# Fertility and child benefits

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Preliminary and incomplete.

**Abstract**

In this paper, I estimate the effect on fertility of a policy change that reduced universal child benefits in the form of cash transfers to families with three or more children in Denmark. The policy change introduced variation in child benefit payments across families, thereby decreasing the annual household income of a family with three children or more by up to 3000 USD per third or higher birth order child. Using a very rich individual panel data set, I find that the policy reduced the number of higher birth order children born in the year following the policy change. I find that an increase in the child benefit equivalent to 25 USD per month induced a 3-4% reduction in all higher order siblings born, but as much as a 35-41% reduction in third order siblings born. Furthermore, the policy reform introduced variation in the timing of benefits. I use this variation to evaluate to what extent families discount future changes to their income stream.

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# 1 Introduction

Fertility is below the replacement rate in almost all OECD countries[1], contributing to population aging and the unsustainability of social security systems. In response to this fact, many countries, including France, Spain, Germany, Australia, and Canada have introduced policies with the explicit purpose of increasing fertility. These policies typically take the form of a subsidy or tax credit, applied after the birth of a child. Hence it is of key interest to know whether these programs have the intended effect on fertility. Conversely, if such effects are quantitatively important, unintended effects on fertility should be taken into account when policymakers consider changes to other taxes or subsidies for families.

In this paper, I address the question of whether financial incentives affect fertility. I analyze the effects on fertility of a policy change which substantially decreased universal child benefits and created variation in benefits across families. In 2010, the Danish government announced that it would introduce a ceiling of 35000 DKK on the total amount of child benefit a family could receive per year. Child benefit payments had until then been universal, paid out quarterly from birth until the child's 18th year of age, with the amount depending only on the age of the child. For families considering having one more child, the additional benefit they would receive for the additional child – the marginal child benefit – was reduced due to the benefit ceiling, but the reduction varied widely according to the number of children already in the household and their age composition. I use this variation in marginal child benefits to analyze the fertility response in a rich panel data set containing the entire Danish population. The panel structure allows us to follow the behavior of individual households before and after the reform. The reform in Denmark mainly affected families with two or more children. Hence, I estimate birth order specific fertility responses to the policy change for families with two or more children.

Because of the particular structure of the phasing-in of the benefit, the policy introduced variation across households in the timing of the benefit payments, with some families seeing an immediate impact, while other families experienced no immediate effect on the marginal benefit but could look ahead to a large delayed impact on marginal child benefits. Using this variation, I analyze how households respond to an incentive near versus far in the future.

There is a large literature on the effect of financial incentives on fertility. As fertility decisions are often made jointly with decisions determining income, many studies use the variation created by policy changes in order to identify a causal effect. (Whittington (1992) analyzes a US tax exemption

[1]Only Turkey, Mexico and Israel had fertility rates above replacement levels in 2014. (OECD)

for dependents in a household panel dataset.[2] Baughman and Dickert-Conlin (2003) use variation across states in the introduction of the EITC and construct a state- and subgroup-level panel from vital statistics. Milligan (2005) analyzes the effect of the introduction of a child benefit in Quebec and compares it to all other canadian states in the 1991 and 1996 waves of a household survey. Azmat and González (2010) and Brewer et al. (2012) use a difference in differences approach in repeated cross section data to analyze policy reforms in Spain and in the UK, respectively. González (2013) use a regression discontinuity approach using data from vital statistics in Spain. Cohen et al. (2013) use variation across households in a large panel data set in Israel. Slonimczyk and Yurko (2014), in a discrete choice framework, use the introduction of a large universal child benefit in a household panel in Russia.) Since the attempt to estimate the effects of financial incentives on fertility is limited by the available data and the nature of the policy changes, most existing studies have faced one or several of the following problems. When treatment status is determined by a time-varying variable which is endogenous to treatment, such as income, then results from repeated cross section data will be biased if the researcher defines treatment status by income. Hence several studies use a proxy such as education, which is close to time invariant in the study population, to define treatment status. This may introduce bias in the case where the available information is a poor proxy for treatment status. Even when this is not the case, estimates will be less precise than when the researcher can observe treatment status. Similarly, when the policy varies with income, but there is no data available on income, researchers are forced to estimate the mean effect of the policy in the treatment population. If the income distribution is not the same across treatment units, this will give biased results. In contrast, we are able to follow individuals over time, and thus we can correctly assign treatment status. Not only are we able to observe which households are affected by the policy change, we are also able to assign the exact benefit amount that it is currently receiving and the exact amount that it would receive if it were to have an additional child.

Furthermore, it is often difficult to disentangle the effect of financial incentives on fertility from effects of other factors affecting fertility. This is the case either when multiple relevant policies are changed at the same time, for example parental leave rules, access to child care or changes to the tax system, or when eligibility for the child benefit is conditional on the labor market status of the mother. We study a setting where child benefits were universal and not dependent on labor market status or income, and there were no significant simultaneous changes to family policy.[3]

[2]Whittington (1990), using time series data, was the first to study the effect of this policy on fertility. However, this approach is sensitive to time trends in unobserved variables. Indeed, Crump et al. (2011), in a reanalysis of the data, conclude that the relationship between changes to the tax exemption and fertility may have been spurious.

[3]There was, in the same year, a change to unemployment benefit eligibility for the long term unemployed, reducing the benefit period from 4 to 2 years. However, long term unemployment was very low in this group of women, making

Another issue facing researchers is when there is no variation in the policy across comparable households. In this case, researchers will typically choose to compare the treatment group with a comparison group which is in substantial ways dissimilar to the treatment group. This puts at question the assumption that there is a common trend between the treatment and comparison groups. In contrast, the changes to child benefit payments introduced by the reform in Denmark varied widely across households. The variation was determined by the number and ages of the children already in the household, and from the phasing-in scheme decided by the government.

Finally, when changes to financial incentives are small relative to the cost of a child, effects will be small and difficult to detect at available sample sizes. The mean changes to benefits in the reform we study were large. The reform decreased the annual household income of a family with three children or more by up to 3000 USD per third or higher birth order child. Furthermore, as our dataset contains detailed information about the entire Danish population, we should be able to detect any relevant effect sizes.

This study is closest related to Cohen et al. (2013), which to my knowledge is the only other paper using exogenous variation in the size of child benefits in an annual household panel dataset. They study the impact of changes in child benefits on fertility in Israel using policy variation in child benefit payments over time in a large panel data set. One of the ways in which Israel is unusual within OECD countries is in terms of fertility. It has by far the highest total fertility rate, and is one of the only three countries having a total fertility rate above replacement level. Hence it is an open question how well the results on Israel generalize to other developed economy settings. The present study may help provide insight on this question.

The policy change in Denmark introduced variation in the time profile of future benefits. This allows us to examine to what extent fertility is sensitive to payments near versus far in the future. Almlund (2013) predicts that if people are credit constrained, they will postpone childbearing. If people are indeed credit constrained in our sample, we should see a stronger fertility response to payments shortly after childbirth, perhaps especially among low income households. To my knowledge, this study is the first to examine the effect of time discounting on fertility behavior.

The paper is stuctured as follows. Section 2 explains the institutional background of the child benefit in Denmark and the details of the 2010 policy reform. In section 3, I describe the data used for the study. Section 4 outlines the empirical strategy, and in section 5, I present the results. Section 6 concludes.

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it unlikely to have had much effect on fertility.

## 2 Institutional Setting

Before 2011, every family with children under 18 in Denmark was entitled to universal child benefits. The child benefit consisted of a payment per child in the household every year until the child were 18. The payment varied with the age of the child, starting at DKK 16988 (2010) for a newborn and decreasing to DKK 13448 at 3 years of age and DKK 10580 at 7 years of age (table 1). The government announced on May 25, 2010 that a parliamentary majority was in favor of introducing a ceiling on the total amount of child benefits a household would receive per year[4]. The first mention in the media of such a ceiling was also on May 25, with the clear expectation that such a law would be passed, although with some room for the negotiation of details. The law was passed on June 26, 2010 with a ceiling of 35000 DKK per year, taking effect from January 1, 2011. The implication of the law was thus that it would only affect families with at least three children, and more so the families with younger children. A gradual phasing-in plan would ensure that households with many children would only feel the full effect of the law change in 2020, with most of the adjustment taking place in 2011 and 2012. Figure A.2 shows the post-reform distribution of benefits conditional on the number of children already in the household. Due to the nature of the phasing-in, the families for whom the marginal child benefit was most affected were those with fewer children. This is because the phasing-in limited the total amount that the child benefit could be reduced by after the reform. Hence, for families with many children who had already had their total benefit reduced by the maximum amount, an additional child would result in the full pre-reform marginal child benefit payment for the familiy.

The media coverage of the new ceiling on child benefits started on May 25, 2010. While the law was not yet passed, a political majority was in favor of such a law, and it would thus potentially affect conceptions taking place after May 25. The first full term births potentially affected by the new law would thus take place on January 25, 2011.[5][6]

The coverage of subsidized childcare in Denmark is very high with almost all children aged 1-5 in subsidized public or private daycare. The government subsidized 75% of the cost of placing a child in care in 2010, with small variation in the cost of public childcare by municipality.

[4]The government coalition consisted of Venstre and Konservative, politically both right-of-centre parties in a Danish context.

[5]A child is considered full term if born between 37 and 42 weeks of gestation, and since by convention, gestation is calculated from two weeks before the child is conceived, the first full term births would take place 35 weeks after initial media coverage.

[6]Abortions are legal until 13 weeks of gestation, implying that the first possible effect on births due to an increase in abortions would be observed 24 weeks after initial media coverage, on November 9, 2010. In this paper, we will not take this potential drop in fertility into account. If this effect is significant, we will tend to underestimate the impact of the reform, since some of the effect is already taking place in the period which we have labeled as pre-reform.

In the current analysis, I have not accounted for a simultaneous change in the unemployment benefit eligibility requirements for long term unemployed, a policy introduced and taking effect in 2010. However, families affected by long term unemployment is less than one percent of the sample used in this paper.

# 3 Data

I use a registry based panel data set on all women in Denmark who were 18 to 41 years old between 2005 and 2011. I restrict the data set to women who cohabited with a male partner in a given year. The data set contains information on the number of own children under 18 and their ages in years, as well as whether the woman had another child (by birth or adoption) in years 2006 to 2012. It also contains years of education, log disposable household income, and whether the woman is an immigrant or a second generation descendant of an immigrant.

I calculate the marginal child benefit before and after the reform, i.e. the additional benefit a household would receive if the parents were to have another child. The marginal child benefit depends on the number of children in the household and their ages. Hence I can calculate the present dicounted value of the stream of future child benefits conditional on whether the household has one more child next year.[7] This calculation takes into account the phasing-in scheme given in the law.[8]

Summary statistics for the full sample and for those affected by the reform, i.e. families with at least two children, are shown in table 4.

# 4 Empirical Strategy

In order to identify the causal effect of marginal child benefits on fertility, I exploit the variation across households in the change in marginal child benefits from 2010 to 2011. The variation across households depends deterministically on the number and ages of children in the household. However, when families have preferences for the number and spacing of children, the probability of having a child next year depends also on the number and age of children. Hence if we do not control for the number and age composition of children in the household, our estimates of the effect of the marginal child benefit on fertility will be biased. Hence we need to carefully control for the variation in family composition so that the coefficient on marginal child benefits does not simply pick up the

[7] I use a discount rate of 0.02.

[8] Child benefits and household incomes are deflated using the consumer price index with 2010=100.

effect of family composition. In order to do this, we construct treatment groups such that each group consists of all women with a particular number of children and a given age composition of these children. Hence we compare families with a given household composition in 2010 with families with the same composition 2011. Since all children become a year older from 2010 to 2011, the same family cannot enter the same treatment group both in 2010 and 2011. After controlling for individual characteristics, there is then variation across family composition groups in the change in marginal child benefits. Assuming that trends in fertility were the same across all treatment groups, we can use this variation in the change in marginal child benefit from 2010 to 2011 across groups to identify the effect of the change in marginal child benefits.

In the basic specification, I follow Cohen et al. (2013) and estimate a linear probability model where the dependent variable is the probability of woman $i$ of becoming pregnant in year $t$.

$$\begin{aligned} pregnant_{it} = \quad & \alpha + marg.\ child\ benefit_{it}\delta \\ & \textstyle\sum_j \sum_k \sum_m 1\left(\#kids_{it} = j\right) \\ & 1\left(\#kids\ in\ age\ range_{it} k = m\right)\beta_{jkm} \\ & + x_{it}\phi + \rho_e + \tau_t + \epsilon_{it} \end{aligned} \tag{1}$$

The independent variable of interest is the present discounted value of the marginal child benefit that woman $i$ will receive if she has another child next year. I follow the approach taken by Cohen et al. (2013) in order to include household composition "fixed effects": We compare year t households with j children who have m kids in age range k to year s households who also have j children who have m children in age range k in year s. The terms inside the summations are the product of an indicator function which is 1 if the household has j kids and an indicator function which is 1 if the household has m kids in age range k. Mother characteristics (log household income, age and years of education) are denoted by$x_{it}$, $\rho_e$ denotes whether the mother has Danish parents, at least one immigrant parent, or is herself an immigrant, and $\tau_t$ are year fixed effects, and $\epsilon_{it}$ is an error term.

Unobservable mother characteristics such as preferences for the number of children are likely to affect fertility. We may attempt to solve this problem by estimating a model with mother fixed effects and without time invariant mother characteristics such as immigration status.

$$\begin{aligned} pregnant_{it} = \quad & \alpha_i + marg.\ child\ benefit_{it}\delta \\ & \textstyle\sum_j \sum_k \sum_m 1\left(\#kids_{it} = j\right) \\ & 1\left(\#kids\ in\ age\ range_{it} k = m\right)\beta_{jkm} \\ & + x_{it}\phi + \tau_t + \epsilon_{it} \end{aligned}$$

However, this specification includes a lagged dependent variable, the number of children in the household, hence estimates will not be consistent.

Hence, I also estimate a separate model by number of children in the household. This has the

problem that we cannot include the group of households with less than two children, and so the 2011 fixed effect may pick up the effect of the reform.

The data description in tables 2 and 3 suggest that the effect of the child benefits on fertility is not absolute, but relative. The number of higher order siblings born declined by 12-14% from 2010 to 2011. Since the total number of first and second order siblings born is much larger than the number of higher order siblings born, a model of absolute changes will put too much weight on the 2011 year effect and too little weight on the reform. Hence we would like to estimate a model where the coefficients can be interpreted as percentage changes. When the dependent variable has a "small" probability (in this case the probability of pregnancy is 9.3% in the full sample), the coefficients in a logit model satisfies this requirement.[9] Hence, I also estimate a logit model using the same independent variables as in model 1.

# 5 Results

Table 8 shows the regression of whether a familiy has a child next year on the marginal child benefit in the pooled and fixed effects specifications. Column (1) includes the number of children, but does not control for the ages of the children already in the household. The coefficient on the marginal child benefit is positive and significant at the 0.1 percent level. The coefficient is interpretable as the change in the probability of having a child next year when the present discounted value of the marginal child benefit is increased by 10,000 DKK. As suspected, including child age dummies in column (2) reduces the coefficient by an order of magnitude, but it remains positive and significant.

Columns (3) and (4) include mother fixed effects. The coefficient on the marginal child benefit is still positive and significant, and very different from the pooled model estimates. Including child age controls does not change the coefficient much - this may be because the fixed effects ensures that the variation in family composition is small within a given household.

In table 11, the model is estimated separately by number of children already in the household. The coefficient is still significant and positive for 2 and 3 children, but no longer so for 4 and 5 children.

In tables 13 and 12, we can see the corresponding logit estimations. In table 13, we include all households . The coefficient on the marginal child benefit is positive and significant and, since the baseline fertility is small, we may interpret it as a 4 pct. increase in number of children born when the present discounted value of the marginal child benefit increases by 10,000 DKK. In table

[9] Let $P(y) = \frac{e^{x\beta}}{1+e^{x\beta}}$, where $P(y)$ is "small". Then $e^{x\beta}$ must be "small", which implies $P(y) \approx e^{x\beta}$, so $lnP(y) \approx x\beta$, and it is then well known that $\beta$ is interpretable as the percentage change in $P(y)$ from a one unit change in $x$.

12, we can see the estimations by number of children already in the household. Again, we find a significantly positive coefficient in 2- and 3- children households, but a significant negative coefficient for 4-children households and no effect for households with 5 children.

The coefficients on the entire sample and on the 2- and 3- children families are summarized in table 15. Using the coefficients and the baseline fertilities within each group, we can calculate the percentage change in fertility implied by an increase in the present discounted value of 25,000 DKK. This is equivalent to a monthly increase of 140 DKK, or approximately 25 USD in 2010-prices. This amount in close to the mean change per family due to the reform, and it is also comparable with results in Cohen et al. (2013) who calculate effects using a change of 150 NIS per month, which corresponds approximately to a lifetime change of 50,000 DKK. Hence, we can compare with half of the effect in Cohen et al. (2013). In the pooled estimation, the baseline fertility for families with at least 2 children is 3.7 percent. A change of 25,000 DKK will thus give rise to a 3.0 pct reduction in fertility for this group. This is based on a large variation in effects depending on the number of children already in the family. For families with 2 children, we find a reduction of 35-41 pct. in baseline fertility, and for 3-children households a reduction of 6.2 pct. in the logit specification and 7.9 pct. in the linear specification. For families with more than 3 children, we did not see consistently significant coefficients.

The overall picture is thus that there is a strong response with respect to the third child, a smaller response for the fourth child, and no significant response for higher order children. This should be interpreted in light of the fact that because of the phasing in, most families with many children none or only a small change in their marginal child benefit within the next few years, making the sample variation very small within this group. However, they did see large changes in their total benefit. This shock, however, is not significant in the pooled specifications for this group. As seen in table 5, the distribution of fertilities by number of children is bimodal, which may be interpreted as some households simply having a very strong preference for many children. It is conceivable that households with stronger preferences for many children will respond less strongly to an income shock when deciding whether to have an additional child. One possibility for the higher order sibling households is that because of the gradual phasing in, they decide to have their fifth child sooner than they would have otherwise. There is some suggestion of this as the effect of the marginal child benefit is negative and borderline significant for four children households. A dynamic model would be required to further investigate this hypothesis.

In the above analysis, we are using a measure of marginal child benefits which assumes standard discounting using a discount rate of 0.02. It is also assumed that potential parents understand the phasing in schedule. It is possible that households put more weight on the change in benefits in the

next few years than implied by exponential discounting, or that they are not aware of or able to understand the phasing in schedule. To investigate this, I compute three alternative measures of the marginal child benefit. In the first, I simply use a higher discount rate of 0.2 rather than 0.02. In the second, I also use a discount rate of 0.2, but assume that families do not take into account the benefits they will receive more than five years from now, i.e., they put zero weight on these. In the third, I again assume a discount rate of 0.02 but assume that the law takes full effect immediately, i.e. that there is no phasing in. The results from using these alternative definitions of the marginal child benefit are shown in table 9. Neither of these alternative measures are as significant as the standard measure, suggesting that households are more likely to take the full future schedule of benefits and phasing in into account. To interpret the change in the size of coefficients, we need to take into account that the scaling changes when we change the discounting assumptions. A monthly change of 140 DKK under the four different set of assumptions is equivalent to a lifetime change of 25500, 8200, 5100, and 25500 respectively. Hence, changing the scaling of the coefficients to the effect of a 140 DKK change per month in the pooled specifications, we get coefficients of 0.00158 in the standard case, 0.000803 in the high discount rate case, 0.000470 in the first five years only case, and 0.00112 in the case where families assume no phasing in. This suggests that the case with standard discounting and full information seems to best capture the beliefs and prefences held in the population.

In an alternative specification, I used only the unexpected change to the marginal child benefit as a regressor. However, the results were exactly the same beyond the precision level used in the reported tables. This is because the non-surprise year on year variation in child benefits is small relative to the 2011 surprise change.

I then split the population by income, estimating the model for the households who were below the poverty line, defined as 50 percent of the median income, as well as in the bottom and top deciles and the middle 80%. Results are reported in table 14. The coefficient on the marginal child benefit is significant in all subpopulations, and the effect of a USD 25 monthly increase in the child benefit is an increase in fertility of 29.6 percent in the below poverty line population, an increase of 15.9 percent in the bottom 10 percent, an increase of 7.8 percent in the middle 80 percent and a decrease of 10.5 percent in the top 10 percent. Hence, there is a plausible income gradient in the effect of the marginal child benefit.

# 6 Conclusion

The present analysis has demonstrated a positive, quantitatively significant effect of child benefit payments on the probability of having a third or higher-order child. I find the largest effects on the probability of having the third and fourth child. A reduction in child benefits of 25000 DKK reduces the probability of having a 3rd or higher order child by 3-10% on average and as much as 35-41% for the third child, and 6-8 percent for the fourth child. However, effects are negative or insignificant for higher order children. One explanation for this may be that there is heterogeneity in preferences across families. One type of family might adhere to the strong Danish social norm of having two children, with a third child being a luxury, and thus exhibits a high elasticity with respect to the marginal child benefit. The other family type may have strong preferences for many children and are not very sensitive to the cost of having an additional child. This could come about if a family has already made permanent adjustments in lifestyle such as less time spent on market work, which makes the cost of an additional child small.

The model does not allow us to answer whether people are permanently reducing or only postponing fertility. A dynamic model of fertility is needed to address this question. I will leave such a model for future work.

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# A Figures and Tables

Table 1: Universal child benefit amount based on each child's age

| Age of child | Benefits (DKK) |
|---|---|
| 0 to 3 years | 16988 |
| 3 to 7 years | 13448 |
| 7 to 18 years | 10580 |

Table 2: Annual percentage change in total live births by birth order

| | 2008 | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 |
|---|---|---|---|---|---|---|---|
| 1st child | 0.3 | -1.0 | -0.1 | -5.8 | -0.7 | -2.7 | 2.5 |
| 2nd child | 1.9 | -2.5 | 1.0 | -4.8 | -3.0 | -1.6 | 2.1 |
| 3rd child | 1.6 | -7.0 | 1.0 | **-12.1** | -0.2 | -1.3 | -0.6 |
| 4th child | 4.3 | -8.7 | -0.6 | **-14.3** | 0.6 | -3.5 | -0.5 |
| >= 5th child | 3.7 | -8.4 | -2.1 | **-9.8** | -0.8 | -14.1 | -1.2 |

Table 3: Annual pct. change in nth order siblings born into n-1-children households

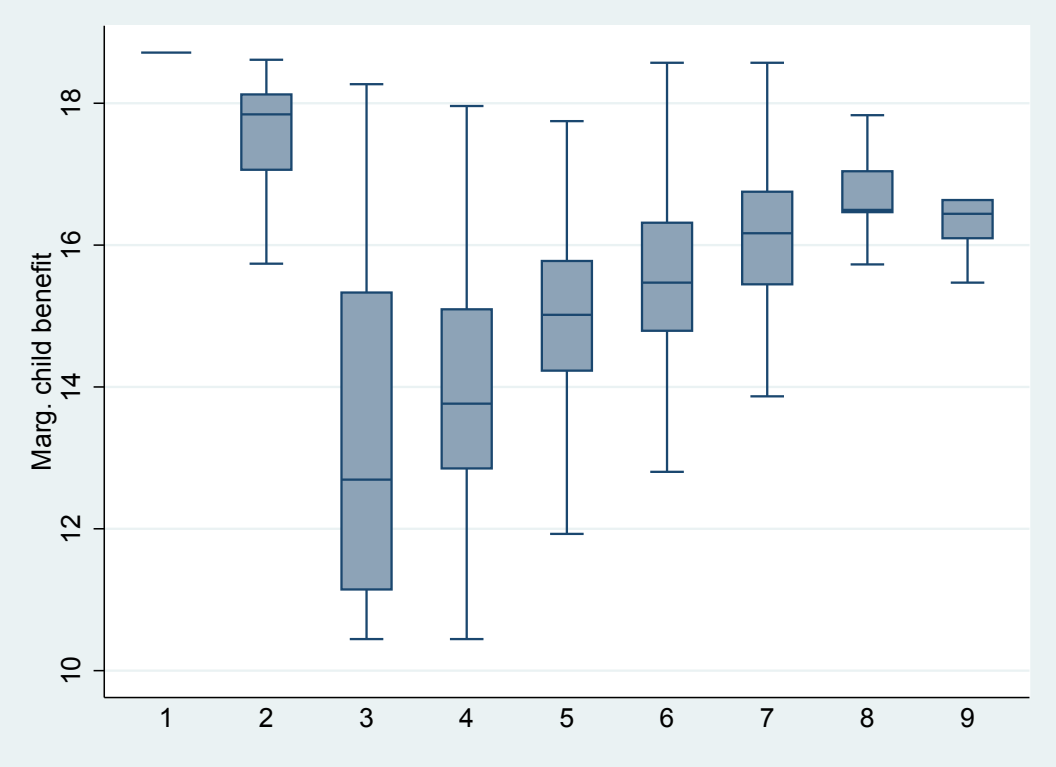
| | 2008 | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 |
|---|---|---|---|---|---|---|---|
| 1st child | -0.2 | -1.2 | -0.4 | -5.9 | -1.3 | -3.3 | 1.5 |
| 2nd child | 1.5 | -3.3 | -0.1 | -5.7 | -3.7 | -2.5 | 1.3 |
| 3rd child | 1.2 | -7.6 | 0.7 | **-12.4** | -0.1 | -1.1 | -0.1 |
| 4th child | 3.4 | -9.7 | -0.8 | **-14.6** | 1.3 | -3.0 | 0.4 |
| 5th child | 1.5 | -5.1 | 3.4 | **-13.4** | 5.1 | -17.7 | 10.7 |
| >= 6th child | 7.7 | -11.0 | -4.5 | **2.5** | -4.9 | -3.0 | -14.1 |

Figure A.1: Total Number of Live Births in Denmark


Table 4: Summary Statistics

| | Full sample | | >1 child | |
|---|---|---|---|---|
| | Mean | Std. dev. | Mean | Std. dev. |
| Probability of child next year | 0.093 | (0.290) | 0.037 | (0.187) |
| Age | 32.4 | (5.9) | 34.8 | (4.2) |
| Years of schooling | 13.56 | (2.84) | 13.46 | (2.84) |
| Number of children under 18 | 1.42 | (1.09) | 2.34 | (0.61) |
| Children age 0-2 | 0.22 | (0.44) | 0.27 | (0.48) |
| Children age 3-6 | 0.43 | (0.62) | 0.71 | (0.70) |
| Children age 7-14 | 0.64 | (0.85) | 1.16 | (0.88) |
| Children age 15-17 | 0.13 | (0.37) | 0.21 | (0.46) |
| Household disp. income | 412932 | (542200) | 457375 | (443990) |
| Immigrant | 0.12 | (0.32) | 0.11 | (0.32) |
| Descendant | 0.009 | (0.095) | 0.008 | (0.089) |
| Sample size | 3504993 | | 1781733 | |

Figure A.2: Marginal child benefits post reform by number of children




Present discounted value of marginal child benefits. In 10,000 DKK.

Table 5: Fertility by number of children

| # children in hh | Prob. child next year | Std. err. | No. of obs. |
|---|---|---|---|
| None | 0.132 | (0.000353) | 919642 |
| 1 | 0.172 | (0.000422) | 803618 |
| 2 | 0.040 | (0.000172) | 1281398 |
| 3 | 0.026 | (0.000245) | 415291 |
| 4 | 0.035 | (0.000697) | 69552 |
| 5 | 0.064 | (0.00225) | 11865 |
| 6 | 0.096 | (0.00572) | 2639 |
| 7 | 0.119 | (0.0121) | 715 |
| 8 | 0.167 | (0.0262) | 204 |
| 9 | 0.143 | (0.0505) | 49 |
| 10 | 0.231 | (0.122) | 13 |

Table 6: Fertility by number of children and immigration status

| # children in hh | Non-immigrant Prob. child next year | Std. dev. | Immigrant Prob. child next year | Std. dev. |
|---|---|---|---|---|
| None | 0.138 | (0.345) | 0.093 | (0.290) |
| 1 | 0.178 | (0.382) | 0.139 | (0.346) |
| 2 | 0.037 | (0.190) | 0.061 | (0.239) |
| 3 | 0.022 | (0.145) | 0.051 | (0.221) |
| 4 | 0.027 | (0.162) | 0.057 | (0.232) |
| 5 | 0.056 | (0.231) | 0.074 | (0.261) |
| 6 | 0.082 | (0.274) | 0.106 | (0.308) |
| 7 | 0.130 | (0.337) | 0.112 | (0.315) |
| 8 | 0.161 | (0.370) | 0.171 | (0.378) |
| 9 | 0.200 | (0.414) | 0.118 | (0.327) |
| 10 | 0.333 | (0.577) | 0.2 | (0.422) |

Table 7: Pooled and fixed effects specifications

| | Pooled | Pooled | Mother FE | Mother FE |
|---|---|---|---|---|
| Marg. child benefit | 0.00407*** | 0.000446*** | 0.0130*** | 0.0141*** |
| | (34.29) | (3.61) | (76.45) | (78.50) |
| log hh. income | 0.0367*** | 0.0342*** | 0.0393*** | 0.0348*** |
| | (94.89) | (87.68) | (52.99) | (48.09) |
| Mother's age 20 | −0.0970*** | −0.0935*** | −0.609*** | −0.635*** |
| | (−62.59) | (−60.69) | (−196.51) | (−197.40) |
| Mother's age 25 | −0.0125*** | −0.00965*** | −0.300*** | −0.299*** |
| | (−8.61) | (−6.70) | (−149.90) | (−146.11) |
| Mother's age 35 | −0.0791*** | −0.0779*** | 0.104*** | 0.0857*** |
| | (−76.77) | (−75.31) | (125.33) | (100.33) |
| Mother's age 40 | −0.144*** | −0.133*** | 0.0362*** | 0.0236*** |
| | (−157.80) | (−141.16) | (96.26) | (73.06) |
| 1 child in hh | 0.0393*** | 0.664** | −0.301*** | 0.684* |
| | (69.89) | (2.77) | (−257.40) | (2.35) |
| 2 children in hh | −0.0805*** | 0.720 | −0.681*** | −0.169 |
| | (−160.08) | (1.52) | (−424.61) | (−1.45) |
| 3 children in hh | −0.0821*** | −0.472 | −0.942*** | −0.367*** |
| | (−145.43) | (.) | (−393.23) | (−5.84) |
| 1 0-2 year old | | −0.667* | | −1.001*** |
| | | (−2.03) | | (−4.54) |
| 1 3-6 year old | | −0.136 | | 0.248 |
| | | (.) | | (0.01) |
| 1 7-14 year old | | −0.708* | | −0.700 |
| | | (−2.22) | | (−1.57) |
| 1 15-17 year old | | −0.0217 | | −0.878** |
| | | (.) | | (−3.02) |
| BA/BSc | 0.0216*** | 0.0205*** | | |
| | (60.71) | (55.23) | | |
| MA/MSc | 0.0269*** | 0.0256*** | | |
| | (51.77) | (46.67) | | |
| Immigrant | −0.00311*** | −0.00441*** | | |
| | (−5.35) | (−7.37) | | |
| Descendant | 0.0203*** | 0.0164*** | | |
| | (11.30) | (8.74) | | |
| Constant | 0.185*** | 0.181*** | 0.626*** | 0.572*** |
| | (174.91) | (173.92) | (412.86) | (369.09) |
| Observations | 3397004 | 3397004 | 3484935 | 3484935 |
| $R^2$ | 0.081 | 0.107 | 0.207 | 0.254 |

$t$ statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.Baseline: Mother's age 30 years, education is high school level, no children, mean income and potentially receiving a pre reform marginal child benefit.

Table 8: Pooled and fixed effects specifications

| | Pooled | Pooled | Mother FE | Mother FE |
|---|---|---|---|---|
| Marg. child benefit | 0.00407*** | 0.000446*** | 0.0130*** | 0.0141*** |
| | (34.29) | (3.61) | (76.45) | (78.50) |
| Log hh. income | 0.0367*** | 0.0342*** | 0.0393*** | 0.0348*** |
| | (94.89) | (87.68) | (52.99) | (48.09) |
| Mother's age 25 | −0.0125*** | −0.00965*** | −0.300*** | −0.299*** |
| | (−8.61) | (−6.70) | (−149.90) | (−146.11) |
| Mother's age 35 | −0.0791*** | −0.0779*** | 0.104*** | 0.0857*** |
| | (−76.77) | (−75.31) | (125.33) | (100.33) |
| BA/BSc | 0.0216*** | 0.0205*** | | |
| | (60.71) | (55.23) | | |
| Immigrant | −0.00311*** | −0.00441*** | | |
| | (−5.35) | (−7.37) | | |
| Child age dummies | *No* | *Yes* | *No* | *Yes* |
| Education dummies | *Yes* | *Yes* | *No* | *No* |
| Mother age dummies | *Yes* | *Yes* | *Yes* | *Yes* |
| Constant | 0.185*** | 0.181*** | 0.626*** | 0.572*** |
| | (174.91) | (173.92) | (412.86) | (369.09) |
| Observations | 3397004 | 3397004 | 3484935 | 3484935 |
| $R^2$ | 0.081 | 0.107 | 0.207 | 0.254 |

$t$ statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Baseline: Mother's age 30 years, education is high school level, no children, mean income and potentially receiving a pre reform marginal child benefit.

Table 9: Pooled and fixed effects, impatient households

| | Pooled | Pooled | Mother FE | Mother FE |
|---|---|---|---|---|
| Exp discounting, 0.02 | | | | |
| Marg. child benefit | 0.00400*** | 0.000619*** | 0.0102*** | 0.0108*** |
| | (23.53) | (3.58) | (49.11) | (49.92) |
| Total child benefit shock | 0.00448 | 0.000489 | 0.00654*** | 0.00541*** |
| | (0.91) | (0.97) | (9.72) | (7.90) |
| Exp discounting, 0.2 | | | | |
| Marg. child benefit | 0.00735*** | 0.000982** | 0.0203*** | 0.0229*** |
| | (21.90) | (2.89) | (50.76) | (55.18) |
| Total child benefit shock | 0.00233** | 0.00118 | 0.0140*** | 0.0108*** |
| | (2.65) | (1.31) | (11.61) | (8.86) |
| Discount rate is 0.2 for five years, zero after five years | | | | |
| Marg. child benefit | 0.00785*** | 0.000925* | 0.0221*** | 0.0255*** |
| | (21.24) | (2.47) | (50.42) | (56.02) |
| Total child benefit shock | 0.00382*** | 0.00147 | 0.0176*** | 0.0142*** |
| | (4.13) | (1.56) | (13.86) | (11.10) |
| No phasing in | | | | |
| Marg. child benefit | 0.00296*** | 0.000441*** | 0.00779*** | 0.00846*** |
| | (22.49) | (3.31) | (48.11) | (50.81) |
| Total child benefit shock | −0.00132*** | 0.000397 | 0.0000459 | −0.00142** |
| | (−3.44) | (0.10) | (0.09) | (−2.74) |
| Child age dummies | *No* | *Yes* | *No* | *Yes* |
| Education dummies | *Yes* | *Yes* | *No* | *No* |
| Mother age dummies | *Yes* | *Yes* | *Yes* | *Yes* |

$t$ statistics in parentheses. $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$. Baseline: Mother's age 30 years, education is high school level, no children, mean income and potentially receiving a pre reform marginal child benefit.

Table 10: Pooled, by number of children

| | No children | 1 child | 2 children | 3 children | 4 children | 5 children |
|---|---|---|---|---|---|---|
| Marg. child benefit | 0 | 0 | 0.00560*** | 0.000822*** | -0.00175 | 0.00237 |
| | (.) | (.) | (16.10) | (3.96) | (-1.64) | (0.57) |
| log hh income | 0.0620*** | 0.0514*** | -0.00446*** | -0.0103*** | -0.00941*** | -0.00476 |
| | (73.35) | (42.59) | (-8.37) | (-14.07) | (-5.49) | (-0.78) |
| Mother's age 20 | -0.0239*** | 0.00116 | -0.0181 | 0.117** | | |
| | (-8.93) | (0.12) | (-0.66) | (2.76) | | |
| Mother's age 25 | 0.0670*** | -0.00249 | -0.0354 | 0.0955*** | 0.103*** | 0.0677 |
| | (23.48) | (-0.27) | (-1.34) | (5.56) | (4.95) | (1.35) |
| Mother's age 30 | 0.113*** | 0.0141 | -0.0536* | 0.0620*** | 0.0698*** | 0.0917*** |
| | (34.21) | (1.57) | (-2.04) | (3.83) | (6.05) | (4.48) |
| Mother's age 35 | -0.00352 | -0.0965*** | -0.0906*** | 0.0402* | 0.0458*** | 0.0508** |
| | (-1.06) | (-10.64) | (-3.45) | (2.49) | (4.21) | (3.06) |
| Mother's age 40 | -0.100*** | -0.191*** | -0.118*** | 0.0229 | 0.0305** | 0.0363* |
| | (-39.14) | (-21.39) | (-4.50) | (1.42) | (2.80) | (2.05) |
| BA/BSc | 0.0201*** | 0.0272*** | -0.00299 | -0.00577** | -0.0133*** | -0.00419 |
| | (6.92) | (8.59) | (-1.61) | (-2.65) | (-3.51) | (-0.46) |
| MA/MSc | 0.00834** | 0.0379*** | 0.00386* | -0.00360 | -0.0144*** | -0.00125 |
| | (2.79) | (11.44) | (2.02) | (-1.58) | (-3.43) | (-0.09) |
| Immigrant | -0.0236*** | -0.0267*** | 0.0129*** | 0.0181*** | 0.0147*** | 0.0124* |
| | (-18.69) | (-17.74) | (15.99) | (15.67) | (6.46) | (2.00) |
| 1 0-2 year old | | -0.0222*** | -0.0139*** | -0.000670 | 0.00787** | 0.0212* |
| | | (-15.31) | (-18.35) | (-0.52) | (2.83) | (2.25) |
| 1 3-6 year old | | 0.171*** | -0.00259*** | 0.000286 | 0.00700** | 0.0238* |
| | | (127.29) | (-4.03) | (0.23) | (2.66) | (2.53) |
| 1 7-14 year old | | -0.0100*** | 0.0172*** | 0.00757*** | 0.00625 | 0.0151 |
| | | (-16.33) | (27.72) | (5.46) | (1.40) | (0.79) |
| 1 15-17 year old | | 0 | -0.0138*** | -0.00115 | 0.00329 | 0.000751 |
| | | (.) | (-21.42) | (-0.93) | (1.29) | (0.08) |
| Year is 2010 | 0.00456*** | -0.000792 | -0.00340*** | -0.00271** | 0.00642* | -0.0157 |
| | (3.45) | (-0.53) | (-4.97) | (-2.89) | (2.32) | (-1.79) |
| Year is 2011 | -0.00390** | -0.0106*** | 0.00168* | 0.000363 | -0.0124* | 0.00182 |
| | (-2.97) | (-7.04) | (2.46) | (0.29) | (-2.37) | (-0.10) |
| Constant | 0.113*** | 0.197*** | 0.136*** | -0.0175 | -0.0321* | -0.0856* |
| | (29.17) | (20.78) | (5.18) | (-1.05) | (-2.44) | (-2.29) |
| Observations | 874050 | 776518 | 1259477 | 405706 | 66772 | 11131 |
| $R^2$ | 0.053 | 0.123 | 0.031 | 0.017 | 0.018 | 0.018 |

$t$ statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Baseline: Mother's age 30 years, education is high school level, no children, mean income and potentially receiving a pre reform marginal child benefit. Includes dummies for years, mother's age and education and children's age groups

Table 11: Pooled, by number of children

| | No children | 1 child | 2 children | 3 children | 4 children | 5 children |
|---|---|---|---|---|---|---|
| Marg. child benefit | | | 0.00560*** | 0.000822*** | -0.00175 | 0.00237 |
| | | | (16.10) | (3.96) | (-1.64) | (0.57) |
| Log hh income | 0.0620*** | 0.0514*** | -0.00446*** | -0.0103*** | -0.00941*** | -0.00476 |
| | (73.35) | (42.59) | (-8.37) | (-14.07) | (-5.49) | (-0.78) |
| BA/BSc | 0.0201*** | 0.0272*** | -0.00299 | -0.00577** | -0.0133*** | -0.00419 |
| | (6.92) | (8.59) | (-1.61) | (-2.65) | (-3.51) | (-0.46) |
| Immigrant | -0.0236*** | -0.0267*** | 0.0129*** | 0.0181*** | 0.0147*** | 0.0124* |
| | (-18.69) | (-17.74) | (15.99) | (15.67) | (6.46) | (2.00) |
| Year is 2010 | 0.00456*** | -0.000792 | -0.00340*** | -0.00271** | 0.00642* | -0.0157 |
| | (3.45) | (-0.53) | (-4.97) | (-2.89) | (2.32) | (-1.79) |
| Year is 2011 | -0.00390** | -0.0106*** | 0.00168* | 0.000363 | -0.0124* | 0.00182 |
| | (-2.97) | (-7.04) | (2.46) | (0.29) | (-2.37) | (-0.10) |
| Constant | 0.113*** | 0.197*** | 0.136*** | -0.0175 | -0.0321* | -0.0856* |
| | (29.17) | (20.78) | (5.18) | (-1.05) | (-2.44) | (-2.29) |
| Observations | 874050 | 776518 | 1259477 | 405706 | 66772 | 11131 |
| $R^2$ | 0.053 | 0.123 | 0.031 | 0.017 | 0.018 | 0.018 |

$t$ statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Baseline: Mother's age 30 years, education is high school level, no children, mean income and potentially receiving a pre reform marginal child benefit. Includes dummies for years, mother's age and education and children's age groups

Table 12: Logit, by number of children

| | 2 children | 3 children | 4 children | 5 children |
|---|---|---|---|---|
| Marg. child benefit | 0.165*** | 0.0246* | −0.0888* | 0.0529 |
| | (12.93) | (2.25) | (−2.13) | (0.67) |
| Log hh income | −0.108*** | −0.413*** | −0.308*** | −0.0876 |
| | (−7.35) | (−17.32) | (−6.55) | (−0.79) |
| BA/BSc | 0.0883** | −0.0294 | −0.272** | −0.0868 |
| | (2.84) | (−0.61) | (−3.06) | (−0.55) |
| Immigrant | 0.305*** | 0.527*** | 0.412*** | 0.208* |
| | (18.35) | (17.95) | (7.25) | (2.06) |
| Constant | −2.144*** | −15.44 | −14.76 | −10.85*** |
| | (−6.36) | (.) | (.) | (−6.88) |
| Observations | 1259477 | 405706 | 66761 | 11069 |
| $R^2$ | | | | |

$t$ statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Baseline: Mother's age 30 years, education is high school level, mean income and potentially receiving a pre reform marginal child benefit. Includes dummies for years, mother's age and education and children's age groups

Table 13: Logit specification

| | Logit |
|---|---|
| Marg. child benefit | 0.0405*** |
| | (3.95) |
| log hh. income | 0.467*** |
| | (24.52) |
| Mother's age 20 | −0.230* |
| | (−2.04) |
| Mother's age 25 | 0.543*** |
| | (5.16) |
| Mother's age 30 | −0.720*** |
| | (6.87) |
| Mother's age 35 | −0.0423 |
| | (−0.39) |
| Mother's age 40 | −1.862*** |
| | (−14.70) |
| 1 child in hh | −3.035** |
| | (−3.03) |
| 2 children in hh | −1.873*** |
| | (−5.54) |
| 3 children in hh | −1.016** |
| | (−2.72) |
| 1 0-2 year old | 1.157 |
| | (0.71) |
| 1 3-6 year old | 11.46* |
| | (1.99) |
| 1 7-14 year old | 9.686*** |
| | (7.02) |
| 1 15-17 year old | 1.329 |
| | (1.34) |
| BA/BSc | −0.140* |
| | (−2.27) |
| MA/MSc | −0.175** |
| | (−2.81) |
| Immigrant | −0.0468 |
| | (−1.80) |
| Descendant | 0.224*** |
| | (3.79) |
| Constant | −1.737*** |
| | (−14.49) |
| Observations | 339177 |

$t$ statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.Baseline: Mother's age 18 years, education is 7 years, no children, mean income and potentially receiving a pre reform marginal child benefit.

Table 14: Pooled, by Income

| | Poverty | Bottom 10 pct. | Middle 80 pct. | Top 10 pct. |
|---|---|---|---|---|
| Marg. child benefit | 0.00638*** | 0.00376*** | 0.00115*** | −0.00131** |
| | (4.96) | (4.55) | (5.84) | (−3.01) |
| Total benefit shock | −0.00332 | 0.000644*** | 0.000428 | 0.000508 |
| | (−0.94) | (0.34) | (0.75) | (0.49) |
| log hh. income | 0.00583*** | 0.00772*** | 0.0356*** | −0.00263 |
| | (7.76) | (11.08) | (46.88) | (−0.23) |
| Mother's age 20 | −0.0503*** | −0.0386*** | −0.0895*** | −0.179*** |
| | (−8.39) | (−9.89) | (−41.67) | (−6.95) |
| Mother's age 25 | −0.0122 | 0.00222 | −0.000825 | −0.0853*** |
| | (−1.85) | (0.52) | (−0.48) | (−8.04) |
| Mother's age 35 | −0.0427*** | −0.0489*** | −0.0810*** | −0.0893*** |
| | (−6.01) | (−11.28) | (−67.44) | (−21.22) |
| Mother's age 40 | −0.0783*** | −0.0826*** | −0.135*** | −0.156*** |
| | (−13.24) | (−22.51) | (−123.52) | (−38.59) |
| 1 child in hh | −0.267 | 0.0187 | −0.584 | −0.0375 |
| | (−1.01) | (.) | (.) | (−0.85) |
| 2 children in hh | 0.235*** | −0.191 | −0.307 | −0.398*** |
| | (4.24) | (.) | (.) | (−7.23) |
| 3 children in hh | 0.197 | 0.0201 | 2.845* | −0.235 |
| | (1.42) | (0.00) | (2.00) | (−1.66) |
| 1 0-2 year old | 0.0466 | 0.118 | 0.568 | 0.0319 |
| | (0.81) | (.) | (.) | (0.73) |
| 1 3-6 year old | 0.148 | 0.138 | 1.420 | 0.222*** |
| | (1.56) | (.) | (0.49) | (5.05) |
| 1 7-14 year old | 0.0536 | −0.00715 | 0.536 | 0.0192 |
| | (1.74) | (.) | (.) | (0.96) |
| 1 15-17 year old | 0.263 | 0.286 | −1.356*** | −1.487*** |
| | (0.99) | (.) | (−3.68) | (−6.15) |
| BA/BSc | −0.00616** | −0.00357** | 0.0254*** | 0.0219*** |
| | (−3.12) | (−2.58) | (56.34) | (18.91) |
| MA/MSc | −0.00864** | −0.00284 | 0.0263*** | 0.0359*** |
| | (−2.90) | (−1.32) | (34.51) | (28.77) |
| Immigrant | −0.00171 | 0.00603*** | −0.00381*** | −0.0154*** |
| | (−0.87) | (4.28) | (−5.03) | (−6.16) |
| Descendant | 0.0215** | 0.0282*** | 0.0125*** | −0.0193* |
| | (11.30) | (8.74) | | |
| Constant | 0.0914*** | 0.0919*** | 0.185*** | 0.241*** |
| | (15.11) | (24.25) | (153.02) | (54.53) |
| Observations | 100465 | 255215 | 2377758 | 309230 |
| $R^2$ | 0.040 | 0.039 | 0.111 | 0.158 |
| Baseline fertility | 0.0539 | 0.0590 | 0.0367 | 0.0312 |
| Effect of 25$ incr | 0.296 | 0.159 | 0.0783 | −0.105 |

$t$ statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.Baseline: Mother's age 30 years, education is high school level, no children, mean income and potentially receiving a pre reform marginal child benefit.

Table 15: Relative effect of a 25000 DKK increase in the present value of the marginal child benefit

| Specification | Coefficient | Baseline fertility | Effect of 25000 DKK incr. |
|---|---|---|---|
| Pooled | 0.00045*** | 0.037 | 3.0 pct. |
| Logit | 0.0405*** | 0.037 | 10.1 pct. |
| 2 children, linear | 0.00560*** | 0.040 | 35.0 pct. |
| 2 children, logit | 0.165*** | 0.040 | 41.3 pct. |
| 3 children, linear | 0.00082*** | 0.026 | 7.9 pct. |
| 3 children, logit | 0.0246** | 0.026 | 6.2 pct. |
| 4 children, linear | -0.00175* | 0.035 | -12.5 pct. |
| 4 children, logit | -0.0888** | 0.035 | -22.2 pct. |

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.