

Direct and Indirect Effects of Cash Transfers on Entrepreneurship*

Rafael P. Ribas

University of Illinois at Urbana-Champaign

ribas1@illinois.edu

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Abstract

This paper exploits a liquidity shock from a large-scale welfare program in Brazil to investigate the importance of credit constraints and informal financial assistance in explaining entrepreneurship. Previous research focuses exclusively on how liquidity shocks change recipients' behavior through direct effects on reducing financial constraints. However, the shock may also produce spillovers from recipients to others through private transfers and thereby indirectly affect decisions to be an entrepreneur. This paper presents a method for decomposing the liquidity shock into direct effects associated with relieving financial constraints, and indirect effects associated with spillovers to other individuals. Results suggest that the program, which assists 20 percent of Brazilian households, has increased the number of small entrepreneurs by 10 percent. However, this increase is almost entirely driven by the indirect effect, which is related to an increase in private transfers among poor households. Thus the creation of small businesses seems to be more responsive to the opportunity cost of mutual assistance between households than to financial constraints.

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1 Introduction

There has been a long debate over whether insufficient liquidity hinder individuals from starting their own business. In general, the literature suggests that financial constraints tend to inhibit those with insufficient funds at their disposal.¹ Under imperfect financial markets, individual savings could be the way that small entrepreneurs cope with startup costs and investment risks (Ghatak et al., 2001), which yet represent a large sacrifice for poor individuals (Buera, 2009).² The formal market, however, is not the only source of investment loans and insurance against business failure. Informal financial arrangements, such as interpersonal lending (Tsai, 2004; Fafchamps and Gubert, 2007; Schechter and Yuskavage, 2012), and mutual insurance (Murgai et al., 2002; Fafchamps and Lund, 2003), are often reported as a form of poor households sharing idiosyncratic risks.

This paper explores the importance of both financial constraints and inter-household transfers by estimating the impact of a liquidity shock on the decision to be an entrepreneur. Unlike other common interventions (e.g., Karlan and Zinman, 2010; Blattman et al., 2013), liquidity is not delivered uniquely to entrepreneurs. The studied intervention is a large-scale conditional cash transfer (CCT) program in Brazil, called *Bolsa Família*. This program offers a small but steady income to poor households that are committed to send their children to school and have regular health check-ups. However, it has absolutely no rule regarding business investment, adult labor supply, or repayment.

If potential entrepreneurs face credit and insurance constraints, the individual liquidity shock may change the occupational choice and investment decisions of program participants (Rosenzweig and Wolpin, 1993; Bianchi and Bobba, 2013). On the other hand, if they pursue risk-sharing strategies with other individuals, the cash transfer may flow into the hands of better entrepreneurs through informal exchanges. Accordingly, my purpose is to study not only the individual effect that this transfer has on participants, but also the indirect effect it has on the whole community. While the size of the direct effect reveals the role of financial constraints in explaining entrepreneurship, the size of the indirect effect reveals the role of other mechanisms that emerge from social interaction.

Very few studies have tried to assess the indirect effect that cash transfer programs have on the whole community.³ For instance, Angelucci and De Giorgi (2009) find that non-poor households

¹A non-exhaustive list of papers includes Evans and Jovanovic (1989), Evans and Leighton (1989), Holtz-Eakin et al. (1994), Lindh and Ohlsson (1996), Blanchflower and Oswald (1998), Blanchflower et al. (2001), Lindh and Ohlsson (1998), Fairlie (1999), Johansson (2000), Taylor (2001), Hurst and Lusardi (2004), Holtz-Eakin and Rosen (2005), Zissimopoulos and Karoly (2007), Nykvist (2008), and Fairlie and Krashinsky (2012).

²See also Banerjee and Newman (1993), Galor and Zeira (1993), Aghion and Bolton (1997), and Banerjee and Duflo (2005).

³See Bobonis and Finan (2009), Lalive and Cattaneo (2009), and Angelucci et al. (2009, 2010). See also Crépon

are also affected by PROGRESA/*Oportunidades* in rural villages in Mexico. They suggest that these households increase food consumption by receiving private transfers from program participants and reducing their precautionary savings.⁴ In another study, Bandiera et al. (2009) assess the effect of asset transfers in Bangladesh. They show that this program has indirect effects on time allocation in risk-sharing networks and on durable consumption in family networks.

In both studies, indirect effects are identified using non-participants, but their definition of direct effect is essentially the definition of ‘effect on the treated.’ As a matter of fact, “treated” households are also subject to spillovers. Even if all households are participating in the program, there may be externalities that either boost or attenuate the direct response to those transfers. This distinction is critical to understand targeted interventions, such as CCT and microfinance. On one hand, findings that are based on the comparison of treated and untreated villages tend to be interpreted as an exclusive consequence of participants’ responses. On the other hand, studies that compare individuals rather than villages might be biased by ignoring spillovers. According to Heckman et al. (1998), the conventional treatment effect model is based on a partial equilibrium framework. If the intervention has general equilibrium consequences, then the net effect also depends on who else is treated and the interaction between the treated and the untreated.

Other studies suggest that the liquidity shock promoted by cash transfers increases entrepreneurial activity at both the intensive margin, raising investments and profits (de Mel et al., 2008; Gertler et al., 2012), and extensive margin, encouraging participants to start their own business (Bianchi and Bobba, 2013; Bandiera et al., 2013; Blattman et al., 2013). In some of these studies, however, the randomization of ‘treatment’ was made at the village-level, which implies that the effect should be viewed as the sum of individual and local responses (Hudgens and Halloran, 2008). Namely, what is often interpreted as an individual shock, which lessens financial constraints, could actually be a locally aggregate shock, which also affects other households in the same village.

Another limitation in the current evidence is that most of randomized controlled trials (RCTs) are either restricted to rural areas, where job opportunities other than work in one’s own farm are scarce, or limited to small-scale pilots, which hold uncertainty about their maintenance. Therefore, little is known about the response of households to those programs once they reach urban centers as a permanent policy of social protection (Behrman et al., 2012). Moreover, the evidence on informal risk-sharing arrangements also comes mostly from rural villages (Fafchamps, 2011).

Unlike those interventions, *Bolsa Família* is a widespread, large-scale program that has been introduced not only in rural and isolated areas, but also in large cities in Brazil. In 2006, about

et al. (2013) on the indirect effect of labor market policies and Kremer and Miguel (2007) on the spillovers of health programs.

⁴Lehmann (2013) contests Angelucci and De Giorgi’s (2009) interpretation and suggests that the indirect effect on food consumption operates by raising non-food prices.

20% of Brazilian households were already covered by the program and 70% of them were living in urban settlements. Accordingly, I exploit this intervention to investigate small entrepreneurial activity and informal risk-sharing mechanisms in urban areas. As most of the literature, I define as entrepreneurs those who are either self-employed or small business owners (e.g., Blanchflower, 2000; Hurst and Lusardi, 2004). Furthermore, to consider self-employment as an investment opportunity rather than a way to conceal earnings, I distinguish entrepreneurs from those who are self-employed in the informal sector. Informal self-employment is considered another type of occupation in which workers are not covered by social security and whose earnings cannot be verified by the government. While small entrepreneurs earn on average 45% more than formal employees per hour, the informal self-employed earn 30% less.

Although the assignment of benefits in *Bolsa Família* is not random, I demonstrate that this is not a concern as long as the endogenous assignment of participants is not related to the overall amount of transfers received in the entire village. Namely, the fact that some poor households are more likely to participate in the program than others only affects the way the transfers are locally distributed. The total number of transfers per city or village is considered given because, from 2003 to 2007, the program was phased in based on a previously drawn poverty map. As a result, each municipality should have a limited number of transfers to be offered. Then instead of comparing participants and non-participants in the same municipality, the overall effect is estimated simply by comparing municipalities using a difference-in-difference model. To relax the assumption of exogenous program size, this variable is also instrumented by the poverty map. Then a verifiable condition for the Instrumental Variable (IV) approach is that the relationship between poverty and entrepreneurship does not change over time. Namely, there is no convergence in the entrepreneurship level across municipalities.

Once the overall effect is consistently estimated, the direct and indirect effects are calculated by a two-step procedure. First, based on the previous assumptions, I estimate the indirect effect of program coverage on non-participants and test whether this effect is equal to the indirect effect on participants. If this hypothesis is not rejected, that estimate can also be used to calculate the direct effect on participants by adjusting for the selection bias. In summary, this empirical strategy allows me to ignore individual selection issues based on verifiable assumptions and decompose the overall effect of the program on eligible individuals.

Previous studies on the effect of *Bolsa Família* usually compare households without dealing with the problems of selection on unobservables and contamination from spillovers.⁵ Despite the weak identification of causal effects, Lichand (2010) shows that participating households present

⁵Exceptions are Glewwe and Kassouf's (2012) and de Janvry et al.'s (2012) in estimating the effect of *Bolsa Família* on schooling.

a higher self-employment rate than other poor households, while de Brauw et al. (2012) suggest that the program has increased the participation in the informal market. Neither of them account for indirect effects that may bias the comparison between households. Also, the latter does not distinguish formal self-employment and small business from informal employees. Similar to my study, Foguel and Barros (2010) also identifies the causal parameter by comparing municipalities over time, but they find no significant overall effect on labor force participation.⁶

My findings suggest that the proportion of entrepreneurs among low-educated men has grown 10% because of the *Bolsa Família* program. At first glance, this finding supports the hypothesis that a small amount of secure cash can have a considerable impact on occupational choice. However, the direct and indirect components go in different directions. While the rise in entrepreneurial activity is entirely driven by spillovers, the direct response of participants reduces the overall effect by 40%. This drawback seems to be induced by households' risk of losing the benefit when their earned income increases. The results also show that the indirect effect on entrepreneurship is associated with an increase in private transfers between households. The role of program participants as money lenders corroborates the existence of informal risk-sharing arrangements. Thus rather than lessening individual credit and insurance constraints, the cash transfer seems to reduce the opportunity cost of informal financing by increasing the overall liquidity in poor communities.

In addition to these main results, I find that the indirect effect on entrepreneurship is followed by a decreasing participation in the informal sector. It suggests that the program has given the financial opportunity to underemployed workers to open their own business. The program, however, has had no significant effect on the occupational choice of non-poor individuals and on job creation, which could be related to increasing investment opportunities. Finally, the estimated effects do not seem to be driven by confounding factors, such as migration, credit expansion, and convergence in the entrepreneurship level across municipalities.

The remainder of the paper is organized as follows. Section 2 presents a simple theoretical framework to explain why cash transfers might have direct and indirect effects on entrepreneurship. Section 3 describes the main features of *Bolsa Família*, including its targeting mechanism based on a poverty map, and the panel data used in the empirical analysis. Section 4 details the identification strategy for the overall effect, as well as for the indirect and direct effects. Section 5 presents the main empirical findings, whereas section 6 presents tests for potential mechanisms, including confounding factors. Section 7 concludes the paper.

⁶Foguel and Barros's (2010) findings confirm what is also shown by Oliveira et al. (2007), Tavares (2008), Ferro et al. (2010), and Teixeira (2010).

2 Theoretical Framework

To understand why cash transfers could have an indirect effect on entrepreneurship, I present a simple model in which being formally self-employed has a fixed cost. For equally poor individuals, this fixed cost cannot be covered by formal credit due to their lack of collateral and high interest rates. The insufficient wealth can also make them unable to insure against business failure and then less willing to take risks (Bianchi and Bobba, 2013). These constraints drive us to conclude that an individual liquidity shock should increase their chances of being self-employed.

On the other hand, the formal market is not the only source of credit and insurance. Bilateral exchanges between neighbors, friends, and relatives might be a way in which small entrepreneurs cope with startup costs and business risks. Although empirical studies suggest that informal risk-sharing mechanisms do not fully compensate market failures (Townsend, 1994; Hayashi et al., 1996; Ravallion and Chaudhuri, 1997),⁷ efficiency is often achieved within social networks (Fafchamps, 2000; Fafchamps and Lund, 2003; De Weerd and Dercon, 2006). According to Bloch et al. (2008), social networks have the role of lessening information asymmetries and commitment constraints among their members. One may call this role social capital, which lowers the transaction costs of obtaining credit and insurance (Murgai et al., 2002; Fafchamps and Minten, 2002).

With low transaction costs, low-skilled individuals do not necessarily spend all the cash transfer, but they may also lend to someone with better entrepreneurial skills to increase their income in the future. At the same time, small entrepreneurs need not count only on their endowments to start their venture. In this model, the fraction of eligible individuals participating in risk-sharing networks is the key to explain the size of direct effects, which lessens financial constraints, and the size of an indirect effect, which reduces the costs of informal credit and insurance.

2.1 Setup

Consider a continuum of individuals who live for two periods and are heterogeneous in their entrepreneurial skills, q . All individuals maximize their expected utility, U , by choosing their consumption in period 1, c_1 , and consumption in period 2, c_2 :

$$U = u(c_1) + E[u(c_2)],$$

where $E[\cdot]$ is the expectation operator and $u(\cdot)$ exhibits decreasing absolute risk aversion, so that $u'' < 0$ and $u''' \geq 0$.⁸

In period 1, these individuals are endowed with an initial wealth, a , and have to choose their future occupation, which can be either working in a low-skilled job (L) or working in their own

⁷See Ogaki and Zhang (2001) for an evidence favoring the full risk-sharing hypothesis at the village level.

⁸A time discount factor could be included, but it is not relevant for this problem.

business (M). Choosing the low-skilled job has no cost and pays w in period 2. To start their business, however, they must acquire capital in the first period, which costs k . This capital, along with the time allocated to self-employment in period 2, yields either q with probability λ or δ otherwise. Namely, q represents the total revenue in case of business success, while δ is what they receive for reselling their capital (after depreciation) in case of failure. Another interpretation is that k represents the cost of formalization for the self-employed and δ is what they receive from social security (Straub, 2005). In summary, individual's income before transfers and savings is:

$$I_1 \equiv \begin{cases} a & \text{if } L \\ a - k & \text{if } M \end{cases} \quad \text{and} \quad I_2 \equiv \begin{cases} w & \text{if } L \\ q & \text{w.p. } \lambda \quad \text{if } M \\ \delta & \text{w.p. } 1 - \lambda \quad \text{if } M \end{cases}$$

Depending on their entrepreneurial skills, q , self-employment (M) increases the expected payoff of some individuals.⁹ Nonetheless, I should also consider that it is riskier than a salaried job (L), so that $\delta < w$ and $\lambda \in (0, 1)$.

In addition to the initial endowment and earnings, poor individuals are entitled to cash transfers in period 1, d_1 , and in period 2, d_2 , with $d_1 = d_2 = d$. However, receiving d_2 is conditional on eligible individuals staying poor based on an eligibility rule. With this rule, only those with verifiable earnings, I_2 , less than or equal to w remain eligible for the benefit. For those whose $q > w$, λ becomes not only the probability of business success, but also the probability of losing the transfer if self-employed. Let ζ indicate whether the eligibility rule is applied ($\zeta = 1$) or not ($\zeta = 0$).

2.2 Analysis

Let $D(q)$ be the utility trade-off between self-employment and wage employment:

$$D(q) \equiv U(M; q) - U(L).$$

If the value of initial endowments is large enough to cover the cost of acquiring capital, $a + d_1 > k$, there exists a level of entrepreneurial skills, \hat{q} , such that the individual is indifferent between wage employment and self-employment, $D(\hat{q}) = 0$. All individuals with $q < \hat{q}$ prefer to be employed in a low-skilled job, whereas all individuals with $q \geq \hat{q}$ prefer to work in their own business.

Let F be the cumulative distribution function of q and y be the entrepreneurship rate, so that $y = 1 - F(\hat{q})$. An upward shift in $D(\hat{q})$ makes marginally less skilled individuals willing to start their business. That is, the effect of cash transfers on the entrepreneurship rate, y , is proportional

⁹Other types of heterogeneity could be assumed, such as in wealth, risk aversion, and probability of success. However, with heterogeneous payoffs and risk-averse individuals, wealth heterogeneity becomes irrelevant. Heterogeneity in either risk aversion or probability of success would essentially yield the same results, but with a more complex insurance market.

to their effect on the trade-off, $D(\hat{q})$.¹⁰ As discussed below, this effect has distinct interpretations in two cases: if only positive, non-contingent savings are allowed; and if individuals can borrow from and trade insurance with other members of their network. A formal analysis is provided in the appendix.

2.2.1 Individual Liquidity Shock with Financial Constraints

Assume that individuals can neither borrow, so that only positive savings are allowed in period 1 (credit constraint), nor trade insurance, so that they cannot transfer earnings across states (insurance constraint). Since there is no market for bonds and insurance, the cash transfer affects the trade-off only in a direct way. That is, the results derive from an individual maximization problem with no general equilibrium effect.

Since individuals cannot optimally allocate transfers from period 2 to period 1, an increase in the initial cash transfer, d_1 , provides the liquidity that some individuals need to pay the cost of capital, k . This is what is defined as the credit effect (CE):

$$CE \equiv \frac{\partial y}{\partial d_1} \propto u' [a + d_1 - k - s_M^*(\hat{q})] - u' [a + d_1 - s_L^*] > 0, \quad (2.1)$$

where $s_M^* \geq 0$ and $s_L^* \geq 0$ are the optimal levels of savings.

As demonstrated by Bianchi and Bobba (2013), if individuals cannot buy insurance, the cash transfer also increases their willingness to bear the risk of self-employment. If the credit constraint does not bind ($s_M^* > 0$) and the eligibility rule is not applied ($\zeta = 0$), then the future transfer, d_2 , provides an insurance against business failure, making the entrepreneurial venture less risky. Accordingly, one of the effects of future transfers is defined as the insurance effect (IE):

$$IE \equiv \left. \frac{\partial y}{\partial d_2} \right|_{\zeta=0} \propto \lambda u' [\hat{q} + d_2 + s_M^*(\hat{q})] + (1 - \lambda) u' [\delta + d_2 + s_M^*(\hat{q})] - u' [w + d_2 + s_L^*] \quad (2.2)$$

$$\geq CE \quad \text{if } s_M^*(\hat{q}) > 0.$$

The insurance effect can be negative only if the credit constraint binds ($s_M^* = 0$). In this case, however, the credit effect is large enough to make the net effect, $CE + IE$, positive.

If the eligibility rule is applied ($\zeta = 1$), then an increase in future transfers, d_2 , will have an ambiguous effect. On one hand, it still provides insurance against business failure (IE). On the

¹⁰An interior solution for \hat{q} is a necessary condition for a marginal change in cash transfers, d , to affect the proportion of self-employed, y . However, despite the interior solution exists, the relationship between d and y is continuous if q is a continuous variable and individuals are risk-averse, $u'' < 0$.

other hand, it increases the return of being wage employed, L , because choosing self-employment reduces the chances of receiving d_2 . This negative response is defined as the eligibility effect (EE):

$$EE \equiv \left. \frac{\partial y}{\partial d_2} \right|_{\zeta=1} - \left. \frac{\partial y}{\partial d_2} \right|_{\zeta=0} \\ \propto -\lambda u' [\hat{q} + d_2 + s_M^* (\hat{q})] < 0 \quad (2.3)$$

Depending on how high is the probability of business success, λ , the eligibility effect can prevail over the insurance and credit effects — i.e., $CE + IE + EE < 0$. Thus individuals at the margin of indifference might prefer keeping receiving a transfer than starting a business that does not pay much more.

Proposition 2.1 (Effect of Cash Transfer with Credit and Insurance Constraints). *Assume that individuals can neither borrow nor trade insurance. Under no eligibility rule, cash transfers have a positive net effect on the entrepreneurship rate. However, if future transfers are subject to an eligibility rule, then the net effect is ambiguous and decreasing in the probability of business success, λ .*

2.2.2 Aggregate Liquidity Shock with Risk-Sharing

Consider a risk-sharing network in which transaction costs are irrelevant, so that its members can efficiently trade bonds and insurance in the first period. The repayment of bonds is assumed to be contingent on business success in period 2.¹¹ If the investment made by entrepreneurs is not successful, then they receive the insurance that they bought rather than paying their loans. Another way of setting this model is assuming that credit and insurance are provided through gift exchanges without commitment (Kocherlakota, 1996; Foster and Rosenzweig, 2001). If the business is successful and the entrepreneur becomes richer, then a more valued gift is expected in return. Otherwise, non-entrepreneurs are expected to help entrepreneurs with their loss. The ratio between what is given in period 1 and what is received in period 2 defines the implicit prices of bonds and insurance.

Given the equilibrium prices in this network, all individuals are now able to optimally transfer utility across periods and states — i.e., they are neither credit constrained nor insurance constrained. Therefore, the *direct* effect of cash transfers on the occupational choice depends only on the eligibility rule. If eligibility rule is not applied, the liquidity shock just changes the individual demand for credit and insurance, but it does not affect their occupational choice, $CE = IE = 0$. Otherwise, an increase in future transfers, d_2 , reduces the relative gain of being self-employed with respect to wage employment (EE).

¹¹Contingent bonds can also be interpreted as an insurance that entrepreneurs sell to non-entrepreneurs. Evidence of contingent loan repayment is presented by Udry (1994) and Fafchamps and Gubert (2007).

On the other hand, the cash transferred in both periods will also lower the cost of risk-sharing by changing the equilibrium prices of bonds and insurance. With more cash in hands, non-entrepreneurs will be more willing to share the risk with entrepreneurs, whereas entrepreneurs will reduce their need for inter-household transfers. As a result, the decreasing cost of risk-sharing gives the opportunity for slightly less-skilled individuals to invest in a more profitable occupation. Therefore, in an efficient risk-sharing arrangement, an aggregate liquidity shock will be used to cover the cost of capital, k , and the possible losses, $w - \delta$, of a larger fraction of entrepreneurs.

Let y^* be the Pareto efficient entrepreneurship rate among individuals in the same network. The general equilibrium effect (GE) of cash transfers is given by the overall effect on y^* minus the direct response, which only comprises the eligibility effect, EE :

$$GE \equiv \frac{dy^*}{dd_1} + \frac{dy^*}{dd_2} - EE > 0. \quad (2.4)$$

Proposition 2.2 (Effect of Cash Transfer in a Risk-Sharing Network). *Assume that individuals belong to a risk-sharing network. The direct effect of cash transfers on the decision of being an entrepreneur is negative due to the eligibility rule. However, the aggregate shock of cash transfers has also a positive indirect effect by lowering the cost of risk-sharing.*

2.2.3 Direct and Indirect Effects and the Size of Risk-Sharing Networks

Finally, consider a population in which some individuals participate in risk-sharing networks and others do not. In particular, let N be the number of risk-sharing networks in this population and α_j be their size with $j = 1, \dots, N$. Note that $\left(1 - \sum_{j=1}^N \alpha_j\right)$ is the fraction of individuals who do not belong to a network, which are labeled as group 0. Also, for any $j = 1, \dots, N$, $\hat{q}_j \leq \hat{q}_0$ — i.e., despite the network size, individuals connected to one has at least as much chance to be an entrepreneur as those who are not. The reason is they can always lean on their own savings if the price of insurance in their network is too high.

If individuals are randomly distributed among these networks, then the relationship between entrepreneurship rate and cash transfers is the following:¹²

$$\Delta y \approx (\beta_1 + \beta_2) \Delta d, \quad (2.5)$$

where

$$\beta_1 \equiv \left(1 - \sum_{j=1}^N \alpha_j\right) [CE(\hat{q}_0) + IE(\hat{q}_0) + EE(\hat{q}_0)] + \sum_{j=1}^N \alpha_j EE(\hat{q}_j)$$

¹²The assumption of exogenous networks is not necessary. Even if individuals are assorted based on q , for any $j = 1, \dots, N$, $\hat{q}_j \leq \hat{q}_0$ still holds.

is the direct effect and

$$\beta_2 \equiv \sum_{j=1}^N \alpha_j GE(\hat{q}_j)$$

is the indirect effect.

By definition, the direct effect of cash transfers on entrepreneurial decision, β_1 , is a function of the credit, insurance, and eligibility effects. Despite how many individuals receive the transfer, those are the components responsive to the individual liquidity shock. The credit (*CE*) and insurance (*IE*) effects tend to be positive and increasing in the proportion of individuals facing financial constraints, $\left(1 - \sum_{j=1}^N \alpha_j\right)$. The eligibility effect (*EE*) is negative but decreasing in entrepreneurial skills. That is, the lower the cut-off skill to be an entrepreneur, \hat{q} , the higher the reduction on entrepreneurship. Since $\hat{q}_0 \leq \hat{q}_j$ and then $EE(\hat{q}_0) \geq EE(\hat{q}_j)$ for any $j = 1, \dots, N$, the eligibility effect is also increasing in the proportion of individuals with financial constraints.

The indirect effect, β_2 , is a function of the general equilibrium component (*GE*), which is responsive to the aggregate liquidity shock in each network. Thus the larger the proportion of individuals involved in risk-sharing networks, $\left(\sum_{j=1}^N \alpha_j\right)$, the larger the indirect effect. In other words, the size of the indirect effect may reveal the importance of informal financial arrangements, to the detriment of financial constraints, in explaining small entrepreneurial activity. Nonetheless, it is worth to remind that the existence of these arrangements is just one of many reasons for cash transfers to have an indirect effect on entrepreneurship.

3 Program and Data Description

In this section, I outline the main characteristics of the *Bolsa Família* program, as well as the panel sample used in my analysis. Most important, I describe how the growth of this program is closely related to the previous level of poverty, making it less likely to be driven by economic opportunities and pork barrel politics at the local level. Furthermore, I explain how the National Household Survey (PNAD) may be used in a panel setting even though it is a rotating cross-sectional survey.

3.1 The *Bolsa Família* Program

In Brazil, the first CCT programs managed by the Federal Government were created in 2001. The first, called *Bolsa Escola*, was conditional on poor children between 6 and 15 years being enrolled and regularly attending primary school. Another program, called *Bolsa Alimentação*, was intended to improve health care and nutrition of children up to 6 years and pregnant women. In 2003, the government created the *Bolsa Família* program, merging all these previous programs in one with the standardization of eligibility criteria, benefit values, information systems, and executing agency. The program also brought in a gradual expansion of CCTs in Brazil, from 5.1

million families in December 2002 to 11.1 million families in October 2006. The target number of 11 million was calculated based on the estimated number of poor families according to the 2001 National Household Survey (PNAD, *Pesquisa Nacional por Amostra de Domicílios*).

In 2006, extremely poor families, whose per capita monthly income was below US\$38, with no child and poor families, whose per capita monthly income was below US\$76, with children up to 15 years old or pregnant women were eligible for the program. The monthly benefit was composed of two parts: a) US\$38 for extremely poor families regardless of the number of children, and b) US\$11 per children, up to three, for poor families. Thus an extremely poor family should receive a benefit between US\$38 and US\$72, whereas a moderately poor family should receive between US\$11 and US\$34.¹³ Like *Bolsa Escola* and *Bolsa Alimentação*, these benefit require a household commitment in terms of child education and health care. However, if the family is registered as extremely poor with no child, the US\$38 transferred is actually considered unconditional.

Families that receive the benefit can be dropped from the program not only in case of not complying with the conditionalities, but also when their per capita income becomes greater than the eligibility cut-off point. During the period covered by this study, whenever it was found that the household per capita income had been above the eligibility threshold, the family would be excluded from the payroll. Moreover, families are required to update their records in the single registry of social policies (*Cadastro Único*) at least once every two years. As for monitoring of the income information, the Federal Government regularly matches beneficiaries' records with other governmental databases, such as the database on formal sector workers from the Ministry of Labor and Employment and the database of pensions and other social assistance programs.

For instance, the government found that 622,476 participant households had earnings above the eligibility cutoff from October 2008 and February 2009. From this total, 451,021 households had their benefit canceled. From cross-checking its databases, the government had canceled the benefit of more than one million households from 2004 to 2008, which represents about 40% of the total number of withdraws.

3.2 Program's Targeting

In order to identify poor families around the country, local governments (municipalities) are free to decide about the priority areas and how the registering process takes place. However, they do receive some guidelines, under the form of quotas on the number of benefits. This cap of benefits is intended to prevent local governments from spending the federal transfers irresponsibly and

¹³In 2004, the extreme poverty line for the program was US\$33, the poverty line was US\$66, and the value of the benefit per child was US\$10.

using them for electoral purposes. As a result, each municipality has a maximum number of benefits that can be distributed, which is given by the estimated number of poor households.

Although the program size cannot grow for electoral purposes, de Janvry et al. (2012) show that its local performance has raised the chances of mayors being re-elected. Namely, politicians cannot take advantage by distributing more benefits, but they can be rewarded by the way the total number of benefits is distributed.

The municipal quotas were initially defined by a poverty map, made by the National Statistics Office (*Instituto Brasileiro de Geografia and Estatística*, IBGE). This map was made using both the 2001 Household Survey and the 2000 Demographic Census and was used for the quotas until 2006, when it started being annually updated. In other words, given the target of 11 million families in the whole country, the 2001 poverty map guided how the program should have gradually grown across municipalities from 2003 to 2006.

Although the local government has the responsibility of registering poor families in the Single Registry (*Cadastro Único*), this registration does not mean automatic selection into the program. Registered families still have to prove they receive per capita income under the eligibility cut-off point and the total number of benefits cannot surpass the local quota. Under this cap, the order of eligible households is managed by the National Government and is based on per capita income and number of children.

Figure 1 confirms that the number of benefits per municipality had strongly depended on the previous number of poor households, estimated using data from 2000 and 2001. In the top panel, we observe the relationship between the proportion of poor households (poverty headcount) in 2000, calculated using the Demographic Census, and the proportion of households covered by the program (program coverage) in 2004 and 2006, according to the official records. The initial poverty headcount explains 77% of municipal coverage in 2004, when the program was still expanding and had not reached the cap in most municipalities. In 2006, when the program reached its target, the relationship became even stronger and closer to the 45-degree line.

FIGURE 1 ABOUT HERE

The bottom of Figure 1 shows the relationship between poverty headcount in 2001 and program coverage in 2004 and 2006, calculated with the sample used in this paper (see data description below). Even though both variables are subject to a larger statistical error, the pattern is similar to that observed in the top panel. Despite this pattern, one may argue that any cash transfer program is naturally more concentrated where poverty is higher. However, the last graph on the bottom right shows that the program size in 2006 is not as strongly correlated to poverty in 2004

as it is to poverty in 2001. Moreover, a Shapley decomposition confirms that controlling for the current level of poverty, the 2001 poverty headcount accounts for at least 50% of the R^2 in 2004 and 2006.¹⁴ Therefore, it is reasonable to assume that the growth of *Bolsa Família* program in this period strongly depended on the previously estimated poverty headcount for each municipality.

A particular characteristic of *Bolsa Família* is its concentration in urban areas. Urban poverty in Brazil has for a long time been considered as critical as rural poverty in the design of social policies (Rocha, 2003). Although the poverty rate is higher in rural areas (see Table 1), most of the poor live in urban settlements. As a result, about 70% of transfers go to urban households. Since the labor market and job opportunities differ between urban and rural areas, impacts of *Bolsa Família* on labor supply and occupational choice are expected to be distinct from those found for other programs concentrated in rural villages.¹⁵

TABLE 1 ABOUT HERE

3.3 Data

3.3.1 Panel Sample and Variables

All the data come from the National Household Survey (*Pesquisa Nacional por Amostra de Domicílios*, PNAD). This survey, which collects a broad set of information on demographic and socio-economic characteristics of households, included a special questionnaire on cash transfer programs in 2004 and 2006. This questionnaire asked whether any member of the household was beneficiary of each cash transfer program that was in place at the time of the survey. Henceforth, I consider as *Bolsa Família* all previous programs that had a similar goal and design (e.g., *Bolsa Alimentação*, *Cartão Alimentação*, *Bolsa Escola*, and PETI).

In addition to these two survey years, I use the 2001 PNAD as a baseline. In 2001, the *Bolsa Família* program had not taken place yet and the other cash transfer programs did not have a significant size. However, I have to control for the small coverage of other programs that might contaminate the baseline outcomes. Accordingly, I identify those households receiving cash transfer from other social programs using the typical-value method developed by Foguel and Barros (2010). This method basically matches parts of household income, under the entry of ‘other incomes,’ with typical values transferred by each program.

¹⁴See Israeli (2007) and Huettnner and Sunder (2012) for details on the Shapley decomposition method.

¹⁵Most of the experimental evidence finds little or no short-run effect of CCTs on job creation and labor supply. See Alzúa et al. (2010) for a comparative evaluation of PRAF II in Honduras, *Oportunidades* in Mexico, and RPS in Nicaragua; Parker and Skoufias (2000), Skoufias and Maro (2008), and Parker et al. (2008) for evaluations of *Oportunidades*; IFS et al. (2006) for an evaluation of *Familias en Acción* in Colombia; and Galasso (2006) for an evaluation of *Chile Solidario*.

The PNAD is a cross-sectional survey, so it does not interview the same households every year. Thus I cannot construct a panel of households or even individuals. However, for each decade — i.e., the period between two Demographic Censuses —, the replacement of households on its sample occurs within the same census tracts.¹⁶ Namely, once a census tract was selected for the sample in 2001, it kept being surveyed until 2009. Although they are not geo-referenced because the key variable is encrypted, we are able to identify the same census tracts and municipalities through the years. This sampling scheme permits the estimation of a fixed-effect model, described later in this paper.

Given the common characteristics of entrepreneurs, the sample is restricted to men who are between 25 and 45 years old and reside in urban areas. Indeed, empirical studies show that men are more likely than women to pursue entrepreneurial activity (Blanchflower, 2000; Karlan and Zinman, 2010). They also show that the probability of being an entrepreneur is increasing in age, but the probability of starting a new business is decreasing after 30 years old (Ardagna and Lusardi, 2010). Moreover, the desire for being self-employed is decreasing in age (Blanchflower et al., 2001).

I also exclude public servants, people with higher education, and employers with more than five employees from the sample. Even though 6% of public servants were participating in the program in 2006, they are less likely to change occupation due to their job stability. The last two groups were excluded because only 1% of them were receiving the benefit in 2006, so they are not considered eligible for the transfer. In addition, business with more than five employees could already be well-established, so they are less sensitive at the extensive margin.¹⁷ Because of the exclusion of observation from the original sample, the survey weights are calibrated so that the three years have the same importance in the analysis.

Table 2 presents the average number of observations per municipality in the final sample. About 130 households and 50 prime-age men are interviewed by municipality on average every year. For some small municipalities, the number of observations may not be large enough to yield accurate estimates. However, the smaller the town, the more homogeneous is the population. Under such a circumstance, the *program coverage* at municipal level, which is the main intervention investigated in this paper, is given by the proportion of prime-age men living in a household that receives the conditional benefit.

TABLE 2 ABOUT HERE

¹⁶A census tract is a neighborhood that has between 250 and 350 households in urban areas, 150 and 250 households in suburban areas, 51 and 350 households in informal settlement areas, 51 and 250 households in rural areas, and at least 20 households in indigenous areas (IBGE, 2003).

¹⁷The exclusion of these employers reduces the sample by 1%, with no implication for the results.

According to Blanchflower (2000) and Blanchflower et al. (2001), self-employment is the primary form of entrepreneurship. For this reason, I classify as entrepreneurs those who either have this type of occupation or are small business owners. However, to distinguish entrepreneurial activity and informality, the definition also requires that they either perform a high-skilled job or contribute to social security. Namely, entrepreneurs are subject to taxes and less vulnerable than informal workers in general. On the other hand, the government cannot track earnings of workers in the informal sector, whereas entrepreneurs have their earnings partially revealed on the government records.

For the sample of prime-age men, I construct the following variables based on their main occupation: (1) *entrepreneur*, equal to one if self-employed in professional or technical occupation (e.g., electrical technician, computer programmers, and visual artists), self-employed in any other occupation and also contributing to social security, employer with more than two employees, or small employer contributing to social security, and zero otherwise; (2) *formal employee*, equal to one if employed with documentation or contributing to social security; (3) *informal employee*, equal to one if employed without documentation and not contributing to social security; (4) *informal self-employed*, equal to one if self-employed in low-skilled occupation (not requiring job-specific training) and not contributing to social security; (5) *jobless*, equal to one if not having a remunerated occupation, including unemployed and inactive adults. The set of entrepreneurs were also subdivided into *service*, *sales*, and *manufacturing*, based on the type of business.

3.3.2 Descriptive Statistics

Table 3 shows the descriptive statistics of outcomes and control variables. From 2001 to 2006, the entrepreneurship rate increased 0.3 percentage points (p.p.). Namely, the percentage of prime-age men who were formally self-employed or small business owner went from 6.9% to 7.2%. Moreover, the type of business changed mostly in 2004, with more entrepreneurs in sales and less in services.

TABLE 3 ABOUT HERE

Despite the overall level has slightly changed, several factors might have affected the decision of low-educated workers to be an entrepreneur. For instance, with better opportunities in the formal sector, some entrepreneurs might have switched to the position of documented employee, while informally employed workers might have perceived opportunities to open their own businesses. Indeed, the participation in the formal sector increased about 5 p.p. in this period, whereas the proportion of informal workers (employed or self-employed) decreased 4 p.p. The remaining difference of 1 p.p. comes from the group of jobless, which decreased from 14% to 13%.

With the creation of *Bolsa Família* in 2003, the percentage of individuals receiving cash transfers (program coverage) went from 4.7% in 2001 to 19.4% in 2006. A simple difference-in-difference

analysis suggests that the rising entrepreneurship rate is associated with the increasing coverage. Since the program is target to the poor, Figure 2 shows that the relationship between program size and entrepreneurship is indeed negative. However, it also shows that the larger the program coverage, the higher the entrepreneurship growth between 2001 and 2006.

FIGURE 2 ABOUT HERE

Besides the increase of the formal sector and gradual expansion of *Bolsa Família*, other socio-economic improvements are observed in Table 3. As regards education, the proportion of adult men with a high school diploma increased 10 p.p. in five years. The same increase is seen in high school enrollment rate. In terms of health care, child mortality decreased from 12.7 deaths per 1,000 children up to 5 years old to 9.8 deaths. Finally, the proportion of houses linked to the sewer system increased 3 p.p. Given all the socio-economic improvements that happened in Brazil, it is critical to control for these variables to account for demographic changes and other social policies.

An important mechanism in which the program may affect entrepreneurship is through private transfers. This type of income is calculated as the sum of donations and other incomes, excluding retirement benefits, other pensions, rental earnings, and social benefits. The percentage of households receiving private transfers should increase along with liquidity in poor communities if they adopt informal risk-sharing strategies. In Table 3, we observe that this rate went from 4.3% in 2001 to 7.7% in 2006.

4 Empirical Strategy

The empirical strategy consists of a difference-in-difference model estimated using a three-period dataset. As discussed above, the program coverage has been strongly driven by observables. According to Proposition 4.1, presented below, this condition is sufficient for the identification of the overall effect of the program using a model with municipality-level fixed effects.

Furthermore, the identification assumption is weak enough to ignore the fact that some households are more likely to go after the benefit than others. The reason is that self-selection at the local level is not a concern when the comparison of treated and control observations occurs between municipalities, and not within municipalities. I call this assumption ‘Partial Aggregate Independence’ (PAI) because the aggregate growth of benefits is assumed to be exogenous even if the individual assignment is endogenous.¹⁸

¹⁸This assumption is the same adopted by Hsieh and Urquiola (2006) to identify the effect of choosing private schools over public schools on students’ achievement.

In order to verify the reliability of the PAI assumption, I also present an Instrumental Variable (IV) strategy. The strategy uses the measure of local poverty in 2001, controlling for the current level of poverty and fixed effects, to predict variations in the program intervention. This instrument eliminates the part of variance in the program assignment that could be related to unobservable changes in the labor market. Moreover, the exclusion restriction is very likely to hold as long as the relationship between poverty and entrepreneurship does not change over time, which is a testable condition.

This section also presents a definition for direct and indirect effects of cash transfer programs. The direct effect is understood as the individual response of households to the program benefit, while the indirect effect results from the interaction of individual responses. In contrast to Angelucci and De Giorgi's (2009) definition, the indirect effect is seen not only as the impact that the program has on ineligible individuals, but also as the impact that it has on the whole community, including individuals receiving the benefit.

Finally, I introduce a formal test to verify whether the indirect effect is different for individuals who receive and do not receive the benefit (Proposition 4.2). Once the homogeneity in the indirect effect is confirmed, the estimated overall effect can be decomposed into the direct and indirect parts, adjusting for the self-selection bias. All proofs are provided in the appendix.

4.1 Fixed-Effect Model

Let y_{ivt} be the decision of individual i living in municipality (city or village) v at time t of being an entrepreneur. Based on equation (2.5), this decision is determined by a linear structural model:

$$y_{ivt} = \beta_0 + \beta_1 d_{ivt} + \beta_2 \bar{d}_{vt} + \mu_v + \mu_t + u_{ivt}, \quad (4.1)$$

where μ_v is the municipality fixed effect, μ_t is the period-specific effect, u_{ivt} is the zero-mean random term, d_{ivt} is the individual treatment indicator, and \bar{d}_{vt} is the treatment coverage in municipality v . Namely, \bar{d}_{vt} is the mean of d_{ivt} conditional on living in v at time t .

Definition (Direct, Indirect, and Overall Effects). *Following equation (4.1):*

- Coefficient β_1 is the direct effect on participants;
- Coefficient β_2 is the indirect effect on participants;
- The sum of these coefficients, $\tau = (\beta_1 + \beta_2)$, is the overall effect on participants.

There are two ways of interpreting these coefficients: as an individual intervention and as a local intervention. Individually, if someone receives the benefit, then the probability of being an

entrepreneur increases β_1 percentages points (p.p.) due to the direct effect and β_2 p.p. due to the indirect effect. Locally, if the program size increases 1 p.p., then the entrepreneurship rate will increase $(\beta_1 \cdot 0.01)$ p.p. due to the direct effect on participants and $(\beta_2 \cdot 0.01)$ p.p. due to the indirect effect on every individual.

Most of evaluation studies that compare treated households in covered villages and untreated households in uncovered villages (e.g., evaluations of PROGRESA/*Oportunidades* in Mexico) actually estimate the overall effect of the intervention, τ .¹⁹ On the other hand, studies that compare individuals in the same cities or villages (e.g., Gasparini et al., 2009; Blattman et al., 2013) are only estimating the direct effect, β_1 . Finally, it is important to stress that eligible individuals are as subject to indirect effects as ineligible individuals in this model — i.e., the indirect effect is not only on those who do not participate in the program.

As explained above, the coverage of *Bolsa Família* at the municipality level has strongly depended on the previously estimated poverty headcount. Therefore, it is reasonable to assume that the program coverage, \bar{d}_{vt} , is independent of the error term, u_{ivt} , once controlling for municipality fixed effects. Accordingly, the consistency of difference-in-difference estimates depends on the following identification assumption.

Assumption 4.1 (Partial Aggregate Independence, PAI). *In equation (4.1),*

$$E[u_{ivt}\bar{d}_{vt}|d_{ivt}] = 0.$$

Given the choice made by individual i of participating in the program, d_{ivt} , the proportion of individuals who are allowed to make this choice is orthogonal to the individual decision of being an entrepreneur. This assumption does not imply that d_{ivt} is exogenous. If the distribution of benefits within municipalities is systematically correlated to unobservables, $E[Cov(u_{ivt}, d_{ivt}|v, t)] \neq 0$, then $E[u_{ivt}d_{ivt}] \neq 0$. Although the program size is defined by the municipality quotas, the assignment of benefits at the local level can still be self-selective. That is, given a restricted number of transfers, some households are more likely to go after the benefit than others. In this case, the estimator for both coefficients, β_1 and β_2 , will be asymptotically biased according to the following lemma.

Lemma 4.1 (Selection Bias). *If the PAI assumption holds, then the least squares estimator for*

¹⁹For instance, Bianchi and Bobba (2013) attribute the effect of future transfers made by PROGRESA in Mexico to the individual willingness of participants to bear risk. However, the difference between future transfers in ‘treated’ villages and no transfer in ‘control’ villages could also be driven by changes in the aggregate demand or in the amount of cash locally available.

β_1 and β_2 have the following asymptotic property:

$$\begin{aligned}\hat{\beta}_1 &\xrightarrow{p} \beta_1 + \frac{E[u_{ivt}d_{ivt}]}{Var(d_{ivt}) - Var(\bar{d}_{vt})}, \\ \hat{\beta}_2 &\xrightarrow{p} \beta_2 - \frac{E[u_{ivt}d_{ivt}]}{Var(d_{ivt}) - Var(\bar{d}_{vt})}.\end{aligned}$$

Note that the asymptotic biases cancel each other, so the estimator for $\tau = (\beta_1 + \beta_2)$ will be consistent if \bar{d}_{vt} is exogenous. Therefore, self-selection may be an issue if one compares individuals in the same city or village, but it is not if one compares cities and villages as a whole. Finally, the following proposition states the consistency of the identification strategy.

Proposition 4.1 (Consistent Estimator for the Overall Effect). *Consider the following equation:*

$$y_{ivt} = \beta_0 + \tau \bar{d}_{vt} + \mu_v + \mu_t + u_{ivt} \quad (4.2)$$

If equation (4.1) is the true model, then the least squares (LS) estimator for τ in equation (4.2) is the sum of the LS estimators for β_1 and β_2 in equation (4.1):

$$\hat{\tau} = \hat{\beta}_1 + \hat{\beta}_2.$$

Moreover, if the PAI Assumption holds, then the LS estimator for τ in equation (4.2) is consistent:

$$\hat{\tau} \xrightarrow{p} \beta_1 + \beta_2.$$

Proposition 4.1 implies that the overall effect of the program, τ , can be consistently estimated if we just omit d_{ivt} in equation (4.1). Accordingly, I estimate equation (4.2) using a three-period data, with the standard errors clustered by municipality. For the sake of robustness, I also include individual and local control variables in the main model and estimate another model with census-tract fixed effects. If the self-selection bias is proportional to the program size, \bar{d}_{vt} , violating the PAI assumption, then estimates conditional on census-tract fixed effects should be different (less biased) than those conditional on municipality fixed effects.

4.2 Instrumental Variable Method

One may argue that the PAI assumption is not reasonable because part of the variance of municipality coverage might be explained by unobservables related to the labor market. To consider only changes predicted by the measure of poverty in 2001, rather than changes caused by idiosyncratic behavior, I also estimate an Instrumental Variable (IV) model. In this model, the local coverage need not be strictly driven by observables, but it can be just partially affected by the program's initial design.

Assumption 4.2 (Instrumental Variable Assumption). *Given the current poverty level, p_{vt} , and unobserved fixed variables, the designed coverage is orthogonal to u_{ivt} .*

The designed coverage is captured by the interaction between the poverty headcount in 2001, p_{v0} , and period dummies. Then the equation for the program coverage, \bar{d}_{vt} , is:

$$\bar{d}_{vt} = \gamma_0 + \gamma_1 p_{v0} \cdot I(t = 2004) + \gamma_2 p_{v0} \cdot I(t = 2006) + \gamma_3 p_{vt} + \theta_v + \theta_t + e_{ivt}. \quad (4.3)$$

The IV assumption implies that the residual relationship between occupational choices and the measure of poverty in 2001 does not change over time, unless by means of the own program coverage. Note that the constant relationship between occupational choices and the initial poverty headcount is controlled by the fixed effect, θ_v . Moreover, the current level of poverty, p_{vt} , is also added as a control variable. Section 6.4 presents a test to verify whether that relationship changes over time.

Since the instrument is defined at the municipality level, the predicted change in the intervention also happens at the municipality level. Therefore, if the program coverage, \bar{d}_{vt} , is replaced by the individual treatment, d_{ivt} , in equations (4.2) and (4.3), the estimated IV coefficient will be the same. See Proposition C.1 in the Appendix.

This result reinforces the concept of *overall effect* defined above. Once the instrument is defined at the cluster level (e.g., randomization of treated villages), the comparison between treated and untreated individuals also happens in the cluster level — i.e., across villages rather than between individuals. On one hand, this IV approach avoids the problem of partial identification of the overall effect if using individual treatment. On the other hand, its interpretation cannot ignore the contribution of indirect effects for the estimated impact.

4.3 Separating Direct and Indirect Effects

Unfortunately, estimating equation (4.2) does not reveal whether the effect of program size comes from either a direct effect on individuals receiving the transfer or an indirect effect that also affects individuals out of the program. Nonetheless, the PAI assumption is also sufficient for the indirect effect, β_2 , to be consistently estimated using only the sample of individuals out of the program (with $d_{ivt} = 0$):

$$y_{ivt|(d=0)} = \beta_{0,(d=0)} + \tau_{(d=0)} \bar{d}_{vt} + \mu_{v,(d=0)} + \mu_{t,(d=0)} + u_{ivt|(d=0)} \quad (4.4)$$

Non-participants are subject to an overall effect, $\tau_{(d=0)}$, that only comprises the indirect impact of the program. Therefore, the estimate of the indirect effect on this group can be obtained by the LS estimator for $\tau_{(d=0)}$:

$$\tilde{\beta}_{2,(d=0)} = \hat{\tau}_{(d=0)}.$$

The next step in the decomposition is to infer whether the indirect effect is similar for participants and non-participants — i.e., $\beta_{2,(d=0)} = \beta_{2,(d=1)} = \beta_2$. If it is different, the marginal indirect effect, as well as the marginal overall effect, should change as new individuals are added to the program. Thus the dose-response function of program coverage should be nonlinear. This idea is formally stated in the next proposition.

Proposition 4.2 (Test for Heterogeneity of the Indirect Effect). *If the indirect effect of the intervention is different for participants and non-participants, then the overall effect of the intervention must be nonlinear.*

Once we verify that the overall effect is linear, we can also infer that $\beta_{2,(d=0)} = \beta_{2,(d=1)} = \beta_2$. Using Lemma 4.1, a consistent estimator for the direct effect can be calculated by subtracting the estimated bias from $\hat{\beta}_1$ in equation (4.1):

$$\tilde{\beta}_1 = \hat{\beta}_1 - \left(\hat{\tau}_{(d=0)} - \hat{\beta}_2 \right).$$

Accordingly, inference on the direct effect is made using seemingly unrelated regressions (SUR) of equations (4.1) and (4.4).

5 Main Results

5.1 Overall Effect

This section presents and discusses the overall effect of *Bolsa Família* on the probability of being an entrepreneur. Table 4 shows the estimates obtained using six different models. Model (1), which does not include location fixed effects, shows that the relationship between entrepreneurship and program coverage is negative. Although this model includes control variables such as individual education level, results tend to be biased due to the program targeting on the poorest municipalities. After including fixed effects, the estimated relationship becomes positive in all other models.

TABLE 4 ABOUT HERE

Models (2) and (3) include fixed effects in different levels, municipality (city, town, or village) and census tract (neighborhood). As predicted by Proposition 4.1, which states that the within-municipality program assignment does not affect estimates for the overall effect, the coefficient does not change if I use lower-level fixed effects. According to these models, a 10 percentage point (p.p.) increase in local coverage raises the entrepreneurship level in 0.4 percentage points. Considering the baseline level of 7 p.p. and the current coverage of 19 p.p. the program might be responsible for an increase of 10% in the entrepreneurship rate, keeping everything else constant.

In models (4) and (5), the PAI assumption is relaxed and the local coverage is instrumented by the initial poverty rate (times year dummies). The estimated effect is slightly higher in these models, but not significantly different. Moreover, model (5) also includes social outcomes that had changed over time, such as child mortality, sewer coverage, share of house owners, and school enrollment rates. Since the estimated effect does not change, it does not seem to be driven by other local improvements in well-being.

In model (6), the local coverage variable is replaced by the dummy of individual benefit, but the instrumental variable is the same as before. As expected, the estimated coefficient barely changes because the local-level instrument makes observations be compared between municipalities and not within municipalities. Namely, local coverage and individual benefit are interchangeable as a treatment variable, whose coefficients can both be interpreted as the overall effect of the program on participants.

The estimated overall effect between 4-5 p.p. is found to be larger than PROGRESA's in Mexico, estimated to be 0.9 p.p. by Bianchi and Bobba (2013). However, it is half as large as the Targeted Ultra-Poor program's in Bangladesh (Bandiera et al., 2013) and the Youth Opportunities Program's in Uganda (Blattman et al., 2013). These two programs, nevertheless, are particularly intended to promote entrepreneurship, with the transfer being conditional on productive investments.

5.1.1 Type of Business Being Affected

In order to analyze the nature of entrepreneurship being affected by the program, entrepreneurs are classified by the type of business that they run. Namely, service, sales (wholesale and retail), and manufacturing. Table 5 shows the estimated coefficient of local coverage for these different types. Almost all the effect on entrepreneurship happens by increasing services, such as tailoring, shoe repair, automotive repair, and taxi driving. The remaining effect comes from sales business, while the effect on manufacturing is very close to zero.

TABLE 5 ABOUT HERE

On one hand, the higher effect of services, followed by sales, is expected due to the lower cost of physical assets in this type of business. Some services do not even require a store and can be operated from home, while most sales and manufacturing business require a larger initial investment in products and physical capital. On the other hand, services usually demand higher skills than sales. Unfortunately, no information on training programs is available, but we know that *Bolsa Família* does not have such a component. This result suggests that part of the transfers goes to the hands of already trained entrepreneurs, giving them the opportunity to formalize their

activity. The creation of services, however, may not generate as many jobs as the creation of manufacturing businesses. The effect of *Bolsa Família* on job creation is discussed in sections 6.2 and 6.3.

5.2 Direct and Indirect Effects

In order to estimate the indirect effect of the program, I first have to verify whether it is homogeneous or not. According to Proposition 4.2, if the overall effect is linear, then the indirect effect of the program is homogeneous for the chosen sample. The first column of Table 6 indicates that the quadratic term for local coverage is very close to zero and not significant.

Since the assumption of linear overall effect is not rejected, we can estimate the indirect effect of the program using only the sample of individuals who are not in the program. Columns (2) and (3) of Table 6 show this estimate. The indirect effect seems to be greater than the overall effect discussed above. That is, the direct effect should be negative. The last two columns show the estimates for the model including both levels of intervention — i.e., local and individual. These estimates are bias-adjusted using the previously estimated indirect effect. Nonetheless, the estimated selection bias is very close to zero.²⁰

TABLE 6 ABOUT HERE

The results indicate that, on one hand, cash transfers reduce the probability of participants starting their own business in 3-4 p.p. On the other hand, the amount of cash transferred to poor towns seems to stimulate the creation of new businesses. A 10 p.p. increase in the program size seems to raise the entrepreneurship rate of poor individuals between 0.7 and 0.8 p.p. Because of this positive indirect effect, the net impact of cash transfers on entrepreneurship is also positive.

This difference between direct and indirect responses is exactly the one predicted by Proposition 2.2. It indicates that small entrepreneurs are not as responsive to financial constraints as to other general equilibrium mechanisms. However, there are several possible explanations for the negative direct effect and the positive indirect effect on entrepreneurship.²¹ In the next section, I show that the indirect response seems to be related to the promotion of informal financing mechanism among poor households. Furthermore, the hypothesis of increasing investment opportunity by shifting the aggregate demand is not supported by the following tests.

²⁰The selection bias is measured with respect to entrepreneurship. Other intended outcomes, such as school enrollment and health care, may have different levels of bias.

²¹The negative direct effect does not seem to be driven by conditionalities on education because participants with no child also reduce entrepreneurial activity. See Appendix Table A1.

6 Potential Mechanisms

6.1 Transfers Between Households

The first explanation for the positive indirect effect on entrepreneurship is the increasing number of households transferring money to each other. Like in Angelucci and De Giorgi’s (2009), the indirect effect of the cash transfer program might be driven by the existence of risk-sharing strategies within communities. If poor households follow these strategies, the increasing liquidity can promote an informal financial market for those who do not have access to formal credit and insurance. Unfortunately, I have no information on money lenders for those who opened a business and on the specific amount of transfers received from other households.

Using another household survey, which reports more detailed information on income and expenditures, I calculated the probability of participating households to lend or transfer money to another house unit. Figure 3 shows that program participants are indeed more likely to transfer money to another household in each section of income distribution. On average, participating households have about 40% more chances of being a money lender than non-participating households with the same level of income. This observed difference cannot be strictly interpreted as a causal effect, but it confirms the presumption that the cash transfer flows in the community through private transfers. Moreover, assuming that program participants declare to be poorer than they look in household surveys, the observed difference represents a lower-bound estimate for the causal effect.

FIGURE 3 ABOUT HERE

Back to the original dataset, PNAD interviewers are oriented to ask households about all their sources of income, including transfers received from other households. The total value of these transfers goes under the entries of ‘donations’ and ‘other incomes’ and can be separated from major sources, such as labor earnings, retirement benefits, other pensions, rental earnings, and social programs.²²

Table 7 presents the estimated effect of program coverage on the probability of non-participants receiving ‘other transfers.’ According to the results in columns (1) and (2), a 10 p.p. increase in local coverage raises this probability in 1.3-1.9 p.p. This result suggests that the higher the proportion of beneficiaries in the community, the higher the probability of being financially helped by another household.

TABLE 7 ABOUT HERE

While individuals with better job opportunities may use these transfers as a safety net, individuals with less job opportunities may use them to start their own business. Since I do not know

²²Social transfers are identified using the typical-value method developed by Foguel and Barros (2010).

if current entrepreneurs had received other transfers before, I cannot conclude that these transfers are actually invested. The only conclusion that can be drawn is that the effect on receiving other transfers is the highest among those who most need them. Namely, the effect is significantly higher for the jobless, followed by informal workers. It is worth to clarify that I am not interested in the relationship between receiving other transfers and type of occupation, which cannot be identified as causal. The regressions presented in column (3) of Table 7 just intend to show the heterogeneity of the indirect effect by type of occupation.

In order to verify whether the indirect effects on entrepreneurship and private transfers are related, I include the interaction between coverage and the predicted effect on private transfers in the regression (columns (4) and (5) of Table 7). This predicted effect is calculated by interacting coverage and several municipality characteristics in the estimation of private transfers. These “first-step” interactions already reveal, for instance, that the indirect effects of cash transfers on both private transfers and entrepreneurship are higher in lower density areas, with higher school enrollment rate and higher labor informality. Using the predicted effect on private transfers, I find that the larger this effect, the higher the indirect effect of *Bolsa Família* on entrepreneurship. Although this is just a back-of-the-envelope calculation, it indicates that entrepreneurial activity has increased through the promotion of informal risk-sharing mechanisms.

6.2 Aggregate Demand and Investment Opportunity

If the indirect effect on entrepreneurship came from a shock in the aggregate demand, we should observe other changes in the labor market. For instance, increasing investment opportunities should also affect the decision of high-educated men to become entrepreneurs. Moreover, with higher purchasing power, either more jobs should be created or higher salaries should be provided. Accordingly, I also estimate the indirect effect of cash transfers on these outcomes.

TABLE 8 ABOUT HERE

The first two columns of Table 8 confirm that the program size has no significant effect on the probability of high-educated men becoming entrepreneurs. Thus we cannot say the program has encouraged the creation of local businesses in general. That is, the effect on entrepreneurship is concentrated among low-educated workers, who are probably connected to a network of eligible households.

Furthermore, the estimates in columns (3) and (4) do not corroborate the hypothesis of job creation. Even though more low-educated men have taken the decision of being entrepreneurs, the program has had no effect on their overall employment rate. This result suggests that the program does not affect the demand side of the labor market. It may have just affected the

occupational choice on the supply side. The direct and indirect effects of *Bolsa Família* on other occupational choices are discussed below.

Although the employment rate has not been significantly affected by *Bolsa Família*, it is possible that the effect on the demand side has been just on wages. It is worth to notice that the estimated effect on wages can be misleading if the program has some influence on local prices. Unfortunately, I do not have information on prices at the municipality level. However, I can use wages of low-educated public employees as a proxy for labor costs. Then the real effect on aggregate demand is assessed by the difference between private documented employees and public servants in terms of changes on wages. Indeed, the estimated coefficient for the interaction between program coverage and private employee, in the last two columns of Table 8, is very close to zero.²³

6.3 Other Occupational Choices

To understand where the responsive entrepreneurs comes from, I also investigate the effect of the program on other occupational choices. Besides entrepreneur, the alternatives are jobless, formal employee, informal employee, and informal self-employed. Table 9 presents the direct and indirect effects of the program on the probability of being in each one of these categories, *vis-à-vis* being in any other category.

TABLE 9 ABOUT HERE

The estimated indirect coefficients indicate that the program has no significant effect on the proportion of jobless in intervened areas. The program does not have a significant indirect effect on the proportion of formal employees either. Once again, the hypothesis that the money injected in local economies shifts the demand for workers is not supported by these results. In other words, the increasing participation of documented employees in the Brazilian labor market in the 2000's cannot be attributed as much to the *Bolsa Família* program as to other demographic and economic changes.²⁴

The strongest indirect effect is on the proportion of informal employees. Assuming that the labor market is partially segregated, the program may have given the financial opportunity to informal workers to open their own business. As already explained, the cash transfered by *Bolsa*

²³ A regression of wages on program coverage, excluding public servants, would show that the effect is significantly positive. However, this effect is not only on private employees. The general effect on wages indicates that the impact does not come from the specific demand for labor, but from general labor costs.

²⁴ Articles in 'The Economist' magazine, published on Feb. 12 2009, and in 'The New York Times', published on July 31 2008, mentioned that *Bolsa Família* was an example of CCT program that has helped to expand formal employment in Brazil. Nonetheless, there is no strong evidence for such a conclusion. See Kakwani et al. (2006) for a review on pro-poor growth in Brazil during the 2000's.

Família has probably flowed into the hands of these workers by means of private transfers among poor households.

As regards the direct impact on program participants, the negative effect on entrepreneurship looks symmetric to the positive effect on the jobless rate. That is, this negative effect seems to be strictly related to the income effect that unearned income has on labor supply. On the other hand, the reduction in labor supply only happens among formal workers (entrepreneurs and documented employees).²⁵ Thus program participants might not reduce labor supply because leisure is a normal good, as the classical model predicts. A more plausible reason is that they do not want to lose the benefit for uncertain earnings. Unlike formal workers, informal workers do not have their income tracked by the government, so they do not need to stop working in order to stay officially eligible for the transfer.²⁶

According to the official records of the Ministry of Social Development and Fight Against Hunger (MDS), almost 40% of cases of benefit cancellation is due to income improvement. Also, the main reported reason for this type of cancellation is the identification of formal workers' earnings in the Ministry of Labor and Employment's dataset, so-called RAIS.

6.4 Confounding Factors

The identification of all effects estimated so far essentially depends on the assumption that the relationship between poverty and entrepreneurship does not change over time, unless by means of the own program growth. In other words, there is no convergence in the entrepreneurship rate across municipalities in Brazil. This convergence could be driven by other social programs or by a process of credit expansion. In the main results shown above (column (5) of Table 4), I already included some social outcomes in order to control for part of these programs. Once again, the estimated effect of *Bolsa Família* barely changed.

A direct way of testing for convergence is by including the interaction between poverty rate and year dummies in the fixed-effect regression. As observed in column (1) of Table 10, the interaction coefficients are close to zero and not significant. Also the overall effect of program coverage remains around 4 p.p., as found before.

TABLE 10 ABOUT HERE

As regards the increasing access to credit, Figure 4 shows that the decline in interest rates and the growth of personal loans started in 2005. Thus there is a small overlap between the

²⁵A similar result is found by Gasparini et al. (2009) in Argentina and Amarante et al. (2011) in Uruguay.

²⁶The direct effects on labor supply in the formal and informal sectors might be distinct due to differences in workers' ability. However, the same pattern emerges in subsamples of individuals with and without high school diploma. See Appendix Table A2.

investigated period (2001-06) and the period of credit expansion in Brazil. Despite this small overlap, columns (2) and (3) of Table 10 confirm that the estimated effect for the 2001-04 period is also around 4-6 p.p.

FIGURE 4 ABOUT HERE

Although the credit expansion started in the late 2000's, other microcredit programs have been in place since the 1990's. To test whether the results are driven by microcredit programs, I exclude from the sample the region where the largest and most significant program was introduced. The *CrediAmigo* program, created in 1997, is considered the largest microfinance program in the country, but it covers only municipalities in the Northeast region. Columns (4) and (5) of Table 10 show that the estimated effect on entrepreneurship slightly increases after omitting that region. Thus the results do not seem to be a consequence of the growth in microcredit either.

Another form of convergence is through the migration of human capital. That is, social programs might have promoted the migration of potential entrepreneurs, as well as other type of workers, to highly covered areas. As shown in Table 11, the program coverage has no significant effect on the probability of migrating from other municipality in the last four years. Therefore, the estimated effects are probably not due to changes in the composition of workers in the labor force, but due to changes in their decisions.

TABLE 11 ABOUT HERE

7 Conclusion

This paper investigated the causal relationship between conditional cash transfer (CCT) programs and the decision of being a small entrepreneur. Entrepreneurship is not usually an intended outcome of CCTs, since their goals are often strictly related to child development and income redistribution. However, investigating this outcome can tell us something about their broader impacts on economic development in the short run. Besides estimating the impact on an urban population, which is rarely seen in the literature about aid programs, the critical distinction of this analysis is the separation between direct and indirect effects. The identification of spillovers might reveal that the impact of those transfers goes well-beyond cash and conditionalities, uncovering the role of inter-household exchanges within the informal economy.

Since the benefit is primarily assigned at the village level in most of the treated-control settings, evaluation designs usually allow only the identification of the overall effects of aid programs. In this study, the decomposition into direct and indirect effects is identified due to the variation in the size of the *Bolsa Família* program across municipalities in Brazil. Despite the issues with selection into the program, the overall effect is identified due to the exogeneity of the local coverage

growth. Then the decomposition of this overall effect is made by adjusting the coefficients for the estimated selection bias. Although this method is applied to observational data, it also introduces a new way of designing experiments, in which only the size (proportion of benefits) rather than the individual benefit is randomized at the cluster level.²⁷

The results indicate that, on one hand, cash transfers have a negative direct effect on entrepreneurship, reducing the probability of beneficiaries to start their own business. This direct effect is associated with the negative impact that transfers have on the participation of workers in the formal sector. It suggests that the program encourage its beneficiaries to either reduce labor supply or move to the informal sector to not lose their cash benefit. This finding ratifies a major concern in welfare programs in general and reveals a caveat in terms of eligibility rules.²⁸

On the other hand, the amount of cash transfered to poor villages seems to encourage the creation of new businesses, mostly in the service sector. There is no evidence, however, that this positive impact is driven by shocks in the aggregate demand. For instance, neither the proportion of high-educated entrepreneurs nor the number of formal jobs grew with the program. The lack of other impacts on the labor market indicates that *Bolsa Família* has indirectly changed the occupational choice of poor workers in the supply side, but not the demand for labor. This finding is not as exceptional as some CCT advocates claim, but it suggests that the program has been responsible for the formalization of low-skilled workers through self-employment.

A plausible explanation for the indirect effect is the existence of informal risk-sharing arrangements. The evidence is that the CCT program has encouraged interpersonal transfers, particularly to those facing income shortage. Then the liquidity shock delivered by the program appears to reduce the opportunity cost of risk-sharing among poor households, rather than lessening individual credit and insurance constraints. That is, entrepreneurship looks to be more responsive to locally aggregate liquidity shocks, which promotes informal financing mechanisms, than to individual liquidity shocks.

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²⁷This new approach can be used to simplify the two-step randomization proposed by Duflo and Saez (2003) and Crépon et al. (2013).

²⁸See Besley and Coate (1992), Kanbur et al. (1994), and Moffitt (2002).

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Appendix A Proofs of Section 2

A.1 Proof of Proposition 2.1

Let G denote the state of business success and B the state of business failure. If only positive and non-contingent savings are allowed, consumption of wage employees (L) and entrepreneurs (M) in periods 1 and 2 is:

$$\begin{aligned} c_1^L &= a + d_1 - s_L^* \\ c_2^L &= w + d_2 + s_L^* \\ c_1^M(q) &= a + d_1 - k - s_M^*(q) \\ c_{2,G}^M(q) &= q + (1 - \zeta) d_2 + s_M^*(q) \\ c_{2,B}^M(q) &= \delta + d_2 + s_M^*(q) \end{aligned}$$

where $s_M^* \geq 0$ and $s_L^* \geq 0$ are the optimal levels of savings.

The utility trade-off at the margin of indifference is:

$$\begin{aligned} D(\hat{q}) &\equiv U(M; \hat{q}) - U(L) \\ &= \lambda u(c_{2,G}^M(\hat{q})) + (1 - \lambda) u(c_{2,B}^M(\hat{q})) + u(c_1^M(\hat{q})) - [u(c_2^L) + u(c_1^L)]. \end{aligned}$$

To simplify the following derivations, let $c_{2,G}^M(\hat{q}) = c_{2,G}^M$, $c_{2,B}^M(\hat{q}) = c_{2,B}^M$, and $c_1^M(\hat{q}) = c_1^M$. Since $D(\hat{q}) = 0$,

$$\lambda u(c_{2,G}^M) + (1 - \lambda) u(c_{2,B}^M) - u(c_2^L) = u(c_1^L) - u(c_1^M). \quad (\text{A.1})$$

The first-order conditions for the individual maximization problem are:

$$u'(c_1^M) = \lambda u'(c_{2,G}^M) + (1 - \lambda) u'(c_{2,B}^M) + \vartheta_M, \quad (\text{A.2})$$

$$u'(c_1^L) = u'(c_2^L) + \vartheta_L, \quad (\text{A.3})$$

where $\vartheta_M, \vartheta_L \geq 0$ are Lagrange multipliers, with $\vartheta_M s_M^* = \vartheta_L s_L^* = 0$.

Given the distribution of entrepreneurial skills, the effect of cash transfers, d , on the entrepreneurship rate is proportional to their effect on the utility trade-off of the indifferent individual, $D(\hat{q})$. Moreover, this effect can be written as the sum of the effects of current transfers, d_1 , and future transfers, d_2 :

$$\frac{dD(\hat{q})}{dd} = \frac{dD(\hat{q})}{dd_1} + \frac{dD(\hat{q})}{dd_2}.$$

While the effect of current transfers, d_1 , is interpreted as the *credit effect* (CE),

$$CE \equiv \frac{dD(\hat{q})}{dd_1};$$

the effect of future transfers is the difference between the *insurance effect* (IE),

$$IE \equiv \left. \frac{dD(\hat{q})}{dd_2} \right|_{\zeta=0},$$

and the *eligibility effect* (EE),

$$EE \equiv \left. \frac{dD(\hat{q})}{dd_2} \right|_{\zeta=1} - \left. \frac{dD(\hat{q})}{dd_2} \right|_{\zeta=0}.$$

By the envelope theorem, the credit effect of current transfers is:

$$CE = \frac{\partial D(\hat{q})}{\partial d_1} = u'(c_1^M) - u'(c_1^L).$$

If $s_L^* = 0$ and u is strictly concave, then $c_1^L > c_1^M$ and

$$u'(c_1^M) > u'(c_1^L) \implies \frac{\partial D(\hat{q})}{\partial d_1} > 0.$$

If $s_L^* > 0$, then from condition (A.3):

$$u'(c_1^L) = u'(c_2^L) \iff u(c_1^L) = u(c_2^L) \quad (\text{A.4})$$

With $u'' < 0$ and $u''' \geq 0$, condition (A.2) implies that:

$$\lambda u(c_{2,G}^M) + (1 - \lambda) u(c_{2,B}^M) \geq u(c_1^M), \quad (\text{A.5})$$

with strict inequality for $\lambda \in (0, 1)$.

With (A.4) and (A.5), (A.1) implies that

$$u(c_1^L) \geq u(c_1^M) \iff u'(c_1^M) \geq u'(c_1^L),$$

with strict inequality for $\lambda \in (0, 1)$. Therefore, for any $s_M^* \geq 0$, $s_L^* \geq 0$, and $\lambda \in (0, 1)$, the credit effect of current transfers, d_1 , is positive:

$$CE = u'(c_1^M) - u'(c_1^L) > 0. \quad (\text{A.6})$$

By the envelope theorem, the effect of future transfers, d_2 , on $D(\hat{q})$ is:

$$\frac{dD(\hat{q})}{dd_2} = \frac{\partial D(\hat{q})}{\partial d_2} = (1 - \zeta) \lambda u'(c_{2,G}^M) + (1 - \lambda) u'(c_{2,B}^M) - u'(c_2^L).$$

To analyze the insurance effect, suppose that the eligibility rule is not applied, $\zeta = 0$. Then

$$IE \equiv \left. \frac{\partial D(\hat{q})}{\partial d_2} \right|_{\zeta=0} = \lambda u'(c_{2,G}^M) + (1 - \lambda) u'(c_{2,B}^M) - u'(c_2^L).$$

If $s_M^* > 0$, then from (A.2), (A.3), and (A.6):

$$\begin{aligned} IE &= u'(c_1^M) - u'(c_2^L) \\ &\geq u'(c_1^M) - u'(c_1^L) \\ &= CE > 0 \end{aligned} \tag{A.7}$$

for any $s_L^* \geq 0$ and $\lambda \in (0, 1)$. That is, with positive savings, $s_M^* > 0$, the insurance effect is as large as the credit effect.

If $s_M^* = 0$, then the insurance effect is decreasing in λ :

$$\frac{\partial IE}{\partial \lambda} = u'(\hat{q} + d_2) - u'(\delta + d_2) < 0. \tag{A.8}$$

Suppose that $\lambda = 1$ and w.l.o.g. $u'(c_2^L) > u'(\hat{q} + d_2)$, such that

$$IE = u'(\hat{q} + d_2) - u'(c_2^L) < 0.$$

Then with (A.1) and (A.3):

$$u(\hat{q} + d_2) > u(c_2^L) \geq u(c_1^L) > u(c_1^M).$$

Moreover, since u is strictly concave:

$$\begin{aligned} u'(c_1^M) - u'(c_1^L) &> u'(c_2^L) - u'(\hat{q} + d_2) \\ CE &> -IE. \end{aligned} \tag{A.9}$$

From condition (A.8), as λ decreases, the insurance effect increases and eventually becomes positive. Thus even if the insurance effect is negative, the credit effect is large enough so that the net effect of cash transfers is positive.

Therefore, with no eligibility rule, $\zeta = 0$, (A.6), (A.7), and (A.9) guarantee that for any $s_M^* \geq 0$, $s_L^* \geq 0$, and $\lambda \in (0, 1)$, the net effect of cash transfers is positive:

$$\left. \frac{dD(\hat{q})}{dd} \right|_{\zeta=0} = CE + IE > 0.$$

Now suppose that the eligibility rule is applied, $\zeta = 1$. The effect of this rule on the trade-off, $D(\hat{q})$, is:

$$\begin{aligned} EE &= \left. \frac{dD(\hat{q})}{dd_2} \right|_{\zeta=1} - \left. \frac{dD(\hat{q})}{dd_2} \right|_{\zeta=0} \\ &= -\lambda u'(c_{2,G}^M) < 0. \end{aligned} \tag{A.10}$$

Because of this negative effect, the net effect of cash transfers becomes ambiguous if the eligibility rule is applied:

$$\begin{aligned} \left. \frac{dD(\hat{q})}{dd_2} \right|_{\zeta=1} &= CE + IE + EE \\ &= u'(c_1^M) - u'(c_1^L) + (1-\lambda)u'(c_{2,B}^M) - u'(c_2^L) \gtrless 0. \end{aligned} \quad (\text{A.11})$$

Even though $u'(c_1^M) - u'(c_1^L) > 0$ and $u'(c_{2,B}^M) - u'(c_2^L) > 0$, we have $(1-\lambda)u'(c_{2,B}^M) - u'(c_2^L) < 0$ for some $\lambda \in (0, 1)$. Since the eligibility effect is increasing in the probability of business success, λ , the net effect is decreasing in λ :

$$\begin{aligned} \left. \frac{d^2 D(\hat{q})}{dd_2 d\lambda} \right|_{\zeta=1} &= [u''(c_1^M) + (1-\lambda)u''(c_{2,B}^M)] \frac{\partial s_M^*}{\partial \lambda} - u'(c_{2,B}^M) \\ &= \begin{cases} \frac{u''(c_1^M) + (1-\lambda)u''(c_{2,B}^M)}{u''(c_1^M) + \lambda u''(c_{2,G}^M) + (1-\lambda)u''(c_{2,B}^M)} [u'(c_{2,B}^M) - u'(c_{2,G}^M)] - u'(c_{2,B}^M) & \text{if } s_M^* > 0 \\ -u'(c_{2,B}^M) & \text{if } s_M^* = 0 \end{cases} \\ &< 0. \end{aligned}$$

Accordingly, there exists some $\bar{\lambda} > 0$ so that the net effect is positive for all $\lambda < \bar{\lambda}$:

$$\bar{\lambda} = \frac{u'(c_1^M) - u'(c_1^L) + u'(c_{2,B}^M) - u'(c_2^L)}{u'(c_{2,B}^M)} > 0.$$

■

A.2 Proof of Proposition 2.2

Let G denote the state of business success and B the state of business failure. Given the price of contingent bonds, r , and the price of business insurance, i , consumption of wage employees (L) and entrepreneurs (M) in periods 1 and 2 is:

$$\begin{aligned} c_1^L &= a + d_1 - rb_L^* + ig_L^* \\ c_{2,G}^L &= w + d_2 + b_L^* \\ c_{2,B}^L &= w + d_2 - g_L^* \\ c_1^M(q) &= a + d_1 - k + rb_M^*(q) - ig_M^*(q) \\ c_{2,G}^M(q) &= q + (1-\zeta)d_2 - b_M^*(q) \\ c_{2,B}^M(q) &= \delta + d_2 + g_M^*(q) \end{aligned}$$

where b_L^* is the individual demand for contingent bonds, b_M^* is the individual supply of contingent bonds, g_L^* is the individual supply of business insurance, and g_M^* is the individual demand for

business insurance. Since wage employees and entrepreneurs trade insurances, the consumption of both types in period 2 will be subject to the state of nature, $\{G, B\}$.

The utility trade-off at the margin of indifference is:

$$\begin{aligned} D(\hat{q}) &\equiv U(M; \hat{q}) - U(L) \\ &= \lambda u(c_{2,G}^M(\hat{q})) + (1-\lambda) u(c_{2,B}^M(\hat{q})) + u(c_1^M(\hat{q})) \\ &\quad - [\lambda u(c_{2,G}^L) + (1-\lambda) u(c_{2,B}^L) + u(c_1^L)]. \end{aligned}$$

To simplify, let $c_{2,G}^M(\hat{q}) = c_{2,G}^M$, $c_{2,B}^M(\hat{q}) = c_{2,B}^M$, and $c_1^M(\hat{q}) = c_1^M$. Since $D(\hat{q}) = 0$,

$$\lambda [u(c_{2,G}^M) - u(c_{2,G}^L)] + (1-\lambda) [u(c_{2,B}^M) - u(c_{2,B}^L)] = u(c_1^L) - u(c_1^M). \quad (\text{A.12})$$

The first-order conditions for the individual maximization problem imply that:

$$r = \lambda \frac{u'(c_{2,G}^M)}{u'(c_1^M)} = \lambda \frac{u'(c_{2,G}^L)}{u'(c_1^L)} \quad (\text{A.13})$$

$$i = (1-\lambda) \frac{u'(c_{2,B}^M)}{u'(c_1^M)} = (1-\lambda) \frac{u'(c_{2,B}^L)}{u'(c_1^L)} \quad (\text{A.14})$$

Let y be the entrepreneurship rate and F be the cumulative distribution of entrepreneurial skills, so that $y = 1 - F(\hat{q})$. The *direct effect* of cash transfers, $d_1 = d_2 = d$, on the entrepreneurship rate is proportional to their effect on the utility trade-off of the indifferent individual, $D(\hat{q})$:

$$\frac{\partial y}{\partial d} \propto \frac{\partial D(\hat{q})}{\partial d_1} + \frac{\partial D(\hat{q})}{\partial d_2},$$

where

$$\frac{\partial D(\hat{q})}{\partial d_1} = u'(c_1^M) - u'(c_1^L),$$

and, with (A.13) and (A.14),

$$\begin{aligned} \frac{\partial D(\hat{q})}{\partial d_2} &= \lambda [(1-\zeta) u'(c_{2,G}^M) - u'(c_{2,G}^L)] + (1-\lambda) [u'(c_{2,B}^M) - u'(c_{2,B}^L)] \\ &= r [u'(c_1^M) - u'(c_1^L)] + i [u'(c_1^M) - u'(c_1^L)] - \zeta \lambda u'(c_{2,G}^M) \\ &= (r+i) \frac{\partial D(\hat{q})}{\partial d_1} - \zeta \lambda u'(c_{2,G}^M). \end{aligned}$$

Suppose $\partial D(\hat{q}) / \partial d_1 > 0$, so that

$$u'(c_1^M) > u'(c_1^L) \Leftrightarrow u(c_1^L) > u(c_1^M) \quad (\text{A.15})$$

Then (A.13) implies that

$$u'(c_{2,G}^M) > u'(c_{2,G}^L) \Leftrightarrow u(c_{2,G}^M) < u(c_{2,G}^L)$$

and (A.12) implies that

$$\lambda [u(c_{2,G}^M) - u(c_{2,G}^L)] + (1 - \lambda) [u(c_{2,B}^M) - u(c_{2,B}^L)] > 0.$$

Hence,

$$u(c_{2,B}^M) > u(c_{2,B}^L) \Leftrightarrow u'(c_{2,B}^L) > u'(c_{2,B}^M).$$

Along with (A.14), it implies that

$$u'(c_1^L) > u'(c_1^M),$$

which contradicts (A.15). Similarly, $\partial D(\hat{q})/\partial d_1$ cannot be less than 0, because it contradicts (A.12), (A.13), and (A.14). Therefore,

$$\frac{\partial D(\hat{q})}{\partial d_1} = 0,$$

$$\frac{\partial D(\hat{q})}{\partial d_2} = \begin{cases} -\lambda u'(c_{2,G}^M) & \text{if } \zeta = 1 \\ 0 & \text{if } \zeta = 0 \end{cases}$$

and

$$\begin{aligned} \frac{\partial y}{\partial d} &\propto \frac{\partial D(\hat{q})}{\partial d_1} + \frac{\partial D(\hat{q})}{\partial d_2} \\ &= \begin{cases} -\lambda u'(c_{2,G}^M) & \text{if } \zeta = 1 \\ 0 & \text{if } \zeta = 0 \end{cases} \end{aligned}$$

i.e., the *direct effect* of cash transfers on entrepreneurship is negative if the eligibility rule is applied ($\zeta = 1$) or zero otherwise.

Since individuals have the same convex preferences, the equilibrium entrepreneurship rate, y^* , can be obtained by solving the social planner's problem:

$$\begin{aligned} \max_{y \in [0,1]} U(y; d_1, d_2) &= u[a + d_1 - y^*k] + \lambda u[Q(y^*) + (1 - y^*)w + (1 - \zeta y^*)d_2] \\ &\quad + (1 - \lambda) u[y^*\delta + (1 - y^*)w + d_2], \end{aligned}$$

where $Q(y^*)$ is the aggregate output produced by all entrepreneurs with $q \geq \hat{q}$.

To simplify the following derivations, let

$$\begin{aligned} c_1 &= a + d_1 - y^*k \\ c_{2,G} &= Q(y^*) + (1 - y^*)w + (1 - \zeta y^*)d_2 \\ c_{2,B} &= y^*\delta + (1 - y^*)w + d_2. \end{aligned}$$

The first- and second-order conditions for the social planner's problem are:

$$\begin{aligned} U' &= -ku'(c_1) + \lambda [Q' - w - \zeta d_2] u'(c_{2,G}) - (1 - \lambda)(w - \delta) u'(c_{2,B}) \\ &= 0, \end{aligned} \quad (\text{A.16})$$

and

$$\begin{aligned} U'' &= k^2 u''(c_1) + \lambda u'(c_{2,G}) Q'' + \lambda [Q' - w - \zeta d_2]^2 u''(c_{2,G}) + (1 - \lambda)(w - \delta)^2 u''(c_{2,B}) \\ &< 0. \end{aligned} \quad (\text{A.17})$$

Differentiating (A.16) with respect to d_1 , we obtain

$$\frac{dy^*}{dd_1} = k \frac{u''(c_1)}{U''} > 0; \quad (\text{A.18})$$

and differentiating (A.16) with respect to d_2 , we obtain

$$\begin{aligned} \frac{dy^*}{dd_2} &= \frac{1}{U''} \{ \zeta \lambda u'(c_{2,G}) + (1 - \lambda)(w - \delta) u''(c_{2,B}) - \lambda(1 - \zeta y^*) [Q' - w - \zeta d_2] u''(c_{2,G}) \} \\ &= \frac{1}{U''} \{ (1 - \lambda)(w - \delta) u''(c_{2,B}) - \lambda(1 - \zeta y^*) [Q' - w - \zeta d_2] u''(c_{2,G}) \} + EE, \end{aligned}$$

where $EE \equiv \zeta \lambda u'(c_{2,G}) / U''$. Note that

$$EE \propto \frac{\partial D(\hat{q})}{\partial d_2} < 0,$$

i.e., EE represents the direct effect that the eligibility rule has on the entrepreneurship rate, y^* .

Let GE denote the *indirect effect* of cash transfers, d , on the entrepreneurship rate, y^* . Since the direct effect is EE , the indirect effect is:

$$GE = \frac{dy^*}{dd_1} + \frac{dy^*}{dd_2} - EE.$$

Note that for individuals to prefer trading insurances rather than saving their wealth privately:

$$c_{2,G} > c_1 > c_{2,B} \quad (\text{A.19})$$

Using (A.16), (A.19), and $u''' \geq 0$, we have

$$\begin{aligned} GE &= \frac{1}{U''} \{ ku''(c_1) + (1 - \lambda)(w - \delta) u''(c_{2,B}) - \lambda(1 - \zeta y^*) [Q' - w - \zeta d_2] u''(c_{2,G}) \} \\ &\geq \frac{u''(c_{2,G})}{U''} \left\{ k \frac{u''(c_1)}{u''(c_{2,G})} + (1 - \lambda)(w - \delta) \frac{u''(c_{2,B})}{u''(c_{2,G})} - \lambda [Q' - w - \zeta d_2] \right\} \\ &\geq \frac{u''(c_{2,G})}{U''} \{ k + (1 - \lambda)(w - \delta) - \lambda [Q' - w - \zeta d_2] \} \\ &> \frac{u''(c_{2,G})}{U'' u'(c_{2,G})} \{ ku'(c_1) + (1 - \lambda)(w - \delta) u'(c_{2,B}) - \lambda [Q' - w - \zeta d_2] u'(c_{2,G}) \} \\ &= 0. \end{aligned}$$

Therefore, the *indirect effect* of cash transfers on the entrepreneurship rate is positive. ■

Appendix B Proofs of Section 4

B.1 Proof of Lemma 4.1

To simplify the proof, we start with the following within-group version of equation (4.1):

$$y_{ivt}^* = \beta_1 d_{ivt}^* + \beta_2 \bar{d}_{vt}^* + u_{ivt} \quad (\text{B.1})$$

where $y_{ivt}^* = (y_{ivt} - \bar{y}_v - \bar{y}_t)$, $d_{ivt}^* = (d_{ivt} - \bar{d}_v - \bar{d}_t)$, and $\bar{d}_{vt}^* = (\bar{d}_{vt} - \bar{\bar{d}}_v - \bar{\bar{d}}_t)$.

Let $S_x \equiv \sum_t \sum_v \sum_i x_{ivt}$. By construction, $S_{d^*} = S_{\bar{d}^*} = 0$ and $S_{d^* \bar{d}^*} = S_{\bar{d}^{*2}}$. Then the least squares (LS) estimator can be written as follows:

$$\begin{aligned} \begin{bmatrix} \hat{\beta}_1 \\ \hat{\beta}_2 \end{bmatrix} &= \frac{1}{[S_{d^{*2}} S_{\bar{d}^{*2}} - (S_{d^* \bar{d}^*})^2]} \begin{bmatrix} S_{\bar{d}^{*2}} & -S_{d^* \bar{d}^*} \\ -S_{d^* \bar{d}^*} & S_{d^{*2}} \end{bmatrix} \begin{bmatrix} S_{d^* y^*} \\ S_{\bar{d}^* y^*} \end{bmatrix} \\ &= \frac{1}{(S_{d^{*2}} - S_{\bar{d}^{*2}})} \begin{bmatrix} S_{d^* y^*} - S_{\bar{d}^* y^*} \\ \frac{S_{d^{*2}}}{S_{\bar{d}^{*2}}} S_{\bar{d}^* y^*} - S_{d^* y^*} \end{bmatrix} \\ &= \begin{bmatrix} \beta_1 + \frac{1}{(S_{d^{*2}} - S_{\bar{d}^{*2}})} (S_{d^* u} - S_{\bar{d}^* u}) \\ \beta_2 + \frac{1}{(S_{d^{*2}} - S_{\bar{d}^{*2}})} \left(\frac{S_{d^{*2}}}{S_{\bar{d}^{*2}}} S_{\bar{d}^* u} - S_{d^* u} \right) \end{bmatrix}. \end{aligned} \quad (\text{B.2})$$

Consider that there exists a sample size N so that for every sample with $n \geq N$, $\bar{d}_{vt} \in (0, 1)$ for some ivt -observation. This condition implies that $S_{d^{*2}} > S_{\bar{d}^{*2}}$ for a large enough sample. Finally, by the Law of Large Numbers:

$$\begin{aligned} \hat{\beta}_1 &\xrightarrow{p} \beta_1 + \frac{1}{[Var(d_{ivt}) - Var(\bar{d}_{vt})]} [E(u_{ivt} d_{ivt}^*) - E(u_{ivt} \bar{d}_{vt}^*)] \\ &= \beta_1 + \frac{E(u_{ivt} d_{ivt}^*)}{[Var(d_{ivt}) - Var(\bar{d}_{vt})]} \end{aligned} \quad (\text{B.3})$$

and

$$\begin{aligned} \hat{\beta}_2 &\xrightarrow{p} \beta_2 + \frac{1}{[Var(d_{ivt}) - Var(\bar{d}_{vt})]} \left[\frac{Var(d_{ivt})}{Var(\bar{d}_{vt})} E(u_{ivt} \bar{d}_{vt}^*) - E(u_{ivt} d_{ivt}^*) \right] \\ &= \beta_2 - \frac{E(u_{ivt} d_{ivt}^*)}{[Var(d_{ivt}) - Var(\bar{d}_{vt})]}, \end{aligned} \quad (\text{B.4})$$

where $E(u_{ivt} \bar{d}_{vt}^*) = 0$ because of the PAI assumption. ■

B.2 Proof of Proposition 4.1

Let $y_{ivt}^*, d_{ivt}^*, \bar{d}_{vt}^*$ be village-period mean-centered versions of $y_{ivt}, d_{ivt}, \bar{d}_{vt}$, respectively.

For the first part, the LS estimator for τ in equation (4.2) is the following:

$$\begin{aligned}
\hat{\tau} &= \frac{\sum_{ivt} \bar{d}_{vt}^* y_{ivt}^*}{\sum_{ivt} \bar{d}_{vt}^{*2}} \\
&= \frac{\hat{\beta}_1 \sum_{ivt} \bar{d}_{vt}^* d_{ivt}^* + \hat{\beta}_2 \sum_{ivt} \bar{d}_{vt}^* \bar{d}_{vt}^* + \sum_{ivt} \bar{d}_{vt}^* \hat{u}_{ivt}}{\sum_{ivt} \bar{d}_{vt}^{*2}} \\
&= \frac{\hat{\beta}_1 \sum_{vt} \bar{d}_{vt}^* \sum_i d_{ivt}^*}{\sum_{vt} \sum_i \bar{d}_{vt}^{*2}} + \hat{\beta}_2 \\
&= \hat{\beta}_1 + \hat{\beta}_2.
\end{aligned} \tag{B.5}$$

For the second part, Lemma 4.1 is applied so that

$$\hat{\tau} \xrightarrow{p} \beta_1 + \beta_2. \tag{B.6}$$

■

B.3 Proof of Proposition 4.2

Suppose the true equation to be estimated is:

$$y_{ivt} = \beta_0 + \beta_1 d_{ivt} + \beta_2 \bar{d}_{vt} + \beta_3 d_{ivt} \bar{d}_{vt} + \mu_v + \mu_t + u_{ivt}, \tag{B.7}$$

so that coefficient β_3 captures the difference in the indirect effect between participants and non-participants.

If we aggregate the observations at the village-period level, then:

$$\bar{y}_{vt} = \beta_0 + (\beta_1 + \beta_2) \bar{d}_{vt} + \beta_3 \bar{d}_{vt}^2 + \mu_v + \mu_t + \bar{u}_{vt}$$

and the overall effect of \bar{d}_{vt} becomes nonlinear. ■

Appendix C IV with a Municipality-Level Instrument

Proposition C.1. *Let z_{ivt} be an instrumental variable. If the period-village conditional variance of z_{ivt} is zero, $\text{Var}(z_{ivt}|v, t) = 0$, then the IV estimator for τ in equation (4.2) is equivalent to the IV estimator for τ in the following equation:*

$$y_{ivt} = \beta_0 + \tau d_{ivt} + \mu_v + \mu_t + u_{ivt}. \tag{C.1}$$

Proof. Let $y_{ivt}^*, d_{ivt}^*, \bar{d}_{vt}^*$ be village-period mean-centered versions of $y_{ivt}, d_{ivt}, \bar{d}_{vt}$, respectively.

Suppose equation (4.1) is the true equation, but we instead estimate the following model:

$$y_{ivt} = \beta_0 + \beta_1 d_{ivt} + \mu_v + \mu_t + u_{ivt}, \quad (\text{C.2})$$

in which \bar{d}_{vt} is omitted.

Let z_{vt} be an instrumental variable such that $\text{Var}_{vt}(z_{vt}) = 0$. Then the (within-group) IV estimator for β_1 in equation (C.2) is:

$$\begin{aligned} \hat{\beta}_1^{IV} &= \frac{\sum_{ivt} z_{vt}^* y_{ivt}^*}{\sum_{ivt} z_{vt}^* d_{ivt}^*} \\ &= \frac{\sum_{ivt} z_{vt}^* y_{ivt}^*}{\sum_{vt} z_{vt}^* \sum_i d_{ivt}^*} \\ &= \frac{\sum_{ivt} z_{vt}^* y_{ivt}^*}{\sum_{ivt} z_{vt}^* \bar{d}_{vt}^*} = \hat{\tau}^{IV}. \end{aligned}$$

Thus the formula is exactly the same as if we estimate equation (4.2) using z_{vt} as an instrumental variable. Using similar steps as in Proposition 4.1, we can show that $\hat{\tau}^{IV}$, as well as $\hat{\beta}_1^{IV}$, is a consistent estimator for the overall effect, $(\beta_1 + \beta_2)$. ■

Table 1: Poverty Headcount and Program Coverage

	2001			2004			2006		
	Total	Urban	Rural	Total	Urban	Rural	Total	Urban	Rural
Poverty headcount	0.301	0.250	0.579	0.285	0.241	0.534	0.225	0.183	0.466
Program coverage	0.064	0.044	0.174	0.178	0.146	0.360	0.227	0.188	0.450
Share of benefits		0.599	0.401		0.686	0.314		0.708	0.292
Number of obs.	368,605	316,793	51,812	378,658	326,322	52,336	389,807	336,502	53,305

Estimates are obtained using the National Household Survey (PNAD). ‘Poverty headcount’ is measured by the proportion of people with household per capita income below the poverty line (1/2 of the 2001 minimum wage). ‘Program coverage’ is measured by the proportion of people participating in the program. ‘Share of benefits’ is the ratio between the total amount of transfers going to either urban or rural areas and the total amount of transfers distributed by conditional cash transfer programs in the country.

Table 2: Number of Observations per Municipality

	Mean	Std. Dev.	Min.	Max.	Number of municipalities
2001					
Number of households	128.1	290.4	19	3,505	796
Number of sample-eligible men	52.4	128.1	5	1,571	796
2004					
Number of households	136.8	305.1	23	3,575	796
Number of sample-eligible men	54.3	131.8	5	1,751	796
2006					
Number of households	143.8	322.7	28	3,884	796
Number of sample-eligible men	56.4	136.1	5	1,753	796

‘Sample-eligible men’ comprise men aged between 25 and 45 years old, with no college degree, and living in urban areas. This sample also excludes public servants and employers with more than five employees.

Table 3: Descriptive Statistics

	2001		2004		2006	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Outcomes						
entrepreneur	0.069	0.254	0.069	0.253	0.072	0.258
entrepreneur - service	0.040	0.197	0.026	0.160	0.028	0.165
entrepreneur - sales	0.022	0.146	0.033	0.177	0.033	0.178
entrepreneur - manufacturing	0.018	0.132	0.020	0.138	0.021	0.143
formal employee	0.431	0.495	0.461	0.498	0.482	0.500
informal employee	0.152	0.359	0.147	0.355	0.140	0.347
informal self-employed	0.206	0.405	0.193	0.394	0.177	0.382
jobless	0.141	0.348	0.130	0.337	0.130	0.336
receiving private transfer	0.043	0.203	0.068	0.252	0.077	0.267
Individual variables						
age	34.3	6.0	34.3	6.0	34.3	6.1
white	0.523	0.499	0.500	0.500	0.475	0.499
black	0.072	0.258	0.075	0.263	0.090	0.287
married	0.725	0.446	0.705	0.456	0.689	0.463
elementary education	0.788	0.409	0.816	0.388	0.838	0.368
primary education	0.445	0.497	0.508	0.500	0.544	0.498
high school	0.247	0.431	0.304	0.460	0.347	0.476
number of kids	1.380	1.280	1.280	1.240	1.210	1.200
number of elderly	0.193	0.493	0.202	0.501	0.209	0.509
migrant - last 5 years	0.057	0.232	0.114	0.318	0.117	0.321
Municipality variables						
program coverage	0.047	0.089	0.150	0.131	0.194	0.155
log of population	12.9	1.38	13.0	1.37	13.0	1.37
poverty headcount	0.257	0.175	0.249	0.170	0.192	0.147
elementary enrollment rate	0.929	0.065	0.939	0.060	0.952	0.049
primary enrollment rate	0.726	0.161	0.775	0.132	0.794	0.123
high school enrollment rate	0.424	0.182	0.504	0.188	0.524	0.170
child mortality	12.7	21.3	11.1	22.4	9.8	17.3
coverage of sewer system	0.483	0.354	0.513	0.363	0.513	0.357
prop. of house owners	0.694	0.107	0.699	0.103	0.695	0.103
Number of observations	41,737		43,183		44,868	

Table 4: Overall Effect of Cash Transfers on Entrepreneurship

	Decision of being a small entrepreneur					
	OLS	FE		IV		
	(1)	(2)	(3)	(4)	(5)	(6)
program coverage, \bar{d}	-0.013*	0.042***	0.040***	0.058***	0.056***	
	(0.008)	(0.013)	(0.013)	(0.022)	(0.021)	
individual benefit, d						0.057**
						(0.024)
age (x10)	0.057***	0.060***	0.063***	0.060***	0.060***	0.056***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.017)
squared age (x100)	-0.002	-0.003	-0.004	-0.003	-0.003	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
white	0.039***	0.032***	0.025***	0.032***	0.032***	0.026***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
black	-0.011***	-0.014***	-0.013***	-0.014***	-0.014***	-0.014***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
married	0.024***	0.025***	0.030***	0.025***	0.025***	0.027***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
elementary education	0.029***	0.027***	0.024***	0.027***	0.027***	0.026***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
primary education	0.028***	0.028***	0.022***	0.028***	0.028***	0.024***
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
high school	0.030***	0.031***	0.020***	0.031***	0.031***	0.021***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
log of population	-0.004***	-0.023	-0.020	-0.025*	-0.021	-0.016
	(0.001)	(0.015)	(0.014)	(0.015)	(0.015)	(0.014)
year = 2001	0.000	0.006*	0.003	0.008*	0.008	0.005
	(0.002)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
year = 2004	-0.002	-0.001	-0.001	0.000	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
poverty headcount					-0.029*	-0.029
					(0.018)	(0.018)
elementary enrollment rate					0.01	0.011
					(0.020)	(0.021)
primary enrollment rate					-0.016	-0.016
					(0.012)	(0.013)
high school enrollment rate					-0.014	-0.014
					(0.012)	(0.011)
child mortality (x1000)					0.019	0.022
					(0.054)	(0.055)
coverage of sewer system					-0.005	-0.007
					(0.012)	(0.013)
prop. of house owners					0.029	0.028
					(0.020)	(0.020)
Municipality Fixed-Effects	No	Yes	No	Yes	Yes	Yes
Census Tract Fixed-Effects	No	No	Yes	No	No	No
Number of observations	129,298	129,298	129,298	129,298	129,298	129,264

***, **, * represent statistical significant at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses are clustered by municipality. Sample includes only men with high school diploma or less. Column (1) presents the regression coefficients obtained by Ordinary Least Squares (OLS). Columns (2) and (3) present the fixed-effect regressions (FE) obtained using the within-group method. Columns (4), (5), and (6) present the fixed-effect, Instrumental-Variable regressions (IV) with ‘program coverage’ and ‘individual benefit’ instrumented by the interactions between poverty headcount in 2001 and year dummies.

Table 5: Overall Effect of Cash Transfers on Different Types of Business

	Decision of being a small entrepreneur in					
	Services		Sales		Manufacturing	
	FE (1)	IV (2)	FE (3)	IV (4)	FE (5)	IV (6)
program coverage, \bar{d}	0.038*** (0.010)	0.053*** (0.017)	0.015** (0.008)	0.019 (0.013)	-0.004 (0.007)	-0.004 (0.011)
age (x10)	0.031*** (0.012)	0.031*** (0.012)	0.023* (0.012)	0.023* (0.012)	0.001 (0.010)	0.001 (0.010)
squared age (x100)	-0.002 (0.002)	-0.002 (0.002)	-0.001 (0.002)	-0.001 (0.002)	0.002 (0.002)	0.002 (0.002)
white	0.016*** (0.002)	0.016*** (0.002)	0.015*** (0.001)	0.015*** (0.001)	0.006*** (0.001)	0.006*** (0.001)
black	-0.006*** (0.002)	-0.006*** (0.002)	-0.005*** (0.002)	-0.005*** (0.002)	-0.005*** (0.001)	-0.005*** (0.001)
married	0.000 (0.001)	0.000 (0.001)	0.012*** (0.001)	0.012*** (0.001)	0.006*** (0.001)	0.006*** (0.001)
elementary education	0.011*** (0.001)	0.011*** (0.001)	0.011*** (0.001)	0.011*** (0.001)	0.008*** (0.001)	0.008*** (0.001)
primary education	0.012*** (0.002)	0.012*** (0.002)	0.015*** (0.002)	0.015*** (0.002)	0.003** (0.002)	0.003** (0.002)
high school	0.022*** (0.002)	0.022*** (0.002)	0.013*** (0.002)	0.013*** (0.002)	-0.002 (0.002)	-0.002 (0.002)
log of population	-0.012 (0.011)	-0.014 (0.011)	-0.016* (0.009)	-0.017* (0.009)	0.003 (0.008)	0.003 (0.008)
year = 2001	0.020*** (0.003)	0.022*** (0.004)	-0.008*** (0.002)	-0.008*** (0.003)	-0.004** (0.002)	-0.004 (0.002)
year = 2004	0.001 (0.001)	0.001 (0.001)	0.001 (0.002)	0.001 (0.002)	-0.002 (0.001)	-0.002 (0.001)
Municipality Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	112,321	112,321	112,321	112,321	112,321	112,321

***, **, * represent statistical significant at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses are clustered by municipality. Sample includes only men with high school diploma or less. FE columns present the fixed-effect regressions obtained using the within-group method. IV columns present the fixed-effect, Instrumental-Variable regressions with ‘program coverage’ instrumented by the interactions between poverty headcount in 2001 and year dummies.

Table 6: Nonlinear, Indirect, and Direct Effects of Cash Transfers on Entrepreneurship

	Decision of being a small entrepreneur				
	All	Non-participants		All sample	
	sample	FE	IV	FE	IV
	(1)	(2)	(3)	(4)	(5)
program coverage, \bar{d}	0.045 (0.028)	0.070*** (0.015)	0.079*** (0.024)	0.070*** (0.015)	0.079*** (0.024)
squared coverage, \bar{d}^2	-0.006 (0.043)				
individual benefit, d				-0.032*** (0.004)	-0.041*** (0.006)
age (x10)	0.060*** (0.016)	0.063*** (0.018)	0.063*** (0.018)	0.064*** (0.016)	0.064*** (0.016)
squared age (x100)	-0.003 (0.002)	-0.003 (0.003)	-0.003 (0.003)	-0.003 (0.002)	-0.003 (0.002)
white	0.032*** (0.002)	0.034*** (0.002)	0.034*** (0.002)	0.031*** (0.002)	0.031*** (0.002)
black	-0.014*** (0.002)	-0.015*** (0.003)	-0.015*** (0.003)	-0.014*** (0.002)	-0.014*** (0.002)
married	0.025*** (0.002)	0.029*** (0.002)	0.029*** (0.002)	0.027*** (0.002)	0.027*** (0.002)
elementary education	0.027*** (0.002)	0.028*** (0.002)	0.028*** (0.002)	0.025*** (0.002)	0.025*** (0.002)
primary education	0.028*** (0.003)	0.028*** (0.003)	0.028*** (0.003)	0.027*** (0.003)	0.027*** (0.003)
high school	0.031*** (0.002)	0.030*** (0.003)	0.030*** (0.003)	0.030*** (0.002)	0.030*** (0.002)
log of population	-0.024 (0.015)	-0.031* (0.017)	-0.032* (0.017)	-0.024 (0.015)	-0.026* (0.015)
year = 2001	0.006 (0.004)	0.004 (0.004)	0.005 (0.005)	0.005 (0.003)	0.008 (0.005)
year = 2004	-0.001 (0.002)	-0.001 (0.002)	-0.001 (0.002)	-0.001 (0.002)	0.000 (0.002)
Municipality Fixed-Effects	Yes	Yes	Yes	Yes	Yes
Number of observations	129,298	113,267	113,267	129,264	129,264

***, **, * represent statistical significant at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses are clustered by municipality. Sample includes only men with high school diploma or less. Column (1) presents the fixed-effect model with quadratic effect of program coverage. Columns (2) and (3) present the estimates of the indirect effect on individuals who do not participate in the program. Columns (4) and (5) present the estimates of the indirect effect (program coverage) and direct effect (individual benefit), with bias correction given by Lemma 4.1. Columns (2) and (4), as well as columns (3) and (5), are jointly estimated using Seemingly Unrelated Regressions (SUR). FE columns show fixed-effect regressions obtained using the within-group method. IV columns show fixed-effect, Instrumental-Variable regressions with ‘program coverage’ instrumented by the interactions between poverty headcount in 2001 and year dummies.

Table 7: Indirect Effect of Cash Transfers on Private Transfers and Entrepreneurship

	Receiving Private Transfers			Entrepreneurial Decision	
	FE (1)	IV (2)	FE (3)	FE (4)	IV (5)
program coverage, \bar{d}	0.127*** (0.019)	0.185*** (0.030)		0.057*** (0.016)	0.068** (0.034)
\bar{d} * effect on transfers				0.463** (0.218)	0.575 (0.399)
\bar{d} * jobless			0.313*** (0.035)		
\bar{d} * informal			0.136*** (0.019)		
\bar{d} * formal			0.052** (0.020)		
\bar{d} * entrepreneur			0.013 (0.029)		
informal			-0.053*** (0.006)		
formal			-0.052*** (0.005)		
entrepreneur			-0.042*** (0.006)		
age (x10)	-0.037** (0.017)	-0.037** (0.017)	-0.022 (0.017)	0.072*** (0.018)	0.072*** (0.018)
squared age (x100)	0.005** (0.002)	0.005** (0.002)	0.003 (0.002)	-0.004 (0.003)	-0.004 (0.003)
white	0.003 (0.002)	0.003 (0.002)	0.003 (0.002)	0.034*** (0.002)	0.034*** (0.002)
black	0.000 (0.003)	0.000 (0.003)	-0.001 (0.003)	-0.015*** (0.003)	-0.015*** (0.003)
married	-0.025*** (0.002)	-0.025*** (0.002)	-0.014*** (0.002)	0.028*** (0.002)	0.028*** (0.002)
elementary education	-0.019*** (0.003)	-0.019*** (0.003)	-0.014*** (0.003)	0.028*** (0.002)	0.028*** (0.002)
primary education	-0.002 (0.002)	-0.002 (0.002)	0.000 (0.002)	0.028*** (0.003)	0.028*** (0.003)
high school	0.007*** (0.002)	0.008*** (0.002)	0.010*** (0.002)	0.030*** (0.003)	0.030*** (0.003)
number of kids	0.007*** (0.001)	0.007*** (0.001)	0.006*** (0.001)	-0.002** (0.001)	-0.002** (0.001)
number of elderly	0.022*** (0.003)	0.022*** (0.003)	0.016*** (0.003)	-0.006*** (0.002)	-0.006*** (0.002)
log of population	0.010 (0.018)	0.004 (0.019)	0.014 (0.018)	-0.027 (0.017)	-0.028 (0.017)
year = 2001	-0.021*** (0.004)	-0.013*** (0.005)	-0.023*** (0.004)	0.003 (0.004)	0.005 (0.005)
year = 2004	-0.005* (0.003)	-0.003 (0.003)	-0.006** (0.003)	-0.001 (0.002)	-0.001 (0.002)
Municipality Fixed-Effects	Yes	Yes	Yes	Yes	Yes
Number of observations	113,115	113,115	113,115	113,233	113,233

***, **, * represent statistical significant at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses are clustered by municipality. Sample includes only men with high school diploma or less. In columns (1), (2), and (3), the outcome is the probability of receiving private transfers. In columns (4) and (5), the outcome is the probability of being an entrepreneur. ‘Effect on transfers’ is calculated by a regression of private transfers on program coverage interacting with 2001 municipality characteristics. The coefficient of interaction between ‘effect on transfers’ and ‘program coverage’ represents how much the effect of program coverage on entrepreneurial decision changes if its predicted effect on private transfers increases. FE columns show fixed-effect regressions obtained using the within-group method. IV columns show fixed-effect, Instrumental-Variable regressions with ‘program coverage’ instrumented by the interactions between poverty headcount in 2001 and year dummies.

Table 8: Indirect Effect of Cash Transfers on Other Entrepreneurs, Employment and Wages

	High-Educated Entrep. Decision		Low-Educated Employment		Low-Educated Employees' Wages	
	FE	IV	FE	IV	FE	IV
	(1)	(2)	(3)	(4)	(5)	(6)
program coverage, \bar{d}	-0.014 (0.141)	0.181 (0.224)	0.004 (0.021)	-0.003 (0.034)	0.202 (0.889)	0.467 (1.117)
\bar{d} * private					0.050 (0.886)	-0.028 (1.109)
private					-0.385* (0.222)	-0.361 (0.279)
age (x10)	0.355*** (0.102)	0.357*** (0.102)	0.149*** (0.022)	0.149*** (0.022)	0.517*** (0.052)	0.517*** (0.051)
squared age (x100)	-0.035** (0.015)	-0.035** (0.015)	-0.023*** (0.003)	-0.023*** (0.003)	-0.045*** (0.007)	-0.045*** (0.007)
white	0.039*** (0.013)	0.039*** (0.013)	-0.001 (0.003)	-0.001 (0.003)	0.116*** (0.008)	0.116*** (0.008)
black	-0.075** (0.032)	-0.075** (0.032)	-0.007 (0.005)	-0.007 (0.005)	-0.025*** (0.009)	-0.025*** (0.009)
married	0.012 (0.011)	0.011 (0.011)	0.167*** (0.003)	0.167*** (0.003)	0.157*** (0.006)	0.157*** (0.006)
elementary education			0.050*** (0.005)	0.050*** (0.005)	0.162*** (0.009)	0.161*** (0.009)
primary education			0.018*** (0.003)	0.018*** (0.003)	0.187*** (0.008)	0.187*** (0.008)
high school			0.028*** (0.003)	0.028*** (0.003)	0.371*** (0.014)	0.371*** (0.014)
log of population	-0.039 (0.110)	-0.055 (0.111)	0.012 (0.021)	0.012 (0.021)	0.075 (0.048)	0.056 (0.048)
year = 2001	0.012 (0.019)	0.026 (0.025)	-0.013*** (0.005)	-0.013** (0.006)	0.061*** (0.020)	0.081*** (0.022)
year = 2004	0.006 (0.013)	0.010 (0.014)	-0.002 (0.004)	-0.002 (0.005)	-0.063*** (0.009)	-0.056*** (0.010)
Municipality Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	9,359	9,229	113,233	113,233	58,282	58,275

***, **, * represent statistical significant at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses are clustered by municipality. In columns (1) and (2), the outcome is the probability of being an entrepreneur and the sample only includes individuals with college degree or equivalent. In columns (3) and (4), the outcome is the probability of low-educated individuals, excluding public servants, being employed in either the formal sector or the informal sector. In columns (5) and (6), the outcome is the log of earnings per hour in the main occupation and the sample only includes low-educated workers formally employed in either private sector or public sector. FE columns show fixed-effect regressions obtained using the within-group method. IV columns show fixed-effect, Instrumental-Variable regressions with 'program coverage' instrumented by the interactions between poverty headcount in 2001 and year dummies.

Table 9: Indirect and Direct Effects of Cash Transfers on Other Occupational Choices

	Fixed-Effect Model				
	Entrep.	Jobless	Formal employee	Informal employee	Informal self-emp.
program coverage, \bar{d}	0.070*** (0.015)	-0.004 (0.021)	0.020 (0.027)	-0.066*** (0.023)	-0.020 (0.027)
individual benefit, d	-0.032*** (0.004)	0.029*** (0.009)	-0.056*** (0.012)	0.029*** (0.010)	0.030*** (0.012)
Municipality Fixed-Effects	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes	Yes
N. of obs. - all sample	129,264	129,264	129,264	129,264	129,264
N. of obs. - $d = 0$	113,267	113,267	113,267	113,267	113,267
	Instrumental Variable Model				
	Entrep.	Jobless	Formal employee	Informal employee	Informal self-emp.
program coverage, \bar{d}	0.079*** (0.024)	0.002 (0.034)	-0.001 (0.040)	-0.092*** (0.034)	0.011 (0.039)
individual benefit, d	-0.041*** (0.006)	0.041*** (0.014)	-0.050*** (0.016)	0.004*** (0.016)	0.046 (0.017)
Municipality Fixed-Effects	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes	Yes
N. of obs. - all sample	129,264	129,264	129,264	129,264	129,264
N. of obs. - $d = 0$	113,267	113,267	113,267	113,267	113,267

***, **, * represent statistical significant at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses are clustered by municipality. Sample includes only men with high school diploma or less. All coefficients are estimated using Seemingly Unrelated Regressions (SUR). The indirect effect (program coverage) is estimated using the sample of non-participants, whereas the direct effect (individual benefit) is estimated using all sample and bias corrected according to Lemma 4.1. Fixed-Effect models are estimated using the within-group method. In the Instrumental-Variable models, ‘program coverage’ is instrumented by the interactions between poverty headcount in 2001 and year dummies.

Table 10: Overall Effect of Cash Transfers on Entrepreneurship, Robustness Analyses

	Decision of being a small entrepreneur				
	FE	2001-2004		excluding Northeast	
		FE	IV	FE	IV
	(1)	(2)	(3)	(4)	(5)
program coverage, \bar{d}	0.036** (0.015)	0.040** (0.018)	0.062* (0.032)	0.055*** (0.019)	0.083** (0.033)
poverty	-0.026 (0.022)				
poverty * year = 2001 ^(a)	-0.004 (0.015)				
poverty * year = 2004 ^(b)	0.004 (0.011)				
age (x10)	0.060*** (0.016)	0.052** (0.020)	0.052** (0.020)	0.071*** (0.020)	0.071*** (0.020)
squared age (x100)	-0.003 (0.002)	-0.002 (0.003)	-0.002 (0.003)	-0.004 (0.003)	-0.004 (0.003)
white	0.032*** (0.002)	0.032*** (0.002)	0.032*** (0.002)	0.037*** (0.002)	0.037*** (0.002)
black	-0.014*** (0.002)	-0.016*** (0.003)	-0.016*** (0.003)	-0.013*** (0.003)	-0.014*** (0.003)
married	0.025*** (0.002)	0.026*** (0.002)	0.026*** (0.002)	0.027*** (0.002)	0.027*** (0.002)
elementary education	0.027*** (0.002)	0.026*** (0.002)	0.026*** (0.002)	0.030*** (0.002)	0.030*** (0.002)
primary education	0.028*** (0.003)	0.028*** (0.003)	0.028*** (0.003)	0.030*** (0.003)	0.030*** (0.003)
high school	0.031*** (0.002)	0.038*** (0.003)	0.038*** (0.003)	0.033*** (0.003)	0.033*** (0.003)
log of population	-0.020 (0.015)	-0.007 (0.022)	-0.008 (0.022)	-0.040** (0.018)	-0.044** (0.019)
year = 2001	0.008* (0.005)	0.007* (0.004)	0.010* (0.005)	0.007 (0.004)	0.009* (0.006)
year = 2004	0.000 (0.003)			-0.001 (0.002)	0.000 (0.002)
test (a) = (b) = 0, p-value	0.820				
Municipality Fixed-Effects	Yes	Yes	Yes	Yes	Yes
Number of observations	129,298	84,543	84,543	91,656	91,656

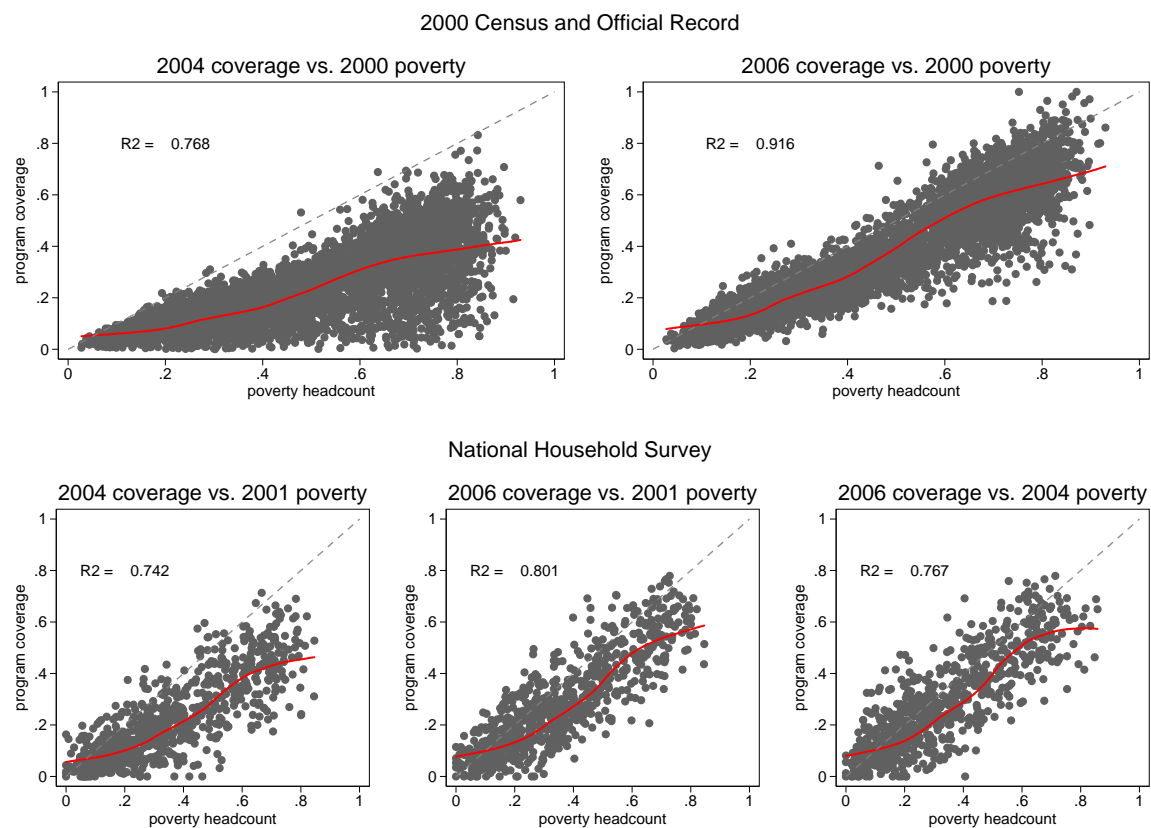
***, **, * represent statistical significant at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses are clustered by municipality. Sample includes only men with high school diploma or less. Column (1) presents the estimate of the overall effect on entrepreneurship controlling for a time-varying relationship with poverty. Columns (2) and (3) present the estimates of the overall effect in the 2001-2004 period (excluding 2006). Columns (4) and (5) present the estimates of the overall effect in regions other than the Northeast. FE columns show fixed-effect regressions obtained using the within-group method. IV columns show fixed-effect, Instrumental-Variable regressions with 'program coverage' instrumented by the interactions between poverty headcount in 2001 and year dummies.

Table 11: Overall Effect of Cash Transfers on Migration

	Migration	
	FE (1)	IV (2)
program coverage, \bar{d}	0.014 (0.023)	-0.030 (0.043)
age (x10)	-0.067*** (0.023)	-0.067*** (0.023)
squared age (x100)	0.005 (0.003)	0.005 (0.003)
white	0.004* (0.003)	0.004* (0.003)
black	0.003 (0.004)	0.003 (0.004)
married	0.021*** (0.003)	0.021*** (0.003)
elementary education	-0.004 (0.004)	-0.004 (0.004)
primary education	0.001 (0.002)	0.001 (0.002)
high school	0.006* (0.003)	0.006* (0.003)
year = 2001	-0.057*** (0.006)	-0.063*** (0.009)
year = 2004	-0.002 (0.003)	-0.003 (0.003)
Municipality Fixed-Effects	Yes	Yes
Number of observations	129,298	129,298

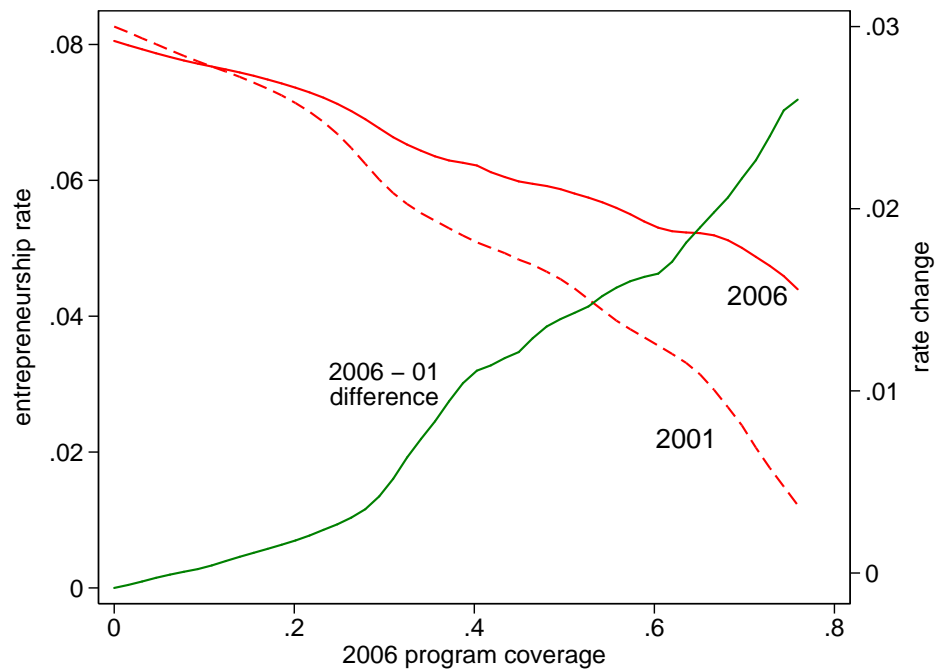
***, **, * represent statistical significant at the 1%, 5%, and 10% levels, respectively. Sample includes only men with high school diploma or less. Standard errors in parentheses are clustered by municipality. Columns (1) and (2) present the estimates of the overall effect on the probability of living in the same municipality for less than five years. FE column shows the fixed-effect regression obtained using the within-group method. IV column shows fixed-effect, Instrumental-Variable regression with ‘program coverage’ instrumented by the interactions between poverty headcount in 2001 and year dummies.

Figure 1: Relationship between Program Coverage and Poverty Headcount



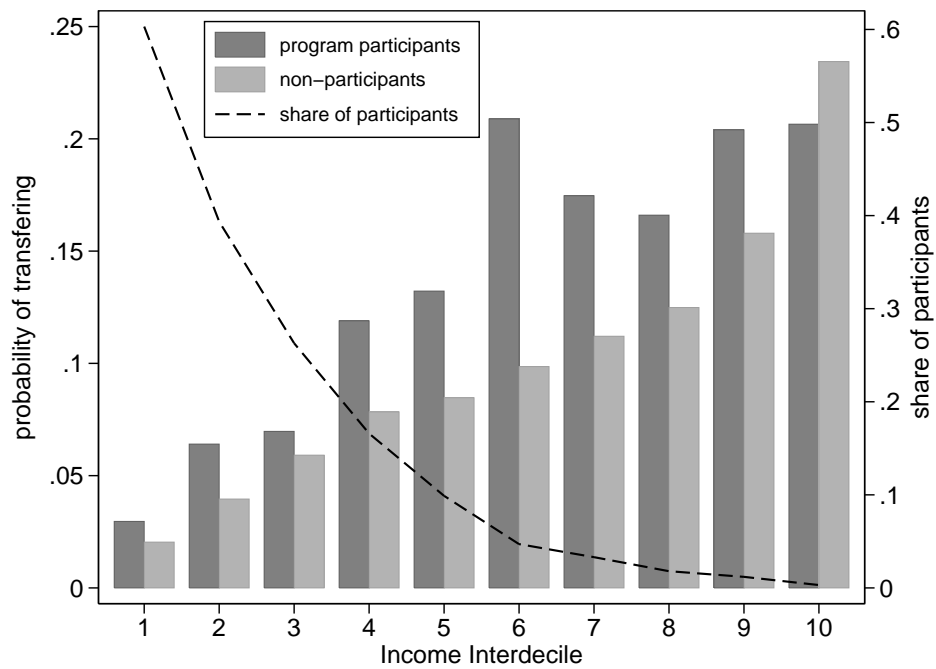
Program coverage is measured by the proportion of households participating in the program. Poverty headcount is measured by the proportion of households with per capita income below the poverty line (1/2 of the 2001 minimum wage). Each point in these graphs represents a municipality. Regressions are weighted by the number of households per municipality.

Figure 2: Relationship between Entrepreneurship and Program Coverage



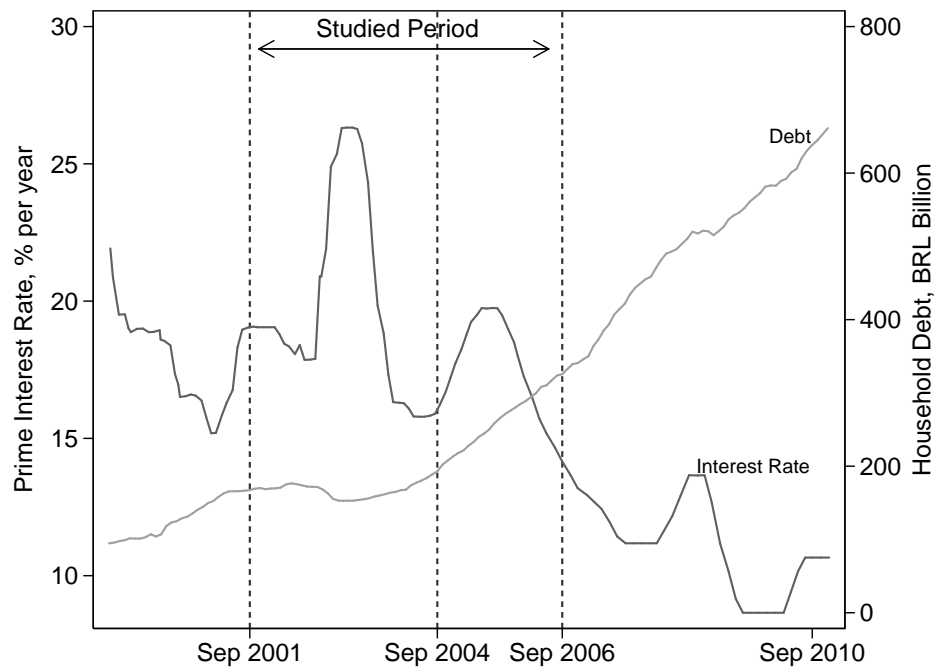
Entrepreneurship rate is measured by the proportion of small entrepreneurs per municipality. '2006 - 01 difference' is the difference between entrepreneurship rates in 2006 and 2001 per municipality. Program coverage is measured by the proportion of individuals participating in the program in 2006. Municipalities where the program coverage was greater than 5 p.p. in 2001 are not included.

Figure 3: Probability of Transferring or Lending Money to Another Household



Estimates made using the Brazilian Consumer Expenditure Survey (POF) 2008-2009. The bars represent the proportion of households that have transferred or lent money to another household in the last 90 days. The dashed line represents the proportion of households receiving a conditional cash transfer. Income deciles are calculated using household per capita income.

Figure 4: Household Debt Outstanding and Interest Rate in Brazil



Source: Central Bank of Brazil. Debt series is deflated by the National Consumer Price Index (INPC).

Table A1: Indirect and Direct Effects on Entrepreneurship, With and Without Children

	Decision of being a small entrepreneur			
	Without children		With children	
	FE	IV	FE	IV
	(1)	(2)	(3)	(4)
program coverage, \bar{d}	0.042** (0.018)	0.044 (0.031)	0.104*** (0.023)	0.118*** (0.032)
individual benefit, d	-0.015*** (0.006)	-0.017** (0.007)	-0.031*** (0.009)	-0.044*** (0.013)
age (x10)	0.071** (0.029)	0.071** (0.029)	0.062** (0.028)	0.062** (0.028)
squared age (x100)	-0.004 (0.004)	-0.004 (0.004)	-0.002 (0.004)	-0.002 (0.004)
white	0.031*** (0.002)	0.031*** (0.002)	0.037*** (0.003)	0.037*** (0.003)
black	-0.010** (0.004)	-0.010** (0.004)	-0.021*** (0.004)	-0.021*** (0.004)
married	0.027*** (0.002)	0.027*** (0.002)	0.029*** (0.003)	0.029*** (0.003)
elementary education	0.027*** (0.003)	0.027*** (0.003)	0.028*** (0.003)	0.028*** (0.003)
primary education	0.027*** (0.003)	0.027*** (0.003)	0.028*** (0.004)	0.028*** (0.004)
high school	0.029*** (0.003)	0.029*** (0.003)	0.031*** (0.004)	0.031*** (0.004)
log of population	-0.002 (0.022)	-0.002 (0.023)	-0.069*** (0.023)	-0.071*** (0.023)
year = 2001	0.005 (0.004)	0.005 (0.006)	0.002 (0.005)	0.003 (0.007)
year = 2004	-0.002 (0.003)	-0.001 (0.003)	-0.002 (0.004)	-0.001 (0.004)
Municipality Fixed-Effects	Yes	Yes	Yes	Yes
Number of observations	63,459	63,459	65,805	65,805

***, **, * represent statistical significant at the 1%, 5%, and 10% levels, respectively. Sample includes only men with high school diploma or less. Standard errors in parentheses are clustered by municipality. All coefficients are estimated using Seemingly Unrelated Regressions (SUR). The indirect effect (program coverage) is estimated using the sample of non-participants, whereas the direct effect (individual benefit) is estimated using all sample and bias corrected according to Lemma 4.1. Columns (1) and (2) present the estimates of effects on individuals without children in their household. Columns (3) and (4) present the estimates of effects on individuals living with children under 15 years old. FE column shows the fixed-effect regression obtained using the within-group method. IV column shows fixed-effect, Instrumental-Variable regression with ‘program coverage’ instrumented by the interactions between poverty headcount in 2001 and year dummies.

Table A2: Indirect and Direct Effects on Occupational Choices, With and Without High School

Panel A: Individuals without High-School Diploma					
	Fixed-Effect Model				
	Entrep.	Jobless	Formal employee	Informal employee	Informal self-emp.
program coverage, \bar{d}	0.056*** (0.015)	-0.014 (0.024)	0.046 (0.031)	-0.062** (0.029)	-0.026 (0.033)
individual benefit, d	-0.033*** (0.005)	0.029*** (0.011)	-0.056*** (0.014)	0.033** (0.013)	0.028* (0.015)
Municipality Fixed-Effects	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes	Yes
N. of obs. - all sample	90,825	90,825	90,825	90,825	90,825
N. of obs. - $d = 0$	76,709	76,709	76,709	76,709	76,709
	Instrumental Variable Model				
	Entrep.	Jobless	Formal employee	Informal employee	Informal self-emp.
program coverage, \bar{d}	0.061*** (0.022)	-0.004 (0.039)	0.008 (0.047)	-0.099** (0.041)	0.033 (0.047)
individual benefit, d	-0.041*** (0.007)	0.050*** (0.017)	-0.069*** (0.019)	0.005 (0.020)	0.055** (0.022)
Municipality Fixed-Effects	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes	Yes
N. of obs. - all sample	90,825	90,825	90,825	90,825	90,825
N. of obs. - $d = 0$	76,709	76,709	76,709	76,709	76,709
Panel B: Individuals with High-School Diploma					
	Fixed-Effect Model				
	Entrep.	Jobless	Formal employee	Informal employee	Informal self-emp.
program coverage, \bar{d}	0.115*** (0.039)	0.013 (0.037)	-0.099* (0.059)	-0.026 (0.033)	-0.002 (0.037)
individual benefit, d	-0.034*** (0.010)	0.039*** (0.014)	-0.070*** (0.020)	0.038*** (0.013)	0.027* (0.014)
Municipality Fixed-Effects	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes	Yes
N. of obs. - all sample	38,439	38,439	38,439	38,439	38,439
N. of obs. - $d = 0$	36,558	36,558	36,558	36,558	36,558
	Instrumental Variable Model				
	Entrep.	Jobless	Formal employee	Informal employee	Informal self-emp.
program coverage, \bar{d}	0.138** (0.064)	0.004 (0.057)	-0.133 (0.100)	0.007 (0.053)	-0.016 (0.063)
individual benefit, d	-0.044*** (0.014)	0.021 (0.020)	-0.027 (0.028)	0.016 (0.020)	0.035* (0.020)
Municipality Fixed-Effects	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes	Yes
N. of obs. - all sample	38,439	38,439	38,439	38,439	38,439
N. of obs. - $d = 0$	36,558	36,558	36,558	36,558	36,558

***, **, * represent statistical significant at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses are clustered by municipality. All coefficients are estimated using Seemingly Unrelated Regressions (SUR). The indirect effect (program coverage) is estimated using the sample of non-participants, whereas the direct effect (individual benefit) is estimated using all sample and bias corrected according to Lemma 4.1. Fixed-Effect models are estimated using the within-group method. In the Instrumental-Variable models, ‘program coverage’ is instrumented by the interactions between poverty headcount in 2001 and year dummies.