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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 and earnings, and fertility histories on brother couples are used to estimate flexible earnings regressions with fixed factors. The main result is that higher earners are more likely to become a father, and not that children make fathers earn higher incomes. Furthermore, men who remain childless and/or unmarried, are on relatively low earnings profiles and contribute therefore significantly to the earnings inequality among men. Finally, most of the earnings variation comes from first childbirth, not from further children or from marriage.

Key words: children, earnings, men, inequality, selection, siblings, twins, panel data.

JEL Code: J220, J240, J310, J130, J160

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 several explanations; 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 the effect of children that makes fathers earn higher incomes, but that higher earners are more likely to become fathers. The results show that selection is captured through family fixed factors and through higher earnings growth even before men are becoming a father. We also show 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 also state an important source of earnings inequality among men. A main source of inequality is not children, but selection of men into the group of childless and never married men.

Knowledge on costs and gains from having children are important because they are directly related to the demand for children. Costs through labor ad-

¹Other 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), Gupta and Smith (2002).

justments (of women) related to children are generally viewed as an important contributor to the gender wage gap², which policy makers try to diminish. It still exists no consensus in the debate of reasons that lie behind the "wage premium" fathers get from children. The view in the recent sociological literature (Budig, 2013) is that also conditional on a large range of observed characteristics the positive effect remains which is plausibly related to positive discrimination by employers. Employers view children for men as a signal of more conservative values, reliability and higher productivity and are therefore willing to pay. A policy that follows is to restrict such behaviour and target employers that pay such a premium. The results of our study question whether such policies are efficient, because another channel, that is selection, may explain part of the observed premium.

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 show, 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 also 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 (Angelov et al. forthcoming).

The comparison of earnings for men with and without children also contributes to the debate about understanding within group inequality. In our analysis following male workers from first entry into the labour market and

²Statistics 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).

around first entry into parenthood shows when inequality arises within the group of men. Both in political debate and academic debate it has been noted that after decades of fighting women’s relatively under-performance and unequal treatment it has become an issue that 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 describing a source of inequality is the effect of children when comparing childless men to fathers. Yet, 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 or correlated with unobserved factors. Credible instrumental variables that can address these potential problems are difficult to find for fertility. IV 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. The most common approach in the literature has been to apply fixed-effects estimation exploiting longitudinal panel data following individuals over time.

In this study, we follow this literature but apply an alternative approach to estimate the mean effect of children on earnings. We compare men’s outcomes to those of their brothers or twin brothers over the most important part of their life cycle and before and after entry into fatherhood. We estimate the effect of entry into fatherhood, which is allowed to be non-linear. The variation identifying the effect of children comes from pairs of brothers where one becomes a father and the other never does. While this exercise is in itself interesting, it also potentially addresses some problems in the literature. We exploit that brothers are genetically more similar than randomly selected men from the population and hence the comparison of earnings, holding standard characteristics constant, reduces the heterogeneity problem. We estimate family fixed factor models that also control for the age differences between brothers, to account for more heterogeneity in family background. Gradually reducing

the age differences to zero, we use only the sub-sample of twins. Using twins offers the advantage that we directly control for family fixed factors, time fixed factors and individual fixed factors. Furthermore, we extend existing earnings models in this literature by controlling for the fact that men who remain childless and men who become fathers-at-some-point may be on different earnings paths from first entry into the labour market; that is when we compare the earnings paths of childless men to those of fathers-at-some-point even before they enter fatherhood.³

To implement the approach we use a large, high quality Norwegian registry data set of the population of brothers and twins born during the period 1955 to 1965. Core to our study is that we observe complete employment and earnings histories from first entry into the labour market. This also includes the complete timing of births histories for every individual in the population, employment and earnings before and after childbirths and the link to match brothers. These data offer several advantages over previous studies. First, data on fertility for men are extremely rare in the literature. Exceptions are studies using Scandinavian register data. (See for a nice discussion in Tertilt, et al. (2015).) The literature on the effect of children on earnings has relied on much smaller samples, mostly, from survey data that do not permit to follow every worker over the complete life cycle in the labour market, observe exact timing of births and complete fertility, and match siblings. For Norway, Petersen et al. (2014) and Petersen et al. (2011) investigated the fatherhood premium with a sample restricted to white collar workers in the private sector. Using OLS within occupation and controlling only for a linear shift parameter in father-at-some-point, they estimate a small and not significant effect of 1 percent. By contrast, this analysis covers all workers from all sectors and we estimate more flexible earnings equations. Our approach is also related to studies that exploit genetically identical twins to estimate the marriage premium (Antonovics and Town, 2004; Krashinsky, 2004). In this literature samples are often small and a much discussed problem is attenuation bias because of measurement error

³While this idea is not completely novel it has not been applied in a flexible earnings framework estimated on siblings data for whole Norway as we do.

in survey data (Bound and Solon, 1999). Therefore, the data we use have an advantage since register data contain process collected information on the offspring to the fathers and a large number of individuals.

The remainder of the paper is organized as follows. Section 2 provides an overview of explanations of why children 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 outlines the empirical framework. Section 5 presents the empirical results and tests of robustness of the results. Section 6 discusses the results and Section 7 concludes.

2 Men's earnings and having children

The finding in the previous literature is that men gain from having children in terms of earnings. This finding is usually derived from ordinary least squares or individual fixed effect estimation of a simple log earnings regression with a dummy variable that is equal to one if a man is a father. There is still no agreement on why children have a causal and positive effect on earnings.

A 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 more invest into their career before they become fathers. There are several potential reasons for why initial earnings and returns to experience may be relatively higher for men who become fathers at some point. For instance, the planning of the timing of births could contribute to the observed patterns (Bergstrom and Schoeni, 1996). Moreover, men who plan to become partners may also self-select into higher-track occupations (Gould, 2008). There is very little attempt in the literature to investigate the issue of selection into parenthood in relation to earnings inequality.

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. This can 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.

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. Studies of the marriage premium in male wages provide little insight into the effect of having children, since either the effect of having children is not separately reported (Korenmark and Neuman, 1991; Gray, 1997), or is reported to be insignificant (Loh, 1996). We show that in Norway children typically precede marriage. One may argue that marriage is also different since only children impose an investment of time, or resources of the couple. This is why we argue from a labour market perspective that children is the more interesting event to study. 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 father's outcomes is quite scarce from the economics literature, and 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 1 percent 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)). Recent studies show increased interest in 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) and child outcomes (Cools et al. 2015).

Our study uses Norwegian registry data on complete employment, earnings and fertility histories for cohorts of men born between 1955 and 1965. All are followed until the year 2005. Novel to the literature we estimate very

flexible earnings regressions and exploit data on brothers and twins in order to control for unobserved heterogeneity related to family background. We present a series of individual fixed effects and type-of-family fixed effects estimation results. We cannot fully account for the fact that, if the timing of fatherhood is anticipated, this may affect earnings and earnings growth before entry into fatherhood. 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.⁴ Second, the effect of children is estimated after controlling for differential entry earnings and differential returns of work experience (squared) for fathers-at-some-point before actually becoming a father compared to childless men. This approach controls for differences in earnings paths, namely if fathers started on different (higher) earnings paths to childless men. Hence, these controls potentially reduce the omitted variable problem. None of the studies in the literature addresses this point.

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, which is low compared to Germany and the US (20-23 per cent), for example.⁵ 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. Female labor force participation is also high by international standards, but a number of women with young children work

⁴This resembles findings in the literature on the return to education showing that pre-market education predicts wages and wage growth.

⁵These are the unadjusted gender wage gaps reported by Eurostat and the US Census.

part-time. Norway underwent a severe recession during the period from 1987 to 1993, with unemployment peaking at 6.7 per cent in 1993.

It has been a long-standing policy goal in Norway to achieve high gender equality and help families to combine having children and work. 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, 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.⁶ Since 1993, the proportion of fathers taking up leave has steadily increased from an initial 30 percent to almost 60 percent in 1998. During paternity leave, men receive a wage replacement up to a cap. 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.

In our empirical analysis we focus on the estimation of the effect of children on earnings for men who had their children before 1993, hence before the introduction of paternity leave. This makes our results for fathers more relevant also for other countries. Still, Norway, likewise the other Nordic countries, has generous parental leave schemes that leads to high take-up of mothers of one year of leave from work and high return rates that contribute to the, by international comparison, high female labour force participation.

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 rates. However, the fatherhood effect may be relatively increased through factors that increase (gender specific) household specialization, such as part-time work of mothers/partners, and overtime work of fathers when the children are very young. One should however keep in mind that according to time use data gender specialization in

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

the household among Norwegian couples is low. Overtime work also plays a minor role. National statistics show that only 20 percent work overtime and usually this is not paid, and in many sectors overtime is in fact restricted. In line with the literature, we take an individual approach and neglect potentially endogenous household choices. Likewise most data set we do not have information on overtime hours of work.

3.2 Data description and summary statistics

The panel data for 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 includes the population of men born between 1955 and 1965 who were the first- or second-born son within a family with the same mother and father.⁷ The sample of twins includes the population of twins of the corresponding birth cohorts. Fraternal and monozygotic twins are included but cannot be distinguished in the data.⁸

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.⁹ We deflate earnings by the Norwegian consumer price index (1998 = 100). Earnings are excluded for workers younger than 20 years

⁷This 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.

⁸Statistically, 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.

⁹The 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.

of age, as they may still be in education. We also exclude observations with very low earnings (earnings less than the annually adjusted basic income according to the social security system). Years of experience are measured as the cumulative number of years with earnings above the yearly basic income. We generate two variables for years of experience. One that counts overall years of employment since first entry into the labor market, and one that counts years of employment from first childbirth. We merge the variables age and years of education to the data. We generate a variable measuring the birth order within the family to control on the right hand side in the earnings regressions for the birth order rank of each of the brothers that we compare.¹⁰

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 2005.¹¹ Note that fathers are reported in the birth registry when they are cohabiting with or married to the mother. In the estimations, we will first focus on the year of the first childbirth and earnings effects before and after this year, the latter will be referred to as the ‘post birth period.’ For supplementary results, we also use the birth year of the second and third child. Hence, we can test for the non-linearity of the earnings effect in number of children. Our main group will be 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, namely those who will never have children across the entire observation window.

We assume that virtually all births for the cohorts in the analysis samples are counted, since men in the sample are followed over most of their life cycle, that is, until they are 50 years old, and the youngest cohort until they are 40. Approximately 20 per cent remain childless by the year 2005, according to the data. National statistics show that the fraction of childless men only declines

¹⁰We keep information on birth order within the family, counting both girls and boys.

¹¹The birth registry is complete, with the exception that the father is not reported if the mother does not want to report him. One birth cohort is around 60,000 in Norway. During the observation period, only 400-500 children were adopted per year and we have no information about those.

by 2 percentage points between the age of 40 and 45, and by 0.6 percentage points between 45 and 50.¹²

We use information on marital status to restrict the comparison group of childless men to men who are childless but married-at-some-point.¹³ Childless men may be a very heterogeneous group, and childless men married at some point may be more similar to fathers-at-some-point at the beginning of their working career. Some of those who married may have planned to become fathers but for some reason did not realize such a plan.¹⁴ Data on marital status is available for the period 1986 to 2005 and used to construct an indicator for being married at some point (until 2005). We define 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 children¹⁵ or marriage, we also construct a control variable based on the same information concerning whether a man is married in period t . Hence, for fathers-at-some-point, we can control for whether the couple is married before, at or after childbirth. 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. 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, when we pool all observations across the entire observation period. Compared to childless men, men with children acquire slightly more years of education, and work less. Differences become smaller when we

¹²The distribution of the number of children in our sample is reported in the Appendix in Table A1.

¹³We do not have access to information on cohabitation for men without children. Hence, we may exclude too many men by this rule.

¹⁴In our empirical analysis, we have to assume that this is not due to health problems.

¹⁵The father is reported on the birth certificate if he is married to or cohabiting with the mother.

compare fathers to childless men married at some point.

4 Empirical Framework

The econometric framework of the earnings equation builds on Heckman and Hotz (1998) where non-random sample selection into treatment, training in their case, in a non-experimental setting is investigated.¹⁶ We modify the model for our purposes and extend the model by unobserved family fixed factor.

We assume that the log earnings, $\ln y_{ift}$, for individual i , in family f , and calendar year t is given by:

$$\ln y_{ift} = \gamma_t a_{ift} + \beta' X_{it} + \delta' Z_{ift} + \nu_f + \mu_{if} + w_{ift} \quad (1)$$

where a_{ift} is an indicator equal to one from the time a person first enters fatherhood, and zero otherwise, X_{it} includes controls for years of education and experience (squared) counted since entry into the labor market, Z contains the indicator variable equal to one if the man is a father-at-some point and its interaction terms with years of experience (squared) since entry. The error term contains three components: an unobserved family fixed component, ν_f , capturing genetically inherited ability¹⁷; an individual varying and family-varying unobserved component, μ_{if} , capturing unobserved ability and genetic traits that vary across individuals and families; and u_{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-varying factors, τ_t , capturing macroeconomic shocks. We assume that $E(w_{ift}X_{it}) = 0$ and $E(w_{ift}Z_{ift}) = 0$ for all i, f

¹⁶See the Appendix B for a more complete derivation of the model.

¹⁷Since 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).

and t .¹⁸

The key parameter is the marginal effect of having children, γ_t , and to make it more flexible, we allow the effect to be a non-linear function in a_{ift} and post-birth work experience, ex_{post} : This may capture time varying costs of children, or effects through further children.

$$\gamma_t(a_i, ex) = \gamma_{t1}1(a = 1)_{it} + \gamma_{t2}(ex_{post})_{it} + \gamma_{t3}(ex_{post}^2)_{it}.$$

Most of the studies on the effect of fatherhood has estimated a more restrictive log earnings equation than equation (1) by OLS or FE, where Z is excluded and the effect of children is only a shift parameter in earnings after (first or/and further) child birth.

As a baseline, we estimate the earnings regression by ordinary least squares. 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. OLS estimation exploits the comparison of all fathers, when they are actually fathers, to all childless men. We also present fixed effects (FE) estimations exploiting the panel structure of the data and within individual variation. Hence, essentially switchers into fatherhood are used and omitted variables bias captured by individual specific factors is taken into account.

Since individual fixed effects (FE) sweep out all time constant variables, it cannot identify the differential effect of entry earnings between the group father-at-some point and childless men. FE will give a consistent estimate of γ if the dummy variable for having children, a , and the common shock, w , conditional on the remaining controls are uncorrelated. A common concern with this type of model is that estimates are biased if past earnings affect current fatherhood status.

Our main estimation results rely on the covariance estimator (CV) (Bound

¹⁸We 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.

and Solon, 1999), which applies ordinary least squares to the regression of the between-siblings differences in log earnings in every period on the between-siblings difference in the children variables, holding other between-sibling differences constant. It exploits the cross-sectional variation for identification. This can be viewed as an alternative way to address selection on unobservables and potentially address some of the caveats of FE. If we assume $\mu_{1f} = \mu_{2f}$ ¹⁹, then the transformed regression can be written as:

$$(\ln y_{1ft} - \ln y_{2ft}) = \gamma_t(a_{1f} - a_{2f}) + \beta'(X_{1ft} - X_{2ft}) + \delta'(Z_{1ft} - Z_{2ft}) + (w_{1ft} - w_{2ft}) \quad (2)$$

The between-sibling difference is always formed by subtracting the variable of the second-born brother in family f from the variable of the first-born brother in family f .

Variation used to identify the parameters δ and γ_t comes from sibling pairs where one sibling has children and the other does not. To identify δ , we need to observe men in the group of father-at-some-point before they actually become a father; that is, we need variation in a , which is independent of Z .²⁰ This highlights the value of the data in which we observe the complete employment, earnings and fertility histories for the selected cohorts and completed fertility for every man in the sample. We also need that a and Z are uncorrelated with w .

The advantage of our approach is that when 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. However, this comparison may still give a biased estimate of the effect of children in our model, since we cannot claim that μ_{1f} is exactly equal to μ_{2f} when we use all brother pairs. We can still reduce these problems

¹⁹Since we cannot make use of data on monozygotic twins, we cannot sweep out the μ completely and therefore have to make assumptions. We also tested whether $\mu_{1f} = \mu_{2f} = 0$. We tested for second and third order serial correlation of the error term from the model in between-sibling differences, observing that serial autocorrelation remains, yet is small. The correlation coefficients are 0.085.

²⁰At the individual level, i , all combinations of Z and a are observed, except for the combination $Z = 0$ and $a = 1$, i.e. childless man after becoming a father.

by using within family variation, if the family fixed factor is correlated with fertility outcomes. If family environment differs between brothers because they are different in age, we can more accurately control for family fixed effects by comparison of siblings who are more similar in age. Possible reasons why brothers who are very different in age vary relatively more in terms of family background than brothers born, for example, only one year apart could be that parents are in different career phases, may differ in terms of time and monetary resources or have different experience duration in parenting. We address this issue by taking advantage of our large sample and run separate regressions on sub groups of brother pairs who are more similar in age. Using twins offers the advantage that we directly control for family fixed factors, time fixed factors and individual fixed factors.

We provide further empirical analysis and robustness checks exploring a number of factors that might explain the results such as whether the effect of children works through first childbirth or higher parity births, and whether the effect of children captures effects through marriage. We also test whether estimates are robust over time.

5 Empirical Results

5.1 The mean effect of having children

Table 3

Table 3 reports the estimation results of the effect of having children on earnings estimated by OLS, FE and CV (covariance estimator). The table shows the key coefficients on the effect of children in the lower panel, while all coefficients of the control variables are shown in the two upper panels. The parameter estimates of the auxiliary variables have the expected signs and sizes. The OLS estimates on the pooled sample of brothers (column 1) show that the conditional effect of having children on earnings at birth is quite large, 7.3, and significantly positive. The marginal post-birth effect in experience is slightly U-shaped but very small.

When we estimate the model by individual fixed effects, the coefficients of the variables of having children (column 2) decrease compared to the OLS coefficients.²¹ The shift parameter right at birth decreases by more than a third. The marginal effect post-birth is negative implying that the profiles of fathers decrease relative to not-fathers.

Column (3) in Table 3 reports the results from the covariance estimator (CV) on the same sample, whereby all brother pairs are pooled.²² They show that earnings significantly increase in the birth year of the first child, but now the point estimate of the shift right after childbirth is 6.4 percent. The marginal effect post-birth is almost constant. For illustration, the model estimates show that 14 to 35 percent of the simple OLS estimated effect of children one year after childbirth is due to positive selection on fixed family-specific and fixed individual unobserved factors. Selectivity on fixed family-specific factors appears significant but relatively small when we use variation from all brother pairs.²³

Siblings might still be quite heterogeneous in terms of family background, which may introduce bias and thus make family fixed factors appear less important. Siblings are genetically more similar than randomly selected men. Siblings also differ due to differences in the family background in terms of the timing of parenting (nurture). Parents' skills and resources develop over time, and the older sibling in our sample is 3.5 years older at the mean than the younger sibling.

In order to control for the potential differences in parenting and age differences between siblings, we re-estimated the regressions on the sub-samples of brothers who are born two years or less apart and one year or less apart

²¹For all fathers-at-some-point, earnings are observed before having a child.

²²Summary statistics for the variations in between-sibling differences are reported in Table A2 in Appendix A, showing that there is still considerable variation in the variables in differences between siblings and twins.

²³The coefficients of the pre-birth differential effects between fathers-at-some-point and the group of childless men reported in the upper panel of Table 3 are quite large and significant. This suggests that fathers-at-some-point start on higher growth earnings paths than childless men.

(see Table 3 columns (4) and (5)). Results using the twin sample completely remove effects due to age differences (see column (8)). Gradually reducing the age differences between brothers leads to a reduction in the coefficients measuring the effects on post-birth earnings of having children. The effect on earnings of having children right after childbirth declines to +4.0-4.1 per cent, from an initial +6.4 per cent. From the second year after birth, the effect remains constant.²⁴ When twins are used, the effect post-birth declines even further. The adjustment of earnings post-birth is now a constant shift of 2 per cent, but is not significant. The F-test 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). Columns 6 and 7 are presented just to show that we can replicate the results from OLS and FE for all brothers with the sub-sample of twins.

We explore further reasons why the effect of children remain relatively large for the sample of brother pairs only one year, or less, different in age compared to twins. First, we use childless men married-at-some-point as an alternative comparison group.²⁵ The summary statistics on mean earnings and education show that men in the group fathers-at-some-point are indeed more similar to childless men married-at-some-point than to all childless men. Table 4 columns 1 and 3 report the new estimation results using the restricted comparison group. The size of the effects post-birth do not change significantly compared to the results in Table 3 columns 2 and 5. The coefficients of the pre-birth differential effects between fathers-at-some-point and the restricted group of childless men reported in the upper panel of the table are now much smaller than those reported in Table 3, but are still significant. This suggests that the large differences between those who are not fathers yet and childless men, as found in the previous estimates reported Table 3, are driven by childless men who are never married and perform worse.

²⁴Only the coefficient of the experience squared variable is significant in column 4, but it is economically small.

²⁵The twin sample would become too small for estimation when we further restrict the group of childless men.

Overall, we find a linear decrease in the post-child effect as we narrow down age differences between siblings. To illustrate, we calculate that decreasing the age difference between brothers to one year, or less, leads to a decrease of the effect one year after childbirth by 62 percent. When we eliminate all age differences between brothers by use of twins the effect one year after childbirth declines by an additional 50 percent. We interpret this as a quite linear decrease in the post-child effect. The latter decrease is driven by the fact that using twins allows us to control for time effects, family fixed effects and age in the best way, given that they are born in exactly the same year.²⁶ 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.²⁷

Table 4 here

We also test whether the estimates are driven by childbirth or relatively more by marital status. An informal test is to add a control variable for the time from which a man is actually married. We run these regressions again using the samples of brothers and the restricted comparison group. (Note that the samples of twins would become too small.) We take to some extent advantage of the timing of marriage pattern showing that men in Norway typically marry after becoming a father. As Figure 1 shows, approximately 3 out of 4 couples get married close to the time of first childbirth or later in our sample.

Figure 1 here

The estimation results reported in Table 4 columns 2 and 4 show that adding a control for being married only slightly reduces the size of the effect on earnings of having children. Note that, technically, all men not married at childbirth are cohabiting if they are reported in the birth register. Therefore,

²⁶All of the estimates using childless men as the comparison group reveal significantly larger returns to experience of fathers-at-some-point compared to childless men even before first entry into fatherhood. CV on the twin sample shows that entry wage differentials remain quite large.

²⁷We cannot tell whether the genetic component drives our results since we cannot distinguish between fraternal and monozygotic twins in our data.

the effect of children can be interpreted as the joint effect of post-first childbirth and cohabiting. However, note that the results now show that, before birth, fathers-at-some-point are on very similar earnings paths to the restricted group of childless men. Hence, even if some men already cohabit before having children, the effects possibly related to household specialization or selection into the fatherhood group at least seem quite small. This pattern is in line with recent time use data for Norway (See for some patterns in the Norwegian time use data in Appendix C Table C1).

The effect of children now is 3.2 percent and economically constant, as we see from the regression results in Table 4 column 4 for brothers who are only one year different in age. These results confirm that the larger part of the estimated positive effect of having children remains after we account for marriage, (28 percent= $1-(0.032/0.044)*100$).²⁸ The relatively small effect of marriage can also be seen from Figure 2, where the simulated earnings profiles of a hypothetical father-at-some-point is plotted, comparing the two estimates with and without a control for being married. These results appear plausible in the context of a Scandinavian society where gender equality is high and marriage traditionally takes place after the first childbirth. Married and cohabiting couples are treated equally under Norwegian law, including tax laws.

Figure 2 here

It is also possible that the effects after the first childbirth capture the effects of second or further births. In extension, we add dummy variables and the corresponding interaction terms with experience (squared) for second and third births to our preferred model. We re-estimated the model for the brother sample used in Table 4 column 4. As seen from Table A3 in the appendix, the estimates of the effect of children remain unchanged and the effects after the second and third birth are not significant. This result is highlighting that selection into fatherhood is important irrespective of number of children. It also suggests that all adjustment if at all takes place at first birth. This is a

²⁸In this calculation, we ignore the curvature parameters, since they are essentially zero.

novel finding and in contrast to the non-linear effects of children on earnings for women.

5.2 Robustness tests

For comparison, 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). Our study uses that also fertility choices taking place later in life and fixed family factors are correlated. This may capture intergenerational transmission of fertility or cultural values (see Booth, et al., 2009; and Fernandex et al., 2006) As Table 5 demonstrates, fertility outcomes in our data are significantly correlated between siblings and correlation coefficients are quite large, at between 11 per cent and 24 per cent. The correlation between two randomly selected men from the population is zero. By comparison, the correlation in years of education for twins is 51 percent and hence, as expected, is higher.

Table 5 here

Identification applying the covariance estimator 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 3 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 finding is reassuring for our identification strategy.

Figure 3 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 sample, which could drive the results. In order to investigate this

possibility, we present means and standard deviations for the sample of sibling pairs where only one sibling has children and the other does not (unequal fertility outcomes). The means of the main characteristics presented in Table 6 are very similar to those of the entire sample. For illustration, 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 4 shows, the patterns are very similar at all levels of education, except at very high levels of education where observations are few.

Figure 4 here, Table 6 here

The corresponding regression results reported in Table 7 tend to confirm that our previous results are not driven by sample composition. However, we note a shift in the levels of the estimate of the post-birth effect; both the OLS estimate of the effect of having children and the CV estimate are lower than the estimates reported in Table 3. The shift post-childbirth now is 1.2 percent, and increasing by 0.5 percent per year. Only the slope coefficient is significant.

Table 7 here FE and CV both indicate an upward bias of ordinary least squares (Table 3), although the point estimates are very 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 panel. 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. The large sample allows us to control for family fixed factors in great detail and in various ways, which we consider an advantage over individual fixed effects.

In order to test robustness across time within Norway of our results we exploit the parental leave reform in 1993 that introduced paternity leave. From 1993, four weeks of parental leave were reserved to fathers for the first time, which led to the effect that some fathers interrupted work for an additional month. In order to ensure that potential negative earnings effects through interruptions related to becoming a father do not affect our findings, we ran

regressions on a restricted sample. Column 3 in Table A3 reports results when we drop earnings following births that took place during the period when paternity leave was available. As can be seen, the main results are unchanged. The complete replication of Table 3 on the restricted sample is reported in the supplementary Table C2.

When we compare the estimation results reported in Table C2 and Table 3 then we notice that Table 3 shows systematically larger effects. This could be a reform effect. Alternatively, it may capture a trend in the decrease of household specialization and hence positive effects on earnings through effort or hours of work. We cannot disentangle with our data at hand whether one of these hypothesis explains the pattern, or something else.

6 Discussion

We started our analysis from the descriptive finding that fathers are on steeper earnings profiles from early on in the career and men who remain childless are on relatively flat profiles. The difference increases following first entry into fatherhood.

The regression analyses reveal that the positive conditional correlation between children and earnings from OLS is overestimating the direct effect of children. Part of the difference captures differences already occurring before entrance into fatherhood, and part of it is related to family fixed effects. When we compare earnings profiles of twin brothers then there is no significant effect of children. Earnings profiles for all childless men, for those never fathers and for those not yet fathers, are not significantly different when we exclude men who never marry. Marriage in this case may be viewed as a crude proxy for those who plan to have a family. This shows that men who remain unmarried and childless are a select group contributing to the observed inequality among men.

Our results contribute to the previous literature on the marriage premium as well as the effect of children on men's earnings. We show that most of

the variation in earnings comes from children, and only a minor part through marriage. We can distinguish these effects through the timing and sequence of the events. As we show in Norway children usually come first and marriage afterwards. In Norway, gender specific household specialization if at all is more related to children than to marriage. Hence, contrary to the literature, we do not find relatively strong effects of marriage, the marriage premium. The latter may reflect that countries differ and a more gender neutral household division of work in Norway may be fostered, for example, by the individual taxation system and high female labour supply, high return rates of mothers into work after parental leave and positive attitudes towards equality in society and culture. In Norway during the observations window, almost all women took paid parental leave between 24 and 42 weeks after childbirth and return to work afterwards during the period of observation. Therefore, it is not clear whether parental leave in itself translates into permanent changes, such as an adjustment of fathers' labour supply. We attempt to test whether labour supply adjustments contributes to the positive effect post-birth that remains in Table 3 column 5. The results reported in Table C3 tend to show quite clearly no effect post-childbirth both when we use employment as a binary dependent variable, or working more than 35 hours of work or not.

The research result contributes to the understanding of sources of inequality among men. We show that observed inequality related to fatherhood increases during the early career even before entry into fatherhood and even between brothers. Our results suggest that this however reflects non-random selection into the group of childless and never married men who are on low earnings profiles. When we look at the differences in earnings profiles between twin brothers there are no significant differences, except for entry earnings. Overall, the results suggest that inequality does not increase through the event of having children.

Regarding the effect of children this research suggests that an important driver of the observed child premium is selection. Our research highlights that family background is important. This means that individuals with relatively high values of the family-specific factor are becoming fathers more likely, which

drives the estimated effect of children upwards. To illustrate the upward bias, Figure 5 plots the simulated profiles for fathers-at-some-point for those who become a first-time father after five years in the labor market and who are working continuously. For illustration, we use brothers close in age for these calculations. Here, the upward bias is approximately 20 percent around the year of childbirth. Hence, between-family differences account for a substantial amount. We leave to future research to investigate why family background is important. This may be for various reasons. Is it neighbourhood or the social environment the family creates? Is it schools in the neighbourhood that vary in quality?

Figure 5

7 Conclusions

In this study, we investigate whether fatherhood has a causal effect on earnings for men using Norwegian registry data on complete employment, earnings and fertility histories for cohorts of men born between 1955-1965. Novel to the literature we estimate flexible earnings regressions and exploit data on brothers and twins in order to control for unobserved heterogeneity related to family background. We present a series of individual fixed effects and type-of-family fixed effects estimation results, which also control for differential entry earnings and returns to experience pre-birth.

We conclude that it is not the effect of children that makes fathers earn higher incomes, but that higher earners are more likely to become fathers. The results show that selection is captured through family fixed factors and through higher earnings growth even before men are becoming a father. This result puts Norway at the lower end of the scale of estimates found in the economics and sociological literature. Our analyses may suggest that upward bias in other estimates controlling for less heterogeneities may exist. However, it may also reflect institutional differences. Education is free in Norway, and hence parents do not need to save for their children's education. Most women

work²⁹ and parental leave is paid. Taken together, this suggests that the labor supply responses of parents are expected to be smaller in Norway than in the US, for example. Our results are consistent with this hypothesis for Norway.

We show that for Norway most of the earnings variation comes from children (first), not from marriage afterwards, which is consistent with relatively low gender specific household division of work. We also show that selection works through the event of first birth entirely, which is consistent the explanation of the effect through non-random selection into fatherhood. In the more descriptive analysis we do not find any adjustment at second or high order childbirths. This pattern has been not discussed in the literature so far and is in contrast to findings for women where the negative effect of childbirths increases with the number of children. This reveals an unsymmetric pattern. If it extends to within households this can lead to new interesting research questions on the explanations of the gender wage gap.

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 wisdom 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 children on men's earnings. Hence, potential effects through redistributive policies at the household level are potentially less effective than would be expected from observed gender wage gap and family gaps.

²⁹In 1990 (2009), 62.5 (68.8) per cent of women were working in Norway, compared to 57 (58) per cent in the U.S. Source: OECD.

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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) OLS	(2) FE extended	(3) CV All	(4) CV <3 yr age diff	(5) CV <2 yr age diff	(6) OLS	(7) FE	(8) CV
years of education	0.04692*** (0.00051)	0.03266*** (0.00070)	0.03088*** (0.00111)	0.03266*** (0.00070)	0.02985*** (0.00181)	0.03465*** (0.00367)	0.07040*** (0.00345)	0.01564** (0.00543)
experience	0.06947*** (0.00085)	0.07280*** (0.00056)	0.06840*** (0.00121)	0.06294*** (0.00215)	0.06012*** (0.00359)	0.06994*** (0.00547)	0.07040*** (0.00345)	0.05923*** (0.00929)
experience ²	-0.00162*** (0.00003)	-0.00187*** (0.00001)	-0.00152*** (0.00004)	-0.00127*** (0.00007)	-0.00109*** (0.00012)	-0.00163*** (0.00017)	-0.00177*** (0.00007)	-0.00097** (0.00031)
father-at-some-point	0.03022*** (0.00543)	0.04911*** (0.00700)	0.04641*** (0.01040)	0.04641*** (0.01040)	0.05800*** (0.01659)	-0.01724 (0.03616)		0.10044* (0.04037)
experience *father	0.00986*** (0.00089)	0.01998*** (0.00042)	0.00550*** (0.00117)	0.00552*** (0.00184)	0.00527 (0.00292)	0.01829** (0.00581)	0.02688*** (0.00243)	0.00083 (0.00674)
-at-some-point	-0.00030*** (0.00003)	-0.00026*** (0.00001)	-0.00022*** (0.00004)	-0.00025*** (0.00007)	-0.00029** (0.00011)	-0.00065*** (0.00019)	-0.00048*** (0.00008)	-0.00032 (0.00022)
post-birth	0.07286*** (0.00201)	0.04816*** (0.00124)	0.06442*** (0.00265)	0.04038*** (0.00408)	0.04096*** (0.00671)	0.07535*** (0.01211)	0.03942*** (0.00764)	0.01962 (0.01482)
experience post-birth	-0.00821*** (0.00045)	-0.01734*** (0.00024)	-0.00591*** (0.00061)	0.00018 (0.00100)	0.00134 (0.00164)	-0.01314*** (0.00274)	-0.02071*** (0.00141)	-0.00027 (0.00356)
experience ² post birth	0.00038*** (0.00002)	0.00046*** (0.00001)	0.00033*** (0.00003)	0.00013** (0.00004)	0.00011 (0.00007)	0.00077*** (0.00012)	0.00061*** (0.00005)	0.00043** (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

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	0.01542 (0.01518)	0.01152 (0.01517)	0.01611 (0.03417)	.01334 (.03399)
experience *father-at-some-point	0.00649** (0.00233)	0.00750** (0.00233)	0.00908 (0.00570)	.01015 (.00567)
experience ² *father-at-some-point	-0.00026*** (0.00008)	-0.00025** (0.00008)	-0.00044* (0.00020)	-.00042** (.00020)
Differential effect of children (post first childbirth)				
post-birth	0.06414*** (0.00281)	0.05284*** (0.00286)	0.04455*** (0.00716)	.03169*** (.00733)
experience post-birth	-0.00666*** (0.00065)	-0.00863*** (0.00065)	0.00083 (0.00177)	-.00122 (.00178)
experience ² post birth	0.00035*** (0.00003)	0.00040*** (0.00003)	0.00013 (0.00008)	.00017*** (.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

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

* significant at 5 percent significance level.

Table 6: Descriptive statistics for sibling brothers: 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

Only sibling couples with unequal fertility outcome (children yes or no).

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

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

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.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

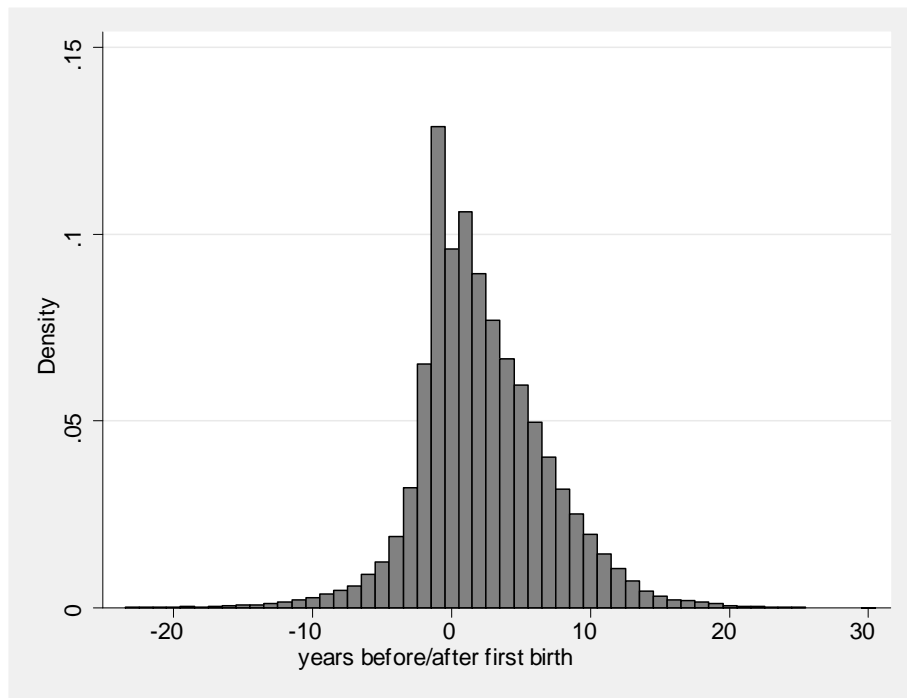


Figure 1: Timing of marriage and first childbirth, own calculations using the sample of all brothers and the observation period 1986 to 2005.



Figure 2: 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.

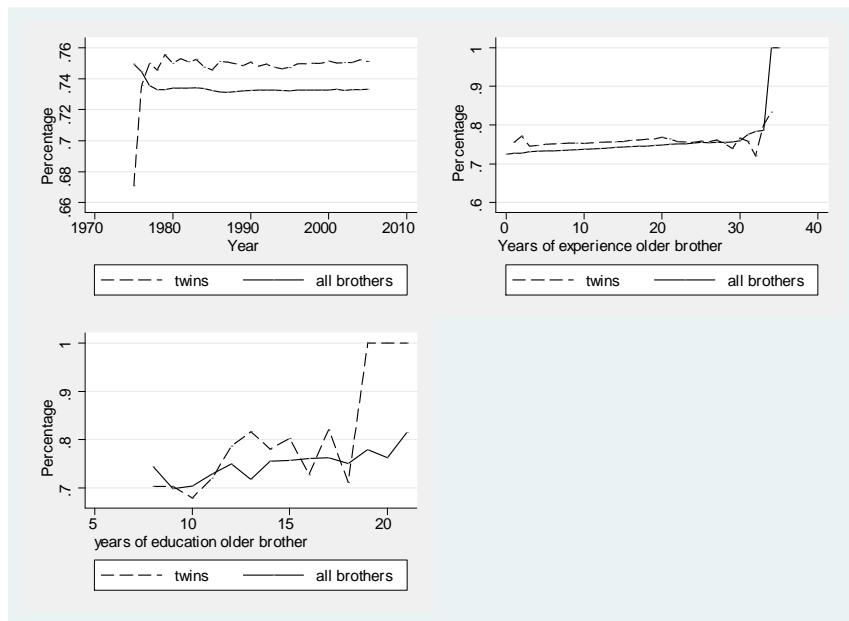


Figure 3: Percentage of brother couples where both of them have children, or both of them have no children. The inverse gives the percentage of brother couples with unequal fertility outcome.

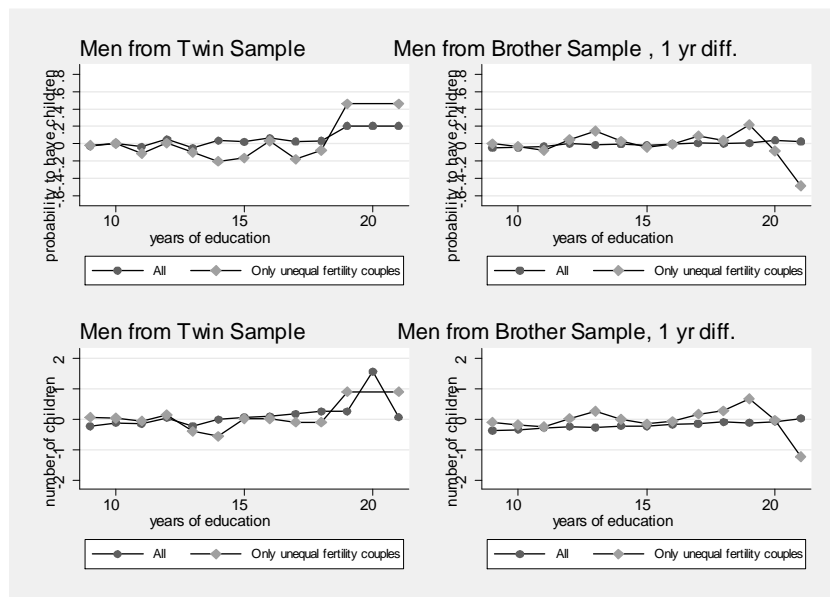


Figure 4: Testing correlation between fertility outcomes and years of education - the normalized graph.

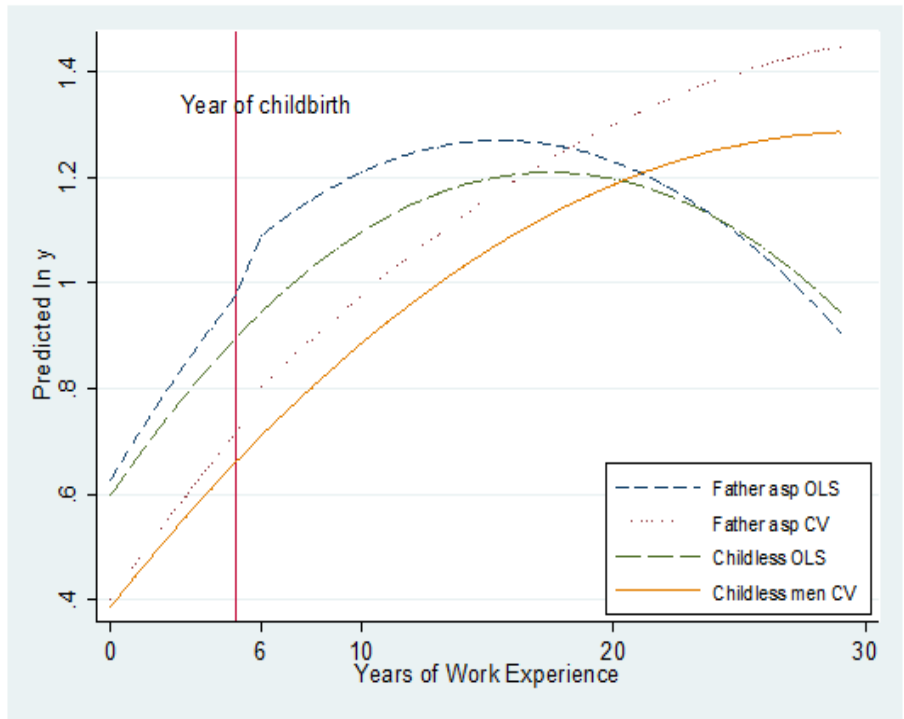


Figure 5: Predicted earnings profiles for a man who is continuously working for 5 years since first entry into the labour market, is then entering fatherhood and continues working until 30 years of work experience are completed. Estimates from Table 3 Column 1 and Table 4 Column 4 are used. See text for explanations.

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

*Source: Statistics Norway.

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

	Brothers		Twin Sample	
	mean	sd	mean	sd.
$\Delta \log(\text{earnings})$.09	.59	-.02	.52
$\Delta(\text{father type})$.02	.51	-.00	.50
$\Delta \text{ yrs of education}$.05	3.19	-.04	2.75
$\Delta \text{ yrs of experience}$	3.00	3.39	.00	2.45
.. experience squared	84.47	113.25	.37	80.61
$\Delta \text{ yrs of experience} * \text{fathertype}$	2.74	7.90	-.00	7.90
... experience squared *fathertype	75.22	178.42	-.29	176.95
$\Delta(\text{post birth})$.12	.57	-.01	.54
$\Delta \text{ yrs of experience} * \text{post-birth}$	2.49	6.76	-.23	6.14
.. experience squared *post-birth	48.73	138.89	-4.66	125.60
Difference in year of birth	-1.16	.41	0	
Number of observations	867028		22224	

Appendix Table A3: Earnings regression results, brothers 1 year age difference: Number of children

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

Extended specification of Table 4, column 3, is estimated. For further explanations see Table 4.

Appendix B: Derivation of empirical framework

This appendix shows how the earnings equation in eq(1) can be derived in a treatment framework as in the study by Heckman and Hotz (1998) that estimated the return to training using non-experimental data. One issue in their application is that those who enter training, the treated, are different before treatment compared to the non-treated, those who do not enter training. This is a similar setting to ours where we want to account for that fathers-at-some-point are on different earnings paths from the beginning of their career in comparison to childless men.

Let $\ln y_{ift}$ be observed logarithmic earnings of individual i in period t , and $\ln y_{it}^*$ the logarithmic earnings in the absence of children. (We add subscript for family f which we return to later.) The indicator variable a_{ift} equals one if a person becomes a father (treated) and zero otherwise (untreated) and γ_t is the effect of children in period t . We assume that the effect of children is identical for all persons. The period of childbirth is denoted as k . Then we can write:

$$\begin{aligned} \ln y_{ift} &= \ln y_{ift}^* + \gamma_t a_{ift}, & a_{ift} &= 1, t > k \\ \ln y_{ift} &= \ln y_{ift}^*, & a_{ift} &= 0, t \leq k \end{aligned} \quad (1)$$

We will focus on estimating the mean effect and the difference in mean post-birth earnings of fathers and non-fathers is:

$$\begin{aligned} &E[\ln y_{ift} | a_{ift} = 1] - E[\ln y_{ift} | a_{ift} = 0] \\ &= E[\gamma_t | a_{ift} = 1] + \{E[\ln y_{ift}^* | a_{ift} = 1] - E[\ln y_{ift}^* | a_{ift} = 0]\}, \end{aligned} \quad (2)$$

The expression in parentheses is the selection bias which is present if the assignment to fatherhood is not random.¹

Suppose $\ln y_{it}^*$ is a linear function of a set of observed characteristics X_{it} and unobserved characteristics ϵ_{it} .

$$\ln y_{ift}^* = X_{ift} \beta_1 + \epsilon_{ift} \quad (3)$$

¹Since 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 make the estimation approach more complicated.

Then observed earnings may be written as

$$\ln y_{ift} = X_{ift}\beta_1 + \gamma_t a_{ift} + \epsilon_{ift} \quad (4)$$

In the empirical application the vector X contains a constant and standard 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 .²

The decision to become a father can be quite generally written in terms of an index-function framework, where the index, father, is a function of both observed, Z , and unobserved, u , characteristics:

$$father_{ift} = Z_{if}\alpha + u_{ift} \quad (5)$$

Then, the i th individual's fatherhood status is

$$a_{ift} = 1 \quad \text{iff} \quad father_{ift} > 0 \quad (6)$$

$$= 0 \quad \text{otherwise} \quad (7)$$

We assume u is iid across individuals and distributed independently of Z_i . This means that the dependence between ϵ and a can arise because of dependence between Z and ϵ , i.e. selection on observables, or dependence between ϵ and u , 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.³

To address selection bias on observable characteristics we employ a linear control function estimator. Inserting a linear version of $E(\epsilon|X, Z)$ ⁴ in equation (4) yields

$$\ln y_{ift} = C_{ift}\delta + \gamma_t a_{ift} + \tilde{\epsilon}_{ift} \quad (8)$$

where C_{ift} denotes the vector of all variables included in either X or the vector of instruments Z , $\tilde{\epsilon}_{it} = \epsilon_{it} - E(\epsilon|a_i, C_i) = \epsilon_{it} - E(\epsilon_{it}|C_i)$. In our application

²The effect of children, $\gamma_t = \gamma_t(a, ex_{post})$, is modelled as a function of the indicator variable a_i and post-birth work experience, ex_{post} , in order to capture the potential non-linear pattern of the effect after the first childbirth.

³Clearly, the direction of selection bias can go either way.

⁴We 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.

Z will include the indicator variable whether the man is a father-at-some point, *fathertype*, or not. We exploit the fact that in the data we observe earnings even before a man becomes a father, and that we can distinguish fathers-at-some point from childless men. Additionally, we use the indicator interaction with years of experience (squared) since entry, *ex*.

In sum, we write the log earnings equation:

$$\ln y_{ift} = \gamma_t a_{ift} + \beta' X_{it} + \delta' Z_{ift} + \nu_f + \mu_{if} + w_{ift} \quad (9)$$

where we write the error term $\tilde{\epsilon}_{ift} = \nu_f + \mu_{if} + w_{ift}$, and where individuals are indexed by i , and family by f , and time by t .

Reference: Heckman, J.J. and V.J. Hotz (1989): Choosing among alternative non-experimental methods for estimating the impact of social programs: The case of manpower training, Journal of the American Statistical Association, 84(408), 862-874.

Appendix C

Appendix Table C1: 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 C2: 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 1 All	CV 2 <3 yr age diff	CV 3 <2 yr age diff	OLS	CV 1
years of education	0.039*** (0.000)		0.025*** (0.000)	0.022*** (0.000)	0.020*** (0.001)	0.028*** (0.001)	0.010*** (0.002)
experience	0.074*** (0.000)	0.076*** (0.001)	0.069*** (0.001)	0.063*** (0.001)	0.060*** (0.002)	0.069*** (0.003)	0.057*** (0.005)
experience ²	-0.002*** (0.000)	-0.002*** (0.000)	-0.002*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.002*** (0.000)	-0.001*** (0.000)
Differential effect in entry earnings and experience pre-birth							
father-at-some-point	0.020*** (0.003)		0.036*** (0.004)	0.040*** (0.006)	0.045*** (0.010)	-0.008 (0.016)	0.100*** (0.023)
experience *father	0.012*** (0.001)	0.026*** (0.000)	0.009*** (0.001)	0.007*** (0.001)	0.009*** (0.002)	0.019*** (0.003)	-0.001 (0.004)
-at-some-point	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.001*** (0.000)	-0.000* (0.000)
experience ² *father							
-at-some-point							
Differential effect of children							
post-birth (<i>a</i>)	0.090*** (0.002)	0.058*** (0.002)	0.078*** (0.002)	0.048*** (0.004)	0.047*** (0.006)	0.085*** (0.010)	0.032* (0.013)
experience post-birth (<i>exp_{post}</i>)	-0.015*** (0.000)	-0.026*** (0.000)	-0.013*** (0.000)	-0.005*** (0.001)	-0.003* (0.001)	-0.021*** (0.002)	-0.003 (0.003)
experience ² post birth (<i>exp_{post}</i> ²)	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.001*** (0.000)	0.001*** (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. Cohort of men born 1955-65.

Earnings potentially affected by the paternity leave reform in 1993 are excluded.

All regressions control for birth order and time effects. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Appendix Table C3: 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*** (0.000)	-0.003*** (0.000)	0.018*** (0.000)	-0.003*** (0.000)
experience	0.021*** (0.000)	0.006*** (0.000)	0.015*** (0.001)	0.005*** (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	0.060*** (0.003)	0.004* (0.002)	0.014 (0.010)	-0.003 (0.005)
experience *father -at-some-point	-0.003*** (0.001)	0.001*** (0.000)	0.001 (0.001)	0.001 (0.001)
experience ² *father -at-some-point	0.000*** (0.000)	-0.000*** (0.000)	0.000 (0.000)	-0.000 (0.000)
Differential effect of children (post first childbirth)				
post-birth	0.013*** (0.001)	0.008*** (0.001)	0.002 (0.002)	0.004*** (0.001)
experience post-birth	-0.003*** (0.000)	-0.001*** (0.000)	-0.004*** (0.000)	-0.001*** (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

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 in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$