. Estimates are weighted to account for sampling methodology and non-response.

## 5 Experimental Results

This section presents estimated treatment impacts on beliefs about ration portability, emigration, and economic outcomes in the family.

### 5.1 Impacts on Beliefs About Ration Portability

The information we provided significantly and immediately increased households' beliefs about ration portability, consistent with limited awareness of the ONORC scheme at the time of the experiment. However, four months later, the information we provided *lowered* households' beliefs about portability. We discuss the reasons for this reversal in detail in Section 5.2.

We assess immediate changes in households' beliefs about ration portability with survey questions asked after the information was provided.<sup>24</sup> Treatment immediately increases beliefs about ration portability within district, across districts, and across states, as shown in Table 3. In the control group, 35% of households believed they could use their ration card in at least one ration shop other than their designated shop. Only 26% of control-group households believed they could use their ration card in a different district, and 20% believed they could use it in a different state. These shares rise by 21–23 percentage points (pp.) after we provide information about the ONORC scheme ( $p$ -values $<0.01$ ). These impacts correspond to a 67% increase in perceived portability overall, and a doubling of perceived portability across state lines. Treatment did not bring perceived portability up to 100%, providing an early indication that many households already had concerns about barriers to PDS access.

Four months later, the treatment impact on perceived portability is negative across all three measures. Treatment-group households were 8 pp. less likely to believe they could use their ration card in at least one other location ( $p$ -val = 0.01). Impacts on inter-district and inter-state portability are similar. Important to note is that beliefs in the control group were substantially higher in the 4-month follow up, with the share believing their ration is portable somewhere rising from 35% to 55%. We investigate the cause of this rapid increase

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<sup>24</sup>Treatment-group households were asked questions about portability before and after the information. Control-group households were only asked once.

in beliefs in the control group, without a commensurate increase in the treatment group, in the following subsection.

Table 3: Treatment Impacts on Portability Beliefs

	(1)	(2)	(3)
	Believes Their Ration is Portable:		
	Somewhere	Across Districts	Across States
<b>Immediate Impacts</b>			
Treatment	0.234*** (0.029) [0.00]	0.207*** (0.028) [0.00]	0.212*** (0.027) [0.00]
Outcome Mean in Control	0.35	0.26	0.20
Observations	36,776	36,776	36,776
<b>4-Month Impacts</b>			
Treatment	-0.079*** (0.029) [0.01]	-0.085*** (0.030) [0.00]	-0.055** (0.028) [0.05]
Outcome Mean in Control	0.55	0.43	0.37
Observations	48,297	48,297	48,297
<b>4-Month Impacts, by Campaign Intensity</b>			
Treatment	-0.118*** (0.040) [0.00]	-0.108** (0.044) [0.02]	-0.093** (0.044) [0.03]
Treatment $\times$ Low-Campaign State	0.088 (0.057) [0.12]	0.050 (0.058) [0.39]	0.083 (0.052) [0.11]
<i>p</i> -val: Treatment in Low-Campaign States = 0	0.47	0.13	0.74
Observations	48,297	48,297	48,297

An observation is a family (household + emigrants). Immediate impacts measured during the baseline survey, immediately after the information was given. *Low-Campaign State* is a dummy equal to 1 if change in beliefs about portability in that state (outside our sample) is below the sample median, as described in Section 5.2. All regressions include a randomization-stratum fixed effect and controls chosen through lasso regression. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided *p*-values in brackets. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

## 5.2 Why Did Information Lower Beliefs About Portability?

The rapid increase in control-group beliefs about ration portability between the information intervention and the 4-month follow-up survey is surprising given the stability of beliefs over the year preceding our experiment. In our pre-experimental research undertaken in January 2021, we found that 34% of households in states that had joined the ONORC scheme prior to March 2020 (the onset of Covid-19 in India) believed their ration was portable. That share is very similar to the 35% of control-group households that believed their ration was portable immediately prior to the information intervention nearly one year later. The change in control-group beliefs after the experiment is unlikely to be explained by spillovers from treated to untreated households, as 1) treatment was assigned at the level of large clusters of households—roughly corresponding to villages or towns—meaning that any spillovers would need to occur across rather than within clusters, and 2) beliefs in the control group do not simply approach, but exceed, beliefs in the treatment group.

Instead, increasing awareness of the ONORC scheme over this period can be explained by concurrent government awareness campaigns. Indian states are responsible for raising public awareness of the ONORC scheme (see footnote 3), and speeches and press releases by the Ministry of Consumer Affairs in 2022 confirm that a “vigorous awareness generation campaign” was undertaken through “Community Radio stations, displaying audio visual spots at railway stations, banners, posters at outdoors and Fair Price Shops” (Ministry of Consumer Affairs, 2022a,b).<sup>25</sup> Google Trends analysis of searches for “Mera Ration”—the official mobile application created to help migrants find ration shops—shows a 60% increase in average search activity during the period of our 4-month follow-up survey compared to our baseline survey, as shown in Figure 2. To the best of our knowledge, state awareness campaigns did not include information about barriers to portability beyond basic eligibility requirements, whereas our information script provided detailed information on both eligibility requirements and *de facto* barriers to access (see footnote 2 for a description).<sup>26</sup>

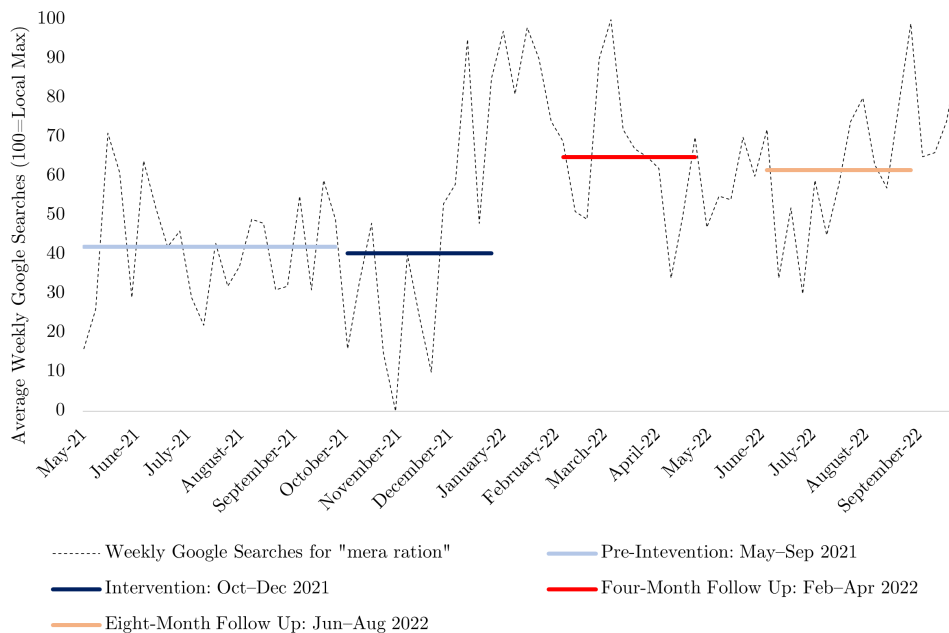
The reversal of the treatment impact on perceived ration portability can thus be ex-

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<sup>25</sup>A description of an awareness campaign in Uttar Pradesh can be found [here](#). “Fair price shops” refer to ration shops.

<sup>26</sup>The awareness campaign undertaken by the NGO MicroSave India Foundation, described [here](#), does not describe any *de facto* barriers to access. We expect awareness campaigns undertaken by state governments to be, if anything, less informative about *de facto* barriers compared to MicroSave’s campaign.

Figure 2: Google Trends Data Show Increased Search Activity for Ration Portability Following Our Intervention.



Horizontal bars show averages within the pre-intervention period and each survey period. Source: Google Trends search activity within India from May 2021 to September 2022. Search activity is normalized so that the maximum within the period shown is 100.

plained by a change in the information environment. At baseline, few households knew about the ONORC scheme, so the treatment impact on awareness of ONORC dominated the impact on concerns about barriers to access. Four months later, when many households had learned about ONORC through state-led campaigns, the treatment impact on concerns about barriers dominated the impact on awareness.<sup>27</sup>

**Distinguishing Between Possible Explanations.** To test whether the observed change in control-group beliefs reflects an increase in awareness of ONORC—as opposed to an artifact of our experiment, such as spillovers from treated to control units—we gathered data from outside our experimental sample on beliefs about ration portability in May 2022, immediately after our 4-month follow-up survey. Because our study launched partway through

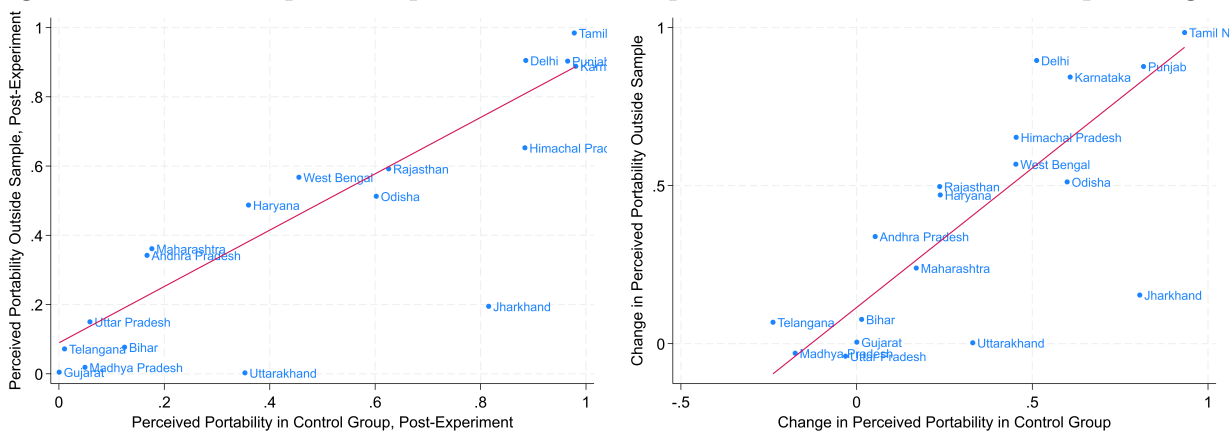
<sup>27</sup>Prior to our experiment, we were not aware of any plans to ramp up government awareness campaigns, so the following analysis was not pre-specified. The earliest official press release mentioning the ONORC scheme we are aware of is from August 2021 (Ministry of Consumer Affairs, 2021), around the same time our experiment launched.



one of the data firm’s survey waves, part of their sample was never exposed to our baseline survey or information intervention.<sup>28</sup> A similar rise in beliefs about portability outside of our experimental sample, compared to our control group, would help confirm a secular increase in awareness of the ONORC scheme, and rule out alternative explanations, such as errors by enumerators in adhering to experimental protocols.<sup>29</sup>

We find that beliefs outside of our experimental sample mirror those in our control group, both in levels and in changes. As shown in Figure 3, there is a tight correspondence between the out-of-sample data and our control group in both levels and changes at the state level. The  $R^2$  statistics from regressions of state-level beliefs, and changes in beliefs, are 0.84 and 0.78 respectively. The two clear outlier states—Jharkhand and Uttarakhand—are the 5th and 2nd smallest states in our sample, and so should have little influence on our overall results.

Figure 3: Out-of-Sample Perceptions Data Correspond Well With Control Group Changes.



Each dot shows the share of households reporting that they can claim ration outside their home state (in the left panel) or the change in that share from before to after our experiment (in the right panel). Red lines show OLS regression estimates weighted by the number of treatment-sample observations. Pre-experimental data collected in January 2021 outside sample, and from October–December 2021 within sample. Post-experimental data collected in May 2022 outside sample, and from June–August 2022 within sample. Averages estimated using sampling and non-response weights.

<sup>28</sup>The CMIE sample is designed so that the probability of being surveyed within each month is proportional to population size (Vyas, 2021b).

<sup>29</sup>Note that the significant treatment impacts observed during the baseline survey also indicate that the experiment was carried out properly.

**Heterogeneous Treatment Impacts by Government Campaign Intensity.** As shown in Figure 3, some states experienced small or no changes in beliefs about ration portability, while others experienced substantial increases. We use the median out-of-sample change in beliefs to divide our sample into households residing in states with a high or low change in beliefs. The median change is 15 pp., and produces a stark divide between our two groups of states: the average out-of-sample change in beliefs in above-median-change states is 52 pp., compared to 2 pp. in below-median-change states. We refer to these two groups as *high-campaign* and *low-campaign* states respectively. We then estimate heterogeneous treatment impacts based on the intensity of government awareness campaigns by modifying Equation 1 to include an interaction between our treatment indicator  $T_i$  and an indicator for whether the household resides in a low-campaign state.

We find that the negative treatment impacts on beliefs about ration portability are entirely driven by high-campaign states, as shown in Table 3. In high-campaign states, treatment reduces overall perceived portability by 12 pp. ( $p\text{-val} < 0.01$ ). Impacts in high-campaign states on perceived inter-district and inter-state portability are similar. Treatment impacts in low-campaign states are small and statistically indistinguishable from zero. The lack of significant treatment effects within low-campaign states is consistent with either the treatment group’s forgetting the information over time, or with some catch-up by the control group due to learning, or both.

### 5.3 Impacts on Migration

Our information treatment led households to decrease urban emigration and increase rural emigration, with little change in the rate of emigration overall, as shown in Table 4. As of the 4-month follow-up survey, treatment-group households had sent 0.06 fewer emigrants to urban destinations (on a base of 0.61,  $p\text{-val} < 0.01$ ) and 0.05 more emigrants to rural destinations (on a base of 0.23,  $p\text{-val} = 0.02$ ).<sup>30</sup> This suggests that beliefs about ration portability do not impact the decision about *whether* to migrate, but do impact whether emigrants choose urban or rural destinations. As households in urban areas do not typically grow their own food, they must find stable employment to meet their food needs, exposing

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<sup>30</sup>The  $q$ -values adjusted for false discovery rate are 0.04 and 0.05 for these outcomes respectively, as shown in Table C4.

them to food insecurity risk (IFPRI, 2017). The shift away from urban destinations is thus consistent with PDS access being an important consideration for prospective urban migrants.

We find no significant treatment effects on other measures of emigration behavior, including planned emigration, new emigration, and cross-district or cross-state emigration, as shown in Appendix Table C4. There is little heterogeneity in treatment impacts on total emigration based on several poverty indicators or prior awareness, as shown in Appendix Table C6.

The shift from urban to rural destinations is driven entirely by high-campaign states. In those states, the number of urban emigrants decreases by 0.08 ( $p\text{-val} < 0.01$ ) and the number of rural emigrants increases by 0.06 ( $p\text{-val} = 0.01$ ), amounting again to a small and insignificant decrease in emigration overall by 0.01 ( $p\text{-val} = 0.57$ ). Treatment impacts in low-campaign states are small and statistically indistinguishable from zero.

As of the 8-month follow-up survey, treatment impacts on rural and urban emigration have disappeared. This suggests that control-group emigrants who would have chosen rural destinations over urban destinations if they had received information about barriers to ration portability—that is, compliers—did not remain in the city for long. This could reflect intentions at the outset to migrate for a short period, or discouragement after arriving in the destination. We return to this question in our discussion of impacts on economic outcomes, in Section 5.5.

Overall, these findings suggest that beliefs about PDS access affect emigration decisions for some Indian families. Treatment, which made households more aware of barriers to accessing ration outside their designated shop, reduced emigration to cities. In the next section, we confirm this interpretation by examining which households changed their emigration decisions in response to the information we provided.

Table 4: Treatment Impacts on Emigration

	(1) # of Emigrants	(2) # of Urban Emigrants	(3) # of Rural Emigrants
<b>4-Month Impacts</b>			
Treatment	-0.010 (0.022) [0.65]	-0.060*** (0.022) [0.01]	0.047** (0.020) [0.02]
Outcome Mean in Control	0.87	0.61	0.23
Observations	52,902	52,902	52,902
<b>4-Month Impacts, by Campaign Intensity</b>			
Treatment	-0.012 (0.021) [0.57]	-0.075*** (0.023) [0.00]	0.060*** (0.021) [0.01]
Treatment $\times$ Low-Campaign State	0.005 (0.047) [0.92]	0.033 (0.047) [0.48]	-0.030 (0.041) [0.47]
<i>p</i> -val: Treatment in Low-Campaign States = 0	0.86	0.30	0.39
Observations	52,902	52,902	52,902
<b>8-Month Impacts</b>			
Treatment	-0.007 (0.021) [0.73]	0.004 (0.025) [0.87]	0.008 (0.019) [0.68]
Outcome Mean in Control	0.99	0.68	0.26
Observations	45,351	45,351	45,351

An observation is a family (household + emigrants). *Low-Campaign State* is a dummy equal to 1 if change in beliefs about portability in that state (outside our sample) is below the sample median, as described in Section 5.2. Excludes emigration for marriage. All regressions include a randomization-stratum fixed effect and controls chosen through lasso regression. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided *p*-values in brackets. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

## 5.4 Mechanisms Behind the Shift from Urban to Rural Migration

Why did the information we provided lead some migrants to choose rural over urban destinations? The negative treatment impact on beliefs about ration portability four months after our intervention suggests that concerns about access to food led migrants to avoid cities. However, other explanations are possible. For example, learning about barriers to PDS access in the destination may exacerbate credit constraints because food in the destination would need to be purchased at market price.<sup>31</sup> Or, the information shared may have simply acted as a nudge, possibly by drawing attention to difficulties faced by migrants. Relatedly, a difference in the mode of our information treatment compared to the government’s—in person and through broadcast, respectively—could in principle be responsible for a treatment impact on migration if, for example, in-person discussions about migration have a tendency to reinforce existing concerns about urban migration, even if those concerns are not related to food security.<sup>32</sup>

To distinguish between these explanations, we estimate heterogeneous treatment impacts along five pre-specified dimensions. Because we find no significant impacts on total emigration, we analyze destination switching, that is, migrating to a rural over an urban destination. To do so, we restrict our sample to all emigrants as of the 4-month follow-up survey—since treatment impacts disappeared by the 8-month survey—and estimate (1) interacting a treatment dummy variable with one dimension of heterogeneity at a time. With this sample restriction, destination switching is well-defined: we code urban emigrants as 1 and rural emigrants as 0. However, such a sample restriction can introduce selection bias if the decision to emigrate at all is an outcome of treatment. Two pieces of evidence suggest that the magnitude of this bias is likely to be small. First, treatment did not impact the number of emigrants, as shown in Table 4. Second, treatment did not impact selection into emigration based on several baseline characteristics—including age, education, and emigration experience—as shown in Appendix Table C1.

We find that urban-to-rural destination switching in response to our experiment was sig-

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<sup>31</sup>Note, however, that this explanation is *ex-ante* unlikely given the emigration patterns discussed in Section 4: namely, that emigration rates are highest among poor households and those experiencing negative consumption shocks.

<sup>32</sup>Such an effect would be inconsistent with the findings of Baseler (2022), who finds that in-person delivery of information about migration increases urban migration.

nificantly greater among households reporting at baseline that finding food in the destination would be a challenge (by 12 pp.,  $p$ -val  $< 0.01$ ), as shown in Table 5. This finding is consistent with concerns about food security in the destination—as opposed to other migration concerns—driving our results. We also find stronger impacts among households unaware of any ration portability at baseline (by 7.5 pp.,  $p$ -val = 0.02), suggesting that it was these households who would have traveled to urban destinations due to government awareness campaigns if they had not received our information.

We do not find significantly different treatment impacts among poor households, poor households without access to credit, or households with low assets at baseline, indicating that credit constraints are not driving our results. A possible explanation for this is that households reporting that food is a migration barrier are *less* likely to be members of any of these three groups, as shown in Appendix Table C2, and that credit constraints are not the predominant barrier to migration in this setting. This finding implies that programs targeting other potential barriers to migration—such as cash transfers to poor households—would be unlikely to substitute for PDS access for food-concerned households.

Table 5: Heterogeneous Impacts on Emigrants’ Destinations

Outcome:	(1)	(2)	(3)	(4)	(5)
Urban Emigrant	Food Is a Migration Barrier	Unaware of Any Ration Portability	Poor Households	Poor Households (No Credit)	Low-Wealth Households
Treatment $\times X$	-0.116*** (0.041) [0.00]	-0.075** (0.032) [0.02]	-0.003 (0.031) [0.93]	-0.010 (0.036) [0.78]	0.006 (0.029) [0.84]
Treatment	-0.046** (0.019) [0.02]	-0.012 (0.024) [0.62]	-0.064*** (0.020) [0.00]	-0.061*** (0.021) [0.00]	-0.067*** (0.024) [0.01]
$X$	0.021 (0.019) [0.26]	0.044** (0.020) [0.03]	0.007 (0.016) [0.66]	0.031 (0.019) [0.10]	-0.046*** (0.017) [0.01]
$q$ -Value: Treatment $\times X = 0$	0.02	0.04	1.00	1.00	1.00
Observations	45,893	45,893	45,893	45,893	45,893

Sample includes all emigrants (excluding international emigrants) as of 4-month follow-up. Column titles show the dimension of heterogeneity,  $X$ , analyzed in that column. All heterogeneity dimensions measured at baseline. All regressions include a randomization-stratum fixed effect. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided  $p$ -values in brackets. Sharpened  $q$ -values computed within a domain that includes all five pre-specified heterogeneity dimensions. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

## 5.5 Impacts on Economic Outcomes in the Family

We find few significant changes in economic outcomes for treated families, as shown in Table 6. Average treatment impacts on income, consumption, and food consumption at 4 months are close to, and statistically indistinguishable from, zero. In high-campaign states, where treatment led to a shift from urban to rural destinations, total income is about 7% lower among treatment-group households at 4 months, consistent with positive income gaps between urban and rural areas. However, total consumption and food consumption were essentially unaffected, even in high-campaign states (effect sizes  $\approx 1\%$ ). At the 8-month survey, the average treatment impact on income is positive (5% increase,  $p$ -val = 0.08), and average impacts on consumption are small and positive. Pooled impacts on total income and consumption are small and positive, and impacts on food consumption are modestly positive (3% increase,  $p$ -val = 0.06), as shown in Appendix Table C5. We find no significant treatment effects on other economic outcomes, including ration claiming and food security among baseline emigrants, as shown in Table C5.

That the shift away from urban and toward rural migration in the treatment group is not accompanied by a decrease in consumption indicates that the compliers in this study—those who would have avoided urban destinations if they had more information about barriers to ration portability—do not benefit in consumption terms from urban migration. In light of the large urban-rural consumption gaps in India, this suggests either that the returns to urban (compared to rural) migration are low for this group, or that barriers to ration access impeded labor market outcomes once these emigrants arrived in the destination. While we cannot definitively distinguish between these two explanations, the modest positive treatment impacts on income and consumption as of the 8-month survey are suggestive of labor market disruptions in the control group, possibly due to difficulty finding food in urban destinations.

Table 6: Treatment Impacts on Income, Consumption, and Remittances

	(1) Total Income	(2) Total Consumption	(3) Food Consumption	(4) Remittances
<b>4-Month Impacts</b>				
Treatment	-0.006 (0.027) [0.82]	-0.004 (0.013) [0.77]	0.020 (0.016) [0.22]	0.012 (0.015) [0.40]
Outcome Mean in Control	279	169	50	6
Observations	52,902	52,902	52,902	52,902
<b>4-Month Impacts, by Campaign Intensity</b>				
Treatment	-0.074** (0.033) [0.02]	-0.007 (0.016) [0.65]	0.012 (0.021) [0.56]	-0.004 (0.013) [0.77]
Treatment $\times$ Low-Campaign State	0.088** (0.040) [0.03]	0.027 (0.024) [0.26]	0.023 (0.031) [0.46]	-0.004 (0.018) [0.81]
<i>p</i> -val: Treatment in Low-Campaign States = 0	0.53	0.27	0.13	0.52
Observations	52,902	52,902	52,902	52,902
<b>8-Month Impacts</b>				
Treatment	0.048* (0.027) [0.08]	0.011 (0.015) [0.45]	0.025 (0.017) [0.14]	0.023 (0.020) [0.25]
Outcome Mean in Control	257	170	48	6
Observations	45,351	45,351	45,351	45,351

An observation is a family (household + emigrants). *Low-Campaign State* is a dummy equal to 1 if change in beliefs about portability in that state (outside our sample) is below the sample median, as described in Section 5.2. Monetary values are measured monthly, converted to USD, and transformed using the inverse hyperbolic sine function. All regressions include a randomization-stratum fixed effect and controls chosen through lasso regression. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided *p*-values in brackets. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .



## 6 Discussion

This paper presents findings from a large, cluster-randomized controlled trial implemented across 18 Indian states. Motivated by evidence of low awareness of the ONORC scheme permitting food ration portability across India, we informed a random subset of around 62,000 households about that scheme. While our information intervention caused an immediate increase in perceived ration portability, this treatment impact turned significant and negative four months after the experiment took place, owing to a substantial increase in perceived portability within the control group. We show that this can be explained by concurrent state government campaigns that made households aware of the ONORC program but not the barriers migrants face when attempting to use it. Our experiment led emigrants to choose rural over urban destinations, with little change in the overall emigration rate, and this impact faded 8 months later.

We find that switching from an urban to a rural destination is driven by concerns about destination food security. We do not find that credit constraints can explain our results, as treatment impacts do not differ by income, access to borrowing, or wealth. This finding implies that targeted cash transfers to the poor would not substitute for access to PDS in alleviating the food security concerns that prevent households from migrating to urban areas.

Altogether, our findings indicate that food insecurity in the destination acts as a barrier to urban migration in India. Because migration and job search—especially to urban areas—is costly and risky, prospective emigrants may be deterred if they cannot insure themselves against consumption risk. The PDS has the potential to partly alleviate these barriers by guaranteeing beneficiaries’ access to food ration once they arrive in the destination, but prospective migrants must be reasonably confident that *de jure* ration portability will function in practice. Our results suggest that concerns about ONORC implementation frictions are holding some prospective urban emigrants back. These findings highlight the importance of reliable access to food ration outside of PDS beneficiaries’ designated ration shops.

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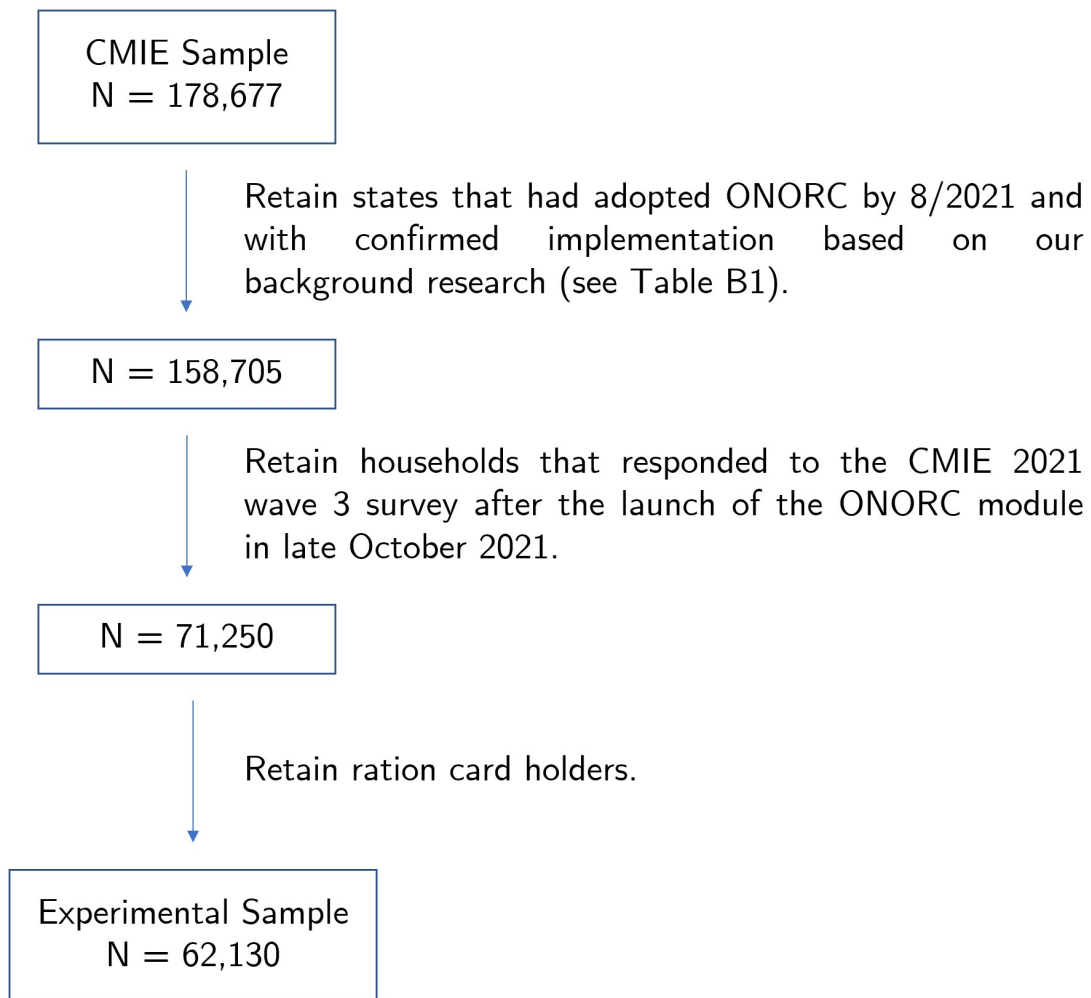
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# Appendix for “Does Food Insecurity Hinder Migration? Experimental Evidence from the Indian Public Distribution System”

## A Intervention Details

Figure A1: Summary of Sample Selection Process



## Figure A2: Information Script (English)

I would now like to share some information with you about your right to claim food ration through the Public Distribution System. Afterward I'll ask you a question about this information to make sure that everything made sense to you. Our team has conducted research in partnership with The World Bank to determine where you are able to claim your ration. The Government of India has recognized that migrants have historically been excluded from the Public Distribution System because ration cards were tied to each household's location of residence. In response to this, the Government has launched a program called One Nation, One Ration Card to ensure that households can use their ration card anywhere in India, not just at their designated ration shop.

As of August 2021, this program has been adopted in all states and union territories except for Assam and Chhattisgarh, which are planning to join in the next few months. What that means is that, if a member of your household travels to one of these states, that person can continue to claim food ration while living there. For example, if a household member travels from \$STATE to \$EXAMPLE.STATE, he or she can claim total or partial ration there. Whatever portion the migrant doesn't claim, his or her family can claim back in \$STATE. The same is true for migration within your state: for example, if someone from your household traveled to a different city in \$STATE, you could claim your ration there. To claim ration, you should bring your Aadhaar and a copy of your ration card, which should be linked to your Aadhaar. If you have any additional ID cards, we recommend you bring a copy of each with you in case the shop owner asks to see it. You must visit a shop with an ePoS machine, which will take a biometric read. Not all ration shop owners may be aware of One Nation One Ration Card, so you may need to visit a few shops. If you have an android smartphone, you can use the Mera Ration app once you arrive to locate ration shops near you. If you have not yet been issued a standardized, 12-digit ration card, you can try adding your 2-digit state code to the beginning of your ration card number, or adding your 2-digit household member code to the end of your ration card number to produce a unique number. Migrating to a new city can be difficult, and a goal of the One Nation, One Ration Card program is to ensure that migrants are not excluded from the government's ration allocations while living away from their home.

Many households in India do not know about the One Nation, One Ration Card program, which is why we are telling you about it today. We are sharing this information with you to help you make the best possible decisions about where to look for jobs and where to claim ration. I'm going to leave this information sheet with you which includes all the information I've told you already.

We have partnered with an organization called LEAD at Krea University to set up a toll-free phone number that you can call to speak with a member of our team who can offer you personalized information on the One Nation, One Ration Card program. You can call this number to learn about claiming ration in the place that you are considering migrating to. Our team member can share contact information and addresses of ration shops at your location of interest. We have information on 29 states, and the availability of information depends on what state you're interested in. You can use this information to call ration shops



ahead of time to ask about what documents they require to give ration to migrants. The toll-free phone number is on the sheet that I will leave with you. This service is completely free to you: you will never be charged for this service, even for the phone call.

I am going to leave this sheet with you so you can remember the main points of our discussion. I'm going to go through and explain each part to you. Stop me at any point if something doesn't make sense.

- One Nation, One Ration Card allows you to claim food ration in any participating state: as of August 2021, that is all states and union territories except for Assam and Chhattisgarh, which are joining in the next few months. You can also claim ration in any district within your state of \$STATE.
- To claim ration at a different shop, you should bring your Aadhaar card and a copy of your ration card. You must visit a shop with an electronic point-of-sale (ePoS) machine.
- A migrant can claim ration in one city at the same time that his or her family claims ration in a different city. In this case, each person will claim part of the household's ration allocation.
- Here is the toll-free hotline number. You can use this number to find the location of ration shops in almost any district in India, and phone numbers for many ration shops as well.
- To use the free hotline service, you will need to enter a unique access code. Your access code is \$ACCESS\_CODE and is listed here on your sheet.
- Not all ration shop owners may be aware of One Nation One Ration Card, so you may need to visit a few shops. We recommend you call several shops in your intended destination to ask about ration portability before migrating. You should also ask about claiming partial ration, if you are interested in that option, when you contact the shop owner. To find phone numbers of shops in your intended destination, you can call the free hotline service I just mentioned at 1800-309-4134. The hotline will run until March 18, 2022.
- If you encounter difficulties claiming ration, contact the local government office in charge of public distribution or consumer protection. You can also call 14445 to report any grievances to the government, or with other questions about the program. That government line is totally separate from the information hotline we are providing.

Do you have any questions for me right now?

Figure A3: Information Sheet (English)

# 1 NATION, 1 RATION CARD



  
**SUMMARY OF PROGRAMS**

The One Nation, One Ration Card (ONORC) scheme allows ration card holders to claim ration at any ration shop in participating states.

**You are not restricted to only your designated ration shop.**

  
**STATE COVERAGE**

By August 2021, every state and union territory has joined ONORC except for Assam and Chhattisgarh, which are planning to join in the next few months.

  
**ELIGIBILITY**

Bring Aadhar and a copy of your ration card, which must be linked. The ration shop must be equipped with an ePoS machine. It is possible to claim partial ration if you want to share between migrant and non-migrant household members.

**FREE INFORMATION HOTLINE**

**Call 1800-309-4134 to learn more about ration portability.**

- Learn ration shop phone numbers
- Learn ration shop addresses
- Free to call
- Information on 29 states
- Open now until March 18, 2022
- Open 9:30–5:30 Mon–Fri

**AVAILABLE IN 9 LANGUAGES. PRESS:**

1. HINDI	6. GUJARATI
2. BENGALI	7. URDU
3. MARATHI	8. KANNADA
4. TELUGU	9. ODIA
5. TAMIL	

**Your unique code for access is:**

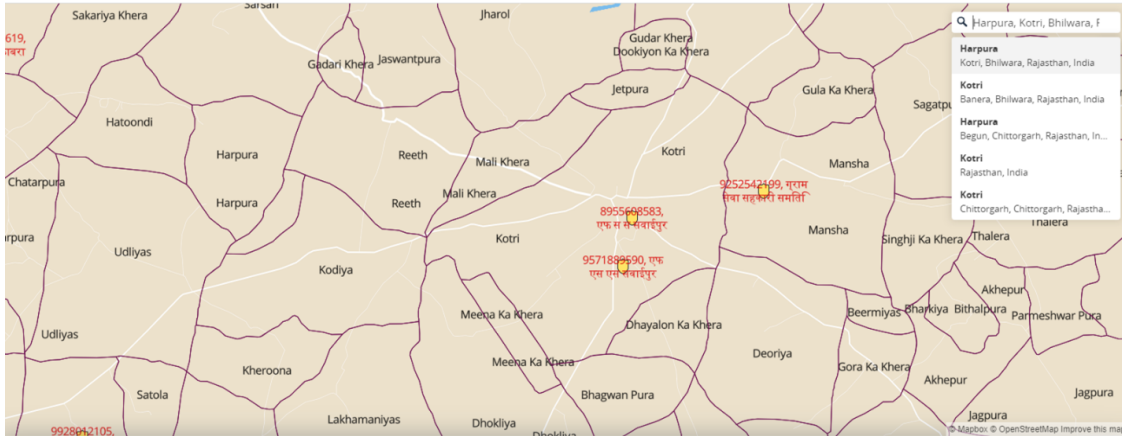
If you encounter difficulties claiming ration, contact the local government office in charge of public distribution or consumer protection. You can also call 14445 to report any grievances to the government, or with other questions about the ONORC program.

Table A1: Summary of Information Provided in Hotline

Potential Destination State:	Location of Shops	Phone Numbers of Shops	General Info About ONORC	Experimental Sample
Andaman & Nicobar Islands			X	
Andhra Pradesh	X		X	X
Arunachal Pradesh			X	
Assam	X	X	X	
Bihar			X	X
Chandigarh			X	
Chhattisgarh	X		X	
Dadra & Nagar Haveli & Daman & Diu	X		X	
Delhi	X		X	X
Goa	X		X	
Gujarat	X	X	X	X
Haryana	X	X	X	X
Himachal Pradesh	X	X	X	X
Jammu & Kashmir	X		X	
Jharkhand	X		X	X
Karnataka	X	X	X	X
Kerala	X		X	
Ladakh	X		X	
Lakshadweep	X		X	
Madhya Pradesh			X	X
Maharashtra	X		X	X
Manipur	X		X	
Meghalaya			X	
Mizoram	X		X	
Nagaland	X		X	
Odisha		X	X	X
Puducherry			X	
Punjab	X	X	X	X
Rajasthan	X	X	X	X
Sikkim	X		X	
Tamil Nadu	X		X	X
Telangana	X		X	X
Tripura	X		X	
Uttar Pradesh	X	X	X	X
Uttarakhand	X	X	X	X
West Bengal	X	X	X	X

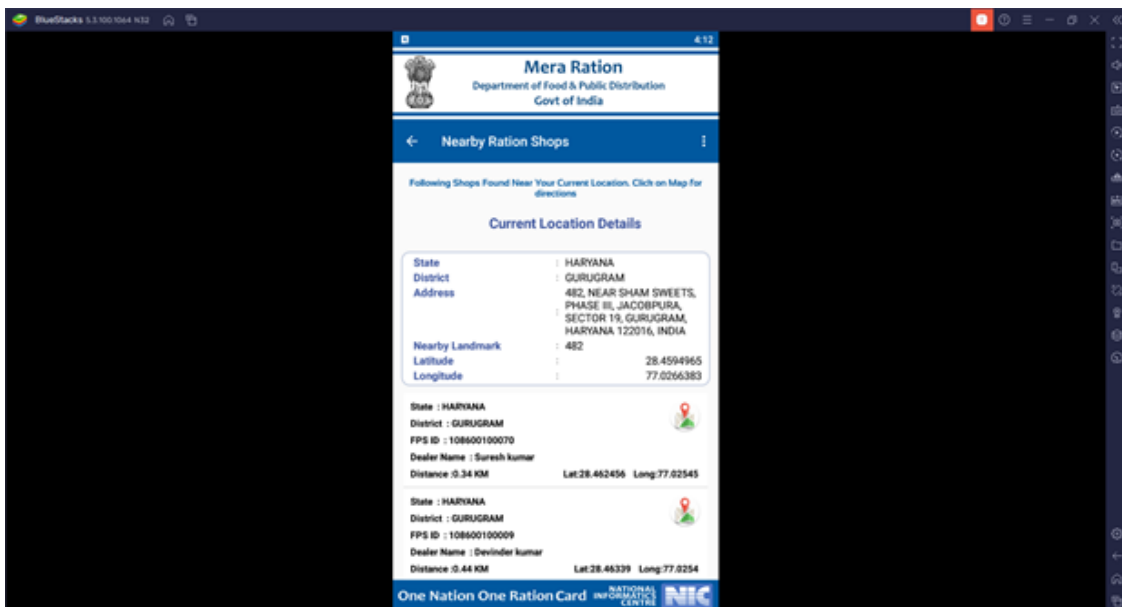
This table shows what information is provided through the hotline as a function of which state the caller is interested in. “Location of Shops” refers to addresses of ration shops. “Phone Numbers of Shops” refers to phone numbers of ration shops. “General Info About ONORC” refers to the information contained in the CMIE script, which the hotline staff can repeat. “Experimental Sample” indicates whether households located in that state were randomized into treatment and control groups.

Figure A4: Map Interface Used by Hotline Staff to Identify Ration Shop Locations



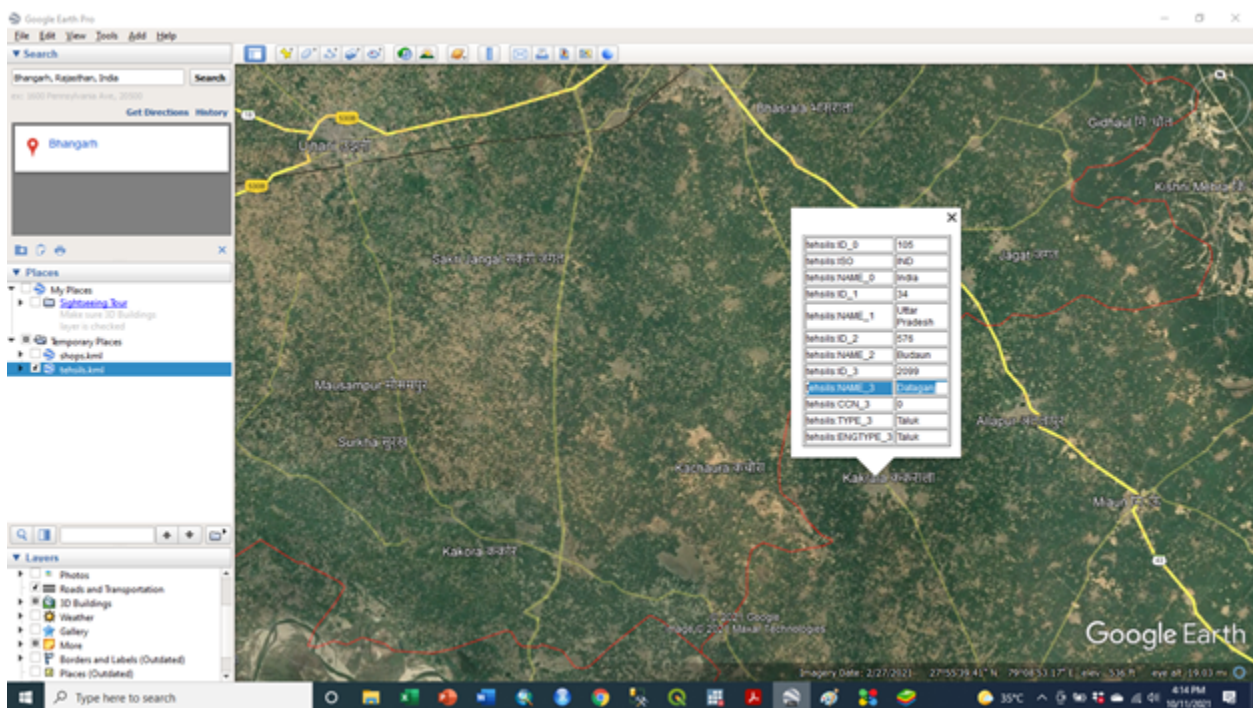
Ration shop addresses and phone numbers were uploaded to Mapbox for easy use by hotline staff.

Figure A5: Mobile App Used by Hotline Staff to Identify Ration Shop Locations



Mera Ration is a mobile app created by the Government of India to help find ration shops nearby. By changing the computer's location information, hotline staff could search for nearby shops in any covered state.

Figure A6: Map Interface Used by Hotline Staff to Identify Ration Shop Phone Numbers



In the event that shop-level phone numbers are missing from an area of interest, hotline staff could pull lists of phone numbers at the sub-district (approximately, township) level. Numbers were matched to district polygons in Google Earth.

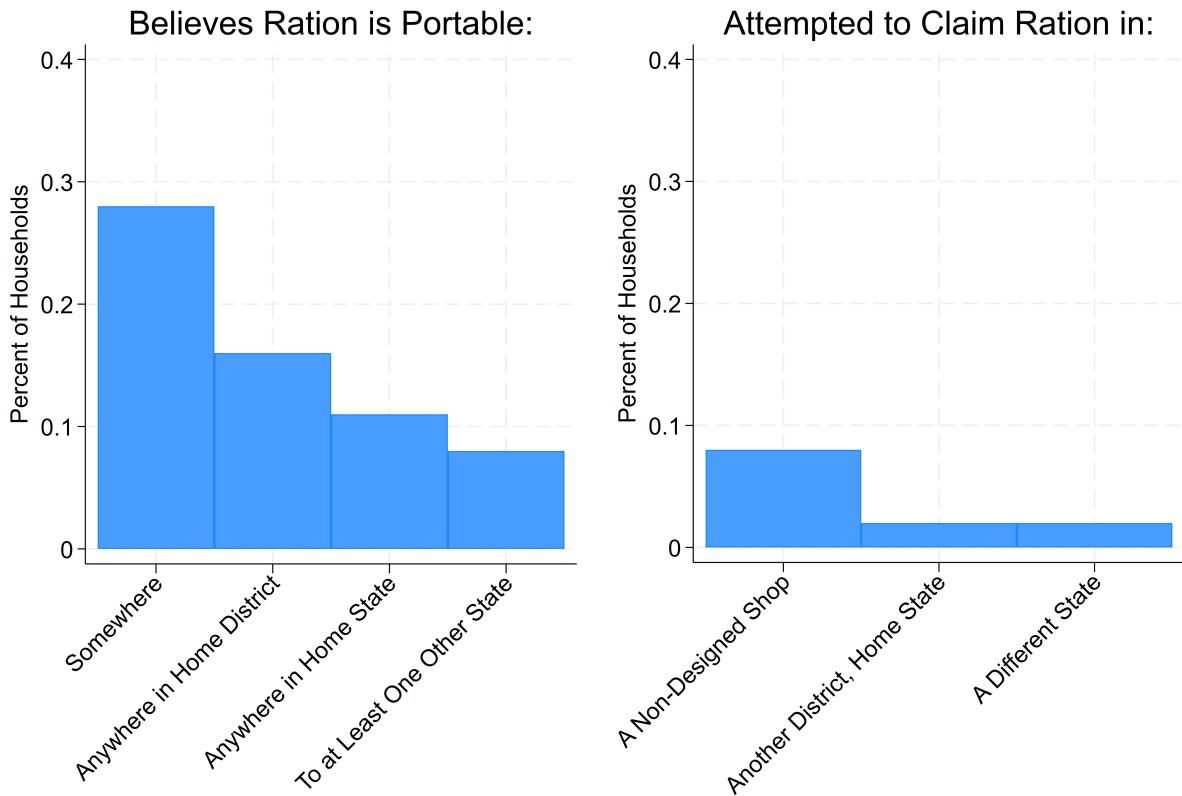
## B Pre-Experimental Research

Table B1: Summary of Research Into Ration Portability

State:	Adopted ONORC	Shop Owner Surveys	Mystery Shoppers	Interstate Transaction Data	Experimental Sample
Andhra Pradesh	X	X		X	X
Assam		X		X	
Bihar	X	X	X	X	X
Chandigarh	X			X	
Chhattisgarh		X		X	
Delhi	X	X	X	X	X
Goa	X			X	
Gujarat	X	X	X	X	X
Haryana	X	X		X	X
Himachal Pradesh	X	X		X	X
Jammu & Kashmir	X			X	
Jharkhand	X	X		X	X
Karnataka	X	X	X	X	X
Kerala	X			X	
Madhya Pradesh	X	X	X	X	X
Maharashtra	X	X	X	X	X
Meghalaya	X			X	
Odisha	X	X		X	X
Puducherry	X			X	
Punjab	X	X		X	X
Rajasthan	X	X		X	X
Sikkim	X			X	
Tamil Nadu	X	X	X	X	X
Telangana	X	X		X	X
Tripura	X			X	
Uttar Pradesh	X	X	X	X	X
Uttarakhand	X	X		X	X
West Bengal	X	X		X	X

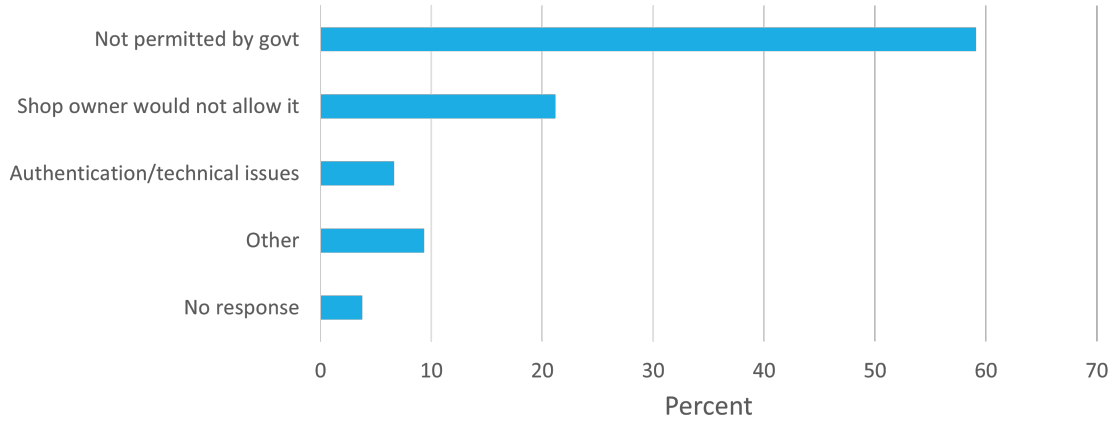
This table shows state-level information for the 28 states in which CMIE operates surveys. *Adopted ONORC* indicates whether the state had implemented interstate ration portability by August 2021. *Shop Owner Surveys* indicates whether we sampled ration shops to survey owners by phone to verify ONORC adoption. *Mystery Shoppers* indicates whether we sent mystery shoppers with eligible ration cards to attempt to claim ration in a different district/state than their home location. *Interstate Transaction Data* indicates whether we could verify ONORC adoption through state-level portability transaction data, provided by the Indian government. *Experimental Sample* indicates whether clusters located in that state were randomized into treatment and control groups.

Figure B1: Awareness and usage of ration portability was low prior to our experiment.



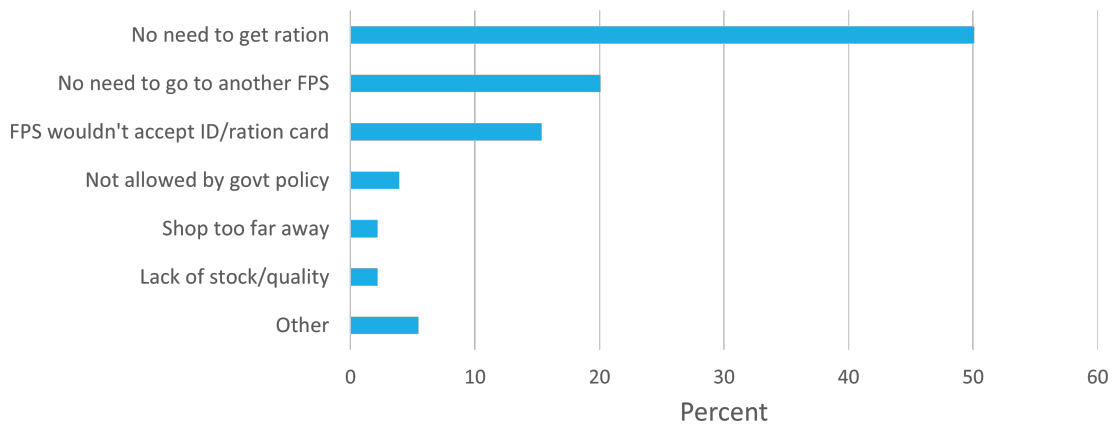
Data collected in January 2021 from surveys of ration card holders across 21 states that had implemented ONORC as of October 2020. A *Non-designated Shop* refers to any ration shop other than the one at which the respondent is listed as a ration claimant.

Figure B2: Reason Why Household Believes Ration Is Not Portable



Data collected from 28,066 surveys of ration card holders across 28 states in January 2021.

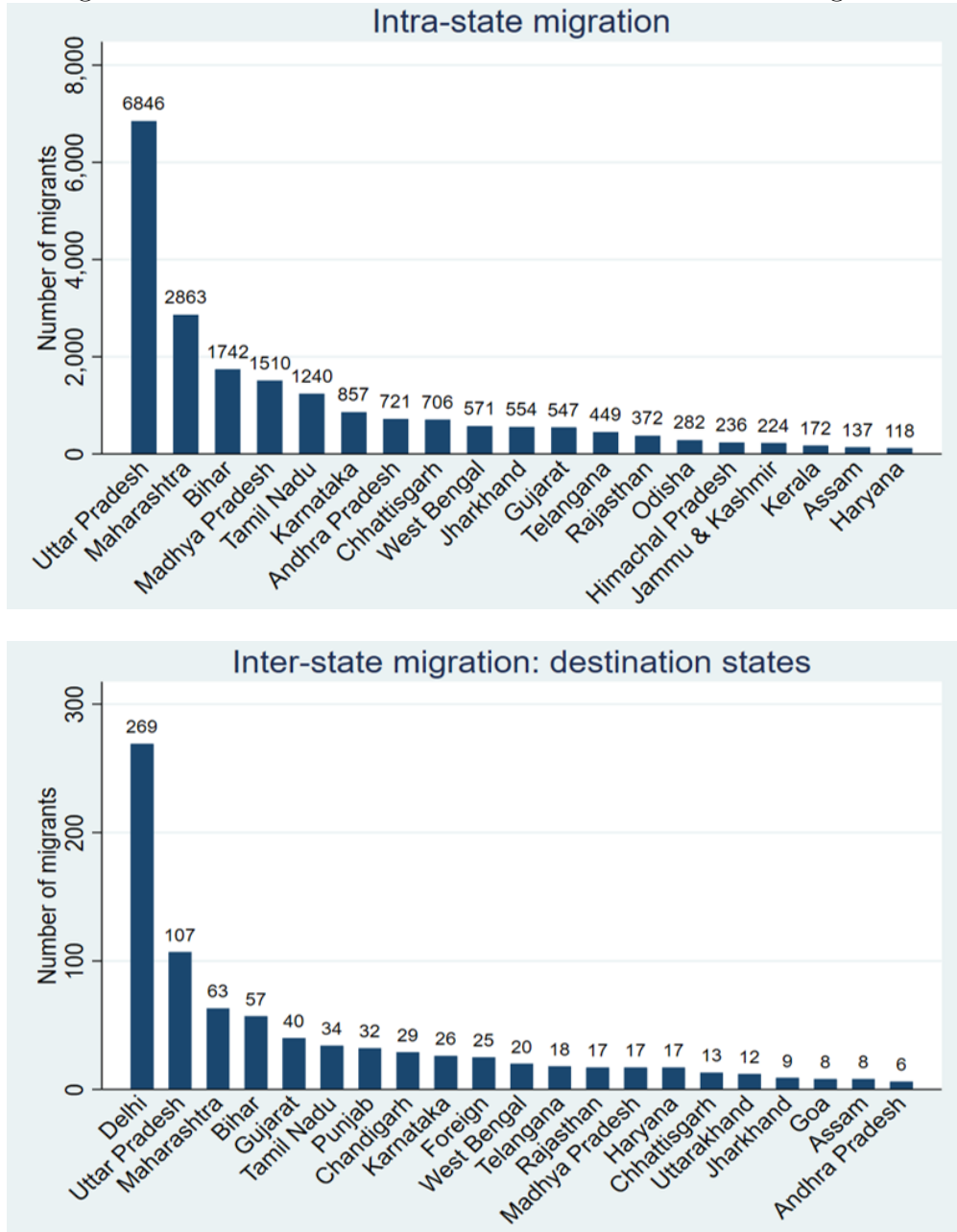
Figure B3: Reason Why Household Has Not Tried to Claim Ration Outside Their Designated Ration Shop.



Data collected from 28,066 surveys of ration card holders across 28 states in January 2021.



Figure B4: Destination States for Intra- and Inter-State Migration



Data collected from CMIE surveys. Top chart shows the number of emigrants that migrated within their home state at the time of survey. Bottom chart shows the number of emigrants by destination state at the time of survey.

## C Additional Tables

Table C1: There are no significant average differences in baseline characteristics between emigrants in the treatment and control groups.

Variable	(1) Control Mean/(SD)	(2) Treatment Mean/(SD)	(1)–(2) Pairwise t-test <i>p</i> -value
Age (Years)	25.76 (43.07)	25.88 (45.07)	0.86
Education (Years)	7.69 (13.42)	7.62 (15.17)	0.71
Head of Household = 1	0.06 (0.69)	0.07 (0.69)	0.60
Literate	0.95 (0.34)	0.95 (0.38)	0.95
OBC/ST/SC Caste	0.65 (2.38)	0.67 (2.06)	0.94
Ever Migrated	0.94 (1.40)	0.94 (1.89)	0.80
Emigrant at Baseline	0.93 (1.53)	0.93 (1.95)	0.88
Urban Emigrant at Baseline	0.66 (4.14)	0.62 (4.16)	0.22
Observations	23,942	22,413	46,355
Clusters	617	664	1,281

First two columns show means within control and treatment emigrants as of 4-month follow-up, respectively. Third column shows *p*-values from a two-sided t-test of equivalence of means, controlling for a randomization-stratum fixed effect and clustering standard errors at the village/town (primary sampling unit) level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table C2: Correlations Between Predictors of Treatment Effect Heterogeneity

	(1) Food Is a Migration Barrier	(2) Unaware of Any Ration Portability	(3) Poor Household	(4) Poor Household (No Credit)
Unaware of Ration Portability	-0.03*** (0.00) [0.00]			
Poor Household	-0.07*** (0.00) [0.00]	0.09*** (0.00) [0.00]		
Poor Household (No Credit)	-0.05*** (0.00) [0.00]	0.06*** (0.00) [0.00]	0.78*** (0.00) [0.00]	
Low-Wealth Household	-0.07*** (0.00) [0.00]	0.10*** (0.00) [0.00]	0.20*** (0.00) [0.00]	0.08*** (0.00) [0.00]
Dep. Var. Mean	0.19	0.70	0.40	0.23
Observations	62,130	62,130	62,130	62,130

An observation is a household at baseline. Each cell shows the coefficient from a bivariate regression of two pre-specified predictors of treatment effect heterogeneity. Measurement details are available in Baseler et al. (2022). All heterogeneity dimensions measured at baseline. Robust standard errors in parentheses; two-sided  $p$ -values in brackets. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

## All Pre-Specified Results

Table C3: Treatment Impacts on Portability Beliefs and Total Emigration

	(1)	(2)	(3)	(4)	(5)
	Believes Ration is Portable:				
	Somewhere	Across Districts	Across States	Number of Emigrants	Any Emigrants
<b>4-Month Impacts</b>					
Treatment	-0.079*** (0.029) [0.01]	-0.085*** (0.030) [0.00]	-0.055** (0.028) [0.05]	-0.010 (0.022) [0.65]	-0.001 (0.006) [0.83]
Outcome Mean in Control	0.55	0.43	0.37	0.87	0.30
Observations	48,297	48,297	48,297	52,902	52,902
<b>8-Month Impacts</b>					
Treatment	-0.058** (0.028) [0.04]	-0.048* (0.026) [0.06]	-0.019 (0.026) [0.46]	-0.007 (0.021) [0.73]	-0.007 (0.009) [0.40]
Outcome Mean in Control	0.53	0.45	0.35	0.99	0.34
Observations	41,388	41,388	41,388	45,351	45,351
<b>Pooled Impact</b>					
Treatment	-0.068*** (0.024) [0.01]	-0.068*** (0.025) [0.01]	-0.039* (0.023) [0.09]	-0.012 (0.018) [0.51]	-0.005 (0.006) [0.41]
Outcome Mean in Control	0.54	0.44	0.36	0.93	0.32
Observations	89,685	89,685	89,685	98,253	98,253

An observation is a family (household + emigrants). Excludes emigration for marriage. All regressions include a randomization-stratum fixed effect and controls chosen through lasso regression. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided  $p$ -values in brackets. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table C4: Treatment Impacts on Emigration Behavior

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	# of Planned Emigrants	# of New Emigrants	# of Urban Emigrants	# of Rural Emigrants	# of Inter-District Emigrants	# of Inter-State Emigrants	# of Emigrants (High Migration Propensity Only)
<b>4-Month Impacts</b>							
Treatment	0.000 (.) [.]	0.003 (0.009) [0.78]	-0.060*** (0.022) [0.01]	0.047** (0.020) [0.02]	-0.009 (0.017) [0.58]	-0.003 (0.003) [0.39]	-0.018 (0.033) [0.58]
Outcome Mean in Control	0.00	0.04	0.61	0.23	0.16	0.02	0.85
$q$ -Value: Treatment = 0	.	1.00	0.04	0.05	0.88	0.88	0.88
Observations	52,902	52,902	52,902	52,902	52,902	52,902	16,409
<b>8-Month Impacts</b>							
Treatment	0.000 (.) [.]	-0.018 (0.011) [0.10]	0.004 (0.025) [0.87]	0.008 (0.019) [0.68]	0.004 (0.018) [0.82]	-0.002 (0.003) [0.46]	-0.031 (0.028) [0.27]
Outcome Mean in Control	0.00	0.07	0.68	0.26	0.17	0.02	1.01
$q$ -Value: Treatment = 0	.	1.00	1.00	1.00	1.00	1.00	1.00
Observations	45,351	45,351	45,351	45,351	45,351	45,351	14,460
<b>Pooled Impact</b>							
Treatment	0.000 (.) [.]	-0.009 (0.008) [0.26]	-0.032 (0.021) [0.12]	0.026* (0.014) [0.07]	-0.002 (0.015) [0.87]	-0.002 (0.002) [0.34]	-0.028 (0.024) [0.25]
Outcome Mean in Control	0.00	0.05	0.64	0.25	0.16	0.02	0.93
$q$ -Value: Treatment = 0	.	0.58	0.58	0.58	0.64	0.58	0.58
Observations	98,253	98,253	98,253	98,253	98,253	98,253	30,869

An observation is a family (household + emigrants). Excludes emigration for marriage. All regressions include a randomization-stratum fixed effect and controls chosen through lasso regression. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided  $p$ -values in brackets. Sharpened  $q$ -values computed within a domain that includes secondary outcomes 2–5 and 16 as described in Baseler et al. (2022). \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table C5: Treatment Impacts on Family Economic Well-Being

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Total Income	Total Con- sumption	Food Con- sumption	Income Score	Finances Improved	Remitt- ances	Well-Being Index	Baseline Emigrant Outcomes		
								Food Security	Ration Claiming	Job Search
<b>4-Month Impacts</b>										
Treatment	-0.006 (0.027) [0.82]	-0.004 (0.013) [0.77]	0.020 (0.016) [0.22]	-57.178 (47.798) [0.23]	-0.001 (0.022) [0.97]	0.012 (0.015) [0.40]	-0.020 (0.020) [0.32]	-0.008 (0.021) [0.72]	-0.001 (0.031) [0.98]	-0.145 (0.440) [0.74]
Outcome Mean in Control	279	169	50	4,509	0.67	6	0.00	0.85	0.47	2.29
<i>q</i> -Value: Treatment = 0	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Observations	52,902	52,902	52,902	52,902	52,902	52,902	52,902	3,351	3,370	3,160
<b>8-Month Impacts</b>										
Treatment	0.048* (0.027) [0.08]	0.011 (0.015) [0.45]	0.025 (0.017) [0.14]	1.610 (38.124) [0.97]	-0.008 (0.023) [0.73]	0.023 (0.020) [0.25]	0.030 (0.033) [0.37]	-0.022 (0.035) [0.52]		
Outcome Mean in Control	257	170	48	5,190	0.73	6	-0.00	0.37		
<i>q</i> -Value: Treatment = 0	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00		
Observations	45,351	45,351	45,351	45,351	45,351	45,351	45,351	13,407		
<b>Pooled Impact</b>										
Treatment	0.018 (0.024) [0.44]	0.005 (0.012) [0.71]	0.026* (0.014) [0.06]	-39.921 (37.868) [0.29]	-0.004 (0.019) [0.82]	0.015 (0.016) [0.35]	0.004 (0.023) [0.86]	-0.017 (0.029) [0.54]		
Outcome Mean in Control	269	169	49	4,823	0.70	6	-0.00	0.39		
<i>q</i> -Value: Treatment = 0	1.00	1.00	0.87	1.00	1.00	1.00	1.00	1.00		
Observations	98,253	98,253	98,253	98,253	98,253	98,253	98,253	16,777		

An observation is a family (household + emigrants). *Low-Campaign State* is a dummy equal to 1 if change in beliefs about portability in that state (outside our sample) is below the sample median, as described in Section 5.2. Monetary values are measured monthly, converted to USD, and transformed using the inverse hyperbolic sine function. *Income Score* is the median occupational income, averaged across household members (Acemoglu and Autor, 2011). *Well-Being Index* is an index combining each measure of economic well-being, following the method of Anderson (2008). All regressions include a randomization-stratum fixed effect and controls chosen through lasso regression. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided *p*-values in brackets. Sharpened *q*-values computed within a domain that includes secondary outcomes 6–15 as described in Baseler et al. (2022). \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table C6: Heterogeneity in Treatment Impacts on Total Emigration

	(1) Food Is a Migration Barrier	(2) Unaware of Any Ration Portability	(3) Poor Households	(4) Poor Households (No Credit)	(5) Low-Wealth Households
<b>4-Month Impacts</b>					
Treatment $\times X$	0.066 (0.043) [0.12]	0.036 (0.038) [0.35]	-0.008 (0.031) [0.80]	-0.041 (0.036) [0.26]	0.020 (0.033) [0.56]
Treatment	-0.022 (0.020) [0.29]	-0.037 (0.032) [0.25]	-0.006 (0.024) [0.81]	0.000 (0.024) [0.99]	-0.020 (0.032) [0.54]
$X$	-0.038 (0.027) [0.15]	-0.018 (0.033) [0.58]	0.002 (0.021) [0.93]	0.023 (0.028) [0.41]	-0.056** (0.025) [0.02]
$q$ -Value: Treatment $\times X = 0$	1.00	1.00	1.00	1.00	1.00
Observations	52,902	52,902	52,902	52,902	52,902
<b>8-Month Impacts</b>					
Treatment $\times X$	0.015 (0.037) [0.69]	-0.038 (0.044) [0.38]	-0.028 (0.026) [0.27]	-0.019 (0.033) [0.56]	0.011 (0.025) [0.65]
Treatment	-0.012 (0.022) [0.59]	0.016 (0.041) [0.70]	0.003 (0.023) [0.89]	-0.005 (0.025) [0.84]	-0.014 (0.026) [0.58]
$X$	-0.035 (0.031) [0.27]	0.021 (0.041) [0.61]	0.001 (0.020) [0.96]	0.037 (0.029) [0.20]	-0.043* (0.026) [0.10]
$q$ -Value: Treatment $\times X = 0$	1.00	1.00	1.00	1.00	1.00
Observations	45,351	45,351	45,351	45,351	45,351
<b>Pooled Impact</b>					
Treatment $\times X$	0.048 (0.033) [0.15]	0.003 (0.036) [0.92]	-0.019 (0.023) [0.42]	-0.028 (0.029) [0.33]	0.011 (0.024) [0.63]
Treatment	-0.021 (0.018) [0.24]	-0.016 (0.033) [0.62]	-0.004 (0.021) [0.84]	-0.006 (0.021) [0.78]	-0.018 (0.025) [0.48]
$X$	-0.039 (0.026) [0.13]	0.004 (0.033) [0.91]	0.001 (0.018) [0.95]	0.028 (0.024) [0.25]	-0.043** (0.021) [0.04]
$q$ -Value: Treatment $\times X = 0$	1.00	1.00	1.00	1.00	1.00
Observations	98,253	98,253	98,253	98,253	98,253

Column titles show the dimension of heterogeneity,  $X$ , analyzed in that column. Measurement details are available in Baseler et al. (2022). All heterogeneity dimensions measured at baseline. All regressions include a randomization-stratum fixed effect and controls chosen through lasso regression. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided  $p$ -values in brackets. Sharpened  $q$ -values computed within a domain that includes all heterogeneous treatment impact tests. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table C7: Treatment Impacts on Family Income, Consumption, and Remittances (Without Hyperbolic Sine Transform)

	(1) Total Income	(2) Total Consumption	(3) Food Consumption	(4) Remittances
<b>4-Month Impacts</b>				
Treatment	-6.6 (4.8) [0.17]	1.2 (2.5) [0.62]	0.9 (1.1) [0.41]	-0.2 (0.3) [0.55]
Outcome Mean in Control Observations	279 52,902	169 52,902	50 52,902	6 52,902
<b>8-Month Impacts</b>				
Treatment	2.9 (4.0) [0.46]	3.0 (2.6) [0.26]	1.2 (0.9) [0.17]	0.2 (0.4) [0.52]
Outcome Mean in Control Observations	257 45,351	170 45,351	48 45,351	6 45,351
<b>Pooled Impact</b>				
Treatment	-2.8 (3.669) [0.44]	2.2 (2.078) [0.30]	1.3 (0.799) [0.11]	0.0 (0.278) [0.96]
Outcome Mean in Control Observations	269 98,253	169 98,253	49 98,253	6 98,253

An observation is a family (household + emigrants). Monetary values are in USD per month. All regressions include a randomization-stratum fixed effect and controls chosen through lasso regression. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided  $p$ -values in brackets. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .



Table C8: Treatment Impacts on Family Income, Consumption, and Remittances (Quantile Transformation)

	(1) Total Income	(2) Total Consumption	(3) Food Consumption	(4) Remittances
<b>4-Month Impacts</b>				
Treatment	-0.007 (0.005) [0.15]	0.001 (0.007) [0.93]	0.013 (0.009) [0.14]	-0.000 (0.001) [0.59]
Outcome Mean in Control	0.43	0.43	0.43	0.43
Observations	52,902	52,901	52,902	52,901
<b>8-Month Impacts</b>				
Treatment	0.008* (0.005) [0.09]	0.011 (0.007) [0.14]	0.016* (0.009) [0.08]	0.001 (0.001) [0.33]
Outcome Mean in Control	0.37	0.37	0.37	0.37
Observations	45,350	45,347	45,350	45,347
<b>Pooled Impact</b>				
Treatment	-0.000 (0.004) [0.93]	0.006 (0.006) [0.38]	0.015* (0.008) [0.06]	0.000 (0.001) [0.73]
Outcome Mean in Control	0.40	0.40	0.40	0.40
Observations	98,252	98,248	98,252	98,248

An observation is a family (household + emigrants). Monetary values are measured monthly and quantile transformed using the methodology of Sterck and Delius (2020). All regressions include a randomization-stratum fixed effect and controls chosen through lasso regression. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided  $p$ -values in brackets. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table C9: Test of Differential Attrition

	(1) Surveyed	(2) Surveyed Individual By Phone
<b>Difference at 4 Months</b>		
Treatment	0.002 (0.014) [0.91]	0.000 (0.015) [0.99]
Outcome Mean in Control Observations	0.85 62,130	0.74 7,216
<b>Difference at 8 Months</b>		
Treatment	0.007 (0.020) [0.74]	
Outcome Mean in Control Observations	0.73 62,130	
<b>Pooled Difference</b>		
Treatment	0.004 (0.013) [0.75]	
Outcome Mean in Control Observations	0.79 124,260	

An observation is a family (household + emigrants). All regressions include a randomization-stratum fixed effect. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided  $p$ -values in brackets. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table C10: Return Migration and Alternative Income Score

	(1)	(2)	(3)
	Income Score (Alt.)	Return Migration Rate (New Emigrants)	Return Migration Rate (New Emigrants)
<b>Difference at 4 Months</b>			
Treatment	-0.004 (0.007) [0.62]		
Outcome Mean in Control Observations	0.81 52,902		
<b>Difference at 8 Months</b>			
Treatment	0.011 (0.007) [0.13]	0.033 (0.042) [0.43]	0.015 (0.055) [0.78]
Lasso Controls?	Yes	Yes	No
Outcome Mean in Control Observations	0.93 45,351	0.59 1,057	0.59 1,057
<b>Pooled Difference</b>			
Treatment	0.000 (0.007) [0.98]		
Outcome Mean in Control Observations	0.86 98,253		

An observation is a family (household + emigrants). Return migration is defined as returning to the household by the 8-month survey, and is measured among new emigrants as of the 4-month survey. *Income Score (Alt.)* is an alternative occupational income score using the labor ministry's NCO-2004 skill classification codes. Excludes emigration for marriage. All regressions include a randomization-stratum fixed effect and controls chosen through lasso regression. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided  $p$ -values in brackets. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

## Weighted Results

Table C11: Treatment Impacts on Portability Beliefs and Total Emigration (Weighted)

	(1)	(2)	(3)	(4)	(5)
	Believes Ration is Portable:				
	Somewhere	Across Districts	Across States	Number of Emigrants	Any Emigrants
<b>4-Month Impacts</b>					
Treatment	-0.050** (0.022) [0.03]	-0.042* (0.022) [0.05]	-0.033 (0.020) [0.11]	-0.037* (0.022) [0.09]	-0.006 (0.006) [0.32]
Outcome Mean in Control Observations	0.55 48,297	0.43 48,297	0.37 48,297	0.87 52,902	0.30 52,902
<b>8-Month Impacts</b>					
Treatment	-0.044** (0.021) [0.03]	-0.042* (0.022) [0.05]	-0.014 (0.019) [0.45]	-0.011 (0.019) [0.54]	-0.007 (0.007) [0.30]
Outcome Mean in Control Observations	0.53 41,388	0.45 41,388	0.35 41,388	0.99 45,351	0.34 45,351
<b>Pooled Impact</b>					
Treatment	-0.047** (0.018) [0.01]	-0.043** (0.020) [0.03]	-0.025 (0.017) [0.15]	-0.034** (0.017) [0.04]	-0.009* (0.005) [0.10]
Outcome Mean in Control Observations	0.54 89,685	0.44 89,685	0.36 89,685	0.93 98,253	0.32 98,253

An observation is a family (household + emigrants). Excludes emigration for marriage. All regressions include a randomization-stratum fixed effect, sampling weights, and controls chosen through lasso regression. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided  $p$ -values in brackets. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table C12: Treatment Impacts on Emigration Behavior (Weighted)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	# of Planned Emigrants	# of New Emigrants	# of Urban Emigrants	# of Rural Emigrants	# of Inter-District Emigrants	# of Inter-State Emigrants	# of Emigrants (High Migration Propensity Only)
<b>4-Month Impacts</b>							
Treatment	0.000 (.) [.]	-0.011 (0.008) [0.20]	-0.046*** (0.017) [0.01]	0.019 (0.022) [0.38]	-0.023 (0.016) [0.14]	-0.004 (0.003) [0.17]	-0.061* (0.035) [0.08]
Outcome Mean in Control	0.00	0.04	0.61	0.23	0.16	0.02	0.85
$q$ -Value: Treatment = 0	.	0.26	0.04	0.33	0.26	0.26	0.25
Observations	52,902	52,902	52,902	52,902	52,902	52,902	16,409
<b>8-Month Impacts</b>							
Treatment	0.000 (.) [.]	-0.010 (0.009) [0.30]	-0.016 (0.023) [0.47]	0.024 (0.023) [0.30]	0.004 (0.018) [0.82]	-0.005 (0.004) [0.18]	-0.029 (0.030) [0.33]
Outcome Mean in Control	0.00	0.07	0.68	0.26	0.17	0.02	1.01
$q$ -Value: Treatment = 0	.	0.99	0.99	0.99	0.99	0.99	0.99
Observations	45,351	45,351	45,351	45,351	45,351	45,351	14,460
<b>Pooled Impact</b>							
Treatment	0.000 (.) [.]	-0.010 (0.007) [0.14]	-0.035** (0.016) [0.03]	0.014 (0.017) [0.42]	-0.009 (0.013) [0.50]	-0.004* (0.003) [0.09]	-0.053** (0.026) [0.04]
Outcome Mean in Control	0.00	0.05	0.64	0.25	0.16	0.02	0.93
$q$ -Value: Treatment = 0	.	0.16	0.13	0.26	0.26	0.14	0.13
Observations	98,253	98,253	98,253	98,253	98,253	98,253	30,869

An observation is a family (household + emigrants). Excludes emigration for marriage. All regressions include a randomization-stratum fixed effect, sampling weights, and controls chosen through lasso regression. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided  $p$ -values in brackets. Sharpened  $q$ -values computed within a domain that includes secondary outcomes 2–5 and 16 as described in Baseler et al. (2022). \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table C13: Treatment Impacts on Family Economic Well-Being (Weighted)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Total Income	Total Con- sumption	Food Con- sumption	Income Score	Finances Improved	Remitt- ances	Well-Being Index	Food Security	Ration Claiming	Job Search
<b>4-Month Impacts</b>										
Treatment	-0.034 (0.033) [0.30]	-0.010 (0.011) [0.38]	-0.005 (0.013) [0.68]	-4.436 (41.660) [0.92]	-0.004 (0.020) [0.84]	0.024 (0.020) [0.25]	-0.024 (0.019) [0.20]	-0.001 (0.022) [0.96]	0.021 (0.032) [0.52]	0.384 (0.498) [0.44]
Outcome Mean in Control	279	169	50	4,509	0.67	6	0.00	0.85	0.47	2.29
<i>q</i> -Value: Treatment = 0	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Observations	52,902	52,902	52,902	52,902	52,902	52,902	52,902	3,351	3,370	3,160
<b>8-Month Impacts</b>										
Treatment	0.024 (0.029) [0.42]	0.008 (0.013) [0.54]	0.015 (0.015) [0.31]	-19.423 (37.698) [0.61]	-0.022 (0.022) [0.32]	0.021 (0.023) [0.37]	-0.014 (0.033) [0.67]	-0.010 (0.027) [0.71]		
Outcome Mean in Control	257	170	48	5,190	0.73	6	-0.00	0.37		
<i>q</i> -Value: Treatment = 0	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00		
Observations	45,351	45,351	45,351	45,351	45,351	45,351	45,351	13,407		
<b>Pooled Impact</b>										
Treatment	-0.011 (0.026) [0.66]	-0.003 (0.011) [0.81]	0.003 (0.010) [0.78]	-57.303 (41.222) [0.16]	-0.010 (0.019) [0.57]	0.019 (0.020) [0.34]	-0.029 (0.024) [0.24]	-0.005 (0.025) [0.83]		
Outcome Mean in Control	269	169	49	4,823	0.70	6	-0.00	0.39		
<i>q</i> -Value: Treatment = 0	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00		
Observations	98,253	98,253	98,253	98,253	98,253	98,253	98,253	16,777		

An observation is a family (household + emigrants). *Low-Campaign State* is a dummy equal to 1 if change in beliefs about portability in that state (outside our sample) is below the sample median, as described in Section 5.2. Monetary values are measured monthly, converted to USD, and transformed using the inverse hyperbolic sine function. *Income Score* is the median occupational income, averaged across household members (Acemoglu and Autor, 2011). *Well-Being Index* is an index combining each measure of economic well-being, following the method of Anderson (2008). All regressions include a randomization-stratum fixed effect, sampling weights, and controls chosen through lasso regression. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided *p*-values in brackets. Sharpened *q*-values computed within a domain that includes secondary outcomes 6–15 as described in Baseler et al. (2022). \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .