Journal of Public Economics 239 (2024) 105201

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

Short communication

# How does parental divorce affect children's long-term outcomes?[☆]

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ARTICLE INFO

*JEL classification:*
J12
D13
J13
J24

*Keywords:*
Divorce
Children
Human capital
Fertility
Workplace gender balance

ABSTRACT

Many papers report a negative association between parental divorce and child outcomes. To provide evidence whether this correlation is causal, we exploit idiosyncratic variation in the extent of gender balance in fathers' workplaces. Fathers who encounter more women in their relevant age–occupation–group at work are more likely to divorce. This result is conditional on the overall proportion of female employees in a firm and on detailed industry affiliation. Parental divorce has persistent, and mostly negative effects on children that differ between boys and girls. Treated boys have lower levels of educational attainment, worse labor market outcomes and are more likely to die early. Treated girls also have lower levels of educational attainment, but they are also more likely to have children at an early age (especially in their teens). However, treated girls lose less in terms of employment. This could be a direct consequence of teenage motherhood, which initiates early entry into the labor market.

## 1. Introduction

There is a strong negative association between parental divorce and a wide range of child outcomes. This nexus is persistent and leaves children from divorced parents worse off even as adults. Compared to other children, they have lower human capital and exhibit lower productivity. Most scholars are aware that it is not clear to which degree this relationship is causal (see, e.g., Manski et al., 1992; Painter and Levine, 2000; Amato, 2010; Bhrolcháin, 2013; Gähler and Palmtag, 2015). A number of confounding factors that provoke parental divorce may also be detrimental to child outcomes.

To answer the question of whether children are causally affected by parental divorce, it is essential to have exogenous variation in the divorce likelihood. Moreover, it is crucial to construct a valid empirical counterfactual to ascertain the causal channels through which children are affected. If one were to use child outcomes emerging from a stable and healthy family background as a benchmark, it would be reasonable to expect a negative effect of divorce. Probably a more relevant counterfactual situation is a family background characterized by parental conflicts. In such a situation, children may even benefit from divorce if the post-divorce situation is comparably more beneficial than growing up in a two-parent household fraught with conflicts.

We exploit idiosyncratic variation in the extent of gender balance in fathers' workplaces within an instrumental variables (IV) approach to establish a causal effect.[1] McKinnish (2004, 2007) and Svarer (2007) show that individuals who have workplaces with a larger fraction of coworkers of the opposite sex are significantly more likely to divorce later. To the best of our knowledge, we are the first to exploit this empirical relationship within an IV approach.

The correlation between the gender balance in the workplace and the likelihood of divorce is in line with the economic model of family formation and dissolution (Becker, 1973, 1974; Becker et al., 1977). This model stresses imperfect information at the time of marriage, and

☆ We thank the Editor Jessica Pan and two anonymous referees for excellent comments that significantly improved the paper. For helpful discussions and comments, we would also like to thank Alexander Ahammer, David Dorn, Christian Dustmann, Martin Huber, Erik Plug, Anja Prummer, Oystein Kravdal, Andrea Leiter-Scheiring, the participants of ESPE and EALE and the conference on early-child development in Munich. The usual disclaimer applies. We gratefully acknowledge financial support from the Austrian Science Fund (FWF) and the Austrian Federal Ministry of Science, Research and Economic Affairs (bmwfw), the Christian-Doppler Laboratory and the National Foundation of Research, Technology and Development.

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[1] We focus on fathers, since our identification strategy requires a persistent labor force participation. Mothers in our sample have significantly less consistent labor force participation over time. A significant share drops out (at least temporarily) after the birth of their first child.

https://doi.org/10.1016/j.jpubeco.2024.105201
Received 19 December 2022; Received in revised form 2 May 2024; Accepted 4 August 2024
Available online 29 August 2024

the acquisition of new information while married as key determinants of divorce. New information regarding alternative outside options, i.e. extramarital relationships, is decisive. Gender-balanced workplaces reduce the cost of extramarital search and allow married individuals to meet alternative mates, which increases the likelihood of divorce. Thus, we aim to identify the causal effect of divorce for the child whose father left the family because he met a new partner at work. We argue that this research design evaluates a realistic divorce–scenario and offers a well-balanced relationship between internal and external validity.

Existing evidence is hard to interpret due to the lack of clarity in defining the counterfactual situation and the lack of a convincing research designs (McLanahan et al., 2013).

One strand of literature tries to exploit variation in the age at divorce among siblings. However, such sibling-fixed-effects estimations are often highly sensitive to specification issues concerning birth order or cohort effects (Sigle-Rushton et al., 2014). Bedard and Deschenes (2005) propose the sex of the firstborn child as an IV for marital dissolution. Gonzalez and Viitanen (2018) use the introduction of divorce in Mediterranean countries to examine cohorts who grew up before and after legal divorce legislation.

Our identifying assumption is that the gender balance in the father's workplace affects his children only through the channel of divorce. While this assumption cannot be directly tested, the richness of our data allows us to address most concerns: Unlike McKinnish (2004, 2007), we can calculate the extent of gender balance not only at the industry–occupation–level, but also at a more disaggregated level. We define the extent of gender balance as the share of female coworkers within a firm who belong to a certain age–occupation group. This plant-age-occupation specific measure has two advantages. First, it captures the actual on-the-job contact possibilities with the opposite gender. Second, it allows us to control for firm characteristics in our estimation analysis. For instance, we use information on firms' NACE three-digit numerical code to include industry fixed-effects. Thus, we do not have to assume that the fathers' selection of industry affiliation is exogenous.[2] We can also control for the *age-specific* share of female coworkers within a firm. Thus, our estimates remain valid even if fathers entering female-dominated firms/industries have unobserved parental characteristics that may affect child outcomes. For instance, one might argue that fathers who enter female-dominated firms/industries pursue a different parenting style, which also affects child outcomes. Alternatively, fathers who intentionally select female-dominated firms/industries to meet more potential partners may be less family-oriented and invest less in their children. We allow for a selection into certain firms/industries, and only have to assume that selection into a firm with a particular age–occupation specific sex ratio within an industry is exogenous. This assumption seems plausible: while job applicants may have a rough idea of whether a firm is male- or female-dominated, specific age–occupation sex ratios may be difficult to observe in advance. Our findings show that father's age–occupation specific sex ratio is not correlated with pre-determined variables (such as maternal education) nor with the child's health at birth. Furthermore, as we measure the IV at the time of the child's birth, a significant share of potential female partners has typically joined the firm after the father.

While the external validity of an estimate is, in general, hard to assess, our approach provides us with a treatment effect at a margin of broad interest. Our estimates inform us about the consequences of divorce in situations where the separation was triggered by the likelihood that the father is meeting a new partner at work. We consider this type of divorce to be a realistic scenario, and in principle preventable. The effect of this kind of divorce on children may differ from that of other divorces where the parents decide that they are no longer a good match or when there is domestic violence.

The importance of specific causal pathways for children's outcomes will depend on the actual post-divorce living arrangements. The most important legal aspects are the allocation of custody and the regulation of the non-custodial parent's support obligations. After divorce, the family is separated into two households and it is no longer possible for the parents to spend time with their child jointly. In addition, one parent (the non-custodial one) typically spends less total time with the child as compared to the counterfactual situation without divorce. It is not possible to determine how this affects the child's development. However, it is reasonable to assume that the child is negatively affected by these changes in time allocation. Related to this, there are two main channels that could reduce the financial investment in children. Firstly, during marriage the family could share a number of non-rival goods. To maintain the pre-divorce consumption level, more financial resources are needed. Secondly, the non-custodial parent's incentives to invest in his or her child are altered (Weiss and Willis, 1985). A reduction in the control over child expenditures and the lack of opportunity to monitor and enforce an optimal level typically result in lower contributions as compared to marriage (Del Boca and Flinn, 1995; Del Boca, 2003). Finally, other aspects of parenting may also change. Most importantly, children in divorced families are less likely to experience good gender role models. An often raised concern is boys lacking a good male role model (Amato, 1993).

Our results show that parental divorce — due to a high number of female potential partners in father's workplaces — has a negative effect on children's long-term outcomes. We find a substantially lower level of educational attainment for both sexes. The effects on family formation behavior, labor market and health outcomes differ by sex. In the case of boys, we find little effect on their fertility or marriage behavior. However, we find a higher likelihood of early mortality and worse labor market outcomes. In the case of girls, we find strong effects on their fertility behavior. Parental divorce increases the likelihood of a pregnancy during teenagehood and up to the early twenties. Regarding labor market outcomes, we find some evidence for more unemployment for girls.

Ignoring the endogeneity of parental divorce leads — in some cases — to a biased estimate showing e.g. less detrimental effects on children's educational attainment. This bias implies that parental divorce is correlated with unobserved family characteristics, which facilitate children's accumulation of human capital. While in some countries there is a small negative correlation between education and divorce (Matysiak et al., 2014), being in a white-collar job in Austria correlates with an increase in divorce risk.

Besides research on the causal effect of parental divorce, our paper is related to two other strands of literature. Firstly, scholars are interested in the effect of growing up under different divorce law regimes. A number of papers compare the long-run outcomes of children who grew up under mutual consent divorce law regime versus a unilateral divorce law regime (see e.g., Wolfers, 2006; Matouschek and Rasul, 2008). The identification of effects on children in these papers is based on variation across U.S. states and across years in which states have moved to unilateral divorce law (see, e.g., Gruber, 2004). Secondly, scholars analyze the effect of parental death on children's outcomes (see, e.g., Corak, 2001; Lang and Zagorsky, 2001). While parental death is certainly more drastic than parental divorce, both events result in children growing up (at least partly) with only one parent.

## 2. Data

The empirical analysis is based on administrative data from Austria. To define our sample we first select all children born to married mothers between 1976 through 1987.[3] To generate our treatment variable,

[2] Due to data restrictions we focus on private-sector jobs, and thus we largely exclude sectors such as education and health.

[3] Among these birth cohorts, about 16% of all children were born out of wedlock. Our analysis does not include cohabiting couples.

we categorize a child as treated if her/his parents divorced before their 18th birthday. Children whose parents never divorced constitute the non-treated. Divorces took place between 1976 and 2005.

To generate our IV, we use the *Austrian Social Security Database* (ASSD). These data are administrative records to verify pension claims and are structured as a matched employer–employee dataset. For each father we can observe the daily data on his workplace and his coworkers. For each worker we obtain his/her basic socio-economic characteristics, such as age, broad occupation, experience, tenure, and earnings; the latter is provided per year and per employer. The limitations of the data are top-coded wages and the lack of information on working hours (Zweimüller et al., 2009).

To use our IV strategy, we focus on fathers employed (as wage earners) at the time of birth of the child.[4] While the employment restriction reduces the external validity of our study, it should be noted that male employment was very high in the relevant cohorts. Between 1976 and 1987, around 93 percent of all men aged 20 to 50 were in employment (see Appendix Figure B.1). Moreover, the divorce risks for fathers in our sample and those outside our sample are not very different (see Appendix Figure B.2).

To assess the long-run impact of divorce on children we analyze their human capital outcomes and own family formation behavior in early adulthood. These are of particular importance to adolescents. The necessary information to generate an educational outcome is from the database of the *Federal Ministry of Labor, Social Affairs and Consumer Protection*. We define a binary variable equal to one if a person has ever attended college.[5] In the context of the Austrian education system, this variable also comprises information on the type of secondary school. College attendance implies that this person graduated from a higher secondary school (grade 12). Labor market outcomes can be tracked in the ASSD. We check the labor market status, fertility, and marriage behavior up to the age of 25 years. Our findings indicate that especially in the case of girls it is essential to study all outcome dimensions to fully understand the effect of parental divorce. Finally, we observe early mortality (i.e., below the age of 26).

## 3. Descriptive statistics

Our estimation sample comprises 355,100 children, about 13.5 percent of whom experienced parental divorce before they turned 18 years of age. A comparison of child outcomes by treatment status suggests that children from divorced parents have worse human capital outcomes (see Appendix Tables B.1 and Table B.2) A comparison of average family outcomes shows that treated children are more likely to be young parents and to marry early. The comparison of the covariates reveals further observable differences in children's and paternal characteristics. For instance, treated children are less likely to be male and more likely to be first-born. The former pattern is consistent with a paternal preference for boys over girls (Dahl and Moretti, 2008). The distribution of parents' religious denomination and ethnic background shows that children from uniformly catholic and Austrian families are least likely to be affected by divorce. We also observe differences in the fathers' employment characteristics. Compared to fathers of non-treated children, fathers of treated children tend to have less favorable labor market outcomes.

Finally, we compare fathers' firm characteristics. Divorcing fathers tend to work in larger firms and in firms with a lower share of blue-collar workers, and in firms with a higher share of female workers in all age groups. This unconditional difference could reflect either the effect of gender balance on divorce or a spurious correlation (i.e., there are more white collar-workers in firms with higher shares of women).

## 4. Estimation strategy

To assess the effect of parental divorce on child $c$ born to parents $p$ we examine several binary long-run outcomes $O_c^p$, for which we estimate the following equation:

$$O_c^p = \alpha + \tau D_c^p + \beta_c \mathbf{X}_c + \beta^p \mathbf{X}^p + \beta^f \mathbf{X}^f + \varepsilon_c^p. \tag{1}$$

The outcome variables capture the child's educational attainment, labor market success, fertility behavior, marriage behavior, or mortality up to 25 years of age. These outcomes capture major socio-economic decisions; the age range 25 is chosen for data availability reasons. The treatment is captured by the binary indicator $D_c^p$, which is equal to one if parents $p$ divorce before their child $c$ turned 18 years old.[6] We include a comprehensive set of covariates capturing child ($\mathbf{X}_c$), parents' ($\mathbf{X}^p$) and father's employment and firm characteristics ($\mathbf{X}^f$). The child characteristics are measured at birth and comprise parity, multiple birth, and birth weight. The parental characteristics capture different dimensions of assortative mating (measured at the time of marriage), which have been shown to affect the divorce hazard in Austria (Frimmel et al., 2013). We control for the father's age, the spouses' age difference, religious denominations, and citizenship. We also include a binary variable capturing the few cases (about five percent) where the parents were employed in the same firm before the birth of the index child. The father's employment characteristics are measured at the time of the child's birth and comprise information on broad occupation, daily wage, and job tenure. The father's firm characteristics are measured at the closest possible date to firm establishment[7] and comprise information on firm size, share of blue-collar workers, age-specific share of women, industry affiliation (215 NACE three-digit codes), and location fixed-effects. To account for secular trends, we include a child birth cohort trend and a parental marriage cohort trend. Finally, we control for the quarter of birth.

### 4.1. Instrumental variable approach

To identify a causal relationship we use variation in the extent of gender balance in fathers' workplace at the time of $c$'s birth.[8] The basic idea is that the availability of potential partners at the workplace will make interaction more likely. As actual interactions at the workplace are unobservable, we have to construct a quantifiable indicator for gender balance at the firm level. We suggest an occupation- and age-specific variable. With regard to occupation, we distinguish between blue and white-collar workers. Due to the different tasks these two groups perform (i.e. manual labor versus desk job) there is plausibly more interaction within groups than across groups. Moreover, given that white-collar workers typically have higher educational attainment than blue-collar workers, prevailing assortative mating patterns make a coworker from the other group a less probable partner. Age is likely to be an even more important factor determining a potential partner. We define potential female partners to be not younger than 8 and not older than 3 years. This specification aligns with actual age-differentials at marriage (Frimmel et al., 2013).[9] Our IV is thus defined as the number

[4] We exclude 21,062 self-employed, 36,176 farmers, 14,260 apprentices, 13,912 unemployed, 1944 fathers on long-term sick leave, and 99,503 fathers who are either out-of-labor force or civil servants. In early years we cannot distinguish between the two latter groups.

[5] More detailed information about education is unavailable.

[6] Appendix Figure B.3 shows the distribution of children's age at divorce. We can see an increasing trend to the age of about three, followed by a rather flat development to the age of nine, and a somewhat inverted u-shaped pattern up to the age of eighteen.

[7] In 23 percent of the cases, we measure the characteristics at the time of the establishment of the firm. The remaining 77 percent of the cases, are firms which were founded before 1972 (i.e., before our data-set starts). Here, we measure the characteristics in January, 1972.

[8] Alternative measurement times, such as the time the father entered the firm or, alternatively, at the time of marriage, give very comparable results (see Appendix Figure B.4).

[9] Other specifications of the "relevant" age range provide very comparable results.

of female employees in the father's occupation group $o$ and age range $a$ relative to the sum of all workers in the same occupation and age range:

$$♀_c^{o,a} = \frac{\sum female_c^{o,a}}{\sum female_c^{o,a} + \sum male_c^{o,a}} \tag{2}$$

A higher $♀_c^{o,a}$ is associated with a greater extent of gender balance. Such a percentage — when compared to the sheer size of the female work force — also captures the fact, that in this matching market other male workers may act as competitors. Fig. 1 displays the distribution of $♀_c^{o,a}$ by the child's sex. Two things are worth noting. First, the distribution looks the same for fathers of girls and boys. Second, there is a substantial degree of sex segregation in Austrian workplaces. Put differently, a significant proportion of fathers have no (or few) female coworkers in the respective age–occupation cell. This skewed distribution can be partly explained by the large number of small firms in Austria, where the probability of having any female colleague in the relevant age–occupation group is simply small. Still, there is considerable variation in the extent of gender balance, which can be exploited in our first-stage estimation:

$$D_c^p = \gamma + \kappa ♀_c^{o,a} + \Gamma_c \mathbf{X}_c + \Gamma^p \mathbf{X}^p + \Gamma^f \mathbf{X}^f + \mu_c^p \tag{3}$$

The parameter of primary interest $\kappa$ shows the increase in parental divorce probability if the gender balance in the father's workplace increases by one (i.e., essentially from the sample minimum of zero to the sample maximum of one).

### 4.2. Identifying assumptions

The identifying assumption is that gender balance in the father's workplace affects his child only through the channel of divorce and is uncorrelated with any confounding factor included in $\varepsilon_c^p$. We see two potential concerns. First, one might be worried that specific men select themselves in occupations or industries with a high share of female workers. For instance, men who choose female-dominated jobs may also have a different parenting style. Alternatively, men who strategically select workplaces to find extramarital affairs may be less inclined to invest in their children. Another concern is related to a potential effect of the gender balance in the father's workplace on the intra-household allocation of resources. So-called *external threat point models* claim that bargaining within marriage is conducted in the shadow of the possibility of divorce (Manser and Brown, 1980; McElroy and Horney, 1981).[10] If a high extent of gender balance in the husbands' workplaces increases their expected well-being outside the marriage (i.e., after divorce), then intra-household distribution within marriage could reflect male preferences more strongly in the case where husbands have more female coworkers in the relevant age–occupation cell. This effect would be problematic for our identification strategy if a strengthened bargaining position of fathers lead to lower investments in children.

An important feature of our set-up is that we control for a comprehensive set of industry fixed-effects (215 groups), and the firm's *age-specific* share of female coworkers. Thus, we not only allow for a selection into certain industries, but also for a selection into firms with many female workers. Upon that, we still need to assume that the share of women in a particular age–occupation cell is exogenous. While this assumption might be harder to defend in case of small firms where for those outside the firm, it could be possible to assess the actual gender balance in a given age–occupation cell, this should be much more challenging in case of larger firms. This is true for men considering a job in a firm, but also for wives of men with existing jobs in a firm. To test whether firm size matters in our IV approach, we split our sample and replicate our analysis (see Appendix Figure B.5). We obtain very comparable effects for families with husbands in small (less than 100 employees) and large firms.

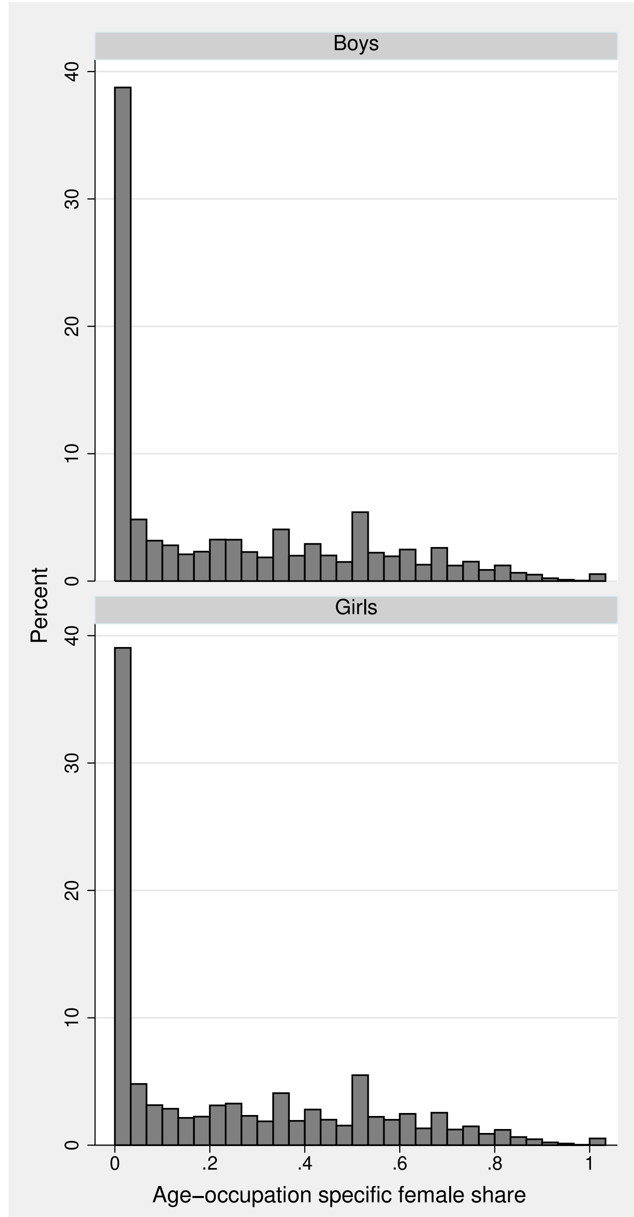


**Fig. 1.** Distribution of the father's age–occupation-specific sex ratio at work, by the child's sex. *Notes*: This figure depicts the father's age-occupation specific sex ratio at work measured at the time of the birth of the child for boys and girls. These figures are calculated based on data from *ASSD*.

### 4.3. Plausibility checks

While our identifying assumption is fundamentally untestable, we provide two types of plausibility checks. First, we check whether our IV is correlated with important pre-determined variables, such as inputs in the production of children's human capital and characteristics of marriage. We consider maternal education and maternal labor force participation as strong predictors of child outcomes. As such, there is a risk that these variables are also correlated with many (unobserved)

[10] In contrast, so-called *internal threat models* (such as separate-spheres model) or common-preference models predict no impact of divorce on relative bargaining power within the household (Lundberg and Pollak, 1996).

determinants of children's long-term outcomes. Thus, if our IV is correlated with these maternal characteristics (measured pre-birth), we would be concerned that it is also correlated with other confounding factors. Our data records mother's educational attainment since 1984. Thus, we can examine the relationship between our IV and maternal education for a subsample of about 39%. The information on maternal labor force participation is taken from the ASSD, and measured in the year before the birth of the child. For the analysis of the latter outcome we use the same subsample as for maternal education.[11]

Panel A of Table 1 summarizes the results from this plausibility check. We perform sex-specific regressions of different measurements of maternal education on our IV along with our basic set of covariates. In columns (I) and (II), the dependent variable is an ordinal variable capturing five different levels of educational attainment. In columns (III) and (IV), the dependent variable is binary and indicates whether the mother has a college degree or not. Across specifications, we do not find a statistically significant conditional correlation between any measurement of maternal education and our IV. The estimated coefficients are also quantitatively negligible. In columns (V) and (VI), the dependent variable is binary and indicates whether the mother was in the labor force in the year before birth. In columns (VII) and (VIII), the dependent variable captures the daily wage for the sub-set of employed mothers. We do not find a significant relation between any maternal labor market outcomes and our IV.

In Panel B of Table 1 we provide an equivalent analysis for different marriage characteristics using all families in our estimation sample. We examine whether our IV is correlated with parents' age at marriage (see columns I and II) or parents' age at first birth (see columns III and IV). In neither case, we see a statistically significant conditional correlation. We also check whether our IV is correlated with the number of marital births (see columns V and VI). While the number of marital births is not pre-determined, it is reassuring that we also do not find an economically or statistically significant correlation in this case. Finally, in columns (VII) and (VIII) we demonstrate that our IV is also not correlated with the timing of the first marital birth (i.e., relative to the date of marriage).

Second, we check whether our IV is correlated with early child outcomes (measured long before divorce). We use information on children's health at birth, and examine their birth weight and their gestational length. A correlation between the child's health at birth and our IV would raise concerns about the validity of our identifying assumption. Our data comprises information on gestational length since 1984. Birth weight is available for a longer period of time; however, for the purpose of comparison we focus on the same sample of children across outcomes. Panel C of Table 1 summarizes sex-specific regressions for four measures for health at birth. We do not find any significant relation between the IV and the respective measure of children's health at birth.

### 4.4. Method of estimation

Our estimation setting has two specific features. First, both the outcome variable(s) and the endogenous treatment are binary. Second, the treatment probability is rather low. In our sample, only 13.5% of the families get divorced before the child's 18th birthday. There are two basic estimation strategies. One ignores the binary structure of the outcome and treatment variables and employs a linear IV model to estimate the treatment effect $\tau$. The second option explicitly accounts for the binary structure and employs a specialized estimation method. Since the recent econometric literature has shown (Chiburis et al., 2012; Basu et al., 2018) that linear IV models perform especially poorly in such a setting, when treatment probabilities are rather low, we have chosen the second option.

We use a *Two-Stage Residual Inclusion* (2SRI) procedure (Terza et al., 2008). The first stage (Eq. (3)) of this *control function approach* is estimated with a logistic regression. The second stage (Eq. (1)) is also estimated with a logistic regression and includes the residual from the first stage as an additional covariate to substitute for unobservable latent factors. In all our estimations, we cluster standard errors on families. This accounts for the fact that our dataset includes siblings (166,387 fathers have one child, and 86,834 fathers have two or more children).

## 5. Estimation results

The naïve logit estimation results tabulated in columns (Ia) and (Ib) of Table 2 confirm the pattern observed in the descriptive statistics: children from divorced parents are less likely to attend college. This holds for boys (minus 6.2%-points or 27% of the sample mean) and for girls (minus 5.4%-points or 18%). Looking at the estimated effects of the covariates, we find that most prior expectations are confirmed: College attendance is more likely for first-borns and for children of older fathers. Among the most important predictors for a child's college attendance are the father's employment characteristics. A child of a blue-collar worker is about 17 to 18%-points less likely to attend college than a child of a white-collar worker. A potentially surprising correlation is that holding other things constant, children with non-native parents are more likely to go to college. We also find some correlations between fathers' firm-level characteristics and children's college attendance. For instance, we see a negative correlation for a high share of blue-collar workers, or a low share of female workers aged between 20 to 30 years. Notably, our IV estimation strategy controls for these and other firm characteristics, and exploits only variation in the share of female coworkers in a given occupation-age cell. Thus, our IV is not a simple firm-level variable, but a variable that varies across workers within a firm.[12] This makes our IV less suspicious to be correlated with confounding factors.

The estimation of our first-stage equations for boys and girls separately (3) are tabulated in columns (IIa) and (IIb) of Table 2. We find statistically significant positive effects for our IV — the age-and occupation-specific share of women in the father's firm — on the likelihood of parental divorce. The estimated effects do not differ for fathers of boys and girls. An increase in the extent of gender balance in the father's workplace from the sample minimum of zero to the sample maximum of almost one is predicted to increase the divorce likelihood by about 2%-points. As the overall divorce probability in our sample is equal to 13.5%, a 2%-point increase in divorce risk corresponds to an approximately 15% increase. This effect size is comparable in magnitude to McKinnish (2004) (11% change in divorce risk) but smaller than reported by Svarer (2007) in the Danish context (36%). The F-statistic of the IV is between 19 and 25. To the best of our knowledge, there is no specific study for 2SRI that provides threshold values for weak identification. For a comparable 2SLS estimation (i.e., with one endogenous variable and one IV) the critical F-value is 16.38 (Stock and Yogo, 2005). Taking this as a reference point, we can conclude that our IV is sufficiently strong.

The estimation output of our child-sex-specific second-stage equations for college attendance is presented in columns (IIIa) and (IIIb) of Table 2. This estimation procedure confirms the qualitative treatment effect obtained by the logit estimations. The 2SRI estimation, however, provides a quantitatively different estimate. Parental divorce, caused by

[11] Our main estimation results do not use information on mother's educational attainment and are based on a larger sample of children. It should be noted that our qualitative results do not change, although we lose some precision of the estimates if we use the reduced sample as in the case of the plausibility checks.

[12] We do not have enough fathers working in the same firm to control for firm fixed-effects.

**Table 1**
Plausibility checks: Conditional correlation between the IV and mother's outcomes and children's health at birth.

| | (I) | (II) | (III) | (IV) | (V) | (VI) | (VII) | (VIII) |
|---|---|---|---|---|---|---|---|---|
| Panel A: Maternal characteristics | | | | | | | | |
| | Maternal education | | | | Maternal labor market outcomes | | | |
| | Categorical var.[a] | | College degree | | Labor force[b] | | Daily wage[c] | |
| | Boys | Girls | Boys | Girls | Boys | Girls | Boys | Girls |
| IV | −0.034 | 0.012 | 0.005 | −0.004 | −0.013 | 0.006 | −0.037 | −0.121 |
| | (0.025) | (0.025) | (0.006) | (0.006) | (0.009) | (0.010) | (0.316) | (0.328) |
| Covariates | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of children | 69,934 | 66,319 | 69,934 | 66,319 | 69,934 | 66,319 | 36,305 | 34,378 |
| Mean of dep. variable | 2.42 | 2.40 | 0.07 | 0.07 | 0.55 | 0.55 | 27.33 | 27.33 |
| Panel B: Marriage characteristics: | | | | | | | | |
| | Age at marriage[d] | | Age at first birth[e] | | No. of marital births[f] | | Time to first birth[f] | |
| | Father | Mother | Father | Mother | Boys | Girls | First | Second |
| IV | −0.008 | 0.003 | −0.006 | −0.002 | 0.003 | −0.005 | 0.021 | 0.022 |
| | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) | (0.011) | (0.020) | (0.037) |
| Covariates | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of families | 79,035 | 79,035 | 79,035 | 79,035 | 79,035 | 79,035 | 79,035 | 13,631 |
| Mean of dep. variable | 26.01 | 23.08 | 28.35 | 25.43 | 0.60 | 0.57 | 2.37 | 3.60 |
| Panel C: Early child outcomes:[g] | | | | | | | | |
| | Birth weight[h] | | Low birth weight[i] | | Gestation length[j] | | Premature birth[k] | |
| | Boys | Girls | Boys | Girls | Boys | Girls | Boys | Girls |
| IV | 0.493 | −1.319 | 0.005 | 0.003 | 0.003 | −0.033 | 0.003 | 0.005 |
| | (1.061) | (1.040) | (0.004) | (0.005) | (0.033) | (0.034) | (0.004) | (0.004) |
| Covariates | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of children | 69,933 | 66,314 | 69,933 | 66,314 | 69,933 | 66,314 | 69,933 | 66,314 |
| Mean of dep. variable | 337.20 | 323.15 | 0.04 | 0.05 | 39.64 | 39.69 | 0.04 | 0.04 |

*Notes*: Estimation method: OLS with standard errors clustered on families in parentheses. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level respectively. Set of covariates equals the set of covariates used in our main estimations.

[a] Maternal education is measured as a categorical variable with value 1 for compulsory schooling, 2 for apprenticeship, 3 for vocational school, 4 for high school degree, and 5 for academic degree. Information on educational attainment is only available since 1984.

[b] The binary indicator 'In labor force' has value 1 if the mother is active on the labor market (either employed or registered as unemployed) in the year before the birth of the child.

[c] The daily wage (in euros) of the mother is measured the year before the birth of the child and is only available for employed mothers.

[d] Measured in years. Set of covariates exclude child characteristics but control for parental birth year.

[e] Measured in years. Set of covariates exclude child birth characteristics but control for age at marriage.

[f] Set of covariates exclude child characteristics. Fertility timing is measured in years.

[g] For comparison reasons we only consider child birth cohorts of 1984 or younger due to missing information before 1984 for some outcome variables.

[h] Birth weight is measured in dekagram, the set of covariates excludes birth weight.

[i] Low birth weight is a binary indicator with value 1 if birth weight is below 250 dekagram.

[j] Gestation length is measured in weeks

[k] Premature birth is a binary indicator with value 1 if gestation length is shorter than 37 weeks.

a higher degree of gender balance in the father's workplace, is predicted to reduce the child's propensity to attend college by about 10%-points for boys and by about 8%-points for girls. This is equivalent to 41% of the sample mean for boys, and about 27% for girls. Therefore, ignoring the endogeneity of parental divorce leads to a biased estimate showing less detrimental effects on children's educational attainment.

The endogeneity of parental divorce in the case of children's education can be assessed more formally with a Wald test on the coefficients of the first-stage residuals included in the second stage. As can be seen in columns (IIIa) and (IIIb), the first-stage residual is statistically highly significant and has a positive sign. This provides two conclusions. First, parental divorce is endogenous. Second, unobserved latent factors that promote divorce are positively correlated with children's human capital. Put differently, divorce is correlated with unobserved family characteristics, which themselves facilitate children to obtain higher educational attainment. This finding is consistent with the observed difference in the estimated treatment effects obtained by a naïve logit estimation and the 2SRI. Further, it is consistent with our finding that families with a blue-collar father, who tend to have a lower educational attainment and a lower socio-economic status, are less likely to divorce.

Table 3 summarizes the estimation results on demographic outcomes. Here, we examine the effect of parental divorce on early fertility, early marriage, and early mortality. We concentrate on the 2SRI results. In the case of fertility, we have two outcomes, which capture parenthood before the ages of 20 and 25 years of age, respectively. Early marriage is defined as having married before 20 years of age, and early mortality refers to death before the age of 25. For boys we find significant effects for early parenthood and early mortality. Teenage parenthood increases by 0.7%-points, and early mortality by 0.5%-points. Given that both outcomes have a low baseline incidence, the estimated effects are quantitatively important. Given that 29% of all deaths in this age range are caused by accidents, and 20% are caused by suicide, the estimated treatment effect most likely reflects either risky behavior or suicide.

In the case of girls, we find larger and statistically significant effects for early fertility. Both teenage parenthood as well as parenthood below 25 years of age increase due to parental divorce. The estimated effects are plus 2.5 and 5.4%-points (plus 68 and 31%), respectively. This finding is in line with the negative effect on educational attainment. We do not find a significant effect for early marriage nor early mortality. One possible interpretation of the effect on early fertility is that parental divorce changes girls' family-oriented behavior leading them to form their own families at an earlier age.

The estimation results for the human capital are summarized in Table 4. The first column reiterates the results for college attendance. In the remaining columns, we summarize the estimated effect of parental

**Table 2**
The effect of parental divorce on college attendance.

| | (Ia) | (Ib) | (IIa) | (IIb) | (IIIa) | (IIIb) |
|---|---|---|---|---|---|---|
| | Naïve Logit | | 2SRI: First stage | | 2SRI: Second stage | |
| | Boys | Girls | Boys | Girls | Boys | Girls |
| Divorce until age of 18 | −0.062*** | −0.054*** | | | −0.097*** | −0.081*** |
| | (0.003) | (0.003) | | | (0.013) | (0.014) |
| *Instrumental variable:* | | | | | | |
| Age-specific share of women | | | 0.020*** | 0.024*** | | |
| | | | (0.005) | (0.005) | | |
| *First-stage residual:* | | | | | | |
| Pearson residual | | | | | 0.012*** | 0.011** |
| | | | | | (0.004) | (0.005) |
| *Characteristics of children:* | | | | | | |
| First born child | 0.067*** | 0.084*** | 0.010*** | 0.010*** | 0.068*** | 0.084*** |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| Twin | 0.016* | 0.003 | −0.006 | −0.007 | 0.015* | 0.003 |
| | (0.009) | (0.010) | (0.008) | (0.008) | (0.009) | (0.010) |
| Birth weight (in dekagrams) | 0.000*** | 0.000*** | −0.000*** | −0.000*** | 0.000*** | 0.000*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| *Father's age at birth (Base group: Age 15–19):* | | | | | | |
| Age 20–24 | 0.080*** | 0.147*** | −0.064*** | −0.095*** | 0.077*** | 0.143*** |
| | (0.023) | (0.025) | (0.010) | (0.011) | (0.021) | (0.025) |
| Age 25–29 | 0.145*** | 0.215*** | −0.118*** | −0.152*** | 0.140*** | 0.209*** |
| | (0.021) | (0.025) | (0.010) | (0.011) | (0.021) | (0.025) |
| Age 30–34 | 0.190*** | 0.265*** | −0.149*** | −0.187*** | 0.183*** | 0.258*** |
| | (0.021) | (0.025) | (0.010) | (0.011) | (0.022) | (0.025) |
| Age 35–39 | 0.215*** | 0.284*** | −0.174*** | −0.214*** | 0.209*** | 0.277*** |
| | (0.022) | (0.025) | (0.011) | (0.012) | (0.022) | (0.025) |
| Age 40+ | 0.245*** | 0.301*** | −0.209*** | −0.257*** | 0.237*** | 0.294*** |
| | (0.022) | (0.026) | (0.012) | (0.013) | (0.022) | (0.026) |
| Age difference of partners | −0.007*** | −0.008*** | 0.006*** | 0.006*** | −0.007*** | −0.008*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| *Distribution of parent's religious denomination (Base group: Both catholic):* | | | | | | |
| Both undenominational | −0.002 | −0.010 | 0.054*** | 0.068*** | 0.000 | −0.008 |
| | (0.007) | (0.008) | (0.006) | (0.006) | (0.007) | (0.009) |
| Both other denomination | 0.006 | 0.004 | −0.033*** | −0.031*** | 0.005 | 0.003 |
| | (0.006) | (0.007) | (0.007) | (0.007) | (0.006) | (0.007) |
| Catholic, undenominational | −0.004 | 0.001 | 0.080*** | 0.086*** | −0.001 | 0.004 |
| | (0.005) | (0.006) | (0.004) | (0.004) | (0.005) | (0.006) |
| Catholic, other denomination | 0.019*** | 0.018*** | 0.044*** | 0.049*** | 0.020*** | 0.019*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.004) | (0.004) |
| Other, undenominational | 0.013 | −0.002 | 0.086*** | 0.076*** | 0.017 | 0.000 |
| | (0.011) | (0.014) | (0.009) | (0.010) | (0.012) | (0.014) |
| *Distribution of parent's citizenship (Base group: Both Austrian):* | | | | | | |
| Father Austrian, Mother Foreign | 0.002 | −0.000 | 0.015*** | 0.013** | 0.002 | 0.000 |
| | (0.006) | (0.007) | (0.006) | (0.006) | (0.006) | (0.007) |
| Father Foreign, Mother Austrian | 0.051*** | 0.057*** | 0.033*** | 0.046*** | 0.052*** | 0.058*** |
| | (0.008) | (0.009) | (0.007) | (0.007) | (0.008) | (0.009) |
| Both foreign citizens | 0.027*** | 0.048*** | 0.173*** | 0.179*** | 0.037*** | 0.056*** |
| | (0.009) | (0.010) | (0.006) | (0.006) | (0.010) | (0.011) |
| *Father's employment and firm characteristics at child's birth* | | | | | | |
| Blue collar worker | −0.176*** | −0.183*** | −0.009*** | −0.012*** | −0.177*** | −0.184*** |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Daily wage | 0.004*** | 0.004*** | −0.001*** | −0.001*** | 0.004*** | 0.004*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Tenure in firm (in years) | −0.004*** | −0.003*** | −0.007*** | −0.006*** | −0.004*** | −0.004*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Mother employed in same firm | −0.002 | −0.006 | 0.003 | −0.009** | −0.002 | −0.006 |
| | (0.004) | (0.005) | (0.004) | (0.004) | (0.004) | (0.005) |
| Firmsize | 0.000*** | 0.000*** | 0.000 | −0.000 | 0.000*** | 0.000*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Share of blue-collar workers | −0.025*** | −0.034*** | −0.017*** | −0.017*** | −0.026*** | −0.035*** |
| | (0.004) | (0.005) | (0.004) | (0.004) | (0.005) | (0.005) |
| Firm's share of women with age <20 | 0.014 | −0.011 | −0.026** | −0.031*** | 0.013 | −0.012 |
| | (0.011) | (0.013) | (0.010) | (0.011) | (0.011) | (0.013) |
| Firm's share of women with age 20–30 | 0.045*** | 0.035*** | 0.023*** | 0.029*** | 0.046*** | 0.036*** |
| | (0.009) | (0.010) | (0.007) | (0.007) | (0.009) | (0.010) |
| Firm's share of women with age 30–40 | 0.015 | 0.027** | 0.012 | 0.022** | 0.016 | 0.028** |
| | (0.011) | (0.012) | (0.008) | (0.009) | (0.011) | (0.012) |
| Firm's share of women with age 40–50 | 0.014 | 0.029** | 0.049*** | 0.041*** | 0.016 | 0.031** |
| | (0.014) | (0.015) | (0.010) | (0.010) | (0.014) | (0.015) |

(*continued on next page*)

**Table 2** (*continued*).

| | (Ia) | (Ib) | (IIa) | (IIb) | (IIIa) | (IIIb) |
|---|---|---|---|---|---|---|
| | Naïve Logit | | 2SRI: First stage | | 2SRI: Second stage | |
| | Boys | Girls | Boys | Girls | Boys | Girls |
| Firm's share of women with age >50 | 0.031* | 0.018 | 0.071*** | 0.039*** | 0.033* | 0.019 |
| | (0.017) | (0.019) | (0.013) | (0.013) | (0.017) | (0.019) |
| Regional, 215 industry, quarter of birth FE | Yes | | Yes | | Yes | |
| Marriage & child cohort trend | Yes | | Yes | | Yes | |
| No. of observations | 182,874 | 172,232 | 182,874 | 172,232 | 182,874 | 172,232 |
| F-Statistic of IV | | | | | 18.75 | 24.80 |

*Notes*: Estimation method: Logistic regressions. We use the first-stage Pearson residual in the 2SRI-estimation. Average marginal effects with standard errors clustered on families in parentheses. *, ** and *** indicate statistical significance at the 10%, 5% and, 1% level respectively.

**Table 3**
The effect of parental divorce on demographic outcomes.

| | (I) | (II) | (III) | (IV) |
|---|---|---|---|---|
| | Fertility | | Marriage | Mortality |
| | Before 20 years of age[a] | Before 25 years of age | Before 20 years of age[b] | Before 25 years of age |
| *Boys* | | | | |
| 2SRI | 0.007* | 0.012 | −0.001 | 0.005* |
| | (0.004) | (0.009) | (0.001) | (0.002) |
| Naïve logit | 0.009*** | 0.023*** | 0.002*** | 0.002*** |
| | (0.001) | (0.002) | (0.000) | (0.000) |
| Mean of dep. variable | 0.019 | 0.092 | 0.004 | 0.006 |
| F-statistic of IV | 18.75 | 18.75 | 18.75 | 18.75 |
| Control variables | Yes | Yes | Yes | Yes |
| No. of observations | 182,847 | 182,733 | 182,733 | 176,601 |
| *Girls* | | | | |
| 2SRI | 0.025*** | 0.054*** | 0.005 | −0.000 |
| | (0.005) | (0.011) | (0.003) | (0.001) |
| Naïve logit | 0.025*** | 0.055*** | 0.006*** | 0.000 |
| | (0.001) | (0.003) | (0.001) | (0.000) |
| Mean of dep. variable | 0.037 | 0.177 | 0.015 | 0.002 |
| F-statistic of IV | 24.80 | 24.80 | 24.80 | 24.80 |
| Control variables | Yes | Yes | Yes | Yes |
| No. of observations | 172,157 | 172,157 | 172,120 | 169,647 |

*Notes*: This table summarizes estimation results of the effect parental divorce on demographic outcomes for boys (upper panel) and girls (lower panel) separately. Two estimation methods are used. Within each panel the first row reports estimates from a naïve logit estimation, and the second row reports estimates from a 2SRI procedure. The latter uses the extent of gender balance in fathers' workplaces as an instrumental variable. Thus each reported estimation result is from a separate estimation. Reported estimates are average marginal effects for divorce until age of 18, with standard errors clustered on families in parentheses below. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level respectively. The number of observations varies due to availability of outcome variables and/or underidentification of the logistic models. Control variables comprise child characteristics measured at birth (parity, multiple birth, and birth weight), parental characteristics of assortative mating measured the time of marriage (father's age, the spouses' age difference, religious denominations, and citizenship), father's employment characteristics measured at the time of the child's birth (broad occupation, daily wage, job tenure, same firm with mother), father's firm characteristics measured at the time of the establishment of the firm (firm size, share of blue-collar workers, age-specific shares of women), 215 industry fixed-effects, regional fixed-effects, quarter of birth fixed-effects, a child birth cohort trend and parental marriage cohort trend.

[a] Teenage parenthood takes the value one if the child becomes mother/father until age 20, and zero otherwise; children with births before parental divorce are excluded.

[b] Early marriage takes the value one if the child marries until age 20, and zero otherwise; children marrying before parental divorce are excluded.

divorce on labor market outcomes measured at the age of 25 years. This is the latest year for which we can observe the outcomes for children from all birth cohorts. We distinguish between five mutually exclusive labor market states: employed, marginally employed, unemployed, parental leave and out of labor force. For treated boys, we find clear negative effects on their labor market success: they are less likely to be employed or marginally employed (minus 5.1 and minus 1.6%-points or minus 7 and minus 34%, respectively) and more likely to be unemployed or out of labor force (plus 2.6 and 2.4%-points, or plus 48 and plus 12%, respectively). Thus, for boys the findings across outcomes provide a consistent pattern: treated boys have worse human capital outcomes.

Effects for girls are similar, but smaller. Treated girls are less likely to be marginally employed (minus 1.4%-points or 22%) and more likely to be unemployed (plus 2.6%-points or 63%). Thus, we find (as compared to boys) smaller effects on labor market outcomes in total. A potential explanation for this different finding is the estimated treatment effect on early fertility. It is possible that the early fertility — which is particularly pronounced during teenage years — leads to a higher degree of sense of responsibility and/or a comparably earlier entry into the labor market. Both effects could explain why the negative employment effects for boys are smaller for girls. Notably, this supposition is supported by the literature on the employment effects of teenage motherhood. Design-based papers have found that the effects of teenage births on subsequent employment are either zero (Geronimus and Korenman, 1992) or even positive (Hotz et al., 2005).[13]

[13] In Appendix A.1, we check the sensitivity of our results to a number of variations, such as the treatment, the definition of the control group, and the method of inference.

**Table 4**
The effect of parental divorce on human capital outcomes.

| | (I) | (II) | (III) | (IV) | (V) | (VI) |
|---|---|---|---|---|---|---|
| | Education | Labor market status at 25 years of age | | | | |
| | College attendance | Employed | Marginally employed[a] | Unemployed | Parental leave | Out of labor force |
| *Boys* | | | | | | |
| 2SRI | −0.097*** | −0.051*** | −0.016** | 0.026*** | 0.001 | 0.024** |
| | (0.013) | (0.014) | (0.006) | (0.006) | (0.001) | (0.012) |
| Naïve logit | −0.063*** | −0.041*** | −0.002 | 0.026*** | 0.000 | 0.012*** |
| | (0.003) | (0.003) | (0.002) | (0.001) | (0.000) | (0.003) |
| Mean of dep. variable | 0.234 | 0.697 | 0.047 | 0.054 | 0.001 | 0.201 |
| F-statistic of IV | 18.75 | 18.75 | 18.75 | 18.06 | 18.75 | 18.75 |
| Control variables | Yes | Yes | Yes | Yes | Yes | Yes |
| No. of observations | 182,847 | 182,847 | 182,847 | 182,847 | 162,417 | 182,847 |
| *Girls* | | | | | | |
| 2SRI | −0.081*** | 0.008 | −0.014* | 0.026*** | −0.011 | −0.019 |
| | (0.014) | (0.015) | (0.007) | (0.005) | (0.009) | (0.012) |
| Naïve logit | −0.054*** | −0.046*** | 0.003** | 0.020*** | 0.009*** | 0.010*** |
| | (0.003) | (0.003) | (0.002) | (0.001) | (0.002) | (0.003) |
| Mean of dep. variable | 0.302 | 0.624 | 0.065 | 0.041 | 0.097 | 0.173 |
| F-statistic of IV | 24.80 | 24.80 | 24.80 | 24.80 | 24.80 | 24.80 |
| Control variables | Yes | Yes | Yes | Yes | Yes | Yes |
| No. of observations | 172,194 | 172,194 | 171,708 | 172,041 | 172,765 | 172,194 |

*Notes*: This table summarizes estimation results of the effect parental divorce on human capital outcomes for boys (upper panel) and girls (lower panel) separately. Two estimation methods are used. Within each panel the first row reports estimates from a naïve logit estimation, and the second row reports estimates from a 2SRI procedure. The latter uses the extent of gender balance in fathers' workplaces as an instrumental variable. Thus each reported estimation result is from a separate estimation. Reported estimates are average marginal effects for divorce until age of 18, with standard errors clustered on families in parentheses below. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level respectively. The number of observations varies due to availability of outcome variables and/or underidentification of the logistic models. Control variables comprise child characteristics measured at birth (parity, multiple birth, and birth weight), parental characteristics of assortative mating measured the time of marriage (father's age, the spouses' age difference, religious denominations, and citizenship), father's employment characteristics measured at the time of the child's birth (broad occupation, daily wage, job tenure, same firm with mother), father's firm characteristics measured at the time of the establishment of the firm (firm size, share of blue-collar workers, age-specific shares of women), 215 industry fixed-effects, regional fixed-effects, quarter of birth fixed-effects, a child birth cohort trend and parental marriage cohort trend.
[a] Marginal employment is employment with a low number of hours and low pay.

## 6. Conclusions

We examine the effect of parental divorce on children's long-term outcomes based on an IV approach that exploits idiosyncratic variation in the extent of gender balance in fathers' workplaces. We find that parental divorce has mostly negative effects on children. While there are no comparable causal estimates for children's labor market outcomes, other studies using sibling fixed-effect find no effects on children's human capital formation (Björklund and Sundström, 2006) or — in line with our results — significant negative effects (Sigle-Rushton et al., 2014; Laird et al., 2020; Chen et al., 2019). Recently, Holm et al. (2023) report a significant negative effect of divorce on children's test scores exploiting randomness in the timing between marital dissolution and reading tests. Our results can complement these design-based studies by looking at divorce effects triggered by the father meeting another partner at work. Divorce effects on children in this realistic scenario may well be different from divorces due to domestic abuse or drifting apart of the partners.

These findings align with theoretical expectations regarding the potential causal pathways. After divorce children typically grow up in female-headed households, since maternal sole custody is the dominant arrangement. These households have lower incomes, tend to live in worse neighborhoods, have fewer and weaker male role models, and access to smaller social networks. Moreover, treated children may suffer from being separated from the father, parental hostility and residential and school dislocation (Painter and Levine, 2000).

## Declaration of competing interest

The authors declares that we have no relevant or material financial interests that relate to the research described in this paper.

## Data availability

The authors do not have permission to share data.

## Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jpubeco.2024.105201.

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