

# Liquidity, Risk, and Occupational Choices\*

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## Abstract

We explore which financial constraints matter most in the choice of becoming an entrepreneur. We consider a randomly assigned welfare program in rural Mexico and show that cash transfers significantly increase entry into entrepreneurship. We then exploit cross-household variation in the timing of these transfers and find that current occupational choices are significantly more responsive to the transfers expected for the future than to those currently received. Guided by a simple occupational choice model, we argue that the program has promoted entrepreneurship by enhancing willingness to bear risk as opposed to simply relaxing current liquidity constraints.

**Keywords:** Financial constraints; entrepreneurship; insurance; liquidity.

**JEL codes:** O16, G20, L26.

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# 1 Introduction

Entrepreneurship is considered a fundamental aspect of the process of development (Hausmann and Rodrik [2003]; Ray [2007]; Naudé [2010]), but one that is often hindered by financial constraints (Banerjee and Duflo [2005]; Levine [2005]). One way in which access to finance may promote economic development is by providing some poor individuals the opportunity to set up their own businesses (Banerjee [2003]; Karlan and Morduch [2009]).

Understanding the link between improved access to finance and occupational choices, however, poses some serious challenges. First, such an improvement seldom occurs in isolation from other changes in the economy, which makes it hard to estimate empirically its effects. Moreover, and perhaps even more fundamentally, occupational choices may be determined by several financial constraints, such as those concerning households' ability to save, borrow and obtain insurance against income shocks. Hence, one would like to open the box of "access to finance" and understand which of the various financial constraints is binding in a given situation. This is often complicated but obviously key for the interpretation of the effects and the design of effective policies.

This paper takes a step along these lines by asking whether financial constraints matter and which financial constraints matter most in the choice of becoming an entrepreneur. We first show that some individuals become entrepreneurs after receiving a positive shock to their households' income. We next analyze whether this effect is due to improved access to start-up capital or rather to an improved ability to insure against entrepreneurial risk. We develop a simple model to highlight how liquidity and insurance constraints respond differently to the time profile of expected income shocks. We then exploit the variation in the timing of these shocks in order to evaluate the relative importance of these two constraints in our setting.

More specifically, we exploit the welfare program *Progresa*, which targets poor households in rural Mexico and provides cash transfers conditional on their behaviors in health and children's education. While Section 2 provides a more detailed description of the program, we here stress some features which make it interesting for our exercise. First, the timing of access into *Progresa* has been randomized, thereby providing us a reliable control group to estimate its effects on occupational choices. Second, transfers are administered for an extended and predictable time period and, albeit partly conditional on schooling behaviors, they typically represent a sizable increase in households' wealth. Moreover, and perhaps most importantly for our purposes, their magnitude and time profile vary substantially according to household demographics; as a result, households face different (and partly exogenous) shocks to their current liquidity and to their ability to insure against future income fluctuations.

We first compare households in treated and control communities; we show that being entitled to the program’s cash transfers significantly increases the probability of entering self-employment. This provides (indirect) evidence that individuals face financial constraints in the decision to become entrepreneurs. We then exploit the fact that, as mentioned, treated households face different time profiles of cash transfers. In particular, the educational scholarship they are entitled to receive in a given year varies substantially with the number, grade in school and gender of their children. Slight cross-household variations in these characteristics might induce significant differences in the amount of current and future transfers. We then ask whether the choice of becoming an entrepreneur in the current period is more responsive to the transfers currently received or to those expected for the future.

We motivate our analysis by developing a simple occupational choice model in which individuals may face liquidity or insurance constraints. If wealth cannot be freely allocated across periods, since for example households cannot borrow, current and future transfers have different effects on the choice of becoming an entrepreneur. The transfers currently received are better suited to meeting start-up costs and thus are more important if liquidity constraints are binding. Conversely, future transfers are better suited to providing insurance against business failure and thus have stronger effects if insurance constraints are binding.

We then show that the probability of becoming an entrepreneur in the current period is significantly more responsive to the amount of transfers expected for the near future than to the amount currently received. This result is robust in various specifications, in which we control for the total amount of transfers received within a given time horizon. We also rule out that the very same household characteristics which determine the profile of transfers determine occupational choices as well.

In our view, these results tend to support the hypothesis that the program has been effective in promoting entrepreneurship, as it has relaxed insurance constraints as opposed to simply relaxing current liquidity constraints. While one may think of alternative stories whereby both current and future transfers matter (for example, future transfers may be used as collateral for moneylenders; or future investments may be needed to keep up with business needs), it is hard to explain that future transfers matter *more* based on liquidity constraints.

In order to obtain further support in favor of this interpretation, we construct a measure of entrepreneurial risk based on the variance of pre-program income of entrepreneurs relative to salaried workers in each village. We show that individuals are less likely to become entrepreneurs in villages where entrepreneurial returns are more volatile; and, indeed, these are the villages in which the treatment has the largest effects. These results

are robust to various definitions of the geographic scope of the relevant local market.

Based on this evidence, we argue that financial barriers to entry into self-employment do not appear as the most important obstacle in our setting (see McKenzie and Woodruff [2006] for similar evidence on micro-enterprises in urban Mexico). Instead, the possibility of better insuring against future income fluctuations may be what induces some individuals to undertake the risky choice of setting up a business.

We conclude our analysis with an exploration of the medium-term effects of the program on entrepreneurship. By exploiting an additional evaluation survey conducted six years after the start of the program, we show that the entrepreneurial dynamics induced by *Progresa* may have a persistent effect. In particular, in villages with a lower pre-program share of entrepreneurs, the exposure to *Progresa*'s transfers is associated with significantly higher rates of entrepreneurship after some years.

**Related literature** This paper builds on the literature on improved access to finance and occupational choices. Exploiting income shocks, Holtz-Eakin et al. [1994] and Blanchflower and Oswald [1998] show that having received an inheritance increases the probability of being or remaining self-employed. In experimental settings, de Mel et al. [2008] consider a sample of individual who already have a business in Sri Lanka and show that a random prize in cash or in kind considerably boosts their profits. More broadly, a substantial literature has explored the effects of improved access to credit and to insurance (see, e.g., Besley [1995] and Banerjee [2003] for reviews). Experimental evidence along these lines is, however, still scarce and very recent. Banerjee et al. [2009] and Karlan and Zinman [2010] provide evidence on the impact of micro-credit on small businesses in India and in the Philippines, respectively. Giné et al. [2008], Giné and Yang [2009] and Cole et al. [2012] study the determinants of take-up of weather insurance in Malawi and India. Differently from our setting, they directly explore the importance of risk aversion by eliciting responses to a specific survey. In general, however, despite liquidity and insurance constraints are often interrelated (Ray [1998]), little has been done to attempt to separate their effects. One notable exception is Dercon and Christiaensen [2011], who distinguish seasonal credit constraints from inter-temporal constraints related to risk on fertilizer adoption in Ethiopia.

There is also a substantial body of research related to *Progresa* and its experimental design, but to our knowledge no study has explicitly looked at its effects on occupational choices. The most closely related paper in this literature is Gertler et al. [2012]. They document that *Progresa*'s transfers were partly used to increase investments in agricultural assets and in business activities (which may or may not coincide with the main occupation) and that as a result the program has long-lasting effects on the welfare of

its beneficiaries. While the starting point of our paper is related, as we also show that *Progresa* induced households to change their income generating activities, the main focus is quite different. Gertler et al. [2012] are interested in the impact of the program on long-term living standards. At the same time, as they acknowledge, their effect may be driven both by relaxed liquidity and insurance constraints. The authors provide no attempt to distinguish the two mechanisms, which is instead the main focus of our paper.

## 2 Background and Data

### 2.1 Program Description

Launched in Mexico in 1997, *Progresa* is a large-scale welfare program mainly aimed at improving health and human capital accumulation in the poorest rural communities.<sup>1</sup> It provides households with conditional cash transfers targeted to specific behaviors in nutrition, health and education. Initially, 506 villages were selected to be part of the program evaluation sample. Within those, 320 villages were randomly allocated to the treatment group and 186 villages to the control group. As we show in Table 1, randomization has been successful in attaining balanced treatment and control populations. Among several individual, household and village characteristics, none displays statistically significant baseline differences.

Households are classified as eligible for the program if their poverty index, assessed using information collected before the start of the program, is above a given threshold. Importantly for our analysis, the eligibility status was fixed for the entire duration of the program (and so insensitive to subsequent changes in asset holdings).<sup>2</sup> Eligible households in treatment communities started receiving benefits in March-April 1998, whereas eligible households in control communities were not incorporated until November 1999. Cash transfers from *Progresa* are given bimonthly and come in two forms. The first is a fixed food stipend of 105 Pesos per month conditional on family members obtaining preventive medical care.<sup>3</sup> The second is an educational scholarships which is provided for each child who is less than 18 years old and enrolled between the third and the ninth grade, conditional on attending school a minimum of 85 percent of the time and not repeating a grade more than twice. Scholarship amounts range from 81 to 269 Pesos per month

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<sup>1</sup>The program is currently ongoing under the name *Oportunidades*.

<sup>2</sup>As an exception to this rule, around 3,000 households (the so-called *densificados*) were classified as non-poor in the baseline but were later reclassified as eligible. In order to avoid arbitrary classifications, we exclude those households from our analysis (the results are unchanged, though, once we include them).

<sup>3</sup>These figures are expressed in current Pesos as of the second semester of 1998. Transfer size has been increased over time in order to adjust for inflation.

per child; they increase with school grade and, in seventh to ninth grades, are larger for girls than for boys.<sup>4</sup> Overall transfer amounts can be substantial: median benefits are 176 Pesos per month (roughly 18 USD in 1998), equivalent to about 28% of the monthly income of beneficiary families.

## 2.2 Sample Description

In our main empirical analysis, we exploit a baseline survey conducted in October 1997 and a series of Household Evaluation Surveys collected every six months starting in October 1998 for a total of five waves after the baseline.<sup>5</sup> These surveys include socioeconomic characteristics at the individual level for 24,077 households, of which about 53% are classified as eligible. We mostly focus on eligible households during the experimental period: in addition to the baseline, we employ the first three waves of the follow-up surveys conducted in October 1998, March 1999 and October 1999.<sup>6</sup> Program take-up is high in this sample: 86% of the treated households are reported to have received positive transfers within 18 months since program offering. Sample attrition is low (11%), and non-response in occupational choice somewhat larger (17%); however, neither is related to the treatment assignment.

In the baseline, we have information on the main occupation of 20,770 eligible adult individuals (18 years old or more). We mainly concentrate on flows into entrepreneurship, i.e., on those individuals who are either salaried or report no paid occupation (we refer to them as unemployed) in the baseline and who become entrepreneurs in the follow-up period. Among those residing in control villages, 4% become entrepreneurs during this period (mostly self-employed), of which roughly 25% were unemployed in the baseline and 20% are women.

A distinctive features of new entrepreneurs is their engagement in micro-business activities not (directly) related to agriculture. In control villages, 11% of new entrepreneurs report being engaged in activities like carpentry, handicraft, and domestic services, whereas the corresponding share for salaried workers is only 3%. Moreover, we note that 34% of new entrepreneurs in control villages have more than one paid occupation vis-a-vis 8% of salaried workers. This is common in many developing settings,

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<sup>4</sup>Specifically, a household is entitled to receive 81 Pesos per month for each child enrolled in the third grade. The corresponding amounts for the following grades are respectively 91, 116, 146, 214, 224 and 239 for males and 91, 116, 146, 224, 249 and 269 for females. In our sample period, no educational transfers are given before the third grade or after the ninth grade.

<sup>5</sup>A second baseline survey was conducted in March 1998, but it did not contain any question about occupational status.

<sup>6</sup>We employ the survey waves of March 2000 and November 2000 as one placebo sample in Section 3. In Section 6 we use an additional survey wave collected in 2003 to study the medium-term effects of the program.

and it is typically interpreted as an income smoothing strategy (see, e.g., Morduch [1995], Banerjee and Duflo [2008]). Indeed, also in our sample, new entrepreneurs face a substantially higher volatility of labor income in their primary occupation, which may increase their need for self-insurance.<sup>7</sup>

### 3 Entrepreneurship and Financial Constraints

#### 3.1 Program Impacts

Consider an individual  $i$  who is either a salaried worker or unemployed in the baseline, and let  $ne_{i,t}$  be a dummy equal to one if the individual has become an entrepreneur in a given program period  $t$  and zero otherwise. We estimate regressions of the following form:

$$ne_{i,t} = \alpha_1 T_l + X'_{i,t_0} \alpha_2 + \epsilon_{i,t}, \quad (1)$$

where  $T_l$  represents the *Progresa* treatment assignment at the locality level  $l$  and the vector  $X_{i,t_0}$  denotes a set of pre-determined covariates: individual age, gender, education, income, spouse's main occupation, household wealth and demographic composition, village shares of entrepreneurs and proxies for agricultural risk. We also include time dummies and state dummies.<sup>8</sup> In order to take into account the potential intra-village correlation of the individual error term  $\epsilon_{i,t}$ , we cluster standard errors at the village level.

Table 2 reports OLS estimates of  $\alpha_1$  in equation (1), which measures the average effect of eligibility for *Progresa* transfers on the transition into entrepreneurship.<sup>9</sup> Program impacts appear to be both statistically and economically significant. As shown in column (1), being entitled to the program increases the probability of entering self-employment by 0.9 percentage points. In relative terms, this represents an increase of 24% with respect to the counterfactual sample averages (equal to 4%). In columns (2)-(3), we show that the program significantly increases the probability of entry into entrepreneurship from both salaried work and unemployment.

In order to explore whether the above results are driven by receiving program benefits, we run two additional tests. First, we estimate equation (1) for periods in which control villages are incorporated into the program (corresponding to the survey waves of March 2000 and November 2000). As shown in column (4), there are no significant treated-control differences in the probability of entry into entrepreneurship when both treated

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<sup>7</sup>The standard deviation of monthly labor income in control villages is 84% of the sample mean for entrepreneurs vis-a-vis 60% for salaried workers.

<sup>8</sup>We cannot specify fixed effects at a more disaggregated geographical level, such as municipality or village, since this would imply losing the exogenous variation induced by the experiment.

<sup>9</sup>All our results are robust if we use a Probit model instead.

and control villages are receiving the transfers. Second, we estimate equation (1) on individuals who are not classified as poor and so are not eligible to receive the transfers. As shown in column (5), there are no significant treated-control differences for these individuals.

### 3.2 Alternative Channels

As described in Section 2, cash transfers are conditional on health and schooling behaviors. In particular, the requirement of sending children to school may have a direct effect on occupational choices: for example, as children become less likely to work at home, adults may have to quit a salaried job and turn to self-employment in search of flexible working hours. According to this interpretation, treatment impacts should be higher for those households who change their schooling behaviors in response to the program, either by enrolling children who were not enrolled before the program or by keeping enrolled children in school longer. As for the former group, we construct a dummy equal to one if the household has eligible children not enrolled in school at the baseline. In order to account for the second possibility, we construct two dummy variables: the first is equal to one if the household has eligible children enrolled in the last two grades of primary school at the baseline; the second is equal to one if the household has eligible female children enrolled in the first two grades of secondary school at the baseline. According to Schultz [2004], the program indeed has a greater impact on the transition between primary and secondary school and on female secondary schooling. In columns (1)-(3) of Table 3, we report that program impacts do not seem to vary along these dimensions. Alternatively, *Progresa* may change parents' expectations about their children's school trajectories, and this may directly impact adults' occupational choices. In this case, our effects may be concentrated on adults reporting lower expectations at the baseline. We then interact our program treatment indicator with pre-program parents' expectations about their children's educational attainments.<sup>10</sup> Again, we see no systematic differences in program impacts with respect to these expectations (column 4).

A different explanation of our findings is based on the potential complementary between *Progresa* and preexisting programs. Indeed, in our sample villages, households may also benefit from welfare programs which are directly related to their occupational choices.<sup>11</sup> If *Progresa* improves the effectiveness of these programs, the observed changes

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<sup>10</sup>Specifically, parents are asked which grade they think each of their children can attain. We take the average of these grades for each household and construct a dummy equal to one if the household reports average expected attainment above secondary school, which corresponds to the sample median, and zero otherwise.

<sup>11</sup>In particular, *Probecat* and *Cimo*, which provide training grants to the unemployed and to those employed in small firms; *Programa de Empleo Temporal*, which provides temporary employment in public projects; and *Procampo*, which provides subsidies to rural workers.



in occupational choices may be only spuriously related to *Progres*a's transfers. In this case, we expect effects to be concentrated among households who are also beneficiaries of alternative programs. We then interact our program treatment indicator with a dummy equal to one if the household has received money from any of these programs during the sample period. As shown in column (5), the data do not suggest that our effects are mediated by complementary welfare programs.<sup>12</sup>

Taken together, this evidence lends support to the view that *Progres*a has affected individuals' occupational choices due to the provision of monetary benefits rather than through other behavioral responses to the program. This suggests that individuals face financial constraints in their decision to become entrepreneurs. We then try to better uncover the nature of these financial constraints.

## 4 Liquidity and Insurance Constraints: Theory

Absent the program, individuals may refrain from becoming entrepreneurs for at least two reasons. First, they may face liquidity constraints which prevent them from undertaking some initial capital investment. The program would then promote entrepreneurship by increasing their current liquidity. Second, individuals may prefer avoiding the risk associated with entrepreneurial returns. In this case, by providing transfers for an extended and predictable period of time, the program would promote entrepreneurship by increasing individuals' ability to cope with future income fluctuations. In this section, we develop a simple model to highlight how liquidity and insurance constraints respond differently to the time profile of expected income shocks.

**Setup** Consider a population of individuals who are heterogeneous in their initial wealth  $a$  and in their risk aversion  $r$ . Individuals live for two periods. In the first period, they choose their occupation: either they become self-employed, which requires a fixed investment of  $k$  units of capital, or they look for a salaried job. In addition, they choose the amount of wealth they wish to save (possibly as a function of their occupational choice), which we denote as  $s$ . We do not allow for borrowing and so impose

$$s \geq 0, \tag{2}$$

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<sup>12</sup>Alternatively, we interact our program indicator with a dummy equal to one if the household has received money from any welfare program (as opposed to only those directly related to occupational choices) and with the total amount of money received from those programs. The results are very similar to those reported in column (5).

and we normalize the returns of saving to one.<sup>13</sup>

In the second period, individuals enjoy the returns from their occupation. The self-employed get  $y$  with probability  $p$  and zero otherwise. Among those who look for a salaried job, a fraction  $\lambda$  finds one while the rest remains unemployed. In the former case, individuals get a fixed wage  $w$ , while if unemployed they enjoy benefits  $b$ , with  $b \leq w$ . We assume that  $py - k \geq \lambda w + (1 - \lambda)b$  and  $p \leq \lambda$ , which imply that self-employment has higher expected returns but higher risk than looking for a job.

Savings and occupation are chosen in order to maximize

$$U = u(x_1) + \mathbb{E}[u(x_2)],$$

where  $\mathbb{E}[\cdot]$  is the expectation operator and  $x_1$  and  $x_2$  denote consumption in period 1 and 2. We assume that  $u$  exhibits decreasing absolute risk aversion, and for simplicity we abstract from time discounting.

Finally, irrespective of their choices, individuals are entitled to cash transfers  $C_1$  in period 1 and  $C_2$  in period 2. Our main interest is in exploring how the share of self-employed varies with the transfers  $C_1$  and  $C_2$ . We here provide an intuitive argument; a more detailed exposition can be found in the Appendix.

**Analysis** As is standard in this class of models (see e.g. Kihlstrom and Laffont [1979]), there exists a threshold level of risk aversion  $r^*$  such that those with  $r \leq r^*$  prefer being self-employed. The self-employed are then defined as those individuals with  $r \leq r^*$  and  $a + C_1 \geq k$ .

Consider first those individuals for whom borrowing constraints do not bind. With simple algebra, it can be shown that their occupational choice is equally responsive to  $C_1$  and to  $C_2$ . As is intuitive, individuals who can optimally allocate wealth across periods see no fundamental difference between transfers received today and those they know they will receive in the future.

Suppose, however, that for some agents borrowing constraints are binding. In this case, current and future transfers are not equivalent. To see this, consider first a world with only liquidity constraints. That is, suppose that  $k > C_1$  and individuals are risk neutral, so that all those with  $a + C_1 \geq k$  become self-employed. In this setting, the share of self-employed in period 1 is more responsive to  $C_1$  than to  $C_2$ . The reason is that current transfers help overcoming liquidity needs while future transfers may not be pledged for obtaining cash in period 1 and finance the investment  $k$ .

Consider now a world in which there are only insurance constraints. That is, suppose

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<sup>13</sup>Borrowing constraints are widely documented (restricting to developing countries, see the surveys in Banerjee [2003] and Karlan and Morduch [2009]).

that  $k \leq C_1$  and individuals are risk-averse, so that all those with  $r \leq r^*$  become self-employed. In order to be willing to take risk, individuals need to have enough wealth in period 2, and this is in turn more likely to occur by increasing  $C_2$  than by increasing  $C_1$ . The reason is that individuals with binding borrowing constraints consume all their wealth in the first period (and still they would prefer consuming more); hence, increasing  $C_1$  does not make them richer in period 2 and so does not affect their willingness to take risk. As a result, self-employment is more responsive to future than to current transfers. We then have the following Proposition.

**Proposition 1** *Suppose individuals face constraints on allocating transfers across periods. Then current occupational choices are more responsive to the size of current transfers if liquidity constraints bind, while they are more responsive to the size of future transfers if insurance constraints bind.*

**Remark** The previous model abstracts from working capital. Yet, if future capital investments were needed, future transfers would matter even in a world without risk. To see this most simply, suppose that the self-employed need to invest  $k_1$  in period 1,  $k_2$  in period 2 and they gain  $\pi_3$  in period 3. Suppose payoffs are deterministic and such that self-employment is more profitable than salaried work. If  $k_2 \geq C_2$ , future transfers are indeed important as they help to meet future liquidity needs. However, in this setting, future transfers cannot matter *more* than current transfers. In fact, individuals who invest in period 1 have  $a + C_1 \geq k_1$ , and so they would always be able to save should that be necessary to pay  $k_2$ . Hence, both increasing  $C_1$  and increasing  $C_2$  would have the same effect on helping them to finance period 2 investment and so become self-employed.

## 5 Liquidity and Insurance Constraints: Evidence

In this section, we empirically explore the mechanisms outlined above by taking advantage of a second source of variation. As described in Section 2, treated households differ in the magnitude and time profile of the transfers they are entitled to, as determined by the number, grade in school and gender of their children. We can then test how individual  $i$ 's probability of becoming an entrepreneur at time  $t$  depends on the cumulative amount of transfers received by the household in the previous period and on the transfers known to be received in the next period.

## 5.1 Empirical Strategy

In what follows, we restrict our attention to eligible individuals who reside in treated villages. In order to take the theoretical predictions to the data, we need to specify the time span of “current” and “future” periods. Given the structure of the program, individuals are entitled to receive transfers for several years. Nonetheless, we wish to focus on transfers to be received in the near future. The further away are future transfers, the more confounded their impact on occupational choices may be, so it may be more difficult to relate current occupational choice to the transfers to be received in a few years than to those to be received in a few months.<sup>14</sup> At the same time, the chosen time period cannot be too short. Since transfers vary with the school calendar year, future transfers need not be systematically different from current transfers if we consider a period shorter than six months. For these reasons, in most of our analysis, we focus on a six-month period. In robustness checks, we consider different time horizons.<sup>15</sup>

Beside being possibly measured with error, the actual amounts received partly depend on households’ behaviors in complying with the program’s conditions, and these are likely to be simultaneously determined with occupational choices. We thus define potential transfers  $P_{h,t}$  and  $P_{h,t+1}$  as the amount of transfers a household would be entitled to, according to the rules described in Section 2, assuming that its children did not change their pre-program enrollment decisions and, when enrolled, progressed by one grade in each year. These transfers are deterministic functions of children’s characteristics at the baseline and by construction they are uncorrelated with any behavioral response to the program.

Motivated by Proposition 1, we then consider (variations of) the following empirical model:

$$ne_{i,t} = \beta_1 P_{h,t+1} + \beta_2 [P_{h,t+1} + P_{h,t}] + Child'_{h,t} \beta_3 + \eta_{i,t}. \quad (3)$$

Our coefficient of interest is  $\beta_1$ , which measures the differential impact of future vs. current transfers on the probability of becoming an entrepreneur (i.e.  $\partial ne_{i,t} / \partial P_{h,t+1} - \partial ne_{i,t} / \partial P_{h,t}$ ). The vector  $Child_{h,t}$  contains age-specific categorical variables for the number of boys and girls who are between 6 and 17 years old in each household  $h$  and post-

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<sup>14</sup>In addition, unreported results show that for those who become entrepreneurs in survey wave  $t$ , there is a drop in expenditures and consumption in wave  $t$  and a recovery already in wave  $t + 1$ , which is six months afterward. This suggests that the time lag between the occupational choice and its (initial) payoffs, which corresponds to period 1 and period 2 in our model, is less than six months.

<sup>15</sup>Since we do not know exactly the date on which individuals have changed occupation between two survey waves, current and future transfers are constructed by taking the month of the interview as the reference. It follows that our future amounts are certainly received after individuals have changed occupation, while part of our current amounts may sometimes still be due at the time in which they switch occupation. If this were the case, our estimates on the differential effects of future vs. current transfers should be interpreted as a lower bound.

treatment period  $t$ , which controls for any independent effect of children demographics on occupational choices. In order to take into account potential intra-household correlation, standard errors are clustered at the household level.

In equation (3), both the level and the time profile of potential transfers may vary across households with identical compositions in terms of children age and gender since these children may differ in their attainment level or enrollment status at the baseline. Indeed, in terms of attainment, due to grade repetition and/or early enrollment in school, on average 65% of the students enrolled within the seven program grades are either younger or older than they would be had they started school at the age of six and proceeded thereafter without setback. Also, in terms of enrollment status, about 90% of children at the baseline are enrolled at the primary level, but only 60% of the boys and 48% of the girls are enrolled at the junior secondary level. Notice also that, since households typically have several eligible children, in many instances these sources of variations in transfers may co-exist within the same family.<sup>16</sup>

These patterns are represented in Figure 1, which reports a scatter plot of per-child monthly educational transfers a household is entitled to receive as a function of the age, gender and baseline schooling status of the child. As described in Section 2, monthly scholarships amount to 81 Pesos per child enrolled in the third grade, and they increase for each grade up to the ninth. Hence, a child who starts school at the age of six and progresses by one grade per year is entitled to receive 81 Pesos at the age of eight and then an increasing amount of transfers up to age fourteen. This is represented by the solid line which we call “theoretical”. Deviations from these theoretical amounts come from children who are not enrolled in school at the baseline, as described by the dots associated with zero amounts, and by children of a given age attending different school grades at the baseline, as described by the remaining dots. Moreover, as documented in Figure 2, cross-household differences in children demographics and their baseline schooling status induce considerable variations in the time profile of potential transfers, which indeed allow us to separately identify  $\beta_1$  and  $\beta_2$  in equation (3).

## 5.2 Results

We first provide a visual inspection of our relationships of interest. In Figure 3, we plot the effects of current and future transfer amounts on the probability to become an entrepreneur, estimated with non-parametric local linear regressions. The shape of the curves suggests that the amount of transfers received in the previous six months does not have any effect on the probability of becoming an entrepreneur. On the contrary, this

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<sup>16</sup>On average, households who are entitled to the educational scholarship have 3.5 children (less than 18 years old), of which about 2 are eligible to receive the scholarship in a given year.

probability seems to depend positively on the amount of transfers that households are entitled to receive in the next six months.

These patterns are confirmed in the OLS estimates reported in Table 4. Column (1) displays the results for the amounts received in the previous six months, which reveal no significant effects. According to column (2), instead, the effect of the transfers a household is entitled to receive in the next six months appears significant and large: a one-standard deviation increase in future transfers increases the average probability of becoming an entrepreneur by 0.5%.<sup>17</sup> This amounts to an 11% increase vis-a-vis the average share of new entrepreneurs in this sample (4.6%). We then directly estimate the differential impact of current vs. future transfers, as measured by the coefficient  $\beta_1$  in equation (3). As reported in column (3), the estimated coefficient is positive and significant, which shows that the probability of becoming an entrepreneur is significantly more responsive to the amount of future transfers than to the amount of current transfers.

A key issue in the interpretation of the above findings is that the underlying relationship between occupational choices and households characteristics may confound the effects of transfer amounts.<sup>18</sup> Our key identifying assumption is that, absent the program, occupational choices respond to children's demographics and not to their baseline attainment level or enrollment status. We test this assumption by looking at two alternative samples: program-eligible households living in control villages and non-eligible households living in treated villages. We construct the transfers they would have been entitled to had they been treated, and estimate the effect of these placebo potential transfers on occupational choices. As shown in columns (4) and (5) of Table 4, there are no effects of  $P_{h,t+1}$  and  $P_{h,t}$  in these samples. This allows us to interpret the estimates in columns (1)-(3) as the result of the transfers induced by *Progresa* and not of the specific household characteristics which determine them.

We further perform some specification checks. We first investigate whether our estimates may be driven by some underlying relationship between the total amount of transfers received and to be received from the program and their time profile over a six-month horizon. In column (1) of Table 5, we consider the total amount of transfers potentially received between March 1998 and September 2000. This corresponds to the longest time period for which we can compute potential transfers without making further assumptions on the schooling decisions of those who were five years old at the baseline. The coefficient associated with total amounts is very small and not significant (as one would expect in a setting in which future wealth cannot be pledged for current wealth),

<sup>17</sup>The average potential transfers received in the past six months are 1,448 Pesos (std. dev. 856) and the average potential transfers to be received in the next six months are 1,548 Pesos (std. dev. 954).

<sup>18</sup>If the very same characteristics which determine the profile of transfers also determine occupational choices, it would not be possible to separately estimate the two in equation (3).

while the estimated coefficient on the differential impact of future vs. current transfers barely changes.<sup>19</sup> In columns (2) and (3), we check the robustness of our findings with respect to the definition of transfer horizon. We consider households' response to transfers which have been received in the past year and to those to be received in a year (as opposed to six months as in our main specification). The results are consistent with the previous ones: current transfers do not matter while future transfers do, even though estimates tend to be less precise (as one would expect, the longer the transfer horizon the noisier our relation of interest). In magnitude, the relative effects for one-year future transfers are comparable to those for six months: a one-standard deviation increase leads to 0.4% more self-employed, which corresponds to a 9% increase.

These results suggest that the time profile of the transfer is key for explaining occupational choices in this setting. Based on our previous occupational choice model, this can be interpreted as evidence of binding insurance constraints, whereby cash transfers promote entrepreneurship by making individuals more willing to take risks as opposed to simply allowing them to incur some start-up investment.

In order to gain further support for this interpretation, we construct a measure of the risk that entrepreneurs face in their local market. The variable Risk(village) is defined as the ratio of the variance of labor income for the entrepreneurs vs. salaried workers at the village level, based on pre-program income statements. As the relevant local market need not coincide with the village (for example, entrepreneurs could trade in neighboring villages), we construct a similar measure based on the relative variance of entrepreneurial income in other evaluation villages situated within 10 kilometers of each village.<sup>20</sup> As we see in columns (4)-(5) of Table 5, irrespective of the definition of the relevant local market, individuals are significantly more reluctant to become entrepreneurs in areas where entrepreneurial returns are more volatile. These are indeed the settings in which the treatment appears to have the largest effects on occupational choices.

## 6 Medium-Term Impacts

Our previous analysis focused on the short-term impact of the program on the probability to become entrepreneur. A key question for a broader understanding of the effects of the

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<sup>19</sup>This result is robust to alternative definitions of total transfers. For example, we have considered a (rough) measure of the total income shock a household expects to receive in the long run based on its baseline composition (including children of all ages and making standard assumptions on their schooling behavior).

<sup>20</sup>We define neighborhoods using simple geodesic distances from each evaluation village. In our sample, 80% of the villages have at least another evaluation locality situated within 10 kilometers. The average variance ratio within a village is 1.48 (std. dev. 1.99) and the average variance ratio within 10 kilometers from each village is 1.43 (std. dev. 1.79).

program transfers is whether *Progresa* induces persistent changes in entrepreneurial activities. In our setting, this question can be addressed by exploiting an additional round of the Household Evaluation Survey collected in 2003. While we cannot compare the original treated and control groups (as the latter started receiving the program benefits since November 1999), the survey contains information not only on the original 506 evaluation localities but also on a new group of 152 localities that were not incorporated into the program as of 2003. The new localities were selected so as to closely match the evaluation localities according to a number of socioeconomic indicators (which include poverty levels, demographic characteristics and labor market conditions). For the individuals who reside in those communities, we rely on recall data on their socioeconomic characteristics (including their main occupation) in 1997.<sup>21</sup> We then obtain a longitudinal database of 17,661 program-eligible adult individuals. Among them, 66% are receiving the treatment in 2003 (either since four or since five and a half years), while the remaining 34% represent the new control group.

Households in the new control group come from different geographic areas than the original evaluation sample. Hence, they may have experienced different (labor market) conditions, and this may have affected their occupational choices. To take such differences into account, we first compare longitudinal changes in the share of entrepreneurs between localities in the original evaluation sample and in the new control group. More specifically, we consider the following difference-in-differences specification:

$$e_{l,t} = \gamma_1 T_l + \gamma_2 d_t + \gamma_3 [T_l \times d_t] + z_{l,t}, \quad (4)$$

where  $e_{l,t}$  is the share of entrepreneurs in village  $l$  in period  $t$ ;  $T_l$  is a dummy equal to one if locality  $l$  belongs to the original evaluation sample;  $d_t$  is a dummy equal to one for the 2003 round of data and zero for the 1997 baseline. We also include state dummies and cluster standard errors at the locality level to account for potential serial correlation.<sup>22</sup> In this specification,  $\gamma_1$  captures any time-invariant difference between the two groups of localities,  $\gamma_2$  any time trend which is common across groups, and  $\gamma_3$  measures the average longer-term impact of the program on the local share of entrepreneurs.

We next compare individual transitions into entrepreneurship between 1997 and 2003 across the two groups. Since the new control group was selected according to socio-

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<sup>21</sup>Retrospective data for 1997 were only collected in the new control communities, so we cannot directly compare them with the actual information collected in 1997. Behrman et al. [2011], however, provide some indirect test of recall bias in this setting. We also notice that our subsequent results for the average share of entrepreneurs across villages require a somewhat weaker assumption than classical measurement error; it suffices that any potentially non-classical recall error cancels out within each village.

<sup>22</sup>We weight each village-level observation by the relative population size in order to better compare the resulting estimates with those at the individual level reported thereafter.



economic characteristics aggregated at the community level, we use matching methods to take into account differences in the support and in the distribution of pre-program individual and household characteristics between the two groups. While analogous to standard difference-in-differences, this approach does not impose functional form restrictions when estimating the conditional expectation of the outcome variable, and it re-weights the observations according to individual probability (propensity) to participate in the program (see, e.g., Heckman et al. [1998]). We refer to Appendix Table A.1 for standard indicators of covariate balancing, and here only notice that the after-matching distribution of the covariates is well balanced, thereby suggesting that the propensity score model is correctly specified and the estimator is consistent.

We report our results in Table 6. Column (1) shows a small and non-significant effect of the program on the village share of self-employed in 2003. This average estimate, however, masks significant heterogeneity. We split the sample according to the local share of entrepreneurs in 1997, based on whether the village share falls above or below the median share. As reported in columns (2) and (3), the share of self-employed decreases in treated communities with a higher pre-program share of entrepreneurs, while it increases in those with a lower pre-program share. In columns (4) and (5), we show that these results are qualitatively similar when estimating equation (4) with the matching procedure outlined above. However, while the negative effect is smaller in magnitude and not statistically different from zero, the positive effect remains statistically significant and barely changes in magnitude. Among villages with a lower share of entrepreneurs in 1997, the treatment is associated with a 2.2% higher share of entrepreneurs in 2003.

One interpretation of this finding may build on a model in which new entrepreneurs compete with existing entrepreneurs in satisfying a local demand (as, for example, in Lucas [1978]). Our aim, however, is not to explore in detail the mechanisms behind this result. Taking a more limited approach, we interpret it as evidence that the short-run response to the program documented in the previous sections appears to have some long-lasting effect on entrepreneurship, at least in a subset of villages in our sample.

## 7 Conclusion

We have explored the response of occupational choices to the income shocks induced by the Mexican program *Progresa*. We have first documented that the probability of becoming an entrepreneur increases by about 25% for treated individuals. We have then shown that the time profile of the transfer is key to explaining these effects: current occupational choices are significantly more responsive to the amount of transfers expected for the future than to those currently received. Based on a simple occupational choice

model, we have interpreted these results as evidence that cash transfers have been effective in promoting entrepreneurship, as they have induced individuals to take more risks as opposed to simply relaxing current liquidity constraints.

Our results include some limitations. For example, we have not fully addressed the possibility of general equilibrium effects induced by the program. As a first step, we have shown that indirect effects on non-eligible households in treated communities are not significant. However, much remains to be done on the extent to which the dynamics described above affect the functioning of local markets (e.g., in terms of increased labor demand or total production). Moreover, while in Section 6 we provide some evidence that our effects may be persistent, a more general analysis of the long-run impacts of *Progresas*' cash transfers is left for further investigation.

Nonetheless, we think our analysis can inform the debate on financial constraints and entrepreneurship in developing countries. First, while some skeptics question whether policy makers can promote entrepreneurship at all (see, e.g., Holtz-Eakin [2000] and Shane [2009] for a discussion), we have shown one instance in which this could be done. Indeed, in line with the evidence in Gertler et al. [2012], it appears that *Progresas*' cash transfers had a significant impact on households' income-generating activities. In addition, we have shown that liquidity constraints need not be a major barrier to entry into entrepreneurship. Instead, individuals may refrain from entering self-employment because of its risky returns. In this view, promoting entrepreneurship may require reducing households' exposure to risk in other dimensions.

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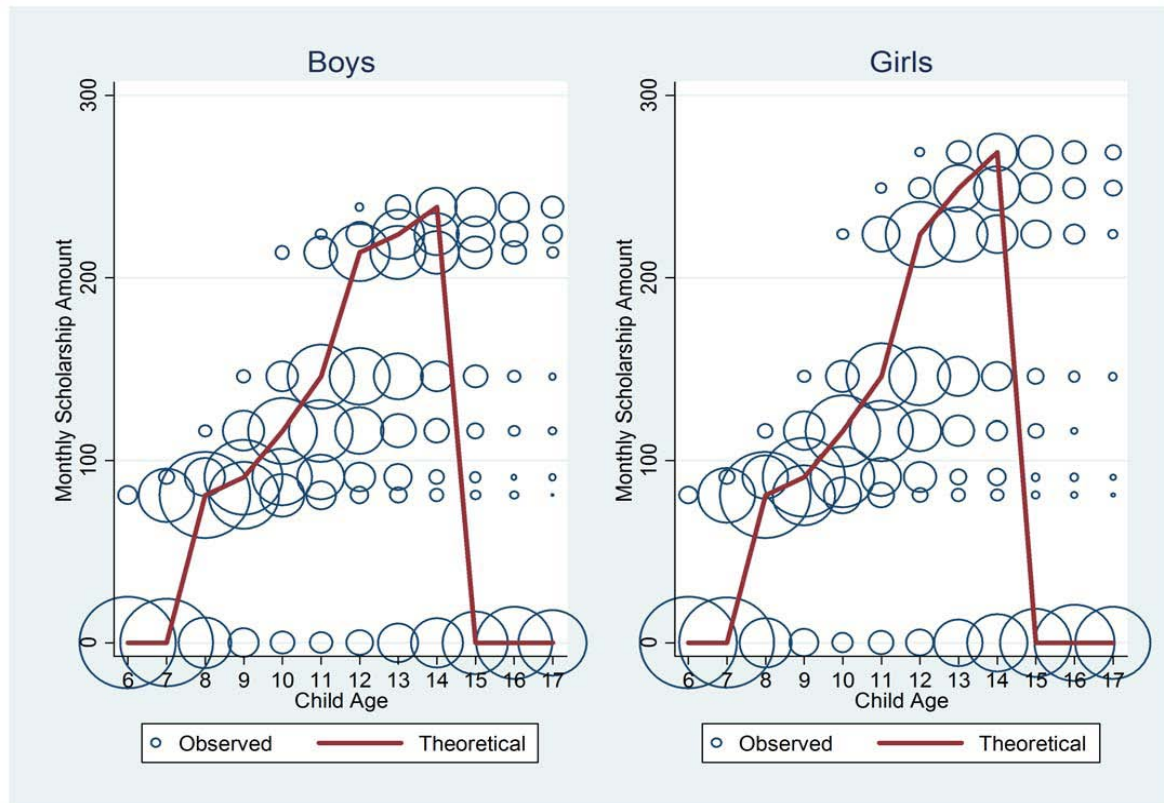
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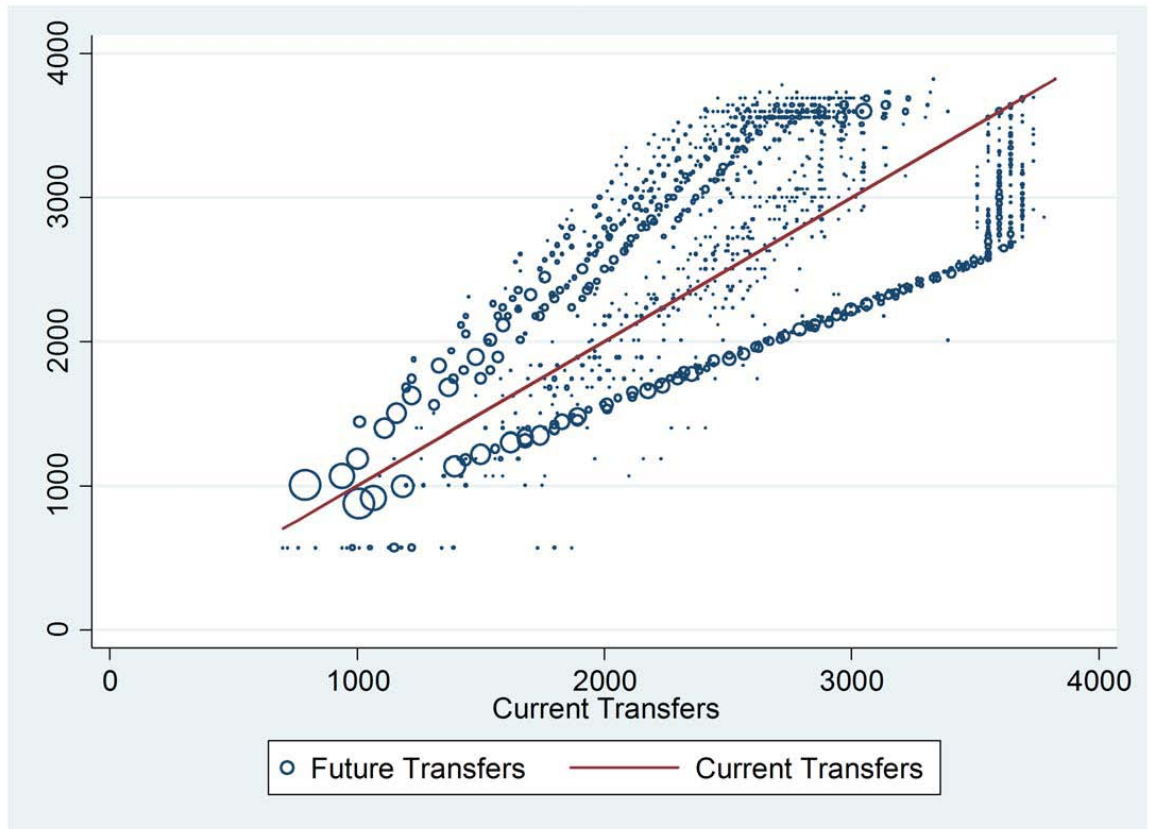
# Figures

Figure 1: Potential Transfers per Child



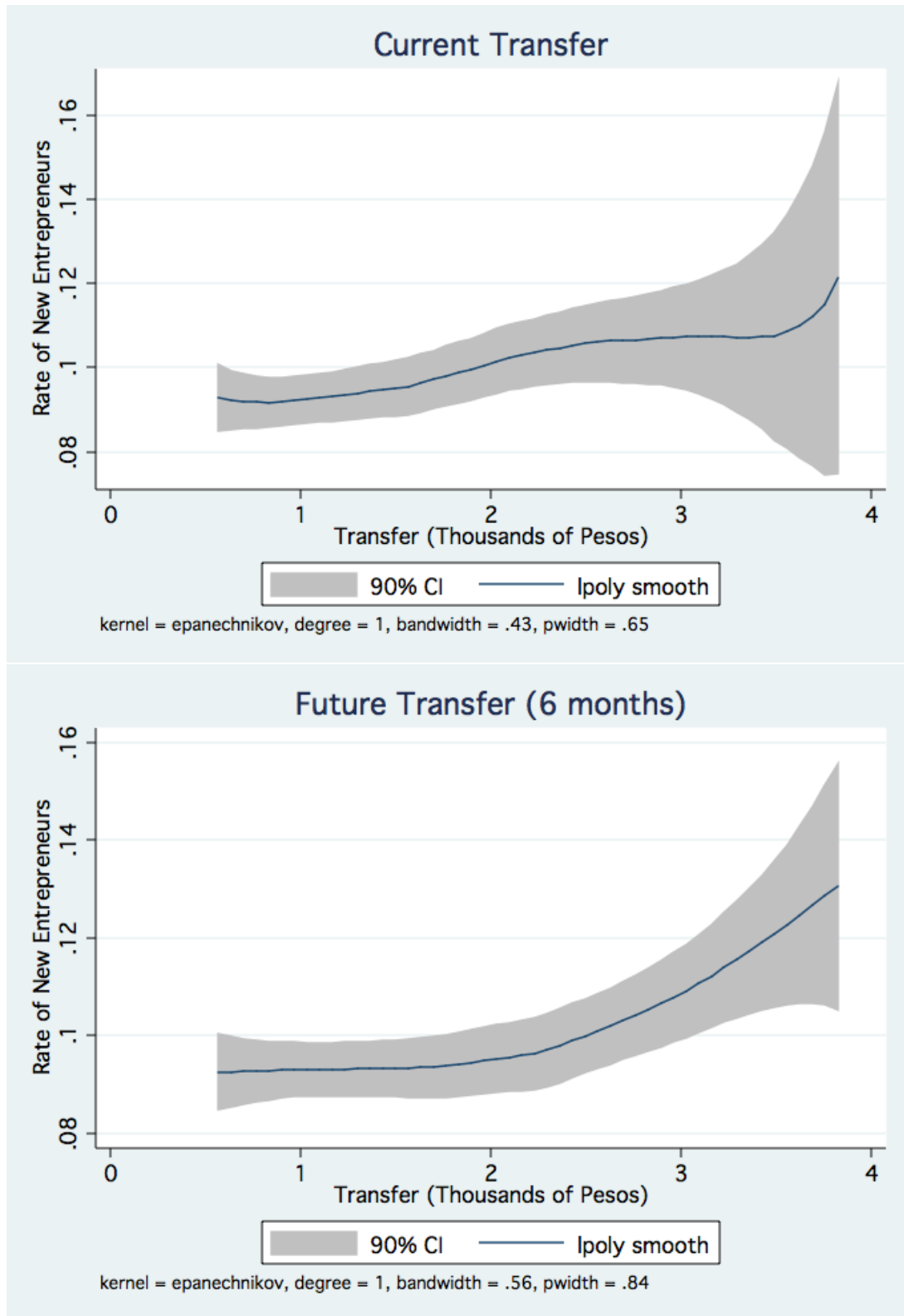
NOTE: This figure shows the variation of per-child monthly transfers a household is entitled to receive as a function of the age and the gender of the child. The solid "theoretical" line represents the monthly amount an household receives for each child who starts school at the age of six and progresses by one grade per year. Deviations from these theoretical amounts come from children who are not enrolled in school at the baseline, as described by the dots associated with zero amounts, and by children of a given age attending different school grades at baseline, as described by the remaining dots. The size of the markers has been adjusted for the relative sample frequency. Amounts are expressed in current Pesos as of the second semester of 1998.

Figure 2: Current and Future Transfers



NOTE: This figure plots future transfers  $P_{h,t+1}$  as a function of current transfers  $P_{h,t}$  as used to estimate equation (3). The size of the markers has been adjusted for the relative sample frequency. Amounts are expressed in current Pesos as of the second semester of 1998.

Figure 3: Current and Future Transfers: Non-parametric Estimates



NOTE: This figure shows non-parametric estimates (based on Local Linear Regression Smoothers) of the effect of current and future transfer amounts on the probability to become entrepreneur.



# Tables

Table 1: Baseline Characteristics and Covariate Balance

Variable	Mean	Std. Dev.	T-C Diff.	t-test	Number of Obs	
	(1)	(2)	(3)	(4)	Treat (5)	Control (6)
<b>Main Occupation</b>						
Salaried	0.392	0.488	-0.013	-1.22	12821	7949
Self-Employed	0.074	0.262	0.019	1.62	12821	7949
Unemployed	0.534	0.499	-0.005	-0.51	12821	7949
<b>Individual Characteristics</b>						
Age	39.26	13.88	-0.254	-0.65	12778	7934
Female	0.541	0.498	0.006	1.09	12819	7946
Income Main Occup.	247.5	344.5	-11.24	-1.29	12527	7805
Income Other Occup.	56.35	339.5	-4.599	-0.72	12821	7949
Hours Worked	3.780	4.112	0.049	0.51	12769	7920
Years of Education	2.707	2.628	0.068	0.51	12778	7924
<b>Household's Assets</b>						
Poverty Index	638.7	82.80	0.399	0.09	7462	4527
Land Used	1.267	2.614	-0.071	-0.76	7389	4490
Land Owned (dummy)	0.579	0.494	0.028	0.98	7452	4554
Working Animals (dummy)	0.330	0.470	0.025	1.10	7467	4557
<b>Household's Composition</b>						
Female HH Head	0.078	0.268	-0.004	-0.62	7466	4555
Children Aged 0-5	0.682	0.465	-0.003	-0.20	7467	4557
Children Aged 6-12	0.700	0.458	-0.014	-1.25	7467	4557
Children Aged 13-15	0.397	0.489	-0.011	-1.04	7467	4557
Children Aged 16-21	0.375	0.484	0.003	0.36	7467	4557
Men Aged 21-39	0.588	0.492	0.002	0.12	7467	4557
Men Aged 40-59	0.346	0.475	-0.002	-0.17	7467	4557
Men Aged 60+	0.131	0.337	0.002	0.20	7467	4557
Women Aged 21-39	0.672	0.469	-0.014	-1.11	7467	4557
Women Aged 40-59	0.301	0.459	-0.003	-0.27	7467	4557
Women Aged 60+	0.134	0.341	-0.002	-0.28	7467	4557
<b>Locality Characteristics</b>						
Number of Shocks	1.480	1.045	-0.036	-0.36	319	185
Share of Entrepreneurs	0.086	0.079	0.003	0.38	319	185
Crop Diversification	2.303	0.741	-0.014	-0.60	319	185

NOTE: This table reports baseline summary statistics for the variables employed in the empirical analysis. Columns (1)-(2) display sample means and standard deviations. In columns (3)-(4), we present estimates from OLS regressions of each baseline variable on a constant and the treatment assignment binary indicator, with standard errors clustered at the village level. In Columns (5)-(6), we report the relative number of observations in the treated and the control group.

Table 2: Probability to Become Entrepreneur: Average Program Impacts

	All (1)	Ex Salaried (2)	Ex Unempl (3)	Non-experiment (4)	Non-eligibles (5)
Treat	0.009 (0.004)**	0.015 (0.008)*	0.006 (0.003)**	0.006 (0.004)	0.004 (0.005)
Mean Dep. Var.	0.037	0.074	0.016		
Number of Obs	46271	17094	26154	35584	15148
R-squared	0.046	0.042	0.084	0.064	0.057
Number of Localities	500	492	500	501	445

NOTE: This table reports OLS estimates of the impact of program on the probability to become entrepreneur. Column (1) refers to the full sample, column (2) to former salaried and column (3) to former unemployed. In column (4), we focus on the period in which control villages have been incorporated into the program. In columns (5), we focus on individuals who are not eligible for the program. State and time dummies and the following baseline control variables are included in each specification: age, age squared, years of education, gender, income from other sources, households' demographics, income, assets (land and animals), poverty index, villages' main economic activity, agricultural shocks, crop diversification and share of entrepreneurs. Standard errors are clustered at the village level. \* denotes significance at 10%; \*\* significance at 5%; \*\*\* significance at 1%.

Table 3: Alternative Channels: Heterogeneous Program Impacts

	(1)	(2)	(3)	(4)	(5)
Treat*Non-Enroll	0.002 (0.005)				
Treat*Cont-Enroll		0.003 (0.005)			
Treat*Cont-Enroll-2			0.005 (0.006)		
Treat*Expect				0.002 (0.005)	
Treat*Other-Program					0.002 (0.007)
Treat	0.008 (0.004)**	0.008 (0.004)**	0.008 (0.004)**	0.008 (0.005)*	0.008 (0.004)**
Non-Enroll	-0.002 (0.004)				
Cont-Enroll		0.003 (0.004)			
Cont-Enroll-2			0.002 (0.005)		
Expect				0.001 (0.003)	
Other-Program					0.009 (0.006)
Number of Obs	46271	46271	46271	46271	46271
R-squared	0.046	0.046	0.046	0.046	0.047
Number of Localities	500	500	500	500	500

NOTE: This table reports OLS estimates of the program on the probability to become entrepreneur. In column (1), Non-Enroll is a dummy equal to one if the household has eligible children not enrolled in school at the baseline. In column (2), Cont-Enroll is a dummy equal to one if the household has eligible children enrolled in the last two grades of primary school at the baseline. In column (3), Cont-Enroll-2 is a dummy equal to one if the household has female eligible children enrolled in the first two grades of secondary school at the baseline. In column (4), Expect is a dummy equal to one if the household reports above the median expected attainment for its children at the baseline. In column (5), Other-Program is a dummy equal to one if the household reports receiving any transfers from other occupation-related welfare programs. State and time dummies and the following baseline control variables are included in each specification: age, age squared, years of education, gender, income from other sources, households' demographics, income, assets (land and animals), poverty index, villages' main economic activity, agricultural shocks, crop diversification and share of entrepreneurs. Standard errors are clustered at the village level. \* denotes significance at 10%; \*\* significance at 5%; \*\*\* significance at 1%.

Table 4: Current and Future Transfers

		Treated		Control	Non-poor
	(1)	(2)	(3)	(4)	(5)
Current	0.002 (0.003)				
Future		0.005 (0.003)**	0.021 (0.010)**	-0.014 (0.010)	0.013 (0.018)
Current+Future			-0.009 (0.005)*	0.008 (0.006)	-0.010 (0.010)
Number of Obs	28946	28946	28946	18273	9305
R-squared	0.018	0.018	0.018	0.016	0.016
Number of Localities	319	319	319	185	280

NOTE: This table reports OLS estimates of the effects of the transfers (in thousand Pesos) on the probability to become entrepreneur. In columns (1)-(3), the sample consists of program eligible individuals in treated villages. In column (4)-(5), which serve as falsification tests, the samples are respectively program eligible individuals in control villages and individuals not eligible to the program in treated villages. Age-specific categorical variables for the number of boys and girls between 6 and 17 years old, time and state dummies are included in each specification. Standard errors are clustered at the household level. \* denotes significance at 10%; \*\* significance at 5%; \*\*\* significance at 1%.

Table 5: Further Evidence

	(1)	(2)	(3)	(4)	(5)
Future (6 months)	0.021 (0.010)**				
Current+Future (6 months)	-0.009 (0.006)				
Total	0.001 (0.003)				
Future (12 months)		0.002 (0.001)*	0.007 (0.005)		
Current+Future (12 months)			-0.002 (0.003)		
Treat*Risk(village)				0.009 (0.004)**	
Risk(village)				-0.009 (0.004)**	
Treat*Risk(10km)					0.015 (0.005)***
Risk(10km)					-0.016 (0.005)***
Treat				0.001 (0.007)	-0.011 (0.008)
Number of Obs	28946	28946	28946	39380	44308
R-squared	0.018	0.018	0.018	0.047	0.048
Number of Localities	319	319	319	396	472

NOTE: In columns (1)-(3), we report OLS estimates of the effects of the transfers on the probability to become entrepreneur. Amounts are expressed in thousand Pesos. Control variables include: age-specific categorical variables for the number of boys and girls between 6 and 17 years old, time and state dummies. Standard errors are clustered at the household level. In columns (4)-(5), we report OLS estimates of the impact of program on the probability to become entrepreneur. The variable Risk(village) is defined as the ratio of the variance of labor income for the entrepreneurs vs. salaried workers at the village level, based on pre-program income statements. Risk(10km) is the corresponding variable based on incomes within 10 kilometers from each village. Control variables include: age, age squared, years of education, gender, income from other sources, households' demographics, income, assets (land and animals), poverty index, villages' main economic activity, agricultural shocks, crop diversification and share of entrepreneurs, time and state dummies. Standard errors are clustered at the village level. \* denotes significance at 10%; \*\* significance at 5%; \*\*\* significance at 1%.

Table 6: Medium-term Program Impacts

	All (1)	High Entrep 97 (2)	Low Entrep 97 (3)	High Entrep 97 (4)	Low Entrep 97 (5)
Treat*Post	-0.004 (0.008)	-0.031 (0.012)**	0.016 (0.006)***	-0.019 (0.017)	0.022 (0.011)**
Treat	-0.017 (0.010)*	-0.001 (0.015)	-0.011 (0.004)***		
Post	0.0001 (0.005)	-0.010 (0.008)	0.017 (0.004)***		
Number of Obs	1300	654	646	8799	8339
Number of Localities	650	327	323	327	323
R-squared	0.08	0.12	0.15		
Median % Bias				4.29	4.25
$P(Treat X)$				0.03	0.02

NOTE: In columns (1)-(3), we report Weighted Least Squares estimates of the program on the village share of entrepreneurs in 2003, with analytic weights based on village population size. In column (2), the sample is restricted to localities with above median share of entrepreneurs in 1997. In column (3), the sample is restricted to localities with below median share of entrepreneurs in 1997. State dummies are included and standard errors are clustered at the village level. In columns (4)-(5), we report local linear matching estimates (bandwidth=0.8) of the program on the individual probability to be entrepreneur in 2003. The estimator imposes common support (trimming=2%), and standard errors are computed using bootstrapping with 300 replications. Median % Bias is the median absolute standardized bias after matching and  $P(Treat|X)$  is the Pseudo R-squared from a Probit of *Treat* on all the regressors in the matched sample. \* Denotes significance at 10%; \*\* significance at 5%; \*\*\* significance at 1%.