WORKING PAPER 2023:25

Paternity leave and child outcomes

Daniel Avdic
Arizo Karimi
Anna Sjögren
Elin Sundberg

IFAU
INSTITUTE FOR EVALUATION OF LABOUR MARKET AND EDUCATION POLICY

The Institute for Evaluation of Labour Market and Education Policy (IFAU) is a research institute under the Swedish Ministry of Employment, situated in Uppsala.

IFAU's objective is to promote, support and carry out scientific evaluations. The assignment includes: the effects of labour market and educational policies, studies of the functioning of the labour market and the labour market effects of social insurance policies. IFAU shall also disseminate its results so that they become accessible to different interested parties in Sweden and abroad.

Papers published in the Working Paper Series should, according to the IFAU policy, have been discussed at seminars held at IFAU and at least one other academic forum, and have been read by one external and one internal referee. They need not, however, have undergone the standard scrutiny for publication in a scientific journal. The purpose of the Working Paper Series is to provide a factual basis for public policy and the public policy discussion.

More information about IFAU and the institute's publications can be found on the website www.ifau.se

ISSN 1651-1166

Paternity leave and child outcomes*

Daniel Avdic† Arizo Karimi ‡ Anna Sjögren§ Elin Sundberg¶

December 8, 2023

Abstract

We study how fathers' time impacts children's human capital using the introduction of earmarked paternity leave in Sweden. We use administrative data on parents' leave uptake and children's educational outcomes in a difference-in-discontinuities design, exploiting the plausibly random timing of childbirth. We show that the reform decreased average school-leaving grade point averages of sons of non-college fathers by 0.07 standard deviations and increased intergenerational persistence of human capital by 30 percent. We give suggestive evidence that these findings are explained by asymmetric impacts on parents' time investments owing to family disruptions and (lack of) substitutability of parents' time inputs.

Keywords: parental leave; socioeconomic gradient; social policy; intergenerational skill transmission; regression discontinuity

JEL Classifications: J12, J13, J16, J18

*We thank Aline Bütikofer, David Seim, Jonas Vlachos, and Björn Öckert for valuable suggestions. We also thank seminar participants at the Institute for Evaluation of Labor Market and Education Policy (IFAU); Monash University, QUT-Brisbane, Rotterdam, Melbourne University, Deakin University, Lund University, CEMFI, SOFI (Stockholm University) and participants at the FEW 2019 workshop, the 2022 EALE Conference, and the 2022 National Conference in Economics, Stockholm. This paper uses confidential data from Sweden maintained by Statistics Sweden. The data can be obtained for replication purposes by filing a request directly with Statistics Sweden. The authors are willing to assist with such requests. The authors have no relevant financial or non-financial interests to disclose.

†Department of Economics, Deakin university. Corresponding Author: Department of Economics, Deakin university, 70 Elgar Road, Burwood VIC 3125, Australia. Phone: +61 (0)3 924 46901. Email: d.avdic@deakin.edu.au.

‡Department of Economics, Uppsala University; Uppsala Center for Labor Studies (UCLS); and Institute for Evaluation of Labor Market and Education Policy (IFAU).

§Institute for Evaluation of Labor Market and Education Policy (IFAU); and Uppsala Center for Labor Studies (UCLS).

¶Department of Economics, Uppsala University.

Contents

1 Introduction

Parental time investments are central to the human capital development of children as key inputs through which skills are accumulated (see e.g., Becker and Tomes, 1986; Cunha and Heckman, 2007; Currie and Almond, 2011; Doepke and Zilibotti, 2017). The scope and quality of such investments have received considerable attention in the economics literature (see e.g., Guryan, Hurst and Kearney, 2008; Fiorini and Keane, 2014; Del Bono et al., 2016; Francesconi and Heckman, 2016). While most scientific evidence concern maternal time investments, the importance of fathers' time has gained recent interest due to increased paternal involvement in parenting, government interventions, and trends towards increased gender equality in the household (see e.g. Kalil et al., 2016; Gould, Simhon and Weinberg, 2020).[1]

In this paper we exploit the introduction of earmarked parental leave in Sweden that provided fathers with additional incentives to stay home with their child to study how an increase in fathers' parental time investments affects children's human capital outcomes. To this end, we use linked Swedish administrative data on parental leave uptake and educational attainment of parents, and compulsory school-leaving grades for their children born around the time of the 1995 reform in a difference-in-discontinuity (RD-DD) empirical design. We first show that eligible parents complied with the incentives to distribute their parental leave more equally between spouses by estimating reform effects on parental leave uptake. We then estimate separate reform effects on children's cognitive development by parental educational level and child sex; factors that have been shown to be important for the formation of skills (see e.g., Cunha et al., 2006; Bertrand and Pan, 2013; Autor et al., 2019).[2] Lastly, we explore potential mechanisms through which changing patterns of parental leave uptake may have altered children's outcomes, such as relationship stability, family size and role model effects.

[1] Based on the American time use study, Parker and Wang (2013) report an increase from 10 to 14 hours per week for mothers and from 2.5 to 7 hours per week for fathers between 1965 and 2011.

[2] Previous economics research on skill formation has suggested that the human capital of the caregiver matters for children's human capital outcomes (see e.g. Cunha et al., 2006). Moreover, there exists mounting empirical evidence that boys are particularly sensitive to childhood conditions (see e.g. Bertrand and Pan, 2013; Autor et al., 2019).

The parental leave reform studied in this paper affected all families with children born on or after January 1st 1995 by earmarking one month of paid parental leave entitlements to each parent, such that the transfer of these days between parents was no longer allowed. Since the reform left the total duration of parental leave entitlements unchanged, parents affected by the reform were faced with the choice to either have the father take one month of leave or to forfeit it altogether. We replicate and extend the results from previous papers that have analyzed the reform (see e.g. Eriksson, 2005; Duvander and Johansson, 2012; Ekberg, Eriksson and Friebel, 2013; Avdic and Karimi, 2018), showing that it was similarly effective in increasing fathers' leave uptake across families with different parental education levels. Importantly, this result allows us to credibly interpret any heterogeneity in the estimated effects across parental education groups as driven by variation in response to changed paternal leave uptake rather than as a result of differences in reform compliance.

Using our empirical RD-DD framework to estimate causal effects of the 1995 reform on children's cognitive outcomes, we find that overall changes in compulsory school-leaving standardized grade point averages (GPA) dropped by a marginally statistically significant 0.03 standard deviations for children born just after the reform was introduced. However, breaking this result down by parental education and child sex reveals that the pooled estimate is entirely driven by sons of non-college educated fathers whose average GPA declined by a strongly significant 0.07 standard deviations. In contrast, we do not find any important effects for girls or for children of college-educated fathers.

To provide a concrete estimate of the impact of the reform on the intergenerational persistence of human capital, we interact our RD-DD model with a standardized measure of fathers' cognitive ability based on test results obtained from Swedish military draft records. We find that the parental leave reform increased the correlation between fathers' cognitive skills and sons' compulsory school-leaving GPA by 0.07 standard deviations, corresponding to an increase in the intergenerational persistence in human capital of around 30 percent. Again, we find no corresponding impact for girls. In addition, estimating the same models for uncles (for both spouses) and for fathers' non-cognitive skills yield zero effects, suggesting that our main effects are not transferred by genetic factors or by fathers' soft skills, including parental motivation and encouragement. We interpret these results as that reform-induced changes in fathers' parental time investments are crucial in determining boys' human capital development, at least in families from lower socioeconomic backgrounds.

Guided by previous evidence on the effects of paternity leave reforms on parental and family outcomes, we explore several causal channels to corroborate our main findings.[3] First, previous research has shown that the parental leave reform increased the probability of relationship dissolution during the child's first years of life (Avdic and Karimi, 2018). Parental separations may be an important mechanism affecting children's human capital accumulation as they are likely to significantly alter both the quality and quantity of parents' time investments (see e.g. Bertrand and Pan, 2013; Gould, Simhon and Weinberg, 2020). We reestimate our RD-DD model using an indicator for parental separation as outcome and find that the parental leave increased the probability of separation by the time the child turned three increased by 2.3 percentage points, or 15 percent, in families where the father had less than college education. This result thus aligns with the findings on school-leaving GPA and suggest that parental time investments was indeed a likely contributor to the effects on children's compulsory school-leaving grades.

The interpretation that parental time investments is a leading explanation for our findings on children's cognitive outcomes is further strengthened by the null effects we estimate for children's disruptive behavioural disorders during years 14–16, measured as the probability of being prescribed ADHD, anti-anxiety or anti-depressant drugs or being hospitalized for a psychological condition. Specifically, our empirical findings from using data on drugs prescribed to children do not support the hypothesis that reform-induced

[3]Considerable academic work has been devoted to understanding the impacts of various family policies on parental labor supply (see e.g., Ruhm, 1998; Waldfogel, 1999; Schönberg and Ludsteck, 2007; Han, Ruhm and Waldfogel, 2009; Lalive et al., 2014; Rossin-Slater, Ruhm and Waldfogel, 2013; Schönberg and Ludsteck, 2014; Bergemann and Riphahn, 2015; Moberg, 2017; Ginja, Jans and Karimi, 2020), fertility (Lalive and Zweimüller, 2009; Baker and Milligan, 2008*a*; Gauthier, 2013; Farré and González, 2019), maternal health (Persson and Rossin-Slater, 2019), child outcomes (e.g. Baker and Milligan, 2015, 2010, 2008*b*; Carneiro, Løken and Salvanes, 2015; Liu and Skans, 2010; Ginja, Jans and Karimi, 2020; Dustmann and Schönberg, 2012; Danzer and Lavy, 2017; Dahl et al., 2016; Stearns, 2015; Rossin, 2011; Rasmussen, 2010), and marital stability (see e.g. Avdic and Karimi, 2018; González and Zoabi, 2021).

parental separations impacted children's cognitive skills primarily through significant increases in mental trauma and neurodevelopmental disorders among children of divorce. In contrast, our results are more in line with the hypothesis that these separations were instrumental in creating a situation where children had less access to their fathers due to shared-custody arrangements after parental separation.

We also study how family size and role model effects may factor in as explanations for our findings on boys' compulsory school-leaving grades, but find little empirical evidence supporting the existence of such channels. Specifically, estimating our models separately for fathers with a specialization in arts and humanities, social sciences, or STEM-related fields, we find some support for the role model hypothesis in that sons of college educated fathers with a specialization in STEM tend to have higher grades in corresponding subjects. However, no such empirical link is found for other subject groups. With respect to family size effects, it is possible that additional children in the household lead to less available parental resources per child and thereby impede children's cognitive development. However, using completed fertility as outcome in our regression model, we do not find any evidence that the parental leave reform had important effects on family size.

While the effects we document on boys' GPA are concentrated to the group with an increased divorce risk due to the reform, we are not able to conclusively pin down parental separations as the main mediating factor. An alternative interpretation of the effects that we document could be that mothers' and fathers' time in child human capital production are imperfectly substitutable. It is implausible that a substitution of parental leave between spouses of around one month would have major impacts on child development. However, Avdic and Karimi (2018) shows that the 1995 paternity leave reform in Sweden affected mainly the extensive margin: eligible fathers went from taking virtually no parental leave to taking an entire month. Thus, the reform implied a major shift in paternal involvement with potentially far-reaching consequences for the intra-household distribution of care responsibilities.

Recent evidence suggests that paternity leave reforms do alter fathers' involvement in child care and housework, even though labor supply is not affected in the long run. For example, González and Zoabi (2021) find that the introduction of a two-week paternity leave in Spain lead to a persistent increase in fathers' housework and childcare time of more than an hour per day each.[4] Similarly, Tamm (2019) exploits variation from a paternity leave reform in Germany and finds long-lasting effects on fathers' involvement in childcare and housework, even while effects on labor supply do not persist over time. Moreover, Farré et al. (2022) combines an introduction of paternity leave in Spain and a large-scale lab-in-the-field experiment conducted with children born around the policy change. They find that at age 12, children whose fathers were eligible for paternity leave exhibit more egalitarian attitudes towards gender roles in the home and in the labor market. These effects are plausibly generated by their own parents' altered allocation of time. If mothers and fathers have different parenting styles, such shifts in the allocation of time may have consequences for children's outcomes. While comparable survey data on household time use is unavailable in Sweden, Ekberg, Eriksson and Friebel (2013) use paid leave for caring for sick children as a proxy for the long run division of parental time. The authors find that the 1995 reform did not seem to have had a significant impact on this indicator. Taken together, our results together with knowledge from previous work thus suggest that data on parental activities/time use, both across mothers and fathers and across families with different structure, would be key to uncovering the black box through which parental leave reform in general, and paternity leave in particular, affect children's long run cognitive outcomes.

Our paper adds to the particular strand of this literature that focuses on the introduction of earmarked paternity leave (Cools, Fiva and Kirkebøen, 2015; Patnaik, 2016; Kotsadam and Finseraas, 2011; Rege and Solli, 2010; Dahl, Løken and Mogstad, 2014; Dahl et al., 2016; Farré and González, 2019; Druedahl, Ejrnæs and Jørgensen, 2019; Ekberg, Eriksson and Friebel, 2013; Duvander and Johansson, 2012; Johnsen, Ku and Salvanes, 2020) by further exploring effects on division of leave and couple separations (Avdic and Karimi, 2018), and by adding effects on child outcomes; a literature that is

[4] González and Zoabi (2021) also find that the marginal group affected by the reform experienced a 3 percentage point drop in the fraction having another child, and a 4 percentage point increase in the divorce rate; the latter result is consistent with results presented in Avdic and Karimi (2018) in Sweden.

still in its infancy due to paternity leave reform being fairly recent interventions and due to data limitations. The paper closest to ours is the study by Cools, Fiva and Kirkebøen (2015), who use a difference-in-difference approach to study the effect of a four weeks paternity leave policy in Norway on children's schooling outcomes. The authors find that children's school performance improves as a result of the reform, particularly in families where the father has higher education than the mother, although the latter set of estimates is statistically imprecise. We expand the evidence put forth in Cools, Fiva and Kirkebøen (2015) in several directions: First, our regression-discontinuity empirical approach and larger sample size allows us to explore sex-specific effects as well as heterogeneity by parental education yielding more precise estimates. Second, we document the implication of the reform for the overall intergenerational skill correlation between fathers and sons using auxiliary measures of fathers' human capital. Third, our study is informative on potential mechanisms for changes in children's schooling outcomes resulting from the reform.

Our paper also contributes to several related research streams. First, previous research has found that the introduction of shorter leave programs improves the health and schooling achievements of children (Rossin, 2011; Carneiro, Løken and Salvanes, 2015; Stearns, 2015), while expanding existing and already generous leave programs has zero to small impacts on children's outcomes (Baker and Milligan, 2008*a*; Rasmussen, 2010; Dustmann and Schönberg, 2012; Dahl et al., 2016; Rossin-Slater, 2018). In line with the results from our study, some papers also report differential effects by children's socioeconomic background and that children from families of higher socioeconomic backgrounds tend to benefit more from increased parental time (see e.g. Liu and Skans, 2010; Danzer and Lavy, 2017; Ginja, Jans and Karimi, 2020). This evidence suggests that the quality of care provided by parents, relative to the counterfactual mode of care, matters for whether longer parental leave duration is beneficial for children or not. In a similar vein, our results suggest that the quality of parental time is relatively lower in families with non-college fathers. Relatedly, our results also contribute to the broader literature on the role of parents' educational attainment for children's human capital accumulation

(Guryan, Hurst and Kearney, 2008; Holmlund, Lindahl and Plug, 2011) and to the understanding of boys' relatively higher sensitivity to childhood conditions Bertrand and Pan (2013); Autor et al. (2019), by pointing to the importance of parental education and cognitive skills.

Finally, our paper also contributes to the literature explicitly studying the relative importance of mothers' and fathers' time investments for the intergenerational transmission of skills (see e.g., Kalil et al., 2016; Gould, Simhon and Weinberg, 2020; Adda, Bjorklund and Holmlund, 2011). Previous analyses have used perturbing events such as parental deaths and divorces, affecting rather limited and select groups, to generate variation in exposure to parents. We instead rely on changes in parental investments due to a change in policy affecting all families with with children born after the reform. Our findings are consistent with the idea that parental time and presence, as opposed to financial resources, are important for the intergenerational transmission of skills.

2 Institutional Setting

2.1 The Swedish parental leave system

Parental leave policies are integral components of the social insurance system in many industrialized countries. The Scandinavian countries; Sweden, Norway and Denmark, were early adopters of publicly financed and job-protected parental leave. The Swedish parental leave system replaced the former maternity leave system in 1974. The new system granted mothers and fathers of newborn children an equal number of fully transferable paid leave days each, at the time in total 180 days per child.

During the time period we focus on in our empirical analysis, years 1991–1995, paid leave had been extended to a total of 450 days, and split into three components as follows: First, parents together received a total of 360 days of leave per child during which benefits replaced earned income at a rate of between 75 to 90 percent. These wage-replaced benefits were conditional on at least 240 days of employment preceding child birth and capped at a relatively generous income ceiling corresponding roughly to the mean salary of 30-40 year old college educated workers at the time. For individuals who did not meet the work requirement, the parental leave days were instead compensated with a low fixed daily amount of 60 SEK. [5] Second, parents were entitled to an additional 90 days of parental leave per child, replaced at a fixed daily amount equal to 60 SEK. Finally, each father received 10 days of wage-replaced leave to be used within the first 60 days of the child's birth. Thus, parents were jointly entitled a total of 450 + 10 days of paid leave per child. While the first two leave components, comprising 360 + 90 days, could not be used by both parents simultaneously, the third component could be used together with maternal leave.[6]

Parental leave in Sweden is fully job protected and may be used flexibly. Both parents are legally entitled to full-time job protected leave, whether collecting benefits or not, during the first 18 months after childbirth. Thereafter, parents have the option of reducing their working hours by up to 25 percent until the child turns eight years old. This means that parents are able to prolong their parental leave by claiming part-time benefits while staying at home full-time. Any remaining parental leave can also be used flexibly until the child turns eight years old to, for example, extend family holidays. While employers normally cannot reject parental leave requests of employees, applications must be made at least two months in advance.

Parental leave is paid out to the legal parents of the children, or to any other legal custodian. For married couples, the law presumes the husband to be the father of his wife's children, and the custody of the children is thus joint by default. For unmarried cohabiting couples, the mother is given sole custody of her child unless paternity is established after birth and parents apply for joint custody.[7] A parent with sole custody of a child is entitled to all 450 days of paid parental leave for a child. In the event of a divorce, parents who previously had joint custody of their children will typically retain joint custody of the children regardless of formal residence of the children. The majority

[5] This level corresponded to 80-90 percent of the mean hourly wage for less than high school educated workers.

[6] Parental leave has since been extended and now there are 390 wage replaced days and 90 days at a fixed benefit. From 2012 and onward, 30 wage-replaced leave days can be used by both parents simultaneously.

[7] In practice, the identity of the father is established for nearly all children in Sweden.

of children of divorced parents reside with the mother, but alternating residency has become more common over time (SOU 2011:51, 2011).

2.2 Introduction of earmarked paternity leave in Sweden

The first "daddy-month" reform in Sweden was introduced in 1995 and provided additional financial incentives for fathers to take up parental leave. Prior to its implementation, both parents were assigned equal shares of the total paid leave but with the option to freely transfer days between each other. In practice, the vast majority of parents used this option to transfer paid leave days from the father to the mother, leaving the latter with an average share of more than 90 percent of the total leave entitlement per child. To encourage more fathers to use parental leave, the 1995 parental leave reform earmarked one month (30 days) of the 360 days wage-replaced leave to each parent. Importantly, the change in policy implied that one month of paid leave would be forfeited should either parent fail or be otherwise unwilling to take any leave. Because subsidized childcare is available only from the child's first birthday, the reform left parents one month short of wage-replaced days unless the father wanted to take up leave. Crucial for our empirical approach outlined below, is that eligibility for the 1995 reform varied deterministically with the child's date of birth, such that parents of children born on or after January 1st 1995 were subject to the new rules.

The effect of the 1995 reform on parental leave uptake has previously been studied by Duvander and Johansson (2012) and Ekberg, Eriksson and Friebel (2013), both finding that it led to a strong increase in fathers' parental leave uptake. In contrast, findings are mixed as to whether the reform provided a more equal division of household work between mothers and fathers in the long-term, measured as the relative share of temporary parental leave taken to care for sick children. Furthermore, Avdic and Karimi (2018) study the impact of the reform on family stability, showing that it increased separations among couples in families with more traditional division of household roles and that low-income mothers compensated for the reduction in paid parental leave days by increasing their uptake of unpaid leave.[8]

In order to further promote fathers' uptake of parental leave, a second month of the entitlement of wage replaced leave was earmarked to each parent of children born on or after January 1st 2002. However, this reform also increased the generosity of paid leave by increasing the total number of eligible parental leave 450 to 480 days. Eriksson (2005) found that the 2002 reform further increased fathers' average parental leave uptake to approximately two months (60 days). However, the reform also increased total parental leave taken by mothers due to the increase in overall entitlement. In this paper, we focus exclusively on the 1995 reform since it is unclear whether a substitution of parental leave days from the mother to the father actually took place in the 2002 reform. Finally, a third earmarked parental leave month for each parent was introduced in 2016. While more similar to the 1995 reform in its design, the 2016 reform does not provide a long enough follow-up period to estimate meaningful effects on children's human capital outcomes using available data sources.

3 Research design

We apply a difference-in-discontinuities (RD-DD) design to study how the shift of parental leave take-up from mothers to fathers impacts child human capital outcomes, exploiting that assignment to new parental leave entitlements was quasi-randomly determined by a child's date of birth.[9] The specific discontinuity we use arises from the reserving to each parent 30 of the 360 parental leave days per child, making them non-transferable, for parents of children born on or after January 1st 1995. Since fathers rarely took any

[8] Cools, Fiva and Kirkebøen (2015) study a similar reform in Norway, finding that fathers increased their parental leave as a result of the reform. However, they also find a *negative* effect on mothers' earnings, suggesting that the gender balance in home and market work did not change as a result of the reform.

[9] Our empirical setting with a sharp policy treatment assignment discontinuity based on a continuous running variable is ideal for the application of regression discontinuity (RD). Nevertheless, we also estimate difference-in-differences (DD) models as an alternative empirical approach. Note that these two approaches estimate two different parameters that are subject to different identifying assumptions. While the latter approach estimates an average treatment effect across the data bandwidth, compared to a local effect at the cutoff for the former, it also relies on stronger assumptions about the comparability of treatment and control observations across the entire bandwidth to be valid. In contrast, the RD approach only requires observations within a neighborhood around the cutoff to be comparable for causal inference.

parental leave prior to the reform, the incentives provided by the new policy led to a sharp and discontinuous increase in fathers' uptake (see, e.g., Avdic and Karimi, 2018). Moreover, the 1995 reform left total parental leave entitlement unchanged, implying that the increase in fathers' uptake may be interpreted as a direct substitution of parental leave from mothers to fathers. We provide further empirical evidence for these conjectures in Section 6 below.

To set the stage for our empirical analysis, we first consider the basic regression-discontinuity design (RDD) which motivates estimation of the following cross-sectional regression model

$$y_i = \alpha + f_l(c - t, \gamma_l) \times \mathbb{1}_{t_i < c} + f_r(t - c, \gamma_r) \times \mathbb{1}_{t_i \geq c} + \beta \mathbb{1}_{t_i \geq c} + \epsilon_i, \tag{1}$$

where y_i is the outcome of interest for child i, t is the child's date of birth, c is the reform cutoff date and $t - c$ is the re-centered time index around the child's birth date. Furthermore, f_l and f_r are polynomial functions with corresponding parameter vectors γ_l and γ_r, capturing separate continuous outcome trends on each side of the cutoff through the indicator functions $\mathbb{1}_{t_i<c}$ and $\mathbb{1}_{t_i \geq c}$, respectively. Finally, the parameter β captures the discontinuous impact of child birth date at the cutoff, provided that the trend parameter vectors sufficiently adjust for any seasonality in the outcome variable.

The validity of Equation (1) to provide causal effects of the parental leave reform on our outcomes of interest hinges on the condition that we are able to distinguish the discontinuous impact at the reform cutoff point, β, from all other determinants of the outcome variable that evolve as smooth continuous functions of the assignment variable. Given that this assumption holds, and that families are unable to precisely manipulate the child's date of birth, we can interpret the parameter estimates from the parental leave take-up of spouses as causal reform effects. We provide analytical results from a battery of diagnostic tests to assess the validity of these identifying assumptions in Section 4 below.

While Equation (1) provides causal reform effects of changes in fathers' and mothers' parental leave uptake under a relatively weak set of assumptions, the situation is more complex when analyzing child outcomes. An additional requirement for valid causal inference in the standard RDD is that the timing of the event triggering the discontinuity, in our case child birth date, must be unique. If other events of relevance for our outcome of interest share the same empirical cutoff, we will be unable to distinguish the impact of the treatment we set out to study from the impacts of such confounding events. It is well documented that season of birth is strongly related to later life outcomes and partly attributed to parental selection (see, e.g., Buckles and Hungerman, 2013). Moreover, age of school entry laws in Sweden stipulate that children start school in mid-August of the year they turn seven. Thus, children born in January are almost one year older when they start school than children born in December. This complicates inference from our RDD due to the well-known documented positive effects of school starting age on educational performance (see, e.g., Black, Devereux and Salvanes, 2011; Fredriksson and Öckert, 2014).[10]

To deal with the issue of confounding bias, we augment Equation (1) with a difference-in-differences (DD) model in order to cancel out any recurring end-of-year impacts of timing of birth on child outcomes. To this end, we extend our RDD model by including end-of-year cutoffs from a set of calendar years in which no parental leave reform took place and subtract the resulting pooled RDD estimate for these non-reform years from the estimate we obtain for the reform year cutoff 1994/1995. This approach is valid under an additional "common intercept" assumption that the end-of-year discontinuous shifts in the child outcomes we study would have been comparable across reform and non-reform years in absence of the 1995 parental leave reform. We investigate the validity of this assumption by means of a set of informal tests in the next section.

To implement the RD-DD model, we define cohorts $m = \{1991/1992, ..., 1994/1995\}$ centered around the end-of-year cutoff and extend Equation (1) by specifying an additional treatment year indicator $T = \{0, 1\}$, equal to one for the reform year cutoff in

[10] Another related problem of a more technical nature is that some of the outcome variables we study, such as couple dissolution, are measured on the calendar-year level while our empirical strategy requires within-year detail. This means that time from treatment will vary mechanically between families whose children are born early and late in the year, respectively. Our RD-DD approach to deal with confounding bias also resolves this problem.

1994/1995 and zero for all remaining non-reform cutoff years. This variable is then interacted with each regressor from the standard RDD model to allow for separate effects in treatment and control years. Formally, we estimate the following regression model

$$\begin{aligned} y_i = \alpha + \sum_{s=0}^{1} \mathbb{1}_{T_i=s} \times \{\delta T_i + f_l(c-t, \gamma_{ls}) \times \mathbb{1}_{t_i<c} + f_r(t-c, \gamma_{rs}) \times \mathbb{1}_{t_i \geq c} + \beta_s \mathbb{1}_{t_i \geq c}\} \\ + \lambda_{m_i} + \epsilon_i. \end{aligned} \tag{2}$$

Equation (2) is essentially a fully interacted version of Equation (1), allowing for separate RDD estimates for reform and non-reform years, with cohort-specific intercepts represented by the $1 \times m$ column vector λ_{m_i}.[11] Our parameter of interest is β_1, which can be interpreted as the causal effect of the introduction of the 1995 parental leave reform net of any other common RDD estimates captured by the non-reform years. We follow the literature and estimate Equation (2) using local linear regression with triangular kernel weights in our preferred specification. However, we also report estimates from alternative specifications to evaluate the robustness of our estimated reform effects to the degree of curvature in the running variable.

To study heterogeneity in the impact of the parental leave reform on child outcomes across fathers with different human capital levels, we estimate Equation (2) by parental education attainment measured at the time of child birth. Specifically, we estimate separate effects for children of college educated and non-college educated fathers and mothers, respectively. Finally, to study potential effect heterogeneity by child sex, we also estimate the RD-DD model separately for boys and girls.

4 Data

Our empirical analysis is based on panel data from linked administrative registers covering the universe of Swedish children born 1991–1995 and their parents. Families (i.e., children and their biological parents) are identified from the multi-generational register, which contains de-identified links across generations as well as the birth order of each child. We

[11] That is, we estimate $\sum_m \theta_m \mathbb{1}\left[\lambda_{m_i=m}\right]$ where θ_m is the specific intercept for cohort m.

subsequently add annual information on educational attainment, annual labor income, year of birth, sex, and other demographic- and labor market variables for all parents using an individual-level longitudinal data set consisting of merged administrative education and tax registers for the entire working-age population (LISA).

To estimate effects on parental leave uptake from the 1995 reform, we use data on parental leave spells from the Swedish Social Insurance Agency. The detailed information allows us to distinguish between maternal and paternal leave uptake for each child in our sample, which we use to calculate the total number of leave days (at the wage-replaced and base levels combined) taken by each parent over the course of the focal child's first 8 years of life. Children's schooling outcomes are obtained from the Swedish National Agency for Education and include cumulative GPAs and subject grades from the end of compulsory school (grade 9). Overall and subject-specific GPAs, grouped into science, technology, engineering and mathematics (STEM) and humanities and social sciences (HUMSAM) subjects, are standardized within school-leaving cohorts.

In addition to these core analysis data sets, we also use supplementary data from other sources to corroborate our main findings. First, we use military enlistment data to explore whether the parental leave reform impacted the intergenerational correlation between father's skills and their children's school-leaving GPA. These data, containing validated measures of both cognitive and non-cognitive abilities, are available for more than 90 percent of Swedish males born 1955–1985 who were subject to military conscription and covering over 80 percent of the families in our sample.[12] We standardize these test scores by enlistment cohort.

Furthermore, we link our analysis sample to data on children's physical and mental health from the Swedish Medical Birth Register, the National Patient Register and the National Prescription Registry to adjust for potentially confounding factors and to study potential effect channels. The medical birth register includes child birth weight and height, gestation (in weeks), Apgar scores, and lists different medical diagnoses at

[12]The military draft's cognitive test was performed by all male Swedish citizens in the year they turned 18. The cognitive test consisted of tasks relating to word knowledge (synonyms), mathematical and logic induction, plate folding, and technical comprehension. The non-cognitive skills measure is based on questions that capture e.g. stress tolerance and emotional stability.

birth according to WHOs ICD-10 classification of diseases and related health problems. The register also reports the predicted date of birth based on ultrasound (sonogram) and date of last menstruation, respectively, which we use to study potential birth manipulation. The patient registry contains the universe of inpatient and outpatient specialist care episodes, including date of admission and discharge in the case of inpatient care, date of outpatient visit, and primary and secondary diagnoses according to the ICD-10 classification. Finally, the prescription registry includes dated information on all medical drugs prescribed by physicians in both specialized and primary care. We use information on diagnoses from the the inpatient register to capture any effects on severe cases of mental health problems and the prescription registry to study the impact of the parental leave reform on child depression, anxiety, stress and conditions related to behavioral problems using crosswalks from the Anatomical Therapeutic Chemical (ATC) drug classification system.

To estimate our RD-DD model defined in Equation (2), we group the children in our sample into separate RDD groups based on the timing of their birth within a data range of six months before and six months after each end-of-year cutoff to avoid cross-cohort overlap. Thus, each cohort between 1991/1992 and 1994/1995 consists of children born between July 1st of the initial year and June 30th of the subsequent year. The treatment group indicator is assigned to children in the 1994/1995 cohort while remaining cohorts are assigned to the control group. We exclude all families in which either the mother or the father earned zero income in the year before the birth of the focal child as the parental leave reform only applied to wage-replaced parental leave benefit days.

Table 1 presents summary statistics for the main variables of interest in our data set, grouped by father's education level and child sex, for the treatment and pooled control group, respectively. The group-specific averages reported in the table suggests a strong gradient in schooling outcomes by father's education level, and substantial gender gaps favoring girls in GPA among children of both non-college and college educated fathers. The mean characteristics across father's educational attainment also display assortative matching of couples based on education level, and an educational gradient in couples' divorce probability. In contrast, differences in outcomes at birth are negligible across parental education categories, and mother's intra-household share of labor income is similar across parental education groups.

Table 1.
Summary statistics

	Control cohorts				Treatment cohort			
	Father Non-college		Father College		Father Non-college		Father College	
	Girls	Boys	Girls	Boys	Girls	Boys	Girls	Boys
Parental characteristics								
Spousal age gap	-2.600	-2.626	-2.613	-2.596	-2.511	-2.563	-2.513	-2.543
Mother's income share pre-birth	0.374	0.374	0.359	0.358	0.387	0.386	0.366	0.371
HH income pre-birth (1000s SEK)	322.736	322.579	423.865	423.502	318.578	319.714	427.694	435.560
Mother college	0.178	0.178	0.593	0.591	0.199	0.199	0.612	0.607
Father cognitive ability	-0.245	-0.243	0.772	0.773	-0.244	-0.233	0.786	0.793
Father non-cognitive ability	-0.054	-0.047	0.583	0.582	-0.025	-0.023	0.593	0.595
Separated 3 yrs post birth	0.140	0.141	0.063	0.063	0.125	0.125	0.063	0.063
Separated 5 yrs post birth	0.191	0.192	0.095	0.093	0.178	0.179	0.095	0.092
Child characteristics								
First-born	0.417	0.418	0.425	0.420	0.416	0.417	0.428	0.431
Pre-term birth	0.045	0.049	0.040	0.046	0.043	0.050	0.039	0.046
Low birth weight	0.032	0.028	0.026	0.026	0.030	0.026	0.028	0.026
Low APGAR	0.144	0.171	0.136	0.167	0.145	0.180	0.146	0.175
GPA	0.119	-0.223	0.712	0.383	0.156	-0.187	0.725	0.387
GPA Maths	-0.007	-0.107	0.550	0.471	0.014	-0.077	0.565	0.490
GPA SWE	0.193	-0.343	0.712	0.198	0.214	-0.322	0.711	0.169
GPA SOCSCI	0.099	-0.186	0.624	0.373	0.130	-0.153	0.634	0.369
N	85,900	89,659	33,247	34,585	22,476	23,357	9,684	10,364

Note.— Means of pre-determined characteristics and outcomes, respectively, by control- and treatment cohort, father's education level, and child gender. The sample includes children born in a six-month window around each included turn-of-year cutoff.

5 Threats to identification

Causal identification in the RDD relies on the assumption of local randomization, which asserts that individuals are unable to perfectly predict their treatment status. In our context, this condition implies that parents cannot systematically manipulate the exact birth date of their children. Such manipulation may, hypothetically, be practiced by parents for several reasons. For example, parents may have personal preferences for a specific parental leave policy regime, or they may know and act on the fact that children born early in the year tend to have better outcomes than children born late in the year. As explained in our previous section, our RD-DD setup accounts for the latter form of bias under the assumption that such manipulation is constant across treatment and control years.[13]

In contrast, birth date manipulation that is directly related to the parental leave policy poses a potentially more severe problem for our analysis. Theoretically, parents may be able to manipulate the birth date of their child around the reform cutoff by advancing or prolonging the pregnancy through medication (e.g., tocolytic drugs) or through surgical or instrumental delivery (e.g., cesarean section or vacuum extraction). However, such decisions must involve assistance from the treating physician. In practice, it is unlikely that such medically-induced birth manipulation is frequent in Sweden, first and foremost because it is illegal. Moreover, the features of the healthcare system, with limited competition, fixed provider market shares and a salaried medical workforce, do not include any direct incentives for physicians to accommodate or condone such requests. On the contrary, clinicians may be subjected to malpractice litigation if these activities are discovered by health authorities. Forgery in the reporting or registration of birth dates is another possible manipulation channel, which is also unlikely to occur for the same reasons.

We nevertheless carry out a series of tests to thoroughly study whether manipulation

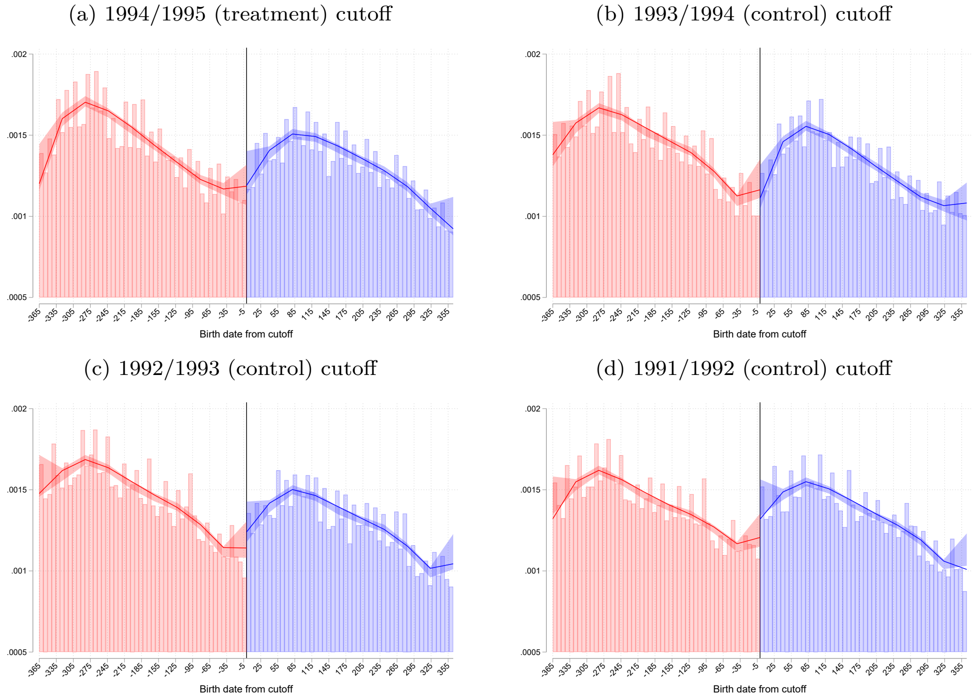
[13]We estimate separate RDD models for each cohort to directly assess this "common intercept" assumption. The results from this exercise are reported in Table A.1 of Appendix A and generally lend support for our empirical approach in that point estimates for our main outcome variables are similar across control cohorts but sometimes markedly different for the treatment cohort.

of birth date is an issue in our setting. First, an indicative assessment of manipulation can be performed using the McCrary test of discontinuity in birth date density around the cutoff. A non-smooth density estimate in the running variable with significant mass points around the end-of-year cutoff is an indication that individuals may be actively manipulating the birth dates of their children in order to be assigned to their preferred treatment state. There may also exist other causes for discontinuities in the density of births around the end-of-year cutoff, such as holiday staff shortages prompting postponement of some low-risk births until after the new year.

Figure 1 presents the results from application of the McCrary test by cutoff year using weekly observation detail. As indicated by the shaded areas around the fitted lines, there are some indications of end-of-year discontinuities in the birth density in the two lower panels, i.e., in the 1991/1992, and 1992/1993 cohorts, respectively. Specifically, more children are born early compared to late in the year in the two former cohorts, while there is no discontinuity in the birth density at the treatment cutoff (1994/1995), or the cutoff preceding the reform (1993/1994).[14]

[14] Table A.2 reports formal RD-DD estimates of the relative density discontinuity in the number of births between the treatment and pooled control cohorts for different polynomial specifications. The estimates confirms the graphical evidence that the end-of-year estimated discontinuity in births is significantly lower in the treatment year compared to other years.

FIGURE 1.
McCrary density tests of manipulation of child birth date



NOTE.— Estimates are based on daily detail on the running variable. The graphs have been produced using the Stata package `rddensity`; see Cattaneo, Jansson and Ma (2016) for details.

As previously mentioned, the McCrary density test does not necessarily imply that active manipulation of birth dates by parents is present in our setting since other potential factors, such as temporary supply constraints in the healthcare system, could produce similar results. A more direct test of birth manipulation is to estimate cohort-specific RDD effects on a set of potentially confounding factors using our sample and econometric model. In particular, if fathers' cognitive and non-cognitive skills vary discontinuously around the end-of-year cutoff, and this variation is different for treatment and control cohorts, this would provide more conclusive evidence on the existence of active manipulation in our data.

Table 2 and Table 3 report cohort-specific RDD (columns 1–4) and combined RD-DD (column 5) estimates for a set of potentially confounding variables for the sample with fathers without and with college education, respectively. Overall, the results show no signs of systematic associations between pre-determined parental or child characteristics around the end-of-year cutoff in the RD-DD specification for either sample subgroups, although estimates are occasionally statistically significant (in line with the probabilistic nature of the test statistic). Importantly, we find no indication that fathers' cognitive skills are systematically different across treatment and control years. Even so, we include all characteristics (except for fathers' cognitive and non-cognitive skill measures) as controls in our regressions to account for any residual bias and to improve precision.[15]

[15]In addition, Table A.3 report RD-DD estimates for the same pre-determined characteristics, but for sub-samples defined by parental education mix, which is the sample delineation used throughout our study.

Table 2.
Common intercepts assumption: pre-determined covariates for families of **non-college** fathers

	RD 91/92	RD 92/93	RD 93/94	RD 94/95	RD-DD
A. Mother's characteristics					
Age	0.935	0.938	1.169	0.903	-0.099
t-stat	10.781	10.093	12.016	9.094	-0.875
N	65,583	58,455	51,521	45,833	221,392
Income pre-birth	1.973	7.199	6.549	7.522	2.543
t-stat	1.567	5.266	4.336	4.642	1.411
N	65,545	58,414	51,226	45,522	220,707
College	0.003	0.020	0.022	0.004	-0.010
t-stat	0.461	2.855	2.855	0.517	-1.056
N	65,583	58,455	51,521	45,833	221,392
B. Father's characteristics					
Age	0.886	0.961	1.087	0.782	-0.185
t-stat	8.587	8.729	9.303	6.498	-1.363
N	65,583	58,455	51,521	45,833	221,392
Income pre-birth	6.003	-0.068	5.104	8.638	4.887
t-stat	3.703	-0.039	2.553	4.134	2.101
N	65,553	58,429	51,253	45,550	220,785
Cognitive skills	0.013	0.020	0.047	0.003	-0.022
t-stat	0.744	1.110	2.413	0.143	-0.987
N	52,660	47,913	43,025	38,560	182,158
Non-cognitive skills	0.053	0.077	0.064	0.033	-0.032
t-stat	2.857	3.932	3.128	1.508	-1.303
N	51,630	46,886	42,127	37,634	178,277
C. Couple characteristics					
Age gap	0.049	-0.024	0.082	0.121	0.087
t-stat	0.665	-0.293	0.953	1.326	0.850
N	65,583	58,455	51,521	45,833	221,392
Mother's share of HH income	0.004	0.025	0.013	0.007	-0.007
t-stat	1.130	6.677	3.149	1.424	-1.367
N	65,519	58,397	51,164	45,428	220,508
HH income (1000s SEK)	7.707	6.977	11.543	15.653	7.108
t-stat	3.510	2.919	4.307	5.478	2.238
N	65,519	58,397	51,164	45,428	220,508
Predicted separation	-0.004	-0.004	-0.007	-0.006	-0.002
t-stat	-3.713	-4.093	-5.649	-4.765	-1.042
N	65,114	57,944	50,764	45,080	218,902
D. Child characteristics					
Pre-term birth	-0.002	-0.007	-0.012	0.001	0.008
t-stat	-0.535	-1.775	-2.568	0.260	1.493
N	65,514	58,396	51,469	45,793	221,172
Low birth-weight	-0.001	-0.009	-0.009	-0.002	0.004
t-stat	-0.322	-2.703	-2.433	-0.448	0.984
N	65,522	58,423	51,415	45,690	221,050
Low APGAR	-0.007	-0.001	-0.005	-0.003	0.002
t-stat	-1.132	-0.132	-0.703	-0.325	0.232
N	65,279	58,078	51,268	45,636	220,261
Birth order, mother	-0.004	0.030	0.020	0.015	0.001
t-stat	-0.261	1.640	1.044	0.767	0.059
N	65,583	58,455	51,521	45,833	221,392
Birth order, father	-0.007	0.031	0.005	-0.012	-0.021
t-stat	-0.388	1.622	0.228	-0.581	-0.890
N	65,583	58,455	51,521	45,833	221,392
Predicted GPA	0.008	0.013	0.017	0.017	0.005
t-stat	1.645	2.500	2.938	2.848	0.701
N	65,115	57,944	50,765	45,080	218,904

Note.— The table reports the discontinuity in the outcome variable for children – and their parents – born around the turn-of-years before the reform (columns 1–3); the treatment turn-of-year (column 4), and the RD–DD estimate that nets out placebo-cohort differences in outcomes. We use a linear function for the running variable and apply triangular regression weights. The sample includes children born in a six-month window around each included turn-of-year cutoff. Estimates are based on daily detail on the running variable.

Table 3.
Common intercepts assumption: pre-determined covariates for families of **college** fathers

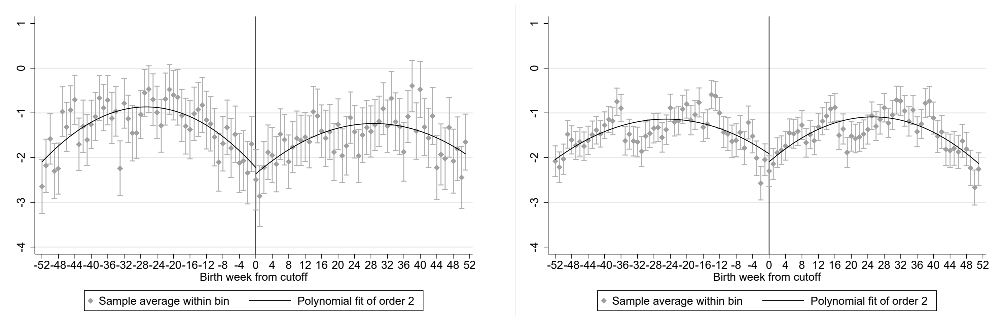
	RD 91/92	RD 92/93	RD 93/94	RD 94/95	RD-DD
A. Mother's characteristics					
Age	0.656	0.636	0.942	0.625	-0.113
t-stat	4.807	4.577	6.720	4.353	-0.688
N	23,833	22,300	21,699	20,048	87,880
Income pre-birth	8.370	12.068	15.510	11.863	0.078
t-stat	3.147	4.138	5.194	3.666	0.022
N	23,822	22,285	21,577	19,889	87,573
College	0.012	0.007	0.021	-0.010	-0.023
t-stat	0.807	0.466	1.338	-0.631	-1.274
N	23,833	22,300	21,699	20,048	87,880
B. Father's characteristics					
Age	0.683	0.754	0.755	0.684	-0.045
t-stat	4.209	4.462	4.345	3.840	-0.221
N	23,833	22,300	21,699	20,048	87,880
Income pre-birth	8.029	7.824	18.029	28.903	17.849
t-stat	1.632	1.853	3.526	3.114	1.843
N	23,823	22,279	21,551	19,860	87,513
Cognitive skills	0.009	-0.029	0.005	0.012	0.016
t-stat	0.351	-1.032	0.182	0.420	0.508
N	18,454	17,720	17,426	16,276	69,876
Non-cognitive skills	0.017	0.018	0.059	0.015	-0.016
t-stat	0.562	0.571	1.864	0.459	-0.429
N	18,372	17,634	17,357	16,180	69,543
C. Couple characteristics					
Age gap	-0.028	-0.118	0.188	-0.059	-0.068
t-stat	-0.225	-0.933	1.405	-0.440	-0.446
N	23,833	22,300	21,699	20,048	87,880
Mother's share of HH income	0.013	0.016	0.016	0.008	-0.007
t-stat	2.397	2.667	2.521	1.229	-0.936
N	23,818	22,273	21,525	19,810	87,426
HH income (1000s SEK)	16.667	19.618	33.891	40.484	17.536
t-stat	2.795	3.497	5.377	3.947	1.621
N	23,818	22,273	21,525	19,810	87,426
Predicted separation	-0.003	-0.002	-0.006	-0.004	0.000
t-stat	-2.846	-1.690	-5.019	-2.827	-0.025
N	23,669	22,138	21,360	19,678	86,845
D. Child characteristics					
Pre-term birth	0.006	-0.016	-0.013	0.007	0.014
t-stat	0.921	-2.342	-1.863	1.052	1.825
N	23,815	22,291	21,687	20,041	87,834
Low birth-weight	0.005	-0.013	-0.002	-0.001	0.002
t-stat	1.116	-2.329	-0.337	-0.207	0.275
N	23,803	22,282	21,662	19,980	87,727
Low APGAR	-0.008	-0.001	-0.007	0.007	0.012
t-stat	-0.755	-0.051	-0.652	0.598	0.919
N	23,723	22,188	21,580	19,975	87,466
Birth order, mother	-0.011	-0.024	0.026	-0.037	-0.033
t-stat	-0.419	-0.860	0.940	-1.293	-1.014
N	23,833	22,300	21,699	20,048	87,880
Birth order, father	-0.002	-0.045	0.009	-0.023	-0.010
t-stat	-0.071	-1.517	0.311	-0.747	-0.286
N	23,833	22,300	21,699	20,048	87,880
Predicted GPA	0.008	0.020	0.033	0.025	0.005
t-stat	0.895	2.110	3.375	2.163	0.384
N	23,669	22,139	21,360	19,679	86,847

Note.— The table reports the discontinuity in the outcome variable for children – and their parents – born around the turn-of-years before the reform (columns 1–3); the treatment turn-of-year (column 4), and the RD–DD estimate that nets out placebo-cohort differences in outcomes. We use a linear function for the running variable and apply triangular regression weights. The sample includes children born in a six-month window around each included turn-of-year cutoff. Estimates are based on daily detail on the running variable.

Finally, the main viable strategy to manipulate the birth date of a child in a small interval around the end-of-year cutoff is to delay or advance a birth via medical intervention. All expecting mothers in Sweden undergo regular medical checkups during their pregnancy to identify any potential problems relating to the upcoming childbirth. During one of these visits, the midwife performs an ultrasound on the fetus to obtain a predicted birth (due) date. Using the predicted due date from the ultrasound analysis available in our data, we construct a test of birth manipulation by estimating the discontinuity in prediction error around the end-of-year cutoff using our preferred RDD specification. In absence of manipulation, the prediction error between the actual and predicted birth dates should not be systematically related to the running variable around the reform cutoff. The results, presented in Figure 2, show that the predicted due date error is generally negative, meaning that births, on average, occur prior to the due date. More importantly, the estimated discontinuity in prediction error at the cutoff is indistinguishable from zero at both the treatment and combined control cutoffs. This evidence hence supports our assumption that systematic manipulation of children's birth dates is unlikely to be an important concern in our context.

FIGURE 2.
Ultrasound predicted birth date error by birth week from reform cutoff

(a) Treated cohort (b) Control cohorts



NOTE.— Estimates are based on daily detail on the running variable. We use a quartic function for the running variable and apply a triangular kernel. The dots illustrate averages of the outcome variable in weekly bins from the cutoff. The sample includes all parents to children born in 1991–1995.

6 Main results

We next analyse the effects of the parental leave reform of 1995, first considering its effects on parental leave uptake of mothers and fathers and on likely consequences for time investments in children before we turn to the effects on children's school results. Lastly, we study how the parental leave reform impacted the intergenerational gradient in human capital by estimating the correlation between fathers' cognitive skills and children's schooling outcomes.

6.1 Parental leave uptake

This section reports the results from quantifying the effect of the 1995 parental leave reform on various measures of mothers' and fathers' parental leave uptake. As explained in Section 3, there are no obvious reasons to expect confounding bias to distort estimated reform effects on these outcomes. For this analysis, we therefore rely on the simple RDD model defined in Equation (1) rather than the RD-DD model in Equation (2) and, hence, restrict our analysis sample to the 1994/1995 cohort.[16]

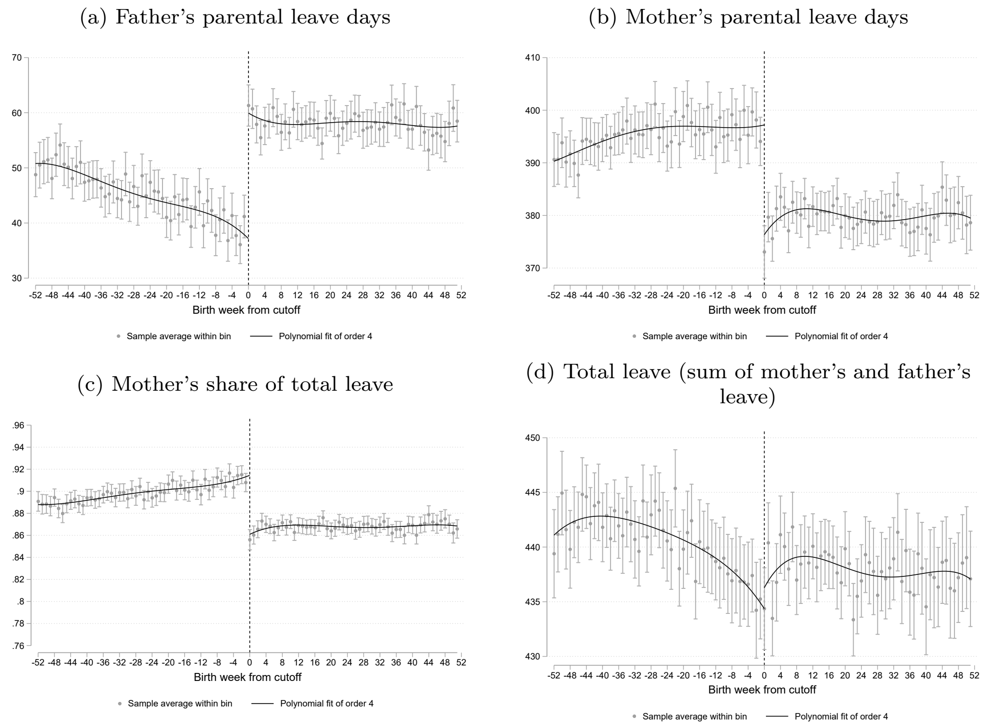
Figure 3 reports binned averages of couples' uptake of parental leave during the child's first 8 years by birth week along with a separate trend fitted on each side of the 1994/1995 end-of-year cutoff. Panels (a) through (d) report, in order, average leave uptake in days for fathers, average leave uptake in days for mothers, mother's share of total family leave taken, and the sum of mothers' and fathers' leave uptake, respectively.[17] Panel (a) shows that fathers' uptake increased by, on average, 21 days for children born just after compared to just before the parental leave reform was introduced, corresponding to a 46 percent increase relative to the pre-reform average uptake among fathers. Similarly, panel (b) shows that the reform decreased mothers' uptake by exactly the same amount, 21 days, on average, corresponding to a reduction in maternal uptake of around five percent relative to the pre-reform average. To analyze how these changes translate into substitution of parental leave within families, panel (c) provides corresponding results for the mother's average within-household share of total uptake per child. This share is estimated to have decreased by 5.4 percentage points, corresponding to 5.9 percent. Finally, panel (d) shows, as expected, that the total days of parental leave taken remained

[16] Avdic and Karimi (2018) (Figures 2 and 8) show that there are discontinuities in parental leave uptake only at December-January cutoffs where there was a parental leave reform, i.e., in 1995 and 2002.

[17] Corresponding local linear estimates of the discontinuity at the cutoff, captured by $\hat{\beta}$ in Equation (1), are reported in Table A.4.

unchanged for children born around the turn of the year. Thus, these results suggest that the parental leave reform implied a direct transfer of leave days from mothers to fathers, and no changes in total leave take-up per child.

FIGURE 3.
Parental leave uptake by birth week from reform cutoff



(a) Father's parental leave days (b) Mother's parental leave days (c) Mother's share of total leave (d) Total leave (sum of mother's and father's leave)

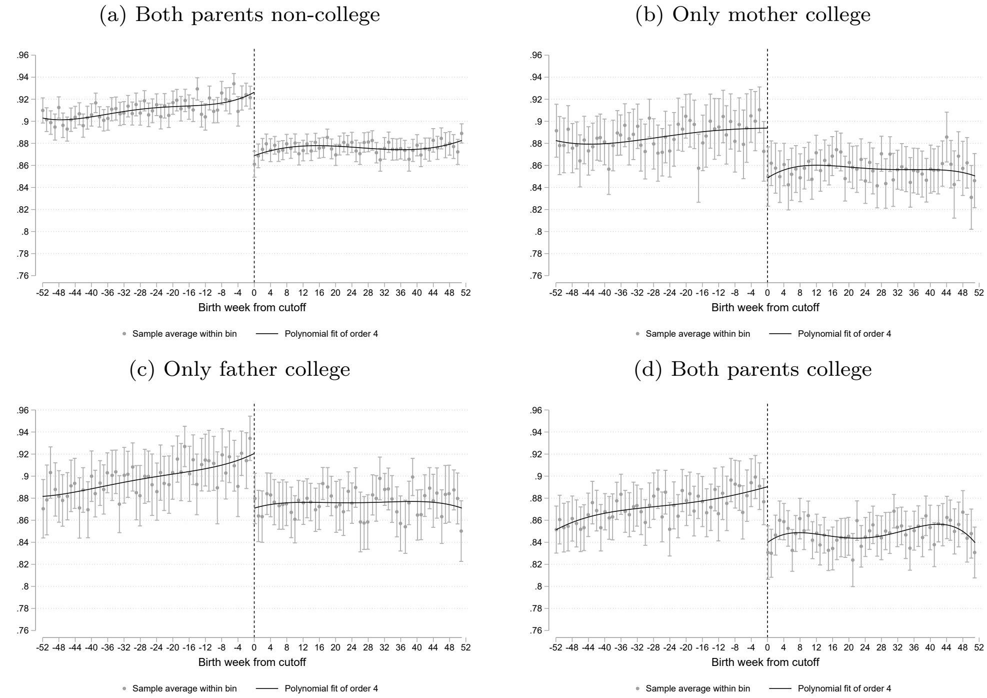
NOTE.— Estimates are based on daily detail on the running variable. We use a quartic function for the running variable and apply a triangular kernel. The dots illustrate averages of the outcome variable in weekly bins from the cutoff. The sample includes all parents to children born in 1994–1995.

Figure 4 presents results from estimating separate reform effects on mothers' within-household share of total leave uptake by parents' level of education, measured as having completed a college degree. Two important results emerge from this analysis: First, there is a statistically and economically significant effect for all four parental groups. Second, differences between the groups are relatively small and cannot be distinguished for conventional levels of statistical significance. The estimated drop in mother's share of parental leave uptake due to the reform ranges between 4.8 to 5.9 percent relative to the pre-reform mean across the four groups.[18] We conclude from this analysis that the impact of the 1995 parental leave reform on the redistribution of parental leave from mothers to

[18] See Table A.4 for a detailed analysis. The estimate across all groups is 5.7 percent. Furthermore, the smallest decline in mother's intra-household share of leave is found for couples where only the father has a college education. For this group, there is also a small net average increase in total leave uptake of around seven days largely due to the small decline in mothers' take up.

fathers is considerable and corresponds to the incentives provided by the policy design, and that heterogeneity in program uptake across parents with different education levels is not significant economically nor statistically.[19]

FIGURE 4.
Mother's share of parental leave uptake by birth week from reform cutoff by parental education level



NOTE.— Estimates are based on daily detail on the running variable. We use a quartic function for the running variable and apply a triangular kernel. The dots illustrate averages of the outcome variable in weekly bins from the cutoff. The sample includes all parents to children born in 1994–1995.

6.2 Reform effects on children's school-leaving GPA

We now turn to analyzing the 1995 parental leave reform's impact on children's human capital accumulation. In doing so, we employ the RD-DD model defined in Equation (2) and compare end-of-year discontinuities between treatment and control cohorts for Swedish children born between 1991 and 1995. Table 4 reports estimation results of the reform effect, β_1. Column (1) presents results for all parents, columns (2)–(3) the results by fathers' education, and columns (4)–(7) present separate results by father's and mother's education mix, respectively. Rows from top to bottom display results for both boys and girls, only boys, and only girls, respectively. The outcome variable

[19]We have also performed these analyses separately for boys and girls revealing only minor differences. See Table A.5–Table A.6 for details.

in all regressions is the child's overall compulsory school-leaving GPA standardized by cohort.[20,21]

The point estimates for the full sample in Column (1) of Table 4, interpreted as the difference in standardized school-leaving GPA, net of end-of-year discontinuities from the pooled control years, for children who were born just after compared to just before the turn of the year, suggest a statistically significant drop in standardized GPA by 0.03 standard deviations. This is roughly 10 percent of the overall gender gap in GPA in our sample, and five percent of the GPA gap between sons of college and non-college fathers. Splitting the sample by child sex, we can see that the effect for boys is stronger, both in terms of statistical and economic significance. On average, boys experience a decline in GPA by 0.05 standard deviations. By contrast, the corresponding estimate for girls of around 0.01 standard deviations is both smaller and non-significant.

To corroborate these findings, columns (2) and (3) present separate reform effect estimates for college and non-college educated fathers, respectively. Focusing on the sample of boys in the second row of the table, we see that the negative effect for boys is entirely driven by sons of non-college fathers, who experienced a large and statistically significant GPA drop of 0.07 standard deviations on average. In contrast, the effect for sons of college educated fathers is near-zero and far from significant. Estimates for girls are not statistically significant for neither parental education group.

In Columns (4)–(7) the reform effects are further broken down by mother's education level. Again, most point estimates are statistically insignificant except for the sample of boys with two non-college educated parents. For this group school-leaving GPA is estimated to have decreased by 0.06 standard deviations. It thus seems that the reform adversely and disproportionately impacted boys from more disadvantaged backgrounds with respect to their parents' educational attainment. The minor differences in reform effects on parental leave uptake by child sex reported in Table A.5 and Table A.6, suggest

[20] Table A.7 reports corresponding results without controlling for the predetermined characteristics listed in Table A.3.

[21] Figure A.1 and Figure A.2 display estimates for different sample bandwidths ranging between one week and one year around the end-of-year cutoff. The vertical dashed line in each panel indicates the baseline bandwidth used throughout our analysis.

that differential reform effects on boys' and girls' GPA stem from differences in their sensitivity to the parental leave changes rather than from differences in uptake changes.[22,23]

TABLE 4.
RD-DD estimates of the 1995 parental leave reform by parents' education level: School leaving GPA

		All mothers		Mother non-college		Mother college	
	All	Father Non-college	Father College	Father Non-college	Father college	Father Non-college	Father College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
GPA, all	-0.030*	-0.027	-0.015	-0.037	-0.006	0.029	-0.007
	(0.017)	(0.021)	(0.030)	(0.023)	(0.047)	(0.045)	(0.037)
N	305,751	218,904	86,847	179,030	35,054	39,874	51,793
GPA, boys	-0.051**	-0.065**	-0.003	-0.061**	0.038	-0.024	-0.021
	(0.024)	(0.027)	(0.041)	(0.030)	(0.064)	(0.061)	(0.052)
N	156,153	111,744	44,409	91,373	17,984	20,371	26,425
GPA, girls	-0.013	0.004	-0.028	-0.010	-0.048	0.022	0.000
	(0.025)	(0.030)	(0.041)	(0.033)	(0.066)	(0.062)	(0.050)
N	149,598	107,160	42,438	87,657	17070	19,503	25,368

NOTE.— Estimated discontinuities from RD-DD model on children's standardized compulsory school leaving grades by father's and mother's educational background (secondary, tertiary). We use a linear function for the running variable and apply triangular regression weights. The sample includes children born in a six-month window around each included turn-of-year cutoff. Estimates are based on daily detail on the running variable. Control variables included. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The reform effects on school-leaving GPAs we have studied so far are group-specific averages that do not convey much information about the dynamics across the distribution of grades within cohorts. To study such heterogeneity and to better understand which children were impacted by the parental leave reform, we estimate separate RD-DD models for different percentiles (10^{th}, 25^{th}, 50^{th}, 75^{th}, and 90^{th}) of the overall grade distribution (i.e., unconditional on parental education group). In particular, this analysis allows us to assess whether educationally weaker or stronger students were differentially affected by the reform.

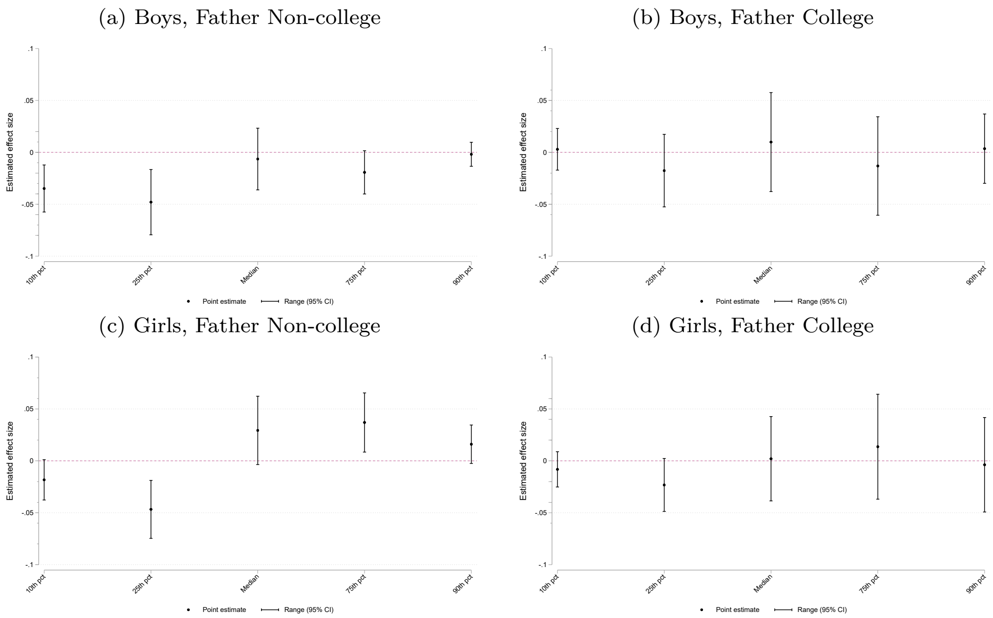
Figure 5 shows percentile-specific RD-DD point estimates of the reform effect on compulsory school GPA by child sex and father's education level. Specifically, panel (a)

[22] Table A.8 presents corresponding difference-in-differences estimates of the reform on children's GPA using the same variable definitions and sample as in our RD-DD model. In addition, Table A.9 show results from a difference-in-differences model where we use *predicted* date of birth from a diagnostic ultrasound conducted by a midwife during routine medical checkups to assign treatment status. Both models yield results that are qualitatively similar to those from our main specification.

[23] We have also estimated separate models for GPAs in STEM and Social Science and Humanities (HUMSAM) subject groups, respectively. These results are reported in Table A.10 and suggest that grades in both subject groups were negatively affected for sons of non-college educated fathers, although the effect on STEM subjects is somewhat greater in magnitude.

reports point estimates for boys with non-college educated fathers, corresponding to column (2) in Table 4. The negative average effect for this group is concentrated in the bottom quartile of the grade distribution. In other words, the adverse effect of the 1995 parental leave reform on average school-leaving GPAs is driven by the lowest performing students in the most disadvantaged socioeconomic group. The remaining panels in Figure 5 provide more mixed findings: While the null average effects on children of college educated fathers, reported in panels (b) and (d) respectively, are evenly distributed across the grade distribution, estimates for daughters of non-college fathers in panel (c) display a non-linearity. In particular, daughters in the bottom quartile have an effect magnitude similar to that of sons of non-college fathers, while the pattern is reversed in the upper part of the distribution. Thus, the null average effect reported in Table 4 masks some important heterogeneity for this group.

FIGURE 5.
RD-DD estimates of the effect of the reform on GPA: heterogeneity by position in the grade distribution



NOTE.— Each point in the graph depicts the RD-DD point estimate of the effect of the reform on the probability of having grades above the 10th, 25th, 50th, 75th, and 90th percentile, respectively. Rankings are calculated based on the student's position in the full grade distribution. We use a linear function for the running variable and apply triangular regression weights. The sample includes children born in a six-month window around each included turn-of-year cutoff. Estimates are based on daily detail on the running variable. Control variables included.

6.3 Intergenerational transmission of skills

The findings we have presented thus far suggest that the 1995 parental leave reform increased the social gradient in schooling outcomes mainly for boys. Put differently, the reform appears to have strengthened the intergenerational correlation between fathers' education levels and the educational outcomes of their sons. To directly assess the parental leave reform's impact on the intergenerational gradient in human capital, we link information on fathers' standardized cognitive and non-cognitive test scores from the Swedish military draft registry to our analysis sample. We then extend our RD-DD model in Equation (2) by interacting all regressors with a continuous measure of the father's standardized cognitive ability. This model alteration effectively converts our model into a triple-difference analysis.[24]

Estimation results are presented in Table 5 in which columns (1), (2), and (3) display point estimates for the full sample, for boys only and for girls only, respectively. The first two rows of the table report estimates for the variables that constitute the main regressors of interest in our RD-DD model: the end-of-year indicator, r, measuring the discontinuous impact of being born early relative to late in the year, and the treatment year indicator, T, measuring the effect of being born in the 12-month window around the end-of-year cutoff in the reform year relative to the corresponding period for the control years. The parameter estimate of the end-of-year cutoff dummy variable is positive and statistically significant, reflecting that children born earlier in the year have, on average, better school-leaving GPAs. In contrast, the effect of being born in the six-month window around the reform year is positive but close to zero, implying that the youngest cohort has, on average, slightly improved GPAs compared to previous cohorts. The third row of the table reproduces our main RD-DD results from Table 4, showing that the 1995 reform had an adverse effect on children's school-leaving GPA, in particular for boys.[25]

The fourth row in the table presents parameter estimates for the continuous measure of the father's cognitive ability at age 18 from the military draft. The estimated coef-

[24]That is, we compare RD estimates for fathers with different levels of cognitive and non-cognitive abilities across treatment and control years.

[25]Note that these results differ slightly from our main RD-DD results in Table 4 since we only have information on fathers' skills for around 80 percent of our analysis sample.

ficient for this variable is strongly positive, reflecting the fact that fathers with higher cognitive skills tend to have children with higher school-leaving GPAs. The estimate of the correlation is 0.25 for both boys and girls, which is consistent with the results in Grönqvist, Öckert and Vlachos (2017).[26]

The next two rows of Table 5 present first-order interactions between the father's cognitive skills and the end-of-year cutoff and reform year indicators. The estimates reported in the former row indicate that children to fathers with higher cognitive ability do not enjoy additional benefits from being born early in the year, relative to children to fathers with lower cognitive ability. This result is reassuring for the validity of our analysis as it suggests that any manipulation of treatment status based on fathers' cognitive ability is unlikely to bias our reform effects. The results displayed in the sixth (second to last) row suggest that there is a small but statistically significant reduction in the intergenerational link between fathers' cognitive ability and sons' schooling outcomes for the reform year cohort, relative to the control year cohorts.

Finally, the main focus of the analysis is the second-order interaction displayed in the last row of the table, which reports the effect of the the 1995 parental leave reform on the strength of the intergenerational correlation of human capital between fathers and their children. The pooled estimate in the first column is relatively small in magnitude and marginally statistically significant at the 10 percent level. However, this estimate masks considerable heterogeneity by child sex: The point estimate of 0.07 for boys is large in relation to the baseline correlation of around 0.25 percent and statistically significant, while the corresponding estimate for girls is very close to zero. This result is hence interpreted as that the reform led to a reduction in the intergenerational mobility of human capital for boys by about 30 percent. Comparing the baseline effect for children at the mean of the paternal cognitive skill distribution displayed in the third row, we see that boys with fathers whose standardized cognitive skills are one standard deviation

[26] Using data from the German Socioeconomic Panel (GSOEP), Anger and Heineck (2010) estimate a larger intergenerational correlation in skills: between 0.45 and 0.5. They also document that skills that are based on learning are more strongly transmitted between generations than innate abilities, which is incompatible with a pure genetic model and thus point to the importance of parental investments for children's cognitive skills.

above the mean of the cognitive skill distribution may have benefited from the parental leave reform as the combined net effect ($-0.054 + 0.074$) is positive. We interpret these findings as supporting the hypothesis that the quality of time investments by the leave-taking father may explain some of the effects of the 1995 parental leave reform on boys' school-leaving GPAs.

TABLE 5.
RD-DD estimates of the 1995 parental leave reform on intergenerational transmission of human capital: Father's cognitive skills

	(1) All	(2) Boys	(3) Girls
r	0.144***	0.148***	0.142***
	(0.009)	(0.012)	(0.013)
T	0.033**	0.012	0.024
	(0.013)	(0.019)	(0.020)
$r \times T$	-0.030*	-0.054**	-0.013
	(0.018)	(0.025)	(0.026)
Father's cognitive skills	0.254***	0.249***	0.252***
	(0.007)	(0.009)	(0.010)
$r \times$ Father's cognitive skills	0.005	0.004	0.014
	(0.009)	(0.012)	(0.013)
$T \times$ Father's cognitive skills	-0.034**	-0.046**	-0.018
	(0.014)	(0.019)	(0.019)
$r \times T \times$ Father's cognitive skills	0.037*	0.074***	-0.002
	(0.019)	(0.026)	(0.027)
N	249,999	127,804	122,195

NOTE.— Estimated discontinuities from RD-DD model on children's intergenerational correlation in human capital (father's standardized cognitive skills from military enlistment). r is an indicator that takes the value 1 if child i is born in January or later, T is an indicator that takes the value 1 if the child is born in the treatment cohort, and zero otherwise. We use a linear function for the running variable and apply triangular regression weights. The sample includes children born in a six-month window around each included turn-of-year cutoff. Estimates are based on daily detail on the running variable. Control variables included. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

We have also estimated models of intergenerational correlation of human capital for other measures of cognitive skill. Table 6 (and Table A.11) show results for the strength of the relationship between children and their paternal (maternal) uncle's cognitive skills. The results show that the correlation between children's and their uncle's skills is unaffected by the parental leave reform for both boys and girls. Hence, these results strengthen our conclusion that the effects we document are driven by fathers' time investments, i.e., a nurture effect.[27]

[27]In Table A.12, we also report results from a corresponding analysis of the reform effect on the correlation between fathers' *non-cognitive* skills and children's GPA. The results suggests that, unlike for cognitive skills, there are no important effects of the parental leave reform on the intergenerational correlation between children's GPA and father's non-cognitive skills.

TABLE 6.
RD-DD estimates of the 1995 parental leave reform on intergenerational transmission of human capital: Child-uncle cognitive skills (father's brother)

	(1) All	(2) Boys	(3) Girls
r	0.151***	0.151***	0.153***
	(0.012)	(0.016)	(0.017)
T	0.049***	0.026	0.045*
	(0.018)	(0.024)	(0.026)
$r \times T$	-0.045*	-0.043	-0.054
	(0.024)	(0.032)	(0.034)
Uncle's cognitive skills	0.175***	0.162***	0.180***
	(0.010)	(0.012)	(0.014)
$r \times$ Uncle's cognitive skills	-0.001	0.029*	-0.022
	(0.013)	(0.017)	(0.018)
$T \times$ Uncle's cognitive skills	0.008	-0.005	0.016
	(0.019)	(0.026)	(0.026)
$r \times T \times$ Uncle's cognitive skills	0.011	0.020	0.003
	(0.026)	(0.035)	(0.038)
N	152,080	78,058	74,022

NOTE.— Estimated discontinuities from RD-DD model on children's intergenerational correlation in human capital (uncle's standardized cognitive skills from military enlistment). r is an indicator that takes the value 1 if child i is born in January or later, T is an indicator that takes the value 1 if the child is born in the treatment cohort, and zero otherwise. We use a linear function for the running variable and apply triangular regression weights. The sample includes children born in a six-month window around each included turn-of-year cutoff. Estimates are based on daily detail on the running variable. Control variables included. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

7 Mechanisms

The results presented in the previous section provided a thorough empirical investigation of the effects of the 1995 parental leave reform on children's school-leaving GPAs. In this section, we shift focus to exploring potential mechanisms underlying these effects. We begin by studying effects on parental stability, followed by children's mental health, subsequent fertility and, lastly, role model effects.

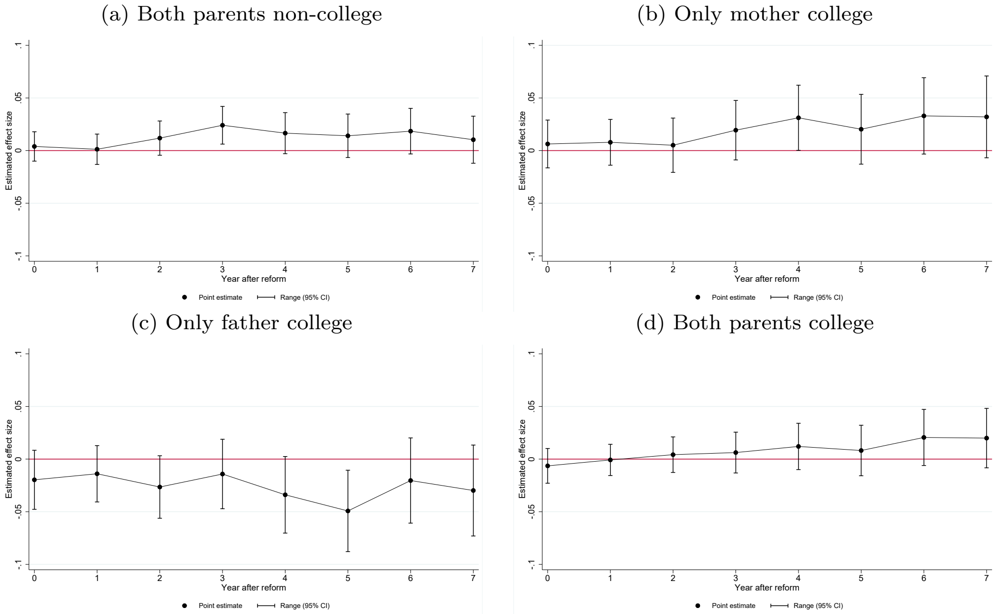
7.1 Marital stability

We first focus on the marital stability channel, which has previously been explored in Avdic and Karimi (2018). In their paper, the authors document an increase in the probability of couple dissolution from the same reform, but stop short of breaking down this result by parental education. Heterogeneity in the propensity of couple dissolution may be present if the economic or labor market consequences of the incentivized behavioral changes or cultural and social perceptions of traditional gender norms with respect to the role of the father in caring for children vary across parental groups based on their educational attainments. If more conservative perceptions of gender norms are concentrated among lower-educated parents or if they face more constrains, we would expect to see an increase in the divorce rates in families with non-college educated fathers. In Sweden at the time of the 1995 parental leave reform, a divorce typically resulted in sole custody of the children for the mother. Consequentially, if the increase in fathers leave taking led to divorce, the children could actually have had less access to their fathers which may adversely have affected cognitive development and subsequent educational outcomes. Conversely, more paternal time investments in joint children could also improve the stability of the relationship, in which case we would expect to observe reduced divorce rates and improved human capital outcomes of children.

To explore this question, we once again estimate our RD-DD model by replacing the outcome with a set of indicators for whether the parents were separated by years since childbirth. Figure 6 plots these results by age of the focal child for each of the four parental education groups, respectively. The results generally suggest that parents in families with non-college fathers, reported in panels (a) and (b), are more likely to separate in the first seven years after the child was born. For non-college couples, there is a significant increase in separation risk when the focal child is three years old, after which the estimated coefficient becomes slightly more muted. For unions between non-college fathers and college mothers there is an increased risk of separation over time, although confidence intervals are large, suggesting that parental education differences and different labor market opportunities may be factors that contribute to additional frictions in the relationship. In line with this reasoning, the pattern for couples with a college-educated father and a non-college educated mother in panel (c) exhibits a *negative* effect of the reform on separation probability, while the impact on families with two college-educated spouses is non-significant throughout the entire followup period.[28]

[28]See Table A.13 in Appendix A for formal regression parameter estimates.

Figure 6.
RD-DD estimates of the 1995 parental leave reform on couple dissolution: Parental education mix



Note.— Each point in the graph represents RD-DD point estimate of the effect of the 1995 paternity leave reform on the likelihood of couple separation at different years after birth. We use a linear function for the running variable and apply triangular regression weights. The sample includes children born in a six-month window around each included turn-of-year cutoff. Estimates are based on daily detail on the running variable. Control variables included. Robust standard errors.

One possible takeaway from these results is that the reform effect on children's school-leaving GPAs may have been mediated by differential changes in fathers' time investments due to asymmetric changes in the propensity to separate after childbirth across different family constellations. In particular, non-college fathers may have become more absent as a consequence of the increased separation probability which negatively impacted the cognitive development of their sons. Interestingly, since we do not find any adverse effects of the 1995 reform on children to college-educated mothers, it appears as they were better able to compensate for the paternal absence compared to non-college educated mothers.

7.2 Child mental health

While parental separation may lead to a lower degree of father's time investments in their children, parental conflict can also imply a less functioning family environment and be traumatic for the children, with potential negative consequences for physical and mental well-being. To analyse these channels, we include data on children's healthcare utilization, measures by inpatient care and drug prescriptions during ages 14–16, when they attended grades 7–9 in compulsory school, as outcomes in our RD-DD model. Specifically, we use data from the national inpatient and prescription drug registers to assess whether children were more likely to be admitted to the hospital and if they were prescribed drugs for behavioral, depression and anxiety-related conditions. We argue that if there are no noticeable effects on children's healthcare use in the parental education groups for which we observe changes in dissolution propensity, it is likely that the reform effect on children's school-leaving GPA is more related to changes in parental time investments and cognitive stimulus than to the potentially disruptive effects of family conflict and separation.

TABLE 7.
RD-DD estimates of the 1995 parental leave reform by father's education level: Received health care for mental-behavioral causes, ages 14-16

		All mothers		Mother non-college		Mother college	
	All	Father Non-college	Father College	Father Non-college	Father college	Father Non-college	Father College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mental health prescriptions							
Any script, all	-0.009	-0.017	0.010	-0.010	-0.000	-0.049*	0.017
	(0.010)	(0.013)	(0.016)	(0.015)	(0.025)	(0.029)	(0.020)
N	305,751	218,904	86,847	179,030	35,054	39,874	51,793
Any script, boys	-0.023	-0.030	-0.008	-0.027	-0.018	-0.051	-0.000
	(0.016)	(0.020)	(0.023)	(0.023)	(0.035)	(0.043)	(0.030)
N	156,153	111,744	44,409	91,373	17,984	20,371	26,425
Any script, girls	0.008	-0.000	0.028	0.011	0.020	-0.043	0.035
	(0.013)	(0.017)	(0.022)	(0.019)	(0.037)	(0.038)	(0.027)
N	149,598	107,160	42,438	87,657	17,070	19,503	25,368
Inpatient care							
Any care episode, all	0.001	0.001	-0.001	0.002	-0.002	-0.008	0.004
	(0.002)	(0.003)	(0.004)	(0.003)	(0.006)	(0.007)	(0.004)
N	305,751	218,904	86,847	179,030	39,874	35,054	51,793
Any care episode, boys	0.001	0.003	-0.005	0.002	0.005	-0.012	0.001
	(0.003)	(0.003)	(0.004)	(0.004)	(0.007)	(0.009)	(0.004)
N	156,153	111,744	44,409	91,373	20,371	17,984	26,425
Any care episode, girls	0.000	-0.001	0.003	0.002	-0.010	-0.004	0.007
	(0.004)	(0.004)	(0.006)	(0.005)	(0.010)	(0.011)	(0.008)
N	149,598	107,160	42,438	87,657	19,503	17,070	25,368

NOTE.— Estimated discontinuities from RD-DD model on the likelihood that children have received outpatient- or inpatient care related to mental- or behavioral issues (chapter "F" of the ICD directory) between ages 14 and 16. We use a linear function for the running variable and apply triangular regression weights. The sample includes children born in a six-month window around each included turn-of-year cutoff. Estimates are based on daily detail on the running variable. Control variables included. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7 reports estimation results from the RD-DD model by parental education group and child sex. Most coefficients are statistically insignificant and close to zero, implying that the 1995 parental leave reform did not change the mental health and well-being of children in lower secondary school to any important extent. In particular, the estimated effects for boys with college and non-college fathers, where we expected any mental health effects to be particularly noticeable, are very similar in magnitude. We interpret these findings as that it is unlikely that children's mental health was significantly affected by the changes in couple dissolution probability arising from the reform during the time period that is relevant for their school-leaving GPA.

7.3 Subsequent fertility

Another possible explanation for the impact of the 1995 parental leave reform on children's human capital accumulation is its potential effect on parents' subsequent fertility. Since time is a fixed resource, additional children in the household will lead to less available parental time per child. To evaluate this conjecture, we estimate our RD-DD model separately by parental education group using completed fertility as outcome. Similar to the analysis on couple dissolution, this analysis will provide information about whether any reform effects on the number of children per family aligns with the results for children's school-leaving GPA.

Table 8 reports points estimates for the 1995 parental leave reform effect on completed family size by parental education group (columns) and by spouse (rows). To capture the full effect on family size, we include all children born to each parent in our data. The estimation results indicate that family size does indeed significantly increase as a consequence of the parental leave reform. While interesting in itself, the economic magnitude of the point estimates are negligible. For example, the average effect of 0.03 children for the full sample implies that, on average, only three out of one hundred couples had another child due to the parental leave reform. Our interpretation is that reform effects on family size and subsequent impact on parents' time investments may, at best, have had a small role in explaining the impact on the focal child's school-leaving GPA.[29]

[29] Interestingly, the fertility effect is concentrated among low-educated couples, which suggests a link to the results on couple dissolution. One possible explanation for this relation is that couple dissolution may lead separated spouses to form families with new partners. This situation would increase fertility in counter-factual cases where couples who separated due to the parental leave reform had completed their fertility. The implication that higher fertility would lead to increased scarcity of parental time would also be relevant in this case.

TABLE 8.
RD-DD estimates of the 1995 parental leave reform by father's education level: Completed family size

		All mothers		Mother non-college		Mother college	
	All	Father Non-college	Father College	Father Non-college	Father college	Father Non-college	Father College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Children (mother)	0.029**	0.030*	0.027	0.038*	0.003	0.046	0.018
	(0.014)	(0.017)	(0.024)	(0.020)	(0.035)	(0.040)	(0.030)
Children (father)	0.035**	0.044**	0.015	0.053**	0.017	0.022	0.014
	(0.015)	(0.019)	(0.026)	(0.021)	(0.038)	(0.043)	(0.033)
N	304,275	218,483	85,792	178721	39,762	34,736	51,056

NOTE.— Estimated discontinuities from RD-DD model on the completed family size (number of children by age 45). We use a linear function for the running variable and apply triangular regression weights. The sample includes children born in a six-month window around each included turn-of-year cutoff. Estimates are based on daily detail on the running variable. Control variables included. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

7.4 Role model effects

Finally, we study whether role model effects can contribute to understanding our main results on school-leaving GPAs. To this end, we split our outcome variable into grades in subjects corresponding to STEM (science, technology, engineering and mathematics) and social science and humanities (HUMSAM) and arts We calculate subject-specific GPAs for each child in the sample. Next, we use the field of education of each father's highest degree to classify fathers into corresponding groups and estimate split-sample regressions for each group by child sex. Since occupations related to the field of education vary across college and non-college educated parents, we also split our analysis into fathers' education level for consistency.

Table 9 presents the estimation results from this analysis. If role-model effects are present in our data, we would expect to see a positive correspondence between children's subject-specific grades and their fathers' field of study, to the extent that the reform led to more exposure to the father as a role model. In other words, we expect the diagonal elements in each 2×2 coefficient matrix in the table to be positive, but more so for college educated fathers. Moreover, we also anticipate the off-diagonal elements to be negative if the father's capacity to transfer skills is not complementary across subject groups (within education level category). The estimated results provide a mixed message: on the one hand, it seems that the correspondence between boys' school-leaving grades and college educated fathers' field of education with respect to STEM subjects is strong and statistically significant. On the other hand, all other point estimates in the table are statistically indistinguishable from zero and without a clear sign pattern. If anything, it appears that college educated fathers with a STEM education improve their sons' grades irrespective of the subject type, whereas the opposite is true for father's with a HUMSAM or Arts background. We conclude that the evidence for role-model effects is inconclusive.

TABLE 9.
RD-DD estimates of the 1995 parental leave reform by fathers' education level and field: role model effects

	Father non-college		Father college	
	HUMSAM	Science/Technical	HUMSAM	Science/Technical
All				
$r \times T \times$ Hum-sam/Arts	-0.004	-0.013	-0.052	0.022
	(0.036)	(0.027)	(0.063)	(0.059)
$r \times \times$ Technical/science	-0.009	-0.023	-0.018	0.072
	(0.034)	(0.026)	(0.046)	(0.045)
N	218,904	218,904	86,847	86,847
Boys				
$r \times T \times$ Hum-sam/Arts	-0.003	-0.050	-0.103	0.086
	(0.045)	(0.041)	(0.085)	(0.085)
$r \times \times$ Technical/science	-0.006	-0.055	-0.088	0.180***
	(0.041)	(0.038)	(0.059)	(0.065)
N	111,744	111,744	44,409	44,409
Girls				
$r \times T \times$ Hum-sam/Arts	-0.031	0.032	-0.020	-0.040
	(0.056)	(0.035)	(0.093)	(0.080)
$r \times \times$ Technical/science	-0.027	0.014	0.057	-0.051
	(0.053)	(0.033)	(0.069)	(0.062)
N	107,160	107,160	42,438	42,438

NOTE.— Estimated discontinuities from RD-DD model on children's subject-specific standardized compulsory school leaving grades by father's educational background (level and field). We use a linear function for the running variable and apply triangular regression weights. The sample includes children born in a six-month window around each included turn-of-year cutoff. Estimates are based on daily detail on the running variable. Control variables included. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

8 Conclusion

This paper analyses how an increase in fathers' uptake of parental leave affects the development of children's cognitive skills in an RD-DD framework. To this end, we exploit the first Swedish parental leave reform that reserved leave for fathers in 1995 with the use of rich Swedish administrative data. Extending previous work by Duvander and Johansson (2012), Ekberg, Eriksson and Friebel (2013), and Avdic and Karimi (2018), we first study how the reform impacted parental leave uptake, finding that the reform was equally effective in increasing fathers' leave uptake across all parental education groups.

Thus, we provide robust evidence that the reform substituted paternal time for maternal time.

While we find only weak evidence that the 1995 parental leave reform had an effect on overall average compulsory school-leaving GPAs, we show that the parental leave reform decreased the average GPA of sons of non-college educated fathers by 0.07 standard deviations. We find no corresponding effects for girls. We test the implications of these results for intergenerational skill correlations and show that the reform increased the correlation between fathers' cognitive skills and sons' school-leaving GPAs by 0.074 standard deviations, or about 30 percent.

To further explore the mechanisms underlying our main effects, we reexamine the findings in Avdic and Karimi (2018) and show that the reform increased the probability of separation for families with two non-college educated parents, whereas partner stability increased in families in which only the father had a college degree. We examine the possibility that family disruption and conflict may have negatively influenced children's mental health, but find no evidence in support of this conjecture.

Combining these results with the results on children's school-leaving GPA suggests that substitution of maternal time investments for time investments of fathers with relatively low human capital, and the increase in divorces in families with less than college educated parents, reducing children's access to their fathers, likely have a role in explaining the reform's negative effects on boy's human capital development. These results are also in line with some of the recent literature on the effects of parental time investments and the sensitivity of boys to family separations (Bertrand and Pan, 2013; Gould, Simhon and Weinberg, 2020). Finally, we explore if role model effects can contribute to the understanding of our results by comparing reform effects on subject-specific grades of children with fathers of different fields of education. We find limited support for the role model hypothesis.

Our findings confirm that parental time investments matter for human capital development. In particular, our results suggest that paternal time investments, presence, education and cognitive skills are important for the schooling outcomes of boys. While increasing beneficial time investments for some children, the 1995 reform also, at least temporarily, had the unintentional consequence of reducing paternal presence for children with non-college educated fathers. Although we are not able to identify precise channels for this effect asymmetry, it is possible that conflicting gender norms may have contributed to the increased separation risk for the latter family type. At any rate, it is an important task for future research to explore the extent to which paternity leave reforms may contribute to changing norms and expectations in the longer run, such that a more gender balanced parental leave uptake might contribute to family stability and be beneficial also for children with lower educated parents.

References

Adda, Jérôme, Anders Bjorklund, and Helena Holmlund. 2011. "The role of mothers and fathers in providing skills: Evidence from parental deaths." *Mimeo.*

Anger, Silke, and Guido Heineck. 2010. "Do smart parents raise smart children? The intergenerational transmission of cognitive abilities." *Journal of population Economics*, 23(3): 1105–1132.

Autor, David, David Figlio, Krzysztof Karbownik, Jeffrey Roth, and Melanie Wasserman. 2019. "Family disadvantage and the gender gap in behavioral and educational outcomes." *American Economic Journal: Applied Economics*, 11(3): 338–81.

Avdic, Daniel, and Arizo Karimi. 2018. "Modern family? Paternity leave and marital stability." *American Economic Journal: Applied Economics*, 10(4): 283–307.

Baker, Michael, and Kevin Milligan. 2008*a*. "How Does Job-Protected Maternity Leave Affect Mothers' Employment?" *Journal of Labor Economics*, 26(4): 655–691.

Baker, Michael, and Kevin Milligan. 2008*b*. "Maternal employment, breastfeeding, and health: Evidence from maternity leave mandates." *Journal of Health Economics*, 27(4): 871–887.

Baker, Michael, and Kevin Milligan. 2010. "Evidence from Maternity Leave Expansions of the Impact of Maternal Care on Early Child Development." *Journal of Human Resources*, 45(1): 1–32.

Baker, Michael, and Kevin Milligan. 2015. "Maternity leave and children's cognitive and behavioral development." *Journal of Population Economics*, 28(2): 373–391.

Becker, Gary S, and Nigel Tomes. 1986. "Human capital and the rise and fall of families." *Journal of labor economics*, 4(3, Part 2): S1–S39.

Bergemann, Annette, and Regina T. Riphahn. 2015. "Maternal Employment Effects of Paid Parental Leave." IZA Working Paper 9073.

Bertrand, Marianne, and Jessica Pan. 2013. "The Trouble with Boys: Social Influences and the Gender Gap in Disruptive Behavior." *American Economic Journal: Applied Economics*, 5(1): 32–64.

Black, Sandra E, Paul J Devereux, and Kjell G Salvanes. 2011. "Too young to leave the nest? The effects of school starting age." *The Review of Economics and Statistics*, 93(2): 455–467.

Buckles, Kasey S, and Daniel M Hungerman. 2013. "Season of birth and later outcomes: Old questions, new answers." *Review of Economics and Statistics*, 95(3): 711–724.

Carneiro, Pedro, Katrine V. Løken, and Kjell G. Salvanes. 2015. "A Flying Start? Maternity Leave Benefits and Long-Run Outcomes of Children." *Journal of Political Economy*, 123(2): 365–412.

Cattaneo, Matias D, Michael Jansson, and Xinwei Ma. 2016. "rddensity: Manipulation testing based on density discontinuity." *The Stata Journal (ii)*, 1–18.

Cools, Sara, Jon H. Fiva, and Lars J. Kirkebøen. 2015. "Causal Effects of Paternity Leave on Children and Parents." *The Scandinavian Journal of Economics*, 117(3): 801–828.

Cunha, Flavio, and James Heckman. 2007. "The technology of skill formation." *American Economic Review*, 97(2): 31–47.

Cunha, Flavio, James J Heckman, Lance Lochner, and Dimitriy V Masterov. 2006. "Interpreting the evidence on life cycle skill formation." *Handbook of the Economics of Education*, 1: 697–812.

Currie, Janet, and Douglas Almond. 2011. "Human capital development before age five." In *Handbook of labor economics.* Vol. 4, 1315–1486. Elsevier.

Dahl, Gordon B, Katrine V Løken, and Magne Mogstad. 2014. "Peer Effects in Program Participation." *The American Economic Review*, 104(7): 2049–2074.

Dahl, Gordon B, Katrine V Løken, Magne Mogstad, and Kari Vea Salvanes. 2016. "What is the case for paid maternity leave?" *Review of Economics and Statistics*, 98(4): 655–670.

Danzer, Natalia, and Victor Lavy. 2017. "Paid parental leave and children's schooling outcomes." *The Economic Journal, forthcoming.*

Del Bono, Emilia, Marco Francesconi, Yvonne Kelly, and Amanda Sacker. 2016. "Early maternal time investment and early child outcomes." *The Economic Journal*, 126(596): F96–F135.

Doepke, Matthias, and Fabrizio Zilibotti. 2017. "Parenting with style: Altruism and paternalism in intergenerational preference transmission." *Econometrica*, 85(5): 1331–1371.

Druedahl, Jeppe, Mette Ejrnæs, and Thomas H Jørgensen. 2019. "Earmarked paternity leave and the relative income within couples." *Economics Letters*, 180: 85–88.

Dustmann, Christian, and Uta Schönberg. 2012. "Expansions in Maternity Leave Coverage and Children's Long-Term Outcomes." *American Economic Journal: Applied Economics*, 4(3): 190–224.

Duvander, Ann-Zofie, and Mats Johansson. 2012. "What are the effects of reforms promoting fathers' parental leave use?" *Journal of European Social Policy*, 22(3): 319–330.

Ekberg, John, Rickard Eriksson, and Guido Friebel. 2013. "Parental leave. A policy evaluation of the Swedish Daddy-Month reform." *Journal of Public Economics*, 97: 131–143.

Eriksson, Rickard. 2005. "Parental Leave in Sweden: The Effects of the Second Daddy Month." Swedish Institute for Social Research (SOFI) Working Paper 9/2005.

Farré, Lídia, and Libertad González. 2019. "Does paternity leave reduce fertility?" *Journal of Public Economics*, 172: 52–66.

Farré, Lídia, Cristina Felfe, Libertad González, Patrick Schneider, et al. 2022. *Changing gender norms across generations: Evidence from a paternity leave reform.* Universitat Pompeu Fabra, Department of Economics and Business.

Fiorini, Mario, and Michael P Keane. 2014. "How the allocation of children's time affects cognitive and noncognitive development." *Journal of Labor Economics*, 32(4): 787–836.

Francesconi, Marco, and James J Heckman. 2016. "Child development and parental investment: Introduction." *The Economic Journal*, 126(596): F1–F27.

Fredriksson, Peter, and Björn Öckert. 2014. "Life-cycle Effects of Age at School Start." *The Economic Journal*, 124(579): 977–1004.

Gauthier, Anne H. 2013. "Family policy and fertility: do policies make a difference?" In *Fertility rates and population decline.* 269–287. Springer.

Ginja, Rita, Jenny Jans, and Arizo Karimi. 2020. "Parental leave benefits, household labor supply, and children's long-run outcomes." *Journal of Labor Economics*, 38(1): 261–320.

González, Libertad, and Hosny Zoabi. 2021. "Does Paternity Leave Promote Gender Equality within Households?"

Gould, Eric D, Avi Simhon, and Bruce A Weinberg. 2020. "Does parental quality matter? Evidence on the transmission of human capital using variation in parental influence from death, divorce, and family size." *Journal of Labor Economics*, 38(2): 569–610.

Grönqvist, Erik, Björn Öckert, and Jonas Vlachos. 2017. "The intergenerational transmission of cognitive and noncognitive abilities." *Journal of Human Resources*, 52(4): 887–918.

Guryan, Jonathan, Erik Hurst, and Melissa Kearney. 2008. "Parental education and parental time with children." *Journal of Economic perspectives*, 22(3): 23–46.

Han, Wen-Jui, Christopher Ruhm, and Jane Waldfogel. 2009. "Parental leave policies and parents' employment and leave-taking." *Journal of Policy Analysis and Management*, 28(1): 29–54.

Holmlund, Helena, Mikael Lindahl, and Erik Plug. 2011. "The causal effect of parents' schooling on children's schooling: A comparison of estimation methods." *Journal of Economic Literature*, 49(3): 615–51.

Johnsen, Julian, Hyejin Ku, and Kjell G Salvanes. 2020. "Competition and Career Advancement: The Hidden Costs of Paid Leave." *NHH Dept. of Economics Discussion Paper*, , (13).

Kalil, Ariel, Magne Mogstad, Mari Rege, and Mark E Votruba. 2016. "Father presence and the intergenerational transmission of educational attainment." *Journal of Human Resources*, 51(4): 869–899.

Kotsadam, Andreas, and Henning Finseraas. 2011. "The State Intervenes in the Battle of the Sexes: Causal Effects of Paternity Leave." *Social Science Research*, 40(6): 1611–1622.

Lalive, Rafael, Analia Schlosser, Andreas Steinhauer, and Josef Zweimüller. 2014. "Parental Leave and Mothers' Careers: The Relative Importance of Job Protection and Cash Benefits." *Review of Economic Studies*, 81(1): 219–265.

Lalive, Rafael, and Josef Zweimüller. 2009. "How Does Parental Leave Affect Fertility and Return to Work? Evidence from Two Natural Experiments." *The Quarterly Journal of Economics*, 124(3): 1363–1402.

Liu, Qian, and Oskar Nordström Skans. 2010. "The duration of paid parental leave and children's scholastic performance." *The BE Journal of Economic Analysis & Policy*, 10(1).

Moberg, Ylva. 2017. "Speedy Responses: Effects of higher benefits on take-up and division of parental leave." Mimeo.

Parker, Kim, and Wendy Wang. 2013. "Modern parenthood." *Pew Research Center's Social & Demographic Trends Project*, 14.

Patnaik, Ankita. 2016. "Reserving Time for Daddy: The Short and Long-Run Consequences of Fathers' Quotas." Unpublished Manuscript.

Persson, Petra, and Maya Rossin-Slater. 2019. "When Dad Can Stay Home: Fathers' Workplace Flexibility and Maternal Health." National Bureau of Economic Research.

Rasmussen, Astrid Würtz. 2010. "Increasing the length of parents' birth-related leave: The effect on children's long-term educational outcomes." *Labour Economics*, 17(1): 91–100.

Rege, Mari, and Ingeborg F Solli. 2010. "The Impact of Paternity Leave on Long-Term Father Involvement." CESifo Group Munich Working Paper 3130, CESifo Working Paper Series.

Rossin, Maya. 2011. "The effects of maternity leave on children's birth and infant health outcomes in the United States." *Journal of Health Economics*, 30(2): 221–239.

Rossin-Slater, Maya. 2018. "Maternity and Family Leave Policy." *The Oxford Handbook of Women and the Economy*, 323.

Rossin-Slater, Maya, Christopher J. Ruhm, and Jane Waldfogel. 2013. "The Effects of California's Paid Family Leave Program on Mothers' Leave-Taking and Subsequent Labor Market Outcomes." *Journal of Policy Analysis and Management*, 32(2): 224–245.

Ruhm, Christopher J. 1998. "The Economic Consequences of Parental Leave Mandates: Lessons from Europe*." *The Quarterly Journal of Economics*, 113(1): 285.

Schönberg, Uta, and Johannes Ludsteck. 2007. "Maternity Leave Legislation, Female Labor Supply, and the Family Wage Gap." The Institute for the Study of Labor (IZA).

Schönberg, Uta, and Johannes Ludsteck. 2014. "Expansions in Maternity Leave Coverage and Mothers' Labor Market Outcomes after Childbirth." *Journal of Labor Economics*, 32(3): 469–505.

SOU 2011:51. 2011. "Fortsatt Föräldrar - om ansvar, ekonomi och samarbete för barnets skull." Swedish Government 2011:51.

Stearns, Jenna. 2015. "The effects of paid maternity leave: Evidence from Temporary Disability Insurance." *Journal of Health Economics*, 43: 85–102.

Tamm, Marcus. 2019. "Fathers' parental leave-taking, childcare involvement and labor market participation." *Labour Economics*, 59: 184–197.

Waldfogel, Jane. 1999. "The impact of the family and medical leave act." *Journal of Policy Analysis and Management*, 18(2): 281–302.

Appendix A

TABLE A.1.
Common intercept test: outcome variables

	RD 91/92	RD 92/93	RD 93/94	RD 94/95	RD-DD
			Father non-college		
GPA, boys	0.146	0.233	0.152	0.117	-0.068
SE	0.021	0.022	0.024	0.024	0.028
t-stat	6.899	10.401	6.356	4.815	-2.431
N	33,417	29,592	25,752	22,983	113,016
GPA, girls	0.117	0.164	0.139	0.147	0.006
SE	0.023	0.025	0.025	0.026	0.030
t-stat	5.136	6.660	5.455	5.579	0.197
N	31,698	28,352	25,013	22,097	108,376
Couple separation, year 3	0.026	0.010	0.011	0.039	0.026
SE	0.006	0.007	0.007	0.007	0.008
t-stat	4.161	1.492	1.597	5.612	3.228
N	65,062	57,884	50,705	45,032	221,167
Couple separation, year 5	0.023	0.015	0.013	0.033	0.019
SE	0.007	0.007	0.008	0.008	0.009
t-stat	3.323	2.073	1.685	4.008	2.069
N	65,030	57,858	50,692	45,017	221,077
			Father college		
GPA, boys	0.145	0.152	0.150	0.166	0.018
SE	0.034	0.035	0.035	0.036	0.042
t-stat	4.311	4.382	4.247	4.563	0.420
N	12,187	11,351	11,047	10,364	44,949
GPA, girls	0.132	0.191	0.146	0.141	-0.015
SE	0.034	0.037	0.036	0.036	0.042
t-stat	3.872	5.158	4.093	3.893	-0.352
N	11,646	10,949	10,652	9,684	42,931
Couple separation, year 3	0.006	0.018	0.006	0.006	-0.004
SE	0.007	0.008	0.008	0.008	0.009
t-stat	0.869	2.266	0.843	0.724	-0.468
N	23,707	22,179	21,551	19,901	87,338
Couple separation, year 5	0.009	0.025	0.016	-0.003	-0.019
SE	0.009	0.009	0.009	0.010	0.011
t-stat	1.036	2.698	1.755	-0.319	-1.788
N	23,638	22,092	21,493	19,877	87,100

NOTE.— The table reports the discontinuity in the outcome variable for children – and their parents – born around the turn-of-years before the reform (columns 1–3); the treatment turn-of-year (column 4), and the RD–DD estimate that nets out placebo-cohort differences in outcomes. We use a linear function for the running variable and apply triangular regression weights.

TABLE A.2.
McCrary RD-DD density test using different polynomials

Order of polynomial	(1) First	(2) Second	(3) Third	(4) Fourth	(5) Fifth	(6) Sixth
$r \times T$	-16.980***	-23.575***	-31.592***	-30.078***	-29.571**	-20.736
	(4.710)	(6.232)	(7.752)	(9.779)	(11.956)	(14.497)
r	64.149***	63.966***	30.237***	30.196***	18.173***	12.262
	(2.604)	(3.451)	(4.306)	(5.386)	(6.469)	(7.681)
T	-11.571***	-8.499*	4.611	2.405	2.714	-9.504
	(2.964)	(4.365)	(5.281)	(6.706)	(8.286)	(10.197)
N	2,920	2,920	2,920	2,920	2,920	2,920
AIC	27,614	27,020	26,786	26,741	26,732	26,696

NOTE.— Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.3.

RD-DD estimates of the 1995 parental leave reform: pre-determined covariates by parental education mix

	(1) All	(2) Both Non-college	(3) Only father Non-college	(4) Only mother Non-college	(5) Both College
Mother characteristics					
Age	-0.143	-0.148	0.299	0.293	-0.313
	(0.096)	(0.123)	(0.272)	(0.243)	(0.197)
Labor income (1000s SEK)	1.696	2.263	-3.154	4.615	3.796
	(1.690)	(1.919)	(4.672)	(4.521)	(5.052)
College	-0.019**				
	(0.009)				
Birth order of focal child	-0.009	-0.008	0.009	0.031	-0.063
	(0.018)	(0.025)	(0.054)	(0.048)	(0.041)
Father characteristics					
Age	-0.181	-0.224	0.118	0.110	-0.102
	(0.116)	(0.149)	(0.329)	(0.310)	(0.254)
Labor income (1000s SEK)	8.496**	4.359*	10.413	8.706	24.310
	(3.456)	(2.537)	(8.754)	(5.683)	(15.015)
College	-0.011				
	(0.009)				
Birth order of focal child	-0.018	-0.025	0.064	-0.009	-0.059
	(0.020)	(0.026)	(0.055)	(0.053)	(0.044)
Cognitive skills index	-0.021	-0.010	0.007	-0.048	0.034
	(0.021)	(0.025)	(0.050)	(0.053)	(0.041)
Non-cognitive skills index	-0.035	-0.035	-0.054	0.017	0.015
	(0.021)	(0.027)	(0.057)	(0.055)	(0.048)
Couple characteristics					
Spousal age gap	0.037	0.076	0.181	0.182	-0.211
	(0.085)	(0.114)	(0.257)	(0.232)	(0.188)
Mother's share of HH income	-0.007	-0.007	-0.011	-0.008	-0.003
	(0.004)	(0.006)	(0.012)	(0.011)	(0.009)
Pre-birth HH income (1000s SEK)	9.903**	6.336*	6.691	12.744*	27.796*
	(4.105)	(3.427)	(10.642)	(7.717)	(16.421)
Child characteristics					
Pre-term birth	0.010**	0.011*	0.001	-0.004	0.023**
	(0.004)	(0.006)	(0.012)	(0.011)	(0.010)
Low birth weight	0.004	0.007	-0.007	-0.006	0.007
	(0.003)	(0.005)	(0.010)	(0.009)	(0.008)
Low APGAR	0.005	0.001	0.005	0.007	0.017
	(0.007)	(0.010)	(0.021)	(0.020)	(0.017)
N	305,751	179,030	35,054	39,874	51,793

Note.— Estimated discontinuities from RD-DD model on families' pre-determined characteristics by father's and mother's educational background (secondary, tertiary). We use a linear function for the running variable and apply triangular regression weights. The sample includes children born in a six-month window around each included turn-of-year cutoff. Estimates are based on daily detail on the running variable. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.4.
Regression discontinuity estimates of the effect of the reform on parental leave usage

	Father PL	Mother PL	Mother's share	Total leave
		All		
Coefficient	20.981	-20.531	-0.051	0.854
t-statistic	15.218	-9.227	-14.139	0.491
SE	1.379	2.225	0.004	1.738
Mean	45.635	395.266	0.898	440.901
% Effect	0.460	-0.052	-0.057	0.002
N	134,075	134,075	134,075	134,075
		Both non-college		
Coefficient	22.515	-22.664	-0.054	0.176
t-statistic	12.785	-8.475	-12.881	0.082
SE	1.761	2.674	0.004	2.157
Mean	39.600	396.356	0.910	435.956
% Effect	0.569	-0.057	-0.059	0.000
N	74,337	74,337	74,337	74,337
		Only mother college		
Coefficient	19.473	-17.095	-0.043	1.631
t-statistic	5.170	-3.634	-5.145	0.452
SE	3.766	4.704	0.008	3.606
Mean	51.722	394.932	0.885	446.654
% Effect	0.376	-0.043	-0.048	0.004
N	18,780	18,780	18,780	18,780
		Only father college		
Coefficient	20.645	-13.501	-0.048	6.867
t-statistic	5.424	-2.958	-5.231	1.818
SE	3.807	4.564	0.009	3.777
Mean	46.269	395.605	0.897	441.874
% Effect	0.446	-0.034	-0.053	0.016
N	15,822	15,822	15,822	15,822
		Both college		
Coefficient	19.204	-19.842	-0.049	1.996
t-statistic	4.427	-3.568	-4.843	0.606
SE	4.338	5.561	0.010	3.294
Mean	58.886	392.010	0.872	450.895
% Effect	0.326	-0.051	-0.057	0.004
N	25,136	25,136	25,136	25,136

Note.— RDD estimates of the effect of the 1995 reform on parents' leave uptake. We use a linear function for the running variable and apply a triangular kernel. The effects are estimated using the `Stata` command `rdrobust` with optimal bandwidth. The sample includes all children born in 1991–1995. Estimates are based on daily detail of the running variable.

Table A.5.

Regression discontinuity estimates of the effect of the reform on parental leave usage: Boys

	(1) Father PL	(2) Mother PL	(3) Mother's share	(4) Total leave
		All		
Coefficient	19.832	-19.626	-0.048	0.643
t-statistic	9.823	-6.399	-9.288	0.273
SE	2.019	3.067	0.005	2.360
Mean	45.842	394.929	0.897	440.771
% Effect	0.433	-0.050	-0.054	0.001
N	68,484	68,484	68,484	68,484
		Both non-college		
Coefficient	21.757	-21.680	-0.055	1.323
t-statistic	9.772	-5.541	-8.594	0.423
SE	2.226	3.913	0.006	3.126
Mean	40.008	395.766	0.909	435.775
% Effect	0.544	-0.055	-0.061	0.003
N	37,860	37,860	37,860	37,860
		Only mother college		
Coefficient	15.156	-15.512	-0.029	-1.220
t-statistic	2.751	-2.017	-2.035	-0.211
SE	5.508	7.690	0.014	5.793
Mean	52.878	393.302	0.883	446.181
% Effect	0.287	-0.039	-0.033	-0.003
N	9,570	9,570	9,570	9,570
		Only father college		
Coefficient	18.752	-13.400	-0.043	5.083
t-statistic	3.300	-2.041	-3.320	0.838
SE	5.682	6.564	0.013	6.066
Mean	46.951	394.407	0.895	441.357
% Effect	0.399	-0.034	-0.047	0.012
N	8,154	8,154	8,154	8,154
		Both college		
Coefficient	18.742	-20.520	-0.048	-1.985
t-statistic	3.001	-2.844	-3.475	-0.434
SE	6.245	7.216	0.014	4.576
Mean	57.593	393.937	0.874	451.529
% Effect	0.325	-0.052	-0.055	-0.004
N	12,900	12,900	12,900	12,900

Note.— RDD estimates of the effect of the 1995 reform on parents' leave uptake. We use a linear function for the running variable and apply a triangular kernel. The sample includes all children born in 1991–1995. The effects are estimated using the `Stata` command `rdrobust` with optimal bandwidth. Estimates are based on daily detail of the running variable.

Table A.6.
Regression discontinuity estimates of the effect of the reform on parental leave usage: Girls

	(1) Father PL	(2) Mother PL	(3) Mother's share	(4) Total leave
		All		
Coefficient	22.424	-21.033	-0.054	2.262
t-statistic	11.181	-7.315	-10.720	1.083
SE	2.006	2.875	0.005	2.089
Mean	45.421	395.616	0.898	441.036
% Effect	0.494	-0.053	-0.060	0.005
N	65,591	65,591	65,591	65,591
		Both non-college		
Coefficient	22.737	-23.811	-0.055	-0.245
t-statistic	8.806	-6.344	-8.839	-0.089
SE	2.582	3.753	0.006	2.758
Mean	39.173	396.971	0.911	436.144
% Effect	0.580	-0.060	-0.060	-0.001
N	36,477	36,477	36,477	36,477
		Only mother college		
Coefficient	22.903	-18.775	-0.050	5.822
t-statistic	3.926	-2.681	-3.729	1.360
SE	5.834	7.003	0.013	4.281
Mean	50.539	396.600	0.888	447.139
% Effect	0.453	-0.047	-0.056	0.013
N	9,210	9,210	9,210	9,210
		Only father college		
Coefficient	24.470	-13.632	-0.052	11.062
t-statistic	4.271	-2.182	-4.177	1.933
SE	5.729	6.248	0.012	5.722
Mean	45.554	396.861	0.899	442.415
% Effect	0.537	-0.034	-0.058	0.025
N	7,668	7,668	7,668	7,668
		Both college		
Coefficient	19.934	-18.675	-0.051	6.253
t-statistic	3.731	-2.550	-4.000	1.356
SE	5.342	7.322	0.013	4.610
Mean	60.219	390.023	0.869	450.242
% Effect	0.331	-0.048	-0.058	0.014
N	12,236	12,236	12,236	12,236

Note.— RDD estimates of the effect of the 1995 reform on parents' leave uptake. We use a linear function for the running variable and apply a triangular kernel. The sample includes all children born in 1991–1995. The effects are estimated using the `Stata` command `rdrobust` with optimal bandwidth. Estimates are based on daily detail of the running variable.

Table A.7.

RD-DD estimates of the 1995 parental leave reform by parents' education level: School leaving GPA, no covariates

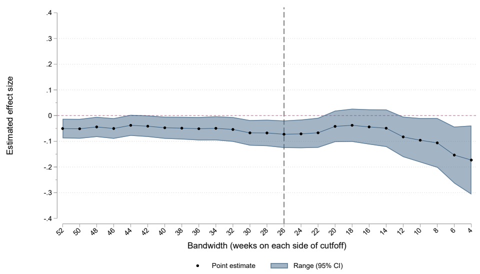
		All mothers		Mother non-college		Mother college	
	All	Father Non-college	Father College	Father Non-college	Father college	Father Non-college	Father College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
GPA, all	-0.027	-0.027	0.003	-0.036	0.018	0.037	0.007
	(0.018)	(0.021)	(0.030)	(0.023)	(0.048)	(0.045)	(0.037)
N	309,272	221,392	87,880	181,064	35,522	40,328	52,358
GPA, boys	-0.049**	-0.068**	0.017	-0.060*	0.069	-0.018	-0.004
	(0.024)	(0.028)	(0.042)	(0.031)	(0.065)	(0.061)	(0.052)
N	157,965	113,016	44,949	92,412	18,228	20,604	26,721
GPA, girls	-0.010	0.006	-0.015	-0.008	-0.041	0.034	0.013
	(0.026)	(0.030)	(0.042)	(0.034)	(0.067)	(0.063)	(0.051)
N	151,307	108,376	42,931	88,652	17,294	19,724	25,637

Note.— Estimated discontinuities from RD-DD model on children's standardized compulsory school leaving grades by father's and mother's educational background (secondary, tertiary). We use a linear function for the running variable and apply triangular regression weights. The sample includes all children born in a six-month window around the turn-of-year cutoffs. Estimates are based on daily detail on the running variable. Control variables not included. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

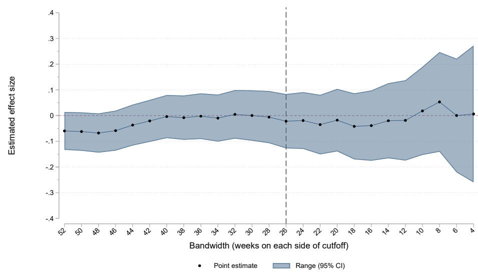
Figure A.1.

Sensitivity of estimates to choice of bandwidth: Boys' GPA

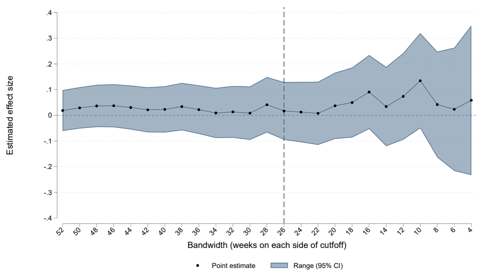
(a) Both parents non-college



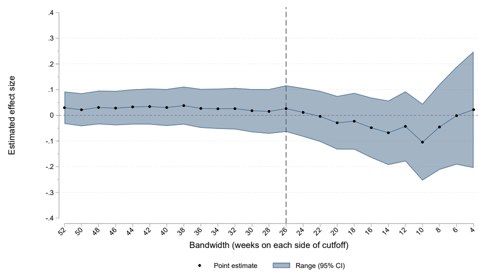
(b) Only mother college



(c) Only father college

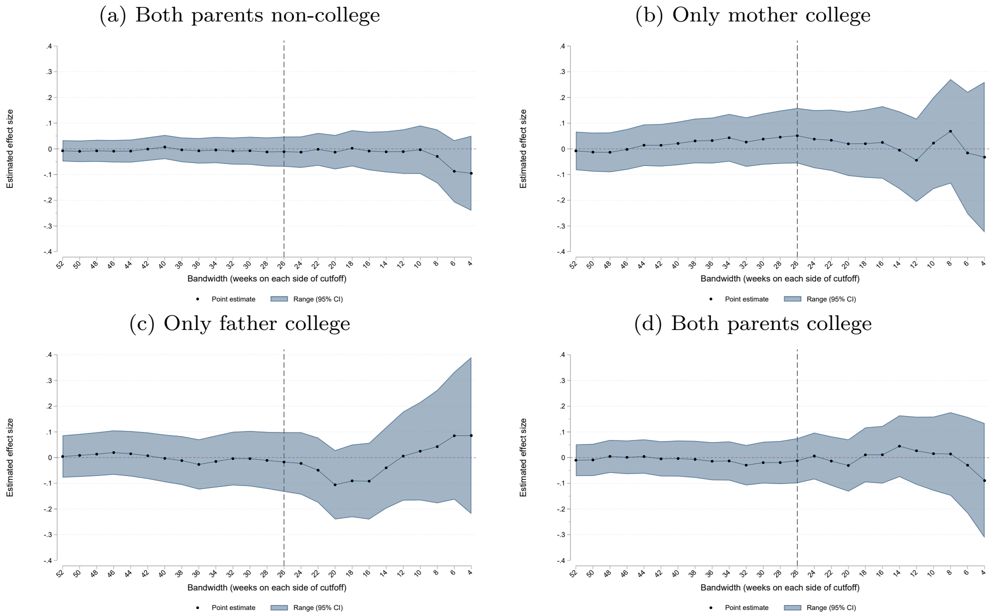


(d) Both parents college



Note.— We use a linear function for the running variable and apply triangular regression weights. Control variables included. Estimates are based on daily detail on the running variable.

Figure A.2.
Sensitivity of estimates to choice of bandwidth: Girls' GPA



Note.— We use a linear function for the running variable and apply triangular regression weights. Control variables included. Estimates are based on daily detail on the running variable.

Table A.8.
Difference-in-differences (DD) estimates of the 1995 parental leave reform by parents' education level: School leaving GPA

		All mothers		Mother non-college		Mother college	
	All	Father Non-college	Father College	Father Non-college	Father college	Father Non-college	Father College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Six-month window on either side of reform cutoff						
GPA, all	-0.020***	-0.019**	-0.002	-0.019*	0.004	-0.008	0.001
	(0.008)	(0.009)	(0.013)	(0.010)	(0.021)	(0.019)	(0.016)
N	305,751	218,904	86,847	179,030	35,054	39,874	51,793
GPA, boys	-0.025**	-0.036***	0.008	-0.033**	0.000	-0.030	0.021
	(0.010)	(0.012)	(0.018)	(0.013)	(0.028)	(0.026)	(0.022)
N	156,153	111,744	44,409	91,373	17,984	20,371	26,425
GPA, girls	-0.017	-0.009	-0.005	-0.011	0.011	0.001	-0.009
	(0.011)	(0.013)	(0.018)	(0.015)	(0.029)	(0.027)	(0.022)
N	149,598	107,160	42,438	87,657	17,070	19,503	25,368
	One-month window on either side of reform cutoff						
GPA, all	-0.033*	-0.036	-0.008	-0.046*	0.020	0.035	-0.010
	(0.020)	(0.023)	(0.034)	(0.026)	(0.054)	(0.050)	(0.042)
N	45,841	32,982	12,859	27,148	5,230	5,834	7,629
GPA, boys	-0.059**	-0.069**	-0.017	-0.072**	0.039	0.022	-0.031
	(0.027)	(0.031)	(0.047)	(0.035)	(0.073)	(0.069)	(0.061)
N	23,461	16,896	6,565	13,919	2,674	2,977	3,891
GPA, girls	-0.013	-0.008	-0.014	-0.018	0.014	-0.000	-0.013
	(0.028)	(0.034)	(0.046)	(0.038)	(0.079)	(0.073)	(0.056)
N	22,380	16,086	6,294	13,229	2,556	2,857	3,738

Note.— Effect on children's standardized compulsory school leaving grades by father's and mother's educational background (secondary, tertiary), based on a DD-model. Control variables included. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.9.
DD estimates of the 1995 parental leave reform by parents' education level: School leaving GPA – based on predicted birth date

		All mothers		Mother non-college		Mother college	
	All	Father Non-college	Father College	Father Non-college	Father college	Father Non-college	Father College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
GPA, all	-0.008	-0.010	0.004	-0.016*	0.004	-0.001	0.010
	(0.007)	(0.008)	(0.012)	(0.009)	(0.017)	(0.019)	(0.015)
N	409,227	291,114	103,891	236,971	47,912	43,505	58,948
GPA, boys	-0.013	-0.027**	0.012	-0.026**	-0.028	-0.011	0.029
	(0.010)	(0.011)	(0.017)	(0.012)	(0.024)	(0.026)	(0.021)
N	208,953	148,563	53,087	120,874	24,456	22,324	30,004
GPA, girls	-0.001	0.004	0.005	-0.008	0.027	0.016	0.005
	(0.010)	(0.012)	(0.017)	(0.013)	(0.024)	(0.027)	(0.021)
N	200,175	142,496	50,772	116,051	23,447	21,172	28,921

Note.— Effect on children's standardized compulsory school leaving grades by father's and mother's educational background (secondary, tertiary), based on a DD-model, where predicted date of birth is used to assign treatment. Control variables included. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.10.
RD-DD estimates of the 1995 parental leave reform by parents' education level: School leaving GPA, STEM/HUMSAM

		All mothers		Mother non-college		Mother college	
	All	Father Non-college	Father College	Father Non-college	Father college	Father Non-college	Father College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
STEM GPA, boys	-0.053**	-0.056*	-0.026	-0.048	0.029	-0.031	-0.057
	(0.025)	(0.029)	(0.043)	(0.032)	(0.067)	(0.064)	(0.055)
HUMSAM GPA, boys	-0.032	-0.046	0.013	-0.040	0.029	-0.011	0.018
	(0.024)	(0.028)	(0.043)	(0.031)	(0.068)	(0.063)	(0.054)
N	156,153	111,744	44,409	91,373	17,984	20,371	26,425
STEM GPA, girls	-0.021	0.000	-0.051	-0.014	-0.003	0.023	-0.068
	(0.025)	(0.030)	(0.042)	(0.033)	(0.067)	(0.064)	(0.053)
HUMSAM GPA, girls	-0.002	0.012	-0.012	-0.001	-0.068	0.032	0.039
	(0.025)	(0.030)	(0.042)	(0.034)	(0.068)	(0.065)	(0.051)
N	149,598	107,160	42,438	87,657	17,070	19,503	25,368

Note.— Estimated discontinuities from RD-DD model on children's subject-specific standardized compulsory school leaving grades by father's and mother's educational background (secondary, tertiary). We use a linear function for the running variable and apply triangular regression weights. Estimates are based on daily detail on the running variable. Control variables included. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A.11.
RD-DD estimates of the 1995 parental leave reform on intergenerational transmission of human capital: Child-uncle cognitive skills (mother's brother)

	(1) All	(2) Boys	(3) Girls
r	0.142***	0.164***	0.120***
	(0.011)	(0.015)	(0.016)
T	0.029*	0.001	0.032
	(0.017)	(0.024)	(0.025)
$r \times T$	-0.001	-0.018	0.010
	(0.023)	(0.031)	(0.033)
Uncle's cognitive skills	0.156***	0.144***	0.171***
	(0.009)	(0.012)	(0.013)
$r \times$ Uncle's cognitive skills	0.003	0.026	-0.027
	(0.012)	(0.017)	(0.017)
$T \times$ Uncle's cognitive skills	-0.011	0.005	-0.025
	(0.018)	(0.023)	(0.026)
$r \times T \times$ Uncle's cognitive skills	0.016	0.001	0.038
	(0.025)	(0.033)	(0.035)
N	162,994	83,201	79,793

NOTE.— Estimated discontinuities from RD-DD model on children's intergenerational correlation in human capital (uncle's standardized cognitive skills from military enlistment). r is an indicator that takes the value 1 if child i is born in January or later, T is an indicator that takes the value 1 if the child is born in the treatment cohort, and zero otherwise. We use a linear function for the running variable and apply triangular regression weights. Control variables included. Estimates are based on daily detail on the running variable. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A.12.
RD-DD estimates of the 1995 parental leave reform on intergenerational transmission of human capital: Father's non-cognitive skills

	(1) All	(2) Boys	(3) Girls
r	0.134***	0.137***	0.130***
	(0.009)	(0.012)	(0.013)
T	0.011	0.009	0.019
	(0.014)	(0.019)	(0.021)
$r \times T$	-0.021	-0.039	-0.010
	(0.019)	(0.026)	(0.028)
Father's non-cognitive skills	0.185***	0.188***	0.175***
	(0.007)	(0.010)	(0.011)
$r \times$ Father's non-cognitive skills	-0.010	-0.016	0.005
	(0.010)	(0.013)	(0.014)
$T \times$ Father's non-cognitive skills	-0.020	-0.030	-0.011
	(0.015)	(0.020)	(0.022)
$r \times T \times$ Father's non-cognitive skills	-0.001	0.015	-0.023
	(0.020)	(0.027)	(0.029)
N	245,828	125,731	120,097

NOTE.— Estimated discontinuities from RD-DD model on children's intergenerational correlation in human capital (father's standardized non-cognitive skills from military enlistment). r is an indicator that takes the value 1 if child i is born in January or later, T is an indicator that takes the value 1 if the child is born in the treatment cohort, and zero otherwise. We use a linear function for the running variable and apply triangular regression weights. Estimates are based on daily detail on the running variable. Control variables included. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

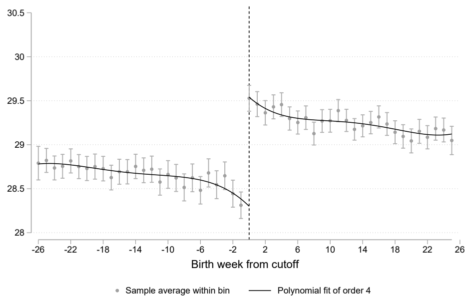
Table A.13.
RD-DD estimates of the 1995 parental leave reform by father's education level: Couple dissolution

		All mothers		Mother non-college		Mother college	
	All	Father Non-college	Father College	Father Non-college	Father college	Father Non-college	Father College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome variable:							
Separated by year 3	0.016***	0.023***	-0.001	0.024***	0.019	-0.014	0.007
	(0.006)	(0.008)	(0.009)	(0.009)	(0.014)	(0.017)	(0.010)
Separated by year 5	0.007	0.016*	-0.015	0.014	0.020	-0.050**	0.009
	(0.007)	(0.009)	(0.011)	(0.011)	(0.017)	(0.020)	(0.012)
N	304,275	218,483	85,792	178,721	39,762	34,736	51,056

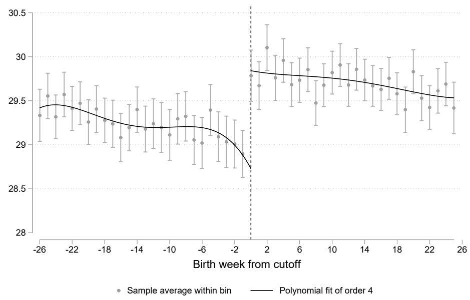
Note.— Estimated discontinuities from RD-DD model on the likelihood that parents divorced/separated by child age 3 or 5. We use a linear function for the running variable and apply triangular regression weights. Control variables included. Estimates are based on daily detail on the running variable. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure A.3.
Covariate balance, control cut-offs vs. treatment cut-off: Mother's characteristics

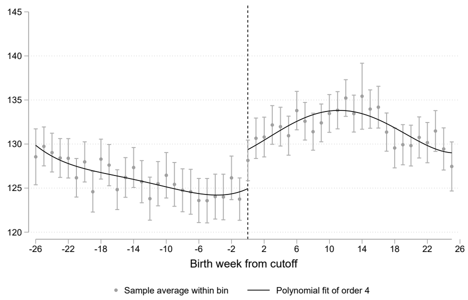
(a) Age mother, control



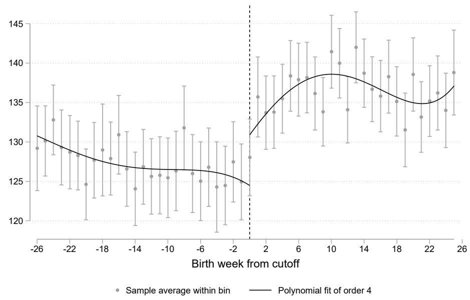
(b) Age mother, treatment



(c) Income mother, control



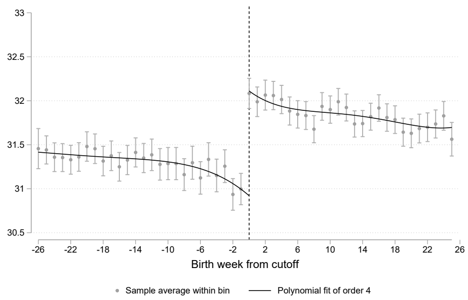
(d) Income mother, treatment



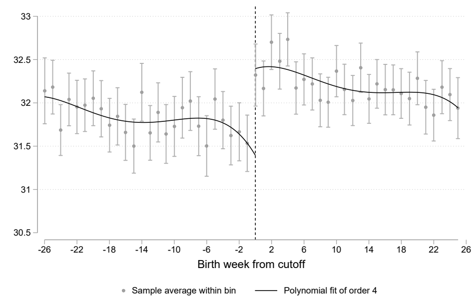
Note.— RD estimates of the effect of the reform on pre-determined characteristics, separately for treatment- and control cohorts. We use a linear function for the running variable and apply triangular regression weights. Estimates are based on daily detail on the running variable. Estimates are based on daily detail on the running variable.

Figure A.4.
Covariate balance, control cut-offs vs. treatment cut-off: Father's characteristics

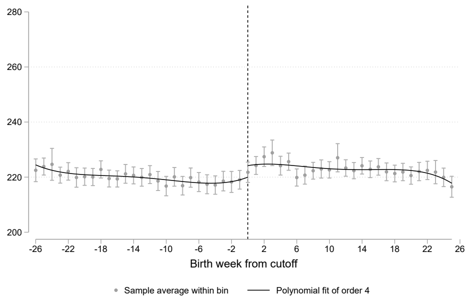
(a) Age father, control



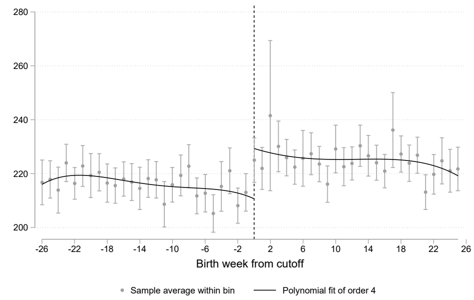
(b) Age father, treatment



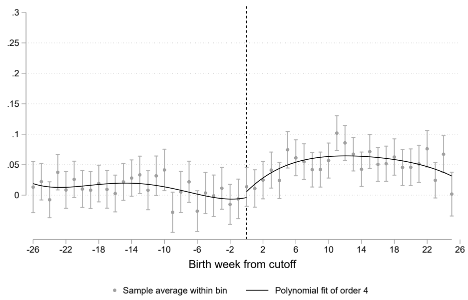
(c) Income father, control



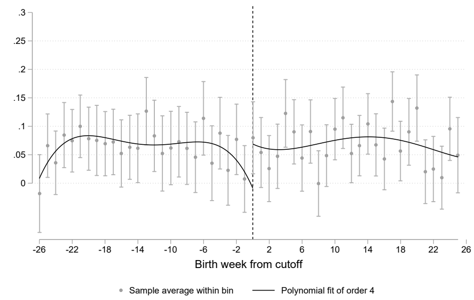
(d) Income father, treatment



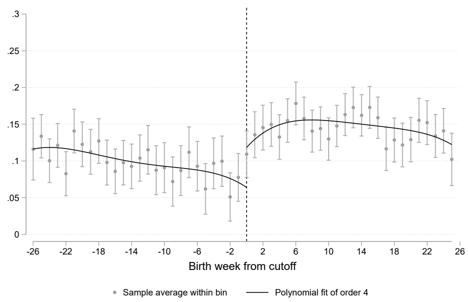
(e) Father's cognitive skills, control



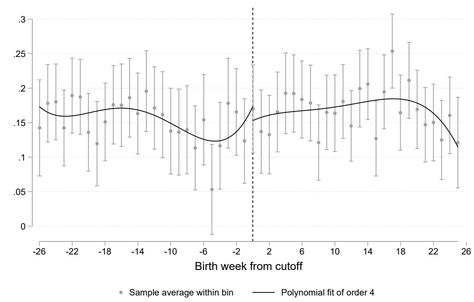
(f) Father's cognitive skills, treatment



(g) Father's non-cognitive skills, control



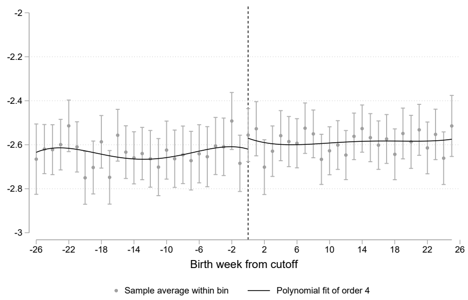
(h) Father's noncognitive skills, treatment



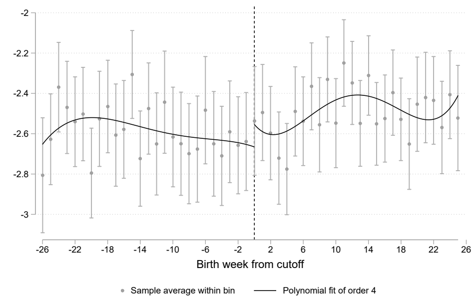
Note.—RD estimates of the effect of the reform on pre-determined characteristics, separately for treatment- and control cohorts. We use a linear function for the running variable and apply triangular regression weights. Estimates are based on weekly detail on the running variable.

Figure A.5.
Covariate balance, control cut-offs vs. treatment cut-off: Couple characteristics

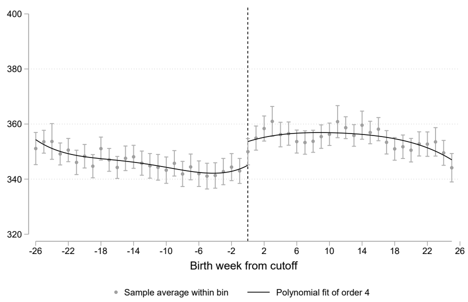
(a) Age gap, control



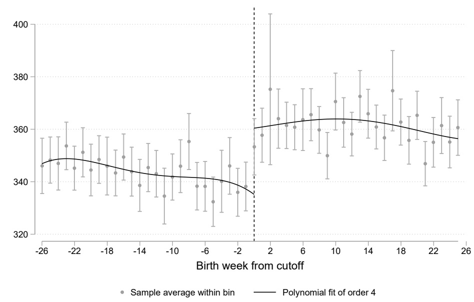
(b) Age gap, treatment



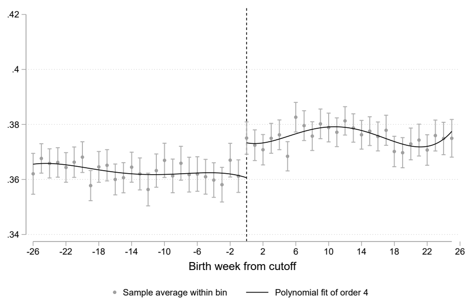
(c) HH income, control



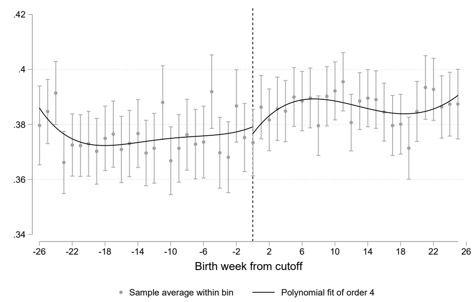
(d) HH income, treatment



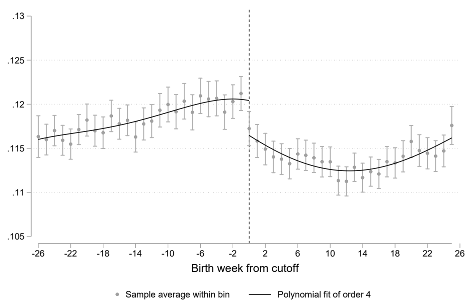
(e) Mother's share of HH income, control



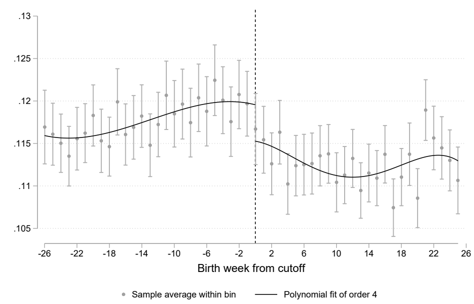
(f) Mother's share of HH income, treatment



(g) Predicted separation, control



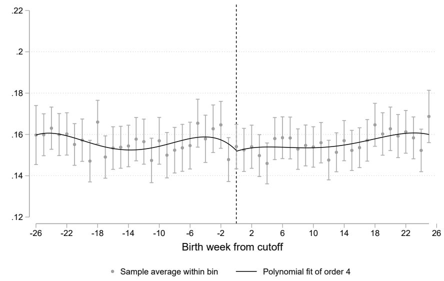
(h) Predicted separation, treatment



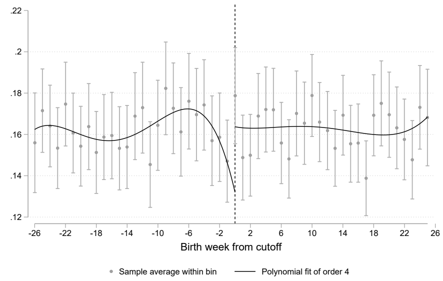
Note.— RD estimates of the effect of the reform on pre-determined characteristics, separately for treatment- and control cohorts. We use a linear function for the running variable and apply triangular regression weights. Estimates are based on weekly detail on the running variable.

Figure A.6.
Covariate balance, control cut-offs vs. treatment cut-off: Child characteristics

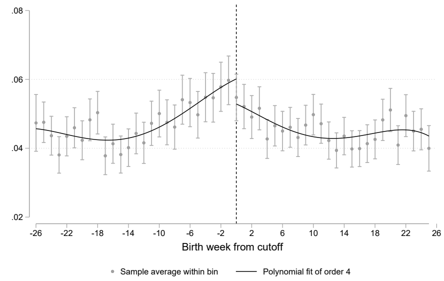
(a) Low APGAR, control



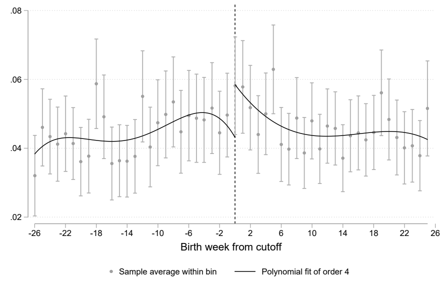
(b) Low APGAR, treatment



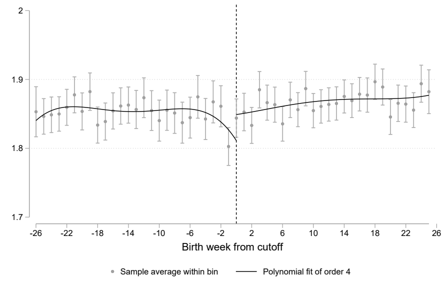
(c) Pre-term birth, control



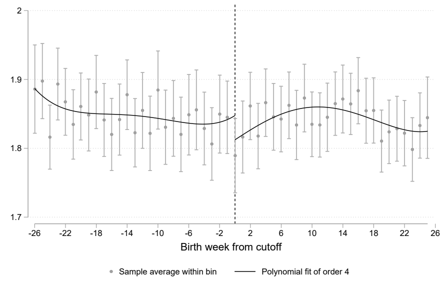
(d) Pre-term birth, treatment



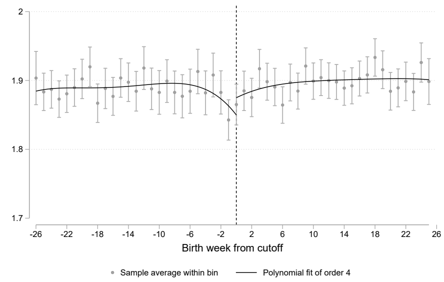
(e) Birth order mother, control



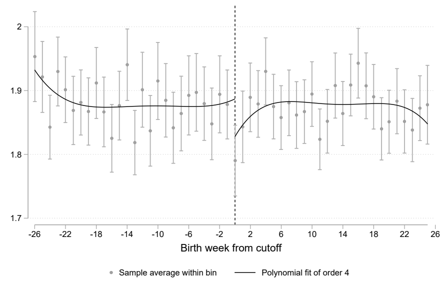
(f) Birth order mother, treatment



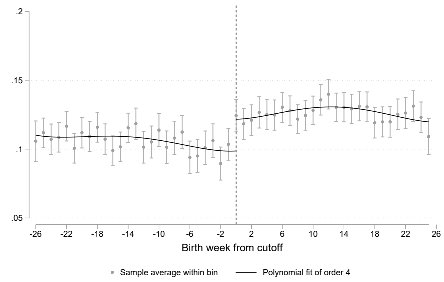
(g) Birth order father, control



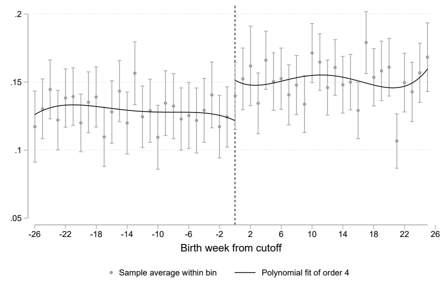
(h) Birth order father, treatment



(i) Predicted GPA, control



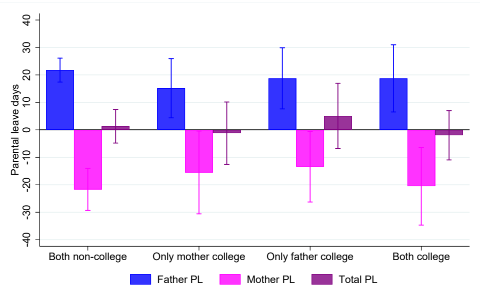
(j) Predicted GPA, treatment



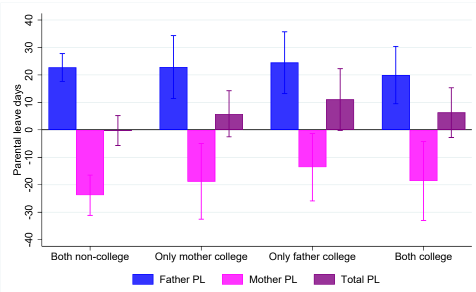
Note.— RD estimates of the effect of the reform on pre-determined characteristics, separately for treatment- and control cohorts.We use a linear function for the running variable and apply triangular regression weights. Estimates are based on weekly detail on the running variable.

Figure A.7.
RD estimates of the effect of the 1995 parental leave reform on fathers' leave, mothers' leave, and total leave days, respectively

(a) Boys



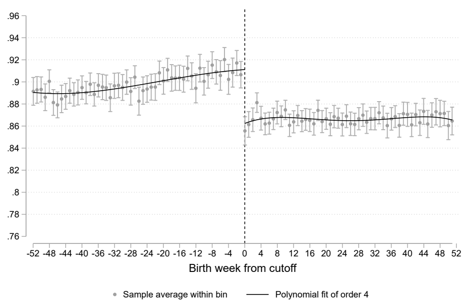
(b) Girls



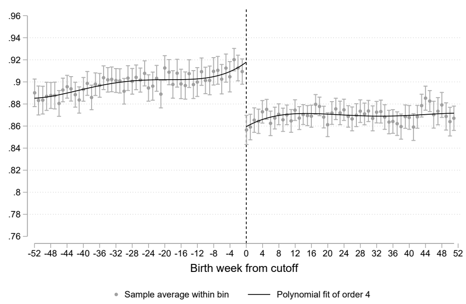
Note.— Estimates are based on daily detail on the running variable. We use a linear function for the running variable and apply a triangular kernel. Estimates are based on weekly detail on the running variable. The sample includes all parents to children born in 1994–1995.

Figure A.8.
RDD estimates of the effect of the reform on mother's share of HH income, by child sex

(a) Boys



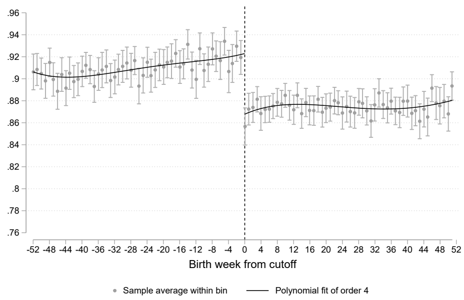
(b) Girls



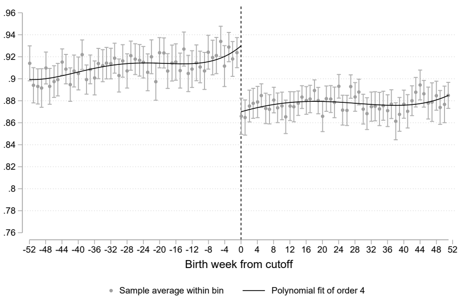
Note.— Estimates are based on weekly detail on the running variable. We use a linear function for the running variable and apply a triangular kernel. The dots illustrate averages of the outcome variable in weekly bins from the cutoff.

Figure A.9.
RDD estimates of the effect of the reform on mother's share of HH income, by child sex and parental education mix

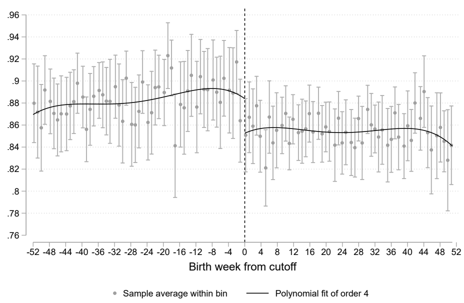
(a) Both parents non-college, boys



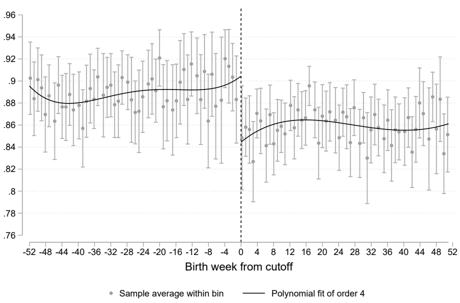
(b) Both parents non-college, girls



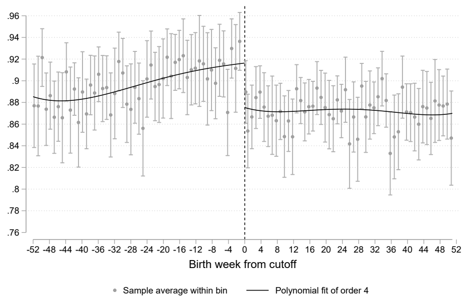
(c) Only mother college, boys



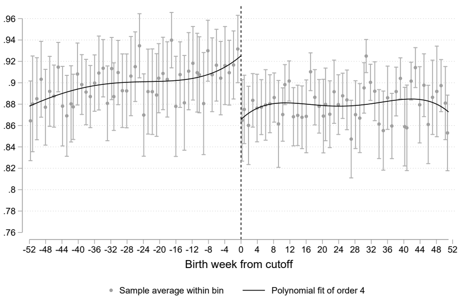
(d) Only mother college, girls



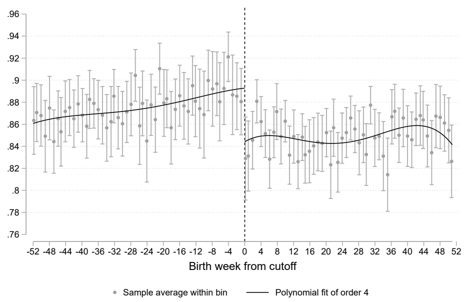
(e) Only father college, boys



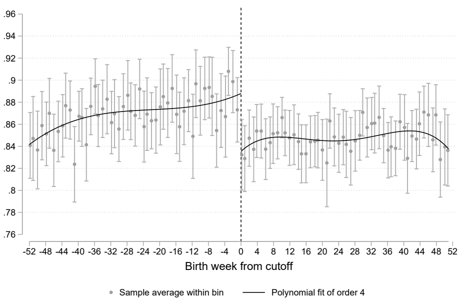
(f) Only father college, girls



(g) Both parents college, boys

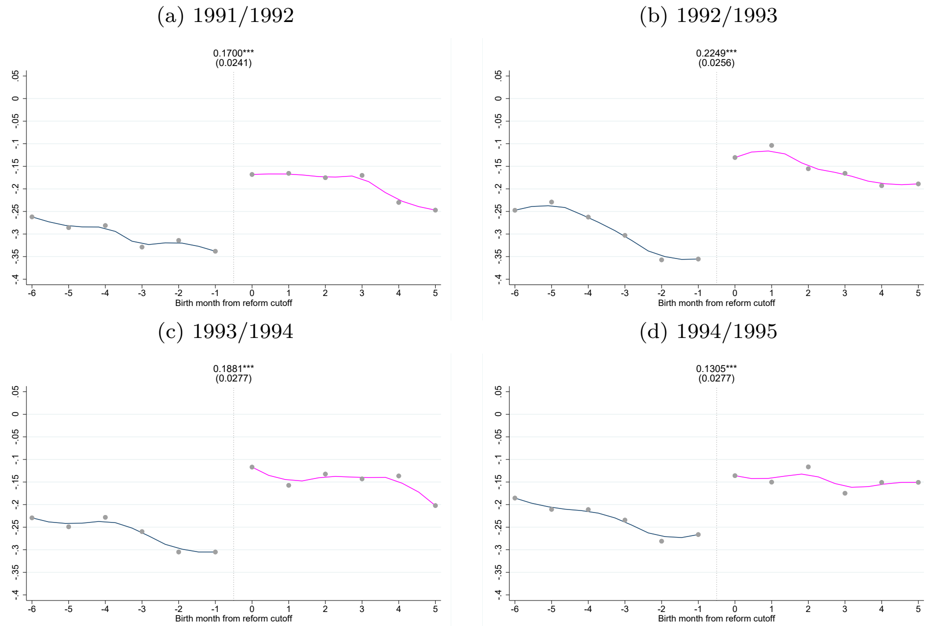


(h) Both parents college, girls



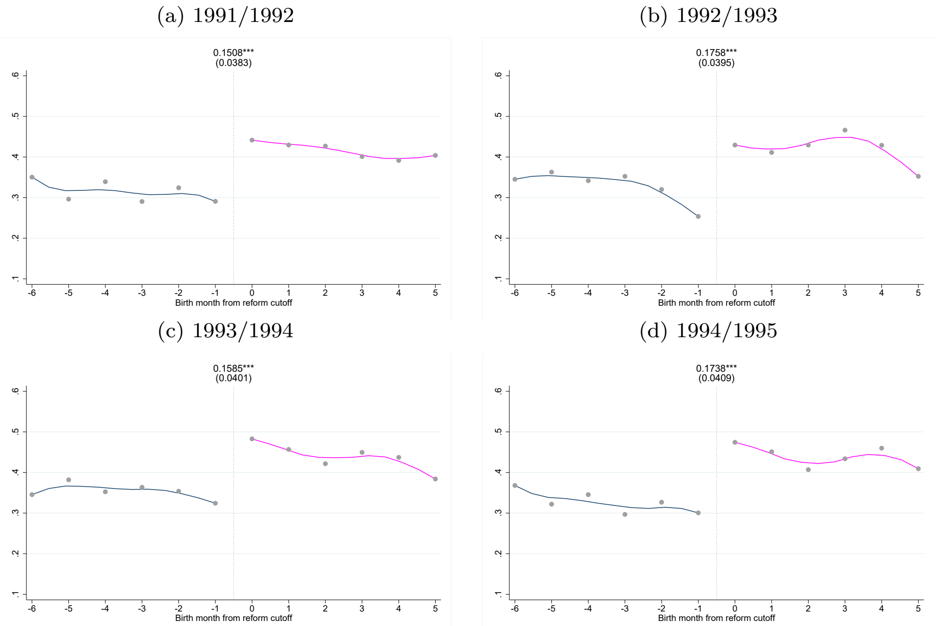
Note.— Estimates are based on weekly detail on the running variable. We use a linear function for the running variable and apply a triangular kernel. The dots illustrate averages of the outcome variable in weekly bins from the cutoff.

FIGURE A.10.
Separate RDD estimates of discontinuities at the cutoff by cohort: GPA of boys to non-college fathers



NOTE.— Each point in the graph represents monthly bin averages of the outcome variable indicated in the figure heading. RDD point estimate of the discontinuity at the control-year cutoffs (panels a, b, and c) and treatment cutoff (panel d), respectively are indicated in the top middle of each graph. Estimates are based on monthly detail on the running variable. We use a linear function for the running variable and apply a triangular kernel.

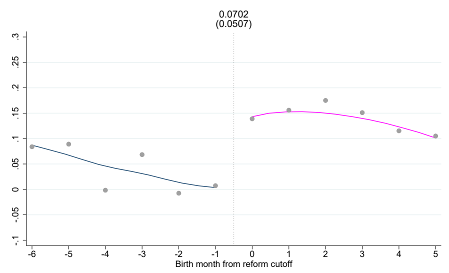
FIGURE A.11.
Separate RDD estimates of discontinuities at the cutoff by cohort: GPA of boys to college fathers



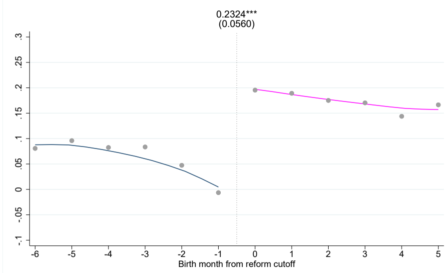
NOTE.— Each point in the graph represents monthly bin averages of the outcome variable indicated in the figure heading. RDD point estimate of the discontinuity at the control-year cutoffs (panels a, b, and c) and treatment cutoff (panel d), respectively are indicated in the top middle of each graph. Estimates are based on monthly detail on the running variable. We use a linear function for the running variable and apply a triangular kernel.

Figure A.12.
Separate RDD estimates of discontinuities at the cutoff by cohort: GPA of girls to non-college fathers

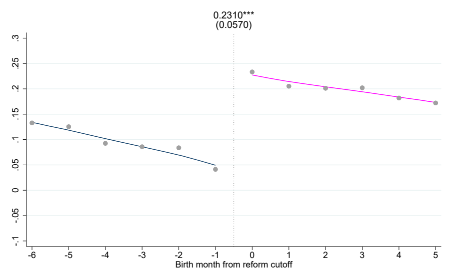
(a) 1991/1992



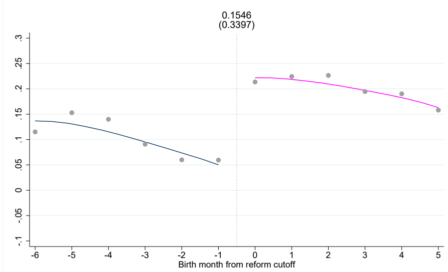
(b) 1992/1993



(c) 1993/1994



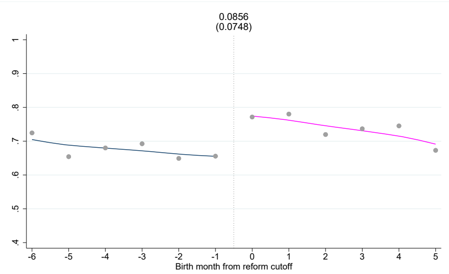
(d) 1994/1995



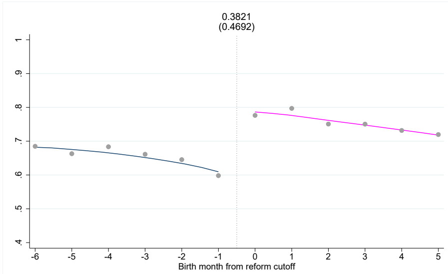
Note.— Each point in the graph represents monthly bin averages of the outcome variable indicated in the figure heading. RDD point estimate of the discontinuity at the control-year cutoffs (panels a, b, and c) and treatment cutoff (panel d), respectively are indicated in the top middle of each graph. Estimates are based on monthly detail on the running variable. We use a linear function for the running variable and apply a triangular kernel.

Figure A.13.
Separate RDD estimates of discontinuities at the cutoff by cohort: GPA of girls to college fathers

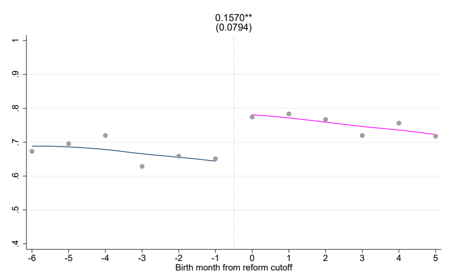
(a) 1991/1992



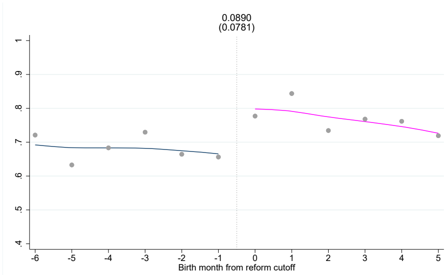
(b) 1992/1993



(c) 1993/1994



(d) 1994/1995

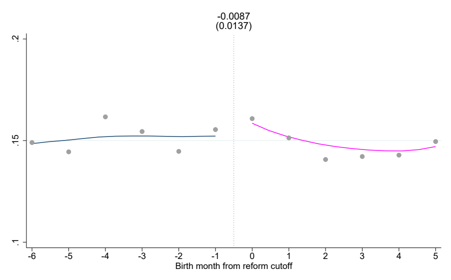


Note.— Each point in the graph represents monthly bin averages of the outcome variable indicated in the figure heading. RDD point estimate of the discontinuity at the control-year cutoffs (panels a, b, and c) and treatment cutoff (panel d), respectively are indicated in the top middle of each graph. Estimates are based on monthly detail on the running variable. We use a linear function for the running variable and apply a triangular kernel.

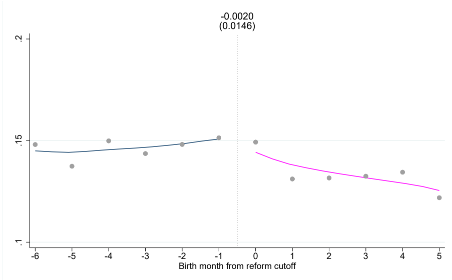
Figure A.14.

Separate RDD estimates of discontinuities at the cutoff by cohort:
Couple separation by year 3 among non-college fathers

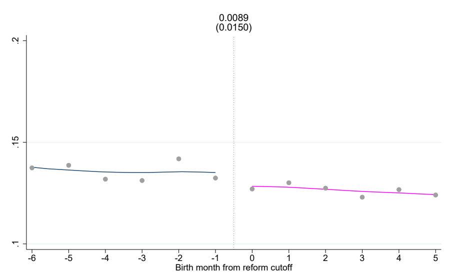
(a) 1991/1992



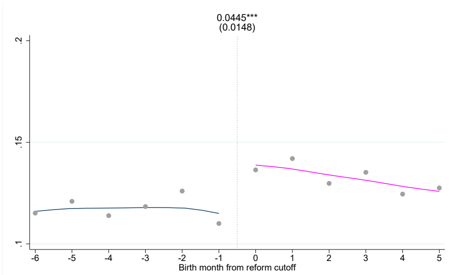
(b) 1992/1993



(c) 1993/1994



(d) 1994/1995

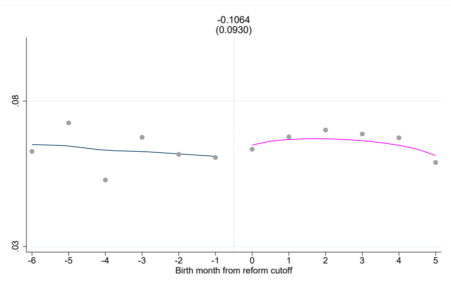


Note.— Each point in the graph represents monthly bin averages of the outcome variable indicated in the figure heading. RDD point estimate of the discontinuity at the control-year cutoffs (panels a, b, and c) and treatment cutoff (panel d), respectively are indicated in the top middle of each graph. Estimates are based on monthly detail on the running variable. We use a linear function for the running variable and apply a triangular kernel.

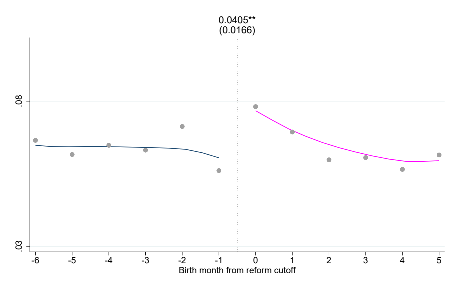
Figure A.15.

Separate RDD estimates of discontinuities at the cutoff by cohort:
Couple separation by year 3 among college fathers

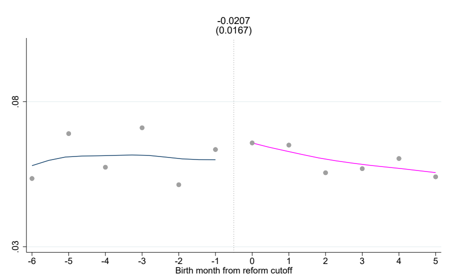
(a) 1991/1992



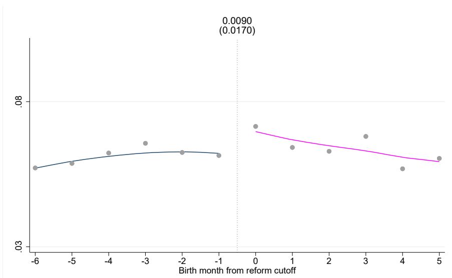
(b) 1992/1993



(c) 1993/1994



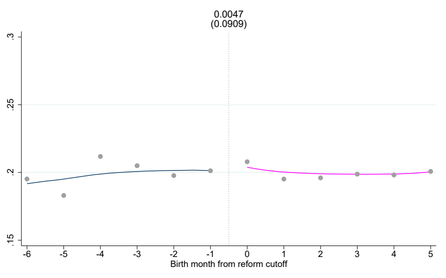
(d) 1994/1995



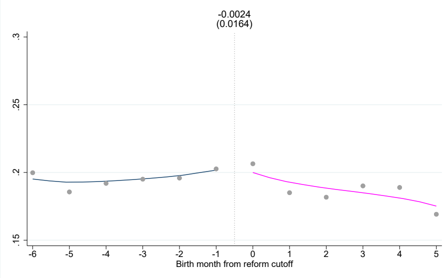
Note.— Each point in the graph represents monthly bin averages of the outcome variable indicated in the figure heading. RDD point estimate of the discontinuity at the control-year cutoffs (panels a, b, and c) and treatment cutoff (panel d), respectively are indicated in the top middle of each graph. Estimates are based on monthly detail on the running variable. We use a linear function for the running variable and apply a triangular kernel.

FIGURE A.16.
Separate RDD estimates of discontinuities at the cutoff by cohort:
Couple separation by year 5 among non-college fathers

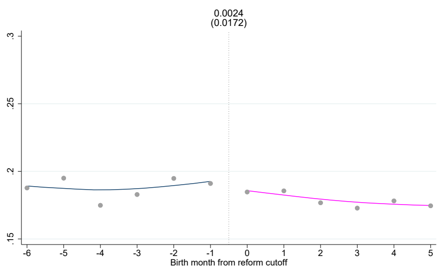
(a) 1991/1992



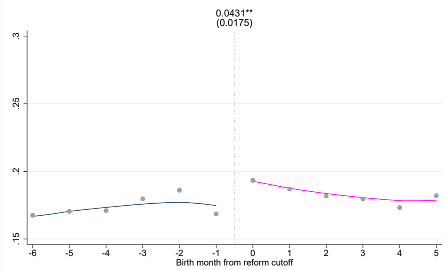
(b) 1992/1993



(c) 1993/1994



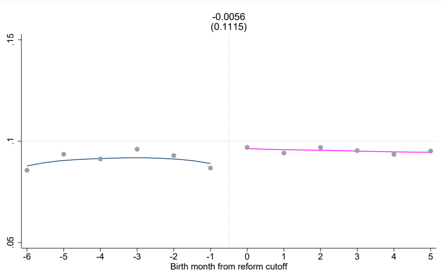
(d) 1994/1995



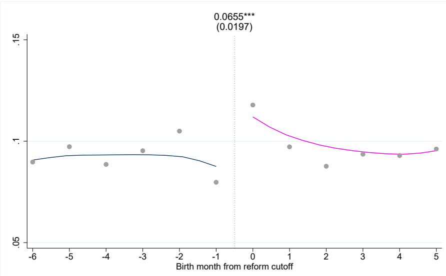
NOTE.— Each point in the graph represents monthly bin averages of the outcome variable indicated in the figure heading. RDD point estimate of the discontinuity at the control-year cutoffs (panels a, b, and c) and treatment cutoff (panel d), respectively are indicated in the top middle of each graph. Estimates are based on monthly detail on the running variable. We use a linear function for the running variable and apply a triangular kernel.

FIGURE A.17.
Separate RDD estimates of discontinuities at the cutoff by cohort:
Couple separation by year 5 among college fathers

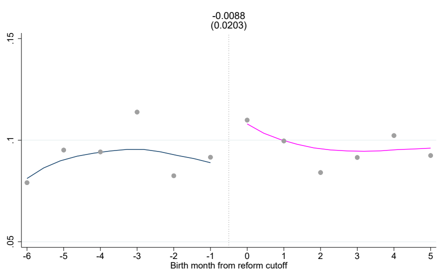
(a) 1991/1992



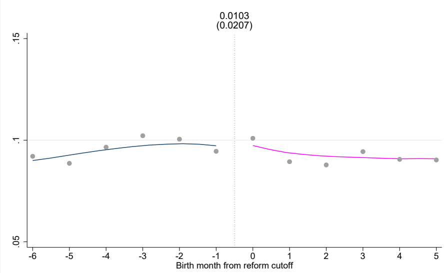
(b) 1992/1993



(c) 1993/1994



(d) 1994/1995



NOTE.— Each point in the graph represents monthly bin averages of the outcome variable indicated in the figure heading. RDD point estimate of the discontinuity at the control-year cutoffs (panels a, b, and c) and treatment cutoff (panel d), respectively are indicated in the top middle of each graph. Estimates are based on monthly detail on the running variable. We use a linear function for the running variable and apply a triangular kernel.