ESSAYS ON DEVELOPMENT AND LABOR ECONOMICS

by

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A DISSERTATION

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DISSERTATION ABSTRACT

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In chapter II and III of my dissertation I strive to understand the potential that family policies have to enhance gender equality in paid and un-paid work in low- and middle-income countries, by analyzing two distinct policies implemented in Uruguay and Ecuador. First, I study a unique subsidy implemented in Uruguay in 2014 that allows mothers to work half-time while receiving full-time pay for four months after maternity leave ends. Utilizing eleven years of employment survey data in a difference-in-differences and triple difference framework I find an increase in the likelihood of employment in the short-run and possible increases in employment in the middle-run driven by a higher portion of part-time work arrangements. These findings indicate that policies easing the transition back from maternity leave can increase female labor force participation and reduce "child-

penalties" in the short and medium-run. Second, I find that paid paternity leave of short duration is effective at increasing the time fathers spend childrearing in the context of Ecuador. More fathers, as a results of the policy, reported spending any time with their children, and the number of hours they report childrearing increased by 20%, up to 4 years after the end of paternity leave benefits. Interestingly, the leave does not lead to changes in employment, number of hours of paid work, or participation in other types of housework, which implies a substitution away from leisure and into time childrearing. Lastly, in chapter IV, my co-authors and I developed a randomized control trial to study the effects of financial inclusion in rural communities in Uganda. In this study existing savings groups (informal finance institutions) gained access to formal group loans from a commercial bank. We show that the bank loan stimulated an immediate and sizable increase in internal lending, which is sustained over time. As a result, members of treated groups had temporarily lower rates of food insecurity, and point estimates suggest sizable increases in income and microenterprise size (which are not statistically significant). However, groups assigned to loans experienced significantly more turnover, suggesting that the possibility of external financing generates powerful selection effects.

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Para mi familia

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CHAPTER I

INTRODUCTION

My dissertation is motivated by the facts that globally women are paid approximately 20 percent less than men, they suffer larger incidences of poverty, and, when we account for their unpaid and paid labor, they spend significantly more than time working than men (International Labor Organization, 2019). To decrease these egregious inequalities requires not only a cultural change, but policies and programs that put the experience of women at the center, increasing their access to financial products, promoting the reconciliation of work and family, and fostering egalitarian values of household responsibilities among men. In chapter II and III of my dissertation I strive to understand the potential that family policies have to enhance gender equality in paid and un-paid work in low- and middle-income countries, by analyzing two distinct policies implemented in Uruguay and Ecuador. Lastly, in chapter 3, co-authored with Alfredo Burlando and Jessica Goldberg, we study the effects of increased financial inclusion on the wellbeing of members of saving groups in rural communities in Uganda, who are mostly women.

In chapter II, I study a unique parental care subsidy implemented in Uruguay that allows parents to work half-time time while receiving full-time pay to care for a newborn. Even though this subsidy is gender neutral, in practice, *98% of beneficiaries are women.* This policy allows for a more gradual return to work after maternity leave which in turn might increase labor force participation and job retention in the short-run for mothers with a young child (Schott, 2012). Using nationally representative survey data, I analyze how this policy affects female employment and income. In a difference-in-differences and triple differences specification, I compare mothers exposed to the policy (treatment group) to ineligible mothers and fathers married to ineligible mothers (control group).

I find that mothers exposed to the policy are more likely to be employed following the implementation of the parental care subsidy in the short and medium-term. I find an increase in the likelihood of employment of 17% for eligible mothers up to 1 year after childbirth, and a decrease in hours worked of 5% without a significant reduction in income up to 3 years after childbirth. These findings demonstrate that policies easing the transition back from maternity leave can increase female labor force participation and reduce "child-penalties" in the short and medium-run. This is the first paper to evaluate a parental leave policy with the *work half-time, receive full-time* pay characteristics. Understanding the effects of this unique approach provides new insights and can inform policy discussions and innovation.

The influence of parental leave policies goes beyond female employment. Family policies directly impact the quantity and quality of early childhood investments. In chapter III, I examine how the implementation of 15 days of paid paternity leave in Ecuador affects paternal involvement in childrearing, housework, and labor market outcomes. Even though this policy provides just a few days of leave, the special timing of these days could set a precedent for the family dynamics and task divisions in the household that otherwise would be determined by traditional gender norms. If fathers are present during the hospital stay and early days at home, it is possible they will begin domestic routines that will continue to influence child rearing well after paternal leave has expired. Using data from the Ecuadorian National Employment Survey (2007-2013) I employ a difference-in-differences (DD) design that compares fathers of children born after 2009, to fathers of children born before 2009, who are employed in the formal sector (treated), versus informal sector (not treated).

I estimate that fathers eligible for PPL increase time with their children by 24 minutes per week on average. This is an increase of 20 % from an average of 2 hours per week before the implementation of PPL. Fathers who, pre-treatment, spend the least amount of time childrearing exhibit the largest gain. I find that paternity leave increases active fathering because fathers establish routines during the initial parenting experience. These early routines are "sticky" and have lasting effects on paternal behavior—a key factor to achieve gender equality in the home. Interestingly, the leave does not lead to changes in employment, number of hours of paid work, or participation in other types of housework, which implies a substitution away from leisure and into time childrearing. This paper complements the nascent body of literature on the effects of parental leave reserved for fathers by analyzing PPL of *short duration* in a non-OECD country which complements the existing studies focused on high income countries which offer more generous parental leave provisions.

The broad goal of the first 2 chapters is to shed light on the importance of family economics for the design of government programs—a perspective that is critical to inform optimal policy design, and that may have important consequences for family wellbeing.

Lastly, in chapter IV, my co-authors and I developed a randomized control trial (RCT) to study the effects of financial inclusion in rural communities in Uganda. In this RCT, existing savings groups (informal, community-based finance institutions) gained facilitated access to formal group loans from a commercial bank in Uganda. We show that the bank loan stimulated an immediate and sizable increase in internal lending. As a result, members of treated groups had temporarily lower rates of food insecurity. Point estimates suggest sizable increases in income and microenterprise size. However, savings groups assigned to treatment experienced significantly more turnover, driven by groups with low levels of cohesion. This project has important implications for banking institutions in the developing world, mainly that working with, rather than replacing, informal financial institutions may provide a more stable venue for providing credit to new communities. For development agencies, it suggests both an opportunity and a cautionary tale. While lifting credit constraints by providing formal credit to existing saving groups can help them scale up, it may generate powerful selection effects with negative externalities on the community that need to be addressed as part of an intervention.

CHAPTER II

WORK HALF-TIME, RECEIVE FULL-TIME PAY: EFFECT OF A NOVEL FAMILY POLICY ON FEMALE LABOR MARKET OUTCOMES

Introduction

While the participation of women in the labor force has increased steadily over the past decades, gender inequalities in the labor market remain substantial (Blau & Kahn, 2017; Goldin, 2006). Significant gender differences in wages, hours of work, and occupational choices continue to exist in the vast majority of countries. Globally, women are paid approximately 20 percent less than men, and this gap is largest in low-income countries (International Labor Organization, 2019). This differential cannot be explained solely by gender differences in schooling, number of hours worked, experience, and job characteristics (Blau & Kahn, 2017; Kleven et al., 2018).

These gender differences start or are expanded when a child is born; the so called "child penalties" refer to the reduction of women's income observed immediately after childbirth that persists for years (Bertrand et al., 2010; Goldin, 2014a; Kleven & Landais, 2017; Kleven et al., 2018). Often, women work part-time while simultaneously being the main providers of child care within the family (Paull, 2008; Ciccia & Verloo, 2012). Recognizing that childbirth is a key moment in the life of workers, countries around the world have invested significant resources in maternity leave and other types of family policies to increase female labor force attachment, improve gender equality, and foster family wellbeing. However, women still experience larger child penalties than men. Even with maternity leave policies in place, women often do not return to their job after maternity leave, and studies show mixed results about how effective extending maternity leave is at improving female empowerment in the long-run (Bailey et al., 2019).

In this paper I study the introduction of a novel form of family policy implemented in Uruguay in 2014: A subsidy that allows parents to work half-time while receiving full-time pay, from the time maternity leave ends, and until the child is 6 month old. The argument for this new form of family policy is that a more gradual return to work after maternity leave might increase labor force attachment and job retention in the short-run for mothers with a young child (Schott, 2012). The hypothesized mechanism to yield these results is increased flexibility, which allows mothers to retain work related human capital that they would otherwise lose on full maternity leave. However, if this subsidy fosters traditional gender divisions of household work it could encourage women's part-time work and lower the probability of upward occupational moves increasing gender wage gaps (Blau & Kahn, 2013; Evertsson & Duvander, 2011). Hence the importance to study empirically its effects in the short and medium-run.

I examine the effect of the parental care subsidy on female labor market outcomes using household survey data in a difference-in-differences (DD) and triple difference (DDD) framework. My identification strategy relies on the facts that only workers employed in the formal-private sector are eligible, and that utilization consists almost entirely of women. Women employed in the public and informal sectors as well as men whose spouse does not work or is employed informally, are not eligible or affected by the policy, and can serve as control groups.

I find that mothers are more likely to be employed following the implementation of the parental care subsidy in the short and medium-run. Mothers are 6.1 percent more likely to be employed during the first year of their child from a pre-treatment mean of 54 percent. This effect is driven by an increase in the likelihood of being employed in the formal-private sector, which increased by 17 percent from a pre-treatment mean of 26 percent. Moreover, I find evidence that this effect is heterogeneous and largest for disadvantaged mothers (those who are less educated).

Next, I study the effect that parental care subsidy has on the intensive margin by looking at income, job experience, and number of jobs, conditional on employment. For this, I leverage the fact that public sector workers and informal workers do not have access to the new subsidy and can therefore serve as a control group within the sample of employed mothers of infants. I find a large increase in subsidies received from the government, consistent with program take-up, with no significant changes in total income for mothers of infants employed in the formal-private sector after the implementation of the policy.

In the medium-run, 1 to 3 years after the birth of a child, I find an increase in the likelihood of employment for mothers. Moreover, mothers of a young child born after the implementation of the policy and employed in the formal-private sector decreased their hours worked by 5% without a significant decrease in income.

Overall, this study provides novel evidence that extensions of flexibilities after birth in the form of fully paid half-time employment causes higher labor force attachment for female workers in the short and medium-run, especially for disadvantaged mothers (those with lower levels of education). The results are robust to changes in the specification and to an extended "event–study" analysis which allows for visualization of pre-treatment trends as well as the evolution of the average treatment effect in the post period.

My paper contributes to the literature on family policies in several ways. This is the first paper to evaluate a parental leave policy with the *work half-time, receive full-time pay* characteristics. Understanding the effects of this unique approach provides new insights and informs policy discussions and innovation. Second, I expand the literature by providing the first analysis of a family policy implemented in a non-OECD country. I leverage differences between formal and informal employment and show that the parental care subsidy is effective at increasing formal employment among mothers of infants. Third, I provide a causal examination of the short and medium-run effects of the parental care subsidy on female labor market outcomes. This complements studies that focus on take-up determinants and short-run effects of parental leave (Bartel et al., 2018; Romero-Balsas, 2012; Tanaka & Waldfogel, 2007).

The paper proceeds as follows: Section 2.2 provides background on existing family policies in Uruguay, and the policy reform. Section 2.3 describes the data. Section 2.4 details the empirical methods used. Section 2.5 presents short and medium-term results in the extensive and intensive margin. Section 2.6 shows robustness checks, and section 2.7 concludes.

Background

In this section I describe the institutional background of Uruguay, the novel parental care subsidy and the main characteristics of maternity and paternity leave provisions that also exist in the country. I briefly outline the history and current state of leave policies across countries and review the literature on the effects of paid family leave, child penalties, and half-time work for mothers of young children.

Parental care subsidy

The two most common types of family policies designed to balance work and child care are the provision of child-care services and the provision of leave for parents around the time of childbirth to care for an infant child. In this paper I focus on a novel family policy that has characteristics of both common provisions but is not easily classified as either child-care or leave.

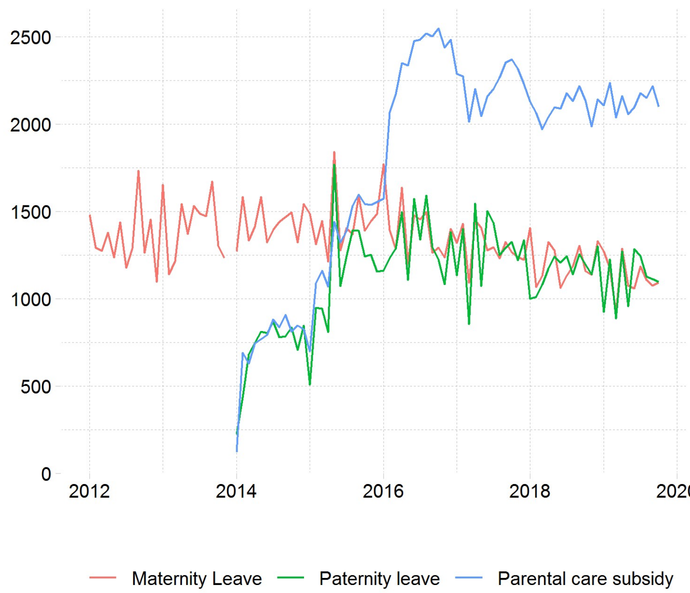
In November of 2013 Uruguay passed a law regulating a novel parental care subsidy for workers in the private sector. Under this new policy, mothers and fathers have access to full-time pay while working half-time from the time maternity leave ends until the child is 6 month old, which in most cases amounts to 4 months of benefits. This half-time period is job-protected and parents can decide whether the mother, father or both (alternating) will receive the subsidy.[1] Beneficiaries receive monthly transfers from the social security authority equivalent to their salary for the reduced hours.

The parental care subsidy was implemented in January 2014 and the introduction was gradual. At the onset, the subsidy covered from the end of maternity leave until the child was 4 months old. In January of 2015 the subsidy was extended to cover until the child was 5 months old. Since 2016 the subsidy covers until the child is 6 months old. Figure 1 depicts the evolution of take up for maternity, paternity and parental care subsidy provisions for workers in the private sector. Take-up was expanded as the coverage of parental care subsidy increased from 2014 to 2016. The average number of beneficiaries in 2014 was 738 and increased to 1327 in 2015 and for the period 2016-2019 it reached 2200.

[1] In the literature "job-protected" refers to strong regulations that protect parent against discrimination or retaliation for taking up paid leave. In practice it means that employers cannot fire or change working conditions during and immediately after the parental care subsidy.

Maternity and paternity leave had an average number of beneficiaries per month in 2019 of 1125 and 1113 respectively.

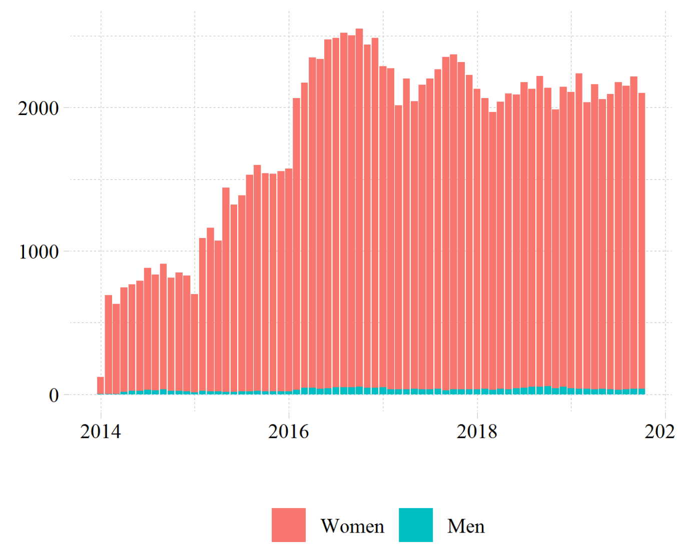
FIGURE 1.
Number of beneficiaries



Notes: Aggregated data from program utilization published by BPS. Number of beneficiaries per month by type of subsidy.

Even though both mothers and fathers can alternate access to the parental care subsidy, in practice, approximately 98 percent of beneficiaries at any given month are women. Figure 2 presents the evolution of the number of beneficiaries by gender. This extreme gender gap in utilization has also been documented in the literature regarding gender-neutral family leave policies in other countries. Additionally, only dual earner families are eligible. For the mother or father to apply for the subsidy, the mother needs to have taken maternity leave. If the father is employed in the private sector but his wife is not employed or employed informally, he is not eligible for the parental care subsidy.

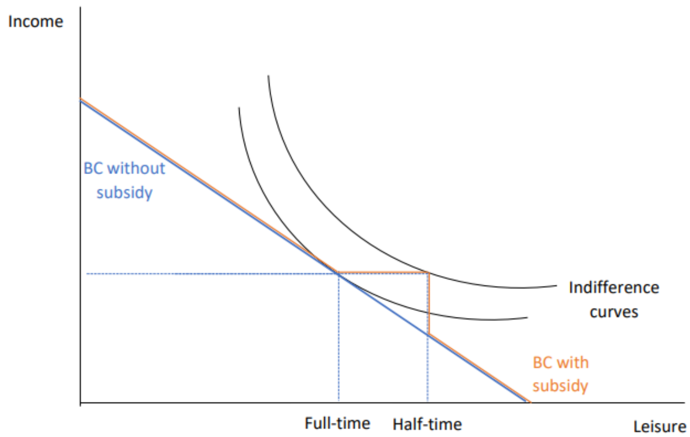
FIGURE 2.
Parental care subsidy by gender of beneficiary



Notes: Aggregated data from program utilization published by BPS. Number of beneficiaries of parental care subsidy per month by gender.

The parental care subsidy is fundamentally different than maternity leave because the benefits are conditional on returning to work after maternity leave ends and staying employed until the end of the benefits. The parental care subsidy changes the budget constraint that the mother faces at the time of returning from maternity leave. Figure 3 illustrates the new labor-leisure choice. With the subsidy, the budget constraint now has a kink. Mothers working half-time receive full-time pay which puts them at a higher indifference curve. If we only consider the income-leisure choice and assume indifference curves do not change with the birth of a child, we should see large take up of the subsidy and increased labor force attachment during the first months of life of the infant. However, the prediction is less clear in the long-run.

FIGURE 3.
Labor-leisure choice with new budget constraint



Maternity and paternity leave in Uruguay

In addition of the parental care subsidy family policies in Uruguay include maternity leave and paternity leave. The details of each program as well as when they where implemented varies according to the sector of employment. Public and private workers are subject to different regulations.

Maternity leave for workers in the private sector was implemented in 1980 and consisted of 12 job-protected, fully-paid weeks. In January 2014 workers' rights in the private sector were expanded in magnitude and coverage.[2] Maternity leave was extended by 2 weeks and included non-dependent workers covered by the social security authority with up to one employee, single-tax workers and workers covered by unemployment insurance. Maternity leave starts 6 weeks before the scheduled day of the birth and ends 8 weeks after. Public sector female workers

[2]Law 19,161 of 2013 replaced the benefits regulated by Law 15,084 of 1980.

have similar benefits, since 1990 they can access maternity leave for 13 weeks that start one week before the estimated birth date of the child. After maternity leave ends workers that are breastfeeding have the right to work half-time while receiving full-time pay until the child is 1 year old maximum.[3] Male public workers have the right to paternity leave for 10 working days from the date of the birth of the child. These policies provide 100 percent of wage replacement paid by the employer (in this case the government). [4]

Paternity leave was introduced gradually from January 2014, and since January 2016 it consists of 13 working days. It includes workers on private and public activity, non-dependents covered by social security with up to one employee and personal contractors who are up to date with the contributions and are not delinquent on alimony payments. In both cases, the social security system provides 100 percent wage replacement during the leave.[5]

Literature on part-time work and the effects of maternity leave

Recent work highlights the importance of parenthood for the persistence of gender inequality in labor market outcomes. Kleven, et al. (2019, May) show that, in the U.S., U.K., Scandinavian countries, Austria, and Germany, the earnings of

[3]The worker is required to provide a medical certification of breastfeeding to the employer during the duration of the subsidy.

[4]Law 19,121 of 2013 replaced the benefits regulated by Law 16,104 of 1990.

[5]Same-sex couples have access to same level of leave, the gestational parent receives maternity leave, while the non-gestational parent receives paternity leave. Public and private workers who adopt a child also have the right to paid leave for 6 weeks, followed by 6 months of half-time work while receiving full-time pay

men and women evolve similarly before parenthood—after adjusting for life cycle and time trends—but diverge sharply after parenthood. Women experience a large, immediate and persistent drop in earnings after the birth of their first child, while men are essentially unaffected. In general, the so called child penalty in earnings can come from three margins: The extensive margin of labor supply (employment), the intensive margin of labor supply (hours worked), and the wage rate. Goldin (2014) argues that the gender earnings gap persists because mothers of infants are more likely to select into flexible jobs and these work-life balance preferences interact with sector-specific long-hours premium.

Family policies, especially maternity leave, are important because they are seen as a potential instruments to decrease the earning gender gap by increasing female labor force attachment after childbirth. However, maternity leave also has the potential to increase the likelihood of mothers selecting into more flexible jobs with lower wage rates.

The International Labor Organization (ILO) guidelines propose the extension of maternity leave to 18 weeks with a benefit equivalent to 100 percent of earnings. Figures A.1 and A.2 in the Appendix plot current maternity and paternity leave regulations across countries. Almost all OECD countries achieve ILO's recommendation: the average number of weeks of paid maternity leave is 18. However, only 13 percent of countries in Latin America and the Caribbean meet ILO's guidance on maternity leave. Given these differences in the length and coverage of leave provisions in low and middle-income countries it is important to study policies that extend flexibilities for workers but are less costly than fully paid maternity leave for governments to implement. Hence the importance of studying the labor market effects of the novel subsidy in the short and middle-term for women.

An extensive literature examines the labor market effects of parental leave policies in high-income countries. Rossin-Slater (2017) provides a review of the literature on the impacts on women and children of the wide variety of family leave policies across Europe and North America. She concludes that parental leave shorter than one year can improve women's job continuity, while longer leave may negatively impact career advancement.

More recently, Stearns (2018) finds that different components of parental leave laws in Great Britain had opposing effects. Whereas wage replacement tended to increase short-term employment, laws granting job protection and increasing leave duration tended to impact career advancement negatively in the longer-term. One possible explanation for these findings is that while paid family leave policies may increase labor-force participation in the short-term, they could also increase statistical discrimination and occupational segregation in the longer term. Women in OECD countries who have more generous paid leave are more likely to work part-time and less likely to hold management positions (Blau and Kahn, 2013).

The evidence of the effects of parental leave on low and middle-income countries is limited. Albagli and Rau (2018) study an extension of maternity leave from 12 to 24 weeks in Chile (also an OECD country) utilizing a linear model. They find an increase in the probability of employment of 5.8 percentage points one year after birth, but do not find effects on wages.

I expand on this literature not only by studying a novel policy but also by considering employment by sectors. Increasing formal employment is a mayor policy goal in low and middle-income countries. Formal jobs are of better quality, provide higher wages and operate within the labor regulations of the country. Increasing access and continuity of formal employment for women is specially relevant to decrease gender wage gaps in resource-constraint households.

Data

The household and individual level data used in this study come from Uruguay's Continuous Household Survey (ECH), for the years 2009-2019. ECH is a nationally representative repeated cross-sectional survey, conducted monthly by the National Institute of Statistics (INE). Each individual in the household is questioned about their demographic characteristics, employment status, sector of employment, and non-labor income.

The data contain information that is crucial for the identification strategy. First, ECH allows me to identify the composition of the household and using the age of the children I can identify parents of infants (a child younger than one year old). Second, it allows me to identify formal versus informal workers by asking individuals whether they make contributions towards their pensions to the social security authority (BPS). I use this question, following the literature, to classify formal versus informal workers in the private sector.[6]

There are two primary limitations to using ECH data. First, ECH lacks of precise information on child birth dates, only reporting the age of the individuals in years. As a result, to identify short term effects as well as evidence of program utilization I focus on women with a child that is less than one year old. Second, the survey only asks the number of hours worked normally, and it does not refer to a specific reference week. Thus, even if a parent is working half-time during the length of the subsidy they might still report the otherwise full-time job hours. Because of this, I cannot distinguish the number of hours worked during the subsidy from the number of hours that the person would have worked otherwise.

I examine labor market outcomes for women with infants by comparing their experiences to those of men that are at a similar stage of life and career but that are not directly or indirectly treated by the new subsidy. Given the extremely low take-up of this program among men (on average lower than 2%) it is unlikely they will be directly affected by the program. However, if their spouses utilize the subsidy many intra-household dynamics might change which in turn will

[6]The formal sector is defined as the group of employed individuals who work for companies that are registered with the tax authority and pay taxes towards social security. The informal sector is formed by employed individuals working for unregistered employers or that do not pay taxes to the social security authority. Public sector workers are in large majority formal workers.

also affect men. To avoid these indirect effects in the control group, I select men whose spouses are not eligible for the parental care subsidy. This consists of men whose spouses are not employed or are employed informally and thus have not experienced any change in benefits.

The sample that I use to conduct the analysis consists of mothers 18 to 45 years old with an infant child at the time of the survey (treatment group); and men 18 to 45 years old, father of an infant at the time of the survey whose spouse or partner is not employed or is employed informally (control group). Table 1 shows the mean and standard deviation of employment, income and hours worked and individual characteristics for treatment and control groups, before and after the implementation of the parental care subsidy in January 2014.

There is a large employment gap between mothers and untreated fathers of infants. Only 54 percent of mothers of infants are employed before the implementation of the policy, whereas 92 percent of fathers of infants are employed during this period. Interestingly, conditional on employment, total labor income is less unequal between the two groups. Before January of 2014 mothers and fathers of infants earned on average UY$ 14200 and UY$ 15300 in total labor income respectively. However, during the post-treatment period the total labor income increased more for mothers than for fathers of infants in the sample.

TABLE 1.
Summary statistics

	Fathers				Mothers			
	Pre-January 2014		Post-January 2014		Pre-January 2014		Post-January 2014	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Panel A: Employment								
Employed	0.92	0.27	0.89	0.32	0.54	0.50	0.55	0.50
Formal	0.62	0.48	0.59	0.49	0.40	0.49	0.45	0.50
Formal-Private	0.51	0.50	0.49	0.50	0.29	0.45	0.33	0.47
Public	0.09	0.29	0.08	0.27	0.09	0.29	0.11	0.31
Informal	0.30	0.46	0.30	0.46	0.14	0.34	0.10	0.31
Inactive-Housework	0.00	0.06	0.00	0.07	0.31	0.46	0.31	0.46
Panel B: Income and Hours Worked								
Income from subsidies	60	814	92	1230	1277	6955	3761	18564
Income from employer	15056	15343	25393	24075	12026	16177	21647	21024
Total labor income	15368	15327	25937	24194	14235	17015	27141	26848
Hours worked	45.46	14.32	42.61	13.65	33.40	15.01	32.41	13.92
Job experience (years)	4.20	5.46	4.18	5.18	4.28	4.56	4.71	4.34
Number of jobs	1.10	0.34	.07	0.29	1.12	0.39	1.13	0.42
Panel C: Individual Characteristics								
Number of children	2.50	1.53	2.32	1.35	2.32	1.45	2.13	1.24
Age	29.77	7.36	29.67	7.49	29.18	7.07	29.61	6.98
Married	0.84	0.37	0.85	0.36	0.75	0.43	0.78	0.41
Ed: Primary	0.47	0.50	0.44	0.50	0.34	0.47	0.30	0.46
Ed: Middle	0.28	0.45	0.28	0.45	0.29	0.45	0.27	0.44
Ed: Secondary	0.19	0.39	0.22	0.42	0.34	0.47	0.41	0.49
Capital city	0.35	0.48	0.31	0.46	0.40	0.49	0.38	0.48
Sample size	3876		3477		8008		7797	

Notes: This table shows summary statistics for mothers of infants (treatment group), and untreated fathers of infants (control group) before and after January 2014, when the parental care subsidy was implemented. Labor market outcomes are dummy variables. *Formal* is not equal to *Formal-Private* plus *Public* because neither category includes entrepreneurs with more than one worker who are registered with the social security authority but are not eligible to receive the parental care subsidy.

Mothers and untreated fathers have similar number of children and ages. However, mothers of infants are less likely to be married or living with her partner, have higher levels of education, and are more likely to live in the capital city than untreated fathers of infants. I control for these differences in individual characteristics in my identification.

Empirical strategy

Employment effects in the short-run

I study the effect of an extension of family policies on female labor force attachment by leveraging the quasi-experiment provided by the implementation of a parental care subsidy in Uruguay in January of 2014. I employ a difference-in-difference (DD) design that compares mothers of infants, to untreated father of infants (as defined in section 3), before and after the implementation of the parental care subsidy.

To understand the effect of the policy on the employment decision of mothers of infants, I estimate the following generalized DD equation:

$$Y_{it} = \beta_1 Woman_i * After_t + \gamma' X_{it} + \delta_i + \theta_t + \epsilon_{it} \tag{2.1}$$

where i is an individual and t represents a month-year. Y_{it} is the outcome of interest. This binary variable takes the value of one if individual i is employed in the formal-private sector at time t. $Woman_i$ is a binary variable that takes the value of one if individual i is a mother, and zero for fathers. $After_t$ is a binary variable that take the value of one for periods after December of 2013, and zero otherwise. The coefficient of interest is β_1 and it captures the effect of the parental care subsidy on employment among mothers of infants. The vector X_{it} includes a set of individual and household controls such as individual's age, education, marital status, ethnicity, number of children in the household, and region. Gender fixed effects (mother or father) are captured by δ_i and control for any characteristics that are common to each gender and do not change over time. Time fixed effects are captured by θ_t and control for shocks that might affect labor market outcomes and are common to all individuals, such as an economic recessions in the country. Standard errors are clustered by month-year to allow for correlation within time periods and provide standard errors that are robust to heteroskedasticity and autocorrelation.

To interpret the estimated β_1 coefficient as the causal effect of the subsidy on employment, it needs to be true that in the absence of the parental care subsidy the control group (untreated fathers of infants) would have had similar trends (but not necessarily level) to the treated group (mothers of infants), and that no other factors affecting labor market outcomes occurred at the same time as the implementation of this new policy. To explore the validity of the design, I extend the DD analysis to an "event-study" analysis. In practice, this means estimating equation (2.1) with year effects interacted with the treatment indicator.

I then plot the treatment effect by year, which allows for visualization of pre-treatment trends and the evolution of the average treatment effect in the post period. Results are included in section 6.

Labor hours and income effects in the short-run

Next, I study the effect that the parental care subsidy has on the intensive margin by looking at hours worked, labor income, total income, job experience, and number of jobs, conditional on employment. For this analysis I restrict the sample to all employed fathers and mothers of infants and employ a triple difference (DDD) design. The first difference compares these outcomes across time (before and after the implementation of the policy), the second difference compares mothers to fathers of infants, and the third difference refers to the sector of employment. In this third difference, I take advantage of the fact that public sector workers as well as informal workers do not have access to this subsidy and thus can serve as a control group within the sample of employed mothers of an infant. This strategy controls for any possible differential trends across sectors of employment that affect all of its workers.

This DDD approach allows me to study the effect of the policy on the intensive margin of work by comparing mothers of infants to fathers of infants, employed in the private sector (treated) versus public and informal sector (control), before and after the parental care subsidy was implemented. Specifically, I estimate the following DDD equation:

$$
\begin{aligned}
Y_{ist} = &\beta_1 After_t * Woman_i * Private_{is} + \beta_2 After_t * Private_{is} + \\
&\beta_3 Woman_i * After_t + \beta_4 Woman_i * Private_{is} + \\
&\gamma' X_{it} + \delta_i + \theta_t + \eta_s + \epsilon_{it}
\end{aligned}
\tag{2.2}
$$

where $Private_{is}$ is a binary variable that takes the value of one if individual i works formally in the private sector and zero otherwise, and η_s is a sector (private, public, and informal) fixed effect. The rest of the variables and parameters are as defined in equation (2.1). The main parameter of interest is β_1 (the triple-difference estimate), and β_2, β_3, and β_4 are the estimates of the double interaction terms. Standard errors are clustered at the month-year level.

Medium-run effects on labor market outcomes

To understand whether the parental care subsidy's effects are persistent, I focus on the labor market outcomes of mothers whose youngest child is aged 1 to 3 years old. These women are not currently exposed to the subsidy, but depending on when their children were born, could have been exposed to the subsidy in the past. At this point one data restriction deserves mention. The ECH does not allow me to directly identify women affected by the program, because I do not observe individuals history of employment and thus I do not know whether the mothers in the sample were employed around the time of childbirth, therefore, I estimate ITT effects.

To study the medium term effects of the policy on employment, I use a DD design that compares mothers of young children (1 to 3 years old) to untreated fathers of young children, that were born after 2013 (exposed to the subsidy), versus before 2013.

$$Y_{it} = \beta_1 Woman_i * Post_t + \gamma' X_{it} + \delta_i + \theta_t + \epsilon_{it} \tag{2.3}$$

where $Post_t$ is a binary variable that takes the value of one if individual i has a child born after 2013, and zero otherwise. In addition to all the controls included in equation (2.1), X_{it} includes fixed effects for the year of birth and age of the youngest child of individual i. These additional fixed effects allow me to compare across individuals whose youngest child has the same age and was born in the same year. The rest of the variables and parameters are as specified in equation (2.1). The coefficient of interest is β_1, it captures the medium-run effect of the parental care subsidy on employment among mothers of infants.

In a similar fashion, I estimate the medium-run effect of the parental care subsidy on hours worked and income using a DDD approach. I compare mothers of young children to fathers of young children, employed in the private sector (treated) versus public and informal sector (control), whose youngest child was born before and after the parental care subsidy was implemented. Specifically, I estimate the following DDD equation:

$$
\begin{aligned}
Y_{ist} = & \beta_1 Post_t * Woman_i * Private_{is} + \beta_2 Private_{is} * Post_t + \\
& \beta_3 Woman_i * Post_t + \beta_4 Woman_i * Private_{is} + \\
& \gamma' X_{it} + \delta_i + \delta_t + \eta_s + \epsilon_{ist}
\end{aligned}
\tag{2.4}
$$

All variables and parameters have the same meaning as above. The main parameter of interest is β_1 (the triple-difference estimate) and it indicates the medium-run causal effects of the parental care benefit on the intensive margin of work (hours worked and income).

Since both, the DD and DDD methods (equations (2.1) and (2.3); and equations (2.2) and (2.4)) leverage variation in the exposure to the subsidy rather than actual take-up, I estimate *intent-to-treat* (ITT) which is preferable to estimates of *treatment on the treated* (TOT) for several reasons.[7] First, TOT estimates could be subject to the same bias from selection into treatment that cross-sectional studies have been criticized for. Second, from a policy-making perspective ITT effects may be more relevant as they allow for feedback effects, whereby the new subsidy could have changed expectations and norms beyond the

[7] ITT refers to the average effect of the policy on everyone exposed to it regardless of whether they where treated, whereas TOT refers to the average effect of the policy for those that where actually treated by it. In this case TOT would represent the effect of parental care subsidy for those mothers that reduced hours worked to four or less per week by receiving the parental care subsidy.

effects of actually using the half-time option. The implementation of this new form of family policies sent a strong public message about the importance of parental involvement in child-rearing which may have incentivized mothers (and potentially also fathers) who were exposed to the policy but not treated to nevertheless be more involved in the home. However, it is safe to assume that feedback effects on parents who were exposed but not treated are smaller than the first-order-effects on parents who reduced hours of work, such that the ITT results presented here underestimate the true causal effect of parental care subsidy on those who take it.

Results

Short-run effects on maternal labor market outcomes

Table 2 presents regression results from equation (2.1) where the dependent variable is an indicator of employment by sector. Overall, I find a statistically significant increase in the likelihood of a mother of an infant being employed following the implementation of the parental care subsidy. The ITT effect is 3.3 percentage points. The magnitude of the estimated effect is considerable. Pre-program employment for the treatment group averages 54 percent. Relative to this baseline, the estimate suggests an increase in employment of 6.1 percent.

TABLE 2.
Short-term effects of parental care subsidy on employment

	Dependent variable:					
	Employed	Formal	Formal-Private	Public	Informal	"Stay-at-home"
	(1)	(2)	(3)	(4)	(5)	(6)
Woman*After	0.033***	0.062***	0.044***	0.022***	−0.004	0.006
	(0.010)	(0.013)	(0.013)	(0.008)	(0.008)	(0.008)
Mean	0.54	0.4	0.26	0.09	0.07	0.31
Time FE	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓
Observations	23,158	23,158	23,158	23,158	23,158	23,158
R^2	0.254	0.227	0.095	0.086	0.051	0.199
Adjusted R^2	0.250	0.222	0.089	0.080	0.044	0.193
Residual Std. Error	0.411	0.441	0.452	0.280	0.265	0.368

Notes: This table contains regression estimates where the treated group consists of all mothers of infants and the control group are untreated fathers of infants (whose wife is unemployed, or employed informally). Each column reports a different regression where the outcome variable is a dummy variable that takes the value of one if the individual is employed (1), employed in a specific sector (2-5), or a "stay-at-home" parent. Individual controls include: Age, number of children, education dummies, marital status dummies, region dummies, and ethnicity dummies. Standard errors are clustered at the year-month level. ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1, 5, and 10 percent levels.

As described in section 2.2, I can distinguish 3 distinct sectors in the labor market of low and middle income countries: Formal-public, formal-private, and informal sectors. The increase in employment among mothers of infants is driven by an increase in employment in the formal sector as shown by column 2. I then further desegregate this effect by looking at private and public employment. The likelihood of being employed in the private sector increased by 17 percent from a pre-treatment mean of 26 percent. This is to be expected because the private-formal sector is the one that was treated. Column 4 also indicates a strong increase in the likelihood of being employed in the public sector of 24 percent from a pre-treatment mean of 9 percent. However, I show in the robustness section that employment in the public sector does not satisfy the underlying identifying assumption of parallel trends, making this result likely bias. Columns 5 and 6 show there are no significant changes in the likelihood of being employed informally or being a "stay-at-home" mom as a result of the policy.

Table 3 presents regression results from equation (2.2) where the sample is restricted to employed mothers and fathers of infants. *After*Woman*Private* captures the effect of parental care subsidy on the outcome variables among mothers of infants employed in the private sector. *Subsidies* are the self-reported amount in transfers received from the social security authority related to medical and maternity subsidies (including the parental care subsidy). If there is take up we should see an increase in subsidies and a decrease in income received from the employer. Indeed, women employed in the private sector experience a large and statistically significant increase in transfers. The amount of these specific transfers increased by UY$3,819 which represents a 166 percent increase from a pre-treatment mean of UY$2,220. The increase in subsidies is equivalent to 29.5 percent of the average pre-treatment salaries. The increase in subsidies is accompanied by a slightly larger decrease in salaries received from employer of UY$5,300 a 40 percent decrease from a pre-treatment mean of UY$12,927. The increase in subsidies balanced with a decrease in income received from the employer produced an statistically insignificant decrease in total income of UY$1,890, a 12 percent increase from a pre-treatment mean of UY$15,503.[8]

Hours worked refers to the self-reported number of hours worked in a standard week, it does not refer to the number of hours in a reference week. Thus, even if women are working half-time during the length of the subsidy they might still report the otherwise full-time job hours. Because of this I cannot distinguish the number of hours worked during the subsidy from the number of hours that the person would have worked otherwise. Keeping this caveat in mind, the parental care subsidy had an small but statistically significant negative effect on hours worked of 2 hours and 21 minutes per week from a pre-treatment mean of 37 hours and 40 minutes.

[8] UY$ denotes Uruguayan pesos, as of December 31, 2019 UY$1 = US$0.0268, or US$100 are equivalent to UY$3,660.13

TABLE 3.
Short-term effects of parental care subsidy, DDD estimates

	Dependent variable:					
	Subsidies	Labor income	Total income	Hrs. worked	Experience	N. of jobs
	(1)	(2)	(3)	(4)	(5)	(6)
After*Private	168.430**	5,195.807***	5,235.082***	2.030***	0.456**	0.027**
	(71.580)	(903.959)	(920.647)	(0.687)	(0.225)	(0.014)
After*Woman	39.858	720.900	1,545.727**	2.404***	0.040	0.008
	(159.679)	(785.143)	(784.051)	(0.701)	(0.243)	(0.015)
Woman*Private	1,967.170***	−2,082.215***	−510.868	3.998***	1.431***	−0.028*
	(213.325)	(726.986)	(736.521)	(0.642)	(0.194)	(0.014)
After*Woman	3,819.157***	−5,300.101***	−1,715.908	−2.346***	0.088	0.015
*Private	(701.963)	(1,126.570)	(1,269.810)	(0.847)	(0.267)	(0.020)
Mean	2220.84	12927.48	15950.34	37.7	4.14	1.1
Time FE	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓
Observations	15,254	15,254	15,254	15,254	15,254	15,254
R^2	0.062	0.299	0.268	0.241	0.266	0.052
Adjusted R^2	0.052	0.292	0.261	0.234	0.259	0.043
Residual Std. Error	10,386.520	16,775.590	19,111.710	13.443	4.186	0.359

Notes: This table contains regression estimates where the treated group consists of mothers of infants employed in the formal-private sector and the control group are mothers of infants employed informally or in the public sector and employed fathers of infants. Each column reports a different regression. *Hrs. worked* refers to number of hours a workers works normally, it does not refer to the number of hours in a reference week. Thus even if women are working part-time during the length of the subsidy they might still report the otherwise full-time job. *Subsidies*, *Labor income-employer* and *Total income* refer to the total amount received last month from the social security system, employer and in total, respectively. Individual controls include: Age, number of children, education dummies, marital status dummies, region dummies, and ethnicity dummies. Standard errors are clustered at the year-month level. ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1, 5, and 10 percent levels.

This could be product of some mothers reporting half-time work during the subsidy, or it could be a result of some mothers reducing their normal hours after the utilization of the parental care subsidy. Given aggregate data of program take up, this effect is likely bias downwards by mothers reporting their contractual work hours and not their actual hours of work during the utilization of the subsidy. Finally, the number of jobs mothers of infants hold does not change with the policy.

Heterogeneity

Prior research suggests that disadvantaged mothers may be more responsive to paid family leave than their more advantaged peers (Baker & Milligan 2008b; Rossin-Slater, Ruhm, & Waldfogel 2013; Carneiro, Loken, & Salvanes 2015; Stearns 2015; Lichtman-Sadot & Pillay Bell 2017). I study whether this is also the case for Uruguay's parental care subsidy by examining whether this policy produces heterogeneous treatment effects according to several markers of disadvantage, including: education, age, and whether the husband or partner lives in the same home. Additionally, there is evidence that suggests that these effects might differ according to birth order, thus I distinguish between mothers that have one child and those that have more children at the time of the survey.

Table 4 shows point estimates, standard errors and pre-treatment means for the *Woman*After* coefficient from equation (2.1) estimated using subsamples according to individual's characteristics mentioned. Column (1) presents the effects on overall employment, columns (2) to (4) present the effect by sectors and column (5) shows the effect on “stay-at-home” mom. The effects of the parental care subsidy on employment are the largest for women that have low-education. The likelihood of employment of a mother of an infant that has not finished middle school increased by 10 percent. This large effect is composed by an increase in employment in both formal-private and public sectors. In particular, parental care subsidy is associated with a 4.9 percentage point (30 percent), and 1.9 percentage point (190 percent) increase in employment in formal-private and public sector respectively. On the other hand, women with higher levels of education show insignificant increases in employment and an increase in the likelihood of being a “stay-at-home” mom of 1.7 percentage points (7.7 percent). Parental care subsidy increases the likelihood of employment for older, married women, with more than one child the most, as a result of large increases in formal employment and some decreases (not always significant) in informal employment and “stay-at-home” mom. Overall the heterogeneity analysis supports evidence from the literature that paid flexibilities, in this case reduction of hours worked without wage loss, benefits disadvantaged mothers the most.

Medium-run effects on maternal labor market outcomes

To understand whether the parental care subsidy’s effects are persistent, I next examine the medium-run effects of the parental care subsidy on labor market outcomes of mothers whose youngest child are 1, 2, or 3 years old.

TABLE 4.
Heterogeneous effects

	Employed	Formal-Private	Public	Informal	"Stay-at-home"
Low	0.036**	0.049**	0.019**	0.003	0.009
Education	(0.017)	(0.019)	(0.008)	(0.013)	(0.016)
	[0.35]	[0.16]	[0.01]	[0.09]	[0.46]
High	0.015	0.026	0.023*	−0.015	0.017**
Education	(0.013)	(0.020)	(0.013)	(0.011)	(0.008)
	[0.65]	[0.33]	[0.14]	[0.05]	[0.22]
1 child	0.021	0.058**	0.021	−0.007	0.015
	(0.019)	(0.026)	(0.015)	(0.015)	(0.013)
	[0.6]	[0.32]	[0.12]	[0.05]	[0.25]
2 or more	0.040***	0.036**	0.026***	−0.003	0.001
children	(0.013)	(0.016)	(0.009)	(0.010)	(0.011)
	[0.5]	[0.23]	[0.08]	[0.07]	[0.34]
Age <30	0.027*	0.052***	0.010	0.0005	0.029**
	(0.015)	(0.018)	(0.010)	(0.014)	(0.012)
	[0.42]	[0.22]	[0.05]	[0.07]	[0.36]
Age>30	0.045***	0.040**	0.039***	−0.008	−0.024**
	(0.012)	(0.019)	(0.013)	(0.010)	(0.010)
	[0.66]	[0.32]	[0.13]	[0.06]	[0.26]
Married	0.034***	0.053***	0.024***	−0.006	−0.005
	(0.010)	(0.016)	(0.009)	(0.008)	(0.008)
	[0.54]	[0.27]	[0.11]	[0.05]	[0.34]
Single	0.073**	0.032	0.014	0.009	0.030
	(0.029)	(0.030)	(0.013)	(0.026)	(0.018)
	[0.52]	[0.25]	[0.05]	[0.12]	[0.22]

Notes: Standard errors in parentheses. Pre-treatment means in brackets. The sample in each row consists of individuals that share that characteristic. All coefficients come from different regressions, where the dependent variable is denoted in the column's name and the subsample is indicated in the panel's name. Regressions control for all other individual characteristics and fixed effects in equation (2.1). Standard errors are clustered at the year-month level. ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1, 5, and 10 percent levels.

Slightly more than 60 percent of mothers with a child that is 3 years old or younger worked in the reference week. Table 5 summarizes the effect of the parental care subsidy on medium-term employment outcomes. There is a significant increase in employment on the extensive margin, between the pre- and post-treatment periods, compared to untreated men. Overall employment increases by 2.7 percent. This effect is driven by an increase in the likelihood of being employed in the public sector, which increased by almost 18 percent from a pre-treatment mean of 9 percent. It is worth mentioning that public sector job arrangements are usually more stable, flexible and provide more benefits for the worker and her family. Additionally, the results suggest a decrease in informal employment, although not significant, and a significant decrease in the likelihood of being a “stay-at-home” mom. These results point at the potential benefit of this policy to foster paid work as well as increase the quality of jobs by increasing formal employment among mothers of young children.

Next, I explore the effects of the parental care subsidy in the intensive margin by looking at income, hours worked, job experience, and number of jobs held by mothers of young children compared to untreated fathers of young children, born before and after the implementation of the policy, conditional on employment. Table 6 presents the DDD estimates from equation (2.4).

TABLE 5.
Medium-term effects of parental care subsidy on employment

	Dependent variable:					
	Employed	Formal	Formal-Private	Public	Informal	"Stay-at-home"
	(1)	(2)	(3)	(4)	(5)	(6)
Woman*Post	0.017**	0.026***	0.012	0.016***	−0.009	−0.011**
	(0.007)	(0.009)	(0.010)	(0.006)	(0.007)	(0.005)
Mean	0.62	0.44	0.33	0.09	0.18	0.24
Time FE	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓
Observations	62,316	62,316	62,316	62,316	62,316	62,316
R^2	0.185	0.200	0.095	0.078	0.065	0.154
Adjusted R^2	0.183	0.198	0.092	0.075	0.062	0.151
Residual Std. Error	0.409	0.448	0.464	0.283	0.392	0.342

Notes: This table contains regression estimates where the treated group consists of all mothers whose youngest child is 1, 2 or 3 years old, and the control group are untreated fathers of young children (whose wife is unemployed, or employed informally). None of these regressions are conditional on employment. Post is a dummy variable that takes the value of one if a child in the family was born during or after 2014. Regressions control for all individual characteristics and fixed effects in equation (2.1) and fixed effects for the age of youngest child in the family and their year of birth. Standard errors are clustered at the year-month level. $***$, $**$, and $*$ denote statistical significance at the 1, 5, and 10 percent levels.

TABLE 6.
Medium-term effects of parental care subsidy, DDD estimates

	Dependent variable:					
	Subsidies	Labor income-employer	Total income	Hrs. worked	Job experience	Number of jobs
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Private	71.231*	4,107.106***	4,203.214***	1.800***	0.745***	−0.001
	(39.356)	(1,065.117)	(1,074.996)	(0.510)	(0.175)	(0.010)
Post*Woman	8.367	−214.544	876.898	2.954***	0.423***	−0.019*
	(15.007)	(754.589)	(762.124)	(0.548)	(0.142)	(0.011)
Woman*Private	18.759	−673.979	−993.611**	4.270***	1.462***	−0.022***
	(18.851)	(440.155)	(433.974)	(0.368)	(0.120)	(0.008)
Post*Woman	−29.984	−526.412	−1,045.497	−2.139***	−0.474**	0.022
*Private	(44.126)	(1,096.909)	(1,106.518)	(0.648)	(0.188)	(0.015)
Mean	109.92	16854.42	18067.87	38.1	4.3	1.12
Time FE	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓
Observations	44,468	44,468	44,468	44,468	44,468	44,468
R^2	0.008	0.289	0.284	0.217	0.273	0.043
Adjusted R^2	0.004	0.286	0.281	0.214	0.270	0.039
Residual Std. Error	981.730	18,545.560	18,570.240	13.933	4.430	0.356

Notes: This table contains regression estimates where the treated group consists of mothers of young children (1 to 3 years old) employed in the formal-private sector and the control group are mothers of young children employed informally or in the public sector and employed fathers of young children. Each column reports a different regression. *Hrs. worked* refers to number of hours a workers works normally, it does not refer to the number of hours in a reference week. Thus even if women are working part-time during the length of the subsidy they might still report the otherwise full-time job. *Subsidies*, *Labor income-employer* and *Total income* refer to the total amount received last month from the social security system, employer and in total, respectively. Post is a dummy variable that takes the value of one if a child in the family was born during or after 2014. Regressions control for all individual characteristics and fixed effects in equation (2.3) and fixed effects for the age of youngest child in the family and their year of birth. Standard errors are clustered at the year-month level. ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1, 5, and 10 percent levels.

Given that parents are eligible for the parental care subsidy only until the child is 6 months old, there should not be any significant differences between subsidies received by mothers and fathers, before and after the implementation of the policy. Column (1) shows that this is case, which also supports the validity of this variable as a proxy for policy take-up in the short-run. Moreover, there is no significant change in total income for mothers of young children employed in the private sector.

The DDD estimates indicate a significant decrease in hours worked, of slightly more than 2 hours per week from for a pre-treatment mean of 38 hours, and a reduction of job experience of almost half a year from a pre-treatment mean of 4.3 years. A possible explanation for these findings is that the parental care subsidy increases preferences for flexible job arrangements which induced some mothers to switch from full-time work to part-time work. However, additional research is needed to identify the exact pathways for the observed effects. Lastly, there are no significant effects of the policy on the numbers of jobs held by mothers of young children in the medium-run.

Robustness

A key assumption in a DD analysis is that in the absence of the parental care subsidy the control group (untreated men) would have similar trends (but not necessarily level) to the treated group (women), and that no other factors affecting labor market outcomes occurred at the same time as the implementation of this new policy. To explore the validity of the design, I extend the DD analysis to an "event-study" analysis. In practice, this means estimating equation (2.1) with year effects interacted with the treatment indicator. I then plot the treatment effect by year, which allows for visualization of pre-treatment trends and the evolution of the average treatment effect in the post period. Figure 4 to 9 present these results.

Figure 4 shows parallel pre-treatment trends on employment as well as a break in the trend in 2015 with a positive and (slightly) significant DD coefficients after that. Analyzing each sector individually reveals different behaviors across sectors. Figure 5 indicates parallel pre-treatment trends and increasing coefficients in the post-treatment period, probably as a result of increased take-up as shown in Figure 1.

FIGURE 4.
Effect of Parental Care Subsidy on Employment

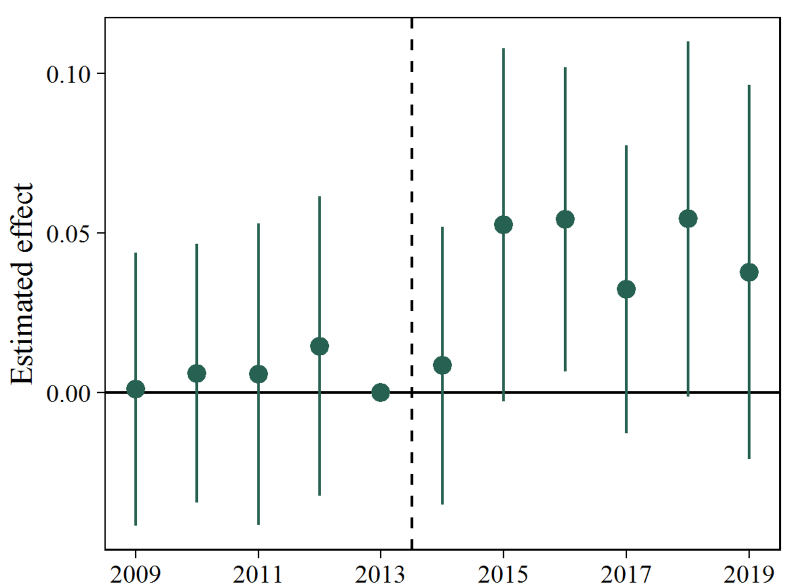


FIGURE 5.
Effect of Parental Care Subsidy on Formal Employment

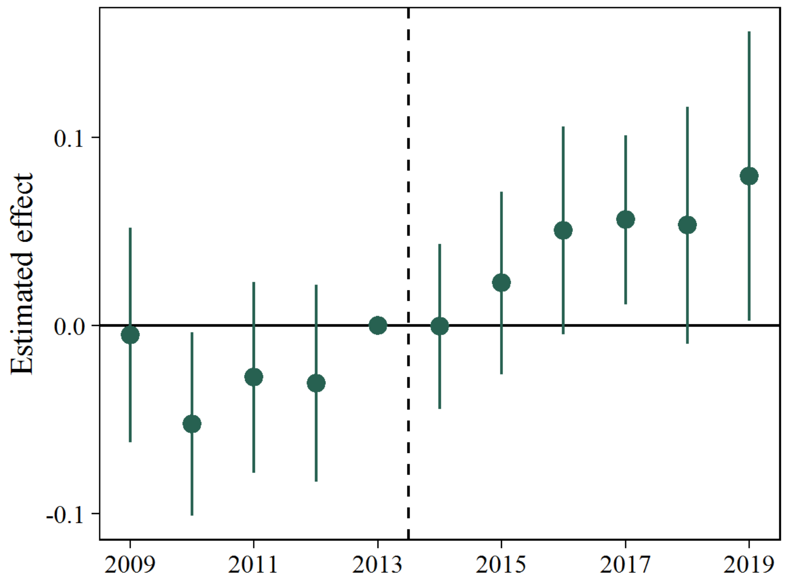


FIGURE 6.
Effect of Parental Care Subsidy on Formal-Private Employment

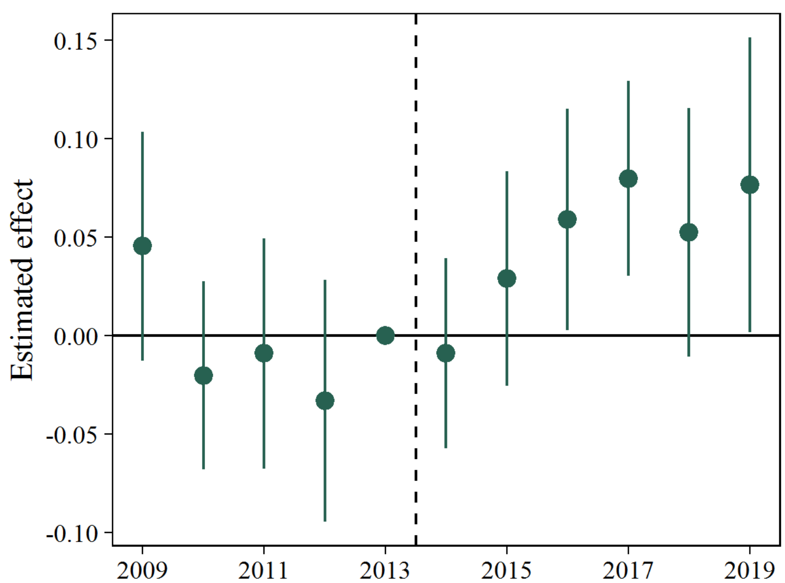


FIGURE 7.
Effect of Parental Care Subsidy on Formal-Public Employment

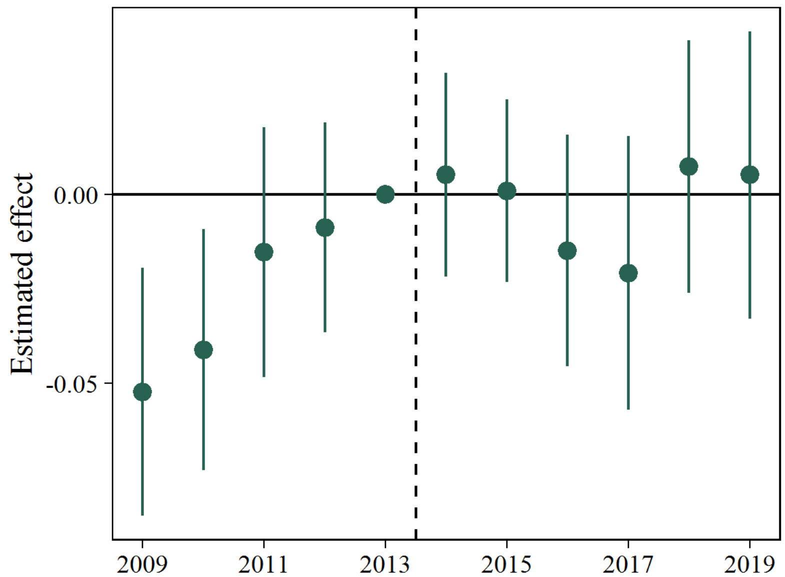


FIGURE 8.
Effect of Parental Care Subsidy on Informal Employment

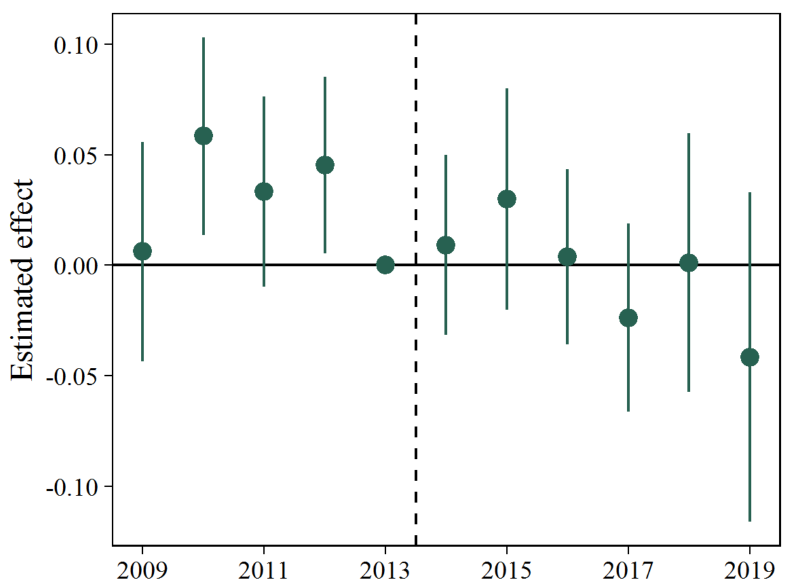


FIGURE 9.
Effect of Parental Care Subsidy on “Stay-at-home”

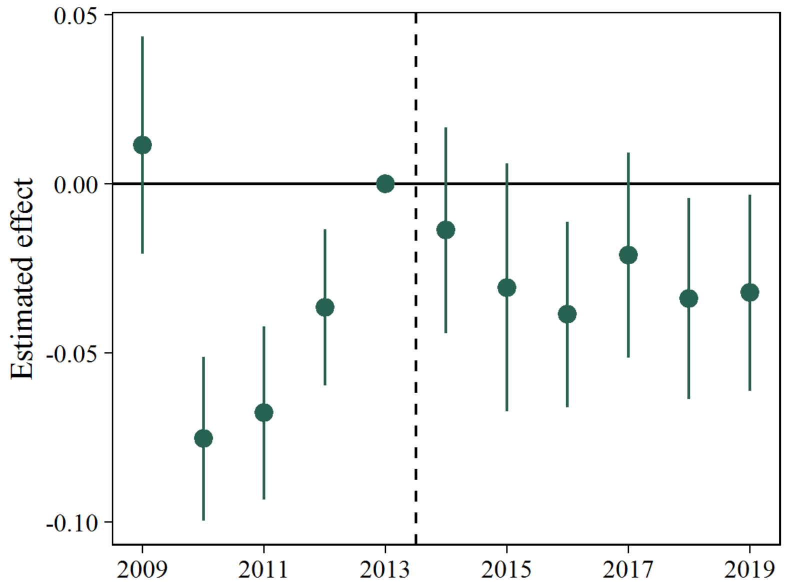


Figure 7, revels a different scenario for employment in the public sector. There seems to be an positive pre-treatment trend where women with infants were already increasing their likelihood of employment, and this trend seems to stop after 2014. The result presented in table 2 is likely bias given the absence of parallel trends. Lastly, Figure 8 shows overall no changes in informal employment from 2009 to 2019.

Additionally, the identification assumption of the DD design would be violated if the parental care subsidy induced selection into the sample through impacts on fertility patterns. Ideally I would like to study whether women employed in the private sector are more or less likely to have an additional child as a result of the policy. Unfortunately the survey does not include information on the history of employment of mothers, thus I do not observe the sector of employment at the time of previous births. However, I do observe the number of children at the time of the survey and their age profile which allows me to address this challenge.

To evaluate the plausibility of selection into or out of the sample of mothers of infants as a result of changes in fertility I estimate regression (2) using number of children as the outcome variable. If women employed in the formal-private sector have more children as a result of the new subsidy, we should see a positive and significant effect on the number of children. In other words, the new born would be of higher birth order compared to those of mothers employed in the public or informal sector. Figure 10 presents the estimated coefficient disaggregated by year. Mothers of infants employed in the formal-private sector do not have more children after the implementation of the PCS compared to mothers of infants employed in public or informal sectors. Overall the effect is small, negative and insignificant.

I then repeat the exercise using the medium-term sample of employed women whose youngest child is 1, 2 or 3 years old. Figure 11 presents the estimated coefficient disaggregated by year of birth of youngest child. Even though the aggregated result is insignificant the event study plot shows that the introduction of the policy is associated with a small increase in the number of children of employed women in the formal sector compared with employed women in the public or informal sector. In line with short term and medium-term results on

FIGURE 10.
Association between PCS and birth order of infant

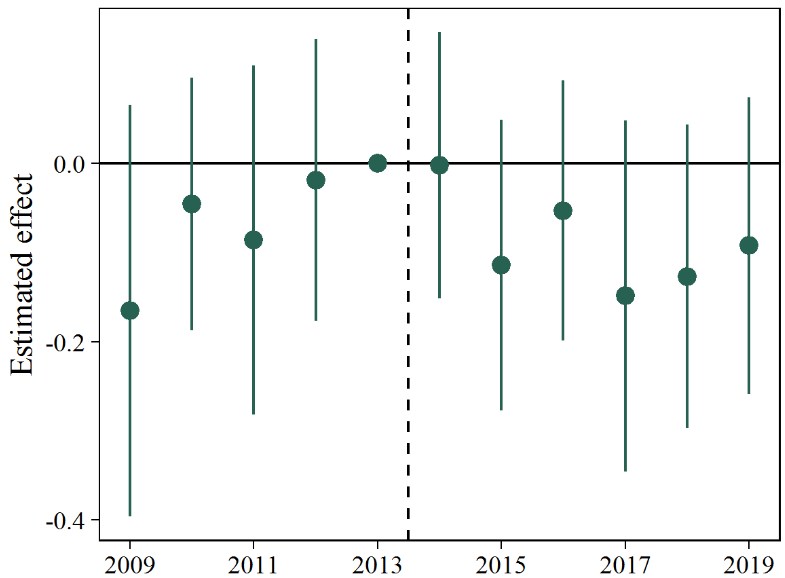
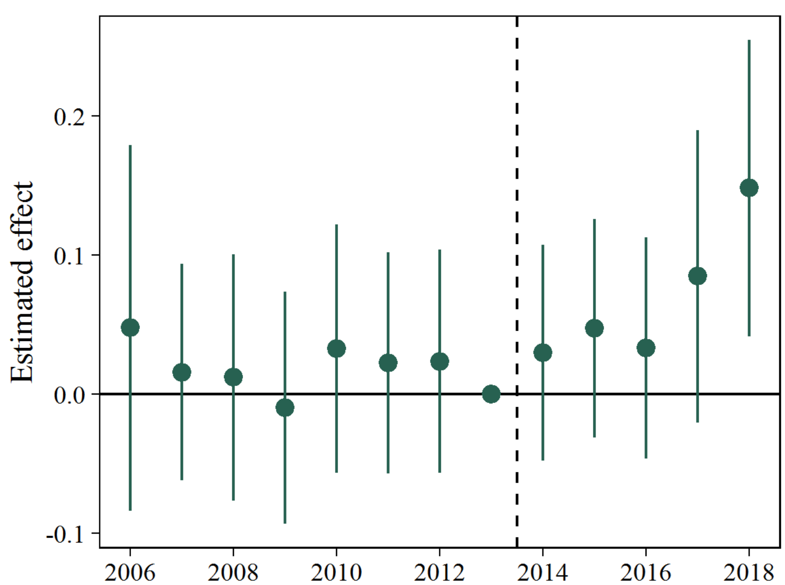


FIGURE 11.
Association between PCS and number of children



employment, this suggests that the parental care subsidy made private sector jobs more attractive for mothers of young children increasing their labor force attachment.

Conclusions

I study the introduction of a novel form of family policy implemented in Uruguay in 2014: A subsidy that allows parents employed in the formal-private sector to work half-time while receiving full-time pay, from the time maternity leave ends, and until the child is 6 month old. Even though both mother and father can alternate access to the parental care subsidy, in practice, approximately 98 percent of beneficiaries at any given month are women, highlighting an extreme gender gap in utilization, which has been documented in the literature for other gender-neutral family leave policies.

In particular, I analyze the effect that this policy has on labor force participation, labor income and hours worked for mothers of infants utilizing household survey data in a difference-in-differences and triple-difference framework.

I find that mothers are more likely to be employed following the implementation of the parental care subsidy in the short and medium-run. Mothers are 6.1 percent more likely to be employed during the first year of their child from a pre-treatment mean of 54 percent. This effect is driven by an increase in the likelihood of being employed in the formal-private sector, especially by disadvantaged mothers (those who are less educated). I study the effect that the parental care subsidy has on the intensive margin by looking at income, job experience, and number of jobs, conditional on employment. For this, I leverage the fact that public sector workers and informal workers do not have access to the new subsidy and thus can serve as a control group within the sample of employed mothers of infants. I find a large increase in subsidies received from the government, consistent with program take-up, with no significant changes in total income for mothers of infants employed in the formal-private sector after the implementation of the policy.

In the medium-run, 1 to 3 years after the birth of a child, I find an increase in the likelihood of employment for mothers. Moreover, mothers of a young child born after the implementation of the policy and employed in the formal-private sector decreased their hours worked by 5% without a significant decrease in income.

Overall, this study provides novel evidence that extensions of flexibilities after birth in the form of fully paid half-time employment causes higher labor force attachment for female workers in the short and medium-run, especially for disadvantaged mothers. The results are robust to changes in the specification and to an extended "event–study" analysis which allows for visualization of pre-treatment trends as well as the evolution of the average treatment effect in the post period.

This paper contribute to the literature on family policies in several ways. First, the analysis of parental leave policies is largely focused on high-income countries, and on the length of leave and who gets it. My paper pushes the literature in a new direction by describing and evaluating a totally different kind of policy. Expanding the scope of work on this topic increases our understanding of the effects of different policy characteristics and can lead to policy innovation. Second, I provide a causal examination of the short and medium-run effects of the parental care subsidy on female labor market outcomes. This complements studies that focus on take-up determinants and short-run effects of parental leave (Bartel et al., 2018; Romero-Balsas, 2012; Tanaka & Waldfogel, 2007). Third, my work has policy implications specially relevant for low and middle-income countries that currently have low levels of parental leave and are specially interested in increasing formal employment for women.

CHAPTER III

EFFECT OF PAID PATERNITY LEAVE ON PATERNAL INVOLVEMENT AND LABOR MARKET OUTCOMES

Introduction

Many countries have implemented parental leave entitlements reserved to fathers and non-transferable to mothers such as paid paternity leave (PPL).[1] These reforms seek to increase paternal involvement in childrearing, advance gender equality, and augment life satisfaction for fathers.

espite sustained increases in female labor force participation in recent decades, women continue to be the main providers of unpaid care and housework. Higher participation of men in unpaid work may allow women to spend more time and effort in paid work, reducing gender inequalities in the labor market (Ray et al., 2009; Rege and Solli, 2013). PPL reforms are also motivated by the idea that active fathering improves child development (Fan and Chen, 2001; Shannon et al., 2002; Jeynes, 2005; Lamb, 2010; El Nokali et al., 2010) and offers additional benefits related to fathers' health and well-being (Cools et al., 2015).

The nascent body of literature on the effects of PPL is unclear about the effects of PPL on fathers' involvement in childrearing, housework and participation in the labor market. Moreover, the limited exciting literature focuses on high-

[1] See figure A.2 in the appendix for a visualization of current paternity leave regulations across countries.

income contexts that reserve a month or more paid leave for fathers, and is silent on the effects of short-duration PPL in low and middle-income countries. In this paper, I analyze the effect that the implementation of 2 to 3 weeks of PPL has on fathers' involvement in childrearing and labor market outcomes in Ecuador, a middle-income county in South America.

Several studies have shown that the generosity of paternity leave policies is correlated with the amount of time fathers spend childrearing (Boll et al., 2014; Fuwa and Cohen, 2007). However, this literature focuses on high-income countries and it is not clear that these findings apply to non high-income settings, for a variety of reasons. First, parental leave policies in high-income countries are often of longer duration and complement a larger set of family policies, such as subsidized child-care and health care. However, in countries that lack a wide support system for families with young children, the marginal benefit of paid paternity leave may be more highly valued.

Second, the composition of families in low and middle-income countries often differs by containing multiple generations in a single household. If, in addition to the mother, the grandparents spend significant time caring for the children in the family, fathers may not be as responsive to paternity leave as families in developed countries where the alternative source of care is often a paid childcare center.

Third, labor markets in low and middle-income countries present two distinct types of sectors; formal and informal. Formal workers enjoy the benefits of a social insurance package (pensions, health care, and other services) in exchange for contributions, normally made by the employer and employee. Informal jobs, on the other hand, refer to a variety of salaried and non-salaried jobs that do not comply with social insurance, regulations, and taxes (Bosch and Schady, 2019). In practice, only workers in the formal sector can access paternity leave. In Ecuador, formal workers are more educated and earn more than informal workers. Thus, PPL increases the gap in benefits between formal and informal sectors. On the contrary, for high-income countries where family policies reach almost everyone, paid leave favors low-income families the most.

My analysis addresses two broad questions. First, does the exposure to PPL lead to higher paternal involvement in childrearing and housework in the long run? Second, does PPL affect labor market outcomes for fathers? This paper explores these questions while conducting the first causal analysis of the effect of PPL on paternal involvement and labor market outcomes in a developing country.

Ecuador was one of the first Latin American countries that implemented PPL. In January 2009, Ecuador passed a law that provided paid paternity leave for workers in the public and private sector for 10 to 25 days. A father has the option to take the fully paid leave from the date of birth of his child. This is a use-it or lose-it benefit. According to the government announcement after passing the law, the goal of this policy is to strengthen the paternal link to the family and promote his integration to the family, based on a relationship of shared responsibility with the mother. PPL allows fathers to spend critical time in the hospital and at home with the mother and newborn. Even though the policy provides just a few days of leave, the special timing of these days could set a precedent for the family dynamics and task divisions in the household that otherwise would be determined by traditional gender roles. If fathers are present during the hospital stay and early days at home, it is possible they will begin domestic routines that will continue to influence child rearing well after paternal leave has expired.

Using data from the Ecuadorian National Employment Survey (2007-2013) I employ a difference-in-differences (DID) design that compares fathers of children born after 2009, to fathers of children born before 2009, who are employed in the formal sector (treated), versus informal sector (not treated).

I estimate that fathers eligible for PPL increase time with their children by 24 minutes per week on average. This is an increase of 20 percent from an average of 2 hours per week before the implementation of PPL. The largest gain is seen among fathers who, pre-treatment, spend 1 hour per week child-rearing. When treated, the time spent with children increases by 2 hours per week. The increase in paternal involvement is driven by new fathers and public-sector employees, depends on the gender of the child, and is highly heterogeneous across education and income levels. This positive effect is higher for fathers of girls and is only present for educated and high-income fathers. Overall, this study provides novel evidence that PPL causes higher paternal involvement among educated and high-income fathers, suggesting that while fostering girls' advancement, PPL may increase existing inequalities in child-wellbeing. PPL does not lead to changes in formal employment, number of hours worked, or participation in housework. The results are robust to changes in the specification and the inclusion of individual fixed effects.

My results extend the literature on family policies in several ways. First, I provide a causal examination of the long-run effects of paternity leave on paternal involvement by looking at the time fathers spend with their children up to 4 years after taking the leave. This complements studies that focus on take-up determinants and short-term effects of parental leave (Bartel et al., 2018; Romero-Balsas, 2012; Haas, 1990; Haas and Hwang, 2008; Nepomnyaschy and Waldfogel, 2007; Tanaka and Waldfogel, 2007). Second, my research closely relates to Patnaik (2019), which studies the implementation of "daddy quotas"specifically in Quebec, Canada. However, I am able to analyze PPL implementation at the national level. Third, this is the first study that explores the effect of PPL in the setting of a non-OECD country.

The paper proceeds as follows: Section 3.2 provides background on the policy reform and literature on the effects of paid paternity leave. Section 3.3 describes the data, Section 3.4 details the empirical methods used. Section 3.5 presents results. Section 3.6 shows robustness checks, and section 3.7 concludes.

Background

Policy

Ecuador is one of several Latin American countries that have implemented paid paternity leave. In January 2009 Ecuador passed a law that regulated paternity leave for workers in the public and private sector. This law created paid paternity leave for a total of 10 to 25 days. The father can take leave for 10 days if the birth is normal. In the case of a multiple birth or c-section the leave is extended to 15 days. If the birth is premature or if there is need for special care for the newborn due to sickness or incapacity the father is allowed to extend the leave to 23 days. If the child was born with a terminal or irreversible illness the father can take up to 25 days of leave. In the case of maternity death, the father is allowed to take the remaining leave that would have corresponded to the mother. The law also provides 15 days of leave for both parents in the case of adoption from the day that the child is legally located with the family. PPL is available for the father from the date of birth of his child and has to be taken continuously. The leave is fully paid by the employer. Additionally, Ecuador offers 12 weeks of paid maternity leave. This leave was not altered at the time that paternity leave was implemented in 2009.

Literature on the effects of paid paternity leave

Different theoretical frameworks have distinct predictions with respect to the effect of PPL on labor market outcomes and intra-household child-care divisions. The time-allocation theory, developed by economists under the human capital theory, predicts that a household decides the most efficient combination of time to allocate to paid work and non-market work, such as housework and childcare (Becker, 1965; Gronau, 1977; Juster and Stafford, 1991). According to this theory, the household will maximize their utility by having the wife take on more of the household work when the wife's income is less than the husband's, or the husband's work hours are greater than the wife's (Aldous et al., 1998). The gender-ideology theory, more widely used in sociology and psychology, assumes that gender norms are a major determinant of the assignment of work (Aldous et al., 1998). This theory predicts that men with non-traditional gender role attitudes are more likely to care for their children (Deutsch et al., 1993). Both theories would predict that PPL affects paternal labor market outcomes and later involvement by altering either fathers' skill at taking care of the child or fathers' sense of proper gender roles (O'Brien, 2004).

Leave take-up by fathers vary greatly across countries and is correlated with the generosity of paternity leave policies (Boll et al., 2014; Fuwa and Cohen, 2007) which in turn are positively associated with gender egalitarian values and preferences for more generous leave offerings (Li et al., 2021). Given these associations, it is no surprise that fathers who take more leave around the time of birth of a child also share child care responsibilities more equally with mothers throughout the child's life (Haas, 1990; Haas and Hwang, 2008; Nepomnyaschy and Waldfogel, 2007; Tanaka and Waldfogel, 2007). It is challenging, however, to assess whether paternity leave policies cause higher paternal involvement, or preferences for gender equality and egalitarian values are the root of both, more generous policies, and higher involvement of fathers in childrearing and housework.

Several papers have explored these relationships by analyzing the effects of new family policies in developed nations. An increasing body of literature supports the notion that paid leave specifically assigned to fathers after the birth of a child increases leave-taking. However, there is no consensus on how these policies affect paternal involvement in childcare and labor market outcomes in the short and long run.

Patnaik (2019) studies the implementation of "daddy quotas"in Quebec, Canada, that increased benefits for all parents and reserved some weeks just for fathers. She finds that fathers' leave participation increased by 51 percentage points and duration increased by 3.1 weeks reaching on average more than 5 weeks of leave after the introduction of this reform. She finds that exposed fathers increase their time in housework by 25%, equivalent to 40 minutes per day, but do not change the time they spend childrearing. Additionally, Fathers' employment is not affected by the reform. Ekberg, Eriksson and Friebel (2013) find similar results in Sweden. They evaluate the introduction of a "daddy month", which reserved 1 month of the parental leave to the father. They find that the reform is effective at increasing fathers' leave taking by 15 days (around 50%). However, they do not find evidence that fathers in the treatment group increase the time they spend caring for children, measured as the proportion of leave taken to care for sick children, or their long-term employment and wages.

Cools et al. (2015) analyze the introduction of a father quota in parental leave in Norway and find that fathers' take-up of parental leave improves children's school performance particularly in families where the father has higher education than the mother, suggesting grater paternal involvement. They also find that men's earnings are not affected by leave-taking contradicting the findings of Rege and Solli (2013) who analyze the same Norwegian reform and show that parental leave reduces fathers' earnings significantly. In the case of Germany, Tamm (2019) shows that the introduction of two month of leave reserved for fathers (out of 14 months of family leave) increased the number of fathers taking leave by around 20 percent. He finds an increase in the time fathers dedicate to childcare (mostly on weekends) and to housework and errands (on weekdays and weekends) which persist even after fathers return from leave. However, he does not find any lasting effects of the reform on fathers' labor market outcomes.

The studies carried out in the context of Nordic countries, Germany and Canada show the effects of generous family policies, which reserve one or more months of leave for fathers. These results may not apply to the context of Ecuador or other low and middle-income countries that reserve just one or two weeks of leave for fathers. In that sense, it is relevant to highlight a few studies that analyze the introduction of less generous leave for fathers. Farré and González (2019) show that the introduction of two weeks of paternity leave in Spain increases fathers' involvement in childrearing. Eligible fathers did almost an hour more childcare per day compared with ineligible fathers 1 and 2 years after the child's birth. They explain that this increase appears to come not from reductions in housework or market work, but most likely, in leisure or sleep. Fernández-Cornejo et al. (2016) also provides suggestive evidence that paternity leave increased fathers' childcare time in Spain, using survey data from Madrid. Similarly, Romero-Balsas (2012) analyses the factors that foster the use of paternity leave in Spain after the same reform and finds that the main determinants for the use of PPL are having egalitarian roles related to child-care and family oriented values.

In the United States were neither paid or unpaid paternity leave is provide at the federal level. Nepomnyaschy and Waldfogel (2007) find that the majority of fathers take at least some leave at the birth of their child, but that the length of that leave, although it varies greatly, is usually a week or less. They also find that fathers who take longer leave are more involved in child care-taking activities nine months later. In the case of California, the first state in the United States to provide paid family leave, Bartel et al. (2018), and Baum and Ruhm (2016) find that paid family leave increases the number of days that fathers take from 1 week to nearly 1.5 weeks on average. This increase is driven in equal parts by fathers taking leave at the same time as the child's mother and by fathers who take leave on their own, while the mother is at work. Interestingly, to my knowledge, there are no studies describing the effects of state level provision of paternity leave on fathers' involvement in childcare

My paper contributes to the literature on the effects of parental leave entitlements reserved for fathers, by analyzing PPL of short duration, during the first days of life of a child. Additionally, this is the first paper to analyze PPL effects in the context of a non-OECD country. Given the mix results from previous literature, plus the difference in the characteristics of the leave and context, this paper is important to further understand whether PPL can advance gender equality in unpaid work and promote early childhood development though more involved fathers.

Data

The household and individual level data used in this study come from Ecuador's National Employment Survey (ENEMDU) for the years 2007-2013. ENEMDU is a nationally representative household survey, conducted quarterly by the census bureau of Ecuador, INEC. This survey interviews every member of the household, and any given household is interviewed during several but not all quarters.[2]

Each individual is questioned about their labor market experience, demographic factors, and use of time. This survey collects data on employment status, hours worked, sector of employment (formal or informal), and household characteristics.[3]

Importantly, the survey includes a section about time-use on housework activities. It starts by asking whether the individual participates in housework, if the response is 'yes', then the individual is asked about the number of hours per week that they spend on different housework activities. This section also includes questions to assess paternal involvement in child rearing. Specifically the question is: "How many hours per week do you spend taking care of children, elderly and sick in your home?" These data are unique because they provide insight into how paternity leave affects future intra-household divisions of childrearing responsibilities in addition to standard labor market outcomes. [4]

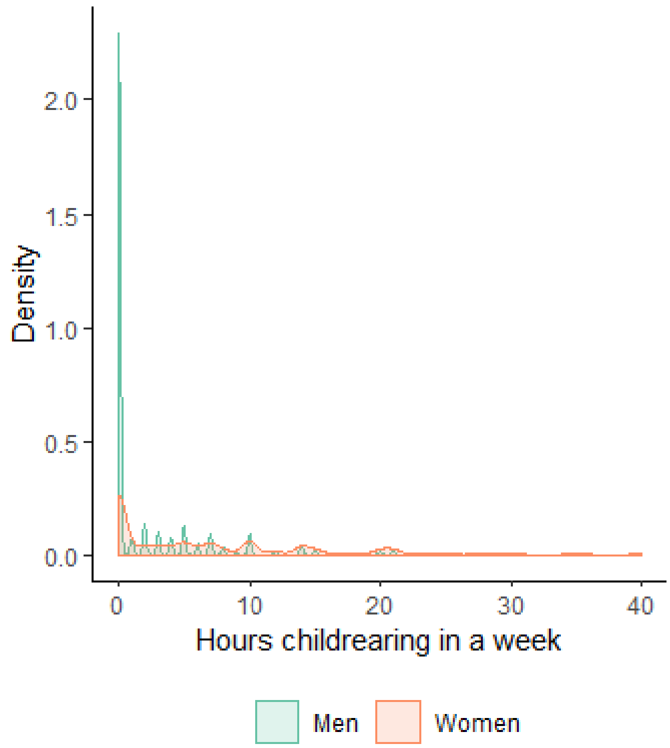
Figure 12 shows that 70 percent of men of 18 to 60 years old that live with a child do not participate in any childrearing activities, compared with 36 percent

[2]On average, the same household is interviewed during 3 or 4 quarters. This allows me to create a panel dataset and include individual fixed effects in the specification for robustness.

[3]The formal sector is defined as the group of employed individuals who work for companies of 10 or more employees and that are registered with the tax authority, the informal sector is formed by self-employed and employed individuals working for informal employers.

[4]If the individual responds that he does not participate in housework then a zero is imputed for hours childrearing and total housework. The number of hours dedicated to housework is self reported and does not necessarily equal the sum of the hours spent in each category of housework.

FIGURE 12.
Density function of hours childrearing for men and women



Notes: Kernel density estimates of number of hours men and women spend childrearing per week. Sample includes men and women between 18 and 60 years old that have a child in the home. Source: INEC.

of women. This highlights the low levels of parental involvement among the population.

The outcomes of interest are: a) whether the father participates in housework, b) the number of hours per week spent childrearing, c) total number of hours per week dedicated to housework, and d) number of hours worked.

There are two primary limitations to using ENEMDU data. First, fathers can only be linked to children who live in the same household. Thus, the analysis excludes fathers who do not live with their children. Assuming that non-resident fathers are less involved with their children than resident fathers, my results will overstate the increase in paternal involvement as a result of PPL for the average father (including those not living with their infants). Second, the ENEMDU lacks precise information on child birth dates, only reporting the age of the child in years.

The sample that I use to conduct the analysis is constructed in the following way. First, I select employed fathers that have at least one child younger than 13 years old, and are ages 18-60. Second, using the age of the children living in the household I identify parents who have at least one child born after 2009. Third, I divide employed fathers according to their employment sector. Individuals employed in the formal sector are treated, and individuals employed in the informal sector serve as the control group in the DID design. The treated group is formed by fathers who have a child born after 2009 and work in the formal sector, and the control group is formed by fathers who work in the informal sector and fathers who work in the formal sector but do not have children born after 2009.

Table 7 shows the mean and standard deviation of the outcome variables and independent variables for treatment and control groups used in the DID design. The treatment and control groups are similar to each other before the implementation of PPL except in terms of education levels and marriage status.

TABLE 7.
Summary Statistics

	Child born before 2009: Mean		Child born before 2009: SD		Child born after 2009: Mean		Child born after 2009: SD	
Employed	0.893		0.309		0.911		0.285	
Formal	0.494				0.479			
Sample size	146995				39081			
	Formal		Informal		Formal		Informal	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Time-Use								
Hours worked	48.50	14.81	42.99	14.87	47.26	12.59	42.48	13.22
Participates in housework?	0.667	0.471	0.618	0.486	0.663	0.473	0.611	0.488
Hours Childrearing	1.856	5.715	1.653	8.718	3.194	5.043	2.706	8.776
Total hours of housework	15.28	25.46	16.11	28.06	9.833	14.37	9.009	15.424
Individual Characteristics								
Age of Youngest Child	5.862	3.505	5.883	3.521	1.060	1.103	1.007	1.111
Number of children	1.660	0.895	1.915	1.130	2.066	1.118	2.493	1.421
Married	0.552	0.497	0.427	0.495	0.489	0.500	0.404	0.491
Age	36.78	10.41	36.93	11.65	33.83	10.26	34.68	11.52
Ed: Primary	0.239	0.426	0.586	0.492	0.252	0.434	0.568	0.495
Ed: Secondary	0.456	0.498	0.362	0.481	0.489	0.500	0.385	0.487
Ed: Tertiary	0.305	0.460	0.0505	0.219	0.259	0.438	0.047	0.212
Elderly in the house	0.105	0.307	0.121	0.326	0.106	0.308	0.120	0.325
Sample size	64917		66312		17086		18516	

Fathers working in the informal sector are less educated and less likely to be married than those working in the informal sector. These differences have potentially important implications for the analysis because, all else equal, more educated and married fathers tend to spend more time childrearing. Hence, it is important to control for these differences and I also analyze these subgroups separately.

With respect to before and after trends, both formal and informal workers show an increase in time childrearing but a decrease in the likelihood of participating in housework, total hours of housework, and number of hours worked. For the independent variables the biggest differences are in the age of the youngest child and the number of children in the home. Fathers of a child born after the implementation of PPL have a younger child and more children on average. Given that these characteristics directly influence the time that parents spend childrearing I control for them.

Empirical Approach

I study the effect that exposure to PPL has on paternal involvement and labor market outcomes by leveraging the quasi-experiment provided by the implementation of PPL in Ecuador in 2009. I leverage the fact that fathers employed in the formal sector benefit from this labor law, while fathers employed in the informal sector do not have access to labor benefits. Hence, I employ a difference-in-difference design that compares fathers of children born after 2009, to fathers of children born before 2009, who are employed in the formal sector, versus informal sector. In this DID design the treated group is formed by fathers who have a child born after 2009 and work in the formal sector, and the control group is formed by fathers who work in the informal sector and fathers who work in the formal sector but do not have children born after 2009. This DID specification allows for differential trends on paternal involvement across sectors and by age of youngest child as long as the difference in the rate of change between fathers of children born after 2009 and fathers of children born before 2009 employed in the formal sector would have been the same as that of fathers employed in the informal sector in the absence of PPL.

Under this assumption, I employ the following DID design to study the causal effect of PPL on outcome Y for fathers,

$$Y_{it} = \beta_1 After_{it} + \beta_2 Formal_{it} + \beta_3 After * Formal_{it} + \beta_4 X_{it} + \gamma_{ct} + \eta_{sy} + \epsilon_{it} \quad (3.1)$$

where i is an individual and t represents a quarter-year. Y_{it} is the outcome of interest. I consider four main outcome variables: a) whether the father participates in housework, b) number of hours per week that a father spends childrearing, c) number of hours per week dedicated to all housework, and d) number of hours worked. $After_{it}$ is a dummy variable that takes the value of one if individual i lives with a child born during or after 2009, and takes the value of zero otherwise.

$Formal_{it}$ is a dummy variables that take the value of one if the individual is employed in the formal sector at the time of the survey.[5]

The vector X_{it} includes a set of individual and household characteristics such as age (in five years bins), education (primary, secondary, and tertiary), marital status, ethnicity, age dummies for the youngest child in the household, and dummies for the number of children. City-time (quarter-year) and sector-year fixed effects are captured by γ_{ct} and η_{st} respectively. Standard errors are clustered at the city level (as a proxy for a labor market) to allow for correlation within cities and provide standard errors that are robust to heteroskedasticity and autocorrelation. The coefficient of interest is β_3, it captures the effect of PPL on the outcome variable among individuals employed in the formal sector. To interpret the DID coefficient as the causal effect of PPL on paternal involvement and labor market outcomes, the implementation of the policy must be uncorrelated with other time-varying determinants of time-use in the sample of employed fathers. I provide support for this assumption in section 6.

The main hypothesis of this analysis is that even though PPL offers few days of leave, the timing of these days is critical and thus facilitates routines at the intra-household level that will remain well after the end of the leave. If these

[5] Because only age in years can be identified in the survey, a reported infant (younger than one year old) in the second quarter of 2009 may have been born as early as April 2008, assuming that births are approximately uniformly distributed throughout the year, only around 25% of surveyed infants would have been born after the implementation of the policy in that quarter, compared with 75% in the fourth quarter of 2009. Therefore, I treat 2009 quarter 4 as the first quarter of the policy.

routines are sticky we should see a bigger effect of PPL for fathers that were able to access it during the birth of their first child. To test this hypothesis I employ a difference-in-difference-in-differences (DDD) design based on equation (3.1) where the third difference indicates that fathers can access PPL for their first child. In the following equation $First_{it}$ is a dummy variable that takes the value of one if the first child of the family was born after 2009. Given that fathers who had access to PPL for their first child are a subset of all treated fathers, the interactions $After{*}First$ and $After{*}Formal{*}First$ are collinear to $First$ and $Formal{*}First$. In the estimation I include the latter. I estimate the following specification:

$$\begin{aligned} Y_{it} = &\beta_1 Formal_{it} + \beta_2 After_{it} + \beta_3 First_{it} + \beta_4 Formal * After_{it} + \\ &\beta_5 Formal * First_{it} + \beta_6 X_{it} + \gamma_{ct} + \eta_{sy} + \epsilon_{it} \end{aligned} \tag{3.2}$$

where i is an individual and t represents a quarter-year. β_4 represents the effect that PPL has on fathers that receive this benefit for their second or later child, β_5 represents the additional effect of the policy for fathers that receive PPL for their first child. Thus, the effect of PPL on first time fathers is $\beta_4 + \beta_5$. X_{it} and fixed effects are the same as described for equation (3.1).

Since both, the DID and DDD methods (equation (3.1) and (3.2)) leverage variation in the exposure to PPL rather than actual participation, I estimate *intent-to-treat* (ITT) which is preferable to estimates of *treatment on the treated*

(TOT) for several reasons.[6] First, TOT estimates could be subject to the same bias from selection into treatment that previous cross-sectional studies have been criticized for. Second, from a policy-making perspective ITT effects may be more relevant as they allow for feedback effects, whereby the PPL could have changed expectations and norms beyond the effects of actually using the leave option. The implementation of PPL sent a strong public message about the importance of paternal involvement in housework which may have incentivized fathers who were exposed to PPL but not treated to nevertheless be more involved in the home. However, it is safe to assume that feedback effects on parents who were exposed but not treated are smaller than the first-order-effects on parents who took the leave, such that the ITT results presented here underestimate the true causal effect of paternity leave on those who take it.

Results

Table 8 presents the results from estimating equation (3.1) of the effect of PPL on paternal involvement and hours worked. Column 1 shows that PPL has a small but positive effect on the likelihood that a father participates in housework. Column 2 indicates that the time fathers spend childrearing increases by 25 minutes. This estimate is statistically and economically significant, representing

[6] ITT refers to the average effect of the policy on everyone expose to it regardless of whether they where treated, whereas OTT refers to the average effect of the policy for those that where actually treated by it. This this case OTT would represent the effect of PPL for those fathers that took the lave.

an increase of 20 percent from an average of 2 hours for formal workers before the implementation of PPL. Column 3 shows a small yet statistically significant increase in the total time dedicated to housework that mostly corresponds to the increase in time childrearing. Column 4 indicates that there is no effect of PPL on hours worked. The increase in father involvement as a result of exposure to PPL does not produce a decline in hours worked, indicating that fathers are not substituting away from labor, but are probably substituting away from leisure activities.

Table 9 presents the estimation of equation (3.2). This table supports one possible mechanism through which PPL affects paternal involvement. Namely, the routines established during the initial parenting experience are "sticky" and can have lasting effects on paternal behavior. In fact, column 2 shows that the increase in paternal involvement seen in table 8 is driven by new fathers. On average fathers that have access to PPL for their first child, increase their time childrearing by 37 minutes (equivalent to adding the two marginal effects β_4 and β_5), which is a 30 percent increase from a 2 hours pre-treatment mean. This increase is significantly larger than the effect of PPL for fathers who were exposed to PPL for their second or later child, who increase time childrearing by 16 minutes. These results support the hypothesis that intra household task-divisions are produced early on and that they are persistent over time.

TABLE 8.
Effect of PPL on time use of fathers

	Dependent variable:			
	Participates on housework	Hours childrearing	Total hours of housework	Hours worked
	(1)	(2)	(3)	(4)
Formal	−0.023***	0.100	−0.565***	3.950***
	(0.008)	(0.072)	(0.169)	(0.335)
After	−0.003	−0.249***	−0.341**	−0.082
	(0.007)	(0.055)	(0.159)	(0.210)
After_Formal	0.014*	0.418***	0.503***	0.098
	(0.008)	(0.063)	(0.166)	(0.258)
Pre-t. Mean	0.677	2.051	20.85	49.58
Observations	166,831	166,825	166,827	166,831
R^2	0.168	0.080	0.835	0.147
Adjusted R^2	0.123	0.076	0.826	0.101
Residual Std. Error	0.449	4.143	10.180	14.916

Notes: OLS estimates from difference-in-differences regressions based on equation (3.1). Participates on housework is an indicator equal to one if the respondent sends at least one hour doing any type of housework and zero otherwise. Hours childrearing, total hours of housework and hours worked are the self-reported number of hours in a week used for each activity. Housework includes childrearing, cleaning, shooping, doing laundry, cooking, and educating. Regressions include individual controls, time-city and sector-year fixed effects. Standard errors (in parentheses) allow for clustering at the city level. ∗∗∗, ∗∗, and ∗ denote statistical signifficance at the 1, 5, and 10 percent levels.

TABLE 9.
Effect of PPL on fathers behavior - First child

	Dependent variable:			
	Participates in housework	Hours childrearing	Total hours of housework	Hours worked
	(1)	(2)	(3)	(4)
Formal	-0.014	-0.202^{***}	-0.583^{***}	3.974^{***}
	(0.018)	(0.064)	(0.167)	(0.342)
After	-0.003	-0.259^{***}	-0.245	-0.173
	(0.008)	(0.091)	(0.165)	(0.201)
First	0.004	0.082	-0.325^{**}	0.281
	(0.008)	(0.081)	(0.165)	(0.260)
Formal_After	0.009	0.275^{***}	0.394^{**}	0.279
	(0.008)	(0.079)	(0.178)	(0.265)
Formal_First	0.013	0.340^{***}	0.337	-0.521
	(0.009)	(0.113)	(0.207)	(0.331)
Pre-t. Mean	0.677	2.051	20.85	49.58
Observations	166,831	166,825	166,827	166,831
R^2	0.168	0.154	0.835	0.147
Adjusted R^2	0.123	0.109	0.826	0.101
Residual Std. Error	0.449	4.068	10.180	14.916

Notes: OLS estimates from difference-in-differences regressions based on equation (3.2). Participates on housework is an indicator equal to one if the respondent sends at least one hour doing any type of housework and zero otherwise. Hours childrearing, total hours of housework and hours worked are the self-reported number of hours in a week used for each activity. Housework includes childrearing, cleaning, shopping, doing laundry, cooking, and educating. Regressions include individual controls, time-city and sector-year fixed effects. Standard errors (in parentheses) allow for clustering at the city level. $***$, $**$, and $*$ denote statistical significance at the 1, 5, and 10 percent levels.

Next, I examine heterogeneity in the effect of PPL on paternal involvement according to father characteristics. Each column in table 10 represents a different regression where the dependent variable is the time, in hours, that a father spends childrearing, and the sample varies according to father characteristics. Column 1 only includes fathers that participate in housework. Among these fathers the effect of PPL is slightly larger in absolute terms compared with the average effect when fathers that do not participate in housework are included. Columns 2 and 3 include fathers whose youngest child is a boy or a girl respectively. The effect of PPL on childrearing depends on the gender of the child. Even tough both groups have similar pre-treatment means the effect of PPL on time childrearing is higher for fathers whose younger child is a girl. Suggesting that PPL can play a role in fostering girl advancement.

Columns 4 and 5 in table 10 condition on being employed in the public or private sector. The effect is significantly larger for public employees. This is probably a result of larger take-up among public sector employees. When a law is passed, public jobs are rapid to communicate, implement the changes and provide employees with benefits whereas the private sector has may take longer to be informed and comply with new regulations, specially in context with lax enforcement.

TABLE 10.
Heterogeneity of PPL effect on time spend childrearing

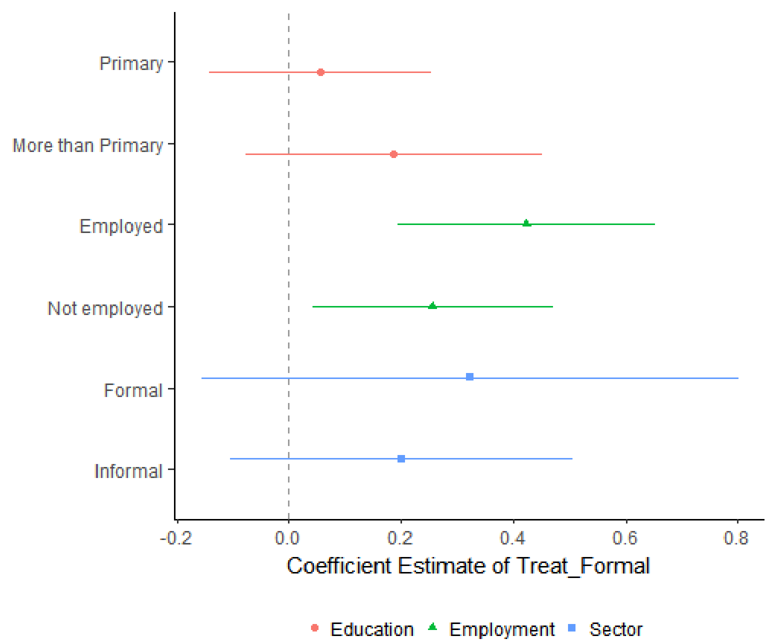
	HW	Gender		Public		Education			Income	
	Yes	Boy	Girl	Yes	No	Primary	Secondary	Tertiary	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Formal	−0.136	−0.028	−0.088	0.414***	−0.074	−0.308**	−0.072	−0.150	−0.144	−0.239*
	(0.118)	(0.121)	(0.092)	(0.148)	(0.069)	(0.121)	(0.104)	(0.294)	(0.096)	(0.130)
After	−0.291**	−0.201*	−0.351***	−0.103	−0.210**	−0.102	−0.151	−0.088	−0.163*	−0.435***
	(0.121)	(0.113)	(0.108)	(0.084)	(0.088)	(0.093)	(0.127)	(0.366)	(0.087)	(0.138)
After*	0.452***	0.286***	0.507***	0.534***	0.350***	0.061	0.238**	−0.009	0.117	0.537***
Formal	(0.084)	(0.102)	(0.100)	(0.151)	(0.067)	(0.104)	(0.110)	(0.297)	(0.097)	(0.090)
Pre-t. Mean	3.027	2.093	2.001	2.161	2.025	1.533	1.931	2.591	1.906	2.130
Ind. Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	106,912	85,752	81,073	100,005	151,643	69,203	69,173	28,449	82,661	82,642
R^2	0.222	0.198	0.203	0.180	0.155	0.208	0.209	0.251	0.194	0.196
Adjusted R^2	0.158	0.116	0.117	0.106	0.106	0.105	0.117	0.131	0.107	0.126
Res. S.E.	4.641	4.095	4.005	4.064	4.037	3.772	4.026	4.667	4.035	4.054

Notes: OLS estimates from difference-in-differences regressions based on equation (3.1) where the dependent variable is the number of hours per week, that an individual spends taking care of children, and the sample varies according to father characteristics. Column 1 includes fathers that participate in housework, columns 2 and 3 include fathers whose youngest child is a boy or a girl respectively, columns 4 and 5 condition on whether the individual had a public job. Columns 6-8 include fathers who have obtained some primary, secondary, or tertiary education and columns 9 and 10 divide fathers according to their income into low and high brackets. Regressions include individual controls, time-city and sector-year fixed effects. Standard errors (in parentheses) allow for clustering at the city level. ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1, 5, and 10 percent levels.

Columns 6-8 include fathers who have obtained some primary, secondary, or tertiary education. The effect of PPL is only statistically significant for those parents that have some level of secondary education. PPL has no effect on individuals with the lowest and highest levels of education. Columns 9 and 10 divide fathers according to their income into the lowest and highest half of the distribution. Only high income fathers present a positive and significant effect of the policy on their time childrearing. Overall, this table provides evidence that PPL causes higher paternal involvement among educated and high-income fathers, suggesting that PPL may increase existing inequalities in child-wellbeing. This result differs from studies in developed countries where the implementation of paid leave benefits low-income families the most (Bartel et al., 2018; Nepomnyaschy and Waldfogel, 2007). This could reflect differences in policy enforcement. It also could reflect larger stigma associated to paternity leave among low-educated workers in developing countries.

Several studies suggest that paternal involvement also depends on maternal characteristics such as education and employment. To address possible heterogeneous effects of exposure to PPL according to maternal characteristics I estimate equation (3.1) restricting the sample according to mothers' level of education, employment status, and sector. Figure 13 plots the DID estimate $Formal\_After$ for each regression. Education does not seem to be an important determinant for PPL, however whether the mother has a job does play an important role in PPL effect. Paternal involvement increases by 25 minutes per week as a result of PPL in households where the mother is employed, compared with 15 minutes in household with a non-working mother. Both effects are significantly different from zero but they are not statistically different from each other. For those fathers whose partner or wife is employed, whether the job is formal or informal does not seem to make a difference in the effect of PPL on paternal involvement.

FIGURE 13.
Heterogeneity of PPL effect by mother characteristics



One question that may arise is, where in the distribution of paternal involvement is the effect of PPL coming from? One possibility is that fathers who were already involved in the care of their children might increase their time by several hours from an already high pre-treatment mean. Another possible case is that the policy has a small but important effect on the majority of fathers and not only on those already involved. Especially those that did not participate in childrearing before the implementation of the policy. To shed light on this question figure 14 presents the effect of PPL as seen in equation (3.1) but using a quantile regression analysis.

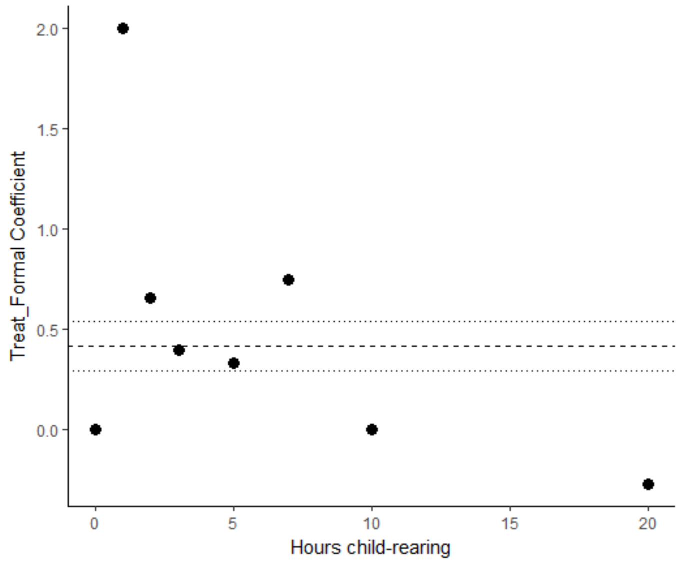
Figure 14 suggests that the increase in paternal involvement is driven by fathers at the lower end of the distribution of time. The largest gain from PPL is seen among fathers who, pre-treatment, spend 1 hour per week childrearing. When treated, the time they spend with children increases by 2 hours per week, a 200 percent increase. Fathers that do not spend any time childrearing do not benefit from the policy and fathers in the top percentile of the distribution (that already spend more than 10 hours childrearing) experience a small decrease. The dotted line marks the average treatment effect presented in table 8.

Robustness

An important limitation of a DID analysis is that one must rely on an assumption that outcomes in treatment and control groups would have followed parallel trends in the absence of the policy reform. I perform a variety of robustness tests that lend credibility to the identifying assumption.

This assumption would be violated if the PPL reform induced selection into the sample through impacts on fathers' employment status, sector, or fertility patterns. Moreover, since I can only observe fathers who reside with their children

FIGURE 14.
PPL effect on time childrearing - Quantile regression



in the data, I face a threat to the identification assumption if the policy influences father–child cohabitation rates or if it is correlated with differential changes into or out of the formal sector. To evaluate the plausibility of these concerns, table 11 presents results from regressions that estimate the DID model (equation (3.1)) using observable paternal characteristics as dependent variables (and omitting that specific variable in X_{it}).

The results in columns 1 and 2 of table 11 show that having a child after 2009 does not significantly affect the probability of being employed or the sector of employment, i.e. there is no selection into the formal sector. Given the short duration of the leave (10 to 15 days), these findings are not surprising. Columns 3 and 5 show that PPL has a significant negative effect on the number of children and the age of the father. This is consistent with the treatment group being younger than the control group as also shown in Table 7. The age of the father and the number of children in the home are positively correlated with time spent childrearing. Hence, if there is bias it will be biased toward zero.[7] Column 4 and 6 show that PPL has no effect on the age of the youngest child of the family, or marriage status. Overall, it is unlikely that differential demographic trends among treated fathers drive the results shown in section 3.5.

Table 11 provides some evidence that fathers main observable characteristics would have remained the same in the absence of PPL. However, even if this is true, I also need to assume that fathers do not sort themselves into treatment according to unobservable characteristics that affect paternal involvement and labor market outcomes, such as preferences for children, ability, or work ethic. Previous literature has addressed this concern by including individual fixed effects (Currie and Walker, 2011).

Table 12 shows that the findings are similar if we add individual fixed effects to equations (3.1) and (3.2). Individual fixed effects control for all observable and unobservable characteristics that do not change over time and that may affect the outcome of interest. The sample used for these estimations includes men that I observe in the data more than once, and that at least in one of the observations had a child in their home.

[7] The average age of the fathers in the treatment and control group is 33.8 and 36.6 years respectively.

TABLE 11.
Correlation between PPL and paternal characteristics

	Dependent variable:					
	Employed	Formal	Children	Age Child	Age	Married
	(1)	(2)	(3)	(4)	(5)	(6)
After	0.007*	0.002	0.051*	−5.177***	0.341**	−0.004
	(0.003)	(0.005)	(0.027)	(0.037)	(0.152)	(0.006)
Formal			−0.023	−0.115***	−0.030	0.052***
			(0.020)	(0.043)	(0.247)	(0.007)
After_Formal			−0.204***	0.019	−0.602***	−0.009
			(0.021)	(0.036)	(0.152)	(0.007)
Pre-treatment Mean	0.908	0.513	1.777	4.844	35.967	0.564
Observations	186,076	166,831	166,831	166,831	166,831	166,831
R^2	0.209	0.320	0.303	0.506	0.261	0.328
Adjusted R^2	0.172	0.283	0.266	0.480	0.222	0.292
Residual Std. Error	0.277	0.423	0.952	2.688	9.781	0.420

Notes: Each column represents a different regression with a different dependent variable. The dependent variable in columns 1-6 are: Whether the father is employed at the time of the survey, whether he is employed in the formal sector, father's age, marriage status, number of children in the family, and age of the youngest child in the family. Column 1 includes all fathers in the sample, while columns 2-6 condition on employment at the time of the survey. All regressions include individual controls (except the dependent variable), and city-time and sector-year fixed effects. Standard errors (in parentheses) allow for clustering at the city level. $***$, $**$, and $*$ denote statistical significance at the 1, 5, and 10 percent levels.

TABLE 12.
Effect of PPL on fathers behavior - Individual FE

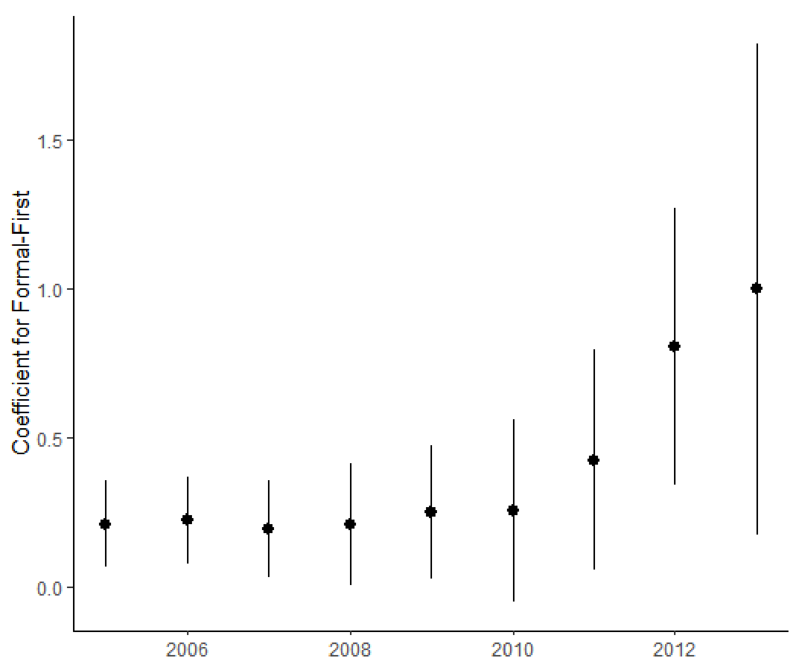
	Dependent variable:				
	Hours Childrearing		Participates in Housework	Total hours of housework	Hours worked
	(1)	(2)	(3)	(4)	(5)
Formal	−0.016	−0.016	−0.002	0.030	5.198***
	(0.122)	(0.122)	(0.014)	(0.334)	(0.528)
After	−0.136	−0.170	−0.007	−0.475	−0.516
	(0.136)	(0.129)	(0.015)	(0.316)	(0.460)
First	−0.079				
	(0.183)				
After_Formal	0.237*	0.334***	0.015	0.497*	0.391
	(0.133)	(0.120)	(0.013)	(0.281)	(0.417)
Formal_First	0.283				
	(0.202)				
Pre-treatment Mean	1.767	1.767	0.682	20.825	49.316
Observations	146,317	146,317	146,323	146,319	146,323
R^2	0.543	0.543	0.544	0.924	0.550
Adjusted R^2	0.262	0.262	0.263	0.878	0.273
Residual Std. Error	3.523	3.523	0.410	8.991	13.721

Notes: Regressions include individual controls, time-city, sector-year, and individual fixed effects. Standard errors (in parentheses) allow for clustering at the individual level. ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1, 5, and 10 percent levels.

The identifying variation for this specification comes from changes over time in the number of children and exposure to PPL within fathers. Columns 1 and 2 of table 12 show similar coefficients to the ones presented in tables 8 and 9. Column 1 shows an increase in time spent childrearing of 19 percent with respect to the pre-treatment mean. Column 2 shows an increase of the same magnitude as seen before with a loss in power given that fathers that receive PPL for their first child are younger than the rest of the fathers in the sample and thus I do not observe all of these fathers more than once. Hence, fewer treated fathers are included in this estimation than in the cross-sectional sample used for table 9. Columns 3, 4 and 5 show similar coefficients as table 8.

To asses the evolution of the effect of PPL Figure 15 plots the coefficient estimate of *Formal_First* of equation (3.2) when I use the full sample period but I vary the time of the policy introduction from 2005 until 2012. This represents the additional effect of being exposed to PPL for the first-born child in the family.

FIGURE 15.
Evolution of PPL effect on paternal involvement



The plot indicates a break in the trend in 2009 with a sustained increase of the effect of the policy after that year. This increase in the effect can result from an increase in PPL take-up.

Throughout the paper I have used time spent childrearing as the main outcome of interest concerning paternal involvement. However, the ENEMDU data also include questions about other categories of housework. Table 13 presents the estimation of equation (3.1) when I use the remaining categories of housework as dependent variable. Exposure to PPL does not have a significant effect on the time that fathers spend shopping, fixing or washing clothes, or cooking. Column 2 and 6 shows an small but significant effect of PPL on time cleaning and educating.

Importantly, exposure to PPL has a negative effect on the time that father spend helping children with school work. Even though this is a smaller decrease than the increase in time childrearing it could indicate some substitution away from older children to time with younger children in the home.

Conclusions

Many developing countries have implemented paid paternity leave (PPL) with the goal of increasing paternal involvement in child-rearing and advancing gender equality. However, little is known about how this labor law affects intra-household dynamics and labor market outcomes in developing countries.

TABLE 13.
Effect of PPL on Fathers behavior - All housework categories

	Dependent variable:						
	Participates in housework	Hours Cleaning	Hours Shopping	Hours Clothing	Hours Cooking	Hours Educating	Total Hours
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Formal	-0.023^{***}	-0.079^{**}	-0.080^{**}	-0.086^{***}	-0.198^{***}	0.071^{*}	-0.565^{***}
	(0.008)	(0.037)	(0.035)	(0.028)	(0.049)	(0.042)	(0.169)
After	-0.003	-0.066^{**}	-0.012	-0.058^{***}	-0.060	0.129^{***}	-0.341^{**}
	(0.007)	(0.030)	(0.023)	(0.022)	(0.041)	(0.035)	(0.159)
After* Formal	0.014^{*}	0.070^{**}	0.001	0.049^{*}	0.091^{*}	-0.137^{***}	0.503^{***}
	(0.008)	(0.032)	(0.024)	(0.026)	(0.048)	(0.036)	(0.166)
Observations	166,831	166,827	166,826	166,827	166,827	166,826	166,827
R^2	0.168	0.143	0.185	0.122	0.110	0.140	0.835
Adjusted R^2	0.123	0.097	0.142	0.075	0.062	0.094	0.826
Residual S.E.	0.449	2.180	1.613	1.543	3.051	2.263	10.180

Notes: Regressions include individual controls, time-city, and sector-year fixed effects. Standard errors (in parentheses) allow for clustering at the city level. $***$, $**$, and $*$ denote statistical significance at the 1, 5, and 10 percent levels.

I leverage the quasi-experiment provided by the implementation of PPL in Ecuador in 2009 to identify the effect that it has on paternal involvement and labor market outcomes. This is the first paper to study the effect of PPL in a developing country. I use data from the Ecuadorian National Employment Survey (2007-2013) to employ a generalized difference-in-differences design that compares fathers of children born after 2009, to fathers of children born before 2009, who are employed in the formal sector (treated), versus informal sector (not treated).

I estimate that fathers eligible for PPL increase time with their children by 24 minutes per week on average. This is an increase of 20 percent from an average of almost 2 hours per week before the implementation of PPL. The largest gain is seen among fathers who, pre-treatment, spend 1 hour per week child-rearing. When treated, the time spent with children increases by 2 hours per week. The increase in paternal involvement is driven by new fathers and public-sector employees, depends on the gender of the child, and is highly heterogeneous across education and income levels. This positive effect is higher for fathers of girls and is only present for educated and high-income fathers.

Overall, this study provides novel evidence that exposure to PPL causes higher paternal involvement among educated and high-income fathers, suggesting that while fostering girls advancement, PPL may increase existing inequalities in child-wellbeing. Further research is needed to establish which aspect of the policy implementation can be improved to benefit low-income families and their children.

CHAPTER IV

BANKING THE GROUP: IMPACT OF CREDIT AND LINKAGES AMONG UGANDAN SAVINGS GROUPS

Introduction

Credit is an important yet often missing element in the production process in low income countries. Farmers need credit to make investments, confront a cyclical earnings cycle, and smooth out unexpected income or consumption shocks. Microentrepreneurs also confront cyclical demand and the need to make relatively large investments in stock or machinery. Despite such needs, many rural communities are often underserved by financial institutions, including microfinance. Traditional lending models, including microfinance, rely on individual or joint liability contracts which generally have strict rules on selection and repayments, and are not very common in rural parts of sub-Saharan Africa.

In this study, we seek to better understand the impact of delegated credit, delivered by a commercial bank to a savings group rather than an individual. Savings groups already provide financial intermediation to millions of households in rural areas of sub-Saharan Africa (Allen and Panetta, 2010, Karlan et. al. 2017), but remain largely disconnected from formal credit markets. With delegated credit, which is better known as “linkage credit”, the bank offers a loan with specific terms (interest rates and repayment plans), and savings groups on-lend the external credit to members, using the (generally more flexible) terms of credit that are prevalent in that group. Repayments to the bank are generated through savings accumulation and repayment of internal loans, and are not tied directly to those who (indirectly) borrowed from the bank.

To understand the impact of this novel type of finance, we randomly introduced a delegated credit product to existing savings groups in five districts in Uganda. Together with a complementary savings account, loans are provided to the group as a whole, and not to any single individual. In this paper, we show how credit linkage generates new internal lending, and then report on the extent to which savings groups participants benefit from delegated bank credit. The potential expansion of credit operates through a very specific *credit rationing* channel: the bank provides additional funds to the group, and the group uses those funds to provide credit to members. Note that groups already provided loans to members; moreover, while the interest rate charged by the bank generally differs from the interest rate charged internally by the group to its members, this internal rate is unaffected by the additional funds. In other words, the product increases the *quantity* of credit, but not its price.

In addition to the expansion of credit, the adoption of formal financial products is likely to impact the groups through a number of other channels. First, the associated savings account allows groups to store excess funds in a safe place, and thus reduce the need to over-lend at the end of each cycle and the ability to lend money saved in the bank (if this is not easily accessible to members). Second, the intervention provided a great deal of personal contact with bank agents, thus improving the information available to members about the banking system. We expect that this might generate spillovers from group accounts to individual accounts. Finally, we expect that the program changes the incentives to join and remain in a treated group. For instance, these groups may become more attractive to households seeking larger loans, or less attractive to savers who face reductions in returns to their savings.

Our study, started in 2015, provided training and facilitated access to these formal financial products through 2016. Our data collection effort took place in February-March 2018, less than two years after the intervention. By the end of the implementation period, two thirds of the targeted groups had submitted a loan request, and one third had received a loan from the bank. We find high rates of pass-through of the loan: internal loans to members are four times higher in the week of the bank loan receipt relative to the expected amount; the increase is around 1 million shillings, or 40% of the average first-time bank loan (2.3 million shillings). We find that the internal loans generated are not larger in size; thus, the increase in lending comes from an increase in the number of loans generated. Despite evidence that internal lending amounts increased in a sustained way, a majority of groups stopped borrowing from the bank after the initial loan allocation, suggesting that the benefits from the program were not sufficient to overcome the costs of continued engagement.

In terms of welfare impacts, in the short run, the intervention raised financial resources available to members, lowered rates of food insecurity; however, the relatively sizable increase in household income does not raise to the level of statistical significance. Moreover, all benefits wore off by the end of the study. On household production, we find fewer households investing in agriculture, and statistically insignificant increases in enterprise sizes (as measured by revenues and costs) as well as profits. As for the other outcomes, point estimates are larger at midline.

These moderate effects are modulated by the finding that groups exposed to the treatment suffered from higher rates of member dropout. This is the result of increased churn within groups, and not of increased group mortality. After three years, the gap in dropout rates between treated and control groups is somewhat smaller and becomes statistically insignificant, indicating some catching up by control groups.

The findings are consistent with the idea that linkage helps relax liquidity constraints in the group, but the average benefit from linkage do not appear to be sufficiently high to cover the significant recurrent costs. The muted impacts on investments are also consistent with a broader literature that finds small average impacts from microfinance interventions (Banerjee et. al. 2015), which is puzzling given that investment returns appear to be high in rural areas among credit borrowers (Beaman et. al. 2014). The fact that external credit generated changes in group membership is consistent with other experiments of delegated credit (Maitra et. al. 2017).

The rest of the paper is organized as follows. Sections 4.2, 4.3 and 4.4 provide information on financial linkages, in Uganda and elsewhere; details on the accounts offered in our study; an explanation of the structure of the intervention; a discussion of study timeline and instruments. Sections 4.5 and 4.6 discussed the estimation strategy adopted and report the results. Section 4.7 concludes.

Background information

Savings Groups

Savings groups are community-based financial institutions, whose members save on a weekly basis, are able to accumulate those savings through a storage technology (typically, a savings box), and use those accumulated savings to generate interest bearing loans to members. Thus, savings groups provide a degree of financial intermediation in the community. Consistent with groups matching savers and borrowers, Cassidy and Fafchamps (2015) show that there is negative assortative matching along time consistency. A number of impact evaluation studies found that the introduction of savings groups improves food security, overall consumption smoothing, livestock holding, household business outcomes and women's empowerment(Ksoll, Lilleør, Lønborg, and Rasmussen, 2015, Beaman, Karlan, and Thuysbaert, 2014, Gash and Odell, 2013, Karlan, Savonitto, Thuysbaert, and Udry, 2017); however, these welfare impacts are quite muted, raising the question of why the increase in financial intermediation created by savings groups does not improve outcomes.

Savings groups are quickly becoming common in both rural and urban areas of Uganda and elsewhere. According to the latest Finscope figures, almost half of households in Uganda belong to one (FinScope, 2018).

Financial Linkage

Formal banking products that are targeted specifically to savings group are called linkage products. Banks may offer a group savings account, which can be used by the group to store excess funds. Group savings account protect savings from theft or misuse; however, they also raise the cost of accessing the group's liquidity, as accessing the funds may involve time and travel to a bank branch of mobile money operator. A second product, and the focus of this paper, is a bank loan, offered to the group. The bank loan raises the liquidity of the group, and allows more internal loans to be generated and issued. According to the State of linkage report, as of 2016 25 banking institutions in 27 sub-Saharan countries offered some type of linkage product to groups. In Uganda, where savings groups are particularly prevalent, at the time of the intervention there were six different financial institutions offering these products.

It is important to explain how these two products integrate with the daily operations of the savings group. Savings account provide an alternative location to members' funds. They are safer than a lock-box, and thus should alleviate the fear of losing funds to theft. On the other hand, because funds in savings accounts are less liquid, accounts may discourage internal lending. The loan product increases the funds available to the group for internal lending; note that the interest rate charged by the bank is lower than the rate charged internally by the group (that rate varies from a minimum of 3% per month to 10% per month), and that internal loans generated by the bank loan are priced at the internal rate.

The Opportunity Bank product

We study a linkage product offered by Opportunity Bank Uganda LTD. (OB) and marketed around the country concurrently with the study. Bank loans range between one and 20 million UGX and carry a monthly interest of 2.75%. Repayment periods vary from three to nine months. The initial loan was always limited to no more than five million UGX, with a three month repayment period. Issued loans are given to the group and not to any one individual, and are used to generate internal loans to members who borrow using the internal rates. Groups repay the bank on a monthly basis, either via cash payments to a bank representative, or through the mobile network or bank branch. Crucially, and unlike more standard microfinance interventions, repayments to the bank are generated through the cashflow of the group, i.e., from savings and internal loan repayments. These cash flows do not need to coincide with the repayments issued by those members who borrowed from the bank's funds.

The process of linking the group to Opportunity Bank is not straightforward. First, groups must be formally registered with local authorities (at the parish level). Usually, registration requires completing a registration form and obtaining signatures from community representatives. Second, groups need to have a (free) group savings account, held at an OB branch. The bank uses the account to manage loan deposit and payments, but groups can also use it to store excess savings. Third, financial regulations require borrowing groups to have financial identification cards, issued by the Government of Uganda. To meet these regulations, three representatives of the group complete a financial card request under their name; deposit a biometric reading of the fingers; and pay a one-time fee of UGX 15,000 (USD 5) each. These actions require a visit to the branch[1]. Fourth, groups complete a loan application form, which include an extensive set of documents. Finally, branch managers take two weeks or longer to decide whether to approve the loan request. Approved loans are then deposited into the group's group saving account, after a number of banking fees and duties totaling UGX 120,000 (USD 35) have been subtracted from the loan.

As the above makes clear, while there are significant one-time learning and financial costs involved in linkage, groups also face large recurring costs

[1]The creation of financial cards turned out to be very time consuming; biometric readers often failed to recognize all ten fingers, took hours to complete, and often were unsuccessful.

in maintaining these linkages. Secondly, linked groups gain access to a savings product, in addition to the bank loan. Part of the way groups respond to linkage may thus be mediated through the acquisition of this savings account. To account for this, our intervention will attempt to separate the effect of savings from those of credit.

The intervention

Our intervention is registered under AEACTR-0003613 and took place in five districts of Central Uganda: Buikwe, Luweero, Nakaseke, Nakasongola, and Wakiso. In each district, we partnered with one of two local NGOs, READ Uganda and Project SCORE, to enroll savings groups in the study and provide support to the research team. These NGOs were chosen due to their focus on savings group formation, their active and ongoing support to groups they formed, and their ability to intermediate between groups, research teams, and representatives of the commercial bank.

Groups enrolled in the study were assigned to one of three treatment arms: a control group, a "savings only" intervention, and a "savings + loan" intervention. Groups assigned to one or both financial products received an intervention package that consisted in a number of activities aimed at lowering the implicit and explicit costs of linking to the bank. Groups received numerous visits from NGO and bank representatives, during which the group was able to learn about the linkage process, the terms of the products, and the requirements needed to successfully obtain a financial product. The study also facilitated the formal registration of the savings group within local authorities, and helped filling out the applications for the savings accounts and loans. To further reduce transaction costs, the research team paid the one-time fees associated with the financial cards. The overall intervention, spread over a period of months, was very intensive, went beyond the standard engagement of commercial banks, and was not cost effective.

One noteworthy difficulty in organizing this linkage product is that the bank branch managing the intervention was located 60 to 100 km away from study communities. To reduce the substantial transaction costs associated with managing the savings and loan accounts, groups had the ability to administer some transactions remotely, through mobile money. In addition, on occasion an OB mobile branch (located inside an armored truck) visited the study communities to carry out banking transactions.

Study timeline

In late 2014 and early 2015, a research team representative visited approximately 300 VSLAs in five Central Region districts served by READ Uganda and SCORE program in order to screen groups based on their overall capacity and performance. The screening tool employed was developed by CARE to help commercial banks identify groups that could benefit from formal bank linkages, and was considered state of the art at the time of the study. Groups that were enrolled in the study scored sufficiently high in the questionnaire, and were thus highly likely to be considered acceptable by the commercial bank. In total, 156 groups were selected for the study, and randomized into the three treatment arms. To avoid cross-treatment spillovers, treatment assignment was done at the level of the village.[2]

In February-April 2015, baseline interviews were carried out in all study groups. For each group, 15 respondents were selected for the baseline. The intervention phase was slated to begin immediately after randomization. However, a series of delays caused by the speed of governmental approvals and commercialization of the product pushed the start date well past the baseline and into early 2016. At that time, the commercial bank hired a field agent solely devoted to marketing the product to savings groups in the study and helping the groups navigate the linkage process.

The active intervention period lasted one year and ended in December 2016. After that date, the bank field agent was relocated to a different branch and support activities to study groups ended. To measure impacts, the research team collected midline surveys in February-April of 2018, and the endline survey one year later, between February and April of 2019.

[2]Because not all groups in a village participated in the study, villages assigned to the loan treatment will generally have groups where linkage did not take place. Groups in study villages might not have been part of the study for a variety of reasons, including: failure to score sufficiently high in the screening tools; not being supported by the SCORE or READ; refusal to participate in the study; refusal to being screened; were not in session at the time of the screening.

It is important to highlight that the product became available in all OB branches at the start of our intervention in 2015, potentially leading to program spillovers. However, the company introduced it in the areas under study in a controlled way, and was not allowed to market other individual products to savings groups members during the intervention period. Indeed, OB followed the protocol closely and there is no evidence of program spillovers in our study areas.

Data

Data for the study comes from a variety of sources. Our main results originate from three rounds of household surveys, carried out at baseline (in 2015), midline (in 2018) and endline (2019); since the intervention took place in 2016, these surveys allow us to measure the impacts of the intervention after two and three years. Surveys included primary outcomes of interest: self reported amount of savings and loans, participation status with savings groups, satisfaction with the group; household assets, earnings, and investments. The sample at midline and endline included all those who were interviewed at baseline. At endline, we also interviewed all other current members of the study groups. To create tracking sheets for this exercise, between December 2018 and January 2019 a small team visited all groups and took pictures of the current participant rosters. We then identified those that had not yet been interviewed by their name. New interviewees thus consisted of long-time members (that is, those who were members in 2015 but were not randomly selected for inclusion in the panel sample) and newcomers, who joined the group at some point between 2016 and 2019.

In addition to the interview sample, our analysis incorporates information from a variety of other sources. We received information on group loans offered in the study area from Opportunity Bank; these include issuance and repayment dates plus loan amounts of all loans to study groups for the year 2016 and 2017. In 2019 we also photographed and digitized loan ledger books belonging to most (but not all) of the study groups. The loan groups provide information on internal loans generated, including the issuance date and loan amounts.

Summary statistics Table 14 provide summary statistics and balance tests from the panel sample, comparing the loan group against the control. The top panel reports average respondent characteristics at baseline. Two thirds of group participants are women, and the average years of education is 2.8. As expected from the mostly rural location of the study, approximately 70% of households are engaged in agriculture. Members are financially active within VSLAs: 82% borrowed at least once in the previous cycle. However, as only 3% of households reported having a loan from a formal lender, the sample is not accustomed to working with the formal financial sector. Characteristics are well balanced between the two treatment arms.

The bottom panel of the table reports summary statistics of the variables that appear in the screening tool. Taken together, loan groups are similar control

TABLE 14.
Balanced arms

	Control	Loans	(1) vs. (2) p-value
Members characteristics (baseline)			
Age	38.485	39.103	0.569
Female	0.780	0.760	0.531
Schooling	2.865	2.900	0.755
Married	0.693	0.688	0.876
Size of household	5.130	4.861	0.169
Fraction farming	0.753	0.690	0.171
Fraction business	0.315	0.273	0.187
Food security index	0.642	0.696	0.381
Satisfaction with group index	0.008	-0.009	0.175
Formal loans	0.029	0.029	0.998
VSLA loans	0.822	0.798	0.425
Screening tool components			
Overall score	76.417	75.316	0.581
Group maturity	0.600	0.579	0.819
Savings volume	2.583	2.474	0.431
Attendance	1.467	1.632	0.242
Portfolio at risk	2.167	1.544	0.049**
Writeoffs	1.750	1.684	0.674
Lending frequency	3.517	5.281	0.539
Value of investments	18.717	23.035	0.539
Disciplined group	0.817	0.877	0.369
Good procedures	1.350	1.474	0.246
Group awareness	1.417	1.579	0.132
Good decisionmaking	0.817	0.895	0.235
High quality records	18.100	22.491	0.535
Number of groups	57	60	

Top panel: summary statistics of individual level data from members interviewed in 2015 as part of the baseline. P-values computed after clustering at the level of randomization (village level). Bottom panel: summary statistics of group level characteristics collected during screening visits to groups.

TABLE 15.
Summary statistics: individual loans

Variable	Mean	Std. Dev.
All groups (N=110)		
Total loans per VSLA	137.45	93.74
Loan Amounts (UGX)	UGX 348,827	UGX 542,338
In borrowing group	0.12	0.32
Number of loans per borrower	2.75	2.59

Value of loans issued by groups on a weekly basis. Information from loan ledgers.

groups: key measures of group performance –savings volumes, writeoffs, value of investments–are similar across the two treatment arms. There is one variable that is unbalanced and that is portfolio at risk. To account for any possible imbalance, we will control for all baseline variables in this table in our regressions.

Table 15 reports the summary statistics of the sample of internal loans collected from the group ledgers. We have information on 110 of the 145 groups; on average, each group reported 140 loans over the period under consideration. Loan Amounts indicates the average value of a loan, which is UGX 350,000.

The provision of credit within the savings group

In this section, we describe the take-up of the bank loan by treated groups, and then show the extent to which the additional funds are on-lent to the membership.

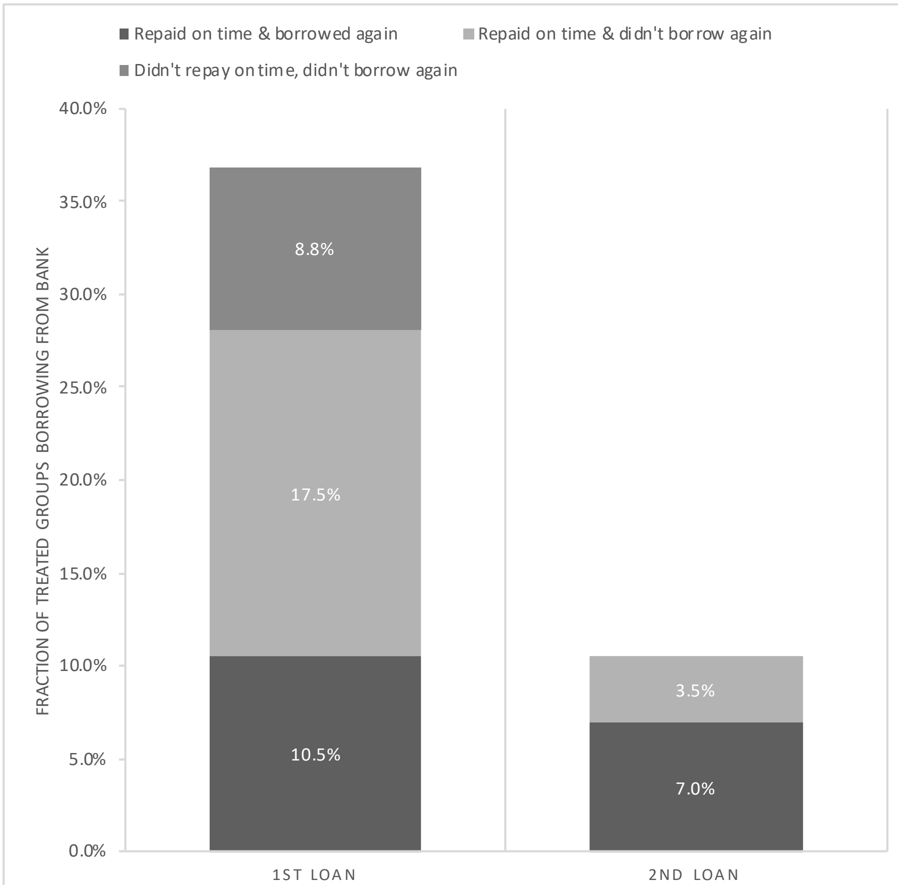
Borrowing from the bank

The intervention led to a significant take-up of the savings product, and a more limited take-up of the loan product. By December 2017, our review of treated groups indicated that over 75% of groups assigned to linkage ended up opening a savings account with the bank. (Comparable figures are not available for control groups, although some take-up was expected). Take-up of the loan product was significantly lower. While over 65% of the groups formally requested a loan from OB, only 36% ended up with one (figure 16). In addition, there was a significant amount of abandonement of the linkage program between the first loan and subsequent loans. As shown in figure 16, all groups that borrowed repaid their initial loan, but 8.8% of treated groups (24% of the groups that took up a loan) ended up repaying late. None of those groups received a subsequent loan. 28% of groups assigned to the treatment borrowed and repaid the loan on time; 37% of these borrowed again from OB, and all of those groups repaid their second loan on time. By the end of our review period, in 2017, only 7% of treated groups had a third loan. Thus, lack of timely repayment appears to be one of several reasons for the lack of repeat borrowing.[3]

Overall, the intervention injected over 100 million UGX (approximately $30,000) in the study areas as loans between 2016 and 2017. All groups received between 1 and 5 million UGX during the first loan cycle. Among those receiving

[3]We have indication that a small subset of groups ended the linkage program and began to borrow from other financial institutions.

FIGURE 16.
Fraction of treatment groups borrowing and repaying on time



Borrowing and repayment patterns by treated groups, separately for first and second OB loans. The first loan is considered repaid on time if repaid within three months (92 days), or within 6 months (185 days) for the second loan. These data cover the period 2016-2017.

the second (third) loan, loan sizes varied from 3 to 5 million (5 to 10 million) UGX.

Impact on internal lending

We next study the extent to which the external loan generated internal loans. To do so, we adopt a leads-lags model of the following form:

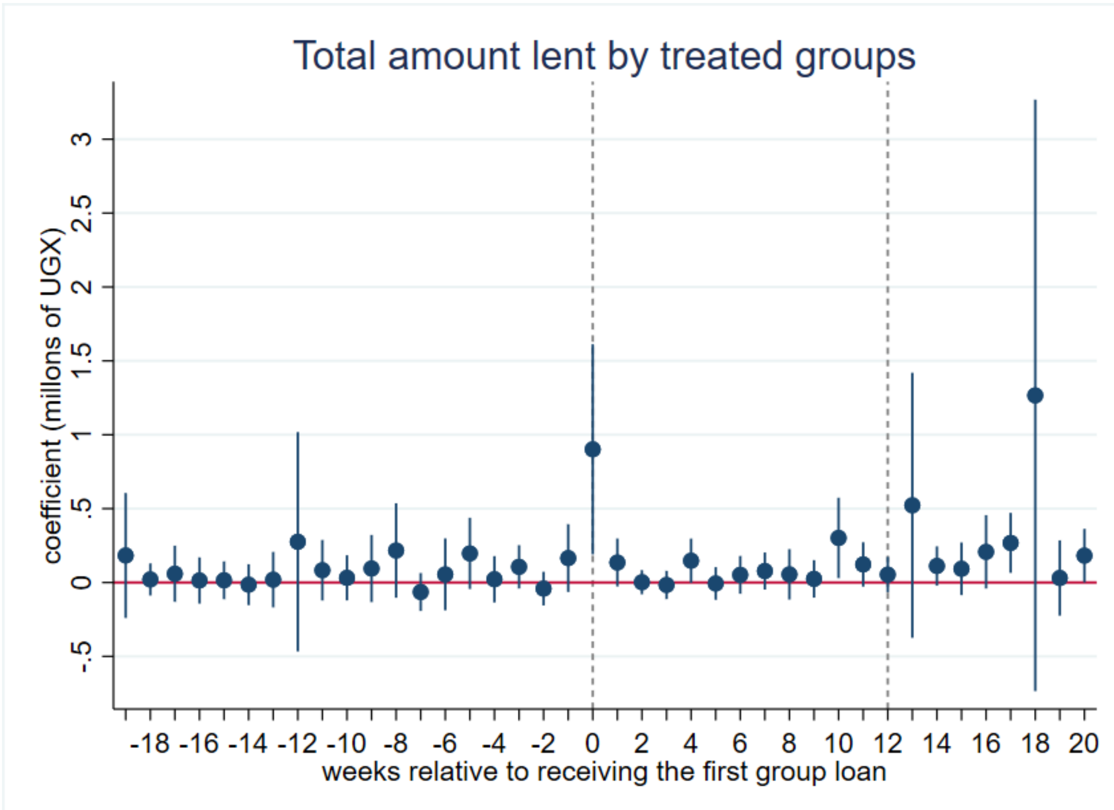
$$LoanAmt_{it} = \sum_{j=-20}^{20} \alpha^j GroupLoan_g \times Week^j_{gt} + \delta_t + \delta_g + \varepsilon_{it}, \tag{4.1}$$

where $LoanAmounts_{gt}$ is the total value of internal loans given out in group g during meeting week t; $GroupLoan_g$ identifies groups that received the loan from Opportunity Bank; $Week^j_{gt}$ is an indicator for week t for group g, which occurred j weeks before or after the provision of the bank loan. The parameters are α^j, which identify deviations of internal lending from the expected amount j weeks before/after the receipt of the bank loan. To control for seasonality and group characteristics, the regression includes VSLA fixed effects and week-year fixed effects; the estimation of parameters α^j arises from the variation in the timing of the receipt of the bank loan. Identification assumes that the timing of receipt is random, and independent of internal loan demand shocks. This is quite reasonable, as the actual delivery of the bank loan depended on when (busy) loan officers gave final approval, and were thus not timed to internal needs. Moreover, if groups did expect the bank loan to arrive, then we should see $\alpha^j \neq 0$ for $j < 0$.

Figure 17 plots the coefficient estimates α^j for the forty week period surrounding the issuance of the bank loan. We can see that the amounts lent increase substantially the week the group receives the funds from the bank. The point estimate is close to one million shillings, which is four times as high as the average amount lent (UGX 230,000) and is 40% of the UGX 2.3 million that linked groups received from the bank. The figure also shows that the amounts do not increase substantially in the periods following the bank loan–the point estimates are all statistically insignificant and close to zero until past the 12th week, which corresponds to when the full bank loan is due for repayment. After the 12th week, point estimates become slightly larger and marginally significant (at the 10 or 15% levels), indicating possibly that the short-term cash infusion led to a slight increase in overall lending once the loan was repaid. Crucially, the periods preceding the bank loans are not characterized by low lending, indicating that, to the extent that there is crowd out between internal and external loans, crowding out is not huge.

Table 16 shows the long-run effects of the bank loan on internal lending. The table regresses $LoanAmt_{gt}$ on three indicators that take the value of 1 for groups that were successfully linked to the bank, for three critical periods following the bank linkage: at the time of the bank loan; during the three-month initial repayment period; and all periods following the repayment period. As before, we control for time factors common to all groups through week-year fixed effects, and account for differences in group characteristics through savings groups fixed effects. The table demonstrates more clearly the dynamics of internal lending. First, lending expands immediately thanks to the bank loan. During the repayment period, the group issues a "normal" amount of loans. Once the bank loan is repaid, on average the group maintains a higher level of lending, which extends beyond the cycle and into future cycles. Overall, linked groups issue between UGX 155,000

FIGURE 17.
Pass through of the bank loan



Coefficient estimates of the interaction between Week j and bank loan indicators from equation (4.1), where $j = 0$ corresponds to the week the group receives the bank loan. Results in UGX.

and UGX 177,000 more per week, which is between 57% and 71% more than the average.

Groups that experienced an increase in overall lending volumes could achieve this by increasing the number of loans given out or by increasing the size of loans. In table 17, we study how individual loan amounts are changed by linkage. We take advantage of the fact that loan records include the name of the borrower to create a person-loan panel. Each observation is an individual loan issued by a savings group, and the dependent variable is the average amount of the loan. The

TABLE 16.
Weekly loan amounts after linkage

	(1) 2016 only	(2) 2016 - 2017	(3) 2016 - 2018
Post ×:			
first week	441,601**	448,776**	447,103**
	(196,427)	(196,531)	(193,827)
repayment period	12,001	18,709	16,832
	(38,307)	(37,354)	(37,658)
post repayment period	155,444**	177,494***	165,562***
	(76,313)	(61,009)	(58,506)
Observations	4,559	9,343	13,425
R-squared	0.143	0.130	0.113
Mean (control)	270168	248665	255455

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

independent variable of interest is *Post*, an indicator variable that identifies loans that were issued after linkage.

Column 1 reports the result of a regression with VSLA, month and year fixed effects. The coefficient estimate is negative, albeit statistically insignificant, indicating that individual loan sizes did not increase with linkage on average. It is however possible that there are heterogeneous effects of the loan: for example, larger loan sizes for existing borrowers, and smaller loans among new borrowers. Because we know the identity of the borrower, we can study this type of heterogeneity. First, in column 2, we control for the members' borrowing history by adding loan number fixed effects. In column 3, we further control for borrower characteristics by including borrower fixed effects. Coefficient estimates do not change much, confirming that there are no borrower selection issues. Finally

TABLE 17.
oans issued per week

VARIABLES	(1) Loan Amount	(2) Loan Amount	(3) Loan Amount	(4) first loans	(5) later loans
Post	-40,392	-39,336	-31,553	-34,697	-38,683
	(46,333)	(47,250)	(43,747)	(89,537)	(53,331)
Observations	14,117	13,459	13,459	4,979	9,138
R-squared	0.112	0.119	0.488	0.107	0.124
VSLA f.e.	Yes	Yes	Yes	Yes	Yes
Year and Month f.e.	Yes	Yes	Yes	Yes	Yes
Loan num. f.e.	No	Yes	Yes	Yes	Yes
Borrower f.e.	No	No	Yes	No	No
Mean (pre)	331264	331264	331264	331264	331264

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

columns 4 and 5 split the sample between first loans and later loans. Coefficient estimates are very similar among both types of loans. Thus, the increase in credit is driven by more frequent lending, and not by changes in loan sizes or borrower characteristics.

Impact of linkage on members

In the previous section, we demonstrated that linkage changed the borrowing patterns within linked savings groups. We next use household level surveys carried out in 2018 and 2019 to study whether exposure to the linkage program impacted living standards of members. In our analysis, we take advantage of the panel feature of our data to use an intent-to-treat methodology that controls for baseline characteristics. For each primary outcome, we run the following regression at the individual i in village v:

$$y_{iv} = \alpha_0 + \alpha_1 Linked\_Loan_v + X_{iv}\beta + \epsilon_{iv}. \tag{4.2}$$

The independent variable of interest is $Loan_v$, which is an indicator for household who participated in groups located in villages v that were assigned to the loan intervention. All specifications include five district fixed effects and X_{ig}, a matrix of group and all household and randomization controls reported in table 14. We also include the full set of employment sector indicators (not reported in the table).

We run regression (4.2) on the midline and endline data separately, using the panel sample. The estimated α_1 will tell us the effect of assignment to a linkage program on those who were targeted by the program.

To account for the fact that outcomes can be correlated at the group and village level, we report standard errors that are clustered at the village level. In addition, we control for the false discovery rate using the methods developed by Benjamini and Hochberg (1995). Since the correction leads to more conservative confidence intervals, we report q-values only for coefficients that are statistically significant without correction. We also report ITT results for the midline (after two years) in panel A of each table, and for the endline (after three years) in panel B. Discussion of panel C is left to section 4.6.

Household savings and credit We begin with the effect of the intervention on savings, credit, and some measure of credit rationing. Table 18 reports results of regression (4.2), where the dependent variable is the amount saved in formal savings accounts and VSLAs (column 1), amount borrowed across available sources icluding VSLAs, banks, MFIs and moneylenders, (column 2), and whether the person reported having had a loan denied by a formal lender (column 3). Due to a coding error in our data collection tool at midline, the questionnaire did not include savings and borrowing from all VSLAs, and thus the total savings and credit amounts are available for the endline only. Point estimates for savings are positive but insignificant, while for loans estimates are also positive (133,000 UGX, or slightly less than $40) (p-value 0.151).

Table 19 disaggregates saving by type (VSLA vs. formal); estimates on savings continue to be statistically insignificant. Table 20 disaggregates credit by lender type. We see that VSLA credit is larger in groups assigned to the loan linkage, by approximately 144,000 UGX (18% of the average borrowed amount in the control group). Estimates from the other sources are statistically insignificant. [4] We also find no evidence that the intervention increased the likelihood of having a savings or loan account (results not shown). Overall, these results clearly show that there are no effects of linkage on external financial utilization. Finally, shareout amounts were somewhat higher in the treated group (table

[4] It should be noted that the proportion of participants obtaining a loan from external sources is very low–only 12% of the control sample did so, and only 2.6% obtained it from a bank. ITT coefficient estimates are likewise very small, and largely statistically insignificant.

TABLE 18.
Financial inclusion

VARIABLES	(1) Total savings	(2) Total loans	(3) Credit Rationing
Panel B: Endline (panel sample)			
ITT: OB loans	21,715	132,981	0.018
	(63,393)	(91,862)	(0.015)
Observations	1,013	1,114	1,114
R-squared	0.123	0.085	0.036
Mean (control)	587439	919579	0.0310
Panel C: Endline (all group members)			
ITT: OB loans	75,384*	129,318*	0.001
	(45,330)	(69,790)	(0.008)
Observations	2,720	3,016	3,016
R-squared	0.036	0.023	0.007
Controls	Yes	Yes	Yes
District f.e.	Yes	Yes	Yes
Mean (control)	618391	1.005e+06	0.0358

Standard errors clustered at the village level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Total savings, total loans are in UGX (exchange rate: approximately 3,000 UGX per USD). Panel A is not presented because Midline does not include targeted VSLA information so the results are not comparable.

21). This is broadly consistent with higher internal fund utilization rate, derived from increased in credit without a change in savings observed in treated groups. However, the estimates fail to achieve statistical significance.

TABLE 19.
Savings amounts by source

	(1)	(2)
VARIABLES	VSLA savings	Formal savings
Panel A: midline (panel sample)		
ITT: OB loans		4,855
		(19,495)
Observations		1,260
R-squared		0.092
Mean (control)		103756
Panel B: endline (panel sample)		
ITT: OB loans	-8,807	-4,956
	(32,494)	(32,810)
Observations	1,114	1,055
R-squared	0.081	0.109
Mean (control)	294181	158286
Panel C: endline (all group members)		
ITT: OB loans	32,953	14,353
	(23,927)	(22,250)
Observations	3,016	2,823
R-squared	0.053	0.026
Controls	Yes	Yes
District f.e.	Yes	Yes
Mean (control)	324594	158633

Standard errors clustered at the village level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 20.
Borrowing amounts by source

VARIABLES	(1) VSLA	(2) Bank	(3) MFI	(4) SACCO	(5) Moneylender
Panel A: midline (panel sample)					
ITT: OB loans		80,241	9,723	-0.034	-0.026
		(71,034)	(23,523)	(0.131)	(0.060)
Observations		1,619	1,620	1,620	1,621
R-squared		0.029	0.009	0.025	0.030
Mean (control)		112485	49145	0.465	0.0687
Panel B: endline (panel sample)					
ITT: OB loans	144,005**	24,736	72,257	0.001	-0.044
	(71,596)	(43,077)	(106,394)	(0.108)	(0.047)
Observations	1,114	1,101	1,096	1,106	1,113
R-squared	0.107	0.039	0.012	0.028	0.023
Mean (control)	768924	41985	72244	0.316	0.102
Panel C: endline (all group members)					
ITT: OB loans	115,690**	-685	28,719	0.167**	-0.051*
	(55,785)	(24,394)	(37,964)	(0.077)	(0.026)
Observations	3,016	2,977	2,968	2,990	3,013
R-squared	0.035	0.009	0.003	0.009	0.008
Controls	Yes	Yes	Yes	Yes	Yes
District f.e.	Yes	Yes	Yes	Yes	Yes
Mean (control)	814632	68758	78819	0.225	0.0660

Standard errors clustered at the village level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

There are few reports of SACCO and Moneylender loans, thus they have been transformed using the inverse hyperbolic sine function. Midline results for column 1 is missing as the midline does not include targeted VSLA information.

TABLE 21.
Shareout amounts

	(1)	(2)
VARIABLES	Total	Average
Panel B: endline (panel sample)		
ITT: OB loans	68,971	56,654
	(87,574)	(41,214)
Observations	1,114	1,114
R-squared	0.103	0.150
Mean (control)	1.028e+06	500687
Panel C: endline (all group members)		
ITT: OB loans	104,586	51,658*
	(64,088)	(30,246)
Observations	3,016	3,016
R-squared	0.052	0.048
Controls	Yes	Yes
District f.e.	Yes	Yes
Mean (control)	1.108e+06	500263

The outcome variable in column 1 is the sum of all shareout amounts received by the household at the end of the cycle of each group. The outcome variable in column 2 is the average shareout, i.e., the total shareout divided by the number of groups the household belongs to. Shareout amounts are computed in Ugandan shillings. Panel A is not presented because Midline does not include targeted VSLA information. Standard errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Impacts on income, investments, and business outcomes We next analyze the impacts on income (table 22), use and amounts of agricultural inputs (table 23), and microenterprise outcomes (table 24). All results are noisy, and lack statistical significance; point estimates are indicative of an increase in income, and a shift away from agricultural investments towards microenterprise. At midline, point estimates for income indicate that members assigned to the treatment had 15% higher income (p-value: 0.19), but these point estimates fall and become even noisier at endline. On agricultural production, (table 23), there is a significant reduction in the likelihood of use of agricultural inputs at endline (column 2), with the effect being driven by nonlabor inputs. However, this result does not survive the FDR correction, and the average amount spent on inputs remains unchanged across the treatment arms. Finally, table 24 measures treatment effects on enterprise development.

It should first be noted that all outcomes in the table are measured with significant noise between one data collection round and the next, possibly indicating a high amount of reporting bias. It is thus perhaps unsurprising that none of the outcomes measured are statistically significant. The likelihood of having an enterprise is 1 p.p higher in the treated group. When looking at those firms with an enterprise, we see positive point estimates for profits (both computed and self reported), costs, and revenues at midline. However, all estimates are very noisy; moreover, the sample size is very small. At endline, point estimates for costs remain as large as the midline, while revenues remain much smaller. Computed profits are thus negative (albeit statistically insignificant). On the other hand, self-reported profits remain positive and are close to significance (p-val 0.13).

TABLE 22.
Household income

	(1)	(2)
VARIABLES	Monthly	Annual
Panel A: midline (panel sample)		
ITT: OB loans	93,517	1,122,204
	(70,490)	(845,875)
Observations	1,621	1,621
R-squared	0.056	0.056
Mean (control)	409083	4.909e+06
Panel B: endline (panel sample)		
ITT: OB loans	58,704	704,449
	(117,895)	(1,414,742)
Observations	1,114	1,114
R-squared	0.117	0.117
Mean (control)	769972	9.240e+06
Panel C: endline (all group members)		
ITT: OB loans	92,429	1,109,143
	(83,654)	(1,003,849)
Observations	3,016	3,016
R-squared	0.028	0.028
Controls	Yes	Yes
District f.e.	Yes	Yes
Mean (control)	828489	9.942e+06

Self reported income from all income sources reported from all members of the household.
Standard errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.

TABLE 23.
Agriculture

VARIABLES	(1) Land cultivated	(2) Any inputs used	(3) Amount inputs used
Panel A: midline (panel sample)			
ITT: OB loans	0.097	-0.028	7,635
	(0.227)	(0.031)	(27,246)
Observations	1,333	1,333	1,333
R-squared	0.001	0.001	0.000
Mean (control)	2.500	0.786	235706
Panel B: endline (panel sample)			
ITT: OB loans	-0.027	-0.064**[a]	-10,527
	(0.229)	(0.030)	(25,874)
Observations	998	1,114	1,079
R-squared	0.029	0.119	0.080
Mean (control)	2.579	0.788	226071
Panel C: endline (all group members)			
ITT: OB loans	-0.067	-0.068**[b]	-7,892
	(0.134)	(0.027)	(14,605)
Observations	2,719	3,016	2,895
R-squared	0.053	0.030	0.015
Controls	Yes	Yes	Yes
District f.e.	Yes	Yes	Yes
Mean (control)	2.381	0.781	202986

Standard errors clustered at the village level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
[a]FDR q−val of 0.111. [b]FDR q−val of 0.039.
Land cultivated and amount inputs used do not include outliers at the top and bottom 1% of the distribution.

TABLE 24.
Enterprise Values

VARIABLES	(1) Has enterp.	(2) Num enterp.	(3) Profits	(4) Self reported profits	(5) Costs	(6) Revenues
Panel A: midline (panel sample)						
Linkage	0.010	0.036	86,237	14,560	2,064,821	2,274,254
	(0.028)	(0.037)	(1,483,479)	(631,126)	(1,919,119)	(2,147,916)
N	1,333	1,333	283	283	283	283
R-squared	0.051	0.058	0.074	0.216	0.187	0.204
Mean (control)	0.197	0.249	689760	2.225e+06	7.010e+06	7.904e+06
Panel B: endline (panel sample)						
Linkage	0.012	0.039	-412,086	1,152,635	2,547,035	1,105,871
	(0.031)	(0.051)	(1,434,876)	(756,837)	(3,162,765)	(2,928,614)
N	1,114	1,114	467	467	467	467
R-squared	0.059	0.066	0.080	0.125	0.135	0.128
Mean (control)	0.426	0.542	1.503e+06	2.595e+06	1.060e+07	1.330e+07
Panel C: endline (all group members)						
Linkage	0.017	0.018	347,990	277,812	1,506,334	1,316,704
	(0.018)	(0.029)	(765,431)	(415,460)	(2,416,706)	(2,188,762)
N	3,016	3,016	1,269	1,269	1,269	1,269
R-squared	0.006	0.009	0.022	0.044	0.035	0.031
Controls	Yes	Yes	Yes	Yes	Yes	Yes
District f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Mean (control)	0.415	0.521	1.261e+06	3.050e+06	1.110e+07	1.340e+07

Regressions on revenues, costs and profits only on respondents with enterprises. Profits are computed as revenues minus costs. Revenues, costs, and profits are winsorized at the top and bottom 1%. Standard errors clustered at the village level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Impacts on food security Table 25 analyzes the effect of the intervention on food insecurity. Food insecurity is measured from five questions, in increasing order of severity. At midline, participants in treated groups report significantly fewer instances of food insecurity, for all issues bar the most severe type. In total, they report 0.25 fewer issues (column 6), i.e., 24% less than the control group. By endline, these differences had shrunk to zero. In particular, the incidence of less severe issues (issues 1 and 2) in the control group do not seem to change much between the two rounds of data collection, while the incidence for the treated group does increase after the first year. The gains from the intervention are short-lived.

Participation and satisfaction with the group We finally analyze the effect of the intervention on members' experiences with the group. First, we analyze whether the interventions caused differential attrition from the group. A priori, the effect of the treatment is ambiguous. On one hand, improved access to safe storage of funds and credit should reduce attrition (at least among borrowers). On the other hand, external credit may reduce savings returns, which is detrimental for savers. More generally, the decision to participate in a linkage program can be controversial, given the low levels of trust in financial institutions by Ugandans. If the program creates more discontent, we would expect to see an increase in attrition.

TABLE 25.
Food Issues of Participants

VARIABLES	(1) Issue 1	(2) Issue 2	(3) Issue 3	(4) Issue 4	(5) Issue 5	(6) Total food issues
Panel A: Midline (panel sample)						
Linkage	-0.079***	-0.063**	-0.057**	-0.036***	-0.010	-0.245***
	(0.021)	(0.029)	(0.026)	(0.011)	(0.008)	(0.069)
FDR q-val	0.001	0.042	0.042	0.003	0.241	
Observations	1,621	1,621	1,621	1,621	1,621	1,621
R-squared	0.077	0.084	0.057	0.038	0.026	0.087
Mean (control)	0.228	0.458	0.246	0.0656	0.0269	1.025
Panel B: Endline (panel sample)						
Linkage	0.036	0.031	-0.028	-0.029*	-0.011	-0.001
	(0.027)	(0.042)	(0.031)	(0.016)	(0.012)	(0.097)
FDR q-val	0.459	0.463	0.459	0.405	0.459	
Observations	1,114	1,114	1,114	1,114	1,114	1,114
R-squared	0.061	0.065	0.046	0.054	0.029	0.063
Mean (control)	0.238	0.470	0.326	0.0832	0.0522	1.170
Panel C: Endline (all group members)						
Linkage	0.024	0.001	-0.010	-0.018	-0.001	-0.003
	(0.020)	(0.027)	(0.024)	(0.011)	(0.006)	(0.072)
Observations	3,016	3,016	3,016	3,016	3,016	3,016
R-squared	0.008	0.017	0.008	0.022	0.011	0.009
Controls	Yes	Yes	Yes	Yes	Yes	Yes
District f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Mean (control)	0.249	0.476	0.319	0.0733	0.0370	1.155

Standard errors clustered at the village level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 26 reports the result of a regression whose dependent variable is whether the member reported not being a participant of the group. The loan treatment is strongly associated with an increase in the likelihood of dropping out at midline: the estimate in column 2 suggests a 8.4 percentage point increase over the control group (19.3%), this represents an increase of 44% over the control mean. The coefficient estimate for the savings only treatment is also positive and large in magnitude, but is not statistically significant. One year later (at endline), more group participants had left both treated and control groups; the proportion leaving was slightly higher in the control group, and the differences between the two are no longer statistically significant. Nonetheless, the 6.2 p.p. difference is 24% of the control mean, which is large. Thus, the intervention changed the composition of the group, but like much else these changes fade over time.[5]

To shed some light on this result, table 27 regresses a "group satisfaction index" variable on our ITT regressions for the midline. On average, study participants associated with the loan intervention report lower (by 0.1 standard deviations) levels of satisfaction relative to control. Importantly, this lower satisfaction comes entirely from the dropouts (column 3), while stayers's satisfaction is unaffected by treatment. While it is not possible to glean the causal chain here, the result is suggestive that the intervention did lead to reductions in overall satisfaction and exit from the group.

[5] It should be noted that this result is not driven by group mortality: *none* of the groups in the treatment dismantled (and only two did in the control group).

TABLE 26.
Participant dropped study savings group

	(1)	(2)
	Dropped from baseline SG	
VARIABLES	Midline	Endline
Linkage	0.084***	0.062
	(0.030)	(0.040)
Observations	1,621	2,040
R-squared	0.037	0.034
Controls	Yes	Yes
District f.e.	Yes	Yes
Mean (control)	0.193	0.268

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

To understand whether this exit of members translates in smaller group membership for treated groups we construct membership levels at the group level. At endline groups exposed to the intervention are on average smaller than control groups. Table 28 reports a reduction of about 4 members in treated group, 19% smaller membership than in control groups. Treatment not only seems to accelerate the process of dropping out (as seen in table 26) but it also decreases the number of new member that the group receives. Table 30 indicates that groups exposed to treatment have on average 3 fewer new members, a 25% reduction with respect to control groups.

Why are treated groups more likely to loose members and less likely to receive new ones? One possible explanation is that being exposed to treatment but not being able to successfully receive a group loan increases friction among members, decreasing satisfaction, and leading some members to leave. To assess

TABLE 27.
Index of satisfaction with group at midline

VARIABLES	(1) All	(2) All	(3) Dropouts	(4) Stayers
Linkage	-0.087*	-0.081	-0.161**	-0.010
	(0.050)	(0.054)	(0.079)	(0.051)
Observations	1,358	1,358	369	989
R-squared	0.007	0.069	0.116	0.049
Controls	No	Yes	Yes	Yes
District f.e.	No	Yes	Yes	Yes
Mean (control)	0.0290	0.0290	-0.201	0.101

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 28.
Number of members of study savings group at endline

VARIABLES	(1)	(2) Membership	(3)	(4)
Treated	-4.31***	-3.87**	-4.73*	-4.86**
	(1.49)	(1.67)	(2.42)	(2.36)
Pr(group loan)			1.46	14.34***
			(3.45)	(4.39)
Treated * Pr(group loan)			1.41	2.51
			(4.32)	(4.57)
Observations	117	117	117	117
R-squared	0.333	0.569	0.339	0.626
Controls	No	Yes	No	Yes
District f.e.	No	Yes	No	Yes
Mean (control)	19.80	19.80	19.80	19.80

*** p<0.01, ** p<0.05, * p<0.1
Notes: Membership is the number os participants that belong to the study VSLA at endline. Pr(group loan) is the predicted probability of success accessing a group loan based on baseline group characteristics picked by lasso lic(ebic) fitted in a probit model. SE are clustered at the village level.

this possibility we used Lasso to predict the probability of a group successfully receiving a group loan using baseline group characteristics. In tables 28-30 Treated

TABLE 29.
Number of participants that dropped from study savings group

VARIABLES	(1) Dropout	(2) Dropout	(3) Dropout	(4) Dropout
Treated	0.56	1.03	1.41	2.16
	(1.03)	(1.06)	(1.43)	(1.39)
Pr(group loan)			0.99	-0.64
			(1.77)	(3.06)
Treated * Pr(group loan)			-2.45	-3.31
			(2.40)	(3.04)
Observations	117	117	117	117
R-squared	0.003	0.395	0.010	0.407
Controls	No	Yes	No	Yes
District f.e.	No	Yes	No	Yes
Mean (control)	7.700	7.700	7.700	7.700

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

Notes: Dropout is the number of participants that belonged to the study VSLA at baseline and dropped out from the group by endline. Pr(group loan) is the predicted probability of success accessing a group loan based on baseline group characteristics picked by lasso lic(ebic) fitted in a probit model. SE are clustered at the village level.

TABLE 30.
Number of participants that joined study savings group

VARIABLES	(1) Joiners	(2) Joiners	(3) Joiners	(5) Joiners
Treated	-3.90***	-3.31**	-3.44*	-2.66
	(1.27)	(1.47)	(2.01)	(2.46)
Pr(group loan)			2.55	13.82**
			(3.47)	(5.41)
Treated * Pr(group loan)			-1.08	-0.66
			(4.17)	(4.76)
Observations	117	117	117	117
R-squared	0.245	0.488	0.253	0.364
Controls	No	Yes	No	Yes
District f.e.	No	Yes	No	Yes
Mean (control)	13.28	13.28	13.28	13.28

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

Notes: Dropout is the number of participants that belonged to the study VSLA at baseline and dropped out from the group by endline. Pr(group loan) is the predicted probability of success accessing a group loan based on baseline group characteristics picked by lasso lic(ebic) fitted in a probit model. SE are clustered at the village level.

* Pr(group loan) indicates how treatment affects group composition for groups with a high probability of receiving a group loan relative to those treated but with a low probability of successfully receiving a group loan. Treatment affects all groups similarly, there are no (significantly) different effects of treatment for groups with high probability of receiving a group loan with respect to those with a low probability of receiving a group while treated.

To better understand the effect of linkage on the average characteristics of group members and what drives turnover, we look in more detail to the individual characteristics of members that dropped out from an study VSLA with respect to those who continued being members from baseline until endline. Table 31 reports demographic and financial characteristics for dropouts and stayers in control groups. On average dropouts have significantly less children and smaller households than stayers.

Column 5 presents the coefficients from a regression where the dependent variable is Dropout =1 and all variables are included as controls. Group members with larger amounts of savings in banks and MM, less amount of savings in VSLAs, that do not have a loan from a VSLA, that have more valuable durable household assets, and have less valuable lifestock, are more likely to leave the group. Interestingly, those with larger amounts borrowed from VSLAs are also more likely to dropout.

TABLE 31.
Dropout analysis

	Dropout (mean, C)	Stayer (mean, C)	(1) vs. (2)	p-value	OLS regression: Dep var is D=1 (C only)	OLS regression: interacted model (T*Var reported)
Demographic						
Female	0.794	0.808	-0.014	0.683	-0.0177	0.0361
Age	43.676	44.400	-0.724	0.539	-0.0017	0.0014
Highest grade completed	2.971	2.910	0.060	0.622	0.0042	-0.0233
Married	0.598	0.641	-0.043	0.306	-0.0565	0.0789
Household size	6.137	6.910	-0.773	0.008	-0.0103	-0.0062
Number of children	3.791	4.318	-0.528	0.025	0.0033	0.0047
Financial (at baseline)						
Farming	0.791	0.776	0.016	0.693	0.0768	-0.0545
Amount saved in VSLAs, banks, MM	2.052	1.930	0.122	0.798	0.0355**	-0.0184
Has savings in VSLA	0.716	0.665	0.051	0.247	0.1098	-0.0043
Amount saved in VSLAs	1.492	1.520	-0.028	0.909	-0.0788**	0.0448
Amount borrowed from bank, MFI and SACCOs	0.226	0.082	0.144	0.314	0.0102	-0.0103
Has loan from VSLA	0.809	0.861	-0.052	0.139	-0.1595**	0.1026
Amount borrowed from all VSLAs	3.899	3.374	0.525	0.237	0.0109**	-0.0138*
Index of assets	-0.030	-0.113	0.083	0.625	0.0130	-0.0306
Value of livestock assets	23.932	170.430	-146.498	0.246	-0.00003***	0.0007***
Value of durable household assets	37.957	16.867	21.090	0.263	0.0001***	0.0011*
Index of housing characteristics	-0.010	-0.042	0.033	0.790	-0.0100	0.0290
Satisfaction index (at baseline)	0.008	0.004	0.004	0.844	0.0083	-0.1079
N	306	245	551	551	559	1,160

Notes: All variables are measured at the individual level. Savings, loans, and assets are measured in hundred of thousands of UGX. Columns 1-4 include individuals in the control groups. Demographic characteristics are measured at endline but are believed to not change with treatment. Financial variables and satisfaction index with group are measured at baseline. Column 3 includes mean differences. Column 4 includes the p-value associated with a joint orthogonality test on dropouts and stayers. Column 5 includes the coefficients for each variable from a regression where the dependent variable is Dropout =1 and all variables are included as control. Column 6 shows the results from regressing Dropout =1 on a treatment indicator, all control variables, and the interaction between treatment and control variables. Each cell includes the coefficient on treatment interacted with that specific control variable. SE are clustered at the village level.

Column 6 in table 31 shows the results from regressing Dropout =1 on a treatment indicator, all control variables, and the interaction between treatment and control variables for a sample that includes all households from the panel dataset. Treatment decreases the likelihood of dropping out of members that have more or larger loans from VSLAs. However, treatment increases the likelihood of dropping out by members that have more assets. Participants with a higher asset index at baseline have larger savings in VSLAs (they have a correlation of 0.22). This suggests a differential effect of treatment according to whether the individual is a net saver or net borrower, retaining net borrowers due to the higher availability of founds and hampering net savers possibly because of reduced returns on their savings.

Table 32 reports the decomposition of savings, loan, and assets at endline according to treatment and whether the participant stayed in the baseline VSLA until endline or dropped out. Dropouts not only have less savings and loans in VSLAs (which is expected) but they are also less financial included in general, they have lower total savings and loans than stayers. The coefficients on the interaction between treatment and the dropout status indicates that the intervention did not affect financial outcomes of dropouts, they are not significantly different than dropouts from control groups.

TABLE 32.
Decomposition of financial variables

	(1) Total savings	(2) Total loans	(3) VSLA savings	(4) VSLA loans	(5) Asset Index
Joiner	14,645	-2,384	-49,097	-27,359	0.027
	(91,936)	(86,490)	(40,099)	(71,815)	(0.101)
Dropout	-298,684***	-319,214***	-310,808***	-468,543***	-0.155
	(85,863)	(98,326)	(48,666)	(78,771)	(0.117)
Loan * Joiner	387,906	-422,963	-10,907	-106,481	-0.149
	(345,931)	(350,066)	(93,547)	(114,365)	(0.176)
Loan * Dropout	-136,678	-433,400	-26,105	-35,146	-0.019
	(181,610)	(339,549)	(97,566)	(124,360)	(0.185)
Loan	112,147	458,546	21,341	133,231	0.136
	(175,111)	(339,874)	(100,477)	(112,700)	(0.179)
Constant	733,007***	1,115,298***	455,980***	979,106***	0.059
	(95,468)	(91,579)	(51,742)	(67,532)	(0.114)
Observations	2,720	3,016	3,016	3,016	3,011
R-squared	0.008	0.008	0.038	0.036	0.002
Mean (control)	655,667	1,024,766	345,238	834,699	0.0288

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

Notes: All variables are measured at the individual level at endline. SE are clustered at the village level.

Impact of linkage on the characteristics of the group

The discussion above indicates that exposure to linkage programs has muted welfare impacts, but does cause an increase in turnover within savings groups. If the new members who replace the leavers have characteristics that are very different from those leavers, they could change the average characteristics of the group. On average, groups that have undergone linkage could thus appear to be different; it would however be incorrect to attribute the difference as a causal effect of linkage of members. The potential for miss-attribution of impacts on linkage is quite possible: anecdotes of groups improving after linkage abound among the savings group community.

To better understand the effect of linkage on the average characteristics of the group, we make use of the full endline sample. As mentioned in the data section, at endline we interviewed all members that were active in 2018, irrespective of whether they joined prior or after the intervention. We then used these data to reconstruct all the (study) groups that a household belonged to at endline, and created a dataset of household-by-group. For each household i belonging to group g, we run the following regressions:

$$y_{ig} = \alpha_0 + \alpha_1 Linked\_Loan_g + X_{ig}\beta + \epsilon_{ig}. \quad (4.3)$$

Note that equation (4.3) differs from (4.2) in a number of ways. First, the regression above allows for multiple observations for each household, if households belong to multiple groups. Second, the assignment to the treatment, *Linked_Loan*, is defined over the group that household i belongs to, and not her village. For members that were present at baseline, we thus ignore their *initial* assignment, and drop baseline observations that are no longer in a study savings group in 2018. The estimated coefficient α_1 thus indicates the difference in outcome y between groups assigned to the linkage and control. The difference is a weighted sum of two factors: the impact of linkage on stayers, and of the difference in the characteristics of newcomers. Given that the first factor is estimated to be close to zero for most outcomes, the coefficient estimate thus indicates the effect of selection.

We revisit all outcomes reported in section 4.5. For simplicity, estimates for equation (4.3) are reported in panel C of each table presented in the previous section.

The results indicate that linkage does make groups appear better off–due to the selection effects. Members of linked groups have higher savings and total loan amounts (table 18), and gain from higher shareout amounts (table 21). On the other hand, it terms of measured outcomes, coefficient estimates do not appear to be significantly larger in treated groups relative to control. Income is higher (table 22) and standard errors are somewhat lower although results remain insignificant;

the patterns for agricultural production is also similar to the endline panel sample. On enterprise, coefficients for self reported and imputed profits are both positive and relatively small. Rates of food insecurity are also indistinguishable between treated and control groups.

Conclusion

In this study, we seek to better understand the impact of credit delivered through savings groups. Our randomized control trial enhances financial intermediation by introducing two formal banking products–a savings account and a loan account– to existing savings groups in five districts in Uganda. he main question we are interested in addressing is whether savings groups participants benefit from an enhanced access to bank credit. The potential expansion of credit operates through a very specific *credit rationing* channel: the bank provides additional funds to the group, and the group uses those funds to provide credit to members. After two years, we find that most (75%) treated groups opened the account with the banking institution. Take-up of loans was considerably lower: only one third of groups were able to successfully receive a loan from the bank. Despite this, we observe a large increase of lending to members coinciding with the bank loans, suggesting that the loan did generate new borrowing opportunities. Our (preliminary and noisy) estimates suggest an increase of 13% in self-reported income, and 17% increase in savings. We find some limited spillover effects on personal use of loans from SACCOs, but these are limited to the savings only intervention arm. Finally, we find no effects on agricultural investments (we have yet to analyze impacts on enterprise). We also find that groups exposed to the treatment suffered from higher rates of member dropout.

CHAPTER V

CONCLUSION

My dissertation intersects the fields of labor, development and gender economics while utilizing quasi-experimental and experimental techniques to causally identify the effect of programs and public policy in the areas of paid family leave (chapters II and III) and financial inclusion (chapter IV).

With only a few exceptions, countries around the world provide paid family leave to increase female labor force participation, improve gender equality, and foster family wellbeing. Yet, a large percentage of women do not return to work after maternity leave. Can a policy that allows flexible reintroduction to work increase labor force participation after childbirth? To answer this question, in chapter II I study a unique subsidy implemented in Uruguay in 2014 that allows mothers to work half-time while receiving full-time pay for four months after maternity leave ends. I use eleven years of employment survey data in a difference-in-differences and triple difference framework and find an increase in the likelihood of employment of 17% for eligible mothers up to 1 year after childbirth, and a decrease in hours worked of 5% without a significant reduction in income up to 3 years after childbirth. These findings demonstrate that policies easing the transition back from maternity leave can increase female labor force participation and reduce “child-penalties” in the short and medium-run.

In chapter III, I leverage the quasi-experiment provided by the implementation of PPL in Ecuador in 2009 to identify the effect that it has on paternal involvement in childrearing, housework, and labor market outcomes. I use data from the Ecuadorian National Employment Survey (2007-2013) to employ a generalized difference-in-differences design that compares fathers of children born after 2009, to fathers of children born before 2009, who are employed in the formal sector (treated), versus informal sector (not treated). I estimate that fathers eligible for PPL increase time with their children by 24 minutes per week on average without any changes in housework. This is an increase of 20 percent from an average of almost 2 hours per week before the implementation of PPL. These are sizable results, considering the short duration of the leave, driven by changes in behaviors of those that receive PPL for their first-born child. PPL does not affect fathers' employment or hours work, suggesting that they substitute leisure for time spend taking care of their children as a result of the reform.

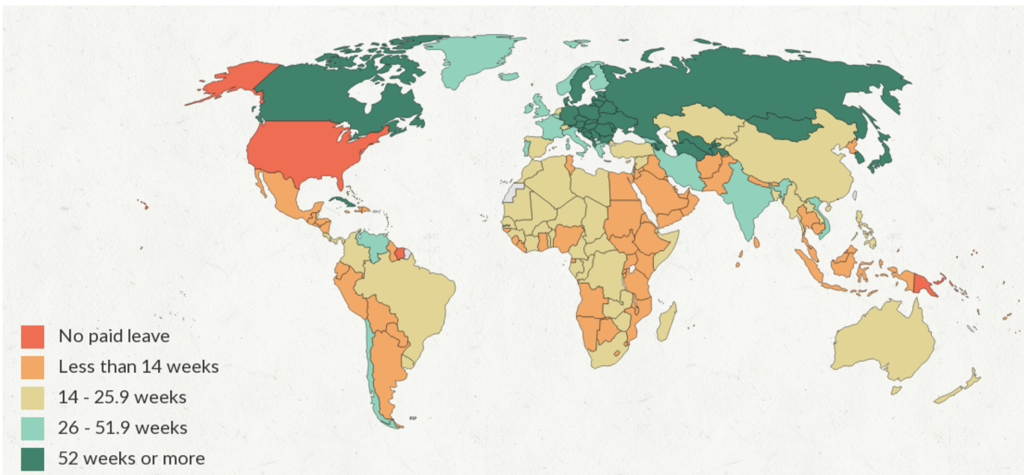
In chapter IV, my coauthors and I study a lending model with the potential to bridge the gap between formal and informal finance in the context of rural communities in Uganda. In this delegated lending model, better known as linkage, a formal financial institution lends to savings groups and lets the group decide the allocation of borrowed funds. In our RCT, a random sample of existing savings groups gained facilitated access to linkage loans from a commercial bank in Uganda. We show that the bank loan stimulated an immediate and sizable increase in internal lending, which is sustained over time. We find that members of treated groups had temporarily lower rates of food insecurity after two years, and point estimates suggest sizable increases in income and microenterprise size (which are not statistically significant). However, groups assigned to treatment also experienced significantly more turnover, suggesting that the possibility of external financing generates powerful selection effects.

All together these chapters provide insights on specific policies that can contribute to the empowerment and flourishing of women and their families by increasing their access to financial products, promoting the reconciliation of work and family, and fostering egalitarian values of household responsibilities among partners.

APPENDIX

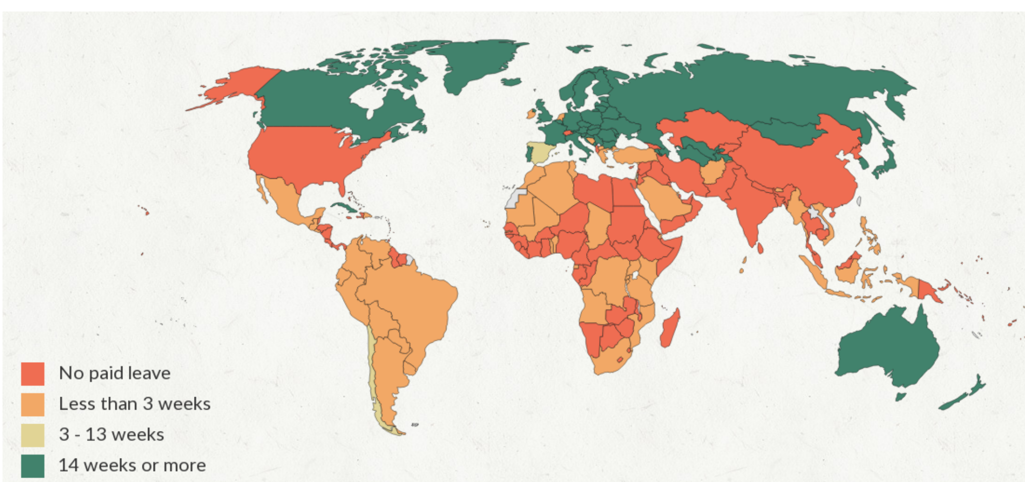
MATERNITY AND PATERNITY LEAVE PROVISIONS ACROSS THE WORD

FIGURE A.1.
Maternity leave regulations across countries



Source: 2020 WORLD Policy Analysis Center

FIGURE A.2.
Paternity leave regulations across countries



Source: 2020 WORLD Policy Analysis Center

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