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Decomposing the Effects of CCTs on Entrepreneurship

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Abstract

Conditional cash transfers boosted a major reduction in poverty and a significant decrease in inequality in developing countries over the past decade. However, their success in promoting economic development is challenged by the claim that they deal with short-term poverty relief without providing the poor with the tools for breaking away from poverty by their own means. This claim, however, could be dismissed if conditional cash transfers had an effect on entrepreneurship. This paper assesses whether Bolsa-Família increases the probability of starting a new venture in Brazil, decomposing its potential effects into three channels: alleviation of the wealth constraint, insurance against negative outcomes of risky activities, and reduction of the labor supply of children (through the effect of the conditionality). The effect of each of these channels is separately estimated using data from National Household Surveys in 2004 and 2006, for which the households of transfer beneficiaries can be identified. The results indicate that entrepreneurship is indeed stimulated by the program in urban areas throughout the insurance and wealth constraint alleviation effects, notwithstanding that new ventures are typically secondary sources of income. Finally, the conditionality seems not to have an impact on the level of entrepreneurship.

This paper— joint product of the Brazil Country Management Unit and the Poverty Reduction and Economic Management Network, Latin America and Caribbean Region— assesses whether Bolsa-Família increases the probability of starting a venture in Brazil, decomposing its potential effects into three channels: wealth constraint alleviation, insurance provision and reduction of children's labor supply (through the effect of the conditionality). Policy Research Working Papers are also posted on the Web at http://econ.worldbank.org. The author may be contacted at glichand@worldbank.org.

Decomposing the effects of CCTs on entrepreneurship[1]

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1. INTRODUCTION

CCTs have had a significant effect on poverty and inequality reduction in developing countries over the last decade (Robalino, Ribe and Walker, 2009). In particular, Bolsa-Família, along with other governmental transfers, is pointed out as responsible for as much as 50% of the recent fall in inequality in Brazil (Barros et al., 2007).

Bolsa-Família is a conditional cash transfer program through which households up to a certain income threshold and with children or pregnant women receive governmental transfers as long as they meet some requirements related to investments in children's human capital. Created in 2003, the program is designed to target the poorest families in the country, and has displayed targeting and coverage performance above any national program and even to the same standards of international reference programs, such as Mexico's OPORTUNIDADES (Soares, Ribas and Soares, 2009).

Potential effects of the program on entrepreneurship have not been assessed so far. Those are particularly relevant in light of the claim – common to all CCTs – that Bolsa-Família deals with short-term poverty relief without providing the poor with the tools for breaking away from poverty by their own means. If the program is to have a positive effect on entrepreneurship, these concerns could be dismissed.

There is anecdotal evidence that the program might indeed foster startups, as the following excerpt from Ministry of Social Development (MDS)'s special magazine about Bolsa-Família reveals, through the story of Cida, who works as a house cleaning maid whenever there are job opportunities and whose household is beneficiary of the program: "In the living-room, on the top of the table, lies a little pile of cosmetics' magazines. Cida re-sells five different brands of cosmetics and earns 20% over costs for the products she manages to sell. 'Profits are tiny, but I have to try it every way, since it is a hard to find a permanent maid job here in Novo Gama'" (MDS, 2010, p. 65).[2]

There is now a somewhat large literature which documents the mechanisms through which CCTs affect individual decisions. Bolsa-Família is reported not to have an impact on parents' labor supply decisions – Medeiros, Britto and Soares (2008) show that recipient household heads do not work fewer hours and do not participate in labor markets to a lesser extent than non-recipient ones. It is reported to affect the school attendance and labor supply of children – Cardoso and de Souza (2004) show that enrollment is positively affected by the program, while male children decrease worked hours in comparison to those in non-recipient households. And Bolsa-Família is shown not to strategically influence fertility decisions – Rocha and Soares (2010) document that women do not on average have an extra child in order to increase the value of the transfer.

Gertler, Martinez and Rubio-Codina (2007) document that Mexico's OPORTUNIDADES had a positive effect on entrepreneurship, allowing individuals to improve their ability to generate income and increase their medium-term consumption. Sadoulet, de Janvry and Davis (2001) claim that the Mexican agricultural support program PROCAMPO increased investment in farm production, whereas Ravallion and Chen (2005)

[2] Free translation of the author.

show that the proceeds of China's anti-poverty program of temporary cash transfers resulted in higher household savings. Also related to our work are the findings of Banerjee and Duflo (2009), which show that microfinance increases the scale of existent ventures as well as the probability of a startup for individuals with a high predicted propensity to start a business; if poor households are wealth-constrained, a cash transfer can have similar impacts to enabling access to credit for small scale projects.

This paper assesses whether Bolsa-Família increases the probability of starting a new venture in Brazil, decomposing its potential effects into three channels: alleviation of wealth constraints, insurance against bad outcomes of risky activities and reduction of labor supply of children (through the effect of the conditionality). I draw upon PNADs 2004 and 2006, which are the only years for which transfer beneficiaries are in fact identified.

The strategy for the decomposition relies on conditioning household head's occupational choice on alternative measures of wealth – proxied by asset ownership and alternative measures of income, which either exclude or include program's cash transfer. Different choices for the wealth measure imply that different individuals are compared in estimating each effect of interest. When ex-ante wealth is used, I assess the wealth constraint alleviation effect, since comparable control and treatment individuals had the same capacity to pay for startup costs (alternatively, borrowing capacity) before the transfer, but the CCT increases the probability that the recipient is able to match upfront payments. When ex-post wealth is used instead, comparable control and treatment individuals have at the end the same ability to pay for startup costs, but the share of income coming from non-risky flows is higher for the CCT recipient, what can alter his/her occupational choice, since entrepreneurship is a risky activity.

Conditioning occupational choice on income is only possible because of previous results that document an absence of effects of Bolsa-Família on labor supply decisions (e.g. Medeiros, Britto and Soares, 2008). The same is true in what comes to the strategy to assess the effect of the conditionality: based upon previous evidence (e.g. Cardoso and de Souza, 2004), I assess, among CCT recipients, whether household head's with only male children have a higher or lower propensity to start a venture than those with only female children, taking as given that the conditionality is only effective, on average, in reducing boys' labor supply.

While the experimental design of Gertler, Martinez and Rubio-Codina (2007) is not available for the Brazilian program, I claim that treatment and control units are comparable after controlling for observable features due to some degree of randomness in beneficiary status at the household level, on account of delays between enrollment and actual transfer, and due to the municipal quoting scheme which drives enrollment efforts. Notwithstanding causality is less clear because of confounding factors, I perform several robustness checks to try to disentangle the mechanism through which the program affects entrepreneurship from other potential attributes that could also drive differences among recipient and non-recipient individuals in their willingness to start a venture.

On the other hand, while Gertler, Martinez and Rubio-Codina (2007) point out that wealth constraint alleviation and insurance against negative outcomes provided by the CCT program could drive entrepreneurship, they do not separately estimate the effect of these channels on the probability of starting a new venture. This paper builds up a strategy to separately estimate each effect of the program on entrepreneurship.

Results are that entrepreneurship is indeed stimulated by the program in urban areas throughout the insurance and wealth constraint alleviation effects, notwithstanding new ventures are typically secondary sources of income. The conditionality does not impact the level of entrepreneurship. Selection concerns are minimized on account of a composition effect: it seems to be the case that parents who previously employed their own children in their ventures transition to self-employment. Whether it is not possible to distinguish if this is only a 'reporting' effect – of parents afraid of losing the benefit on account of their children's participation in the labor market – or an actual re-arrangement due to program's conditionality, for the sake of our argument it suffices that this pattern cannot be justified based upon an unobserved propensity to entrepreneurship of enrolled individuals as opposed to those who do not benefit from Bolsa-Família.

The remainder of the paper is structured as follows: section 2 presents a simple model that motivates each of these potential channels; section 3 presents the data and empirical strategy; section 4 presents results, followed by decomposition and robustness checks in section 5; section 6 concludes the paper.

2. MODEL

Consider a father with one child, with concave Bernoulli utility function $V(\cdot)$, $V(\cdot)''' > 0$ (a necessary condition for decreasing absolute risk-aversion)[3], which maximizes expected utility over states of nature. For his occupational choice problem, I can safely ignore any form in which his utility might depend on his children future outcomes.[4] His simplified occupational choice is between, on one hand, supplying labor, and starting a new venture on the other hand, with the given structure of returns:

$$U^E = \begin{cases} V\left(b + w_p + l_c w_c\right) & ,if\ employee \\ \int_s V(b + R(l_c'\,;\, s) - t)\, ds\,, if\ (b \geq t)\ and\ entrepreneur \end{cases}$$

where b is initial wealth, w_p is parents' market wage, w_c is children's market wage, $l_c\,, l_c' \in [0,1]$ are children's labor supply 'outside' and 'inside' the firm; and $R(l_c'\,;\, s)$ is project's value in state of nature $s \sim U[0,1]$, while t is a startup cost. I remark two assumptions of the model:

Hypothesis 1: Collateral cannot be perfectly executed, such that only a fraction α of individual's initial wealth can recovered by a bank under lack of loan repayment.

Hypothesis 2: There is a transportation cost ω which must be paid by the parent if the child is to work separately from him.

First hypothesis implies that, in order to satisfy individual's incentive compatibility, loans should be limited to a share of individual's initial wealth (normalized by the interest rate) that corresponds exactly to the

[3] The $\log(\cdot)$ function, for instance, displays the required properties.

[4] Unless children could 'inherit' the firm, hence avoiding the future need of paying startup costs. I ignore this possibility for simplification. I also ignore the problem of human capital accumulation.

extent to which collateral can be executed.[5] For the sake of simplicity and without great loss of generality, I take this share to be zero ($\alpha = 0$). Hence, there are no credit markets, such that only individuals with $b \geq t$ are able to become entrepreneurs.

Second hypothesis implies that, the higher the transportation cost, the higher child's propensity to work along with his parent, since ω functions as a tax on child's return on other activities. Again, for simplicity, I take ω to be large enough such that children work only in parents' firm whenever they are entrepreneurs, supplying labor 'outside' only if parents also do so. This seems reasonable and is technically interesting to highlight potential differential impacts of the conditionality.

Introducing a conditional cash-transfer (CCT) for which this household is eligible, his expected utility becomes – assuming that the cash-trasfer is high enough[6] so that $l_c = l_c{}' = 0$, for simplicity:

$$U^{E'} = \begin{cases} V(b + w_p + T) & ,if\ employee \\ \int_s V(b + R(0\,;\,s) - t + T)\, ds & ,if\ (b + T \geq t)\ and\ \ entrepreneur \end{cases}$$

where T is cash-transfer.

One can observe three effects of introducing a CCT:

Effect 1: The individual's wealth constraint is alleviated, since a positive T might 'cross the line', allowing him to pay the startup cost;

Effect 2: The individual is guaranteed to receive T under any state of nature, what, combined with decreasing absolute risk-aversion of $V(\cdot)$, implies that he will be less risk-averse with respect to the lottery involving $R(0\,;\,s)$;

Effect 3: The individual loses the contribution given by his child's supply of labor under any occupational choice. The relative change in the attractiveness of each occupation depends on how the profile $\left\{\frac{\partial R(l_c\,;\,s)}{\partial l_c}\right\}_{s\,\in\,[0,1]}$ compares to w_c.

While effects 1 and 2 are expected to increase entrepreneurship, effect 3 is ambiguous. As the latter is the only one associated with the conditionality, I shall pay particular attention to it, on account of the policy-relevant implications linked to program's bundle design (the C in CCT).

It is worth remarking that effect 3 is only present if the conditionality is actually binding. Cardoso and de Souza (2004), for instance, show that this is only the case for boys. I will take advantage of this fact in the empirical section.

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[5] Individual's incentive constraint is given by $p[b_i + y_i - (1+r)l_i] \geq p[y_i + (1-\alpha)b_i]$, where p is the probability of project yielding success, with return y_i, while failure happens with probability $(1-p)$, yielding zero-return; l_i is the loan amount which must be repaid with an interest rate of r. This leads to $l_i \leq \frac{\alpha b_i}{(1+r)}$.

[6] A sufficient condition is that the CCT is suspended if the child misses any school-time and that $T > w_c$ and $T > \max_s \frac{\partial R(l_c'\,;\,s)}{\partial l_c'}$.

3. DATA AND EMPIRICAL STRATEGY

Bolsa-Família was created in 2003, targeting extremely poor families with or without children and those poor families with pregnant women or with children up to 15 years-old. Initially, minimum and maximum income thresholds were, respectively, R$ 50 and R$ 100 (about US$ 30 and US$ 60, in 2010 dollars), which were changed to R$ 60 and R$ 120 in 2006, and to R$ 70 and R$ 140 in 2010. From 2007 on, also, the program was extended to include youngsters in their 16 and 17, and created new benefits to target those families facing emergencies or for which Bolsa-Família institutionalization, which meant and aggregation of several previously existent benefits, would in principle mean a reduction in the total amount received.

The benefit is made of a fixed component, for which only households below the minimum income threshold are eligible, and a component per child (up to three children per household) – cumulative with the fixed one – as long as complying with the conditionality. For pregnant women, the conditionality stands for follow-up clinical exams, for those with up to 6-year-old children, for keeping child's vaccine record updated, and for those with children between 6 and 15 years old, for being enrolled in school with a minimum attendance of 85%. The fixed and per child components were, respectively, R$ 50 and R$ 10 until 2006, when they were increased to R$ 60 and R$ 15; in 2010, they were further increased to R$ 70 and R$ 20.

According to the Ministry of Social Development, Bolsa-Família increases family income on average on almost 50%. In case of non-compliance with program's conditionality, benefits are suspended for the entire household for the next month; in case of eventual re-incidence by the same child, benefits are not resumed.

The strategy to be able to separately estimate each of the effects of interest consists in comparing individuals similar in every observed relevant characteristic except that one group receives the treatment and the other, not. Lindert et al. (2007) and Barros (2008) support the view that the relevant targeting dimension is the municipality; along with Soares et al. (2009) claim that there is a considerable delay between enrolling in the program and actually receiving the benefit[7], it is as if there was randomization in program's recipient status at the local level.

While this attribute of the program design makes it unfeasible to use information on official thresholds to track the treatment group when actual information of participation is unavailable, it supports a strategy based on the hypothesis of identical potential results for treated and control units, conditional on observed attributes, when defining the treatment as actually receiving the transfer – in the sense that two individuals identical in every observed characteristic do not differ in treatment status due to unobserved features.[8]

[7] "In September, 2006, there were some 1.5 million cards [*, what corresponds to 13.6% of current beneficiaries,*] in the Mail and their recipients still had not actually received the benefit." (Soares et al., 2009, p. 12).

[8] I cannot use information on enrollment versus actual recipient status, since PNADs do make this distinction for Bolsa-Família; this is just to motivate that control and treatment might actually be comparable, despite somewhat large average differences in observable features. There is a natural concern that the order of enrollment (and hence first receiving the

I use PNADs 2004 and 2006 which specifically provide information about actual recipient households. For some of the effects of interest I need information on household income prior to Bolsa-Família transfer, but since PNADs only identify the recipient household, and not the particular individuals enrolled in the program – and since there is evidence that even inside the same household actual enrolment in social programs varies along eligible children (see Helfand and de Souza, 2009, for instance) –, I cannot calculate the ex-ante income prior to Bolsa-Família, only that prior to all transfers.

In order to the estimate program's effect on wealth constraints, I compare individuals based upon their income[9] *prior to the transfer*. Conditioning occupational choice on income is only possible because of previous results that document an absence of effects of Bolsa-Família on labor supply decisions (e.g. Medeiros, Britto and Soares, 2008).

Hence, if individuals who receive the transfer have a higher probability of starting a venture, then it is because they were initially wealth-constrained. To be able to condition on ex-ante wealth I rely on an instrumental variables approach, instrumenting ex-post income with its ex-ante component, as the following:

$$\begin{cases} Y_i = \beta_0 + \beta_1 \widehat{income}_{ex-post\,,i} + \sum_{k=2}^{m} \beta_k X_{k,i} + \gamma BF_i + \epsilon_i \\ income_{ex-post\,,i} = \alpha_0 + \alpha_1 income_{ex-ante\,,i} + \sum_{k=2}^{m} \alpha_k X_{k,i} + \alpha_{m+1} BF_i + \eta_i \end{cases},$$

where Y_i is the outcome of interest, BF_i stands for treatment status (=1 if the household reports to receive programs' transfer and = 0 otherwise), and the hypothesis of interest is $\gamma = 0$. The identification hypothesis is that ex-ante income only affects the entrepreneurial decision through its effect on ex-post income, what is trivially satisfied.[10]

To estimate program's insurance effect, I compare individuals based upon their income *after the transfer*. Therefore I compare individuals that have the same ability to pay for startup costs, but differ in the share of their wealth that is non-contingent to labor market outcomes. As such, I rely on an OLS strategy; the estimated equation is the following:

$$Y_i = \beta_0 + \beta_1 income_{ex-post\,,i} + \gamma BF_i + \sum_{k=2}^{m} \beta_k X_{k,i} + \epsilon_i \quad ,$$

where, again, the hypothesis of interest is $\gamma = 0$.

Finally, to estimate the effect of the conditionality, which (potentially) decreases the labor supply of children, I explore a stylized fact of Bolsa-Família in Brazil, which is that only boys decrease hours worked as

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benefit in a given community) could be explained by social capital or 'connections', which could also influence entrepreneurship; I analyze this claim in the robustness section.

[9] Controlling for every other observed features of wealth, such as household attributes, higher current income should mean higher wealth.

[10] The IV strategy in this context is just a trick, since I am not addressing potential concerns that ex-post income might be endogenous to extensive-margin labor decisions.

a response to the program (Cardoso and Portela, 2004). Hence, I restrict the sample to one-child households to compare whether the probability of starting a venture is heterogeneous according to child's gender. For that I add to the specifications above two other variables: a dummy variable that indicates whether the household has a son between 6 and 15 years old ($male\ eligible\ child$) and an interaction term of treatment status with the latter variable ($BF * male\ eligible\ child$). The hypothesis of interest is whether the coefficient of the interaction term is zero. If this hypothesis is rejected, there is a conditionality level-effect on entrepreneurship.

I pool 2004 and 2006 observations, including year fixed-effects. The sample is restricted to men who are household heads that are part of the active and not unemployed.[11]

Dependent variables are defined as the following: for the main occupation, $employer$ equals one if the firm owner employs at least one person, and zero otherwhise; $self - employed$ equals one if the firm owner does not employ anyone, and zero otherwhise; $entrepreneur$ equals one if the individual is either an employer or entrepreneur; $scale$ is a measure of firm size, given by its number of employees. Additionally, the occupational definitions are augmented to include secondary sources of income. All definitions are presented in Appendix A.

Table 1 below reports the distribution of treatment status in each sample year. One can notice a remarkable expansion of the program from 2004 to 2006; treated units stand to roughly 10.7% of the pooled sample.

Table 1 – Incidence of treatment by year

	Year		
Bolsa-Família	**2004**	**2006**	**Total**
0	80,402	73,829	154,231
1	3,924	14,600	18,524
Observations	84,326	88,429	172,755

Tables 2 and 3 present the distribution of covariates and outcomes of interest for the treatment and control groups:

[11] I focus on men because of the remarkably different women's participation decision, as is standard of this literature.

Table 2 – Distribution of covariates by treatment status

	Control		Treatment		C = T	
Variable	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	**t-statistic**	**p-value**
household ownership	0.71	0.45	0.75	0.43	11.14	0.0000
water	0.78	0.41	0.60	0.49	-48.23	0.0000
sewage	0.53	0.50	0.26	0.44	-77.35	0.0000
electricity	0.97	0.17	0.93	0.25	-19.87	0.0000
Telephone	0.73	0.44	0.46	0.50	-70.66	0.0000
computer	0.22	0.41	0.02	0.14	-135.60	0.0000
internet	0.16	0.37	0.01	0.09	-135.63	0.0000
urban	0.85	0.36	0.66	0.47	-51.56	0.0000
age	42.68	13.06	40.71	10.96	-22.69	0.0000
white	0.50	0.50	0.29	0.45	-61.10	0.0000
schooling	7.17	4.60	3.88	3.46	-117.57	0.0000
ex-ante household pc income	589.38	1094.09	123.49	108.58	-160.77	0.0000
ex-post household pc income	594.64	1113.21	134.95	110.13	-155.94	0.0000

Table 3 – Distribution of dependent variables by treatment status

	Control		Treatment		C = T	
Variable	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	**t-statistic**	**p-value**
entrepreneur	0.36	0.48	0.41	0.49	13.41	0.0000
employer	0.07	0.25	0.02	0.14	-41.57	0.0000
self-employed	0.29	0.45	0.39	0.49	26.87	0.0000
scale	4.09	2.53	2.45	1.97	-12.72	0.0000
employer2	0.003	0.057	0.001	0.037	-6.142	0.0000
self-employed2	0.019	0.137	0.016	0.126	-3.031	0.0024

These descriptive statistics present nothing different than expected given the program's reported efficient targeting: treated units are poorer, have less access to public goods and have lower human capital. Moreover, poor individuals are known to recur to self-employment more often, as an alternative to unemployment (Robalino, Rime and Walker, 2010).

Restricting attention to the lower quintile based upon income *ex-ante* to transfers (households with income below R$ 96.67 [12]), an unconditional comparison between treatment and control provides the following:

[12] Values have been deflated to R$ 2006.

Table 4 – Distribution of covariates by treatment status for the first *ex-ante* income quintile

	Control		Treatment		C = T	
Variable	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	**t-statistic**	**p-value**
household ownership	0.75	0.43	0.77	0.42	3.27	0.0011
water	0.56	0.50	0.50	0.50	-11.27	0.0000
sewage	0.27	0.44	0.18	0.38	-19.11	0.0000
electricity	0.90	0.30	0.90	0.30	-0.24	0.8085
Telephone	0.38	0.49	0.31	0.46	-13.04	0.0000
computer	0.04	0.20	0.00	0.06	-26.56	0.0000
internet	0.03	0.17	0.00	0.04	-24.82	0.0000
urban	0.64	0.48	0.55	0.50	-14.95	0.0000
age	45.49	15.59	40.96	11.39	-29.82	0.0000
white	0.35	0.48	0.26	0.44	-16.63	0.0000
schooling	3.74	3.79	2.98	3.09	-19.25	0.0000
ex-ante household pc income	66.49	33.04	61.00	30.33	-15.06	0.0000
ex-post household pc income	71.00	41.03	72.22	32.92	2.93	0.0034

Table 4 shows that notwithstanding a significant narrowing of differences in covariates between the groups, they remain positive and significant: it confirms that program recipients are, ex-ante, the poorest among the poor. A distinctive feature, though, is that among lowest income quintile individuals, recipient households have higher average ex-post per capita income, what attests the relevance of Bolsa Família's cash transfer for the very poor. That also remarks the need to restrict attention to the marginal individuals – those whose occupational choice might have been affected by the CCT – in order to adequately capture the effects of interest under a conditional analysis.

Table 5 – Distribution of dependent variables by treatment status the first *ex-ante* income quintile

	Control		Treatment		C = T	
Variable	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	**t-statistic**	**p-value**
entrepreneur	0.43	0.50	0.46	0.50	4.84	0.0000
employer	0.02	0.14	0.01	0.10	-7.93	0.0000
self-employed	0.41	0.49	0.45	0.50	6.70	0.0000
scale	3.43	2.52	1.64	1.39	-7.40	0.0000
employer2	0.001	0.029	0.000	0.020	-1.696	0.0898
self-employed2	0.011	0.106	0.014	0.119	2.107	0.0352

Interestingly, despite the somewhat narrowing in differences in covariates displayed in table 4, table 5 shows that the difference in outcomes continues to be significant once I restrict attention to the lower income quintile. Moreover, while self-employment is more prevalent among treated individuals, employer status is actually less prevalent among them. This feature pushes for further investigation of whether these results are maintained under a conditional analysis.

4. RESULTS

4.1 – Baseline results

All regression tables are displayed in Appendix B. Table 6 presents results for IV estimates of the wealth constraint alleviation channel. The results – which change quite substantially throughout specifications, pointing to non-random treatment adoption, something I already accounted for trough last sections – are that entrepreneurship is estimated to be slightly less than 3% higher among those who receive Bolsa-Família when compared to individuals who had the same ex-ante wealth but were not part of the treatment group.

[Insert Table 6 here]

Next, Table 7 is concerned with the OLS results for this effect of the program. Once again the results fluctuate pretty much throughout specifications but, after controlling for individual and household attributes, the share of the ex-post income explained by Bolsa-Família is reported to have a positive and significant effect on entrepreneurial status.

[Insert Table 7 here]

It is worth remarking that, since the difference between ex-ante and ex-post income is due to transfers and income from financial assets, for most of the population this difference is zero, and hence the estimated effects for the first two channels cannot be distinguished without restricting attention to the subsample of eligible individuals, what we pursue throughout next section.

In what comes to the effect of the conditionality, table 8 reports results for the restricted sample of households heads with only-male or only-female eligible children (between 6 and 15 years old). It turns out that although recipient households with a male child seem to have a slightly lower propensity to start a venture, this difference is not statistically significant.

[Insert Table 8 here]

Of course, these results might not reflect causal estimates due to a variety of reasons. It might be the case that there are state-level attributes which condition the entrepreneurial decision and are also correlated with individual observable characteristics, what would bias previous estimates. Also, it might be the case that enrolled individuals just have a higher unobserved propensity to be entrepreneurs that is correlated with the enrollment decision, due to higher average social capital than individuals in the control group, for instance. Moreover, it might be the case that, since drawing upon a cross section, reverse causality is at stake, with entrepreneurs (especially because my definition includes self-employed) having a higher propensity to be program's beneficiaries. Finally, it might just be that a linear model cannot properly

estimate the difference in outcomes for truly comparable control and treatment units. I address these concerns throughout the next subsections.

4.2 – State fixed-effects, employer and self-employed

First, I include State fixed-effects and decompose entrepreneurship among employer status and self-employment, also looking at firm's scale. Tables 9 to 11 present the results for each of the transmission channels, just displaying the coefficients of the variables of interest. Last section results are qualitatively maintained, although quantitatively smaller, pointing to an increased probability of starting a new venture of 1.76% through the wealth constraint alleviation channel and 1.73% through the insurance one.

Alternative explanations for this effect, linked to concerns that enrolled individuals might also have higher social capital, or a higher unobserved propensity to entrepreneurship, seem to be dismissed by a composition effect: the increased propensity to start a venture comes entirely from self-employment, while there seems to be some transitioning from employers to self-employment. Whether this points to the effect of the binding conditionality – through parents dismissing their children of working in their parent's firm in order to attend school –, or to misreporting of parents – afraid of losing benefits in account of keeping their children in the business – is not relevant for my concerns: what matters is that higher social capital or a higher unobserved propensity to start a venture cannot account for this effect. When looking at firm's scale, it turns out that even those who remain employers (along with potentially new employers) have a smaller number of employees, what is consistent with the above-stated results.

4.3 – Secondary sources of income

Tables 12 to 14 look at entrepreneurship in all occupations, since it could be the case that the program has a differential effect on the main occupation and on secondary ones. Previous pattern is maintained but while the coefficient is basically unchanged for employer status, it increases for self-employed individuals, hence increasing for overall entrepreneurship. Collecting these results with the previous one points to a rearrangement effect for the main occupation, with some employers transitioning to self-employment, and a spark of self-employment in secondary occupations – individuals who are primarily employees start a venture as an alternative source of income.

4.4 – Lower income quintiles and other transfers

It could be the case that, since I am including individuals with much higher income in the control group, treated individual are still much poorer, and poor individuals are known to have a lower propensity to be employers and a higher propensity to be self-employed than their richer counterparts. An additional concern is that poor individuals are eligible for many governmental transfers, and since households usually receive a cluster of transfers, the effect captured could be attributed to programs other than Bolsa-Família. Taking advantage of the fact that PNADs 2004 and 2006 also survey households on recipient status

concerning every other governmental transfer[13], and restricting the sample to the two lower ex-ante income quintiles, I reassess the effect of the program on entrepreneurship through each channel of interest.

Tables 15 to 18 condense the results. It turns out that the wealth constraint alleviation and insurance effects are no longer significant for an individual's main occupation: there is only a composition effect, lowering the probability of becoming an employer but increasing that of self-employment – although the punctual statistic is higher for the latter, this difference is not statistically significant. However, when I look at all occupations, it turns out that the wealth constraint alleviation channel is present for overall entrepreneurship, but this comes from increased self-employment in a secondary occupation. This means that Bolsa-Família is not effective in supporting credit constrained individuals in starting their own business as their main occupation by allowing them to pay a startup cost, but this effect is present (around 1.8%) for ventures that serve as a secondary source of income.

Surprisingly, other transfers do not have any significant effect in providing insurance, and this might be due to the size of the transfer: Bolsa-Família is on average much larger in value, standing for up to 49% of the lowest decile beneficiary's average household income.[14]

In what comes to the level effect of the conditionality, it is still the case that it is not statistically significant. Additionally, there is no contradiction between the documented fact that only boys respond to the conditionality by working fewer hours and the absence of a heterogeneous composition effect – in the sense that it should be expected that boys were more often dismissed from their jobs in parent's firms – since its effect on reported occupational choice would depend on an extensive margin decision for children (not merely working less hours, but not working at all), on the firm having more than one employee, etc.

4.5 – Collateral and wealth constraint

Table 19 backs-up the interpretation of wealth-constraint alleviation: when I interact treatment with an indicator variable of house ownership, it turns out that program's effect is smaller among those individuals who report to own their houses (and hence would be able to use them as collateral), but this effect is not statistically significant, maybe because survey's question is about ownership as opposed to tenure.[15]

4.6 – Rural vs. urban

I further decompose the estimates into rural vs. urban sub-samples to assess the program's potential heterogeneity in this dimension. Tables 20 to 23 show that previous results come entirely from the urban sub-sample, while program's positive effect on entrepreneurship is concentrated on secondary sources of income. When I investigate the activity composition for those individuals allocated to self-employment in secondary sources of income, as displayed in table 24, it turns out they are mainly involved in small scale

[13] No individual in this subsample lives in a household which receives pensions or social security benefits from INSS, what reflects the very low formality rates among these households.

[14] I thank Romero Barreto for these figures; calculations based upon PNADs.

[15] What would imply that tenure is measured with error by this variable, in the sense that only a fraction of the individuals who report to own their houses would actually be able to use it as collateral. In turn, this clearly leads to an attenuation of the estimated coefficient.

commerce and service ventures, activities that are better suited to urban markets and, hence, might involve lower startup costs when compared to those in rural environments.

These are also consistent with evidence provided by the Ministry of Social Development concerning a cross-tabulation of Bolsa-Família beneficiaries and those of the government's microcredit programs, *Crediamigo and Agroamigo*. For the former, available at urban areas, data from December 2009 reveal that "the majority of the clients who became entrepreneurs were already beneficiaries of Ministry of Social Development (MDS)'s transfer: 51.5% (or 204 thousand families)", whereas for the latter, available at rural areas, only 37.3% of the clients were already Bolsa-Família beneficiaries, according to the MDS' assessment.[16]

Results are reassuring because they also push away concerns with reverse causality: if it was actually the case that employer individuals are just less likely to enroll in Bolsa-Família, as opposed to a higher propensity to enroll of those self-employed, why would it be the case only in urban areas? Conversely, I claim that the lack of a composition effect of the program in the rural sub-sample is due to the different nature of rural ventures, with a much lower flexibility to dismiss children from work to keep the business running as opposed to those traditionally found in urban areas.

4.7 – Capital-intensity

Table 25 shows that the wealth-constraint alleviation effect is stronger for activities with higher startup costs, which confirms the interpretation that it is the program which increases the propensity to engage in a startup, and not the opposite. Breaking up activities by commerce versus services, it turns out that the positive effect on entrepreneurship comes entirely from services, what can be reconciled with higher startup costs for this activity, since small scale commerce is often carried out through consignation – meaning that goods can be returned to the supplier at no charge to the seller were him or her unable to sell them –, while service provision typically involves acquiring tools or inputs which require upfront payments.

4.8 – Mechanism

Moving on to investigate the composition effect found for the main occupation, working with a sample of eligible children, I find evidence that children in recipient households have a smaller propensity to work as compared to those whose parents are also employers but do not receive the transfer; the results are displayed in Table 26.

4.9 – Robustness checks

Last, Tables 27 and 28 address concerns related to the potential endogeneity of the income variables, and to possible dependence of the results on specification: the former displays estimates of program's effect on entrepreneurship comparing individuals only upon the available measures of household wealth, while the latter displays the results of a nearest-neighbor matching estimation for each subsample. If anything, results are even stronger in these alternative specifications.

4.10 – Taking stock

[16] MDS (2010, p. 70).

Collecting results, the claim that enrolled individuals – due to self-selection into the program – have a higher unobserved propensity to start a venture is not compatible with the estimated negative result for employer status. It turns out that it is difficult to reconcile these findings with any other explanation than the transitioning from small firms where parents employed their own children to firms without employers under the school attendance requirements for enrolled children (be it either an actual re-arrangement or a mere 'reporting' one), along with some new startups – self-employed individuals in secondary sources of income – since the net effect on entrepreneurship is positive.

5. CONCLUDING REMARKS

This paper assesses whether Bolsa-Família increases the probability of starting a new venture in Brazil, decomposing its potential effects into three channels: alleviation of wealth constraints, insurance against bad outcomes of risky activities, and reduction of labor supply of children (through the effect of the conditionality). I draw upon PNADs 2004 and 2006, which are the only years for which transfer beneficiaries are in fact identified.

The results are that entrepreneurship is indeed stimulated by the program in urban areas through the insurance and wealth constraint alleviation effects, notwithstanding that new ventures are typically secondary sources of income. The conditionality does not impact the level of entrepreneurship. Alternative explanations for this effect, linked to concerns that enrolled individuals might also have higher social capital, or a higher unobserved propensity to entrepreneurship, seem to be dismissed by a composition effect in what comes to the main occupation: there seems to be some transitioning from employers to self-employment. Whether this points to the effect of the binding conditionality – through parents dismissing their children of working in their parent's firm in order to attend school – or to misreporting of parents – afraid of losing benefits in account of keeping their children in the business – is not relevant for my concerns: what matters is that higher social capital or a higher unobserved propensity to start a venture cannot account for this effect.

Recipient household heads use the cash transfer to diversify their income portfolio; this can also be regarded as positive effect of the program, potentially enhancing the ability of the poor to protect against adverse economic shocks. Moreover, this effect is concentrated in urban areas, where Bolsa-Família first-order effect, that of reducing current poverty, was shown to be less pervasive than in rural areas. Also of interest is the fact that Bolsa-Família is different from other transfers in what comes to providing insurance, what is reasonable given that the cash transfer stands, in some cases, for a large share of poor household's income.

Finally, I do not find any negative effect of the program on entrepreneurship due to the conditionality (be it because children's productivity is not indeed higher 'inside' the firm or because children actually do not stop working in parents' venture); had I found a negative effect, it might have had implications for the CCT bundle's redesign if the government thought this kind of occupational choice should be further stimulated by the program. It turns out that this short-term vs. long-term productivity trade-off (since enrolled children are more likely to become more educated grownups, hence with a higher propensity towards entrepreneurial activities) does not exist in what comes to program's conditionality.

These findings point to two relevant issues. First, the program stimulates self-employment; if this is regarded as a higher productivity activity, then Bolsa-Família might have a positive long-term effect, more than just offering short-term poverty relief – a possibility that I entertain on a sounder basis given Gertler, Martinez and Rubio-Codina's (2007) results that the proceeds from the transfer were used in the course of five years to increase family income-generating capacity. Second, it challenges the view that employer status is inherently superior to self-employment, especially among the poor. If poor parents systematically employ their children when they are entrepreneurs, then it might actually be superior, in what comes to social welfare, to be self-employed than an employer, if that has an overall positive effect on children's human capital accumulation.

Of course, it is hard to be strongly convincing about the evidence here presented without being able to follow individuals through time in order to observe how their choice of occupation varies with treatment status, and also whether their children's occupational status actually changes after program enrollment. Nonetheless, so far there is no such database which would allow one to carry on this 'higher-quality' assessment. At the least, this paper contributes with a methodology to empirically distinguish among the several transmission channels of CCTs to entrepreneurship, and entertains sensitive hypotheses that could drive further research in order to refine its findings. The likelihood of alternative explanations is minimized to the utmost given data availability and the results are shown to be very robust to specification-choice, though operating through very specific channels, in a sub-sample of the recipient population.

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Appendix A – Dependent variables

Main occupation:

$$employer = \begin{cases} 1\,, if\ employees \geq 1 \\ 0\,, otherwise \end{cases}$$

$$self - employed = \begin{cases} 1\,, if\ employees = 0 \\ 0\,, otherwise \end{cases}$$

$$entrepreneur = \begin{cases} 1\,, if\ employees \geq 0 \\ 0\,, otherwise \end{cases}$$

$$scale = \begin{cases} 1\,, if\ employees = 1 \\ 3\,, if\ employees = 2 \\ 5\,, if\ employees \in [3,5] \\ 7\,, if\ employees \in [6,10] \\ 8\,, if\ employees \geq 11 \end{cases}$$

Secondary sources of income:

$$employer2 = \begin{cases} 1\,, if\ employees \geq 1 \\ 0\,, otherwise \end{cases}$$

$$self - employed2 = \begin{cases} 1\,, if\ employees = 0 \\ 0\,, otherwise \end{cases}$$

$$entrepreneur2 = \begin{cases} 1\,, if\ employees \geq 0 \\ 0\,, otherwise \end{cases}$$

Appendix B – Regression tables

Table 6 – IV Results: Wealth constraint alleviation effect

	(1) entrepreneur	(2) entrepreneur	(3) entrepreneur
Bolsa-Família	0.0657***	0.0222***	0.0290***
	(0.0038)	(0.0039)	(0.0039)
ex-ante household pc income	1.82e-05***	2.58e-05***	2.65e-05***
	(0.0000)	(0.0000)	(0.0000)
household ownership		0.133***	0.0886***
		(0.0025)	(0.0026)
Water		-0.0293***	-0.0235***
		(0.0036)	(0.0035)
Sewage		-0.0349***	-0.0394***
		(0.0027)	(0.0027)
Electricity		-0.114***	-0.111***
		(0.0067)	(0.0066)
Telephone		-0.00592**	0.00671**
		(0.0029)	(0.0029)
Computer		0.0363***	0.0551***
		(0.0054)	(0.0054)
Internet		-0.0209***	-0.0130**
		(0.0060)	(0.0059)
Urban		-0.126***	-0.0827***
		(0.0040)	(0.0040)
Age			0.00614***
			(0.0001)
age^2			-6.70e-06***
			(0.0000)
White			0.0434***
			(0.0024)
Schooling			-0.00991***
			(0.0003)
Constant	0.355***	0.511***	0.288***
	(0.0018)	(0.0065)	(0.0078)
YEAR FIXED-EFFECTS	YES	YES	YES
UF FIXED-EFFECTS	NO	NO	NO
Observations	172,755	172,755	171,909
R-squared	0.003	0.044	0.077

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 7 – OLS Results: Insurance effect

	(4) entrepreneur	(5) entrepreneur	(6) entrepreneur
Bolsa-Família	0.0659***	0.0222***	0.0290***
	(0.0038)	(0.0039)	(0.0039)
ex-ante household pc income	1.85e-05***	2.60e-05***	2.65e-05***
	(0.0000)	(0.0000)	(0.0000)
household ownership		0.133***	0.0886***
		(0.0025)	(0.0026)
Water		-0.0293***	-0.0235***
		(0.0036)	(0.0035)
Sewage		-0.0349***	-0.0394***
		(0.0027)	(0.0027)
Electricity		-0.114***	-0.111***
		(0.0067)	(0.0066)
Telephone		-0.00594**	0.00671**
		(0.0029)	(0.0029)
Computer		0.0362***	0.0551***
		(0.0054)	(0.0054)
Internet		-0.0210***	-0.0130**
		(0.0060)	(0.0059)
Urban		-0.126***	-0.0827***
		(0.0040)	(0.0040)
Age			0.00614***
			(0.0001)
age^2			-6.70e-06***
			(0.0000)
White			0.0434***
			(0.0024)
Schooling			-0.00991***
			(0.0003)
Constant	0.355***	0.510***	0.288***
	(0.0018)	(0.0065)	(0.0078)
YEAR FIXED-EFFECTS	YES	YES	YES
UF FIXED-EFFECTS	NO	NO	NO
Observations	172,755	172,755	171,909
R-squared	0.003	0.044	0.077

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 8 – Conditionality effects

	Wealth constraint alleviation (IV)			Insurance (OLS)		
	(5) entrepreneur	(6) entrepreneur	(7) entrepreneur	(5) entrepreneur	(6) entrepreneur	(7) entrepreneur
Bolsa-Família	0.0780***	0.0355***	0.0272***	0.0783***	0.0356***	0.0273***
	(0.0091)	(0.0091)	(0.0091)	(0.0091)	(0.0091)	(0.0091)
male eligible son	0.000106	0.00264	0.00388	0.0000985	0.00264	0.00388
	(0.0091)	(0.0089)	(0.0088)	(0.0091)	(0.0089)	(0.0088)
BF * male eligible son	0.00870**	0.00475	0.00453	0.00871**	0.00476	0.00453
	(0.0043)	(0.0042)	(0.0042)	(0.0043)	(0.0042)	(0.0042)
ex-ante household pc income	4.09e-05***	5.53e-05***	6.15e-05***	4.18e-05***	5.61e-05***	6.22e-05***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
household ownership		0.110***	0.0914***		0.110***	0.0914***
		(0.0057)	(0.0057)		(0.0057)	(0.0057)
Water		-0.0230***	-0.0165**		-0.0230***	-0.0164**
		(0.0077)	(0.0076)		(0.0077)	(0.0076)
Sewage		-0.0465***	-0.0469***		-0.0466***	-0.0469***
		(0.0059)	(0.0059)		(0.0059)	(0.0059)
Electricity		-0.143***	-0.134***		-0.143***	-0.134***
		(0.0151)	(0.0149)		(0.0151)	(0.0149)
Telephone		-0.0116*	0.00127		-0.0116*	0.00123
		(0.0063)	(0.0064)		(0.0063)	(0.0064)
Computer		0.0786***	0.0870***		0.0784***	0.0869***
		(0.0114)	(0.0114)		(0.0114)	(0.0114)
Internet		-0.0509***	-0.0454***		-0.0513***	-0.0456***
		(0.0130)	(0.0129)		(0.0130)	(0.0129)
Urban		-0.133***	-0.102***		-0.133***	-0.102***
		(0.0086)	(0.0086)		(0.0086)	(0.0086)
Age			0.00692***			0.00692***
			(0.0003)			(0.0003)
age^2			-7.04e-06***			-7.04e-06***
			(0.0000)			(0.0000)
White			0.0421***			0.0420***
			(0.0052)			(0.0052)
Schooling			-0.00994***			-0.00996***
			(0.0008)			(0.0008)
Constant	0.340***	0.546***	0.289***	0.340***	0.546***	0.289***
	(0.0048)	(0.0148)	(0.0197)	(0.0048)	(0.0148)	(0.0197)
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	NO	NO	NO	NO	NO	NO
Observations	35,179	35,179	34,954	35,179	35,179	34,954
R-squared	0.005	0.046	0.068	0.005	0.046	0.068

Table 9 – Level and composition wealth constraint effects with State fixed-effects

	(1) entrepreneur	(2) employer	(3) self-employed	(4) scale
Bolsa-Família	0.0176***	-0.0125***	0.0301***	-0.561***
	(0.0039)	(0.0020)	(0.0037)	(0.1590)
Individual controls	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES
Observations	171,909	171,909	171,909	10,242
R-squared	0.085	0.069	0.088	0.139

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 10 – Level and composition insurance effects with UF fixed-effects

	(1) entrepreneur	(2) employer	(3) self-employed	(4) scale
Bolsa-Família	0.0176***	-0.0125***	0.0301***	-0.562***
	(0.0039)	(0.0020)	(0.0037)	(0.1590)
Individual controls	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES
Observations	171,909	171,909	171,909	10,242
R-squared	0.085	0.069	0.088	0.139

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 11 – Level and composition conditionality effects with UF fixed-effects

	Wealth constraint alleviation (IV)				Insurance (OLS)			
	(1) entrepreneur	(2) employer	(3) self-employed	(4) scale	(1) entrepreneur	(2) employer	(3) self-employed	(4) scale
Bolsa-Família	0.0163*	-0.00945**	0.0257***	-0.785**	0.0163*	-0.00946**	0.0257***	-0.786**
	(0.0092)	(0.0046)	(0.0087)	(0.3640)	(0.0092)	(0.0046)	(0.0087)	(0.3640)
male eligible son	0.00381	-0.00355	0.00736	0.0282	0.0038	-0.00355	0.00736	0.0284
	(0.0088)	(0.0044)	(0.0083)	(0.4350)	(0.0088)	(0.0044)	(0.0083)	(0.4350)
BF * male eligible son	0.00459	0.00283	0.00176	0.0532	0.0046	0.00283	0.00176	0.053
	(0.0042)	(0.0021)	(0.0040)	(0.0858)	(0.0042)	(0.0021)	(0.0040)	(0.0858)
Individual controls	YES	YES	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES	YES	YES
Observations	34,954	34,954	34,954	2,006	34,954	34,954	34,954	2,006
R-squared	0.078	0.09	0.086	0.157	0.078	0.09	0.086	0.157

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 12 – Level and composition wealth constraint effects in secondary sources of income

	(1) entrepreneur2	(2) employer2	(3) self-employed2
Bolsa-Família	0.00272**	0.000127	0.00259**
	(0.0012)	(0.0005)	(0.0011)
Individual controls	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES
Observations	171,909	171,909	171,909
R-squared	0.008	0.006	0.005

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 13 – Level and composition insurance effects in secondary sources of income

	(1) entrepreneur2	(2) employer2	(3) self-employed2
Bolsa-Família	0.00271**	0.000122	0.00259**
	(0.0012)	(0.0005)	(0.0011)
Individual controls	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES
Observations	171,909	171,909	171,909
R-squared	0.008	0.006	0.005

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 14 – Level and composition conditionality effects in secondary sources of income

	Wealth constraint alleviation (IV)			Insurance (OLS)		
	(1) entrepreneur2	(2) employer2	(3) self-employed2	(1) entrepreneur2	(2) employer2	(3) self-employed2
Bolsa-Família	-0.00191	0.00101	-0.00291	-0.00191	0.001	-0.00291
	(0.0029)	(0.0011)	(0.0027)	(0.0029)	(0.0011)	(0.0027)
male eligible son	-0.00154	-0.00139	-0.000148	-0.00154	-0.00139	-0.000148
	(0.0028)	(0.0011)	(0.0026)	(0.0028)	(0.0011)	(0.0026)
BF * male eligible son	0.000652	0.00053	0.000119	0.000651	0.000532	0.000119
	(0.0013)	(0.0005)	(0.0012)	(0.0013)	(0.0005)	(0.0012)
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
Observations	34,954	34,954	34,954	34,954	34,954	34,954
R-squared	0.01	0.01	0.006	0.01	0.01	0.006

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 15 – Wealth constraint alleviation and insurance effects: lower income quintiles and other transfers

	Wealth constraint alleviation (IV)			Insurance (OLS)		
	(1) entrepreneur	(2) employer	(3) self-employed	(1) entrepreneur	(2) employer	(3) self-employed
Bolsa-Família	0.0105**	-0.00404***	0.0145***	0.0130***	-0.00392***	0.0169***
	(0.0046)	(0.0014)	(0.0046)	(0.0046)	(0.0014)	(0.0045)
Other transfers	-0.00384	-0.00171	-0.00213	-0.00216	-0.00163	-0.000528
	(0.0049)	(0.0015)	(0.0048)	(0.0049)	(0.0015)	(0.0048)
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
Observations	68,740	68,740	68,740	68,740	68,740	68,740
R-squared	0.099	0.022	0.095	0.1	0.022	0.095

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 16 – Conditionality effects: lower income quintiles and other transfers

	Wealth constraint alleviation (IV)			Insurance (OLS)		
	(1) entrepreneur	(2) employer	(3) self-employed	(1) entrepreneur	(2) employer	(3) self-employed
Bolsa-Família	0.0157	-0.00325	0.0190*	0.0171	-0.0032	0.0203*
	(0.0105)	(0.0031)	(0.0104)	(0.0105)	(0.0031)	(0.0104)
Other transfers	-0.0103	-0.00491*	-0.00543	-0.00939	-0.00487*	-0.00451
	(0.0086)	(0.0025)	(0.0085)	(0.0086)	(0.0025)	(0.0085)
male eligible son	0.0011	0.0000242	0.00107	0.00139	0.0000346	0.00136
	(0.0057)	(0.0017)	(0.0056)	(0.0057)	(0.0017)	(0.0056)
BF * male eligible son	0.0058	-0.00134	0.00714	0.0061	-0.00133	0.00743
	(0.0098)	(0.0029)	(0.0097)	(0.0098)	(0.0029)	(0.0097)
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
Observations	19,012	19,012	19,012	19,012	19,012	19,012
R-squared	0.1	0.02	0.099	0.101	0.02	0.099

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 17 – Wealth constraint alleviation and insurance effects: lower income quintiles and other transfers – secondary sources of income

	Wealth constraint alleviation (IV)			Insurance (OLS)		
	(1) entrepreneur2	(2) employer2	(3) self-employed2	(1) entrepreneur2	(2) employer2	(3) self-employed2
Bolsa-Família	0.00364***	-0.00037	0.00401***	0.00348***	-0.000367	0.00385***
	(0.0012)	(0.0003)	(0.0011)	(0.0012)	(0.0003)	(0.0011)
Other transfers	0.00559***	0.000334	0.00526***	0.00549***	0.000335	0.00515***
	(0.0012)	(0.0003)	(0.0012)	(0.0012)	(0.0003)	(0.0012)
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
Observations	68,740	68,740	68,740	68,740	68,740	68,740
R-squared	0.008	0.004	0.007	0.008	0.004	0.007

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 18 – Conditionality effects: lower income quintiles and other transfers – secondary sources of income

	Wealth constraint alleviation (IV)			Insurance (OLS)		
	(1) entrepreneur	(2) employer	(3) self-employed	(1) entrepreneur	(2) employer	(3) self-employed
Bolsa-Família	0.000931	0.0022	0.0171	0.00309	0.000914	0.00218
	(0.0007)	(0.0027)	(0.0105)	(0.0027)	(0.0007)	(0.0027)
Other transfers	0.0000802	0.00430**	-0.00939	0.00436*	0.0000688	0.00429**
	(0.0006)	(0.0022)	(0.0086)	(0.0023)	(0.0006)	(0.0022)
male eligible son	0.0000533	0.00349**	0.00139	0.00354**	0.0000497	0.00349**
	(0.0004)	(0.0014)	(0.0057)	(0.0015)	(0.0004)	(0.0014)
BF * male eligible son	-0.000903	-0.00224	0.0061	-0.00315	-0.000906	-0.00224
	(0.0006)	(0.0025)	(0.0098)	(0.0026)	(0.0006)	(0.0025)
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
Observations	19,012	19,012	19,012	19,012	19,012	19,012
R-squared	0.013	0.004	0.012	0.013	0.004	0.012

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 19 – Wealth constraint alleviation effect: mechanism

	(1) entrepreneur2	(2) employer2	(3) self-employed2
Bolsa-Família	0.00395*	-0.000527	0.00447**
	(0.0022)	(0.0005)	(0.0021)
house ownership	-0.000169	-0.000136	-0.0000337
	(0.0012)	(0.0003)	(0.0011)
BF * house ownership	-0.000422	0.000212	-0.000635
	(0.0024)	(0.0006)	(0.0024)
Other transfers	0.00559***	0.000335	0.00525***
	(0.0012)	(0.0003)	(0.0012)
Individual controls	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES
Observations	68,740	68,740	68,740
R-squared	0.008	0.004	0.007

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Tables 20A and 20B – Wealth constraint alleviation and insurance effects: lower income quintiles and other transfers (rural sub-sample)

	Wealth constraint alleviation (IV)			Insurance (OLS)		
	(1) entrepreneur	(2) employer	(3) self-employed	(1) entrepreneur	(2) employer	(3) self-employed
Bolsa-Família	0.0101	-0.00109	0.0112	0.0115	-0.00115	0.0126
	(0.0082)	(0.0024)	(0.0083)	(0.0082)	(0.0024)	(0.0083)
Other transfers	-0.00773	0.00361	-0.0113	-0.00694	0.00358	-0.0105
	(0.0088)	(0.0026)	(0.0089)	(0.0088)	(0.0026)	(0.0089)
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
Observations	19,152	19,152	19,152	19,152	19,152	19,152
R-squared	0.175	0.026	0.161	0.015	0.007	0.013

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

	Wealth constraint alleviation (IV)			Insurance (OLS)		
	(1) entrepreneur2	(2) employer2	(3) self-employed2	(1) entrepreneur2	(2) employer2	(3) self-employed2
Bolsa-Família	0.00329	-0.00084	0.00413	0.00301	-0.000788	0.0038
	(0.0026)	(0.0007)	(0.0026)	(0.0026)	(0.0007)	(0.0026)
Other transfers	0.00714**	0.00106	0.00608**	0.00698**	0.00108	0.00590**
	(0.0028)	(0.0007)	(0.0028)	(0.0028)	(0.0007)	(0.0028)
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
Observations	19,152	19,152	19,152	19,152	19,152	19,152
R-squared	0.175	0.026	0.161	0.015	0.007	0.013

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Tables 21A and 21B – Conditionality effects: lower income quintiles and other transfers (rural sub-sample)

	Wealth constraint alleviation (IV)			Insurance (OLS)		
	(1) entrepreneur	(2) employer	(3) self-employed	(1) entrepreneur	(2) employer	(3) self-employed
Bolsa-Família	0.000409	0.000446	-0.0000364	0.00168	0.000533	0.00114
	(0.0194)	(0.0050)	(0.0196)	(0.0194)	(0.0050)	(0.0196)
Other transfers	-0.0201	-0.00159	-0.0185	-0.02	-0.00159	-0.0184
	(0.0166)	(0.0043)	(0.0167)	(0.0166)	(0.0043)	(0.0167)
male eligible son	-0.0138	-0.000395	-0.0134	-0.0135	-0.000374	-0.0131
	(0.0114)	(0.0029)	(0.0115)	(0.0114)	(0.0029)	(0.0115)
BF * male eligible son	0.0187	0.000317	0.0184	0.019	0.000335	0.0186
	(0.0175)	(0.0045)	(0.0176)	(0.0175)	(0.0045)	(0.0176)
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
Observations	5,022	5,022	5,022	5,022	5,022	5,022
R-squared	0.182	0.035	0.171	0.025	0.028	0.021

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

	Wealth constraint alleviation (IV)			Insurance (OLS)		
	(1) entrepreneur2	(2) employer2	(3) self-employed2	(1) entrepreneur2	(2) employer2	(3) self-employed2
Bolsa-Família	0.00129	0.000399	0.000892	0.00119	0.000367	0.000826
	(0.0066)	(0.0017)	(0.0065)	(0.0066)	(0.0017)	(0.0065)
Other transfers	0.00574	0.000329	0.00542	0.00574	0.000326	0.00541
	(0.0057)	(0.0014)	(0.0055)	(0.0057)	(0.0014)	(0.0055)
male eligible son	0.00904**	-0.000673	0.00972**	0.00902**	-0.00068	0.00970**
	(0.0039)	(0.0010)	(0.0038)	(0.0039)	(0.0010)	(0.0038)
BF * male eligible son	-0.00963	-0.000396	-0.00924	-0.00966	-0.000403	-0.00925
	(0.0060)	(0.0015)	(0.0058)	(0.0060)	(0.0015)	(0.0058)
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
Observations	5,022	5,022	5,022	5,022	5,022	5,022
R-squared	0.183	0.035	0.171	0.025	0.028	0.021

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Tables 22A and 22B – Wealth constraint alleviation and insurance effects: lower income quintiles and other transfers (urban sub-sample)

	Wealth constraint alleviation (IV)			Insurance (OLS)		
	(1) entrepreneur	(2) employer	(3) self-employed	(1) entrepreneur	(2) employer	(3) self-employed
Bolsa-Família	0.00435	-0.00532***	0.00967*	0.00689	-0.00517***	0.0121**
	(0.0055)	(0.0017)	(0.0054)	(0.0055)	(0.0017)	(0.0054)
Other transfers	-0.00719	-0.00433**	-0.00286	-0.00526	-0.00421**	-0.00105
	(0.0058)	(0.0018)	(0.0057)	(0.0058)	(0.0018)	(0.0057)
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
Observations	49,588	49,588	49,588	49,588	49,588	49,588
R-squared	0.06	0.024	0.056	0.06	0.024	0.057

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

	Wealth constraint alleviation (IV)			Insurance (OLS)		
	(1) entrepreneur2	(2) employer2	(3) self-employed2	(1) entrepreneur2	(2) employer2	(3) self-employed2
Bolsa-Família	0.00319**	-0.000158	0.00334***	0.00307**	-0.00017	0.00324***
	(0.0013)	(0.0003)	(0.0012)	(0.0013)	(0.0003)	(0.0012)
Other transfers	0.00400***	-0.000098	0.00410***	0.00391***	-0.000107	0.00402***
	(0.0013)	(0.0003)	(0.0013)	(0.0013)	(0.0003)	(0.0013)
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
Observations	49,588	49,588	49,588	49,588	49,588	49,588
R-squared	0.006	0.004	0.005	0.006	0.004	0.005

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Tables 23A and 23B – Conditionality effects: lower income quintiles and other transfers (urban sub-sample)

	Wealth constraint alleviation (IV)			Insurance (OLS)		
	(1) entrepreneur	(2) employer	(3) self-employed	(1) entrepreneur	(2) employer	(3) self-employed
Bolsa-Família	0.0153	-0.00465	0.02	0.0165	-0.00464	0.0212*
	(0.0124)	(0.0038)	(0.0122)	(0.0124)	(0.0038)	(0.0122)
Other transfers	-0.0123	-0.00616**	-0.00616	-0.0112	-0.00615**	-0.00506
	(0.0101)	(0.0031)	(0.0099)	(0.0101)	(0.0031)	(0.0099)
male eligible son	0.00563	0.000211	0.00542	0.0059	0.000214	0.00569
	(0.0065)	(0.0020)	(0.0064)	(0.0065)	(0.0020)	(0.0064)
BF * male eligible son	0.00436	-0.00245	0.00682	0.00464	-0.00245	0.00709
	(0.0119)	(0.0036)	(0.0117)	(0.0119)	(0.0036)	(0.0117)
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
Observations	13,990	13,990	13,990	13,990	13,990	13,990
R-squared	0.052	0.02	0.05	0.053	0.02	0.05

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

	Wealth constraint alleviation (IV)			Insurance (OLS)		
	(1) entrepreneur2	(2) employer2	(3) self-employed2	(1) entrepreneur2	(2) employer2	(3) self-employed2
Bolsa-Família	0.00329	0.00122*	0.00206	0.00327	0.00121*	0.00206
	(0.0028)	(0.0007)	(0.0028)	(0.0028)	(0.0007)	(0.0028)
Other transfers	0.00227	-0.0003	0.00257	0.00225	-0.000311	0.00256
	(0.0023)	(0.0006)	(0.0022)	(0.0023)	(0.0006)	(0.0022)
male eligible son	0.0016	0.00021	0.00139	0.0016	0.000207	0.00139
	(0.0015)	(0.0004)	(0.0014)	(0.0015)	(0.0004)	(0.0014)
BF * male eligible son	-0.000553	-0.000925	0.000371	-0.000558	-0.000927	0.00037
	(0.0027)	(0.0007)	(0.0026)	(0.0027)	(0.0007)	(0.0026)
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
Observations	13,990	13,990	13,990	13,990	13,990	13,990
R-squared	0.011	0.004	0.011	0.011	0.004	0.011

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 24 – Distribution of activities for self-employed individuals in secondary sources of income

Secondary activity (ordered from largest to lowest share)	Percent	Cumulative
Construction	7.76	7.76
Informal commerce	7.61	15.37
Health support services	6.37	21.74
Brewery	5.42	27.16
Butchery	3.95	31.11
Business Management	3.73	34.84
Cattle raising and commerce	3.22	38.06
Barber shop	3	41.06
Music	2.86	43.92
Arts	2.34	46.26
Vehicles	2.27	48.53
Car repair shop	1.98	50.51
Clothing	1.9	52.41
Security	1.76	54.17
Electronic repair	1.68	55.85
Commerce	1.54	57.39
Handcrafting	1.54	58.93
Sewing	1.46	60.39
Copying services	1.46	61.85
Dog training	1.46	63.31
Pineapple crops	1.39	64.7
Intermediation	1.39	66.09
Corn crops	1.24	67.33
LAN house	1.24	68.57
Gym	1.24	69.81
Internet commerce	1.17	70.98

Table 25 – Wealth-constraint alleviation: Commerce VS. Services

	(1) entrepreneur	(2) employer	(3) self-employed	(1) entrepreneur2	(2) employer2	(3) self-employed2
Bolsa-Família	0.0155***	-0.00132	0.0168***	0.00402***	-0.00041	0.00443***
	(0.0049)	(0.0015)	(0.0048)	(0.0013)	(0.0003)	(0.0012)
BF * commerce	0.00809	-0.0147***	0.0228**	-0.00345	0.000313	-0.00376
	(0.0114)	(0.0035)	(0.0113)	(0.0030)	(0.0008)	(0.0029)
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
Observations	68,740	68,740	68,740	68,740	68,740	68,740
R-squared	0.13	0.02	0.12	0.01	0.00	0.01

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 26 – Children: mechanism

	(1) worker
Bolsa-Família	0.00687***
	(0.0026)
employer parent	0.0573***
	(0.0128)
BF * employer parent	-0.0551*
	(0.0306)
Individual controls	YES
YEAR FIXED-EFFECTS	YES
UF FIXED-EFFECTS	YES
Observations	49,588
R-squared	0.006

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 27A and 27B – No income independent variables

	Rural			Urban		
	(1) entrepreneur	(2) employer	(3) self-employed	(1) entrepreneur	(2) employer	(3) self-employed
Bolsa-Família	0.0152*	-0.0031	0.0183**	0.0155***	-0.00442***	0.0199***
	(0.0081)	(0.0024)	(0.0082)	(0.0055)	(0.0017)	(0.0054)
Other transfers	-0.0049	0.00249	-0.00739	0.00128	-0.00364**	0.00492
	(0.0088)	(0.0026)	(0.0089)	(0.0058)	(0.0018)	(0.0057)
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
Observations	19,152	19,152	19,152	49,588	49,588	49,588
R-squared	0.175	0.024	0.16	0.055	0.023	0.052

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

	Rural			Urban		
	(1) entrepreneur2	(2) employer2	(3) self-employed2	(1) entrepreneur2	(2) employer2	(3) self-employed2
Bolsa-Família	0.00154	-0.00097	0.00251	0.00242*	-0.000133	0.00256**
	(0.0026)	(0.0006)	(0.0025)	(0.0013)	(0.0003)	(0.0012)
Other transfers	0.00616**	0.000982	0.00518*	0.00342***	-0.000079	0.00350***
	(0.0028)	(0.0007)	(0.0027)	(0.0013)	(0.0003)	(0.0013)
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
Observations	19,152	19,152	19,152	49,588	49,588	49,588
R-squared	0.014	0.007	0.012	0.005	0.004	0.004

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 28A and 28B – Nearest-neighbor matching

	Rural			Urban		
	(1) entrepreneur	(2) employer	(3) self-employed	(1) entrepreneur	(2) employer	(3) self-employed
Bolsa-Família	0.017	-0.003	0.02*	-0.009	-0.003	-0.007
	(0.0130)	(0.0040)	(0.0130)	0.0080	0.0020	0.0080
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
N-treatment	5,777	5,777	5,777	10,185	10,185	10,185
N-control	3,286	3,286	3,286	7,359	7,359	7,359

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

	Rural			Urban		
	(1) entrepreneur2	(2) employer2	(3) self-employed2	(1) entrepreneur2	(2) employer2	(3) self-employed2
Bolsa-Família	0.007*	-0.001	0.007*	0.006***	-0.001**	0.007***
	(0.0040)	(0.0010)	(0.0040)	(0.0020)	(0.0000)	0.0020
Individual controls	YES	YES	YES	YES	YES	YES
YEAR FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
UF FIXED-EFFECTS	YES	YES	YES	YES	YES	YES
N-treatment	5,777	5,777	5,777	10,185	10,185	10,185
N-control	3,286	3,286	3,286	7,359	7,359	7,359

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1