PARIS SCHOOL OF ECONOMICS
ÉCOLE D'ÉCONOMIE DE PARIS

WORKING PAPER N° 2010 - 29

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Liquidity, Risk and Occupational Choices*

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October 2010

Abstract

We explore whether financial constraints matter and which financial constraints matter the most in the choice of becoming an entrepreneur. We exploit a randomly assigned welfare program in rural Mexico to show that cash transfers significantly increase entry into entrepreneurship, thereby providing evidence of financial constraints. We then develop a simple model to highlight how liquidity and insurance constraints respond differently to the time profile of expected cash transfers. Exploiting the cross-households variation in the timing of these transfers, we find 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. We interpret these findings as evidence that the program has been effective in promoting micro-entrepreneurship by enhancing the willingness to bear risk.

Keywords: Financial constraints; entrepreneurship; insurance; liquidity.

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*We thank Abhijit Banerjee, Francois Bourguignon, Eric Edmonds, Marc Gurgand, Sylvie Lambert, Jean-Marc Robin 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.

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1 Introduction

Poor households face multiple financial constraints. They often lack the possibility to attain optimal levels of saving, borrowing and insurance against income shocks. Several dimensions of their lives are affected by these constraints, so much that "access to finance" is today recognized as a fundamental ingredient of economic development (see e.g. Banerjee [2003]; Karlan and Morduch [2009]).

At the same time, understanding the effects of an improved access to finance poses some serious challenges. First, such an improvement seldom occurs for random reasons, which makes it hard to empirically estimate its effects. Moreover, and perhaps even more fundamentally, one would like to open the box of "access to finance" and understand which of the various financial constraints are most binding 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. The possibility for poor households to set up their own business is recognized as a key 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]).

We address these questions by first exploiting a random variation in household income to show that financial constraints prevent some individuals the possibility to become entrepreneurs. We then decompose financial constraints by distinguishing in particular whether individuals refrain from becoming entrepreneurs as they lack enough liquidity to undertake some initial capital investment -what we call liquidity constraints- or rather as they lack the ability to insure their income against the risk posed by entrepreneurial returns -what we call insurance constraints. We develop a simple model to highlight how these constraints respond differently to the time profile of expected income shocks and exploit the variation in the timing of these shocks to try separating the effects of liquidity and insurance constraints.

More specifically, we exploit the welfare program *Progresa/Oportunidades*, 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 households 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 simply 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 can 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 can interpret the treatment impacts as the result of income shocks and thus as (indirect) evidence that households face financial constraints.

In search of a better understanding of which financial constraints the program has relaxed the most, and distinguishing in particular liquidity from insurance constraints, we exploit the fact that, as mentioned, treated households face significantly different patterns of transfers. For example, an household with a child in the ninth grade is entitled to a substantial amount of current transfers but very little transfers in a year (since in our sample period children stop being eligible after the ninth grade), while an household with a child in the eighth grade is entitled to somewhat lower current transfers but much higher future transfers. We then ask in which household adult members are more likely to become entrepreneurs, and more generally whether this choice is more responsive to the size of transfers currently received or to the size of transfers expected for the future.

In order to guide our interpretation, we develop a simple occupational choice model in which individuals may face liquidity or insurance constraints. If wealth cannot be freely allocated across periods, due for example to borrowing or saving constraints, current and future transfers have different effects on the choice of becoming entrepreneur. The amount of transfers currently received is better suited to help incurring start-up costs and so it is 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 the choice of becoming entrepreneur in the

current period is more responsive to the size of transfers recently received or to those expected for the future. 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 is significantly more responsive to the amount of transfers expected for the near future than to the amount currently received.

In our view, these results tend to support the hypothesis that the program has been effective in promoting micro-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 investment 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, future transfers matter as they enhance the possibility to insure against future income fluctuations. In our case, this translates into some salaried individuals being willing to undertake the risky choice of setting up a business.

1.1 Related Literature

This paper builds on the literature on the effects of improved access to finance on occupational choices. Under non-experimental research designs, 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 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). In this literature, however, experimental evidence is 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 excep-

tion is Dercon and Christiaensen [2007], who attempt to 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 et al. [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. It provides households with conditional cash transfers targeted to specific behaviors in three key areas: nutrition, health and education. Initially, 506 rural 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. To check the effectiveness of randomization, Table 1 presents baseline summary statistics of several individual, household and village characteristics for the treatment and control groups, as well as the two-sided t-test that the difference in means is different from zero. None of the variables displays statistically significant baseline differences, hence confirming that randomization has been successful in attaining balanced treatment and control populations.

Cash transfers from *Progresa* are given bimonthly to the female head of eligible households and they come in two forms.[1] The first is a fixed food stipend of 105 Pesos per month conditional on family members obtaining preventive medical care. The second is an educational scholarships which is provided to each child who is less than 18 years old and enrolled between the 3rd and the 9th grade, conditional on

[1]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 3000 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).

attending school a minimum of 85% of the time and not repeating a grade more than twice. As shown in Figure 1, these transfers vary between 105 and 375 Pesos per month per child, they increase with school grade and, in grades 7th to 9th, they are larger for girls than for boys.[2] These 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 The Sample

The evaluation surveys of *Progresa* consist of socioeconomic characteristics at the individual level repeatedly collected for 24,077 households, of which about 53% classified as eligible. A baseline survey was conducted in October 1997 and it has been followed by Household Evaluation Surveys collected every 6 months for a total of 5 waves after the baseline. Eligible households in treatment communities start receiving benefits in March-April 1998; whereas eligible households in control communities were not incorporated until November 1999. In most of our analysis, we 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 1998 to October 1999. Within this sample, program take-up was remarkably high: 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). Among them, 8% are entrepreneurs (mostly self-employed), 39% are salaried, and the remaining 53% do not have a paid occupation (we refer to them as unemployed). The great majority (93%) of the unemployed are women and the reverse hold for salaried workers, whereas about 25% of the entrepreneurs are women.

We mainly concentrate on the flows into entrepreneurship, i.e. on those individuals who are either salaried or unemployed at the baseline and become entrepreneurs in the follow-up period. Amongst those residing in control villages, 4% become entrepreneur during in this period, of which roughly 25% were unemployed in the baseline and 20% are women. Agricultural assets are their main capital endowment:

[2]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.

54% of those who become entrepreneurs own productive land, 25% own working animals such as horses, donkeys and bullocks. These figures are however very similar to the asset holdings of salaried workers. A distinctive features of new entrepreneurs is instead 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.[3]

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 the program impacts arise from individual responses to the cash transfers rather than to the conditions (such as schooling behaviors) attached to these transfers.

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

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 post-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 then 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 levels: 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 and state dummies, δ_t and η_s.[4] 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. 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 program 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 effect, whereby treated villages are richer and so have an higher demand for entrepreneurs. If this were the case, the treatment

[4]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.

effect would hold for all households in a treated village, whether eligible for the program or not. The results provided in Columns 3-4 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 increases the probability to become entrepreneur. 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 are less likely to work at home, mothers may be induced to quit a salaried job and turn to self-employment in search for flexible working hours or home working. This seems however 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 post-program period, and only 5% of them 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.[5]

We also notice that if individuals were pushed into entrepreneurship because of conditionality, one 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 results should be interpreted as simple correlations (as we show the choice of becoming entrepreneur is in 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).[6]

[5]Specifically, we consider those who were working longer hours, 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]).

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

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

Taken together, this evidence tends to rule out that conditionality is driving our results, which leads us to 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 received in 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

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

investment of k units of capital, or they become salaried. In addition, they choose the amount of wealth they wish to save from period 1 to period 2. We allow savings to be positive or negative and normalize the net returns of both saving and borrowing to zero. 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 becomes a worker.

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

$$py - k > w.$$

Savings and occupation are chosen in order to maximize

$$U = u(x_1) + u(x_2),$$

where 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

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

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. In addition, these individuals must have sufficient wealth $a \geq k - C_1$ to incur the initial capital investment.

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

$$\frac{dE}{dC_1} = u^{'}(a - k - s^e + C_1) - u^{'}(a - s^w + C_1), \tag{2}$$

and

$$\frac{dE}{dC_2} = pu^{'}(s^e + y + C_2) + (1 - p)u^{'}(s^e + C_2) - u^{'}(s^w + w + C_2). \tag{3}$$

To set a benchmark, we first describe an ideal world in which individuals face no constraints in allocating wealth across periods and so current and future transfers are equivalent.

4.1.1 Equivalence between current and future transfers

Suppose there are no constraints on borrowing or saving. Individuals who become workers set s^w such that their marginal utility is equalized across periods, i.e.

$$u^{'}(a - s^w + C_1) = u^{'}(s^w + w + C_2), \tag{4}$$

and in the same way those who become entrepreneurs choose s^e such that

$$u^{'}(a - k - s^e + C_1) = pu^{'}(s^e + y + C_2) + (1 - p)u^{'}(s^e + C_2). \tag{5}$$

Substituting (4) and (5) into (2) and (3), we can see that

$$\frac{dne}{dC_1} = \frac{dne}{dC_2}, \tag{6}$$

and so occupational choices respond in the same way to current and future transfers. This result is not surprising. In a world in which wealth can be freely and costlessly allocated across periods, individuals 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 typically the case. Borrowing constraints are widely documented (restricting to developing countries, see the surveys in Banerjee [2003] and Karlan and Morduch [2009]). Households may also face saving constraints, as the result of present-biased preferences (Ashraf et al. [2006], Dupas and Robinson [2009], Banerjee and Mullainathan [2010]), social norms (Platteau [2000]), or simply unavailability of a safe storage technology (see Collins et al. [2009] and the survey by Karlan and Morduch [2009]). We then turn to a setting in which some individuals may face constraints in their choice of s^e and s^w. 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 ($k > 0$ 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 individuals are risk neutral. Given that in this case everyone would like to become entrepreneur and so invest in period 1, saving constraints are never binding (and we are back to the case in which s^e and s^w can freely move). On the other hand, borrowing constraints may bind. Consider for

the sake of illustration (extreme) borrowing constraints which impose $s^e \geq 0$ and $s^w \geq 0$.[7]

In this case, only those with $a \geq k - C_1$ become entrepreneurs. Hence, for them, we would have

$$\frac{\partial ne}{\partial C_1} = f(k - C_1) > 0 \text{ and } \frac{\partial ne}{\partial C_2} = 0. \tag{7}$$

The effect of changing C_1 and C_2 in this setting depends on the fraction of the population for whom borrowing constraints do not bind, and so equation (6) hold, and the fraction for whom borrowing constraints bind, and so equation (7) hold. Still, we can say that in general the share of self-employed in period 1 is more responsive to the amount of period 1 transfers (as these help overcome liquidity needs) than to period 2 transfers (as these may not be pledged for obtaining cash in period 1 and incur the investment).

4.1.3 Insurance constraints

We now abstract from liquidity constraints by assuming $k = 0$. In this case, all those who are sufficiently tolerant toward risk become self-employed, i.e. $ne = G(r^*)$. Suppose there are (extreme) borrowing constraints ($s^e \geq 0$ and $s^w \geq 0$) so that some individuals, even by not saving, are consuming too little in the first period (as they would like to borrow). This requires that for $s^w = 0$

$$u'(a + C_1) > u'(w + C_2), \tag{8}$$

and for $s^e = 0$

$$u'(a + C_1) > pu'(y + C_2) + (1 - p)u'(C_2). \tag{9}$$

Consider first those for whom (8) and (9) hold. By substituting $s^e = s^w = 0$ into (2) and (3), we see that for them

$$\frac{\partial ne}{\partial C_1} = 0 \text{ and } \frac{\partial ne}{\partial C_2} = g(r^*)\frac{\partial r^*}{\partial C_2} > 0, \tag{10}$$

where $\partial r^*/\partial C_2 > 0$ 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 (8) 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 (2) and (3) and combining (5) and (8).[8]

[7] Our results would hold with less extreme assumptions on savings or borrowing constraints.

[8] Notice that no individual who is marginal in the occupational choice sets $s^e = 0$ and $s^w > 0$.

Suppose instead there are (extreme) saving constraints ($s^e \leq 0$ and $s^w \leq 0$) so that some individuals, even by not borrowing, are consuming too much in the first period (as they would like to save). This requires that for $s^w = 0$

$$u^{'}(a + C_1) < u^{'}(w + C_2), \tag{11}$$

and for $s^e = 0$

$$u^{'}(a + C_1) < pu^{'}(y + C_2) + (1 - p)u^{'}(C_2). \tag{12}$$

Those for which (11) and (12) hold set $s^e = s^w = 0$ and we are back to the case presented in equation (10). Also those for whom only (12) holds (and so they set $s^e = 0$ and $s^w < 0$) are more responsive to future than to current transfers. This can be shown by substituting $s^e = 0$ into (2) and (3) and combining (4) and (12).[9]

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 or borrowing and the fraction for whom either borrowing or saving 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 have enough wealth in period 2. In case of binding saving constraints, they cannot transfer wealth from period 1 to period 2, which makes them insensitive to C_1. At the same time, 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.

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.*

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 would save too much when salaried and so he would always set $s^e \geq s^w$.

[9] Again, for the same argument developed in Footnote 9, the case $s^e < 0$ and $s^w = 0$ is irrelevant as inconsistent with $s^e \geq s^w$.

4.2 Empirical Strategy

In what follows, we restrict our attention to eligible salaried workers residing in treated villages.[10] We are then interested in evaluating how the probability to become entrepreneur $ne_{i,t}$ depends on the amount of transfers received by household h in the previous six months $C_{h,t}$ and on the transfers they know they will receive in the next six months $C_{h,t+1}$.[11]

As an example of the variation in transfer amounts, suppose we are at the end of the academic year and we consider two households with a 15 years old daughter as a single child. In the first household the daughter is enrolled in the eighth grade and so, according to the rules described in Section 2, the household has received 2130 Pesos in the previous six months and will receive 2250 Pesos in the following six months. In the second household, the daughter is enrolled in the ninth grade, and as a result the household has received 2250 Pesos in the previous six months but will only receive 630 Pesos in the following six months (since as mentioned after the ninth grade children are no more eligible for the educational component of the transfer). We then ask in which household adult members are more likely to become entrepreneurs. Of course, this is just one of the several discontinuities in transfer amounts induced by the program's rules. In what follows, we pool those discontinuities across program eligible children by defining for each household the potential transfers it is entitled to receive.

We use potential transfers and not actual transfers since, beside being possibly measured with error, the latter partly depend on the household's behavior with respect to children enrollment, and this is likely to be simultaneously determined with occupational choices. We define potential transfers $P_{h,t}$ and $P_{h,t+1}$ as the amount of transfers a household would be entitled to if 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

[10] Unemployment seems unlikely to be driven by risk preferences in our setting.

[11] Six months correspond to the shortest time frame we can define such that future transfers are systematically different from current transfers according to the school calendar year. We later consider a one-year time horizon for robustness. Also, 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.

at baseline and by construction they are uncorrelated with any behavioral response to the program.[12]

We then estimate the following model using alternatively current and future potential transfers as explanatory variable:

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

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

where 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. This controls for any independent effect of children demographics on occupational choices.

The key identifying assumption for estimation of the α parameters in equations (13) and (14) is that, absent the program, occupational choices respond to children demographics and not to the specific school grade in which children are enrolled. Therefore, partial variations in potential transfers across households with children of the same age but attending different grades should be exogenous. 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 look at whether entry into self-employment is directly affected by these transfers. If this were the case, our approach would be invalid since occupational choices would be driven by the exact household characteristics that determine the transfers rather than by the transfers themselves. As shown in Table 7, however, estimates reveal no direct effect of potential transfers on occupational choices in these samples.

Finally, instead of estimating the effects of current and future transfers separately, we directly test for their differential impact on the probability to become entrepreneur. For this purpose, we consider the following alternative specification:

$$ne_{i,t} = \alpha_3 D_{h,t} + Child'_{h,t}\beta_3 + \eta_{h,t}, \tag{15}$$

where $D_{h,t}$ is defined as the difference between future and current transfers, $P_{h,t+1} - P_{h,t}$.

[12]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).

4.3 Results

In order to first provide a visual inspection of our relationships of interest, we estimate equations (13) and (14) non-parametrically. As shown in Figure 2, the shape of the curves suggests that current transfers 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 near future, especially after a certain amount (around 2000 Pesos).

These patterns are confirmed in standard probit estimation of equations (13) and (14). In Table 8, we report the marginal effects of current and future cash transfers on the likelihood to switch from salaried work to self-employment. Columns (1)-(2) display the results for the transfers received in the last six months. There is weak evidence in favor of a positive effect, which however vanishes once control variables are included. This reveals no significant effect of current transfers on the probability to become entrepreneur. Columns (3)-(6) report the results for future transfers using either a 6-months or a 1-year horizon. The size of future transfers appears a significant determinant of the probability to switch to self-employment. This effect is substantial: a one standard deviation increase in 6-months future transfers increases the average probability to become entrepreneur by 1.2%. This amounts to a 12% increase vis-à-vis the average share of new entrepreneurs in this sample (9.6%). In relative terms, the corresponding effects for 1-year future transfers are similar: a one standard deviation increase leads to 0.9% more self-employed, which is a 10% increase.

In order directly estimate any differential impact of current vs. future transfers, and so provide a sharper test of liquidity vs. insurance channels, we now turn to the model in equation (15). As shown in columns (1)-(2) of Table 9, these estimates provide evidence that the probability to become entrepreneur is significantly more responsive to the amount of future transfers than to the amount of current transfers. In terms of magnitude, a one standard deviation increase in the difference between future and current transfers (equal to 0.42) increases the probability to shift to self-employment by 1.2%, which matches our previous estimates in levels. Moreover, in columns (3)-(6), we have included the amount of current transfers and the difference between future and current transfers in wave 1, respectively, in order to compare similar households in terms of children demographics that are facing an upward or downward stream of transfers. Results barely change: households facing an increasing stream of transfers are on average more likely to switch occupation and

become self-employed.

It is also interesting to 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 the difference between future and current transfers increases such probability by 1.2%. This suggests that the time profile of the transfers is key for explaining the program effects on occupational choices.

To summarize, in our view, these results tend to support the hypothesis that the cash transfers have been effective in promoting micro-entrepreneurship as they have enhanced the willingness to bear risk as opposed to simply relaxing current liquidity constraints.

5 Conclusions

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 households. 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 insurance constraints are fundamental determinants of the choice of becoming entrepreneurs.

Our results feature some limitations. For example, little is known on the long run effects of these dynamics. In a related study, Gertler et al. [2006] argue 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 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, we have shown that it is possible to promote welfare-enhancing entrepreneurship.[13] 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.

[13]Instead, in face of many failed attempts, skeptics question whether policy makers can promote entrepreneurship at all (see e.g. Holtz-Eakin [2000], Acs and Szerb [2007], Parker [2007], Shane [2009] for a discussion).

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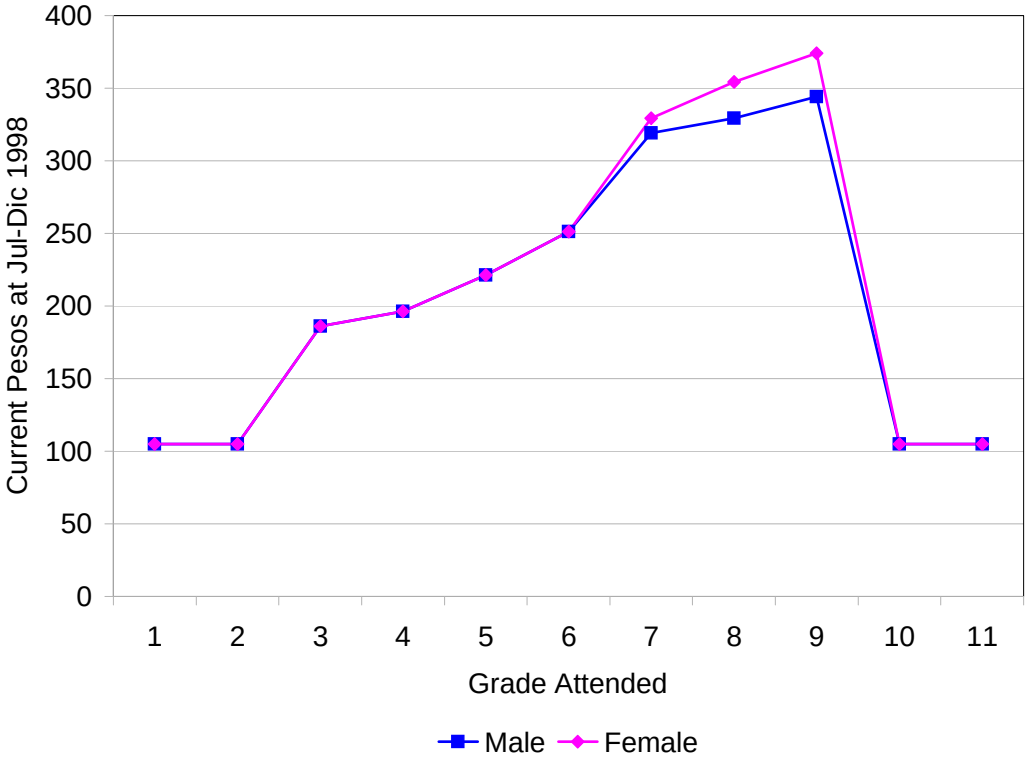
Tables and Figures

Table 1: Baseline Characteristics and Covariate Balance

Variable	Mean	Std. Dev.	Treated-Control Diff.	T-Stats
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.

Figure 1: Monthly Transfers per Child



NOTE: This figure shows per-child monthly transfers an eligible household is potentially entitled to receive as a function of the grade and the gender of the child. Amounts are expressed in current Pesos as of the second semester of 1998 and they have been increased over time in order to adjust for inflation.

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 vs. 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. * 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	Handicraft	Agri Expend	Animal	Agri Product	Land
	(1)	(2)	(3)	(4)	(5)	(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

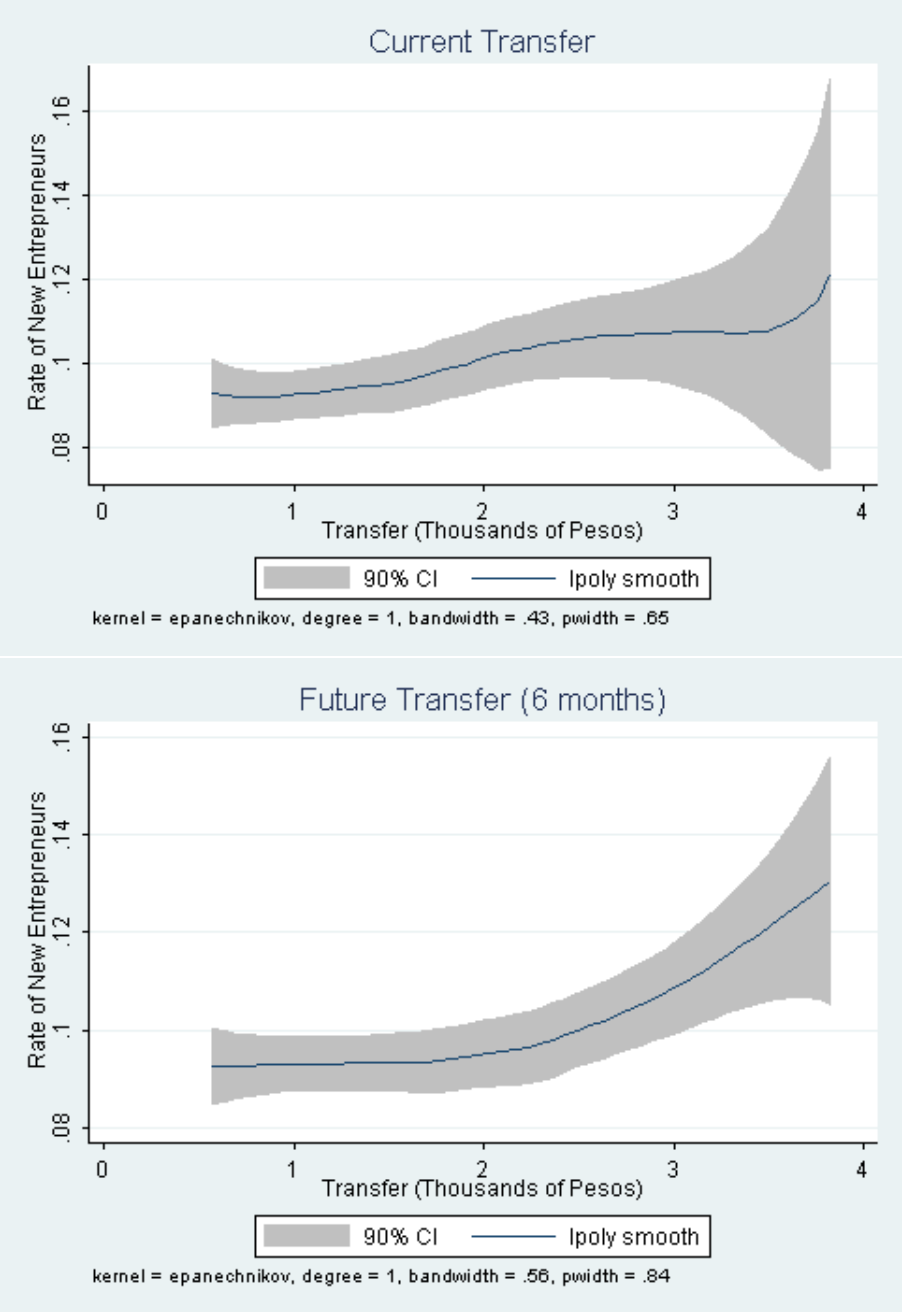
NOTE: This table reports OLS estimates of the program on the probability to be engaged in carpentry (column 1), handicraft (column 2), agricultural expenditures (column 3), animal stocks (column 4), agricultural production (column 5) and land owned or used (column 5). * denotes significance at 10%; ** 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.

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 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: Levels

	(1)	(2)	(3)	(4)	(5)	(6)
Current (6 months)	0.008	0.005				
	(0.004)*	(0.005)				
Future (6 months)			0.009	0.012		
			(0.004)**	(0.005)***		
Future (1 year)					0.004	0.005
					(0.002)**	(0.002)**
Controls	No	Yes	No	Yes	No	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	0.045	0.049	0.046	0.050	0.045	0.050
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. * 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: Differences

	(1)	(2)	(3)	(4)	(5)	(6)
Future-Current (6 months)	0.026	0.028	0.024	0.030	0.029	0.032
	(0.009)***	(0.009)***	(0.009)***	(0.009)***	(0.009)***	(0.009)***
Current (6 months)			0.007	0.007		
			(0.004)*	(0.005)		
Past Trend (6 months)					0.007	-0.006
					(0.011)	(0.013)
Controls	No	Yes	No	Yes	No	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Wave Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	10607	10607	10607	10607	10175	10175
Pseudo R-squared	0.046	0.051	0.046	0.051	0.047	0.052
Number of Localities	315	315	315	315	308	308

NOTE: This table reports probit marginal effects of the transfers 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.