Social Welfare Portability and Migration: Evidence from India's Public Distribution System*

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Abstract

This paper studies a new program designed to make food entitlements portable throughout India. We first characterize the state of food entitlement portability using mystery shoppers and surveys of migrants and distributors. We then inform households about the program, and barriers to using it, through a cluster-randomized controlled trial. Treatment impacts on beliefs about entitlement portability were initially positive, but later turned negative following a general rise in beliefs. These patterns are consistent with our experiment increasing awareness of the program but decreasing trust in its implementation. Migration to cities decreased, suggesting that access to food affects migrants' destination choices.

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1 Introduction

Throughout most of the world, real average incomes are higher in cities than in rural areas (Caselli, 2005, Gollin, Lagakos and Waugh, 2014). At the same time, social welfare programs are often tied to a beneficiary's location (Imbert and Papp, 2019, Tombe and Zhu, 2019), which may disincentivize migration. In 2019, the Government of India introduced One Nation, One Ration Card (ONORC), aiming to ensure the portability of food entitlements, or "food ration," across the country. Portability guarantees like ONORC may also alleviate other barriers to migration by ensuring access to necessities like food in the destination. These barriers are likely to be acute in urban areas, where job-seekers face significant search frictions and high, persistent levels of unemployment (Harris and Todaro, 1970, Franklin, 2018, Banerjee et al., 2023). However, there is little evidence as to whether migrants in India can obtain their food ration in practice given the significant technical challenges involved in implementing ONORC, including modifying back-end protocols to manage fluctuations in demand caused by portability, maintaining network infrastructure for biometric authentication, and raising awareness among distributors (Panda, 2022, Dalberg, 2022). Moreover, the scope for social welfare portability to affect migration decisions is largely unknown.

This paper uses an audit study to test the functionality of the ONORC program and a cluster-randomized information experiment to test how beliefs about food ration portability affect migration decisions. In India, food ration is provided through the Public Distribution System (PDS), a social welfare scheme used by 63% of the Indian population and costing more than 1 percent of GDP each year (World Bank, 2018, MicroSave, 2020, Gadenne et al., 2021). 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, in principle, allows beneficiaries to collect food ration across the entire country. To better understand how ration portability works in practice, we surveyed around 500 migrants and 2,000 ration shop owners and hired mystery shoppers to attempt to claim their ration using a ration card registered to another district or state. Based on the audit results, we designed an information experiment with two components: 1) basic information about the ONORC program and access to a call-center service where households could receive personalized information about ONORC, and 2) information about practical barriers to using the program. Note that these two components may have counteracting effects on beliefs.[1] We embedded our information experiment into a panel survey of around 62,000 Indian households across 18 states, allowing us to track beliefs about ration portability and migration over time.

[1] As described in our pre-analysis plan (Baseler et al., 2022*b*), our prior was that our experiment would increase perceived ration portability. Instead, the information we shared about barriers to using the program dominated, leading perceived ration portability to decrease on average by our first follow-up survey.

Our audit study reveals that ONORC was partly successfully implemented. The majority of ration shop owners report that migrants can claim ration at their shop and at least one mystery shopper transaction was successfully completed in all 14 cities where we conducted them. However, our audit findings also reveal significant barriers to using the program. Beyond the minority of shop owners reporting that migrants are not allowed to claim ration in their shop, many shop owners report frequent stock-outs—with about half reporting that these stock-outs caused them to prioritize local or regular customers—and electronic authentication issues. Mystery shopper transactions were also denied in several cases: the most common reason for failure was error in the electronic verification system. Surveys with migrants in the same 14 cities confirm a similar failure rate in attempted ration transactions.

Informing households about the program and barriers to using it led to an immediate increase in 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, indicating that by this time the information we shared about barriers dominated the effect on awareness.

We show that these patterns are consistent with 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.[2] To do so, we provide a simple framework that decomposes portability beliefs into awareness of the ONORC program and (potentially latent) trust in the government's capacity to implement it. We show that our information intervention should increase portability beliefs when awareness is low but decrease them when awareness is high, as it was after the government campaigns. Under additional assumptions which are supported by our data, we identify the direct impact of our experiment on trust among those aware of ONORC to be a drop of 6–9 percentage points (pp.).

While our experiment did not change the overall emigration rate, it led to a significant reduction in emigration to urban areas—and a corresponding increase in emigration to rural areas—over the four months following the intervention. These results imply that migrants view food ration access as especially important in cities. Eight months after our intervention, treatment impacts on emigration disappear, implying that concerns about ration access deterred short-term emigration to urban areas. We find a small, positive impact on income ($p = 0.08$) but no significant change in consumption eight months after the intervention, consistent with low or negative returns to migrating to urban areas for compliers.

[2]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.

To disentangle the mechanisms behind the shift from urban to rural destinations, we analyze heterogeneous treatment impacts along five pre-specified dimensions. The shift from urban to rural destinations is not differential for emigrants from poor households, poor households without access to credit, or households with low assets at baseline. We also show, using prior waves of our panel data, that emigration rates are highest among households with low levels of consumption and especially among households experiencing a negative consumption shock.[3] These findings suggest that credit constraints are unlikely to be driving our results, as a credit-constraint mechanism would predict a lower emigration rate—and a greater treatment response—among poor or credit-constrained households.

Rather, the shift away from urban destinations appears to be driven by concerns about food access. Labor force participation and employment rates are lower in urban, compared to rural, parts of India (PLFS, 2023), and urban households do not typically grow their own food, meaning that those without stable employment face a greater risk of food insecurity (IFPRI, 2017). Many migrants seeking work do not have jobs lined up upon arrival in the destination, making their future incomes difficult to predict.[4] Consistent with a food access mechanism, we find that the shift to rural destinations is concentrated among households that report at baseline that finding food after migrating to a city would be a major challenge.

While the ONORC program has the potential to improve food security for migrants, our findings show that significant technical barriers remain and that concerns over these barriers are central to households' beliefs about *de facto* food access outside their home locations. Because these barriers influence households' decisions about whether to emigrate to cities, they may distort the spatial distribution of economic activity by tying beneficiaries to their local ration shops.[5] Finally, we find that households reporting being concerned about food access after migrating are *less* likely to be poor, be poor and credit constrained, or have low assets according to our measures. 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.

Related Literature. This paper contributes to the study of social welfare programs in low-income countries, recently reviewed by Banerjee et al. (2024). One puzzle in this literature is low take-up among eligible households (Bhattacharya et al., 2015, Demirguc-Kunt,

[3]This suggests that households in this context use emigration to cope with negative shocks, similar to the finding of Lagakos, Mobarak and Waugh (2023).

[4]In our data, 31% of people migrating to urban areas for reasons other than marriage reported searching for a week or more after arriving before finding their first job (the analogous share for emigrants to rural areas is lower, at 17%). Eleven percent searched for three or more weeks.

[5]The emigration response we document also implies that households cannot perfectly substitute for the ONORC program through intra-household transfers, a point we return to in Section 6.1.

Klapper and Prasad, 2017), which appears to be partly due to hassle and information costs (Carneiro, Galasso and Ginja, 2018). We also document low take-up of the ONORC program, due in part to concerns about barriers to using it. Another focus of this literature is beneficiary targeting (Banerjee et al., 2023). We show 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. These households would likely benefit from improving access to the ONORC program, most likely by reducing administrative barriers.

We also contribute to the study of barriers to internal migration in low-income countries, which distort workers' location decisions and likely contribute to large sectoral productivity gaps (Gollin, Lagakos and Waugh, 2014, Bryan and Morten, 2019). This literature, reviewed by Lagakos (2020), has focused largely on inadequate information (Baseler, 2023), financial constraints (Bryan, Chowdhury and Mobarak, 2014, Cai, 2020), costs of migrating (Lagakos, Mobarak and Waugh, 2023, Imbert and Papp, 2020, Morten and Oliveira, 2024), cultural differences (Atkin, 2016), and land market frictions (De Janvry et al., 2015). Our results suggest an additional barrier: uninsured consumption risk in urban destinations, which better access to the PDS could partly alleviate.

We contribute to the literature studying the interaction between consumption risk and migration. As emphasized 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 low-income 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 (Munshi and Rosenzweig, 2016, Morten, 2019).[6] Social welfare programs can reduce migration if they increase the value of remaining in the origin (Imbert and Papp, 2019, 2020). In contrast, this paper studies the effects of a social welfare program that—in principle—reduces the risk incurred by migrating by offering access to subsidized food. While most studies of risk and migration examine risk sharing in the origin, we examine the role of insurance provided by social welfare programs in the destination. In this respect, two related papers are Bryan, Chowdhury and Mobarak (2014) and Akram, Chowdhury and Mobarak (2017), which study migration responses to programs that reduce consumption risk in the destination. We contribute to this literature by experimentally varying knowledge about an insurance scheme available in the destination while holding other determinants of the migration decision fixed.

[6]Migration can also improve informal risk sharing in the origin by increasing the resources available to the village (Meghir et al., 2021).

Our findings also relate to the literature studying the role of information in migration decisions. Several papers have found information gaps between perceived and actual earnings abroad (McKenzie, Gibson and Stillman, 2013, Shrestha, 2020), about migration intermediaries (Bazzi et al., 2021), 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, Nyarko and Wang, 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, McKenzie and Yang, 2016), although information about the risks *en route* was found to reduce intentions to migrate from The Gambia to Europe (Bah et al., 2023), information about mortality and wages in the destination affected migration decisions out of Nepal (Shrestha, 2020), and information about urban incomes increased emigration to cities in Kenya (Baseler, 2023). We build on these findings by showing that information about the portability of a major food ration program affects decisions about whether to migrate to a city.

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 welfare programs in India—combined with significant administrative barriers, especially across district or state lines—increases the relative value of staying home.[7] Our study also offers a rare look into short-term work migration, whereas most of the migration literature studies long-term migration or uses smaller or more localized surveys (Banerjee and Duflo, 2007, Morten, 2019, Imbert and Papp, 2020).[8] 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.

2 Background

This section summarizes India's food ration portability scheme and presents descriptive statistics on beliefs about ration portability collected prior to our experiment.

[7]This explanation is consistent with the large utility costs of migrating out of state in India found in Bhatiya et al. (2023) and with observation evidence on social welfare programs (Kone et al., 2018, Nayyar and Kim, 2018).

[8]One exception is Imbert and Papp (2019), which captures migration spells between one and six months.

2.1 India's Public Distribution System and Portability Scheme

The largest social welfare 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).[9]

Until recently, beneficiaries were required to claim ration in a designated PDS shop near their home. The One Nation, One Ration Card (ONORC) scheme aims to ensure that beneficiaries can claim their ration anywhere in the country. ONORC was introduced in four states in August 2019 and subsequently rolled out across the entire country. In principle, ONORC allows households with migrants to alternate claiming ration across months, or to split a given month's ration (Government of India, 2021). Our data indicate that, as of early 2022, many migrants are using the PDS: in our sample, 51% of migrants had claimed ration at some point in the destination.[10]

2.2 Pre-Experimental Beliefs

In January 2021, we launched an exploratory module to assess awareness of and interest in ration portability. The survey was conducted across 28 states with around 30,000 ration card holders outside our experimental sample, but we restrict our analysis to the 12 states that had implemented ONORC by March 2020 (the onset of Covid-19 in India). We found that beliefs about ration portability were low: only 35% of households in these states believed their ration was portable anywhere. Only 10% believed that it was portable to at least one other state. The most common answer given for why they believed their ration was not portable was that it was not permitted by the government (66% of answers), suggesting that many households were not aware of the ONORC program at this point. However, other common answers included that the shop owner would not allow it (13% of answers) and that there would be technical issues (5% of answers), pointing to existing concerns about implementation frictions. Only a small fraction of households reported attempting to use their ration card at a non-designated shop: 8% had tried to do so anywhere, and 2% had tried to do so in another state. These results are presented in Appendix D.1.

[9] 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, Niehaus and Sukhtankar, 2023), a state with relatively generous NREGS benefits.

[10] Choudhury et al. (2020) find that ONORC led existing migrants to stay in the destination longer.

3 Assessing Functionality of the ONORC Program

In October 2021, we gathered information on *de jure* eligibility requirements from Indian government websites and mobile applications, as well as *de facto* barriers to using ONORC from surveys of migrants and ration shop owners and mystery shoppers sent to ration shops. Appendix Table C1 lists the states that each of these activities was conducted in.

3.1 Audit Study Design

Migrant Surveys. We recruited a convenience sample of 575 migrants in 14 cities across seven states, all of which had adopted ONORC by August 2021. To be included in the survey, the migrant needed to have a valid ration card from a different district or state linked to their Aadhar identification.

Ration Shop Owner Surveys. We surveyed ration shop owners by phone in 20 states, 18 of which had adopted ONORC by August 2021 (we exclude the two that had not—Assam and Chhattisgarh—from our results). In 11 of these 20 states, we found comprehensive lists of ration shops through government websites and drew a random sample of these. In the remaining nine states, ration shops were identified through online searches and therefore form a convenience sample.

Mystery Shoppers. We recruited staff members of our survey firm to attempt to use their ration card in the same 14 cities that comprise the migrant survey sample. All staff members attempted to use either an out-of-state or out-of-district active ration card which was linked to their Aadhar identification. Staff were instructed to attempt several shops throughout the city. After each visit, the staff member recorded whether the transaction had been approved and, if not, the reason for failure.

3.2 Audit Study Results

Our audit study confirms at least partly successful implementation of ration portability, as shown in Table 1. However, they also indicate sizable frictions.

In our survey of migrants—who were about evenly split between having out-of-state and out-of-district ration cards and had lived in the destination for an average of 10.8 years—79% were aware that they were eligible to claim ration. Only 42% had ever tried to, and among those, 42% were always successful in their attempts and 59% were always or sometimes successful in their attempts.

In our survey of shop owners, almost all reported that their shop was equipped with the electronic point-of-sale (ePoS) machine necessary to use an out-of-district ration card and almost all reported being familiar with the ONORC program. However, only 72% said that migrants can claim ration in their shop. The two most common issues with processing migrant ration transactions, as reported by shop owners, were issues with the network and with the ePoS machine. Overall, just over half of owners saying that they serve migrant customers reported facing issues completing transactions, with 38% of these reporting stock-outs in more than six months per year. Of those reporting issues, just under half said that they prioritize local or regular customers when they are short on stock.

In our mystery shopper activity—again with staff about evenly split between out-of-state and out-of-district ration cards—48% of transactions were approved. Among failed transactions, half were due to system error such as network connectivity issues or the system not reading or accepting the ration card. An additional 16% were due to owner refusal and 10% to stock-outs. As our staff members tried multiple shops per city (an average of 9.7 shops per person per city), we can test whether refusal is partly idiosyncratic at the shop (or time of day) level. If so, this implies that migrants can improve their odds of successfully claiming ration by visiting multiple shops. To test this, we aggregate our data to the person-city level and compute an indicator for whether any transaction was successful. We find that the average rate of ever having a successful transaction is 75% across staff members and cities. This rate ranges from 50% in the state of Bihar to 100% in the states of Karnataka and Maharashtra. 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—these results are likely a lower bound on the ONORC barriers faced by migrants in practice.[11]

4 Design of the Information Experiment

This section describes the design of the cluster-randomized controlled trial we implemented to test whether information about ration portability affects migration decisions. Additional details are available in Appendix C.

[11] Other studies of PDS transactions have identified 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, Dalberg, 2022). See also this report.

Table 1: Results of Audit Study

	Mean	N
Migrant Surveys:		
Has Ration Card from Another State	0.44	575
Years in Current Location	10.80	573
Aware of Ration Portability	0.79	575
Attempted to Claim Ration in Current Location	0.42	575
of which, Ration Claims Always Successful	0.42	239
of which, Ration Claims Sometimes Successful	0.59	239
Shop Owner Surveys:		
Shop Equipped With ePoS Reader	0.97	1,855
Has Heard of ONORC Program	0.97	1,855
Says Migrants Can Claim Ration	0.72	1,855
of which, Migrants Can Claim Partial Ration	0.46	1,330
of which, Faces Issues Completing Transactions	0.57	1,330
of which, Faces Stock-Outs 6+ Months/Year	0.38	745
of which, Prioritizes Local or Regular Customers	0.44	745
Mystery Shopper Transactions:		
Has Ration Card from Another State	0.52	506
Transaction Successful	0.48	506
of which, Owner Confirmed Partial Ration Allowed	0.38	242
Failure Due to: System Error	0.50	264
Failure Due to: Owner Refusal	0.16	264
Failure Due to: Stock-Out	0.10	264
Number of Attempted Transactions (City-Person Level)	9.73	52
Any Transaction Successful (City-Person Level)	0.75	52

This analysis was not pre-specified. Top panel shows results from a survey of migrants in 14 cities across 7 states, all of whom had a valid ration card linked to their Aadhar identification. Middle panel results from phone surveys of ration shop owners in 18 states (two states that had not adopted ONORC by Aug 2021 are excluded from the sample). Bottom panel shows results from mystery shopper transactions, where survey firm staff who were eligible to claim ration attempted to do so at ration shops in the same 14 cities forming the migrant survey sample. *Number of Attempted Transactions (City-Person Level)* is the number of visits a given staff member in a given city conducted overall. *Any Transaction Successful (City-Person Level)* indicates whether at least one of those transactions was successful. Reason for failure recorded by the staff. "Don't Know" responses are coded as missing. See Appendix Table C1 for a list of sampled states for each activity.

4.1 Sample Selection

We restricted our experiment to states satisfying the following criteria: 1) they had adopted ONORC by August 2021, and 2) we were able to confirm that claiming ration with an out-of-state card was possible in that state. We decided to exclude the remaining states 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 Table 2.

We conducted our intervention with a subset of the survey sample covered by the Centre

for Monitoring Indian Economy, or CMIE (Centre for Monitoring Indian Economy, 2022).[12] 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 baseline survey, after our project launched in October 2021 and 3) have a ration card.[13] Appendix Figure C1 summarizes our sample selection process.

4.2 Data Collection

Our information intervention was embedded into our baseline survey, which ran from October 2021 through December 2021. We collected basic data on beliefs about ration portability immediately after the intervention. Our primary follow-up data were collected four months later (from February 2022 through April 2022) and more limited outcomes were collected eight months later (from June 2022 through August 2022).

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 C2. Moreover, randomization balance is maintained in the sample of surveyed households (see Appendix Tables C4 and C5), indicating that differential attrition is unlikely to be significantly affecting our estimates.

Shortly after the 4-month follow-up survey, we conducted additional surveys with emigrants 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 72% of sampled emigrants. Attrition is not differential by treatment status, as shown in Appendix Table C2. We use data on emigrant outcomes gathered from phone surveys with emigrants in place of indirect reports from household surveys when available.

4.3 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, 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 to

[12] In the CMIE sample, the probability of being surveyed within each month is proportional to population size (Vyas, 2021*b*). CMIE's survey methodology is described in greater detail in Vyas (2021*a*,*b*). Comparisons with other benchmark national representative surveys can be found in Sinha Roy and van der Weide (2022).

[13] In the most recently available data, about 87% of households in our sample have a ration card (Bhattacharya and Sinha Roy, 2021).

control households. Treatment status was assigned using a stratified permutation method (details in Appendix Section C.1).

4.4 Information Intervention

Households in our treatment group were read an information 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, which states had joined the ONORC program by the time of the survey, the documents required to claim ration outside of their designated PDS shop, and contacts of government offices responsible for resolving ration portability issues. The script was read by an enumerator and an information sheet was left with the respondent.

The script also included information about the following potential barriers to using ONORC: that not all ration shop owners are aware of ONORC and so the migrant may need to visit multiple shops, that migrants 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 number, and that the shop must be equipped with an ePoS system.

All treated households were provided with access to an information hotline which they could call to obtain local information (phone numbers and/or addresses) on ration shops across 29 states. Households in the control group did not receive the script, sheet, or hotline access. Appendix C.2 shows the script and sheet in English, and a description of the information provided through the hotline.

4.5 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 term *household* to refer to individuals who are usually residents of the household at the time of the survey, *emigrant* to refer to former household members whose reason for emigration is not marriage, and *family* to refer to the household plus any emigrants. We distinguish between *baseline emigrants* who were emigrants at baseline and *new emigrants* who emigrated after baseline. 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. For our primary emigration outcomes of interest, we compute the total number of emigrants in a family at a given point in time.

Portability Beliefs. To measure beliefs about 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. The first question was, "This question is about food ration claimed through the Public Distribution System. Can you use your ration card at ration shops other than your designated shop?" and was followed by analogous questions on shops in other districts and states.[14] If a person did not answer "Yes" 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.[15]

Estimation. We measure intent-to-treat effects using the following specification:[16]

$$y_{ict} = \beta T_c + \gamma y_{ic0} + \eta X_{ic0} + \theta_t + \alpha_c + \epsilon_{ict} \tag{1}$$

[14]Treated households were asked questions about portability before and after the information. Control households were only asked once. 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 beliefs at baseline using the 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.

[15]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.

[16]As described in our pre-analysis plan, we had originally planned to estimate treatment impacts using ANCOVA regression. However, the sign of our treatment impact on portability beliefs changed over time (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 E presents the full set of pre-specified analysis, including ANCOVA estimates and weighted results. Relative to the estimating equation specified in our pre-analysis plan, Equation 1 omits the variable M_{ic0}, indicating missing values of y_{ic0}. This is because we have no missing values for y_{ic0}.

where y_{ict} is an outcome for family i living in cluster c measured at time t with $t = 0$ corresponding to baseline (pre-treatment) values,[17] T_c is a treatment dummy assigned at the cluster level, X_{ic0} is a vector of baseline controls chosen through double lasso,[18] θ_t is a survey-round fixed effect, α_c is a randomization-stratum fixed effect, and ϵ_{ict} is an error term. Standard errors are clustered at the primary sampling unit (cluster) 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*b*), 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 (we label new analysis as such in the text). The analysis presented in Appendices A through D was not pre-specified. The full set of pre-specified results is presented in Appendix E, 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).

4.6 Summary Statistics and Balance

Table 2 provides a descriptive summary of emigration patterns during our study period. 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. 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.[19]

Summary statistics and tests of randomization balance are shown in Appendix Table C3. The average household has about 3.7 members and earns $257 per month. Forty-one percent

[17] As we only elicited portability beliefs for the control group once in the baseline survey, we impute y_{ic0} to be the same as y_{ic1} in control—that is, we assign the "immediate post-intervention" belief in control to be the same as the "pre-intervention" belief.

[18] 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.

[19] 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
Among Household Members at Baseline:		
Emigrated Over Following 4 Months	0.020	98,030
Emigrated Over Following 8 Months	0.031	106,763
Among New Emigrants Over Next 4 Months:		
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

This analysis was not pre-specified. Sample includes household members (that is, those residing at the household who did not return in that survey wave) ages 18–45 as of our baseline survey. Members whose reason for emigration is "Shifted to in-laws/new residence after marriage" are not counted as emigrated. Urban and rural destination categorizations are collected from survey data for emigrants who were household members in the previous wave; for emigrants who left and returned within the same survey round, they are inferred from the urban share of the destination district. Baseline data collected from Oct–Dec 2021; 4- and 8-month results are measured in follow-up surveys from Feb–Apr 2022 and Jun–Aug 2022 respectively. Estimates are weighted to account for sampling methodology and non-response.

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. 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.

Average portability beliefs and their baseline correlates are shown in Appendix Table B1. Literate, better-educated, and and richer households are more likely to report that their ration is portable at baseline.

5 Impacts on Beliefs About Ration Portability

This section analyzes beliefs about ration portability, beginning with treatment impact estimates in Section 5.1. The sign of these impacts changes from positive to negative over time,

which we argue is consistent with concurrent government awareness campaigns in Section 5.2. Section 5.3 provides a framework to decompose beliefs into awareness of the program and trust in the government's capacity to implement it, and Section 5.4 discusses under what conditions we can identify these impacts separately. Section 5.5 analyzes heterogeneity in impacts on beliefs.

5.1 Treatment Effects on Beliefs

The information we provided significantly and immediately increased households' beliefs about ration portability, 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 pp. in survey questions immediately following the information (p-values < 0.01). These impacts correspond to a 67% increase in portability beliefs overall, and a doubling of beliefs across state lines. Treatment did not bring portability beliefs up to 100%, providing an early indication that many households already had concerns about barriers to PDS access.

Four months later, treatment impacts are negative across all three belief measures. Treated 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 compared to baseline, with the share believing their ration is portable somewhere rising from 35% to 55%. These results are robust to excluding baseline controls, as shown in Appendix Table B2, and the reversal from baseline to follow-up is not due to sampling variation, as shown in Appendix Table B3 which uses a balanced sampled.[20]

Appendix Table B4 shows transition matrices of beliefs about ration portability from just before to just after the intervention (in the treatment group only) and from just after the intervention to the follow-up survey (separately by treatment group). Among those believing that their food ration is not portable at all before the intervention, 37% update their beliefs, with the majority (25% overall) updating fully and reporting that their ration is portable across state lines. There is also positive updating on average among those believing only in within-district or within-state portability: more of these groups update toward greater than toward less portability. However, a small share (7–16% depending on the initial belief)

[20]To test for the role of sampling variation, we restrict our estimation sample in Appendix Table B3 to individuals who i) were surveyed at both baseline and follow-up and ii) had a ration card linked to their Aadhar at follow-up, as beliefs questions were only asked to these households during that survey round. These restrictions have very small impacts on our estimates.

Table 3: Treatment Impacts on Portability Beliefs

	(1)	(2)	(3)
	Believes Their Ration is Portable:		
	Somewhere	Across Districts	Across States
Immediate Impacts			
Treatment	0.234***	0.207***	0.212***
	(0.029)	(0.028)	(0.027)
	[0.00]	[0.00]	[0.00]
Outcome Mean in Control	0.35	0.26	0.20
Observations	36,776	36,776	36,776
4-Month Impacts			
Treatment	-0.079***	-0.085***	-0.055**
	(0.029)	(0.030)	(0.028)
	[0.01]	[0.00]	[0.05]
Outcome Mean in Control	0.55	0.43	0.37
Observations	48,297	48,297	48,297

An observation is a family (household + emigrants). Ration portability beliefs are binary variables indicating whether the respondent replied "Yes" to the question "This question is about food ration claimed through the Public Distribution System. Can you use your ration card at ration shops other than your designated shop?" and two analogous follow-up questions about out-of-district and out-of-state shops respectively. If a person did not answer "Yes" to one of these questions, we code the following questions as "No." Immediate impacts are measured in the baseline survey (Oct–Dec 2021) after information is provided; 4-month results are measured in a follow-up survey (Feb–Apr 2022). Baseline sample is restricted to respondents who were asked updated beliefs questions (see Section 4.5). All regressions include a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable, and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Pre-treatment beliefs y_{ic0} in the control group are imputed to be the same as "immediate post-intervention" beliefs y_{ic1}. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

update toward less portability. There is substantial learning between baseline and follow-up in the control group for all levels of initial beliefs, with around 50% of those who believed their ration was portable within district or within state at baseline reporting at follow-up that their ration is not portable at all. Each of these changes is pronounced in the treatment group. Overall, these results point to a generalized loss in trust in portability, as opposed to concerns specific to portability across state or district lines.

5.2 Explaining the Treatment Effect Reversal

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—roughly corresponding to villages or towns—meaning that cross-treatment

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.

Government Awareness Campaigns. Instead, we show suggestive evidence that 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 2), 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, 2022*a*,*b*). 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 Appendix Figure D4. 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.[21]

The reversal of the treatment impact on portability beliefs can thus be explained 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 external campaigns, the treatment impact on concerns about barriers dominated the impact on awareness.[22] We formalize this intuitive argument in Section 5.3.

Alternative Explanations of the Reversal. To help rule out alternative explanations of the reversal, we gathered data from outside our experimental sample on beliefs about ration portability in May 2022, immediately after our 4-month follow-up survey. A similar rise in beliefs about portability outside of our experimental sample, compared with our control group, would help confirm an externally driven increase in awareness of the ONORC scheme, as opposed to explanations related to internal validity, such as errors by enumerators

[21] A description of the MicroSave campaign can be found here. "Fair price shops" refer to ration shops. Campaigns undertaken directly by state governments should be, if anything, less informative about *de facto* barriers compared to MicroSave's campaign. The absence of a discussion of *de facto* barriers is also present in reports of these awareness campaigns: see here, here, and here.

[22] Prior to our experiment, we were not aware of any plans to ramp up government awareness campaigns, and the analysis in Sections 5.2 through 5.5 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 time our experiment launched.

in adhering to experimental protocols.[23] As detailed in Appendix Section D.2, we find that beliefs outside of our experimental sample match those in our control group, both in levels and in changes (see Appendix Figure D5). Moreover, treatment impacts on beliefs and migration were pronounced in states experiencing above-median changes in out-of-sample beliefs, and close to zero in states with below-median changes, though these differences are generally not statistically significant, as shown in Appendix Table D1. Overall, these results support an external explanation of the treatment effect reversal, such as through government campaigns.

5.3 A Framework to Decompose Beliefs

This section provides a framework that decomposes reported beliefs about ration portability into *awareness* of the ONORC program and *trust* in its implementation. We show that the findings of Section 5.1 can be rationalized by a positive treatment impact on awareness and a negative treatment impact on trust, together with a general rise in awareness caused by government campaigns. Under stronger assumptions, we can identify treatment impacts on awareness and trust separately. In summary, 6–9% of those aware of ONORC by the end of our study lost trust in its implementation as a result of the information we provided.

Letting $Port_{it} \in \{0,1\}$ denote whether person i believes their food ration is portable at time t, consider the following decomposition:

$$Port_{it} = Aware_{it} \times Trust_{it},$$

where $Aware_{it}$ and $Trust_{it}$ are binary variables denoting person i's awareness of the ONORC program, and (potentially latent) trust in the government's capacity to implement it, at time t, respectively. We argue below that respondents' answers to our questions about whether they can claim ration outside their designated shop correspond to $Port_{it}$, as opposed to $Aware_{it}$ (see footnote 25). Letting α_t denote average awareness in a population at time t and τ_t denote average trust among those in the population aware of ONORC at time t, we obtain $E_i[Port_{it}] = \alpha_t \times \tau_t$.[24]

Finally, letting impacts on α and τ be given by Δ_α^X and Δ_τ^X, where $X \in \{\text{RCT,GOV,BOTH}\}$ refers to the randomized controlled trial (RCT) only, the government campaigns only, and the RCT and government campaigns together, respectively, we can summarize average porta-

[23] Note that the significant treatment impacts observed during the baseline survey also indicate that the experiment was carried out properly.

[24] This final expression follows from the law of total expectation: $E_i[Aware_{it} \times Trust_{it}] = Pr(Aware_{it} = 1)E_i[Aware_{it} \times Trust_{it}|Aware_{it} = 1] + Pr(Aware_{it} = 0)E_i[Aware_{it} \times Trust_{it}|Aware_{it} = 0] = Pr(Aware_{it} = 1)E_i[Trust_{it}|Aware_{it} = 1] = \alpha_t \times \tau_t$.

bility beliefs across our experiment as shown in Table 4. Note that because τ_t is a conditional expectation over those for whom $Aware_{it} = 1$, impacts on τ_t consist of both direct effects on trust and a selection effect through changes in awareness (see Appendix A.1 for a formal decomposition of Δ_τ). Note also that Δ^{BOTH} incorporates both interactions between the RCT and government campaigns as well as potential time trends (due, for example, to respondents' forgetting of the information we provided).

Table 4: Decomposed Average Portability Beliefs Over Time

	Control	Treatment
Before Experiment ($t = 0$)	$\alpha_0 \times \tau_0$	$\alpha_0 \times \tau_0$
After Experiment, Before Government Campaigns ($t = 1$)	$\alpha_0 \times \tau_0$	$\left(\alpha_0 + \Delta_\alpha^{RCT}\right) \times \left(\tau_0 + \Delta_\tau^{RCT}\right)$
After Experiment, After Government Campaigns ($t = 2$)	$\left(\alpha_0 + \Delta_\alpha^{GOV}\right) \times \left(\tau_0 + \Delta_\tau^{GOV}\right)$	$\left(\alpha_0 + \Delta_\alpha^{BOTH}\right) \times \left(\tau_0 + \Delta_\tau^{BOTH}\right)$

Each cell shows average beliefs in the control or treatment group about whether food ration is portable expressed in terms of initial levels of average awareness and trust and treatment impacts on those averages. $t = 0$ corresponds to the baseline survey (Oct–Dec 2021), $t = 1$ to the 4-month follow-up survey (Feb–Apr 2022), and $t = 2$ to the 8-month follow-up survey (Jun–Aug 2022).

Treatment Impacts Before Government Campaigns ($t = 1$). Initial treatment impacts (immediately following the information provision) on portability beliefs were large and positive. This implies:

$$\Delta_\alpha^{RCT}\left(\tau_0 + \Delta_\tau^{RCT}\right) > -\Delta_\tau^{RCT} \times \alpha_0. \tag{2}$$

In other words, the RCT's impact on ONORC awareness—scaled down by the share trusting in its implementation after the information was given—was larger in magnitude than any reduction in trust multiplied by the share of the population that was already aware of the program. Note that condition (2) is more likely to hold when α_0 is low, consistent with limited awareness of ONORC prior to our experiment.

Treatment Impacts After Government Campaigns ($t = 2$). Treatment impacts on portability beliefs at the 4-month follow-up survey (after government information campaigns had begun) were negative. This implies either that $\Delta_\alpha^{GOV} > \Delta_\alpha^{BOTH}$ or $\Delta_\tau^{GOV} > \Delta_\tau^{BOTH}$

must hold. The former is highly unlikely, as two awareness campaigns should increase awareness weakly more than one. The latter indicates that our experiment, combined with the governments' campaigns, reduced trust to a greater degree than the governments' campaigns alone. Given that government campaigns focused on simple awareness and should have little if any impact on trust, this is unsurprising. As we show in Appendix A.2, $\Delta_\tau^{GOV} > \Delta_\tau^{BOTH}$ cannot be driven by selection effects alone: a negative treatment impact on portability beliefs implies a negative direct impact on trust among those already aware of the program.

As shown in Table 3, portability beliefs are lower in the treatment group at the 4-month survey than immediately following the experiment (47% and 58% for portability somewhere, respectively). This could reflect forgetting of the information by some of the compliers over this period. It could also indicate that our intervention led some respondents to seek out additional information about ONORC which reduced their trust in its implementation. The trust and awareness parameters we identify in Section 5.4 should thus be interpreted as being inclusive of awareness dissipation and/or knock-on effects of the RCT on trust.

5.4 Identifying Trust and Awareness Impacts

Our data support additional assumptions that allow us to more precisely decompose impacts on beliefs:

Assumption 1. *No direct impact of government campaigns on trust in control regions.*
Assumption 2. *Awareness is balanced across treatment and control after government campaigns ($\Delta_\alpha^{GOV} = \Delta_\alpha^{BOTH}$).*
Assumption 3. *No secular time trends in control regions.*

Trust Impacts. Under Assumptions 1 and 2, the treatment impact on portability beliefs at the 4-month follow-up (after government campaigns) shown in Table 3—a reduction by 6 to 9 pp. across beliefs about any, inter-district, and inter-state portability—identifies the direct impact of the RCT on trust among those aware of portability after the campaigns. See Appendix A.3 for a derivation.

Awareness Impacts. Under Assumptions 1 and 3, the change in control-group portability beliefs from the experiment to the 4-month follow-up survey—17–20 pp. across our three measures of beliefs (see Table 3)—identifies the awareness impact of government campaigns scaled down by average trust among the newly aware, or $\Delta_\alpha^{GOV} E[Trust_0 | Aware_0 = 0, Aware_2^{GOV} = 1]$. Under Assumption 2, this expression is equal to the joint impact of

the RCT and government campaigns, $\Delta_{\alpha}^{BOTH} E[Trust_0 | Aware_0 = 0, Aware_2^{BOTH} = 1]$. See Appendix A.4 for a derivation.

Testing the Assumptions. Assumption 3, while fundamentally not testable, has some support in our data: beliefs about ration portability were stable over the year preceding our experiment. In our pre-experimental research undertaken in January 2021 and summarized in Section 2.2, we found that 35% of households believed their ration was portable. That share is very similar to the 33% of households that believed their ration was portable immediately prior to the information intervention nearly one year later. This suggests that, absent the onset of government awareness campaigns, beliefs would not have changed much in control regions. Support for Assumption 1 comes from the nature of the government campaigns, which were designed to raise awareness, and so should not impact trust except by changing the set of people aware of the program—a selection effect. Our best available information on these campaigns indicates that barriers to access were not mentioned (see footnote 21). Assumption 2 is likely the strongest of the three, but reasonable under a "saturation" assumption that those sufficiently interested in the ONORC program to remember the information provided in our experiment four months later were also reached by government campaigns. We believe the most likely violation of Assumption 2 is that, within our experimental sample, government campaigns did not have the same reach as our experiment, that is, $\Delta_{\alpha}^{GOV} < \Delta_{\alpha}^{BOTH}$. In this case, we would underestimate the magnitude of the trust impact Δ_{τ}^{RCT}, as higher awareness in the treatment group at $t = 2$ implies a greater reduction in trust to rationalize a given level of portability beliefs.

To test Assumptions 1 and 2 empirically, we restrict our sample to the set of states where portability beliefs outside our experimental sample were very high after government campaigns. There is a discontinuity in mean beliefs at the state level at 89%, as shown in Appendix Figure D5: four states have mean reported beliefs between 89% and 98%. In these states, Assumptions 1 and 2 are very likely to hold, as beliefs are too high for government campaigns to have reduced trust substantially (Assumption 1) or for there to be substantial scope for our experiment to have increased awareness beyond the government campaigns (Assumption 2). Estimating treatment impacts on beliefs in these states gives an estimated 5 pp. reduction as of the 4-month follow-up: similar, though noisier, than estimates from the overall sample, as shown in Appendix Table B5.[25]

[25]This result further supports our assumption that most respondents answered "Yes" to our portability beliefs questions only if they were aware of the program and believed it functioned in practice. If respondents were answering the question as a pure awareness measure, then Assumption 2 implies that portability beliefs in control are a lower bound for beliefs in treatment, but the results in Table 3 show that they are not. Another piece of evidence is that reported beliefs were around 40–60% in the treatment group immediately following the information, much lower than we would expect given that those respondents had heard the

5.5 Heterogeneity in Impacts on Beliefs

Table 5 presents heterogeneous treatment effect estimates on portability beliefs anywhere along six baseline dimensions.[26] We find that the increase in portability beliefs immediately after the information was provided, and the reversal four months later, holds in all 12 groups (each sub-sample defined by six binary variables). Households reporting that food access is a migration barrier were slightly more likely to update in both directions (5 pp. more positive at baseline, p-val = 0.21, and 4 pp. more negative at 4-month follow-up, p-val = 0.39). As these households are particularly likely to benefit from the ONORC scheme, it is likely that they paid close attention to both the *de-jure* and *de-facto* information we shared; that is, both Δ_{α}^{RCT} and Δ_{τ}^{RCT} were large for this group. Poor households (those in the bottom 40% of per-adult-equivalent consumption in our sample), and especially credit-constrained poor households (those without any borrowing), were less likely to update their beliefs immediately after the information was given: this is consistent with greater initial concerns about program implementation in this group (that is, a low τ_0). Interestingly, low-wealth households (those in the bottom 40% of the first principal component of several durable asset measures) updated their beliefs to a greater extent initially, and these beliefs remained persistently higher than the control group even after the onset of government awareness campaigns. This is potentially consistent with limited attention: these households have less education and are less likely to migrate, as shown in Appendix Table B6. Finally, we find that urban households updated slightly more immediately after the information was given, and then experienced a starker drop in the 4-month follow-up ($p = 0.16$). This is consistent with greater exposure to government campaigns, which relied largely on banners on buses, audio announcements at railway stations, displays at government offices, and other methods targeting urban households (Government of India, 2021).

6 Impacts on Migration and Economic Outcomes

This section presents estimated treatment impacts on emigration and economic outcomes in the family. We interpret these effects in light of the reduction in portability beliefs caused by our experiment over the four months following the intervention.

information only minutes earlier.

[26] We pre-specified five dimensions to assess heterogeneity in migration impacts. We use those same dimensions here—except baseline beliefs in ration portability, since treatment differences there are mechanical—plus two additional dimensions.

Table 5: Heterogeneity in Treatment Impacts on Perceived Ration Portability

Outcome: Believes Their Ration Is Portable Somewhere	(1) Food Is a Migration Barrier	(2) Has Migrant at Baseline	(3) Poor Households	(4) Poor Households (No Credit)	(5) Low-Wealth Households	(6) Urban Households
Immediate Impacts						
Treatment × X	0.054	-0.024	-0.077**	-0.124***	0.138***	0.024
	(0.043)	(0.020)	(0.032)	(0.040)	(0.043)	(0.046)
	[0.21]	[0.24]	[0.02]	[0.00]	[0.00]	[0.61]
Treatment	0.224***	0.242***	0.267***	0.265***	0.157***	0.218***
	(0.030)	(0.030)	(0.032)	(0.029)	(0.037)	(0.017)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
X	-0.029	0.015	0.005	0.065**	-0.110***	0.287**
	(0.025)	(0.012)	(0.020)	(0.029)	(0.025)	(0.140)
	[0.23]	[0.21]	[0.79]	[0.03]	[0.00]	[0.04]
q-Value: Treat. × $X = 0$	0.17	0.17	0.03	0.01	0.01	0.39
Observations	36,776	36,776	36,776	36,776	36,776	36,776
4-Month Impacts						
Treatment × X	-0.038	-0.005	0.041	0.030	0.137***	-0.065
	(0.044)	(0.019)	(0.036)	(0.048)	(0.045)	(0.046)
	[0.39]	[0.78]	[0.26]	[0.53]	[0.00]	[0.16]
Treatment	-0.071**	-0.078***	-0.094***	-0.083***	-0.156***	-0.035*
	(0.030)	(0.029)	(0.033)	(0.031)	(0.042)	(0.020)
	[0.02]	[0.01]	[0.01]	[0.01]	[0.00]	[0.08]
X	0.047*	0.011	0.017	0.002	-0.046	-0.152
	(0.028)	(0.013)	(0.021)	(0.027)	(0.031)	(0.137)
	[0.09]	[0.38]	[0.42]	[0.94]	[0.13]	[0.27]
q-Value: Treat. × $X = 0$	0.88	1.00	0.78	0.88	0.01	0.64
Observations	48,297	48,297	48,297	48,297	48,297	48,297

This analysis was not pre-specified. An observation is a family (household + emigrants). The outcome variable is a binary variable indicating whether the respondent replied "Yes" to the question "This question is about food ration claimed through the Public Distribution System. Can you use your ration card at ration shops other than your designated shop?" Column titles show the dimension of heterogeneity, X, interacted with treatment in that column's regression. All heterogeneity dimensions are binary variables measured at baseline. *Food Is a Migration Barrier* equals 1 if the household reports finding food at the destination as one of the top three challenges a hypothetical migrant would face. *Has Migrant at Baseline* equals 1 if the family had any emigrants as of the baseline survey. *Poor Households* are those in the bottom 40% of per-adult-equivalent household consumption in our sample. *Poor Households (No Credit)* adds the additional restriction that the household does not have an outstanding loan. *Low-Wealth Households* are those in the bottom 40% of the first principal component of a set of 12 durable asset measures. Immediate impacts are measured in the baseline survey (Oct–Dec 2021) after information is provided; 4-month results are measured in a follow-up survey (Feb–Apr 2022). All regressions include a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable, and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. 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 each heterogeneous treatment impact tests. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6.1 Impacts on Migration

While our information experiment did not change the overall emigration rate, it led to a significant reduction in emigration to urban areas and a corresponding increase in emigration to rural areas. 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, effect size = 10%, p-val $<$ 0.01) and 0.05 more emigrants to rural destinations (on a base of 0.23, effect size = 22%, p-val = 0.02), as shown in Table 6.[27] This suggests that those emigrants who were dissuaded from traveling to cities switched to rural destinations rather than not emigrating at all (see Appendix A.5 for a formal decomposition of treatment effects on emigration destinations). Relative to the magnitude of the treatment impact on beliefs about ration portability as of the 4-month follow-up—an 8 pp. reduction in beliefs about portability somewhere other than the home locality—the impact on emigration to urban areas is quite large (0.75 fewer emigrants per changed belief). The shift away from urban destinations is thus consistent with PDS access being an important consideration for prospective emigrants to urban areas.

We find no significant treatment effects on other measures of emigration behavior, including inter-district or inter-state emigration, as shown in Appendix Table E2. Most migration occurs within districts, leaving little scope for influence on out-of-district emigration. There is little heterogeneity in treatment impacts on total emigration based on several poverty indicators or prior awareness, as shown in Appendix Table E4.

The shift away from emigration to urban areas implies that households cannot freely move resources across space: otherwise, they could in principle substitute for the ONORC program through intra-household transfers. Possible explanations of this finding include that urban market prices are typically higher than rural prices and the value of the PDS subsidy is increasing in market price (Gadenne et al., 2021); the PDS insulates beneficiaries from price variation, which may be different in rural compared to urban areas (Gadenne et al., 2021); and remittance frictions are high (Mobarak, Vernot and Kharel, 2023).

As of the 8-month follow-up survey, treatment impacts on emigration to urban and rural areas 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 point in Section 7.

[27] The q-values adjusted for false discovery rate are 0.04 and 0.05 for these outcomes respectively, as shown in Appendix Table E2.

Table 6: Treatment Impacts on Emigration

	(1) Number of Emigrants	(2) # of Emigrants to Urban Areas	(3) # of Emigrants to Rural Areas
4-Month Impacts			
Treatment	-0.010	-0.060***	0.047**
	(0.022)	(0.022)	(0.020)
	[0.65]	[0.01]	[0.02]
Outcome Mean in Control	0.87	0.61	0.23
Observations	52,902	52,902	52,902
8-Month Impacts			
Treatment	-0.007	0.004	0.008
	(0.021)	(0.025)	(0.019)
	[0.73]	[0.87]	[0.68]
Outcome Mean in Control	0.99	0.68	0.26
Observations	45,351	45,351	45,351

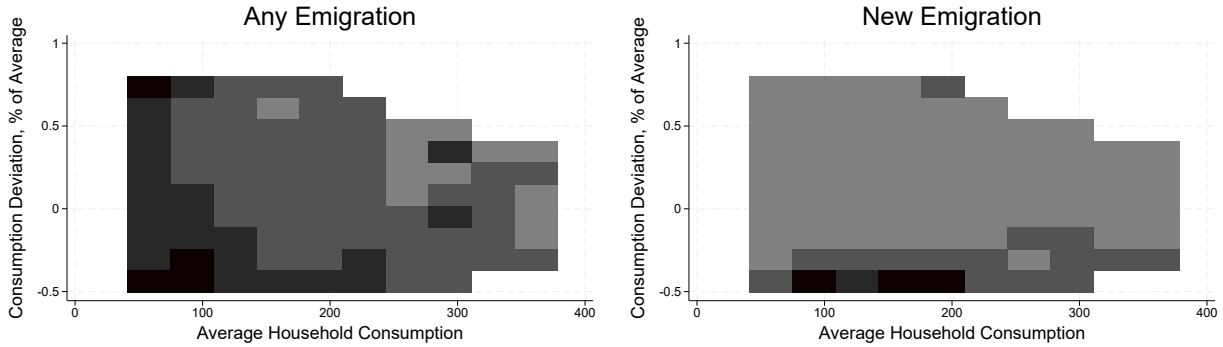
An observation is a family (household + emigrants). *Number of Emigrants* is the number of family members listed as emigrants in that survey round whose reason for emigration is not "Shifted to in-laws/new residence after marriage." Urban and rural destination categorizations are collected from survey data for emigrants who were household members in the previous wave; for emigrants who left and returned within the same survey round, they are inferred from the urban share of the destination district. Four- and eight-month results are measured in follow-up surveys in Feb–Apr 2022 and Jun–Aug 2022 respectively. All regressions include a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable, and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Pre-treatment beliefs y_{ic0} in the control group are imputed to be the same as "immediate post-intervention" beliefs y_{ic1}. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6.2 Mechanisms Behind the Shift to Rural Destinations

Why did the information we provided decrease emigration to urban areas while increasing it to rural areas? The negative treatment impact on beliefs about ration portability four months after our intervention suggests that concerns about access to food led emigrants to avoid cities. However, other explanations are possible. For example, learning about barriers to PDS access may exacerbate credit constraints because food in the destination would need to be purchased at market prices. Or, beliefs about ration access in the destination could affect emigration decisions simply through the value of the PDS transfer. In this section we examine selection patterns into emigrating together with heterogeneity in treatment impacts on emigration to help distinguish between these potential explanations.

Emigrant Selection at Baseline. Emigrants at baseline 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 from households experiencing a negative consumption shock, measured by the percent deviation of 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 emigration 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, Mobarak and Waugh (2023). Because new emigrants come largely from households experiencing negative consumption shocks, the value of insurance against poor outcomes in the destination—which food ration portability can in principle offer—is likely to be high.

Figure 1: Emigration rates are highest among poor households with recent negative consumption shocks.



This analysis was not pre-specified. Data from baseline surveys (Oct–Dec 2021) and three survey waves preceding the baseline. Each cell shows an average emigration rate among families in our baseline sample, computed as the mean of a binary variable indicating whether the family has any emigrants, excluding emigrants whose reason for emigration is "Shifted to in-laws/new residence after marriage." Darker colors indicate higher emigration rates. Horizontal axis shows average monthly household consumption in USD 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. Estimates are weighted to account for sampling methodology and non-response.

Heterogeneous Impacts on Emigration. We test for heterogeneous treatment impacts on emigration decisions along five pre-specified dimensions. Because we find no significant impacts on total emigration, we analyze destination switching, that is, emigrating to a rural over an urban destination.[28] 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 code emigrants to urban areas as 1 and to rural areas as 0. Such a sample restriction could 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 6. Second, treatment did not impact selection into emigration based on several baseline characteristics—including age, education, and emigration experience—as shown in Appendix Table B7.

We find that impacts on urban-to-rural switching were significantly greater among households reporting at baseline that finding food in the destination would be a challenge (by 12 pp., p-val < 0.01, q-val $= 0.02$), as shown in Table 7. These households' beliefs about overall ration portability were lower at the 4-month follow-up by 11 pp. (see Table 5). These findings point to increased concerns about food security in the destination—due to lower beliefs about PDS access there—as the mechanism driving impacts on emigration behavior.

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 unlikely to be driving our results. A possible explanation for this is that households reporting that food is an emigration barrier are less likely to be members of any of these three groups, as shown in Appendix Table B8. This finding implies that programs targeting other potential barriers to emigration—such as cash transfers to poor households—would be unlikely to substitute for PDS access for food-concerned households.

6.3 Impacts on Economic Outcomes in the Family

We find few significant changes in economic outcomes for treated families, as shown in Table 8. Average treatment impacts on income, consumption, and food consumption at 4 months are close to, and statistically indistinguishable from, zero. At the 8-month survey, the average treatment impact on income is positive (5% increase, p-val $= 0.08$), and average impacts on consumption and remittances are small and positive (1–3% increase, p-vals $>$ 0.10). Pooled impacts on total income and consumption are small and positive (0.4–2% increase, p-vals > 0.10), and impacts on food consumption are modestly positive (3% increase,

[28] Appendix Section A.5 argues that impacts on emigration are most consistent with urban-to-rural destination switching. The choice to study urban-to-rural destination switching was not pre-specified.

Table 7: Heterogeneous Impacts on Emigrants' Destinations

Outcome: Emigrant to Urban Area	(1) Food Is a Migration Barrier	(2) Says Ration Is Not Portable	(3) Poor Households	(4) Poor Households (No Credit)	(5) Low-Wealth Households
Treatment × X	-0.116***	-0.064	-0.004	-0.012	0.006
	(0.041)	(0.042)	(0.030)	(0.036)	(0.029)
	[0.00]	[0.13]	[0.89]	[0.73]	[0.84]
Treatment	-0.046**	-0.024	-0.063***	-0.061***	-0.067***
	(0.019)	(0.031)	(0.020)	(0.021)	(0.024)
	[0.02]	[0.43]	[0.00]	[0.00]	[0.01]
X	0.021	0.053	0.038**	0.054***	-0.047**
	(0.019)	(0.036)	(0.017)	(0.020)	(0.018)
	[0.26]	[0.14]	[0.02]	[0.01]	[0.01]
q-Value: Treatment × $X = 0$	0.02	0.35	1.00	1.00	1.00
Observations	45,893	27,498	45,893	45,893	45,893

While the dimensions of heterogeneity, X, were pre-specified, the outcome in this table was not. An observation is an emigrant (excluding international emigrants) as of the 4-month follow-up (Feb–Apr 2022). The outcome variable is a binary indicator for whether the emigrant traveled to an urban area. Urban and rural destination categorizations are collected from survey data for emigrants who were household members in the previous wave; for emigrants who left and returned within the same survey round, they are inferred from the urban share of the destination district. Column titles show the dimension of heterogeneity, X, interacted with treatment in that column's regression. All heterogeneity dimensions are binary variables measured at baseline. *Food Is a Migration Barrier* equals 1 if the household reports finding food at the destination as one of the top three challenges a hypothetical migrant would face. *Says Ration Is Not Portable* equals 1 if the household reported it cannot claim ration outside its designated shop. *Poor Households* are those in the bottom 40% of per-adult-equivalent household consumption in our sample. *Poor Households (No Credit)* adds the additional restriction that the household does not have an outstanding loan. *Low-Wealth Households* are those in the bottom 40% of the first principal component of a set of 12 durable asset measures. All regressions include a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable, and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Sample in Column 2 is restricted to respondents who were asked updated beliefs questions (see Section 4.5). 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 each heterogeneous treatment impact tests. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

p-val = 0.06), as shown in Appendix Table E3. We find no significant treatment effects on other economic outcomes, including food security among baseline emigrants.

That the shift from urban to rural destinations 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 migrating to urban areas. In light of the large urban-rural consumption gaps in India, this suggests either that the returns to emigrating to an urban destination (compared to a rural one) are low for this group, or that barriers to ration access impeded labor market outcomes once these emigrants arrived in the destination.

Table 8: Treatment Impacts on Income, Consumption, and Remittances

	(1) Total Income	(2) Total Consumption	(3) Food Consumption	(4) Remittances
4-Month Impacts				
Treatment	-0.006	-0.004	0.020	0.012
	(0.027)	(0.013)	(0.016)	(0.015)
	[0.82]	[0.77]	[0.22]	[0.40]
Outcome Mean in Control	279	168	50	6
Observations	52,902	52,902	52,902	52,902
8-Month Impacts				
Treatment	0.048*	0.011	0.025	0.023
	(0.027)	(0.015)	(0.017)	(0.020)
	[0.08]	[0.45]	[0.14]	[0.25]
Outcome Mean in Control	259	171	48	6
Observations	45,351	45,351	45,351	45,351

An observation is a family (household + emigrants). Monetary values are measured in USD/month and transformed using the inverse hyperbolic sine function. *Total Income* is the sum of monthly individual wage income for all household members, agricultural self-production, and household business profit (averaged over the preceding four months) added to wage, salary, casual labor earnings, and business profits earned by emigrants over the preceding month. *Total Consumption* is computed by adding average monthly household expenditure (over the preceding four months) to the preceding month's value of each emigrant's expenditure. *Food Consumption* restricts to food expenditure only. *Remittances* are monetary transfers from all emigrants to the household, measured over the preceding month. Four- and eight-month results are measured in follow-up surveys in Feb–Apr 2022 and Jun–Aug 2022 respectively. All regressions include a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable, and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

7 Discussion

This paper presents findings from an audit study and a cluster-randomized controlled trial implemented across 18 Indian states. We randomly provided information about a new scheme permitting food ration portability across India, which reduced beliefs about ration portability four months after our trial began. Our experiment increased emigration to rural areas while decreasing it to urban areas, with no persistent effects after eight months.

Our evidence is most consistent with a food security mechanism, in which switching from an urban to a rural destination is driven by concerns about finding food in the destination given anticipated difficulties accessing ration there. Reliable access to food provides an insurance value to migrants against bad migration outcomes, and our findings suggest that this insurance value influences whether migrants travel to urban or rural areas.

Ours is likely not the only intervention to have been, in effect, scaled up by an external agent—in our case, by state governments—during the course of the experiment. While the concurrence of our campaign and the governments' complicated our analysis, we exploited differences between the two campaigns to study the impact of beliefs on emigration. Researchers anticipating large-scale campaigns that interact with their own information experiments may find it helpful to collect data—indirectly, if necessary—on which aspects of the information the respondents found to be trustworthy, or paid the most attention to. This is likely to be especially important in contexts where *de jure* rules diverge from respondents' expectations of access in practice. In such a case, formulating a plan to separately measure awareness and trust impacts—following the logic of Section 5.3—may prove useful.

Migrating to search for a job is costly and risky—especially in urban areas—and prospective emigrants may be deterred if they cannot insure themselves against consumption risk. One interpretation of the modest positive treatment impacts on income and consumption as of the 8-month survey is that there was a higher rate of "failed migration" in the control arm—perhaps because of difficulty accessing food ration in urban destinations—a possibility that we believe future work could assess. 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 emigrants must be reasonably confident that *de jure* ration portability will function in practice. These findings highlight the importance of reliable access to food ration even outside of PDS beneficiaries' designated ration shops.

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Online Appendix for "Social Welfare Portability and Migration: Evidence from the Indian Public Distribution System"

Contents

A Treatment Effect Decompositions

A.1 Decomposing Trust Impacts Into Direct Effects and a Selection Effect

We show that impacts on trust can be decomposed into two direct effects and a selection effect within the framework introduced in Section 5.3. For a change in trust from period t to $t+1$, we obtain:

$$\begin{aligned}\tau_{t+1} - \tau_t = &\frac{\alpha_t}{\alpha_{t+1}} E[Trust_{t+1}|Aware_t = 1] \\ &+ \frac{\alpha_{t+1} - \alpha_t}{\alpha_{t+1}} E[Trust_{t+1}|Aware_t = 0,\, Aware_{t+1} = 1] \\ &- E[Trust_t|Aware_t = 1],\end{aligned}$$

noting that τ_{t+1} is a weighted average of trust in period $t+1$ in the sets of people already aware of ONORC in period t, and those newly aware of ONORC in period $t+1$. Breaking

the last term into two and rearranging gives:

$$\begin{aligned}\tau_{t+1} - \tau_t = &\frac{\alpha_t}{\alpha_{t+1}} E[Trust_{t+1} - Trust_t | Aware_t = 1] \\ &+ \frac{\alpha_{t+1} - \alpha_t}{\alpha_{t+1}} \left(E[Trust_{t+1} | Aware_t = 0,\, Aware_{t+1} = 1] - E[Trust_t | Aware_t = 1] \right).\end{aligned}$$

Finally, adding and subtracting $E[Trust_t | Aware_t = 0,\, Aware_{t+1} = 1]$ gives:

$$\begin{aligned}\tau_{t+1} - \tau_t = &\frac{\alpha_t}{\alpha_{t+1}} E[Trust_{t+1} - Trust_t | Aware_t = 1] \\ &+ \frac{\alpha_{t+1} - \alpha_t}{\alpha_{t+1}} E[Trust_{t+1} - Trust_t | Aware_t = 0,\, Aware_{t+1} = 1] \\ &+ \frac{\alpha_{t+1} - \alpha_t}{\alpha_{t+1}} \left(E[Trust_t | Aware_t = 0,\, Aware_{t+1} = 1] - E[Trust_t | Aware_t = 1] \right).\end{aligned}$$

The first line represents the direct impact on trust among those already aware of ONORC at time t. The second line represents the direct impact on trust among those newly aware of ONORC at time $t+1$. The third line represents the selection effect on average trust created by any differences between (latent) trust at time t between those newly aware of ONORC at time $t+1$ and those already aware at time t.

A.2 A Decline in Portability Beliefs Implies a Negative Direct Effect on Trust Among Those Already Aware of the ONORC Program

We show that negative treatment impacts on portability beliefs imply negative direct effects on trust among those already aware of the ONORC program, assuming treatment impacts on awareness are non-negative. We begin by expressing a change in average portability beliefs from time t to $t+1$ following the notation of Table 4, then plug in using the expression for $\tau_{t+1} - \tau_t$ obtained in Section A.1:

$$\begin{aligned}E[Port_{i,t+1} - Port_{i,t}] &= (\alpha_{t+1} - \alpha_t)\tau_t + (\tau_{t+1} - \tau_t)\alpha_t + (\alpha_{t+1} - \alpha_t)(\tau_{t+1} - \tau_t) \\ &= (\tau_{t+1} - \tau_t)\alpha_{t+1} + (\alpha_{t+1} - \alpha_t)\tau_t \\ &= \alpha_t E[Trust_{t+1} - Trust_t | Aware_t = 1] \\ &\quad + (\alpha_{t+1} - \alpha_t) E[Trust_{t+1} - Trust_t | Aware_t = 0,\, Aware_{t+1} = 1] \\ &\quad + (\alpha_{t+1} - \alpha_t) \left(E[Trust_t | Aware_t = 0,\, Aware_{t+1} = 1] - E[Trust_t | Aware_t = 1] \right) \\ &\quad + (\alpha_{t+1} - \alpha_t) E[Trust_t | Aware_t = 1].\end{aligned}$$

Simplifying yields:

$$\begin{aligned}E[Port_{i,t+1} - Port_{i,t}] = &\alpha_t E[Trust_{t+1} - Trust_t | Aware_t = 1] \\ &+ (\alpha_{t+1} - \alpha_t) E[Trust_{t+1} | Aware_t = 0,\, Aware_{t+1} = 1].\end{aligned}$$

That is, a change in portability beliefs is equal to the direct effect on trust among those

already aware of ONORC plus mean post-treatment trust among those newly aware of ONORC, with terms weighted by the respective population shares α_t and $(\alpha_{t+1} - \alpha_t)$.

Now, assuming $(\alpha_{t+1} - \alpha_t) \geq 0$, the second term in the above expression is non-negative. Therefore, $E[Port_{i,t+1}] < E[Port_{it}]$ can only come about by $E[Trust_{t+1} - Trust_t | Aware_t = 1] < 0$, that is, a negative direct effect on trust among those already aware of the ONORC program at time t.

A.3 Identifying Trust Impacts

The treatment impact on average portability beliefs at $t = 2$ is given by:

$$(\alpha_0 + \Delta_\alpha^{BOTH}) \times (\tau_0 + \Delta_\tau^{BOTH}) - (\alpha_0 + \Delta_\alpha^{GOV}) \times (\tau_0 + \Delta_\tau^{GOV}).$$

Applying Assumption 2, that $\Delta_\alpha^{GOV} = \Delta_\alpha^{BOTH}$, and pulling out $\alpha_0 + \Delta_\alpha^{BOTH} = \alpha_2$ gives:

$$\alpha_2 \times (\Delta_\tau^{BOTH} - \Delta_\tau^{GOV}).$$

Applying the decomposition of Δ_τ from Section A.1 gives:

$$\begin{aligned}
&\alpha_2 \times \frac{\alpha_0}{\alpha_2} E[Trust_2^{BOTH} - Trust_0 | Aware_0 = 1] \\
&+ \alpha_2 \times \frac{\alpha_2 - \alpha_0}{\alpha_2} E[Trust_2^{BOTH} - Trust_0 | Aware_0 = 0,\ Aware_2^{BOTH} = 1] \\
&+ \alpha_2 \times \frac{\alpha_2 - \alpha_0}{\alpha_2} \left(E[Trust_0 | Aware_0 = 0,\ Aware_2^{BOTH} = 1] - E[Trust_0 | Aware_0 = 1] \right) \\
&- \alpha_2 \times \frac{\alpha_0}{\alpha_2} E[Trust_2^{GOV} - Trust_0 | Aware_0 = 1] \\
&- \alpha_2 \times \frac{\alpha_2 - \alpha_0}{\alpha_2} E[Trust_2^{GOV} - Trust_0 | Aware_0 = 0,\ Aware_2^{GOV} = 1] \\
&- \alpha_2 \times \frac{\alpha_2 - \alpha_0}{\alpha_2} \left(E[Trust_0 | Aware_0 = 0,\ Aware_2^{GOV} = 1] - E[Trust_0 | Aware_0 = 1] \right).
\end{aligned}$$

The fourth and fifth lines are both zero by Assumption 1: that there were no direct impacts on trust of government campaigns. The third and sixth lines cancel: the selection component of the trust impacts are equal for the government and combined campaigns by virtue of their equal impacts on awareness (Assumption 2). This leaves:

$$\begin{aligned}
&\alpha_0 E[Trust_2^{BOTH} - Trust_0 | Aware_0 = 1] \\
&+ \Delta_\alpha^{BOTH} E[Trust_2^{BOTH} - Trust_0 | Aware_0 = 0,\ Aware_2^{BOTH} = 1].
\end{aligned}$$

This final expression represents the sum of two direct effects on trust: one on those already aware of ONORC at $t = 0$ and one on those newly aware of ONORC at $t = 2$).

A.4 Identifying Awareness Impacts

Under Assumption 3—that changes in awareness and trust in the control group are due solely to government campaigns—the change in portability beliefs from $t = 1$ to $t = 2$ in the control group identifies the impact of the government campaign:

$$\begin{aligned}
&(\alpha_0 + \Delta_\alpha^{GOV}) \times (\tau_0 + \Delta_\tau^{GOV}) - \alpha_0 \times \tau_0 \\
&= \Delta_\tau^{GOV}(\alpha_0 + \Delta_\alpha^{GOV}) + \tau_0 \Delta_\alpha^{GOV} \\
&= \Delta_\alpha^{GOV} \left(E[Trust_0 | Aware_0 = 0,\, Aware_2^{GOV} = 1] - \tau_0 \right) + \tau_0 \Delta_\alpha^{GOV} \\
&= \Delta_\alpha^{GOV} E[Trust_0 | Aware_0 = 0,\, Aware_2^{GOV} = 1],
\end{aligned}$$

where the third line follows by plugging in the decomposition of $\Delta_\tau^{GOV} \equiv \tau_2 - \tau_1$ from Section A.1 and applying Assumption 1 (that direct effects on trust are zero).

A.5 Decomposing Impacts on Emigration by Destination Type

We show that treatment impacts on emigration overall, emigration to rural destinations, and emigration to urban destinations (as shown in Table 6) jointly imply a positive treatment impact either on urban-to-rural switching or on non-emigrants switching to rural destinations, and argue that our impacts on beliefs are most consistent with the former.

Consider a decomposition of the population into nine "response types" based on three potential emigration outcomes—rural, urban, and non-emigration—which depend on a person's treatment status—treated or untreated. Let G_{XY} denote the gross flows per capita represented by the response type choosing destination X when untreated and Y when treated, with $X, Y \in \{R, U, N\}$ denoting a rural destination, urban destination, or non-emigration. For example, G_{NU} is the number of people per household who switch from non-emigration to an urban destination when treated. There are nine response types. Let $N_{XY} \equiv G_{XY} - G_{YX}$ denote net flows. For example, $N_{UR} = G_{UR} - G_{RU}$ is the number of people per household induced to travel to a rural destination instead of an urban destination minus the number induced to travel to an urban destination instead of a rural destination.

The estimate on any emigration (Table 6 Column 1) identifies (the equations below omit always-takers who appear in both treatment conditions):

$$\begin{aligned}
&\text{(Migrants if Treated)} - \text{(Migrants if Untreated)} \\
&= (G_{NU} + G_{NR}) - (G_{UN} + G_{RN}) \\
&= N_{NU} + N_{NR}.
\end{aligned} \tag{3}$$

The estimate on emigration to urban areas (Table 6 Column 2) identifies:

$$\begin{aligned}
&\text{(Emigrants to Urban Areas if Treated)} - \text{(Emigrants to Urban Areas if Untreated)} \\
&= (G_{NU} + G_{RU}) - (G_{UR} + G_{UN}) \\
&= N_{NU} - N_{UR}.
\end{aligned} \tag{4}$$

The estimate on emigration to rural areas (Table 6 Column 3) identifies:

$$\begin{aligned}&\text{(Emigrants to Rural Areas if Treated)} - \text{(Emigrants to Rural Areas if Untreated)}\\ &= (G_{NR} + G_{UR}) - (G_{RU} + G_{RN})\\ &= N_{NR} + N_{UR}.\end{aligned} \tag{5}$$

Subtracting (4) from (5) and plugging in $-N_{NU} = N_{NR} + 0.010$ from (3) gives:

$$\begin{aligned}N_{NR} - N_{NU} + 2N_{UR} &= 0.107\\ \implies N_{NR} + N_{UR} &= 0.049.\end{aligned}$$

That is, our treatment either increased net urban-to-rural switching, N_{UR}, or it increased net staying-to-rural switching, N_{NR}, while also increasing net urban-to-staying switching. However, a positive effect on N_{NR} is difficult to reconcile with the decrease in portability beliefs, which should decrease the perceived return to emigrating regardless of destination (but more so for urban destinations). Our finding that treatment did not impact selection into emigration (see Appendix Table B7) also suggests that treatment did not lead to both staying-to-rural switching and urban-to-staying switching in different sets of people. This leads us to conclude that the findings of Table 6 are driven by urban-to-rural switching.

B Additional Tables

Table B1: Correlates of Baseline Portability Beliefs

	(1)	(2)	(3)
	Believes Their Ration is Portable:		
	Somewhere	Across Districts	Across States
Household Size			0.00
Highest Education			0.01
OBC/ST/SC Caste	-0.01		-0.00
Literacy Rate	0.01	0.00	0.01
# of Current Migrants	-0.00		
Household Income	0.02	0.04	
Total Consumption			
Food Consumption	0.01	0.01	
Employment Rate	-0.01	-0.01	-0.01
Outcome Mean	0.33	0.22	0.17
Observations	36,732	36,732	36,732

An observation is a family (household + emigrants) at baseline. Ration portability beliefs are binary variables indicating whether the respondent replied "Yes" to the question "This question is about food ration claimed through the Public Distribution System. Can you use your ration card at ration shops other than your designated shop?" and two analogous follow-up questions about out-of-district and out-of-state shops respectively. If a person did not answer "Yes" to one of these questions, we code the following questions as "No." Each column shows post-lasso OLS coefficients from a lasso regression (Belloni and Chernozhukov, 2013). All continuous variables are standardized to mean 0, standard deviation 1. Sample is restricted to respondents who were asked updated beliefs questions (see Section 4.5).

Table B2: Treatment Impacts on Portability Beliefs (No Controls)

	(1)	(2)	(3)
	Believes Their Ration is Portable:		
	Somewhere	Across Districts	Across States
Immediate Impacts			
Treatment	0.201***	0.145***	0.155***
	(0.044)	(0.039)	(0.036)
	[0.00]	[0.00]	[0.00]
Outcome Mean in Control	0.35	0.26	0.20
Observations	36,776	36,776	36,776
4-Month Impacts			
Treatment	-0.087***	-0.099***	-0.072**
	(0.033)	(0.035)	(0.034)
	[0.01]	[0.01]	[0.03]
Outcome Mean in Control	0.55	0.43	0.37
Observations	48,297	48,297	48,297

An observation is a family (household + emigrants). Ration portability beliefs are binary variables indicating whether the respondent replied "Yes" to the question "This question is about food ration claimed through the Public Distribution System. Can you use your ration card at ration shops other than your designated shop?" and two analogous follow-up questions about out-of-district and out-of-state shops respectively. If a person did not answer "Yes" to one of these questions, we code the following questions as "No." Immediate impacts are measured in the baseline survey (Oct–Dec 2021) after information is provided; 4-month results are measured in a follow-up survey (Feb–Apr 2022). Baseline sample is restricted to respondents who were asked updated beliefs questions (see Section 4.5). 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B3: Treatment Impacts on Portability Beliefs (Balanced Sample)

	(1)	(2)	(3)
	Believes Their Ration is Portable:		
	Somewhere	Across Districts	Across States
Immediate Impacts			
Treatment	0.246***	0.218***	0.222***
	(0.033)	(0.031)	(0.031)
	[0.00]	[0.00]	[0.00]
Outcome Mean in Control	0.38	0.28	0.23
Observations	28,510	28,510	28,510
4-Month Impacts			
Treatment	-0.086**	-0.086**	-0.050
	(0.037)	(0.038)	(0.036)
	[0.02]	[0.02]	[0.16]
Outcome Mean in Control	0.56	0.45	0.38
Observations	28,510	28,510	28,510

An observation is a family (household + emigrants). Ration portability beliefs are binary variables indicating whether the respondent replied "Yes" to the question "This question is about food ration claimed through the Public Distribution System. Can you use your ration card at ration shops other than your designated shop?" and two analogous follow-up questions about out-of-district and out-of-state shops respectively. If a person did not answer "Yes" to one of these questions, we code the following questions as "No." Immediate impacts are measured in the baseline survey (Oct–Dec 2021) after information is provided; 4-month results are measured in a follow-up survey (Feb–Apr 2022). Both the baseline and the 4-month samples are restricted to respondents who were asked updated beliefs questions at baseline (see Section 4.5), were surveyed at both baseline and followup, and had a ration card linked to Aadhar in the follow-up survey. All regressions include a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable, and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Pre-treatment beliefs y_{ic0} in the control group are imputed to be the same as "immediate post-intervention" beliefs y_{ic1}. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B4: Transition Matrices of Beliefs About Ration Portability

	Pre-Intervention Belief (Treatment Group):			
	Nowhere	In-District Only	In-State Only	Anywhere
Post-Intervention Belief (%):				
Nowhere	63	7	1	12
In-District Only	9	70	6	3
In-State Only	3	4	27	1
Anywhere	25	19	66	84
Observations	12,648	2,421	854	2,415

	Post-Intervention Belief (Control Group):			
	Nowhere	In-District Only	In-State Only	Anywhere
Follow-Up Belief (%):				
Nowhere	55	39	43	16
In-District Only	7	29	21	13
In-State Only	9	5	5	3
Anywhere	28	27	31	69
Observations	8,837	1,473	775	3,263

	Post-Intervention Belief (Treatment Group):			
	Nowhere	In-District Only	In-State Only	Anywhere
Follow-Up Belief (%):				
Nowhere	72	49	69	38
In-District Only	11	16	13	11
In-State Only	3	5	1	2
Anywhere	14	31	16	49
Observations	6,020	2,274	632	5,236

An observation is a family (household + emigrants). Ration portability beliefs are binary variables indicating whether the respondent replied "Yes" to the question "This question is about food ration claimed through the Public Distribution System. Can you use your ration card at ration shops other than your designated shop?" and two analogous follow-up questions about out-of-district and out-of-state shops respectively. If a person did not answer "Yes" to one of these questions, we code the following questions as "No." Each panel shows a transition matrix of beliefs about ration portability from right before to right after treatment (measured in the baseline survey, treatment group only) or right after treatment to the follow-up survey (shown separately for treatment and control groups). Beliefs are categorized into four groups: believes ration is not portable, believes it is portable within district but not within state, believes it is portable within state but not outside of state lines, and believes it is portable outside of state lines. Each cell shows the percent of respondents in a given belief category; column totals within a panel sum to 100 (net of rounding). Pre- and post-intervention beliefs are measured in the baseline survey (Oct–Dec 2021); follow-up beliefs are measured in the 4-month follow-up survey (Feb–Apr 2022). Baseline sample is restricted to respondents who were asked updated beliefs questions (see Section 4.5).

Table B5: Treatment Impacts on Portability Beliefs (In States Where Assumptions 1 and 2 Are Likely to Hold)

	(1)	(2)	(3)
	Believes Their Ration is Portable:		
	Somewhere	Across Districts	Across States
Immediate Impacts			
Treatment	0.386***	0.392***	0.470***
	(0.073)	(0.077)	(0.074)
	[0.00]	[0.00]	[0.00]
Outcome Mean in Control	0.42	0.37	0.33
Observations	6,837	6,837	6,837
4-Month Impacts			
Treatment	-0.051	-0.046	-0.053
	(0.049)	(0.050)	(0.052)
	[0.30]	[0.36]	[0.31]
Outcome Mean in Control	0.89	0.78	0.77
Observations	8,415	8,415	8,415

An observation is a family (household + emigrants). Ration portability beliefs are binary variables indicating whether the respondent replied "Yes" to the question "This question is about food ration claimed through the Public Distribution System. Can you use your ration card at ration shops other than your designated shop?" and two analogous follow-up questions about out-of-district and out-of-state shops respectively. If a person did not answer "Yes" to one of these questions, we code the following questions as "No." Immediate impacts are measured in the baseline survey (Oct–Dec 2021) after information is provided; 4-month results are measured in a follow-up survey (Feb–Apr 2022). Baseline sample is restricted to respondents who were asked updated beliefs questions (see Section 4.5). Sample includes the four states with the highest out-of-sample beliefs about across-state ration portability, as shown in Appdendix Figure D5—Delhi, Karnataka, Punjab, and Tamil Nadu. All regressions include a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable, and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Pre-treatment beliefs y_{ic0} in the control group are imputed to be the same as "immediate post-intervention" beliefs y_{ic1}. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B6: Baseline Correlates of Predictors of Treatment Effect Heterogeneity

	(1) Food Is a Migration Barrier	(2) Says Ration Is Not Portable	(3) Poor Household	(4) Poor Household (No Credit)	(5) Low-Wealth Households	(6) Had Any Emigrant at Baseline
Household Size	-0.02		0.24	0.12	0.01	-0.04
Highest Education			-0.06	-0.02	-0.06	-0.03
OBC/ST/SC Caste		0.01	0.05	0.00	0.07	0.01
Literacy Rate	0.00	-0.01	-0.01	-0.02		
# of Current Migrants	-0.00	0.00	0.01	0.02	-0.01	
Household Income	0.01	-0.02			-0.05	-0.00
Total Consumption					-0.09	-0.03
Food Consumption	0.01	-0.01			-0.06	
Employment Rate		0.01	-0.03	-0.03	0.02	-0.02
Outcome Mean	0.19	0.67	0.40	0.23	0.55	0.33
Observations	62,074	36,732	62,074	62,074	62,074	62,074

An observation is a family (household + emigrants) at baseline. Each column shows post-lasso OLS coefficients from a lasso regression (Belloni and Chernozhukov, 2013). *Food Is a Migration Barrier* equals 1 if the household reports finding food at the destination as one of the top three challenges a hypothetical migrant would face. *Says Ration Is Not Portable* equals 1 if the household reported it cannot claim ration outside its designated shop. *Poor Households* are those in the bottom 40% of per-adult-equivalent household consumption in our sample. *Poor Households (No Credit)* adds the additional restriction that the household does not have an outstanding loan. *Low-Wealth Households* are those in the bottom 40% of the first principal component of a set of 12 durable asset measures. *Had Any Emigrant at Baseline* equals 1 if the household had non-zero emigrants as of the baseline survey. All continuous variables are standardized to mean 0, standard deviation 1. Sample in Column 2 is restricted to respondents who were asked updated beliefs questions (see Section 4.5). Consumption and income variables are excluded from the regression predicting *Poor Household* and *Poor Household (No Credit)*; number of migrants is excluded from the regression predicting *Had Any Emigrant at Baseline*.

Table B7: 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 p-Value
Age (Years)	25.76	25.88	0.86
	(43.07)	(45.07)	
Education (Years)	7.69	7.62	0.71
	(13.42)	(15.17)	
Head of Household = 1	0.06	0.07	0.60
	(0.69)	(0.69)	
Literate	0.95	0.95	0.95
	(0.34)	(0.38)	
OBC/ST/SC Caste	0.65	0.67	0.94
	(2.38)	(2.06)	
Ever Migrated	0.94	0.94	0.80
	(1.40)	(1.89)	
Emigrant at Baseline	0.93	0.93	0.88
	(1.53)	(1.95)	
Emigrant to Urban Area at Baseline	0.66	0.62	0.22
	(4.14)	(4.16)	
Observations	23,942	22,413	46,355
Clusters	617	664	1,281

An observation is an emigrant in the 4-month follow-up survey. Baseline characteristics are measured in the baseline survey (Oct–Dec 2021); 4-month results are measured in a follow-up survey (Feb–Apr 2022). First two columns show means of baseline characteristics within control and treatment emigrants, 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B8: Correlations Between Predictors of Treatment Effect Heterogeneity

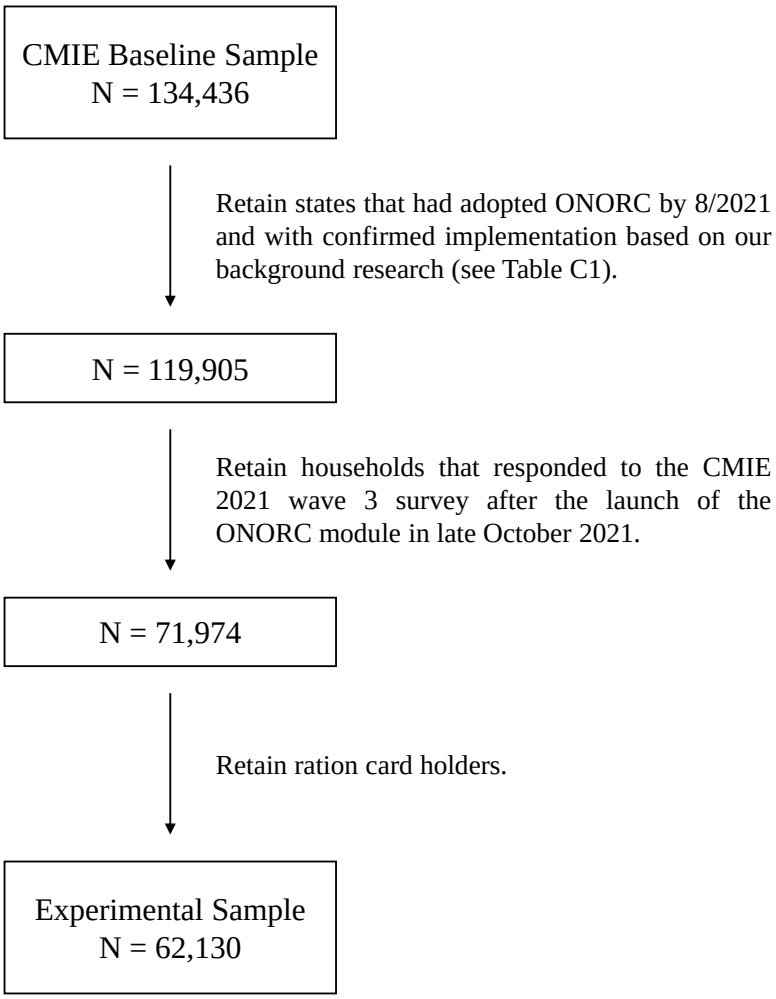
	(1) Says Ration Is Not Portable	(2) Food Is a Migration Barrier	(3) Poor Household	(4) Poor Household (No Credit)
Food Is a Migration Barrier	0.00			
	(0.01)			
	[0.74]			
Poor Household	0.07***	-0.07***		
	(0.00)	(0.00)		
	[0.00]	[0.00]		
Poor Household (No Credit)	0.01**	-0.05***	0.78***	
	(0.01)	(0.00)	(0.00)	
	[0.01]	[0.00]	[0.00]	
Low-Wealth Household	0.12***	-0.07***	0.20***	0.08***
	(0.00)	(0.00)	(0.00)	(0.00)
	[0.00]	[0.00]	[0.00]	[0.00]
Outcome Mean	0.67	0.19	0.40	0.23
Observations	36,776	62,130	62,130	62,130

An observation is a family (household + emigrants) at baseline. Each cell shows the coefficient from a bivariate regression of two pre-specified predictors of treatment effect heterogeneity. *Food Is a Migration Barrier* equals 1 if the household reports finding food at the destination as one of the top three challenges a hypothetical migrant would face. *Says Ration Is Not Portable* equals 1 if the household reported it cannot claim ration outside its designated shop. *Poor Households* are those in the bottom 40% of per-adult-equivalent household consumption in our sample. *Poor Households (No Credit)* adds the additional restriction that the household does not have an outstanding loan. *Low-Wealth Households* are those in the bottom 40% of the first principal component of a set of 12 durable asset measures. Sample in Column 1 is restricted to respondents who were asked updated beliefs questions (see Section 4.5). Heteroskedasticity-robust standard errors in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C Additional Details on Experimental Design

C.1 Sample and Randomization

Figure C1: Summary of Sample Selection Process



Details on Randomization. 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.

Table C1: Summary of Pre-Experimental Research Into Ration Portability

State:	Adopted ONORC by Aug 2021	Shop Owner Surveys	Mig. Surveys and 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
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. *Mig. Surveys and Mystery Shoppers* indicates whether we surveyed migrants with out-of-state or out-of-district ration cards, and sent mystery shoppers with eligible ration cards to attempt to claim ration in a different district or 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 (Department of Food & Public Distribution, 2021). *Experimental Sample* indicates whether clusters located in that state were assigned to a treatment or control condition.

Table C2: Test of Differential Attrition

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

An observation is a family (household + emigrants). *Surveyed* is equal to 1 if the family was surveyed during the given round, and 0 otherwise. *Surveyed Individual by Phone* is equal to 1 if at least one emigrant from the family sampled for a phone survey was successfully surveyed, and 0 otherwise (see 4.2 for details on emigrant phone survey sampling). Four- and eight-month results are measured in follow-up surveys in Feb–Apr 2022 and Jun–Aug 2022 respectively. All regressions include a randomization-stratum fixed effect. Pooled impacts measured using ANCOVA regression including a survey-wave fixed effect. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C3: Baseline Sample Statistics and Randomization Balance

Variable	(1) Control Mean/(SD)	(2) Treatment Mean/(SD)	(1)-(2) Pairwise t-Test p-Value
Highest Education (Years)	11.32	11.18	0.82
	(18.31)	(16.99)	
Household Size	3.72	3.77	0.76
	(8.44)	(7.99)	
# of Adult-Equivalents	3.51	3.55	0.80
	(7.53)	(7.14)	
OBC/ST/SC Castes, % of Household	0.67	0.68	0.74
	(2.59)	(2.15)	
Literacy, % of Adults	0.99	0.99	0.12
	(0.17)	(0.38)	
Ever Migrated	0.56	0.56	0.94
	(1.98)	(1.92)	
Any Current Migrant	0.32	0.33	0.92
	(1.83)	(1.87)	
# of Current Migrants	0.94	0.91	0.33
	(6.25)	(5.82)	
# of Current Urban Migrants	0.63	0.56	0.12
	(6.83)	(6.23)	
# of Current Inter-State Migrants	0.02	0.03	0.08*
	(0.51)	(1.52)	
Household Income (USD/Month)	263	253	0.75
	(1,508)	(1,183)	
Total Consumption (USD/Month)	159	158	0.59
	(583)	(619)	
Food Consumption (USD/Month)	45	46	0.12
	(147)	(168)	
Employment, % of Adults	0.40	0.41	0.80
	(0.90)	(0.80)	
Has a Bank Account	1.00	1.00	0.38
	(0.01)	(0.01)	
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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C4: Randomization Balance (Among Households Surveyed at 4-Month Follow-Up)

Variable	(1) Control Mean/(SD)	(2) Treatment Mean/(SD)	(1)-(2) Pairwise t-Test p-Value
Highest Education (Years)	11.38	11.24	0.71
	(16.91)	(16.16)	
Household Size	3.74	3.78	0.82
	(8.23)	(7.67)	
# of Adult-Equivalents	3.52	3.56	0.83
	(7.35)	(6.83)	
OBC/ST/SC Castes, % of Household	0.67	0.69	0.58
	(2.47)	(2.06)	
Literacy, % of Adults	0.99	0.99	0.08*
	(0.17)	(0.38)	
Ever Migrated	0.57	0.57	0.89
	(1.91)	(1.91)	
Any Current Migrant	0.33	0.33	0.99
	(1.81)	(1.90)	
# of Current Migrants	0.95	0.93	0.40
	(6.21)	(5.92)	
# of Current Urban Migrants	0.65	0.59	0.17
	(6.72)	(6.19)	
# of Current Inter-State Migrants	0.02	0.03	0.08*
	(0.51)	(1.41)	
Household Income (USD/Month)	271	259	0.54
	(1,480)	(1,191)	
Total Consumption (USD/Month)	160	1560	0.48
	(556)	(619)	
Food Consumption (USD/Month)	46	46	0.08*
	(144)	(166)	
Employment, % of Adults	0.40	0.41	0.82
	(0.82)	(0.79)	
Has a Bank Account	1.00	1.00	0.79
	(0.01)	(0.01)	
Observations	26,852	26,050	52,902
Clusters	735	760	1,495

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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C5: Randomization Balance (Among Households Surveyed at 8-Month Follow-Up)

Variable	(1) Control Mean/(SD)	(2) Treatment Mean/(SD)	(1)-(2) Pairwise t-Test p-Value
Highest Education (Years)	11.35	11.25	0.85
	(16.70)	(16.07)	
Household Size	3.74	3.79	0.96
	(8.21)	(7.21)	
# of Adult-Equivalents	3.53	3.57	0.98
	(7.33)	(6.45)	
OBC/ST/SC Castes, % of Household	0.67	0.69	0.35
	(2.25)	(2.00)	
Literacy, % of Adults	0.99	0.99	0.18
	(0.18)	(0.32)	
Ever Migrated	0.58	0.58	0.79
	(1.84)	(1.78)	
Any Current Migrant	0.33	0.33	0.93
	(1.74)	(1.84)	
# of Current Migrants	0.97	0.94	0.35
	(6.05)	(5.81)	
# of Current Urban Migrants	0.67	0.60	0.13
	(6.70)	(6.23)	
# of Current Inter-State Migrants	0.02	0.04	0.08*
	(0.49)	(1.63)	
Household Income (USD/Month)	265	256	0.71
	(1,409)	(1,104)	
Total Consumption (USD/Month)	159	158	0.59
	(552)	(568)	
Food Consumption (USD/Month)	45	46	0.12
	(142)	(156)	
Employment, % of Adults	0.40	0.41	0.35
	(0.83)	(0.73)	
Has a Bank Account	1.00	1.00	0.86
	(0.01)	(0.01)	
Observations	22,980	22,371	45,351
Clusters	641	651	1,292

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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.2 Information Intervention

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 C2: Information Sheet (English)

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

Your unique code for access is:

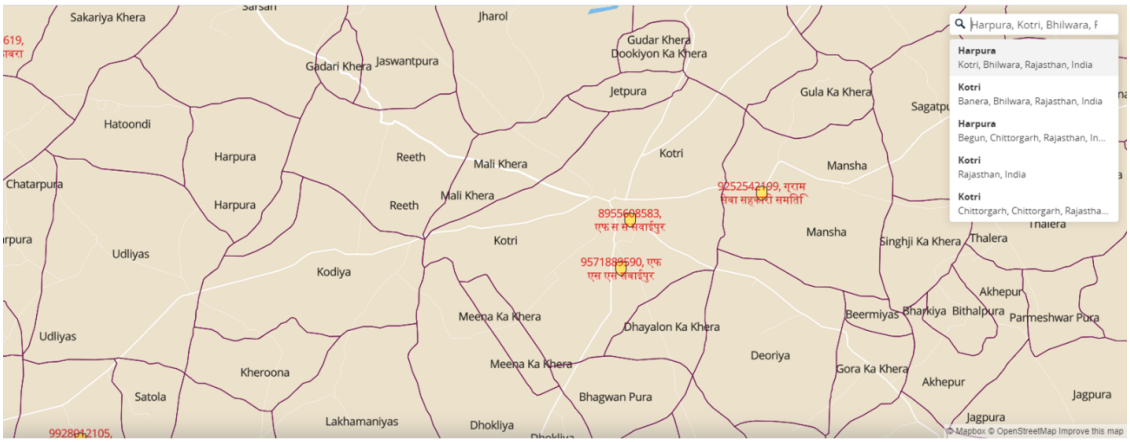
If you encounter difficulties claiming ration, contact the local government office in charge of public distribution or consumer protection. You can also call 14445 to report any grievances to the government, or with other questions about the ONORC program.

Table C6: 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

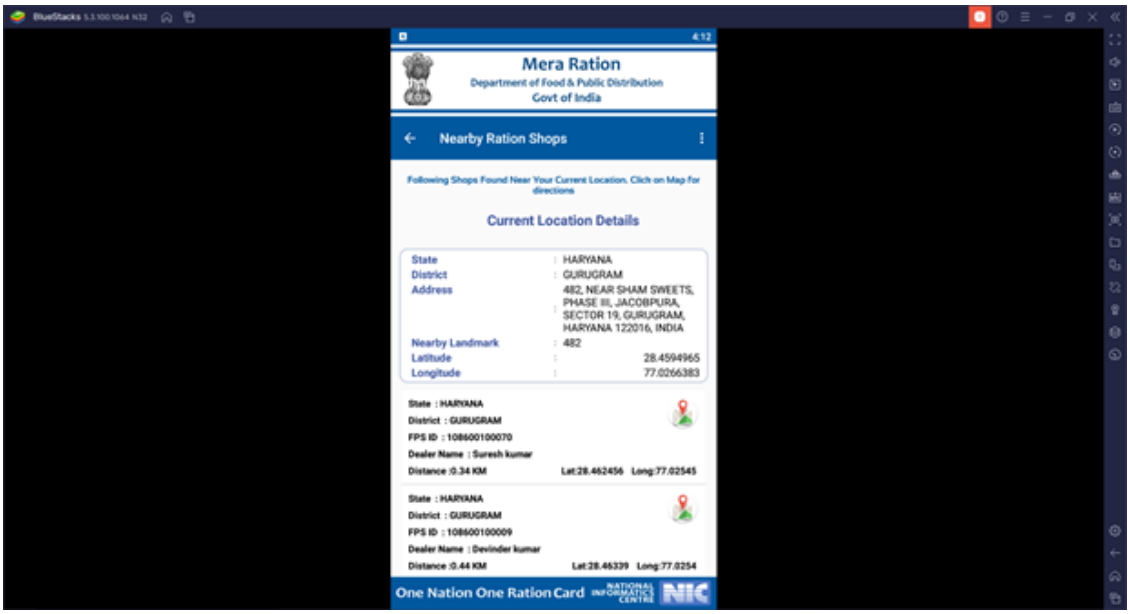
This table shows which information was provided through the hotline as a function of which state the caller was interested in. *Location of Shops* refers to addresses of ration shops. *Phone Numbers of Shops* refers to phone numbers of ration shops. *General Info About ONORC* refers to the information contained in the CMIE script, which the hotline staff can repeat. *Experimental Sample* indicates whether households located in that state were assigned to a treatment or control condition.

Figure C3: Map Interface Used by Hotline Staff to Identify Ration Shop Locations



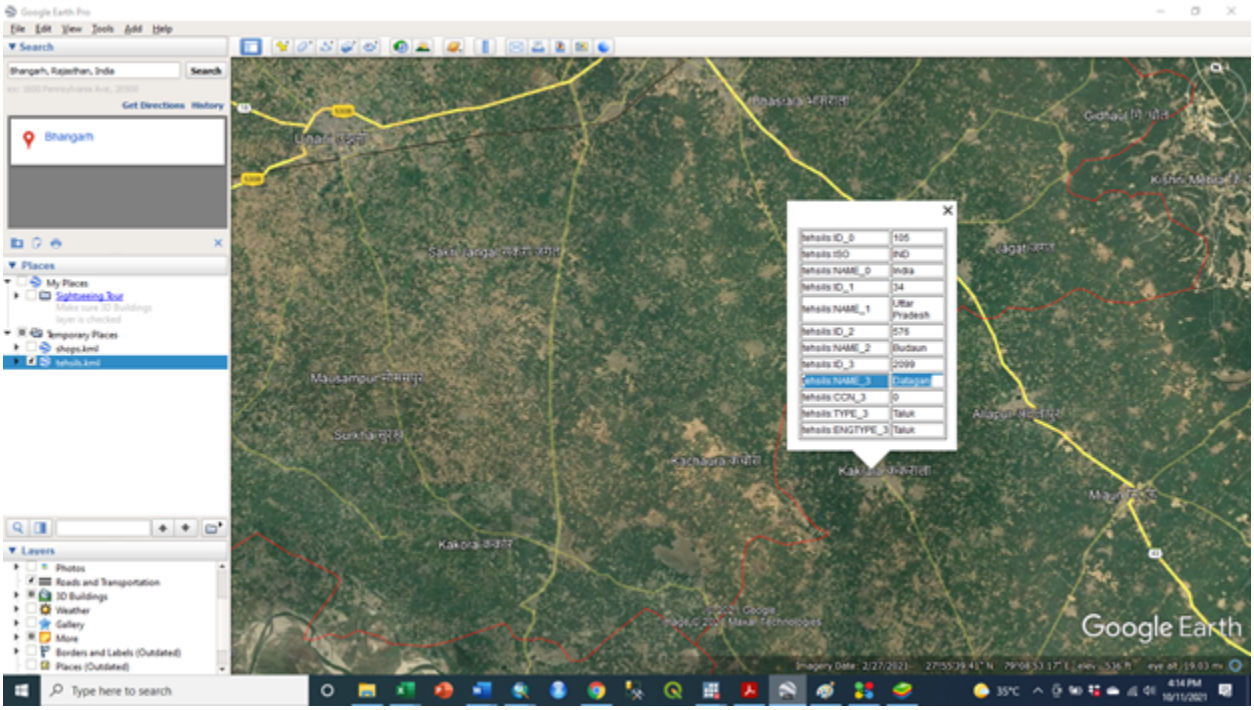
Ration shop addresses and phone numbers were uploaded to Mapbox for easy use by hotline staff.

Figure C4: 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 C5: Map Interface Used by Hotline Staff to Identify Ration Shop Phone Numbers

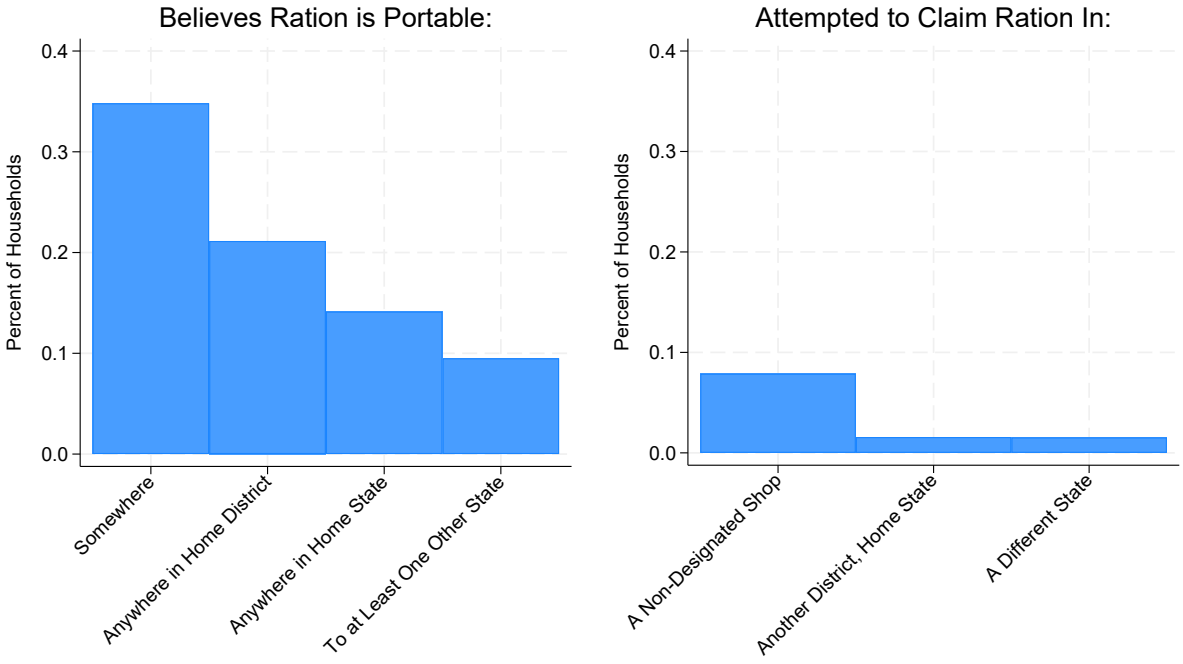


In the event that shop-level phone numbers were missing from an area of interest, hotline staff could pull lists of phone numbers at the sub-district (approximately, township) level. Numbers were matched to district polygons in Google Earth.

D Additional Surveys on Portability Beliefs

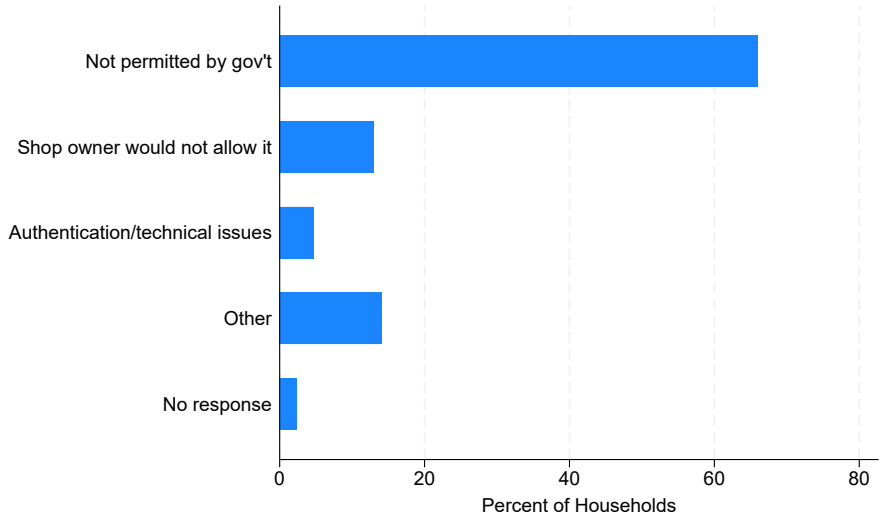
D.1 Pre-Experimental Research

Figure D1: Beliefs and usage of ration portability were low prior to our experiment.



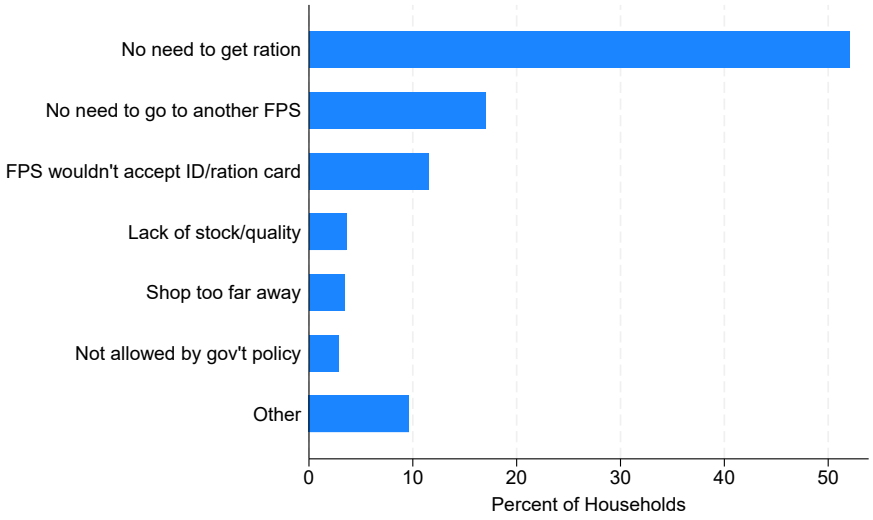
Data collected in January 2021 from surveys of ration card holders across 12 states that had implemented ONORC as of March 2020. Ration portability beliefs were measured with a series of four questions: "Can you use your ration card at a ration store/fair price shop other than your designated store?" If the respondent answered "Yes," they were asked "If you wanted to, could you use your ration card in a fair price shop that is located... Anywhere within your home district? Anywhere within your home state? At least one state other than your home state?" Attempts to claim ration were measured using the question "Except at your designated FPS, have you tried to use a ration card in any of the following places?... Somewhere within your home district? Somewhere in your home state but outside your home district? At least one state other than your home state?" *A Non-designated Shop* refers to any ration shop other than the one at which the respondent is listed as a ration claimant.

Figure D2: Reason Why Household Believes Ration Is Not Portable



Data collected in January 2021 from surveys of ration card holders across 12 states that had implemented ONORC as of March 2020. This question was asked to all respondents who responded "No" to any of the portability questions shown in Figure D1 as a follow-up question: "Why not?"

Figure D3: Reason Why Household Has Not Tried to Claim Ration Outside Their Designated Ration Shop



Data collected in January 2021 from surveys of ration card holders across 12 states that had implemented ONORC as of March 2020. This question was asked to all respondents who responded "No" to any of the questions about attempts to use their ration card outside their designated ration shop shown in Figure D1 as a follow-up question: "What was the main reason that the household has not tried to use a ration card in those places?"

D.2 Measuring External Changes in Beliefs

In this section, we provide additional details on the out-of-sample data on portability beliefs described in Section 5.2. Because our study launched partway through one of the data firm's survey waves, part of the sample was never exposed to our baseline survey or information intervention. We use this unexposed sample to test whether changes in beliefs in our control group were reflected in general changes occurring outside of our sample.

Because changes in perceived inter-state portability are most likely among our belief measures to reflect exposure to an awareness campaign—as opposed to self-experimentation—we assess goodness-of-fit according to these changes. We find that some states experienced small or no changes in beliefs about ration portability, while others experienced substantial increases, as shown in Appendix Figure D5. 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—with the caveat that this intensity is measured by proxy—by modifying (1) to include an interaction between our treatment indicator T_c and an indicator for residing in a low-campaign state.

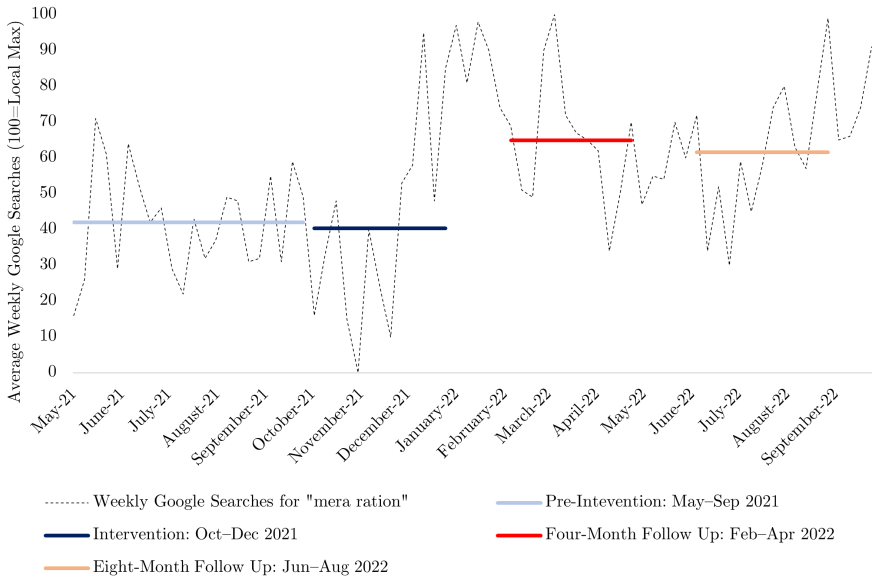
We find that the negative treatment impacts on beliefs about ration portability are pronounced in high-campaign states, as shown in Appendix Table D1, although the differences between low-and high-campaign states are generally not statistically significant. In high-campaign states, treatment reduces overall portability beliefs by 12 pp. (p-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. This null result is largely driven by the much lower initial treatment impacts in low-campaign states compared to high-campaign states (coeffs. = 11 pp. and 34 pp. respectively). The dissipation of the initially small treatment effect on beliefs in 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.

Differences in impacts on migration are consistent with patterns in beliefs. The shift from urban to rural destinations is pronounced in high-campaign states, although again the differences are not statistically significant. In those states, the number of emigrants to urban areas decreases by 0.08 (p-val < 0.01) and the number of emigrants to rural areas increases by 0.06 (p-val $= 0.01$), amounting again to a small and insignificant decrease in the total number of emigrants by 0.01 (p-val $= 0.57$). Treatment impacts in low-campaign states are small and statistically indistinguishable from zero.

In high-campaign states, 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 sates (effect sizes $\approx 1\%$).[29]

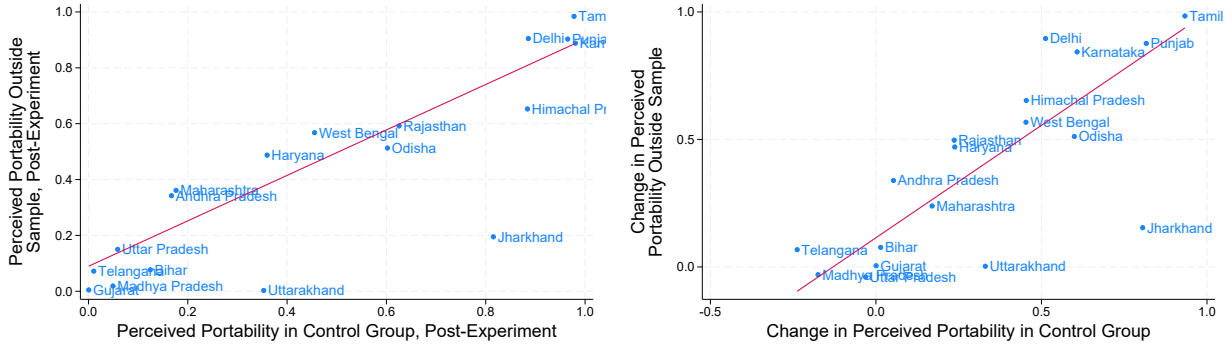
[29] Although we deflate monetary values by an urban-rural-specific CPI, migrants may pay a premium for

Figure D4: Google Trends Data on Searches for Ration Portability



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.

Figure D5: Out-of-sample beliefs 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.

short-term housing. This could explain the negative impact on income in high-campaign states without an associated reduction in consumption.

Table D1: Treatment impacts are more pronounced in states where out-of-sample beliefs changed more.

	(1)	(2)	(3)	(4)	(5)	(6)
	Believes Their Ration is Portable:					
Beliefs and Emigration Outcomes:	Somewhere	Across Districts	Across States	# of Emigrants	# of Emigrants to Urban Areas	# of Emigrants to Rural Areas
Treatment	-0.118***	-0.108**	-0.093**	-0.012	-0.075***	0.060***
	(0.040)	(0.044)	(0.044)	(0.021)	(0.023)	(0.021)
	[0.00]	[0.02]	[0.03]	[0.57]	[0.00]	[0.01]
Treatment × Low-Campaign State	0.088	0.050	0.083	0.005	0.033	-0.030
	(0.057)	(0.058)	(0.052)	(0.047)	(0.047)	(0.041)
	[0.12]	[0.39]	[0.11]	[0.92]	[0.48]	[0.47]
p-val: Treatment in Low-Campaign States = 0	0.47	0.13	0.74	0.86	0.30	0.39
Observations	48,297	48,297	48,297	52,902	52,902	52,902

	(1)	(2)	(3)	(4)
Economic Outcomes:	Total Income	Total Consumption	Food Consumption	Remittances
Treatment	-0.074**	-0.007	0.012	-0.004
	(0.033)	(0.016)	(0.021)	(0.013)
	[0.02]	[0.65]	[0.56]	[0.78]
Treatment × Low-Campaign State	0.089**	0.027	0.023	-0.004
	(0.040)	(0.024)	(0.031)	(0.018)
	[0.03]	[0.26]	[0.46]	[0.81]
p-val: Treatment in Low-Campaign States = 0	0.53	0.27	0.13	0.52
Observations	52,902	52,902	52,902	52,902

An observation is a family (household + emigrants). *Low-Campaign State* is a dummy equal to 1 if change in beliefs about ration portability in that state (outside our sample) is below the median change. Ration portability beliefs are binary variables indicating whether the respondent reports they can claim ration somewhere outside their designated shop, across district lines, or across states lines. Monetary values are measured in USD/month and transformed using the inverse hyperbolic sine function. Four-month impacts are measured in a follow-up survey (Feb–Apr 2022). All regressions include a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable, and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Pre-treatment beliefs y_{ic0} in the control group are imputed to be the same as "immediate post-intervention" beliefs y_{ic1}. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

E All Pre-Specified Results

Table E1: 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
4-Month Impacts					
Treatment	-0.079***	-0.085***	-0.055**	-0.010	-0.001
	(0.029)	(0.030)	(0.028)	(0.022)	(0.006)
	[0.01]	[0.00]	[0.05]	[0.65]	[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
8-Month Impacts					
Treatment	-0.058**	-0.048*	-0.019	-0.007	-0.007
	(0.028)	(0.026)	(0.026)	(0.021)	(0.009)
	[0.04]	[0.06]	[0.46]	[0.73]	[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
Pooled Impact					
Treatment	-0.068***	-0.068***	-0.039*	-0.012	-0.005
	(0.024)	(0.025)	(0.023)	(0.018)	(0.006)
	[0.01]	[0.01]	[0.09]	[0.51]	[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). Ration portability beliefs in Columns 1–3 are binary variables indicating whether the respondent replied "Yes" to the question "This question is about food ration claimed through the Public Distribution System. Can you use your ration card at ration shops other than your designated shop?" and two analogous follow-up questions about out-of-district and out-of-state shops respectively. If a person did not answer "Yes" to one of these questions, we code the following questions as "No." *Number of Emigrants* is the number of family members listed as emigrants in that survey round whose reason for emigration is not "Shifted to in-laws/new residence after marriage." *Any Emigrants* is a binary variable indicating whether the family had a non-zero number of emigrants. All regressions include a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable, and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Pre-treatment beliefs y_{ic0} in the control group are imputed to be the same as "immediate post-intervention" beliefs y_{ic1}. Pooled impacts measured using ANCOVA regression including a survey-wave fixed effect. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E2: Treatment Impacts on Emigration Behavior

	(1) # of Planned Emigrants	(2) # of New Emigrants	(3) # of Emigrants to Urban Areas	(4) # of Emigrants to Rural Areas	(5) # of Inter-District Emigrants	(6) # of Inter-State Emigrants	(7) # of Emigrants (High Migration Propensity Only)
4-Month Impacts							
Treatment	0.000	0.003	-0.060***	0.047**	-0.009	-0.003	-0.019
	(.)	(0.009)	(0.022)	(0.020)	(0.017)	(0.003)	(0.033)
	[.]	[0.78]	[0.01]	[0.02]	[0.58]	[0.39]	[0.57]
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	16,407
8-Month Impacts							
Treatment	0.000	-0.018	0.004	0.008	0.004	-0.002	-0.031
	(.)	(0.011)	(0.025)	(0.019)	(0.018)	(0.003)	(0.028)
	[.]	[0.10]	[0.87]	[0.68]	[0.82]	[0.46]	[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	14,458
Pooled Impact							
Treatment	0.000	-0.009	-0.032	0.026*	-0.002	-0.002	-0.028
	(.)	(0.008)	(0.021)	(0.014)	(0.015)	(0.002)	(0.024)
	[.]	[0.26]	[0.12]	[0.07]	[0.87]	[0.34]	[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	30,865

Each outcome is a count of emigrants (excluding marriage migrants). *Planned Emigrants* are those saying they intend to emigrate in the future (this variable is zero for all observations). *New Emigrants* are those who were not emigrants as of the baseline survey. *High Migration Propensity* is a binary household-level indicator measured at baseline and equal to 1 if the household has used their ration card within the past month, is in the bottom 40% of per-adult-equivalent consumption in our sample, and has at least one male household member aged 18–45. All regressions include a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable, and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Pooled impacts measured using ANCOVA regression including a survey-wave fixed effect (McKenzie, 2012). 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*b*). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E3: Treatment Impacts on Family Economic Well-Being

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
								Baseline Emigrant Outcomes		
	Total Income	Total Consumption	Food Consumption	Income Score	Finances Improved	Remittances	Well-Being Index	Food Security	Ration Claiming	Job Search
Treatment (At 4 Months)	-0.006	-0.004	0.020	-57.2	-0.001	0.012	-0.020	-0.008	-0.001	-0.145
	(0.027)	(0.013)	(0.016)	(47.8)	(0.022)	(0.015)	(0.020)	(0.021)	(0.031)	(0.440)
	[0.82]	[0.77]	[0.22]	[0.23]	[0.97]	[0.40]	[0.32]	[0.72]	[0.98]	[0.74]
Outcome Mean in Control	279	168	50	4,509	0.67	6	0.00	0.85	0.47	2.29
q-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
Treatment (At 8 Months)	0.048*	0.011	0.025	1.6	-0.008	0.023	0.030	-0.022		
	(0.027)	(0.015)	(0.017)	(38.1)	(0.023)	(0.020)	(0.033)	(0.035)		
	[0.08]	[0.45]	[0.14]	[0.97]	[0.73]	[0.25]	[0.37]	[0.52]		
Outcome Mean in Control	259	171	48	5,190	0.73	6	-0.00	0.37		
q-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		
Treatment (Pooled)	0.018	0.005	0.026*	-39.9	-0.004	0.015	0.004	-0.017		
	(0.024)	(0.012)	(0.014)	(37.9)	(0.019)	(0.016)	(0.023)	(0.029)		
	[0.44]	[0.71]	[0.06]	[0.29]	[0.82]	[0.35]	[0.85]	[0.54]		
Outcome Mean in Control	270	170	49	4,823	0.70	6	0.00	0.39		
q-Value: Treatment = 0	1.00	1.00	0.85	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		

Columns 8–10 show averages across baseline emigrants. Monetary values are measured in USD/month and transformed using the inverse hyperbolic sine function. *Total Income* includes wage income, agricultural production, and business profit of the household averaged over the past four months plus emigrants' incomes over the past month. *Total Consumption* is computed by adding average monthly household expenditure (over the past four months) to the past month's value of each emigrant's expenditure. *Food Consumption* restricts to food expenditure only. *Remittances* are monetary transfers from all emigrants to the household over the preceding month. *Income Score* is the median occupational income averaged across household members. *Finances Improved* equals 1 if the household says its financial situation improved from one year ago. *Well-Being Index* is an Anderson (2008) index combining each measure of economic well-being. *Food Security* is an indicator for whether the migrant frequently skipped a meal. *Ration Claiming* is an indicator for whether the migrant claimed ration. *Job Search* is the hours per week spent looking for a job. All regressions include a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable (except in Columns 8–10), and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Pooled impacts measured using ANCOVA regression including a survey-wave 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 secondary outcomes 6–15 as described in Baseler et al. (2022*b*). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E4: Heterogeneity in Treatment Impacts on Total Emigration

	(1) Food Is a Migration Barrier	(2) Says Ration Is Not Portable	(3) Poor Households	(4) Poor Households (No Credit)	(5) Low-Wealth Households
4-Month Impacts					
Treatment × X	0.071	0.026	-0.008	-0.041	0.020
	(0.044)	(0.039)	(0.031)	(0.036)	(0.033)
	[0.11]	[0.50]	[0.79]	[0.26]	[0.56]
Treatment	-0.024	-0.028	-0.006	0.000	-0.020
	(0.022)	(0.034)	(0.024)	(0.024)	(0.032)
	[0.27]	[0.42]	[0.82]	[0.99]	[0.54]
q-Value: Treatment × $X = 0$	1.00	1.00	1.00	1.00	1.00
Observations	52,902	52,902	52,902	52,902	52,902
8-Month Impacts					
Treatment × X	0.020	-0.048	-0.028	-0.019	0.011
	(0.039)	(0.049)	(0.026)	(0.033)	(0.025)
	[0.61]	[0.32]	[0.27]	[0.56]	[0.65]
Treatment	-0.012	0.026	0.003	-0.005	-0.014
	(0.023)	(0.046)	(0.023)	(0.025)	(0.026)
	[0.61]	[0.57]	[0.89]	[0.84]	[0.58]
q-Value: Treatment × $X = 0$	1.00	1.00	1.00	1.00	1.00
Observations	45,351	45,351	45,351	45,351	45,351
Pooled Impact					
Treatment × X	0.052	-0.004	-0.019	-0.028	0.011
	(0.034)	(0.038)	(0.023)	(0.029)	(0.024)
	[0.12]	[0.91]	[0.41]	[0.33]	[0.63]
Treatment	-0.022	-0.009	-0.004	-0.006	-0.018
	(0.019)	(0.036)	(0.021)	(0.021)	(0.025)
	[0.25]	[0.80]	[0.84]	[0.78]	[0.48]
q-Value: Treatment × $X = 0$	1.00	1.00	1.00	1.00	1.00
Observations	98,253	98,253	98,253	98,253	98,253

An observation is a family (household + emigrants). The outcome is a count of emigrants (excluding marriage migrants). Column titles show the dimension of heterogeneity, X (not shown in output), interacted with treatment. All heterogeneity dimensions are binary variables measured at baseline. *Food Is a Migration Barrier* equals 1 if the household reports finding food at the destination as one of the top three challenges a hypothetical migrant would face. *Says Ration Is Not Portable* equals 1 if the household reported it cannot claim ration outside its designated shop. *Poor Households* are those in the bottom 40% of per-adult-equivalent household consumption in our sample. *Poor Households (No Credit)* are poor households without any outstanding loan. *Low-Wealth Households* are those in the bottom 40% of the first principal component of a set of 12 durable asset measures. All regressions include a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable, and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Pooled impacts measured using ANCOVA regression including a survey-wave fixed effect. Standard errors in parentheses are clustered at the village/town level; two-sided p-values in brackets. Sharpened q-values computed within a domain that includes each heterogeneous treatment impact test. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E5: Treatment Impacts on Family Income, Consumption, and Remittances (Without Hyperbolic Sine Transform)

	(1) Total Income	(2) Total Consumption	(3) Food Consumption	(4) Remittances
4-Month Impacts				
Treatment	-6.652	1.195	0.933	-0.147
	(4.839)	(2.469)	(1.140)	(0.250)
	[0.17]	[0.63]	[0.41]	[0.56]
Outcome Mean in Control	279	168	50	6
Observations	52,902	52,902	52,902	52,902
8-Month Impacts				
Treatment	2.972	3.008	1.178	0.244
	(3.978)	(2.664)	(0.866)	(0.382)
	[0.46]	[0.26]	[0.17]	[0.52]
Outcome Mean in Control	259	171	48	6
Observations	45,351	45,351	45,351	45,351
Pooled Impact				
Treatment	-2.766	2.156	1.273	0.017
	(3.676)	(2.078)	(0.800)	(0.279)
	[0.45]	[0.30]	[0.11]	[0.95]
Outcome Mean in Control	270	170	49	6
Observations	98,253	98,253	98,253	98,253

An observation is a family (household + emigrants). Monetary values are measured in USD/month. All regressions include a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable, and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Pooled impacts measured using ANCOVA regression including a survey-wave fixed effect. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E6: Treatment Impacts on Family Income, Consumption, and Remittances (Quantile Transformation)

	(1) Total Income	(2) Total Consumption	(3) Food Consumption	(4) Remittances
4-Month Impacts				
Treatment	-0.007	0.001	0.013	-0.000
	(0.005)	(0.007)	(0.009)	(0.001)
	[0.15]	[0.93]	[0.14]	[0.59]
Outcome Mean in Control	0.43	0.43	0.43	0.43
Observations	52,902	52,901	52,902	52,901
8-Month Impacts				
Treatment	0.008*	0.011	0.016*	0.001
	(0.005)	(0.007)	(0.009)	(0.001)
	[0.09]	[0.15]	[0.08]	[0.33]
Outcome Mean in Control	0.37	0.37	0.37	0.37
Observations	45,350	45,347	45,350	45,347
Pooled Impact				
Treatment	-0.000	0.006	0.015*	0.000
	(0.004)	(0.006)	(0.008)	(0.001)
	[0.93]	[0.38]	[0.06]	[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 in USD/month and quantile transformed using the methodology of Delius and Sterck (2024). All regressions include a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable, and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Pooled impacts measured using ANCOVA regression including a survey-wave fixed effect. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E7: Return Migration and Alternative Income Score

	(1) Income Score (Alt.)	(2) Return Migration Rate (New Emigrants)	(3) Return Migration Rate (New Emigrants)
Difference at 4 Months			
Treatment	-0.004		
	(0.007)		
	[0.62]		
Outcome Mean in Control	0.81		
Observations	52,902		
Difference at 8 Months			
Treatment	0.011	0.033	0.015
	(0.007)	(0.042)	(0.055)
	[0.13]	[0.43]	[0.78]
Lasso Controls?	Yes	Yes	No
Outcome Mean in Control	0.93	0.59	0.59
Observations	45,351	1,057	1,057
Pooled Difference			
Treatment	0.000		
	(0.007)		
	[0.98]		
Outcome Mean in Control	0.86		
Observations	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. All regressions include a randomization-stratum fixed effect; Columns 1 and 2 also include other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Pooled impacts measured using ANCOVA regression including a survey-wave 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 secondary outcomes 6–15 as described in Baseler et al. (2022*b*). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Weighted Results

Table E8: 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
4-Month Impacts					
Treatment	-0.050**	-0.042*	-0.033	-0.037*	-0.006
	(0.022)	(0.022)	(0.020)	(0.022)	(0.006)
	[0.03]	[0.05]	[0.11]	[0.09]	[0.32]
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
8-Month Impacts					
Treatment	-0.044**	-0.042*	-0.014	-0.011	-0.007
	(0.021)	(0.022)	(0.019)	(0.019)	(0.007)
	[0.03]	[0.05]	[0.45]	[0.54]	[0.30]
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
Pooled Impact					
Treatment	-0.047**	-0.043**	-0.025	-0.034**	-0.009*
	(0.018)	(0.020)	(0.017)	(0.017)	(0.005)
	[0.01]	[0.03]	[0.15]	[0.04]	[0.10]
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). Ration portability beliefs in Columns 1–3 are binary variables indicating whether the respondent replied "Yes" to the question "This question is about food ration claimed through the Public Distribution System. Can you use your ration card at ration shops other than your designated shop?" and two analogous follow-up questions about out-of-district and out-of-state shops respectively. If a person did not answer "Yes" to one of these questions, we code the following questions as "No." *Number of Emigrants* is the number of family members listed as emigrants in that survey round whose reason for emigration is not "Shifted to in-laws/new residence after marriage." *Any Emigrants* is a binary variable indicating whether the family had a non-zero number of emigrants. All regressions include sampling and non-response weights, a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable, and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Pre-treatment beliefs y_{ic0} in the control group are imputed to be the same as "immediate post-intervention" beliefs y_{ic1}. Pooled impacts measured using ANCOVA regression including a survey-wave fixed effect. Standard errors in parentheses are clustered at the village/town (primary sampling unit) level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E9: Treatment Impacts on Emigration Behavior (Weighted)

	(1) # of Planned Emigrants	(2) # of New Emigrants	(3) # of Emigrants to Urban Areas	(4) # of Emigrants to Rural Areas	(5) # of Inter-District Emigrants	(6) # of Inter-State Emigrants	(7) # of Emigrants (High Migration Propensity Only)
4-Month Impacts							
Treatment	0.000	-0.011	-0.046***	0.019	-0.023	-0.004	-0.062*
	(.)	(0.008)	(0.017)	(0.022)	(0.016)	(0.003)	(0.035)
	[.]	[0.20]	[0.01]	[0.38]	[0.14]	[0.17]	[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	0.23
Observations	52,902	52,902	52,902	52,902	52,902	52,902	16,407
8-Month Impacts							
Treatment	0.000	-0.010	-0.016	0.024	0.004	-0.005	-0.029
	(.)	(0.009)	(0.023)	(0.023)	(0.018)	(0.004)	(0.030)
	[.]	[0.30]	[0.47]	[0.30]	[0.82]	[0.18]	[0.33]
Outcome Mean in Control	0.00	0.07	0.68	0.26	0.17	0.02	1.01
q-Value: Treatment = 0	.	0.98	0.98	0.98	0.98	0.98	0.98
Observations	45,351	45,351	45,351	45,351	45,351	45,351	14,458
Pooled Impact							
Treatment	0.000	-0.010	-0.035**	0.014	-0.009	-0.004*	-0.054**
	(.)	(0.007)	(0.016)	(0.017)	(0.013)	(0.003)	(0.026)
	[.]	[0.14]	[0.03]	[0.42]	[0.50]	[0.09]	[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	0.12	0.26	0.26	0.14	0.12
Observations	98,253	98,253	98,253	98,253	98,253	98,253	30,865

Each outcome is a count of emigrants (excluding marriage migrants). *Planned Emigrants* are those saying they intend to emigrate in the future (this variable is zero for all observations). *New Emigrants* are those who were not emigrants as of the baseline survey. *High Migration Propensity* is a binary household-level indicator measured at baseline and equal to 1 if the household has used their ration card within the past month, is in the bottom 40% of per-adult-equivalent consumption in our sample, and has at least one male household member aged 18–45. All regressions include sampling and non-response weights, a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable, and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Pooled impacts measured using ANCOVA regression including a survey-wave fixed effect (McKenzie, 2012). 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*b*). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E10: Treatment Impacts on Family Economic Well-Being (Weighted)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
								Baseline Emigrant Outcomes		
	Total Income	Total Consumption	Food Consumption	Income Score	Finances Improved	Remittances	Well-Being Index	Food Security	Ration Claiming	Job Search
Treatment (At 4 Months)	-0.035	-0.010	-0.005	-4.4	-0.004	0.024	-0.024	-0.001	0.021	0.384
	(0.033)	(0.011)	(0.013)	(41.7)	(0.020)	(0.020)	(0.019)	(0.022)	(0.032)	(0.498)
	[0.28]	[0.38]	[0.68]	[0.92]	[0.84]	[0.25]	[0.20]	[0.96]	[0.52]	[0.44]
Outcome Mean in Control	279	168	50	4,509	0.67	6	0.00	0.85	0.47	2.29
q-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
Treatment (At 8 Months)	0.024	0.008	0.015	-19.4	-0.022	0.021	-0.014	-0.010		
	(0.029)	(0.013)	(0.015)	(37.7)	(0.022)	(0.023)	(0.033)	(0.027)		
	[0.42]	[0.54]	[0.31]	[0.61]	[0.32]	[0.37]	[0.67]	[0.71]		
Outcome Mean in Control	259	171	48	5,190	0.73	6	-0.00	0.37		
q-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		
Treatment (Pooled)	-0.011	-0.003	0.003	-57.3	-0.010	0.019	-0.029	-0.005		
	(0.026)	(0.011)	(0.010)	(41.2)	(0.019)	(0.020)	(0.024)	(0.025)		
	[0.66]	[0.77]	[0.77]	[0.16]	[0.57]	[0.33]	[0.24]	[0.83]		
Outcome Mean in Control	270	170	49	4,823	0.70	6	0.00	0.39		
q-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		

Columns 8–10 show averages across baseline emigrants. Monetary values are measured in USD/month and transformed using the inverse hyperbolic sine function. *Total Income* includes wage income, agricultural production, and business profit of the household averaged over the past four months plus emigrants' incomes over the past month. *Total Consumption* is computed by adding average monthly household expenditure (over the past four months) to the past month's value of each emigrant's expenditure. *Food Consumption* restricts to food expenditure only. *Remittances* are monetary transfers from all emigrants to the household over the preceding month. *Income Score* is the median occupational income averaged across household members. *Finances Improved* equals 1 if the household says its financial situation improved from one year ago. *Well-Being Index* is an Anderson (2008) index combining each measure of economic well-being. *Food Security* is an indicator for whether the migrant frequently skipped a meal. *Ration Claiming* is an indicator for whether the migrant claimed ration. *Job Search* is the hours per week spent looking for a job. All regressions include sampling and non-response weights a randomization-stratum fixed effect, a control for the pre-treatment value of the outcome variable (except in Columns 8–10), and other pre-treatment controls chosen through lasso regression from the set of all baseline variables. Pooled impacts measured using ANCOVA regression including a survey-wave 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 secondary outcomes 6–15 as described in Baseler et al. (2022*b*). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.