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The Effect of Childcare on Parental Earnings Trajectories

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Abstract

We study the effect of institutional childcare on child penalties. Using Swiss administrative data, we exploit the staggered opening of childcare facilities across municipalities in the canton of Bern. We find that the presence of childcare facilities in the year of birth of the first child reduces the child penalty. The availability of childcare increases maternal earnings and decreases the compensating increase in fathers' earnings in households with below median earnings, but not in households with above median earnings. Although childcare affects relative earnings contributions within the household, there is no effect on total household earnings.

JEL-Codes: D100, J000, J130, J160, H310.

Keywords: child penalty, childcare, parental employment, gender wage gap.

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1 Introduction

The narrowing of the gender gap in earnings has been one of the major advances in society over the past decades. Goldin (2006) pointedly describes how over the past century, women have evolved from passive workers, whose life cycle patterns were largely determined by economic necessity and marriage, to decision makers, whose "occupation and employment define [their] societal worth." Women's higher investments in their education and careers reflect increased intrinsic motivation to succeed in the labor market.

Despite the advances in women's educational attainment and labor market attachment, the gender earnings gap still widens substantially with age, especially in the 15 to 20 years after leaving school (Goldin et al., 2017) and the birth of a first child still heavily affects women's life cycle earnings patterns.[1] Goldin and Mitchell (2017) reason that the drop in women's earnings observed during their early to mid 30s has become more apparent precisely because of the increase in employment at younger ages.

Recent literature argues that the divergence of female and male earnings following the birth of their first child, also referred to as the "child penalty", nowadays accounts for most of the gender inequality in earnings. Kleven et al. (2019a) find that in Denmark, earnings of mothers and fathers diverge by 20 percent in the long run.[2] The resulting differential impact of childbirth on lifetime earnings might adversely affect women in their pension payments, savings, economic self-sufficiency, and their power in household bargaining compared to men (Goldin et al., 2017). This suggests that addressing the child penalty is first order when it comes to reducing gender inequality in earnings and its consequences. It is therefore crucial to understand whether the differential is a deliberate preference-based choice or whether it is brought about by a lack of institutions hindering women, who would otherwise prefer to remain employed, to stay attached to the labor market. A lack of affordable and available formal childcare arrangements is seen as one major obstacle to the labor market attachment of mothers (see, e.g., Kleven et al., 2019a; Olivetti and Petrongolo, 2017).[3]

In this paper, we contribute to a better understanding of the child penalty by studying whether policies to provide institutional childcare attenuate the drop in labor earnings, henceforth referred to as earnings, women experience after the birth of their first child. We propose, and document, that the institutional environment when the first child is born is a major determinant of female earnings and employment patterns in subsequent years. This claim is motivated by the idea that in the year of birth of their first child, parents

[1] See, e.g. Goldin et al. (2017) or Goldin (2014) for more detailed discussions of the persisting gender earnings gap and how it is affected by children.

[2] Kleven et al. (2019b) extend the analysis in Kleven et al. (2019a) to six countries with varying policies and norms, Austria, Denmark, Germany, Sweden, the United Kingdom, and the United States. They show that the gender gap in earnings 10 years after the birth of the first child varies substantially between countries, ranging from 20 percent in Denmark to 60 percent in Germany.

[3] Kleven et al. (2019a) and Olivetti and Petrongolo (2017), for example, argue that childcare is one of the main reasons for the comparatively high labor market attachment of mothers in Scandinavian countries.

decide upon employment and care arrangements, which are then likely to be sticky. The availability of childcare at the time this decision is made decreases direct as well as indirect costs for women to remain employed.

In order to establish the effect of childcare availability on the child penalty, we introduce institutions into the event study framework. Switzerland is well-suited to study the impact of childcare on the earnings penalty. Childcare facilities have only become more prevalent in the past 20 years. The combination of administrative data on individuals' earnings and the staggered opening of childcare facilities across municipalities in the canton of Bern allows us to compare child penalties of parents in municipalities after the introduction of childcare facilities to child penalties of parents in the same municipalities before the introduction of childcare facilities in an otherwise homogeneous institutional setting. We find a female child penalty (in earnings) of about 70 percent, which is partly driven by a participation reduction of about 20 percent six years after the birth of the first child. The documented earnings penalty is comparable to the one found for Germany (for numbers on Germany and five other countries, see Kleven et al., 2019b). However, while the reduction in female participation rates in Germany is a major factor in explaining the earnings gap after the birth of a first child, in Switzerland the intensive margin, i.e., the decision how much to work, seems especially relevant.

We find that the presence of a childcare facility in the municipality of residence at the time of birth of a first child decreases the total child penalty by 8.2 percentage points (10.4 percent) from 79 percent to 70.8 percent. More than 50 percent of this effect are due to a reduction of the female child penalty by 4.5 percentage points (6.3 percent) from 71.4 percent to 66.9 percent. Childcare reduces the female child penalty in households with below-median household earnings by 11.2 percentage points, which fully accounts for the overall effect among women. We do not observe any statistically significant response among women in households with above median earnings.[4]

A reduction in the increase in male earnings in the presence of childcare explains the other half of the reduction in the child penalty. Without childcare, we find a positive long-run response of male earnings to parenthood, which also contributes to the child penalty. We find a substantial impact of parenthood for men in households with below median earnings, whose earnings increase by about 20 percent. Childcare reduces this increase by 8.4 percentage points. This is consistent with the idea that men have to make up for lost earnings of their partner.[5]

The increase in female earnings and the decrease in male earnings cancel each other out. The availability of childcare thus has no effect on total household earnings, but increases the relative earning contribution by women by about 2.7 percentage points.

[4]We define households according to their pre-child earnings. Also, note that we define household earnings as the combined earnings of both parents irrespective of whether they actually form a household.

[5]Note that part of the increase in male earnings is mechanical as it includes child benefits, which are declared as part of their earnings.

Furthermore, we are able to empirically study how the availability of childcare and financial support for childcare costs interact with one another because some municipalities in the canton of Bern introduce subsidies sometime after the opening of a childcare facility. We find that the main part of the overall effect on the female child penalty is most likely driven by subsidized childcare. There is only a slight effect of childcare without subsidies in the three years following the birth of the first child with no longer term effect. Subsidized childcare, however, reduces the child penalty by about 10 percent.

Our results render valuable insights to assessing the effectiveness of formal childcare as a policy instrument. Generally, policy makers tend to provide childcare with two goals in mind: First, they want to increase women's labor market attachment, thus increasing their economic independence and reducing the gender earnings gap. Second, they want to increase the earnings potential of households at the bottom of the earnings distribution. Our results suggest that childcare provision meets the first goal in lower earnings households. Childcare reduces the female child penalty of mothers below the median of the households earnings distribution. A decrease in male earnings compared to a situation with no childcare, however, counterbalances this increase in female earnings, resulting in a zero effect on earnings at the household level. The finding that childcare reduces the strain on fathers in lower-earnings households to compensate for at least part of the earnings drop of their partner represents a desirable, though frequently ignored, side effect. Childbirth thus decreases household earnings by around 20 percent independently of whether childcare is available or not, but reduces the gender earnings gap by reducing the child penalty and increasing the female income share within the household.

A supplementary analysis confirms our claim that it is the availability of childcare in the year of birth that matters most for the effect on female earnings trajectories. Using different treatment definitions relative to the year of birth, we document that the observed effects are primarily driven by individuals having their child after or exactly in the year a childcare facility is opened in their municipality of residence. There are virtually no effects if childcare is introduced afterwards. We validate the interpretation of our findings by studying competing alternative mechanisms behind the effects. We show that i) the exploited treatment is effective in increasing the take-up of childcare, ii) individuals do not change their fertility decision in anticipation of the introduction of childcare, that iii) selective mobility is unlikely to drive our results, iv) the municipalities that open childcare facilities between 2001 and 2015 do not systematically differ from other municipalities in the canton of Bern with respect to the impact of children on female earnings, v) our results are not driven by differential time trends in municipalities introducing childcare, and vi) our estimation approach does not yield a significant effects in case of placebo introduction dates of childcare facilities. In a final step, we briefly discuss the role of gender norms as a driving force of the child penalty.

Our findings contribute to several strands of literature. First, we contribute to the growing literature on employment effects of parenthood (see, e.g. Angelov et al., 2016; Bütikofer et al., 2018; Kleven et al., 2019a,b) and extend it by studying the differential impact of childcare. Our findings are in line with the evidence provided by Olivetti and Petrongolo (2017) that the institutional setting is crucial for the extent of the child penalty. Our work is complementary to three ongoing projects (Kleven et al., 2020b; Girsberger et al., 2019; Bönke et al., 2019) which assess the effect of parental leave policies on the child penalty. Our paper is most closely related to the very recent contribution by Kleven et al. (2020b). They evaluate the impact of 60 years of family policy (maternity leave and childcare expansions) on gender inequality in Austria and find very limited effects. The differences between their findings and ours suggest that institutional context matters. In contrast to Kleven et al. (2020b), who focus on a long term effect of historical childcare expansions in an institutional setting with generous maternity leave, we concentrate on the immediate impact of recent childcare introductions in an institutional setting with short statutory maternity leave. The effectiveness of institutional childcare policies might well be affected by parental leave policies. The historical context and changed gender roles, as well as the potential difference between expansion of existing childcare facilities versus new introductions offer additional explanations for why the effects differ.

Second, our findings contribute to the large literature on the effect of childcare on maternal employment in general. Most of the empirical evidence on the effects of childcare availability on maternal labor outcomes focuses on the extensive margin. The evidence in early work, reviewed by Blau (2003) and Blau and Currie (2006), is mixed. More recent studies take the inherent endogeneity between childcare availability, childcare costs, and maternal labor supply into account. While childcare could affect maternal employment, employed mothers could also induce a higher demand for such services and drive its supply. These studies exploit quasi-experimental settings to draw causal conclusions.

One line of studies exploits large childcare expansions with a simultaneous introduction of childcare subsidies for mothers in general. Some of these studies find effects only for subgroups, such as single mothers (see Gelbach, 2002, and Cascio, 2009, for the US, and Goux and Maurin, 2010, for France) or less skilled mothers (see Cascio and Schanzenbach, 2013, for the US). Other studies find more substantial effects, documenting increases in maternal participation rates and hours worked (see Lefebvre et al., 2009, Haeck et al., 2015, and Lefebvre and Merrigan, 2008, for Canada, Müller and Wrohlich, 2020, and Bauernschuster and Schlotter, 2015, for Germany, and Nollenberger and Rodríguez-Planas, 2015, for Spain). The heterogeneous findings in these prior studies might be explained by private childcare arrangements, with a larger availability in the US or France than in Germany or Spain. The differences described above may therefore reflect differences in take-up rates related to the intervention, with the expansion of public childcare crowding out private arrangements in the former case and increasing availability of childcare in the latter.

Another line of studies uses large increases in childcare subsidies, with no increase in availability or take-up rates (see Givord and Marbot, 2015, for France, Lundin et al., 2008, for Sweden, or Havnes and Mogstad, 2011, for Norway) and finds no or very modest effects on mothers' employment or earnings.

Importantly, the existing literature studies the impact of childcare independently of the timing of the expansion with respect to the birth of the first child. We, in contrast, argue that it is the access to childcare at this time which is particularly relevant. We focus on the effect of childcare at the moment mothers make the decision to return to work after the birth of their first child. In that, we combine the literature on employment effects of parenthood and the effect of childcare on parental employment. Administrative data on the whole population of the canton of Bern in Switzerland allow us to study both the intensive and the extensive margin of mothers' and fathers' labor market decisions. Over the years 2001-15, for which we have data, 59 of 401 municipalities in the canton of Bern introduced childcare, whereas 26 already had childcare facilities before 2001.[6] On top of this considerable expansion in childcare provision, we exploit the introduction of subsidized care, which some municipalities only started offering several years after the opening of a childcare facility. Though we have to rely on limited variation for this analysis, it allows us to distinguish between an availability and a price effect and contribute to the reconciliation of previous strands of literature.

The remainder of the paper is organized as follows. Section 2 lays out our empirical strategy, before Section 3 describes the data basis. Section 4 presents our results on the overall child penalty in Switzerland and on the differential impact of childcare availability. Section 5 validates our estimation strategy and discusses the role of norms in the child penalty, before Section 6 offers some concluding remarks.

2 Empirical strategy

We rely on an event study approach to investigate the effect of childcare on parents' work arrangements after the birth of their first child. We study how female and male earnings and participation rates change after the birth of their first child and estimate differential effects depending on whether there is access to childcare in the year of birth. We briefly describe our baseline event study approach before we present the strategy to identify the differential effect of childcare availability.

We build upon the event study approach proposed by Kleven et al. (2019a) to study parental labor market outcomes after the birth of their first child conditional on pre-child choices. In this setting, the birth year of the first child is denoted as event time $t = 0$ for each parent, and all prior and subsequent years are defined relative to this event. We regress labor market outcomes on event time indicators and covariates using equation

$$Y^g_{ist} = \alpha + \sum_{j=-3}^{j=6} \beta^g_j \times I[j = t] + \gamma^g_{age} + \delta^g_s + \mu^g_{married} + \nu^g_i + u^g_{ist}, \tag{1}$$

where Y^g_{ist} describes the labor market outcome of individual i of gender g in year s at event time t. The event time parameters β^g_j, with $\beta^g_{-1} = 0$, identify the change in outcomes

[6] In 2001, around 19.0% of taxpayers in our data were living in municipalities that introduced childcare between 2001-15, whereas 35.3% were living in municipalities, including the canton's largest municipalities Bern and Biel, which already had childcare before 2001.

relative to the year prior to the birth of the first child, $t = -1$. We further add a full set of age effects to control for life-cycle effects (γ^g_{age}), year effects to control for any time varying shocks (δ^g_s), and individual fixed effects to control for time invariant individual characteristics (ν^g_i).[7] We further include an indicator variable for being married prior to the birth of the first child ($\mu^g_{married}$).

In line with a recent contribution by Schmidheiny and Siegloch (2019), we apply binning at both ends of our event time window. We keep individuals in years outside of the range of our event time window, which runs from three years before to six years after birth of the first child, in our sample. Outside this window, however, we bin the event time dummies at -3 and 6. The event time estimate $\beta^g_{-j} = \beta^g_{-3}$ thus identifies the effect for all $t \leq -3$ and $\beta^g_j = \beta^g_6$ identifies the effect of $t \geq 6$. Binning ensures the separate identification of the event time indicators while flexibly controlling for age and time in the presence of individual fixed effects.

We run the estimation in levels rather than in logs. This allows us to keep all individuals in our sample, even if their earnings drop to zero at some point. A non-negligible fraction of women stops working for some time after the birth of the first child. To convert the estimated earnings changes after the birth of the first child into percentage changes relative to the year before the birth of the first child, we divide the event time estimates $\hat{\beta}^g_j$ by counterfactual earnings, i.e. predicted earnings in the absence of a child at event time t. The percentage effect is thus defined as $\hat{P}^g_t = \hat{\beta}^g_j / E[\hat{Y}^g_{ist}|t]$ with $\hat{Y}^g_{ist} = \hat{\alpha} + \hat{\gamma}^g_{age} + \hat{\delta}^g_s + \hat{\mu}^g_{married}$. Due to this nonlinear transformation of the parameter estimates, we apply a non-parametric bootstrap to get consistent standard errors for $\hat{P}^g_t$. For identification, this event study approach relies on the assumption that average earnings would develop smoothly in the absence of children (Kleven et al., 2019a).

In addition to estimating the child penalty, we aim to identify the differential effect depending on whether there is childcare provision in the municipality of residence in the year of birth of the first child. To this end, we exploit the staggered introduction of childcare facilities across the municipalities in the canton of Bern.[8] While the introduction of childcare in a given municipality is likely endogenous to demand to some extent, we assume that its precise timing is not. We validate this assumption by comparing child penalties in municipalities opening a childcare facility in our observational window to child penalties in municipalities which do not have a childcare facility by the end of the time window we study. Our results show, that in the absence of childcare, their child penalties are comparable. This alleviates the concern that individuals living in municipalities opening childcare facilities earlier systematically differ from individuals living in municipalities with no childcare facilities. Furthermore, we focus on municipalities which

[7]By construction, the individual specific effects also capture municipality of child birth fixed effects, thus controlling for level differences between municipalities.

[8]A federal impulse program provided financial support for the expansion in childcare facilities during our sample period. Starting in 2003, the federal government provided funding to public and private entities to cover the fixed costs of opening new facilities if they could document that there was sufficient demand.

did not have childcare at the beginning of our sample period, but introduced it at some point within our observational window.This strategy allows us to identify the effect of childcare on the child penalty under the assumptions that individuals do not change their fertility decisions in anticipation of the introduction of childcare, and that there is no spatial selection of couples with a preference for continuing their employment at a high level into municipalities offering childcare. We validate the robustness of our results with respect to selective fertility and spatial selection in Sections 5.3 and 5.4.

In order to study whether the overall child penalty depends on the availability of childcare and to estimate the differential effect, we interact the event time dummies of our baseline model with the indicator $Care_i$, which assumes value 1 if there is a childcare facility in the municipality of residence in the year of birth of the first child, and 0 otherwise. This results in the specification

$$\begin{aligned} Y^g_{ist} = \alpha + \sum_{j=-3}^{6} \beta^g_j I[j=t] + \sum_{j\neq -1} \rho^g_j I[j=t] \times Care_i \\ + \gamma^g_{age} + \delta^g_s + \mu^g_{\text{married}} + \nu^g_i + u^g_{ist}, \end{aligned} \tag{2}$$

where ρ^g_j captures the difference in child penalties if in the year of birth of the first child there is a childcare facility in the municipality of residence, and thus measures the differential effect of childcare. The percentage effect is defined as $\Delta\hat{P}^g_t = \hat{\rho}^g_j / E[\hat{Y}^g_{ist}|t]$. All other variables are defined as above. As before, we use a non-parametric bootstrap to calculate consistent standard errors.

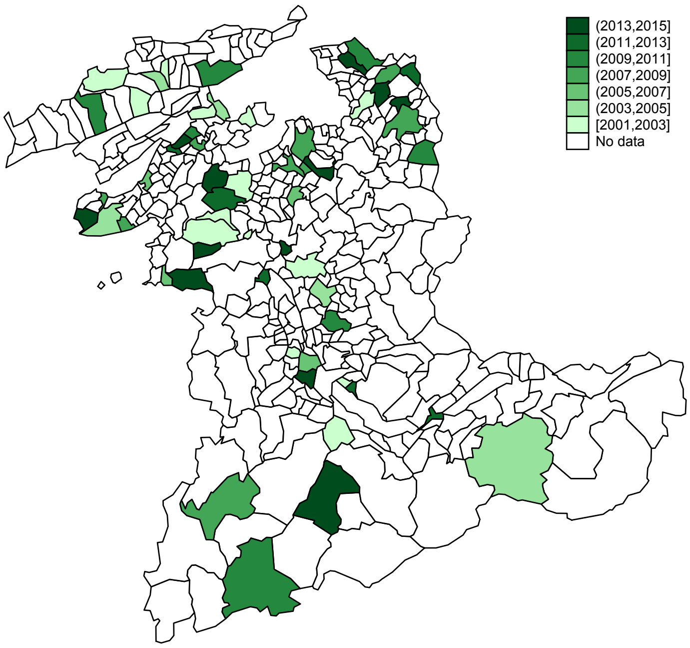
3 Data

Municipality level childcare data

We complement the administrative individual data with self-collected information on the availability of childcare in the municipality of residence.

In 2001, there were 401 municipalities in the canton of Bern. Provision of childcare varies substantially across municipalities. The office of public education of the canton of Bern ("Erziehungsdirektion Bern") provided us with a list of childcare facilities for the year 2018. We then contacted each of these facilities via mail and/or phone individually to inquire the year in which they opened and whether there were any comparable services in the municipality before that date. The covered time span is marked by a considerable expansion of childcare availability in Bern. While there were only 26 out of 401 municipalities (6.4% of municipalities, covering around 35.3% of the canton's population) offering childcare before 2001, there were 97 (24% of municipalities covering 70.2% of the canton's population) in 2015 (at the end of our sample period). We managed to collect the opening year of the first childcare facility for 59 of the 71 municipalities which opened a childcare facility between 2001 and 2015. These cover around 19.0% of the canton's population.[9] The treatment year is defined as the year in which the first facility opened. Figure 1 visualizes the location and the childcare opening year of these 59 municipalities within the canton of Bern.

Figure 1: Municipalities in Bern by opening year of their first childcare facility



Note: This map visualizes the municipalities of the canton of Bern. The 59 municipalities that opened their first childcare facility between 2001 and 2015 are marked in green. The later the year in which the first childcare facility opened, the darker the color. "No data" indicates municipalities that introduced childcare before 2001, after 2015, not yet at all, or for which we were not able to determine the exact opening year.

In the canton of Bern, there are both facilities with subsidized childcare places and facilities without. The main distinction between the two is that facilities with subsidized childcare places are subject to additional regulation concerning their pricing and prioritization of children.[10] Prices for subsidized childcare are set at the cantonal level and depend on parents' income, wealth, and family size, with a minimum price of CHF 0.77 per hour and a maximum price of CHF 12.15 per hour in 2018.[11] Additionally, for subsidized places, priority is given to families relying on two incomes and families requiring

[9]In the remaining 12 municipalities, there was at least one facility which we were not able to reach or that refused to communicate their opening year. As we are not able to define whether there was a childcare facility at the time of birth of the first child, we exclude these municipalities from our sample.

[10]Regulations for facilities with subsidized care are available at https://www.belex.sites.be.ch/frontend/versions/1620 and regulations for facilities with no subsidized care at https://www.belex.sites.be.ch/frontend/versions/1223. This information is available in German and French.

[11]1 Swiss franc corresponded to $1.10 on August 6, 2020.

childcare due to their social situation or for integrative purposes.[12]

Individual data and sample

Our analysis draws on administrative tax data for the Swiss canton of Bern. With a population of about 1 million, the canton of Bern represents approximately one-eighth of the total population of Switzerland and includes both urban centers and rural municipalities, some of which are located in alpine regions. Around 11 percent of the population live in municipalities that are French speaking, whereas in the majority of municipalities German is the official language. This makes the canton of Bern a good mirror of Switzerland as a whole. The tax data covers all individuals paying taxes in the canton of Bern, i.e., all individuals who live in the canton of Bern on 31 December of a year and are aged 18 or older.[13]

We draw on data for the years 2001 to 2015 and are able to track individuals over time as long as they do not leave the canton. [14]

To identify the birth year of the first child, we need to restrict our sample to parents who are married in at least one year during our observational period.[15] Relying on marriage allows us to identify both parents under reasonable assumptions. This restriction is necessary because if parents are not married, either of them can file for the deduction.[16] We define the birth year of an individual's first child as the year in which he or she first files child deductions in their tax return.[17]

While we do not require our panel data to be balanced, we exclude individuals with missing information between the first and the last year they appear in our data.[18]

In total, of the 340,233 ever-married couples in our sample, we identify 44,008 couples that had their first child in our observational period (2002 to 2015). They are associated with 2.15 million couple-year observations. Since 2005, there have been 14 weeks of paid

[12] A two parent family with one child, no wealth, and a net income of CHF 46,800 would pay the minimum price. The price is decreasing in the number of children in the household and increasing in family wealth and income. The maximum price is applicable to a two parent family with one child, no wealth, and a net income of CHF 163,800.

[13] Individuals under the age of 18 with considerable earnings also pay taxes. This is, however, quite rare and, as explained below, we exclude individuals below the age of 20 from our sample.

[14] Even though Swiss law treats married couples as one entity, the data allow us to distinguish between labor earnings contributed by each spouse. In the year a couple get married or divorced, e.g. there are multiple entries, one for the time period the couple are taxed jointly and one for the time period they are taxed individually. In those cases, we aggregate the entries to determine total earnings for that year.

[15] Given that at least 76 percent of individuals who have children in our sample are married at some point, this is not a restrictive sample definition.

[16] We have separate identifiers for both spouses in couples, which the tax authority continues to use before and after individuals are married. We exclude same-sex couples, who can also file their taxes jointly. We also exclude individuals who were married to different spouses during our sample period because this makes it harder to identify both parents.

[17] Note that this procedure does not allow us to distinguish between biological children and adoptions. Using data from Denmark, however, research by Kleven et al. (2020a) suggests that the child penalty is the same for biological and adopted children.

[18] Observations for some years in between the first time an individual is observed and the final year the individual is observed may be missing if an individual does not continuously live in the same canton. Our data does not allow us to follow individuals across cantonal borders.

maternity leave in Switzerland. As benefits received during maternity leave depend on pre-child earnings, the introduction of paid maternity leave may not only affect outcomes after the birth of a child but potentially also maternal earnings before giving birth. To avoid any bias induced by the introduction of paid maternity leave, we further restrict the sample to parents of children born after 2004. This reduces the sample to 32,636 couples and 427,740 couple-year observations.

We restrict the estimation sample along several dimensions. We keep couples that we observe during at least five consecutive years between two years before and two years after the birth of the first child. This reduces the sample size to 21,277 couples, associated with with 236,873 couple-year observations. We further restrict our sample to mixed sex couples where both partners are aged 20 to 45. This results in 19,774 couples with 206,021 million couple-year observations.We only include couples where neither spouse declares self-employed earnings, which reduces the sample to 15,747 couples associated with 162,155 couple-year observations.[19] We further exclude couples where either spouse relocates in the year prior to or after the birth of their first child, which reduces the sample to 11,086 couples associated with 116,395 couple-year observations. [20] Finally, we exclude all couples where either partner was not employed during the three years before the birth of the first child. In the end, our baseline sample includes 10,372 couples associated with 112,500 couple-year observations.

Of the 10,372 couples in this baseline sample, there are 4,721 who, in the year of birth of their first child, live in municipalities which either have no childcare facilities or which have facilities but were not able to provide information on opening dates. Another 3,692 live in municipalities that offer childcare, but opened their facilities before 2001. The exclusion of these cases leaves us with a sample of 1,959 couples, associated with 21,437 couple-year observations, who, in the year of birth of their first child, live in one of the 59 municipalities opening a childcare facility between 2001 and 2015. Descriptive statistics for this sample, as well as for the sample of couples living in municipalities with no childcare facilities as of 2015 and for couples living in municipalities which open a childcare facility before 2001, can be found in Appendix Tables A.1 and A.2. We discuss the differences in the characteristics of individuals in our main sample and individuals in municipalities introducing childcare early. While they are partly statistically significant, they can largely be explained by the fact that the urban centers introduce childcare early. Furthermore, the differences are quantitatively limited. Our sample thus seems to be rather representative for municipalities in Bern.

Our main outcome of interest are total earnings. We additionally also also study the extensive margin of employment. We consider an individual to be employed if their earnings are positive in a given year.

[19]We exclude couples with earnings from self-employment because tax considerations might incentivize self-employed to shift earnings across years and between spouses.

[20]Restricting to non-movers alleviates concerns of selective mobility. We discuss this point in more detail in Section 5.4.

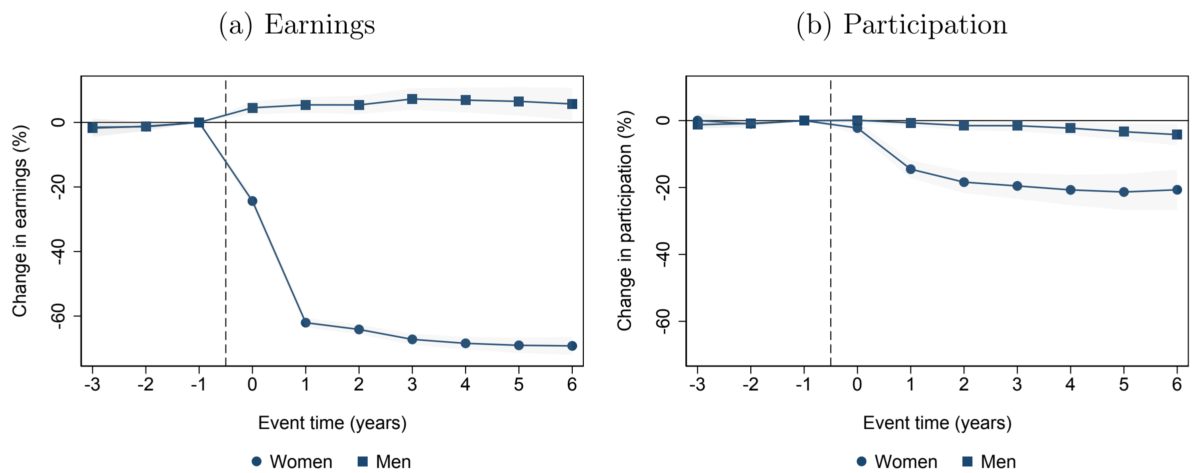
4 Results

We present our empirical analysis in four steps. In a first step, we show how the birth of a first child affects female and male earnings and participation rates. This allows us to compare the size of the estimated penalty with previous studies. In a second step, we turn to the impact of the presence of childcare in the year the first child is born on parental earnings and participation rates. Here, we additionally evaluate the differential impact of subsidized childcare vs. non-subsidized childcare. In a third step, we study the heterogeneity of the effect depending on household earnings, and finally, we analyze the overall effect on total household earnings and female earnings shares.

4.1 Assessing the child penalty

We start by estimating the earnings and participation effects of becoming a parent. As described above, we condition our estimates on time fixed effects, life-cycle effects (age effects), and individual fixed effects (see Equation 1 for more details). Figure 2 presents the results for men and women and for our two main outcomes, individuals' earnings (total effect) and labor market participation (extensive margin).

Figure 2: Effect of parenthood on earnings and labor market participation



Note: Percentage effect of parenthood on earnings and participation relative to the year prior to the birth of the first child ($t = -1$) for women and men. Confidence bounds are calculated applying a non-parametric bootstrap and are reported at the 5 percent level. The corresponding estimates can be found in Appendix A, column (1) of Tables A.3 (earnings) and A.5 (participation) for women, and column (1) of Tables A.4 (earnings) and A.6 (participation) for men.

The left-hand panel of Figure 2 visualizes the effect of parenthood on earnings and the right-hand panel on individuals' labor market participation for both women and men. Before the birth of their first child, earnings and participation patterns of men and women evolve in parallel. After the birth of their first child, earnings diverge substantially. The earnings of mothers drop by 62 percent between the year prior to childbirth and the year after.[21] The earnings of men, in contrast, display a slight increase of 5.4 percent in the year after childbirth.[22] Starting in year 2 after childbirth, the earnings of women and men start evolving in a roughly parallel fashion again. Six years or more after childbirth, we observe a decrease in women's earnings of 69 percent and an increase in men's earnings of 6 percent, which add up to a total child penalty of 75 percent. The participation rate of women initially drops by about 15 percent and by 21 percent in the long term. About 30 percent of the drop in women's earnings due to parenthood can be explained by participation in the labor market. The participation rate of men, in contrast, remains virtually unaffected by parenthood. It falls by about 4 percent. This is consistent with studies for other countries.[23]

The earnings penalty we document for Swiss women is comparable to the one documented for Germany (for numbers on Germany and 5 other countries, see Kleven et al., 2019b). However, while female participation rates in Germany are a major factor in explaining the earnings gap, with a reduction of 40 percent six years after the birth of the first child, female participation rates in Switzerland only decrease by 20 percent in the long run and remain stable after year three following the birth of the first child.

4.2 The impact of childcare

We next turn to evaluating whether the availability of childcare facilities in the year the first child is born impacts mothers' (and fathers') labor market attachment and helps reduce the child penalty. As described above, we estimate a fully interacted event study using an indicator set to one if there is childcare available in the year of birth of the first child.

The upper panel of Figure 3 shows the effect of the birth of the first child on earnings, while the lower panel shows the effect on participation rates. As described before, we consider municipalities opening their first childcare facility between 2001 and 2015. Our strategy is to compare parents who had their first child after a childcare facility opened with parents who had their first child before. Again, we see that before the birth of their

[21] In the year of birth ($t = 0$), earnings include maternity leave compensation, which corresponds to 80 percent of earnings in the year prior to the birth of the child (with a maximum of CHF 196) per day. Paid maternity leave is 14 weeks. There is no paternity leave in Switzerland.

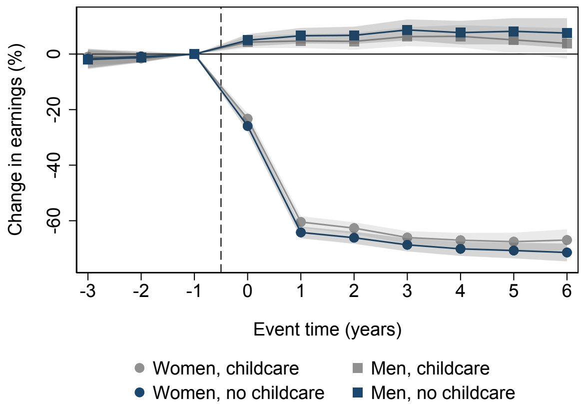
[22] This effect is to some extent mechanical. With the birth of the first child, employees receive child benefits as part of their earnings. Child benefits are due to the parent with higher earnings, which is usually the man. Statutory child benefits are approximately CHF 2,760 per child and year (rates may vary slightly from year to year). Many employers grant additional benefits which are often of similar magnitude for the first child, however, with the total amount of this additional benefit decreasing in the number of children. For employees of the canton, e.g., the total annual child allowance for the first child amounts to CHF 5,760, CHF 7,680 for two children, CHF 9,600 for three children, CHF 11,520 for four children, and 2,760 times the number of children for five and more children. Considering that within sample mean earnings of men at $t = -1$ are CHF 69,284, the child benefits for the first child correspond to an increase in mean earnings of 8.3 percent.

[23] The effect of parenthood on women's and men's earnings is rather similar for the other subgroups of municipalities, i.e., the municipalities that either open childcare facilities before 2001 or municipalities with no childcare facilities as of 2015. These results are discussed in Section 5.5.

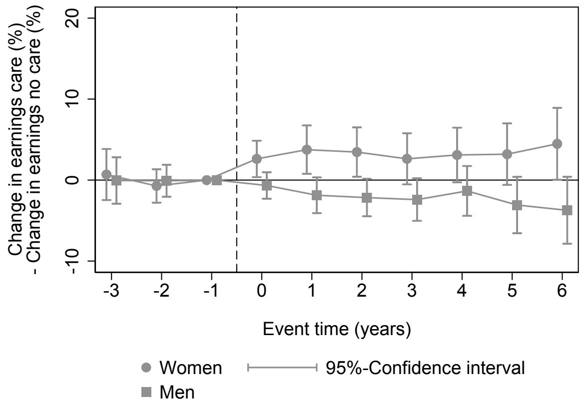
Figure 3: Effect of having a childcare facility in the year of birth of the first child

Outcome: Earnings

(a) Effect of parenthood

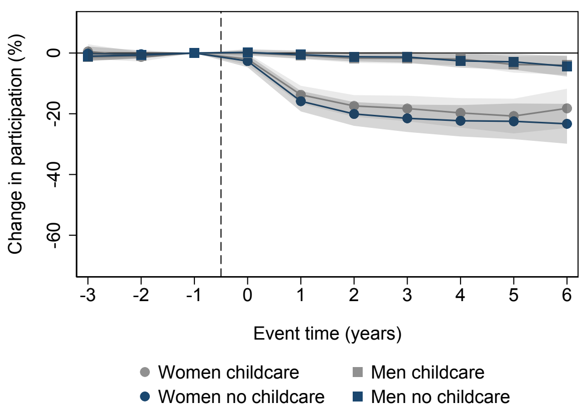


(b) Effect of childcare

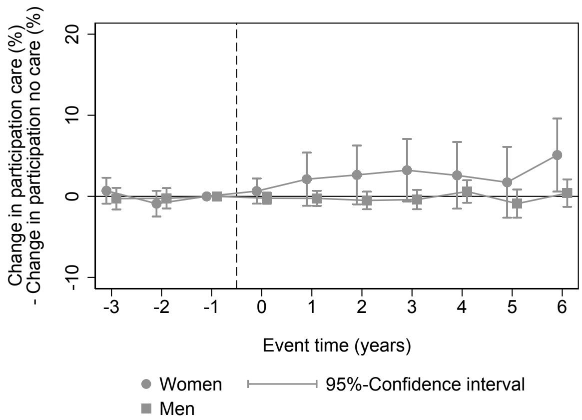


Outcome: Participation

(a) Effect of parenthood



(b) Effect of childcare



Note: These graphs show the effect of having a childcare facility in the municipality of residence in the year of birth of the first child. Panels (a) show the percentage effect of parenthood on earnings and the participation rate relative to the year prior to the birth of the first child ($t = -1$) for women and men. Panels (b) display the difference in female and male earnings by the presence of childcare (ρ_j^g). The confidence bounds are calculated applying a non-parametric bootstrap and are displayed at the 5 percent level. The corresponding estimates can be found in Appendix A, column (2) of Tables A.3 (earnings) and A.5 (participation) for women, and column (2) of Tables A.4 (earnings) and A.6 (participation) for men.

first child, the earnings of men and women evolve in parallel. Additionally, we see that before the birth of the first child, earnings of women and men, who at the time of birth live in a municipality with childcare and individuals who do not, evolve in parallel. After the birth of the first child, female earnings decrease by less if there is childcare compared to if there is not. The right hand panel shows this difference more clearly by plotting only the difference in the child penalties of individuals in municipalities with childcare at the time of birth compared to individuals in municipalities without. In the long run, female earnings are approximately 4.5 percentage points higher if there is childcare in their municipality of residence in the year of birth of their first child. In other words, childcare reduces the female child penalty by 6.3 percent. The difference is significant at the 5 percent level. Male earnings are approximately 3.7 percentage points lower in the long run if there is childcare in their municipality of residence in the year of birth of their first child. This result is intuitive given that, with the positive effect of childcare on female earnings, men do not have to increase their earnings by the same amount after the birth a child to, at least partly, make up for lost partner earnings. This difference is significant at the 10 percent level.

The bottom panel of Figure 3 reveals that childcare does not affect men's labor market participation. In the short run, there is no clear effect on female labor market participation either. Though less precisely estimated, there is a long term effect. The presence of childcare at the time of birth increases female participation by 5.1 percentage points six or more years after childbirth, corresponding to a 22 percent reduction in the drop of labor market participation for women we observe after the birth of the first child. This implies that participation fully accounts for the effect of childcare on the female child penalty when a woman's first child is in school. In the year of birth itself, as well as in the five subsequent years, however, the effect on participation is never statistically significant and the point estimate is mostly smaller than the point estimate on the total effect. This suggests that childcare allows mothers of young children to increase their workload through part-time work arrangements, which are common in Switzerland.[24]

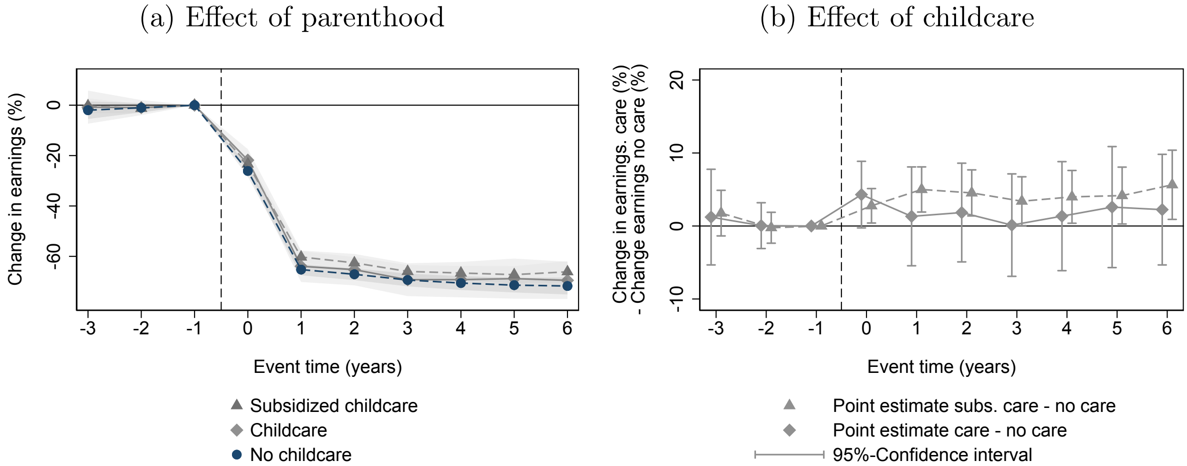
The role of subsidized childcare

The above results indicate that the presence of childcare increases earnings of women after the birth of the first child (reduces the child penalty). This raises the question whether the increase is driven by a pure availability effect or whether it is driven by a price effect. Subsidized childcare creates an incentive to substitute away from home care or more expensive private arrangements and to paid employment. In order to study this mechanism, we gather information on whether there is subsidized childcare in the municipality. Subsidies depend on parental income and can reduce hourly prices by more than 90 percent.

We are able to distinguish between the introduction of non-subsidized and subsidized care because 11 of the 59 municipalities which introduced childcare started offering financial support on average 4.5 years later. 353 of the 1,959 couples in our sample live in those 11 municipalities. In order to estimate the differential impact, we, technically, include an additional term in our estimation equation. We interact the event time dummies with a

[24]Appendix Figure A.3 and Appendix Tables A.15 and A.16 show results using the same treatment group, but a broader control group. Recall that in the baseline, the control group consists of parents who live in a municipality introducing childcare between 2001 and 2015 and have their first child before the introduction of childcare, but after 2004. In this alternative specification, we add parents who live in a municipality that did not have childcare by the end of 2015, but who had their first child between 2005 and 2015. The point estimates are robust, but precision improves in this alternative specification.

Figure 4: Effect of having subsidized childcare in the year of birth of the first child for women



Note: Effect of having a childcare facility in the municipality of residence in the year of birth of the first child, distinguishing between subsidized and non-subsidized care. Panel (a) shows the percentage effect of parenthood on earnings relative to the year prior to the birth of the first child ($t = -1$) for women. Panel (b) displays the estimated effect of childcare on the child penalty for women. The solid line displays the effect for non-subsidized care and the dashed line for subsidized care. Confidence bounds are calculated applying a non-parametric bootstrap and are displayed at the 5 percent level. The corresponding estimates can be found in column (5) of Table A.3 in Appendix A. The corresponding estimates for men can be found in column (5) of Table A.4 in Appendix A.

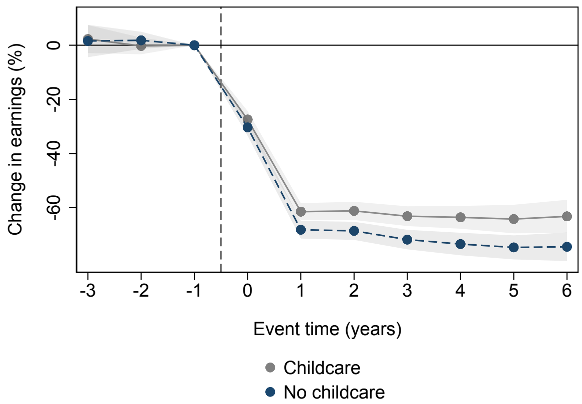
childcare indicator and an indicator for whether childcare is subsidized, as described in Equation 2. The left hand side of Figure 4 presents how female earnings change after the birth of the first child. We distinguish between mothers living in a municipality with no childcare at the time of birth of their first child, mothers living in a municipality with childcare but no subsidies, and mothers in a municipality with subsidized childcare. The right hand side shows the differences between no childcare and non-subsidized childcare (solid), and no childcare and subsidized childcare (dashed).

While these results are based on rather few observations in municipalities switching from non-subsidized to subsidized childcare, the results indicate that financial support amplifies the abating effect of childcare on the child penalty. The introduction of subsidized childcare increases maternal earnings by 7.8 percent compared to no childcare, which is displayed by the dashed line, or by one-tenth of the total penalty of mothers in municipalities without childcare. The effect of childcare without financial support is still positive, albeit small and not significantly different from no childcare, as shown by the solid line in the right panel of Figure 4. We do not find a differential effect for participation rates. The corresponding results can be found in column (5) Appendix Table A.5. These results suggest that subsidies play a complementary role in childcare provision and without subsidies, reductions in the child penalty may be considerably smaller. The

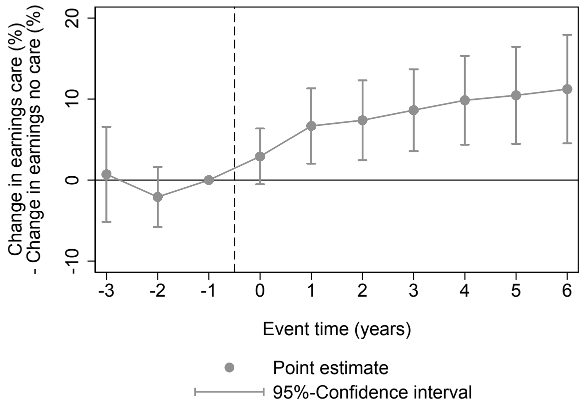
Figure 5: Effect of childcare for women by pre-child household earnings.

Outcome: Household earnings below sample median at t=-1

(a) Effect of parenthood

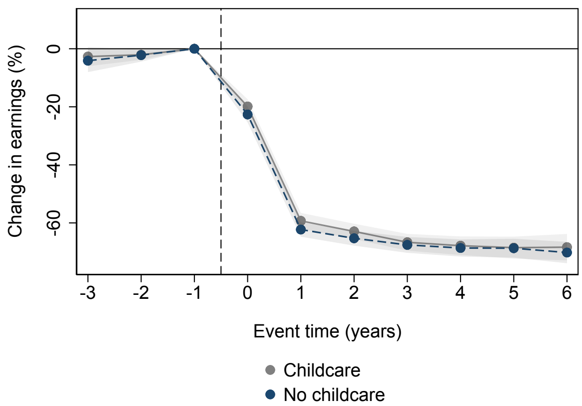


(b) Effect of childcare

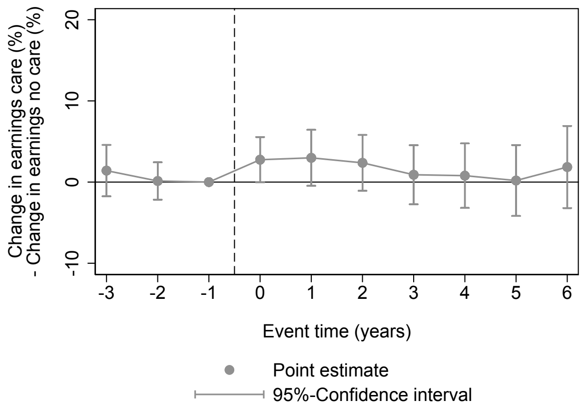


Outcome: Household earnings above sample median at t=-1

(a) Effect of parenthood



(b) Effect of childcare



Note: Effect of having a childcare facilities in the municipality of residence in the year of birth of the first child on female earnings, by pre-child household earnings. The top panels show the effect for households with pre-child earnings below the sample median, the bottom panels for households with pre-child earnings above the sample median. The panels on the left show the percentage effect of parenthood on female earnings relative to the year prior to the birth of the first child ($t = -1$). The panels on the right display the effect estimates of childcare. Confidence bounds are calculated at the 5 percent level of significance applying a non-parametric bootstrap. The corresponding estimates can be found in column (3) and column (4) of Table A.3 in Appendix A. The corresponding estimates for men can be found in column (3) and column (4) of Table A.4 in Appendix A.

corresponding results for men also suggest that the childcare effect is driven by subsidized care (see column (5) Appendix Table A.6).

Heterogeneity by household earnings

Given the importance of subsidized childcare, one might expect to see a stronger effect for families with lower earnings (household earnings below or equal to sample median at $t = -1$) than for families with higher earnings (household earnings above sample median at $t = -1$). Figure 5 shows the estimates of the female child penalty and the effect of childcare for families with pre-child earnings below the sample median and families with pre-child earnings above the sample median. Panels (a) show that the long-term child penalty for women is comparable for both groups (around 70 percent of earnings). Panels (b) visualize the estimated effect of childcare for women by earnings group. In the presence of childcare, long-term maternal earnings are approximately 11.2 percentage points higher in lower-earnings families. Childcare thus reduces the female child penalty in lower-earnings families by about 15 percent. As shown in Appendix Table A.4, the documented increase in male earnings after the birth of the first child is also purely driven by lower-earnings households. In the absence of childcare, earnings of men in lower-earnings households increase by about 20 percent. Childcare reduces this increase in earnings of men in lower-earnings families by about 8.4 percentage points or by 38 percent, suggesting that childcare reduces the burden on men in lower-earnings families to make up for a loss in female earnings. The effect of childcare among higher-earnings families is negligible for both women and men, and not statistically significant in most event time periods.

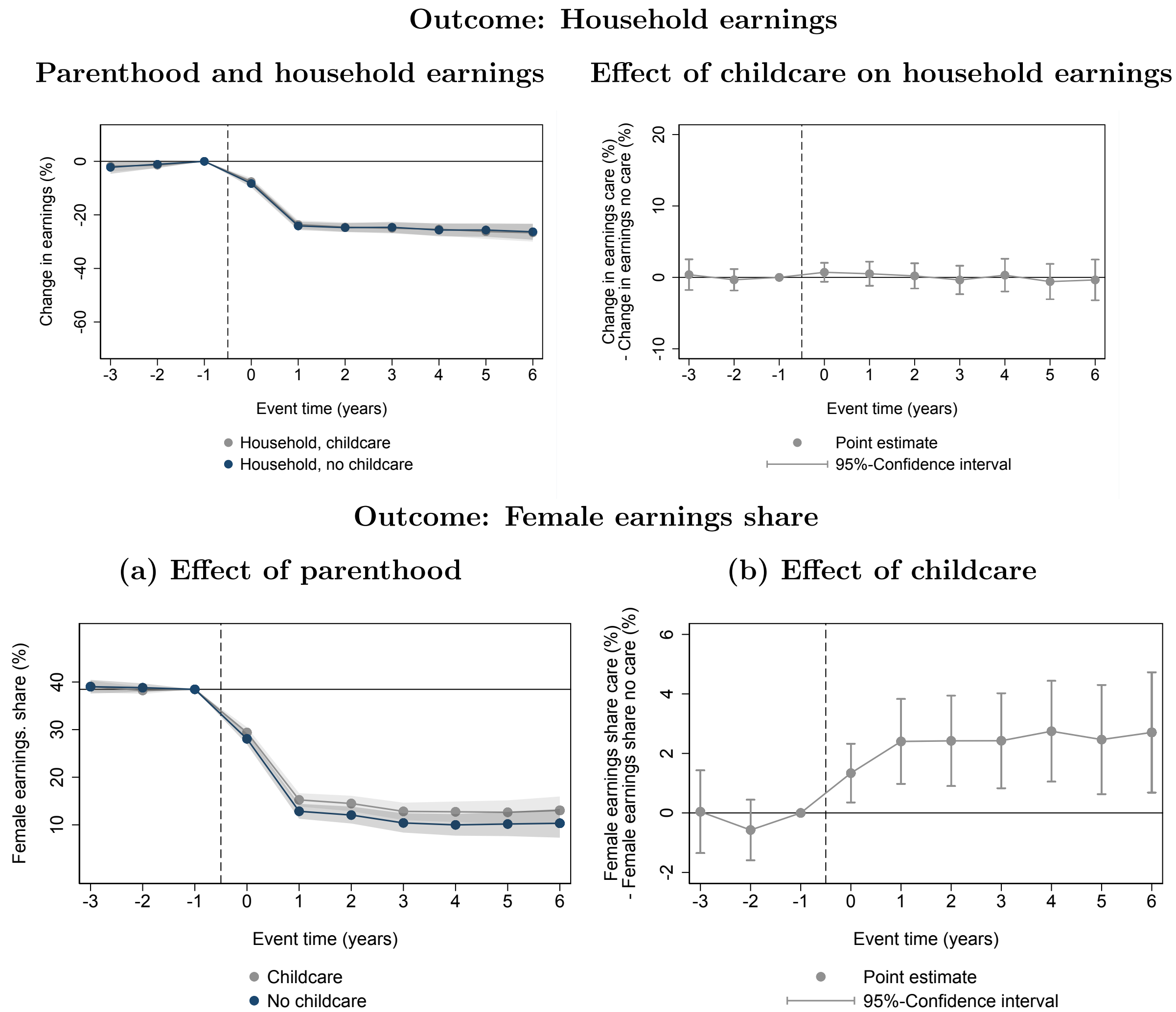
The results for the labor force participation are in line with these findings. We find a somewhat less precisely estimated but positive long term impact for women lower-earnings households and no effect for higher-earnings households. The increase in the long term participation rate for lower-earnings households amounts to a substantial (37 percent) reduction in the participation drop in the absence of childcare. The corresponding estimates can be found in column (3) and column (4) of Table A.3 in Appendix A. The corresponding estimates for men can be found in column (3) and column (4) of Table A.4 in Appendix A.

Our results confirm that the effect of childcare is indeed stronger among lower-earnings families than among higher-earnings families. The effects on mothers' and fathers' earnings add up to a reduction of the total child penalty in lower-earning families by 20 percent.

Childcare and earnings at the household level

One policy goal of introducing childcare might be to reduce the drop in household earnings associated with the birth of a first child. As shown above, the presence of childcare at the time of birth of the first child increases female earnings and decreases male earnings compared to a scenario with no childcare. Both of these effects contribute to a reduction in the child penalty defined as the child-induced increase in the gender pay gap. If and how these two effects impact total household earnings, is a priori unclear.

Figure 6: Effect of childcare on household earnings and the female earnings share



Note: This analysis follows a similar specification as the analyses in Figures 3 and 5, but with couple's total labor earnings and the female earnings share as dependent variable. Effect of having a childcare facilities in the municipality of residence in the year of birth of the first child. The top panels show the effect on household income, the bottom panels the effect on the female earnings share. The panels on the left show the percentage effect of parenthood relative to the year prior to the birth of the first child ($t = -1$). The panels on the right display the differential effect of childcare on household earnings and the female earnings share respectively. The corresponding estimates can be found in Tables A.7 and A.10 respectively. Confidence bounds are calculated at the 5 percent level of significance applying a non-parametric bootstrap.

We present results on the development of total household earnings around the birth of the first child in the first row of Figure 6. On average, household earnings drop by more than 20 percent relative to the year before the child is born. We find no evidence for a systematic impact of childcare availability on total household earnings. While the effect of childcare is somewhat positive in the lower-earnings sample, it is small and not statistically significant at conventional levels (see Appendix Table A.7). The second row in Figure 6 shows the result of running an event study on female earnings shares as the dependent variable. We find that with childcare women's contribution to household earnings increases by 2.7 percentage points on average. The increase is more distinct in lower-earnings households where it amounts to 5.1 percentage points (see Table A.10 in the Appendix). This leads to the conclusion that while availability of childcare impacts relative earnings within the household, with women earning more and men earning less than in the absence of childcare, it does not impact overall earnings. This finding might of course be specific to high income countries like Switzerland.

5 Discussion

The results in Section 4 provide evidence for a positive impact of childcare on female earnings. In what follows, we first validate our claim that the availability of childcare in the year of birth of the first child is most relevant for female earnings trajectories. We then explore a number of alternative mechanisms that could potentially bias our results. In these robustness checks, we mainly focus on the impact of childcare on the child penalty of women. We validate that i) the exploited treatment is effective in increasing take-up of childcare, ii) individuals do not change their fertility decision in anticipation of the introduction of childcare, iii) selective mobility is unlikely to drive our results, iv) municipalities opening childcare facilities between 2001 and 2015 do not systematically differ from other municipalities in the canton of Bern with respect to the impact of children on female earnings, v) our results are not driven by differential time trends in municipalities introducing childcare, and vi) our estimation approach does not yield a significant effect in case of placebo introduction dates of childcare facilities. In a final step, we briefly discuss the role of gender norms as a driving force of the child penalty.

5.1 Childcare availability in the year of birth as most influential period

In our main specification, we define the treatment as the availability of childcare in the year of the birth of the first child. The treatment indicator is thus set to one if a childcare facility opens in the municipality of residence either in the exact year of birth or prior to that. This specification is motivated by our reasoning that parents make decisions on employment and care arrangements in the year of birth of their first child. The availability of childcare in the municipality of residence decreases direct as well as indirect costs for women to remain employed. If childcare facilities open after the birth of the first child, parents have already made alternative arrangements, i.e., they have either organized alternative childcare or reduced their work hours and the opening of a childcare facility is therefor no longer as salient.

In this section, we show that the availability of childcare in the year of birth of the first child is in fact the determining factor for parents' employment decisions. We re-estimate our baseline specification and re-define the treatment indicator to the introduction of childcare at different points in time around the birth of the first child. We distinguish between the effects of individuals whose fist child is born (i) after the opening of a childcare facility, (ii) in the same year as a childcare facility opens, (iii) in the 1 to 3 years before the opening of a childcare facility (pre-kindergarten), or (iv) in the 4 to 6 years before the opening of a childcare facility. Keep in mind that if the childcare facility opens after the birth of a child, we would expect to see an effect only from that event time forward and not directly after birth.[25]

We present the results of this analysis in Figure 7. There are systematic positive effects of childcare availability on female earnings of women who give birth to their first child after or in the same year a childcare facility is opened in their municipality of residence (blue dots and diamonds in panel (a) of Figure 7). The positive effects are largest if a childcare facility opens exactly in the year of birth of the first child. In the long term, these women, on average, earn up to 12 percentage points more. We do not see systematic effects for the case in which childcare facilities open between one and three years after birth, nor when they open in the four to six years after (gray squares and triangles in panel (a) of Figure 7).[26]

For men, as before, the effect in the overall sample is small. Consistent with the results for women, there seems to be a negative effect on men's earnings if childcare opens in the year of birth. This mirrors the fact that these mothers earn considerably more.

These results confirm that the age of the child at the point in time when formal childcare becomes available matters. While we see systematic effects of childcare availability on female earnings if childcare is available from the time the child is born, we do not find any systematic effects if childcare facilities open when the child is older. This result might be especially strong for the Swiss setting. Statutory maternity leave is restricted to 14 weeks. As a consequence, most women who want to stay home for longer will have to quit their job. Furthermore, it is easiest to find an available spot in a childcare facility when the facility just opened. This explains the fact that the opening just in the year of birth renders the largest effect.

5.2 Take-up

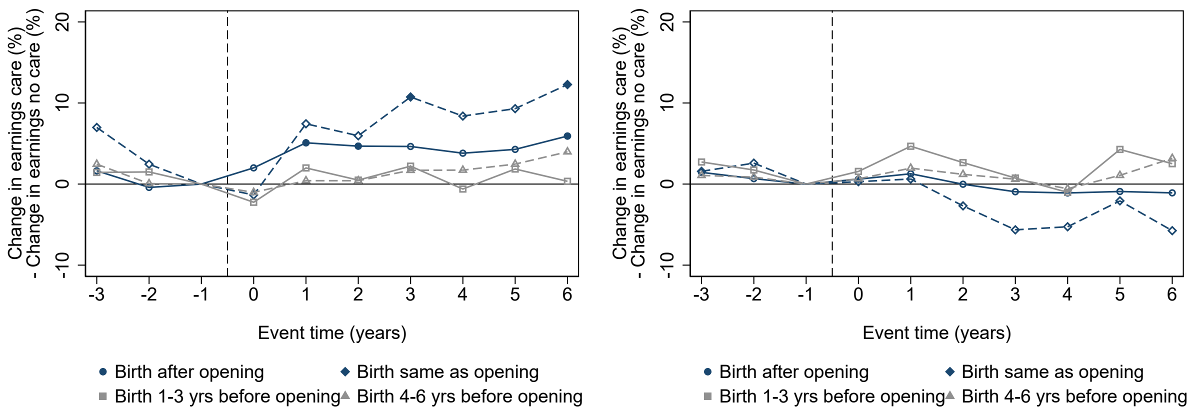
If our treatment indeed captures the increased availability of childcare, it should also affect take-up rates. While we cannot directly observe whether individuals in our sample have children enrolled in the facilities under consideration, the tax data include a variable on tax deductions for formal childcare arrangements. This provides us with information

[25] In this exercises our control group consists of individuals who do not have childcare in their municipality of residence until their first child is aged 7 or older. We add municipalities, that do not have childcare by 2015 to the sample to increase precision. Furthermore, for some treatment definitions, the sample of men below the age of 25 becomes very small. We therefore group the age effects of men at 25 for ages below 25 in this section.

[26] This conclusion also holds if we do not group the years around the year of the birth of the child. Using single years in the treatment definition renders similar results. The results of this additional analysis are available upon request.

Figure 7: Effect of childcare depending on the year childcare was introduced

(a) Effect of childcare for women **(b) Effect of childcare for men**



Note: Effect of having a childcare facilities in the municipality of residence on female (panel (a)) and male (panel (b)) earnings, depending on the year of the introduction of childcare relative to the year of birth of the first child. The panels show the effect of parenthood on female earnings relative to the year prior to the birth of the first child ($t = -1$) for four different definitions of the treatment indicator. The corresponding estimates can be found in Tables A.8 and A.9 in Appendix A. Filled markers indicate estimates that are significant at the 5% level.

on whether parents make use of any kind of formal childcare. If our findings are driven by increased availability, we should observe an increase in tax deductions filed for childcare in the municipalities which open a childcare facility.[27] While one might expect that the opening of childcare facilities induces some substitution away from privately organized care, overall we expect to see an increase in the claims of childcare deductions as there likely are households that previously could not afford childcare.

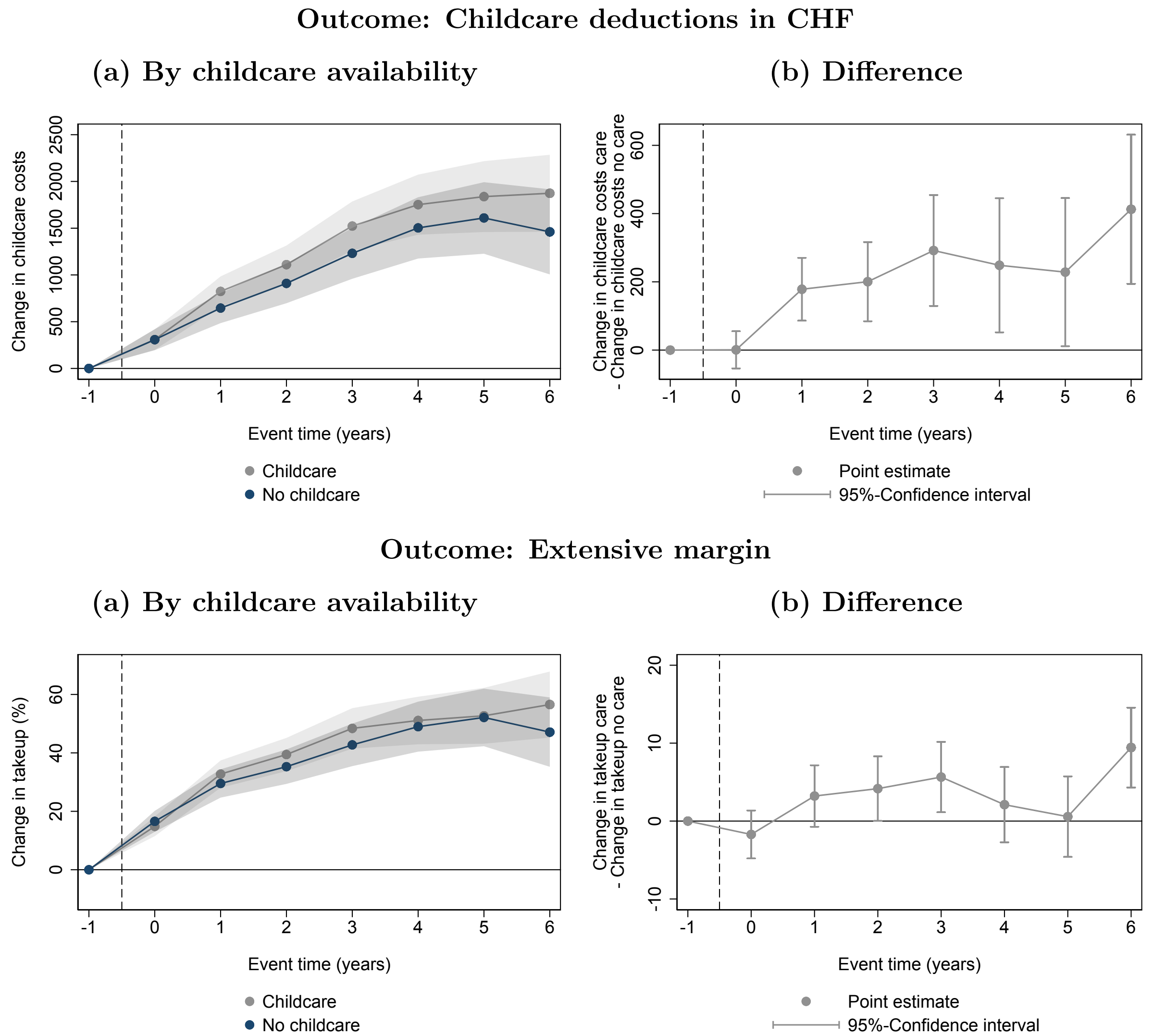
To verify that take-up does indeed increase, we estimate an event study model where we compare filings for childcare deductions of couples who, in the year of birth of their first child, live in a municipality with and without childcare. We use two outcomes. The first outcome is the total amount deducted for childcare.[28] The second outcome is an indicator variable set to one if a household files any childcare deductions, capturing the extensive margin. Our analysis of take-up is restricted in that we cannot compare pre-trends because filings for childcare deductions are naturally zero before the birth of the first child.

The upper panel of Figure 8 shows that parents' filings for childcare deductions in municipalities with childcare at the time of the birth of the first child exceed filings of

[27] We are not able to distinguish between types of childcare, namely whether it is claimed for care by nannies, by in-home daycare providers, or for institutional childcare. Nevertheless, if the effect on parental earnings is driven by the increase in childcare availability, we should see an increase in take-up rates of affected parents relative to parent who in the year of birth of their first child live in a municipality without childcare.

[28] Note that this likely presents a lower bound. There is an upper bound on filings for childcare deductions per child and year of CHF 1,500 until 2008, CHF 3,000 in 2009 and 2010, and CHF 3,100 from 2011 onward. Average childcare deductions filed are CHF 2,672.

Figure 8: Effect of childcare on take-up



Note: Effect of having a childcare facility in the municipality of residence in the year of birth of the first child on childcare take-up. The top panels display the effect on the amount of total childcare deductions filed in CHF. The bottom panels display the effect on the extensive margin (whether any childcare deductions are filed). The panels on the left show how filings for childcare deductions evolve by availability and event time starting with the year of birth ($t = 0$). The panels on the right display the difference in take-up between individuals in municipalities with and without childcare. The corresponding estimation results can be found in Table A.11 in the Appendix.

parents in municipalities without childcare six years after the first child by, on average, CHF 413, or 28 percent. The positive differential materializes already in the first year after childbirth and increases thereafter. While there is no immediate effect at the extensive margin. Six years after the birth of their first child, parents in municipalities with childcare are 9.4 percentage points more likely to file childcare deductions.

These findings suggest that the exploited treatment indeed increases childcare take-up. It is, however, likely that we measure a lower bound in this analysis as there likely is some substitution between private and institutional care.

5.3 Selective fertility

One potential source of bias is selective or delayed fertility. If the woman is more attached to the labor market, couples might, for example, wait to have a child until there is childcare in their municipality of residence. If selective fertility were an issue, we would expect to observe an increase in fertility after the opening of a childcare facility. We run an event study on the probability of child birth around the opening of a childcare facility to test whether such selective fertility is a concern. In this analysis, the event time does not relate to the birth year of a first child, but to the year in which a childcare facility opens in the respective municipality. Similar to the above analyses, we include all individuals aged 20-45 who live in a municipality which introduced childcare during the years 2001-2015. This time, however, we do not restrict ourselves to individuals who necessarily have children at some point as the dependent variable is an indicator for whether an individual has a child in the respective year.

We find no indication for a change in fertility after the opening of a childcare facility, as shown in Figure A.1 in the Appendix.[29] Selective fertility consequently appears unlikely to bias our results.

5.4 Selective mobility

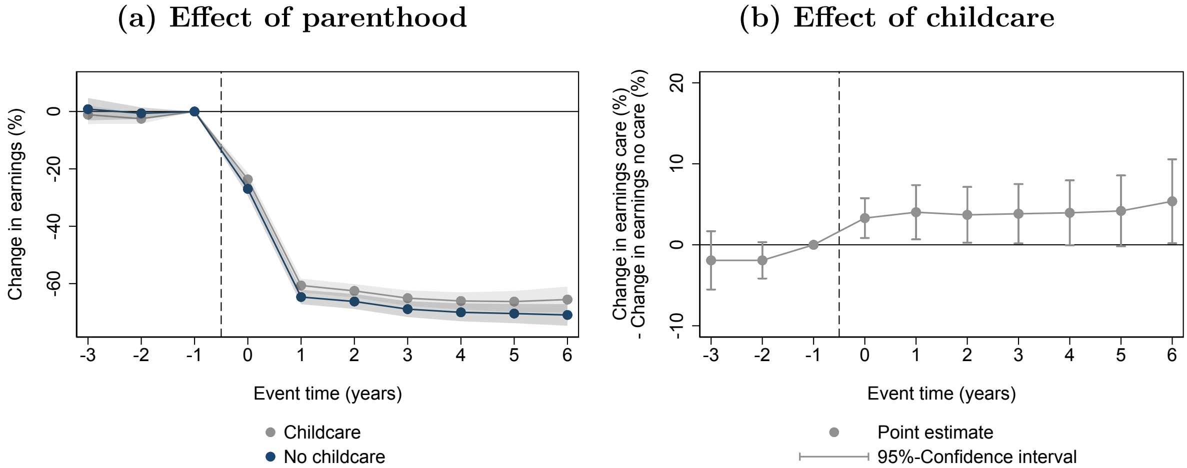
Selective mobility is another potential source of bias. If parents with a strong preference to continue formal employment are more likely to move to municipalities with childcare around the birth of their first child, the effect we observe could, at least partially, be driven by changes in population characteristics. Individuals who selectively move to municipalities opening a childcare facility could also be more likely to remain in formal employment. In this case we would overestimate the impact of childcare on earnings and employment.

For our main estimation sample, we only consider individuals who remain in the same municipality during the year prior to and the year after birth. To check for the robustness of our findings, we now restrict the sample to individuals staying in the same municipality during the two years prior to and two years after the birth of their first child, i.e., we only use individuals who stay in the same municipality before pregnancy and for longer after the birth of the child. This should exclude couples which have selectively moved to the municipality. This restriction reduces our sample by about 25 percent. If these excluded couples were to drive our effect, the reduced sample should show a weaker or no impact of childcare.

Figure 9 (b) presents results of the differential effect of childcare based on this restricted sample of 'long-term' stayers. If anything, the results show a slightly stronger effect of the introduction of childcare on mothers' earnings. While the raw female child

[29] Appendix Table A.12 also shows estimates for an alternative difference-in-differences specification, which includes individuals in municipalities that did not have childcare by 2015 as a control group. Again, we do not find any indication that the introduction of childcare had an impact on fertility decisions.

Figure 9: Effect of childcare for women who do not move between $t = -2$ and $t = 2$



Note: Percentage effect of parenthood on female earnings relative to the year prior to the birth of the first child ($t = -1$). The sample is restricted to individuals living in the same municipality between $t = -2$ and $t = 2$. Confidence bounds are calculated at the 5 percent level applying a non-parametric bootstrap. The corresponding estimates can be found in column (1) of Table A.13 in Appendix A.

penalty without childcare is comparable among the original and the stayer sample, we find that the introduction of childcare reduces the adverse impact of parenthood on female earnings of long-term stayers by about 5.4 percentage points compared to a reduction of 4.5 percentage points in the main sample. Again, the effect seems to be driven by subsidized childcare (see columns (1) and (2) of Appendix Table A.13). The results for men are hardly affected (see column (1) and (2) of Appendix Table A.14). We conclude, that selective moving behavior is unlikely to bias our results. If anything, the introduction of childcare seems to affect the existing resident population more.

5.5 External validity

For our main results, we only use the sample of municipalities within the canton of Bern which open a childcare facility between 2001 and 2015. So far, we have excluded all municipalities that either introduce childcare before 2001 or which have not introduced childcare facilities by the end of 2015. The sample of municipalities with no childcare by the end of 2015 can be used to test whether individuals who live in municipalities introducing childcare show different earnings patterns around the birth of a child than those in our main sample. Such differences might point to a systematic selection of the population in municipalities introducing childcare. Such selection might be a threat to the external validity of our results.

To test whether our results are likely to generalize, we test whether individuals residing in municipalities introducing childcare between 2001 and 2015 show similar child penalties in the years before the introduction of childcare as individuals in municipalities which have not introduced childcare by the end of 2015. To this end, we introduce an interaction term with an indicator ($care = 1$) set to one for municipalities which introduce childcare at some point. Using only years before the introduction of childcare, this comparison allows us to test whether the child penalty differs systematically between the two types of municipalities. If they are comparable before the introduction of childcare, it might be valid to expect that they would also be comparable in their response to the introduction of childcare.

We find no evidence that female earnings evolve differently around the birth of a first child in the two types of municipalities, as shown in Appendix Figure A.2. Mothers' earnings decrease by about 60 percent in the year of birth and the following year. Earnings patterns of individuals living in municipalities which do not have a childcare facility by the year 2015 and municipalities where a childcare facility opened between 2001 and 2015 do not differ before the opening of the childcare facility. We interpret this as evidence that our results likely generalize to other municipalities.

5.6 Differential time trends of municipalities introducing childcare

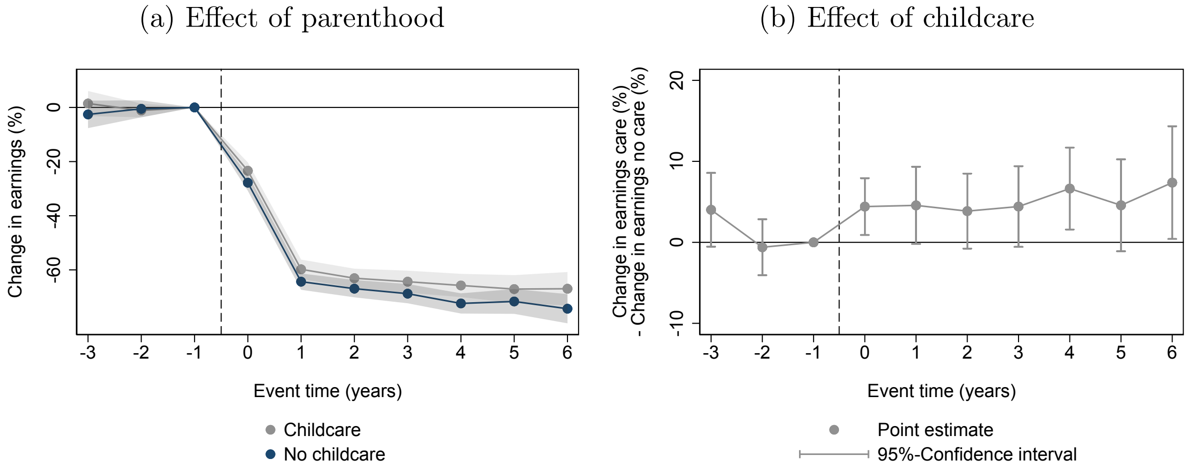
We have shown that there is no systematic difference in the child penalty between municipalities which introduce childcare at some point and municipalities which do not. Furthermore, based on pre-trends, we have shown that individuals who, in the birth year of their first child, live in a municipality with childcare do not differ from individuals who do not. In a last step, we now want to make sure that the results are not driven by differing time trends of municipalities opening a childcare facility compared to municipalities with no childcare facility. To do that, we only use parents of children who are born within a narrow window around the introduction of childcare facilities, i.e., three years around the opening of a childcare facility. As these children were born "narrowly" before and after the introduction, it is less likely that any difference is driven by,for example, an attitudinal change brought about by the introduction.

The restriction to children born within three years around the opening of a childcare facility reduces the sample by about 50 percent. This is reflected in the larger confidence bands in Figure 10. Still, we see that childcare decreases the child penalty of women by about 7 percentage points. The effect is significant at the 5 percent level for most event time periods. More importantly, the effect is of similar magnitude as the one in our main sample.

5.7 Placebo test

We perform a placebo test to address concerns that we may not capture the effect of childcare introductions but some trend, or that our fixed effects strategy may not be able to control for potential confounders. The placebo test confirms that our estimation approach does not lead to significant estimates when no effect should exist, i.e., when no childcare has been introduced. We use the sample of individuals living in municipalities

Figure 10: Effect of childcare for women and births within three years around facility opening



Note: Percentage effect of parenthood on female earnings relative to the year prior to the birth of the first child ($t = -1$) using only individuals whose first child is born within 3 years around the opening of the childcare facility. Confidence bounds are calculated at the 5 percent level applying a non-parametric bootstrap. The corresponding estimates can be found in column (4) of Table A.13 in Appendix A.

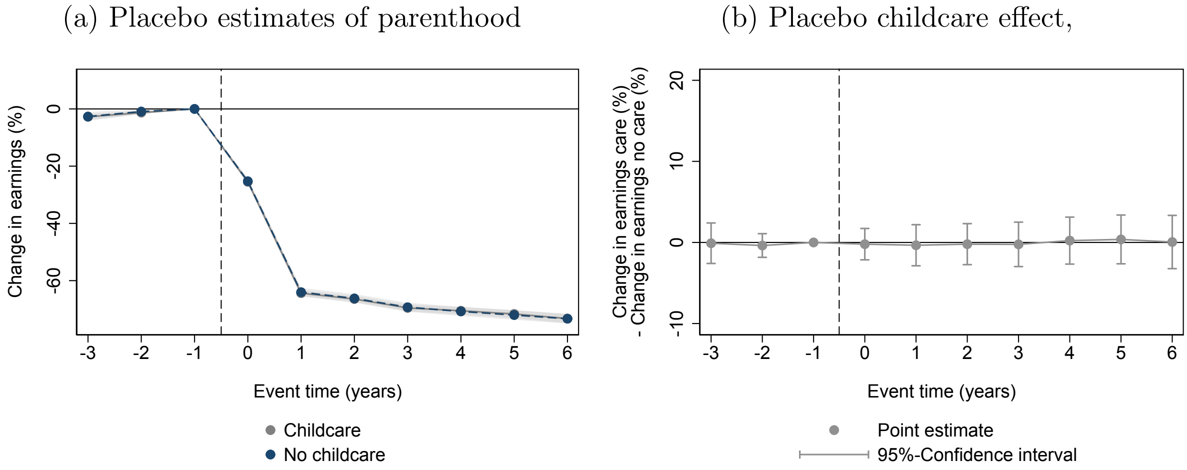
which do not have childcare facilities by the end of 2015 and randomly assign a childcare facility-opening year between 2001 and 2015 to each of the municipalities assuming a uniform distribution. We then apply Equation 2 to measure the effect of these placebo childcare facilities. Figure 11 displays the average result for 1,000 replications of the random assignment of opening dates.

We find no statistically significant difference between parents who, in the year of birth of their first child, live in a municipality randomly assigned with a childcare facility and parents living in a municipality without. This is clearly visible in panel (b) where we display the differential effect. The distributions of each of the event time estimates based on this random assignment is visualized in Figures A.4 and A.5 in the Appendix. In these graphs, we also compare the distribution of the placebo estimates to the point estimates in the upper right panel of Figure 3.

5.8 Female breadwinners and the child penalty

While we can show that the presence of childcare helps decrease the child penalty and increases female earnings shares, there is still a steep drop in female earnings after the birth of their first child, while male earnings show a slight increase. There are several arguments as to what drives the child penalty for women and why women's earnings respond more strongly than those of men. In this study, we demonstrate that formal institutions are an important determinant of the child penalty. While our findings suggest that childcare reduces the child penalty, childcare availability does in no way eliminate it.

Figure 11: Placebo estimates of childcare for women



Note: Placebo effect of childcare. Percentage effect of parenthood on female earnings relative to the year prior to the birth of the first child ($t = -1$) using only municipalities which do not have childcare facilities by the end of 2015 and randomly assigning opening dates for childcare facilities. Confidence bounds are drawn from the distributions of the placebo event time estimates. The corresponding estimates can be found in Figure A.4 in Appendix A.

The large remaining (unexplained) drop in female earnings after childbirth leaves room for additional explanations.

One popular argument is that the child penalty may emerge endogenously on efficiency grounds.[30] Assuming that, typically, at least one parent needs to reduce labor supply to take care of the child and given that men earn more, on average, it may seem reasonable that the woman reduces her work hours in order to maximize household income given the constraint.

The efficiency argument can be put to a test by analyzing the child penalty depending on the female earnings share within a household. If the efficiency argument were to hold, households where the woman outearns her partner prior to the birth of their first child (*female breadwinner*) would show a smaller female child penalty than households where the man outearns the woman (*male breadwinner*). Appendix Figure A.6 and Table A.17 show a comparison of child penalties in couples with a female vs. couples with a male breadwinner. In the baseline specification, we define the breadwinner as the person who earns at least 50 percent of household earnings in the year before the birth of the first child.[31] Overall, slightly more than one fifth of all couples have a female breadwinner.

[30]One further argument is that biological costs related to child bearing might drive part of the earnings drop. Recent work by Kleven et al. (2020a), however, finds no evidence for differences in child penalties between biological and adopted children.

[31]In order to achieve a reasonable sample size for this additional analysis, we use all couples in our data in this analysis. We do not restrict the sample to those municipalities that opened childcare facilities between years 2001 and 2015.

We find that the child penalty for women in female-breadwinner couples is virtually indistinguishable from that of women in male-breadwinner couples. Except for the year of the child's birth itself, in which the female child penalty is slightly smaller if the woman was the breadwinner, there is no statistically significant difference between child penalties of women who were breadwinners and women who were not. The earnings of men, however, increase slightly more after the birth of the first child if their spouse was the breadwinner in the year before their child was born. This is consistent with the idea that he has to make up for a larger earnings drop.[32] The results remain unchanged if we apply an alternative definition of the breadwinner of at least 60 percent of a couple's total earnings.

These findings suggest that gender norms play an important role in the explanation of the large part of the child penalty unaffected by childcare institutions. This is consistent with Kleven et al. (2019b), who present evidence showing a correlation between the child penalty and gender norms. If gender norms are a major driver of the child penalty, then we would not expect relative earnings contributions within the household to be related to the child penalty.

6 Conclusion

We study the impact of the provision of formal childcare, one of the most widely used family policies to increase women's labor market attachment. We propose that childcare availability in the year the first child is born is particularly relevant for the development of parental earnings trajectories. We leverage the recently proposed event study approach estimating the effect of a first child on parents' labor market outcomes, the child penalty, to evaluate the differential effect of the availability of formal childcare.

Our empirical study draws on administrative data for the canton of Bern, one of the largest Swiss cantons. Bern is particularly interesting as it underwent a considerable expansion in the availability of formal childcare at the municipality level, from 6.4 to 24.2 percent of municipalities, between 2001 and 2015. We find that the observed child penalty in earnings for mothers, which amounts to about 70 percent, is comparable to estimates found for other countries. The introduction of childcare reduces the child penalty for women by about 4.5 percentage points (by 6.3 percent). This effect is primarily driven by an even stronger reduction in the child penalty of women in lower-earnings households (11.2 percentage points or 15.1 percent), and by subsidized childcare.

[32]Note that the earnings of both women and men increase more steeply in the pre-birth period than earnings of non-breadwinners. Such slight pre-trends also appear in studies for other countries, see e.g. Kleven et al. (2019a) and Bütikofer et al. (2018). They probably reflect that for some groups, especially well educated individuals, earnings increase steeply in the 3 to 5 years before the birth of their first child, which usually coincides with the end of their education and subsequent employment. This is also reflected by the fact that we see the largest increases in pre-child earnings for higher-earnings families and that they are more pronounced for women. For lower-earnings families, many of whom probably finished their education longer before having a child, year and age effects are able to fully capture earnings trajectories, as seen in column (4) of Appendix Tables A.3 and A.4.

In line with previous literature, we find that men's earnings respond much less to the birth of the first child than women's earnings. However, we find that while male earnings generally increase after the birth of their first child, the increase is lower in the presence of childcare. This effect is particularly relevant for households in the lower part of the earnings distribution. This result suggests that the burden for men to make up for the drop in their partner's earnings is considerable in lower-earnings households. The availability of childcare seems to reduce this burden on fathers in lower-earnings households. It attenuates the increase in their earnings by 8.4 percentage points.

Our results contribute to the growing literature on the effect of children on parents' earnings trajectories and expand it by rendering causal estimates of childcare on parents labor market outcomes in an early phase of parenthood, just after the birth of a child. While the availability of childcare increases the labor market attachment of (especially lower-earnings) mothers, and thus the distribution of earnings within a household, we do not find an impact on total earnings at the household level. Childcare does not reduce the drop in total household earnings (of about 20 percent) related to the birth of the first child, but the composition of earnings between mothers and fathers. According to our findings for Switzerland, formal childcare does not seem to be a policy tool which could improve total earnings at the household level. However, it seems to be effective in increasing the labor market attachment of mothers, which is a second frequently mentioned goal of formal childcare. In addition, childcare reduces the burden on new fathers in households with below median earnings who otherwise have to make up for the drop in female earnings. This constitutes relaxing effect on a so far ignored but potentially important stress factor.

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A Appendix

Table A.1: Descriptive statistics: municipalities that introduced childcare in 2001 or later

	(1) Main sample: childcare				(2) Intro post 2015			(3) Intro undetermined		
	Mean	S.D.	Min.	Max.	Mean	S.D.	p-val	Mean	S.D.	p-val
Earnings women (t=-1)	51,113	21,188	0	232,005	51,077	18,941	0.95	53,595	23,123	0.00
Earnings men (t=-1)	69,601	27,342	0	330,532	67,613	24,123	0.01	73,263	35,232	0.00
Age women (t=-1)	29.43	4.10	21	42	29.54	4.10	0.35	30.05	4.10	0.00
Age men (t=-1)	31.95	4.43	21	42	32.11	4.42	0.22	32.27	4.34	0.03
Married (t=-1)	0.71	0.45	0	1	0.71	0.45	0.90	0.69	0.46	0.17
Number of children (t=3)	1.74	0.51	1	4	1.75	0.51	0.74	1.67	0.53	0.00
Number of children (t=5)	2.00	0.58	1	4	1.99	0.60	0.88	1.92	0.59	0.00
N municipalities	59				304			12		
Mean pop 2001 municip.	3,025				921			12,341		

Notes: The left-hand panel displays information for couples from our main analysis who live in municipalities that introduced childcare between 2001 and 2015. The middle panel displays corresponding figures for couples in municipalities which did not have childcare by the end of 2015. The right-hand panel displays the figures for couples in municipalities for which we were not able to determine the year of the opening of the facility. Earnings are in Swiss francs.

Table A.2: Descriptive statistics: municipalities that introduced childcare before 2001

	(1) Intro pre 2001			(2) Non-urban only			(3) Bern & Biel		
	Mean	S.D.	p-val	Mean	S.D.	p-val	Mean	S.D.	p-val
Earnings women (t=-1)	51,490	23,501	0.55	49,863	20,420	0.06	53,252	26,331	0.06
Earnings men (t=-1)	69,107	31,619	0.56	68,973	28,932	0.49	69,252	34,302	0.73
Age women (t=-1)	29.87	4.10	0.00	29.22	4.05	0.12	30.56	4.05	0.00
Age men (t=-1)	32.16	4.45	0.10	31.60	4.39	0.01	32.76	4.43	0.00
Married (t=-1)	0.68	0.47	0.01	0.72	0.45	0.62	0.64	0.48	0.00
Number of children (t=3)	1.68	0.53	0.00	1.71	0.51	0.06	1.64	0.54	0.00
Number of children (t=5)	1.94	0.59	0.01	1.97	0.57	0.27	1.90	0.60	0.00
N municipalities	26			24			2		
Mean pop 2001 municip.	13,845			6,736			85,300		

Notes: The left-hand panel displays information for couples in municipalities that introduced childcare before 2001. The large urban centers of Bern and Biel are among those. We, therefore, also display descriptive statistics for couples from non-urban municipalities and from Bern and Biel separately in the middle and right-hand panels. P-values correspond to differences relative to main sample in Table A.1. Earnings are in Swiss francs.

Tables A.1 and A.2 show descriptive statistics for the different samples in our analysis. In our main estimation sample of 59 municipalities (panel (1) of Table A.1), men's earnings in the year before they have their first child are higher, on average, than those of their female partners.They are, however, on average also more than 2.5 years older in that year. 71 percent of couples in our analysis are married in the year before childbirth. The number of children increases to on average 1.74 three years after birth of the first child, and to 2 children five years after. The municipalities in our main sample are rather small with an average population of around 3,000 inhabitants.

Panel (2) of Table A.1 displays corresponding numbers for couples living in municipalities which did not have childcare by the end of 2015. We use those for placebo tests. The summary statistics are hardly distinguishable for the two groups, though they are on average even smaller with an average population of around 1,000. Only average male earnings differ significantly between the two groups with a t-statistic of 2.69. The difference of about 2000 Swiss francs a year is however not very large.

Panel (3) shows descriptive statistics for couples in municipalities for which we were not able to determine the date of childcare introduction. We do not use observations from the latter two groups in any part of our analysis. Mean numbers for most non-monetary characteristics of these groups differ significantly from the means in our main sample, they are also much larger with an average population of 12,341.

Earnings tend to be slightly higher among couples in municipalities with an undetermined year of introduction. As indicated by the population in these municipalities, municipalities with an undetermined date tend to have more inhabitants and may, thus, be more urban. The larger average size of these municipalities also explains why we were not able to determine the year during which they introduced childcare. There tend to be multiple facilities in each of these municipalities, which raised the chance that we were not able to learn about the opening date of at least one of these.

Table A.2 shows summary statistics for couples who had their first child in municipalities which introduced institutional childcare before 2001. We further report the p-values of tests against our main estimation sample. Note that these municipalities include the urban centers of Bern and Biel. We therefore also display the corresponding numbers for couples in municipalities excluding Bern and Biel but which also introduced childcare before 2001, and for Bern and Biel, separately.

Couples in the municipalities that introduced childcare before 2001 differ systematically from those in out main sample along several dimensions. Women are somewhat older, couples are less likely to be married, and the average number of children is a bit lower. Average incomes are not systematically different. While these differences are quantitatively rather small they are still significant. Among those municipalities introducing childcare early are the two urban centers of the canton, Biel and Bern. It might thus not be surprising to see that the characteristics differ in these dimensions. The average population of about 13,000 in these municipalities is much higher than in our sample. Excluding these urban centers in panel (2), the differences decrease and sometimes even switch signs. The remaining municipalities are still larger than those in our sample, but the population difference is strongly reduced. The differences between Biel and Bern and our main sample increase, as shown in (panel 3). It is not surprising to see that urban centers introduce childcare early and that the population differs from the more rural places introducing it later. While the differences make sense and are partly statistically significant, their magnitude is limited. The differences in incomes amount to a maximum of about 2000 Swiss francs a year, the differences in age to about one year, and the differences in the number of children to 0.1. Our sample therefore still seems to be quite representative for the municipalities of Bern.

Table A.3: Earnings penalty, women

	Overall	Childcare	Childcare & Hh inc.>p50	Childcare & Hh inc.≤p50	Subsidized childcare
	(1)	(2)	(3)	(4)	(5)
Event −3	-0.015	-0.016	-0.041**	0.016	-0.020
	(0.014)	(0.018)	(0.020)	(0.031)	(0.018)
Event −2	-0.013*	-0.008	-0.022*	0.018	-0.010
	(0.007)	(0.010)	(0.012)	(0.016)	(0.009)
Event 0	-0.243***	-0.259***	-0.226***	-0.304***	-0.261***
	(0.009)	(0.011)	(0.014)	(0.016)	(0.011)
Event +1	-0.620***	-0.642***	-0.623***	-0.681***	-0.653***
	(0.008)	(0.011)	(0.013)	(0.017)	(0.012)
Event +2	-0.641***	-0.661***	-0.653***	-0.686***	-0.671***
	(0.008)	(0.011)	(0.013)	(0.017)	(0.012)
Event +3	-0.672***	-0.687***	-0.676***	-0.718***	-0.694***
	(0.009)	(0.012)	(0.014)	(0.018)	(0.013)
Event +4	-0.684***	-0.701***	-0.686***	-0.734***	-0.706***
	(0.010)	(0.013)	(0.015)	(0.021)	(0.014)
Event +5	-0.691***	-0.707***	-0.687***	-0.747***	-0.714***
	(0.012)	(0.014)	(0.017)	(0.023)	(0.015)
Event +6	-0.692***	-0.714***	-0.702***	-0.744***	-0.718***
	(0.014)	(0.016)	(0.019)	(0.027)	(0.017)
Event −3· care		0.007	0.014	0.007	0.012
		(0.016)	(0.016)	(0.030)	(0.033)
Event −2· care		-0.007	0.001	-0.021	0.000
		(0.011)	(0.012)	(0.019)	(0.016)
Event 0· care		0.026**	0.028*	0.029*	0.043*
		(0.011)	(0.014)	(0.018)	(0.023)
Event +1· care		0.038**	0.030*	0.067***	0.013
		(0.015)	(0.018)	(0.024)	(0.035)
Event +2· care		0.035**	0.024	0.074***	0.018
		(0.016)	(0.018)	(0.025)	(0.034)
Event +3· care		0.026	0.009	0.086***	0.001
		(0.016)	(0.019)	(0.026)	(0.036)
Event +4· care		0.031*	0.008	0.099***	0.013
		(0.017)	(0.020)	(0.028)	(0.038)
Event +5· care		0.032*	0.002	0.105***	0.026
		(0.019)	(0.022)	(0.031)	(0.042)
Event +6· care		0.045**	0.019	0.112***	0.022
		(0.023)	(0.026)	(0.034)	(0.039)
Event −3· subs. care					0.018
					(0.016)
Event −2· subs. care					-0.002
					(0.011)
Event 0· subs. care					0.028**
					(0.012)
Event +1· subs. care					0.050***
					(0.016)
Event +2· subs. care					0.045***
					(0.016)
Event +3· subs. care					0.034**
					(0.017)
Event +4· subs. care					0.040**
					(0.018)
Event +5· subs. care					0.042**
					(0.020)
Event +6· subs. care					0.056**
					(0.024)
Indiv. FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Married (t<0)	Yes	Yes	Yes	Yes	Yes
N indiv.	1959	1959	1001	958	1767
N	21437	21437	11194	10243	19359

Notes: Event study estimates of female earnings relative to the year prior to the birth of their first child. The interaction terms with childcare (*care*) indicate the differential effect if at the birth of the first child, there are childcare facilities in the municipality of residence. The interaction terms with subsidized childcare (*subs. care*) indicate the difference if at the birth of the first child, there are subsidized childcare facilities in the municipality of residence compared to if there is no childcare. Standard errors are bootstrapped (1000 replications) as the estimates of earnings changes are transformed into percentage changes after the estimation by dividing the estimates by $\widehat{earnings}$, i.e. predicted earnings when setting event time dummies to zero.
Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Earnings penalty, men

	Overall	Childcare	Childcare & Hh inc.>p50	Childcare & Hh inc.≤p50	Subsidized childcare
	(1)	(2)	(3)	(4)	(5)
Event −3	-0.018	-0.020	-0.051**	0.027	-0.013
	(0.015)	(0.018)	(0.021)	(0.029)	(0.019)
Event −2	-0.012*	-0.012	-0.022*	-0.001	-0.006
	(0.007)	(0.009)	(0.012)	(0.015)	(0.010)
Event 0	0.045***	0.050***	0.011	0.096***	0.046***
	(0.010)	(0.011)	(0.015)	(0.019)	(0.011)
Event +1	0.054***	0.066***	0.020	0.129***	0.062***
	(0.012)	(0.014)	(0.019)	(0.025)	(0.015)
Event +2	0.054***	0.068***	0.021	0.136***	0.062***
	(0.014)	(0.016)	(0.021)	(0.029)	(0.017)
Event +3	0.073***	0.087***	0.027	0.183***	0.081***
	(0.017)	(0.019)	(0.025)	(0.036)	(0.021)
Event +4	0.069***	0.077***	0.008	0.191***	0.073***
	(0.020)	(0.022)	(0.028)	(0.040)	(0.024)
Event +5	0.065***	0.082***	0.017	0.194***	0.075***
	(0.023)	(0.024)	(0.030)	(0.042)	(0.026)
Event +6	0.057**	0.076***	0.001	0.215***	0.069**
	(0.026)	(0.027)	(0.034)	(0.051)	(0.030)
Event −3· care		-0.001	0.017	-0.017	0.010
		(0.015)	(0.017)	(0.023)	(0.028)
Event −2· care		-0.001	-0.003	0.007	0.011
		(0.010)	(0.013)	(0.015)	(0.018)
Event 0· care		-0.007	-0.003	-0.012	-0.018
		(0.008)	(0.011)	(0.013)	(0.019)
Event +1· care		-0.019*	-0.021	-0.017	-0.017
		(0.011)	(0.014)	(0.017)	(0.025)
Event +2· care		-0.022*	-0.022	-0.024	-0.014
		(0.012)	(0.014)	(0.019)	(0.030)
Event +3· care		-0.024*	-0.017	-0.044*	-0.014
		(0.013)	(0.016)	(0.024)	(0.023)
Event +4· care		-0.013	-0.009	-0.025	-0.006
		(0.016)	(0.018)	(0.028)	(0.029)
Event +5· care		-0.031*	-0.035*	-0.030	-0.021
		(0.018)	(0.021)	(0.030)	(0.034)
Event +6· care		-0.037*	-0.017	-0.084**	-0.067*
		(0.021)	(0.024)	(0.037)	(0.037)
Event −3· subs. care					-0.008
					(0.016)
Event −2· subs. care					-0.010
					(0.011)
Event 0· subs. care					-0.003
					(0.008)
Event +1· subs. care					-0.017
					(0.012)
Event +2· subs. care					-0.022*
					(0.012)
Event +3· subs. care					-0.023
					(0.014)
Event +4· subs. care					-0.017
					(0.017)
Event +5· subs. care					-0.035*
					(0.019)
Event +6· subs. care					-0.034
					(0.023)
Indiv. FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Married (t<0)	Yes	Yes	Yes	Yes	Yes
N indiv.	1959	1959	1001	958	1767
N	21437	21437	11194	10243	19359

Notes: Event study estimates of male earnings relative to the year prior to the birth of their first child. The interaction terms with childcare (*care*) indicate the differential effect if at the birth of the first child, there are childcare facilities in the municipality of residence. The interaction terms with subsidized childcare (*subs. care*) indicate the difference if at the birth of the first child, there are subsidized childcare facilities in the municipality of residence compared to if there is no childcare. Standard errors are bootstrapped (1000 replications) as the estimates of earnings changes are transformed into percentage changes after the estimation by dividing the estimates by $\widehat{earnings}$, i.e. predicted earnings when setting event time dummies to zero.

Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Participation penalty, women

	Overall	Childcare	Childcare & Hh inc.>p50	Childcare & Hh inc.≤p50	Subsidized childcare
	(1)	(2)	(3)	(4)	(5)
Event −3	-0.000	-0.002	0.011	-0.010	-0.006
	(0.012)	(0.012)	(0.014)	(0.020)	(0.012)
Event −2	-0.010	-0.004	0.002	-0.007	-0.005
	(0.006)	(0.006)	(0.007)	(0.011)	(0.007)
Event 0	-0.022**	-0.027**	-0.013	-0.046***	-0.019*
	(0.009)	(0.010)	(0.012)	(0.017)	(0.011)
Event +1	-0.145***	-0.159***	-0.129***	-0.195***	-0.155***
	(0.014)	(0.017)	(0.022)	(0.027)	(0.018)
Event +2	-0.184***	-0.201***	-0.195***	-0.217***	-0.195***
	(0.017)	(0.020)	(0.026)	(0.031)	(0.021)
Event +3	-0.195***	-0.215***	-0.216***	-0.226***	-0.208***
	(0.020)	(0.023)	(0.030)	(0.036)	(0.024)
Event +4	-0.207***	-0.223***	-0.227***	-0.235***	-0.213***
	(0.023)	(0.026)	(0.034)	(0.042)	(0.028)
Event +5	-0.213***	-0.225***	-0.229***	-0.239***	-0.211***
	(0.027)	(0.030)	(0.038)	(0.048)	(0.031)
Event +6	-0.207***	-0.233***	-0.244***	-0.239***	-0.221***
	(0.031)	(0.034)	(0.042)	(0.055)	(0.035)
Event −3· care		0.007	0.010	0.003	0.021
		(0.008)	(0.008)	(0.015)	(0.018)
Event −2· care		-0.009	-0.003	-0.015	0.003
		(0.008)	(0.008)	(0.015)	(0.022)
Event 0· care		0.006	0.002	0.009	0.015
		(0.008)	(0.005)	(0.015)	(0.010)
Event +1· care		0.021	0.002	0.037	0.052*
		(0.017)	(0.021)	(0.026)	(0.031)
Event +2· care		0.026	0.013	0.038	0.024
		(0.019)	(0.025)	(0.028)	(0.038)
Event +3· care		0.032	0.014	0.048*	0.040
		(0.020)	(0.026)	(0.029)	(0.039)
Event +4· care		0.026	0.012	0.040	0.007
		(0.021)	(0.028)	(0.032)	(0.042)
Event +5· care		0.017	-0.008	0.045	0.003
		(0.022)	(0.030)	(0.034)	(0.043)
Event +6· care		0.051**	0.015	0.089**	0.055
		(0.023)	(0.030)	(0.036)	(0.044)
Event −3· subs. care					-0.014
					(0.018)
Event −2· subs. care					-0.010
					(0.023)
Event 0· subs. care					-0.012
					(0.010)
Event +1· subs. care					-0.036
					(0.031)
Event +2· subs. care					0.001
					(0.037)
Event +3· subs. care					-0.012
					(0.039)
Event +4· subs. care					0.021
					(0.042)
Event +5· subs. care					0.009
					(0.043)
Event +6· subs. care					-0.007
					(0.044)
Indiv. FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Married (t<0)	Yes	Yes	Yes	Yes	Yes
N indiv.	1959	1959	1001	958	1767
N	21437	21437	11194	10243	19359

Notes: Event study estimates of female participation rates relative to the year prior to the birth of their first child. The interaction terms with childcare (*care*) indicate the differential effect if at the birth of the first child, there are childcare facilities in the municipality of residence. The interaction terms with subsidized childcare (*subs. care*) indicate the difference if at the birth of the first child, there are subsidized childcare facilities in the municipality of residence compared to if there is no childcare. Standard errors are bootstrapped (1000 replications) as the estimates of earnings changes are transformed into percentage changes after the estimation by dividing the estimates by $\widehat{earnings}$, i.e. predicted earnings when setting event time dummies to zero. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Participation penalty, men

	Overall	Childcare	Childcare & Hh inc.>p50	Childcare & Hh inc.≤p50	Subsidized childcare
	(1)	(2)	(3)	(4)	(5)
Event −3	-0.012**	-0.011	-0.006	-0.015	-0.008
	(0.006)	(0.008)	(0.009)	(0.014)	(0.008)
Event −2	-0.008**	-0.007	0.001	-0.016	-0.006
	(0.004)	(0.006)	(0.004)	(0.010)	(0.006)
Event 0	0.001	0.002	-0.003	0.009	0.002
	(0.004)	(0.005)	(0.005)	(0.009)	(0.006)
Event +1	-0.007	-0.005	-0.006	-0.001	-0.007
	(0.006)	(0.007)	(0.007)	(0.012)	(0.007)
Event +2	-0.015**	-0.012	-0.009	-0.011	-0.013*
	(0.007)	(0.008)	(0.008)	(0.013)	(0.008)
Event +3	-0.015*	-0.013	-0.014	-0.007	-0.013
	(0.009)	(0.009)	(0.010)	(0.016)	(0.010)
Event +4	-0.023**	-0.026**	-0.025**	-0.019	-0.024**
	(0.011)	(0.012)	(0.013)	(0.020)	(0.012)
Event +5	-0.033***	-0.029**	-0.023*	-0.027	-0.031**
	(0.013)	(0.013)	(0.014)	(0.023)	(0.014)
Event +6	-0.042**	-0.044**	-0.040**	-0.038	-0.046**
	(0.016)	(0.017)	(0.019)	(0.029)	(0.018)
Event −3· care		-0.003	0.003	-0.011	0.010
		(0.007)	(0.007)	(0.012)	(0.013)
Event −2· care		-0.002	-0.007	0.002	0.012
		(0.006)	(0.006)	(0.011)	(0.013)
Event 0· care		-0.002	-0.001	-0.003	-0.002
		(0.004)	(0.004)	(0.006)	(0.012)
Event +1· care		-0.002	-0.004	-0.001	-0.005
		(0.005)	(0.004)	(0.009)	(0.015)
Event +2· care		-0.005	-0.007	-0.004	-0.018
		(0.006)	(0.005)	(0.010)	(0.019)
Event +3· care		-0.004	-0.009	0.001	0.001
		(0.006)	(0.006)	(0.010)	(0.012)
Event +4· care		0.006	-0.003	0.014	-0.011
		(0.007)	(0.009)	(0.011)	(0.018)
Event +5· care		-0.009	-0.023**	0.006	-0.014
		(0.009)	(0.010)	(0.015)	(0.020)
Event +6· ccare		0.004	-0.002	0.010	-0.005
		(0.009)	(0.009)	(0.015)	(0.021)
Event −3· subs. care					-0.016
					(0.013)
Event −2· subs. care					-0.016
					(0.013)
Event 0· subs. care					-0.001
					(0.012)
Event +1· subs. care					0.004
					(0.015)
Event +2· subs. care					0.014
					(0.019)
Event +3· subs. care					-0.007
					(0.012)
Event +4· subs. care					0.017
					(0.017)
Event +5· subs. care					0.006
					(0.020)
Event +6· subs. care					0.008
					(0.021)
Indiv. FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Married (t<0)	Yes	Yes	Yes	Yes	Yes
N indiv.	1959	1959	1001	958	1767
N	21437	21437	11194	10243	19359

Notes: Event study estimates of male participation rates relative to the year prior to the birth of their first child. The interaction terms with childcare (*care*) indicate the differential effect if at the birth of the first child, there are childcare facilities in the municipality of residence. The interaction terms with subsidized childcare (*subs. care*) indicate the difference if at the birth of the first child, there are subsidized childcare facilities in the municipality of residence compared to if there is no childcare. Standard errors are bootstrapped (1000 replications) as the estimates of earnings changes are transformed into percentage changes after the estimation by dividing the estimates by $\widehat{earnings}$, i.e. predicted earnings when setting event time dummies to zero. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Earnings penalty, household level

	Overall	Childcare	Childcare & Hh earn.>p50	Childcare & Hh earn.≤p50
	(1)	(2)	(3)	(4)
Event −3	-0.020**	-0.022*	-0.050***	0.017
	(0.010)	(0.013)	(0.016)	(0.020)
Event −2	-0.013**	-0.011	-0.022**	0.006
	(0.005)	(0.007)	(0.009)	(0.011)
Event 0	-0.079***	-0.083***	-0.088***	-0.079***
	(0.006)	(0.007)	(0.010)	(0.011)
Event +1	-0.238***	-0.241***	-0.253***	-0.231***
	(0.007)	(0.008)	(0.010)	(0.013)
Event +2	-0.247***	-0.248***	-0.266***	-0.228***
	(0.008)	(0.008)	(0.011)	(0.015)
Event +3	-0.248***	-0.246***	-0.272***	-0.215***
	(0.009)	(0.010)	(0.013)	(0.019)
Event +4	-0.255***	-0.257***	-0.288***	-0.214***
	(0.010)	(0.012)	(0.015)	(0.022)
Event +5	-0.260***	-0.257***	-0.283***	-0.221***
	(0.012)	(0.013)	(0.016)	(0.023)
Event +6	-0.265***	-0.263***	-0.301***	-0.206***
	(0.014)	(0.015)	(0.018)	(0.028)
Event −3· care		0.004	0.017	-0.003
		(0.011)	(0.012)	(0.017)
Event −2· care		-0.003	-0.001	-0.004
		(0.008)	(0.010)	(0.012)
Event 0· care		0.007	0.009	0.006
		(0.007)	(0.009)	(0.011)
Event +1· care		0.005	0.000	0.021
		(0.009)	(0.010)	(0.014)
Event +2· care		0.002	-0.003	0.019
		(0.009)	(0.010)	(0.015)
Event +3· care		-0.004	-0.007	0.012
		(0.010)	(0.012)	(0.017)
Event +4· care		0.003	-0.003	0.027
		(0.012)	(0.013)	(0.019)
Event +5· care		-0.006	-0.021	0.030
		(0.013)	(0.014)	(0.020)
Event +6· care		-0.004	-0.003	0.002
		(0.015)	(0.017)	(0.023)
Indiv. FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Married (t<0)	Yes	Yes	Yes	Yes
N indiv.	1959	1959	1001	958
N	21437	21437	11194	10243

Notes: Event study estimates of both parents' combined earnings relative to the year prior to the birth of their first child in columns (1)-(4) and of the female earnings share in column (5). The interaction terms with childcare (*care*) indicate the differential effect if at the birth of the first child, there are childcare facilities in the municipality of residence. Standard errors are bootstrapped (1000 replications) as the estimates of earnings changes are transformed into percentage changes after the estimation by dividing the estimates by $\widehat{earnings}$, i.e., predicted earnings when setting event time dummies to zero. In specification (5), the dependent variable is the female earnings share and estimates do not need to be transformed. Standard errors here are clustered at the individual level. There are fewer observations in specification (5) as the female earnings share cannot be calculated if both partners have an income of 0. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Earnings penalty women by year of childbirth relative to facility opening year

	Birth year after opening	Birth year same as opening	Birth 1-3 years before opening	Birth 4-6 years before opening
	(1)	(2)	(3)	(4)
Event −3	-0.036	-0.032	-0.021	-0.027
	(0.031)	(0.048)	(0.039)	(0.039)
Event −2	-0.015	-0.016	-0.014	-0.014
	(0.016)	(0.022)	(0.019)	(0.020)
Event 0	-0.247***	-0.239***	-0.239***	-0.237***
	(0.018)	(0.024)	(0.020)	(0.021)
Event +1	-0.647***	-0.635***	-0.639***	-0.633***
	(0.020)	(0.021)	(0.020)	(0.020)
Event +2	-0.665***	-0.651***	-0.653***	-0.647***
	(0.020)	(0.022)	(0.021)	(0.021)
Event +3	-0.700***	-0.689***	-0.694***	-0.687***
	(0.022)	(0.024)	(0.023)	(0.022)
Event +4	-0.700***	-0.688***	-0.699***	-0.693***
	(0.024)	(0.028)	(0.026)	(0.025)
Event +5	-0.710***	-0.697***	-0.716***	-0.713***
	(0.024)	(0.029)	(0.027)	(0.024)
Event +6	-0.720***	-0.702***	-0.729***	-0.726***
	(0.026)	(0.030)	(0.028)	(0.027)
Event −3· care	0.016	0.070	0.014	0.025
	(0.029)	(0.048)	(0.038)	(0.038)
Event −2· care	-0.004	0.025	0.015	0.001
	(0.017)	(0.028)	(0.022)	(0.021)
Event 0· care	0.020	-0.013	-0.022	-0.010
	(0.018)	(0.034)	(0.022)	(0.023)
Event +1· care	0.051**	0.074*	0.020	0.004
	(0.023)	(0.040)	(0.026)	(0.026)
Event +2· care	0.047**	0.060	0.005	0.004
	(0.023)	(0.043)	(0.028)	(0.028)
Event +3· care	0.046*	0.107**	0.022	0.017
	(0.025)	(0.043)	(0.030)	(0.030)
Event +4· care	0.038	0.084*	-0.006	0.017
	(0.027)	(0.050)	(0.032)	(0.031)
Event +5· care	0.043	0.093*	0.019	0.025
	(0.027)	(0.049)	(0.033)	(0.030)
Event +6· care	0.059**	0.123***	0.003	0.040
	(0.030)	(0.047)	(0.034)	(0.032)
Indiv. FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Married (t<0)	Yes	Yes	Yes	Yes
N indiv.	1298	317	563	543
N	14158	3981	6523	6736

Notes: Event study estimates of female earnings relative to the year prior to the birth of their first child. The "treatment" dummy (*treat*) indicates a group of mothers for whom the difference between the year of birth of their first child and the opening year of the first childcare facility in their municipality fell into the interval indicated at the top of each column. The control group, for whom *treat* is zero, consists of mothers who had their first child more than 6 years before the opening of the first childcare facility. In contrast to our main analyses, we also include municipalities that introduced childcare after 2015 to increase our sample size. Standard errors are bootstrapped (1000 replications) as the estimates of earnings changes are transformed into percentage changes after the estimation by dividing the estimates by $\widehat{earnings}$, i.e., predicted earnings when setting event time dummies to zero. Standard errors here are clustered at the individual level.
Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Earnings penalty men by year of childbirth relative to facility opening year

	Birth year after opening	Birth year same as opening	Birth 1-3 years before opening	Birth 4-6 years before opening
	(1)	(2)	(3)	(4)
Event -3	-0.021	-0.019	-0.036	-0.031
	(0.030)	(0.123)	(0.039)	(0.039)
Event -2	-0.016	-0.023	-0.025	-0.025
	(0.018)	(0.023)	(0.021)	(0.020)
Event 0	0.045**	0.035	0.029	0.022
	(0.018)	(0.030)	(0.022)	(0.025)
Event +1	0.041*	0.031	0.020	0.022
	(0.022)	(0.038)	(0.027)	(0.030)
Event +2	0.053**	0.042	0.028	0.033
	(0.026)	(0.045)	(0.032)	(0.036)
Event +3	0.081**	0.070	0.053	0.057
	(0.034)	(0.052)	(0.039)	(0.043)
Event +4	0.082**	0.069	0.053	0.052
	(0.037)	(0.056)	(0.042)	(0.045)
Event +5	0.064*	0.053	0.039	0.033
	(0.036)	(0.058)	(0.043)	(0.046)
Event +6	0.053	0.046	0.029	0.019
	(0.039)	(0.060)	(0.045)	(0.050)
Event $-3\cdot$ care	0.014	0.016	0.027	0.011
	(0.026)	(0.085)	(0.032)	(0.030)
Event $-2\cdot$ care	0.007	0.026	0.017	0.009
	(0.018)	(0.024)	(0.023)	(0.021)
Event $0\cdot$ care	0.006	0.003	0.016	0.006
	(0.014)	(0.021)	(0.020)	(0.015)
Event $+1\cdot$ care	0.013	0.006	0.047*	0.020
	(0.018)	(0.025)	(0.025)	(0.022)
Event $+2\cdot$ care	-0.000	-0.027	0.026	0.012
	(0.020)	(0.036)	(0.027)	(0.024)
Event $+3\cdot$ care	-0.009	-0.056	0.007	0.006
	(0.027)	(0.038)	(0.035)	(0.030)
Event $+4\cdot$ care	-0.011	-0.053	-0.010	-0.006
	(0.030)	(0.038)	(0.037)	(0.031)
Event $+5\cdot$ care	-0.009	-0.021	0.043	0.011
	(0.029)	(0.035)	(0.039)	(0.030)
Event $+6\cdot$ care	-0.011	-0.057	0.025	0.032
	(0.030)	(0.044)	(0.039)	(0.033)
Indiv. FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Married (t<0)	Yes	Yes	Yes	Yes
N indiv.	1298	317	563	543
N	14158	3981	6523	6736

Notes: Event study estimates of female earnings relative to the year prior to the birth of their first child. The "treatment" dummy (*treat*) indicates a group of mothers for whom the difference between the year of birth of their first child and the opening year of the first childcare facility in their municipality fell into the interval indicated at the top of each column. The control group, for whom *treat* is zero, consists of mothers who had their first child more than 6 years before the opening of the first childcare facility. In contrast to our main analyses, we also include municipalities that introduced childcare after 2015 to increase our sample size. Standard errors are bootstrapped (1000 replications) as the estimates of earnings changes are transformed into percentage changes after the estimation by dividing the estimates by $\widehat{earnings}$, i.e., predicted earnings when setting event time dummies to zero. Standard errors here are clustered at the individual level.
Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Female earnings share

	Overall	Hh inc. > p50	Hh inc. ≤ p50
	(1)	(2)	(3)
Event −3	0.555	0.857	0.491
	(0.735)	(0.825)	(1.228)
Event −2	0.347	-0.127	1.061
	(0.463)	(0.482)	(0.788)
Event 0	-10.398***	-7.947***	-13.193***
	(0.602)	(0.739)	(0.947)
Event +1	-25.617***	-23.336***	-28.389***
	(0.800)	(0.958)	(1.268)
Event +2	-26.404***	-25.078***	-28.356***
	(0.907)	(1.081)	(1.458)
Event +3	-28.063***	-26.445***	-30.516***
	(1.034)	(1.230)	(1.661)
Event +4	-28.468***	-26.667***	-31.129***
	(1.157)	(1.368)	(1.877)
Event +5	-28.285***	-26.647***	-30.711***
	(1.298)	(1.516)	(2.131)
Event +6	-28.133***	-26.440***	-30.615***
	(1.543)	(1.776)	(2.570)
Event −3· care	0.041	-0.465	0.620
	(0.710)	(0.789)	(1.208)
Event −2· care	-0.573	0.099	-1.279
	(0.520)	(0.513)	(0.913)
Event 0· care	1.338***	1.217**	1.311
	(0.504)	(0.564)	(0.836)
Event +1· care	2.403***	1.444*	3.280***
	(0.728)	(0.850)	(1.179)
Event +2· care	2.423***	1.565*	3.345***
	(0.775)	(0.898)	(1.263)
Event +3· care	2.427***	1.053	4.082***
	(0.814)	(0.960)	(1.310)
Event +4· care	2.746***	1.185	4.551***
	(0.864)	(1.030)	(1.400)
Event +5· care	2.466***	1.042	3.989***
	(0.936)	(1.110)	(1.524)
Event +6· care	2.706***	0.615	5.089***
	(1.031)	(1.196)	(1.708)
Constant	38.462***	45.624***	37.699***
	(2.085)	(4.440)	(2.792)
Indiv. FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Age FE	Yes	Yes	Yes
Married (t<0)	Yes	Yes	Yes
N indiv.	1959	1001	958
N	21294	11149	10145

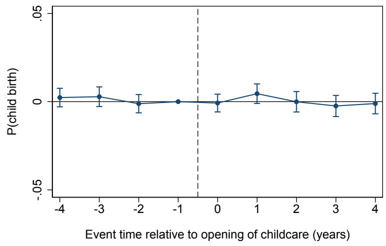
Notes: Event study estimates of the earnings share contributed by the woman to total household earnings relative to the year prior to the birth of their first child. The interaction terms with childcare (*care*) indicate the differential effect if at the birth of the first child, there are childcare facilities in the municipality of residence. Constant refers to the average female income share after controlling for year FE, age FE, married, and all the event indicators. Standard errors are clustered at the individual level. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Take-up: Effect on childcare deductions

	Total amount	P(deduction)
	(1)	(2)
Event 0	307.583***	0.166***
	(57.387)	(0.018)
Event +1	645.668***	0.295***
	(82.153)	(0.025)
Event +2	909.776***	0.353***
	(108.323)	(0.030)
Event +3	1231.285***	0.427***
	(138.909)	(0.037)
Event +4	1503.369***	0.490***
	(167.279)	(0.044)
Event +5	1609.405***	0.521***
	(195.250)	(0.050)
Event +6	1460.833***	0.471***
	(232.263)	(0.061)
Event 0· care	0.739	-0.017
	(27.827)	(0.016)
Event +1· care	178.320***	0.032
	(46.934)	(0.020)
Event +2· care	200.419***	0.042**
	(59.161)	(0.021)
Event +3· care	291.782***	0.056**
	(82.931)	(0.023)
Event +4· care	248.515**	0.021
	(100.277)	(0.025)
Event +5· care	228.496**	0.006
	(110.802)	(0.026)
Event +6· care	412.706***	0.094***
	(111.475)	(0.026)
Indiv. FE	Yes	Yes
Year FE	Yes	Yes
Age FE	Yes	Yes
Married (t<0)	Yes	Yes
N indiv.	1959	1959
N	14760	14760

Notes: Event study estimates of filings for child deductions, in specification (1) using total amount deduced in CHF as dependent variable and in specification (2) using an indicator set to one if a positive amount is claimed as dependent variable. *care*, takes a value of one for treated municipalities after the introduction of childcare and zero otherwise. The event time dummies capture changes relative to the year before the opening of a childcare facility $t = -1$. Robust standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.1: Probability of childbirth relative to the year before the opening of childcare



Note: Probability of childbirth relative to the year before the opening of a childcare facility. The specification controls for year effects and individual fixed effects. Event time $t = -4$ includes years ≤ -4 before the opening of a childcare facility. Event time $t = 4$ includes years ≥ 4 after the opening of a childcare facility. Standard errors are clustered at the individual level. The corresponding estimates can be found in column (2) of Table A.12 in Appendix A.

Table A.12: Effect of childcare on fertility

	DiD	Event study
	(1)	(2)
Care	0.000	
	(0.001)	
Mun. with care	0.001	
	(0.001)	
Event −4		0.002
		(0.003)
Event −3		0.003
		(0.003)
Event −2		-0.001
		(0.003)
Event 0		-0.001
		(0.003)
Event +1		0.005
		(0.003)
Event +2		-0.000
		(0.003)
Event +3		-0.002
		(0.003)
Event +4		-0.001
		(0.003)
Year FE	Yes	Yes
Individual FE	No	Yes
N indiv.		30,480
N	494,601	190,453
N cluster	366	

Notes: Event study estimates of fertility rates, once as a DiD around the opening of a childcare facility and once as an event study around the opening of a childcare facility. In the DiD specification, municipalities opening a childcare facility between 2001 and 2015 are defined as the treatment group (mun. with care). Municipalities that have not opened a childcare facility by the end of 2015 are used as the control group. The indicator *care* takes value one for treated municipalities after the introduction of childcare and zero otherwise. Specification (2) only includes individuals in municipalities that did introduce childcare. The event time indicators capture changes relative to the year before the opening of a childcare facility $t = -1$. Standard errors are clustered at the municipality level for the DiD and at the individual level for the event study. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A.13: Robustness tests, women

	Stayer	Stayer subsid. care	Selection	+- 3 yrs around opening
	(1)	(2)	(3)	(4)
Event −3	0.008	0.009	-0.019**	-0.026
	(0.020)	(0.021)	(0.009)	(0.026)
Event −2	-0.006	-0.008	-0.009*	-0.005
	(0.010)	(0.011)	(0.005)	(0.016)
Event 0	-0.270***	-0.271***	-0.253***	-0.278***
	(0.013)	(0.013)	(0.006)	(0.015)
Event +1	-0.647***	-0.656***	-0.644***	-0.643***
	(0.013)	(0.013)	(0.006)	(0.015)
Event +2	-0.662***	-0.671***	-0.667***	-0.669***
	(0.013)	(0.014)	(0.006)	(0.016)
Event +3	-0.689***	-0.697***	-0.697***	-0.688***
	(0.014)	(0.014)	(0.006)	(0.018)
Event +4	-0.700***	-0.705***	-0.708***	-0.724***
	(0.016)	(0.016)	(0.007)	(0.019)
Event +5	-0.704***	-0.709***	-0.721***	-0.717***
	(0.017)	(0.017)	(0.008)	(0.023)
Event +6	-0.709***	-0.713***	-0.734***	-0.743***
	(0.019)	(0.019)	(0.009)	(0.028)
Event −3· care	-0.019	-0.010	-0.016	0.040*
	(0.018)	(0.035)	(0.011)	(0.023)
Event −2· care	-0.019*	-0.005	-0.005	-0.006
	(0.011)	(0.017)	(0.008)	(0.018)
Event 0· care	0.033***	0.064***	-0.002	0.044**
	(0.013)	(0.024)	(0.009)	(0.018)
Event +1· care	0.040**	0.003	-0.006	0.046*
	(0.017)	(0.037)	(0.011)	(0.024)
Event +2· care	0.037**	0.003	0.007	0.039
	(0.018)	(0.037)	(0.012)	(0.024)
Event +3· care	0.038**	-0.006	0.001	0.044*
	(0.019)	(0.039)	(0.013)	(0.025)
Event +4· care	0.040*	0.002	0.010	0.066**
	(0.020)	(0.043)	(0.015)	(0.026)
Event +5· care	0.042*	0.009	0.011	0.046
	(0.022)	(0.048)	(0.016)	(0.029)
Event +6· care	0.054**	0.013	0.006	0.074**
	(0.026)	(0.045)	(0.019)	(0.035)
Event −3· subs. care		-0.010		
		(0.020)		
Event −2· subs. care		-0.014		
		(0.013)		
Event 0· subs. care		0.036**		
		(0.014)		
Event +1· subs. care		0.057***		
		(0.019)		
Event +2· subs. care		0.053***		
		(0.020)		
Event +3· subs. care		0.050**		
		(0.021)		
Event +4· subs. care		0.053**		
		(0.022)		
Event +5· subs. care		0.055**		
		(0.024)		
Event +6· subs. care		0.070**		
		(0.029)		
Indiv. FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Married (t<0)	Yes	Yes	Yes	Yes
N indiv.	1460	1336	4310	760
N	15978	14648	41595	8102

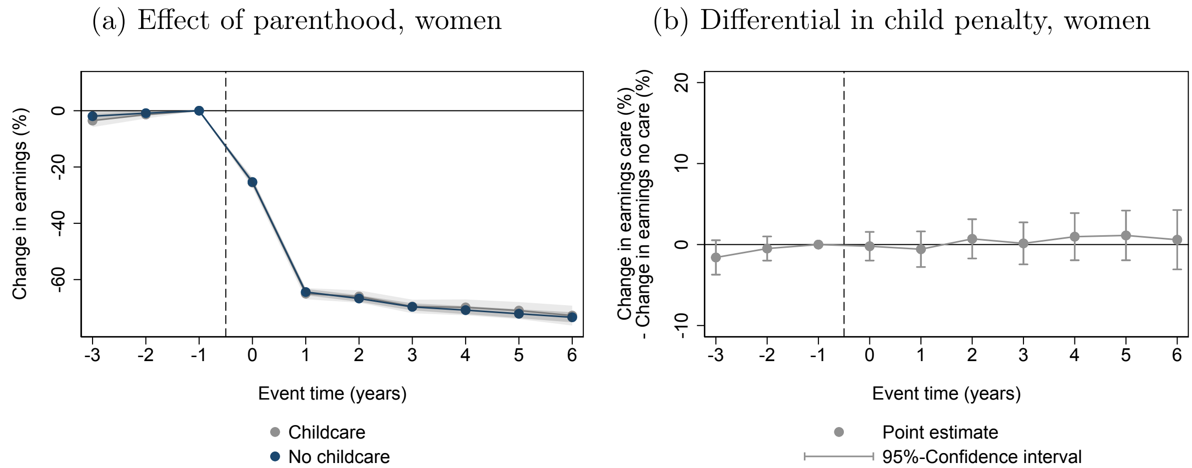
Notes: Event study estimates of female earnings relative to the year prior to the birth of their first child for the robustness tests. The interaction terms with childcare (*care*) indicate the differential effect if at the birth of the first child, there are childcare facilities in the municipality of residence. The interaction terms with subsidized childcare (*subs. care*) indicate the difference if at the birth of the first child, there are subsidized childcare facilities in the municipality of residence compared to if there is no childcare. Standard errors are bootstrapped (1000 replications). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.14: Robustness tests, men

	Stayer	Stayer subsid. care	Selection	+- 3 yrs around opening
	(1)	(2)	(3)	(4)
Event −3	-0.020	-0.017	-0.009	-0.041*
	(0.019)	(0.020)	(0.010)	(0.024)
Event −2	-0.011	-0.009	-0.010**	-0.016
	(0.010)	(0.010)	(0.005)	(0.016)
Event 0	0.041***	0.040***	0.037***	0.055***
	(0.013)	(0.013)	(0.008)	(0.017)
Event +1	0.061***	0.061***	0.042***	0.088***
	(0.018)	(0.018)	(0.009)	(0.022)
Event +2	0.062***	0.062***	0.046***	0.083***
	(0.020)	(0.021)	(0.011)	(0.024)
Event +3	0.082***	0.083***	0.052***	0.095***
	(0.023)	(0.025)	(0.013)	(0.031)
Event +4	0.075***	0.077***	0.057***	0.083**
	(0.026)	(0.027)	(0.016)	(0.037)
Event +5	0.085***	0.085***	0.069***	0.131***
	(0.028)	(0.029)	(0.019)	(0.043)
Event +6	0.076**	0.074**	0.079***	0.111**
	(0.032)	(0.033)	(0.024)	(0.052)
Event −3· care	-0.003	0.020	-0.031***	0.004
	(0.016)	(0.028)	(0.011)	(0.022)
Event −2· care	-0.006	0.015	-0.011	-0.007
	(0.010)	(0.015)	(0.008)	(0.016)
Event 0· care	-0.001	-0.011	0.007	-0.011
	(0.008)	(0.019)	(0.008)	(0.013)
Event +1· care	-0.014	-0.023	0.022**	-0.028*
	(0.013)	(0.025)	(0.009)	(0.016)
Event +2· care	-0.017	-0.020	0.026**	-0.032**
	(0.014)	(0.030)	(0.012)	(0.016)
Event +3· care	-0.019	-0.024	0.041***	-0.034*
	(0.016)	(0.025)	(0.015)	(0.020)
Event +4· care	-0.008	-0.010	0.039**	-0.018
	(0.019)	(0.029)	(0.016)	(0.024)
Event +5· care	-0.030	-0.033	0.028	-0.064**
	(0.021)	(0.034)	(0.021)	(0.029)
Event +6· care	-0.030	-0.072*	0.017	-0.033
	(0.025)	(0.039)	(0.025)	(0.035)
Event −3· subs. care		-0.010		
		(0.017)		
Event −2· subs. care		-0.012		
		(0.012)		
Event 0· subs. care		0.002		
		(0.009)		
Event +1· subs. care		-0.016		
		(0.013)		
Event +2· subs. care		-0.021		
		(0.015)		
Event +3· subs. care		-0.022		
		(0.018)		
Event +4· subs. care		-0.015		
		(0.020)		
Event +5· subs. care		-0.036		
		(0.022)		
Event +6· subs. care		-0.022		
		(0.027)		
Indiv. FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Married (t<0)	Yes	Yes	Yes	Yes
N indiv.	1460	1336	4310	760
N	15978	14648	41595	8102

Notes: Event study estimates of male earnings relative to the year prior to the birth of their first child for the robustness tests. The interaction terms with childcare (*care*) indicate the differential effect if at the birth of the first child, there are childcare facilities in the municipality of residence. The interaction terms with subsidized childcare (*subs. care*) indicate the difference if at the birth of the first child, there are subsidized childcare facilities in the municipality of residence compared to if there is no childcare. Standard errors are bootstrapped (1000 replications). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.2: Child penalty in municipalities in main sample compared to municipalities which do not have a childcare facility by the end of 2015



Note: Panel (a) visualizes the percentage effect of parenthood on female earnings relative to the year prior to the birth of the first child ($t = -1$) in municipalities that introduce childcare at some point (*Childcare*), in years before they do, and in municipalities that do not have childcare by the end of 2015 (*No childcare*). Confidence bounds are calculated applying a non-parametric bootstrap and are displayed at the 5 percent level. Panel (b) plots the estimated differences in the child penalty between the two groups of municipalities.

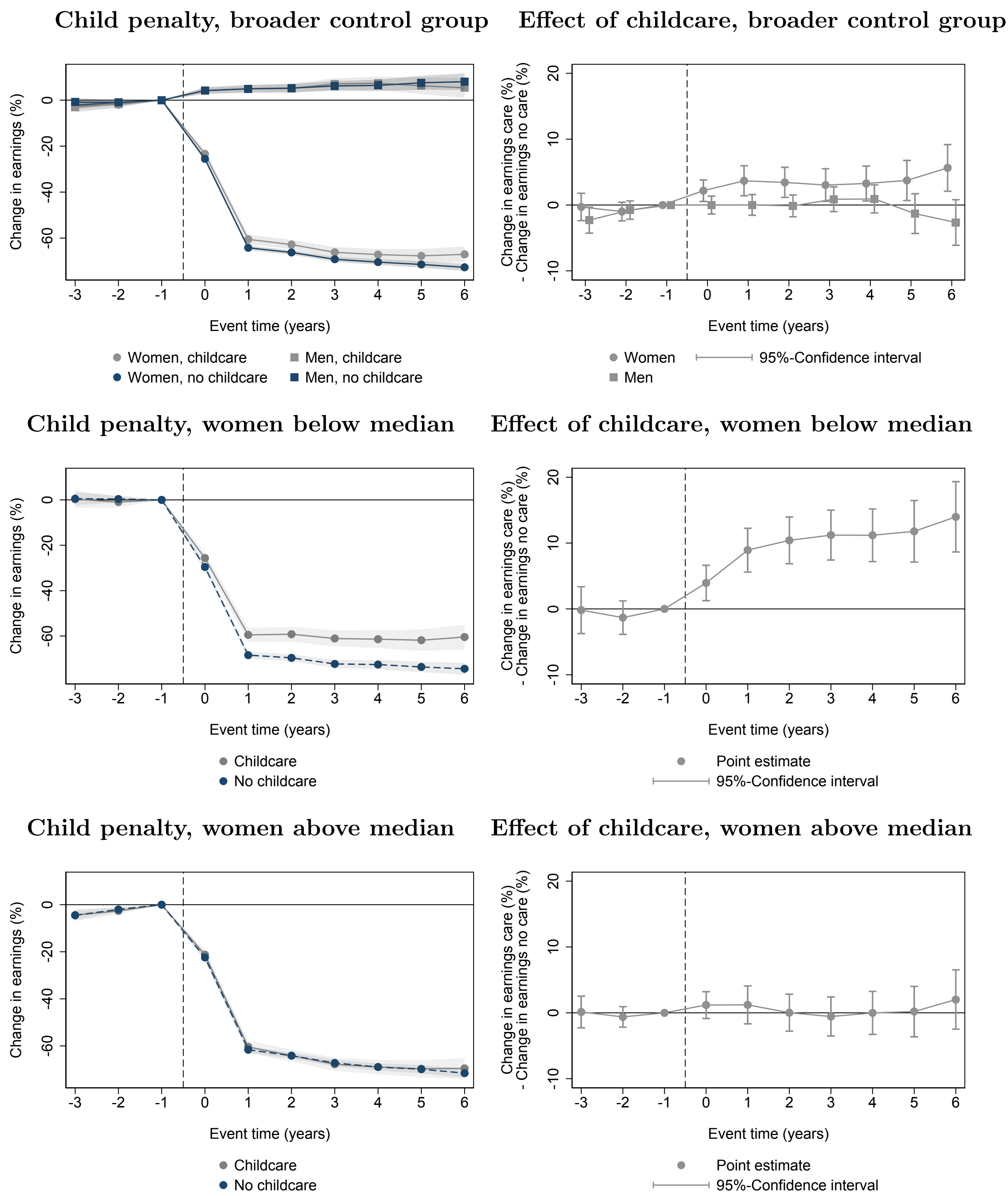


Figure A.3: Similar to analysis in Figures 3 and 5, but with a **broader control group** that also includes first births in municipalities that introduced childcare after 2015 or did not offer childcare by 2018. Effect of having a childcare facilities in the municipality of residence in the year of birth of the first child. The top panels show the effect for all households, the panels in the middle for households with pre-child earnings below the sample median, the bottom panels for households with pre-child earnings above the sample median. The panels on the left show the percentage effect of parenthood on household earnings relative to the year prior to the birth of the first child ($t = -1$). The panels on the right display the difference in female earnings by the presence of childcare. Confidence bounds are calculated at the 5 percent level of significance applying a non-parametric bootstrap.

Table A.15: Earnings penalty including municipalities which do not have a childcare facility by the end of 2015, women

	Overall	Childcare	Childcare & Hh inc.>p50	Childcare & Hh inc.≤p50	Subsidized childcare
	(1)	(2)	(3)	(4)	(5)
Event −3	-0.023***	-0.021**	-0.045***	0.005	-0.022***
	(0.008)	(0.008)	(0.010)	(0.014)	(0.008)
Event −2	-0.013***	-0.010**	-0.020***	0.004	-0.009**
	(0.004)	(0.004)	(0.005)	(0.008)	(0.004)
Event 0	-0.251***	-0.255***	-0.224***	-0.296***	-0.256***
	(0.005)	(0.005)	(0.007)	(0.009)	(0.006)
Event +1	-0.634***	-0.642***	-0.616***	-0.684***	-0.645***
	(0.004)	(0.005)	(0.007)	(0.007)	(0.005)
Event +2	-0.655***	-0.663***	-0.641***	-0.696***	-0.665***
	(0.004)	(0.005)	(0.007)	(0.008)	(0.005)
Event +3	-0.686***	-0.692***	-0.672***	-0.723***	-0.694***
	(0.005)	(0.005)	(0.007)	(0.009)	(0.006)
Event +4	-0.698***	-0.704***	-0.690***	-0.726***	-0.706***
	(0.006)	(0.006)	(0.008)	(0.010)	(0.007)
Event +5	-0.707***	-0.715***	-0.699***	-0.736***	-0.717***
	(0.007)	(0.007)	(0.009)	(0.012)	(0.007)
Event +6	-0.716***	-0.727***	-0.716***	-0.744***	-0.729***
	(0.008)	(0.008)	(0.010)	(0.014)	(0.008)
Event −3· care		-0.003	0.001	-0.002	-0.007
		(0.011)	(0.012)	(0.018)	(0.031)
Event −2· care		-0.010	-0.006	-0.013	-0.007
		(0.007)	(0.008)	(0.013)	(0.015)
Event 0· care		0.022***	0.012	0.039***	0.037*
		(0.008)	(0.010)	(0.014)	(0.022)
Event +1· care		0.037***	0.012	0.089***	0.007
		(0.012)	(0.015)	(0.017)	(0.034)
Event +2· care		0.034***	0.000	0.104***	0.013
		(0.012)	(0.014)	(0.018)	(0.033)
Event +3· care		0.030**	-0.006	0.112***	0.000
		(0.013)	(0.015)	(0.019)	(0.034)
Event +4· care		0.033**	-0.000	0.112***	0.010
		(0.013)	(0.017)	(0.020)	(0.036)
Event +5· care		0.037**	0.002	0.118***	0.024
		(0.016)	(0.020)	(0.024)	(0.041)
Event +6· care		0.056***	0.020	0.140***	0.029
		(0.018)	(0.023)	(0.027)	(0.038)
Event −3· subs. care					0.005
					(0.012)
Event −2· subs. care					-0.008
					(0.008)
Event 0· subs. care					0.020**
					(0.009)
Event +1· subs. care					0.041***
					(0.013)
Event +2· subs. care					0.037***
					(0.013)
Event +3· subs. care					0.032**
					(0.013)
Event +4· subs. care					0.037**
					(0.014)
Event +5· subs. care					0.041**
					(0.017)
Event +6· subs. care					0.064***
					(0.020)
Year FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Married (t<0)	Yes	Yes	Yes	Yes	Yes
N indiv.	4979	4979	2493	2486	4748
N	54558	54558	27882	26676	52048

Notes: Event study estimates of female earnings relative to the year prior to the birth of their first child, including municipalities which have not introduced childcare by the end of 2015. The interaction terms with childcare (*care*) indicate the differential effect if at the birth of the first child, there are childcare facilities in the municipality of residence. The interaction terms with subsidized childcare (*subs. care*) indicate the difference if at the birth of the first child, there are subsidized childcare facilities in the municipality of residence compared to if there is no childcare. Standard errors are bootstrapped (1000 replications). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

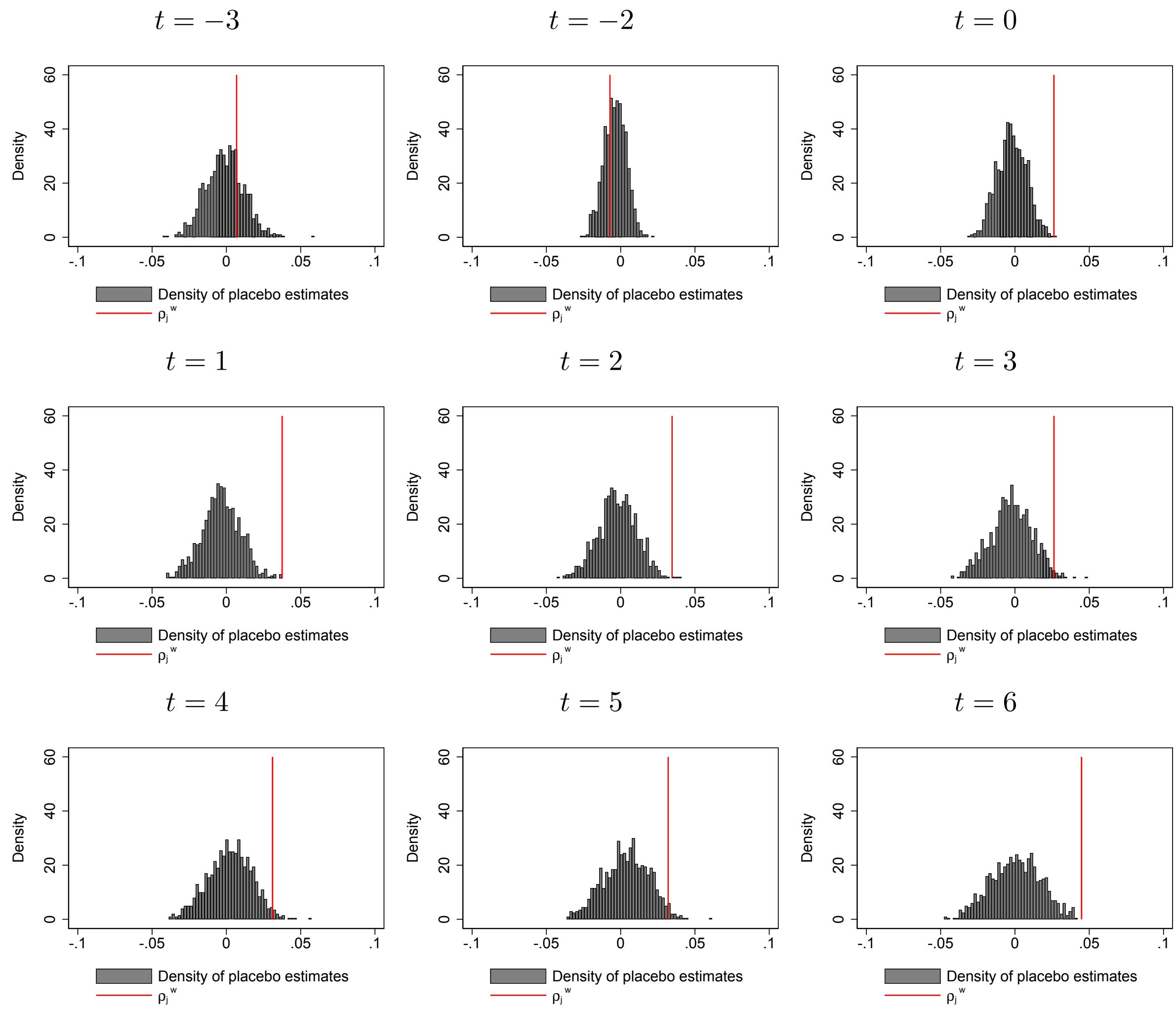
Table A.16: Earnings penalty including municipalities which do not have a childcare facility by the end of 2015, men

	Overall	Childcare	Childcare & Hh inc.>p50	Childcare & Hh inc.≤p50	Subsidized childcare
	(1)	(2)	(3)	(4)	(5)
Event −3	-0.013 (0.009)	-0.008 (0.009)	-0.033*** (0.012)	0.028** (0.014)	-0.005 (0.009)
Event −2	-0.011*** (0.004)	-0.010** (0.005)	-0.020*** (0.006)	0.002 (0.007)	-0.008* (0.005)
Event 0	0.041*** (0.006)	0.042*** (0.007)	0.013 (0.009)	0.075*** (0.010)	0.040*** (0.007)
Event +1	0.049*** (0.007)	0.049*** (0.008)	0.011 (0.010)	0.099*** (0.012)	0.047*** (0.008)
Event +2	0.051*** (0.009)	0.052*** (0.009)	0.010 (0.012)	0.110*** (0.014)	0.049*** (0.009)
Event +3	0.063*** (0.011)	0.062*** (0.011)	0.012 (0.015)	0.134*** (0.018)	0.058*** (0.011)
Event +4	0.065*** (0.012)	0.064*** (0.013)	0.009 (0.017)	0.149*** (0.020)	0.060*** (0.013)
Event +5	0.072*** (0.015)	0.076*** (0.016)	0.024 (0.022)	0.157*** (0.023)	0.070*** (0.017)
Event +6	0.074*** (0.018)	0.080*** (0.019)	0.025 (0.026)	0.179*** (0.028)	0.075*** (0.020)
Event −3· care		-0.023** (0.010)	-0.020 (0.013)	-0.014 (0.016)	-0.012 (0.027)
Event −2· care		-0.008 (0.007)	-0.014 (0.008)	0.007 (0.011)	0.009 (0.016)
Event 0· care		-0.000 (0.007)	-0.004 (0.009)	0.007 (0.010)	-0.014 (0.019)
Event +1· care		0.000 (0.008)	-0.007 (0.010)	0.011 (0.013)	-0.002 (0.025)
Event +2· care		-0.001 (0.008)	-0.003 (0.011)	0.001 (0.014)	0.003 (0.029)
Event +3· care		0.009 (0.010)	0.009 (0.011)	0.004 (0.016)	0.014 (0.022)
Event +4· care		0.009 (0.011)	0.004 (0.012)	0.017 (0.019)	0.015 (0.027)
Event +5· care		-0.013 (0.015)	-0.026 (0.019)	0.005 (0.020)	-0.005 (0.033)
Event +6· care		-0.027 (0.018)	-0.019 (0.021)	-0.047* (0.027)	-0.056 (0.036)
Event −3· subs. care					-0.029** (0.012)
Event −2· subs. care					-0.014* (0.008)
Event 0· subs. care					0.001 (0.007)
Event +1· subs. care					0.000 (0.008)
Event +2· subs. care					-0.002 (0.009)
Event +3· subs. care					0.008 (0.010)
Event +4· subs. care					0.007 (0.012)
Event +5· subs. care					-0.017 (0.017)
Event +6· subs. care					-0.022 (0.020)
Year FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Married (t<0)	Yes	Yes	Yes	Yes	Yes
N indiv.	4979	4979	2493	2486	4748
N	54558	54558	27882	26676	52048

Notes: Event study estimates of male earnings relative to the year prior to the birth of their first child, including municipalities which have not introduced childcare by the end of 2015. The interaction terms with childcare (*care*) indicate the differential effect if at the birth of the first child, there are childcare facilities in the municipality of residence. The interaction terms with subsidized childcare (*subs. care*) indicate the difference if at the birth of the first child, there are subsidized childcare facilities in the municipality of residence compared to if there is no childcare. Standard errors are bootstrapped (1000 replications). Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

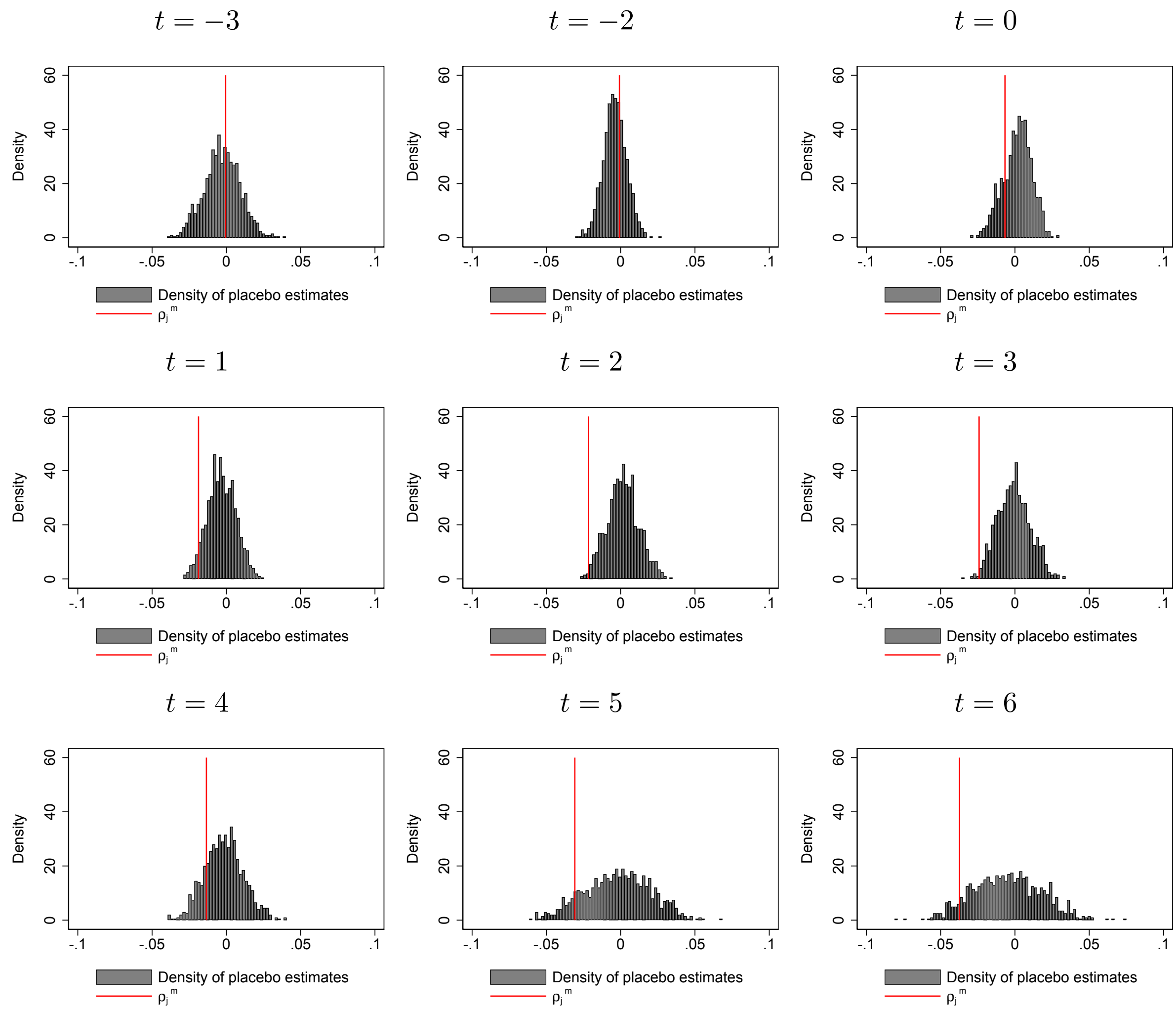
Histograms for placebo test

Figure A.4: Histograms of placebo test, women



Note: Results of the placebo test where opening dates of childcare are randomly assigned to municipalities that do not have childcare for women. Based on these placebo opening dates, we replicate the analysis of the effect of childcare on earnings based on this sample. The densities are the result of 1000 random assignments of opening dates. The red lines show the actual estimates of the effect of childcare.

Figure A.5: Histograms of placebo test, men

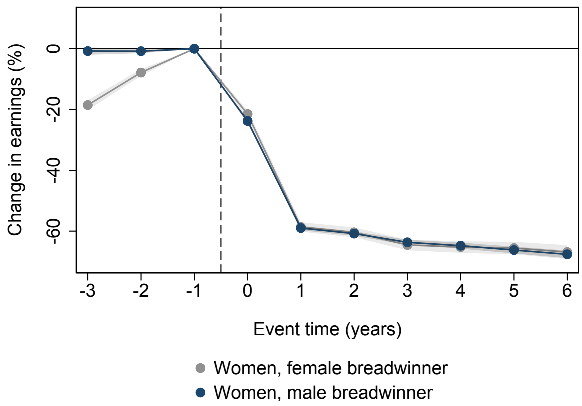


Note: Results of the placebo test where opening dates of childcare are randomly assigned to municipalities that do not have childcare for men. Based on these placebo opening dates, we replicate the analysis of the effect of childcare on earnings based on this sample. The densities are the result of 1000 random assignments of opening dates. The red lines show the actual estimates of the effect of childcare.

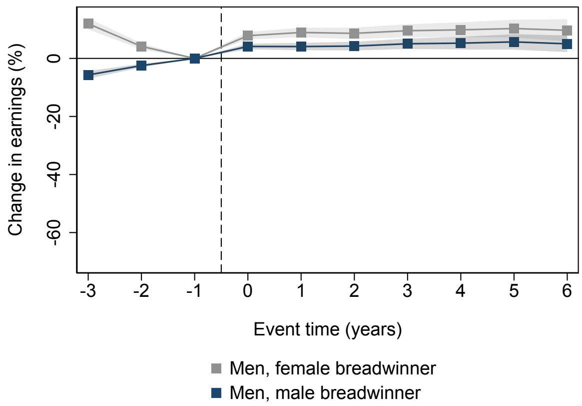
Figure A.6: Child penalty of households depending on the gender of the breadwinner in the household

Breadwinner earns > 50% of hh earnings

a) **Female earnings penalty**

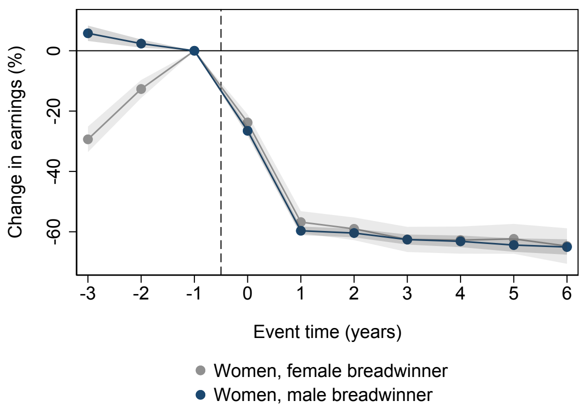


b) **Male earnings penalty**

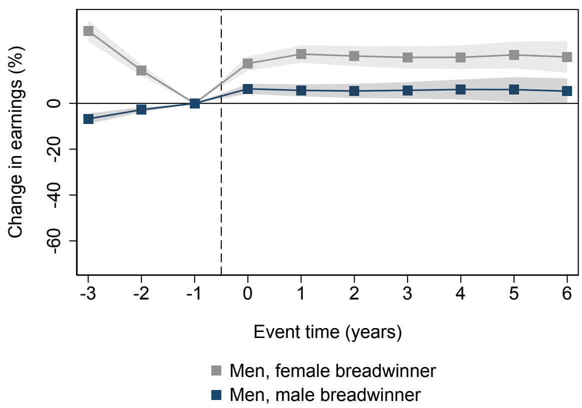


Breadwinner earns > 60% of hh earnings

c) **Female earnings penalty**



d) **Male earnings penalty**



Note: Comparison of child penalty among couples with female breadwinner and couples with male breadwinner. The panels on the left display child penalties. Panel a) displays the child penalty for women, depending on whether the man or the woman is the breadwinner (earns more) in period $t = -1$. Panel b) reports the child penalty for men and depending on who is the breadwinner in in period $t = -1$. Panels c) and d) are the same as above, except that breadwinners are defined as earning at least 60% of the couple's combined earnings in $t = -1$. Note that, these estimates use all couples independent of whether they had their first child in a municipality with childcare. Confidence bounds are calculated at the 5 percent level of significance applying a non-parametric bootstrap. The corresponding estimates are displayed in Table A.17.

Table A.17: Child penalty conditional on gender of the breadwinner in the household

	Breadwinner > 50%		Breadwinner > 60%	
	Women	Men	Women	Men
	(1)	(2)	(3)	(4)
Event −3	-0.008	-0.057***	0.058***	-0.067***
	(0.006)	(0.007)	(0.013)	(0.012)
Event −2	-0.008**	-0.025***	0.024***	-0.028***
	(0.003)	(0.003)	(0.007)	(0.006)
Event 0	-0.238***	0.041***	-0.265***	0.063***
	(0.004)	(0.005)	(0.008)	(0.011)
Event +1	-0.590***	0.041***	-0.597***	0.057***
	(0.004)	(0.006)	(0.007)	(0.014)
Event +2	-0.608***	0.043***	-0.604***	0.054***
	(0.004)	(0.008)	(0.007)	(0.016)
Event +3	-0.637***	0.050***	-0.626***	0.057***
	(0.004)	(0.009)	(0.008)	(0.018)
Event +4	-0.648***	0.053***	-0.632***	0.061***
	(0.005)	(0.011)	(0.010)	(0.022)
Event +5	-0.662***	0.057***	-0.644***	0.060**
	(0.006)	(0.014)	(0.011)	(0.028)
Event +6	-0.676***	0.050***	-0.650***	0.053*
	(0.006)	(0.015)	(0.013)	(0.029)
Event −3· female breadwinner	-0.178***	0.177***	-0.351***	0.384***
	(0.008)	(0.008)	(0.024)	(0.019)
Event −2· female breadwinner	-0.070***	0.066***	-0.151***	0.172***
	(0.006)	(0.006)	(0.016)	(0.013)
Event 0· female breadwinner	0.023***	0.037***	0.028**	0.110***
	(0.006)	(0.005)	(0.014)	(0.013)
Event +1· female breadwinner	0.004	0.048***	0.029	0.158***
	(0.008)	(0.007)	(0.019)	(0.015)
Event +2· female breadwinner	0.004	0.044***	0.014	0.153***
	(0.009)	(0.008)	(0.020)	(0.017)
Event +3· female breadwinner	-0.009	0.045***	0.000	0.144***
	(0.009)	(0.008)	(0.022)	(0.018)
Event +4· female breadwinner	-0.005	0.046***	0.004	0.140***
	(0.010)	(0.009)	(0.024)	(0.018)
Event +5· female breadwinner	0.007	0.046***	0.021	0.151***
	(0.010)	(0.012)	(0.025)	(0.021)
Event +6· female breadwinner	0.007	0.047***	0.003	0.149***
	(0.011)	(0.014)	(0.030)	(0.025)
Indiv. FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Married (t<0)	Yes	Yes	Yes	Yes
N indiv.	10355	10355	4250	4250
N	112351	112351	45384	45384

Notes: Event study estimates of female and male earnings relative to the year prior to the birth of their first child. The interaction terms with a female breadwinner dummy indicate the differential effect between couples with a female breadwinner and couples with male breadwinner in period $t-1$. The sample includes couples in all municipalities, independent of information on childcare. In columns (1) and (2), breadwinners are defined as accounting for more than 50% of a couple's earnings, whereas in columns (3) and (4), we only use couples, where the breadwinner accounts for more than 60% of combined earnings. The breadwinner is female in 22.3% of all cases in columns (1) and (2) and in 14.7% of all cases in columns (3) and (4). Standard errors are bootstrapped (1000 replications). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.