9-20-2018

Essays on Family-Friendly Policies, Child Planning and Children's Early-Age Outcomes

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by

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A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
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College of Arts and Sciences
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Date of Approval:
October 25, 2018

Keywords: fertility, child development, family allowances, maternity leave

DEDICATION

I dedicate this dissertation to some very important people in my life:

*To my **parents and grandparents**: Thank you for your unconditional love and encouragement; for being next to me when I succeed and when I fail; and for always being patient and supportive!*

*To my loving **husband:** Thank you for your love and support; for being able to make me smile even when I want to cry; for reminding me what the most important things in life really are; and for putting up with me even when I am not as good of a wife as I would like to be!*

I love you and I always will!

ACKNOWLEDGMENTS

I would like to express my sincere gratitude to Dr. Barbos, my major professor, for his invaluable feedback, guidance and support during the process of writing the dissertation and all years I have spent at the University of South Florida.

I also thank Dr. Ayyagari, Dr. Wilde and Dr. Cavendish for their comments and assistance, and for agreeing to be part of the Dissertation Committee. The helpful feedback from Dr. LaMattina is greatly appreciated as well.

I am also grateful to all my professors at the University of South Florida who helped me to obtain the knowledge I need in order to successfully start my career path, and to the University of South Florida and the Office of Graduate Studies for awarding me a Presidential Fellowship without which my graduate studies would have not been possible.

Finally, I am exceptionally grateful to my husband, parents and grandparents for their infinite love and support.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

ABSTRACT

This dissertation consists of three chapters which examine family-friendly reforms, child planning or children's early-age outcomes. The following are the titles of the chapters of the present dissertation:

- CHAPTER 1: Are Parents and Grandparents Substitutes or Complements? The Effect of Parental and Grandparental Supervision Time Investment on Children's Early-Age Development.
- CHAPTER 2: The Effect of Family Welfare Support on the Likelihood of Having Another Child and Parents' Labor Supply.
- CHAPTER 3: The Effect of Maternity Leave Expansions on Fertility Intentions: Evidence from Switzerland. (With Dr. Barbos)

Chapter 1 uses evidence from Scotland to examine the effect of grandparents' childcare provision relative to the effect of parents' time input on children's cognitive, social and behavioral development at an early age. We construct a model of skills and knowledge accumulation, taking into account the time investment in the child since birth, and empirically test the hypothesis that both grandparents' and parents' supervision time influence different aspects of early-age child development. The findings provide evidence of complementarity between parental and grandparental involvement in the child-rearing process. Specifically, while parents' supervision time has a larger impact on children's social and behavioral development

than an additional hour spent with grandparents, the grandparents' effect on children's vocabulary enhancement is larger than that of the parents. These results are consistent with the findings of the psychology literature that not only parents but also other relatives and people children socialize with determine children's development at an early age. Our findings imply a beneficial role of grandparents, and provide a strong argument in favor of policy considerations aimed to promote grandparental involvement in the child-rearing process in the first few years of life.

In Chapter 2, we investigate the impact of family allowances on the likelihood of having another child, and the extensive and intensive margins of labor supply. Due to a measurement error in the key explanatory variable, we adopt an instrumental variable approach as a major estimation technique. More specifically, as instruments for the support reported by individuals, we use the eligibility amount of benefits, as well as indicator variables reflecting a change in the allowances after the introduction of the Federal Family Allowance Act (FamZG) which imposed a federal floor on child benefits, common to all 26 cantons in Switzerland. In addition to examining the effect of child allowances on fertility and labor market outcomes, we also study a second, closely related but distinct matter. Specifically, we identify the causal impact of the introduction of a universal floor on child benefits on the previously-mentioned outcomes.

The findings indicate that benefits play a large role in family decision-making. First, using an instrumental variable approach, we find that an increase in the family support improves the likelihood of having another child, but does not affect labor market outcomes. Second, a difference-in-differences analysis of the effect of the introduction of a minimum amount of child allowances all cantons are obliged to pay indicates that a floor on benefits led to a significant increase in the likelihood of having a child in affected cantons relative to unaffected ones while

there was no significant difference between the differential effect of the policy on labor supply in affected and unaffected cantons. From a policy perspective, the results imply that providing financial support to families for raising children at a lower cost is likely to positively affect fertility without having an adverse effect on the labor market in countries where federal decision-makers aim to improve fertility rates.

Finally, Chapter 3 utilizes data from the European Social Survey to study the effect of the expansion of the mandatory paid maternity leave implemented in Switzerland in 2005, on individuals' fertility intentions. Several earlier papers examined the effect on fertility outcomes or fertility intentions of changes in the duration of the paid parental leave of a relatively large magnitude of one year. The maternity benefit expansion implemented in Switzerland in 2005 was of a relatively small magnitude, from 8 unpaid weeks to 14 mandatory paid weeks, and thus its effect on fertility choices is less evident ex ante. Nevertheless, 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 maternity benefit expansion on fertility intentions. We further evaluate three channels though which this positive effect may affect individuals' child planning.

CHAPTER 1:

ARE PARENTS AND GRANDPARENTS SUBSTITUTES OR COMPLEMENTS? THE EFFECT OF PARENTAL AND GRANDPARENTAL SUPERVISION TIME INVESTMENT ON CHILDREN'S EARLY-AGE DEVELOPMENT[1]

ABSTRACT

This article explores the impact of grandparents' supervision time input relative to the effect of parents' childcare provision on children's cognitive, social and behavioral development at an early age. We identify the effects of interest through panel data estimation methods and Zellner's seemingly unrelated regressions (SUR). The findings provide evidence of complementarity between parental and grandparental involvement in the child-rearing process. Specifically, grandparental care has a stronger effect than parental intervention on the vocabulary skills of the child. However, parents' time input in the child has a larger impact than does the supervision time investment of grandparents on the socio-behavioral development and the picture similarities measure of cognitive ability of children between 3 and 6 years old.

Keywords: Cognitive, Behavioral, Development, Grandchildren, Grandparent, Early age

[1] I thank helpful comments from Andrei Barbos, Padmaja Ayyagari and Joshua Wilde.

1. INTRODUCTION

Cognitive, social and behavioral development at an early age has an effect on later educational attainment, health, behavioral and socio-economic outcomes of children. These different aspects of the maturity process can be influenced by all caregivers who supervise the child. Therefore it is interesting to explore the relative importance of grandparents, other childcare providers and parents in the enhancement of early childhood outcomes for the following reasons. Grandparents can influence the early development of children through intergenerational transfer of experience, wisdom, knowledge and skills. They can help parents in the child-rearing process and might have more time, vigor and willingness to spend quality in addition to supervision time with the child. Grandparents can also directly affect early educational attainment of the child through helping him/ her learn letters and numbers, do homework, and develop practical skills which the child is likely to use later in life. However, this does not necessarily imply that grandparental care is sufficient for adequate child development. This paper addresses the question of whether grandparents and parents can be thought of as substitutes or complements in the development process of children, and quantifies the relative effect of parental and grandparental supervision time on child outcomes.

Previous literature focused on either the impact of grandparental provision of child care on grandparents, or the effect of grandparental resources, mainly material and financial, on the educational outcome of the grandchild. However, existing articles on the effect of downward transfers (i.e. transfers from grandparents to children) on child behavior and educational achievement are limited and inconclusive. We contribute to the existing literature by exploring the importance of raising grandchildren on their early-age development. We extend a previously developed model of skills and knowledge accumulation to take into account the supervision time

investment in the child not only by parents but also by grandparents. We employ Scottish data in FE panel data regression analysis and seemingly unrelated regressions (SUR) in order to identify the effect of the number of hours of childcare provision by grandparents relative to that of parents on three measures of child development at an early age.

Our findings are indicative of a significant difference between the effect of grandparental childcare provision and parental time investment in the child on the social and behavioral outcomes of children under 6, as well as on their cognitive attainment. While parents' supervision time has a larger impact on children's social and behavioral development than an additional hour spent with grandparents, the grandparents' effect on children's vocabulary enhancement is larger than that of the parents. Transferring 10 hours a week from the parents to the grandparents improves children's cognitive ability by 2.2%. These results are consistent with the findings of the psychology literature that not only parents but also other relatives and people children socialize with determine children's development at an early age (Harriss 2009).

Our findings imply a beneficial role of grandparents, and provide a strong argument in favor of policy considerations aimed to promote grandparental involvement in the child-rearing process in the first few years of life. Such policies include national insurance credit grants, financial allowances and paid leave, such as the ones recently implemented in the UK, Germany, Portugal and other European countries. In the context of Scotland and other countries in which childcare is not sufficient in some areas or cost-prohibitive country-wide, parents have to be aware of the consequences of employing a grandparent as a substitute for childcare or themselves.

The remainder of this paper is organized as follows. Section 2 summarizes the existing literature. Section 3 presents the empirical models, explicates the identification strategies, and discusses some threats to the validity of the findings. Section 4 describes the data used in the

empirical analysis of the paper. Section 5 presents the findings of this study. Finally, Section 6 discusses the policy implications, and Section 7 concludes the paper.

2. Literature Review

This section summarizes the existing literature related to grandparenthood and the effect of investing in children on their outcomes.

2.1. Literature on the Effect of Grandparenthood

A number of articles, such as Jendrek (1993) and Arpino et al. (2014), considered how providing care to grandchildren affects the elder generation. Specifically, they examined the effect of grandparenting on the cognitive score of grandmothers. They found that providing care to a grandchild had a positive impact on the verbal fluency of the grandparent but did not have a significant impact on three other measures of cognitive development: numeracy, immediate recalls, and delayed recalls. Jendrek (1993) utilized the results from an interview of grandparents who have taken care of their grandchildren every day in order to investigate whether taking care of a grandchild changes grandparent's lifestyle, friendships, relationship with the family and with the spouse. The article was purely descriptive, and the effect on children was not included in the analysis. Another study, conducted by Bowers et al. (1999) contributed to the literature by showing a correlation between caregiving and grandmothers' life satisfaction, stress and feeling of a burden. The results showed that behavioral problems of the grandchild made a grandmother feel a larger burden, and decreased her satisfaction from taking care of a grandchild.

Vendell et al. (2003) studied the factors, such as mother's age, ethnicity, employment status and others, which influence the likelihood of observing four types of grandparental childcare: extended full-time, extended part-time, sporadic and no routine care. The results indicated that all

types of care were more likely when the grandparent lives in the household. The probability of sporadic care was higher when the mother was younger and worked non-standard hours, while the chance of full-time care relative to extended part-time care was higher for mothers who worked full-time.

Our study is different from the above-mentioned ones in that we are interested in the comparison of the importance of grandparental involvement relative to that of parents for the development outcomes of the child.

2.2. Literature on the Effect of Investing in Children on Their Outcomes

Prior literature also considered the effect of grandparents' financial resources, human capital, social status and acquaintances, rather than time investment, on grandchildren. Some of these papers confirmed the existence of an effect on child outcomes (e.g., Zeng and Xie 2014) while others did not (e.g., Bol et al. 2016; Erola et al. 2007). For example, Bol et al. (2016) studied the effect of grandparents' education, occupational status and culture on the educational outcome of the grandchild. Pedersen et al. (2015) investigated the effect of grandparents' economic, cultural, and social capital resources on grandchildren's choice of secondary education (academic, vocational, or none). They showed that cultural capital possessed by grandparents had a positive effect on the probability that the grandchild would choose academic education, but economic and social resources of the grandparents did not influence educational choice (Pedersen et al. 2015). Further, evidence from the Netherlands was indicative of the lack of a significant impact of grandparent's resources on the educational success of grandchildren.

In contrast, this article investigates the effect of time investment in the child by the parents and grandparents rather than the impact of financial support or social status. We also test the hypothesis that there is a difference in the effect of time input in child development, provided by

grandparents relative to parents. The differential effect has been extracted from a model similar to the ones developed by Blau et al. (1992), Duncan et al. (2003), Bernal (2008) and Bernal and Keane (2010, 2011). However, while they investigated the effect of child care and the time parents supervise their children on child development, we also incorporate grandparents' supervision time in this article.

Specifically, Blau et al. (1992) studied the effect of maternal employment on child's cognitive development. Their findings indicated that maternal employment of 100% a week was associated with a decline in the standardized cognitive ability score of the child by 5.8 points in the first year, but had a positive effect of 4.2 points in the following three years. These results imply absence of a net effect on child's development in the first few years of life.[2]

Duncan et al. (2003) examined the effect of childcare quality on academic and cognitive skills of children and found a positive impact. Bernal (2008) developed a structural model of the decision of married mothers about whether to use child care or not and whether to work part-time, full-time, or not work, and then, examined the effect of the combination of the two decisions on the cognitive outcome of the child. He evaluated the effect of maternal employment, day care child inputs and household income on the reading and mathematical skills of children between 3 and 6. His empirical findings suggested that an increase in full-time employment of the mother by 1 year lowers the scores of the child by 1%, and utilization of child care for a year more reduces the child cognition test score by 0.8%. Thus, if a mother is employed full-time and uses child care for an extra year, the ability test score of her child decreases by 1.8%.

[2] The authors provide 2 possible explanations of the impact after the first year: mother's employment is associated first, with higher household income, and second, with more contacts of the child with children and adults as a result of non-maternal care, which may affect the cognitive development of the child.

Bernal and Keane (2010) extended Bernal's work (2008) to single mothers, and obtained similar results, with the latter effect of interest being 2.7% instead of 1.8%. They investigated the effect of maternal time, alternative childcare and goods inputs on child cognitive achievement at ages 4-6 in the case of single mothers, and created a model of the employment and child care decisions of a mother in order to deal with potential selection bias.

Finally, Bernal and Keane (2011) estimated the effect of single-mother time input on child cognitive development. Using welfare reforms, including TANF, the Earned Income Tax Credit (EITC), subsidies provided by the Childcare Development Fund, and Child support enforcement for single mothers, as instruments for childcare use, they found that an additional year of child care lowered test scores of children by 2.1%.

2.3. CONTRIBUTION

As shown in the previous subsections, evidence from previous literature is mixed. Furthermore, none of the prior papers emphasized the effect of grandparental caregiving.

This article delves more deeply in both the predictors of success and the outcomes of the child. It contributes to the literature in several ways. First, instead of looking at the effect of material and connection support provided by grandparents, as it has been done in most previous papers, we explain the impact of the amount of supervision time grandparents devote to grandchildren, in addition to the effect of parental supervision time investment and non-family-based child care. In other words, the focus is on non-material support influencing child outcomes through transmission of experience, multi-generational and potentially multi-cultural exposure to ideas, assistance in daily tasks and attention.

Second, unlike previous papers which restrict attention to either educational or cognitive attainment, we verify whether grandparental intervention can explain some of the variability in

both cognitive achievement, and social and behavioral development of the child relative to the effect of parents. This is different from most of the papers whose dependent variable captures cognition or life satisfaction of grandparents.

Third, to the best of our knowledge, there is no previous paper whose analysis has been based on Scottish data. Focusing on this particular country is important due to the high childcare costs there, as well as the unavailability of formal care in some areas.

Finally, most studies, with the exception of Black et al. (2005), were based on data obtained from interviews with few observational subjects. In addition, most studies except for Bernal (2008) and Keane and Bernal (2010, 2011), used cross-sectional data which did not allow making a distinction between past and present effects. We address this shortcoming by using a longitudinal dataset, following more than 5000 children for 6 years. This allows us to distinguish between time investment into the child in the current moment and time inputs in previous periods because skills and abilities are continuously acquired over time, and parental and grandparental involvement may have both an immediate and a delayed effect on the cognitive, social and behavioral formation of the child.

3. EMPIRICAL STRATEGY AND IDENTIFICATION

This section explains the identification strategies employed in our study. Subsection 3.1 presents the methodology selected to identify the effects of interest. Subsection 3.2 discusses the reasoning behind the use of seemingly unrelated regressions in addition to the major specification. Subsection 3.3 describes the potential validity threats and the methods we use to check the legitimacy of our analysis.

3.1. MODEL SPECIFICATION

The goal of our analysis is to explore the determinants of a full set of child development outcomes, including measures of early-age cognitive, social and behavioral development. To identify the causal effect of childcare provided by grandparents, we consider a model similar to the one developed by Bernal and Keane (2011). Our model is as follows:

$$lnScore_{it} = lnE_{it} + \boldsymbol{\alpha HF}_{it} + \alpha_i + \tau_t + \epsilon_{it} \qquad (1)$$

In this equation, $Score_{it}$ is a measure of child development. $\boldsymbol{HF}$ is a vector of covariates controlling for observed child's health, family and household characteristics at time period t. E_{it} captures child's acquisition of ability and skills through the process described in the following paragraph. α_i are child-specific fixed effects, or unobserved cross-sectional heterogeneity whose purpose is to control for unobserved skill endowment of the child. τ_t are time fixed effects, and ε_{it} is an unobserved, idiosyncratic error component capturing errors and transitory shocks. Subscripts i indicate that the variable is defined at children level, and subscripts t denote years.

Following Bernal and Keane (2011), the development of child's abilities is assumed to be a function of his inherent, initial endowment E_{i0} with which child i was born, and a set of inputs (parental and grandparental supervision time spent with the child, child care provided by non-parental and non-grandparental sources, and goods inputs), enhancing the innate E_{i0} over time. Intrinsic E_{i0} is correlated with a set of observable characteristics of the parents and observable characteristics of the child at birth, $\boldsymbol{X}_{io}$, including educational attainment of the parents and gender of the child, as well as a component ω_{i0}, capturing unobserved heterogeneity in the inherited endowment of the child, partly explained by unobserved ability endowment of the parents, and partly due to unexplained endowment of the child itself:

$$lnE_{i0} = \boldsymbol{X}'_{i0}\boldsymbol{\vartheta} + \omega_{i0} \qquad (2)$$

Then, similarly to Bernal and Keane (2011) and Leibowitz (1974), we assume that a production function of child development or acquisition of human capital is given by:

$$lnE_{it} = f(P_{i\tau}, Grp_{i\tau}, CC_{i\tau}, I_{i\tau}, E_{i0} | \tau: 0 < \tau \leq t) \qquad (3)$$

In (3), $P_{i\tau}, Grp_{i\tau}, CC_{i\tau}, I_{i\tau}$ are inputs in the development of child i. Specifically, P_{it} is a measure of parental supervision time inputs. I_{it} are goods inputs used in the production of child development. $Grp_{i\tau}$ is grandparental supervision time input into the child, and CC_{it} is child care supervision time input (excluding parental and grandparental time) t years after the birth of the child.

As noted by previous authors, it is not easy, if possible at all, to estimate (3) if the inputs and ω_{i0} have a different effect on E_{it} at different ages (Bernal and Keane 2011). Therefore, we make three assumptions, standard in the literature. First, we assume that cumulative rather than per period time inputs are pertinent to the framework of human capital production. The second assumption is that the ability component ω_{i0} is invariant over time. Finally, we assume a linear relationship between E_{it} and the inputs.

Given (2) and the simplifying assumptions, the behavioral and cognitive ability (development) production function in (3) can be expressed as:

$$lnE_{it} = lnE_{i0} + \pi_1 \frac{\sum_{\tau=1}^{t} P_{i\tau}}{t} + \pi_2 \frac{\sum_{\tau=1}^{t} Grp_{i\tau}}{t} + \pi_3 \frac{\sum_{\tau=1}^{t} CC_{i\tau}}{t} + \pi_4 lnI_{it} + u_{it} \qquad (4)$$

Here, u_{it} is an idiosyncratic error term, $u_{it} \sim N(0, \sigma_u^2)$. In the construction of this equation, by including cumulative terms capturing cumulative parental, grandparental, and external childcare over time we take into account the possibility that current possession of skills can be influenced by both past and present inputs in the development of the child. This is an important distinction and improvement of other papers which ignore historical attainment of skills because

specifications which incorporate only present inputs impose the strong assumption that present outcomes are independent of past investments in the child.

We use annual household income as a proxy for the unobserved I_{it}. Data on caregivers' inputs, including grandparents, are available. We assume that parents are residual caregivers, and express parental supervision time P_{it} as follows:

$$P_{it} = T - Grp_{it} - CC_{it} \qquad (5)$$

In this equation, T is total time in a period, which is a week (168 hours) in our specification. The expression $T - Grp_{it} - CC_{it}$ can be interpreted as the number of hours in a week which a child certainly does not spend with either a grandparent, or an alternative child care provider.[3] Under the assumptions that there should always be an adult supervising children at an early age and that parents are the only residual caregivers, can we rewrite (4) as follows:

$$\begin{aligned} lnE_{it} &= \boldsymbol{X}'_{i0}\boldsymbol{\vartheta} + \omega_{i0} + \pi_1 \frac{\sum_{\tau=1}^{t}(T - Grp_{it} - CC_{it})}{t} + \pi_2 \frac{\sum_{\tau=1}^{t} Grp_{i\tau}}{t} + \pi_3 \frac{\sum_{\tau=1}^{t} CC_{i\tau}}{t} + \pi_4 lnI_{it} \\ &\quad + u_{it} \\ &= \boldsymbol{X}'_{i0}\boldsymbol{\vartheta} + \omega_{i0} + \pi_1 T + (\pi_2 - \pi_1)\frac{\sum_{\tau=1}^{t} Grp_{i\tau}}{t} + (\pi_3 - \pi_1)\frac{\sum_{\tau=1}^{t} CC_{i\tau}}{t} + \pi_4 lnI_{it} \\ &\quad + u_{it} = \beta_0 + \boldsymbol{X}'_{i0}\boldsymbol{\vartheta} + \beta_2 \frac{\sum_{\tau=1}^{t} Grp_{i\tau}}{t} + \beta_3 \frac{\sum_{\tau=1}^{t} CC_{i\tau}}{t} + \pi_4 lnI_{it} + u_{it} \qquad (6)^4 \end{aligned}$$

[3] We assume that parents supervision time is given by the difference between the total number of hours in a week and grandparents' and other care providers' supervision time spent with a child. However, it is likely that a parent and a grandparent sometimes supervise a child simultaneously. In that case, the assumption that parents are residual caregivers may lead to underestimation of the time parents spend with a child because the amount of time of simultaneous care-giving has already been counted as time under grandparents' supervision. Therefore, the effect of grandparents on child outcomes will be over-estimated while the impact of parents' supervision on development of the child will be under-estimated.

[4] Alternatively, one may not take the average number of hours spent with the child (i.e. may not divide by the number of periods t) because in a production function, one might want to take into account the total amount of inputs (here, number of hours invested in the child). As the child grows older (i.e. as t increases), the number of hours would increase. However, if one chooses this alternative specification of the model, he/ she has to necessarily include a time trend to control for the unavoidable increase in the total number of hours spent with a child as t increases.

It is important that $\beta_2 \equiv \pi_2 - \pi_1$ in (6) captures the effect of time the child spends with grandparents *relative* to the effect of parental supervision time investment in the child on child development, and similarly, $\beta_3 \equiv \pi_3 - \pi_1$ shows the effect of time in external childcare *relative* to the effect of parental supervision time with the child on child development outcomes.

Given equations (1) and (6), we specify the final regression model of interest in the following form:

$$lnScore_{it} = \beta_0 + \boldsymbol{X}'_{i0}\boldsymbol{\vartheta} + \beta_2 \frac{\sum_{\tau=1}^{t} Grp_{i\tau}}{t} + \beta_3 \frac{\sum_{\tau=1}^{t} CC_{i\tau}}{t} + \pi_4 lnI_{it} + \boldsymbol{\alpha HF}_{it} + \alpha_i + \tau_t + \varepsilon_{it} \quad (7)$$

We estimate different specifications of this model taking into account only individual FE, individual and time FE, and individual, time and regional effects. Appendix A2 contains all variables used in the analysis and their descriptions.

As an alternative specification of the model, since acquisition of skills and ability is a continuing process and time investment into the child adds up over time, we distinguish between the effect of time investment in the past and in the present in order to examine the immediate effect of time investment distinctly from the one which affects child outcomes with a lag. We do so by separating previous periods from the current period, and rewrite (7) as:

$$lnScore_{it} = \beta_0 + \boldsymbol{X}'_{i0}\boldsymbol{\vartheta} + \beta_{2past} \frac{\sum_{\tau=1}^{t-1} Grp_{i\tau}}{t-1} + \beta_{2present} Grp_{it} + \beta_{3past} \frac{\sum_{\tau=1}^{t-1} CC_{i\tau}}{t-1} + \beta_{3present} CC_{it} + \pi_4 lnI_{it} + \boldsymbol{\alpha HF}_{it} + \alpha_i + \tau_t + \varepsilon_{it} \quad (8)$$

The interpretation of the coefficients $\beta_{2past}, \beta_{2present}, \beta_{3past}$, and $\beta_{3present}$ in (8) is similar to that of β_2 and β_3 in (6), described earlier.

It is also worth mentioning that differentiating between past and present effects would make sense given that prior and current involvement into child's development are not highly

correlated. In the context of Scotland, data suggest that the correlation coefficient between the sum of hours of all prior-period grandparental childcare provision and current number of hours of child care provided by the grandparents is 0.5257, while the correlation coefficient between the same variables but for external child care is even smaller, or 0.3832. Both correlation coefficients are suggestive of a reasonable motive to tease out past and current time input into the child skill development production function.

Finally, while we identify the effect of time spent under parents' or grandparents' supervision which is available in the dataset, we fail to capture the impact of "quality time" spent with the child which is unobserved. Thus we are able to quantify only the impact of supervision time investment on child scores. Because supervision time always has to add up to 168 hours per week, an additional hour under the supervision of one provider, e.g. a grandparent, is implicitly associated with a subtraction of one hour from the time another provider would otherwise spend with the child. Thus one cannot estimate the effect of the marginal hour holding everything else fixed.

3.2. SEEMINGLY UNRELATED REGRESSIONS

While using the panel feature of the dataset and applying fixed effects estimations enables us to establish causal relationships between the variables of interest and the outcome, examining each outcome, one at a time, independently of the others, may not be sufficient in the context of child development. The reason is that a given outcome or ability can be a major predictor of another capability in the same or subsequent years. In other words, two measures of cognition for a given child are likely to be related although the outcomes of one child are not related to those of another. If different measures of cognitive development and/ or their values at different ages are correlated, and a formal Breusch-Pagan independence test provides evidence of such correlation,

then the effects of interest should be explored through a system of equations, related stochastically through the correlation between the error terms. This requires the usage of Zellner's seemingly unrelated regressions (SUR) model (Zellner 1962). We construct a SUR model with 4 equations of the following form:

$$lnCognitiveOutcome_{ki} = \alpha_k + \beta_{ki}\beta HrsChdCareGrp_{ki} + \boldsymbol{\gamma_k X}_{ki} + \varepsilon_i \quad (9)$$

Here, $k = 1, \ldots 4$ denotes 4 cognitive development outcomes: scores based on children's ability to identify picture similarities and to name items given pictures, both observed at ages 3 and 5. The parameters in each of the 4 equations in the system are identified simultaneously using generalized least squares (GLS).

Obtaining Aitken's GLS estimates has two advantages over running a set of multiple OLS regressions. First, despite the fact that OLS estimation for each equation separately would yield consistent estimation of the parameters even if the disturbances were correlated, GLS estimation would improve efficiency. Second, it allows the outcomes to be correlated for a given child while still being uncorrelated across children. A formal justification showing the correlation between the disturbances in our application using a Breusch-Pagan test is provided in the results section of this article.

3.3. Validity of the Assumptions

3.3.1. Assumptions Made for Identification of the Fixed Effects Regressions

To employ FE panel data method to identify the parameters in each of the regressions presented in subsection 3.1, we assume cross-sectional heterogeneity, i.e. children differ in terms of their unobserved, time-invariant characteristics, and we also allow the latent effect or the unobserved, time-constant heterogeneity to be correlated with the covariates. We conduct a formal Hausman test to detect whether a FE model is the optimal estimation method.

3.3.2. SEEMINGLY UNRELATED REGRESSIONS ASSUMPTIONS

A major identification assumption of the SUR model is that for a given child, the stochastic error terms are correlated across the 4 equations in the system, while the error terms across children are independent and homoscedastic. If the error terms are not correlated across equations, the OLS equation-by-equation estimates will be identical to the ones obtained using GLS estimation and the SUR model.

In order to justify using SUR, we conduct a Breusch-Pagan chi-square test for independence of the errors. A rejection of the null hypothesis would confirm that GLS would improve efficiency, and is thus preferred.

4. DATA

4.1. VARIABLES

All data for this study are extracted from Growing Up in Scotland (GUS) – Birth Cohort 1, a panel dataset following 5217 children from birth to early childhood, or the age of 10. This dataset contains variables which describe the composition of each participating household. Given the relationship of each family member to the child, we find the number of hours of care provided by grandparents. Growing Up in Scotland also provides a set of variables controlling for household, family, child, and parents characteristics, variables measuring child development and early childhood outcomes, as well as different sources and length of childcare provision, necessary for our analysis.

Social, cognitive, and behavioral development, ability, mental and physical health, behavior, educational and health well-being of children included in Birth Cohort 1 have been tracked for all years of the Growing Up in Scotland study. Families have been first interviewed in 2004/2005 when the study child in the family was 10 ½ months old (wave 1). Interviews have

been conducted face-to-face every year with the respondent being either the mother (in 95% of the cases), or the main child-care provider. The respondent has been the same for each family in all waves, whenever possible.

The dependent variables selected for this study include officially reported measures of cognitive, social and behavioral development in the survey. More specifically, picture similarity (PS) and naming vocabulary (NV) raw scores measure the *cognitive development* of the child at the ages of 34 months (wave 3, year 2007) and 58 months (wave 5, year 2009), while the total difficulties (TD) score is a predictor of the *behavioral and social development* of the child at the ages of 46 months (wave 4, year 2008), 58 months (wave 5, year 2009) and 70 months (wave 6, year 2010). In what follows, we describe the method in which these three development scores have been obtained in the Growing Up in Scotland study.

First, PS and NV raw scores are extracted from two subtests of the BAS II test, designed to estimate the cognitive ability of children at the age of 2 years and 6 months, and 17 years and 11 months. BAS is individually administered and considered to be appropriate for "administration in a non-clinical setting" (Bromley, 2009). In the first experiment, children were given four cards with pictures. Then, they were provided a fifth card identical to one of the first four cards, and were asked to match the two identical pictures. Based on this experiment, the interviewer assigned a PS score, measuring the problem solving capacity of each child. An NV score as a predictor of the language skills was obtained by asking children to name items which they were observing on pictures provided by the experimenters.

Second, the TD score was derived based on the computer-based report completed by one of the parents of each child (usually the mother). The report contained the answers to a 25-question Goodman's behavioral screening Strength and Difficulties Questionnaire (SDQ), specifically designed for children between 3 and 16 and aimed to take into account peer relationship

problems, emotional symptoms, inattention or hyperactivity, and conduct issues. The total score is calculated as the sum of four scores, assigned to each of the upper-mentioned scales. In the original dataset, a lower score indicates a lower level of difficulties, and is thus preferable. In order to facilitate interpretation of the results and be consistent with the other development measures in this study (for which a higher score is preferred), we rescale the TD score. In particular, a score of 35 in the original dataset corresponds to a TD score of 1 in our analysis, the original 34 is transformed to 2, and so on. Thus a higher (transformed) TD score is preferable in this article.

Data on each household member and his/ her relationship to the study child are also available in the original dataset. We use this information to derive a variable for the presence of a mother and a father in the household. Given the legal marital status of each person in the household, we derive whether the parents are legally married. The health status of the study child is also taken into account by generating a dummy variable for the child not being in good health condition, which is equal to one in the instances where the respondent has reported that child's health is in fair, bad or very bad condition. Further, the respondents were asked to release information about each childcare provider and the number of hours of paid or unpaid care provided each week. This allows us to construct a variable denoting the number of hours of childcare provision by a grandparent. We also use the number of hours of care given by all other providers, where the weekly hours of care by external providers are calculated as the sum of the number of hours of child care by all providers, different from the child's grandparents. Thus we are able to evaluate the effects of those hours on child outcomes with greater precision. This gives us a slight advantage over previous papers (e.g., Bernal and Keane 2010) which due to data limitations, had to make imputations about whether alternative sources of child care provided

full-time or part-time care, based only on the information whether alternative child care was provided for at least 10 hours per week.

It is important to mention that although the number of siblings the study child has may not be a direct input into the production of skills and ability of the child of interest, it is useful to include it as a control because it may indirectly affect child development because the amount of resources allocated to each child may be different depending on the number of children the family has. Appendix A2 contains the complete set of all variables utilized in this article, as well as their descriptions.

4.2. DESCRIPTIVE STATISTICS

Tables 1 and 2 provide summary statistics respectively, of the continuous and binary variables used in the empirical analysis. Evidence suggests that children perform slightly better on the NV test of cognitive ability when they are 3 years old, as compared to their score two years later, and the result is reversed when comparing PS raw scores although the standards remain unchanged. Despite these differences, the mean values of both measures of cognition vary between 14.318 and 16.887 (out of a total of 31) in both years. Further, the average TD scores measured at ages 4, 5 and 6, seem very close to each other, i.e. we do not observe significant differences in this score at different ages, although there is a small upward trend and rising dispersion of the scores as the child grows older. The latter observation is suggestive of children having fewer social and behavioral difficulties as they grow up. The average transformed TD score is 27.53, where the total is 35.

The statistics related to the time different providers supervise children indicate that the average number of hours of childcare provision by grandparents is about 5 hours per week, with also being 168 in some exceptional cases, which we exclude in one specification to conduct a

robustness check. The average number of hours of childcare provided by grandparents does not vary much across the first few years of life of the children. The mean of the number of hours given by other sources of childcare changes over time: it increases up until the age of 3, and decreases slightly at later age. This is expected because external childcare providers take in children only of a certain minimum age.

Figures 1 and 2 show the distribution of the number of supervision hours provided by grandparents and external care givers. The distribution of hours grandparents supervise grandchildren is highly skewed to the right with the majority of grandparents providing care fewer than 10 hours per week. The shape of the distribution of supervision hours provided by external providers is very similar, with a minor number of external providers supervising a child more than 50 hours a week.

The average number of family members is between 3 and 4, and the average number of siblings is about 1. There is a mother in more than 99% of the households, while a father is present in about a little more than 82% of the families. Both parents are present in the household for 81.83% of the children in the study. 43.83% of the parents of a 1-year-old child living in the household are married, but the percentage declines slightly as the child grows older, which may be an indication of family destruction.

The percentage of people being employed ranges from 59 to 79% for both genders in all years of the study. Fathers are slightly more likely to be employed than mothers. Mothers are more likely to have higher education than fathers. The majority of study children have attended pre-school and primary school during the survey period. More specifically, 96.42% of all children have already attended pre-school at the age of 6, and more than 98% of the 6-year-old children have already started primary school.

5. RESULTS

This section presents the main findings of the article, some secondary results, as well as the diagnostic checks we perform in order to verify the validity of our empirical strategies.

5.1. MAIN RESULTS

The results from the FE estimations under various specifications are summarized in Tables 3 to 5. In what follows, we present these results, organized by an area of development.

First, we look at the TD measure of *social and behavioral development*. Evidence from the FE regression analysis in Table 3 suggests that an increase in the average number of hours per week a child has been supervised by the parents at the expense of an additional hour spent with a grandparent leads to an increase in the social and behavioral development of the child by 0.1%, or a 1% increase for 10 extra hours of parental care per week (Column 1). The significance and the magnitudes of the differences between the effects of an additional hour of care provided by grandparents relative to that provided by parents in the current period remain the same if we separate current period from past periods (Column 5), or if we add one (Column 6) or two (Column 7) lags of the hours each care provider supervises the child. All effects are highly significant, and imply that spending more time with parents rather than with grandparents is beneficial for the improvement of the social and behavioral components of child development. In contrast, substituting supervision time with parents for time with external care providers does not have a significant impact on children's TD score.

Second, analyzing the results for the NV (Table 4) and PS (Table 5) scores of *cognitive development*, we find that grandparents have a larger influence than parents on the vocabulary enhancement of children. An additional hour of care per week provided by the grandparents in the current period on child's NV score is 0.22% higher than the effect of an extra hour of parental

care, holding all other factors constant. Put differently, transferring 10 hours of supervision per week from parents to grandparents enhances child's vocabulary skills by 2.2% (Column 1). Contrary to the effect on the TD score, the effect of alternative child care on the NV measure of cognitive ability is also 0.28% higher than the effect of the marginal hour of care provided by parents, and the difference is statistically significant. The same estimates on the PS measure of cognitive development provide evidence that an additional hour of parental care has a larger marginal effect on the PS score relative to the effect of an extra hour of care provided either by a grandparent or an external caregiver. More specifically, an increase in the time input into the child by the parents by one hour per week at the expense of one hour of grandparental care or alternative childcare time leads to an increase in child's PS score by 0.22% and 0.26%, respectively. Similarly to our finding for the TD score, when we distinguish between present and average past effect of care, we find that the significance and the magnitude of the estimates remain the same (Column 5). The initial results are also consistent with the estimates obtained when we add lags of the number of hours different providers spend with children (Columns 6 and 7).

For comparison and illustration of the robustness of all findings, we provide the pooled OLS and the RE estimates in Columns (2) and (3) of Tables 3, 4 and 5. As expected, the results are similar to the ones yielded by the FE regression model. Including time and residential effects does not change the significance, direction or magnitude of the effects of interest as well. These modifications of the regression are presented in Columns (8) and (9) of Tables 3, 4, and 5.

The set of the presented FE estimations provides strong evidence of the grandparental and parental effect on the cognitive, social and behavioral outcomes of children, but fails to capture the potential relationship between these different measures of child development. A formal Breusch-Pagan independence test for verification of the existence of a correlation between the

error terms of the equations in the system of equations used in the SUR model produces high chi-square statistic of 1698.943, and a p-value of 0.000, as indicated in Table 6. This result suggests a high, statistically significant correlation between the disturbances across the equations, and thus justifies the usage of seemingly unrelated regressions.

Under the assumption that all three outcomes are correlated for a given individual but uncorrelated across children, SUR estimation yields the estimates presented in Table 6. A simultaneous consideration of different cognitive development measures and the measures at different ages indicates that grandparental involvement has a positive impact on the NV score of children at age 5, and a negative such impact on the PS score at that age, but no significant influence on either of the two scores at the age of 3. More specifically, an additional hour of child care provided by a grandparent is associated with an increase in the NV subsection of the BAS II test score by 0.66%, but contributes to a reduction in the PS score by 0.08%. Both of these results are consistent with the findings from the FE estimation.

Overall, our findings from both the major FE and the SUR estimations provide evidence that on average the grandparental effect exceeds parental involvement on children's vocabulary skills, but parents are more important for the PS aspect of the cognitive development of children, as well as their behavioral and social skills. These results are suggestive of complementarity between parents and grandparents in the child development process.

Our findings are consistent with the development psychology literature. First, although some research in the field showed that children who are more emotionally connected to their grandparents are subject to fewer social and emotional problems, have fewer symptoms of anxiety and depression, and demonstrate more pro-social behavioral (e.g., Ruiz et al. 2007; Kenny et al. 2006), other studies found evidence that as people age, they become more accepting and tolerant, lower their expectations, and become more willing to forgive misbehavior of their

grandchildren (Seltzer 2016). As a result, although children can obtain specific knowledge and skills from their grandparents which they are less likely to be taught by parents, grandchildren are less likely to be punished for misconduct by their grandparents than by their parents. Therefore a child benefits in the long run if his parents impose rules aimed at disciplining the child and enhancing his behavioral development. This is in accordance with our finding that parents have a larger effect on the social and behavioral aspects of child development than grandparents do. The result is also consistent with the finding of Md-Yunus (2017) that about 13% of children being raised by grandparents exhibit a large degree of behavioral problems between the age of 6 and 17 (Md-Yunus 2017).

Second, the results of this article confirm Dr. Harris's theory (2009) related to child development. He disproved the so called "nurture assumption" commonly made in the development psychology literature. According to this assumption, children's development and personality depend only on the way they are raised by their parents. The parents are responsible for what children become. Harris challenged this assumption by stating that this was not necessarily true. The environment in which they are raised also plays a role. There is a difference between "nature," or genes, which are highly dependent on the parents, and "nurture," which refers to the way or the environment in which children are raised. Who children socialize with and who they spend time with determine different aspects of their development, such as social, cognitive, behavioral and mental enhancement. This implies that it is likely that some measures of child development can be affected by grandparental involvement more than they are influenced by parental time investment. Our results are suggestive of this theory.

5.2. SECONDARY RESULTS

The FE estimation provides additional evidence that poor health has an adverse effect on the behavioral development of children between 4 and 6. In addition, higher birth order has a significant beneficial effect on the TD and the NV scores of the child (2.63% and 11.82%, correspondingly, or 2.84% and 12.48% when past and present involvement of grandparents have been separated). The latter finding is expected because older children in the family are likely to help their younger siblings to learn, and thus have a beneficial impact on their cognitive, social and behavioral development although this does not exclude the presence of other channels affecting child growth.

We also use OLS to test the hypothesis that development scores at specific ages are determined by the hours of supervision provided by different providers at each year of childhood from birth until the age at which the specific score has been recorded. The findings indicate that grandparental involvement has a negative impact on the behavioral development of children at the period when the TD score has been measured, but past supervision is insignificant. However, the time different care providers supervise children at any specific age does not significantly affect their cognitive development. Thus we conclude that, as previously found, only the average number of hours of supervision throughout the years and current involvement, rather than the time at any given age, are significant determinants of child development at an early age.

In addition, we perform the major fixed effects estimations using data only on the subsample of children supervised by grandparents fewer than a certain number of hours per week, and separately, only on a subsample of children who are under grandparents' supervision more than a given threshold of hours per week. The former experiment is performed as a robustness check the purpose of which is to verify whether the main estimates from the whole sample are driven by outliers, where we define outliers as children whose grandparents are their main care

providers, i.e. supervise the child more than a given number of hours per week. The selected thresholds are 20 and 30 hours per week. The results are presented in Table 7, and are discussed in more detail in Section 5.4. They are robust to the choice of an upper bound on the hours of grandparental supervision time, regardless of the development score.

The second experiment involves imposing a lower bound on the number of hours a grandparent supervises a child. It allows us to test the hypothesis that grandparents have a different from the previously found effect on various aspects of child development provided that the child spends a sufficient amount of time under the supervision of a grandparent. The results are located in Table 7. Our previous findings of the impact of grandparents on the behavioral development of grandchildren have been confirmed and are robust to the imposition of a lower or no bound on the amount of supervision time provided by grandparents. However, an interesting finding is that if a grandparent supervises a grandchild for more than 13 hours per week, an additional hour of grandparental supervision has a significant, positive effect on the PS score of cognition of the grandchild. This effect becomes larger with an increase in the lower bound on grandparental supervision time. The latter result suggests that even though on average parents have a larger impact on the cognitive development of children as measured by their PS score, if a sufficiently large number of hours of supervision has been transferred from parents to grandparents making the grandparent a main supervisor of the child, grandparents can have a significant, positive effect on the cognitive enhancement of children at an early age. This observation indicates that grandparents might accept the role of parents in the cognitive development of children given that they supervise the child more than 13 hours per week.

5.3. Diagnostic Tests and Validation of the Empirical Findings

The results from the diagnostic tests are located in Table 8 and lead to the following conclusions. Formal Hausman tests reject the null hypothesis of the existence of non-systematic differences in the coefficients estimated using FE and RE, and thus support employing FE over RE in all regressions, independent of whether the dependent variable is a measure of social and behavioral, or cognitive development. Simultaneously with this finding, using a Breusch and Pagan Lagrangian multiplier for RE, we find that in all instances in this article, a FE regression model is preferred to pooled OLS. The combination of the latter two results implies that FE estimation would be preferred over RE and pooled OLS, and is thus our choice for estimating the parameters in all panel data models in this paper. Further, a modified test for group-wise heteroskedasticty in a FE model rejects the null hypothesis of homoscedastic error terms in all three equations. In order to be certain of the robustness of the standard errors and to correct for heteroskedasticity, we use robust Huber/ White standard errors in all regressions in this article.

5.4. Robustness Checks

We verify whether the results we obtain are robust to various specifications.

First, as mentioned earlier, identification of the parameters in the FE regressions is not influenced by the inclusion of individual, time and regional effects.

Second, we run the two major FE models (only with current supervision time investment in the child, and separated past and present involvement) only for children in good or very good health, children whose parents are married, children whose parents are not married, children whose parents both live in the same household, and children living in a single-mother household. All results are identical to the ones reported in the previous subsection, and are available upon request.

Finally, we keep only observations for which the hours of grandparental supervision are fewer than 30 (and separately, 20) hours per week, and estimate the main model of interest. Such an exclusion of outliers does not change the significance of the estimates, and does not yield large changes in their magnitudes. In particular, as shown in Table 7, transferring one hour of care from the parents to the grandparents increases the NV score of the child by 0.29% (or 0.32% when the threshold is 20), and the effect of an extra hour per week of parental involvement increases child's TD and PS scores, respectively, by 0.11% (regardless of the threshold) and 0.47% (or 0.50% when the threshold is 20) more than does an additional hour of care provided by the grandparents. Thus the effects of care of various providers on children's development remain unchanged when we exclude outliers.

6. Discussion

6.1. Possible Channels of the Effect of Intergenerational Transfers on Child Outcomes

In this section, we discuss several mechanisms through which intergenerational time transfers can influence child outcomes.

There are ways in which both parents and grandparents can affect child development, but it is possible that some of these effects can be intensified if a grandparent is involved in the child-rearing process. This effect is expected to be larger especially if a non-working or retired grandparent supervises the child because such a grandparent is likely to have not only more time, but also more vigor and willingness to spend quality time in addition to supervision time with the child. Some factors which might determine the impact of supervision time on child development and his/ her later outcomes include personal characteristics of the care provider (Modin and Fritzell 2009), his family background (Jager 2012), academic achievements (Modin and Fritzell

2009; Osler et al. 2005), resources (LaFave et al. 2017), and time investment.[5] Both parents and grandparents can read to children which fosters greater interest of the child in reading at a later age (Arnold et al. 1994), creates an advantage of the child over his peers in primary school (Wade et al. 2000), and has a positive effect on the literacy achievement of the child (Weinberger 1996). Parents and grandparents can also transfer skills and knowledge to children by playing with them, teaching them the alphabet and the numbers, academic concepts and math problems, creating an enjoyable environment which facilitates giving educational advice to the child, engaging them in activities stimulating learning and creativity, and helping them do homework and handle with academic and personal difficulties at school. The impact of all of the above-mentioned activities is likely to be higher for grandparents than for parents, provided that grandparents' supervision is more valuable than parents' time with the child and/ or that children are more willing to learn from lessons taught by someone different from their parents.

In addition, the effect of grandparental care on the cultural, social, educational, and moral development of the child may be expanded over parental one through grandparents' ability to serve as mentors or role models to the child, and through transmission of life wisdom, which less-experienced parents may not be able to pass on children. Specifically, grandparents' stories about their life experience help children draw lessons and morals, define values, teach them how to handle with obstacles in life, help them learn about family history and culture, and teach them to

[5] Jager (2012) finds an association between family background of the extended family (parents, grandparents, aunts and uncles) and years of completed schooling of a child (Jager 2012). Personal characteristics of the extended family are correlated with cognitive development (Modin and Fritzell 2009), academic outcomes and health (Modin and Fritzell 2009; Osler et al. 2005) of the child as well. Evidence from Indonesia also shows that the resources of parents, grandparents, aunts and uncles affect child's height as a measure of health, non-verbal cognitive assessment performance and age at which the child starts school (LaFave et al. 2017).

listen. Such factors prepare children for listening to their teachers at school, and affect their educational attainment at a later age. However, it is crucial that wisdom, experience and knowledge transmission is performed at an early age because studies provide evidence that contact frequency between grandparents and grandchildren declines with age between ages 18 and 35 (Geurts et al. 2009) because contact is initiated by parents and grandparents when children are small (Brown 2003), while it is initiated by children when they grow up (Roberto and Stroes 1992). In addition, grandchildren may prefer peer relationships when they enter adulthood in order to obtain information and establish contacts (Carstensen 1992).

In addition, while children are small, grandparents can also provide emotional and mental support which may be an integral part of the development of the child while sometimes being impossible to be provided by the parents. For example, a study conducted in Boston College found that "an emotionally close relationship between grandparent and grandchildren is associated with fewer symptoms of depression for both generations."

6.2. POLICY IMPLICATIONS

The determinants of child development at an early age are important for the implementation of policies whose goal is to either improve child outcomes directly, or improve other economic outcomes without forfeiting child development.

First, if provision of child care by the grandparents has a positive influence on child outcomes, it may be worthwhile that governments take action to provide incentives to grandparents to assist in the child-rearing process of their grandchildren. Some countries in Europe have started to exploit this opportunity. For instance, the UK implemented such a policy in 2011. Grandparents who renounce work in order to take care of a grandchild under 12 at least 20 hours per week are eligible for national insurance credits contributing to their basic state

pension. In Portugal, working grandparents are entitled to claim a financial allowance and leave of work up to 30 days per year to provide child care to a sick grandchild. Germany and Hungary have transferable parental leave and allowances, i.e. parents are allowed to transfer leave to grandparents.

In addition, such policies may be found to have even greater impact than other Head Start programs which promote education and development at an early age. The reason is that if grandparental childcare provision has a positive impact on grandchildren or the effect is the same as that of care provided by the parents, then such programs could not only contribute to child development but can also be used as tools to improve other economic outcomes without negatively affecting child success. Such economic outcomes include improved fertility rates, and higher female labor participation rates. In particular, Del Boca (2002) shows that grandparenting is associated with higher probability of the mother being in the labor force and a higher probability that the family has had a child in the last two years (Del Boca, 2002). Receiving help from grandparents for child-rearing reduces the cost of childcare, and thus may increase fertility. In fact, a study of 11 countries in Europe provides evidence that receiving help from grandparents increases the likelihood of child-rearing, potentially because receiving help facilitates raising children and reduces child care costs. The effect is stronger in Southern Europe where public childcare is less common. Given that, policies encouraging time intergenerational transfer from grandparents to grandchildren may potentially increase fertility in Europe where natality rates have recently been declining. Such a reform is likely to contribute to the improvement of the demographic composition of European countries.

Receiving help from grandparents with child-rearing is also associated with higher female labor force participation rate (Ogawa et al. 1996; Del Boca 2002; Marenzi et al. 2008). Such an

increase in the available labor resources of a country can on its part improve the potential of these countries to produce goods and services and to initiate growth.

Therefore, if grandparents have a positive effect on children, then it might be worthwhile that governments attempt to implement policies encouraging grandparenting as a tool to improve child development and to solve other impending economic issues.

7. CONCLUSIONS

This article compares the effect of grandparents' and parents' investment of supervision time in a child on the cognitive, social and behavioral development of children during the first 6 years of life. We find that grandparents have a larger effect than parents on children's vocabulary skills. Although parents' supervision time on average influences the picture similarities score of cognitive ability of children more than does the time children are supervised by grandparents, grandparental care has a significant, positive impact larger than that of the parents provided that a child has been supervised by a grandparent at least 13 hours per week. This positive impact of grandparents' supervision time rises with an increase in the threshold of the time a grandparent supervises a grandchild at the expense of parental supervision time. However, parents influence social and behavioral development of children more than grandparents do, regardless of the number of hours the child has been supervised by each care provider. More specifically, the difference between the effects of an additional hour per week under the supervision of the grandparents and parents on the TD score is negative 0.1%, while the same difference for the NV score is 0.22%. The opposite signs for different development outcomes provide evidence that parents and grandparents complement rather than substitute each other in the mental and behavioral development of the child.

The article provides the grounds for the investigation of the effect of allocating supervision time, rather than material and financial assets, to children on their outcomes. However, the study also has limitations. First, none of the development measures we take into account is available for more than 2 or 3 years of childhood. This prevents a researcher from employing more dynamic panel data methods, such as Arellano-Bond, Arellano-Bover and Blundell dynamic approaches. Finding a unified measure of development available for more or all years of childhood analyzed in the study would solve this issue. It would even be sufficient to have different child outcomes for each year if they measure the same type of development, i.e. either cognitive, or social and behavioral.[6]

Second, we are able to identify the relative effect of grandparental time input as compared to the effect of time investment of the parents but fail to quantify the precise effect of grandparental care on child's outcomes. Estimating the latter effect would be a potential extension of the paper if one could obtain data on the "quality time" each provider spends with a child.

Finally, fixed effects estimation yields consistent estimates, given that all covariates are exogenously determined. In the case of Scotland, external care is a response to its exogenously determined availability[7] and cost[8] so exogenous factors drive the decision of the number of hours

[6] If the latter kind of data were available, we could apply a method similar to the one used by Bernal and Keane (2010, 2011). Under the assumption that the parameters in the production function are invariant across all test outcomes, they pool all scores, and run the main regression, but including dummy variables for each outcome, except one base outcome, as well as interaction terms of the score indicators with a subset of controls. Such a strategy improves efficiency due to the higher sample size, but is infeasible if the outcomes indicate different aspects of development.

[7] External child care is not available to all children and in all areas across Scotland, especially in some remote parts of the country. Some nurseries are willing to provide more openings but only if they can cover the delivery costs. In order to cover those costs, they either provide lower-quality care, or charge more than some parents are willing or able to afford. In either case, families often turn to alternatives either due to the shortage of childcare, or due to its lack of affordability.

[8] The cost of childcare in Scotland is high relative to other OECD countries, and it is rising much faster than the rate of inflation. According to Citizens Advice Scotland (CAS), such costs put many families at the threshold for poverty if they do not look for alternative sources of care. The Scottish government tried to alleviate the issue by increasing

of care provided by external care providers, and thus influence the utilization of its substitutes, such as parental and grandparental care. Our analysis can be generalized to countries where external child care is cost-prohibitive or restricted depending on the area. However, if similar analysis is conducted for another country where external care is affordable and ubiquitously available across all parts of the country, then childcare provision would not be randomly determined but instead, it would more likely be a result of a strategic choice of the parents, the grandparents or both. In that case, appropriate instruments have to be found to alleviate endogeneity concerns.[9]

Further research is needed to solve the above-mentioned limitations of this study. In addition, the analysis can be extended by accounting for more factors which are likely to affect development, and later-age outcomes can also be investigated.

free annual care from 475 to 600 hours for 3- and 4-year-old children, as well as for disadvantaged 2-year-old children. However, the majority of the childcare cost still has to be paid by the parents. More specifically, the approximate annual cost of placing a child under 2 in a nursery for 25 hours a week is £5514, and the same cost but for children between 2 and 5 is £5307 on average. The agency's reports show that families spend 27% of their annual household income on childcare, as compared to 12% on average across the OECD countries. Although not part of our study, childcare costs increased even further in 2016 and 2017. In 2017, the Family and Childcare Trust estimated that childcare costs became 4.5% higher as compared to the previous year. Such statistics incentivize families to search for alternatives, and parenting and grandparenting are the common solutions. Even if care provided by parents and/ or grandparents would not be the preferred choice of families, some of them have no alternative if external childcare is cost-prohibitive for them. Some families in which a grandparent is unavailable to serve as a childcare provider, even prefer one of the parents to not work rather than spend all or the majority of their income on child care.

[9] Additionally, as long as the number of hours of childcare provided by external sources is driven to a large extent by availability and affordability of care, and given the number of hours worked by the parents, the number of hours of grandparental involvement in the child-rearing process is mostly affected by these exterior factors. Furthermore, although supervision of the child in the past can affect present outcomes, reverse causality is unlikely in this context. Therefore there is little reason to expect that the allocation of supervision time between different care givers is endogenous. Despite that, it is worth mentioning that the estimates would be biased if grandparents' and parents' hours of care are not exogenous. This might be the case if part of the ability endowment of the child, and parents' or child's tastes for care are unobserved determinants of the outcomes of a child, and the time each provider devotes to a child is influenced by this endowment and preferences.

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TABLES

Table 1.1. Summary statistics of the continuous variables used in the analysis (by wave of the survey)

Variable	Grand Mean	Std. dev.	Min	Max	Mean Wave 1	Mean Wave 2	Mean Wave 3	Mean Wave 4	Mean Wave 5	Mean Wave 6
PSscore	15.142	4.253	0	31	-	-	14.318	-	16.016	-
NVscore	15.871	4.370	0	31	-	-	16.887	-	14.798	-
TDscore	27.535	4.602	1	35	-	-	-	27.275	27.471	27.856
HrsChdCareGrp	4.980	9.719	0	168	5.797	5.892	5.389	4.835	4.296	3.094
TotalNumPplInHhld	3.925	1.049	2	12	3.738	3.793	3.906	4.019	4.065	4.126
Income	23131.34	12716.47	1930.309	68965.52	20396.22	22309	23547.68	24189.38	24534.44	24751.31
NumSiblings	1.005	0.928	0	11	0.770	0.863	0.991	1.108	1.175	1.239
ChildBirthOrder	1.740	0.883	1	9	-	-	-	-	-	-
HoursMomWorksPerWk	15.873	17.482	0	97	24.961	5.212	3.566	31.118	26.095	3.379
HoursDadWorksPerWk	12.795	20.551	0	97	43.145	19.900	2.398	3.276	3.218	3.453
HrsOtherChdCare	7.700	12.735	0	168	7.021	9.512	11.532	8.271	5.635	3.588

Note: Source: Data are obtained from Growing Up in Scotland – Birth cohort 1 longitudinal dataset.

Table 1.2. Summary statistics of the binary (dichotomous) variables used in the analysis (by wave of the survey and total)

Variable	Percent Total	Percent Sw. 1	Percent Sw. 2	Percent Sw. 3	Percent Sw. 4	Percent Sw. 5	Percent Sw. 6
GrpInHhld	3.67	6.08	3.95	3.43	2.95	2.48	2.21
MotherInHhld	99.52	99.83	99.73	99.55	99.40	99.27	99.18
FatherInHhld	82.06	80.99	82.62	82.78	83.02	82.05	81.00
BothParInHhld	81.83	80.93	82.54	82.61	82.75	82.71	80.50
ParInHhldMarried	37.28	43.82	39.12	36.49	34.83	33.63	33.11
HigherEducMom	75.55	71.82	74.05	75.98	77.02	77.85	78.23
HigherEducDad	12.54	57.35	0.62	0.72	1.18	0.99	1.39
MaleChd	51.43	-	-	-	-	-	-
HealthChdNotGood	5.74	5.75	7.09	5.25	6.61	4.41	5.06

Note: Source: Data are obtained from Growing Up in Scotland – Birth cohort 1 longitudinal dataset.

Table 1.3. Estimates from regressions under different specifications of the effect of grandparental, parental and alternative child care supervision time input on child's TD score, years 2008 - 2010

Dependent variable: lnTDscore									
Variable	(1) Only current period hours	(2) Only current period hours	(3) Only current period hours	(4) (Combined) Average number of hours	(5) Average hours in the past & separately, present hours	(6) Current period hours and 1 lag	(7) Current period hours, lag 1, and lag 2	(8) Time FE included	(9) Time and residential area FE included
HrsChdCareGrp	-0.001***	-0.001***	-0.001***	-	-0.0012***	-0.0011***	-0.0011***	-0.001***	-0.0009***
	(0.0003)	(0.0003)	(0.0003)		(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
HrsOtherChdCare	-0.0003	-0.0002	-0.0002	-	-0.0003	-0.0002	-0.0003	0.00005	0.0001
	(0.0002)	(0.0002)	(0.0002)		(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
L.HrsChdCareGrp	-	-	-	-	-	-0.0003	-0.0004	-	-
						(0.0002)	(0.0003)		
L.HrsOtherChdCare	-	-	-	-	-	-0.0004**	-0.0004**	-	-
						(0.0002)	(0.0002)		
L2.HrsChdCareGrp	-	-	-	-	-	-	0.0001	-	-
							(0.0002)		
L2.HrsOtherChdCare	-	-	-	-	-	-	-0.0003*	-	-
							(0.0057)		
AvgHrsGrpCare	-	-	-	-0.0038***	-	-	-	-	-
				(0.0011)					
AvgHrsCC	-	-	-	-0.0021**	-	-	-	-	-
				(0.0008)					
L.AvgHrsGrpCare	-	-	-	-	-0.0011	-	-	-	-
					(0.0009)				
L.AvgHrsCC	-	-	-	-	-0.0017*	-	-	-	-
					(0.0007)				
Method	FE	RE	POLS	FE	FE	FE	FE	FE	FE
Time FE	No	No	No	No	No	No	No	Yes	Yes
Residential area FE	No	No	No	No	No	No	No	No	Yes
Excluded outliers	No	No	No	No	No	No	No	No	No
Obs.	10,724	10,724	10,724	10,098	10,098	10,417	10,234	10,724	10,723
(Within) R-sq	0.0110	0.0079	0.0079	0.0122	0.0134	0.0132	0.0140	0.0169	0.0171

Notes: All regressions are estimated using data from 2008 to 2010 for which the TD score is relevant. Robust standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10. Columns (1), (2) and (3) contain the results respectively, from FE, RE and pooled OLS regression models, using only the current number of supervision time provided by grandparents and external child care providers. Column (4) presents estimates from a FE model taking into account the average effect of involvement of grandparents, parents and alternative childcare from the first round of the survey until the present, while Column (5) shows the estimates from a model which distinguishes between the effect of the average supervision time in the past, and the current period time input into child development. Columns (6) and (7) represent the results obtained from the same model considered in (1) but with added lags of the time spent with the child by grandparents and external child care providers. One and two lags of the latter variables have been included in the models in Columns (6) and (7), respectively. Columns (8) and (9) present the results from the specification of the model in (1) with fixed effects accounting for different types of unobserved, time-invariant heterogeneity, specifically time and residential FE. Since the dependent variable is used in log terms, one has to multiply the estimate by 100 in order to obtain the effect of the corresponding variable on the TD score in percentages. The following variables have been used as controls: MaleChd, HigherEducMom, HigherEducDad, ChildBirthOrder, lnIncome, HealthChdNotGood, TotalNumPplInHhld, NumSiblings, BothParInHhld, and ParInHhldMarried.

Table 1.4. Estimates from regressions under different specifications of the effect of grandparental, parental and alternative child care supervision time input on child's NV score, years 2007 and 2009

Dependent variable: lnNVscore									
Variable	(1) Only current period hours	(2) Only current period hours	(3) Only current period hours	(4) (Combined) Average number of hours	(5) Average hours in the past & separately, present hours	(6) Current period hours and 1 lag	(7) Current period hours, lag 1, and lag 2	(8) Time FE included	(9) Time and residential area FE included
HrsChdCareGrp	0.0022***	0.0015***	0.0015***	-	0.0020***	0.0018**	0.0019**	0.0010	0.0010
	(0.0007)	(0.0004)	(0.0004)		(0.0008)	(0.0008)	(0.0009)	(0.0007)	(0.0007)
HrsOtherChdCare	0.0028***	0.0018***	0.0018***	-	0.0029***	0.0030***	0.0024***	0.0002	0.0002
	(0.0004)	(0.0003)	(0.0003)		(0.0004)	(0.0004)	(0.0005)	(0.0004)	(0.0004)
L.HrsChdCareGrp	-	-	-	-	-	0.0011	0.0008	-	-
						(0.0008)	(0.0008)		
L.HrsOtherChdCare	-	-	-	-	-	-0.0003	0.0001	-	-
						(0.0005)	(0.0005)		
L2.HrsChdCareGrp	-	-	-	-	-	-	0.0004	-	-
							(0.0005)		
L2.HrsOtherChdCare	-	-	-	-	-	-	-0.0012**	-	-
							(0.0006)		
AvgHrsGrpCare	-	-	-	-0.0071***	-	-	-	-	-
				(0.0020)					
AvgHrsCC	-	-	-	-0.0060***	-	-	-	-	-
				(0.0013)					
L.AvgHrsGrpCare	-	-	-	-	0.0024	-	-	-	-
					(0.0015)				
L.AvgHrsCC	-	-	-	-	-0.0021*	-	-	-	-
					(0.0012)				
Method	FE	RE	POLS	FE	FE	FE	FE	FE	FE
Time FE	No	No	No	No	No	No	No	Yes	Yes
Residential area FE	No	No	No	No	No	No	No	No	Yes
Excluded outliers	No	No	No	No	No	No	No	No	No
Obs.	7,123	7,123	7,123	6,871	6,871	6,982	6,914	7,123	7,123
(Within) R-sq	0.0590	0.0301	0.0301	0.0502	0.0628	0.0619	0.0636	0.1485	0.1524

Notes: All regressions are estimated using data from 2007 and 2009 for which the NV score is relevant. Robust standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10. Columns (1), (2) and (3) contain the results respectively, from FE, RE and pooled OLS regression models, using only the current number of supervision time provided by grandparents and external child care providers. Column (4) presents estimates from a FE model taking into account the average effect of involvement of grandparents, parents and alternative childcare from the first round of the survey until the present, while Column (5) shows the estimates from a model which distinguishes between the effect of the average supervision time in the past, and the current period time input into child development. Columns (6) and (7) represent the results obtained from the same model considered in (1) but with added lags of the time spent with the child by grandparents and external child care providers. One and two lags of the latter variables have been included in the models in Columns (6) and (7), respectively. Columns (8) and (9) present the results from the specification of the model in (1) with fixed effects accounting for different types of unobserved, time-invariant heterogeneity, specifically time and residential FE. Since the dependent variable is used in log terms, one has to multiply the estimate by 100 in order to obtain the effect of the corresponding variable on the NV score in percentages. The following variables have been used as controls: MaleChd, HigherEducMom, HigherEducDad, ChildBirthOrder, lnIncome, HealthChdNotGood, TotalNumPplInHhld, NumSiblings, BothParInHhld, and ParInHhldMarried.

Table 1.5. Estimates from regressions under different specifications of the effect of grandparental, parental and alternative child care supervision time input on child's PS score, years 2007 and 2009

Dependent variable: lnPSscore									
Variable	(1) Only current period hours	(2) Only current period hours	(3) Only current period hours	(4) (Combined) Average number of hours	(5) Average hours in the past & separately, present hours	(6) Current period hours and 1 lag	(7) Current period hours, lag 1, and lag 2	(8) Time FE included	(9) Time and residential area FE included
HrsChdCareGrp	-0.0022**	-0.0013***	-0.0013***	-	-0.0022**	-0.0022**	-0.0021*	-0.0008	-0.0008
	(0.001)	(0.0005)	(0.0005)		(0.0010)	(0.0011)	(0.0012)	(0.001)	(0.001)
HrsOtherChdCare	-0.0026***	-0.0015***	-0.0015***	-	-0.0026***	-0.0029***	-0.0026***	0.0006	0.0006
	(0.0005)	(0.0003)	(0.0003)		(0.0005)	(0.0006)	(0.0007)	(0.0006)	(0.0006)
L.HrsChdCareGrp	-	-	-	-	-	-0.0003	-0.0003	-	-
						(0.0012)	(0.0013)		
L.HrsOtherChdCare	-	-	-	-	-	0.0007	0.0006	-	-
						(0.0007)	(0.0007)		
L2.HrsChdCareGrp	-	-	-	-	-	-	7.85e-06	-	-
							(0.0008)		
L2.HrsOtherChdCare	-	-	-	-	-	-	0.0007	-	-
							(0.0008)		
AvgHrsGrpCare	-	-	-	-0.0054*	-	-	-	-	-
				(0.0030)					
AvgHrsCC	-	-	-	-0.0046***	-	-	-	-	-
				(0.0018)					
L.AvgHrsGrpCare	-	-	-	-	-0.0004	-	-	-	-
					(0.0022)				
L.AvgHrsCC	-	-	-	-	0.0025	-	-	-	-
					(0.0018)				
Method	FE	RE	POLS	FE	FE	FE	FE	FE	FE
Time FE	No	No	No	No	No	No	No	Yes	Yes
Residential area FE	No	No	No	No	No	No	No	No	Yes
Excluded outliers	No	No	No	No	No	No	No	No	No
Obs.	7,145	7,145	7,145	6,891	6,891	7,002	6,934	7,145	7,145
(Within) R-sq	0.0274	0.0190	0.0190	0.0236	0.0309	0.0307	0.0309	0.0920	0.0941

Notes: All regressions are estimated using data from 2007 and 2009 for which the PS score is relevant. Robust standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10. Columns (1), (2) and (3) contain the results respectively, from FE, RE and pooled OLS regression models, using only the current number of supervision time provided by grandparents and external child care providers. Column (4) presents estimates from a FE model taking into account the average effect of involvement of grandparents, parents and alternative childcare from the first round of the survey until the present, while Column (5) shows the estimates from a model which distinguishes between the effect of the average supervision time in the past, and the current period time input into child development. Columns (6) and (7) represent the results obtained from the same model considered in (1) but with added lags of the time spent with the child by grandparents and external child care providers. One and two lags of the latter variables have been included in the models in Columns (6) and (7), respectively. Columns (8) and (9) present the results from the specification of the model in (1) with fixed effects accounting for different types of unobserved, time-invariant heterogeneity, specifically time and residential FE. Since the dependent variable is used in log terms, one has to multiply the estimate by 100 in order to obtain the effect of the corresponding variable on the PS score in percentages. The following variables have been used as controls: MaleChd, HigherEducMom, HigherEducDad, ChildBirthOrder, lnIncome, HealthChdNotGood, TotalNumPplInHhld, NumSiblings, BothParInHhld, and ParInHhldMarried.

Table 1.6. Estimates from a SUR model	
Outcome: Log of picture similarity raw score, 2007 (age 3)	
HrsChdCareGrp	-0.0011 (0.001)
Outcome: Log of naming vocabulary raw score, 2007 (age 3)	
HrsChdCareGrp	0.0007 (0.001)
Outcome: Log of picture similarity raw score, 2009 (age 5)	
HrsChdCareGrp	-0.0008* (0.0005)
Outcome: Log of naming vocabulary raw score, 2009 (age 5)	
HrsChdCareGrp	0.0007* (0.0004)
Breusch-Pagan test of independence:	
Chi2 – statistic	1698.943
p-value	0.000

Notes: All estimates are obtained from the estimation of a SUR model consisting of a system with 4 equations. The variables taken into account are the following: MaleChd, HigherEducMom, HigherEducDad, ChildBirthOrder, HrsChdCareGrp, HrsOtherChdCare, lnIncome, HealthChdNotGood, TotalNumPplInHhld, NumSiblings, BothParInHhld, and ParInHhldMarried. All of them are considered only in the current time period. Robust standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10.

Table 1.7. Test of the hypothesis that grandparents accept a parents' role if they supervise a grandchild more than a certain amount of time per week: Effect of grandparents' supervision time for various thresholds of grandparents' supervision time

Hours of grandparents' supervision time per week	(1) Effect on TD score	(2) Effect on NV score	(3) Effect on PS score
<20	-0.0011***	0.0032***	-0.0050***
	[0.0003]	[0.0010]	[0.0013]
<30	-0.0011***	0.0029***	-0.0047***
	[0.0003]	[0.0008]	[0.0012]
≥ 10	-0.0013***	0.0007	0.0018
	[0.0004]	[0.0011]	[0.0015]
≥ 12	-0.0013***	0.0007	0.0024
	[0.0004]	[0.0012]	[0.0016]
≥ 13	-0.0013***	0.0007	0.0034**
	[0.0004]	[0.0013]	[0.0017]
≥ 15	-0.0013***	0.0008	0.0034**
	[0.0004]	[0.0013]	[0.0017]
≥ 20	-0.0014***	0.0009	0.0052**
	[0.0004]	[0.0017]	[0.0021]
≥ 30	-0.0015***	0.0047	0.0092***
	[0.0005]	[0.0032]	[0.0020]

Notes: All regressions are estimated using FE estimation using the years for which each score is relevant. Robust standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10. Only current period hours different care providers supervise the child are taken into account. The table provides the effect of supervision time provided by grandparents under different restrictions on grandparental supervision time. Column (1) presents the effect of grandparents' supervision time on the TD score of social and behavioral development. Column (2) shows the effect of the same variable on the NV score of cognition or the vocabulary skills of the grandchild, and Column (3) presents the effect of the supervision time provided by grandparents on PS score of cognitive ability of the grandchild.

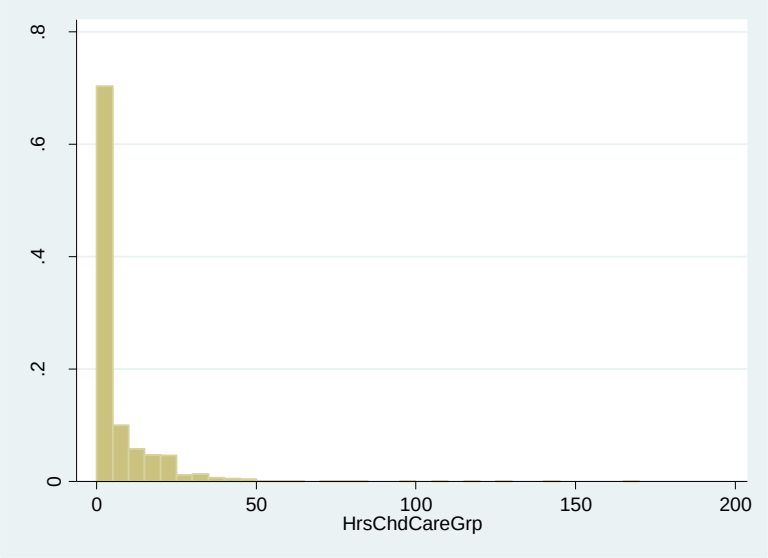
Table 1.8. Diagnostic tests of the FE estimation of the effect of grandparental, parental and alternative childcare time input on child TD score (years 2008 – 2010), NV and PS score (years 2007 and 2009)

Verification test\ Results	(1) TD	(2) NV	(3) PS
Hausman test: FE vs. RE Ho: non-systematic differences in coefficients	Chi2 = 162.46 p-value = 0.000 Rejection of Ho, use FE	Chi2 = 154.89 p-value = 0.000 Rejection of Ho, use FE	Chi2 = 41.88 p-value = 0.000 Rejection of Ho, use FE
Breusch-Pagan LM test for random effects: Pooled OLS vs. RE	Chibarsq = 3501.76 p-value = 0.000 Rejection of Ho, RE is preferred to pooled OLS	Chibarsq = 316.03 p-value = 0.000 Rejection of Ho, RE is preferred to pooled OLS	Chibarsq = 95.59 p-value = 0.000 Rejection of Ho, RE is preferred to pooled OLS
Test for time fixed effects (testparm)	F-statistic = 16.14 p-value = 0.0000 Rejection of Ho, Presence of time effects	F-statistic = 289.81 p-value = 0.000 Rejection of Ho, Presence of time effects	F-statistic = 201.08 p-value = 0.000 Rejection of Ho, Presence of time effects
Modified Wald test for group-wise heteroskedasticity in FE model	Chi2 = 2.3e+35 p-value = 0.0000 Rejection of Ho, Exists evidence of heteroscedasticity Solution: usage of robust standard errors	Chi2 = 8.9e+34 p-value = 0.0000 Rejection of Ho, Exists evidence of heteroscedasticity Solution: usage of robust standard errors	Chi2 = 9.1e+35 p-value = 0.0000 Rejection of Ho, Exists evidence of heteroscedasticity Solution: usage of robust standard errors

Notes: All diagnostic checks have been conducted based on the FE regression model whose results have been presented in Tables 5 (Column 1), 6 (Column 1) and 7 (Column 1). Columns (1), (2) and (3) of this table present the results from the verification tests for the regressions with dependent variables, respectively TD score, NV score, and PS score.

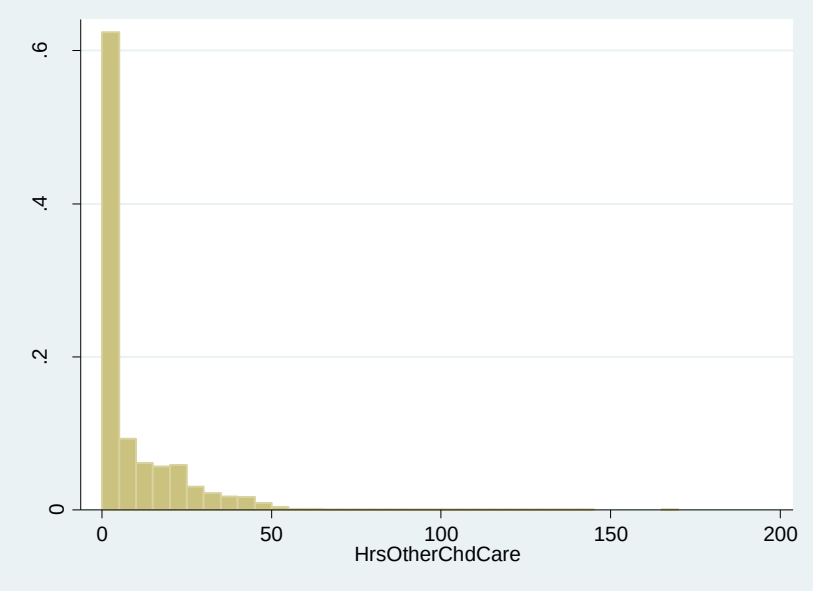
FIGURES

Figure 1.1. Distribution of the number of hours grandparents supervise grandchildren per week



Notes: The graph represents the distribution of the number of supervision hours provided by grandparents. Data are obtained from Growing Up in Scotland – Birth Cohort 1.

Figure 1.2. Distribution of the number of hours other childcare providers supervise a child per week



Notes: The graph represents the distribution of the number of supervision hours provided by external care providers. Data are obtained from Growing Up in Scotland – Birth Cohort 1.

CHAPTER 2:

THE EFFECT OF FAMILY WELFARE SUPPORT ON THE LIKELIHOOD OF HAVING ANOTHER CHILD AND PARENTS' LABOR SUPPLY[10]

ABSTRACT

Governments have recently attempted to reverse the below-replacement fertility rates in Europe by reducing child-rearing costs through child benefits, grants and paid leaves. This article examines the causal effect of family allowances on the likelihood of having another child, and on the extensive and intensive margins of labor supply. Evidence from Switzerland suggests that higher child benefits incentivize parents to have more children but do not affect their employment choice. The effect is larger for low-income families. These findings imply that policies aimed at improving the economic well-being of families are likely to increase fertility rates without distorting labor market outcomes.

Keywords: allowance, fertility, employment, endogeneity, instrument, DD

[10] I thank helpful comments from Andrei Barbos, Padmaja Ayyagari, and Joshua Wilde.

1. INTRODUCTION

Child allowances, or benefits regularly granted to families to partially compensate the expenses associated with child-rearing and to facilitate the balance between work and family responsibilities, can be an important source of income support, especially for low-income families. Because family benefits reduce the financial burden of raising a child, they are expected to have a positive impact on fertility. That is why many countries provide financial assistance to families with children, especially in European countries where the recent decline in the fertility rate contributes to an aging population and raises concerns about future labor supply and sustainability. However, due to the costs such policies impose on local governments, it is useful to evaluate the potential impact of the implementation of programs providing family grants.

The effect of such policies on fertility, mothers' or parents' labor supply, child outcomes and maternal mental health has been studied by prior authors in the context of Canada, Hungary, Norway, Spain, and Australia. This article extends previous literature in several ways. First, we explore the effect of family welfare support through child allowances on the likelihood of having another child, the extensive margin of labor supply measured by the dichotomous outcome of being employed or not, and the intensive margin of labor supply captured by the hours worked by the parents. More generally, we investigate whether the benefits received by Swiss families have an impact on family decision-making.

As a measure of the actual amount of allowances families receive we employ the benefits reported by respondents in the Swiss Household Panel survey. However, there is a discrepancy between the reported and the actual amount of assistance, attributable to one of the following reasons: respondents' misunderstanding of the survey question asking them to report the benefits they receive, reporting additional benefits provided by some but not all employers at their

discretion, or misreporting due to imperfect information or inability to distinguish between different income sources when salary and child allowances are received simultaneously.

In either case, a mismatch between the reported benefits and the actual amount received indicates that the reported allowances are a noisy measure of the actual ones. Using an imperfect measure of the actual assistance creates an attenuation bias in the OLS estimates of the effect of allowances on the outcomes of interest. In order to avert this bias, we adopt an instrumental variable approach. Specifically, we use the eligibility amount of benefits, as well as indicators reflecting a change in the allowances after the introduction of the Federal Family Allowance Act (FamZG) which imposed a federal floor on child benefits, common to all 26 cantons in Switzerland[11] as instruments for the reported benefits. The proposed instruments have not been used by previous authors in the context of Switzerland.

Finally, in addition to examining the effect of child allowances on fertility and labor market outcomes, we also study a second, related but distinct matter. Specifically, we identify the causal impact of the introduction of a universal floor on child benefits in all cantons in Switzerland.

The findings indicate that an increase in the family support by 1% improves the likelihood of having another child by 0.01%, but does not affect labor market outcomes. Moreover, the analysis of the effect of the introduction of a minimum amount of child allowances all cantons are obliged to pay indicates that a floor on benefits leads to a significant increase in the likelihood of having a child by 4.3% in affected cantons relative to unaffected ones while there is no

[11] Our approach is similar to that of Milligan et al. (2011) in that we also consider both the reported and the eligibility amount of benefits. However, while Milligan et al. (2011) examines the effect of those amounts on child mental, physical and test scores and mother's deprivation level, we use the eligibility amount as an instrument for the reported amount. We also investigate the impact of the benefits received by individuals on a different set of outcomes.

significant difference between the differential effect of the policy on labor supply in the treated cantons and the controls.

From a policy perspective, these results imply that providing financial support to families to lower the cost of raising children is likely to positively affect fertility without having an adverse effect on the labor market in countries where federal decision-makers aim to improve fertility rates. In addition, such a reform would have a greater positive impact if targeted at families at the lower tail of the household income distribution.

The remainder of this paper is structured as follows. Section 2 reviews prior literature, and provides an overview of the federal law about child allowances in Switzerland. Section 3 presents the identification strategy. In Section 4, we describe the data used for the empirical analysis. Section 5 contains a summary of the findings. In Section 6, we examine the causal effect of the imposition of a universal floor on benefits in all cantons in Switzerland. Section 7 discusses a potential harvesting effect and the policy implications of the study, and Section 8 concludes the paper.

2. Literature Review

2.1. Prior Literature on Child Benefits

In some countries where a decline in the fertility rate has been observed, governments have introduced child benefits, family allowances, child tax credits and tax exemptions, to incentivize families to have more children. Prior papers which investigate the impact of such family-incentivizing policies on fertility find a positive effect (Zhang et al. 1994; Gauthier et al. 1997; Gabos et al. 2009; and Gonzalez 2013). For example, Zhang et al. (1994) found a positive, significant effect of child tax credit, family allowances, and tax exemption on the fertility rate in

Canada from 1921 to 1988. Gauthier et al. (1997) found that governmental support in the form of family allowances had a positive, but economically small effect on fertility in 22 OECD countries between 1970 and 1990. The results of Gabos et al. (2009) suggested a small, positive effect of child benefits on fertility rate in Hungary from 1950 to 2006. The magnitude of this effect increased with birth order up to order 4 (and then, dropped to the effect found on first-order birth), implying that the elasticity of demand was higher for higher birth orders, i.e. higher-order births were more responsive to child benefits.

Another study, conducted by Gonzalez (2013), examined the effect of a one-time child benefit of 2500 euros introduced unexpectedly in Spain on fertility, household expenditure patterns, and mother's labor supply. The findings suggested that the benefit led to an increase in fertility by 6% per year, and there was no significant change in the consumption of child-related goods and services. After the introduction of the child benefit, eligible mothers also stayed out of the labor force longer after delivery.

The implementation of policies introducing child benefits does not affect only fertility but also child and maternal outcomes, as well as parents' employment. Specifically, Havnes et al. (2011) found that an expansion of subsidized child care had a positive, long-run impact on the educational achievement of children in Norway, and the largest effect was extracted from the subsample of girls and children of low-educated mothers (Haynes et al. 2011). Milligan and Stabile (2009) also investigated the impact of benefits on family outcomes in the context of the expansion of family benefits for children under the age of 5 in the province of Manitoba in Canada in 2001 and the subsequent expansion to age 11 in 2003. Both reforms allowed recipient families to invest more in children, while working less and spending more time with them at home. Milligan et al. (2009) examined the effect of the policy change by comparing the treated

families from Manitoba to those from the rest of Canada, but found no significant effect of the reform on labor supply and income after the policy change in 2001, positive effects on social assistance income and child outcomes, and a difference in the effect on child outcomes for girls and boys (Milligan et al. 2009). The latter paper by Milligan et al. (2009) has been further extended by Milligan et al. (2011)[12] who studied the effect of both reported and derived from eligibility criteria (based on year, province, number of children and/ or income) child benefits in Canada on child mental, physical, and test scores, and mother's deprivation level.[13] They found that an expansion of child benefits had a positive impact on child scores and maternal mental health. The reform also had a larger effect on mental health for girls than for boys, while the impact on physical health and educational outcomes was stronger for boys than for girls (Milligan et al. 2011).[14] Similarly to Milligan et al. (2011), we look at both reported and eligibility amounts, but unlike them, we use the eligibility amount as an instrument for the benefits received. We also discuss the effect of these allowances on a different set of outcomes.

[12] Milligan et al. (2011) paper was motivated by Currie and Stabile (2009) and Currie et al. (2010) who find a strong relationship between child early mental health and both short- and long-run educational outcomes, and Oreopoulos (2008) who finds a strong relationship between shocks to parental socioeconomic status, specifically the displacement of the father from work, and child well-being measured by future earnings and unemployment.

[13] Since OLS estimates may be biased if individuals who receive benefits are systematically different from those who do not, Milligan et al. (2011) use variation in eligibility for benefits, which is not related to individual characteristics, but only to the number of children and province (an approach similar to the one proposed by Currie and Gruber, 1996). More precisely, they take a random sample from the SLID dataset, and simulate the benefits each family would be eligible for in each province, in each year from 1994 to 2004, and for each number of children combination. Then, using family characteristics, they impute the simulated values (child benefits the family is eligible for) in the NLSCY dataset, use the simulated cells as instruments for benefits and perform a 2SLS estimation.

[14] The motivation of almost all authors who examine child benefits, including Milligan et al. (2011), relies on insights offered by Yeung et al. (2002). Yeung et al. (2002) describes the channels through which child benefits can improve child outcomes. He distinguishes between a direct ("family resources") channel and an indirect ("family process") channel. According to the former channel, higher family income allows families to purchase more goods (food, books, etc.) for the child, which has a positive effect on him/ her. The "family process" channel suggests that higher family income reduces stress, and thus, improves household relations and family well-being, which benefits both parents and children, and improves child outcomes by facilitation of learning, improvement of ability to function, and so on. In addition to this theory, Yeung et al. (2002) explore the validity of the two channels using data from the US. His empirical results provide strong evidence only for the "family" channel: higher family income reduces parents' stress about whether they provide adequate resources for the child, which improves parents – children interaction, and thus, boosts child's outcomes (Yeung et al. 2002).

In addition to Milligan et al. (2009) and Gonzalez (2013), Guest (2013) also explored the effect of child benefits and family allowances on employment. He developed a utility-maximization theoretical model of at-birth support, cash family benefits and childcare subsidies, calibrated it, and estimated it empirically. His findings suggested that a reduction in childcare expenditure induced by a government childcare subsidy imposed on employed parents led to an increase in household labor supply in Australia.

Our article differs from prior literature in the following ways. First, it examines the case of Switzerland, and provides evidence that the effect of family allowances on fertility and labor market outcomes of the parents can be quite different when measurement errors are taken into account. Second, we propose instruments to control for attenuation bias due to this measurement error. Finally, separately from the IV approach, we examine the causal effect of the introduction of a federal floor on child benefits in Switzerland on family decision-making. To the best of our knowledge, no previous paper has investigated the impact of this policy change.

2.2. Background Information

Family allowances in Switzerland include child and education allowances given to families to facilitate child rearing by compensating at least part of the costs of raising a child. The Swiss Family Allowance Law (Familienzulagengesetz) was announced in 2006, and came into effect in 2009. Its purpose was to harmonize the legal systems of various cantons by entitling all families, regardless of the canton, to a legal minimum of child benefits of 200 Swiss francs (appr. $181) per month per child under 16. Although all cantons are obliged to comply with the minimum allowance federal law, they are also allowed to grant aid larger than the statutory floor. Payments are not automatically transferred but rather a parent has to submit an application which then, has

to be approved by a cantonal office. Either of the two parents can apply for allowances but only one allowance is permitted per child.

In order to be eligible for allowances, a person has to satisfy the following two requirements. First, he has to live in Switzerland. Second, he must be either employed/ self-employed and earn at least 587 Swiss francs per month, or not employed and not receiving more than 42,300 Swiss francs of annual taxable income. If an applicant lives in a EU or EFTA country but commutes to Switzerland, (s)he is entitled to the benefits in Switzerland as well. Natural, adoptive, foster and step parents, as well as grandparents and elder siblings who provide care are eligible to apply. Employed individuals have to submit an application to the employer. Then, the employer has to transfer the application to a relevant family compensation fund administered at cantonal level. Therefore applications of employed parents are submitted in the canton of employment rather than the canton of residence. After submission, a compensation fund processes the application, and the employer pays the monthly allowance to the employee simultaneously with the monthly salary. Unemployed and self-employed parents have to submit an application directly to the family compensation fund in their canton of residence.

The family compensation fund is funded by employers and self-employed individuals. In particular, employers are obliged to contribute to the fund an amount equal to some percentage of the salary of eligible workers. Self-employed people make contributions to the fund as well, cantons contribute to the fund for unemployed people and are allowed to impose taxes to finance the system. Valais is the only canton where employers require employees to contribute to the fund.

3. Empirical Framework and Identification

This section presents the empirical model and the identification strategy. Subsections 3.1 and 3.2 specify respectively, a preliminary model and an instrumental variable approach for studying the effect of child benefits on fertility and labor market outcomes. We discuss identification challenges in Subsection 3.2.1.

3.1. Model Specification

We explore the effect of child allowances on the likelihood of having a first or a higher order child and the extensive and intensive margins of labor supply. The extensive margin is captured by the binary outcome of being employed or not, while the intensive margin is measured by the number of hours worked by the parents.

Because two of the outcomes are binary and one is quantitative, we estimate two Probit and one OLS regression models specified as follows:

$$Prob(another\ child)_{it} = \Phi(\varphi lnAllowancesTotal_{it} + \boldsymbol{\beta X}_{it} + \alpha_i + \tau_t + \epsilon_{it}) \quad (1)$$

$$Prob(parent\ is\ employed)_{it} = \Phi(\varphi lnAllowancesTotal_{it} + \boldsymbol{\beta X}_{it} + \alpha_i + \tau_t + \epsilon_{it}) \quad (2)$$

$$Hours\ Worked_{it} = \varphi lnAllowancesTotal_{it} + \boldsymbol{\beta X}_{it} + \alpha_i + \tau_t + \epsilon_{it} \quad (3)$$

In these equations, $Prob(another\ child)$, $Prob(parent\ is\ employed)$, and $Hours\ Worked$ are the previously mentioned dependent variables used in the analysis. $AllowancesTotal_{it}$ captures the annual amount of benefits families have received, and varies across individuals (based on the number and age of children), cantons and time. We use natural logarithm of this variable. The set of controls $\boldsymbol{X}_{it}$ captures personal characteristics of individual i at time period t. The term α_i denotes individual-specific fixed effects, or unobserved, time-invariant cross-

sectional heterogeneity, and τ_t are time trends which allow for systematic differences across years. Finally, ϵ_{it} is an idiosyncratic error term. Subscript i identifies individuals and subscript t denotes time.

The set of controls employed in the regression for the probability of having another child includes age, education and hours worked per week of the individual, net household income, number of children and age of the youngest child in the prior period, and indicators for whether the individual is married and whether (s)he is in average or better health. In equations (2) and (3), we control for the number of children, household income, marital status of the parents, their presence in the household, age, health status and educational attainment of the respondent, income of the spouse, number of people in the household, age of the youngest child, presence of a child in worse than the average health, and incidence of problems with children.

In addition to estimating (1), (2) and (3) in the whole population, we also estimate them in various subsamples. Specifically, we distinguish between high and low-income families, where a high-income family is defined as one whose annual household income exceeds the average annual income of the families in the sample for the given year. The rationale behind estimating the effects for families earning above and below-average income separately is that child allowances are expected to have a larger impact on low-income individuals for whom the assistance contributes to a larger percentage of their wealth.

3.2. TWO-STEP MODEL SPECIFICATION

A mismatch between the amount of benefits individuals report and the true amount they receive indicates that the reported benefits are a noisy measure of the actual allowances. Using an imperfect measure of the actual support leads to an attenuation bias in the OLS estimates of the

effect of allowances on the outcomes in models (1), (2) and (3) due to a measurement error in a key explanatory variable.

Reported benefits above the actual amount indicate that either the family is a recipient of "excessive" support from non-federal sources, usually employers, or the respondent has imperfect information about the amount the family receives, or there is an error in the reported amount. In contrast, reporting benefits below the actual amount signals either imperfect information about the allowances received and perceptions lower than the true amount, or an error in the reported benefits. A family is likely to perceive lower allowances because when the parent submitting the application is employed, his/ her child benefits are received simultaneously with the employment income. Thus it is often difficult to distinguish between salary and allowances.

Given that a major covariate used in the analysis is measured imperfectly, a remedy aimed to correct for attenuation bias and endogeneity driven by this measurement error is required. As a solution, we adopt a 2-stage least square approach with instruments for the actual benefits. In what follows we motivate the choice of instrumental variables.

We consider the introduction of the Federal Act on Family Allowances, better known as FamZG, which standardized cantonal laws concerning minimum child benefits. Based on the eligibility criterion, we derive the amount each family is entitled to receive, and use the simulated values (EligibilityAmnt) as instruments for the actual amount of benefits. Additional instruments we take into account in some of the specifications rely on the facts that FamZG was passed in March 2006 but came into effect on January 1st, 2009 when the standardization of benefits was implemented. Given this, the additional instruments we utilize include an indicator for the transition period, i.e. the period between the time when the law was introduced and the time when

it came into effect (DBn2006and2009), and an indicator for the post-reform period (D2009andAfter).

The eligibility amount of allowances by legislation captures the family-level variation in the actual amount of benefits received which is determined by changes in the amount families are entitled to within cantons across time. Therefore the estimated allowances are highly correlated with the endogenous actual allowances, but uncorrelated with other determinants of the outcomes. There are also no confounding canton-year-number of children trends, and there are no other policies which make the exclusion restriction invalid. As a result all conditions for acceptably strong instruments have been satisfied.

In addition to this theoretical motivation, in order to formally verify that the chosen instruments are good predictors of the actual benefits and are acceptably strong, we use the conventional criteria based on the F-test from the first-stage regression, proposed by Stock et al. (2002).

Given that the instruments are not weak, the reduced-form equation would be a regression of the imputed benefits on the benefits a family is eligible for and exogenous covariates controlling for observable characteristics. The first and second stage of the two-stage least squares (2SLS) approach, respectively, have the following forms:

$$lnAllowancesTotal_{it} = \boldsymbol{\xi Z_{it}} + \boldsymbol{\gamma X}_{it} + \alpha_i + \tau_t + \varepsilon_{it} \qquad (4)$$

$$Outcome_{it} = \mu + \varphi \widehat{lnAllowancesTotal_{it}} + \boldsymbol{\beta X}_{it} + \alpha_i + \tau_t + \epsilon_{it} \qquad (5)$$

where $\boldsymbol{Z_{it}}$ is a vector of instruments legitimate for individual i at time period t, and $\widehat{lnAllowancesTotal_{it}}$ are the predicted values from the first stage.

3.2.1. IDENTIFICATION CHALLENGES

In this subsection, we discuss the challenges to our identification strategy.

First, canton of residence must be exogenous in order for the identification strategy to be valid. Canton of residence is exogenous under the assumption that families do not move in order to obtain higher benefits. Given the relatively small amount of the allowances compared with the average household income, this is unlikely because the costs of the change would exceed its benefits. However, this assumption imposes limitations on our study if families change cantons driven by the amount of benefits or unobserved factors.

Another potential concern is that our analysis implicitly assumes constant marginal reported benefits for all children of one individual, regardless of child birth order. This assumption might be violated given that some cantons provide a significantly larger amount of allowances for higher-order children which contributes to a considerable change in individuals' family planning and labor market outcomes. However, it is unlikely that this would be of a concern in our analysis because the amount of child allowances varies with birth order only in 8 cantons[15] in Switzerland, it differs between first and second child only in one canton and the difference is about 15 Swiss francs, and the gap between the benefits provided for a third and higher-order children is not of high magnitude. Specifically, in 6 out of the 8 cantons where allowances vary with child order, the increase in benefits for a third child is between 5 and 50 Swiss francs, or in the range from 2.78% to 16.25% in percentage terms. Only the increase in benefits for a third child in Valais and Vaud vary respectively, between 84 and 100 Swiss francs (or 32.23-36.36%), and between 120 and 170 Swiss francs (or approximately 100%), depending on the year. Although we do not

[15] The cantons in which child benefits are a function of child order are Zug, Fribourg, Appenzell Innerrhoden, St. Gallen, Vaud, Valais, Neuchatel and Jura.

expect that our findings would be affected by the above-mentioned assumption, to formally explore the possibility of spurious results due to its failure, we run all regressions using data excluding the canton of Vaud in which benefits significantly increase for higher-order births, separately leaving out Vaud and Valais, and finally, again distinctly dropping the 8 cantons where the amount of benefits for third and higher-order children is higher than the amount granted for first and second child. The results are reported in Section 5.2 where we discuss the robustness checks. They are robust to the exclusion of the above-mentioned cantons.

4. Data and Descriptive Statistics

This section discusses the data used for the empirical analysis and their sources. In Subsection 4.1, we describe the variables, and the way some of them have been constructed. Subsection 4.2 presents summary statistics.

4.1. Variables

For this empirical work, we merge individual and family-level data from the Swiss Household Panel (SHP) for the period from 2004 to 2016. The major purpose of SHP is to trace the social dynamics in Switzerland by interviewing the same representative sample of individuals every year. It records information about households, income, employment, leisure, health, quality of life, values, and so on. Individuals were also asked to report the amount of child benefits they receive every year. In this article, we refer to these allowances as reported benefits. They are the level that is perceived by the respondents in the survey. We use them as a proxy for the actual support, and identify their impact on family decision-making.

In addition to the SHP, we use administrative data about the family allowances households are eligible for, based on certain criteria, collected from the Federal Social Insurance Office

(FSIO) in Bern, Switzerland, and from the Cantonal Compensation Offices in each of the 26 cantons in Switzerland. Each canton started to implement or expand allowances at different times and at different paces. Therefore the amount families are eligible for, whose precise amounts are presented in Appendix B1, differs across cantons, years, number and age of children. We use these geographical and temporal sources of variation in order to calculate the amount of child benefits a family is entitled to receive based on the above-mentioned criterion, and given that it applies for allowances. The latter variable is what we refer to as eligibility amount of benefits in this article.

Then, we match the SHP dataset with the eligibility benefits based on the data collected from the Cantonal Offices. The matching is based on canton, year, and number of children belonging to the age groups under 12, between 12 and 15, 15, and 16 and above. We use data from the canton of residence rather than the canton of employment because the SHP contains information only about the canton of residence but not about the canton where the parents work although benefits received by each family are based on the canton of employment of the applicant. We employ both the reported amount of benefits received by each family, and the amount of benefits each family is entitled to receive. The latter two variables serve respectively, as a main explanatory and an instrumental variable, as previously explained in section 3.2.

It is important that the reported and the actual amounts are correlated but not identical. From correspondence with the Swiss Center of Expertise in Social Sciences (FORS) in Switzerland, we obtained a better understanding of what the "family and child benefits" variable in the dataset includes and what drives the difference. Respondents report allowances as they understand the term, and FORS is not able to verify the reported amount with the registry. Therefore errors due

to misunderstanding and/ or misreporting are inevitable. Apart from these errors, there are several configurations in which a difference is expected.

The first, most commonly observed reason for the discrepancy, is that there is no clear definition of what child and family allowances are. There are mandatory allowances defined by federal floors and cantonal legislation, but many employers grant their workers with supplementary, non-compulsory benefits, which are termed based on the companies' preferences. Second, families sometimes receive allowances for children who live outside of the household. If the parents are separated or divorced, the parent living with the child usually receives the allowance. In case this parent is not employed or self-employed, the employed parent usually receives the benefits but is required to forward them to the parent residing with the child. It is not clear in the dataset whether the parent living with the child declares this amount as "income from another person" or "child and family allowance." Similarly, it is possible that a parent not living with the child has declared to receive child benefits. Third, the administrative procedure for determining whether a family is eligible for allowances, and if it is, for calculating the amount the family is entitled to, sometimes takes several months. This is the reason why benefits are sometimes received retrospectively in which case it is not clear whether individuals report benefits in the year when the allowances are obtained, or the year when the application has been filed.

Finally, some individuals forget to declare their family allowance either in reality, or just in the survey. Some parents do not take the time to submit an application to request their benefits. Even when they do apply and receive allowances, this additional source of income is collected with the salary, and therefore, some parents are not aware of its exact amount. It is also likely that the parent who receives the allowance is not taking part in the survey and the participating parent

fails to report all child benefits granted to the household. In such cases, some allowances are not detected accurately in the dataset.

The latter configuration would lead to a reported amount which is lower than the benefits actually received while all previous ones explain cases where family and child allowances indicated by the household exceed the actual allowances.

In addition, there are two reasons for why a household may not receive benefits: first, a parent has not applied for allowances, or second, there is no child in the eligible age range in the family. Due to the latter possibility, we use only families in which there is at least one child under the age of 16 for the majority of the empirical work.

The outcome variables used in the analysis are the likelihood of having another or a first child, the likelihood of being employed, and labor supply, measured by the number of hours worked by the mother and the father. The explanatory variable of interest is the amount of child benefits as reported by respondents. We also use a set of controls presented in Appendix B2. They include age, education, hours worked per week, net household income, number of children, age of the youngest child, marital status, health status, number of household members, presence of a child in worse than the average health, and incidence of problems with children.

All household members different from parents and children are irrelevant to the analysis, and thus have been excluded. We use information about the relationship of each family member to the reference person to construct indicators for children, mothers and fathers. This allows us to identify the role of each member of the household (parent, child, or other) which is not directly

available in the Swiss Household Panel.[16] After excluding the irrelevant household members, our dataset contains a total of 143,581 individuals belonging to 70,994 families.

4.2. SUMMARY STATISTICS

Summary statistics of the variables presented in section 4.1 are reported in Table 1. They indicate that the average annual amount of child benefits a family reports is 634.63 Swiss francs while the average allowance families are eligible for is 1723.57 Swiss francs. The legitimate amount is larger than the reported amount. However, the maximum allowance an individual has reported exceeds the maximum federal amount one can be eligible for based on cantonal legislation. This is indicative of the presence of supplementary child benefits provided by some employers in Switzerland or over-reporting in some instances, but also, on average, of under-perception or under-estimation of the amount received.

The average number of children a family has is 1.271 with a standard deviation of 1.333, but this number drops below 1 if we exclude children above the age of 16. Children under 16 are on average 8.431 years old, and the mean age of the youngest child in a household is about the same. The majority of the respondents (97.54%) enjoy average or better health, and about 7% of them claim that they have a problem with their children.

More than 60% of all respondents (excluding children) are currently married, and in 44.27% of the households, the parents of the child are married and present in the household.

[16] Given the information in the dataset, there were three households for which identification of the role of each family member was not possible, which is the reason why we dropped these three households from the original dataset. There was a reference person, spouse, 2 (or 3 in one case) children, and 1 grandchild in these three households. The grandchild must be the child of one of the children but we do not have information about which one of the 3 children is the parent so we cannot even say whether that child lives with his/her mother or father, as well as the characteristics (education, etc.) of the parent the child lives with. That is why, we drop these three households.

The net household income has a mean of 105041.3 Swiss francs with a standard deviation of 92016.04, indicating a large variability and income inequality between families in Switzerland.

5. Results

This section presents the findings of the study. In Subsection 5.1, we elaborate on the effect of child allowances on fertility and labor market outcomes. Subsection 5.2 summarizes some robustness checks.

5.1. Effects of Child Benefits On Fertility and Employment

Table 2 summarizes the OLS and the 2SLS estimates of the effect of child support on the likelihood that a family has another child, as well as the first-stage regression results. Regardless of whether we distinguish between individuals in the upper or lower tail of the household income distribution, the higher the family allowances, the higher the likelihood of having another child. However, OLS significantly and consistently underestimates the effect of interest.

First, without distinguishing between categories, the results in Table 2 show that a 1% increase in allowances increases the likelihood of having another child by 0.00055%. When using one instrument, this probability becomes about 18 times larger. Specifically, it rises to 0.01%, or an increase in benefits by 10% leads to 0.1% higher probability of having another child. Using three instruments rather than one reduces this effect by a negligible amount.

Second, as indicated in Table 2, an increase in the benefits has a larger impact on low-income families. Precisely, using one instrument, families are 0.01167% and 0.01147% more likely to have another child as benefits rise by 10%, correspondingly when their household income is below and above the average earned by families in the sample in the corresponding

year. Estimating our models using data only on families with children under the age of 16 enhances these effects to 0.01250% and 0.01218%, respectively.

These results are consistent and unbiased, given that the instruments are strong enough. The latter is confirmed by the results from the first-stage regressions. They show that the actual amount of benefits received by families and the eligibility federal amount used as an instrument are strongly correlated. Specifically, a 1% increase in the amount a person is eligible for is associated with an increase in the reported benefits by 0.17% for all families, by 0.16% for below-average income families, and by 0.13% for families in the upper tail of the household income distribution. In addition, the F-statistic on the amount of allowances individuals are entitled to varies between 13.92 and 67.76 in all specifications. These F-statistics are far beyond the conventional threshold of 10, implying a strong enough instrument.

In Tables 3 to 6, we explore the effect of child benefits on the employment decisions of the parents. The effect of the amount of allowances on the extensive margin of labor supply is reported in Table 3, and the remaining tables focus on the intensive margin. We present the results from an analysis of mothers and fathers separately, as well as of all parents regardless of the gender. Similarly to the fertility analysis, we distinguish between families with household income below and above the average. However, irrespective of the specification, family allowances have no significant effect on either the choice whether to work or not, or the number of hours either of the parents chooses to work given that (s)he is employed.

5.2. Robustness Checks

First, in order to explore the possibility that the results might be imprecise in case of a violation of the assumption that the marginal benefits for any child of a given individual are constant, we perform all major estimations on data excluding individuals residing in Vaud (the

canton in which the benefits are significantly higher for a third and higher-order children), separately excluding individuals from both Vaud and Valais (the two cantons in which the increase in benefits for children of higher order is highest in percentage terms), and also, dropping all 8 cantons where there is some variation in benefits depending on child order even if the difference is negligible. The results are presented in Table 7. Comparing these estimates to the ones obtained from the major estimation, we do not observe a significant difference in either the significance, or the magnitude of the effects of interest. Therefore the potential concern that the primary estimates may be spurious due to a variation in the marginal allowances across children of a single individual is not problematic in our analysis.

Second, we estimate all major models in the analysis including cantonal effects and time trends, in addition to individual-specific effects. As suggested by the results reported in Table 8, taking into account time trends and canton-specific fixed effects does not lead to any significant changes in either the magnitude, or the significance of the effects of interest. In addition, it is possible that the effect of the allowances on the outcomes is driven by the largest cantons because they typically provide higher benefits and employers running businesses there are generally more willing to distribute additional benefits to remain competitive and to improve their reputation among potential employees. Therefore, as an additional robustness check, we exclude each one of the three biggest cantons, one at a time. The estimates reported in Table 9 confirm that the results presented in the previous section are robust to the exclusion of the largest (Grisons/ Graubünden), the second largest (Bern), and the third largest (Valais) canton in Switzerland.

Finally, we distinguish between the effect of allowances on the likelihood of giving a first and subsequent births. The number of observations is heavily reduced when we take into account only first births, so due to power issues, these results are inconclusive. Contrary, the results for

subsequent births, presented in Table 10, confirm the significant, positive impact of child benefits on the likelihood of having another child. This result is consistent with the findings of the main estimation.

6. Effect of a Universal Floor on Benefits

Besides studying the effect of child allowances on fertility and labor market outcomes presented earlier, we also distinctly investigate a related research question. Specifically, we examine the causal effect of the imposition of a statutory floor on child benefits each canton is obliged to pay families with children, on the same outcomes. Subsection 6.1 presents some background information. Subsection 6.2 describes the method we use to estimate the effect of interest, the identification assumption and the tests performed to check its validity. In Subsection 6.3, we elaborate on the results and the validity of the identifying assumption.

6.1. Background Information

The Swiss government publicly announced the Swiss Family Allowance Law (Familienzulagengesetz) in 2006. As a result, all 26 cantons became obliged to grant eligible applicants a minimum of 200 Swiss francs (appr. $181) of child benefits per month per child under 16. Although the policy officially came into effect in 2009, the majority of cantons did not alter the benefits at the time of implementation of the reform but instead, initiated gradual increases starting immediately after the floor has been announced and progressing between 2006 and 2009. Because the increase in benefits had been taking place in most cantons since 2006 and because the upcoming floor was publicly declared, families have been experiencing the higher allowances and have been taking the new amount into account in family decision-making since

the announcement rather than the introduction of the policy. We use this fact in the choice of a post-reform period in the following section.

6.2. Model Specification

We employ a difference-in-differences design to analyze the causal effect of the announcement of a universal floor on child benefits in Switzerland.

Defining the treatment group relies on the fact that each canton offers a different amount of family allowances, and increases them in different years, while the floor was imposed on a country level. As a result, the floor was binding only for cantons paying benefits lower than the cutoff amount of 200 Swiss francs. Therefore affected families living in cantons which used to offer benefits below the universal threshold and were thus influenced by the reform constitute the treatment group in our difference-in-differences analysis.

We further define the post-intervention period as the years strictly after 2006. The reason is that cantons started to increase the benefits after the announcement of the floor in 2006, and the new amount of allowances families were eligible for had been taken into account in household choices since 2006. However, given that child planning is a process, we allow for a one-year response time to the incentives triggered by the reform.

Given the fore-mentioned choice of a treatment group and a post-reform period, and under the assumptions that families in affected and unaffected cantons in Switzerland would have evolved similarly in the absence of the reform (which we later verify through formal tests), the causal effect of the floor can be identified through the following difference-in-differences model:

$$Outcome_{it} = \delta_0 + \delta_1 DAfter2006_{it} + \delta_2 Treatment_{it} + \delta_3 DAfter2006 * Treatment_{it} + \boldsymbol{\vartheta X}_{it} + \alpha_i + \epsilon_{it} \qquad (6)$$

In this equation, $DAfter2006$ is an indicator for the post-intervention period. $Treatment$ is a dummy variable for offering benefits below the universal threshold. $DAfter2006 * Treatment$ is an interaction term, and $\boldsymbol{X}$ is a vector of controls identical to the ones taken into account in the analysis of the effect of child benefits on fertility and labor market outcomes discussed previously. The outcomes are also the same as the ones previously chosen and described in previous sections: the likelihood of having another child for parents in the most fertile age under 45, and the extensive and intensive margins of labor supply of the parents.

A key identification assumption of the difference-in-differences method above is that there would have been no difference in the relevant outcomes of the control and the treatment group in the absence of the imposition of a floor on benefits. In order to show that the two groups exhibited parallel trends prior to the enactment of the policy, we perform the following tests. First, we run a falsification test which uses an earlier treatment period. Specifically, we use an alternative date of the announcement of the reform, and thus adopt years 2004 and 2005 as a pre-reform period. A significant interaction term would indicate spurious results from the difference-in-differences estimation while an insignificant effect would be desirable. Second, we perform a formal test proposed by Autor (2003), in which we interact year dummy variables with the treatment indicator.[17] Insignificant interactions with periods prior to the imposition of the floor on benefits would indicate no difference in the underlying trends between the affected and the unaffected cantons.

[17] We omit the interactions for 2004 and 2005 pre-treatment periods in order to avoid a dummy variable trap.

6.3. SUMMARY STATISTICS

Table 11 provides summary statistics separately for the treatment and the control group prior and after the reform. Based on the statistics presented for the covariates, the two groups appear to be very similar both before and after the introduction of the benefits floor. Specifically, 51.7% of the respondents belonging to the treatment group and 50.6% of those in the control group were married before the intervention, while the percent of married parents was 44.9% and 49.4% after the reform, respectively for treated and control individuals. The average age of the parents varies between 34.8 and 35.7 years depending on the category of the group. The mean household income is also quite stable with a mean value of 100,832 Swiss francs for the treated individuals and 111,719 Swiss francs for the controls prior to the intervention, and slightly higher for both groups after 2006. Individuals in both groups have an average of approximately 14 years of education both pre and post-reform, and more than 98% of them enjoy above-average health.

6.4. RESULTS AND VALIDITY OF THE PARALLEL TRENDS ASSUMPTION

The results from the difference-in-differences estimation presented in Table 12 confirm our previous findings. Specifically, they suggest that compared to families in cantons unaffected by the reform, families in cantons affected by the benefits floor become 4.3% more likely to have another child after the policy has been announced. There is no significant change in the extensive or intensive margins of labor supply between affected and unaffected cantons, before and after the policy change. These results imply a beneficial impact of the introduction of a universal floor on child allowances on the child planning behavior of families in Switzerland.

We also present the results from the tests used to confirm the validity of the parallel trends assumption in Table 13. Since we found that the enactment of the policy has a significant impact only on fertility, we perform tests validating the parallel trends assumption using only the

likelihood of having another child as an outcome variable. First, we run a refutability test using a counterfactual year of the introduction of the reform, particularly 2005. We run the difference-in-differences regression using this alternative post-treatment period. The results from the falsification test are presented in Panel A of Table 13. The insignificant interaction term implies that the parallel trends assumptions is satisfied, and identification of the parameters in the true difference-in-differences model does not yield spurious estimates.

Second, we perform a test proposed by Autor (2003). The results presented in Panel B of Table 13 indicate that the lead is insignificant. This implies that the difference-in-differences is not significantly different between the cantons affected by the floor and the ones which have not been affected by the reform, i.e. the two groups of cantons moved on the same trend before the announcement of the benefits floor and there would have been no difference in the fertility trends between the two groups in its absence. Autor's test also shows that the lags are positive and the positive significant effect of the policy change on fertility is primarily driven by families who had a child in 2011 or 2014.

Additionally, we provide a scatter plot of the average likelihood of having a child for individuals residing in cantons affected and unaffected by the floor on family allowances in each year of the study. Figure 1 suggests that prior to the announcement of the reform in 2006, individuals living in cantons eventually not affected by the change were more likely to have a child. After the floor was imposed the two groups became much more equal in their child-rearing decisions, and the average likelihood of having a child in cantons affected by the reform even exceeded that in unaffected cantons in 2008, 2011 and 2014. Therefore the net change in the mean probability of having a child after relative to before the reform is higher in affected than in unaffected cantons. This implies a successful policy change, in that it achieved higher equity in

the child rearing opportunities driven by an equalization of the granted child benefits across cantons.

7. DISCUSSIONS

7.1. POTENTIAL HARVESTING EFFECT

The results from the investigation of both the effect of family allowances and the introduction of a universal floor on child benefits on the likelihood of having further children presented previously suggest that higher child support increases the likelihood of having another child. This positive effect can be explained in two ways.

First, since higher allowances reduce the costs of bearing and raising a child, higher benefits might provide an incentive to families who have not planned a child or higher-order children to have a first or a further child, respectively. An effect through this channel has a direct beneficial impact on fertility.

Second, an increase in the likelihood of having a further child might be indicative of a change in the preference of families towards a reduction of the spacing between births. If the positive effect is driven mainly by this channel, then an increase in child allowances is likely to induce more births in earlier time periods rather than lead to an overall increase in fertility. To test this hypothesis, we examine whether there is a significant difference in the average spacing between subsequent children by child order in the cantons which were affected by the imposition of the floor on child benefits and those which were not influenced by the reform[18]. The results are

[18] In the calculation of the average spacing between children born in each year, we had to exclude third- and higher-order children born prior to 2004 who have a younger sibling. The reason is that we cannot determine the year those children were born given only the age of the youngest and the oldest of all siblings, and thus cannot determine the age gap between such children and their siblings.

illustrated in Figure 2. There is no apparent difference in the net change in the average gap between children in the two groups. The age gap between first and second children of a given individual living in a treated canton seems relatively constant over time with a minor decline after 2013. This decline is also observed in cantons unaffected by the reform. The average spacing between second and third children is higher in the treatment group both before and after 2006, and although it is slightly lower after 2006 as compared to the pre-reform period, the decline is also present in unaffected cantons. This implies that the decline in the gap between children is likely to be driven by factors different from the imposition of a floor on child benefits. We do not observe any significant change in the spacing between third and fourth children in the treatment group in the pre-reform relative to the post-reform period. These findings are suggestive of a potential increase in overall births or an earlier timing of the first birth rather than a reduction in spacing as a result of the reform although there is no way to test how the reform affected the timing of first births because one cannot know when a family would have had their first child in the absence of the reform.

Alternatively, we interact indicators for each year after the reform with the treatment dummy variable, and estimate a model similar to the main difference-in-differences but including all of the previously mentioned interactions and year indicators. Positive, significant interaction terms for the majority of the years after the policy implementation would indicate a sustained differential effect on fertility rather than only an immediate impact altering solely the timing of giving birth. The results show that all but one interaction terms are positive and the largest (and highly significant) differential effect is extracted in 2011, followed by the years of 2007, 2009 and 2014, suggestive of the possibility that the reform contributed to an increase in overall fertility in addition to or rather than a shift of the timing of having a child.

To further investigate the harvesting hypothesis, we examine the dynamic relationship between child benefits and the likelihood of having another child. The possibility of such a relationship is introduced by allowing the outcome in year t to be affected by the reported benefits for up to 5 years prior to year t. Although the magnitude of the coefficient is highest one year after the disbursement of the benefits, there is a positive impact of allowances on fertility 2 and 5 years later as well. This positive effect of the lagged values of child benefits indicates both a short and a long-run, sustained beneficial impact of family support on fertility. These results suggest that higher child benefits are more likely to improve overall fertility rather than affect only the timing of child birth in the case of Switzerland.

In addition, previous authors (e.g., Lalive and Zweimuller 2009) provide evidence that any factor which induces births earlier is likely to increase fertility in the long run because giving a birth earlier reduces the likelihood of not having a further child due to shocks to health, relationships and economic issues which are more prevalent in the longer run. Therefore, even if higher child benefits reduce the spacing between children, they also improve overall fertility rate either in the short, or in the long run.

As an additional insight, data on benefits in Switzerland suggests that child allowances have been increasing since 2004 in Switzerland, and the tendency is towards further expansion. Such a trend makes it less likely for individuals to induce births earlier unless the policy incentivized them to change their preferences over the number of children they would like to have over their most fertile age range. In addition, having a child at an earlier time period implies that this child will reach fertile age earlier than it would have if he was born later, and thus, would be able to bear and raise children of his own sooner. This suggests that even if benefits do not lead to an

increase in total births of the current generation, they are likely to increase fertility in the long run.

7.2. POLICY IMPLICATIONS

Many countries, including Germany, the UK, Belgium, Greece, Denmark, Italy, Finland, Norway, Australia, Poland, Ireland and Austria grant child subsidies in the form of either a recurrent payment, or a tax reduction of a different magnitude based on the number of children. The purpose of the assistance is to support families and to promote child-rearing. In the context of providing larger child allowances, our study shows that this attempt is likely to encourage families to have a child or higher order children. This result has at least two policy implications.

First, awareness of the effect of child allowances on fertility is important for social assistance planning by policy decision makers and evaluation of various reforms. Although a thorough evaluation of child support policies would require considering not only the benefits which family-friendly programs provide to beneficiaries, but also the costs of the social program which are not a matter of investigation in this article, our study provides a step towards such an assessment. It reveals that financial support to families tends to promote child rearing, a finding which can be used to partially evaluate the impact of reforms implemented or proposed in other countries. For instance, Canada recently increased child benefits to provide Canadians with more opportunities to succeed and to enhance citizens' confidence in the future. Our study suggests that it is possible that this policy will additionally promote child rearing.

Second, various European countries, especially Germany, Italy, Spain and Greece, have recently been experiencing a decline in the birth rate, which tends to be lower than the replacement rate. The average number of live births per woman necessary to keep the population at a constant level is not met. Combined with the increased life expectancy, this trend has

contributed to aging of the European population since 2010. There are not enough workers to support the elderly who retire. The trends are expected to negatively affect growth and the demographics of the continent, and thus challenge governments to investigate potential remedies to reverse the present demographic decline. According to this study, child benefits have a positive impact on fertility which suggests that increasing child support is likely to be a potential mechanism to improve fertility rates and to initiate growth in Europe.

8. CONCLUSION

This article studies the effect of family allowances on child rearing and employment in the context of Switzerland, and uses the amount of benefits families are eligible for as an instrument for the reported assistance. It also examines the effect of the introduction of a universal floor on child benefits in all cantons in Switzerland.

Results from a two-stage least squares estimation show that a 10% increase in the federal family support increases the likelihood of having another child by 0.1%. They further indicate that benefits have a larger impact on the child-rearing inclination of low-income families although all families are incentivized by the allowances. While higher benefits positively affect fertility, they do not have a significant effect on the extensive margin of labor supply and do not seem to reduce the number of hours employed parents choose to work. In addition, difference-in-differences estimation of the impact of the introduction of a minimum amount of benefits cantons are obliged to pay families with children, shows that families in cantons affected by the policy become 4.3% more likely to have another child relative to unaffected families, after relative to before the reform.

These findings provide evidence that financial help intended to reduce the costs of raising a child is likely to improve the willingness of families to have children and thus to reverse the trend towards having fewer babies in Europe. Such a policy or a reform making child rearing more affordable to families can be considered a potential remedy for the declining birth rate, aging population and widening gap between the number of children and elderly individuals.

Although this article evaluates the total beneficial impact of such a reform, it does not examine the costs of its implementation. This is a limitation of the study and a potential area of further investigation. In addition, allowances received are self-reported in the Swiss Household Panel, and therefore errors are likely to occur. We attempt to solve this issue through an instrumental variable approach, but if one could use registry data or combine the SHP survey with registry data, a more precise evaluation of the effect of benefits on the outcomes would be obtained. This is another potential improvement of this article and an area of future research.

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TABLES

Table 2.1. Summary statistics

Panel A: Summary statistics of the quantitative variables

Variable	Mean	Std. dev.	Min	Max
NumKids	1.271	1.333	0	14
AllowancesTotal (for the family, annual)	634.628	1916.222	0	42000
Age (of children under 16)	8.431	4.477	0	15
Age (of parents)	54.153	16.307	14	99
AgeYoungestChildInHh	8.647	5.322	0	17
HrsWorked (of parents)	1.945	8.818	0	112
HrsWorkedOfMomInHh	1.932	8.064	0	112
HrsWorkedOfFatherInHh	2.006	9.825	0	110
YrsEduc (of parents)	13.426	3.005	0	21
YrsEduc (of mothers)	12.894	2.845	0	21
YrsEduc (of fathers)	14.145	3.065	0	21
NetHhldIncome	105041.3	92016.04	0	6185400
NetIncomeFatherInHh	58416.6	62603.13	0	2430000
NetIncomeMotherInHh	33018.68	51076.41	0	5502500
NumHhld	2.422	1.289	1	12
NumChildrenInHh	0.696	1.035	0	8
EligibilityAmntAnnual	1723.566	2725.819	0	24000

Panel B: Summary statistics of the binary (dichotomous) variables

Variable	Percent
AnotherChild	5.48
BothParInHh	32.36
Married (excluding children)	60.76
MarriedParInHh	44.27
AvgOrBetterHealth (excluding children)	97.54
ExistsChInBadHealthInHh	0.15
ProblemsWithKids	7.12

Panel C: Summary statistics of received annual allowances and amount of benefits a family is eligible for based on cantonal legislation, by year

Year	Mean		Std. dev.		Min		Max	
	Allowance	Eligibility Amount	Allowance	Eligibility Amount	Allowance	Eligibility Amount	Allowance	Eligibility Amount
2004	380.703	1090.323	1459.845	2022.325	0	0	19200	15420
2005	353.778	1113.331	1341.629	2084.266	0	0	14400	15720
2006	555.539	1101.932	1716.964	2090.578	0	0	36000	15720
2007	571.452	1059.041	1829.379	2104.364	0	0	37800	16920
2008	608.980	1040.548	1938.475	2107.908	0	0	30000	18120
2009	665.376	1093.185	1931.357	2247.909	0	0	28800	18120
2010	713.266	1058.771	1993.892	2201.587	0	0	17400	15600
2011	770.300	1018.463	2050.149	2130.861	0	0	18000	15600
2012	766.190	977.033	2103.593	2119.265	0	0	19200	15600
2013	404.500	1049.383	1567.354	2213.758	0	0	24000	21600
2014	797.802	1039.803	2091.055	2212.679	0	0	18000	21600
2015	845.864	1005.007	2250.553	2201.065	0	0	42000	22800
2016	783.440	968.697	2184.891	2160.53	0	0	40000	24000
Overall	634.628	1044.055	1916.222	2154.533	0	0	42000	24000

Note: Source: Data are obtained from the Swiss Household Panel longitudinal dataset, the Federal Social Insurance Office (FSIO) in Bern, Switzerland for child benefits by canton by year, and the Cantonal Compensation Offices in the 26 cantons in Switzerland.

Table 2.2. Effect of family allowances on the likelihood of another child: OLS FE and IV results

Panel A: Reduced-form (first-stage) regression results with FE

Dependent variable: lnAllowancesTotal

	(1) All income levels		(2) Below average income households		(3) Above average income households	
	1 IV	**3 IV**	**1 IV**	**3 IV**	**1 IV**	**3 IV**
lnEligibility AmntAnnual	0.173***	0.171***	0.160***	0.161***	0.134***	0.132***
	(0.030)	(0.030)	(0.049)	(0.049)	(0.044)	(0.044)
- all	0.168***	0.165***	0.151***	0.152***	0.129***	0.126***
- only families with a child under 16	(0.031)	(0.031)	(0.050)	(0.050)	(0.044)	(0.044)
DBn2006and2009	-	0.030	-	-0.005	-	0.072
		(0.036)		(0.055)		(0.053)
- all	-	0.035	-	0.0002	-	0.074
- only families with a child under 16		(0.036)		(0.055)		(0.053)
D2009andAfter	-	0.059	-	-0.023	-	0.113*
- all		(0.046)		(0.071)		(0.067)
- only families with a child under 16	-	0.068	-	-0.016	-	0.121*
		(0.046)		(0.071)		(0.067)
Individual-specific effects		Yes	Yes	Yes	Yes	Yes
F-statistic						
- all	40.71	33.46	17.69	14.47	16.68	13.92
- only families with a child under 16	39.65	32.65	17.06	13.95	16.39	13.72
Obs.						
- all	4,497	4,497	2,042	2,042	2,455	2,455
- only families with a child under 16	4,437	4,437	2,020	2,020	2,417	2,417
Within R^2						
- all	0.1130	0.1136	0.1197	0.1198	0.0951	0.0970
- only families with a child under 16	0.1118	0.1125	0.1175	0.1176	0.0947	0.0968
Between R^2						
- all	0.0442	0.0451	0.0451	0.0445	0.0263	0.0275
- only families with a child under 16	0.0436	0.0445	0.0436	0.0431	0.0255	0.0267
Overall R^2						
- all	0.0447	0.0462	0.0385	0.0375	0.0313	0.0328
- only families with a child under 16	0.0439	0.0455	0.0378	0.0370	0.0297	0.0312

Panel B: OLS and second-stage regression IV results

Dependent variable: Probability (another child)

	(1)			(2)			(3)		
	OLS	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**
$\widehat{lnAllowancesTot}$ - all	0.811***	19.342***	19.423***	0.845***	20.703***	19.801***	0.870***	25.983***	24.207***
	(0.105)	(1.281)	(1.343)	(0.157)	(1.967)	(1.854)	(0.160)	(2.677)	(2.703)
	[0.055***]	[1.009***]	[0.962***]	[0.060***]	[1.167***]	[1.156***]	[0.051***]	[1.147***]	[1.010***]
- only families with a child under 16	0.923***	20.851***	20.430***	0.910***	21.944***	21.345***	0.990***	28.474***	24.243***
	(0.111)	(1.390)	(1.465)	(0.163)	(2.072)	(1.988)	(0.165)	(3.043)	(3.092)
	[0.064***]	[0.989***]	[0.150***]	[0.068***]	[1.250***]	[1.244***]	[0.057***]	[1.218***]	[1.030***]
Obs.									
- all	5,463	9,645	9,645	2,407	4,573	4,573	3,056	5,072	5,072
	4,400	9,525	9,525	2,022	4,538	4,538	2,418	4,987	4,987

- only families with a child under 16	

Notes: All regressions include individual-specific effects. Panel A reports the results from the first-stage regressions, while Panel B reports both OLS estimates, as well as the results from 2-stage least squares estimation. The first column in (1), (2), and (3) of Panel B always reports the results from OLS estimation with FE, without controlling for endogeneity; the second column always reports the results from the second stage of 2-stage least squares estimation using 1 instrument with FE, and the third column of (1), (2), and (3) in Panel B presents the estimates from two-stage least squares estimation using 3 instruments. (1) does not distinguish between low- and high-income families, and between individuals who report higher or lower benefits than the amount they are eligible for. The estimates in (2) have been obtained using data only for low-income (below average) income families, while (3) takes into account only families with household income above the average for the particular year. The following are the controls used in all regressions: lnNetHhldIncome, Married, Age, HrsWorked, YrsEduc, AvgOrBetterHealth, l.AgeYoungestChildInHh, l.NumKids. Robust standard errors are reported in parentheses. Marginal effects are specified in square parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 2.3. Effect of family allowances on employment status (extensive margin) by the parents: OLS FE and IV results

Panel A: Reduced-form (first-stage) regression results with FE

Dependent variable: lnAllowancesTotal

	(1) Both parents		(2) Mothers		(3) Fathers	
	1 IV	**3IV**	**1 IV**	**3 IV**	**1 IV**	**3 IV**
lnEligibility AmntAnnual	0.191***	0.188***	0.126*	0.114	0.206***	0.211***
	(0.039)	(0.039)	(0.074)	(0.074)	(0.043)	(0.044)
DBn2006and2009	-	0.087***	-	0.095	-	0.076**
		(0.033)		(0.060)		(0.037)
D2009andAfter	-	0.099**	-	0.222***	-	0.003
		(0.046)		(0.085)		(0.051)
Individual-specific effects	Yes	Yes	Yes	Yes	Yes	Yes
F-statistic	49.37	42.45	13.68	12.17	45.86	39.39
Obs.	4,076	4,076	1,745	1,745	2,331	2,331
Within R^2	0.2038	0.2065	0.1442	0.1508	0.2929	0.2963
Between R^2	0.2638	0.2726	0.1810	0.1872	0.3872	0.3786
Overall R^2	0.2569	0.2654	0.1606	0.1660	0.3868	0.3800

Panel B: OLS and second-stage regression IV results

Dependent variable: Prob (being employed)

	(1)			(2)			(3)		
	OLS	**1 IV**	**3IV**	**OLS**	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**
$\widehat{lnAllowancesTotal}$	0.249***	-0.755	0.443	0.169	-0.874	0.917***	0.216	-0.544	-0.124
	(0.093)	(0.588)	(0.435)	(0.109)	(0.960)	(0.353)	(0.192)	(1.510)	(1.436)
	[0.009***]	[-0.065]	[0.038]	[0.009]	[-0.103]	[0.109***]	[0.004]	[-0.019]	[-0.004]
Individual-specific effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	4,579	8,935	8,935	2,013	5,849	5,849	2,566	3,086	3,086

Notes: All regressions include individual-specific effects. Panel A reports the results from the first-stage regressions, while Panel B reports both OLS estimates, as well as the results from 2-stage least squares estimation. The first column in (1), (2) and (3) of Panel B always reports the results from OLS estimation with FE, without controlling for endogeneity; the second column always reports the results from the second stage of 2-stage least squares estimation using 1 instrument with FE, and the third column of (1), (2) and (3) in Panel B presents the estimates from two-stage least squares estimation using 3 instruments. (1) does not distinguish between parents' gender. The estimates in (2) have been obtained using data only for mothers, while (3) takes into account only fathers. The following are the controls used in all regressions: lnNetHhldIncome, BothParInHh, Married, Age, ProblemsWithKids, AvgOrBetterHealth, NumHhld, AgeYoungestChildInHh, YrsEduc, NumKids, ExistsChInBadHealthInHh. Robust standard errors are reported in parentheses. Marginal effects are specified in square parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 2.4. Effect of family allowances on hours worked by the mother: OLS FE and IV results

Panel A: Reduced-form (first-stage) regression results with FE

Dependent variable: lnAllowancesTotal

	(1) All income levels		(2) Below average income households		(3) Above average income households	
	1 IV	**3 IV**	**1 IV**	**3 IV**	**1 IV**	**3 IV**
lnEligibilityAmnt Annual	0.122*	0.107	-0.128	-0.135	0.089	0.090
	(0.075)	(0.075)	(0.110)	(0.111)	(0.130)	(0.129)
DBn2006and2009	-	0.068	-	0.036	-	0.090
		(0.060)		(0.079)		(0.100)
D2009andAfter	-	0.243***	-	0.061	-	0.379***
		(0.086)		(0.114)		(0.144)
Individual-specific effects	Yes	Yes	Yes	Yes	Yes	Yes
F-statistic	14.66	13.15	8.62	7.28	4.30	4.45
Obs.	1,690	1,690	865	865	825	825
Within R^2	0.1573	0.1655	0.1975	0.1982	0.1191	0.1424
Between R^2	0.1628	0.1768	0.0905	0.0908	0.1335	0.2055
Overall R^2	0.1466	0.1586	0.0843	0.0851	0.1384	0.2077

Panel B: OLS and second-stage regression IV results

Dependent variable: Hours worked by the mother

	(1)			(2)			(3)		
	OLS	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**
$\widehat{lnAllowancesTotal}$	-0.293	-1.738	-2.606	-0.652	7.522	5.673	0.360	5.455	-5.791**
	(0.548)	(5.091)	(2.497)	(0.859)	(8.127)	(7.231)	(0.877)	(10.367)	(2.359)
Individual-specific effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	1,960	4,713	4,713	980	2,257	2,257	980	2,456	2,456
Within R^2	0.0127	0.0199	0.0203	0.0639	0.0303	0.0301	0.0226	0.0170	0.0224
Between R^2	0.0003	0.0001	0.0002	0.0077	0.0004	0.0002	0.0090	0.0001	0.0001
Overall R^2	0.0010	0.0015	0.0016	0.0170	0.0039	0.0040	0.0001	0.0000	0.0002

Notes: All regressions include individual-specific effects. Panel A reports the results from the first-stage regressions, while Panel B reports both OLS estimates, as well as the results from 2-stage least squares estimation. The first column in (1), (2), and (3) of Panel B always reports the results from OLS estimation with FE, without controlling for endogeneity; the second column always reports the results from the second stage of 2-stage least squares estimation using 1 instrument with FE, and the third column of (1), (2), and (3) in Panel B presents the estimates from two-stage least squares estimation using 3 instruments. (1) does not distinguish between low- and high-income families, and between individuals who report higher or lower benefits than the amount they are eligible for. The estimates in (2) have been obtained using data only for low-income (below average) income families, while (3) takes into account only families with household income above the average for the particular year. The following are the controls used in all regressions: lnNetHhldIncome, BothParInHh, Married, Age, ProblemsWithKids, AvgOrBetterHealth, NetIncomeFatherInHh, NumHhld, AgeYoungestChildInHh, YrsEduc, NumKids, ExistsChInBadHealthInHh. Robust standard errors are reported in parentheses. Marginal effects are specified in square parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 2.5. Effect of family allowances on hours worked by the father: OLS FE and IV results

Panel A: Reduced-form (first-stage) regression results with FE

Dependent variable: lnAllowancesTotal

	(1) All income levels		(2) Below average income households		(3) Above average income households	
	1 IV	**3 IV**	**1 IV**	**3 IV**	**1 IV**	**3 IV**
lnEligibilityAmnt Annual	0.205*** (0.043)	0.209*** (0.044)	0.319*** (0.085)	0.325*** (0.087)	0.108* (0.058)	0.109* (0.059)
DBn2006and2009	-	0.065* (0.037)	-	0.054 (0.065)	-	0.104* (0.054)
D2009andAfter	-	-0.004 (0.051)	-	0.007 (0.089)	-	0.017 (0.070)
Individual-specific effects	Yes	Yes	Yes	Yes	Yes	Yes
F-statistic	43.53	37.29	17.72	14.81	29.22	16.71
Obs.	2,306	2,306	891	891	1,415	1,415
Within R^2	0.2845	0.2874	0.3256	0.3274	0.2353	0.2407
Between R^2	0.3979	0.3877	0.3947	0.3926	0.2927	0.2864
Overall R^2	0.3930	0.3846	0.4042	0.4036	0.2834	0.2884

Panel B: OLS and second-stage regression IV results

Dependent variable: Hours worked by the father

	(1)			(2)			(3)		
	OLS	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**
$\widehat{lnAllowancesTotal}$	1.029 (0.808)	3.579 (5.765)	0.446 (5.242)	2.406 (1.712)	1.921 (8.483)	2.181 (8.170)	0.152 (0.981)	22.383 (13.381)	-5.781 (8.595)
Individual-specific effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	2,549	2,943	2,943	961	1,107	1,107	1,588	1,836	1,836
Within R^2	0.0114	0.0056	0.0054	0.0567	0.0325	0.0326	0.0087	0.0080	0.0055
Between R^2	0.0000	0.0000	0.0000	0.0017	0.0028	0.0028	0.0000	0.0000	0.0003
Overall R^2	0.0003	0.0004	0.0004	0.0001	0.0011	0.0011	0.0009	0.0002	0.0000

Notes: All regressions include individual-specific effects. Panel A reports the results from the first-stage regressions, while Panel B reports both OLS estimates, as well as the results from 2-stage least squares estimation. The first column in (1), (2), and (3) of Panel B always reports the results from OLS estimation with FE, without controlling for endogeneity; the second column always reports the results from the second stage of 2-stage least squares estimation using 1 instrument with FE, and the third column of (1), (2), and (3) in Panel B presents the estimates from two-stage least squares estimation using 3 instruments. (1) does not distinguish between low- and high-income families, and between individuals who report higher or lower benefits than the amount they are eligible for. The estimates in (2) have been obtained using data only for low-income (below average) income families, while (3) takes into account only families with household income above the average for the particular year. The following are the controls used in all regressions: lnNetHhldIncome, BothParInHh, Married, Age, ProblemsWithKids, AvgOrBetterHealth, NetIncomeMotherInHh, NumHhld, AgeYoungestChildInHh, YrsEduc, NumKids, ExistsChInBadHealthInHh. Robust standard errors are reported in parentheses. Marginal effects are specified in square parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 2.6. Effect of family allowances on hours worked by the parents: OLS FE and IV results

Panel A: Reduced-form (first-stage) regression results with FE

Dependent variable: lnAllowancesTotal

	(1) All income levels		(2) Below average income households		(3) Above average income households	
	1 IV	**3 IV**	**1 IV**	**3 IV**	**1 IV**	**3 IV**
lnEligibilityAmntAnnual	0.187***	0.182***	0.112	0.112	0.115**	0.111**
	(0.039)	(0.039)	(0.069)	(0.070)	(0.056)	(0.056)
DBn2006and2009	-	0.069**	-	0.035	-	0.073
		(0.033)		(0.052)		(0.050)
D2009andAfter	-	0.101**	-	0.023	-	0.127*
		(0.047)		(0.072)		(0.067)
Individual-specific effects	Yes	Yes	Yes	Yes	Yes	Yes
F-statistic	49.23	42.16	19.44	16.46	18.92	16.32
Obs.	3,996	3,996	1,756	1,756	2,240	2,240
Within R^2	0.2067	0.2088	0.2192	0.2197	0.1657	0.1686
Between R^2	0.2615	0.2720	0.1843	0.1835	0.2259	0.2268
Overall R^2	0.2542	0.2641	0.2000	0.1995	0.2205	0.2314

Panel B: OLS and second-stage regression IV results

Dependent variable: Hours worked

	(1)			(2)			(3)		
	OLS	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**
$\widehat{lnAllowancesTotal}$	0.283	1.449	1.265	0.591	0.170	0.393	0.256	11.589*	4.387
	(0.472)	(3.083)	(2.775)	(0.850)	(9.386)	(8.643)	(0.649)	(6.959)	(5.102)
Individual-specific effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	4,509	7,656	7,656	1,941	3,364	3,364	2,568	4,292	4,292
Within R^2	0.0058	0.0076	0.0076	0.0290	0.0160	0.0160	0.0082	0.0073	0.0064
Between R^2	0.0010	0.0010	0.0009	0.0001	0.0000	0.0000	0.0003	0.0001	0.0002
Overall R^2	0.0008	0.0022	0.0022	0.0003	0.0000	0.0000	0.0003	0.0007	0.0009

Notes: All regressions include individual-specific effects. Panel A reports the results from the first-stage regressions, while Panel B reports both OLS estimates, as well as the results from 2-stage least squares estimation. The first column in (1), (2), and (3) of Panel B always reports the results from OLS estimation with FE, without controlling for endogeneity; the second column always reports the results from the second stage of 2-stage least squares estimation using 1 instrument with FE, and the third column of (1), (2), and (3) in Panel B presents the estimates from two-stage least squares estimation using 3 instruments. (1) does not distinguish between low- and high-income families, and between individuals who report higher or lower benefits than the amount they are eligible for. The estimates in (2) have been obtained using data only for low-income (below average) income families, while (3) takes into account only families with household income above the average for the particular year. The following are the controls used in all regressions: lnNetHhldIncome, BothParInHh, Married, Age, ProblemsWithKids, AvgOrBetterHealth, NumHhld, AgeYoungestChildInHh, YrsEduc, NumKids, ExistsChInBadHealthInHh. Robust standard errors are reported in parentheses. Marginal effects are specified in square parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 2.7. Robustness tests: Excluding cantons in which benefits vary with birth order

Panel A: OLS and second-stage regression IV results

Dependent variable: Probability (another child)

	(1)		(2)		(3)		(4)	
	All cantons		**Vaud excluded**		**Vaud and Valais excluded**		**8 cantons excluded**	
	OLS	**1 IV**	**OLS**	**1 IV**	**OLS**	**1 IV**	**OLS**	**1 IV**
$\widehat{lnAllowances}$	0.811***	19.342***	0.804***	18.423***	0.795***	18.322***	0.776***	20.995***
	(0.105)	(1.281)	(0.112)	(1.263)	(0.112)	(1.310)	(0.122)	(1.643)
	[0.055***]	[1.009***]	[0.055***]	[0.975***]	[0.056***]	[0.986***]	[0.057***]	[1.118***]
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	5,463	9,645	4,821	8,618	4,633	8,231	3,690	6,483

Panel B: OLS and second-stage regression IV results

Dependent variable: Hours worked

	(1)		(2)		(3)		(4)	
	OLS	**1 IV**	**OLS**	**1 IV**	**OLS**	**1 IV**	**OLS**	**1 IV**
$\widehat{lnAllowances}$	0.283	1.449	0.215	0.323	0.213	-0.258	0.413	1.034
	(0.472)	(3.083)	(0.502)	(3.209)	(0.513)	(3.308)	(0.574)	(3.297)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	4,509	7,656	4,010	6,843	3,842	6,558	3,051	5,207

Notes: All regressions are estimated using either OLS FE or 2SLS, and include individual-specific effects. Panel A reports the results from regressions in which the dependent variable is the probability of having another child, while the dependent variable in Panel B is the hours worked by either of the parents. The basic specification in (1) does not exclude individuals from any canton. The regressions in (2) exclude residents of Vaud (the canton in which benefits increase the most for third and higher-order children); those in (3) exclude residents of Vaud and Valais (the two cantons with highest increase in benefits for third and higher-order children), and (4) excludes individuals from all 8 cantons where benefits vary with child order. Robust standard errors are reported in parentheses. Marginal effects are presented in square parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 2.8. Robustness tests: including year and canton FE

Panel A: OLS and second-stage regression IV results

Dependent variable: Probability (another child)

	(1)			(2)			(3)		
	OLS	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**
$\widehat{lnAllowances\ Total}$	0.811***	19.342***	19.423***	0.642***	19.799***	19.799***	0.828***	20.943***	21.065***
	(0.105)	(1.281)	(1.343)	(0.102)	(1.634)	(1.634)	(0.106)	(1.381)	(1.459)
	[0.055***]	[1.009***]	[0.962***]	[0.042***]	[0.930***]	[0.930***]	[0.055***]	[1.092***]	[1.030***]
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	Yes	Yes	Yes	No	No	No
Canton FE	No	No	No	No	No	No	Yes	Yes	Yes
Obs.	5,463	9,645	9,645	5,463	9,645	9,645	5,441	9,645	9,645

Panel B: OLS and second-stage regression IV results

Dependent variable: Hours worked

	(1)			(2)			(3)		
	OLS	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**
$\widehat{lnAllowances\ Total}$	0.283	1.449	1.265	0.177	1.487	1.487	0.267	1.507	1.279
	(0.472)	(3.083)	(2.775)	(0.474)	(3.253)	(3.253)	(0.482)	(3.231)	(2.950)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	Yes	Yes	Yes	No	No	No
Canton FE	No	No	No	No	No	No	Yes	Yes	Yes
Obs.	4,509	7,656	7,656	4,509	7,656	7,656	4,509	7,656	7,656

Notes: All regressions are estimated using either OLS FE or 2SLS, and include individual-specific effects. Panel A reports the results from regressions in which the dependent variable is the probability of having another child, while the dependent variable in Panel B is the hours worked by either of the parents. The basic specification in (1) includes only individual FE. The regressions in (2) include both individual effects and time trends. The regressions in (3) include individual and cantonal FE, while those in (4) take into account individual and interacted cantonal and year FE. Robust standard errors are reported in parentheses. Marginal effects are presented in square parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 2.9. Robustness tests: Excluding largest cantons one-by-one

Panel A: OLS and second-stage regression IV results

Dependent variable: Probability (another child)

	(1) All cantons			(2) Grisons/ Graubünden excluded		
	OLS	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**
$\widehat{lnAllowances\ Total}$	0.811***	19.342***	19.423***	0.790***	18.938***	19.189***
	(0.105)	(1.281)	(1.343)	(0.105)	(1.274)	(1.338)
	[0.055***]	[1.009***]	[0.962***]	[0.054***]	[0.995***]	[0.954]
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	5,463	9,645	9,645	5,365	9,437	9,437

Panel B: OLS and second-stage regression IV results

Dependent variable: Hours worked

	(1)			(2)		
	OLS	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**
$\widehat{lnAllowances\ Total}$	0.283	1.449	1.265	0.322	1.351	1.256
	(0.472)	(3.083)	(2.775)	(0.479)	(3.096)	(2.833)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	4,509	7,656	7,656	4,424	7,492	7,492

Panel A: OLS and second-stage regression IV results – cont.

Dependent variable: Probability (another child)

	(3) Bern excluded			(4) Valais excluded		
	OLS	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**
$\widehat{lnAllowances\ Total}$	0.873***	18.742***	18.560***	0.813***	19.369***	19.347***
	(0.116)	(1.330)	(1.310)	(0.106)	(1.310)	(1.379)
	[0.058***]	[0.979***]	[0.982***]	[0.056***]	[1.027***]	[0.971***]
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	4,828	8,530	8,530	5,275	9,258	9,258

Panel B: OLS and second-stage regression IV results – cont.

Dependent variable: Hours worked

	(3)			(4)		
	OLS	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**
$\widehat{lnAllowances\ Total}$	0.086	0.815	1.240	0.283	0.975	0.689
	(0.503)	(3.031)	(2.907)	(0.483)	(3.105)	(2.773)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	3,964	6,708	6,708	4,341	7,371	7,371

Notes: All regressions are estimated using either OLS FE or 2SLS, and include individual-specific effects. Panel A reports the results from regressions in which the dependent variable is the probability of having another child, while the dependent variable in Panel B is the hours worked by either of the parents. The basic specification in (1) does not exclude individuals from any canton. The regressions in (2) exclude residents of Grisons/ Graubünden (the largest canton in Switzerland); those in (3) exclude residents of Bern (the second largest canton), and (4) excludes individuals from Valais (the third largest canton). Robust standard errors are reported in parentheses. Marginal effects are presented in square parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 2.10. Effect of family allowances on the likelihood of another child: Distinguished first and subsequent births

Panel A: Reduced-form (first-stage) regression results with FE

Dependent variable: lnAllowancesTotal

	(1) All		(2) Subsequent births	
	1 IV	**3 IV**	**1 IV**	**3 IV**
lnEligibilityAmntAnnual	0.173***	0.171***	0.177***	0.175***
	(0.030)	(0.030)	(0.032)	(0.032)
DBn2006and2009	-	0.030	-	0.022
		(0.036)		(0.037)
D2009andAfter	-	0.059	-	0.043
		(0.046)		(0.048)
Individual-specific effects		Yes	Yes	Yes
F-statistic	40.71	33.46	38.24	31.35
Obs.	4,497	4,497	4,094	4,094
Within R^2	0.1130	0.1136	0.1162	0.1164
Between R^2	0.0442	0.0451	0.0481	0.0491
Overall R^2	0.0447	0.0462	0.0511	0.0525

Panel B: OLS and second-stage regression IV results

Dependent variable: Probability (another child)

	(1)			(2)		
	OLS	**1 IV**	**3 IV**	**OLS**	**1 IV**	**3 IV**
$\widehat{lnAllowancesTotal}$	0.811***	19.342***	19.423***	0.185**	9.874***	9.597***
	(0.105)	(1.281)	(1.343)	(0.080)	(1.359)	(1.423)
	[0.055***]	[1.009***]	[0.962***]	[0.011**]	[0.496***]	[0.479***]
Obs.	5,463	9,645	9,645	4,982	8,846	8,846

Notes: All regressions include individual-specific effects. Panel A reports the results from the first-stage regressions, while Panel B reports both OLS estimates, as well as the results from 2-stage least squares estimation. The first column in (1), (2) and (3) of Panel B always reports the results from OLS estimation with FE, without controlling for endogeneity; the second column always reports the results from the second stage of 2-stage least squares estimation using 1 instrument with FE, and the third column of (1), (2) and (3) in Panel B presents the estimates from two-stage least squares estimation using 3 instruments. (1) does not distinguish between first and subsequent births while (2) reports the effect of allowances only on subsequent births. The following are the controls used all regressions: lnNetHhldIncome, Married, Age, HrsWorked, YrsEduc, AvgOrBetterHealth, l.AgeYoungestChildInHh, l.NumKids. Robust standard errors are reported in parentheses. Marginal effects are specified in square parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 2.11. Summary statistics for the treatment and the control group

Variable	Treatment group				Control group			
	Before		After		Before		After	
	Mean	Std. deviation	Mean	Std. deviation	Mean	Std. deviation	Mean	Std. deviation
Married	0.517	0.500	0.449	0.497	0.506	0.500	0.494	0.500
Age	35.151	7.015	34.803	7.220	35.512	6.585	35.704	6.963
NetHhldIncome	100832.4	60328.09	112243.1	68760.48	111719.2	101748.6	115932.4	61928.09
HrsWorked	2.490	9.908	2.866	10.638	2.459	10.244	2.579	10.131
YrsEduc	13.534	2.816	14.159	3.014	13.666	2.997	14.158	3.141
AvgOrBetterHealth	0.985	0.121	0.987	0.111	0.980	0.140	0.986	0.119
AgeYoungestChildInHh	7.064	4.826	6.513	4.807	6.828	4.734	6.613	4.769
NumKids	1.059	1.214	0.813	1.156	0.009	0.109	0.002	0.047

Note: Source: Data are obtained from the Swiss Household Panel longitudinal dataset, the Federal Social Insurance Office (FSIO) in Bern, Switzerland for child benefits by canton by year, and the Cantonal Compensation Offices in the 26 cantons in Switzerland.

Table 2.12. Difference-in-differences results

	(1) Prob (another child)	(2) Prob (being employed)	(3) Hours worked	(4) Hours worked by mother, given employed	(5) Hours worked by father, given employed
DAfter2006	0.207	0.261*	0.989	1.702**	-0.820
	(0.272)	(0.149)	(0.804)	(0.843)	(1.737)
Treatment	-0.268	0.080	1.226	-0.054	3.562
	(0.293)	(0.161)	(1.916)	(2.142)	(3.674)
DAfter2006xTreatment	0.561*	0.070	-0.702	-1.285	0.711
	(0.311)	(0.164)	(0.830)	(0.875)	(1.779)
	[0.043*]	[0.006]			
Individual-specific effects	Yes	Yes	Yes	Yes	Yes
Obs.	7,354	10,254	8,816	5,454	3,362

Notes: All regressions are estimated using OLS/ OLS FE. The variables of interest are Dafter2006, TreatedCanton and the interaction of the latter two variables. A DD approach has been adopted. The dependent variables are the following: Prob (another child) in Column (1); Prob (being employed) in Column (2); Hours worked by the parents, given that they are employed in Column (3); Hours worked by the mother, given employment in Column (4); and Hours worked by the father, given employment in Column (5). The following are the controls: lnNetHhldIncome, Married, Age, HrsWorked, YrsEduc, AvgOrBetterHealth, l.AgeYoungestChildInHh, and l.NumKids (Column (1)); BothParInHh, Married, Age, ProblemsWithKids, AvgOrBetterHealth, NumHhld, AgeYoungestChildInHh, YrsEduc, NumKids, and ExistsChInBadHealthInHh (Columns (2)); BothParInHh, Married, Age, ProblemsWithKids, AvgOrBetterHealth, NumHhld, AgeYoungestChildInHh, YrsEduc, NumKids, and ExistsChInBadHealthInHh (Columns (3), (4) and (5)). Regressions (3), (4) and (5) include individual-specific fixed effects. Robust standard errors are reported in parentheses. Marginal effects are specified in square parentheses in the cases of probit regression models. *** p<0.01, ** p<0.05, * p<0.10.

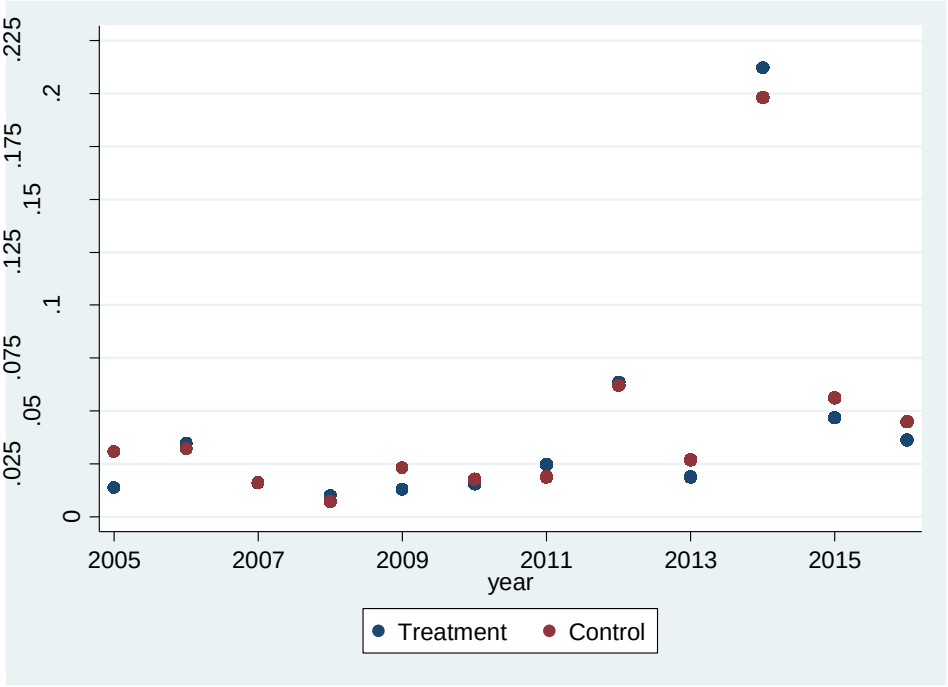
Table 2.13. Verification of the validity of the parallel trends identification assumption of the difference-in-differences

Panel A: Falsification test (Counterfactual pre- and post-reform period)	
	(1)
AfterxTreatment	0.594
	(0.421)
	[0.045]
Obs.	7,354
Panel B: Autor's test	
	(1)
year2006xTreatment	0.253 (0.484)
year2007xTreatment	0.836 (0.701)
year2008xTreatment	0.723 (0.694)
year2009xTreatment	0.787 (0.633)
year2010xTreatment	0.654 (0.548)
year2011xTreatment	1.808*** (0.610)
year2012xTreatment	0.344 (0.447)
year2013xTreatment	0.447 (0.485)
year2014xTreatment	0.712* (0.408)
year2015xTreatment	0.076 (0.430)
year2016xTreatment	0.589 (0.470)
Obs.	7,354

Notes: The table represents the results from tests of the validity of the parallel trends identification assumption of the difference-in-differences. All regressions are estimated using FE. The dependent variable is the Prob (another child), while the controls are the following: lnNetHhldIncome, Married, Age, HrsWorked, YrsEduc, AvgOrBetterHealth, l.AgeYoungestChildInHh, and l.NumKids. Panels A reports the results from a falsification test using an earlier year (2005) of the implementation of the reform. The effect is insignificant, implying that the parallel trends assumption is not violated. Panel B reports the results from formal Autor's test. Insignificant interaction terms of the treatment indicator with the years before the reform indicate parallel trends. Robust standard errors are reported in parentheses. Marginal effects are specified in square parentheses. *** p<0.01, ** p<0.05, * p<0.10.

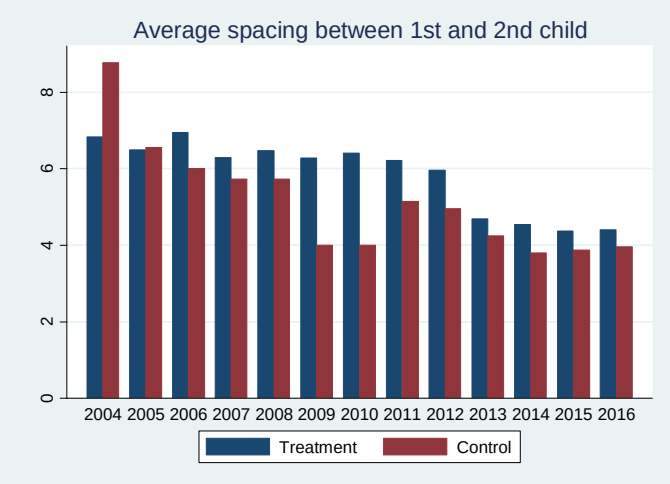
FIGURES

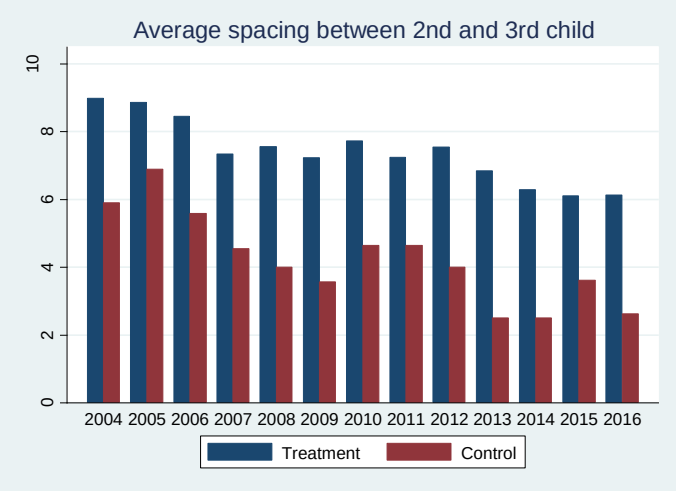
Figure 2.1. Mean likelihood of having a child in each year in cantons affected and unaffected by the floor on family benefits

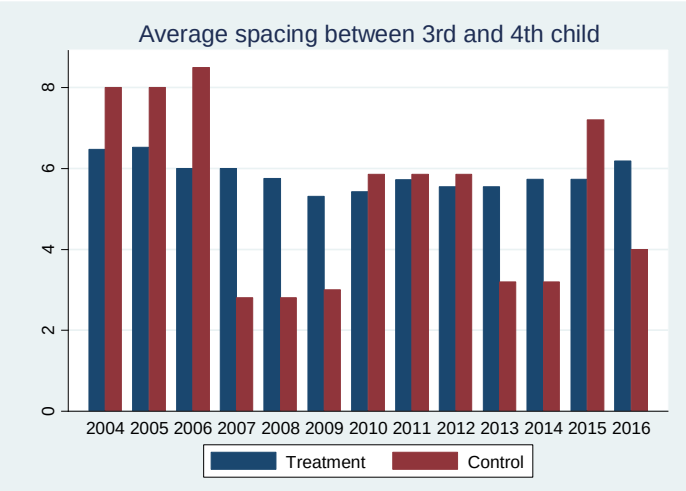


Notes: The graph illustrates the likelihood of having a child in each year on average in cantons affected and unaffected by the imposition of a universal floor on child benefits across all 26 cantons in Switzerland.

Figure 2.2. Average spacing between children







Notes: The graph represents the average spacing (in years) between children separately for cantons affected and cantons unaffected by the imposition of a floor on benefits.

CHAPTER 3:

THE EFFECT OF MATERNITY LEAVE EXPANSIONS ON FERTILITY INTENTIONS: EVIDENCE FROM SWITZERLAND[19]

(Co-authored with Dr. Barbos)

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 one 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 2 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.

[19] We thank helpful comments from Padmaja Ayyagari, Giulia La Mattina, and Joshua Wilde.

1. 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.[20] 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

[20] 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).

MB expansion implemented in Switzerland in 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. 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 fertility intentions of the individuals who have been eligible for these benefits, and thus may have considered them in their fertility decisions, 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.

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 the 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.[21] As a control group, we utilize individuals with no children at the time of the interview. While eligible for the expanded MB in 2010, such individuals have not yet experienced them. The role of this control group in our analysis

[21] 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.

is to capture the counterfactual underlying trend in fertility intentions between 2004 and 2010 that would have manifested 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. We account for this possibility when evaluating the possible channels through which the reform affected fertility planning. To test the validity of this control group, 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 decisions.

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 2 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 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-difference 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.[22] 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.[23] 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 Section 2, we present a summary of the relevant existing literature. Section 3 provides background information regarding the context and content of the policy reform. Section 4 presents the empirical model and the identification strategy. In Section 5, we describe the data used in this study. Section 6 presents the results of the analysis. Section 7

[22] 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).
[23] 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).

discusses the possible channels through which the reform may have affected fertility intentions. Section 8 concludes.

2. LITERATURE REVIEW

Lalive and Zweimuller (2009) investigated the effect of the 1990 extension by one 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 one year paid maternity leave implemented in Soviet Russia starting with 1981. The paper showed that fertility increased by approximately 10% 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 week 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.

3. 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.[24] 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

[24] 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.

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 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.[25] 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 hours 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 6. Employers are also obliged to provide mothers a couch and a 1-hour 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-minute lunch break.

4. Empirical Strategy and Identification

[25] 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).

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. [26] 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 in section 7 the possible channels that can generate

[26] 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.

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:

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

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 1 if the individual belongs to the treatment group, and 0 otherwise. The variable $After$ equals 1 if the individual is interviewed in 2010, i.e. after the MB expansion from 2005, and equals 0 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 $\boldsymbol{\delta_3}$ 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. $\boldsymbol{\delta_3}$ does not capture the full effect of the policy on fertility intentions, though, because individuals who have not experienced the 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 coefficient $\boldsymbol{\delta_3}$ suggests a successful policy reform implementation through at least other channels.

5. DATA AND DESCRIPTIVE STATISTICS

This section describes the source of the data employed in our study, and various descriptive statistics. Subsection 4.1 presents the variables, and subsection 4.2 contains the summary statistics.

5.1. 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 2 (2004) and Round 5 (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,[27] 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

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

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 5 possible ordered answers: "Definitely Not," "Probably Not," "Don't Know," "Definitely Yes," and "Probably Yes". Based on these answers, we construct 3 categorical variables that are employed in the empirical analysis as dependent variables. The first of these variables considers all these 5 answers separately, taking thus 5 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 3 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 2 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 appendix C1. 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.

5.2. 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%.[28] 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 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[29], 42%

[28] 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.

[29] CHF denotes Swiss franc.

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.

6. RESULTS

This section reports the main findings of the different specifications of the model.

6.1. 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 2 and 3 outcomes. It is also positive, but statistically insignificant with a 5-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 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 2 possible outcomes for the dependent variable. Since with only 2 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 3 or 5 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 stronger 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. [30] The interaction coefficient is negative and especially large in magnitude when the dependent variable allows for 5 outcomes.

Table 6 presents results from estimations that restrict the treatment group to individuals with 1 or 2 children, respectively. In the model specification with a treatment group of individuals with 1 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 2 or 3 outcomes, driven again primarily by men. On the other hand, when the treatment group consists of

[30] 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 section 7 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 explanation 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.

individuals with 2 children, the coefficient on the interaction term is positive and statistically significant, or nearly so, under most estimations. These results suggest that individuals with 2 children respond more strongly to the extended maternity benefits, although it is likely that individuals with 1 child respond as well. Not surprisingly, the coefficient on the variable *Treated* is also positive, but statistically significant when the treatment group are individuals with 1 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 2 children explained by the frequent choice of families to have exactly 2 children.

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

7. 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 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.[31] We analyze this latter potential channel formally in appendix C2, and show 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.

8. 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

[31] 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.

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 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. Further research employing additional data may measure the precise effects through each of the potential channels that the reform affects child planning.

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TABLES

Table 3.1. 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

Notes: 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 Group consists of individuals who had a child in the 6 calendar years prior to the year of the interview. The Control Group consists of individuals with no children at the time of the interview.

Table 3.2. 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	After reform	Before reform	After reform	$\Delta Prob_{tr}$	$\Delta Prob_{contr}$	DD
	(1)	(2)	(3)	(4)	(5)	(6)	(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			

Notes: 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).

Table 3.3. Results from the DD estimation

Dependent variable: Fertility intentions

	2 outcomes			3 outcomes			5 outcomes		
	All	**Women**	**Men**	**All**	**Women**	**Men**	**All**	**Women**	**Men**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
After X Treated	0.766**	0.577	0.988*	0.652*	0.294	1.004**	0.170	0.269	0.121
	(0.041)	(0.311)	(0.058)	(0.064)	(0.565)	(0.015)	(0.567)	(0.532)	(0.784)
After	-0.432*	-0.329	-0.421	-0.417*	-0.320	-0.455	-0.265	-0.298	-0.196
	(0.074)	(0.392)	(0.192)	(0.059)	(0.364)	(0.116)	(0.149)	(0.341)	(0.406)
Treated	2.611***	3.321***	2.036***	2.650***	3.141***	2.198***	2.170***	2.816***	1.946***
	(0.000)	(0.000)	(0.006)	(0.000)	(0.000)	(0.004)	(0.000)	(0.000)	(0.007)
Number Children	-2.126***	-2.581***	-1.742***	-2.104***	-2.389***	-1.820***	-1.535***	-2.069***	-1.252***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Gender	0.390**	omitted	omitted	0.326*	omitted	omitted	0.152	omitted	omitted
	(0.036)			(0.059)			(0.299)		
Age Respondent	-.054**	-0.088***	-0.015	-0.055***	-0.089***	-0.017	-0.066***	-0.099***	-0.027
	(0.013)	(0.006)	(0.638)	(0.005)	(0.002)	(0.534)	(0.000)	(0.000)	(0.229)
Years Education	0.090***	0.118***	0.068*	0.082***	0.120***	0.051	0.069***	0.089***	0.049*
	(0.000)	(0.002)	(0.056)	(0.000)	(0.001)	(0.105)	(0.000)	(0.001)	(0.064)
Weekly Work Hours	0.005	0.003***	0.004	0.005	0.002	0.005	0.012**	0.006	0.011
	(0.391)	(0.738)	(0.682)	(0.393)	(0.768)	(0.570)	(0.026)	(0.401)	(0.156)
Income									
<24000	0.622*	0.955*	0.209	0.486*	0.880*	0.078	0.451*	0.824*	0.101
	(0.061)	(0.071)	(0.630)	(0.095)	(0.069)	(0.828)	(0.073)	(0.051)	(0.744)
24000<60000	0.272	0.412	0.118	0.140	0.288	0.016	0.134	0.333	-0.019
	(0.311)	(0.296)	(0.748)	(0.542)	(0.426)	(0.956)	(0.509)	(0.300)	(0.940)
60000<90000	0.147	-0.003	0.214	0.018	0.037	0.015	0.139	0.276	0.104
	(0.637)	(0.995)	(0.616)	(0.949)	(0.930)	(0.968)	(0.572)	(0.471)	(0.739)
Partner	1.795**	0.886	3.181***	1.763**	0.986	3.124***	1.175	-0.066	3.139***
	(0.020)	(0.328)	(0.009)	(0.027)	(0.273)	(0.006)	(0.156)	(0.938)	(0.001)
Age Partner	-0.021	0.003	-0.065*	-0.023	0.000	-0.069**	-0.013	0.025	-0.080***
	(0.351)	(0.890)	(0.077)	(0.326)	(0.970)	(0.049)	(0.604)	(0.298)	(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

Notes: All regressions are estimated using an ordered Logit model. P-values are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10. The variable "After" takes value 1 for individuals interviewed after the reform, and 0 otherwise. The variable "Treated" takes value 1 for individuals in the Treatment Group, and 0 otherwise. Columns (1-3) present regressions with the dependent variable taking 2 possible ordered values "Definitely or Probably Yes" and "Probably or Definitely Not." Columns (4-6) present regressions with the dependent variable taking 3 possible values "Definitely or Probably Yes," "Don't Know," and "Probably or Definitely Not." Columns (7-9) present regressions with the dependent variable taking 5 possible values "Definitely Yes," "Probably Yes," "Don't Know," "Probably Not," and "Definitely Not."

Table 3.4. Average marginal effects with Logit and Probit models

Dependent variable: Probability of an answer "*Definitely Yes*" or "*Probably Yes*" to the question "*Do you plan to have a child in the next 3 years?*" with all observations with neutral answers excluded

	Logit Model			**Probit Model**		
	All	**Women**	**Men**	**All**	**Women**	**Men**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
After	-0.032	-0.021	-0.019	-0.030	-0.019	-0.018
	(0.399)	(0.704)	(0.713)	(0.439)	(0.736)	(0.735)
Treated	0.396***	0.399***	0.379***	0.387***	0.401***	0.360***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number Children	-0.400***	-0.465***	-0.328***	-0.370***	-0.446***	-0.301***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Gender	0.074**	omitted	omitted	0.071	omitted	omitted
	(0.035)			(0.043)		
Age Respondent	-0.010**	-0.015***	-0.002	-0.003	-0.016***	-0.004
	(0.012)	(0.004)	(0.639)	(0.355)	(0.002)	(0.448)
Years Education	0.017***	0.021***	0.012*	0.017***	0.021***	0.013**
	(0.000)	(0.001)	(0.052)	(0.000)	(0.000)	(0.041)
Weekly Work Hours	0.001	0.000	0.000	0.001	0.000	0.000
	(0.390)	(0.738)	(0.682)	(0.345)	(0.809)	(0.627)
Income						
<24000	0.117*	0.167**	.039	0.119*	0.152*	0.051
	(0.056)	(0.050)	(0.632)	(0.051)	(0.070)	(0.547)
24000<60000	0.051	0.073	0.022	0.052	0.065	0.028
	(0.307)	(0.285)	(0.747)	(0.291)	(0.335)	(0.691)
60000<90000	0.027	-0.000	0.040	0.032	-0.004	0.050
	(0.637)	(0.995)	(0.619)	(0.581)	(0.959)	(0.538)
Partner	0.317***	0.159	0.599***	0.311***	0.162	0.531**
	(0.003)	(0.327)	(0.005)	(0.001)	(0.321)	(0.016)
Age Partner	-0.003	0.000	-0.012*	-0.003	0.000	-0.009
	(0.348)	(0.889)	(0.067)	(0.355)	(0.912)	(0.156)
Observations	799	386	413	799	386	413

Notes. 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<0.01, ** p<0.05, * p<0.10. In the model with 2 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.

Table 3.5. 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	0.915	0.854	1.219	1.553	1.162	0.265	0.122	-0.439
	(0.303)	(0.472)	(0.525)	(0.105)	(0.216)	(0.401)	(0.656)	(0.888)	(0.735)
After	-0.867**	-1.138**	-0.619	-0.868**	-0.999*	-0.697	-0.675**	-0.850*	-0.350
	(0.027)	(0.037)	(0.256)	(0.019)	(0.060)	(0.180)	(0.039)	(0.086)	(0.432)
Treated	1.439	1.952	1.588	1.531	2.048	1.256	2.534***	2.646**	3.339*
	(0.214)	(0.197)	(0.444)	(0.155)	(0.123)	(0.508)	(0.005)	(0.019)	(0.096)
Observations	248	118	130	276	134	142	276	134	142
Age Group 31-36									
After X Treated	1.070	0.566	1.788*	0.896	0.547	1.524	0.478	0.711	0.374
	(0.113)	(0.544)	(0.074)	(0.157)	(0.543)	(0.106)	(0.367)	(0.324)	(0.668)
After	-0.403	-0.282	-0.403	-0.272	-0.261	-0.299	-0.312	-0.405	-0.311
	(0.370)	(0.713)	(0.483)	(0.504)	(0.726)	(0.569)	(0.376)	(0.523)	(0.513)
Treated	3.949***	4.121**	4.351**	4.076**	4.223*	4.455***	3.063***	2.973**	4.066*
	(0.000)	*	*	*	**	(0.000)	(0.000)	*	**
		(0.001)	(0.000)	(0.000)	(0.001)			(0.001)	(0.000)
Observations	313	162	151	330	167	163	330	167	163
Age Group 37-42									
After X Treated	-0.301	-0.835	0.215	-0.504	-1.755	0.073	-1.165**	-2.534**	-0.735
	(0.671)	(0.479)	(0.831)	(0.458)	(0.157)	(0.938)	(0.048)	(0.029)	(0.344)
After	0.883*	1.659*	0.140	0.928*	1.764*	0.218	1.071***	2.220**	0.492
	(0.079)	(0.055)	(0.847)	(0.052)	(0.056)	(0.723)	(0.010)	(0.019)	(0.289)
Treated	1.772*	3.618*	1.319	1.749*	2.902*	1.449	1.171	2.924**	1.155
	(0.060)	(0.056)	(0.280)	(0.055)	(0.066)	(0.230)	(0.174)	(0.038)	(0.325)

Table 3.6. Results from the DD estimation with different treatment groups

Dependent variable: Fertility intentions

	2 outcomes			3 outcomes			5 outcomes		
	All	**Women**	**Men**	**All**	**Women**	**Men**	**All**	**Women**	**Men**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
Treatment group: Individuals with 1 child									
After X	0.609	0.196	0.883	0.475	-0.133	0.922	0.044	-0.016	-0.219
Treated	(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	1.077**	1.056	1.025	0.798*	0.530	1.002	0.375	0.763	-0.126
Treated	(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

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

Table 3.7. Placebo test on data from France and Germany

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)**
France									
After X	0.058	-0.807	0.271	-0.029	-0.788	0.255	-0.166	-0.668	0.087
Treated	(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	-0.368	-0.660	-0.031	-0.267	-0.458	0.110	-0.482*	-0.634*	-0.153
Treated	(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

Notes: 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<0.01, ** p<0.05, * p<0.10. See Table 3 for additional estimation details.

APPENDIX A1

Age of children at different stages of GUS, Birth cohort 1		
Sweep/ Wave	**Year**	**Age of child**
1	2005/ 2006	10 ½ months
2	2006/ 2007	2 years
3	2007/ 2008	3 years
4	2008/ 2009	4 years
5	2009/ 2010	5 years
6	2010/ 2011	6 years

Note: Data are obtained from Growing Up in Scotland – Birth cohort 1 longitudinal dataset.

APPENDIX A2

Set of variables used in the analysis and their descriptions	
Dependent variables:	
PSscore	Picture similarity raw score used as a measure of cognitive ability of the study child at the age of 34 months (wave 3) and 58 months (wave 5)
NVscore	Naming vocabulary raw score used as a measure of cognitive ability of the study child at the age of 34 months (wave 3) and 58 months (wave 5)
TDscore	Total difficulties score used as a measure of the social and behavioral development of the study child at the age of 46 months (wave 4), 58 months (wave 5) and 70 months (wave 6)
Explanatory variable of interest (used in all wave*):*	
HrsChdCareGrp	Number of hours of childcare provided by (a) grandparent(s)
Controls to account for observed household composition characteristics (used in all waves):	
TotalNumPplInHhld	Total number of individuals in the household
Ln(Income)	Natural logarithm of total household income
BothParInHhld	An indicator = 1 if both the biological/ adoptive/ foster mother and father reside with the child, and 0, otherwise
Controls to account for observed parents characteristics (used in all waves):	
ParInHhldMarried	An indicator = 1 if the parents in the household are married, and 0, otherwise
HigherEducMom	An indicator = 1 if the mother has higher education or higher, and 0, otherwise
HigherEducDad	An indicator = 1 if the father has higher education or higher, and 0, otherwise
Controls to account for observed characteristics of the study child (used in all waves):	
NumSiblings	Number of siblings the study child has
MaleChd	An indicator = 1 if the study child is a boy, and 0, otherwise
ChildBirthOrder	Birth order of the study child
HealthChdNotGood	An indicator = 1 if the health condition of the study child is fair, bad or very bad, and 0, otherwise
Controls to account for other forms of child care (used in all waves):	
HrsOtherChdCare	Number of hours of external childcare provision (i.e. not provided by grandparents)

Note: Source: Data are obtained from Growing Up in Scotland – Birth cohort 1 longitudinal dataset.

APPENDIX B1

Legal eligibility for family allowances by canton, year, number and age of children

Canton	Canton code	Year(s)	Child allowance eligibility	Age limit
Zürich	ZH	[2004 – 2008]	170/ 195 (above 12 yo)	16
Zürich	ZH	2009 - 2017	200/ 250 (above 12)	16
Bern	BE	2004 - 2008	160/190 (above 12 yo)	16
Bern	BE	2009 - 2017	230	16
Luzern	LU	2004 - 2005	180/ 200 (above 12 yo)	16
Luzern	LU	2006 - 2017	200/ 210 (above 12 yo)	16
Uri	UR	2004 - 2007	190	16
Uri	UR	2008 - 2017	200	16
Schwyz	SZ	2004 - 2014	200	16
Schwyz	SZ	2015 - 2016	210	16
Schwyz	SZ	2017	220	16
Obwalden	OW	2004	170	16
Obwalden	OW	2005 - 2017	200	16
Nidwalden	NW	2004	175	16
Nidwalden	NW	2005 - 2007	200	16
Nidwalden	NW	2008	220	16
Nidwalden	NW	2009 - 2017	240	16
Glarus	GL	2004 - 2007	170	16
Glarus	GL	2008 - 2017	200	16
Zug	ZG	2004 - 2008	250/ 300 (kid 3)	18
Zug	ZG	2009 - 2017	300	18
Fribourg	FR	2004	210/ 230 (kid 3)	15
Fribourg	FR	2005 - 2006	220/ 240 (kid 3)	15
Fribourg	FR	2007 - 2012	230/ 250 (kid 3)	15
Fribourg	FR	2013 - 2017	245/ 265 (kid 3)	16
Solothurn	SO	2004	175	18
Solothurn	SO	2005 - 2007	190	18
Solothurn	SO	2008 - 2017	200	18
Basel-Stadt	BS	2004 - 2006	170	16
Basel-Stadt	BS	2007 - 2017	200	16
Basel-Landschaft	BL	2004 - 2005	170	16
Basel-Landschaft	BL	2006 - 2017	200	16
Schaffhausen	SH	2004 - 2008	180	16

Schaffhausen	SH	2009 - 2017	200	16
Appenzell Ausserrhoden	AR	2004 - 2007	190	16
Appenzell Ausserrhoden	AR	2008 - 2017	200	16
Appenzell Innerrhoden	AI	2004 - 2007	180/ 185 (kid 3)	16
Appenzell Innerrhoden	AI	2008 - 2017	200	16
St. Gallen	SG	2004 - 2007	170/ 190 (kid 3)	16
St. Gallen	SG	2008 - 2017	200	16
Grisons/ Graubünden	GR	2004	175	16
Grisons/ Graubünden	GR	2005 - 2006	185	16
Grisons/ Graubünden	GR	2007 - 2008	195	16
Grisons/ Graubünden	GR	2009 - 2017	220	16
Aargau	AG	2004	150	16
Aargau	AG	2005 - 2008	170	16
Aargau	AG	2009 - 2017	200	16
Thurgau	TG	2004 - 2007	190	16
Thurgau	TG	2008 - 2017	200	16
Ticino	TI	2004 - 2007	183	16
Ticino	TI	2008 - 2017	200	16
Vaud	VD	2004	150/ 320 (kid 3)	16
Vaud	VD	2005 - 2006	160/ 330 (kid 3)	16
Vaud	VD	2007	180/ 350 (kid 3)	16
Vaud	VD	2008 - 2013	200/ 370 (kid 3)	16
Vaud	VD	2014 - 2015	230/ 370 (kid 3)	16
Vaud	VD	2016 - 2017	250/ 370 (kid 3)	16
Valais	VS	2004 - 2007	260/ 344 (kid 3)	16
Valais	VS	2008	273/ 361 (kid 3)	16
Valais	VS	2009 - 2017	275/ 375 (kid 3)	16
Neuchâtel	NE	2004	163 (first)/ 180 (second)/ 200 (third)/ 250 (fourth)	16
Neuchâtel	NE	2005	164 (first)/ 180 (second)/ 200 (third)/ 250 (fourth)	16
Neuchâtel	NE	2006	165 (first)/ 180 (second)/ 200 (third)/ 250 (fourth)	16
Neuchâtel	NE	2007	170 (first)/ 190 (second)/ 200 (third)/ 250 (fourth)	16
Neuchâtel	NE	2008	180 (first)/ 200 (second)/ 200 (third)/ 250 (fourth)	16
Neuchâtel	NE	2009 - 2014	200/ 250 (kid 3)	16
Neuchâtel	NE	2015 - 2017	220/ 250 (kid 3)	16
Geneva	GE	2004 - 2008	200/ 220 (above 15 yo)	18
Geneva	GE	2009 - 2011	200/ 300 (above 16 yo)	18
Geneva	GE	2012 - 2017	300/ 400 (above 16 yo)	18
Jura	JU	2004 - 2006	154/ 178 (>=3 kids)	16
Jura	JU	2007 - 2008	160/ 186 (>=3 kids)	16
Jura	JU	2009 - 2017	250	16

Note: Source: Data in this table are obtained from the Federal Social Insurance Office (FSIO) in Bern, Switzerland for child benefits by canton by year, and the Cantonal Compensation Offices in the 26 cantons in Switzerland.

APPENDIX B2

A complete set of the variables used in the empirical analysis and their descriptions

Variable	Description
AnotherChild	➢ Indicator for whether the family has had another child since the previous year
NumKids	➢ Number of children the family has
AllowancesTotal	➢ Annual amount of child (family) allowances the family has reported to receive
Age	➢ Age of the respondent
AgeYoungestChildInHh	➢ Age of the youngest child
HrsWorked	➢ Hours worked per week by the respondent
HrsWorkedOfMomInHh	➢ Hours worked per week by the mother in the family
HrsWorkedOfFatherInHh	➢ Hours worked per week by the father in the family
YrsEduc	➢ Years of education of the respondent
NetHhldIncome	➢ Annual net household income
NetIncomeFatherInHh	➢ Annual net income of the father in the family
NetIncomeMotherInHh	➢ Annual net income of the mother in the family
NumHhld	➢ Number of people in the household
EligibilityAmntAnnual	➢ Annual amount of child (family) benefits the family is eligible for according to the cantonal and federal legislation
BothParInHh	➢ Indicator for whether both parents are present in the household
Married (excluding children)	➢ Indicator for whether the respondent is married
AvgOrBetterHealth (excluding children)	➢ Indicator for whether the respondent is in average or better health, i.e. it is equal to 1 if the health status of the individual is "very well," "well," or "so, so (average)," and it is equal to 0, otherwise ("not very well", or "not well at all")
ExistsChInBadHealthInHh	➢ Indicator for whether there is a child in worse than the average (i.e. bad) health present in the household
ProblemsWithKids	➢ Indicator for whether the respondent has reported that (s)he has problems with his/ her children
DBn2006and2009	➢ Indicator for the transition period between the announcement of the upcoming reform at national level and the time it came into effect
D2009andAfter	➢ Indicator for 2009 and after it (after the benefits floor became mandatory for all cantons)

Note: Source: Data are obtained from the Swiss Household Panel (SHP) longitudinal dataset, the Federal Social Insurance Office (FSIO) in Bern, Switzerland, and the Cantonal Compensation Offices in the 26 cantons in Switzerland.

APPENDIX C1

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
*HhldIncomeLess*24000	➢ A dummy variable indicating that the total annual income of the household is less than 24000 euro (= 1 if true, and = 0 otherwise)
*HhldIncomeBn*24000*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)
*HhldIncomeBn*60000*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

Notes: Source: Rounds 2 (2004) and 5 (2010) of the ESS survey for Switzerland. $Number\ Children$ 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 3 household income dummy variables described in the table have a value of 0, 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.

APPENDIX C2

To simplify the exposition, we focus on the model specification with two outcomes for the dependent variable, *PlanChild*, which takes the value 1 if the respondent is inclined to have a child in the 3 years following the interview, and 0 otherwise.

We employ throughout the following notation:

- $p_k^b \equiv Prob(PlanChild = 1|X, NChildren = k)$ denotes the probability that $PlanChild = 1$ for an individual with set of controls X and k children in 2004 (*before*);
- $p_k^a \equiv Prob(PlanChild = 1|X, NChildren = k)$ denotes the probability that $PlanChild = 1$ for an individual with set of controls X and k children in 2010, after the policy reform extending the maternity leave was implemented (*after*);
- $p_k^c \equiv Prob(PlanChild = 1|X, NChildren = k)$ denotes the probability that $PlanChild = 1$ for an individual with set of controls X and k children in 2010 if the policy reform extending the maternity leave had not been implemented (*counterfactual*).

As a reminder, the choice of the treatment group in the difference-in-differences estimation implies that out of the set of individuals with children, i.e. with $k>0$, the analysis restricts attention

to those who had a child in 6 calendar years prior to the year of the interview. The data allow estimating p_k^b and p_k^a, but not p_k^c.

Our logistic regression results presented in Table 6, where we consider as treatment groups individuals with 1 and 2 children, respectively, imply the following:

$$\log\frac{p_1^a}{1-p_1^a} - \log\frac{p_1^b}{1-p_1^b} \geq \log\frac{p_0^a}{1-p_0^a} - \log\frac{p_0^b}{1-p_0^b} \quad (1)$$

$$\log\frac{p_2^a}{1-p_2^a} - \log\frac{p_2^b}{1-p_2^b} > \log\frac{p_0^a}{1-p_0^a} - \log\frac{p_0^b}{1-p_0^b} \quad (2)$$

The first equation captures the fact that the coefficient on the interaction term between the *Treated* and *After* variables is insignificant when the treatment group is defined to consist of the individuals who had 1 child born in the 6 years prior to the interview year, and the control group is the set of individuals with no children, but the coefficient is positive for all specifications of the model, and for most specifications, the null hypothesis that the coefficient is negative can be rejected at 10% significance level or close to it. The second equation captures the finding that the same coefficient is positive at a significance level of 10% or higher when the treatment group is the set of individuals who have 2 children, with at least one of them born in the 6 years prior to the interview year.

Focusing on the case of a treatment group of individuals with 2 children, we can rewrite (2) as:

$$\left(\log\frac{p_2^a}{1-p_2^a} - \log\frac{p_2^c}{1-p_2^c}\right) + \left(\log\frac{p_2^c}{1-p_2^c} - \log\frac{p_2^b}{1-p_2^b}\right) > \left(\log\frac{p_0^a}{1-p_0^a} - \log\frac{p_0^c}{1-p_0^c}\right) + \left(\log\frac{p_0^c}{1-p_0^c} - \log\frac{p_0^b}{1-p_0^b}\right) \quad (3)$$

Our underlying parallel trends assumption implies that

$$\log\frac{p_2^c}{1-p_2^c} - \log\frac{p_2^b}{1-p_2^b} = \log\frac{p_0^c}{1-p_0^c} - \log\frac{p_0^b}{1-p_0^b} \quad (4)$$

i.e. that in the absence of the reform, the two groups of individuals would have moved on the same trend. Employing (4), we can rewrite equation (3) as follows:

$$\left(\log\frac{p_2^a}{1-p_2^a} - \log\frac{p_2^c}{1-p_2^c}\right) - \left(\log\frac{p_0^a}{1-p_0^a} - \log\frac{p_0^c}{1-p_0^c}\right) > 0 \quad (5)$$

Thus if the parallel trends assumption is satisfied, then the policy reform must have affected differently the individuals with 0 children and those with 2 children. Specifically, the *net* increase in the latter's probability of planning a child is higher than that of the individuals from the former category. We investigate next the potential channels through which this effect may emerge.

To this aim, note first that given (1), we either have

$$\log\frac{p_1^a}{1-p_1^a} - \log\frac{p_1^c}{1-p_1^c} > \log\frac{p_0^a}{1-p_0^a} - \log\frac{p_0^c}{1-p_0^c} \quad (6)$$

or, if not, then $\log\frac{p_2^a}{1-p_2^a} - \log\frac{p_2^c}{1-p_2^c} > \log\frac{p_0^a}{1-p_0^a} - \log\frac{p_0^c}{1-p_0^c} = \log\frac{p_1^a}{1-p_1^a} - \log\frac{p_1^c}{1-p_1^c}$, where the inequality follows from (5), and the equality from the fact that (1) must hold with equality. Thus

$$\log\frac{p_2^a}{1-p_2^a} - \log\frac{p_2^c}{1-p_2^c} > \log\frac{p_1^a}{1-p_1^a} - \log\frac{p_1^c}{1-p_1^c} \quad (7)$$

We can combine (6) and (7) into the requirement that

$$\log\frac{p_k^a}{1-p_k^a} - \log\frac{p_k^c}{1-p_k^c} > \log\frac{p_{k-1}^a}{1-p_{k-1}^a} - \log\frac{p_{k-1}^c}{1-p_{k-1}^c} \quad (8)$$

must hold for at least one value of $k \in \{1,2\}$.

Now, note that if the policy reform had no effect on the fertility intentions of individuals with 1 or 2 children, then $p_k^a = p_k^c$ and $p_{k-1}^a = p_{k-1}^c$, contradicting condition (8). Therefore, either the group of individuals with 1 child or the group of individuals with 2 children must have changed their child planning because of the reform. There are several ways in which this change may have occurred so as to generate values of the relevant probabilities p_k^a and p_{k-1}^a that would satisfy condition (8).

First, p_1^a and/or p_2^a may have increased proportionally more than p_0^a either because of a behavioral effect, since individuals who have had one child since the reform have experienced the benefits of the extended maternity leave, or because individuals who have already conceived at least one child since the reform planned higher order children within the next 3 years following the interview rather than later. Alternatively, equation (8) could have been satisfied because the reform induced a change in the composition of the groups by determining some individuals to have a child that they would yet have not had otherwise by 2010. In the following, we argue that if equation (8) is satisfied solely due to an effect of the reform through this latter channel, then it must be that a subset of the individuals who made a child because of the reform continued to plan higher order children, rather than stopping their fertility.

Thus, note that (8) could be satisfied if some individuals who would have otherwise had *k-1* children at the time of the interview in 2010 made a child because of the reform, and thus at the time of the interview had *k* children, but would continue to plan an additional child within the next 3 years. Such an effect would increase p_k^a above p_k^c, as it would expand the set of *k*-child individuals who plan a child in the next 3 years, and at the same time would decrease p_{k-1}^a below p_{k-1}^c since it would diminish the set of (*k-1*)-child individuals who would plan a child in the next 3 years. The combined effect of these two adjustments would lead condition (8) to be satisfied. If all individuals who move between these sets would have planned at least *k+1* children to begin with, and those plans would have materialized under both policy regimes, then the effect of the policy reform would be solely to reduce the gaps between the times when they conceive their children, without an effect on the long term overall fertility rate.

The second way in which condition (8) could be satisfied is if some individuals who would have otherwise had only *k-1* children in 2010 made a child because of the reform, and thus would have *k* children in 2010, but would stop planning additional children. Such an effect would decrease p_{k-1}^a below p_{k-1}^c, and would simultaneously decrease p_k^a below p_k^c. In principle, these adjustments may induce condition (8) to be satisfied. We argue next that this cannot be the only way in which the reform affected individuals' child planning so as to lead to satisfy condition (8).

We will use the following notation:

- $C_k \equiv$ the number of individuals with *k* children at the time of the interview, with one of them born between 2005 and 2010, who would have planned a child at the time of the interview in 2010 in the absence of the policy reform.

- $A_k \equiv$ the number of individuals with *k* children at the time of the interview, with one of them born between 2005 and 2010, who would have planned a child at the time of the interview in 2010 with the policy reform.

Also, denote by $\bar{S}$ the number of individuals in the complement of a set *S*. Then, $p_k^a = \frac{A_k}{A_k+\bar{A}_k}$, and $p_k^c = \frac{C_k}{C_k+\bar{C}_k}$, so $\frac{p_k^a}{1-p_k^a} = \frac{A_k}{\bar{A}_k}$, and $\frac{p_k^c}{1-p_k^c} = \frac{C_k}{\bar{C}_k}$. Assume therefore by contradiction that the only effect of the policy was that a number $\Delta > 0$ of individuals moved from the set whose cardinality is denoted by A_{k-1} to the set whose cardinality is denoted by $\bar{A}_k$. Therefore, $A_{k-1} = C_{k-1} - \Delta, \overline{A}_{k-1} = \bar{C}_{k-1}, A_k = C_k$, and $\bar{A}_k = \bar{C}_k + \Delta$ (*). Substituting these in the expressions for the various probabilities stated above, condition (8) becomes $\frac{C_k}{\bar{C}_k+\Delta}\frac{\bar{C}_k}{C_k} > \frac{C_{k-1}-\Delta}{\bar{C}_{k-1}}\ \frac{\bar{C}_{k-1}}{C_{k-1}}$, which with some straightforward manipulations, can be rewritten as $\bar{C}_k + \Delta > C_{k-1}$, and then employing the expressions derived above in (*), as $\overline{A}_k > A_{k-1} + \Delta$. For this condition to be satisfied for some value of Δ, it is sufficient that it is satisfied when $\Delta = 1$, i.e. it is sufficient that $\overline{A}_k > A_{k-1} + 1$.

Restricting attention to the individuals between 25 and 42 years old, which we consider in our preferred model specification, we have $\overline{A}_2 = 15, A_1 = 17,\ \overline{A}_1 = 40$ and $A_0 = 185$. Thus, $\overline{A}_2 < A_1 + 1$ and $\overline{A}_1 < A_0 + 1$. Additionally, $\overline{A}_2 < A_1 + 1$ when we categorize individuals according to the control variables that indicate the respondent's gender and whether or not he or she has a partner. The same relationship holds for all but one category defined by the age group, the hours worked per week and the income bracket, and to all but two categories when categorizing based on the partner's age. On the other hand, the inequality $\overline{A}_1 < A_0 + 1$ holds for all categories defined by the control variables. Finally, a weighted averages of the difference between indicator variables corresponding to $\overline{A}_2$ and A_1, on the one hand, and between $\overline{A}_1$ and

A_0, that accounts for the sample weights, are negative. These findings allow us to conclude that $\overline{A}_2 < A_1 + 1$ and $\overline{A}_1 < A_0 + 1$ in our dataset. Note that this does not mean that the sizes of these two populations can be ordered in this manner in general. We did not perform a statistical test to check such a hypothesis. Instead, we argued that in the case of our specific dataset and estimation, the sizes of the two populations satisfy this property aiming to demonstrate that the results from our estimation could not have been driven solely by this channel. This argument suggests thus that the second potential channel through which equation (8) could be satisfied does not exist.

ABOUT THE AUTHOR

Stefani Milovanska-Farrington graduated from the American University of Bulgaria in 2015 with a CGPA of 3.98 which made her the salutatorian of her class. In the same year, she was admitted to a graduate program in Economics at the University of South Florida, and was awarded a Graduate Presidential Fellowship for her doctoral study.

At high school, Stefani focused mainly on Mathematics, and won 1 third, 2 second and 9 first places in mathematical contests. She became the valedictorian of her class with the highest possible CGPA.

As an undergraduate student at the American University in Bulgaria, she worked as a teaching assistant in both Economics and Mathematics. She received an Award for Best Research Publication in the *Journal of International Finance and Economics*, as well as an Award for Outstanding Achievements in Economics in 2015. One of her research papers was placed among the top 15 papers in Economics in the New Economics Talent 2015 Competition, organized by CERGE – EI, Prague, Czech Republic.

In 2016, Stefani obtained her Master's Degree in Economics from the University of South Florida with a CGPA of 4.00. She used to be a teaching assistant and an instructor of Economics in the same institution.

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