Liquidity, Risk, and Occupational Choices*

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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 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]), while being 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 business (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 empirically estimate 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 binds 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 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 from becoming 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'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 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 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 help incurring start-up costs and thus 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 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.

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 Zinman and Karlan [2009] 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. [2010] 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 it with a specific survey. In general, however, despite liquidity and insurance constraints are often interrelated (Ray [1998]), little has been done to try separating 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. [2011]. They document that part of *Progresa*'s transfers were 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 both show that *Progresa* induced households to change their income generating activities, the main focus is quite different. Gertler et al. [2011] 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.[1] 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).[2] 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.[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 percent of the time and not repeating a grade more than twice. Scholarship amounts range from 81 to 269 Pesos per month

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

[2] 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 re-classified as eligible. In order to avoid arbitrary classifications, we exclude those households from our analysis (the results are unchanged, though, 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.

per child; they increase with school grade and, in seventh to ninth grades, 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 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.[5] 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.[6] Program take-up is remarkably 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. Amongst 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-à-vis 8% of salaried workers. This is common in many developing settings,

[4]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 and after the ninth grade.

[5]A second baseline survey was conducted in March 1998, but it did not contain any question about occupational status.

[6]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.[7]

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 entrepreneur in a given program period t and zero otherwise. We estimate regressions of the following form:

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

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.[8] 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 α_1 in equation (1), which measures the average effect of the eligibility to the *Progresa* transfers on the transition into entrepreneurship.[9] 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 the receipt of the program benefits, we run two falsification 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 indeed no significant treated-control differences in the probability of entry into entrepreneurship

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

[8] 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.

[9] All our results are robust if we use a Probit model instead.

when both treated 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: it appears that being entitled to receive the transfers, as opposed to simply living in a treated village, is what drives the program impacts on individuals' occupational choices.[10]

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 having children remaining enrolled for 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 in 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.[11] Again, we see no systematic differences in the 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

[10] These estimates suggest that our results are not driven by a pure demand effect, whereby treated villages are richer and so have a higher demand for entrepreneurs. They also tend to exclude within-village spillovers between eligible and non-eligible households in the choice to become entrepreneur.

[11] Specifically, parents are asked which grade they think each of their child can attain. We take the average of these grades for each households 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.

may also benefit from welfare programs which are directly related to their occupational choices.[12] If *Progresa* improves the effectiveness of these programs, the observed changes in occupational choices may be only spuriously related to *Progresa*'s transfers. In this case, we expect effects to be concentrated among households who are also beneficiary 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.[13]

Taken together, this evidence lends support to the view that *Progresa* 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

In the absence of 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 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 an 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.

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

[12] 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.

[13] 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. Results are very similar to those reported in column (5).

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 look for a salaried job. In addition, they choose the amount of wealth they wish to save 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 he looks for a salaried job. 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 of saving to one.[14]

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 λ finds one while the rest remain unemployed. In the former case, individuals get a fixed wage w, while if unemployed they enjoy benefits b (e.g., non-monetary benefits, non-market production), with $b \leq w$. We further assume that

$$py - k \geq \lambda w + (1-\lambda)b, \tag{3}$$

and

$$p \leq \lambda, \tag{4}$$

which implies that self-employment is a profitable yet risky activity.[15] 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 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.

The expected utility of those who become entrepreneurs is

$$U^E = u(a - k - s^e + C_1) + pu(s^e + y + C_2) + (1-p)u(s^e + C_2),$$

[14] Our results would hold with less extreme assumptions on borrowing constraints. Moreover, as it will become clear, the presence of saving constraints would reinforce our results.

[15] The variance of expected income as self-employed is $p(1-p)y^2$ while the corresponding variance for those who look for a salaried job is $\lambda(1-\lambda)(w-b)^2$. Equations (3) and (4) are sufficient to show that the former exceeds the latter.

while for those who look for a job it is

$$U^W = u(a - s^w + C_1) + \lambda u(s^w + w + C_2) + (1-\lambda)u(s^w + b + C_2).$$

We can then define the difference

$$D = U^E - U^W,$$

and say that an individual prefers being self-employed if $D \geq 0$. 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 $D \geq 0$ for those with $r \leq r^*$. The equilibrium share of self-employed, denoted with ne, is then defined as

$$ne = G(r^*)[1 - F(k - C_1)].$$

Those with $r > r^*$ or $a + C_1 < k$ instead are either salaried or unemployed. Our main interest is in exploring how ne varies with the transfers C_1 and C_2.

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 expected marginal utility is equalized across periods, i.e.,

$$u^{'}(a - s^w + C_1) = \lambda u^{'}(s^w + w + C_2) + (1-\lambda)u^{'}(s^w + b + C_2), \tag{5}$$

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

$$u^{'}(a - k - s^e + C_1) = pu^{'}(s^e + y + C_2) + (1-p)u^{'}(s^e + C_2). \tag{6}$$

Moreover, choosing $s^e > 0$ implies $a + C_1 > k$; hence, they become entrepreneurs if and only if $D \geq 0$. Notice also that by the envelope theorem we have

$$\frac{dD}{dC_1} = u^{'}(a - k - s^e + C_1) - u^{'}(a - s^w + C_1), \tag{7}$$

and

$$\frac{dD}{dC_2} = pu^{'}(s^e + y + C_2) + (1-p)u^{'}(s^e + C_2) - \lambda u^{'}(s^w + w + C_2) - (1-\lambda)u^{'}(s^w + b + C_2). \tag{8}$$

Substituting (5) and (6) into (7) and (8), we can see that for these individuals

$$\frac{dD}{dC_1} = \frac{dD}{dC_2}, \tag{9}$$

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 receive today and those they know they will receive tomorrow.

We do not expect however this to be the case for everyone. Borrowing constraints are widely documented (restricting to developing countries, see the surveys in Banerjee [2003] and Karlan and Morduch [2009]). These constraints break the equivalence between current and future transfers and allow us to compare the effects of these transfers in two extreme cases: one in which there are only liquidity constraints ($k > C_1$ and individuals are risk neutral) and one in which there are only insurance constraints ($k \leq C_1$ and individuals are risk averse).

Liquidity Constraints Consider the case in which $k > C_1$ and individuals are risk neutral. In this case, due to (3), everyone would like to be an entrepreneur, that is $D \geq 0$ for all individuals, while only those with $a + C_1 \geq k$ can do so. Hence, we would have $ne = 1 - F(k - C_1)$ and so

$$\frac{\partial ne}{\partial C_1} = f(k - C_1) \geq 0 \text{ and } \frac{\partial ne}{\partial C_2} = 0. \tag{10}$$

In this setting, the share of self-employed in period 1 is more responsive to the amount of period 1 transfers, as these help to overcome liquidity needs, than to period 2 transfers, as these may not be pledged for obtaining cash in period 1 and incur the investment.

Insurance Constraints Consider the case in which start-up capital is low, that is $k \leq C_1$, but individuals are risk-averse. In this case, all those for whom $D \geq 0$ become self-employed, i.e. $ne = G(r^*)$. Consider an individual who is marginal in the occupational choice. For this individual, $s^e \geq s^w$ and so he never sets $s^e = 0$ and $s^w > 0$.[16] Hence, we have that

$$u'(a - s^w + C_1) - \lambda u'(s^w + w + C_2) - (1-\lambda)u'(s^w + b + C_2) >$$
$$u'(a - k - s^e + C_1) - pu'(s^e + y + C_2) - (1-p)u'(s^e + C_2). \tag{11}$$

Combining (11) with (7) and (8), we have

$$\frac{dD}{dC_2} > \frac{dD}{dC_1}. \tag{12}$$

[16]In fact, suppose he sets $s^e < s^w$. Due to DARA utility, C_2 increases risk-taking through a classic wealth effect (Pratt [1964]), and so $\lambda u'(s^w + w + C_2) + (1-\lambda)u'(s^w + b + C_2) < pu'(s^e + y + C_2) + (1-p)u'(s^e + C_2)$, when $D = 0$. Combining the last inequality with (5) and (6), we have $u'(a - s^w + C_1) < u'(a - k - s^e + C_1)$, which requires $s^w < k + s^e$ and so it is inconsistent with $s^e < s^w$.

The total effect of changing C_1 and C_2 on ne depends on the fraction of the population who can optimally set its savings, for which equation (9) holds, and the fraction with binding borrowing constraints, for which equation (12) holds. Still, combining (9) and (12), we can say that the share of self-employed in period 1 is more responsive to period 2 than to period 1 transfers. In fact, in order to be willing to take risk, households 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 summary of the above results, we state the following Proposition.

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.*

Remark The previous model abstracted from working capital. Yet, if the self-employed need additional investments before the initial investment starts to pay off, future transfers can influence current occupational choices even in a world with no risk. To explore this argument in the simplest framework, suppose entrepreneurs need to invest k_1 in period 1, k_2 in period 2 and they gain π_3 in period 3. Suppose payoffs are deterministic and such that self-employment is more profitable than salaried work. In order to become self-employed, it is necessary that

$$a + C_1 \geq k_1, \tag{13}$$

and

$$C_2 + (a + C_1 - k_1) \geq k_2, \tag{14}$$

where $(a+C_1-k_1)$ is the maximal amount of period 1 savings. If $k_2 < C_2$, then (13) binds and so $ne = 1-F(k_1-C_1)$. Hence, equation (10) holds, and the share of self-employed in period 1 is more responsive to the amount of period 1 transfers than to period 2 transfers. If instead $k_2 \geq C_2$, then (14) binds and so $ne = 1 - F(k_2 - C_2 - C_1 + k_1)$. In this case,

$$\frac{\partial ne}{\partial C_1} = f(k_1 + k_2 - C_1 - C_2) = \frac{\partial ne}{\partial C_2},$$

and so the share of self-employed in period 1 is equally responsive to period 1 and to period 2 transfers. Hence, future transfers may indeed be relevant even with no insurance constraints. However, at least in this simple form, allowing for future investments is not sufficient to predict that future transfers matter more than current transfers.[17]

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 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 lag between the "current" and the "next" period. 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.[18] 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.[19]

Beside being possibly measured with error, the actual amounts received partly depend

[17] Intuitively, in order to incur period 1 investment, an individual must have $a + C_1 \geq k_1$ and so that individual would always be able to save in period 1 should that be necessary to finance period 2 investment. Hence, both increasing C_1 and increasing C_2 would have the same effect on helping him to incur k_2 and so become self-employed.

[18] In addition, unreported results show that for those who become entrepreneur 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.

[19] 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.

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:

$$ne_{i,t} = \beta_1 P_{h,t+1} + \beta_2 \left[P_{h,t+1} + P_{h,t}\right] + Child'_{h,t}\beta_3 + \eta_{i,t}. \tag{15}$$

Our coefficient of interest is β_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-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 (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.[20]

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

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

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 enrollment status induce considerable variations in the time profile of potential transfers, which indeed allow us to separately identify β_1 and β_2 in equation (15).

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 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 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 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 entrepreneur by 0.5%.[21] This amounts to a 11% increase vis-à-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 β_1 in equation (15). 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.[22] 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

[21] 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).

[22] 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 (15).

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), while the estimated coefficient on the differential impact of future vs. current transfers barely changes.[23] 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). 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 if the further away are transfers the more noise is introduced in 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 of this interpretation, we construct a measure of the risk 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

[23] This result is robust to alternative definitions of total transfers. For example, we have considered a (rough) measure of the total income shock an 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).

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 from each village.[24] 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 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 are 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, such as housing attributes, demographic structure, poverty levels, labor force participation and ownership of durable goods. For the individuals who reside in those communities, we can rely on recall data on their socioeconomic characteristics (including their main occupation) in 1997. 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 ago), 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:

$$e_{l,t} = \gamma_1 T_l + \gamma_2 d_t + \gamma_3 [T_l \times d_t] + z_{l,t}, \tag{16}$$

[24]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).

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. In addition, we weight each observation by the relative population size in order to control for potential intra-village correlation in occupational choices. In this specification, γ_1 captures any time-invariant difference between the two groups of localities, γ_2 any time trend which is common across groups, and γ_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-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 the 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 masks, however, significant heterogeneity in the medium-term impacts of the program. 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 (16) at the individual level 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 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 the cash transfers have been effective in promoting entrepreneurship, as they have induced individuals to take more risk 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 above described dynamics 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 *Progresa*'s 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], 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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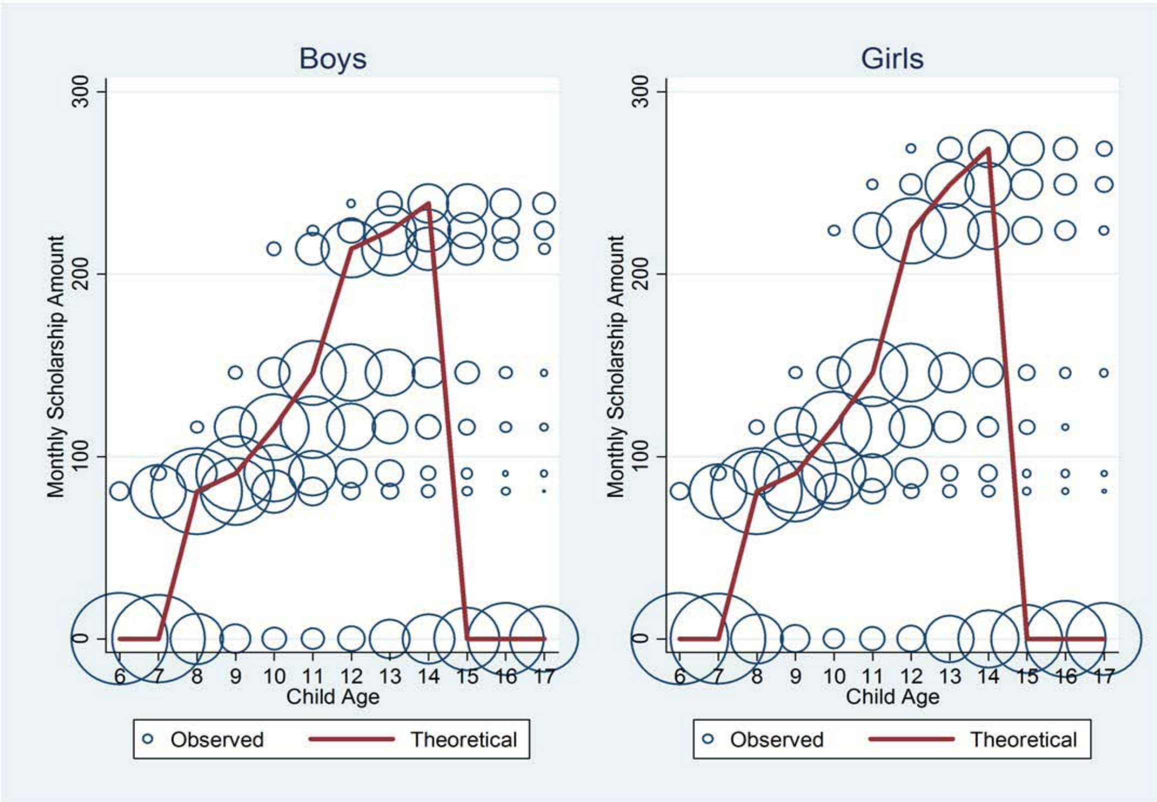
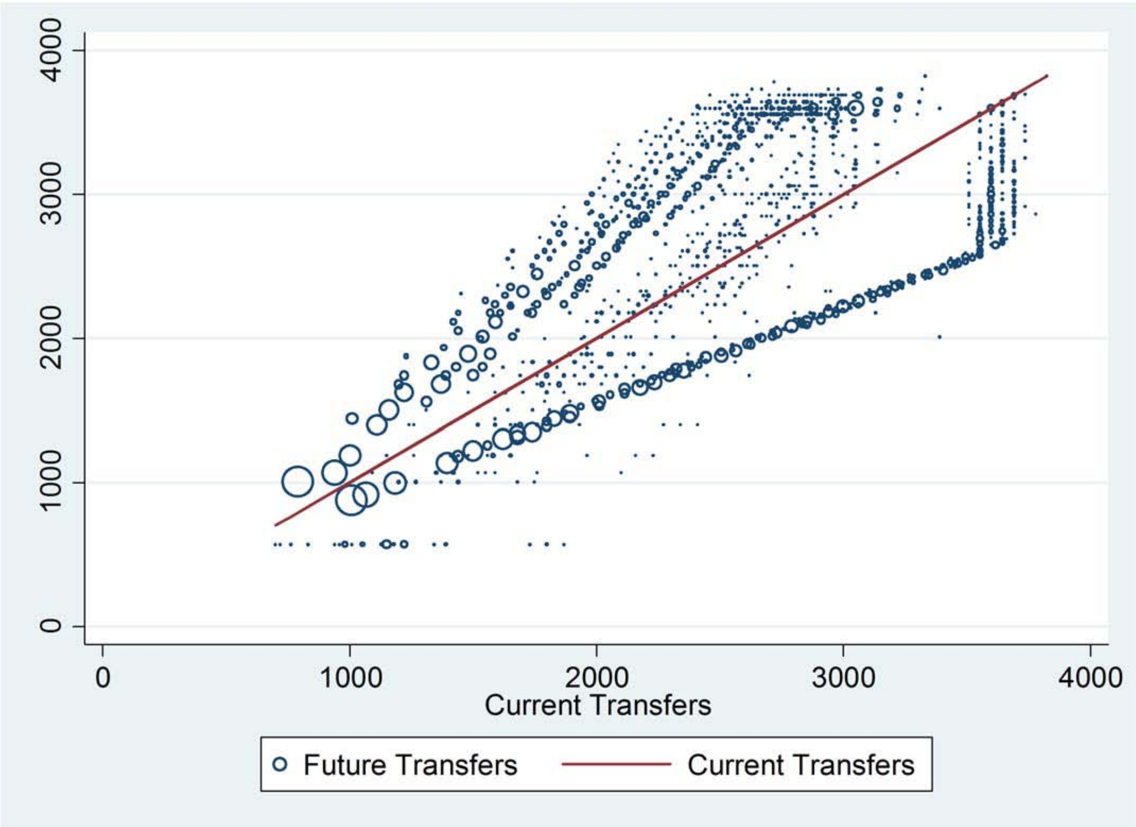
Figures

Figure 1: Potential Transfers per Child



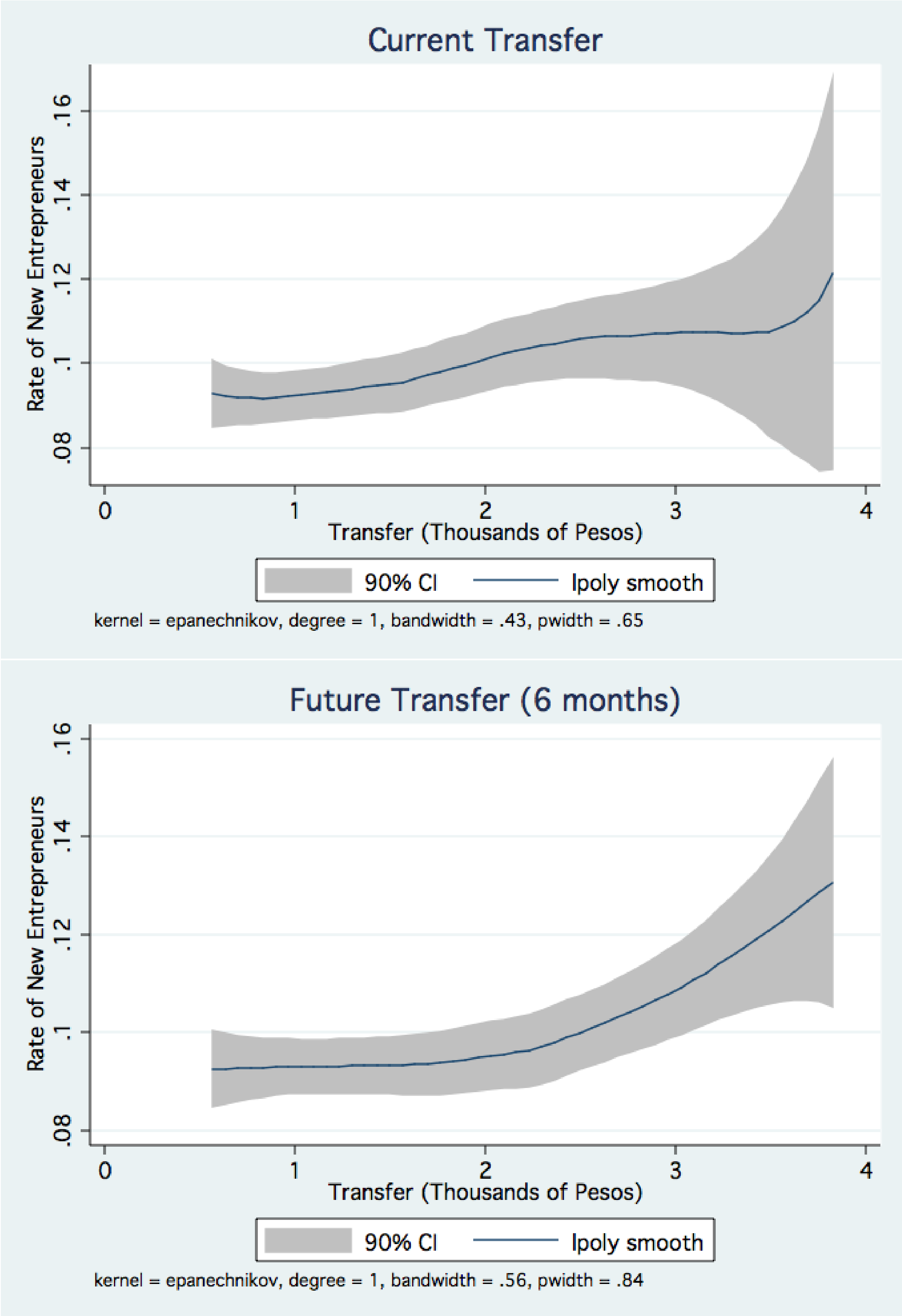
Note: This figure shows the variation of per-child monthly transfers a household is entitled to receive as a function of the age and the gender of the child. The solid "theoretical" line represents the monthly amount an household receives for each child who starts school at the age of six and progresses 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 (15). 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 Treat	Number of Obs Control
	(1)	(2)	(3)	(4)	(5)	(6)
Main Occupation						
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
Individual Characteristics						
Age	39.263	13.877	-0.254	-0.65	12778	7934
Female	0.541	0.498	0.006	1.09	12819	7946
Income Main Occup.	247.445	344.452	-11.243	-1.29	12527	7805
Income Other Occup.	56.354	339.52	-4.599	-0.72	12821	7949
Labor Supply	20.054	23.148	-0.002	-0.01	12769	7920
Years of Education	2.707	2.628	0.068	0.51	12778	7924
Household's Assets						
Poverty Index	638.14	82.489	0.399	0.23	7462	4527
Land Used	1.219	2.697	-0.071	-0.62	7389	4490
Land Owned	0.561	0.496	0.028	0.97	7452	4554
Working Animals	0.318	0.466	0.025	1.10	7467	4557
Household's Composition						
Female HH Head	0.048	0.213	-0.004	-0.46	7466	4555
child05	0.700	0.458	-0.003	-0.19	7467	4557
child612	0.708	0.455	-0.014	-1.20	7467	4557
child1315	0.394	0.489	-0.011	-0.76	7467	4557
child1621	0.370	0.483	0.003	0.35	7467	4557
men2139	0.606	0.489	0.002	0.16	7467	4557
men4059	0.352	0.478	-0.002	-0.17	7467	4557
men60	0.128	0.334	0.002	0.11	7467	4557
women2139	0.692	0.462	-0.014	-0.74	7467	4557
women4059	0.295	0.456	-0.003	-0.43	7467	4557
women60	0.125	0.33	-0.002	-0.29	7467	4557
Locality Characteristics						
Number of Shocks	1.62	1.088	-0.036	-0.69	319	185
Share of Entrepreneurs	0.092	0.086	0.003	-0.18	319	185
Crop Diversification	2.336	0.705	-0.014	1.41	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	Ex Salaried	Ex Unempl	Non-experiment	Non-eligibles
	(1)	(2)	(3)	(4)	(5)
Treat	0.009	0.015	0.006	0.006	0.004
	(0.004)**	(0.008)*	(0.003)**	(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	0.008	0.008	0.008	0.008
	(0.004)**	(0.004)**	(0.004)**	(0.005)*	(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	0.021	-0.014	0.013
		(0.003)**	(0.010)**	(0.010)	(0.018)
Current+Future			-0.009	0.008	-0.010
			(0.005)*	(0.006)	(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	0.007		
		(0.001)*	(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	-0.011
				(0.007)	(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	High Entrep 97	Low Entrep 97	High Entrep 97	Low Entrep 97
	(1)	(2)	(3)	(4)	(5)
Treat*Post	-0.004	-0.031	0.016	-0.019	0.022
	(0.008)	(0.012)**	(0.006)***	(0.017)	(0.011)**
Treat	-0.017	-0.001	-0.011		
	(0.010)*	(0.015)	(0.004)***		
Post	0.0001	-0.010	0.017		
	(0.005)	(0.008)	(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%.

Appendix

Table A.1: Indicators of Covariate Balancing, Before and After Matching

Variable	Sample	Treated	Control	%Bias	T-C Diff (t-test)	Poly Reg (F-test)
	(1)	(2)	(3)	(4)	(5)	(6)
Years of Education	Unmatched	3.0346	2.2737	28.5		
	Matched	3.0448	3.0755	-1.1	0.505	0.899
Female	Unmatched	0.58911	0.58481	0.9		
	Matched	0.58781	0.58658	0.2	0.343	0.214
Age	Unmatched	39.681	42.807	-23.3		
	Matched	39.692	40.392	-5.2	0.241	0.001
Indigenous	Unmatched	0.41775	0.32586	19.1		
	Matched	0.41833	0.41916	-0.2	0.338	0.201
Individual Income	Unmatched	270.74	508.24	-38		
	Matched	272.34	267.88	0.7	0.251	0.779
Households Income	Unmatched	810.49	1370.9	-52.6		
	Matched	815.23	790.24	2.3	0.226	0.685
Nb of Rooms	Unmatched	1.559	1.4882	7.6		
	Matched	1.5615	1.5512	1.1	0.458	0.973
Poverty Index	Unmatched	635.12	658.79	-33.4		
	Matched	639.97	645.62	-8	0.113	0.001
Land Ownership	Unmatched	0.5591	0.43787	24.4		
	Matched	0.55917	0.52548	6.8	0.426	0.370

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Table A.1 – Continued

	Sample	Treated	Control	%Bias	T-C Diff (t-test)	Poly Reg (F-test)
	(1)	(2)	(3)	(4)	(5)	(6)
Animals Ownership	Unmatched	0.31931	0.23227	19.6		
	Matched	0.31605	0.31462	0.3	0.390	0.550
Female HH Head	Unmatched	0.02939	0.07317	-19.9		
	Matched	0.02981	0.0292	0.3	0.517	0.996
child05	Unmatched	0.74158	0.68239	13.1		
	Matched	0.73729	0.747	-2.1	0.309	0.010
child612	Unmatched	0.74127	0.58194	34.2		
	Matched	0.73667	0.72606	2.3	0.492	0.472
child1315	Unmatched	0.40061	0.24678	33.3		
	Matched	0.39301	0.43118	-8.3	0.305	0.185
child1621	Unmatched	0.35519	0.32855	5.6		
	Matched	0.35504	0.38403	-6.1	0.275	0.048
women2139	Unmatched	0.73358	0.62061	24.3		
	Matched	0.7303	0.73345	-0.7	0.228	0.116
women4059	Unmatched	0.26804	0.31787	-11		
	Matched	0.27046	0.28385	-2.9	0.317	0.636
women60	Unmatched	0.09009	0.20803	-33.6		
	Matched	0.09081	0.08147	2.7	0.287	0.256
men2139	Unmatched	0.64199	0.56569	15.6		
	Matched	0.64164	0.63501	1.4	0.419	0.353

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Table A.1 – Continued

	Sample	Treated	Control	%Bias	T-C Diff (t-test)	Poly Reg (F-test)
	(1)	(2)	(3)	(4)	(5)	(6)
men4059	Unmatched	0.34492	0.35992	-3.1		
	Matched	0.34491	0.36067	-3.3	0.280	0.326
men60	Unmatched	0.08531	0.22636	-39.6		
	Matched	0.08615	0.08115	1.4	0.492	0.550
Share of Entrep.	Unmatched	0.09076	0.09655	-7.3		
	Matched	0.09101	0.0894	2	0.335	0.687

NOTE: This table reports matching quality indicators for each covariate included in the propensity score specification. Columns (2)-(3) display sample means for the treated and control (or matched) groups. In column (4), we report the median absolute standardized bias, defined as the difference of the sample means in the treated and control (or matched) sub-samples as a percentage of the square root of the average of the relative sample variances. In column (5), we present the p-values of the two sided t-test of mean differences between the treated and the control group within deciles of the estimated propensity score. In column (6), we report p-values of the F-test of the joint null that the coefficients on all of the terms involving the treatment dummy equal zero in a polynomial regression of degree 5 of each covariate on the estimated propensity score, the treatment dummy and the interaction terms.