

Migration and financial constraints: Evidence from Mexico*

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Abstract

This paper shows that poor households' entitlement to an exogenous, temporary, but guaranteed income stream increases Mexican migration to the U.S., 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 anti-poverty and micro-finance programs relax financial constraints for the poor, low-skilled Mexican migration to the U.S. will likely increase.

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1 Introduction

This paper argues that Mexican immigrants in the United States - about 12 million in 2011, half of whom are undocumented (Passel *et al.* 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, its costs are also high.¹ Since poverty is inversely correlated with skills, financial constraints likely prevent some low-skilled individuals from migrating (Chiquiar and Hanson 2005, McKenzie and Rapoport 2010, and Belot and 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 anti-poverty 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 which likely relaxes financial constraints for some of its recipients, increases labor migration to the U.S.A..

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,

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

come from the middle of the local predicted wage distribution, and worsen migrant skills.

These findings suggest that access to credit for the poor and anti-poverty 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.

2 Data

I use a census of 506 villages collected in September 1997, November 1998, and November 1999 to evaluate *Oportunidades*, Mexico's anti-poverty 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 3rd to 9th 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, 2006, and Angelucci, 2012).

To evaluate the program, the transfers were offered only in a random subset of 320 villages for the first 18 months of the program existence. The treatment villages receive the first transfers in April 1998 and the control villages in November 1999.

My key data are restricted to (1) labor migrations, which are 85 percent of total international migrations in November 1998, (2) households initially classified as eligible, whose average transfer is 200 *pesos*, 22% of their counterfactual income, and (3) individuals between

ages 14 and 40, who undertake 95% of all trips in the data.^{2,3} 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 (NF_{98}) of labor immigrants to the U.S. in November 1998. To measure it, I compare the stock of current migrants in 1998 (S_{98}) from treatment and control villages (T and C). This is equivalent to comparing net migration flows in 1998, i.e. $S_{98}^T - S_{98}^C = S_{97}^T - S_{97}^C + NF_{98}^T - NF_{98}^C = NF_{98}^T - NF_{98}^C$, if 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 and Todd 1999).

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*

²The findings are unchanged if one uses total U.S. migration rather than U.S. labor migrations.

³Most of the households whose status was reclassified from ineligible to eligible did not receive the transfers in 1998 because of administrative problems.

(Kaestner and Malamud, 2010).

3 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 β_1 identifies the average effect of being eligible for the *Oportunidades* transfers T (henceforth, ATE), identified under the assumptions that the randomization was effective and that the program has no spillover effects in control villages:

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + u_i \quad (1)$$

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, 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 non-irrigated 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; region dummies. I cluster the standard errors at the village level.

4 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 percent, 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 (lower 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 7th grade either 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 0.3% to 0.8%.

5 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 and 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 and 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 (lower 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 also in control villages.

If households neither use 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 ATE downwards, given that (1) one person per household provides details about the loans contracted by all current household members in the previous 6 months, (2) these loans are primarily informal, (3) if the borrower is the migrant himself, the loan information is missing, as it is asked only about *current* household residents, and (4) some loans to finance migrations may already have been repaid, and therefore not listed by the respondent, at the time the data are collected.⁴

⁴The informal nature of the loan would make the family responsible for repaying the loan on the migrant's behalf. In 1993-1998, in one third of the cases the family ends up paying

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 two predicted wage terciles, with statistically significant ATE's of 330 and 400 *pesos*, increases of 50 and 57 percent, while the ATE for people in the third tercile is neither statistically nor economically significant (columns 3 to 5).

The exercise above 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 double, increasing from 612 to 1217 *pesos* (column 10), while for non-US labor migrants it increases only by 35%, from 748 to 1012 *pesos* (column 11). However, these are likely lower bound estimates of the respective ATEs because the treatment causes endogenous selection of new migrants, the smuggler fees of illegal migrations undertaken by a single family member (from Mexican Migration Project (MMP134, 2012) communities with fewer than 5000 residents from the same states as my sample).

as I will show in the next Section. An important channel through which *Oportunidades*, therefore, seems to affect U.S. migration is because the *entitlement* to the transfer 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 USD. The average monthly transfer is 200 *pesos*. As such, the increase in borrowing per migrant is approximately equivalent to 4 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 percent of the total increase in collateral. From the Mexican Migration Project (MMP134) sample described above, the median cost of hiring a smuggler for a person crossing alone is 200 USD.⁵ Compared with the lower-bound estimate of 80 USD per migrant, these numbers suggest that loans cover *at least* 40 percent of the crossing costs. Indeed, the migration costs may be paid in installments - a part upfront, another part upon safe delivery, and the remaining part while the migrants works, consistent with some of the evidence documented by López Castro (1998).

6 Financial constraints and self-selection

Figure 1 shows the predicted wage distribution for non-migrants from control villages, U.S. migrants from control villages, and U.S. migrants from treatment villages.⁶ In control villages, the migrant predicted wage distribution (dashed grey line) is shifted to the right, has more mass around its middle, and a thinner left tail than the non-migrant distribution (solid grey line), consistent with positive self-selection of U.S. migrants in these villages. However,

⁵The median smuggler cost is 600 USD per solo migrant, and only a third of solo migrants hire a smuggler.

⁶The data omit households for which the program may affect U.S. migration in additional ways besides its effect on income and financial constraints.

the skill distribution is shifted to the left for migrants from treatment villages (dot-dashed black line), compared to migrants from control villages.⁷ That is, when *Oportunidades* relaxes financial constraints, U.S. migrants become less positively self-selected.

7 Conclusions

I exploit an exogenous income variation occurring in poor rural Mexican villages to test whether financial constraints prevent some unskilled Mexican 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 U.S.. 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 micro-finance institutions, and as it successfully implements anti-poverty programs, U.S. migration will likely increase and the quality of illegal migrants worsen, as these policies relax financial constraints for the low skilled.

⁷The 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.

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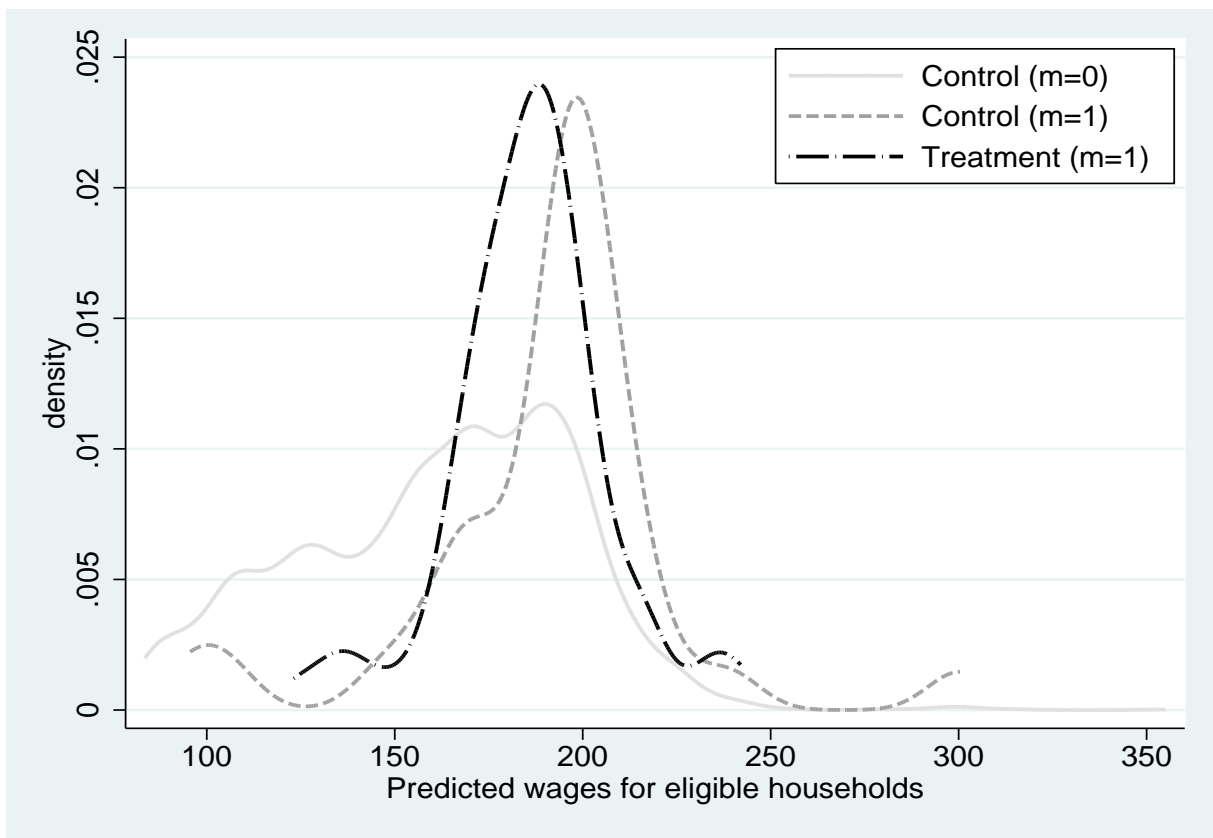


Figure 1: Predicted wage distributions for non-migrants ($m=0$) in Control villages and U.S. migrants ($m=1$) in Treatment and Control villages, November 1998.

Table 1: Estimating the effect of *Oportunidades* on U.S. labor migration from eligible households at baseline (upper panel) and follow-up (middle panel), and robustness checks (lower panel).

<i>Testing for baseline difference in US migration (1997):</i>		
Dependent variable:	Y=1 if US migrant	Y=1 if ≥ 1 US mig. in household
Unit:	Individual	Household
Year :	1997	1997
<i>Pooled marriage, education, and labor migration to the US (1997):</i>		
ATE	0.0018 [0.0016]	0.0041 [0.0027]
Control village mean :	0.0049	0.0080
Observations	26532	10981
<i>Temporary labor migration to the US (1997):</i>		
ATE	-0.0025 [0.0041]	-0.0047 [0.0097]
Control village mean :	0.0163	0.0407
Observations	26114	10949
<i>Estimating the ATE on US labor migration (1998):</i>		
Dependent variable:	Y=1 if US migrant	Y=1 if ≥ 1 US mig. in household
Unit:	Individual	Household
Year :	1998	1998
ATE	0.0037 [0.0018]**	0.0067 [0.0029]**
Control village mean :	0.0072	0.0126
Observations	26946	10736
<i>Robustness checks (1998 and 1999):</i>		
Dependent variable:	Y=1 if MX migrant	Y=1 if US mig.
Unit:	Individual	Individual
Year :	1998	1999
ATE	-0.0033 [0.0046]	-0.0024 [0.0023]
Control village mean:	0.0393	0.0109
Observations	26946	22166

*, **, *** = statistically significant at the 90, 95, and 99 percent confidence level.
Standard errors [in brackets] clustered at the village level. OLS estimates. All estimated regressions include the variables listed in Section 3, except for education in the upper panel, as migrant education is unknown at baseline.

Table 2: Differential effect of *Oportunidades* on U.S. labor migration by predicted wage (\hat{w}) excluding households with conditional transfers, November 1998.

Dependent variable:	Y=1 if US migrant		
Unit:	Individual		
	1 st \hat{w} tercile	2 nd \hat{w} tercile	3 rd \hat{w} tercile
ATE	0.0010	0.0051	0.0024
	[0.0014]	[0.0022]**	[0.0047]
Control village mean:	0.0015	0.0030	0.0119
Observations	15435		

*, **, *** = statistically significant at the 90, 95, and 99 percent confidence level.

Standard errors [in brackets] clustered at the village level. OLS estimates. Eligible households with members aged 10 to 16 who completed 5th or 6th grade in June 1997 are omitted from the sample, because they are the group most likely to be entitled to *de facto* conditional transfers, which might affect the incentives to migrate. All the estimated regressions include the variables listed in Section 3 except for age, gender, and schooling, used to predict wages.

Table 3: effect of *Oportunidades* on loans for eligible households, November 1998

Dependent variable:	$Y=1$ if loan in last 6 mos. Individual	$Y=$ loan amount Individual	$Y=$ loan amount Individual	$Y=$ loan amount Individual	$Y=$ loan amount Individual	$Y=1$ if loan in last 6 mos. Household	$Y=$ loan amount Household	$Y=1$ if loan & US migrant Individual	$Y=1$ if no loan & US migrant Individual
Unit:	1	2	3	4	5	6	7	8	9
By:			1 st \hat{w} tercile	2 nd \hat{w} tercile	3 rd \hat{w} tercile				
ATE	-0.0006 [0.0054]	265 [109]**	330 [141]**	400 [146]***	95 [143]	-0.0012 [0.0046]	245 [96]**	0.0012 [0.0006]*	0.0025 [0.0016]
Control mean	0.0310	746	665	697	875	0.0293	727	0.0004	0.0068
Obs	26943	763	261	251	251	10735	284	26946	26946

*, **, *** = statistically significant at the 90, 95, and 99 percent confidence level. Standard errors [in brackets] clustered at the village level. OLS estimates. These estimates are obtained trimming the top and bottom percentiles of the loan quantity variable. The ATE's on the likelihood of having outstanding loans by predicted wage terciles are 0.0001 (se=0.0064), 0.0005 (0.0061), and -0.0023 (0.0063). Columns 7-11 consider only individuals aged 14 to 40 only, or households with members in this age interval. All estimated regressions include the variables listed in Section 3, except the one in column 10, which has too few observations. The differences in loan sizes conditional on migration in columns 10 and 11 are likely biased estimates of the ATE because the treatment causes a potentially endogenous self-selection into migration.