# Aid programs' *unintended* effects: the case of Progresa and migration

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July 2005

**Abstract**

This paper analyzes the effect of aid on international and domestic migration and explores the causal effect of income on migration. The theoretical model predicts that the effect of aid on migration is ambiguous, depending on both the size and type of transfers. For some household types, e.g. those that are credit constrained, conditional transfers, where the potential recipient has to comply with some requirement in order to qualify for eligibility, may decrease contemporaneous migration. In contrast, unconditional grants are expected to increase the level of migration. Randomized data from a Mexican development program, Progresa, are used to test these hypotheses. The empirical analysis verifies that unconditional transfers increase current migration, while conditional transfers reduce it, at least for some households. Overall, the program generates an increase in international migration but no change in domestic migration.

*I am grateful to Dan Ackerberg, Gian Luigi Albano, Manuel Arellano, Orazio Attanasio, Marina Capparucci, Maitreesh Gathak, Kei Hirano, Costas Meghir, Adam Szeidl and seminar participants at UCLA, Stanford University, 2005 SOLE/EALE Meeting, University College London, University of Rome "La Sapienza" and University of Arizona for their constructive and helpful comments. The financial support of the University of Rome "La Sapienza" is gratefully acknowledged. The usual disclaimer applies. An earlier version of the paper circulated with the title "Aid and Migration: an analysis of the impact of Progresa on the timing and size of labor migration".

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## 1 Introduction

The evaluation of aid programs in developing countries has been the objective of numerous recent papers, because of both the importance of the issue and improvements in data quality. However, the focus of existing analyses has been primarily on the intended program effects, e.g. education, health, consumption, employment and the like. In contrast, this paper studies an *unintended* effect of aid programs - the impact on migration of individuals from indigent households. Unintended effects such as this may arise because aid programs can change recipients' income and time allocation, impact human and physical capital endowments, relax credit constraints, or insure against risk. In addition to quantifying this unintended effect of aid, I estimate the causal effect of income on migration for poor individuals. This is done by exploiting the randomization used in the Mexican development program Progresa.

The relationship between aid and migration is complex. On one hand, transfers are likely to improve home conditions, lowering the incentives to leave. On the other hand, the program may cause migration to increase. This may occur, for example, by loosening existing credit constraints and permitting households to fund additional trips, or by reducing relative income variance, thus enabling households to undertake riskier activities, such as an international trip.

Importantly, the type of transfer provided may affect migration level and timing differently. One important distinction among grant types is whether they are conditional or not. I define conditional grants all transfers whose receipt entails complying with specific requirements. In Progresa, one such grant is a secondary school scholarship. Unconditional grants are instead all transfers that do not constraint the recipients behavior. For instance, suppose a household cannot fund its desired number of migrations because of credit constraints. An unconditional transfer would likely cause an increase in contemporaneous migration in this case. Conversely, a conditional transfer that targets potential migrants and requires their presence at home may reduce current migration (although some individuals may simply postpone the migration until after the end of the program). A program that provides additional non-monetary benefits, such as increases in human capital endowments, may also affect the decision of where, as well as whether to migrate. These cases will be considered in greater detail in Section 2.

Regarding the impact of income on migration, Faini and Venturini (1993) suggest that credit constraints may generate an inverse u-shaped relationship. This pattern is observed empirically by, among others, Faini and Venturini (1993, 1994), who examine intra-European migration using cross-country aggregate data, and by Massey (1988) and Hatton and Williamson (1998, 2002), who provide historical evidence from immigration to the U.S. in the nineteenth century. I add to this mainly descriptive literature by studying the causal effect of income on migration exploiting the exogenous income variation in the data.

In the last few years a specific type of aid policy has been adopted by a number of Latin American countries. This program, called Progresa in Mexico, is aimed at improving education, health and nutrition of poor households. Among the various components of the program are both unconditional and conditional cash transfers. The latter can involve scholarships conditional on attendance at school. Some eligible school children are potential migrants.

Using data collected during its first two years of implementation, I study the impact of Progresa on the level of labor migration. This is defined as the act of leaving one's hometown to seek employment elsewhere. I show that the two types of grants have different impacts on migration: in particular, unconditional cash transfers are associated with increased migration, while conditional grants reduce migration levels of direct recipients, and, in some cases, of whole households. Identification of the causal effect of Progresa on migration hinges on the exogenous variation introduced by village-level randomization, according to which one third of the surveyed villages were delayed program access for the first three years of implementation.

The structure of the paper is the following. Section 2 studies the relationship between migration and aid by sketching a model of migration and schooling choice. Section 3 describes the data used in the empirical analysis. Section 4 describes the specifications used and discusses identification of the parameters of interest. Section 5 presents the results. Section 6 discusses some policy implications and concludes.

## 2 Aid and migration: theoretical considerations

In this section I illustrate the effect of aid, in the form of both conditional and unconditional transfers, on migration level and timing using a model of schooling and migration decisions. Importantly, I define unconditional grants as all transfers that do not entail a change in a recipient's behavior, i.e. if receipt of the transfer is either completely unconditional or if it is associated with conditions that are not binding. For instance, I consider a scholarship to a child who would have attended school irrespective of the program as unconditional. Conversely, the constraints are *de facto* binding (and hence the grant conditional) for all children who attend school because of the program. I define the income change caused by the program as an "income effect", and the move into education of potential migrants who would have not attended school otherwise as a "participation effect". Obviously, both unconditional and conditional grants have an income effect. However, according to the above definition, only conditional grants have a participation effect, as the recipient's time allocation changes to comply with the program rules. I will show below how the income and participation effects have opposite implications for migration.

## 2.1 Effect of an unconditional transfer

The effect of an unconditional transfer can be analyzed in a static framework. Assume that there is a continuum of poor households whose utility ($u$) depends positively on consumption. Households' pooled resources are labelled $Y \geq 0$ (income) and are continuously distributed with some density function $f(Y)$. Suppose as a starting point that the only source of heterogeneity across households is $Y$. Household members include one potential migrant and a number of remaining family members, who earn Y. There are two locations, "home" and "away". The potential migrant may choose between working in the home village ($m = 0$) and earning a wage $w^h$, or migrating ($m = 1$) and earning a wage $w^a > w^h$.

Migration has an exogenous monetary cost $K$ (such as travel expenses). I introduce financial constraints by ruling out saving and borrowing. Households consume all income in the current period.[1] I further assume that the migration cost is borne up-front, at the beginning of the period. This allows credit constraints to influence migration decisions, as only households with sufficiently high income ($Y \geq K$) are able to finance the migration.[2] I also assume for simplicity that utility is a linear function of the parameters of the model.

The maximization problem is

$$\max_{m=\{0,1\}} u = Y + (1-m)w^h + m(w^a - K)$$

For $w^a > w^h + K$, all households potentially benefit monetarily from the migration, but only those with income such that $Y \geq K$ will undertake it. Very poor families (with income $Y < K$) are unable to finance the trip because of credit constraints.[3]

[1] Note that ruling savings out may be an unrealistic assumption. However, in practice one does not expect these households to save much because they are indigent.

[2] There are alternatives to credit constraints that can generate similar predictions to the ones discussed below.

[3] This is a note for the referees that will be dropped from the final version of the paper. In an earlier draft of the paper I introduced an additional intangible migration cost defined by $(Y + w^h)/\alpha$, with $\alpha > 1$. This intangible component can be thought of as individuals suffering from being (or consuming) apart. This assumed that the wealthier the household, the larger is the intangible cost of being away. $1/\alpha$ measures the comparative dislike for "away" consumption, or for not consuming together at home. The smaller the fraction, i.e. the larger the $\alpha$, the smaller is the disutility from consuming away from home. When $\alpha \to \infty$, household members are indifferent between the location where consumption occurs. I considered this additional cost to be consistent with the migration literature that uses this "home bias" as a way to explain the observed low migration levels, despite the high income differentials between developed and developing countries. The implication of considering this additional cost is that the effect of the program's unconditional transfer on migration would be ambiguous. This occurs because the income increase enables some credit-constrained families to finance the migration. These are families with income $Y \in [K - T; K)$. At the same time, the transfer increases the intangible migration costs, $K + \frac{(Y+w^h)}{\alpha} < K + \frac{(Y+w^h)+T}{\alpha}$. As a result, households with income $Y \in [(w^a - w^h - K)\alpha - w^h - T; (w^a - w^h - K)\alpha - w^h)$ stop migrating. I later decided to drop this intangible migration cost because the aid programs I am

In this case, an unconditional transfer ($T$) increases household income, relaxing the financial constraints of some of the families for which migration was precluded in the baseline case. Hence, some previously credit-constrained households become able to finance the migration. These are families with income $Y \in [K - T; K)$.

### 2.2 Effect of a conditional transfer

I now proceed to illustrate the effect of a conditional transfer, e.g. a scholarship to an individual who chooses secondary education because of the program. Unlike unconditional transfers, conditional grants have both income and participation effects. Treated households receive money (income effect), and if the grant results into an income increase, they may use it to fund additional migrations. This is the same effect that occurs for unconditional grant. However, now some individuals cannot leave, because they have to attend school to qualify for the transfer (participation effect). If the students are potential migrants, contemporaneous household migration may decrease.

Since I already discussed the consequences of the income effect on migration in the previous subsection, I will first illustrate the impact of the participation effect separately by modelling the case where the the household has only one potential migrant, who is also the conditional transfer recipient. The grant does change the household income but this income cannot be used to finance new migrations because the potential migrant must stay in the home village (and go to school) in order to qualify for the program. I will then relax this assumption, considering the likely effect of unconditional transfers on households with multiple potential migrants, when not all of them are direct program recipients.

#### 2.2.1 The program recipient is the sole potential migrant

I consider the case of households with one potential migrant, who is also the only direct beneficiary of the program, to study how the conditional grant participation effect changes contemporaneous and future migration. The transfer recipient now has to choose whether to work (either at home or migrating) or go to school. The nature of this problem is dynamic, because current education is associated with higher future earnings. I capture the dynamic effects by adding a second period to the model and by modelling explicitly the discrete schooling choice ($s = 0, 1$). As before, first period wages are represented by $w^h$ and $w^a$ depending on whether the individual

considering target poor households. Hence, program recipients are unlikely to be wealthy enough to decide not to have migrants, if they can afford to. In other words, their incomes are never bigger than $(w^a - w^h - K)\alpha - w^h$. While I can easily add this cost back to the model, I think it is not relevant for the current exercise. I would like to know the referees' opinions on this point.

stays at home or migrates. Second period wages will be denoted as $w^h(k)$ and $w^a(k)$ where $k$ represents human capital in period two and depends on schooling choices.

I assume that migration and schooling are not compatible, i.e. individuals who attend school cannot migrate at the same time.[4] School attendance has a positive price, $p$, which represents both its direct cost (e.g. travel expenses) and its opportunity cost (forgone earnings). Given the borrowing constraints, only households whose income exceeds direct education costs ($Y > p - w^h$) are able to finance education. Schooling generates home and away-specific human capital, $k = (k^h, k^a)'$, which will determine second period wages in both locations ($w^h(k)$ and $w^a(k)$).[5] Second period human capital for individuals who do not attend school is normalized to 0, and I assume that for these individuals wages are identical in the two periods, i.e. $w^j(0) = w^j$, for $j = h, a$. I also assume that schooling increases wages, $w^j(k) > w^j(0)$, for $j = h, a$ and for all $k$, and that skilled "away" wages are larger than skilled "home" ones, $w^a(k) > w^h(k)$. Thus, education changes the incentives to migrate both by increasing next period's home and away wages and by raising the intangible cost of migration (through a higher home wage). The strength of these effects depends on both household income and $k$, which I allow to vary across individuals, and I suppose known in advance. The maximization problem in this case is

$$\max_{m_{1,2}=\{0,1\}, s=\{0,1\}} u_1 + u_2$$

$$\begin{aligned} u_1 &= Y + (1 - m_1)(w^h - ps) + m_1(w^a - K) \\ u_2 &= Y + (1 - m_2)w^h(k) + m_2[w^a(k) - K\mathbf{1}\{m_1 = 0\}] \end{aligned}$$

where the inter-temporal discount rate is set to zero for simplicity.

Migrants who are away in period one will also do so in period 2. This occurs because once the migration cost has been borne, consumption is maximized by staying away for as long as possible. This implies that there is a positive migration cost $K$ in period 2 only for is borne in period 2 only if the individual was not a migrant in period 1, as shown by the indicator function $\mathbf{1}\{m_1 = 0\}$.

There are four possible outcomes, since schooling and contemporaneous migration are not possible, depending on whether one migrates or not in either period and on the schooling choice in period one. I represent the various outcomes with the triplet $(m_1, m_2, s)$, where each parameter takes two possible values (0 or 1). Hence, the triplet $(0, 1, 1)$ represents a household where the son goes to school in the first period and migrates in the second one. The utility from each of

[4] I make this assumption because in Progresa the individual has to attend at least 85% of classes to qualify for the transfer, as I discuss below.

[5] One possible example of location-specific human capital $k^a$ is English fluency. This is likely to influence U.S. wages but not the salary that the prospective migrant would earn by staying in the village.

these choices depends on a particular household's levels of $Y$ and $k$. Hence, different outcomes are chosen according to these values.

To restrict the set of possible inter-temporal choices, I further assume that: first, the cost of education, $p$, is lower than the migration cost, $K$. This implies that households whose income is $Y \in [p - w^h; K)$ may have their child attend school but not migrate. Second, income $Y$ is constant over time. Combined with the zero-saving assumption, this means that individuals who cannot afford to migrate in period 1 will not be able to do it in period 2 either. These relationships are summarized in Table 1, and discussed below.

Table 1: Inter-temporal schooling and migration decisions

| | (1) $Y < p - w^h$ | (2) $Y \in [p - w^h, K)$ | (3) $Y \geq K$ |
|---|---|---|---|
| Low $k$ | $(0,0,0)$ | $(0,0,0)$ | $(1,1,0)$ |
| High enough $k^h$ | $(0,0,0)$ | $(0,0,1)$ | $(0,0,1)$ |
| High enough $k^a$ | $(0,0,0)$ | $(0,0,0)$ | $(0,1,1)$ |

Individuals from families with $Y < p - w^h$ neither migrate nor go to school (column (1) in Table 1). Those from households with income $Y \in [p - w^h, K)$ cannot migrate, but may go to school, if the benefit is high enough (column 2). This depends on $k$, which determines the level of skilled home wages in period 2, $w^h(k)$. Thus, they will go to school if the wage gain exceeds the cost of schooling: $w^h(k) - w^h > p$. It follows that for households with income $Y \in [p - w^h, K)$ and sufficiently high $k^h$, the outcome $(0,0,1)$ yields a higher utility than $(0,0,0)$.

Individuals from households with income $Y \geq K$ are potential migrants in this model (column 3). Whether they do so or not depends on their level of potential human capital $k$. Given that if one migrates in the first period, she will continue being a migrant also in period 2, there are only three possible outcomes: $(1,1,0)$, $(0,1,1)$ or $(0,0,1)$, according to their potential human capital. Individuals with sufficiently high $k^h$ or $k^a$ go to school in period one and either stay (0,0,1) or leave (0,1,1) in period 2, depending on the relative values of these location specific human capital terms.[6] Note that the outcome $(0,0,0)$ is ruled out for individuals from this

[6]For instance, outcome $(1,1,0)$ is chosen by households with pairs of human capital $k$ and income $Y$ such that

$$2w^a - K - 2(Y + w^h)/\alpha > w^h + w^h(k) - p$$

i.e. the utility associated with $(1,1,0)$ is higher than the one associated with $(0,0,1)$, and

$$2w^a > Y/\alpha + w^a(k) + w^h(2/\alpha + 1)/\alpha - w^h(k)/\alpha - p$$

i.e. migrating in both periods ($(1,1,0)$) entails a higher utility than going to school and migrating in the second group. This occurs because the net benefit of migration is sufficiently high, relative to unskilled "home" wages, that the utility from $(1, 1, 0)$ always exceeds the utility from $(0, 0, 0)$.

Summarizing, two classes of individuals do not migrate in either period. The ones from low income households, $Y < K$, who cannot afford to migrate; and those who could afford to migrate (i.e. $Y \geq K$), but have high enough $k^h$ to make schooling plus "home" skilled wages preferable to migrating in one or two periods.

A conditional scholarship reduces the price of education by the amount $\Delta p$. The main effect of such a subsidy is an increase in recipients' school attendance. Moreover, the schooling subsidy may reduce both contemporaneous and future migration from households with sufficiently high income ($Y \geq K$), provided that the education wage premium either at "home" or "away" is sufficiently high. Contemporaneous migration decreases because the lower cost of education induces some individuals to move from migration to education. Thus, for households with income $Y \geq K$, the subsidy is associated with a reduction in the number of $(1, 1, 0)$ choices in favor of $(0, 0, 1)$ and $(0, 1, 1)$ outcomes - i.e. a move from row 1 to rows 2 and 3 in column (3). Future migration either does not change or decreases, depending on location-specific human capital: individuals with sufficiently high $k^h$ (relative to $k^a$) will now choose not to leave in the second period, i.e. they switch from $(1, 1, 0)$ to $(0, 0, 1)$, whereas students with a relatively large $k^a$ will go to school in the first period and then migrate in the following one to access "away" skilled wages, moving from $(1, 1, 0)$ to $(0, 1, 1)$.[7]

One aspect of my data that is not captured by the model regards migration destination. In the model there is no distinction between domestic and international migration, both are combined into the "away" category. It is possible that the program's conditional grant will change the destination mix of migrants. If individuals can only enter the U.S. illegally, relative net benefits of an international versus a domestic migration are likely lower for educated than uneducated migrants, since it is probable that illegal migrant wages in the U.S. labor market are not sensitive to changes in their education levels. This might increase the relative share of domestic migration. In my empirical work, I examine these destination choices.

period only ($(0, 1, 1)$.

[7]Note for the referees. Adding the intangible migration cost as a positive function of household income would result in the conditional grant causing an increase in migration in period 2 from the wealthiest households. Among these households, the lower price of education can induce children with sufficiently high $k^a$ to attend secondary school in order to migrate in the second period, moving from $(0, 0, 0)$ to $(0, 1, 1)$. However, as already mentioned I do not expect to have this type of households in my data.

### 2.2.2 Relaxing the assumption that the program recipient is the sole potential migrant

While the above theoretical model highlights some effects of conditional aid programs on migration, it is also a fairly stylized representation of reality. In particular, the assumption of only one potential migrant per household is somewhat unrealistic. Allowing for multiple potential migrants would have no effect on the sign of the models' prediction for unconditional grants (since the other potential migrants will also be more likely to migrate). However, it can affect the impact of conditional grants, if the revenues from sending one individual to school are used to finance the migration of another person. This occurs when sending the eligible child to school results in an increase in household income. For example, consider the example of one household with two potential migrants, one of whom is also eligible for a scholarship. Suppose that, without the program, there is neither migration (because the household is too poor, i.e. $Y < K$) nor schooling (because the associated human capital $k^h$ is not high enough, hence $s = 0$). The lower cost of education caused by the program scholarship $\Delta p$ is such that the eligible child goes to school ($s = 1$). If the scholarship is bigger than the opportunity cost($\Delta p > p$) and if the move into education makes household income larger than migration cost, i.e. $Y + \Delta p - p > K > Y$, the second potential migrant will go to work in the "away" location. In this case, the program causes an increase in both schooling and migration.[8]

In the empirical application the conditional scholarship amounts to approximately two thirds of the wage a person of the corresponding age might earn if working in the village of residence (Schultz, 2004). Thus, $\Delta p = \frac{2}{3}w^h$. Given that the cost of schooling $p$ encompasses the opportunity cost, it is likely that $\Delta p < p$ for individuals who switch from work to schooling.[9] However, not all conditional grant recipients move from employment to education. In 1997, only 24% of poor households with potentially eligible children (and 18% of households with potentially eligible secondary school children) have at least one employed child. Hence, there is a substantial fraction of households for which $\Delta p > p$, i.e. the conditional transfer increases household income. If these households have multiple potential migrants, the conditional transfer's income effect may more than offset the participation effect, increasing migration. This migration increase may occur both during and after the program implementation.

The point of this discussion is that making the theoretical model more realistic shows that

[8]Different cases are possible. If the household considered in the above example had a pre-program income $Y \geq K$, one person might migrate irrespective of the program, while the eligible child would go to school because of the program. In this case the grant would have no effect on migration.

[9]Note that for households with $\Delta p < p$, the conditional grant may also reduce the contemporaneous migration of other household members besides the program recipient, since the move into education decreases household income (from $Y + w^h$ to $Y + \frac{5}{3}w^h - p$, with $p > w^h$), but this doesn't change the sign of the hypothesized effect.

the effect of conditional transfers on current and future migration depends on the number of potential migrants and on whether the grant increases total household income, which depends on whether the program recipient would have worked in the absence of the program.

#### 2.2.3 Summary of the likely effects of the program on migration

The effect of aid programs that provide financial assistance through both conditional and unconditional grants on migration is complex. This is due to various facts. First, the program's different components may provide opposing incentives to migrate, at least to specific household types. Second, the effect of conditional transfers on contemporaneous and future migration varies according to households' poverty level ($Y$), eligible children's potential skilled wages at "home" and "away" ($w^h(k)$ and $w^a(k)$), number of potential migrants, and to whether the grant increases total household income.

While many of these theoretical results are ambiguous, the following hypotheses are likely to hold: first, that unconditional grants result in a net migration increase. Second, that conditional grants have a negative effect on migration, at least for some households (for example, from families where the recipients move from labor to schooling), and for some members (direct program recipients). In addition, I can test whether conditional grants reduce or increase future migration levels of former recipients and of their relatives. To conclude, note that testing for the effect of the unconditional grant on migration, (or the effect of total grant controlling for the participation effect), permits one to assess the impact of an exogenous income change on migration. The remaining part of the paper deals with the estimation of the effect that Progresa and its various components have on migration.

## 3 Progresa: program structure and data characteristics

Progresa, initiated in 1998, is an ongoing program that targets Mexican poor rural households, providing grants to improve education, health, and nutrition. At the end of 1999, Progresa covered approximately 2.6 million families (about 40% of all rural households) in more than 2,000 municipalities, at a cost of approximately 0.2% of Mexico's GDP. Its main monetary components include nutritional subsidies as well as scholarships for children attending the last four grades of primary school or the first three grades of secondary school.

Women are the transfer recipients in Progresa. Grants, paid bimonthly, are conditional upon family visits to health centers, women's participation in informal workshops on health and nutrition issues, and verification that children attended classes at least 85% of the time during the previous sixty days. Scholarships are larger for higher school grades and for females attending secondary school. For instance, a boy or girl attending sixth grade received 270 (330) *pesos* every two months in November 1998 (1999). In ninth grade, the boy received 440 (530) *pesos*; the girl 530 (610). One dollar was roughly 10 *pesos* in 1998 and 1999. Table 10 in the Appendix provides a detailed breakdown of transfer size by school grade and gender.

Variation in grant size by household depends entirely on the number, grade, and gender of eligible children. However, households with different numbers of children, genders, or school grades may end up at the same grant level. For instance, in 1998 a household with one child in third grade, two in fourth grade, and two in fifth grades received 860 *pesos*, the same as a household with one boy in eighth grade and one in ninth grade. Childless recipient households receive the nutritional subsidy only, which amounted to 200 (250) *pesos* bimonthly in November 1998 (1999). The maximum bimonthly subsidy to a family was capped at 1250 (1500) *pesos*. I provide further details on grant size in Section 4.

The Progresa data contain information on households from 506 marginalized rural villages both before the program starts (in May 1998) and during the first 18 months of its implementation. An interesting feature of the data set is that 186 villages were randomized out. Poor residents of these villages were not administered the program until 2000. Households are classified into poor and non-poor according to the information collected in the pre-program September 1997 census of Progresa localities. All residents of both control and treatment villages were then interviewed every six months. Information on migration was collected in the September 1997, October/November 1998 and November 1999 waves, i.e. rounds 1, 3 and 5 of the survey.

There were two rounds of selection of eligible households in Progresa. 52 percent of households were initially classified as poor in 1997.[10] The following year, a further group of households, initially classified as non-poor, were added into the beneficiary group. However, most of this latter set of families did not receive the transfers for other exogenous reasons (administrative problems), irrespective of their compliance with the eligibility rules. Because of this, I restrict my sample to the households initially classified as poor. As such, my estimates of the effects of Progresa may only be valid for this group. [11]

Given my question of study, a key component of my data is that on migration. As extensively documented by the literature, Mexican migration, especially to the United States, tends to be temporary rather than permanent. Household resources are pooled to finance the migration of

[10]The poverty measure was created using a discriminant analysis, i.e. computing discriminant scores from a combination of wealth- and income-related variables that explain most of the variation in per-capita income. These scores are continuous and have region-specific cutoff points that separate the poor from the non-poor.

[11]It would be possible to estimate the effect of Progresa on the migration rate of the omitted group (or at least those who actually received the transfers). However, such an approach is not pursued here because of the small sample size of the relevant group and the very few observed migrations.

one or more of its members, who leave their family to spend time away. These migrants typically remit money at regular intervals or bring back their savings with them. Remittances are often a sizeable proportion of the non-migrating relatives' incomes.

In my empirical work, I consider only labor migrations. This is possible because both the 1998 and the 1999 surveys (unlike 1997) record the motivations for leaving one's household of origin, identifying whether people leave to, e.g., work, study or get married. I am also able to distinguish between domestic and international migration. I will refer to international or U.S. migration interchangeably, since the U.S. is the most likely destination for international migrants.

95 percent of all trips in the data are undertaken by individuals between ages 14 and 40. Thus, I only consider the subset of people within this age interval as potential migrants, discarding older and younger individuals. The resulting sample contains approximately 27,000 individuals from 10,000 households. About 17,000 of these individuals (7,000 households) belong to the 320 treated communities, as shown in Table 2.

Table 2: Sample size by group

| | 1997 | 1998 | | 1999 | |
|---|---|---|---|---|---|
| | All | B | NC | B | NC |
| Treatment | | | | | |
| Individuals | 16702 | 16,562 | 621 | 15,930 | 502 |
| (Percentage) | | (96.4) | (3.6) | (96.9) | (3.1) |
| Households | 7193 | 6623 | 266 | 6138 | 199 |
| (Percentage) | | (96.1) | (3.9) | (96.8) | (3.2) |
| Control | | | | | |
| Individuals | 10023 | 10310 | | 10260 | |
| Households | 4314 | 4097 | | 3911 | |

B=actual beneficiary; NC=non-complier (eligible but not participating)

Note that the table contains separate columns for actual beneficiaries $B$ and non-compliers $NC$. Non-compliers are eligible individuals who do not receive the treatment. This could be due to a number of reasons, but the data do not report why. Fortunately, this group amounts to only 3 percent of total potential beneficiaries. Thus, the difference between the average treatment effect on the treated and the average intention to treat effect is negligible.[12]

[12]The results from the empirical analysis are robust to the inclusion or omission of non-compliers.

# 4 Econometric analysis

In this section, I define the variables of interest and explain how they are created; discuss the assumptions under which the parameters of interest are identified; and provide indirect evidence that, because of the randomization, cross sectional variation is sufficient to identify such parameters.

## 4.1 Variables of interest: definition and creation

I am interested in the effect of Progresa on domestic and international labor migration. As already mentioned, 95 percent of observed labor migrations are undertaken by individuals aged between 14 and 40 (I assume that younger children would not work). Hence, I restrict the valid sample to individuals (or households with at least one member) within these age limits.

I want to assess whether the program changes migration levels. To do so, I compare the stock of current migrants (i.e. the proportion of the population who is away) from treated and control villages. Given the absence of pre-program mean differences in migration levels between control and treatment households (which will be shown later), this is equivalent to comparing net migration flows:

$$\begin{aligned} S_{98}^{T} - S_{98}^{C} &= S_{97}^{T} - S_{97}^{C} + NF_{98}^{T} - NF_{98}^{C} = NF_{98}^{T} - NF_{98}^{C} \\ \text{if} \quad S_{97}^{T} &= S_{97}^{C} \end{aligned}$$

where $S$ and $NF$ indicate migrant stock and net flow, the subscripts refer to the relevant year and the superscripts to treatment and control group[13]. When examining migrant stocks in 1999, I am looking at the difference in net flows in the two years after the pre-program data are collected. I create migration variables at the household level, consistent with the model setup: I have two dummy variables - one for domestic and the other one for international migration - measuring whether the household has at least one migrant.

I am interested in estimating two sets of effects: first, the overall program effect on migration, and second, the separate impacts of conditional and unconditional grants. While I do not observe the actual grant received by beneficiaries, I can use the specifics of Progresa to construct a variable measuring the potential grant size for each household (were all their children to attend school). This obviously depends on a household's demographics. Given this definition, the parameters I can estimate have clear policy relevance, since they measure the impact of what is under the control of the policy maker, rather than depending on households' acceptance of the

[13]Note that this variable captures changes in both inflow and outflow of migrants. Hence, it considers the possibility that some individuals may return from a migration to participate to the program.

treatment. Potential grant size ($g$) varies between 200 and 250 *pesos* (the base income support subsidy that all eligible households receive) and is capped at a maximum of 1250 and 1500 *pesos* (in November 1998 and 1999, respectively). These potential grants were quite large - 200 (1250) pesos represents 18 (48) percent of median 1997 household labor earnings (for households reporting positive earnings). The increases in grant levels between 1998 and 1999 were intended to adjust for inflation.

I now proceed to compare average potential grants with the costs and benefits of migration. Average bimonthly potential grant size for families with children is 659 and 875 (1053 and 1309 for families with secondary school recipients) *pesos*, and the secondary school subsidy varies between 545 and 765 *pesos* in 1998 and 1999. These amounts do not seem sufficient to fund an international trip. This is because in addition to travel expenses to reach the U.S. border, the vast majority of international migrants are illegal and tend to hire smugglers to cross the border: 77 percent of illegal migrants resorted to hiring a smuggler in the 1980s and 1990s, paying on average 540 1990 dollars for the years 1993-1998. This corresponds to roughly 5000 pesos, using the 1998 exchange rate.[14] Thus, if recipients intend to use the grant to fund new migrations, they either have to save for a sufficiently long time, or borrow, or pool resources with other households to finance one single trip. Domestic migrations are clearly cheaper to fund, although also less profitable. One way to measure the value of migration is through remittances. A comparison of average remittance levels and the Progresa grant size may provide an indication of how likely the program is to affect migration patterns. International and domestic migrants remit on average 800 and 160 *pesos* bimonthly, respectively.[15] Secondary school grants vary between 400 and 610 *pesos* every two months.[16] In short, if there are credit constraints they are more likely to be binding for international trips - expensive, but with high returns - than for domestic trips, which are relatively inexpensive to fund. At the same time, the grant size is not sufficient to fully finance new U.S. migrations. Lastly, the incentives to return from a migration to become a program recipient appear to be much larger for domestic than for an international migrant.

Virtually all children in the sample attended primary school before the program implementation. Hence, this subsidy was *de facto* unconditional for the treated households. Pre-program secondary school attendance was much lower, depending to a large extent on low transition rates from primary to secondary school.[17] Thus, the condition for transfer eligibility may have been

[14]Information on smuggler hiring and associated cost has been computed from data collected by the Mexican Migration Project (2001).

[15]Migrant number and monthly remittances have been computed using control group data from the November 1998 survey.

[16]Table 10 in the Appendix presents a detailed breakdown of transfers by school grade.

[17]While re-enrollment rates are quite high for children with some secondary schooling, the likelihood of beginning

binding for households with secondary school children. Given the differences in pre-program enrollment rates, I classify both the income support and primary school grants as unconditional transfers, and the secondary school subsidies as conditional ones.

Given the above, to compute the proportion of a household's potential grant that is conditional, I use the proportion of the potential grant associated with secondary school eligible children. At most 88% of the total potential grant may come from secondary school scholarships because of the income support provided to all eligible families.[18] The main advantage of considering the conditional proportion of the overall potential grant is that it is a continuous variable with a relative high level of variation among households. Moreover, it takes account of the difference in grant for each grade and gender.[19] Its main disadvantage is that it may take the same value for households with different number or type of secondary school recipients (e.g. two households with 50% conditional grant but different level of absolute grant).

I create both a discrete and continuous version of these grant variables, using upper case for categorial variables and lower case for continuous ones. In the former case, I group subjects according to the household potential grant size ($G_i$), and the proportion that is conditional ($P_i$). $G_i$ takes two values: 0 for below average grant sizes ($g_i$ <725 and 875 *pesos* in 1998 and 1999, respectively), and 1 for larger than average transfers. $P_i$ has three levels: 0, for households with all unconditional grant ($p = 0$); 1, for up to 50% conditional grant ($p \leq .5$); 2, for more than 50% conditional grant ($p > .5$).

I also create a dummy, $C_i$, grouping households for which the subsidy cap is binding due to having a large number of eligible children. Such households have fewer incentives to send all their children to school, and more incentives for some migrations. Examining simple means confirms this intuition: mean U.S. migration for treated households - but not for control ones - decreases when capped households are excluded from the computation both in 1998 and 1999. I experimented with a dummy for all "capped" households and one for "capped" households with at least one eligible secondary school child. The effect on the parameters of interest is similar.

high school is low: before the program, less than 40 percent of children aged up to 16 with complete primary school enroll in high school (Attanasio *et al.*, 2002). One reason for the low transition rate from primary to secondary education, which are both free in Mexico, may be the higher distance from high schools, which often do not exist in the village of residence (unlike primary schools, present in most localities). A second reason may be the higher opportunity cost, as forgone earnings are likely to be higher for teenagers than for younger children. This is especially true given that teenagers are potential migrants. In November 1999, 28 percent of all labor migrants from poor households in the control group were aged 14 to 18 at migration.

[18]Further details regarding the creation of these variables and the amount of variation of the two grant variables are provided in the Appendix.

[19]Indeed, scholarship sizes are such that some families with the same total potential grant have a different number and composition of eligible children.

Note that, as with my grant and composition variables, this cap variable changes over time, as the number of potential recipients changes.

### 4.2 Program effect: definition of the parameters of interests and identification

In this section I define the parameters of interest in terms of potential outcomes, and discuss their identification using the Progresa data. I discuss how I estimate these parameters and provide a more intuitive explanation of each estimate in the next section. I am interested in three different sets of parameters that measure the program effect on migration: the overall program impact, the different effects of unconditional and conditional grants, and the impact of exogenous income changes.

Define $m_i^k$ as a variable measuring whether household $i$ has at least one international ($k = US$) or domestic ($k = MX$) migrant, $m(1)_i^k$ as the household's potential migration outcome under Progresa, and $m(0)_i^k$ as the potential outcome in the absence of Progresa. The first parameter of interest is the average causal effect of Progresa on the migration level of treated subjects ($T_i = 1$), i.e. $TTE^k = E[m(1)_i^k|T_i = 1] - E[m(0)_i^k|T_i = 1]$. While the second conditional mean is not observed, the randomization of Progresa solves the missing counterfactual problem because assignment to the treatment is independent of potential outcomes. Hence, I replace the second conditional mean with the expected migration level in the control group. Thus, $TTE^k = E[m(1)_i^k|T_i = 1] - E[m(0)_i^k|T_i = 0]$. I rely on random assignment to solve the missing counterfactual problem also in the estimation of all the remaining parameters of interest, which I discuss below.

While this first parameter already measures whether household migration changes in response to the program, the program is a mix of unconditional and conditional grants, which might have different effects. I am also interested in assessing the impact of conditional and unconditional grants separately. First, I want to test whether contemporaneous migration is a positive function of total potential grant, i.e. whether larger grant sizes have a different impact than smaller ones. Define $m(1)_{iG}^k$ and $m(0)_{iG}^k$ as the potential outcomes with and without treatment for household $i$. This household's demographic composition would entitle it to a grant of size $G$ under the treatment. The average treatment on the treated effects[20] for small ($G = 0$) and large potential

[20]Note that this parameter and the following ones are not actually treatment on the treated effects, in the sense that they do not measure the effect of changing the actual grant size. These parameters in fact measure the effect of changes in the potential grant size. In this sense, they are closer to "intent to treat" parameters. They would be treatment on the treated effects only if all household recipients were sent to school, which may not be the case. I will continue to refer to them as treatment on the treated effects, misusing the term, for lack of a more appropriate one.

grant recipients ($G = 1$) are

$$TTE^k_{G_0} = E[m(1)^k_{iG_0} - m(0)^k_{iG_0} | T_i = 1]$$

and

$$TTE^k_{G_1} = E[m(1)^k_{iG_1} - m(0)^k_{iG_1} | T_i = 1]$$

I want to test the hypothesis that the effect of the program for large grant recipients is bigger than the effect for small grant recipients:

$$\Delta TTE^k_{G_{10}} = TTE^k_{G_1} - TTE^k_{G_0} > 0 \tag{1}$$

Total grants are the sum of conditional and unconditional transfers. Hence, the sign of (1) is positive if the incentives to migrate given by the larger income more than offset the fact that there are fewer individuals available to migrate (because some of them are now going to school).

One way to test whether contemporaneous migration is a positive function of unconditional grants is to repeat the above exercise, conditioning on grant composition. Thus, I can compare the migration rates of households with similar composition ($P$), but different total grants. Define $m(1)^k_{iGP}$ and $m(0)^k_{iGP}$ as the potential outcomes with and without treatment for a household $i$ entitled to a grant of size $G$, and with composition $P$. The average treatment on the treated effect for, e.g., large grant recipients ($G = 1$) and composition level $P = j$ (with $j = 0, 1, 2$) is

$$TTE^k_{G_1 P_j} = E[m(1)^k_{iG_1 P_j} - m(0)^k_{iG_1 P_j} | T_i = 1]$$

I want to test the hypothesis that

$$\Delta TTE^k_{G_{10} P_j} = TTE^k_{G_1 P_j} - TTE^k_{G_0 P_j} > 0 \tag{2}$$

with $j = 0, 1, 2$. For example, suppose I want to compare the program effect on households with a similar grant composition, say up to half of the grant coming from secondary school recipients ($P = 1$), but different total grant size, say larger ($G = 1$) and smaller ($G = 0$) than average. This is what $\Delta TTE^k_{G_{10} P_1}$ measures. If positive, it means that the effect of Progresa on migration is bigger for families with larger grant size, among households where up to half the grant comes from scholarships for grades 7 to 9.

The parameters in (2) test the effect of exogenous income increases on migration. If positive, they mean that higher income causes a higher migration level among recipients. Given that grant composition is held constant, (2) is expected to be higher than (1). This is because the theoretical model predicts that the impact of grant changes on migration keeping grant composition constant (measured by (2)) has a pure income effect, and is larger than the impact of an overall grant change (measured by (1)) which has both income and substitution effects. Thus, a comparison of these two parameters is the first (indirect) test of the impact of unconditional grants.

There are several other parameters that test the impact of unconditional grants on migration. One way to achieve this is to compare households with similar total grant sizes, but different composition (or different conditional grants). For example, consider two households entitled to the same total grant, but one has only primary school recipients, while the other has only secondary school recipients. According to the model, the program impact on migration is expected to be lower in the latter household. Using the above potential outcome notation, this is equivalent to testing the hypothesis that

$$\Delta TTE^k_{P_{jl}G_f} = TTE^k_{P_jG_f} - TTE^k_{P_lG_f} < 0 \quad (3)$$

with $l = 0, 1, 2$, $f = 0, 1$, and $j > l$. The subscripts $j$ and $l$ refer to the three possible pairwise comparisons of the conditional grant proportions ($P$ equal to zero, low and high, as explained above).

(1), (2), and (3) compare migration levels of households with different demographic characteristics (the variation in potential grant size depends entirely on child number, age, gender and school grade). I also estimate continuous versions of these parameters. Define $m(1)^k_{igp}$ and $m(0)^k_{igp}$ as the potential outcomes with and without treatment for a household $i$ with demographics corresponding to grant size $g$ and composition $p$, where $g$ and $p$ are continuous. The average treatment on the treated effect for recipients of average grant size ($g = \bar{g}$) and composition $p = \bar{p}$ is

$$TTE^k_{\bar{g}\bar{p}} = E[m(1)^k_{i\bar{g}\bar{p}} - m(0)^k_{i\bar{g}\bar{p}}|T_i = 1]$$

The parameter

$$\delta TTE^{gk}_{\bar{g}\bar{p}} = \partial TTE^k_{\bar{g}\bar{p}}/\partial g \quad (4)$$

measures the change in the program effect on migration level caused by a marginal increase in the money received by the average treated household. I also estimate analogous parameters for marginal changes in grant composition:

$$\delta TTE^{pk}_{\bar{g}\bar{p}} = \partial TTE^k_{\bar{g}\bar{p}}/\partial p \quad (5)$$

### 4.3 Program effects: estimation

In this section I discuss how I obtain estimates of the parameters of interest presented above. I estimate the first parameter of interest, the overall program effect on migration, by computing average migration levels for the treatment and control group and testing for their statistical difference.

For the remaining parameters, I estimate different versions of the following regression model:

$$
\begin{aligned}
&P(m_i^k = 1) = P(m_i^{k*} > 0) \\
&m_i^{k*} = \alpha^k + \beta_1^k T_i + \beta_2^k G_i + \sum_j \beta_{3j}^k P_{ij} + \beta_4^k G_i T_i + \sum_j \beta_{5j}^k P_{ij} T_i + \gamma^k X_i \\
&+\delta^k C_i P_i + \theta^k C_i P_i T_i + u_i^k \qquad (6) \\
&j = 1, 2
\end{aligned}
$$

The household is the unit of analysis, and $P(m_i^k = 1)$ is the probability that household $i$ has at least one domestic ($k = MX$) or one international ($k = US$) migrant.[21] $m_i^{k*}$ is some latent variable that measures household $i$'s utility from having either domestic or international migrants. $T$ is the treatment dummy (1 for households in treated communities, 0 for households in control communities). I described the variables $G$ and $P$ in the data section. The partial effect of $G$ measures whether the probability of having at least one migrant in the household is different for control group families with different demographic compositions (roughly grouping families with large and small groups of children who may attend school grades 3 to 9, when G is 1 and 0 respectively). For example, if some of these children are potential migrants, then one may expect this effect to be positive. The partial effect of $G \cdot T$ instead measures whether the difference in migration levels from households with large and small numbers of children is bigger for treated than for control households. When the variables $P$ and $T \cdot P$ are excluded from the above regression, the estimated partial effect of $G \cdot T$ provides an unbiased estimate of (1). When the above variables are included, instead, this partial effect is estimating (2).[22] If one suspects that larger grant sizes increase migration, as the model predicts, then one expects these partial effects to be positive and significantly different from zero.

Similar reasoning applies to the interpretation of the partial effects of $P$ and $P \cdot T$. The estimated partial effect of $P \cdot T$ is an estimate of (3). Note that the interpretation of the partial effect of $G \cdot T$ changes according to whether one conditions on $P$ or not. In the first case, one estimates the effect of changing the total grant (both conditional and unconditional grants), keeping the grant composition constant. This is an income effect. In the second case, instead, the change in total grant may have both an income and a participation effect (because the conditional grant is not held constant). Thus, one expect the estimate of the latter to be

[21] I also experimented with dependent variables measuring the number of migrants from each household. However, count data models provided very unstable estimates.

[22] One implicit assumption in (6) is that the partial effects of grant size and composition are independent of each other, for instance $\Delta TTE^k_{G_{10}P_0} = \Delta TTE^k_{G_{10}P_1} = \Delta TTE^k_{G_{10}P}$. I do not interact the $G$ and $P$ variables because I do not want to create too small cells for fear that identification may be driven by comparing the behavior of very few households. I relax this assumption when using the continuous version of potential grant size and composition, $g$ and $p$.

larger than the estimate of the first two effects, if conditional transfers have a lower impact on migration than unconditional transfers.

One advantage of using categorical variables to estimate the program effects is that identification does not hinge on functional form assumptions, besides the assumptions on the distribution of the error term. One is simply comparing average migration levels of various groups of treatment and control households. However, its main shortcoming is that one may end up comparing families with a different demographic composition. For instance, suppose that the estimated partial effect of $G \cdot T$ is positive and significantly different from zero. This means that the effect of the program on migration is larger for families with numerous children who receive a larger than average grant than for families with fewer children who receive a smaller than average grant. I control for the fact that households with different demographic composition may have different migration rates irrespective of the program by conditioning on $G$. However, the program effect on migration may vary for households with different child numbers and ages. For instance, larger families may increase migration after the program more simply because they have more potential migrants, rather than because they receive more money. I address this issue by adding a set of conditioning variables to control for household characteristics that may result in different program effect - the variables ($X_i$) in (6).[23] These additional controls are: number of school children who may attend grades 3 to 9, divided by gender and school type (primary or secondary); number of household members aged 17 to 40, and 41 and older; head of household's gender and age; size of owned land; dummies for whether household suffered from a series of "shocks" during the interview year (ranging from death or illness of a relative to natural disasters); household and village poverty levels; region dummies.[24] All variables are from the 1997 survey, excluding shocks. Ages and school grades are extrapolated from pre-program data.

I also estimate versions of (6) where I replace the discrete $G$ and $P$ with the continuous variables $g$ and $p$:

$$
\begin{aligned}
&P(m_i^k = 1) = P(m_i^{k*} > 0) \\
&m_i^{k*} = \alpha^k + \beta_1^k T_i + \beta_2^k g_i + \beta_3^k p_i + \beta_4^k g_i T_i + \beta_5^k p_i T_i + \gamma^k X_i \\
&+\delta^k C_i p_i + \theta^k C_i p_i T_i + u_i^k \qquad (7) \\
&\qquad (8)
\end{aligned}
$$

In this case, I am comparing the program effect for families with a very similar demographic

[23]A further advantage of adding conditioning variables is the improvement in the precision of the estimates (which may be an issue, given the few observed migrations in the sample).

[24]I also experimented with additional regressors, such as presence of disabled individuals; village migration intensity; type and number of animals owned; state dummies. Including these variables does not change the results and has no sizeable effect on the standard errors.

structure, but slightly different grant size and composition. Thus, the assumption that the estimated partial effects are caused by changing the grant size and composition (and not to different households reacting differently to the same program) is less strong. As before, the partial effects of $g \cdot T$ and $p \cdot T$ provide unbiased estimates of (4) and (5). The set of additional conditioning variables $X$ remains the same. In some specifications I also add the variable $C_i$ - a dummy that measures whether the household's potential transfers are capped. I interact $C$ with measures of conditional grant proportions because I expect the likelihood of migration from capped households to be a positive function of the quantity of potential migrants (some of whom are potential program recipients).

I estimate different versions of equations (6) and (7) separately for 1998 and 1999 data, in order to test the program effects after 6 and 18 months since its implementation. I use household-specific (rather than individual-specific) data to be consistent with the theoretical model and with the existing literature, which suggests that migration decisions occur at the family level. I estimate linear probability, univariate and bivariate probit models, with standard errors clustered at the village level. All estimations provide similar sets of results.[25]

### 4.4 Does the randomization work? Pre-program means

The identification of the parameters of interest is based on cross-sectional variation. Thus, it relies on the validity of the randomization, meaning that there are no unobservable differences in migration patterns among treatment and control groups. Difference-in-difference (DD) estimators cannot be used here because there is no comparable measure of labor migration before and during the program. Hence, I test whether pre-program migration levels differ between treatment and control households, both overall and for subgroups of families with different number and type of school children.

The 1997 data have two pre-program measures of migration. The first variable records trips - both completed and not - undertaken within the previous 12 months. The second variable counts all household members who are away at the time the interview, irrespective of whether they left to work, to get married or to study.[26]

I classify migration according to its location, computing two different variables (domestic and international). For each of these variables, I consider both the proportion of families with at

[25]The point estimates from the linear probability models differ slightly from the probit models.

[26]Suppose that the "true" effect of Progresa is a surge in labor migration and a decrease in other types of migration (to study, to get married). The different effects may offset each other. Hence, if one were to use this variable, DD estimates of the effect of Progresa on labor migration would be downward biased. This may explain the difference between the current results and those by Steckov *et al.* (2003), who find that Progresa reduces international migration.

Table 3: Pre-program mean migration levels

| | Currently away (to work, study, get married) | | Migrated in previous year | |
|---|---|---|---|---|
| | US mig | MX mig | US mig | MX mig |
| Households with at least one migrant | | | | |
| Treatment | .0128 | .0177 | .0313 | .1390 |
| Control | .0083 | .0155 | .0353 | .1248 |
| P-value of mean difference | [.122][b] | [.569] | [.652] | [.400] |
| Migrants per household[a] | | | | |
| Treatment | 1.352 | 1.426 | 1.088 | 1.346 |
| Control | 1.527 | 1.447 | 1.098 | 1.279 |
| P-value of mean difference | [.199] | [.864] | [.790] | [.148] |

[a] average migrant number for households with at least one migrant. [b] p-value of difference increases to .245 when I condition on the set of regressors described above. P-values of differences obtained from regressions of migration variables on treatment dummy. Standard errors clustered at the village level.

least one migrant and the average household migration (for households with a positive number of migrants). Table 3 presents average pre-program migration rates. It shows that there is no statistically significant difference between migration levels of treatment and control households. This is consistent with the hypothesis that, because of the village randomization, there is no systematic difference in the migration of households from treated and control localities.

Table 3 also shows that the number of individuals who are away from the household in 1997 is extremely low. About one percent of the sampled households have at least one member who is in the United States, the proportion rising only slightly for individuals who left the household to move elsewhere within Mexico. Within these families, approximately 1.5 individuals are away on average. The second migration variable shows that some 3% of sampled households had one international migrant in the last 12 months, while about 13% of them had at least one domestic migration. Note also that while there is typically only one international migrant per household (the average is one), some households had more than one domestic migrant (the average is 1.3). The lower rate for international than for domestic migration is consistent with the financial constraint hypothesis: though more profitable than domestic trips, some U.S. migrations may not have been funded because of the impossibility to borrow.

Tables 11 and 12 in the Appendix test for different migration rates among households with different demographic characteristics by computing the pre-program equivalent of $\Delta TTEs$ and $\delta TTEs$ using 1997 data. There is no difference in pre-program migration rate for the various subsets of the treatment and control groups. Overall, this evidence supports the view that one can interpret the following estimates as the causal effects of Progresa on migration.

# 5 Aid and migration: results

This section presents estimates of the program's impact on migration.

## 5.1 Average program effect on migration

Table 4 compares migration averages during the program, using the proportion of households with at least one migrant, and the average number of migrants (for households with at least one migrant).[27] I test for differences between the treated and the control group, considering alternatively domestic and international labor migrants. Average U.S. migration is significantly higher for the treatment group both at the individual and at the household level. Both in 1998 and 1999, the proportion of control households with at least one U.S. migrants is only 60% of the level among treated households. Given that there are no significant differences between the average number of migrants (in households with at least one migrant), this implies that there is a significantly higher migration level in the treated communities. Hence, program availability is associated with a 60 percent increase in average migration rate.

Table 4: 1998 and 1999 average migration levels

| | Valid 1998 observations | | Valid 1999 observations | |
|---|---|---|---|---|
| | US mig | MX mig | US mig | MX mig |
| Households with at least one migrant | | | | |
| Treatment | .0141 * | .0192 | .0150* | .0187 |
| Control | .0088* | .0153 | .0087* | .0188 |
| P-value of mean difference | [0.100]* | [0.364] | [0.070]* | [0.989] |
| Migrants per household[a] | | | | |
| Treatment | 1.407 | 1.471 | 1.353 | 1.459 |
| Control | 1.552 | 1.454 | 1.486 | 1.291 |
| P-value of mean difference | [0.389] | [0.920] | [0.441] | [0.151] |

[a] average migrant number for household with at least one migrant. P-values of differences obtained from regressions of migration variables on treatment dummy. *: difference significant at the 90% level.

[27] In 1998 and 1999 I can compute these variables for all individuals who are away from the households for work-related purposes, while in 1997 I can only compute similar measures for all trips undertaken in the previous 12 months, and for all members who are away from the household, irrespective of their motives.

Various facts are worth noticing. First, the program transfers are associated with new households beginning to send their members abroad, rather than households with existing U.S. migrants intensifying their members' migration rates (otherwise there would be a higher number of migrants per households in the treatment group). This happens despite migration being cheaper (using current migrant members to provide information, housing, and help with the job search) and more easily financed (using the income from remittances) from households with existing migrants. Thus, this fact is consistent with the hypothesis that the transfers enable individuals to migrate by relaxing credit constrains (or other capital market imperfections). Second, there is already a significant increase in international migration a few months after the program begins. Progresa started in May 1998 and very little money had been transferred by October-November (when the data were collected) because there is no schooling subsidy in the summer months. It is possible that a number of households may pool resources to finance one migration. In addition, the program may change the recipients' ability to borrow or their behavior (e.g. undertaking riskier activities because of the decreased income variance), as response to the expectation of future transfers. These issues are not further investigated here because they go beyond the scope of the current analysis. However, they clearly deserve more attention. Domestic migration rates do not differ between the control and treatment group. This fact is also consistent with the two possible explanations provided: it is only the costlier, riskier activities that increase overall after the program implementation.

Finally, note how the used measure of migration is likely to underestimate the actual number of trips undertaken because it does not account for temporary migrants who left and returned within the previous 12 months. An indication of this underestimation can be inferred by comparing the rate of 1997 trips within the previous year with the level of migration in 1998 and 1999 for the control group (Tables 3 and 4).[28] The underestimation appears to be particularly severe for domestic migration, probably because U.S. migrants traditionally tend to return home in December, hence most international migrants are likely to be away when the data are collected. In this case, one possibility is to interpret the available estimates as a lower bound to the true effect. Alternatively, one may want to concentrate on the sign, rather than on the magnitude of the point estimates, since the sign is not likely to be affected by the underestimation of the migration levels.

### 5.2 Effect of unconditional and conditional grants

In the following discussion I focus on U.S. migration is because there are no effects for domestic migration. Table 5 presents estimates of the program's impact on 1998 international migration.

[28] However, this difference may also be attributed to an aggregate decrease in migration level.

The upper part of the table provides probit estimates of selected partial effects. The lower part reports the estimates of the parameters of interest. The first four columns provide estimates of the partial effect of total grant size (both discrete, $G$, and continuous, $g$) without conditioning on grant composition, which is then added to the specifications in columns 5 to 10. Hence, columns 1 to 4 provide estimates of (1) and (**??**). Columns 5 to 10 instead estimate (2) and (4). I assume that the partial effects of $g$ and $p$ are independent of each other in columns 5 to 8. In columns 9 and 10, instead, I relax this independence assumption by interacting the linear terms $p$ and $g$: in this case the effect of, e.g., the conditional grant is a function of the unconditional grant.[29] Note that, after conditioning on grant composition, one can interpret the estimates of the (1) and (**??**) parameters as the effect of an exogenous income increase on migration. The upper part of the Table reports the point estimates of the partial effects, while the lower part shows the estimates of the parameters of interest.[30] Given the few observed migrations, which are possibly underestimating the true migration level, I am primarily interested in the signs of the partial effects, rather than in their magnitude.

The first four columns show that the effect of the program on international migration is a positive function of the potential grant size. The effect of a large grant on recipient households' international migration is almost one percentage point higher than for families with low grant levels. The estimates from columns 3 and 4 imply that a one hundred *peso* increase (about 10 dollar increase) in potential grant causes a 0.6-0.7 percentage point increase in the proportion of households with at least one migrant. This is quite a sizeable increase, considering that average migration level is 1.41%. The estimates of the $\delta TTEg$ parameters are higher after conditioning on grant composition (columns 7 to 10). This result makes sense, because the estimates of $\delta TTEg$ in columns 3 and 4 encompass the grants' income and the participation effects, which are expected to be positive and negative, respectively. Instead, the specifications in the last 4 columns control for the participation effect separately, hence the larger point estimate of $\delta TTEg$. This indirectly confirms the prediction that a higher proportion of secondary school recipients may reduce migration. However, the difference is not statistically significant. Moreover, the partial effect of grant composition is very imprecisely estimated, and seldom statistically different from zero, although its point estimates are often negative, consistent with expectations. When significant, these estimates suggest that, conditional on total grant, the effect of the program on migration is 0.45 percentage points lower for households with a large proportion of conditional grant than for households with a medium proportion (columns 5 and 6).

[29]The particular functional forms chosen should not be interpreted as representations of a "true" model but as fitting the data to different specifications to test whether the signs of the coefficients are consistent with expectations under alternative *ad hoc* assumptions.

[30]These two sets of estimates differ only in the last two columns.

Regarding the effect of exogenous income changes on migration, the estimates from columns 7 to 10 show that a one hundred *peso* increase in potential grant increases the proportion of households with at least one migrant by roughly one percentage point. As already noticed, the magnitude of this effect is quite sizeable, compared to the migration level in the control group. These estimates come from one particular group of villages, and as such it may be presumptuous to draw general conclusions from them. Nevertheless, these results bear important implications, because they suggest that poverty alleviation measures, or in general growth in indigent areas, may result in higher Mexican migration to the United States.

Table 6 presents the results from 1999 data. The main results are unchanged: the effect of the program on migration is a positive function of potential transfer both with and without conditioning for grant composition. Moreover, there is weak evidence that migration is a negative function of the conditional grant proportion. However, the confidence intervals are larger. This may depend on the fact that potential grant sizes are measured with more error in 1999. I computed the potential grant amount and composition by assuming that all children complete the school grade they are attending in 1998. Given the high retention rate, my grade estimates are likely to be an upper bound to the true ones, hence the estimated partial effects may be biased towards zero.

To summarize, the data strongly support the hypothesis that the positive effect of the program on migration is increasing in grant size. Since the effect of total grant on migration is positive even after conditioning on grant composition, I can conclude that in these data exogenous income increases cause higher international migration. This is consistent with the model predictions. The evidence regarding the negative effect of conditional grants on migration is weak. I suspect that there are two possible reasons for this. One is the data limitations, with the low and possibly underestimated number of observed migration. An additional explanation may be that the effect of conditional grants may be negative for some households and positive for others, depending on how the transfer changes income, and on the number of potential migrants in the household. Domestic migration, instead, is not affected by changes in grant size and composition. The relevant Tables are in the Appendix (Tables 13 and 14).

Table 5: Program effect on 1998 international migration considering grant size and composition

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|
| $T$ | -0.0007 | -0.0007 | -0.0021 | -0.0028 | -0.0004 | -0.0003 | -0.003 | -0.0034 | -0.0024 | -0.0018 |
| | [0.0016] | [0.0016] | [0.0022] | [0.0026] | [0.0016] | [0.0016] | [0.0026] | [0.0029] | [0.0032] | [0.0029] |
| $T \cdot G$ | 0.0079 | 0.0092 | | | 0.0078 | 0.0075 | | | | |
| | [0.0036]** | [0.0049]* | | | [0.0072] | [0.0073] | | | | |
| $T \cdot g$ | | | 0.006 | 0.0073 | | | 0.0093 | 0.01 | 0.0077 | 0.0058 |
| | | | [0.0024]** | [0.0037]** | | | [0.0051]* | [0.0056]* | [0.0061] | [0.0055] |
| $T \cdot P_1$ | | | | | 0.0062 | 0.0064 | | | | |
| | | | | | [0.0113] | [0.0112] | | | | |
| $T \cdot P_2$ | | | | | -0.0021 | -0.0022 | | | | |
| | | | | | [0.0028] | [0.0028] | | | | |
| $T \cdot p$ | | | | | | | -0.0047 | -0.0042 | -0.0088 | -0.0225 |
| | | | | | | | [0.0062] | [0.0063] | [0.0129] | [0.0130]* |
| $T \cdot p \cdot g$ | | | | | | | | | 0.0062 | 0.0274 |
| | | | | | | | | | [0.0159] | [0.0176]* |
| $T \cdot C^b$ | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| $\Delta T\hat{T}EG_{10}{}^a$ | 0.0079 | 0.0092 | | | 0.0078 | 0.0075 | | | | |
| | [0.0036]** | [0.0049]* | | | [0.0072] | [0.0073] | | | | |
| $\delta T\hat{T}Eg$ | | | 0.006 | 0.0073 | | | 0.0093 | 0.01 | 0.0091 | 0.0121 |
| | | | [0.0024]** | [0.0037]** | | | [0.0051]* | [0.0056]* | [0.0041]** | [0.0041]*** |
| $\Delta T\hat{T}EP^a_{10}$ | | | | | 0.0062 | 0.0064 | | | | |
| | | | | | [0.0113] | [0.0112] | | | | |
| $\Delta T\hat{T}EP^a_{20}$ | | | | | -0.0021 | -0.0022 | | | | |
| | | | | | [0.0028] | [0.0028] | | | | |
| $\Delta T\hat{T}EP^a_{21}$ | | | | | -0.0045 | -0.0046 | | | | |
| | | | | | [0.0023]** | [0.0021]** | | | | |
| $\delta T\hat{T}Ep$ | | | | | | | -0.0047 | -0.0042 | -0.0053 | -0.007 |
| | | | | | | | [0.0062] | [0.0063] | [0.0061] | [0.0054] |

[a]: note the assumptions that $\Delta TTE_{G_{10}P_j} = \Delta TTE_{G_{10}P}$ and $\Delta TTE_{P_{jl}G_k} = \Delta TTE_{P_{jl}G}$ for $j, l = 0, 1, 2$, $j > l$ and $k = 0, 1$ in columns 5 to 10. [b]: the term $T \cdot C$ is interacted with the proportional grant measure $p$, when applicable. Standard errors clustered at the village level.

Table 6: 1999 household international migration difference for varying grant size and composition

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|
| $T$ | | | -0.0013 | -0.0007 | 0.0006 | 0.0006 | -0.002 | -0.0013 | 0.0007 | 0.001 |
| | | | [0.0023] | [0.0026] | [0.0016] | [0.0016] | [0.0026] | [0.0028] | [0.0027] | [0.0026] |
| $T \cdot G$ | 0.0054 | 0.0044 | | | 0.0147 | 0.0148 | | | | |
| | [0.0034]* | [0.0039] | | | [0.0096]* | [0.0113] | | | | |
| $T \cdot g$ | | | 0.0034 | 0.0024 | | | 0.0059 | 0.005 | 0.0012 | 0.0005 |
| | | | [0.0020]* | [0.0029] | | | [0.0042] | [0.0046] | [0.0047] | [0.0043] |
| $T \cdot P_1$ | | | | | -0.0032 | -0.0032 | | | | |
| | | | | | [0.0020] | [0.0020] | | | | |
| $T \cdot P_2$ | | | | | -0.0044 | -0.0045 | | | | |
| | | | | | [0.0013]*** | [0.0014]*** | | | | |
| $T \cdot p$ | | | | | | | -0.0046 | -0.0053 | -0.0198 | -0.0249 |
| | | | | | | | [0.0063] | [0.0063] | [0.0122] | [0.0145]* |
| $T \cdot p \cdot g$ | | | | | | | | | 0.016 | 0.0225 |
| | | | | | | | | | [0.0108] | [0.0130]* |
| $T \cdot C$ | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| $\Delta T\hat{T}EG_{10}$ | 0.0054 | 0.0044 | | | 0.0147 | 0.0148 | | | | |
| | [0.0034]* | [0.0039] | | | [0.0096]* | [0.0113] | | | | |
| $\delta T\hat{T}Eg$ | | | 0.0034 | 0.0024 | | | 0.0059 | 0.005 | 0.0055 | 0.0066 |
| | | | [0.0020]* | [0.0029] | | | [0.0042] | [0.0046] | [0.0034]* | [0.0041]* |
| $\Delta T\hat{T}EP_{10}$ | | | | | -0.0044 | -0.0045 | | | | |
| | | | | | [0.0013]*** | [0.0014]*** | | | | |
| $\Delta T\hat{T}EP_{20}$ | | | | | -0.0032 | -0.0032 | | | | |
| | | | | | [0.0020] | [0.0020] | | | | |
| $\Delta T\hat{T}EP_{21}$ | | | | | 0.0077 | 0.0076 | | | | |
| | | | | | [0.0068] | [0.0077] | | | | |
| $\delta T\hat{T}Ep$ | | | | | | | -0.0046 | -0.0053 | -0.0071 | -0.007 |
| | | | | | | | [0.0063] | [0.0063] | [0.0058] | [0.0056] |

[a]: note the assumptions that $\Delta TTE_{G_{10}P_j} = \Delta TTE_{G_{10}P}$ and $\Delta TTE_{P_{jl}G_k} = \Delta TTE_{P_{jl}G}$ for $j, l = 0, 1, 2$, $j > l$ and $k = 0, 1$ in columns 5 to 10. [b]: the term $T \cdot C$ is interacted with the proportional grant measure $p$, when applicable. Standard errors clustered at the village level.

### 5.3 Further tests of the effect of conditional grants

In this section I perform additional tests of the effect of conditional grants on migration. The model shows that direct grant recipients migrate less because of the program. However, their relatives may use the transfers to migrate more. I use individual-level data (as before, restricted to individuals aged 14 to 40) to test these predictions. I create two "direct recipient" dummy variables that group all individuals aged 14 to 18 who may attend school grades 3 to 9, and grades 7 to 9 only. I regress the probability that individual $j$ is a migrant (domestic and international, alternatively), $P(m_j^k = 1)$, on these "direct recipient" dummies $D_j$ (one at a time), the treatment dummy $T_j$ and their interaction. I additionally condition on the set of $X$ variables, adding also age (as a second order polynomial) and gender.

$$P(m_j^k = 1) = \alpha^k + \beta_1^k T_j + \beta_2^k D_j + \beta_3^k D_j T_j + \gamma^k X_j + u_j^k$$

The partial effect of the treatment dummy measures the impact of living in a treated village without being a direct program recipient. The effect of being a direct program recipient, instead, is given by the sum of the partial effects of the treatment dummy and the interacted term.

Table 7 presents probit estimates of these partial effects using the whole sample (columns 1 to 4).[31] I then estimate the same regressions for individuals from the subset of households with secondary school children only (columns 5 to 8). In this way I can check whether relatives of conditional grant recipients use the scholarship to migrate more. While there is no significant difference for domestic migration, the results in columns 1 and 3 show that relatives of children who may enroll to grades 3 to 9 are 0.15 percentage point more likely to be U.S. migrants if they live in treated villages than if they live in control localities. Direct program recipients are 0.21 percentage points less likely to be U.S. migrants than their relatives. Children in treatment communities who may enrol to grades 3 to 9 are 0.06 percentage points less likely to be international migrants than children in control localities (this difference is statistically significant). This confirms the model predictions that the effect of the conditional grant, while reducing the migration of direct recipients, may or may not increase total household migration depending on whether program participation results into lower, equal or higher household income. As expected, there is no difference between total recipients and secondary school recipients only (columns 1 and 3), given that they are in the same age group, 14 to 18 years old. This may suggest that age, rather than school level, determines whether individuals are potential migrants.

[31] I can perform this test only with 1998 data, because I need 1998 information on last school grade completed in the 1997-1998 academic year. Since there is no unique personal identifier for the 1998 and 1999 data, I cannot match 1998 school grade with 1999 individual data. I cannot use 1998-1999 school grade because it is endogenous to the program implementation.

In column 5 I restrict the sample to families with secondary school children only, obtaining similar results to the ones in column 3. Column 7 shows that the program has no overall effect on international migration for households with secondary school recipients only. Given that the overall program effect on U.S. migration is positive, this suggests that the effect is highest among households with high unconditional grant levels, once more consistent with the model prediction.

Table 7: Effect of the program for potential scholarship recipients, 1998 data

| | Whole sample | | | | Households with secondary school children | | | |
|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
| | US | MX | US | MX | US | MX | US | MX |
| T | 0.0015 | -0.0016 | 0.0016 | -0.001 | 0.0017 | -0.0039 | 0.0017 | -0.0031 |
| | [0.0008]** | [0.0037] | [0.0008]** | [0.0038] | [0.0011] | [0.0047] | [0.0013] | [0.0047] |
| T*recipient | -0.0021 | 0.0159 | | | | | | |
| | [0.0009]** | [0.0109] | | | | | | |
| T*secondary | | | -0.0021 | 0.0061 | -0.0026 | 0.0063 | | |
| school recipient | | | [0.0010]** | [0.0100] | [0.0013]** | [0.0085] | | |
| Observations | 26873 | 26873 | 26873 | 26873 | 9281 | 9281 | 9281 | 9281 |

Marginal effects from a probit model of the individual likelihood of being an international or domestic migrant, alternatively. $T$ is the treatment dummy. Additional controls include the same set of conditioning variables ($X$ and $C$) used in (6). Standard errors clustered at the village level.

I now proceed to test the prediction that conditional grants cause an overall reduction in migration in the household, when the move into education decreases household income. I obviously cannot observe what treated households' income would have been without Progresa. However, I can infer whether the move into education of eligible children was more or less costly. The move into schooling is likely to have the largest cost for individuals who would have worked otherwise. For these households, the opportunity cost of schooling may be larger than the scholarship, resulting in an overall income decrease. I assume that the households with children working in 1997 are the ones most likely to have had child labor also in 1998 and 1999, in the absence of the program. Thus, I want to test whether the program reduces migration from households with child labor in 1997. For this purpose, I create the dummy $CL$. This dummy is one for all households with at least one potential program recipient who was working in 1997.[32] In

[32]By potential recipient here I mean each child whose age and highest school grade are such that he or she would be a Progresa beneficiary, if the program were implemented in his or her village of residence.

particular, I am interested in the following hypotheses: first, that migration is lower among treated households with $CL = 1$ than among treated households with similar grant size and composition, but with $CL = 0$ (hypothesis 1). Second, that the effect of any grant size and composition on migration is lower for $CL = 1$ households than for $CL = 0$ ones (hypothesis 2).[33] Third, that the overall program effect on migration of $CL = 1$ households is negative, i.e. the same household would migrate more without Progresa (hypothesis 3).

I test these hypotheses by estimating equation (7) using household-level data again, and adding the CL dummy interacted by the treatment dummy:

$$P(m_i^k = 1) = \alpha^k + \beta_1^k T_i + \beta_2^k g_i + \beta_3^k p_i + \beta_4^k g_i T_i + \beta_5^k p_i T_i + \beta_6^k CL_i + \beta_7^k CL_i T_i$$
$$+\gamma^k X_i + \delta^k C_i p_i + \theta^k C_i p_i T_i + u_i^k$$

I can test the first hypothesis by testing for the joint significance of the partial effects of $CL$ and $CL \cdot T$, the second hypothesis by testing for the significance of the $CL \cdot T$ partial effect, and the third hypothesis by testing for the significance of the following object (assuming for simplicity that I am estimating the above regression by linear probability model): $\beta_1^k + \beta_4^k \bar{g} + \beta_5^k \bar{p} + \beta_7^k$, where $\bar{g}$ and $\bar{p}$ are the average values of $g$ and $p$. The estimates of the relevant parameters are shown in Table 8.

Although there is no significant effect in 1998, all three hypotheses are consistent with expectations in 1999. In particular, treated households with child labor in 1997 are 0.28 percentage points less likely to have U.S. migrants than treated households without child labor (hypothesis 1); the effect of Progresa on U.S. migration is half a percentage point lower for households with child labor than for households without child labor (hypothesis 2). Finally, the overall program effect for households with child labor is a reduction in U.S. migration of 0.27 percentage points (hypothesis 3).

To conclude, the evidence provided so far is consistent with the picture emerged from the theoretical model, at least for international migration: the latter is affected by the size and type of transfers received. Exogenous income increases in the form of unconditional transfers are associated with higher U.S. migration levels. Conditional transfers in the form of secondary school scholarships instead appear to reduce U.S. migration, although there is the possibility that part of the reduced migration level due to potential grant recipients moving from employment to schooling may be partially offset by the increased migration of their relatives. Lastly, the program is associated with an overall migration decrease (at least in 1999) for households whose participation may entail an income reduction.

[33] Households with working children are likely to be poorer than households without child labor. The conditioning variables added to the regression include household and village poverty level.

Table 8: Effect of the program for households with child labor in 1997.

| | 1998 | | 1999 | |
|---|---|---|---|---|
| | 1 | 2 | 3 | 4 |
| | US | MX | US | MX |
| $T$ | -0.006 | -0.0141 | -0.0794 | 0.0153 |
| | [0.0204] | [0.0490] | [0.0864] | [0.0272] |
| $CL$ | -0.0032 | 0.0001 | 0.0024 | 0.0059 |
| | [0.0029] | [0.0100] | [0.0038] | [0.0078] |
| $T \cdot CL$ | -0.0003 | -0.0047 | -0.0052 | 0.0015 |
| | [0.0044] | [0.0119] | [0.0027]* | [0.0090] |
| $T \cdot g$ | 0.0295 | 0.0341 | 0.0244 | 0.0036 |
| | [0.0136]** | [0.0389] | [0.0123]** | [0.0188] |
| $T \cdot p$ | -0.0242 | -0.0097 | 0.0141 | -0.0432 |
| | [0.0207] | [0.0416] | [0.0188] | [0.0321] |
| $T \cdot C$ | Yes | Yes | Yes | Yes |
| Observations | 4062 | 4068 | 4476 | 4493 |
| Test of hypothesis 1 | -0.0035 | -0.0046 | -0.0028 | 0.0074 |
| | [0.0051] | [0.0766] | [0.0014]** | [0.0116] |
| Test of hypothesis 2 | -0.0003 | -0.0047 | -0.0052 | 0.0015 |
| | [0.0044] | [0.0119] | [0.0027]* | [0.0090] |
| Test of hypothesis 3 | -0.0034 | -0.0045 | -0.0027 | 0.0073 |
| | [0.0026] | [0.0080] | [0.0015]* | [0.0057] |

Marginal effects from a probit model of the likelihood of having at least one international or domestic migrant in the household, alternatively. Hypotheses 1, 2, 1 and 3 are: first, that migration is lower among treated households with $CL = 1$ than among treated households with similar grant size and composition, but with $CL = 0$. Second, that the effect of any grant size and composition on migration is lower for $CL = 1$ households than for $CL = 0$ ones. Third, that the overall program effect on migration of $CL = 1$ households is negative, i.e. the same household would migrate more without Progresa. Standard errors clustered at the village level.

## 5.4 Medium-term migration

One of the model predictions is that conditional transfers targeting prospective migrants may achieve a migration reduction in the medium run. However, relaxing some of the less realistic assumptions shows that the scholarships may in fact increase future migration. One way to assess the medium-term program effect on migration is to look at migration rates of individuals with completed secondary school and of their relatives. I consider the sub-set of individuals aged 14 to 18 with complete secondary school education by November 1999. The ones living in treated villages are potential program recipients for the 1998-1999 academic year (although some of them may have completed 9th grade the previous academic year). I test whether their 1999 domestic and international migration levels, and their relatives', are significantly higher among the treated group.

The effect of the program on migration for individuals with completed secondary school is reported in Table 9, columns 1 and 3; columns 2 and 4 test the existence of a medium-term program effect on the migration level of relatives of individuals aged 14 to 18 with complete secondary school education by November 1999. Finally, column 5 looks at the likelihood of having at least one migrant (irrespective of destination).[34] In no case there is a significant difference between the treatment and the control group. Although the size of the sample is quite small, and the fact they do not migrate immediately after the end of the transfer does not prevent them from undertaking future migration, this result suggests that the program may not cause higher migration in the medium term.

Table 9: Average migration rate of 1999 secondary schoolchildren

| | US | | MX | | All |
|---|---|---|---|---|---|
| | Recipients | HH members | Recipients | HH members | Households |
| | 1 | 2 | 3 | 4 | 5 |
| $T$ | 0.0070 | 0.0000 | 0.0000 | -0.0012 | 0.0096 |
| | [0.0041] | [0.0000] | [0.0000] | [0.0047] | [0.0144] |
| Obs. | 724 | 2229 | 604 | 2147 | 627 |

Standard errors (in brackets) clustered at the village level.

One shortcoming of the current analysis is that ideally one would want to observe post-schooling migration behavior of individuals who began to go to secondary school because of the program implementation, rather than those who took advantage of the scholarship while they

[34] I pool domestic and international migration because there are few observed trips.

had already started the schooling cycle. Unfortunately, no data are available for 2001, when the first such cohort graduates.

## 6 Conclusions

This paper studies one important unintended consequence of aid programs: their potential impact on migration. Moreover, it analyzes the causal effect of income on migration using data on the Mexican development program Progresa. More generally, it stresses the importance of understanding policy impacts in a broader sense, beyond their primary purpose. The paper also focuses on how different transfer types may provide varying incentives to migrate. A theoretical model shows that unconditional cash transfers may increase migration rates of indigent households, for example by loosening credit constraints. Thus, the model predicts that an exogenous income increase would result in higher migration levels. In contrast, conditional transfers requiring recipients to go to school reduce the rate of migration of their direct recipients, resulting in some cases in lower migration from the whole household. Part of the reduction in current migration may be offset by increases in the future, with program recipients postponing the migration until after the subsidies are exhausted.

The empirical analysis uncovers interesting facts. First, the overall effect of the program is to increase international migration. Second, this additional migration comes mainly from households with no migrants before the program. Third, average domestic migration levels are not affected by the program. Several explanations are consistent with these findings: one is that aid relaxes credit-constraints for households, allowing them to finance the desired number of trips. Another is that the subsidies reduce relative income variance, inducing households to undertake risker activities such as an international migration.

Another interesting finding is that these international migration rates rise very early in the program, before significant amounts of money are transferred to recipients. It is possible that the program may increase the level of borrowing among poor households, or that different families may pool resources to finance one trip. The model prediction that different grant types affect migration in different ways is also confirmed by the empirical analysis: unconditional subsidies have a larger positive effect on contemporaneous migration than conditional subsidies. The positive effect of unconditional grants also implies that exogenous income increases raise migration levels. Conditional grants result in lower migration levels among their direct recipients (the children who have to go to school to receive the scholarship). This decrease in migration is in some cases offset by higher migration rates from their relatives. Lastly, the conditional secondary school subsidy does not appear to be associated with increases in migration after the recipients stop being eligible for the program.

These findings have interesting policy implications. First, by showing that aid affects migration, they confirm that development programs have consequences that go beyond changing their direct outcomes of interest. Assessments of aid policies should go beyond their direct, or intended, effects, including also their indirect, or unintended impact. Second, they reveal that different types of transfers have differential impacts on migration: for example, a transfer of a given size increases migration more if it is unconditional than conditional. Conditional transfers may be an interesting policy tool if one were interested in reducing the positive effect of aid on migration. Lastly, they suggest that migration levels, or at least the capability to migrate, are likely to increase in the short run as income levels of poor rural Mexicans grow, provided that one is willing to generalize the current results. This implies that dealing with illegal Mexican migration may remain remain a priority in the U.S. agenda also in the near future.

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## 8 Appendix

Here I present further details on variable creation, provide the result from the tests for pre-program different migration levels between treatment and control groups, and show the Tables of program effects for domestic migration.

### 8.1 Variable creation

In this section, further information is provided on the creation of the variables of interest, potential grant size and composition.

No information is available on effective size of the received transfer. Hence, it is only possible to compute the potential transfer size. This approach rules out endogeneity issues related to partial acceptance of the program, while it permits the estimation of parameters with strong policy relevance: the impact of the availability of a transfer (the policy maker cannot force individuals to fully comply with the program requirements). Its drawback is the difficulty of assessing the actual monetary impact of the program.

Potential grant size is computed considering all unmarried, non-parent children, grandchildren, great-grandchildren, nieces and nephews of the household head aged 5 to 18 in the November 1998 survey using last completed school grade and projecting future grades assuming nobody fails the grade.[35] Again, this assumption rules out endogenous grade failure, but it comes at the cost of making the unrealistic assumption of zero retention rate.

I have information on highest completed school grade both from the 1997 and the 1998 data. I use 1998 information because the 1998 data include schooling level of migrants, unlike the 1997 data. Although the data I use are collected after the program started, they refer to the 1997-1998 academic year, hence they are exogenous to the program implementation (unless there are anticipation effects among treated households). A further advantage of this choice versus using 1997 maximum grade data is that I do not have to assume a zero retention rate in the 1997-1998 academic year.

I compute that total grant ($g$) and conditional grant proportion ($p$) variables summing up the values of the scholarships to which the household would be entitled, should all its potential beneficiaries attend school. The scholarship values are shown in Table 10. I add to these value the income support trasnfer, which amounts to 190 and 250 *pesos* in November 1998 and 1999, respectively. When the potential grant exceeds the maximum value (e.g. when the household transfer is "capped"), I replace its value with the maximum grant size, which is 1250 and 1500 *pesos* in November 1998 and 1999.

Note that there is variation in the number, gender and school level of eligible children by family also within treated households. Indeed, the correlation between grant size and composition is positive and significant, amounting to 0.47 (0.46 for treatment poor only) in 1997, but it is far from one. Also, the specific grant amounts are such that different combinations of primary and secondary school beneficiaries yield the same potential transfer level. Hence, households entitled

[35]Children aged 5 in November 1998 are included in case they reach third grade in the 1999-2000 academic year.

Table 10: Breakdown of Progresa school subsidy by grade and gender

| | Nov. 1998 | | Nov. 1999 | |
|---|---|---|---|---|
| Grade | Male | Female | Male | Female |
| 3 | 140 | | 160 | |
| 4 | 160 | | 190 | |
| 5 | 200 | | 250 | |
| 6 | 270 | | 330 | |
| 7 | 400 | 410 | 480 | 500 |
| 8 | 420 | 470 | 500 | 560 |
| 9 | 440 | 510 | 530 | 610 |

to roughly equivalent grant levels may vary in the proportion of secondary school children.

### 8.2 Pre-program differences and effects of the program on domestic migration

Below are the tables containing results for pre-program differences. Tables 11 and 12 estimate all the equations presented above using 1997 data. With a slight abuse of notation, I label the estimated effects as $\Delta pTTEs$ and $\delta pTTEs$, meaning pre-program differences. As already mentioned, the 1997 measure of migration regards trips initiated in the previous 12 months. This is the information I use to compute the tables shown below. Additionally, households are asked how many members live elsewhere, irrespective of the reasons for the move (which may be to get married, study, work or other things). I perform the analysis also for this latter variable. The results for the additional migration variable are not reported because in no case there appeared to be a significant difference in the migration rates of control and treatment group.[36] Tables 11 and 12 reveal that there is no significance pre-program difference in the migration rates of poor households in treatment and control villages.

Tables 13 and 14 present the estimated effects of the program on domestic migration. There are no significant effects.

[36]Results available upon request.

Table 11: Differences in pre-program international migration levels considering potential grant size and composition

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|
| $T$ | -0.0043 | -0.0043 | -0.0044 | -0.0047 | -0.0039 | -0.0039 | -0.0054 | -0.0055 | -0.006 | -0.0057 |
| | [0.0053] | [0.0053] | [0.0061] | [0.0061] | [0.0053] | [0.0053] | [0.0064] | [0.0064] | [0.0068] | [0.0068] |
| $T \cdot G$ | -0.0003 | 0.0002 | | | 0.0065 | 0.0071 | | | | |
| | [0.0041] | [0.0043] | | | [0.0084] | [0.0085] | | | | |
| $T \cdot g$ | | | -0.0001 | 0.0005 | | | 0.0034 | 0.0033 | 0.0046 | 0.0039 |
| | | | [0.0050] | [0.0051] | | | [0.0087] | [0.0088] | [0.0092] | [0.0093] |
| $T \cdot P_1$ | | | | | -0.0077 | -0.008 | | | | |
| | | | | | [0.0070] | [0.0068] | | | | |
| $T \cdot P_2$ | | | | | -0.006 | -0.0056 | | | | |
| | | | | | [0.0056] | [0.0058] | | | | |
| $T \cdot p$ | | | | | | | -0.0064 | -0.0051 | 0.0019 | -0.0008 |
| | | | | | | | [0.0118] | [0.0123] | [0.0342] | [0.0342] |
| $T \cdot p \cdot g$ | | | | | | | | | -0.008 | -0.0042 |
| | | | | | | | | | [0.0299] | [0.0302] |
| $T \cdot C^b$ | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| $\Delta p\hat{TTE}G_{10}{}^a$ | -0.0003 | 0.0002 | | | 0.0065 | 0.0071 | | | | |
| | [0.0041] | [0.0043] | | | [0.0084] | [0.0085] | | | | |
| $\delta p\hat{TTE}g$ | | | -0.0001 | 0.0005 | | | 0.0034 | 0.0033 | 0.0028 | 0.0029 |
| | | | [0.0050] | [0.0051] | | | [0.0087] | [0.0088] | [0.0069] | [0.0069] |
| $\Delta p\hat{TTE}P_{10}^a$ | | | | | -0.0077 | -0.008 | | | | |
| | | | | | [0.0070] | [0.0068] | | | | |
| $\Delta p\hat{TTE}P_{20}^a$ | | | | | -0.006 | -0.0056 | | | | |
| | | | | | [0.0056] | [0.0058] | | | | |
| $\Delta p\hat{TTE}P_{21}^a$ | | | | | 0.0027 | 0.0037 | | | | |
| | | | | | [0.0086] | [0.0089] | | | | |
| $\delta p\hat{TTE}p$ | | | | | | | -0.0064 | -0.0051 | -0.0044 | -0.0041 |
| | | | | | | | [0.0118] | [0.0123] | [0.0090] | [0.0087] |

[a]: note the assumptions that $\Delta TTE_{G_{10}P_j} = \Delta TTE_{G_{10}P}$ and $\Delta TTE_{P_{jl}G_k} = \Delta TTE_{P_{jl}G}$ for $j, l = 0, 1, 2$, $j > l$ and $k = 0, 1$ in columns 5 to 10. [b]: the term $T \cdot C$ is interacted with the proportional grant measure $p$, when applicable. Standard errors clustered at the village level.

Table 12: Differences in pre-program domestic migration levels considering potential grant size and composition

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|
| $T$ | 0.0106 | 0.0106 | 0.0149 | 0.0137 | 0.0133 | 0.0132 | 0.009 | 0.0086 | 0.0138 | 0.015 |
| | [0.0175] | [0.0175] | [0.0220] | [0.0219] | [0.0178] | [0.0178] | [0.0221] | [0.0221] | [0.0226] | [0.0227] |
| $T \cdot G$ | 0.007 | 0.0092 | | | 0.0303 | 0.032 | | | | |
| | [0.0173] | [0.0174] | | | [0.0243] | [0.0244] | | | | |
| $T \cdot g$ | | | -0.0024 | 0.0004 | | | 0.0183 | 0.0183 | 0.007 | 0.0031 |
| | | | [0.0189] | [0.0189] | | | [0.0276] | [0.0276] | [0.0319] | [0.0326] |
| $T \cdot P_1$ | | | | | -0.0129 | -0.0142 | | | | |
| | | | | | [0.0266] | [0.0266] | | | | |
| $T \cdot P_2$ | | | | | -0.033 | -0.0318 | | | | |
| | | | | | [0.0191] | [0.0193] | | | | |
| $T \cdot p$ | | | | | | | -0.0361 | -0.0318 | -0.099 | -0.1133 |
| | | | | | | | [0.0382] | [0.0388] | [0.0811] | [0.0819] |
| $T \cdot p \cdot g$ | | | | | | | | | 0.0639 | 0.0846 |
| | | | | | | | | | [0.0750] | [0.0779] |
| $T \cdot C^b$ | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| $\Delta p\hat{TTE}G_{10}{}^a$ | 0.007 | 0.0092 | | | 0.0303 | 0.032 | | | | |
| | [0.0173] | [0.0174] | | | [0.0243] | [0.0244] | | | | |
| $\delta p\hat{TTE}g$ | | | -0.0024 | 0.0004 | | | 0.0183 | 0.0183 | 0.0217 | 0.0226 |
| | | | [0.0189] | [0.0189] | | | [0.0276] | [0.0276] | [0.0241] | [0.0257] |
| $\Delta p\hat{TTE}P_{10}^a$ | | | | | -0.0129 | -0.0142 | | | | |
| | | | | | [0.0266] | [0.0266] | | | | |
| $\Delta p\hat{TTE}P_{20}^a$ | | | | | -0.033 | -0.0318 | | | | |
| | | | | | [0.0191] | [0.0193] | | | | |
| $\Delta p\hat{TTE}P_{21}^a$ | | | | | -0.0213 | -0.0186 | | | | |
| | | | | | [0.0206] | [0.0213] | | | | |
| $\delta p\hat{TTE}p$ | | | | | | | -0.0361 | -0.0318 | -0.0485 | -0.0465 |
| | | | | | | | [0.0382] | [0.0388] | [0.0483] | [0.0461] |

[a]: note the assumptions that $\Delta TTE_{G_{10}P_j} = \Delta TTE_{G_{10}P}$ and $\Delta TTE_{P_{jl}G_k} = \Delta TTE_{P_{jl}G}$ for $j, l = 0, 1, 2$, $j > l$ and $k = 0, 1$ in columns 5 to 10. [b]: the term $T \cdot C$ is interacted with the proportional grant measure $p$, when applicable. Standard errors clustered at the village level.

Table 13: Program effects on 1998 domestic migration levels considering potential grant size and composition

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|
| $T$ | -0.0001 | -0.0001 | -0.0005 | -0.0034 | -0.0003 | -0.0005 | -0.0009 | -0.0034 | -0.0029 | -0.0017 |
| | [0.0044] | [0.0044] | [0.0058] | [0.0062] | [0.0045] | [0.0046] | [0.0061] | [0.0064] | [0.0067] | [0.0066] |
| $T \cdot G$ | 0.0029 | 0.0061 | | | 0.0001 | 0.003 | | | | |
| | [0.0052] | [0.0060] | | | [0.0086] | [0.0091] | | | | |
| $T \cdot g$ | | | 0.0027 | 0.0095 | | | 0.0044 | 0.0093 | 0.0091 | 0.0054 |
| | | | [0.0069] | [0.0081] | | | [0.0119] | [0.0120] | [0.0133] | [0.0135] |
| $T \cdot P_1$ | | | | | 0.0078 | 0.0053 | | | | |
| | | | | | [0.0141] | [0.0131] | | | | |
| $T \cdot P_2$ | | | | | 0.001 | 0.0026 | | | | |
| | | | | | [0.0089] | [0.0093] | | | | |
| $T \cdot p$ | | | | | | | -0.0023 | 0.0002 | 0.009 | -0.0169 |
| | | | | | | | [0.0138] | [0.0138] | [0.0272] | [0.0328] |
| $T \cdot p \cdot g$ | | | | | | | | | -0.0157 | 0.0236 |
| | | | | | | | | | [0.0313] | [0.0420] |
| $T \cdot C^b$ | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| $\Delta \hat{TTE} G_{10}{}^a$ | 0.0029 | 0.0061 | | | 0.0001 | 0.003 | | | | |
| | [0.0052] | [0.0060] | | | [0.0086] | [0.0091] | | | | |
| $\delta \hat{TTE} g$ | | | 0.0027 | 0.0095 | | | 0.0044 | 0.0093 | 0.0055 | 0.0108 |
| | | | [0.0069] | [0.0081] | | | [0.0119] | [0.0120] | [0.0061] | [0.0137] |
| $\Delta \hat{TTE} P_{10}^a$ | | | | | 0.0078 | 0.0053 | | | | |
| | | | | | [0.0141] | [0.0131] | | | | |
| $\Delta \hat{TTE} P_{20}^a$ | | | | | 0.001 | 0.0026 | | | | |
| | | | | | [0.0089] | [0.0093] | | | | |
| $\Delta \hat{TTE} P_{21}^a$ | | | | | -0.0055 | -0.0023 | | | | |
| | | | | | [0.0069] | [0.0079] | | | | |
| $\delta \hat{TTE} p$ | | | | | | | -0.0023 | 0.0002 | -0.0034 | 0.0017 |
| | | | | | | | [0.0138] | [0.0138] | [0.0072] | [0.0054] |

[a]: note the assumptions that $\Delta TTE_{G_{10}P_j} = \Delta TTE_{G_{10}P}$ and $\Delta TTE_{P_{jl}G_k} = \Delta TTE_{P_{jl}G}$ for $j, l = 0, 1, 2$, $j > l$ and $k = 0, 1$ in columns 5 to 10. [b]: the term $T \cdot C$ is interacted with the proportional grant measure $p$, when applicable. Standard errors clustered at the village level.

Table 14: Program effects on 1999 domestic migration levels considering potential grant size and composition

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|
| $T$ | -0.0047 | -0.0047 | -0.0077 | -0.0051 | -0.0059 | -0.0055 | -0.0056 | -0.0038 | -0.0028 | -0.0032 |
| | [0.0036] | [0.0035] | [0.0054] | [0.0053] | [0.0038] | [0.0038] | [0.0054] | [0.0053] | [0.0057] | [0.0059] |
| $T \cdot G$ | 0.0044 | 0.0011 | | | -0.004 | -0.0066 | | | | |
| | [0.0046] | [0.0045] | | | [0.0056] | [0.0053] | | | | |
| $T \cdot g$ | | | 0.0054 | 0.001 | | | -0.0004 | -0.0028 | -0.005 | -0.0038 |
| | | | [0.0044] | [0.0054] | | | [0.0076] | [0.0076] | [0.0088] | [0.0092] |
| $T \cdot P_1$ | | | | | 0.0196 | 0.0238 | | | | |
| | | | | | [0.0152] | [0.0169] | | | | |
| $T \cdot P_2$ | | | | | 0.0119 | 0.0083 | | | | |
| | | | | | [0.0106] | [0.0101] | | | | |
| $T \cdot p$ | | | | | | | 0.0105 | 0.0077 | -0.0079 | 0.0029 |
| | | | | | | | [0.0119] | [0.0125] | [0.0234] | [0.0281] |
| $T \cdot p \cdot g$ | | | | | | | | | 0.0179 | 0.0053 |
| | | | | | | | | | [0.0190] | [0.0276] |
| $T \cdot C^b$ | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| $\Delta \hat{TTE} G_{10}{}^{a}$ | 0.0044 | 0.0011 | | | -0.004 | -0.0066 | | | | |
| | [0.0046] | [0.0045] | | | [0.0056] | [0.0053] | | | | |
| $\delta \hat{TTE} g$ | | | 0.0054 | 0.001 | | | -0.0004 | -0.0028 | -0.0009 | -0.0026 |
| | | | [0.0044] | [0.0054] | | | [0.0076] | [0.0076] | [0.0024] | [0.077] |
| $\Delta \hat{TTE} P_{10}^{a}$ | | | | | 0.0196 | 0.0238 | | | | |
| | | | | | [0.0152] | [0.0169] | | | | |
| $\Delta \hat{TTE} P_{20}^{a}$ | | | | | 0.0119 | 0.0083 | | | | |
| | | | | | [0.0106] | [0.0101] | | | | |
| $\Delta \hat{TTE} P_{21}^{a}$ | | | | | -0.0036 | -0.0072 | | | | |
| | | | | | [0.0052] | [0.0050] | | | | |
| $\delta \hat{TTE} p$ | | | | | | | 0.0105 | 0.0077 | 0.0062 | 0.0071 |
| | | | | | | | [0.0119] | [0.0125] | [0.0075] | [0.0084] |

[a]: note the assumptions that $\Delta TTE_{G_{10}P_j} = \Delta TTE_{G_{10}P}$ and $\Delta TTE_{P_{jl}G_k} = \Delta TTE_{P_{jl}G}$ for $j, l = 0, 1, 2$, $j > l$ and $k = 0, 1$ in columns 5 to 10. [b]: the term $T \cdot C$ is interacted with the proportional grant measure $p$, when applicable. Standard errors clustered at the village level.