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The effect of children on male earnings and inequality

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Abstract
This study investigates the effect of children on male earnings and how earnings inequality among men arises over the life cycle. We use panel register data on earnings and fertility for sibling brothers and twins, and present estimates from flexible earnings regressions. We find that OLS estimates are confounded by selection effects through family fixed factors. The comparison of twin brothers shows that overall earnings growth does not vary between those who ever become fathers and those remaining childless, and there is no significant effect of children on earnings. We also show that controls for marriage explain only a relatively small part of the effect of children. Men who remain childless and unmarried are on relatively low earnings profiles and therefore contribute significantly to the earnings inequality among men.

Keywords Children · Marriage · Earnings · Men

1 Introduction

Statistics show that unadjusted male-female earnings differentials still remain significant, between 15 and 23%, and have remained surprisingly stable in many countries over recent decades (OECD 2017). In order to understand remaining gender wage gaps, one of the important questions is whether having children affects earnings. A large body of empirical evidence exists on women showing a negative association between earnings and children, and suggesting that women are falling behind because of children (Adda et al. 2017; Bertrand et al. 2010; Ejrnæs and Kunze 2013; Anderson et al. 2002; Joshi et al. 1999; Waldfogel 1998; Datta Gupta and Smith 2002). Existing studies on men suggest that the presence of children is positively associated with men's earnings. However, evidence on more direct explanations of this finding is still scarce. There exists no consensus on the reasons

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for the "fatherhood wage premium" and interpretations diverge on whether this reflects gendered household specialization, positive discrimination or something else. Alternatively, factors that positively affect the likelihood to become a father may also be positively related to earnings, suggesting that fathers may be a (positively) selected group. Evidence so far however suggests negative selection which is not consistent with a fatherhood premium (Lundberg and Rose 2000, 2002).[1]

Part of the controversy regarding the fatherhood premium is that it is complicated to interpret correlations of children and earnings as a (direct) effect of children. This is because parenthood might be endogenous with respect to earnings and correlated with unobserved factors. The most common approach in the literature has been to estimate the mean effect of the arrival of children from Mincerian earnings equations exploiting cross-sectional variation, or the comparison of individual outcomes post versus pre-birth (fixed effects estimation). Instrumental variables have been used to estimate the earnings effect of the increase in number of children from two to three (Angrist and Evans 1998), teenage pregnancy (Hotz et al. 2005), and delayed motherhood (Miller 2011). A caveat of these estimates is that effects at particular parity may not be generalizable to other parities. Nor does it allow us to test for non-linearity in the effect of children.

The goal of this study is to estimate the effect of children on male earnings and earnings inequality over the male life cycle. We exploit data on brothers and twins, in order to explore another channel of non-random selection on unobserved family fixed factors. Such unobserved family fixed factors may capture several characteristics that are common to brothers such as neighborhood and socioeconomic status, or parental investment into cognitive development during childhood.[2] Our data offer several advantages compared to the literature to investigate this channel. First, we use panel register data on earnings and fertility for several cohorts of men which is rarely available to researchers and facilitate a clean study of the male earnings effects of children.[3] Second, we observe individual earnings trajectories during the period 1975 to 2005, and link male siblings and twin brothers. Since we have population data this results in large samples.[4] Third, our study does not suffer from attenuation bias problems arising from measurement error in survey data (Bound and Solon 1999) on earnings and fertility. Fourth, we use data on Norway that allow us to investigate whether family background affects outcomes in a country with a high degree of equality.[5] We use these data to estimate flexible Mincerian earnings regressions that control for family fixed effects.

[1] Ludwig and Brüderl (2018) have found positive selection into marriage as a driver of the marriage premium.

[2] In addition, siblings and twins are genetically more similar which may make them also in terms of ability more similar.

[3] For a detailed discussion, see Field et al. (2016).

[4] Studies on the marriage premium such as Antonovics and Town (2004) and Krashinsky (2004) have relied on much smaller samples.

[5] One my hypothesize that in a country with high income equality, free education and low gender differences in the labor market, the expected raw fatherhood premium is small but also that there is relatively little role for family background as determinants of labor market outcomes. For example, the literature on intergenerational income mobility suggest high mobility coefficients in the Scandinavian countries compared to the United States (Bratberg et al. 2017).

We find that OLS estimates which use the cross-sectional comparison of men who have a child and men who remain childless is plagued by selection effects into fatherhood and overestimates the (positive) effect of having children on male earnings, which has been found in existing studies. We show that the earnings profiles of men who have a child over the lifetime and childless men differ already before the arrival of the first child. Thus, not accounting for these differences in trends makes (all) childless men not a suitable control group. Our estimation approach accounts for these differences in a flexible way, and in addition through family fixed effects. Our coefficient estimations exploit the variation in differences across siblings within the same family throughout the lifecycle, and essentially exploit the variation from sibling couples where one brother has children and the other does not. When we compare earnings profiles of twin brothers we find that they are on equal earnings growth paths and there is no significant effect of children on earnings. Robustness tests show that the general results still hold when we change the control group to childless men who will eventually get married. We also show that the post-birth effect only to a minor extent captures a marriage effect over and above the arrival of a child effect. Controls for being married explain only a relatively small part of the effect of the arrival of a child, 28%. We also show that the shift in earnings post births is entirely driven by the event of a first child. Regarding inequality in earnings among men, this result highlights that it is not the fact of having children that is driving inequality over the life cycle, but selection into the group of childless and never married men, versus fathers or fathers-to-be. These findings for Norway, a country with a high degree of income equality may suggest further interest in investigating variation across countries.

Our results on men and children add to the policy debate on family-work balance, which is no longer limited to women and work. Politicians in some countries set incentives for fathers to stay home through paternity leave policies. As survey data shows, men have increased the weight they place on family values (see e.g. Goldin 2006) and family time. A new and growing academic literature sets the focus on understanding various aspects of the interaction between fathers and children such as effects on peer behavior (Dahl et al. 2014) and child outcomes (Cools et al. 2015).[6] The group of men and boys has also gained interest since it seems that more recently it is men who are falling behind (see Autor and Wasserman 2013). We also add to recent more descriptive studies on Scandinavia comparing relative earnings trajectories of fathers and mothers, and couples, around the event of childbirth (Angelov et al. 2016; Kleven et al. 2018). Our extensions are that we focus on sibling brothers that are followed over time in the labor market, and that we broaden the view to inequality over the lifecycle among men.

The remainder of the paper is organized as follows. Section 2 provides an overview of the economic explanations as to why having children affects male earnings. Section 3 presents the data and summary statistics. Section 4 derives the earnings equation. Section 5 presents the empirical results, followed by a number of robustness tests and the discussion. Section 6 concludes.

[6] Angelov et al. (2016) show the earnings profile of Swedish men with children, before and after first childbirth. However, they do not estimate the effect of children per se, nor do they take account of non-random selection.

2 Men's earnings and having children

Given that husbands' and wives' labor market outcomes are interdependent, we would expect the reallocation of mothers' time and effort after childbirth from market to home to be accompanied by some labor market response among fathers. Hence, two explanations would motivate a causal effect of children on male earnings. If mothers specialize more in home production then this can lead to an increased specialization of fathers in market production (Becker 1985). This is particularly likely if mothers also take over other household activities previously conducted by the father in order to gain economies of scale. The positive earnings effect of fathers can then be driven by increased effort, or by accumulation of human capital over time. For the U.S., for example, studies have shown that part of the child premium is related to increased hours of work (Lundberg and Rose 2002; Pencavel 1986). An earnings increase can also be caused by preferential treatment by employers of fathers, or positive discrimination. Employers may view having children for men as a signal of more conservative values, reliability and higher productivity and are therefore willing to pay a premium (Correll et al. 2007).

Another potential explanation is that earnings advantages of fathers compared to childless men may capture decisions made earlier in life related to the plan to become a father, or in other words that the group of those who become fathers is a selected group. This explanation suggests that the correlation between children and earnings is due to omitted variable bias. If men expect to make gains in the labor market after child birth, then it may be optimal for them to invest more into their career even before they become fathers. This may, for example, lead to self-selection into higher-track occupations (Gould 2008).

A related, but different, question is whether cohabitation or marriage explains the relatively higher earnings growth of fathers-at-some-point, even before childbirth (Peters and Siow 2002). It is related because in many countries marriage and childbirth occur in close proximity and hence the effects are difficult to distinguish. However, studies of the marriage premium for men provide little insight into the effect of having children; either the effect of having children is not separately reported (Korenman and Neumark 1991; Gray 1997), or is reported to be insignificant (Loh 1996).

One hypothesis is that marriage itself leads to gender-specific household specialization, whereby men specialize more in market work and women in home production. An alternative hypothesis is that men with relatively high productivity-related skills are more likely to marry. A large group of international studies has shown that married men earn between 10 and 40 per cent more than comparable single men (Korenman and Neumark 1991; Ginther et al. 2001). However, the precise nature of the effects remains unclear. Time-use data offers little support for the specialization hypothesis (Hersch and Stratton 2000). Time-use data suggest that gender-specific household specialization is not related to cohabitation or marriage, but rather to the presence of children and particularly to spending more time on child care (Dribe and Stanfors 2009). In a recent study, Ludwig and Brüderl (2018) show that selection is driving the marriage premium.

Evidence on the effect of having children on men's earnings, and on earnings inequality among men, is scarce in the economics literature but there is wider body of literature in sociology. Studies rely either on cross-sectional data and ordinary least squares estimation, or panel data and individual fixed effects estimates. Panel data studies exploit the before-after comparison and, hence, account for individual heterogeneity conditional on a sample of fathers-at-some-point. These studies find estimates of the effect of children on male earnings in the range between 3 and 10% per year, which varies somewhat depending on the country and model specification (see Lundberg and Rose 2000, 2002; Pencavel 1986; Waldfogel 1998; Killewald 2012; Glauber 2008; Hodges and Budig 2010 for the U.S., Blomquist and Hansson-Brusewitz 1990 for Sweden, and van Soest et al. 1990 for the Netherlands).[7] Datta Gupta et al. (2007) reported fixed-effects estimates of the effect of children ranging between 0.3 and 1.2% depending on age for Denmark. Simonsen and Skipper (2010) exploit Danish data on a cross-sectional sample of twins in 2006 and report a significant wage premium for men. They can compare fathers only to the group of never and not yet fathers, which induces likely a downward bias. For Norway, Petersen et al. (2014) estimate a 1% wage premium per child, controlling for occupation fixed effects in a sample restricted to white collar workers in the private sector. Only a limited number of studies have looked at both the effect of having children and the effect of marriage (Hodges and Budig 2010; Hundley 2000; Loughran and Zissimopoulos 2009; Lundberg and Rose 2002; Petersen et al. 2011, 2014).

2.1 The fatherhood premium and policy context

It has been a long-standing policy goal in Norway to achieve high gender equality and help families to combine work and having children. The main policies to achieve these goals have been anti-discrimination laws introduced during the 1970s, parental leave and child care. Parental leave was first introduced in the 1970s, and a major reform took place in 1993 when leave was extended to 42 weeks at full compensation but capped, while four weeks were reserved to the father (paternity leave). Prior to 1993, not more than 3% of fathers took leave, but almost 80% of mothers took the maximum amount.[8] Since 1993, the proportion of fathers taking up leave has steadily increased from an initial 30% to almost 60% in 1998. If maternity leave is longer this may increase the opportunity for fathers to invest more in market production. On the other hand, paternity leave may reduce time in market work after childbirth. During the 1970s, publicly funded child care programs were expanded for 3 to 6 year old. Between 2002 and 2008, child care programs were also expanded to full coverage for 1 to 2 year old children. Overall, we expect to find a relatively smaller fatherhood premiums in countries like Norway, where the institutional environment promotes gender equality and equality overall and attempts to shift cultural understandings of fatherhood (Boeckmann and Budig 2013). Furthermore, we may also expect to much less degree non-random selection into fatherhood.

Compared to other countries, we would expect that the fatherhood effect in Norway is relatively small because of, for example, the relatively high wage

[7] Studies tend to find a relatively high fatherhood premium for co-residing and biological fathers (Killewald 2012), which is the main group we focus on in the empirical analysis.

[8] The remaining women were not eligible. Workers are eligible if they have been working for 6 out of 10 months before the date of birth.

compression and high female labor force participation.[9] Related to this hypothesis, previous research (Blau and Kahn 1996) has shown that the gender wage gap increases with wage inequality, and vice versa. However, the fatherhood effect may be relatively increased through factors that increase (gender specific) household specialization, or gender attitudes towards traditional role models (Fortin 2005). One such factor could be part-time work of women. Overtime work of fathers when the children are very young could be another factor. Overtime work however seems to play a minor role according to National statistics showing that only 20% work overtime. Overtime is usually unpaid and in many sectors restricted. According to time use data gender specialization in the household among Norwegian couples is low.[10] Following the previous literature, we take an individual approach to study earnings of men, and neglect potentially endogenous household choices. Likewise most data sets, we do not have information on overtime hours of work.

3 Data description and summary statistics

Our empirical analysis exploits panel data on the population of male siblings and male twins born in the period 1955 to 1965 and followed from 1975 until 2005. They are extracted from Norwegian registry data. We focus on selected birth cohorts to ensure that we can observe the complete individual earnings and employment histories from first entry into the labor market, as well as complete fertility histories. The Norwegian multi-generational birth registry was used to match sibling and twin brothers to each other and their offspring. We include only the first- and second-born son within a family with the same mother and father.[11] Fraternal and monozygotic twins are included but cannot be distinguished in the data.[12]

Drawing on a data set starting in 1967, we generate work and earnings histories from first entry into the labor market. This ensures that we measure entry earnings accurately for every individual in our sample. The main outcome variable is the logarithm of real annual earnings that we use to measure earnings from work.[13] We deflate earnings by the Norwegian consumer price index (1998 = 100). Earnings are excluded for workers younger than 20 years of age, as they may still be in education. We also exclude observations with very low earnings (earnings less than the annually adjusted basic income according to the social security system). Years of experience are measured as the cumulative number of years with earnings above the yearly basic income. We generate two variables for years of experience. One that counts overall

[9] Other contributing factors may include free higher education.

[10] Time use data on hours of household work and market work for couples suggest that couples in Norway distribute time equally if they do not have children. These data are only available for 2010, somewhat outside our observation window and not earlier (Vaage 2012).

[11] This results in dropping ~50% of men and the main groups we exclude are sons from one-child families and families with fewer than two boys.

[12] Statistically, ~30% of all twins are monozygotic. Only monozygotic twins are genetically 100% identical at birth. Siblings are genetically more similar than two randomly selected men.

[13] The earnings variable measures all taxable earnings, including unemployment insurance, disability benefits, parental leave, and sick pay, but not means-tested social assistance and interest on financial assets.

years of employment since first entry into the labor market (work experience), and one that counts years of employment from the year of having a child (work experience post birth). We start with the event of first childbirth. We merge the variables age and years of education to the data. We generate a variable to control in the earnings regressions for the birth order rank of each of the brothers that we compare.[14]

From the birth registry, we obtain the complete record of the timing of offspring and the complete number of offspring for every man, counted up to 2005. The birth registry contains the information from the birth certificates where the mother and father are reported. In the estimations, we first focus on the year of the first childbirth and earnings before and after this year, the latter being referred to as the 'post-birth period'. For supplementary results, we use also the birth year of the second and third child and count the corresponding years of work experience post-second and post-third birth. Hence, we can test for any non-linearity of the post-birth earnings effects of the number of children.

The main treatment group is the group fathers-at-some-point, which includes all men for whom we observe at least one child in the birth registry at some point in the observation period.[15] The group of men without any children in the birth registry are denoted as childless men. In fact, we include in the control group only men in this group who never have children across the entire observation window (life cycle). Approximately 20% remain childless by the year 2005, according to the data. Among those born in 1955 the proportion is slightly lower, 16.6%. To a small extent this is because only the oldest cohort is followed until completion of fertility, or here age 50.[16] Given the very large and long panel, the bias of our estimation results due to relatively few men that are included in the group of childless men, even though they became fathers for first time rather late in life after 2005, is negligible. Looking at the distribution of number of children with our data, we note that 19 to 21% never have children, and most men, approximately a third, have two children.

We use information on marital status to restrict the comparison group of childless men to men who are childless but married-at-some-point.[17] Childless men may be a very heterogeneous group, and childless men married-at-some-point may be more similar to fathers-at-some-point at the beginning of their working career. Some of those who married may have planned to become fathers but for some reason did not realize such a plan.

[14] We keep information on birth order within the family, counting both girls and boys.

[15] One birth cohort is around 60,000 in Norway. The birth registry is complete. So we can study the effect of children on earnings when considering all fathers who could be directly affected by having children. Note that fathers are always reported when they are cohabiting with, or married to, the mother around the time of birth of the child. A small group that we cannot observe are those fathers not reported, for example, because the mother does not want to name him and the father has no contact with the mother and child. During the observation period, only 400–500 children were adopted per year and we have no information on those.

[16] National statistics show that the fraction of childless men declines only by 2 percentage points between the age of 40 and 45, and by 0.6 percentage points between 45 and 50.

[17] We do not have access to information on cohabitation for men without children. Hence, we also exclude those cohabiting without children.

Data on marital status is available from the Norwegian registry for the period 1986 to 2005. We use this information to construct an indicator for being married. We define the indicator variable married as equal to one if a man is reported as married, divorced or separated, and zero otherwise. The variable married-at-some-point is then the indicator variable equal to 1 if in any period in our observation window a man has ever been married, and zero otherwise. Information on being married and the timing relative to first childbirth is useful in order to disentangle whether earnings increases are related to children or marriage. For the group fathers-at-some-point, we can control for whether the man is married and has a child. For childless married-at-some-point men, we can control for potential changes in earnings after the time of marriage. One should note that it is typical in Norway to marry after becoming a father. This is shown in Fig. 1. Approximately 3 out of 4 couples get married after childbirth.

Table 1 reports the sample means and standard deviations for the main variables separately for fathers-at-some-point, the broad comparison group of childless men and the restricted comparison group of childless men married-at-some-point. We pool all observations across the entire observation period and report statistics for the sample of brothers and twins separately. The unconditional difference in the mean of logarithmic earnings between fathers and childless men is 17% for the sample of brothers and 15% for the sample of twins separately. Compared to childless men, men with children acquire slightly more years of education, and work less. Differences become smaller when we compare fathers to childless men married-at-some-point. Men entered fatherhood on average at age 28 in 1988. The findings are similar for the samples of brothers and twins. Reported means do suggest greater similarity of the more restricted group of childless men to the group of fathers-at-some-point.

Figure 2 describes the earnings paths for fathers-at-some-point around childbirth in comparison to childless men. We use the samples from Table 1 columns 1 and 2.

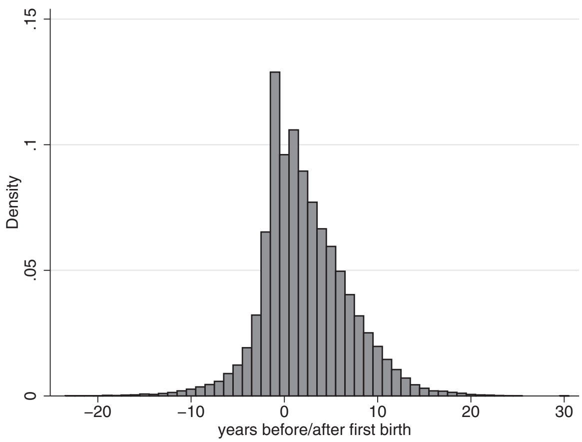

Fig. 1 Timing of marriage and first childbirth, own calculations using the sample of all brothers followed from 1986 to 2005

Table 1 Descriptive statistics: Means and standard deviations, by groups

	Fathers-at-some-point		Childless men		Childless men Married-at-some-point	
	Mean	sd	Mean	sd.	Mean	sd.
Brothers excluding twins[b]						
Log (earnings)	12.42	0.52	12.25	0.52	12.33	0.51
Real annual earnings (1000 Nkr)	267.3	271.3	219.8	153.6	237.5	154.0
Years of education	12.28	2.47	11.90	2.59	12.04	2.54
Age	33.61	7.18	33.11	7.16	33.88	7.29
Age at first marriage[a]	30.32	4.41	34.66	6.00	34.66	6.00
Age at first birth	28.35	5.47	–	–	–	–
Number of children	2.38	0.95	0	0	0	0
Year first job	1982	2.81	1982	2.96	1980	3.30
Years of experience	13.57	7.13	12.64	7.07	13.53	7.20
Years of experience before first birth	1.82	3.81	12.64	7.07	13.53	7.20
Year of birth	1960	2.98	1960	2.98	1959	3.03
Year of birth first child	1988	6.39			–	–
Year of birth second child	1991	6.05			–	–
Married-at-some-point	0.81	0.39	0.20	0.4	1	
Number of obs. brothers	1,461,807		272,249		51,351	
Twin brothers[c]						
Log (earnings)	12.38	0.51	12.23	0.52	12.35	0.45
Real annual earnings (1000 Nkr)	250.4	192.4	211.2	146.8	228.5	147.7
Years of education	12.11	2.48	11.70	2.50	11.12	2.18
Age	33.22	7.51	32.78	7.44	33.45	7.75
Age at first marriage	30.74	4.37	34.73	5.41	34.73	5.41
Age at first birth	28.47	5.39	–	–	–	–
Number of children	2.33	0.98	0	0	0	0
Year first job	1980	3.57	1981	3.79	1979	3.14
Years of experience	14.25	7.45	13.25	7.30	14.69	7.63
Years of experience before first birth	2.16	4.12	13.25	7.30	14.69	7.63
Year of birth	1959	3.21	1959	3.23	1958	3.02
Year of birth first child	1988	6.54	–	–	–	–
Year of birth second child	1991	6.15	–	–	–	–
Married-at-some-point	0.81	0.39	0.20	0.4	1	
Number of obs. twin brothers	36,218		8,230		1,515	

Note: Norwegian register data until 2005. All means reported for the sample of fathers-at-some-point are significantly different from the means reported for the sample of childless men and childless men married-at-some-point.

[a]Available since 1986.

[b]Pooled sample of first and second born brothers, excluding twin brothers, born 1955 to 65. In total 1,734,056 observations and 45,345 sibling pairs.

[c]Pooled sample of first and second born twin brothers born between 1955–65. In total 44,448 observations and 1069 twin pairs.

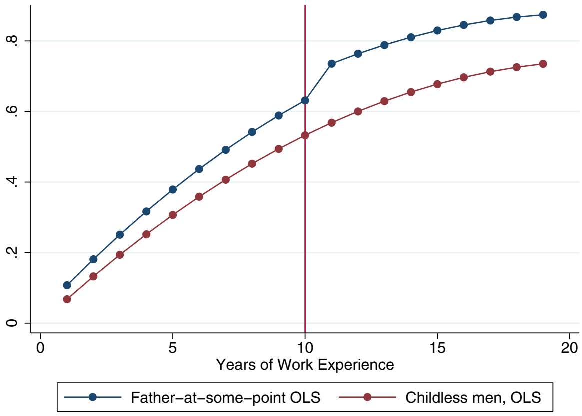

Fig. 2 Predicted earnings profiles for a man who is continuously working and is entering fatherhood in year 10. Childless men are continuously working from first entry. Predictions use the estimates reported in Table 2 column 1. Years of education is set equal to zero

For illustration, earnings in this figure are predicted for a man who continuously works and becomes a father in year 10 (Father at-some-point OLS). For comparison, the earnings path for a continuously working childless man is plotted across work experience (Childless men OLS). The graph is based on the coefficients from a flexible log earnings regression estimated by simple OLS to which we will return in more detail (results reported in Table 3, column 1). The figure highlights two descriptive findings. First, childless men and fathers-at-some-point differ in the earnings paths from early on in their working career and the difference increases with work experience starting from close to zero at entry. Hence, we observe earnings differences even before men actually 'reveal' having children, or not. Second, for men in the father-at-some-point group, earnings increase at the time of having their first child. This pattern is also revealed when we plot earnings against age before and after first entry into fatherhood selecting on specific ages, e.g. the mean age of 27, when entering fatherhood (graphs available on request).

4 Empirical framework

4.1 The selection problem and the model

We can derive the flexible earnings equation that we are estimating in a treatment framework.[18] In our application, we observe that fathers are on different earnings paths pre-treatment (i.e. from the beginning of their career before entry into fatherhood) in comparison to childless (across the entire life cycle) men. We allow for selection on unobservables through a component that is common to brothers in the

[18] The basic framework builds on Heckman and Hotz (1989) who investigated the return to training where training is not assigned randomly but self-selected. The selection problem in their study is that those who enter training (the treated) are different—in terms of labor market characteristics—before treatment, compared to the non-treated (those who do not enter training).

earnings equation. More generally, we estimate a more flexible Mincerian earnings equation (Mincer 1974) and allow for non-linear post-childbirth earnings effects.

Let $ln\, y_{ift}$ be observed logarithmic earnings of individual i in period t, and $ln\, y^*_{ift}$ the logarithmic earnings in the absence of children. (We add a subscript for family f to which we return later.) The indicator variable a_{it} equals one from the time a person i first enters fatherhood which is in calendar year t (treated), and zero otherwise (untreated). The parameter γ is the effect of fatherhood, which is an overall mean.[19] The period of childbirth is denoted as k. Then we can write:

$$\begin{aligned} lny_{ift} &= lny^*_{ift} + a_{it}\gamma, \qquad t>k \\ lny_{ift} &= lny^*_{ift}, \qquad t<=k \end{aligned} \tag{1}$$

The difference in mean post-birth earnings of fathers and non-fathers can be written as:

$$E[lny_{ift}|a_{it}=1] - E[lny_{ift}|a_{it}=0] = E[\gamma] + \{E[lny^*_{ift}|a_{it}=1] - E[lny^*_{ift}|a_{it}=0]\}, \tag{2}$$

The expression in parenthesis is the selection bias, which is present if the assignment to fatherhood is not random.[20]

Suppose lny^*_{it} is a linear function of a set of observed characteristics X_{it}, weighted by the parameter vector β^*, and unobserved characteristics ϵ_{ift}.

$$lny^*_{ift} = X_{it}\beta^* + \epsilon_{ift} \tag{3}$$

Then observed earnings can be written as

$$lny_{ift} = X_{it}\beta^* + a_{it}\gamma + \epsilon_{ift} \tag{4}$$

In the empirical application the vector X contains a constant and controls for years of education and experience (squared) counted since entry into the labor market. We assume that $E(\epsilon_{ift}X_{it}) = 0$ for all i and t.

The decision to become a father can be quite generally written in terms of an index-function framework, where the index, $father_{it}$, is a function of both observed, Z_{it}, and unobserved, u_{it}, characteristics:

$$father_{it} = Z_{it}\alpha + u_{it} \tag{5}$$

Then, the ith individual's fatherhood status is

$$a_{it} = 1 \quad iff \quad father_{it} > 0 \tag{6}$$

$$= 0 \qquad otherwise \tag{7}$$

Note that entry into fatherhood can take place at different times and therefore the variable is indexed by a subscript t. We assume u_{it} is iid across individuals and distributed independently of Z_{it}. This means that the dependence between ϵ_{ift} and a_{it} can arise because of dependence between Z_{it} and ϵ_{ift}, i.e. selection on observables, or

[19] In a robustness test, we test whether there is some variation across time using the Norwegian paternity reform; that is we test the assumption $\gamma_t = \gamma$.

[20] Since men typically work continuously we can neglect non-random selection into work. To incorporate women with more disrupted careers would demand further assumptions.

dependence between ϵ_{ift} and u_{it}, selection on unobservables. Men who become fathers at some point may have already invested previously more into their careers. In this case, omitted variable bias may arise.[21]

We present an estimate using a linear control function to address selection bias on observable characteristics. We assume $E[\gamma_{it}|a_{it}=1, X_{it}, Z_{it}] = \gamma$. Inserting a linear version of $E(\epsilon|X, Z)$[22] in Eq. (4) yields

$$lny_{ift} = a_{it}\gamma + C_{it}\delta^* + \tilde{\epsilon}_{ift} \tag{8}$$

where C_{it} denotes the vector of all variables included in either X_{it} or the vector of instruments Z_{it} and $\tilde{\epsilon}_{it} = \epsilon_{ift} - E(\epsilon|a_i, C_i) = \epsilon_{ift} - E(\epsilon_{ift}|C_i)$. δ^* is a parameter vector. In our application Z_{it} will be the vector:

$$Z_{it} = Z(father - asp_i, ex_{it} * father - asp_i, ex_{it}^2 * father - asp_i).$$

where $father - asp_i$ is an indicator variable equal to 1 for individual i if becoming a father ever during the life time, and zero if remaining childless. We also include the indicator variable interacted with years of work experience, and work experience squared. If the indicator is equal to 1 then all of the instrumental variables in Z are switched on. This is used to instrument whether the father has a child, and is to be independent of the father's wage conditional on a family fixed effect. Crucial for employing this strategy is that we observe complete earnings histories from first entry into the labor market, and we can distinguish fathers-at-some point from childless men.[23]

Replacing C leads to:

$$lny_{ift} = a_{it}\gamma + X_{it}\beta + Z_{it}\delta + \tilde{\epsilon}_{ift} \tag{9}$$

To allow the post-childbirth effect to be non-linear, we use a non-linear function $\gamma()$ in the indicator $1(a=1)$—which is equal to 1 after childbirth and zero otherwise —and the years of post-birth work experience, ex_{it}^{post}.

$$\gamma(a_{it}, ex_{it}^{post}) = \gamma_1 1(a=1)_{it} + \gamma_2 1(a=1)_{it} * (ex^{post})_{it} + \gamma_3 1(a=1)_{it} * (ex^{post2})_{it}.$$

The first term can be interpreted as a pure shift parameter that is an estimate of the change in earnings from the time of first entry into fatherhood, whilst the second and third term capture the curvature of earnings post entry into fatherhood. The non-linearity may capture time varying costs in the age of the child, or effects through further children. Later we test whether the second or third childbirth has a significantly different effect on earnings profiles than the first childbirth. If the estimates of γ_2 and γ_3 are not significant then there is only a constant shift of earnings post entry into fatherhood.

Substitution of the function for the flexible post-childbirth effect into the earnings equation gives then the earnings regression that we are going to estimate:

$$lny_{ift} = \gamma(a_{it}, ex_{it}^{post}) + X_{it}\beta + Z_{it}\delta + \tilde{\epsilon}_{ift}. \tag{10}$$

[21] The direction of selection bias can go either way.

[22] We use that $E(\epsilon|a, X, Z) = E(\epsilon|X, Z)$. In this case controlling for the observed selection variables (Z) solves the (observed) selection bias problem.

[23] Hence, we do not rely on the assumption that trends are the same before treatment, or childbirth.

The error term contains three components:

$$\tilde{\epsilon}_{ift} = \nu_f + \mu_{if} + w_{ift}, \tag{11}$$

these are an unobserved family fixed component, ν_f, capturing genetically inherited ability[24]; an individual varying and family-varying unobserved component, μ_{if}, capturing unobserved ability and genetic traits that vary across individuals and families; and w_{ift} capturing other idiosyncratic variation (or luck). In the empirical estimation, we control for birth order effects, capturing the fact that first or second born brothers within a family differ in birth order rank, as well as time fixed effects capturing macroeconomic shocks. (These are not shown in Eq. (10)). We assume that $E(w_{ift}X_{it}) = 0$ and $E(w_{ift}Z_{it}) = 0$ for all i, f and t.[25]

4.2 The covariance estimator

The key parameter is the mean effect of having children, γ, which is a vector of three parameters: the shift right after childbirth, and the squared polynomial term in years of work experience post childbirth. Most studies on the effect of fatherhood have estimated a more restrictive log earnings equation by simple OLS or fixed effects (FE), where Z_{it} is excluded, no unobserved family fixed effects are considered and the effect of children is only a shift parameter in earnings after first childbirth. In addition, these studies only use the group of fathers-at-some-point.

We employ the covariance estimator (CV) (Bound and Solon 1999) which applies OLS to the regression of the between-siblings differences in log earnings on the between-siblings differences in the children variables, holding other between-sibling differences constant. This is an alternative to controlling for family fixed effects in the main earnings regression in levels, which is taking into account that we have only two siblings within each family. This approach allows us to use variation from the entire sample of men including childless men, and the conditional cross-sectional variation for identification.

This approach attempts to address selection problems due to unobservables, in addition to selection on observables as discussed before. If we write down the full regression (Eq. 10) for the first born brother in family f' and the second brother in family f' and then subtract the latter from the former we can derive the regression in differences between brothers. Note that we form the between-sibling difference always by subtracting the variable of the second-born brother (indicated by the

[24] Since we cannot distinguish identical twins from fraternal twins we cannot use their comparison to disentangle nature and nurture effects. Another reason why we want to control for family fixed factors is that they are potentially correlated with fertility outcomes if, for example, families pass on fixed values to their offspring that are important traits for having a family later in life (Fernandez and Fogli 2006).

[25] We follow the common assumption in the literature, but acknowledge that it might be restrictive to assume no reverse causality. Identification depends on this assumption for both the family fixed estimator and the individual fixed effect estimator. This assumption can be relaxed only in case of a valid instrumental variable.

subscript 2) in family $f^{'}$ from the variable of the first-born brother (indicated by the subscript 1) in family $f^{'}$. If we assume $\mu_{1f^{'}}=\mu_{2f^{'}}$[26], then the transformed regression can be written as:

$$\begin{aligned}(lny_{1f^{'}t} - lny_{2f^{'}t}) = {} & \gamma_1(1(a=1)_{1t} - 1(a=1)_{2t}) \\ & +\gamma_2(1(a=1)_{1t} * (ex^{post})_{1t} - 1(a=1)_{2t} * (ex^{post})_{2t}) \\ & +\gamma_3(1(a=1)_{1t} * (ex^{post2})_{1t} - 1(a=1)_{2t} * (ex^{post2})_{2t}) \\ & +(X_{1t} - X_{2t})\beta + (Z_{1t} - Z_{2t})\delta + (w_{1f^{'}t} - w_{2f^{'}t})\end{aligned}$$

Under the set of assumptions that we made, this specification can be estimated by OLS to identify the key parameters. Variation used to identify the parameters γ_1, γ_2, γ_3 and δ comes from sibling pairs where one sibling has children and the other does not. To identify δ, we need to observe men in the group of father-at-some-point in employment before they actually become a father; that is, we need variation in a_{it}, which is independent of Z_{it}.[27] This highlights the value of our data, in which we observe the complete employment and earnings history before and after becoming a father, as well as the complete fertility histories. We also require that a_{it} and Z_{it} are uncorrelated with w_{ift}.

Our approach has some advantages over previous approaches. First, the identification of the non-linear post-childbirth effect relies on cross-sectional variation since we take differences between brothers in the same period. Since we can distinguish father-at-some-point compared to childless men, it is not the timing of births that generates the variation. Second, the CV estimator does not rely on the strict exogeneity assumption unlike fixed effects estimators that introduce dynamics into the error term and therefore potential feedback effects. Also gaps in individual panels do not cause problems as in FE (see Loughran and Zissimopoulos (2009), Ludwig and Brüderl (2018)). Third, since we compare two men from the same family (same mother and same father), they are more similar in terms of the unobserved component than two randomly selected men from the population. This is likely to reduce the bias. We show compelling evidence that this is the case and taking out heterogeneity related to family fixed effects does reduce the bias.

In order to reduce further bias—because strict equality of μ_{1if} and μ_{2if} within each couple of brothers may not hold—we also compare siblings who are more similar in age. By reducing within sibling differences in age, we also reduce differences in family environment. That is, we are then comparing outcomes for brothers whose parents are in more similar career phases, may have more similar time and monetary resources or have more similar parenting experience.[28] Differences in these characteristics decrease between brothers who are more similar in age, and related

[26] Since we cannot make use of data on monozygotic twins, we cannot sweep out μ completely and, therefore, have to make assumptions. We also tested whether $\mu_{1f^{'}} = \mu_{2f^{'}} = 0$. We tested for second and third order serial correlation of the error term from the model in between-sibling differences, observing that serial autocorrelation remains, yet is small. The results are available on request.

[27] At the individual level, i, all combinations of Z_{it} and a_{it} are observed, except for the combination $Z_{it} = 0$ and $a_{it} = 1$ (i.e. childless man after becoming a father).

[28] This has been used in the literature exploiting sibling data, as in Griliches (1979).

heterogeneity can be further reduced. Note that, due to the large data sample, we are able to run separate regressions on sub-groups of siblings with a specific age difference. In the sample of first-and second-born brothers, the mean age difference is 3.5 years and it is reduced to zero when we use only twins. Using twins offers the advantage that we directly control for family fixed factors, individual fixed factors and time fixed factors.

5 Empirical results

5.1 Does having children affect male earnings?

Table 2 reports the estimation results of the main earnings regression in Eq. (10). We report the coefficients together with standard errors that are clustered at the sibling pair level. We report estimation results from ordinary least squares that exploits only the cross-sectional variation and from the covariance estimator, that uses the within family or sibling couple variation.[29] The top three rows (upper panel) report the parameter estimates of the auxiliary variables; the common return to education, and the return to experience (squared). The next panel of coefficients reports the differential effect in the intercept, that captures differences at first entry into the labor market, and the differential return to experience (squared) between the fathers-at-some-point and childless men. The three coefficients reported in the lower panel are the key coefficients of interest; the differential post-childbirth effects on earnings. OLS gives a parametric description of the earnings profiles as we showed them in Fig. 2. The results reveal a positive average return to education. The return to years of work experience for the comparison group of childless men is 6.9% in the first year and then declines. The differential effects on entry earnings (i.e. the intercept) and work experience since first entry into the labor market between fathers-at-some-point and the group of childless men are significant and positive (middle panel of Table 2). Hence the descriptive finding is that fathers-at-some-point start on higher and steeper earnings paths than childless men. After the first childbirth, the immediate shift in earnings is 0.073 which gives a 7.3% increase in earnings compared to the period before fatherhood. The differential effect in experience post birth is slightly nonlinear.

Table 2, column 2 reports the results from the CV estimator. Compared to the sample used for OLS, we now have half the number of observations because CV uses variables in differences between brothers. The CV estimates of the coefficient for the post-birth variable shows that earnings significantly shift upward in the birth year of the first child. The point estimate of the effect after childbirth is 6.4%. The curvature in experience post birth reveals that the marginal effect of experience post birth is

[29] Identification applying the CV estimator depends on sibling pairs where one brother has children and the other does not. In our sample of brothers, 27.94% of all siblings have the combination ‘no children’ and ‘children’. For twins, the corresponding value is 25.73. Hence, the data contain sufficient variation. We also tested for the possibility that sibling pairs that identify the effect of having children in the family fixed effects model are different from the random sibling pairs in our total sample. This could bias the results. The estimation results from a restricted sample of brother siblings with unequal fertility outcomes show that compositional effects do not explain our main findings.

Table 2 The effect of children on log earnings: OLS and the family fixed effect (CV) estimation results

	Sample of brothers excl. twins				Sample of twins	
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	CV	CV	CV	OLS	CV
		All	<3 year age diff	<2 year age diff		
Years of education	0.04692***	0.03266***	0.03088***	0.02985***	0.03465***	0.01564**
	(0.00051)	(0.00070)	(0.00111)	(0.00181)	(0.00367)	(0.00543)
Experience	0.06947***	0.06840***	0.06294***	0.06012***	0.06994***	0.05923***
	(0.00085)	(0.00121)	(0.00215)	(0.00359)	(0.00547)	(0.00929)
Experience squared	−0.00162***	−0.00152***	−0.00127***	−0.00109***	−0.00163***	−0.00097**
	(0.00003)	(0.00004)	(0.00007)	(0.00012)	(0.00017)	(0.00031)
Differential effect in entry and experience (father-at-some-point)						
Father-at-some-point	0.03022***	0.04911***	0.04641***	0.05800***	−0.01724	0.10044*
	(0.00543)	(0.00700)	(0.01040)	(0.01659)	(0.03616)	(0.04037)
Experience × father-at-some-point	0.00986***	0.00550***	0.00552**	0.00527	0.01829**	0.00083
	(0.00089)	(0.00117)	(0.00184)	(0.00292)	(0.00581)	(0.00674)
Experience squared × father-at-some-point	−0.00030***	−0.00022***	−0.00025***	−0.00029**	−0.00065***	−0.00032
	(0.00003)	(0.00004)	(0.00007)	(0.00011)	(0.00019)	(0.00022)
Differential effect of having children (post first childbirth)						
Post birth	0.07286***	0.06442***	0.04038***	0.04096***	0.07535***	0.01962
	(0.00201)	(0.00265)	(0.00408)	(0.00671)	(0.01211)	(0.01482)
Experience post birth	−0.00821***	−0.00591***	0.00018	0.00134	−0.01314***	−0.00027
	(0.00045)	(0.00061)	(0.00100)	(0.00164)	(0.00274)	(0.00356)
Experience squared post birth	0.00038***	0.00033***	0.00013**	0.00011	0.00077***	0.00043**
	(0.00002)	(0.00003)	(0.00004)	(0.00007)	(0.00012)	(0.00015)
Observations/pairs	1,734,056	867,028	349,699	129,847	44,448	22,224
R^2	0.32149	0.06938	0.05008	0.04939	0.03591	0.41838

Note: Norwegian register data until 2005. Sample of first and second born brothers (excluding twin brothers) and twin brothers, born 1955 to 65.

All regressions control for birth order- and time effects. The control group includes all childless men. Standard errors are clustered at the sibling pair level and reported in parentheses. We test with the Hausman test the H_0: that the post-birth effects estimated by OLS in column (1) is equal to the corresponding coefficients estimated by CV in Column (4). We can reject the Null hypothesis at the 5% significance level. An F-test to test whether the vector of marginal effects in columns (4) and (6) are the same is rejected.

almost zero. In comparison to OLS, the CV estimate gives a relatively smaller increase in earnings post birth at any level of experience post birth. The estimates using CV suggest that positive selection on unobserved factors biases OLS upwards. The CV estimates show that 13%, that is $((1 - 0.064)/0.072) \times 100$, of the simple OLS estimated shift effect post birth, that is immediately or less than one year after childbirth, is due to positive selection on fixed family-specific characteristics.

Siblings are genetically more similar than randomly selected men, which is why CV controls for more heterogeneity than OLS. Still, siblings might be quite heterogeneous in terms of family background, which may introduce bias and thus make family fixed factors appear less important. We explore remaining biases through potential time-varying differences between siblings—such as differences in parenting experience, parent characteristics and age by using more homogeneous sub-samples of brothers in terms of age. The new estimation results are reported in Table 2, where we gradually decrease the age differences in the sample of brothers to two years or less (column 3), one year or less (column 4), to zero (where we use the sample of twins (column 6)). Column 5 is reported in order to show that we can replicate the main results from OLS for the sample of all brothers with the sample of twins.

When we select brothers who are less than three years apart in age (column 3), the shift effect on earnings after childbirth diminishes to 4%. The differential effect in experience post birth is economically negligible and only the coefficient of the squared experience post-birth variable is significant. The estimate is substantially smaller than the post-birth estimate from CV when we used all siblings. Recall that we estimate the effect post birth conditional on actual work experience so that age differences between brothers capture additional factors, which could be time varying family background factors.

When we reduce the age differences between brothers further to less than two years (column 4) then the results on the post-childbirth effect remain unchanged. We now see even more clearly that the effect post birth is only an upward shift at birth in earnings of 4%. Hence, there is no further adjustment in terms of earnings during the years after first child-birth. This may be surprising given that most have a second or further child which may induce further adjustments.

When we use the sample of twin brothers (see column 6), the effect post birth declines even further. The coefficients of all of the post-birth variables become statistically and economically insignificant. Note that even though the coefficient of experience post-birth squared is statistically significant, the estimate coefficient is economically negligible (0.00043). The point estimate of the shift in earnings post birth is small, and not significant. With a F-test (not reported in the table) we can also show that estimates from brothers one year different in age, or less, and twins are jointly significantly different (Table 2, column 4 compared to column 6).

When we look at the estimation results of the other coefficients, we find that the differential effect on entry earnings (i.e. the intercept) and experience remain significant between men in the group father-at-some-point and in the group childless men. There is one noticeable difference between the estimates from CV on the sample of brothers and CV on the sample of twins. When we use brothers of different ages, we find differences in the earnings trajectories from early on in the careers. When we use twins, differences in earnings are only significant at first entry into the labor market. Hence, men who remain childless all their lives, and fathers-at-some-point, are on the same earnings profile—except that those who become fathers at

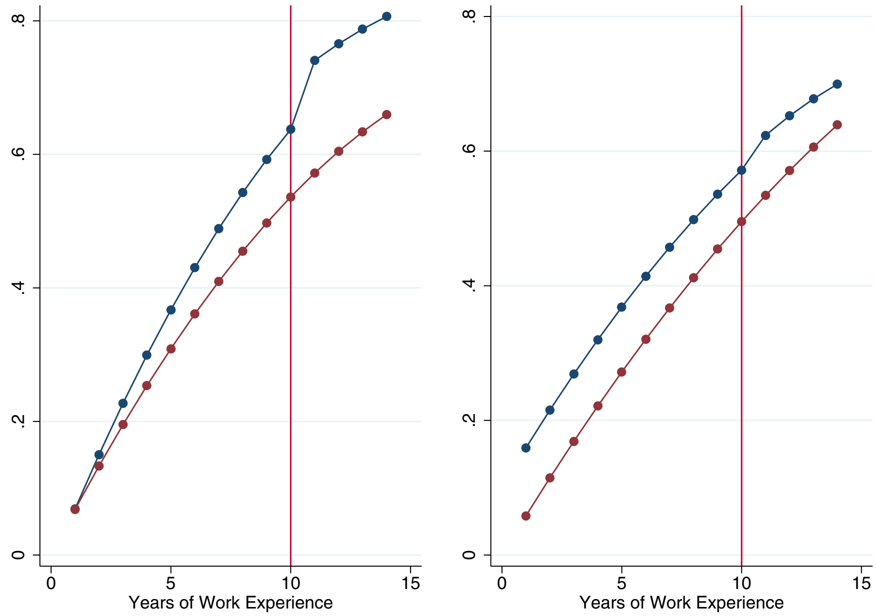

Fig. 3 Results for twins: Predicted earnings profiles for a man who is continuously working and enters fatherhood in year 10 (blue line). Childless men (red line) are continuously working from first entry. The predictions use the estimates reported in Table 2 column 5 (OLS)—left figure—and Table 2 column 6 (CV)—right figure

some point in life start on relatively higher pay. This implies that we do not find any evidence that twin brothers invest differently into their careers over time. This makes these estimates more convincing than those from brothers since essentially we can compare two men with parallel trends (until childbirth). This gives an unbiased estimate of the effect of childbirth. The main results for twins are illustrated in Fig. 3 where we present predicted earnings profiles from OLS and CV estimates.

The estimate using differences between twin brothers accounts for time fixed effects, family fixed effects, and individual fixed effects. Note also that the mean pair is now genetically more similar, because around 30% of twins in the sample are monozygotic and hence genetically identical at birth, which motivates the assumption that we can control for individual fixed effects.[30]

5.1.1 Testing who to compare to

We explore further factors that may explain differences in the earnings profiles from first entry into the labor market between the fathers-at-some-point and others, and which may also be positively correlated with the post-birth effect of children. First, we change the control group and now use childless men married-at-some-point as an alternative comparison rather than all childless men. We may argue that those who

[30] We acknowledge that still we have to make this assumption. We cannot tell whether the genetic component drives our results since we cannot distinguish between fraternal and monozygotic twins in our data.

marry at some point but remain childless, may have also had plans to have children at some point, but did not realize those plans for reasons uncorrelated with labor market behavior. Since the group of all childless men that we have used so far may be quite heterogeneous, taking the sub-group of childless men who also married-at-some-point makes the comparison group more homogeneous and more similar to the group of fathers-at-some-point. This contention is supported in Table 1, where we show that men in the group fathers-at-some-point are more similar with respect to mean years of education and work experience to childless men married-at-some-point than to all childless men.

In Table 3, we report the corresponding estimation results of the earnings regression only for the brother samples.[31] Columns 1 and 3 report selected coefficients showing the differential effects of childbirth. The differential effect on entry earnings (i.e. the intercept) between fathers-at-some-point and the restricted group of childless men married-at-some-point is now much smaller than those reported before in Table 2, columns 2 and 4. In column 1 of Table 3, the estimated difference between mean entry earnings is not significant, and the differences in slope coefficients are very small. When we restrict the age difference between brothers to less than two years, then the differential effects since entry into the labor market become statistically insignificant and economically close to zero (Table 3, column 3). This suggests that the large differences between those who are not yet fathers and all childless men are, to a large extent, driven by childless men who never marry. The result shows that this group performs relatively worse in the labor market. The result is robust to restricting the treatment group to ever married fathers-at-some-point. Regarding our main results, however, the size of the post-birth coefficient (that is, the coefficient of the shift and the experience post-birth variables) do not change significantly compared to our main results in Table 2.

5.1.2 Testing whether the post-first birth effect is also capturing marriage

Returning to the results for brothers (Table 3, columns 1 and 3), a question remains as to whether the remaining effect of entry into fatherhood is driven by childbirth or by marital status. As a simple test we add as a control variable an indicator variable switching to one when a man is actually married. We re-estimate these regressions using the samples of brothers (excluding twins) where the comparison group is restricted.

In Table 3, columns 2 and 4, adding a control for being married to our previous specifications reduces the size of the post-birth earnings effect. For brothers who are only one year apart in age (Table 3, column 4), the effect of children now is 3.2% and economically constant. Hence, the effect is reduced by 28% $(=1-(0.032/0.044)\times 100))$.[32] We interpret this as a simple test showing that the estimated effect is capturing mainly variation related to having children, rather than marriage.

The post-birth effect could still be the joint effect of entry into fatherhood and cohabitation, since men not married at childbirth may be cohabiting. The results show that even if some men in the father-at-some-point group already cohabit before

[31] The twin sample would become too small for estimation if we further restricted the group of childless men, at only 1515 observations or ~700 sets of twins. Summary statistics are reported in Table 1, column 3.

[32] In this calculation, we ignore the curvature parameters, since they are essentially zero.

Table 3 The effect of children on log earnings estimated by CV: comparison group is childless men who are married-at-some-point

(Selected coefficients are reported, brother sample excl. twins)

	(1) CV All	(2) CV All	(3) CV <2 year age diff	(4) CV <2 year age diff	(5) CV <2 year age diff
Differential effect in entry and experience (father-at-some-point)					
Father-at-some-point of .. children (any parity)					
	0.01542	0.01152	0.01611	0.01334	
	(0.01518)	(0.01517)	(0.03417)	(0.03399)	
1 child					−0.02372
					(0.03531)
2 children					0.02551
					(0.03430)
3 children					0.02211
					(0.03453)
4 children					0.01383
					(0.03599)
More than 5 children					−0.03835
					(0.03904)
Exp. × father-at-some-point	0.00649**	0.00750**	0.00908	0.01015	0.01061
	(0.00233)	(0.00233)	(0.00570)	(0.00567)	(0.00568)
Exp.^2 × father-at-some-point	−0.00026***	−0.00025**	−0.00044*	−0.00042**	−0.00043*
	(0.00008)	(0.00008)	(0.00020)	(0.00020)	(0.00020)
Differential Effect of having children (post childbirth)					
Post-birth 1st child	0.06414***	0.05284***	0.04455***	0.03169***	0.02982***
	(0.00281)	(0.00286)	(0.00716)	(0.00733)	(0.00738)
Experience post birth	−0.00666***	−0.00863***	0.00083	−0.00122	−0.00189
	(0.00065)	(0.00065)	(0.00177)	(0.00178)	(0.00180)
Experience squared post birth	0.00035***	0.00040***	0.00013	0.00017***	0.00019*
	(0.00003)	(0.00003)	(0.00008)	(0.00007)	(0.00008)
Control for being married	No	Yes	No	Yes	No
Observations	675,679	675,679	99,917	99,917	99,917
R^2	0.05501	0.05753	0.03362	0.0373	0.03929

Note: Norwegian register data until 2005. Sample of first and second born brothers (excluding twin brothers) less than two years different in age, born 1955 to 65.

Control group are childless men married at some point. Specifications are extensions of Table 2, column 4.

Standard errors are clustered at the sibling pair level and reported in parentheses.

All regressions control for birth order- and time effects.

$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

having children, it is noteworthy that we do not observe differential effects from first entry into the labor market—or before childbirth. So household specialization does not explain pre-birth differences.[33]

[33] These results are unchanged when we use only ever married fathers-at-some-point.

5.1.3 Testing whether the post-birth effect captures second or further births

It is also possible that the earnings effect at first childbirth captures the effects of second or subsequent births. To test this hypothesis we extend our specification in column 3 and add dummy variables for second and further births up to 5 or more. As can be seen from column 5 in Table 3 the differential effect at entry into the labor market does not vary significantly across parity compared to childless men married-at-some-point. The shift in post-first childbirth earnings remains significant and robust in size. Estimates that are not reported here would also show that the shift and the return to post-childbirth years of experience does not change after the second childbirth or further births. This result highlights that the event of (first) fatherhood is important for earnings and not the number of children.

5.2 Testing robustness across time and the paternity leave reform

We exploit the exogenous variation arising from the 1993 parental leave policy reform, in order to test the robustness of our results over time within Norway. The reform introduced four weeks of paid paternity leave. The proportion of fathers taking leave increased from 4% in 1993 to 33% in 1994 (see www.SSB.no). The reform applied to parents of children born after 1 April 1993. Since we rely on yearly data, we assume that post-1993 births are eligible.[34]

In order to test whether our results hold over time, we re-estimate the main regressions from Table 2 on a restricted sample. We drop earnings from the individual time series if the earnings are post-childbirth and the childbirth was in or after 1993. Hence, we keep all individuals in the data panel, and the restriction is primarily on the individual time series of earnings. This strategy should remove any potential effects occurring through the reform, or through the post-reform take up of paternal leave. The replication of Table 2 on the restricted sample is reported in Table 4.

The descriptive estimate (OLS) in Table 4, column 1 replicates the pattern that we have found before. Men who become fathers at some point are on higher earnings paths already, even before entering fatherhood. Post-childbirth earnings increase non-linearly. If we estimate the model using the CV estimator, and use brothers excluding twins, then we see that the post-childbirth effect is smaller than from using OLS. As we decrease the age difference between brothers the post-childbirth increase in earnings tends to decrease. For the sample of twins the estimate of the post-birth earnings effect is 3.2% and economically constant across years of post-birth work experience. We interpret these estimates as upper and lower bound estimates, although they are not statistically different from the estimates on the full period taking the standard errors into account. As before for the twin sample estimates, we see that earnings paths from first entry into the labor market are not significantly different, except that entry wages are higher for those in the father-at-some-point group than for childless men.

[34] Since we will drop annual earnings that are potentially affected by the reform, this rule will minimize measurement problems. Usually fathers take leave at the end of the parental leave period, which is then in 1994 for births in April 1993.

Table 4 Robustness Checks: Regression results on a restricted sample to exclude effects through paternity leave reform

	Brothers				Twins	
	OLS	CV All	CV <3 yr age diff	CV <2 yr age diff	OLS	CV
Years of education	0.039***	0.025***	0.022***	0.020***	0.028***	0.010***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)
Experience	0.074***	0.069***	0.063***	0.060***	0.069***	0.057***
	(0.000)	(0.001)	(0.001)	(0.002)	(0.003)	(0.005)
Experience squared	−0.002***	−0.002***	−0.001***	−0.001***	−0.002***	−0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Differential effect in entry and experience (father-at-some-point)						
Father-at-some-point	0.020***	0.036***	0.040***	0.045***	−0.008	0.100***
	(0.003)	(0.004)	(0.006)	(0.010)	(0.016)	(0.023)
Experience × father-at-some-point	0.012***	0.009***	0.007***	0.009***	0.019***	−0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)	(0.004)
Experience squared × father-at-some-point	−0.000***	−0.000***	−0.000***	−0.000***	−0.001***	−0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Differential effect of having children (post first childbirth)						
Post birth (a)	0.090***	0.078***	0.048***	0.047***	0.085***	0.032*
	(0.002)	(0.002)	(0.004)	(0.006)	(0.010)	(0.013)
Experience post birth (exp_{post})	−0.015***	−0.013***	−0.005***	−0.003*	−0.021***	−0.003
	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)	(0.003)
Experience squared post birth (exp^2_{post})	0.001***	0.001***	0.000***	0.000***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1,172,084	586,042	237,455	87,894	32,454	16,227
R^2	0.283	0.066	0.044	0.044	0.305	0.033

Note: Samples from Norwegian register data until 2005. Sample of first and second born brothers (excluding twin brothers) and twin brothers, born 1955 to 65. Earnings potentially affected by the paternity leave reform in 1993 are excluded.

This means that the sample is the same as in Table 2 except that individual earnings following childbirths are dropped if the childbirth is reported after 1993. See text for further explanations.

All regressions control for birth order and time effects.

Standard errors are clustered at the sibling pair level and reported in parentheses.

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

A more careful comparison of the estimation results for the post-birth period (lower panel) reported in Table 4 and Table 2 reveals that Table 4 shows systematically larger post-child birth effects when we exclude effects through the paternity reform than Table 2. Hence, it seems that the decline in the marginal effect post birth could be a reform effect and could be interpreted as an intention to treat effect. Alternatively, it may capture other trends, such as a decrease in the gender-specific household specialization related to having children. Note, in order to explain the pattern plausibly, the change would have to happen on a more permanent basis rather than simply being an adjustment during the period when the child is very young.

5.3 Testing for responses in labor supply

In Table 5 we provide more evidence on the question of whether temporary paternal labor supply adjustments are one of the mechanisms explaining the positive post-childbirth point estimates in the sibling data (excluding twins). We re-estimate our empirical model on the sample of siblings but now replace the outcome variable by a measure of labor supply. From our data we can construct an indicator variable for whether an individual is employed or not, and whether working hours are larger than 30. We find that childless men are less likely to be employed and work fewer hours,

Table 5 Linear Probability Model results for employment and hours of work

	All[a]		Restricted Comparison Group[b]	
	Employment	More than 30 h work	Employment	More than 30 h work
Years of education	0.018***	−0.003***	0.018***	−0.003***
	(0.000)	(0.000)	(0.000)	(0.000)
Experience	0.021***	0.006***	0.015***	0.005***
	(0.000)	(0.000)	(0.001)	(0.001)
Experience squared	−0.000***	−0.000***	−0.000***	−0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Differential effect in entry and experience (father-at-some-point)				
Father-at-some-point	0.060***	0.004*	0.014	−0.003
	(0.003)	(0.002)	(0.010)	(0.005)
Experience × father-at-some-point	−0.003***	0.001***	0.001	0.001
	(0.001)	(0.000)	(0.001)	(0.001)
Experience squared × father-at-some-point	0.000***	−0.000***	0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Differential effect of having children (post first childbirth)				
Post birth	0.013***	0.008***	0.002	0.004***
	(0.001)	(0.001)	(0.002)	(0.001)
Experience post birth	−0.003***	−0.001***	−0.004***	−0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Experience squared post birth	0.000***	0.000**	0.000***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1,655,259	1,655,259	697,224	697,224
R^2	0.021	0.007	0.025	0.005

Note: Norwegian register data until 2005. Sample of first and second born brothers (excluding twin brothers), born 1955 to 65.

All regressions control for birth order and time effects.

Standard errors are clustered at the sibling pair level and reported in parentheses.

$^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$

[a]All means all fathers-at-some-point and all childless men

[b]Restricted Comparison Group uses only childless men married at some point as comparison group

which is consistent with the findings on earnings. When we restrict the control group to childless men married-at-some-point (columns 3 and 4) most of the coefficients turn insignificant. An exception is the economically small increase, 0.004 percentage points, in the probability of working more than 30 h. Calculations show, however, an economically zero effect during the 3 to 4 years following entry into fatherhood (column 4). Overall, these results present little support for the hypothesis that the remaining effect captures labor supply responses.[35] Regarding inequality between all childless men and fathers-at-some-point we learn however that childless men who are never married have more spells of non-employment and shorter hours of work.

5.4 Discussion

The main result of this study is that the effect of children on earnings is not significant when we estimate flexible Mincerian earnings regressions, controlling for a differential effect on entry earnings and experience since first entry into the labor market and family fixed effects. When we compare twin brothers, there is no significant effect of children on earnings. Twins have similar earnings growth paths all through their careers independent of whether they have children or not. These results refute explanations related to gendered household specialization and employment discrimination.

Given the Norwegian context, we may expect that the fatherhood effect in Norway is relatively small. This might be due to family policy, but not only, and perhaps even more so due to high wage compression and high female labor force participation.[36] Our findings confirm this. It may seem surprising that we find strong selection effects through family fixed effects indicating that the variation between families is relatively important. Family fixed effects may capture socioeconomic background, investment by parents into children, or related factors that some typically regard of relatively less importance in high equality countries.

We also show that most of the variation in earnings comes from children, and only a minor part through marriage. This is in line with new studies that also find that after controlling for wage growth, the marriage premium becomes smaller and not significant (Killewald and Lundberg 2017; Ludwig and Brüderl 2018). Other studies on Scandinavia also find small marriage premiums in fixed effects models (for Denmark, see Datta Gupta et al. 2007, for Norway, see Petersen et al. 2011). Our results contribute to the small literature that has investigated both the marriage premium and the effect of having children on men's earnings. For Denmark, Datta Gupta et al. (2007) estimated a marriage premium of 1.2% and a positive effect of 0.9% per year for having children younger than 3 years. Hence, the Danish study suggests temporary post-childbirth earnings effects which we do not find. Temporary adjustments after child birth could be related to gender specific household specialization during the child care intensive infant period.

[35] Note, that we present OLS estimation results that condition on the work history in a flexible way, but do not control for family fixed effects. The reason is that we would not expect more insights from the covariance estimation results since the differential effects are already not significant.

[36] The female employment rate was 1990 (2009), 62.5 (68.8)% for women in Norway, compared to 57 (58)% in the U.S. Source: OECD. For more background see NOU (2008): 6 and NOU (2012): 15.

Our results add to the understanding of sources of male earnings inequality. We show that differences in observed earnings profiles between childless men and fathers-at-some point are increasing from the beginning of their working careers. This can be interpreted as effects through differential investment into human capital, which is a key driver of inequality. We find that men who remain unmarried and childless are a negatively selected group, contributing to the observed earnings inequality among men. This finding is consistent with findings for Sweden showing that single men earn less than fathers (Boschini et al. 2011). We also find that earnings inequality is affected only by the arrival of the first child. This contrasts with the literature on women showing that the shift effect of children on post-birth earnings is negative and increasing in the number of children (Waldfogel 1998).

Our results show strong evidence that family background is an important cause of the observed child earnings premium for men. This supplements the few previous studies that have used models with family fixed effects in the earnings equation.[37] We find that OLS regressions which use the cross-sectional comparison of men who have a child and who remain childless are plagued by selection effects into fatherhood. OLS regressions overestimate the (positive) effect of having children on male earnings. This implies that individuals with relatively high values of the (unobserved) family-specific factor in our model are more likely to become fathers.

This research suggests that selection on family background into fatherhood is important. We conclude that there is positive selection although this could differ in other countries and settings. Our findings also raise questions as to why family background is important. In order to answer this question we would need more research with richer data on potential mechanisms. For example, explanations could be directly related to the social environment created within the family, or the neighborhood, cognitive development or the values engendered within the family.

6 Conclusions

This study investigates whether having children has a causal effect on earnings for men with an additional focus on the effects on earnings inequality among men. The empirical analysis employs novel data on the population of siblings drawn from longitudinal Norwegian registry data that contains the complete employment, earnings and fertility histories for all brothers and, more specifically, twins. We estimate flexible earnings regressions where the unobserved component is common to brothers and non-random selection into fatherhood is taken into account. From descriptive analysis, we find that during the early career and before entry into fatherhood earnings inequality between fathers-at-some-point and childless men gradually increases. Men who remain childless and never marry contribute considerably to the increase in earnings inequality. The effect of fatherhood is capturing observed variation at first childbirth. Our results show that the conditional effect of first entry into fatherhood is declining the more effectively we control for family fixed effects. The large data set allows us to control for family fixed factors using

[37] However, our results suggest that studies controlling for family background but not conditioning on pre-birth histories are likely to suffer from upward bias.

differences between first- and second-born siblings, and by restricting the sibling age differences up to zero by using data on twins. Novel to the literature, we show that non-random selection into fatherhood is captured through family fixed factors and higher earnings growth even before entry into fatherhood. Most compelling are our results for twins, where we find that the conditional earnings effect of children for men post birth is insignificant. For other samples, we also find a relatively small yet still significant effect of having children. In summary, we conclude that it is not primarily the effect of children that makes fathers earn higher incomes, but that higher earners are more likely to become fathers.

The evidence in this paper adds to the debate about the sources of inequality among men and the gender wage gap. This research highlights that men who remain childless, and unmarried, are a negatively selected group on relatively low earnings profiles. This makes them a potentially higher risk group in the labor market more generally. Our results suggest that this group is less likely to be employed and less likely to be working long hours. One interesting question is whether these men are more likely to be unemployed, or on sickness leave. Regarding the gender wage gap, the conventional view is that having children has a negative effect on mothers' earnings and a positive effect on fathers' earnings. This suggests that, other things being equal, the redistribution of household time and time spent with children would potentially reduce the gender wage gap through a decrease in male work time. But our results reveal that the observed child premium for men is an upward biased estimate of the direct effect of fatherhood on men's earnings. Hence, redistributive policies at the household level are potentially less effective than would be expected from observed gender gaps.

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Conflict of interest The author declares that she has no conflict of interest.

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