Liquidity constraints, spillovers, and entrepreneurship: evidence from a cash transfer program

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Abstract This paper exploits a liquidity shock from a welfare program in Brazil to investigate the role of financial constraints, in opposition to general equilibrium mechanisms, in explaining entrepreneurship. Previous research focuses exclusively on how liquidity changes recipients' behavior through direct effects on reducing constraints. However, liquidity shocks may also produce spillovers from recipients to others and thereby indirectly affect entrepreneurial decisions. This paper presents a method for decomposing the liquidity shock into direct effects associated with relieving individual constraints, and indirect effects associated with spillovers to other individuals. Results suggest that the program, which assists 20 percent of Brazilian households, increased the number of small entrepreneurs by 10 percent. However, this increase is entirely driven by the indirect effect. Further tests suggest that this effect is associated with an increase in private transfers between households. Thus, entrepreneurship tends to

respond more to the interaction between households than to financial constraints.

Keywords Entrepreneurship · Financial constraint · Cash transfers · Informality · Indirect effect · Private transfers

JEL Classification C21 · H31 · I38 · J24 · R28 · R51 · L26

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

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

that small entrepreneurs cope with start-up costs and investment risks (Ghatak et al. 2001), which yet represent a large sacrifice for poor individuals (Buera 2009).² On the other hand, the latent entrepreneurial activity could also be explained by the lack of investment opportunities in underdeveloped areas (Sadoulet et al. 2001; Davies and Davey 2008). Also, 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 making investments and sharing idiosyncratic risks.

This paper explores the importance of both financial constraints and general equilibrium mechanisms by estimating the impact of a liquidity shock on the decision to be an entrepreneur in urban areas. Unlike other interventions (e.g., Karlan and Zinman 2010; Blattman et al. 2014), liquidity is not delivered uniquely to entrepreneurs. The studied intervention is a large-scale conditional cash transfer (CCT) program in Brazil, called *Bolsa Família*. This program offers a small but steady income to poor households that are committed to send their children to school and have regular health check-ups. However, it has absolutely no rule regarding business investment, adult labor supply or repayment. Thus, its benefit is viewed as a simple liquidity shock on the beneficiaries' budget.

In addition to the *direct* effect on CCT recipients' behavior, we also attempt to identify the *indirect* effect that this program has on local economies, without necessarily assuming an economic multiplier. If potential entrepreneurs face financial constraints, the individual liquidity shock should change recipients' occupational choice because it gives them a chance to invest more without borrowing and to protect their earnings without insurance (Rosenzweig and Wolpin 1993; Bianchi and Bobba 2013). However, entrepreneurs may also rely on family and friends when they decide to start a business (Persson and Lee 2016). In this case, their decision is not as affected by their cash benefit as it is by the disposable income in their network. By raising the income of family and friends, entrepreneurs will be more willing to take risks. Therefore, while the size of the direct effect reveals the role of financial

constraints, the indirect effect reveals the role of the opportunity cost in an existent network.

Some studies have already attempted to assess the indirect effect that cash transfers have on the whole community.³ For instance, Angelucci and De Giorgi (2009) find that non-poor households are also affected by PROGRESA/*Oportunidades* in rural villages in Mexico. They suggest that these households increase food consumption by receiving private transfers from program participants and reducing their precautionary savings. In another study, Bandiera et al. (2009) assess the effect of asset transfers in Bangladesh. They show that this program has indirect effects on time allocation in risk-sharing networks and on durable consumption in family networks.

Although formal entrepreneurship is a rare event among households, the estimation of direct and indirect effects is possible because *Bolsa Família* is a widespread program that was gradually introduced in many different areas in Brazil. The main identification issue is that the individual assignment is not random. Yet we demonstrate that this is not a concern as long as the selection of participants is not related to the overall amount of transfers received in the entire municipality. Namely, the eligibility of households only affects the local distribution of benefits.

In the case of *Bolsa Família*, the total number of transfers per municipality is considered given because, from 2003 to 2007, its growth was strongly driven by municipal quotas (Bastagli 2008; Barros et al. 2008). These quotas were defined by the estimated number of poor households in 2001. Even though this number is related to actual poverty, it is also subject to error. Moreover, the poverty headcount in some municipalities changed from 2001 to 2003, before the quotas were actually applied. Controlling for actual poverty, the municipal quotas are used as an instrument to verify whether the local growth of the program is endogenous.

Instead of comparing participants and non-participants in the same municipality, the overall effect is estimated simply by comparing municipalities over time. The direct and indirect effects, however, are

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

³See Bobonis and Finan (2009), Lalive and Cattaneo (2009), Angelucci et al. (2009, 2010), and Bobba and Gignoux (2019). See also Crépon et al. (2013), and Beegle et al. (2017) on the indirect effect of labor market policies, and Miguel and Kremer (2004) and Kremer and Miguel (2007) on the spillovers of health programs.

calculated by a two-step procedure. First, we estimate the indirect effect of program coverage on non-participants and test whether this effect is linear and hence equal to the indirect impact on participants. As long as this hypothesis is not rejected, estimating the direct response does not require individual-level randomization of the treatment, unlike Duflo and Saez (2003). The estimated indirect effect is used to calculate the selection bias of the estimated direct effect. This empirical strategy allows me to ignore selection within municipalities and, based on a verifiable assumption, decompose the overall effect of the program on eligible individuals.

The findings suggest that the proportion of entrepreneurs among less-educated men has grown by 10 percent because of the *Bolsa Família* program, which had covered about 20 percent of households by 2006. 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 percent. This drawback seems to be induced by households' risk of losing the benefit when their earned income increases.⁴ Informal self-employment, on the other hand, increases among recipients, which is consistent with the view of self-employment as a way to manipulate taxable income (Saez 2010; Chetty et al. 2013). We also find that the positive spillover happens exclusively in small towns. In dense cities, the negative direct effect on entrepreneurial activity is significantly larger than the indirect effect.

The contribution of this study is twofold. First, it suggests individual liquidity constraints are not as important as aggregate constraints in hindering entrepreneurship among the poor. Studies that find a relationship between liquidity shocks and entrepreneurship (e.g., Gertler et al. 2012; Bianchi and Bobba 2013; Bandiera et al. 2017) often exploit interventions that are

clustered at the village level. In this case, the estimated effect should be viewed as the sum of individual and local responses (Hudgens and Halloran 2008). What is usually interpreted as an individual shock, which lessens liquidity constraints, may actually be a locally aggregate shock, which affects the entire village. This misconception explains, for instance, why microcredit programs usually have low take-up, crowd out informal lending, and are not so effective in terms of business creation (Banerjee et al. 2015b).

The second main contribution of this study relates to the impact of cash transfer programs on income-generating activities. Although these programs are only intended to alleviate poverty in the short run and improve children's opportunities in the long run, they are also found to encourage the creation of small enterprises. Nevertheless, since this positive impact is not on the individual but on the economy, it tends to be more important for less dynamic areas (i.e., small towns). Even though the poor in large cities are also entitled to the benefit, the negative response from beneficiaries outweighs the potential spillovers.

The remainder of the paper is organized as follows. Section 2 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 3 details the identification strategy for the overall effect, as well as for the indirect and direct effects. Section 4 presents the main empirical findings, whereas Section 5 presents tests for potential mechanisms, including increasing private transfers, demand shocks, and confounding factors. Section 6 concludes the paper.

2 Program and data description

In this section, we outline the main characteristics of the *Bolsa Família* program, as well as the panel data used in the analysis. Most important, we describe how the growth of this program is closely related to the municipal quotas, making it less likely to be driven by economic opportunities and pork barrel politics at the local level. Furthermore, we 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.

⁴This finding is consistent with the direct effect of *Bolsa Família* on informality presented by de Brauw et al. (2015), but it challenges previous studies that find no significant effects on labor supply (Oliveira et al. 2007; Tavares 2008; Foguel and Barros 2010; Ferro et al. 2010; Teixeira 2010).

2.1 The *Bolsa Família* program

In Brazil, the first Conditional Cash Transfer (CCT) programs 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 program, 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* program, merging all these previous programs into one with the standardization of eligibility criteria, benefit values, information systems, and executing agency. The program also brought in a gradual expansion of CCTs in Brazil, from 5.1 million families in December 2002 to 11.1 million families in October 2006. The target number of 11 million was calculated based on the estimated number of poor families according to the PNAD in 2001.

In 2006, extremely poor families with no children, whose per capita monthly income was below US\$38, and poor families with children up to 15 years old or pregnant women, whose per capita monthly income was below US\$76, were eligible for the program. The monthly benefit was composed of two parts: US\$38 for extremely poor families regardless of the number of children, and US\$11 per child, up to three children, for poor families.⁵ 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 US\$38 transfer is considered unconditional.

Families that receive the benefit can be dropped from the program not only in case of not complying with the conditionalities, but also when their per capita income becomes greater than the eligibility cut-off point. During the period covered by this study, whenever it was found that the household per capita income had been above the eligibility threshold, the family would be excluded from the payroll. Moreover, families are required to update their records 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 workers' salaries from the Ministry of Labor 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 cutoff between October 2008 and February 2009. From this total, 451,021 households had their benefit canceled. From cross-checking its databases, the government had canceled the benefit of more than 1 million households between 2004 and 2008, which represents about 40% of the total number of withdrawals.

2.2 Program's targeting

To identify poor families around the country, the 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. Hence, each municipality has a limited number of benefits to be distributed, which is given by the estimated number of poor households.

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 program should have gradually grown across municipalities from 2003 to 2006.

Figure 1 confirms that the number of benefits per municipality depended heavily on the municipal quotas. In 2004, when the program was still expanding and had not reached the cap in most municipalities, the quotas explain 78% of the municipal coverage. In 2006, when the program reached its target, the relationship is even stronger and close to the 45-degree line. Almost 92% of the distribution of transfers across municipalities is explained by the target coverage, measured five years earlier. In Section 4.1, we show

⁵In 2004, the extreme poverty line for the program was US\$33, the poverty line was US\$66, and the value of the benefit per child was US\$10.

Fig. 1 Relationship between program coverage and municipal quotas. Program coverage is measured by the proportion of households participating in the program. Target coverage is the

municipal quota divided by the number of households. Each point represents a municipality. Regressions are weighted by the number of households per municipality

how the quotas still explain the actual coverage, after controlling for individual characteristics and poverty level.

2.3 Data

2.3.1 Panel sample

The main data source is PNAD. This survey, which collects a broad set of information on demographic and socio-economic characteristics of households, included a special questionnaire on cash transfer programs in 2004 and 2006. This questionnaire asked whether any member of the household was a beneficiary of each cash transfer program that was in place at the time of the survey. Henceforth, we consider as *Bolsa Família* all previous programs that had a similar goal and design (e.g., *Bolsa Alimentação*, *Cartão Alimentação*, *Bolsa Escola*, and PETI).

In addition to these two survey years, we use the 2001 PNAD as a baseline. Although the *Bolsa Família* program had not taken place yet and the other cash transfer programs were not a significant size before 2003, we identify those households receiving cash transfers using the typical-value method developed by Foguel and Barros (2010). This method basically matches parts of household income, under the entry of “other incomes,” with typical values transferred by

each program. The 2002 PNAD is also used to estimate local poverty and changes in entrepreneurship before 2003.

PNAD is a cross-sectional survey, so it does not interview the same household twice. Thus, we 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.⁶ 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.

2.3.2 Defining entrepreneurship

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

⁶ A census tract is a neighborhood that has between 250 and 350 households in urban areas, 150 and 250 households in suburban areas, 51 and 350 households in informal settlement areas, 51 and 250 households in rural areas, and at least 20 households in indigenous areas (IBGE 2000).

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).

We also exclude public servants, people with higher education, and employers with more than five employees from the sample. Even though 6% of public servants were participating in the program in 2006, they are less likely to change occupation due to the stability of their job. The last two groups were excluded because only 1% 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.⁷

Following Blanchflower (2000) and Blanchflower et al. (2001), we classify as entrepreneurs those who are either self-employed or small-business owners. For Brazil in particular, small entrepreneurs are those who are either self-employed, excluding domestic workers, or business owners with at most five employees (Fajnzylber et al. 2011; Monteiro and Assunção 2012).⁸ 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, we 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.

Based on these categories, entrepreneurs earn on average 77% more per hour worked than the informal self-employed, and 45% more than formal employees. These earnings differentials are also identified at any quantile, confirming that entrepreneurship is more rewarding than other types of occupation (see Fig. 4 in the Appendix).

2.3.3 Municipal quotas and program coverage

The municipal quotas, as well as the number of benefits per municipality, are obtained from the Ministry of Social Development and Fight Against Hunger (MDS). To calculate the target and actual coverage, these numbers are divided by the number of households per municipality in 2000, according to the Demographic Census. The match between MDS data and PNAD is possible only for large cities, which can be identified in PNAD based on their size, metropolitan status and state. These cities account for 51% of the final sample. The other half is matched through clusters of municipalities, also defined by population size, metropolitan status and state. Out of 788 municipalities sampled by PNAD in 27 states, we are able to create 178 clusters.

To give more variability within those clusters, we predict the target and actual coverage per municipality as follows. First, we calculate the program coverage in 2001, 2004, and 2006 and the poverty headcount in 2001 per municipality using PNAD. Then, we take the difference between these variables, $\bar{d}_{vc}$, and the official rates from MDS, $\tilde{d}_c$. While the rates from MDS are more accurate but have less variance, the rates from PNAD are noisier but have more variance. Then, the difference is estimated as a function of the official

⁷The exclusion of these employers reduces the sample by 1%, with no implication for the results.

⁸In 2008, the Brazilian Government officially introduced the concept of micro-entrepreneur (MEI) in the legislation. To be classified as MEI, the entrepreneur cannot employ more than one worker, who must receive the minimum wage.

Table 1 Descriptive statistics

	2001		2004		2006	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Outcomes						
Entrepreneur	0.069	0.254	0.068	0.252	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	0.131	0.019	0.138	0.021	0.143
Formal employee	0.431	0.495	0.461	0.499	0.482	0.500
Informal employee	0.152	0.359	0.147	0.354	0.140	0.347
Informal self-employed	0.206	0.405	0.192	0.394	0.177	0.381
Jobless	0.141	0.348	0.131	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	6.1	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	0.387	0.838	0.368
Primary education	0.445	0.497	0.508	0.500	0.545	0.498
High school	0.247	0.431	0.304	0.460	0.347	0.476
Number of kids	1.380	1.280	1.280	1.240	1.210	1.200
Number of elderly	0.193	0.493	0.201	0.501	0.209	0.510
Migrant—last 5 years	0.057	0.231	0.114	0.318	0.116	0.321
Municipality variables						
Program coverage	0.047	0.088	0.156	0.109	0.201	0.141
Target coverage					0.270	0.163
Poverty headcount in 2002					0.254	0.176
Poverty headcount	0.257	0.175	0.248	0.169	0.192	0.147
log of population	12.9	1.38	13.0	1.37	13.0	1.37
log of population density	5.44	2.37	5.42	2.38	5.41	2.38
Elementary enrollment rate	0.929	0.065	0.939	0.060	0.952	0.049
Primary enrollment rate	0.726	0.161	0.776	0.131	0.795	0.123
High school enrollment rate	0.425	0.182	0.505	0.188	0.524	0.170
Child mortality	12.7	21.3	11.0	22.2	9.8	17.2
Coverage of sewer system	0.484	0.354	0.514	0.363	0.514	0.357
Prop. of house owners	0.693	0.107	0.699	0.103	0.695	0.103
Number of observations	41,663		43,105		44,802	

rate, population size, pop_v , number of municipalities in the cluster, n_c , and all pairwise interactions:

$$(\bar{d}_{vc} - \bar{d}_c) = \psi_0 + \psi_1 \bar{d}_c + \psi_2 pop_v + \psi_3 n_c + \psi_4 \bar{d}_c pop_v + \psi_5 \bar{d}_c n_c + \varepsilon_{vc} \quad (1)$$

Finally, the predicted rate is equal to the official rate plus the predicted difference:

$$\hat{d}_{vc} = \begin{cases} \bar{d}_c & \text{if } n_c = 1 \\ \bar{d}_c + \hat{\psi}_0 + \hat{\psi}_1 \bar{d}_c + \hat{\psi}_2 pop_v + \hat{\psi}_3 n_c + \hat{\psi}_4 \bar{d}_c pop_v + \hat{\psi}_5 \bar{d}_c n_c & \text{if } n_c > 1 \end{cases}$$

2.3.4 Descriptive statistics

Table 1 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 to 7.2%. 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.

With the creation of *Bolsa Família* in 2003, the percentage of individuals receiving cash transfers (program coverage) went from 4.7% in 2001 to 19.4% in 2006. A simple difference-in-difference analysis indicates that the rising entrepreneurship rate is associated with the increasing coverage. Since the program is targeted at the poor, who are less likely to be entrepreneurs, the relationship between program size and entrepreneurship is indeed negative across municipalities. However, Fig. 2 also shows that this relationship is flatter in 2006, which suggests that entrepreneurship has grown more in areas with higher program coverage.

Nonetheless, that relationship might have already been changing before the program started. In this case, the curve in 2002 would be flatter than the one in 2001. Figure 2 reveals that these curves are rather parallel, indicating that entrepreneurial rates in poor and rich municipalities had followed similar trends up to 2003.

An important way in which the program 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 program. In Table 1, we observe that this rate rose from 4.3% in 2001 to 7.7% in 2006.

3 Empirical strategy

The empirical strategy consists of a difference-in-difference model estimated using a three-period dataset. In this model, the first difference is across municipalities, which vary in terms of future coverage. The systematic distinctions among them are controlled by fixed effects. The second difference is between before (2001), during (2004), and after (2006) the expansion of the program. As discussed above, the program coverage has been strongly driven by the municipal quotas, so it grew independently from idiosyncratic changes in the entrepreneurship rate across municipalities. According to Proposition 1, presented below, this condition is sufficient for the identification of the overall effect of the program using a model with municipality-level fixed effects. Moreover, 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.

Fig. 2 Relationship between entrepreneurship and program coverage. Entrepreneurship rate is measured by the proportion of small entrepreneurs per municipality. ‘2006 - 01 difference’ is the difference between entrepreneurship rates in 2006 and 2001 per municipality. Program coverage is measured by the proportion of households participating in the program in 2006. Municipalities where the program coverage was greater than 5 p.p. in 2001 are not included

Furthermore, we introduce a formal test to verify whether the indirect effect is different for individuals who receive and do not receive the benefit (Proposition 2). This test replaces the individual-level randomization 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.⁹

3.1 Fixed-effect model

Let y_{ivt} be the decision of individual i living in municipality v at time t to be an entrepreneur. This decision is determined by a linear model:

$$y_{ivt} = \beta_0 + \beta_1 d_{ivt} + \beta_2 \bar{d}_{vt} + \mu_v + \mu_t + u_{ivt}, \quad (2)$$

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 (program coverage).

Following Eq. (2), coefficient β_1 is defined as 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. 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 program 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 eligible individual.

Most studies that compare treated households in covered villages and untreated households in uncovered villages (e.g., evaluations of PROGRESA/Oportunidades in Mexico) actually estimate the overall effect of the intervention, τ . On the other hand, studies that compare individuals in the same cities or villages (e.g., Gasparini et al. 2009, Karlan and Zinman 2010, Blattman et al. 2014) are only estimating the direct effect, β_1 . It is important to stress that recipients are as subject to indirect effects as other individuals in this model.

As explained above, the coverage of *Bolsa Família* at the municipality level has strongly depended on the

previously estimated poverty headcount. Therefore, it is reasonable to assume that the program coverage, $\bar{d}_{vt}$, is independent of the error term, u_{ivt} , once controlling for municipality fixed effects. Accordingly, the consistency of difference-in-difference estimates depends on the following identification assumption.

Assumption 1 (Partial Aggregate Independence, PAI) In Eq. 2,

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

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

Lemma 1 (Selection Bias) *If the PAI assumption holds, then the least squares estimators for β_1 and β_2 have the following asymptotic property:*

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

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

Proposition 1 (Consistent estimator for the overall effect) *Consider the following equation:*

$$y_{ivt} = \beta_0 + \tau \bar{d}_{vt} + \mu_v + \mu_t + u_{ivt} \quad (3)$$

⁹All proofs are presented in the Appendix.

If Eq. (2) is the true model, then the least squares (LS) estimator for τ in Eq. (3) is the sum of the LS estimators for β_1 and β_2 in Eq. (2):

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

Moreover, if the PAI Assumption holds, then the LS estimator for τ in Eq. (3) is consistent:

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

Proposition 1 implies that the overall effect of the program, τ , can be consistently estimated if we just omit d_{ivt} in Eq. (2). Accordingly, we estimate Eq. (3) using a three-period data, with the standard errors clustered by municipality. For the sake of robustness, we also include individual and local control variables in the main model and estimate another model with census-tract fixed effects. If the self-selection bias is proportional to the program size, $\bar{d}_{vt}$, violating the PAI assumption, then estimates conditional on census-tract fixed effects should be different (less biased) than those conditional on municipality fixed effects.

3.2 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 labor market. To consider only changes predicted by the municipal quotas, rather than changes caused by idiosyncratic behavior, we 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 program's initial design.

Assumption 2 (Instrumental variable assumption) *Given the ex ante poverty level, p_{vt} , and unobserved fixed variables, the municipal quota, q_{vt} , is orthogonal to u_{ivt} .*

The municipal quota is proxied by the interaction between the target coverage in 2006 (calculated in 2001), q_{v2} , and year dummies, whereas the ex-ante poverty level is proxied by the interaction between the

poverty headcount in 2002, p_{v0} , and year dummies. Then the equation for the program coverage, $\bar{d}_{vt}$, is:

$$\begin{aligned} \bar{d}_{vt} = & \gamma_0 + \gamma_1 q_{v2} \cdot I(t=2004) + \gamma_2 q_{v2} \cdot I(t=2006) \\ & + \gamma_3 p_{v0} \cdot I(t=2004) + \gamma_4 p_{v0} \cdot I(t=2006) \\ & + \theta_v + \theta_t + e_{ivt}. \end{aligned} \quad (4)$$

The IV assumption implies that the residual relationship between occupational choice and municipal quota does not change over time, unless by means of the growth of the program. Note that the constant relationship between occupational choices and the initial poverty headcount is controlled by the fixed effect, θ_v , and changes in this relationship is controlled by the interaction between p_{v0} and year dummies. The remaining variance in municipal quotas should be driven by the fact that they were outdated in 2004 and 2006 and subject to error.

Since the instrument is defined at the municipality level, the predicted change in the intervention also happens at the municipality level. Therefore, if the program coverage, $\bar{d}_{vt}$, is replaced by the individual treatment, d_{ivt} , in Eqs. (3) and (4), the IV estimator will remain the same.¹⁰ This result reinforces the concept of *overall effect* defined above. Once the instrument is defined at the cluster level (e.g., randomization of treated villages), the comparison between treated and untreated individuals also happens 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. 2015). On the other hand, the estimand cannot be interpreted as the result of a direct effect only.

3.3 Separating direct and indirect effects

Unfortunately, estimating Eq. (3) does not reveal whether the effect of program size comes from either a direct response of individuals receiving the transfer or an indirect effect that also affects individuals outside the program. 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 $d_{ivt} = 0$):

$$\begin{aligned} y_{ivt|(d=0)} = & \beta_{0,(d=0)} + \tau_{(d=0)} \bar{d}_{vt} \\ & + \mu_{v,(d=0)} + \mu_{t,(d=0)} + u_{ivt|(d=0)} \end{aligned} \quad (5)$$

¹⁰See Section A in the online Appendix.

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

Proposition 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 nonlinear.*

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

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

Accordingly, inference on the direct effect is made using seemingly unrelated regressions (SUR) of Eqs. (2) and (5).

4 Main results

4.1 First-stage regressions

Before moving to the main results, we first analyze the relationship between program coverage and municipal quotas, shown in Table 2. Even controlling for poverty in 2002 and changes in the relationship between poverty and program coverage over time, the municipal quotas are found to have a strong correlation with coverage. As the program grows, the relationship with quotas becomes stronger, but the relationship with initial poverty becomes weaker. Moreover, other social indicators, such as enrollment rate, child mortality, coverage of sewer system and proportion of home owners, do not have a significant relationship with the treatment.

The way that treatment is defined is also critical. After controlling for local fixed effects, all individual

covariates are balanced with respect to program coverage. On the other hand, covariates remain heavily unbalanced with respect to the individual benefit. This finding illustrates the essence of the PAI assumption. Within municipalities, the distribution of benefits is far from random, which makes the individual assignment to treatment endogenous even if we control for local fixed effects. In this setting, however, the individual assignment does not affect the distribution of benefits across municipalities.

4.2 Overall effect

This section presents and discusses the overall effect of *Bolsa Família* on the probability of being an entrepreneur. Table 3 shows the estimates obtained using eight different models. Model (1), which does not include fixed effects and does not control for poverty, suggests that the relationship between entrepreneurship and program coverage is small and negative. Although this model includes covariates such as race, age and education, results tend to be biased because the program targets the poorest municipalities. After controlling for initial poverty, in model (2), the estimated relationship becomes positive and significant. This result holds for all models that include local fixed effects.

Models (3), (4) and (5) include fixed effects in different levels — i.e., municipality and census tract. As predicted by Proposition 1, which states that the within-municipality program assignment does not affect estimates for the overall effect, the coefficient does not change if we 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% and the current coverage of 19%, the program might be responsible for an increase of 10% in the entrepreneurship rate, keeping everything else constant.¹¹

In models (6) and (7), the PAI assumption is relaxed and the local coverage is instrumented by the municipal quota. The estimated effect is higher in these models, but not significantly different. Model (7) also includes the ex-ante poverty and social indicators that

¹¹This effect happens mostly 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. See Table B3 in the online Appendix.

Table 2 First-stage regression, relationship between quota and program coverage

	Program coverage, $\bar{d}$					Ind. benef., d
	(1)	(2)	(3)	(4)	(5)	
Quota	0.044 (0.036)					
Quota $\times$ year = 2004 (<i>a</i>)	0.508*** (0.036)	0.400*** (0.023)	0.400*** (0.023)	0.513*** (0.035)	0.516*** (0.035)	0.095** (0.045)
Quota $\times$ year = 2006 (<i>b</i>)	0.713*** (0.043)	0.616*** (0.030)	0.616*** (0.030)	0.732*** (0.044)	0.734*** (0.044)	0.114** (0.051)
Poverty in 2002	0.179*** (0.033)					
Poverty in 2002 $\times$ year = 2004	−0.119*** (0.034)			−0.120*** (0.033)	−0.123*** (0.034)	0.268*** (0.042)
Poverty in 2002 $\times$ year = 2006	−0.145*** (0.035)			−0.128*** (0.034)	−0.130*** (0.034)	0.374*** (0.042)
Age ($\times 10$)	0.004 (0.004)	0.003 (0.002)	0.003 (0.003)	0.003 (0.002)	0.003 (0.002)	0.135*** (0.021)
Aquared age ($\times 100$)	−0.001 (0.001)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	−0.018*** (0.003)
White	0.008*** (0.001)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	−0.023*** (0.002)
Black	−0.003* (0.002)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)	0.007* (0.004)
Married	0.000 (0.001)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.058*** (0.005)
Elementary education	−0.003*** (0.001)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	−0.036*** (0.004)
Primary education	−0.001* (0.001)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	−0.037*** (0.003)
High school	0.000 (0.001)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	−0.023*** (0.003)
log of population	−0.009*** (0.002)	0.029* (0.017)	0.029* (0.017)	0.029* (0.017)	0.029* (0.017)	0.066*** (0.024)
Elementary enrollment rate				0.035 (0.027)	0.035 (0.027)	0.057 (0.038)
Primary enrollment rate				−0.017 (0.015)	−0.018 (0.015)	−0.005 (0.022)
High school enrollment rate				0.010 (0.011)	0.010 (0.011)	−0.007 (0.016)
Child mortality ($\times 1000$)				−0.029 (0.059)	−0.029 (0.059)	−0.091 (0.089)
Coverage of sewer system				−0.006 (0.014)	−0.006 (0.014)	0.011 (0.021)
Prop. of home owners				−0.022 (0.024)	−0.021 (0.024)	−0.019 (0.036)

Table 2 (continued)

	Program coverage, $\bar{d}$					Ind. benef., d
	(1)	(2)	(3)	(4)	(5)	
Municipality fixed-effects	No	Yes	No	Yes	No	No
Census tract fixed-effects	No	No	Yes	No	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	129,080	129,080	129,080	129,080	129,080	129,046
test $(a) = (b) = 0$, F -stat	142.0***	249.7***	250.6***	150.2***	150.9***	3.43**

***, **, *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 3 Overall effect of cash transfers on entrepreneurship

	Decision of being a small entrepreneur							
	OLS		FE			IV		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
program coverage, $\bar{d}$	-0.012 (0.009)	0.037*** (0.014)	0.047*** (0.016)	0.038** (0.018)	0.039** (0.018)	0.063*** (0.023)	0.067* (0.034)	
individual benefit, d								0.063*** (0.024)
Poverty in 2002		-0.059*** (0.010)						
Poverty in 2002 $\times$ year = 2004		0.012 (0.012)		0.014 (0.012)	0.011 (0.012)		0.008 (0.014)	
Poverty in 2002 $\times$ year = 2006		0.011 (0.012)		0.011 (0.013)	0.008 (0.013)		0.004 (0.018)	
Municipality fixed-effects	No	No	Yes	Yes	No	Yes	Yes	Yes
Census tract fixed-effects	No	No	No	No	Yes	No	No	No
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Social indicators	No	No	No	No	No	No	Yes	No
Number of observations	129,080	129,080	129,080	129,080	129,080	129,080	129,080	129,046
Underidentification LM test						80.1***	94.8***	110.3***
Weak identification F -test						249.7***	150.2***	195.0***
Overidentification J -test						1.157	0.738	0.105

***, **, *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. Columns (1) and (2) present the regression coefficients obtained by ordinary least squares (OLS). Columns (3), (4), and (5) present the fixed-effect regressions (FE) obtained using the within-group method. Columns (6), (7), and (8) present the fixed-effect, Instrumental-variable regressions (IV) with “program coverage” and “individual benefit” instrumented by the interactions between municipal quota and year dummies

Table 4 Nonlinear, indirect, and direct effects of cash transfers on entrepreneurship

	Decision of being a small entrepreneur					
	O		FE		IV	
	(1)	(2)	(3)	(4)	(5)	(6)
program coverage, $\bar{d}$	0.067* (0.039)	0.066 (0.040)	0.062*** (0.018)	0.048** (0.021)	0.078*** (0.024)	0.062* (0.037)
Squared coverage, $\bar{d}^2$	−0.041 (0.061)	−0.066 (0.073)				
Individual benefit, d			−0.037*** (0.005)	−0.036*** (0.007)	−0.043*** (0.006)	−0.037*** (0.010)
Poverty in 2002 $\times$ year = 2004		0.015 (0.012)		0.013 (0.013)		0.009 (0.016)
Poverty in 2002 $\times$ year = 2006		0.017 (0.015)		0.018 (0.016)		0.011 (0.021)
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Individual covariates	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.—all sample	129,080	129,080	129,046	129,046	129,046	129,046
No. of obs.— $d = 0$			113,088	113,088	113,088	113,088
Overidentification J -test					2.68	1.44

***, **, *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. Columns (1) and (2) present the fixed-effect model with quadratic effect of program coverage. Columns (3)–(6) present the estimates of the indirect effect (program coverage) and direct effect (individual benefit), with bias correction given by Lemma 1, estimated using seemingly unrelated regressions (SUR). FE columns show fixed-effect regressions obtained using the within-group method. IV columns show fixed-effect, instrumental-variable regressions with “program coverage” instrumented by the interactions between municipal quotas and year dummies

had changed over time, such as child mortality, sewer coverage, share of house owners, and school enrollment rates. Although the standard error increases, the estimated effect does not change. Thus, the overall change in entrepreneurship does not seem to be driven by initial poverty or other socioeconomic improvements.

In model (8), 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 program on participants.

The estimated overall effect between 4 and 6 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 programs, which do not look to increase entrepreneurship at the extensive

margin (Banerjee et al. 2015a; Angelucci et al 2015; Crépon et al 2015).¹² However, it is half as large as the Targeted Ultra-Poor Program's in Bangladesh (Bandiera et al. 2017) and the Youth Opportunities Program's in Uganda (Blattman et al. 2014). These two programs, nevertheless, are particularly intended to promote entrepreneurship, with the transfer being conditional on productive investments.

4.3 Direct and indirect effects

To estimate the indirect effect of the program, we first have to verify whether it is constant or not. According to Proposition 2, if the overall effect is linear, then the indirect effect of the program is similar for participants and non-participants. The first columns of Table 4 show that the coefficient of squared coverage is, in fact, not significant. Since the hypothesis of linear overall effect is not rejected, we estimate the

¹²Exceptions are the programs studied by Attanasio et al. (2015) and Augsburg et al. (2015).

Table 5 Indirect and direct effects on entrepreneurship by population density

	Entrepreneurial Decision			
	FE		IV	
	(1)	(2)	(3)	(4)
Regression coefficients				
program coverage, $\bar{d}$	0.039* (0.022)	0.017 (0.031)	0.060** (0.026)	0.026 (0.048)
$\bar{d} * \log \text{ pop. density}$	-0.010** (0.004)	-0.013 (0.012)	-0.007* (0.004)	-0.014 (0.017)
Individual benefit, d	-0.046*** (0.005)	-0.048*** (0.007)	-0.053*** (0.007)	-0.045*** (0.013)
$d \times \log \text{ pop. density}$	-0.000 (0.001)	-0.002 (0.003)	-0.000 (0.002)	0.001 (0.005)
Effect on the bottom 20% of density				
program coverage, $\bar{d}$	0.062*** (0.017)	0.049*** (0.021)	0.077*** (0.023)	0.060 (0.038)
Individual benefit, d	-0.045*** (0.005)	-0.044*** (0.008)	-0.053*** (0.006)	-0.048*** (0.011)
Effect on the top 20% of density				
program coverage, $\bar{d}$	0.012 (0.031)	0.021 (0.059)	0.040 (0.033)	-0.014 (0.087)
Individual benefit, d	-0.047*** (0.007)	-0.053*** (0.011)	-0.054*** (0.010)	-0.041* (0.024)
Municipality fixed-effects	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Individual covariates	Yes	Yes	Yes	Yes
No. of obs.—all sample	129,046	129,046	129,046	129,046
No. of obs.— $d = 0$	113,088	113,088	113,088	113,088
Overidentification J -test			10.49**	4.94

***, **, *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. Columns (1) and (2) have the results from the fixed-effect model (FE) estimated using the within-group method. Columns (3) and (4) have the results from the fixed-effect, instrumental-variable regression (IV) with “program coverage” instrumented by the interactions between municipal quota and year dummies. Columns (2) and (4) include poverty in 2002 times year dummies as control. In each column, coefficients are estimated using seemingly unrelated regressions (SUR) with bias correction given by Lemma 1

indirect effect of the program using only the sample of non-participants. In all models, these estimates are greater than the overall effect discussed above. Then the direct response to the program should be negative, after adjusting for the bias according to Lemma 1.

The results indicate that, on one hand, cash transfers reduce the probability of participants starting their own business by 3–4 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 program size tends to raise the entrepreneurship rate of poor individuals by 0.5–0.8 p.p. Because of this positive indirect effect, the net

impact of cash transfers on entrepreneurship is also positive.

This difference 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.¹³ In Section 5, we will find that the

¹³The negative direct effect is not likely to be driven by conditionalities on education because participants with no children also reduce entrepreneurial activity. See Table B4 in the online Appendix.

indirect response is related to the promotion of informal financing mechanisms among poor households.

4.4 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, we 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, we match clusters of municipalities defined by their size, metropolitan status, and state.

The findings, presented in Table 5, 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 program is not effective in promoting entrepreneurship in large cities, because the negative direct response from recipients offsets the small indirect effect. The program's impact is concentrated in small villages, which probably have less aggregate liquidity.

5 Potential mechanisms

5.1 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 (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, we 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, we calculated the probability of participating households to lend or transfer money to another household unit. Figure 3 shows that program participants are indeed more likely to make transfers to other households in each section of income distribution. On average, participants are about 40% 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

Fig. 3 Probability of transferring or lending money to another household. Estimates made using the Brazilian Consumer Expenditure Survey (POF) 2008–2009. The bars represent the proportion of households that have transferred or lent money to another household in the last 90 days. The dashed line represents the proportion of households receiving a conditional cash transfer. Income deciles are calculated using household per capita income

confirms the presumption that the cash transfer flows in the community through private transfers. Moreover, assuming that program 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 labor earnings, retirement benefits, other pensions, rental earnings, and social programs. Table 6 presents the

estimated effect of program coverage on the probability of non-participants receiving “other transfers.” According to the results in columns (1) and (2), a 10 p.p. increase in local coverage raises this probability by 1.1–1.6 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. (2015) and Karlan and Zinman (2010), who show that access to credit increases mutual assistance between households.

While individuals with better job opportunities may use these transfers as a safety net, individuals with fewer job opportunities may use them to start

Table 6 Indirect effect of cash transfers on private transfers and entrepreneurship

	Receiving private transfers			Entrepreneurial decision	
	FE (1)	IV (2)	FE (3)	FE (4)	IV (5)
program coverage, $\bar{d}$	0.111*** (0.021)	0.160*** (0.026)		0.046** (0.019)	0.042 (0.026)
$\bar{d} \times$ effect on private transfers				0.450*** (0.162)	0.617** (0.270)
$\bar{d} \times$ jobless			0.297*** (0.037)		
$\bar{d} \times$ informal			0.116*** (0.021)		
$\bar{d} \times$ formal			0.031 (0.023)		
$\bar{d} \times$ entrepreneur			0.011 (0.031)		
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Individual covariates	Yes	Yes	Yes	Yes	Yes
Number of observations	112,976	112,976	112,976	113,054	113,054
Underidentification LM test		86.48***			68.97***
Weak identification <i>F</i> -test		301.6***			180.7***
Overidentification <i>J</i> -test		1.61			0.00

***, **, *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 program coverage interacting with 2001 municipality characteristics. The coefficient of interaction between “effect on private transfers” and “program coverage” represents how much the effect of program coverage on entrepreneurial decision changes if its predicted effect on private transfers increases. FE columns show fixed-effect regressions obtained using the within-group method. IV columns show fixed-effect, instrumental-variable regressions with “program coverage” instrumented by the interactions between municipal quotas and year dummies

their own business. Since we do not know if current entrepreneurs received other transfers before, we cannot conclude that these transfers are actually invested. The only conclusion that can be drawn is that the effect on receiving other transfers is the highest among those who need them most. Namely, the effect is significantly higher for the jobless, followed by informal workers (see column (3) in Table 6).

To verify whether the indirect effects on entrepreneurship and private transfers are related, we include the interaction between program coverage and the predicted effect on private transfers in the regression (columns (4) and (5) in Table 6). 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 enrollment rates and higher labor informality. Using the predicted effect on private transfers, we 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.

5.2 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 labor 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, we also estimate the indirect effect of cash transfers on these outcomes.

The first two columns of Table 7 confirm that the program size has no significant effect on the probability of well-educated men becoming entrepreneurs. Thus we cannot say the program has encouraged the creation of local businesses in general. The effect on

Table 7 Indirect effect of cash transfers on other entrepreneurs, employment and wages

	High-educated entrep. decision		Low-educated employment		Low-educated employees' wages	
	FE (1)	IV (2)	FE (3)	IV (4)	FE (5)	IV (6)
program coverage, $\bar{d}$	0.028 (0.126)	0.098 (0.158)	−0.024 (0.025)	−0.048 (0.030)	−0.373 (1.019)	−0.553 (1.391)
$\bar{d} \times$ private sector					0.698 (1.006)	0.960 (1.378)
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Individual covariates	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	9,340	9,340	113,054	113,054	58,066	58,066
Underidentification LM test		38.12***		86.42***		88.68***
Weak identification <i>F</i> -test		88.26***		302.1***		73.85***
Overidentification <i>J</i> -test		1.95		0.04		1.94

***, **, *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 “program coverage” instrumented by the interactions between municipal quota and year dummies

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 program has had no effect on their overall employment rate. This result suggests that the program does not affect the demand side of the labor market. It may have just affected the occupational choice on the supply side.

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 program has some influence on local prices. Accordingly, we use wages of less-educated

public employees as a proxy for labor 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 program coverage and the private sector, in the last two columns of Table 7, is not significant.

5.3 Other occupational choices and the eligibility effect

To understand where the marginal entrepreneurs come from, we also investigate the effect of the program on other occupational choices. Besides entrepreneur, the alternatives are jobless, formal employee, informal employee, and informal self-employed. Table 8 presents the direct and indirect effects of the program

Table 8 Indirect and direct effects of cash transfers on other occupational choices

	Entrep.	Jobless	Formal employee	Informal employee	Informal self-emp.
Fixed-effect model					
program coverage, $\bar{d}$	0.062*** (0.018)	0.023 (0.025)	0.032 (0.029)	-0.081*** (0.027)	-0.037 (0.028)
Individual benefit, d	-0.037*** (0.005)	0.030*** (0.009)	-0.040*** (0.012)	0.024** (0.010)	0.023** (0.011)
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Individual covariates	Yes	Yes	Yes	Yes	Yes
No. of obs.—all sample	129,046	129,046	129,046	129,046	129,046
No. of obs.— $d = 0$	113,088	113,088	113,088	113,088	113,088
Instrumental-variable model					
program coverage, $\bar{d}$	0.078*** (0.024)	0.047 (0.03)	-0.002 (0.038)	-0.097*** (0.033)	-0.026 (0.037)
Individual benefit, d	-0.043*** (0.006)	0.032*** (0.012)	-0.037** (0.015)	0.008 (0.014)	0.040*** (0.015)
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Individual covariates	Yes	Yes	Yes	Yes	Yes
No. of obs.—all sample	129,046	129,046	129,046	129,046	129,046
No. of obs.— $d = 0$	113,088	113,088	113,088	113,088	113,088

***, **, *Statistical significant at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses are clustered by municipality. Sample includes only men with high school diploma or less. All coefficients are estimated using seemingly unrelated regressions (SUR). The indirect effect (program coverage) is estimated using the sample of non-participants, whereas the direct effect (individual benefit) is estimated using all sample and bias corrected according to Lemma 1. Fixed-effect models are estimated using the within-group method. In the instrumental-variable models, “program coverage” is instrumented by the interactions between municipal quotas and year dummies

on the probability of being in each one of these categories, vis-à-vis being in any other category.

The estimated indirect coefficients indicate that the program has no significant effect on the proportion of jobless men in the areas covered. The program 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. The strongest indirect effect is on the proportion of informal employees. Assuming that the labor market is segregated, the program 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 program participants, the negative effect on entrepreneurship looks symmetric to the positive effect on the jobless rate. This negative effect could be related to the income effect that unearned income has on labor supply. However, the reduction in labor supply only happens among formal workers (entrepreneurs and documented employees).¹⁴ Thus program participants reduce labor 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.¹⁵

According to the official records of MDS, almost 40% 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 Labor and Employment's dataset, the so-called RAIS.

¹⁴A similar result is found by Gasparini et al. (2009) in Argentina and Amarante et al. (2011) in Uruguay.

¹⁵The direct effects on labor supply in the formal and informal sectors might be distinct due to differences in workers' ability. However, the same pattern emerges in subsamples of individuals with and without a high school diploma. See Table B5 in the online Appendix.

5.4 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 program. In other words, there is no convergence in the entrepreneurship rate across municipalities in Brazil. This convergence could be driven by other social programs or by the process of credit expansion. In the main results shown above (in Tables 3 and 4), we already include the interaction between initial poverty rate and year dummies. Once again, the estimated effect of *Bolsa Família* barely changed. Most of the endogeneity in the distribution of benefits across municipalities—but not between individuals—is controlled for using fixed effects, in addition to the instrumental variable (municipal quotas). Thus, poverty itself does not explain the growth in entrepreneurship unless by means of the growth of the program.

Another placebo test, presented in Table 9, is of changes before the program expanded in 2003. Columns (1) and (2) show that the growth of the program from 2001 to 2006 does not explain changes in entrepreneurship between 2001 and 2002. As regards the increasing access to credit, 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.¹⁶ Despite this small overlap, columns (3), (4) and (5) confirm that the estimated effect between 2001 and 2004 is still significantly positive.

Although the credit expansion started in the late 2000s, other microcredit programs have been in place since the 1990s. To test whether the results are driven by microcredit programs, we exclude from the sample the region where the largest and most significant program was introduced. The *CrediAmigo* program, created in 1997, is considered the largest microfinance program in the country, but it covers only municipalities in the Northeast region. Furthermore, the Northeast was the focus of other social programs intended to boost local economies. Columns (6), (7), and (8) of

¹⁶See Fig. 5 in the Appendix.

Table 9 Overall effect of cash transfers on entrepreneurship, robustness analyses

	Decision of being a small entrepreneur							
	2001–2002		2001–2004			excluding Northeast		
	FE (1)	IV (2)	FE (3)	IV (4)	IV (5)	FE (6)	IV (7)	IV (8)
program coverage, $\bar{d}$			0.062*** (0.022)	0.087** (0.038)	0.093* (0.050)	0.076*** (0.028)	0.100*** (0.035)	0.097** (0.044)
$\bar{d}$ in 2001–2006	0.023 (0.016)	0.033 (0.021)						
Poverty in 2002 <i>times</i> year = 2004					–0.003 (0.016)			0.015 (0.021)
Poverty in 2002 $\times$ year = 2006								0.001 (0.025)
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	83,570	83,570	84,391	84,391	84,391	91,514	91,514	91,514
Underidentification LM test		74.08***		78.77***	87.41***		76.28***	90.57***
Weak identification <i>F</i> -test		483.6***		308.7***	211.9***		261.4***	149.4***
Overidentification <i>J</i> -test		0.00		0.00	0.00		2.24	1.54

***, **, *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. Columns (1) and (2) present the relationship between changes in entrepreneurship in 2001–2002 and program growth in 2001–2006. Columns (3)–(5) present the estimates of the overall effect between 2001 and 2004 (excluding 2006). Columns (6)–(8) present the estimates of the overall effect in regions other than the Northeast. FE columns show fixed-effect regressions obtained using the within-group method. IV columns show fixed-effect, instrumental-variable regressions with “program coverage” instrumented by the interactions between municipal quota and year dummies

Table 9 show that the estimated effect on entrepreneurship increases after omitting that region. Thus the results do not seem to be a consequence of the growth in microcredit or other development programs.

6 Conclusion

Designing evaluations to assess direct and indirect effects of public policies is critical to understanding their implications. In the case of a cash transfer program, the identification of spillovers reveals that its impact goes well beyond cash and conditionalities, uncovering the role of inter-household exchanges within the informal economy. Thus, the overall effect, which is often estimated by comparing villages in this type of intervention, is not always driven by the direct response of recipients to the program.

Although the rate of small entrepreneurs has grown 10% due to the CCT programs in Brazil, it does not mean that their recipients could start a business if they were not financially constrained. The cash transfer has actually reduced their willingness to start a formal business in comparison to their neighbors. This negative response is related to increasing informality, which seems to be the best option for program participants to keep receiving both labor earnings and social benefits.

On the other hand, the amount of cash transferred to poor areas has indirectly lifted the creation of new businesses, particularly among those who would be otherwise in the informal sector. Therefore, it was not the increase in household income, or the relief from the financial constraint, that has encouraged these entrepreneurs. A plausible explanation for the indirect effect is the existence of informal risk-sharing

arrangements among the poor. Once the program increases the liquidity in these arrangements, recipients will be more willing to assist other households with their investments.

Since financial constraints do not necessarily hinder the process of business creation,¹⁷ providing grants or access to credit could just crowd out informal financial sources. This may explain why micro-entrepreneurship programs are sometimes not so effective at the extensive margin. To increase their effectiveness, these programs could be built upon the informal system in place by, for instance, including the participation of local micro-investors and group monitoring.

Finally, it is worth stressing that this study has limitations. Although we do not need randomization of the individual benefit to identify direct and indirect effects, the growth of the program across municipalities was not fully random. Its growth may have followed other local initiatives, which we cannot control for. Only a setting in which villages are randomly covered by the program would yield consistent results. Also, the suggested mechanism of indirect effects is just speculative. More detailed information on households' social network and transfers between households would help us to give better answers.

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Appendix. Proofs of Section 3

A.1 Proof of Lemma 1

To simplify the proof, we start with the following within-group version of Eq. (2):

$$y_{ivt}^* = \beta_1 d_{ivt}^* + \beta_2 \bar{d}_{vt}^* + u_{ivt} \quad (\text{A.6})$$

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

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

$$\begin{aligned} \begin{bmatrix} \hat{\beta}_1 \\ \hat{\beta}_2 \end{bmatrix} &= \frac{1}{[S_{d^{*2}} S_{\bar{d}^{*2}} - (S_{d^* \bar{d}^*})^2]} \begin{bmatrix} S_{d^{*2}} & -S_{d^* \bar{d}^*} \\ -S_{d^* \bar{d}^*} & S_{\bar{d}^{*2}} \end{bmatrix} \begin{bmatrix} S_{d^* y^*} \\ S_{\bar{d}^* y^*} \end{bmatrix} \\ &= \frac{1}{(S_{d^{*2}} - S_{\bar{d}^{*2}})} \begin{bmatrix} S_{d^* y^*} - S_{\bar{d}^* y^*} \\ \frac{S_{d^{*2}}}{S_{\bar{d}^{*2}}} S_{\bar{d}^* y^*} - S_{d^* y^*} \end{bmatrix} \\ &= \begin{bmatrix} \beta_1 + \frac{1}{(S_{d^{*2}} - S_{\bar{d}^{*2}})} (S_{d^* u} - S_{\bar{d}^* u}) \\ \beta_2 + \frac{1}{(S_{d^{*2}} - S_{\bar{d}^{*2}})} \left(\frac{S_{d^{*2}}}{S_{\bar{d}^{*2}}} S_{\bar{d}^* u} - S_{d^* u} \right) \end{bmatrix}. \quad (\text{A.7}) \end{aligned}$$

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

$$\begin{aligned} \hat{\beta}_1 &\xrightarrow{p} \beta_1 + \frac{1}{[Var(d_{ivt}) - Var(\bar{d}_{vt})]} [E(u_{ivt} d_{ivt}^*) - E(u_{ivt} \bar{d}_{vt}^*)] \\ &= \beta_1 + \frac{E(u_{ivt} d_{ivt}^*)}{[Var(d_{ivt}) - Var(\bar{d}_{vt})]} \end{aligned} \quad (\text{A.8})$$

¹⁷It does not mean that small firms are not financially constrained.

and

$$\begin{aligned}\hat{\beta}_2 &\xrightarrow{p} \beta_2 + \frac{1}{[Var(d_{i_{vt}}) - Var(\bar{d}_{vt})]} \left[\frac{Var(d_{i_{vt}})}{Var(\bar{d}_{vt})} E(u_{i_{vt}} \bar{d}_{vt}^*) - E(u_{i_{vt}} d_{i_{vt}}^*) \right] \\ &= \beta_2 - \frac{E(u_{i_{vt}} d_{i_{vt}}^*)}{[Var(d_{i_{vt}}) - Var(\bar{d}_{vt})]},\end{aligned}\quad (A.9)$$

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

A.2 Proof of Proposition 1

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

For the first part, the LS estimator for τ in Eq. (3) is the following:

$$\begin{aligned}\hat{\tau} &= \frac{\sum_{i_{vt}} \bar{d}_{vt}^* y_{i_{vt}}^*}{\sum_{i_{vt}} \bar{d}_{vt}^{*2}} \\ &= \frac{\hat{\beta}_1 \sum_{i_{vt}} \bar{d}_{vt}^* d_{i_{vt}}^* + \hat{\beta}_2 \sum_{i_{vt}} \bar{d}_{vt}^* \bar{d}_{vt}^* + \sum_{i_{vt}} \bar{d}_{vt}^* \hat{u}_{i_{vt}}}{\sum_{i_{vt}} \bar{d}_{vt}^{*2}} \\ &= \frac{\hat{\beta}_1 \sum_{vt} \bar{d}_{vt}^* \sum_i d_{i_{vt}}^*}{\sum_{vt} \sum_i \bar{d}_{vt}^{*2}} + \hat{\beta}_2 \\ &= \hat{\beta}_1 + \hat{\beta}_2.\end{aligned}\quad (A.10)$$

For the second part, Lemma 1 is applied so that

$$\hat{\tau} \xrightarrow{p} \beta_1 + \beta_2. \quad (A.11)$$

A.3 Proof of Proposition 2

Suppose the true equation to be estimated is:

$$y_{i_{vt}} = \beta_0 + \beta_1 d_{i_{vt}} + \beta_2 \bar{d}_{vt} + \beta_3 d_{i_{vt}} \bar{d}_{vt} + \mu_v + \mu_t + u_{i_{vt}}, \quad (A.12)$$

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.

Fig. 4 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

Fig. 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)

Table 10 Description of variables

	Definition
Outcomes	
Entrepreneur	Equal to one if self-employed in professional or technical occupation, 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
Formal employee	Equal to one if employed with documentation or contributing to social security, and zero otherwise
Informal employee	Equal to one if employed without documentation and not contributing to social security, and zero otherwise
Informal self-employed	Equal to one if self-employed in low-skilled occupation and not contributing to social security, and zero otherwise
Jobless	Equal to one if not having a remunerated occupation, including unemployed and inactive adults, and zero otherwise
receiving private transfer	Equal to one if household received any non-labor income from private sources in the last 30 days, and zero otherwise.
Individual variables	
Age	Age of individual in years
White	Equal to one if self-declaration of skin color is white and zero otherwise
Black	Equal to one if self-declaration of skin color is black and zero otherwise
Married	Equal to one if married and zero otherwise
Elementary education	Equal to one if completed elementary education and zero otherwise
Primary education	Equal to one if completed primary education and zero otherwise
High school	Equal to one if completed high school education and zero otherwise
Number of kids	Number of people who are 14 years or less cohabiting the same residence
Number of elderly	Number of people who are 60 years or more cohabiting the same residence
Migrant—last 5 years	Equal to one if migrated to the municipality in the last 5 years and zero otherwise

Table 10 (continued)

	Definition
Municipality variables	
Program coverage	Proportion of households receiving conditional cash transfers in the municipality
Target coverage	Proportion of households in the municipality that should receive a conditional cash transfer according to the official quota
Poverty headcount	Proportion of households whose per capita income is less than two minimum wages in the municipality
Population	Number of inhabitants in the municipality
Population density	Number of inhabitants in the group of municipality over total area of the group
Elementary enrollment rate	Proportion of kids between 7 and 10 years old enrolled at elementary school in the municipality
Primary enrollment rate	Proportion of kids between 11 and 14 years old enrolled at primary school in the municipality
High school enrollment rate	Proportion of kids between 15 and 17 years old enrolled at high school in the municipality
Child mortality	Deaths per 1000 children under 5 years old in the municipality
Coverage of sewer system	Proportion of residences connected to sewer system in the municipality
Prop. of house owners	Proportion of households living in their owned houses in the municipality

Table 11 Overall effect of cash transfers on entrepreneurship, estimated by bootstrap

	Decision of being a small entrepreneur							
	OLS		FE			IV		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
program coverage, $\bar{d}$	-0.012 (0.009)	0.037*** (0.013)	0.047*** (0.016)	0.038** (0.020)	0.039** (0.019)	0.063*** (0.024)	0.067* (0.031)	
individual benefit, d								0.063*** (0.024)
Poverty in 2002		-0.059*** (0.010)						
Poverty in 2002 $\times$ year = 2004		0.012 (0.013)		0.014 (0.011)	0.011 (0.012)		0.008 (0.013)	
Poverty in 2002 $\times$ year = 2006		0.011 (0.012)		0.011 (0.013)	0.008 (0.013)		0.004 (0.017)	
Municipality fixed-effects	No	No	Yes	Yes	No	Yes	Yes	Yes
Census tract fixed-effects	No	No	No	No	Yes	No	No	No
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Social indicators	No	No	No	No	No	No	Yes	No
Number of observations	129,080	129,080	129,080	129,080	129,080	129,080	129,080	129,046
Underidentification LM test						80.1***	94.8***	110.3***
Weak identification F -test						249.7***	150.2***	195.0***
Overidentification J -test						1.157	0.738	0.105

***, **, *Statistical significant at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses are calculated by bootstrap with 200 replications. Sample includes only men with high school diploma or less. Columns (1) and (2) present the regression coefficients obtained by ordinary least squares (OLS). Columns (3), (4), and (5) present the fixed-effect regressions (FE) obtained using the within-group method. Columns (6), (7), and (8) present the fixed-effect, instrumental-variable regressions (IV) with “program coverage” and “individual benefit” instrumented by the interactions between municipal quota and year dummies

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