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LIQUIDITY CONSTRAINTS, INFORMAL FINANCING, AND ENTREPRENEURSHIP: DIRECT AND INDIRECT EFFECTS OF A CASH TRANSFER PROGRAMME*

Rafael P. Ribas

ABSTRACT

This paper exploits a liquidity shock from a large-scale welfare programme 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' behaviour 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 programme, which assists 20 per cent of Brazilian households, has increased the number of small entrepreneurs by 10 per cent. 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 tends to be more responsive to the opportunity cost of mutual assistance between households than to financial constraints.

JEL Classification: C21, H31, I38, J24, L26.

Keywords: Entrepreneurship, Financial Constraints, Informal Financing, Risk-Sharing, Cash Transfer, Indirect Effect.

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1 INTRODUCTION

There has been a long debate over whether insufficient liquidity hinders 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 start-up costs and investment risks (Ghatak, Morelli and Sjostrom, 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 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, Fiala and Martinez, 2013), liquidity is not delivered uniquely to entrepreneurs. The studied intervention is a large-scale conditional cash transfer (CCT) programme in Brazil, called *Bolsa Família*. This programme 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 rules regarding business investment, adult labour supply or repayment.

If potential entrepreneurs face credit and insurance constraints, the individual liquidity shock may change the occupational choice and investment decisions of programme 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 programmes have on the whole community.[3] For instance, Angelucci and De Giorgi (2009) find that non-poor households are also affected by PROGRESA/*Opportunidades* in rural villages in Mexico. They suggest that these households increase food consumption by receiving private transfers from programme 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 programme 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 programme, 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 CCTs and microfinance. On the 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.

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, McKenzie and Woodruff, 2008; Gertler, Martinez and Rubio-Codina, 2012), and the extensive margin, encouraging participants to start their own business (Bianchi and Bobba, 2013; Bandiera et al., 2013; Blattman, Fiala and Martinez, 2013). In some of these studies, however, the randomisation 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 randomised 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 programmes once they reach urban centres 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 programme that has been introduced not only in rural and isolated areas but also in large cities in Brazil. In 2006 the programme had already covered about 20 per cent of Brazilian households, and 70 per cent 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 per cent more than formal employees per hour, the informal self-employed earn 30 per cent 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 programme 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 programme 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 programme 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 programme coverage on non-participants and test whether this effect is equal to the indirect impact on participants. If this hypothesis is not rejected, estimating the

direct response does not require individual-level randomisation of the treatment, unlike Duflo and Saez (2003). Then the estimated indirect effect is used to calculate the selection bias of the estimated direct effect. In summary, this empirical strategy allows me to ignore individual selection issues based on verifiable assumptions and decompose the overall effect of the programme 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 a higher self-employment rate than other poor households, while Brauw et al. (2012) suggest that the programme has increased participation in the informal market. Neither of them accounts 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 identify the causal parameter by comparing municipalities over time, but they find no significant overall effect on participation in the labour force.[6]

My findings suggest that the proportion of entrepreneurs among low-educated men has grown by 10 per cent because of the *Bolsa Família* programme. 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 opposite directions. While the rise in entrepreneurial activity is entirely driven by spillovers, the direct response of participants reduces the overall effect by 40 per cent. 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 programme participants as moneylenders corroborates the existence of informal risk-sharing arrangements. Thus, rather than lessening individual credit and insurance constraints, the cash transfer tends 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 programme has given the financial opportunity to underemployed workers to open their own business. The programme, 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 organised 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.

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; Karlan et al., 2014). 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 neighbours, friends and relatives might be a way in which small entrepreneurs cope with start-up costs and business risks. Although empirical studies suggest that informal risk-sharing mechanisms do not fully compensate market failures (Townsend, 1994; Hayashi, Altonji and Kotlikoff, 1996; Ravallion and Chaudhuri, 1997),[7] efficiency is often achieved within social networks (Fafchamps, 2000; Fafchamps and Lund, 2003; De Weerdt and Dercon, 2006). According to Bloch, Genicot and Ray (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 the direct effect, which lessens financial constraints, and the size of the indirect effect, which reduces the costs of informal credit and insurance.

2.1 SET-UP

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

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

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

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 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 formalisation for the self-employed, and δ is what

they receive from social security (Straub, 2005). In summary, the 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 pay-off of some individuals.[8] 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 to their effect on the trade-off, $D(\hat{q})$.[9] 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 maximisation problem with no general equilibrium.

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):

$$\begin{aligned} CE &\equiv \frac{\partial y}{\partial d_1} \\ &\propto u'[a + d_1 - k - s_M^*(\hat{q})] - u'[a + d_1 - s_L^*] > 0, \end{aligned} \tag{0.1}$$

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):

$$\begin{aligned} IE &\equiv \left.\frac{\partial y}{\partial d_2}\right|_{\zeta=0} \\ &\propto \lambda u'[\hat{q} + d_2 + s_M^*(\hat{q})] + (1-\lambda)u'[\delta + d_2 + s_M^*(\hat{q})] - u'[w + d_2 + s_L^*] \\ &\geq CE \quad \text{if } s_M^*(\hat{q}) > 0. \end{aligned} \tag{0.2}$$

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 other hand, it increases the opportunity cost of self-employment, which reduces the chances of receiving d_2. This negative response is defined as the eligibility effect (EE):

$$\begin{aligned} EE &\equiv \left.\frac{\partial y}{\partial d_2}\right|_{\zeta=1} - \left.\frac{\partial y}{\partial d_2}\right|_{\zeta=0} \\ &\propto -\lambda u'[\hat{q} + d_2 + s_M^*(\hat{q})] < 0 \end{aligned} \tag{0.3}$$

Depending on how high the probability of business success, λ, is, the eligibility effect can prevail over the insurance and credit effects—i.e. $CE + IE + EE < 0$. Therefore, despite their preferences for work and leisure, individuals at the margin of indifference might prefer to continue 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.[10] If the investment made by entrepreneurs is not successful, then they receive the insurance 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. Thus the direct effect of cash transfers on the occupational choice depends only on the eligibility rule. If the 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, raises the opportunity cost of self-employment (EE).

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 (negative) eligibility effect, EE:

$$GE \equiv \frac{\mathrm{d}y^*}{\mathrm{d}d_1} + \frac{\mathrm{d}y^*}{\mathrm{d}d_2} - EE > 0. \tag{0.4}$$

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 labelled 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 have at least as much chance to be an entrepreneur as those who are not. The reason for this is that 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:[11]

$$\Delta y \approx (\beta_1 + \beta_2)\, \Delta d, \tag{0.5}$$

Where

$$\beta_1 \equiv \left(1 - \sum_{j=1}^{N} \alpha_j\right)[CE(\hat{q}_0) + IE(\hat{q}_0) + EE(\hat{q}_0)] + \sum_{j=1}^{N} \alpha_j\, EE\left(\hat{q}_j\right) \tag{0.6}$$

is the direct effect, and

$$\beta_2 \equiv \sum_{j=1}^{N} \alpha_j\, GE\left(\hat{q}_j\right) \tag{0.7}$$

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. Regardless of 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 $\hat{q}_0 \geq \hat{q}_j$ and then $EE(\hat{q}_0) \leq EE\left(\hat{q}_j\right) < 0$ 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 impact. 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 noting that the existence of these arrangements is just one of many reasons for cash transfers to have an indirect effect on entrepreneurship.

3 PROGRAMME AND DATA DESCRIPTION

In this section, I outline the main characteristics of the *Bolsa Família* programme, as well as the panel data used in my analysis. Most important, I describe how the growth of this programme 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 (*Pesquisa Nacional por Amostra de Domicílios*—PNAD) may be used in a panel setting even though it is a rotating cross-sectional survey.

3.1 THE BOLSA FAMÍLIA PROGRAMME

In Brazil, the first CCT programmes managed by the federal government were created in 2001. The first, called *Bolsa Escola*, was conditional on poor children between six and 15 years being enrolled in and regularly attending primary school. Another programme, called *Bolsa Alimentação*, was intended to improve the health care and nutrition of children up to six years and pregnant women. In 2003 the government created the *Bolsa Família* programme, merging all these previous programmes into one with the standardisation of eligibility criteria, benefit values, information systems and executing agency. The programme 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 PNAD in 2001.

In 2006, extremely poor families with no children, whose per capita monthly income was below USD38, and poor families with children up to 15 years old or pregnant women, whose per capita monthly income was below USD76, were eligible for the programme. The monthly benefit was composed of two parts: USD38 for extremely poor families regardless of the number of children, and USD11 per child, up to three children, for poor families. Thus an extremely poor family should receive a benefit between USD38 and USD72, whereas a moderately poor family should receive between USD11 and USD34.[12] Like *Bolsa Escola* and *Bolsa Alimentação*, this benefit requires household commitment in terms of children's education and health care. However, if the family is registered as extremely poor with no children, the USD38 transfer is considered unconditional.

Families that receive the benefit can be dropped from the programme 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 on 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 government databases, such as the salaries of registered workers from the Ministry of Labour and Employment and the value of pensions and contributions from the Ministry of Social Security.

For instance, the government found that 622,476 participant households had earnings above the eligibility cut-off between October 2008 and February 2009. From this total, 451,021 households had their benefit cancelled. From cross-checking its databases, the government had cancelled the benefit of more than 1 million households between 2004 and 2008, which represents about 40 per cent of the total number of withdrawals.

3.2 PROGRAMME TARGETING

To identify poor families around the country, local governments (municipalities) are free to decide about the priority areas and how the registration process takes place. However, they do receive some guidelines, in the form of quotas on the number of benefits. This cap on benefits is intended to prevent local governments from spending the federal transfers irresponsibly and 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 programme size cannot grow for electoral purposes, de Janvry, Finan and Sadoulet (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.

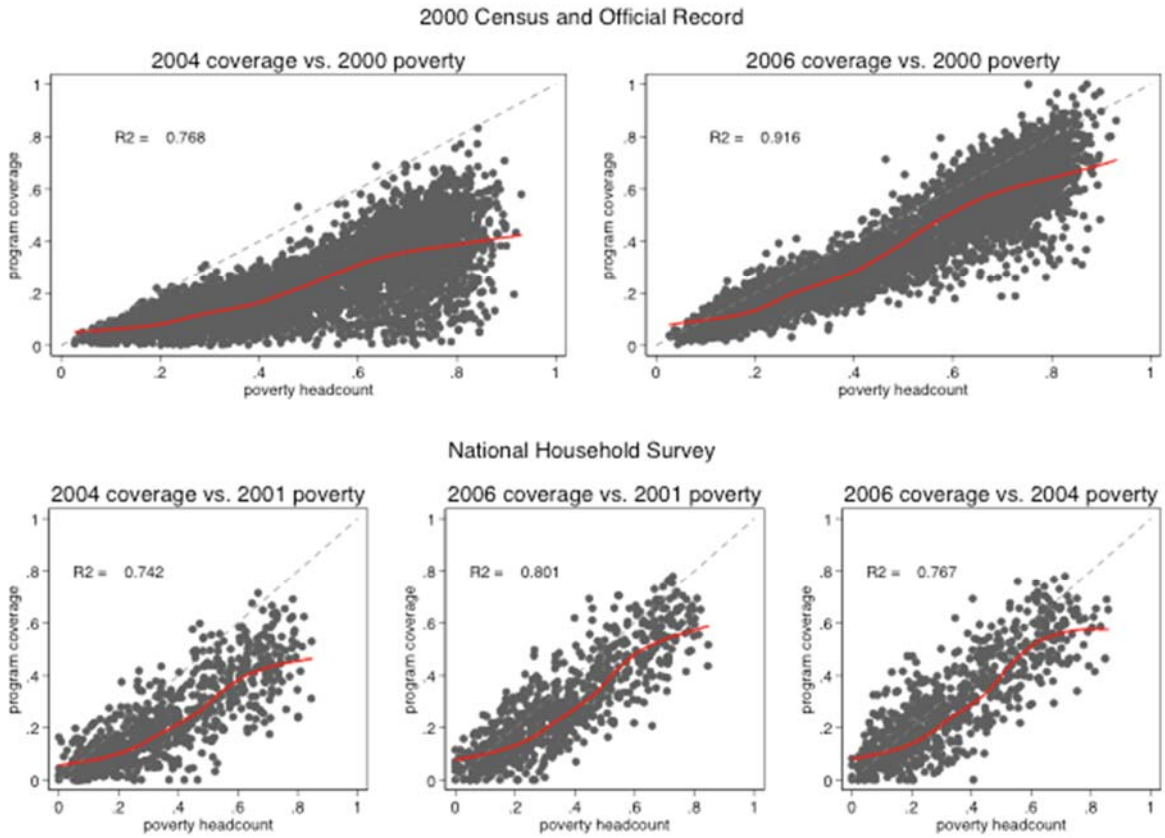
The municipal quotas were initially defined by a poverty map made by the National Statistics Office (*Instituto Brasileiro de Geografia and Estatística*—IBGE). This map was made using both the 2001 PNAD and the 2000 Demographic Census and was used for the quotas until 2006, when it started being updated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

Although the local government has the responsibility of registering poor families in the *Cadastro Único*, this registration does not mean automatic selection for the programme. Registered families still have to prove that 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 the number of children.

Figure 1 confirms that the number of benefits per municipality depended heavily 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 programme (programme coverage) in 2004 and 2006, according to the official records. The initial poverty headcount explains 77 per cent of municipal coverage in 2004, when the programme was still expanding and had not reached the cap in most municipalities. In 2006, when the programme reached its target, the relationship became even stronger and closer to the 45-degree line.

FIGURE 1

Relationship between Programme Coverage and Poverty Headcount



Programme coverage is measured by the proportion of households participating in the programme. Poverty headcount is measured by the proportion of households with per capita income below the poverty line (half of the 2001 minimum wage). Each point represents a municipality. Regressions are weighted by the number per municipality.

The bottom of Figure 1 shows the relationship between poverty headcount in 2001 and programme coverage in 2004 and 2006, calculated with the data 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 programme is naturally more concentrated where poverty is higher. However, the last graph on the bottom right shows that the programme size in 2006 is not as strongly correlated to poverty in 2004 as it is in 2001. A Shapley decomposition confirms that controlling for the current level of poverty, the 2001 poverty headcount accounts for at least 50 per cent of the R^2 in 2004 and 2006.[13] Therefore, it is reasonable to assume that the growth of the *Bolsa Família* programme in this period depended heavily on the previously estimated poverty headcount for each municipality. Moreover, Table D.1 in the Appendix shows that individual characteristics and several social outcomes are balanced across municipalities once we control for the poverty rate.

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 poor households live in urban settlements. As a result, about 70 per cent of transfers go

to urban households. Since the labour market and job opportunities differ between urban and rural areas, the impacts of *Bolsa Família* on labour supply and occupational choice are expected to be distinct from those found for other programmes concentrated in rural villages.[14]

TABLE 1

Poverty Headcount and Programme 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
Programme 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 observations	368,605	316,793	51,812	378,658	326,322	52,336	389,807	336,502	53,305

Estimates are obtained using the PNAD. 'Poverty headcount' is measured by the proportion of people with household per capita income below the poverty line (half of the 2001 minimum wage). 'Programme coverage' is measured by the proportion of people participating in the programme. '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 CCT programmes in the country.

3.3 DATA

3.3.1 Panel Sample and Variables

All the data come from the 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 programmes in 2004 and 2006. This questionnaire asked whether any member of the household was a beneficiary of each cash transfer programme that was in place at the time of the survey. Henceforth, I consider as *Bolsa Família* all previous programmes 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* programme had not yet taken place, and the other cash transfer programmes were not a significant size. However, I have to control for the small coverage of other programmes that might affect the baseline outcomes. Accordingly, I identify those households receiving cash transfers from other social programmes 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 programme.

The PNAD is a cross-sectional survey, so it does not interview the same household twice. 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.[15] 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 with age, but the probability of starting a new business is decreasing after 30 years of age (Ardagna and Lusardi, 2010). Moreover, the desire for being self-employed is decreasing with age (Blanchflower, Oswald and Stutzer, 2001).

I also exclude public servants, people with higher education, and employers with more than five employees from the sample. Even though 6 per cent of public servants were participating in the programme in 2006, they are less likely to change occupation due to the stability of their job. The last two groups were excluded because only 1 per cent of them were receiving the benefit in 2006, so they are practically ineligible for the transfer. In addition, businesses with more than five employees might already be well established, so they are less sensitive at the extensive margin.[16] 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. The survey interviews about 130 households and 50 prime-age men on average per municipality 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 **programme 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

Number of Observations per Municipality

	Mean	Std. Dev.	Min.	Max.	Number of municipalities
2001					
Number of households	128.1	290.4	19	3505	796
Sample size	52.4	128.1	5	1571	796
2004					
Number of households	136.8	305.1	23	3575	796
Sample size	54.3	131.8	5	1751	796
2006					
Number of households	143.8	322.7	28	3884	796
Sample size	56.4	136.1	5	1753	796

The sample comprises 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.

According to Blanchflower (2000) and Blanchflower, Oswald and Stutzer (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.

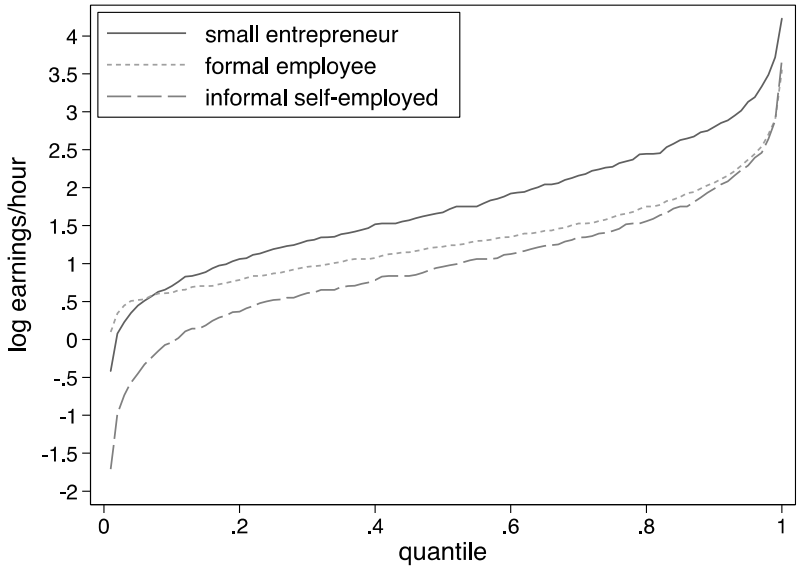
However, to distinguish between entrepreneurial activity and informality, the definition also requires that they either perform a high-skilled job or contribute to social security. Namely, entrepreneurs are more likely to pay taxes and are less vulnerable than informal workers in general (La Porta and Shleifer, 2014). Furthermore, the government cannot track earnings of workers in the informal sector, whereas entrepreneurs have their earnings partially revealed in 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 is also sub-divided into **service**, **sales** and **manufacturing**, based on the type of business.

Based on these categories, entrepreneurs earn on average 77 per cent more per hour worked than the informal self-employed, and 45 per cent more than formal employees. These earnings differentials are also identified at any quantile (see Figure 2), confirmingthat entrepreneurship is more rewarding than other types of occupation.

FIGURE 2

Quantiles of Log Earnings per Hour Worked



'Small entrepreneurs' are those who are self-employed in a high-skilled job, contribute to social security and/or have more than two employees. 'Formal employees' are employed with documentation and/or contribute to social security. The 'informal self-employed' perform a low-skilled activity and do not contribute to social security. Earnings comprise the net gain of the self-employed and salary and bonuses of employees.

3.3.2 Descriptive Statistics

Table 3 shows the descriptive statistics of outcomes and control variables. From 2001 to 2006, the entrepreneurship rate increased by 0.3 percentage points (p.p.), from 6.9 per cent to 7.2 per cent. Moreover, the type of business changed mostly in 2004, with more entrepreneurs in sales and fewer in services.

Although the overall level has changed slightly, 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, participation in the formal sector increased by 5 p.p. in this period, whereas the proportion of informal workers (employed or self-employed) decreased by 4 p.p. The remaining difference of 1 p.p. comes from the group of jobless, which decreased from 14 per cent to 13 per cent.

With the creation of *Bolsa Família* in 2003, the percentage of individuals receiving cash transfers (programme coverage) went from 4.7 per cent in 2001 to 19.4 per cent in 2006. A simple difference-in-difference analysis indicates that the rising entrepreneurship rate is associated with the increasing coverage. Since the programme is targeted at poor people, who are less likely to be entrepreneurs, the relationship between programme size and entrepreneurship is indeed negative across municipalities. However, Figure 3 also shows that this relationship is flatter in 2006, which suggests that entrepreneurship has grown more in areas with higher programme coverage.

Nonetheless, that relationship might have already been changing before the programme started. In this case, the curve in 2002 would be flatter than the one in 2001. Figure 3 reveals that these curves are rather parallel, indicating that entrepreneurial rates in poor and rich municipalities had followed similar trends up to 2003.

Besides the gradual expansion of *Bolsa Família*, other socio-economic improvements are observed in Table 3. For instance, the percentage of adult men with a high school diploma increased by 10 p.p. in five years. The same increase is seen in high school enrolment rates. Also, the percentage of houses linked to the sewer system increased by 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 way in which the programme may affect entrepreneurship is through private transfers. This form of income is calculated as the sum of donations and other incomes, excluding retirement benefits, other pensions, rental earnings and social benefits. If poor households adopt informal risk-sharing strategies, the percentage of them receiving private transfers should increase along with the liquidity provided by the programme. In Table 3, we observe that this rate rose from 4.3 per cent in 2001 to 7.7 per cent in 2006.

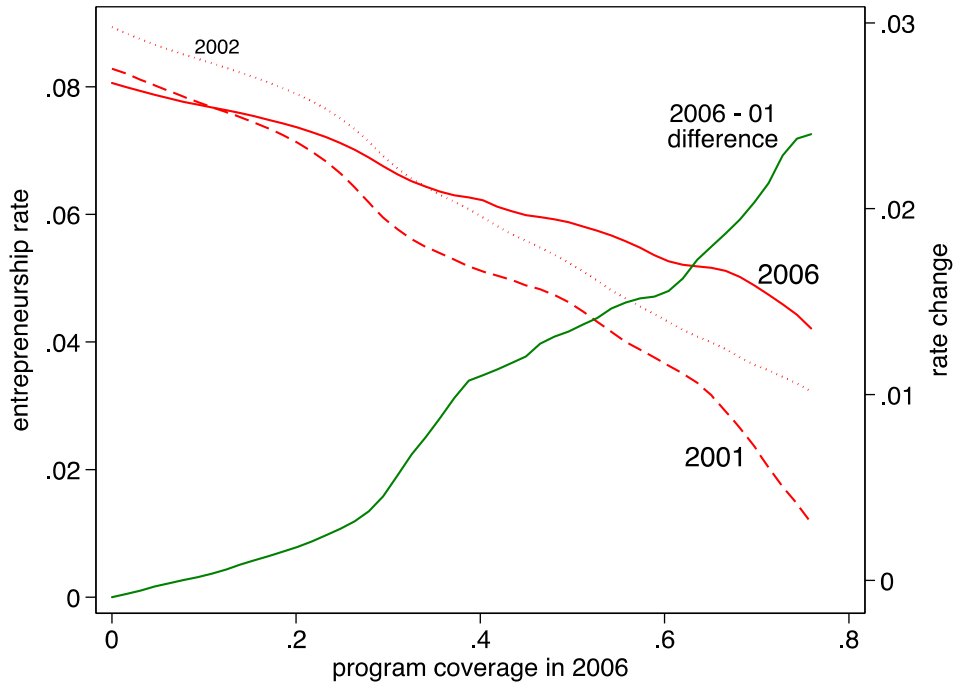
TABLE 3

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 children	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						
programme 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
log of population density	5.43	2.37	5.42	2.38	5.40	2.38
poverty headcount	0.257	0.175	0.249	0.170	0.192	0.147
elementary enrolment rate	0.929	0.065	0.939	0.060	0.952	0.049
primary enrolment rate	0.726	0.161	0.775	0.132	0.794	0.123
high school enrolment 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
proportion of house owners	0.694	0.107	0.699	0.103	0.695	0.103
Number of observations	41,737		43,183		44,868	

FIGURE 3

Relationship between Entrepreneurship and Programme 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. Programme coverage is measured by the proportion of individuals participating in the programme in 2006. Municipalities where the programme coverage was greater than 5 p.p. in 2001 are not included.

4 EMPIRICAL STRATEGY

The empirical strategy consists of a difference-in-difference model estimated using a three-period dataset. As discussed above, the programme 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 programme 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.[17]

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 programme intervention. This instrument eliminates the part of variance in the programme assignment that could be related to unobservable changes in the labour 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 programmes. The direct effect is understood as the individual response of households to the programme 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 programme 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). This test replaces the individual-level randomisation adopted by Duflo and Saez (2003) and Crépon et al. (2013) to separate the direct effect from the indirect effect. Once the homogeneity in the indirect effect is confirmed, the overall effect can be decomposed, 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 to be an entrepreneur. Based on equation (2.5), this decision is determined by a linear structural model:

$$y_{ivt} = \beta_0 + \beta_1 d_{ivt} + \beta_2 \bar{d}_{vt} + \mu_v + \mu_t + u_{ivt}, \qquad (0.1)$$

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 proportion of individuals receiving treatment in the same municipality (programme coverage). Namely, $\bar{d}_{vt}$ is the mean of d_{ivt} conditional on living in v at time t.

Definition (direct, indirect and overall effect): *Following equation (4.1):*

- *coefficient β_1 is the **direct effect** on participants;*
- *coefficient β_2 is the **indirect effect** on participants; and*
- *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 their being an entrepreneur increases β_1 p.p. due to the direct effect and β_2 p.p. due to the indirect effect. Locally, if the programme coverage 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 studies that compare treated households in covered villages and untreated households in uncovered villages (e.g. evaluations of PROGRESA/*Opportunidades* in Mexico) actually estimate the overall effect of the intervention, τ. On the other hand, studies that compare individuals in the same cities or villages (e.g. Gasparini, Haimovich and Olivieri, 2009; Karlan and Zinman, 2010; Blattman, Fiala and Martinez, 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 programme.

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 programme 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):*

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

Given the choice made by individual i of participating in the programme, 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 programme size is defined by the municipal 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 estimators for β_1 and β_2 have the following asymptotic property:*

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

Note that the asymptotic biases cancel each other, so the estimator for $\tau = (\beta_1 + \beta_2)$ will be consistent if 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:*

$$y_{ivt} = \beta_0 + \tau\bar{d}_{vt} + \mu_v + \mu_t + u_{ivt} \qquad (0.2)$$

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 programme, τ, can be consistently estimated if we just omit d_i 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 programme 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 APPROACH

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 labour market. To consider only changes predicted by the measure of poverty in 2001, rather than changes caused by idiosyncratic behaviour, I also estimate an IV model. In this model, the local coverage need not be strictly driven by observables, but it can be just partially affected by the programme's initial design.

Assumption 4.2 (IV assumption): *Given the current poverty level, p_{vt}, and unobserved fixed variables, the municipal quota is orthogonal to u_{ivt}.*

The municipal quota is proxied by the interaction between the poverty headcount in 2001, p_{v0}, and period dummies. Then the equation for the programme coverage, $\bar{d}_{vt}$, is:

$$\bar{d}_{vt} = \gamma_0 + \gamma_1 p_{v0} \cdot I(t = 2004) + \gamma_2 p_{v0} \cdot I(t = 2006) + \gamma_3 p_{vt} + \theta_v + \theta_t + e_{ivt}. \quad (0.3)$$

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 growth of the programme. 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. Table D.1 in the Appendix confirms that the municipal quota is a strong instrument and that individual characteristics and other social outcomes are balanced across municipalities once we control for that variable.

Since the instrument is defined at the municipality level, the predicted change in the intervention also happens at the municipality level. Therefore, if the programme coverage, $\bar{d}_{vt}$,

is replaced by the individual treatment, d_{ivt}, in equations (4.2) and (4.3), the IV estimator will remain 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. randomisation of treated villages), the comparison between treated and untreated individuals also happens at the cluster level—i.e. across villages rather than between individuals. On the one hand, this IV approach can be used to separate the actual treatment effect from the intention-to-treat effect (Crépon et al., 2014). On the other hand, the estimand cannot be interpreted as the result of a direct effect only.

4.3 SEPARATING DIRECT AND INDIRECT EFFECTS

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

$$y_{ivt|(d=0)} = \beta_{0,(d=0)} + \tau_{(d=0)}\bar{d}_{vt} + \mu_{v,(d=0)} + \mu_{t,(d=0)} + u_{ivt|(d=0)} \qquad (0.4)$$

Non-participants are subject to an overall effect, $\tau_{(d=0)}$, that only comprises the indirect impact of the programme. Therefore, the indirect effect on this group can be estimated 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. $\beta_{2,(d=1)} = \beta_{2,(d=0)} = \beta_2$. If it is different, the marginal indirect effect, as well as the marginal overall effect, should change as new individuals are added to the programme. Thus the dose-response function of programme coverage should be non-linear. 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 must be non-linear.*

As long as the overall effect is linear, we can also infer that $\beta_{2,(d=1)} = \beta_{2,(d=0)} = \beta_2$. 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 fixed effects, suggests that the relationship between entrepreneurship and programme coverage is negative. Although this model includes control variables such as race, age and education, results tend to be biased due to the programme targeting the poorest municipalities. After including local fixed effects, the estimated relationship becomes positive in all other models.

TABLE 4

Overall Effect of Cash Transfers on Entrepreneurship

	Decision of being a small entrepreneur					
	OLS	**FE**		**IV**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
programme 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 enrolment rate					0.01	0.011
					(0.020)	(0.021)
primary enrolment rate					-0.016	-0.016
					(0.012)	(0.013)
high school enrolment 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)
proportion 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	129,298

***, **, * 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 'programme coverage' and 'individual benefit' instrumented by the interactions between poverty headcount in 2001 and year dummies.

Models (2) and (3) include fixed effects in different levels, municipality (city, town or village) and census tract (neighbourhood). As predicted by Proposition 4.1, which states that the within-municipality programme assignment does not affect estimates for the overall effect, the coefficient does not change if I use fixed effects at a lower level. According to these models, a 10 p.p. increase in local coverage raises the entrepreneurship rate by 0.4 p.p. Considering the baseline rate of 7 per cent and the current coverage of 19 per cent, the programme might be responsible for an increase of 10 per cent 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 enrolment 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 dummy of individual benefit replaces the local coverage, but the instrumental variable is the same as before. As expected, the estimated coefficient barely changes because the cluster-level instrument compares observations 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 programme on participants.

The estimated overall effect between 4 and 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), and some microcredit programmes, which do not look to increase entrepreneurship at the extensive margin

(e.g. Banerjee et al., 2013; Angelucci, Karlan and Zinman, 2014; Crépon et al., 2014).[18] However, it is half as large as the Targeted Ultra-Poor Programme's in Bangladesh (Bandiera et al., 2013) and the Youth Opportunities Programme's in Uganda (Blattman, Fiala and Martinez, 2013). These two programmes, nevertheless, are particularly intended to promote entrepreneurship, with the transfer being conditional on productive investments.

5.5.1 Types of Business Being Affected

To verify the nature of entrepreneurship being affected by the programme, entrepreneurs are classified by the type of business 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

Overall Effect of Cash Transfers on Different Types of Business

	Decision of being a small entrepreneur in					
	Services		**Sales**		**Manufacturing**	
	FE	**IV**	**FE**	**IV**	**FE**	**IV**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
programme coverage, $\bar{d}$	0.038***	0.053***	0.015**	0.019	-0.004	-0.004
	(0.010)	(0.017)	(0.008)	(0.013)	(0.007)	(0.011)
age (x10)	0.031***	0.031***	0.023*	0.023*	0.001	0.001
	(0.012)	(0.012)	(0.012)	(0.012)	(0.010)	(0.010)
squared age (x100)	-0.002	-0.002	-0.001	-0.001	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
white	0.016***	0.016***	0.015***	0.015***	0.006***	0.006***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
black	-0.006***	-0.006***	-0.005***	-0.005***	-0.005***	-0.005***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
married	0.000	0.000	0.012***	0.012***	0.006***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
elementary education	0.011***	0.011***	0.011***	0.011***	0.008***	0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
primary education	0.012***	0.012***	0.015***	0.015***	0.003**	0.003**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
high school	0.022***	0.022***	0.013***	0.013***	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
log of population	-0.012	-0.014	-0.016*	-0.017*	0.003	0.003
	(0.011)	(0.011)	(0.009)	(0.009)	(0.008)	(0.008)
year = 2001	0.020***	0.022***	-0.008***	-0.008***	-0.004**	-0.004
	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)	(0.002)
year = 2004	0.001	0.001	0.001	0.001	-0.002	-0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(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 'programme coverage' instrumented by the interactions between poverty headcount in 2001 and year dummies.

On the one hand, the higher effect on 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 programmes is available, but we know that *Bolsa Família* did not have such a component. This result suggests that part of the transfers goes into the hands of already trained entrepreneurs, giving them the opportunity to formalise their activity. However, services may not create as many jobs as 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

To estimate the indirect effect of the programme, I first have to verify whether it is constant or not. According to Proposition 4.2, if the overall effect is linear, then the indirect effect of the programme is similar for participants and non-participants. The first column of Table 6 shows that the coefficient of squared coverage is very close to zero and not significant.

Since the hypothesis of linear overall effect is not rejected, I estimate the indirect effect of the programme using only the sample of non-participants. Columns (2) and (3) of Table 6 present the estimated indirect effect, which is greater than the overall effect discussed above. Then the direct response to the programme 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.[19]

The results indicate that, on the one hand, cash transfers reduce the probability of participants starting their own business by 3 to 4 p.p. On the other hand, the amount of cash transferred to poor villages seems to encourage the creation of new businesses. A 10 p.p. increase in the programme size tends to raise the entrepreneurship rate of poor individuals by 0.7 to 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 suggests that small entrepreneurs are not as responsive to financial constraints as to other general equilibrium mechanisms. Nevertheless, there are several possible explanations for the negative direct response and the positive indirect effect on entrepreneurship.[20] In Section 6 we will find that the indirect response is related to the promotion of informal financing mechanisms among poor households. Moreover, the hypothesis of increasing investment opportunity by shifting the aggregate demand is not supported by the following tests.

5.3 INDIRECT EFFECT AND POPULATION DENSITY

In terms of policy implications, it is worth knowing where the indirect effect of cash transfers is higher. To identify large cities and small villages and have a sense of geographical differences, I construct a variable of population density using the Demographic Census. However, the matching between these data and the ones used so far is not possible for all municipalities, particularly the small ones. Accordingly, I match clusters of municipalities defined by their size, metropolitan status and state.

TABLE 6

Non-linear, 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)
programme coverage, $\bar{d}$	0.045	0.070***	0.079***	0.070***	0.079***
	(0.028)	(0.015)	(0.024)	(0.015)	(0.024)
squared coverage, $\bar{d}^2$	-0.006				
	(0.043)				
individual benefit, d				-0.032***	-0.041***
				(0.004)	(0.006)
age (x10)	0.060***	0.063***	0.063***	0.064***	0.064***
	(0.016)	(0.018)	(0.018)	(0.016)	(0.016)
squared age (x100)	-0.003	-0.003	-0.003	-0.003	-0.003
	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
white	0.032***	0.034***	0.034***	0.031***	0.031***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
black	-0.014***	-0.015***	-0.015***	-0.014***	-0.014***
	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
married	0.025***	0.029***	0.029***	0.027***	0.027***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
elementary education	0.027***	0.028***	0.028***	0.025***	0.025***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
primary education	0.028***	0.028***	0.028***	0.027***	0.027***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
high school	0.031***	0.030***	0.030***	0.030***	0.030***
	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
log of population	-0.024	-0.031*	-0.032*	-0.024	-0.026*
	(0.015)	(0.017)	(0.017)	(0.015)	(0.015)
year = 2001	0.006	0.004	0.005	0.005	0.008
	(0.004)	(0.004)	(0.005)	(0.003)	(0.005)
year = 2004	-0.001	-0.001	-0.001	-0.001	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(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 programme coverage. Columns (2) and (3) present the estimates of the indirect effect on individuals who do not participate in the programme. Columns (4) and (5) present the estimates of the indirect effect (programme 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 'programme coverage' instrumented by the interactions between poverty headcount in 2001 and year dummies.

TABLE 7

Indirect and Direct Effects of Cash Transfers on Entrepreneurship by Population Density

	Entrepreneurial decision	
	FE	**IV**
	(1)	**(2)**
Regression coefficients		
programme coverage, $\bar{d}$	0.053***	0.060**
	(0.018)	(0.029)
$\bar{d}$ $*$ log population density	-0.008*	-0.009*
	(0.004)	(0.005)
individual benefit, d	-0.039***	-0.049***
	(0.005)	(0.007)
d $*$ log population density	0.000	0.000
	(0.001)	(0.002)
Effect on the bottom 20% of density		
programme coverage, $\bar{d}$	0.072***	0.082***
	(0.015)	(0.024)
individual benefit, d	-0.040***	-0.049***
	(0.005)	(0.006)
Effect on the top 20% of density		
programme coverage, $\bar{d}$	0.031	0.034
	(0.027)	(0.039)
individual benefit, d	-0.038***	-0.048***
	(0.007)	(0.010)
Municipality fixed effects	Yes	Yes
Year dummies	Yes	Yes
Demographic	Yes	Yes
Number of observations – all sample	129,264	129,264
Number of observations – $d = 0$	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. The top panel presents the regression coefficients of interest. The bottom panel presents the predicted effects on the top 20% and the bottom 20% of the population density distribution. Column (1) has the results from the fixed-effect model (FE) estimated using the within-group method. Column (2) has the results from the fixed-effect, Instrumental-Variable regression (IV) with 'programme coverage' instrumented by the interactions between poverty headcount in 2001 and year dummies. In each column, coefficients are estimated using Seemingly Unrelated Regressions (SUR) with bias correction given by Lemma 4.1.

My findings, presented in Table 7, reveal that the higher the population density, the lower the indirect effect. The direct effect, on the other hand, does not change with the population density. As a result, the net effect of cash transfers is significantly positive in low-density areas, as observed before, and insignificant in highly populated areas. This result implies that the programme is not effective in promoting entrepreneurship in large cities, because the negative direct response from recipients offsets the small indirect effect. The programme's impact is concentrated in small villages, which probably have less aggregate liquidity.

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. As in Angelucci and De Giorgi's (2009) study, the indirect effect of cash transfers might be driven by the existence of risk-sharing strategies within villages. 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 lenders for those who start a business and on the specific amount of transfers received from other households.

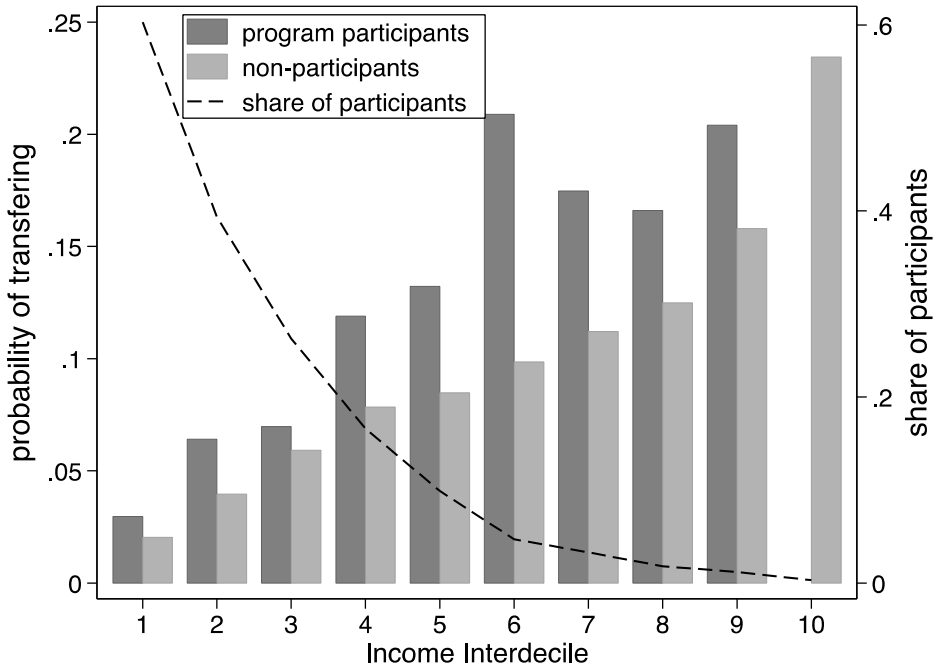
Using another household survey, which reports more detailed information on income and expenditures, I calculated the probability of participating households to lend or transfer money to another household unit. Figure 4 shows that programme participants are indeed more likely to make transfers to other households in each section of income distribution. On average, participants are about 40 per cent more likely to be a moneylender than non-participants 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 programme participants declare that they are poorer than they look in household surveys, the observed difference represents a lower-bound estimate for the causal effect.

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 labour earnings, retirement benefits, other pensions, rental earnings and social programmes.

Table 8 presents the estimated effect of programme 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 by 1.3 to 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. This result is consistent with the ones found by Attanasio et al. (2011) and Karlan and Zinman (2010), who show that access to credit increases mutual assistance between households.

FIGURE 4

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 CCT. Income deciles are calculated using household per capita income.

TABLE 8

Indirect Effect of Cash Transfers on Private Transfers and Entrepreneurship

	Receiving private transfers			Entrepreneurial decision	
	FE	IV	FE	FE	IV
	(1)	(2)	(3)	(4)	(5)
programme coverage, $\bar{d}$	0.127***	0.185***		0.057***	0.068**
	(0.019)	(0.030)		(0.016)	(0.034)
$\bar{d}$ * effect on private transfers				0.463**	0.575
				(0.218)	(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**	-0.037**	-0.022	0.072***	0.072***
	(0.017)	(0.017)	(0.017)	(0.018)	(0.018)
squared age (x100)	0.005**	0.005**	0.003	-0.004	-0.004
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
white	0.003	0.003	0.003	0.034***	0.034***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
black	0.000	0.000	-0.001	-0.015***	-0.015***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
married	-0.025***	-0.025***	-0.014***	0.028***	0.028***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
elementary education	-0.019***	-0.019***	-0.014***	0.028***	0.028***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
primary education	-0.002	-0.002	0.000	0.028***	0.028***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
high school	0.007***	0.008***	0.010***	0.030***	0.030***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
number of children	0.007***	0.007***	0.006***	-0.002**	-0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
number of elderly	0.022***	0.022***	0.016***	-0.006***	-0.006***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
log of population	0.010	0.004	0.014	-0.027	-0.028
	(0.018)	(0.019)	(0.018)	(0.017)	(0.017)
year = 2001	-0.021***	-0.013***	-0.023***	0.003	0.005
	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)
year = 2004	-0.005*	-0.003	-0.006**	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	113,115	113,115	113,115	113,115	113,115

***, **, * 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 private transfers' is calculated by a regression of private transfers on programme coverage interacting with 2001 municipality characteristics. The coefficient of interaction between 'effect on private transfers' and 'programme coverage' represents how much the effect of programme 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 'programme coverage' instrumented by the interactions between poverty headcount in 2001 and year dummies.

While individuals with better job opportunities may use these transfers as a safety net, individuals with fewer job opportunities may use them to start their own business. Since I do not know if current entrepreneurs received other transfers before, I cannot conclude that these

transfers are actually invested. The only conclusion being made is that the effect on receiving other transfers is the highest among those who need them most. Namely, the effect is significantly higher for the jobless, followed by informal workers—see column (3) in Table 8.

To verify whether the indirect effects on entrepreneurship and private transfers are related, I include the interaction between programme coverage and the predicted effect on private transfers in the regression (columns (4) and (5) in Table 8). 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 effect of cash transfers on both private transfers and entrepreneurship is higher in less populated areas, with higher school enrolment rates and higher labour informality. Using the predicted effect on private transfers, I find that the larger this effect, the higher the indirect effect on entrepreneurship. Although this is just a rough estimation, it indicates that entrepreneurial activity has increased through the promotion of informal risk-sharing mechanisms.

6.2 AGGREGATE DEMAND AND INVESTMENT OPPORTUNITIES

If the indirect effect on entrepreneurship came from a shock in the aggregate demand, we should observe other changes in the labour market. For instance, increasing investment opportunities should also affect the decision of well-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 9

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

	Well-educated entrepreneurial decision		**Less-educated employment**		**Less-educated employees' wages**	
	FE	**IV**	**FE**	**IV**	**FE**	**IV**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
programme coverage, $\bar{d}$	-0.014	0.181	0.004	-0.003	0.202	0.467
	(0.141)	(0.224)	(0.021)	(0.034)	(0.889)	(1.117)
$\bar{d} *$ private sector					0.050	-0.028
					(0.886)	(1.109)
private sector					-0.385*	-0.361
					(0.222)	(0.279)
age (x10)	0.355***	0.357***	0.149***	0.149***	0.517***	0.517***
	(0.102)	(0.102)	(0.022)	(0.022)	(0.052)	(0.051)
squared age (x100)	-0.035**	-0.035**	-0.023***	-0.023***	-0.045***	-0.045***
	(0.015)	(0.015)	(0.003)	(0.003)	(0.007)	(0.007)
white	0.039***	0.039***	-0.001	-0.001	0.116***	0.116***
	(0.013)	(0.013)	(0.003)	(0.003)	(0.008)	(0.008)

→

black	-0.075**	-0.075**	-0.007	-0.007	-0.025***	-0.025***
	(0.032)	(0.032)	(0.005)	(0.005)	(0.009)	(0.009)
married	0.012	0.011	0.167***	0.167***	0.157***	0.157***
	(0.011)	(0.011)	(0.003)	(0.003)	(0.006)	(0.006)
elementary education			0.050***	0.050***	0.162***	0.161***
			(0.005)	(0.005)	(0.009)	(0.009)
primary education			0.018***	0.018***	0.187***	0.187***
			(0.003)	(0.003)	(0.008)	(0.008)
high school			0.028***	0.028***	0.371***	0.371***
			(0.003)	(0.003)	(0.014)	(0.014)
log of population	-0.039	-0.055	0.012	0.012	0.075	0.056
	(0.110)	(0.111)	(0.021)	(0.021)	(0.048)	(0.048)
year = 2001	0.012	0.026	-0.013***	-0.013**	0.061***	0.081***
	(0.019)	(0.025)	(0.005)	(0.006)	(0.020)	(0.022)
year = 2004	0.006	0.010	-0.002	-0.002	-0.063***	-0.056***
	(0.013)	(0.014)	(0.004)	(0.005)	(0.009)	(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 less-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 less-educated workers formally employed in either the private or public sector. FE columns show fixed-effect regressions obtained using the within-group method. IV columns show fixed-effect, Instrumental-Variable regressions with 'programme coverage' instrumented by the interactions between poverty headcount in 2001 and year dummies.

The first two columns of Table 9 confirm that the programme size has no significant effect on the probability of well-educated men becoming entrepreneurs. Thus we cannot say that the programme has encouraged the creation of local businesses in general. That is, the effect on entrepreneurship is concentrated among less-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 less-educated men have taken the decision to be an entrepreneur, the programme has had no effect on their overall employment rate. This result suggests that the programme does not affect the demand side of the labour 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 *Bolsa Família* has not significantly affected the employment rate, the effect on the demand side could have been just on wages. It is worth noting that the estimated effect on wages can be misleading if the programme has some influence on local prices. Accordingly, I use wages of less-educated public employees as a proxy for labour costs. Then the real effect on aggregate demand is assessed by the difference between documented employees in the private sector and public servants. Indeed, the estimated coefficient for the interaction between programme coverage and the private sector, in the last two columns of Table 9, is very close to zero.

6.3 OTHER OCCUPATIONAL CHOICES

To understand where the marginal entrepreneurs come from, I also investigate the effect of the programme on other occupational choices. Besides entrepreneur, the alternatives are jobless, formal employee, informal employee and informal self-employed. Table 10 presents the direct and indirect effects of the programme on the probability of being in each one of these categories, *vis-à-vis* being in any other category.

TABLE 10

Indirect and Direct Effects of Cash Transfers on Other Occupational Choices

	Fixed-effect model				
	Entrepreneur	**Jobless**	**Formal employee**	**Informal employee**	**Informal self-employed**
programme coverage, $\bar{d}$	0.070***	-0.004	0.020	-0.066***	-0.020
	(0.015)	(0.021)	(0.027)	(0.023)	(0.027)
individual benefit, d	-0.032***	0.029***	-0.056***	0.029***	0.030***
	(0.004)	(0.009)	(0.012)	(0.010)	(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 observations – all sample	129,264	129,264	129,264	129,264	129,264
N. of observations – $d = 0$	113,267	113,267	113,267	113,267	113,267
	Instrumental-variable model				
	Entrepreneur	**Jobless**	**Formal employee**	**Informal employee**	**Informal self-employed**
programme coverage, $\bar{d}$	0.079***	0.002	-0.001	-0.092***	0.011
	(0.024)	(0.034)	(0.040)	(0.034)	(0.039)
individual benefit, d	-0.041***	0.041***	-0.050***	0.004***	0.046
	(0.006)	(0.014)	(0.016)	(0.016)	(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 observations – all sample	129,264	129,264	129,264	129,264	129,264
N. of observations – $d = 0$	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 (programme 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, 'programme coverage' is instrumented by the interactions between poverty headcount in 2001 and year dummies.

The estimated indirect coefficients indicate that the programme has no significant effect on the proportion of jobless men in the areas covered. The programme does not have a significant indirect effect on the proportion of formal employees either. Once again, the hypothesis that the money injected into 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 labour market in the 2000s cannot be attributed as much to *Bolsa Família* as to other demographic and economic changes.[21]

The strongest indirect effect is on the proportion of informal employees. Assuming that the labour market is segregated, the programme may have given the financial opportunity to informal workers to open their own business. As already explained, the cash transferred by *Bolsa Família* has probably flowed into the hands of these workers by means of private transfers among poor households.

As regards the direct impact on programme participants, the negative effect on entrepreneurship looks symmetric to the positive effect on the jobless rate. That is, this negative effect is strictly related to the income effect that unearned income has on labour supply. On the other hand, the reduction in labour supply only happens among formal workers (entrepreneurs and documented employees).[22] Thus programme participants reduce labour supply not 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 to stay officially eligible for the transfer.[23]

According to the official records of the Ministry of Social Development and Fight Against Hunger (MDS), almost 40 per cent of cases of benefit cancellation are 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 Labour and Employment's dataset, the 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 growth of the programme. In other words, there is no convergence in the entrepreneurship rate between poor and rich municipalities in Brazil. This convergence could be driven by other social programmes or by the process of credit expansion. In the main results shown above (column (5) in Table 4), I already included some social outcomes to control for part of these programmes. 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 11, the interaction coefficients are close to zero and not significant. That is, poverty itself does not explain the growth in entrepreneurship unless by means of the growth of the programme. Also the overall effect of programme coverage remains around 4 p.p., as found before.

TABLE 11

Overall Effect of Cash Transfers on Entrepreneurship, Robustness Analyses

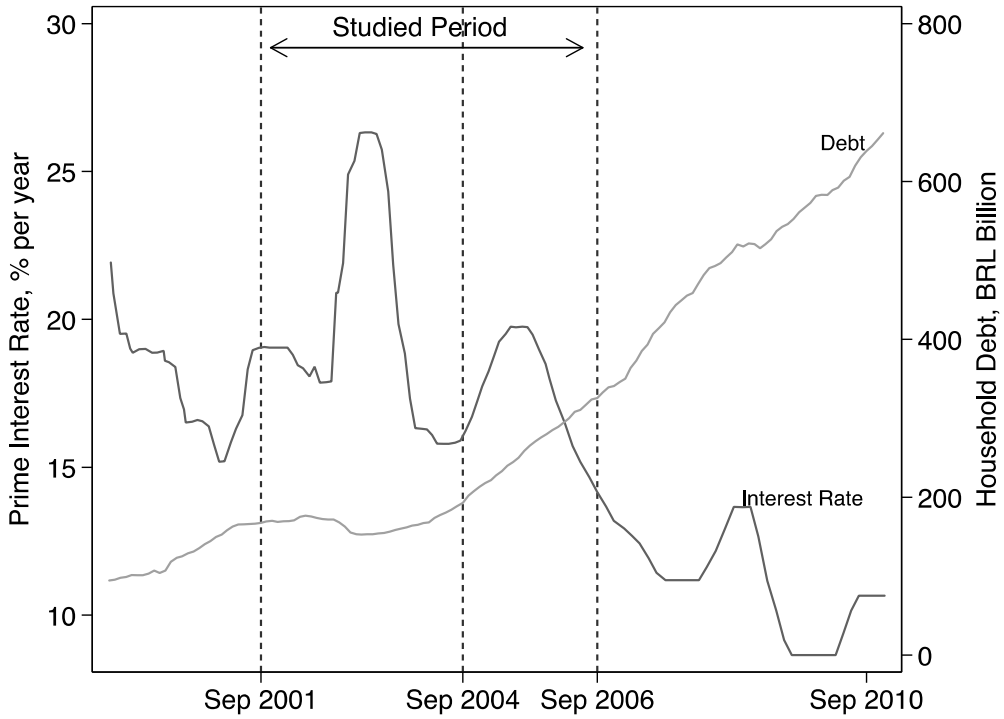
	Decision of being a small entrepreneur				
		2001–2004		excluding Northeast	
	FE	FE	IV	FE	IV
	(1)	(2)	(3)	(4)	(5)
programme coverage, $\bar{d}$	0.036**	0.040**	0.062*	0.055***	0.083**
	(0.015)	(0.018)	(0.032)	(0.019)	(0.033)
poverty	-0.026				
	(0.022)				
poverty * year = 2001	-0.004				
	(0.015)				
poverty * year = $2004^{(b)}$	0.004				
	(0.011)				
age (x10)	0.060***	0.052**	0.052**	0.071***	0.071***
	(0.016)	(0.020)	(0.020)	(0.020)	(0.020)
squared age (x100)	-0.003	-0.002	-0.002	-0.004	-0.004
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
white	0.032***	0.032***	0.032***	0.037***	0.037***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
black	-0.014***	-0.016***	-0.016***	-0.013***	-0.014***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
married	0.025***	0.026***	0.026***	0.027***	0.027***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
elementary education	0.027***	0.026***	0.026***	0.030***	0.030***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
primary education	0.028***	0.028***	0.028***	0.030***	0.030***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
high school	0.031***	0.038***	0.038***	0.033***	0.033***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
log of population	-0.020	-0.007	-0.008	-0.040**	-0.044**
	(0.015)	(0.022)	(0.022)	(0.018)	(0.019)
year = 2001	0.008*	0.007*	0.010*	0.007	0.009*
	(0.005)	(0.004)	(0.005)	(0.004)	(0.006)
year = 2004	0.000			-0.001	0.000
	(0.003)			(0.002)	(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 between 2001 and 2004 (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 'programme coverage' instrumented by the interactions between poverty headcount in 2001 and year dummies.

As regards the increasing access to credit, Figure 5 shows that the decline in interest rates and the growth of personal loans started in 2005. Thus there is a small overlap between the investigated period (2001–2006) and the period of credit expansion in Brazil. Despite this small overlap, columns (2) and (3) of Table 11 confirm that the estimated effect between 2001 and 2004 is still around 4 to 6 p.p.

FIGURE 5

Household Debt Outstanding and Interest Rate in Brazil



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

Even though the credit expansion started in the late 2000s, other microcredit programmes have been in place since the 1990s. To test whether the results are driven by microcredit programmes, I exclude from the sample the region where the largest and most significant programme was introduced. The *CrediAmigo* programme, created in 1997, is considered the largest microfinance programme in the country, but it covers only municipalities in the Northeast region. Columns (4) and (5) of Table 11 show that the estimated effect on entrepreneurship increases slightly 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 programmes might have promoted the migration of potential entrepreneurs, as well as other type of workers, to highly covered areas. As shown in Table 12, programme coverage has no significant effect on the probability of migrating from another municipality in the last four years. Therefore, the estimated effects are probably not due to changes in the composition of workers in the labour force, but due to changes in their decisions.

TABLE 12

Overall Effect of Cash Transfers on Migration

	Migration	
	FE (1)	IV (2)
program coverage, $\overline{d}$	0.014	-0.030
	(0.023)	(0.043)
age (x10)	-0.067***	-0.067***
	(0.023)	(0.023)
squared age (x100)	0.005	0.005
	(0.003)	(0.003)
white	0.004*	0.004*
	(0.003)	(0.003)
black	0.003	0.003
	(0.004)	(0.004)
married	0.021***	0.021***
	(0.003)	(0.003)
elementary education	-0.004	-0.004
	(0.004)	(0.004)
primary education	0.001	0.001
	(0.002)	(0.002)
high school	0.006*	0.006*
	(0.003)	(0.003)
year = 2001	-0.057***	-0.063***
	(0.006)	(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.

7 CONCLUSION

Entrepreneurship is not usually an intended outcome of CCT programmes, 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 programmes, 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 typically allow only the identification of the overall effects of aid programmes. In this study, the decomposition into direct and indirect effects is identified due to the variation in the size of the *Bolsa Família* programme across municipalities in Brazil, despite the way that the benefit is distributed within municipalities. 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 randomised at the cluster level.

The results indicate that, on the one hand, cash transfers have a negative direct effect on entrepreneurship, reducing the probability of beneficiaries starting 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 programme encourage its beneficiaries to either reduce labour supply or move to the informal sector to keep their cash benefit. This finding ratifies a major concern in welfare programmes in general and reveals a caveat in terms of eligibility rules.[24]

On the other hand, the amount of cash transferred to poor villages tends 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 well-educated entrepreneurs nor the number of formal jobs has grown with the programme. The lack of other impacts on the labour market indicates that *Bolsa Família* has indirectly changed the occupational choice of poor workers on the supply side, but not the demand for labour. This finding is not as exceptional as some CCT advocates claim, but it suggests that the programme has been responsible for the formalisation 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 programme has encouraged interpersonal transfers, particularly to those facing income shortages. Then the liquidity shock delivered by the programme appears to reduce the opportunity cost of risk-sharing among poor households, rather than lessening individual financial constraints. That is, entrepreneurship looks to be more responsive to locally aggregate liquidity shocks, which promote informal financing mechanisms, than to individual liquidity shocks.

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:

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

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:

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

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

$$\lambda u\left(c_{2,G}^M\right) + (1-\lambda)u\left(c_{2,B}^M\right) - u(c_2^L) = u(c_1^L) - u(c_1^M). \tag{A.8}$$

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

$$u'(c_1^M) = \lambda u'\left(c_{2,G}^M\right) + (1-\lambda)u'\left(c_{2,B}^M\right) + \vartheta_M \tag{A.9}$$

$$u'(c_1^L) = u'(c_2^L) + \vartheta_L \tag{A.10}$$

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:

$$\frac{\mathrm{d}D(\hat{q})}{\mathrm{d}d} = \frac{\mathrm{d}D(\hat{q})}{\mathrm{d}d_1} + \frac{\mathrm{d}D(\hat{q})}{\mathrm{d}d_2}.$$

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

$$CE \equiv \frac{\mathrm{d}D(\hat{q})}{\mathrm{d}d_1};$$

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

$$IE \equiv \left.\frac{\mathrm{d}D(\hat{q})}{\mathrm{d}d_2}\right|_{\zeta=0},$$

and the eligibility effect (EE),

$$EE \equiv \left.\frac{\mathrm{d}D(\hat{q})}{\mathrm{d}d_2}\right|_{\zeta=1} - \left.\frac{\mathrm{d}D(\hat{q})}{\mathrm{d}d_2}\right|_{\zeta=0}.$$

From 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

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

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

$$u'(c_1^L) = u'(c_2^L) \Leftrightarrow u(c_1^L) = u(c_2^L). \tag{A.11}$$

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

$$\lambda u\left(c_{2,G}^M\right) + (1-\lambda)u\left(c_{2,B}^M\right) \geq u(c_1^M), \tag{A.12}$$

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

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

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

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:

$$CE = u'(c_1^M) - u'(c_1^L) > 0. \tag{A.13}$$

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

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

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

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

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

$$\begin{aligned} IE &= u'(c_1^M) - u'(c_2^L) \\ &\geq u'(c_1^M) - u'(c_1^L) \\ &= CE > 0 \end{aligned} \tag{A.14}$$

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:

$$\frac{\partial IE}{\partial \lambda} = u'(\hat{q} + d_2) - u'(\delta + d_2) < 0. \tag{A.15}$$

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:

$$\begin{aligned} u'(c_1^M) - u'(c_1^L) &> u'(c_2^L) - u'(\hat{q} + d_2) \\ CE &> -IE \end{aligned} \tag{A.16}$$

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 effect of cash transfers is positive:

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

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

$$\begin{aligned} EE &= \left.\frac{\mathrm{d}D(\hat{q})}{\mathrm{d}d_2}\right|_{\zeta=1} - \left.\frac{\mathrm{d}D(\hat{q})}{\mathrm{d}d_2}\right|_{\zeta=0} \\ &= -\lambda u'\left(c_{2,G}^M\right) < 0. \end{aligned} \tag{A.17}$$

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

$$\left.\frac{\mathrm{d}D(\hat{q})}{\mathrm{d}d}\right|_{\zeta=1} = CE + IE + EE$$
$$= u'(c_1^M) - u'(c_1^L) + (1-\lambda)u'\left(c_{2,B}^M\right) - u'(c_2^L) \gtreqless 0. \qquad \text{(A.18)}$$

Even though $u'(c_1^M) - u'(c_1^L) > 0$ and $u'\left(c_{2,B}^M\right) - u'(c_2^L) > 0$, we have $(1-\lambda)u'\left(c_{2,B}^M\right) - u'(c_2^L) < 0$ 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:

$$\left.\frac{\mathrm{d}^2 D(\hat{q})}{\mathrm{d}d_2 \mathrm{d}\lambda}\right|_{\zeta=1} = \left[u''(c_1^M) + (1-\lambda)u''\left(c_{2,B}^M\right)\right]\frac{\partial s_M^*}{\partial \lambda} - u'\left(c_{2,B}^M\right)$$
$$= \begin{cases} \frac{u''(c_1^M)+(1-\lambda)u''(c_{2,B}^M)}{u''(c_1^M)+\lambda u''(c_{2,G}^M)+(1-\lambda)u''(c_{2,B}^M)}\left[u'\left(c_{2,B}^M\right) - u'\left(c_{2,G}^M\right)\right] - u'\left(c_{2,B}^M\right) & \text{if } s_M^* > 0 \\ -u'\left(c_{2,B}^M\right) & \text{if } s_M^* = 0 \end{cases}$$
$$< 0$$

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

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

■

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:

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

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

$$\lambda\left[u\left(c_{2,G}^{M}\right) - u\left(c_{2,G}^{L}\right)\right] + (1-\lambda)\left[u\left(c_{2,B}^{M}\right) - u\left(c_{2,B}^{L}\right)\right] = u(c_{1}^{L}) - u(c_{1}^{M}). \quad \text{(A.19)}$$

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

$$r = \lambda\frac{u'\left(c_{2,G}^{M}\right)}{u'(c_{1}^{M})} = \lambda\frac{u'\left(c_{2,G}^{L}\right)}{u'(c_{1}^{L})} \quad \text{(A.20)}$$

$$i = (1-\lambda)\frac{u'\left(c_{2,B}^{M}\right)}{u'(c_{1}^{M})} = (1-\lambda)\frac{u'\left(c_{2,B}^{L}\right)}{u'(c_{1}^{L})} \quad \text{(A.21)}$$

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),

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

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

$$u'(c_1^M) > u'(c_1^L) \Leftrightarrow u(c_1^L) > u(c_1^M). \tag{A.22}$$

Then (A.13) implies that

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

and (A.12) implies that

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

Hence,

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

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,

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

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

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:

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

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:

$$\begin{aligned} U' &= -ku'(c_1)+\lambda[Q'-w-\zeta d_2]u'(c_{2,G})-(1-\lambda)(w-\delta)u'(c_{2,B}) \\ &= 0 \end{aligned} \quad \text{(A.23)}$$

and

$$\begin{aligned} U'' &= k^2u''(c_1)+\lambda u'(c_{2,G})Q''+\lambda[Q'-w-\zeta d_2]^2u''(c_{2,G})+(1-\lambda)(w-\delta)^2u''(c_{2,B}) \\ &< 0. \end{aligned} \quad \text{(A.24)}$$

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

$$\frac{\mathrm{d}y^*}{\mathrm{d}d_1} = k\frac{u''(c_1)}{U''} > 0; \quad \text{(A.25)}$$

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

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

where $EE = \zeta\lambda u'(c_{2,G})/U''$. 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:

$$GE = \frac{\mathrm{d}y^*}{\mathrm{d}d_1} + \frac{\mathrm{d}y^*}{\mathrm{d}d_2} - EE.$$

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

$$c_{2,G} > c_1 > c_{2,B}. \tag{A.26}$$

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):

$$y^*_{ivt} = \beta_1 d^*_{ivt} + \beta_2 \bar{d}^*_{vt} + u_{ivt} \tag{A.1}$$

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

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:

$$\begin{aligned}
\begin{bmatrix}\hat{\beta}_1 \\ \hat{\beta}_2\end{bmatrix} &= \frac{1}{S_{d^{*2}} S_{\bar{d}^{*2}} - S^2_{d^*\bar{d}^*}} \begin{bmatrix} S_{\bar{d}^{*2}} & -S_{d^*\bar{d}^*} \\ -S_{d^*\bar{d}^*} & S_{d^{*2}} \end{bmatrix} \begin{bmatrix} S_{d^*y^*} \\ S_{\bar{d}^*y^*} \end{bmatrix} \\
&= \frac{1}{S_{d^{*2}} - S_{\bar{d}^{*2}}} \begin{bmatrix} S_{d^*y^*} - S_{\bar{d}^*y^*} \\ \dfrac{S_{d^{*2}}}{S_{\bar{d}^{*2}}} S_{\bar{d}^*y^*} - S_{d^*y^*} \end{bmatrix} \\
&= \begin{bmatrix} \beta_1 + \dfrac{1}{S_{d^{*2}} - S_{\bar{d}^{*2}}} (S_{d^*u} - S_{\bar{d}^*u}) \\ \beta_2 + \dfrac{1}{S_{d^{*2}} - S_{\bar{d}^{*2}}} \left(\dfrac{S_{d^{*2}}}{S_{\bar{d}^{*2}}} S_{\bar{d}^*u} - S_{d^*u} \right) \end{bmatrix}.
\end{aligned} \tag{A.2}$$

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:

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

and

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

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

B.2 PROOF OF PROPOSITION 4.1

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

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

$$\hat{\tau} = \frac{\sum_{ivt}\bar{d}^*_{vt}y^*_{ivt}}{\sum_{ivt}\bar{d}^{*2}_{vt}}$$
$$= \frac{\hat{\beta}_1\sum_{ivt}\bar{d}^*_{vt}d^*_{ivt} + \hat{\beta}_2\sum_{ivt}\bar{d}^*_{vt}\bar{d}^*_{vt} + \sum_{ivt}\bar{d}^*_{vt}\hat{u}_{ivt}}{\sum_{ivt}\bar{d}^{*2}_{vt}}$$
$$= \frac{\hat{\beta}_1\sum_{vt}\bar{d}^*_{vt}\sum_i d^*_{ivt}}{\sum_{vt}\sum_i\bar{d}^{*2}_{vt}} + \hat{\beta}_2$$
$$= \hat{\beta}_1 + \hat{\beta}_2. \quad \text{(A.5)}$$

For the second part, Lemma 4.1 is applied so that

$$\hat{\tau} \xrightarrow{p} \beta_1 + \beta_2. \quad \text{(A.6)}$$

■

B.3 PROOF OF PROPOSITION 4.2

Suppose the true equation to be estimated is:

$$y_{ivt} = \beta_0 + \beta_1 d_{ivt} + \beta_2 \bar{d}_{vt} + \beta_3 d_{ivt} \bar{d}_{vt} + \mu_v + \mu_t + u_{ivt}, \qquad \text{(A.7)}$$

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 non-linear. ■

Appendix C IV with a cluster-level instrument

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

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

Proof: Let y^*_{ivt}, d^*_{ivt}, and $\bar{d}^*_{vt}$ be cluster-period mean-centred versions of y_{ivt}, d_{ivt} and $\bar{d}_{vt}$, respectively.

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

$$y_{ivt} = \beta_0 + \beta_1 d_{ivt} + \mu_v + \mu_t + u_{ivt}, \tag{A.2}$$

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

Let z_{vt} be an instrumental variable such that $Var(z_{vt}|v,t) = 0$. 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 D.1

First-stage Regression Relationship between Poverty in 2001 and Programme Coverage

	Programme coverage, $\overline{d}$			
	(1)	**(2)**	**(3)**	**(4)**
poverty in 2001 * year = $2004^{(a)}$	0.391***	0.391***	0.427***	0.427***
	(0.021)	(0.021)	(0.022)	(0.022)
poverty in 2001 * year = $2006^{(b)}$	0.531***	0.530***	0.602***	0.600***
	(0.025)	(0.025)	(0.027)	(0.027)
age (x10)	0.004	0.004	0.004	0.003
	(0.003)	(0.003)	(0.003)	(0.003)
squared age (x100)	-0.001	-0.001	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
white	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
black	0.000	0.000	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
married	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
elementary education	0.001	0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
primary education	-0.001*	-0.001*	-0.001	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)
high school	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
log of population	0.115***	0.115***	0.088***	0.089***
	(0.022)	(0.022)	(0.022)	(0.022)
year = 2001	0.004	0.003	-0.001	-0.002
	(0.007)	(0.007)	(0.007)	(0.007)
year = 2004	-0.002	-0.003	-0.010**	-0.010**
	(0.005)	(0.005)	(0.005)	(0.005)
poverty headcount			0.268***	0.266***
			(0.035)	(0.035)
elementary enrolment rate			0.012	0.012
			(0.033)	(0.033)
primary enrolment rate			-0.023	-0.024
			(0.020)	(0.020)
high school enrolment rate			-0.008	-0.008
			(0.014)	(0.014)
child mortality (x1000)			-0.079	-0.077
			(0.075)	(0.075)
coverage of sewer system			-0.005	-0.005
			(0.020)	(0.020)
proportion of house owners			-0.043	-0.044
			(0.034)	(0.034)
test $(a) = (b) = 0$, F-stat	226.17	225.69	258.86	258.00
Municipality fixed effects	Yes	No	Yes	No
Census-tract fixed effects	No	Yes	No	Yes
Number of observations	129,298	129,298	129,298	129,264

***, **, * 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.

TABLE D.2

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)
programme coverage, $\bar{d}$	0.042**	0.044	0.104***	0.118***
	(0.018)	(0.031)	(0.023)	(0.032)
individual benefit, d	-0.015***	-0.017**	-0.031***	-0.044***
	(0.006)	(0.007)	(0.009)	(0.013)
age (x10)	0.071**	0.071**	0.062**	0.062**
	(0.029)	(0.029)	(0.028)	(0.028)
squared age (x100)	-0.004	-0.004	-0.002	-0.002
	(0.004)	(0.004)	(0.004)	(0.004)
white	0.031***	0.031***	0.037***	0.037***
	(0.002)	(0.002)	(0.003)	(0.003)
black	-0.010**	-0.010**	-0.021***	-0.021***
	(0.004)	(0.004)	(0.004)	(0.004)
married	0.027***	0.027***	0.029***	0.029***
	(0.002)	(0.002)	(0.003)	(0.003)
elementary education	0.027***	0.027***	0.028***	0.028***
	(0.003)	(0.003)	(0.003)	(0.003)
primary education	0.027***	0.027***	0.028***	0.028***
	(0.003)	(0.003)	(0.004)	(0.004)
high school	0.029***	0.029***	0.031***	0.031***
	(0.003)	(0.003)	(0.004)	(0.004)
log of population	-0.002	-0.002	-0.069***	-0.071***
	(0.022)	(0.023)	(0.023)	(0.023)
year = 2001	0.005	0.005	0.002	0.003
	(0.004)	(0.006)	(0.005)	(0.007)
year = 2004	-0.002	-0.001	-0.002	-0.001
	(0.003)	(0.003)	(0.004)	(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 (programme 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. The FE column shows the fixed-effect regression obtained using the within-group method. The IV column shows fixed-effect, Instrumental-Variable regression with 'programme coverage' instrumented by the interactions between poverty headcount in 2001 and year dummies.

TABLE D.3

Indirect and Direct Effects on Occupational Choices, with and without High School

Panel A: Individuals without high-school diploma

	Fixed-effect model				
	Entrepreneur	Jobless	Formal employee	Informal employee	Informal self-employed
programme coverage, $\bar{d}$	0.056***	-0.014	0.046	-0.062**	-0.026
	(0.015)	(0.024)	(0.031)	(0.029)	(0.033)
individual benefit, d	-0.033***	0.029***	-0.056***	0.033**	0.028*
	(0.005)	(0.011)	(0.014)	(0.013)	(0.015)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes	Yes
No. of observations – all sample	90,825	90,825	90,825	90,825	90,825
No. of observations – $d = 0$	76,709	76,709	76,709	76,709	76,709
	Instrumental-variable model				
	Entrepreneur	Jobless	Formal employee	Informal employee	Informal self-employed
programme coverage, $\bar{d}$	0.061***	-0.004	0.008	-0.099**	0.033
	(0.022)	(0.039)	(0.047)	(0.041)	(0.047)
individual benefit, d	-0.041***	0.050***	-0.069***	0.005	0.055**
	(0.007)	(0.017)	(0.019)	(0.020)	(0.022)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes	Yes
No. of observations – all sample	90,825	90,825	90,825	90,825	90,825
No. of observations – $d = 0$	76,709	76,709	76,709	76,709	76,709

Panel B: Individuals with high-school diploma

	Fixed-effect model				
	Entrepreneur	Jobless	Formal employee	Informal employee	Informal self-employed
programme coverage, $\bar{d}$	0.115***	0.013	-0.099*	-0.026	-0.002
	(0.039)	(0.037)	(0.059)	(0.033)	(0.037)
individual benefit, d	-0.034***	0.039***	-0.070***	0.038***	0.027*
	(0.010)	(0.014)	(0.020)	(0.013)	(0.014)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes	Yes
No. of observations – all sample	38,439	38,439	38,439	38,439	38,439
No. of observations – $d = 0$	36,558	36,558	36,558	36,558	36,558

→

	Instrumental-variable model				
	Entrepreneur	Jobless	Formal employee	Informal employee	Informal self-employed
programme coverage, $\bar{d}$	0.138**	0.004	-0.133	0.007	-0.016
	(0.064)	(0.057)	(0.100)	(0.053)	(0.063)
individual benefit, d	-0.044***	0.021	-0.027	0.016	0.035*
	(0.014)	(0.020)	(0.028)	(0.020)	(0.020)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes	Yes
No. of observations – all sample	38,439	38,439	38,439	38,439	38,439
No. of observations – $d = 0$	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 (programme 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, 'programme coverage' is instrumented by the interactions between poverty headcount in 2001 and year dummies.

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NOTES

1. A non-exhaustive list of papers includes Evans and Jovanovic (1989), Evans and Leighton (1989), Holtz-Eakin, Joulfaian and Rosen (1994), Lindh and Ohlsson (1996), Blanchflower and Oswald (1998), Blanchflower, Oswald and Stutzer (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).

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

3. See Bobonis and Finan (2009), Lalive and Cattaneo (2009) and Angelucci et al. (2009; 2010). See also Crépon et al. (2013) on the indirect effect of labour-market policies and Kremer and Miguel (2007) on the spillovers of health programmes.

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

5. Exceptions are Glewwe and Kassouf's (2012) and de Janvry, Finan and Sadoulet's (2012) in estimating the effect of Bolsa Família on schooling.

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

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

8. Other types of heterogeneity could be assumed, such as in wealth, risk aversion and probability of success. However, with heterogeneous pay-offs 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.

9. An 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 existence of an interior solution, the relationship between d and y is continuous if q is a continuous variable and individuals are risk-averse, $u'' < 0$.

10. Contingent 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).

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

12. In 2004, the extreme poverty line for the programme was USD33, the poverty line was USD66, and the value of the benefit per child was USD10.

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

14. Most of the experimental evidence finds little or no short-run effect of CCTs on job creation and labour supply. See Alzúa, Cruces and Ripani (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, Rubalcava and Teruel (2008) for evaluations of *Oportunidades*; IFS, Econometría and SEI (2006) for an evaluation of *Familias en Acción* in Colombia; and Galasso (2006) for an evaluation of *Chile Solidario*.

15. A census tract is a neighbourhood 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).

16. The exclusion of these employers reduces the sample by 1 per cent, with no implication for the results.

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

18. Exceptions are the programmes studied by Attanasio et al. (2011) and Augsburg et al. (2014).

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

20. The negative direct effect is not likely to be driven by conditionalities on education because participants with no children also reduce entrepreneurial activity. See Table D.2 in the Appendix.

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

22. A similar result is found by Gasparini, Haimovich and Olivieri (2009) in Argentina and Amarante et al. (2011) in Uruguay.

23. The direct effects on labour 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 a high school diploma. See Appendix Table D.3.

24. See Besley and Coate (1992), Kanbur, Keen and Tuomala (1994) and Moffitt (2002).

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