Why are returns to internal migration so small? Evidence from competing migration destinations

Sacha Kapoor Fatma Selcen Palut Dinand Webbink *

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Abstract

Recent evidence suggests the economic returns to internal migration are close to zero, consistent with efficient spatial sorting of labor. We show this finding may be an artifact of the unobservability of alternative migration destinations. For example, when individuals emigrate internationally, they disappear from the sample. Those who remain are selected, biasing estimates of internal migration returns toward zero—or beyond, reversing their sign entirely. The bias can be large. Using data from Turkey and Tanzania, we show that destination-specific instruments—those that do not affect alternative migration choices—recover substantial positive returns to internal migration for individuals whose migration decisions are shifted by cohort-level norms. When we instead use instruments that affect multiple destinations, estimates collapse toward zero or become negative, mimicking patterns in the broader literature.

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*Erasmus University Rotterdam, Erasmus School of Economics, Burgemeester Oudlaan 50, 3062 PA Rotterdam, The Netherlands, kapoor@ese.eur.nl, palut@ese.eur.nl, webbink@ese.eur.nl. We thank Anna Baiardi, Maarten Bosker, Robert Dur, Guido Imbens, and Matthijs Oosterveen for helpful comments.

Internal migration appears not to pay. Using increasingly demanding identification strategies, recent studies estimate modest causal returns to internal migration. Using family fixed effects, Beegle, De Weerdt, and Dercon [2011] estimate returns of around 40 percent. Lagakos, Marshall, Mobarak, Vernot, and Waugh [2020], using individual fixed effects, find returns between 16 and 34 percent. Allowing for heterogeneous returns via random coefficients, Cenci, Kleemans, and Tjernström [2024] estimate returns of just 7 to 16 percent. By contrast, estimates of international migration returns are routinely much larger: Clemens [2010] estimates returns of 500 percent; Gibson, McKenzie, Rohorua, and Stillman [2018] find 263 percent; and Mobarak, Sharif, and Shrestha [2021] find returns of around 200 percent. The downward trend in internal migration estimates points to a clear conclusion: sorting within countries is relatively efficient, observed wage gaps largely reflect selection, and the causal return to internal migration is close to zero.

This conclusion has direct policy implications. If workers are already well-matched to locations, there is little scope for welfare-improving migration interventions. Spatial wage gaps reflect differences in worker characteristics rather than frictions, and policies aimed at reducing barriers to internal mobility are unlikely to generate large gains. If, however, the near-zero estimates are misleading—if frictions do impede efficient sorting—then migration policies could yield substantial welfare improvements. Whether internal migration truly does not pay is therefore a first-order question.

We offer an alternative explanation. Potential migrants can stay, move domestically, or move abroad. Most studies collapse these options into a single binary: migrate or not. This can be problematic. Some individuals migrate to destinations that remove them from the sample entirely, such as moving abroad. Others choose observed destinations, but their decision to forgo alternative migration options is unobserved. As a result, when researchers estimate returns to one migration destination, the comparison group is contaminated by individuals whose choices involved other destinations. In this setting, many standard instrumental-variables estimators confound returns across destinations, biasing estimated returns toward zero and, in extreme cases, reversing their sign.

To illustrate the implications for causal identification, we formalize this intuition with a simple example. Our example centers on a randomized experiment that offers incentives for internal migration. Under textbook conditions, this design recovers the causal return. We then introduce a third option: international migration. Some in the control group emigrate and disappear from the sample. Others with access to international migration remain — either migrating internally or staying put. All of these possibilities contaminate the comparison group,

biasing returns downward.

The resulting bias can be summarized by a simple expression:

$$\text{Bias} = -\frac{(1-p_I)}{\Delta_U} Y(R) \tag{1}$$

where p_I is the probability of international migration among those with access to international opportunities but no internal migration incentive, Δ_U is the observed first stage for internal migration, and $Y(R)$ is income in the origin location. The bias is negative and depends on three factors. First, bias arises when international compliance is imperfect ($p_I < 1$): non-emigrants remain in the sample and contaminate the control group. The lower the compliance, the larger the bias. Second, when compliance is imperfect, some individuals with access to international migration choose internal migration instead, which reduces the share of internal migrants attributable to the instrument. This amplifies whatever bias exists. Third, the magnitude of the bias scales with origin income $Y(R)$—the higher the income that contaminates the control group, the larger the bias. Under conservative assumptions—high international compliance and minimal cross-effects—the bias is modest. Under more realistic conditions—lower compliance and substantial cross-effects—it can be large, dwarfing any plausible true return.

The simple example assumes homogeneous returns. We generalize to allow for heterogeneous returns and continuous instruments. The core insight carries over: when instruments affect only the targeted destination—when they exhibit no cross-effects—estimates are unbiased regardless of how researchers handle alternative migrants. They can pool all migrants together, group alternative migrants with the control group, or estimate returns separately for each destination. All three approaches yield consistent results. When instruments do exhibit cross-effects, by contrast, estimates become unstable and sensitive to specification. This motivates our empirical strategy: we construct destination-specific instruments and test directly for the absence of cross-effects.

We test these ideas using data from Turkey and Tanzania. Our main empirical setting is Turkish migration during the 1960s and 1970s, when rural individuals faced three options: remain in the village, migrate to domestic cities, or migrate internationally to Europe under government-sponsored guest worker programs. The 2000 Families Survey tracks individuals and their descendants across all three destinations, allowing us to observe the full set of migration choices that are typically unobserved in household surveys.

Our instruments exploit variation in local migration culture. For each individual, we compute the migration propensity of other families from the same origin—separately for urban and

international migration. These leave-family-out propensities predict migration to the targeted destination without affecting alternative migration choices. First-stage tests confirm the absence of cross-effects. Using destination-specific instruments, we estimate large income gains from internal urban migration for individuals whose migration decisions are shifted by cohort-level norms. Quantitatively, the estimated return to urban migration is on the order of 200 percent. In contrast, we find no significant income gains from international migration in this setting. The large urban migration return is economically plausible given the margin of adjustment induced by our instruments: urban migrants are disproportionately educated individuals transitioning from subsistence agriculture into professional employment, while later-generation international migration largely occurs through family reunification rather than labor recruitment. When we deliberately construct instruments that violate the no-cross-effects requirement, estimated returns collapse toward zero or become negative, closely mirroring patterns in the broader literature.

We replicate this analysis using data from Beegle, De Weerdt, and Dercon [2011] on Tanzanian migration, which distinguishes migration within the Kagera region from migration to other parts of Tanzania. Using instruments that satisfy the no-cross-effects requirement, we recover positive returns consistent with the original study. When we instead use instruments with cross-effects, estimates become unstable and statistically insignificant.

Our findings have implications for both research and policy. Methodologically, researchers estimating migration returns should account explicitly for alternative destinations and test whether their instruments affect multiple migration margins. Substantively, our results suggest that the efficient sorting interpretation of near-zero internal migration returns deserves closer scrutiny. True returns to internal migration may be substantially positive, implying greater scope for welfare-improving migration policies than suggested by the existing literature.

The remainder of the paper proceeds as follows. Section 1 develops the theoretical framework. Section 2 provides historical context on Turkish migration. Section 3 describes the data. Sections 4 through 7 present the empirical results. Section 8 concludes.

1 Empirical framework

We formalize how unobserved alternative migration destinations can bias estimates of internal migration returns. We begin with a simple illustration using a hypothetical experiment that generates randomized instruments. The key implication is simple: instrumental variables estimates recover well-defined destination-specific returns only when instruments affect a single

migration margin. When instruments shift multiple margins, estimates mechanically confound returns across destinations.

1.1. Setup. Consider a population of rural individuals who face three options: staying rural (R), migrating to urban areas within their country (U), or migrating internationally (I). Researchers want to estimate the return to internal urban migration: $\beta_U = Y(U) - Y(R)$, where $Y(U)$ and $Y(R)$ are potential earnings in urban and rural locations. For simplicity, we initially assume these returns are homogeneous across individuals.

1.2. The problem: a simple illustration.

A two-destination baseline. Suppose first that international migration does not exist. Researchers randomly assign rural individuals to two groups:

- Group 1 receives no incentive ($Z_U = 0$)
- Group 2 receives an incentive to migrate internally ($Z_U = 1$)

Let $p_U(z) = P(D_U = 1|Z_U = z)$ denote the probability of internal migration given instrument value z. The standard IV estimator is:

$$\beta_U^{IV} = \frac{\mathbb{E}[Y|Z_U = 1] - \mathbb{E}[Y|Z_U = 0]}{p_U(1) - p_U(0)} = Y(U) - Y(R) = \beta_U$$

The estimator recovers the true return.

Introducing a third destination. Now suppose international migration exists. We add:

- Group 3 receives an incentive to migrate internationally ($Z_I = 1$), but no internal incentive ($Z_U = 0$)

With two instruments, let $p_U(z_U, z_I)$ denote the probability of internal migration given instrument values (z_U, z_I), and similarly for $p_I(z_U, z_I)$ and $p_R(z_U, z_I)$. Our three groups correspond to:

	(Z_U, Z_I)	Internal	Rural	International
Group 1	$(0,0)$	$p_U(0,0)$	$1 - p_U(0,0)$	—
Group 2	$(1,0)$	$p_U(1,0)$	$1 - p_U(1,0)$	—
Group 3	$(0,1)$	$p_U(0,1)$	$p_R(0,1)$	$p_I(0,1)$

Under perfect compliance with the international incentive, all Group 3 members emigrate: $p_I(0,1) = 1$. They disappear from the sample. Researchers comparing Groups 1 and 2 recover the true return β_U.

But with imperfect compliance, some Group 3 members stay. They appear in the data with $Z_U = 0$, observationally indistinguishable from Group 1. The researcher, unaware of the international incentive, pools Groups 1 and 3 as the control group.

The contaminated estimator. Define $\Delta_U = p_U(1,0) - p_U(0,0) - p_U(0,1)$ as the contaminated first stage. When Group 3 non-emigrants are pooled with Group 1, the IV estimator becomes:

$$\begin{aligned}\beta_U^{IV} &= \frac{[p_U(1,0)Y(U) + p_R(1,0)Y(R)] - [p_U(0,0)Y(U) + p_R(0,0)Y(R) + p_U(0,1)Y(U) + p_R(0,1)Y(R)]}{p_U(1,0) - p_U(0,0) - p_U(0,1)} \\ &= Y(U) - Y(R) - \frac{(1 - p_I(0,1))}{\Delta_U} Y(R) \\ &= \beta_U - \frac{(1 - p_I(0,1))}{\Delta_U} Y(R)\end{aligned}$$

The bias term is $-\frac{(1-p_I(0,1))}{\Delta_U} Y(R)$. With a positive first stage ($\Delta_U > 0$) and positive rural earnings ($Y(R) > 0$), the bias is negative: we underestimate the internal migration return.

Two channels of bias. The bias operates through two distinct channels.

The first channel is *numerator contamination*. When $p_I(0,1) < 1$, some individuals with access to international migration do not emigrate. These non-emigrants remain in the sample as rural stayers, and their rural income $Y(R)$ contaminates the $Z_U = 0$ comparison group. The further $p_I(0,1)$ falls below one, the greater this contamination.

The second channel is *denominator erosion*. Recall that $\Delta_U = p_U(1,0) - p_U(0,0) - p_U(0,1)$. When $p_U(0,1) > 0$—that is, when some individuals with the international incentive migrate internally rather than internationally—the observed first stage shrinks. A smaller denominator amplifies whatever bias exists in the numerator.

These channels interact. If international compliance is perfect ($p_I(0,1) = 1$), the numerator is zero and there is no bias regardless of $p_U(0,1)$. If compliance is imperfect but no one in the $(0,1)$ cell migrates internally ($p_U(0,1) = 0$), then the denominator is not eroded and the bias remains bounded. But when both channels operate simultaneously, bias can become arbitrarily large.

Numerical illustration. To illustrate magnitudes, suppose rural income is $Y(R) = \log(5000) \approx 8.5$ log points, and the internal first stage absent international migration would be $p_U(1,0) - p_U(0,0) = 0.10$. Consider two scenarios:

	Scenario A (Clean recruitment)	Scenario B (Leaky incentive)
$p_I(0,1)$	0.95	0.70
$p_U(0,1)$	0.02	0.08
Δ_U	0.08	0.02
Bias (log points)	-5	-127

In Scenario A, a dedicated international recruitment instrument (such as a visa lottery) generates high compliance and minimal cross-effects. The bias is modest—5 log points, unlikely to overturn qualitative conclusions. In Scenario B, a general push factor (such as a drought or economic shock) weakly predicts international emigration, and many non-emigrants choose cities over villages. The bias exceeds 127 log points—larger than any plausible true return, potentially reversing its sign.

Heterogeneous returns and cross effects. The analysis above assumes homogeneous potential outcomes. With heterogeneous returns and two instruments, the TSLS estimand takes the more general form:

$$\beta_U^{TSLS} = \beta_U + \beta_I \cdot \frac{\text{Cov}(D_I, Z_U)}{\text{Cov}(D_U, Z_U)} \tag{2}$$

where β_U and β_I are appropriately weighted average returns. The bias term captures cross effects: when the internal migration instrument Z_U also affects international migration decisions, the estimand confounds internal and international returns.

This formula reveals a testable implication. If $\text{Cov}(D_I, Z_U) = 0$ — that is, if the internal migration instrument does not affect international migration — then the bias term vanishes. Researchers can test for cross effects directly in the first stage. We extend this analysis to continuous instruments and heterogeneous returns in the online appendix, where we show that different pooling strategies — control pooling, treatment pooling, and no pooling — yield equivalent results when cross effects are absent.

2 Historical context

The Turkish mass migration to Europe between 1960 and 1974 was engineered by European and Turkish governments. Between 1961 and 1973, Turkey established formal guest worker agreements with six European nations—Germany (1961), the Netherlands (1964), Belgium (1964), Austria (1964), France (1965), and Sweden (1967). Under these agreements, an estimated 800,000 Turkish citizens were officially sent abroad through formal channels, while the Turkish Employment Agency (İŞKUR) estimates that between 1.5-2 million Turkish citizens (representing 10-12% of Turkey's contemporaneous labor force) migrated to Europe when including those who initially entered on tourist visas but subsequently obtained employment.

European governments were motivated by labour shortages in certain sector occupations. The Turkish government was initially motivated by several developments inside Turkey during the 1950s and 1960s. The Marshall Plan was followed by widescale adoption of capital intensive technologies in agriculture, and subsequently by large scale domestic migration of low skilled labor to large urban areas, and finally by the development of slums in and around these areas. The Turkish government viewed the international migration of low skilled labor as a potential solution to the problem of slums in urban areas.

Turkey's 1962 First Five-Year Development Plan explicitly framed international migration as addressing multiple policy objectives:

> "Another aspect of employment policy is the export surplus labor to those countries of Western Europe which suffer from a dearth of labor. However, Turkey, while having surplus unskilled labor, is lacking in sufficient numbers of skilled labor. Therefore, it is essential that measures be taken to ensure that Turkey's deficiency in skilled labor not to be exacerbated by Turkey's labor export policy."

This policy vision assumed temporary migration, with workers returning to Turkey with enhanced skills to fuel domestic industrialization. However, this transfer was limited by three factors: (1) many "guest workers" became permanent residents rather than returning, (2) returnees were negatively selected on skill acquisition potential, and (3) some migrants experienced occupational downgrading, exemplified by approximately 9,000 Turkish teachers working as industrial laborers in Germany between 1964-1973 [Abadan-Unat, 2011].

Concurrently, non-migrants in Turkey experienced significant economic structural change, with agriculture's share of the economy shrinking from 54% to 24% between 1960-1981 [World Bank, 2019]. The expanding manufacturing and service sectors increased urban labor demand,

partially addressing the slum conditions that had motivated the government's international migration policy.

From 1973 onwards the choice of which country to live in as a family became more imperative as various labor agreements between Turkey and European countries were cancelled. Later generations could no longer migrate under these agreements. As such, later generations tended to migrate to Europe via family unification rules. Consequently, the post 1973 migration was also sizeable. Furthermore, later generations tended to originate from the same towns and families of earlier generations.

Our research contributes to a growing literature on this historically significant migration episode, complementing studies on return migration dynamics [Adda, Dustmann, and Görlach, 2022], savings behavior [Dustmann and Mestres, 2010a], remittance patterns [Dustmann and Mestres, 2010b], and development impacts [Dustmann and Kirchkamp, 2002] within this same historical context.

3 Data

Our main empirical setting uses the 2000 Families Survey, which tracks Turkish individuals and their descendants across multiple migration destinations. Unlike most datasets used to estimate migration returns, this survey observes the full set of migration choices, including international migrants who would otherwise disappear from the sample. This feature allows us to distinguish rural stayers, internal migrants, and international migrants within the same origin communities, and to construct destination-specific instruments that explicitly satisfy the no-cross-effects condition developed above.

3.1. 2000 Families survey. The 2000 Families survey was conducted between 2010-2012 in rural areas with a significant history of international migration to Europe between 1960 and 1974. Five towns were selected from around the country (see Figure 1): Acıpayam (in Denizli); Akçaabat (in Trabzon); Emirdağ (in Afyon); Kulu (in Konya); Şarkışla (in Sivas). Urban areas were excluded because of low urbanization rates in the 1960s and 1970s (32% of total population in 1960, 38% in 1970 [Kiziroglu, 2017]). Towns were selected to represent diversity in both the country of destination and ethnicity. The sample covers a large group of destination countries for first generation men: Germany, Netherlands, France, Austria, Belgium, Denmark, Sweden, Switzerland, UK, Italy, Spain, Finland and Norway. It also covers several ethnic groups: Kurdish, Yorouk, etc.

The 2000 Families survey asks about the reasons for international migration. We summarize the responses by generation in Table 1. 98 percent of first generation migrants migrated for a job. 81 percent of GEN2 migration was linked to family reunification. 79 percent of GEN3 migration was linked to family reunification. These patterns are consistent with the historical context of the mass migrations to Europe.

3.2. Sampling. We describe some details of the sampling process. Further detail can be found in [Guveli et al., 2017].

Family units were drawn at random from the selected towns. Interviewers used the Turkish Statistical Institute's (TÜİK's) Address Based Population Registration System to draw a clustered probability sample and detect 100 sampling points in each town. Two interviewer groups then walked in opposite directions from each sampling point. Interviewers skipped one door and knocked on every other door, turned left at the end of next street, then right at the end of the next one [Spierings, 2016]. Households that opened the door and were willing to talk were asked

> "Amongst your, or your partner's close or distant relatives, is there a man who is alive or dead, is (would have been) between 65 and 90 years old, grew up in [REGION] (i.e. lived here until he was at least sixteen), who migrated to Europe between the years 1960 and 1974 and stayed in Europe for at least five years?".

If they answered yes, a full interview was requested. After detecting 4 families that answered yes, interviewers searched for families with no migrant ancestor. The walk from each primary sampling unit was ended after 60 households were screened or 8 families were recruited. In the final sample, 80% of families had a migrant ancestor.

The survey had three parts: 1. a family tree survey; 2. a proxy questionnaire; 3. a personal questionnaire. In the family tree survey, there was usually one respondent who was asked about the migration status of the generation 1 ancestor (GEN1), as well as questions about all children (GEN2), grandchildren (GEN3), and great-grandchildren (GEN4). For these remaining members it only asks gender, age, birth rank, and mortality status. In the proxy questionnaire, there were usually one or two respondents from the family. These respondents answer questions on behalf of other family members if they know the answers. These questions relate to migration status, marital status, and education levels for all adult members of the family.

The personal questionnaire is the most relevant for our analysis. Here family members were selected purposefully or at random. For GEN1, all living ancestors are asked to fill the survey. For GEN2, two adult and living children of living ancestors are selected for interviewing. For GEN3, two adult and living children of each selected second generation members are selected.

The same selection process is repeated for the fourth generation. The random selection is conducted using an "AZ" rule, where the siblings selected from each generation have the closest initial to A and Z[Guveli et al., 2016].[1]

Collecting such extensive data on 4 generations of thousands of families who live throughout the world is challenging. 81% of the personal interviews are conducted through telephone as the sample was living all over Europe. Moreover, surveys were prepared in 8 languages and interviewers that are fluent for each language were employed, but most were conducted in Turkish [Guveli et al., 2016]. Though the design may be biased towards migrants with ties to the home town, this bias is reduced by asking if the doorstep informant has a migrant relative rather than asking for a direct ancestor. This questionnaire covered a broad range of topics including political affiliation, social networks, Turkish identity, discrimination, etc.

For the family tree survey, we have 1992 families, 48,978 individuals, and a 100 percent response rate. For the proxy questionnaire, we have 1580 families, 19,666 individuals, and a 78% response rate. However, since the proxy may not know the answers for every family member and for some families there were no proxies volunteered, these questions are answered for 19,666 people. For the personal questionnaire, we have 5,980 individuals, and a 61% response rate.

While several researchers have utilized the 2000 Families dataset, our contribution differs in both approach and focus. The original research team produced a comprehensive book documenting Turkish migrants' occupational status, social networks, religiosity, and identity [Guveli et al., 2016], along with focused studies on electoral participation [Spierings, 2016], marriage patterns [Baykara-Krumme, 2015], and return migrant mobility [Baykara-Krumme, 2013]. These sociological investigations primarily examine value transmission within families and cultural adaptation across international contexts. Our study, by contrast, adopts an economic lens and employs causal identification strategies to isolate the returns to migration—addressing fundamental questions about selection bias that descriptive approaches cannot resolve.

We restrict the sample to Turkish born respondents, effectively excluding GEN2 respondents born in Europe. Respondents who were born in but never left Turkey are classified as non-migrants. Respondents who migrated within Turkey are classified as domestic migrants. Respondents who migrated to Europe are classified as international migrants. International migrants who returned to Turkey at some point are classified as return migrants. Permanent migrants never returned to Turkey as of 2011.

Online Appendix Table A.6.1 shows the migration status of each generation for those who

[1] There are no known Turkish naming patterns that favors one initial over others.

are born in Turkey. GEN1 consists 1646 persons, of which 79.4% migrated internationally. GEN2 has 7312 persons with 43.4% migrating internationally. GEN3 has 7083 persons with 20.0% migrating internationally. GEN4 has 147 persons with 17.0% migrating internationally.

3.3. Outcome variables. Our analysis focuses on the effects of urban and international migration on income, occupation, and assets. We describe the construction of these variables in turn.

INCOME- Income is solicited in the personal interview in the interview year (2010, 2011, or 2012). Our benchmark regression analysis measures income returns to migration using annual PPP (purchasing power parity)-adjusted household income. We exclude the top and bottom 5% to deal with outliers. This restriction changes the support of the realized income distribution from [2.6; 8,415,842] to [2,610; 61,728]. For the GEN2 family members born in Turkey, this information exists for 1,821 people. In all regressions we use logged income.

To increase the number of observations with income information, we follow Abramitzky et al. [2012] in imputing income on the basis of education, country of residence, and occupation. We first analyze the relationship of education, country of residence and occupation to income in the personal interview. Then we impute income in the proxy interview on the basis of these variables and their estimated coefficients.

OCCUPATION- The occupational status variable was developed by Ganzeboom et al. [1992]. It reflects the role of occupational position in the earnings-education gradient. It was constructed using a database of 198.500 men and women from several countries with valid education, occupation, and (personal) incomes derived from the combined 2002-2007 issues of the International Social Survey Programme [ISSP]. The variable ranges between 16 (for jobs like domestic helpers & subsistence agricultural work) and 90 (for judges).

A summary of occupations by migration choice is found in Table 2. The top occupation for stayers was in agriculture. For urban migrants it was in office administration. For migrants to Europe it was in manufacturing. The patterns for the top occupation align with our historical description of the mass migration as well as with the notion that urban migrants upgrade occupations whereas migrants to Europe downgrade their occupation.

ASSETS- Individuals in the personal interviews are asked about their ownership stakes in three types of assets: housing, business, and land. We construct an ownership indicator that equals 1 under full ownership for every category both in Turkey and Europe, equals 0 under no ownership, and equals 1/2 under shared ownership. Individuals were also asked about the location

of said property (in Turkey or Europe). We do not have information on asset values, however asset measurement is difficult for any context, especially while comparing assets from many different countries.

3.4. Selection variables. Our main exogenous selection variables are age, gender, ancestral hometown, sibling rank within their immediate family, and education.

Education is collected in both the proxy and personal interview. From the proxy interview, the education categories are primary dropout, primary completed, low secondary, high secondary, low tertiary, high tertiary. From the personal interview, the categories are primary dropout, primary completed, extended primary, lower secondary general, lower secondary vocational, higher/post secondary general, higher/post secondary vocational, lower tertiary vocational, lower tertiary general (BA), higher tertiary (Masters), post-tertiary (PhD). We aggregate the classification in the personal interview to match the classification in the proxy interview.

4 Descriptives

4.1. Basic statistics. Table 3 presents key demographic patterns across migration categories for the first three generations. The data reveals distinct gender patterns: GEN1 migrants were exclusively male, while in GEN2 and GEN3, females show significantly higher propensities to remain in rural Turkey. We observe a downward intergenerational trend in family size (the number of children), though this may be due to censoring for GEN3, who are still in their early in their child-bearing years. We observe an upward intergenerational trend in education. There appears some convergence in incomes across migrants or stayers within Turkey and migrants to Europe in GEN3.

Figure 3(a) depicts the distribution of family line sizes. We see some large family lines in our sample. The presence of large family lines, and the number of families, increases the chances of observing family lines with one of the migrant types (urban or international or non-migrant), with two of the three, and even with all three. We confirm this in Figure 3(b). Notably, 21 percent of family lines have migrants moving to both urban and international destinations. This diversity of migration patterns within family lines demonstrates that our identifying variation does not simply reflect family-specific destination preferences.

4.2. Migrant selection. We characterize the first stage migration decisions. Women and younger siblings have a propensity for urban migration within Turkey relative to non-migrants. They

have less propensity for international migration relative to nonmigrants. The evidence is consistent with the Turkish cultural norm of allocating a disproportionate share of financial obligations to the eldest son. We show further that home town is important for migration decisions, consistent with regional heterogeneity in said norms. Finally, individuals who completed any level of education above a primary school dropout have a propensity for urban migration. By contrast, individuals with a vocational education have a propensity for international migration to Europe. These patterns characterize later generations, who had the option migrate after the Turkish economy had improved relative to the 1960s and early 1970s, and for whom the relative return to international migration had eroded relative to the initial period of mass migration.

To characterize migrant selection, we estimate

$$\mathrm{D}_{ij} = \mathbf{X}_i \boldsymbol{\beta}_j + \mathrm{e}_{ij}$$

where i denotes the migrant and $j \in \{\mathrm{U}, \mathrm{I}\}$ the migration decision. $\mathbf{X}_i$ includes the following predetermined covariates: gender, age, sibling rank within the family, and home town. e_{ij} is a random variable that reflects unobserved factors behind migration decisions. We estimate the specification separately for each generation.

Estimates are reported in Online Appendix Table A.6.2. Note that European migrants are removed from the comparison group for internal urban migrants, and vice versa. $\mathrm{D}_{ij} = 0$ means the potential migrant decided to stay in the village for both j, in other words. Thus, we have 2 regressions for each generation, one that compares stayers with internal urban migrants, and another that compares them with migrants to Europe.

The middle panel shows rural to urban (R to U) moves are 3.7 percentage points more likely for GEN2 females. Rural to international (R to I) moves are 10.1 percentage points less likely for GEN2 females. The gender difference in R to I moves is statistically significant at the 1 percent level. A qualitatively similar pattern emerges for GEN3 (rightmost panel).

In Turkish culture the oldest son bears a disproportionate share of family financial obligations. On this basis, our initial hypothesis was that oldest sons would have the strongest propensity for R to I moves. We observe evidence of this cultural trait in GEN2 and GEN3, where in addition to females having a lower propensity for R to I moves, the top ranked (oldest) sibling has a propensity for R to I moves that is 1.3 and 1.7 percentage points higher than their next ranked sibling in GEN2 and GEN3 respectively. Moreover, in GEN3, migration propensities are higher for older Turks.[2] Our evidence from GEN1 also supports this narrative, at least from the

[2] Higher migration propensities for older Turks in GEN3 may reflect censoring. Younger Turks in GEN3 may eventually move but are not observed to in our sample.

perspective of R to U moves.

There is heterogeneity on the basis of home town. For GEN1 migrants the patterns are mechanical, reflecting the nature the sampling procedure, which targeted 1 household without a European migrant ancestor for every 4 households with a European migrant ancestor. For GEN2 and GEN3 migrants this is not the case. Turks from Şarkışla had the highest propensity for R to U moves (61 percent). Turks from Emirdag had the highest propensity for R to I moves (approximately 83 percent). We observe similar patterns in GEN3 but with Kulu Turks having the highest propensity for R to I moves.

Our migrant selection equation omits several factors which influence migration decisions and wages at the destination. These factors include expected wages in all possible destinations, the expected value of amenities in these destinations, unobserved ability, among others. Our identification strategy hinges on the capacity of our instruments to isolate variation in migration decisions that cannot be explained by such factors.

4.3. Migrant selection among siblings. Our data appears well-suited for identification via family fixed effects estimators, as we observe a large number of immediate families with multiple individuals enumerated within each family. We estimated specifications conditional on immediate family fixed effects (IFFE), age decile, gender and sibling rank within the family, and standard errors clustered on the broader family line. We found returns to urban and international migration of 12 ($p < 0.05$) and 23 ($p < 0.01$) log points, respectively. The urban return is commensurate with the individual fixed effects estimates reported by Lagakos, Marshall, Mobarak, Vernot, and Waugh [2020]. The international return is smaller than the family fixed effects estimates in Beegle, De Weerdt, and Dercon [2011].

We explore an alternative to IFFE identification for three reasons.

The first reason relates to internal validity. The key identifying assumption underlying IFFE is orthogonality of sibling selection into migration and individual-specific migration returns: $Y(U) - Y(R)$ and $Y(I) - Y(R)$. This assumption is satisfied if returns are homogeneous across family members. However, when returns are heterogeneous—varying with individual ability, motivation, or comparative advantage—migration may systematically depend on which sibling generates the largest expected return. In such cases, IFFE may not purge selection bias.

The second reason relates to external validity, *i.e.* selection into identification [Miller, Shenhav, and Grosz, 2023]. IFFE estimators condition identification exclusively on families where migration decisions vary across siblings. This raises the fundamental question of whether return estimates derived from these "identification families" are representative of the broader

population's returns to migration. Families with heterogeneous migration patterns may differ systematically from families where all members make similar migration choices.

The third reason relates to sample size. We lose more than 1000 observations when we condition on the immediate family. The information loss weakens the first stage of our analysis significantly.

We investigate the potential for selection bias within families in Table 4 by regressing migration decisions on education, age deciles, gender, birth order rank, and IFFE.[3] Our key hypothesis is that if ability bias operates beyond general migration norms among Turkish families, we should observe systematic correlations between educational attainment and migration decisions even within families.

The results provide strong evidence consistent with this concern. Column 1 shows positive and statistically significant effects of university education on urban migration, with particularly pronounced effects for individuals holding Masters degrees. Column 2 demonstrates a positive and statistically significant effect of lower secondary vocational education on international migration decisions. These patterns are mostly consistent with the migration-education gradients observed by Girsberger [2017], who shows an increased propensity for rural migration among the highly educated, as well as a U-shape in the gradient among urban migrants.

The patterns persist when we exclude IFFE or restrict the analysis to Turks who completed their education within Turkey, suggesting the relationships are robust across specifications and unlikely to be driven by migration for educational purposes.

This evidence points to migrant selection based on ability or comparative advantage operating within families.[4] The pattern is consistent with families optimizing migration decisions based on comparative advantage, sending the member with the highest expected returns rather than making choices based purely on demographic characteristics or random factors.

The evidence also raises questions about whether a 12 log point return is economically plausible given the pronounced educational selection we observe in urban migration. Our results show that university-educated individuals - particularly those with advanced degrees - systematically migrate from rural areas (where agricultural work predominates) to cities for presumably higher-skilled occupations. This represents a substantial shift in both geographic location and occupational matching. For such dramatic transitions by highly educated workers to yield

[3] We keep all three types of migrants in both regressions to maintain a consistent sample size, however the results are similar if drop one type from each regression.

[4] We acknowledge that this evidence is not strictly causal. For instance, some individuals may migrate specifically to pursue certain types of education, such as moving to urban areas to obtain university degrees. However, among the 559 urban migrants in our pilot study who reported their migration reasons, only 10% cited educational purposes, suggesting this reverse causality is unlikely to drive our main findings.

only a 12 log point return suggests either that rural-urban wage premiums are surprisingly modest, that migration costs are substantial, or that our estimates may be attenuated by remaining selection bias. In contrast, the 23 log point return to international migration appears more plausible, as these migrants are primarily lower-skilled workers facing potentially large wage gaps between origin and destination countries.

Following the advice of [Miller, Shenhav, and Grosz, 2023], we evaluate external validity using comparisons of potential migrant characteristics from families with and without migration variation across siblings. Our comparisons are found in Online Appendix Table A.6.3. Since IFFE identification relies exclusively on within-family variation in migration decisions, our estimates are identified only from families where at least one sibling migrates while others do not (switcher families).

The results reveal stark differences between switcher and non-switcher families. Individuals in switcher families have educational levels that are a full point higher on average, are more than 14 years younger, and exhibit 18 percentage points greater female representation among siblings. Notably, family size does not differ significantly between the two groups.

These concerns about internal and external validity motivate our instrumental variables strategy, which we develop in the next section.

5 Multi-treatment IV first stage

5.1. Instruments. Like Abramitzky, Boustan, and Eriksson [2012] and Beegle, De Weerdt, and Dercon [2011], our IV strategy leverages institutional knowledge of migration norms at origin. Unlike these papers, we exploit cohort- and town-specific norms to identify migration returns. More specifically, we instrument for D_{iU} and D_{iI} using the continuous variables

$$Z_{-f(i)U} = \frac{\sum_{j \in C_{-f(i)}} D_{jU}}{|C_{-f(i)}|} \qquad \text{and} \qquad Z_{-f(i)I} = \frac{\sum_{j \in C_{-f(i)}} D_{jI}}{|C_{-f(i)}|}, \tag{3}$$

where $C_{-f(i)}$ excludes members of i's immediate family from the same generation.

Destination-specific migration propensities of similar individuals have been used to construct selection-corrected earnings in migrant selection studies [Bertoli et al., 2013, Dahl, 2002]. Dahl [2002] studies state-to-state migration in the United States and defines cells by education, family situation, and birth state. Bertoli et al. [2013] studies Ecuadorian migration to the United States and Spain and defines cells by age, education, gender, marital status, and household size. Our cells are defined by ancestral hometown, generation, birth order (eldest or not), and gender.

The leave-family-out structure ensures that our instruments are not mechanically linked to individual ability or communication within immediate families. Instead, they capture cohort-level migration norms operating at the hometown–generation–gender–rank level.

5.2. Instrument construction and intuition. To build intuition for our instruments, consider the following example. Suppose individual i is 25 years old, an eldest brother, from generation 2, and migrated from Emirdağ to Europe. We first compute the total number of individuals with that exact profile—age, sibling rank, generation, and destination—from Emirdağ. We then subtract the number of individuals from i's immediate family and generation who share that profile. The instrument equals the ratio of the remaining count to the size of the cohort, excluding i and members of their family from the same generation.

This construction allows us to exclude other potentially similar individuals from the family, such as siblings and cousins, ensuring that the instrument reflects broader migration culture rather than within-family coordination. As a result, the instruments are not directly linked to individual ability or communication between immediate families in the same generation.

Our instruments identify individuals whose migration decisions are shifted by cohort-level migration norms—those on the margin of migrating for whom prevailing migration culture in their hometown-generation-gender-rank cell tips the decision. Table 4 shows that these compliers are disproportionately university educated for urban migration and vocationally trained for international migration, which informs the interpretation of our estimates.

Figure 3 illustrates how our instruments relate to sibling rank, age, and gender norms. Older males and females are more likely to migrate to urban areas compared to their younger siblings, while the opposite pattern holds for international migration. The parallel gender-specific evolution of migration propensities across sibling rank supports our choice to build instruments around rank and gender rather than finer disaggregation. Note, however, that our main results are robust to disaggregation into oldest female, oldest male, and gender.

An alternative approach would be to define cells based on discrete sibling rank rather than a binary indicator for eldest status. We do not adopt this approach for two reasons. First, it is not clear that distinct, finely graded migration norms exist for each rank. Second, this alternative definition introduces substantial additional variance, weakening the first stage. Indeed, when implemented, this alternative yields weak instruments that fail conventional first-stage tests.

Figure 4 plots the two instruments against each other. Migration propensities vary substantially across cohorts within the same hometown, and the instruments are negatively but not perfectly correlated. This within-town variation is central to our identification strategy.

Our main identification concerns arise from indirect links between our instruments and earnings. For example, depressed rural labor markets could simultaneously reduce potential earnings in ancestral hometowns and increase outmigration propensities. Identification would then fail if the migration response varies systematically with unobserved individual characteristics—such as ability or risk tolerance—that independently affect earnings. Similarly, our instruments may capture variation in the strength of social networks linking ancestral hometowns to specific destinations. Such networks could influence migration decisions, through information flows and reduced migration costs, while also affecting destination earnings, through job referrals and integration assistance. If these channels were quantitatively important, we would expect our estimates to be sensitive to controls for individual ability and network characteristics. We show later that this is not the case.

A separate concern is that high outmigration could tighten local labor markets, raising wages for non-migrants. This would bias our estimates toward zero, implying that our results, if anything, understate the true returns to migration.

5.3. First-stage (cross effects) specification. A key identifying requirement of our framework is that instruments for one migration destination do not affect migration choices to alternative destinations. We refer to violations of this requirement as *cross-effects*. When cross-effects are present, instrumental-variables estimands mechanically confound returns across destinations and become sensitive to pooling assumptions.

We test for cross-effects directly by estimating a system of first-stage regressions in which each migration outcome is regressed on both destination-specific instruments:

$$\mathrm{D_U} = \pi_{\mathrm{UU}}\mathrm{Z}_{-f\mathrm{U}} + \pi_{\mathrm{UI}}\mathrm{Z}_{-f\mathrm{I}} + age + \varepsilon_{\mathrm{U}} \tag{4}$$

$$\mathrm{D_I} = \pi_{\mathrm{IU}}\mathrm{Z}_{-f\mathrm{U}} + \pi_{\mathrm{II}}\mathrm{Z}_{-f\mathrm{I}} + age + \varepsilon_{\mathrm{I}}. \tag{5}$$

The absence of cross-effects corresponds to $\pi_{\mathrm{UI}} = 0$ and $\pi_{\mathrm{IU}} = 0$.

Our specifications include flexible age controls to address a confounder arising from the historical process governing migration opportunities. The staggered implementation of guest worker and subsequent family reunification programs generated distinct migration waves, with cohorts from the 1960s and early 1970s disproportionately selecting international destinations relative to domestic urban centers. This temporal structure is embedded in our sampling frame, as the 2000 Families Survey targeted individuals who were potential migrants during the initial European recruitment phase. As a result, respondent age is mechanically correlated with our instruments: older cohorts were more likely to be exposed to early international migration op-

portunities, while younger cohorts faced a different destination choice set.

Our first stage is isomorphic to a linear-in-means endogenous social interactions model that does not allow for contextual effects. Endogenous social interaction models typically allow for contextual effects by including, as controls, reference group averages of $\mathbf{X}$: $\overline{\mathbf{X}}_{-f} = \frac{\sum_{j \in C_{-f}} \mathbf{X}}{|C_{-f}|}$. We do not include contextual effects because it is not clear how or why they would help us proxy for social migration norms. In addition, if we included the full set of reference group averages for gender, rank, etc., then we would have linear dependence between our instruments and $1, \mathbf{X}, \overline{\mathbf{X}}_{-f}$. This linear dependence would lead to a violation of the full rank assumption, *i.e.* to the reflection problem [Manski, 1993].

5.4. First-stage estimates. Table 5 reports the estimates. Each instrument strongly predicts migration to its targeted destination. The estimated cross-effects are small and statistically insignificant: a one standard deviation increase in the international migration instrument decreases the probability of urban migration by 1.3 percentage points, while a one standard deviation increase in the urban migration instrument increases the probability of international migration by 1 percentage point. Neither estimate is statistically significant.

While the absence of cross-effects does not guarantee full exclusion, it is a necessary condition for recovering well-defined destination-specific returns in our framework. The evidence supports interpreting our estimates as destination-specific causal effects.

The first-stage estimates imply that a one standard deviation increase in the urban leave-family-out migration rate increases the probability of urban migration by 5.5 percentage points. A one standard deviation increase in the international leave-family-out migration rate increases the probability of international migration by 20.1 percentage points. Both estimates are statistically significant at the one percent level.

We report test statistics for the joint significance of the excluded instruments at the bottom of Table 5. In all cases, the null of no joint effect is strongly rejected.

6 Multi-treatment IV results

6.1. Destination-specific returns with no pooling. Our NP specification is

$$Y = \beta_0 + \beta_1 D_U + \beta_2 D_I + age + \varepsilon \tag{6}$$

where Y is long run household income, occupation, or assets. ε is a mean zero random variable. β_1 and β_2 are our target parameters. β_1 measures income differences across individuals with and without urban migration experience. β_2 measures income differences across individuals with and without international migration experience.

Second-stage estimates with no pooling are reported in Column 6 of Table 6. Urban migration leads to large and statistically significant income gains, while international migration yields effects close to zero. Quantitatively, urban migration increases household income by 110 log points (approximately 200 percent), whereas the estimated effect of international migration is 2.8 log points and statistically indistinguishable from zero.

The 200% return to urban migration is large relative to the recent literature, which finds returns converging toward zero. The magnitude reflects the population identified by our instruments. As shown in Section 5, our compliers are disproportionately university-educated individuals whose migration decisions respond to local migration norms. Table 4 confirms this selection pattern: within families, it is the highly educated—particularly those with advanced degrees—who choose urban destinations over rural origins.

For this population, large returns are economically plausible. These are workers transitioning from subsistence agriculture to professional urban employment—from field crop work to office administration, as Table 2 illustrates. A 200% return to such a dramatic occupational upgrade is more credible than the 12 log point return implied by immediate family fixed effects, which—as we showed in Section 4—fails to account for ability-based selection within families.

Descriptive evidence from the Turkish labor market is consistent with these magnitudes. Using Turkish Labor Force Survey data, Ercan [2011] documents that agricultural day laborers earn approximately two-thirds of the minimum wage, while professional urban occupations earn five to seven times the minimum wage—a raw gap exceeding 600 percent. Our causal estimate of 200 percent is considerably smaller, consistent with positive selection into urban migration.

The near-zero return to international migration is also interpretable. Over 80% of post-1973 migration occurred through family reunification rather than labor recruitment (Table 1). These migrants were not selected on economic returns. The absence of income gains is consistent with migration driven by family considerations rather than labor market opportunities.

6.2. IV results under different pooling choices. A central implication of the no-cross-effects condition is that estimated returns should be invariant to how migrants are pooled into treatment and control groups. The first five columns of Table 6 show how the IV estimates change

under different pooling choices. Moving left to right, we report the return to: out migration when we use both instruments (column 1); urban migration inside Turkey when the comparison group is non-migrants and international migrants (column 2); international migration when the comparison group is everyone who stays in Turkey (column 3); out migration when we use only the urban migration IV (column 4); out migration when we use only the international migration IV (column 5). The estimates in Columns 3 and 5 measure the returns to international migration under control and treatment pooling. The estimates in Columns 2 and 4 measure the returns to urban migration under control and treatment pooling.

Column 2 shows a 110 log point ($\approx 200\%$) return to urban migration under control pooling when we use the leave-family-out urban migration instrument. Column 4 shows a 105 log point ($\approx 186\%$) return to out migration under treatment pooling when we use the leave-family-out urban migration instrument. Columns 3 and 5 suggest small negative returns to international and outmigration under control and treatment pooling. The different pooling assumptions do indeed deliver similar estimates of the returns to urban and international migration.

We see very similar results with instruments defined over slightly coarser or finer cells. See Appendix Tables A.6.5 and A.6.6 for details.

6.3. Formal tests of A3 and A4. We find strong evidence of no cross effects in the aggregate. This means that the requirements (A3 and A4) for identifying a proper weighted average of the true returns are the same under control pooling, treatment pooling, and no pooling. In this subsection, we look for evidence that would allow us to reject A3 and A4.

Specifically, we implement a test developed by Bhuller and Sigstad [2024]. We estimate variants of:

$$\begin{aligned}
\mathbb{1}[Y \geq Y_{50}] \times \mathrm{D_U} &= \gamma_{\mathrm{UU}}^{(a)} \widehat{\mathrm{D}}_{\mathrm{U}} + \gamma_{\mathrm{UI}}^{(a)} \widehat{\mathrm{D}}_{\mathrm{I}} + \varepsilon_{\mathrm{U}}^{(a)} \\
\mathbb{1}[Y \geq Y_{50}] \times \mathrm{D_I} &= \gamma_{\mathrm{IU}}^{(a)} \widehat{\mathrm{D}}_{\mathrm{U}} + \gamma_{\mathrm{II}}^{(a)} \widehat{\mathrm{D}}_{\mathrm{I}} + \varepsilon_{\mathrm{I}}^{(a)} \\
\mathbb{1}[Y < Y_{50}] \times \mathrm{D_U} &= \gamma_{\mathrm{UU}}^{(b)} \widehat{\mathrm{D}}_{\mathrm{U}} + \gamma_{\mathrm{UI}}^{(b)} \widehat{\mathrm{D}}_{\mathrm{I}} + \varepsilon_{\mathrm{U}}^{(b)} \\
\mathbb{1}[Y < Y_{50}] \times \mathrm{D_I} &= \gamma_{\mathrm{IU}}^{(b)} \widehat{\mathrm{D}}_{\mathrm{U}} + \gamma_{\mathrm{II}}^{(b)} \widehat{\mathrm{D}}_{\mathrm{I}} + \varepsilon_{\mathrm{I}}^{(b)}
\end{aligned}$$

where Y_{50} is the median of household income (in logs) and in practice our specification also includes family fixed effects, gender, age, rank within the family, home town, and generation in the family tree. Bhuller and Sigstad [2024] show that average conditional monotonicity and no cross effects imply:

- $\gamma_{\mathrm{UU}}^{(a)} \geq 0$, $\gamma_{\mathrm{UU}}^{(b)} \geq 0$, $\gamma_{\mathrm{II}}^{(a)} \geq 0$, $\gamma_{\mathrm{II}}^{(b)} \geq 0$;

- $\gamma_{UI}^{(a)} = 0$, $\gamma_{UI}^{(b)} = 0$, $\gamma_{IU}^{(a)} = 0$, $\gamma_{IU}^{(b)} = 0$.

The test is premised on the idea that average conditional monotonicity and no cross effects hold on the subsamples of high or low potential income migrants migrating to each destination.

Estimates are found in Table 7. The evidence strongly supports A3 and A4.[5]

6.4. Returns under manufactured cross effects. To illustrate the importance of the no cross effects assumption, we construct instruments that violate this requirement. In particular, we construct two alternative instruments: i) a leave-family-out outmigration propensity measure that captures the general tendency for similar individuals to leave their hometown; ii) a binary indicator for being the oldest female in the immediate family. Our leave-family-out instrument predicts both migration choices by design. We did have a prior belief about our eldest female instrument - we experimented with different options to find a second instrument where the first stage strength was not completely eroded.

Table 8 confirms that our alternative specification generates the intended cross effects. The leave-family-out outmigration instrument significantly predicts both urban migration (coefficient = 0.130, $p < 0.01$) and international migration (coefficient = 0.594, $p < 0.01$). The oldest female indicator significantly predicts urban migration (coefficient = 0.059, $p < 0.01$) but has no significant effect on international migration.

Table 9 reveals the consequences of using instruments with cross effects. The second-stage estimates become highly sensitive to pooling assumptions, producing results that vary dramatically across specifications. Under control pooling (Column 2), urban migration yields a large, negative return. Under treatment pooling (Columns 1, 4, 5), results range from small positive effects to implausibly large estimates with enormous standard errors. With no pooling (Column 6), urban migration shows a negative return while international migration shows a positive return. The large negative returns to urban migration contradict the broader empirical literature documenting positive gains from rural-to-urban migration. These results demonstrate how instruments with cross-effects mechanically generate unstable and misleading estimates in multi-destination settings.

6.5. Robustness. We consider the possibility that our results are simply an artefact of our econometric specification, presented in Online Appendix Table A.6.4.

We begin with no controls (Column 1), then progressively add demographic controls: age variables (Column 2), and more detailed demographics including gender and birth order (Col-

[5]The Bhuller and Sigstad [2024] test is in fact a test of A1, A3, and A4.

umn 3). To address potential biases in our income measures, we examine results after excluding return migrants (Column 4) and using the ABE[2012] income imputation method instead of self-reported income (Column 5). These adjustments are particularly important since self-reported current income may be affected by return migration, especially among earlier cohorts.[6] To account for ability bias and network effects, we incorporate increasingly restrictive fixed effects: family line and generation fixed effects (Column 6), which also control for destination networks and local labor market conditions through nested ancestral hometown effects. Our last specification adds immediate family fixed effects (Column 7).

Our estimated returns to urban migration are robust to specification choice. The magnitude ranges from 99 to 117 log points (169-222%) in Columns 1-5. With family line and generation fixed effects (Column 6), estimated returns decrease to 86 log points (136%). Adding immediate family fixed effects (Column 7) further reduces the estimate to 70 log points, though it becomes statistically insignificant due to the substantial reduction in sample size.

Our estimated returns to international migration remain near zero across all specifications, with point estimates oscillating around zero accompanied by relatively wide confidence intervals.

We examine returns across multiple economic outcomes in Figure 5: household assets (top left), personal income (bottom left), and occupational status (bottom right). We report separate estimates by gender.

The occupational status estimates (bottom right) reinforce our main findings. Urban migration facilitates occupational upgrading, while international migration leads to occupational downgrading—consistent with the historical pattern of educated Turks moving to cities for professional employment while guest workers took manufacturing jobs in Europe.

The other outcomes suggest our household income estimates may not fully capture the economic returns to each migration type. Urban migrants experience decreased household assets despite substantial income gains, suggesting the overall economic benefits of urban migration, while positive, may be smaller than household income alone indicates. International migrants show positive personal income returns (though imprecise) while household income remains flat, suggesting our household income estimates may understate the true returns to international migration.

[6] Migrants who intend to return may accumulate savings while living in the destination with the intention of enjoying more leisure upon returning home [Dustmann and Mestres, 2010a]. Accordingly, the reported income of migrants who have already returned can be lower than the returns accrued to rural non-migrants [Dustmann and Görlach, 2016].

7 Multi-treatment IV in another setting

We investigate the problem of estimating the returns to migration in another setting where there are multiple migration choices. Our setting is the Tanzanian context first studied by Beegle, De Weerdt, and Dercon [2011]. To our knowledge, the Beegle, De Weerdt, and Dercon [2011] study was one of the first to build tracking data for the purposes of measuring the economic returns to migration. While there are now several tracking datasets, we could not find one with complete information on the migration origin and destination for every potential migrant in the sample. Further to this point, their dataset has information on internal migration inside and outside 1 of the 30 plus administrative regions (Kagera) in Tanzania. It does not have information on international migration decisions. We analyze this case of internal migration because we want to further validate the implications of relaxing the no cross effects assumption and because the Beegle, De Weerdt, and Dercon [2011] dataset lets us do this.

7.1. Background. The starting point for the Beegle, De Weerdt, and Dercon [2011] tracking data was the Kagera Health and Development Study (KHDS) conducted from 1991 to 1994. The 1991-1994 KHDS focused on collecting health information from individuals in 915 households in the administrative region of Kagera. Beegle, De Weerdt, and Dercon [2011] tracked down the individuals from these households in 2004: 68% were still living in the same village or a nearby village; 20% moved to a distant village inside Kagera; 12% moved outside Kagera. Tracked individuals were then asked to complete a consumption module as well as a host of other relevant questions. The other questions and the baseline 1991-1994 survey allowed them to build 6 instrumental variables for outmigration from the origin village and ultimately to causally identify the consumption return.

7.2. Empirical Specification. In their main specification, Beegle, De Weerdt, and Dercon [2011] pool three types of migrants into treatment: i) to nearby villages inside Kagera; ii) to distant villages inside Kagera; iii) outside Kagera. To make our point, we pool non-migrants and migrants to nearby villages into control and focus on the returns to distant migration inside Kagera and to migration outside Kagera. We consider three types of specifications as before: i) TP that pools distant inside-Kagera migrants with outside-Kagera migrants; ii) CP that pools distant inside-Kagera migrants with the control group; iii) NP that distinguishes between distant inside-Kagera and outside-Kagera migration.

Specifically, we estimate variants of

$$Y = \beta_0 + \beta_1 D_{In} + \beta_2 D_{Out} + \mathbf{X}\boldsymbol{\beta} + \alpha_f + \varepsilon.$$

where Y is log consumption in 2004.[7] D_{In} is a binary variable indicating migration to a distant village within Kagera. D_{Out} indicates migration outside Kagera. The above specification is the NP variant. The TP variant assumes $\beta_1 = \beta_2$ and, by consequence, estimates the return to $D_{In} + D_{Out}$. The CP variant pools one type of migration with control. It estimates the effect of D_{In} when the outside-Kagera migrants are part of control and the effect of D_{Out} when all inside-Kagera migrants are pooled into control.

7.3. Results with no cross effects IVs. We inspected the Beegle, De Weerdt, and Dercon [2011] instrument set and found two instruments that have no cross effects: a binary variable indicating whether the respondent is the household head or their spouse; the interaction of age rank within the household and an indicator for age (between 5-15) in the baseline 1991-1994 survey.

We report first stage estimates based on these instruments in Table A.6.7. Household head or their spouse decreases inside-Kagera migration. It has no statistical effect on outside-Kagera migration. The interaction between rank and age increases inside- and outside-Kagera migration, but the effect on inside migration is imprecisely estimated. Both magnitudes seem large but this is an artefact of a rescaling of ranks implemented by Beegle, De Weerdt, and Dercon [2011]. The two first stage F-statistics are inconsistent with weak overall first stage and with instrument redundancy in the reduced form. The second stage F-statistic are inconsistent with no joint effect of the endogenous variables on consumption.

PANEL A of Table 10 provides explicit detail on the second stage coefficients. Moving left to right, we report the return to: out migration when we use both instruments (column 1); distant migration inside Kagera when the comparison group is non-migrants, migrants to nearby villages, migrants to outside Kagera (column 2); outside-Kagera migration when the comparison group is everyone who stays in Kagera (column 3); out migration when we use only the inside-Kagera IV (column 4); out migration when we use only the outside-Kagera IV (column 5); inside and outside migration when estimated simultaneously (column 6).

Column 1 shows a 68.4 log point return to out migration when both instruments are used. Column 4 shows a return of 48 log points when we use the inside-Kagera IV alone. Column 5

[7] Beegle, De Weerdt, and Dercon [2011] use the change in log consumption between the first and second wave of their survey. We use log consumption to facilitate comparability with our and other results in the literature. Moreover, we do not observe any difference in effect sizes when we replicate their specification with log consumption instead of the difference in log consumption.

shows a return of 159 log points when we use the outside-Kagera IV alone. These estimates are comparable to the estimates we obtain when the other type of migration is assigned to the comparison group (columns 2 and 3) and when they are allowed to have separate returns (column 6).

7.4. Results with cross effects IVs. We repeat the exercise of the last subsection except now we examine the implications of instruments with cross effects. More specifically, we looked for two instruments with cross effects and a meaningful first stage according to both our F-tests. We found two such instruments: our leave-family-out instrument for migrants who migrate inside Kagera and a binary variable that indicates whether the respondent is the child of the head of household.

The first stage results are reported in Table A.6.8. The leave-family-out instrument is positively correlated with inside-Kagera migration decisions but negatively correlated with outside-Kagera migration decisions. The child of the household head instrument is negatively correlated with inside- and outside-Kagera moves. Both instruments are inconsistent with a weak first stage and with instrument redundancy in the reduced form. By the second stage F-statistic, however, we cannot reject no joint effect of the endogenous variables on consumption.

PANEL B of Table 10 reports the second stage estimates of the returns to migration. All estimates are negative and imprecisely estimated. Because the confidence intervals are quite wide, we cannot rule out large effects with confidence.

8 Conclusion

This study advances the literature on migration returns by explicitly accounting for multiple migration destinations. Whereas most existing work estimates returns to different migration types in isolation, we analyze a framework in which individuals choose among rural non-migration, urban migration, and international migration. We characterize the identifying conditions under which two-stage least squares estimands recover well-defined weighted averages of destination - specific returns. A central implication is that estimating returns separately or jointly yields equivalent results when instruments satisfy a no cross-effects condition—each instrument affects only one migration margin.

We apply this framework to Turkish migration during the 1960s and 1970s, constructing leave-family-out instruments for urban and international migration that satisfy the no-cross-effects requirement. Our estimates indicate large income gains from urban migration—on the

order of 200 percent—and no statistically significant income gains from international migration. Consistent with these findings, urban migrants experience occupational upgrading, while international migrants experience occupational downgrading. At the same time, results for assets and individual income suggest that household income overstates the overall economic return to urban migration and understates the return to international migration.

We further illustrate the importance of instrument choice through deliberate stress tests in both Turkey and Tanzania. In the Turkish setting, instruments that violate the no-cross-effects condition generate estimates that are highly sensitive to pooling assumptions and economically implausible. Using Tanzanian data, we show that instruments without cross-effects deliver stable estimates across pooling strategies, while instruments with cross-effects yield unstable and statistically insignificant results. Together, these findings underscore the central role of instrument properties in multi-destination migration settings and help reconcile conflicting evidence on migration returns in the existing literature.

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9 Figures and Tables

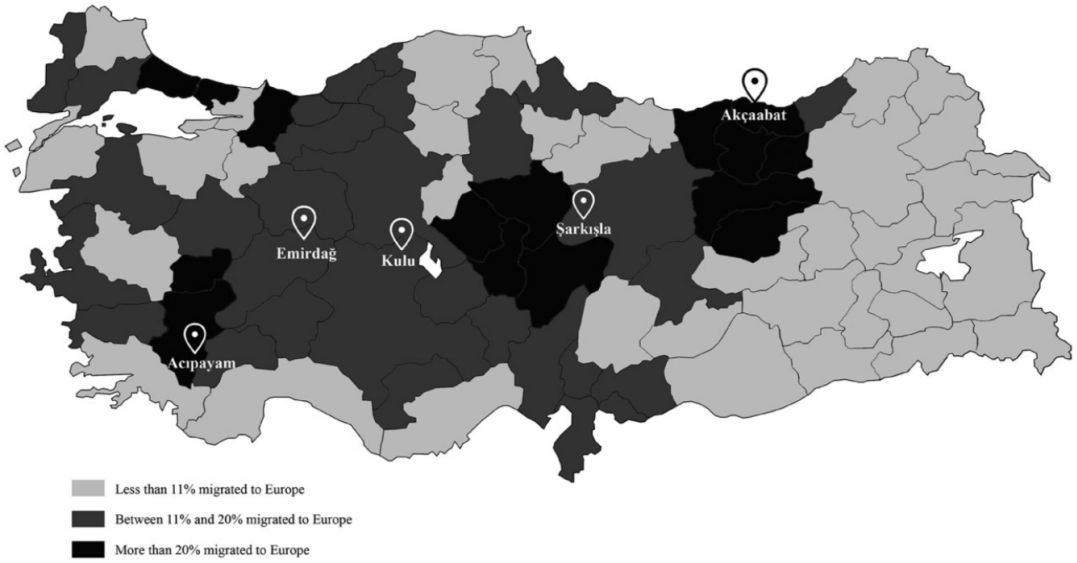


Figure 1: Ratio of emigrants to the labor force between 1960 and 1974 for regions in Turkey. Guveli et al. [2017]

Table 1: Reason for migration.

	GEN1	GEN2	GEN3
Get married/join spouse	0.24	14.83	25.90
Join family other than spouse	0.24	49.78	41.27
Moved with parent[s]/ spouse	0.36	16.35	11.75
For a job	98.08	14.83	10.24
For study	0.12	1.97	7.23
For asylum	0.00	0.27	0.00
Other	0.96	1.97	3.61
Observations	833	1,119	332

[1] Table reports percentages for each question and generation.

[2] GEN1 references the patriarch of the family line. Patriarchs were candidates for the Turkey-Europe guest migration program between 1960 and 1974.

[3] GEN2 and GEN3 migrants were the children and grandchildren of GEN1. GEN2 and GEN3 migrants were candidates for the family reunification programs that largely replaced the guest migration programs after 1974.

[4] Great grandchildren (GEN4) are excluded because of censoring.

Table 2: Top 10 Occupations by migration type.

	Migration type		
	Rural (Stayers)	Urban	International
1)	field crop & vegetable growers	other office clerks	assembling labourers
2)	car taxi & van drivers	car taxi & van drivers	sweepers etc labourers
3)	other teaching professionals	other teaching professionals	helpers & cleaners in estab
4)	general managers wholesale & retail	helpers & cleaners in estab	building construction labourers
5)	building construction labourers	general managers wholesale & retail	general managers restaurants & hotels
6)	assembling labourers	field crop & vegetable growers	waiters waitresses & bartenders
7)	other office clerks	teaching professionals	car taxi & van drivers
8)	handicraft workers in textile, leather	cooks	cooks
9)	waiters waitresses & bartenders	building construction labourers	handicraft workers in textile, leather
10)	general managers	domestic helpers & cleaners	plant & machine operators & assemblers

Table 3: Descriptive statistics.

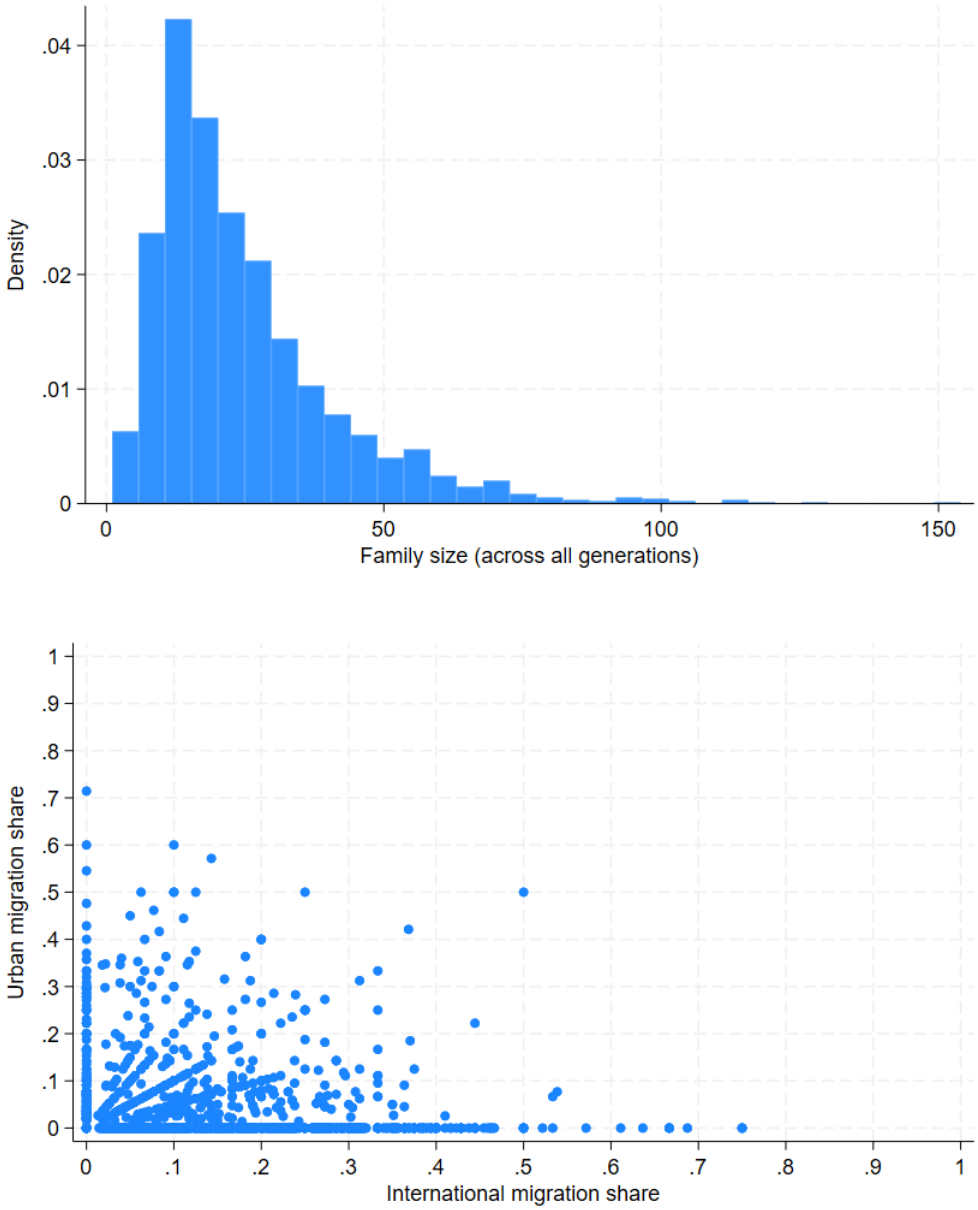
	GEN1		GEN2		GEN3	
	Urban	International	Urban	International	Urban	International
Age	71.45	71.57	42.37	44.58	24.83	27.85
	(5.93)	(4.99)	(8.64)	(6.72)	(4.94)	(5.72)
Female	0	0	0.45	0.38	0.47	0.44
			(0.50)	(0.49)	(0.50)	(0.50)
Rank	2.88	3.07	3.29	3.05	1.95	2.07
	(1.53)	(1.79)	(1.99)	(1.82)	(1.14)	(1.39)
Siblings	4.05	4.58	5.24	5.32	3.42	3.95
	(1.79)	(2.26)	(1.90)	(1.95)	(1.34)	(1.70)
Children	5.30	4.86	2.54	2.92	0.54	1.07
	(2.15)	(1.64)	(1.32)	(1.34)	(0.87)	(1.16)
Marriage	0.94	0.94	0.91	0.93	0.46	0.68
	(0.24)	(0.24)	(0.29)	(0.26)	(0.50)	(0.47)
Education	2.70	2.10	3.58	4.42	5.90	5.18
	(2.02)	(0.98)	(2.37)	(2.27)	(2.46)	(1.98)
Income	12,296	16,318	16,324	24,580	18,115	21,424
	(6,931)	(10,631)	(10,122)	(14,808)	(10,400)	(15,640)
Observations	180	870	1,350	1,079	401	73

[1] Table reports descriptive statistics for key variables by generation and migration status. migrants.

[2] Female and Marriage are binary variables. Rank=1 for oldest sibling in the immediate family.

[3] Standard deviations in parentheses.

Figure 2: Family-line sizes and propensities for urban and international migration.



Notes:

1 The top figure is a histogram of family-line sizes.

2 In the bottom figure, the unit of analysis is the family line. Each dot represents the international and urban migration share for a given family line. This figure shows several family lines (21% to be precise) have both international and urban migrants.

Table 4: Immediate families with and without migrants.

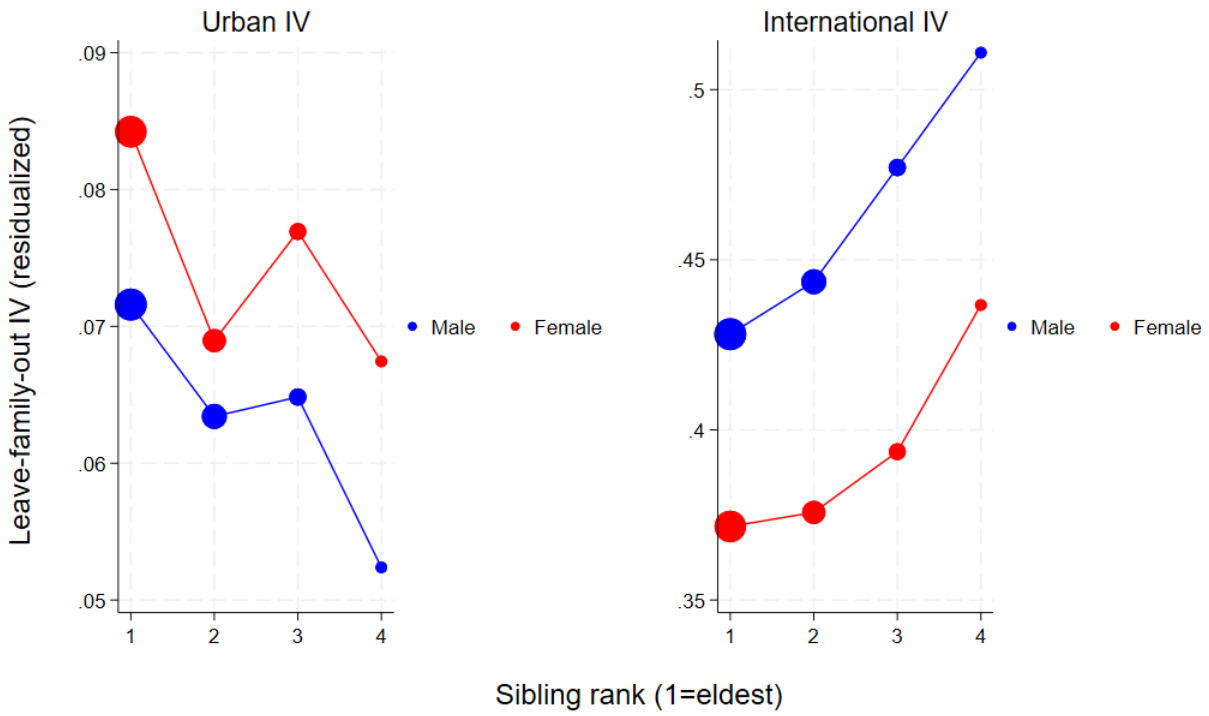
	Migration choice	
	Urban	International
	(1)	(2)
Primary school	-0.046	0.112
	(0.053)	(0.083)
Extended primary	0.053	0.140
	(0.070)	(0.092)
Lower secondary, general	0.056	0.137
	(0.059)	(0.088)
Lower secondary, vocational	-0.025	0.268***
	(0.070)	(0.103)
Higher/post-secondary, general	-0.053	0.078
	(0.065)	(0.090)
Higher/post-secondary, vocational	0.014	0.129
	(0.065)	(0.092)
Lower tertiary, vocational	0.113	0.151
	(0.100)	(0.113)
Lower tertiary, general (BA)	0.181**	0.009
	(0.073)	(0.092)
Higher tertiary (Masters)	1.050***	0.090
	(0.118)	(0.109)
Post-tertiary (PhD)	0.122*	0.044
	(0.073)	(0.093)
Observations	1389	1389

[1] This table considers the internal validity of within immediate family comparisons. It reports estimates of regressions of migration urban and international migration decisions on education, age deciles, gender, and sibling rank. All specifications include fixed effects for the immediate family.

[2] The base group for education is primary school dropout.

[3] Standard errors are in parentheses and clustered on the family line. ***, **, * denote statistical significance at the 1, 5, and 10 percent levels.

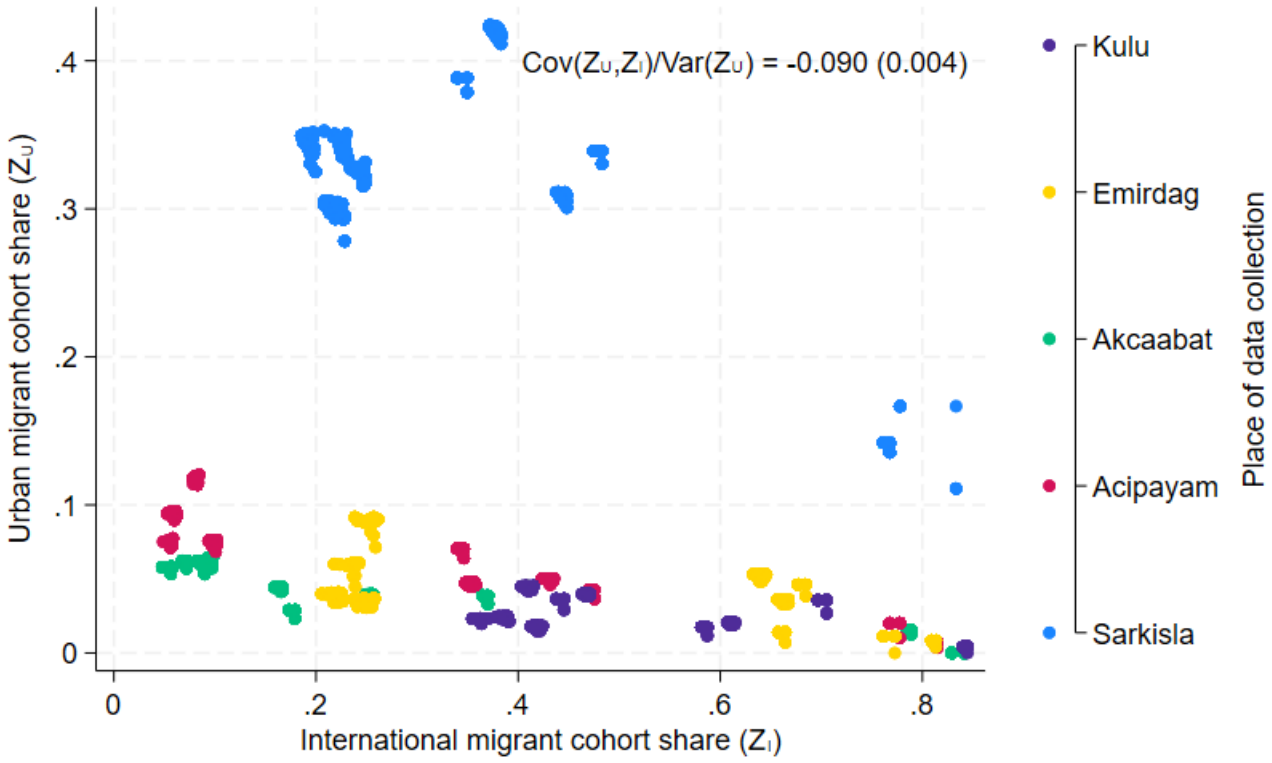
Figure 3: IVs by gender and sibling rank.



Notes:

1 Leave-family-out instruments are computed using age, gender, sibling rank amongst males (oldest=1), sibling rank amongst females (oldest=1), region, and generation.
2 Instruments are residualized by age decile dummies. We add back the mean for the purposes of visualization.
3 Dots are weighted by the sample size at that rank.

Figure 4: Instrument variation.



Notes:

1 Our instruments for urban and international migration leave out potential migrant i and their family members from the same generation $f(i)$:

$$Z_{-f(i)U} = \frac{\sum_{j \in C_{-f(i)}} D_{jU}}{|C_{-f(i)}|} \quad \text{and} \quad Z_{-f(i)I} = \frac{\sum_{j \in C_{-f(i)}} D_{jI}}{|C_{-f(i)}|}.$$

$C_{-f(i)}$ is the leave-family-out set. C is defined by age, gender, and sibling age rank within the immediate family, home town, and generation.

2 Figure only uses shares where the leave-out group has at least 10 people.

3 We report the coefficient from a simple regression of $Z_{-f(i)U}$ on $Z_{-f(i)I}$, -0.090, and the associated standard error, 0.004.

Table 5: First stage estimates.

	Migration choice	
	Urban	International
	(1)	(2)
Leave-family-out Urban	0.577***	0.105
	(0.095)	(0.105)
Leave-family-out International	-0.050	0.775***
	(0.046)	(0.056)
Age decile		
2 (22-24)	0.009	0.071***
	(0.034)	(0.027)
3 (25-28)	0.085**	0.028
	(0.036)	(0.025)
4 (29-33)	0.044	0.098***
	(0.034)	(0.030)
5 (34-38)	-0.016	0.136***
	(0.032)	(0.034)
6 (39-43)	-0.046	0.194***
	(0.030)	(0.032)
7 (44-48)	-0.050*	0.194***
	(0.030)	(0.033)
8 (49-57)	-0.044	0.152***
	(0.032)	(0.034)
9 (58-70)	-0.108***	0.235***
	(0.036)	(0.044)
10 (71+)	-0.108***	0.292***
	(0.037)	(0.005)
F tests for excluded instruments		
F(2,1484)	20.59	95.27
Sanderson-Windmeijer F(1,1484)	41.90	207.76
Anderson-Rubin F(2,1484)	9.37	9.37
p-value	0.0001	0.0001
Observations	3408	3408

[1] Table reports first stage estimates for instruments without cross effects.

[2] Unit of observation is the individual.

[3] The base group for urban and international migration is rural non-migrants.

[4] Anderson-Rubin robust F-statistic tests for a second stage effect on income and, at the same time, for the validity of the overidentifying restrictions. The Anderson-Rubin statistic is robust to weak instruments.

[5] Standard errors are in parentheses and clustered on the family line. ***, **, * denote statistical significance at the 1, 5, and 10 percent levels.

Table 6: IV estimates by pooling option.

	Household income (in logs)					
	TP	CP	CP	TP	TP	NP
	(1)	(2)	(3)	(4)	(5)	(6)
Urban		1.102***				1.100***
		(0.312)				(0.308)
International			-0.073			0.028
			(0.123)			(0.130)
Out Migration	0.184*			1.048***	-0.080	
	(0.112)			(0.276)	(0.137)	
Leave-family-out Urban IV	yes	yes	no	yes	no	yes
Leave-family-out International IV	yes	no	yes	no	yes	yes
Observations	3408	3408	3408	3408	3408	3408

[1] Table reports second stage estimates of the effects of migration decisions on household income. All specifications include age decile dummies.
[2] TP references treatment pooling. CP references control pooling. NP references no pooling.
[3] Unit of observation is the individual.
[4] The base group is rural non-migrants.
[5] Standard errors are in parentheses and clustered on the family line. ***, **, * denote statistical significance at the 1, 5, and 10 percent levels.

Table 7: Testing average conditional monotonicity (A3) and no cross effects (A4).

	$\mathbb{1}[Y \geq Y_{50}] \times \mathrm{D_U}$	$\mathbb{1}[Y \geq Y_{50}] \times \mathrm{D_I}$	$\mathbb{1}[Y < Y_{50}] \times \mathrm{D_U}$	$\mathbb{1}[Y < Y_{50}] \times \mathrm{D_I}$
	(1)	(2)	(3)	(4)
$\widehat{\mathrm{D}}_\mathrm{U}$	0.752***	0.004	0.248**	-0.004
	(0.133)	(0.164)	(0.111)	(0.133)
	[0.000]	[0.985]	[0.026]	[0.977]
$\widehat{\mathrm{D}}_\mathrm{I}$	-0.041	0.613***	0.041	0.387***
	(0.050)	(0.063)	(0.039)	(0.051)
	[0.402]	[0.000]	[0.300]	[0.000]
Observations	3408	2866	2866	2866

[1] Table reports estimates of regressions of $\mathbb{1}[Y \geq Y_{50}] \times \mathrm{D_U}$, $\mathbb{1}[Y < Y_{50}] \times \mathrm{D_U}$, $\mathbb{1}[Y \geq Y_{50}] \times \mathrm{D_I}$, and $\mathbb{1}[Y < Y_{50}] \times \mathrm{D_I}$ on $\widehat{\mathrm{D}}_\mathrm{U}$ and $\widehat{\mathrm{D}}_\mathrm{U}$, where Y_{50} is the median of household income (in logs).

[2] Unit of observation is the individual. All specifications include age decile dummies.

[2] The average conditional independence and no cross effects assumptions imply: i) positive coefficients on $\widehat{\mathrm{D}}_\mathrm{U}$ in the left panel and zero coefficients on the right; ii) zero coefficients on $\widehat{\mathrm{D}}_\mathrm{U}$ in the left panel and positive coefficients on the right.

[3] Standard errors are in round parentheses and clustered on family line. ***, **, * denote statistical significance at the 1, 5, and 10 percent levels. Bootstrap p-values are in square parentheses.

Table 8: First stage estimates with cross effects.

	Migration choice	
	Urban	International
	(1)	(2)
Oldest female in immediate family	0.059***	-0.023
	(0.019)	(0.018)
Leave-family-out Out-migration	0.130***	0.594***
	(0.048)	(0.052)
Age decile		
2 (22-24)	-0.005	0.087***
	(0.034)	(0.027)
3 (25-28)	0.079**	0.036
	(0.037)	(0.026)
4 (29-33)	0.026	0.121***
	(0.034)	(0.030)
5 (34-38)	-0.041	0.166***
	(0.033)	(0.034)
6 (39-43)	-0.093***	0.247***
	(0.030)	(0.032)
7 (44-48)	-0.099***	0.246***
	(0.030)	(0.032)
8 (49-57)	-0.095***	0.205***
	(0.031)	(0.033)
9 (58-70)	-0.225***	0.363***
	(0.035)	(0.040)
10 (71+)	-0.237***	0.438***
	(0.037)	(0.041)
F tests for excluded instruments		
F(2,1475)	7.57	70.65
Sanderson-Windmeijer F(1,1475)	10.20	18.85
Anderson-Rubin F(2,1475)	4.27	4.27
p-value	0.0142	0.0142
Observations	3373	3373

[1] Table reports first stage estimates for instruments with cross effects.

[2] Unit of observation is the individual.

[3] The base group for urban and international migration is rural non-migrants.

[4] Anderson-Rubin robust F-statistic tests for a second stage effect on income and, at the same time, for the validity of the overidentifying restrictions. The Anderson-Rubin statistic is robust to weak instruments.

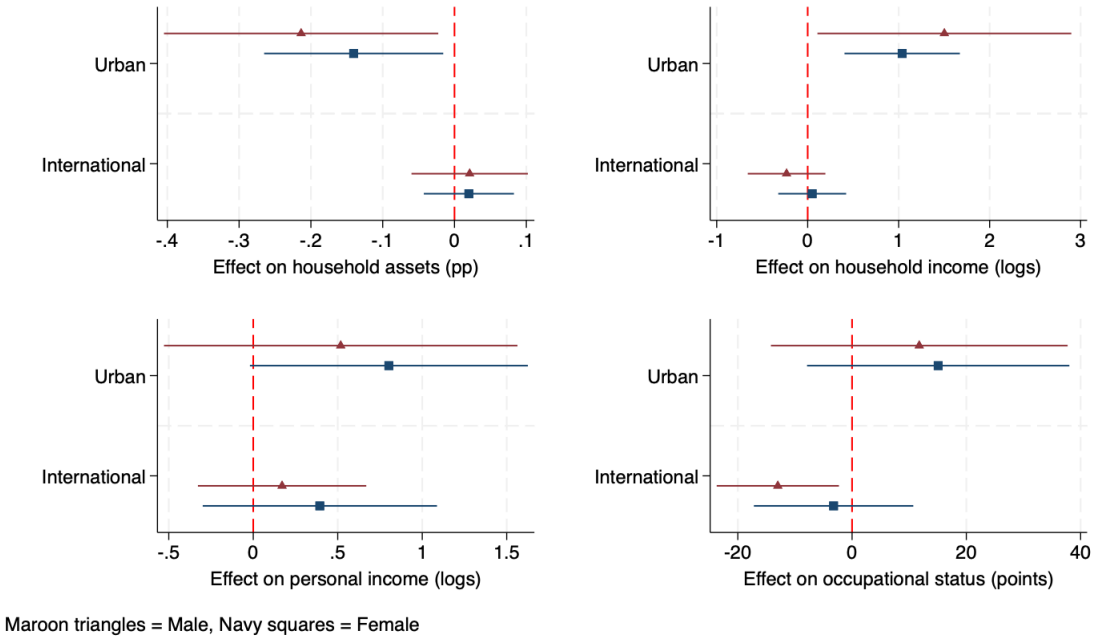
[5] Standard errors are in parentheses and clustered on the family line. ***, **, * denote statistical significance at the 1, 5, and 10 percent levels.

Table 9: IV estimates by pooling option with cross effects in first stage.

	Household income (in logs)					
	TP	CP	CP	TP	TP	NP
	(1)	(2)	(3)	(4)	(5)	(6)
Urban		-1.569*				-1.072*
		(0.865)				(0.641)
International			0.233*			0.419**
			(0.134)			(0.194)
Out Migration	0.157			8.481	0.197*	
	(0.111)			(19.399)	(0.112)	
Oldest female in immediate family IV	yes	yes	no	yes	no	yes
Leave-family-out Out-migration IV	yes	no	yes	no	yes	yes
Observations	3373	3373	3408	3373	3408	3373

[1] Table reports second stage estimates of the effects of migration decisions on household income when there are cross effects in the first stage. All specifications include age decile dummies.
[2] TP references treatment pooling. CP references control pooling. NP references no pooling.
[3] Unit of observation is the individual.
[4] The base group is rural non-migrants.
[5] Standard errors are in parentheses and clustered on the family line. ***, **, * denote statistical significance at the 1, 5, and 10 percent levels.

Figure 5: IV estimates for other outcomes and by gender.



Notes:

1 Estimated are based on regressions of the outcome on urban migration, international migration, and age decile dummies.

2 We instrument urban and international migration using our leave-family-out IVs.

3 Estimates are based on our main estimation sample (the sample used for Column 6 of Table 6). Our data on other economic outcomes is less complete than our data on household income. Our sample sizes are (M=male, F=female): 1. household assets (1577 M, 1078 F); 2. household income (2198 M, 1210 F); 3. personal income (926 M, 255 F); 4. occupational status (1664 M, 354 F).

4 Confidence intervals are based on standard errors that are clustered on the family line.

Table 10: IV estimates for Beegle, De Weerdt, and Dercon [2011]

	Consumption (in logs)					
	TP	CP	CP	TP	TP	NP
	(1)	(2)	(3)	(4)	(5)	(6)
	PANEL A. IVs without cross effects					
Inside Kagera		0.250				0.360*
		(0.190)				(0.194)
Outside Kagera			1.551*			1.456*
			(0.803)			(0.776)
Out Migration	0.684**			0.480	1.590	
	(0.318)			(0.347)	(1.056)	
Inside Kagera IV	yes	yes	no	yes	no	yes
Outside Kagera IV	yes	no	yes	no	yes	yes
	PANEL B. IVs with cross effects					
Inside Kagera		-0.026				-0.051
		(0.207)				(0.209)
Outside Kagera			-0.244			-0.106
			(0.889)			(0.684)
Out Migration	-0.071			-0.061	-0.097	
	(0.282)			(0.482)	(0.348)	
Leave-out Inside Kagera IV	yes	yes	no	yes	no	yes
Child of household head IV	yes	no	yes	no	yes	yes
Observations	3227	3227	3227	3227	3227	3227

[1] Table reports second stage estimates of the effects of migration decisions on consumption. All specifications include Beegle et al. [2011] Covariates and family fixed effects.

[2] TP references treatment pooling. CP references control pooling. NP references no pooling.

[3] Unit of observation is the individual.

[4] The base group stays in the village. Inside Kagera migration aggregates migration to nearby villages and migration to more distant places inside Kagera.

[5] Standard errors are in parentheses and clustered on the family. ***, **, * denote statistical significance at the 1, 5, and 10 percent levels.

Why are returns to internal migration so small? Evidence from competing migration destinations

Online Appendix

Sacha Kapoor and Fatma Palut and Dinand Webbink

January 14, 2026

This appendix formalizes the identification results summarized in Section 1 of the main text. The main text demonstrates the core identification issue — bias from unobserved alternative migration destinations — in a simplified setting with homogeneous returns and binary instruments. Here we extend the analysis to allow for heterogeneous returns across response types, introducing the model setup and notation before deriving the TSLS estimands under each of our three estimation strategies.

A.1 Model setup

We borrow our notation and general approach from Bhuller and Sigstad [2024], who study identification of multiple treatment effects when there is potential for treatment effect heterogeneity. Consider a potential migrant characterized by:

- Migration status $T \in \{R, U, I\}$ indicating rural stay, urban migration, or international migration
- Potential earnings $Y(R)$, $Y(U)$, and $Y(I)$ under each choice
- Instrumental variables $\mathbf{Z} = (Z_U, Z_I)$ that shift migration incentives
- Response type $S : \mathbf{Z} \to T$ that determines how migration decisions respond to the instruments.

The response type captures heterogeneity in how individuals respond to the instruments—the generalization of compliers, always-takers, and never-takers to the multi-treatment setting. Using binary indicators $D_U = \mathbb{1}[T = U]$ and $D_I = \mathbb{1}[T = I]$, observed earnings are:

$$Y = \underbrace{Y(R)}_{\beta_R} + \underbrace{[Y(U) - Y(R)]}_{\beta_U} D_U + \underbrace{[Y(I) - Y(R)]}_{\beta_I} D_I$$

where β_{U} and β_{I} are the economic returns to urban and international migration. These returns may vary across individuals. Our target parameters are averages: $\mathbb{E}[\beta_U]$ and $\mathbb{E}[\beta_I]$, or equivalently $\mathbb{E}[\beta_U^S]$ and $\mathbb{E}[\beta_I^S]$ where $\beta_U^S = \mathbb{E}[\beta_U|S]$ and $\beta_I^S = \mathbb{E}[\beta_I|S]$.

We maintain the following assumptions throughout:

A1 $\mathrm{Y(R)}, \mathrm{Y(U)}, \mathrm{Y(I)}, \mathrm{S} \perp \mathbf{Z}$ (Exogeneity and exclusion)

A2 $\mathrm{Cov}(\mathbf{Z}, \mathbf{D})$ has full rank (Full rank)

Under these assumptions, the TSLS estimand in each estimation strategy is a weighted average of the true returns β_U^S and β_I^S across response types. Table A.1.1 summarizes these weights for each strategy.

A.2 Control pooling (CP) approach

We first consider the standard approach in the literature—identifying one migration return while treating the other migration type as part of the control group. For concreteness, we analyze international migration returns ($\mathbb{E}[\beta_I^S]$), though the same logic applies to urban migration returns. In this approach, we estimate:

$$\beta_{\mathrm{I}}^{\mathrm{TSLS}} = \frac{\mathrm{Cov(Y, Z_I)}}{\mathrm{Cov(D_I, Z_I)}}$$

Substituting our earnings equation and expanding terms reveals:

$$\beta_{\mathrm{I}}^{\mathrm{TSLS}} = \mathbb{E}_{\mathrm{S}}\left[w_I^S \beta_{\mathrm{I}}^{\mathrm{S}} + w_U^S \beta_{\mathrm{U}}^{\mathrm{S}} \right] \tag{7}$$

where

$$w_I^S = \frac{\mathrm{Cov(S_I(\mathbf{Z}), Z_I|S)}}{\mathrm{Cov(D_I, Z_I)}} \quad \text{and} \quad w_U^S = \frac{\mathrm{Cov(S_U(\mathbf{Z}), Z_I|S)}}{\mathrm{Cov(D_I, Z_I)}} \tag{8}$$

Table A.1.1: TSLS estimands under different pooling assumptions.

Pooling choice	w_I^S (1)	w_U^S (2)	$\mathbb{E}[w_I^S]$ (3)	$\mathbb{E}[w_U^S]$ (4)
control pooling $D = D_I$ IVs : Z_I	$\frac{Cov(S_I(\mathbf{Z}),Z_I\|S)}{Cov(D_I,Z_I)}$	$\frac{Cov(S_U(\mathbf{Z}),Z_I\|S)}{Cov(D_I,Z_I)}$	1	$\frac{Cov(D_U,Z_I)}{Cov(D_I,Z_I)}$
treatment pooling $D = D_I + D_U$ IVs : Z	$\frac{Cov(S_I(Z),Z\|S)}{Cov(D,Z)}$	$\frac{Cov(S_U(Z),Z\|S)}{Cov(D,Z)}$	$\frac{Cov(D_I,Z)}{Cov(D,Z)}$	$\frac{Cov(D_U,Z)}{Cov(D,Z)}$
no pooling D_I, D_U IVs : Z_I, Z_U	$w_I \frac{Cov(S_I(\mathbf{Z}),Z_I\|S)}{Cov(D_I,Z_I)} + w_U \frac{Cov(S_I(\mathbf{Z}),Z_U\|S)}{Cov(D_U,Z_U)}$ $w_I = 1 \Big/ \left[1 - \frac{Cov(D_I,Z_U)Cov(D_U,Z_I)}{Cov(D_I,Z_I)Cov(D_U,Z_U)}\right]$	$w_I \frac{Cov(S_U(\mathbf{Z}),Z_I\|S)}{Cov(D_I,Z_I)} + w_U \frac{Cov(S_U(\mathbf{Z}),Z_U\|S)}{Cov(D_U,Z_U)}$ $w_U = -\frac{Cov(D_I,Z_U)Cov(D_U,Z_I)}{Cov(D_I,Z_I)Cov(D_U,Z_U)} w_I$	$w_I + w_U \frac{Cov(D_I,Z_U)}{Cov(D_U,Z_I)}$	$w_I \frac{Cov(D_U,Z_I) - Cov(D_I,Z_U)}{Cov(D_I,Z_I)}$

[1] The TSLS estimand of β_I equals $\mathbb{E}_S[w_I^S \beta_I^S + w_U^S \beta_U^S]$ in each of the three cases: control pooling, treatment pooling, and no pooling.

[2] Table focuses on TSLS estimand of β_I. The results for the TSLS estimand of β_U are the same by symmetry.

[3] All derivations are based on the conventional IV assumptions of exogeneity, exclusion, full rank.

[4] Assume returns are homogenous across response types: $\beta_I^S = \beta_I$ and $\beta_U^S = \beta_U$. The TSLS estimand identifies β_I if $\mathbb{E}[w_I^S] = 1$ and $\mathbb{E}[w_U^S] = 0$.

[5] If returns are heterogeneous then the TSLS estimand identifies a proper weighted average of the β_I^S if $\mathbb{E}[w_I^S] = 1$, $w_I^S \geq 0$ with strict inequality for some S, and $w_U^S = 0$ for all S.

[6] Key result: Under no cross effects ($Cov(D_U, Z_I) = Cov(D_I, Z_U) = 0$), column 3 equals 1 and column 4 equals 0 for all approaches.

There is no bias in $\beta_{\mathrm{I}}^{\mathrm{TSLS}}$ if the true returns $\beta_{\mathrm{I}}^{\mathrm{S}}$ and $\beta_{\mathrm{U}}^{\mathrm{S}}$ equal 0 for all S. The potential for zero returns is nontrivial, especially for domestic rural to urban migration, where migration frictions are less pronounced than in the case of international migration. If sorting is efficient, i.e. migrants have already sorted into the locations that yield the highest income return, then the equilibrium return to migration will be zero. Any differential in raw earnings will reflect differential selection alone.

This estimator faces two potential biases when the true returns ($\beta_{\mathrm{I}}^{\mathrm{S}}$ and $\beta_{\mathrm{U}}^{\mathrm{S}}$) are non-zero:

1. *Negative weights*: The weights on international returns ($\frac{\mathrm{Cov}(\mathrm{S_I}(\mathbf{Z}),\mathrm{Z_I}|\mathrm{S})}{\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_I})}$) could be negative for some response types. For instance, highly productive individuals who choose not to migrate despite high potential returns abroad could generate negative weights.

2. *Cross-effect contamination*: The estimator may place non-zero weights on urban migration returns if the international migration instrument affects urban migration decisions. This could happen if the international migration instrument shifts the cost of migrating anywhere.

Below we show the weights on the true returns to international migration sum to one while the weights on the true returns to urban migration sum to $\frac{\mathrm{Cov}(\mathrm{D_U},\mathrm{Z_I})}{\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_I})}$.

To understand the implications, consider the case of homogeneous returns across response types ($\beta_{\mathrm{I}}^{\mathrm{S}} = \beta_{\mathrm{I}}$ and $\beta_{\mathrm{U}}^{\mathrm{S}} = \beta_{\mathrm{U}}$). Here the estimator simplifies to:

$$\beta_{\mathrm{I}}^{TSLS} = \beta_{\mathrm{I}} + \frac{\mathrm{Cov}(D_{\mathrm{U}}, Z_{\mathrm{I}})}{\mathrm{Cov}(D_{\mathrm{I}}, Z_{\mathrm{I}})}\beta_{\mathrm{U}} \tag{9}$$

This formula generalizes the bias derived in Section 1.2. As in that section, the formula shows that identification requires no cross effects from the international migration instrument to urban migration decisions ($\mathrm{Cov}(\mathrm{D_U}, \mathrm{Z_I}) = 0$).

More generally, following Bhuller and Sigstad [2024], identification requires "proper weights"—non-negative weights on β_{I}^{S} and zero weights on β_{U}^{S} for each response type. This is achieved

when:

CP-A3 $\mathbb{E}[Z_I|S_I(\mathbf{Z})=1,S] \geq \mathbb{E}[Z_I|S_I(\mathbf{Z})=0,S]$ with strict inequality for some S (average conditional monotonicity)

CP-A4 $\mathbb{E}[Z_I|S_U(\mathbf{Z})=1,S] = \mathbb{E}[Z_I|S_U(\mathbf{Z})=0,S]$ for all S (no cross effects per type)

These conditions need only hold on average within response types, not for every individual. The first assumption requires that the instrument increases international migration probability (monotonicity), while the second ensures the instrument doesn't affect urban migration decisions.

A.2.1. Derivation of equation (7) The TSLS estimand in this case is

$$\beta_I^{TSLS} = \frac{\text{Cov}(Y, Z_I)}{\text{Cov}(D_I, Z_I)}$$

Plugging in $Y = \beta_R + \beta_U D_U + \beta_I D_I$ gives $\text{Cov}(Y, Z_I) = \text{Cov}(\beta_U D_U, Z_I) + \text{Cov}(\beta_I D_I, Z_I)$. The first covariance $\text{Cov}(\beta_U D_U, Z_I)$ equals

$$\begin{aligned}
&\mathbb{E}_{Z,S}\Big[\mathbb{E}[\beta_U D_U Z_I|\mathbf{Z},S]\Big] - \mathbb{E}_{Z,S}\Big[\mathbb{E}[\beta_U D_U|\mathbf{Z},S]\Big]\mathbb{E}_{Z,S}[Z_I] \\
&= \mathbb{E}_{Z,S}\Big[\mathbb{E}[\beta_U S_U(\mathbf{Z}) Z_I|\mathbf{Z},S]\Big] - \mathbb{E}_{Z,S}\Big[\mathbb{E}[\beta_U S_U(\mathbf{Z})|\mathbf{Z},S]\Big]\mathbb{E}_Z[Z_I] \\
&= \mathbb{E}_{Z,S}\Big[S_U(\mathbf{Z}) Z_I \mathbb{E}[\beta_U|\mathbf{Z},S]\Big] - \mathbb{E}_{Z,S}\Big[S_U(\mathbf{Z})\mathbb{E}[\beta_U|\mathbf{Z},S]\Big]\mathbb{E}_Z[Z_I] \\
&= \mathbb{E}_{Z,S}\Big[S_U(\mathbf{Z}) Z_I \mathbb{E}[\beta_U|S]\Big] - \mathbb{E}_{Z,S}\Big[S_U(\mathbf{Z})\mathbb{E}[\beta_U|S]\Big]\mathbb{E}_Z[Z_I] \\
&= \mathbb{E}_{Z,S}\Big[S_U(\mathbf{Z}) Z_I \mathbb{E}[\beta_U|S]\Big] - \mathbb{E}_{Z,S}\Big[S_U(\mathbf{Z})\mathbb{E}[\beta_U|S]\Big]\mathbb{E}_Z[Z_I|S] \\
&= \mathbb{E}_{Z,S}\Big[S_U(\mathbf{Z})\big(Z_I - \mathbb{E}_Z[Z_I|S]\big)\mathbb{E}[\beta_U|S]\Big] \\
&= \mathbb{E}_S\Big[\mathbb{E}_{Z|S}\Big[S_U(\mathbf{Z})\big(Z_I - \mathbb{E}_Z[Z_I|S]\big)\Big]\mathbb{E}[\beta_U|S]\Big] \\
&= \mathbb{E}_S\Big[\text{Cov}(S_U(\mathbf{Z}), Z_I|S)\mathbb{E}[\beta_U|S]\Big]
\end{aligned}$$

where the subscripts on $\mathbb{E}$ indicate which distribution the expectation is taken over. By similar calculations, the second covariance $\text{Cov}(\beta_I D_I, Z_I)$ equals

$$\mathbb{E}_{Z,S}\Big[\mathbb{E}[\beta_I D_I Z_I|\mathbf{Z},S]\Big] - \mathbb{E}_{Z,S}\Big[\mathbb{E}[\beta_I D_I|\mathbf{Z},S]\Big]\mathbb{E}[Z_I] = \mathbb{E}_S\Big[\text{Cov}(S_I(\mathbf{Z}),Z_I|S)\mathbb{E}[\beta_I|S]\Big]$$

The TSLS estimand is then

$$\beta_I^{\text{TSLS}} = \mathbb{E}_S\left[\frac{\text{Cov}(S_I(\mathbf{Z}),Z_I|S)}{\text{Cov}(D_I,Z_I)}\beta_I^S + \frac{\text{Cov}(S_U(\mathbf{Z}),Z_I|S)}{\text{Cov}(D_I,Z_I)}\beta_U^S\right]$$

A.2.2. CP weights sum to one and cross-effect ratio We show that the weights on β_I^S sum to 1 while the weights on β_U^S sum to $\frac{\text{Cov}(D_U,Z_I)}{\text{Cov}(D_I,Z_I)}$. The denominator of the first set of weights equals

$$\begin{aligned}
\text{Cov}(D_I,Z_I) &= \mathbb{E}_S[\mathbb{E}[D_I Z_I|S]] - \mathbb{E}_S[\mathbb{E}[D_I|S]]\mathbb{E}_S[\mathbb{E}[Z_I|S]] \\
&= \mathbb{E}_S[\mathbb{E}[S_I(\mathbf{Z})Z_I|S]] - \mathbb{E}_S[\mathbb{E}[S_I(\mathbf{Z})|S]]\mathbb{E}_S[\mathbb{E}[Z_I|S]] \\
&= \mathbb{E}_S[\mathbb{E}[S_I(\mathbf{Z})Z_I|S]] - \mathbb{E}_S[\mathbb{E}[S_I(\mathbf{Z})|S]]\mathbb{E}[Z_I] \\
&= \mathbb{E}_S[\mathbb{E}[S_I(\mathbf{Z})(Z_I - \mathbb{E}[Z_I])|S]] \\
&= \mathbb{E}_S[\text{Cov}(S_I(\mathbf{Z}),Z_I|S)].
\end{aligned}$$

It follows that $\mathbb{E}_S\left[\frac{\text{Cov}(S_I(\mathbf{Z}),Z_I|S)}{\text{Cov}(D_I,Z_I)}\right] = 1$. By a similar calculation, the second set of weights adds up to $\frac{\text{Cov}(D_U,Z_I)}{\text{Cov}(D_I,Z_I)}$.

A.2.3. Proper CP weights The sign of the weights on β_{I}^{S} are determined by

$$\begin{aligned}
\mathrm{Cov}(\mathrm{S_I}(\mathbf{Z}),\mathrm{Z_I}|\mathrm{S}) =& \mathbb{E}[\mathrm{S_I}(\mathbf{Z})\mathrm{Z_I}|\mathrm{S}] - \mathbb{E}[\mathrm{S_I}(\mathbf{Z})|\mathrm{S}]\mathbb{E}[\mathrm{Z_I}|\mathrm{S}] \\
=& \mathbb{E}[\mathrm{Z_I}|\mathrm{S_I}(\mathbf{Z})=1,\mathrm{S}]\mathbb{P}(\mathrm{S_I}(\mathbf{Z})=1|\mathrm{S}) - \mathbb{E}[\mathrm{S_I}(\mathbf{Z})|\mathrm{S}]\mathbb{E}[\mathrm{Z_I}|\mathrm{S_I}(\mathbf{Z})=1,\mathrm{S}]\mathbb{P}(\mathrm{S_I}(\mathbf{Z})=1|\mathrm{S}) \\
& - \mathbb{E}[\mathrm{S_I}(\mathbf{Z})|\mathrm{S}]\mathbb{E}[\mathrm{Z_I}|\mathrm{S_I}(\mathbf{Z})=0,\mathrm{S}]\mathbb{P}(\mathrm{S_I}(\mathbf{Z})=0|\mathrm{S}) \\
=& \mathbb{E}[\mathrm{Z_I}|\mathrm{S_I}(\mathbf{Z})=1,\mathrm{S}]\mathbb{P}(\mathrm{S_I}(\mathbf{Z})=0|\mathrm{S})\mathbb{P}(\mathrm{S_I}(\mathbf{Z})=1|\mathrm{S}) \\
& - \mathbb{E}[\mathrm{Z_I}|\mathrm{S_I}(\mathbf{Z})=0,\mathrm{S}]\mathbb{P}(\mathrm{S_I}(\mathbf{Z})=0|\mathrm{S})\mathbb{P}(\mathrm{S_I}(\mathbf{Z})=1|\mathrm{S}) \\
=& \Big(\mathbb{E}[\mathrm{Z_I}|\mathrm{S_I}(\mathbf{Z})=1,\mathrm{S}] - \mathbb{E}[\mathrm{Z_I}|\mathrm{S_I}(\mathbf{Z})=0,\mathrm{S}]\Big)\mathbb{P}(\mathrm{S_I}(\mathbf{Z})=0|\mathrm{S})\mathbb{P}(\mathrm{S_I}(\mathbf{Z})=1|\mathrm{S}).
\end{aligned}$$

Similarly, the weights on β_{U}^{S} equal

$$\mathrm{Cov}(\mathrm{S_U}(\mathbf{Z}),\mathrm{Z_I}|\mathrm{S}) = \Big(\mathbb{E}[\mathrm{Z_I}|\mathrm{S_U}(\mathbf{Z})=1,\mathrm{S}] - \mathbb{E}[\mathrm{Z_I}|\mathrm{S_U}(\mathbf{Z})=0,\mathrm{S}]\Big)\mathbb{P}(\mathrm{S_U}(\mathbf{Z})=0|\mathrm{S})\mathbb{P}(\mathrm{S_U}(\mathbf{Z})=1|\mathrm{S}).$$

A.3 Treatment pooling (TP) approach

Another common approach treats all migration as a single decision. Here, the treatment variable is $\mathrm{D} = \mathrm{D_U} + \mathrm{D_I}$, representing "outmigration" without distinguishing between international and domestic destinations. The TSLS estimand becomes: $\beta^{\mathrm{TSLS}} = \mathrm{Cov}(\mathrm{Y},\mathrm{Z})/\mathrm{Cov}(\mathrm{D},\mathrm{Z})$. Following the same substitution process as before yields $\beta^{\mathrm{TSLS}} = \mathbb{E}_{\mathrm{S}}\left[w_I^S\beta_{\mathrm{I}}^{\mathrm{S}} + w_U^S\beta_{\mathrm{U}}^{\mathrm{S}}\right]$ where

$$w_I^S = \frac{\mathrm{Cov}(\mathrm{S_I}(\mathrm{Z}),\mathrm{Z}|\mathrm{S})}{\mathrm{Cov}(\mathrm{D},\mathrm{Z})} \quad \text{and} \quad w_U^S = \frac{\mathrm{Cov}(\mathrm{S_U}(\mathrm{Z}),\mathrm{Z}|\mathrm{S})}{\mathrm{Cov}(\mathrm{D},\mathrm{Z})} \tag{10}$$

This estimand is a weighted average of both migration returns. The weights sum to 1 because $\mathbb{E}_{\mathrm{S}}[\mathrm{Cov}(\mathrm{S_I}(\mathrm{Z}),\mathrm{Z}|\mathrm{S})] = \mathrm{Cov}(\mathrm{D_I},\mathrm{Z})$ and $\mathbb{E}_{\mathrm{S}}[\mathrm{Cov}(\mathrm{S_U}(\mathrm{Z}),\mathrm{Z}|\mathrm{S})] = \mathrm{Cov}(\mathrm{D_U},\mathrm{Z})$. With homogeneous returns, β^{TSLS} recovers a weighted average of β_{I} and β_{U}.

However, negative weights can arise with certain instruments. Consider instruments based on urban proximity: for some response types, greater urban proximity might increase urban migration while decreasing international migration. For these types, the weight on β_I^S becomes negative while the weight on β_U^S is positive, potentially leading to misleading estimates.

If the target parameter is the average return to outmigration generally, TSLS assigns proper weights under treatment pooling when:

TP-A3 $\mathbb{E}[Z|S_I(Z) = 1, S] \geq \mathbb{E}[Z|S_I(Z) = 0, S]$ with strict inequality for some S (monotonicity for international)

TP-A4 $\mathbb{E}[Z|S_U(Z) = 1, S] \geq \mathbb{E}[Z|S_U(Z) = 0, S]$ with strict inequality for some S (monotonicity for urban)

These conditions require that both migration choices respond monotonically to the instrument and in the same direction for each response type.

Some instruments effectively satisfy these monotonicity requirements. For instance, Beegle et al. [2011]'s household head status indicator likely affects migration decisions in a uniform direction—we would not expect household heads to have systematically higher migration propensities to any destination. In contrast, instruments related to destination proximity often violate these conditions by affecting different migration pathways in opposite directions. When such violations occur, the TSLS estimand places negative weights on true returns for some response types, potentially yielding misleading or even wrong-signed average effect estimates.

A.4 No pooling (NP) approach

Our third approach estimates returns to both migration types simultaneously without pooling destinations. This represents a multiple treatment framework where we directly estimate separate returns for urban and international migration.

The multitreatment TSLS estimand for international migration returns is

$$\beta_I^{TSLS} = \mathbb{E}_S\Big[w_I^S \beta_I^S + w_U^S \beta_U^S \Big] \tag{11}$$

where

$$w_I^S = w_I \frac{\text{Cov}(S_I(\mathbf{Z}), Z_I|S)}{Cov(D_I, Z_I)} + w_U \frac{\text{Cov}(S_I(\mathbf{Z}), Z_U|S)}{Cov(D_U, Z_U)} \tag{12}$$

$$w_U^S = w_I \frac{\text{Cov}(S_U(\mathbf{Z}), Z_I|S)}{Cov(D_I, Z_I)} + w_U \frac{\text{Cov}(S_U(\mathbf{Z}), Z_U|S)}{Cov(D_U, Z_U)} \tag{13}$$

and

$$w_I = 1 \Big/ \Big[1 - \frac{\text{Cov}(D_I, Z_U)\text{Cov}(D_U, Z_I)}{\text{Cov}(D_I, Z_I)\text{Cov}(D_U, Z_U)}\Big] \quad \text{and} \quad w_U = -\frac{\text{Cov}(D_I, Z_U)\text{Cov}(D_U, Z_I)}{\text{Cov}(D_I, Z_I)\text{Cov}(D_U, Z_U)} w_I. \tag{14}$$

Below we demonstrate that the first set of weights sum to 1 while the second set sums to 0 if and only if the cross effects are symmetric: $\text{Cov}(D_I, Z_U) = \text{Cov}(D_U, Z_I)$. Under this symmetry condition, proper identification requires:

NP-A3 $\mathbb{E}[\tilde{Z}_I|S_I(\mathbf{Z}) = 1, S] \geq \mathbb{E}[\tilde{Z}_I|S_I(\mathbf{Z}) = 0, S]$ (Average conditional monotonicity)

NP-A4 $\mathbb{E}[\tilde{Z}_I|S_U(\mathbf{Z}) = 1, S] = \mathbb{E}[\tilde{Z}_I|S_U(\mathbf{Z}) = 0, S]$ for all S (No cross effects per type)

where $\tilde{Z}_I = Z_I - \frac{\text{Cov}(Z_I, D_U)}{\text{Cov}(Z_U, D_U)} Z_U$ is a residualized instrument.

A.4.1. Derivation of NP estimand The multi-treatment TSLS estimand is

$$\boldsymbol{\beta}_{2x1}^{TSLS} = \text{Cov}(\mathbf{D}, \mathbf{Z})_{2x2}^{-1} \text{Cov}(\mathbf{Y}, \mathbf{Z})_{2x1}.$$

This equals

$$\boldsymbol{\beta}_{1x2}^{TSLS} = \begin{pmatrix} \frac{\text{Cov}(\text{D}_\text{U},\text{Z}_\text{U})\text{Cov}(\text{Y},\text{Z}_\text{I}) - \text{Cov}(\text{D}_\text{I},\text{Z}_\text{U})\text{Cov}(\text{Y},\text{Z}_\text{U})}{\text{Cov}(\text{D}_\text{I},\text{Z}_\text{I})\text{Cov}(\text{D}_\text{U},\text{Z}_\text{U}) - \text{Cov}(\text{D}_\text{I},\text{Z}_\text{U})\text{Cov}(\text{D}_\text{U},\text{Z}_\text{I})} & \frac{-\text{Cov}(\text{D}_\text{U},\text{Z}_\text{I})\text{Cov}(\text{Y},\text{Z}_\text{I}) + \text{Cov}(\text{D}_\text{I},\text{Z}_\text{I})\text{Cov}(\text{Y},\text{Z}_\text{U})}{\text{Cov}(\text{D}_\text{I},\text{Z}_\text{I})\text{Cov}(\text{D}_\text{U},\text{Z}_\text{U}) - \text{Cov}(\text{D}_\text{I},\text{Z}_\text{U})\text{Cov}(\text{D}_\text{U},\text{Z}_\text{I})} \end{pmatrix}$$

where we have taken the transpose to simplify the exposition. Note that the denominator in both cases equals the determinant: $det(\text{Cov}(\mathbf{D},\mathbf{Z})_{2x2})$. We first simplify the first entry

$$\begin{aligned} \beta_I^{TSLS} &= \frac{\text{Cov}(\text{D}_\text{I},\text{Z}_\text{I})\left[\text{Cov}(\text{D}_\text{U},\text{Z}_\text{U})\frac{\text{Cov}(\text{Y},\text{Z}_\text{I})}{\text{Cov}(\text{D}_\text{I},\text{Z}_\text{I})} - \frac{\text{Cov}(\text{D}_\text{I},\text{Z}_\text{U})}{\text{Cov}(\text{D}_\text{I},\text{Z}_\text{I})}\text{Cov}(\text{Y},\text{Z}_\text{U})\right]}{\text{Cov}(\text{D}_\text{I},\text{Z}_\text{I})\left[\text{Cov}(\text{D}_\text{U},\text{Z}_\text{U}) - \frac{\text{Cov}(\text{D}_\text{I},\text{Z}_\text{U})\text{Cov}(\text{D}_\text{U},\text{Z}_\text{I})}{\text{Cov}(\text{D}_\text{I},\text{Z}_\text{I})}\right]} \\ &= \frac{\left[\frac{\text{Cov}(\text{Y},\text{Z}_\text{I})}{\text{Cov}(\text{D}_\text{I},\text{Z}_\text{I})} - \frac{\text{Cov}(\text{D}_\text{I},\text{Z}_\text{U})}{\text{Cov}(\text{D}_\text{I},\text{Z}_\text{I})}\frac{\text{Cov}(\text{Y},\text{Z}_\text{U})}{\text{Cov}(\text{D}_\text{U},\text{Z}_\text{U})}\right]}{\left[1 - \frac{\text{Cov}(\text{D}_\text{I},\text{Z}_\text{U})\text{Cov}(\text{D}_\text{U},\text{Z}_\text{I})}{\text{Cov}(\text{D}_\text{I},\text{Z}_\text{I})\text{Cov}(\text{D}_\text{U},\text{Z}_\text{U})}\right]} \\ &= w_\text{I}\frac{\text{Cov}(\text{Y},\text{Z}_\text{I})}{\text{Cov}(\text{D}_\text{I},\text{Z}_\text{I})} + w_\text{U}\frac{\text{Cov}(\text{Y},\text{Z}_\text{U})}{\text{Cov}(\text{D}_\text{U},\text{Z}_\text{U})}. \end{aligned}$$

To simplify the notation for the next few calculations, we set $w_\text{I}^* = w_\text{I}/\text{Cov}(\text{D}_\text{I},\text{Z}_\text{I})$ and $w_\text{U}^* = w_\text{U}/\text{Cov}(\text{D}_\text{U},\text{Z}_\text{U})$. Plugging in $\text{Y} = \beta_\text{R} + \beta_\text{U}\text{D}_\text{U} + \beta_\text{I}\text{D}_\text{I}$ gives

$$\beta_I^{TSLS} = w_\text{I}^*\text{Cov}(\beta_\text{I}\text{D}_\text{I},\text{Z}_\text{I}) + w_\text{I}^*\text{Cov}(\beta_\text{U}\text{D}_\text{U},\text{Z}_\text{I}) + w_\text{U}^*\text{Cov}(\beta_\text{I}\text{D}_\text{I},\text{Z}_\text{U}) + w_\text{U}^*\text{Cov}(\beta_\text{U}\text{D}_\text{U},\text{Z}_\text{U})$$

The first covariance equals

$$\begin{aligned}
\text{Cov}(\beta_\text{I} \text{D}_\text{I}, \text{Z}_\text{I}) =& \mathbb{E}_{\text{Z,S}}\Big[\mathbb{E}[\beta_\text{I} \text{D}_\text{I} \text{Z}_\text{I} | \text{S}, \mathbf{Z}]\Big] - \mathbb{E}_{\text{Z,S}}\Big[\mathbb{E}[\beta_\text{I} \text{D}_\text{I} | \text{S}, \mathbf{Z}]]\mathbb{E}[\text{Z}_\text{I}\Big] \\
=& \mathbb{E}_{\text{Z,S}}\Big[\mathbb{E}[\beta_\text{I} \text{S}_\text{I}(\mathbf{Z}) \text{Z}_\text{I} | \text{S}, \mathbf{Z}]\Big] - \mathbb{E}_{\text{Z,S}}\Big[\mathbb{E}[\beta_\text{I} \text{S}_\text{I}(\mathbf{Z}) | \text{S}, \mathbf{Z}]]\mathbb{E}[\text{Z}_\text{I}\Big] \\
=& \mathbb{E}_{\text{Z,S}}\Big[\text{S}_\text{I}(\mathbf{Z}) \text{Z}_\text{I} \mathbb{E}[\beta_\text{I} | \text{S}, \mathbf{Z}]\Big] - \mathbb{E}_{\text{Z,S}}\Big[\text{S}_\text{I}(\mathbf{Z}) \mathbb{E}[\beta_\text{I} | \text{S}, \mathbf{Z}]]\mathbb{E}[\text{Z}_\text{I}\Big] \\
=& \mathbb{E}_{\text{Z,S}}\Big[\text{S}_\text{I}(\mathbf{Z}) \text{Z}_\text{I} \mathbb{E}[\beta_\text{I} | \text{S}]] - \mathbb{E}_{\text{Z,S}}[\text{S}_\text{I}(\mathbf{Z}) \mathbb{E}[\beta_\text{I} | \text{S}]]\mathbb{E}[\text{Z}_\text{I}\Big] \\
=& \mathbb{E}_{\text{Z,S}}\Big[\text{S}_\text{I}(\mathbf{Z}) \text{Z}_\text{I} \mathbb{E}[\beta_\text{I} | \text{S}]] - \mathbb{E}_{\text{Z,S}}[\text{S}_\text{I}(\mathbf{Z}) \mathbb{E}[\beta_\text{I} | \text{S}]]\mathbb{E}[\text{Z}_\text{I} | \text{S}\Big] \\
=& \mathbb{E}_{\text{Z,S}}\Big[\text{S}_\text{I}(\mathbf{Z}) \big(\text{Z}_\text{I} - \mathbb{E}[\text{Z}_\text{I} | \text{S}]\big) \mathbb{E}[\beta_\text{I} | \text{S}]\Big] \\
=& \mathbb{E}_\text{S}\Big[\mathbb{E}_{\text{Z}|\text{S}}\Big[\text{S}_\text{I}(\mathbf{Z}) \big(\text{Z}_\text{I} - \mathbb{E}[\text{Z}_\text{I} | \text{S}]\big)\Big] \mathbb{E}[\beta_\text{I} | \text{S}]\Big] \\
=& \mathbb{E}_\text{S}\Big[\text{Cov}(\text{S}_\text{I}(\mathbf{Z}), \text{Z}_\text{I} | \text{S}) \mathbb{E}[\beta_\text{I} | \text{S}]\Big]
\end{aligned}$$

The three remaining covariances have the same structure. We have

$$\beta_I^{TSLS} = \mathbb{E}_\text{S}\Big[\Big(w_\text{I}^* \text{Cov}(\text{S}_\text{I}(\mathbf{Z}), \text{Z}_\text{I} | \text{S}) + w_\text{U}^* \text{Cov}(\text{S}_\text{I}(\mathbf{Z}), \text{Z}_\text{U} | \text{S})\Big) \mathbb{E}[\beta_\text{I} | \text{S}] + \Big(w_\text{I}^* \text{Cov}(\text{S}_\text{U}(\mathbf{Z}), \text{Z}_\text{I} | \text{S}) + w_\text{U}^* \text{Cov}(\text{S}_\text{U}(\mathbf{Z}), \text{Z}_\text{U} | \text{S})\Big) \mathbb{E}[\beta_\text{U} | \text{S}]\Big]$$

or equivalently with our original weights

$$\beta_I^{TSLS} = \mathbb{E}_\text{S}\Big[\Big(w_\text{I} \frac{\text{Cov}(\text{S}_\text{I}(\mathbf{Z}), \text{Z}_\text{I} | \text{S})}{Cov(\text{D}_\text{I}, \text{Z}_\text{I})} + w_\text{U} \frac{\text{Cov}(\text{S}_\text{I}(\mathbf{Z}), \text{Z}_\text{U} | \text{S})}{Cov(\text{D}_\text{U}, \text{Z}_\text{U})}\Big) \mathbb{E}[\beta_\text{I} | \text{S}] + \Big(w_\text{I} \frac{\text{Cov}(\text{S}_\text{U}(\mathbf{Z}), \text{Z}_\text{I} | \text{S})}{Cov(\text{D}_\text{I}, \text{Z}_\text{I})} + w_\text{U} \frac{\text{Cov}(\text{S}_\text{U}(\mathbf{Z}), \text{Z}_\text{U} | \text{S})}{Cov(\text{D}_\text{U}, \text{Z}_\text{U})}\Big) \mathbb{E}[\beta_\text{U} | \text{S}]\Big].$$

A.4.2. NP weights We investigate the conditions that make the first set of weights sum to 1 and the second set sum to 0. The sum for the first set equals

$$\begin{aligned}
&\mathbb{E}_\text{S}\Big[w_\text{I} \frac{\text{Cov}(\text{S}_\text{I}(\mathbf{Z}), \text{Z}_\text{I} | \text{S})}{Cov(\text{D}_\text{I}, \text{Z}_\text{I})} + w_\text{U} \frac{\text{Cov}(\text{S}_\text{I}(\mathbf{Z}), \text{Z}_\text{U} | \text{S})}{Cov(\text{D}_\text{U}, \text{Z}_\text{U})}\Big] \\
&= w_\text{I} + w_\text{U} \mathbb{E}_\text{S}\Big[\frac{\text{Cov}(\text{S}_\text{I}(\mathbf{Z}), \text{Z}_\text{U} | \text{S})}{Cov(\text{D}_\text{U}, \text{Z}_\text{U})}\Big] \\
&= \frac{1}{\Big[1 - \frac{\text{Cov}(\text{D}_\text{I}, \text{Z}_\text{U}) \text{Cov}(\text{D}_\text{U}, \text{Z}_\text{I})}{\text{Cov}(\text{D}_\text{I}, \text{Z}_\text{I}) \text{Cov}(\text{D}_\text{U}, \text{Z}_\text{U})}\Big]} \Big(1 - \frac{\text{Cov}(\text{D}_\text{I}, \text{Z}_\text{U})}{\text{Cov}(\text{D}_\text{I}, \text{Z}_\text{I})} \mathbb{E}_\text{S}\Big[\frac{\text{Cov}(\text{S}_\text{I}(\mathbf{Z}), \text{Z}_\text{U} | \text{S})}{Cov(\text{D}_\text{U}, \text{Z}_\text{U})}\Big]\Big) \\
&= \frac{1}{\Big[1 - \frac{\text{Cov}(\text{D}_\text{I}, \text{Z}_\text{U}) \text{Cov}(\text{D}_\text{U}, \text{Z}_\text{I})}{\text{Cov}(\text{D}_\text{I}, \text{Z}_\text{I}) \text{Cov}(\text{D}_\text{U}, \text{Z}_\text{U})}\Big]} \Big(1 - \frac{\text{Cov}(\text{D}_\text{I}, \text{Z}_\text{U}) \text{Cov}(\text{D}_\text{I}, \text{Z}_\text{U})}{\text{Cov}(\text{D}_\text{I}, \text{Z}_\text{I}) \text{Cov}(\text{D}_\text{U}, \text{Z}_\text{U})}\Big)
\end{aligned}$$

The sum for the second set equals

$$
\begin{aligned}
&w_{\mathrm{I}}\mathbb{E}_{\mathrm{S}}\Big[\frac{\mathrm{Cov}(\mathrm{S_U}(\mathbf{Z}),\mathrm{Z_I}|\mathrm{S})}{Cov(\mathrm{D_I},\mathrm{Z_I})}\Big]+w_{\mathrm{U}}\\
&=\frac{1}{\Big[1-\frac{\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_U})\mathrm{Cov}(\mathrm{D_U},\mathrm{Z_I})}{\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_I})\mathrm{Cov}(\mathrm{D_U},\mathrm{Z_U})}\Big]}\Big(\mathbb{E}_{\mathrm{S}}\Big[\frac{\mathrm{Cov}(\mathrm{S_U}(\mathbf{Z}),\mathrm{Z_I}|\mathrm{S})}{Cov(\mathrm{D_I},\mathrm{Z_I})}\Big]-\frac{\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_U})}{\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_I})}\Big)
\end{aligned}
$$

The first set of weights will sum to 1 and the second weights will sum to 0 if $\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_U}) = \mathrm{Cov}(\mathrm{D_U},\mathrm{Z_I})$.

A.4.3. Proper NP weights The first set of weights are nonnegative if and only if

$$
\Big(w_{\mathrm{I}}\frac{\mathrm{Cov}(\mathrm{S_I}(\mathbf{Z}),\mathrm{Z_I}|\mathrm{S})}{Cov(\mathrm{D_I},\mathrm{Z_I})}+w_{\mathrm{U}}\frac{\mathrm{Cov}(\mathrm{S_I}(\mathbf{Z}),\mathrm{Z_U}|\mathrm{S})}{Cov(\mathrm{D_U},\mathrm{Z_U})}\Big)\geq 0 \quad \text{for all} \quad \mathrm{S}.
$$

This is equivalent to

$$
det(\mathrm{Cov}(\mathbf{D},\mathbf{Z}))^{-1}\frac{\mathrm{Cov}(\mathrm{S_I}(\mathbf{Z}),\mathrm{Z_I}|\mathrm{S})}{\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_I})}\geq det(\mathrm{Cov}(\mathbf{D},\mathbf{Z}))^{-1}\frac{\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_U})}{\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_I})}\frac{\mathrm{Cov}(\mathrm{S_I}(\mathbf{Z}),\mathrm{Z_U}|\mathrm{S})}{\mathrm{Cov}(\mathrm{D_U},\mathrm{Z_U})}
$$

and then

$$
\mathrm{Cov}(\mathrm{S_I}(\mathbf{Z}),\mathrm{Z_I}|\mathrm{S})\geq\frac{\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_U})}{\mathrm{Cov}(\mathrm{D_U},\mathrm{Z_U})}\mathrm{Cov}(\mathrm{S_I}(\mathbf{Z}),\mathrm{Z_U}|\mathrm{S}).
$$

We let $\tilde{\mathrm{Z}}_{\mathrm{I}} = \mathrm{Z_I} - \frac{\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_U})}{\mathrm{Cov}(\mathrm{Z_U},\mathrm{D_U})}\mathrm{Z_U}$. By symmetry, $\tilde{\mathrm{Z}}_{\mathrm{I}} = \mathrm{Z_I} - \frac{\mathrm{Cov}(\mathrm{Z_I},\mathrm{D_U})}{\mathrm{Cov}(\mathrm{Z_U},\mathrm{D_U})}\mathrm{Z_U}$. This means that $\mathrm{Z_I}$ is residualized by an IV estimator of the predicted $\mathrm{Z_I}$. Simplifying gives

$$
\mathrm{Cov}(\mathrm{S_I}(\mathbf{Z}),\tilde{\mathrm{Z}}_{\mathrm{I}}|\mathrm{S})\geq 0. \tag{15}
$$

From this, and similar calculations as before, we get the average conditional monotonicity assumption.

The second set of weights are all 0 if and only if

$$\left(w_{\mathrm{I}}\frac{\mathrm{Cov}(\mathrm{S_U}(\mathbf{Z}),\mathrm{Z_I}|\mathrm{S})}{\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_I})}+w_{\mathrm{U}}\frac{\mathrm{Cov}(\mathrm{S_U}(\mathbf{Z}),\mathrm{Z_U}|\mathrm{S})}{\mathrm{Cov}(\mathrm{D_U},\mathrm{Z_U})}\right)=0 \quad \text{for all} \quad \mathrm{S}.$$

This is equivalent to

$$det(\mathrm{Cov}(\mathbf{D},\mathbf{Z}))^{-1}\frac{\mathrm{Cov}(\mathrm{S_U}(\mathbf{Z}),\mathrm{Z_I}|\mathrm{S})}{\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_I})}=det(\mathrm{Cov}(\mathbf{D},\mathbf{Z}))^{-1}\frac{\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_U})}{\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_I})}\frac{\mathrm{Cov}(\mathrm{S_U}(\mathbf{Z}),\mathrm{Z_U}|\mathrm{S})}{\mathrm{Cov}(\mathrm{D_U},\mathrm{Z_U})}$$

and then

$$\mathrm{Cov}(\mathrm{S_U}(\mathbf{Z}),\mathrm{Z_I}|\mathrm{S})=\frac{\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_U})}{\mathrm{Cov}(\mathrm{D_U},\mathrm{Z_U})}\mathrm{Cov}(\mathrm{S_U}(\mathbf{Z}),\mathrm{Z_U}|\mathrm{S}). \tag{16}$$

and finally $\mathbb{E}[\tilde{\mathrm{Z}}_{\mathrm{I}}|\mathrm{S_U}(\mathbf{Z})=1,\mathrm{S}]=\mathbb{E}[\tilde{\mathrm{Z}}_{\mathrm{I}}|\mathrm{S_U}(\mathbf{Z})=0,\mathrm{S}]$ for all S. From this, and similar calculations as before, we get the no cross effects assumption.

A.5 Near equivalence across estimation approaches

All three estimation approaches collapse to a proper weighted average (though with different weights) under a testable condition: the absence of cross effects ($\mathrm{Cov}(\mathrm{D_I},\mathrm{Z_U})=\mathrm{Cov}(\mathrm{D_U},\mathrm{Z_I})=0$). Table A.1.1 illustrates this equivalence precisely—when cross effects are zero:

- The expected weights on international migration returns (column 3) equal exactly one across all approaches;
- The expected weights on urban migration returns (column 4) equal exactly zero across all approaches;
- The residualized instrument in the no-pooling approach reduces to the standard instrument: $\tilde{\mathrm{Z}}_{\mathrm{I}}=\mathrm{Z_I}$;

- The response-type-specific weights (columns 1-2) become identical between control pooling and no pooling, and with treatment pooling when using the same instrument ($Z = Z_I$).

This equivalence motivates our empirical strategy in the main text: we construct destination-specific instruments and test directly for the absence of cross effects. When the test is satisfied, we can interpret our estimates as proper weighted averages regardless of which pooling assumption we adopt.

A.6 Tables

Table A.6.1: Distribution of migrants amongst generations for those born in Turkey.

Generation	Non-Migrant	Migrant	Total
GEN1	339	1,307	1,646
GEN2	4,142	3,170	7,312
GEN3	5,667	1,416	7,083
GEN4	122	25	147
Total	10,270	5,918	16,188

[1] Non-migrant is defined as people who do not have international migration experience. Migrant is defined as people who lived more than 5 years in Europe.

Table A.6.2: Selection into domestic and international migration.

	GEN1		GEN2		GEN3	
	Urban	International	Urban	International	Urban	International
	(1)	(2)	(3)	(4)	(5)	(6)
Female			0.037	-0.101***	0.024	-0.028
			(0.023)	(0.021)	(0.027)	(0.024)
Age	0.000	0.001	-0.002	0.000	0.010***	0.018***
	(0.003)	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)
Rank	-0.018*	0.004	0.001	-0.013**	-0.009	-0.017
	(0.009)	(0.006)	(0.007)	(0.007)	(0.012)	(0.010)
Ancestral home town						
Acıpayam	-0.610**	-0.168***	-0.388***	-0.163***	-0.152***	-0.175***
	(0.277)	(0.029)	(0.042)	(0.043)	(0.051)	(0.050)
Akçaabat	-0.566**	-0.169***	-0.407***	-0.274***	-0.181***	-0.138***
	(0.278)	(0.031)	(0.042)	(0.041)	(0.048)	(0.051)
Emirdağ	-0.595**	-0.144***	-0.190***	0.168***	-0.047	0.045
	(0.278)	(0.029)	(0.062)	(0.044)	(0.071)	(0.075)
Kulu	-0.511*	-0.123***	-0.328***	0.021	-0.154***	0.209***
	(0.283)	(0.032)	(0.046)	(0.043)	(0.054)	(0.061)
Intercept	0.669*	0.903***	0.611***	0.663***	0.178**	-0.137*
	(0.378)	(0.168)	(0.085)	(0.087)	(0.082)	(0.077)
Observations	167	962	1323	2031	1185	1111

[1] Table reports estimates of regressions of the migration decision on exogenous covariates.
[2] Unit of observation is the individual. Odd numbered columns compare urban to rural non-migrants. Even numbered columns compare international to rural non-migrants.
[3] Rank refers to sibling age rank within the immediate family. The oldest sibling is assigned a rank of 1.
[4] Şarkışla is the base group for ancestral home town.
[5] Standard errors are in parentheses and clustered on the family line. ***, **, * denote statistical significance at the 1, 5, and 10 percent levels.

Table A.6.3: Immediate families with and without migrants.

	Non-switcher families			Switcher families			
	mean	sd	count	mean	sd	count	
	(1)	(2)	(3)	(4)	(5)	(6)	(4) - (1)
education	3.889	2.598	1657	4.943	2.680	1460	1.054***
age	51.114	19.978	1863	36.559	11.063	1545	-14.555***
female	0.275	0.447	1863	0.452	0.498	1545	0.177***
kids	4.541	2.074	1097	4.540	1.863	1545	-0.001

[1] This table considers the external validity of within immediate family comparisons. It compares observables of potential migrants from immediate families with no migrants (switchers) and at least one migrant.

[2] Sample restricted to main estimation sample used in IV analysis.

[3] Education levels are 1 - primary school dropout, 2 - primary school, 3 - extended primary, 4 - lower secondary, general, 5 - lower secondary, vocational, 6 - higher/post-secondary, general, 7 - higher/post-secondary, vocational, 8 - Lower tertiary, vocational, 9 - Lower tertiary, general (BA), 10 - Higher tertiary (Masters), 11 - Post-tertiary (PhD).

[4] Standard errors are in parentheses and clustered on the family line. ***, **, * denote statistical significance at the 1, 5, and 10 percent levels.

Table A.6.4: Robustness of IV estimates.

	Dependent variable = household income					
	(1)	(2)	(3)	(4)	(5)	(6)
Urban	1.141***	1.060***	1.177***	0.991***	1.101***	0.856**
	(0.281)	(0.295)	(0.335)	(0.282)	(0.309)	(0.395)
International	0.028	0.069	-0.116	0.044	0.027	-0.016
	(0.087)	(0.122)	(0.141)	(0.125)	(0.130)	(0.282)
controls	N	Y	Y	Y	Y	Y
age, age^2	N	Y	N	N	N	N
age deciles	N	N	Y	Y	Y	Y
gender	N	N	Y	N	N	Y
eldest sibling	N	N	Y	N	N	Y
family line	N	N	N	N	N	Y
generation	N	N	N	N	N	Y
other restrictions						
exclude return migrants	N	N	N	Y	N	N
income measure	self-report	self-report	self-report	self report	ABE[2012] imputation	self-report
Observations	3476	3408	3408	2749	3407	2866

[1] Table reports IV estimates when income is the dependent variable.
[2] Unit of observation is the individual.
[3] The base group for urban and international migration is rural non-migrants.
[4] ABE[2012] references Abramitzky et al. [2012]. Columns 4 and 5 follows Abramitzky et al. [2012] in using imputed income based on occupation.
[5] Standard errors are in parentheses and clustered on family line. ***, **, * denote statistical significance at the 1, 5, and 10 percent levels.

Table A.6.5: IV estimates by pooling option with coarser cells.

	Household income (in logs)					
	TP	CP	CP	TP	TP	NP
	(1)	(2)	(3)	(4)	(5)	(6)
Out Migration	0.197*			1.030***	-0.063	
	(0.112)			(0.273)	(0.137)	
Urban		1.073***				1.071***
		(0.304)				(0.300)
International			-0.057			0.042
			(0.124)			(0.129)
Leave-family-out Urban IV	yes	yes	no	yes	no	yes
Leave-family-out International IV	yes	no	yes	no	yes	yes
Observations	3408	3408	3408	3408	3408	3408

[1] Table reports second stage estimates of the effects of migration decisions on household income. All specifications include age decile dummies. Cells are defined by gender, ancestral home town, and generation.

[2] TP references treatment pooling. CP references control pooling. NP references no pooling.

[3] Unit of observation is the individual.

[4] The base group is rural non-migrants.

[5] Standard errors are in parentheses and clustered on the family line. ***, **, * denote statistical significance at the 1, 5, and 10 percent levels.

Table A.6.6: IV estimates by pooling option with finer cells.

	Household income (in logs)					
	TP	CP	CP	TP	TP	NP
	(1)	(2)	(3)	(4)	(5)	(6)
Out Migration	0.169			0.999***	-0.069	
	(0.114)			(0.290)	(0.139)	
Urban		1.054***				1.053***
		(0.333)				(0.331)
International			-0.063			0.017
			(0.127)			(0.133)
Leave-family-out Urban IV	yes	yes	no	yes	no	yes
Leave-family-out International IV	yes	no	yes	no	yes	yes
Observations	3370	3370	3370	3370	3370	3370

[1] Table reports second stage estimates of the effects of migration decisions on household income. All specifications include age decile dummies. Cells are defined by sibling rank within the immediate family (1,2,3,...), gender, ancestral home town, and generation.
[2] TP references treatment pooling. CP references control pooling. NP references no pooling.
[3] Unit of observation is the individual.
[4] The base group is rural non-migrants.
[5] Standard errors are in parentheses and clustered on the family line. ***, **, * denote statistical significance at the 1, 5, and 10 percent levels.

Table A.6.7: First stage estimates for Beegle, De Weerdt, and Dercon [2011].

	Migration choice	
	Inside Kagera	Outside Kagera
	(1)	(2)
Head or spouse of head	-0.253***	0.016
	(0.055)	(0.022)
Age rank within hh × age5-15 at baseline	6.332	13.530***
	(12.431)	(4.877)
Beegle et al. [2011] Covariates	yes	yes
Family fixed effects	yes	yes
F statistics		
Overall strength of excluded instruments		
F(2,2516)	10.44	4.23
p-value	0.00	0.015
Redundancy of excluded instruments		
Sanderson-Windmeijer F(1,2516)	20.89	8.47
p-value	0.00	0.004
Joint significance of second stage effects		
Anderson-Rubin F(2,2056)	3.04	3.04
p-value	0.048	0.048
Observations	3277	3277

[1] Table reports first stage estimates when consumption is the dependent variable.

[2] Unit of observation is the individual.

[3] The base group stays in the village. Inside Kagera migration aggregates migration to nearby villages and migration to more distant places inside Kagera.

[4] Standard errors are in parentheses and clustered on the family. ***, **, * denote statistical significance at the 1, 5, and 10 percent levels.

Table A.6.8: First stage estimates for Beegle, De Weerdt, and Dercon [2011].

	Migration choice	
	Inside Kagera	Outside Kagera
	(1)	(2)
Leave-out Inside Kagera	0.208***	-0.048**
	(0.051)	(0.0199)
Child of household head	-0.066*	-0.038**
	(0.039)	(0.015)
Beegle et al. [2011] Covariates	yes	yes
Family fixed effects	yes	yes
F statistics		
Overall strength of excluded instruments		
F(2,2516)	9.92	6.01
p-value	0.00	0.003
Redundancy of excluded instruments		
Sanderson-Windmeijer F(1,2516)	16.89	10.87
p-value	0.00	0.001
Joint significance of second stage effects		
Anderson-Rubin F(2,2056)	0.03	0.03
p-value	0.97	0.97
Observations	3220	3220

[1] Table reports first stage estimates when consumption is the dependent variable.
[2] Unit of observation is the individual.
[3] The base group stays in the village. Inside Kagera migration aggregates migration to nearby villages and migration to more distant places inside Kagera.
[4] Standard errors are in parentheses and clustered on the family. ***, **, * denote statistical significance at the 1, 5, and 10 percent levels.