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Mothers' income recovery after childbearing

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WORKING PAPER 2012:20

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ISSN 1651-1166

Mothers' income recovery after childbearing[1]

by

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October 2012

Abstract

This paper examines the time profile of the effect of fertility on female labour earnings with respect to time since birth. To address endogeneity of fertility to labour income, we use the same-sex instrument (Angrist and Evans, 1998) in a novel way on a panel data set to uncover the time profile of the fertility effect. Our OLS estimates suggest that the largest impact takes place during the child's first years of life, and then gradually diminishes over the lifecycle, with full recovery of income 15 years after birth. Our IV estimates support this finding, but suggest a faster recovery of earnings, although the estimates are now less precise. We are also able to reproduce this finding with a one-period cross-section and disaggregating the sample by years since third birth to estimate the time profile.

Keywords: Fertility, female labour supply, same-sex siblings, instrumental variables
JEL-codes: J13, J22

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[1] We would like to thank Per Johansson, Peter Skogman Thoursie, Ann-Zofie Duvander and Johan Vikström for valuable comments and suggestions, as well as seminar participants at IFAU. We would also like to thank members of the Skills- and Education sector at the Institute for Fiscal Studies (IFS) for helpful comments on an earlier version of this paper.

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1 Introduction

One of the most prominent labour market phenomena of the 20th century is the dramatic increase in the labour supply of married women, and the parallel decline in fertility rates in OECD countries. Consequently, researchers have devoted much effort to understand the relationship between fertility and female labour supply. In particular, recent studies have focused on establishing a causal link between childbearing and labour supply, recognizing that the two might be jointly determined. In this paper, we exploit parents' preferences for a mixed sex sibling composition as a source of exogenous variation in family size; a method originally applied by Angrist and Evans (1998).[4] An important feature of the present paper, however, is that we employ the sex-mix strategy in a novel way in order to uncover the *time profile* of the fertility effect, with respect to time since birth, on mothers' labour income.

Previous studies relying on the sex-mix instrument to measure to the causal impacts of childbearing are often based on one-period cross-sectional data. This implies that women who very recently gave birth are grouped together with women who gave birth, say, ten years before their labour market status is observed. Thus, women will have different years since "treatment". Nevertheless, fertility is considered as a treatment indicating the mere existence of more than two children, without attention paid to the possibility of varying impacts of treatment with respect to time since birth. However, it seems likely that children have different impacts on their parents' labour market behaviour as they age. For example, a common finding in the previous literature is that the labour force participation of mothers is increasing in the age of the youngest child (Browning, 1992).[5]

The present paper contributes to this existing literature by being able to follow the *same mothers* on a yearly basis over a 15-year horizon after birth. We fix the year of "treatment", which is defined as giving birth to a third child, to occur in one specific year, namely, 1990. This means that we estimate the effect of having a third child in

[4] Other studies that have used the sex-mix strategy include Iacovou (2001) for the UK; Cruces and Galiani (2007) for Argentina and Mexico; Maurin and Moschion (2006) on data from France; and Hirvonen (2009) on Swedish data.

[5] This pattern seems to hold also for Sweden: Figure A1 illustrates the average labour earnings for the 1995 cross section of Swedish mothers, against the age of their youngest child. The graph shows an increasing labour income in the age of the youngest child, with a very steep increase between child ages one and two.

1990, for a sample of mothers with exactly two children by the end of the preceding year. We then explore how this effect evolves over time since birth, over a 15-year horizon for the same mothers. To this end, we use a longitudinal data set based on population-wide Swedish registers.[6] We also provide an analysis based on a one-period cross-section using the same data sources as used in our main analysis, and explore whether we can reproduce our main results using the approach of a synthetic life cycle constructed by estimating the impacts separately for subgroups of mothers defined by years since third birth.

Understanding the dynamics of individuals' labour supply responses to an increase in fertility over the life cycle is crucial since it has important policy implications. This is especially true considering the substantial number of existing studies suggesting adverse consequences of lengthy career interruptions due to childbearing, and their potential to explain a non-negligible proportion of the male-female wage gap (see e.g. Goldin and Polachek, 1987; Gronau, 1988; or Fuchs, 1989). Despite the importance of knowledge about the degree of persistence of the effect of childbearing, the literature on such dynamics is relatively scarce. Nevertheless, the existing evidence often suggests that the impact of childbearing is short run. For instance, using Swedish data, Stafford and Sundström (1996) find that for both men and women, earnings rebound for time out for childcare reasons in the longer-run. Hirvonen (2009), using the sex-mix instrument and Swedish data, finds that Swedish mothers' earnings catch-up in the long run after an initial negative impact of childbearing.[7] For the U.S., Jacobsen et. al. (1999), using the occurrence of twins in a first birth as an instrument for the number of children, find that although the overall effects of childbearing are small, there are significant effects in the years immediately following birth. Also using the twins-first instrument, Bronars and Grogger (1994) find large short run impacts of childbearing on labour force participation among unwed mothers, with most of the adverse effect dissipating over time for whites, but a more persistent negative effect on black unwed mothers. Vere (2011) also finds that the effects of fertility on female labour supply are greatest when

[6] In addition, we replicate the main analysis for five additional treatment years, which allows us to study whether our main findings are specific to the 1990 cohort.

[7] We use a different approach than Hirvonen (2009), which is explained in Section 4.2

the child is born and then rapidly decline. Finally, Rondinelli and Zizza (2010), using exogenous variation in family size caused by infertility shocks also find that the effect of childbearing on Italian women's labour market outcomes do not persist in the longer run.

One possible limitation of the above results is that the effect variation over the life cycle is often estimated by means of sample disaggregation by years since birth using a single cross-section. In essence, this implies that one constructs a synthetic life cycle. One concern with this approach is the possibility of confounding of age- and cohort effects. Bronars and Grogger (1994) address this issue by instead following the same cohort of mothers at two different points in time using successive cross-sections. However, with a long time interval in between these points, the year-to-year evolution of the fertility effect cannot be investigated.

Our OLS estimates suggest that having a third child is associated with a statistically and economically significant decrease in mothers' labour earnings that largely takes place the first couple of years after giving birth, followed by a gradual catching-up effect over the life-cycle, and nearly full recovery 15 years after birth. Our IV estimates support this story, but suggest an even faster recovery of earnings compared to the OLS analysis: using an indicator for same-sex children as an instrument for fertility, we find that the effect of a third child on mother's earnings is only statistically significant in the first two years after birth. After the first two years, the point estimates are smaller in magnitude compared to the OLS estimates, and not significant. These findings hold true also when we replicate the analysis for five additional separate treatment years – 1991 through 1995.

Finally, based on a one-period cross-section, the analysis from regressions on subgroups of mothers defined by time since third birth reveals a time profile very similar to that obtained using panel data. This suggests that estimates based on cross-sectional data are mainly driven by mothers with very young children. Also, the analysis suggests that, in the absence of longitudinal data, constructing a synthetic life cycle by means of sample disaggregation seems a valid approach to study long-run impacts of fertility. It should be noted that our IV estimates are not very precisely estimated. Nevertheless, all our analyses result in more or less the same time profile of the fertility effect: a large negative effect on earnings in the years immediately following birth that "bounces back" quite rapidly.

The paper proceeds with a discussion on causal inference and fertility in Section 2. Section 3 contains a description of the data and the main results are presented in Section 4. Sections 5 and 6 contain a sensitivity analysis and the one-period cross-section results, and Section 7 concludes the paper.

2 Causal inference and fertility

2.1 Identification

The effect of fertility on female labour supply has been of longstanding interest in the economics literature, and can partly be motivated by the potential of career-interruptions due to childbearing to explain a non-negligible proportion of the male-female wage gap. However, assessing the causal relationship between fertility and female labour supply has shown to be difficult: the potential endogeneity of fertility in labour supply equations is by now well recognized (see e.g. Browning, 1992, for an overview of the literature). For example, fertility decisions are likely made taking into account one's earnings potential. Moreover, unobserved individual heterogeneity in preferences might affect both labour supply and fertility decisions. Failing to account for such endogeneity means that an estimated relationship between children and their parental labour market behaviour will not have a causal interpretation. To address these issues, we follow Angrist and Evans (1998) and exploit parents' preferences for a mixed-sex sibling composition as a source of exogenous variation in family size: parents whose first two children are of the same sex are more likely to have a third child compared to parents whose first two children are of mixed sex. While the sex-mix of children, which is in essence randomly assigned, has an impact on the number of children, it is not likely to have a direct impact on parental labour market behaviour. Therefore, a dummy variable indicating whether an individual's first two children are of the same sex can be used as an instrument for higher order fertility among individuals with at least two children.

Interest then lies in the difference in labour market outcome of a mother with and without further childbearing. More formally, we can think about this question in a potential outcomes framework.

First, for a population of mothers with exactly two children by the end of 1989[8], we will fix the year of treatment, which is defined as giving birth to a third child, to occur in one particular year, namely in 1990. We then let D_i be an indicator for women who gave birth to their *third* child in 1990 in the population of women who had exactly two children by the end of 1989. Moreover, let Y_{1i} denote individual *i*:s labour market outcome if $D_i = 1$, and let Y_{0i} denote individual *i*:s outcome if $D_i = 0$. Both of these potential outcomes are well-defined for every individual, although only one is ever observed for any one individual. Furthermore, let Z_i be a binary variable with $Z_i = 1$ if individual *i*:s first two children are of the same sex, and let $Z_i = 0$ otherwise.

The key assumptions for IV to consistently estimate the effect of interest are (1) *independence*: $Y_{0i}, Y_{1i}, D_{1i}, D_{0i} \perp Z_i$, (2) *first stage*: $P(D_i = 1 \,|Z_i = 1) \neq P(D_i = 1 \,|Z_i = 0)$, (3) *monotoniciy*: either $D_{1i} \geq D_{0i}$ for all *i* or vice versa.

The independence condition ensures that (i) the instrument is "as good as randomly assigned", and that (ii) there is no direct effect of the instrument on the outcome. The first stage condition merely states that the instrument affects the probability of treatment, and, finally, the monotonicity assumption captures the notion that the instrument affects all individuals in the same way. Imbens and Angrist (1994) show that together these assumptions imply:

$$\frac{E(Y_i|Z_i = 1) - E(Y_i|Z_i = 0)}{E(D_i|Z_i = 1) - E(D_i|Z_i = 0)} = E(Y_{1i} - Y_{0i}|D_{1i} > D_{0i})$$

The left-hand side of this expression is the population analogue of the Wald estimator, and the right-hand side the Local Average Treatment Effect (LATE). In this setting, the LATE can be interpreted as the average effect of fertility on the labour market outcome variable, for those individuals that progress to higher parity because their first two children are of the same sex.

[8] The sample selection and the consequent implications for the identifying strategy are discussed in detail in the section 5.2.

2.2 Estimation

We relate the labour market response, y_i, to the treatment, indicated by D_i, by the following regression equation:

$$y_i = \alpha + \boldsymbol{x}_i'\beta + \delta D_i + \varepsilon_i, \qquad (1)$$

where $\boldsymbol{x}_i'$ is a vector of covariates including mother *i*:s age at first birth, a full set of dummy variables indicating mothers' birth year, and dummy variables indicating first- and second born boys. The latter two variables are included to control for potential additive effects of child gender which could arise if, for example, parents behave differently towards boys and girls (Angrist and Evans, 1998). Equation (1) will be estimated using both OLS and 2SLS where *same-sex* (indicated by Z_i) is used as an instrument for D_i.

Since our "treatment" is defined as giving birth to a third child in 1990, and our data allow us to follow all individuals until 2005, we can investigate how the effect of treatment evolves over time for the *same individuals*. Specifically, we estimate separate yearly regressions for the effect of having a third child on mothers' labour market earnings at child ages 0 (1990) to 15 (2005), using both OLS and 2SLS.

Because we are interested in how the effect of a third birth evolves over child age, we always want to compare mothers who have a third child with mothers who have only two children. For this reason, when estimating the regression equation (1), individuals in the "control group", i.e., individuals with $D_i = 0$, are only included in the estimations until they potentially have a third child, and are censored starting from the year that they potentially get a third child and onwards. The same censoring is applied to the "treatment group", i.e., to individuals with $D_i = 1$, if an individual ever gives birth to a fourth child.

So, by being able to measure our outcome variable for consecutive years over a 15-year horizon – from the year that they give birth up to 15 years after birth – we are able to investigate how persistent fertility effects are with respect to time since birth.

3 Data and summary statistics

3.1 Data sources and selection of sample

The analysis in this paper are based on Swedish population-wide administrative registers. First, the multigenerational register links all children to their biological parents, and provides us with information on birth year, birth order and gender of each of the individuals' children. To these data we have matched individual level background characteristics as well as annual labour earnings.

A necessary restriction we must make to the data due to the identification strategy is to include mothers with at least two children. More specifically, we restrict attention to mothers with exactly two children by the end of 1989. Furthermore, we make the additional restrictions that mothers are at most 45 years old in 1989, and that their second child was born between 1981 and 1988. This leaves us with, in total, 212,994 individuals. We follow individuals from 1990 through 2005.

The outcome variable studied in this article is *annual labour earnings*, which do not include parental leave benefits or transfers. Labour earnings are a good summary measure of labour market attachment since they reflect both hours of work and hourly wages.

In Table A1 in the Appendix, the data is described by presenting the means of a few demographic characteristics for our sample. From Table A1 we see that the average number of children these women had given birth to by the end of 2007 is about 2.3. Furthermore, about 7 percent of the sample gave birth to a third child in 1990, i.e., 7 percent of the sample consists of treated individuals. About 51 percent of the first- and second born children are boys; 23 percent of mothers had two first born girls, and about 26 percent had two first born boys. The mothers in our sample were on average 25 years old at the time of their first birth, and the majority of the women, 52 percent, had attained no more than high school education by the end of 1989. Finally, the average labour income in 1989 is about 107,000 SEK (expressed in 2008 years' prices).

3.2 Summary statistics by the value of the instrument

A key condition needed for consistency of the IV estimates is that the instrument is as good as randomly assigned. One simple way to check whether this assumption holds is to investigate whether mothers whose first two children are of the same sex and mothers whose first two children are of mixed sex differ with respect to demographic characteristics.

Table A2 in the Appendix presents the means of the same demographic characteristics presented in Table 1, as well as the average number of years between mothers' first two births, by the value of the instrument Z_i. The third column of Table A2 present the differences in means between mothers whose first two children are of mixed sex, i.e., those with $Z_i = 0$, and mothers whose first two children are of the same sex, i.e., those with $Z_i = 1$. The results in Table A2 indicate that there are no statistically significant differences in the above characteristics between the two groups, which is reassuring. There does seem to be a statistically significant difference in labour earnings in 1989 between the two groups of mothers, however this difference is very small: about 700 SEK.

Moreover, Table A2 shows that mothers whose first two children were of the same sex are more likely to be in the treatment group, i.e., to give birth to a third child in 1990, which indicates that a first stage does seem to exist. We also see that mothers whose first two children are of the same sex are more likely to have two boys than two girls.

In columns 3 and 4 of Table A2 we present the means of the same demographic characteristics, however, this time by *treatment status* i.e., by the value of D_i. The results show that mothers in the control group were slightly older when they gave birth to their first child, and have somewhat larger spacing between the first and second births. Furthermore, the mothers in the control group earned higher labour income in 1989 compared to the treatment group.

Another key identifying assumption for consistency of the IV estimates is that the instrument does not directly affect the outcome variable of interest, that is, that the instrument can be excluded from equation (1). One possible threat to the validity of the exclusion restriction of the *same-sex* instrument is discussed in Rosenzweig and Wolpin (2000). They study expenditure per children in rural India, and find that same-sex siblings are associated with significantly lower levels of expenditures due to hand-me-down savings for e.g. clothing. However, while this might be an issue in developing countries, in Sweden, expenditures on children’s clothing is not likely to stand for a large fraction of total family expenditures. For example, the Household Budget survey, reports the share of total consumption per household on clothes and shoes in 2007-2009 to range between 5.0 and 6.3, depending on the number of children and whether the household is a single or two-parent household (Statistics Sweden, 2010). Furthermore, in a recent paper, Huber (2012) performs a joint statistical test for the validity of the exclusion restriction and the monotonicity assumption. The test is performed for the same-sex instrument and violations are found to be small if not close to absent.

One potential concern is that, since the sample is somewhat different in each regression due to the censoring described in chapter 2, the instrument, although as good as randomly assigned at the time of the treatment - in year 0 - it may not be randomly assigned later on due to the censoring that we apply to our sample in the estimation. To examine this issue, we look at the differences in the means of the same characteristics presented in Tables A1 and A2, but this time in year 10. The results are given in Table A3 and suggest that there are no significant differences in characteristics between individuals with same-sex children and mixed-sex children in the final sample, apart from a small difference in the extent of college graduates of 0.003, and the average number of years between the first two births corresponding to about 0.6 months.

4 Results

4.1 Parental sex preferences and fertility

Before exploring the effects of fertility on mothers' labour earnings, we investigate the presence of mixed-sex preferences among the mothers in our sample in more detail. Parental preferences for a mixed-sex sibling composition have been documented in several industrialized countries. For the Nordic countries, Andersson et. al. (2006) find a distinct preference for at least one child of each sex among parents of two children. However, they do not find an effect of the sex of the firstborn child on second-birth probabilities. Furthermore, for Denmark, Norway and Sweden, they find that parents develop a preference for daughters in third births. In this section, we study the relevance of Z_i, which indicates same-sex children, as an instrument for D_i, which indicates giving birth to a third child in 1990, for our sample of Swedish mothers. The first stage regressions without covariates are presented in the first row of Table 1, for each year starting from the year that the third child is born until the year that the child turns 10 years old, separately. The first column of the top row in Table 1 therefore shows the first stage regression results on the complete sample, i.e., before any censoring is made, and suggests that having two children of the same sex increases the likelihood of having a third child in 1990 by about 0.14 percentage points. This estimate differs significantly from the ones provided in e.g. Angrist and Evans (1998) who find the effect of the same-sex instrument to be around 6-7 percentage points. However, the difference between our first stage estimate and that of Angrist and Evans (1998) and other papers that use the sex-mix strategy, is expected: earlier studies estimate the effect of same-sex children for a "pooled" sample of individuals who had their third child in several different years, while we estimate the effect of same-sex children on the likelihood of having a third child during a *a particular* year, namely in 1990. So our estimate should be lower.[9] [10]

Moreover, the censoring that we apply to the sample in our estimation does not lead to dramatic changes in the first stage estimate: in year 10, the estimated effect of having two children of the same sex on the likelihood of having progressed to higher parity in 1990 is 0.15 percentage points, only slightly higher than in year 0. The F-statistic for joint significance in the first stage equation ranges between 165 and 180, which is well above the rule of thumb value of 10 that is sometimes suggested. This means that the same-sex instrument does not seem to be a weak one.

Finally, the first row of the lower panel of Table 1 depicts the first stage regression results when we include controls for mothers' age at first birth, dummy variables indicating mothers' birth year, and dummy variables indicating first- and second born boys. Including covariates barely alters the first stage estimates.

[9] When using a cross-sectional data set from 1990 and the same-sex strategy, we get a first-stage estimate of about 5 percentage points. This is further discussed in a subsequent section of the paper.

[10] The first-stage estimate is slightly lower for mothers with two girls compared to mothers with two boys, which confirms that, for third births, Swedish parents have a slight preference for daughters as found in e.g. Andersson et. al. (2006).The results from this analysis are available upon request.

4.2 The effect of fertility on mothers' labour earnings over the life cycle

Since young children require 24 hour supervision, having an additional child increases the value of home time versus time in market work. As a response to the increased value of home time, mothers may reduce the number of hours worked, or fully withdraw from the labour market. Furthermore, since the amount of time required for child care differs depending on the age of the child, it is reasonable to assume that the labour market outcome responses to an additional child varies with time since birth. At the time of the study period covered in this paper, Swedish parents were entitled to 15 months of paid parental leave, and also the right to stay on full-time leave for the first 18 months after birth. Since mothers stand for the majority of parental leave take-up, we expect to find a nearly full withdrawal from the labour market in the short-run. Furthermore, Swedish parents are allowed to reduce their working hours with up to 25 percent until the child turns 8 years old, so there might also be a longer-run reduction of working hours.

In order to investigate such a potential time profile of labour earnings responses to fertility, we estimate equation (1) using both OLS and 2SLS on annual labour earnings over a 15-year horizon. Specifically, we estimate the effect of having a *third* child on labour earnings, year-by-year from the year that a mother gives birth to a third child, until the year that the child turns 15 years old.

Table 1 presents the first-stage results, and the results from estimating these separate regressions for the effect of a third birth on mothers' labour earnings, expressed in 1000s SEK (in 2008 prices) from the birth year of the child up to the year that the child turns 10 years old. Due to space limitations we only present the estimates until year 10 in the tables, however the results until year 15 are presented in Figures 1 and 2 below.

The upper panel of Table 1 depicts the first stage, OLS and 2SLS estimates from models without covariates, i.e., Wald estimates and simple treatment-control comparisons, while the lower panel shows estimates from models including controls for mothers' age at first birth, dummy variables for mothers' birth year, and indicators for first- and second born boys.

The OLS estimates from the model without covariates reveal that having a third child reduces mothers' labour earnings by, on average, 71,940 SEK in the year that the child is born. One year after birth, the effect is even larger, with a reduction in earnings by, on average, 90,700 SEK.[11] The estimated effects using OLS are statistically significant for all years that we measure the outcome variable, and thus persist as long as 10 years after child birth. Although the OLS results show a gradual catching-up effect in earnings, we do not observe a full recovery of earnings: by the year that the child turns 10 years old, the estimated effect of a third child on earnings shows a reduction of about 25,000 SEK on average. This is still a substantial effect. The OLS estimates from the models including covariates, presented in the lower panel of Table 1, are all somewhat smaller in magnitude compared to the OLS estimates without covariates, suggesting that some of the difference in labour earnings between the treatment and control group is due to differences in demographic characteristics such as age and age at first birth. To summarize, the OLS estimates suggest that the impact of a third child on mothers' labour earnings are largest in the first few years after giving birth, to then decrease sharply with earnings gradually catching up over a 15-year horizon (see Figure 1 for OLS estimates from year 0 to 15).

Turning to the IV strategy, the 2SLS estimates using *same*-sex as an instrument for D_i support this finding, but suggest an even faster recovery of earnings compared to the OLS estimates. The 2SLS estimates in the models without covariates (i.e. the Wald-estimates), depicted in the third row of Table 1, suggests that mothers' earnings decrease by, on average, 71,910 SEK in year 0, and in year 1 after birth, a reduction of 52,000 SEK, with the estimated effects being statistically significant on the 1 percent level. After year 1, the point estimates are much smaller in magnitude, even positive for years 3 to 10, but not statistically significant. However, the standard errors of the 2SLS estimates are very large. Including covariates does not seem to increase the precision of the 2SLS estimates, but the magnitude of the estimates changes slightly for years 0-2, and the point estimates for years 3-10 have now changed signs to be negative. Because of the large confidence intervals, it is difficult to draw any clear cut conclusions from the IV analysis, but the time profile of the fertility effect as suggested by the point

[11] The estimate in year 0 is likely lower compared to year 1 due to the fact that we are dealing with annual data: individuals who give birth late in the year have a longer time in the labour market compared to individuals who give birth early in the year.

estimates look similar to the one provided by OLS, and indicate that most of the earnings drop after giving birth is recovered even in the short run.

In Figures 1 and 2 we present the OLS and IV estimates (including covariates), respectively, for the whole 15-year horizon that our data allows us to study. From Figure 1 we see that the OLS estimates suggest a nearly full recovery 15 years after birth. Figure 2 also depicts a gradual catching-up effect where the point estimates are more or less centered around the zero line, although the estimates are not statistically different from zero after year 1.

Our findings that there seems to be a larger direct effect than longer-run effects are in line with previous studies that find that fertility effects are larger for women with young children (Jacobsen et al., 1999; Bronars and Grogger, 1994; Rondinelli and Zizza, 2010; Vere, 2011), and with a recent paper showing a gradual catching-up effect in earnings after giving birth in Sweden (Hirvonen, 2009).[12] An important thing to note here is that the IV strategy used in this paper only allows us to investigate the margin of giving birth to a third child. It is possible that the impact of childbearing is non-linear in the number of children, such that the allocation of time devoted to family and market work is decided upon in connection with the first birth, and simply maintained at higher parity. In that case, we would not find any impacts of third births over and above the time corresponding to formal parental leave, i.e., during the child's first two years of life.

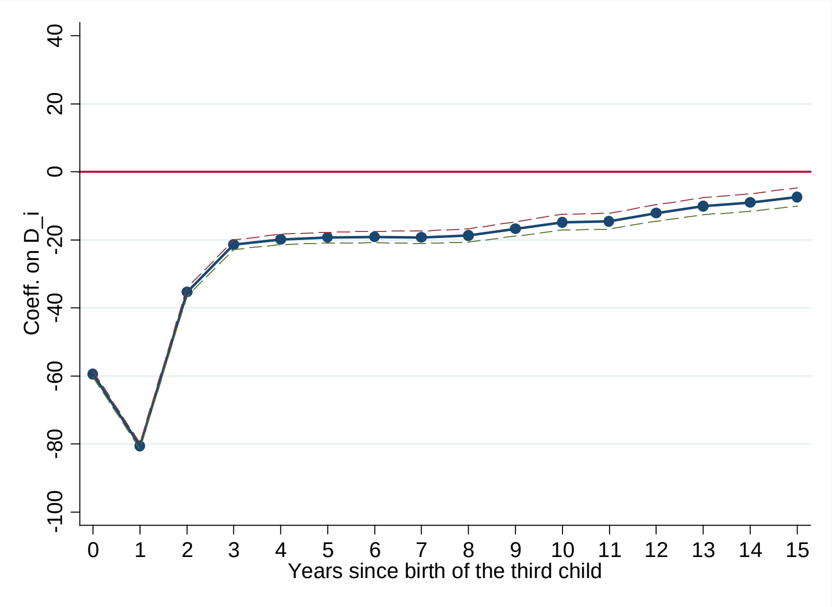
[12] Our approach differs from that of Hirvonen (2009) in that the present paper censors individuals in the control group as they get "treated" over time. Hirvonen (2009) instead moves these corresponding individuals to the treatment group, which means that each estimate is a weighted average of giving birth to a third child in that period and having given birth to a third child earlier.

Table 1: First-stage-, OLS- and 2SLS estimates of the impact of a third child on mothers’ labour earnings by years since birth

Years s. birth	0	1	2	3	4	5	6	7	8	9	10
No covariates											
1st stage											
Third child	0.014***	0.016***	0.016***	0.016***	0.016***	0.016***	0.016***	0.016***	0.016***	0.016***	0.016***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
OLS											
Labour income	-71.94***	-90.70***	-45.72***	-32.45***	-31.21***	-30.49***	-30.69***	-31.20***	-30.69***	-28.33***	-25.08***
	(0.45)	(0.46)	(0.66)	(0.72)	(0.79)	(0.81)	(0.88)	(0.95)	(1.02)	(1.09)	(1.20)
IV											
Labour income	-71.91***	-52.09**	2.29	8.05	22.82	39.84	31.68	27.40	47.39	33.35	17.38
	(23.32)	(20.98)	(23.84)	(24.54)	(27.58)	(28.04)	(30.14)	(32.01)	(34.58)	(36.13)	(38.17)
With covariates											
1st stage											
Third child	0.014***	0.016***	0.017***	0.017***	0.017***	0.017***	0.017***	0.017***	0.017***	0.017***	0.017***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
							b/se	b/se	b/se	b/se	b/se
OLS											
Labour income	-59.45***	-80.52***	-35.38***	-21.41***	-19.83***	-19.31***	-19.13***	-19.20***	-18.69***	-16.76***	-14.76***
	(0.45)	(0.46)	(0.65)	(0.71)	(0.78)	(0.79)	(0.86)	(0.93)	(1.00)	(1.07)	(1.19)
IV											
Labour income	-52.65**	-58.67***	-18.04	-18.87	-11.29	3.94	-5.80	-11.42	5.30	-7.34	-20.57
	(22.23)	(20.06)	(22.33)	(22.77)	(25.37)	(25.52)	(27.49)	(29.26)	(31.45)	(33.12)	(35.31)
Observations	212994	197844	186088	178256	172926	169389	167210	165733	164613	163766	163092

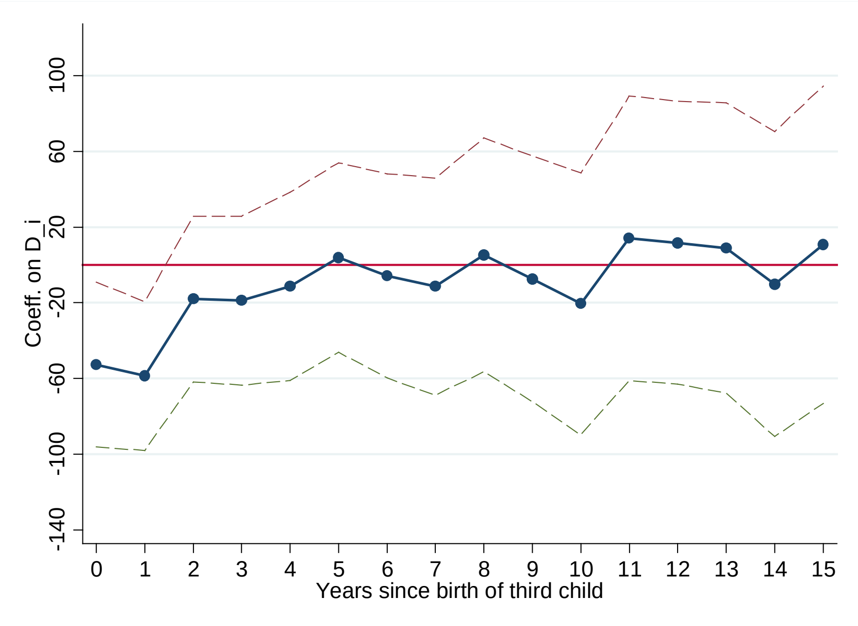
Note: Included covariates in panel (b) are a full set of dummies for mothers’ birth year, mothers’ age at first birth, and dummy variables indicating first- and second born boys. Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Labour income is reported in 1000’ SEK (2008 years prices).

Figure 1: OLS estimates of the impact of a third child on mothers' labour income



Note: the graph illustrates the OLS estimates depicted in panel (b) of Table 1. Included covariates are a full set of dummy variables for mothers' birth year, mothers age at first birth and indicators for first- and second-born boys. Labour income is expressed in 1000s SEK (2008 years prices).

Figure 2: IV estimates of the impact of a third child on mothers' labour income



Note: the graph illustrates the IV estimates depicted in panel (b) of Table 1. Included covariates are a full set of dummy variables for mothers' birth year, mothers age at first birth and indicators for first- and second-born boys. Labour income is expressed in 1000s SEK (2008 years prices).

5 Sensitivity analysis

5.1 Replication for different treatment cohorts

Our choice of fixing the year of treatment to 1990, and thereby the sample under study in this article, may seem an arbitrary one. One potential concern, therefore, is that our findings are specific to the cohort of mothers particularly under study. In this section, we wish to examine the sensitivity of our findings with respect to the year of treatment. In order to do so, we construct five additional data sets in the same manner described above, now for mothers who had exactly two children by the end of 1990, 1991, 1992, 1993 and 1994. We then let D_i be an indicator for mothers who gave birth to a third child in 1991, 1992, 1993, 1994 and 1995, respectively, and estimate the same model outlined in chapter 2 using 2SLS.[13]

The findings from this analysis are presented in Table 2, where the results from only the 2SLS estimations are depicted. The results show that the estimated effect of having a third child in child ages 0 and 1 is similar for all the samples. Furthermore, the estimates are statistically different from zero for only the first two years after birth, for all of the six samples. The qualitative pattern for the remaining (over and above the first two years) horizon differs somewhat between the samples, ranging from negative (but small) point estimates from in the early years to quite large positive estimates. Again, the 2SLS estimates does not provide a clear cut conclusion, but overall, they might indicate that the earnings of mothers recover quite fast after giving birth, even faster than suggested by the OLS estimates. If this is true, then not taking endogeneity into account does not only exaggerate the magnitude of the negative impact of giving birth to a third child on mothers' earnings, but it might also exaggerate the degree of persistence of this negative effect.

[13] To increase the precision of the estimates, one alternative would be to pool the separate samples together. Inference in this setting is unfortunately not an easy task. Pooling the separate samples together would imply non-overlapping treatment cohorts (i.e., women who get their third child in 1995, 1996, etc.), while each cohort's control group would be partly overlapping (i.e., women with two children in 1995, 1996, etc.). It is unclear how to perform inference in this setting because an individual can be in the control group as well as in the treatment group.

Table 2: 2SLS estimates of the impact of having a third child in 1990–1995 on mothers' labour earnings

	1990	1991	1992	1993	1994	1995
Years since birth						
0	-52.65**	-75.44***	-55.44**	-49.75**	-59.48**	-81.15***
1	-58.67***	-62.02***	-58.66***	-51.90**	-76.40***	-72.06**
2	-18.04	-23.56	-22.01	3.12	-25.28	-24.76
3	-18.87	-5.99	-4.00	16.45	-9.32	12.10
4	-11.29	-0.57	-8.68	11.42	27.51	19.53
5	-3.94	-4.74	-18.71	40.08	26.08	-0.49
6	-5.80	-16.50	9.41	37.04	19.22	-10.28
7	-11.42	20.66	2.44	45.13	22.65	-6.42
8	5.30	14.06	5.40	54.73	34.57	17.64
9	-7.34	-14.28	31.28	66.61*	43.07	-19.38
10	-20.57	12.77	20.89	65.04*	21.25	-16.89

Note: the table shows the 2SLS estimates of the impacts of having a third child in 1990,1991, 1992, 1993,1994 or 1995 for samples of mothers with exactly two children in the preceding year, by years since birth. Third births are instrumented by the same-sex indicator. Included covariates are a full set of dummies for mothers' birth year, mothers' age at first birth, and dummy variables indicating first- and second born boys. Labour income is expressed in 1000s SEK (2008 years prices).

5.2 Implications of the sample selection procedure – potential bias

In this section, we explore if and how our sample selection procedure might affect our estimates and the interpretation of our results. Specifically, in our main analysis, we are studying the population of mothers with exactly two children by the end of 1989, and whose second child was born between 1981 and 1988. This means that some mothers in our population have had several years to react to the *same-sex* instrument (at most 7 years), but have not. Those mothers who might have reacted to the instrument and thereby given birth to a third child do not belong to our population. The value of the instrument, which is observed by mothers at the time of the second birth) will likely affect which mothers are sampled to belong in our population.

To see how this sampling procedure might affect our results, and consequently the interpretation of our findings, we investigate this in the following way. For all mothers who gave birth to their second child in 1985, we explore the randomness of the instrument with respect to demographic characteristics. Secondly, we look at the same demographic characteristics by the value of the instrument again, this time five years later (i.e. in 1990), both for the full sample of mothers who gave birth to their second child in 1985, and for a censored sample where mothers who between 1986 and 1990 have given birth to a third child are dropped from the studied sample (this corresponds to how we select our population of interest). Finally, we do the same thing for the year 1995, i.e., we investigate the differences in mean characteristics between same- and mixed-sex mothers for the full sample, and for the censored sample, respectively.

The results from this exercise are presented in Table A4 in the Appendix, and show that, for the full sample, the instrument is as good as randomly assigned with respect to observable characteristics in 1985. In 1990, this still holds true for the full sample, and for the censored sample (where we keep only mothers who still have not progressed to higher parity until 1990), there is only a very small difference in the extent of college education, significant on the 5 percent level, suggesting that the mixed-sex mothers that are left after censoring are slightly higher educated compared to the same-sex mothers left after censoring. For the censored sample in 1995, there are a few differences in the mean characteristics between same- and mixed-sex mothers. Specifically, the mixed-sex mothers left in the data are somewhat younger compared to the same-sex mothers left in the data, they are slightly higher educated, and they are to a slightly higher extent married compared to the same-sex mothers left in the data. This means that the sampling procedure implies that the mixed-sex mothers that are left probably have a stronger preference for work. This in turn means that, if our estimates are biased, they should be biased in the direction of finding a negative effect of childbearing on the labour earnings. Therefore, given that we find no statistically significant effects in the medium- or long-run, it's not likely that we would have found larger negative effect in absence of the sampling procedure. In fact, we should have found even smaller negative effects.

Furthermore, as shown in Figure A2, the share of mothers with same-sex children decreases as we move along time and censor individuals as they go on to have a third child.

Finally, to see whether this pattern holds up also in the censoring applied after the initial selection of the population under study, that is, the censoring that is applied in the estimations as individuals go on to have a third child (in the control group) or a fourth child (in the treatment group) we provide the following analysis. For the population of mothers with exactly two children by the end of 1994[14], and where treatment is defined as giving birth to a third child in 1995, we look at how mean earnings evolve for the control and treatment group, respectively, from 1985 to 2005, both for the full sample and for the samples where we apply the censoring (i.e., the sample on which the estimations are based). The results from this analysis is provided in Figure A3 and confirms the hypothesis that the mothers that are kept in the analysis are mothers who likely have stronger preferences for work.

6 Analysis based on a one-period cross-section

Our main findings so far suggest that there is significant heterogeneity in the causal impact of childbearing over the life cycle. Specifically, our results indicate that there is only a very short run effect of fertility at higher parity on women's labour earnings. One potential concern, however, is that our findings are specific to the Swedish setting rather than to how we treat fertility: whether we consider treatment to be “constant” over the life cycle, or to have varying impacts over time. In this section we therefore explore how using cross-sectional data covering one year, based on the same data sources described in Section 3, compares with other papers using the sex-mix instrument as well as with our main results.

Now, we let D_i indicate whether individual i has more than two children, in a sample of women with at least two children in 1990. We then estimate equation (1) using both OLS and 2SLS on this cross-section of mothers.

Table 3 presents the results from these estimations, along with the first stage regression results. The results suggest that having two children of the same sex increases the likelihood of having three children by about 5 percentage points, which can be compared to a first stage effect of about 6 percentage points found by Angrist and Evans (1998) in the US. Furthermore, the OLS estimate for earnings suggests that having a third child decreases labour earnings by 29,574 SEK, on average, in the model

[14] The reason for choosing this sample here is a data limitation one: for these mothers we observe the earnings from the year that they give birth to their second child and onwards.

without covariates. The corresponding IV estimate is somewhat lower: 21,562 SEK. Including covariates increases the OLS estimate to 33,147 SEK, but the IV estimate is virtually unchanged at 21,720 SEK. This corresponds to a reduction in earnings of about 17 percent, which is lower in magnitude compared to the US case studied by Angrist and Evans (1998) who found that having a third child causes a 20-30 percent reduction in women's labour supply and earnings.

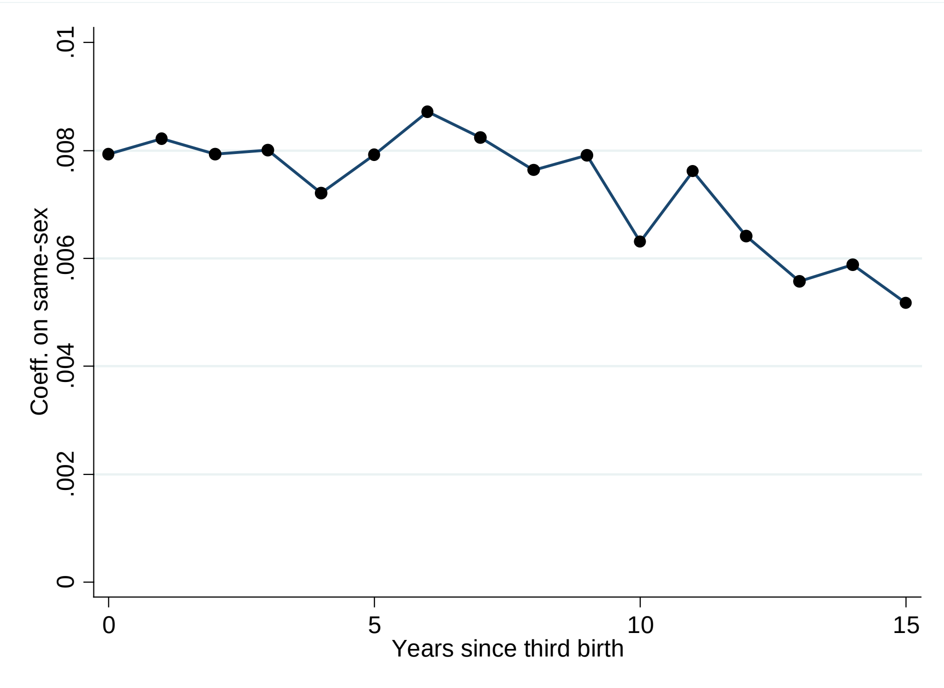
One way to interpret the IV estimates based on this cross-sectional data is that they are mainly driven by mothers with young children. In order to investigate this we follow the previous literature and divide the sample into subgroups of mothers defined by the time since they had their third birth. We then estimate the impact of childbearing separately for the different subgroups. The first stage estimates for these subgroups are presented in Figure 3 below and show that the first stage effect is stronger for mothers with less time elapsed since their third birth. Furthermore, we explore the causal effect of third children on mothers' labour income for these different subgroups using *same* sex as an instrument for third births. These estimates are depicted graphically in Figure 4. Interestingly, the point estimates showing the time profile of the effect of childbearing are very similar to the corresponding estimates based on the panel data set. However, the estimates are not significantly different from zero for any year; the sample for each subgroup is very small. This 'synthetic' life cycle effect seems thus to a large extent able to reproduce the findings based on the panel data set.

Table 3: First-stage, OLS- and 2SLS estimates of the effect of having a third child on mothers' labour earnings using a one-period cross-section

Dependent variable	OLS	IV
No covariates		
Third child	0.053*** (0.001)	
Outcome variable		
Labour earnings	-29.574*** (0.215)	-21.562*** (3.938)
With covariates		
Third child	0.053*** (0.001)	
Outcome variables		
Labour earnings	-33.147*** (0.208)	-21.720*** (3.673)
Observations	680931	680931

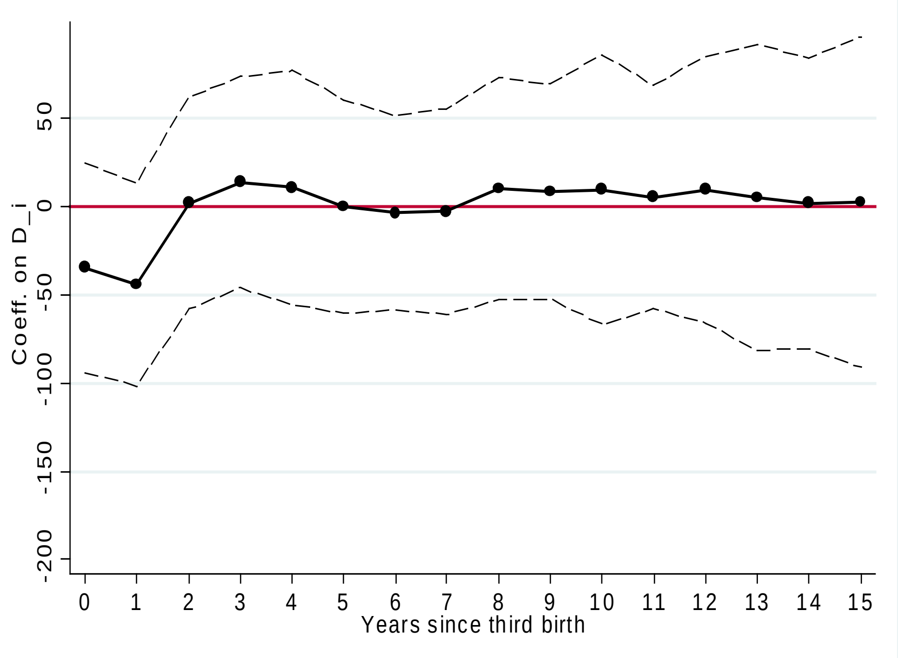
Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Included covariates in panel (b) are a full set of dummies for mothers' birth year, mothers' age at first birth, and dummy variables indicating first- and second born boys. Labour income is expressed in 1000s SEK.

Figure 3 First-stage estimates for subgroups of mothers defined by years since birth



Note: the graph plots the estimated coefficients on the same-sex instrument in linear probability regression of third births on same sex children for each subgroup. Included covariates are a full set of dummies for mothers' birth year, mothers' age at first birth, and dummy variables indicating first- and second-born boys.

Figure 4 2SLS estimates of the impact of a third child for subgroups of mothers defined by years since birth



Note: The graph plots the estimated coefficients on the treatment variable for each subgroup, along with the corresponding 95 percent confidence intervals, where treatment is instrumented by the same-sex indicator. Included covariates are a full set of dummies for mothers' birth year, mothers' age at first birth, and dummy variables indicating first- and second born boys.

7 Concluding remarks

The purpose of this paper is primarily to explore the time profile of the effect of childbearing on female labour earnings. Although there is by now a large literature on the causal impacts of fertility on female labour supply and earnings, the evidence on the dynamics of such effects is more lacking. Understanding the dynamics of the fertility effect on women's labour market behaviour is crucial to a number of policy debates concerning the role of family policy to help parents reconcile market work and family life.

Although scarce, the existing evidence on the life cycle effect of childbearing suggest that fertility only affects women's labour market outcomes in the short run. One possible limitation to the approaches used in the previous literature, however, might be that life cycle effects are often estimated by means of disaggregating the sample by age groups or time since birth using one-period cross-sections, which may be associated with confounding of age and cohort effects. We contribute to this literature by following the same individuals over time, on a yearly basis from the year of birth up to 15 years later. To account for endogeneity, we use parents' preferences for a mixed-sex sibling composition as a source of exogenous variation in family size; a method originally applied by Angrist and Evans (1998) to examine the impacts of fertility on Swedish mothers' labour earnings. Moreover, we fix the year of treatment, defined as giving birth to a third child, to occur in one particular year (1990) and estimate the impact of having this third child for varying years since treatment over a 15-year horizon.

Our OLS estimates suggest that having a third child (in 1990) reduces mothers' labour earnings considerably during the first two years after birth, with the impact diminishing rapidly after the first two years, along with a gradual catching-up effect in earnings such that they almost recover fully 15 years after birth. Our IV estimates supports this story, and taken at face value suggest an even faster recovery of earnings compared to the OLS analysis. The IV analysis suggests that having a third child (in 1990) reduces' mothers labour earnings only during the first two years after giving birth, but there are no statistically significant effects after that. This is consistent with withdrawing from the labour market in favour of paid parental leave during the first couple of years after birth, and then returning to work.[15] However, it should be noted that the IV estimates are imprecisely estimated, such that it is difficult to draw any clear cut conclusions. Nevertheless, the time profile of the fertility impact as suggested by the point estimates indicates that most of the impact takes place in the years immediately following birth, to then “bounce back”.

Furthermore, replicating this analysis for five additional treatment years, 1991-1995, we find the exact same pattern, which means that our main findings are not specific to the sample under study.

We also provide an analysis based on a cross-sectional data set covering one year (1990) and estimate the causal impact of fertility in the spirit of Angrist and Evans (1998). Our findings there are comparable to the findings based on US data, which assures us that the results from our “dynamic” approach are not specific to the Swedish

[15] Swedish parents were at the time entitled to 15 months of job protected paid parental leave. In addition, parents were entitled to reduce their working hours with up to 25 percent until the child turned 8 years old.

case, but more likely to the method being used. Moreover, to be able to more closely investigate how this estimate can be interpreted, we explore whether mothers of third children of different ages contribute differently to this estimated causal effect. We do this by estimating the labour earnings response to a third child separately for subgroups of mothers defined by years elapsed since third birth. We find that the time profile of the fertility effect using the cross-section, given by 2SLS estimates for the subgroups of mothers with different years elapsed since third birth, is very similar to the time profile obtained using the panel data set. However, due to very small samples for each subgroup, the estimates are not statistically significant for any year. This analysis gives us two insights. First, “synthetic” life-cycle patterns obtained by subgroups of mothers defined by the time since birth seem to provide a good approximation of the life-cycle impact of fertility obtained when following the same individuals over time. Second, the IV estimate of the causal effect of childbearing on mothers' labour earnings based on a single cross-section, including mothers with different years since treatment, seem to be largely driven by mothers with young children.

Taken together, the results from all our analyses suggest that there is only a very short run effect of childbearing on mothers' labour market earnings: having a third child does not seem to affect the labour market situation of mothers. This is quite interesting considering the discussion about how time off for child care may have adverse effects for women's labour market careers. One potential explanation to our findings might be the institutional setting: Sweden has a very generous parental leave system with wage replacement, conditional employment before giving birth, and with job protection. This system creates incentives for women to be attached to the labour market before they have children, and makes sure that they are able to return to work after parental leave through job protection. Another possible explanation to the absent labour market effects is the fact that we are studying the impact of moving from two to three children. We cannot rule out that the effect of childbearing is non-linear in the number of children. For example, if mothers optimize their allocation of time to market work and family already in the connection to the first child, they may simply maintain this division of time at higher parity, which means that, over and above the mechanical effect when the child is newborn and when formal parental leave is available, there are no further reductions in the labour supply. Finally, our outcome measure - annual labour earnings - reflects both hours of work and hourly wages. Thus, employers' expectations might also play a role: if employers consider all female employees of childbearing ages to be under the same "risk" of further childbearing, then there would be no difference in the wage trajectory between women with fewer and more children. All these possible explanation should provide interesting avenues for future research.

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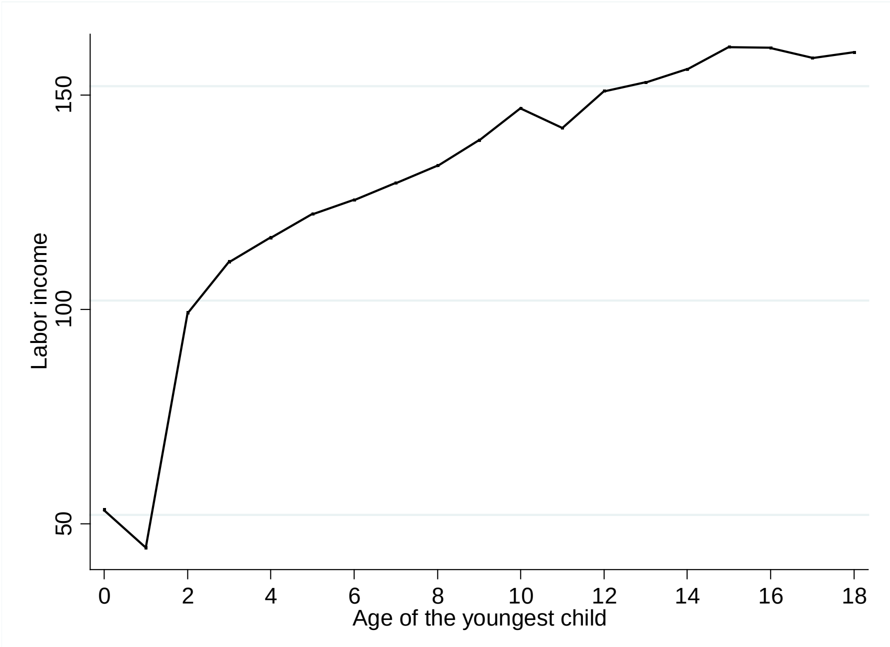
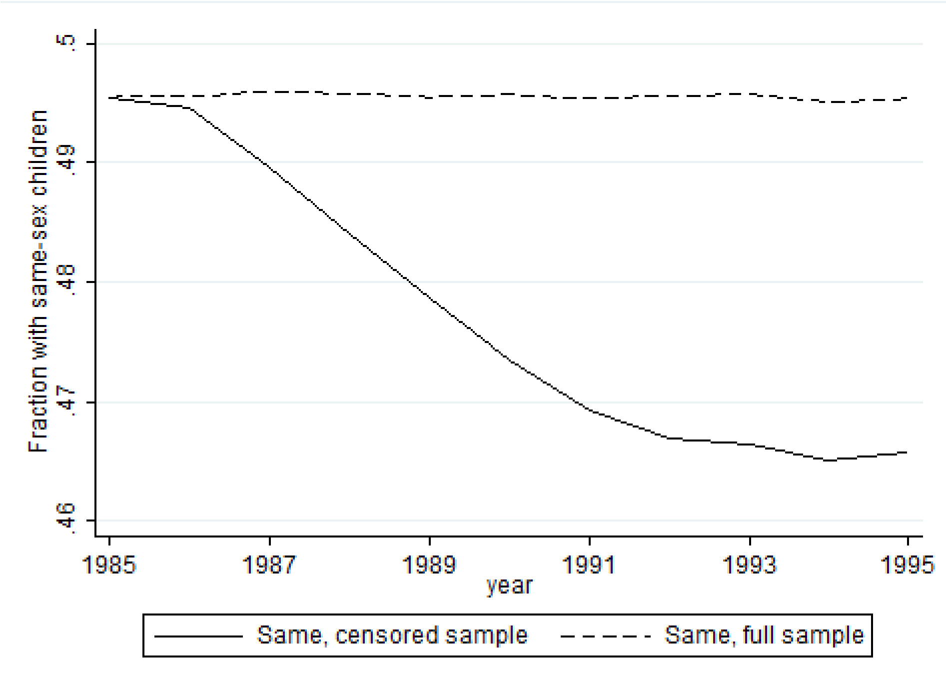
Appendix

Figure A1 Average labour income of mothers in 1995 by the age of the youngest child



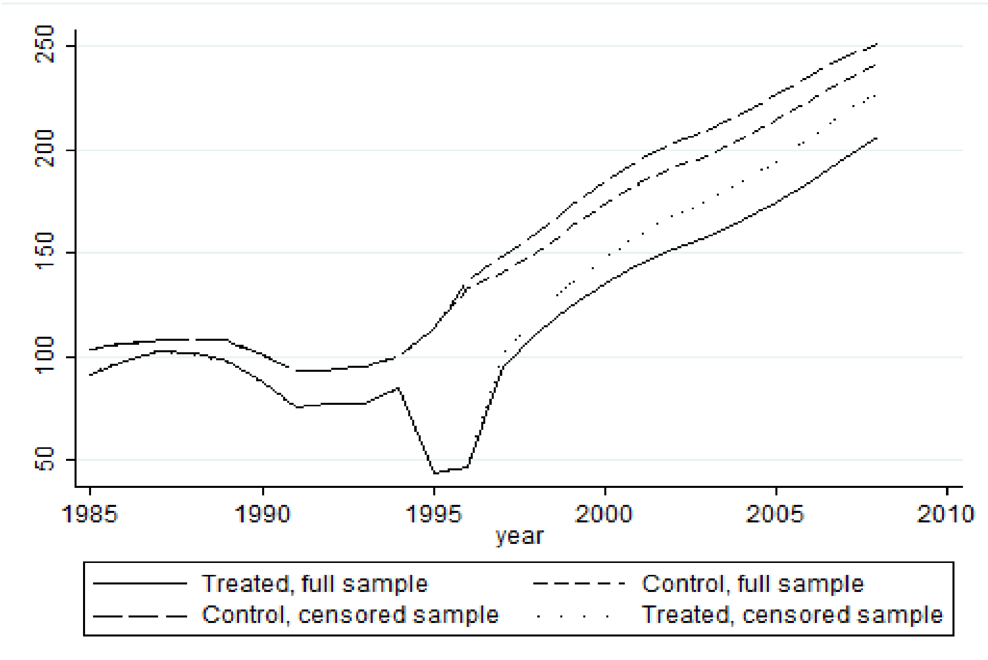
Note: the graph A1 plots the average labour earnings in 1000s SEK (expressed in 2008 years' prices) of mothers in a 1995 cross section, by the age of their youngest child.

Figur A2 The fraction of same-sex children for the full and censored samples of mothers who gave birth to their second child in 1985



Note: the graph A2 plots the share of mothers with same-sex children among mothers who gave birth to their second child in 1985, and for a sample in which mothers are kept in the data until they give birth to a third child .

Figur A3 Labour earnings for the full and censored samples of mothers with two children in 1989



Note: the graph A3 plots the average labour earnings of mothers who had two children by the end of 1994 and gave birth to a third child in 1995 (treated), and did not give birth to a third child in 1995 (control). The censored samples refer to when the case where we drop observations from the year that they give birth to a third child (control group) or a fourth child (treatment group) and onwards.

Table A1 Sample statistics for mothers with two children in 1989

	Mean (std dev.)
Number of children 2007	2.37 (0.67)
Third child 1990 (treated)	0.07 (0.26)
Boy 1st	0.51 (0.50)
Boy 2nd	0.51 (0.50)
Two boys	0.26 (0.44)
Two girls	0.23 (0.42)
Age at first birth	25.08 (3.88)
Elementary schooling	0.21 (0.41)
High school	0.52 (0.50)
College education	0.27 (0.44)
Non-Nordic background	0.04 (0.18)
Labour income 1989	107.38 (75.96)

Note: the table shows means and standard deviations (in parentheses) for demographic variables for mothers with two children in 1989.

Table A2 Demographic variables by the value of the same-sex instrument and by treatment status.

	(1)	(2)	(1)-(2)	(3)	(4)	(3)-(4)
	Mixed	Same	Difference	Control group	Treatment group	Difference
Number of children 2007	2.349	2.401	-0.052*** (0.003)	2.296	3.381	-1.084*** (0.005)
Third child 1990 (treated)	0.065	0.080	-0.015*** (0.001)	0	1	-
Boy 1st	0.500	0.528	-0.028*** (0.002)	0.513	0.519	-0.006 (0,004)
Boy 2nd	0.500	0.528	-0.028*** (0.002)	0.513	0.524	-0.011*** (0.004)
Two boys	0	0.528	-0.528*** (0.002)	0.255	0.291	-0.036*** (0.004)
Two girls	0	0.422	-0.472*** (0.002)	0.229	0.247	-0.018*** (0.004)
Age at first birth	25.05	25.11	-0.056*** (0.017)	25.160	24.010	1.150*** (0.032)
Elementary schooling	0.210	0.208	0.002 (0.002)	0.208	0.226	-0.019*** (0.003)
High school	0.523	0.524	-0.001 (0.002)	0.524	0.512	0.013** (0.004)
College education	0.267	0.268	-0.001 (0.002)	0.268	0.262	0.006 (0.004)
Non-Nordic background	0.035	0.035	-0.000 (0.001)	0.035	0.039	-0.004* (0.002)
Years between 1st and 2nd birth	3.839	3.851	-0.012 (0.011)	3.886	3.316	0.570*** (0.022)
Labour income 1989	107.7	107.0	0.731* (0.329)	108.200	96.280	11.960*** (0.635)

Note: the table shows the means of demographic characteristics by same-sex, i.e., separately for individuals whose first two children were of mixed sex and same sex, and separately for treatment status, i.e., separately for individuals who did not give birth to a third child in 1990 (control) and individuals who gave birth to a third child in 1990 (treated). * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3 Demographic statistics by the value of the same-sex instrument in year 10

	Mixed (1)	Same (2)	Difference (1-2)
Number of children in 2007	2.071	2.088	-0.0163*** (0.001)
Third child 1990 (treated)	0.061	0.078	-0.016*** (0.001)
Boy 1st	0.501	0.525	-0.024*** (0.002)
Boy 2nd	0.499	0.525	-0.027*** (0.002)
Two boys	0	0.525	-0.525*** (0.002)
Two girls	0	0.475	-0.475*** (0.002)
Age at first birth	25.43	25.52	-0.086*** (0.019)
Elementary schooling	0.200	0.200	-0.000 (0.002)
High school	0.522	0.519	0.003 (0.002)
College education	0.279	0.282	-0.003 (0.002)
Non-Nordic background	0.034	0.034	0.000 (0.001)
Years between 1st and 2nd birth	4.067	4.125	-0.057*** (0.014)
Labour income1989	114.900	115.500	-0.566 (0.380)

Note: the table shows the means of demographic characteristics by same-sex, i.e., separately for individuals whose first two children were of mixed sex and same sex, now measured in year 10, when the censoring has been finalized. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A4: Demographic variables by the value of the same-sex instrument for mothers who gave birth to their 2nd child in 1985, for a full and censored sample, respectively

	Mixed	Same	Diff	Mixed	Same	Diff
1985		*Full*			*Censored*	
Age	27.69	27.71	-0.0227	.	.	.
Age at first birth	24.14	24.15	-0.0189	.	.	.
High school	0.526	0.530	-0.00351	.	.	.
Some college	0.160	0.154	0.00640	.	.	.
College grad	0.0739	0.0777	-0.00386	.	.	.
Non-Nordic born	0.0230	0.0238	-0.000771	.	.	.
1990						
Age	32.51	32.54	-0.0231	32.94	33.02	-0.0866
Age at first birth	23.94	23.95	-0.00777	24.22	24.06	-0.0451
High school	0.540	0.542	-0.00267	0.555	0.558	-0.00229
Some college	0.167	0.160	0.00692	0.171	0.159	0.0122*
College grad	0.0811	0.0853	-0.00416	0.0767	0.0832	-0.00644
Non-Nordic born	0.0717	0.0763	-0.00469	0.0705	0.0754	-0.00490
Married	0.761	0.765	-0.00379	0.737	0.729	0.00796
Single	0.181	0.179	0.00162	0.194	0.200	-0.00562
Divorced	0.0558	0.0531	0.00264	0.0659	0.0673	-0.00142
1995						
Age	37.44	37.46	-0.0192	38.23	38.34	-0.109*
Age at first birth	23.88	23.89	-0.00330	24.39	24.43	-0.0342
High school	0.531	0.533	-0.00265	0.547	0.549	-0.00276
Some college	0.182	0.172	0.00973*	0.192	0.177	0.0147*
College grad	0.0949	0.0995	-0.00464	0.0930	0.102	-0.00930
Non-Nordic born	0.0988	0.104	-0.00512	0.0917	0.101	-0.00981*
Married	0.726	0.728	-0.00158	0.710	0.693	0.0167*
Single	0.149	0.152	-0.00258	0.158	0.169	-0.0111
Divorced	0.119	0.114	0.00522	0.126	0.130	-0.00312

Note: the censored sample includes mothers who have not yet given birth to a third child in the year that their characteristics are observed (1990 and 1995). Those censored are thus mothers who have given birth to a third child between 1985 and 1990; or 1995, respectively. Marital status is not observed before 1990. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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