The effect of children on earnings inequality among men

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Abstract

This study investigates empirically whether fatherhood has a causal effect on earnings inequality among men. Rich register data on life cycle employment, earnings and fertility histories on brothers and twins are used. We show that OLS estimates are confounded by the selection effects through the differences in entry earnings and returns to experience since first entry into the labour market, that is factors pre-birth, and family fixed factors. We show that higher earners are more likely to become a father, and not that children make fathers earn higher incomes. Men who remain childless and/or unmarried, are on relatively low earnings profiles and contribute therefore significantly to the earnings inequality among men.

Key words: children, earnings, men, inequality, selection, siblings, twins, panel data.
JEL Code: J220, J240, J310, J130, J160

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1 Introduction

Women traditionally take greater responsibility for rearing children and the general finding is that women's earnings drop when they have children. Part of this drop captures decreased labour supply post childbirth, through periods of leave or reduced hours of work, as well as the depreciation of human capital during leave periods. The unexplained part of the earnings drop that studies in this literature find, even after controlling for many productivity-related factors, is consistent with, for example, compensating earnings differentials if mothers trade more family friendly working conditions for earnings after childbirth.[1] Evidence on men's earnings and children is scarce, and existing evidence suggests that men's earnings increase after having children. This seems a paradox in light of the standard economic explanations applied in the literature on women. Traditionally, fathers do not adjust their labour supply to care for their children and therefore we would not expect an effect of children.

This study presents new evidence on the question whether fatherhood has a causal effect on earnings and earnings inequality over the life cycle for men. 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 results show non-random selection into fatherhood is captured through family fixed factors and relatively higher earnings growth even before becoming a father. We find that selection works through the event of first birth entirely, and that post-birth earnings variation is primarily driven by fatherhood rather than marriage. The results highlight that it is not having children that is driving inequality among men, but selection into the group of childless and never married men, versus fathers.

Costs and gains from having children are directly related to the demand for children and therefore knowledge on these are important. Costs through labor adjustments (of women) related to children are generally viewed as an important contributor to the gen-

[1] Other explanations are reduced work effort (Becker, 1985) and employer discrimination. For empirical studies, see e.g. Adda et al. (2015), Bertrand et al. (2010), Waldfogel (1998), Joshi et al. (1999), and Anderson et al. (2002) and Gupta and Smith (2002).

der wage gap, which policy makers try to diminish.[2] There is no consensus in the debate of reasons that lie behind the "wage premium" fathers get from children. The view in the recent sociological literature is that also conditional on a large range of observed characteristics the positive effect remains (Budig, 2013). This is plausibly related to positive discrimination by employers. 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. The results of our study suggest that a neglected confounding factor of the effect of fatherhood is the non-random selection into fatherhood. We show that part of the selection is captured by (fixed) family background and pre-birth employment history characteristics.

The debate on family-work balance is no longer only a topic on women and work. This leads to the increased interest to learn about the effects of fatherhood on work outcomes. As survey data shows, men have increased their weight on family values (see e.g. Goldin, 2006). Politicians in some countries set incentives through paternity leave policies for fathers to take leave from work. Some proponents also view father's increased involvement in child rearing as a premise of more gender equality (see e.g. Sandberg (2013) for an interesting discussion of the effects for high performing women). A new and growing literature sets the focus on understanding various aspects of the interaction between fathers and children such as effects on father's involvement (Rege and Solli, 2010, Rossin-Slater, 2013), father's peer behaviour (Dahl et al., 2014), child outcomes (Cools et al. 2015), and within household gender gap for couples with children (Angelov et al., 2016).[3] This strand of the literature focuses entirely on fathers and has not used men who remain childless as a potential control group or looked into the non-random selection into the group of men who become fathers.

[2]Statistics show that unadjusted male-female earnings differentials still remain significant, between 15 and 23 percent, and have remained surprisingly stable in many countries over recent decades. Blau and Kahn (2006) show the slowing down of convergence for the U.S. in the 1990s. For an international overview, see Tijdens and Van Klaveren (2012).

[3]Angelov et al. (2016) show for Sweden the earnings profile of men with children before and after first childbirth. They do, however, not estimate the effect of children, nor do they take account of non-random selection.

Considering the comparison of earnings for men with children and men without contributes to the understanding on within group inequality. Both in the political debate and the academic debate it has been noted that after decades of fighting women's relative under-performance and unequal treatment it has become an issue that men fall behind in some areas. For example, boys do worse at school than girls, and single men in the labour market are doing worse than married men (see for recent feature in The economist, 30 May 2015, and a recent research report by Autor et al., 2013). A key parameter measuring inequality is the effect of children when comparing childless men to fathers.

The primary goal of this study is to estimate the mean effect of fatherhood in a flexible earnings regression with family fixed effects that also accounts for selection into fatherhood. We use longitudinal population registry data for Norway on first and second born brothers within a family who can be followed from first entry into the labour market and across the most important part of the life cycle in terms of earnings growth and fertility. The effect of fatherhood is allowed to be non-linear in years of work experience since entry into fatherhood or the year of childbirth. The main benefit of the approach is that we can compare outcomes for men (brothers) who grew up in the same environment (family) and are genetically more similar than randomly selected men from the population. Therefore, the comparison of earnings between brothers, holding other characteristics constant, reduces the heterogeneity problem. While this exercise is in itself interesting from a descriptive perspective, it also potentially addresses some problems in the literature.

It is complicated to interpret correlations of children and earnings as a causal effect of children because parenthood might be endogenous with respect to earnings and correlated with unobserved factors. It is difficult to find credible instrumental variables for fertility that can address these potential problems. Instrumental variable estimation has been applied 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 delay of motherhood (Miller, 2011). A caveat of these estimates is that effects at particular parity may not be generalizable to other parities. Hence, it does not allow to test for non-linearity in the effect of children. The most common approach in the literature to estimate the mean effect of children has been to apply fixed-effects estimation exploiting longitudinal panel data following individuals over time.[4]

Novel to the literature, we estimate flexible earnings regressions allowing for differential returns to work experience before and after entry into fatherhood and between men who have a child at some point during their life cycle (later referred to as *fathers-at-some-point*) and those men who never have a child (later referred to as *childless men*). We discuss the problem of identification of the effect of children (post childbirth) and derive the earnings equation in a treatment framework, building on Heckman and Hotz (1989). It is challenging to fully account for the fact that, if the timing of fatherhood is anticipated, this may affect earnings and earnings growth even before entry into fatherhood. To our best knowledge none of the studies in the literature addresses this point. We address this problem in two ways. First, potentially self-selection into the group of fathers works through family fixed effects. In this case, family fixed factors are predictors of individual earnings levels and earnings growth.[5] Second, the effect of children is estimated after controlling for differential entry earnings and differential returns to work experience (squared) between fathers-at-some-point and childless men, those who never have children.[6] This approach controls for differences in earnings paths, namely if fathers started on different (higher) earnings paths than childless men, and potentially reduces the omitted variable problem.

Core to our study is that we observe for these birth cohorts complete employment and earnings histories from first entry into the labour market, the complete timing of births histories for every individual in the population as well as earnings before and after childbirths. In addition, we have intergenerational registers to match brothers. The data offers several advantages for our study and compared to previous studies. First, we have

[4]Twin births could also be exploited as a potential instrumental variable to estimate the effect of children, which would however only help to estimate the effect going from parity one to two.

[5]This resembles findings in the literature on the return to education showing that pre-market education predicts wages and wage growth.

[6]Hence, we do not need to rely on the assumption of common pre-childbirth trends.

data on fertility for men which is rarely available to researchers. Exceptions are studies using Scandinavian register data. (See for a detailed discussion in Tertilt, et al. (2015).) Second, our data set is large longitudinal population register data. The literature on the effect of children on earnings has relied on much smaller samples from representative surveys. Examples are studies that exploit genetically identical twins to estimate the marriage premium (Antonovics and Town, 2004; Krashinsky, 2004). In addition, a well-known problem in this literature is attenuation bias because of measurement error in survey data (Bound and Solon, 1999). Therefore, our registry data has advantages primarily since the sample size is large, the individual time series is long and they contain process collected information.

The remainder of the paper is organized as follows. Section 2 provides an overview of explanations of why having children may affect earnings of men. Section 3 presents background on the Norwegian labour market and institutions, and the description of the data and summary statistics. Section 4 discusses the selection problem and the econometric specification. Section 5 presents the empirical results, a number of robustness tests and a discussion. Section 6 concludes.

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 for men. If the mothers specialize more in home production, this can lead to an increased specialization of fathers in market production; particularly, if mothers also take over other household activities previously conducted by the partner because of economies of scale effects. The positive earnings effect of fathers can then be driven by increased effort, or 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 (Pencavel, 1986; Lundberg and Rose, 2002). An earnings increase can also be caused by preferential treatment by employers of fathers, or positive discrimination.

Another potential explanation is that earnings advantages of men with children 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 non-randomly 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 labour market after child birth, then it is optimal for them to already invest more into their career before they become fathers. One potential reason for why initial earnings and returns to experience may be relatively higher for men who become fathers at some point is that men who plan to become partners self-select into higher-track occupations (Gould, 2008).

A related, but different, question is whether cohabitation or marriage even before actually becoming a father explains the relatively higher earnings growth of fathers-at-some-point (Peters and Siow, 2002). It is related since in many countries the event of marriage and children are often close in timing and hence effects of those are difficult to distinguish. Albeit, 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 (Korenmark and Neuman, 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 suggests that gender-specific household specialization is not related to cohabitation or marriage, but rather to the presence of children and particularly to when more time is spent on child care (see Dribe and Stanfors, 2009; Hodges and Budig, 2010).

Evidence on the effect of having children on men's earnings and on earnings inequality among men is scarce in the economics literature. Yet, the fatherhood premium has been more extensively studied in the sociological literature. Most previous studies on the effect of children on men's earnings rely on individual fixed effects estimates, ranging between 3 and 10 per cent per year, varying somewhat depending on the country and model specification (see Lundberg and Rose (2000; 2002), Pencavel (1986), Waldfogel (1998), Killewald (2013), Glauber (2008), Hodges and Budig (2010) for the US, Blomquist and Hansson-Brusewitz (1990) for Sweden, and van Soest et al. (1990) for the Netherlands). Datta Gupta, et al. (2002) reported fixed-effects estimates of the effect of children ranging between 0.3 and 1.2 percent depending on age for Denmark. Related to our approach, Simonsen and Skipper (2010) exploit Danish data on a sample of twins in 2006, but they estimate more restricted models than we do and cannot distinguish childless men from not yet fathers. They find a significant wage premium for men. For Norway, Petersen et al. (2014) reported estimates of a 1 percent wage premium per child from employer-employee matched data controlling for occupation fixed effects on 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 (Loughran et al., 2009, Hodges and Budig, 2010, Hundley, 2000, Lundberg and Rose, 2002, Petersen et al., 2011, 2014).

3 Institutional settings and data

3.1 Institutional settings

The Norwegian labor market is characterized by centrally coordinated wage bargaining and high wage compression (see NOU 2008:6 and NOU 2012:15). Internationally, Norway ranks high in terms of gender equality and family friendliness during recent decades. Gender wage gap indicators show a quite stable difference of 15 percent in Norway during the previous two decades, which is low compared to Germany and the US (20-23 per cent), for example.[7] Male labor force participation is high and men typically work full-time, which is defined during recent periods as working 37.5 hours per week.

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 percent of fathers took leave, but almost 80 percent of mothers took the maximum amount.[8] Since 1993, the proportion of fathers taking up leave has steadily increased from an initial 30 percent to almost 60 percent in 1998. 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.

Compared to other countries, we would expect that the fatherhood effect in Norway is relatively small because of the relatively high wage compression and high female labour force participation.[9] However, the fatherhood effect may be relatively increased through factors that increase (gender specific) household specialization. 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 percent work overtime. Overtime is usually unpaid and in many sectors restricted. Overall, according to time use data gender specialization in the household among Norwegian couples is low. 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.

[7] These are the unadjusted gender wage gaps reported by Eurostat and the US Census for 2012.

[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.

[9] The female employment rate was 1990 (2009), 62.5 (68.8) per cent for women in Norway, compared to 57 (58) per cent in the U.S. Source: OECD.

3.2 Data description and summary statistics

The panel data for the population of sibling and twin men born between 1955 and 1965 is extracted from Norwegian registry data for the period from 1975 until 2005. We focus on these birth cohorts to ensure that we can observe the complete individual earnings and employment histories from first entry into the labour market, and complete fertility histories. The Norwegian multi-generational birth registry was used to match sibling and twin brothers to each other and their offspring. The sample of brothers and twins includes the first- and second-born son within a family with the same mother and father.[10] Fraternal and monozygotic twins are included but cannot be distinguished in the data.[11]

Pulling from a data set dating back to 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.[12] 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 and use two variables for years of experience. One that counts overall 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

[10]This means that we keep the main group but exclude sons from one-child families, as well as those from families with fewer than two boys.

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

[12]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.

data. We generate a variable measuring the birth order within the family to control in the earnings regressions for the birth order rank of each of the brothers that we compare.[13]

From the birth registry, we obtain the complete record of the timing of offspring and the complete number of offspring for every man, counted by 2005.[14] 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 effects before and after this year, the latter is referred to as the 'post-birth period.' For supplementary results, we also use the birth year of the second and third child and count the corresponding years of work experience post-second and third birth. Hence, we can test for the non-linearity of the earnings effect post-birth in number of children. Our 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. The group of men without any children in the birth registry are denoted as childless men. Men in this group never have children across the entire observation window, or the life cycle.

Approximately 20 per cent remain childless by the year 2005, according to the data. The oldest cohort is followed until they are 50 years old, and the youngest cohort until they are 40. National statistics show that the fraction of childless men only declines by 2 percentage points between the age of 40 and 45, and by 0.6 percentage points between 45 and 50. Thus, given the very large sample we have 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 become fathers first time after 2005 is negligible.[15]

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

[14]One birth cohort is around 60,000 in Norway. The birth registry is complete in order to study the effect of children on earnings, when considering all fathers that 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 we cannot observe are those fathers not reported, for example, because the mother does not want to but then fathers have no contact to mother and child. During the observation period, only 400-500 children were adopted per year and we have no information about those.

[15]The distribution of the number of children in our sample is reported in the Appendix in Table A1.

We use information on marital status to restrict the comparison group of childless men, that is men who never become a father during their lifetime, to men who are childless but married-at-some-point.[16] 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.[17] 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 at some point (until 2005). We define the indicator variable married-at-some-point as equal to one if a man is ever reported as married, divorced or separated, and zero otherwise. In order to disentangle whether earnings increases are related to children[18] or marriage, we also construct a control variable based on the same information concerning whether a man is married in a given year. Hence, for fathers-at-some-point, we can control for whether the couple is married. For childless married-at-some-point men, we can control for potential changes in earnings after the time of marriage.

Tables 1 and 2 here

Tables 1 and 2 report the sample means and standard deviations for the main variables separately for fathers-at-some-point, the comparison group childless men and the restricted comparison group of childless men married-at-some-point. We pool all observations across the entire observation period. The unconditional difference in mean log earnings between fathers and childless men is 17 per cent for the sample of brothers and 15 per cent for the sample of twins. 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 in 1988.

[16]We do not have access to information on cohabitation for men without children. Hence, we may exclude too many men by this rule.

[17]In our empirical analysis, we assume that this is not due to health problems. Health information is not available in our data.

[18]The father is reported on the birth certificate if he is married to or cohabiting with the mother.

Figure 1 here

Figure 1 describes the earnings paths for fathers-at-some point around childbirth in comparison to childless men. For illustration, earnings are in this figure predicted for a man who is continuously working for 5 years, then becomes a father, and is working continuously afterwards (Father asp (at some point) OLS). For comparison, the earnings path for a childless man is plotted across work experience (Childless OLS). The graph is based on the coefficients estimated from a flexible log earnings regression estimated by simple ordinary least squares, which we will return to in more detail (Estimation results are 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 the working career and the difference increases with work experience starting from close to zero at entry. Hence, we observe earnings diferences even before men actually 'reveal' having children, or not. Second, for men in the father-at-some-point group earnings seem to increase at the time of having their first child. After 18 to 20 years the earnings paths tend to decline and converge. We focus on the pattern during the first 10-15 years.

4 Empirical Framework

4.1 The selection problem and the model

In the following we discuss the selection into fatherhood problem and the derivation of the earnings equation in a treatment framework. The basic framework builds on Heckman and Hotz (1989) that investigated the return to training using non-experimental data. The selection problem in their study is that those who enter training, the treated, are different in terms of labour market characteristics before treatment compared to the non-treated, those who do not enter training. In our data we see that fathers are on different earnings paths from the beginning of their career in comparison to childless (across the entire life cycle) men. In extension to their model we allow for an unobserved family fixed component in the error term of the earnings equation.

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 which we return to 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. We assume that the effect of fatherhood is identical for all persons.[19] The period of childbirth is denoted as k. Then we can write:

$$lny_{ift} = lny^*_{ift} + a_{it}\gamma, \quad t > k \tag{1}$$
$$lny_{ift} = lny^*_{ift}, \quad t <= k$$

We focus on the estimation of the mean effect, and the difference in mean post-birth earnings of fathers and non-fathers can be written as:

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

The expression in parentheses 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_{it}.

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

Then observed earnings may 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 $E(\epsilon_{it}X_{it}) = 0$ for all i and t.

[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 non-random selection into work is not important and we can neglect this issue. To incorporate women with more disruptive careers would demand further assumptions.

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:

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

Z_{it} may include all of the variables in X_{it}. Then, the ith individual's fatherhood status is

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

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

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

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

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

It includes the indicator variable whether the man is a father-at-some point, $fathertype_i$, or not, and the indicator interaction with years of experience (squared) since first entry into the labour market, ex_{it}. Hence, crucial is the fact that in the data we observe complete earnings histories from first entry into the labour market, and that we can distinguish

[21] Clearly, 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.

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 in a_{it} and the years of post-birth work experience, ex_{it}^{post}. This is to capture potential time varying costs of children, or effects through further children. Later we test whether the second or third childbirth has a significant different effect on earnings profiles than the first childbirth.

$$\gamma(a_{it}, ex_{it}^{post}) = \gamma_1 1(a=1)_{it} + \gamma_2 1(a=1)_{it} * (ex^{post})_{it} + \gamma_3 1(a=1)_{it} * (ex^{post^2})_{it}.$$

where the first term a_{it} is a pure shift parameter that is an estimate of the change in earnings from the time of first entry into fatherhood, and the second and third term capture the curvature of earnings post entry into fatherhood. If the estimates of γ_2 and γ_3 are not significant then there is only a constant shift of earnings post-childbirth.

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:

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

The error term contains three components:

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

that is an unobserved family fixed component, ν_f, capturing genetically inherited ability[24]; an individual varying and family-varying unobserved component, μ_{if}, capturing

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

[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).

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 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 equation (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 Family fixed effects estimation

The key parameter vector is the mean effect of having children, γ, which we is a vector of three parameters: the shift right after childbirth, and the squared polynomial term in years of work experience post childbirth. Most of the studies on the effect of fatherhood have estimated a more restrictive log earnings equation than equation (1) by simple ordinary least squares (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 child birth. As a baseline estimate, we present OLS estimates of equation (10). Note, however, the concern is then that the effect of children (treatment) is not consistently estimated only by accounting for selection on observables through the variables interacted with father-at-some-point.

We present fixed effects (FE) estimation results exploiting the individual panel structure of the data and within individual variation. FE exploits that we observe earnings for an individual before and after entry into fatherhood and exploits yearly changes in the time series of switchers. If omitted variable bias is captured by (unobserved) individual specific factors then this source is swept out by FE. FE is a consistent estimate of γ if the dummy variable for having children, a_{it}, and the common shock, w_{ift}, conditional on the remaining controls are uncorrelated. A concern with this type of model is that

[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 only be relaxed in case of a valid instrumental variable.

estimates are biased if past earnings affect current fatherhood status. Since individual fixed effects (FE) sweep out all time constant variables, it cannot directly identify the differential effect of entry earnings, between the group father-at-some point and childless men.

The main estimation results employ the covariance estimator (CV) (Bound and Solon, 1999) which applies ordinary least squares 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. Hence, it exploits the cross-sectional variation for identification. This can be viewed as an alternative way to address selection on unobservables and potentially addresses some of the caveats of FE. If we write down the full regression equation (10) for the first born brother in family f' and the second brother in family f' and 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 subscript 2) in family f' from the variable of the first-born brother (indicated by the subscript 2) in family f'. If we assume $\mu_{1f'} = \mu_{2f'}$[26], then the transformed regression can be written as:

$$\begin{aligned}(lny_{1f't} - lny_{2f't}) &= \\ \gamma_1(1(a=1)_{1t} - 1(a=1)_{2t}) + \\ \gamma_2(1(a=1)_{1t} * (ex^{post})_{1t} - 1(a=1)_{2t} * (ex^{post})_{2t}) + \\ \gamma_3(1(a=1)_{1t} * (ex^{post2})_{1t} - 1(a=1)_{2t} * (ex^{post2})_{2t}) + \\ (X_{1t} - X_{2t})\beta + (Z_{1t} - Z_{2t})\delta + (w_{1f't} - w_{2f't})\end{aligned} \tag{12}$$

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

[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 (equation (12)), observing that serial autocorrelation remains, yet is small. The results are available on request.

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 the data in which we observe the complete employment, earnings and fertility histories. We also need that a_{it} and Z_{it} are uncorrelated with w_{ift}.

Our approach arguably 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, 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, or potentially remove it. We show compelling evidence that this is the case and taking out heterogeneity related to family fixed effects does reduce the bias.

Our statistics clearly show that fertility is more highly correlated between brothers and even more so for twins, than between randomly selected men. Hence, the family fixed effect is a potential contaminating factor. In order to reduce 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 the reduction of within brother couple differences in age, we may also reduce differences in family environment. That is we then compare outcomes for brothers whose parents are in more similar career phases, may have more similar time and monetary resources or have more similar experience duration in parenting. Differences in these characteristics arguably increase between brothers who are more different in age. Note that due to the large data sample we are able to run separate regressions on sub groups of brother pairs reducing the age difference, that is from 3.5 years at the mean in our sample, to zero; when we use only twins. Using twins offers the advantage that we directly control for family fixed factors, time fixed factors and individual fixed factors.

[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.

5 Empirical Results

5.1 Does having children affect earnings of men?

Table 3 here

Table 3 reports the estimation results of the main earnings regression in equation (10). We report OLS, FE and CV (covariance estimator) estimation results. 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) for the control group which are men who remain childless all through their lives (childless men). The next panel of coefficients reports the differential effect in entry earnings, that is at the first entry into the labour market, and the differential return to experience (squared) between the father-at-some-point and childless men (middle panel). The following three coefficients reported in the lower panel are the key coefficients of interest. These are the estimates of the post-childbirth effect on earnings. The OLS estimate uses the sample where we just pool all observations (column 1) and the results can be interpreted as a description of the earnings profiles of the group childless men, and the group father-at-some-point before and after entry into fatherhood. The parameter estimates of the return to education is positive. The return to years of work experience for the comparison group of childless men is 6.9 percent in the first year and afterwards declining.

The differential effects in entry earnings and years of work experience since first entry into the labour market between fathers-at-some-point and the group of childless men reported in the middle panel of Table 3 are significant and positive. Hence, the findings suggest that fathers-at-some-point start on higher earnings growth paths than childless men. The immediate shift in earnings post-birth is 0.073 which gives a 7.3 percent increase in earnings compared to before entry into fatherhood. The differential effect in experience post-birth is slightly non-linear. For illustration, the predicated mean profiles in earnings from the OLS estimates are plotted in the beforementioned Figure 1 for the groups childless men (Childless OLS) and father-at-some-point (Fathers asp OLS) where we assume a specific employment history as detailed in the note to the figure.

When we estimate the model by FE (column 2), the differential effect of children declines in comparison to OLS at any value of years of post-child work experience. This we can see when we do the calculations using the FE coefficient estimates of the shift parameter coefficient (post-birth), 0.048, and the experience post-birth (square) parameters, -0.017 and 0.00046. From the table, we see that the post-birth variable estimate, that is the shift parameter from childbirth onwards, decreases by more than a third, (0.04816/0.07286). The marginal effect of experience post-birth estimated by FE is negative at low levels of experience. This implies that the profile post-birth declines at a relatively fast rate. 5 years after childbirth the loss is 5 percent estimated by FE compared to before childbirth. OLS predicts an approximately 3 percent increase in earnings.

Table 3 column (3) reports the first results from the covariance estimator (CV)[28] which uses the same sample as for OLS and FE. This means that we have half the number of observations because CV uses variables in differences between brothers. The estimated 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 shift right after childbirth is 6.4 percent. The curvature in experience post-birth reveals that the marginal effect in experience post-birth is almost zero. In comparison to OLS, the CV estimate reveals therefore a relatively smaller increase in earnings post-birth at any level of experience post-birth. Both FE and CV suggest that positive selection on unobserved factors biases OLS upwards. For illustration, the model estimates show that 13 percent (CV), that is $(0.064/0.072) * 100$, to 34 percent (FE), that is $(0.048/0.072) * 100$, of the simple OLS estimated shift effect of post-birth, that is immediately or less than one year after childbirth, is due to positive selection on fixed family-specific and individual specific unobserved factors. Selectivity on fixed family-specific factors appears significant but relatively small when we use the entire sample of brothers.

Siblings are genetically more similar than randomly selected men. Still, siblings might

[28]Summary statistics for the variations in between-sibling differences are reported in Appendix Table A2, showing that there is still considerable variation in the variables in differences between siblings and twins.

be quite heterogeneous in terms of family background, which may introduce bias and thus make family fixed factors appear less important in our CV estimation results in column (3). In order to control for potential differences, such as differences in parenting each of the brothers experienced by their parent and age differences between siblings, we re-estimate the regressions using sub-samples of brothers. The estimation results are reported in the following columns (4) to (8) of Table 3, where we gradually decrease the age differences in the sample of brothers; to two years or less, column (4), one year or less, column (5), to zero where we use the sample of twins , column (8). (Columns (6) and (7) are presented to show that we can replicate the results from OLS and FE 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 (4), we reduce the sample size by more than half. The shift effect on earnings after childbirth is now 4 per cent and still highly significant. The differential effect in experience post-birth is economically negligible and only the coefficient of the experience post-birth squared variable is significant. Hence, we find a positive and constant shift in earnings post-childbirth of 4 percent; that is at any level of post-birth experience. This estimate is substantially smaller than the post-birth estimate from CV when we used all brother couples. This is true at any level of experience post-birth. Recall that we estimate the effect post-birth conditional on actual work experience so that age differences between brothers capture additional factors, which we interpret as family background factors.

When we reduce the age differences between brothers further to less than two years, column (5), the results on the post-childbirth effect remain robust. Note that the sample size is reduced. We see now even more clearly, the effect post-birth is only an upward shift at birth in earnings of 4 percent. Hence, there is no further adjustment years after first child-birth. This may be surprising given that most have a second or further child.

When we use the sample of twin brothers, see column (8), age differences between brothers are completely removed and the effect post-birth declines even further. As we see, the coefficients of all of the post-birth variables are now not statistically and economically significant. Note even though the coefficient of experience post-birth squared is statistically significant, the coefficient estimate is economically very small and negligible, 0.00043. The point estimate of the shift in earnings post-birth is 2 per cent per year, but is not significant. The F-test (not reported in the table) shows that estimates from brothers one year different in age, or less, and twins are jointly significantly different (Table 3 column 5 compared to column 8).

From all of the estimation results, we see that the differential effect in entry earnings 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, column (5) and column (8) respectively. When we use brothers who are different in age, the entry earnings and return to experience are significantly larger for fathers-at-some-point. But when we use twins, differences in earnings are only significant at first entry into the labour market. Hence, men who remain childless all their lives and fathers-at-some-point are on the same earnings profile except for that those who become fathers at some point in life start on relatively higher pay.

We explore further factors that may explain differences in the earnings profiles from first entry into the labour market between the fathers-at-some-point and others, and which may also be positively correlated with the effect of children post-birth. First, we change control group and now use childless men married-at-some-point as an alternative comparison group instead of all childless men. Intuition is that those who marry at some point but remain childless may also had plans to have children, but did not realize those for reasons uncorrelated with labour market behaviour. Since all childless men we used so far may be a quite heterogeneous group, restricting the childless men to those also married-at-some-point makes the comparison group more homogeneous and more comparable to the group fathers-at-some-point.[29] We show that men in the group fathers-at-some-point are more similar with respect to years of education and work experience to childless men married-at-some-point than to all childless men. In Table 4, we report the corresponding estimation results of the earnings regression only for the brother samples.[30]

[29] This could already be seen from our summary statistics reported in Table 1

[30] The twin sample would become too small for estimation when we further restrict the group of childless

Table 4 here

Table 4 columns (1) and (3) report selected estimation results on the differential effects using now the restricted comparison group. The differential effect in entry earnings and experience between fathers-at-some-point and the restricted group of childless men married-at-some-point that we report in the upper panel of the table are now much smaller than those reported before in Table 3, columns 3 and 5. In column (1) of Table 4, the estimated difference between mean entry earnings is now not significant, and the differences in slope coefficients are very small, still significant however. For going from zero to one year of work experience after entry into the labour market the differential return between the two groups of men is now only 0.6 percent compared to the previous estimated return of 6 percent. When we restrict the age difference between brothers to less than two years, then the differential effects since entry into the labour market become not significant and economically close to zero (Table 4, column 3). This suggests that the large differences between those who are not fathers yet and childless men, as found in the previous estimates reported in Table 3, are to a large extent driven by childless men who are never married. This is because this group is performing relatively worse in the labour market. Regarding our main results, however, the size of the coefficient estimates post birth, that is the coefficient of the shift and the experience post-birth variables, it is noticeable that they do not change significantly compared to our main results in Table 3 columns (2) and (5).

Table 4 here

Returning to our main results from table 3, the CV estimation results clearly show a decrease in the post-child effect as we narrow down age differences between siblings. To illustrate, when we compare the estimates using all brothers to the estimates restricting the age differences between brothers to less than two years the effect one year after childbirth decreases by 72 percent.[31] When we eliminate all age differences by use of

men, 1515 observations or approx. 700 twin couples. Summary statistics are reported in Table 2, column 3.

[31] $(= ((0.04096 + 0.00134 + 0.00011)/(0.064420.00591 + 0.00033) * 100)))$

twins the point estimate of the effect one year after childbirth declines by an additional 46 percent.[32] Taking (non)significance into account the latter decline is 100 percent. The estimate using differences in variables between twin brothers also accounts for time fixed effects, family fixed effects, and individual fixed effects. Note also that the mean pair is now genetically more similar, because some twins in the sample are monozygotic and hence genetically identical at birth which motivates the assumption that we can control for individual fixed effects.[33]

Testing whether the effect post first birth captures marriage

Returning to the results for brothers (Table 4, columns 1 and 3), we also test 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 the indicator variable switching to one when a man is actually married. We re-estimate these regressions using the samples of brothers (excluding twins) and the restricted comparison group. Note that in Norway the typical timing of marriage pattern is to marry after becoming a father. As Figure 2 shows, approximately 3 out of 4 couples get married close to the time of first childbirth or later in our sample.

Figure 2 here

Table 4 columns 2 and 4 show that adding a control for being married to our previous specifications slightly reduces the size of the effect on earnings post-birth. For brothers who are only one year different in age (Table 4 column 4), the effect of children now is 3.2 percent and economically constant. Hence, the effect is reduced by 28 percent (=1-(0.032/0.044)*100)).[34] These results confirm that the greater part of the estimated positive effect of having children remains after we account for marriage, The relatively small effect of marriage can also be seen from Figure 3, where the simulated earnings profiles of a hypothetical father-at-some-point is plotted, comparing the two estimates

[32] $(= ((0.01978 - 0.00029 + 0.00043)/(0.04096 + 0.00134 + 0.00011) * 100))$

[33] 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.

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

with and without a control for being married.

Figure 3 here

The post-birth effect could be the joint effect of entry into fatherhood and cohabitation, since men not married at childbirth may be cohabiting. We estimated that the only significant difference in conditional earnings between these groups is post-first childbirth, we find that from first entry into the labour market fathers-at-some-point are on the same earnings paths as the restricted group of childless men married-at-some-point. Hence, even if some men in the father-at-some-point group already cohabit before having children, it is noteworthy that we do not observe differential effects from first entry into the labour market - or before childbirth. So we seem not to observe indication of, for example, gender biased household specialization before entry into fatherhood. This pattern is in line with recent time use data for Norway showing almost no gender differences in hours of market work and household work among couples without children. Gender differences are only observed among couples with children younger than 6 years. (We report the numbers in Appendix Table A4 for 2010. Earlier figures are not reported since they are not available for couples without children.).

Testing whether the effect post birth captures second or further births

It is also possible that the effects at entry into fatherhood or after the first childbirth capture the effects of second or further births. To test this hypothesis we therefore extend our previous specifications and add dummy variables for second and third births to our preferred model, as well as the corresponding interaction terms with years of experience (squared). We re-estimated the model on the same sample and our preferred specification used in Table 4 column 3. Note, we keep the restricted control group. As can be seen from Appendix Table A3 columns (1) and (2), the differential effect at entry into the labour market does not vary significantly across parity compared to childless men married at some point. The shift in earnings post first childbirth remains significant and robust in size. The marginal effects post-second or third childbirth are all not significant; that is both the shift and the return to post-childbirth years of experience. This result highlights that the event of (first) fatherhood is important for earnings and not the number of children.

5.2 Robustness tests

Our study uses that fertility choices and fixed family factors are correlated. In the literature, it has been widely shown that educational choices taken relatively early in life are highly correlated with family fixed factors, as well as highly correlated between siblings and twins (e.g. Ashenfelter and Rouse, 1998). The correlation in fertility may capture intergenerational transmission of fertility or cultural values (see Booth, et al., 2009; and Fernandez et al., 2006) As Table 5 demonstrates, fertility outcomes in our data are significantly correlated between siblings and correlation coefficients are quite large, between 11 per cent and 24 per cent. The correlation between two randomly selected men from the population however is zero. By comparison, the correlation in years of education for twins is 51 percent and hence, as expected, higher.

Table 5 here

Identification applying the covariance estimator (CV) depends on sibling pairs where one brother has children and the other does not. At the mean in our sample for brothers, 27.94 per cent of all siblings have the combination 'no children' and 'children', whereas 73.06 percent of brothers either both have children or both have none. For twins, the corresponding values are 25.73 and 74.27. The panel of graphs in Figure 4 plots the percentage of pairs for whom both brothers have children or neither has children separately across years, years of education and years of experience. The proportions are constant across years. Hence, the inverse or the percentage of sibling couples different in fertility is also constant and, hence, this pattern suggests that there are no corresponding correlated factors violating our estimation strategy.

Figure 4 here

It is possible that sibling pairs that identify the effect of having children in the family fixed effects model estimates are different from the random sibling pair in our total sample, which could bias the results. In order to explore this possibility, we re-estimate the basic specifications from Table 3 (OLS and CV with brothers less than 2 years age difference) using only couples with unequal fertility outcomes. Table 6 presents means and standard deviations for the sample where only one sibling has children and the other does not (unequal fertility outcomes). The means of the main characteristics are very similar to those of the entire sample, reported in Table 1. We also investigated whether fertility patterns and their correlations with education are different for the total sample and the restricted sample of sibling pairs. As Figure 5 shows, the patterns are in fact very similar at all levels of education, except at very high levels of education where observations are few.

Figure 5 here

Table 6 here

The corresponding regression results are reported in Table 7 and tend to confirm that our previous results are not driven by sample composition. However, we note a downward shift in the levels of the estimate of the post-birth effect; both the OLS estimate of the effect post-birth and the CV estimate are lower than the estimates reported in Table 3. The shift post-childbirth now is 1.2 percent and not significant, but increasing by 0.5 percent per year. Since these results are not very far from our previous results, we exclude compositional effects as a posible explanation of our findings.

Table 7 here

The estimates of the effect on earnings post-birth by FE and CV both indicate an upward bias of ordinary least squares, already shown in Table 3. However, the point estimates at given levels of experience post-birth are quite different. One reason might be that the FE and CV transformations of the main equation sweep out different sources of variation. The estimates may represent complementary findings. The FE model sweeps out all time-constant unobserved and observed variables using the individual panel; variables are demeaned which potentially introduces other endogeneity issues. The CV model takes differences cross-sectionally between siblings in every period of the life-cycle and then applies OLS, conditional on the set of controls. We consider an advantage over individual fixed effects that CV exploits the cross-section variation, avoiding the introduction of time-series correlation.

Testing robustness of the results across time

In order to test robustness of our main results across time within Norway, we exploit the parental leave policy reform in 1993 that introduced paternity leave for the first time. From 1993, four weeks of paid parental leave were reserved to fathers, which led to the effect that some fathers interrupted work for one month. The reform applied to parents of children born after 1 April 1993. Since we rely on yearly data we assume births post 1993 to be eligible.[35] In order to ensure that potential negative earnings effects through work interruptions related to becoming a father do not affect our findings, hence lead to downward bias of our estimates, we estimate the regressions on a restricted sample. The restriction on the full sample is that earnings are dropped from the individual time series if the earnings are post childbirth and the childbirth was after 1993. Hence, we will keep all individuals in the data panel, and the restriction is primarily on the individual time series of earnings. The replication of Table 3 on the restricted sample is reported in Table 8.

Table 8 here

The descriptive estimate (OLS) in Table 8 column 1 replicates the pattern we have shown before. Men who become fathers at some point are on higher earnings paths already before entering fatherhood. Post-childbirth earnings increase non-linearly. If we estimate the model by the CV estimator and use brothers excluding twins we see that the effect post-childbirth is smaller than from OLS. As we decrease the age differences 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 percent and economically constant across years of work experience post-birth. A statistical test shows that 3.2 percent is not significantly different form 1.9 percent estimated on the full sample of twins (Table

[35] Since we will drop yearly 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 for births in April 1993 in 1994.

3, column 8). So we may interpret these estimates as upper and lower bound estimates. As before, we also see that earnings paths from first entry into the labour market are not significantly different except for that entry wages are higher for those in the father-at-some-point group than for childless men.

A more careful look at the estimation results for the post-birth period (lower panel) reported in Table 8 and Table 3 reveals that Table 3 shows systematically larger effects post-child birth than Table 8. This could be a reform effect interpreted as an intention to treat effect. Alternatively, it may capture other trends, such as a decrease of gender specific household specialization that happens after having children; in order to explain the pattern they must happen on a more permanent basis rather than adjustments during the period when the child is very young. We can also show that changes follow first entry into fatherhood, and not after each child birth in case of further children (see Column 3 in Table A3).

In the appendix Table A5 we present a final robustness test providing more evidence on the question whether temporary labour supply adjustments of fathers are one mechanism explaining the positive point estimates after child birth in our data. We re-estimate our empirical model on the sample of brothers where we now replace the outcome variable by a measure of labour 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 employed and work less hours 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 non-significant. An exception is the economically small increase, 0.004, in the probability to work more than 30 hours during the 3 to 4 years following entry into fatherhood (column 4). Overall, these results present little support for the hypothesis.

5.3 Discussion of the results

Our approach addresses the potential problem in the literature that estimates of the effect of children do not account for potential differences that occur already before some men enter fatherhood. Hence, our approach does not rely on the assumption of parallel trends. In the literature, little attention has been paid to the selection process. Our approach is novel to exploit the complete work history on first and second born brothers within a family. Doing this, we find that men who remain unmarried and childless are a selected group contributing to the observed earnings inequality among men. This finding is consistent with findings for Sweden, for example, showing that single men earn less than fathers (Boschini, et al., 2011). We also find that earnings inequality is only potentially affected by the event of the first child. This result contrasts findings from the literature on women showing that the shift effect of children on earnings post-birth is negative and increasing in the number of children (Waldfogel, 1998).

Furthermore, and perhaps the most important finding is that the effect of entry into fatherhood is not significant when we estimate the flexible earnings regression by the covariance estimator on the sample of twins across the entire observation window (see Table 3, column 8). Tests of robustness across time show that the positive shift in earnings post-birth has slightly declined following the paternity leave reform in 1993. The estimation results show that an upper bound estimate during earlier periods of the shift effect in earnings post-birth is 3.1 percent per year which is however no significantly different from the lower bound estimate of 1.9 percent per year when we use the entire sample. The relatively small decline seems though consistent with a study by Rege and Solli (2013) that focuses on the evaluation of the reform effects for fathers.

We also show that most of the variation in earnings comes from children, and only a minor part through marriage. Hence, contrary to the literature on the marriage premium that finds a premium up to 40 percent, we do not find strong effects of marriage. This seems however in line with other Scandinavian studies, such as for Sweden (Richardson, 2000). 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, Gupta et al. (2007) estimated a marriage premium of 1.2 percent and a positive effect of 0.9 percent per year for having children younger than 3 years. Hence, the Danish study suggests temporary earnings effects post-childbirth which we do not confirm by our findings. Temporary adjustments after child birth could be related to gender specific household specialization during the child care intensive infant period. Our results indicate instead more permanent shifts, if at all.

Our research results broaden the focus on the understanding of sources of inequality among men. The results highlight that differences in earnings profiles between childless men and fathers-at-some point are increasing from the beginning of the working career. The point estimates show that the event of having children contributes little, or even not at all, to a further increase of this difference.

Our results show strong evidence that family background plays an important role for our understanding of the observed child premium for men.[36] Within family estimates are much lower than between family estimated effects. This implies that individuals with relatively high values of the (unobserved) family-specific factor in our model are more likely to become fathers. Our estimates give an upward bias of 28 percent.[37]

This research suggests the general hypothesis that selection on family background into fatherhood is important. It would be interesting to test this hypothesis with other data sets for other countries with different institutions. In addition, questions arise regarding the direction of the selection bias and whether it varies, for example, across countries. The direction of the bias is informative when we think of tax income from families in order to finance family policies. Our findings also raises questions why family background is important. In order to answer this question we would need more research on potential mechanisms. For example, explanations could be directly related to the social environment that a family creates within the family, social values or the neighbourhood. The analyses would demand richer data than in our study.

[36] However, our results results suggest that studies controlling for family background but not conditioning on pre-birth histories are likely to suffer from upward bias. For example, Simonsen and Skipper (2010) show for Denmark a positive wage child premium for men when the exploit between twin differences.

[37] See calculations in footnote 31.

6 Conclusions

This study reconsiders the question 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 analyses employs novel data on the population of siblings that is drawn from longitudinal Norwegian registry data and contains the complete employment, earnings and fertility histories for brothers and 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 the earnings regression results 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 considerable 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 detailed we control for family fixed effects. The large data set allows us to control for family fixed factors by use of differences between first and second born siblings and by limiting the age differences between the siblings up to zero when we rely on the use of 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 effect of children for men post-birth becomes not significant. 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 select group on relatively low earnings profiles. This makes this a group potentially of higher risks in the labour market more generally. For example, a question is whether these men are more likely unemployed, or on sickness leave. The conventional view is that having children has a negative effect on mothers' earnings and a positive effect on fathers' earnings, which suggests that, all other things being equal, the redistribution of household time and time spent with children would potentially reduce the gender wage gap through a decrease of the premium to men. The results in this study highlight 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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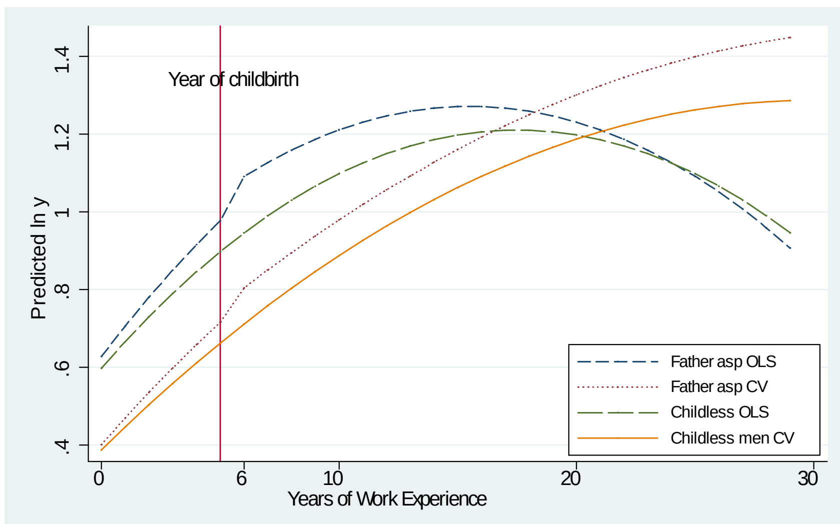

Figure 1: Predicted earnings profiles for a man who is continuously working for 5 years since first entry into the labour market, is then entering fatherhood and continues working. Childless men are continuously working from first entry. Predictions use the estimates reported in Table 3 Column 1 and Table 4 Column 4.

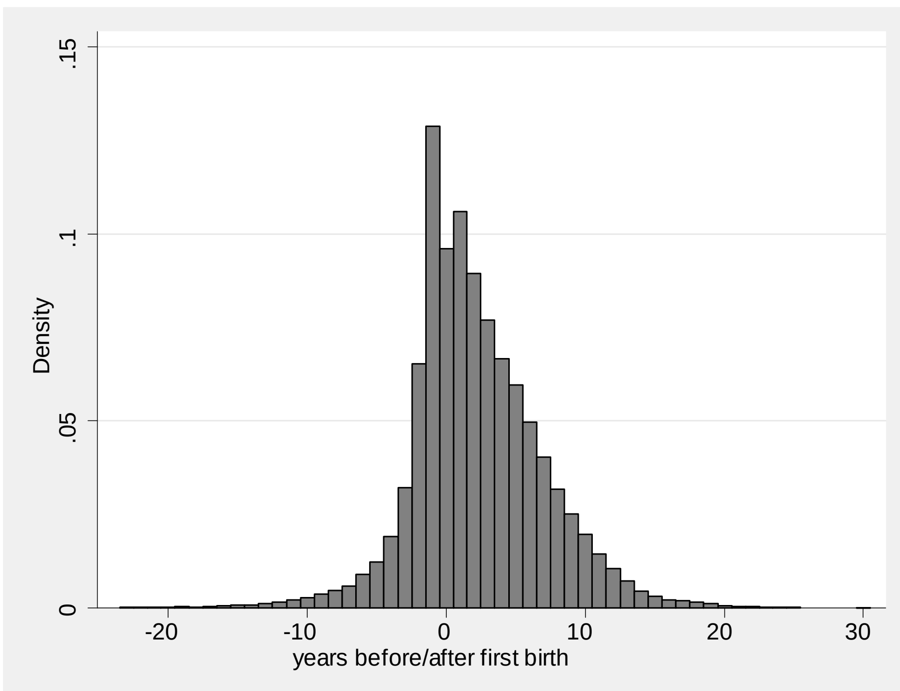

Figure 2: Timing of marriage and first childbirth, own calculations using the sample of all brothers followed from 1986 to 2005.

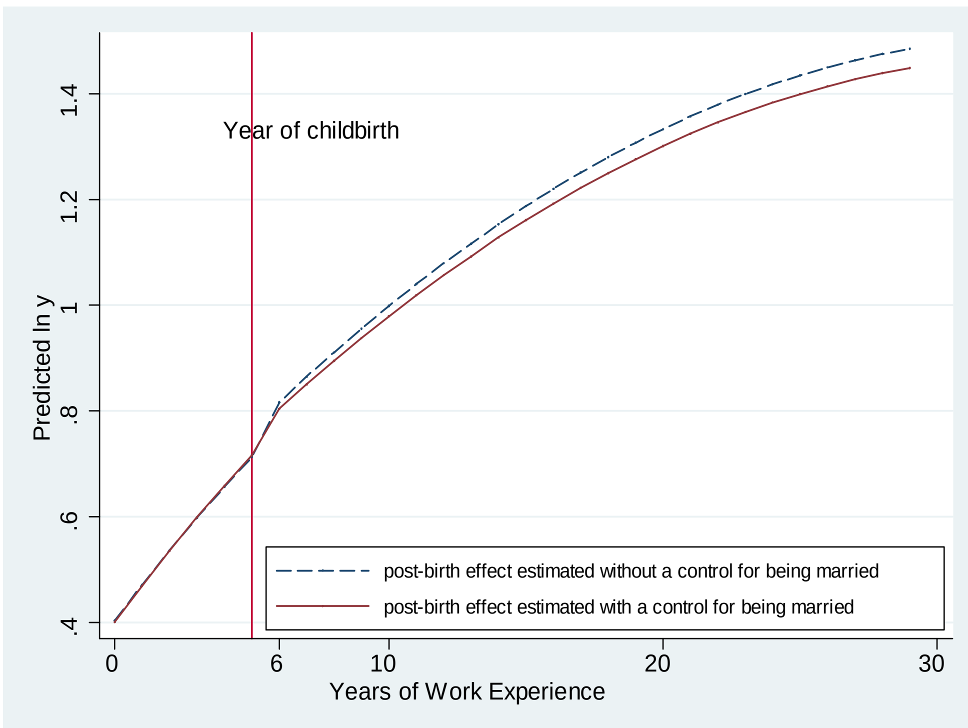

Figure 3: Predicted earnings using estimates of the post-birth effect without (Tab. 4 Col. 3) and with a control (Tab. 4 Col. 4) for being married Note: For illustration a man is used who is continuously working for 5 years since first entry into the labour market, is then entering fatherhood and continues working until 30 years of work experience are completed.

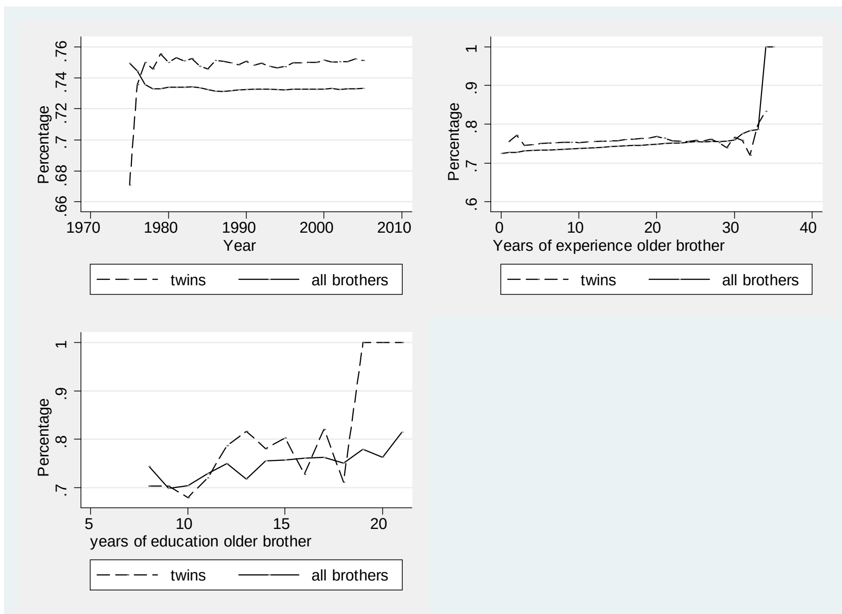

Figure 4: Percentage of brother couples where both of them have children, or both of them have no children. The inverse gives the percentage of brother couples with unequal fertility outcome.

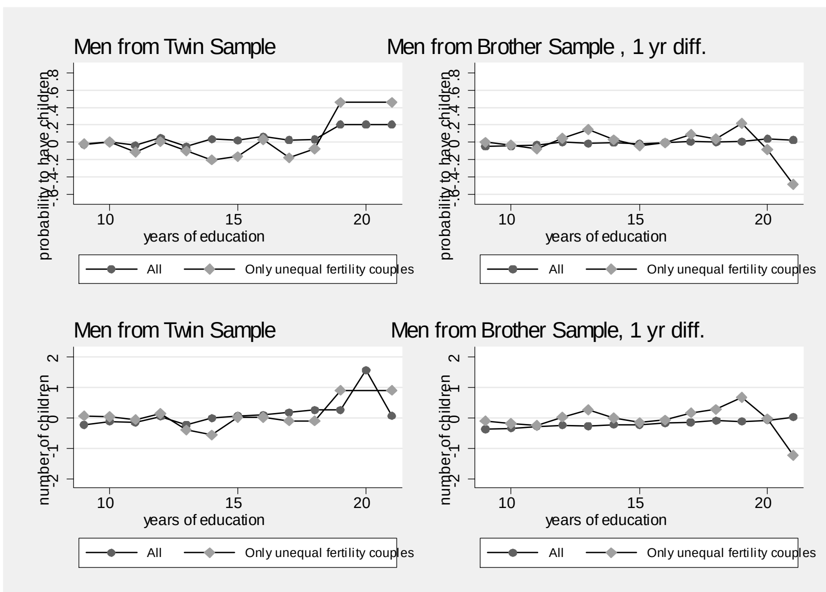

Figure 5: Testing correlation between fertility outcomes and years of education - the normalized graph is shown.

Table 1: Descriptive statistics for sibling brothers: Means and standard deviations

	fathers-at-some-point		childless men		childless men married-at-some-point	
	mean	sd	mean	sd.	mean	sd.
log(earnings)	12.42	.52	12.25	.52	12.33	.51
real annual earnings (1000 Nkr)	267.3	271.3	219.8	153.6	237.5	154.0
yrs 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*	30.32	4.41	34.66	6.00	34.47	6.01
age at first birth	28.35	5.47	.	.	.	.
number of children	2.38	.95	0	0	0	0
year first job	1982	2.81	1982	2.96	1980	3.30
yrs of experience	13.57	7.13	12.64	7.07	13.53	7.20
yrs 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	1461807		272249		51351	

Data: Norwegian register data 1975 until 2005. * available since 1986.

Pooled sample of first and second born brothers, excluding twin brothers, born between 1955-65.

In total 1,734,056 observations and 45345 sibling pairs.

Table 2: Descriptive statistics for twin brothers: Means and standard deviations

	fathers-at-some-point		childless men		childless men married-at-some-point	
	mean	sd.	mean	sd.	mean	sd.
log(earnings)	12.38	.51	12.23	.52	12.35	.45
real annual earnings (1000 Nkr)	250.4	192.4	211.2	146.8	228.5	147.7
yrs 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	.98	0	0	0	0
year first job	1980	3.57	1981	3.79	1979	3.14
yrs of experience	14.25	7.45	13.25	7.30	14.69	7.63
yrs 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	36218		8230		1515	

Data: Norwegian register data until 2005. * available since 1986.

Pooled sample of first and second born twin brothers born between 1955-65.

In total 44448 observations and 1069 twin pairs.

Table 3: The effect of children on log earnings: OLS, FE and the family fixed effect (CV) estimation results

	Sample of Brothers excl. Twins					Sample of Twins		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	FE	CV	CV	CV	OLS	FE	CV
		extended	All	<3 yr age diff	<2 yr 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***	0.07280***	0.06840***	0.06294***	0.06012***	0.06994***	0.07040***	0.05923***
	(0.00085)	(0.00056)	(0.00121)	(0.00215)	(0.00359)	(0.00547)	(0.00345)	(0.00929)
experience^2	-0.00162***	-0.00187***	-0.00152***	-0.00127***	-0.00109***	-0.00163***	-0.00177***	-0.00097**
	(0.00003)	(0.00001)	(0.00004)	(0.00007)	(0.00012)	(0.00017)	(0.00007)	(0.00031)
Differential effect in entry earnings 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	0.00986***	0.01998***	0.00550***	0.00552**	0.00527	0.01829**	0.02688***	0.00083
-at-some-point	(0.00089)	(0.00042)	(0.00117)	(0.00184)	(0.00292)	(0.00581)	(0.00243)	(0.00674)
experience^2*father	-0.00030***	-0.00026***	-0.00022***	-0.00025***	-0.00029**	-0.00065***	-0.00048***	-0.00032
-at-some-point	(0.00003)	(0.00001)	(0.00004)	(0.00007)	(0.00011)	(0.00019)	(0.00008)	(0.00022)
Differential effect of having children (post first childbirth)								
post-birth	0.07286***	0.04816***	0.06442***	0.04038***	0.04096***	0.07535***	0.03942***	0.01962
	(0.00201)	(0.00124)	(0.00265)	(0.00408)	(0.00671)	(0.01211)	(0.00764)	(0.01482)
experience post-birth	-0.00821***	-0.01734***	-0.00591***	0.00018	0.00134	-0.01314***	-0.02071***	-0.00027
	(0.00045)	(0.00024)	(0.00061)	(0.00100)	(0.00164)	(0.00274)	(0.00141)	(0.00356)
experience^2 post birth	0.00038***	0.00046***	0.00033***	0.00013**	0.00011	0.00077***	0.00061***	0.00043**
	(0.00002)	(0.00001)	(0.00003)	(0.00004)	(0.00007)	(0.00012)	(0.00005)	(0.00015)
Observations/Pairs	1734056	1734056	867028	349699	129847	44448	44448	22224
R^2	0.32149	0.38472	0.06938	0.05008	0.04939	0.03591	0.34252	0.41838

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

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. The Hausman test that the post-birth effects in col(1) and col(2) or col(5) are equal is rejected.

The F-test on the pooled regression of the samples in col (5) and col (8) to test whether the marginal differences are significantly different is rejected.

Table 4: 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)	(2)	(3)	(4)
	CV	CV	CV	CV
	All	All	<2 yr age diff	<2 yr age diff
Differential effect in entry earnings and experience (father-at-some-point)				
father-at-some-point	0.01542	0.01152	0.01611	.01334
	(0.01518)	(0.01517)	(0.03417)	(.03399)
experience *father-at-some-point	0.00649**	0.00750**	0.00908	.01015
	(0.00233)	(0.00233)	(0.00570)	(.00567)
exerience^2*father-at-some-point	-0.00026***	-0.00025**	-0.00044*	-.00042**
	(0.00008)	(0.00008)	(0.00020)	(.00020)
Differential effect of having children (post first childbirth)				
post-birth	0.06414***	0.05284***	0.04455***	.03169***
	(0.00281)	(0.00286)	(0.00716)	(.00733)
experience post-birth	-0.00666***	-0.00863***	0.00083	-.00122
	(0.00065)	(0.00065)	(0.00177)	(.00178)
experience^2 post birth	0.00035***	0.00040***	0.00013	.00017***
	(0.00003)	(0.00003)	(0.00008)	(.00007)
Control for being married	No	Yes	No	Yes
Observations/Pairs	675679	675679	99917	99917
R^2	0.05501	0.05753	0.03362	0.0373

Data: Norwegian register data until 2005. Sample of first and second born brothers (excluding twin brothers) .
born between 1955-65.
The control group excludes childless never married men.
Standard errors are clustered at the sibling couple
level and reported in parentheses. All regressions control for birth order- and time effects.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Correlations of education and completed fertility between siblings

	Years of education	whether children or not 1 if yes	number of children
Brothers, all	0.3567*	0.1128*	0.1325*
Number of sibling couples	45345	45345	45345
Brothers, < 3 yrs age difference	0.3697*	0.1169*	0.1335*
Number of sibling couples	18256	18256	18256
Twins	.5170*	.2420*	.2402*
Number of sibling couples	1069	1069	1069
2 randomly selected men	.002	-.009	-.003
Number of random couples	20000	20000	20000

Data: Norwegian register data until 2005. Sample of first and second born brothers (excluding twin brothers) and twin brothers, born between 1955-65.
* significant at 5 percent significance level.

Table 6: Descriptive statistics for sibling brothers with unequal fertility (children yes or no) outcomes: Means and standard deviations

	fathers-at-some-point		childless men	
	mean	sd.	mean	sd.
log(earnings)	12.35	.525	12.22	.53
yrs of education	12.16	2.5	11.91	2.64
yrs of experience	12.50	7.01	12.15	7.0

Data: Norwegian register data until 2005. Sample of first and second born brothers (excluding twin brothers), born between 1955-65.
From the full sample only sibling couples with unequal fertility outcome (children yes or no) are used. Means and standard deviations are only shown for this subgroup.

Table 7: Robustness test for compositional effects: Earnings regression results only using brother couples with unequal fertility outcome (0/1)

	(1) OLS	(2) CV less than 2 years age difference
years of education	0.044***	0.033***
	(0.001)	(0.001)
experience	0.074***	0.067***
	(0.002)	(0.003)
experience^2	-0.002***	-0.001***
	(0.000)	(0.000)
Differential effect in entry earnings and experience (father-at-some-point)		
father-at-some-point	0.038***	0.038**
	(0.011)	(0.011)
experience *father-at-some-point	0.009***	0.009***
	(0.002)	(0.002)
exerience^2*father-at-some-point	-0.000***	-0.000***
	(0.000)	(0.000)
Differential Effect of having Children (post first childbirth)		
post-birth	0.041***	0.012
	(0.009)	(0.013)
experience post-birth	-0.004*	0.005*
	(0.002)	(0.002)
experience^2 post birth	0.000	-0.000
	(0.000)	(0.000)
Observations/Pairs	64364	32182
R^2	0.314	0.093

Data: Norwegian register data until 2005. Sample of first and second born
brothers (excluding twin brothers), born between 1955-65.
For this table only sibling couples with unequal fertility outcome (children yes or no) are selected from the full sample.
The regression results are using only this subgroup.
All regressions control for birth order- and time effects.
The control group includes all childless men.
Standard errors are clustered at the sibling couple level and reported in parentheses.
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Table 8: Robustness Checks: Regression results restricted sample to exclude effects through paternity leave reform (compare to Table 3 full sample)

	Brothers					Twins	
	OLS	FE extended	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***	0.076***	0.069***	0.063***	0.060***	0.069***	0.057***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)	(0.005)
experience^2	-0.002***	-0.002***	-0.002***	-0.001***	-0.001***	-0.002***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Differential effect in entry earnings and experience pre-birth (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	0.012***	0.026***	0.009***	0.007***	0.009***	0.019***	-0.001
-at-some-point	(0.001)	(0.000)	(0.001)	(0.001)	(0.002)	(0.003)	(0.004)
experience^2*father	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.001***	-0.000*
-at-some-point	(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***	0.058***	0.078***	0.048***	0.047***	0.085***	0.032*
	(0.002)	(0.002)	(0.002)	(0.004)	(0.006)	(0.010)	(0.013)
experience post-birth (exp_{post})	-0.015***	-0.026***	-0.013***	-0.005***	-0.003*	-0.021***	-0.003
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)	(0.003)
experience^2 post birth (exp^2_{post})	0.001***	0.001***	0.001***	0.000***	0.000***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1172084	1172084	586042	237455	87894	32454	16227
R^2	0.283	0.309	0.066	0.044	0.044	0.305	0.033

Data: Samples from Norwegian register data until 2005. Sample of first and second born brothers (excluding twin brothers) and twin brothers, born between 1955-65. Earnings potentially affected by the paternity leave reform in 1993 are excluded. This means that the sample is the same as in Table 3 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. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Appendix

Appendix Table A1: Distribution of total number of children of a person, in 2005

Number of children	Birth Cohorts 1955-65			Birth Cohort 1955
	All men	Brothers	Twins	National Statistics, men*
zero children	19.46	19.03	21.78	16.6
one child	13.94	13.65	14.80	13.2
two children	36.64	36.15	35.04	37.1
three children	23.37	22.96	20.57	23.3
four or more	8.14	8.21	7.82	9.9
Total	100	100	100	100

Data: Norwegian register data until 2005. Sample of first and second born brothers (excluding twin brothers) and twin brothers, born between 1955-65.
*Source: Statistics Norway.

Appendix Table A2: Summary statistics: Variables in differences=$X_{firstborn} - X_{secondborn}$

	Sample of Brothers, all		Sample of Twins	
	mean	sd	mean	sd.
Δ log(earnings)	.09	.59	-.02	.52
Δ(father type)	.02	.51	-.00	.50
Δ yrs of education	.05	3.19	-.04	2.75
Δ yrs of experience	3.00	3.39	.00	2.45
.. experience squared	84.47	113.25	.37	80.61
Δ father-at-some-point* experience	2.74	7.90	-.00	7.90
... father-at-some-point*experience^2	75.22	178.42	-.29	176.95
Δ(post birth)	.12	.57	-.01	.54
Δ yrs of experience*post-birth	2.49	6.76	-.23	6.14
.. experience squared *post-birth	48.73	138.89	-4.66	125.60
Difference in age	3.5		0	
Number of observations	867028		22224	

Data: Norwegian register data until 2005. Sample of first and second born brothers (excluding twin brothers) and twin brothers, born between 1955-65.
Summary statistics in this table refer to the samples used in Table 3, col (3) and col (7).

Appendix Table A3: Earnings regression results estimated by CV: Number of children

education	0.02676***	0.02673***	0.02295***
	(0.00210)	(0.00210)	(0.00219)
experience	0.04881***	0.04854***	0.04511***
	(0.00619)	(0.00619)	(0.00663)
experience^2	-0.00083***	-0.00081***	-0.00065**
	(0.00021)	(0.00021)	(0.00023)
Differential effect in entry earnings and experience (father-at-some-point)			
father type separated into			
= 1 child	-0.02372	-0.01646	-0.03061
	(0.03531)	(0.03532)	(0.03696)
= 2 children	0.02551	0.02188	-0.00061
	(0.03430)	(0.03430)	(0.03602)
= 3 children	0.02211	0.02156	-0.00022
	(0.03453)	(0.03453)	(0.03632)
= 4 children	0.01383	0.01233	-0.00399
	(0.03599)	(0.03603)	(0.03775)
more than 5 children	-0.03835	-0.04191	-0.06173
	(0.03904)	(0.03920)	(0.04097)
experience*father-at-some-point	0.01061	0.01082	0.01641**
	(0.00568)	(0.00568)	(0.00615)
experience^2*father-at-some-point	-0.00043*	-0.00045*	-0.00069**
	(0.00020)	(0.00020)	(0.00022)
Differential Effect of having children (post childbirth)			
post-birth 1st child	0.02982***	0.03090***	0.03867***
	(0.00738)	(0.00793)	(0.00970)
experience post-first birth	-0.00189	-0.00213	-0.00391
	(0.00180)	(0.00255)	(0.00295)
experience^2 post-first birth	0.00019*	0.00009	0.00015
	(0.00008)	(0.00012)	(0.00014)
post-birth 2st child		0.00274	-0.00042
		(0.00753)	(0.00869)
experience post 2nd birth		0.00336	0.00459
		(0.00257)	(0.00292)
experience squared post 2nd birth		-0.00000	-0.00001
		(0.00015)	(0.00016)
post-birth 3st child		-0.01292	-0.00871
		(0.01017)	(0.01150)
experience post 3rd birth		-0.00308	-0.00427
		(0.00286)	(0.00308)
experience squared post 3rd birth		0.00034	0.00039*
		(0.00017)	(0.00018)
Observations	99917	99917	82747
Comment	sibling couples less than 2 years diff	sibling couples less than 2 years diff	excluding earnings post 1993 if childbirth post 1993
R^2	0.03929	0.03991	0.03803

Data: Norwegian register data until 2005. Sample of first and second born brothers (excluding twin brothers) less than two years different in age, born between 1955-65.
Control group are childless men married at some point. Specifications are extensions of Table 4, column 3.
Standard errors are clustered at the sibling pair level and reported in parentheses. $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Appendix Table A4: Time use of men and women per day, 2010

	Market Work	Household work
Single men, 24-44 yrs old	8.24	2.12
Single women, 24-44	8.24	2.22
	Couple without child, 16-44	
Men	8.58	2.38
Women	8.04	2.4
	Single parent	
men	7.47	4.19
women	7.32	4.17
	Couple with child age 0-6	
Men	8.45	4.29
Women	7.29	5.57

Collected from O.F. Vaage (2012): Tidene skifter: Tidsbruk 1971-2010,

Statistics Norway, Oslo Kongsvinger.

Couples include married and cohabiting couples.

Numbers disaggregated by parenthood status are not available before 2010 from this report.

Appendix Table A5: Linear Probability Model results for employment and hours of work

	All[1]		Restricted Comparison Group[2]	
	Employment	More than 30 hours work	Employment	More than 30 hours 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^2	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Differential effect in entry earnings 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	-0.003***	0.001***	0.001	0.001
-at-some-point	(0.001)	(0.000)	(0.001)	(0.001)
experience^2*father	0.000***	-0.000***	0.000	-0.000
-at-some-point	(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^2 post birth	0.000***	0.000**	0.000***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1655259	1655259	697224	697224
R^2	0.021	0.007	0.025	0.005

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

All regressions control for birth order and time effects.

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

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

Standard errors are clustered at the sibling pair level and reported in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$