How Migrating Overseas Shapes Political Preferences: Evidence from a Field Experiment [*]

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Abstract

Debates about how immigration affects the welfare state have often emphasized migrants' demands for high levels of redistribution. While existing work has examined natives' attitudes toward the welfare state, we know very little about migrants' preferences about redistribution and how these are shaped by the experience of migration. This paper demonstrates that access to overseas employment reduces support for taxation and redistribution by bolstering individuals' economic independence. We present results from a randomized controlled trial to facilitate migration from India to the Middle East for work. The intervention resulted in high rates of cross-border migration and significantly reduced support for taxation and redistribution among migrants, but not among their left-behind family members. We show that both migrants and their family members registered significant economic gains. We attribute their diverging redistribution preferences to migrants' increasing financial independence from earnings compared to family members' increasing dependence on remittances. Our results speak to longstanding debates about how economic gains shape preferences for redistribution and shed new light on the micro-level mechanisms by which globalization impacts welfare state politics.

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1 Introduction

How does immigration affect the politics of the welfare state? Scholars and policymakers have long debated this question from the perspective of the native-born. On the one hand, concerned over the fiscal burdens that migrants may place on welfare programs, citizens and their political representatives in many host communities have responded by limiting these programs. For example, the Trump administration cited similar concerns in expanding the "public charge" clause in US immigration law, which denies admission to noncitizens deemed likely to be dependent on the government for subsistence. On the other hand, many host communities have responded to in-migration by expanding welfare services for immigrants. Missing in these political contestations are the preferences of immigrants, who numbered 272 million in 2019 (United Nations 2019, 2). Whether and how immigration affects the welfare state preferences of individuals who cross national borders are important open questions because migrants have long been known as agents of political and economic change not only in host societies but also in their countries of origin.

The responses of native-born individuals described above typically assume that migrants depend on, and therefore support, expanded welfare programs. If migration leaves migrants without other forms of social and economic support systems, they are likely to become more supportive of the state in response. Yet there are also important theoretical reasons to expect that the act of moving overseas leads migrants to support lower rates of taxation and redistribution. Numerous studies show that personal economic advancement is one of the primary drivers of migration (Massey et al. 1993) and that migrants stand to make significantly greater wages in host societies vis-à-vis home countries (Naidu, Nyarko and Wang 2023; Mobarak, Sharif and Shreshta 2021; McKenzie, Stillman and Gibson 2010). Meanwhile, we have strong theoretical reasons to expect that pecuniary gains generally lead individuals to embrace more fiscally conservative policies (Meltzer and Richard 1981; Black 1948). These diverging perspectives give rise to opposing predictions. Does immigration lead migrants to become more or less supportive of taxation and redistribution?

We argue that when migrants see substantial economic gains from working overseas, they become significantly less supportive of taxation and redistribution. First, higher paying jobs abroad provide an opportunity for migrants to improve their wages and send remittances to their household members at home (Massey et al. 1993; Mobarak, Sharif and Shreshta 2021). Such opportunities also bolster migrants' confidence in their economic futures and shape intra-household investments, for example by leading them to delay marriage and childbearing (Jensen 2012; Goldin 2006). This change in economic standing and outlook, in turn, pushes migrants to feel less dependent on, and therefore be less supportive of, taxation and redistribution. Improved economic standing alters individuals' preferences regarding the size of the welfare state because wealthier

and higher-income individuals are less likely to benefit from, and thus less likely to support, an expanded welfare state (Almond and Verba 2015; Ansell 2014; Margalit and Shayo 2021).

Second, we argue that an additional set of mechanisms play an important role in this effect: migration makes migrants perceive themselves as more financially independent and self-sufficient. Prior work has shown that support for redistribution is associated with individuals' desire for insurance from the state (Edlund and Pande 2002; Iversen and Rosenbluth 2006) and with risk aversion (Gärtner, Mollerstrom and Seim 2017). This suggests that those who perceive themselves as more dependent on others for their incomes face an increased risk of economic vulnerability and, therefore, are more likely to demand greater redistribution from the state. Conversely, those who are financially independent and provide for others are less likely to support an expanded welfare state. Given that migration inculcates greater independence and self-reliance on the part of migrants (Bazzi, Fiszbein and Gebresilasse 2020) and is frequently associated with economic gain, we predict that migration should make migrants more opposed to redistribution. Migrants' household members, however—who gain economically from remittances but become financially reliant on migrants—are predicted to become more supportive of the welfare state because it provides safety nets during periods of volatility. Together this implies that migration's impact on welfare state preferences depends not only on migrants' and their families' economic standing, but also on individuals' perceptions of financial independence. These mechanisms help explain why some studies have shown that higher incomes and wealth alone may not necessarily lower support for taxation (Andersen et al. 2023).

Studying the effects of migration on redistributive preferences is methodologically challenging. Individuals who are interested and successful in moving abroad are almost certainly systematically different from those who are not, confounding comparisons across groups. Without altering the legal frameworks that structure cross-border migration, efforts to promote international migration and analyze its effects have largely proven unsuccessful. A central challenge in facilitating overseas migration has been identifying individuals who have a desire to migrate but lack the capacity to do so.

Our research design, geographical setting, and sample selection process allow us to overcome these hurdles. The experiment connected individuals in India seeking employment abroad with job opportunities in the hospitality sector in the Middle East. The sending region in our study is the North-East Indian state of Mizoram. Because Mizoram has traditionally been isolated from outside labor markets, international migration opportunities were both novel and potentially lucrative for residents facing curtailed domestic employment prospects. Working with local governmental and non-governmental agencies, as well as with training and recruitment firms, we identified individuals interested in overseas employment and randomly selected half for a training and placement program for employment in carefully-vetted hospitality sector jobs in Gulf Cooperation

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Council (GCC) countries. Program participants were surveyed at three points: at baseline before treatment assignment, at midline after the training program ended but before migration, and at endline following employment recruitment and international migration.

This setting is instructive for examining the impact of migration for at least three reasons. First, we situate our study within South-South labor migration channels. More migrants from developing countries now resettle in other developing regions than in industrialized nations; the India-UAE migration corridor is the second largest in the world, following only the Mexico-United States corridor (United Nations 2017). Second, our intervention took place in a geographically-isolated region of India, with little history or opportunity for out-migration. This enables us to cleanly identify the effect of our intervention; control group subjects lacked the networks or contacts to migrate overseas on their own. Third, targeting the hospitality sector for jobs allowed us to reduce potential risks to migrants that tend to be concentrated in the GCC's domestic labor and construction sectors.

To obtain a fuller picture of how migration affects migrants and sending communities, we analyzed the impact of our interventions on both potential migrants and their household members. Our experiment yielded a rich set of findings that shed light on the channels by which international mobility affects individuals' political attitudes, earnings, and intra-household investments. Focusing on the first-stage impact, our intervention was highly effective in enabling young, educated Indians to move overseas. 23 percent of individuals in the treatment group migrated overseas for work, while the corresponding figure in the control group was just 3 percent. Although program participants across the board preferred overseas opportunities to domestic ones, only those in the treatment group were able to substitute the former for the latter. Notably, control group individuals were just as likely as those in the treatment group to move outside of Mizoram, but they did so for lower-paying jobs elsewhere in India.

We find that the economic opportunities opened by labor migration substantially reduced migrants' support for state-led taxation and redistribution. At endline, our pre-registered economic policy index measuring preferences for fiscally conservative welfare state policies was 0.35 standard deviations higher in the treatment group than in the control group. Treatment group individuals were less likely to support taxation and redistribution, more likely to believe the poor should work their way out of poverty without government assistance, and more likely to express the belief that economic success was a result of individual effort rather than circumstance. Conversely, we find that migrants' household members became more supportive of redistribution following our intervention.

These results appear to be driven not only by migrants' economic gains, but also by their newfound financial independence and confidence. First, we find that overseas migration opportunities generated substantial economic benefits. Two years after the program began,

individuals in the treatment group were earning more than double the monthly wages of those in the control group, notwithstanding similar rates of employment. This significantly improved not only migrants', but also their families' economic conditions; those selected into treatment reported substantially higher household incomes, assets, and remittance-sending. Overall, treatment group individuals scored nearly 0.6 standard deviations higher on our pre-registered index of economic welfare. As an economic development program, the intervention was incredibly cost effective. For a cost of approximately 200 USD per job candidate, it generated more than 900 USD per year in increased wages, even when accounting for the fact that less than one quarter of the treatment group migrated. Second, we find that migrants became more confident in their future careers and financial independence. Before and after they moved, individuals who were selected for the opportunity expressed significantly more optimism in their futures and willingness to prioritize their careers over marriage and childbearing. Third, we find that individuals selected for the program became significantly more opposed to taxation and redistribution even before they received job offers and moved overseas. This illustrates how the promise of financial independence has significant effects on migrants' economic outlooks, even before material gains are realized. Lastly, we use evidence from long-form qualitative interviews to show that migrants felt significantly more economically confident and financially independent following migration; the qualitative evidence illustrates the mechanisms by which migration shapes immigrants' political preferences.

We investigate several alternative explanations. First, we test whether interacting with and learning about high-capacity, low-taxation regimes abroad may better explain migration's impact on welfare preferences. Plausibly, for example, experiencing a new political environment in the Gulf made migrants less trusting of Indian institutions, reducing their willingness to pay taxes. However, we find the opposite: individuals in the treatment group became more trusting of the Indian state. Second, we examine whether the training program associated with our intervention alone—as opposed to migration—can explain greater economic independence and altered political preferences. We show that this is unlikely to be the case because most of the control group participated in similar local training initiatives and because our findings hold even after controlling for training program attendance. Lastly, we study whether potentially different impacts of Covid-19 shutdowns in India verus the Persian Gulf may explain why the treatment group had substantially higher earnings compared to the control group. We do not find significant differences across the treatment and control groups in terms of the rates of unemployment and suspension or in the number of hours worked. Together, this implies that our proposed mechanism of improved economic circumstances and financial independence best explains migration's impact on political preferences.

This paper makes several theoretical and empirical contributions. First, we demonstrate that migration has nuanced impacts on individuals' preferences regarding the welfare state. While

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prior work has found that the challenging experiences associated with migration to the American frontier helped cultivate "rugged individualism" and, thus, lowered demand for redistribution (Bazzi, Fiszbein and Gebresilasse 2020), findings on the impact of remittances on preferences for the size of the welfare state have been mixed (Acevedo 2020; Ikuho and Rodriguez 2010; Adida 2014). Our argument regarding the role of financial (in)dependence ties these previous findings together and explains why migration reduces demand for redistribution among migrants, but not necessarily among their left-behind family members. These result also suggest that decreasing rates of social spending in migrant-sending countries may be driven not by voters, but rather by political elites (cf. Doyle 2015).

Second, we leverage the context of migration to explore the broader mechanisms by which economic gain impacts the politics of redistribution. Long-running debates in political economy have questioned whether reduced support for taxation and redistribution is a result of economic resources alone (Meltzer and Richard 1981; Black 1948), or by related processes that often accompany economic gain: increasing confidence in upward mobility (Benabou and Ok 2001), shifting risk preferences (Gärtner, Mollerstrom and Seim 2017), and changing perceptions of deservingness and need (Edlund and Pande 2002). We attempt to parse out these mechanisms by combining surveys of migrants at various points in time, surveys of migrants' family members, and long-form interviews with migrants. In doing so, we find substantial evidence that shifts in support for taxation and redistribution are connected to financial independence and self-sufficiency, not just economic gains *per se*.

Third, we provide rigorous causal evidence on the impacts of overseas migration on earnings, household wealth, and family investments. Using a randomized controlled trial, we confirm prior findings on the large positive impact of migration on migrants' wages based on analyses of oversubscribed lotteries of government migration programs (Mobarak, Sharif and Shreshta 2021; Gibson and McKenzie 2014; see also Abramitzky, Boustan and Eriksson 2012). We expand upon these results by focusing on a larger set of socio-political outcomes to more fully capture the impact of migration on different domains of well-being and political outcomes. For example, we demonstrate that migration improves left-behind households' economic standing. In addition, our finding that emigration-induced economic gains lead migrants to delay marriage and childbearing extends work illustrating how domestic labor market opportunities alter individuals' intra-household investments (Goldin 2006; Jensen 2012). Together, these results on emigration contribute to research that has focused on the economic effects of *internal* migration (Bryan, Chowdhury and Mobarak 2014; Derenoncourt 2022; Meghir et al. 2022; Beegle, De Weerdt and Dercon 2011), as well as work investigating how immigration impacts *natives'* labor market opportunities (Tabellini 2020; Piyapromdee 2021; Dustmann, Frattini and Preston 2013; Sequeira, Nunn and Qian 2020; Bazzi et al. 2016).

Fourth, our research design sheds light on the drivers of migration (Massey et al. 1993); by identifying and focusing on individuals who wished to move abroad, we are able to disentangle desire and capacity to migrate and demonstrate that interventions that build capabilities can be effective policy levers spurring emigration (Beam, McKenzie and Yang 2016; McKenzie, Stillman and Gibson 2010).

Lastly, our findings contribute to a longstanding debate in the social sciences regarding the mechanisms by which globalization impacts the politics of the welfare state. Past work analyzing aggregate trends in developed economies emphasizes that globalization creates pressures for an expanded welfare state (Rodrik 1998; Cameron 1978), although new evidence from developing countries suggests just the opposite (Wibbels 2006; Linardi and Rudra 2020). Here, we show how cross-border migration undermines migrants' support for the welfare state and alters the policy preferences of left-behind families, providing microfoundations on how integration into the global economy reshapes the politics of sending regions.

The rest of the paper is structured as follows. Section 2 provides an overview of the setting, intervention, and estimation strategy. Section 3 reports the impact of our intervention on migration and redistribution preferences. In sections 4 and 5, we probe potential mechanisms underpinning the effects, focusing on economic gain and perceived independence. Sections 6 and 7 discuss external validity and theoretical implications, and Section 8 concludes.

2 Research Design

2.1 Setting

Our study focuses on hospitality sector employment opportunities in the GCC states for individuals from Mizoram (see also Gaikwad, Hanson and Toth 2022). Mizoram is a small, geographically-isolated border region that is home to Mizos, an ethnic group classified by the Indian government as Scheduled Tribe (ST) to denote its historical marginalization. Like India's other ST groups, Mizos fare poorly on welfare indicators and face substantial obstacles in domestic labor markets. In Mizoram, private sector employment is anemic and government employment is highly politicized. Meanwhile, even educated English-speaking Mizos have difficulty finding work in mainland India, where they face discrimination as discernible racial and religious minorities (McDuie-Ra 2012). Mizos are phenotypically closer to Southeast Asian communities than South Asian population groups, and the vast majority follow Christianity rather Hinduism or Islam, which predominate in South Asia. For additional information on our study setting, see *Appendix A.1*.

GCC employment opportunities, meanwhile, fuel a large and growing migration corridor from South Asia, Southeast Asia, and Africa to the Middle East. More than 40 percent of the world's migrant population comes from countries in Asia, and more than 60 percent of these emigrate to

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other Asian and Middle Eastern countries (United Nations 2017). India is the world's largest source of emigrants (16.6 million per year) and recipient of remittances (USD 79 billion); migrants from South Asia account for large proportions of the populations of GCC countries (United Nations 2017).

The UAE and other GCC countries have a sizeable demand for foreign English-speaking workers to serve in the hospitality sector. Labor migrants earn far higher wages in the GCC than in similar work at home, and remittances from temporary workers frequently serve as engines of growth and investment for migrant-sending communities. Other regions of South Asia, such as Kerala, central Bangladesh, and Nepal, have leveraged labor migration and remittances into substantial economic growth (World Bank 2019). Because of Mizoram's remoteness and small population, the state has previously had few connections to employers abroad and little emigration to date. Following the example of these other regions, however, the Mizoram state government and local NGOs encouraged workers to seek employment opportunities abroad, and sought assistance to evaluate a program to place Mizos in hospitality sector jobs in the Gulf region.

2.2   Recruitment Strategy and Sample

We identified and recruited a group of prospective candidates interested in migrating to GCC countries for employment, but lacking the know-how and connections to do so. We relied on a variety of different media to advertise the job placement opportunity. We posted advertisements in leading Mizo newspapers as well as on local Mizo television networks (specifically, Zonet and LPS). We sent recruitment materials and application forms to regional offices of local skills training organizations and visited job fairs organized by the government. One of the job fairs took place in a suburb of Aizawl (the state's capital), while the other one took place in a neighboring district's headquarter. Additionally, we placed banners around Aizawl advertising the program. Finally, we reached out to the largest Mizo community organization, Mizo Zirlai Pawl (MZP) to advertise on their social media platforms. Advertisement materials were translated to Mizo to reach a wide audience. The advertisement period lasted for two months in summer 2018. While we targeted the entire state of Mizoram with our advertising strategy, the majority of applicants came from Aizawl, which was unsurprising given the higher educational attainment and English skills in the capital.

All our advertising materials asked applicants to be above the age of 18 and have at least Grade 10 standard education. We also required English competency. Once registration for the program took place, our team in Aizawl called back all registered applicants and screened them for their English skills over the phone (see *Appendix A.2* for additional details). We randomly assigned treatment status using the final list of applicants who passed the English language screening.

Prior to treatment assignment, all subjects were surveyed at baseline by a Delhi-based survey

firm (CVoter, Inc.) to record basic demographics and pre-treatment outcome measures (*Appendix A.3* discusses our survey methodology).

Table 1: Demographics of Subjects

N	389
Mean Age	22.9
Pct Male	56
Pct Completed Grade 12	72
Pct Employed	14
Pct Married	2
Pct ST	95

The resulting pool of 392 candidates[1] is broadly reflective of the upwardly-mobile population that stands to benefit from work abroad: young, educated, and unemployed (Table 1). The average age in our sample was 23. More than 70% of participants had completed Grade 12 and more than 85% were unemployed at the start of the program. These characteristics are similar to those of South Asian migrants in the UAE and other Gulf countries more broadly (Section 6 compares our sample to Gulf migrants from other parts of India). From this pool, half were randomly selected to attend a training and recruitment module (T=196, C=196). Before selecting individuals into treatment and control, we used a matching algorithm to generate blocked pairs to ensure balance along key covariates which might predict economic prospects: age, gender, education level, and English proficiency (judged in the English screening).[2] We then randomized between each pair, assigning one to treatment and the other to control. Our randomizations resulted in observably similar groups of respondents distributed between each treatment condition (see *Appendix B.1* for balance tables).

2.3   Treatment: Job Training and Recruitment

The main treatment in this study has two parts, designed specifically to connect subjects with potentially lucrative employment opportunities in the GCC.

First, all selected individuals were eligible for a free, five-week hospitality training program (from October through November 2018) administered by a Bangalore-based job-training firm

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[1]Due to an administrative error matching registrations to surveys, we only have baseline demographic data on 389 of these candidates.

[2]English is a main language of instruction, apart from Mizo, in Mizoram schools; thus a large proportion of candidates had the required skills.

(Free Climb, Inc.) and hosted by a local NGO (Social Justice and Development India, SJnDI) in conjunction with the Government of Mizoram's Mizoram Youth Commission (MYC). This program was designed to impart basic service and interview skills for service jobs in the Gulf and consisted of two parts. The first, classroom-based part (3 weeks, full time), included instruction and role-playing on basic food preparation, counter service, casual dining service, and housekeeping. Concurrently, instructors also helped participants prepare resumes for foreign employers and practice interview skills, while also providing basic information on regulations and resources in the Gulf Region. During the second part (2 weeks, part time), managers of local hotels and restaurants showed participants how their establishments function. This part of the training was intended only to provide candidates with a basic understanding of the industry in order to credibly interview for positions with employers abroad; employers in GCC regions provide extensive job-specific training once employees are hired.

In the second component of the intervention, individuals in the treatment group were invited for interviews with employers in the hospitality sector in the GCC. Our recruitment partner, Mumbai-based Vira International, vetted for ethical labor practices and selected potential employers interested in recruiting and sponsoring workers from Mizoram. Prospective employers ranged from multinational food and beverage service outlets such as Pizza Hut and Costa Coffee to luxury hotels such as Mandarin Oriental. These employers conducted several rounds of remote and in-person interviews between March and July 2019. Every individual in the treatment group was invited for interviews, typically multiple times, and employers offered jobs to the majority of those who attended interviews. Upon the offer of employment, employers applied for visas on behalf of job candidates. Individuals with job offers received logistical assistance in obtaining immigration documents and medical certificates, requirements for employment in the GCC. The recruitment firm and our local project manager scheduled meetings and checked paperwork for candidates. Although seemingly basic, these tasks represent significant barriers for potential migrants living in areas where migration is rare. Additional details about our intervention are provided in *Appendix A.4*.

The treatment bundles two elements: the training program and opportunity for overseas placement. This was both theoretically motivated and by necessity. First, migration requires access to information about destination countries as well as the required know-how to migrate (McKenzie and Rapoport 2010; Massey et al. 1993). This means that most labor migrants already have access to information specific to their destination countries and industries. Our treatment was designed to make the migration process within the context of a field experiment more realistic by providing similar information and connections to potential migrants. Second, our recruitment partner and foreign employers, who have limited information about the labor market in Mizoram, wanted assurance from an outside training firm that candidates had the basic knowledge of hospitality

sector jobs. That being said, an array of qualitative and quantitative evidence suggests that the placement opportunity itself, not the training, explains any significant differences between the treatment and control groups. We investigate this evidence more fully in Section 5.2.

2.4 Ethical Considerations

Careful consideration was given to the ethics of this study, which was approved by IRB committees at Columbia University, Stanford University, Dartmouth College, and the US Naval War College. While international employment provides otherwise unattainable economic opportunities for many would-be migrants, it potentially poses a risk to their physical and psychological wellbeing. There have been reported cases of migrant worker exploitation in the GCC (Sasikumar and Thimothy 2015). This study was embedded within the Research & Empirical Analysis of Labor Migration Program (REALM), which aims to improve empirical knowledge regarding labor migration to the Gulf in order to promote fairer migration processes and better outcomes for migrants. The goal of our project was to evaluate a blueprint for ethical cross-border labor migration, for governments' and NGOs' future use. We worked closely with partners to minimize potential risks that participants might face, to ensure that the benefits of the program flowed to participants, and to protect participants' informed consent (Humphreys 2015).

We situated the study in Mizoram because of the demand for international employment, both from individuals and the state government. The Government's MYC, Mizoram's Chief Minister, and local NGOs sought to create recruitment opportunities for Mizo workers in the GCC, and called upon researchers to assist in scientifically evaluating training and overseas placement processes that were already underway. By helping connect government and community organizations with reputable partners both inside and outside of India, the program enabled local stakeholders to better screen potential employers, protect citizens during their employment tenures abroad, and facilitate migrant integration. We carefully vetted project partners; selected the hospitality sector that is relatively reputable compared to others (e.g., construction); screened employers for fair recruitment and labor practices; connected would-be migrants with agencies safeguarding migrants' rights; and offered subjects extensive information on risks, rights, and resources. In particular, the program was designed to improve recruitment processes for prospective migrants relative to those who migrate on their own. Future government initiatives in the region were expected to benefit from the knowledge generated and the connections created. *Appendix A.5* provides an extended discussion on ethics.

2.5 Outcomes and Estimation

The main endline survey was conducted in January—March 2021, roughly two and a half years after the beginning of the program. 248 out of 392 (63%) pre-treatment subjects responded to this survey. These surveys lasted around 30–45 minutes and asked a variety of economic, social, and political questions. By contacting participants via WhatsApp as well as phone, the survey firm was able to reach both those in India and those overseas. Following this survey, we conducted a similar survey of subjects' family members—mostly parents, with a few older siblings—based on contact information collected from subjects on the baseline survey. This survey had a higher response rate, with 303 out of 392 (77%) family members responding. A brief review of similar studies on migration and development programs in low- and middle-income countries (see Appendix B.5) shows that these response rates are fairly typical for studies attempting to recontact specific individuals in low- and middle-income countries.[3] Young job seekers frequently change residence or contact information, especially over the course of a two-and-a-half year project.

A host of statistical tests indicate that attrition resulted in no systematic bias in the results among the main subjects (see Appendix B.2). First, based on multi-sample t-tests, there are no significant differences in response rates between the treatment and control groups. Second, there are no significant patterns in attrition based on pre-treatment characteristics: OLS models predicting response rates based on these characteristics have no predictive value according to omnibus F-tests. Third, there is no evidence of any significant imbalance between the treatment and control groups before or after attrition. OLS models predicting treatment status by pre-treatment covariates provide no predictive value in baseline, midline, or endline respondents, based on omnibus F-tests. Fourth, for the same reason, controlling for a variety of pre-treatment covariates to OLS models has almost no effect on the main results. Lastly, a sensitivity analysis conducted on the main results suggests that any bias in attrition would have to be very large to affect the main results. Even if redistribution attitudes in the treatment group were twice as strongly correlated with attrition as they were with family income, the results would still be positive and statistically significant. Together, these results indicate that differences-in-means between treatment and control respondents are likely to be valid estimates of the treatment effect among respondents, and possibly among non-respondents as well.

The equivalent tests for the (less important) household member survey reveal that the response rate for family members is significantly higher in the control group than in the treatment group and there is some evidence of demographic imbalance between the two groups. Therefore, in Appendix B.2, we evaluated the potential bias resulting from these imbalances in two ways. First,

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[3]See, for example, Beam, McKenzie and Yang (2016), Blattman, Fiala and Martinez (2020), and Naidu, Nyarko and Wang (2023).

we re-estimated the main results while controlling for all pre-treatment covariates. We found that these imbalances bias slightly against our findings (that household members of treatment individuals became more pro-redistribution). Second, we conducted a sensitivity analysis using the approach of Lee (2009) to assess how widely the results may vary if we make different assumptions about differential attrition. We cannot conclusively rule out that the true effect is null, but the results show that migrants' family members did not adopt *more* anti-redistribution attitudes than non-migrants' family members. This confirms the larger point that the economic gains of migration had systematically different effects on the redistribution attitudes of migrants' families than on the migrants themselves.

We evaluated migration and economic outcomes, alongside attitudinal and behavioral effects, associated with international job opportunities corresponding to the main pre-registered hypotheses in our pre-analysis plans, asking two to six survey questions for each. We present all components individually (see *Appendix C* for question wording). However, our main test of each hypothesis measures the effect of the treatment on a single, z-score index combining all of the measures (as per our pre-analysis plan). Combining multiple measures into a single index has several advantages. It reduces the number of comparisons (and therefore the risk of false positives) and statistical noise (and increases the power of our tests). Results tables display all outcome variables such that the hypothesized direction of the effect is positive. Each index is constructed such that the mean of the control group is zero and the standard deviation of the control group is one, so effect sizes can be interpreted in standard deviations of the dependent variable.

We analyzed all data with an *intention-to-treat* framework, substituting the endogenous treatment (decision to migrate) with the exogenous assignment to treatment (invitation to attend the placement program). As an additional pre-registered analysis, we assessed the *complier average causal effect* of treatment using two stage least squares to assess the likely effect of migration.

All of our major hypotheses posit an effect of treatment assignment on some attitude or behavior. For every primary outcomes, we have a measure of the same outcome from the baseline survey. We therefore test these hypotheses with an OLS model of outcome y on treatment τ, with the baseline outcome measure X:

$$y_i = \beta_0 + \beta_1 \tau_i + \alpha X_i + \varepsilon_i \tag{1}$$

We did not preregister any additional covariates in the model because we matched treatment and control respondents prior to treatment selection (that is, we created pre-treatment blocks of two) based on several key covariates. Indeed, we found that including additional covariates did not improve the predictiveness of the model. In Appendix D, we also found nearly identical results with simple difference-in-means comparisons. Due to the limited number of observations, small

size of blocks, and the possibility of attrition, we did not include block (pair) fixed effects.

Because of the nature of the randomization (blocked to reduce imbalance between treatment and control groups), we used randomization inference to calculate our primary p-values. This involves simulating the treatment assignment and estimation process 100,000 additional times, calculating the expected distribution of estimated effects under the (strict) null hypothesis. We report the one-sided p-value for one-sided hypotheses and two-sided p-value for two-sided hypotheses. Given that this analysis does not yield meaningful confidence intervals, the figures in the paper show the equivalent confidence intervals from a parametric OLS analysis – which was preregistered as a robustness check. The p-values from the two analyses are nearly identical. All hypotheses and procedures were pre-registered on the Experiments in Governance and Politics online registry (20210608AE and 20190327AB). We note that this pre-analysis plan includes hypotheses on other topics examined in two other working papers.[4]

2.6 Evaluating Mechanisms

We are interested in understanding if and why the placement program changed political attitudes: were these changes the result of economic gains alone, or of greater financial independence and self-confidence? While migrants generally experience all of these processes together, their family members may experience the former without the latter. Therefore, we took two major steps to disentangle these processes and explain potential differences between the attitudes of migrants and those of their families. First, we conducted long-form, semi-structured interviews with members of the treatment group who moved abroad, as well as matched "likely migrants" in the control group in 2021 following our endline survey. The purpose of these interviews was to investigate possible causal processes in greater detail. The interviews covered topics including motivations for moving abroad, experiences in a new country, comparisons between Mizoram and the host country, descriptions of social acquaintances, and subjects' views on their economic circumstances and life plans. Second, we conducted a midline survey of treatment and control individuals in January—March 2019, after selection to the training program but before individuals received job offers or moved abroad. 290 individuals responded (74%) to this midline survey. Comparing individuals' attitudes at this point helps to separate out realized economic gains (which only came later) from the psychological preparation and anticipated financial independence that may begin beforehand.

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[4]Appendix F provides a Benjamini-Hochberg false discovery rate analysis for the main pre-registered hypotheses evaluated in this paper.

3   Main Results

Overall, the endline survey produced strong evidence that opportunities to work overseas shape political preferences and economic conditions. Treatment group individuals were significantly more likely to move overseas for work, and they registered significant gains in personal and family income. The treatment group individuals became significantly more opposed to taxation and redistribution than control group individuals. However, family members of treatment group individuals did not experience the same effect – if anything becoming more supportive of taxation and redistribution despite benefiting significantly from remittances.

3.1   Migration

First, we find that the treatment had a large first-stage effect on individuals' ability to migrate abroad, as Table 2 documents. While 23% of the treatment group lived overseas at some point during the two years following the program, only 3% of the control group did (see also Appendix Table D.13). This effect size is large relative to other field experiments facilitating overseas migration. Beam, McKenzie and Yang (2016) provide assistance and information about migration to potential migrants, but post-intervention find that only 2.2 percent of the entire sample migrated with the treatment having no significant impact on migration rates.[5] A novelty of our research design is that it identified a sending region without an established history of out-migration and it selected subjects at baseline who wished to emigrate abroad for employment. This allows us to isolate capacity from desire to emigrate, and cleanly identify the impact of interventions that increase individuals' capabilities to pursue employment overseas.

The vast majority of the migrants moved to Kuwait and the UAE, with a handful moving to Qatar, Saudi Arabia, and Bahrain. About half of these migrants returned home at the end of their initial one-year contract, while half remained overseas, as some accepted multi-year contracts with greater stability and status. These results are consistent with migration patterns between South Asia and the Gulf, where most labor migrants return home after one- to five-year employment stints.

The endline survey illuminates how the program assisted with the migration process. Individuals in the treatment and control group faced several hurdles in moving overseas, but the results suggest that the recruitment program reduced barriers to migration at every step (see Appendix Table D.14): individuals in the treatment group were more likely to apply for a job overseas, receive a job offer if they applied, receive a visa if they received an offer, and move

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[5]Bazzi et al. (2016) in a similar experiment provide information on migration intermediaries, lowering migration rates.

Table 2: International and Internal Migration Results

	Migrated	Training	Offer	Internal
OLS	+ .20	+ .14	+ .25	-.19
	(.04)	(.06)	(.05)	(.05)
RI P-Value	.000	.009	.000	.000
Baseline Control?	Yes	Yes	Yes	Yes
Control Mean	.03	.43	.08	.32
N	248	245	231	247

Note: *Migrated:* Migrated overseas during the period of the study. *Training:* Attended a hospitality job training program. *Offer:* Offered a job overseas. *Internal:* Moved elsewhere in India.

overseas if they received a visa. Given the barriers that individuals face in accessing overseas employment and completing emigration logistics, it is unsurprising that nearly all of those in the treatment group who moved abroad did so with the connections and help of our recruitment partner.

Strikingly, the treatment did not significantly increase the proportion of individuals who left Mizoram for work. While individuals in the treatment group were more likely to move *overseas* for work (23% vs. 3%), those in the control group were more likely to move *elsewhere within India* (32% vs. 13%). In lieu of international placement opportunities, control group subjects took domestic jobs elsewhere—particularly in the hospitality sector hubs of Goa, Delhi, and Mumbai. Appendix Figure D.5 temporally illustrates these trends, demonstrating the proportions of each group that migrated domestically or overseas over the study period.

Given the opportunity to work overseas, individuals in the treatment group resoundingly chose to take up the opportunity rather than work elsewhere in India. Before migration, the vast majority of individuals in both treatment and control perceived job opportunities in the Gulf to provide workers with higher pay, more upward mobility, better treatment, and less anti-Mizo discrimination (see Appendix Table E.25). This is consistent with prior work showing that wage differentials are far higher across national borders than within domestic borders (McKenzie, Gibson and Stillman 2013) and that migrants anticipate significantly more wage gains from overseas migration than internal migration (Clemens, Montenegro and Pritchett 2019).

3.2 Political Attitudes

Consistent with our main argument, we find that international employment opportunities significantly reduced individuals' support for state-led taxation and redistribution. Table 3 shows the effect of overseas employment opportunities on three measures of support for state-led taxation and redistribution in their home country. At the endline, treatment group individuals were more

likely to strongly oppose high taxes for social spending in India (36% vs. 30%) and more likely to disagree with the government intervening to reduce income inequality (16% vs. 7%) than those in the control group. They also were more likely to agree with a sentiment that is known to underpin opposition to the welfare state:[6] that it is "very possible" for the poor to advance economically with hard work alone (63% vs. 50%). When these measures are indexed together in our primary pre-registered index, we find that receiving access to overseas migration opportunities shifted the welfare state attitudes of the treatment group by more than one third of a standard deviation relative to the control group (see also Appendix Table D.19). These effects are quite large given the strongly pro-redistributive views of most low-income voters in India. The changes in general attitudes toward taxation and redistribution are particularly noteworthy given that treatment group individuals were themselves beneficiaries of a subsidized government program.

Table 3: Redistribution Attitudes Results (Main Subjects)

| | **Index** | *Components* | | |
		Taxes	Mobility	Inequality
Treatment Effect	+ .350	+ .13	+ .10	+ .23
(SE)	(.142)	(.14)	(.08)	(.15)
RI P-Value	.005	.163	.079	.062
Baseline Control?	Yes	Yes	Yes	Yes
Control Mean	0	3.78	2.46	1.81
N	248	246	247	248

Note: Treatment effects measured by OLS, controlling for baseline measure of DV. P-values are one-sided according to pre-registered hypothesis. Items 1 and 3 are measured on a scale from 1-5, with 5 representing strong agreement with the anti-redistribution position. Item 2 is measured on a 1-3 scale. *Taxes:* Should the government lower taxes for ordinary people, even if that means it will have less funding for public services to help the poor in Mizoram? *Inequality:* Should the government reduce income differences between the rich and the poor? *Mobility:* In general, do you think it is possible for someone who is born poor to become rich by working hard?

Strikingly, these estimated treatment effects are larger than the effects of other comparable determinants of welfare state preferences highlighted in the literature, both within our data and in analogous attitudes measured in the World Values Survey. Within our data, the difference in redistribution views between the treatment and control groups was three times larger than the difference between those with high and low income at the baseline. An individual's treatment assignment was far more predictive of an individual's endline redistribution attitudes than was their baseline redistribution attitudes. We also benchmarked our main effects using World Values Survey data from three comparable large countries (Pakistan, Bangladesh, and

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[6]See Margalit and Shayo 2021; Alesina and La Ferrara 2005.

Indonesia) as well as the United States. Using the WVS data, we created a standardized index of redistribution views with the three most comparable questions to our own and examined the bivariate effects of three commonly-discussed determinants of redistribution views: education (completed secondary education), gender, and union membership. Strikingly, none of these three correlates of redistribution views had as large an effect in any of the four countries as treatment assignment had in our sample (see Appendix D.1).

Next, turning to migrants' family members, we find no evidence that migration decreased support for redistribution. To the contrary, we find that parents and siblings of migrants became significantly *more* supportive of state-led welfare policies. Household members of treatment group individuals were much more likely to strongly support the government intervening to reduce income inequality (73% vs. 61%) and about as likely to support high taxes (23% vs. 22%) than those of control group individuals. They also showed evidence of an underlying attitude change, as they were much less likely to believe it was "very possible" for the poor to advance economically with hard work alone (49% vs. 56%).

Table 4: Redistribution Attitudes Results (Household Members)

	Index	Components		
		Taxes	Inequality	Mobility
Treatment Effect	-.228	-.06	-.20	-.10
(SE)	(.117)	(.14)	(.11)	(.08)
RI P-Value*	.974	.675	.963	.901
Baseline Control?	Yes	Yes	Yes	Yes
Control Mean	—	2.81	1.63	2.49
N	304	303	298	295

Note: Same outcome questions as Table 3. Hypotheses were one-sided (that the treatment would increase opposition to redistribution), so effects in the opposite direction show up as high p-values.

Our results carry implications for long-standing debates regarding the impact of globalization on welfare policy. According to seminal studies (Cameron 1978; Rodrik 1998), globalization creates expansionary pressures on the welfare state as those who do not benefit from new economic opportunities demand increased state support and as increased volatility in global markets motivates citizens to demand larger safety nets from governments. Yet recent work focusing on developing countries argues that globalization, to the contrary, hollows out the welfare state (Wibbels 2006; Linardi and Rudra 2020). Our micro-level evidence helps reconcile these contrasting claims. Migrants in our study, who benefit from overseas opportunities after becoming financially independent and self-sufficient, become less supportive of taxation and redistribution. In contrast, migrants' household members, who gain economically from higher remittances but who become more reliant on others abroad for their incomes and thus face increased financial

vulnerability in the global economy, develop more pro-redistribution attitudes. This is evidence in favor of our theory that it is not economic opportunity alone, but its combination with economic (in)dependence that shapes welfare state attitudes. The next section investigates the impact of migration on economic well-being and financial (in)dependence in further detail.

4   Mechanisms: Economic Gains vs. Financial Independence

We have argued that migrants become less supportive of redistribution not just because of the economic gains they experience due to migration, but because of a perception of increased economic independence and confidence that comes with these gains. While migrants themselves become more financially independent from their families as part of this process, migrants' family members become more dependent on their migrant relations. In this section, we present four pre-registered analyses (see Section 2.6) that parse the mechanisms for this difference. We find strong evidence that while both migrants and their families gained significant financial benefits from job opportunities overseas, future-oriented confidence and financial independence played an important role in migrants' shifting views on redistribution.

4.1   Shifts in Economic Standing

Table 5: Economic Standing Results

	Index	*Components*			
		Employed	Wages	Family	Goods
Treatment Effect	+ .558	+ .05	+ 5530	+ .61	+ .35
	(.153)	(.06)	(1780)	(.22)	(.12)
RI P-Value	.000	.217	.001	.004	.001
Baseline Control?	Yes	Yes	Yes	Yes	Yes
Control Mean	0	.39	4790	4.45	0
N	248	246	234	238	248

Note: Treatment effects measured by OLS, controlling for baseline measure of DV. P-values are one-sided according to pre-registered hypothesis. *Employed:* Employed at endline survey. *Wages:* Personal monthly wages, in INR. *Family:* Family income on 1-8 scale. *Goods:* Standardized index of 6 household goods.

First and most importantly, both migrants and their families experienced significant economic benefits from overseas employment. As of the endline survey (Table 5), individuals in the treatment group were on average earning more than double the wages of individuals in the control group. The mean wage in the control group was approximately 4,800 INR per month (approximately 65 USD),

while the mean wage in the treatment group was over 10,400 INR (140 USD). This is particularly striking given that the majority of individuals in both groups remained unemployed and rates of employment were not different between treatment and control groups (44% vs. 39%) at endline. The wage increase was nearly entirely driven by the relatively small number of individuals in the treatment group (23%) who moved overseas for work. At endline, the mean monthly wage was 40,100 INR (approx. 540 USD) for those currently employed overseas; 18,400 INR (250 USD) for those currently employed in Mainland India; and 12,800 INR (170 USD) for those currently employed in Mizoram (see Appendix Figure D.6).

Figure 1: Family Income, Treatment vs. Control
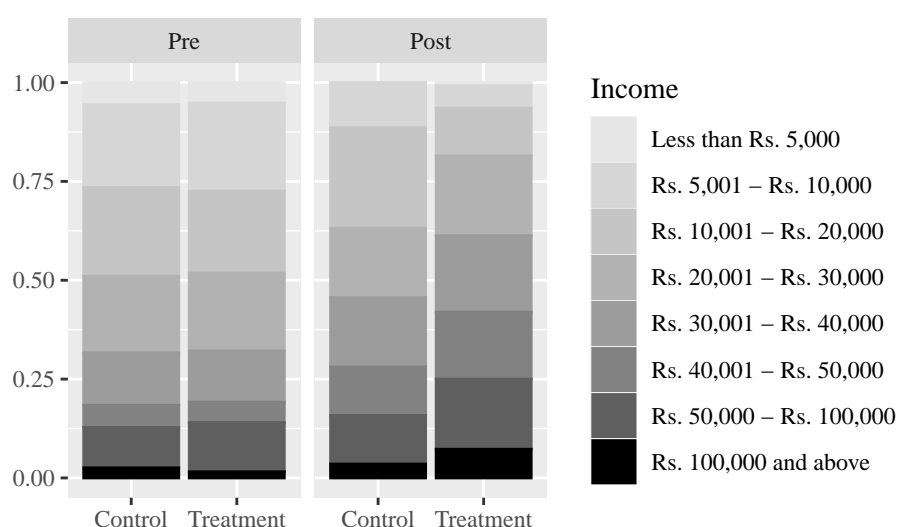
Table 6: Remittances Results

	Self-Reported	Family-Reported
Treatment Effect	+ 3150	+ 1380
(SE)	(730)	(750)
RI P-Value	.000	.032
Baseline Control?	Yes	Yes
Control Mean	150	560
N	248	303

Note: Treatment effects measured by OLS, controlling for pre-treatment income. P-values are one-sided according to pre-registered hypothesis.

These individual wages, moreover, had a significant effect on the economic standing of participants' families (see Figure 1 and Appendix Table D.16). On average, treatment group

individuals who moved overseas reported sending 14,000 INR (200 USD) per month, or about half of their wages, home to their families. Selection to the treatment group, therefore, had a significant and positive effect on self-reported remittances, on the (Table 6). Likewise, the family members of migrants—and of the treatment group generally in the household survey—reported receiving significant boosts in remittances from overseas.[7] Consequently, selection to the treatment group resulted in a substantial increase in their families' overall economic circumstances, both in monetary terms and in lifestyle goods. Treatment group individuals were half as likely to report a family income below 20,000 INR (18% vs. 36%) and nearly twice as likely to report a family income above 50,000 INR (25% vs. 16%). These differences also manifested in an index of household material goods. Treatment group individuals were more likely to report their families owning at least one computer (62% vs. 53%), refrigerator (99% vs. 95%), and motorbike (76% vs. 68%).

4.2 Shifts in Confidence and Life Planning

Second, our results also show that access to migration overseas significantly affected migrants' economic confidence and their willingness to further invest in their careers. Table 7 shows the effect of the treatment on an index of four measures of economic confidence (see also Appendix Table D.18). At the endline survey, individuals were modestly more likely to express confidence that they would be able to advance professionally, and that their next job would pay well. Overall, treatment group individuals' economic confidence was approximately 0.2 standard deviations higher than in the control group ($p < .10$).

Additionally, Figure 2 shows the effects of the treatment on more durable measures of economic expectations. We analyze whether individuals were more willing to focus on their careers and delay marriage and childbearing plans. Compared to the control group, treatment group individuals expressed a significantly greater preference for delaying marriage and childbearing by the endline. Asked at what ages they intend to marry and have children, those in the treatment group gave ages that were nearly two years older than those given by the control group ($p < .001$). This difference is notable; it is larger, for instance, than the difference between men and women's preferences at baseline (see also Appendix Table D.17). To the extent that the welfare state

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[7]The treatment-control difference in reported remittances was somewhat smaller in the household survey. Two factors may have contributed to this. First, the control mean for family-reported remittances was higher because remittances included those from family members living elsewhere in India. Second, the response rate was lower among the family members of migrants than among migrants themselves, slightly skewing the family member survey toward non-migrant families.

Table 7: Economic Confidence Results

	Index	Components			
		Mobility	Wages	Family	Lifetime
Treatment Effect	+ .197	+ .12	+ .28	+ .10	- .09
	(.139)	(.11)	(.09)	(.08)	(.08)
RI P-Value	.090	.118	.002	.128	.859
Baseline Control?	Yes	Yes	Yes	Yes	Yes
Control Mean	0	4.20	3.46	3.79	4.03
N	243	243	243	243	243

Note: Treatment effects measured by OLS, controlling for baseline measure of DV. P-values are one-sided according to pre-registered hypothesis. All components are measured on a scale from 1 (strongly disagree) to 5 (strongly agree). *Mobility:* In the future, will you be able to advance professionally? *Wages:* Do you think your next job will pay better or worse than the average salary in Mizoram? *Family:* In the next year, do you think your own and your family's economic situation will be better or worse? *Lifetime:* When you are the age your parents are now, do you think you will be better off or worse off financially than them?

alleviates the financial burden of family expansion and child-rearing, these shifts in the household decisions of treatment group subjects aligns with their diminished support for state-led taxation and redistribution.

Figure 2: Family Planning Preferences over Time
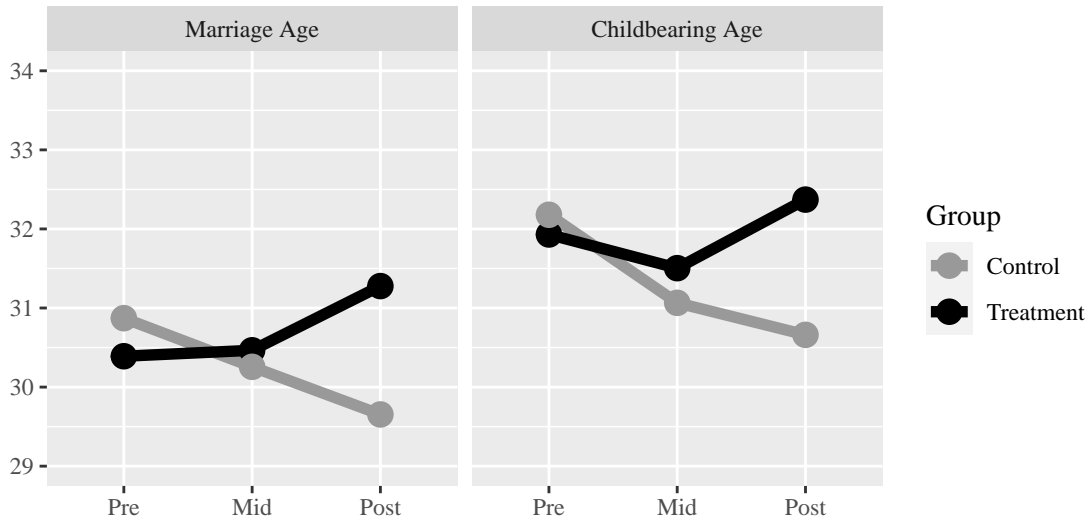
Note: Differences are statistically significant only in the endline survey.

4.3 Over Time Comparisons

Third, we find that treatment group individuals' economic and political attitudes began to shift even before they migrated and worked overseas (Figure 3 and Appendix Table D.19). For this analysis, we conducted a midline survey in early 2019, after candidates had been selected for the opportunity but before they accepted jobs or moved overseas. At this point, individuals in the treatment group had not yet gained materially from overseas opportunities, but they had begun to anticipate future gains and see themselves and financially independent.

We find that treatment group subjects experienced an increase in economic confidence relative to the control group by the midline. Even before they interviewed with foreign employers or were offered jobs, treatment group individuals perceived their economic opportunities to be significantly greater than those in the control group. In fact, the difference between treatment and control was greater at the midline, before individuals migrated overseas, than at the endline. Long before securing jobs or migrating, individuals who gained access to overseas opportunities became more confident in their economic futures. Any changes in political attitudes at the midline survey, therefore, are likely the result not of realized benefits but of anticipated opportunities and perceived financial independence.

Figure 3: Redistribution Attitudes over Time
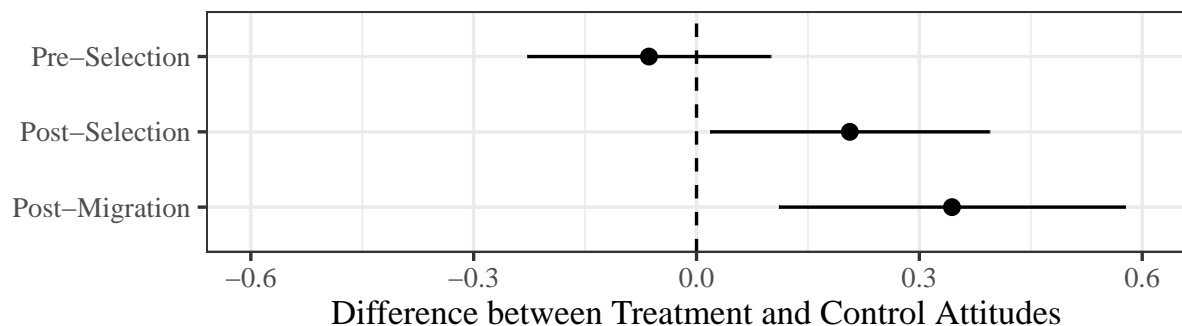
Note: Comparison of 3-question redistribution index from Table 3. 1 unit = 1 SD of index in control group.

More importantly, individuals in the treatment group experienced a significant shift in political preferences by the midline survey, becoming more fiscally conservative even before realizing the income gains from migration. As Figure 3 shows, members of the treatment group were .21 standard deviations more anti-redistribution than the control group at the midline ($p < .05$). At the endline, after some in the treatment group had received two years of high salaries overseas, these differences were qualitatively larger and more statistically significant (.34 SD, $p < .01$).

These results suggest not only that higher-income individuals are less apt to favor taxation and redistribution (Meltzer and Richard 1981), but also that the perception of financial independence is sufficient to trigger similar attitudinal shifts. At midline, the treatment group had neither realized any material benefits nor moved overseas. Instead, their increase in opposition to taxation and redistribution appears to be based merely on prospective gains.

4.4 Qualitative Evidence

Lastly, we conducted in-depth qualitative interviews with migrants from the treatment group and with statistically-matched non-migrants from the control group about their experiences as well as their political and economic attitudes. Two major themes were particularly relevant to the mechanisms in our study.

The first theme was that overseas migrants expressed much more confidence in their economic prospects and pride in their independence. Subjects interviewed in the treatment group emphasized that jobs abroad afforded them more security and stability, noting that "in Mizoram [they] would not be getting this much pay and unless [they] work with the government—there is no job security, but in Dubai even during the pandemic [they were] able to work and get [their] salary regularly" (Respondent #59). Others corroborated this by stating that they are "the most secure one among [their] friends financially" and that they are now able to save and no longer have to rely on their families for financial support (Respondent #228). Others noted that thanks to job opportunities in the Gulf, "[they were] seeing a lot of improvement in [themselves] and financial security is also there to some extent" (Respondent #44).

In contrast, control group respondents described instability in their economic lives. These interviewees reported varied trajectories: some migrated within the country and returned later, while others stayed in Mizoram. A few respondents reported not having any salaried job at all. Regarding jobs, one respondent said that he has not been able to work "because of the pandemic and the road block by [neighbouring] government [causing a] shortage of supply" (Respondent #3). Despite his savings, he reported: "I don't think I am stable." Another respondent noted that due to the pandemic "[she] had to spend without earning so [she is not] secure right now" (Respondent #16). Even those who have returned from employment elsewhere in India expressed a lack of financial stability due to lower salaries in Aizawl, Mizoram's capital: "I don't think I am secure because I come from a poor family background and I am renting a flat here in Aizawl, so there are many things to spend money on, and it is difficult to have proper savings" (Respondent #23). Together, these interviews show that our results were not driven by the treatment group's access to greater wages alone, but also by international job opportunities spurring greater economic confidence.

The second and more striking theme was that migrants expressed a great deal of pride in their economic successes and attributed them primarily to hard work rather than circumstance. One respondent felt that they had become "more mature and confident," and "more stable, and secure" than their friends back home (Respondent #179). Another commented, "I think I am more mature than when I was in Mizoram and I am more disciplined, and I have now the mindset to become better and do well and improve in the future" (Respondent #40). One respondent remarked that he felt more confident than his friends at home, not only because of his higher earnings but "[because he] was able to work in a country where [he] knew no one and ... adjust very well" (Respondent #80). These statements reflect migrants' belief that their higher earnings were the result of their ability to succeed in a new environment. They also expressed a desire to protect their economic gains, both now and in the future. One interviewee stated, "One thing I realised is when it is our own money that we earn, we tend to spend it wisely and save up more than before" (Respondent #44). Another noted that in Kuwait, "the government here does not take tax from the people, so I think that is good and better" (Respondent #88). Migrants' greater opposition to redistribution and belief in the power of hard work dovetails with prior work that shows that success in unequal environments is associated with a greater belief in meritocracy (Newman, Johnston and Lown 2015).

5 Alternative Explanations

By contrast, we do not find evidence for other alternative explanations such as experience with Gulf economic institutions, the effects of the training course, or the differential effects of the COVID-19 pandemic.

5.1 Experiences and Trust in Government

Overseas migrants may shift their economic and political views as a result of experiencing new economic and political institutions. Migrants have been shown to return to their home countries having absorbed new ideas from their host countries (Levitt 1998; Spilimbergo 2009). In our context, this has at least two possible implications for migrants' views on redistribution.

On one hand, Indian migrants living in the Gulf may witness the strong welfare system providing for citizens of the U.A.E. and other Gulf states and wish to promote a similar system in their country of origin. This would predict we would see an effect in the opposite direction, with migrants becoming more pro-redistribution.

On the other hand, those same migrants may see the relatively high-capacity governments running on low tax rates (funded, instead, by oil production). This does suggest a possible alternative explanation for our findings: the migrants in our study may have come to admire

Gulf governments and distrust the Indian government while living overseas. Lower trust in the government, in turn, is often linked to less support for taxation and redistribution. Migrants' families, who did not experience life in the Gulf, would likely not experience these changes.

However, we do not find that individuals in the treatment group grew to distrust the Indian government. To the contrary, treatment individuals were significantly more likely than control individuals to say that their national, state, and local governments back in India were trustworthy and capable of solving problems (see Appendix E). For example, a much higher proportion of the treatment group said that Mizoram's government was "mostly" or "completely" trustworthy (75% vs. 66%) and capable of solving the state's problems (78% vs. 69%). These results suggest that migrants' experiences overseas, if anything, made them more trusting of the Indian government, which does not explain migrants' decreased support for redistribution or the divergence between migrants and their family members.

5.2 Training Program

Did the job training program itself alter economic standing or political attitudes? We have significant reasons, both theoretical and empirical, to suggest that our results are not driven by the training program.

First, overseas job opportunities are extremely rare in Mizoram, while hospitality training programs are common. Local government organizations and NGOs regularly conduct free skills training programs as a way to reduce the region's high unemployment rate. A large proportion of both treatment and control individuals had previously attended one of these programs and typically reported that the content was similar to our program. More importantly, 43% of individuals in the control group reported attending a job training program in the months following the baseline survey (compared to 58% of treatment group individuals). In fact, most of these control group individuals attended a course that was inspired by our program and conducted by two of our project partners. By contrast, reliable connections to overseas jobs are scarce and thus overwhelmingly desired in Mizoram's isolated economy. In the baseline survey, just three percent of respondents had any friends who had moved abroad, and only ten percent knew anyone in their extended family who had emigrated. At endline, just three percent of the control group worked overseas in the two years after the program began, and all of them had returned to Mizoram by the endline survey.

Second, treatment group individuals who migrated emphasized that the Mizoram-based training program was basic and that in fact the consequential job-specific training that they received was given by the foreign companies that eventually hired them. For example, one respondent told us that he spent his first week at his new job doing theoretical training by the company and only afterward was assigned to a line of work based on his skills (Respondent #44). Another respondent

told us that "[they] went through training and [they] were taught how to make a pizza according to the brand's standard" once hired by his company (Respondent #80). Others were taught by their superiors abroad how to set the tables according to the restaurant standard or how to be a barista (Respondent #156). This illustrates how the training program in the study did not provide specialized skills required for particular hospitality sector jobs; rather it served to signal potential candidates' interest and basic interviewing eligibility to foreign employers who were unfamiliar with Mizoram's local labor market. All participants acquired the specialized skills required for their jobs when they were trained by their employer.

Third, quantitative evidence from the survey results strongly and consistently shows that job training by itself had little effect on economic standing or political attitudes. In *Appendix E*, we examine differences in our four main outcomes across two sets of comparisons (controlling for pre-treatment covariates). First, there were no significant differences within the control group between individuals who attended our program partners' training program versus those who did not attend. Second, within the treatment group, there were no significant differences between individuals who attended our training program and those that did not when we controlled for subsequent migration. These results strongly suggest that, independent of overseas job opportunities, the training programs did not boost candidates' economic prospects or affect attitudes on taxation or redistribution.

5.3 COVID-19 Shutdowns

Given that our program timeline (Aug 2018 - Mar 2021) included the first year of the COVID-19 pandemic, it is worth asking whether the effects we observe are due in part to the effects of shutdowns or other economic impacts on individuals. However, we find little evidence that shutdowns differentially impacted the economic standing of treatment and control groups. Asked about COVID-related shutdowns, individuals in the treatment and control groups were about equally likely to have been laid off (15% vs. 18%), had work temporarily suspended (32% vs. 32%), and had hours or wages cut (23% vs. 22%). Even at the endline survey, which was conducted during the peak of India's 2021 shutdowns, there was no significant difference between the overall employment rate of the treatment and control groups (43% vs. 39%). Instead, the economic effects were driven entirely by the income differences between employed individuals in the treatment and control groups, which are much less likely to have been COVID-related.

6 Representativeness and External Validity

How generalizable are the findings from this study to other migrant populations and cross-border migration corridors? We make three sets of observations, and expand upon these

further in *Appendix G*.[8]

Representativeness We examine how similar our sample and context are to international migrants, both those from India and others around the world. *Appendix G.1* compares our experimental sample with nationally representative survey data on India's overseas migrants in the Indian Human Development Survey (IHDS) and data in the Kerala Migration Study (KMS), a comprehensive household survey of the South Indian state of Kerala that has some of India's highest historic rates of out-migration. We do so to assess the extent to which our sample conforms to the demographic traits of the country's overseas migrant population. Comparing the profiles of migrants involved in overseas migration and non-migrants, we highlight three key respects in which our sample generalizes. Like our subjects, this broader population of cross-border migrants are disproportionately young (nearly 60% were under 30 when they migrated), highly educated (more than 50% have completed secondary education), and belong to underrepresented minority groups (Muslims and Christians are overrepresented both in Kerala and in India as a whole). Additionally, we find that short-term migration to autocracies in the Gulf region constitutes a large share of migration from India and from Asia in general. India's cross-border migrants typically live overseas for 1-5 years; Gulf autocracies are fare more common destinations than Western democracies. We also analyze data from the World Values Survey (WVS, Round 7) to show that the majority of out-migration from Asia as whole targets destinations that are autocracies rather than democracies.

External validity to different subjects Next, we examine how the findings from our particular experimental sample generalize to demographically different migrant population groups (see *Appendix G.2*). It is plausible, for example, that young, educated members of marginalized groups may see particularly large gains from migration, where they can put their skills to productive use without the barriers to upward mobility they may face in the domestic context (Banerjee and Knight 1985). Examining heterogeneous effects within our sample, we find that the effects documented in our study appear to generalize outside our sample (Egami and Hartman 2022). First, we run pairwise interactions between pre-treatment covariates and our key outcomes: individual and household redistribution attitudes and economic standing. We find no significant interactions between treatment effects and any of our pre-treatment demographic characteristics (e.g., age, gender, education, income, religion, tribal identity) or any baseline measures of economic standing (Appendix Table G.28). Second, using machine-learning estimators, we investigate treatment effect heterogeneity agnostically by estimating individual-level treatment effects for all individuals in the sample based on estimates of the heterogenous effects of the treatment using all pre-treatment

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[8]See also Gaikwad, Hanson and Toth 2022.

covariates (Devaux and Egami 2022). Appendix Figure G.9 shows essentially no systematic heterogeneity in the treatment effects on redistribution attitudes or economic standing. Together, these analyses suggest that the treatment would have similar effects for individuals with different demographic profiles.

External validity to different contexts Would our findings from Indian migrants working in the Gulf hospitality industry generalize to other industries, other origin countries, and other destination countries? It is challenging to empirically address such contextual concerns with only one experiment (Egami and Hartman 2022). Due to constraints stemming from resources, logistical capabilities, ethical considerations, and real-world immigration policy environments, we did not attempt to replicate our study in other cross-border migration contexts; indeed, the intensive and focused nature of our efforts were necessary to successfully induce migration in contrast to the null effects on migration interventions reported in prior work (Beam, McKenzie and Yang 2016). Nevertheless, based on insights gleaned from theory and fieldwork, in *Appendix G.3* we propose a set of key site-level, contextual factors that potentially moderate the effects of overseas migration on migrants' redistribution attitudes, offer theoretical predictions, and suggest future research designs to evaluate the effects of migration in these other contexts (Appendix Table G.29). For example, we hypothesize that the effect of migration on redistribution attitudes is likely to be smaller when migrants work in lower-paying or more exploitative industries, such as construction or domestic work. The evidence presented above suggests that our migrants' improved economic standing and financial independence drove anti-redistribution attitudes, which suggests that less remunerative industries are likely to have smaller effects. By contrast, we hypothesize that the effects are likely to be larger when migrants originate in regions and countries with higher tax rates, because they are likely to be more protective of their newfound financial resources.

7 Discussion

Our results provide causally-identified evidence on the effects of overseas migration on political preferences, migrants' material well-being, and intra-household decisions. We argue that migration changes individuals preferences over redistribution through a channel of economic independence. In line with this theory, we show that individuals in the treatment group became significantly less supportive of state-led taxation and redistribution. We document that migration also significantly improved migrants' (and their household members') economic standing and made migrants more willing to delay marriage and child-bearing. While both migrants and their left-behind household members became wealthier, we show that only migrants turn more fiscally conservative. This indicates that economic gains alone cannot explain changes in political

preferences. Using long-form interviews with migrants, we show that changes in perceived financial independence helps explain this divergence in migrants' and their household members' attitudes. Together, these findings imply that access to even short-term cross-border labor migration opportunities profoundly reshape political preferences and economic conditions.

Migration and Preferences for Redistribution   Our results contribute to the literature on how migration shapes welfare state politics in sending regions. Prior work has primarily focused on the impact of migrant remittances on public good provision and the preferences of migrants' household members.[9] By-and-large, this literature suggests that out-migration dampens demand for state-provided goods and redistribution (Doyle 2015; Goodman and Hiskey 2008; Adida and Girod 2011), although others argue that migration may enable those left behind to bargain more effectively with the state (Karadja and Prawitz 2019). There has been less attention to paid to the preferences of migrants themselves. Here, we show that migration makes migrants more opposed to taxation and redistribution, but by contrast increases their household members' demands for redistribution. Our results on migrants' more fiscally conservative preferences are in line with findings by Bazzi, Fiszbein and Gebresilasse (2020), which shows that internal migration to the US frontier persistently lowered demands for redistribution. Given the divergence between migrants' and their household members' preferences, our results imply that lower public goods provision observed in high out-migration areas may be driven not by the political efforts of left-behind family members, but by the strategies of political elites and migrants who return home with newfound economic and political clout.

We note that our study focuses on South-South migration, whereas the majority of research on the political economy effects of migration is centered on South-North migration. Yet the majority of migration flows in the global economy today is South-South. In 2017, more international migrants from developing countries had resettled in other developing regions than in industrialized nations; migration within Asia and the Middle East now comprises the largest regional migration corridor in the world (United Nations 2017, 1-3). Sending community effects are particularly important in these contexts because labor migrants typically maintain citizenship and social ties to their home countries, return home after employment stints overseas, and alter the economic landscape in sending regions through remittances. Consequently, evaluating whether and how overseas opportunities influence the economic outlooks and political behavior of potential migrants is essential for understanding the impact of migration on political and policy outcomes in the Global South today.

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[9]There has also been a large literature on how immigration shapes natives' political preferences; for a review, see Edo et al. (2018) and Hainmueller and Hopkins (2014).

Economic Independence and Redistribution Our findings also contribute to debates on whether and how economic gains explain preferences for taxation and redistribution. Conventional wisdom and models of taxation hold that wealthier voters typically demand lower redistribution than the poor (Meltzer and Richard 1981; Black 1948). Others argue that it is not one's present economic circumstances, but rather their chances for upward mobility that shape their fiscal preferences (Benabou and Ok 2001). Here, we build on prior work showing that other psychological mechanisms such as individuals' desire for insurance from the state (Edlund and Pande 2002) and risk aversion (Gärtner, Mollerstrom and Seim 2017) also underpin one's attitudes towards redistribution. Since in our study both migrants and their household members at home experienced economic gains, but only migrants lowered their demand for redistribution, we can rule out that economic gains alone shape preferences toward the welfare state.

Rather, we argue that *how* individuals get wealthier matters just as much. Migrants in our program generally saw their economic gains as earned achievements rather than fortuitous windfalls. In interviews, individuals in our treatment group repeatedly emphasized the pride they took in succeeding in a foreign environment and attributed their newfound financial independence to hard work. The treatment group in our study received significant benefits, but also made costly investments in their futures and overcame important hurdles to do so: applying for various approvals and certifications, living in a foreign country, and working for their wages. Migrants' household members, while significantly improving their economic conditions from remittances, became more dependent economically on their migrant family members. This may partially explain the divergent results between migrants and their household members: migrants in the treatment group became less supportive of redistribution and government assistance, despite benefiting themselves from a government assistance program, while household members became more supportive. Together, this indicates that one's sense of economic (in)dependence is an important mediator in the relationship between economic resources and preferences for redistribution.

These results suggest we should expect similar changes in political preferences in other globalization-related domains where beneficiaries of government programs register similar gains. Our results imply that government cash transfers and other economic rewards that require less individual effort and potentially make individuals feel more economically dependent may boost support for redistribution. In converse, economic processes that generate employment and improved financial independence, such as foreign direct investment or trade-related job gains, might trigger a similar backlash to taxation and redistribution (Linardi and Rudra 2020).

Facilitating Employment Migration. This project represents, to our knowledge, one of the first significant randomized controlled trials that spurred overseas labor migration (Naidu, Nyarko and

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Wang 2023). Therefore, comparing our program to prior attempts at spurring migration (Beam, McKenzie and Yang 2016; McKenzie and Sasin 2007), can shed light on the factors that limit migration more generally. In particular, we highlight two ways that our project departs from prior cross-border migration experiments.

First, our program was based in a region, Mizoram, where overseas migration opportunities are scarce. In regions where migration is more common, such as in the Greater Manila region, potential migrants are likely to have more connections and know-how to find jobs overseas. Programs designed to encourage migration with information and logistical support, therefore, may struggle to encourage further migration. In Mizoram, by contrast, there are many individuals interested in migration who have difficulty finding opportunities to do so. Our study's success, therefore, suggests that development programs to encourage migration may be most effective in regions where migration is comparatively low. It also suggests that the political and economic effects of migration are likely to be felt most keenly in newly-opened migration corridors.

Second, our program focused on providing connections for potential migrants to overseas employers. Prior experiments have provided a host of services to potential migrants: job training programs, information about employers, and assistance with application and migration processes. Our program provided these elements, but none of them were unusual in Mizoram or in other migration RCTs. What was unusual was the connections to vetted, reliable overseas employers through a recruitment agency. In contexts with little prior migration, placement agencies serve as critical "migration institutions" (Goss and Lindquist 1995; Sasikumar and Thimothy 2015), closing gaps in knowledge and access and enabling migration. The success of our program suggests that these connections are essential for encouraging migration.

Labor Migration as a Development Program. Our findings show that migration can provide otherwise unattainable economic benefits for individuals in developing countries. Many observational studies argue that migration has significant economic benefits (Yang and Choi 2007; Abramitzky and Braggion 2006; Abramitzky, Boustan and Eriksson 2012; Doyle 2015; Ahmed 2012), yet ascertaining these effects is difficult because migrants differ systematically from non-migrants. In the absence of strong affirmative evidence that migration can be economically beneficial, changes in migration policy is often politically fraught.

Our study confirms results on the economic impact of migration from oversubscribed government lotteries (Gibson and McKenzie 2014; Mobarak, Sharif and Shreshta 2021) and estimates the cost effectiveness of policies encouraging migration. Individuals who were randomly selected to receive migration opportunities earned more than double the wages of the control group subjects two years later—despite the fact that less than a quarter actually took jobs overseas. Comparing these benefits to the costs of our placement program (*Appendix A.6*)

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suggests that migration can serve as a cost-effective path to upward mobility for individuals in developing countries, particularly those from marginalized communities who face limited prospects domestically. All told, the program cost about 200 USD per person and generated nearly 900 USD per person per year in additional wages. These results have important implications for governments in low- and middle-income countries seeking to promote development. Our experiment demonstrates that supporting safe and ethical labor migration (e.g., connecting migrants with job opportunities and helping them navigate the migration process) can be a valuable tool for development.

8 Conclusion

Randomized controlled trials have often been criticized for not being suitable to understand the impact of transformative policies that matter for reducing poverty (Ravallion 2014; Rodrik 2008). Our intervention captures the enormous potential of cross-border migration opportunities in the developing world to reshape economic prospects and political attitudes. We go one step further by examining not only migrants, but also their household members' economic status and political preferences. In particular, we confirm prior findings on the significant positive impact of migration on earnings and remittances based on oversubscribed migration lotteries using an experimental design (McKenzie and Sasin 2007; Gibson and McKenzie 2014; Mobarak, Sharif and Shreshta 2021). We expand the scope of inquiry to capture other aspects of migrant welfare such as marriage and childbearing preferences and political attitudes.

As one of the first field experiments to successfully facilitate international migration, this research also contributes to the literature on the drivers of migration. Theoretically, migration is the product of both the willingness and the capacity to move. However, past research has found it difficult to identify individuals who have the desire to move but not the means, because in most places where migration is an established livelihood strategy individuals who are willing to migrate tend to have the information and networks to do so. Because prior experimental research has often situated interventions in high out-migration areas, studies have often found no impact of lessening capacity constraints on outmigration (Beam, McKenzie and Yang 2016; McKenzie and Sasin 2007; Bazzi et al. 2021). Our study overcomes this hurdle by focusing on a region of India—a country with otherwise large volumes of out-migration—where individuals had little information or access to overseas employers. This suggests that capacity constraints are most likely to bind in areas with relatively low out-migration.

Together, our results illustrate the complex role that globalization plays for individuals in low- and middle-income countries. On the one hand, like other aspects of international economic integration, overseas employment can generate significant economic gains, especially for those

from marginalized communities (Adams Jr. and Page 2005; Yang and Choi 2007). At the same time, an implication of our finding is that globalization can seriously undermine the welfare state by increasing opposition to redistribution among its economic winners. While this closely aligns with Linardi and Rudra (2020), which shows that foreign direct investment can depress demands for redistribution (see also Wibbels 2006), it runs against conventional wisdom that suggests that globalization gives rise to pressures for a larger welfare state (Cameron 1978; Rodrik 1998). Our differential findings across migrants and their left-behind family members help reconcile these contrasting claims: The migrants in our study benefited from overseas opportunities by becoming financially independent and self-sufficient; their left-behind household members gained economically from higher remittances but also become more reliant on others abroad and thus more exposed to globalization-induced vulnerability and volatility. Our findings imply that an important unintended consequence of policies promoting greater global integration could be rising inequality as a consequence of a shrinking welfare state. Future research can build on the micro-level experimental evidence that we have presented in order to analyze whether and how new out-migration opportunities alter broader policy trends in regions integrating into the global economy.

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Supporting Information (For Online Publication)

How Migrating Overseas Shapes Political Preferences: Evidence from a Field Experiment

Contents

A   Additional Information about the Experiment

Note: Some of the material in these appendices, particularly in Sections A-B, also appears in the appendices of: Gaikwad, Hanson and Toth (2022), which examines a different set of results from the same field experiment.

A.1   Intervention Location and Study Context

Figure A.1: Map of Mizoram, India

Mizoram is situated in northeastern India, bordering Bangladesh on the west and Myanmar on the east. The state is sparsely populated, with around one million residents. Aizawl, the capital city, hosts a third

of this population with 300,000 residents. Mizoram has a highly educated population: the literacy rate is 91.33 percent according to the 2011 Census. Female literacy is 89.27 percent, which puts Mizoram amongst the highest literacy and female literacy rates in India (Government of India 2011). Mizoram also has one of the highest female-to-male demographic ratios as well as one of the lowest literacy gender gaps in the country (Government of India 2011). While most people in Mizoram speak the local language, Mizo, English is also widely spoken and used as the other official language of the state. The vast majority of the state's population belong to various tribes that are collectively known as Mizos. These tribes have been classified under the Indian Constitution as Scheduled Tribes, a category indicating groups that have been historically marginalized and discriminated. Today, the Indian Constitution guarantees Scheduled Tribes quotas in government jobs, educational institutions, as well as elected positions. The majority of Mizos identify as Christians and only a small minority identifies as Hindus or Buddhists (Government of Mizoram 2014).

Mizos migrated to current Mizoram from upper Burma sometime between the 15th and 18th centuries (Government of Mizoram 2014). British colonization was formalized in 1895 after the Lushai Hills were declared to be part of British India. Mizoram administratively became a district of the province of Assam. This was also the time when Christian missionaries arrived to the area and set up schools. Missionaries achieved wide-reaching changes in Mizo society by converting the majority of the population to Christianity, opening schools, and educating the masses (Government of Mizoram 2014). After India's independence, Mizoram remained a part of the state of Assam, but centralized control from Assam frustrated Mizos and in the 1960s the Mizo National Front (MNF) started an armed insurgency. Mizoram became the 23rd state of India in 1986, following a peace accord between the Government of India and the MNF (Government of Mizoram 2014).

Subsequently, the MNF reformed itself as a political party and contested elections in 1987. The Indian National Congress (INC)—established in 1961—is the other major political party in the state (Government of Mizoram 2014). The INC and the MNF have regularly alternated in power in the state's legislative assembly. At the local level, after the abolition of chieftainship, village councils were established in 1957. Mizoram, as a Sixth Schedule state, is excluded from quotas instituted for women, Scheduled Castes, and Other Backward Classes (OBCs), in village councils under the 73rd amendment of the Indian constitution (Government of Mizoram 2014).

Despite its high human capital, Mizoram lacks employment opportunities. The relative geographic isolation and mountainous topography have constrained industrial growth and produced high unemployment rates. Mizoram's GDP per capita is around US$1,600, which puts it at 19th amongst 27 Indian states (Government of Mizoram 2014). The majority of the population remains employed in agriculture, even though the contribution of agriculture to GDP has been declining (Government of Mizoram 2014). Industrial output is only 19.39 percent of the state's GDP, whereas the tertiary sector makes up 66.29 percent of the GDP. The largest employer within the services sector, however, remains the government. Taken together, Mizoram has struggled to create employment opportunities outside of small-scale agriculture and the public sector, which leaves its educated population without adequate employment opportunities.

Why focus on the India-GCC migration corridor? Much of prior research on migration has analyzed population flows from the Global South to the North, but migration across countries in the Global South has increased exponentially in the past twenty years. According to the 2017 United Nations Migration Report, migrants around the world are most likely to originate from Asia, which sends 41 percent of the world's migrant population (United Nations 2017). India alone sends 16.6 million migrants abroad making it the country with the largest number of emigrants in absolute terms. Furthermore, Sasikumar and Thimothy (2015) estimates that around 600,000–800,000 migrants leave India annually, whereas annually the country adds 7–8 million new workers to the labor force. This makes out-migration one of the major sources of new employment for Indian workers.

Due to the role of economic incentives, social networks, and immigration policy regimes, migrants often end up in a small set of countries. Around 60 percent of Asian migrants, for instance, migrate to another Asian or Middle Eastern country, and only a much smaller subset, 16 and 19 percent, migrate to Europe and North America, respectively.[10] Moreover, the 2017 United Nations Migration Report estimates that more than 67 percent of the world's migrant population live in only twenty countries. Out of these twenty, Saudi Arabia has the second largest migrant population, the United Arab Emirates the eighth and Kuwait the twentieth. This has not always been the case. Countries outside of a small group of Western industrialized countries have been registering rapid growth in migrant populations only in the past twenty years (United Nations 2017). GCC countries are amongst the world's most significant migrant destinations today both in terms of volume and growth in migration.

When looking at migration flows between countries, Indian migration to the UAE is second only to the Mexico-US migration corridor (United Nations 2017). However, migration between India and the Gulf is growing much more rapidly. Migration between India and the UAE nearly tripled in the past twenty years, with migration from India to Saudi Arabia doubling within the same time period (United Nations 2017).

It is not only the size of migration within the Global South that warrants scholarly and policy attention, but also its economic impact. India is the largest recipient of overseas migrants' remittances, with US$78.6 billion received in 2018 (World Bank 2019). For comparison, India received US$44.37 billion in foreign direct investment. Over half of these remittances are sent from GCC countries by Indian migrants. For low or middle income countries, the size of these remittances often make up a significant portion of the economy. For India's northeastern neighbor, Nepal, remittances equal 28 percent of its gross domestic product (World Bank 2019). Unlike development assistance, remittances flow directly to recipient households making it an important source for consumption and investment.

An important difference between South-South migration and South-North migration is that many Western industrialized countries offer a route to citizenship, although they restrict labor migration flows tightly and often privilege educated and skilled migrants in the case of employment-based immigration (Peters 2017). By contrast, countries in the Global South usually welcome labor migrants of varying skill levels, but make it very difficult for newcomers to obtain citizenship and permanent residency status. This means that most migrants return home after temporary work abroad in the case of South-South migration.

A.2 Recruitment Strategy

We identified and recruited a group of prospective candidates interested in migrating to GCC countries for employment, but lacking the know-how and connections to do so. We relied on a variety of different media to advertise the job training and placement opportunity. We posted advertisements in leading Mizo newspapers as well as on local Mizo television networks (specifically, Zonet and LPS). We sent recruitment materials and application forms to regional offices of local skills training organizations and visited job fairs organized by the government. One of the job fairs took place in a suburb of Aizawl, while the other one took place in a neighboring district's headquarter. Additionally, we placed banners around Aizawl advertising the program. Finally, we reached out to the largest Mizo community organization, Mizo Zirlai Pawl (MZP) to advertise on their social media platforms. Advertisement materials were translated to Mizo to reach a wide audience. The advertisement period lasted for two months over the summer of 2018. While we targeted the entire state of Mizoram with our advertising strategy, the majority of applicants came from Aizawl, which was unsurprising given the higher educational attainment and English skills in the capital city.

All our advertising materials asked applicants to be above the age of 18 and have at least Grade 10

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[10]In absolute terms this means that out of 105 million Asian migrants in 2017, 63 million migrated within Asia, 20 million migrated to Europe, and 17 million migrated to North America.

standard education. We also required English competency. Once registration for the program took place, our team in Aizawl called back all registered applicants and screened them for their English skills over the phone.

We randomly assigned treatment status using the final list of applicants who passed the English language screening. We matched these applicants into blocked pairs based on age, gender, education level, and English proficiency (judged in the English screening). We then randomized between each pair, assigning one to treatment and the other to control.

A.3   Survey Methodology

We were interested in examining the prospective effects of economic opportunity, as distinct from the effects of realized economic gains as well as the effects of migrating abroad, on our theoretical outcome variables of interest. Therefore, we interviewed subjects (both treatment and control) in three survey rounds: a baseline survey before participants were selected for the treatment, a midline survey after the training program for the treatment group had finished but before individuals secured jobs and began migrating abroad, and an endline survey after migration had occurred.

All surveys were administered by a New Delhi-based survey company (CVoter Inc.), that hired twenty local, Mizo-speaking male and female enumerators to conduct the surveys. This ensured that participants had access to enumerators of the same gender. The surveys were written in English and then translated into Mizo and back-translated into English by CVoter's team. We offered subjects the choice of Mizo and English versions of the survey. The topics that formed the basis of our surveys are socio-political topics that are routinely discussed in Indian society and that are identical or similar to questions that are commonly asked in many types of preexisting surveys, including government surveys (notably, the National Family and Health Surveys) carried out across India on a regular basis.

The baseline survey was a face-to-face survey that took place in Aizawl. Survey subjects were invited to the research team's offices in central Aizawl, where they were asked to fill out a survey by enumerators using handheld tablets. In order to facilitate re-contacting, we collected the phone numbers and addresses of each respondent as well as back-up family member contact information. Shortly after the baseline survey, we contacted our respondents via telephone to ensure that appropriate contact information had been given and to verify respondents' willingness to participate in future surveys.

After our training sessions were concluded, we fielded our second survey round. The survey was administered as a 30-minute computer assisted telephone interview (CATI) by CVoter enumerators. To boost participation, we offered phone credits worth a month of free calls, text messages, and 1 GB of data to participants for taking the survey.[11] The third survey was conducted about two years following the second survey round. This survey was administered as a 45-minute CATI survey fielded by CVoter enumerators. Respondents were offered cash incentives of 1,000 INR that were deposited directly in their bank accounts.

A.4   Training and Recruitment Program

In this section, we provide further details regarding the treatment component related to the training program geared toward employment opportunities abroad. The training program was designed to equip individuals with the skills required to access employment opportunities overseas and overcome logistical barriers to migration. Individuals selected for the program had the opportunity to attend a five-week job training program designed to impart skills that would be useful in hospitality sector employment in GCC

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[11]Depending on the telephone operator, this cost around 169–199 INR (US$2.36–2.78) per person.

countries. Individuals were also informed that upon completion of the program, they would be contacted for employment opportunities by a recruitment firm partnering with the training program.

During the first half of the program, participants attended classroom training sessions administered by a Bangalore-based training firm, Free Climb. This component of the program included modules on restaurant food service, beverage and counter service, and housekeeping. Specifically, the training sessions included instructions on food preparation (e.g., food safety, knife skills, cooking methods, kitchen equipment handling and maintenance), beverage production (e.g., beverage equipment handling, inventory and storage principles, cleaning schedules, safety and accident prevention), counter services (e.g., customer interaction, communication, order-taking principles, cash register control, cleanliness and hygiene), casual dining service (e.g., table set-up, communication, billing standards and cash control, handling of complaints, food handling principles), and housekeeping (e.g., making beds, cleaning guest rooms and baths, re-stocking guest amenities, handling special requests, managing household equipment), among others. Students attended class five days a week for six hours a day.

Figure A.2: Photos of Training Program and Participants

In the second half of the program, participants conducted on-the-job training in hotels, restaurants, and fast food chains in Aizawl. Overall, this part of the intervention was designed to upgrade candidates' skills, equipping them with basic knowledge required to demonstrate eligibility for hospitality-sector job opportunities in international destinations at the interview stage. Concurrently, instructors also helped

participants prepare resumes and practice interview skills. Resume formats and interview preparations were designed with the input of our Mumbai-based recruitment firm to ensure that participants' job application materials were consistent with GCC hiring standards. To prepare participants for integration into the GCC countries, instructors also provided them with information on regulations and resources abroad. The focus on preparing trainees for jobs abroad distinguished the training program from other skills-training initiatives that were geared toward domestic employment opportunities. Upon completion of the training session, participants were given a course completion certificate.

In the recruitment stage of the intervention, program participants were invited for interviews with several employers. These interviews were organized by our recruitment partner, Vira International. Every program participant was invited to interview, and most were offered multiple opportunities to do so. The vast majority of those who chose to attend interviews received job offers. Following job offers, Vira and our project manager assisted program participants in obtaining passports and medical certifications. The employers were responsible for providing everything else: work visas, airline tickets, and room and board.

A.5   Ethical Considerations

Researchers have both moral and professional obligations to minimize harm and maximize potential benefits for research participants. This section details the steps we took to protect research participants from potential harm in this project. We organize our discussion following the "Principles and Guidance for Human Subjects Research" of the American Political Science Association.

Principle 1: Political science researchers should respect autonomy, consider the wellbeing of participants and other people affected by their research, and be open about the ethical issues they face and the decisions they make when conducting their research.   While international employment offers otherwise unattainable economic opportunities for many immigrants, it potentially poses certain costs and risks to their physical or psychological wellbeing. Labor migrants sometimes struggle to integrate into new political and social environments. Relocating for work, especially overseas, requires navigating a complex, often uncertain set of costs and benefits. International employment can be lucrative but it also requires migration-specific knowledge that is difficult to obtain. This explains why individuals who could gain the most from migration often do not migrate (Bryan, Chowdhury and Mobarak 2014). Specifically, in the context of the GCC, there have been documented instances of migrants facing extortion by recruitment agencies that charge illegal recruitment fees (Sasikumar and Thimothy 2015). Furthermore, Gulf countries have also faced criticism for overlooking employer exploitation, such as the withholding of workers' passports or employers' reneging on promised salaries (Human Rights Watch 2019). Reports of labor code violations have been concentrated in the construction sector; domestic household workers have also experienced exploitation (Human Rights Watch 2019).

This study was conceptualized and embedded within Research & Empirical Analysis of Labor Migration Program (REALM): "REALM aims to shed light on the processes that sustain unfair migrant labor by improving our empirical understanding of the structures and dynamics implicated in recruitment for temporary work in the Gulf region (and, where relevant, elsewhere)." REALM was founded in order to generate scientific knowledge regarding labor migration as a way to remedy labor recruitment practices in the Persian Gulf that are often private, unsupervised, and opaque, and to help develop and promote fairer migrant labor processes that can lead to better outcomes for migrants and their communities.

Within REALM, the goal of our project was to design and evaluate a blueprint for ethical and safe cross-border labor migration, to be used by governments and NGOs in the future. While designing our project, we paid significant consideration to the ethics of the study. We were mindful of the general obligation of researchers "to anticipate and protect participants from trauma stemming from participation in

research" (APSA 2020). We worked closely with our partners to minimize the potential risks and costs that participants might face, to ensure that the benefits of this program flow to participants and their communities, and to protect participants' informed consent (Humphreys 2015; Teele 2014).

We situated the study in Mizoram because of the demand for international employment opportunities, both from individuals and from the state government, in this region. The Government of Mizoram's earlier attempts at training and recruitment had drawn large numbers of youth looking for lucrative international work, given the scarcity of employment opportunities within Mizoram. The Government's Mizoram Youth Commission (MYC), the Chief Minister of Mizoram, and several leading Mizo community organizations sought to create recruitment opportunities for Mizo workers in GCC countries, and called upon researchers to assist in scientifically evaluating processes of skills training and overseas placement that were already underway. By helping connect government and community organizations with reputable partners both inside and outside of India, the program enabled local stakeholders to better screen potential employers, protect citizens during their employment tenures abroad, and facilitate migrant integration. Although we (and the government) could not possibly facilitate supervised employment opportunities for *all* individuals seeking employment abroad, our goal was to help the government and NGOs build an ethical template for future skills development and employment placement programs in the region.

Principle 2: Political science researchers have an individual responsibility to consider the ethics of their research-related activities and cannot outsource ethical reflection to review boards, other institutional bodies, or regulatory agencies. This research project has received IRB approval from Columbia University, Stanford University, Dartmouth College, and the US Naval War College. The project proposal was also reviewed by the grant selection committee of REALM and an advisory committee of five social science faculty unaffiliated with the research team. Apart from the formal IRB reviews, we strove to ensure that our involvement minimized risk to participants and that the benefits of the program flowed directly to participants (Teele 2014; Humphreys 2015). In particular, we worked closely with New York University–Abu Dhabi Office for Compliance & Risk Management to select an employment sector (hospitality) that is relatively reputable compared to sectors where labor violations had previously been reported (e.g. construction), and to choose a recruitment partner with a long and tested history for fair recruitment practices in the hospitality sector in the Persian Gulf. Additionally, we screened specific employers who participated in the job placement component of the study for reputable labor practices.

Principle 3: These principles describe the standards of conduct and reflexive openness that are expected of political science researchers. In some cases, researchers may have good reasons to deviate from these principles (for example, when the principles conflict with each other). In such cases, researchers should acknowledge and justify deviations in scholarly publications and presentations of their work. There were no significant deviations from the principles. Below we discuss the ethical considerations that guided our study.

Principle 4: When designing and conducting research, political scientists should be aware of power differentials between researcher and researched, and the ways in which such power differentials can affect the voluntariness of consent and the evaluation of risk and benefit. Given the economic opportunities presented by our program and the potential power imbalances between the research team and the individuals in our study, we took two major steps to protect the sanctity of the informed consent process. First, we decided that PIs would not interact directly with any of the research subjects. We made this decision so as to not put pressure on potential research participants to take part in the program. The main point of contact for subjects was our project manager in Aizawl. The project manager is Mizo, of a similar age and background as the subjects. Most of these interactions happened in person or

by phone/WhatsApp, in the Mizo language. Similarly, all surveys and interviews were also conducted by Mizos, by either our project manager or local enumerators hired by the survey firm. Subjects were given the option to conduct the surveys and interviews in either Mizo or English.

Second, the recruitment for the program and the three survey waves created distinct decision points for individuals in which they were informed that they could withdraw from the study without any negative impact. In addition, we did not make participation in the training program a condition for attending overseas job interviews. Consequently, many individuals in the treatment group decided against participating in either the training or placement interviews. In addition to the formal consent processes, we specifically trained our project manager to be honest and clear about the potential costs and benefits in any informal interactions with the participants. Our recruitment partner also conducted extensive information sessions with subjects, in which they were provided information about various aspects related to the risks and benefits of working abroad and in the Persian Gulf in particular. Finally, information sessions about the program conducted by the Mizoram Youth Commission and local community organizations were also designed to provide even-handed information about the risks and opportunities associated with pursuing employment abroad.

Principle 5: Political science researchers should generally seek informed consent from individuals who are directly engaged by the research process, especially if research involves more than minimal risk of harm or if it is plausible to expect that engaged individuals would withhold consent if consent were sought. Subjects were required to provide informed consent prior to participating in the study and had the right to withdraw from the project at any point. Additionally, participants had distinct decision points (from participating in surveys and attending the training program, to sitting for placement interviews and deciding to accept employment contracts) where they were able to reaffirm or withdraw consent. For example, participants were asked to provide informed consent at each survey wave: baseline, midline, and endline. The informed consent process is central to the study design (Humphreys 2015; APSA 2020): the participants themselves were the parties most affected by the intervention, and they had clearly marked opportunities throughout the process in which to provide and withdraw consent.

Principle 6: Political science researchers should carefully consider any use of deception and the ways in which deception can conflict with participant autonomy. No deception was used in this study.

Principle 7: Political science researchers should consider the harms associated with their research. One of the major obstacles to fair labor migration is the high costs of migration, often due to illegal recruitment fees (Sasikumar and Thimothy 2015). Prospective migrants may also be subject to the possibility of exploitation overseas. We strived to minimize both of these costs and risks for participants. We designed our skills training and placement program for employment within the hospitality sector, which is relatively reputable, remunerative, and desirable compared to sectors where labor violations had previously been reported (e.g., construction or household work). We worked closely with New York University–Abu Dhabi Office for Compliance & Risk Management to carefully vet project partners and employers. We scrutinized our recruitment partner closely and worked alongside them to screen and assess specific employers that entered the placement program for fair recruitment practices, working conditions, and migrant worker treatment. Employers agreed to charge no recruitment fees, sponsor and guide prospective employees through the work visa authorization process for the receiving country, cover expenses for round-trip flights, visas, and other immigration costs, help recruited workers relocate and find housing abroad, provide competitive salaries and benefits, and enter into labor contracts that permitted

workers to switch employers or leave their jobs at any time. All labor contracts were registered with governmental agencies in both home and host countries. To minimize participants' financial obligations, training (including tuition, course materials, and on-the-job training) was provided free of charge. While not all participants may eventually obtain employment in the GCC, their training was deemed broadly useful for jobs in the hospitality sector.

Cognizant of potential power differentials between employees and employers, we strove to empower participants by informing them of their rights and resources in destination countries. The GCC states have passed several decrees in recent years that require employers to cover recruitment expenses (including visas and costs of travel), provide competitive salaries and benefits, and furnish housing and health fees for foreign workers. New reforms allow workers to leave their jobs at any time (subject to contractual obligations) and make it easier for workers to switch employers. Under the new policies in the U.A.E., for instance, prospective migrants sign a standard employment offer in their home country that is registered at the Ministry of Human Resources and Emiratisation (MoHRE) before a work permit is issued. Once the worker arrives in the country, the agreement becomes registered as the contract and no changes are allowed unless the employer extends further benefits to the worker. Our project provided subjects with detailed information regarding the locations and helpline numbers of MoHRE offices. Additionally, the Ministry of External Affairs of the Government of India has established Indian Workers Resource Centres in GCC countries that provide helplines and conduct awareness classes and counseling programs on legal, financial, and social issues. Our project ensured that subjects were aware of these resources and had access to them. In addition, in order to assist with integration and reintegration, our project provided participants with access to comprehensive information regarding legal and counseling services both in the GCC states and in Mizoram. They were made aware of the option of availing counseling services free of cost (with the cost of these services covered by the project).

We took a number of steps to guarantee that participants were provided extensive information regarding the potential risks associated with international employment before agreeing to participate in the training and recruitment program. Individuals attended information sessions detailing opportunities and challenges associated with overseas employment. During these presentations, subjects were informed about the potential risks associated with the process of international employment, including the risk of labor law violations by employers. Additionally, we designed the project such that our field research team would follow up regularly with all participants who undertook employment abroad to check on their wellbeing and safety.

Principle 8: Political science researchers should anticipate and protect individual participants from trauma stemming from participation in research. Under Principles 1 and 7, we discussed the steps taken to protect participants from harms stemming from this research project. In addition to providing migrants with information on risks, rights, and resources for working in the GCC, we have followed up with subjects regularly outside of the three survey waves.

We wished to ensure that those who have received job offers abroad, in particular, did not face harm from employment practices in the GCC. To address this possibility, our local research manager contacted research subjects regularly to make sure that they received help from our recruitment partner in obtaining necessary documents and information prior to migration, that after arrival to the host country employers did not violate their rights, and that during the Covid-19 pandemic they had the resources to return home or to stay in the GCC, according to their wishes. After the endline survey, we also conducted long-form, semi-structured interviews with individual subjects who had migrated abroad in order to better understand the migration experience and to provide access to counselling, if needed. Within these interviews, we specifically asked respondents if they had experienced any discrimination in the workplace and none of the respondents indicated any such experience.

Principle 9: Political science researchers should generally keep the identities of research participants confidential; when circumstances require, researchers should adopt the higher standard of ensuring anonymity. We took steps to keep our participants' identities confidential in this project. Enumerators collected the names and contact information of respondents, but that information was immediately encrypted and uploaded to a secure central server. Only the project investigators and the survey team' project manager were able to access the file linking the encrypted identifying information to the anonymous numerical ID associated with each respondent. In other words, anyone else working on the survey (e.g., enumerators, other employees of the survey firm, etc.), was only able to see a number ID associated with the survey responses. In any reproduction material, we will only make the numerical IDs of respondents available, stripped of any identifying information.

Principle 10: Political science researchers conducting studies on political processes should consider the broader social impacts of the research process as well as the impact on the experience of individuals directly engaged by the research. In general, political science researchers should not compromise the integrity of political processes for research purposes without the consent of individuals that are directly engaged by the research process. Besides the research subjects, one other group of individuals directly impacted by our study was the subjects' family members. Therefore, it was important that families were aware of the process, costs, and benefits of the program. During the registration process, the project manager encouraged subjects to take information home to their families and discuss the opportunity before signing up. We also held public information sessions open to the community, particularly to interested individuals and their families. At these sessions, the project manager, the head of our local NGO training partner, and one of our co-PIs answered any questions, attempting to be as honest as possible about the purpose, costs, and benefits of the program. Additionally, our study was conducted in conjunction with the Government of Mizoram's Mizoram Youth Commission, with the permission of the Chief Minister of Mizoram, and prominent local community organizations such as the MZP. Receiving government and community buy-in for the study helped ensure that the broader social impacts of the research were understood by relevant stakeholders apart from the research subjects themselves. Note that our study was designed to not interfere with nor compromise the integrity of political processes either in the home country or in any of the host countries.

Principle 11: Political science researchers should be aware of relevant laws and regulations governing their research related activities. Given that India does not have laws about non-clinical human subjects research, the guidelines of the Indian Council of Medical Research to have ethical review boards examine research design were followed by obtaining IRB approval from the home institutions of all members of the research team. In addition, this research project has also complied with all applicable Indian and GCC laws about labor migration by making sure with our recruitment partner that all labor contracts were registered at the appropriate agencies prior to migration. Overall, the program was designed to significantly improve and safeguard recruitment and employment processes for prospective migrants as compared to individuals who decided to migrate on their own accord or through unsupervised private channels. It was anticipated that future government initiatives in the region would be able to benefit from the knowledge generated and the connections created by the program.

A principal reason for working with partners was to ensure that our project followed relevant laws and regulations, both in Mizoram and in the Gulf Region. In Mizoram, we partnered with a state government office (Mizoram Youth Commission) and a local non-governmental organization (SJnDI), who helped us navigate local laws and regulations. In the Gulf Region, our recruitment partner assisted our research subjects in navigating immigration laws and provided legal recourse for any workplace issues. Subjects were also provided a list of counseling services in both Mizoram and the GCC, and were given the option

of availing these services with the cost covered by the program budget.

Principle 12: The responsibility to promote ethical research goes beyond the individual researcher or research team. Throughout the research design and implementation phase, we workshopped the research design and solicited feedback on research ethics with scholars in several social scientific scholarly venues, including conferences on migration, gender, and experimental research (notably Evidence in Governance and Politics).

A.6 Cost-Benefit Comparison for Intervention

Lastly, we conducted a rough estimate of the costs and benefits of our training and recruitment program. This is valuable for two reasons. First, it acts as an impact evaluation for the program as an economic development intervention. Second, it helps inform the discussion of ethical considerations to weigh the benefits for candidates against the costs for researchers.

For costs, we estimated all major costs of conducting the training and recruitment program in 2018 and 2019. This did not include, for example, the costs of the surveys and the time of the research team. It did, however, include travel costs for researchers and for the training program team, as well as all costs for training and placement.

For benefits, we used the endline survey's estimates of individuals' monthly wages at endline. On average, individuals in the treatment group had monthly wages approximately 5,650 INR higher than those in the control group, or 5,530 INR when controlling for pre-treatment covariates. Using the more conservative estimate, we estimated the annual increase in candidates' wages.

Overall, we estimate that the program generated nearly 900 USD per person per year in benefits (despite just 23% of the treatment group migrating) against just over 200 USD per person in costs. Though much of this financial benefit accrued to the migrants themselves, beneficiaries of the program sent significant remittances home to family. We estimate that treatment individuals sent home between 200 and 500 USD more per year than their counterparts in the control group, depending on which measures are used. Even using the more conservative estimates, this suggests that the program paid yearly dividends to the families of treatment individuals that approximately matched the total cost of the program. As an economic development program, the intervention was extremely cost-effective.

B   Balance and Attrition

B.1   Balance Table

The following regressions attempt to predict treatment status by pre-treatment covariates, among each of the four survey stages: baseline (pre-treatment), midline (post-treatment but pre-migration), endline (two years post-migration), and the household survey. The covariates include both demographic characteristics and pre-treatment measures of key outcome variables. We find little evidence of significant differences between treatment and control group in any of the three survey stages, even after attrition. In fact, the treatment groups were remarkably balanced. Not one of the ten pre-treatment covariates predicted treatment status at any stage, and the omnibus F-test (p-values at the bottom) shows that even the combination of all ten variables provides no predictive value on treatment group at any stage. This balance is partly because the subjects were grouped into demographically similar pairs for treatment assignment.

Table A.1: Costs and Benefits of the Program

Costs of Intervention	
Training Program (USD)	22,200
Location Rental for Training (USD)	4,000
Advertising & Registration Costs (USD)	1,000
Visa & Certification Assistance for Candidates (USD)	1,700
One Year of Salary for Program Manager (USD)	8,000
Travel Costs for Research Team (USD)	6,000
Total Cost (USD)	**42,900**
Cost Per Person (USD)	**220**
Benefits to Candidates (Per Year)	
Monthly Wages Increase Per Person (INR)	5,530
Yearly Wages Increase Per Person (USD)	**885**
Benefits to Families (Per Year)	
Monthly Remittances Per Person, Self-Reported (INR)	3,150
Monthly Remittances Per Household, Family-Reported (INR)	1,340
Yearly Remittances Per Person (USD)	**200-475**

B.2 Tests for Attrition Bias

In addition to the balance tests before and after treatment (and attrition), we also conducted two tests for attrition bias in the midline, endline, and household surveys.

First, we tested whether attrition was greatly affected by treatment assignment itself – i.e. whether the differences in response rates between the treatment and control groups are larger than what might be expected based purely on chance. For the midline and endline surveys, there is no significant evidence that the treatment affected response rates. The RI-based test shows that even if the treatment had no effect on attrition in any individual case, the randomization procedure would have resulted in larger differences between the two groups in almost 30% of cases for the endline and 40% of cases for the midline. For the household survey, there is a clear difference in response rates between the treatment and control groups: the control group responded at a much higher rate (84%) than the control group (70%).

Second, we tested whether attrition rates for the midline or endline surveys were affected by any pre-treatment covariates. For each survey, we ran three regressions predicting survey response based on pre-treatment covariates. The first column predicts response rates based on the seven key demographic covariates. The second column adds in the pre-treatment measures of the key outcome variables: economic status, economic confidence, and economic policy attitudes. For the midline and endline surveys, these variables provide no additional predictive value, as shown by the F-tests at the bottom of the tables, whose p-values range roughly from .2 to .5. The household surveys do suggest some shift in respondents, with family members of those employed and economically confident at the baseline less likely to respond to the household survey.

The third column of each table adds in interaction terms to test whether each of these covariates differentially affected attrition in treatment and control groups. For the midline and endline surveys, there was no evidence overall that pre-treatment characteristics systematically predicted attrition in the treatment or control group. While there are a few significant effects on response rate, these are to be expected because so many explanatory variables are being tested. Omnibus F-tests show that these models also do not

Table B.2: Balance Test on Four Surveys

	Baseline	Midline	Endline	Household
	Dependent variable: Treatment			
Age	−0.008	−0.005	−0.006	−0.010
	(0.009)	(0.011)	(0.011)	(0.010)
Male	0.004	0.016	−0.041	−0.043
	(0.053)	(0.062)	(0.067)	(0.060)
Education	0.028	0.008	0.060	0.014
	(0.033)	(0.038)	(0.041)	(0.036)
Employed	0.016	−0.125	−0.130	−0.134
	(0.111)	(0.130)	(0.145)	(0.134)
Scheduled Tribe	−0.045	−0.057	−0.087	−0.063
	(0.123)	(0.162)	(0.166)	(0.134)
Married	0.125	0.142	0.254	0.108
	(0.203)	(0.310)	(0.315)	(0.263)
English Ability	0.001	−0.015	−0.005	−0.006
	(0.026)	(0.030)	(0.032)	(0.029)
Economic Status	−0.015	0.036	0.060	0.049
	(0.042)	(0.051)	(0.053)	(0.049)
Economic Confidence	−0.014	0.023	0.011	−0.028
	(0.039)	(0.045)	(0.052)	(0.047)
Redistribution Attitudes	−0.015	−0.017	0.018	0.002
	(0.025)	(0.029)	(0.030)	(0.029)
Observations	384	286	244	299
F-Stat P-Value	.994	.992	.893	.970
F-Stat P-Value (RI)	.959	.978	.823	.922

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table B.3: Response Rates: Treatment vs. Control Group

	Midline	*Endline*	*Household*
Response Rate: Treatment Group	76.0 %	65.8%	70.4%
Response Rate: Control Group	71.9 %	60.7%	84.2%
Difference in Response Rate	4.1 %	5.1%	13.8%
P-Value: Two-Sample T-Test	.358	.296	.001
P-Value: RI-based Test	.392	.268	.002

provide any predictive value beyond what would be expected from randomly-generated covariates. For the household survey, these models suggest that the bias in response toward the families of lower-income and unemployed individuals happens primarily in the treatment group. Given that higher-income individuals are generally less supportive of redistribution, this should bias against our results.

Table B.4: Predictors of Response Rate:Midline   Table B.5: Predictors of Response Rate: Endline

Table B.4: Predictors of Response Rate:Midline

	Dependent variable: Response		
Age	−0.010	−0.010	−0.014
	(0.008)	(0.008)	(0.010)
Education	0.0001	0.004	0.030
	(0.028)	(0.028)	(0.038)
Scheduled Tribe	0.128	0.131	0.099
	(0.105)	(0.106)	(0.164)
Employed	−0.024	0.031	0.250*
	(0.065)	(0.094)	(0.136)
Married	−0.236	−0.208	−0.171
	(0.172)	(0.175)	(0.295)
Male	−0.046	−0.039	−0.071
	(0.045)	(0.046)	(0.065)
English Ability	0.006	0.012	0.033
	(0.022)	(0.022)	(0.031)
Economic Status		−0.035	−0.104**
		(0.035)	(0.047)
Economic Confidence		−0.021	−0.067
		(0.034)	(0.047)
Redistribution Attitudes		−0.004	−0.004
		(0.022)	(0.030)
Treatment			−0.391
			(0.550)
Treat x Age			0.010
			(0.016)
Treat x Education			−0.061
			(0.057)
Treat x ST			0.055
			(0.221)
Treat x Employed			−0.437**
			(0.190)
Treat x Married			−0.136
			(0.373)
Treat x Male			0.046
			(0.093)
Treat x English			−0.043
			(0.045)
Treat x Econ. Status			0.148**
			(0.071)
Treat x Econ. Confidence			0.095
			(0.069)
Treat x Redist. Attitudes			0.0002
			(0.043)
Observations	389	384	384
F-Stat P-Value	.252	.399	.417
Note:		*p<0.1; **p<0.05; ***p<0.01	

Table B.5: Predictors of Response Rate: Endline

	Dependent variable: Response		
Age	0.001	−0.001	−0.006
	(0.008)	(0.009)	(0.011)
Education	0.018	0.014	−0.018
	(0.031)	(0.031)	(0.042)
Scheduled Tribe	0.123	0.108	0.154
	(0.116)	(0.117)	(0.180)
Employed	−0.098	−0.110	0.042
	(0.072)	(0.104)	(0.150)
Married	−0.137	−0.140	−0.072
	(0.190)	(0.193)	(0.324)
Male	−0.031	−0.042	0.014
	(0.050)	(0.051)	(0.072)
English Ability	0.043*	0.036	0.040
	(0.024)	(0.024)	(0.034)
Economic Status		0.008	−0.069
		(0.039)	(0.052)
Economic Confidence		0.047	0.024
		(0.038)	(0.052)
Redistribution Attitudes		−0.004	−0.048
		(0.024)	(0.032)
Treatment			−0.714
			(0.604)
Treat x Age			0.011
			(0.017)
Treat x Education			0.066
			(0.062)
Treat x ST			−0.064
			(0.242)
Treat x Employed			−0.317
			(0.209)
Treat x Married			−0.093
			(0.410)
Treat x Male			−0.135
			(0.102)
Treat x English			−0.014
			(0.049)
Treat x Econ. Status			0.175**
			(0.078)
Treat x Econ. Confidence			0.061
			(0.075)
Treat x Redist. Attitudes			0.087*
			(0.048)
Observations	389	384	384
F-Stat P-Value	.314	.461	.220
Note:		*p<0.1; **p<0.05; ***p<0.01	

Table B.6: Predictors of Response Rate: Household

	Dependent variable: Response		
Age	0.010	0.009	0.011
	(0.007)	(0.007)	(0.010)
Education	0.023	0.016	0.044
	(0.026)	(0.026)	(0.035)
Scheduled Tribe	−0.057	−0.058	−0.058
	(0.100)	(0.100)	(0.153)
Employed	−0.128**	−0.191**	−0.002
	(0.062)	(0.090)	(0.130)
Married	−0.191	−0.212	−0.225
	(0.163)	(0.164)	(0.274)
Male	−0.019	−0.036	0.042
	(0.043)	(0.043)	(0.061)
English Ability	0.031	0.021	0.024
	(0.021)	(0.021)	(0.029)
Economic Status		0.034	−0.041
		(0.034)	(0.045)
Economic Confidence		0.058*	0.085*
		(0.032)	(0.043)
Redistribution Attitudes		0.020	−0.004
		(0.020)	(0.027)
Treatment			0.379
			(0.509)
Treat x Age			−0.008
			(0.015)
Treat x Education			−0.045
			(0.053)
Treat x ST			0.006
			(0.205)
Treat x Employed			−0.372**
			(0.180)
Treat x Married			−0.041
			(0.347)
Treat x Male			−0.152*
			(0.086)
Treat x English			−0.012
			(0.041)
Treat x Econ. Status			0.165**
			(0.068)
Treat x Econ. Status			−0.056
			(0.064)
Treat x Redist. Attitudes			0.048
			(0.040)
Observations	389	384	384
F-Stat P-Value	.080	.022	.015

Note: $^*p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

B.3 Controlling for Demographic Imbalances

Given the attrition in our surveys – and especially the slight imbalances in attrition in the household survey – it is worth asking whether our main findings on redistribution attitudes would be robust to including pre-treatment demographic controls. Therefore, we expand on the results in Table 3 and Table 4 by controlling for all pre-treatment demographic characteristics.

Main Endline Results

For the main individual-level survey, we find nearly identical results when including baseline demographic controls. Individuals in the treatment group were approximately one third of a standard deviation more fiscally conservative than those in the treatment group. The findings are slightly less precise because of the inclusion of more unpredictive pre-treatment covariates, but otherwise are extremely similar.

Table B.7: Redistribution Attitudes Results with Controls (Endline)

	Index	Components		
		Taxes	Mobility	Inequality
Treatment	0.330**	0.115	0.094	0.232
	(0.145)	(0.140)	(0.077)	(0.149)
Age	−0.005	0.030	−0.005	−0.026
	(0.025)	(0.024)	(0.013)	(0.026)
Male	−0.105	−0.014	−0.067	−0.025
	(0.148)	(0.143)	(0.079)	(0.152)
Education	0.049	−0.033	0.010	0.089
	(0.091)	(0.087)	(0.048)	(0.093)
Employed	−0.597*	−0.111	−0.041	−0.750**
	(0.321)	(0.310)	(0.172)	(0.331)
Scheduled Tribe	−0.097	0.046	0.028	−0.245
	(0.367)	(0.354)	(0.195)	(0.377)
Married	−0.849	−0.420	−0.257	−0.448
	(0.696)	(0.672)	(0.371)	(0.717)
English	−0.070	0.008	−0.017	−0.084
	(0.071)	(0.069)	(0.038)	(0.073)
Econ. Status	0.224*	0.050	0.080	0.155
	(0.117)	(0.113)	(0.063)	(0.121)
Econ. Confidence	0.030	−0.021	0.065	−0.054
	(0.115)	(0.111)	(0.061)	(0.118)
Redist. Attitudes	0.022	0.097	−0.041	0.018
	(0.067)	(0.065)	(0.036)	(0.069)
Observations	244	242	243	244
Note:			*p<0.1; **p<0.05; ***p<0.01	

Household Results

For the household survey, the results in B.8 suggest that our results for the household may actually be stronger and more surprising when adjusting for these biases. When controlling for pre-treatment demographics, the parents and siblings of treatment individuals are even more supportive of redistribution relative to those of control individuals. This, notably, is the opposite effect as the effect on the attitudes of treatment individuals themselves: while migrants become less supportive of redistribution, their parents and siblings become more supportive.

Table B.8: Redistribution Attitudes Results with Controls (Household)

|	Index	Components		
		Taxes	Inequality	Mobility
Treatment	−0.271**	−0.100	−0.212**	−0.119
	(0.114)	(0.138)	(0.106)	(0.075)
Age	0.006	−0.015	0.011	0.009
	(0.020)	(0.024)	(0.018)	(0.013)
Male	−0.049	−0.147	0.135	−0.071
	(0.117)	(0.141)	(0.109)	(0.076)
Education	0.021	−0.136	0.085	0.046
	(0.069)	(0.084)	(0.064)	(0.046)
Employed	−0.334	−0.163	−0.008	−0.295*
	(0.260)	(0.313)	(0.241)	(0.170)
Scheduled Tribe	0.743***	0.531*	0.537**	0.187
	(0.260)	(0.314)	(0.240)	(0.168)
Married	1.261**	0.774	0.414	0.718**
	(0.511)	(0.616)	(0.470)	(0.330)
English Ability	0.061	0.119*	−0.013	0.016
	(0.057)	(0.068)	(0.053)	(0.037)
Econ. Status	0.056	0.079	−0.051	0.056
	(0.096)	(0.115)	(0.089)	(0.063)
Econ. Confidence	−0.290***	−0.245**	−0.096	−0.144**
	(0.092)	(0.112)	(0.086)	(0.061)
Redist. Attitudes	0.037	0.090	0.008	−0.012
	(0.056)	(0.067)	(0.051)	(0.037)
Observations	300	299	294	291
Note:			*p<0.1; **p<0.05; ***p<0.01	

B.4 Sensitivity Analysis

Sensitivity Analysis for Endline Attrition

Given the relatively low response rate (64%) in the main survey, we investigate how robust our results would be to various scenarios of "missing" redistribution views. To do so, we generated hypothetical outcomes on the redistribution index for those who attrited from our sample and calculated the same ATE estimates as our main results. Each of the seven scenarios projects a different treatment effect among attriters. The most important of these are estimates of the highest and lowest individual-level treatment effects estimated in our subsequent hetereogeneity analysis (Appendix G) and likely migrants / likely non-migrants analysis (Appendix E.3). These both use demographic subgroups to estimate the range of possible individual treatment effects. We also added three other hypothetical ATEs: the ATE estimated among the full sample ($ATE = +.35$), no effect ($ATE = 0$), and an equally strong effect in the opposite direction ($ATE = -.35$). The results are displayed below in Figure B.3.

Of all of these, the only scenario that reduces the effect size even close to statistical insignificance is the final one, which poses a strong treatment effect among attriters in the opposite direction. The only scenario in which this could be true is if attrition were highly positively correlated to redistribution views in the control group or highly negatively correlated with redistribution views in the treatment group. For example, if it were coming from the treatment group, the individuals in the treatment group would have to have a higher correlation between redistribution views and attrition ($r = .23$) than between baseline and endline measures of economic standing ($r = .21$). Even in this extreme case, the ATE estimate would still be relatively large ($ATE = .19$) and statistically significant at the $p < .05$ level.

Figure B.3: Sensitivity Analysis: Main Redistribution Results
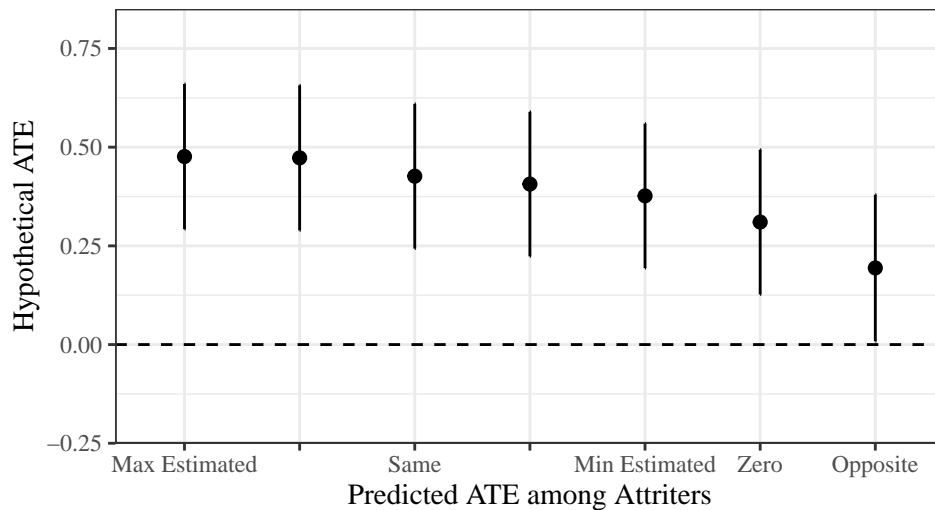
Estimated ATE with seven sets of hypothetical outcomes for attriters. ATE estimates, from left to right: highest estimated effects in heterogeneity analysis, estimated effect among likely migrants, estimated effect among full sample, estimated effect among likely non-migrants, lowest estimated effect in heterogeneity analysis, effect = 0, and effect equally strong as full sample but in the opposite direction (ATE= -.35). 90% confidence intervals shown.

Sensitivity Analysis for Differential Attrition (Household)

Given that response rates differ by treatment condition in the household survey (84% in control, 70% in treatment), it is worth investigating the range of biases that this might introduce to the household results. To do so, we adopted the Gerber and Green (2012) and Lee (2009) "trimming bounds" approach. If there is more attrition in the treatment group, we can drop different groups of 26 control units (to equalize the response rates) and estimate the treatment effect among the remaining observations. This yields a range of different hypothetical treatment effects we might expect to observe among the subset of the population who would have responded regardless of their treatment group ("always-responders").

Figure B.4 shows the range of potential treatment effect estimates. The leftmost and rightmost estimates drop the 26 most pro-redistribution and 26 most anti-redistribution control respondents, respectively, in order to hypothetically correct for the most extreme possible bias from differential attrition. The intermediate scenarios drop random selections of different units (drawing from the top half, the top three quarters, the bottom three quarters, and the bottom half, respectively.

The results suggest that the treatment had somewhere between no effect and a large pro-redistribution effect on the attitudes of family members. Even making the most extreme possible assumption, that differential attrition erased only the most anti-redistribution family members in the treatment group, the estimated treatment effect is estimated to be almost exactly zero – and not, notably, a anti-redistribution effect as observed among migrants themselves. In the other scenarios, the ATE was estimated to be negative, albeit only statistically significant if the bias was neutral or against the pro-redistribution finding (as the prior analysis with demographic controls suggests).

Figure B.4: Sensitivity Analysis: Household Redistribution Results
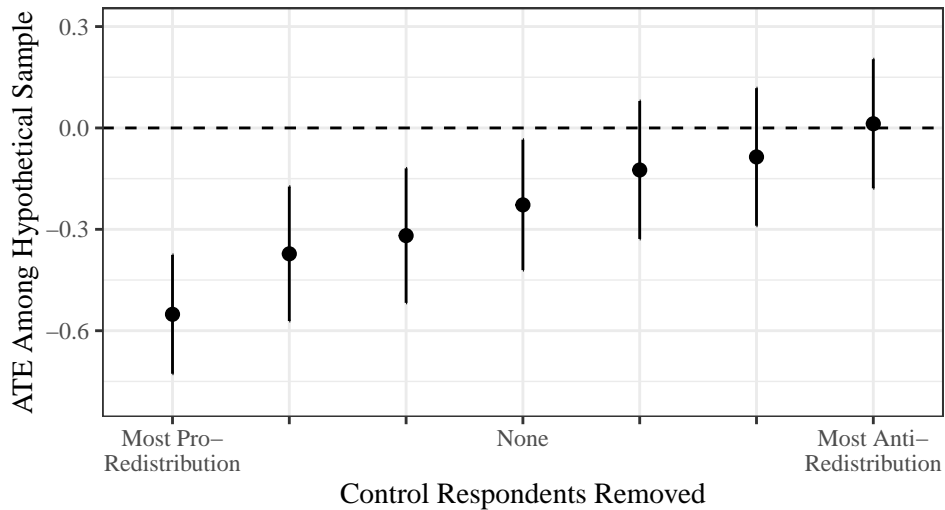
Estimated ATE with current household sample (center) and six hypothetical samples removing a subset of control respondents. Respondents removed, from left to right: 26 most pro-redistribution control respondents, random selection from 50th percentile and below, random selection from 75th percentile and below, (none), random selection from 25th percentile and above, random selection from 50th percentile and above, and 26 most anti-redistribution respondents. 90% confidence intervals shown.

B.5 Attrition Rates on Similar RCTs

For comparison, we also looked at response rates on a set of comparable studies. In particular, we were interested in RCTs and observational studies that met three conditions that define our attrition scenario. First, they had to attempt to recontact specific individuals (or at least individual households) in order to study individual-level effects. Recontact is much harder if it requires reaching specific individuals rather than any individual in a community. Second, the studies had to be development or migration-oriented studies working with relatively low-income populations in low- or middle-income countries. These populations tend to be mobile and have tenuous contact information. Third, the studies had to attempt recontact with these individuals multiple years after the initial contact. In our study, participants had more than two and a half years to move, change contact information, and change priorities. This was not intended to be an exhaustive search, but a brief examination of promisingly similar papers – including all of the papers cited elsewhere in the paper.

On 2-5 year follow-up surveys of specific individuals, the response rates in these surveys ranged from 61% to 91%. They are described below in descending order of response rate.

- Jensen (2012) reports a 91% response rate on a two-year follow-up survey on a job program for women in rural India. They also report that the survey took a team six months to administer, and that the population was more geographically stable than ours.

- Gibson and McKenzie (2014) report a 78% response rate on a two-year follow-up on a migration lottery in Tonga. Importantly, the unit of interest is the household, so any family member of the individual could fill out the survey, as in our family member survey, which had a higher response rate.

- Beam, McKenzie and Yang (2016) report a 73% response rate on a two-year follow-up on a migration encouragement RCT in the Philippines. As with our study, they had a higher response rate in an additional household survey.

- Beegle, De Weerdt and Dercon (2011) report a 70% response rate in a ten-year follow-up survey on a migration study in Tanzania. They spent significant resources to track all baseline survey participants throughout the duration of the study.

- Mobarak, Sharif and Shreshta (2021) report 69% and 68% response rates among the main treatment and control group in a five-year follow-up on a visa lottery for Bangladeshi workers to move to Malaysia.

- Naidu, Nyarko and Wang (2023) report a 65% response rate on an initial follow-up survey approximately one-and-a-half years after a migration RCT in India. Additional tracking surveys, family surveys, and administrative data provide data on an aditional 16% of their sample.

- Blattman, Fiala and Martinez (2020) report 63% and 61% response rates on two- and four-year follow-ups, respectively, examining a youth jobs program in Uganda.

Given the response rates on these comparable surveys, and given the challenge of reaching our highly mobile, young population, one should probably expect a response rate between 60% and 75% on a two-and-a-half-year follow-up survey like ours.

C   Key Outcome Questions

Table C.9: Questions: Economic Position

Question	Options
Are you currently employed?	Yes
	No
[If employed] What are your currently monthly wages?	Amount (in INR)
What category best describes your total monthly household income?	Less than Rs. 5,000
	Rs. 5,001 - Rs. 10,000
	Rs. 10,001 - Rs. 20,000
	Rs. 20,001 - Rs. 30,000
	Rs. 30,001 - Rs. 40,000
	Rs. 40,001 - Rs. 50,000
	Rs. 50,001 - Rs. 100,000
	Rs. 100,001 and above
Please indicate the number of the following items in your home: Car Motorbike Refrigerator Mobile phone with internet connection Computer Washing Machine	[Number for each] [For analysis, we reduced this to a standardized index of material possessions.]

Table C.10: Questions: Family Planning

Question	Options
[If not married] At what age do you plan to marry?	[Number]
[If no children] At what age do you plan to have children?	[Number]

Table C.11: Questions: Confidence in Economic Prospects

Question	Options
Do you think your next job will pay better or worse than the average salary in Mizoram?	Much better
	Somewhat better
	About the same
	Somewhat worse
	Much worse
In the next year, do you think your own and your family's economic situation will be better or worse?	Much better
	Somewhat better
	About the same
	Somewhat worse
	Much worse
When you are the age your parents are now, do you think you will be better off or worse off financially than them?	Much better off
	Somewhat better off
	About the same
	Somewhat worse off
	Much worse off
Do you agree or disagree that in the future you will be able to advance professionally, get promoted, and earn higher incomes?	Strongly agree
	Somewhat agree
	Neither agree nor disagree
	Somewhat disagree
	Strongly disagree

Table C.12: Questions: Economy Policy Preferences

Question	Options
In general, do you think that it is possible for someone who is born poor to become rich by working hard?	It is almost impossible
	It is somewhat possible
	It is very possible
Do you agree or disagree: Should the government reduce income differences between the rich and the poor?	Strongly agree
	Somewhat agree
	Neither agree nor disagree
	Somewhat disagree
	Strongly disagree
Do you agree or disagree: The government should lower taxes for ordinary people, even if it means that it will have less funding for public services to help the poor in Mizoram.	Strongly agree
	Somewhat agree
	Neither agree nor disagree
	Somewhat disagree
	Strongly disagree

D Main Results

All of our major hypotheses posit an effect of treatment assignment (τ) on some attitude or behavior (y). For each outcome, we also have a measure of the same outcome (or a similar outcome) from the baseline survey (X). The results, then are estimated using OLS with the following model:

$$y_i = \beta_0 + \beta_1 \tau_i + \alpha X_i + \varepsilon_i \qquad (2)$$

Due to the limited number of observations, small size of blocks, and the possibility of attrition, we do not use block (pair) fixed effects. The main p-values given in the paper are calculated using randomization inference with this model, but here we also include the p-values derived from OLS standard errors. We also include the RI-based p-values for the difference-in-means between the treatment and control group.

Table D.13: Full Results: Migration

	Diff-in-Means			OLS			
	Treat	*Ctrl*	*P(RI)*	*ATE*	*P(RI)*	*P(OLS)*	*N*
Moved Overseas	.23	.03	.000	+ .20	.000	.000	248
Training Program	.58	.43	.011	+ .14	.009	.012	245
Job Offer	.34	.08	.000	+ .26	.000	.000	231
Moved in India	.13	.32	.000	− .19	.000	.000	247

Figure D.5: Location of Subjects over Time

23
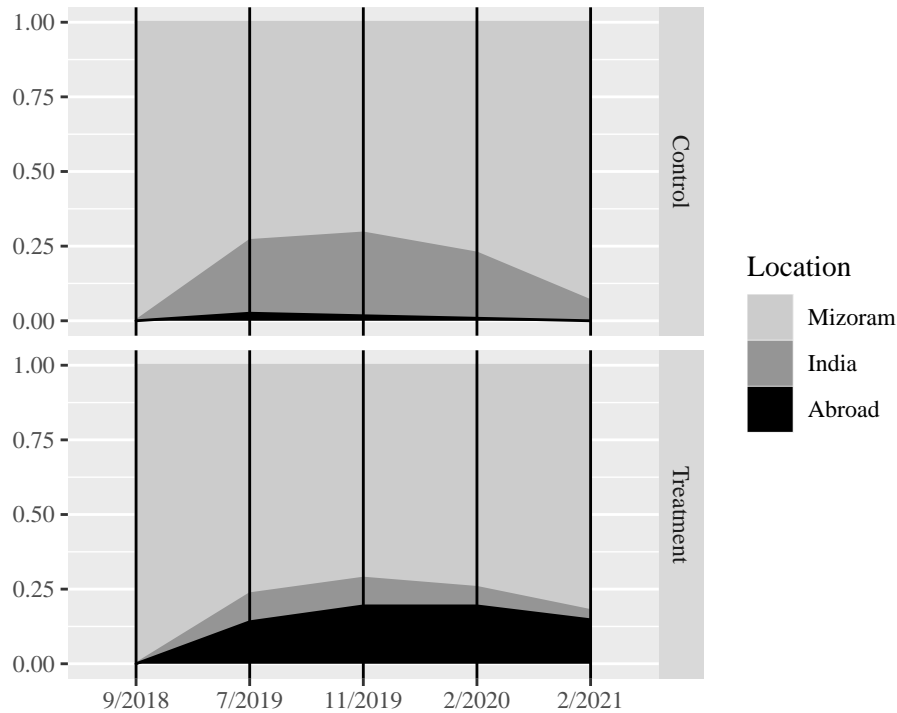
Table D.14: Barriers in the Migration Process for Treatment and Control Subjects

	Treatment	*Control*
Did not apply for a job abroad	48	64
Applied, but did not receive an offer	18	27
Received an offer, but did not accept	7	4
Accepted a job, but did not receive a visa	2	3
Received a visa, but did not move	1	0
Moved abroad	23	2

Percentage of each group that stopped at a certain step of moving abroad.

Table D.15: Results: Efforts to Migrate

	Diff-in-Means			OLS			
	Treat	*Ctrl*	*P(RI)*	*ATE*	*P(RI)*	*P(OLS)*	*N*
Index	**.742**	**—**	**.000**	**+ .746**	**.000**	**.000**	**290**
Obtained Passport	.80	.40	.000	+ .40	.000	.000	289
Researched Labor Laws (1-3)	1.91	1.70	.004	+ .21	.004	.004	288
Researched Employers (1-3)	1.70	1.54	.002	+ .17	.002	.002	287

Figure D.6: Monthly Wages over Time
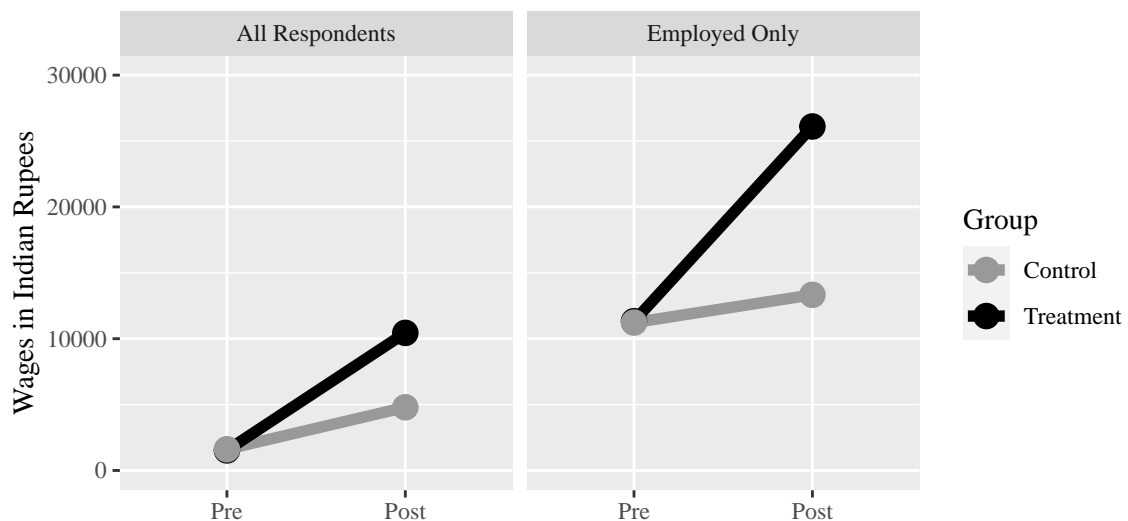
Endline wages in the treatment group translate to approximately 140 USD/month for all respondents and 350 USD/month for employed respondents.

Table D.16: Full Results: Economic Standing

	Diff-in-Means			OLS			
	Treat	*Ctrl*	*P(RI)*	*ATE*	*P(RI)*	*P(OLS)*	*N*
Economic Index	.584	—	.000	+ .558	.000	.000	248
Employed	.43	.39	.233	+ .05	.217	.222	246
Family Income	5.10	4.45	.003	+ .61	.004	.003	238
Material Goods Index	.386	—	.002	+ .349	.001	.001	248
Wages	10,440	4,790	.001	+ 5,530	.001	.001	234

Table D.17: Full Results: Family Planning

	Diff-in-Means			OLS			
	Treat	*Ctrl*	*P(RI)*	*ATE*	*P(RI)*	*P(OLS)*	*N*
Endline: Life Plans Index	.635	—	.000	+ .649	.000	.000	234
Marriage Age	31.3	29.7	.000	+ 1.8	.000	.000	223
Childbearing Age	32.4	30.7	.000	+ 1.7	.000	.000	233
Midline: Life Plans Index	.118	—	.188	+ .122	.169	.171	267
Marriage Age	30.5	30.3	.283	+ 0.3	.192	.191	250
Childbearing Age	31.5	31.1	.131	+ 0.4	.142	.139	251

Table D.18: Full Results: Economic Confidence

	Index	*Components*			
		Mobility	Wages	Family	Lifetime
Treatment Effect	+ .197	+ .12	+ .28	+ .10	- .09
	(.139)	(.11)	(.09)	(.08)	(.08)
RI P-Value	.090	.118	.002	.128	.859
Baseline Control?	Yes	Yes	Yes	Yes	Yes
Control Mean	0	4.20	3.46	3.79	4.03
N	243	243	243	243	243

Note: Treatment effects measured by OLS, controlling for baseline measure of DV. P-values are one-sided according to pre-registered hypothesis. All components are measured on a scale from 1 (strongly disagree) to 5 (strongly agree). *Mobility:* In the future, will you be able to advance professionally? *Wages:* Do you think your next job will pay better or worse than the average salary in Mizoram? *Family:* In the next year, do you think your own and your family's economic situation will be better or worse? *Lifetime:* When you are the age your parents are now, do you think you will be better off or worse off financially than them?

Table D.19: Full Results: Economic Policy Preferences

	Diff-in-Means			OLS			
	Treat	*Ctrl*	*P(RI)*	*ATE*	*P(RI)*	*P(OLS)*	*N*
Endline: Policy Index	.344	—	.006	+ .350	.005	.007	248
Govt Should Lower Taxes	3.91	3.78	.168	+ .13	.163	.174	246
Govt Shouldn't Reduce Inequality	2.04	1.81	.061	+ .23	.062	.059	248
Poor Can Advance	2.56	2.46	.090	+ .10	.079	.085	247
Midline: Policy Index	.206	—	.028	+ .208	.029	.036	288
Govt Should Lower Taxes	4.00	3.89	.145	+ .12	.139	.141	288
Govt Shouldn't Reduce Inequality	2.15	2.06	.267	+ .09	.266	.270	288
Poor Can Advance	2.70	2.62	.080	+ .08	.086	.086	288

D.1 Benchmarking Effect Size

How large is our main effect (+.35 Standard Deviations on a standardized index) relative to other predictors of redistribution attitudes? Here, we benchmark this finding using additional covariates from our own data as well as data from the World Values Survey. To make these effects comparable, we reduced all predictors to binary dummies. From the World Values Survey (Wave 7), we examined the three most comparable questions to our three measures of redistribution attitudes (Questions 106,108, and 110, respectively), and, because these questions were on a different scale (1-9 vs. 1-5), created a standardized index for each country comparable to our main findings. The most recent round of WVS data was not yet available from India itself, but we examined data from the three most comparable close countries available – Pakistan, Bangladesh, and Indonesia – as well as from the United States. We tested the bivariate treatment effect of three commonly-discussed predictors of redistribution attitudes: union membership, gender, and education.

The results (Table D.20) show that our main treatment effect is larger than all 14 of these comparisons. Our main treatment effect is larger than the comparable effect of income, union membership, gender, education, and even pre-existing redistribution attitudes.

Table D.20: Comparing Main Treatment Effect to Other Comparable Effects

Data Source	Independent Variable	ATE
Experiment Data	*Treatment Assignment*	*+ .35*
Experiment Data	Baseline Economic Standing (Above Mean)	+ .16
Experiment Data	Baseline Redistribution Attitudes (Above Mean)	+ .01
WVS (Pakistan)	Union Member	- .13
WVS (Pakistan)	Male	+ .04
WVS (Pakistan)	Completed Secondary Education	+.19
WVS (Bangladesh)	Union Member	+.06
WVS (Bangladesh)	Male	+ .09
WVS (Bangladesh)	Completed Secondary Education	+.12
WVS (Indonesia)	Union Member	- .08
WVS (Indonesia)	Male	+ .09
WVS (Indonesia)	Completed Secondary Eudcation	+ .23
WVS (USA)	Union Member	- .13
WVS (USA)	Male	+ .19
WVS (USA)	Completed Secondary Eudcation	- .10

E Exploratory Tests for Mechanisms

E.1 Institutional Trust

Table E.21: Trust In Institutions

	Index	*Components*					
		Trust?			Capable?		
		Natl	State	Local	Natl	State	Local
Treatment Effect	+.248	+.05	+.22	+.29	+.12	+.33	+.07
(SE)	(.13)	(.10)	(.11)	(.15)	(.11)	(.12)	(.16)
RI P-value	.056	.605	.053	.054	.282	.006	.627
Control Mean	—	2.93	2.70	3.03	2.97	2.70	3.10
N	248	248	248	248	248	248	248

Note: *Trust:* On a scale of 1 to 4, where 1 is "not at all," and 4 is "completely," could you please tell me how much you TRUST each government? *Capable:* On a scale of 1 to 4, where 1 is "not at all" and 4 is "completely," could you tell me how much you think each government is capable of solving problems in Mizoram?

E.2 Job Training

One question regarding our results is whether our treatment effects are due to unrelated aspects of the training program itself (e.g., such as social interactions with other participants) rather than subjects' improved economic prospects. As stated in the paper, our job training program does not appear to be particularly unusual in the local context. More than one-third of our control group attended a similar training program offered by an alternate training firm, and many more had attended similar programs in the past. To probe this question further, we tested whether job training attendance was predictive of our key outcomes in two ways: (1) within the control group, and (2) within the treatment group, controlling for actually migrating. These tests are not causally-identified, but we control for our standard battery of pre-treatment demographics (age, gender, employment status, marriage status, education level, and scheduled tribe status) and the pre-treatment measure of each outcome variable. We do not see any evidence that training has any significant effect on the primary outcomes. These tests were not pre-registered, and we view them as exploratory and suggestive non-experimental investigations.

Table E.22: Effect of Job Training on Key Outcomes (Control Group)

	Dependent variable:			
	Econ. Status	Life Planning	Confidence	Redist. Attitudes
Attended Training	0.003	0.028	0.116	0.198
	(0.173)	(0.178)	(0.187)	(0.187)
Pre-Econ. Status	0.582***			
	(0.128)			
Pre-Life Planning		0.488***		
		(0.116)		
Pre-Confidence			−0.032	
			(0.141)	
Pre-Attitudes				0.040
				(0.082)
Controls?	Yes	Yes	Yes	Yes
Observations	118	103	116	117

Note: *p<0.1; **p<0.05; ***p<0.01

Pre-treatment controls: age, gender, employment status, marriage status, education level, and scheduled tribe status.

Table E.23: Effect of Job Training on Key Outcomes (Treatment Group)

	Dependent variable:			
	Econ. Status	Life Planning	Confidence	Redist. Attitudes
Attended Training	0.038	0.015	0.063	−0.301
	(0.246)	(0.271)	(0.221)	(0.238)
Migrated	1.404***	0.280	0.448*	0.246
	(0.293)	(0.332)	(0.259)	(0.284)
Pre-Econ. Status	0.410*			
	(0.208)			
Pre-Life Planning		0.847***		
		(0.171)		
Pre-Confidence			0.360**	
			(0.171)	
Pre-Attitudes				0.004
				(0.109)
Controls?	Yes	Yes	Yes	Yes
Observations	127	118	122	126

Note: *p<0.1; **p<0.05; ***p<0.01

Pre-treatment controls: age, gender, employment status, marriage status, education level, and scheduled tribe status.

E.3 Comparing Effects among Likely Migrants and Non-Migrants

Was this shift primarily about migration, or about economic opportunities that the program gave all participants in the program? To answer this question, we examined how attitudes toward redistribution changed among likely migrants and likely non-migrants. While all treatment group individuals were offered the chance to migrate overseas, some demographic groups were more likely than others to actually do so if given the opportunity. We find that even among individuals who were very unlikely to migrate, the treatment still had a significant effect on redistribution preferences. In other words, the mere exit option of overseas employment appears to have shifted political views.

To identify those likely (and unlikely) to migrate if selected, we conducted an analysis in two steps using a machine-learning algorithm called Bayesian Additive Regression Trees (BART), included in the pre-analysis plan. First, we used BART to identify which pre-treatment characteristics (from the baseline survey) best predicted an individual's decision to migrate among the treatment group. Second, we used this model to identify the individuals in both the treatment and control groups who most resembled the migrants

Table E.24: Main Effects: Likely Migrants vs. Likely Non-Migrants

| | Effect Size | | |
	Likely Migrants	Likely Non-Migrants	Difference
Migrated Overseas	+ .59	+ .06	.53
	(.07)	(.04)	(.08)
Economic Standing	+ 1.12	+ .35	.77
	(.29)	(.18)	(.34)
Redistribution Attitudes	+ .49	+ .29	.20
	(.27)	(.17)	(.32)
N	68	180	

Note: Each row comes from an OLS regression of treatment (with an interaction term by respondent group) on the index of each outcome variable.

in the treatment group. For example, men in our sample were far more likely to migrate, so they received higher propensity scores on average. This resulted in two subgroups based on pre-treatment covariates: "likely migrants," of whom 59% migrated if selected for the program, and "likely non-migrants," of whom just 6% migrated if selected. We then test the effect of the main treatment (selection to the program) within each subgroup. Unlike the comparisons in a standard mediation analysis, these comparisons are each causally identified, although the difference between the two comparisons is not.

First, the results (Table E.24) show that while most of the economic benefits appear to be driven by migration itself, likely non-migrants also benefited from receiving the opportunity to migrate. Among the likely migrants, the treatment had a large effect on economic standing: more than one standard deviation measured by the index, more than three times as large as for the likely non-migrants. However, in the likely non-migrant group, the treatment still had a sizable and statistically significant effect: providing merely the opportunity to move overseas still paid economic dividends. This result is mirrored in the observational data. Among those who remained in Mizoram, employed individuals in the treatment group were earning significantly higher wages (15,700 INR [approx 200 USD] per month) than those in the control group (10,100 INR [125 USD] per month).

Second, the treatment had a large effect on the political opinions of those who were unlikely to move as well as those who were likely to move. Despite having considerably lower economic gains from the treatment, likely non-migrants still became significantly less supportive of taxation and redistribution if selected for the program, and the difference between the treatment effect in the two subgroups is not statistically significant. These results suggest that the mere option to migrate shaped individuals' attitudes toward taxation and redistribution—even for those who did not migrate. This result, too, is reflected in observational comparisons. Even among those who remained in Mizoram, individuals in the treatment group held views that were significantly more fiscally conservative—nearly 0.3 standard deviations in our index.

E.4   Reasons for Migrating

Why did so many individuals in the treatment group choose to migrate overseas rather than stay in India? Because wage differentials are starker across national boundaries than internally (Clemens, Montenegro and Pritchett 2019), migrants likely anticipated greater economic gains internationally than domestically (McKenzie, Gibson and Stillman 2013). Additionally, members of historically marginalized ethnic groups

face systemic barriers to economic advancement in local labor markets (Banerjee and Knight 1985) and thus might find international employment opportunities to be especially appealing.[12] Thus, in low-income countries, particularly for members of historically marginalized groups, international employment provides economic opportunities that are otherwise unattainable in domestic labor markets.

Our results provide strong evidence for this claim. First, program participants viewed international employment as uniquely rewarding. In our midline survey, we asked all participants to rate their interest in job opportunities in the Gulf compared to other parts of India (Table E.25). Respondents consistently reported that compared to mainland India, jobs in the GCC would be better-paying, provide more opportunities for promotions, feature better treatment by employers, and involve less ethnicity-based discrimination. This evidence supports the claim that members of marginalized groups look toward employment in the global economy to escape discriminatory practices in domestic labor markets.

Table E.25: Individuals Perceive International Job Opportunities as More Valuable

	Gulf Region	*Mainland India*
In which place...		
will you be able to get a better paying job?	81%	3%
will employers value your skills more?	61%	10%
are you more likely to be promoted?	40%	4%
are employers more likely to treat you better?	54%	7%
will Mizos face less employment discrimination?	55%	8%

Note: Hypothesis was pre-registered that Gulf jobs would be preferable to Mainland India jobs across all dimensions. Both treatment and control group subjects were polled. Remainder of responses were "Don't Know / Can't Say."

Second, many individuals in the treatment group considered moving abroad, even if they eventually decided not to do so. Treatment group subjects were more likely than those in the control group to have applied for a passport and sought out information about employers and labor laws abroad.[13] None of these activities were included in the training program; therefore, they are evidence of a credible desire—one requiring considerable time and effort—to prepare to move abroad for work. That international opportunities are uniquely attractive for those who obtain access to them explains the sizable effect of our treatment on cross-border labor migration.

F Multiple Comparisons Analysis

As specified in the pre-analysis plan, we also provide a Benjamini-Hochberg false discovery rate analysis for the main pre-registered hypotheses—besides the primary hypothesis related to international migration,

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[12]Minorities encounter discrimination in hiring and promotion, lack access to kinship-based professional networks, and frequently face wage differentials in identical jobs performed by members of majority groups (Gaikwad and Suryanarayan 2021). Overseas employers, by contrast, have fewer incentives to discriminate in hiring and promotion based on social hierarchies in migrant-origin regions, making international employment especially attractive for members of these groups (Osgood and Peters 2017).

[13]Appendix Table D.15 presents the results of a pre-registered hypothesis that the treatment would lead individuals to take concrete steps toward emigration. Passport fees in India range between Rs.1,500 and Rs.4,000, half of the average month's wage for employed individuals at baseline.

which was listed separately. The below analysis uses a conservative false discovery rate of $Q < .05$. The analysis largely confirms the main results of the paper. The correction confirms that the main featured hypotheses at the endline reported in the manuscript all fall below the threshold for false discovery rate. The treatment effect on future confidence at the endline meets neither the $p < .05$ threshold nor the adjusted B-H threshold, which is somewhat unsurprising given that the treatment group had already experienced significant economic gains (unlike at the midline survey, where the effect passed both thresholds). The PAP also included one other hypothesis which we did not include in the paper: that the treatment group would be more likely to say that they were satisfied with their economic situation compared to a year before. We did not include this hypothesis because the wording was accidentally awkward given that the treatment group would have already realized substantial economic gains a year before the endline survey. The treatment group did express more current economic satisfaction, but the difference was relatively small and statistically insignificant.

Table F.26: Benjamini-Hochberg Correction

	P-Value	Target
(H2): Material Economic Status	.000	.01
(H2c): Marriage & Family Decisions	.001	.02
(H2d): Views on Redistribution	.005	.03
(H2b): Future Economic Confidence	.090	.04
(H2a): Current Economic Satisfaction	.172	.05

Pre-analysis plan hypothesis numbers in parentheses.

G   Representatitiveness and External Validity

How might the effects in our sample generalize to other populations of migrants and different contexts? We begin by probing how our experimental sample compares to other samples of overseas migrants in India and Asia. Next, we evaluate external validity concerns by considering whether treatment effects might vary across different populations and contexts, what (Egami and Hartman 2022) term "X-validity" and "C-validity" concerns, respectively.

G.1   Representativeness

To what degree is our experimental sample and context representative of migration from India, the world's largest source of emigrants? In this section, we characterize India's overseas migrant population using data in the Kerala Migration Study (KMS), a comprehensive household survey of the South Indian state of Kerala that has some of India's highest historic rates of out-migration, and overseas migrants in the Indian Human Development Survey (IHDS), a nationally representative survey of Indian citizens. We do so to assess the extent to which our sample conforms to the demographic traits of the country's overseas migrant population. Additionally, we analyze data from the World Values Survey (Round 7), which is one of the few existing nationally representative global surveys that collects information regarding the immigration status of respondents, and the countries of origin for immigrants.

We first compare the profiles of migrants involved in overseas migration and non-migrants. Across both the KMS and IHDS datasets, cross-border migrants are younger, more likely to belong to minority and historically disadvantaged religions, and more likely to have higher educational qualifications than

non-migrants (see Table G.27). Similar to subjects in our study, overseas migrants from Kerala are considerably more likely than non-migrants to have completed secondary education (75 vs 52 percent). This is also true in the IHDS data: overseas migrants from India are 10 percentage points more likely to have completed high school than the general population. Kerala migrants are also significantly more likely to hail from religious minority communities compared to non-migrants (64 vs 45 percent). The IHDS data similarly shows that 26 percent of overseas Indian migrants are Muslim or Christian, compared to the population-wide Muslim or Christian rate of 16 percent. Finally, like in our study, overseas migrants in both the KMS and IHDS data were younger than the average non-migrant.

Table G.27: Characteristics of Overseas Migrants in IHDS and KMS

	IHDS		KMS	
	Gen. Pop.	Overseas Migrants	Gen. Pop.	Overseas Migrants
Age <30	0.57	0.36	0.46	0.81
Age: 31-50	0.25	0.60	0.28	0.19
Age >50	0.18	0.04	0.26	0.01
Male	0.50	0.95	0.47	0.86
At least 10th Standard Educ.	0.23	0.32	0.52	0.75
Minority Religion	0.16	0.26	0.45	0.64

IHDS and KMS asks different questions about migrant's age. While IHDS asks about the current age of migrant household members, KMS asks about age at first migration.

We next assess whether key contextual factors in our study are common in broader out-migration flows from India and Asia. Our study focused on migration from India to autocratic countries in the Persian Gulf. The KMS data provides a breakdown of the destination countries of overseas migrants; as Appendix Figure G.7 shows, the top 6 destination countries for Kerala's migrants are GCC countries, and migrants to these countries far outnumber migrants to democracies such as the United States and United Kingdom. Additionally, the KMS data shows that the average number of return migrants per household was more than half the average number of migrants per household, indicating that circular migration, like in our study, is common in the Kerala context. This is also true of the IHDS data, which shows that the average overseas migrant from India returned home after 20 months, similar to our study.

Figure G.7: Destination Countries of Kerala Migrants
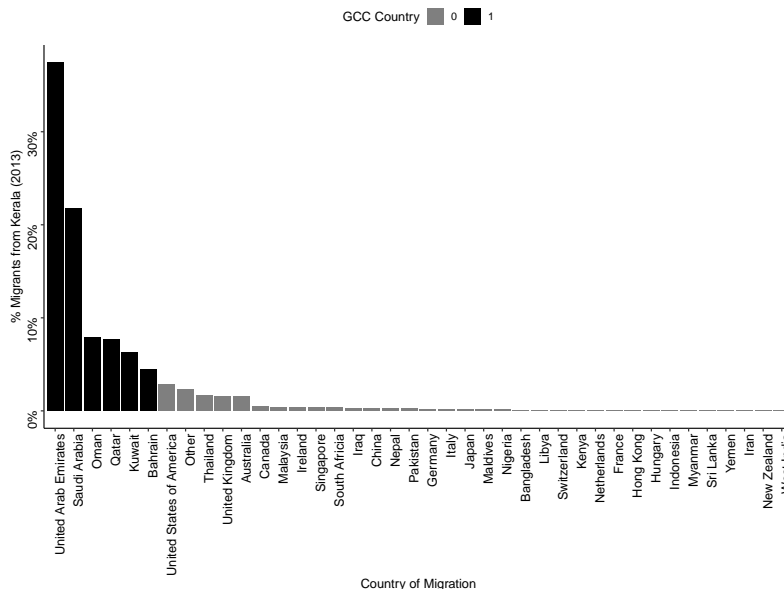
Kerala Migration Survey Round (2013)

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These patterns are corroborated in the WVS data. As Appendix Figure G.8a shows, significant proportions of immigrants from India tend to reside in autocratic nations, as defined by the Varieties of Democracy database. This data actually understates the proportion living in autocratic states because WVS excludes most Gulf states, which are the most common destination for Indian migrants. Appendix Figure G.8b, which considers immigrants from Asia as a whole, shows that more immigrants from Asia reside in autocratic nations than in democratic nations.

Figure G.8: Immigrants, by Political System of Destination Countries

(a) Immigrants from India (b) Immigrants from Asia
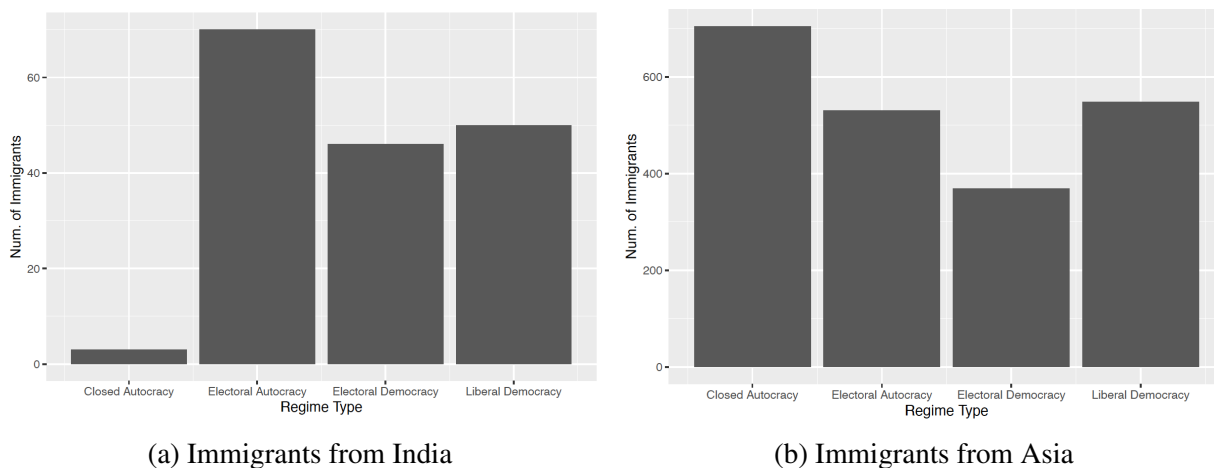
Taken together, analyses of existing datasets on overseas migrants reveals that although our experimental sample is far from representative of India's general population, it generalizes to India's overseas migrant population with respect to age, minority status, and educational qualifications. Additionally, key contextual factors in our study, such as migration to autocracies and circular migration, feature commonly in broader cross-border patterns in India and Asia.

G.2   External Validity by Sample (X-Validity)

"X-validity" concerns relate to the idea that the composition of subjects in experimental samples often varies from those in target populations (Egami and Hartman 2022). The subjects in our study were relatively young, educated, low-income, and largely hailed from minority backgrounds. How might the findings from this sample generalize to other population groups? It is plausible, for example, that less educated or older individuals may still remain pro-redistribution even as young educated individuals became more anti-redistribution.

We investigated X-validity concerns empirically by testing for heterogeneous effects within the sample to assess potential effects outside of the sample. First, we examine pairwise interactions between pre-treatment covariates and our key outcomes: individual and household redistribution attitudes and economic standing (Table G.28). We find no evidence that the treatment effects interacted with any of the demographic covariates we collected, or with baseline measures of economic standing.

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Table G.28: Pairwise Heterogeneous Effects

	Redistribution Views	HH Redistribution Views	Economic Standing
Age	-0.11	-0.43	1.28
Gender	0.82	-0.84	1.81
Education	-0.35	-1.35	0.10
Scheduled Tribe	-0.90	-0.18	1.11
Christian	-0.97	0.36	1.47
Employed (Baseline)	-0.13	0.35	0.60
Wages (Baseline)	0.01	0.21	-0.33
Family Income (Baseline)	0.14	-0.37	-0.92
Assets (Baseline)	-0.43	-0.04	0.96

T-Statistics of pairwise interaction terms between treatment and key covariates for each main outcome.

Second, we used machine-learning estimators to investigate heterogeneity agnostically, following Devaux and Egami (2022), which proposes estimating individual-level treatment effects for all individuals in the sample based on estimates of the heterogenous effects of the treatment using all pre-treatment covariates. The results, presented in Appendix Figure G.9, generally show very little systematic heterogeneity in the treatment effects. Taken together, these two sets of findings suggest that the effects we observe of migration on the redistribution attitudes of migrants and their families are likely to extend to individuals from other socio-economic and demographic groups.

Figure G.9: Estimated Treatment Effects for Each Subject
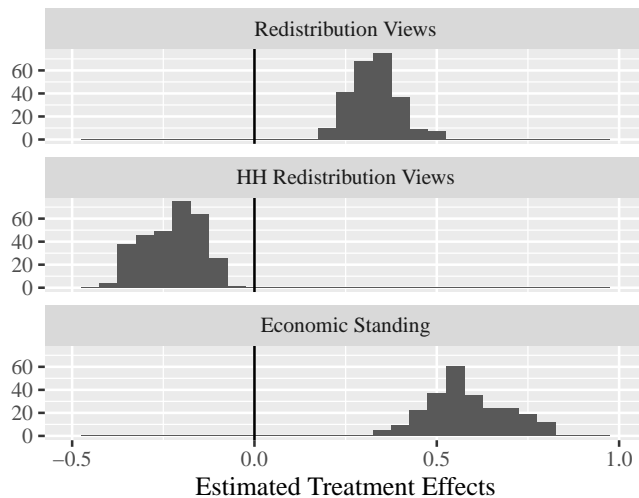
Predicted treatment effects for each individual in our sample, estimated using `exr` package (CRAN). Machine-learning algorithm estimates heterogeneity of treatment effect using all pre-treatment covariates, then predicts treatment effect for each unit.

G.3 External Validity by Context (C-Validity)

"C-validity" concerns question whether experimental results based on one context can generalize to other contexts (Egami and Hartman 2022). Would our findings from Indian migrants working in the Gulf hospitality industry generalize to other industries, other origin countries, and other destination countries? C-validity concerns are very difficult to address empirically with only one context. Due to constraints stemming from resources, logistical capabilities, ethical considerations, and policy environments, we were unable to replicate our study in other cross-border migration contexts; indeed, the intensive and focused nature of our efforts were necessary to successfully induce migration in contrast to the null effects on migration interventions reported in prior work (Beam, McKenzie and Yang 2016).

Nevertheless, based on insights gleaned from theory and fieldwork, we propose a set of key site-level, contextual factors that potentially moderate the effects of overseas migration. In Appendix Table G.29, we hypothesize the effect of migration on redistribution attitudes in a range of different migration contexts, and offer suggestions for research designs that can be employed in future work seeking to study the effects of migration in these alternate contexts. For example, we conjecture that the effect of migration on redistribution attitudes may be smaller in industries with lower average wages and greater exploitation, such as construction or domestic work, may be significantly smaller than those observed in this context. Given that our evidence suggest the results are driven by migrants' greater economic opportunities and independence, industries with less favorable opportunities should see smaller effects. The results are likely to be more similar in relatively high-paying industries like health care and retail.

Table G.29: Key Contextual Factors, Predicted Effects, and Suggested Designs

Context	Hypothesized Effect on Redistribution Attitudes	Suggested Research Designs
Lower-paying industries	Smaller anti-redistribution effects because of smaller effects on income and financial independence	Research Design: Comparing effects of visa lotteries for higher-paying industries (health care, hospitality) and lower-paying industries (construction, domestic work).
Less exploitative migration corridors	Smaller anti-redistribution effects because of smaller or negative effects on financial security.	Research Design: Comparing effects of visa lotteries for destination countries with less exploitation and those with greater exploitation.
Longer-term migration	Larger anti-redistribution effect because migrants develop longer-term horizons and invest in host society integration	Research Design: Comparing effects of migration in countries that permit longer-term versus shorter-term employment contracts.
Destination country with higher taxes	Smaller anti-redistribution effect because migrants may observe efficacy of taxation and redistribution in alleviating poverty	Research Design: Comparing effects of visa lotteries to otherwise similar countries with more and less generous welfare states.
Destination country with less generous welfare state	Larger anti-redistribution effect because migrants may observe efficacy of smaller government in stimulating growth	Research Design: Comparing effects of visa lotteries to otherwise similar countries with more and less generous welfare programs.
Origin region with higher taxes	Larger anti-redistribution effect because migrants may be more sensitive to concerns about taxation of their overseas financial gains	Research Design: Comparing effects of visa lotteries from higher and lower tax regions within India.

This list of contextual factors is not intended to be exhaustive; additional theoretical and empirical work is needed to ascertain whether and in which direction contextual elements condition the effects of migration. The study sites and research designs that we have proposed delineate avenues for future research seeking to investigate whether and how context matters for the effect of migration on migrant attitudes.

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