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Dependence or Constraints?

Labor Supply Responses from a Cash Transfer Program

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Abstract*

Decreases in labor supply among cash-transfer recipients are often cited as potential drawbacks of social-assistance programs. However, cash transfers can also increase employment. Using variation across cohorts and over time in the eligibility criteria of a nationwide conditional cash-transfer program in Bolivian public schools, this paper shows that employment increases among parents of eligible children, particularly for females. The increase in employment coincides with increases in self-employment and in the probability of investing in family businesses. These effects are mostly driven by females from areas with limited access to financial services. As mothers work more, overworked fathers reduce work hours. The results suggest that there are (positive) unintended consequences of cash-transfer programs targeting households with school-age children: Cash transfers may relax liquidity constraints and boost entrepreneurship, and also relieve overworked adults.

JEL Codes: D13, J46, J21, J22, O12, O18.

Keywords: Cash transfers, Labor Supply, Gender, Entrepreneurship.

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I. Introduction

For several years, cash-transfer programs have delivered welfare-increasing effects in several dimensions (Filmer and Schady, 2011; Baird, McIntosh and Ozler, 2011), particularly to the most vulnerable populations across the globe. Critics of these programs argue that they could reduce the incentives to work among recipients and create a culture of dependence.[1] However, in contexts of limited access to credit markets, cash-aid programs could increase employment by relaxing liquidity constraints, promoting entrepreneurship, or inducing more-profitable career-path decisions.

The relationship between labor supply and cash reception in developing countries has been widely studied, but there are still some important gaps. First, most of the evidence comes from targeted programs that deliver resources to the poorest subpopulations.[2] Less is known about the work-incentive effects of cash transfers in non-targeted, nationwide programs. Such evidence is important, as targeting can be costly, and the idea of programs with universal coverage such as UBIs (Universal Basic Income programs) is increasingly popular among policymakers (Banerjee, Niehaus and Suri, 2019; Hoynes and Rothstein, 2019). Second, a common practice in cash-transfer programs is to disburse resources to women, but it is unclear whether changes in female employment affect the work decisions of their spouses. Clarifying this is important, as several studies show that social-protection programs affect the allocation of labor within the household (Cullen and Gruber, 2000; Autor et al., 2017; Fadlon and Nielsen, 2019).

This paper aims at filling these gaps by using the context of the Bono Juancito Pinto (BJP) in Bolivia. This nationwide program provided an annual lump-

[1]In advanced economies, this idea has led to cash-aid programs that impose work requirements or phase out relatively fast (Moffitt, 2002)

[2]In developing countries, several studies have consistently found that cash-transfer programs, at least on average, do not reduce labor supply (Alzua, Cruces and Ripani, 2013; Banerjee et al., 2017; Bosch and Schady, 2019). There is also evidence of positive effects, although only from programs that complement cash with work requirements, job training, or that specifically target entrepreneurs (See (Baird, McKenzie and Özler, 2018) for a detailed review of the evidence on several programs involving cash transfers in developing countries.).

sum cash transfer of \$ Bolivianos (Bs.) 200 (approximately 25 U.S. dollars), conditional on 80% attendance for every child enrolled in public school. During its first year of implementation, around 90% of eligible households received transfers, averaging 1.5 beneficiary children per household. The transfers were generally delivered to mothers. The average per-household transfer represented 42 and 23% of the earnings of mothers and fathers, respectively, and covered 53% of mean annual per child household education spending in urban areas and over 100% in rural areas.

One key feature of the Bolivian context is the almost-universal coverage of the program. As program eligibility is based on neither socioeconomic status nor geographical location, the study sample includes subpopulations who might be on the margin of participating in labor markets, as opposed to only including the most vulnerable subpopulations, who might face higher barriers to employment.[3] The Bolivian context also provides quasi-experimental variation to identify effects on labor supply. During the first wave of the BJP program (2006), only households with children in first to fifth grade were eligible to receive the transfers, but two subsequent expansions included households with sixth-grade children in 2007, and with seventh- to eighth-grade children in 2008. Using data from eight years of nationally representative household surveys to construct a pooled cross-section dataset, I exploit changes in household eligibility to estimate the effects of the program on adult labor supply. Specifically, I compare changes in trends of work outcomes as households became eligible to receive the transfers.

I find that exposure to the program increased employment among parents of eligible children. The probability that either parent worked increased by 2 percentage points. The increase at the extensive margin coincides with an 8-hour increase in parental weekly work time (8.6% increase, relative to the baseline).

As mothers of school-age children were 30 percentage points less likely to work

[3]For instance, the 6 programs analyzed by Banerjee et al. (2017) are all targeted at poorer households or regions.

than their male counterparts during the pre-program years, I analyze whether the program helped narrow or widen this gap. First, I find that the effects on the extensive margin is larger for female parents. The program increased the probability of working by 9 percentage points in the case of mothers, and by only 5 percentage points in the case of fathers. Second, I find that the effects on the intensive margin are driven by mothers. The program increased weekly hours worked by 7.8 in the case of mothers—a 25% increase with respect to the baseline mean—and also increased earnings. In contrast, there were neither statistically nor economically significant effects on hours worked and earnings for males. One explanation is that liquidity or time constraints were more binding in the case of mothers of eligible children and that the program relaxed such constraints.

The positive program effects on work outcomes seem to better match models of liquidity constraints rather than models of time constraints. For instance, baseline school attendance rates were higher than the threshold for receiving the transfers (80%), and, when it comes to child outcomes, there is no evidence of effects on enrollment, attendance, or child labor.[4] In contrast, the positive effects on employment come largely from self-employment, and coincide with increases in the probability of purchasing inputs for businesses owned by mothers. One explanation is that the program relaxed liquidity constraints by providing capital to start small businesses. Indeed, using administrative pre-program data regarding the number of financial branches per 100,000 inhabitants at the municipality level, I find that the effects on work and business outcomes among mothers are concentrated in areas with a lower number of financial branches per capita.

I then analyze whether the program led to within-household reallocation of labor. First, I find that the program induced the entrance of an extra worker into the labor force. Examining the subsample of biparental households, the program reduced the probability that only one parent works—typically the father—while

[4]Note that the program could have improved child outcomes in other dimensions that are beyond the scope of this study. However, the results suggest that the program had no effect on the margins that are more likely to unlock time for mothers.

increasing the probability that both parents work. This result suggests that the program did not lead to substitution in the extensive margin of employment. Second, I find reductions in labor supply among fathers, but only for those working very long weekly hours.[5] Quantile regressions reveal decreases in the top of the distribution of hours worked. This pattern coincides with decreases in the probability of working more than 60 hours per week for fathers. One explanation is that overworked males were able to substitute their own labor with that of their spouses.

This paper contributes to the literature studying the effects of cash transfers on recipient labor supply in three ways. First, previous studies show that cash-aid does not reduce labor supply in developing countries (Alzua, Cruces and Ripani, 2013; Banerjee et al., 2017; Bosch and Schady, 2019). This study contributes with previous evidence by showing that non-targeted cash-transfer programs can increase employment.

Second, two other studies have documented increases in labor supply among recipients of non-targeted cash-transfer programs South Africa (Ardington, Case and Hosegood, 2009) and in Iran (Salehi-Isfahani and Mostafavi-Dehzooei, 2018).[6] In particular, Salehi-Isfahani and Mostafavi-Dehzooei (2018) find that the increases in employment are driven by females. This paper confirms such findings by documenting similar results in a different context. Moreover, this paper builds on previous studies by showing that access to financial services and the reallocation of labor between spouses are key to the understanding the effects. Although, previous studies have shown that the provision of capital grants to selected groups of entrepreneurs can spur business creation and growth (de Mel, McKenzie and Woodruff, 2008; McKenzie, 2008; Blattman, Fiala and Martinez,

[5]This is possible, as in the study sample, the median weekly hours among employed males is 48. Thus, half of the population works more hours a week than what is suggested by ILO conventions and the Bolivian Labor Act, both setting a cap of 48 hours.

[6]Salehi-Isfahani and Mostafavi-Dehzooei (2018) show that the reception of cash as compensation for the removal of energy and bread subsidies in Iran increased female labor supply, and Ardington, Case and Hosegood (2009) show that a non-contributory pension program in South Africa increased work-related migration for prime-age adults.

2014), it is unclear whether such results hold in cases of programs with universal coverage (Banerjee, Niehaus and Suri, 2019). By highlighting the importance of liquidity constraints, this paper shows that, in the context of a non-targeted, nationwide program, cash transfers to households with school-age children can operate as capital grants.

Third, although the overall effect of the program on labor supply is positive, the program also reduced labor supply for some subpopulations. This result is consistent with evidence of small labor-supply reductions in advanced economies (Moffitt, 2002; Hoynes, 1996; Picchio, Suetens and van Ours, 2018). However, the result is unlikely to support the idea that cash-aid generates dependence. Labor supply decreases, but only among overworked fathers of eligible children, and is more than compensated for by an increase in employment among mothers. This mechanism is important, as there is cross-country evidence showing that hours worked are longer in developing countries (Bick, Fuchs-Schndeln and Lagakos, 2018), and that leisure helps explain differences in welfare across countries (Jones and Klenow, 2016). In turn, by showing that higher female employment reduces the pressure on males to overwork,[7] this paper also contributes to studies documenting that women's decisions lead to increases in welfare at the household level (Duflo, 2003; Bernhardt et al., 2017; Benhassine et al., 2015). Overall, rather than creating work disincentives, the results show that cash transfers in developing countries can unlock interesting within-household dynamics that boost employment.

II. Context and Data

The Bono Juancito Pinto (BJP) program was first announced in October 2006—towards the end of the school year.[8] The program provided a cash transfer (CCT)

[7]Evidence from advanced economies suggests that signaling, respect, and addiction can explain the prevalence of overwork (Landers, Rebitzer and Taylor, 1996; Ellingsen and Johannesson, 2007; Hamermesh and Slemrod, 2008). The results in this paper suggest a different explanation in developing countries: adults may overwork due to frictions preventing the entrance of their spouses into the labor force.

[8]The Bolivian school year usually starts in late February and ends in early November. There are usually two weeks of winter break between June and July. Summer break starts in late November and

of $ Bolivianos (Bs.) 200 (approximately 25 U.S. dollars) conditional on 80% school attendance for every child enrolled in public school, covering approximately the households of 90% of school-age children in the country.[9]

As opposed to most CCT programs in Latin America, this program was not means-tested, and the eligibility criterion was based on the grade in which the child was enrolled and not on the socioeconomic status of the child's family. This transfer represented around one-third of the monthly minimum wage for the baseline year, 53% of the annual household per-capita education spending in urban areas and more than 100% in rural areas.[10] As of 2005, the school enrollment rate was already high, at 90%. Moreover, dropout and non-passing rates were below 10% before the program was implemented.[11]

In the first stage, the potential beneficiaries were children enrolled in first to fifth grades; children who met the attendance threshold and fulfilled additional documentation requirements received the transfer at the end of the school year (November). A birth certificate or ID was required; in addition, children had to be accompanied by a parent or guardian to receive the money. Most often, the mother accompanied the child. After the second round of the program, children who did not possess a birth certificate or an ID could receive the money if they presented two witnesses who testified to their identity.

The funds were disbursed in each school by personnel from the Armed Forces and reached 89% of eligible children during the first year of the program. In October 2007, the program was unexpectedly extended to children in sixth grade, again with disbursement of the funds at the end of the school year. The set of beneficiaries was expanded to children in seventh and eighth grades in July 2008, but the disbursement schedule was changed to two payments, one in July and one in November 2008. Although the funds were disbursed in two payments, the

lasts until early February.

[9] In 2005, the year preceding the program implementation, only 10% of children age 6 to 18 enrolled in schools were enrolled in private schools.

[10] Calculations are based on the 2005 Household Surveys from the National Bureau of Statistics (INE).

[11] Source: Ministry of Education; see Murillo et al. (2004).

total amount given to each student did not change.

A. Data and Measurement

The data for this study come from national household surveys conducted by the National Bureau of Statistics (INE) for the years 2002–2009. I constructed a pooled cross-section dataset based on eight waves of nationally representative household surveys. This dataset allows me to analyze cohorts at least four years before they were eligible for the program and formally test for the presence of parallel pre-program trends, which is crucial for identifying the program effects. Thus, I focus the analysis on a four-year window before and after each cohort was eligible for the program. The surveys were independent cross-section samples of individuals drawn from a common sample frame based on the 2001 population census. Surveys for the years 2002 and 2005 to 2009 were conducted between late November and December of each year. In particular, information from post-program surveys (2006–2009) was collected after the program's announcements. The 2003–2004 survey was a continuous survey conducted with different households in two rounds: November 2003–April 2004 (2003 round) and May–October 2004 (2004 round).[12]

As children enrolled in private schools were not eligible to participate in the program, I focus on the 90% of school-age children who were not enrolled in private schools. Within this subsample, the study restricts the analysis to the set of households with school-age children in first to eighth grade. For each household, I computed information regarding labor-market variables associated with childrens parents—i.e., the head of household and her (his) spouse. Throughout the analysis I mostly focus on household heads and heads' spouses as, on average, their earnings represented most of the household income and they were more likely to

[12]There were further expansions to older students in ninth through twelfth grades in 2014. However, the household surveys corresponding to the years following this last expansion where not available when this project started. Also, the more recent surveys analyze samples drawn from a different sampling frame based on the 2012 Population Census.

administer the resources obtained from the program.

I use two main work outcomes. First, I measure employment by computing an indicator of whether each household member reported having worked or performed remunerated activities or tasks for a family business during the week preceding the survey. The second measure refers to the average weekly work hours. To construct this measure, I use self-reported information regarding the average number of hours worked per day and the number of days worked in the week preceding the interview. In the case of unemployed people, the number of hours is 0. To prevent outliers from driving the results, I truncate the value of work hours with respect to the 99th percentile of the work-hours distribution in each survey wave. I focus on these two measures, as they are the standard measures used in experimental studies analyzing responses of labor supply to cash transfers in developing countries such as (Alzua, Cruces and Ripani (2013) and Banerjee et al. (2017)). Finally, I also include self-reported measures of monthly labor earnings, either from wage labor or self-employment.

I complement this dataset with administrative records regarding the number of branches of financial institutions and population at the municipality level. Information regarding the number of branches of financial institutions comes from the national regulator, the Authority of Supervision for Financial Institutions (ASFI), and only covers municipalities that are also provincial capitals (89 out of 339 municipalities). However, this subset of localities accounts for two-thirds of the observations in my sample. Population data comes from the 2001 National Population Census conducted by INE.

Appendix Table A1 presents means of work outcomes and demographic characteristics measured at the end of 2005, the year before the program was announced. Column (1) reports means corresponding to the study sample. On average, 1.7 adults reported working and there are differences by gender among household heads or spouses: 93% of male heads reported working, while only 66% of female heads reported working at baseline. One out of four children in the study sample

reported working, although enrollment in school was high (93%).[13]

Columns (2) and (3) report statistics corresponding to the subsample of households that were eligible in the first round (those whose youngest children had 1 to 5 years of schooling), and households that became eligible in subsequent program expansions (those whose youngest children had 6 or more years of schooling). On average, adult labor-market outcomes are fairly similar. However, there are some important differences when it comes to household demographic characteristics: households that entered the treatment during its first wave are more likely to have children under 5 among their household members than those households who would join the program in the subsequent expansions. There are also differences in child employment that could be due to the age composition of the households, as children from households that entered the program later were older. To prevent these differences from driving the results, Section III discusses the importance of controlling for flexible trends that vary with child age and household size.

Turning to external validity, Column (4) reports statistics corresponding to all the households in the 2005 survey wave with at least one child of school age (6-18 years old). Relative to the means in Column (1), there are no substantial differences other than, in the case of the full sample, there being a higher chance of having a child under 5 years old. Overall, the study sample is fairly representative of the full population, as the program did not target households either based on wealth or on location.

III. Empirical Strategy

I exploit the staggered timing and eligibility criteria of the program as the identifying sources of variation. Although the program was implemented in all regions of the country at the same time, households were included as beneficiaries of the program gradually, based on years of schooling of their children. Thus, the design provides variation over time and across households in a given year,

[13] This is possible because the school day in Bolivia lasts generally 4 and 1/2 hours.

suggesting a difference-in-differences approach.

The timing of the program's announcements, which is arguably exogenous to households' decisions, provides a first source of variation. The program was originally announced during the first year in office of a new government (2006), which suggests that the announcement was unexpected with respect to the set of information the population had in 2005.[14] The president announced the implementation of the program in October 2006. Two other expansions were announced later, in October 2007 and July 2008.

The design of the program provides cross-sectional variation during each year, based on the program's eligibility criterion. During the first round of the program, households with children enrolled in first to fifth grade were eligible (children with 1 to 5 years of schooling in the sample), while households of children enrolled in sixth to eighth grade (6 to 8 years of schooling) were ineligible. Households with children enrolled in sixth grade became eligible during the second year of the program, and households with children in seventh to eighth grade two years after that. Thus, the analysis in this paper is based on comparisons of changes in trends of work outcomes between households of eligible and non-eligible children, before and after the rollout of the program.

While program eligibility was based on children's years of schooling, the unit of analysis in this study is the household. The program initially targeted younger children and subsequently included older children.Thus, in each year, household eligibility was determined by the years of schooling of the children enrolled in the lowest school grade, typically the youngest school-age child in the household.

To assess the impact of the program on adult labor supply, I estimate a flexible difference-in-differences model using the following specification:

$$(1) \qquad Y_{i,s,t} = \delta_t + \theta_s + \sum_{j=-4, j\neq -1}^{j=4} \beta_j \mathbf{I}[\tau_{s,t} = j] + \mu_{d,t} + X_{i,t}\gamma + \epsilon_{i,s,t}$$

[14]Indeed, the winning candidate even refused to debate during his electoral campaign.

$Y_{i,s,t}$ represents the work outcome of interest of household i, whose youngest school-age child has s years of schooling, and that is observed in year t. θ_s denotes fixed effects corresponding to the years of schooling s of the youngest child in household i, and δ_t denotes year fixed effects. Time to treatment, measured in years, is denoted by τ_{st}, and varies with respect to the years of schooling of the youngest child and the program rollout. The omitted category is $\tau_{st} = -1$, which denotes the year preceding the entrance of households with children with s years of schooling into the treatment group.

I include state-year fixed effects ($\mu_{d,t}$) to control for state time-varying shocks to labor markets.[15] I also include a vector of demographic controls $X_{i,t}$ which includes gender and age of the household head, indicators of whether the household head speaks Spanish, and whether the household lives in an urban or rural area. Note that, by design, entrance into the program might be correlated with childrens age. To account for the influence of time-varying shocks affecting households of younger children, I control the average age of school-age children in the household, as well as their interactions with time fixed effects. In a similar way, as larger households might be more likely to have eligible children, I control for household size and interactions of year fixed effects and household size.

The coefficients of interest are β_j $(\beta_{-4}, \beta_{-3}, ..., \beta_4)$ and capture differences in changes in work outcomes, relative to the year preceding changes in eligibility, between eligible and non-eligible households. Inference is based on clustered standard errors at the municipality level, which allow for flexible correlation across households and over time within each locality. All estimations use survey sampling weights to obtain coefficients that are representative at the population level.

To capture the average impact of the program during the four post-program periods in the sample, I estimate treatment effects following a standard difference-in-differences approach:

[15]Bolivia is divided into nine departamentos (states) that constitute the second most important administrative units in the country.

$$(2) \qquad Y_{i,s,t} = \delta_t + \theta_s + \beta Post_{s,t} + \mu_{d,t} + X_{i,t}\gamma + \epsilon_{i,s,t}$$

$Post_{s,t}$ is an indicator that takes the value of 1 for the periods in which households with children with at least s years of schooling enter the treatment group—i.e., $\tau_{s,t} \geq 0$. The coefficient of interest in this case is β, which captures differences in changes in work outcomes before and after entrance into treatment, with respect to ineligible households.

IV. The Effects of Conditional Cash Transfers on Adult Labor Supply

Figure 1 depicts means of self-reported information regarding cash reception during the periods before and after the implementation of the program. In particular, the 2007–2009 survey waves include a question asking whether each child of school age received the transfer during the previous academic year. The probability that any child in a household received the cash transfer increases sharply as households became eligible. On average, 92% of households with eligible children receive the transfer. The right panel of Figure 1 shows that, on average, 1.5 children per household end up receiving the transfer. This translates to approximately \$ Bs. 300 per household each year (approximately \$ USD 43). The per-household transfer represents 7% of the annual per-capita household income at baseline, and 42 and 23% of the earnings of mothers and fathers, respectively.

Figure 2 plots means of the probability that either parent (i.e., the household head or her spouse) works and the weekly work hours for parents, normalized with respect to the pre-program means. As households became eligible, there is a clear jump in employment and work hours, suggesting that the program might have promoted participation in the labor force.

Figure 3 plots the point estimates for β_j from equation (1), and their respective 95% confidence intervals for work outcomes. The top left panel shows that

the probability that either the households head or the spouse works increases as the household enters the treatment group. The bottom left panel shows that the number of working adults significantly increases as households became eligible. In both cases, the increases in employment coincide with increases in work hours, as shown in the top right and bottom right panels. Note that none of the confidence bands include economically meaningful negative coefficients. This result suggests that traditional concerns about reductions in labor supply due to cash-aid are rather misplaced in the Bolivian context. Importantly, none of the pre-program coefficients ($\beta_j, \forall j < 0$) is significant, supporting the parallel pre-trends assumption needed for the validity of our empirical design.

Table 1 presents difference-in-differences estimates for work outcomes, following several specifications. Panel A reports results corresponding to parental outcomes, while Panel B reports results corresponding to outcomes of all adults in the household. For each outcome, the table reports four specifications that sequentially add different sets of controls. For instance, Column (1) in Panel A reports estimates of a regression of the probability that either parent works on demographic controls as well as region, time, years-of-schooling fixed effects, and an indicator that denotes exposure to the program.[16] Column (2) reports estimates that add flexible time trends interacted with the average age of school-age children in the households, with the aim of controlling for time-varying shocks that affect households with older (younger) children. Column (3) includes flexible time trends interacted with household size to account for time-varying shocks affecting larger households. Finally, Column (4) includes state-specific time trends that allow for within-state comparisons.

Turning to the results, Columns (1) to (4) show that the program increased the probability that either parent works by 2 percentage points. Note that while significance is lost as more trends are added ($p - val < 0.11$ in Column (4)),

[16]All regressions include age, gender, years of schooling, and language of the head of household as well as urban-rural indicators, household size, average age of school-age children, and the number of children under 5 who live in the household.

the point estimate is constant across specifications. In terms of magnitudes, the effect is unsurprisingly small given that, before the program was rolled out, in 92% of households either the household head or spouse worked. Columns (5) to (8) show that the program increased the number of hours worked by 8.6 (a 9% increase relative to the pre-program mean). All point estimates are robust across specifications. Columns (9) to (12) report the effects of exposure to the program on total labor earnings, either from wage labor or from self-employment. While the point estimates are positive, only the point estimate in the model with neither age, neither size nor region trend is significant. Throughout the rest of this paper, I base my results on the specification that accounts for age, size, and state trends, as that specification leads to more-conservative results.

The patterns are similar in Panel B. Table 1 shows that exposure to the program increases the number of working adults in the household by 0.11–0.13 percentage points (an 8% increase relative to the pre-program mean). This increase coincides with an increase in weekly work hours due to the program. This increase is similar in magnitude to the one reported in Panel A, suggesting that household heads and spouses drive the increase in work hours at the household level. Put together, the results show that the program increased adult labor-force participation.

A. *Effects on Work Outcomes by Gender*

In the study sample, 83% of household heads are males, and 95% of them work. Thus, most program effects might be driven by females entering the workforce. Moreover, there are gender disparities in employment rates, work hours, and earnings. On average, males from two-parent households are 30 percentage points more likely to work than females—the probability that a male head or spouse works is 0.96 while that of their female counterparts is 0.65. When they work, fathers work on average 51 hours per week, while mothers work 41 hours. These differences relate to gender differences in earnings: among those who work, fathers obtain earnings that are three times those obtained by their spouses. While

comparative advantages may explain why mothers tend to work less (Becker, 1985), these gaps also suggest that mothers faced labor-market constraints. Thus, it is important to test whether the program relieved such constraints.

Figure 4 shows means of the probability of working for female heads or head spouses (left panel) and male heads or head spouses (right panel), normalized with respect to the pre-program average. There is a clear jump in the probability of working as households became eligible to participate in the program. The jump is larger for females. Figure 5 presents flexible difference-in-differences estimates corresponding to the specification in equation (1). The top panel shows that, while there is an increase in the probability of working for fathers, the increase is much higher for mothers. The bottom panel shows that the program led to an increase in work hours for mothers. It also shows that there does not seem to be significant evidence of effects of the program on work hours for male parents.

Table 2 provides estimates of the effect of the program on work outcomes. Due to the program, employment for mothers increases by 9 percentage points, a 14% increase with respect to the baseline probability of working. This increase is higher than the increase for fathers, both in absolute and relative terms, more so in the latter. Column (4) shows that due to the program male employment increased by 5 percentage points, which represents only a 5% increase relative to the baseline mean. The gender-based heterogeneity in the program effects is more notorious when it comes to work hours. Column (2) reports that the number of weekly work hours increased by 7.8 due to the program, a 20% increase with respect to the baseline mean, while Column (5) reports small insignificant effects in the case of males. A Chow test of the equality of coefficients show that the estimates are statistically different. Column (3) also shows that the increase in work hours for females coincides with an increase in earnings.

Previous studies have shown that cash transfers targeted at poor regions or subpopulations do not reduce labor supply (Alzua, Cruces and Ripani, 2013; Banerjee et al., 2017; Bosch and Schady, 2019). The evidence presented in this

section strengthens this idea by documenting that cash transfers can induce employment in the context of a non-targeted, nationwide program. Interestingly, the existing evidence of positive effects on employment due to cash-transfer programs comes from programs that are not means-tested, as is the case of the Bolivian program.[17] One explanation is that by studying non-targeted programs, it is possible to capture behavioral responses of adults who are on the margin of working and are better able to use the transfers to relieve constraints preventing their entrance into the labor force. Section V analyzes the salience of time and liquidity constraints in explaining the positive effects of the program on parental work outcomes.

It is also worth noting that the evidence in this paper comes from a program that was implemented during a high-growth period (6% annual GDP growth on average). It is not obvious that the positive effects on employment would exist when there is a less-favorable business environment, particularly in economies in which self-employment is the most important source of income. For instance, several cash-transfer programs target impoverished populations in areas of civil conflict, political instability, natural disasters, health epidemics, or facing recessions. In such settings, cash transfers have been shown to trigger a variety of welfare-increasing outcomes, but it is unclear whether the option of using the transfers to enter the workforce is available to the beneficiaries of cash transfer programs in these settings.

B. Robustness

The empirical strategy in this paper identifies the causal effect of the program on adult employment under the assumption that, in the absence of the program, employment would have changed similarly in the case of treated and untreated

[17] The results from this paper are consistent with evidence from a non-targeted program in Iran (Salehi-Isfahani and Mostafavi-Dehzooei, 2018) that shows that the reception of cash as compensation for the removal of energy and bread subsidies increased female labor supply, and with evidence from South Africa showing that a non-contributory pension program increased work-related migration for prime-age adults (Ardington, Case and Hosegood, 2009).

households. Appendix Table A2 tests for parallel pre-program trends. Panel A reports the point estimates corresponding to equation (1) for the main outcomes. Reassuringly, it shows no evidence of significant pre-program coefficients. Panel B reports results from testing the null hypotheses that the sum of pre-program coefficients is zero ($\beta_{-4} + \beta_{-3} + \beta_{-2} = 0$). Again, it is not possible to reject the null in any of the cases. Panel C reports results from testing the null that the pre-program coefficients are jointly equal to zero ($\beta_{-4} = \beta_{-3} = \beta_{-2} = 0$). The null is rejected in only one out of eight cases (see column 3).

To further test the sensitivity of the estimates to potential violations of the parallel-trends assumption, I first include group-of-entry linear trends. To do so, I group households based on when they enter the treatment group: those entering in 2006, those entering in 2007, and those entering in 2008. I then include group dummies interacted with linear trends.[18] Panel A of Appendix Table A3 shows that the results are robust to this specification.

A second potential concern is that pre-program trends in work outcomes differ across households with younger or older children. One would imagine that households whose youngest child has only one year of schooling could differ in trends with respect to those whose younger child has eight years of schooling. I test the salience of this potential source of bias by restricting the sample to households whose youngest child has five, six, or seven years of schooling. Thus, this approach excludes observations from households with younger children (those having had one to four years of schooling), and from households with older children (those having had eight years of schooling at minimum). Panel B from Table A3 shows that all the results are qualitatively similar to those from the main specification.

[18] Note that adding group-year specific fixed effects would be collinear with the variable capturing treatment status.

V. Conditional Cash Transfers and Time and Liquidity Constraints

The previous section documented novel evidence that conditional cash transfers can increase employment, mainly for female parents. This result is consistent with a context of large gender disparities in labor markets, and it suggests that females may face more binding constraints than males. In theory, there are two types of constraints that could be relieved by the reception of conditional cash transfers: time constraints and liquidity constraints. Sections V.A and V.B examine these two possible channels.

A. Time Constraints

It is possible that the program relieved time constraints for adults. For instance, the condition component of the program required that children had to attend school for 80% of the school year in order to receive the transfer. If the program increased enrollment or attendance, then it could have relaxed binding parental time constraints. In addition, cash transfers may reduce child labor (Edmonds and Schady, 2012), and mothers could be induced to work more to make up for the forgone earnings of their children.

Table 3 reports estimates of the effect of the program on enrollment, attendance, and child labor. Columns (1) to (3) present estimates of the effects of the program on the number of enrolled children per household, the number of children who attended school during the week preceding the interview or reported not attending because of vacation,[19] and the number of working children. There are no significant effects on any of these margins. Moreover, the point estimates are not economically meaningful when compared to the pre-program mean. For instance, the point estimates of the effect of the program on the number of enrolled children and children who attended school represent only 4% and 3% of the baseline mean, respectively. Columns (4) to (6) present estimates from a specification

[19] Unfortunately, the surveys are conducted every November, the final month of the academic year.

similar to the one in equation (2), but they are estimated using child-level data as opposed to the household level. Such specification allows for the comparison of child outcomes as each child became eligible for the program.[20] The results are qualitatively similar to those from the household-level specification.

Overall, the lack of effects on childrens outcomes is consistent with a context in which attendance rates were higher than the threshold attendance rate required for receiving transfers. According to administrative records, pre-program attendance rates were already above 80% (Murillo et al., 2004). However, it is important to note that I only examine outcomes that could modify the time availability of the mother. The program could have improved children outcomes in other margins, as suggested by the literature (Fiszbein et al., 2009). Such analysis is beyond the scope of this paper.

B. Liquidity Constraints

In the study sample, 50% of working adults operate their own businesses as their main occupation. In the case of males, small-scale farms are the primary type, being 52% of the businesses mentioned in the study sample and 85% in rural areas. In the case of self-employed females, 43% of their businesses are in the retail sector, and this share increases to 51% in rural areas. Thus, the program transfers could have been used to create new businesses or scale up pre-existing ones.

I begin by analyzing the extent to which the program increased self-employment as well as revenues of and input spending in family businesses. To do so, I exploit data on monthly revenues (sales) and input expenses associated with family businesses, typically operated by family members.[21] Panel A in Table 4 reports

[20] This specification exploits the staggered entry into treatment of children. Concretely, I ran regressions of childrens outcomes on years-of-schooling fixed effects, time fixed effects and an indicator of whether each child is eligible for the program, plus the controls associated with the main study specification.

[21] Interviewees are asked how much gross earnings they obtain from their businesses. Input spending includes the purchase of materials or, in the case of a retail business, merchandise. It also includes wages paid to employees and remunerations of services. It excludes payments of rents, utilities, and taxes, as

difference-in-differences estimates of the effect of the program on the probability that either parent is self-employed or reports working for the family business, on the probability of working as wage workers, business revenues, and input spending. It shows rather noisy increases in the probability of being self-employed or a family worker, and no significant effects on business outcomes.

Panel B shows that mothers are more likely to become self-employed or family workers due to the program. Note that the effect on the probability of being self-employed or working for a family businesses is larger than the effect of the program on probability of working (see Table 2). One explanation is that the program might have also induced female wage workers to become self-employed. This is consistent with evidence from an unconditional cash transfer program in Ecuador showing that mothers of beneficiary children switch from the formal sector to the informal sector (Bosch and Schady, 2019).

Columns (3) and (4) from Panel B show that while there are no significant effects on total revenues and input spending associated to their businesses, Column (5) shows that the program increased the probability of investing in business inputs by 7 percentage points (a 25% increase with respect to the mean). This result suggests that the program might have led to the creation of small businesses among mothers. Interestingly, Panel C shows that the probability of making positive input spending for businesses operated by fathers decreases by 8 percentage points. Thus, households could have substituted for businesses operated by fathers with mother-operated businesses. Section VI analyzes the effects of the program on within-household reallocation of labor.

If the program resources indeed relieved liquidity constraints, one would expect that the effects are driven by families in areas with limited access to formal finance. To test this hypothesis, I exploit cross-municipality heterogeneity in the pre-program supply of financial services to analyze whether the effects are higher in regions with more branches of financial institutions per 100,000 inhabitants.[22]

such a breakdown was not available for all the survey waves.

[22]I interpret this cross-municipality variation as a shift in credit market imperfections. Areas with low

Appendix Figure A1 shows the spatial distribution for the municipalities with available data. It shows that the supply of financial services is not particularly concentrated in any region. As mentioned in Section II.A, while I only observe the supply of financial services for a subset of the municipalities, these municipalities capture over two-thirds of the observations in the study sample.

Equation (3) estimates a triple-difference model of the effects of the program on work outcomes in areas with high and low access to finance—i.e., above and below the pre-program median.

$$\begin{aligned} (3) \qquad Y_{i,s,t} = {} & \beta_1 Post_{s,t} + \beta_2 Post_{s,t} \times Access_m + \delta_t + \theta_s + \delta_t \times Access_m \\ & + \theta_s \times Access_m + \kappa Access_m + \mu_{d,t} + X_{i,t}\gamma + \epsilon_{i,s,t} \end{aligned}$$

In this case, $Access_m$ is an indicator of whether the number of financial branches per 100,000 inhabitants in municipality m is above the median for 2005, the year preceding the program. The parameters of interests are β_1 and β_2. β_1 captures the effect of the program on work outcomes for households living in municipalities with low access to financial services. β_2 captures the relative effect of the program on municipalities with high access to financial services, with respect to the effect for households in low-access municipalities. To make sure the results are not driven by urban-rural time-varying shocks that may be correlated with access to finance, I add urban-year fixed effects to the main specification.

Table 5 reports triple-difference estimates corresponding to equation (3). Panel A reports effects on outcomes corresponding to either parent. It shows that for households in low-access areas, the positive effects on employment (see Column 1) are higher. In contrast, these effects are almost fully attenuated in the case of households living in areas with high access to financial services. Figures 6 to

supply of financial services have a limited set of financing options for local households, leading to higher credit constraints; they also exhibit less competition for informal lenders, allowing repayment rates to be potentially higher.

8 present flexible difference-in-differences estimates of equation (1), by baseline access to financial services and parent gender. They document increases in the probability of working, work hours, and earnings for mothers in areas with low access to financial services. These patterns are corroborated by Panels B and C. In particular, Panel B also shows evidence of heterogeneity in the case of mothers. The program led to an increase in the probability of employment of 17 percentage points for females in low-access areas, while the effect was 12 percentage points lower in areas with high access to financial services. A similar pattern is observed in work hours. Work hours of mothers increased by 10 hours/week in areas with lower access to financial services. This increase is significantly different than that of mothers located in higher-access areas. Turning to labor earnings and self-employment, although the differences with high-access areas are imprecisely estimated, the effects of the program were substantially higher in the case of females in low-access areas.

I also find that the program increased business spending for female businesses in areas with lower access to formal financial services. Panel B of Appendix Table A4 shows that the program led to substantial increases in input spending for businesses operated by females in low-access areas. Column (2) shows that the program led to an increase in business spending of $ Bs 211, which represents 27% of the baseline mean. Column (3) shows that mothers in low-access areas experienced an increase in the probability of spending in their businesses of 16 percentage points due to the program. This effect is twice as large as the one found in higher-access areas. The results in this section are consistent with evidence showing that capital grants to entrepreneurs translate into investment in small businesses, and increase hours worked of business owners (de Mel, McKenzie and Woodruff (2008) and McKenzie (2008), among others). In the case of non-targeted programs such as the Bolivian case, cash transfers delivered to people who have limited access to finance ended up operating operate as capital grants.

VI. Cash Transfers and Intra-Household Reallocation of Labor

It is possible that the program, while allowing the entrance into employment of females, leads to substitution of labor within households. However, it is also possible that the program provided opportunities for an added worker to enter the labor force. Figure 9 provides evidence supporting the latter hypothesis. It reports flexible difference-in-differences estimates of the effect of the program on work outcomes corresponding to equation (1) for the subsample of biparental households, which represents over 80% of the study sample. It shows that the program reduced the probability that only one parent works (top panel), but it increased the probability that both parents work (bottom panel).

Table 6 further supports these results. It shows that the program reduced the probability that only one parent works by 10 percentage points (a 28% decrease with respect to the baseline mean). This effect is fully driven by a decrease in the probability that only fathers work (see Column (3)). In contrast, Column (5) shows that the program increased the probability that both parents work by 14 percentage points (a 25% increase relative to the pre-program mean). As labor-force participation was substantially lower for mothers, and the program mostly increased employment for them, the program may have allowed biparental households to gain a second source of income by allowing mothers to enter the labor force.

I then turn to analyzing the possibility of intra-household substitution of labor at the intensive margin. The responses of labor supply to unearned income (cash transfers) may be different along the distribution of work hours. For instance, in Section IV I found that, while the program increased the probability of employment among fathers, there were no effects on the number of work hours. Thus, it is possible that the positive effects of the program on the lower end of the distribution of work hours were offset by reductions in work hours for males with long working weeks.

In the study sample, the median number of weekly work hours among working

males is 48. Thus, almost half of working males are likely to be overworked: they work more than the maximum amount of weekly work hours suggested by the International Labor Organization (ILO) and by the Bolivian Labor Law.[23] In this setting, it is possible that cash transfers allow them to work less. To test this idea, I estimate (2) along the distribution of work hours:

$$(4) \qquad Prob(hours_{i,s,t} > h) = \delta_t + \theta_s + \beta(h)Post_{s,t} + \mu_{d,t} + X_{i,t}\gamma + \epsilon_{i,s,t}$$

Concretely, I estimate the previous specification for the probability of working at all ($Prob(hours > 0)$), the probability of working at least 10 hours a week, 20 hours, and so on. I then plot the resulting estimates of $\beta(h)$ as a function of work hours and analyze the slope of this function.

Figure 10 plots the effects of the program on the probability of working at least h hours ($h \in [0, 90]$) and the distribution of work hours by gender of the household head or spouse. The top left figure shows that, in the case of mothers, the program mainly increased the probability of working and had no effects on the probability of working more than 50 hours. The pattern in the figure suggests that the positive average effects on work hours documented in Section IV was mainly driven by entrance into employment: $\beta(h)$ is a non-increasing function of the number of work hours. This result is consistent with a context in which employment for mothers of beneficiary children was rather low (see bottom left panel). The results suggest that fixed costs to work prevented females from working, and that the program resources may have helped females to overcome such costs.

In the case of fathers, the program increased the probability of working more than zero, 10, and 20 hours, had no effects on the probability of working more than 30, 40, and 50 hours, and significantly reduced the probability of working

[23] In 1919, the ILO's first convention established a maximum number of weekly work hours of 48, initially applying to industry jobs. Later on, the ILO established similar guidelines for jobs in other sectors. The Bolivian Labor Act of 1930 adopted the ILO conventions.

more than 60 hours. This suggests that, while the program might have increased the probability of working for some males, it actually reduced the probability of having long working weeks. This decrease is associated with a 9-hour decrease in the top quartile of the distribution of weekly work hours for parents as depicted in Appendix Figure A2.[24] Thus, the program might have allowed overworked adults to afford fewer work hours or to substitute male labor with female labor.

The results from this section are important in two ways. First, they show that although there are decreases in labor supply, the negative effects are concentrated among overworked adults. By allowing the household to add one worker to the labor force, the program relieved male heads from working very long hours. This is critical, as both national and international legislation focusing on worker protection suggest a working week of 48 hours. These behavioral changes could also be welfare-increasing. Jones and Klenow (2016) show that leisure explains differences in welfare across countries. In the Bolivian context, it is possible that the marginal utility from an extra hour of leisure corresponding to overworked fathers is larger than the marginal disutility of working an extra hour in the case of work-constrained mothers.

Second, the results contribute to the literature studying the prevalence of long working hours. Although there is evidence that hours worked are higher in low per-capita GDP countries (Bick, Fuchs-Schndeln and Lagakos, 2018), most of the evidence studying the case of overworked individuals comes from advanced economies and suggests signaling, respect, or addiction can lead to long working hours (Hamermesh and Slemrod, 2008; Landers, Rebitzer and Taylor, 1996; Ellingsen and Johannesson, 2007), and that occupations demanding long work hours can lead to low labor force participation among high-skilled females Cortes and Pan (2017). This paper shows that adults may overwork due to frictions preventing the entrance of an extra worker in developing countries.

[24]The quantile treatment effects in Appendix Figure A2 are estimated using the Recentered Influence Function approach (RIF) proposed by Firpo, Fortin and Lemieux (2009), which provides consistent estimates in the presence of controls.

VII. Concluding Remarks

The results from this paper have a number of policy implications. First, the evidence in this paper suggests that several concerns about negative unintended consequences of cash-transfer programs are unwarranted with respect to developing countries. This paper shows that employment can actually increase among participants in cash transfer programs. Gertler, Martinez and Rubio-Codina (2012) showed that several years after the introduction of a conditional cash transfer program in Mexico, beneficiary households used their savings to make business investments. This paper shows that similar positive unintended consequences can arise in the short run as well.

Second, analyzing the effects of cash-transfer programs on labor supply entails understanding the complex process of within-household reallocation of labor. Although there is evidence of reduction of work hours for males, the reductions are driven by overworked males and are offset by increases in labor supply of spouses. Thus, cash transfers could provide a relief to overworked parents. Previous empirical studies in advanced economies analyze the role of signaling (Landers, Rebitzer and Taylor, 1996), respect or reputation (Ellingsen and Johannesson, 2007), and addiction (Hamermesh and Slemrod, 2008) as possible explanations of long work hours, and that long work hours required by employers affect the occupational choice of highly skilled women(Cortes and Pan, 2017). The results from this paper contribute to these studies by showing that in developing countries adults may overwork due to frictions preventing the entrance of an extra worker.

Third, studies of means-tested cash transfer programs systematically show that labor supply does not seem to respond to cash transfers, but, as in this study, the evidence of increases in labor supply come from non-targeted programs (Ardington, Case and Hosegood, 2009; Salehi-Isfahani and Mostafavi-Dehzooei, 2018). One explanation for this contrast is that the results could be driven by liquidity-constrained households that are not necessarily among the poorest. One implication is that, as several programs expand their coverage, policymakers may

maximize the positive unintended effects of cash transfer programs by targeting subpopulations with limited access to financial services.

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VIII. Figures and Tables

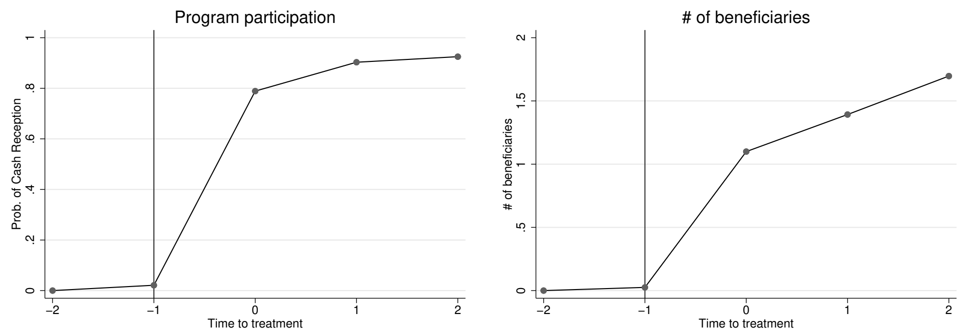

Figure 1. Program Participation

Note: The figure depicts means of self-reported indicators of whether any household member received the program's cash transfer (left panel), and the number of children who received the transfer within each household (right panel). The horizontal axis denotes time to treatment in years. Year 0 corresponds to the first year in which households became eligible. Sample only includes data from the 2007 to 2009 survey waves, as those alone asked whether each school-age child received the transfer during the previous year.

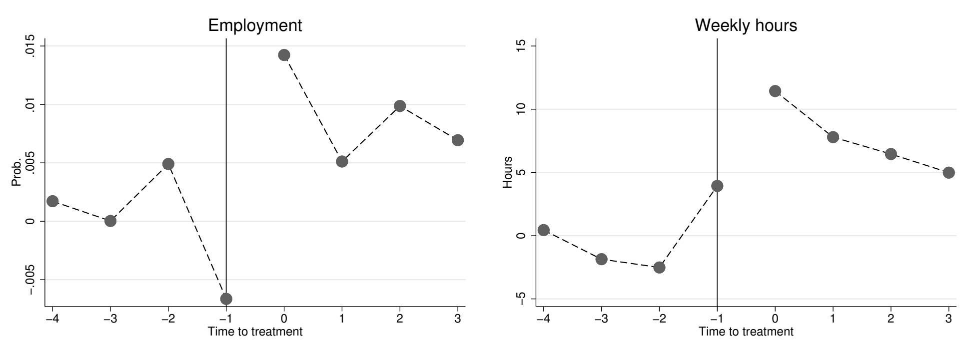

Figure 2. Employment and Work Hours of Household Heads and Spouses before and after the Program's Rollout

Note: The figure depicts means of employment and weekly work hours corresponding to household heads and spouses. Means are normalized with respect to the pre-program mean. Employment is measured as an indicator of whether either the household head or the spouse reported working during the week preceding the interview. Weekly work hours are computed as the sum of work hours corresponding to either the head of households or the spouse. Weekly work hours are winsorized with respect to the top 1% of the distribution of work hours in each survey wave. The horizontal axis denotes time to treatment in years. Year 0 corresponds to the first year in which households became eligible.

FIGURE 3. FLEXIBLE DIFFERENCE-IN-DIFFERENCES EFFECTS OF THE PROGRAM ON HOUSEHOLD EMPLOYMENT

Note: The figure depicts OLS coefficients from equation (1). All regressions control for region-year fixed effects, gender, age, and primary language of household head, average age of children in school age, household size, and interactions between the latter two variables and year fixed effects. Standard errors are clustered at the municipality level (291 clusters). Each coefficient estimates differences in differences on work outcomes between eligible and non-eligible households with respect to the period just before the program was implemented ($\tau = -1$). The dependent variable in the top left panel is the count of working adults in each household. The dependent variable in the top right panel is the total number of work hours provided by each household. The bottom panel reports effects on the probability that either the head or the spouse work (left) and the number of weekly work hours associated with the head and the spouse (right). Weekly work hours are winsorized with respect to the top 1% of the distribution of work hours in each survey wave. The horizontal axis denotes time to treatment in years. Year 0 corresponds to the first year in which households became eligible.

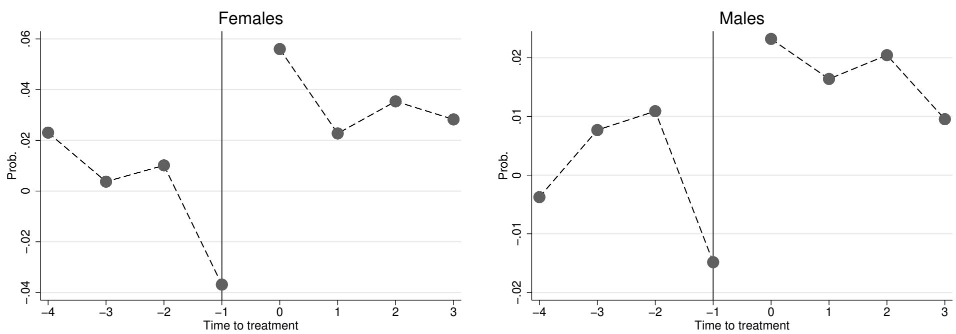

Figure 4. Employment of Household Heads and Spouses before and after the Program's Roll-out, by Gender

Note: The figure depicts means of employment corresponding to female household heads and spouses (left) and male heads and spouses (right). Means are normalized with respect to the pre-program mean. Employment is measured as an indicator of whether either the household head or the spouse reported working during the week preceding the survey interview. The horizontal axis denotes time to treatment in years. Year 0 corresponds to the first year in which households became eligible.

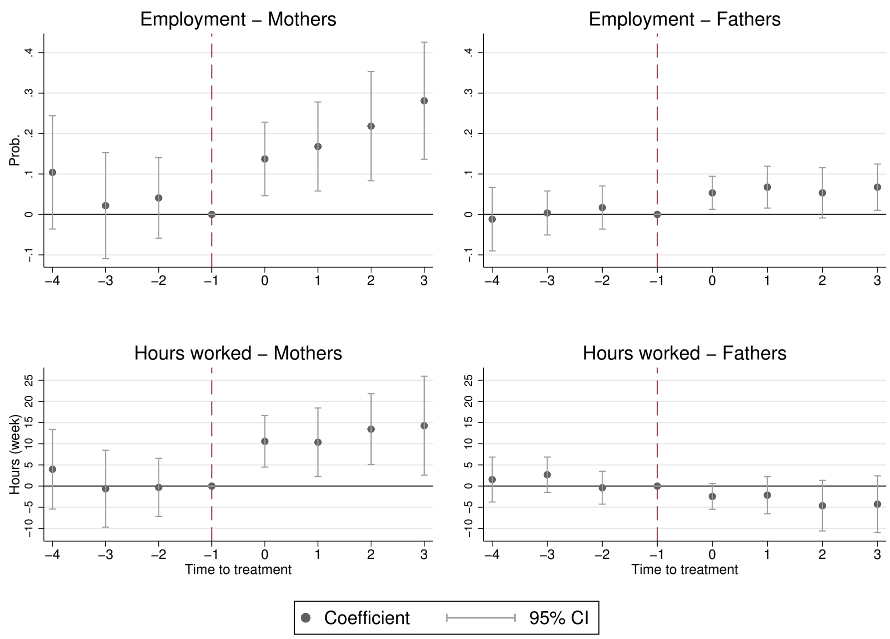

Figure 5. Flexible Difference-in-Differences Effects of the Program on Household Employment, by Gender

Note: The figure depicts OLS coefficients from equation (1). All regressions control for state-year fixed effects, gender, age, and primary language of household head, number of children under 5 years old, average age of children in school age, household size, and interactions between the latter two variables and year fixed effects. Standard errors are clustered at the municipality level (291 clusters). Each coefficient estimates differences-in-differences on work outcomes between eligible and non-eligible households, with respect to the period just before the program was implemented ($\tau = -1$). The dependent variable in the top panels is the probability of working corresponding to household heads or spouses, by gender. The dependent variable in the bottom panels measures the weekly work hours corresponding to household heads or spouses, by gender. Weekly work hours are winsorized with respect to the top 1% of the distribution of work hours in each survey wave. The horizontal axis denotes time to treatment in years. Year 0 corresponds to the first year in which households became eligible.

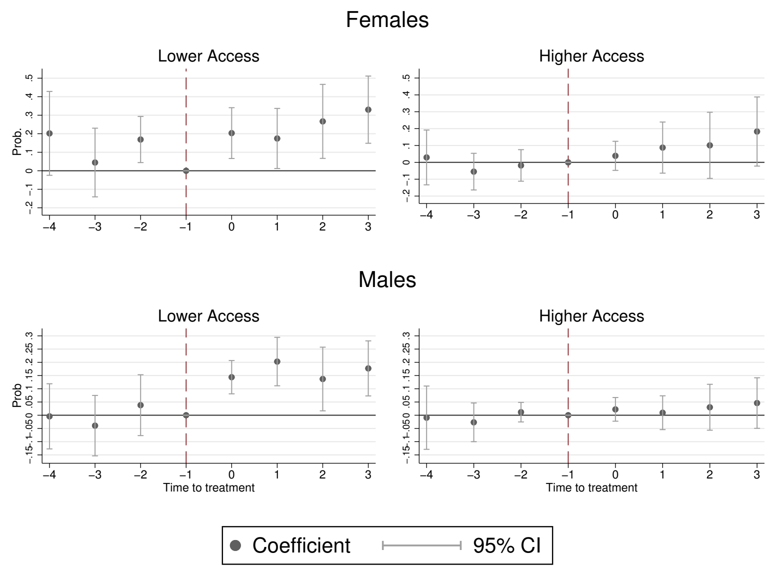

Figure 6. Flexible Difference-in-Differences Effects of the Program on Employment, by Pre-Program Access to Financial Services

Note: The figure depicts OLS coefficients from equation (1). All regressions control for state-year fixed effects, gender, age, and primary language of household head, number of children under 5 years old, average age of children in school age, household size, and interactions between the latter two variables and year fixed effects. Each coefficient estimates differences in differences on work outcomes between eligible and non-eligible households with respect to the period just before the program was implemented ($\tau = -1$). The dependent variable in the regressions is the probability of working. Each panel reports results by parental gender and by access to financial services. High Access: number of pre-program financial branches per 100,000 inhabitants above the median. Low Access: number of pre-program financial branches per 100,000 inhabitants below the median. The horizontal axis denotes time to treatment in years. Year 0 corresponds to the first year in which households became eligible. Standard errors are clustered at the municipality level (291 clusters).

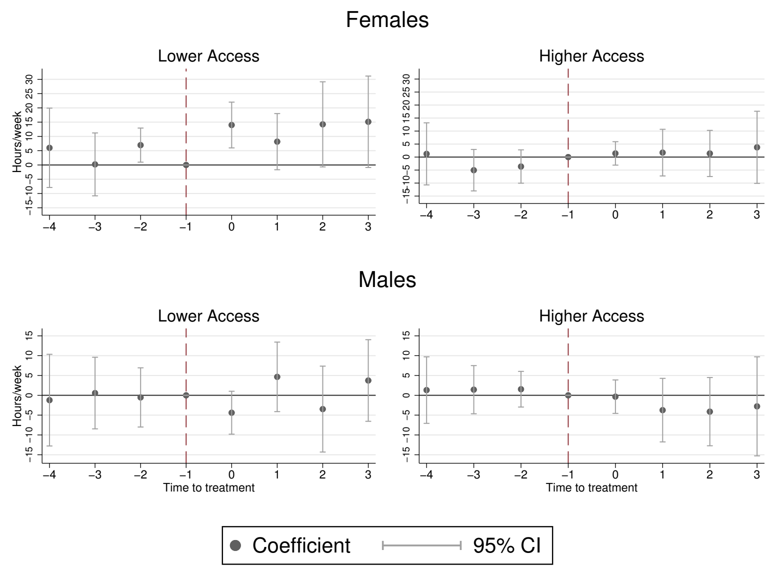

Figure 7. Flexible Difference-in-Differences Effects of the Program on Hours Worked, by Pre-Program Access to Financial Services

Note: The figure depicts OLS coefficients from equation (1). All regressions control for state-year fixed effects, gender, age, and primary language of household head, number of children under 5 years old, average age of children in school age, household size, and interactions between the latter two variables and year fixed effects. Each coefficient estimates differences in differences on work outcomes between eligible and non-eligible households with respect to the period just before the program was implemented ($\tau = -1$). The dependent variable in the regressions is the number of weekly work hours. Each panel reports results by parental gender and by access to financial services. High Access: number of pre-program financial branches per 100,000 inhabitants above the median. Low Access: number of pre-program financial branches per 100,000 inhabitants below the median. The horizontal axis denotes time to treatment in years. Year 0 corresponds to the first year in which households became eligible. Standard errors are clustered at the municipality level (291 clusters).

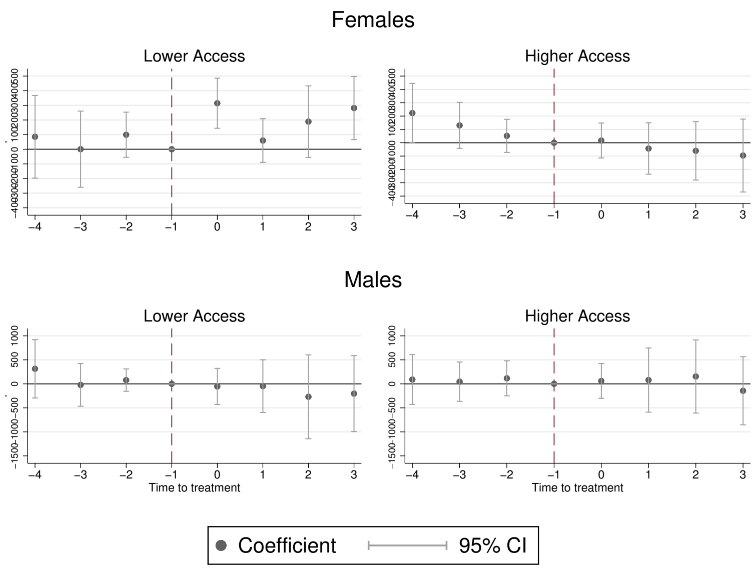

Figure 8. Flexible Difference-in-Differences Effects of the Program on Earnings, by Pre-Program Access to Financial Services

Note: The figure depicts OLS coefficients from equation (1). All regressions control for state-year fixed effects, gender, age, and primary language of household head, number of children under 5 years old, average age of children in school age, household size, and interactions between the latter two variables and year fixed effects. Each coefficient estimates differences in differences on work outcomes between eligible and non-eligible households with respect to the period just before the program was implemented ($\tau = -1$). The dependent variable in the regressions is labor earnings. Each panel reports results by parental gender and by access to financial services. High Access: number of pre-program financial branches per 100,000 inhabitants above the median. Low Access: number of pre-program financial branches per 100,000 inhabitants below the median. The horizontal axis denotes time to treatment in years. Year 0 corresponds to the first year in which households became eligible. Standard errors are clustered at the municipality level (291 clusters).

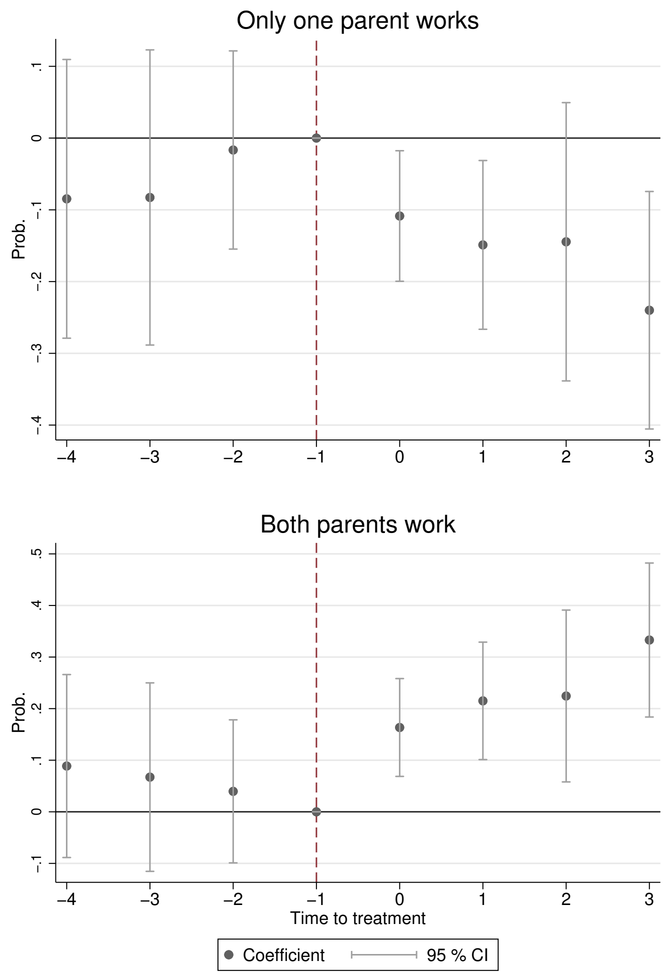

Figure 9. Effects on Work Outcomes for Biparental Households

Note: The figure depicts OLS coefficients from equation (1) using only the subsample of biparental households. All regressions control for state-year fixed effects, gender, age, and primary language of household head, number of children under 5 years old, average age of children in school age, household size, and interactions between the latter two variables and year fixed effects. Standard errors are clustered at the municipality level (291 clusters). Each coefficient estimates differences in differences on work outcomes between eligible and non-eligible households with respect to the period just before the program was implemented ($\tau = -1$). The dependent variable in the top panel is the probability that only the head or the head's spouse works. The dependent variable in the bottom panel is the probability that both the head and spouse work. The horizontal axis denotes time to treatment in years. Year 0 corresponds to the first year in which households became eligible.

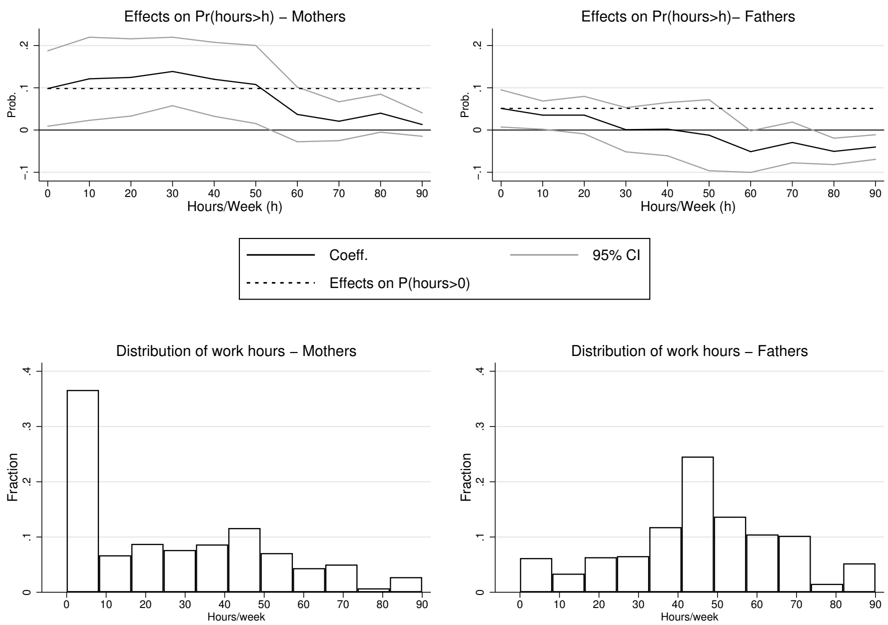

Figure 10. Effects on Working at Least h Hours per Week

Note: The top panel depicts treatment effects estimated through OLS based on equation (4). All regressions control for state-year fixed effects, gender, age, and primary language of household head, number of children under 5 years old, average age of children in school age, household size, and interactions between the latter two variables and year fixed effects. Each coefficient estimates differences in differences on the probability of working at least h hours between adults from households of exposed and unexposed children, before and after the program. Standard errors are clustered at the municipality level (291 clusters). The bottom panel depicts the distribution of work hours. Work hours are coded as zero for non-working adults.

Table 1—Effects of the Program on Adult Labor Supply

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Household head and head's spouse (Parents)												
	Prob. of working				Total hours/week				Labor earnings			
Post	0.019**	0.020*	0.020	0.021	5.784**	6.508**	7.958***	8.640***	437.208***	332.328*	131.279	106.371
	(0.009)	(0.012)	(0.015)	(0.014)	(2.291)	(2.572)	(2.806)	(2.861)	(143.773)	(169.395)	(179.774)	(184.518)
Observations	10,873	10,873	10,873	10,873	10,873	10,873	10,873	10,873	10,873	10,873	10,873	10,873
R-squared	0.035	0.037	0.038	0.045	0.248	0.248	0.250	0.256	0.252	0.255	0.283	0.288
Clusters	291	291	291	291	291	291	291	291	291	291	291	291
Mean DV	0.973	0.973	0.973	0.973	92.03	92.03	92.03	92.03	1126	1126	1126	1126
Panel B: All adults in the household												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	# of working adults				Total hours/week				Adult labor earnings			
Post	0.131*	0.109*	0.122*	0.122*	7.504**	5.351	7.052*	7.491**	254.938***	180.548	198.181	175.761
	(0.073)	(0.064)	(0.071)	(0.071)	(3.206)	(3.688)	(3.741)	(3.691)	(96.330)	(137.212)	(156.962)	(154.663)
Observations	10,873	10,873	10,873	10,873	10,873	10,873	10,873	10,873	10,873	10,873	10,873	10,873
R-squared	0.355	0.355	0.358	0.366	0.232	0.233	0.236	0.244	0.232	0.233	0.234	0.244
Avg. age X year FE	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
HH size X year FE	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES
Region X year FE	NO	NO	NO	YES	NO	NO	NO	YES	NO	NO	NO	YES
Clusters	291	291	291	291	291	291	291	291	291	291	291	291
Mean DV	1.686	1.686	1.686	1.686	81.32	81.32	81.32	81.32	1388	1388	1388	1388

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

Note: The table reports OLS coefficients from equation (2). All regressions control for year fixed effects and dummies corresponding to the years of schooling of the youngest child in the household. Each coefficient estimates differences in differences on work outcomes between eligible and non-eligible households with respect to the pre-program periods. Panel A reports results corresponding to work outcomes of parents, while Panel B reports effects of the program on work outcomes of adults in the household. Columns (1), (5), and (9) report estimates that control for state fixed effects, gender, age, and primary language of household head, number of children under 5 years old, average age of children in school, and household size. Columns (2), (6), and (10) include year fixed effects interacted with the average age of children in school in each household. Columns (3), (7) and (11) also include year fixed effects interacted with household size. Finally, Columns (4), (8), and (12) add state-year fixed effects. Weekly work hours and labor earnings are winsorized with respect to the top 1% of the distribution of work hours and earnings in each survey wave. Labor earnings include earnings from wage labor and from self-employment. All estimations are conducted over a sample including four pre- and post-program years. Standard errors are clustered at the municipality level.

Table 2—Effects of the Program on Parental Work Outcomes, by Gender

	(1)	(2)	(3)	(4)	(5)	(6)
	Mothers			Fathers		
	Works	Hours/week	Earnings	Works	Hours/week	Earnings
Post	0.098**	7.829***	148.778*	0.052**	0.517	60.624
	(0.046)	(2.490)	(77.039)	(0.022)	(2.148)	(117.413)
Chi2 stat(Females=Males)	1.31	5.66	0.63			
p-val (Females=Males)	0.25	0.02	0.43			
Observations	10,331	10,331	10,331	9,070	9,070	9,070
R-squared	0.079	0.057	0.177	0.075	0.069	0.17
Clusters	291	291	291	290	290	290
Mean DV	0.641	32.67	295.2	0.926	59.37	831.2

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

Note: The table reports OLS coefficients from equation (2). All regressions control for year fixed effects and dummies corresponding to the years of schooling of the youngest child in the household. Regressions also include state fixed effects, gender, age, schooling, and primary language of household head, the number of children under 5 in each household, average age of children in school, household size, and interactions of average age, household size, and state with year fixed effects. Each coefficient estimates differences in differences on work outcomes between eligible and non-eligible households with respect to the pre-program periods. Weekly work hours and labor earnings are winsorized with respect to the top 1% of the distribution of work hours and earnings in each survey wave. Labor earnings include earnings from wage labor and from self-employment. All estimations are conducted over a sample including four pre- and post-program years. Standard errors are clustered at the municipality level.

Table 3—Effects of the Program on Child Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Household-level data				Child-level data			
VARIABLES	# enrolled	# attend	# working	Hours worked	Enrolled	Attends	Works	Hours worked
Post	0.119	0.078	0.012	-0.771	0.015	-0.006	0.017	0.461
	(0.080)	(0.091)	(0.070)	(2.674)	(0.015)	(0.016)	(0.019)	(0.833)
Observations	11,013	10,051	10,051	10,051	22,150	28,502	27,427	28,519
R-squared	0.365	0.356	0.248	0.164	0.298	0.291	0.309	0.234
Clusters	283	291	291	291	284	293	293	293
Mean DV	2.291	2.235	0.520	13.03	0.924	0.927	0.256	5.854

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

Note: The table reports OLS coefficients from equation (2). All regressions control for year fixed effects and dummies corresponding to the years of schooling of the youngest child in the household. All regressions also include state fixed effects, gender, age, schooling, and primary language of household head, the number of children under 5 in each household, average age of children in school, household size, and interactions of average age, household size, and state with year fixed effects. Columns (1) to (3) report differences in differences on child outcomes between eligible and non-eligible households with respect to the pre-program periods. Columns (4) to (6) exploit variation at the child level and report differences in differences on child outcomes between eligible and non-eligible children with respect to the pre-program periods. All estimations are conducted over a sample including four pre- and post-program years. Observations in Columns (1) and (4) are lower, as the period of reference of the survey question regarding enrollment is the beginning of the academic year. Standard errors are clustered at the municipality level.

Table 4—Effects of the Program on Family Businesses

	(1)	(2)	(3)	(4)	(5)
	Panel A: Heads or Spouses (Either parent)				
	Self-employed	Employee	Biz. Revenues	Biz. Spending	Any Biz. Spending
Post	0.047	0.004	-136.133	-88.720	-0.040
	(0.032)	(0.033)	(308.945)	(289.512)	(0.046)
Observations	10,873	10,873	10,873	9,233	9,233
R-squared	0.135	0.194	0.632	0.656	0.156
Clusters	291	291	291	283	283
Mean DV	0.652	0.388	4785	3531	0.655
	Panel B: Mothers				
	Self-employed	Employee	Biz. Revenues	Biz. Spending	Any Biz. Spending
Post	0.130***	-0.032	79.153	73.766	0.078**
	(0.033)	(0.031)	(112.659)	(112.081)	(0.039)
Observations	10,331	10,331	10,331	8,793	8,793
R-squared	0.148	0.141	0.061	0.048	0.082
Clusters	291	291	291	283	283
Mean DV	0.481	0.160	1183	900.6	0.287
	Panel C: Fathers				
	Self-employed	Employee	Biz. Revenues	Biz. Spending	Any Biz. Spending
Post	-0.003	0.055	-267.734	-196.026	-0.086*
	(0.041)	(0.034)	(187.744)	(178.104)	(0.047)
Observations	9,070	9,070	9,070	7,710	7,710
R-squared	0.161	0.168	0.032	0.031	0.092
Clusters	290	290	290	282	282
Mean DV	0.558	0.368	3601	2630	0.522

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

Note: The table reports OLS coefficients from equation (2). All regressions control for year fixed effects and dummies corresponding to the years of schooling of the youngest child in the household. All regressions also include state fixed effects, gender, age, schooling, and primary language of household head, the number of children under 5 in each household, average age of children in school, household size, and interactions of average age, household size, and state with year fixed effects. Panel A reports effects on outcomes for both parents (household heads and spouses), while Panels B and C report effects on outcomes of female and male parents. All estimations are conducted over a sample including four pre- and post-program years. Standard errors are clustered at the municipality level.

Table 5—Effects of the Program on Work Outcomes by Access to Financial Services

Panel A: Household heads or spouses				
	(1) Worked	(2) Hours/week	(3) Earnings	(4) Self-employed
Post	0.046**	8.129**	239.849	0.062
	(0.021)	(4.053)	(388.039)	(0.069)
Post X Access	-0.040**	-3.829	-230.525	-0.025
	(0.019)	(3.550)	(391.717)	(0.060)
Observations	8,150	8,150	8,150	8,150
R-squared	0.054	0.260	0.281	0.102
Clusters	98	98	98	98
Mean DV (Low Access)	0.962	93.10	1130	0.568
Panel B: Female heads or spouses				
	(1) Worked	(2) Hours/week	(3) Earnings	(4) Self-employed
Post	0.176**	10.307***	252.357**	0.124*
	(0.082)	(3.393)	(108.273)	(0.066)
Post X Access	-0.129*	-6.058*	-116.400	-0.076
	(0.077)	(3.381)	(102.314)	(0.066)
Observations	7,775	7,775	7,775	7,775
R-squared	0.070	0.059	0.157	0.083
Clusters	98	98	98	98
Mean DV (Low Access)	0.644	32.10	256.8	0.326
Panel C: Male heads or spouses				
	(1) Worked	(2) Hours/week	(3) Earnings	(4) Self-employed
Post	0.073**	-5.424*	-65.054	0.040
	(0.029)	(2.844)	(133.156)	(0.064)
Post X Access	-0.032	4.908*	254.229	-0.004
	(0.029)	(2.678)	(153.840)	(0.067)
Observations	6,764	6,764	6,764	6,764
R-squared	0.105	0.093	0.148	0.131
Clusters	98	98	98	98
Mean DV (Low Access)	0.918	61	873.7	0.437

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

Note: The table reports OLS coefficients from equation (3). All regressions control for year fixed effects and dummies corresponding to the years of schooling of the youngest child in the household. Regressions also include state fixed effects, gender, age, schooling, and primary language of household head, the number of children under 5 in each household, average age of children in school, household size, and interactions of average age, household size, and state with year fixed effects. Panel A reports effects on outcomes for household heads and spouses, Panels B and C report effects on outcomes of female and male parents. All estimations are conducted over a sample including four pre- and post-program years. Standard errors are clustered at the municipality level.

Table 6—Effects of the Program on the Probability of Working for Biparental Households

VARIABLES	(1) Either	(2) Only one	(3) Only males	(4) Only Females	(5) Both
Post	0.036	-0.109**	-0.099**	-0.010	0.144***
	(0.022)	(0.047)	(0.049)	(0.012)	(0.051)
Observations	8,528	8,528	8,528	8,528	8,528
R-squared	0.070	0.077	0.077	0.037	0.087
Avg. age X year FE	YES	YES	YES	YES	YES
HH size X year FE	YES	YES	YES	YES	YES
Clusters	289	289	289	289	289
Mean DV	0.963	0.388	0.363	0.0253	0.575

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

Note: The table reports OLS coefficients from equation (2), restricting the sample to only biparental households (household head lives with a partner or spouse). Column (1) reports the probability that either parent or both work. Column (2) reports the probability that only one parent works. Columns (3) and (4) report results for the probability that only fathers or mothers work. Column (5) reports results for the probability that both parents work. Regressions control for year fixed effects and dummies corresponding to the years of schooling of the youngest child in the household. All regressions also include state fixed effects, gender, age, schooling, and primary language of household head, the number of children under 5 in each household, average age of children in school, household size, and interactions of average age, household size and state with year fixed effects. Each coefficient estimates differences in differences on work outcomes between eligible and non-eligible households with respect to the pre-program periods. All estimations are conducted over a sample including four pre- and post-program years. Standard errors are clustered at the municipality level.

Appendix: Supporting Evidence

Table A1—Summary Statistics at Baseline (year 2005)

	(1)	(2)	(3)	(4)
	Study sample N of hhs.=1539 Mean	**Early treatment N of hhs.=1134 Mean**	**Late treatment N of hhs.=405 Mean**	**Full sample N of hhs.=2,477 Mean**
Panel A: Work outcomes - All household adults				
# of working adults	1.7	1.7	1.6	1.7
Weekly work hours	81	83	75	79
# of self-employed adults	0.7	0.71	0.7	0.76
Panel B: Work outcomes - Female heads or female spouses (Mothers)				
Works	0.66	0.64	0.72	0.62
Weekly work hours	29	29	30	27
Weekly non-zero work hours	50	49	50	43
Self-employed	0.31	0.29	0.37	0.29
Panel C: Work outcomes - Male heads or male spouses (Fathers)				
Works	0.93	0.93	0.93	0.75
Weekly work hours	48	49	45	38
Weekly non-zero work hours	52	53	48	51
Self-employed	0.45	0.44	0.47	0.42
Panel D: Child outcomes (6 to 18 years old)				
Probability of working	0.25	0.19	0.31	0.25
Weekly work hours	5.9	3.7	8.2	6.5
Weekly non-zero work hours	24	19	27	25
Enrollment	0.93	0.96	0.92	0.91
Attendance	0.93	0.96	0.92	0.91
Panel E: Household characteristics				
Urban area	0.65	0.63	0.71	0.63
Age (head)	44	43	46	43
Primary language is Spanish (head)	0.54	0.53	0.56	0.57
Years of schooling (head)	7.1	6.9	7.4	7.6
# of hh members	5	5.3	4.2	5.1
# of adults	2.3	2.3	2.2	2.3
# of children under 5	0.45	0.54	0.21	0.64
Poverty incidence	0.65	0.7	0.53	0.61

Note: The table reports means and standard deviations of baseline work outcomes and demographic characteristics for different subsamples. Column (1) presents means corresponding to households whose youngest child had had at least one and no more than eight years of schooling. Columns (2) and (3) report means corresponding to households that entered the treatment group during the first and subsequent rounds of the program. Column (4) presents means of the sample of households with any school-age children during the pre-program year.

Table A2—Flexible Difference-in-Differences Estimates of the Effect of the Program on Adult Labor Supply

Panel A: Effects of the program on work outcomes.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Parents (head or spouse)		Total adults		Mothers		Fathers	
Time to treatment	Prob.	Hours	#	Hours	Prob.	Hours	Prob.	Hours
-4	0.009	5.520	-0.046	4.239	0.104	3.975	-0.012	1.546
	(0.026)	(5.546)	(0.118)	(7.519)	(0.072)	(4.794)	(0.040)	(2.707)
-3	-0.020	2.062	-0.114	0.345	0.022	-0.616	0.004	2.679
	(0.020)	(5.138)	(0.120)	(5.359)	(0.067)	(4.638)	(0.028)	(2.132)
-2	0.011	-0.696	-0.054	-4.077	0.041	-0.309	0.017	-0.387
	(0.010)	(3.964)	(0.095)	(3.837)	(0.051)	(3.502)	(0.027)	(1.981)
0	0.036**	8.140***	0.134**	4.439	0.137***	10.577***	0.053**	-2.438
	(0.017)	(3.064)	(0.059)	(3.419)	(0.046)	(3.112)	(0.021)	(1.557)
1	0.044**	8.225	0.243**	5.618	0.168***	10.367**	0.067**	-2.143
	(0.022)	(5.564)	(0.094)	(6.923)	(0.056)	(4.131)	(0.026)	(2.236)
2	0.051	8.841	0.259**	4.728	0.218***	13.464***	0.053*	-4.623
	(0.032)	(5.436)	(0.120)	(7.418)	(0.069)	(4.271)	(0.032)	(3.047)
3	0.074***	9.995	0.250	1.082	0.281***	14.263**	0.067**	-4.269
	(0.026)	(7.544)	(0.195)	(9.777)	(0.074)	(5.963)	(0.029)	(3.417)
Observations	10,873	10,873	10,873	10,873	10,332	10,873	9,070	10,873
R-squared	0.046	0.256	0.367	0.244	0.079	0.062	0.073	0.474
Panel B: $H_0 : \beta_{-4} + \beta_{-3} + \beta_{-2} = 0$								
F-stat	0.00	0.77	0.54	0.00	0.95	1.19	0.41	0.94
P- val	0.99	0.60	0.46	0.97	0.29	0.80	0.91	0.42
Panel C: $H_0 : \beta_{-4} = \beta_{-3} = \beta_{-2} = 0$								
F-stat	3.03	0.27	0.40	0.96	1.25	0.06	0.01	0.43
P-val	0.03	0.51	0.75	0.41	0.33	0.31	0.75	0.51

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

Note: The table reports OLS coefficients from equation (1). All regressions control for state-year fixed effects, gender, age, and primary language of household head, average age of children in school, household size, and interactions between the latter two variables and year fixed effects. Each coefficient estimates differences in differences on work outcomes between eligible and non-eligible households, with respect to the period just before the program was implemented ($\tau = -1$). The dependent variable in Columns (1) and (2) is the probability that either parent works and the number of weekly work hours associated with parents. Columns (3) and (4) report results for the count of working adults in each household and for the total hours worked by all the adults in the household. Columns (5) and (7) report results for the probability of employment, and Columns (6) and (8) report results for work hours. Weekly work hours are winsorized with respect to the top 1% of the distribution of work hours in each survey wave. The bottom panel presents F-statistics and p-values associated with tests of the null hypotheses that the sum of pre-program coefficients is different than zero and that the pre-program coefficients of each column are jointly equal to zero. Year 0 corresponds to the first year in which households became eligible. Standard errors are clustered at the municipality level (291 clusters).

Table A3—Robustness to Alternative Specifications

Panel A: Controlling for group linear trends

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Parents (Head or spouse)		Total adults		Female head or spouse		Male head or spouse	
	Prob.	Hours	#	Hours	Prob.	Hours	Prob.	Hours
Post	0.023	9.171***	0.122*	7.359**	0.118**	9.743***	0.051**	-0.572
	(0.014)	(3.024)	(0.069)	(3.590)	(0.048)	(2.869)	(0.022)	(1.552)
Observations	10,873	10,873	10,873	10,873	10,332	10,873	9,070	10,873
R-squared	0.045	0.256	0.366	0.244	0.079	0.062	0.072	0.474
Sample	1st-8th	1st-8th	1st-8th	1st-8th	1st-8th	1st-8th	1st-8th	1st-8th
Group linear trends	YES	YES	YES	YES	YES	YES	YES	YES
Clusters	291	291	291	291	291	291	290	291
Mean DV	0.971	90.05	1.702	80.42	0.644	31.69	0.927	58.37

Panel B: Restricting the sample to children in 5th to 7th grade

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Parents (Head or spouse)		Total adults		Female head or spouse		Male head or spouse	
	Prob.	Hours	#	Hours	Prob.	Hours	Prob.	Hours
Post	0.034*	14.017***	0.102	10.370**	0.128**	14.858***	0.034	-0.841
	(0.019)	(4.090)	(0.062)	(4.521)	(0.056)	(4.471)	(0.028)	(1.841)
Observations	2,811	2,811	2,811	2,811	2,623	2,811	2,263	2,811
R-squared	0.069	0.295	0.428	0.300	0.095	0.090	0.109	0.520
Sample	5th-7th	5th-7th	5th-7th	5th-7th	5th-7th	5th-7th	5th-7th	5th-7th
Group linear trends	NO	NO	NO	NO	NO	NO	NO	NO
Clusters	256	256	256	256	252	256	249	256
Mean DV	0.949	91.11	1.685	77.45	0.600	31.10	0.893	60.01

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

Note: The table reports OLS coefficients from equation (2). All regressions control for year fixed effects and dummies corresponding to the years of schooling of the youngest child in the household. All regressions also include state fixed effects, gender, age, schooling, and primary language of household head, the number of children under 5 in each household, average age of children in school, household size, and interactions of average age, household size, and state with year fixed effects. Each coefficient estimates differences in differences on work outcomes between eligible and non-eligible households with respect to the pre-program periods. Weekly work hours and labor earnings are winsorized with respect to the top 1% of the distribution of work hours and earnings in each survey wave. Labor earnings include earnings from wage labor and from self-employment. Panel A reports results from including group-of-entry linear trends. Panel B reports results from only focusing on a subset of households whose youngest children are in fifth, sixth, and seventh grades. Standard errors are clustered at the municipality level (291 clusters).

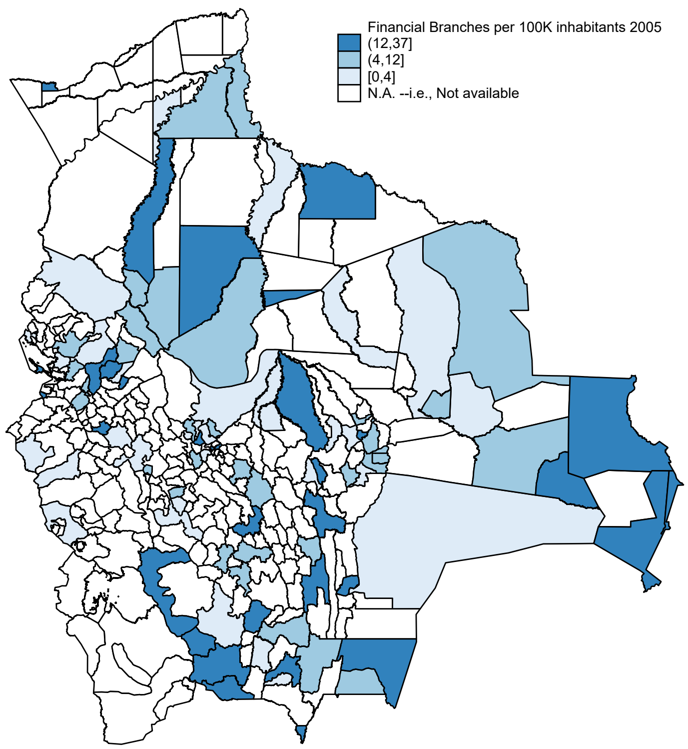

Figure A1. Geographical Distribution of the Number of Financial Branches per 100,000 Inhabitants in 2005

Note: The figure illustrates the distribution of financial branches across municipalities in Bolivia in 2005. Data, only available for 112 municipalities, is presented in scales of blue.

Table A4—Effects of the Program on Family Businesses by Access to Financial Services

Panel A: Household heads or spouses (Either parent)

	(1) Revenues	(2) Biz. Spending	(3) Any biz. Spending
(a) Post	-53.208	-21.682	0.067*
	(362.272)	(389.360)	(0.038)
(b) Post X High Access	-18.043	-46.347	-0.057
	(485.872)	(496.997)	(0.043)
Effect High Acccess (a+b)	-71.25	-68.03	0.0100
p-val	0.871	0.862	0.770
Observations	8,150	8,150	8,150
R-squared	0.618	0.614	0.298
Clusters	98	98	98
Mean DV (Low Access)	4672	3434	0.650

Panel B: Mothers

	(1) Revenues	(2) Biz. Spending	(3) Any biz. Spending
(a) Post	178.990	211.637*	0.160***
	(131.409)	(127.201)	(0.049)
(b) Post X High Access	-43.481	-123.645	-0.083*
	(145.961)	(146.718)	(0.047)
Effect High Acccess (a+b)	135.5	87.99	0.0778
p-val	0.453	0.614	0.0248
Observations	7,775	7,775	7,775
R-squared	0.056	0.055	0.114
Clusters	98	98	98
Mean DV (Low Access)	1014	759.2	0.319

Panel C: Fathers

	(1) Revenues	(2) Biz. Spending	(3) Any biz. Spending
(a) Post	-751.872**	-607.603	-0.038
	(346.504)	(366.376)	(0.060)
(b) Post X High Access	759.079*	625.568	0.012
	(452.650)	(418.454)	(0.066)
Effect High Acccess (a+b)	7.207	17.97	-0.0257
p-val	0.974	0.904	0.616
Observations	6,764	6,764	6,764
R-squared	0.031	0.044	0.138
Clusters	98	98	98
Mean DV (Low Access)	3658	2675	0.543

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

Note: The table reports OLS coefficients from equation (3). All regressions control for year fixed effects and dummies corresponding to the years of schooling of the youngest child in the household. Regressions also include state fixed effects, gender, age, schooling, and primary language of household head, the number of children under 5 in each household, average age of children in school, household size, and interactions of average age, household size, and state with year fixed effects. Panel A reports effects on outcomes for household heads and spouses, while Panels B and C report effects on outcomes of mothers and fathers. All estimations are conducted over a sample including four pre- and post-program years. The estimation sample only includes data from municipalities with available data regarding the number of financial branches in 2005, the baseline year. Standard errors are clustered at the municipality level.

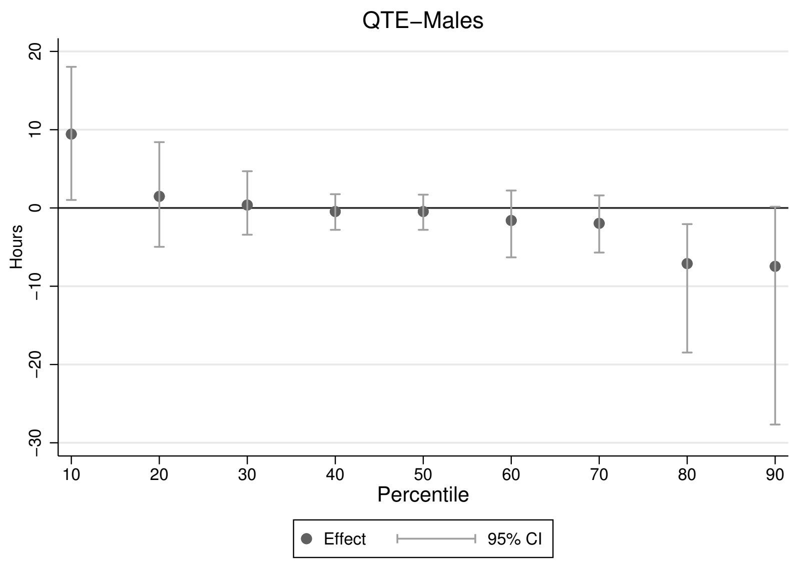

Figure A2. Quantile Treatment Effects: Weekly Hours Worked (fathers)

Note: The figure presents estimates from quantile regressions following the specification in equation (2), and estimated using the RIF regression approach following Firpo, Fortin and Lemieux (2009). Confidence bands are computed by using block-bootstrapped standard errors after 500 iterations, clustered at the municipality level.