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ESSAYS IN LABOR AND EDUCATION ECONOMICS

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FACULTÉ DES HAUTES ÉTUDES COMMERCIALES

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pour l'obtention du grade de
Docteure ès Sciences Économiques,
mention « Économie politique »

par

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Jury

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Introduction

This thesis consists of three independent essays focusing on the fields of labor economics and the economics of education. These essays study the factors and policies that impact how people make education and labor supply decisions. The findings inform public policy related to family policies and social insurance to ensure more equitable outcomes for vulnerable and disadvantaged groups.

Low-income households in both developed and developing countries experience several barriers to education. One characteristic of such households, besides their low income levels, is that they also face uncertain and volatile income streams. How uncertainty over future income, and hence income risk, affects investments into education at all levels have not been well studied. In the first chapter, "***Human Capital Investments Under Uncertainty: Income Risk and Children's Education***", I discover that income risk has a large and negative ex-ante impact on educational attainments among poor, rural households in India dependent on income from rainfed agriculture. Specifically, high income risk decreases the number of completed years of schooling, increases the share of people with only primary level education, and decreases the share of people with secondary education or more. In addition, it increases the likelihood that girls' education gets interrupted leading them to fall behind, and decreases the amount that households spend on girls' education. Since it is difficult to disentangle the ex-ante effects of income risk from the ex-post effects of past income shocks, I adopt a novel design using rainfall volatility and rainfall shocks as proxies for income risk and income shocks respectively, and in addition, exploit the additional variation introduced by irrigation dam construction. With climate change causing increased volatility in agricultural incomes, these findings indicate an important and urgent role for public policies that help protect households from the consequences of severe negative income shocks, such as weather, crop and health insurance.

The second and third chapters relate to family policies, female labor supply, fertility, and gender gaps. Despite parity being achieved on education in most developed countries, women continue to work and earn significantly less than men. Recent work finds that these differences emerge upon family formation, indicating that family responsibilities pose a barrier to women's full labor market participation. Therefore, family policies that enable women to better balance work and family life could be one solution.

In the second chapter, "***Mothers at Work: How Mandating a Short Maternity Leave Affects Work and Fertility***", using administrative data, my co-authors and I find that introducing a new national mandate providing paid maternity leave only had small and temporary effects on the labor market outcomes of first-time mothers. But it did raise the share of women having a second child by three percentage points. These results are mainly driven by women working in firms that were already providing some paid maternity leave. This suggests that the cost-savings introduced by the mandate allowed these firms to implement other policies that then enabled their female employees to better balance the demands of work and family and achieve their desired fertility.

In the third and final chapter, "***Paid Maternity Leave Mandate, Labor Market Behavior and Child Penalty: The Case of Switzerland***", I explore the impact of this new mandate further, this time using labor force survey data and allowing for selection both into motherhood and the labor market. I find that the mandate had an overall negative impact on first-time mothers, decreasing employment and earnings, especially among those with an older first child and who likely benefitted from the mandate again for subsequent children. Using a triple difference model, I find that the mandate increased slightly the gender gap in total employment but decreased substantially the gap in earnings and wages among those with an older child. This implies that women were able to continue working at a higher intensity after starting their families and were then able to narrow the gaps in earnings and wages. Therefore, even in environments with high female and maternal labor force participation rates, and privately provided benefits, family policy mandates could have important effects on labor market outcomes, family size, social norms, and social equity.

Chapter One

Human Capital Investments Under Uncertainty: Income Risk and Education[1]

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Abstract

Uncertainty about future income potentially affects current decisions that risk-averse poor households with credit and insurance constraints make related to human capital investments. I find that income risk has a large and negative ex-ante impact on educational attainments among poor, rural households in India dependent on income from rainfed agriculture. Specifically, high income risk decreases the number of completed years of schooling, increases the share of people with only primary level education, and decreases the share of those with secondary education or more. In addition, it increases the likelihood that girls' education gets interrupted leading them to fall behind, and decreases the amount that households spend on girls' education. Since it is difficult to disentangle the ex-ante effects of income risk from the ex-post effects of past income shocks, I adopt a novel design using rainfall volatility and rainfall shocks as proxies for income risk and income shocks respectively, and in addition, exploit the additional variation introduced by irrigation dam construction. These findings suggest an important role for public policies that help protect households from the potential consequences of severe negative income shocks, such as weather, crop and health insurance.

Keywords: income uncertainty, education, human capital, rainfall variability
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I. Introduction

Despite substantial progress over past decades, worldwide 60 million children of primary school age remain out of school in 2021 (constituting eight percent of the primary school age population).[2] In India alone, over six million children were not in school in 2020.[3] These children come from households that are not only poor but also face highly uncertain income, since they are particularly vulnerable to various income shocks, whether due to adverse weather events, crop failures, job losses, illnesses and deaths, or civil conflicts (Dercon, 2002 and 2004). In addition, they face severe credit and insurance constraints that limit their ability to cope with such shocks ex-post. While many of these households are able to smooth consumption somewhat through variance mechanisms[4], it is not complete, and they still face large unmitigated risks as shown by tests of perfect insurance (Townsend, 1994; and Morduch, 1995). Especially, repeated household specific adverse events and covariate risks overwhelm the ability of households to cope (Dercon et al, 2004). Therefore, the risk mitigation strategies and consumption smoothing channels adopted by poor households ex-ante could be economically costly, leading to poverty traps, and perpetuating poverty over generations, particularly when they affect human capital accumulation.[5]

However, there has not been much recent research into the role that income risk could play ex-ante on economic decision-making related to productive activities,[6] and in particular, human capital accumulation. In this paper, I study the direct ex-ante effects of income risk on household investments into children's education. Households dependent on agricultural income choose economic activities and make productive investments that respond

[2] 'Access to Basic Education: Almost 60 Million Children in Primary School Age Are Not in School'. n.d. Our World in Data. Accessed 18 October 2022. https://ourworldindata.org/children-not-in-school.
[3] UNESCO Institute for Statistics (http://uis.unesco.org/). Data as of June 2022.
[4] Such as through building up assets and buffer stocks (Rosenzweig and Wolpin, 1993; Fafchamps, Udry and Czukas, 1998; Kazianga and Udry, 2006; and Carter and Lybbert, 2012), savings (Paxson, 1992), as well as through various informal private risk sharing mechanisms, for example, through migrating for marriage (Rosenzweig and Stark, 1989).
[5] See, for example, Morduch (1995), Dercon et al (2004), Chetty and Looney (2006), Elbers et al (2007), Oviedo and Moroz (2013), and Carter and Lybbert (2012).
[6] Rosenzweig and Binswanger (1993) and Rosenzweig and Wolpin (1993) show that risk-averse poor households make agricultural investments that yield lower returns but are more likely to provide smoother consumption. Bandyopadhyay and Skoufias (2013) find that rural households in Bangladesh diversified income-generating activities ex-ante in response to rainfall variability and that this led to lower levels of consumption in some cases. The fact that poor households participate in many different economic activities that each produce low returns and the lack of specialization is also highlighted by Banerjee and Duflo (2012).

differentially to weather variability according to their risk preferences and their capacity to cope with income shocks ex-post. They then make decisions on how much to invest in their children's education. Various features of education could make it particularly sensitive to income risk, especially features such as delayed and increasing returns to educational investments, dynamic complementarities, large and lumpy cost of schooling, among others. In addition, children are considered a form of "buffer stock", and households expect to take children out of school when income shocks occur (Cain, 1982; Jacoby and Skoufias, 1997; and Pörtner, 2001). Given beliefs about future income shocks and capacity to cope with shocks ex-post, households could choose three levels of education for their children: none (not enrolling in school at all), primary only (enrolling in primary school and investing enough to attain at least a few years of primary education), or at least some secondary (investing sufficiently in primary education in order to progress on to and enroll in secondary school). Therefore, for poor households with limited capacity to deal with shocks ex-post, high income risk ex-ante limits human capital accumulation and leads to lower educational attainments.

Since it is difficult to disentangle the ex-ante effects of income risk from any persistent ex-post effects of past income shocks, I adopt a novel design using rainfall volatility and rainfall shocks as proxies for income risk and income shocks respectively, and in addition, I exploit the additional exogenous variation introduced by irrigation dam construction. Since the relationship between rainfall and agricultural output and incomes in India has been well established,[7] using rainfall volatility as a direct proxy for income risk is a reasonable move. The constructed income risk variable captures the substantial variation in rainfall across seasons, across years, and across the country, which in turn affects agricultural productivity and incomes. In addition, I include different measures of past rainfall shocks, both positive and negative, in order to dis-entangle the ex-ante impact of income risk from any ex-post effects of past shocks. Furthermore, by including the variation in irrigation dam construction, which de-couples the link somewhat between rainfall volatility and income risk (Duflo and Pande, 2007), the relationship between ex-ante income risk and educational attainments could be more credibly identified. My identification strategy follows Duflo (2001) where similarly, I use year of birth to designate 'cohorts' born before and after dam construction. I estimate separately by cohort, age (adult or school-age children), and gender using the Rural Economic and

[7] By, for example, Evenson and McKinsey (1999), Rose (2001), Jayachandran (2006), Duflo and Pande (2007), and Kaur (2019).

Demographic Survey (REDS), a large dataset that is representative of rural households across India, and which contains extensive measures capturing various dimensions of educational investments and attainments.

I find that higher levels of income risk led to reduced educational attainments for both adults and children. For adults, it leads to lower completed years of schooling (for both men and women), a higher share of those with only primary education (mostly men), and a lower share of those with at least some secondary education, which is also mainly driven by the effect on men. In real terms, those living in places with high income risk (90th percentile) have 2.9 fewer years of schooling as compared to those living in places with low income risk (10^{th} percentile) and they are 25 percentage points less likely to enroll in secondary school. I find that these effects are strongly driven by the impact on men. Men from places with high income risk have on average 6.5 fewer years of schooling than those from places with low income risk, while their likelihood of having only primary education is 55 percentage points higher, and their likelihood to have some secondary education, 71 percentage points lower. Since women in my sample have extremely low levels of educational attainments in general, this suggests that higher income risk has a greater impact on boys than for girls. However, for school-age children in my sample, I find the reverse. High income risk overall reduces the amount households spend on children's education, especially for girls who are also less likely to be on track at school due to more frequent schooling interruptions. These findings suggest an important role for public policies that help protect households from the consequences of severe negative income shocks, such as weather, crop and health insurance.

A large strand of literature has studied the ex-post effects of various types of realized income shocks on children's human capital: their cognitive and non-cognitive skills, education and incidence of child labor,[8] as well as on health.[9] But only four papers have looked at the possible

[8] Jacoby and Skoufias, 1997; Jensen, 2000; Ranjan, 2001; Sawada, 2003; Thomas et al, 2004; Beegle et al, 2006; Duryea, Lam and Levison (2007), Gubert and Robilliard (2008), Maccini and Yang, 2009; Björkman-Nyqvist, 2013; Alam, 2015; Fabre and Pallage, 2015; Bandara, Dehejia, and Lavie-Rouse, 2015; Landmann and Frolich, 2015; Mottaleb, Mohanty and Mishra, 2015; Randell and Gray, 2016; Shah and Steinberg (2017); Bau et al (2021), among others, have extensively studied the ex-post impacts of early life income shocks on children's education and child labor incidence.

[9] Jensen (2000) finds that children are more likely to be malnourished and less likely to visit a healthcare worker if they happen to fall ill when there is an income shock. Similarly, Maccini and Yang (2009) find that those who had an income shock in their first year of birth were less likely to enjoy good health as an adult. Berman et al (2020) find positive shocks in world crop prices, especially those that occurred

ex-ante effects of income risk on education. Fitzsimons (2007), the first to do so, finds that aggregate village level income risk (instrumented by historical rainfall volatility) has an ex-ante impact on reducing total years of completed schooling amongst children aged 10-15 years old in rural Indonesia. She estimates that the difference between those facing risk at the 90th percentile to those facing risk at the 10th percentile is 1.3 fewer years of schooling. Kazianga (2012) studies the ex-ante impact of income risk on children's education from rural households in Burkina Faso and also finds a strong negative impact. Using historical rainfall variation to predict variations in agricultural income, he finds that high income volatility decreased total years of completed schooling and household educational expenditures and increased the likelihood of never having been enrolled in school for children aged between seven and 15 years old. Foster and Gehrke (2020) study how household schooling investments respond to ex-ante income risk in the presence of dynamic complementarity in the human capital production function.[10] They look at the time rural children aged between six and 15 years old spend in school or studying during the three agricultural seasons of the year in India and find an elasticity of -0.05 to -0.04 between study time and their household income risk variable, which is based on the variability of household consumption resulting from rainfall variability interacted with access to irrigation. Finally, Colmer (2021) examines the effects of income uncertainty, also using rainfall volatility as a proxy, on human capital investments among children in rural Ethiopia. However, contrary to previous findings, he discovers that increased rainfall variability is associated with less child labor and more schooling, in line with the diversification mechanism of portfolio theory.

I contribute to this nascent literature by using a novel model exploiting the timing in the construction of irrigation dams that reduced the volatility of agricultural income and hence, income risk for some in the villages that I study. Therefore, I am able to exploit both across village and within village variation. The proxy that I use for income risk more plausibly captures unobserved income risk for the households in the sample, while I include many different variables to control for the effects of any realized income shocks. The dataset used is large, covering over 8,000 households in 233 villages across India, and contains measures that capture many different dimensions of human capital investments, both at the extensive and

while in-utero and in the first year of life, improved child health indicators, and increased parental health investments up to five years after.

[10] Cunha and Heckman (2007) first proposed the existence of dynamic complementarity in the human capital production function, where early parental investments into a child's human capital lead to greater returns on latter investments.

intensive margins. Finally, I explore the ex-ante impact of income risk on educational attainments among adults, and not only school-age children, which has previously not been covered in the literature.

The rest of the paper is structured as follows. In Section II, I provide the Indian institutional background and explain why educational investments could be particularly sensitive to ex-ante income risk. Section III describes the data sources and presents some descriptive evidence. Section IV outlines the empirical strategy. Results are discussed in Section V while Section VI covers potential mechanisms. In Section VII., I address some threats to identification and conduct sensitivity analyses. Section VII concludes.

II. Background

A. Education in India

In India, K-12 education is separated into pre-primary (early childhood), primary, secondary or lower secondary, and higher secondary education. Pre-primary education consists of pre-school or nursery for children younger than four years old, and two years of kindergarten (termed LKG and UKG) for children aged between four and six years old. About 61 percent of children aged three to five years old were enrolled in pre-primary education in 2020.[11]

Primary education starts between the ages of five to six years and lasts for eight years. It is further split into lower primary (grades one to four for children aged between six to 10 years old) and upper primary (grades five to eight for children aged from 10 to 14 years old, which is known as middle school in other countries). Secondary education is for children aged between 15 to 16 years old and at the end of which students sit for an official centrally administered board exam in order to progress on to higher education. Higher secondary education is another two years for children aged between 17 to 18 years old, and there is another board exam at the end, which is often required for entry into higher education (although, many colleges and universities require in addition passing their own admissions tests for entry).

The academic year starts in some states in April and in other states in June. Most children aged between six to 14 years old (over 70 percent) attend public schools run by the central, state or local government.[12] However, a significant proportion of students in urban areas attend privately run schools. There are no official school fees charged by public schools, although families have to bear the costs of books, materials, uniforms and transport.

Education is legally compulsory for children aged from six to 14 years old. However, about 6.1 million children are out of school in 2014 and 29 percent drop out of school before completing primary education.[13] Half of primary school-going children, nearly 50 million children, are not achieving grade appropriate learning levels.[14] While about 50 percent of adolescents do not complete secondary education.[15] Girls and children from the lower castes

[11] The World Bank, World Development Indicators.
[12] According to the latest Annual Status of Education Report (ASER 2021).
[13] National Sample Survey of Estimation of Out-of-School Children in the Age 6-13 in India (2014).
[14] The National Achievement Survey (NAS) – State Reports (2017).
[15] Rapid Survey On Children (RSOC 2013-14).

in particular face substantial barriers to education. The literacy rate for adults aged 15 or older was 74 percent in 2018.[16] For women, it was 65 percent, while for men it was 82 percent. These figures indicate that there is gross under-investment in children's human capital in India and that this is particularly problematic for girls.

Public schools also offer free lunches as part of the Midday Meal Scheme that was launched nationwide in 2001 (though some territories and states had started similar programs earlier such as Tamil Nadu in 1956) in order to combat malnutrition among children (over 34 percent of children under age five are stunted). About 120 million children benefit from this scheme.

B. Income Risk and Education

There are a number of features of education that could make it particularly sensitive to income risk ex-ante and make households' decisions to invest in children's education depend strongly on their expectations of their own future income.

First, the choice of consumption smoothing channels adopted ex-ante could lead to reductions in human capital investments, especially in education (Chetty and Looney, 2005 and 2006; and Oviedo and Moroz, 2013). Poor households sometimes consider children as a form of "buffer stock" or insurance against negative income shocks (Cain, 1982; Jacoby and Skoufias, 1997; and Pörtner, 2001), and expect to rely on child labor to supply market wages or home production goods when the household experiences a negative income shock.

Second, investments into education have non-linear returns. Cunha and Heckman (2007) show that education has dynamic complementarities, where early parental investments in child human capital lead to greater returns for latter investments.[17] Following on work by Cunha and Heckman (2007) and Cunha, Heckman and Schennach (2010), Foster and Gehrke (2020) develop a model that includes an education production function with dynamic complementarity, where the marginal utility of investment in education in the second period increases with the level of investment in education in the first period. They show, using data from India, that when households anticipate that they would be unable to continue investments into education in the second period, they would then reduce investments in the current period.

[16] The World Bank, World Development Indicators.

[17] Thomas et al (2004) find that poor households apparently sought to protect investments in the schooling of older children at the expense of the education of younger children.

Therefore, if households expect that it is unlikely that they would be able to continue their investments over the entire period of primary or secondary education until completion, they may be then less likely to enroll their children at all or invest sufficiently on the intensive margin to ensure that their children would be able to complete at least primary education and progress on to secondary education.

Third, there is evidence to suggest that returns to schooling in the agricultural sector and informal sector are low whereas low levels of schooling also produce low returns in the formal sector. Given the low quality of primary education,[18] attainment in the board exams at the end of secondary education could serve as a signal for quality of human capital and productivity. Therefore, investments in education only start to pay off at higher levels of schooling and households only benefit from their children's education when they have completed at least secondary education.[19] This non-linearity in returns to education makes it a very long-term as well as an irreversible investment that increases its risk-return ratio. Kazianga (2012) notes that risk-averse households facing frequent negative income shocks and, hence high uninsurable risk, would prefer to pursue economic activities that can generate immediate income rather than make irreversible investments with a highly uncertain return. Education could be considered as such an irreversible investment. In addition, the precautionary savings motive could cause prudent households to hold more liquid assets, which could in turn also hamper their abilities to invest more in education. Therefore, when households have limited access to formal credit and insurance markets, and are on the margin of subsistence consumption, investments into children's education are very likely to vary directly with expected income risk.

In addition, low-income households poorly perceive the actual returns to education and such expectations could also play an important role in reducing educational attainments even further. Attanasio and Kaufmann (2009) show that youths' and parents' expectations of the returns to

[18] As found by The National Achievement Survey (NAS) – State Reports (2017), the various Annual Status of Education Reports (ASER), etc. Two states in India (Tamil Nadu and Himachal Pradesh) took part in the Programme for International Student Assessment (PISA), a study conducted by the Organisation for Economic Co-operation and Development (OECD) to evaluate educational systems by measuring 15-year-old school students' scholastic performance on mathematics, science, and reading, in 2009. Both states ranked at the bottom of the table, only above Kyrgyzstan.

[19] I make no assumptions regarding parental motives for investing in their children's human capital. Whether fully or partially motivated by altruism, or by hopes of capturing some returns through future transfers, should have no bearing in this context.

schooling have an effect on high school attendance in Mexico. Similarly, Jensen (2010) finds that students in the Dominican Republic are poorly informed about the returns to education and that providing more accurate information led to students completing 0.2 more years of school on average. Jensen (2010) also finds that providing information on potential jobs to young women and girls in India increased their likelihood of being in school and increased their BMI.

Furthermore, the cost of schooling could be another factor for poor households. It could be large as a share of household income, or it could be "lumpy", such that households need to make a high upfront investment in order to enroll children in primary or secondary school, as well as being timely. This "cost" could be actual costs of education such as enrollment and tuition fees, books, uniforms, etc. Jensen (2000) finds that for Cote d'Ivoire, education costs can be considerable, as much as one-third of the median household income per capita. While this may not be true for countries such as India, where public schooling is free, nevertheless, there could be associated costs such as expenditure on uniforms and books (Banerjee, 2004). Kazianga (2012) finds that for households in Burkina Faso, where public education is also free, households nevertheless spend a significant percentage of their annual income on education for each child. And such costs are usually timely, where failing to meet these costs on time could lead to children being forced to drop out or never enrolling. Edmonds and Shrestha (2014) find that most schooling expenses occur at the beginning of the school year and providing scholarships that cover these costs boosts enrollment. Since costs are large, lumpy and timely, liquidity and credit constrained households have to anticipate and plan for such costs. Households also have to account for foregone expected wages from children working in the labor market or foregone expected home production, which would be greater for older children. Shah and Steinberg (2019) find that the opportunity cost of schooling is an important determinant of education. Therefore, when households have an a priori belief about the risk distribution and anticipate frequent negative income shocks, then when they decide on their mix of economic activities and investments to potentially smooth consumption, such large, upfront and timely costs could then amplify the effect of income volatility on education.

Finally, Banerjee and Duflo (2012) clearly highlight the magnitudes of risks that poor people face, and the often severe negative, even life-or-death, consequences that income shocks or poor economic decisions could lead to. In addition, both scarcity (Mullainathan and Shafir, 2013) and precarity (Azmanova and Auerback, 2021) lead to increased difficulties in making long-term, expensive decisions with uncertain returns and large downsides for poor people living on the margins of subsistence consumption. In this context, households' perceptions of income risk ex-ante could play an important part in determining the educational attainments of children.

However, while all the factors outlined above suggest a negative relationship between income risk and educational attainments, another potential mechanism could lead to an opposite effect. If agricultural income is too volatile, then households may choose to diversify away into other economic activities, which may require higher levels of human capital. In this case, income risk would have a positive ex-ante impact. Indeed Colmer (2021) finds a positive association between increased income volatility and higher schooling, both at the intensive and extensive margins, for children in rural Ethiopia, which he attributes to the diversification mechanism of portfolio theory. Therefore, the final effect of income risk on education would be ambiguous.

C. Dams in India

According to Duflo and Pande (2007), India has over 4,000 large dams, which was the predominant form of public investment in irrigation. It was the largest beneficiary of the World Bank lending for irrigation between 1950 and 1993. The main objective of such extensive dam construction was to facilitate agricultural growth and reduce rural poverty. Over 90 percent of large dams was for irrigation. The typical Indian irrigation dam is an earth dam where water is impounded to create a reservoir behind an artificial wall built across a river valley. Artificial canals are constructed to channel water from the reservoir to downstream regions for irrigation. Such irrigation dams reduce the dependence of agricultural productivity on rainfall. Evenson and McKinsey (1999) find that irrigation reduces the volatility of agricultural production by mitigating the effects of rainfall and temperature shocks. Their findings are echoed by Duflo and Pande (2007), who show that negative rainfall shocks have a smaller impact on district agricultural output after the construction of a dam in the upstream district.

III. Data and Descriptive Evidence

A. Data

The primary data source that I draw on is the Rural Economic and Demographic Survey (REDS) from India. This is a nationally representative rural sample of Indian households collected by the National Council of Applied Economic Research (NCAER). The REDS is the follow-up survey of the Additional Rural Incomes Survey (ARIS), which was carried out between 1969 and 1971. There are three waves of the REDS data, which were collected in 1981-1982, 1998-1999, and 2007-2008. In this paper, I only use data from the latest round, REDS 2006, which was conducted over 2007 and 2008. The latest wave covers about 8,500 households in 241 villages across 17 major states in India. REDS contains detailed economic and demographic information for both households and villages. Therefore, the survey represents households affected by very different agro-climatic conditions, and who are largely dependent on agriculture for income and consumption. Most importantly, it includes various education related variables that allows me to capture all forms of investments into education such as current enrollment of children, enrollment ages for both adults and children, highest class completed, completed years of schooling, etc.

For data on rainfall, I draw on the University of Delaware's Center for Climatic Research, which provides data on monthly rainfall estimates for every 0.5° longitude and latitude nodes (which is approximately an area of 55km by 55km at the equator) across the world for the years 1901 to 2014. I use this data to derive monthly rainfall estimates for the longitude and latitude points located within India and identify nodes closest to each village centroid. For the historical districts based on the Indian Census 1961 boundaries, I identify the 10 closest nodes to the district centroids, which should cover the entire area of at least 95 per cent of the districts and take the simple average.

Since the effect of rainfall variability on agricultural output is moderated by irrigation dams as shown by Duflo and Pande (2007), I use their dataset of dam construction in India. They use data provided by the World Registry of Large Dams to identify irrigation dams constructed both within and upstream of a district over the period from 1971 to 1999 (Figure 1). Since dam construction is endogenous, they use various measures of the gradients of the rivers located in each district as well as elevations and their various interactions to construct instruments that predict whether a dam will be constructed in a given district in a given year.

Duflo and Pande's dataset also uses the Evenson–McKinsey India Agriculture and Climate Dataset (World Bank), which is based on historical districts as defined by the 1961 Indian Census. This dataset covers 271 districts across 13 states (Kerala and Assam are the important missing agricultural states) from 1950 to 1987. Duflo and Pande (2007) extend this to 1999. Since this dataset also contains the geographic coordinates of each district centroid, I use this to map villages in the REDS dataset to the historical districts. Figure 2 shows the share of villages where an irrigation dam was built in an upstream district over time. Among the 223 villages that I was able to map to a historical district, 98 already had an upstream district dam in 1961. This increases to 130 by 1985 (from 44 percent to 58 percent).

Following Jayachandran (2006), I eliminate villages in districts with altitudes that are higher than 600m, since rainfall's effects on crop yields are much weaker in these districts, and following Foster and Gehrke (2020), I remove villages with a population over 10,000 according to the 2001 Indian Census. The results remain similar but with larger standard errors when these village are included.

The final dataset comprises of 38,589 individuals from 7,464 households in 206 villages located across 92 districts in 13 states (districts and states are as defined in the 1961 Indian Census). There are 22,208 adults (those aged 21 years old or more) and 8,720 school-age (those aged between six and 15 years old). The individual, household and village characteristics are as captured in the REDS 2006 data. From the other datasets, I have information, such as on agricultural production, dam construction, etc., at the district level over the period from 1961 to 1999. Table 1 presents the descriptive statistics for demographic characteristics as well as the dependent variables.

The average household in my sample has about 6.5 members and have between two to three school-age children. The mean age of household members is 29 years old. The average population size of the villages is about 2,200. Women and girls are almost half of the population. Looking at educational attainments, the average age when adults and school-age children started school is 5.8 years old. Only 54 per cent of adults have ever attended school. About 19 per cent of adults have at most only primary level education while 35 per cent have received at least some secondary education. The omitted category is those with no education at all, at 46 percent, and are defined as illiterate in the data. The average number of years of completed schooling by adults is 4.6 years. About 91 per cent of school-age children were enrolled in school at the time of the survey, while of these 95 per cent are at the right level for their age. The average amount spent by households on each child for education is about Rs. 1,000.

In Table 2, I compare educational attainments by gender. This reveals a stark disparity between men and women. While 69 percent of men have ever enrolled in school, only 38 percent of women did, which is only about half the enrollment rate of men. Only 16 percent of women who did enroll have received at least some primary education while 22 percent of men do and while 47 percent of men have received at least some secondary education, only 22 percent of women report the same. When we look at children, we see that the enrollment rate for boys is 93 percent while it is 89 percent for girls. Also, 96 percent of boys are on track at school while 94 percent of girls are, and the average amount spent on education by households on boys' education is Rs. 1,100 while they spend only Rs. 830 on girls' education. Figure 3 shows the enrollment rates by age for boys and girls. It shows that girls start school later than boys and are more likely to face interruptions in their education.

B. Rainfall Shocks and Volatility

Following the literature (surveyed in Dell, Jones and Olken, 2014), I use rainfall volatility as a proxy for income risk that rural households face and rainfall shocks as a proxy for income shocks. Dell, Jones and Olken (2014) analyze the validity of using exogenous weather variables such as rainfall to instrument for various economic variables including agricultural output and point to papers documenting the impact that rainfall has on rural incomes.[20] Importantly, they highlight the non-linear effects of weather on agricultural output.[21] While Fishman (2011) as well as Duflo and Pande (2007) find evidence that irrigation moderates these effects of rainfall. Duflo and Pande (2007) show that after an irrigation dam has been constructed in an upstream district in India, negative shocks have a much smaller impact on reducing agricultural output in the downstream district.

[20] Paxson (1992), Jayachandran (2006), Yang and Choi (2007), Hidalgo et al (2010) and Amare et al (2018) find strong impacts of rainfall on agricultural output, wages, incomes, and consumption.
[21] Schlenker and Roberts (2009) and Lobell, Schlenker and Costa-Roberts (2011) study the non-linear effects of weather on agricultural output.

Agriculture in India largely depends on the seasonal monsoon rains. The timeliness as well as the level of rainfall are important factors for agricultural productivity.[22] Similar to Kaur (2019), I focus on rainfall levels in the starting month of the monsoon, which is usually between June to July for most districts in India. Following Jayachandran (2006) and Kaur (2019), I calculate the distribution of rainfall in each month for each village. A shock happens when rainfall deviates from the monthly village mean. A negative rainfall shock is defined as rainfall that is below the twentieth percentile value, while a positive shock is defined as rainfall that is above the eightieth percentile value. Jayachandran (2006) and Kaur (2019) find that these discrete cutoffs capture the nonlinear relationship between rainfall and agricultural productivity in India, where a negative shock has a negative impact on agricultural output, while a positive shock has a positive impact, and that using these definitions increase power. For each year for each village, I calculate rainfall shocks in the monsoon starting months. The rainfall shock variable takes the value of *-1* if it is a negative shock, *1* if it is a positive shock, and *0* if there is no shock.

Following Fitzsimons (2007) and Colmer (2021), the primary measure of rainfall volatility is the coefficient of variation over almost the entire sample of rainfall data, starting from 1901 to 2006 when the REDS 2006 survey began. Rainfall is a stationary process[23] and hence, this parameter should not be affected by the time period over which it is calculated, provided that the data has been consistently measured. Kaur (2019) finds that rainfall is serially uncorrelated across years. By using almost the full period of the available rainfall time series, this measure should arrive closer to the true value of rainfall volatility for the geographic location in question. The coefficient of variation is simply the standard deviation divided by the mean (σ/μ). It presents a unit-less measure of the dispersion of rainfall. I calculate for each village and district the coefficient of variation based on the means and standard deviations of rainfall in the respective monsoon starting months over the years 1901-2006.

There is a wide variation in the levels of annual rainfall among the villages and districts, from about 275mm to 2,259mm for districts and from about 253mm to 4,380mm for villages as

[22] Rosenweig and Udry (2013) explain the importance of the monsoon arrival month on farmers' choices that then affect agricultural profits in India and how farmers without access to good insurance markets act conservatively, investing less on their farms and choosing crop mixes and cultivation techniques that reduce the volatility of farm profits at the expense of lower expected profits.

[23] See Sun, Fubao and Farquhar (2018) for further details on assessing the stationarity of rainfall time series.

shown in Table 1. However, the average annual rainfall is about 990mm for both villages and districts. The average rainfall volatility in the monsoon starting month for districts is 0.64 with values ranging from 0.26 to 1.08. For villages, it is 0.74 and the values range from 0.32 to 1.25.

Finally, I split my sample between those who had completed their schooling years before a dam was constructed in an upstream district, that is, they were already aged 15 years old or older when the dam was built, and those who were born after the upstream district dam was constructed. I find that 48 percent of my sample were born after the upstream dam was constructed. Using Duflo and Pande's (2007) predicted dam variable, the share of those born after a predicted upstream district dam construction is 40 percent.

C. Descriptive Evidence

Figure 4 shows some descriptive evidence of the effects of village rainfall volatility on various measures of educational attainments that have been aggregated at the village level. As expected, most of them show a negative relationship with village rainfall volatility. The share of adults who have ever enrolled in school decreases strongly with increasing rainfall volatility (Figure 4A), similar to the share of adults with at most only primary education (Figure 4B). Likewise, the share of adults with at least some secondary education is also decreasing (Figure 4C). The total number of years of completed schooling also sharply decreases with increasing volatility (Figure 4D). For school-age children, current enrollment rates decrease slightly with volatility (Figure 4E), as do the share of enrolled children who are on track at school, that is, they have attained the appropriate number of years of completed schooling for their age (Figure 4F). Whereas the total expenditure on education per child do not seem to show any correlation with rainfall volatility (Figure 4G). Finally, the age at which people started school among those aged at least six years old or more increases slightly with increasing rainfall volatility (Figure 4H).

IV. Empirical Design

My identification strategy is as follows. I posit that income risk has an ex-ante impact on household's choices regarding investments into human capital that is independent of any (contemporaneous or persistent) ex-post effects of realized income shocks. Figure 5 shows a hypothetical household income distribution in the present, showing past realized income, future expected income and future actual income. Households form beliefs about their income risk and future income stream based on historical and recent occurrences of income shocks. They choose economic activities and productive investments based on these beliefs as well as on their current capacity to cope with any potential income shocks ex-post to ensure smooth consumption. They then choose how much resources to invest in education, in terms of time allocation of household members (both child and adult time) as well as financial outlays, in order to achieve their desired level of human capital. Households could choose three possible levels of education for their children: none (not enrolling in school at all), primary only (enrolling in primary school and investing just enough to attain at least a few years of primary education), or at least some secondary (investing sufficiently in primary education in order to progress on to and enroll in and potentially complete secondary school). Therefore, if a hypothetical household had faced high income volatility with many negative income shocks in the past, they would decide on a lower level of education in the present, even if their future income stream turned out to be more stable with fewer or less severe negative income shocks. And this level of education would be lower than a similar household with the same average income but lower income volatility.

Since income volatility is correlated with past income shocks, to dis-entangle these two effects, I include a number of variables for each individual. First, since many papers find long-term impacts of early childhood shocks, I include a measure for the shocks that took place in early life (from the year before birth to age five), as well as another measure for the shocks that took place during the schooling years, between the ages of six and fifteen. Shah and Steinberg (2017) studying the effects of income shocks on current human capital and education measures among children in rural India find that positive income shocks in early childhood have a positive effect, while negative income shocks have a negative effect. But they find that both positive and negative income shocks during the schooling years have a negative impact.[24]

[24] Subsequent work by the authors in Bau et al (2020) also show a similar impact.

Therefore, I construct two variables that capture all income shocks, both positive and negative, for these two periods of each individual's life. The early life rainfall shock variable sums over shocks that occurred when the individual is in utero (year before birth) up to the fifth year of life. Thus, the aggregate shock variable could ranges from -6 to +6. Similarly, second variable sum over the shocks that occurred during the schooling years from age six to 15. This could range from -10 to +10.

In addition, since households facing higher income risk would have experienced more negative income shocks in the past that could have reduced both current household incomes and wealth, I include a variable that sums all historical negative income shocks that occurred before the birth of the individual. This variable should capture the persistent impact of all historical negative income shocks on the household up until the birth of the individual. This variable is constructed by summing all negative deviations from the mean of rainfall in the monsoon starting month whenever there was a severe negative rainfall shock (rainfall below the 20th percentile) in the period from the start of the rainfall time series in 1901 until the year before the birth of the individual.

Therefore, my model is as follows:

$$y_{ihvd} = \beta_o + \beta_1 risk_{hvd} + \beta_2 pastshocks_{ihvd} + \beta_3 earlylifeshocks_{ihvd} + \beta_4 schoolageshocks_{ihvd} + \beta_5 V_{vd} + \beta_6 H_{hvd} + \lambda_d + \tau_t + \gamma_b + \epsilon_{ihvdt} \tag{1}$$

The dependent variable, y_{ihvd}, are the various education variables presented in Table 1 and shown in Figure 3. The main independent variable is household income risk, $risk_{hvd}$. The variable, $pastshocks_{ihvd}$ captures any persistent effect of historical negative income shocks that affected the household before the birth of the individual. While $earlylifeshocks_{ihvd}$ are income shocks that took place over the period just before the birth of the individual until age five, and $schoolageshocks_{ihvd}$ are income shocks that took place when the individual was of school age (six to 15 years old). The latter two variables capture both positive and negative shocks that have been found to have an impact, both positive and negative, on educational attainments. V_{vd} represents a vector of village-level control variables. And H_{hvd} is a vector of household-level controls such as caste and religion. β_0 is a constant, λ_d is district fixed effect,

τ_t is the survey year fixed effect, and γ_b is the year of birth fixed effect.[25] Finally, ϵ_{ihvd} is the error term capturing unobserved variation and is clustered at the village level. The subscripts, *h*, *v*, *d*, and *t*, index the household, village, district, and year of the REDS survey respectively. The key identifying assumption is that, after controlling for both historical (before the lifetime of the individual) and recent past shocks (during the lifetime of the individual from just before birth until school-leaving age), the remaining impact is due to the ex-ante effect of income risk.

Since $risk_{hvd}$ is not directly observable, I use the coefficient of variation of village rainfall in the monsoon starting month calculated over the years from 1901 to 2006 as a direct proxy. As I include district fixed effects to capture time-invariant unobserved characteristics at the district level, my identification is based on the within-district variation in rainfall. Though village and district rainfall are strongly correlated, village rainfall remains more variable. Finally, as explained earlier, I use both positive and negative rainfall shocks to proxy for income shocks that occurred in early life and during the schooling years, $earlylifeshocks_{ihvd}$ and $schoolageshocks_{ihvd}$. As village level controls, I only include the average rainfall in the monsoon starting month over the same period, from 1901 to 2006.

The model I estimate is therefore:

$$\begin{aligned} y_{ihvd} = \beta_o + \beta_1 risk_{vd} + \beta_2 pastshocks_{ihvd} + \beta_3 earlylifeshocks_{ihvd} \\ + \beta_4 schoolageshocks_{ihvd} + \beta_5 meanrain_{vd} + \beta_6 H_{hvd} + \beta_7 nregs_{ihvd} \\ + \lambda_d + \tau_t + \gamma_b + \epsilon_{ihvd} \end{aligned} \tag{2}$$

Finally, in order to exploit the additional exogenous variation introduced by the construction of irrigation dams, which attenuate the effects of rainfall shocks on agricultural incomes (Duflo and Pande, 2007), I use the variation in the ages and hence year of birth of individuals in my sample. I split my sample into those who were already aged 15 years old or older when a dam was constructed in the upstream district (and hence could not have benefitted from the risk reducing effect of the dam) and those who were born after the dam construction, following the strategy used by Duflo (2001). Therefore, I estimate Equation 2 separately for those who

[25] I only observe age at time of the survey so to obtain the year of birth, I subtract age from year of survey.

benefitted from an upstream district dam and those who did not.[26] I also estimate Equation 2 separately on adults and school-age children and by gender. When estimating on the sample of school-age children, H_{hvd} includes controls for total household landholdings in acres per capita, total household assets and savings per capita, total household income per capita and household size. I also include a control, $nregs_{ihvd}$, if the child lived in a district that was covered by the national public works program, the National Rural Employment Guarantee Scheme (NREGS), when they were surveyed, and which Shah and Steinberg (2019) find affected children's education. When estimating on the sample of adults, I omit the NREGS control variable.

I expect that the first sample in all my regressions (those who were aged at least 15 years old before an upstream district dam was built and therefore, should have been completed most of their schooling prior) would have experienced a strong impact of rainfall volatility (reflecting ex-ante income risk) on their educational attainments. While for those in the second sample (who were born after the upstream district dam was built and whom I term the "dam cohort"), the link between rainfall volatility and income risk would have become much weaker or even decoupled given the moderating effects of irrigation dams, and therefore, would see little impact of rainfall volatility. If the differences in the estimates between the two regressions are statistically significant, that lends strong credence to my identification strategy.

By estimating separately, I also allow for the district fixed effects to vary by exposure to irrigation dam construction in an upstream district. I do this because dam construction is associated with other economic investments into the region, for example, new infrastructure developments, such as building of new roads, etc. As a robustness check, I estimate my model by using interaction terms between dam exposure and maintaining the district fixed effects to be the same for both samples. My estimates have similar sizes and signs; however, precision varies.

One caveat however presents itself. Dam construction raises agricultural productivity and thereby, also raises agricultural wages. Shah and Steinberg (2017 and 2019) find that shocks that increase rural wages or incomes increase the human capital of very young children through an income effect, but however, decreases educational attainments among school-age children

[26] I could equally estimate by interacting the dummy variable indicating the two samples with all the regressors. The estimates are the same. Estimating in this manner shows the shift in impact of the risk variable before and after upstream district dams were constructed more clearly.

through a substitution effect, by increasing the opportunity cost of schooling. Therefore, the estimates on the dam “cohort” may be ambiguous. However, it is the initial shift in wages immediately follow dam construction that is likely to cause this negative effect, and the long-term effect of higher and more stable income produced by dams could be positive.[27] I define the sample likely to benefit from the construction of a dam in the upstream district as those who were born after its construction and therefore, by the time they reach school age, wages should have adjusted to a new stable equilibrium, barring any subsequent shocks that are controlled for, and also, households would have internalized the risk moderating effects of the dam.

[27] Foster and Gerhke (2020) make a similar claim for the negative impact on children’s education caused by the roll-out of NREGS.

V. Results

The estimates from Equation 2 for adults are shown in Tables 3-5 and for school-age children in Tables 6-8. For adults who did not benefit from an upstream irrigation dam, rainfall volatility leads to a reduction in the total years of completed schooling, a decrease in the share of those who ever enrolled in school, an increase in the share of those with only some primary education, a decrease in the share of those who went on to have at least some secondary education, and a delay in the start of schooling for those who did enroll (Table 2). This shows a consistent negative impact, although only the estimates on years of schooling, share of those with primary education only, and share of those with at least some secondary education are statistically significant at the 10 percent or five percent levels.

For the sample of adults who were born after an upstream dam was constructed, the impact of rainfall volatility turns positive for all these variables, although none are statistically significant. However, the differences in the estimates for completed years of schooling and share of those with secondary education is statistically significant at the 10 percent or five percent levels. In real terms, those living in places at the 90th percentile of risk have 5.3 fewer years of education while those living in places at the 10th percentile of risk have 2.4 fewer years, and the difference is 2.9 years, which is more than double what Fitzsimons (2007) finds.[28] Similarly, those in the very high-risk places are 46 percentage points less likely to enroll in secondary school whereas those in the low-risk places are only 21 percentage points less likely to enroll leading to a difference of 25 percentage points. Therefore, this indicates that the ex-ante impact of income risk does lead to reductions in investments in human capital such that many people who do enroll in school attain at most some primary education.

When I estimate Equation 2 separately by gender, I find that these effects are strongly driven by the impact on men. Their total years of schooling is lower, by 6.5 years for those at the

[28] Her lower estimate could be explained by two factors. First, she only includes children aged between 10 and 15 years old, who were born just after one of the largest public school construction programs in history, which had already increased enrollment and schooling levels significantly (Duflo, 2001). Second, she uses historical variation in rainfall as an instrument for both village-level risk and household-level risk (by interacting with household head's occupation). However, the relationship between rainfall and agricultural income is somewhat ambiguous in Indonesia, which has lower seasonal and annual variation than India, given its geographic distribution west to east along the equator while the landmass of India is distributed north to south, as well as having more locations with higher elevations where rainfall has a weaker relationship to agricultural productivity.

ninetieth percentile of risk. They are much more likely to only have primary education, by 55 percentage points, and much less likely to have some secondary education, by 71 percentage points. However, this disparity is not surprising given the differences in the levels of education between men and women as shown in Table 2. Women are much less likely to have ever been enrolled in school and have much lower educational attainments even when they did. Therefore, the adjustments to differences in exposure to income risk are mainly driven by the impact on boys. However, among women who did enroll in school, high risk reduces years of schooling by 3.6 years (among those at the 90th percentile of risk).

For school-age children overall, there does not seem to be any important impact of risk on current enrollment, age when they started school, or their likelihood of being on track at school. However, household expenditure on education is lower by about Rs. 1,400 for those that did not benefit from dams and were facing high volatility. It becomes positive for those that did benefit, and this difference is statistically significant at the five percent level. When analyzing separately by gender, I find that this effect is mainly driven by the impact on girls, who are also less likely to be on track at school (as shown in Figure 4), but the difference is not statistically significant. It should be noted that the data on dam construction ends in 1999, so for school-age children surveyed between 2006 and 2008, the variation only comes from across villages and districts rather than also from across time, which may lead to less precise estimates.

These results indicate that income risk does have a strong ex-ante impact on education. The estimates for adults on having been ever enrolled in school are consistently negative and large but are imprecisely estimated. The estimates on the shares with only primary education and with at least some secondary education are negative, large and significant among men. While the estimate on total years of completed schooling among both men and women is also negative and large. Among current children, income risk seems to play a smaller role, but reduces the financial resources that households devote to girls' education, and girls also seem to face more interruptions in their schooling and are less likely to be on track.

VI. Mechanisms

There are various potential mechanisms through which income risk plays an ex-ante role in determining educational attainments. From the macroeconomic canon, we know that when future income is uncertain and markets are incomplete, prudent agents save more in order to ensure a smooth consumption path, what has been termed as precautionary savings. This leaves less income for current consumption and investments. Most of the empirical work providing evidence of the precautionary motive has been conducted using data from advanced economies. For poor households in developing countries, any income shocks that are not smoothed could lead to literal starvation. Among these households, diversification of economic activities and lack of specialization has been extensively documented, such as by Banerjee and Duflo (2012) among others. The preference for immediate income generating activities and reluctance to specialize have been considered as a response to income uncertainty. Rosenzweig and Binswanger (1993), among others, show farmers' preference for low risk and also low return crops such as sweet potatoes when they face higher weather variability. Kazianga (2012) highlight that the precautionary savings motive could lead to households having less income available for investments, and also explain a lower preference for making expensive, long-term investments that need fixed inputs of resources (time and money), such as education. In addition, lack of specialization could also reduce demand for more human capital.

Furthermore, as highlighted earlier, poor households consider children as a form of "buffer stock", where they expect to draw on child labor in the event of adverse income shocks in order to either directly provide market wages or take over home production of adults who then enter the labor market. There is evidence that India's NREGS, which favored the hiring of women, adversely affected older girls' education who supplemented for adult female home production or child caring activities (Shah and Steinberg, 2019). In this case, households would also be less willing to invest more in children's human capital in inverse relationship with the income uncertainty that they face.

On the other hand, the economic diversification argument could also be used to explain increased demand for human capital if it leads households to diversify away from a risky economic activity such as agriculture. Colmer (2021) finds a positive impact of weather variability on schooling in Ethiopia, which he attributes to households wanting to diversify away from reliance on agriculture. However, this depends on both the capacity of households to have available resources to invest in education and on the alternative economic activities available to these households. Rural areas often offer few alternative economic opportunities to agriculture, though migration remains an option, and for households subsisting on the margin, it may be difficult to invest sufficiently such that migration becomes viable.

Finally, the key question underpinning all the above remains: how do households form beliefs about future income? Barring data on beliefs, this can only be inferred. Malmendier and Nagel (2011) show that those exposed to lower stock market returns over their lifetimes tend to be more risk averse and are less likely to hold stocks or invest less when they do, a demonstration of the role of the experience effect in shaping beliefs and choices (Malmendier, 2021). Similarly, households are likely to base their beliefs about future income (Figure 5) on past realized income. In this case, rather than the *levels* of realized income, I focus on the *volatility*. Two similar households could have had the same average realized income, but if one household's experienced income volatility is greater than the other's, then it would invest less in education. Therefore, the extent to which rainfall volatility reflects income volatility would determine the impact of income volatility on educational investments in my model.

VII. Robustness Checks

A. Threats to Identification

There are a few potential threats to identification, mainly from sample selection issues. First, there could be differing mortality rates between those with lower versus higher educational attainments and this could bias my results. If the better educated have a higher mortality rate, then they would be under-represented in my sample, and therefore, my results would be biased upwards. However, it is difficult to identify reasons why the better educated should have higher mortality rates, and more plausibly, mortality rates should be higher for those with lower educational attainments since more education is correlated with better health. In this case, the bias should actually be downwards. Therefore, varying mortality rates by education level is not a critical issue.

Second, and more importantly, since I do not have information on the place of birth, I assume that most adults have not migrated from their place of birth or from where most of their schooling took place. Specifically, since my data only covers those who lived in rural areas when surveyed, I have to rule out both rural-urban migration and rural-rural migration within or across districts, due to shocks or for other reasons. Jayachandran (2006) notes that migration rates in India are relatively low, and Duflo and Pande (2007) also note that migration across districts are low, especially in response to shocks. With respect to rural-rural migration, I find that the share of household heads who migrated into the village in my sample is very small and that the distances from origin village or town are also quite small. This seems to indicate that rural-rural migration is quite rare, mainly takes place within the district, and after any schooling was completed.

However, rural-urban migration even within the same district, especially of those with higher educational attainments, would affect my identification since those who migrated would be missing in my sample and the better educated are more likely to migrate. I examine how likely that those with higher educational attainments migrated permanently out to urban areas. The REDS 2006 contains information on all non-co-resident children of the current household head. The majority consists of women, which is not surprising given that it is customary to marry daughters to households outside the village. Indeed, this could be evidence of one income risk mitigation strategy (Rosenzweig and Stark, 1989).

Finally, the income risk variable could be correlated with dam construction, such that villages in districts with higher risk would be more likely to see dam construction in the upstream district. In addition, dam construction could directly affect educational attainments and not just through its effect on income levels and income risk and subsequent increased demand for education. For example, the supply of schools could increase after dam construction. To test this, I use Duflo and Pande's (2007) predicted dam variable using various geographic instruments and run the same regressions again. The estimated effects are similar, though there is less precision (Appendix C. Tables 9 to 14).

Finally, I assume that there are no other omitted time varying and region-specific trends separate from dam construction that could also affect either the demand for or supply of education (and that are also potentially not correlated with dam construction).

B. Sensitivity Checks

I construct different measures for income risk such as the coefficient of variation over the entire agricultural and calendar year as well as the skewness of rainfall in the monsoon starting month. Estimating Equation 2 again using these variables yields similar estimates with varying precision.

VII. Conclusion

The ex-ante impact of income volatility and hence income risk on human capital accumulation among the poor facing issues of both scarcity and precarity have not been very well studied and could potentially explain the low levels of human capital beyond what is attributed to lack of resources, ex-post effects of adverse income shocks, and credit and insurance constraints. Using a novel design exploiting the strong link between rainfall and incomes among poor rural households in India and the additional exogenous variation introduced by irrigation dam construction, I find clear evidence that high income risk led to substantially lower levels of educational attainments, in terms of completed years of schooling, a higher share with only primary education and a smaller share with any secondary education. Among adults, it led to 2.9 fewer years of completed schooling, and a 25-percentage point reduction in secondary school enrollment, with the effects mainly being driven by men. While income risk seems to play a smaller role among school-age children, it seems to disproportionately affect girls such that households spend less on their education, and they are also more likely to face interruptions reducing both the quality and quantity of their human capital.

These findings show that, while poverty alleviation policies and other economic policies aimed at improving the income levels of very poor people remain important in reducing the ex-ante impact of income risk by increasing households' own capacity to self-insure and buffer the impact of negative income shocks and protect their productive investments, more could be done by policymakers. As Dercon et al (2004) highlight, the scope of the informal risk mitigation mechanisms used by poor households are limited, and repeated household specific shocks or covariate risks overwhelm their ability to cope with risk. In addition, as Chetty and Looney (2006) point out, estimating welfare gains from social safety nets in low-income economies using consumption responses to income shocks have historically undervalued their benefits. Furthermore, most of these studies used ex-post responses to income shocks to capture the total effect of risk, neglecting the ex-ante impact. Elbers et al (2007) find that about two-thirds of the impact of risk is due to the ex-ante effect and conclude that policy interventions that reduce households' exposure to shocks or help them manage risk could be much more effective than is commonly thought.

Therefore, as Dercon et al (2004) emphasize, effective public policies should focus on providing both ex-ante and ex-post protection mechanisms. Safety nets and various forms of insurance, from weather, crop to health insurance, that help protect households from the consequences of severe negative income shocks could also help them choose “riskier” investments, whether related to production or human capital accumulation, and which in turn could improve their economic prospects in the long term. For example, Jacobsen (2009) find that those who took up health and life insurance policies in rural Pakistan pursued less diversified income portfolios. Similarly, Landman and Frolich (2015) find that an extension of a health and accident insurance scheme by a Pakistani microfinance institution led to reduced incidence of child labor, especially in hazardous occupations, and child labor earnings.

However, other policies aimed at improving educational attainments or helping households with income smoothing may have limited impact or even be counterproductive when it comes to education. Edmonds and Shrestha (2014) find limited impact of scholarships covering education related expenses and conditional cash transfers linked to school attendance. While public works programs such as India’s NREGS aim to help poor households supplement income in the event of negative income shocks, and thereby, serve as an income and consumption smoothing policy, initial impact on children’s education was found to be negative. Imbert and Papp (2015) show that NREGS led to increases in rural wages. This, according to Shah and Steinberg (2019), led to decreases in schooling among children exposed to the program. They find enrollment decreased by 1 to 3.5 percentage points and labor increased by 4 percentage points amongst adolescents. They argue that the opportunity cost of schooling is an important determinant of educational investment for rural households in India. Similarly, Li and Sekhri (2020), among others, also find a negative impact of NREGS on children’s human capital. However, Adukia (2019) determines that the adverse effects are small and could be easily countered by suitable policy interventions. Foster and Gerhke (2020) claim that while it is unclear how the direct effect of rising wages and the indirect effect of less variable incomes will balance out in the longer term, their policy simulation show that the long-run effects could be positive for human capital. Therefore, policies that specifically insure against catastrophic income shocks, and hence reduce the ex-ante impact of income risk, seem to be the best options.

With climate change increasing weather variability, communities dependent on rain fed agriculture will see even higher income volatility, such as those in South Asia and Sub-Saharan Africa. These are also the regions with the lowest levels of human capital and educational attainments, revealing another dimension to the negative impact threatened by climate change. This adds further urgency for policymakers to implement various risk reduction mechanisms, including climate change mitigation and adaptation interventions, that could help these communities better weather the weather.

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Appendix

A. Tables

TABLE 1—DESCRIPTIVE STATISTICS

	OBS	MEAN	SD	MIN	MAX	DATA SOURCE
A. Demographic characteristics						
Household size	7,464	6.46	3.15	1	25	REDS 2006
Children/household (school-age)	7,464	2.26	1.85	0	12	REDS 2006
Current age	38,589	29.22	20.04	1	106	REDS 2006
Share female	38,589	.48	.5	0	1	REDS 2006
Village population	206	2,249.13	1,822.11	51	9,778	REDS 2006/Census 2001
B. Education						
Age started school – those aged six years old or older	23,225	5.79	.8	1	13	REDS 2006
Ever attended school – adults only	22,208	.54	.5	0	1	REDS 2006
Primary education only – adults only	22,208	.19	.39	0	1	REDS 2006
Secondary education or more – adults only	22,208	.35	.48	0	1	REDS 2006
Years of completed school – adults only	22,208	4.59	5.01	0	20	REDS 2006
Currently enrolled – school-age children only	8,720	.91	.29	0	1	REDS 2006
On track – school-age children only	8,720	.95	.21	0	1	REDS 2006
Education expenditure – school-age children only (Rs.)	8,720	977.63	1,616.85	0	31,000	REDS 2006
C. Rainfall						
Mean annual rainfall – District (mm)	92	993.63	382.86	275.03	2,258.88	University of Delaware
Mean annual rainfall – Village (mm)	206	989.42	471.68	252.75	4,379.67	University of Delaware
Rainfall variability in monsoon start month - District	92	.64	.2	.26	1.08	University of Delaware
Rainfall variability in monsoon start month - Village	206	.74	.21	.32	1.25	University of Delaware
D. Dams						
Born after upstream dam constructed	38,589	.48	.5	0	1	REDS 2006/Duflo and Pande (2007)
Born after predicted upstream dam	38,589	.4	.49	0	1	REDS 2006/Duflo and Pande (2007)

Notes: Sample constructed from the respective data sources listed. Villages with a population greater than 10,000 and districts with an elevation higher than 600m are excluded from the sample.

TABLE 2—DESCRIPTIVE STATISTICS (EDUCATION BY GENDER)

	OBS	MEAN	SD	MIN	MAX	DATA SOURCE
A. Education - male						
Age started school – those aged six years old or older	13,947	5.82	.8	1	13	REDS 2006
Ever attended school – adults only	11,049	.69	.46	0	1	REDS 2006
Primary education only – adults only	11,166	.22	.41	0	1	REDS 2006
Secondary education or more – adults only	11,049	.47	.5	0	1	REDS 2006
Years of completed school – adults only	11,145	6.2	5.14	0	20	REDS 2006
Currently enrolled – school-age children only	4,732	.93	.26	0	1	REDS 2006
On track – school-age children only	4,364	.96	.19	0	1	REDS 2006
Education expenditure – school-age children only (Rs.)	4,732	1,101.65	1,821.94	0	31,000	REDS 2006
B. Education - female						
Age started school – those aged six years old or older	9,278	5.76	.81	1	12	REDS 2006
Ever attended school – adults only	10,960	.38	.48	0	1	REDS 2006
Primary education only – adults only	11,042	.16	.37	0	1	REDS 2006
Secondary education or more – adults only	10,960	.22	.41	0	1	REDS 2006
Years of completed school – adults only	11,011	2.95	4.29	0	19	REDS 2006
Currently enrolled – school-age children only	3,988	.89	.32	0	1	REDS 2006
On track – school-age children only	3,503	.94	.23	0	1	REDS 2006
Education expenditure – school-age children only (Rs.)	3,988	830.49	1,318.37	0	20,320	REDS 2006

Notes: Sample constructed from the respective data sources listed. Villages with a population greater than 10,000 and districts with an elevation higher than 600m are excluded from the sample.

TABLE 3 — RESULTS (ALL ADULTS)

VARIABLES	Years of schooling		Diff	Ever enrolled		Diff	Primary only		Diff	Secondary +		Diff	Age started school		Diff
	(1)	(2)		(3)	(4)		(5)	(6)		(7)	(8)		(9)	(10)	
Risk	-5.265*	2.547	7.811**	-0.240	0.273	0.513	0.221**	0.122	-0.099	-0.457*	0.152	0.609*	0.394	-0.114	-0.508
	(2.791)	(2.192)	(3.465)	(0.260)	(0.217)	(0.327)	(0.098)	(0.092)	(0.124)	(0.259)	(0.206)	(0.325)	(0.244)	(0.207)	(0.315)
Mean village rainfall	-0.332	1.264	1.596	0.030	-0.016	-0.046	0.073	-0.192***	-0.265***	-0.042	0.179***	0.221**	0.059	-0.053	-0.112
	(0.914)	(0.846)	(1.254)	(0.074)	(0.081)	(0.108)	(0.050)	(0.047)	(0.068)	(0.090)	(0.066)	(0.109)	(0.045)	(0.090)	(0.093)
Sum of all negative deviations in rainfall	0.010	-0.116	-0.126	-0.007	-0.017	-0.010	-0.008*	-0.003	0.005	0.001	-0.013	-0.014	0.014	-0.020	-0.034*
	(0.080)	(0.096)	(0.124)	(0.007)	(0.011)	(0.013)	(0.004)	(0.005)	(0.006)	(0.008)	(0.009)	(0.012)	(0.013)	(0.013)	(0.018)
Total early life shocks	0.046	-0.032	-0.078	-0.001	-0.002	-0.001	-0.003	0.000	0.003	0.002	-0.002	-0.004	-0.008	0.002	0.010
	(0.084)	(0.085)	(0.123)	(0.008)	(0.008)	(0.012)	(0.007)	(0.008)	(0.011)	(0.008)	(0.009)	(0.012)	(0.016)	(0.017)	(0.024)
Total shocks during schooling years	-0.018	0.031	0.049	0.000	-0.005	-0.006	-0.003	-0.006**	-0.003	0.003	0.001	-0.002	0.012	0.011	-0.001
	(0.035)	(0.043)	(0.055)	(0.003)	(0.004)	(0.005)	(0.002)	(0.003)	(0.004)	(0.003)	(0.004)	(0.005)	(0.008)	(0.007)	(0.010)
Observations	11,253	7,338		11,228	7,232		11,274	7,356		11,228	7,232		5,527	4,714	
R-squared	0.273	0.232		0.249	0.199		0.084	0.081		0.230	0.188		0.268	0.209	

Notes: Results from estimating Equation 2 on the sample of adults (those aged 21 years old or older). The sample is split and estimated separately for those who were born after a dam was constructed in an upstream district and those who were aged at least 15 years old or older when the upstream district dam was constructed. The dependent variables are the total years of completed schooling, the share who ever enrolled in school, the share with only primary education, the share with at least some secondary education, and the age at which schooling started for those who ever enrolled. Risk is the coefficient of variation of village rainfall in the monsoon starting month. All regressions include controls for caste (a dummy indicating low caste, whether scheduled castes/tribes or not), religion (a variable indicating whether Hindu, Muslim, or other), and fixed effects for district, year of birth, and survey year. Standard errors are clustered by village.

Sources: REDS 2006, University of Delaware Center for Climatic Research, Duflo and Pande (2007), and Evenson and McKinsey (World Bank).

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

TABLE 4 — RESULTS (ALL ADULTS: FEMALE)

VARIABLES	Years of schooling		Diff	Ever enrolled		Diff	Primary only		Diff	Secondary +		Diff	Age started school		Diff
	(1)	(2)		(3)	(4)		(5)	(6)		(7)	(8)		(9)	(10)	
Risk	-3.612	2.380	5.991*	-0.247	0.292	0.539	-0.077	0.080	0.157	-0.166	0.202	0.368	0.279	0.030	-0.250
	(2.740)	(1.611)	(3.168)	(0.301)	(0.227)	(0.368)	(0.166)	(0.156)	(0.216)	(0.223)	(0.158)	(0.275)	(0.262)	(0.334)	(0.407)
Mean village rainfall	-0.154	1.199***	1.353	-0.009	-0.001	0.008	-0.064	-0.219***	-0.155*	0.056	0.217***	0.161	0.025	-0.047	-0.073
	(0.877)	(0.404)	(0.968)	(0.076)	(0.065)	(0.095)	(0.075)	(0.063)	(0.093)	(0.088)	(0.044)	(0.099)	(0.037)	(0.109)	(0.107)
Sum of all negative deviations in rainfall	0.118	-0.113*	-0.231**	0.005	-0.013	-0.018	-0.003	-0.003	0.001	0.008	-0.011	-0.018*	0.007	0.003	-0.004
	(0.088)	(0.068)	(0.111)	(0.009)	(0.010)	(0.013)	(0.005)	(0.007)	(0.008)	(0.008)	(0.006)	(0.011)	(0.019)	(0.022)	(0.028)
Total early life shocks	0.059	-0.051	-0.111	0.004	-0.009	-0.013	-0.013	-0.006	0.007	0.017*	-0.001	-0.018	0.004	-0.028	-0.032
	(0.093)	(0.107)	(0.142)	(0.010)	(0.012)	(0.016)	(0.008)	(0.013)	(0.016)	(0.010)	(0.011)	(0.015)	(0.026)	(0.022)	(0.035)
Total shocks during schooling years	0.010	-0.016	-0.027	0.004	-0.006	-0.010	-0.001	-0.004	-0.003	0.005	-0.002	-0.007	0.017	0.015*	-0.002
	(0.040)	(0.047)	(0.061)	(0.005)	(0.006)	(0.007)	(0.003)	(0.005)	(0.006)	(0.004)	(0.004)	(0.006)	(0.011)	(0.009)	(0.014)
Observations	5,532	3,693		5,530	3,648		5,545	3,704		5,530	3,648		1,856	1,824	5,532
R-squared	0.372	0.324		0.357	0.286		0.102	0.097		0.303	0.244		0.310	0.243	0.372

Notes: Results from estimating Equation 2 on the sample of adults (those aged 21 years old or older). The sample is split and estimated separately for those who were born after a dam was constructed in an upstream district and those who were aged at least 15 years old or older when the upstream district dam was constructed. The dependent variables are the total years of completed schooling, the share who ever enrolled in school, the share with only primary education, the share with at least some secondary education, and the age at which schooling started for those who ever enrolled. Risk is the coefficient of variation of village rainfall in the monsoon starting month. All regressions include controls for caste (a dummy indicating low caste, whether scheduled castes/tribes or not), religion (a variable indicating whether Hindu, Muslim, or other), and fixed effects for district, year of birth, and survey year. Standard errors are clustered by village.

Sources: REDS 2006, University of Delaware Center for Climatic Research, Duflo and Pande (2007), and Evenson and McKinsey (World Bank).

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

TABLE 5 — RESULTS (ALL ADULTS: MALE)

VARIABLES	Years of schooling		Diff	Ever enrolled		Diff	Primary only		Diff	Secondary +		Diff	Age started school		Diff
	(1)	(2)		(3)	(4)		(5)	(6)		(7)	(8)		(9)	(10)	
Risk	-6.496**	3.550	10.046**	-0.172	0.282	0.453	0.546***	0.124	-0.422**	-0.713**	0.173	0.885**	0.436	-0.134	-0.570
	(3.134)	(3.374)	(4.461)	(0.256)	(0.285)	(0.372)	(0.180)	(0.125)	(0.202)	(0.335)	(0.302)	(0.443)	(0.317)	(0.187)	(0.361)
Mean village rainfall	-0.636	1.206	1.843	0.060	-0.051	-0.110	0.209**	-0.175***	-0.384***	-0.149	0.130	0.279*	0.085	-0.062	-0.147
	(0.996)	(1.508)	(1.809)	(0.075)	(0.139)	(0.157)	(0.081)	(0.063)	(0.100)	(0.123)	(0.107)	(0.156)	(0.071)	(0.095)	(0.112)
Sum of all negative deviations in rainfall	-0.075	-0.093	-0.018	-0.014*	-0.019	-0.005	-0.009	-0.003	0.005	-0.006	-0.013	-0.007	0.013	-0.030**	-0.043**
	(0.096)	(0.131)	(0.161)	(0.009)	(0.014)	(0.016)	(0.007)	(0.007)	(0.010)	(0.010)	(0.012)	(0.016)	(0.016)	(0.012)	(0.019)
Total early life shocks	0.108	-0.072	-0.180	-0.000	0.000	0.000	0.005	0.006	0.001	-0.005	-0.006	-0.001	-0.011	0.017	0.028
	(0.117)	(0.139)	(0.184)	(0.011)	(0.011)	(0.015)	(0.008)	(0.012)	(0.015)	(0.011)	(0.014)	(0.018)	(0.021)	(0.022)	(0.032)
Total shocks during schooling years	-0.027	0.054	0.081	-0.002	-0.007	-0.005	-0.005*	-0.008*	-0.003	0.004	0.002	-0.002	0.007	0.009	0.001
	(0.041)	(0.055)	(0.067)	(0.004)	(0.005)	(0.006)	(0.003)	(0.004)	(0.005)	(0.004)	(0.006)	(0.007)	(0.010)	(0.009)	(0.013)
Observations	5,721	3,645		5,698	3,584		5,729	3,652		5,698	3,584		3,671	2,890	
R-squared	0.292	0.259		0.246	0.220		0.143	0.116		0.271	0.216		0.271	0.213	

Notes: Results from estimating Equation 2 on the sample of adults (those aged 21 years old or older). The sample is split and estimated separately for those who were born after a dam was constructed in an upstream district and those who were aged at least 15 years old or older when the upstream district dam was constructed. The dependent variables are the total years of completed schooling, the share who ever enrolled in school, the share with only primary education, the share with at least some secondary education, and the age at which schooling started for those who ever enrolled. Risk is the coefficient of variation of village rainfall in the monsoon starting month. All regressions include controls for caste (a dummy indicating low caste, whether scheduled castes/tribes or not), religion (a variable indicating whether Hindu, Muslim, or other), and fixed effects for district, year of birth, and survey year. Standard errors are clustered by village.

Sources: REDS 2006, University of Delaware Center for Climatic Research, Duflo and Pande (2007), and Evenson and McKinsey (World Bank).

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

TABLE 6 — RESULTS (ALL SCHOOL-AGE CHILDREN)

VARIABLES	**Currently enrolled**		**Diff**	**Age started school**		**Diff**	**On track**		**Diff**	**Total educational exp**		**Diff**
	(1)	(2)		(3)	(4)		(5)	(6)		(7)	(8)	
Risk	0.032	0.047	0.014	0.628	0.436	-0.193	-0.089	-0.079	0.009	-1,352.932*	241.693	1,594.626*
	(0.145)	(0.177)	(0.228)	(0.387)	(0.430)	(0.578)	(0.107)	(0.078)	(0.132)	(771.359)	(439.444)	(884.983)
Mean village rainfall	0.038	-0.002	-0.040	-0.036	0.099	0.136	0.040	0.039*	-0.000	-138.528	497.825**	636.352**
	(0.036)	(0.043)	(0.056)	(0.108)	(0.105)	(0.150)	(0.031)	(0.023)	(0.038)	(189.926)	(205.539)	(279.150)
Sum of all negative deviations in rainfall	0.011	-0.021*	-0.031**	0.024	0.013	-0.010	-0.004	-0.002	0.002	-29.214	-24.430**	4.784
	(0.007)	(0.011)	(0.013)	(0.019)	(0.012)	(0.022)	(0.006)	(0.004)	(0.007)	(42.045)	(10.523)	(43.191)
Total early life shocks	-0.005	0.007	0.012	0.001	-0.043*	-0.044	-0.004	0.003	0.007	8.076	-26.574	-34.649
	(0.009)	(0.009)	(0.013)	(0.029)	(0.022)	(0.036)	(0.008)	(0.006)	(0.010)	(45.993)	(23.971)	(51.699)
Total shocks during schooling years	-0.004	0.010	0.014	0.074**	0.030**	-0.043	-0.012	-0.002	0.010	-86.127*	28.699	114.825**
	(0.008)	(0.007)	(0.010)	(0.028)	(0.014)	(0.031)	(0.007)	(0.007)	(0.010)	(45.848)	(20.459)	(50.041)
Observations	3,698	5,022		3,363	4,554		3,339	4,528		3,698	5,022	
R-squared	0.136	0.207		0.245	0.282		0.094	0.093		0.270	0.233	

Notes: Results from estimating Equation 2 on the sample of school-age children (those aged between six and 15 years old or older). The sample is split and estimated separately for those who were born after a dam was constructed in an upstream district and those who were aged at least 15 years old or older when the upstream district dam was constructed. The dependent variables are the total years of completed schooling, the share who ever enrolled in school, the share with only primary education, the share with at least some secondary education, and the age at which schooling started for those who ever enrolled. Risk is the coefficient of variation of village rainfall in the monsoon starting month. All regressions include controls for per capita household landholdings (acres), per capita household income (Rs.), per capita household savings and assets (Rs.), household size, whether the district was included in the NREGS, as well as controls for caste (a dummy indicating low caste, whether scheduled castes/tribes or not), religion (a variable indicating whether Hindu, Muslim, or other), and fixed effects for district, year of birth, and survey year. Standard errors are clustered by village.

Sources: REDS 2006, University of Delaware Center for Climatic Research, Duflo and Pande (2007), and Evenson and McKinsey (World Bank).

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

TABLE 7 — RESULTS (ALL SCHOOL-AGE CHILDREN: FEMALE)

VARIABLES	Currently enrolled		Diff	Age started school		Diff	On track		Diff	Total educational exp		Diff
	(1)	(2)		(3)	(4)		(5)	(6)		(7)	(8)	
Risk	0.191	0.217	0.025	0.538	0.709	0.171	-0.308*	-0.107	0.201	-2,186.057**	46.833	2,232.890**
	(0.223)	(0.203)	(0.301)	(0.663)	(0.566)	(0.869)	(0.183)	(0.095)	(0.205)	(848.333)	(449.101)	(957.060)
Mean village rainfall	0.118*	0.050	-0.068	0.033	0.117	0.084	0.013	0.060	0.048	-141.356	417.561**	558.918
	(0.060)	(0.046)	(0.076)	(0.156)	(0.127)	(0.200)	(0.038)	(0.038)	(0.053)	(283.119)	(187.325)	(338.531)
Sum of all negative deviations in rainfall	0.034***	-0.022	-0.056***	0.030	0.008	-0.022	-0.014	0.000	0.014	-64.981	-37.983**	26.998
	(0.010)	(0.014)	(0.017)	(0.034)	(0.016)	(0.038)	(0.010)	(0.004)	(0.011)	(44.253)	(14.710)	(46.488)
Total early life shocks	0.010	0.013	0.003	0.006	-0.001	-0.007	-0.019*	0.004	0.022	22.993	-4.407	-27.400
	(0.015)	(0.013)	(0.020)	(0.045)	(0.032)	(0.055)	(0.011)	(0.010)	(0.014)	(61.925)	(35.367)	(71.107)
Total shocks during schooling years	0.005	0.013	0.008	0.047	0.029	-0.018	-0.017*	-0.003	0.014	-28.493	37.271	65.764
	(0.011)	(0.009)	(0.015)	(0.048)	(0.023)	(0.053)	(0.010)	(0.008)	(0.012)	(56.894)	(27.217)	(62.881)
Observations	1,717	2,271		1,509	2,014		1,500	2,003		1,717	2,271	
R-squared	0.174	0.228		0.266	0.279		0.155	0.149		0.279	0.244	

Notes: Results from estimating Equation 2 on the sample of school-age children (those aged between six and 15 years old or older). The sample is split and estimated separately for those who were born after a dam was constructed in an upstream district and those who were aged at least 15 years old or older when the upstream district dam was constructed. The dependent variables are the total years of completed schooling, the share who ever enrolled in school, the share with only primary education, the share with at least some secondary education, and the age at which schooling started for those who ever enrolled. Risk is the coefficient of variation of village rainfall in the monsoon starting month. All regressions include controls for per capita household landholdings (acres), per capita household income (Rs.), per capita household savings and assets (Rs.), household size, whether the district was included in the NREGS, as well as controls for caste (a dummy indicating low caste, whether scheduled castes/tribes or not), religion (a variable indicating whether Hindu, Muslim, or other), and fixed effects for district, year of birth, and survey year. Standard errors are clustered by village.

Sources: REDS 2006, University of Delaware Center for Climatic Research, Duflo and Pande (2007), and Evenson and McKinsey (World Bank).

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

TABLE 8 — RESULTS (ALL SCHOOL-AGE CHILDREN: MALE)

VARIABLES	**Currently enrolled**		**Diff**	**Age started school**		**Diff**	**On track**		**Diff**	**Total educational exp**		**Diff**
	(1)	(2)		(3)	(4)		(5)	(6)		(7)	(8)	
Risk	0.003	-0.167	-0.170	0.758	0.183	-0.575	0.124	-0.046	-0.170	-696.305	468.691	1,164.996
	(0.124)	(0.202)	(0.236)	(0.590)	(0.373)	(0.696)	(0.097)	(0.105)	(0.143)	(1,094.687)	(540.086)	(1,216.648)
Mean village rainfall	-0.023	-0.062	-0.039	-0.106	0.075	0.181	0.061**	0.035	-0.026	-22.907	580.452**	603.359*
	(0.028)	(0.052)	(0.059)	(0.121)	(0.120)	(0.170)	(0.029)	(0.035)	(0.045)	(214.165)	(265.515)	(340.331)
Sum of all negative deviations in rainfall	-0.005	-0.022**	-0.018	0.011	0.013	0.002	0.006	-0.005	-0.011	4.054	-9.601	-13.656
	(0.008)	(0.011)	(0.013)	(0.026)	(0.011)	(0.028)	(0.007)	(0.005)	(0.009)	(66.715)	(11.407)	(67.434)
Total early life shocks	-0.012	0.002	0.014	-0.014	-0.068**	-0.054	0.012	-0.000	-0.012	27.800	-47.550	-75.350
	(0.011)	(0.010)	(0.015)	(0.036)	(0.027)	(0.045)	(0.009)	(0.007)	(0.011)	(69.447)	(32.231)	(76.307)
Total shocks during schooling years	-0.007	0.009	0.015	0.083***	0.030*	-0.053	-0.007	-0.002	0.005	-138.683**	17.780	156.463**
	(0.008)	(0.007)	(0.010)	(0.028)	(0.018)	(0.033)	(0.010)	(0.007)	(0.012)	(67.097)	(23.584)	(70.872)
Observations	1,981	2,751		1,854	2,540		1,839	2,525		1,981	2,751	
R-squared	0.142	0.213		0.252	0.305		0.083	0.091		0.295	0.258	

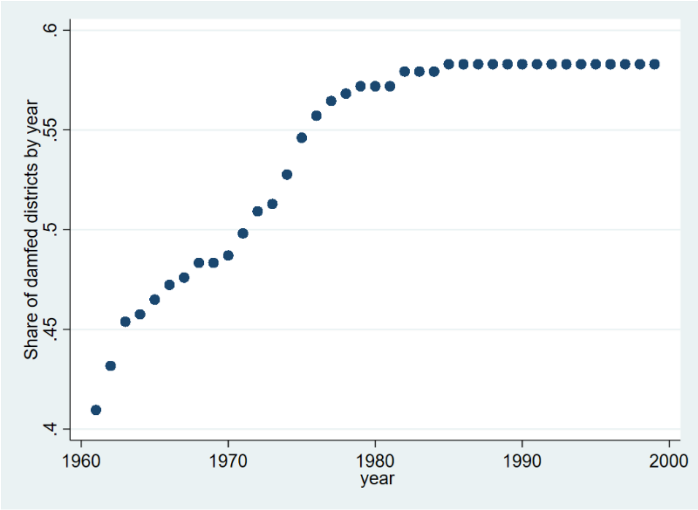
Notes: Results from estimating Equation 2 on the sample of school-age children (those aged between six and 15 years old or older). The sample is split and estimated separately for those who were born after a dam was constructed in an upstream district and those who were aged at least 15 years old or older when the upstream district dam was constructed. The dependent variables are the total years of completed schooling, the share who ever enrolled in school, the share with only primary education, the share with at least some secondary education, and the age at which schooling started for those who ever enrolled. Risk is the coefficient of variation of village rainfall in the monsoon starting month. All regressions include controls for per capita household landholdings (acres), per capita household income (Rs.), per capita household savings and assets (Rs.), household size, whether the district was included in the NREGS, as well as controls for caste (a dummy indicating low caste, whether scheduled castes/tribes or not), religion (a variable indicating whether Hindu, Muslim, or other), and fixed effects for district, year of birth, and survey year. Standard errors are clustered by village.

Sources: REDS 2006, University of Delaware Center for Climatic Research, Duflo and Pande (2007), and Evenson and McKinsey (World Bank).

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

B. Figures

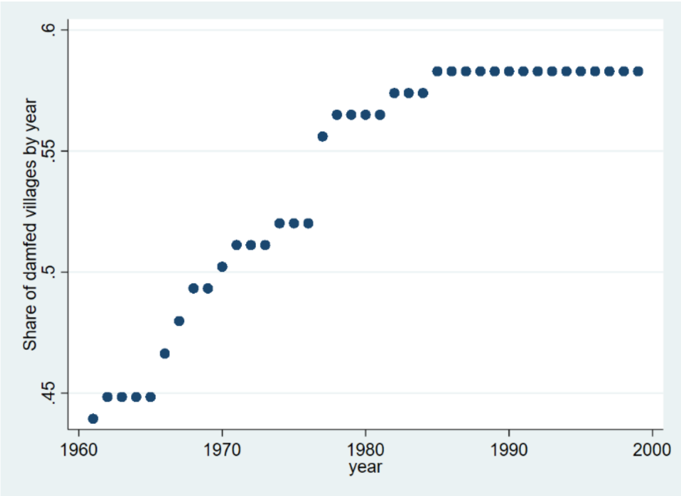
FIGURE 1 — DAM CONSTRUCTION BY DISTRICT



Notes: Share of districts with an irrigation dam constructed in an upstream district.

Sources: REDS 2006 and Duflo and Pande (2007).

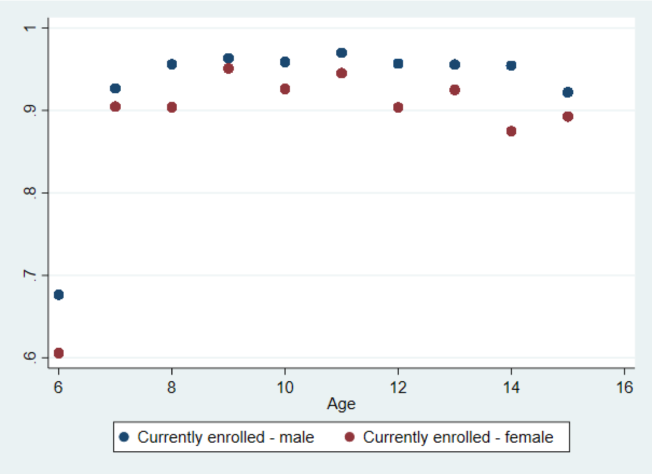
FIGURE 2 — DAM CONSTRUCTION BY VILLAGE



Notes: Share of villages with an irrigation dam constructed in an upstream district.

Sources: REDS 2006 and Duflo and Pande (2007).

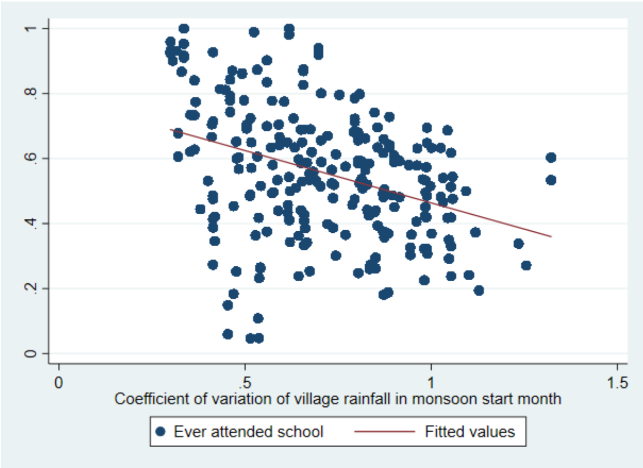
FIGURE 3 — ENROLLMENT RATES BY GENDER



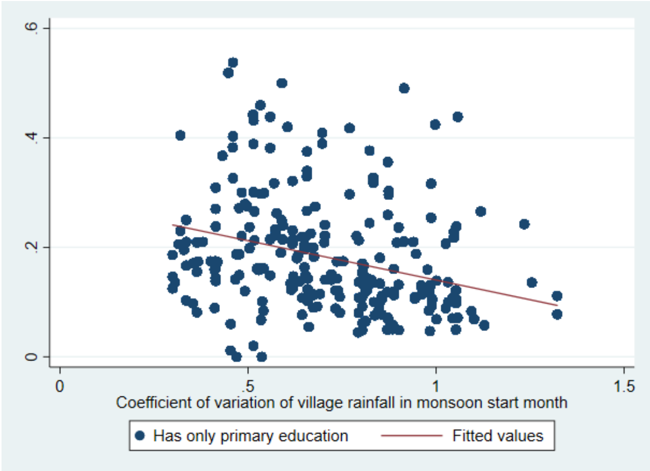
Notes: Share of school-age children (aged 6 to 15 years old) enrolled in school by age and gender.

Sources: REDS 2006.

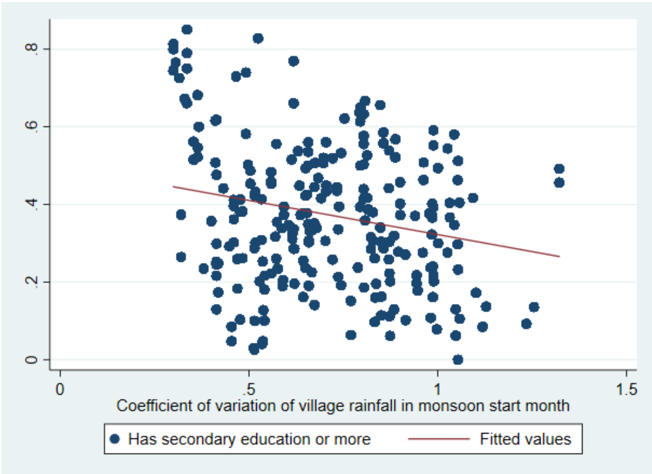
FIGURE 4 — DESCRIPTIVE EVIDENCE



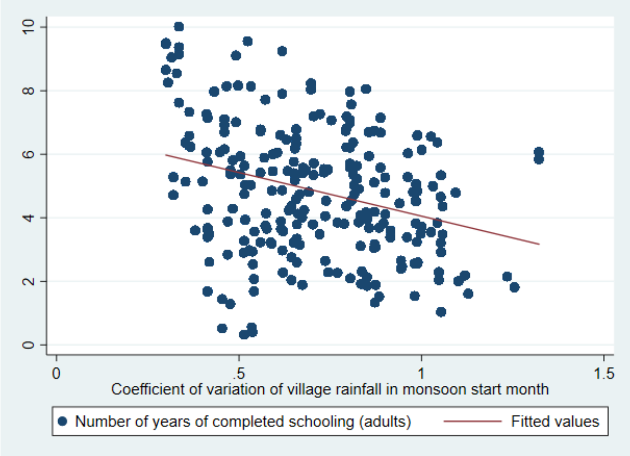
A. SHARE OF ADULTS EVER ENROLLED IN SCHOOL



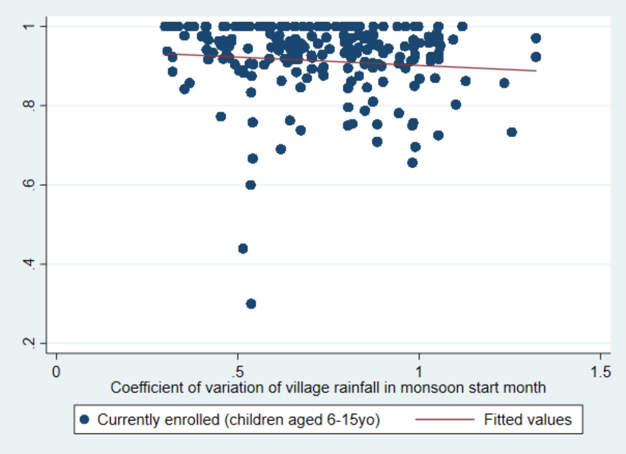
B. SHARE OF ADULTS WITH ONLY PRIMARY EDUCATION



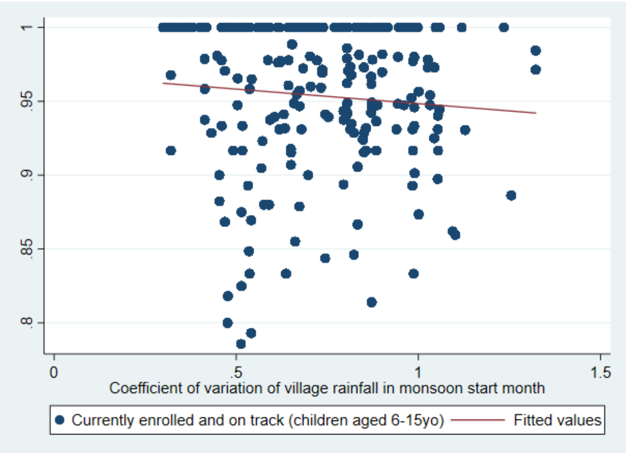
C. SHARE OF ADULTS WITH SECONDARY EDUCATION OR MORE



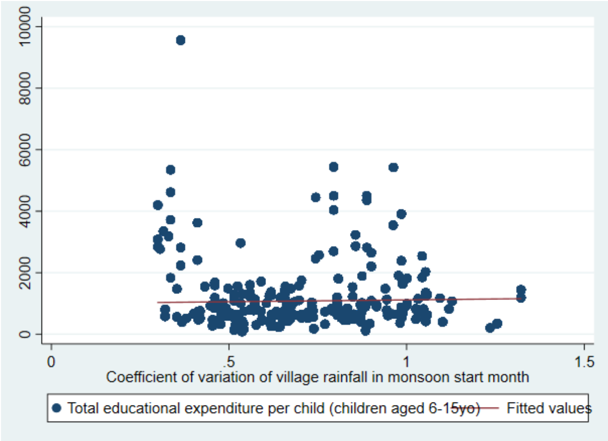
D. NUMBER OF YEARS OF COMPLETED SCHOOLING (ADULTS ONLY)



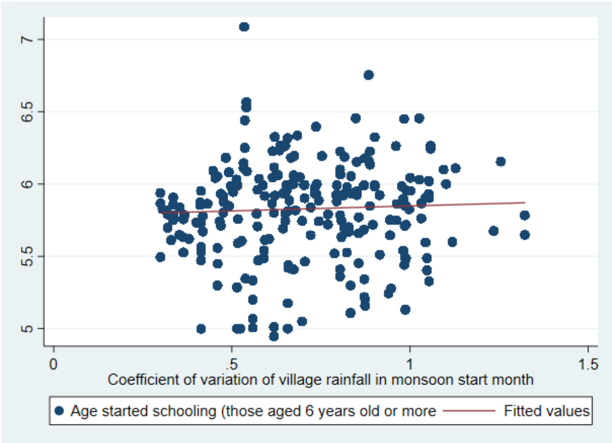
E. SHARE OF SCHOOL-AGE CHILDREN CURRENTLY ENROLLED IN SCHOOL



F. SHARE OF SCHOOL-AGE CHILDREN CURRENTLY ENROLLED AND ON TRACK IN SCHOOL



G. TOTAL EDUCATIONAL EXPENDITURE PER SCHOOL-AGE CHILD (RS.)



H. AGE WHEN STARTED SCHOOL (THOSE AGED SIX YEARS OLD OR OLDER)

Notes: Own calculations.

Sources: Sample constructed from various datasets listed in Table 1.

FIGURE 5— FORMATION OF BELIEFS ON INCOME RISK

Notes: Realized, future and expected income for a hypothetical household.

Sources: None.

C. Complementary Tables and Figures

Estimations using the predicted dam variable from Duflo and Pande (2007)

TABLE 9 — RESULTS (ALL ADULTS)

VARIABLES	Years of schooling		Diff	Ever enrolled		Diff	Primary only		Diff	Secondary +		Diff	Age started school		Diff
	(1)	(2)		(3)	(4)		(5)	(6)		(7)	(8)		(9)	(10)	
Risk	-3.223	2.410	5.633	-0.102	0.393	0.495	0.191**	0.366***	0.174	-0.287	0.038	0.325	0.206	0.226	0.020
	(2.273)	(3.050)	(3.519)	(0.218)	(0.286)	(0.326)	(0.086)	(0.126)	(0.123)	(0.218)	(0.262)	(0.319)	(0.239)	(0.231)	(0.314)
Mean village rainfall	0.030	0.604	0.574	0.057	0.017	-0.040	0.058	-0.045	-0.103*	0.000	0.070	0.070	0.087	-0.007	-0.094
	(0.895)	(1.087)	(1.393)	(0.076)	(0.096)	(0.118)	(0.045)	(0.042)	(0.057)	(0.091)	(0.100)	(0.130)	(0.071)	(0.092)	(0.103)
Sum of all negative deviations in rainfall	-0.012	-0.162	-0.150	-0.006	-0.023*	-0.017	-0.003	0.002	0.006	-0.002	-0.023**	-0.020*	0.003	-0.010	-0.013
	(0.065)	(0.102)	(0.119)	(0.006)	(0.012)	(0.014)	(0.004)	(0.006)	(0.007)	(0.006)	(0.010)	(0.011)	(0.013)	(0.008)	(0.015)
Total early life shocks	0.044	0.022	-0.022	-0.003	0.003	0.006	-0.007	-0.008	-0.001	0.004	0.012	0.008	-0.005	-0.044**	-0.040
	(0.074)	(0.112)	(0.135)	(0.007)	(0.011)	(0.013)	(0.006)	(0.010)	(0.012)	(0.007)	(0.012)	(0.014)	(0.015)	(0.019)	(0.025)
Total shocks during schooling years	0.004	0.052	0.048	-0.001	-0.003	-0.002	-0.004**	-0.006	-0.001	0.004	0.004	0.000	0.004	-0.006	-0.010
	(0.026)	(0.057)	(0.063)	(0.002)	(0.006)	(0.006)	(0.002)	(0.004)	(0.005)	(0.003)	(0.006)	(0.006)	(0.008)	(0.007)	(0.010)
Observations	13,380	4,702		13,343	4,621		13,412	4,712		13,343	4,621		6,596	3,293	
R-squared	0.285	0.205		0.259	0.189		0.088	0.104		0.221	0.183		0.258	0.234	

Notes: Results from estimating Equation 2 on the sample of adults (those aged 21 years old or older). The sample is split and estimated separately for those who were born after a dam was predicted to be constructed in an upstream district and those who were aged at least 15 years old or older when the upstream district dam was predicted to be constructed. The dependent variables are the total years of completed schooling, the share who ever enrolled in school, the share with only primary education, the share with at least some secondary education, and the age at which schooling started for those who ever enrolled. Risk is the coefficient of variation of village rainfall in the monsoon starting month. All regressions include controls for caste (a dummy indicating low caste, whether scheduled castes/tribes or not), religion (a variable indicating whether Hindu, Muslim, or other), and fixed effects for district, year of birth, and survey year. Standard errors are clustered by village.

Sources: REDS 2006, University of Delaware Center for Climatic Research, Duflo and Pande (2007), and Evenson and McKinsey (World Bank).

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

TABLE 10 — RESULTS (ALL ADULTS: FEMALE)

VARIABLES	Years of schooling		Diff	Ever enrolled		Diff	Primary only		Diff	Secondary +		Diff	Age started school		Diff
	(1)	(2)		(3)	(4)		(5)	(6)		(7)	(8)		(9)	(10)	
Risk	-1.746	1.968	3.713	-0.049	0.529**	0.578	0.009	0.414**	0.404*	-0.054	0.113	0.167	0.224	0.319	0.095
	(2.219)	(2.362)	(3.252)	(0.260)	(0.244)	(0.358)	(0.151)	(0.188)	(0.229)	(0.175)	(0.217)	(0.282)	(0.300)	(0.395)	(0.491)
Mean village rainfall	0.257	0.394	0.138	0.044	0.013	-0.030	-0.045	-0.094	-0.049	0.091	0.111	0.020	0.071	-0.030	-0.101
	(0.876)	(0.638)	(1.114)	(0.091)	(0.067)	(0.118)	(0.074)	(0.058)	(0.089)	(0.079)	(0.070)	(0.108)	(0.066)	(0.147)	(0.150)
Sum of all negative deviations in rainfall	0.067	-0.152**	-0.218**	0.004	-0.019*	-0.023*	0.003	-0.007	-0.010	0.001	-0.011	-0.012	0.008	-0.014	-0.022
	(0.072)	(0.067)	(0.098)	(0.007)	(0.010)	(0.012)	(0.005)	(0.007)	(0.009)	(0.007)	(0.008)	(0.010)	(0.020)	(0.015)	(0.025)
Total early life shocks	0.066	0.159	0.093	0.001	0.004	0.003	-0.017**	-0.013	0.004	0.018**	0.021	0.003	-0.029	-0.045	-0.017
	(0.079)	(0.145)	(0.162)	(0.009)	(0.016)	(0.018)	(0.008)	(0.016)	(0.018)	(0.008)	(0.015)	(0.017)	(0.023)	(0.030)	(0.038)
Total shocks during schooling years	0.045	-0.051	-0.096	0.003	-0.007	-0.011	-0.003	0.000	0.004	0.007**	-0.007	-0.014*	0.007	0.008	0.001
	(0.031)	(0.073)	(0.078)	(0.003)	(0.008)	(0.009)	(0.003)	(0.006)	(0.007)	(0.003)	(0.007)	(0.008)	(0.012)	(0.012)	(0.017)
Observations	6,625	2,353		6,618	2,320		6,646	2,359		6,618	2,320		2,226	1,345	
R-squared	0.392	0.280		0.373	0.249		0.126	0.127		0.297	0.242		0.300	0.257	

Notes: Results from estimating Equation 2 on the sample of adults (those aged 21 years old or older). The sample is split and estimated separately for those who were born after a dam was predicted to be constructed in an upstream district and those who were aged at least 15 years old or older when the upstream district dam was predicted to be constructed. The dependent variables are the total years of completed schooling, the share who ever enrolled in school, the share with only primary education, the share with at least some secondary education, and the age at which schooling started for those who ever enrolled. Risk is the coefficient of variation of village rainfall in the monsoon starting month. All regressions include controls for caste (a dummy indicating low caste, whether scheduled castes/tribes or not), religion (a variable indicating whether Hindu, Muslim, or other), and fixed effects for district, year of birth, and survey year. Standard errors are clustered by village.

Sources: REDS 2006, University of Delaware Center for Climatic Research, Duflo and Pande (2007), and Evenson and McKinsey (World Bank).

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

TABLE 11 — RESULTS (ALL ADULTS: MALE)

VARIABLES	Years of schooling		Diff	Ever enrolled		Diff	Primary only		Diff	Secondary +		Diff	Age started school		Diff
	(1)	(2)		(3)	(4)		(5)	(6)		(7)	(8)		(9)	(10)	
Risk	-4.338	2.849	7.187	-0.099	0.210	0.309	0.407**	0.273**	-0.134	-0.499*	-0.028	0.471	0.213	0.083	-0.130
	(2.633)	(4.250)	(4.427)	(0.219)	(0.363)	(0.364)	(0.172)	(0.122)	(0.191)	(0.296)	(0.357)	(0.416)	(0.284)	(0.191)	(0.332)
Mean village rainfall	-0.227	0.811	1.038	0.072	0.012	-0.060	0.162*	-0.000	-0.162*	-0.090	0.024	0.114	0.107	-0.010	-0.118
	(0.978)	(1.698)	(1.872)	(0.070)	(0.143)	(0.150)	(0.085)	(0.053)	(0.098)	(0.128)	(0.153)	(0.183)	(0.088)	(0.085)	(0.117)
Sum of all negative deviations in rainfall	-0.060	-0.135	-0.075	-0.011	-0.024	-0.013	-0.005	0.012	0.017*	-0.005	-0.032**	-0.027*	-0.005	-0.010	-0.004
	(0.081)	(0.150)	(0.166)	(0.008)	(0.017)	(0.018)	(0.006)	(0.008)	(0.010)	(0.008)	(0.012)	(0.014)	(0.015)	(0.009)	(0.017)
Total early life shocks	0.096	-0.073	-0.169	0.001	0.002	0.001	0.005	-0.008	-0.013	-0.004	0.011	0.015	0.010	-0.044*	-0.053
	(0.100)	(0.190)	(0.216)	(0.009)	(0.014)	(0.017)	(0.008)	(0.013)	(0.015)	(0.010)	(0.019)	(0.021)	(0.018)	(0.026)	(0.033)
Total shocks during schooling years	-0.022	0.153**	0.175**	-0.003	0.003	0.006	-0.005	-0.010	-0.005	0.002	0.014*	0.012	0.002	-0.016*	-0.018
	(0.032)	(0.068)	(0.077)	(0.003)	(0.006)	(0.006)	(0.003)	(0.006)	(0.007)	(0.004)	(0.007)	(0.008)	(0.008)	(0.009)	(0.012)
Observations	6,755	2,349		6,725	2,301		6,766	2,353		6,725	2,301		4,370	1,948	
R-squared	0.305	0.230		0.252	0.234		0.124	0.139		0.258	0.206		0.259	0.247	

Notes: Results from estimating Equation 2 on the sample of adults (those aged 21 years old or older). The sample is split and estimated separately for those who were born after a dam was predicted to be constructed in an upstream district and those who were aged at least 15 years old or older when the upstream district dam was predicted to be constructed. The dependent variables are the total years of completed schooling, the share who ever enrolled in school, the share with only primary education, the share with at least some secondary education, and the age at which schooling started for those who ever enrolled. Risk is the coefficient of variation of village rainfall in the monsoon starting month. All regressions include controls for caste (a dummy indicating low caste, whether scheduled castes/tribes or not), religion (a variable indicating whether Hindu, Muslim, or other), and fixed effects for district, year of birth, and survey year. Standard errors are clustered by village.

Sources: REDS 2006, University of Delaware Center for Climatic Research, Duflo and Pande (2007), and Evenson and McKinsey (World Bank).

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

TABLE 12 — RESULTS (ALL SCHOOL-AGE CHILDREN)

VARIABLES	Currently enrolled		Diff	Age started school		Diff	On track		Diff	Total educational exp		Diff
	(1)	(2)		(3)	(4)		(5)	(6)		(7)	(8)	
Risk	0.136	0.002	-0.134	0.298	0.630	0.331	0.015	-0.093	-0.109	-787.798	-49.666	738.132
	(0.134)	(0.167)	(0.214)	(0.436)	(0.411)	(0.597)	(0.098)	(0.077)	(0.125)	(820.473)	(430.644)	(923.936)
Mean village rainfall	0.042	-0.003	-0.044	-0.069	0.137	0.206	0.038	0.053*	0.015	-110.545	464.808**	575.353**
	(0.035)	(0.040)	(0.053)	(0.107)	(0.103)	(0.148)	(0.024)	(0.027)	(0.036)	(187.130)	(193.768)	(268.716)
Sum of all negative deviations in rainfall	0.012*	-0.020*	-0.032**	0.005	0.021**	0.016	0.001	-0.003	-0.003	-8.553	-28.922**	-20.369
	(0.007)	(0.012)	(0.013)	(0.022)	(0.009)	(0.024)	(0.006)	(0.004)	(0.007)	(39.591)	(11.307)	(41.046)
Total early life shocks	-0.000	0.006	0.006	-0.001	-0.047**	-0.046	0.003	-0.002	-0.005	14.739	-20.339	-35.078
	(0.008)	(0.010)	(0.012)	(0.028)	(0.022)	(0.035)	(0.007)	(0.007)	(0.009)	(38.949)	(27.484)	(47.540)
Total shocks during schooling years	-0.003	0.008	0.011	0.068**	0.034**	-0.034	-0.008	-0.002	0.005	-61.368	17.614	78.982
	(0.007)	(0.007)	(0.010)	(0.028)	(0.014)	(0.031)	(0.007)	(0.007)	(0.010)	(45.604)	(22.387)	(50.654)
Observations	3,810	4,886		3,449	4,448		3,433	4,414		3,810	4,886	
R-squared	0.143	0.208		0.251	0.268		0.105	0.084		0.280	0.226	

Notes: Results from estimating Equation 2 on the sample of school-age children (those aged between six and 15 years old or older). The sample is split and estimated separately for those who were born after a dam was predicted to be constructed in an upstream district and those who were aged at least 15 years old or older when the upstream district dam was predicted to be constructed. The dependent variables are the total years of completed schooling, the share who ever enrolled in school, the share with only primary education, the share with at least some secondary education, and the age at which schooling started for those who ever enrolled. Risk is the coefficient of variation of village rainfall in the monsoon starting month. All regressions include controls for per capita household landholdings (acres), per capita household income (Rs.), per capita household savings and assets (Rs.), household size, whether the district was included in the NREGS, as well as controls for caste (a dummy indicating low caste, whether scheduled castes/tribes or not), religion (a variable indicating whether Hindu, Muslim, or other), and fixed effects for district, year of birth, and survey year. Standard errors are clustered by village.

Sources: REDS 2006, University of Delaware Center for Climatic Research, Duflo and Pande (2007), and Evenson and McKinsey (World Bank).

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

TABLE 13 — RESULTS (ALL SCHOOL-AGE CHILDREN: FEMALE)

VARIABLES	**Currently enrolled**		**Diff**	**Age started school**		**Diff**	**On track**		**Diff**	**Total educational exp**		**Diff**
	(1)	(2)		(3)	(4)		(5)	(6)		(7)	(8)	
Risk	0.269	0.171	-0.098	-0.015	0.957*	0.973	-0.157	-0.118	0.039	-1,466.727	-287.882	1,178.845
	(0.223)	(0.196)	(0.296)	(0.702)	(0.553)	(0.891)	(0.185)	(0.097)	(0.209)	(897.858)	(426.566)	(991.255)
Mean village rainfall	0.111*	0.052	-0.060	-0.053	0.158	0.211	0.022	0.062	0.040	-19.119	354.904**	374.024
	(0.059)	(0.045)	(0.074)	(0.139)	(0.130)	(0.190)	(0.035)	(0.038)	(0.052)	(267.372)	(171.479)	(316.791)
Sum of all negative deviations in rainfall	0.035***	-0.021	-0.056***	0.007	0.018	0.011	-0.004	0.000	0.004	-31.606	-46.861***	-15.254
	(0.010)	(0.014)	(0.017)	(0.034)	(0.014)	(0.037)	(0.010)	(0.004)	(0.010)	(41.837)	(13.822)	(43.933)
Total early life shocks	0.011	0.015	0.003	0.030	-0.025	-0.055	0.002	-0.007	-0.009	48.521	-3.100	-51.621
	(0.014)	(0.013)	(0.019)	(0.041)	(0.033)	(0.053)	(0.010)	(0.011)	(0.015)	(56.298)	(38.873)	(68.236)
Total shocks during schooling years	0.006	0.009	0.003	0.042	0.032	-0.009	-0.014	-0.003	0.011	-17.496	26.991	44.486
	(0.011)	(0.009)	(0.014)	(0.041)	(0.023)	(0.047)	(0.010)	(0.008)	(0.013)	(58.627)	(27.685)	(64.653)
Observations	1,771	2,208		1,551	1,967		1,547	1,951		1,771	2,208	
R-squared	0.189	0.222		0.273	0.271		0.155	0.144		0.281	0.252	

Notes: Results from estimating Equation 2 on the sample of school-age children (those aged between six and 15 years old or older). The sample is split and estimated separately for those who were born after a dam was predicted to be constructed in an upstream district and those who were aged at least 15 years old or older when the upstream district dam was predicted to be constructed. The dependent variables are the total years of completed schooling, the share who ever enrolled in school, the share with only primary education, the share with at least some secondary education, and the age at which schooling started for those who ever enrolled. Risk is the coefficient of variation of village rainfall in the monsoon starting month. All regressions include controls for per capita household landholdings (acres), per capita household income (Rs.), per capita household savings and assets (Rs.), household size, whether the district was included in the NREGS, as well as controls for caste (a dummy indicating low caste, whether scheduled castes/tribes or not), religion (a variable indicating whether Hindu, Muslim, or other), and fixed effects for district, year of birth, and survey year. Standard errors are clustered by village.

Sources: REDS 2006, University of Delaware Center for Climatic Research, Duflo and Pande (2007), and Evenson and McKinsey (World Bank).

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

TABLE 14 — RESULTS (ALL SCHOOL-AGE CHILDREN: MALE)

VARIABLES	Currently enrolled		Diff	Age started school		Diff	On track		Diff	Total educational exp		Diff
	(1)	(2)		(3)	(4)		(5)	(6)		(7)	(8)	
Risk	0.121	-0.208	-0.329	0.612	0.337	-0.275	0.191*	-0.073	-0.264*	-300.826	224.184	525.010
	(0.124)	(0.186)	(0.223)	(0.625)	(0.371)	(0.724)	(0.109)	(0.095)	(0.144)	(1,180.280)	(546.045)	(1,296.399)
Mean village rainfall	-0.009	-0.063	-0.053	-0.090	0.107	0.196	0.052*	0.051	-0.001	-54.070	560.721**	614.791*
	(0.028)	(0.049)	(0.056)	(0.124)	(0.122)	(0.174)	(0.026)	(0.039)	(0.047)	(215.281)	(259.097)	(336.095)
Sum of all negative deviations in rainfall	-0.004	-0.022*	-0.017	-0.007	0.020**	0.028	0.007	-0.005	-0.013	10.984	-13.303	-24.287
	(0.008)	(0.011)	(0.014)	(0.027)	(0.010)	(0.029)	(0.007)	(0.005)	(0.009)	(63.126)	(15.653)	(64.818)
Total early life shocks	-0.006	-0.004	0.003	-0.030	-0.060**	-0.029	0.008	-0.001	-0.009	15.711	-41.134	-56.846
	(0.010)	(0.010)	(0.014)	(0.035)	(0.027)	(0.044)	(0.007)	(0.008)	(0.011)	(58.919)	(36.337)	(69.021)
Total shocks during schooling years	-0.008	0.009	0.017*	0.076**	0.033*	-0.044	-0.001	-0.003	-0.002	-104.791	8.342	113.133
	(0.008)	(0.007)	(0.010)	(0.031)	(0.019)	(0.036)	(0.008)	(0.008)	(0.011)	(64.172)	(27.127)	(69.449)
Observations	2,039	2,678		1,898	2,481		1,886	2,463		2,039	2,678	
R-squared	0.137	0.222		0.265	0.284		0.104	0.076		0.309	0.241	

Notes: Results from estimating Equation 2 on the sample of school-age children (those aged between six and 15 years old or older. The sample is split and estimated separately for those who were born after a dam was predicted to be constructed in an upstream district and those who were aged at least 15 years old or older when the upstream district dam was predicted to be constructed. The dependent variables are the total years of completed schooling, the share who ever enrolled in school, the share with only primary education, the share with at least some secondary education, and the age at which schooling started for those who ever enrolled. Risk is the coefficient of variation of village rainfall in the monsoon starting month. All regressions include controls for per capita household landholdings (acres), per capita household income (Rs.), per capita household savings and assets (Rs.), household size, whether the district was included in the NREGS, as well as controls for caste (a dummy indicating low caste, whether scheduled castes/tribes or not), religion (a variable indicating whether Hindu, Muslim, or other), and fixed effects for district, year of birth, and survey year. Standard errors are clustered by village.

Sources: REDS 2006, University of Delaware Center for Climatic Research, Duflo and Pande (2007), and Evenson and McKinsey (World Bank).

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

Chapter Two

Mothers at Work: How Mandating a Short Maternity Leave Affects Work and Fertility *

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Abstract

Switzerland mandated a 14-week paid maternity leave in 2005 when many firms already offered a similar benefit. While the mandate had only small and temporary effects on labor market outcomes of first-time mothers, it raised the share of those having a second child by three percentage points. Women employed in firms with prior paid leave sharply increased their subsequent fertility. In contrast, women employed in other firms did not change their fertility behaviour, but instead saw a persistent increase in their earnings after birth. This pattern of results suggests that firms with pre-mandate leave passed on (some of) their resulting cost-savings to their employees - "trickle down effects" - by making their maternity leave more generous than mandated, hiring temporary replacement workers and/or supporting mothers' return to work in other ways.

Keywords: Female labor supply, maternity leave, return-to-work, earnings, fertility.

JEL Categories: J13, J22, J78

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1 Introduction

Over the past century, women's labor force participation rates in high-income countries have increased substantially. This trend paralleled the adoption of many family friendly policies, among which paid maternity leave played a key role (Olivetti and Petrongolo, 2017). By the late 20th century, most high-income countries had adopted national mandates for paid maternity leave (Rossin-Slater, 2017). In contrast, Australia, New Zealand, Switzerland, and the United Kingdom introduced such mandates only at the dawn of the 21st century. In 2022, the United States remains the only OECD country without a federal provision for paid maternity leave, and even today paid maternity leave is not universally available to self-employed, domestic, part-time and casual workers in some other OECD countries.[1]

Family leave policies aim to help parents of young children reconcile the demands of work and family. Considering their large costs to taxpayers and firms, a better understanding of how a short paid leave could support mothers continue participating in the labor market is important. A rich literature documents the impact of these policies on female labor market outcomes, child outcomes, and fertility. However, most of these papers study the effects of *extensions in the duration* of existing family policies[2], and focus on labor market outcomes, while only a few study effects on fertility.[3] Therefore, we lack understanding of the value of paid maternity leave in the period right after birth, especially if the value of maternity leave varies substantially in the very early months of a child's life. A too short leave right after birth may not help meet working parents' needs, whereas, a somewhat longer leave could be highly valuable, especially if the leave covers the period when finding alternative modes of care is very challenging.[4]

This paper studies the dynamic impact that introducing the first federal paid maternity leave had on women's labor market outcomes around the birth of their first child and on their subsequent fertility in Switzerland. The mandate became effective from 1st July 2005 and provided 14 weeks of maternity

[1] In some countries, such as Japan, maternity leave is available as insurance through the employer. In Japan, only regular workers qualify for this insurance, that is, full-time employees with permanent contracts who are covered by the social insurance programs (see Asai (2015)).

[2] Studies on extensions of parental (mostly maternity) leave include: Austria (Lalive et al. (2013)), Germany (Ruhm (1998), Kluve and Tamm (2013), Schönberg and Ludsteck (2014), and Geyer et al. (2015)), Scandinavian countries (Ruhm (1998), and Dahl et al. (2016)), Czech Republic (Bicakova and Kaliskova (2019)), Japan (Asai (2015) and Yamaguchi (2019)), and Canada (Hanratty and Trzcinski (2009)), among others.

[3] The impact of an introduction of a short paid family leave on employment (and sometimes earnings) in California is studied by Rossin-Slater et al. (2013), Baum and Ruhm (2016), and Byker (2016)), in New Jersey by Byker (2016), and in Australia by Broadway et al. (2020). Only Baum and Ruhm (2016) and Byker (2016) analyze the anticipatory effects of these policies. None of these articles investigate the impact on subsequent fertility.

[4] Rossin-Slater (2017) discusses different estimated effects of short (i.e. less than one year) versus long maternity leaves on labor market outcomes across countries. Carneiro et al. (2015) use a similar argument to explain their findings of why a 4-month paid maternity leave introduction had significant (positive) impact on child outcomes in Norway while the previous literature had not found any effects. Parental leave taken by mothers plays a role for children's outcomes (Ginja et al., 2020), but leave taken by fathers does not improve the long-run gender balance in housework (Ekberg et al., 2013).

leave benefits and job protection during pregnancy and the 16-week period following birth. Before the mandate was introduced, around 40% of employers already offered their female workforce access to paid maternity leave, but such leave was not universal and leave provisions differed enormously (see Guillet et al. (2016) and Aeppli (2012)). The mandate aimed to provide a minimum level of paid maternity leave to all eligible women and thereby, reduce inequalities in coverage.

Studying the Swiss mandate is interesting for several reasons. First, the total leave duration is short, and since there is no other parental leave mandate in Switzerland, the maternity leave mandate constitutes all such publicly provided leave.[5] However, the benefit level is fairly generous at 80% of previous earnings for most women. Most mandates in other European countries are longer and sometimes even more generous.[6] Second, the mandate was implemented in a context where about four out of ten firms already offered paid maternity leave. Thus, the mandate led to two different changes. It *introduced a short, paid maternity leave* in firms that did not offer this benefit, while it *reduced costs* for firms and public administrations that already provided a similar benefit prior to the reform. Comparing the effects of the mandate across firms with and without prior leave, we provide evidence on these heterogenous effects. Firms offering prior paid leave that is now suerpseded by a publicly-financed scheme could pass on their resulting cost-savings to their employees in the form of *support on the job* or *expansion of family policies* over and above the mandated minimum, suggesting possible "trickle-down effects". Third, the Swiss labor labor market is characterized by high rates of part-time employment among mothers, indicating problems in reconciling the demands of work and family. We study complementarity of the maternity leave mandate with another family policy, *early child care*, by exploiting regional differences in the availability of early child care. Finally, the timing of the announcement of the new mandate and its implementation enable us to study both the *anticipatory and treatment effects*.

For our analysis, we compile a unique and rich dataset by linking several administrative registers. These include the social security register, which provides information on earnings and social security benefits, the vital statistics register, which provides information on life events, and the census. Our main population of interest are Swiss women who gave birth to their first child shortly before and after the mandate was introduced on 1st July 2005. We construct a dataset of women's complete labor market and fertility histories at a monthly frequency starting from five years before birth to nine years after birth. We employ a difference-in-differences approach where we compare the difference

[5] Switzerland introduced two weeks of paternity leave in 2021.

[6] Note that European mandates are among the most generous worldwide. Anglo-Saxon countries like Australia, Canada, Ireland, New Zealand and the UK offer a benefit level which is similar to Switzerland or even lower (see indicator PF 2.4 in the OECD Family database OECD, Social Policy Division - Directorate of Employment, Labour and Social Affairs (2017)).

in outcomes of women who gave birth to their first child in the three months before and after the introduction of the mandate in 2005, with the difference in a control cohort of women who gave birth in the same three-month windows in the year prior to the reform. This identification strategy allows us to estimate the causal effects of being covered by the mandate around the time of birth of the first child.[7] By including in our analysis pre-birth periods, we examine behavioral responses in anticipation of the mandate. We also investigate the heterogeneous effects of the mandate by the availability of early child care in the mother's state (or canton) of residence at the time of birth of her first child and across firms with and without pre-mandate leave. As pre-mandate leave availability is not directly observed in the data, we identify firms with prior leave using information on the pre-mandate incidence of significant positive earnings immediately after birth when women are not allowed to work by law.

Our empirical findings can be summarized as follows. First, our results reveal no or only small effects on most labor market outcomes. We do find increased job continuity with the pre-birth employer in the two to three years after birth but little or no effects on employment rates. In the long run, up to five years after birth, all labor market effects dissipate. Second, our estimates uncover sizeable anticipatory responses by women covered by the mandate at the intensive margin of labor supply. That is, the earnings of these women increase compared to the control group prior to the birth of their first child, reflecting a relative increase, or a smaller decrease, in the hours worked prior to birth. Third, we find a significant and persistent impact of the maternity leave mandate on subsequent fertility. An additional three out of 100 women exposed to the mandate gave birth to a second child in the long run, that is, in the nine years after the birth of their first child.[8] Fourth, the mandate allows women to reconcile demands of work with those of family life: it raises the proportion of women who care for a young child while working a job with similar earnings as prior to birth.

The effects of the mandate on fertility differs across regions with different levels of early child care availability. The mandate increases subsequent fertility in regions that offer above-median slots in early child care by four percentage points, but does not have a statistically significant impact in regions with below-median slots in early child care. This evidence suggests complementarity between the maternity leave mandate and the availability of early child care leading to the effect we see on subsequent fertility.

Women who benefitted from paid maternity leave for the first time see improved labor market outcomes after returning from leave. Their employment rate increases slightly (albeit not statistically)

[7] A similar approach was used by Lalive et al. (2013) and Schönberg and Ludsteck (2014) to study long expansions of maternity leave in Austria and Germany. Lalive et al. (2013) study two Austrian reforms that extended maternity leave durations from 12 to 24 and then to 30 months, while Schönberg and Ludsteck (2014) study German reforms extending the benefit duration from two to six months, and later up to 24 months.

[8] This result is in line with the findings of Barbos and Milovanska-Farrington (2019) that the 2005 mandatory paid maternity leave in Switzerland affected fertility intentions through an experience effect.

between 18 to 30 months after the birth of their first child. Moreover, their monthly earnings after birth increase by almost 300 Swiss francs. This corresponds to a 6% increase compared to the median pre-birth earnings and persists in the long run. After five years, these women have earned 16,000 Swiss francs more as a result of the mandate (i.e. 3.6 months of their pre-birth earnings). Introducting the mandated minimum thus boosts post-birth earnings, but does not affect subsequent fertility. Surprisingly, women employed in firms that offered paid maternity leave prior to the mandate also reacted sharply to the reform, yet in a very different way. Five in 100 of these women give birth to an additional child and subsequent fertility remains persistently higher throughout our observation period. At the same time, the mandate only has a small overall financial impact, which dissipates after two years, while improvements in work-life balance are strong: job continuity increases signifcantly after birth and more women combine caring for a young child aged less than two years with earning around the same as prior to the first birth. Firms with prior leave available used some of the funds freed up by the mandate to expand family policies beyond the mandated minimum and made other adjustments to help women better balance work and family commitments.

This pattern of evidence suggests that the value of maternity leave, in terms of improving work-life balance, evolves non-linearly throughout the "duration" of maternity leave. Women working in firms that did not have prior leave experience improvements in the “work”-side of the work-life balance. They have higher earnings after returning from maternity leave, indicating that they are working a greater number of hours. Whereas women working in firms with prior leave see improvements in the “family life”-side since more of them go on to have a second child. Many of these firms implemented additional policies such as extended leave, higher maternity leave benefits, temporary replacement worker arrangements, and employer-provided child care - the trickle-down effects from the cost-savings resulting from the mandate - which all support the return to work of new mothers and reduce the costs of having additional children.[9]

Our paper ties into a growing literature on the effects of maternity leave on female labor market outcomes and fertility in developed countries. A large part of the literature has investigated the impact of parental leave policies on female labor market outcomes (for excellent recent reviews, see Rossin-Slater (2017) and Olivetti and Petrongolo (2017)), while fertility has received less attention.[10]

[9] We note that women working for firms with prior leave and without prior leave are not quite comparable and were differently affected by the reform. Comparing the estimated causal effects of the mandate across the two groups therefore remains challenging.

[10] Some papers investigating the effect of maternity leave reforms on fertility include Lalive and Zweimüller (2009) for Austria, Dahl et al. (2016) for Norway, Malkova (2018) for Soviet Russia, Golightly and Meyerhofer (2021) for California and Cygan-Rehm (2016) and Raute (2019) for differential effects on earnings subgroups in Germany. All of these papers analyse (extensions of) relatively long (i.e. more than six months) maternity leave provisions. Studies on the effect of parental leave reforms on fertility *intentions* include Bassford and Fisher (2020) for Australia and Barbos and Milovanska-Farrington (2019) for Switzerland.

Analysing the recent Swiss mandate extends our understanding of how a short maternity leave affects work and fertility of mothers in four important ways.

First, we include pre-birth labor market outcomes in our analysis to gauge if anticipatory behavioral effects are present and to determine their quantitative importance. Our results indeed reveal sizeable adjustments at the intensive margin of labor supply before birth. Such behavioral adjustments are likely to occur for other parental leave reforms as well (unless such reforms are announced very late or implemented ex-post) and should be taken into account when quantifying the overall effects of such reforms.[11]

Second, our paper sheds light on the heterogeneous effects of a universal maternity leave mandate that supersedes prior employer-provided maternity leave for a subset of women. While these women were not directly affected by the mandate, since they had already been covered by employer-provided maternity leave, their employers see their costs of providing maternity leave reduced. This can in turn trickle down to female workers through extended maternity leave provisions, increased job continuity, more flexible work options, and employer provided child care. Our results highlight that such "trickle down" effects can be large and affect different outcomes than the direct effects of introducing a short paid leave.

Third, our analysis also encompasses the effect of the maternity leave mandate on subsequent fertility. While labor market effects of a short maternity leave reform could be temporary and limited, this does not preclude sizeable and long-lasting impacts on subsequent fertility decisions as our findings show.

Finally, our paper contributes to improve our understanding of how different family policies, such as parental leave and provision of child care places or child care subsidies, interact. As highlighted by Olivetti and Petrongolo (2017), family policies should not be analyzed in isolation, since the impact of a paid maternity leave could be determined not only by the duration and level of benefits, but also by the cost and availability of child care when the leave ends. Our heterogeneity analysis provides suggestive empirical evidence of such a complementarity between a short paid maternity leave mandate and higher availability of child care for younger children, at least in the subsequent fertility dimension and - to a lesser extent - in terms of post-birth earnings.[12]

[11]Sizeable anticipatory effects have also been documented for welfare reforms (Blundell et al. (2011)), tort reforms (Malani and Reif (2015)) and health care reforms (Alpert (2016)).

[12]Danzer et al. (2020) analyse the impact of the Australian maternity leave expansion in the 1990s from one to two years on children's outcomes, maternal labor market outcomes and fertility in communities with and without formal childcare. They find evidence of an initially larger decrease followed by no effect on maternal full-time employment in communities with formal childcare compared to communities without formal care, but a slightly larger family size in the former communities. Ravazzini (2018) investigates how expansions in child care from 2002 to 2009 affect maternal full-time and part-time employment. She uses variations in the implementation of paid maternity leave for public sector employees in Switzerland as a proxy for maternity leave availability. She does not find any medium-term labor market effect of the 2005 mandate on maternal employment. Kleven et al. (2020) estimate the joint effect of parental leave and

These four important findings warrant further attention from researchers and should inform policy makers on how to shape family policies to help women better reconcile the demands of work and family in the future.

2 Institutional Background and Possible Mechanisms

2.1 Institutional Background

While Switzerland was among the first countries in the world to mandate (unpaid) leave from work for women giving birth, it was not until July 2005 that it implemented a federal mandate providing for paid maternity leave with job protection.[13] Since 1877, women in Switzerland were forbidden to work for eight weeks around the time of birth of their child. While this leave was unpaid, their jobs remained protected during this period. A federal mandate adopted in 1945 requested the government to implement some form of paid maternity leave. Subsequently, job protection during pregnancy and 16 weeks following birth, as well as a wage payment during at least 3 weeks after birth were introduced in 1989.

In Switzerland, national referenda are usually held in order to pass contested new federal legislation. Several referenda on paid maternity leave were held between 1945 and 2000, but all of them failed.[14] The canton of Geneva implemented its own paid maternity leave mandate with job protection on 1st July 2001. A new federal initiative for maternity leave was launched in June 2001 and passed parliamentary approval in October 2003. However, one major party opposed it and called for a federal referendum in January 2004. The referendum vote was held on 26th September 2004 and gained 55.4% of votes in favor of the maternity leave mandate. At this time, the implementation date of the new mandate was not yet known. On 24th November 2004, the federal council announced that the new maternity leave mandate - officially titled in French *Loi sur les Allocations pour Perte de Gains* (LAPG) - would become effective on 1st July 2005.

The mandate provides women with 14 weeks (98 days) of paid maternity leave beginning at the birth of the child. It also ensures job protection against dismissal during pregnancy and in the first 16 weeks after birth. The maternity benefits are set at 80% of average labor earnings (including from self-employment) prior to birth, subject to a daily cap. At the time of the mandate's introduction, the cap amounted to 172 CHF per day or 5,160 CHF per month.[15] The benefits are financed through

child care subsidies for several policies reforms in Austria since the 1950s on the gender earnings gap. They find virtually no effect of either policy on gender earnings gap convergence.

[13] See the OECD Family data base on oe.cd/fdb and the PF2.5 Annex accessed on 5/02/2021 here: https://www.oecd.org/els/family/PF2_5_Trends_in_leave_entitlements_around_childbirth_Annex.pdf.

[14] The last unsuccessful referendum on paid maternity leave was held in 1999, which failed to pass with 61.1% voting against.

[15] Hence, women with average monthly pre-birth earnings above 6,450 CHF would see their maternity leave benefits

employee and employer contributions similar to other existing social insurance schemes. The mandate fully covers all women who had a child on or after 1st July 2005 subject to meeting certain employment eligibility requirements. Women can request for a two-week extension after the end of the mandated 98 days, which, on account of the post-birth 16-week job protection period, is rarely refused by the employer. However, the employer is not required by the mandate to pay wages for these two extra weeks of leave.

In order to qualify for paid maternity leave, women need to: (1) have worked and contributed to social security for nine months in total before the birth; (2) have worked for at least five months during the nine months before birth, that is, during the pregnancy; and (3) be employed at the time of birth. Or alternatively, they need to have been receiving unemployment benefits during the pregnancy for an equivalent period and be officially unemployed at the time of birth.[16]

A majority of women, mainly employees in federal and cantonal public administrations, all women working in Geneva, as well as a considerable share of women working in the private sector (mostly in large firms and the banking/IT/insurance/consulting sector) had access to some form of employer-provided paid maternity leave prior to the implementation of the federal maternity leave mandate on 1st July 2005 (Guillet et al., 2016; Aeppli, 2012).[17] Eligibility for many of these employer-sponsored maternity leave insurance schemes was tied to tenure with the same employer, sometimes requiring up to nine years of tenure to become eligible for full, that is, three months of paid maternity leave. This practice disadvantaged younger women, those with frequent job changes, and those working in small and medium sized firms, which often did not offer paid maternity leave.

After the adoption of the new mandate, cantonal legislations and employer arrangements had to meet at least the federal standards, but those that were more generous such as that of Geneva remained in force.[18] Moreover, the federally guaranteed maternity leave was now paid by the federal government, and hence, it freed up the considerable funds used to pay for maternity leave arrangements covered by employers prior to the adoption of the federal mandate.[19] How firms used the freed up funds is

capped at 5,160 CHF (unless their employer paid the difference). In 2009, the cap was increased from 172 to 196 CHF per day. On 30th June 2005, 1 CHF corresponded to 0.79 USD.

[16]Every woman who met the eligibility criteria and had a child in the 98 days before the mandate came into effect, that is, they gave birth between 25 March and 30 June 2005, received *partial* benefits. They would receive benefits from the 1st July 2005 for the remaining number of days of the 14-week maternity leave period. Therefore, their maternity leave benefits lasted from one to 97 days. We define these women as *partially treated*. We do not include first-time mothers who gave birth between 1st April and 30 June 2005 in our main analysis.

[17]While most of these private schemes were at least as generous as the federal mandate in terms of the benefit level (i.e., 80% of previous earnings or more), a third offered a maternity leave payment duration of less than 14 weeks, which is the federally mandated duration.

[18]The Geneva legislation provides for 16 weeks (112 days) of paid maternity leave. The maternity benefits are at 80% of previous average earnings, subject to a minimum of 62 CHF per day and a maximum of 237 CHF in 2005, which was higher than the maximum level of federal benefits at the time (172 CHF).

[19]Estimates suggest that employers annually incurred maternity leave expenditures of 353 million CHF prior to the votation, while the total cost of the maternity leave mandate implementation for the government was expected to be 483 million CHF (Bundeskanzlei, 2004).

critical for interpreting the estimates we report below. Unfortunately, no administrative data source provides detailed insights on how firms that provided paid maternity leave before the mandate used the funds that were freed up. A survey in 2011 of 402 firms suggests that 33% of firms used these funds to support families (through longer maternity leaves, paternity leave, child care, etc.), 20% hired a replacement worker, and the remaining firms did not use the funds in a particular way or did not answer the question (Aeppli, 2012).

2.2 Discussion of Mechanism and Motivation of Outcome Variables

Mandated benefits will, on average, increase *incomes* of women with newborn children after the policy change for the duration of the mandated leave (14 weeks).[20] This increase will be substantial for those women who were not covered by paid employer provided ML benefits before the policy change. The mandate will not directly affect incomes of women who are already covered by paid leave through the previous employer, except for those with prior coverage below the mandated leave or where the employer extends the previous leave scheme further. The previous employer will, however, benefit from the transfer and possibly use this transfer to finance longer maternity leaves or improvements to the jobs held by women returning from maternity leave.[21]

Introducing paid maternity leave (ML) has consequences on behavior before and after giving birth (*outcome variables* in *italics*). Prior to the mandate, some women tended to reduce employment and hours already before giving birth. With the introduction of the paid ML, women will increase (or decrease less) employment upon learning that they are pregnant to meet the employment requirement for ML before childbirth. Moreover, women will possibly increase hours to accumulate higher average *earnings* compared to the situation without paid ML because the marginal benefit of working an extra hour increases, as higher average prior earnings raise the ML benefit. We observe *employment* at the extensive margin, and *employment earnings*, which reflects both hours – the intensive margin of labor supply – but also wages. We denote these pre-birth effects as anticipation effects.

Paid ML could reduce post-birth labor market *participation* of women, through an income effect, or increase it through job protection (Lalive et al., 2013). But since job protection was already available

[20] Maternity leave (ML) offers job protection and benefits to all eligible women. Job protection was already available under previous regulations, but benefits were not mandatory before the policy change in 2005 and were only available to those women employed in firms offering employer-funded maternity leave with varying lengths, eligibility criteria and levels of benefits. The benefits under the federal mandate became proportional to average earnings prior to birth (up to a cap), and conditional on an active employment history.

[21] Mandated employer provided ML is costly to firms and it can lower wages of women (Gruber, 1994). Firms that are more highly exposed to ML tend to hire more replacement workers, increase hours on incumbent workers, thereby increasing the wage bill. These effects are especially strong for small firms (Brenøe et al., 2020; Gallen, 2019; Bartel et al., 2021; Ginja et al., 2022). In our context, the federal mandate is financed through a small social security contribution increase, and many employers decided to offer paid leave before the federal mandate. The federal mandate thus lowers the costs of employing women on ML, and employers who previously offered paid ML could raise women's wages, extend the leave beyond the mandated level or offer other family friendly policies to women returning from paid leave.

to women before the policy change, its effects are likely to be limited. Paid ML likely affects the share of women in *employment*, and especially the share *employed at pre-birth employer* because women invest more into their jobs prior to birth, so the value of returning to the pre-birth employer increases. Also, women who work in firms that offered paid ML before the mandate may be offered better jobs or more flexibility upon returning to work, since employers can offer paid ML at a lower cost with the mandate compared to without it. If women are employed more, they need to rely less on other forms of transfer, e.g. unemployment insurance.

Introducing paid ML raises the *cumulative income* of families who have one child, both through working more prior to birth, and through the ML benefit after birth. This increase in income may contribute to increase *subsequent fertility*. Family income increases directly for women whose employer did not offer paid ML before the mandate. Women who work for an employer that already offers paid ML before the mandate may not receive a higher monetary transfer, but their employers could offer better work conditions, or child care, which in turn lower the costs of having an additional child.

Child-care is a second key policy to support working parents. The costs of having an additional child are low in areas that offer generous child care, and high in areas that offer little child care. The fertility effects are thus expected to be stronger in areas with generous child care compared to areas with little child care. Employment effects may also be heterogeneous with respect to the availability of child care. Generous availability of child care limits the extent to which women depend on the pre-birth employer to offer child care, and women could be less likely to return to the pre-birth employer.

Maternity leave intends to facilitate having children while pursuing a career, but whether the short paid leave offered in the Swiss context has any impact is not clear. The Swiss mandate introduces a short paid maternity leave in firms that did not offer it. Firms that already offered leave can decide to complement it with other family policies, so we can compare the effects of introducing a short paid leave to complementing an existing leave, which goes beyond the mandated level. Women who benefit from a short leave might be better off in terms of income, but may not be able to fully return to the careers held prior to birth. Complementing a short leave with more family policies, e.g. child-care, might then be more valuable than introducing a short leave – evidence of non-linearity in the effects of leave. If firms that already offered the leave do not complement it with other policies, the effects of the mandate will be weaker for women in firms that already provided leave compared to those where leave gets introduced.

3 Data and Descriptive Evidence

3.1 Data Sources

Our analysis is based on data compiled from three different administrative registers provided by the Swiss Federal Office of Statistics (FOS) and the Central Compensation Office (CCO). These are the Swiss federal population census (FOS), the Swiss social security register (CCO), and the vital statistics register of Switzerland (FOS).

The federal population census contains sociodemographic information about the residential population of Switzerland in December 2010 and December 2012. It includes information on an individuals' status within a household (head, spouse or child), sex, date of birth, marital status, date of last change in marital status, current municipality of residence, past municipality or country of residence and more. In addition, the population census links individuals within a household and parents with their children. All individuals can be identified through their unique (anonymized) social security number called 'AVS13'. Our baseline sample are women (and their partners) who had a child between 1st January 2003 and 31st December 2007, and who were living in Switzerland in December 2010.

For each mother and partner in our sample, we retrieve their social security register information from 1995 to 2014 using the AVS13. The social security register records all individual earnings from employment and self-employment, as well as any federal benefits received for maternity leave, unemployment, disability, military service, and more.[22] The information is provided for spells of various lengths (from one day to one year) within the same calendar year. We aggregate all data at a monthly frequency and transform the nominal earnings data into real earnings using the CPI with base year 2010.

We complement this data with the vital statistics register covering the period from 1995 until 2014. This register for life events records information on individuals' marriage, divorce, live births, as well as complementary data such as residence at different life events, paternal acknowledgments of births (for unmarried parents), divorce arrangements, and more. From 2011 onward, the AVS13 is recorded for all involved individuals of a life event.

We merge the first two registers using individuals' AVS13. The third register is merged using the AVS13 for events from 2011 onwards and using unique combinations of date of life events, woman's date of birth and partner's/children's date of birth for life events prior to 2011.[23] From this merged

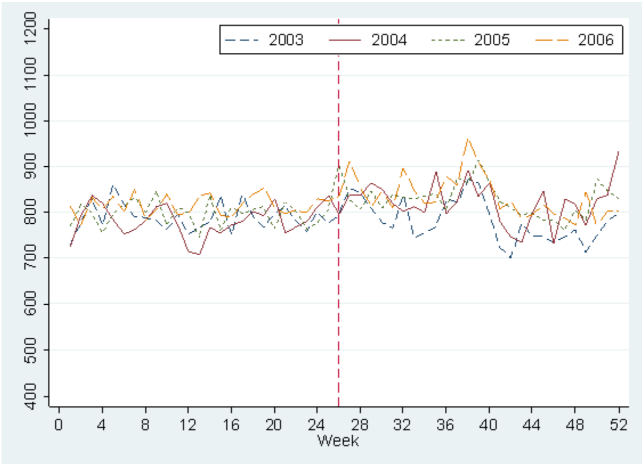
[22]Every resident aged 18 years and above with annual earnings above 2,300 CHF must contribute to social security. Those with annual earnings below 2,300 CHF (corresponding to less than half of median monthly earnings) can choose to contribute voluntarily.

[23]This procedure allows us to match 96% of all births and 80% of all marriages. The unmatched marriages almost uniquely concern foreign individuals who are likely to have been married abroad. We do not include them in our main analysis.

data set, we construct a monthly panel of every woman's labor market status, earnings, federal social security benefits received (including paid maternity leave), marital status, canton of residence and all living children born to her since she appeared in the social security register (usually between the ages of 18 and 20 years).[24] Our final data set spans the period from January 1995 to December 2014.

3.2 Descriptive Evidence

Figure 1 plots the total weekly number of births of Swiss women in Switzerland for the years 2003 to 2006. The vertical red line marks the week of implementation of the maternity leave mandate on 1st July 2005.



Notes: Figure provides the number of births by week. The dashed vertical line identifies week 26 (week of year of 1st July 2005).
Source: Authors' calculations using Swiss vital statistics register.

Figure 1: Weekly number of children born to Swiss women 2003 - 2006

Total numbers of births vary from week to week and over different years. Yet, while we observe some seasonal patterns in the total numbers of births, for example, an increase followed by a drop around 38 to 40 weeks after Christmas/New Year, there is no evidence of a drop in fertility prior to the introduction of the maternity leave mandate or an increase after its implementation on 1st of July 2005, nor is there any apparent time trend.

Following this descriptive evidence, we construct two samples of women who had their first child in two three-month periods in 2005, one before and one after the mandate became effective. Our pre-reform group comprises of women who had their first child in the period from 1st January 2005 to 31st March 2005, our post-reform group are first-time mothers of children born from 1st July 2005 to 30th September 2005.[25] We restrict our sample to women with Swiss nationality, who were not

[24]The source of earnings is used to construct the labor market status. If an individual has any earnings from (self-)employment, they are classified as "employed" even if they receive some unemployment benefits. Similarly, months with some days of employment and unemployment are classified as "employed". We do not directly observe hours worked but construct a measure of "full-time" and "high part-time" employment by comparing current earnings to those one year prior to birth (when most women work full-time).

[25]We exclude first-time mothers giving birth between 1st April and 30th June 2005 as they received partial benefits.

Table 1: Descriptive Statistics

A Demographics	Jan-March05 Before	Jul-Sept05 After	Mean Difference
Age at first birth	30.488	30.005	-0.483
	(0.071)	(0.068)	(0.098)
Age first observed	18.756	18.711	-0.045
	(0.032)	(0.030)	(0.044)
Married at first birth	0.764	0.776	0.012
	(0.006)	(0.006)	(0.008)
B Labour market history			
Share in labour force (LF) 12m prior to first birth	0.903	0.912	0.009
	(0.004)	(0.004)	(0.006)
Share employed among those in LF 12m prior to first birth	0.981	0.980	-0.001
	(0.002)	(0.002)	(0.003)
Monthly income from employment (CHF) 12m prior to first birth	5217.162	5234.869	17.707
	(40.465)	(41.131)	(57.789)
Cum. experience (months) from 6y to 12m prior to first birth	50.702	51.061	0.359
	(0.232)	(0.223)	(0.322)
C Eligibility and treatment			
Eligible	0.841	0.853	0.012
	(0.005)	(0.005)	(0.007)
Received federal paid maternity leave	0.000	0.808	0.808
	(0.000)	(0.005)	(0.006)
Received federal paid maternity leave among eligible	0.000	0.896	0.896
	(0.000)	(0.004)	(0.005)
Share with 50% of pre-birth income 1m to 3m after birth	0.552	0.850	0.298
	(0.007)	(0.005)	(0.008)
Observations	**5,119**	**5,412**	

Mothers who had their first child between January and March in 2005 were not affected by the reform and are classified as before the reform. Those who had their first child between July-September in 2005 are classified as after the reform. The third column displays the difference between the first two columns. We define as eligible those women who had been in the labor force for eight months prior to the actual birth of their child and had been employed (or officially unemployed) for at least five months during pregnancy. Standard errors are in parentheses.

living in the canton of Geneva, and who were aged between 15 and 45 years old at the time of birth following the literature (Lalive and Zweimüller (2009)).[26] The pre-reform and post-reform groups comprise of 5,119 and 5,412 first-time mothers, respectively. Table 1 presents descriptive statistics on demographics, labor market outcomes and potential federal maternity leave eligibility for these two groups in our sample.[27]

Pre-reform and post-reform first-time mothers are similar in many respects, in particular in terms of labor market histories and potential eligibility for maternity leave. Twelve months prior to giving birth, pre-reform and post-reform women have almost identical labor force participation rates (90.3%

Effects of the reform on partially treated mothers are quantitatively smaller, as one would expect from attenuation bias. Results are available upon request.

[26]For women without Swiss nationality, we often lack complete marital and residence histories. In addition, the period covered coincides with intensified economic relationships with the European Union (EU) that allowed for the free movement of persons between the EU and Switzerland. This drastically changed the composition of non-Swiss women in the sample over this period. We also exclude first-time mothers from Geneva since this canton already had an existing paid maternity leave mandate since 2001 and women in these cantons would, therefore, have been unlikely to respond to the new federal mandate.

[27]Eligibility for federal maternity leave benefits depends on the *expected* date of birth, which we do not observe in our data. To define potential eligibility in our data set, we use information on the *actual* date of birth of a child but reduce the requirement of being in the labor force prior to birth to eight months (instead of nine) and keep the employment requirement unchanged at five months.

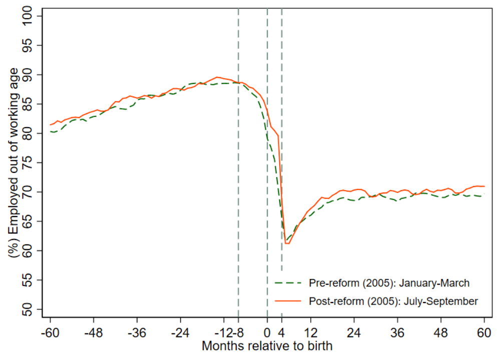
and 91.1%, respectively), employment rates (98.1% and 98.0%, respectively), monthly earnings (5,224 CHF and 5,242 CHF, respectively) and cumulative work experience (50.7 vs 51.1 months over the last 60 months, respectively). Furthermore, the eligibility for federal maternity leave is also very similar at 84.1% and 85.3%, respectively. None of these differences are statistically significant at the 5% significance level. Given that many firms offered paid maternity leave prior to the mandate, the discontinuity in the share of women receiving federally paid maternity leave overestimates the share of women affected by the introduction of the mandate. We thus also report the share of women who earned at least 50% of their pre-birth income one to three months after giving birth. This share increased from 55% in the pre-reform cohort to 85% in the post-reform cohort.

One dimension in which pre-reform and post-reform mothers differ slightly are socio-demographic characteristics. The average age when mothers give first birth drops from 30.5 to 30 years among post-reform mothers (a statistically significant difference at the 1% level), and the share of married mothers at first birth increases from 76.4% to 77.6% (albeit not statistically significant). However, these differences are driven by seasonality effects unrelated to the reform and will be taken care of by our estimation strategy (see Section 4).

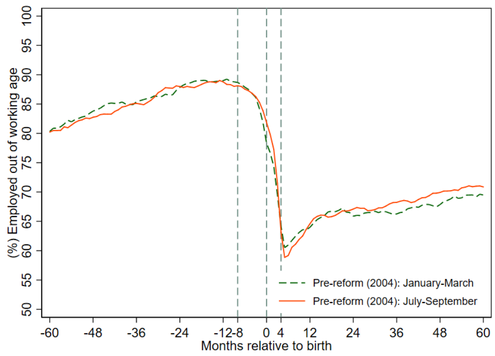
Figure 2 sheds further light on the dynamics of various labor market outcomes around the birth of the first child and subsequent fertility. It plots employment, monthly earnings, job continuity (i.e., employment at pre-birth employer) and the share with a second child of pre-reform (dashed line) and post-reform women (bold line) in a 10-year-window around the birth of the first child.[28] The first column presents the outcomes for pre- and post-reform women who had a child in the year 2005. The second column shows the same outcomes for women who had a child in the same two three-month periods in the year 2004. These later women were not affected by the maternity leave mandate for their first child. The horizontal axis represents time in months relative to the birth month of the first child (marked by a dashed vertical line at month zero). The dashed vertical line at four months marks the approximate end of the federal paid maternity leave period. The dashed vertical line at eight months prior to birth represents the start of the period of employment during pregnancy that is needed to become eligible for the federal paid maternity leave if the woman did not work previously.

Figure 2a shows a share of female employment of almost 90% one year prior to giving birth. Employment declines to about 80% at the time of birth followed by a further drop, reaching a minimum at 60% four months after birth. It subsequently increases to about 70% within one year post-birth

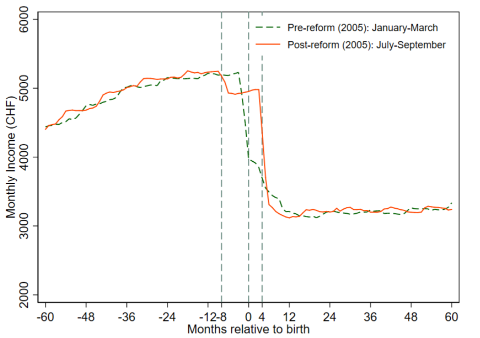
[28] We define women who receive federal maternity leave benefits (or maternity leave payments from their employer) as being in the labor force and employed. However, those on unpaid leave are considered being out of the labor force. We adopt this definition as we only observe income and the source, but not the effective labor market status (i.e., hours worked, paid and unpaid leave, being out of the labor market). We define as pre-birth employer the main employer of a women at 12 months prior to first birth.



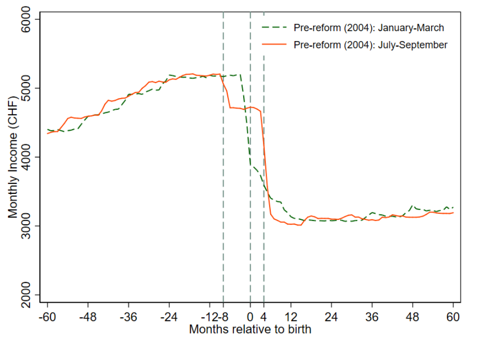
(a) Share Employed - 2005



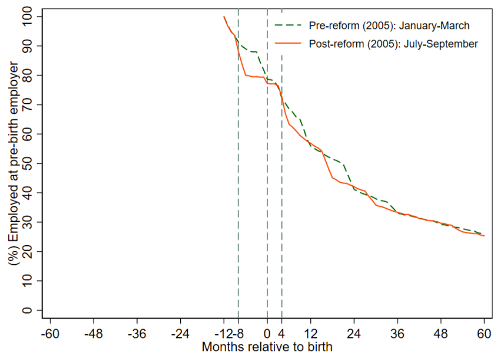
(b) Share Employed - 2004



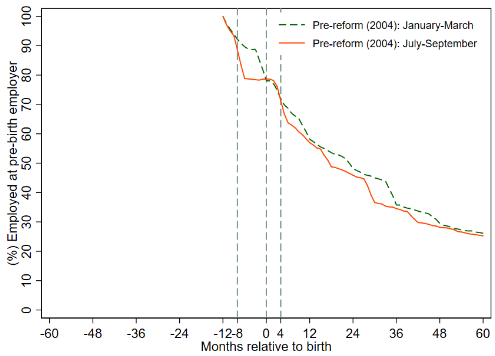
(c) Monthly Earnings - 2005



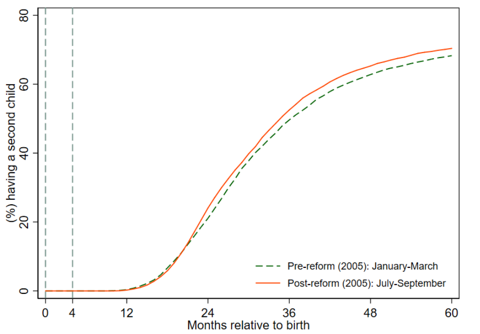
(d) Monthly Earnings - 2004



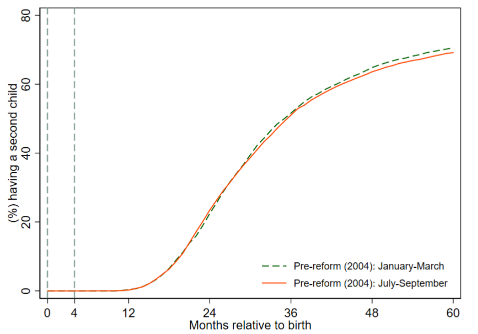
(e) Share Employed at Pre-Birth Employer - 2005



(f) Share Employed at Pre-Birth Employer - 2004



(g) Share with Second Child - 2005



(h) Share with Second Child - 2004

Notes: The figures of employment and fertility include all Swiss women (excl. Geneva). Employment at pre-birth employer (i.e., 12 months prior to the first birth) and monthly earnings are computed using the sample of employed Swiss women only. Women on paid maternity leave are classified as employed. Women on unpaid maternity leave are classified as out of the labor force.
Source: Authors' calculations using the merged data set.

Figure 2: Main Outcomes

and remains fairly constant afterwards. The trend before birth is very similar for pre-reform and post-reform women. After birth, however, post-reform women are slightly more likely to be employed than pre-reform women during the four months following birth (a direct result of the federal maternity leave mandate) and in the three years following birth. For women who had their first child in 2004, the overall trends are similar (see Figure 2b).

Figures 2c and 2d present monthly earnings including maternity leave benefits of employed women. These earnings patterns could be interpreted as the intensive margin of labor supply, that is, the hours worked, if we assume that hourly wages remain constant over this period. The trends in earnings leading up to 12 months prior to birth, as well as earnings trends 12 months after birth are very similar not only across years, but also between pre-reform and post-reform women. Moreover, both year cohorts and groups of women see important drops in earnings - though at different times relative to birth. Women giving birth between January and March experience a sharp decrease in earnings in the three months leading up to birth, while earnings of women giving birth between July and September drop at seven months prior to birth (though to a smaller extent for the 2005 cohort) and four months after birth. These seasonal patterns are observed across both year cohorts and point towards strong end-of-year effects when working contracts are re-negotiated. Strong seasonal patterns are also apparent for the share of first-time mothers employed with their pre-birth employer (see Figures 2e and 2f).

Finally, Figures 2g and 2h depict the share of women who had a second child in the five years after the birth of their first child. Post-reform women in 2005 were slightly more likely to have a second child around 24 months after the birth of their first child than the pre-reform women in the same year. This difference is not merely a temporary gap but it remains (and slightly widens even) until the end of the five years analyzed. For women giving birth in 2004, we find no evidence of a difference across the two three-month periods (if anything, those giving birth between July and September are slightly less likely to have a second child).

Overall, the descriptive evidence points towards small to no changes in employment, strong seasonal patterns, drops in earnings and job continuity both before and after birth, as well as slight differences in subsequent fertility.[29] The observed differences between the pre-reform and the post-reform women in 2005 could be the result of the federal maternity leave mandate or they could be caused by other factors. In the next section, we present the identification strategy which we use to pin down the causal effects of being covered by the federal maternity leave mandate for the first child.

[29]Appendices A.A and A.C present further descriptive evidence on marital status changes and unemployment.

4 Empirical Design

We employ a difference-in-differences design (similar to Lalive et al. (2013) and Schönberg and Ludsteck (2014)) to estimate the causal effects of the federal maternity leave mandate on first-time mothers' labor market outcomes and subsequent fertility. Our identification strategy hinges on comparing the outcomes of women who had their first child in a three-month period prior to the reform (1st January to 31st March 2005) with those who had their first child in a three-month period after the federal mandate became effective (1st July to 30th September 2005).[30] To isolate the causal effects of the federal mandate from seasonal differences across birth months, we use women who had their first child in the same three-month periods in the year preceding the reform, that is, 1st January to 31st March 2004 and 1st July to 30th September 2004, as the control group.

We estimate the following regression on all first-time mothers with Swiss nationality (excl. Geneva):

$$Y_{it} = \beta_{0t} + \beta_{1t}Reform_i + \beta_{2t}Months_i + \beta_{3t}Reform_i \times Months_i + x_i'\theta + \epsilon_{it}, \tag{1}$$

where i indexes women, and t indexes months relative to the first child's birth-month (t runs from 12 months before birth, to 60 or 108 months after birth in our main analyses). The binary variable $Reform_i$ is equal to one if mother i gave birth to her first child in the reform year 2005 and zero otherwise. $Months_i$ is a binary variable equal to one if mother i gave birth to her first child between 1st July and 30th September, and zero otherwise. The interaction term between $Reform_i$ and $Months_i$ reports the difference in outcomes of exposed and non-exposed mothers in 2005 relative to the difference in outcomes of mothers who had their first child in the same months in 2004. The coefficient on the interaction term, i.e., β_{3t}, is the coefficient of interest as it identifies the causal effect of the federal maternity leave mandate on first-time mothers' outcomes in month t relative to the first child's month of birth. x_i' is a vector of individual characteristics of the mother including her age at birth, her marital status one year prior to birth and her pre-birth employment characteristics, such as cumulative work experience and cumulative income from six years to 12 months prior to birth.

For the dependent variable Y_{it}, we use different contemporaneous and cumulative labor market outcomes, as well as subsequent fertility of first-time mothers. The contemporaneous measures include labor force participation, share in employment, share in unemployment, real earnings from employment, share employed at pre-birth employer among employed mothers, the share working full-time (defined as earning at least 80% of pre-birth earnings) and the share working high part-time (earning

[30] While women who had their first child before 25th March 2005 were not exposed to the mandate at all, women who had their first child between 25th March and 31st March were partially treated and potentially eligible for one to six days of paid maternity leave. This is negligeable in comparison to the 98 days provided by the mandate and if anything, would only bias our estimates towards zero.

between 50% and 80% of pre-birth earnings). The cumulative measures include the share ever returned to employment, cumulative employment earnings post-birth (all since six months post-birth) and cumulative total earnings (including maternity leave benefits and other transfers) since nine months prior to the first birth. Finally, the share of women who had a second child measures subsequent fertility. We also construct a measure of reconciling (full-time) work with young children, that is, the share of women who work full-time and have their youngest child below 2 years old.

We estimate Equation 1 for different outcomes at different points in time relative to the month of birth of the first child indexed by subscript t. t equaling zero signifies the birth month of the first child for a woman i. Positive values indicate the months after birth for each woman i, while negative values indicate the months before birth. We estimate the equation for each outcome at 6, 12, and every 6 months until 60 months after birth (108 for subsequent fertility). Moreover, for labor market outcomes, we also report the estimation results for -12, -9, -6, -3 and -1 month prior to birth to uncover possible anticipatory effects of the mandate. For example, when we estimate Equation 1 for labor force participation at six months after birth, the coefficient β_{3t} reports the causal impact of the reform on labor force participation of mothers at six months after the birth of their first child.

There is one potential threat to our identification strategy and two caveats for interpreting the results. These are i) the selection into treatment through deferred fertility and timing of births, ii) the selection into eligibility for the federal maternity leave policy, and iii) the use of the 2004 cohort as a control group. The first threat, selection into our post-reform treatment group through timing of fertility and births, seems unlikely for three reasons. First, the implementation date of the reform on 1st July 2005 only became known on 24th November 2004. On this date, most (though not all) of the women in our post-reform group would have already conceived their child.[31] Secondly, we do not find any evidence of a significant change in the number of births between early July and end of September 2005 when compared to other years before the reform (see Section 3.2 for more details).[32] Finally, the sample of first-time mothers giving birth between January and March 2005 is very similar in terms of observed demographic and labor market characteristics to the sample of first-time mothers giving birth between July and September 2005. The observed differences in mothers' age at birth and the share married at birth are related to seasonal effects unrelated to the reform.[33]

[31]For the remaining women, one should bear in mind that only 30% of all couples conceive spontaneously within the first month of trying (Taylor (2003)).

[32]This does not preclude, however, selection into first-time fertility further away from the implementation date of the reform. In fact, changes in maternity leave benefits can have strong effects on first-order fertility as shown by Raute (2019) for a German reform in 2007.

[33]Figure A.1 in Appendix A.A presents the cumulative share of married women at any month relative to the birth of the first child who were single one year before birth. To formally test if the demographic composition of first time mothers changes across groups, we run the same DiD regression as described above using age at birth and marital status (i.e., a dummy indicator for being married) as dependent variables. The interaction coefficient of $Reform_i$ and $Months_i$ is not statistically significant at any conventional level for age at birth and the marital status. The estimation results

While it is unlikely that post-reform women were able to time their births to invalidate our identification strategy, they could have affected their eligibility for the federal maternity leave prior to giving birth through increased labor force participation (extensive margin of labor supply) or by increasing (or not decreasing) the hours worked (intensive margin of labor supply). Thereby, they would qualify for higher maternity leave benefits, since these benefits are calculated based on average pre-birth earnings. To alleviate concerns about potential biases due to endogeneity of eligibility, we include all women who were exposed to the reform - except for those living in Geneva - irrespective of whether they actually received maternity leave benefits. Therefore, we estimate an intent-to-treat effect. Moreover, we include some months prior to birth in the analysis, which allows us to quantify anticipatory effects along several dimensions.

The use of the preceding year as the control group is common in the literature (see Lalive et al. (2013) and Schönberg and Ludsteck (2014)), yet it is important to recognize that the causal effect we identify relates to having been potentially covered by the *federal mandate for the first child* rather than the effect of the federal mandate per se. For all outcomes measured at 12 months or more after birth, the control group could also become eligible for paid maternity leave if they have another child. If the federal mandate has only temporary effects for the first child without any follow-on effects, we would not expect to see any significant effects beyond the 12-month threshold. For robustness, we also report the results using the 2003 cohort of first-time mothers as an alternative control group.

5 Results

We report the causal impact of the introduction of the federal paid maternity leave mandate on various contemporaneous labor market outcomes of first-time mothers as well as on their subsequent fertility. For all estimated effects of the mandate, we report the corresponding confidence intervals using robust standard errors.[34]

5.1 Work and Fertility Outcomes

Figure 3 depicts the estimated coefficient of interest at different times, that is, the coefficient β_{3t} from Equation 1. It captures the causal effect of being covered by the federal maternity leave mandate for the first child on mothers' labor market outcomes (Panels (a) to (c)), subsequent fertility (Panel (d)), and the cumulative financial impact (Panel (e)) at different months t relative to the first child's month

are shown in Appendix A.B.

[34]Given that the policy implementation was universal and that our administrative data set covers the population of women in the reform year and control year cohorts, we rely on robust rather than clustered standard errors (see Abadie et al. (2017)). However, using clustered standard errors at the local labor market level (with more than 100 clusters) yields very similar inference results.

of birth. Light and dark vertical lines indicate the 95% and 90% confidence intervals respectively. Tables C.1 and C.8 in Appendix C present the corresponding estimated effects, with robust standard errors and p-values.

Overall, we do not find any significant effect of the federal mandate on employment prior to or after the birth of the first child (Figure 3(a)). Our results show a weak S-shaped pattern in employment with a moderate, positive employment effect of 1.6 percentage points at 18 months after birth followed by small, negative effects from 30 months onward. None of these estimates are statistically significant. Our estimates on labor force participation and unemployment are quantitatively even smaller (see Figure B.1 in Appendix B).

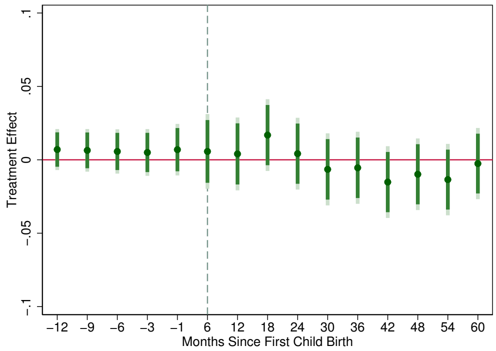
While there is little evidence that the federal mandate led to labor supply adjustments at the *extensive* margin prior to or after the birth of the first child, real earnings from employment reveal that the *intensive* margin was affected (Figure 3(b)). Our results show an increase in the real earnings of first-time mothers covered by the mandate, both before birth as well as after, though the later increase is much smaller and not statistically significant.[35] Monthly real earnings increase by more than 200 CHF (or four percent) in the months prior to birth. We interpret these statistically significant estimates as anticipatory effects of the reform. Assuming constant hourly wages during this period, women who are likely to be covered by the federal leave mandate increase their hours worked (or decrease them less) prior to giving birth and before the mandate is implemented compared to pre-reform women. Most of this effect arises from women continuing to work full-time during pregnancy rather than reducing their work-time to a high part-time percentage (see Figure B.1, Panels (e) and (f) in the Appendix). By doing so, they stand to qualify for higher maternity leave benefits, since this is calculated at a rate of 80% of pre-birth earnings. After birth, these earnings effects remain positive, but they are much smaller in size and are not statistically significant.

We also find moderate, positive effects in terms of job continuity.[36] Women exposed to the reform are slightly more likely to stay with their pre-birth employer during pregnancy and significantly more likely to be working for the same employer in the medium term after the birth of their first child (Figure 3(c)). This improvement in job continuity is closely related to the impact of the mandate on higher-order fertility, which will be discussed later.

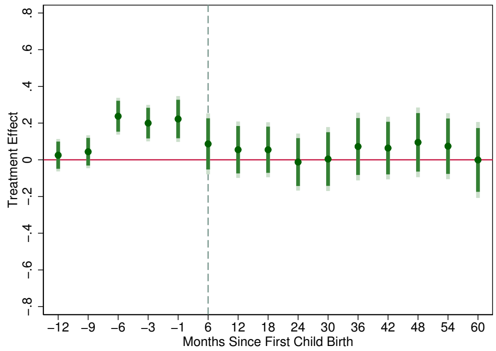
The mandate increases subsequent fertility to an important extent (Figure 3(d)). Post-reform women are two percentage points more likely to have a second child 24 months after the birth of the first. In the long run, that is, nine years after the first child's birth, the fertility gap still persists

[35] Earnings are adjusted for yearly inflation by using the CPI with base year 2010.

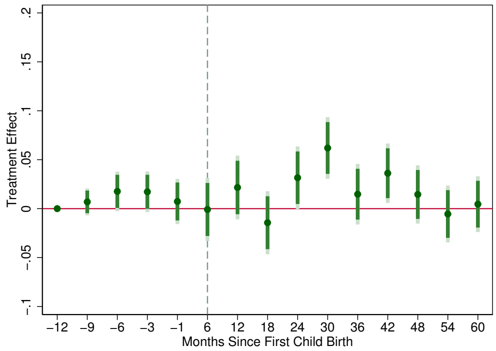
[36] We measure job continuity with an indicator variable that is equal to one if an employed woman in month t still works for the same employer as at 12 months prior to birth.



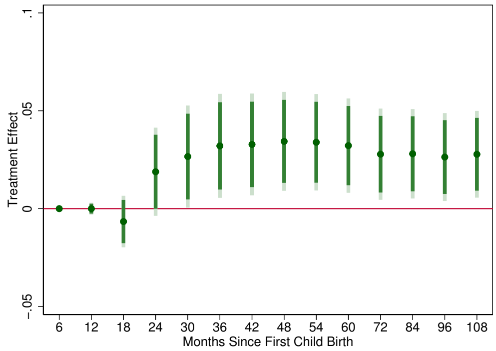
(a) Share Employed



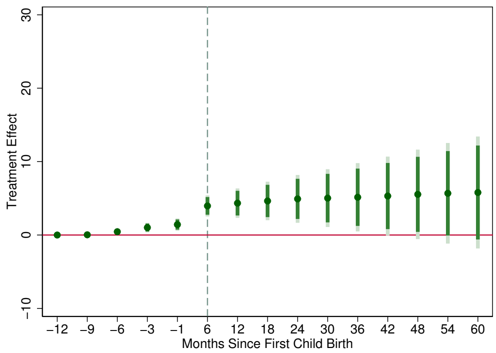
(b) Employment Earnings (in 1000s CHF)



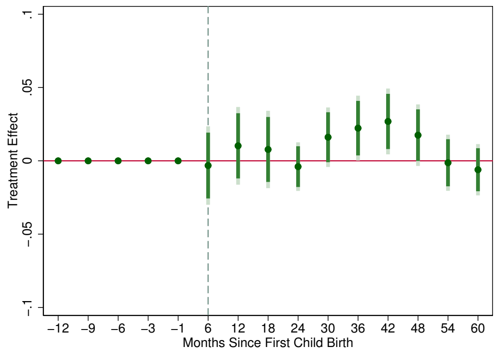
(c) Share Employed at Pre-Birth Employer



(d) Share with Second Child



(e) Cumulative Total Earnings Since 9 Months Pre-Birth (in 1000s CHF)



(f) Share Working FT with Child Below 2

Notes: Treatment effects identified by our DiD model for all Swiss women in our sample (excl. Geneva). All regressions control for mothers' characteristics such as age at first birth, indicator of marital status (married) one year prior to birth, cumulative work experience and cumulative income from six to one year prior to first birth of first childbirth. Subfigure (a) shows the effects of the federal mandate on the share of women in employment at various points in time pre- and post-birth. Subfigures (b) and (c) relate to employed women. They show the effects on real earnings from employment and the share returning to their pre-birth employer (i.e., the main employer 12 months prior to birth). Subfigure (d) shows the effect on the share of women who had (at least) a second child up in the period up to 9 nine years after the birth of first birthchild. Subfigure (e) presents the cumulative total real earnings of all women (including earnings from employment, self-employment, maternity leave benefits, unemployment and other social insurance benefits) since nine months prior to the first birth (i.e., around the time of conception). All earnings are adjusted for inflation by using the CPI with base year 2010. Subfigure (f) shows the effect on the share of women who work full-time (i.e. earning at least 80% of pre-birth earnings) and whose youngest child is less than 2 years old. Light vertical lines indicate the 95% confidence intervals, the dark vertical lines indicate the 90% confidence intervals. The dashed vertical line separates the time horizon into a pre-birth and post-birth period. Robust standard errors are used.

Figure 3: Results on Employment, Earnings and Subsequent Fertility

and stands at 2.8 percentage points (statistically significant at the 5% level). Given that around 70% of all first-time mothers have another child in the control group, this corresponds to an increase in subsequent fertility of four percent. The weak S-shaped pattern in employment and increased job continuity post-birth are best understood in relation to the timing of the second child's birth. As discussed above, the share of employed mothers among those covered by the mandate increases 18 months after the first birth (albeit not statistically significant), most likely with the aim of achieving eligibility for maternity leave benefits for the second child. Moreover, job continuity also increases around (and after) the second child's birth, only to dissipate in the long run.

The subsequent fertility effect is particularly interesting because pre-reform women could also become eligible for paid maternity leave for subsequent children, yet fewer of them go on to have a second child. While identifying the unambiguous cause of this result proves difficult with our data set, our findings suggest that a positive overall financial impact of the mandate and an improvement in reconciling full-time work with young children are at the core of this increase in subsequent fertility. To measure the financial impact of the federal mandate, we construct cumulative earnings since nine months prior to the first child's birth.[37] We find that women covered by the mandate accumulated significantly higher earnings during pregnancy, after maternity leave ends, and in the medium run after the first birth (Figure 3(e)). By the time couples consider whether to have a second child or not (that is, from around the time the first child has turned one year old), mothers under the mandate have experienced a statistically significant, positive total financial impact of the reform of around 4,340 CHF. This amount equals almost one month of median pre-birth earnings. However, there is a second effect of the mandate which goes beyond its financial impact which is reconciling full-time work and having young children. Figure 3(f) depicts the share of mothers working full-time (measured as having at least 80% of pre-birth earnings) whose youngest child is below two years old. While the mandate did not improve the reconciliation of full-time work and having a young child around the birth of the first child in a significant way, it did increase the share of full-time working mothers with young children by around two percentage points around the birth of the *second* child (i.e. 30 to 48 months after the first child's birth).

We find similar quantitative effects on subsequent fertility as reported by Lalive and Zweimüller (2009) for the Austrian reform in 1991. This result is interesting since their paper studies an extension of a *long* maternity leave from one to two years with *relatively low benefits* (i.e. a benefit of 240 euros

[37]The cumulative earnings measure adds up all earnings from employment, self-employment, maternity leave, unemployment and other social security benefits since 9 months prior to the birth of the first child. Given the nature of the data, we cannot distinguish employment earnings from maternity leave earnings paid by the employer prior to the federal maternity leave mandate. Moreover, the mandate led to unemployment insurance benefits during the first 14 weeks after birth being displaced by maternity leave benefits. Hence, cumulative total earnings provide a more accurate measure of the total financial impact of the reform than a measure summing employment and maternity leave benefits only.

per month, approximately 31% of median gross female earnings), while our findings are the result of a *short* maternity leave mandate with *moderate-high benefits*. This comparison suggests that the length (i.e. short versus long) and benefit level (i.e. moderate versus low) across different leave policies might not be as crucial for achieving similar fertility outcomes if the total financial impact is comparable. However, the total financial impact appears too small to cause such a sizeable increase in subsequent fertility. Instead, our results on full-time working mothers with young children indicate that a shift in social norms could have taken place as a result of the mandate, similar to the one found for a maternity leave reform in Germany in 2007 by Bergemann and Riphahn (2015). Our estimated subsequent fertility effect is likely to be an underestimate of the *overall* fertility effect. As shown by Raute (2019) for Germany and González and Trommlerová (2021) for Spain, increases in maternity leave payments and baby bonuses also increase first-order fertility and hence, one would expect an even larger overall fertility effect in Switzerland.

Both the mandate's total financial effect and its impact on improved reconciliation of full-time work and having young children are the likely explanations of the higher subsequent fertility rates of first-time mothers affected by the reform compared to the control group mothers. While both groups of mothers could become eligible for paid maternity leave for their second child, the post-reform mothers have more financial means at their disposal at this point in time and they have personally experienced the federal mandate, both of which have probably led to a higher share of these women having a second child. Several other mechanisms could potentially explain the effect of introducing maternity leave on subsequent births. Subsequent fertility might increase through higher marital stability. In our data, however, marital stability is not affected by the maternity leave mandate. Moreover, Avdic and Karimi (2018) show that parental leave taken by fathers can even decrease marital stability. A second alternative mechanism for increased subsequent fertility could be an improvement in maternal health due to the mandated leave. Bütikofer et al. (2021) find evidence of improved maternal health (even in absence of income effects) as a result of the introduction of 18 weeks of paid maternity leave in Norway in 1977. We cannot investigate the role of this second alternative mechanism - and how it affects subsequent fertility - due to a lack of health data. A third mechanism would work through a better experience after birth due to the ability to take a longer leave which lowers the perceived psychological cost of the next birth. Given that stress rises the most right after a birth, it may be very valuable to have maternity leave right after a birth. This mechanism would arguably imply that subsequent birth effects are strongest among groups of women that were less covered by pre-reform leave, and weaker among women who were already covered. We investigate this conjecture in subsection 5.3.

5.2 Child Care Availability

One key determinant of mothers' labor market and fertility outcomes is the availability of child care services when maternity leave ends (Olivetti and Petrongolo (2017)). As a result, we expect complementarities between maternity leave and child care policies. Such complementarities may be particularly prevalent in a context like Switzerland where the demand for child care services by far exceeds its supply (Bundesamt für Sozialversicherungen (2006)).[38] To investigate if these two family policy instruments complement each other, we estimate the effects of the federal maternity leave mandate among women living in cantons with high child care availability and contrast them with those from cantons with low availability. To do so, we use the cantonal child care availability index of Ravazzini (2018) which measures the number of slots for children aged 0 to 3 years in all recognized private and public child care facilities of a canton relative to its population of children of the same age group. We define as high child care availability cantons where more than 10 slots per 100 children aged 0 to 3 years were available in 2002 (roughly corresponding to the median), and low child care availability otherwise. If the federal maternity leave mandate has an overall positive, medium run impact on labor market outcomes, we expect a larger effect where child care slots are relatively more abundant.[39]

Figure 5 depicts the estimated causal effect of the mandate at different times for women living at first birth in cantons with high child care availability (left column) and women living in cantons with low child care availability (right column). Tables C.2 to C.4 and C.8 in Appendix C present the corresponding estimated effects, difference in estimated effects, robust standard errors and p-values.

Women living in cantons with high child care availability (Column A in Figure 5) generally reacted more strongly to the federal maternity leave mandate than those in cantons with low availability (Column B in Figure 5).[40] The difference is particularly notable for the mandate's impact on subsequent fertility. Women living in cantons with above-median child care availability showed a strong and statistically significant subsequent fertility response of around four percentage points from two years post-birth onward, while the effect was much weaker at two percentage points (and not statistically significant) among the group of women living in cantons with lower child care availability. This

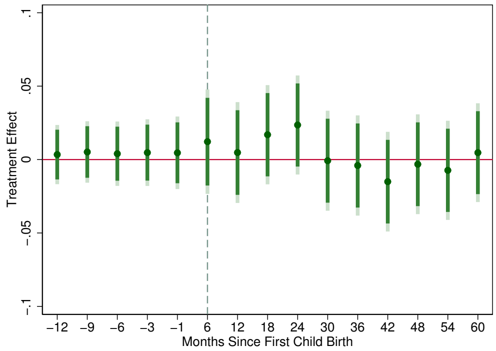
[38]Krapf et al. (2020) study the effect of child care availability on child penalties across municipalities in the canton of Bern in Switzerland from 2005 to 2015. They find that the presence of child care facilities increases female earnings (and decreases the compensating increase in male earnings) in the first year after a child's birth among below median earning households.

[39]Table A.2 in the Appendix presents descriptive statistics on first-time mothers in high child care cantons and in low child care cantons before and after the mandate came into effect. The women across these two broad areas are relatively similar both in their demographic and labor market characteristics, yet those in high child care cantons have slightly higher earnings pre-birth.

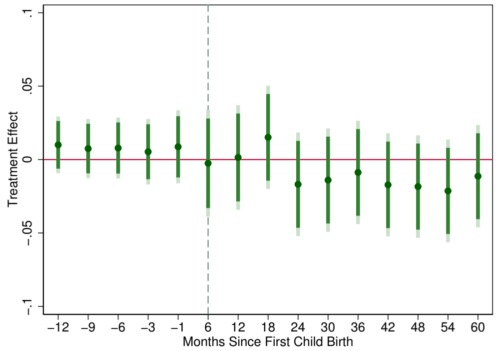
[40]Table C.4 and C.8 in Appendix C presents the estimated difference-in-difference-in-difference results between the two groups of women from high and low child care cantons. Note that as a result of the relatively large standard errors, we cannot reject the null hypothesis of an absence of difference-in-means for most estimated coefficients. However, we believe that the quantitative effects and the difference (and its sign) in quantitative effects across groups are interesting and should be seen as a lower-bound estimate due to possible attenuation bias.

Column A. High Child Care Availability

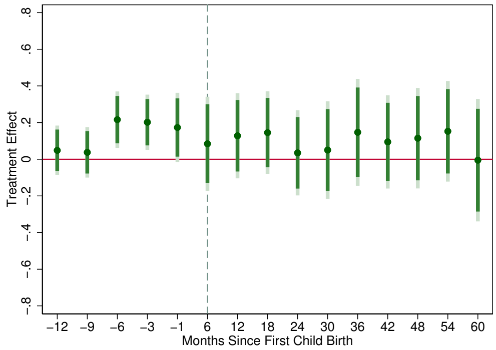
Column B. Low Child Care Availability



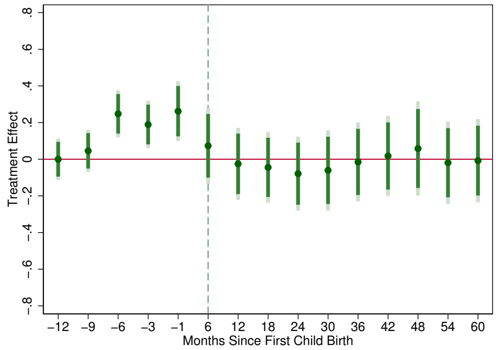
(a) Share Employed



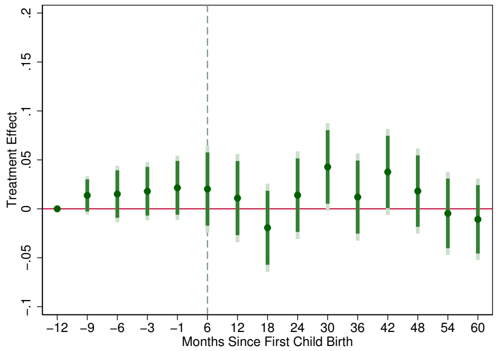
(b) Share Employed



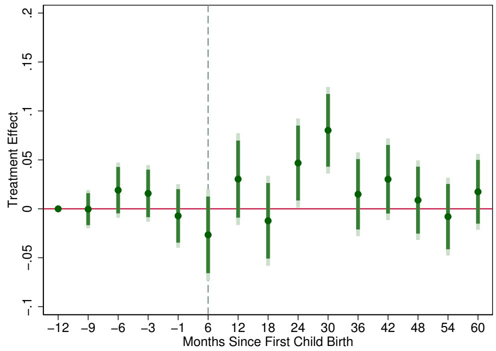
(c) Employment Earnings (in 1000s CHF)



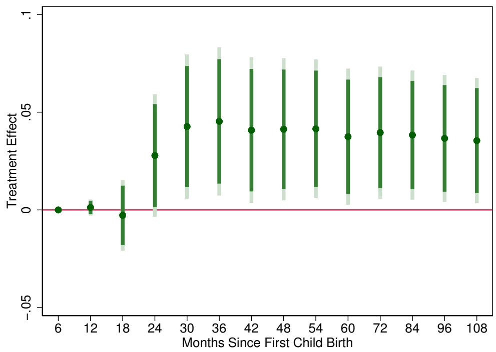
(d) Employment Earnings (in 1000s CHF)



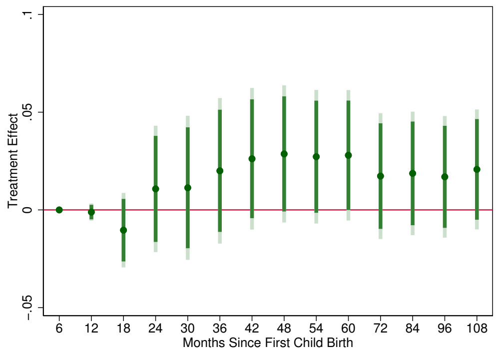
(e) Share Employed at Pre-Birth Employer



(f) Share Employed at Pre-Birth Employer



(g) Share with Second Child



(h) Share with Second Child

Notes: This figure shows the treatment effects for women according to the availability of child care slots in the canton of residence for children aged 0 to 3 years in 2002 (excl. Geneva). All regressions control for mothers' characteristics such as age at first birth, marital status one year prior to birth, cumulative work experience and cumulative income from six to one year prior to birth of first child. We distinguish cantons by whether they offer above or below median number of child care slots in the year 2002 (i.e., 10 slots and more per 100 children corresponds to above-median, while below 10 slots per 100 children corresponds to below-median child care availability). Light vertical lines indicate the 95% confidence intervals, the dark vertical lines indicate the 90% confidence intervals. The dashed vertical line separates the time horizon into a pre-birth and post-birth period. Robust standard errors are used.

Figure 5: Heterogeneous Effects by Child Care Availability

finding on subsequent fertility is surprising. Women in high child care cantons are characterized by a stronger attachment to the labor market, in terms of employment and hours worked as proxied by earnings, and hence, would face a higher opportunity cost of having another child. Women living in high-child care cantons were also slightly more likely to be employed and saw their monthly post-birth employment earnings increase by 100 to 250 CHF as a result of the mandate, though none of these effects are found to be statistically significant.

Women living in cantons with low child care availability, in contrast, showed slightly stronger anticipatory effects in terms of earnings prior to birth and attachment to their pre-birth employer around 24 to 30 months after the birth of the first child, which is for many women around the birth of their second child.

In terms of the cumulative financial impact of the mandate (see Figure B.3 in Appendix B), we find similar effects of the mandate six months after the birth of the first child in both low- and high-child care cantons. However, while this cumulative financial impact dwindles away in low-child care cantons over the following months, it continues to grow and remains statistically significant in high-child care cantons.

All in all, our estimation results point towards some complementarity between maternity leave policies and the availability of formal child care for very young children. Unless child care is widely available for children below three years of age, little impact of maternity leave reforms should be expected beyond the duration of the maternity leave itself. However, if child care is sufficiently available, a short maternity leave mandate could have some small labor market effects in the medium run, and persistent and large effects on subsequent fertility. Danzer et al. (2020) report a similar fertility result for the Australian maternity leave expansion from one to two years in the 1990s on increased family size in communities with formal childcare. Their results on maternal full-time employment suggest - somewhat counter-intuitively - that in communities with formal childcare women had relatively lower full-time employment after birth compared to communities without formal childcare.

5.3 Firms with and without prior paid leave

The mandate affected women differently depending on their employers. It did not directly affect women who worked in firms that already offered a similar paid maternity leave but reduced their employers' costs of providing this benefit significantly. In contrast, the mandate introduced paid maternity leave for the first time to those women working in firms that did not provide such a benefit. Therefore, this raises the following key questions. How did the mandate's impact differ across these two groups of women? Did the mandate have any impact on women who were already covered through their employers? That is, did firms that provided prior paid maternity leave pass on the resulting cost-savings to their female employees - potential “trickle down effects”?

We do not directly observe if a firm offered paid maternity leave prior to the mandate.[41] However, it is well known that larger firms were significantly more likely to have offered paid maternity leave prior to the mandate than smaller firms.[42] In addition, the availability of pre-mandate paid leave also differed across industries and regions (Aeppli, 2012).

To analyse how the impact of the mandate differed between women working in these two types of firms, we adopt the following two-step procedure.[43] First, we predict the likelihood of receiving paid maternity leave prior to the mandate among the control group based on the firm size of the employed woman before birth, her labour market region, and the social security fund of her employer, which is a proxy for the industry.[44] We exclude women not in the labor force one year prior to the birth of their first child. For the dependent variable, we construct an indicator variable that takes a value of 1 if a woman has earned at least 50% of her pre-birth income (i.e. the average monthly income earned between nine and eleven months prior to the birth of her first child) in the first three months after giving birth (i.e. months one, two and three post-birth) and 0 otherwise. This indicator variable is a proxy for paid maternity leave prior to the mandate because women are not allowed to work in the first eight weeks after birth.[45] We regress this “paid leave” indicator of woman i in the control group (i.e. p_i) on the logarithm of her firm's size $size_f$, and include fixed effects for her labour market region λ_j and her social security fund α_k as follows:

$$p_i = \beta_0 + \beta_1 * log(size_f) + \lambda_j + \alpha_k + \epsilon_i$$

Second, we recover the estimated coefficients and fixed effects to predict the likelihood of pre-mandate paid maternity leave coverage among both the control group and the treatment group in years 2004 and 2005.[46] We then split our sample into two groups. First-time mothers are classified

[41] As our data set also includes workers who are self-employed, this information would also be required for self-employed workers.

[42] A non-representative firm survey conducted by Aeppli (2012) revealed that only 42% of small firms (defined as those with less than 50 employees) offered paid maternity leave prior to the mandate, but the share amounted to 67% among large firms (those with 250 employees and more).

[43] Employers who provided paid leave before the reform differ from employers that did not provide prior leave in many other ways. These differences could interact with the provision of *mandated* paid maternity leave. Our results may therefore not solely represent the different impacts of the reform, but it could also reflect the different employers and employees who experienced the mandate.

[44] Our data set does not contain any direct information about the size of firms or industry. However, we can approximate the size of every firm by using the number of all mothers and fathers in our data set (which corresponds to around 11% of the working population at the time) working in a specific firm in January 2004. As a proxy for the industry, we use information on the social security fund. Every firm in Switzerland has to be linked to a social security fund. Some of these funds are private, some of them are for public employers, and others are mixed by covering both public administrations and private employers. Many funds are specifically linked to a certain industry such as a the insurance sector, retail, construction, the hotel and restaurant sector, or the watch making industry. In total, there are more than 70 different funds.

[45] This law has been in place since 1877 and was not affected by the maternity leave mandate.

[46] Predicted paid leave is given by $\hat{p}_i = \hat{\beta}_0 + \hat{\beta}_1 * log(size_f) + \hat{\lambda}_j + \hat{\alpha}_k$.

as employed by "firms with prior leave" if the predicted probability exceeds 50%. If the predicted probability is equal to or below 50%, first-time mothers are classified as employed in "other firms". We then run separate difference-in-difference regressions for women in the two types of firms.

We use this classification based on women's pre-birth employment characteristics to uncover possible differential treatment effects and trickle-down effects. In the January to March 2005 cohort, almost 70% of women classified as employed in firms with pre-mandate leave had substantial positive earnings in the first three months post-birth, while only 30% of women classified as employed in other firms (see Table A.3 in Appendix A.C). In the first cohort after the mandate came into effect, the shares had increased by approximately 20pp and 50pp, respectively, shrinking the difference in coverage between the two groups to less than 10pp. First-time mothers in "other firms" were directly affected since the mandate introduced of paid maternity leave for many of these women. For women employed in firms with paid leave prior to the mandate, the direct effect of the mandate was arguably smaller. However, for these firms, the mandate represented a decrease in the costs of providing this benefit, and they could potentially pass on these additional funds to their female employees, the trickle-down effects. Whether a very short paid leave affects work and fertility outcomes more than the same paid leave combined with further trickle-down effects is not clear.

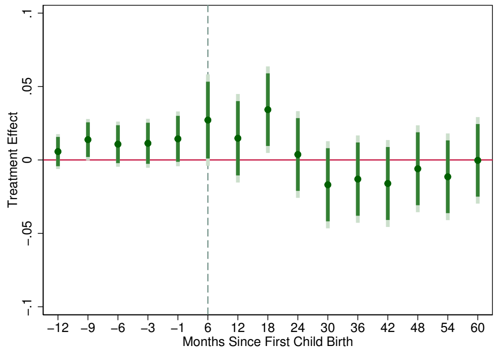
Figures 6 and 7 depict the mandate's estimated causal effect on several labour market and fertility outcomes at different times for women employed in firms offering paid leave prior to the mandate (left column) and women employed in other firms (right column). Tables C.5 to C.7 and C.8 in Appendix C present the corresponding estimated effects, difference in estimated effects, robust standard errors and p-values.

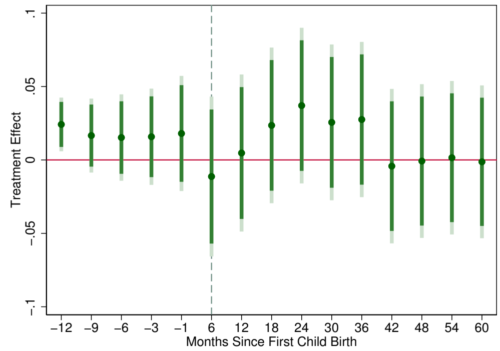
First-time mothers employed in firms with prior leave reacted strongly to the policy reform. The mandate temporarily increased the likelihood of employment in the first 18 months post-birth by nearly three percentage points (Figure 6 Panel (a)) and it boosted job continuity in the first four years after birth (Figure 6 Panel (c)). Most importantly, however, the mandate led to a substantial increase in subsequent fertility. An additional four to five out of 100 of these women had a second child (Figure 6 Panels (g)), a sizeable and significant increase which persists even in the long run (i.e. after nine years). While there are no discernable effects of the mandate on the extensive (Figure 6 Panel (a)) or intensive margin of labour supply (Figure 7 Panels (a) and (c)) in the medium and long term, we find that these women were more likely to work full-time after the birth of their second child (Figure 7 Panel (e)). Taken together, this evidence suggests that the mandate has led to an improvement in reconciling a larger family with (full-time) work for this group.

Women employed in firms without prior leave, in contrast, were affected quite differently by the

Column A. Firms with Prior Leave

Column B. Other Firms

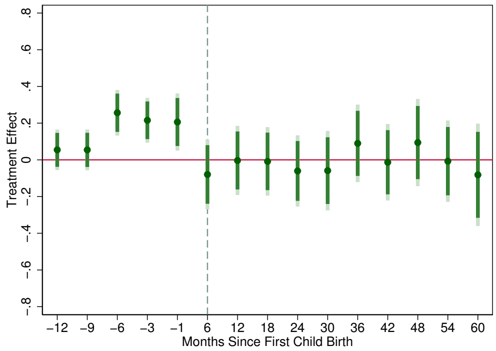


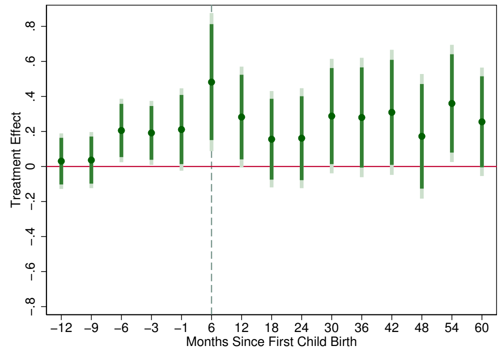


ic

(a) Share Employed

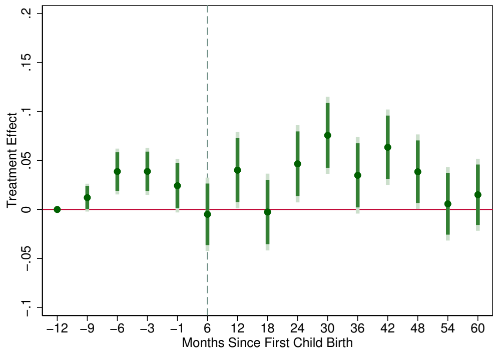
(b) Share Employed

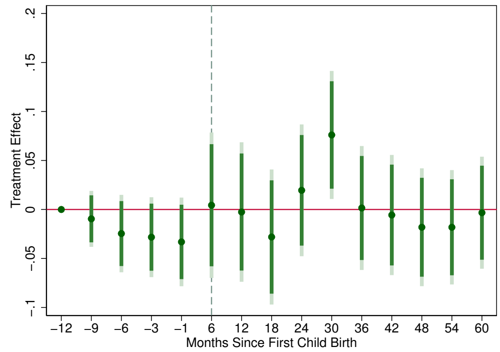




(c) Employment Earnings (in 1000s CHF)

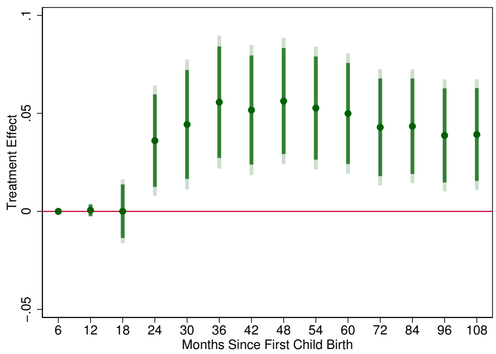
(d) Employment Earnings (in 1000s CHF)

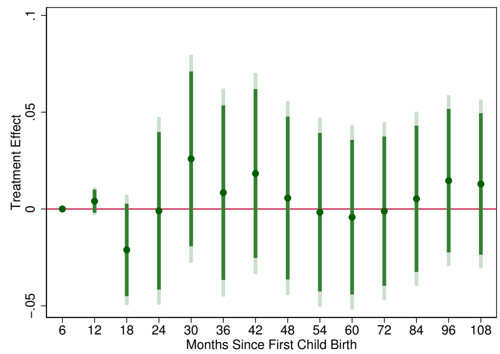




(e) Share Employed at Pre-Birth Employer

(f) Share Employed at Pre-Birth Employer





(g) Share with Second Child

(h) Share with Second Child

Notes: This figure shows the treatment effects by the type of firm (i.e. whether it was likely to offer paid maternity leave prior to the mandate or not) where a woman was employed one year before birth on contemporaneous outcomes (excl. Geneva). All regressions control for mothers' characteristics such as age at first birth, marital status one year prior to birth, cumulative work experience and cumulative income from six to one year prior to birth of first child. Figures in the left column (Panel A) show the effects for those women who were employed in firms with pre-mandate leave one year prior to birth while figures in the right column (Panel B) show the same effects for women employed in other firms one year before birth. Light vertical lines indicate the 95% confidence intervals, the dark vertical lines indicate the 90% confidence intervals (both based on robust standard errors). The dashed vertical line separates the time horizon into a pre-birth and post-birth period.

Figure 6: Heterogeneeous Effects by Firm Type

mandate. First, there is no statistically significant effect on subsequent fertility among this group of women (Figure 6 Panel (f)). Second, the mandate instead increased earnings from employment both in the short and long run (Figure 6 Panel (d)) caused by a slight increase in full-time work immediately after returning from leave (Figure 7 Panel (b)) and increased shares of high part-time work in the medium run (Figure 7 Panels (d)).

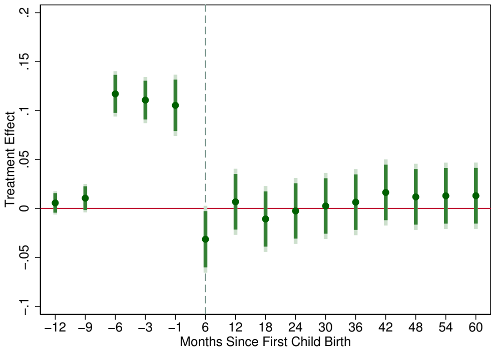
Comparing the cumulative earnings of women in firms with prior paid leave and those in other firms provides an insight into the mandate's (direct) financial effect. The cumulative *anticipatory* effect of the mandate on earnings was similar (and not statistically different) across the two groups of women. It amounted to 1,680 Swiss francs (with prior leave) and to 1,860 Swiss francs (without prior leave) one month prior to birth (see Figure 7 Panels (g) and (h), Table C.7 in Appendix C). Six months after birth, women employed in firms without prior leave had earned an additional 7,440 Swiss francs as a result of the reform - more than 1.5 months of their median pre-birth income - while the mandate's financial effect was only 3,420 Swiss francs among those working in firms with prior leave. The difference in treatment illustrates a significantly larger direct financial effect among women for whom the mandate introduced paid maternity leave. Moreover, the cumulative financial effect keeps growing over time for women without access to leave prior to the mandate, while it plateaus two years after the birth of the first child for the other women.

Overall, the mandate thus projected previously uncovered women onto a better financial trajectory. At the same time, the mandate allowed women with prior leave provided through their employer to have more children without sacrificing their careers. Given the limited financial impact of the mandate on this latter group of women, this suggests that other factors induced the sizeable shift in subsequent fertility. Non-financial trickle-down effects from affected firms to their female employees could have been important. Survey results from Aeppli (2012) indicate that almost 60% of interviewed firms found that the mandate lowered the financial burden of offering paid maternity leave. According to the same survey, 56% of firms that had offered prior paid leave used some of the freed-up funds to finance other family-friendly benefits. Almost one out of four firms made their paid maternity leave more generous (length and/or benefit level) than the mandated minimum and one in five firms hired a temporary replacement worker. Some firms also created or extended paternity leave, contributed to child care costs or made other adjustments (Aeppli, 2012). In addition to these trickle-down effects, two other effects could have been at work. First, the mandate probably created goodwill among employers who previously covered the cost of maternity leave themselves. Second, the media coverage surrounding the referendum could have shifted social norms about women continuing working after having children.[47]

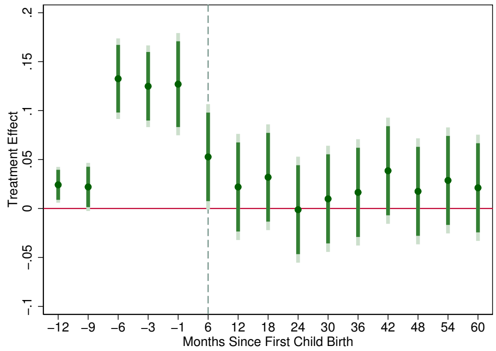
[47]For example, Kluve and Schmitz (2014) argue that a parental benefit reform in Germany had a profound effect on social norms about when mothers should return to work after giving birth. In their review article covering many reforms,

Column A. Firms with Prior Leave

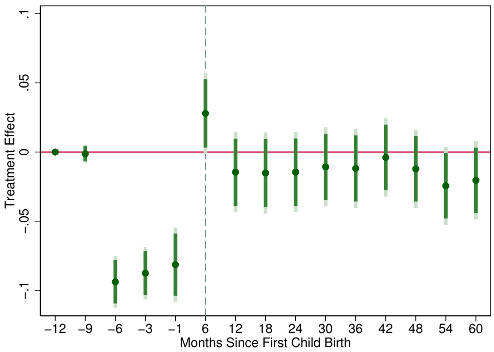
Column B. Other Firms



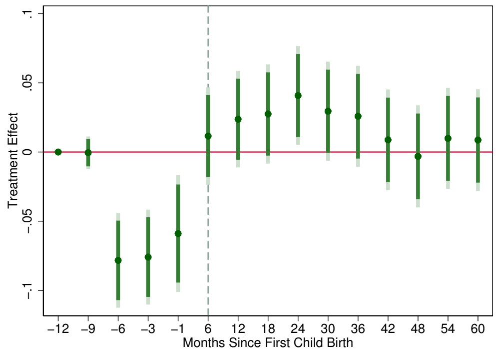
(a) Share working plus 80%



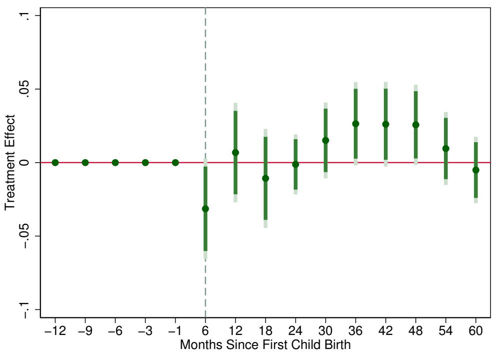
(b) Share working plus 80%



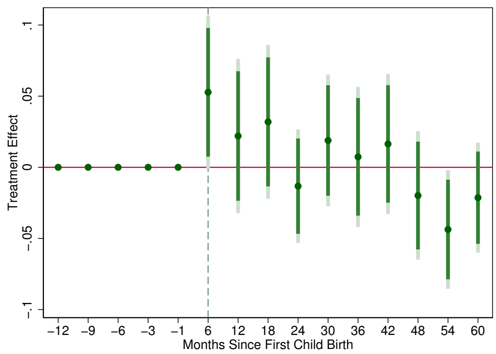
(c) Share working 50% to 80%



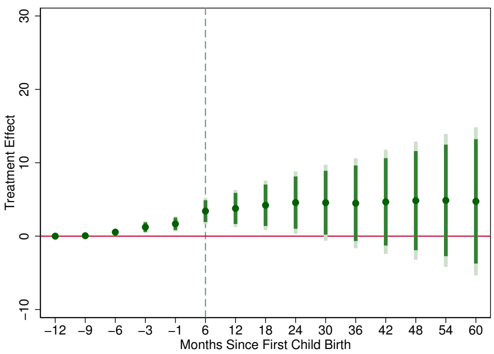
(d) Share working 50% to 80%



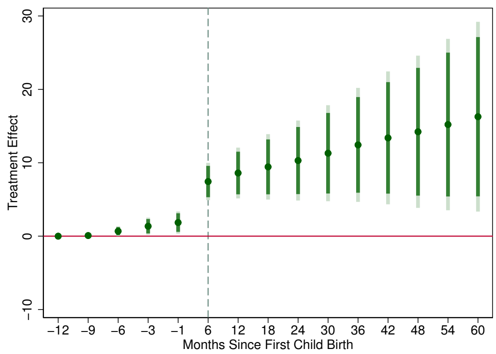
(e) Share working FT with child below 2



(f) Share working FT with child below 2



(g) Cumulative Total Earnings Since 9 Months Pre-Birth (in 1000s CHF)



(h) Cumulative Total Earnings Since 9 Months Pre-Birth (in 1000s CHF)

Notes: This figure shows the treatment effects of the mandate by the firms size where a woman was employed one year before birth on contemporaneous outcomes and cumulative total earnings (excl. Geneva). All regressions control for mothers' characteristics such as age at first birth, marital status one year prior to birth,marital status one year prior to birth, cumulative work experience and cumulative income from six to one year prior to birth of first child. Figures in the left column (Panel A) show the effects for those women who were employed in large firms one year prior to birth while figures in the right column (Panel B) show the same effects for women employed in small firms one year before birth. Light vertical lines indicate the 95% confidence intervals, the dark vertical lines indicate the 90% confidence intervals (based on robust standard errors). The dashed vertical line separates the time horizon into a pre-birth and post-birth period.

Figure 7: Heterogeneous Effects by Firm Size: further outcomes

All of these factors would help women better balance work and family commitments.

5.4 Robustness

In our main specification, we use as the control group those women who had their first child in the same three-month periods in the year preceding the reform, which is the year 2004. This implies that for all coefficients estimated at 12 months and later relative to the first birth, the control group women would have also been eligible for paid maternity leave for subsequent children. This could raise concerns regarding the interpretation of estimated effects at 12 months and later relative to the first birth.

We also run the same regression analyses as shown before, but which use the 2003 cohort of women as control group.[48] Our robustness analyses reveal very similar qualitative and quantitative patterns as in our previously presented results, indicating that our results are generally robust to the choice of the control group. For some outcome variables the significance level changes slightly. Our preferred specification remains the one with the 2004 cohort of first-time mothers as the control group since the common trend assumption is more likely to hold than for the 2003 cohort.

5.5 Discussion

Table 2 (panels A to C) summarises our key findings of the mandate's causal impact on employment, earnings, subsequent fertility and its cumulative financial effect for the main sample (column (1)), as well as across regions with high and low child care availability (columns (2) and (3)), and firms with and without prior leave (columns (4) and (5)). Panel D shows women's total earnings in the first four months following birth and the maternity leave payments made by the social security insurance for this period (APG payments). The difference between these two numbers corresponds to the firms' voluntary top-up of mandated maternity leave payments.

All in all, the mandate primarily had small and temporary effects on women's employment and earnings after the birth of their first child. Only mothers that worked in firms without paid maternity leave prior to the mandate saw changes in their earnings that persisted in the long run. These women's monthly earnings increased by around 260-280 Swiss francs after birth, an effect that corresponds to around 6% of their median pre-birth earnings.

The overall financial impact of the mandate, that is, the sum of the direct effect and the indirect effect through endogenous responses in labour supply and fertility, can be measured by the change in

Bergsvik et al. (2021) highlight that the symbolic meaning and signalling effect of parental leave and other pronatalist policies should not be underestimated.

[48] These figures are not included in this draft, but they are available upon request.

Table 2: Summary of main results

	Overall	Child care availability		Firm type	
		High	Low	Prior leave	Other
	(1)	(2)	(3)	(4)	(5)
A. Labour market treatment effects					
Employment 12m post-birth	0.00	0.01	0.00	0.02	0.01
	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)
Employment 60m post-birth	-0.00	0.01	-0.01	-0.00	-0.00
	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)
Earnings 12m post-birth (1,000 CHF)	0.05	0.13	-0.03	-0.00	0.28
	(0.08)	(0.12)	(0.02)	(0.10)	(0.15)
Earnings 60m post-birth (1,000 CHF)	-0.00	-0.01	-0.01	-0.08	0.26
	(0.11)	(0.17)	(0.12)	(0.14)	(0.16)
B. Cumulative financial treatment effects (1,000 CHF)					
Sum of earnings 0m to 3m post-birth	1.10	0.79	1.36	0.68	2.64
	(0.23)	(0.35)	(0.31)	(0.30)	(0.43)
Cumulative earnings 12m post-birth	4.34	4.38	4.12	3.78	8.61
	(1.02)	(1.55)	(1.31)	(1.29)	(1.76)
Cumulative earnings 60m post-birth	5.80	8.66	2.09	4.74	16.27
	(3.89)	(5.98)	(4.91)	(5.15)	(6.59)
C. Subsequent fertility treatment effects					
Second child 60m post-birth	0.03	0.04	0.03	0.05	-0.00
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
D. Women's earnings vs. APG payments in treated cohort (1,000 CHF)					
Total earnings 0m to 3m post-birth	16.16	17.16	15.12	18.73	13.34
Total APG payments	10.34	10.39	10.29	11.38	9.98
Observations	21,146	10,438	10,708	13,115	5,113

Panels A to C show a selection of the diff-in-diff coefficient estimates for different outcome variables in different months after the first birth. Panel D displays the average amount of earnings received by first-time mothers in the four months following birth (incl. maternity leave payments) and the APG payments for the same group of women made by the social insurance scheme. Both lines are given for the treated cohort (i.e. first-time mothers giving birth in July to September 2005. Across columns, we show results for the overall sample, and we distinguish women by their canton of residence for the child care availability results and by their employer's type (prior leave vs not). Low child care availability cantons offered below 10 slots per 100 children aged 0 to 3 in year 2002, while high child care cantons offered 10 slots and more per 100 children. Employed women are classified by whether they worked one year prior to birth in a firm which had a high likelihood of offering paid maternity leave prior to the mandate or not. See section 5.3 for more details on the firm classification procedure. Standard errors are in parentheses.

cumulative earnings five years after the first birth. On average, the financial effect amounts to around 5,800 Swiss francs (1.1 months of median pre-birth earnings), but it varies greatly across regions with different levels of child care availability, and between firms with and without prior leave. The overall financial effect is larger in regions with a higher availability of child care at 8,660 Swiss francs, whereas in regions with lower availability it amounted to 2,090 Swiss francs. This suggests that the availability of formal child care complements paid maternity leave. We note, however, that the difference is not precisely estimated and therefore, not statistically significant. For women in firms without prior leave the overall financial effect exceeds 16,000 Swiss francs (3.6 months of their pre-birth earnings), while it was substantially smaller at 4,700 Swiss francs among women in firms with prior coverage (0.9 months of their pre-birth earnings). Interestingly, the increase in earnings during the months of paid maternity leave (i.e., from 0 to 3m post-birth) contributes only a small share to the overall effect, while the endogenous responses that happen after the paid maternity leave expires, make up the bulk.

Similar to prior research on the effect of extensions in maternity leave benefits (Lalive and Zweimüller, 2009; Raute, 2019) and cash transfers upon birth (González and Trommlerová, 2021), we find that the maternity leave mandate significantly increased subsequent fertility.[49] However, the mechanism behind our result is different. In fact, we find that those women who were financially *less affected* (i.e., those with prior maternity leave) by the reform reacted more by having *a second child*, while it did not affect the subsequent fertility of those who benefitted most from the mandate.

Our results also contribute to the recent discussion on the costs and benefits of voluntary and mandated paid maternity leave for firms. Prior research has documented that extensions in paid maternity leave mandates could have important adjustment costs for firms (Ginja et al., 2022) or negatively affect their probability to survive (Gallen, 2019), even though some of these effects might be primarily confined to small firms (Brenøe et al., 2020). However, firms could also benefit from voluntarily offering paid maternity leave in order to attract women who are more qualified and more committed to remaining in the labour force and to retain female employees after they started their families, and therefore, preserve valuable firm-specific human capital (Uribe et al., 2019).

The mandate is financed through a marginal increase in social security contributions levied on all employers and employees. Therefore, it helped reduce costs for firms that previously covered this benefit directly or indirectly through taking out maternity leave insurance for their female employees with newborn children. Among the first cohort that was fully affected by the mandate, new mothers earned 16,160 Swiss francs in the first quarter after birth. The federal social security fund (APG) contributed about two thirds, or 10,340 Swiss francs to this amount, a contribution that would have been paid by the employer before the reform (Table 2). This contribution varied only marginally across regions with high and low child care coverage, but it was on average slightly larger in firms that had previously offered paid maternity leave, reflecting the fact that women working in these firms also had on average higher pre-birth earnings than those working in other firms (see Table A.3 in the Appendix). Our data does not allow us to study adjustment costs or the survival probability of firms. However, since the mandated leave duration at 14 weeks is short, and the mandate was implemented when voluntary coverage was already widespread, it most probably had only small negative effects on firms (if any at all) and the benefits seem to have outweighed the costs.[50]

[49] Raute (2019) studies a massive extension in maternity leave benefits in Germany which affected high-earning and low-earning women differently. For high-earning women, it corresponded to a transfer of around 21,000 euro and led to a 23% increase in birth rates. The author estimates that an increase of 1,000 euro in benefits would lead to a 2.1% fertility increase. González and Trommlerová (2021) study a 2,500 euro cash transfer upon birth in Spain. The real value of the cash transfer amounts to 220% for the median female monthly earnings. The authors find that the cash transfer increased birth rates by 3%. One should note that both of these studies evaluate the financial effect on *contemporaneous* fertility, while our study is similar to Lalive and Zweimüller (2009) and provides insights into how coverage by a contemporaneous maternity leave mandate affects *future* fertility.

[50] According to Aeppli (2012), two thirds of the surveyed firms find the administrative burden of the maternity leave mandate low and more than 9 out of 10 firms consider that the currently mandated leave length is either appropriate (68%) or even too short (23%).

6 Conclusion

In this paper, we evaluate the impact of the first federal mandate providing paid maternity leave in Switzerland on various labor market outcomes and subsequent fertility. Many women - especially those employed in large firms and public administrations - had access to paid maternity leave through their employer prior to the mandate. Hence, the mandate introduced paid maternity leave for some women (mostly those working in small firms or who were self-employed) and reduced the labor and insurance costs for all employers that had already offered paid maternity leave prior to the mandate. Our main findings and their implications for policy makers can be summarized as follows.

First, the mandate had some small, mostly positive effects on (full-time) employment, job continuity, and real earnings in the medium run, but these effects dissipate in the long run. In contrast, we find sizeable anticipatory effects prior to birth. Earnings increase for women in the last six months of pregnancy (but employment does not), reflecting a relative increase (or lower decrease) in hours worked. In addition, the positive medium run employment effects, starting from 18 months after the birth of the first child, could be interpreted as anticipatory effects for a potential second child. These results indicate that a short leave affects post-birth labor market outcomes only marginally - if at all. However, the large and significant anticipatory effects in terms of increased earnings prior to birth indicate that any future studies should include the pre-birth period to capture the overall impact of similar reforms. Comparing only post-birth outcomes would probably underestimate the full impact of such mandates.

Second, we find a strong and significant impact of the mandate on *subsequent* fertility. Thirty months after the birth of their first child, women affected by the reform were three percentage points more likely to have a second child, an effect that persists in the long run. The mandate not only significantly increased the financial means of women, but it also promoted full-time work among mothers with young children. The fertility effect is similar to that reported by Lalive and Zweimüller (2009) for the Austrian reform in 1991, a context with an extension of parental leave with low payments. This comparison suggests that effects of family leave might be highly non-linear with respect to duration.

Third, our analysis offers empirical evidence on some complementarity between a maternity leave mandate and the availability of child care for very young children. In cantons with higher child care availability, the mandate had significant effects on cumulative earnings and subsequent fertility. We also estimate a small, positive impact on employment and earnings in the medium run after birth in high-child care cantons, yet these effects are not statistically signifant. Our findings on the complementarity between paid maternity leave and availability of formal child care, in particular for subsequent fertility, are similar to those found by Danzer et al. (2020) for an Austrian reform and suggest that these two important family policy tools should not be analysed and implemented separately.[51] This result warrants further attention both from researchers as well as policy makers to improve the work-life balance of families around the globe.

Finally, our novel findings reveal large and significant trickle down effects of the mandate where it supersedes prior employer-provided maternity leave insurance. Firms with prior leave benefited from reduced labor and insurance costs and passed some of these savings on to their female workers by extending leave beyond the mandated minimum level and hiring replacement workers. Women employed in firms with pre-mandate leave have higher job continuity and subsequent fertility as a result of the mandate (but only moderately additional financial means). While women without prior access to paid leave work more intensively upon returning from leave, which results in a very large overall financial gain after the mandate comes into effect. Our results suggest that the marginal value of augmenting a short paid maternity leave is very high: the trickle down effects in firms with prior leave can be just as consequential as the effects of introducing the paid leave for women working in firms without prior leave. Other universal paid maternity leave mandates have been found to support the disadvantaged (e.g. low-income) women more (Olivetti and Petrongolo (2017); Broadway et al. (2020)), whereas in our context, all women were positively affected by the mandate - but along different dimensions. Understanding the importance of trickle down effects is also particularly relevant in a context like the U.S., where the introduction of a federal paid family leave mandate is currently being debated and where some employers already offer some form of paid leave (Bartel et al. (2021)).

[51] Our results stand in contrast to those of Kleven et al. (2020) for Austria where the authors cannot find any interaction effects between parental leave and child care provision in Austria on gender earnings gaps. Malkova (2018) finds large fertility effects of paid maternity leave in Soviet Russia and discusses the availability of widespread and affordable preschool care for children of all ages at the same time as a reason for this finding.

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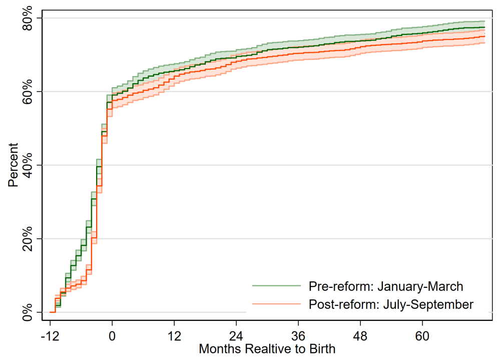
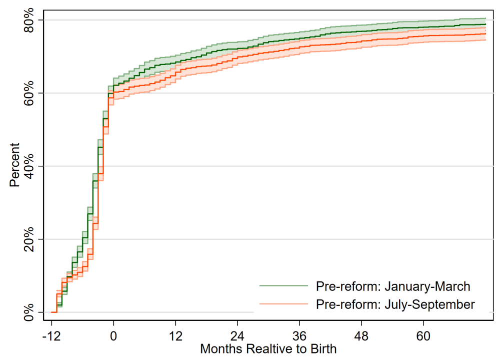
A Further Descriptive Evidence

Appendix A.A Descriptive Evidence on Changes in Marital Status

Figure A.1 presents the cumulative share of women (and its 95% confidence interval) who had been single one year prior to birth and were married in month t relative to the birth of their first child. The difference in marriage rates prior to birth observed between the pre-reform and post-reform mothers in 2005 are also apparent for the 2004 cohort. This suggest the presence of strong seasonal effects.



(a) July-September Treated - 2005



(b) July-September Treated - 2004

Figure A.1: Single one year before birth to married

Appendix A.B Stability in Demographic Composition of Groups

Figure A.1 shows the results from the same DiD regression specification as used for the main results where the dependent variable is age at first birth and marital status (i.e. an indicator for being married) one year prior to birth and at birth. The interaction coefficient of $Reform_i$ and $Months_i$ is not statistically significant at any conventional level for these regressions. These results suggest that the demographic composition of our sample of women was not affected by the mandate during the period studied.

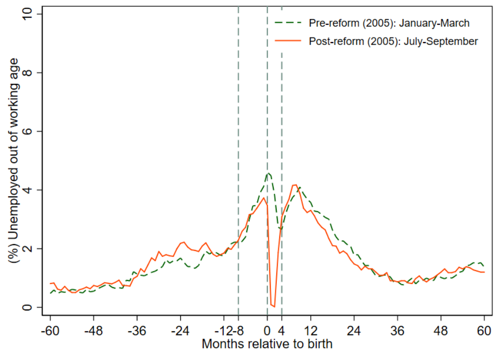
Table A.1: Diff-in-Diff on demographic control variables

	Age		Marital status			
	at birth		12m prior birth		at birth	
DiD coefficient	-0.055 (0.137)	-0.075 (0.115)	0.007 (0.014)	0.008 (0.014)	0.015 (0.011)	0.010 (0.011)
Controls		✓		✓		✓

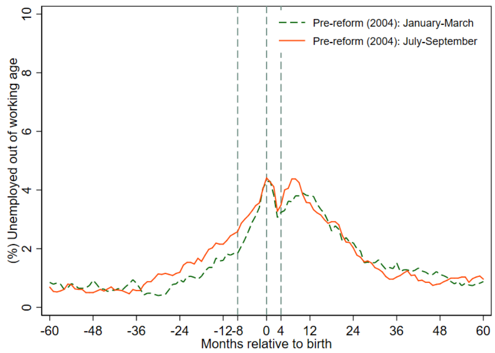
Notes: Treatment effects identified by our DiD model for all Swiss women in our sample (excl. Geneva) where the dependent variable is age at birth of first child or the marital status (i.e. indicator of being married) one year prior to birth and at birth. Regression columns with controls include mothers' characteristics such as age at first birth (only for marital status regressions), indicator of marital status one year prior to birth (only for age at first birth regression), cumulative work experience and cumulative income from six to one year prior to first birth of first childbirth. Robust standard errors are in parentheses. In each regression there are 21,146 observations.

Appendix A.C Descriptive Evidence on Unemployment

Figure A.2 provides descriptive evidence on the dynamics of unemployment in a 10-year window around the birth of the first child. It plots unemployment of pre-reform (dashed line) and post-reform women (bold line). The first column shows the outcomes for pre- and post-reform women who had a child in year 2005. The second column shows the same outcomes for women who had a child in the same two three-month periods in year 2004.



(a) Share Unemployed - 2005



(b) Share Unemployed - 2004

Notes: The figures of unemployment include all Swiss women. Monthly earnings are computed using the sample of employed Swiss women only. Women on paid maternity leave are classified as employed. Women on unpaid maternity leave are classified as out of the labour force.

Source: Authors' calculations using the merged data set.

Figure A.2: Unemployment - All Swiss women in sample

Figures A.2a and A.2b reveal hump-shaped unemployment rates around the time of giving birth both in 2004 and 2005. Generally, unemployment increases until birth (doubling from below 2 per cent 12 months prior to birth), plateaues until 12 months after birth and then decreases within another 12 months almost to its pre-birth level. For post-reform women in 2005 we observe virtually no unemployment in the four months following birth, a direct result of the implementation of the federal maternity leave mandate which also covers unemployed women as long as they are fulfilling the labour force eligibility criteria. For these women the difference is insofar important as paid maternity leave comes without obligations and does not require a minimum number of applications to remain eligible in contrast to unemployment insurance.

Appendix A.D Descriptive Statistics by Child Care Availability

Table A.2 presents descriptive statistics on first-time mothers living in low- and high-child care availability cantons before and after the mandate came into effect. Low-child care availability cantons offered below 10 places per 100 children aged 0 to 3 in year 2002, while high child care cantons offered 10 places and more per 100 children.

Table A.2: Descriptive Statistics by Child Care Availability

	High child care availability			Low child care availability		
	Jan-March05	Jul-Sept05	Mean Difference	Jan-March05	Jul-Sept05	Mean Difference
A. Demographics						
Age at First Birth	30.771	30.193	-0.578	30.212	29.813	-0.398
	(0.101)	(0.096)	(0.139)	(0.099)	(0.096)	(0.138)
Age First Observed	18.865	18.846	-0.019	18.647	18.574	-0.073
	(0.048)	(0.048)	(0.068)	(0.042)	(0.036)	(0.055)
Married	0.762	0.785	0.023	0.766	0.767	0.000
	(0.008)	(0.008)	(0.012)	(0.008)	(0.008)	(0.012)
B. Labor market history						
In LF 12 months prior to birth	0.903	0.909	0.006	0.903	0.914	0.012
	(0.006)	(0.005)	(0.008)	(0.006)	(0.005)	(0.008)
Employed 12 months prior to birth	0.979	0.979	0.000	0.982	0.980	-0.002
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)
Monthly income from employment (CHF) 12m prior to first birth	5502.620	5447.484	-55.136	4939.287	5018.345	79.058
	(65.417)	(61.798)	(89.938)	(47.488)	(53.797)	(71.975)
Cum. experience (months) from 6y to 12m prior to first birth	50.386	50.622	0.236	51.012	51.512	0.500
	(0.333)	(0.317)	(0.459)	(0.324)	(0.315)	(0.452)
C. Eligibility and treatment						
Eligible	0.847	0.849	0.002	0.835	0.856	0.021
	(0.007)	(0.007)	(0.010)	(0.007)	(0.007)	(0.010)
Received federal paid maternity leave	0.000	0.799	0.799	0.000	0.817	0.817
	(0.000)	(0.008)	(0.008)	(0.000)	(0.007)	(0.008)
Received federal paid maternity leave among eligible	0.000	0.890	0.890	0.000	0.903	0.903
	(0.000)	(0.007)	(0.007)	(0.000)	(0.006)	(0.006)
Share with 50% of pre-birth income 1m to 3m after birth	0.604	0.861	0.257	0.501	0.839	0.338
	(0.010)	(0.007)	(0.012)	(0.010)	(0.007)	(0.012)
Observations	2,529	2,740		2,590	2,672	

Mothers who had their first child between January and March in 2005 were not affected by the mandate and those who had their first child between July-September in 2005 are classified as after the mandate (excl. Geneva). Standard errors are in parentheses. We distinguish women by their canton of residence. Low child care availability cantons offered below 10 places per 100 children aged 0 to 3 in year 2002, while high child care cantons offered 10 places and more per 100 children.

Appendix A.E Descriptive Statistics by Firm Type (with and without prior maternity leave)

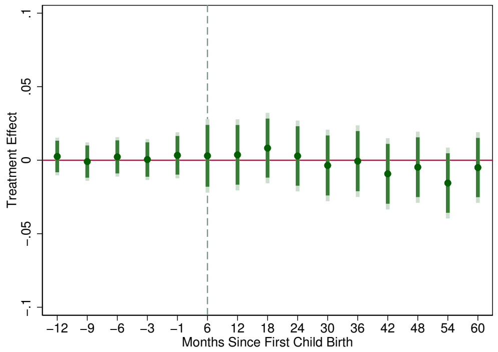
Table A.3: Descriptive statistics by type of firm (with or without pre-mandate leave)

	Firm with Prior Paid Leave			Other Firm		
	Jan-March05	Jul-Sept05	Mean Difference	Jan-March05	Jul-Sept05	Mean Difference
A. Demographics						
Age at First Birth	31.107	30.574	-0.533	29.808	29.393	-0.415
	(0.086)	(0.080)	(0.117)	(0.137)	(0.138)	(0.194)
Age First Observed	18.769	18.741	-0.028	18.552	18.507	-0.045
	(0.037)	(0.036)	(0.052)	(0.061)	(0.053)	(0.080)
Married	0.768	0.784	0.016	0.771	0.781	0.010
	(0.008)	(0.007)	(0.010)	(0.012)	(0.012)	(0.017)
B. Labor market history						
In LF 12 months prior to birth	0.983	0.987	0.004	0.983	0.993	0.009
	(0.002)	(0.002)	(0.003)	(0.004)	(0.002)	(0.004)
Employed 12 months prior to birth	0.981	0.977	-0.004	0.980	0.985	0.004
	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)
Monthly income from employment (CHF) 12m prior to first birth	5542.664	5486.471	-56.193	4413.376	4523.605	110.229
	(51.387)	(49.445)	(71.331)	(66.074)	(83.922)	(106.531)
Cum. experience (months) from 6y to 12m prior to first birth	53.476	53.893	0.417	52.883	53.428	0.545
	(0.233)	(0.213)	(0.315)	(0.384)	(0.372)	(0.535)
C. Eligibility and treatment						
Eligible	0.921	0.925	0.004	0.859	0.870	0.011
	(0.005)	(0.004)	(0.007)	(0.010)	(0.010)	(0.014)
Received federal paid maternity leave	0.000	0.860	0.860	0.000	0.842	0.842
	(0.000)	(0.006)	(0.006)	(0.000)	(0.011)	(0.010)
Received federal paid maternity leave among eligible	0.000	0.892	0.892	0.000	0.918	0.918
	(0.000)	(0.005)	(0.006)	(0.000)	(0.009)	(0.008)
Share with 50% of pre-birth income 1m to 3m after birth	0.689	0.916	0.227	0.302	0.826	0.524
	(0.008)	(0.005)	(0.009)	(0.013)	(0.011)	(0.017)
Observations	3,259	3,452		1,247	1,201	

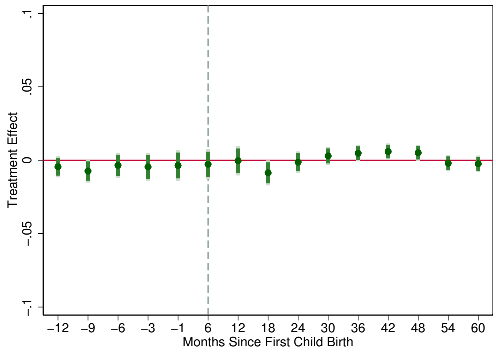
Mothers who had their first child between January and March in 2005 were not affected by the mandate and those who had their first child between July-September in 2005 are classified as after the mandate (excl. Geneva). Standard errors are in parentheses. We distinguish employed women by whether they worked in firm which had a high likelihood of offering paid maternity leave prior to the mandate or another firm. To do so, we predict the likelihood of employed mothers in the control group receiving paid maternity leave prior to the mandate based on her firm's characteristics (firm size, labour market region and social security fund as a proxy for the industry). Firms with a predicted likelihood above 50% are classified as "firms with prior paid leave" and all other firms are classified as "other firms". See main text for more details.

B Additional estimation results

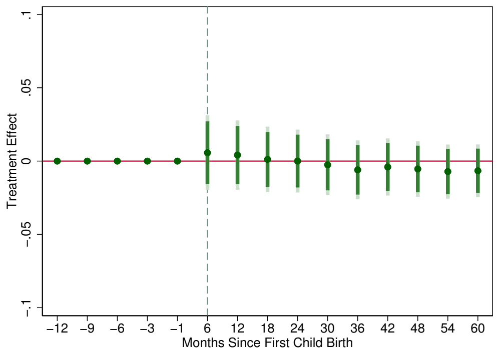
This section presents additional estimation results on contemporaneous and cumulative outcomes which have not been included in the main text.



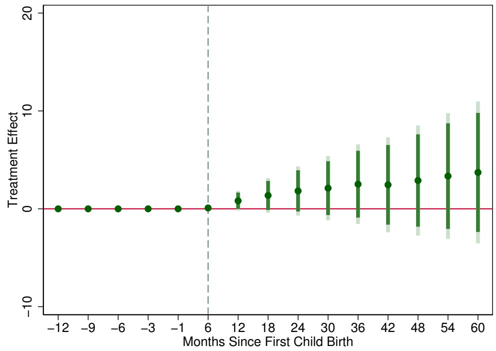
(a) Labor Force



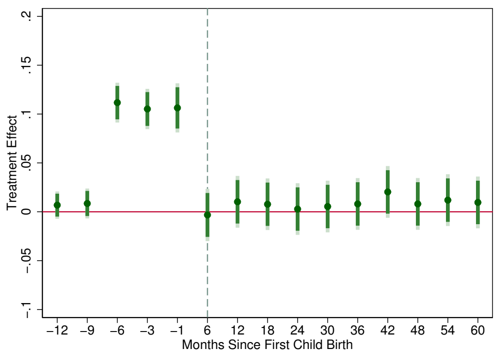
(b) Share Unemployed



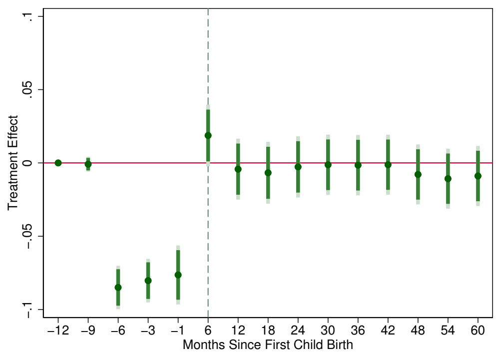
(c) Ever Returned to Employment Post-Birth



(d) Cumulative Employment Earnings Post-Birth (in 1000s CHF)



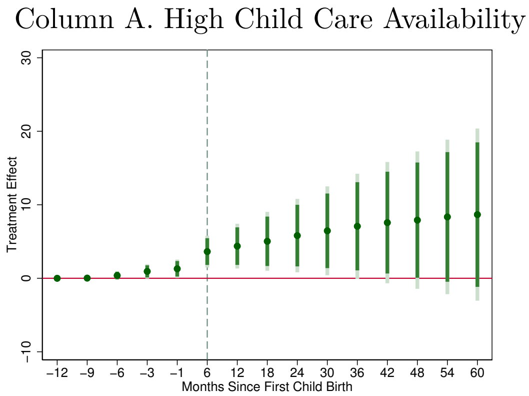
(e) Share Working Full-time (more than 80%)



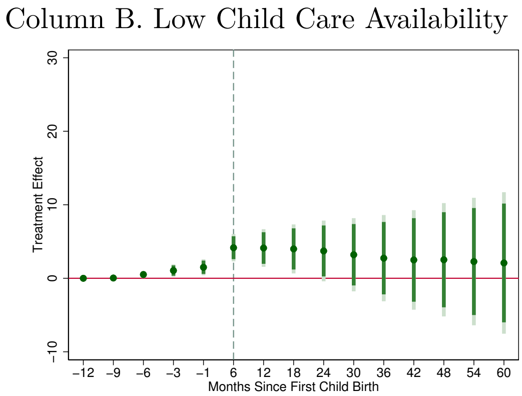
(f) Share Working High Part-time (50 to 80%)

Notes: Treatment effects identified by our DiD model for all Swiss women in our sample. Subfigures (a) and (b) show the effects of the federal mandate on the share of women in the labor force and unemployment at various points in time pre- and post-birth. Subfigures (c) and (d) show the effects of the federal mandate on the share of women who ever returned to employment after birth and cumulative real employment earnings of employed women since 6 months after the first birth. Subfigures (e) and (f) show the effects of the federal mandate on the share of women working full-time (measured as earning at least 80% of earnings 12 months prior to the first child's birth) and on the share of women working a high part-time (measured as earning 50 to 80% of pre-birth earnings). Light vertical lines indicate the 95 per cent confidence intervals, the dark vertical lines indicate the 90 per cent confidence intervals. The dashed vertical line separates the time horizon into a pre-birth and post-birth period. Robust standard errors are used.

Figure B.1: Further Results on Labor Market Outcomes



(a) Cumulative Total Earnings Since 9 Months Pre-Birth (in 1000s CHF)



(b) Cumulative Total Earnings Since 9 Months Pre-Birth (in 1000s CHF)

Notes: This figure shows the treatment effects for women according to the availability of childcare places for children aged 0 to 3 years in the canton of residence. We distinguish cantons by whether they offer above (Figures in left column, Panel A) or below median (Figures in right column, Panel B) number of childcare places in the year 2002 (i.e., 10 places and more per 100 children corresponds to above-median, below 10 places per 100 children corresponds to below-median childcare availability). Light vertical lines indicate the 95% confidence intervals, the dark vertical lines indicate the 90% confidence intervals. The dashed vertical line separates the time horizon into a pre-birth and post-birth period. Robust standard errors are used.

Figure B.3: Heterogeneity by Child Care Availability: Cumulative Effects

C Tables with Regression Results

Table C.1: Results on labour market outcomes and financial impact

	-12	-9	-6	-3	-1	6	12	18	24	30	36	42	48	54	60
Employed	0.007	0.006	0.006	0.005	0.007	0.006	0.004	0.017	0.004	-0.007	-0.005	-0.015	-0.010	-0.013	-0.003
Ste	0.007	0.007	0.008	0.008	0.009	0.013	0.013	0.012	0.012	0.013	0.013	0.012	0.012	0.012	0.012
P-value	0.327	0.385	0.460	0.538	0.440	0.661	0.751	0.177	0.738	0.603	0.664	0.224	0.429	0.277	0.836
Income	0.025	0.044	0.237	0.200	0.222	0.087	0.055	0.055	-0.012	0.004	0.072	0.064	0.095	0.074	-0.001
Ste	0.045	0.046	0.051	0.051	0.064	0.085	0.078	0.076	0.079	0.089	0.094	0.087	0.097	0.092	0.106
P-value	0.577	0.336	0.000	0.000	0.000	0.306	0.483	0.473	0.877	0.963	0.442	0.463	0.326	0.419	0.994
Same Employer	0.000	0.007	0.018	0.017	0.007	-0.001	0.022	-0.014	0.032	0.062	0.015	0.036	0.015	-0.005	0.005
Ste	.	0.007	0.010	0.011	0.012	0.016	0.017	0.016	0.016	0.016	0.016	0.015	0.015	0.015	0.015
P-value	.	0.323	0.087	0.103	0.533	0.959	0.193	0.382	0.053	0.000	0.349	0.019	0.340	0.712	0.753
Cum. Income	0.000	0.032	0.452	1.015	1.424	3.965	4.343	4.640	4.923	5.027	5.145	5.313	5.541	5.684	5.793
Ste	.	0.048	0.195	0.331	0.424	0.731	1.020	1.337	1.662	2.009	2.372	2.738	3.109	3.496	3.890
P-value	.	0.513	0.020	0.002	0.001	0.000	0.000	0.001	0.003	0.012	0.030	0.052	0.075	0.104	0.136
FT Employed	0.007	0.009	0.112	0.105	0.106	-0.003	0.010	0.008	0.003	0.005	0.008	0.020	0.008	0.012	0.010
Ste	0.007	0.008	0.010	0.011	0.013	0.014	0.014	0.013	0.013	0.013	0.014	0.014	0.014	0.014	0.014
P-value	0.337	0.275	0.000	0.000	0.000	0.814	0.449	0.565	0.835	0.688	0.551	0.132	0.550	0.377	0.478
High PT Employed	0.000	-0.001	-0.085	-0.080	-0.076	0.019	-0.004	-0.007	-0.003	-0.001	-0.002	-0.001	-0.008	-0.011	-0.009
Ste	.	0.003	0.008	0.008	0.010	0.011	0.011	0.011	0.011	0.010	0.011	0.010	0.010	0.010	0.010
P-value	.	0.741	0.000	0.000	0.000	0.081	0.690	0.533	0.800	0.906	0.885	0.908	0.452	0.304	0.395
FT & child<2	0.000	0.000	0.000	0.000	0.000	-0.003	0.010	0.008	-0.004	0.016	0.022	0.027	0.017	-0.001	-0.006
Ste	.	.	.	.	.	0.014	0.014	0.013	0.008	0.010	0.011	0.011	0.011	0.010	0.009
P-value	.	.	.	.	.	0.814	0.449	0.565	0.640	0.120	0.049	0.019	0.102	0.893	0.496

Notes: Treatment effects identified by a DiD model for all Swiss women in the sample (excl. Geneva). All regressions control for mothers' characteristics such as age at first birth, marital status, cumulative work experience and cumulative income over 6 to 1 years prior to first childbirth. The table shows the effects of the federal mandate on the share of women in employment (*employed*) at various points in time pre- and post-birth. The line denoted *Income* shows the effects on real earnings from employment on employed women and *Same employer* the effects on the share of employed women returning to their pre-birth employer (i.e., the main employer 12 months prior to birth). Line *Cum. Income* presents the effects on the cumulative total real earnings of all women (including earnings from employment, self-employment, maternity leave benefits, unemployment and other social insurance benefits) since 9 months prior to the first birth. All earnings are adjusted for inflation using the CPI (with base year 2010). Line *FT Employed* presents the effects on the share working full-time (i.e. earning at least 80% compared to 1 year prior to birth), line *High PT Employed* presents the effects on the share working a high part-time (i.e. earning between 50% and 80% compared to 1 year prior to birth). Line *FT & child<2* presents the effects on the share working full-time (i.e. earning at least 80% compared to 1 year prior to birth) whose youngest child is less than 2 years old. Robust standard errors and p-values of individual significance test are reported below each estimate.

Table C.2: Heterogeneneous Effects by Child Care Availability: A. High Care Availability

	-12	-9	-6	-3	-1	6	12	18	24	30	36	42	48	54	60
Employed	0.003	0.005	0.004	0.005	0.005	0.012	0.005	0.017	0.024	-0.001	-0.004	-0.015	-0.003	-0.007	0.005
Ste	0.010	0.011	0.011	0.012	0.013	0.018	0.018	0.017	0.017	0.017	0.017	0.017	0.017	0.017	0.017
P-value	0.741	0.629	0.722	0.683	0.715	0.502	0.785	0.326	0.171	0.964	0.816	0.385	0.854	0.670	0.784
Income	0.048	0.037	0.216	0.202	0.173	0.085	0.128	0.145	0.035	0.050	0.147	0.095	0.115	0.152	-0.005
Ste	0.069	0.070	0.079	0.077	0.097	0.131	0.119	0.115	0.118	0.136	0.149	0.130	0.140	0.140	0.170
P-value	0.485	0.593	0.006	0.009	0.073	0.518	0.280	0.207	0.768	0.712	0.323	0.466	0.411	0.276	0.977
Same Employer	0.000	0.014	0.015	0.018	0.021	0.020	0.011	-0.019	0.014	0.043	0.012	0.038	0.018	-0.005	-0.011
Ste	.	0.010	0.015	0.015	0.017	0.023	0.023	0.023	0.023	0.023	0.023	0.022	0.022	0.022	0.021
P-value	.	0.170	0.305	0.236	0.199	0.375	0.635	0.400	0.542	0.061	0.598	0.093	0.414	0.829	0.613
Cum. Income	0.000	0.023	0.378	0.938	1.292	3.634	4.373	5.032	5.808	6.458	7.075	7.573	7.911	8.348	8.662
Ste	.	0.074	0.301	0.501	0.641	1.106	1.553	2.043	2.547	3.085	3.647	4.209	4.767	5.362	5.975
P-value	.	0.757	0.210	0.061	0.044	0.001	0.005	0.014	0.023	0.036	0.052	0.072	0.097	0.120	0.147
FT Employed	0.003	0.007	0.095	0.092	0.086	-0.023	0.012	0.005	-0.010	-0.005	-0.003	0.016	0.002	0.009	0.007
Ste	0.010	0.011	0.015	0.015	0.018	0.019	0.019	0.019	0.019	0.019	0.019	0.019	0.019	0.019	0.019
P-value	0.751	0.506	0.000	0.000	0.000	0.228	0.519	0.786	0.613	0.811	0.883	0.403	0.913	0.624	0.726
High PT Employed	0.000	-0.004	-0.079	-0.074	-0.075	0.027	-0.005	-0.010	-0.007	0.007	0.015	0.002	-0.011	-0.008	-0.003
Ste	.	0.004	0.011	0.011	0.015	0.016	0.016	0.016	0.016	0.015	0.015	0.015	0.015	0.015	0.015
P-value	.	0.302	0.000	0.000	0.000	0.084	0.738	0.531	0.671	0.639	0.321	0.893	0.488	0.608	0.847
FT & Child<2	0.000	0.000	0.000	0.000	0.000	-0.023	0.012	0.005	-0.009	0.012	0.015	0.027	0.019	-0.007	-0.010
Ste	.	.	.	.	.	0.019	0.019	0.019	0.012	0.015	0.016	0.016	0.015	0.014	0.013
P-value	.	.	.	.	.	0.228	0.519	0.786	0.458	0.409	0.353	0.102	0.214	0.619	0.452

Notes: Treatment effects identified by a DiD model for all Swiss women (excl. Geneva). Panel A presents estimates for mothers residing in cantons with high child care availability, Panel B presents the corresponding estimates for those in low child care availability cantons. Panel C presents the difference in estimates between Panels A and B. All regressions control for mothers' characteristics such as age at first birth, marital status, cumulative work experience and cumulative income over 6 to 1 years prior to first childbirth. The table shows the effects of the federal mandate on the share of women in employment (*employed*) at various points in time pre- and post-birth. *Income* shows the effects on real earnings from employment of employed women and *Same employer* the effects on the share of employed women working for their pre-birth employer. *Cum. Income* presents the effects on the cumulative total real earnings of all women (including earnings from employment, self-employment, maternity leave benefits, unemployment and other social insurance benefits) since 9 months prior to the first birth. All earnings are adjusted for inflation using the CPI (with base year 2010). Line *FT Employed* presents the effects on the share working full-time (i.e. earning at least 80% compared to 1 year prior to birth), line *High PT Employed* presents the effects on the share working a high part-time (i.e. earning between 50% and 80% compared to 1 year prior to birth). Line *FT & child<2* presents the effects on the share working full-time (i.e. earning at least 80% compared to 1 year prior to birth) whose youngest child is less than 2 years old. Robust standard errors and p-values of individual significance test are reported below each estimate.

Table C.3: Heterogeneneous Effects by Child Care Availability: B. Low Care Availability

	-12	-9	-6	-3	-1	6	12	18	24	30	36	42	48	54	60
Employed	0.010	0.007	0.008	0.005	0.009	-0.003	0.001	0.015	-0.017	-0.014	-0.009	-0.017	-0.018	-0.021	-0.011
Ste	0.010	0.010	0.011	0.011	0.013	0.019	0.018	0.018	0.018	0.018	0.018	0.018	0.018	0.018	0.018
P-value	0.310	0.467	0.458	0.640	0.493	0.891	0.937	0.399	0.348	0.437	0.625	0.336	0.302	0.231	0.524
Income	0.000	0.046	0.248	0.189	0.262	0.074	-0.025	-0.044	-0.079	-0.061	-0.015	0.018	0.058	-0.019	-0.008
Ste	0.058	0.059	0.066	0.066	0.083	0.105	0.100	0.098	0.103	0.111	0.110	0.111	0.131	0.115	0.116
P-value	1.000	0.435	0.000	0.004	0.002	0.486	0.803	0.651	0.445	0.587	0.892	0.874	0.656	0.869	0.948
Same Employer	0.000	-0.000	0.019	0.016	-0.007	-0.027	0.030	-0.012	0.047	0.080	0.015	0.030	0.009	-0.008	0.017
Ste	.	0.010	0.014	0.015	0.017	0.024	0.024	0.023	0.023	0.023	0.022	0.021	0.021	0.020	0.020
P-value	.	0.971	0.186	0.287	0.663	0.264	0.205	0.602	0.044	0.000	0.497	0.156	0.670	0.694	0.380
Cum. Income	0.000	0.036	0.509	1.052	1.497	4.158	4.118	3.995	3.718	3.203	2.737	2.494	2.522	2.278	2.086
Ste	.	0.063	0.245	0.429	0.551	0.947	1.306	1.700	2.107	2.539	2.993	3.455	3.933	4.425	4.911
P-value	.	0.568	0.037	0.014	0.007	0.000	0.002	0.019	0.078	0.207	0.360	0.470	0.521	0.607	0.671
FT Employed	0.010	0.009	0.126	0.116	0.125	0.016	0.008	0.010	0.015	0.015	0.019	0.024	0.014	0.014	0.011
Ste	0.010	0.011	0.015	0.015	0.018	0.019	0.019	0.019	0.019	0.019	0.019	0.019	0.019	0.019	0.019
P-value	0.317	0.410	0.000	0.000	0.000	0.397	0.662	0.585	0.417	0.425	0.325	0.197	0.467	0.466	0.549
High PT Employed	0.000	0.002	-0.089	-0.086	-0.077	0.009	-0.004	-0.005	-0.000	-0.011	-0.019	-0.006	-0.006	-0.014	-0.015
Ste	.	0.004	0.011	0.011	0.014	0.015	0.014	0.015	0.014	0.014	0.014	0.014	0.014	0.014	0.014
P-value	.	0.528	0.000	0.000	0.000	0.519	0.771	0.732	0.978	0.455	0.186	0.689	0.662	0.313	0.290
FT & child<2	0.000	0.000	0.000	0.000	0.000	0.016	0.008	0.010	0.001	0.021	0.030	0.028	0.017	0.005	-0.003
Ste	.	.	.	.	.	0.019	0.019	0.019	0.012	0.015	0.016	0.016	0.015	0.014	0.013
P-value	.	.	.	.	.	0.397	0.662	0.585	0.929	0.165	0.059	0.082	0.264	0.744	0.821

Notes: Treatment effects identified by a DiD model for all Swiss women (excl. Geneva). Panel A presents estimates for mothers residing in cantons with high child care availability, Panel B presents the corresponding estimates for those in low child care availability cantons. Panel C presents the difference in estimates between Panels A and B. All regressions control for mothers' characteristics such as age at first birth, marital status, cumulative work experience and cumulative income over 6 to 1 years prior to first childbirth. The table shows the effects of the federal mandate on the share of women in employment (*employed*) at various points in time pre- and post-birth. *Income* shows the effects on real earnings from employment of employed women and *Same employer* the effects on the share of employed women working for their pre-birth employer. *Cum. Income* presents the effects on the cumulative total real earnings of all women (including earnings from employment, self-employment, maternity leave benefits, unemployment and other social insurance benefits) since 9 months prior to the first birth. All earnings are adjusted for inflation using the CPI (with base year 2010). Line *FT Employed* presents the effects on the share working full-time (i.e. earning at least 80% compared to 1 year prior to birth), line *High PT Employed* presents the effects on the share working a high part-time (i.e. earning between 50% and 80% compared to 1 year prior to birth). Line *FT & child<2* presents the effects on the share working full-time (i.e. earning at least 80% compared to 1 year prior to birth) whose youngest child is less than 2 years old. Robust standard errors and p-values of individual significance test are reported below each estimate.

Table C.4: Heterogeneneous Effects by Child Care Availability: C. Difference in Care Availability

	-12	-9	-6	-3	-1	6	12	18	24	30	36	42	48	54	60
Employed	-0.007	-0.002	-0.004	-0.001	-0.004	0.015	0.003	0.002	0.040	0.013	0.005	0.002	0.015	0.014	0.016
Ste	0.014	0.015	0.015	0.016	0.018	0.026	0.025	0.025	0.025	0.025	0.025	0.025	0.025	0.025	0.025
P-value	0.643	0.876	0.801	0.970	0.819	0.570	0.895	0.942	0.104	0.598	0.850	0.930	0.540	0.572	0.516
Income	0.048	-0.008	-0.032	0.013	-0.089	0.011	0.153	0.190	0.114	0.111	0.162	0.077	0.056	0.171	0.003
Ste	0.090	0.091	0.102	0.101	0.128	0.168	0.155	0.151	0.157	0.176	0.185	0.171	0.191	0.181	0.206
P-value	0.592	0.928	0.755	0.901	0.485	0.948	0.324	0.210	0.469	0.528	0.381	0.653	0.768	0.343	0.990
Same Employer	0.000	0.014	-0.004	0.002	0.029	0.047	-0.019	-0.007	-0.033	-0.037	-0.003	0.007	0.009	0.003	-0.028
Ste	.	0.014	0.021	0.021	0.024	0.033	0.033	0.033	0.033	0.032	0.032	0.031	0.030	0.030	0.029
P-value	.	0.318	0.847	0.917	0.223	0.155	0.559	0.829	0.314	0.244	0.927	0.809	0.761	0.911	0.333
Cum. Income	0.000	-0.013	-0.132	-0.114	-0.205	-0.524	0.255	1.037	2.089	3.255	4.338	5.079	5.390	6.070	6.576
Ste	.	0.097	0.388	0.660	0.845	1.456	2.029	2.658	3.305	3.995	4.718	5.445	6.180	6.951	7.734
P-value	.	0.893	0.734	0.863	0.808	0.719	0.900	0.696	0.527	0.415	0.358	0.351	0.383	0.383	0.395
FT Employed	-0.007	-0.001	-0.031	-0.024	-0.040	-0.040	0.004	-0.005	-0.025	-0.020	-0.021	-0.008	-0.012	-0.004	-0.005
Ste	0.014	0.016	0.021	0.021	0.026	0.027	0.027	0.027	0.027	0.027	0.027	0.027	0.027	0.027	0.027
P-value	0.644	0.926	0.140	0.255	0.123	0.146	0.879	0.850	0.352	0.466	0.427	0.760	0.667	0.873	0.865
High PT Employed	0.000	-0.006	0.010	0.012	0.001	0.018	-0.001	-0.005	-0.006	0.018	0.034	0.008	-0.004	0.007	0.012
Ste	.	0.005	0.015	0.015	0.021	0.021	0.021	0.022	0.021	0.021	0.021	0.021	0.021	0.021	0.021
P-value	.	0.237	0.501	0.437	0.947	0.414	0.962	0.820	0.769	0.394	0.104	0.711	0.836	0.750	0.558
FT & Child<2	0.000	0.000	0.000	0.000	0.000	-0.040	0.004	-0.005	-0.010	-0.009	-0.016	-0.002	0.002	-0.011	-0.007
Ste	.	.	.	.	.	0.027	0.027	0.027	0.017	0.021	0.023	0.023	0.021	0.020	0.018
P-value	.	.	.	.	.	0.146	0.879	0.850	0.568	0.680	0.490	0.943	0.920	0.560	0.705

Notes: Treatment effects identified by a DiD model for all Swiss women (excl. Geneva). Panel A presents estimates for mothers residing in cantons with high child care availability, Panel B presents the corresponding estimates for those in low child care availability cantons. Panel C presents the difference in estimates between Panels A and B. All regressions control for mothers' characteristics such as age at first birth, marital status, cumulative work experience and cumulative income over 6 to 1 years prior to first childbirth. The table shows the effects of the federal mandate on the share of women in employment (*employed*) at various points in time pre- and post-birth. *Income* shows the effects on real earnings from employment of employed women and *Same employer* the effects on the share of employed women working for their pre-birth employer. *Cum. Income* presents the effects on the cumulative total real earnings of all women (including earnings from employment, self-employment, maternity leave benefits, unemployment and other social insurance benefits) since 9 months prior to the first birth. All earnings are adjusted for inflation using the CPI (with base year 2010). Line *FT Employed* presents the effects on the share working full-time (i.e. earning at least 80% compared to 1 year prior to birth), line *High PT Employed* presents the effects on the share working a high part-time (i.e. earning between 50% and 80% compared to 1 year prior to birth). Line *FT & child<2* presents the effects on the share working full-time (i.e. earning at least 80% compared to 1 year prior to birth) whose youngest child is less than 2 years old. Robust standard errors and p-values of individual significance test are reported below each estimate.

Table C.5: Heterogeneneous Effects by Firms Size: A. Large firms

	-12	-9	-6	-3	-1	6	12	18	24	30	36	42	48	54	60
Employed	0.006	0.014	0.011	0.011	0.014	0.027	0.015	0.034	0.004	-0.017	-0.013	-0.016	-0.006	-0.011	-0.000
Ste	0.006	0.007	0.008	0.009	0.010	0.016	0.015	0.015	0.015	0.015	0.015	0.015	0.015	0.015	0.015
P-value	0.346	0.054	0.171	0.182	0.131	0.088	0.337	0.023	0.805	0.264	0.390	0.288	0.691	0.447	0.985
Income	0.054	0.054	0.257	0.216	0.206	-0.080	-0.003	-0.008	-0.060	-0.059	0.090	-0.013	0.094	-0.007	-0.082
Ste	0.056	0.057	0.063	0.062	0.079	0.097	0.096	0.095	0.099	0.111	0.108	0.106	0.121	0.113	0.142
P-value	0.333	0.338	0.000	0.001	0.009	0.412	0.972	0.931	0.542	0.595	0.405	0.902	0.437	0.950	0.566
Same Employer	0.000	0.012	0.039	0.039	0.024	-0.005	0.040	-0.003	0.047	0.076	0.035	0.063	0.038	0.006	0.015
Ste	.	0.007	0.012	0.012	0.014	0.019	0.020	0.020	0.020	0.020	0.020	0.020	0.019	0.019	0.019
P-value	.	0.095	0.001	0.002	0.081	0.796	0.044	0.897	0.020	0.000	0.080	0.001	0.048	0.765	0.421
Cum. Income	0.000	0.062	0.547	1.228	1.681	3.415	3.777	4.210	4.576	4.560	4.486	4.675	4.839	4.873	4.744
Ste	.	0.058	0.240	0.402	0.517	0.903	1.286	1.716	2.166	2.641	3.129	3.617	4.105	4.618	5.149
P-value	.	0.286	0.023	0.002	0.001	0.000	0.003	0.014	0.035	0.084	0.152	0.196	0.239	0.291	0.357
FT Employed	0.006	0.011	0.117	0.111	0.105	-0.031	0.007	-0.011	-0.002	0.003	0.006	0.016	0.012	0.013	0.013
Ste	0.006	0.007	0.012	0.012	0.016	0.017	0.017	0.017	0.017	0.017	0.017	0.017	0.017	0.017	0.017
P-value	0.346	0.154	0.000	0.000	0.000	0.072	0.692	0.532	0.885	0.882	0.707	0.342	0.491	0.454	0.451
High PT Employed	0.000	-0.001	-0.094	-0.088	-0.081	0.028	-0.015	-0.015	-0.015	-0.011	-0.012	-0.004	-0.012	-0.024	-0.020
Ste	.	0.003	0.010	0.010	0.014	0.015	0.015	0.015	0.015	0.015	0.015	0.014	0.014	0.014	0.014
P-value	.	0.696	0.000	0.000	0.000	0.063	0.323	0.312	0.325	0.462	0.414	0.786	0.396	0.088	0.155
FT & Child<2	0.000	0.000	0.000	0.000	0.000	-0.031	0.007	-0.011	-0.001	0.015	0.026	0.026	0.026	0.010	-0.005
Ste	.	.	.	.	.	0.017	0.017	0.017	0.010	0.013	0.014	0.015	0.014	0.013	0.012
P-value	.	.	.	.	.	0.072	0.692	0.532	0.906	0.250	0.067	0.076	0.065	0.450	0.659

Notes: Treatment effects identified by a DiD model for all Swiss women (excl. Geneva and unless otherwise noted). Panel A presents estimates for mothers working in large firms, Panel B presents the corresponding estimates for those in small firms. Panel C presents the difference in estimates between Panels A and B. All regressions control for mothers' characteristics such as age at first birth, marital status, cumulative work experience and cumulative income over 6 to 1 years prior to first childbirth. The table shows the effects of the federal mandate on the share of women in employment (*employed*) at various points in time pre- and post-birth. *Income* shows the effects on real earnings from employment of employed women and *Same employer* the effects on the share of employed women working for their pre-birth employer. *Cum. Income* presents the effects on the cumulative total real earnings of all women (including earnings from employment, self-employment, maternity leave benefits, unemployment and other social insurance benefits) since 9 months prior to the first birth. All earnings are adjusted for inflation using the CPI (with base year 2010). Line *FT Employed* presents the effects on the share working full-time (i.e. earning at least 80% compared to 1 year prior to birth), line *High PT Employed* presents the effects on the share working a high part-time (i.e. earning between 50% and 80% compared to 1 year prior to birth). Line *FT & child<2* presents the effects on the share working full-time (i.e. earning at least 80% compared to 1 year prior to birth) whose youngest child is less than 2 years old. Robust standard errors and p-values of individual significance test are reported below each estimate.

Table C.6: Heterogeneneous Effects by Firms Size: B. Small firms

	-12	-9	-6	-3	-1	6	12	18	24	30	36	42	48	54	60
Employed	0.024	0.017	0.015	0.016	0.018	-0.011	0.005	0.024	0.037	0.026	0.028	-0.004	-0.001	0.002	-0.001
Ste	0.009	0.013	0.015	0.017	0.020	0.028	0.027	0.027	0.027	0.027	0.027	0.027	0.027	0.027	0.027
P-value	0.009	0.196	0.310	0.345	0.368	0.684	0.863	0.384	0.171	0.344	0.307	0.875	0.977	0.955	0.962
Income	0.031	0.037	0.206	0.192	0.211	0.482	0.282	0.156	0.162	0.288	0.280	0.309	0.172	0.360	0.255
Ste	0.081	0.082	0.092	0.093	0.120	0.201	0.147	0.140	0.146	0.167	0.174	0.182	0.181	0.171	0.158
P-value	0.706	0.655	0.026	0.040	0.079	0.017	0.055	0.267	0.267	0.084	0.108	0.090	0.343	0.035	0.106
Same Employer	0.000	-0.010	-0.025	-0.028	-0.033	0.004	-0.003	-0.028	0.020	0.076	0.002	-0.006	-0.018	-0.018	-0.003
Ste	.	0.015	0.020	0.021	0.023	0.038	0.036	0.035	0.034	0.033	0.032	0.031	0.031	0.030	0.029
P-value	.	0.512	0.223	0.174	0.152	0.909	0.943	0.424	0.569	0.023	0.963	0.856	0.554	0.542	0.911
Cum. Income	0.000	0.085	0.681	1.354	1.855	7.441	8.605	9.442	10.302	11.301	12.434	13.391	14.223	15.206	16.274
Ste	.	0.086	0.334	0.596	0.770	1.297	1.764	2.273	2.779	3.336	3.960	4.616	5.289	5.955	6.593
P-value	.	0.322	0.042	0.023	0.016	0.000	0.000	0.000	0.000	0.001	0.002	0.004	0.007	0.011	0.014
FT Employed	0.024	0.022	0.133	0.125	0.127	0.053	0.022	0.032	-0.001	0.010	0.016	0.039	0.018	0.029	0.021
Ste	0.009	0.013	0.021	0.021	0.027	0.027	0.028	0.028	0.028	0.028	0.028	0.028	0.028	0.028	0.028
P-value	0.009	0.079	0.000	0.000	0.000	0.055	0.426	0.247	0.965	0.721	0.552	0.163	0.526	0.299	0.445
High PT Employed	0.000	-0.001	-0.078	-0.076	-0.059	0.012	0.024	0.027	0.041	0.029	0.026	0.009	-0.003	0.010	0.009
Ste	.	0.006	0.017	0.017	0.022	0.018	0.018	0.018	0.018	0.018	0.019	0.019	0.019	0.019	0.019
P-value	.	0.931	0.000	0.000	0.006	0.519	0.182	0.133	0.025	0.107	0.165	0.636	0.867	0.596	0.643
FT & Child<2	0.000	0.000	0.000	0.000	0.000	0.053	0.022	0.032	-0.013	0.019	0.007	0.016	-0.020	-0.044	-0.021
Ste	.	.	.	.	.	0.027	0.028	0.028	0.020	0.024	0.025	0.025	0.023	0.021	0.020
P-value	.	.	.	.	.	0.055	0.426	0.247	0.516	0.424	0.770	0.513	0.389	0.040	0.279

Notes: Treatment effects identified by a DiD model for all Swiss women (excl. Geneva and unless otherwise noted). Panel A presents estimates for mothers working in large firms, Panel B presents the corresponding estimates for those in small firms. Panel C presents the difference in estimates between Panels A and B. All regressions control for mothers' characteristics such as age at first birth, marital status, cumulative work experience and cumulative income over 6 to 1 years prior to first childbirth. The table shows the effects of the federal mandate on the share of women in employment (*employed*) at various points in time pre- and post-birth. *Income* shows the effects on real earnings from employment of employed women and *Same employer* the effects on the share of employed women working for their pre-birth employer. *Cum. Income* presents the effects on the cumulative total real earnings of all women (including earnings from employment, self-employment, maternity leave benefits, unemployment and other social insurance benefits) since 9 months prior to the first birth. All earnings are adjusted for inflation using the CPI (with base year 2010). Line *FT Employed* presents the effects on the share working full-time (i.e. earning at least 80% compared to 1 year prior to birth), line *High PT Employed* presents the effects on the share working a high part-time (i.e. earning between 50% and 80% compared to 1 year prior to birth). Line *FT & child<2* presents the effects on the share working full-time (i.e. earning at least 80% compared to 1 year prior to birth) whose youngest child is less than 2 years old. Robust standard errors and p-values of individual significance test are reported below each estimate.

Table C.7: Heterogeneneous Effects by Firms Size: C. Difference by firm size

	-12	-9	-6	-3	-1	6	12	18	24	30	36	42	48	54	60
Employed	-0.018	-0.003	-0.005	-0.004	-0.004	0.038	0.010	0.011	-0.033	-0.042	-0.041	-0.012	-0.005	-0.013	0.001
Ste	0.011	0.015	0.017	0.019	0.022	0.032	0.031	0.031	0.031	0.031	0.031	0.031	0.031	0.031	0.031
P-value	0.097	0.850	0.790	0.813	0.869	0.229	0.748	0.729	0.282	0.171	0.190	0.701	0.864	0.672	0.974
Income	0.024	0.018	0.051	0.024	-0.005	-0.561	-0.286	-0.164	-0.222	-0.346	-0.190	-0.322	-0.078	-0.367	-0.337
Ste	0.099	0.099	0.112	0.112	0.144	0.223	0.175	0.169	0.176	0.200	0.205	0.211	0.218	0.205	0.213
P-value	0.809	0.858	0.648	0.832	0.973	0.012	0.104	0.333	0.207	0.083	0.353	0.127	0.721	0.073	0.113
Same Employer	0.000	0.022	0.063	0.067	0.057	-0.009	0.043	0.025	0.027	-0.000	0.033	0.069	0.057	0.024	0.018
Ste	.	0.016	0.023	0.024	0.027	0.042	0.041	0.040	0.040	0.039	0.038	0.037	0.036	0.035	0.035
P-value	.	0.183	0.007	0.005	0.033	0.827	0.302	0.528	0.496	0.992	0.379	0.061	0.119	0.500	0.597
Cum. Income	0.000	-0.023	-0.133	-0.127	-0.174	-4.026	-4.828	-5.231	-5.726	-6.741	-7.948	-8.716	-9.384	-10.334	-11.530
Ste	.	0.104	0.411	0.719	0.927	1.580	2.183	2.848	3.523	4.254	5.047	5.865	6.695	7.536	8.365
P-value	.	0.821	0.746	0.860	0.851	0.011	0.027	0.066	0.104	0.113	0.115	0.137	0.161	0.170	0.168
FT Employed	-0.018	-0.011	-0.015	-0.014	-0.022	-0.084	-0.015	-0.043	-0.001	-0.007	-0.010	-0.022	-0.006	-0.016	-0.008
Ste	0.011	0.015	0.024	0.024	0.031	0.033	0.033	0.032	0.033	0.033	0.033	0.033	0.033	0.033	0.033
P-value	0.097	0.430	0.520	0.563	0.486	0.010	0.641	0.189	0.969	0.823	0.759	0.496	0.862	0.629	0.803
High PT Employed	0.000	-0.001	-0.015	-0.012	-0.022	0.016	-0.038	-0.043	-0.055	-0.040	-0.038	-0.013	-0.009	-0.034	-0.029
Ste	.	0.007	0.020	0.020	0.026	0.023	0.023	0.024	0.023	0.023	0.024	0.024	0.024	0.023	0.024
P-value	.	0.910	0.436	0.562	0.378	0.485	0.097	0.071	0.018	0.085	0.110	0.589	0.702	0.144	0.217
FT & Child<2	0.000	0.000	0.000	0.000	0.000	-0.084	-0.015	-0.043	0.012	-0.004	0.019	0.010	0.046	0.053	0.016
Ste	.	.	.	.	.	0.033	0.033	0.032	0.023	0.027	0.029	0.029	0.027	0.025	0.023
P-value	.	.	.	.	.	0.010	0.641	0.189	0.600	0.889	0.510	0.739	0.090	0.031	0.476

Notes: Treatment effects identified by a DiD model for all Swiss women (excl. Geneva and unless otherwise noted). Panel A presents estimates for mothers working in large firms, Panel B presents the corresponding estimates for those in small firms. Panel C presents the difference in estimates between Panels A and B. All regressions control for mothers' characteristics such as age at first birth, marital status, cumulative work experience and cumulative income over 6 to 1 years prior to first childbirth. The table shows the effects of the federal mandate on the share of women in employment (*employed*) at various points in time pre- and post-birth. *Income* shows the effects on real earnings from employment of employed women and *Same employer* the effects on the share of employed women working for their pre-birth employer. *Cum. Income* presents the effects on the cumulative total real earnings of all women (including earnings from employment, self-employment, maternity leave benefits, unemployment and other social insurance benefits) since 9 months prior to the first birth. All earnings are adjusted for inflation using the CPI (with base year 2010). Line *FT Employed* presents the effects on the share working full-time (i.e. earning at least 80% compared to 1 year prior to birth), line *High PT Employed* presents the effects on the share working a high part-time (i.e. earning between 50% and 80% compared to 1 year prior to birth). Line *FT & child<2* presents the effects on the share working full-time (i.e. earning at least 80% compared to 1 year prior to birth) whose youngest child is less than 2 years old. Robust standard errors and p-values of individual significance test are reported below each estimate.

Table C.8: Fertility results and heterogeneous effects by child care availability and firm size

	6	12	18	24	30	36	42	48	54	60	72	84	96	108
A.Overall	0.000	-0.000	-0.007	0.019	0.027	0.032	0.033	0.034	0.034	0.032	0.028	0.028	0.026	0.028
Ste	.	0.002	0.007	0.011	0.013	0.014	0.013	0.013	0.013	0.012	0.012	0.012	0.011	0.011
P-value	.	0.995	0.326	0.101	0.045	0.018	0.013	0.008	0.007	0.009	0.019	0.016	0.022	0.014
B. High Ccare	0.000	0.001	-0.003	0.028	0.043	0.045	0.041	0.041	0.041	0.037	0.040	0.038	0.037	0.035
Ste	.	0.002	0.009	0.016	0.019	0.019	0.019	0.019	0.018	0.018	0.017	0.017	0.017	0.016
P-value	.	0.553	0.763	0.082	0.024	0.019	0.032	0.026	0.022	0.035	0.022	0.023	0.027	0.030
C. Low Ccare	0.000	-0.001	-0.010	0.011	0.011	0.020	0.026	0.029	0.027	0.028	0.017	0.019	0.017	0.021
Ste	.	0.002	0.010	0.016	0.019	0.019	0.018	0.018	0.017	0.017	0.016	0.016	0.016	0.016
P-value	.	0.622	0.286	0.514	0.547	0.292	0.157	0.110	0.118	0.101	0.292	0.246	0.286	0.185
D. Diff Ccare	0.000	0.002	0.008	0.017	0.031	0.025	0.015	0.013	0.014	0.009	0.022	0.020	0.020	0.015
Ste	.	0.003	0.013	0.023	0.027	0.027	0.027	0.026	0.025	0.025	0.024	0.023	0.023	0.023
P-value	.	0.444	0.572	0.458	0.239	0.351	0.582	0.624	0.570	0.700	0.350	0.400	0.391	0.515
E. Pre mandate Firm	0.000	0.001	0.000	0.036	0.044	0.056	0.052	0.056	0.053	0.050	0.043	0.043	0.039	0.039
Ste	.	0.002	0.008	0.014	0.017	0.017	0.017	0.016	0.016	0.016	0.015	0.015	0.015	0.014
P-value	.	0.740	0.992	0.012	0.009	0.001	0.002	0.001	0.001	0.001	0.005	0.003	0.008	0.006
F. no pre mandate Firm	0.000	0.004	-0.021	-0.001	0.026	0.008	0.018	0.006	-0.002	-0.004	-0.001	0.005	0.015	0.013
Ste	.	0.004	0.015	0.025	0.027	0.027	0.027	0.026	0.025	0.024	0.023	0.023	0.023	0.022
P-value	.	0.264	0.145	0.970	0.345	0.758	0.489	0.825	0.947	0.862	0.963	0.818	0.516	0.560
G. Diff Firm size	0.000	-0.003	0.021	0.037	0.018	0.047	0.033	0.051	0.054	0.054	0.044	0.038	0.024	0.026
Ste	.	0.004	0.017	0.029	0.032	0.032	0.031	0.030	0.030	0.029	0.028	0.027	0.027	0.026
P-value	.	0.388	0.204	0.195	0.567	0.145	0.289	0.096	0.066	0.061	0.115	0.162	0.368	0.321

Notes: Treatment effects on likelihood of having a second child identified by a DiD model for all Swiss women at various points in time after the first childbirth (excl. Geneva). Panel A presents overall estimates, Panel B and C split the sample by child care availability in mothers' canton of residence at first childbirth and panel D reports the corresponding difference by child care availability. Panels E and F report estimates split by firm size and panel G reports the corresponding difference. Robust standard errors and p-values of individual significance test are reported below each estimate.

Chapter Three

Paid Maternity Leave Mandate, Labor Market Behavior and Child Penalty: The Case of Switzerland

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Abstract

I study the impact of the introduction of Switzerland's first paid maternity leave mandate on women's labor market outcomes in the first four years after having their first child, in order to understand how the mandate affected women's employment and earnings during their transition to parenthood. Using labor force survey data and a difference-in-differences empirical strategy, I examine women's employment rate, the intensity of their labor force participation, and their earnings and wages, as well as a number of other labor market variables. I find that the mandate had an overall negative impact, decreasing employment and earnings, especially among those with an older first child and who likely benefitted from the mandate twice for subsequent children. In addition, I study how the mandate affected gender gaps in employment, earnings and wages. Many recent studies show that these gender gaps emerge or widen after women start their families and then persist. Therefore, understanding how family policies, such as this mandate, affect these gaps is important for policymaking. Using a triple difference model, I find that the mandate increased slightly the gender gap in total employment but decreased substantially the gap in earnings and wages among those with an older child. However, most of my findings are imprecisely estimated.

Keywords: Female labor supply, maternity leave, return-to-work, earnings, gender gaps.
JEL Categories: J13, J22, J78

I. Introduction

The recent policy debate in the US over instituting a federal paid maternity leave mandate, one of the last few countries in the world yet to do so, as well as recent papers examining the persistent gender gaps in employment and earnings, have again raised the importance of family policies. Starting primarily from the 1950s, many advanced economies introduced paid maternity leave mandates and other family policies, such as public childcare provision, when female labor force participation rates started to increase rapidly. However, despite substantial initial improvements, significant gender gaps still remain, have proven to be highly persistent, and are largely attributed to family formation, the so-called *Motherhood or Child Penalty*.

Switzerland was one of the last advanced economies and the last country in Europe to introduce a national paid maternity leave mandate in 2005 (Kalb 2018). The federal mandate provides for 14 weeks of paid maternity leave after the child is born with mandated benefits set at 80 percent of the average previous monthly earnings (subject to a ceiling of 196 CHF per day). Prior to this mandate, women were entitled to a compulsory unpaid maternity leave of eight weeks in duration, with job protection during pregnancy and until 16 weeks after birth, which was maintained in the new mandate. There are no additional provisions for extended parental leave, whether paid or unpaid. This is in sharp contrast to neighboring countries such as Austria, France, Germany and Italy, which like most other European countries, had already mandated paid maternity leave since the 1950s, some for up to or even longer than one year. But the Swiss mandate is similar to Anglo-Saxon countries such as Australia, New Zealand, and the UK, which only introduced such mandates more recently, and whose mandates are also likewise short (Kalb, 2018). However, despite there being few family policies geared towards working mothers, Switzerland has a high maternal labor force participation rate of 77.7 percent, which is one of the highest in Europe (OECD 2019).

Using labor force survey data and a Difference-in-Differences empirical strategy exploiting the quasi-experimental setting created by the mandate, I study the labor market behaviors of new mothers who had their first child in the four-year window around the introduction of the mandate in 2005, and whose first child was aged between zero and four years old at the time that they were observed. I find that, in the short term, among women starting their families and whose first child was aged between zero and two years old, the mandate had an overall small and negative impact. It led to small decreases in total employment, hours worked per week, and high part-time employment. Most importantly, it seemed to have led to increased separation from the pre-birth employer. It had a similar negative impact in the medium term, among those whose first child was aged between three and four years old, though results suggest many continued working at a higher intensity. Since many of these women went on to have a second child, this suggests that the overall impact of the mandate among those who benefitted from the first child onwards was negative. It led to bigger decreases in employment and full-time employment but increases in high part-time employment. These women saw steep decreases in monthly earnings as well as in hourly wages. When examining the impact on gender gaps, by comparing women to similar men who also started their families in this same period, I find small increases in the total employment and full-time employment gaps unsurprisingly. While the earnings and wages gaps increase among those with a young first child, it decreases substantially among those with an older first child. However, almost all of these results are imprecisely estimated, which is likely due to the small sample sizes.

This paper contributes to two strands of literature. First, this paper contributes to a long-standing and large strand of literature that studies the effects of various types of family leave policies on women's labor market outcomes (for a review, see Olivetti and Petrongolo, 2017, and Rossin-Slater, 2017). The majority, however, have focused on the impact of extensions and expansions of pre-existing, usually of long-duration, parental leave policies, rather than the introduction of new mandates providing paid family leave for the first time, since paid maternity leave has been long-standing policy in continental European countries (unlike among Anglo-Saxon countries such as Australia, New Zealand, the UK, and the US). Since it is likely that the effects of paid maternity leave will be highly non-linear, with the largest effects seen upon the introduction of new mandates, and in the first few weeks after birth, and then diminishing substantially and rapidly after, it is important to study how the introduction of new mandates affected women's labor market behaviors.

Waldfogel (1999) and Baum (2003) look at *unpaid* maternity leave mandates in the US. Waldfogel looks that the federal mandate titled the Family and Medical Leave Act (FMLA) that was introduced in 1993, which mandates 12 weeks of unpaid leave and includes job protection. She finds no significant negative effects on women's employment or wages. Baum studies the impact of the introduction of various state mandates in the US as well as the FMLA. From 1972 to 1992, 12 states as well as the District of Columbia introduced mandates for unpaid maternity leave ranging from 6 to 17 weeks with various tenure and work requirements.

He finds that state mandates had small but statistically insignificant effects on employment and wages and argues that the short and unpaid leave had little impact because many employers had already provided paid maternity leave benefits prior to these mandates. In addition, since the FMLA only applies to firms with more than 50 employees, many women were excluded (Waldfogel, 1999). Baker and Milligan (2008) study the impact of the introduction of *unpaid* maternity leave mandates introduced by provinces in Canada starting from the 1960s. By 1980s, most provinces had introduced mandates of between 12-18 weeks in duration with job protection. Income replacement for some women who satisfy the eligibility criteria could come from the Employment Insurance Act, which provides income replacement for various reasons but only included child birth from 1971 onwards. Using labor force survey data to identify women who recently had a child in the months in which they were surveyed, the authors find that the mandates increased employment rates as well as job continuity with the pre-birth employer among women in the months just after they had a child. Women substituted between leaving employment with staying employed but on maternity leave.

In one of the early papers on this topic, Ruhm (1998) looks at the introduction of *paid* maternity leave mandates in nine countries in Europe over the period from 1969 to 1993. He finds that the mandates increased the female employment-to-population ratios by three to four percentage points, which effect was larger when only considering women of childbearing age, and that short leaves did not adversely affect their hourly wages while longer leave entitlements did when compared to men. Rossin-Slater et al. (2013) and Baum and Ruhm (2016) investigate the paid family leave program introduced in California, the first US state to do so in 2004. The program provides six weeks of paid leave to mothers but did not include job protection. Rossin-Slater et al. (2013) find that it increased weekly hours worked by 10-17 percent and also similarly increased wages among women with a young child aged between one and three years old. Baum and Ruhm (2016) find that it increased employment rates, with more mothers returning to work, and especially to the pre-birth employer, and that it also increased work intensity in terms of the number of weeks and hours worked. Byker (2016) looks at both the California and New Jersey mandates. New Jersey also introduced a very similar paid family leave program in 2009 that also provides six weeks of paid leave. She finds that short duration paid family leave increases labor force attachment of women who otherwise would have exited the labor force temporarily in the months around the birth of their children with possible long-term employment benefits for affected women. Broadway et al. (2020) study the introduction of Australia's first mandate in 2011 providing 18 weeks of paid maternity leave. They find that women who benefitted from the mandate returned to work more slowly in the first six months after the birth of their child but then faster after this period and that this mandate increased the overall probability of returning to work.

However, the American states' paid family leave policies differ from the Swiss mandate in terms of the duration of paid leave, the income replacement rate, and the lack of job protection, as well as coverage rates. The states' mandate duration is only for up to six weeks and the income replacement rate is relatively low. In California, it is 55 percent but with a maximum of 1,067 USD per week, which is the state's average weekly wage. In New Jersey, it is 66 percent, but the maximum is 584 USD per week. Moreover, they lack job protection, unless the women also qualify for the FMLA. While the mandate duration for Australia is longer, at 18 weeks, the replacement rate is a flat rate set at the national minimum wage of 656.90 AUD (~480 USD) per week. Given the various requirements related to employment, tenure and firm size, coverage rates are also not very high. Not surprising, these papers find that the effects of the mandates are primarily coming from those earning lower wages. This contrasts with Switzerland's relatively more generous replacement rate of 80 percent, although with a ceiling set at 196 CHF per day. In effect, this ceiling translates to being just below the median gross daily earnings for women in 2005. Therefore, for those earning below the median earnings, the mandate provides an 80 percent replacement of their income, and for those earning above, it provides 3,920 CHF per month. In addition, the Swiss mandate covers all employed women, subject to a relatively low employment requirement and prior contributions to social security.

The second strand of literature looks at the effects that family formation have on women's labor market outcomes. As women transition into motherhood, they experience the so-called *Motherhood Penalty* or *Child Penalty*. Waldfogel (1998), Lundberg and Rose (2000), Blau and Kahn (2000), Anderson, Binder and Krause (2003), Bičáková (2016), and Kleven et al. (2019), among many others, document that women with children have lower employment rates, work fewer hours, earn less and are paid lower wages than men, or women without children. And as Blau and Kahn (2000), Kleven et al (2019) and others have shown, among the youngest cohorts of workers, the gaps between similar men and women in employment rates, hours worked, earnings and wages are almost non-existent and only emerge, dramatically, when women start their families and then persist for the remainder of their work trajectories. For this reason, Olivetti and Petrongolo (2017) argue that family policies could potentially mitigate the Motherhood Penalty and reduce these remaining gender gaps. Therefore, it becomes especially important to examine how the introduction of paid family leave mandates affect women during this transition, when the impact could be expected to be the greatest.

Only two papers have looked at the impact of paid family leave policies on women as they transitioned into motherhood. Kleven et al (2020) look at the effects of the introduction of paid maternity leave in Austria on women's employment and earnings, and then later expansions of parental leave polices on the Child Penalty (which they define as the differences in the labor market trajectories of women relative to men with respect to employment and earnings). Austria introduced a paid maternity leave of 52 weeks in 1961 with income replacement set at the same rate as for unemployment insurance benefits. Using an event study approach, they find that for both employment and earnings, there are actually small negative short- and medium-term effects, which however disappear by the third year after the first child is born. Their overall conclusion is that family policies, including paid maternity leave as well as subsidized public childcare provision, have had no impact on these gender gaps. However, studying the introduction of historical mandates is somewhat problematic since the expansion of family policies paralleled or rather tracked the increase in female labor force participation in the second half of the 19th century, when norms regarding women's roles were also rapidly changing (Goldin, 2006).

Therefore, Switzerland presents a unique opportunity to study how mandating family policies could affect women's labor supply decisions in the short and medium terms just after they become mothers in an advanced economy with already high female and maternal labor force participation rates but large gender gaps and fairly persistent conservation norms regarding working mothers. Girsberger et al (2022) also study the Swiss mandate on women who had their first child just before and after the mandate became effective on 1 July 2005. They find small, positive effects on total employment, full-time employment, and on total and monthly earnings as well as job continuity, which they attribute to the income effect of the mandate. Interestingly, they also find that the mandate increased fertility, especially among those exposed to the reform for their first child and who likely already had access to a similar benefit from their employers. They claim that these firms that saw reduced costs in providing this benefit privately likely invested the cost-savings in other policies that made it easier for mothers to continue working after starting their families. However, because they restrict their sample to women who had a first child three months before and after the mandate's introduction and given the short period from legislation to implementation, women in their sample did not have the opportunity to select into the labor market prior to starting their families nor into motherhood. In my analysis, I allow for the possibility that the mandate could have incentivized some women who might not have been in the labor market or who would have dropped out of the labor market before starting their families to start or continue working, and for the possibility that the mandate could have encouraged some women to start families who otherwise might not have done so at the time.

In the next section, I present the institutional background and the Swiss context in more detail. In Section Three, I describe the data used and in Section Four, outline the empirical strategy and discuss its applicability. Section Five presents and analyses the results. Finally, Section Six concludes.

II. Institutional Background

Women's labor force participation (LFPR) in Switzerland, like in many advanced economies over the past decades, has increased substantially. From 68 percent in 1991, it has increased to almost 80 percent in 2016, while men's LFPR has remained stable at around 90 percent over this same period (OECD 2018). Maternal labor force participation is also high at 73.2 percent in 2015. However, despite these high female and high maternal labor force participation rates, full-time employment among women is still low. After the Netherlands, Swiss women have the highest part-time work rates in the OECD - women's share of all part-time employment is 78 percent and 59.9 percent of women with at least one child work part-time (OECD 2019).

In addition, gender gaps in employment and earnings have proved as persistent as in many other countries. In 2015, the gender gap in total employment among those aged between 15 to 64 years old was 10.9 percent while the gap in full-time employment was 46.3 percent. Meanwhile, the raw gap in the median monthly earnings was 40.3 percent and the raw gap in median hourly wages was 13.8 percent (OFS 2019). When looking only at those who had at least one child below 18 years old, the equivalent gaps in total employment was 20.6 percent and in full-time employment was 68.6 percent (only 21.9 percent of women with a child in the household worked at a full-time rate). And the equivalent raw gaps in median monthly earnings was 54.3 percent and in hourly wages 18.2 percent. Therefore, it is clear that motherhood is driving a large share of these gender gaps, with few family policies at the federal and cantonal levels to affect this situation.

A number of factors could explain this situation. First, widely held, conservative values about women's roles continue to prevail. More than 90 percent surveyed held the view that women should work only part-time or not at all when there is a child under school age in the household and almost 80 percent continue to think that women should work only part-time even when the youngest child starts school according to the International Social Survey Program 2012 (see Appendix Figure B.1). This is a far higher share than in many other European countries, including neighboring ones such as Austria, France and Germany, which have similar cultures. Second, early education is also frequently part-time. Only from 2005, Swiss cantons started introducing mandatory kindergarten for children turning four years old. The time the child spends in kindergarten varies by canton and could be as low as 9 hours per week (Gangl and

Huber, 2022).[1] Primary education starts in the morning between 8:30 to 9:30 am and there is a long lunch break of 1.5 hours at noon, during which children are often expected to return home for meals. They then return to school for additional lessons until 3:30 or 4:30pm. There are no lessons on Wednesday afternoons. Compulsory schooling lasts 11 years starting from kindergarten and homeschooling rates are low. Finally, there are limited childcare places and after-school care facilities, which are also among the costliest in Europe (Ravazzini, 2018). Since 2003, the government increased funding to promote the expansion of childcare and after-school care places. This policy, however, only increased the number of available places and did not reduce their cost. Despite this, there remains a large unmet demand for childcare.

Switzerland was the first country to mandate (unpaid) compulsory leave from work after the birth of a child in 1877 (Kalb, 2018).[2] On 25 November 1945, an article was included in the Swiss constitution charging the Confederation to establish paid maternity leave at the federal level. However, enacting a paid maternity leave mandate proved difficult. In Switzerland, contested new federal legislation need to be passed by a national referendum. Several referenda on paid maternity leave were held between 1945 and 2000, but all of them failed to secure a majority of votes in favor of a mandate. The last unsuccessful referendum on paid maternity leave was held in 1999, with 61.1 percent voting against. A new federal initiative for paid maternity leave was created on 20 June 2001 and received parliamentary approval on 3 October 2003. Due to opposition by one political party, a national referendum was announced in January 2004. The referendum was held on 26 September 2004 and this time, 55.4 percent voted in favor. When the votes were counted, the implementation date of the new mandate was as yet unknown. On 24 November 2004, the Swiss Federal Council announced that the mandate - officially titled in French as *Loi sur les Allocations pour Perte de Gains* (LAPG) - would become effective as of 1 July 2005.

The paid maternity leave provided by the mandate is short – for just 14 weeks (98 days) starting from the birth of the child, and there is no additional parental leave. The mandate

[1] The authors find that employment rates increased moderately among mothers of children exposed to this reform.
[2] Switzerland is known officially as the Swiss Confederation. It is a semi-direct democratic federal republic composed of 26 cantons, which are the member states of the Swiss Confederation. The Swiss government consists of the Federal Council, which holds executive power and consists of nine members elected by the Federal Assembly (also known as the Swiss Parliament). The Federal Assembly consists of two houses, the upper chamber called the Council of States, and the lower chamber called the National Council. The Council of States has 46 representatives (two from each canton and one from each half-canton), while the National Council consists of 200 members who are elected under a system of proportional representation, depending on the population of each canton.

includes job protection during pregnancy and until 16 weeks after the birth of the child. The earnings replacement rate is 80 percent of the average previous earnings and is subject to a current ceiling of 196 CHF per day (which translates roughly to 3,920 CHF per month).[3] Therefore, women earning above 4,900 CHF per month approximately before birth would have their monthly benefits capped at 3,920 CHF. The median gross earnings for women in Switzerland in 2005 was 192.5 CHF per day or 3,850 CHF per month. The mandate is in line with the International Labour Organisation (ILO) Maternity Protection Convention, 2000 (No. 183), which represents the minimum standards of 14 weeks of maternity leave duration and maternity leave cash benefits of not less than two-thirds of the woman's earnings prior to taking leave.[4]

The mandate is "grandfathered", such that women who had a child within the 98 days before the policy came into effect on 1 July 2005 could receive *partial* benefits. That is, they could receive paid leave for the remaining number of days out of the 98 days since the birth of their child that occurred from 1 July 2005 onwards. In addition, women could request for a two-week extension of the leave after the end of the mandated 98 days, which, on account of the post-birth 16-week job protection period, is rarely refused by the employer. However, the employer is not required by the mandate to pay wages for these two extra weeks of leave.

In order to qualify, women should have been employed and contributing to social security for a minimum total of nine months before birth and had to have worked at least five months during the pregnancy as well as be employed at the time of birth. Or, they need to have been on official unemployment leave and receiving unemployment insurance benefits during pregnancy and/or at time of birth for equivalent periods. According to these criteria, more than 80 percent of women who had their first child in 2005 would have qualified for the mandate (Girsberger et al, 2022). The mandate is financed through employee and employer payroll taxes, similar to other existing social insurance schemes, for example, unemployment insurance, disability, etc.

The mandate applies to all 26 cantons. The canton of Geneva implemented its own paid maternity leave mandate with job protection on 1 July 2001. In Geneva, the paid maternity

[3] The cap was increased from 172 CHF to 196 CHF per day in 2009. On 30 June 2005, 1 CHF corresponded to 0.79 USD.

[4] The accompanying ILO Maternity Protection Recommendation, 2000 (No. 191) encourages additional measures of at least 18 weeks of paid leave with cash benefits set at 100 percent of the woman's prior earnings.

leave mandate is for 16 weeks (112 days) after the birth of the child. The mandate cash benefits are also set at 80 per cent of average previous earnings, subject to a minimum of 62 CHF and a maximum of 237 CHF per day in 2005, which was higher than the maximum level of federal benefits at the time (172 CHF). After the adoption of the new mandate, any prior cantonal legislations (as well as prior employer provided private arrangements) had to meet at least the federal standards, but those that were more generous such as that of Geneva remained in force.

The federal mandate is less generous than what public sector employers and many private sector firms had already provided, which was 16 weeks of maternity leave with full pay (Künzi, 2005). Therefore, a majority of working women already enjoyed some form of private paid maternity leave prior to the implementation of the federal mandate on 1 July 2005 (Guillet et al., 2016; Aeppli, 2012). However, about a third of these private schemes offered a maternity leave payment duration of less than 14 weeks, which is the federally mandated duration, and eligibility for many of these employer-provided maternity leave was dependent on tenure with the same employer - in some cases, it could be up to nine years - in order to become eligible for full leave, usually three months of paid maternity leave. Therefore, younger women, those with frequent job changes, and those working in small- and medium-sized firms, that often did not offer paid maternity leave, were the ones more likely to benefit from the federal mandate.

The arguments presented in the Swiss Parliament in favor of the paid maternity leave mandate were to allow the mother time to recover from the demands of pregnancy and childbirth, and to be able to take full care of her child and allow for breastfeeding during the first few months without facing financial pressure to return to work.[5] Therefore, objectives such as enabling women to better reconcile the demands of work and family, reducing the Child (or Motherhood) Penalty, promoting gender equality in the labor market, or closing the gender gaps, were not put forth as the primary arguments in favor of the mandate.

On 1 January 2021, Switzerland implemented a new paid paternity leave mandate. Eligible fathers could take two weeks of paid leave in the first six months after the birth of their child. This mandate is also financed in a similar manner as the paid maternity leave mandate. There are no other forms of parental leaves, paid or unpaid, as is common among neighboring European countries.

[5] https://www.parlament.ch/fr/ratsbetrieb/suche-curia-vista/geschaeft?AffairId=20073156

III. Data

I use data from the Swiss Labor Force Survey (SLFS) with additional data provided by the Swiss Social Protection and Labour Market module (SESAM). The SLFS is an annual survey that studies the employment situation of the permanent resident population aged 15 and older in Switzerland.[6] The SLFS provides information on labor force participation, labor market earnings, working hours, as well as socio-demographic characteristics such as marital status, age, education and nationality. It also includes some information about other household members, such as their age, gender and education, as well as their relationship to the person surveyed so that it is possible to identify spouses or partners and their labor market situation, as well as to identify the number of children and their ages. There is also some limited information on the employer. The SLFS is a rotating panel where about half the respondents (51.1 percent) are surveyed more than once, in contiguous years, and a small percentage up to five times (4 percent). However, in my final samples, I observe each individual only once since I only include the latest observation. The SLFS data covers the years from 1991 to 2018.

The Swiss Social Protection and Labor Market (SESAM) module links the SLFS sample with different social security registers. This combination of data makes it possible to broaden the analyses by taking into account variables such as social security contributions and unemployment benefits received. It is available for the years 1999 to 2017. It also provides more accurate information on total employment earnings and wages. Finally, I use data on inflation rates from 1990 to 2019 provided by the State Secretariat for Economic Affairs (SECO) to deflate the earnings and wages using the base year 2005.

I merge these two datasets to create my samples. In order to study how the mandate affected women's labor market behavior as they transitioned to parenthood, i.e., when they started their families, I construct a sample of women who had a first child born in the years around the introduction of the mandate in 2005, which I term the treatment group. I limit my study to the two years before and after the mandate's introduction. The SLFS does not include the months of birth of children, only their ages from which I construct their respective years of birth. Since the policy became effective from 1 July 2005, I construct my pre-mandate treatment sample of those whose first child was born two years before 2005, which are the years 2003 and 2004.

[6] Since 2010, the SLFS has been conducted on a continuous basis.

Similarly, I construct my post-mandate treatment sample of those whose first child was born after 2005, that is, either in 2006 or 2007. Therefore, I exclude those women whose first child was born in 2005, since I am not able to observe the month of birth and hence, whether they stood to benefit from the mandate or not.

I restrict my samples to women who were aged between 15 to 45 years old at the time their first child was born, following the literature, and whose first child was aged four years old or younger at the time when they were surveyed, since children start attending mandatory kindergarten at that age. By including first-time mothers of young children aged between zero and four years old, I could study both the short- and medium-term effects of the mandate. I then construct my control group, which consists of women with the same characteristics, but with a youngest child aged between five and 17 years old when they were observed in the data in these same years. Since the canton of Geneva already mandated paid maternity leave since 2001, I exclude all women residing in Geneva,[7] in both the treatment and control groups. Finally, in order to study gender gaps, I construct similar samples of men with the same characteristics.

Tables 1 to 7 present the descriptive statistics of the samples based on data from SLFS and SESAM. Table 1 shows the descriptive statistics for the whole sample of men and women in my data. It is fairly evenly composed of men and women (46.7 percent versus 53.3 percent). However, men are more likely to be married (59 percent versus 51 percent), to be born outside Switzerland (68 percent were born in Switzerland as compared to 75 percent for women), to be better educated (23 percent had tertiary education whereas only 15 percent of women did), to be employed at the time of the survey (65 percent unlike 49 percent for women), to be working full-time (88 percent as opposed to 45 percent for women) and to have higher real monthly earnings (8,197 CHF versus 4,598 CHF for women) and higher hourly wages (51 CHF as opposed to 40 CHF for women).

Tables 2 and 3 present the descriptive statistics for two groups of women. The first group, which I term the treatment group, consists of women who had their first child in the two years before (2003-2004) and two years after (2006-2007) the policy reform took place in 2005. This group I further split based on the age of their first child. Table 2 presents the statistics for the

[7]The mandate required all firms located in Geneva to provide paid maternity leave to eligible employees. Therefore, I exclude all women residing in the canton since most of them worked in a firm located in Geneva.

women in the treatment group whose first child was aged between zero and two years old, while Table 3 presents the statistics for those in the treatment group whose first child was aged between three and four years old. The second group, the control group, consists of women who had a youngest child living in the household aged between five and 17 years old in these same years and their descriptive statistics are presented in both tables.

Comparing women in the treatment group whose first child was born either before (pre-mandate) or after the mandate was introduced (post-mandate) and were aged between zero and two years old (Table 2), we can see that they are very similar. They are both on average slightly older than 31 years old, were aged around 30 years old when their first child was born and are equally likely to be married and to have been born in Switzerland. The post-mandate group is more likely to have a lower total number of children (1.3 instead of 1.4) and to have only one child (70 percent as opposed to 60 percent) and to have better educational attainments (fewer have only secondary education and more have tertiary education). In addition, more in the post-mandate group are employed (50 percent instead of 40 percent) and have higher monthly earnings (3,862 CHF versus 3,658 CHF) but lower hourly wages (41.7 CHF versus 43.0 CHF). But among those who are employed, their participation rates are similar.

Table 3 shows the statistics for the sample whose first child was aged between three and four years old. Again, they are very similar with small differences. The pre-mandate group has a slightly higher number of children in total and are less likely to have only one child. They are also less likely to have been born in Switzerland. They are again less likely to be employed (50 percent rather than 60 percent) but have similar earnings and slightly higher wages (41 CHF instead of 40 CHF).

An important point to note is that while the treatment group of women is divided into the pre-mandate and post-mandate samples based on whether they could have benefitted from the mandate for their first child, many of the women in the pre-mandate sample who went on to have a second child would have likely benefitted from the mandate for the second and subsequent children (among the 40 percent of those whose first child was aged between zero and two years old and 70 percent of those whose first child was aged between three and four years old many have a second child born on or after 2005). Therefore, my study only captures the difference in the treatment effect between those who stood to benefit from the mandate from their first child onwards versus those who likely benefitted only for their second and subsequent children.

Comparing the control groups of women before and after the mandate was introduced, they have very similar characteristics. They are on average about 41 years old and were aged around 28 years old when their first child was born. They have about the same total number of children, 1.8 to 1.9 per woman and 40 percent have only one child. Both were equally likely to be married, at 80 percent. While 70 percent of the pre-mandate group were born in Switzerland, it is 60 percent of the post-mandate group. They also have similar educational attainments, with only 10 percent having attained tertiary education. Both groups are equally likely to be employed (70 percent), equally likely to work full-time (30 percent) and equally likely to work at high part-time rates, defined as working at least 20 hours or more per week (40 percent). They also worked about the same number of hours per week, around 24 to 25 hours. The post-mandate group has higher monthly earnings (3,489 CHF versus 3,346 CHF) but about the same hourly wages (37 CHF).

Comparing the treatment and control groups, the primary demographic differences are age, age at birth of first child, the share born in Switzerland, and the share with tertiary education. Naturally, the control group women are older, but were slightly younger when they had their first child. More of them were born in Switzerland (60 to 70 percent) and only 10 percent have tertiary education. They are more likely to be working (70 percent) and work about one more hour per week. They also earn much less (300-400 CHF less) and have lower wages (37 CHF as opposed to 42-43 CHF). Their lower earnings and wages despite working at about the same intensity as the treatment group and being on average 10 years older and hence presumably having more work experience, could be partially explained by their lower human capital, since they have lower educational attainments as can be seen in Tables 2 and 3 (only 10 percent have tertiary education as compared to 20-30 percent of women in the treatment group). In addition, they could also have had reduced time in the labor market, due to interrupted job trajectories around the time they had their children, which however I could not observe.

Tables 4 and 5 present the descriptive statistics for similar samples of men. The pre- and post-mandate groups of men are very similar across most characteristics. Most of them work (90 percent) and most work full-time (around 42 hours per week). The only difference is seen in wages, with the post-mandate sample of men earning higher wages (49 CHF instead of 47.5

CHF for those with a first child aged zero to two years old and 53.8 CHF instead of 51.9 CHF for those with a first child aged three to four years old), which translates into higher earnings as well. The characteristics of the control group of men, before and after the mandate was passed, are almost the same. The key differences between the treatment and control groups are that the control group men are older, on average about 10 years older for those with very young children but were slightly younger when their first child was born, as is the case for the equivalent sample of women. And they have much higher wages (3 to 7 CHF more) and earnings (8,594-8,737 CHF as compared to 7,665-8,019 CHF).

Tables 6 and 7 compare the same men and women in the treatment groups only. Men are, on average, about two years older, and were also two years older when their first child was born. Most other characteristics are similar. While only 20 percent of pre-mandate women had tertiary education, 30 percent of the post-mandate women had tertiary education, the same as for both samples of men. The differences emerge when we look at their labor market behaviors. Men's employment rate is 90 percent, but women's are only between 40 to 50 percent. Most men work full-time (90 percent), about 42 hours per week, while only 20 percent of women work full-time and on average, they work about 24 hours per week. And naturally, this is reflected in their reduced earnings and wages. Women's monthly earnings are only about half that of men, and their hourly wages are lower (42-43 CHF versus 48-49 CHF). Since their socio-economic characteristics are very similar, we can, therefore, safely assume, that parenting responsibilities impede women's full participation in the labor market and this leads to the big gaps we see in the employment rates, hours worked, total earnings and wages.

IV. Empirical Strategy

Following most of the literature that studied the impact of similar family leave mandates (Gruber (1994), Ruhm (1998), Waldfogel (1999), Baum (2003), Baker and Milligan (2008), Rossin-Slater et al (2013), Baum and Ruhm (2016), Byker (2016), and Girsberger et al (2022) among others), the Swiss mandate also presents a quasi-experimental setting and hence, lends itself to being studied using a Difference-in-Differences (DiD) empirical design. However, unlike most of these papers, I do not exploit geographical variation in mandates across the states (or countries), even though the canton of Geneva, which already had a prior mandate, was not affected by the federal mandate. Geneva has economic characteristics and trends that differ from the other cantons and hence, residents in that canton would not serve as a good control group. As mentioned earlier, for this reason, I exclude residents of Geneva in my samples. Rather, following Rossin-Slater et al (2013), I use women who have a youngest child aged between five- and 17-years old living in the household as the control group in my main specification. Most women who choose to have an additional child do so within four years after the last birth. Therefore, the number of women who wait more than five years to have an additional child would likely be very small, and therefore, these women would be unlikely to respond to a pro-natal reform such as the paid maternity leave mandate.

Given the cross-sectional nature of my data, my estimation equation is as follows:

$$y_{ict} = \beta_0 + \beta_1 PostReform_{ict} + \beta_2 Treatment_{ict} + \beta_3 * (PostReform_{ict} * Treatment_{ict}) + \beta_4 X_{ict} + unemp_{ct} + \mu_c + \lambda_t + \epsilon_{ict} \tag{1}$$

The term, y_{ict}, are the different outcome variables related to women's labor supply decisions. The variable, $PostReform_{ict}$, is a binary variable that indicates for the treatment group if the first child was born after the mandate, that is, whether she was born in the years 2006 or 2007, in which case it will be equal to one, or if the first child was born in the years before the mandate was applied, specifically the years 2003 and 2004 and if so, it is equal to zero. For the control group, it indicates when they were observed in the data. They would be in the pre-reform control group if they were observed in the years 2003 or 2004 (the variable takes the value zero) and they would belong to the post-reform control group if they were observed in the years 2006 or 2007, in which case the variable takes the value of one. The variable, $Treatment_{ict}$, is another binary variable that indicates if the mother belongs to the treatment group, i.e., has a first child aged between zero and four years old, in which case it is equal to one, or has a youngest child aged between five and 17 years old, in which case it takes the value of zero. The coefficient of interest is β_3. This captures the effect of the mandate on the women who were exposed to it and likely to have benefitted. Therefore, it captures the Intent-To-Treat (ITT) effect among those potentially eligible for the mandate, since I am not able to identify either those women who were actually eligible for the benefit as I do not have the requisite data on their complete pre-birth employment history, nor those who actually benefitted from the mandate as that was not asked in the surveys. Given also that many women in the sample may already have access to some form of paid maternity leave through their employer, and therefore, could theoretically be assumed to respond much less or not at all to the mandate itself,[8] this estimate could be considered as the lower bound of the ITT effect of the mandate.

Finally, X_{ict}, refers to the vector of individual characteristics. These include age and its quadratic, age at birth of first child, education (three dummy variables to indicate having attained only up to secondary, post-secondary, or tertiary levels of education), a dummy to indicate marital status, and another to indicate whether she was born in Switzerland. For the regressions on earnings and wages, I also include occupation and industry dummies and a firm size variable based on the number of employees. Also included as a control is the annual cantonal unemployment rate, $unemp_{ct}$. Finally, μ_c are canton fixed effects, λ_t are year fixed effects, and ϵ_{ict} is the error term, which is clustered at the canton level. Here, i references the individual, c, the canton of residence, t, the year of birth of first child for the control group, and y, the year of observation for the control group.

The outcome variables of interest, y_{ict}, are employment status (whether employed or not), the number of hours worked in the past week, whether the mother worked full-time (defined as 35 or more hours worked in the past week), or high part-time (defined as at least 20 hours or more worked in the past week), tenure, which is number of years of employment with the current employer, and which also serves as a bounded proxy for job continuity, as well as the log real monthly employment earnings and log real hourly wages. In addition, I also look at whether the mother searched for a new job in the past week, and the size of the firm in terms of the number of employees, which serves as a proxy for change of employer. I estimate linear

[8] Interestingly, Girsberger et al (2022) find that women who worked in firms that likely had already provided some form of paid maternity leave responded the most to the mandate in terms of subsequent fertility. They posit that such firms may have used the cost savings generated by the universal mandate to offer other employee benefits that better allowed women to balance work and family and hence, to continue working after starting their families.

probability models, including for the binary outcome variables, rather than a probit model for ease of interpretation.

When employing a DiD empirical model, the key identification assumption is that women in the treatment group and the control group are subject to the same forces over the timeframe sampled and would respond in the same way to those forces, in the absence of any policy intervention (i.e., the parallel trends assumption). The prevailing and persistent socio-cultural norms and the high maternal labor force participation rates in Switzerland suggest that those women who were just starting their families would be subject to the same economic trends and would make similar labor market decisions, as women who had completed their families, in the absence of the mandate. While there are some demographic differences between women who had completed their families prior to the mandate and those women who started their families around the time the mandate was introduced, essentially cohort differences, there is no reason to think that there would be other unobserved differences such that the labor market would affect these two groups of women differently or that these groups would respond differentially to the same shocks.

To assess whether the identification assumption is potentially satisfied, I compare the parallel trends of the two groups of women before and after the reform. Figure 1 shows the trends in the labor market behavior of women in the treatment and control groups over the years 2000 to 2010, which is five years before and after the reform. Column (I) compares the trends for treatment group women with a first child aged zero to two years old while Column (II) shows the trends for those women with a first child aged three to four years old. The control group remains the same in both panels and consists of women who had a youngest child aged between five- and 17-years old. The values shown are three-point moving averages.

The control group have levels that are higher than the treatment group since these are women who have completed their families and their youngest child is in school. Similar to the information presented in Tables 2 and 3, they have higher employment rates, work at a higher intensity, but have comparable log monthly earnings and wages. There is a small increasing trend in all of these variables over the sample period (2000-2010). As compared to the treatment group, there is less volatility since the sample sizes are larger.

While in general the pre-mandate trends are similar for both the treatment and control groups, there are some differences in trends before the mandate in the weekly hours worked, monthly earnings and wages. For the treatment group with a first child aged between zero and two years old (Column (I)), there is a decrease in the weekly hours worked from almost 24 hours per week in 2000, similar to the control group, to around 22 hours per week from 2001-2003. But then, they catch up to the control group and the trends become similar from 2005 onwards. This is closely reflected in the log monthly earnings, since we again see that the treatment group women with a very young child have almost similar earnings to the control group, which decreases from 2001 and then catches up by 2005 to the control group. With respect to the hourly wages, there is increased volatility pre-mandate, with the treatment group having lower wages, but they become similar around 2003 to 2004. For the treatment group women with a first child aged between three and four years old, they start off with weekly hours worked of 20 hours in 2000, which increases to about 23 hours per week by 2004, and then the trends become similar to the control group. Similarly, they start off with lower monthly earnings and also catch up by 2005. Their hourly wages are higher than the control group’s but have very similar trend from 2001 onwards. Therefore, I confine my study to only the four-year window around the introduction of the mandate, when the trends are more similar.

In addition, I conduct a sensitivity test, where I include a linear time trend interacted with the treatment group in Equation 1, to control for differing group-specific time trends, and assess whether that affect my results. As a further check, I also conduct a placebo test, using the year 2002 as the placebo treatment year, in order to check the robustness of my results.

V. Results and Discussion

A. Women's labor market outcomes

I estimate Equation 1 separately on two different subsamples of women. First, I take a subsample of women whose first child was aged between zero and two years old when they were surveyed. Second, I estimate Equation 1 again using a subsample of women whose first child was aged between three and four years old. Estimating on these two subsamples separately allows me to identify the short-term and medium-term effects of the mandate. The control group in both estimations remain the same – they consist of women whose youngest child was aged between five- and 17-years old when observed.

Tables 8 to 10 present the estimates for different labor market outcomes. The effects are small, and not statistically significant, indicating that the mandate had little effects on both the extensive and intensive margins of labor supply. For those with a young first child aged between zero and two years old, there is a very small decrease in employment of one percentage point, a decrease in hours worked per week of 1.3 hours, a very small increase in full-time employment rate of 0.7 percentage point and a decrease in high part-time work rate of 5 percentage points. For those with an older child aged between three and four years old, there is a bigger decrease in employment of 12 percentage points, but an increase in hours worked of 1.1 hours. There is a decrease of 6 percentage points in full-time work but an increase of 17 percentage points in high part-time work.

Table 9 shows the effects on various other labor market behaviors. For the same sample of women with a very young child aged between zero and two years old, we see a small decrease in tenure, of about four months, which is significant at 10 percent level, suggesting more frequent separation from the pre-birth employer and time taken off work. Those who searched for a new job decreased by 0.5 percentage point and there is a positive change in firm size (proxying for change in employer), but both these estimates are not significant. Among those with an older child, there is again a decrease in tenure, of about one month, a small increase in job search behavior, of 2 percentage points, and a small decrease in firm size. All these estimates are not significant.

Table 10 shows the effects on earnings and wages. Among those with a very young child, there is a small decrease in real monthly earnings of about 9 percent but an increase in real hourly wages of about 4 percent. Among those with an older child, there is also a decrease in monthly earnings of 7 percent, and a decrease in hourly wages of 16 percent. But these are all not statistically significant.

In current policy discussions, family leave policies are promoted as a means to increase mothers' labor market attachment, that is, to encourage women who become parents to continue to participate in the labor market. The paid maternity leave mandate should theoretically provide an incentive for women to become or stay employed before and during their pregnancies (which are required to qualify for the mandated benefits) and to increase their participation rates during their family formation years, since they receive higher benefits by doing so. This should lead to higher employment rates, longer tenures with the pre-birth employers, and greater hours worked among women starting their families, and therefore higher earnings and wages.[9] However, increased leave durations and the income effect provided by the mandate may cause women to delay their return to employment after the paid leave ends. At the same time, if firms find the mandate imposes certain costs (for example, the need to hire replacement workers), then there may be less likely to hire women of child-bearing age, leading to a negative effect on their employment rates, or if wages are fully flexible, then they may pass on these costs through lower wages.[10] In addition, given the strong cultural influences leading to women being the primary childcare providers in Switzerland, as well as the limited and costly childcare available, there are strong barriers that could prevent women from being able to continue working once they become mothers, especially at full-time rates. Nevertheless, for those women who intend to have a second child, we may see them staying employed or re-entering employment after the birth of the first child in order to become eligible for paid maternity leave again as well as increasing the intensity of their labor supply to qualify for higher benefits. Therefore, the immediate post-birth and medium-term effects are ambiguous. There may not be any impact on labor market participation, either at the extensive or at intensive margins, after women start their families in the short- and medium-term periods after the births of their first child.

[9] However, I do not analyze pre-birth labor supply outcomes in this paper since I do not have the necessary data. See Girsberger et al (2022) for results and discussion on the pre-birth (anticipatory) effects of the Swiss mandate.

[10] Oesch, Lipps and McDonald (2017) find evidence from a vignette study conducted in Switzerland of discrimination by employers who assign lower wages to mothers and that this wage penalty is larger for younger mothers.

I find decreases in employment rates, which is greater for those with a first child aged three to four years old. Among those with a very young child aged zero to two years old and who stayed employed, they seem to have decreased work intensity overall. While among those with an older child, they have increased work intensity but at part-time rates. These results seem to suggest that the mandate is not sufficient in incentivizing mothers to transition back to work after having their first child and when the mandated paid leave ends. In fact, the mandate could have provided a small income effect, which could partially explain the decrease in employment when women have their child. The stronger effect we see for those with an older child could be capturing the impact of the mandate for a second child. As shown in Table 3, the treatment group has almost the same number of children as the control group implying most have had a second child by the time their first child is aged three to four years old.

The decrease in tenure length among those with a very young child, of about four months, which is about the usual amount of time that women take off work after birth, seems to suggest that women separated from their pre-birth employers and lends further support to the income effect provided by the mandate. The smaller reduction in tenure among those with an older child indicates that women who planned to have a second child took less time off work, most likely in order to quality for maternity leave benefits again. The expected effect of the mandate on tenure is ambiguous. For those women who were incentivized by the mandate to become or stay employed, we may see longer tenures with the pre-birth employer. Alternatively, women, after becoming mothers, may choose to take more time off from employment than provided by the mandate or their employers, thanks to the income effect, and may also choose to switch employers in order to seek non-wage job amenities such as flexible hours, part-time work, telecommuting, childcare provision, etc., which would allow them to better balance work and family. Finally, since job protection ends 16 weeks after birth (federal mandate), there may be employer discrimination as well, which could then decrease employment rates, and this would then also negatively affect tenures. Therefore, the final effect on tenure is ambiguous. It seems the latter two motivations explain the results that I find. New mothers are quitting their pre-birth employers in order to take more time off work to spend with their newborns and/or or switching employers once they start a family that could enable them to better manage the demands of motherhood and careers.

Since the mandate benefits depend on past earnings, there is an incentive for women to increase labor force participation intensity before having their child. While I am not able to observe pre-birth employment history, the small increase in hours worked per week and the higher part-time rate among those with an older child, who would be most likely planning to have a second child during this period or already had a second child, could be an effect of the mandate. However, for both samples I find reductions in monthly earnings of about 9 percent and 7 percent respectively. For those with a young child, there is a small increase in the hourly wages of about 4 percent, indicating that the reduction in earnings came from the reduction in work hours overall. But for those with an older child, there is a decrease in wages of 16 percent. Among this group of women, the reduction in full-time employment in favor of part-time work, the decrease in tenure as well as wages suggest a change in employment that came with a steep reduction in wages but perhaps offered other non-pecuniary benefits.

In order to assess the robustness of the estimates from the main specification, I conduct a number of checks. First, since the labor market outcomes are observed at different times for the treatment and control groups, I estimate Equation 1 on the same two samples again and include interaction terms between the treatment group indicator and year, which capture differing linear time trends affecting the treatment and control groups. These results are presented in Tables 11 to 13. The estimates are very similar, in the same order of magnitude, with mostly the same signs, and are also imprecisely estimated. The two main differences are that tenure for those with a very young child becomes smaller and is no longer significant while firm size increases and becomes significant at the 10 percent level. This suggests that group time trends do not affect the estimates from my main specification.

Second, I conduct a placebo test. I designate the year 2002 as a "pseudo" treatment year. The treatment group consists of those with a first child aged between zero and four born in the years 2000-2001 (pre-treatment group) or 2003-2004 (post-treatment group) while the control group consists of those with a youngest child aged between five and 17 years old in these same years. I then estimate Equation 1 on these samples. Tables 14 to 16 present these estimates. The estimates from these regressions are very similar, go in the same direction, and are also not statistically significant, except for the estimate related to tenure among those with an older child, which is now positive, and hence go in the opposite direction as the estimate from the main specification. This suggests that the mandate had little impact in altering women's labor market decisions from what they would have made if they had become parents in the absence of the mandate.

In addition, since the control group is composed of women with a youngest child aged between 5- and 17-years old when observed, there could be a selection issue. While most women who choose to have an additional child do so within four years after the last birth, some of these women may have responded to the mandate by having an additional child and therefore, would have self-selected out of the control group. To check this, I look at the total number of children in the household for women whose youngest child was born in 2003 or 2004, and whose previous child was already aged five years old or older, and compare it with that for women whose youngest child was born in 2006 or 2007 and who also had a previous child aged five years old or older. I then test the difference in the total number of children between these two groups of women (Table C.1). I find a very small difference and that this difference is not statistically significant, suggesting that the mandate had little effect in incentivizing women who already had at least one child aged five years old or older to have an additional child.

As a further check, I restrict the control group to women with at least two children, since most of these women would likely have already attained their desired fertility, and so, would have been less likely to respond to the mandate by having an additional child. I then re-run my regressions. The estimates are reported in Tables C.2-4 in Appendix C.

Comparing these results with the main results reported in Tables 8-10, there are mainly small differences in magnitudes and no change in the statistical significance levels. Among those with a young child aged zero to two years old, employment decreases by 13 percentage points rather than 11, hours worked per week decreases by 1.7 instead of 1.3, full-time employment decreases by 0.2 percentage point rather than increasing by 0.8 percentage point, while high part-time employment decreases by the same amount. In terms of job continuity, we see almost exactly the same values. While we see a very small change in monthly earnings but a bigger increase in wages, 9 percent instead of 3 percent.

Comparing those with an older child aged between two to four years old, we see little changes in total employment, full-time and part-time employment, but a small decrease in weekly hours worked, from 1.1 to 0.6. While there are some small changes in employment attachment. Tenure decreases further to 0.3 instead of by 0.1, job search decreases to one percentage point instead of by two, and there is a bigger decrease in firm size, by 0.3 instead of 0.06. Finally, we see a slighter bigger decrease in monthly earnings, of 9 percent instead of 7 percent, while wages decrease by about the same amount. None of these estimates are statistically significant.

Moreover, the estimates on all variables other than employment, such as hours worked, full-time rate, high part-time rate, earnings, etc., are made on the sample of employed women only, since I find negligible effects on employment. To check whether this affects my results, I estimate again for various labor market outcomes on the extensive and intensive margins using all women in my sample by including unemployed women and using the level rather than the log of monthly earnings and wages. Also, for the estimates on earnings and wages, I omit the industry and occupational controls in order to maintain the same sample size. These results are presented in Tables C.5-6 in Appendix C.

Again, comparing with the main results in Tables 8 and 10, we see little changes among those with a very young child aged zero to two years old. The hours worked remains almost the same, while full-time employment decreases by one percentage point rather than increasing by one percentage point, and part-time employment decreases to four percentage points instead of five. When comparing earnings and wages (Table C.6), monthly earnings decrease by 222 CHF instead of 181 CHF while wages decrease to 5 CHF rather than 7 CHF.

Among those with an older child aged three to four years old, hours worked per week decreases by two hours instead of increasing by one hour, but full-time employment decreases by the same amount. Part-time employment increases only marginally, by four percentage points instead of 17. In terms of revenue, monthly earnings decrease more, by 587 CHF instead of 493 CHF, but wages decrease by about the same amount. None of these estimates are statistically significant.

Finally, I assess whether the mandate created selection effects into the labor market or into parenthood. I estimate Equation 1 on various socio-economic characteristic (without any controls). Table 26 present these estimates. I find that those who had a young first child post-mandate were more likely to have higher levels of education. This suggests that women who were better educated respond more to the mandate. And among those with an older first child, they were less likely to be married, more likely to have been born in Switzerland, and less likely to have fewer children.

The small effect sizes that I find among those with a young first child, and the lack of statistical significance of these estimates, seem to suggest that the mandate had very little, if any, effects on women's labor market behavior, both on the extensive and intensive margins. This could be because many firms had already provided some form of paid maternity leave of similar duration prior to the mandate. In addition, the short period of mandated maternity leave, at less than four months, could also explain the lack of impact. While the bigger effects for those with an older first child seems to capture the cumulative effects of the mandate among those who likely benefitted from the mandate again for their second child. These effects show that the mandate had a negative impact on total and full-time employment and also on monthly earnings and wages but did raise high part-time employment. Finally, the small sample sizes of the treatment groups pose some difficulty in identifying these effects due to the mandate with higher precision.

B. Gender Gaps

In order to examine how family policies such as the paid maternity leave mandate could affect gender gaps in employment and earnings, I conduct the same analysis as in the previous section using men with similar characteristics. First, I assess whether men were affected by the mandate indirectly through adjustments in labor supply within the household. Therefore, I estimate Equation 1 using samples constructed of men, similar to how I constructed the samples of women used in the main specification, that is, I identify men with a first child aged between zero and four years old who were born in the years 2003-2004 (pre-mandate) or 2006-2007 (post-mandate). Figure 2 shows the trends in these treatment groups as compared to a similar control group of men, who have a youngest child aged between five and 17 years old when they were observed. While the trends in the employment rates, weekly hours worked, full-time employment rate and part-time employment rate do not differ too greatly, the trends in the monthly earnings and wages are more divergent

The results from estimating Equation 1 on these samples of men are presented in Tables 17 to 19. I find that the estimates are mostly very small and not statistically significant, except in two instances. Among those with a younger child, there is an increase in employment of 0.5 percentage point, a small increase in hours worked of 0.6 hour, a very small decrease in full-time employment of 0.9 percentage point, and a similar decrease in high part-time work (Table 17). There is a very small decrease in tenure, a small increase in job search, and a small increase in firm size. All of these estimates are not statistically significant. However, there is a decrease in monthly earnings of 15 percent that is statistically significant at the 10 percent level, and a corresponding decrease in hourly wages of 15 percent, although this is not statistically significant. Therefore, it seems like the decrease in earnings for this group comes about through the decrease in wages among those who are employed. However, we do not see the same results in earnings and wages for those with an older child.

Among those with an older child aged between three and four years old, there is a decrease in employment of about six percentage points, which is significant at the 10 percent level. There is also a decrease in hours worked of 1.9 hour, and in full-time employment of two percentage points, but a small increase in high part-time employment of one percentage point. This group also has an increase in tenure, a small decrease in job search and an increase in firm size. In terms of income, there is a small decrease of 0.7 percent in monthly earnings but an increase in wages of 4 percent. All the later estimates are not statistically significant.

I then use this sample of men as the control group in a difference-in-differences-in-differences or triple difference model (DDD) as follows:

$$\begin{aligned} y_{ict} = \beta_0 + {} & \beta_1 PostReform_{ict} + \beta_2 Treatment_{ict} + \beta_3 Sex_{ict} + \beta_4 \\ & * (PostReform_{ict} * Treatment_{ict}) + \beta_5 * (PostReform_{ict} * Sex_{ict}) + \beta_6 \\ & * (Treatment_{ict} * Sex_{ict}) + \beta_7 \\ & * (PostReform_{ict} * Treatment_{ict} * Sex_{ict}) + \beta_8 X_{ict} + unemp_{ct} + \mu_c + \lambda_t \\ & + \epsilon_{ict} \end{aligned} \tag{2}$$

The terms remain defined exactly the same as for Equation 1, with the addition of the term Sex_{ict}, which is a binary variable that equals zero if the individual is male and one if female. The error term here, ϵ_{ict}, is clustered at the sex and canton level. The results from estimating Equation 2 are shown in Tables 20 to 22 and are summarized together with the earlier estimates from the DiD models in Tables 20B to 22B.

Most of the results from estimating Equation 2 are small and not statistically significant. Among those with a young first child, we see a small increase in the employment gap of 3 percentage points, a decrease in the gap in hours worked of 1.4 hours, and a very small increase in the full-time employment gap of 0.3 percentage point (Table 20B). In addition, there is an increase in the earnings gap of 0.2 percent and an increase in the wage gap of 10 percent (Table 22B). Among those with an older child, we see also an increase in the employment gap of 5 percentage point, a decrease in the hours worked gap of 1.3 hours, and a very small increase in the full-time employment gap of 0.06 percentage point. Finally, there is a decrease in the monthly earnings gap of 12 percent, which is statistically significant at the five percent level, and a decrease in the wage gap of 2 percent. All of these estimates, unless otherwise noted, are not statistically significant.

To check whether trends affecting the control groups are driving these results, I estimate again Equation 1 but this time, I only use men in the treatment group. That is, women with a first child aged between zero and four years old are compared to men with a similar aged first child. I compare the trends in these groups, which are shown in Figure 3.

The results are presented in Tables 23 to 25. We see again similar increases in the gender employment gaps of 3 and 4 percentage points for those with a very young and older first child respectively. The estimates on the gap in the hours worked are much smaller – the gap decreases for those with a young child but increases for those with an older child. There are again increases in the full-time employment gap of 1 percentage point for both samples. We see increases in the monthly earnings gap and the wage gap among those with a young first child, of 7 and 5 percent respectively, but decreases of about 6 percent among those with an older child, with the decrease in the wage gap being statistically significant at the 10 percent level. All the other estimates are not statistically significant. Therefore, this suggests that the mandate led to small increases in the employment and full-time employment gaps among first-time parents. And while the monthly earnings and wage gaps also increase among those with a young first child, there were substantial decreases in these gaps among those with an older child. My approach here is similar to Angelov, Johansson and Lindahl (2016), who study the variation over time in the within couple earnings and wage gaps after the birth of their first child. They find that after 15 years these gaps have increased to 32 and 10 percentage points respectively. In an earlier paper, Lundberg and Rose (2000) find that in households in which mothers experienced interruptions in employment after birth of their first child, their wages and hours worked saw large decreases while fathers' hours and wages increase, but in households in which the mother remains continuously attached to the labor force, there is no evidence of a wage decline for mothers, and the hours worked by fathers decrease substantially. While the limitations of my data do not allow me to study gender gaps over a similar period, the findings do suggest that family policies could play an important role in reducing these gaps, at least in the critical period leading up to and right after family formation.

VI. Conclusion

Switzerland was the last European country and one of the last advanced economies to implement a federal paid maternity leave mandate in 2005. The mandate provides for 14 weeks of paid leave at 80 percent of the average previous earnings, in line with the minimum standards set forth in the International Labour Organisation (ILO) Maternity Protection Convention, 2000 (No. 183), and includes job protection during pregnancy and for a 16-week period after the birth of the child. The mandate is universal and applies to all women, subject to certain prior employment criteria.

Using primarily the Swiss Labor Force Survey, as well as other data, and a Difference-in-Differences empirical strategy appropriate to the policy context, I find that the mandate had small effects overall on the labor market behavior of new mothers who had their first child in the four-year window around the introduction of the mandate in 2005 and whose first child was aged between zero and four years old at the time that they were observed. In the short term, when the first child was aged between zero and two years old, the mandate had a very small, mostly negative, impact on the labor market outcomes of new mothers. Most importantly, it seemed to have led to increased separation from the pre-birth employer. Similarly, it had a mostly negative impact in the medium term, among those whose first child was aged between three and four years old. Since many of these women now have a second child, this suggests that the overall impact of the mandate among those who benefitted from the first child onwards was negative. It led to bigger decreases in employment and full-time employment rates after women started their families, but it seemed to have encouraged more to work at high part-time rates. These women also saw decreases in monthly earnings as well as in hourly wages. However, almost all of these results are imprecisely estimated.

Among those with a first child aged between zero and two years old, I find a very small decrease in employment of one percentage point, a decrease in hours worked per week of 1.3 hours, a very small increase in full-time employment rate of 0.7 percentage point and a decrease in high part-time work rate of 5 percentage points. I also find a small decrease in tenure, of about four months, which is significant at the 10 percent level, and a decrease in job search of 0.5 percentage point, as well as a positive change in firm size. Finally, there is a decrease in real monthly earnings of about 9 percent but an increase in real hourly wages of about 4 percent. All these estimates, except that for tenure, are not statistically significant. This seems to indicate that the mandate led to a small decrease in employment among women who had just started their families and that they were less likely to return to their pre-birth employers. This is contradictory to the findings in papers looking at the recent introduction of paid maternity leave mandates in two states in the US, which found increased leave-taking, but higher employment, longer tenures with the pre-birth employer, increased work hours and higher wages in the few months after the birth of a child. However, there is a key difference between the American and Swiss contexts. In Switzerland, health insurance is universally mandated, and health insurance premiums are paid directly by residents (who also select their own insurers), and not by the employer as in the US. Therefore, the financial incentive in staying employed, beyond earnings and wages, especially for those with children, is much greater in the US than it is in Switzerland. In addition, Switzerland has one of the most expensive public child care and demand far exceeds supply. These findings are, however, consistent with those made by Kleven et al (2020) who find that the introduction of Austria's first maternity leave mandate in 1961 led to decreases in employment and earnings in the first year after the first child was born.

Among those with an older child aged between three and four years old, which the literature has largely not studied, I find a bigger decrease in employment of 12 percentage points, an increase in hours worked of 1.1 hours, a decrease of 6 percentage points in full-time work, but an increase of 17 percentage points in high part-time work. There is also a decrease in tenure, of about one month, a small increase in job search behavior of 2 percentage points, and a small decrease in firm size. Finally, there is a decrease in monthly earnings of 7 percent, and a decrease in hourly wages of 16 percent. But these are all again not statistically significant. Since many of these women have had a second child, this suggests that the total effect of the mandate among those who benefitted from the first child onwards is negative with respect to their attachment to the labor market. More women left employment and those who stay employed experience steep reductions in their earnings and wages. In this case, my findings are contradictory to those of Kleven et al (2020) since they do not find any medium to long term impact of Austria's first paid maternity leave mandate.

When I compare new mothers with new fathers using both a DiD and a DDD model, I find small increases in the total employment and full-time employment gaps. However, while the earnings and wages gaps increase among those with a young first child, it decreases substantially among those with an older first child. These findings suggest that family policies could play an important role in reducing gender gaps, at least in the critical period when women start their families and when research shows the gender gaps emerge and widen significantly.

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Appendix A. Tables and Figures

TABLE 1 —DESCRIPTIVE STATISTICS (FULL SAMPLE)

VARIABLES	**MALE**	**FEMALE**	**TOTAL**
PANEL A			
AGE	48.07	50.05	49.13
MARRIED	59%	51%	55%
SWISS-BORN	68%	75%	72%
SECONDARY EDUCATION ONLY	18%	27%	23%
POST-SECONDARY OR SOME TERTIARY EDUCATION	59%	58%	58%
TERTIARY EDUCATION	23%	15%	18%
HAS AT LEAST ONE CHILD BELOW 18 YEARS OLD	28%	28%	28%
PANEL B			
CURRENTLY EMPLOYED	65%	49%	57%
HOURS/WEEK	40.88	29.64	35.64
FULL-TIME	0.88	0.45	0.68
HIGH-PART TIME	0.08	0.31	0.19
TENURE	3.19	3.02	3.11
FIRM SIZE	0.80	0.69	0.75
REAL MONTHLY EARNINGS (CHF)	8,197	4,598	6,519
REAL HOURLY WAGE (CHF)	51	40	46
NUMBER OF UNIQUE OBSERVATIONS	**273,429**	**311,671**	**584,992**

Notes: Sample constructed from the Swiss Labor Force Survey and SESAM datasets ().

Sources: FOS, own calculations.

TABLE 2 —DESCRIPTIVE STATISTICS (WOMEN ONLY)

VARIABLES	TREATMENT GROUP		CONTROL GROUP	
	BEFORE	AFTER	BEFORE	AFTER
PANEL A				
AGE	31.4	31.7	41.2	41.6
AGE AT BIRTH OF FIRST CHILD	29.9	30.1	28.4	28.7
TOTAL NUMBER OF CHILDREN	1.4	1.3	1.8	1.9
HAS ONLY ONE CHILD	0.6	0.7	0.4	0.4
MARRIED	0.9	0.9	0.8	0.8
SWISS-BORN	0.4	0.4	0.7	0.6
SECONDARY EDUCATION ONLY	0.2	0.1	0.2	0.3
POST-SECONDARY OR SOME TERTIARY EDUCATION	0.6	0.5	0.7	0.6
TERTIARY EDUCATION	0.2	0.3	0.1	0.1
PANEL B				
CURRENTLY EMPLOYED	0.4	0.5	0.7	0.7
HOURS/WEEK	23.6	23.8	24.4	25.3
FULL-TIME	0.2	0.2	0.3	0.3
HIGH-PART TIME	0.4	0.4	0.4	0.4
TENURE	2.8	2.8	3.0	3.1
EMPLOYED AT PRE-BIRTH EMPLOYER	0.6	0.7	-	-
FIRM SIZE	0.7	0.8	0.6	0.6
REAL MONTHLY EARNINGS (CHF)	3,658.1	3,862.5	3,345.5	3,489.0
REAL HOURLY WAGE (CHF)	43.0	41.7	37.4	37.0
NUMBER OF UNIQUE OBSERVATIONS	**913**	**835**	**3,556**	**2,806**

Notes: Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treated group had their first child in 2003-2004 (before) or 2006-2007 (after) and whose first child was aged between zero and two years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed in 2003-2004 (before) or 2006-2007 (after). Women residing in the canton of Geneva are excluded. Total sample size is 8,110.

Sources: FOS, own calculations.

TABLE 3 —DESCRIPTIVE STATISTICS (WOMEN ONLY)

VARIABLES	TREATMENT GROUP		CONTROL GROUP	
	BEFORE	AFTER	BEFORE	AFTER
PANEL A				
AGE	33.7	33.9	41.2	41.6
AGE AT BIRTH OF FIRST CHILD	29.9	30.3	28.4	28.7
TOTAL NUMBER OF CHILDREN	1.8	1.7	1.8	1.9
HAS ONLY ONE CHILD	0.3	0.4	0.4	0.4
MARRIED	0.9	0.9	0.8	0.8
SWISS-BORN	0.4	0.5	0.7	0.6
SECONDARY EDUCATION ONLY	0.2	0.1	0.2	0.3
POST-SECONDARY OR SOME TERTIARY EDUCATION	0.6	0.6	0.7	0.6
TERTIARY EDUCATION	0.2	0.3	0.1	0.1
PANEL B				
CURRENTLY EMPLOYED	0.5	0.6	0.7	0.7
HOURS/WEEK	22.3	22.7	24.4	25.3
FULL-TIME	0.2	0.2	0.3	0.3
HIGH-PART TIME	0.4	0.4	0.4	0.4
TENURE	2.8	2.9	3.0	3.1
EMPLOYED AT PRE-BIRTH EMPLOYER	0.5	0.5	-	-
FIRM SIZE	0.6	0.7	0.6	0.6
REAL MONTHLY EARNINGS (CHF)	3,570.5	3,586.0	3,345.5	3,489.0
REAL HOURLY WAGE (CHF)	41.9	40.3	37.4	37.0
NUMBER OF UNIQUE OBSERVATIONS	**815**	**1,350**	**3,556**	**2,806**

Notes: Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treated group had their first child in 2003-2004 (before) or 2006-2007 (after) and whose first child was aged between three and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed in 2003-2004 (before) or 2006-2007 (after). Women residing in the canton of Geneva are excluded. Total sample size is 8,527.

Sources: FOS, own calculations.

TABLE 4 —DESCRIPTIVE STATISTICS (MEN ONLY)

VARIABLES	TREATMENT GROUP		CONTROL GROUP	
	BEFORE	AFTER	BEFORE	AFTER
PANEL A				
AGE	33.8	34.1	43.9	44.3
AGE AT BIRTH OF FIRST CHILD	32.4	32.6	30.9	31.2
TOTAL NUMBER OF CHILDREN	1.4	1.3	1.9	1.9
HAS ONLY ONE CHILD	0.7	0.7	0.4	0.3
MARRIED	0.9	0.9	0.9	0.9
SWISS-BORN	0.4	0.4	0.5	0.5
SECONDARY EDUCATION ONLY	0.1	0.1	0.2	0.2
POST-SECONDARY OR SOME TERTIARY EDUCATION	0.6	0.6	0.6	0.6
TERTIARY EDUCATION	0.3	0.3	0.2	0.2
PANEL B				
CURRENTLY EMPLOYED	0.9	0.9	0.9	0.9
HOURS/WEEK	41.8	42.1	43.4	42.7
FULL-TIME	0.9	0.9	1.0	1.0
HIGH-PART TIME	0.1	0.1	0.0	0.0
TENURE	2.9	2.8	3.4	3.4
FIRM SIZE	0.8	0.9	0.8	0.8
REAL MONTHLY EARNINGS (CHF)	7665.2	8019.2	8593.5	8737.2
REAL HOURLY WAGE (CHF)	47.5	49.0	50.5	56.7
NUMBER OF UNIQUE OBSERVATIONS	**871**	**776**	**3163**	**2345**

Notes: Sample restricted to only those men who were aged between 15 to 45 years old at the birth of their first child. Those in the treated group had their first child in 2003-2004 (before) or 2006-2007 (after) and whose first child was aged between zero and two years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed in 2003-2004 (before) or 2006-2007 (after). Men residing in the canton of Geneva are excluded. Total sample size is 7,155.

Sources: FOS, own calculations.

TABLE 5 —DESCRIPTIVE STATISTICS (MEN ONLY)

VARIABLES	TREATMENT GROUP		CONTROL GROUP	
	BEFORE	AFTER	BEFORE	AFTER
PANEL A				
AGE	36.0	36.3	43.9	44.3
AGE AT BIRTH OF FIRST CHILD	32.3	32.7	30.9	31.2
TOTAL NUMBER OF CHILDREN	1.8	1.8	1.9	1.9
HAS ONLY ONE CHILD	0.3	0.3	0.4	0.3
MARRIED	0.9	0.9	0.9	0.9
SWISS-BORN	0.4	0.5	0.5	0.5
SECONDARY EDUCATION ONLY	0.1	0.1	0.2	0.2
POST-SECONDARY OR SOME TERTIARY EDUCATION	0.5	0.6	0.6	0.6
TERTIARY EDUCATION	0.3	0.3	0.2	0.2
PANEL B				
CURRENTLY EMPLOYED	0.9	0.9	0.9	0.9
HOURS/WEEK	42.3	42.1	43.4	42.7
FULL-TIME	0.9	0.9	1.0	1.0
HIGH-PART TIME	0.0	0.1	0.0	0.0
TENURE	3.1	3.1	3.4	3.4
FIRM SIZE	0.9	0.8	0.8	0.8
REAL MONTHLY EARNINGS (CHF)	8318.7	8823.7	8593.5	8737.2
REAL HOURLY WAGE (CHF)	51.9	53.8	50.5	56.7
NUMBER OF UNIQUE OBSERVATIONS	**629**	**1215**	**3163**	**2345**

Notes: Sample restricted to only those men who were aged between 15 to 45 years old at the birth of their first child. Those in the treated group had their first child in 2003-2004 (before) or 2006-2007 (after) and whose first child was aged between three and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed in 2003-2004 (before) or 2006-2007 (after). Men residing in the canton of Geneva are excluded. Total sample size is 7,352.

Sources: FOS, own calculations.

TABLE 6 —DESCRIPTIVE STATISTICS (MEN AND WOMEN)

VARIABLES	WOMEN		MEN	
	BEFORE	AFTER	BEFORE	AFTER
PANEL A				
AGE	31.4	31.7	33.8	34.1
AGE AT BIRTH OF FIRST CHILD	29.9	30.1	32.4	32.6
TOTAL NUMBER OF CHILDREN	1.4	1.3	1.4	1.3
HAS ONLY ONE CHILD	0.6	0.7	0.7	0.7
MARRIED	0.9	0.9	0.9	0.9
SWISS-BORN	0.4	0.4	0.4	0.4
SECONDARY EDUCATION ONLY	0.2	0.1	0.1	0.1
POST-SECONDARY OR SOME TERTIARY EDUCATION	0.6	0.5	0.6	0.6
TERTIARY EDUCATION	0.2	0.3	0.3	0.3
PANEL B				
CURRENTLY EMPLOYED	0.4	0.5	0.9	0.9
HOURS/WEEK	23.6	23.8	41.8	42.1
FULL-TIME	0.2	0.2	0.9	0.9
HIGH-PART TIME	0.4	0.4	0.1	0.1
TENURE	2.8	2.8	2.9	2.8
FIRM SIZE	0.7	0.8	0.8	0.9
REAL MONTHLY EARNINGS (CHF)	3658.1	3862.5	7665.2	8019.2
REAL HOURLY WAGE (CHF)	43.0	41.7	47.5	49.0
NUMBER OF UNIQUE OBSERVATIONS	**913**	**835**	**871**	**776**

Notes: Sample restricted to only those men and women who were aged between 15 to 45 years old at the birth of their first child. These men and women had their first child in 2003-2004 (before) or 2006-2007 (after) and their first child was aged between zero and two years old at the time that they were surveyed. Those residing in the canton of Geneva are excluded. Total sample size is 3,395.

Sources: FOS, own calculations.

TABLE 7 —DESCRIPTIVE STATISTICS (MEN AND WOMEN)

VARIABLES	WOMEN		MEN	
	BEFORE	AFTER	BEFORE	AFTER
PANEL A				
AGE	33.7	33.9	36.0	36.3
AGE AT BIRTH OF FIRST CHILD	29.9	30.3	32.3	32.7
TOTAL NUMBER OF CHILDREN	1.8	1.7	1.8	1.8
HAS ONLY ONE CHILD	0.3	0.4	0.3	0.3
MARRIED	0.9	0.9	0.9	0.9
SWISS-BORN	0.4	0.5	0.4	0.5
SECONDARY EDUCATION ONLY	0.2	0.1	0.1	0.1
POST-SECONDARY OR SOME TERTIARY EDUCATION	0.6	0.6	0.5	0.6
TERTIARY EDUCATION	0.2	0.3	0.3	0.3
PANEL B				
CURRENTLY EMPLOYED	0.5	0.6	0.9	0.9
HOURS/WEEK	22.3	22.7	42.3	42.1
FULL-TIME	0.2	0.2	0.9	0.9
HIGH-PART TIME	0.4	0.4	0.0	0.1
TENURE	2.8	2.9	3.1	3.1
FIRM SIZE	0.6	0.7	0.9	0.8
REAL MONTHLY EARNINGS (CHF)	3570.5	3586.0	8318.7	8823.7
REAL HOURLY WAGE (CHF)	41.9	40.3	51.9	53.8
NUMBER OF UNIQUE OBSERVATIONS	**815**	**1350**	**629**	**1215**

Notes: Sample restricted to only those men and women who were aged between 15 to 45 years old at the birth of their first child. These men and women had their first child in 2003-2004 (before) or 2006-2007 (after) and their first child was aged between three and four years old at the time that they were surveyed. Those residing in the canton of Geneva are excluded. Total sample size is 4,009.

Sources: FOS, own calculations.

TABLE 8 — EMPLOYMENT (WOMEN ONLY)

SAMPLES	(1) EMPLOYED	(2) HOURS/WEEK	(3) FULL-TIME	(4) HIGH PART-TIME
CHILD AGED ZERO TO TWO YEARS OLD	-0.0105	-1.328	0.00761	-0.0548
	(0.0378)	(2.341)	(0.0642)	(0.0506)
NO OF OBSERVATIONS	8,110	5,120	5,120	5,120
CHILD AGED THREE TO FOUR YEARS OLD	-0.118	1.053	-0.0631	0.171
	(0.0724)	(1.944)	(0.0494)	(0.107)
NO OF OBSERVATIONS	8,527	5,492	5,492	5,492
DEMOGRAPHIC CONTROLS	X	X	X	X
CANTON FE	X	X	X	X
YEAR FE	X	X	X	X

Notes: Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Women residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, dummies for education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

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

TABLE 9 — JOB CONTINUITY (WOMEN ONLY)

SAMPLES	(1) TENURE	(2) JOB SEARCH	(3) FIRM SIZE
CHILD AGED ZERO TO TWO YEARS OLD	-0.321*	-0.00543	0.194
	(0.164)	(0.0454)	(0.160)
NO OF OBSERVATIONS	5,117	5,120	4,738
CHILD AGED THREE TO FOUR YEARS OLD	-0.104	0.0205	-0.0652
	(0.209)	(0.0466)	(0.186)
NO OF OBSERVATIONS	5,489	5,492	5,081
DEMOGRAPHIC CONTROLS	X	X	X
CANTON FE	X	X	X
YEAR FE	X	X	X

Notes: Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Women residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

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

TABLE 10 — EARNINGS AND WAGES (WOMEN ONLY)

SAMPLES	(1) LOG MONTHLY EARNINGS	(2) LOG HOURLY WAGES
CHILD AGED ZERO TO TWO YEARS OLD	-0.0896	0.0385
	(0.173)	(0.112)
NO OF OBSERVATIONS	4,269	4,269
CHILD AGED THREE TO FOUR YEARS OLD	-0.0694	-0.155
	(0.118)	(0.103)
NO OF OBSERVATIONS	4,567	4,567
DEMOGRAPHIC CONTROLS	X	X
OCCUPATION DUMMIES	X	X
INDUSTRY DUMMIES	X	X
FIRM SIZE	X	X
CANTON FE	X	X
YEAR FE	X	X

Notes: Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Women residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Earnings and wages are reported in Swiss Franc and have been adjusted for inflation using the base year 2005. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

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

TABLE 11 — EMPLOYMENT (WOMEN ONLY AND WITHOUT GROUP TIME TREND)

SAMPLES	(1) EMPLOYED	(2) HOURS/WEEK	(3) FULL-TIME	(4) HIGH PART-TIME
CHILD AGED ZERO TO TWO YEARS OLD	0.00832	-0.995	0.0374	-0.0441
	(0.0589)	(3.540)	(0.102)	(0.0672)
NO OF OBSERVATIONS	8,110	5,120	5,120	5,120
CHILD AGED THREE TO FOUR YEARS OLD	-0.0898	0.652	-0.0676	0.151
	(0.0720)	(1.956)	(0.0492)	(0.107)
NO OF OBSERVATIONS	8,527	5,492	5,492	5,492
DEMOGRAPHIC CONTROLS	X	X	X	X
CANTON FE	X	X	X	X
YEAR FE	X	X	X	X

Notes: Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Women residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, dummies for education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

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

TABLE 12 — JOB CONTINUITY (WOMEN ONLY AND WITHOUT GROUP TIME TREND)

SAMPLES	(1) TENURE	(2) JOB SEARCH	(3) FIRM SIZE
CHILD AGED ZERO TO TWO YEARS OLD	-0.123	0.0328	0.428**
	(0.164)	(0.0357)	(0.161)
NO OF OBSERVATIONS	5,117	5,120	4,738
CHILD AGED THREE TO FOUR YEARS OLD	-0.169	0.0182	-0.136
	(0.210)	(0.0481)	(0.195)
NO OF OBSERVATIONS	5,489	5,492	5,081
DEMOGRAPHIC CONTROLS	X	X	X
CANTON FE	X	X	X
YEAR FE	X	X	X

Notes: Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Women residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

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

TABLE 13 — EARNINGS AND WAGES (WOMEN ONLY AND WITHOUT GROUP TIME TREND)

SAMPLES	(1) LOG MONTHLY EARNINGS	(2) LOG HOURLY WAGES
CHILD AGED ZERO TO TWO YEARS OLD	-0.136	0.0368
	(0.213)	(0.123)
NO OF OBSERVATIONS	4,269	4,269
CHILD AGED THREE TO FOUR YEARS OLD	-0.0762	-0.141
	(0.119)	(0.107)
NO OF OBSERVATIONS	4,567	4,567
DEMOGRAPHIC CONTROLS	X	X
OCCUPATION DUMMIES	X	X
INDUSTRY DUMMIES	X	X
FIRM SIZE	X	X
CANTON FE	X	X
YEAR FE	X	X

Notes: Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Women residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Earnings and wages are reported in Swiss Franc and have been adjusted for inflation using the base year 2005. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

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

TABLE 14 — PLACEBO TEST: EMPLOYMENT (WOMEN ONLY)

SAMPLES	(1) EMPLOYED	(2) HOURS/WEEK	(3) FULL-TIME	(4) HIGH PART-TIME
CHILD AGED ZERO TO TWO YEARS OLD	-0.0494	-0.901	-0.0201	-0.00386
	(0.0515)	(1.704)	(0.0481)	(0.0582)
NO OF OBSERVATIONS	9,049	6,079	6,079	6,079
CHILD AGED THREE TO FOUR YEARS OLD	0.0241	1.377	0.00785	0.0481
	(0.0446)	(1.508)	(0.0406)	(0.0361)
NO OF OBSERVATIONS	10,440	6,875	6,875	6,875
DEMOGRAPHIC CONTROLS	X	X	X	X
CANTON FE	X	X	X	X
YEAR FE	X	X	X	X

Notes: Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2000-2001 or 2003-2004 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Women residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

TABLE 15 — PLACEBO TEST: JOB CONTINUITY (WOMEN ONLY)

SAMPLES	(1) TENURE	(2) JOB SEARCH	(3) FIRM SIZE
CHILD AGED ZERO TO TWO YEARS OLD	0.163	-0.0643	0.0170
	(0.145)	(0.0451)	(0.148)
NO OF OBSERVATIONS	6,073	6,079	5,667
CHILD AGED THREE TO FOUR YEARS OLD	0.270**	-0.0270	0.0679
	(0.129)	(0.0388)	(0.112)
NO OF OBSERVATIONS	6,867	6,875	6,409
DEMOGRAPHIC CONTROLS	X	X	X
CANTON FE	X	X	X
YEAR FE	X	X	X

Notes: Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2000-2001 or 2003-2004 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Women residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

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

TABLE 16 — PLACEBO TEST: EARNINGS AND WAGES (WOMEN ONLY)

SAMPLES	(1) LOG MONTHLY EARNINGS	(2) LOG HOURLY WAGES
CHILD AGED ZERO TO TWO YEARS OLD	-0.0401	0.126
	(0.130)	(0.139)
NO OF OBSERVATIONS	4,738	4,738
CHILD AGED THREE TO FOUR YEARS OLD	-0.0519	-0.118
	(0.138)	(0.0923)
NO OF OBSERVATIONS	5,393	5,393
DEMOGRAPHIC CONTROLS	X	X
OCCUPATION DUMMIES	X	X
INDUSTRY DUMMIES	X	X
FIRM SIZE	X	X
CANTON FE	X	X
YEAR FE	X	X

Notes: Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2000-2001 or 2003-2004 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Women residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Earnings and wages are reported in Swiss Franc and have been adjusted for inflation using the base year 2005. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

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

TABLE 17 — EMPLOYMENT (MEN ONLY)

SAMPLES	(1) EMPLOYED	(2) HOURS/WEEK	(3) FULL-TIME	(4) HIGH PART-TIME
CHILD AGED ZERO TO TWO YEARS OLD	0.00455	0.611	-0.00860	-0.00837
	(0.0215)	(0.681)	(0.0195)	(0.0222)
NO OF OBSERVATIONS	7,155	6,279	6,280	6,280
CHILD AGED THREE TO FOUR YEARS OLD	-0.0595*	-1.898	-0.0234	0.0139
	(0.0322)	(1.212)	(0.0337)	(0.0360)
NO OF OBSERVATIONS	7,352	6,454	6,454	6,454
DEMOGRAPHIC CONTROLS	X	X	X	X
CANTON FE	X	X	X	X
YEAR FE	X	X	X	X

Notes: Sample restricted to only those men who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Men residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, dummies for education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

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

TABLE 18 — JOB CONTINUITY (MEN ONLY)

SAMPLES	(1) TENURE	(2) JOB SEARCH	(3) FIRM SIZE
CHILD AGED ZERO TO TWO YEARS OLD	-0.0146	0.0389	0.0415
	(0.105)	(0.0420)	(0.129)
NO OF OBSERVATIONS	6,268	6,280	6,136
CHILD AGED THREE TO FOUR YEARS OLD	0.193	-0.0395	0.194
	(0.147)	(0.0594)	(0.153)
NO OF OBSERVATIONS	6,444	6,454	6,305
DEMOGRAPHIC CONTROLS	X	X	X
CANTON FE	X	X	X
YEAR FE	X	X	X

Notes: Sample restricted to only those men who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Men residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

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

TABLE 19 — EARNINGS AND WAGES (MEN ONLY)

SAMPLES	(1) LOG MONTHLY EARNINGS	(2) LOG HOURLY WAGES
CHILD AGED ZERO TO TWO YEARS OLD	-0.154*	-0.149
	(0.0866)	(0.0968)
NO OF OBSERVATIONS	5,672	5,672
CHILD AGED THREE TO FOUR YEARS OLD	-0.00732	0.0381
	(0.0974)	(0.104)
NO OF OBSERVATIONS	5,807	5,807
DEMOGRAPHIC CONTROLS	X	X
OCCUPATION DUMMIES	X	X
INDUSTRY DUMMIES	X	X
FIRM SIZE	X	X
CANTON FE	X	X
YEAR FE	X	X

Notes: Sample restricted to only those men who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Men residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Earnings and wages are reported in Swiss Franc and have been adjusted for inflation using the base year 2005. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

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

TABLE 20 — EMPLOYMENT (MEN AND WOMEN)

SAMPLES	(1) EMPLOYED	(2) HOURS/WEEK	(3) FULL-TIME	(4) HIGH PART-TIME
CHILD AGED ZERO TO TWO YEARS OLD	0.0298	-1.437	0.00348	-0.0152
	(0.0249)	(1.171)	(0.0415)	(0.0316)
NO OF OBSERVATIONS	15,265	11,399	11,400	11,400
CHILD AGED THREE TO FOUR YEARS OLD	0.0475	-1.318	-0.000670	-0.0437
	(0.0292)	(1.106)	(0.0309)	(0.0302)
NO OF OBSERVATIONS	15,879	11,946	11,946	11,946
DEMOGRAPHIC CONTROLS	X	X	X	X
CANTON FE	X	X	X	X
YEAR FE	X	X	X	X

Notes: Estimates from a DDD model (Equation 2). Sample restricted to only those men and women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Those residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, dummies for education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the sex and canton level.

Sources: FOS and SECO.

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

TABLE 21 — JOB CONTINUITY (MEN AND WOMEN)

SAMPLES	(1) TENURE	(2) JOB SEARCH	(3) FIRM SIZE
CHILD AGED ZERO TO TWO YEARS OLD	-0.0639	-0.0610**	-0.0176
	(0.107)	(0.0268)	(0.0863)
NO OF OBSERVATIONS	11,385	11,400	10,874
CHILD AGED THREE TO FOUR YEARS OLD	-0.0837	0.0204	0.0726
	(0.0896)	(0.0268)	(0.0697)
NO OF OBSERVATIONS	11,933	11,946	11,386
DEMOGRAPHIC CONTROLS	X	X	X
CANTON FE	X	X	X
YEAR FE	X	X	X

Notes: Estimates from a DDD model (Equation 2). Sample restricted to only those men and women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Those residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, dummies for education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the sex and canton level.

Sources: FOS and SECO.

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

TABLE 22 — EARNINGS AND WAGES (MEN AND WOMEN)

SAMPLES	(1) LOG MONTHLY EARNINGS	(2) LOG HOURLY WAGES
CHILD AGED ZERO TO TWO YEARS OLD	0.00258	0.0966
	(0.0844)	(0.0727)
NO OF OBSERVATIONS	9,941	9,941
CHILD AGED THREE TO FOUR YEARS OLD	-0.116**	-0.0216
	(0.0510)	(0.0486)
NO OF OBSERVATIONS	10,374	10,374
DEMOGRAPHIC CONTROLS	X	X
OCCUPATION DUMMIES	X	X
INDUSTRY DUMMIES	X	X
FIRM SIZE	X	X
CANTON FE	X	X
YEAR FE	X	X

Notes: Estimates from a DDD model (Equation 2). Sample restricted to only those men and women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Those residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Earnings and wages are reported in Swiss Franc and have been adjusted for inflation using the base year 2005. Standard errors are clustered at the sex and canton level.

Sources: FOS and SECO.

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

TABLE 20B — EMPLOYMENT (MEN AND WOMEN)

SAMPLES	(1) EMPLOYED	(2) HOURS/WEEK	(3) FULL-TIME	(4) HIGH PART-TIME
WOMEN – FIRST CHILD AGED ZERO TO TWO YEARS OLD	-0.0105	-1.328	0.00761	-0.0548
	(0.0378)	(2.341)	(0.0642)	(0.0506)
MEN - FIRST CHILD AGED ZERO TO TWO YEARS OLD	0.00455	0.611	-0.00860	-0.00837
	(0.0215)	(0.681)	(0.0195)	(0.0222)
DDD MODEL - FIRST CHILD AGED ZERO TO TWO YEARS O	0.0298	-1.437	0.00348	-0.0152
	(0.0249)	(1.171)	(0.0415)	(0.0316)
WOMEN - FIRST CHILD AGED THREE TO FOUR YEARS OLD	-0.118	1.053	-0.0631	0.171
	(0.0724)	(1.944)	(0.0494)	(0.107)
MEN - FIRST CHILD AGED THREE TO FOUR YEARS OLD	-0.0595*	-1.898	-0.0234	0.0139
	(0.0322)	(1.212)	(0.0337)	(0.0360)
DDD MODEL - FIRST CHILD AGED THREE TO FOUR YEARS	0.0475	-1.318	-0.000670	-0.0437
	(0.0292)	(1.106)	(0.0309)	(0.0302)
DEMOGRAPHIC CONTROLS	X	X	X	X
CANTON FE	X	X	X	X
YEAR FE	X	X	X	X

Notes: Sample restricted to only those men and women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Those residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, dummies for education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the sex and canton level.

Sources: FOS and SECO.

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

TABLE 21B — JOB CONTINUITY (MEN AND WOMEN)

SAMPLES	(1) TENURE	(2) JOB SEARCH	(3) FIRM SIZE
WOMEN - FIRST CHILD AGED ZERO TO TWO YEARS OLD	-0.321*	-0.00543	0.194
	(0.164)	(0.0454)	(0.160)
MEN - FIRST CHILD AGED ZERO TO TWO YEARS OLD	-0.0146	0.0389	0.0415
	(0.105)	(0.0420)	(0.129)
DDD MODEL - FIRST CHILD AGED ZERO TO TWO YEARS O	-0.0639	-0.0610**	-0.0176
	(0.107)	(0.0268)	(0.0863)
WOMEN - FIRST CHILD AGED THREE TO FOUR YEARS OLD	-0.104	0.0205	-0.0652
	(0.209)	(0.0466)	(0.186)
MEN - FIRST CHILD AGED THREE TO FOUR YEARS OLD	0.193	-0.0395	0.194
	(0.147)	(0.0594)	(0.153)
DDD MODEL - FIRST CHILD AGED THREE TO FOUR YEARS	-0.0837	0.0204	0.0726
	(0.0896)	(0.0268)	(0.0697)
DEMOGRAPHIC CONTROLS	X	X	X
CANTON FE	X	X	X
YEAR FE	X	X	X

Notes: Sample restricted to only those men and women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Those residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, dummies for education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the sex and canton level.

Sources: FOS and SECO.

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

TABLE 22B — EARNINGS AND WAGES (MEN AND WOMEN)

SAMPLES	(1) LOG MONTHLY EARNINGS	(2) LOG HOURLY WAGES
WOMEN - FIRST CHILD AGED ZERO TO TWO YEARS OLD	-0.0896	0.0385
	(0.173)	(0.112)
MEN - FIRST CHILD AGED ZERO TO TWO YEARS OLD	-0.154*	-0.149
	(0.0866)	(0.0968)
DDD MODEL - FIRST CHILD AGED ZERO TO TWO YEARS O	0.00258	0.0966
	(0.0844)	(0.0727)
WOMEN - FIRST CHILD AGED THREE TO FOUR YEARS OLD	-0.0694	-0.155
	(0.118)	(0.103)
MEN - FIRST CHILD AGED THREE TO FOUR YEARS OLD	-0.00732	0.0381
	(0.0974)	(0.104)
DDD MODEL - FIRST CHILD AGED THREE TO FOUR YEARS	-0.116**	-0.0216
	(0.0510)	(0.0486)
DEMOGRAPHIC CONTROLS	X	X
CANTON FE	X	X
YEAR FE	X	X

Notes: Sample restricted to only those men and women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Those residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, dummies for education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the sex and canton level.

Sources: FOS and SECO.

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

TABLE 23 — EMPLOYMENT (MEN AND WOMEN)

SAMPLES	(1) EMPLOYED	(2) HOURS/WEEK	(3) FULL-TIME	(4) HIGH PART-TIME
CHILD AGED ZERO TO TWO YEARS OLD	0.0274	-0.00788	0.0114	0.0198
	(0.0251)	(0.881)	(0.0315)	(0.0275)
NO OF OBSERVATIONS	3,395	2,309	2,310	2,310
CHILD AGED THREE TO FOUR YEARS OLD	0.0390	0.121	0.0106	-0.0138
	(0.0277)	(0.826)	(0.0260)	(0.0341)
NO OF OBSERVATIONS	4,009	2,856	2,856	2,856
DEMOGRAPHIC CONTROLS	X	X	X	X
CANTON FE	X	X	X	X
YEAR FE	X	X	X	X

Notes: Estimates from a DiD model (Equation 1). Sample restricted to only those men and women who were aged between 15 to 45 years old at the birth of their first child. All those in the sample had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, dummies for education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the sex and canton level.

Sources: FOS and SECO.

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

TABLE 24 — JOB CONTINUITY (MEN AND WOMEN)

SAMPLES	(1) TENURE	(2) JOB SEARCH	(3) FIRM SIZE
CHILD AGED ZERO TO TWO YEARS OLD	0.0243	-0.0389	-0.0159
	(0.0999)	(0.0239)	(0.0736)
NO OF OBSERVATIONS	2,305	2,310	2,231
CHILD AGED THREE TO FOUR YEARS OLD	0.0225	0.0335	0.0918
	(0.0829)	(0.0304)	(0.0686)
NO OF OBSERVATIONS	2,853	2,856	2,743
DEMOGRAPHIC CONTROLS	X	X	X
CANTON FE	X	X	X
YEAR FE	X	X	X

Notes: Estimates from a DiD model (Equation 1). Sample restricted to only those men and women who were aged between 15 to 45 years old at the birth of their first child. All those in the sample had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, dummies for education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the sex and canton level.

Sources: FOS and SECO.

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

TABLE 25 — EARNINGS AND WAGES (MEN AND WOMEN)

SAMPLES	(1) LOG MONTHLY EARNINGS	(2) LOG HOURLY WAGES
CHILD AGED ZERO TO TWO YEARS OLD	0.0658	0.0546
	(0.0755)	(0.0610)
NO OF OBSERVATIONS	2,032	2,032
CHILD AGED THREE TO FOUR YEARS OLD	-0.0607	-0.0650*
	(0.0446)	(0.0366)
NO OF OBSERVATIONS	2,465	2,465
DEMOGRAPHIC CONTROLS	X	X
OCCUPATION DUMMIES	X	X
INDUSTRY DUMMIES	X	X
FIRM SIZE	X	X
CANTON FE	X	X
YEAR FE	X	X

Notes: Estimates from a DiD model (Equation 1). Sample restricted to only those men and women who were aged between 15 to 45 years old at the birth of their first child. All those in the sample had their first child in 2003-2007 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Earnings and wages are reported in Swiss Franc and have been adjusted for inflation using the base year 2005. Standard errors are clustered at the sex and canton level.

Sources: FOS and SECO.

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

TABLE 26 — SAMPLE COMPOSITION

SAMPLES	(1) CURRENT AGE	(2) AGE AT FIRST CHILD BIRTH	(3) LOWER SEC	(4) UPPER SEC/ SOME TERTIA	(5) TERTIARY	(6) MARRIED	(7) SWISS BORI	(8) TOTAL NO OF CHILDRE
FIRST CHILD AGED ZERO TO T	0.266	0.282	-0.0772***	-0.0137	0.0904**	-0.0256	0.0880	-0.0334
	(0.487)	(0.450)	(0.0224)	(0.0445)	(0.0399)	(0.0266)	(0.0519)	(0.0457)
NO OF OBSERVATIONS	8,110	8,110	8,110	8,110	8,110	8,110	8,110	8,110
FIRST CHILD AGED THREE TO	-0.917	-0.434	-0.0268	0.0299	-0.00473	-0.0876**	0.230***	-0.461***
	(0.562)	(0.580)	(0.0536)	(0.0513)	(0.0383)	(0.0384)	(0.0823)	(0.0635)
NO OF OBSERVATIONS	8,527	8,527	8,527	8,527	8,527	8,527	8,527	8,527
CANTON FE	X	X	X	X	X	X	X	X
YEAR FE	X	X	X	X	X	X	X	X

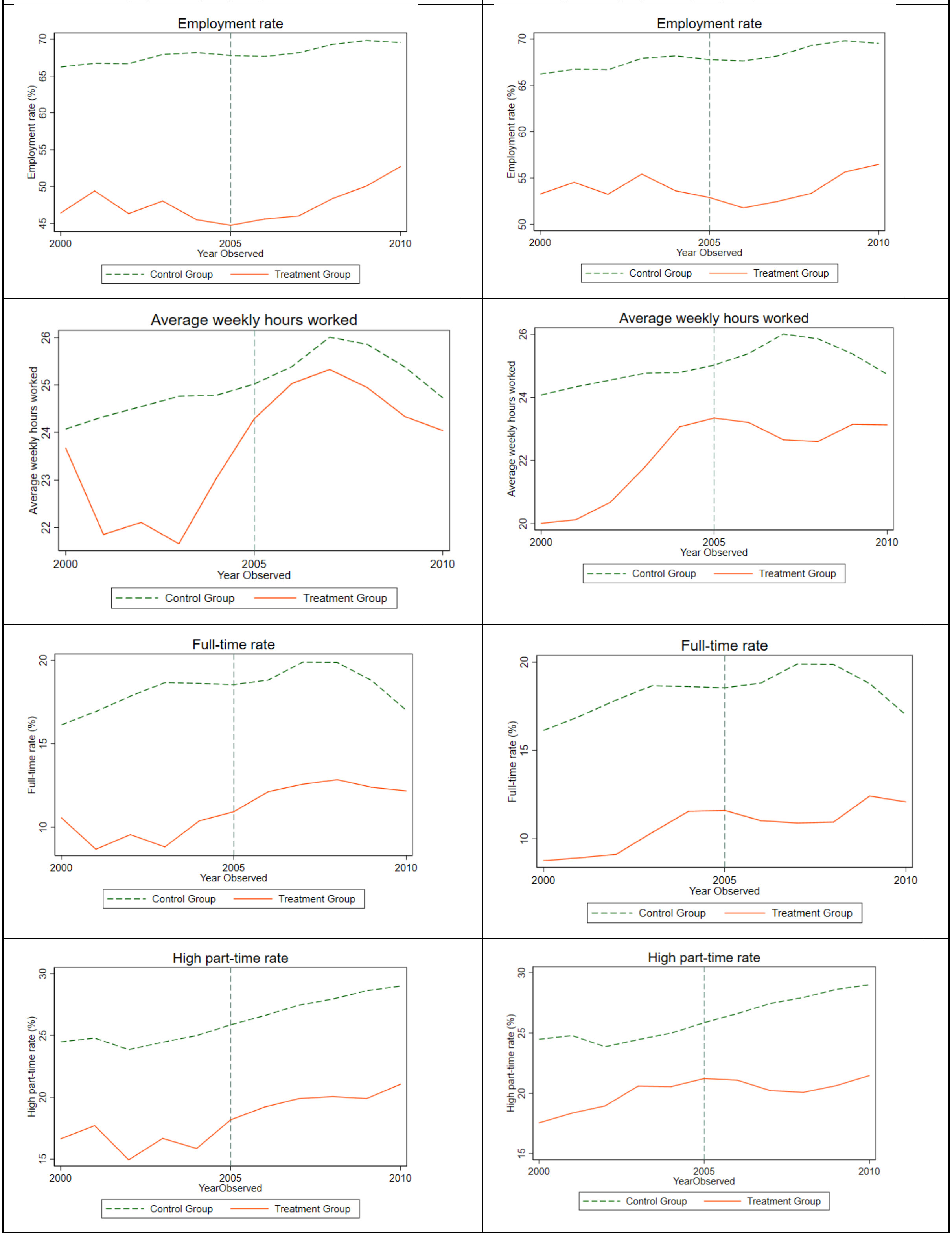
Notes: Estimates from a DiD model. Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. All those in the sample had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those residing in the canton of Geneva are excluded. Standard errors are clustered at the canton level.

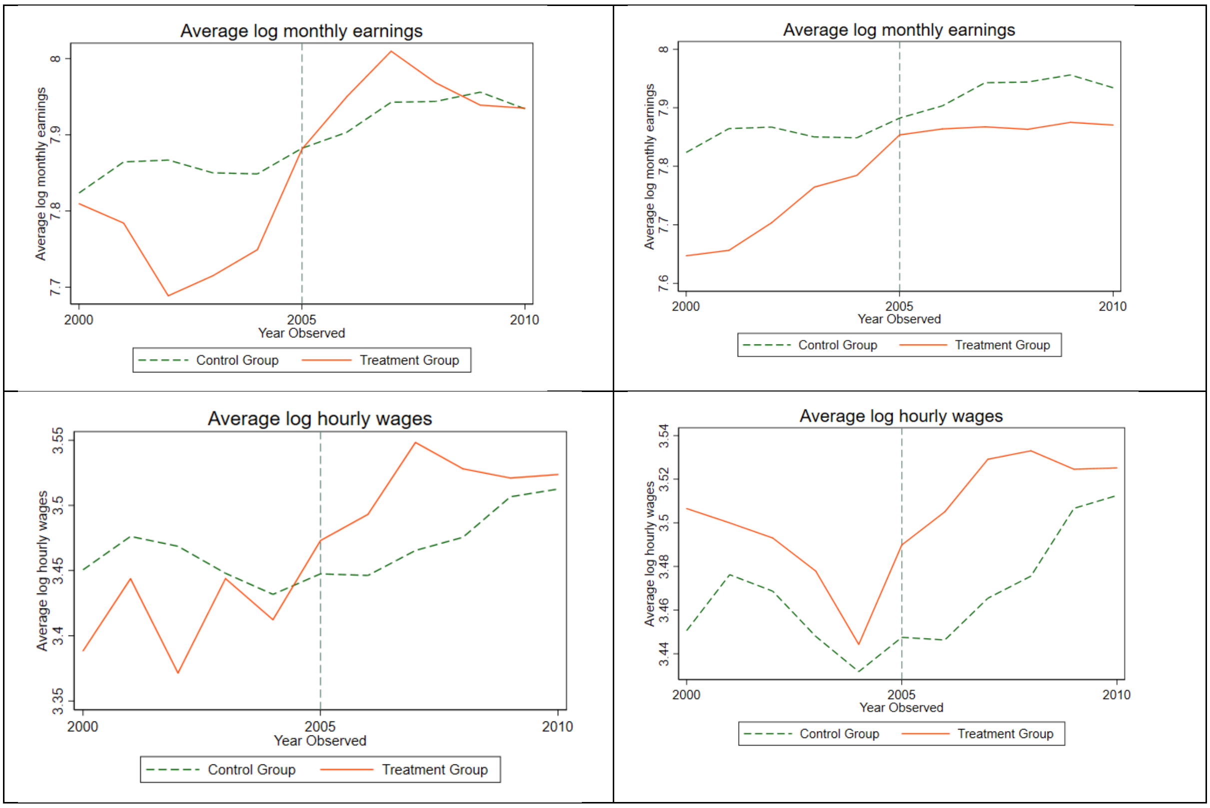
Sources: FOS and SECO.

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

FIGURE 1 —DESCRIPTIVE EVIDENCE (WOMEN ONLY)

COLUMN (I): TREATMENT GROUP OF WOMEN WITH FIRST CHILD AGED 0-2YO	COLUMN (II): TREATMENT GROUP OF WOMEN WITH FIRST CHILD AGED 3-4YO



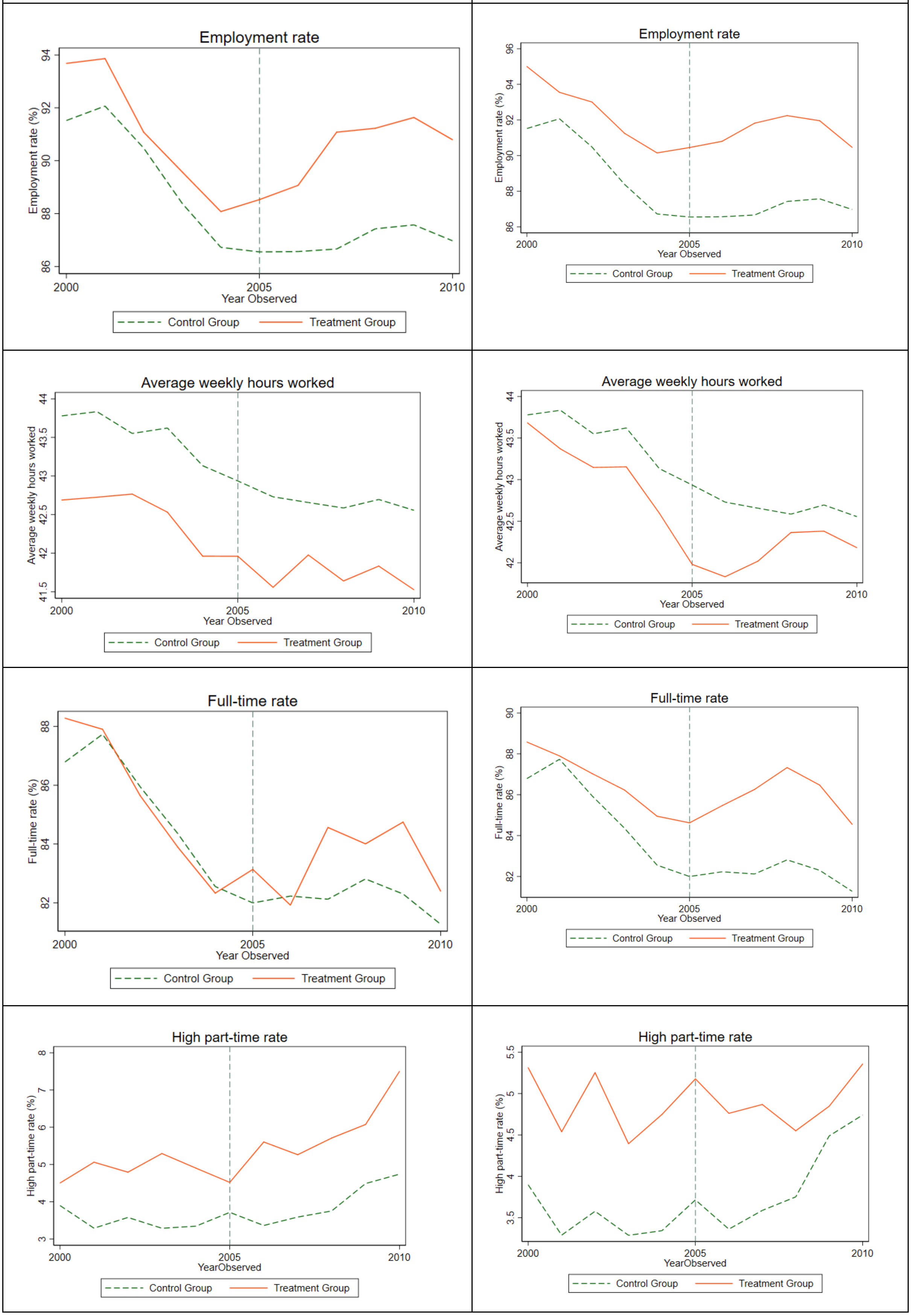


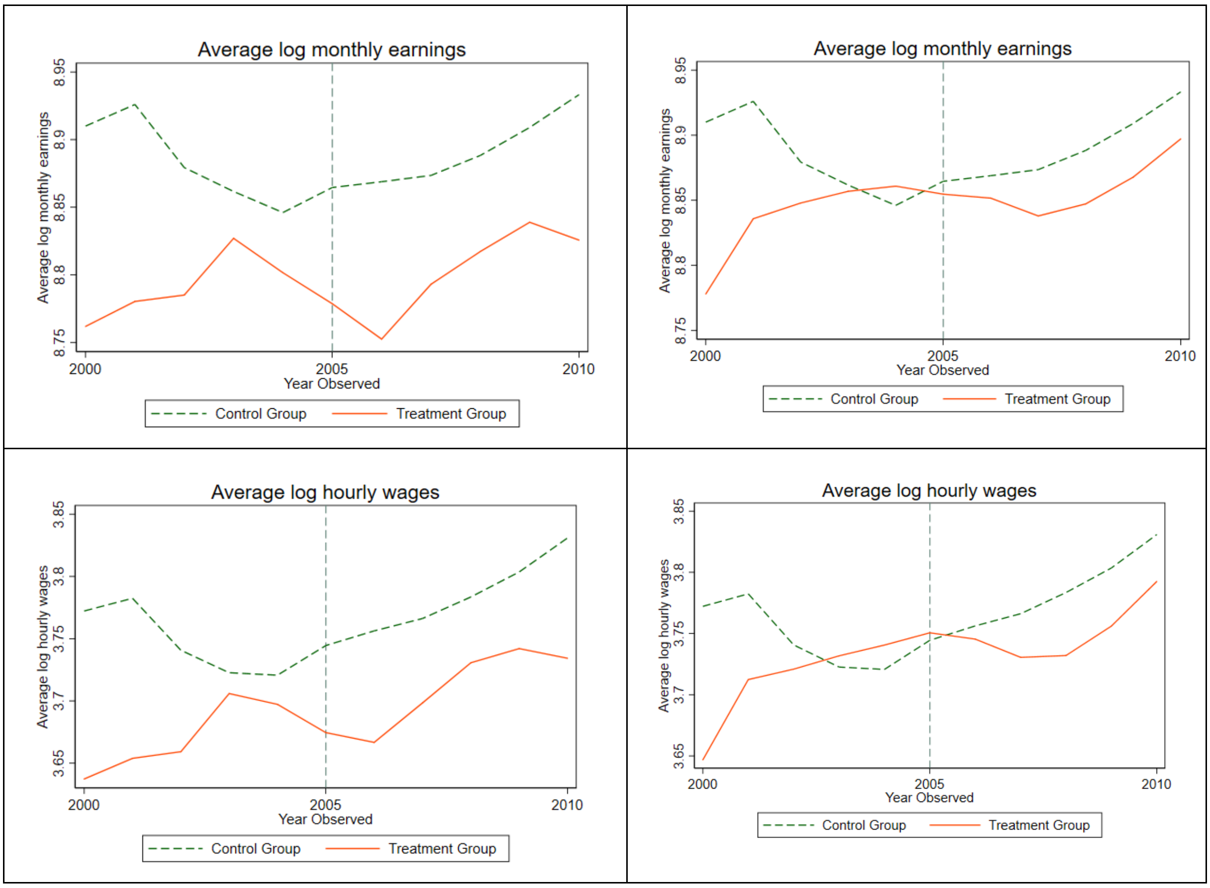
Notes: Values shown are three-point moving averages. Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treated group had a first child aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Women residing in the canton of Geneva are excluded.

Sources: FOS, own calculations.

FIGURE 2 —DESCRIPTIVE EVIDENCE (MEN ONLY)

COLUMN (I): TREATMENT GROUP OF MEN WITH FIRST CHILD AGED 0-2YO	COLUMN (II): TREATMENT GROUP OF MEN WITH FIRST CHILD AGED 3-4YO



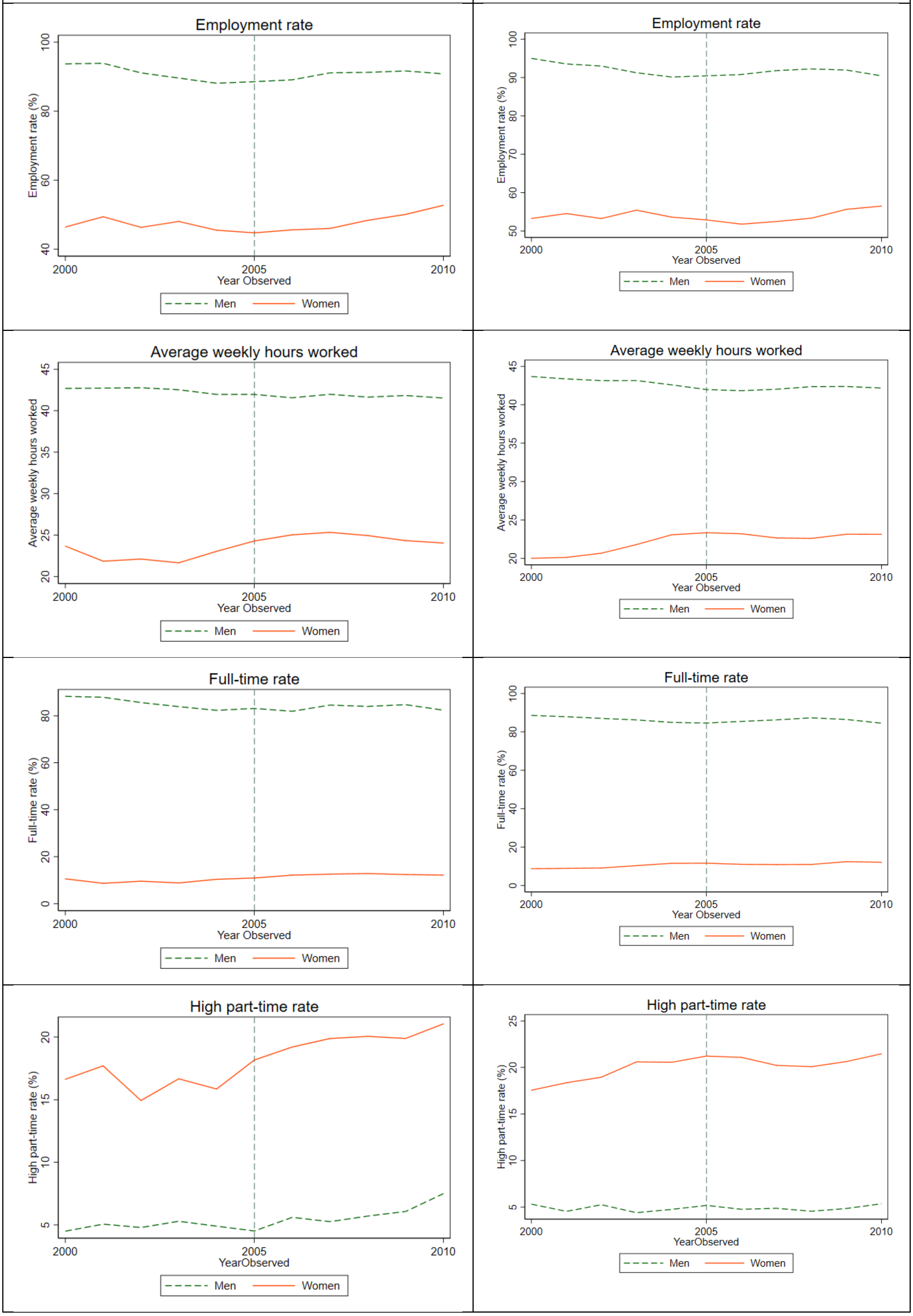


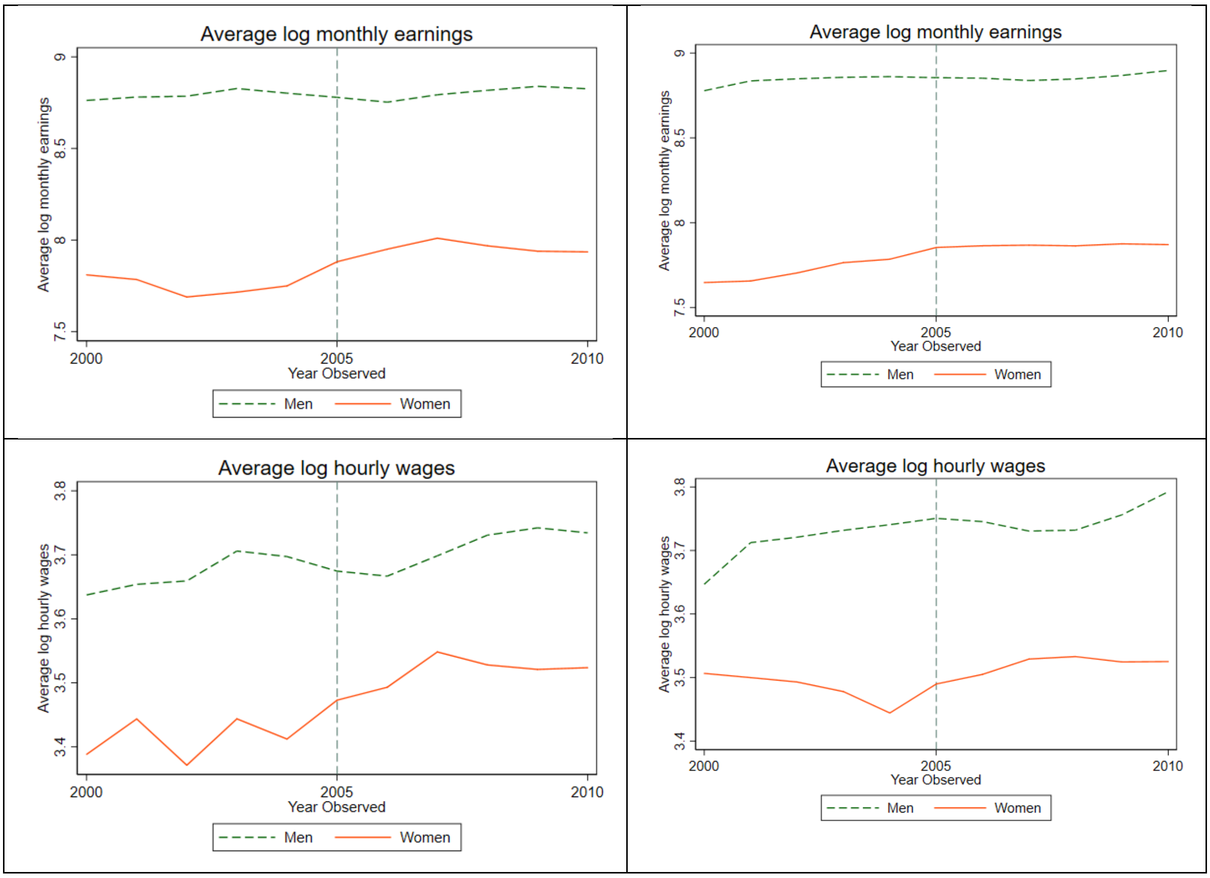
Notes: Values shown are three-point moving averages. Sample restricted to only those men who were aged between 15 to 45 years old at the birth of their first child. Those in the treated group had a first child aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Men residing in the canton of Geneva are excluded.

Sources: FOS, own calculations.

FIGURE 3 —DESCRIPTIVE EVIDENCE (TREATMENT GROUP OF MEN AND WOMEN)

COLUMN (I): TREATMENT GROUP OF MEN AND WOMEN WITH FIRST CHILD AGED 0-2YO	COLUMN (II): TREATMENT GROUP OF MEN AND WOMEN WITH FIRST CHILD AGED 3-4YO



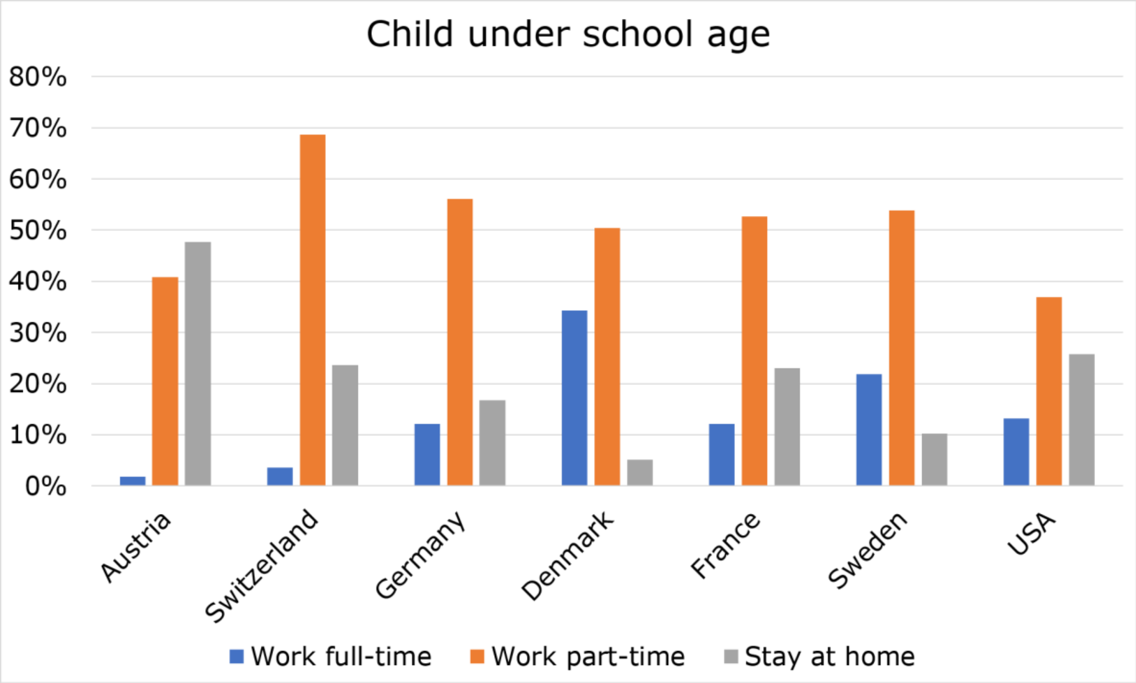


Notes: Values shown are three-point moving averages. Sample restricted to only those men and women who were aged between 15 to 45 years old at the birth of their first child and who had a first child aged between zero and four years old at the time that they were surveyed. Men and women residing in the canton of Geneva are excluded.

Sources: FOS, own calculations.

Appendix B. Supplementary Tables and Figures

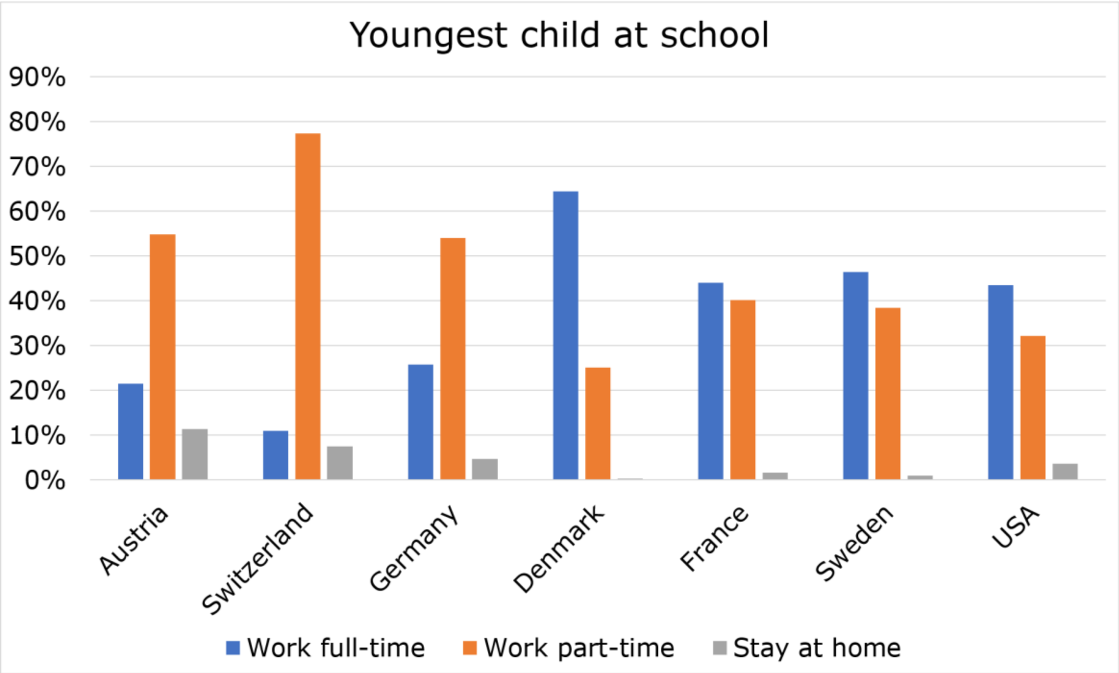
FIGURE B.1 —CONSERVATIVE VALUES REGARDING GENDER ROLES



Notes: Responses to the International Social Survey Program (ISSP) question, "*Do you think that women should work outside the home full-time, part-time or not at all under the following circumstances: When there is a child under school age?*".

Sources: ISSP 2012.

FIGURE B.2 —CONSERVATIVE VALUES REGARDING GENDER ROLES



Notes: Responses to the International Social Survey Program (ISSP) question, "*Do you think that women should work outside the home full-time, part-time or not at all under the following circumstances: After the youngest child starts school?*".

Sources: ISSP 2012.

Appendix C. Robustness Checks

TABLE C.1 — EMPLOYMENT (WOMEN ONLY)

VARIABLE	(1)	(2)	DIFF
TOTAL NO. OF CHILDREN	2.35	2.36	0.01
	(0.01)	(0.01)	(0.021)
NO OF OBSERVATIONS	1,601	1,663	3,264

Notes: Sample of women who were aged between 15 to 45 years old at the birth of their first child and whose youngest child was born in 2003-2004 (column 1) or 2006-2007 (column 2) and who had a previous child aged five years old or older. Women residing in the canton of Geneva are excluded.

Sources: FOS.

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

TABLE C.2 — EMPLOYMENT (WOMEN ONLY)

SAMPLES	(1) EMPLOYED	(2) HOURS/WEEK	(3) FULL-TIME	(4) HIGH PART-TIME
CHILD AGED ZERO TO TWO YEARS OLD	-0.0127	-1.737	-0.00292	-0.0525
	(0.0356)	(2.307)	(0.0591)	(0.0510)
NO OF OBSERVATIONS	5,724	3,463	3,463	3,463
CHILD AGED THREE TO FOUR YEARS OLD	-0.114	0.624	-0.0615	0.127
	(0.0854)	(1.891)	(0.0544)	(0.108)
NO OF OBSERVATIONS	6,141	3,835	3,835	3,835
DEMOGRAPHIC CONTROLS	X	X	X	X
CANTON FE	X	X	X	X
YEAR FE	X	X	X	X

Notes: Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed and had at least two children. Women residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, dummies for education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

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

TABLE C.3 — JOB CONTINUITY (WOMEN ONLY)

SAMPLES	(1) TENURE	(2) JOB SEARCH	(3) FIRM SIZE
CHILD AGED ZERO TO TWO YEARS OLD	-0.345*	0.00501	0.172
	(0.174)	(0.0499)	(0.180)
NO OF OBSERVATIONS	3,462	3,463	3,217
CHILD AGED THREE TO FOUR YEARS OLD	-0.288	0.0129	-0.252
	(0.212)	(0.0427)	(0.187)
NO OF OBSERVATIONS	3,834	3,835	3,560
DEMOGRAPHIC CONTROLS	X	X	X
CANTON FE	X	X	X
YEAR FE	X	X	X

Notes: Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed and had at least two children. Women residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

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

TABLE C.4 — EARNINGS AND WAGES (WOMEN ONLY)

SAMPLES	(1) LOG MONTHLY EARNINGS	(2) LOG HOURLY WAGES
CHILD AGED ZERO TO TWO YEARS OLD	-0.0702	0.0910
	(0.172)	(0.118)
NO OF OBSERVATIONS	2,906	2,906
CHILD AGED THREE TO FOUR YEARS OLD	-0.0908	-0.141
	(0.125)	(0.103)
NO OF OBSERVATIONS	3,204	3,204
DEMOGRAPHIC CONTROLS	X	X
OCCUPATION DUMMIES	X	X
INDUSTRY DUMMIES	X	X
FIRM SIZE	X	X
CANTON FE	X	X
YEAR FE	X	X

Notes: Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed and had at least two children. Women residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Earnings and wages are reported in Swiss Franc and have been adjusted for inflation using the base year 2005. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

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

TABLE C.5 — EMPLOYMENT (WOMEN ONLY)

SAMPLES	(1) EMPLOYED	(2) HOURS/WEEK	(3) FULL-TIME	(4) HIGH PART-TIME
CHILD AGED ZERO TO TWO YEARS OLD	-0.0105	-1.302	-0.00978	-0.0366
	(0.0378)	(1.315)	(0.0303)	(0.0280)
NO OF OBSERVATIONS	8,110	8,110	8,110	8,110
CHILD AGED THREE TO FOUR YEARS OLD	-0.118	-2.159	-0.0580	0.0409
	(0.0724)	(1.887)	(0.0377)	(0.0548)
NO OF OBSERVATIONS	8,527	8,527	8,527	8,527
DEMOGRAPHIC CONTROLS	X	X	X	X
CANTON FE	X	X	X	X
YEAR FE	X	X	X	X

Notes: Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Women residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, dummies for education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

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

TABLE C.6 — EARNINGS AND WAGES (WOMEN ONLY)

SAMPLES	(1) MONTHLY EARNINGS	(2) HOURLY WAGES	(3) MONTHLY EARNINGS	(4) HOURLY WAGES
CHILD AGED ZERO TO TWO YEARS OLD	-180.9	-7.094	-222.2	-4.964
	(548.9)	(11.65)	(211.6)	(4.308)
NO OF OBSERVATIONS	4,270	4,270	7,599	7,599
CHILD AGED THREE TO FOUR YEARS OLD	-492.7	-11.81	-587.0	-10.56*
	(965.1)	(9.567)	(545.0)	(5.281)
NO OF OBSERVATIONS	4,568	4,568	7,962	7,962
DEMOGRAPHIC CONTROLS	X	X	X	X
OCCUPATION DUMMIES	X	X		
INDUSTRY DUMMIES	X	X		
FIRM SIZE	X	X		
CANTON FE	X	X	X	X
YEAR FE	X	X	X	X

Notes: Sample restricted to only those women who were aged between 15 to 45 years old at the birth of their first child. Those in the treatment group had their first child in 2003-2004 or 2006-2007 and whose first child was aged between zero and four years old at the time that they were surveyed. Those in the control group had a youngest child aged between five to 17 years old at the time that they were surveyed. Women residing in the canton of Geneva are excluded. Demographic controls include age and its quadratic, age at birth of first child, education, marital status, and whether born in Switzerland. Also included as a control is the annual cantonal unemployment rate. Earnings and wages are reported in Swiss Franc and have been adjusted for inflation using the base year 2005. Standard errors are clustered at the canton level.

Sources: FOS and SECO.

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