NOTE

MIGRATION AND FINANCIAL CONSTRAINTS: EVIDENCE FROM MEXICO

Manuela Angelucci*

Abstract—This paper shows that poor households’ entitlement to an exogenous, temporary, but guaranteed income stream increases Mexican migration to the United States, although this income is mainly consumed. Some households use the entitlement to this income stream as collateral to finance the migration. The new migrations come from previously constrained individuals and households and worsen migrant skills. In sum, financial constraints to international migration are binding for poor Mexicans, some of whom would like to migrate but cannot afford to. As growth and antipoverty and microfinance programs relax financial constraints for the poor, low-skilled Mexican migration to the United States will likely increase.

I. Introduction

This paper argues that Mexican immigrants in the United States—about 12 million in 2011, half of whom are undocumented (Passel, D’Vera Cohn, Gonzalez-Barrera, 2012)—and especially illegal immigrants, would be more numerous and their skill composition worse in the absence of financial constraints for some low-skilled prospective migrants. The idea is that while the net benefits from migration are high for unskilled workers, the costs are also high.1 Since poverty is inversely correlated with skills, financial constraints likely prevent some low-skilled individuals from migrating (Chiquiar & Hanson, 2005; McKenzie & Rapoport, 2010; Belot & Hatton, 2012). Relaxing these constraints for some low-skilled individuals therefore may increase their rate of U.S. migration. The new migrants should be neither the most skilled (who were not previously constrained) nor the least skilled (for whom the constraints are still binding). Rather, they should come from somewhere in the middle of the skill distribution. Relaxing these constraints might thus increase the size and worsen the skill composition of Mexican migrants in the United States.

I test these hypotheses using a census of 506 poor rural villages collected for the evaluation of Oportunidades, Mexico’s flagship antipoverty program. The sampled households are poor and low skilled, a population that likely faces financial constraints. Since the program was initially randomized by village, one can study whether the eligibility to its cash transfers, an exogenous income increase that likely relaxes financial constraints for some of its recipients, increases labor migration to the United States.

I find that U.S. labor migration from eligible individuals in treatment villages increases by about 50%, from 0.7% to 1.1%, a few months after receiving the first transfers. The entitlement to the program transfers, guaranteed for at least two years, enhances some households’ ability to fund migrations through loans. The individuals who start migrating because of this income shock belong to households with no counterfactual U.S. migrants, come from the middle of the local predicted wage distribution, and worsen migrant skills.

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* University of Michigan.

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1 The PPP-adjusted U.S.-Mexico wage ratio for an unskilled individual undertaking the same job varies between 6.57 and 7.49 (Freeman & Oostendorp, 2005; Hoeft & Hofer, 2007) and the costs of hiring a smuggler are a few hundred dollars (MMP134).

These findings suggest that access to credit for the poor and antipoverty programs may actually increase low-skilled migration and that as long as relaxing financial constraints increases the benefits from migration more than it increases its opportunity cost, more people with lower-than-average skills will leave. This migration will most likely be illegal, for lack of visa availability for the low skilled.

II. Data

I use a census of 506 villages collected in September 1997, November 1998, and November 1999 to evaluate Oportunidades, Mexico’s antipoverty conditional cash transfer program. The villages are a random sample of most of the rural localities eligible for Oportunidades (Coady, 2000).

The transfers are nutritional subsidies and scholarships, conditional on children attending third to ninth grade, which do not depend on the migrant status of relatives of eligible children. By financing education, Oportunidades may decrease the incentives to undertake a U.S. migration for some, while by relaxing financial constraints, it may increase these incentives for others. Therefore, the estimate of the short-term effect of Oportunidades on U.S. migration may be a lower bound of its effect through loosened financial constraints only. The program may also increase the medium-term likelihood of migration by favoring the accumulation of human capital (Caponi, 2010; Angelucci, 2012).

To evaluate the program, the transfers were offered only in a random subset of 320 villages for the first eighteen months of the program. The treatment villages received the first transfers in April 1998 and the control villages in November 1999.

My key data are restricted to (a) labor migrations, which are 85% of total international migration in November 1998; (b) households initially classified as eligible, whose average transfer is 200 pesos, 22% of their counterfactual income; and (c) individuals between ages 14 and 40, who undertake 95% of all trips in the data.2 The resulting sample includes approximately 27,000 individuals from about 11,800 eligible households in November 1998.

The 1998 migration data have no individual identifier and cannot be matched with the baseline data. Moreover, the 1997 data do not specify the purpose of the migration, unlike the 1998 data. There is no attrition in the data from September 1997 to November 1998, as all the households in the baseline survey are also present in the November 1998 one. However, the sample sizes vary over time because some people age in and out of the relevant sample.

The key outcome of interest is the net flow (NFT) of labor immigrants to the United States in November 1998. To measure it, I compare the stock of current migrants in 1998 (S98) from treatment and control villages (T and C). This is equivalent to comparing net migration flows in 1998, $S_{98} - S_{97} = S_{79} - S_{71} + NF_{98} - NF_{97} = NF_{98} - NF_{97}$, where the 1997 stock is the same in treatment and control villages, which is shown to be the case in the top panel of table 1, confirming that the randomization “works” (Behrman & Todd, 1999).

2 The findings are unchanged if one uses total U.S. migration rather than U.S. labor migrations. Most of the households whose status was reclassified from ineligible to eligible did not receive the transfers in 1998 because of administrative problems.
Parameter sions, but also loan dummies and quantity. For each outcome, the national average of 18 pesos (Kaestner & Malamud, 2010). The weekly wage does not vary by treatment status and has a median of because they include the wages of would-be migrants. This predicted are predetermined and cannot be affected by the program existence and education men and women. I use September 1997 wages because they are very low in September 1997—0.7% among eligible individuals in village level. OLS estimates. All estimated regressions include the variables listed in section III, except for education (top panel), as migrant education is unknown at baseline.

Table 1.—Estimating the Effect of Oportunidades on U.S. Labor Migration from Eligible Households at Baseline, Follow-Up, and Robustness Checks

<table>
<thead>
<tr>
<th>Testing for Baseline Difference in U.S. Migration (1997)</th>
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<tbody>
<tr>
<td>Dependent variable: Y=1 if U.S. migrant Y=1 if ≥ 1 U.S. migrant</td>
</tr>
<tr>
<td>Unit Individual Household</td>
</tr>
<tr>
<td>Year 1997 1997</td>
</tr>
<tr>
<td>Pooled marriage, education, and labor migration to the United States (1997)</td>
</tr>
<tr>
<td>ATE 0.0018 0.0041 (0.0016) (0.0027)</td>
</tr>
<tr>
<td>Control village mean 0.0049 0.0080</td>
</tr>
<tr>
<td>Observations 26,532 10,981</td>
</tr>
<tr>
<td>Temporary labor migration to the United States (1997)</td>
</tr>
<tr>
<td>ATE −0.0025 −0.0047 (0.0041) (0.0097)</td>
</tr>
<tr>
<td>Control village mean 0.0163 0.0407</td>
</tr>
<tr>
<td>Observations 26,114 10,949</td>
</tr>
<tr>
<td>Dependent variable: Y=1 if U.S. migrant Y=1 if ≥ 1 U.S. migrant</td>
</tr>
<tr>
<td>Unit Individual Household</td>
</tr>
<tr>
<td>Year 1998 1998</td>
</tr>
<tr>
<td>ATE 0.0037 0.0067 (0.0018)** (0.0029)**</td>
</tr>
<tr>
<td>Control village mean 0.0072 0.0126</td>
</tr>
<tr>
<td>Observations 26,946 10,736</td>
</tr>
<tr>
<td>Robustness Checks (1998 and 1999)</td>
</tr>
<tr>
<td>Dependent variable: Y=1 if MX migrant Y=1 if US migrant</td>
</tr>
<tr>
<td>Unit Individual Individual</td>
</tr>
<tr>
<td>Year 1998 1998</td>
</tr>
<tr>
<td>ATE −0.0033 −0.0024 (0.0046) (0.0023)</td>
</tr>
<tr>
<td>Control village mean 0.0393 0.0109</td>
</tr>
<tr>
<td>Observations 26,946 22,166</td>
</tr>
</tbody>
</table>

Statistically significant at *90%, **95%, ***99%. Standard errors (in brackets) are clustered at the village level. OLS estimates. All estimated regressions include the variables listed in section III, except for education (top panel), as migrant education is unknown at baseline.

The rates of international migration in the Oportunidades villages are very low in September 1997—0.7% among eligible individuals in treatment villages, consistent with the hypothesis that the poor cannot easily finance potentially lucrative U.S. migrations.

I use predicted wages as a proxy for skills. I regress weekly wages for employed individuals on education, age, and gender, interacting age and education dummies by a gender dummy. I do not fully saturate the model because there are many empty age-by-education cells for high-education men and women. I use September 1997 wages because they are predetermined and cannot be affected by the program existence and because they include the wages of would-be migrants. This predicted weekly wage does not vary by treatment status and has a median of 165 pesos; its 25th and 75th percentiles are 134 and 187 pesos, and its hourly mean is about 4 pesos (5 pesos at 2000 prices), lower than the national average of 18 pesos (Kaestner & Malamud, 2010).

III. Identification and Estimation of Treatment Effects

Define $Y$ as an outcome of interest—migration in the main regressions, but also loan dummies and quantity. For each outcome, the parameter $\beta_1$ identifies the average effect of being eligible for the Oportunidades transfers $T$ (ATE), identified under the assumptions that the randomization was effective and that the program has no spillover effects in control villages:

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + u_i.$$  \tag{1}

The variables $X_i$ are temporary labor U.S. migrants in the household between September 1996 and 1997 and current U.S. migrants in the household in September 1997; age, gender, and schooling (excluded from regressions at the household level); household head’s age, gender, literacy, and ethnicity; household children who may attend grades 3 to 6 and 7 to 9; household members aged 0 to 7, 8 to 14, 15 to 18, 19 to 21, and 22 and older; ownership or use of irrigated and nonirrigated land; weather shock dummy; household wealth index; dummy for having first-degree relatives in the village and their wealth index and weather shocks; village marginalization index; and region dummies. I cluster the standard errors at the village level.

IV. Do Positive Income Shocks Increase U.S. Migration?

Table 1’s middle panel shows that the November 1998 ATE on U.S. migration is 0.37 percentage points, a statistically significant 50% increase from 0.7% to 1.1%, and thus proportionally large but small in absolute terms. Given that there are 190 U.S. migrants from eligible households in treatment villages, the program caused about 64 new migrations from this group. The treatment also increases the likelihood of having at least one international migrant in the household by 0.7 percentage points, a 53% increase. Given that there are 131 such eligible households in treatment villages, the program caused about 46 more households to have at least one U.S. migrant. The ratio, 46/64, is 72%, indicating that almost three-quarters of the additional migrations occur in households that would have had no U.S. migrant in the absence of the program. This is a lower bound to the true share of migrations from households without migrants in the counterfactual state, as, for example, two household members may undertake a migration together.

The income shock should not increase investments that were previously unconstrained, consistent with the statistically insignificant ATE of −0.0033 for domestic migration, which is much cheaper (bottom panel). These findings are therefore consistent with the financial constraint hypothesis.

Since the exogenous income shock should increase migration only for previously constrained individuals, I also look at the effect of Oportunidades by predicted wage terciles. To do so, I need to account for the transfer’s conditionality: for households that start sending their children to school because of the program, the time allocation and budget might change, affecting migration incentives through additional mechanisms, besides its effect on credit constraints. Therefore, I omit households for which this conditionality likely binds: households with children likely to start seventh grade in the 1997–1998 or 1998–1999 academic years, and thus with a 66% counterfactual enrollment rate (unlike the almost universal enrollment rate for earlier grades).

Table 2 shows that while all the estimated ATEs are positive, only the one for the second tercile is statistically significant and different from the first-tercile ATE. This suggests that the program relaxes financial constraints for people in the middle of the predicted wage distribution. Migration in the second tercile almost triples, as it increases from 0.7% to 0.8%.

V. Understanding the Mechanisms

The new migrations may theoretically be financed by spending the transfer itself, using savings or borrowing. One can rule out that the
transfer is used to pay the migration costs because the transfer is almost entirely consumed (Angelucci & De Giorgi 2009) and, besides, very little money had been transferred by November 1998.

The new U.S. migrations are not financed by savings either, as the buffer stock does not decrease for eligible households and in fact the stock of poultry, a commonly held commodity, increases for eligible households in November 1998 (Angelucci & De Giorgi, 2009). Moreover, if U.S. migration were financed through savings, it would trend upward, as more and more households save enough to finance new trips over time. One would therefore expect migration in November 1999 to be higher in treatment than in control villages. However, the average treatment effect on U.S. migration in November 1999 for eligible households (bottom panel of table 1) is \(-0.02\%) and not statistically significant. Since by that time 50\% to 80\% of eligible households in control villages begin to receive the transfers and there is an understanding that the program will continue, migration may have increased in the control villages too.

If households use neither the transfer nor their savings to finance the new trips, they must increase their debt. The eligible households, whose eligibility is publicly announced, may use their entitlement to the cash transfer to induce lenders—shop owners, informal moneylenders, family or friends, or other people in 75\% of the cases—to make them loans they would not have received otherwise.

To provide further evidence about this mechanism, I use November 1998 loan data, the only wave in which they are available. These data are measured with error and may bias the estimate of the loan ATEs down, given that (a) one person per household provides details about the loans contracted by all current household members in the previous six months, (b) these loans are primarily informal, (c) if the borrower is the migrant, the loan information is missing, as it is asked only about current loans but not the ones already have been repaid, and therefore are not listed by the respondent, and (d) some loans to finance migrations may already have been repaid, and therefore are not listed by the respondent, at the time the data are collected.³

The loan hypothesis is nevertheless consistent with the findings from table 3. Since the program has no significant effect on loans at the extensive margin (column 1), I can compare the average loan of borrowers in treatment and control villages, which significantly increases by 265 pesos (column 2). The increase in loan size is for people in the first

³The informal nature of the loan would make the family responsible for repaying the loan on the migrant's behalf. For 1993 to 1998, in one-third of the cases, the family ends up paying the smuggler fees of illegal migrations undertaken by a single family member (from Mexican Migration Project, 2012, with communities with fewer than 5,000 residents from the same states as my sample).
two predicted wage terciles, with statistically significant ATEs of 330 and 400 pesos, increases of 50% and 57%, respectively, while the ATE for people in the third tercile is neither statistically nor economically significant (columns 3 to 5).

This exercise is slightly incorrect because the data on loans are at the household level. However, by looking at the individual level, I can test whether the increase in loans occurs for people with different levels of predicted wages. The household-level estimate of the ATEs on the likelihood of having a loan is small, −0.0012, and not statistically significant, while the average loan increases by 245 pesos (columns 6 and 7).

The ATEs on the joint likelihood of being a U.S. labor migrant and belonging to a household with loans is 0.12, a statistically significant 300% increase (column 8), while the joint likelihood of being a U.S. labor migrant and not belonging to a household with outstanding debt is 0.25, a statistically insignificant 36% increase (column 9). That is, while most of the U.S. migrations in control villages occur from households without loans, the increase in migration in treatment villages is statistically significant only for migrations from households with loans and proportionally much larger than the increase in migrations from households without loans. Recall, moreover, that the estimate of the ATE for migrants from households with loans is likely downward biased and the one for migrants from households without loans is likely upward biased.

Indeed, the loan size for U.S. labor migrants doubles, increasing from 612 to 1,217 pesos (column 10), while for non-U.S. labor migrants, it increases only by 35%, from 748 to 1,012 pesos (column 11). However, these are likely lower-bound estimates of the respective ATEs because the treatment causes endogenous selection of new migrants, as I show in the next section. An important channel through which Oportunidades seems to affect U.S. migration is the entitlement to the transfer, which enhances some households’ ability to obtain loans.

One can use the loan ATE to compute a lower bound of the amount borrowed per U.S. labor migrant. The program caused 64 new migrations from eligible households in treatment villages. Since the loan ATE is 245 pesos and there are 208 households with outstanding loans in treatment villages, the total increase in borrowing amounts to about 800 pesos per migrant—roughly $80.00. The average monthly transfer is 200 pesos. As such, the increase in borrowing per migrant is approximately equivalent to four months’ worth of transfers, or, since the program is initially guaranteed to last for two years, the increase in borrowing per migrant is about 17% of the total increase in collateral.

From the Mexican Migration Project (2012) sample described above, the median cost of hiring a smuggler for a person crossing alone is $200.00. Compared with the lower-bound estimate of $80.00 per migrant, these numbers suggest that loans cover at least 40% of the crossing costs. Indeed, the migration costs may be paid in installments: a part upfront, another part on safe delivery, and the remaining part while the migrants work, consistent with some of the evidence documented by López Castro (1998).

VI. Financial Constraints and Self-Selection

Figure 1 shows the predicted wage distribution for nonmigrants from control villages, U.S. migrants from control villages, and U.S. migrants from treatment villages. In control villages, the migrant predicted wage distribution (dashed gray line) is shifted to the right, has more mass around its middle, and a thinner left tail than the nonmigrant distribution (solid gray line), consistent with positive self-selection of U.S. migrants in these villages. However, the skill distribution is shifted to the left for migrants from treatment villages (dot-dashed black line) compared to migrants from control villages. That is, when Oportunidades relaxes financial constraints, U.S. migrants become less positively self-selected.

VII. Conclusion

I exploit an exogenous income variation occurring in poor rural Mexican villages to test whether financial constraints prevent some unskilled Mexicans from migrating to the United States. After their household becomes entitled to a transfer, some individuals from the middle of the local skill distribution start migrating to the United States. The transfer entitlement, guaranteed for two years, provides some households with access to loans to fund the costly U.S. trip. The new migrants worsen the migrant skill distribution.

As Mexico develops a financial sector that serves the poor, such as microfinance institutions, and as it successfully implements antipoverty programs, U.S. migration will likely increase and the quality of illegal migrants worsen as these policies relax financial constraints for the low skilled.

5 The Mann-Whitney test rejects the null that the two samples are from identical distributions in both cases, with p-values of 0.000 and 0.045. The findings are similar when I consider all village residents.

REFERENCES


