Journal of Family and Economic Issues (2019) 40:323–337
https://doi.org/10.1007/s10834-019-09609-3

ORIGINAL PAPER

The Effect of Maternity Leave Expansions on Fertility Intentions: Evidence from Switzerland

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Published online: 31 January 2019

Abstract

We study the effect of the expansion of the mandatory paid maternity leave, implemented in Switzerland in 2005, on individuals' fertility intentions. Earlier literature found evidence of fertility increases induced by maternity leave expansions from other countries of a relatively large magnitude of 1 year. The expansion that we consider was smaller, from 8 unpaid weeks to 14 mandatory paid weeks, and thus its effect on fertility decisions is less evident ex ante. Nevertheless, we find that it positively impacts fertility planning even though, by construction, our model specification cannot capture its full effect. The strongest effects are elicited in the subsamples of men, individuals with two children, and individuals aged between 31 and 36. There are several channels through which the maternity leave expansion may affect individuals' child planning, all indicative of a positive effect on the fertility rate.

Keywords Maternity leave · Child planning · Fertility

JEL Classification D04 · J13 · J18 · N34

Introduction

Birth rates in most European countries have been below replacement levels for several decades (OECD 2017a). Combined with an increasing life expectancy, this has led to the aging of the population, raising concerns among policy makers about potential adverse long term socio-economic consequences. To address this issue, governments have attempted to stimulate fertility, primarily by means of lowering the economic costs of bearing and raising a child, with a key policy tool in this respect being laws that define maternity leave benefits (OECD 2017b). Expansions of maternity benefits have been implemented throughout most of Europe over the past 30 years. Due to the significant costs incurred by these expansions on the government and private sector entities, an important public policy question is whether they have made a contribution towards an increased fertility rate.[1] Our paper examines this question in the context of the expansion of the maternity benefits (MB) implemented in Switzerland in 2005, which included an expansion from 8 weeks of maternity leave, which depending on the choice of the employer, could be paid or unpaid, to 14 weeks of mandatory paid leave. We investigate the effect of this policy reform on individuals' medium-term fertility intentions, as captured by their self-declared likelihood that they plan to conceive a child in the 3 years following the date they were interviewed.

Several earlier papers examined the effect on fertility outcomes or fertility intentions of changes in the duration of the paid parental leave from Austria (Lalive and Zweimuller 2009), Russia (Malkova 2017) and Australia (Bassford and Fisher 2016). They found that the relatively large 1 year extensions implemented in Austria and Russia led to significant increases in fertility rates, whereas the smaller extension implemented in Australia had a more ambiguous impact. The MB expansion implemented in Switzerland in

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[1] While cost considerations, particularly to private enterprises, play a secondary role in the public debate surrounding the topic of maternity leave extensions from European countries, they are the main consideration in the corresponding debate in the United States, where concerns about the national fertility rate are currently less serious (Averett and Wittington 2001).

2005 was of a relatively small magnitude, and thus its effect on fertility choices is less evident ex ante.

Since MB expansions, including the one from Switzerland that we investigate, typically provide potential benefits to the whole population of child-bearing age, disentangling the effect of a particular policy change from an underlying trend often poses an identification challenge in the absence of a valid control group. In our study, we circumvent this issue by examining instead the effect of *experiencing* the expanded MB. Note that a MB expansion can potentially impact an individual's fertility intentions in two ways. First, if the individual is aware of the policy change and thus these newly introduced benefits are expected, there is a potential "anticipation effect" occurring before the individual conceives a child. Second, there may also exist an "experience effect," occurring after the child is born and the expanded MB are enjoyed; the individual can learn of these new benefits if he had not been aware before, or accurately assess their actual impact on reducing the cost of child bearing. This "experience effect" may then impact the individual's fertility intentions regarding higher order children.

Our analysis aims to investigate whether such an "experience effect" existed in the context of the MB expansion we consider. More specifically, we test the hypothesis that the net change in the fertility intentions of past beneficiaries of the reform after its implementation i.e., individuals who have already experienced the expanded MB, is higher than the net change in the fertility intentions of the individuals who were eligible for these benefits, but who have not yet experienced the benefits. If confirmed, this hypothesis would imply a successful policy reform, even if our exercise would not capture its unidentifiable full effect on the fertility intentions which needs to also capture the "anticipation effect."

We examine our hypothesis by estimating a difference-in-differences model, employing data from two waves of the European Social Survey (ESS) from the years 2004 and 2010. The treatment group in this estimation consists of individuals who conceived a child in the 6 years prior to the time of the interview. A subset of the individuals from the treatment group interviewed in 2010 experienced the expanded MB because of the policy reform.[2] As a control group, we utilize individuals with no children at the time of the interview. While eligible for the expanded MB in 2010, and thus possibly affected by the "anticipation effect" of the policy change, such individuals have not yet experienced them. The role of this control group in our analysis is to capture the counterfactual underlying trend in fertility intentions between 2004 and 2010 that would have materialized in the absence of the MB expansion. Note however that the composition of both the treatment and control groups may have changed specifically because of the reform. More precisely, some individuals who in the absence of the MB expansion would have conceived no children by 2010, and thus be in the control group, may have been induced by the MB expansion to conceive a child by 2010. This is one of the channels through which the policy change may have affected fertility planning and we account for it in our interpretation of the results.

To test the validity of the control group in capturing the underlying trend in the absence of a policy change, we run a placebo test on data from France and Germany, the two neighboring countries of Switzerland, for which the available data allow estimating the same model. The test shows no significant difference in trends between the two groups in those two other countries in the absence of a policy change.

While most benefits introduced by the new law applied exclusively to mothers, we also examine the effect of this law on men's fertility intentions, as men also benefit from a longer maternity leave, even if only indirectly, and their willingness to conceive a child plays a role in a couple's fertility decision.

The findings from our estimations provide evidence of a differential change in fertility intentions between the two waves in the treatment group relative to the control group, suggestive of a positive effect of the implementation of the MB expansion on fertility intentions. An analysis of the heterogeneity of responses elicits the strongest effects in the subsample of men, the subsample of individuals with two children, and the subsample of individuals aged between 31 and 36. The key interaction coefficient is however positive in most subsamples, and is statistically significant in other subsamples.

An evaluation of the channels though which this effect may take place identifies three possibilities. The first channel corresponds to a "behavioral effect" by which individuals who have already experienced the benefits became more likely to plan higher order children either because they became aware of these new benefits or because they learned their impact on their child bearing costs.

The second possible channel is determined by the specific dependent variable that we employ in the analysis, which elicits the medium term individuals' fertility intentions i.e., for the 3 years following the date of the interview, rather than their lifetime fertility intentions. It is possible that individuals who had a child after the reform were more likely to reduce the spacing between higher order births, as the extended maternity leave for a future child may also reduce

[2] Some individuals benefited from a paid maternity leave as generous as the one implemented by the policy reform even before 2005, either because their employers offered them voluntarily, or because they lived in the Bern canton which was mandating these more generous benefits even before 2005 (the dataset we employ does not allow identifying either of these two categories of individuals, and therefore they cannot be used as a control group). Our analysis thus elicits intended-to-treat effects.

the cost of raising the existing older children. An individual who recently had a child and who, with a reduced maternity leave, would have planned another one later than the 3 years subsequent to the date of the interview, may choose to plan the future child sooner. This is because the benefit on the current child of the extended leave due to be received because of the future child, is higher when the current child is younger. This effect is referred to in Lalive and Zweimuller (2009) as the "future child effect" in the context of the maternity leave expansion from Austria. However, unlike the expansion from Austria, which increased the maternity leave from 1 to 2 years, the expansion from Switzerland is of a much smaller magnitude, from 8 to 14 weeks. While the additional 6 weeks can significantly reduce the cost of bearing a newborn child, their effect on reducing the cost of raising an older child is arguably less significant. If this effect is present in our case, it is thus likely to be less strong than the one that Lalive and Zweimuller (2009) measure in their analysis.

Finally, a third possible channel is created by the possibility that some individuals may have been induced to conceive a child earlier by the expanded maternity leave, and thus be in the treatment group by the time of the second wave interview specifically because of the policy reform. The key result from our difference-in-differences estimation may be generated in this manner for certain prior distributions of fertility intentions in the population. In our analysis of this channel, we argue that if the policy reform affected fertility intentions in this manner, then it could not have been determined solely by individuals who would complete their fertility by 2010. In other words, some individuals who had a child earlier because of the reform must have continued to plan higher order children after 2010.

If the maternity leave reform affected fertility intentions primarily through the behavioral channel, it would determine a clear increase in long term fertility rate.[3] On the other hand, if the effect that we uncover is determined solely through the latter two channels, then, in principle, it could be that the reform only changed the timing of an individual's child bearing rather than the total number of children. Nevertheless, as Lalive and Zweimuller (2009) argue, any policy change that induces individuals to conceive children earlier is likely to increase long term fertility, since having a child earlier alleviates the potential adverse effects to fertility induced by shocks to health, relationships, or economic circumstances that can emerge in long term.[4]

While our data do not allow disentangling these possible channels, or measure the full effect of the policy change, the analysis suggests that despite the Swiss expansion of the maternity leave being significantly smaller in magnitude than the expansions studied in other articles, it does have a positive impact on individuals' child planning, likely to determine an increase in fertility.

The paper is structured as follows. In the following section, we present a summary of the relevant existing literature. Then, we provide background information regarding the context and content of the policy reform. We further present the empirical model and the identification strategy, and describe the data used in this study. The results of the analysis are presented. Finally, we discuss the possible channels through which the reform may have affected fertility intentions and conclude.

Literature Review

Lalive and Zweimuller (2009) investigated the effect of the 1990 extension by 1 year, and subsequent 1996 reduction, in the length of the parental leave in Austria on women's higher-order fertility choices and subsequent career outcomes. As their main results, they found that following the 1990 extension, short term fertility (within 3 years) increased by about 36% relative to the baseline, while longer term fertility (between 3 and 10 years) also increased. When evaluating the effect on higher-order fertility, they distinguished between the so-called "current child effect" and "future child effect." The former is a consequence of the relatively long maternity leave offered in Austria (1–2 years) and of the fact that according to the Austrian law, a mother is exempt from the work requirement, typically imposed for applying for maternity leave benefits, if she gives birth to another child within a certain period after the expiration of the maternity leave offered for giving birth to the previous child. No such regulations exist in the Swiss maternity leave laws, and thus this effect cannot emerge in our analysis. On the other hand, the "future child effect" is due to the fact that a longer maternity leave for a future child also reduces the cost of raising the current child, and this effect is stronger the younger the current child is. This is one of the channels through which the results in our study can be generated.

Malkova (2017) studied the short-term and long-term fertility responses of a cash benefit and a 1 year paid maternity leave implemented in Soviet Russia starting with 1981. The paper showed that fertility increased by approximately 10%

[3] Bassford and Fisher (2016) present a review of evidence that fertility intentions predict fertility outcomes from studies such as Morgan (2001), Schoen et al. (1999), and Berrington (2004).

[4] On the other hand, several studies showed an adverse effect of a reduction in birth spacing on children's educational attainment (Pettersson-Lidbom and Thoursie 2009; Buckles and Munnich 2012; Hill and Slusky 2017), and on the mother's labor outcomes (Karimi 2014; Gough 2017).

in the year following the implementation, and continued to stay at that elevated level in the long term over a 10 year period evaluated in the analysis.

Besides the fact that we investigate the effect of a maternity leave reform from a different country, there are two key differences between our study and this earlier literature. First, we examine the effect of an increase in the maternity leave duration of a much smaller magnitude, for which the individuals' response is ex-ante less evident. A second difference is that our analysis studies the effect of a maternity leave extension on fertility intentions rather than fertility outcomes. While outcomes are generally more relevant from a policy perspective, in this context, due to the significant delay with which individuals' preferences over fertility outcomes can typically be implemented, real outcomes may also be affected by various exogenous shocks. The fertility intentions allow thus for a less noisy measurement of the impact of the policy reform on individuals' preferences.

Closer to our study, the effect of an increase in the duration of the maternity leave of a relatively smaller magnitude was studied in Bassford and Fisher (2016), who examined the impact of a newly introduced 18 weeks paid maternity leave in Australia in 2001. They found that this policy change did not raise the probability of a woman's intending to bear a child, but that for those women who did intend to have children, the planned number of children increased on average by 13% relative to the baseline level.

Several other papers have presented findings relevant to our study. Averett and Wittington (2001) and Cannonier (2014) showed that the introduction of an *unpaid* mandatory parental leave increased fertility in the United States. Another stream of literature studied the effect of financial incentives on fertility decisions. Cohen et al. (2013) studied the effect of an increase in the child subsidy received from the Israeli government, showing that it increased the probability that a woman conceives a child in a given year. Milligan (2005) and Ang (2014) found that the introduction in the Canadian province of Quebec of a cash subsidy for the birth of a child, and the implementation of a financially more generous paid maternity leave, respectively, were both associated with higher birth rates. Cygan-Rehm (2016) studied the effect of a policy shift of parental leave benefits from a means tested scheme, aimed primarily at lower income mothers, to a payment scheme that substituted pre-birth earnings, and thus offered more benefits to higher income individuals. Their main finding was that low-income mothers, whose benefits were reduced, extended the spacing of their higher order births without seeming to catch up later, possibly leading to a reduced fertility rate in this group.

To summarize, earlier literature examining the impact of maternity leave expansions on fertility found significant effects of the relatively large extensions implemented in Austria and Russia, but more ambiguous effects of a smaller expansion implemented in Australia. Our contribution to this literature is twofold: first, our subject of study is a different expansion implemented in Switzerland, and second, we show that even though this expansion was of a relatively small magnitude, it is likely to positively impact the fertility rate, even though we cannot estimate the full effect of this policy change.

Background Information

Some form of maternity benefits has been available in Switzerland since 1945, when voters approved in a referendum to include such benefits in the Swiss Constitution. Prior to the reform from 2005, the mothers of a newborn were entitled by law to 8 weeks of maternity leave, which, depending on the choice of her employer, could be paid or unpaid. This rule set a lower bound on MB, but certain public or private employers were voluntarily offering up to 16 weeks of fully paid maternity leave ("Maternity leave..." 2005). Additionally, a mandatory paid maternity leave of 16 weeks was in effect prior to 2005 in the Bern canton.[5] After being rejected at the federal level in four previous referendums, a law mandating universal paid maternity leave was approved in a referendum in September 2004, and announced on November 24th, 2004 to be implemented starting with July 1st, 2005. The new entitlement is funded by a proportional tax on wages, with equal contribution from the employer and the employee. With this change of legislation, mothers became entitled to 14 weeks of job-protected, paid maternity leave that starts on the day of birth of the baby. Women are required by law to take a maternity leave of 8 weeks, while the remaining 6 weeks are optional. Additionally, after the expiration of the fourteenth week, a mother can take additional 2 unpaid weeks of leave. Fathers are not entitled to any paid parental leave, and neither parent is entitled to any additional unpaid homecare leave (OECD 2017b).

The eligibility for MB extends to both natural and adoptive mothers. To claim these benefits, a new mother must be either employed for at least 5 months during pregnancy (full-time or part-time), self-employed, or, if unemployed, then she must have been receiving social security or disability benefits. Additionally, she must have had public health insurance for 9 months prior to birth in Switzerland, or any country of the European Union or of the European

[5] The new maternity leave policy did not apply to mothers residing in Bern since their prior benefits were more generous. They also did not actually provide benefits to new mothers working for employers that had been offering benefits who were at least as generous as the mandatory minimum levels before July 1st, 2005. Since the observations in our dataset do not have geographical information, we cannot use individuals from the Bern canton as a control group.

Free Trade Association (Ray 2008). A mother who returns to work prior to the expiration of the maternity leave, loses her eligibility. During the maternity leave period, mothers are compensated at a level of up to 80% of their previous earnings.[6] While the extension of the mandatory paid maternity leave constitutes the main benefit implemented by the policy reform of 2005, several other maternity benefits have been introduced into law on this occasion. Employers are not allowed to fire women during their maternity leave. During the whole pregnancy, a woman cannot be asked to work more than 9 h per day, during the night, or under hazardous working conditions, and after her sixth month of pregnancy, the maximum number of hours that a woman can work reduces to six. Employers are also obliged to provide mothers a couch and a 1 h nursing break at the workplace until the baby turns one (Ray 2008). Both parents of a child under 15 can refuse working overtime, and can request a 90 min lunch break.

Empirical Strategy and Identification

Our analysis relies on a difference-in-differences estimation in which the treatment group consists of individuals with at least one child born during the 6 years prior to the interview, and the control group consists of individuals with no children. Thus, treated individuals interviewed in 2004 had at least one child born in one of the years 1999–2004, while treated individuals interviewed in 2010 had at least one child born in one of the years 2005–2010. A subset, but not all, of the individuals from the latter group experienced the benefits of the expanded maternity leave implemented starting with 2005. [7] Some of the individuals interviewed in 2004 were already benefiting from a paid maternity leave at least as generous as the one mandated in the new law if they were working for employers offering such benefits even before 2005, or, in case of men, if their partners were in that situation. The key coefficient that we obtain in our estimation, which elicits the effect of the MB expansion, is thus likely biased towards zero.

The identifying assumption, which allows us to disentangle the effect of the policy reform from an underlying trend in fertility intentions, is that in the absence of this reform, the treatment and control groups would have moved on the same trend. The validity of this assumption is verified with a placebo test on data from France and Germany, the two countries neighboring Switzerland for which the EES dataset allows performing this test. It is worth mentioning here that, *due to the reform*, some individuals who would otherwise be in the control group at the time of the interview in 2010 may have moved into the treatment group i.e., they may have conceived a child by 2010. We account for this possibility when discussing the possible channels that can generate our key findings. The placebo test only shows that the two groups would have moved on the same trend in the absence of an exogenous shock.

Since our dependent variable is ordinal, we estimate an ordered Logit regression as follows:

$$\Pr(PlanChild_i = j|X_i) = f(\delta_1 After_i + \delta_2 Treated_i + \delta_3 After_i \times Treated_i + X_i^{'}\beta, \propto_j, \propto_{j+1})$$

where j is one of the possible ordered responses to the interview question that elicited individuals' fertility intentions (or a transformation of it), while $\propto_j$ and $\propto_{j+1}$are the underlying threshold parameters from the latent variable model. The variable *Treated* equals one if the individual belongs to the treatment group, and zero otherwise. The variable after equals one if the individual is interviewed in 2010 i.e., after the MB expansion from 2005, and equals zero if the individual is interviewed in 2004. $\boldsymbol{X_i}$ is the vector of control variables for individual i, which includes gender, age, number of children, income bracket, education level, number of hours worked per week, relationship status, and age of partner, if a partner exists. In our main modeling specification, we estimate a Logit regression. As a robustness check, we verify that the marginal effects are similar to those from a Probit regression.

The causal impact of the policy change on individuals who have experienced benefits from the reform is reflected in the coefficient $\delta_3$of the interaction term. This coefficient captures the differential effect of the policy on the fertility intentions of individuals who have enjoyed the expanded MB relative to those that have not yet enjoyed them. $\delta_3$does not capture the full effect of the policy on fertility intentions, though, because individuals who have not experienced the

[6] At the time of the policy change in 2005, most cantons did not set a universal ceiling on the payments. The exception was canton Thurgau, which set a ceiling of CHF172/day (https://www.swissinfo.ch/eng/maternity-benefit-finally-sees-light-of-day/8578). Currently, a universal ceiling of CHF196/day is in place everywhere. The resulting average payment rate in 2016 was 56.4% of the mother's previous earnings (OECD 2017b).

[7] The new maternity leave benefits that were to be offered starting with July 1st, 2005 were announced on November 24th, 2004. Women who gave birth to children between the date of the announcement and July 1st, 2015 were eligible to claim the paid maternity leave starting with July 1st, 2005 (https://www.swissinfo.ch/eng/maternity-benefit-to-become-reality-from-july/4215604) but only for the remaining number of weeks up to 14 from the day of the birth. For instance, a mother who gave birth to a child 2 weeks before July 1st, was eligible for only the 12 weeks of paid maternity leave instead of 14. Thus, women who gave birth to children after April 1st, 2005 experienced these expanded benefits, at least partially, provided that their employer had not offered them before the reform. To obtain a larger treatment group, we included the respondents who had a child in 2005 in the analysis, at the expense of having the key coefficient potentially biased towards zero.

benefits after 2005 may have still been aware of them, and thus their child planning may have been influenced by the implementation of the policy. Nevertheless, a positive and significant coefficientδ_3 suggests a successful policy reform implementation through at least other channels.

Data and Descriptive Statistics

Variables

Our data consist of a repeated cross-section dataset extracted from the 2004 and 2010 waves of the European Social Survey (ESS). This is an academically-driven annual survey designed to study changes in behavior and perceptions of the general population in Europe, with the aim of encouraging and facilitating the research of academics and policy-makers in social sciences. Participants are interviewed face-to-face and have to answer questions regarding their behavior, attitudes, perceptions, and beliefs. Since 2002, when the ESS was initiated, 36 countries have taken part in at least one of its rounds. There are slight differences in the questions that participants are asked in each round. For instance, only Round two (2004) and Round five (2010) contain information about the individuals' fertility intentions, which explains our choice of utilizing data from these two rounds.

Our analysis is performed on individuals aged between 25 and 42 years at the time of the survey. The typical ages at which individuals conceive children range from 21 to 45,[8] and in fact, EES only elicits the fertility intentions of individuals that are at most 45 years old. The lower end of the age group that we consider in our analysis is chosen to allow for sufficient individuals in the treatment group for each age, so as to increase the similarity between the treatment and control groups. The higher end allows for an additional period of 3 years of potential child bearing age (the fertility intentions measured in the survey are over 3 years after the interview). The results are robust to the choice of the age bracket, although the value of the key interaction coefficient decreases when more individuals outside of the prime age for child bearing are included in the analysis. This is expected since these individuals are less likely to contemplate conceiving a child in the near future, and thus to respond to the policy reform.

The key dependent variable of interest in the paper is a categorical variable indicating an individual's fertility intentions in the 3 years following the time of the interview. After dropping observations with uninformative answers about the individual's fertility intentions, such as "Not Applicable," "Refusal," or "No Answer," we are left with five possible ordered answers: "Definitely Not," "Probably Not," "Don't Know," "Definitely Yes," and "Probably Yes." Based on these answers, we construct three categorical variables that are employed in the empirical analysis as dependent variables. The first of these variables considers all these five answers separately, taking thus five possible values. The second variable combines the two responses "Definitely Not" and "Probably Not" into one value, and the two responses "Definitely Yes" and "Probably Yes" into another value. This second variable can take thus three possible values. Finally, the last categorical dependent variable is constructed from the preceding variable by attributing a missing value to entries with answer "Don't Know." The corresponding observations are therefore dropped from the analysis, when employing this variable, which can thus take two values. We estimate our model and report the results for each of the three specifications of the dependent variable.

The complete set of variables used in the analysis is presented in the appendix. The conditioning variables elicit information regarding the number of children, gender, age, relationship status, age of partner if in a relationship, income, education, and number of hours worked in a week.

After extracting the observations as outlined above, the Swiss sample contains 593 respondents in the 2004 wave (215 in the treatment group, and 378 in the control group), and 309 respondents in the 2010 wave (132 in the treatment group, and 177 in the control group), for a total of 902 observations. Women are 302 and 138, in 2004 and 2010, respectively, for a total of 440, or 44.80% of the sample.

Summary Statistics

Table 1 provides summary statistics of the variables used in the analysis, categorized by the four groups employed in the difference-in-differences estimation. The average number of children for the individuals in the treatment group is 1.87, whereas the individuals in the control group have no children, by the design of this group. The treatment group has a higher proportion of women than the control group, 53.60% versus 45.76%.[9] Not surprisingly, individuals in the treatment group also tend to be slightly older: 34.53 years versus 32.36. Looking more closely at the distribution of the individuals across age groups, the treatment group has more individuals in the upper age brackets: 49.57% in the 31–36

[8] See, for instance, page 6 in OECD (2017c).

[9] This could be explained by the fact that women tend to have children earlier in life than men since in the typical couple, the woman is younger. In the fixed age bracket that we consider, a person with a child is thus more likely to be a woman, whereas one without is more likely to be a man. Another possible explanation is that men answered that they do not have a child if they did not have custody of their children, which in a case of separation of parents is predominantly the case.

Table 1 Summary statistics of the quantitative variables used in the analysis

Variable	Treatment group		Control group	
	Mean	Std. deviation	Mean	Std. deviation
Number of children	1.876	0.821	0	0
Female	0.536	0.499	0.457	0.498
Age	34.53	4.125	32.365	5.192
Age 25–30	0.167	0.373	0.423	0.494
Age 31–36	0.495	0.500	0.318	0.466
Age 37–42	0.337	0.473	0.257	0.437
Years of education	11.79	3.441	12.119	3.867
Weekly work hours	36.832	16.105	42.018	12.549
Household income				
< 24,000	0.236	0.425	0.218	0.413
24,000–60,000	0.429	0.495	0.427	0.495
60,000–90,000	0.152	0.360	0.169	0.375
< 90,000	0.181	0.386	0.185	0.389
Partner	0.962	0.190	0.412	0.492

The source of the data are Rounds two (2004) and five (2010) of the ESS survey for Switzerland. Sample restricted to individuals between 25 and 42 years old at the time of the interview. The Treatment Group consists of individuals who had a child in the six calendar years prior to the year of the interview. The Control Group consists of individuals with no children at the time of the interview

bracket, and 33.71% in the 37–42 bracket, and only 16.71% in the 25–30 bracket. On the other hand, the control group is more evenly distributed, with 42.34% of the individuals in the 25–30 bracket, 31.89% in the 31–36 bracket, and 25.76% in the 37–42 bracket. The mean number of years of education is similar in the two groups. Individuals in the control group work on average more hours per week, 42.02 versus 36.83, which is again expected. The household income distribution between the two groups is also very similar, with approximately 21% of the individuals having a household income below 24,000 CHF/year[10], 42% between 24,000 and 60,000 CHF/year, 16% between 60,000 and 90,000 CHF/year, and 18% above 90,000 CHF/year. Finally, unsurprisingly, individuals in the treatment group are significantly more likely to have a partner than those in the control group: 96.25% versus 41.26%.

Table 2 presents the distribution of responses to the survey question eliciting the individuals' fertility intentions. As expected, these distributions differ between the treatment and the control group, both before and after the reform. When looking into the whole population, the individuals in the treatment group exhibit a clear shift towards an increased likelihood of planning a child after the reform, while a shift in the control group is much less evident. In the subsample of women, a shift towards increased fertility intentions is observed after the reform both for the treatment and the control groups. On the other hand, in the subsample of male respondents, there is a clear shift towards increased fertility intentions in the treatment group, and towards decreased intentions in the control group. As a preliminary insight suggested by these observations, it is apparent that experiencing the extended MB is likely to have a stronger positive impact on the fertility intentions of the male respondents. This is confirmed later by our regression analysis.

Results

Analysis of the Effect of the Reform

The results from our main specification are reported in Table 3, with p-values reported in parenthesis. We estimate our model for each of the three variants of the dependent variable that we consider i.e., corresponding to 2, 3 or 5 possible outcomes. For each of these variants, we perform an estimation on the whole population of individuals in the sample, and two additional estimations on the samples of female and male respondents, respectively.

The results of this analysis confirm the hypothesis of a differential impact of the policy reform on fertility intentions in the two groups employed in the difference-in-differences analysis. The coefficient on the interaction term between the *Treated* and *After* variables is positive and statistically significant in the estimations corresponding to a specification of the dependent variable based on two and three outcomes. It is also positive, but statistically insignificant with a five-outcome dependent variable (the coefficient does become statistically significant with certain choices of the age brackets that restrict the estimation to age groups which are more likely to conceive a child). While the coefficient is positive in the subsample of women respondents, its value is higher and becomes statistically significant in the male population, suggesting that its significance in the whole population is primarily driven by the male respondents.

The results of the estimation also suggest that the fertility intentions increase after the reform and, as expected, that the individuals from the treatment group have significantly stronger intentions to conceive children in the future than those from the control group. The coefficients on the control variables uncover intuitive findings. Fertility intentions are stronger for women than men, and for individuals who have a partner than for those without, they decrease in the number of children that the individual already has, and in both the age of the respondent and the age of the partner. Being in the lower bracket of the income distribution or having more years of education is associated with stronger intentions to

[10] CHF denotes Swiss franc.

Table 2 Summary statistics of the fertility intentions for the treatment and control groups

Do you plan to have a child in the next 3 years?	Treatment group		Control group				
	Before reform (1)	After reform (2)	Before reform (3)	After reform (4)	$\Delta Prob_{tr}$ (5)	$\Delta Prob_{control}$ (6)	DD (7)
All individuals							
Definitely Not	45.54%	41.67%	25.93%	24.86%	−3.87%	−1.07%	−2.80%
Probably Not	18.78%	12.12%	23.02%	28.81%	−6.66%	5.79%	−12.45%
Neutral	4.23%	2.27%	10.58%	7.34%	−1.96%	−3.24%	1.28%
Probably Yes	13.15%	21.21%	25.93%	24.86%	8.06%	−1.07%	9.13%
Definitely Yes	18.31%	22.73%	14.55%	14.12%	4.42%	−0.43%	4.85%
Observations	213	132	378	177			
Women							
Definitely not	47.54%	41.27%	25.70%	22.67%	−6.27%	−3.03%	−3.24%
Probably Not	12.30%	11.11%	21.23%	17.33%	−1.19%	−3.9%	2.71%
Neutral	4.92%	3.17%	8.38%	9.33%	−1.75%	0.95%	−2.70%
Probably Yes	15.57%	19.05%	27.93%	32.00%	3.48%	4.07%	−0.59%
Definitely Yes	19.67%	25.40%	16.76%	18.67%	5.73%	1.91%	3.82%
Observations	122	63	176	75			
Men							
Definitely Not	42.86%	42.03%	26.13%	26.47%	−0.83%	0.34%	−1.17%
Probably Not	27.47%	13.04%	24.62%	37.25%	−14.82%	12.63%	−27.45%
Neutral	3.30%	1.45%	12.56%	5.88%	−1.85%	−6.68%	4.83%
Probably Yes	9.89%	23.19%	24.12%	19.61%	13.3%	−4.51%	17.81%
Definitely Yes	16.48%	20.29%	12.56%	10.78%	3.81%	−1.78%	5.59%
Observations	91	69	199	102			

The source of the data are Rounds 2 (2004) and 5 (2010) of the ESS survey for Switzerland. Sample restricted to individuals between 25 and 42 years old at the time of the interview. The Treatment and Control Groups are as defined in Table 1. Columns (1–4) elicit the percentages of the respondents in the group specified in the column header who chose the answer specified in the row header to the question about their fertility intentions. Columns (5–7) are computed as follows: Column (5) = Column (2) − Column (1); Column (6) = Column (4) − Column (3); Column (7) = Column (5) − Column (6)

conceive children, while working more hours per week has a positive but small effect.

Table 4 presents the marginal effects implied by the estimation of the model with two possible outcomes for the dependent variable. Since with only two outcomes, the marginal effect on the probability of an outcome is the negative of the marginal effect on the probability of the alternative outcome, the table presents only the marginal effects on the probability that the individual chooses "Definitely Yes" or "Probably Yes" to the question eliciting fertility intentions. As a robustness check, we also calculate and include in Table 4 the marginal effects implied by a Probit model. The marginal effects implied by the Logit and Probit models are similar. The unreported marginal effects implied by the two alternative models are also similar with a specification of the dependent variable with three or five possible outcomes.

To identify the categories of individuals most likely to respond to the extended MB, we investigate the degree of heterogeneity in responses by age groups and by the number of children that an individual already has at the time of the interview.

Table 5 reports the estimation results from subsamples determined by age groups. While the lower number of observations in each estimation exercise reduces the statistical power of the results, the analysis identifies the main driver of the positive response to the policy reform to be the individuals from the middle group, aged between 31 and 36. When looking into the whole population, they are seconded by the younger individuals between 25 and 30. When evaluating the responses according to the individuals' gender, the younger women respond more strongly to the reform than the women from the middle group. Finally, there is a negative differential impact on the fertility intentions of the individuals from the older group, aged between 37 and 41, driven primarily by women. [11] The interaction coefficient is negative and

[11] A possible explanation for this fact is that some of the individuals from this age group made a child after the reform was implemented, induced by its expanded MB, and then completed their fertility (we consider this channel in the discussion section where we analyze the possible channels that can drive the key results of our analysis). Such an effect would reduce the likelihood that this group would plan a higher order child in the future, explaining the negative coefficient in the regression. Nevertheless, as argued earlier, if this is the explana-

Table 3 Results from the DD estimation

	2 outcomes			3 outcomes			5 outcomes		
	All (1)	Women (2)	Men (3)	All (4)	Women (5)	Men (6)	All (7)	Women (8)	Men (9)
After X treated	0.766* (0.041)	0.577 (0.311)	0.988† (0.058)	0.652† (0.064)	0.294 (0.565)	1.004* (0.015)	0.170 (0.567)	0.269 (0.532)	0.121 (0.784)
After	−0.432† (0.074)	−0.329 (0.392)	−0.421 (0.192)	−0.417† (0.059)	−0.320 (0.364)	−0.455 (0.116)	−0.265 (0.149)	−0.298 (0.341)	−0.196 (0.406)
Treated	2.611*** (0.000)	3.321*** (0.000)	2.036** (0.006)	2.650*** (0.000)	3.141*** (0.000)	2.198** (0.004)	2.170*** (0.000)	2.816*** (0.000)	1.946** (0.007)
Number children	−2.126*** (0.000)	−2.581*** (0.000)	−1.742*** (0.000)	−2.104*** (0.000)	−2.389*** (0.000)	−1.820*** (0.000)	−1.535*** (0.000)	−2.069*** (0.000)	−1.252*** (0.000)
Gender	0.390** (0.036)	Omitted	Omitted	0.326* (0.059)	Omitted	Omitted	0.152 (0.299)	Omitted	Omitted
Age respondent	−0.054* (0.013)	−0.088** (0.006)	−0.015 (0.638)	−0.055** (0.005)	−0.089** (0.002)	−0.017 (0.534)	−0.066*** (0.000)	−0.099*** (0.000)	−0.027 (0.229)
Years education	0.090*** (0.000)	0.118** (0.002)	0.068† (0.056)	0.082*** (0.000)	0.120*** (0.001)	0.051 (0.105)	0.069*** (0.000)	0.089*** (0.001)	0.049† (0.064)
Weekly work hours	0.005 (0.391)	0.003 (0.738)	0.004 (0.682)	0.005 (0.393)	0.002 (0.768)	0.005 (0.570)	0.012* (0.026)	0.006 (0.401)	0.011 (0.156)
Income									
< 24000	0.622† (0.061)	0.955† (0.071)	0.209 (0.630)	0.486† (0.095)	0.880† (0.069)	0.078 (0.828)	0.451† (0.073)	0.824† (0.051)	0.101 (0.744)
24000 < 60000	0.272 (0.311)	0.412 (0.296)	0.118 (0.748)	0.140 (0.542)	0.288 (0.426)	0.016 (0.956)	0.134 (0.509)	0.333 (0.300)	−0.019 (0.940)
60000 < 90000	0.147 (0.637)	−0.003 (0.995)	0.214 (0.616)	0.018 (0.949)	0.037 (0.930)	0.015 (0.968)	0.139 (0.572)	0.276 (0.471)	0.104 (0.739)
Partner	1.795* (0.020)	0.886 (0.328)	3.181** (0.009)	1.763* (0.027)	0.986 (0.273)	3.124** (0.006)	1.175 (0.156)	−0.066 (0.938)	3.139*** (0.001)
Age partner	−0.021 (0.351)	0.003 (0.890)	−0.065† (0.077)	−0.023 (0.326)	0.000 (0.970)	−0.069* (0.049)	−0.013 (0.604)	0.025 (0.298)	−0.080** (0.006)
Pseudo R^2	0.1908	0.2292	0.1638	0.1485	0.1784	0.1274	0.0832	0.1134	0.0700
Wald chi2	108.25	63.31	49.51	121.63	75.76	50.16	112.67	86.30	47.19
Observations	799	386	413	858	414	444	858	414	444

All regressions are estimated using an ordered Logit model. P-values are reported in parenthesis. The variable "After" takes value one for individuals interviewed after the reform, and zero otherwise. The variable "Treated" takes value one for individuals in the Treatment Group, and zero otherwise. Columns (1–3) present regressions with the dependent variable taking two possible ordered values "Definitely or Probably Yes" and "Probably or Definitely Not." Columns (4–6) present regressions with the dependent variable taking three possible values "Definitely or Probably Yes," "Don't Know," and "Probably or Definitely Not." Columns (7–9) present regressions with the dependent variable taking five possible values "Definitely Yes," "Probably Yes," "Don't Know," "Probably Not," and "Definitely Not."

†$p < 0.10$, *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$

especially large in magnitude when the dependent variable allows for five outcomes.

Table 6 presents results from estimations that restrict the treatment group to individuals with one or two children, respectively. In the model specification with a treatment group of individuals with one child, the coefficient on the interaction variable is positive in most estimations, and is statistically non-negative with near 10% confidence level when the dependent variable allows for two or three outcomes, driven again primarily by men. On the other hand, when the treatment group consists of individuals with two children, the coefficient on the interaction term is positive and statistically significant, or nearly so, under most estimations. These results suggest that individuals with two children respond more strongly to the extended maternity benefits, although it is likely that individuals with one child respond as well. Not surprisingly, the coefficient on the variable *Treated* is also positive, but statistically significant

Footnote 11 (continued)

tion for the negative coefficient, it is likely that the overall effect on the fertility rate is positive in this age group as well because earlier births reduce the impact of unexpected negative shocks to fertility.

Table 4 Average marginal effects with logit and probit models

	Logit model			Probit model		
	All (1)	Women (2)	Men (3)	Women (5)	Men	(6)
After	−0.032 (0.399)	−0.021 (0.704)	−0.019 (0.713)	−0.030 (0.439)	−0.019 (0.736)	−0.018 (0.735)
Treated	0.396*** (0.000)	0.399*** (0.000)	0.379*** (0.000)	0.387*** (0.000)	0.401*** (0.000)	0.360*** (0.000)
Number children	−0.400***(0.000)	−0.465*** (0.000)	−0.328*** (0.000)	−0.370*** (0.000)	−0.446*** (0.000)	−0.301*** (0.000)
Gender	0.074** (0.035)	Omitted	Omitted	0.071 (0.043)	Omitted	Omitted
Age respondent	−0.010* (0.012)	−0.015** (0.004)	−0.002 (0.639)	−0.003 (0.355)	−0.016** (0.002)	−0.004 (0.448)
Years education	0.017*** (0.000)	0.021*** (0.001)	0.012† (0.052)	0.017*** (0.000)	0.021*** (0.000)	0.013* (0.041)
Weekly work hours	0.001 (0.390)	0.000 (0.738)	0.000 (0.682)	0.001 (0.345)	0.000 (0.809)	0.000 (0.627)
Income						
< 24000	0.117† (0.056)	0.167* (0.050)	0.039 (0.632)	0.119† (0.051)	0.152† (0.070)	0.051 (0.547)
24000 < 60000	0.051 (0.307)	0.073 (0.285)	0.022 (0.747)	0.052 (0.291)	0.065 (0.335)	0.028 (0.691)
60000 < 90000	0.027 (0.637)	−0.000 (0.995)	0.040 (0.619)	0.032 (0.581)	−0.004 (0.959)	0.050 (0.538)
Partner	0.317** (0.003)	0.159 (0.327)	0.599** (0.005)	0.311*** (0.001)	0.162 (0.321)	0.531* (0.016)
Age partner	−0.003 (0.348)	0.000 (0.889)	−0.012† (0.067)	−0.003 (0.355)	0.000 (0.912)	−0.009 (0.156)
Observations	799	386	413	799	386	413

Regressions are estimated using an ordered Logit model in columns (1–3), and using an ordered Probit model in columns (4–6). P-values are reported in parenthesis. †p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001. In the model with two possible outcomes for the dependent variable, the marginal effects on the probability that the individual responds either "Probably Not" or "Definitely Not" are the negative of the values listed in the table

when the treatment group are individuals with one child, since these individuals are likely to plan another child in the near future. The same coefficient is negative and large in magnitude for individuals with two children explained by the frequent choice of families to have exactly two children.

Test of the Parallel Trends Assumption

Testing the common trends assumption using consecutive waves of the survey from before or after the reform is not feasible in our context, as the only waves in the ESS that contain information about the individuals' fertility intentions are the waves from 2004 to 2010, which we employ in our analysis.

As an alternative, we perform a placebo test on data from France and Germany to verify whether typically there would be a differential change in the fertility intentions between the treatment and the control group from 2004 to 2010, by defining the two groups using the same criteria as in our main estimation exercise on the Swiss data. France and Germany are the two neighbor countries of Switzerland for which the ESS dataset contains information on the individuals' fertility intentions, and thus allows performing this test. The results are reported in Table 7.

The estimation is again performed for each of the three variants of the dependent variable, in the whole sample, and then separately in the subsamples of women and men. The coefficient on the interaction term is statistically insignificant in most specifications, and when it is statistically significant, its sign is negative i.e., opposite to the one elicited from the estimation of the model on Swiss data. Additionally, for several of the estimations with an insignificant coefficient, we can statistically reject the null hypothesis of a positive value of the interaction coefficient. These results suggest that there is no evidence that the individuals from the treatment group are more likely to increase their fertility intentions than those from the control group in the absence of an exogenous shock, such as a change in maternity benefits,

Table 5 Results from the DD estimation by age groups

Dependent variable: fertility intentions									
	2 outcomes			3 outcomes			5 outcomes		
	All (1)	Women (2)	Men (3)	All (4)	Women (5)	Men (6)	All (7)	Women (8)	Men (9)
Age group 25–30									
After X Treated	0.845 (0.303)	0.915 (0.472)	0.854 (0.525)	1.219 (0.105)	1.553 (0.216)	1.162 (0.401)	0.265 (0.656)	0.122 (0.888)	−0.439 (0.735)
After	−0.867* (0.027)	−1.138* (0.037)	−0.619 (0.256)	−0.868* (0.019)	−0.999† (0.060)	−0.697 (0.180)	−0.675* (0.039)	−0.850† (0.086)	−0.350 (0.432)
Treated	1.439 (0.214)	1.952 (0.197)	1.588 (0.444)	1.531 (0.155)	2.048 (0.123)	1.256 (0.508)	2.534** (0.005)	2.646* (0.019)	3.339† (0.096)
Observations	248	118	130	276	134	142	276	134	142
Age group 31–36									
After X treated	1.070 (0.113)	0.566 (0.544)	1.788† (0.074)	0.896 (0.157)	0.547 (0.543)	1.524 (0.106)	0.478 (0.367)	0.711 (0.324)	0.374 (0.668)
After	−0.403 (0.370)	−0.282 (0.713)	−0.403 (0.483)	−0.272 (0.504)	−0.261 (0.726)	−0.299 (0.569)	−0.312 (0.376)	−0.405 (0.523)	−0.311 (0.513)
Treated	3.949*** (0.000)	4.121*** (0.001)	4.351*** (0.000)	4.076*** (0.000)	4.223*** (0.001)	4.455*** (0.000)	3.063*** (0.000)	2.973*** (0.001)	4.066*** (0.000)
Observations	313	162	151	330	167	163	330	167	163
Age group 37–42									
After X treated	−0.301 (0.671)	−0.835 (0.479)	0.215 (0.831)	−0.504 (0.458)	−1.755 (0.157)	0.073 (0.938)	−1.165* (0.048)	−2.534* (0.029)	−0.735 (0.344)
After	0.883† (0.079)	1.659† (0.055)	0.140 (0.847)	0.928† (0.052)	1.764† (0.056)	0.218 (0.723)	1.071** (0.010)	2.220* (0.019)	0.492 (0.289)
Treated	1.772† (0.060)	3.618† (0.056)	1.319 (0.280)	1.749† (0.055)	2.902† (0.066)	1.449 (0.230)	1.171 (0.174)	2.924* (0.038)	1.155 (0.325)
Observations	238	106	132	252	113	139	252	113	139

All regressions are estimated using an ordered Logit model. P-values are reported in parenthesis. †$p<0.10$, *$p<0.05$, **$p<0.01$, ***$p<0.001$. See Table 3 for additional estimation details

supporting thus the validity of the estimation exercise we perform on the Swiss data.

Discussion of Possible Channels

In this section, we discuss the possible channels through which the policy reform may have had a different impact on the fertility intentions in the group of individuals who experienced its benefits, relative to a group of individuals who have not yet experienced them. The first possible channel is behavioral: some individuals who conceived a child after the reform, and who would have completed their fertility with that child in the absence of the reform, may have decided to plan another child because *experiencing* the expanded MB improved their perception about the cost of having a child. By the universal nature of the expansion of the MB that we investigate, all individuals were eligible for these benefits. Therefore, in a population of fully rational individuals, who know all information relevant to a decision problem and have well defined preferences over the relevant outcomes, experiencing these benefits should have no impact on fertility intentions. If experiencing the expanded benefits did impact fertility planning, there are two behavioral mechanisms that can explain it. First, experiencing the benefits may have induced a change in the individual's information by making her or him aware of the existence of these expanded MB. Additionally, even an individual fully aware of the expanded MB may not fully comprehend their effect on the cost of having a child. A second potential mechanism consists, thus, of an update in an individual's preferences over whether to conceive another child once they learned the new diminished cost.

A second channel is determined by a possible change in individuals' preferences over the spacing of child births. An extended maternity leave received because of a future child may reduce the cost of raising the current child. As this impact is arguably stronger when the current child is younger, this may induce individuals to plan a future child earlier. Since in our analysis, the fertility intentions are measured over the 3 years following the date of the interview, rather than as intended lifetime fertility, it is possible that individuals who would have otherwise waited a longer

Table 6 Results from the DD estimation with different treatment groups

Dependent variable: Fertility intentions	2 outcomes			3 outcomes			5 outcomes		
	All (1)	Women (2)	Men (3)	All (4)	Women (5)	Men (6)	All (7)	Women (8)	Men (9)
Treatment group: individuals with 1 child									
After X treated	0.609	0.196	0.883	0.475	−0.133	0.922	0.044	−0.016	−0.219
	(0.244)	(0.793)	(0.236)	(0.338)	(0.837)	(0.231)	(0.914)	(0.978)	(0.773)
After	−0.403	−0.261	−0.431	−0.393*	−0.259	−0.470	−0.192	−0.174	−0.166
	(0.102)	(0.492)	(0.196)	(0.079)	(0.459)	(0.117)	(0.313)	(0.587)	(0.502)
Treated	0.787*	1.040*	0.681	0.831*	1.032*	0.762	1.118***	1.033**	1.588*
	(0.020)	(0.017)	(0.219)	(0.015)	(0.016)	(0.195)	(0.001)	(0.006)	(0.025)
Observations	598	286	312	650	310	340	650	310	340
Treatment group: individuals with 2 children									
After X treated	1.077*	1.056	1.025	0.798†	0.530	1.002	0.375	0.763	−0.126
	(0.042)	(0.155)	(0.189)	(0.090)	(0.405)	(0.166)	(0.344)	(0.156)	(0.842)
After	−0.426†	−0.332	−0.382	−0.405†	−0.312	−0.432	−0.254	−0.284	−0.152
	(0.078)	(0.383)	(0.238)	(0.068)	(0.374)	(0.142)	(0.189)	(0.389)	(0.542)
Treated	−2.243***	−2.350***	−2.037***	−1.977***	−1.866***	−1.945***	−1.641***	−1.913***	−1.233***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Observations	627	301	326	683	326	357	683	326	357

All regressions are estimated using an ordered Logit model. P-values are reported in parenthesis. †p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001. See Table 3 for additional estimation details

Table 7 Placebo test on data from France and Germany

Depedependent variable: fertility intentions	2 outcomes			3 outcomes			5 outcomes		
	All (1)	Women (2)	Men (3)	All (4)	Women (5)	Men (6)	All (7)	Women (8)	Men (9)
France									
After X treated	0.058	−0.807	0.271	−0.029	−0.788	0.255	−0.166	−0.668	0.087
	(0.889)	(0.207)	(0.695)	(0.938)	(0.157)	(0.673)	(0.602)	(0.141)	(0.862)
After	0.060	0.760	−0.366	0.076	0.669	-0.304	−0.006	0.505	−0.444
	(0.851)	(0.159)	(0.387)	(0.782)	(0.129)	(0.412)	(0.976)	(0.143)	(0.147)
Treated	−0.322	−0.625	2.012†	−0.288	−0.626	1.720	0.204	−0.107	1.412†
	(0.683)	(0.447)	(0.078)	(0.692)	(0.426)	(0.110)	(0.742)	(0.889)	(0.084)
Observations	677	366	311	719	382	337	719	382	337
Germany									
After X treated	−0.368	−0.660	−0.031	−0.267	−0.458	0.110	−0.482†	−0.634†	−0.153
	(0.297)	(0.206)	(0.949)	(0.418)	(0.332)	(0.814)	(0.070)	(0.090)	(0.695)
After	0.473*	0.247	0.664*	0.363*	0.155	0.499*	0.260†	0.162	0.286
	(0.021)	(0.426)	(0.016)	(0.035)	(0.555)	(0.030)	(0.085)	(0.497)	(0.156)
Treated	0.664	0.270	1.137	0.689	0.303	1.166	0.719*	0.554	0.972†
	(0.196)	(0.691)	(0.169)	(0.158)	(0.623)	(0.152)	(0.048)	(0.233)	(0.099)
Observations	979	459	520	1065	501	564	1065	501	564

The source of the data are Rounds 2 (2004) and 5 (2010) of the ESS survey for France and Germany. Sample restricted to individuals between 25 and 42 years old at the time of the interview. All regressions are estimated using an ordered Logit model. P-values are reported in parenthesis †p < 0.10, *p < 0.05, **p < 0.01, ***p < 0.001. See Table 3 for additional estimation details

period before conceiving another child, move their intended child birth earlier. While our data do not allow evaluating whether such an effect did exist, due to the relatively small increase in the duration of the maternity leave implemented by the reform, it is unlikely that the perceived effect on the cost of raising an older child is large. We conjecture thus that the magnitude of the effect of the reform through this channel is probably small.

The third channel through which the expanded MB may have affected the fertility intentions measured in the two groups is by changing the composition of these groups. Since our control group is made up of the individuals with no children, if the reform prompted some individuals who would have otherwise not yet conceived a child, to conceive one, this would change the composition of both the treatment and the control groups. The net effect on the average fertility intentions in the two groups depends on the preferences of individuals who are shifted between the two groups. If the reform moved individuals who were more likely to continue having higher order children, and thus express intentions to continue conceiving children at the time of the second wave in 2010, the effect is positive. On the other hand, if the extended MB affected mostly individuals who would complete their fertility intentions after the birth of the child, then the sign of the effect is generically ambiguous, but may result in a positive sign of the relevant interaction coefficient from our regression analysis, depending on the prior distribution of fertility intentions in the two groups.[12] We analyzed this latter potential channel formally in an appendix available upon request, and showed that our key estimation results could not be due solely to such a shift between the two groups of individuals who complete their fertility. The significance of this insight is that if the differential effect in the fertility intentions between the two groups were to be solely determined by a change in the composition of the treatment and control groups, this fact would leave open the possibility that the reform improved the fertility rate not just by inducing a shift in the timing of the birth of a child (and thus, as argued earlier, by pre-empting the potential adverse shocks to fertility), but also by increasing the total number of children that an individual plans to conceive.

Limitations of Our Study

The main limitation of our study is that while we can show that the policy is likely to have successfully affected the fertility rate, we cannot fully measure the effect of this policy change. To address this limitation, one solution would be to employ a dataset with geographical identification of the observations or with data on the precise length of the maternity leave offered by the employer prior to the policy change. These would allow constructing a control group of individuals unaffected by the policy and thus precisely identifying the effect of the policy change rather than just of experiencing the benefits of this maternity leave expansion. Additionally, with more waves of observations following the policy change, one could measure the effect not only on fertility intentions, but also on the fertility rate. The present study shows that further research along these lines is likely to deliver significant findings. Finally, the policy change examined in this paper combines an extension of the maternity leave with a requirement that the additional weeks of leave be paid. It would be an interesting question to identify natural experiments that would allow examining separately the effects of an unpaid maternity leave extension, on the one hand, and of a change from an unpaid maternity leave to a paid maternity leave, on the other, in order to evaluate whether the former would suffice in inducing an increase in fertility rate.

Conclusion

The goal of this study is to examine the impact of an extended and more generous maternity benefits policy implemented in 2005 in Switzerland on the fertility intentions of individuals who have experienced the extended benefits induced by the reform, relative to individuals who have been eligible for the extended benefits, but have not yet experienced them. Our analysis unveils a significant difference between the changes in the fertility intentions for the two groups after the maternity leave expansion.

There are three channels that can explain the differential effect of the policy reform on the fertility intentions in the two groups. The first is a behavioral channel, which is created by the possibility that experiencing the benefits increases the likelihood that an individual would plan additional children. The second is an intertemporal substitution channel determined by the fact that the extended maternity leave reduces the cost of raising older children, which may induce individuals to reduce the spacing between child births, potentially with or without affecting lifetime fertility intentions. Finally, the last channel is determined by the fact that the reform may have induced some individuals who would have delayed the birth of their first child, conceive this child earlier, while continuing to plan higher order children. Since the intertemporal substitution that would be induced through the latter two channels at individual level is towards earlier births, it is likely that the country's fertility rate would increase not just in the short term, but also in the longer term, by preventing potential negative shocks to an individual's fertility choices that may occur if the birth of the child is delayed. Our paper offers thus evidence suggesting

[12] An intuitive explanation is as follows. Shifting an individual from the subset of individuals of the control group who would have responded in 2010 that they plan a child into the subset of individuals from the treatment group who respond in 2010 that they do not plan a child decreases the probabilities that the individuals from both the treatment and the control group respond in 2010 that they plan an additional child. It can be shown mathematically that it would decrease this probability more in the control group if the number of individuals from the control group who respond that they plan to have a child is smaller than the number of individuals from the control group who respond that they do not plan a child.

that the expansion of the maternity leave benefits is likely to have contributed to a higher fertility rate in Switzerland, despite the lower magnitude of the additional benefits relative to the extensions from other countries studied in the earlier literature.

Acknowledgements We thank helpful comments from Padmaja Ayyagari, Giulia La Mattina, and Joshua Wilde.

Compliance with Ethical Standards

Conflict of interest Andrei Barbos declares that he has no conflict of interest, Stefani Milovanska-Farrington declares that she has no conflict of interest.

Ethical Approval This article does not contain any studies with human participants or animals performed by any of the authors.

Appendix

See Table 8.

Table 8 A description of the variables used in the analysis

PlanChild5Outcomes	An ordered categorical variable indicating the respondent's intention to have a child in the next three years. The 5 possible values in decreasing order are: Definitely Yes, Probably Yes, Don't Know, Probably Not, and Definitely Not
PlanChild3Outcomes	An ordered categorical variable indicating the respondent's intention to have a child in the next three years. The 3 possible values in decreasing order are: Probably/Definitely Yes, Don't Know, and Probably/ Definitely Not
PlanChild2Outcomes	An ordered categorical variable indicating the respondent's intention to have a child in the next three years. The 2 possible values in decreasing order are: Probably/Definitely Yes, and Probably/ Definitely Not
Number children	Number of children an individual already has
Gender	Gender of the respondent
Age respondent	Age of the respondent
Years education respondent	Years of full-time education completed
Weekly work hours	Hours worked per week
HhldIncomeLess24000	A dummy variable indicating that the total annual income of the household is less than 24000 euro (= 1 if true, and = 0 otherwise)
HhldIncomeBn24000*and*60000	A dummy variable indicating that the total annual income of the household is between 24 000 and 60 000 euro (= 1 if true, and = 0 otherwise)
HhldIncomeBn60000*and*90000	A dummy variable indicating that the total annual income of the household is between 60 000 and 90 000 euro (= 1 if true, and = 0 otherwise)
Partner	A dummy variable for whether the respondent has a partner living in the same household (= 1 if yes, = 0 otherwise)
Age partner	Age of the partner

Source: Rounds 2 (2004) and 5 (2010) of the ESS survey for Switzerland. *NumberChildren* has been calculated by counting the number of family members whose relationship to the respondent is son/ daughter/ step/ foster/ adopted child. For each individual, if all three household income dummy variables described in the table have a value of zero, then the total annual income of the household exceeds 90 000 euros. *Partner* takes the value 1 if there exists a member in the household whose relationship to the respondent is husband/ wife/ partner

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