Financial Constraints and Occupational Choices: Evidence from Rural Mexico*

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Abstract

This paper explores 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 the probability to become entrepreneur, 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.

JEL codes: O16, G20, L26.

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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 interpreting the effects and design 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 considered as heavily 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 incomes 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 we postpone a more detailed description of the program to Section 2, we here stress some features which makes it interesting for our exercise. First, the timing of access into the program 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. The size of the effect is substantial: having access to the transfers increases the likelihood to become entrepreneur of about 20-25% in the program period.

We then explore the potential channels underlying these treatment impacts. We first consider whether the fact that transfers were conditional on sending children to school may explain our results (as for example it may induce a reallocation of labor within the household). After performing a series of tests, we can safely rule out this hypothesis, thereby interpreting 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 by 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. We show that, under standard assumptions, the choice of becoming entrepreneur in the current period is more responsive to the amount of transfers recently 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.

In the following empirical analysis, we first rule out that the very same households characteristics which determine the profile of transfers determine also occupational choices. We then show that the probability to become entrepreneur is not significantly affected by the size of transfers currently received. Instead, future transfers matter: a one standard deviation increase in the transfers received in the next six months or one year increases the average probability to become entrepreneur by about 10% vis-a-vis the average share of new entrepreneurs in our sample. Similarly, individuals who face an increasing pattern of transfers are significantly more likely to become entrepreneurs than those who face a decreasing pattern.

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.

1.1 Related Literature

This paper contributes to the literature on the effects of income shocks 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, several papers have studied the effects of improved access to finance on both occupational choices and other business-related outcomes. Related to our paper, a substantial literature has explored the effects of improved access to credit (see e.g. Banerjee [2003] for a review) and, to a somewhat lesser extent, of improved access to insurance (see e.g. Besley [1995] for a review). 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 exception 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 on 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. Randomization of treatment should ensure that treatment and control groups have on average similar baseline characteristics. To check this, 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 statistically significantly different from zero. None of the variables displays 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 less than 18 years old and enrolled between the 3rd and the 9th grade, conditional on 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

[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).

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 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, neither sample attrition nor non-response are related to the treatment assignment.[3]

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 employed working for wage, 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: 54% of those who become entrepreneurs own productive land, 25% own working animals such as horses, donkeys and bullocks. These shares are however very similar to

[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.

[3]Results available upon request.

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 handcrafts, 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-a-vis 8% of salaried workers. This is common in many developing countries, 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.[4]

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 also control for several observed socioeconomic characteristics that may affect occupational choices in order to improve the power of the estimates and check the robustness of our findings. Moreover, although villages were randomly assigned to treatment, the data are unlikely to be independent across individual observations. In particular, occupational choices of individuals in the same village tend to be correlated as they share background characteristics and are exposed to the same labor market environment and natural shocks. We thus introduce the following empirical framework.

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

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

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.[5] In order to take into account for 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 18% 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 in 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.

Finally, we 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 effect would hold for all households in a treated village, whether eligible or not. The results provided in Table 3 (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 on the choice to become entrepreneur.

[5] 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.

3.1 Conditionality

As described in Section 2, cash transfers were 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, treated individuals may become self-employed due to an intra-household reallocation of labor, whereby child labor at home becomes less available and mothers are induced to quit a salaried job and turn to self-employment in search for flexible working hours or home working.

If this were the main channel, we would expect our effects to be concentrated on those who are more likely to respond to the conditionality constraint, such as salaried women or members of households with eligible children not enrolled in school in the baseline.[6] The results presented in Table 4 show no heterogeneous impact of the program on individuals for whom, according to a series of pre-program characteristics, we expect conditionality to be more or less binding.[7]

As a second test, we notice that if conditionality were the main channel, we would expect new entrepreneurs to work less, earn less and in general face a decrease in their welfare (as proxied by their consumption and expenditure patters). To explore this hypothesis, for each of these outputs $y_{i,t}$, we estimate the following equation:

$$y_{i,t} = \alpha T_l + \beta ne_{i,t} + \gamma T_l * ne_{i,t} + X^{'}_{i,t_0}\lambda + u_{i,t}. \tag{2}$$

We are interested in the differential impact estimated by γ, while netting out the pure treatment effect -which affects everyone in treated villages independently of their occupational choice- and the pure effect of becoming entrepreneur -which affects all new entrepreneurs independently of their treatment status. 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 offer little support to the conditionality hypothesis. On the contrary, as shown in Table 5, new entrepreneurs in treated villages appear to have significantly higher labor earnings and higher non-food expenditures (columns 1-2), and they are less likely to be engaged in a second paid occupation (column 6). On the other hand, their food consumption and their

[6] In our sample, however, there are only 55 women who were salaried and became entrepreneurs in treated villages, so this is unlikely to drive our results.

[7] 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]).

labor supply do not seem to be different (columns 3-5).[8]

Overall, these results tend to rule out that conditionality is driving the effect of the treatment on the choice to become entrepreneur. Hence, we interpret the treatment impacts as the result of an income shock and thus as (indirect) evidence that households face financial constraints. In what follows, we aim at better understanding which financial constraints the program has relaxed the most and try to distinguish liquidity from insurance constraints.

4 Liquidity and Insurance Constraints

Beyond random program treatment assignment, households differ in the number, grade and gender of their children. As described in Section 2, depending on these characteristics each household faces a different stream of transfers. We now exploit this additional source of variation and test whether new entrepreneurs are more responsive to the amount of money currently received or expected for the near future. In doing so, we aim at shedding some light on the potential channels underlying the treatment impacts presented above.

4.1 A Simple Occupational Choice Model

In order to clarify the idea behind our empirical approach, we present a stylized occupational choice model in which individuals may refrain from becoming entrepreneurs either because they lack sufficient liquidity to incur the initial capital investment or because they prefer avoiding the risky returns entrepreneurs face. We show that, under standard assumptions on utility functions and on financial markets imperfections, 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 consider a two periods setting. In the first period, individuals choose their occupation. They have two options: either they become self-employed or salaried. Becoming self-employed requires a fixed investment of k units of capital. In the

[8] A similar strategy is used to get a sense of how new entrepreneurs invest the money. We notice 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. These results are shown in Table 6.

second period, individuals get the returns from their occupation. The self-employed get y with probability p and zero otherwise; salaried get a wage w where

$$py - k > w.$$

Individuals are heterogeneous in two dimensions: their initial wealth a and their risk aversion r, drawn respectively by smooth distributions F and G with density f and g. We assume no time discounting and no possibility of saving or borrowing. In this way, we take as given the fact that current and future transfers are not equivalent (since households cannot freely allocate them across periods) and focus on which conditions make one or the other more likely to matter. In the Appendix, we consider a more general setting in which individuals can save or borrow and show that our results hold irrespectively of whether the time profile in the transfers matters because of saving *or* borrowing constraints.

Individuals choose their occupation to maximize

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

where x_1 and x_2 denote consumption in period 1 and 2 and we make the standard assumption that u exhibits decreasing absolute risk aversion (DARA). Suppose also that each individual is entitled to cash transfers C_1 and C_2 in period 1 and 2 respectively.

Consider an individual with wealth a, cash transfers $C_{1,}C_2$ and risk aversion r. If he becomes self-employed, he expects

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

and if he becomes salaried he enjoys

$$U^w = u(a + C_1, r) + u(w + C_2, r).$$

As standard in this class of models (see e.g. Kihlstrom and Laffont [1979]), there exists a 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 denote the equilibrium population share of self-employed as ne and we are interested in exploring how ne varies with the transfers C_1 and C_2 in two extreme cases. In the first setting, we abstract from insurance constraints by assuming that

everyone is risk neutral. If this were the case, since $py - k > w$, all those with enough wealth become self-employed, i.e.

$$ne = 1 - F(k - C_1).$$

Hence, we would have

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

The share of self-employed in period 1 depends positively on the amount of period 1 transfers (as these help overcome liquidity needs) while it is insensitive to period 2 transfers (as these cannot be pledged for obtaining cash in period 1 and incur the investment).

At the other extreme, we abstract from liquidity constraints by assuming $k = 0$. In this case, all those who are sufficiently tolerant toward risk become self-employed, i.e.

$$ne = G(r^*).$$

It can be shown that now

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

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]).

4.2 Empirical Strategy and Results

In what follows, we restrict our attention to eligible salaried workers residing in treated villages.[9] We then evaluate 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 $C_{h,t+1}$.[10]

However, beside being possibly measured with error, actual transfer amounts partly depend on the household's behavior with respect to children enrollment, which is likely to be simultaneously determined with occupational choices. Instead, we consider the amount of transfers a household would be entitled to if its children did not

[9] Unemployment is unlikely to be driven by risk preferences in our setting.

[10] 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.

change their pre-program enrollment decisions and, when enrolled, progressed by one grade in each year. These potential transfers $P_{h,t}$ and $P_{h,t+1}$ are deterministic functions of children's characteristics at baseline and by construction they are uncorrelated with any behavioral response to the program.

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, potentially, the household has received 2130 Pesos in the previous 6 months and will receive 2250 Pesos in the following 6 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 6 months but will only receive 630 Pesos in the following 6 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 then estimate the following model using alternatively current and future potential transfers as explanatory variable:

$$ne_{i,t} = \alpha_1 P_{h,t} + Child'_{h,t}\beta_1 + \epsilon_{i,t}, \tag{3}$$

$$ne_{i,t} = \alpha_2 P_{h,t+1} + Child'_{h,t}\beta_2 + u_{i,t}, \tag{4}$$

where the vector $Child_{h,t}$, contains age-specific categorical variables for the number of boys and girls aged 6 to 17 years old in each household h in any post-treatment period t and thus controls for spurious effects of children's age on occupational choices.

The key identifying assumption for estimation of α_1 and α_2 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 $P_{h,t}$ 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, occupational choices would be driven by the exact household characteristics that determine the transfers, rather than by

the transfers themselves, and this would invalidate our approach. As shown in Table 7, however, reduced form estimates of the direct effects of potential transfers reveal no effects on occupational choices in these samples.

In order to provide a visual inspection of our relationships of interest, we first estimate (3)-(4) non-parametrically. Figure 2 displays the estimated effects of both current and future transfers on occupational choices. The shapes of the curves suggest 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.

These patterns are confirmed in standard Probit estimation of equations (3) and (4). 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 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 a 6-months and 1-year horizon respectively. 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 (i.e. roughly 1000 Pesos) increases the average probability to become entrepreneur by 1.2%. This amounts to a 12% increase vis-a-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, that is a 10% increase.

Finally, instead of estimating the effects of current and future transfers separately, we can define the difference $D_{h,t} = P_{h,t+1} - P_{h,t}$ and directly test for the differential impact of future vs. current transfers on the occupational choice. We consider the following alternative specification:

$$ne_{i,t} = \alpha D_{h,t} + Child'_{h,t}\beta + \eta_{h,t}. \tag{5}$$

Table 9 reports Probit marginal effects for the model in equation (5). As shown in columns (1)-(2), these estimates confirm the results shown in Table 8 in both signs and magnitudes.[11] Moreover, in columns (3)-(6), we have included the amount of

[11] In terms of magnitude, a one standard deviation increase in the difference between future and current transfer (equal to 0.42) increases the probability to shift to self-employment by 1.2%, which matches our previous estimates.

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.

Overall, the magnitude of those effects is consistent with the mean treatment impacts discussed 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 amount of future 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.

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. This may suggest that financial barriers to entry into self-employment are not the most important obstacle in our setting; they may be below the fixed food stipend each household gets (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 some salaried individuals being willing to undertake the risky choice of setting up a business.

5 Conclusions

We have explored the response of occupational choices to the income shocks induced by the Mexican program *Progresa*. We have documented that the treatment significantly increases the probability to become entrepreneur and 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 have used this evidence to discuss how in our setting liquidity and insurance constraints shape occupational choices.

Our results features 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 for 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 occupational choice dynamics described above are not only improving the welfare of those directly interested, but 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. Second, according to our estimates, financial barriers to entry into entrepreneurship do not seem insurmountable. Instead, a major barrier may come from the risky prospects self-employment offers. In this view, promoting entrepreneurship requires reducing households' exposure to risk in other dimensions.

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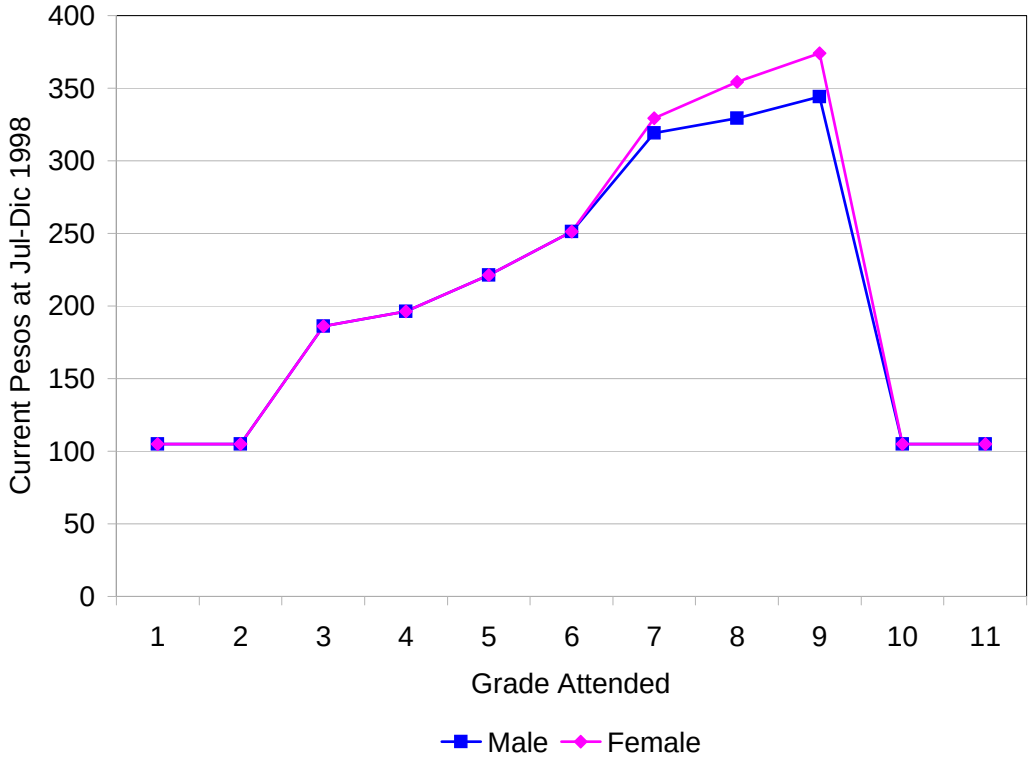
Tables and Figures

Table 1: Baseline Characteristics and Covariate Balance

Variable	Mean	Std. Dev.	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 as a function of the grade and 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

	All Sample		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: * significant at 10%; ** significant at 5%; *** significant at 1%.
Probit marginal effects reported for discrete change of dummy variable from 0 to 1. Standard errors clustered at the village level. Baseline control variables include individuals' 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: * significant at 10%; ** significant at 5%; *** significant at 1%. Probit marginal effects reported for discrete change of dummy variable from 0 to 1. Standard errors clustered at the village level. Baseline control variables include individuals' 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: Heterogenous Treatment Impacts: Conditionality

	Labor Supply (1)	Female (2)	Non Enroll (3)	Non Elig (4)	Prim vs. Sec (5)
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	0.016	0.012
	(0.020)	(0.008)	(0.010)*	(0.008)*	(0.021)
Controls	Yes	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes
Wave Dummies	Yes	Yes	Yes	Yes	Yes
Number of Obs	16966	17094	12630	17094	8744
Pseudo R-squared	0.055	0.056	0.056	0.056	0.054
Number of Localities	492	492	488	492	480

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%.
Probit marginal effects reported for discrete change of dummy variable from 0 to 1. Standard errors clustered at the village level. Baseline control variables include individuals' 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: Heterogenous Treatment Impacts: Welfare and Labor Supply

	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: * significant at 10%; ** significant at 5%; *** significant at 1%.

OLS estimates. Standard errors clustered at the village level. Baseline control variables include individuals' 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: Heterogenous Treatment Impacts: Investments

	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: * significant at 10%; ** significant at 5%; *** significant at 1%.

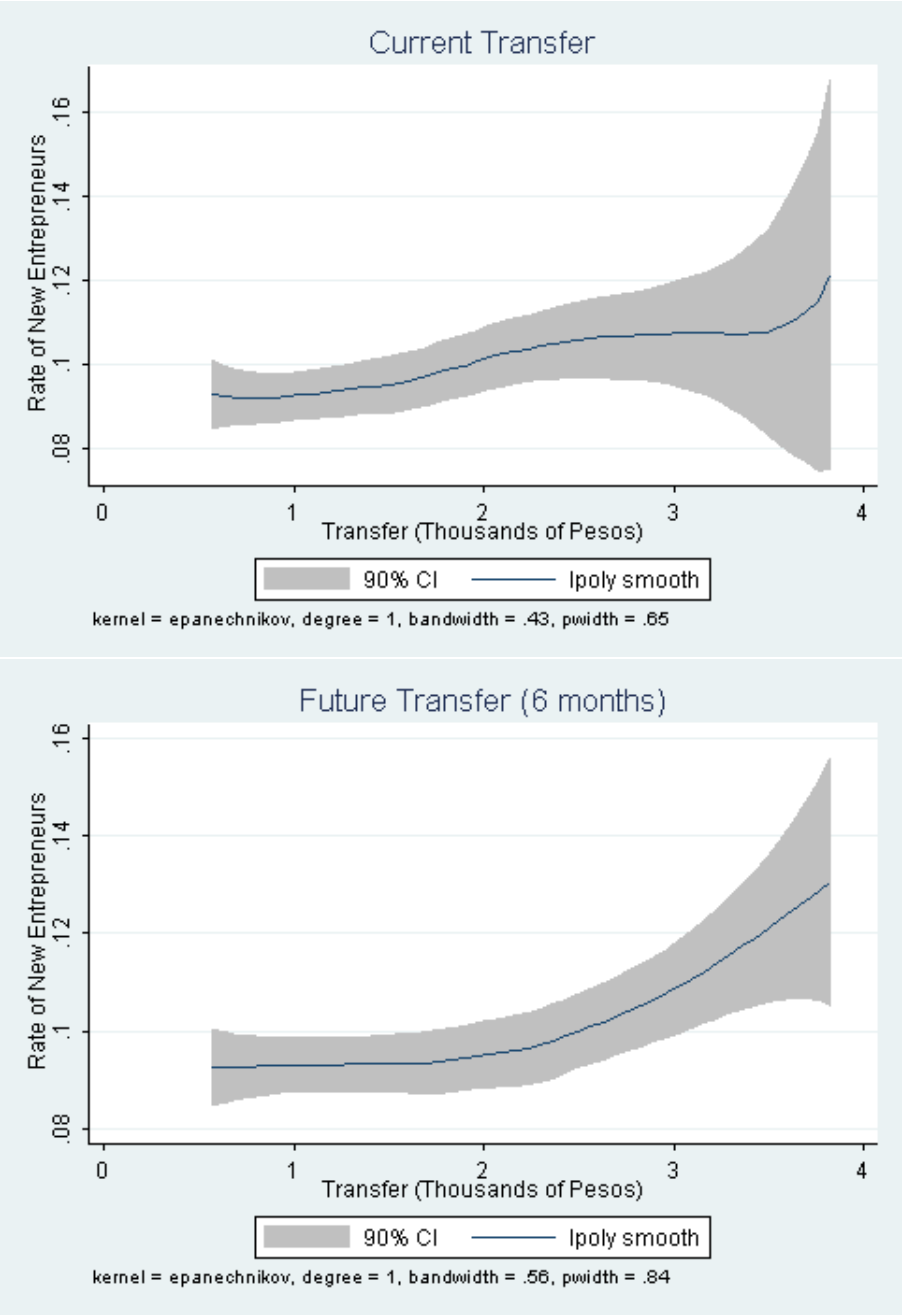
OLS estimates. Standard errors clustered at the village level. Baseline control variables include individuals' age, age squared, years of education, gender, income (labor and other sources), households' demographics, assets (land and animals), welfare index (score) and villages' main economic activity, 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: OLS estimates. Standard errors 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 effects 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: * significant at 10%; ** significant at 5%; *** significant at 1%.
Probit marginal effects reported. Standard errors 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: * significant at 10%; ** significant at 5%; *** significant at 1%.

Probit marginal effects reported. Standard errors 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.

Appendix: A model with saving and borrowing

We now extend our model by including the possibility to save or borrow in period 1. We consider the same setting as above but we let individuals choose the amount of wealth s they wish to transfer from period 1 to period 2. In principle s may be positive or negative, and we assume that both saving and lending have the same gross return which we normalize to one. Moreover, s may depend on occupational choices, so we denote with s^e and s^w the transfer decided by an individual who chooses to be entrepreneur or worker, respectively.

As above, we are interested in exploring the effects of current and future transfers C_1 and C_2 depending on whether liquidity or insurance constraints are binding. We start by considering the benchmark case in which there are no constraints on borrowing or savings. If everyone is risk neutral (no insurance constraints), $py - k > w$ implies that everyone chooses to become self-employed. If s can be set freely, those who have not enough wealth can borrow and become entrepreneurs. Hence, ne does not depend on C_1 or C_2. If instead $k = 0$ (no liquidity constraints), individuals who become workers set s^w such that their marginal utility is equalized across periods, i.e.

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

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

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

It follows that those who become entrepreneurs wish to save more than those who become salaried. To see this, notice that due to DARA utility, for any given s

$$u^{'}(s + w + C_2) < pu^{'}(s + y + C_2) + (1 - p)u^{'}(s + C_2),$$

thus setting $s^w = s^e = s$ would imply that salaried individuals are saving too much. Hence, in optimum we must have

$$s^w < s^e.$$

To see the effect of C_1 and C_2 on occupational choices, notice that an individual becomes entrepreneur if his expected utility exceeds what he would enjoy as a worker, where this difference writes

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

By the envelope theorem, it can be easily shown that

$$\frac{dG}{dC_1} = u^{'}(a - s^e + C_1) - u^{'}(a - s^w + C_1), \tag{8}$$

and

$$\frac{dG}{dC_2} = pu^{'}(s^e + y + C_2) + (1-p)u^{'}(s^e + C_2) - u^{'}(s^w + w + C_2). \tag{9}$$

Given (6) and (7), we can then conclude that

$$\frac{dG}{dC_1} = \frac{dG}{dC_2},$$

and so occupational choices respond in the same way to current and future transfers. These results are 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 generally the case. Borrowing constraints are widely documented (restricting to developing countries, see the surveys in Banerjee [2003]). Saving constraints are also prominent in developing countries. These may stem from present-biased preferences (as for example in 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.

Consider 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 not binding (and we are back to the case in which s can freely move) while borrowing constraints may bind; only those with $a \geq k - C_1$ become entrepreneurs. Hence, for them, C_1 would affect occupational choices while C_2 would not.

Suppose instead $k = 0$ and individuals are risk averse. Consider first the case of (extreme) borrowing constraints which impose $s^e \geq 0$ and $s^w \geq 0$. It may then be 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 $s^w = 0$

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

and for $s^e = 0$

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

Those for whom (10) and (11) bind set $s^e = s^w = 0$ and we are back to the case in the main text in which future transfers matter more than current transfers. The same applies to those for whom only (10) holds and so they set $s^e > 0$ and $s^w = 0$.[12] This can be shown by substituting $s^w = 0$ into (8) and (9) and combining (7) and (10). Consider next (extreme) saving constraints such that $s^e \leq 0$ and $s^w \leq 0$. It may then be 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 $s^w = 0$

$$u^{'}(a + C_1) < u^{'}(w + C_2). \tag{12}$$

and for $s^e = 0$

$$u^{'}(a + C_1) < pu^{'}(y + C_2) + (1 - p)u^{'}(C_2). \tag{13}$$

Those for which (12) and (13) bind set $s^e = s^w = 0$ and we are back to the case in the main text in which future transfers matter more than current transfers. The same applies to those for whom only (13) holds and so they set $s^e = 0$ and $s^w < 0$.[13] This can be shown by substituting $s^e = 0$ into (8) and (9) and combining (6) and (13).

In sum, current and future transfers have different effects to the extent that financial markets are imperfect and so households cannot costlessly transfer money between periods. If borrowing *or* saving constraints bind for some individuals, the results obtained in the simple model presented in the main text would hold. We can then conclude that, given financial constraints, current transfers tend to be more important in case of liquidity needs and future transfers tend to be more important in case of insurance constraints.

[12] Setting $s^e = 0$ and $s^w > 0$ would never be optimal as we showed that $s^e \geq s^w$.

[13] Again, setting $s^e < 0$ and $s^w = 0$ would never be optimal as we showed that $s^e \geq s^w$.