Liquidity, Risk, and Occupational Choices*

Milo Bianchi† Matteo Bobba‡

December 2010

Abstract

We explore which financial constraints matter the 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 the 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 the willingness to bear risk as opposed to simply relaxing current liquidity constraints.

Keywords: Financial constraints; entrepreneurship; insurance; liquidity.

JEL codes: O16, G20, L26.

*We thank Abhijit Banerjee, Francois Bourguignon, Eric Edmonds, Marc Gurgand, Erik Lindqvist, Sylvie Lambert, Jean-Marc Robin, Jesper Roine for useful comments and Christian Lehmann for excellent research assistantship. The financial support of Région Ile-de-France and of the Risk Foundation (Groupama Chair) is gratefully acknowledged.

†Université Paris Dauphine; E-mail: milo.bianchi@dauphine.fr

‡Ph.D. Candidate, Paris School of Economics; E-mail: bobba@pse.ens.fr

1 Introduction

Entrepreneurship is considered a fundamental aspect in the process of development (Hausmann and Rodrik [2003]; Ray [2007]; Naudé [2010]), while at the same time being often hindered by financial constraints (Banerjee and Duflo [2005]; Levine [2005]). Hence, one way in which access to finance may promote economic development is by allowing some poor individuals the possibility to set up their own business (Banerjee [2003]; Karlan and Morduch [2009]).

Understanding the link between improved access to finance and occupational choices poses however some serious challenges. First, such an improvement seldom occurs in isolation from other changes in the economy, which makes it hard to empirically estimate its effects. Moreover, and perhaps even more fundamentally, occupational choices may be determined by several financial constraints, for example concerning households' ability to save, borrow and get insurance against income shocks. One would then like to open the box of "access to finance" and understand which of the various financial constraints binds in a given situation. This is often complicate 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 the most in the choice of becoming an entrepreneur. We first exploit a random variation in household income to show that financial constraints prevent some individuals the possibility to become entrepreneurs. We next decompose financial constraints by distinguishing in particular whether individuals refrain from becoming entrepreneurs as they lack enough liquidity to undertake some initial capital investment or rather as they lack the ability to insure their income against the risk posed by entrepreneurial returns. 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 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 start our empirical analysis by comparing households in treated and control communities; we show that living in a treated community significantly increases the probability of entering self-employment both from salaried work and from unemployment. Furthermore, after a series of test, we rule out that the fact that transfers are conditional on sending children to school may explain our results (as for example it may induce a reallocation of labor within the household). Hence, we interpret the treatment impacts as the result of income shocks and thus as (indirect) evidence that households face financial constraints.

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 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 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 entrepreneur. The transfers currently received are better suited to help incurring start-up costs and so they are more important if liquidity constraints are binding. Conversely, future transfers are better suited to provide insurance against future income drops due to business failure and so they have stronger effects if insurance constraints are binding.

We then test empirically whether it is the amount of current transfers or the amount of future transfers which matters the most in the choice of becoming entrepreneur. In order to do so, we first rule out that the very same household characteristics which determine the profile of transfers determine also occupational choices. We then show that the probability to become 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 example for the total amount of transfers received within a given time horizon.

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 the business needs), it is hard to explain that future transfers matter *more* based on liquidity constraints. This may suggest that financial barriers to entry into self-employment are not the most important obstacle in our setting (see McKenzie and Woodruff [2006] for similar evidence on micro-enterprises in urban Mexico). Instead, the possibility to better insure against future income fluctuations may be what induces some salaried individuals to undertake the risky choice of setting up a business.

This paper builds on the literature on the effects of improved access to finance on occupational choices. Exploiting income shocks, Holtz-Eakin, Joulfaian and Rosen [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, McKenzie and Woodruff [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 generally, 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 (see Banerjee et al. [2009] and Zinman and Karlan [2009] for evidence on micro-credit in India and in the Philippines, respectively, and Giné and Yang [2009] for evidence on weather insurance in Malawi). Moreover, despite liquidity and insurance constraints are often interrelated (Ray [1998]), little has been done to try separating their effects, which is the main focus of our paper. One notable exception is Dercon and Christiaensen [2007], who distinguish seasonal credit constraints from inter-temporal constraints related to risk on fertilizer adoption in rural Ethiopia. Finally, in spite of the substantial body of research related to *Progresa* and its experimental design, to our knowledge no study has explicitly looked at the effects of the cash transfer on occupational choices. Related and complementary evidence is provided in Skoufias and Di Maro [2008] who study the incentive effects of *Progresa* on adult labor supply and in Gertler, Martinez and Rubio-Codina [2006], who show that the program increased productive investments and so long-term welfare.

2 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.[1] It

[1]The program is currently ongoing under the name *Oportunidades*.

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.

Eligible households in treatment communities start receiving benefits in March-April 1998; whereas eligible households in control communities were not incorporated until November 1999.[2] Cash transfers from *Progresa* are given bimonthly to the female head of eligible households and they come in two forms. The first is a fixed food stipend of 105 Pesos per month conditional on family members obtaining preventive medical care.[3] 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% of the time and not repeating a grade more than twice. Scholarship amounts vary between 81 and 269 Pesos per month per child, they increase with school grade and, in grades seventh to ninth, they are larger for girls than for boys.[4] 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 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. 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, from October

[2]The status of eligible household is based on a welfare index built on asset holdings in the baseline and it was intended to remain unchanged for the entire duration of the program. However, around 3,000 households were classified as non-poor in the baseline but were later re-classified as eligible. In order to avoid arbitrary classifications, we exclude them from our analysis (results are unchanged once we include them).

[3]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.

[4]Specifically, an 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 trasfers are given before the third grade and after the ninth grade.

1998 to October 1999. Program take-up is remarkably high in this sample: 94% of the treated households and 96% of the control households are reported receiving positive transfers within 18 months since program offering. Sample attrition is low (11%) and non response in the occupational choice somewhat larger (17%); however, none are 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 the 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. Amongst those residing in control villages, 4% become entrepreneur 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: 11% of new entrepreneurs declare to be engaged in activities like handicraft, sewing clothes and domestic services, whereas the corresponding share for salaried workers is only 3%. Moreover, we note that 34% of new entrepreneurs have more than one paid occupation vis-à-vis 8% of salaried workers. This is common in many developing settings, 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.[5]

3 Entrepreneurship and Financial Constraints

Random treatment assignment implies that a simple comparison of treated-control mean outcomes will likely provide an unbiased estimate of the program impacts. However, we additionally control for several socioeconomic characteristics that may affect occupational choices so as to improve the power of the estimates and check the robustness of our findings. Moreover, although villages were randomly assigned to the treatment, data are unlikely to be independent across individual observations. In particular, occupational choices of individuals in the same village may be correlated as they share background characteristics and are exposed to the same market environment and natural shocks. In this section, we first introduce a standard reduced-form empirical framework to evaluate whether the exposure to the treatment induces some individuals to become entrepreneur. We then provide some additional evidence to suggest that our effects stem from individual

[5]The standard deviation of monthly labor income is 84% of the sample mean for entrepreneurs vis-à-vis 60% for salaried workers.

responses to the cash transfers rather than to the conditions (such as schooling behaviors) attached to these transfers.

3.1 Treatment Impacts

Consider an individual i who is either salaried worker or unemployed in the baseline and let $ne^*_{i,t}$ be a dummy equal to one if the individual has become entrepreneur in a given program wave t and zero otherwise. Suppose $ne^*_{i,t}$ is determined by the latent variable $ne_{i,t}$, which denotes individual i's probability of becoming entrepreneur. We estimate regressions of the following form:

$$ne_{i,t} = \alpha T_l + X'_{i,t_0}\gamma + \delta_t + \eta_s + \epsilon_{i,t}, \tag{1}$$

where T_l represents the *Progresa* experimental treatment assignment at the locality level l and the vector X_{i,t_0} denotes a set of pre-determined covariates at the individual, household and locality level: individual age, gender, education, income, spouse main occupation, household wealth and demographic composition, village shares of entrepreneurs and proxies for agricultural risk. We also include wave dummies δ_t and state dummies η_s.[6] In order to take into account the potential intra-village correlation of $\epsilon_{i,t}$ mentioned above, we cluster standard errors at the village level.

Table 2 reports probit marginal effects of the program on the transition into entrepreneurship.[7] Treatment impacts appear to be both statistically and economically significant. As shown in columns (1) and (2), living in a treated community increases the probability of entering self-employment by 0.7 percentage points. This represents an increase of 19% with respect to the counterfactual sample averages (equal to 4%). In columns (3)-(6), we show that the treatment significantly increases the probability of entry into entrepreneurship both from salaried work and from unemployment. In relative terms, the effects across subsamples are of comparable magnitudes: having access to this stable source of extra income increases the likelihood to become entrepreneur of about 20% in the program period.

As further evidence that the above results are due to the treatment, we also include the period in which control villages are incorporated into the program (survey waves 4 and 5), and we slightly modify equation (1) so as to allow for interaction effects of the treatment indicator with each survey wave dummy. The results provided in Table 3 (columns 1-2) show that indeed treated-control differences tend to vanish once the control group is incorporated. We also investigate whether our results may be driven by a pure demand

[6] We cannot specify fixed effects at a more disaggregated geographical level -say, municipality or village- since this would imply loosing the exogenous variation induced by the experiment.

[7] All our results are robust if we instead use a Linear Probability Model.

effect, whereby treated villages are richer and so have a higher demand for entrepreneurs. If this were the case, the treatment effect would hold for all households in a treated village, whether eligible for the program or not. The results provided in columns (3) and (4) do not support this hypothesis: there are no treated-control differences for non-eligible individuals. It appears that being entitled to receive the treatment, as opposed to simply living in a treated village, is what drives our effects. This result also tends to exclude within-village spillovers between eligible and non-eligible households in the choice to become entrepreneur.

3.2 Conditionality

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, mothers may have to quit a salaried job and turn to self-employment in search for flexible working hours or home working. This however seems unlikely to drive our results. First, among those who were salaried and became entrepreneurs in treated villages, only 6% have children who returned to school in the program period, of which only 5% are women. Moreover, as shown in Table 4, we find no differential program impacts on individuals for whom, according to a series of pre-program characteristics, we expect conditionality to be more or less binding.[8]

We also notice that if individuals were pushed into entrepreneurship because of conditionality, we would expect their labor supply to change and in general their welfare to decrease. The results presented in Table 5, however, offer little support to this hypothesis. Despite these estimates should be interpreted as simple correlations, as the choice of becoming entrepreneur is itself dependent on the treatment, they show that new entrepreneurs in treated villages have significantly higher labor earnings and higher non-food expenditures (columns 1-2), not significantly different food consumption and labor supply (columns 3-5), and they are less likely to be engaged in a second paid occupation (column 6).[9]

[8]Specifically, we consider those who were working longer hours at the baseline, females, those who had eligible children not enrolled in school (who had to actually change their behavior in order to receive the treatment), those who had eligible children only in primary school age vs. those who had female children in secondary school age (enrollment in primary school is very high irrespective of the treatment, while the treatment has a bigger effect on female secondary schooling; see Schultz [2004]).

[9]These results come from estimating, for each of output $y_{i,t}$, the parameter γ in the following equation:

$$y_{i,t} = \alpha T_l + \beta ne_{i,t} + \gamma T_l * ne_{i,t} + X'_{i,t_0}\lambda + u_{i,t}.$$

A similar strategy is used to get a sense of how new entrepreneurs invest the money. We notice in Table 6 that there is no evidence of increased investment in agricultural activities, such as acquisition of land,

Taken together, this evidence tends to rule out that conditionality is driving our results, so we can interpret the program impacts as the result of an income shock and thus as (indirect) evidence that households face financial constraints.

4 Liquidity and Insurance Constraints

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 households' 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 households' ability to cope with future income fluctuations. In this section, we first develop a simple model to highlight how liquidity and insurance constraints respond differently to the time profile of expected income shocks. We show that, under standard assumptions, the choice of becoming entrepreneur is more responsive to the amount of transfers currently received if liquidity constraints are binding, while it is more responsive to the amount of transfers expected for the future if insurance constraints are binding. We then empirically explore these mechanisms by taking advantage of a second source of variation. As described in Section 2, beyond random treatment assignment, households differ in the magnitude and time profile of the transfers they are entitled to, as determined by the number, grade and gender of their children. We can then test whether new entrepreneurs are more responsive to the amount of money currently received or to those expected for the near future.

4.1 A Simple Occupational Choice Model

Consider a population of individuals who are heterogeneous in their initial wealth a and in their risk aversion r, drawn respectively by smooth distributions F and G with density f and g. 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 become salaried. In addition, they choose the amount of wealth they wish to transfer from period 1 to period 2. We denote with s^e the amount of savings decided by an individual in case he becomes entrepreneur and with s^w the amount decided in case

animals or agricultural expenditures or production. On the other hand, there is evidence of increased nonagricultural activities, in particular carpentry and handicraft.

he becomes a worker. We do not allow borrowing and so impose

$$s^w \geq 0 \text{ and } s^e \geq 0, \tag{2}$$

and we normalize the returns to saving to one.[10] In the second period, individuals enjoy the returns from their occupation. The self-employed get y with probability p and zero otherwise; salaried workers get a fixed wage w, where

$$py - k > w. \tag{3}$$

Savings and occupation are chosen in order to maximize

$$u(x_1) + E[u(x_2)],$$

where $E[.]$ is the expectation operator and x_1 and x_2 denote consumption in period 1 and 2. We make the standard assumption that u exhibits decreasing absolute risk aversion (DARA) 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.

An individual becomes entrepreneur if his expected utility exceeds what he would enjoy as a worker, where this difference writes

$$D = u(a-k-s^e+C_1)+pu(s^e+y+C_2)+(1-p)u(s^e+C_2)-u(a-s^w+C_1)-u(s^w+w+C_2).$$

As 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 entrepreneurs.

We are interested in exploring how the equilibrium share of self-employed, denoted with ne, varies with the transfers C_1 and C_2. Using the envelope theorem, it can be shown that

$$\frac{dD}{dC_1} = u'(a - k - s^e + C_1) - u'(a - s^w + C_1), \tag{4}$$

and

$$\frac{dD}{dC_2} = pu'(s^e + y + C_2) + (1 - p)u'(s^e + C_2) - u'(s^w + w + C_2). \tag{5}$$

4.1.1 Equivalence between current and future transfers

To set a benchmark, consider first those individuals for whom borrowing constraints in (2) do not bind. These individuals set s^w such that their marginal utility is equalized

[10] Our results would hold with less extreme assumptions on borrowing constraints. The case of saving constraints is discussed at the end of the section.

across periods, i.e.

$$u^{'}(a - s^{w} + C_1) = u^{'}(s^{w} + w + C_2), \tag{6}$$

and in the same way they choose s^e such that

$$u^{'}(a - k - s^{e} + C_1) = pu^{'}(s^{e} + y + C_2) + (1 - p)u^{'}(s^{e} + C_2). \tag{7}$$

Substituting (6) and (7) into (4) and (5), we can see that

$$\frac{dD}{dC_1} = \frac{dD}{dC_2}, \tag{8}$$

and so their occupational choice respond in the same way to current and future transfers. This result is not surprising. Those individuals who can optimally allocate wealth across periods see no fundamental difference between the transfers they have received today and those they known they will receive tomorrow.

We do not expect however this to be the case for everyone in our population. Borrowing constraints are widely documented (restricting to developing countries, see the surveys in Banerjee [2003] and Karlan and Morduch [2009]), and these constraints break the equivalence between current and future transfers. We can then compare the effects of these transfers in two extreme cases: one in which there are only liquidity constraints ($k > 0$ and individuals are risk neutral) and one in which there are only insurance constraints ($k = 0$ and individuals are risk averse).

4.1.2 Liquidity constraints

Consider first the case in which all individuals are risk neutral. In this case, due to (3), everyone would like to become entrepreneur but only those with $a \geq k - C_1$ can do so. Hence, we would have $ne = 1 - F(k - C_1)$ and so

$$\frac{dne}{dC_1} = f(k - C_1) > 0 \text{ and } \frac{dne}{dC_2} = 0. \tag{9}$$

The total effect of changing C_1 and C_2 in this setting depends on the fraction of the population who can optimally choose its savings, for which equation (8) holds, and the fraction who face binding borrowing constraints, for which equation (9) holds. Still, by (8) and (9), we can say that in general the share of self-employed in period 1 is more responsive to the amount of period 1 transfers than to period 2 transfers. Current transfers help to overcome liquidity needs, while future transfers may not be pledged for obtaining cash in period 1 so as to incur the investment.

4.1.3 Insurance constraints

We now abstract from liquidity constraints by assuming $k = 0$. In this case, all individuals who are sufficiently tolerant toward risk become self-employed, i.e. $ne = G(r^*)$. As in the previous section, those who can optimally decide their savings see no difference between current and future transfers. To see the effects on those who face binding borrowing constraints, instead, notice that for $s^w = 0$ it must be that

$$u^{'}(a + C_1) > u^{'}(w + C_2), \tag{10}$$

and for $s^e = 0$ it must be that

$$u^{'}(a + C_1) > pu^{'}(y + C_2) + (1 - p)u^{'}(C_2). \tag{11}$$

These individuals, even by not saving, are consuming too little in the first period (as they would like to borrow). Consider first those for whom both (10) and (11) hold. By substituting $s^e = s^w = 0$ into (4) and (5), we see that for them

$$\frac{dne}{dC_1} = 0 \text{ and } \frac{dne}{dC_2} = g(r^*)\frac{dr^*}{dC_2} > 0, \tag{12}$$

where $dr^*/dC_2 > 0$ follows from the fact that u is DARA and so increasing C_2 increases risk-taking through a classic wealth effect (Pratt [1964]). Those for whom only (10) holds set $s^e > 0$ and $s^w = 0$ and they too are more responsive to future than to current transfers. This can be shown by substituting $s^w = 0$ into (4) and (5) and combining (7) and (10).[11]

As in the previous section, the effect of changing C_1 and C_2 depends on the fraction of the population who can optimally set its savings and the fraction for whom borrowing constraints bind. Still, we can say that, in this setting, the share of self-employed in period 1 is more responsive to period 2 than to period 1 transfers. The reasons is that, in order to self-insure, households need to dispose of enough wealth in period 2. Those with binding borrowing constraints consume all their wealth in period 1 (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 summary of the above results, we state the following Proposition.

[11]Notice that no individual who is marginal in the occupational choice sets $s^e = 0$ and $s^w > 0$. In fact, due to DARA utility, we have $u^{'}(s + w + C_2) < pu^{'}(s + y + C_2) + (1 - p)u^{'}(s + C_2)$ when $E = 0$. Hence, imposing $s^w = s^e = s$ would imply that this individual saves too much when salaried and so he would always set $s^e \geq s^w$.

Proposition 1 *Suppose individuals face constraints in 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.*

As a final remark notice that, for simplicity of exposition, we have modeled constraints in allocating transfers across periods only as borrowing constraints. It may also be the case that households face saving constraints, as the result of present-biased preferences (Ashraf, Karlan and Yin [2006], Dupas and Robinson [2009], Banerjee and Mullainathan [2010]), social norms (Platteau [2000]), or simply unavailability of a safe storage technology (Collins et al. [2009] and Karlan and Morduch [2009]). As intuitive, Proposition 1 would hold also in this case. In fact, in case of liquidity constraints, saving constraints are never binding so equation (8) applies; in case of insurance constraints, individuals cannot transfer wealth from period 1 to period 2 and so they are insensitive to C_1.

4.2 Empirical Strategy

In what follows, we restrict our attention to eligible individuals who reside in treated villages and were salaried at the baseline.[12] We are then interested in evaluating how individual i's probability to become 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. In most of our analysis, we define such a period as six months since that corresponds to the shortest time frame such that future transfers are systematically different from current transfers according to the school calendar year. In robustness checks, we consider different time horizons.[13]

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

[12] Unemployment seems unlikely to be driven by risk preferences in this setting.

[13] Since we do not know exactly the date in 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.

program. Motivated by Proposition 1, we then consider the following empirical model which employs alternatively current and future potential transfers as explanatory variable:

$$ne_{i,t} = \alpha_1 P_{h,t} + Child'_{h,t}\beta_1 + \epsilon_{i,t}, \tag{13}$$

$$ne_{i,t} = \alpha_2 P_{h,t+1} + Child'_{h,t}\beta_2 + u_{i,t}. \tag{14}$$

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-treatment period t, which controls for any independent effect of children demographics on occupational choices. We further directly test whether current and future transfers have a differential impact on the probability to become entrepreneur and so estimate:

$$ne_{i,t} = \alpha_3 P_{h,t+1} + \alpha_4 \left[P_{h,t+1} + P_{h,t}\right] + Child'_{h,t}\beta_3 + \eta_{h,t}. \tag{15}$$

In this specification, our coefficient of interest is α_3, which measures the *differential* impact of future vs. current transfers (i.e. $\partial ne_{i,t}/\partial P_{h,t+1} - \partial ne_{i,t}/\partial P_{h,t}$).

In equations (13)-(15), 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 have typically several eligible children, in many instances these sources of variations in transfers may co-exist within the same family.[14]

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 baseline, as described by the dots

[14]On average, each household has 3.5 children (less than 18 years old), of which about 2 are eligible to receive the educational scholarship in a given year.

associated with zero amounts, and by children of a given age attending different school grades at the baseline, as described by the remaining dots. In order to shed light on the relative importance of these different sources of variation, the figure reports larger dots for observations with higher sample frequency.

The key identifying assumption for estimating the effects of $P_{h,t+1}$ and $P_{h,t}$ in the above specifications is that, absent the program, occupational choices respond to children's demographics and not to their baseline attainment level or enrollment status. In order to test this assumption, we look at two alternative placebo 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 Table 7, there are no effects of $P_{h,t+1}$ and $P_{h,t}$ in these samples. We can then rule out that the very same household characteristics which determine the profile of transfers determine also occupational choices and so proceed in estimating the effects of current and future transfers among treated households.

4.3 Results

In order to first provide a visual inspection of our relationships of interest, we estimate equations (13) and (14) non-parametrically using local linear regressions. As shown in Figure 2, the shape of the curves suggests that transfers received in the previous six months do not have any effect on the probability to become entrepreneur. On the contrary, this 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 standard probit estimations. In Table 8, we report the marginal effects of current and future cash transfers (in thousand Pesos) on the likelihood to switch from salaried work to self-employment. 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 to become entrepreneur by 1.2%.[15] This amounts to a 12% increase vis-à-vis the average share of new entrepreneurs in this sample (9.6%).

In order to directly estimate any differential impact of current vs. future transfers, we then turn to the specification (15), in which such differential effect is captured by the coefficient α_3. As shown in column (3), α_3 is positive and significant, which shows that

[15]The average potential transfers received in the past six months are 1446 Pesos (std. dev. 863) and the average potential transfers to be received in the next six months are 1553 Pesos (std. dev. 964).

the probability to become entrepreneur is significantly more responsive to the amount of future transfers than to the amount of current transfers. We then test whether this pattern holds once we control for the total amount of transfers received in the previous six months and on those to be received in the next six months. In column (4), we show that an increasing stream of transfers is more likely to promote entrepreneurship, which confirms the result in column (3). Also, having higher total transfers increases the probability to switch to self-employment (column 5). However, including both the total amount of transfers and their time profile, we see that only the latter matters (column 6). Future transfers matter more than current transfers even when we control for their total amount.

We further perform some specification checks. We first investigate whether our estimates may be driven by some specific relation between the total amount of transfers received and to be received from the program and the time profile of these transfers. In column (1) of Table 9, we consider the cumulative amounts received in the last year (as opposed to six months as in the previous specification) and see no significant change. In column (2), we consider the total amount of transfers potentially received between March 1998 and September 2000, which we call the medium run. 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. In column (3), we control for the total amount of transfers potentially received according to the baseline children demographics (including children of all ages) assuming that all children go to school at the age of six, they progress one grade per year, and the program continues unchanged throughout. This is a (rough) measure of the total income shock an household expects to receive in the long run based on its baseline composition. The coefficients associated to these total amounts are very small and not significant (as one would expect in a setting in which future wealth cannot be pledged for current wealth), while the estimated coefficients on the time profile of the transfers barely change.

Finally, in columns (4)-(6), we define the time profile with a one year horizon instead of six months. Results are consistent with the previous ones: current transfers do not matter while future transfers do, even though estimates tend to be less precise. In magnitude, the relative effects for one year future transfers are similar to those for six months: a one standard deviation increase leads to 0.9% more self-employed, which is a 10% increase.

Taken together, these results suggest that the time profile of the transfer is key for explaining occupational choices in our setting. As further suggestive evidence, we notice that the magnitude of these effects is consistent with the reduced-form treatment impacts described in Section 3, in spite of the fact that they arise from two potentially different sources of variation. For salaried individuals in treated villages, the treatment increases

the probability to become entrepreneur by 1.5% with respect to the control group (see Table 2, column 4), while a standard deviation increase in future transfers increases such probability by 1.2%.

5 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 to become entrepreneur increases by about 20% for treated individuals. We have then shown that current occupational choices are significantly more responsive to the amount of transfers expected for the future than to the amount of transfers currently received. Moreover, according to our estimates, the differential impact of future vs. current transfers is comparable in magnitude to the treated-control difference, which confirms that the time profile of the transfers is key in explaining the program effects. We have interpreted these results as evidence that in our setting the cash transfers have been effective in promoting entrepreneurship as they have enhanced the willingness to bear risk as opposed to simply relaxing current liquidity constraints.

Our results feature some limitations. For example, little is known on the long run effects of these dynamics. As mentioned, a study by Gertler, Martinez and Rubio-Codina [2006] shows that productive investments induced by *Progresa* had persistent effects on individual welfare. We conjecture that changes in occupational choices are likely to display similar features, but a detailed analysis of this issue is left to further investigation. Moreover, 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, we cannot say whether the above described dynamics are only improving the welfare of those who have changed occupation (as it appears in Table 5), or they are also altering the functioning of some markets (e.g. in terms of increased labor demand or total production).

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], Parker [2007], Shane [2009] for a discussion), we have shown one instance in which this could be done. Second, according to our estimates, financial barriers to entry into entrepreneurship do not seem insurmountable. Instead, a major barrier may come from the risky prospects self-employment offers. In this view, promoting entrepreneurship requires reducing households' exposure to risk in other dimensions.

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Tables and Figures

Table 1: Baseline Characteristics and Covariate Balance

Variable	Mean	Std. Dev.	T-C Diff.	t-test
Main Occupation				
Self-Employed	0.074	0.262	0.019	1.62
Unemployed	0.534	0.499	-0.005	-0.51
Salaried	0.392	0.488	-0.013	-1.22
Individual Characteristics				
Age	39.263	13.877	-0.254	-0.65
Female	0.541	0.498	0.006	1.09
Income Main Occup.	247.445	344.452	-11.243	-1.29
Income Other Occup.	56.354	339.52	-4.599	-0.72
Labor Supply	20.054	23.148	-0.002	-0.01
Years of Education	2.707	2.628	0.068	0.51
Household's Assets				
Asset Index (Score)	638.14	82.489	0.399	0.23
Land Used	1.219	2.697	-0.071	-0.62
Land Owned	0.561	0.496	0.028	0.97
Working Animals	0.318	0.466	0.025	1.10
Household's Composition				
Female HH Head	0.048	0.213	-0.004	-0.46
child05	0.700	0.458	-0.003	-0.19
child612	0.708	0.455	-0.014	-1.20
child1315	0.394	0.489	-0.011	-0.76
child1621	0.370	0.483	0.003	0.35
men2139	0.606	0.489	0.002	0.16
men4059	0.352	0.478	-0.002	-0.17
men60	0.128	0.334	0.002	0.11
women2139	0.692	0.462	-0.014	-0.74
women4059	0.295	0.456	-0.003	-0.43
women60	0.125	0.33	-0.002	-0.29
Locality Characteristics				
Number of Shocks	1.62	1.088	-0.036	-0.69
Share of Entrepreneurs	0.092	0.086	0.003	-0.18
Crop Diversification	2.336	0.705	-0.014	1.41

NOTE: This table presents baseline summary statistics for the treatment and control groups and the two-sided t-test that the difference in means is statistically significantly different from zero; standard errors are clustered at the village level.

Table 2: Probability to Become Entrepreneur: Average Treatment Impacts

Sample	All		Former Salaried		Former Unemployed	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.009	0.007	0.017	0.015	0.006	0.004
	(0.004)***	(0.003)***	(0.008)**	(0.008)*	(0.003)**	(0.002)**
Mean Dep. Var.	0.037		0.074		0.016	
Controls	No	Yes	No	Yes	No	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Wave Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R-squared	0.033	0.124	0.040	0.055	0.046	0.199
Number of Obs	47219	46271	17421	17094	26680	26154
Number of Localities	504	500	496	492	504	500

NOTE: This table reports probit marginal effects of the program on the probability to become entrepreneur. * denotes significance at 10%; ** significance at 5%; *** significance at 1%. Standard errors are clustered at the village level. Baseline control variables include age, age squared, years of education, gender, income (labor and other sources), households' demographics, assets (land and animals), welfare index (score) and villages' main economic activity, agricultural shocks, crop diversification and share of entrepreneurs.

Table 3: Probability to Become Entrepreneur: Placebo

Sample	Eligibles		Non Eligibles	
	(1)	(2)	(3)	(4)
Treat*Wave1	0.0004	0.0001		
	(0.0061)	(0.0047)		
Treat*Wave2	0.0155	0.0115		
	(0.0069)**	(0.0054)**		
Treat*Wave3	0.0172	0.0131		
	(0.0092)**	(0.0074)**		
Treat*Wave4	0.0092	0.0076		
	(0.0066)	(0.0053)		
Treat*Wave5	0.0053	0.0035		
	(0.0066)	(0.0048)		
Treat			0.004	0.004
			(0.005)	(0.004)
Controls	No	Yes	No	Yes
State Dummies	Yes	Yes	Yes	Yes
Wave Dummies	Yes	Yes	Yes	Yes
Number of Obs	78115	76560	15464	15148
Pseudo R-squared	0.034	0.128	0.02	0.13
Number of Localities	505	501	450	445

NOTE: This table reports probit marginal effects of the program on the probability to become entrepreneur. * denotes significance at 10%; ** significance at 5%; *** significance at 1%. Standard errors are clustered at the village level. Baseline control variables include age, age squared, years of education, gender, income (labor and other sources), households' demographics, assets (land and animals), welfare index (score) and villages' main economic activity, agricultural shocks, crop diversification and share of entrepreneurs.

Table 4: Baseline Characteristics and Conditionality

	(1)	(2)	(3)	(4)
Treat*Labor	0.00005			
	(0.0004)			
Labor	-0.0002			
	(0.0003)			
Treat*Female		0.052		
		(0.039)		
Female		0.066		
		(0.042)*		
Treat*Non Enroll			-0.005	
			(0.012)	
Non Enroll			0.010	
			(0.011)	
Treat*Prim Sec				0.007
				(0.023)
Prim Sec				-0.043
				(0.025)*
Treat	0.012	0.013	0.018	0.012
	(0.020)	(0.008)	(0.010)*	(0.021)
Controls	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes
Wave Dummies	Yes	Yes	Yes	Yes
Number of Obs	16966	17094	12630	8744
Pseudo R-squared	0.055	0.056	0.056	0.054
Number of Localities	492	492	488	480

NOTE: This table reports probit marginal effects of the program on the probability to become entrepreneur. The variable Labor measures the number of hours worked at the baseline; the variable Non Enroll is a dummy equal one if the household has eligible children not enrolled in school at the baseline; the variable Prim Sec is a dummy equal one if the household has children in primary school age and zero if the household has female children in secondary school age. * denotes significance at 10%; ** significance at 5%; *** significance at 1%. Standard errors are clustered at the village level. Baseline control variables include age, age squared, years of education, gender, income (labor and other sources), households' demographics, assets (land and animals), welfare index (score) and villages' main economic activity, agricultural shocks, crop diversification and share of entrepreneurs.

Table 5: Welfare and Labor Supply

Dependent Variable	Labor Earn	Non-food Exp	Food Cons	Hrs Work	Days Work	Sec Occup
	(1)	(2)	(3)	(4)	(5)	(6)
Treat*New Entrep	17.389	33.003	12.552	0.044	-0.085	-0.117
	(8.055)**	(13.944)**	(9.755)	(0.193)	(0.179)	(0.055)**
Treat	-3.902	16.770	17.950	-0.017	-0.038	-0.010
	(4.039)	(7.512)**	(5.452)***	(0.034)	(0.041)	(0.009)
New Entrep	-77.698	-22.441	-9.414	-0.169	-0.219	0.235
	(6.074)***	(11.756)*	(8.160)	(0.143)	(0.134)	(0.045)***
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Wave Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	32988	33036	30863	10441	15219	10763
R-squared	0.152	0.120	0.030	0.020	0.032	0.051
Number of Localities	494	495	495	488	488	483

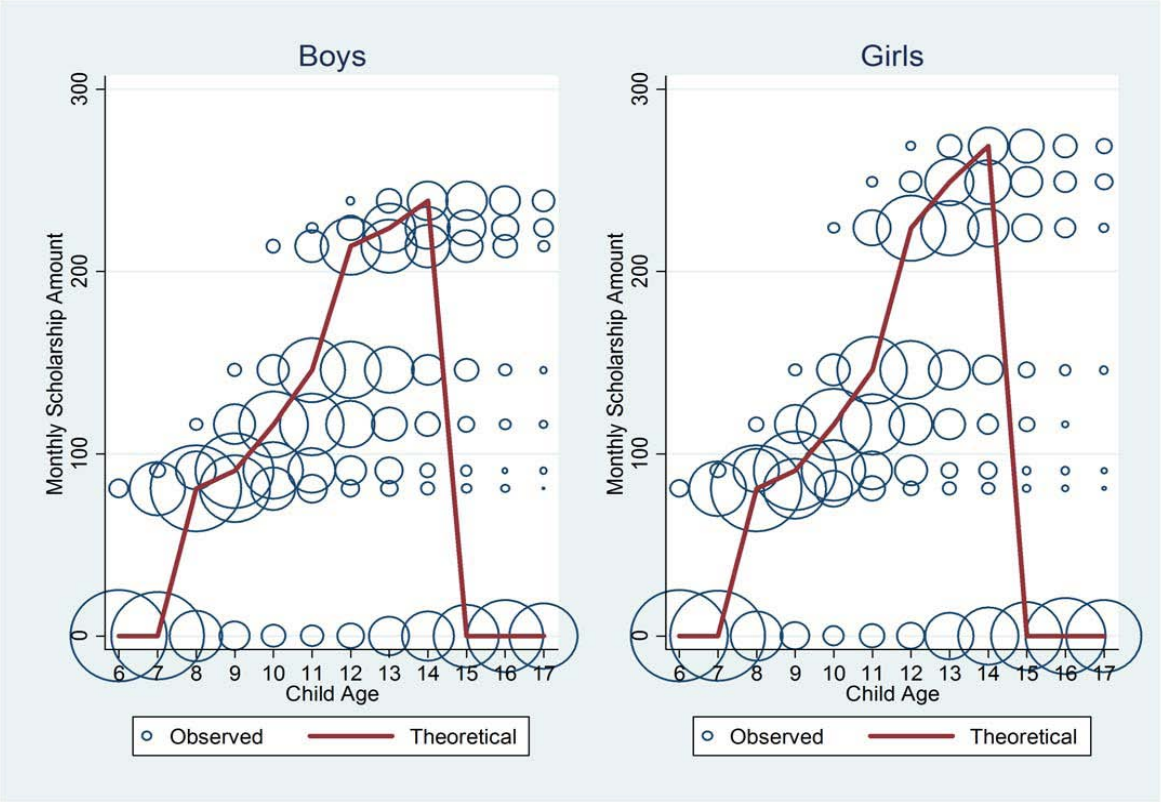
NOTE: This table reports OLS estimates of the program on labor earnings (column 1), non-food expenditures (column 2), food consumption (column 3), hours worked (column 4), days worked (column 5) and on the probability to be engaged in a second paid occupation (column 5). * denotes significance at 10%; ** significance at 5%; *** significance at 1%. Standard errors are clustered at the village level. Baseline control variables include age, age squared, years of education, gender, income (labor and other sources), households' demographics, assets (land and animals), welfare index (score) and villages' main economic activity, agricultural shocks, crop diversification and share of entrepreneurs.

Table 6: Investments

Dependent Variable	Carpenter (1)	Handicraft (2)	Agri Expend (3)	Animal (4)	Agri Product (5)	Land (6)
Treat*New Entrep	0.012	0.048	56.983	30.883	-3.946	-0.037
	(0.004)***	(0.021)**	(57.937)	(34.977)	(8.337)	(0.044)
Treat	-0.005	0.010	-50.395	0.841	-6.583	0.044
	(0.004)	(0.006)*	(34.229)	(2.347)	(4.477)	(0.023)*
New Entrep	-0.003	0.028	-112.147	2.738	5.423	0.083
	(0.002)	(0.010)***	(51.669)**	(8.525)	(5.832)	(0.034)**
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Wave Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	53195	53195	15996	17584	15617	35333
R-squared	0.038	0.094	0.079	0.006	0.009	0.081
Number of Localities	503	503	481	497	497	497

NOTE: This table reports OLS estimates of the program on the probability to be engaged in carpentry (column 1) and handicraft (column 2); on agricultural expenditures (column 3), animal stocks (column 4), agricultural production (column 5) and land owned or used (column 5). * denotes significance at 10%; ** significance at 5%; *** significance at 1%. Standard errors are clustered at the village level. Baseline control variables include age, age squared, years of education, gender, income (labor and other sources), households' demographics, assets (land and animals), welfare index (score) and villages' main economic activity, agricultural shocks, crop diversification and share of entrepreneurs.

Figure 1: Potential Transfers per Child



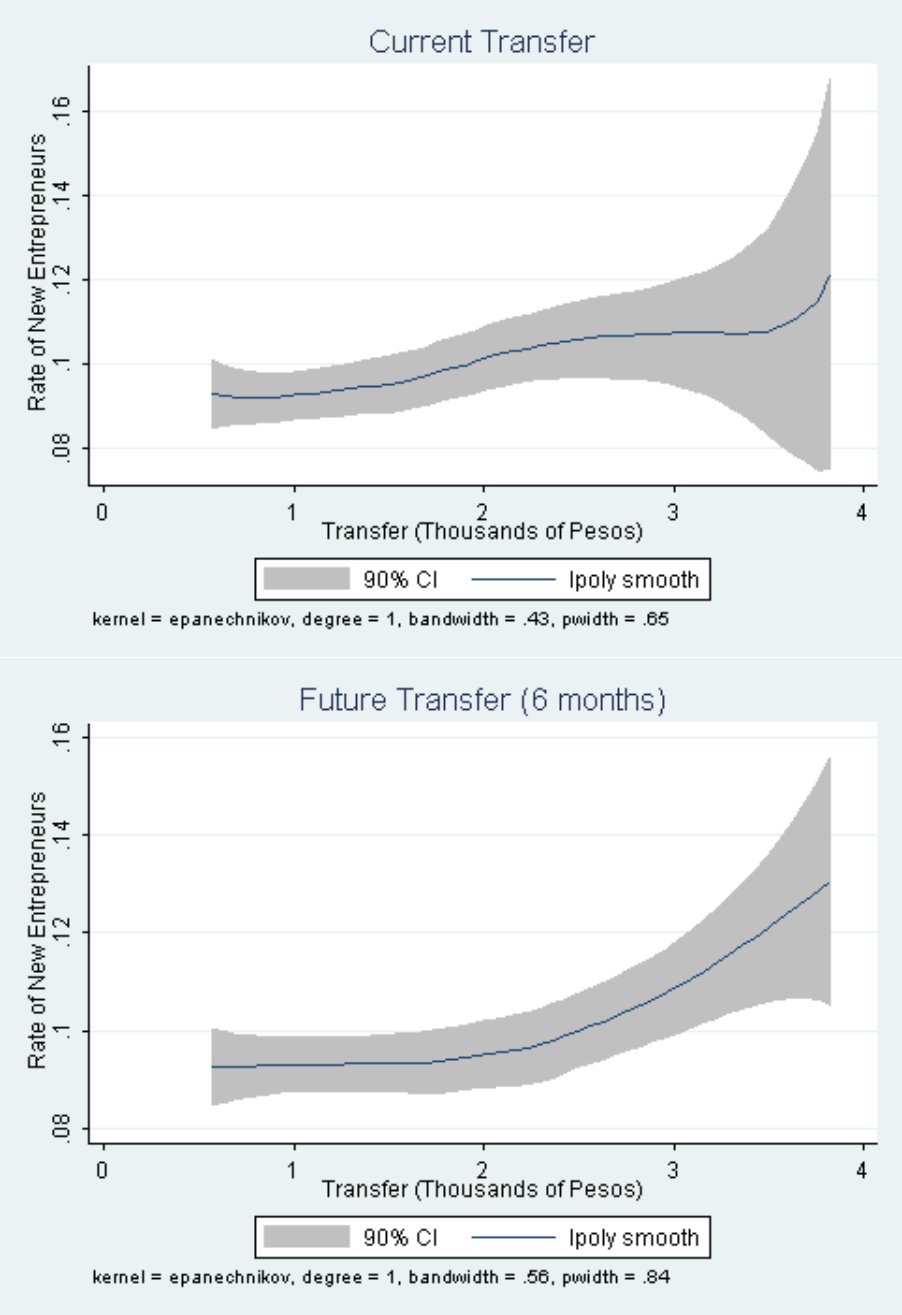
NOTE: This figure shows the variation of per-child monthly transfers a household is entitled to receive as a function of the age and the gender of the child. The solid "theoretical" line represents the monthly amount an household receives for each child who starts school at the age of six and progresses 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 have been adjusted for the relative sample frequency. Amounts are expressed in current Pesos as of the second semester of 1998.

Table 7: Current and Future Transfers: Placebo

Sample	Poor in Control Villages			Non-poor in Treated Villages		
	(1)	(2)	(3)	(4)	(5)	(6)
Current (6 months)	-0.0004			-0.0002		
	(0.0072)			(0.0151)		
Future (6 months)		-0.0071			-0.0077	
		(0.0063)			(0.0153)	
Future (1 year)			-0.0025			-0.0050
			(0.0033)			(0.0089)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Wave Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	6814	6814	6814	2846	2846	2846
Pseudo R-squared	0.038	0.039	0.039	0.038	0.038	0.038
Number of Localities	181	181	181	255	255	255

NOTE: This table reports probit marginal effects of the transfers (in thousand Pesos) on the probability to become entrepreneur. Standard errors are clustered at the village level. Control variables include age-specific categorical variables for the number of boys and girls between 6 and 17 years old.

Figure 2: 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.

Table 8: Current and Future Transfers: Six Months Horizon

	(1)	(2)	(3)	(4)	(5)	(6)
Current	0.005					
	(0.005)					
Future		0.013	0.053			
		(0.005)***	(0.018)***			
Future-Current				0.029		0.027
				(0.009)***		(0.009)***
Future+Current			-0.023		0.005	0.004
			(0.009)**		(0.002)**	(0.002)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Wave Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	10607	10607	10607	10607	10607	10607
Pseudo R-squared	0.051	0.052	0.052	0.051	0.052	0.051
Number of Localities	315	315	315	315	315	315

NOTE: This table reports probit marginal effects of the transfers (in thousand Pesos) on the probability to become entrepreneur. * denotes significance at 10%; ** significance at 5%; *** significance at 1%. Standard errors are clustered at the village level. Control variables include age-specific categorical variables for the number of boys and girls between 6 and 17 years old.

Table 9: Current and Future Transfers: Robustness

	(1)	(2)	(3)	(4)	(5)	(6)
Future-Current (6 months)	0.029	0.030	0.030			
	(0.009)***	(0.009)***	(0.009)***			
Current (1 year)	0.004			0.003		
	(0.002)			(0.002)		
Total (medium run)		-0.0001				
		(0.0001)				
Total (long run)			-0.00005			
			(0.00004)			
Future (1 year)					0.005	0.016
					(0.002)**	(0.011)
Future+Current (1 year)						-0.006
						(0.006)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Wave Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	10607	10607	10607	10607	10607	10607
Pseudo R-squared	0.052	0.055	0.055	0.051	0.051	0.051
Number of Localities	315	315	315	315	315	315

NOTE: This table reports probit marginal effects of the transfers on the probability to become entrepreneur. Amounts are expressed in thousand Pesos, except for Total (long run) which is in 100 thousand Pesos. * denotes significance at 10%; ** significance at 5%; *** significance at 1%. Standard errors are clustered at the village level. Control variables include age-specific categorical variables for the number of boys and girls between 6 and 17 years old (in columns 1,4,5,6) and between 1 and 17 years old (in columns 2,3).